

Unemployment Insurance, Starting Salaries, and Jobs

Gordon B. Dahl* and Matthew Knepper†

May 17, 2022

We study the labor market effects of permanent 23-50% reductions in unemployment insurance benefits available in seven states. Leveraging linked firm-establishment data, we find that establishments based in reform states experience 1.5-2.4% faster employment growth relative to the same firm's establishments in other states. Using a similar multi-state firm design, starting salaries are 1.8-7.2% lower in reform states and posted salaries for the same job fall by 1.4-5.5%. These labor supply shocks yield an average labor demand elasticity of -1.0. Our results reveal a substantial decline in match quality and worker bargaining power as UI benefits become less generous.

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

JEL Codes: J64, J65, J38

*UC San Diego, Norwegian School of Economics, NBER, ifo Institute, CESifo, CEPR, IZA. email: gdahl@ucsd.edu

†University of Georgia. email: mknepper@uga.edu.

We are grateful to Ron Edwards and the EEOC for their guidance and provision of the EEO-1 microdata, to Andrew Chamberlain for generously sharing Glassdoor data, and to Burning Glass Technologies for making their data available. We thank colleagues and seminar participants at several universities and conferences for valuable feedback and suggestions.

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 potential duration or benefit amounts, will increase match quality and worker bargaining power by affording individuals more time to find a job and generate competing offers (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, with general equilibrium effects muting 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.² An alternative explanation is that previously studied reforms, particularly in the US context, were either not large enough to generate substantial changes in workers' outside options or were enacted temporarily during periods of high unemployment when moral hazard costs are likely to be lower.³

¹For 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. (2021); Chodorow-Reich et al. (2019); Dieterle et al. (2020) find no effect, Schmieder et al. (2012) report small negative effects, while Hagedorn et al. (2015) 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). A lone exception finding a positive effect is Nekoei and Weber (2017).

²See Michaillat (2012), Lalive et al. (2015), Marinescu (2017), Landais et al. (2018), Ganong and Noel (2019) and Kekre (2021).

³ See Schmieder et al. (2010); Rothstein (2011); Farber and Valletta (2015); Farber et al. (2015); Hagedorn

Relative to the existing literature, we make several contributions to the evidence on how UI affects wages and employment. First, we bring to bear an approach never before used to study these questions: (i) we compare employment growth across different establishments within the same firm but operating in treated versus untreated states and (ii) we compare posted wage growth within the same job ad and firm but likewise across establishments in treated versus 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), which allows us to distinguish between worker composition, match quality, and bargaining power channels. Third, we focus on cuts to state UI programs which are large, permanent, and enacted during a period of economic growth. Using this approach and data, we estimate large increases in employment and decreases in starting salaries, consistent with the idea that workers reduce their reservation wages in response to depressed outside options, and that firms respond to cheaper labor costs by ramping up their hiring.

Specifically, we study the employment and earnings responses to reforms in 7 different states which sharply cut the generosity of their state UI programs in the 2010s. The largest of these state-level reforms occurred in North Carolina in 2013. This reform is well-suited for understanding the consequences of cutting UI benefits for two reasons. First, it was larger than any previous rollback implemented in the U.S. The change simultaneously reduced the maximum weekly benefit from \$535 to \$350 and the maximum duration from 26 to 20 weeks. The combined reductions permanently reduced the maximum value of UI benefits by 50%. Second, the cuts were implemented based on the insolvency of North Carolina's state UI fund, rather than local labor market conditions. This allows us to compare workers in North Carolina to other states with similar labor market trends, but which had more prudent funding of their state UI programs.

Six other states also enacted sizable reductions in UI generosity, but which were more

et al. (2015); Kroft and Notowidigdo (2016); Chodorow-Reich et al. (2019); Dieterle et al. (2020); Boone et al. (2021).

modest compared to North Carolina. These “moderate reform” states (Florida, Georgia, Kansas, Michigan, Missouri, and South Carolina) cut maximum benefit durations by 6 weeks but holding fixed weekly benefit amounts, resulting in the maximum value of benefits falling permanently by 23%. We analyze North Carolina’s reform separately due to its more drastic nature, and combine the six moderate reform states to gain precision. In contrast to most studies, we estimate the effects of reductions in UI generosity, rather than expansions, during a period when the labor market was recovering, rather than languishing.

We begin by deploying microdata and an identification strategy which leverages linked firm-establishment panel data on employment that 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 first compare employment growth 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 due to the tendency of establishments, and particularly franchisees, to offer similar jobs and have similar workplace structures.⁴ Our identifying assumption is that intra-firm employment growth across establishments is not trending differently prior to the UI reform, a fact we confirm empirically. This strategy diverges from the recent literature which uses a border county-pair design coupled with federal or state UI extensions and expirations, and finds mixed results (see footnote 3).

Our first key finding is that following the reform, North Carolina-based establishments experience 2.4% faster employment growth than do their same-firm counterparts in other states over the two years after the reform. For the six moderate reform states, employment grows 1.5% relative to same-firm counterparts in other states. These results suggest that any contractionary effect on consumer spending and aggregate demand, or increased competition

⁴For 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. 2021). 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.

for jobs are not large enough to overturn the incentive effects of finding a new job quickly.

Instead, our results are more consistent with workers' employment and reservation wages depending on their outside options, which decrease when UI benefits become less generous, as suggested by Mortensen and Pissarides (1994) and Mitman and Rabinovich (2015). We focus 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, our second key finding is that the earnings of new hires fall by an economically and statistically significant 7.2% in North Carolina establishments relative to the same firm's establishments in control states. In the moderate reform states, the corresponding effect is a 1.8% decline in starting salaries.⁵ These drops combine not only the effect for new hires transitioning from unemployment and changing jobs, but also those entering the labor force.⁶

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 in the CPS. Using the Glassdoor data, we estimate that lower match quality in the form of firm and occupational downgrading can account for approximately 40% of the wage effect. The remaining 60%—a 5.9% and 1.6% drop in starting salaries in North Carolina and the moderate reform states, respectively—is due to a decline in either unobserved match quality or worker bargaining power. One limitation of the Glassdoor data is that we cannot entirely rule out 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. We use the near-universe of posted wages in online job ads from Burning Glass Technologies, which as noted in Hazell and Taska (2020), has the key advantage that they are not contaminated by

⁵A robustness analysis using workers with less than one year of tenure in Current Population Survey (CPS) data confirms these findings for starting salaries.

⁶We calculate the previously unemployed account for 34.5% of all new hires, based on transition rates from Lise and Robin (2017) and Kudlyak and Lange (2018) combined with population shares.

the compositional or match quality effects which could be present in the wages of new hires. We estimate that the reforms generated a 5.5% and 1.4% reduction in posted wages, effect sizes which are nearly identical to the Glassdoor estimates that account for occupational and firm fixed effects.⁷

If workers view re-employment to be more urgent following a reduction in UI benefits, they should not only return to work at lower wages, but also return to work sooner. We test this using data from the CPS, combining North Carolina with the six moderate reform states for increased precision in a dynamic event study design. We find that the reductions in UI generosity result in an average 2.8 week decline (8% drop) in unemployment spell lengths.

The unexpected UI reductions represent exogenous negative shocks to workers' outside options, which shift labor supply curves outwards. Marginal revenue product of labor declines for two reasons: increases in employment due to lower reservation wages and reduced match quality. With two points along firms' labor demand curves, we can calculate the labor demand elasticity.⁸ Combining our estimates of the changes in employment and in posted wages from the EEOC and Burning Glass analyses, respectively, we derive an average labor demand elasticity of -1.0 across all of the 7 reform states. These are in line with historical estimates of labor demand elasticities calculated based on data from British plants and coal mines, American women following World War II, and manufacturing labor in Germany.⁹ Our elasticity lies at the higher end of estimates based on changes to the minimum wage, which could be explained by UI covering low, middle, and higher wage workers.

This 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 reductions stimulate employment growth and reduce the fiscal burden of the program. But counterbalancing this is a nontrivial reduction in worker wages—a reduction generated not by way of drawing into

⁷These estimates are likely to be a lower bound given recent evidence that 35% of firms are national wage setters, constraining wages to be identical within an occupation but across all locations (Hazell et al. 2021).

⁸This assumes labor markets are competitive. If, instead, the wage is declining because firms are paying workers a lower share of their marginal revenue product, the ratio of changes in employment to wages is not directly interpretable as a labor demand elasticity.

⁹See Hamermesh (1996); Acemoglu et al. (2004); Addison et al. (2008), respectively.

the workforce new low-wage individuals, but rather through current job seekers settling for worse jobs or the same jobs at lower pay.

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 identification strategy. Section 4 describes our empirical results, while Section 5 offers concluding remarks.

2 Setting and Data

2.1 State UI Reforms

During the protracted recovery from the Great Recession, unemployment trust funds in several states neared insolvency as a result of record benefit payouts. In response, 7 states passed legislation that permanently cut the number of weeks available through regular UI from the long-established norm of 26 weeks to 20 weeks (GAO 2015). These states were Florida, Georgia, Kansas, Michigan, Missouri, North Carolina, and South Carolina.¹⁰ North Carolina went a step further than the other states, becoming the only one to contemporaneously reduce its maximum weekly benefits (by nearly 35% from \$535 to \$350 per week) while also reducing the maximum benefit duration by 6 weeks.¹¹

Due to its draconian cut, we will highlight North Carolina separately from the other reform states throughout the paper. North Carolina’s House Bill 4, “An Act to Address the Unemployment Insurance Debt and to Focus North Carolina’s Unemployment Insurance Program on Putting Claimants back to Work,” was passed in February of 2013 and became effective in July of that same year. As detailed in Table 1, the maximum level of state UI benefits available to North Carolinians fell from approximately \$14,000 to \$7,000 (ignoring discounting).

¹⁰Arkansas 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.

¹¹Following the reform in North Carolina maximum duration drops to 12 weeks when the state UR rate is less than 5.5%, and increases by 1 week for each 0.5% increase until reaching a maximum of 20 weeks when the state UR reaches 9%. Kansas, Georgia, and Florida likewise used sliding scales.

North Carolina’s policy environment is ideal for studying the effects of UI generosity on labor supply outcomes for four related reasons. First, its cuts were far larger than any previous rollback implemented in the United States, and these cuts were permanent. The simultaneous reduction in potential benefit duration (PBD) and replacement rates reduced the maximum value of benefits by 50%. While the labor supply response to increases in PBD has been found to be relatively moderate,¹² responses to changing benefit levels are somewhat higher,¹³ so a combined cut should elicit a stronger response. The unparalleled magnitude of the reform also raises the likelihood that any attendant effects on reservation wages, which have been found to be empirically small in much of the previous literature,¹⁴ will be detectable.

Second, North Carolina’s cuts were highly salient. The cuts outlined in House Bill 4 (HB4) 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.¹⁵ The number of individuals affected by the reform was sizable. Figures 1a and 1b show that the fraction of short-term unemployed workers receiving any UI benefits plunged from 30% to just 10% in the three years following the reform, while reciprocity rates held steady in control states.

Third, the North Carolina cuts were made on the basis of insolvency issues surrounding its state UI fund, rather than on changing local labor market conditions. This is in contrast to state Extended Benefits (EB) and federal Emergency Unemployment Compensation (EUC) programs, which use explicit thresholds based on state unemployment or insured unemployment rates to determine the duration of benefits (Rothstein 2011; Farber and Valletta

¹²Duration 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.

¹³Benefit elasticities range from 0.10 to 1.2 in the United States, with a median of 0.38. See 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 (See Katz and Meyer (1990); Carling et al. (1996); Card and Levine (2000); Røed and Zhang (2003); Van Ours and Vodopivec (2006); Dahl (2011)).

¹⁴See 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).

¹⁵<https://www.usatoday.com/story/news/nation/2013/07/21/north-carolina-unemployment/2571889/>.

2015; Farber et al. 2015; Marinescu 2017). In contrast to many prior studies, we are able to estimate responses to discrete changes in UI generosity as the labor market is recovering rather than languishing. Thus, our estimates are better able to rule out measured effects being driven by worsening local economic conditions (Schmieder and Von Wachter 2016).

Lastly, regular state UI benefits are the first source of benefits from which individuals claiming UI draw. Only after exhausting regular UI benefits are individuals eligible for the federal EB and EUC. Thus, access to regular state UI benefits are relatively more valuable as they are discounted less heavily. We note the draconian cuts enacted by North Carolina made residents ineligible for the federal EB and EUC programs, but that these programs ended in shortly after North Carolina’s reform (6 months later).

One source of confounding variation is that just 5 months after the new UI rules became effective, North Carolina 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 6 states which also reduced the generosity of their UI programs in a similar manner, but which did not modify their income tax code.

While these “moderate reform” states also permanently cut maximum benefit durations by 6 weeks, they left weekly benefit levels unchanged, so that maximum benefit generosity fell by half as much as in North Carolina.¹⁶ For conciseness and to increase precision, we combine the six reform states for analysis in dynamic event studies, leaving state-specific analyses to the Appendix. As shown in Figure 1c there is a nontrivial effect on UI benefit receipt in these six moderate reform states, with a 6 percentage point drop in the fraction

¹⁶The details of each reform are provided in Table 1. In addition, the cuts triggered a reduction in federal extended UI benefits in moderate reform states; North Carolina’s more draconian cuts made workers ineligible for any amount of federal benefits.

of short-term unemployed workers receiving UI benefits (compared to a 20 percentage point drop in North Carolina).

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 finds that the less expansive UI benefit reduction in Missouri (one of our moderate reform states) shrank unemployment spell lengths and decreased the overall unemployment rate by 1%.

2.2 Data

We use a variety of data sources to study a broad array of labor market outcomes. To measure changes in state-level unemployment rates, we leverage BLS Local Area Unemployment Statistics (LAUS) from one year following the official end of the Great Recession (July of 2010) through December of 2018. Additionally, we combine Department of Labor (DOL) administrative data¹⁷ with monthly Current Population Survey (CPS) data to calculate short-term UI recipiency rates. This measure divides the monthly number of individuals receiving weekly UI benefits by the number of short-term unemployed—defined as those unemployed for 26 weeks or fewer—as in Schaefer and Evangelist (2014).

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. The EEO-1 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.¹⁸

¹⁷DOL ETA Form 5159 data.

¹⁸See <https://www.eeoc.gov/employers/eo-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.

Appendix Figures A1a and A1b show the fraction of total U.S. private employees and establishments covered by the EEO-1 Survey, respectively, 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 EEO-1 size reporting thresholds. Appendix Figure A1c shows that over three-quarters of all U.S. workers are employed in these relatively large establishments, thus minimizing concerns about the generalizability 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). These restrictions generate a study sample of nearly 1 million and 1.5 million establishment-year observations for the North Carolina Reform and Moderate Reform analyses, respectively.¹⁹ We also use Basic Monthly Current Population Survey (CPS) data to validate all of our employment results obtained from the EEOC microdata.

To isolate worker 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. 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 experience are new hires, so that the reported salary is likely to be their starting salary. This distinction is important as it allows us to differentiate between wage effects for existing employees and those of new hires, whose reservation wages are more likely to be affected by contemporaneous UI generosity.

We add to this the near-universe of 10 million posted wages from online job ads between 2010-2017 from Burning Glass Technologies (BGT). As documented in Hazell and Taska (2020), the BGT data cover 70% of all online job postings. However, just 17% of all ads

¹⁹Analogous unrestricted samples have 3.2 million and 4.8 million observations.

include posted wages, which implies that total coverage is approximately 10%. Nonetheless, Hazell and Taska (2020) 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. It is worth noting that the average posted salaries (\$57,000) in BGT data are slightly lower compared to those in Glassdoor for new hires (\$67,000).

We supplement the Glassdoor and Burning Glass analyses with data that merges the Basic CPS with the Outgoing Rotation Group and the Job Tenure and Occupational Mobility Supplement. Doing so allows us to replicate the same study design on a publicly accessible data source. However, because the Job Tenure and Occupational Mobility Supplement is circulated only every other January, the merged CPS data includes far fewer observations. For example, for the North Carolina analysis, there are just 25,000 relevant worker observations compared to more than 524,000 Glassdoor observations (with over 1 million observations in the Moderate Reform State analysis) and 500,000 BGT observations (1.4 million in the Moderate Reform State Analysis). Basic monthly CPS data also allows us to estimate unemployment durations in UI reform states versus nontreated states.

3 Research Design

We first discuss our research designs 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 first regression model using DOL, CPS, and LAUS data is for aggregate state-level outcomes and takes the following form:

$$Y_{st} = \sum_{t=-3}^5 \beta_t \times NC_s + \alpha_s + \theta_t + \epsilon_{st} \quad (1)$$

where Y_{st} is either the monthly short-term UI reciprocity rate or deseasonalized unemployment rate in state s and year-month t . NC_s is an indicator for North Carolina, and the main coefficients of interest are β_t , which represent the effect of the UI reform. The coefficient on β_{t-1} is set to zero so that it can serve as the reference period. For this aggregate analysis,

we restrict our primary sample of control states to all 20 Southern and Midwestern states that did not implement a moderate reform. The regression controls for state fixed effects, α_s , and year-month effects, θ_t , to control for average labor market conditions in a state and overall trends in other states. We cluster our standard errors at the state level.

Our second 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 growth and starting salary growth using panel fixed effects:

$$Y_{efst} = \sum_{t=-3}^2 \beta_t \times NC_{efs} + \phi_f + \alpha_s + \theta_t + \epsilon_{efst} \quad (2)$$

where Y_{efst} is either log employment or starting salaries in establishment e located in state s which belongs to firm f in year t (or quarter q in the BGT specifications). 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. Thus, our treatment variable, NC_{efs} , equals one whenever an establishment is located in North Carolina. We include firm fixed effects, ϕ_f , in the regression in addition to the state and year fixed effects.

Each β_t coefficient can be interpreted as the intra-firm difference in employment growth between North Carolina and other same-firm establishments t years after the reform. For example, the estimates will capture employment (starting salary) growth for a Walmart in North Carolina versus a Walmart 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 a causal effect of the UI reform if the pre-trends are parallel, a condition we verify empirically. It must also be true that no other state-level policy changes were enacted simultaneously. As we noted earlier, North Carolina implemented corporate and personal income tax reductions at around the same time, we address this confound by bounding the associated employment and wage effects, and by leveraging a similar design on moderate reform states, where no

such legislation was passed. Standard errors are clustered at the firm level, but are also robust to state level clustering.

When estimating the effect of the reform on posted salaries from BGT, we make two important changes: (1) we construct quarterly panels of establishment-jobs, as in Hazell and Taska (2020) and (2) we replace ϕ_f with ϕ_{fj} to capture employer-job fixed effects. One additional important point of clarification is that 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. We note that the results are robust to the exclusion of partially treated years from the analyses.

Our third regression model uses worker-level outcomes from the CPS:

$$Y_{ist} = \sum_{t=-4}^6 \beta_t \times NC_{is} + \gamma X_i + \alpha_s + \theta_t + \epsilon_{ist} \quad (3)$$

where Y_{ist} is the employment probability, starting salary, or unemployment duration for individual i in state s and year t . In addition to state and year fixed effects, we also control for predetermined characteristics, X_i . As in equation 1, the other specification for which we do not observe firm-establishment panels, we restrict the set of controls to individuals living in nontreated Southern or Midwestern states. Standard errors are clustered at the state level.

For the moderate reform states, we analyze the same set of outcomes as for North Carolina, but combine data from the 6 states into a single regression because they all cut maximum duration by 6 weeks. Since the reforms are implemented at different times in different states, we estimate effects in event time using a dynamic difference-in-difference framework. Here we use the approaches of De Chaisemartin and d’Haultfoeuille (2020), or

in models which include firm or employer-job fixed effects, Borusyak et al. (2021).

4 Results

4.1 Short-term UI Receipt

We first document the effect of the reforms on UI receipt. To do so, we calculate monthly short-term reciprocity rates following Schaefer and Evangelist (2014), dividing the number of individuals receiving state UI benefits (DOL administrative data) by the number of short-term unemployed individuals (CPS data). Short-term is defined as 26 weeks or less, which corresponds to the maximum UI benefit duration before the reform for treated states, and the maximum UI benefit duration during the entire sample period for all states in our control group.

In Figure 1, we plot the evolution of the short-term reciprocity rate over time in North Carolina, starting after the official end of the Great Recession and continuing for 9 years. The blue line in panel (a) plots the rate in North Carolina at the monthly level, while the red line plots the corresponding rate for other Southern and Midwestern states which did not change their UI program over this time period. The dashed vertical line marks the date North Carolina passed their UI reform bill, while the solid vertical line denotes the date the reform became effective for newly filed claims.

Prior to the implementation of the reform, North Carolina (blue line) and other states (red line) exhibit a similar level and trend for short term UI receipt. Since the economy was recovering from the Great Recession, UI participation is gradually falling in the pre-period, with some seasonal patterns present as well. After the reform, the control states' reciprocity rate continues its mild decline. In sharp contrast, North Carolina's rate drops shortly after the reform's implementation.²⁰ Two years after the reform, North Carolina's reciprocity rate

²⁰Only newly filed claims are subject to the new rules after the reform date, so immediately after the reform there are a mix of participants under the old and new rules.

is 10% compared to a rate of 30% in control states.

Panel (b) graphs the difference between the blue and red lines, along with 95% confidence intervals. There is no evidence for differential pre-trends, with the UI reciprocity rate difference between North Carolina and the control states bouncing around zero. After the reform, the gap becomes negative and widens to a 20 percentage point difference roughly two years after the reform, with this gap persisting until the end of our sample period.

Estimates for the moderate reform states can be found in panel (c). Using a dynamic event study design, there is a 5.5 percentage point reduction in the short-term UI reciprocity rate in treated states relative to controls in the post-reform period. This smaller effect for moderate reform states is expected given the more extreme cuts enacted by North Carolina.

These analyses confirm that the UI reforms sharply curtailed the use of UI, which was the intent of the law changes. The reduction in use is partly mechanical, as individuals were eligible for 6 fewer weeks of benefits after the reform. But it could also be partly driven by the 35% reduction in weekly benefit levels in North Carolina, which could have caused individuals to exit UI earlier.

4.2 Aggregate Labor Supply

Having established the salience and magnitude of the reform, we turn to its effects on aggregate labor supply using the BLS's LAUS data. In Figure 2 we plot the unemployment rate for North Carolina (blue line) versus other nontreated Southern and Midwestern states (red line). North Carolina has a higher unemployment rate in the pre-period, so to make visual comparisons easier, we shift up the line for the other states so they have the same mean the month prior to the reform (dashed red line). The pre-reform unemployment rate in North Carolina is approximately 2 percentage points higher than in control states, consistent with North Carolina having more generous UI benefits (a maximum of \$14,000 compared to \$8,300 in control states).

The figure reveals that unemployment rate trends in North Carolina and in other states

closely tracked one another prior to the reform. But in February of 2013, when the governor signed into law the bill setting the UI rollback parameters that would take effect in July, we begin to observe a drop in North Carolina’s unemployment rate. By December 2013, it declines a full 2 percentage points relative to the other states, after which the rates continue to track one another for the subsequent 5 years. This initial decline was first documented in an unpublished note by Hagedorn et al. (2014).²¹

Figure 2, panel (c) shows the event study graph for the moderate reform states. The average effect is smaller at approximately 1.2 percentage points, as expected, and grows in magnitude over time. This decline comports with the results in Johnston and Mas (2018), which finds a 1 percentage point drop in the unemployment rate in Missouri (one of our moderate reform states).

4.3 Employment Growth for Multi-state Firms

The literature often finds 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). We investigate this hypothesis using an approach which leverages multi-state firms, comparing the 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.

We use the EEOC’s census (the EEO-1 files) of all establishments in the United States which are required to report employment data annually. 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 growth evolves in establishments that are part of the same multi-state firm but reside in different states.

Figure 3 panel (a) plots the average log employment for establishments in North Carolina

²¹The Hagedorn et al. (2014) note was written in 2014, shortly after the change in North Carolina. The authors caution that “only a few months of data are available and sample sizes available in most data sets are too small to yield reliable predictions of month to month changes in variables such as employment, unemployment, etc. So the evidence provided below should be interpreted with extreme caution.”

versus other states. We first residualize log employment by netting out establishment, year, and state fixed effects. 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 growth in North Carolina establishments catches up to and eventually surpasses the same firm’s establishments in other states. As before, we shift down the line for establishments in other states so they have the same mean the year prior to the reform (dashed red line), which makes the post-reform divergence in trends easier to see. Panel (b) provides the corresponding event-study plot confirming this basic pattern. There is no statistical evidence of differential pre-trends, but significant effects in each of the post-reform periods. By the second full year post reform, intra-firm employment growth is 3.6% higher in North Carolina establishments. As shown in Table 2, the average effect in the post period is a 2.4% increase.

In panel (c) of Figure 3, we conduct a similar dynamic event-study analysis for the moderate reform states. Employment 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 (1.9% versus 3.6% two years after the reform, respectively). The event-study coefficients and average effect sizes for the North Carolina and moderate reform states are provided in columns (1) and (2) of Table 2, respectively.²² Moreover, individual employment graphs for each of the 6 moderate reform states are provided in Appendix Figure A2.

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 this estimated effect may have been due to the 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 growth, we use estimates from Giroud and Rauh (2019), who estimate the effects of tax

²²These estimates use a balanced panel of establishments. If we instead include all establishments, the estimates are similar: 2.4% for the North Carolina analysis and 1.4% for the moderate state analysis.

cuts on employment growth. They estimate that the corporate and personal income tax elasticities are -0.4 and 0, respectively, which implies that 0.76% of the estimated 2.4% increase is attributable to the tax reform.²³ From this we conclude that the North Carolina UI reform is responsible for a 1.68% increase in employment growth, which is slightly higher than the 1.54% estimate for moderate reform states.

These combined results 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. To assess the robustness of these results, we deploy data from the CPS to estimate the effect of the UI reforms on employment probabilities. We note that the identification strategy is less convincing as our controls are other Southern and Midwestern states which did not undergo a permanent UI reform, rather than the same firm’s establishments in other states. With this caveat in mind, column (1) of Table 6 demonstrates that employment probabilities increase by approximately 1 percentage point (s.e. = 0.43) off of a baseline of 64.8% employment probability in UI reform states. 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. Overall, employment growth increased by just over 1.5%, which is qualitatively consistent with our headline estimates.²⁴

4.4 Starting Salaries for Workers at Multi-State Firms

We now analyze earnings patterns in treatment versus control states before versus after the reforms. We focus on newly hired employees, since the UI cuts reduced the outside option for unemployed individuals and since new hires entering the labor force or switching jobs

²³This corporate tax elasticity is somewhat larger compared to much of the literature; for example, Gruber (1997) and Anderson and Meyer (1997) each document no employment effects with full incidence on wages. Since UI taxes on firms are experience rated, it is also possible that the reform lowered UI taxes imposed on firms. However, as reported by the Department of Labor, the change in the average UI tax rate was not statistically different in North Carolina versus control states after the reform (a similar result holds for moderate reform states).

²⁴These findings are consistent with Mitman et al. (2022), 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.

will have to compete with those exiting unemployment as the groups are likely to be close substitutes in hiring.

Our main 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 524,000 of these workers (and just over 1 million workers in the moderate reform state sample). We cannot distinguish between new hires exiting unemployment versus those entering the labor force or switching jobs in the Glassdoor data. We calculate that the previously unemployed account for roughly 35% of all new hires based on other datasets (see footnote 6).

Figure 4 panel (a) provides a picture of how the starting salaries of new hires changes in North Carolina establishments compared to those in other states, but within the same firm, following the reform, with panel (b) providing the accompanying event study. There is little evidence of differential pre-trends prior to the reform. In contrast, by the first full year after the reform, we see a stark drop in the relative starting salaries of new hires in North Carolina. Table 3 reports that there was an average 7.2% decline in starting salaries in the post period relative to other states.

In panel (c), we plot the dynamic event study estimates for the moderate reform states. While the pre-reform event-study coefficients are flat and not different from zero, there is a decrease in starting salaries at establishments located in moderate reform states, an effect which grows over time. As Table 3 documents, there was an average 1.8% decline in starting salaries in these moderate reform states relative to controls. Individual graphs for each of the moderate reform states appear in Appendix Figure A3.

One advantage of the Glassdoor data is that it contains wage information for a large number of workers. Although the sample size is considerably smaller, we replicate the same study design using merged CPS Outgoing Rotation Group and Job Tenure and Occupational Mobility Supplement data. For this analysis, we combine North Carolina with the moderate reform states to improve precision. While more noisily estimated, 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 6 shows, the combined effect of the reform over the entire post-reform period is a 6% reduction in wages.

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. What's more, we note that the overall estimated effect is 7.2%, and so 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 5% of the reduction in starting salaries. And as with our employment analysis, the estimated effects for moderate reform states are untainted by any concomitant tax reforms.

Determining why the UI reductions decrease starting wages is important. If the UI cuts draw less productive workers into the labor market, this has different welfare implications than if the reform induces workers to accept worse matches or compels them to accept lower wages due to reduced bargaining power. We test for negative compositional effects in column (1) of Table 4. We first predict whether a worker will earn high wages based on their observable characteristics (educational attainment, head of household status, sex, age, and age squared). We use this prediction as the outcome variable in a dynamic difference-in-difference regression and find no evidence that individuals are positively selected after the reform.

This suggests the salary effects are due to a decline in match quality or bargaining power. The decline in match quality could be driven by unemployed workers taking jobs at firms and in occupations for which they are less-well suited. Likewise, the decline in

bargaining power could be explained by the need to take a job before being able to generate counteroffers. We unpack the relative contribution of each channel in columns (2)-(5) of Table 4. We first repeat the baseline regression specification using the same multi-state firm sample, but removing the 4,800 firm fixed effects. The next column reinserts the firm fixed effects in addition to 2,000 detailed occupation fixed effects. Column (2) estimates a wage effect of -9.6%, which compares to an estimate in column (3) of -5.9% without the extra fixed effects. In columns (4) and (5), we perform an analogous exercise among the moderate reform state sample, and find that the inclusion of firm and occupational fixed effects reduces the wage effect from -2.4% to -1.6%. These decompositions suggests that firm and occupational downgrading explain roughly 33-40% of the reforms' effects on wages, with the remaining portion likely arising from a combination of unobserved degradations in match quality and depressed worker bargaining power.

4.5 Posted Wages for Jobs within Multi-State Firms

To more definitively rule out the prospect that the estimated wage effects are driven by compositional effects and to parse out the role played by decreases in match quality relative to bargaining power, we turn to Burning Glass Technologies data. The use of BGT posted wages eliminates the impact of worker choices on our estimates since we see the firm's offered wages for the same job over time.²⁵ To appreciate the power of this data, imagine a world in which in response to a UI cut, the wage for any particular job remains unchanged, yet starting salaries fall because workers downgrade to a lower-paying occupation or lower-paying firm. Comparing posted wages for identical jobs within the same company but across establishments in treated versus untreated states would correctly estimate no change in wages.

However, as can be seen in panels (a) and (b) of Figure 5, there is a relative decrease of 5.5% in the posted salaries for the same job within the same firm if the posting establishment

²⁵Following Hazell and Taska (2020), we define a job as a standard occupation code-pay frequency-salary type. Examples of salary type are base pay versus commission.

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 (2020) and (Fallick et al. 2020). Moreover, we find a smaller and slightly delayed decrease in the posted wages for establishments residing in the 6 moderate reform states in panel (c).

Table 5 provides an estimate for the average treatment effect on the treated for each of these sets of reforms. Specifically, the North Carolina reform reduced posted wages by 5.5% while posted wages fell by 1.4% in moderate reform states. These estimates can be compared to columns (3) and (5) of Table 4, the Glassdoor estimates accounting for firm and occupational fixed effects. The estimates are nearly identical (-5.9% and -1.6%), which suggests that unobserved occupational and firm downgrading or worker composition effects explain little of the wage decline. Instead, we argue that the reduction in UI benefits produced relatively large declines in worker bargaining power, which materialized as substantially lower wage offerings in treated establishments.

Following the same logic used at the end of Section 4.4, even in the most conservative case where a 2 percentage point reduction in personal income taxes was fully passed through to North Carolina workers and the 1.9 percentage point reduction in corporate income taxes led to no wage increase, the NC reform still generates a 3.5 percentage point reduction in starting salaries.

4.6 Unemployment Duration

As a final exercise, we estimate the effect of the UI reforms on unemployment duration using CPS data. For precision, we again combine North Carolina with the moderate reform states. In Table 6 column (3), we report the dynamic event-study coefficients. Prior to the reforms, there is no differential effect. In the first two years after the reform, there is evidence of

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 in each year of the post-reform period. This translates to an 8% drop in unemployment duration relative to control states.²⁶ This suggests that when UI generosity falls, this incentivizes individuals to accept jobs more quickly.

5 Conclusion

We study the effect of large reductions in the value of UI benefits on employment, starting salaries, and posted salaries. Using a multi-state firm identification strategy, we find that establishments in North Carolina experience 2.4% faster employment growth 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 7.2% decline in starting salaries. Similar results, albeit smaller in magnitude, are found for the 6 states which enacted less draconian UI benefit cuts. We also leverage data on job ads, and similarly find that wage postings for the same job in the same firm fall by 5.5% in North Carolina and by 1.4% in moderate reform states. Notably, these latter estimates remove all match quality and compositional effects, implying a decline in worker bargaining power when the outside option of remaining unemployed falls. The UI cuts represent negative shocks to workers' outside options and hence shift their labor supply curve to the right; we estimate a labor demand elasticity of -1.0.

Whether unemployment insurance cuts are desirable from a policy perspective depends on the benefits versus costs. On the positive side, these UI reforms stimulated employment growth and lowered benefit payouts. But counterbalancing this was a reduction in the wages of new hires, due to a combination of lower match quality and reduced bargaining power.

²⁶This 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.

This tradeoff adds a layer of complexity to debates on the optimal level of unemployment insurance benefits.

References

- Acemoglu, Daron and Robert Shimer**, “Efficient Unemployment Insurance,” *Journal of Political Economy*, 1999, 107 (5), 893–928.
- , **David H Autor**, and **David Lyle**, “Women, War, and Wages: The Effect of Female Labor Supply on the Wage Structure at Midcentury,” *Journal of Political Economy*, 2004, 112 (3), 497–551.
- Addison, John T, Lutz Bellmann, Thorsten Schank, and Paulino Teixeira**, “The Demand for Labor: An Analysis using Matched Employer–Employee data from the German LIAB. Will the High Unskilled Worker Own-wage Elasticity Please Stand Up?,” *Journal of Labor Research*, 2008, 29 (2), 114–137.
- Anderson, Patricia M and Bruce D Meyer**, “The Effects of Firm Specific Taxes and Government Mandates with an Application to the US Unemployment Insurance Program,” *Journal of Public Economics*, 1997, 65 (2), 119–145.
- Baker, Scott R and Andrey Fradkin**, “The Impact of Unemployment Insurance on Job Search: Evidence from Google Search Data,” *Review of Economics and Statistics*, 2017, 99 (5), 756–768.
- Barbanchon, Thomas Le, Roland Rathelot, and Alexandra Roulet**, “Unemployment Insurance and Reservation Wages: Evidence from Administrative Data,” *Journal of Public Economics*, 2019, 171, 1–17.
- Barron, John M and Wesley Mellow**, “Search Effort in the Labor Market,” *Journal of Human Resources*, 1979, pp. 389–404.
- Boone, Christopher, Arindrajit Dube, Lucas Goodman, and Ethan Kaplan**, “Unemployment Insurance Generosity and Aggregate Employment,” *American Economic Journal: Economic Policy*, 2021, 13 (2), 58–99.
- Borusyak, Kirill, Xavier Jaravel, and Jann Spiess**, “Revisiting Event Study Designs: Robust and Efficient Estimation,” *arXiv preprint arXiv:2108.12419*, 2021.
- Card, David and Phillip B Levine**, “Extended Benefits and the Duration of UI Spells: Evidence from the New Jersey Extended Benefit Program,” *Journal of Public Economics*, 2000, 78 (1-2), 107–138.
- , **Andrew Johnston, Pauline Leung, Alexandre Mas, and Zhuan Pei**, “The Effect of Unemployment Benefits on the Duration of Unemployment Insurance Receipt: New Evidence from a Regression Kink Design in Missouri, 2003-2013,” *American Economic Review*, 2015, 105 (5), 126–30.
- , **Raj Chetty, and Andrea Weber**, “Cash-on-hand and Competing Models of Intertemporal Behavior: New Evidence from the Labor Market,” *The Quarterly Journal of Economics*, 2007, 122 (4), 1511–1560.
- Carling, Kenneth, Per-Anders Edin, Anders Harkman, and Bertil Holmlund**, “Unemployment Duration, Unemployment Benefits, and Labor Market Programs in Sweden,” *Journal of Public Economics*, 1996, 59 (3), 313–334.
- Centeno, Mário and Álvaro A Novo**, “Reemployment Wages and UI Liquidity Effect: A Regression Discontinuity Approach,” *Portuguese Economic Journal*, 2009, 8 (1), 45–52.
- Chaisemartin, Clément De and Xavier d’Haultfoeuille**, “Two-way Fixed Effects Estimators with Heterogeneous Treatment Effects,” *American Economic Review*, 2020, 110

- (9), 2964–96.
- Chetty, Raj**, “Moral Hazard versus Liquidity and Optimal Unemployment Insurance,” *Journal of Political Economy*, 2008, 116 (2), 173–234.
- Chodorow-Reich, Gabriel, John Coglianesi, and Loukas Karabarbounis**, “The Macro Effects of Unemployment Benefit Extensions: A Measurement Error Approach,” *The Quarterly Journal of Economics*, 2019, 134 (1), 227–279.
- Dahl, Gordon B**, “Latent and Behavioral Responses to Extensions in Unemployment Insurance Benefits,” *unpublished paper, UC San Diego*, 2011.
- Degen, Kathrin and Rafael Lalive**, “How do Reductions in Potential Benefit Duration Affect Medium-run Earnings and Employment?,” *Manuscript, University of Lausanne*, 2013.
- DellaVigna, Stefano and M Daniele Paserman**, “Job Search and Impatience,” *Journal of Labor Economics*, 2005, 23 (3), 527–588.
- Dieterle, Steven, Otávio Bartalotti, and Quentin Brummet**, “Revisiting the Effects of Unemployment Insurance Extensions on Unemployment: A Measurement-Error-Corrected Regression Discontinuity Approach,” *American Economic Journal: Economic Policy*, 2020, 12 (2), 84–114.
- Fallick, Bruce, Daniel Villar, and William Wascher**, “Downward Nominal Wage Rigidity in the United States during and after the Great Recession,” *Federal Reserve Bank of Cleveland Working Paper*, 2020, 16-02R.
- Farber, Henry S and Robert G Valletta**, “Do Extended Unemployment Benefits Lengthen Unemployment Spells? Evidence from Recent Cycles in the US Labor Market,” *Journal of Human Resources*, 2015, 50 (4), 873–909.
- , **Jesse Rothstein, and Robert G Valletta**, “The Effect of Extended Unemployment Insurance Benefits: Evidence from the 2012-2013 Phase-out,” *American Economic Review*, 2015, 105 (5), 171–76.
- Ganong, Peter and Pascal Noel**, “Consumer Spending during Unemployment: Positive and Normative Implications,” *American Economic Review*, 2019, 109 (7), 2383–2424.
- , **Fiona Greig, Max Liebeskind, Pascal Noel, Daniel Sullivan, and Joseph Vavra**, “Spending and Job Search Impacts of Expanded Unemployment Benefits: Evidence from Administrative Micro Data,” *University of Chicago, Becker Friedman Institute for Economics Working Paper*, 2021, (2021-19).
- GAO**, “Unemployment Insurance: States’ Reductions in Maximum Benefit Durations have Implications for Federal Costs,” Technical Report, Report 15-281 2015.
- Giroud, Xavier and Joshua Rauh**, “State Taxation and the Reallocation of Business Activity: Evidence from Establishment-level Data,” *Journal of Political Economy*, 2019, 127 (3), 1262–1316.
- Gruber, Jonathan**, “The Incidence of Payroll Taxation: Evidence from Chile,” *Journal of Labor Economics*, 1997, 15 (S3), S72–S101.
- , *Public Finance and Public Policy, 2nd. Ed*, New York: Worth, 2007.
- Haefke, Christian, Marcus Sonntag, and Thijs Van Rens**, “Wage Rigidity and Job Creation,” *Journal of Monetary Economics*, 2013, 60 (8), 887–899.
- Hagedorn, Marcus, Fatih Karahan, Iourii Manovskii, and Kurt Mitman**, “Case

- Study of Unemployment Insurance Reform in North Carolina,” *Working Paper*, 2014.
- , **Iouri Manovskii**, and **Kurt Mitman**, “The Impact of Unemployment Benefit Extensions on Employment: The 2014 Employment Miracle?,” Technical Report, National Bureau of Economic Research 2015.
- Hamermesh, Daniel S**, *Labor Demand*, Princeton University Press, 1996.
- Hazell, Jonathon and Bledi Taska**, “Downward Rigidity in the Wage for New Hires,” Available at SSRN 3728939, 2020.
- , **Christina Patterson, Heather Sarsons, and Bledi Taska**, “National Wage Setting,” Technical Report, Working Paper 2021.
- Jäger, Simon, Benjamin Schoefer, Samuel Young, and Josef Zweimüller**, “Wages and the Value of Nonemployment,” *The Quarterly Journal of Economics*, 2020, 135 (4), 1905–1963.
- Johnston, Andrew C and Alexandre Mas**, “Potential Unemployment Insurance Duration and Labor Supply: The Individual and Market-level Response to a Benefit Cut,” *Journal of Political Economy*, 2018, 126 (6), 2480–2522.
- Kahn, Shulamit**, “Evidence of Nominal Wage Stickiness from Microdata,” *The American Economic Review*, 1997, 87 (5), 993–1008.
- Katz, Lawrence F and Bruce D Meyer**, “The Impact of the Potential Duration of Unemployment Benefits on the Duration of Unemployment,” *Journal of Public Economics*, 1990, 41 (1), 45–72.
- Kekre, Rohan**, “Unemployment insurance in macroeconomic stabilization,” *University of Chicago, Becker Friedman Institute for Economics Working Paper*, 2021, (2021-28).
- Kline, Patrick M, Evan K Rose, and Christopher R Walters**, “Systemic Discrimination among Large US Employers,” Technical Report, National Bureau of Economic Research 2021.
- Kroft, Kory and Matthew J Notowidigdo**, “Should Unemployment Insurance vary with the Unemployment Rate? Theory and Evidence,” *The Review of Economic Studies*, 2016, 83 (3), 1092–1124.
- Krueger, Alan and Andreas Mueller**, “Job Search and Unemployment Insurance: New Evidence from Time Use Data,” *Journal of Public Economics*, 2010, 94 (3-4), 298–307.
- and – , “The Lot of the Unemployed: A Time Use Perspective,” *Journal of the European Economic Association*, 2012, 10 (4), 765–794.
- Kudlyak, Marianna and Fabian Lange**, “Measuring Heterogeneity in Job Finding Rates among the Non-Employed Using Labor Force Status Histories,” Technical Report, Federal Reserve Bank of San Francisco Working Paper 2018.
- Lalive, Rafael**, “Unemployment Benefits, Unemployment Duration, and Post-unemployment Jobs: A Regression Discontinuity Approach,” *American Economic Review*, 2007, 97 (2), 108–112.
- , **Camille Landais, and Josef Zweimüller**, “Market Externalities of Large Unemployment Insurance Extension Programs,” *American Economic Review*, 2015, 105 (12), 3564–96.
- , **Jan Van Ours, and Josef Zweimüller**, “How Changes in Financial Incentives affect the Duration of Unemployment,” *The Review of Economic Studies*, 2006, 73 (4), 1009–

1038.

- Landais, Camille**, “Assessing the Welfare Effects of Unemployment Benefits using the Regression Kink Design,” *American Economic Journal: Economic Policy*, 2015, 7 (4), 243–78.
- , **Pascal Michaillat**, and **Emmanuel Saez**, “A Macroeconomic Approach to Optimal Unemployment Insurance: Applications,” *American Economic Journal: Economic Policy*, 2018, 10 (2), 182–216.
- Lise, Jeremy and Jean-Marc Robin**, “The Macrodynamics of Sorting between Workers and Firms,” *American Economic Review*, 2017, 107 (4), 1104–35.
- Marinescu, Ioana**, “The General Equilibrium Impacts of Unemployment Insurance: Evidence from a Large Online Job Board,” *Journal of Public Economics*, 2017, 150, 14–29.
- , **Daphne Skandalis**, and **Daniel Zhao**, “The Impact of the Federal Pandemic Unemployment Compensation on Job Search and Vacancy Creation,” *Journal of Public Economics*, 2021, 200, 104471.
- Meyer, Bruce D and Wallace KC Mok**, “Quasi-experimental Evidence on the Effects of Unemployment Insurance from New York State,” Technical Report, National Bureau of Economic Research 2007.
- Michaillat, Pascal**, “Do Matching Frictions Explain Unemployment? Not in Bad Times,” *American Economic Review*, 2012, 102 (4), 1721–50.
- Mitman, Kurt and Stanislav Rabinovich**, “Optimal Unemployment Insurance in an Equilibrium Business-Cycle Model,” *Journal of Monetary Economics*, 2015, 71, 99–118.
- , **Fatih Karahan**, and **Brendan Moore**, “Micro and Macro Effects of UI Policies: Evidence from Missouri,” 2022.
- Moffitt, Robert**, “Unemployment insurance and the distribution of unemployment spells,” *Journal of Econometrics*, 1985, 28 (1), 85–101.
- Mortensen, Dale T**, “Job Search, the Duration of Unemployment, and the Phillips Curve,” *The American Economic Review*, 1970, 60 (5), 847–862.
- and **Christopher A Pissarides**, “Job Creation and Job Destruction in the Theory of Unemployment,” *The Review of Economic Studies*, 1994, 61 (3), 397–415.
- Nekoei, Arash and Andrea Weber**, “Does Extending Unemployment Benefits Improve Job Quality?,” *American Economic Review*, 2017, 107 (2), 527–61.
- Ours, Jan C Van and Milan Vodopivec**, “How Shortening the Potential Duration of Unemployment Benefits affects the Duration of Unemployment: Evidence from a Natural Experiment,” *Journal of Labor Economics*, 2006, 24 (2), 351–378.
- and – , “Does Reducing Unemployment Insurance Generosity Reduce Job Match Quality?,” *Journal of Public Economics*, 2008, 92 (3-4), 684–695.
- Pissarides, Christopher A**, “The Unemployment Volatility Puzzle: Is Wage Stickiness the Answer?,” *Econometrica*, 2009, 77 (5), 1339–1369.
- Røed, Knut and Tao Zhang**, “Does Unemployment Compensation affect Unemployment Duration?,” *The Economic Journal*, 2003, 113 (484), 190–206.
- Rothstein, Jesse**, “Unemployment Insurance and Job Search in the Great Recession,” *Brookings Papers on Economic Activity*, 2011, 2011 (2), 143–213.
- Schaefer, Luke and Michael Evangelist**, “The impact of the 2011 Changes to Michigan’s

Unemployment Insurance Program on Unemployed Workers and their Families,” Technical Report, Michigan Unemployment Insurance Project 2014.

Schmieder, Johannes F and Till Von Wachter, “The effects of unemployment insurance benefits: New evidence and interpretation,” *Annual Review of Economics*, 2016, 8, 547–581.

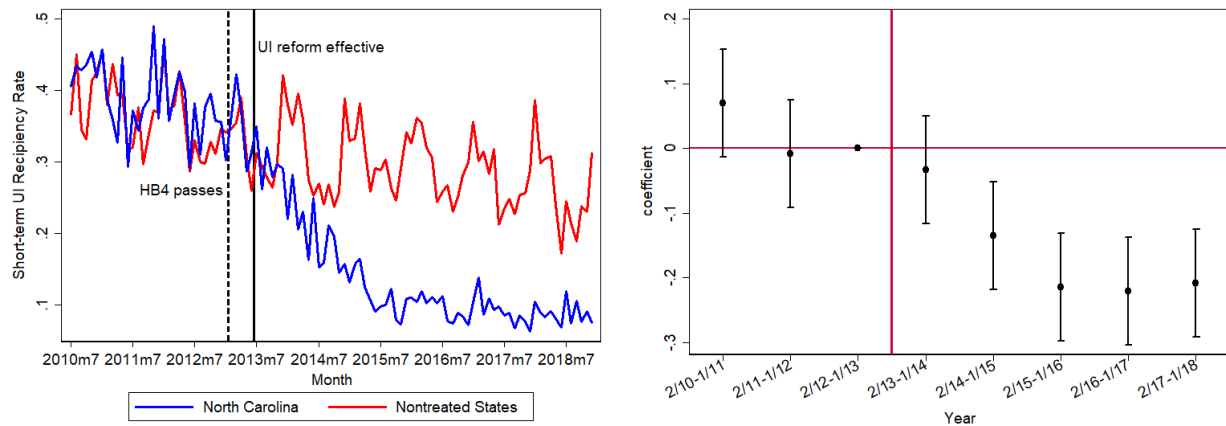
– , **Till von Wachter, and Stefan Bender**, “The Effects of Unemployment Insurance on Labor Supply and Search Outcomes: Regression Discontinuity Estimates from Germany,” Technical Report, IAB-Discussion Paper 2010.

– , **Till Von Wachter, and Stefan Bender**, “The Long-term Effects of UI Extensions on Employment,” *American Economic Review*, 2012, 102 (3), 514–19.

Shavell, Steven and Laurence Weiss, “The Optimal Payment of Unemployment Insurance Benefits over time,” *Journal of political Economy*, 1979, 87 (6), 1347–1362.

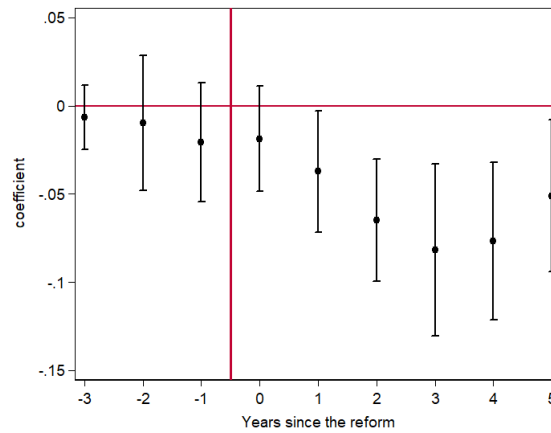
Solon, Gary, “Work Incentive Effects of Taxing Unemployment Benefits,” *Econometrica*, 1985, pp. 295–306.

6 Figures and Tables



(a) UI Reciprocity Rate, North Carolina

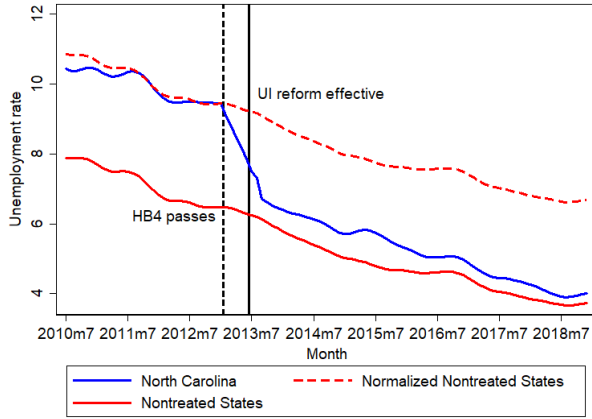
(b) UI Reciprocity Rate, North Carolina



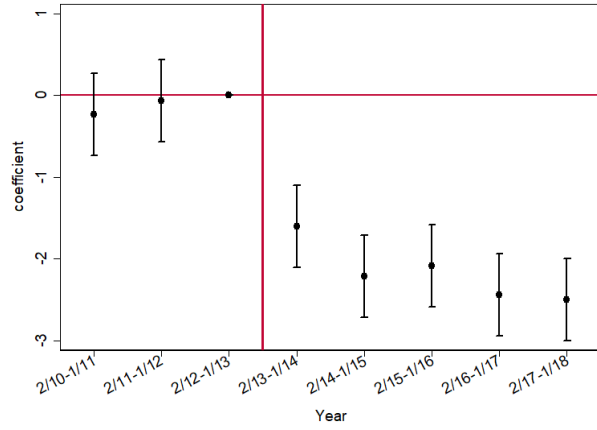
(c) UI Reciprocity Rate, Moderate Reform States

Figure 1: Short-term UI Reciprocity Rate

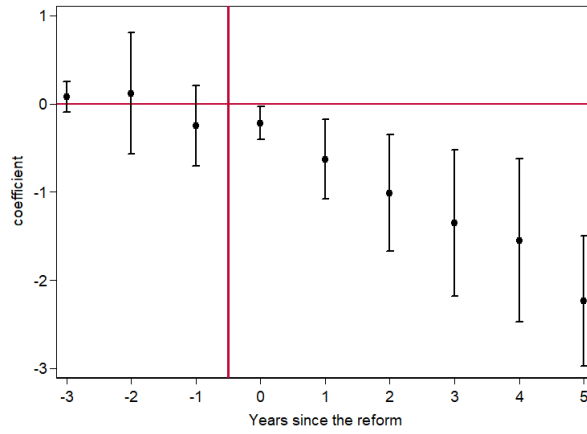
Panel 1a shows the monthly average of total weeks compensated under regular state UI programs divided by the monthly number of short-term unemployed workers, for North Carolina as compared to nontreated Southern and Midwestern states. “HB4 passes” indicates when the NC state legislature signed the UI bill into law, and “UI reform effective” indicates the date after which all newly filed UI claims were subject to the new restrictions. Panel 1b is the corresponding event-study graph (with 95% confidence intervals). Panel 1c is a dynamic event-study plot which shows the difference in UI reciprocity rates (and 95% confidence intervals) for moderate reform states (FL, GA, KS, MI, MO, and SC) relative to other Southern and Midwestern states. Coefficients and standard errors are computed using the dynamic difference-in-difference procedure outlined in De Chaisemartin and d’Haultfoeuille (2020).



(a) Unemployment Rate, North Carolina



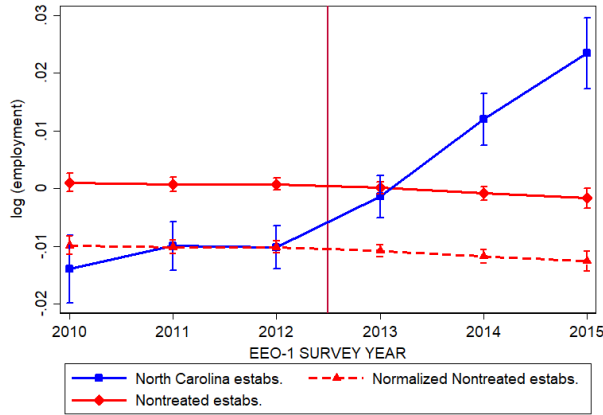
(b) Unemployment Rate, North Carolina



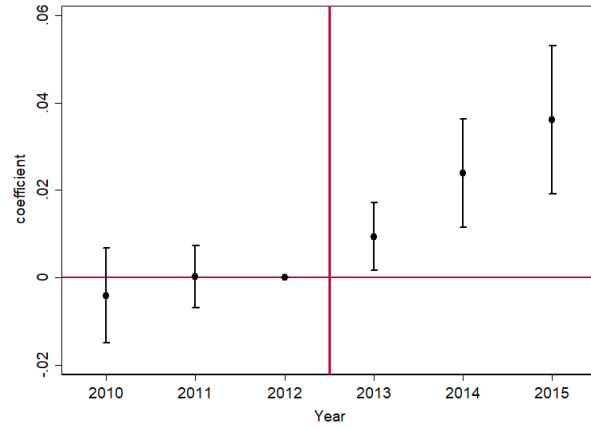
(c) Unemployment Rate, Moderate Reform States

Figure 2: Unemployment Rates

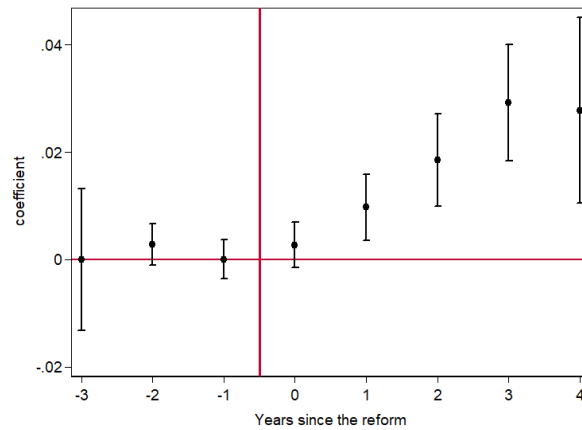
Panel 2a shows the monthly seasonally adjusted unemployment rates using LAUS data, for North Carolina as compared to all Nontreated states. Panel 2b is the corresponding event-study graph (with 95% confidence intervals). Panel 2c is a dynamic event-study plot which shows the difference in unemployment rates (and 95% confidence intervals) for moderate reform states (FL, GA, KS, MI, MO, and SC) relative to other Southern and Midwestern states. Coefficients and standard errors are computed using the dynamic difference-in-difference procedure outlined in De Chaisemartin and d'Haultfoeuille (2020).



(a) Log Employment, North Carolina



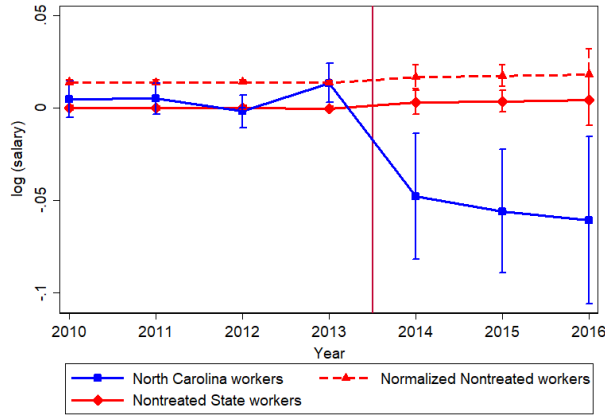
(b) Log Employment, North Carolina



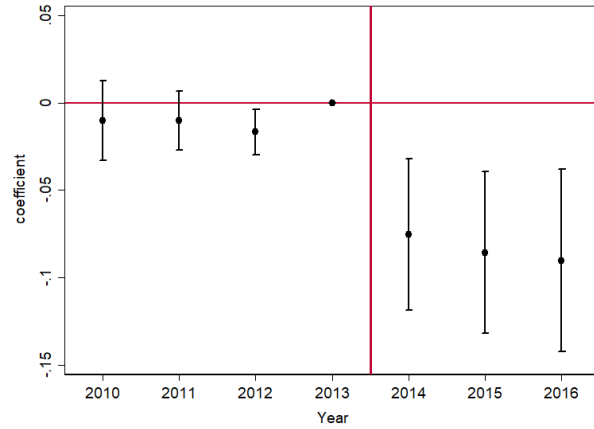
(c) Log Employment, Moderate Reform States

Figure 3: Establishment Employment in Multi-state Firms

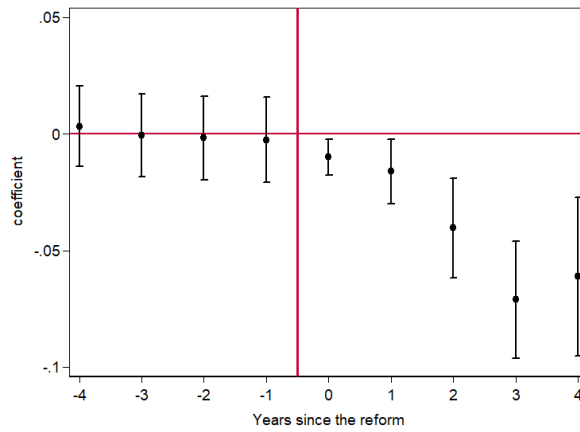
Panel 3a shows the change in residualized employment growth using EEOC data (and 95% confidence intervals) for North Carolina-based establishments versus other Nontreated state-based establishments within the same multi-state firm over time. Netted out are firm, year, industry, and state fixed effects. Panel 3b is an event-study plot which shows the difference in log employment (and 95% confidence intervals) between NC-based and other Nontreated state-based establishments within a given multi-state firm. Panel 3c is a dynamic event-study plot which shows the difference in log employment (and 95% confidence intervals) for establishments based in moderate reform states (FL, GA, KS, MI, MO, and SC) relative to non-reform state-based establishments within a given multi-state firm. Coefficients and standard errors are computed using the dynamic difference-in-difference procedure outlined in De Chaisemartin and d'Haultfoeuille (2020).



(a) New Hire Salaries, North Carolina



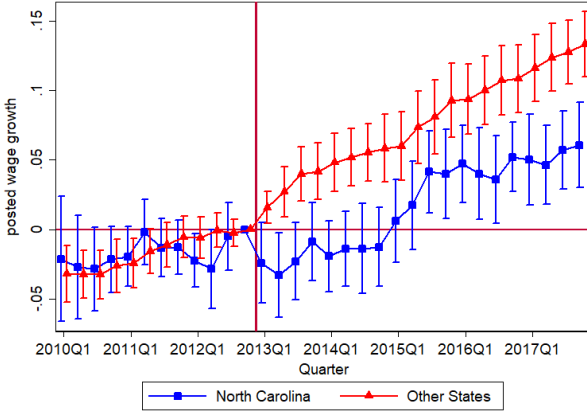
(b) New Hire Salaries, North Carolina



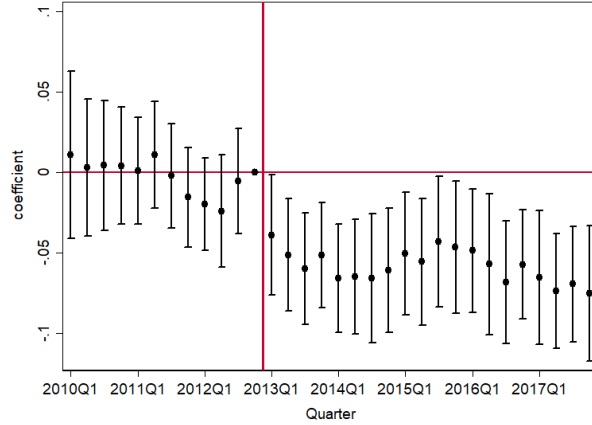
(c) New Hire Salaries, Moderate Reform States

Figure 4: Salaries of New Hires in Multi-state Firms

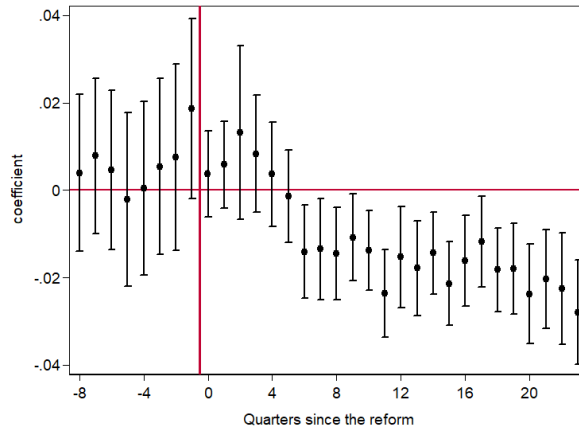
Panel 4a shows residualized self-reported annual salaries from Glassdoor for newly hired workers based in North Carolina versus those working in other Nontreated states within the same multi-state firm. Netted out are firm fixed effects, state and metro area fixed effects, gender, and education. Panel 4b is an event-study plot which shows the difference in log annual salaries (and 95% confidence intervals) for new hires in North Carolina establishments relative to those in establishments in other Nontreated states within the same firm. Panel 4c is a dynamic event-study plot which shows the difference in log annual salaries (and 95% confidence intervals) for new hires in moderate reform state establishments relative to those in Nontreated states within the same multi-state firm, using the Borusyak et al. (2021) method.



(a) Posted Salaries, North Carolina



(b) Posted Salaries, North Carolina



(c) Posted Salaries, Moderate Reform States

Figure 5: Posted Salaries in Multi-state Firms

Panel 5a shows posted salaries from Burning Glass for workers based in North Carolina versus those working in other Nontreated states within the same multi-state firm. Netted out are state, quarter, and employer-job fixed effects. Panel 5b is an event-study plot which shows the difference in posted salaries (and 95% confidence intervals) for new hires in North Carolina establishments relative to those in establishments in other Nontreated states within the same firm. Panel 5c is a dynamic event-study plot which shows the difference in posted salaries (and 95% confidence intervals) for new hires in moderate reform state establishments relative to those in Nontreated states within the same multi-state firm, using the Borusyak et al. (2021) method.

Table 1: Permanent Cuts to Regular State UI Programs

State	Date	Max. duration	Max. weekly benefit	Max. total benefit
North Carolina	July 2013	26→20 weeks ¹	\$535→\$350	\$13,910→\$7,000
Florida	January 2012	26→20 weeks ²	No change	\$7,150→\$5,500
Georgia	July 2012	26→20 weeks ³	No change	\$8,580→\$6,600
South Carolina	June 2011	26→20 weeks	No change	\$8,476→\$6,520
Michigan	January 2012	26→20 weeks	No change	\$9,412→\$7,240
Missouri	April 2011	26→20 wks.	No change	\$8,320→\$6,400
Kansas	January 2014	26→20 weeks ⁴	No change	\$12,194→\$9,380

All states also implemented eligibility restrictions, such as disqualifying individuals who had lost a job for “good cause” reasons (e.g., providing family caregiving or following a spouse forced to relocate for work-related reasons), and imposing additional work search requirements. Arkansas also permanently reduced its maximum duration for regular UI benefits by 1 week in April of 2011.

¹ NC’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. Following the reform, maximum duration drops to 12 weeks when the state UR rate is less than 5.5%, and increases by 1 week for each 0.5% increase until reaching a maximum of 20 weeks once the UR reaches 9%.

² Maximum duration drops to 12 weeks when the state UR rate is less than 5%, and increased by 1 week for each 0.5% increase until reaching a maximum of 23 weeks at a 10.5% UR.

³ Maximum duration drops to 14 weeks when the state UR rate is less than 6.5%, and increased by 1 week for each 0.5% increase until reaching a maximum of 20 weeks at a 9% UR.

⁴ Maximum duration drops to 16 weeks when the state UR rate is less than 4.5%, increases to 20 weeks when the UR is between 4.5% and 6%, and remains at 26 weeks if the UR exceeds 6%.

Table 2: Effect of the UI Reforms on Establishment Employment for Multi-state Firms

	North Carolina Reform	Moderate UI Reforms
<i>dep var = log(employment)</i>	(1)	(2)
treated $\times \mathbb{1}_{t=-3}$	-0.0041 (0.0055)	0.0000 (0.0067)
treated $\times \mathbb{1}_{t=-2}$	0.0002 (0.0036)	0.0028 (0.0020)
treated $\times \mathbb{1}_{t=-1}$		0.0000 (0.0018)
treated $\times \mathbb{1}_{t=0}$	0.0094** (0.0039)	0.0026 (0.0022)
treated $\times \mathbb{1}_{t=1}$	0.0238*** (0.0063)	0.0097*** (0.0031)
treated $\times \mathbb{1}_{t=2}$	0.0361*** (0.0086)	0.0185*** (0.0044)
treated $\times \mathbb{1}_{t=3}$		0.0292*** (0.0055)
treated $\times \mathbb{1}_{t=4}$		0.0278*** (0.0088)
ATT	0.0244*** (0.0066)	0.0154*** (0.0034)
mean(EEO-1 emp.) †	84.36	101.40
N	948,625	1,445,917
Firms	3,519	7,820
R ²	0.970	0.966

Estimates are of employment growth in reform-based establishments versus non-reform-based establishments within the same firm, using EEOC employment data. Specifications control for state, year, and firm fixed effects. Dynamic DiD estimates in columns (3)-(4) are obtained using the De Chaisemartin and d'Haultfoeuille (2020) method. The sample period begins three years prior to a state's reform (2008, 2009, 2010, or 2011) and ends in 2015. Standard errors are clustered at the firm level.

† The mean corresponds to the average number of employees at not-yet-treated establishments.

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

Table 3: Effect of the UI Reforms on Glassdoor Starting Salaries for Multi-state Firms

	North Carolina Reform	Moderate UI Reforms
<i>dep var = log(starting salary)</i>	(1)	(2)
treated $\times \mathbb{1}_{t=-4}$	-0.0104 (0.0116)	0.0032 (0.0088)
treated $\times \mathbb{1}_{t=-3}$	-0.0102 (0.0085)	-0.0006 (0.0091)
treated $\times \mathbb{1}_{t=-2}$	-0.0167** (0.0066)	-0.0018 (0.0091)
treated $\times \mathbb{1}_{t=-1}$		-0.0026 (0.0093)
treated $\times \mathbb{1}_{t=0}$	-0.0753*** (0.0220)	-0.0100** (0.0039)
treated $\times \mathbb{1}_{t=1}$	-0.0856*** (0.0236)	-0.0161** (0.0070)
treated $\times \mathbb{1}_{t=2}$	-0.0902*** (0.0266)	-0.0404*** (0.0108)
treated $\times \mathbb{1}_{t=3}$		-0.0711*** (0.0128)
treated $\times \mathbb{1}_{t=4}$		-0.0613*** (0.0174)
ATT	-0.0719*** (0.0177)	-0.0176*** (0.0046)
mean(starting salary)	\$67,634	\$74,438
N	524,610	1,073,262
Firms	4,815	14,308
R ²	0.585	0.573

Estimates are of the log starting salaries of workers in reform state-based establishments versus non-reform state-based establishments within the same firm, using self-reported annual salary data from Glassdoor. Netted out are state, metro area, year, and firm FEs, as well as sex and education controls. Dynamic DiD estimates in column (2) are obtained using the Borusyak et al. (2021) method. The sample period begins four years prior to a state's reform (2008, 2009, 2010, or 2011) through 2016. Standard errors are clustered by firm.

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

Table 4: Compositional, Match Quality, and Bargaining Power Effects on New Hires

	All UI Reforms	North Carolina Reform		Moderate UI Reforms	
<i>dependent variable data</i>	$\mathbb{1}(\text{predicted high wage})$ CPS (1)	(2)	$\log(\text{starting salary})$ Glassdoor (3)	(4)	(5)
ATT	-0.005 (0.0262)	-0.0959*** (0.0229)	-0.0590*** (0.0149)	-0.0242*** (0.0068)	-0.0163*** (0.0045)
Occupational and Firm FEs			X		X
mean(hourly wage)	\$17.1	\$67,634	\$67,634	\$74,438	\$74,479
N	24,858	525,496	524,404	1,073,262	1,069,383
R ²	0.018	0.331	0.695	0.238	0.688

Estimates in column (1) use starting salaries from the Current Population Survey and Job Tenure and Occupational Mobility Supplement. Education, head of household status, sex, race, age, age squared, and marital status are used to predict pre-reform wages, and $\mathbb{1}(\text{predicted high wage})$ is an indicator variable for having a predicted wage above the median on the basis of these characteristics. The sample period is from 2009-2018 in column (1). Columns (2) and (4) replicate the multi-state firm analysis using Glassdoor data, except removing all firm fixed effects. Columns (3) and (5) control for occupational and firm fixed effects. See Table 3 for further details. Dynamic DiD estimates are obtained using the De Chaisemartin and d'Haultfoeuille (2020) method in column (1) and using the Borusyak et al. (2021) method in columns (2)-(5). Standard errors are clustered by state in column (1) and at the firm level in columns (2)-(5).

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

Table 5: Effect of the UI Reforms on Burning Glass Posted Salaries for Multi-state Firms

	North Carolina Reform	Moderate UI Reforms
<i>dep var = log(posted salary)</i>	(1)	(2)
treated $\times \mathbb{1}_{t=-3}$	-0.0177 (0.0130)	
treated $\times \mathbb{1}_{t=-2}$	-0.004 (0.0129)	0.0072 (0.0089)
treated $\times \mathbb{1}_{t=-1}$		0.0121 (0.0095)
treated $\times \mathbb{1}_{t=0}$	-0.0563*** (0.0148)	0.0070 (0.0047)
treated $\times \mathbb{1}_{t=1}$	-0.0681*** (0.0160)	-0.0075** (0.0037)
treated $\times \mathbb{1}_{t=2}$	-0.0536*** (0.0157)	-0.0154*** (0.0035)
treated $\times \mathbb{1}_{t=3}$	-0.0629*** (0.0152)	-0.0165*** (0.0039)
treated $\times \mathbb{1}_{t=4}$	-0.0745*** (0.0170)	-0.0143*** (0.0038)
treated $\times \mathbb{1}_{t=5}$		-0.0230*** (0.0045)
ATT	-0.0553*** (0.0134)	-0.0135*** (0.0031)
mean(posted salary)	\$57,040	\$59,149
N	498,839	1,377,320
Firms	5,183	19,470
R ²	0.947	0.894

Estimates use the log posted salaries in reform state-based establishments versus non-reform state-based establishments for the same employer-job, using job ads from Burning Glass Technologies. Netted out are state, quarter, and employer-job FEs. Dynamic DiD estimates in column (2) are obtained using the Borusyak et al. (2021) method. The sample period is from 2010-2017. Standard errors are clustered by firm.

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

Table 6: Effect of the Reforms on CPS Employment, Starting Salaries, Weeks Unemployed

All UI Reforms			
<i>dependent variable</i>	$\mathbb{1}\{employed\}$	log(starting salary)	weeks unemployed
	(1)	(2)	(3)
treated $\times \mathbb{1}_{t=-4}$	-0.0060 (0.0067)		1.634 (1.175)
treated $\times \mathbb{1}_{t=-3}$	-0.0059 (0.0036)	0.0083 (0.0281)	1.399 (1.787)
treated $\times \mathbb{1}_{t=-2}$	0.0032 (0.0025)	0.0225 (0.0234)	1.053 (0.734)
treated $\times \mathbb{1}_{t=-1}$	-0.0026 (0.0027)	0.0008 (0.0224)	0.689 (1.339)
treated $\times \mathbb{1}_{t=0}$	0.0018 (0.0023)	-0.0411 (0.0399)	-0.994 (1.191)
treated $\times \mathbb{1}_{t=1}$	0.0059 (0.0049)	-0.0657** (0.0331)	-1.212 (1.625)
treated $\times \mathbb{1}_{t=2}$	0.0078* (0.0047)	-0.0499 (0.0342)	-1.715 (1.125)
treated $\times \mathbb{1}_{t=3}$	0.0099* (0.0057)	-0.0506 (0.0503)	-3.549*** (1.102)
treated $\times \mathbb{1}_{t=4}$	0.0159** (0.0066)	-0.0846** (0.0413)	-4.154*** (1.173)
treated $\times \mathbb{1}_{t=5}$	0.0138** (0.0063)	-0.1007** (0.0487)	-5.589*** (1.597)
treated $\times \mathbb{1}_{t=6}$	0.0159** (0.0073)		-7.310*** (2.365)
ATT	0.0098** (0.0043)	-0.0625* (0.0327)	-2.785*** (0.983)
State and Year FEs	X	X	X
Predetermined Characteristics		X	
mean(dep var)	0.648	\$17.1	33.48
N	5,722,126	24,858	263,806
R ²	0.008	0.281	0.047

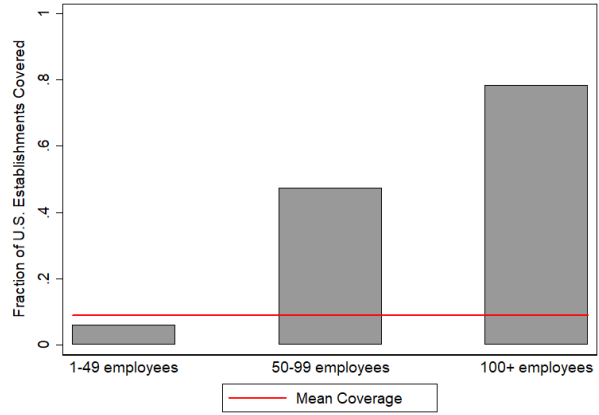
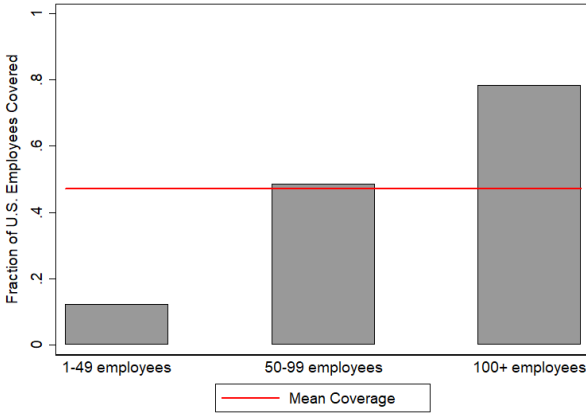
Estimates are of starting salaries from the Current Population Survey and Job Tenure and Occupational Mobility Supplement. Sample includes all individuals whose job tenure is less than 1 year. Netted out are state, metro, and year FEs, as well as sex, education, and age controls. Dynamic DiD estimates are obtained using the De Chaisemartin and d'Haultfoeuille (2020) method. The sample period is from 2009-2018. Standard errors are clustered at the state level.

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

7 Online Appendix

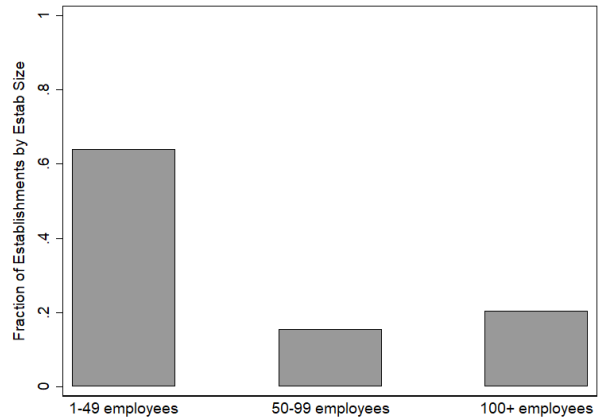
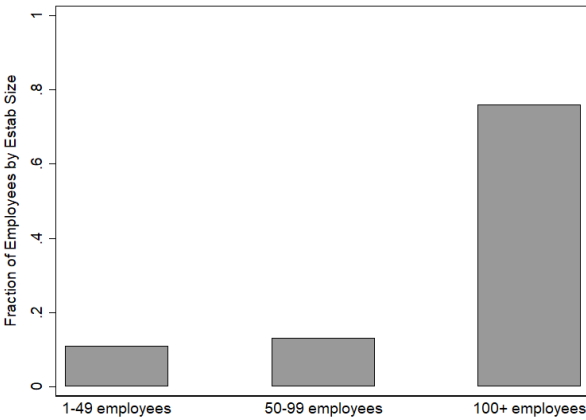
“Unemployment Insurance, Starting Salaries, and Jobs”
By Gordon B. Dahl and Matthew Knepper

Appendix Figures and Tables



(a) % of Employees Covered by EEO-1 Survey

(b) % of Estabs Covered by EEO-1 Survey

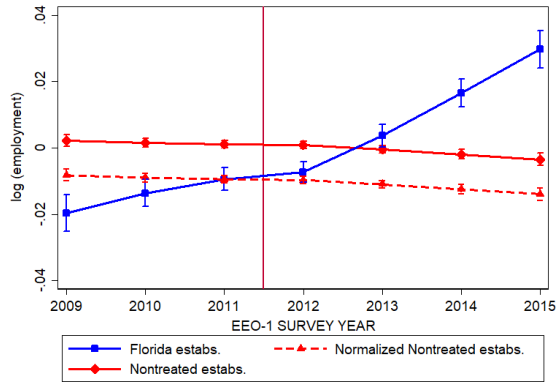


(c) % of Employees by Size in Population

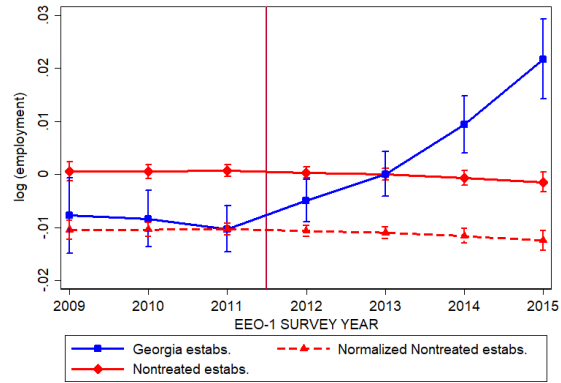
(d) % of Establishments by Size in Population

Figure A1: EEOC Coverage of Employees and Establishments, by Establishment Size

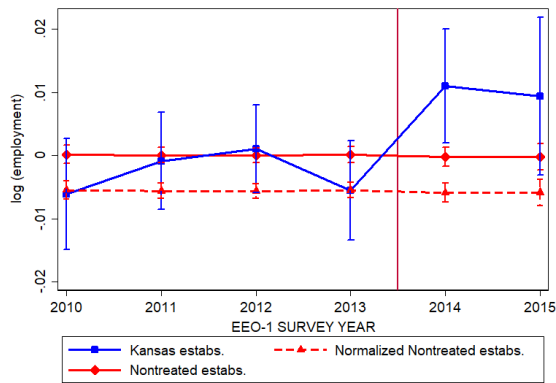
Panel A1a shows the fraction of all U.S. workers accounted for in the EEOC's EEO-1 data, by establishment size. Panel A1b shows the fraction of all U.S. establishments accounted for in the EEOC data, by establishment size. Panel A1c shows the fraction of all U.S. workers, by establishment size. Panel A1d shows the fraction of all U.S. establishments, by establishment size. Establishments are required to file an EEO-1 report if they employ at least 50 workers and their enveloping company has at least 100 employees, or if they are a federal contractor with at least 50 employees.



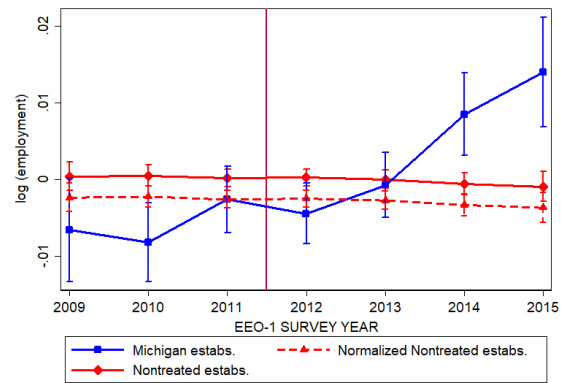
(a) Florida reform



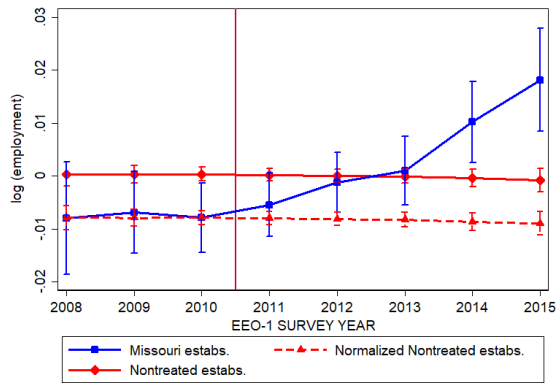
(b) Georgia reform



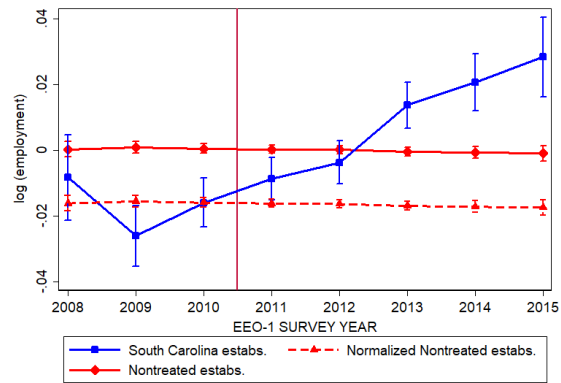
(c) Kansas reform



(d) Michigan reform



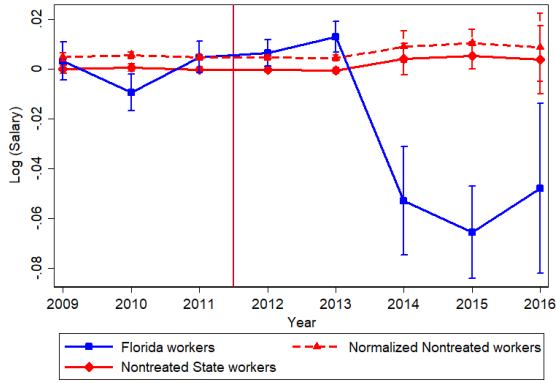
(e) Missouri reform



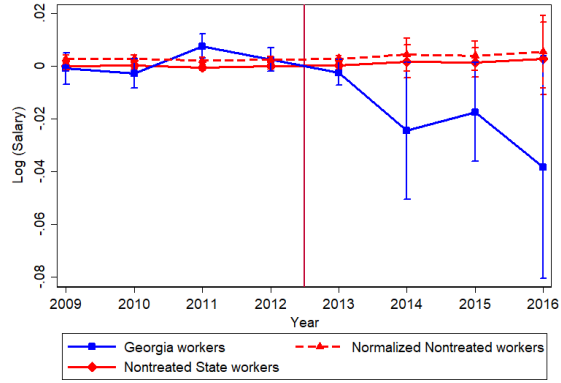
(f) South Carolina reform

Figure A2: Establishment Employment in Multi-state Firms for Moderate Reform States

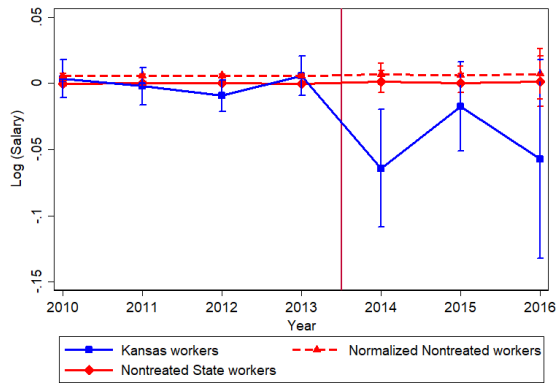
These plots show residualized EEO-1 employment growth (and 95% confidence intervals) for reform state-based establishments versus other nontreated state-based establishments within the same multi-state firm over time. Netted out are firm, year, industry, and state fixed effects.



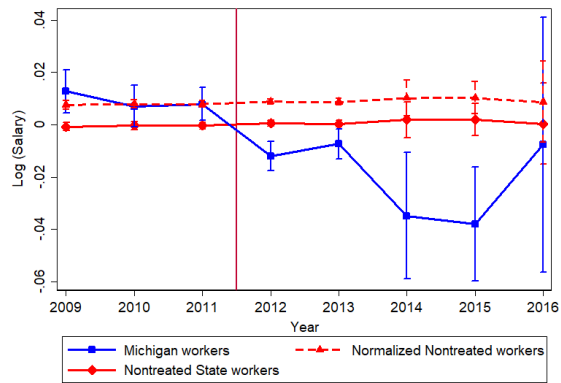
(a) Florida reform



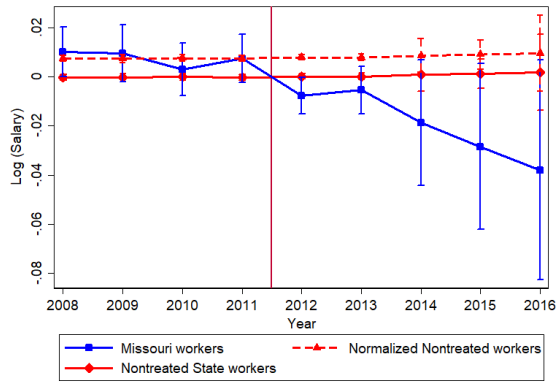
(b) Georgia reform



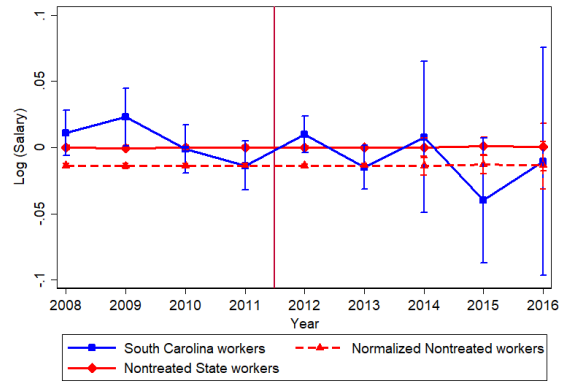
(c) Kansas reform



(d) Michigan reform



(e) Missouri reform



(f) South Carolina reform

Figure A3: Log Starting Salaries in Multi-state Firms for Moderate Reform States

These plots show residualized self-reported annual salaries from Glassdoor.com (and 95% confidence intervals) for newly hired workers based in the indicated reform state versus those working in nontreated states within the same multi-state firm. Netted out are firm fixed effects, state and metro area fixed effects, gender, and education.