UNC CHARLOTTE ECONOMICS WORKING PAPER SERIES

THE IMPACTS OF ENDING LONG-TERM UNEMPLOYMENT INSURANCE: EVIDENCE FROM NORTH CAROLINA

Craig A. Depken, II
Paul Gaggl

Working Paper No. 2016-001

THE UNIVERSITY OF NORTH CAROLINA AT CHARLOTTE
BELK COLLEGE OF BUSINESS
DEPARTMENT OF ECONOMICS
9201 University City Blvd
Charlotte, NC 28223-0001
February 2016

ABSTRACT

We investigate the causal effect of ending long-term emergency unemployment insurance (EUI) payments on labor market outcomes. Federal EUI payments in North Carolina (NC) expired on July 1, 2013, while EUI provision continued in all other states through December 31, 2013. We exploit cross-state variation generated by this unexpected NC law change to identify the causal effect of ending EUI payments. Our results indicate that the policy change induced a 1pp decrease in NC's unemployment rate, primarily due to unemployed individuals finding employment. However, these employment gains were concentrated in part-time jobs and among single females with children.

The Impacts of Ending Long-Term Unemployment Insurance: Evidence from North Carolina

February 14, 2016

Abstract: We investigate the causal effect of ending long-term emergency unemployment insurance (EUI) payments on labor market outcomes. Federal EUI payments in North Carolina (NC) expired on July 1, 2013, while EUI provision continued in all other states through December 31, 2013. We exploit cross-state variation generated by this unexpected NC law change to identify the causal effect of ending EUI payments. Our results indicate that the policy change induced a 1pp decrease in NC's unemployment rate, primarily due to unemployed individuals finding employment. However, these employment gains were concentrated in part-time jobs and among single females with children.

JEL Classifications: J30, J65
Keywords: unemployment insurance, long-term unemployment

Craig A. Depken, II[1]

University of North Carolina at Charlotte
Belk College of Business
Department of Economics
9201 University City Blvd
Charlotte, North Carolina 28223-0001
Email: cdepken@uncc.edu

Paul Gaggl[1]

University of North Carolina at Charlotte
Belk College of Business
Department of Economics
9201 University City Blvd
Charlotte, North Carolina 28223-0001
Email: pgaggl@uncc.edu

[1]We would like to thank Steve Billings, Matt Friedman, Jason Lindo, Jeremy Moulton, Lisa Schulkind, Ann Stevens, Rob Valetta, Aaron Yelowitz and participants at the 2014 SEA meetings, the South Carolina Appliead Micro Day (SCAM'd) as well as seminar participants at UNC Charlotte, Clemson University, Appalachian State University, and West Virginia University for their valuable comments and feedback. We are further grateful for generous financial support by a Belk College of Business Dean's Scholar Grant.

1. Introduction

In the wake of the Great Recession of 2008/2009, job losses and unemployment durations in the United States reached historical levels. Given these extraordinary circumstances, the usual 26 weeks of federal unemployment insurance (UI) payments were extended to an unprecedented 99 weeks for the majority of unemployed individuals. However, despite persistently anemic job growth at the time, the U.S. Congress terminated these extended programs as of January 1st, 2014.

This decision immediately raised wide-spread public debate about what would happen to the long-term unemployed after the program expiration. Would it encourage the affected individuals to take employment or would the affected individuals simply drop out of the workforce? If employment were taken, what form of employment would it be? Would it be in the same occupation and industry in which the individual was previously employed? Would it be part-time or full-time?

While the answers to these questions bear far-reaching implications for social welfare, the federal scope of the policy change makes convincing empirical analysis at the national level inherently difficult (see for example Farber, Rothstein and Valletta, 2015; Farber and Valletta, 2015; Rothstein, 2011). However, in February 2013 the North Carolina (NC) General Assembly passed Session Law 2013-02 which, among other things, reduced maximum weekly UI payments in North Carolina from $535 to $350 starting on July 1, 2013. At the time, federal law stipulated that any reduction in a state's maximum UI payments would trigger the immediate end of the federal extended UI programs (EUI) within that state. Therefore, starting on July 1, 2013 federal EUI payments ceased in North Carolina—reducing benefit eligibility from a maximum of 73 weeks to 19 weeks—while they continued to be provided in the rest of the country until the end of 2013. We argue that this change in North Carolina legislation creates a unique natural experiment that provides the opportunity to estimate the causal impact of the end of EUI payments on a variety of labor market outcomes in North Carolina. To the extent that North Carolina is representative of general labor market dynamics in the U.S., the case of North Carolina may provide useful insight into the implications of ending EUI payments more generally.

In our main analysis, we employ data from the U.S. Current Population Survey (CPS) and exploit cross state variation between North Carolina and its border states: South Carolina (SC), Georgia (GA), Tennessee (TN), and Virginia (VA). We first show that relevant labor market outcomes and demographic characteristics in North Carolina and its border states were comparable and followed parallel trends prior to the policy change, thus providing meaningful counterfactual economies to assess the causal impact of policy change. Using a standard difference-in-differences (DID) framework we then quantify the causal effect of ending EUI payments on the unemployment rate (UR), finding an approximate one percentage point decline. Moreover, we show that about half

of this decline is concentrated among individuals who would have received federal EUI payments in the absence of the policy change—those with unemployment spells lasting 20-73 weeks.

However, while much of the public debate is centered around the UR, it is well known that the UR is an inherently poor measure to assess the questions raised above, even if the estimated fall in the UR is the true causal effect of ending EUI. Not only would an unemployed individual who finds work reduce the UR, the same change could occur if the individual leaves the labor force (i.e. stops looking for work), moves out of the state, or an employed individual moves into North Carolina. While all four scenarios would lead to a fall in the UR, only the first would support the claim that the end of EUI induced unemployed individuals to find gainful employment—one of the key supporting arguments brought forward by proponents of the policy change.

To distinguish between the four distinct scenarios mentioned above, we decompose the estimated change in the UR into its components, finding a significant increase in employment (E), an almost one-for-one decrease in unemployment (U), and no significant change in the number of individuals "not in the labor force" (NILF). Our decomposition is therefore consistent with the first scenario mentioned above, in which unemployed individuals find gainful employment in response to the end of EUI.

While this suggests a "success" of the policy action, in terms of encouraging long-term unemployed individuals to take gainful employment, it neither implies that the observed effect is necessarily socially efficient, nor does it guarantee that the observed changes will persist. For example, it is likely that, when faced with the sudden end of EUI payments, affected individuals may prefer "sub-optimal," temporary or part-time employment over unemployment without UI support, even if a more efficient employer-employee match could be achieved with continued job search. While the long-run implications for social efficiency are obviously ambiguous, we address some of these concerns by providing evidence that suggests that the majority of the employment impact was in part-time jobs, in the manufacturing and construction sectors, and among single females with children.

Previous research has primarily focused on the impact of *extending* UI payments for the simple reason that there have been few chances to investigate the unexpected *end* of UI payments. For example, Card and Levine (2000) investigate how extending unemployment payments influenced unemployment spells in New Jersey. They take advantage of a natural experiment in that state which extended unemployment insurance payments for a twenty-five week period in 1996 while neighboring states did not. They find that the extension of UI payments caused up to a three percent increase in the number of UI claimants who exhausted their regular payments. A host of other studies have also investigated how extending payments alters labor market outcomes in various countries: for example, Katz and Meyer (1990) in the United States; Ham and Rea

(1987) in Canada; and Winter-Ebmer (1998) in Austria. Finally, a number of recent papers has focused specifically on the effects of EUI during the most recent US business cycles (e.g., Farber et al., 2015; Farber and Valletta, 2015), finding a small but statistically significant increase in labor force attachment due to extended UI at the micro level. At the macro level, Hagedorn, Karahan, Manovskii and Mitman (2013) find quantitatively important general equilibrium effects of UI extension. While closely related this body of work, the current study explicitly focuses on how an unexpected *end* rather than *expansion* of UI payments influences labor market outcomes.

The remainder of the paper is organized as follows. We start with a discussion of the institutional background surrounding the end of EUI in North Carolina in Section 2; Section 3 discusses the research design and policy variation; Section 4 describes the data employed in the empirical analysis; Section 5 presents the empirical results; Section 6 presents decomposed impacts of ending EUI on part-time and full-time employment, across major occupation groups, and across martial, gender, and child status. The final section offers concluding remarks.

2. Institutional Background

After the 1990-1991 recession, the state of North Carolina deliberately lowered the state's unemployment insurance tax paid by employers and in turn made the state's unemployment insurance trust fund a pay-as-you-go fund. The motivation was to encourage hiring by the state's employers as the country recovered from the recession.

As a consequence, the state was required to borrow heavily from the federal government to fund UI payments, when substantial demand for UI payments during the 2000-2001 recession swamped the trust fund. Nevertheless, the state's unemployment taxes remained unchanged after the recession and the state's unemployment trust fund was again funded on a pay-as-you-go basis through the mid 2000s—a time during which the trust fund had a steady-state balance of approximately $19 million. With the onset of the Great Recession in 2008, the state's ability to fully fund unemployment compensation was again quickly exhausted and by February 2009 North Carolina was again required to borrow money from the federal government to fund UI payments. By the end of 2012, North Carolina had accumulated more than $2.8 billion in debt to fund UI payments (North Carolina Department of Commerce, 2015).

In 2013, the newly elected General Assembly of North Carolina addressed concerns over the efficacy of the state's unemployment insurance system through Session Law 2013-02. This law introduced a number of reforms to the state's UI system that applied to employers, employees, and current and future recipients of UI payments. For instance, the law increased the state unemployment tax on employers, changed the rules of eligibility, and changed what are considered

acceptable reasons for leaving a job and still qualifying for unemployment insurance payments. Most important for the current study, the law reduced the maximum weekly benefit available from $535 to $350. The reduction in maximum payments had the dual goals of putting the state's UI system in a better financial position and motivating individuals to substitute employment for UI payments. However, federal law at the time stipulated that the two federal long-term UI programs would cease immediately in any state that reduced the maximum payments offered in the state's UI program.

When the North Carolina law went into effect on July 1, 2013, all federal long-term UI payments also ended on that day. Opponents of the North Carolina reforms argued that the early termination of the federal programs would put undue financial hardship on the long-term unemployed (beyond that of reducing the maximum weekly benefit), would increase the demand for services from other public and private assistance programs, and might encourage the long-term unemployed to drop out of the labor force. Proponents of the reforms argued that terminating the federal programs would save the state millions of dollars in accumulated debt to the federal government, would encourage the long-term unemployed to find employment, and would have little impact on the size of the state's active labor force. To this date, the resolution of this debate is an open empirical question.

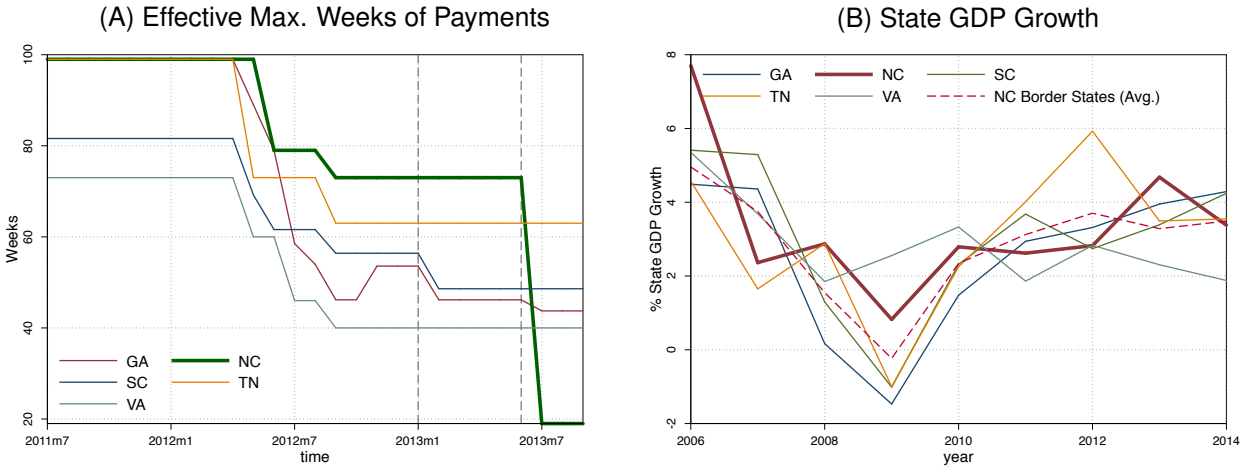
3. Policy Variation and Research Design

At the end of 2013, a sufficient number of months had elapsed so that back-of-the-envelope calculations could be made concerning the impact of the end of the federal UI programs in the state of North Carolina. Proponents of the reforms pointed to a reduction in the state's unemployment rate from 9.1% in July to 6.9% in December of 2013, ostensibly because of an increase in employment. Opponents argued that the unemployment rate fell during that period because the state's active work force fell from 4.75 million in July to 4.62 million in December.

Unfortunately, both sides of this debate compare North Carolina in December 2013 with North Carolina in July 2013. As is well known in policy evaluation, it is not enough to consider only a before-after comparison within the population affected by a particular policy. Rather, it is first-best to compare changes in the "treated" population with changes in a similar but "non-treated" counterfactual population over the same time horizon. Such a comparison helps eliminate the possibility that different pre-treatment trends are the underlying source of any observed differences in outcomes.

Our empirical strategy is to implement a difference-in-differences (DID) research design in the spirit of Card and Krueger (1994), which relies on two identifying assumptions: first, no external

Figure 1: Unemployment Insurance Eligibility Limits and State GDP Growth



Notes: Panel A is based on data collected by Rob Valetta and reports the effective maximum weeks of eligibility within each state between 2011m7 and 2013m9. Panel B reports annual state level GDP growth between 2006 and 2013.

shocks coincided with the timing of the policy change that disparately impacted North Carolina relative to its border states; second, the relevant labor market outcomes in North Carolina and its border states were following parallel trajectories prior to the policy change.

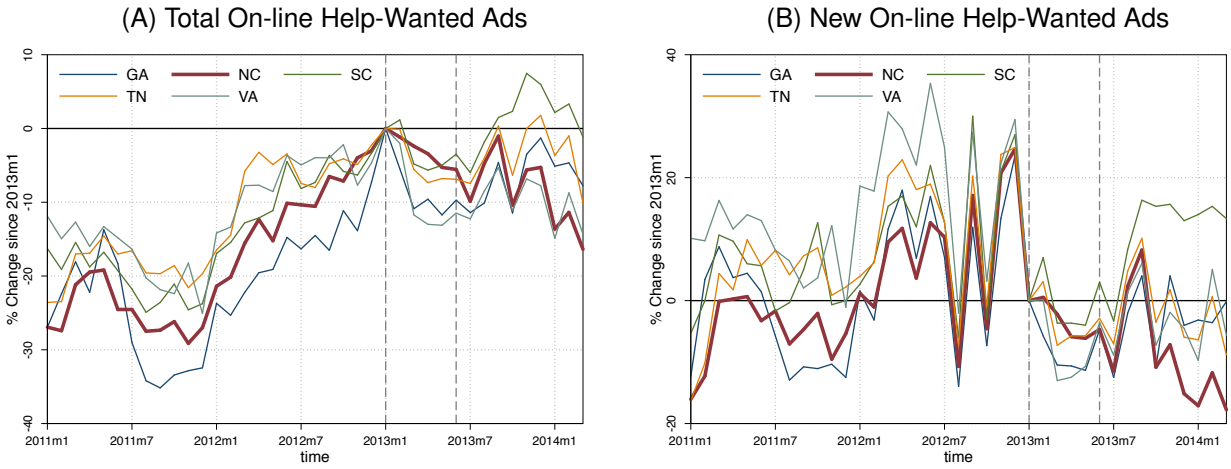
3.1. Validity of Identifying Assumptions

Given the importance of the two identifying assumptions mentioned above, this section provides evidence in favor of both. The first concern are other obvious legislative efforts within North Carolina or its border states that focused specifically on labor market outcomes, e.g., changes in the minimum wage, changes in technical training for the long-term unemployed, changes in disability eligibility, etc. To the best of our knowledge, there were no such legislative efforts around the same time as the law change in North Carolina investigated here and we therefore do not expect any other "policy shocks" to labor markets that might bias our empirical results.

Shifting the focus to UI benefits, Panel A of Figure 1 illustrates the changes in the maximum number of weeks of UI eligibility in North Carolina and its four border states from July 2011 through December 2014.[1] From July 2011 it is apparent that the eligibility rules in North Carolina and its border states followed a common trend up until the massive drop from 73 to 19 maximum weeks of unemployment benefits in North Carolina in July 2013. The "common" downward steps in eligibility prior to 2012m7 occur in all states at roughly the same time because the various states

[1] We thank Rob Valetta for sharing his data on maximum weeks of UI eligibility for the construction of this graph.

Figure 2: On-line Help Wanted Advertisements



Notes: Panel A depicts the total number of on-line help wanted ads in each state as reported by the Conference Board and gathered by Haver Analytics. Panel B depicts the number of new advertisements in each state. All data are reported monthly in thousands and are seasonally adjusted.

have similar formulas for relating the state's economic activity and unemployment levels to weeks of eligibility. Therefore, we conclude that there were no changes to long-term UI eligibility in North Carolina's border states aside from automatic business-cycle-induced shifts that occurred both in North Carolina *and* its border states.

While the parallel movement in eligibility rules is indicative of comparable business cycle patterns in all five states, it is conceivable that there were other state level shocks that were not directly related to UI eligibility. To assess this possibility Panel B of Figure 1 illustrates the annual growth rate of state GDP for North Carolina and its border states. As is readily apparent from this figure, the business cycle in all five states moved largely in lockstep.[2]

Finally, while the comparison of state GDP suggests a lack of differential demand and/or supply shocks, across the five states in the sample, we consider an additional indicator to specifically test for differences in the demand for labor: the monthly number of help-wanted advertisements online, as reported by the Conference Board and gathered by Haver Analytics. The monthly total number of advertisements within North Carolina and its border states are depicted in Panel A of Figure 2 and the monthly number of new advertisements are depicted in Panel B of Figure 2. In both cases the total number of advertisements have been normalized to zero at 2013m1. The graphs suggest no noticeable departures from previous trends before the time of the policy change.[3]

[2]DID estimates similar to our main analysis suggest that there was no statistically significant increase in state GDP growth in North Carolina after the policy change. The results are available upon request

[3]DID estimates similar to our main analysis suggest that there was no statistically significant increase in total or

Thus, we conclude that the period after 2011m7 is one during which North Carolina and its four border states were likely not exposed to any external shocks—in addition to the policy under study—that *differentially* affected either the eligibility for UI payments or the labor market more generally. We therefore argue that this makes the border states of North Carolina a useful set of counterfactual economies for the period after the early end of EUI.

4. Data Description

To estimate the impact of early EUI expiration on individual labor market outcomes, we employ individual level data from the U.S. Current Population Survey (CPS).[4] The CPS provides a monthly, nationally representative sample of the U.S. population, which facilitates exploiting the timing of the early termination of long-term UI payments in North Carolina.[5] We analyze individuals aged 16 or older and account for individual heterogeneity using a host of individual level characteristics, such as age, gender, ethnicity, education, union status, occupation, industry, etc.

An alternative to using individual-level data would be to use data aggregated at the state level. However, there are a number of benefits from working directly with individual-level data. First, each respondent is self-associated with a particular occupation category, whether they are employed or not.[6] This facilitates sorting individuals by major occupation category to test for disparate impacts of the end of long-term UI payments across these categories.

Second, it is possible to test whether those who were previously unemployed and took employment after July 1, 2013, did so in a part-time or full-time job. Evidence that those who faced the unexpected end of their long-term UI payments were more likely to take a part-time job would suggest that the policy change led to less than optimal worker-job matches. On the other hand, evidence of more full time jobs would support the hypothesis that many individuals were using long-term UI payments as a substitute for full-time employment.

Third, the individual data identify the sector and occupation in which the individual is employed or, if they are currently unemployed, the sector and occupation in which they previously worked. Following Autor, Levy and Murnane (2003) the individual's reported occupation is considered a proxy for the tasks he/she performs on the job. While some occupations tend to be highly

new on-line help-wanted advertisements in the state of North Carolina relative to the border states after the policy change. The results are available upon request.

[4]We obtain the data and auxiliary material from the NBER's website http://www.nber.org/data/cps_basic.html. Details about the survey itself as well as a variety of links to detailed technical documenation are provided on the same webpage.

[5]While the CPS is designed to be representative of the U.S. population, several previous studies have used the CPS to conduct state level analyses. See for example Hoynes, Miller and Schaller (2012) and references therein.

[6]Individuals without a job are asked to identify their most recent occupation, if any.

concentrated in particular sectors—e.g. machine operators in manufacturing—, there are a host of occupations that are not particular to industries—e.g. secretaries and accountants work in almost all industries. Therefore, the distinction between occupations and industries allows us to differentiate the impacts on different types of occupations/tasks from those on workers in particular industries—potentially performing a variety of tasks.[7]

Fourth, it is possible to narrow the focus to only long-term unemployed individuals, using the number of weeks unemployed as in Mukoyama and Sahin (2009). This restricts the sample to those individuals who were most directly affected by the loss of UI payments in North Carolina and similar individuals, although not impacted, in the border states of North Carolina. This allows for an estimate of the treatment effect on the treated, that is, the impact of the end of long-term UI payments on those for whom the loss of UI payments was most binding, rather than estimating the potentially misleading average treatment effect for the entire population.

Finally, the individual-level allow us to control for a set of observable differences across individuals—such as education level, gender, marital status, race, or number of children—within our difference-in-differences framework.

To shed some light on the relative concentration of unemployed as well as long-term unemployed individuals in North Carolina and its five border states, Table 1 reports the distribution of employed, unemployed, and long-term unemployed individuals identifying with one of six major occupation groups.

As can be seen, there is considerable variation in the percentage of individuals considered unemployed in each occupation group for both North Carolina and its border states; however, the variation is consistent across states. For example, in all five states the percentage of individuals who associate with "Management, professional and technical" occupations between three and five percent were unemployed; and yet of those who were unemployed 27-41 percent were "long-term" unemployed and potentially affected by the NC policy change, i.e., unemployed for 20-73 weeks. In "Construction and Maintenance" approximately 10-14 percent of individuals who associated with this occupation were unemployed and 20-40 percent of those were long-term unemployed.

To the extent that the end of long-term UI payments influenced the labor market in North Carolina, we would expect that those impacted by the end of the payments were more likely to respond—either by taking employment or leaving the labor force. The averages reported in Table 1 suggest that "Service occupations," "Sales and office occupations," and "Construction and maintenance" might be the major occupation groups that would experience the largest improve-

[7] See for example Acemoglu and Autor (2011) for a comprehensive review of the literature surouding the relationship between skills, tasks, and occupations.

Table 1: Long-Term Unemployed By Major Occupation

Major Occupation Groups	Employed	Unemployed	Long Term Unemp. (20 – 73 weeks) % of LF	Long Term Unemp. (20 – 73 weeks) % of Unemp.
A. North Carolina				
Management, professional, and	94.90	4.44	1.85	41.75
Service occupations	87.65	11.00	4.69	42.63
Sales and office occupations	89.79	9.17	3.71	40.40
Farming, fishing, and forestry	82.65	15.58	2.98	19.14
Construction, and maintenance	87.51	11.87	4.25	35.81
Production, transportation,	85.59	13.60	4.46	32.77
B. South Carolina				
Management, professional, and	95.29	4.30	1.35	31.36
Service occupations	87.10	10.97	3.73	33.97
Sales and office occupations	90.52	8.50	3.31	39.01
Farming, fishing, and forestry	85.35	13.54	2.52	18.59
Construction, and maintenance	88.55	10.80	4.43	40.99
Production, transportation,	87.62	11.86	4.51	38.02
C. Georgia				
Management, professional, and	94.39	5.02	1.98	39.39
Service occupations	87.47	11.52	4.38	38.02
Sales and office occupations	90.28	8.86	3.08	34.77
Farming, fishing, and forestry	91.25	7.10	1.45	20.38
Construction, and maintenance	87.92	11.68	2.84	24.35
Production, transportation,	87.57	11.75	3.87	32.97
D. Tennesee				
Management, professional, and	95.03	4.38	1.61	36.79
Service occupations	88.22	9.97	3.67	36.79
Sales and office occupations	91.49	7.45	2.64	35.47
Farming, fishing, and forestry	69.02	27.19	7.51	27.63
Construction, and maintenance	88.89	10.27	3.32	32.33
Production, transportation,	90.52	8.38	2.74	32.69
E. Virginia				
Management, professional, and	96.45	3.06	0.84	27.48
Service occupations	90.39	8.16	3.11	38.14
Sales and office occupations	92.97	6.09	2.47	40.63
Farming, fishing, and forestry	90.13	9.87	6.03	61.11
Construction, and maintenance	92.81	6.85	2.11	30.72
Production, transportation,	91.88	7.43	2.23	29.97

Notes: The table is based on the monthly CPS basic files and representative cell means are estimated using the CPS sampling weights.

Table 2: Summary Statistics

	2011m7 – 2013m1		2013m2 – 2013m6		2013m7 – 2014m8	
	NC	Border States	NC	Border States	NC	Border States
A. All Individuals						
Avg. Obs./Month	1193.34	5074.84	1698.40	7118.00	1656.17	7087.50
Population (Mill.)	4.36	15.34	6.12	21.50	6.16	21.58
% EMP/POP	65.81	66.48	64.46	66.45	67.75	66.49
% UN/LF	6.50	5.75	6.50	5.75	5.33	5.13
% LTU/LF	2.40	1.92	2.33	1.94	1.68	1.64
% Male	48.12	48.51	47.92	48.49	48.09	48.67
% HS+	83.61	85.15	82.48	85.16	84.21	85.65
% College+	26.26	27.02	25.20	27.78	26.51	27.44
% Serv. Occ.	12.82	12.67	13.99	12.71	13.46	13.10
Avg. Age	39.84	39.83	39.54	39.84	39.89	39.83
Avg. Weeks UN	37.12	33.32	36.67	31.42	36.99	30.76
B. Long-Term Unemployed (20-73 weeks)						
Avg. Obs./Month	27.39	94.47	37.80	135.60	26.17	110.17
Population (Mill.)	0.10	0.30	0.14	0.42	0.10	0.35
% Male	47.83	49.74	48.01	49.25	55.85	49.77
% HS+	80.03	81.33	73.93	79.69	77.64	82.78
% College+	14.22	13.54	15.06	11.37	12.62	14.84
% Serv. Occ.	23.03	21.99	28.77	27.75	19.16	23.52
Avg. Age	36.66	34.97	35.39	35.69	36.84	36.10
Avg. Weeks UN	40.28	40.29	41.65	40.26	41.15	40.78

Notes: The table is based on the monthly CPS basic files and representative cell means are estimated using the CPS sampling weights.

ments in employment after the policy change. This is because these categories represent the largest percentage of the work force, have some of the largest percentage of long-term unemployed, and anecdotally have substantial turnover.

Table 2 reports summary statistics for a variety of individual characteristics within North Carolina and its border states. Panel A shows statistics for all individuals while Panel B reports statistics for those who were "long-term" unemployed (20-73 weeks). The table also distinguishes across time by presenting statistics for the period before the announcement of the end of EUI benefits, the period between the announcement and the actual change in the policy, and the period after the change in the policy and the end of the sample period. As can be seen, the percent employed in the general population increases in North Carolina after the policy change whereas the percent of the population employed in the border states does not change much over the sample period. Likewise, the percent of unemployed and long-term unemployed as a percentage of the labor force

decreases in North Carolina after the policy change and there appears to be little change in the border states. The remaining characteristics do not change much between North Carolina and its border states or over time.

Panel B reports statistics for the long-term unemployed that are relatively stable over time and across states. The notable exceptions are: the population of the long-term unemployed which drops from the post-announcement period to the post-policy change period; the percentage of observations with a high school education which drops somewhat in North Carolina during the post-announcement but pre-policy change period; and the percentage of individuals who indicated they previously worked in the service sector, which increased for both North Carolina and its border states from the pre-announcement period to the post-announcement period, and declined for both sets of states after the policy change.

5. Empirical Methodology and Results

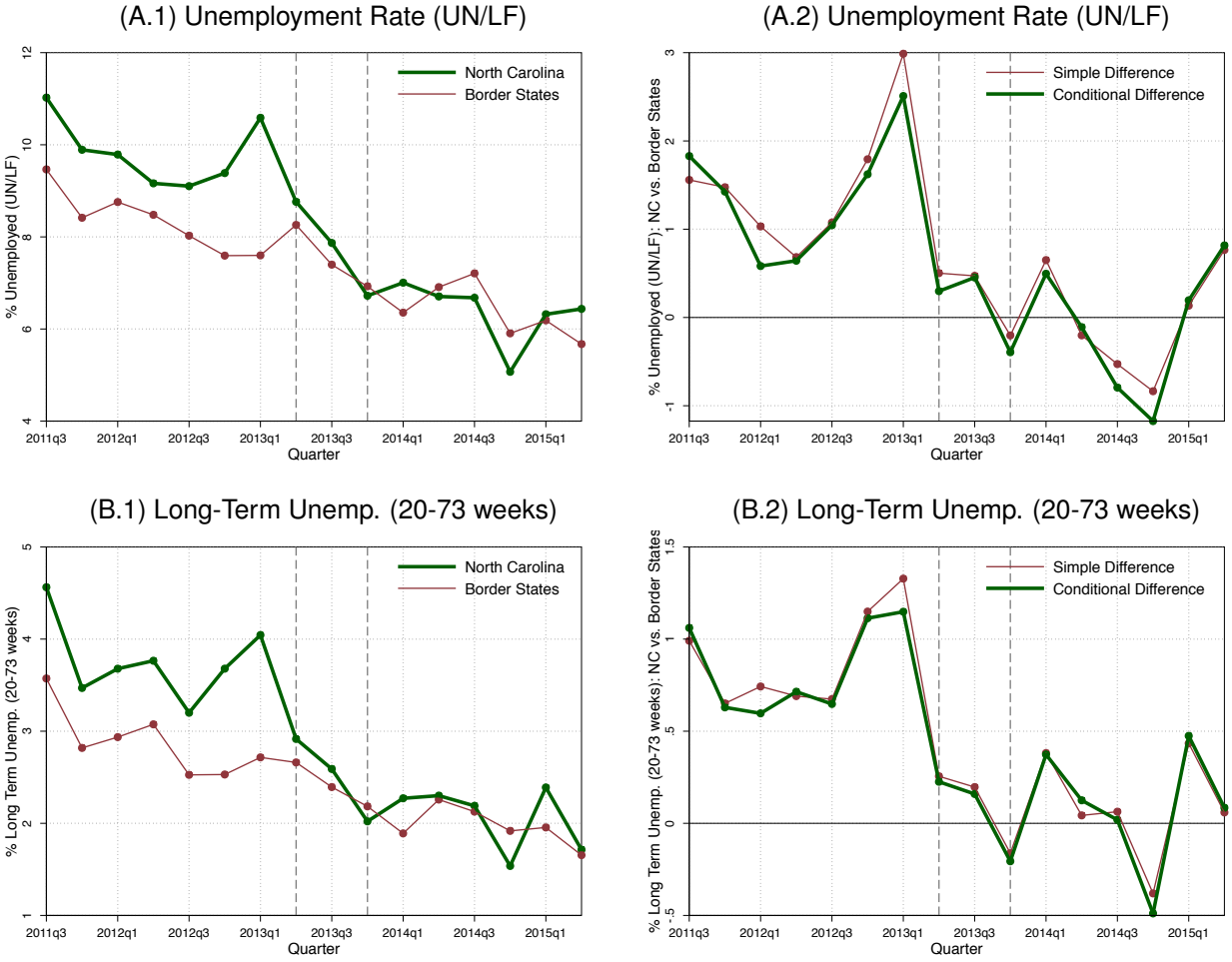
To assess the impact of North Carolina's change in UI benefit eligibility, we start our empirical analysis with a simple comparison of quarterly unemployment and long-term unemployment rates in North Carolina and its border states. We estimates unconditional and conditional means by running the following cross-sectional regression model separately for each quarter $t \in \{2011q3, ..., 2015q2\}$:

$$y_{ist} = \alpha_t + \beta_{NC,t} + \gamma_t X_{ist} + \varepsilon_{ist} \quad \text{for all } t \in \{2011q3, ..., 2015q2\}, \tag{1}$$

where s indicates a state and i an individual. Further, y_{ist} is an indicator for either an unemployed or "long-term" unemployed (20-73 weeks) individual, α_t is a time specific constant, $\beta_{t,NC}$ a time specific North Carolina effect and γ_t a time specific regression coefficient on a vector of demographic control variables, X_{ist}. Finally, ε_{ist} is a zero-mean error term. We note that we consider "long-term" unemployed individuals as those who were unemployed for at least 20 but at most 73 weeks during the sample period. These are precisely the individuals who would have been eligible for federal UI payments in North Carolina between 2013m7 and 2013m12 in the absence of the policy change.

We employ the monthly CPS basic files to estimate the above regression model quarter by quarter and use the resulting estimates for α_t and $\beta_{NC,t}$ to construct unconditional quarterly averages for unemployment and long-term unemployment rates within NC and its border states separately. Panel A.1 of Figure 3 displays the resulting unconditional unemployment rates. The dashed vertical lines indicate the quarter immediately before federal EUI payments expired in North Carolina (2013q2) and the last quarter immediately before federal EUI payments expired nation wide

Figure 3: Unemployment Rate (CPS Basic Files)



Notes: The graphs are based on regression model (1) and data from the monthly basic CPS files. The solid lines in panels A.1 and B.1 plot quarterly state specific averages ($\hat{\alpha}_t$ for the border states and $\hat{\alpha}_t + \hat{\beta}_{NC,t}$ for North Carolina). Panels A.2 and B.2 plot quarterly differences between North Carolina and border state averages (i.e., $\hat{\beta}_{NC,t}$). The dashed vertical lines indicate the quarter prior to the early termination of long-term UI payments in North Carolina (2013q2).

(2013q4). Based on estimates $\beta_{NC,t}$, Panel A.2 plots the difference in the unemployment rate between North Carolina and its border states, both unconditionally and conditional on a vector of demographic control variables X_{ist}. As can be seen, there is very little difference between the two plots in Panel A.2 illustrating that demographic heterogeneity does not systematically affect the between-state difference in the unemployment rate.

These figures reveal two interesting insights: first, the estimates suggest that North Carolina and its border states did not experience *differential* trends prior to the policy announcement. Rather, unemployment rates evolved in parallel yet at different levels. Second, it is clear from Panel A.2 that the unconditional unemployment rate in North Carolina was consistently higher than in its

border states before the policy change. However, this gap of roughly one percentage point starts to vanish with the announcement of the change in benefit eligibility during the first quarter of 2013 and is essentially closed by the time the new UI eligibility rules became effective in July of 2013. Moreover, this gap remains closed until the end of our sample in 2015m6. Panels B.1 and B.2 of Figure 3 highlight another interesting observation: the majority of the gap in unemployment rates prior to January 2013 appears to be concentrated among individuals who were unemployed for 20-73 weeks—precisely the range of individuals who "lost" extended UI eligibility in July of 2013 (at the latest).

The observations from this graphical illustration are confirmed by point estimates obtained from the following difference in differences model:

$$y_{ist} = \alpha_s + \beta \textit{POLICY}_t + \gamma \textit{POLICY}_t \cdot \mathbf{1}\left[s = NC\right] + \delta' X_{ist} + \epsilon_{ist}, \tag{2}$$

where y_{ist} is an indicator variable that takes a value of one if the individul is unemployed and zero otherwise, α_s are state effects for $s \in \{\textit{NC}, \textit{GA}, \textit{SC}, \textit{TN}, \textit{VA}\}$, $\textit{POLICY}_t$ indicates periods during which the new policy was in place, $\mathbf{1}\left[s = NC\right]$ indicates that individual i lives in North Carolina at time t, X_{ist} is a vector of additional control variables, and ϵ_{ist} is a disturbance term with $E[\epsilon_{ist}] = 0$.

Within regression model (2), an estimate for γ measures the average *differential* impact of the policy change on outcomes y_{ist}. To account for potential differences in the demographic composition across North Carolina and its border states, we include the following control variables in X_{ist} described below.

Table 3 summarizes the results obtained from regression model (2). Panel A reports estimates for the overall unemployment rate, suggesting that the drastic reduction in the maximum weeks of UI eligibility caused an approximately 1.2 percentage point decrease in the unemployment rate in North Carolina. The effect is net of other time trends and macroeconomic influences common to all states in the sample. Panel B indicates that roughly 0.5 percentage points can be directly attributed to individuals with unemployment spells between 20-73 weeks—approximately 42% of the drop in the aggregate unemployment rate.

For both panels in Table 3, column (1) reports a pooled model without control variables, column (2) adds state specific time trends, and column (3) adds the following demographic control variables: quadratic polynomials in age by education (five education categories) and complete sets of fixed effects for gender, race (white, black, other), marital status (married, single), children (yes/no), and quarter (for seasonality). Finally, columns (4) and (5) add industry (14 major sectors) and occupation (7 major occupations) fixed effects, respectively.

Table 3: Unemployment Rate: North Carolina vs. Border States

	(1)	(2)	(3)	(4)	(5)
A. Unemployment Rate (% of labor force)					
Diff. in Diff.	-1.359***	-1.442***	-1.386***	-1.281***	-1.219**
	(0.262)	(0.533)	(0.515)	(0.494)	(0.493)
B. "Long Term" Unemployment Rate (20 - 73 weeks, % of labor force)					
Diff. in Diff.	-0.732***	-0.719**	-0.697**	-0.543*	-0.515*
	(0.159)	(0.321)	(0.318)	(0.299)	(0.299)
State Trends		yes	yes	yes	yes
Controls			yes	yes	yes
Ind. FEs				yes	yes
Occ. FEs					yes
Obs.	305147	305147	305147	302528	302528

Notes: The table reports difference in difference coefficients measuring differential percentage point changes. The dependent variable in panel A is 100 times an indicator for unemployment and in panel B for individuals with 20-73 weeks of unemployment, restricted to individuals in the labor force (employed or unemployed). All regressions are run at the individual level and monthly frequency, weighted by the CPS sampling weights. Regressions 2-5 include state specific quadratic time trends (at monthly frequency). Regressions 3-5 additionally include quadratic polynomials in age by education (five education bins) and complete sets of fixed effects for gender, race (white/black/other), marital status (married/single), children (yes/no), and quarter. Regressions 4 and 5 include industry fixed effects (14 major industries) and occupation fixed effects (7 major occupations), respectively, for individuals who report an industry or occupation. The number of observations is the unweighted labor force in our sample summed over all states and months. Robust standard errors are reported in parentheses below each coefficient. Significance levels are indicated by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

The magnitude is slightly reduced for the estimates from models that include industry and occupation fixed effects but all coefficients are precisely estimated and indicate a reduction in the unemployment rate in North Carolina attributable to the UI policy change of between 1.2-1.4 percentage points. Similarly, the estimates suggest a reduction in the unemployment rate of 0.5-0.7 percentage points attributable to long-term unemployed individuals, with a slight loss of precision in the specifications that include industry and occupation fixed effects.[8]

[8]Note that the loss of precision is in part driven by the much narrower demographic groups and in part by the fact that not all individuals report their industry and/or occupation. This is also visible in the lower number of observations in specifications (4) and (5) in Table 3.

5.1. Decomposing Changes in the Unemployment Rate

Our results from the previous section suggest that the end of long-term UI in North Carolina led to a decline in the unemployment rate. However, despite the popularity of using changes in the unemployment rate (UR) in policy discussions, it is well known that changes in this measure are hard to interpret. To see why, define the unemployment rate as $UR = U/LF = U/(E+U)$, where E denotes the number of employed individuals of working age, U the number of unemployed individuals of working age, and the total labor force is defined as $LF \equiv E + U$. A reduction in the UR can be caused by any of four distinct reasons: (1) an unemployed individual finds employment (U falls and E rises), (2) an unemployed individual leaves the labor force (U falls and *NILF* increases, where *NILF* indicates individuals not in the labor force), (3) an unemployed individual moves out of state (U falls), and (4) an employed individual moves into North Carolina (E increases).

To avoid the observational equivalence that arises from the various sources for change in the UR, it is more practical to work directly with the definition of the state's working-age population, defined as $N = E + U + NILF$, which directly implies the following relation:

$$0 = d(E/N) + d(U/N) + d(\textit{NILF}/N), \tag{3}$$

where $d(z)$ denotes the month-to-month change in variable z. It is straight forward to verify that, in principle, this alternative decomposition allows us to unambiguously distinguish the four cases mentioned above based on changes in E/N, U/N, and *NILF*$/N$. Specifically, case 1 implies that $d(E/N) = -d(U/N) > 0$ and $d(\textit{NILF}/N) = 0$, while case 2 implies that $d(\textit{NILF}/N) = -d(U/N) > 0$ and $d(E/N) = 0$. Cases 3 and 4 imply that $dN \neq 0$ and changes in all three ratios will therefore be non-zero in both of these scenarios.

Table 4 shows our estimates of the policy-induced flows for these quantities, again using our difference in differences (DID) specification (2). The dependent variables are now indicators for individuals who are employed (panel A), unemployed (panel B), or not in the labor force (panel C), respectively, where the underlying population reflects the state's working-age population (N). Thus, the D-in-D coefficients reflect estimates of the three distinct flows in relation (3), expressed as percentage point changes in the fraction of the state population within the three respective groups of individuals (E, U, and *NILF*). In analogy to Table 3, column (1) reports the pooled estimates, while columns (2)-(5) add additional demographic control variables.

Mindful of potential measurement issues with regards to individuals in the labor force, the estimates in Table 4 deliver a clear picture: Panels A and B suggest that the decrease in the UR is primarily driven by a reduction in U and an increase of an approximately equal magnitude in E.

Table 4: Employed, Unemployed & NILF: North Carolina vs. Border States

	(1)	(2)	(3)	(4)	(5)
A. Employed Individuals (% of population)					
Diff. in Diff.	1.225***	1.397*	1.151*	1.106**	1.036**
	(0.377)	(0.757)	(0.689)	(0.515)	(0.514)
B. Unemployed Individuals (% of population)					
Diff. in Diff.	-0.967***	-1.012***	-0.998***	-1.283***	-1.220**
	(0.191)	(0.390)	(0.385)	(0.490)	(0.490)
C. Individuals Not in the Labor Force (% of population)					
Diff. in Diff.	-0.258	-0.385	-0.153	0.177	0.184
	(0.357)	(0.715)	(0.651)	(0.176)	(0.176)
State Trends		yes	yes	yes	yes
Controls			yes	yes	yes
Ind. FEs				yes	yes
Occ. FEs					yes
Obs.	425978	425978	425978	305231	305231

Notes: The table rep orts difference in difference coefficients measuring differential percentage point changes. The dependent variables are 100 times an indicator for employment (panel A), unemployed (panel B), and not in the labor force (panel C). The number of observations is the unweighted population in our sample summed over all states and months. See the notes in Table 3 for details on the included control variables. Robust standard errors are reported in parentheses below each coefficient. Significance levels are indicated by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

The insignificant effects on $NILF$ are in the neighborhood of zero, and further suggest that neither individuals who drop out of the labor force nor between-state migration seems to play an important role for the estimated changes in the UR associated with the policy change in North Carolina.

6. Decomposing the Aggregate Effects

The results in Section 5 suggest that the end of federal EUI benefits in North Carolina induced unemployed individuals to take gainful employment, which in turn lead to a 1.2 percentage point decrease in the unemployment rate within North Carolina. This finding is perhaps not surprising, given the potentially substantial negative income shock to suddenly ineligible EUI recipients. However, to better understand the implications of the policy for the state of North Carolina, it is instructive to decompose the overall effect across the type of jobs taken and particular demographic

Table 5: Full-Time/Part-Time Employment

	(1)	(2)	(3)	(4)	(5)
A. Full Time Employed Individuals (≥35 hours, % of population)					
Diff. in Diff.	0.969**	0.509	0.207	-0.194	-0.475
	(0.398)	(0.799)	(0.719)	(0.805)	(0.802)
B. Part Time Employed Individuals (<35 hours, % of population)					
Diff. in Diff.	0.257	0.888	0.944	1.300*	1.510**
	(0.288)	(0.581)	(0.574)	(0.737)	(0.736)
State Trends		yes	yes	yes	yes
Controls			yes	yes	yes
Ind. FEs				yes	yes
Occ. FEs					yes
Obs.	425978	425978	425978	305231	305231

Notes: The table reports difference in difference coefficients measuring differential percentage point changes. The dependent variables are 100 times an indicator for full-time (¿35 hours) employment (panel A) and part-time (≥35 hours) employment (panel B). The number of observations is the unweighted population in our sample summed over all states and months. See the notes in Table 3 for details on the included control variables. Robust standard errors are reported in parentheses below each coefficient. Significance levels are indicated by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

groups.

The former decomposition can provide information about the "efficiency" of the new employer-employee matches, while the latter allows us to gauge whether particular demographic groups were more affected by the policy. For example, did a former secretary find another position as a secretary, or did she/he instead take a job waiting tables? Did a former accountant find another job in accounting or did she/he take a job selling cars? Given the net increase in employment, are the majority of new jobs taken part-time or full-time? Similarly, we would like to know whether the policy predominantly affected single mothers or particular minority groups, or whether the effects were evenly distributed across demographic groups.

Table 5 illustrates that, after conditioning on industrial composition, there was no notable effect on the rate of full-time (at least 35 hours) employment. To the contrary, we see an increase in part-time employment that is approximately of the same magnitude as the main employment effect in Table 4, suggesting that essentially all of the new jobs were part-time jobs. This is a first indication that the seemingly positive effects of the policy action—i.e. more jobs and fewer unemployed individuals—may have been an inefficient outcome in the long run, assuming that individuals

Table 6: Decomposition: Major Occupation Groups

	Man. Prof.	Serv.	Sales Office	Farm. Fish.	Constr. Manu.	Prod. Transp.
	(1)	(2)	(3)	(4)	(5)	(6)
A. Employed Individuals (% of population)						
Diff. in Diff.	1.755***	-1.508***	-0.150	0.0197	0.935**	0.0987
	(0.581)	(0.514)	(0.564)	(0.118)	(0.367)	(0.432)
Pre-Avg. NC	24.0	11.6	15.1	0.4	6.2	8.1
Pre-Avg. Border	24.8	11.2	15.4	0.4	5.9	8.7
B. Unemployed Individuals (% of population)						
Diff. in Diff.	0.168	-0.249	-0.510***	0.0769**	0.179	-0.542***
	(0.145)	(0.195)	(0.187)	(0.0382)	(0.127)	(0.163)
Pre-Avg. NC	1.1	1.5	1.5	0.1	0.8	1.2
Pre-Avg. Border	1.0	1.3	1.3	0.0	0.7	1.0
C. Employed or Unemployed Individuals (Labor Force, % of population)						
Diff. in Diff.	1.923***	-1.757***	-0.660	0.0966	1.114***	-0.443
	(0.586)	(0.538)	(0.583)	(0.124)	(0.382)	(0.453)
Pre-Avg. NC	25.1	13.1	16.7	0.5	7.0	9.4
Pre-Avg. Border	25.8	12.5	16.7	0.4	6.5	9.7
State Trends	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
Obs.	425978	425978	425978	425978	425978	425978

Notes: The table reports difference in difference coefficients measuring differential percentage point changes within major occupations: (1) managerial and professional, (2) service, (3) sales and office, (4) farming and fishing, (5) construction and manufacturing, (6) production and transportation. The table is constructed in analogy to Table 4 and the included control variables are the same as in Table 3. Due to the much narrower demographic groups we do not include industry fixed effects. The number of observations is the unweighted population in our sample summed over all states and months. Robust standard errors are reported in parentheses below each coefficient. Significance levels are indicated by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

would prefer full-time employment over part-time employment.

Table 6 decomposes the effects by major occupation groups, potentially delivering further clues on the efficiency of the policy induced re-allocation of labor. Panel A suggests that the net employment gains were predominately concentrated in managerial professional occupations as well as construction and manufacturing jobs. Panel B suggests that the bulk of the net decrease in people looking for jobs occurred in sales and office occupations, as well as in production and transportation occupations. Panel C of Table 6 confirms that there was a shift in the allocation of

Table 7: Decomposition: Major Industries

	Agric. Mining	Constr.	Manu.	Wholes. Retail	Inform. Finance	Edu. Health	Service	Public Armed
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Employed Individuals (% of population)								
Diff. in Diff.	0.277*	0.511	0.482	0.00542	-0.252	0.576	-0.335	-0.114
	(0.159)	(0.326)	(0.415)	(0.518)	(0.515)	(0.532)	(0.471)	(0.277)
Pre-Avg. NC	0.9	4.8	7.5	12.2	12.7	15.3	9.3	2.8
Pre-Avg. Border	1.0	4.5	7.2	13.3	13.4	14.0	9.3	3.9
B. Unemployed Individuals (% of population)								
Diff. in Diff.	0.0425	0.271**	-0.102	0.0167	-0.374**	-0.434***	-0.309*	0.0364
	(0.0387)	(0.116)	(0.140)	(0.157)	(0.161)	(0.145)	(0.175)	(0.0549)
Pre-Avg. NC	0.1	0.7	0.8	1.1	1.2	1.0	1.2	0.2
Pre-Avg. Border	0.1	0.6	0.6	1.0	1.0	0.7	1.1	0.1
C. Employed or Unemployed Individuals (Labor Force, % of population)								
Diff. in Diff.	0.320*	0.782**	0.380	0.0222	-0.627	0.142	-0.644	-0.0774
	(0.164)	(0.341)	(0.434)	(0.534)	(0.533)	(0.543)	(0.495)	(0.282)
Pre-Avg. NC	1.0	5.6	8.4	13.3	13.9	16.2	10.5	2.9
Pre-Avg. Border	1.1	5.0	7.8	14.3	14.4	14.7	10.4	4.0
State Trends	yes	yes	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes	yes	yes
Obs.	425978	425978	425978	425978	425978	425978	425978	425978

Notes: The table reports difference in difference coefficients measuring differential percentage point changes within major industries. The table is constructed in analogy to Table 4 and the included control variables are the same as in Table 3. Due to the much narrower demographic groups we do not include occupation fixed effects. The number of observations is the unweighted population in our sample summed over all states and months. Robust standard errors are reported in parentheses below each coefficient. Significance levels are indicated by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

labor in North Carolina relative to its border states away from the service sector and to professional/management and construction jobs. There was no notable net change in sales, agricultural, or production and transportation jobs.

Closely related, Table 7 suggests that the construction and manufacturing sectors combined saw the largest gains in employment. Similarly, a drop in the number of individuals looking for sales and office jobs appear to have originated in the information, finance, and health sectors. Furthermore, we see a decline in the number of individuals looking for jobs in the service sector, in line with the results in Table 6. Again consistent with the results on occupations, we see the largest increase in people looking or employed within the construction and manufacturing sectors

Table 8: Decomposition: Marital Status, Gender, Children

	Married	Single	Male	Female	No Child	≥1Child(ren)
	(1)	(2)	(3)	(4)	(5)	(6)
A. Employed Individuals (% of population)						
Diff. in Diff.	0.726	0.466	0.193	0.939	-0.0432	1.193**
	(0.661)	(0.683)	(0.742)	(0.714)	(0.749)	(0.599)
Pre-Avg. NC	37.9	27.6	34.5	31.0	41.5	23.9
Pre-Avg. Border	37.8	28.6	34.8	31.6	42.2	24.2
B. Unemployed Individuals (% of population)						
Diff. in Diff.	-0.006	-1.022***	-0.171	-0.828***	-0.653**	-0.344
	(0.212)	(0.327)	(0.289)	(0.267)	(0.327)	(0.213)
Pre-Avg. NC	2.3	4.7	3.6	3.4	4.8	2.2
Pre-Avg. Border	1.9	4.1	3.1	2.9	4.2	1.8
C. Employed or Unemployed Individuals (Labor Force, % of population)						
Diff. in Diff.	0.720	-0.556	0.0221	0.111	-0.696	0.849
	(0.664)	(0.696)	(0.759)	(0.733)	(0.754)	(0.612)
Pre-Avg. NC	40.2	32.3	38.1	34.4	46.3	26.1
Pre-Avg. Border	39.8	32.7	37.9	34.5	46.4	26.0
State Trends	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
Obs.	425978	425978	425978	425978	425978	425978

Notes: The table reports difference in difference coefficients measuring differential percentage point changes within conditioning on six demographic groups: married/single, male/female, and at least one child/no child. The table is constructed in analogy to Table 4 and the included control variables are the same as in Table 3 except that we exclude the fixed effects for the demographic group we are conditioning on. Due to the much narrower demographic groups we do not include industry or occupation fixed effects. The number of observations is the unweighted population in our sample summed over all states and months. Robust standard errors are reported in parentheses below each coefficient. Significance levels are indicated by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

combined.

Taken together with the results on part-time employment, these estimates suggest that the largest net gain in employment appears to be concentrated in part-time jobs within construction and manufacturing occupations. On the flip side, individuals appear have quit looking for sales and administrative jobs within the health, information, and finance sectors.

6.1. Demographic Groups

We next focus on differential effects across a number of demographic groups. Specifically, we focus on differences in the effects by gender, marital status (married/single), and the presence of children in the household (child/no child). Table 8 illustrates the effects when considering these three groupings separately. When comparing married and single individuals we see that significantly more singles report to be looking for a job, while it is not clear whether this reduction in unemployed individuals is due to new gainful employment or whether they drop out of the labor force. A slightly more precise picture emerges when comparing men and women, which suggests that our estimated aggregate effect is almost exclusively concentrated among women. Finally, when differentiating between households with and without children, the majority of the decline in unemployed individuals is within households without children, while the bulk of the increase in employment appears to be stemming from households with children.

The mixed results from the decomposition displayed in Table 8 suggests that the interaction of these three attributes may be more informative. Table 9 illustrates this disaggregation and delivers a striking result. The majority of our aggregate effect appears to be driven by two flows. First, our results indicate a decrease in unemployed single mothers with a roughly equal increase in employed single mothers. This result appears plausible, given that single mothers are likely a group of individuals who suffer dramatically from a substantial income shock like the loss of extended unemployment benefits.

Interestingly, we also find a strong increase in the number employed married mothers, yet no notable change in the number of unemployed married mothers. Rather, the highly significant coefficient on labor force participation for this group suggests that married mothers joined the labor force to take gainful employment. This result is a little less straight forward to interpret. However, while this is clearly suggestive evidence, these estimates are consistent with the following scenario. Imagine a married couple, in which the husband is long term unemployed but the wife did previously not work. Given the long term unemployment of the husband, it is unlikely that the type of job he is looking for is currently available. Thus, either he finds a suboptimal job, or his wife finds temporary part time employment and supports the continuation of his job search.

Finally, we also see decline in unemployed single females without children, but no concurrent rise in employment.

7. Conclusions

On January 1, 2014, long-term UI payments were discontinued by the Federal Government. In the lead up to the policy change there was considerable public debate about the impact of

Table 9: Decomposition: Marital Status × Gender × Children

	Married				Single			
	Male		Female		Male		Female	
	No Ch.	Child	No Ch.	Child	No Ch.	Child	No Ch.	Child
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
A. Employed Individuals (% of population)								
Diff. in Diff.	0.114	-0.0204	-0.408	1.193***	0.365	-0.179	-0.409	0.522*
	(0.457)	(0.475)	(0.425)	(0.409)	(0.533)	(0.191)	(0.487)	(0.317)
Pre-Avg. NC	10.0	11.0	9.0	7.9	12.1	1.4	10.5	3.6
Pre-Avg. Border	10.0	11.0	8.8	8.0	12.6	1.3	10.8	3.9
B. Unemployed Individuals (% of population)								
Diff. in Diff.	-0.0552	0.0188	-0.0295	0.0684	-0.225	0.0680	-0.387**	-0.490***
	(0.114)	(0.107)	(0.105)	(0.102)	(0.236)	(0.0733)	(0.179)	(0.139)
Pre-Avg. NC	0.6	0.6	0.6	0.5	2.2	0.2	1.4	0.9
Pre-Avg. Border	0.5	0.5	0.5	0.5	2.0	0.2	1.2	0.7
C. Employed or Unemployed Individuals (Labor Force, % of population)								
Diff. in Diff.	0.0592	-0.00165	-0.437	1.261***	0.140	-0.111	-0.795	0.0316
	(0.467)	(0.483)	(0.435)	(0.419)	(0.565)	(0.204)	(0.511)	(0.342)
Pre-Avg. NC	10.6	11.6	9.6	8.4	14.3	1.6	11.9	4.5
Pre-Avg. Border	10.5	11.4	9.3	8.5	14.5	1.5	12.1	4.6
State Trends	yes	yes	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes	yes	yes
Obs.	425978	425978	425978	425978	425978	425978	425978	425978

Notes: The table reports difference in difference coefficients measuring differential percentage point changes within conditioning on marital status × gender × number of children binds (at least one child/no child). The table is constructed in analogy to Table 4 and the included control variables are the same as in Table 3 except that we exclude the fixed effects for the demographic group we are conditioning on. Due to the much narrower demographic groups we do not include industry or occupation fixed effects. The number of observations is the unweighted population in our sample summed over all states and months. Robust standard errors are reported in parentheses below each coefficient. Significance levels are indicated by * $p < 0.1$, ** $p < 0.05$, and *** $p < 0.01$.

the end of the UI payments on the long-term unemployed. Proponents of ending the payments claimed that the long-term unemployed were using the payments as a substitute for wages and would therefore be inclined to take employment. Opponents of ending the payments suggested that the long-term unemployed would instead be pushed out of the labor force and into the shadow economy. Proponents suggested that ending payments would spur growth in the labor markets and therefore help with the economic recovery after the Great Recession. Opponents suggested that ending payments would remove a vital line of support for the long-term unemployed with potentially negative effects on overall consumer spending, thereby slowing down the recovery

from the Great Recession.

The academic debate can be informed, however, from a natural experiment in North Carolina which ended federally financed long-term UI payments in July 2013 because of state specific legislation passed earlier in that year. While North Carolina ended its payments in July the remaining 49 states did not. We take advantage of this natural experiment to estimate the causal impact of ending long-term UI payments on various labor market outcomes in North Carolina.

We use a difference-in-differences approach where North Carolina is considered the "treated" state and the border states of North Carolina (Georgia, South Carolina, Tennessee, and Virginia) are considered the "control" states. Based on individual-level data from the U.S. Current Population Survey, we find that the end of the long-term UI payments reduced the unemployment rate by approximately one percentage point in North Carolina, that the majority of this reduction came from a reduction in the long-term unemployed in that state, that there was an increase in the number of employed, a reduction in the number of unemployed, and no change in the size of the labor force.

At least in the case of North Carolina, the end of long-term UI payments seems to have impacted the long-term unemployed, as intended, and to have encouraged many to seek employment, again as intended. However, our results also indicate that the majority of additional employment was concentrated in part-time employment and among particular demographic groups, most notably mothers. Interestingly, the results suggest that married mothers appear to have joined the labor force, while single mothers likely left unemployment to go to work. To the extent that North Carolina is representative for the nation at large, the end of long-term UI payments nationwide might be expected to have had similar effects.

References

Acemoglu D, Autor D. 2011. *Skills, Tasks and Technologies: Implications for Employment and Earnings*, volume 4 of *Handbook of Labor Economics*, chapter 12. Elsevier, 1043–1171. URL http://ideas.repec.org/h/eee/labchp/5-12.html

Autor DH, Levy F, Murnane RJ. 2003. The skill content of recent technological change: An empirical exploration. *The Quarterly Journal of Economics* **118**: 1279–1333. URL http://ideas.repec.org/a/tpr/qjecon/v118y2003i4p1279-1333.html

Card D, Krueger AB. 1994. Minimum Wages and Employment: A Case Study of the Fast-Food Industry in New Jersey and Pennsylvania. *American Economic Review* **84**: 772–93. URL http://ideas.repec.org/a/aea/aecrev/v84y1994i4p772-93.html

Card D, Levine PB. 2000. Extended benefits and the duration of UI spells: evidence from the New Jersey extended benefit program. *Journal of Public Economics* **78**: 107–138.
URL http://ideas.repec.org/a/eee/pubeco/v78y2000i1-2p107-138.html

Farber HS, Rothstein J, Valletta RG. 2015. The Effect of Extended Unemployment Insurance Benefits: Evidence from the 2012-2013 Phase-Out. *American Economic Review* **105**: 171–76.
URL https://ideas.repec.org/a/aea/aecrev/v105y2015i5p171-76.html

Farber HS, Valletta RG. 2015. Do extended unemployment benefits lengthen unemployment spells? evidence from recent cycles in the us labor market. *Journal of Human Resources* **50**: 873–909.

Hagedorn M, Karahan F, Manovskii I, Mitman K. 2013. Unemployment benefits and unemployment in the great recession: The role of macro effects. Working Paper 19499, National Bureau of Economic Research.
URL http://www.nber.org/papers/w19499

Ham JC, Rea J Samuel A. 1987. Unemployment Insurance and Male Unemployment Duration in Canada. *Journal of Labor Economics, University of Chicago Press* **5**: 325–53.
URL http://ideas.repec.org/a/ucp/jlabec/v5y1987i3p325-53.html

Hoynes H, Miller DL, Schaller J. 2012. Who Suffers during Recessions? *Journal of Economic Perspectives* **26**: 27–48.
URL http://ideas.repec.org/a/aea/jecper/v26y2012i3p27-48.html

Katz LF, Meyer BD. 1990. The impact of the potential duration of unemployment benefits on the duration of unemployment. *Journal of Public Economics* **41**: 45–72.
URL http://ideas.repec.org/a/eee/pubeco/v41y1990i1p45-72.html

Mukoyama T, Sahin A. 2009. Why did the average duration of unemployment become so much longer? *Journal of Monetary Economics* **56**: 200–209.
URL http://ideas.repec.org/a/eee/moneco/v56y2009i2p200-209.html

North Carolina Department of Commerce. 2015. North carolina pays off unemployment debt to federal government. Governor's Press Office Press Release 5/5/2015.
URL http://www.nccommerce.com/news/press-releases?udt_4733_param_detail=189400

Rothstein J. 2011. Unemployment Insurance and Job Search in the Great Recession. *Brookings Papers on Economic Activity* **43**: 143–213.
URL https://ideas.repec.org/a/bin/bpeajo/v43y2011i2011-02p143-213.html

Winter-Ebmer R. 1998. Potential Unemployment Benefit Duration and Spell Length: Lessons from a Quasi-Experiment in Austria. *Oxford Bulletin of Economics and Statistics* **60**: 33–45.
URL http://ideas.repec.org/a/bla/obuest/v60y1998i1p33-45.html