Unemployment Insurance, Starting Salaries, and Jobs: Evidence from Multi-state Firms

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We study the labor market effects of permanent 30-64% reductions in unemployment insurance benefits available in seven states. Leveraging linked firm-establishment data, we find that establishments based in reform states experience employment increases that are 0.8-1.3% larger than those of the same firm's establishments in other states. Using a similar multi-state firm design, starting salaries are 1.2-5.5% lower in reform states and posted salaries for the same job fall by 3.2-3.5%. The negative co-movement of employment and wages after the reform suggests a labor supply shock, and mitigates against confounding changes in labor demand driving the results. Our findings are consistent with workers lowering their reservation wages as outside options fall, and employers taking advantage of this by offering lower wages and increasing employment.

Key Words: unemployment insurance, reservation wages, intra-firm employment

JEL Codes: J64, J65, J38

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1 Introduction

Unemployment insurance (UI) is a pillar of the social safety net, providing temporary income to laid-off workers. Standard search models predict that more generous benefits, in terms of duration or benefit payments, will afford individuals more time to find a job, generate competing offers, and increase their match quality (Mortensen 1970; Acemoglu and Shimer 1999; Chetty 2008). However, standard labor supply models also argue that by subsidizing a lengthier job search and allowing workers to substitute leisure for work without enduring a steep loss in income, UI expansions could lower employment growth (e.g., Shavell and Weiss 1979; Gruber 2007). Despite these theoretical predictions as well as empirical evidence that UI crowds out search effort, the existing literature has generally not found sizeable employment and wage responses to changes in UI benefits.[1]

Recent work approaches this puzzle by highlighting the distinction between micro- and macro-level impacts of UI generosity. General equilibrium effects could mute individual-level responses. In particular, job rationing could limit the extent to which increased search effort from UI cuts translates to overall employment gains, or a reduction in UI benefits could exert downward pressure on aggregate demand since job seekers are also consumers (Michaillat 2012; Lalive et al. 2015; Marinescu 2017; Landais et al. 2018; Ganong and Noel 2019; Kekre 2021). However, general equilibrium effects on firms could work in the opposite direction. If reservation wages fall in response to a UI cut, this could increase the demand for labor (Hagedorn et al. 2013; Karahan et al. 2025).

Relative to the existing literature, we make several contributions to the evidence on how UI affects wages and employment at the macro level. First, we bring to bear an approach never before used to study these questions: (i) we compare changes in employment across different establishments within the same firm but operating in treated versus untreated states and (ii) we compare changes in posted wages in ads for the same job and firm but likewise across establishments in treated versus

[1] For the search effort margin, see Barron and Mellow (1979); Krueger and Mueller (2010; 2012); Baker and Fradkin (2017); Marinescu (2017); Marinescu et al. (2021). For employment, Schmieder et al. (2010); Boone et al. (2021a); Chodorow-Reich et al. (2019); Dieterle et al. (2020) find no effect, Schmieder et al. (2012) report small negative effects, while Hagedorn et al. (2025) and Johnston and Mas (2018) document larger negative macro effects. For wages, recent evidence fails to find meaningful positive effects (DellaVigna and Paserman 2005; Card et al. 2007; Lalive 2007; Van Ours and Vodopivec 2008; Johnston and Mas 2018; Le Barbanchon et al. 2019; Jäger et al. 2020) or even detects a negative effect (Schmieder and Von Wachter 2016). Two notable exceptions find a positive effect (Hagedorn et al. 2013; Nekoei and Weber 2017).

untreated states. Second, we use novel microdata on establishment-level employment (from the EEOC), starting salaries (from Glassdoor), and posted wages (from Burning Glass Technologies). Third, we focus on cuts to state UI programs which are large, permanent, and enacted during a period of economic growth. In the penultimate section of the paper, we discuss how these empirical innovations help to explain why our findings diverge from much of the existing literature.

Using this approach and data, we estimate an increase in employment and a decrease in starting salaries following cuts to the generosity of UI. This is consistent with workers reducing their reservation wages in response to depressed outside options, and employers taking advantage of this by offering lower wages and ramping up their hiring. The negative co-movement of employment and wages post reform suggests a labor supply shock, and mitigates against confounding changes in labor demand driving the results.

Specifically, we study the employment and wage responses to reforms in 7 different states which permanently cut the generosity of their state UI programs in the 2010s in what would become the largest rollback ever implemented in the programs' nearly 80-year history. Six "moderate reform" states (Florida, Georgia, Kansas, Michigan, Missouri, and South Carolina) reduced maximum UI duration by an average of 30% (from 26 to 18 weeks). North Carolina also reduced maximum duration, but took the additional step of cutting maximum weekly UI benefits by 35% (from \$535 to \$350).

A threat to identifying the causal effects of these reforms is the possibility that these cuts were enacted because these states had been subjected to larger than average Great Recession shocks which depleted their Unemployment Insurance Trust Funds (UTFs). However, evidence suggests that the reason for the rollbacks was political rather than economic, as the UTFs of 27 other states also became insolvent in the early 2010s, yet none of these states trimmed their UI programs (Bivens et al. 2014).[2] Empirically, we show that using as controls other states whose UTF accounts became insolvent or who experienced similar Great Recession shocks yields similar results. This helps rule out policy endogeneity or mean reversion as competing explanations. We also show robustness to

[2]Bivens et al. (2014) write: "Trust fund imbalances largely cannot explain why some states shortened UI Durations while others did not. Only eight of the 35 states whose UTF accounts became insolvent following the Great Recession tried to address the situation by cutting the duration of their benefits. These states' UTF accounts as a whole were not appreciably worse off... What most of the eight states do share is a recent history of not supporting safety-net programs."

excluding states with competing reforms (minimum wage increases, Medicaid expansions, right-to-work laws, personal tax changes, and corporate tax reforms).

We first document the effect of the reforms on UI receipt and unemployment. After establishing the salience and magnitude of the reform, we turn to our microdata and novel identification strategy which leverages linked firm-establishment panel data. To study establishment-level employment effects, we use data which the Equal Employment Opportunity Commission (EEOC) collects to help fulfill their mandate. These data cover roughly 55 million private employees annually, or approximately 46% of the entire US private workforce. We compare employment levels across establishments operating in different states, but which are part of the same multi-state firm, before and after a UI reform. This takes advantage of the fact that establishments within the same multi-state firm provide a good counterfactual, as argued by Yagan (2019), due to the tendency of establishments to offer similar jobs and have similar workplace structures.[3]

Our identifying assumption is that, conditional on the included establishment and firm-year fixed effects, intra-firm employment across establishments would have evolved similarly after the reform in the absence of the reform. This is a similar assumption used in work which evaluates the end of Emergency Unemployment Compensation (EUC) after the Great Recession (e.g., Hagedorn et al. 2025). As we document, there are parallel pre-trends for treated and control states prior to the UI reform, consistent with this assumption.

Our first key finding is that following the reform, North Carolina-based establishments experience employment increases that are 1.3% larger than their same-firm counterparts in other states over the two years after the reform. For the 6 moderate reform states, employment increases 0.8% more relative to same-firm counterparts in other states. These results suggest that any contractionary effect on consumer spending and aggregate demand, or increased competition for jobs, are not large enough to overturn the incentive effects of finding a new job quickly.

We then turn to our analysis of how the UI reforms affected starting salaries. Our second key finding is that the realized and posted wages for new hires falls in response to the reform. We focus

[3]For example, recent evidence indicates that discriminatory hiring practices are highly concentrated among particular firms, with little geographic variation across their constituent establishments (Kline et al. 2022). A 2015 survey of 2,000 firms by the Society for Human Resource Management likewise finds that more than 70% use a centralized HR decision-making authority.

on the wages of new hires given downward wage stickiness for already employed workers (Kahn 1997; Pissarides 2009; Haefke et al. 2013). Using data from Glassdoor, we find that the earnings of new hires fall by an economically and statistically significant 5.5% in North Carolina establishments relative to the same firm's establishments in control states. In the moderate reform states, the corresponding effect is a statistically significant 1.2% decline.[4] The drop in earnings is unlikely to be explained by negative worker composition effects, as new hires in the post-reform period are not negatively selected based on demographic characteristics using CPS data. However, one limitation of the Glassdoor data is that we cannot rule out the impact of unobserved degradations in match quality or unobserved changes in the composition of new hires.

We overcome this limitation by estimating the reforms' effects on posted wages for the same job, within the same firm, but across treated versus non-treated establishments.[5] The data come from the near-universe of posted wages in online job ads collected by Burning Glass Technologies. As noted by Hazell and Taska (2025), posted wages have the key advantage that they are not contaminated by the compositional or match quality effects which could be present in the observed wages of new hires. To appreciate the power of this data, imagine a world in which the wage for any particular job remains unchanged, yet starting salaries fall because workers downgrade to a lower-paying occupation or to a lower-paying firm in response to a UI cut. In this world, comparing posted wages for the same job within the same company but across establishments in treated versus untreated states would correctly estimate no change in wages. However, we estimate that the reforms generated a 3.5% reduction in posted wages in North Carolina and a 3.2% reduction in the moderate reform states.

The negative effect on wages argues against confounding changes in labor demand driving the employment results, and instead argues for a labor supply shock. Likewise, these negative wage effects are inconsistent with mean reversion as the explanation for why reform states experienced faster employment growth following the Great Recession. Specifically, if establishments in reform states had been growing faster only due to having experienced a larger decline in aggregate em-

[4] A robustness analysis using workers with less than one year of tenure in Current Population Survey (CPS) data confirms these findings for starting salaries.

[5] Following Hazell and Taska (2025), we define a job as a standard occupation code-pay frequency-salary type. Examples of salary type are base pay versus commission. Importantly, the 831 occupation codes we use are highly detailed (e.g., pest control worker or home health aide).

ployment during the Great Recession, starting wages would have likewise risen faster than those offered in control establishments. But we find the opposite.

As we discuss at the end of the paper, our findings are consistent with the seminal models of Mortensen and Pissarides (1994), where workers' reservation wages fall when outside options worsen, and Chodorow-Reich et al. (2019), which emphasizes the opportunity costs of employment and the tightness of the labor market. But our estimated effects on employment and wages stand in contrast to much of the existing literature, which finds little effect on either margin. There are several possible explanations. First, we study cuts which were permanent and large in percent terms, affected short-term benefits, and were enacted during a period of economic growth. This contrasts with other work which leverages reforms which were smaller in percent terms, affected in-the-distant-future extensions, and enacted temporarily during periods of high unemployment. Another reason is that our multi-state firm design compares establishments within the same firm, but operating in different states and therefore in different local labor markets for the most part. In contrast, designs using a border county-pair design are often comparing establishments which compete for the same pool of workers. Finally, our novel microdata combined with a multi-state firm design allow us to better control for variation in job types across treatment and control states.

Our study brings into sharp focus the important policy tradeoffs that must be considered when deciding on the generosity of UI benefits. On the one hand, UI benefit reductions stimulate employment and reduce the fiscal burden of UI programs. But counterbalancing this is a nontrivial reduction in worker wages – a reduction generated by job seekers facing lower starting salaries for the same jobs.

The next section provides background information on the reforms and describes the data sources we use in the paper. Section 3 describes our multi-state firm research design. Section B documents effects on UI receipt and unemployment. Sections 4 and 5, respectively, present our main empirical findings for employment and wages. Section 6 provides supporting evidence using CPS data. Section 7 discusses possible reasons why our estimates diverge from prior research, with Section 8 offering concluding remarks.

2 Setting and Data

2.1 State UI Reforms

During the protracted recovery from the Great Recession, unemployment trust funds in 35 states reached insolvency as a result of record benefit payouts. In response, 7 of these states passed legislation that permanently cut the number of weeks available through regular UI from the long-established norm of 26 weeks. Three of these states cut benefits to 20 weeks, while the others had cuts which depended on the state unemployment rate and could be even larger (GAO 2015). These 7 states were Florida, Georgia, Kansas, Michigan, Missouri, North Carolina, and South Carolina.[6] North Carolina went a step further than the other states, becoming the only one to contemporaneously reduce its maximum weekly benefits (by 35% from \$535 to \$350 per week) while also reducing the maximum benefit duration by at least 6 weeks. Details on each of these reforms are found in Table 1.

Table 1: Permanent Cuts to Regular State UI Programs

State	**Date**	**Max. duration**	**Max. weekly benefit**	**Ave. post-reform total reduction**
North Carolina	July 2013	26→12-20 weeks	\$535→\$350	64%
	Notes: Max. duration drops to 12 when state UR < 5.5%, increasing by 1 week for each 0.5% increase until reaching a maximum of 20 weeks at a 9% UR			
Florida	January 2012	26→12-23 weeks	No change	38%
	Notes: Max. duration drops to 12 when state UR < 5%, increasing by 1 week for each 0.5% increase until reaching a maximum of 23 weeks at a 10.5% UR			
Georgia	July 2012	26→14-20 weeks	No change	39%
	Notes: Max. duration drops to 14 when state UR < 6.5%, increasing by 1 week for each 0.5% increase until reaching a maximum of 20 weeks at a 9% UR			
Kansas	January 2014	26→14-26 weeks	No change	35%
	Notes: Max. duration drops to 16 when state UR < 4.5%, increasing to 20 when 4.5% > UR ≤ 6%, and increasing to 26 when UR > 6%			
Michigan	January 2012	26→20 weeks	No change	23%
Missouri	April 2011	26→20 weeks	No change	23%
South Carolina	June 2011	26→20 weeks	No change	23%

Notes: These states also implemented a variety of stricter eligibility restrictions, such as longer waiting periods and disqualifying individuals who had lost a job for "good cause" (e.g., providing family caregiving or following a spouse forced to relocate for work), and imposing additional work search requirements. Ave. post-reform total reduction is the average cut in maximum benefits in the post-reform periods covered by our analysis.

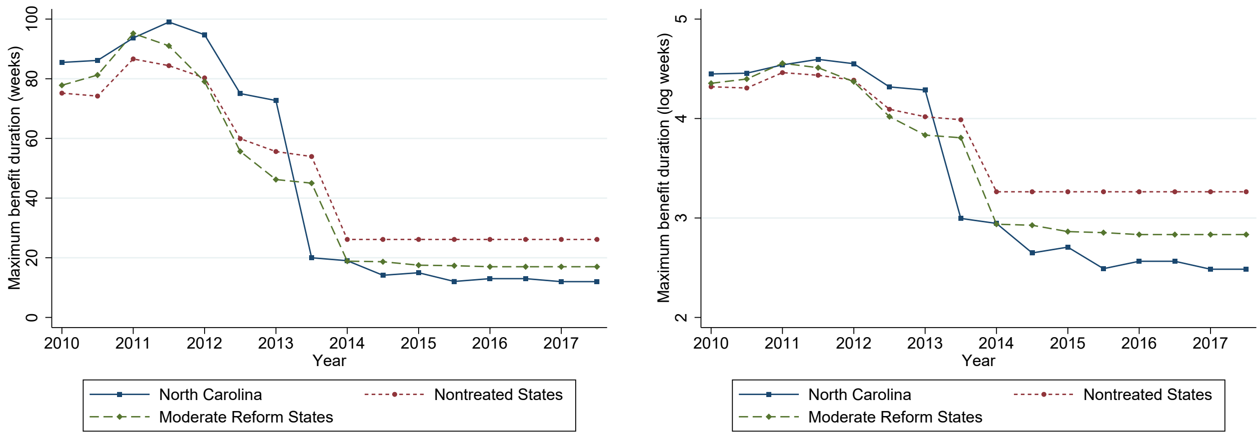
[6] Arkansas permanently reduced its maximum duration to 25 weeks, and Illinois implemented a temporary 1 week cut. We exclude both of these states throughout the paper.

These reforms provide an ideal setting for studying the effects of UI generosity on employment and wages for four related reasons. First, the cuts to regular state UI programs were both permanent and the largest in the programs' nearly 80-year history. In the moderate reform states (i.e., excluding North Carolina), maximum duration decreased on average by 30% (from 26 to 18 weeks) during the post reform periods we study. In North Carolina, the combined drop in benefit weeks and amounts reduced maximum benefits in dollar terms by at least 50%, from $14,000 to $7,000, and by as much as 70% if the state unemployment rate fell enough; the average post-reform reduction was 64%. The unparalleled magnitude of the reforms raises the likelihood that attendant effects on wages, which have been found to be empirically small in much of the previous literature,[7] will be detectable.

Figure 1 plots the maximum combined benefit duration from all sources (regular state benefits, Extended Benefits, and Emergency Unemployment Compensation) over time for North Carolina, the moderate reform states (equally weighted), and the nontreated states (equally weighted) (Boone et al. (2021b); U.S. Department of Labor (2024b). Panel (a) plots maximum duration in weeks, while panel (b) uses the natural log of weeks. Before the exhaustion of the Extended Benefit (EB) program midway through 2012, maximum duration was at historically high levels in all states. Maximum duration remained relatively high until the end of the federal Emergency Unemployment Compensation (EUC) program at the start of 2014. Note that North Carolina's decision to reduce benefit amounts and duration violated the "non-reduction" rule, thus terminating its federal EUC agreement, which further reduced maximum UI eligibility duration by an additional 47 additional weeks between July 2013 and January 2014.

As panel (b) shows, in percent terms the differences between the three groups of states was relatively modest prior to states implementing UI reforms. If anything, North Carolina's maximum duration was slightly higher in the 18 months prior to its reform date of July 2013. However, once the EB and EUC programs end and the reforms start to kick in, the differences between treatment and control states become sizable. Post reform, North Carolina's maximum duration reaches a nadir of 12 weeks compared to 26 in nontreated states, amounting to more than a 50% reduction.

[7] See Card et al. (2007); Lalive (2007); Van Ours and Vodopivec (2008); Centeno and Novo (2009); Degen and Lalive (2013); Schmieder and Von Wachter (2016); Nekoei and Weber (2017).



(a) In levels

(b) In logs

Figure 1: Maximum Combined Benefit Duration over Time

Notes: Combined duration adds up regular benefits, Extended Benefits, and Emergency Unemployment Compensation. See Appendix Table A1 for a breakdown by type of benefit.

Likewise, average duration across the 6 reform states reaches a low of 17 weeks. Appendix Figure A1 shows the relative contributions of regular, EUC, and EB to the total maximum duration over time, separately for North Carolina, the moderate reform states, and the nontreated states.

A second reason these reforms are ideal is that the cuts were highly salient. For example, the cuts in North Carolina were so extreme that between April and July of 2013, thousands of protesters organized at the state capitol in Raleigh each Monday (referred to as "Moral Mondays") to voice their disapprobation of the reduction in state UI benefits.[8] The number of individuals affected by the reform was sizable. The fraction of short-term unemployed workers receiving any UI benefits in North Carolina plunged from 30% before the reform to 10% two years following the reform.

Third, the UI benefit rollbacks were arguably made on the basis of politics, rather than on changing local labor market conditions. While over two-thirds of states depleted their Unemployment Insurance Trust Funds during the Great Recession, only the 7 reform states permanently cut UI benefits, while another 27 insolvent states did not (Bivens et al. 2014).

Lastly, the reforms changed regular state UI benefits, which are the first source from which individuals claiming UI draw. This is in contrast to changes in state Extended Benefits (EB) and federal Emergency Unemployment Compensation (EUC) programs, which only become available

[8]https://www.usatoday.com/story/news/nation/2013/07/21/north-carolina-unemployment/2571889/.

after the exhaustion of regular UI. Thus, access to regular state UI benefits are relatively more valuable as they are discounted less heavily. Our setting therefore provides a nice complement to papers which study EB and EUC changes (Rothstein 2011; Farber and Valletta 2015; Farber et al. 2015; Marinescu 2017; Chodorow-Reich et al. 2019).

Due to North Carolina changing both the duration and benefit amount, we separate it out from the other reform states throughout the paper. While labor supply responses to benefit durations have been found to be relatively moderate,[9] responses to benefit levels are somewhat higher,[10] so a combined cut should elicit a stronger response. For conciseness and to increase precision, we combine the 6 reform states and analyze them using a staggered event study design.

One limitation of the North Carolina analysis is that 5 months after the new UI rules became effective, it implemented a tax reform which reduced the state corporate income tax rate from 6.9% to 5% and additionally reduced personal income taxes from between 6-7.75% to a flat rate of 5.75% between 2013 and 2015. We disentangle the impacts of the tax cuts from that of the UI reforms on employment in two ways. First, we use estimates of corporate and personal income tax elasticities from the literature to bound the employment effects attributable to the contemporaneous tax reforms. Second, we leverage the same study design for moderate reform states which did not implement major changes to their income tax codes.

In spite of the many advantages of studying changes to UI generosity generated from states' legislative rollbacks in the aftermath of the Great Recession, there is limited research leveraging this variation. One notable exception is Johnston and Mas (2018), which uses a regression kink design and finds that the UI benefit reduction in Missouri (one of our moderate reform states) shrank unemployment spell lengths and decreased the overall unemployment rate by 1%. In subsequent work, Karahan et al. (2025) show that the Missouri reform increased job-finding rates among the unemployed, with over half of the effect owing to a rise in vacancy creation.

[9] Duration elasticities for unemployment spell lengths range from 0.10 to 0.41 in the United States (Moffitt (1985); Katz and Meyer (1990); Card and Levine (2000); Johnston and Mas (2018)) with a median of 0.33.

[10] Benefit elasticities range form 0.10 to 1.2 in the United States, with a median of 0.38 (Moffitt (1985); Solon (1985); Katz and Meyer (1990); Meyer and Mok (2007); Chetty (2008); Card et al. (2015); Landais (2015); Ganong et al. (2021)). A related literature documents "spikes" in exits from unemployment just prior to benefit exhaustion (Katz and Meyer (1990); Carling et al. (1996); Card and Levine (2000); Røed and Zhang (2003); Van Ours and Vodopivec (2006); Dahl (2011)).

2.2 Salience of the Reforms

Before continuing, we discuss the the salience and scale of the UI reforms as measured by UI receipt and unemployment. A detailed analysis is found in Appendix B; here we summarize the main findings. Consistent with the intent of the law changes, the reforms sharply curtailed the use of UI. Prior to North Carolina's reform, the levels and trends for short-term UI receipt (≤ 26 weeks) were similar in North Carolina and control states. But by two years after the reform, the recipiency rate in North Carolina was 10% compared to 30% in control states, for a 20 percentage point difference. In the moderate reform states, a staggered event study design reveals a 5.5 percentage point reduction. The smaller effect for moderate reform states is expected given the more extreme cuts enacted by North Carolina. We also show in Appendix B that new UI claims (divided by employment) fell by approximately 60% in North Carolina relative to control states and by a more modest amount in the moderate reform states.

These drops in UI receipt were accompanied by a decrease in aggregate unemployment. An event study reveals that unemployment rate trends in North Carolina tracked control states prior to the reform. But within six months of implementation, unemployment was 2 percentage points lower compared to control states. For moderate reform states the average treatment effect was smaller, at approximately 1.2 percentage points. Complementing this analysis, the reforms resulted in a drop in labor force participation rates in North Carolina, but no significant change in moderate reform states.

2.3 Data

We use a variety of data sources to study labor market outcomes using our multi-state firm design. For employer-level employment outcomes, we use an administrative dataset spanning 2010-2015 (North Carolina) and 2008-2015 (moderate reform states) from the Equal Employment Opportunity Commission (EEOC), the EEO-1 files (Equal Employment Opportunity Commission 2008–2015). The EEOC survey provides a rich census of all private establishments in the United States with at least 50 employees and whose enveloping firm employs at least 100 individuals, along with federal contractors with at least 50 employees. These data, which cover approximately 56 of the 120 million private employees in the country over the sample period, detail the number of workers within an establishment at a point in time between October and December of each survey year.[11]

Appendix Figure A2 shows the fraction of total U.S. private employees and establishments covered by the EEOC survey, with each stratified by establishment size. The fraction of covered employees and establishments are each nearly 80% for establishments with over 100 employees, which is unsurprising based on the EEOC size reporting thresholds. The figure also shows that over three-quarters of all U.S. workers are employed in these relatively large establishments, thus minimizing concerns about the general applicability of the results.

From these data, we construct establishment-level balanced panels which can be linked to their parent firm. For the purposes of the current study, we limit the sample to multi-state firms whose subsidiary establishments operate in North Carolina and at least one other state (and similarly for the moderate reform states). Starting with the North Carolina reform sample, panel A in Table 2 shows there are 50 establishments per multi-state firm on average, with each firm operating in North Carolina plus 13 other states on average. Roughly 7% of establishments are in the treated state of North Carolina. The average number of employees per firm is 4,450, with 89 employees per establishment. Turning to the moderate reform sample in column B, the number of establishments per firm (27) and the number of states per firm (12) are somewhat smaller. This is because the Moderate Reform sample, which requires that a firm has a presence in KS, MI, MO, FL, GA, or SC plus at least one other state, contains a larger fraction of regional firms operating exclusively in the Midwest or Southeast. Roughly 22% of establishments in column B are in treated states.

To isolate worker wage outcomes, we use a proprietary dataset from Glassdoor covering 2008-2016, which includes self-reported salary data along with the affiliated company and workplace location (Glassdoor 2008–2016). Most importantly for the purposes of detecting reservation wage effects, workers also report the number of years of relevant experience. We assume that workers who report having had less than 1 year of relevant experience are new hires, so that the reported salary is likely to be their starting salary.[12] This allows us to hone in on wage effects for new hires,

[11] See https://www.eeoc.gov/employers/eeo-data-collections. Given this timing, the 2013 survey belongs to the post-reform period for North Carolina, as its reform was implemented in July of that same year.

[12] We note that applying this restriction means that we are identifying the subset of new hires who are beginning new roles, as we exclude new hires who have prior related experience.

Table 2: Summary Statistics for Multi-State Firm Samples

	North Carolina	Moderate Reform States
A. EEO-1 Employment		
Establishments per Firm	49.8	27.2
# States per Firm	13.8	11.5
Employees per Firm	4,450	2,865
Employees per Establishment	89.4	105.3
Share of Establishments treated	0.066	0.215
N (Establishment-years)	946,545	1,436,686
B. Glassdoor Salaries		
# States per Firm	18.2	12.3
Starting Salary	$82,214	$81,380
N (Person-years)	500,757	942,219
C. Burning Glass Job Ads		
Establishments per Firm	29.6	28.0
# States per Firm	20.1	11.4
Jobs per Firm	6.7	8.2
Posted Wage	$57,711	$57,264
N (Establishment-job-quarters)	709,226	1,180,096

The sample period for panel A is three years before each UI reform (2008, 2009, or 2010) through 2015. The sample period for panel B is the same except it runs through 2016. The sample period for panel C is 2010-2017.

whose reservation wages are more likely to be affected by contemporaneous UI generosity. One limitation of the Glassdoor data is that it does not allow us to separate out the composition of jobs from changes in wages for the same job.

To address this issue, we leverage data from Burning Glass Technologies (BGT), which allows us to compare posted wages for the same job in the same establishment over time (Burning Glass Technologies 2010–2017). The BGT data contains the near-universe of 10 million posted wages from online job ads between 2010-2017. As documented in Hazell and Taska (2025), the BGT data cover 70% of all online job postings. However, just 17% of all ads include posted wages, which implies that total coverage is approximately 10%.[13] Hazell and Taska validate the BGT data against publicly available data sources, such as the CPS, and find that the data are broadly representative and tend to co-move with CPS wages.[14] We define a job to be the same if it is

[13] We additionally note that the fraction of job postings that include wage information is constant across the sample, and does not respond to the UI reforms.

[14] In contrast, Batra et al. (2023) argue that the BGT has several problems. To clean and harmonize firm names in the BGT data, we use the code developed by Schubert et al. (2024) and Hazell and Taska (2025).

posted by the same firm, has the same Standard Occupational Classification (SOC) code (using the crosswalk over time based on IPUMS-CPS (2025)), and is the same job type (full-time, part-time, or hourly). As panel C shows, there are approximately 7 to 8 different job ads per firm among their establishments per quarter in both of our analysis samples.

As can be seen further in Table 2, the average posted salaries in BGT data are lower than those in Glassdoor data. This is due to two reasons: first, firms that post an explicit wage tend to offer approximately 40% less than those that do not include salary information in the job ad (Banfi and Villena-Roldan 2019). Secondly, Glassdoor oversamples from the higher end of the earnings distribution, both across industries and within them (Sockin and Sockin 2019). By using the Glassdoor and BGT data side-by-side, we get a fuller picture of the effects of UI reforms on wages.

In Appendix Table A1, we report summary statistics for the pre-reform period of our multi-state firm samples, separately for treated and control states. For EEO-1 data, the number of employees per establishment is slightly higher in treated versus control states; for Glassdoor data, starting salaries are somewhat lower for treated workers; and for Burning Glass data, posted wages are similar. Of course, our identification strategy relies on parallel trends, not level balance, and as we will show there is little evidence for differential trends in our event study analyses.

We supplement the Glassdoor and Burning Glass analyses with data that uses the Basic CPS and Job Tenure and Occupational Mobility Supplement (IPUMS-CPS 2009–2018) merged with the CPS Outgoing Rotation Group (Center for Economic and Policy Research 2020). However, because the Job Tenure and Occupational Mobility Supplement is conducted only every other January, there are far fewer observations. For example, for the North Carolina starting salary analysis, there are just 25,000 relevant worker observations compared to 500,000 Glassdoor observations and over 700,000 BGT observations. Basic monthly CPS data also allows us to estimate unemployment durations in UI reform states versus nontreated states.

3 Research Designs

3.1 Multi-State Firm Design

To start, we discuss our multi-state firm research design for North Carolina, where we use a series of difference-in-difference and event-study specifications which track labor market outcomes before and after the UI reform. Our main regression model leverages data on firms from the EEOC, Glassdoor, and BGT which operate in multiple states, similar to Giroud and Rauh (2019) but in a different context. We estimate event studies for establishment-level employment (EEOC data) using panel fixed effects:

$$Y_{eft} = \sum_{t=-3}^{2} \beta_t \times NC_e + \eta_e + \phi_{ft} + \epsilon_{eft} \tag{1}$$

where Y_{eft} is log employment in establishment e which belongs to firm f in year t. We limit our sample to multi-state firms that have at least one establishment in North Carolina and at least one establishment in another state. Our treatment variable, NC_e, equals one whenever an establishment is located in North Carolina. The regression includes establishment fixed effects, η_e, as well as firm-by-year fixed effects, ϕ_{ft}. Note that establishment fixed effects subsume both firm and state fixed effects. We omit period $t-1$, which serves as the reference period. We cluster standard errors at the firm level.

Each β_t coefficient can be interpreted as the intra-firm difference in employment growth between North Carolina and other same-firm establishments in other states t years after the reform. For example, the estimates will capture employment growth for an Ace Hardware in North Carolina versus an Ace Hardware in Alabama. This approach circumvents confounding variation that might both be correlated with employment growth and a firm's decision to operate in one state but not another. The event-study design captures the causal effect of the UI reform if the timing of the North Carolina reform is "as good as random"; i.e., if North Carolina's reform was not enacted in response to or in tandem with other factors that would have directly affected the outcomes relative to the control states. One partial test for this is whether the pre-trends are parallel between North Carolina and the control states, a condition we verify empirically. In a series of robustness checks, we explore several possible threats to identification, including policy endogeneity, mean reversion, and other policy changes.

When estimating the effect of the reform on posted wages from BGT, we make two changes: (i) we construct quarterly panels of establishment-jobs, as in Hazell and Taska (2025) and (ii) we replace η_e with η_{ej} to capture establishment-job fixed effects, where the subscript j is the specific job being advertised in the posting. This allows us to compare changes in posted wages in North Carolina versus control states for the same job in the same firm. When estimating the effect of the reform on starting salaries using Glassdoor data, we make two changes. First, instead of including establishment and firm-by-year fixed effects, we include job and firm-by-year fixed effects. This is because there is not an establishment variable in the dataset. Second, we add individual-level controls for gender, education, state, metro area, sector, and part-time/full-time/hourly status.[15]

For the moderate reform states, we analyze the same set of primary outcomes as for North Carolina, but combine data from the 6 states into a single regression because they all cut maximum durations, but did not change benefit amounts (see Table 1). Since the reforms are implemented at different times in different states, we estimate effects in event time using stacked difference-in-difference designs.

We use the de Chaisemartin and d'Haultfœuille (2020) estimator for models which include establishment or establishment-job fixed effects (EEO1 employment and Burning Glass posted wages), but the Borusyak et al. (2021) estimator for models which do not (Glassdoor starting salaries). The reason is that de Chaisemartin and d'Haultfœuille (2020) works well when the fixed effects are at the level at which treatment and control are defined, even if the number of such fixed effects is large – as with establishment or establishment-job fixed effects. While the estimator allows for other types of fixed effects in theory, in practice Stata runs into memory management issues when they are not defined at the level of treatment and there are many of them – as with firm-job fixed effects. In contrast, the Borusyak et al. (2021) estimator can struggle to estimate an effect when the data is unbalanced and there are a large number of fixed effects, as is the case for our analyses of job postings which include firm-job fixed effects. For robustness, we also report

[15] The timing of the reforms are recorded differently for the employment and starting salary analyses due to the underlying data structure. Specifically, because all UI reforms were implemented prior to when the employment counts were tabulated in each EEOC survey year (October through December), we allocate each partially treated year to the post-reform period. By contrast, the month during which each salary is reported is not recorded in the Glassdoor data, and so we allocate each partially treated year to the pre-reform period.

estimates based on Sun and Abraham (2021).[16]

Our multi-state firm design which restricts the sample to establishments whose parent firm operates in both treatment and control states, has two advantages. First, it provides a re-weighting of the control states that may provide a better counterfactual for the reform states. This is due to the tendency of firms to offer similar jobs and have similar workplace structures across establishments. Second, it has the potential to increase precision by controlling for residual variation in employment and wages that is establishment specific.

3.2 Design Using CPS Data

For our analysis of CPS data, we do not observe either firm or establishment identifiers, and hence are required to use a different design. We estimate the following regression for North Carolina:

$$Y_{ist} = \sum_{t=-4}^{6} \beta_t \times \ NC_{is} + \gamma X_i + \alpha_s + \theta_t + \epsilon_{ist} \tag{2}$$

where Y_{ist} is employment, starting salary, or unemployment duration for individual i in state s and year t. In addition to state (α_s) and year (θ_t) fixed effects, we control for predetermined characteristics (X_i). We restrict the set of controls to individuals living in Southern or Midwestern states that did not implement a moderate reform and cluster standard errors at the state level. For the six moderate reform states, we combine the data and estimate similar regressions in event time using the stacked difference-in-difference estimator of de Chaisemartin and d'Haultfœuille (2020).

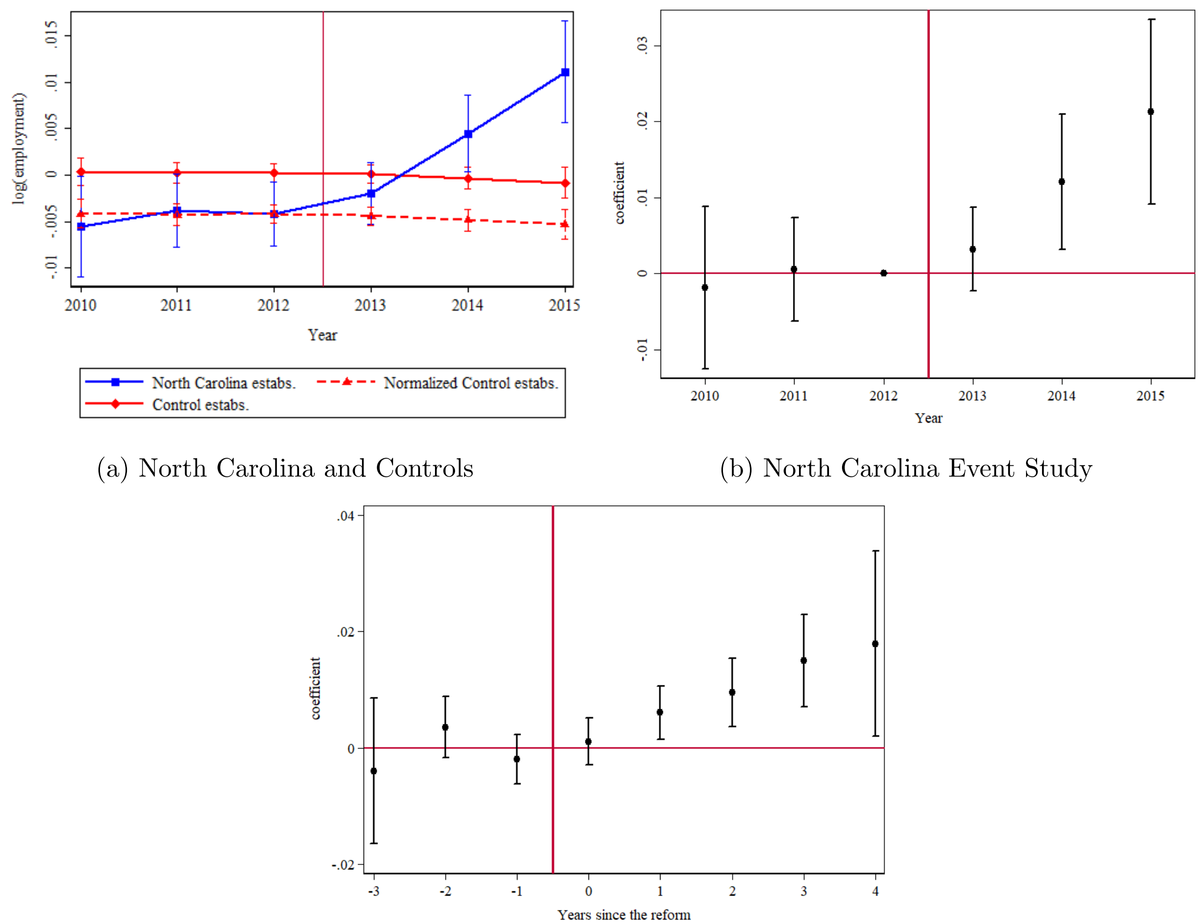
4 Employment in Multi-state Firms

We now turn to our analysis of employment which leverages multi-state firms, comparing employment counts in North Carolina-based establishments (or moderate reform state-based establishments) relative to those operating in other states with the same parent firm. This type of analysis allows us to investigate the hypothesis that general equilibrium effects temper micro-labor supply effects, with an increase in search effort crowding out employment outcomes for other unemployed individuals (Michaillat 2012; Lalive et al. 2015; Marinescu 2017; Landais et al. 2018).

[16] We use the following Stata commands to calculate event-time coefficients as well as the average effect of treatment on the treated (ATT): "did_multiplegt.do" for de Chaisemartin and d'Haultfœuille (2020), "did_imputation.do" for Borusyak et al. (2021), and "did_v2.do" for Sun and Abraham (2021) (taking an equally-weighted average across horizons for the ATT with Sun and Abraham). Since these commands do not provide summary statistics (number of unique observations or firms), we report those using the corresponding "reghdfe.do" command.

4.1 Main Employment Results

To study employment, we use the EEOC's census of private establishments in the United States which are required to report annually. As discussed in Section B.1, this dataset covers roughly 46% of all employees. Using these data, we construct a 6-year balanced panel of linked firm-establishments and test how employment evolves in establishments that are part of the same multi-state firm but are located in different states.



(a) North Carolina and Controls

(b) North Carolina Event Study

(c) Moderate Reform Staggered Design

Figure 2: Employment in Multi-state Firms

Notes: Establishment employment data come from the EEOC. Multi-state firms are those which operate both in a treated state and at least one control state. Panel (a) presents residualized log employment after netting out establishment and firm-by-year fixed effects, with control states centered at zero in the year prior to treatment. To make visual comparisons easier, the dashed red line shifts down the solid red line for control states to have the same mean as North Carolina in the year prior to the reform. Panel (b) plots the corresponding event-study estimates based on equation 3. Panel (c) plots the estimates for moderate reform states using the staggered design of de Chaisemartin and d'Haultfœuille (2020). Vertical bars denote 95% confidence intervals.

Panel (a) of Figure 2 plots the average log employment for establishments in North Carolina versus other states. For this panel, we first residualize log employment by netting out establishment and firm-by-year fixed effects, centering around zero for control states in the year prior to treatment. In the three years before the reform, log employment has a flat trend line, both for North Carolina-based establishments and the controls. Beginning in 2013, after the reform is implemented, employment in North Carolina establishments catches up to and eventually surpasses the same firm's establishments in other states. To make the post-reform divergence in trends easier to see, we shift down the line for establishments in control states so they have the same mean the year prior to the reform (dashed red line) as North Carolina does.

Panel (b) provides the corresponding event-study plot using the regression specification in equation 1, which includes both establishment and firm-by-year fixed effects.[17] There is no statistical evidence of differential pre-trends, but rising effects in each of the post-reform periods. By the second full year post reform, intra-firm employment growth is 2% higher in North Carolina establishments.

In panel (c) of Figure 2, we conduct a similar event-study analysis for the moderate reform states, but using a staggered design. Employment in moderate reform states is flat relative to controls before the reform, but rises afterwards. Over a similar event-time horizon, the effect is roughly half as large for the moderate reform states compared to North Carolina (1.0% versus 2.1% two years after the reform, respectively).

Table 3 reports the corresponding event-study coefficients and average treatment effects for North Carolina and the moderate reform states. As shown in column (1), for North Carolina the average effect in the post period is a 1.3% increase in employment. As shown in column (4), for the moderate reform states the average effect is 0.8%. Based on these results, we conclude that there are nontrivial increases in employment after UI benefits become less generous. We interpret these findings and compare them to the existing literature later in Section 7, after we probe the robustness of the employment results and present our findings for starting salaries and posted wages.

[17] For both the North Carolina and moderate reform analyses, we use a sample period that begins 4 years prior to a state's reform and our data allows us to estimate effects through 2015. Since North Carolina implemented their reform in July 2013, we have fewer post-reform years compared to the moderate reform states, which implemented reforms between 2011 and 2014. For the moderate reform states, since the reforms are staggered over time, no omitted reference period is needed.

Table 3: Effect of the UI Reforms on Employment

	North Carolina Reform			Moderate Reforms		
Control Group	All	Insolvent	Similar GR shock	All	Insolvent	Similar GR shock
dep var = log(employment)	(1)	(2)	(3)	(4)	(5)	(6)
treated × $\mathbb{1}_{t=-3}$	-0.00189	-0.0014	-0.00199	-0.00398	-0.0029	0.0016
	(0.00543)	(0.0054)	(0.0059)	(0.0064)	(0.0058)	(0.0065)
treated × $\mathbb{1}_{t=-2}$	0.000516	0.0007	0.00028	0.00355	0.0033	0.0027
	(0.00347)	(0.0035)	(0.0040)	(0.00267)	(0.0027)	(0.0027)
treated × $\mathbb{1}_{t=-1}$	0	0	0	-0.0020	-0.0014	-0.0005
				(0.0021)	(0.0020)	(0.0025)
treated × $\mathbb{1}_{t=0}$	0.00319	0.00292	0.00075	0.00108	0.0012	0.0031
	(0.00281)	(0.00288)	(0.0032)	(0.0020)	(0.0026)	(0.0021)
treated × $\mathbb{1}_{t=1}$	0.0120***	0.0121***	0.0105**	0.00604***	0.0056*	0.0089***
	(0.00454)	(0.00465)	(0.00503)	(0.0023)	(0.0031)	(0.0028)
treated × $\mathbb{1}_{t=2}$	0.0212***	0.0207***	0.0191***	0.00948***	0.0083**	0.0136***
	(0.00621)	(0.0064)	(0.0069)	(0.0030)	(0.0037)	(0.0037)
treated × $\mathbb{1}_{t=3}$				0.0150***	0.0138***	0.0202***
				(0.0040)	(0.0042)	(0.0043)
treated × $\mathbb{1}_{t=4}$				0.0179**	0.0101	0.0108
				(0.0081)	(0.0076)	(0.0073)
ATT	0.0126**	0.0121**	0.0109*	0.0083***	0.0072**	0.0113***
	(0.0052)	(0.0054)	(0.0059)	(0.0022)	(0.0029)	(0.0029)
Estab, Firm × Year FEs	X	X	X	X	X	X
mean(EEO-1 emp.)	83.85	83.55	85.98	99.85	99.44	99.38
N	946,545	805,175	495,333	1,436,686	1,231,768	1,004,626
Firms	3,172	3,094	2,594	6,697	6,359	5,834
States	43	27	16	48	32	26
R^2	0.973	0.973	0.974			

Notes: Establishment employment data come from the EEOC. Insolvent states are those whose UI trust fund balances fell below 0 in the pre-reform period. States with similar Great Recession exposure are the subset of insolvent states that experienced a peak-to-trough unemployment rate shock of within 2 percentage points of the treated state(s). See Appendix Table A2 for lists of insolvent and similar GR shock states. Columns (4)-(6) use the staggered design estimator of de Chaisemartin and d'Haultfœuille (2020). The sample period begins three years prior to a state's reform (2008, 2009, 2010, or 2011) and ends in 2015. The mean corresponds to the average number of employees at not-yet-treated establishments. Standard errors are clustered by firm and for the moderate reform analyses are based on 100 bootstrap replications.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

4.2 Robustness

We now examine the robustness of the EEOC employment results to possible policy endogeneity, mean reversion, and other policy changes. Policy endogeneity would occur if UI reforms were implemented in response to particularly severe Great Recession shocks which both drained UI trust funds and affected the local labor market. However, as argued by Bivens et al. (2014), the

UI cuts in reform states were motivated by politics (see footnote 2). Indeed, there were 27 other states whose UI trust funds became insolvent in the early 2010s, but which did not implement any cuts to their UI programs (Appendix Table A2, panel A). In columns (2) and (5) of Table 3, we restrict the set of control states to those with insolvent UI funds (U.S. Department of Labor 2024c) to account for this type of possible policy endogeneity. The estimates remain nearly identical.

The top panel of Appendix Figure A3 provides supplementary evidence against policy endogeneity. The figure plots the net UI trust fund balance per covered worker for the reform states versus the other insolvent states, where the other insolvent states are normalized to have the same level in the year prior to a reform. There are different lines for the reform states and their controls based on implementation date. Consider the solid blue line for North Carolina. Prior to the reform date indicated by the vertical blue line, both North Carolina's and the other insolvent states' trust fund balances are on similar trends, but they diverge sharply thereafter with North Carolina's reform improving trust fund balances. Similar patterns are found for the moderate reform states.

A related concern is that UI taxes fell in treated versus untreated states due to the reforms, causing firms to increase employment in reform states (Guo 2023). Yet we find that UI taxes do not fall more in treated versus untreated states during our analysis period (U.S. Department of Labor 2024a).[18]

Mean reversion could occur if reform states suffered larger than average employment shocks, and hence had more room to expand employment after the Great Recession. In columns (3) and (6) of Table 3, we explore this by restricting the sample to insolvent states that were hit similarly hard by the Great Recession.[19] Specifically, we use the subset that experienced a peak-to-trough unemployment rate shock within $\pm$ 2 pp of the treated states. The estimated employment effects do not depart materially from the baseline estimates. Moreover, note that mean reversion in employment implies that wages should rise faster in reform states, but as we show later, we find

[18]For North Carolina, the UI tax rate was 1.03% in the year prior to the reform and 0.99% on average in the three years post reform; this compares to 1.03% before and 0.87% afterwards in control states. In other words, if anything, North Carolina's UI tax rate was relatively higher post reform, not lower, at least over the time period of our analysis. Likewise, for the 6 moderate reform states, the UI tax rate was 0.76% in the year prior and 0.80% in the three years post reform, versus 0.97% and 0.95%, respectively, for control states. Note that experience ratings take several years to adjust, which means there will not be an immediate mechanical decline in UI taxes.

[19]See panels B and C of Appendix Table A2 for the list of states. As the bottom panel of Appendix Figure A3 shows, UI trust fund balances for these control states have similar pre-trends.

the opposite.[20]

We next explore the impact of other policy changes. Because North Carolina reduced its corporate tax rate by 1.9 percentage points and personal income tax rates by 0.25 or 2 percentage points over the same time horizon, we recognize that part of its estimated effect may have been due to an increase in labor demand and labor supply (assuming substitution effects dominate income effects) from these confounding policies. To bound the estimated effect of the tax reforms on employment, we consider the effects of tax cuts on employment. Some papers find zero, but imprecisely estimated, employment effects of corporate taxes on employment Gruber (1997); Anderson and Meyer (1997) while Giroud and Rauh (2019) estimates a corporate tax elasticity of -0.4 (s.e.=.05). This range of estimates implies that between 0 and 0.76 percentage points of the estimated 1.26% increase found in Table 3 column (1) is attributable to the corporate tax reform.[21] From this we conclude that the North Carolina UI reform is responsible for between a 0.5% and 1.26% increase in employment, which is similar in magnitude to the 0.83% effect found for the moderate reform states in Table 3 column (4).[22]

In Table 4, we examine robustness to a variety of policy changes. Appendix Table A2 lists the states experiencing each policy change. We first drop treatment and control states which had changes in right-to-work laws during our time period, and find little change in the estimates for either the North Carolina or Moderate Reform state analyses (columns (1) and (4)). Columns (2) and (5) drop treatment and control states which experienced minimum wage changes during the sample period. The results are robust, despite the number of states (and the number of observations) falling by more than half. Columns (3) and (6) drop states which had medicaid expansions; the estimate is marginally insignificant for North Carolina, but there are only 15 control states left in the sample. In the final two columns, we omit states with the most extensive corporate income tax reforms and personal income tax reforms, respectively.[23] Again the results are robust. We recognize, of course, that while we have attempted to categorize a variety of policies,

[20] It is also worth noting that mean reversion would have predicted a gradual increase in relative employment for reform states, but the estimates are flat in the pre-period.

[21] Giroud and Rauh (2019) finds no effect of personal tax changes on employment.

[22] Using the delta method, the standard error on the lower-bound estimate of 0.5% is 0.22%.

[23] Almost all states had some type of tax reform, even if modest; we identified those which were described as having the largest reforms from internet searches.

there may be others we have not taken into account which could have affected the labor market.

Table 4: Effect on Employment, Policy Robustness

	NC Reform			Moderate Reforms				
dep var = log(employment)	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
ATT	0.0125***	0.0103**	0.0080	0.0113***	0.0118***	0.0082**	0.0114***	0.0082***
	(0.0053)	(0.0047)	(0.0051)	(0.0032)	(0.0045)	(0.0035)	(0.0032)	(0.0029)
No RTW Law Passed	X			X				
No Minimum Wage Change		X			X			
No Medicaid Expansion			X			X		
No Major Corporate Tax Changes							X	
No Major Personal Tax Changes								X
Estab, Firm × Year FEs	X	X	X	X	X	X	X	X
mean(EEO1 emp.)	89.02	87.56	86.16	100.38	97.75	99.17	99.33	98.54
N	901,056	453,605	344,794	1,284,309	563,768	596,975	1,274,319	1,075,877
States	41	19	16	44	21	20	44	42
Firms	3,130	2,631	2,382	5,971	3,434	4,167	5,759	5,813
R^2	0.973	0.975	0.975					

Notes: See notes to Table 3. For lists of states with other policy changes, see Appendix Table A2.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

As an additional robustness exercise, we implement the Fisher exact test outlined in chapter 5 of Imbens and Rubin (2015).[24] For the North Carolina analysis, this test runs a series of placebo regressions where treatment is randomly assigned to one of the states in our sample. There are two placebo states (out of a possible 42) which have t-statistics which are more extreme than North Carolina, which is about what one would expect for the one-sided test recommended in Imbens and Rubin (2015). Doing a Fisher exact test for the moderate reform analysis is not feasible for two reasons (i) there are 5,253,824 ways to choose 6 states out of 42 possible states and (ii) the estimation of each staggered design takes several hours.

4.3 Alternative Specifications and Estimators

In Table 5, we report results using different fixed effects and different estimators. Starting with the North Carolina analysis, columns (1) and (2) report estimates with a less-saturated set of fixed effects. As a reminder, our baseline specification includes establishment and firm-by-year fixed effects. The baseline estimate is repeated in column (3) for comparison purposes. The first column includes firm, state, and year fixed effects. This specification does not account for establishment level differences or firm-specific trends over time. The second column includes establishment fixed effects and year fixed effects. While this specification controls for establishment-level differences,

[24] We also looked into using the new variance estimator of Abadie et al. (2022), but as pointed out by Rambachan and Roth (2020), "their results are not directly applicable to inference in quasi-experimental settings where treatment probabilities may systematically differ across clusters in ways potentially related to treatment outcomes."

it does not control for firm-specific trends. The estimates in columns (1) and (2) are both roughly twice as large as our baseline estimate in column (3). This contrast highlights the importance of accounting for firm-specific employment growth.

Table 5: Effect on Employment, Different Fixed Effects and Estimators

	North Carolina Reform			Moderate Reforms		
dep var = log(employment)	(1)	(2)	(3)	(4)	(5)	(6)
ATT TWFE	0.0250***	0.0242***	0.0126**			
	(0.0066)	(0.0066)	(0.0052)			
ATT dCdH					0.0154***	0.008255***
					(0.0034)	(0.0022)
ATT BJS				0.0174***	0.0175***	0.0079**
				(0.0043)	(0.0041)	(0.0031)
ATT AS				0.0175***	0.0178***	0.0061**
				(0.0040)	(0.0040)	(0.0029)
Firm, State, Year FEs	X			X		
Estab, Year FEs		X			X	
Estab, Firm × Year FEs			X			X
mean(EEO-1 emp.)	84.36	84.36	83.85	101.56	101.56	99.85
N	948,631	948,625	946,545	1,445,938	1,445,917	1,436,686
Firms	3,519	3,519	3,172	7,820	7,820	6,697
States	43	43	43	48	48	48
R^2	0.587	0.970	0.973			

Notes: See notes to Table 3. Each row uses a different estimation method to calculate the ATT. TWFE stands for two-way fixed effects, dCdH for de Chaisemartin and d'Haultfœuille (2020), BJS for Borusyak et al. (2021), and AS for Sun and Abraham (2021). For the AS method in column (4), N=1,445,917.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

In columns (4)-(6), we repeat this exercise of varying the set of fixed effects for the moderate reform analysis, but additionally also employ different staggered design estimators. Our baseline estimate uses the de Chaisemartin and d'Haultfœuille (2020) estimator, and is repeated in column (6) for comparison. The table also reports results using the estimators proposed by Borusyak et al. (2021) and Sun and Abraham (2021). The first thing to note is that all three estimators yield similar results within a column.[25] Second, the estimates in columns (4) and (5), which include a less-saturated set of fixed effects, yield estimates which are roughly twice as large. This mirrors what was found for the North Carolina analysis.

In Appendix Table A3 we estimate regressions for two different samples of firms. First, we expand the sample to include establishments from all firms, rather than restricting the sample

[25] As explained in Section 3, the de Chaisemartin and d'Haultfœuille (2020) estimator is not feasible for column (4).

to multi-state firms which operate in both a reform and control state. This adds two types of firms: (i) multi-state firms which do not operate in a reform state, and (ii) single-state firms which only operate in a reform or control state. Columns (1) and (3) in Appendix Table A3 use this broadened sample to estimate models for North Carolina and the moderate reform states which include establishment and year fixed effects. The estimated effects are roughly two-thirds as large as the corresponding estimates which include the same set of fixed effects found in Table 5 columns (2) and (5).[26] As a second exercise, we limit the sample to single-state firms. These estimates are small and statistically insignificant, as shown in columns (2) and (4) of Appendix Table A3. One potential explanation for why the estimates using alternative samples differ is that they apply to different populations. Another explanation is that our preferred multi-state research design includes establishment and firm-by-year fixed effects; this compelling design cannot be applied to these two alternative samples.

4.4 Overall Employment Increase or Reallocation?

The results above suggest a limited scope for crowdout of other job-seekers, with firms being willing to expand relative employment in treated states to take advantage of a larger pool of workers. From a welfare perspective, however, it is important to know whether the increase in relative employment in reform-state based establishments represents an increase in overall employment or simply a reallocation of employment away from non-treated establishments. If all employment increases in reform states were mirrored by declines in non-reform states, the elasticity we calculate later in the paper would be double the true impact on establishments in reform states.

We explore the extent to which reallocation effects are responsible for employment gains in two ways. First, we remove firms operating in tradable industries, as defined by Mian and Sufi (2014), and re-estimate our main specifications.[27] The idea is that labor should be more mobile across establishments within these types of firms as they tend not to serve local markets. Thus, removing tradable industries limits the sample to firms where the reallocation of workers across

[26] The standard errors are fairly similar using either our baseline sample or this unrestricted sample, but this comparison needs to take into account the fact that the unrestricted sample sizes are twice as large (and the standard errors do not come down by a factor of root-N).

[27] Specifically, a four-digit NAICS industry is classified as tradable if its exports plus imports sum to greater than $500 million.

establishments is more difficult. Columns (1) and (4) of Table 6 estimate employment effects which are similar to the main estimates from Table 3. This provides evidence against a reallocation hypothesis, although we note that less than 10% of firms are classified as tradable.[28]

Table 6: Reallocation of Firm Employment across States

	North Carolina Reform			**Moderate Reforms**		
dep var = log(employment)	(1)	(2)	(3)	(4)	(5)	(6)
ATT	0.0127**		0.0202***	0.0069**		0.0089**
	(0.0057)		(0.0055)	(0.0030)		(0.0035)
post-reform × firm has presence in treated states		0.0000			-0.0052	
		(0.0061)			(0.0055)	
Drop Tradable Industries	X			X		
Drop Treated Establishments		X			X	
Drop National Firms		X	X		X	X
Estab, Firm × Year FEs	X		X	X		X
Estab, State, Year FEs		X			X	
mean(EEO-1 emp.)	68.76	101.89	89.58	88.74	116.40	110.55
N	859,505	1,025,567	559,151	1,273,115	1,037,591	916,246
Firms	2,487	13,774	3,068	5,083	13,167	6,559
States	43	42	43	48	42	48
R^2	0.973	0.967	0.975			

See notes to Table 3. Tradable industries (using four-digit NAICS codes) are those with exports plus imports summing to greater than $500 million, as defined by Mian and Sufi (2014). Columns (2) and (5) compare employment growth for establishments located in non-reform states but which differ based on whether the enveloping firm has any presence in a reform state. National firms are defined as those operating in more than 39 states (90% of the states in the sample).

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

As a second exercise, we consider the employment changes for establishments located in non-reform states based on whether their enveloping firm has a presence in reform states. If firms were simply shifting workers from non-reform to reform-state based establishments, we should expect to see a relative decline in employment at non-reform state based establishments whose firms already have operations set up in reform states (relative to a set of control establishments whose firms have no presence in reform states). To implement this test, we first restrict the sample to non-national firms.[29] Columns (2) and (5) of Table 6 reveal that these indirectly treated establishments do

[28] If we limit the sample to only tradable firms, the resulting estimates are similar to baseline, but more noisily estimated. The North Carolina reform estimate is 0.101 (s.e.=0.0126) and the moderate reform estimate is 0.0136 (s.e.=0.0080). We can also estimate effects for non-tradable firms, which Mian and Sufi (2014) define as retail- and restaurant-related industries. While these firms have a large number of employees, there are only 334 such firms (10% of all firms). We find smaller and insignificant estimates: 0.001 (s.e.=0.0076) for the North Carolina reform and 0.0066 (s.e.=0.0053) for the moderate reforms. We note that these two industries have larger shares of minimum wage workers, which could constrain wages. Since most of the workers and firms in our main sample belong to industries which are not classified as tradable or non-tradable, the unclassified industries drive our main findings.

[29] We define non-national firms as those which operate in no more than 90% of the states in the sample. This makes the indirectly treated and control establishments more comparable. This is because firms with representation

not exhibit any employment reductions following the passage of the UI reforms, which provides additional evidence against a reallocation hypothesis. Finally, for comparison purposes, columns (3) and (6) show that our main employment estimates are robust to only including non-national firms.

A related question is whether the UI reform affected the number of establishments or establishment survival. In terms of the number of establishments over time, in North Carolina establishment counts were, if anything, increasing more slowly than controls prior to the reform, but more quickly afterwards.[30] A similar pattern is found for the moderate reform states.[31] Moreover, using an unbalanced panel which includes all establishments, we estimate no difference in survival rates of UI reform versus nonreform state-based establishments.[32]

5 Salaries and Posted Wages in Multi-state Firms

We now turn to our analysis of how worker compensation is affected by the UI reforms. We start by discussing starting salaries and then continue with an analysis of posted wages.

5.1 Starting Salaries for Workers at Multi-State Firms

For our analysis of salaries, we focus on newly hired employees, since the UI cuts directly reduced the outside option for unemployed individuals and since new hires entering the labor force or switching jobs will have to compete with those exiting unemployment since the groups are likely to be close substitutes in hiring.

Our starting salary analysis leverages a proprietary dataset from Glassdoor which contains self-reported wages. We define new hires as those with less than one year of experience in multi-state firms which have an establishment in North Carolina; there are roughly 500,000 of these workers (and 900,000 in the moderate reform state sample). We cannot distinguish between new

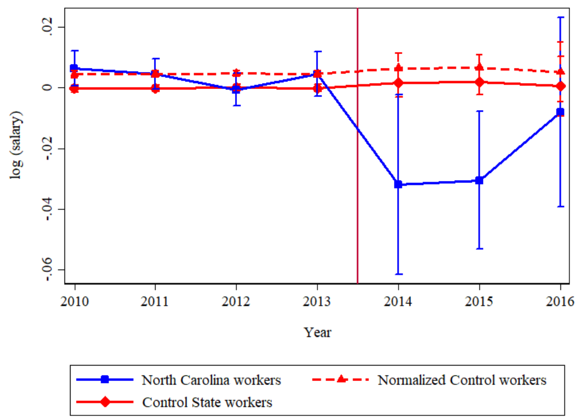
in reform states will tend to be larger national chains with presence in every state while those without any reform state presence are predominantly smaller regional firms.

[30] In event time, the establishment counts in North Carolina were 17,235 (t=-3), 17,488 (t=-2), 17,680 (t=-1), 18,456 (t=0), 19,443 (t=1), and 20,993 (t=2). For control states over the same horizon they were 248,103, 255,540, 259,731, 265,180, 273,407, and 286,552.

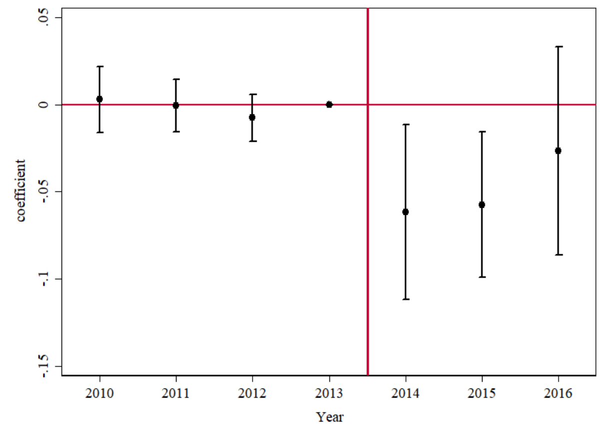
[31] In event time, counts for the moderate reform states were 81.984 (t=-3), 82,968 (t=-2), 84,552 (t=-1), 85,755 (t=0), 85,961 (t=1), 90,838 (t=2), and 95149 (t=3). For control states over the same horizon (omitting Kansas since it was a late adopter), they were 302,547, 316,075, 324,854, 332,204, 340,027, 349,499, and 363,073.

[32] Controlling for state and year fixed effects, and using establishment exit as the outcome, we estimate relatively precise null effects, both for North Carolina (0.004, s.e.=0.008) and the moderate reform states (-0.007, s.e.=0.006).

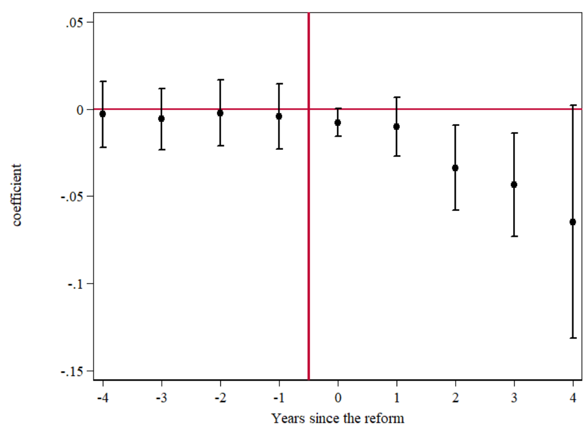
hires exiting unemployment versus those entering the labor force or switching jobs in the Glassdoor data, but based on other datasets, we calculate that the previously unemployed account for roughly 35% of all new hires.[33]



(a) North Carolina and Controls



(b) North Carolina Event Study



(c) Moderate Reform Staggered Design

Figure 3: Starting Salaries in Multi-state Firms

Notes: See notes to Figure 2. Starting salary data come from Glassdoor, using workers who report having less than one year of relevant experience. Panel (c) implements the staggered design of Borusyak et al. (2021).

Figure 3 panel (a) provides a picture of how the starting salaries of new hires changes in North Carolina establishments compared to those in other states following the reform. Panel (b) provides the accompanying event study.[34] There is no evidence of differential pre-trends prior to the reform. In contrast, by the first full year after the reform, we see a sharp drop in the starting salaries of

[33]This calculation is based on transition rates from Lise and Robin (2017) and Kudlyak and Lange (2018) combined with population shares.

[34]Panels (a) and (b) differ in implied treatment effects since the event study coefficients in panel (b) include controls for firm × year fixed effects, are hence are identified only from firm-year cells containing both NC and control workers.

new hires in North Carolina but no change in control states. Table 7 reports the corresponding estimates in a model which includes job and firm-by-year fixed effects. We define a job to be the same if it has the same Standard Occupational Classification (SOC) code, and is the same job type (full-time, part-time, or hourly). We estimate an ATT of 5.5%, i.e., that starting salaries decline by 5.5% in the post-reform period in North Carolina relative to other states.

In panel (c), we plot the staggered event study estimates for the moderate reform states. While the pre-reform coefficients are flat and not different from zero, by the second year after the reform, starting salaries in moderate reform states begin to decline significantly. As Table 7 documents, there is an average 1.2% decline in starting salaries in these moderate reform states relative to controls after the reforms.

In Appendix Table A4 we explore whether the effect of the reform is concentrated in jobs where UI has more relevance. We split the sample into jobs with starting salaries below versus above $100,000 per year. The logic is that UI replaces a smaller fraction of lost earnings for high-wage jobs, and so is less relevant as an outside option. Consistent with this, we estimate a negative wage effect in North Carolina among jobs paying $100,000 or less, with no statistically significant effect for high-wage jobs. For the Moderate Reform states, when we split by salary level we find negative effects for both groups, with somewhat larger estimates for those earning $100,000 or less (see Appendix Table A4).

We now briefly discuss additional robustness checks and alternative specifications. These estimations mirror those for the employment results reported above. Appendix Table A5 shows robustness when using two alternative control groups–establishments operating in states whose UI funds became insolvent and those that experienced similar Great Recession shocks. Appendix Table A6 reveals that our results are likewise robust to excluding states which experienced other major policy changes during the sample period, although some of the estimates are imprecise. Appendix Table A7 reports estimates using different fixed effects and estimators. As a reminder, the Glassdoor data does not identify establishments, which means that we cannot estimate models with establishment fixed effects or feasibly implement the de Chaisemartin and d'Haultfœuille (2020) estimator (see Section 3). But we can extend our baseline model to include firm-by-year-by-job fixed effects to allow for firm-specific trends in pay for specific jobs. With these considerations in

Table 7: Effect of the UI Reforms on Starting Salaries

	North Carolina Reform	Moderate Reforms
dep var = log(starting salary)	(1)	(2)
treated × $\mathbb{1}_{t=-4}$	0.00288	0.0031
	(0.00962)	(0.0096)
treated × $\mathbb{1}_{t=-3}$	-0.000483	-0.0058
	(0.00766)	(0.0089)
treated × $\mathbb{1}_{t=-2}$	-0.00768	-0.0022
	(0.00678)	(0.0097)
treated × $\mathbb{1}_{t=-1}$	0	-0.0043
		(0.0095)
treated × $\mathbb{1}_{t=0}$	-0.0617**	-0.0077*
	(0.0255)	(0.0041)
treated × $\mathbb{1}_{t=1}$	-0.0574***	-0.0103
	(0.0213)	(0.0086)
treated × $\mathbb{1}_{t=2}$	-0.0266	-0.0338***
	(0.0305)	(0.0124)
treated × $\mathbb{1}_{t=3}$		-0.0435***
		(0.0151)
treated × $\mathbb{1}_{t=4}$		-0.0648*
		(0.0340)
ATT	-0.0552**	-0.0118**
	(0.0189)	(0.0051)
Job, Firm × Year FEs	X	X
mean(starting salary)	\$77,185	\$73,934
N	500,757	942,219
Firms	4,657	16,258
R^2	0.773	

Notes: Starting salary data come from Glassdoor, using workers who report having less than one year of relevant experience. Column (2) uses the staggered design estimator of Borusyak et al. (2021). The sample period begins four years prior to a state's reform (2008, 2009, 2010, or 2011) and ends in 2016. All regressions include individual-level controls for gender, education, state, metro area, sector, and part-time/full-time/hourly status. The mean corresponds to the average starting salary for not-yet-treated jobs. Standard errors are clustered by firm and for the moderate reform analysis are based on 100 bootstrap replications.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

mind, we continue to find significant effects on starting wages, with somewhat larger estimates using the Sun and Abraham (2021) estimator. As a contrast to our multi-state firm design, Appendix Table A8 reports results for all states (regardless of whether they are multi-state establishments which operate in both treatment and reform states) and for single-state firms. Finally, we conduct a Fisher exact test and find that there are two placebo states (out of a possible 42) which have t-statistics which are more extreme than North Carolina, which is roughly what one would expect for the one-sided test recommended in Imbens and Rubin (2015).

As before, we acknowledge that reductions in corporate and personal income tax cuts could have affected starting salaries in North Carolina. Unlike with our employment analysis, however, the predicted effect of these simultaneously enacted tax cuts should have opposite-signed effects on starting salaries: while personal income tax rate reductions may have lowered pre-tax reservation wages, corporate income tax rate reductions should have increased starting salaries through positive shocks to labor demand. Moreover, since the estimated effect is 5.5%, even in the most conservative scenario in which there is full pass-through of a 2% reduction in personal income taxes on wages and no labor demand effects arising from the corporate tax cut, the North Carolina UI reforms account for no less than a 3.5% reduction in starting salaries.

A natural question is why the UI reductions decrease starting wages. One possible explanation is that UI cuts draw less productive workers into the labor market. We test for negative compositional effects using CPS data, which contains a fairly rich set of demographic characteristics. We first predict whether a worker will earn above average wages based on educational attainment, head of household status, sex, age, and age squared. We then use this prediction as the outcome variable in a staggered difference-in-difference regression (combining the North Carolina and moderate reform states for precision) and find no evidence that individuals are positively selected after the reform.[35]

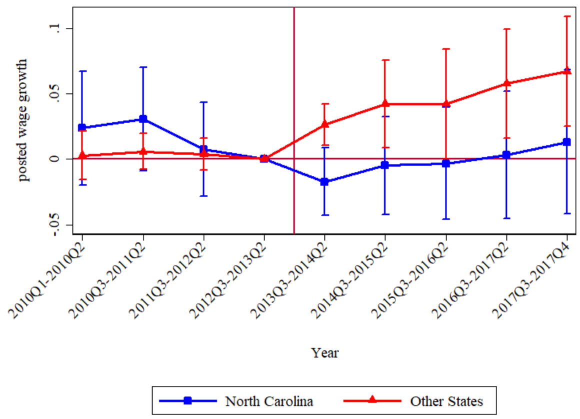
UI cuts could also induce workers to accept worse matches where they are less productive, which could also lower starting salaries. In the next section, we analyze *posted* wages, rather than *realized* wages, which should not directly be affected by these two channels. Posted wage should only change if there are general equilibrium responses by firms to the worsened outside options of workers after UI cuts.

5.2 Posted Wages for Jobs within Multi-State Firms

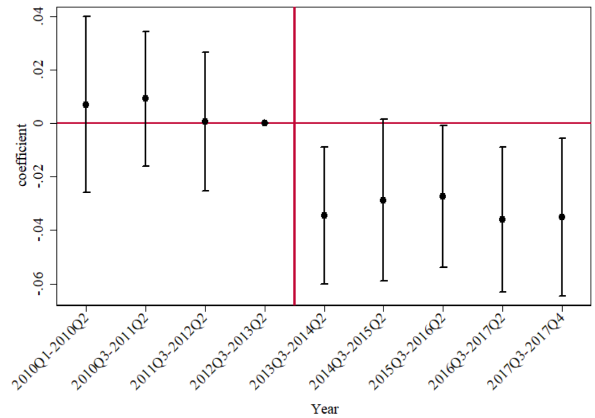
To examine whether UI cuts have general equilibrium effects on the wages offered by firms, we turn to Burning Glass Technologies (BGT) data. The use of BGT posted wages eliminates the direct impact of worker choices on our estimates since we see the firm's offered wages for the same job over time. For example, even if UI reductions do not cause firms to reduce the wage for any

[35] The estimated coefficient is -0.005 (s.e.=0.026).

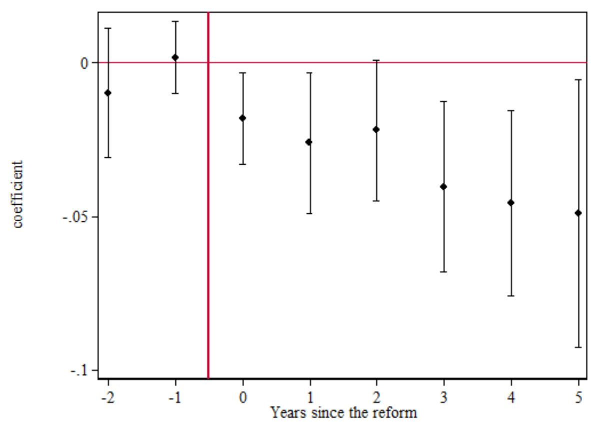
particular job, realized starting salaries may fall if workers begin matching with worse jobs or worse firms. In such a scenario, starting salaries would fall but posted wages would experience no change.



(a) North Carolina and Controls



(b) North Carolina Staggered Design



(c) Moderate Reform Staggered Design

Figure 4: Posted Salaries in Multi-state Firms

Notes: See notes to Figure 2. Posted salary data at the establishment-job level come from Burning Glass Technologies. Panel (c) implements the staggered design of de Chaisemartin and d'Haultfœuille (2020).

However, as can be seen in panels (a) and (b) of Figure 4, there is a relative decrease of in the posted wages for the same job within the same firm if the posting establishment happens to reside in North Carolina. Interestingly, panel (a) makes clear that relative wages are falling in North Carolina not because the posted wages are falling, but rather because they are failing to keep up with rising wages at other establishments within the firm as the economy continues to recover from the Great Recession. This provides additional evidence in support of the existence of downward nominal wage rigidity, as in Hazell and Taska (2025) and Fallick et al. (2020). We also find a decrease of roughly the same size in posted wages for the 6 moderate reform states in panel (c).

Table 8 provides estimates for the average treatment effect on the treated for the UI reforms. Specifically, the North Carolina reform reduced posted wages by 3.5% and in moderate reform states posted wages fell by 3.2%. To the extent that our sample of multi-state firms disproportionately includes national wage setters who do not respond to local labor market conditions, these effects will be an underestimate of the effect that would be found for the full sample of all firms (Hazell et al. 2022).[36] If we drop firms which, in the pre-reform period, exhibit no variation in wages across all establishments for the same job and quarter, posted wages decrease by slightly more in North Carolina (-4.0% versus -3.5%) and the moderate reform states (-3.3% versus -3.2%). The reason the estimates do not change by much is that the sample restriction does not drop many firms; the sample falls by 10% in North Carolina and by 8% in the moderate reform states.

Interestingly, the estimates for posted wages in Table 8 are the same order of magnitude compared to the starting wage estimates appearing in Table 7. This suggests that the drop in starting salaries is not primarily due to worse matches or compositional effects, but rather reflects firms taking advantage of workers' reduced outside options by lowering wage offerings.

Following the same logic used at the end of Section 5.1, even in the most conservative case where a 2 percentage point reduction in personal income taxes is fully passed through to North Carolina workers and the 1.9 percentage point reduction in corporate income taxes leads to no wage increase, the NC reform still generates a 1.5 percentage point reduction in starting salaries.

The estimates are robust to various sample restrictions and specification checks. Appendix Table A9 shows that the results are similar when we restrict the set of control states to those with insolvent UI funds or those which experienced a Great Recession shock similar to reform states. Appendix Table A10 reveals that our results are likewise robust to excluding states which experienced other major policy changes during the sample period. Appendix Table A11 reports estimates using different fixed effects and estimators and continues to find sizable effects on posted wages. As a reminder, we cannot feasibly implement some of the estimators for the reasons described in Section 3. And as a contrast to our multi-state firm design, Appendix Table A12 reports results

[36] In related work on product pricing for national firms, there is mixed evidence, with DellaVigna and Gentzkow (2019) finding uniform pricing and Butters et al. (2022) finding that prices respond to local cost shocks.

Table 8: Effect of the UI Reforms on Posted Wages

	North Carolina Reform	Moderate Reforms
dep var = log(starting wage)	(1)	(2)
treated × $\mathbb{1}_{t=-4}$	0.0070	
	(0.0168)	
treated × $\mathbb{1}_{t=-3}$	0.0092	
	(0.0128)	
treated × $\mathbb{1}_{t=-2}$	0.0006	-0.0132
	(0.0133)	(0.0101)
treated × $\mathbb{1}_{t=-1}$	0	0.0040
		(0.0060)
treated × $\mathbb{1}_{t=0}$	-0.0346***	-0.0182**
	(0.0130)	(0.0086)
treated × $\mathbb{1}_{t=1}$	-0.0288*	-0.0284***
	(0.0154)	(0.0114)
treated × $\mathbb{1}_{t=2}$	-0.0273**	-0.0235*
	(0.0135)	(0.0123)
treated × $\mathbb{1}_{t=3}$	-0.0360***	-0.0414***
	(0.0138)	(0.0123)
treated × $\mathbb{1}_{t=4}$	-0.0350**	-0.0516***
	(0.0150)	(0.0162)
treated × $\mathbb{1}_{t=5}$		-0.0620**
		(0.0185)
ATT	-0.0347**	-0.0324***
	(0.0134)	(0.0071)
Estab-Job, Firm × Year FEs	X	X
mean(posted salary)	$59,082	$60,242
N	709,226	1,180,096
Firms	3,974	9,557
States	43	48
R^2	0.900	

Notes: Posted wage data at the establishment-job level come from Burning Glass Technologies. Column (2) uses the staggered design estimator of de Chaisemartin and d'Haultfœuille (2020). The mean corresponds to the average posted salary for not-yet-treated establishment-jobs. The sample period is from 2010-2017. Standard errors are clustered by firm and for the moderate reform analysis are based on 100 bootstrap replications.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

for all states (regardless of whether they are multi-state establishments which operate in both treatment and reform states) and for single-state firms. Finally, we conduct the Fisher exact test recommended by Imbens and Rubin (2015) and find that there are no placebo states (out of a possible 42) which have t-statistics which are more extreme than North Carolina.

We further point out that the large negative wage effects uncovered in this study are inconsistent with a mean reversion explanation for the employment effects. Specifically, if establishments in reform states had been growing faster only due to having experienced a larger decline in aggregate employment during the Great Recession, starting wages would have likewise outpaced those offered in control establishments.

A final exercise is motivated by the fact that some workers are more likely to be affected by the reform than others. To identify workers who are less likely to be affected by the reforms, and hence where we would expect to see little to no effect, we split occupations into two groups. We categorize "low exposure" occupations as those which have an above median fraction of workers who were either unemployed for less than 20 weeks or employed for less than 20 weeks based on CPS data in the pre-reform period. This restriction captures two ideas. First, if unemployment duration is lower than 20 weeks, the reforms which reduced maximum duration from 26 to 20 weeks should not be binding. Second, if employment duration is less than 20 weeks, an individual is unlikely to qualify for UI benefits and so again the reforms should not matter as much (the exact number of weeks and earnings required for eligibility varies by state; this is a conservative cutoff). "High exposure" occupations, where we expect to see a larger effect, are defined as the complement to the "low exposure" group.

The top panel of Table 9 first reports estimates for high exposure occupations, and finds sizable estimates for both North Carolina and the moderate reform states, although the estimate for North Carolina is no longer statistically significant due to a larger standard error. In sharp contrast, the estimates for low exposure occupations are close to zero and statistically insignificant, both for North Carolina and the moderate reform states.

6 Results Using Current Population Survey Data

In this short section, we add results for several outcomes using CPS data. Since we cannot match the same firm's establishments across states, we cannot use our multi-state firm design with CPS data. Instead we use as controls other Southern and Midwestern states which did not undergo a permanent UI reform. And to improve precision, we combine North Carolina with the moderate reform states.

With these caveats in mind, column (1) of Table 10 estimates whether an individual is employed

Table 9: Effects on Posted Wages, High versus Low Exposure Occupations

	North Carolina Reform	**Moderate Reforms**
dep var = log(posted wages)	(1)	(2)
	Panel A: High Exposure Occupations	
ATT	-0.0345	-0.0414***
	(0.0259)	(0.0100)
mean(posted salary)	\$49,599	\$52,011
N	233,066	398,270
Firms	1,768	4,422
R^2	0.917	
	Panel B: Low Exposure Occupations	
ATT	-0.0084	0.0129
	(0.0194)	(0.0152)
mean(posted wages)	\$69,841	\$66,822
N	91,468	185,812
Firms	1,352	3,891
R^2	0.913	
Estab-Job, Firm × Year FEs	X	X

Notes: See notes to Table 8. Low exposure occupations are defined as those which have an above median fraction of workers who were either unemployed for less than 20 weeks or employed for less than 20 weeks based on CPS data in the pre-reform period. High exposure occupations are the complement.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

using the staggered design of de Chaisemartin and d'Haultfœuille (2020). Employment probabilities increase by approximately 1 percentage point (s.e. = 0.43). Relative to the mean employment rate of 65%, this amounts to a 1.5% increase in employment. As with the multi-state firm analysis, the event-study coefficients indicate no evidence of pre-trends but rather a gradual increase in employment that begins in the year in which the reforms were implemented. This is qualitatively consistent with our headline employment estimates using EEO1 data and our multi-state firm design.[37]

We can also use CPS data to analyze starting salaries, looking at individuals whose job tenure is less than one year. Compared to the Glassdoor data used in our main analysis, the CPS sample size for starting salaries is much smaller. While more noisily estimated than the Glassdoor results, the time pattern is similar: there is no effect on wages prior to the reform, but a divergence afterwards. As the second column in Table 10 shows, the combined effect of the reform over the post-reform

[37] These findings are also consistent with Karahan et al. (2025), who show that the Missouri reform increased job-finding rates among the unemployed, with half of the effect owing to a rise in vacancy creation.

Table 10: Effect of the UI Reforms on Outcomes Measured in the CPS

	All Reforms		
dependent variable	$\mathbb{1}\{employed\}$	log(starting salary)	weeks unemployed
	(1)	(2)	(3)
treated × $\mathbb{1}_{t=-4}$	-0.0060		1.634
	(0.0067)		(1.175)
treated × $\mathbb{1}_{t=-3}$	-0.0059	0.0083	1.399
	(0.0036)	(0.0281)	(1.787)
treated × $\mathbb{1}_{t=-2}$	0.0032	0.0225	1.053
	(0.0025)	(0.0234)	(0.734)
treated × $\mathbb{1}_{t=-1}$	-0.0026	0.0008	0.689
	(0.0027)	(0.0224)	(1.339)
treated × $\mathbb{1}_{t=0}$	0.0018	-0.0411	-0.994
	(0.0023)	(0.0399)	(1.191)
treated × $\mathbb{1}_{t=1}$	0.0059	-0.0657**	-1.212
	(0.0049)	(0.0331)	(1.625)
treated × $\mathbb{1}_{t=2}$	0.0078*	-0.0499	-1.715
	(0.0047)	(0.0342)	(1.125)
treated × $\mathbb{1}_{t=3}$	0.0099*	-0.0506	-3.549***
	(0.0057)	(0.0503)	(1.102)
treated × $\mathbb{1}_{t=4}$	0.0159**	-0.0846**	-4.154***
	(0.0066)	(0.0413)	(1.173)
treated × $\mathbb{1}_{t=5}$	0.0138**	-0.1007**	-5.589***
	(0.0063)	(0.0487)	(1.597)
treated × $\mathbb{1}_{t=6}$	0.0159**		-7.310***
	(0.0073)		(2.365)
ATT	0.0098**	-0.0625*	-2.785***
	(0.0043)	(0.0327)	(0.983)
State, Year FEs	X	X	X
Predetermined Characteristics		X	
mean(dep var)	0.568	$17.1	31.69
N	7,112,362	24,858	269,946
R^2	0.006	0.281	0.047

Notes: Data come from the Basic monthly CPS merged with the CPS Job Tenure and Occupational Mobility Supplements. For starting salary, the sample includes all individuals whose job tenure is less than 1 year. Predetermined characteristics include sex, education, and age controls. Column (2) uses the staggered design estimator of de Chaisemartin and d'Haultfœuille (2020). The sample period is from 2009-2018. Standard errors are clustered at the state level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

period is a 6.3% reduction in wages.

As a final exercise using CPS data, we estimate the effect of the UI reforms on unemployment duration. As reported in column (3), prior to the reforms there are no statistically significant differential effects, although the point estimates suggest a possible pre-trend. In the first two years after the reform, there is a small, but statistically insignificant, drop in unemployment duration. Starting in year 3, the coefficients become larger, with an average decline of 2.8 weeks per year in the post-reform period. This translates to an 8% drop in unemployment duration relative to control states.[38] This provides suggestive evidence that when UI generosity falls, individuals are incentivized to accept jobs more quickly.

7 Discussion

The twin results in our paper are that UI benefit reductions result in both an increase in employment and a decrease in starting salaries. A natural explanation is that workers reduce their reservation wages in response to depressed outside options, and employers take advantage of this by offering lower wages and more jobs. The negative co-movement of employment and wages post reform strongly suggests a labor supply shock, and mitigates against confounding changes in labor demand driving the results.

Our findings are consistent with the seminal model of Mortensen and Pissarides (1994), where workers' reservation wages fall when outside options worsen. As noted by Hagedorn et al. (2019), in the Mortensen-Pissarides paradigm the effect of lower UI benefits on employment (and labor force participation) is a priori ambiguous, but should rise if the job creation channel is strong enough. To see this, consider the 3-state modeling framework of Krusell et al. (2017) where individuals can be employed, unemployed, or not in the labor force. A cut in UI benefits should cause the unemployed to either search harder or drop out of the labor force (if they were only unemployed to collect benefits). Hagedorn et al. (2019) argue that a lower level of UI benefits reduces labor market frictions because it triggers job creation and increases the probability of finding a job. Our results indicate that a job creation channel driven by lower wages is large enough to increase aggregate employment.

Our wage results can also be understood using the model found in Chodorow-Reich et al. (2019).

[38] This finding is broadly consistent with Katz and Meyer (1990), which estimates that a one-week increase in potential benefit duration increases duration by .16-.20 weeks and other studies, such as Card and Levine (2000); Lalive et al. (2006); Lalive (2007); Van Ours and Vodopivec (2006), which find positive effects of UI benefits on duration or exhaustion rates.

In their model, wages depend on two things that could change in response to a UI reform: the opportunity cost of employment (for the unemployed) and the tightness of the labor market (defined as vacancies over unemployment). As UI benefits decrease, the opportunity cost of employment will fall, which is predicted to lower wages. Tightness is likely to increase and hence raise wages for two reasons: some individuals could drop out of the labor force (reducing unemployment) and firms could create new jobs to take advantage of lower wages (potentially increasing vacancies). What we can say is that if tightness increases, the opportunity cost channel dominates, since we find a reduction in posted wages.

With these models and the appropriate caveats in mind, we can calculate a non-standard labor demand elasticity by dividing our baseline estimate of the change in employment (Table 3, column 1 or 4) by our baseline estimate of the change in posted wages (Table 8, column 1 or 2), and use the delta method to calculate standard errors. We estimate an elasticity of -0.363 (s.e.=0.205) for North Carolina and -0.415 (s.e.=0.177) for the moderate reform states. These elasticities fall within the range of estimates derived in other settings.[39]

Our findings for employment and wages stand in contrast to much of the existing literature, which finds little to no effect of UI duration on either margin (see footnote 1). There are several possible explanations for this divergence. First, we study cuts to state UI programs which are (i) large in percentage terms, (ii) permanent, and (iii) enacted during a period of economic growth. In contrast, most other recent well-identified work studies reforms which were smaller in percent terms and enacted temporarily during periods of high unemployment, when moral hazard costs were likely to be lower (e.g., see Schmieder et al. 2010; Rothstein 2011; Farber and Valletta 2015; Farber et al. 2015; Kroft and Notowidigdo 2016; Chodorow-Reich et al. 2019; Dieterle et al. 2020; Boone et al. 2021a; Coombs et al. 2022). Notable exceptions include Karahan et al. (2025) and Johnston and Mas (2018) which also study a large, permanent, post-recession reform and find sizable effects on employment as we do.

To illustrate why these contextual differences could matter, consider work which leverages the

[39]These are somewhat smaller than historical estimates calculated based on data from British plants and coal mines, American women following World War II, and manufacturing labor in Germany (Hamermesh 1996; Acemoglu et al. 2004; Addison et al. 2008), and somewhat larger than the median estimate using increases in minimum wages (Dube and Zipperer 2024).

extensions in UI duration arising from the Emergency Unemployment Compensation (EUC) and Extended Benefits (EB) programs enacted in the aftermath of the Great Recession. These benefits varied based on whether unemployment in a state exceeded various thresholds. For example, in 2010, individuals in states with an unemployment rate *just above* 8.5% were eligible for 26 weeks of regular UI, plus 20 weeks of EB, plus 37 weeks of EUC, for a total of 83 weeks, while individuals in states with an unemployment rate *just below* 8.5% were eligible for a total of 70 weeks. While this is a 13 week contrast (83 versus 70 weeks), it is only a 16% cut off of a high base. It therefore should arguably have less impact than the average 11.5 week cut in regular benefits for North Carolina's reform off of a low base of 26 weeks; this amounted to a 44% cut in maximum duration relative to control states. Since North Carolina also simultaneously cut benefit amounts permanently, the maximum value of benefits fell even further, by 64% (see Table 1).[40] In the moderate reform states, the average post-reform reduction was also large, ranging from 23-39%.

Additionally, since the reforms we study were permanent and enacted during a period of economic growth, establishments may have been more willing and able to expand employment. These differences in context likely explain a sizable part of the reason we find larger impacts for the reforms we study.

Another reason the estimates could differ is that we compare employment and wage changes across different establishments within the same firm but operating in treated versus untreated states. This strategy diverges from recent literature which uses a border county-pair design coupled with federal or state UI extensions and expirations, and finds mixed results (Chodorow-Reich et al. 2019; Hagedorn et al. 2025). Many border county-pairs are in the same labor market. This makes it more difficult for establishments on one side of the border to offer a different wage compared to establishments on the other side of the border, since both establishments are competing for the same workers. In other words, establishments may not be able to fully take advantage of workers' lower reservation wages by offering lower wages and expanding employment in a border county-pair design.

A final reason for why our estimates may differ is that we use novel microdata on establishment-

[40] Moreover, the UI reforms in North Carolina resulted in their federal EUC agreement being terminated 6 months before it expired for other states.

level employment (from the EEOC), starting salaries (from Glassdoor), and posted wages (from Burning Glass Technologies). By comparing establishments within the same firm but operating in different states, these data allow us to better control for any variation in job types across treatment and control states. A key advantage of the Burning Glass Technology data in particular is that we can look at posted wages, which has not been done before.

8 Conclusion

We study the effect of large reductions in the value of UI benefits on employment, starting salaries, and posted wages. Using a multi-state firm identification strategy, we find that establishments in North Carolina see employment rise by 1.3% more, on average, than do establishments belonging to the same firm but which are located in states not subject to the reform. We find evidence indicating that the mechanism is a drop in reservation wages; there is a 5.5% decline in starting salaries. Similar results, albeit smaller in magnitude, are found for the 6 states which enacted cuts to maximum duration but not weekly benefit amounts. We also leverage data on job ads, and find that wage postings for the same job in the same firm fall by 3.5% in North Carolina and by 3.2% in moderate reform states.

Our paper contributes to evidence on the macro effects of UI reductions on employment and wages, but from the new perspective of firms with presence in multiple states where UI policies differ. The general equilibrium effects which emerge paint a more complicated picture of the desirability of unemployment insurance reductions compared to most of the prior literature. On the positive side, these UI reforms stimulated employment and lowered benefit payouts. But counterbalancing this was a reduction in the starting wages of workers.

Data Availability Statement

The replication package for this paper is available at https://doi.org/10.5281/zenodo.20014224. The EEO-1 data used in this paper are confidential and were accessed through an Inter-Personnel agreement with the Equal Employment Opportunity Commission. New requests for data access can be made through the Office of Enterprise Data and Analytics (OEDA). The Glassdoor salary data are proprietary and were obtained directly from Andrew Chamberlain, then Chief Economist of

Glassdoor; new requests for access should be directed to Glassdoor. The Burning Glass Technologies job posting data are proprietary and were obtained through a data purchase from Burning Glass Technologies (since renamed Lightcast); new requests for access should be directed to Lightcast. All publicly available data sources are included in the replication package. The authors commit to providing reasonable assistance to researchers seeking to replicate the results.

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