North Carolina's Employment Record:

What role did Unemployment Insurance Reform play?

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Abstract:

In 2013, the state of North Carolina reformed its state Unemployment Insurance (UI) program by reducing the maximum number of payment weeks and reducing the maximum weekly insurance payment. This paper investigates two hypotheses associated with such reform. The first hypothesis is a moral-hazard argument: limiting or removing UI payments should boost job search effort by the unemployed and lead to more rapid return to employment. The second hypothesis is a labor-force participation incentive: given the job-search requirement for receiving UI payments, limiting or removing those payments should reduce those actively unemployed and increase those who exit the labor force.

I use panel data created from the Current Population Survey to create time-varying transition probabilities between employed, unemployed and non-participating labor status for individuals aged 25-54. I test the difference in these transition probabilities for North Carolina against those for the rest of the US. Using an event study based on the implementation of UI reform in North Carolina, I find no evidence for the moral-hazard hypothesis but strong evidence of exit from the labor force in North Carolina associated with the UI reform. Simulations calibrated to historical North Carolina labor force statistics indicate that 80,000 workers transitioned from unemployment to non-participation at the time of the reform, while employment creation for the unemployed underperformed that in the rest of the US.

I also consider the impact of this reform on employer behavior through analysis of job vacancies recorded by Help Wanted Online. The reform coincided with negative growth in help-wanted ads in North Carolina: this performance is a negative outlier in comparisons with the job-creation record in other United States. There is no indication that the reform triggered an increase in job creation.

Keywords: Unemployment Insurance, labor force participation, job creation, job-market transitions

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North Carolina's labor force has experienced a turbulent past decade. The turbulence began with the financial crisis of 2007/2008: unemployment and the unemployment rate surged in that year and the years to follow to a level last seen during the Great Depression. In 2011, with the election of a Republican majority in both houses of the state legislature, further changes were put in motion. In February 2013, the legislature passed a sweeping reform of the unemployment insurance program. That reform reduced the maximum weekly insurance payment and reduced the maximum number of weeks for which the unemployed can appeal for insurance payment. The reform became law on 1 July 2013.

The rapid fall in the unemployment rate in North Carolina in the months after the reform was taken as evidence that reducing the payments for unemployment insurance led more individuals to seek work. Those who disagreed attributed the fall in the unemployment rate to another factor – a decision by a larger share of the working-age population to cease participation in the labor force. This distinction has important real-world consequences. If removing unemployment insurance leads to greater job search and job creation, then the reform has a positive effect on labor-force outcomes. By contrast, if removing unemployment insurance leads to a reduction in labor force participation, there is no positive effect – the reduction in the unemployment rate simply masks the loss of work for the state's residents.

This is a question of individual choice. The individual worker chooses whether to work, to search for worker, or to leave the labor force. The employer chooses whether to advertise a new position, to lay off workers, or to retain the same-sized labor force. For evidence I turn to the Current Population Survey (CPS) with its household-level information about the decisions taken by those of working age.[1] I consider the entire monthly record from 2000 to 2017, with behavior after the job-market reform (or after its announcement) taken as an event to be studied for its impact. Individuals between the ages of 25 and 54 in the rest of the United States are taken as the control group and individuals of those ages in North Carolina as the treatment group.

[1] In using the CPS database to create a panel of respondent behavioral choices in this way I follow the lead of Kroft et al. (2016). Thanks to those authors, and especially Matt Notowidigdo, for sharing their expertise in this technique.

There are two results of note from this analysis. First, there is no evidence that the unemployment insurance reform caused a significant move from unemployment to employment among North Carolina residents. Second, there is a significant increase in transition from unemployment to non-participation in North Carolina relative to the rest of the United States at the time of the reform.

The first section describes the unemployment insurance reform undertaken in North Carolina in 2013. In the second section I report results from academic studies of the impact of UI program reforms, both reductions and extension, in the recent past. In the third section I provide aggregate statistics on labor market outcomes in North Carolina and the US as a whole to illustrate the facts motivating the two sides of this debate. In the fourth section I describe the gross labor flow data derived from the CPS. I use a nonlinear smoothing estimator to illustrate its properties and perform a series of difference-in-difference tests of the hypotheses. In the fifth section I create a simulation model for the period 2012-2014 based upon the estimated transition probabilities and calibrated to historical North Carolina labor-market aggregates. The sixth section concludes.

1. The Unemployment Insurance Reform in North Carolina.

The unemployment insurance system (UI) is a partnership between the Federal government and the individual state to support people who have lost their jobs by temporarily replacing part of their wages while they look for work. Both Federal and state UI taxes collected from employers are paid into the system to provide income support if workers lose their jobs through no fault of their own. Although states are subject to a few Federal requirements, they are generally able to set their own eligibility criteria and benefit levels. States are also able to borrow from the Federal UI Trust Fund in times of recession if their own reserves are exhausted by large and continuing payouts.

In addition to the basic benefits, states and the Federal government typically provide an additional 13 or 20 weeks of extended benefit (EB) compensation to jobless workers who have exhausted their regular benefits in states where the unemployment situation has worsened dramatically regardless of whether the national economy is in recession. The total number of weeks available depends on a state's unemployment rate and its UI laws. Normally the Federal government and the states split the cost of EB compensation, but the 2009 Recovery Act authorized temporary full Federal funding, which continued through 2013. The Federal government can also authorize temporary extensions of benefits beyond the time period covered by EB during deep recessions. The cost of these temporary extensions are borne by the Federal government. The Emergency Unemployment Compensation (EUC) program was the most recent of these temporary programs and was in place from June 2008 to December 2013.

Once the financial-crisis downturn began in 2007, North Carolina rapidly used up the balance in its North Carolina Unemployment Insurance Trust Fund. It borrowed from the Federal UI Trust Fund to continue making UI payments. The Republican majorities in both legislative houses passed a sweeping reform to the UI law in North Carolina in early 2013, and North Carolina governor Pat McCrory signed the bill into law on 19 February 2013.[2] The reform became law on 1 July 2013.

There were two salient changes to the UI payment policy. First, maximum weekly benefits were cut from $530 to $350. Second, the number of weeks for which recipients are eligible for benefits fell from 26 (or up to 99, with Federal extensions) to 20. This eligibility limit was linked to the unemployment rate, so that as the unemployment rate fell the eligibility limit fell as well: it became 12 weeks in the last half of 2015. These reforms were so sweeping that the US Department of Labor ruled that the UI system in North Carolina had been qualitatively altered; it cut off the access of North Carolina residents to EUC payments as of 1 July 2013. The UI tax on employers at the same time was raised by 0.0006 (sixth-tenths of one percent).

While nine states reformed their UI programs during the financial-crisis downturn, North Carolina's reform was the only one of the nine to lose EUC and EB benefits for its residents (GAO, 2015). EUC benefits disappeared on 1 July 2013 in North Carolina; for the rest of the US they were eliminated as of 31 December 2013.

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[2] Jen Wilson: "NC Gov. Pat McCrory signs Unemployment Insurance Reform Bill", Charlotte Business Journal, 19 February 2013.

The reform of the UI program is only one of the policy initiatives taken by the state of North Carolina. The corporate income tax in early 2010 was 6.9 percent, but is currently 3 percent (falling further to 2.5 percent in January 2019). The UI tax on employers in North Carolina rose slightly with the reform but has been reduced in steps since then.

2. Academic Research on Labor Market Effects of UI and this paper's contribution.

Shavell and Weiss (1979) is an early theoretical consideration of UI program characteristics that identifies the tension inherent in the program. As insurance, it should provide an alternative source of income for those who lose their jobs for no fault of their own. However, its existence can change workers' behavior through moral hazard – it provides an incentive that the unemployed worker search less hard than she would in the absence of the program. They conclude that the optimal UI sequence of benefits will be declining over time to counteract that incentive. More recently, Mitman and Rabinovich (2015, 2018) have reexamined the question of optimal UI benefits in a general-equilibrium model of labor-market equilibrium. In a search-matching model calibrated to the US labor market, they conclude that both benefit levels and benefit duration should be pro-cyclical – i.e., smaller during recessions. They also conclude that the extended-benefit programs of the last decade contribute to the observed jobless recovery from the 2007/2008 recession.

The preceding theoretical approaches considered just two labor outcomes for the worker: employed or unemployed. A third outcome, non-participation in the labor force, is also possible – and as Elsby, et al. (2015) have noted it is an important component of labor-market volatility in the last decade. When non-participation is considered, UI programs provide a different incentive to workers: their requirement that the worker continue searching for a job while receiving UI benefits can lead to greater labor-market participation and thus more successful job search for the unemployed. Rothstein (2011) investigated the impact of extended UI benefits on both of these incentives – the hypothesized moral-hazard reduction in search intensity while unemployed and the hypothesized increase in search intensity receiving UI payments and not exiting the labor force. He concluded that the UI benefit extensions in the US led to an increase in the unemployment rate in early 2011 of only 0.5 to 1 percentage point, with at least half of this effect due to reduced exit from the labor force.

Farber, Rothstein and Valletta (2015, hereafter FRV) use the evidence from the end of the EUC and EB programs nationwide in 2013 to evaluate the impact of UI programs on unemployment and labor force participation. As they state in that article, “the phasing out of extended and emergency benefits reduced the unemployment rate mainly by moving people out of the labor force rather than by increasing the job-finding rate.”

Hagedorn, Manovskii and Mitman (2015, hereafter HMM) reach a conclusion diametrically opposed to that of FRV using a different research strategy. FRV used panel data from households derived from the Current Population Survey (CPS). HMM used county-level data for 2014 from the Local Area Unemployment Statistics survey. The UI programs at the state level had between 0 and 47 weeks of UI benefits remaining when the Federal government terminated them in December 2013, and the authors used the difference in expected but lost benefits across states to identify the impact on households of that policy reform. They compared employment growth in matched counties on either side of a state border, and concluded that “2.1 million individuals secured employment in 2014 due to the benefit cut. More than 1.1 million of these workers would not have participated in the labor market had benefit extensions been reauthorized.” Thus, the benefits were associated with both flows from unemployment to employment and from non-participation (or migration) to employment – contrary to the conclusions of FRV. Hagedorn, Karahan, Manovskii and Mitman (2016, hereafter HKMM) use the same research design as HMM and reach the same conclusions on employment and labor force effects. In addition, they find a strong reduction in listed vacancies by firms on average in counties with UI extensions. Boone, Dube, Goodman, and Kaplan (2016) contest these results. They find near zero and insignificant effects of UI extensions on employment using a county border design and a more flexible empirical model. They further show that using newer vintages of the unemployment data substantially reduces or eliminates the positive effect of benefit extensions on unemployment found in HMM and in HKMM.

There have been relatively few state-level analyses of UI reforms. The Government Accountability Office (2015) documented the reforms taken by nine states, including North Carolina, during the recent recession. These reforms were generally reductions in the maximum duration of UI benefits, although the extent of the reduction differed by state. Hagedorn et al. (2014) examined the 1 July 2013 reform in North Carolina as a "treatment" event, and looked for differences from behavior in "control" states such as South Carolina and Georgia. (These were also states that had reformed their unemployment-insurance policy, although not in as sweeping a manner as in North Carolina.) The analysis is done with aggregate state-level statistics, and finds employment growth in the aftermath of the reform. The indications on labor force response are inconsistent across data sources, with one indicating an increase in labor force and another indicating a reduction.

The recent paper most closely targeted at the question of this paper is the study by Johnston and Mas (forthcoming 2018) of labor-force responses to the unexpected reduction in UI benefits observed in Missouri in 2011. The authors use administrative data to track those benefit recipients directly affected by the reduction. They use a regression discontinuity design to estimate a marginal effect of maximum duration on UI receipt of 0.45 – in other words, reducing the maximum potential duration of UI for an individual by 10 weeks will reduce the expected duration of the individual in UI by 4.5 weeks. There is no such response for individuals who had exhausted their benefits. In addition, re-employment rates of the long-term unemployed are similar before and after the UI duration reduction in Missouri.

I have two research questions to address in this paper:

(1) Did the UI reform in North Carolina cause an increase in the rate at which unemployed workers became employed? This will follow from the reduced moral hazard associated with the Shavell/Weiss theoretical argument.

(2) Did the UI reform in North Carolina cause an increase in the rate at which unemployed workers left the labor force? This will follow from reduced incentive to continue job search, as in Rothstein's argument.

Given the questions, I use an empirical research design based upon measures of gross labor flows between three labor outcomes – employed (E), unemployed (U) and non-participant in the labor market (N). Labor market outcomes are measured for the US labor market on a monthly basis from January 2000 to March 2017. North Carolina residents are the treatment group, while residents in the rest of the US serve as controls. The introduction of the UI reform on 1 July 2013 defines an event. My null hypothesis is that the probability of transition among labor outcomes each month is identically distributed across US states, but that the probability of transition can and will change from year to year (as, for example, following the business cycle). The alternative hypothesis that I test is that North Carolina differed significantly from the other states in its transition probabilities from unemployment to either employment or non-participation during the months immediately following this unemployment-insurance reform. A second alternative hypothesis tests whether these significant differences are found as "announcement effects" around the signature of the UI reform law in February 2013.

The data I use for these hypothesis tests come from the Current Population Survey (CPS), as did the data used by Elsby et al. (2015), Rothstein (2011) and Farber, Rothstein and Valletta (2015). These data have the advantage of measuring directly individual labor-market outcomes; this is in contrast to HHM, HHKM, and Hagedorn et al. (2014) that work with county-level or state-level aggregations. The CPS data have the disadvantage, however, of suspected misclassifications in individuals' responses to the survey. I address those misclassifications and my controls for them in section 4. The results of the hypothesis tests are found in section 4 as well, after a general empirical review of labor-market outcomes in North Carolina and the US in section 3.

3. Labor market evolution in North Carolina

In this section I illustrate the features of North Carolina's labor-market evolution since January 2000 through reference to the US record.[3] Figure 1 reports the growth in employment in the US as a whole and in North Carolina. The US faced two labor-market downturns since 2000. The first, associated with the "dot com" bubble in the stock market, occurred in 2001-2002. The second, associated with the financial crisis, began in 2007. The dot-com downturn was relatively minor: the number employed began growing again in January 2002. The financial-crisis downturn, by contrast, was severe. It began in 2007 but did not reach its low point until December 2009. Since the beginning of 2010, the number of employed US residents aged 18 or older has been growing. The red line in Figure 1 illustrates these developments for the US.

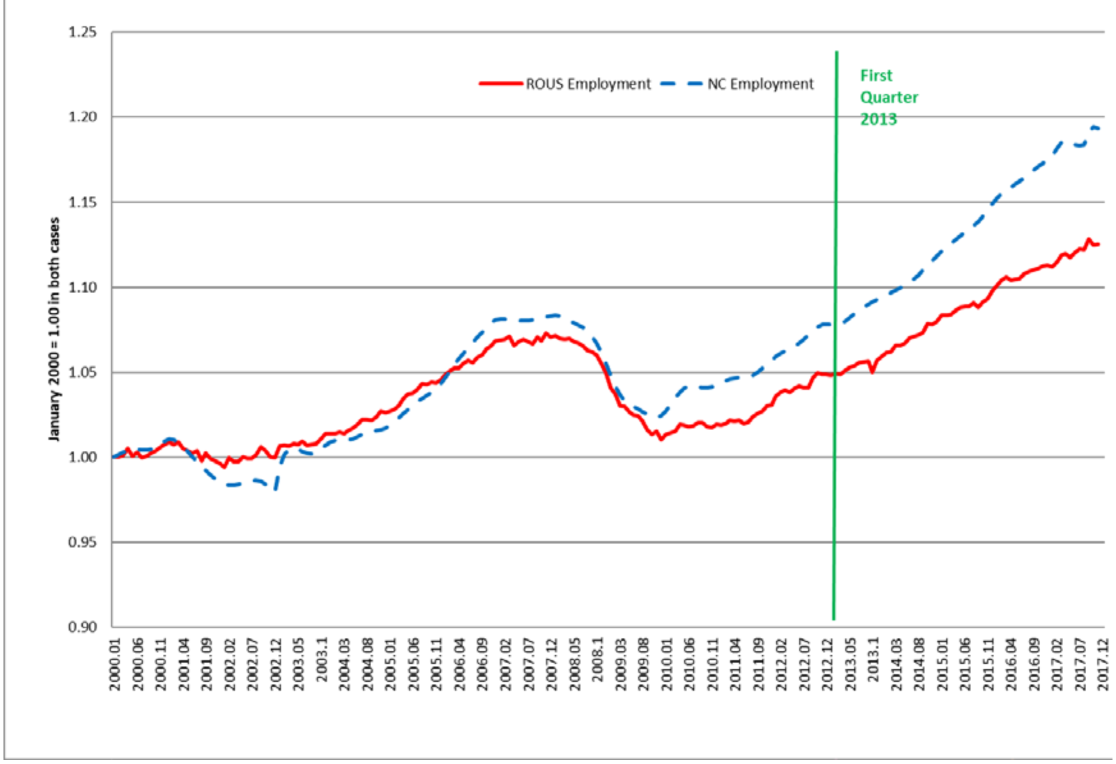


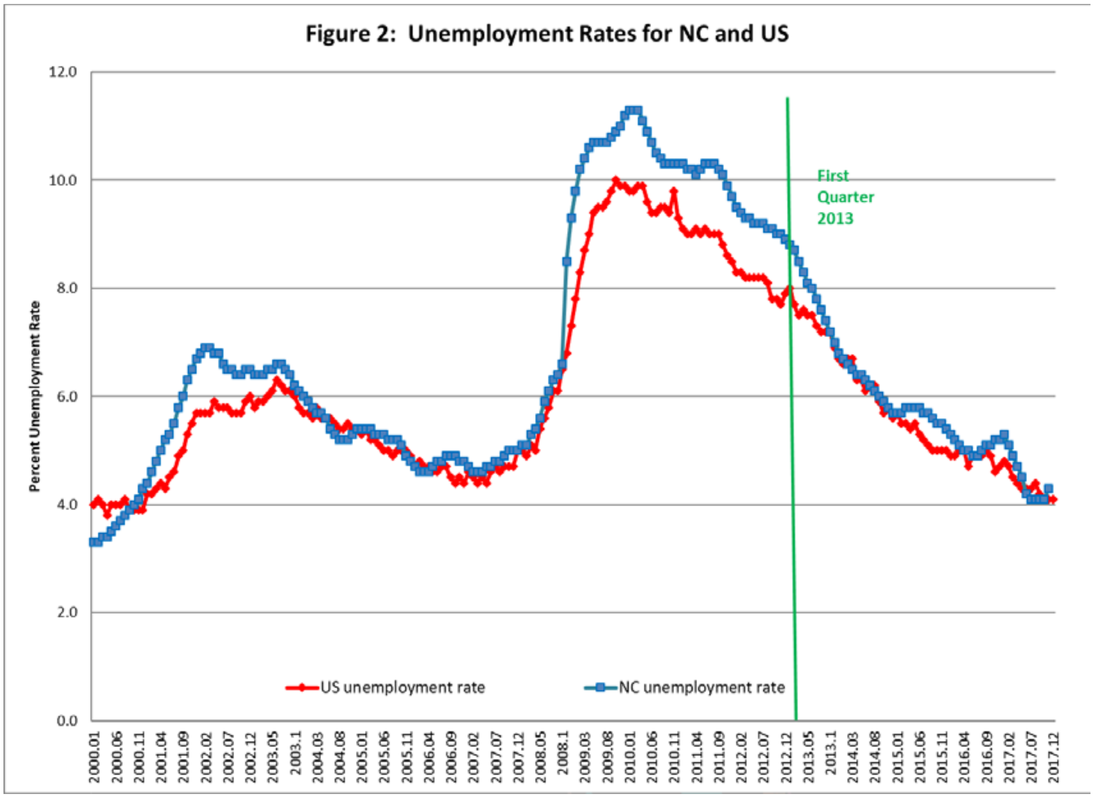
Figure 1: Evolution of Employment: US and NC

[3] In this section I use the data of the Local Area Unemployment Statistics (LAUS) database of the Bureau of Labor Statistics (BLS) in seasonally adjusted form. These data of the working-age population include all individuals aged 18 and older.

North Carolina has gone through a very similar pattern of employment growth, as is evidenced by the blue dashed line in Figure 1. North Carolina's reduction in employment was more pronounced than that for the US during the dot-com downturn, but by 2006 the state had reversed that relative drop. From the beginning of the financial-crisis downturn, North Carolina has exhibited more rapid employment growth over the 2009-2017 period.

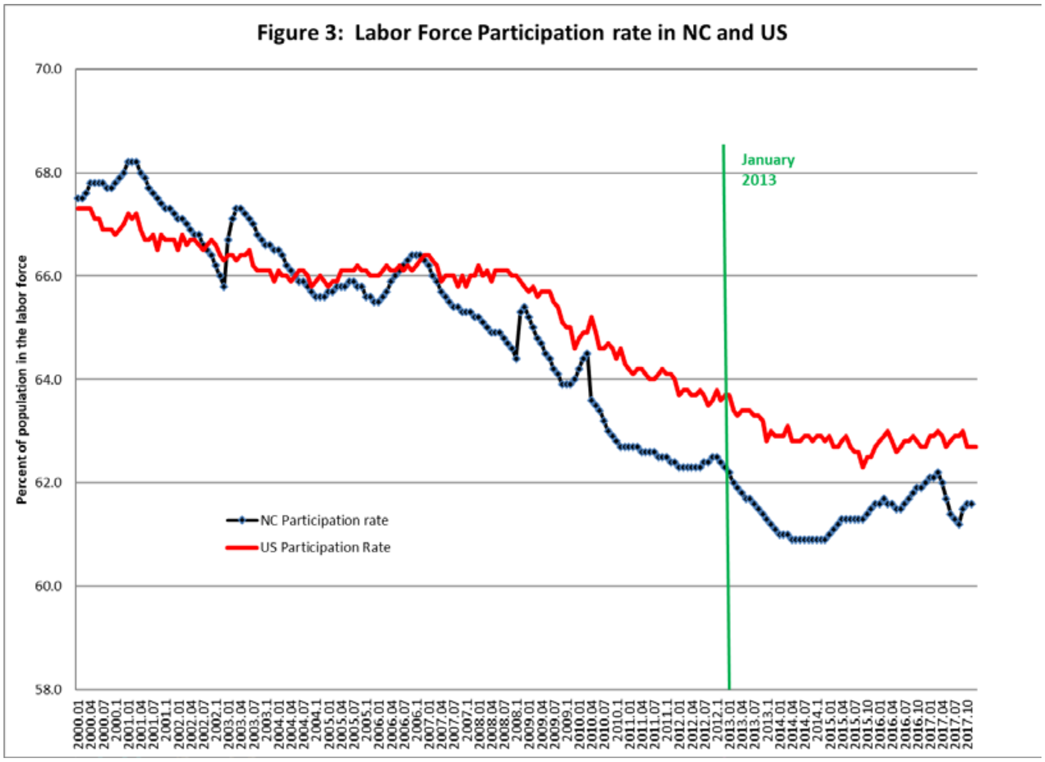
A cumulative six percentage-point higher employment growth for NC than in the US as a whole since January 2000 (or March 2009) did not keep up with the more rapid working-age population growth rate of North Carolina when compared with the US as a whole. The populations of the US and North Carolina grew at roughly the same rate from 2000 to 2004, but after that time the North Carolina population grew more rapidly. By January 2017, the NC working-age population had grown cumulatively nine percent more than the US. While employment in North Carolina grew more quickly than in the US as a whole, that growth was not sufficient to offset the even-more-rapid growth in working-age population. This in-migration to North Carolina makes simple calculations of employment growth over time an inaccurate indicator of the individual's labor-market choice.

The unemployment rates in North Carolina and the US diverged during the recession, but later converged and fell towards 4 percent by the end of 2017. In Figure 2 I illustrate the evolution of the unemployment rate since 2000. The NC unemployment rate (blue line) rose above the US rate (red line) during both downturns: slightly during the dot-com downturn, and more significantly during the financial-crisis downturn. From November 2008 to August 2013 the NC rate was above that of the US; since August 2013, however, the two have been quite similar. While employment growth in North Carolina has not kept up with the state's population growth, North Carolina's unemployment rate is nearly identical with that of the US as a whole.



One explanation for this contradiction is found in the percent of people of working age who are either employed or actively seeking work (the labor force participation rate, or LFPR). This percent has been declining steadily over time in the US as a whole, in part because of demographic shifts toward a greater percentage of residents entering retirement age. Figure 3 illustrates the LFPR for the US as a whole and for North Carolina. While the two ratios differed from month to month, they followed a similar trajectory through December 2006: each ratio fell by about one percentage point from January 2000 through that time. Beginning with January 2007, the two series diverged with North Carolina's ratio falling at a more rapid rate.

Between January 2007 and November 2017, the participation rate in North Carolina fell by 4.6 percentage points, while the US participation rate fell by 3.7 percentage points. The lowest labor-force participation rate in this period for North Carolina came in early 2014; at that time, the gap between US and NC rates was 2.1 percentage points. A fall in the labor force participation rate is an indication that individuals of working age have chosen not to seek out work. One explanation is that of the "discouraged worker": someone who sought work for some time, had no success, and decided to stop looking.



This helps to explain the evolution of the unemployment rate. The unemployment rate is calculated as the number of those actively seeking work divided by the number of individuals in the labor force. Since the discouraged worker is excluded from the numerator and denominator, the unemployment rate is reduced by decreases in the labor force participation rate due to discouraged workers.

There is little evidence in Figures 1 through 3 that the reform on 1 July 2013 (or even, if we believe that workers anticipated the effect of the reform in earlier months, looking back to 1 January 2013) had any differential effect on employment in North Carolina. Figure 1 describes the evolution of employment in North Carolina and in the US – there is no evidence of a differential effect in North Carolina at that time. Figure 2 illustrates an unemployment rate in North Carolina falling more rapidly than in the US as a whole, but that more rapid decline is a continuation of a trend that dates back to early 2010. In Figure 3, the labor participation rate does exhibit a decline more rapid than that observed in the rest of the US at the time of the reform.

A cursory view of these data series thus suggests that employment behavior is unchanged but the labor force participation is substantially different. However, it is difficult to draw conclusions based on aggregate data such as these. To evaluate more precisely, I turn to the evidence found in the CPS.

4. Labor-force choices as highlighted in the Current Population Survey.

The question at hand is really one of worker choice and opportunity: do workers choose to participate in the labor force, and if so do they have the opportunity to take a job? To address this, I have collected employment information from household respondents to the Current Population Survey (CPS) by month from January 2000 to October 2017 inclusive. The CPS queries household respondents about the labor-market behavior of each working-age adult in the household for four consecutive months. Then, after a break of eight months, the CPS queries the household for an additional four months.

A panel of data is created by matching observations across successive months by household identifier, line number (in the survey questionnaire), age, gender and race. I retain in the data panel all examples of two consecutive non-overlapping months of responses by individuals aged 25 through 54 in these households.[4] For each household, there are at most four of these pairs.[5] This data panel is used to estimate the historical transition probabilities into and out of employment (E), unemployment (U) and non-participation (N) status for each worker.

The aggregate data of the last section on employment, unemployment rate and labor force participation rate were calculated for the working-age population aged 18 and above. The hypothesis tests of this paper, though, have to do with decision-making by individuals of prime working age. To put focus on these individuals, the CPS database is filtered to include only individuals between the ages of 25 and 54.

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[4] I also use a three-month window of responses from the same households for comparable robustness checks that a labor transition continues for two periods after the transition. Those results will be reported in part C of this section.

[5] Consider a household that responds to the survey in January, February, March and April of 2015. The household then takes eight months off and returns to respond to the survey again in January, February, March and April of 2016. The four pairs are (1) – January and February 2015; (2) – March and April 2015; (3) – January and February 2016; (4) – March and April 2016.

Table 1: Average Number of Observations per Quarter in CPS Panel (age 25-54)

A. Rest of the US

	Total	Employed	Unemployed	Non-participating
2000	66275	54347	1686	10243
2001	71063	57796	2210	11057
2002	77158	62196	2879	12083
2003	75951	60702	2993	12256
2004	73704	59051	2612	12041
2005	72534	58357	2365	11812
2006	71392	57770	2116	11506
2007	70382	56963	2106	11313
2008	69680	55852	2601	11227
2009	70583	54374	4570	11640
2010	69650	53316	4597	11737
2011	67969	52017	4099	11853
2012	66774	51489	3585	11699
2013	65442	50601	3140	11701
2014	64630	50293	2600	11736
2015	62553	48802	2211	11541
2016	61713	48425	2047	11241
2017	60084	47676	1766	10642

B. North Carolina

	Total	Employed	Unemployed	Non-participating
2000	1743	1445	36	262
2001	1665	1355	53	257
2002	1636	1284	72	280
2003	1645	1290	67	288
2004	1591	1263	52	276
2005	1508	1206	47	255
2006	1433	1154	43	236
2007	1376	1106	39	231
2008	1416	1105	66	245
2009	1360	1063	46	251
2010	1403	1045	108	250
2011	1320	987	98	235
2012	1360	1036	80	244
2013	1344	1021	80	243
2014	1341	1032	53	256
2015	1429	1087	57	285
2016	1438	1114	51	273
2017	1370	1051	77	272

Table 1 reports the number of panel observations for the rest of the US (top) and for North Carolina (bottom) in the CPS data. The number reported in each case is the average number of quarterly observations for that year.

a. A non-parametric test of the UI reform hypotheses.

As a non-parametric test of the two hypotheses, I create smoothed series of the conditional probability of transition from labor status to labor status for both North Carolina (NC) and for the rest of the US (ROUS). A locally weighted least-squares regression of a univariate polynomial process is used to smooth the series over time.[6] I also aggregate the observations to quarterly cells to increase the number of observations in each cell; the individual observations remain month-to-month transitions. The results of this exercise for all nine labor transitions are illustrated in Appendix A. The smoothed series for ROUS is illustrated in red, while the series for North Carolina is pictured in blue. The quarter-by-quarter standard errors of the estimate of the NC transition probability are used to create a 95 percent level confidence interval, and that confidence interval is illustrated by the dotted blue lines.

The Shavell-Weiss moral-hazard hypothesis will be true if there is evidence that the transition from unemployment to employment is made more likely by the UI reform. In Figure 4 I illustrate the month-to-month transition probabilities from unemployment to employment for NC and ROUS over the time period 2001-2017. As is evident, the NC transition probability has been below that of the ROUS since the fourth quarter of 2001. The NC probability remained below that of ROUS in July 2013, and there is no evidence of a relatively larger increase in the transition probability coincident with the reform event. Based on this, I cannot reject the null hypothesis that UI reform had no independent effect on the probability of transition from unemployment to employment.

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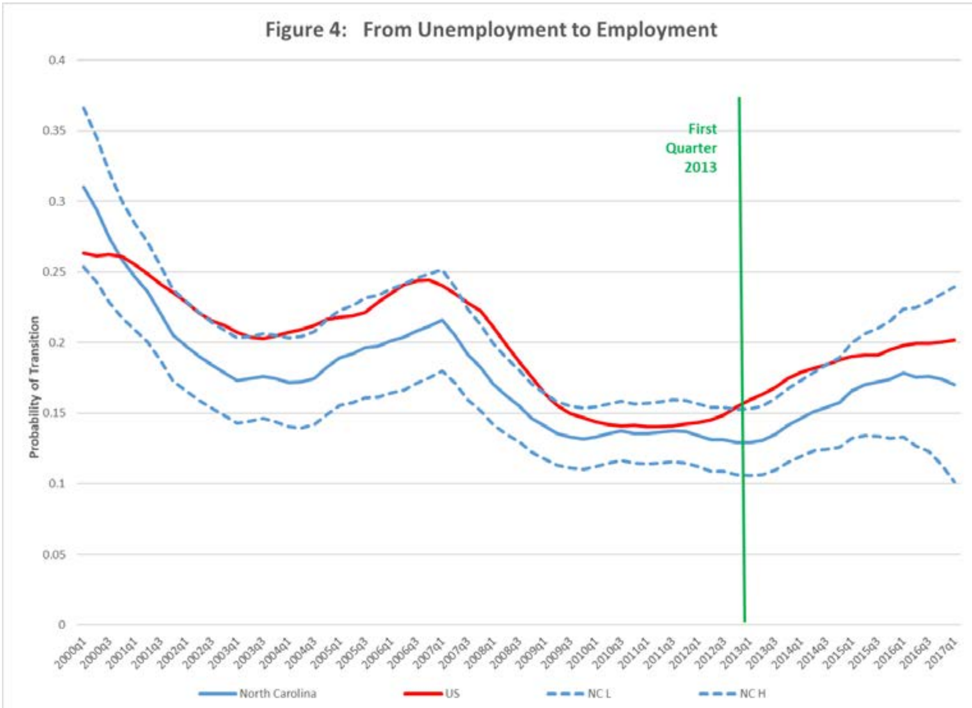
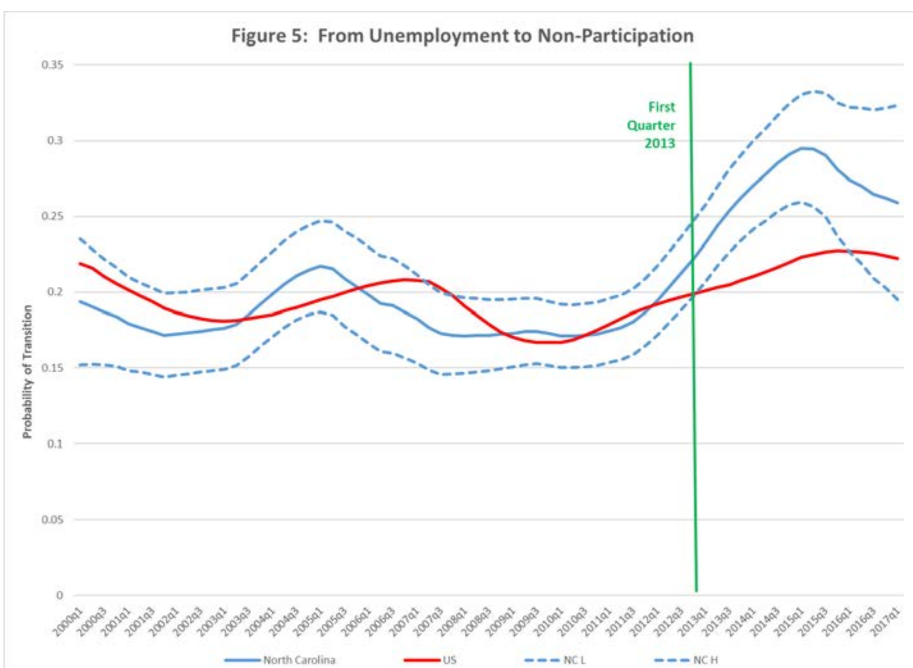
[6] I use the Stata command lpoly to create these average transition probabilities, with epanechnikov kernel, degree zero, and bandwidth chosen optimally by the program. The standard errors are derived from the conditional variance calculated for the locally weighted regression for each quarter in the sample.



The Rothstein hypothesis, if valid, will be evident in the transition from unemployment to non-participation. Figure 5 illustrates that transition for both NC and ROUS.



Given the 25-54 age group, this month-to-month transition probability should be small. NC and ROUS exhibited similar and fairly stable transition probabilities throughout the period 2000-2012, but in 2013 diverged due to a rapid rise in this transition probability in North Carolina. Beginning in 2013q1 the NC probability was significantly above that of ROUS, and remained so until 2016q1. In this instance, we can reject the null hypothesis that NC and ROUS followed a similar transition from unemployment to non-participation: NC's probability was significantly above that of ROUS.

I have redone these non-parametric calculations for all CPS respondents aged 18 and older. While there is a greater concentration of non-participants in that sample, the distinctions between NC and ROUS in terms of labor force transitions out of unemployment are robust to that specification.

The non-parametric technique used in this section is attractive for illustrating differences over time between the NC and ROUS, but it has drawbacks in testing the significance of UI reform effects. First, it is by design a smoothed estimator: it will smooth out sharp effects across time, and thus is unreliable in measuring the impact of an event like the UI reform. Second, the NC and ROUS non-parametric estimators are estimated separately; an efficient test of differences between the two series requires jointly estimated coefficients (and standard errors). In the next section I repeat these comparisons using joint estimation in an event-study framework.

b. A Joint Test of the Alternative Hypothesis.

The null hypothesis of this study is that individuals across the US at any point in time have identical probabilities of transition among labor status. The alternative hypothesis is that individuals in North Carolina will behave differently from those in ROUS after (a) the announcement (in 2013q1) or (b) the introduction (in 2013q3) of UI reform.

The data used are the individual labor participation responses from the CPS as described above. The data are separated into three subsets: those who are initially unemployed (U), those employed (E), and those not participating (N) in the first month. For each of these subsets, I run three regressions of the form:

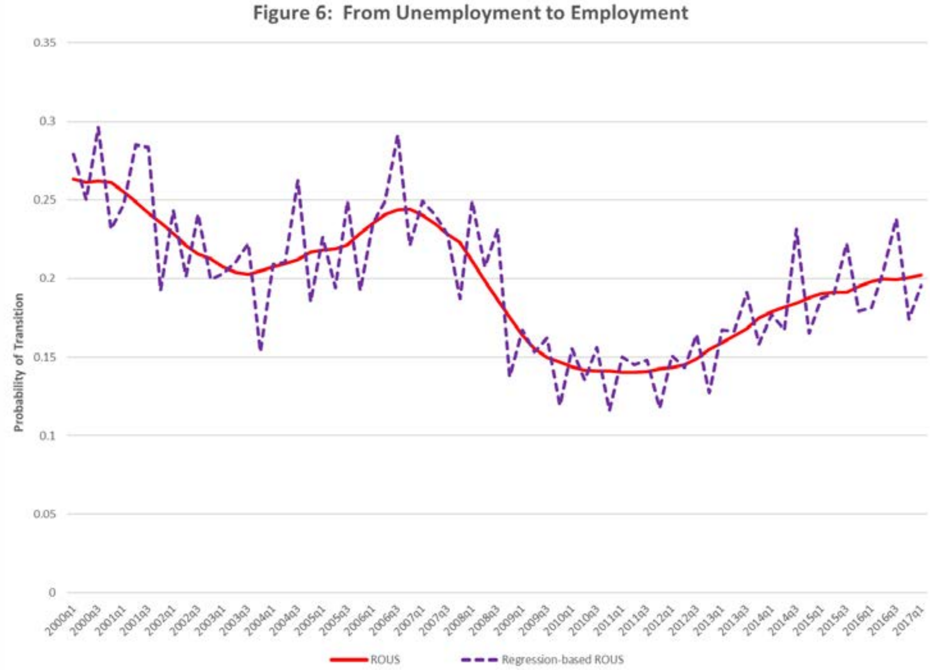
$$L_{ijkt} = \alpha_{jk} + \Sigma_{t=2}\, \beta_{jkt}\, I_t + \Sigma_t\, \gamma_{jkt}\, I_t * I_i^{NC} + \varepsilon_{ijkt} \qquad (1)$$

L_{ijkt} is the binary response to the question to the household: what labor status k (k = E, U, N) does individual i have this month, given that she had status j last month? I_t is a vector of binary variables taking the value one in quarter t and 0 otherwise. I_i^{NC} is a binary variable taking the value one if the household lives in North Carolina and zero otherwise. ε_{ijkt} is a zero-mean random variable that captures the other determinants of this household choice. In estimation, ε_{ijkt} is clustered by household.

The coefficient α_{jk} is the estimate of the conditional transition probability from j to k for the ROUS in the first quarter of 2000, while $\alpha_{jk} + \beta_{jkt}$ is the estimate of the conditional transition probability for the ROUS as a whole in quarter t. γ_{jkt} is the estimate of the deviation between the conditional probability for North Carolina and the conditional probability for the ROUS in quarter t.

The results of this estimation represent a benchmark of ROUS behavior very similar to that uncovered in the non-parametric regression of the earlier section. Figure 6 provides the example of the conditional probability of transition from unemployment to employment. The red line represents the smoothed series for the ROUS presented in the last section, while the purple line illustrates the estimated period-by-period transition probability for ROUS of this section.

Figure 6: From Unemployment to Employment



Regression results for the γ_{jkt} from equation (1) using the panel of responses for individuals aged 25-54 are given in Table 2.[7] s_{jkt} is the standard error for the coefficient, and z is the test statistic for the test that the coefficient is significantly different from zero. The cutoff z value for 95 percent level of confidence is 1.96 and for 90 percent level of confidence is 1.65. In what follows, I will use the 95 percent level for hypothesis testing. A full set of γ_{jkt} were included for all t, and the ones reported here test the hypotheses related to introduction of UI reform. The first variant of the hypothesis focuses on the implementation date of 1 July 2013. If UI reform had an implementation effect, it should be evident in the γ_{jkt} of 2013q3 and possibly 2013q4. If the announcement of UI reform in February 2013 had an additional effect, it should be evident in 2013q1 and possibly 2013q2. Overall goodness of fit in estimation is reported by the Wald statistic (or, in the weighted least squares case, by an F test).

- The Shavell/Weiss moral-hazard effect of UI reform should be to encourage more rapid transition from unemployment to employment. There is insignificant evidence of either announcement effect in 2013q1 or implementation effect in 2013q3. The announcement effect takes the expected positive sign, but is less than the negative effect in 2013q2. The estimates of implementation effects are insignificant and have a negative sign – the opposite of that anticipated by the moral-hazard hypothesis.
- The Rothstein effect of this UI reform will be evident in unemployed workers who transition into non-participation. There is little evidence of an announcement effect, but there are large significant implementation effects in 2013q3 and 2013q4. In the third quarter of 2013, the transition probability from U to N in North Carolina was 14 percentage points higher than in ROUS.
- There is an adding-up constraint across these coefficients of excess transition. Since the U-to-N effect is large and positive in 2013q3, the U-to-U and U-to-E effects together must be large and negative. The critical point for our test: in NC for that quarter, what little movement in U to E is

[7] Complete regression results, including all coefficients, standard errors and diagnostic tests, are available on demand.

observed is towards less employment than in ROUS. Those who leave unemployment or employment go into non-participation.

Table 2: Hypothesis Test whether NC differs from ROUS in the period of the UI Reform											
	γ_{jkt}	s_{jkt}	z		γ_{jkt}	s_{jkt}	Z		γ_{jkt}	s_{jkt}	z
	E to N				N to N				U to N		
2013q1	0.001	0.004	0.22		-0.020	0.020	0.91		0.036	0.040	0.90
2013q2	-0.005	0.004	1.25		-0.006	0.020	0.29		-0.004	0.044	0.10
2013q3	0.005	0.004	1.24		-0.018	0.021	0.83		**0.143**	0.046	3.07
2013q4	0.005	0.004	1.15		-0.007	0.020	0.35		**0.108**	0.047	2.27
	E to U				N to U				U to U		
2013q1	-0.002	0.004	0.54		0.015	0.013	1.20		-0.097	0.050	1.92
2013q2	-0.005	0.003	1.60		0.006	0.013	0.43		0.080	0.055	1.46
2013q3	-0.002	0.003	0.46		-0.023	0.014	1.65		-0.082	0.058	1.40
2013q4	-0.001	0.003	0.42		0.008	0.013	0.59		-0.108	0.060	1.81
	E to E				N to E				U to E		
2013q1	0.001	0.006	0.17		0.004	0.016	0.23		0.061	0.043	1.41
2013q2	**0.011**	0.005	1.98		-0.000	0.016	0.08		-0.080	0.047	1.69
2013q3	-0.004	0.005	0.70		**0.039**	0.017	2.25		-0.069	0.050	1.38
2013q4	-0.004	0.005	0.65		-0.000	0.016	0.02		-0.001	0.051	0.01
N Obs	3,969,815				840,957				204,181		
Avg Obs per quarter	55,136				11,680				2,836		
Wald (143)											
	522.3	E to N			**548.7**	N to N			**696.7**	U to N	
	1521.9	E to U			**1425.5**	N to U			**3018.0**	U to U	
	727.2	E to E			**989.8**	N to E			**2497.1**	U to E	

GLS estimation, with errors clustered by household.

While the hypothesis tests of this paper do not provide sharp predictions on transition out of the N or E states, it is useful to review the results for North Carolina in those regressions. For individuals who begin as employed, there is only one significant difference between North Carolina and ROUS behavior: in 2013q2, those in North Carolina are less likely to transition from employment to another state. For individuals who begin a month as “not in the labor force”, there is little evidence of an announcement effect. There was one significant effect when the reform became law: those beginning with N status in North

Carolina were significantly more likely to transition to E.[8] This estimation does not provide a rationale for this result, but I think that it will be productive to look for links between those who lose their UI payments unexpectedly and others of working age in their household who return to the labor force.

c. Robustness checks of the joint test.

This is a purely statistical test of the impact of UI reform, and so it is important to check that anomalies in the data are not causing a spurious significance effect. I consider four robustness checks in this section. First, I eliminate the possibility of cross-observation dependence by eliminating household observations that occur in successive months. Second, following Elsby et al. (2015) I redefine the transitions from one status to another in the spirit of their "deNUNification" approach. Third, I redo the analysis using the survey weights specified in the CPS. Fourth, I replace "rest of the US" as the baseline for the test with the Southern states. These changes in research design do not change the results of hypothesis tests, and in some cases make the test results sharper.

(1) **Eliminating successive household observations from the database.** The regressions of Table 2 are conducted on household choices that are not overlapping but which occur in immediate succession.[9] In Table 3 I investigate whether the results are different if I use only the transitions observed one year apart.

[8] The non-parametric regressions in Appendix A indicated the significance of these, and more. There are two reasons for divergence in the inference of the two methods. First, the non-parametric smoothed estimates were derived separately for ROUS and for NC; the standard errors for the NC estimates are used to construct the confidence intervals. These should be seen as conservative bands; the standard errors of coefficients in the regressions in this section are smaller than those used in the graphs because the NC and ROUS coefficients are jointly estimated. Second, the polynomial smoothing used in the graphs is useful in depicting medium term trends in variables, but is inappropriate for statistical tests of an event such as this UI reform. For both of these reasons, the inference from Table 2 is preferred.

[9] For example: a household that entered the CPS in January 2016 will potentially have its reports of January, February, March and April from 2016 used as data, as well as reports from January, February, March and April of 2017. (In the CPS nomenclature, January 2016 is mis (Months in Survey) = 1, February 2016 is mis = 2, March 2016 is mis=3, April 2016 is mis=4, January 2017 is mis=5, and so on. The joint test of Table 2 calculated the transitions from January to February and from March to April in each year for each individual in this household and treated the transitions as separate observations (albeit with errors clustered by households). Consider an individual who reports "U" in January, "U" in February, "N" in March and "N" in April 2016. The mis=1 transition in the database is UU, and the mis=3 transition in the database is NN. While they are temporally separate, it is valid to ask whether the NN decision is dependent upon the immediately prior UU history of the individual. To check for these concerns, in this

The coefficient estimates of Table 3 tell the same story as those of Table 2, with two additional coefficient estimates in unemployment transitions becoming significantly different from zero. The finding of significant positive U-to-N implementation effects is observed here as well, with coefficients magnified in size: this affirms the Rothstein effect. We also observe a significant negative coefficient in U-to-E transition that is opposite in sign to the Shavell-Weiss hypothesis. For those beginning out of the labor force, there is an significant increase in the N-to-E transition probability with UI reform implementation.

Table 3: Hypothesis Test whether NC differs from ROUS in the period of the UI Reform												
	γ_{jkt}	s_{jkt}	z		γ_{jkt}	s_{jkt}	Z		γ_{jkt}	s_{jkt}	z
	E to N				N to N				U to N		
2013q1	-0.000	0.006	0.06		0.006	0.029	0.19		0.024	0.055	0.44
2013q2	-0.002	0.006	0.35		-0.026	0.030	0.90		0.014	0.065	0.21
2013q3	0.009	0.006	1.40		-0.023	0.031	0.74		**0.194**	0.066	2.93
2013q4	0.005	0.006	0.75		-0.036	0.028	1.27		0.101	0.069	1.45
	E to U				N to U				U to U		
2013q1	0.004	0.005	0.82		0.006	0.018	0.30		-0.100	0.067	1.47
2013q2	-0.005	0.005	1.04		0.011	0.019	0.56		0.155	0.080	1.92
2013q3	-0.000	0.005	0.08		**-0.040**	0.020	2.00		-0.130	0.082	1.59
2013q4	0.000	0.005	0.01		0.025	0.018	1.38		-0.062	0.086	0.72
	E to E				N to E				U to E		
2013q1	-0.005	0.008	0.57		-0.015	0.023	0.63		0.075	0.058	1.30
2013q2	0.007	0.008	0.92		0.013	0.024	0.53		**-0.178**	0.069	2.59
2013q3	-0.008	0.008	1.08		**0.064**	0.025	2.55		-0.058	0.070	0.83
2013q4	-0.005	0.008	0.61		-0.009	0.022	0.39		-0.036	0.074	0.49
N Obs			1,947,697				405,616				103,438
Avg Obs per period			27,051				5,634				1,437
Wald (143)											
	383.6	E to N			**330.2**	N to N			**480.4**	U to N	
	847.6	E to U			**797.6**	N to U			**1673.6**	U to U	
	433.1	E to E			**542.0**	N to E			**1398.2**	U to E	

GLS estimation with errors clustered by household.

robustness check I use mis=1 and mis=5 transitions for each individual. These are in all cases separated by one calendar year.

(2) **Redefining transitions to ensure persistence in transition**. Abowd and Zellner (1985) noted that the number of labor force transitions away from current status deduced from CPS responses will be inflated by response errors. They note that as many as 10 percent of transitions out of unemployment in their sample from the early 1980s could be due to response error. (They estimate that only about 1 percent of transitions out of the E or N states were erroneously misreported.)[10] Elsby et al. (2015) note this possibility as well, and suggest a workaround: ad hoc reclassification of a NUN sequence over three periods as an NNN sequence, and reclassification of a UNU sequence as a UUU sequence.[11]

Both Abowd and Zellner (1985) and Elsby et al. (2015) examined the labor transitions of all working-age individuals. Specifically, they included both those of school age and those of retirement age for whom the distinction between non-participation and unemployment can be more confusing to the respondent. In this study I limit consideration to individuals between ages 25 and 54, and as such this confusion should be lessened. As a robustness check of the results of Table 2, however, I redefine transitions in the spirit of Elsby et al. (2015).[12] The results of that set of regressions are reported in Table 4. As is evident there, the results under this formulation for transitions out of unemployment are qualitatively identical to the unadjusted specifications of Tables 2 and 3. In particular, the significant coefficients in the U_to_N and the U_to_E transitions remain significant in this formulation. One N_to_E transition coefficient retains its

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[10] This observation by Abowd and Zellner (1985) is separate from their note that a significant percent of potentially matched observations in the CPS cannot in fact be matched due to errors in entering identifying information. Abowd and Zellner note that these errors do not occur randomly, but cluster by labor status; they use reinterview data to create a mathematical adjustment to gross flows that corrects for these errors on average. While they found that errors clustered by labor status, my research design relies upon differences between NC and ROUS within each labor status. Their mathematical adjustment of the data thus should not be necessary. Its use may in fact be misleading, given that their technique is predicated on the entire working-age population and the data used in this paper are drawn from those aged 25-55.

[11] Elsby et al. (2015) stresses that this is not a correction of the data, but rather a lower bound on the volatility associated with these individuals' labor market histories. There will be some respondents for whom NUN or UNU describe accurately their job-market experience, and for them this reclassification will inaccurately represent the volatility of their experience.

[12] Specifically, I require a two-month duration of a labor state for the transition to be valid. If we take the example of an individual unemployed in the first month: the U_to_N transition in this instance requires that the individual report N status in the second and third month, and the U_to_E transition requires that the individual report E status in both the second and third month. Following Elsby et al. (2015) I define the U_to_U transition to include the three-month sequences UUU, UNU, UEU, UUN, and UUE. Those with transitions UNE and UEN are not given a transition status, though they remain in the dataset. The transition definitions for those beginning employed or non-participating are defined analogously.

value and significance in this formulation, while an N-to-U transition coefficient becomes significant taking the opposite sign.

Table 4: Robustness test: Two-period duration of transition

	γ_{jkt}	s_{jkt}	z		γ_{jkt}	s_{jkt}	z		γ_{jkt}	s_{jkt}	Z
	E to N				N to N				U to N		
2013q1	-0.003	0.005	0.64		0.005	0.026	0.42		0.036	0.047	0.77
2013q2	0.001	0.005	0.24		-0.033	0.028	1.14		0.036	0.055	0.65
2013q3	0.008	0.005	1.63		-0.025	0.028	1.14		**0.156**	0.054	2.88
2013q4	0.006	0.005	1.20		0.004	0.026	0.13		**0.140**	0.060	2.35
	E to U				N to U				U to U		
2013q1	0.004	0.004	1.09		0.010	0.014	0.75		-0.129	0.069	1.85
2013q2	-0.001	0.003	0.16		0.012	0.015	0.81		0.111	0.082	1.36
2013q3	0.002	0.003	0.55		-0.017	0.015	1.14		-0.127	0.080	1.58
2013q4	0.003	0.003	0.29		0.010	0.014	0.72		-0.124	0.088	1.41
	E to E				N to E				U to E		
2013q1	0.002	0.007	0.28		-0.026	0.014	1.23		0.062	0.057	1.07
2013q2	-0.001	0.006	0.13		0.016	0.015	0.68		**-0.149**	0.068	2.20
2013q3	-0.007	0.006	1.05		**0.053**	0.016	2.33		-0.035	0.066	0.53
2013q4	-0.008	0.006	1.02		-0.020	0.014	0.97		-0.018	0.073	0.25
N Obs			1,687,939				348,678				87,779
Avg Obs			23,444				4843				1219
Wald (143)											
	222.2	E to N			**336.68**	N to N			**375.0**	U to N	
	782.9	E to U			**726.7**	N to U			**1534.5**	U to U	
	397.5	E to E			**502.9**	N to E			**1217.7**	U to E	

GLS estimation with errors clustered by household.

(3) **Regressions weighted by CPS sampling weights**. The CPS provides sampling weights associated with each household interviewed. The previous results treated each individual as equally representative, but it is also sensible to consider the results from analysis using the CPS sampling weights.

In the second and third columns of Table 5 I report the unweighted means and standard deviations of the nine labor transitions, while in the fourth and fifth columns I report the same statistics calculated using the

CPS sampling weights. Incorporating the CPS sampling weights has little effect on the unconditional means and standard deviations of the data. As is evident, there are only small differences in the statistics.

Table 5: Considering the sampling weights of the CPS

Transition probability	Unweighted mean	Unweighted standard deviation	Weighted mean	Weighted standard deviation
U_to_N	0.187	0.390	0.189	0.391
U_to_E	0.233	0.422	0.226	0.418
U_to_U	0.580	0.493	0.585	0.493
N_to_N	0.889	0.314	0.888	0.315
N_to_E	0.070	0.254	0.069	0.254
N_to_U	0.041	0.199	0.042	0.202
E_to_N	0.017	0.131	0.018	0.133
E_to_E	0.971	0.167	0.970	0.170
E_to_U	0.011	0.105	0.012	0.108

Redoing the hypothesis tests of this paper using the sampling weights in a weighted-least-squares specification yields the results of Table 6. (For this calculation we return to the original definition of labor transitions used in Table 2.) Once again, there is a significant difference between North Carolina and the rest of the US in terms of transition from U to N: the implementation of the UI reform coincides with a large positive differential in 2013q3 and 2013q4. In the transition from U to E, the negative signs of the coefficients in 2013q2, 2013q3 and 2013q4 indicate that the Shavell/Weiss employment boost of the policy is not in evidence in implementation of the reform. The effects observed in transitions from N or E during this period take the same signs as those of the previous regressions, although with this weighting there is a significant 2013q2 reduction in the transition of employed to unemployed that manifests itself in a greater propensity to remain employed.

Table 6: Robustness check: Representative CPS sampling weights											
	γ_{jkt}	s_{jkt}	z		γ_{jkt}	s_{jkt}	z		γ_{jkt}	s_{jkt}	z
	E to N				N to N				U to N		
2013q1	-0.001	0.004	0.17		-0.017	0.021	0.80		0.030	0.043	0.71
2013q2	-0.006	0.004	1.51		-0.007	0.021	0.31		-0.016	0.042	0.38
2013q3	0.004	0.005	0.86		-0.013	0.023	0.57		**0.153**	0.059	2.60
2013q4	0.003	0.004	0.12		0.001	0.021	0.03		0.089	0.057	1.56
	E to U				N to U				U to U		
2013q1	-0.002	0.003	0.81		0.008	0.014	0.54		-0.089	0.052	1.70
2013q2	**-0.008**	0.002	3.07		0.001	0.014	0.10		0.079	0.052	1.52
2013q3	-0.003	0.003	1.05		**-0.032**	0.008	3.71		-0.106	0.061	1.73
2013q4	-0.004	0.003	1.25		0.004	0.014	0.10		-0.087	0.062	1.41
	E to E				N to E				U to E		
2013q1	0.003	0.005	0.61		-0.009	0.016	0.56		0.058	0.045	1.29
2013q2	**0.013**	0.004	2.96		0.005	0.017	0.32		-0.063	0.039	1.62
2013q3	-0.001	0.006	0.09		**0.045**	0.022	2.06		-0.047	0.043	1.09
2013q4	0.001	0.005	0.18		-0.005	0.016	0.29		-0.002	0.047	0.03
N Obs	3,969,815				840,957				204,181		
Avg Obs in period	55,136				11,680				2,836		
F (143)											
	3.2	E to N			**2.7**	N to N			**4.6**	U to N	
	9.0	E to U			**8.4**	N to U			**19.8**	U to U	
	4.6	E to E			**5.3**	N to E			**16.4**	U to E	

Weighted least squares, using sampling weights provided by the CPS(4).

(4) Considering a Southern-state control group. As a final robustness check, I investigate the importance of the control group. Using the rest of the United States is sensible, but one could argue that the Southern states are closer to North Carolina in economic structure and will provide a better comparator group. To do this, I reduce the sample to include North Carolina and eight Southern states.[13] The

[13] The states included in the control group are Alabama, Florida, Georgia, Maryland, Mississippi, South Carolina, Virginia and West Virginia. This is a conservative choice, since the GAO (2015) identifies Florida, Georgia and South Carolina among these as states that also undertook UI reform after 2011.

estimationestimation technique is the same as that used in Table 2. The results of redoing this event study for the Southern control group are reported in Table 7.[14]

Table 7: Robustness check using a group of eight comparator states from the South											
	γ_{jkt}	s_{jkt}	z		γ_{jkt}	s_{jkt}	z		γ_{jkt}	s_{jkt}	z
	E to N				N to N				U to N		
2013q1	0.002	0.005	0.35		-0.031	0.020	1.53		0.007	0.044	0.15
2013q2	-0.004	0.004	0.97		-0.009	0.020	0.46		-0.021	0.048	0.45
2013q3	0.003	0.004	0.76		-0.034	0.021	1.58		**0.122**	0.050	2.43
2013q4	0.004	0.004	0.83		-0.014	0.021	0.69		0.066	0.052	1.29
	E to U				N to U				U to U		
2013q1	-0.000	0.004	0.07		0.024	0.014	1.83		-0.085	0.054	1.58
2013q2	**-0.009**	0.004	2.53		0.004	0.015	0.31		0.049	0.058	0.84
2013q3	-0.003	0.004	0.78		-0.012	0.015	0.85		-0.093	0.062	1.51
2013q4	-0.001	0.004	0.09		0.009	0.014	0.67		-0.072	0.063	1.15
	E to E				N to E				U to E		
2013q1	-0.001	0.006	0.24		0.007	0.017	0.41		0.077	0.045	1.73
2013q2	**0.013**	0.006	2.33		0.005	0.018	0.31		-0.032	0.048	0.67
2013q3	-0.001	0.006	0.13		**0.044**	0.018	2.57		-0.037	0.051	0.73
2013q4	-0.004	0.006	0.61		0.006	0.017	0.34		-0.006	0.052	0.11
N Obs			666,441				157,861				34,947
Avg Obs in period			9256				2192				485
Wald (143)											
	224.8	E to N			**231.5**	N to N			**313.9**	U to N	
	448.8	E to U			**421.5**	N to U			**750.8**	U to U	
	298.1	E to E			**725.2**	N to E			**636.2**	U to E	

GLS estimation, with errors clustered by households.

The results of this regression confirm those of the preceding analyses. The transition from unemployment to employment is positive in the announcement quarter, followed by negative effects in the next three quarters: none of these coefficients is significantly different from zero. The transition from unemployment to non-participation is both positive and significantly different from zero, just as in the previous results. The results of these four robustness exercises confirm the statistical evidence of the joint test. The

[14] This regression uses the original specification: unweighted observations and two-period definition of transition. The appropriate comparison of results is with those of Table 2.

Shavell/Weiss moral hazard effect is not evident in these estimates, while the Rothstein "discouraged worker" effect is evident and statistically significant in all four.

(5) Creating an "optimal" counterfactual through synthetic control. Choosing the group of states to form the counterfactual is generally done on a spatial basis, and I have followed that lead in choosing (first) residents of all other states and (second) residents of all Southern states as the counterfactual group. Abadie, Diamond and Hainmueller (2010, hereafter ADH; and 2015) propose a "synthetic control" method of generating the counterfactual.[15] Firpo and Possebom (2017) demonstrate that this synthetic control estimator can have more statistical power than the difference-in-difference estimators used in the previous sections.

Suppose that we observe an average labor transition propensity (L_{jkst}) from labor status j to labor status k in state s in period t. We wish to create a counterfactual (ℓ_{jkst}) and an unbiased estimate of the treatment effect $\Delta_{jkst} = (L_{jkst} - \ell_{jkst})$ in periods t post-reform. Following ADH, we do so in five steps:

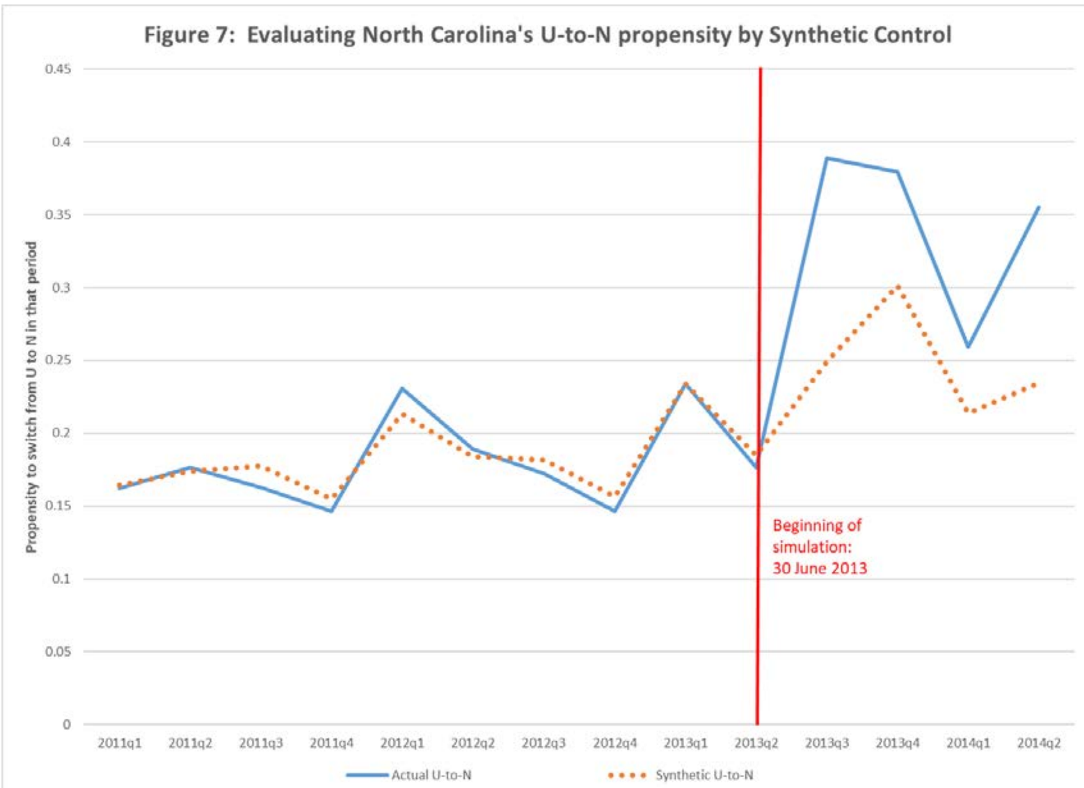
- Define vector L_{jks} and ℓ_{jks} with elements L_{jkst} and ℓ_{jkst} for the pre-reform period (2000q1 – 2013q2), respectively. Create the matrix $\Lambda_{jkS\backslash s}$ that includes the vectors of L_{jkst} for the non-s states.
- Define a vector $w_{jS\backslash s}$ with weights for each of the 49 other states, and create $\ell_{jks} = \Sigma_{S\backslash s}\, \Lambda_{jkS\backslash s}\, w_{S\backslash s}$.
- Choose $w^{*}_{S\backslash s}$ by minimizing the sum of squared deviations ($L_{jks} - \ell_{jks}$) over the pre-reform period.
- Calculate the post-reform counterfactual by calculating the post-reform counterfactual vector $\ell_{jkst} = \Sigma_{S\backslash s}\, L_{jkS\backslash st}\, w^{*}_{S\backslash s}$ for four post-reform periods (2013q3 – 2014q2).
- Calculate the impact of the reform as $\Delta_{jkst} = (L_{jkst} - \ell_{jkst})$ for the post-reform periods.

As a robustness check to my findings of the previous section, I use synthetic control to derive the impact effect of the NC unemployment insurance reform on the U-to-N transition probability in North Carolina.[16] The explanatory variables for the counterfactual are the average propensity of someone

[15] This statistical technique is operationalized by the authors in the program "Synth" for Stata. I use their software specification in this estimation.

[16] I have also considered synthetic control analysis for other transition propensities; those results are available on request.

with labor status U to switch to labor status N in the next period. The weighted-average counterfactual equation is defined through minimization of squared errors in the period 2009q4 through 2013q2.[17] The counterfactual for evaluating the reform is created by using the state-specific weights estimated in-sample with the out-of-sample realizations of the average propensities in the other states. The result is found in Figure 7.



Just as in the earlier analysis, there is a clear jump in the transition probability from U to N that is only partially reflected in the counterfactual. That jump persists over the next four quarters as well.

ADH suggests that perturbation analysis is a sensible way to test for the significance of this shift, and propose the RMSPE statistic: a ratio of out-of-sample root mean squared prediction error to in-sample root mean squared prediction error. I obtain the RMSPE statistic for each US state, imposing the same transition

[17] Surprisingly (at least to me), many non-NC states had zero weight in the synthetic control. The included states, and their weights, are Alabama (.104), Hawaii (.022), Louisiana (.159), Maryland (.05), Missouri (.166), Ohio (.165), Oregon (.076), Rhode Island (.078), South Carolina (.036), South Dakota (.102) and Virginia (.042).

quarter of 2013q3. If the shift in labor transition propensity in North Carolina were due to a national shift, for example, all states would demonstrate this large jump in U-to-N propensities and have similar SMSPE statistics.

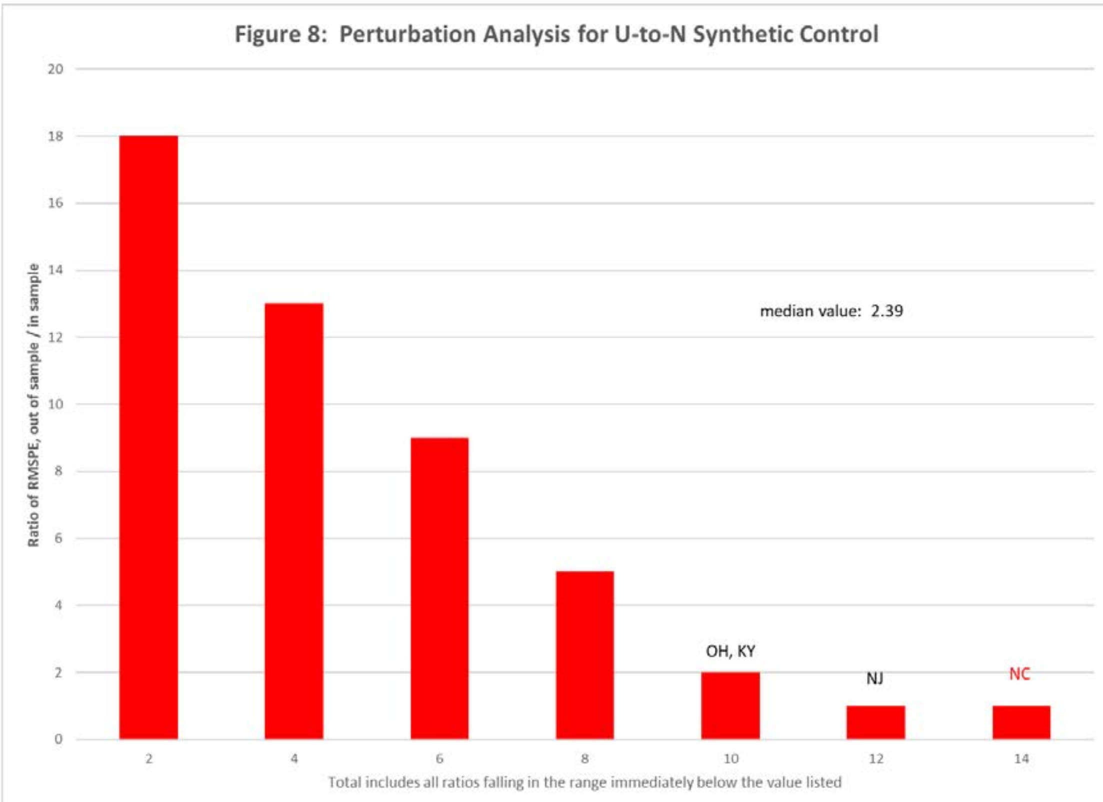


Figure 8 illustrates the result of this analysis.[18] The median value of this ratio for all states is 2.39. The ratio for North Carolina is 12.60 – over five times the median value.[19] This methodology supports the conclusions of the earlier section: the North Carolina reform was followed by an extreme jump in transition from unemployment to non-participation relative to comparator states in the US.

[18] The RMSPE in each case is the ratio of the root mean squared prediction errors for the four periods 2013q3-2014q2 (out-of-sample) to 2012q3-2013q2 (in-sample).

[19] Firpo and Possebom (2017) have begun work on providing confidence sets for statistical tests of the RMSPE statistic in synthetic control analysis. They demonstrate that RMSPE has uniformly greater power than other perturbation test and difference-in-difference statistics.

5. Simulating UI reform's impact using a calibrated model.

The regression results of the previous section demonstrate the significance of the change in labor-market transition from unemployment towards non-participation beginning in July 2013, but do not give a complete picture of the aggregate effect of UI reform on labor market aggregates over time. First, the transition probabilities change from quarter to quarter. Second, the transition probabilities are applied to very different stocks of workers. The BLS reports that in North Carolina between the ages of 25 and 54 there were on average in 2013 2.9 million employed,. 0.7 million out of the labor force, and 0.2 million unemployed: an increase in one percentage point in the E-to-U transition probability will lead to more than four times as many unemployed in the next period than an increase in one percentage point in the N-to-U transition probability. Third, the impact effect of the UI reform on the U-to-N transition probability could be counteracted over time by offsetting changes in the U-to-E and the E-to-N transition probabilities.

To trace out the magnitude of UI reform's effect on employment and unemployment in addition to non-participation I simulate a model using the NC transition probabilities calibrated to North Carolina data and compare it to a counterfactual created using the ROUS transition coefficients and the same NC starting values.

In principle, one can begin with starting values for the populations of North Carolina residents in the E, U and N categories in a given month (e.g., January 2012) and then use the coefficients estimated here for each quarter to update those stocks. In practice, though, the coefficients are estimated only for a sample of individuals from the CPS and the dataset does not include changes in stocks through demographics or migration. Kroft et al. (2016) create what they call a "brute force" calibration by normalizing the working-age population at each time period to one; this succeeds in making stocks and flows consistent, but at the cost of matching the observed magnitudes in each period.

My approach takes two parts. The conditional probabilities from the regression underlying Table 2 serve as month-by-month updating factors for a set of initial values for E, N and U in North Carolina over the 36 months from January 2012 to December 2014. The annual averages of these data are compared to the annual averages for employed, unemployed and not-in-the-labor-force individuals for the civilian non-institutional population in North Carolina with ages from 25 to 54 provided by the Bureau of Labor Statistics. The initial values are adjusted and marginal changes in conditional probabilities are made until the model converges to these annual averages.[20] The resulting month-by-month values for U, E and N in North Carolina are compared in the following graphs to a counterfactual: what if North Carolina had not diverged from the US as a whole? To generate the counterfactual I use the same initial values of E, N and U for North Carolina, but update them using the conditional probabilities from the regression underlying Table 2 for the rest of the US.

Appendix C provides details on the calibration, while Figure 9 illustrates the major conclusion of this paper. When compared with the counterfactual based on behavior in the rest of the US, North Carolina observes a large increase in those exiting the labor force in the months following UI reform. By December 2013 there were 80,000 individuals not in the labor force who would have been forecast to be in the labor force; by December 2014 there were still 50,000 individuals not in the labor force who would have been forecast to be participating.

[20] These data are accessed at https://www.bls.gov/lau/ex14tables.htm.

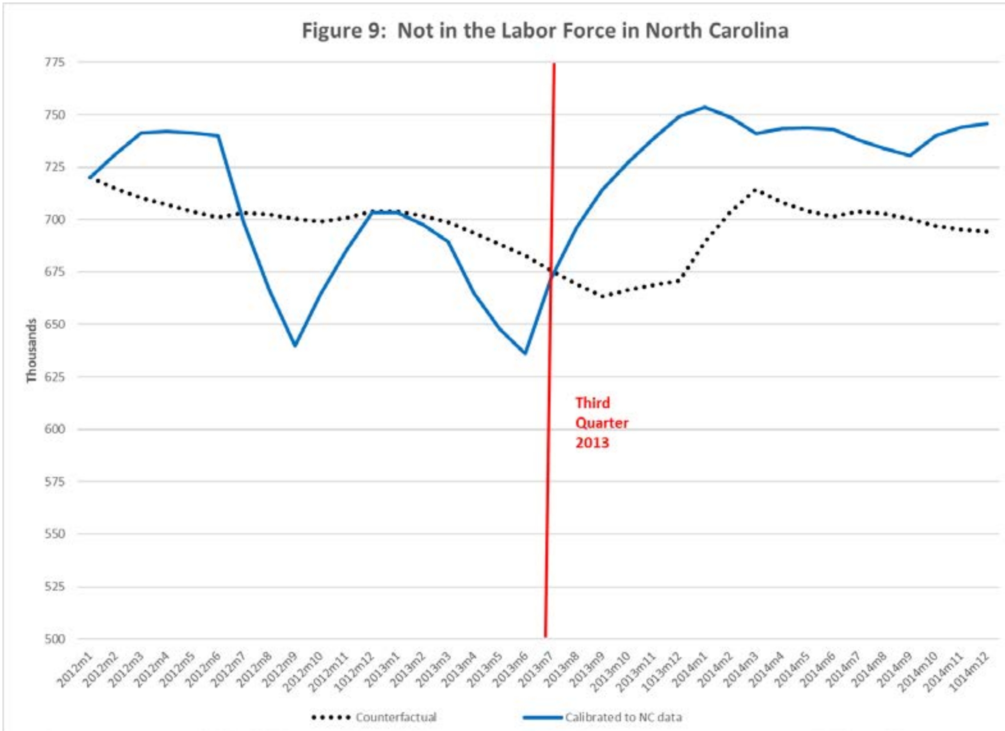
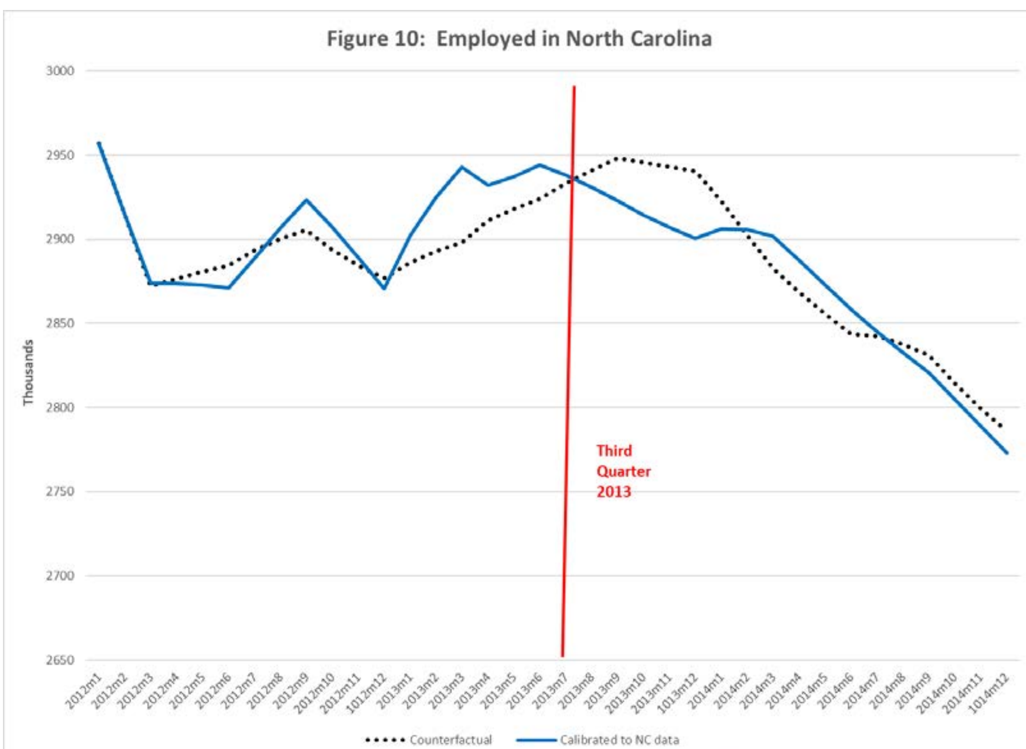


Figure 10 points out that the Shavell-Weiss effect of UI reform encouraging greater employment is not in evidence.



Employment is trending downward in this age group from the beginning of 2013, but the calibrated results fall below the counterfactual during the months following implementation of UI reform on 1 July 2013.[21] The UI reform does coincide with a drop in the unemployment rate of those aged 25-54, as Figure 11 illustrates.[22] The fall in the rate, though, is due to the drop in labor force participation. There is no evidence of individuals switching from unemployed to employed, but significant evidence of individuals switching from unemployment to non-participation.

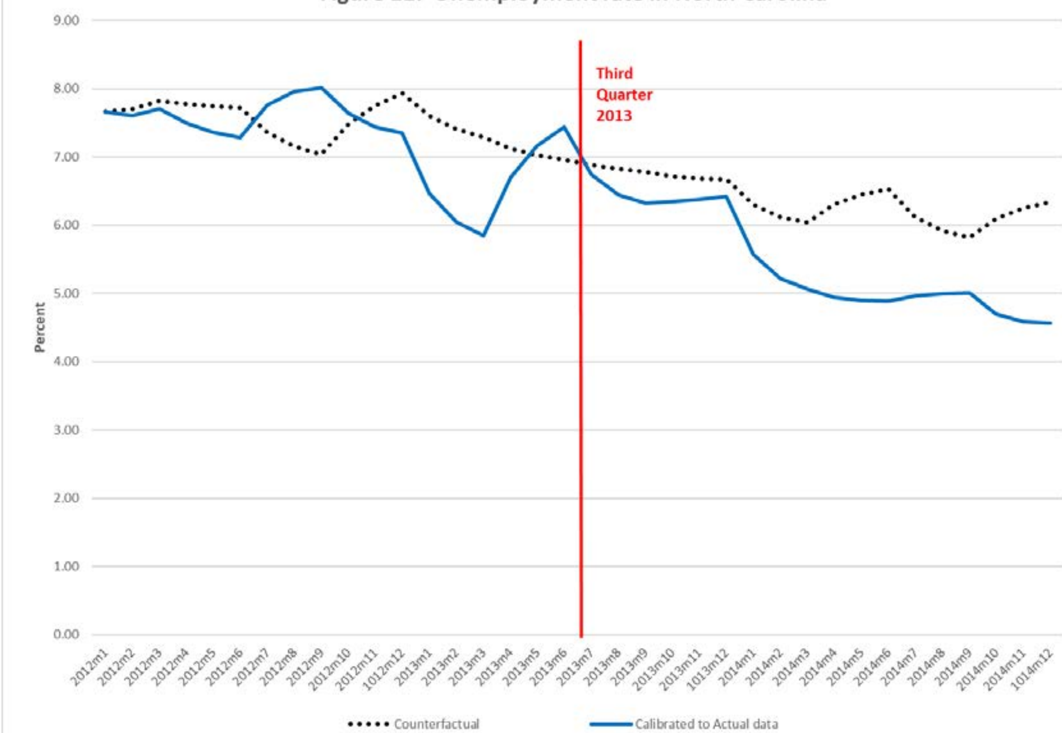


Figure 11: Unemployment rate in North Carolina

[21] It is surprising to compare Figures 1 and 10. The total employment in North Carolina over 2013 in Figure 1 is increasing steadily, but the employment of workers aged 25-54 in Figure 11 is falling. Both statistics come from the LAUS database of the Bureau of Labor Statistics.

[22] These unemployment rates do not coincide with those in Figure 3. The rates of Figure 3 are based on data for all residents 18 years of age or older, while the unemployment rates reported in Figure 11 are based on individuals in the 25-54 age bracket.

6. Evidence from the employer side: was there a spike in employment?

The evidence from the Current Population Survey is persuasive, but it provides only an indirect measure of the responses of employers to the UI reform. If UI reform induces the unemployed to return to jobs sooner, then the jobs must be available and employers must have listed those jobs. There is economic logic to this response, since employers will view UI reform of the type observed in North Carolina as a reduction in the cost of employing a worker and can respond by increasing the number of desired workers.

The Conference Board created a summary count of job listings across the country through compilation of advertisements of jobs on online boards in a given month that it calls the Help Wanted Online Listing (HWOL). There are two series: all ads observed in a given month, and new ads observed.[23] If the UI reform in North Carolina led to a surge in job creation, then I anticipate that the ratio of ads observed in North Carolina relative to the rest of the US should rise. (If job creation was due to national growth trends, by contrast, then both North Carolina and Rest of US ads will rise proportionally and the ratio will be largely unchanged.)

In Figure 12 I report the evolution of that ratio over time. The period between 2005 and 2010 is characterized by rapid growth in both ratios: I interpret this as an indication that firms in North Carolina were relatively slow to turn to online job listings. The peak in these ratios was in March 2013; they declined rapidly after that time. The vertical red line indicates June 2013 – the month before the UI reform became law. There is no evidence here that North Carolina firms expanded their job listings more rapidly than the rest of the US when the UI reform was enacted. (While it is not visible in this figure, it is true that job listings in North Carolina fell in absolute count in July 2013 while those in the rest of the US rose.)

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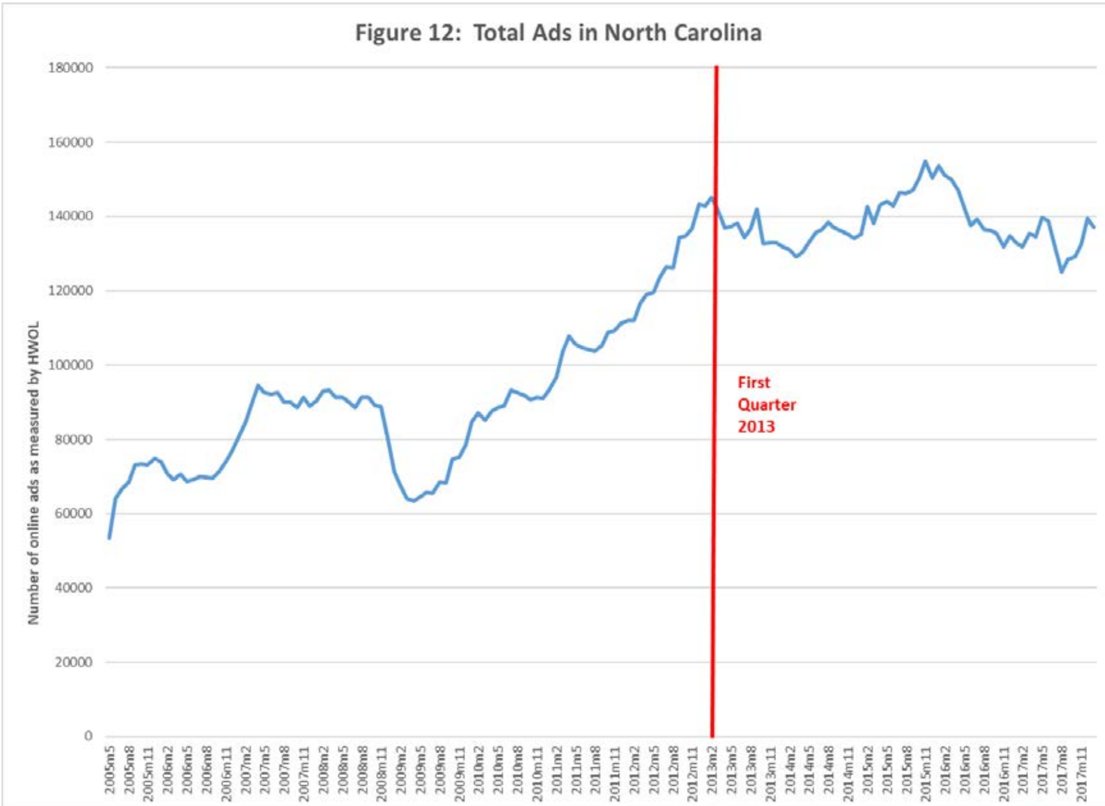
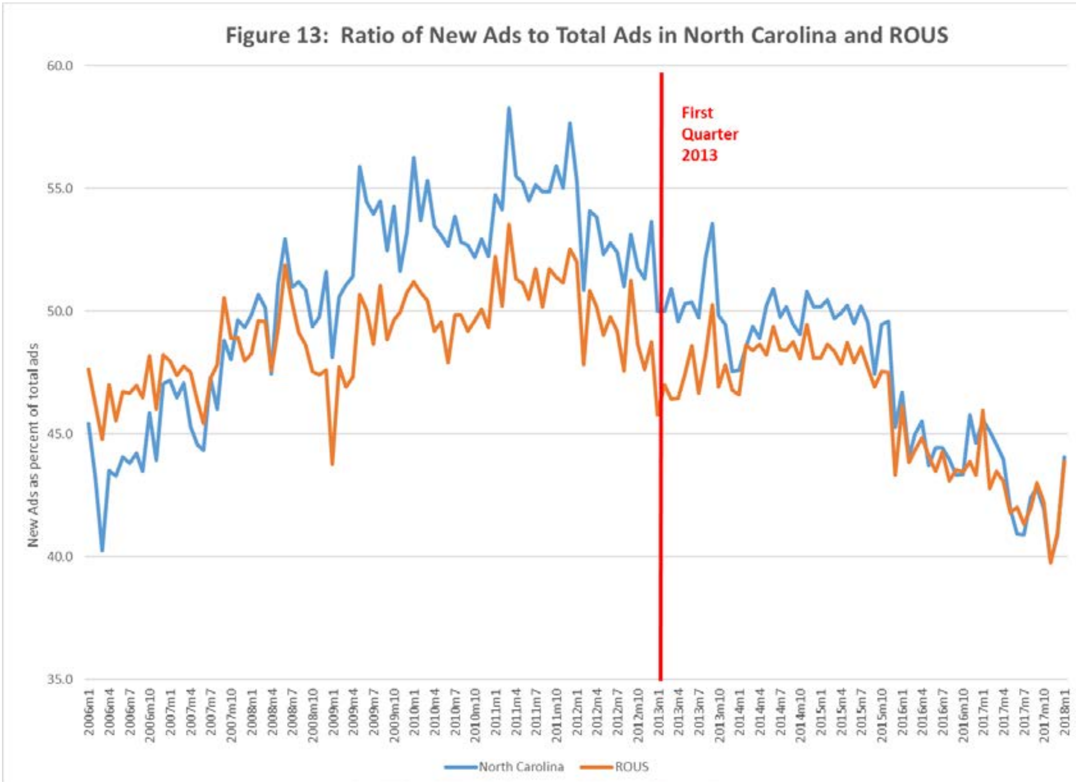
[23] These series are reported on a monthly basis from May 2005 to the present; they are available for a fee through Haver Analytics. I use the seasonally adjusted series in what follow. Please note that the Conference Board's method of seasonal adjustment may not coincide with that of the Bureau of Labor Statistics.



Source: Help Wanted Online Database

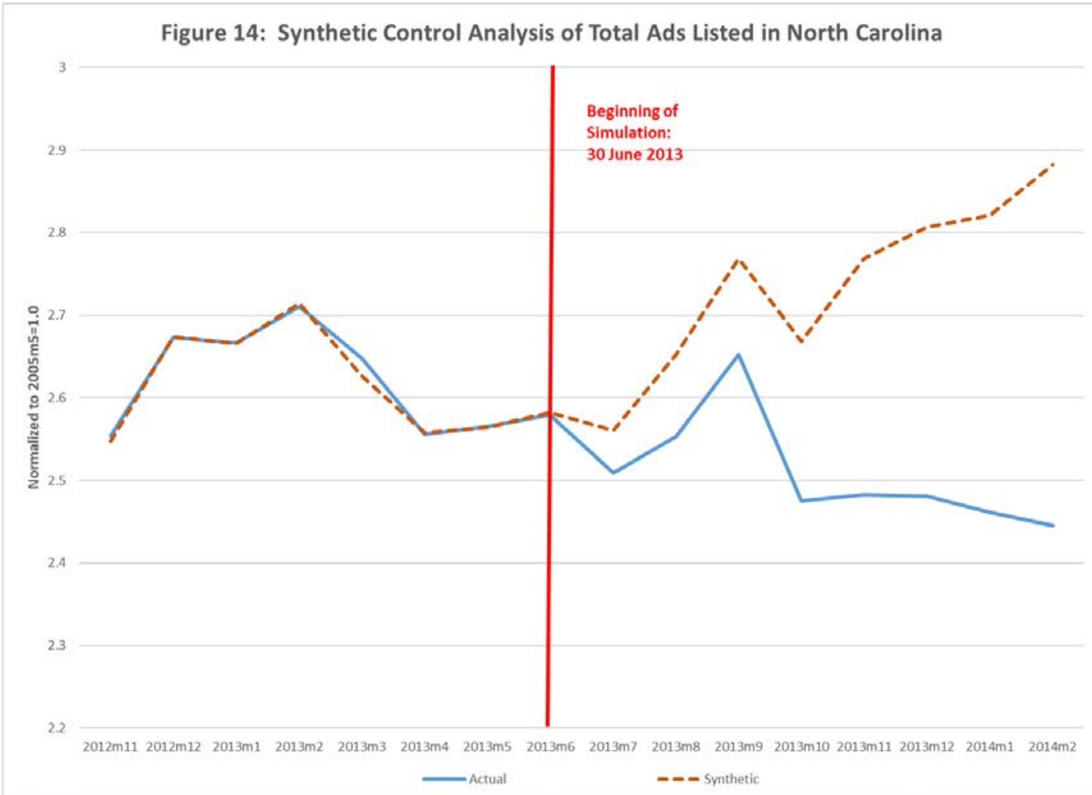
Figure 13 provides an alternative look at this job listing process by taking the ratio of new ads listed to all ads listed for North Carolina and for the rest of the US. If UI reform were to lead to new job creation, then these new jobs will need new ads and the ratio of new ads to all ads will rise in North Carolina while remaining stagnant in the rest of the US. This is not evident in the figure.



Source: Help Wanted Online Database

There is a spike in the ratio of new ads in September 2013 in North Carolina, but this spike is mirrored in the rest of the US. The ratio of new ads to all ads is declining both in North Carolina and in the rest of the US during this period.

I investigate this record more formally using the synthetic-control methodology of the previous section. I have the data on total number of online ads by month from HWOL over the period May 2005 to December 2017. I normalize these series by state to be equal to 1.0 in May 2005, and then create the optimal counterfactual for North Carolina's series by minimizing the mean squared prediction error for the period January 2011 to June 2013. Figure 14 illustrates the actual normalized number of ads and the counterfactual number of ads around UI reform introduction on 1 July 2013. The synthetic counterfactual fits very well in the periods leading up to July 2013, but diverges strongly thereafter. While the counterfactual suggests positive growth for the number of ads in North Carolina, the actual series exhibits negative growth.



To investigate the statistical significance of this result, I conduct a perturbation analysis by calculating the RMSPE for each state in the US for the evolution of total ads. Figure 15 summarizes this exercise. The value on the vertical axis is the RMSPE ratio, and the abbreviations to the right indicate the states with values in that range.[24] The median value of the ratio when all states are considered is 4.89, and the value for North Carolina is 31.85.

[24] One state, Massachusetts, is excluded. Its mean squared error for in-sample estimation was so close to zero that the RMSPE ratio was over 200. The actual value of out-of-sample mean squared prediction error was not large in comparison to the other states, as shown in Figure 16.

Figure 15: RMSPE Ratios for synthetic control with break in July 2013

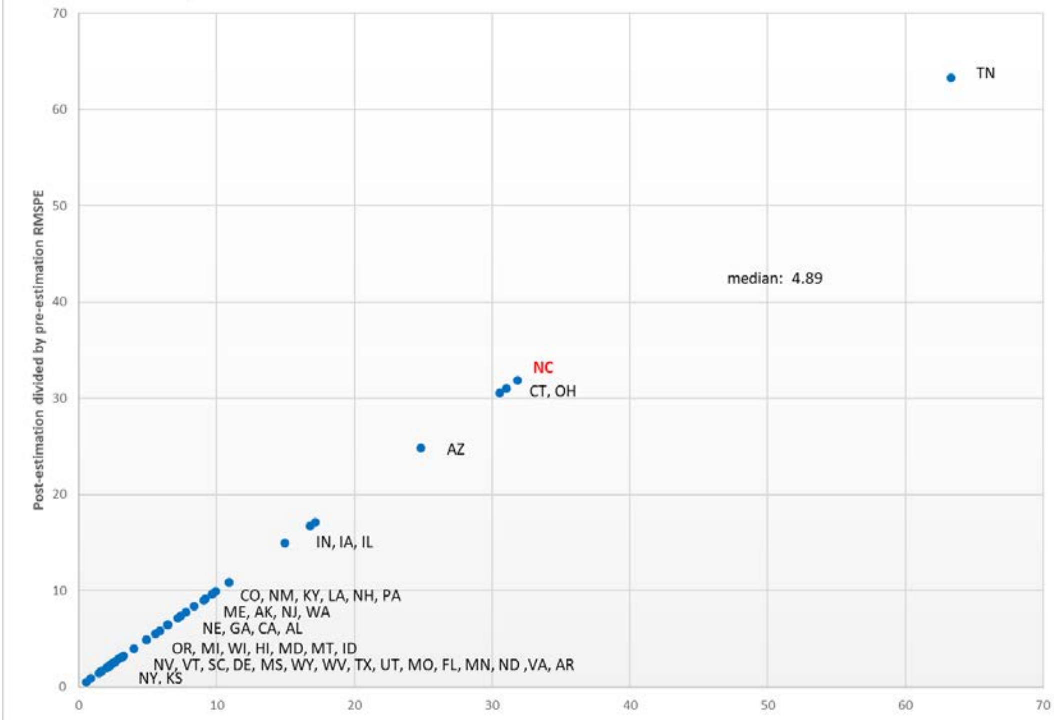
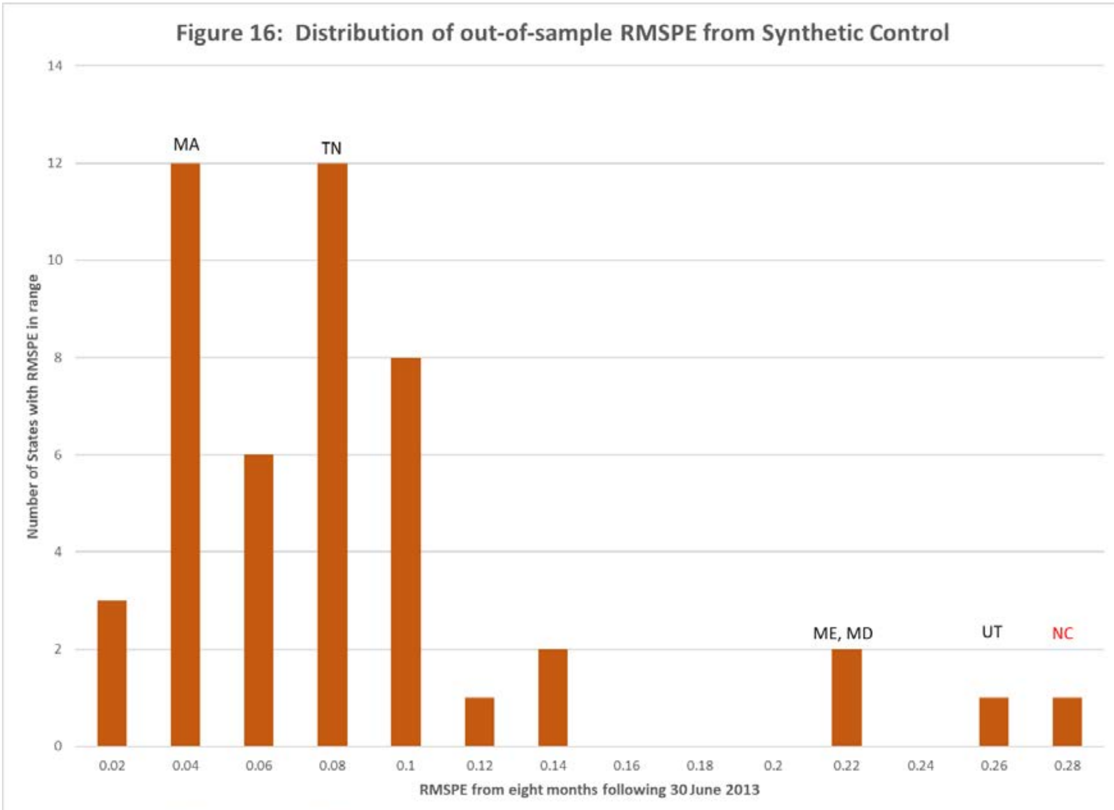


Figure 16 provides another look at North Carolina's position among US states. If the US states are sorted by size of out-of-sample root mean squared prediction error, the result is this histogram. The median value is 0.062. The abbreviations indicate the bin in which a selected set of states fall. Both Massachusetts and Tennessee have much smaller value. North Carolina, with value 0.267, has an out-of-sample root mean squared prediction error over four times larger than the median.

This divergence in out-of-sample RMSPE could either be due to undershooting or overshooting (or both) of the synthetic counterfactual. Figure 12 illustrates that North Carolina falls short of the counterfactual. Utah and Maine are also outliers in Figure 14, but are states with total ads that exceed their counterfactuals; Maryland has actual total ads that fall short of the counterfactual, but less significantly than North Carolina. If Help Wanted ads provide an indicator of firms' intention to create jobs, then the historical record provides no evidence that employers in North Carolina had an increased desire to hire more workers at the time of UI reform. In fact, the number of total ads posted online fell in the eight months after the UI reform.



7. Conclusions and suggestions for further research.

Empirical evidence from individual labor status transitions indicates that the UI reform in North Carolina in 2013 did not lead to a significantly larger percentage of unemployed workers becoming employed. By contrast, there is significant statistical evidence that the UI reform in North Carolina is associated with an increased percentage of the unemployed choosing to exit the labor force. Simulation results indicate that the number of formerly unemployed workers of ages from 25 to 54 exiting the labor force peaked at 80,000 in late 2013 and remained at 50,000 in late 2014. This does not support the Shavell/Weiss view that removing the moral hazard of unemployment insurance will lead to greater employment take-up. It does confirm for North Carolina Rothstein's conjecture that UI reform will create discouraged workers who exit the labor force prior to typical retirement age.

This empirical evidence is persuasive, but it is indirect. It is important to note that the Current Population Survey does not provide evidence on whether the individual is receiving unemployment insurance, and so I cannot test the impact of UI reform on those specifically whose benefits were removed. Rather, I observe the general choice of transitioning to unemployment or to non-participation and measure the change in the probability of making that choice at the time that the UI reform was implemented (or announced). These results can be taken as further support for the results on labor-force exit in response to UI benefit reductions in Rothstein (2011).

This conclusion is in contrast with the empirical findings of Johnston and Mas (forthcoming 2018), and it is important to consider the differences. Johnston and Mas use administrative data for Missouri to consider a UI reform that reduced unexpectedly the duration of benefits. Those facing a reduction in duration of benefits demonstrated a significant Shavell/Weiss response – they reduced on average the time to employment by roughly half the loss in duration time of benefits. The control group in this case is the group of unemployed in Missouri who had exhausted their benefits, and they as a group had no response on average to the reduction in duration of benefits.

The Johnston/Mas results are important for design of UI policy, but the results of this paper provide a useful complement. First, the Shavell/Weiss response found in Johnston/Mas is crucially dependent upon the UI recipient being early in her scheduled payments: the individual remains unemployed but increases job-search effort while still receiving payments. Johnston/Mas does not find a similar response for those who had exhausted, or were nearing exhaustion, of benefits. Missouri's UI reform was less sweeping than that of North Carolina, and so as a result the Missouri recipients retained their Federal extended benefits. In North Carolina those were eliminated for a majority of recipients on 1 July 2013. This immediate exhaustion of benefits left no leeway for the forward-looking search intensity at the heart of Johnston and Mas' result. Second, the Missouri database includes only eligible workers who filed claims and ineligible workers whose claims were denied. In North Carolina there is a third group – those eligible who do not file claims or have their claims denied -- and this is a large and growing group over the sample period in the sample of individuals considered here. If these individuals believe that they are retaining the option value of UI payments for the future, the UI reform will have a pro-exit effect on them that will be evident in this paper's results and not in those of Johnston/Mas. Third, the simulation results of this paper include those employed and those not in the labor force as well as the unemployed. I model the possibility of multiple labor force transitions for an individual worker over the time horizon. The results of this paper then provide a better forecast of the aggregate impact of the UI reform over time.

An optimal UI policy will balance the insurance aspect of the policy with the incentives the policy provides to become re-employed. The Shavell/Weiss incentive is moral hazard: insurance subsidizes unemployment and thus reduces the incentive to take a new job. The Rothstein incentive is to continue job search to retain UI payments. The North Carolina UI reform failed as insurance: according to the US Secretary of Labor, it removed Federal UI benefits unexpectedly from 170,000 recipients.[25] In terms of providing incentives to intensify job search (as in Missouri), I find little evidence of success. This may in fact be due to the concurrent tightening of rules for having UI claims approved. In 2013q2, North Carolina provided UI benefits to 39 percent of jobless workers. In 2015q3, 11 percent of jobless workers received UI benefits.[26]

There is one puzzle remaining from this analysis: how does a state with above-average employment growth have at the same time a growing group of apparently discouraged workers? A definitive answer to this question is beyond the scope of this paper. My conjecture, though, is that the answer lies in net migration into the state. As Rebecca Tippett of Carolina Demography has pointed out, net migration into the state is forecast to be three times as large as the natural population increase in North Carolina over the period 2010-2020.[27] Net in-migration totaled about 75,000 individuals in 2013 alone. This paper's picture of workers moving out of the labor force can then be juxtaposed with the picture of new migrants moving into newly created jobs.

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[25] https://www.dol.gov/opa/media/press/eta/ETA20130260.htm

[26] US Department of Labor, UI Data Summary, hpps://workforcesecurity.doleta.gov/unemploy/content/data.asp

[27] Tippett, Rebecca: "Past, Present or Future, Net Migration is the Main Driver of North Carolina Growth", 12 July 2017. Accessed at http://demography.cpc.unc.edu/2017/07/12/past-present-or-future-net-migration-is-the-main-driver-of-nc-growth/

Appendix A: **Non-parametric estimation of the labor transition probabilities.**

1. Transition from Employment.

Figures A1 and A2 indicate the average probability of transitioning from being employed (E) to being unemployed (U) or non-participating (N) for residents of North Carolina (NC) and for residencts of the rest of the US (ROUS). The probability is calculated within a quarter, and then smoothly adjusts for the following quarter. We can conclude from the two transitions that:

- Probability of transition out of employment are not constant over time.
- The North Carolina experience rarely differs significantly from the experience of the ROUS. One important exception is in the transition from employment to non-participation: the NC probability of that transition is significantly greater than in the ROUS from 2013 to the end of the period.

The transition from employment to employment – i.e., the choice of remaining employed – also demonstrates a statistically significant difference between North Carolina and the ROUS. This is evident in Figure A3. For North Carolina, this "non-transition" transition probability has been below that of the ROUS since the beginning of the sample. It was significantly below that of the ROUS in 2007-2008 and in 2013-2017.

2. Transition from Unemployment.

For those unemployed, the transition probabilities vary with the business cycle. In Figures A4 and A5 the transitions to employment and to non-participation are illustrated. In North Carolina, the probability of transition to employment is less than that in ROUS from the fourth quarter in 2001. It is significantly less on in 2007-2008 and in 2013-2014. The probability of transition from unemployment to non-participation, by contrast, diverges greatly for NC and ROUS beginning in 2013.

In 2012 the NC probability of non-participation rose rapidly: by 2015 this percentage had risen to just below 30 percent. It has fallen more recently but remains above 25 percent in the first quarter of 2017. The confidence intervals for the NC transition probability indicate that the North Carolina probability was rarely significantly different from that of ROUS for the initial 12 years of the sample. Beginning in 2013, those unemployed in NC were significantly more likely to transition to non-participation in the labor force.

The "non-transition" transition probability – i.e., remaining unemployed is illustrated in Figure 9. For both ROUS and NC in the first quarter of 2000 this was 48 percent. After that time the percentage rose during recessions and fell with recovery. The probability in NC was significantly higher than in the ROUS in 2001-2003 and in 2006-2008, but has been very close to the ROUS value in the post-Great Recession years.

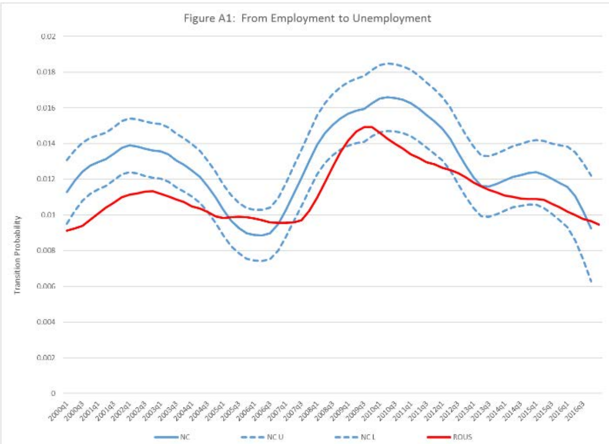
Figure A1: From Employment to Unemployment
Transition Probability
NC
NC U
NC L
ROUS

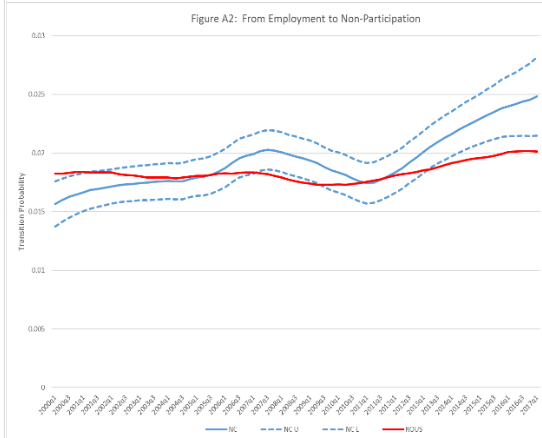
Figure A2: From Employment to Non-Participation
Transition Probability
NC
NC U
NC L
ROUS

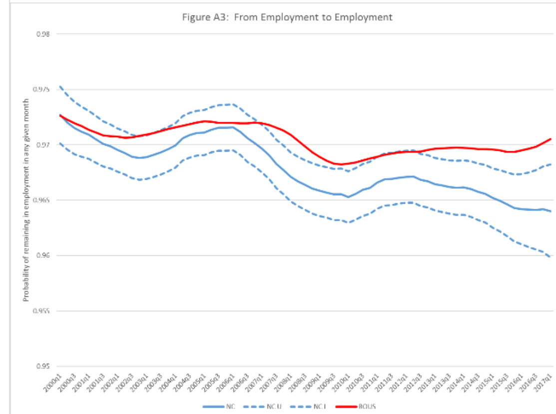
Figure A3: From Employment to Employment
ROUS

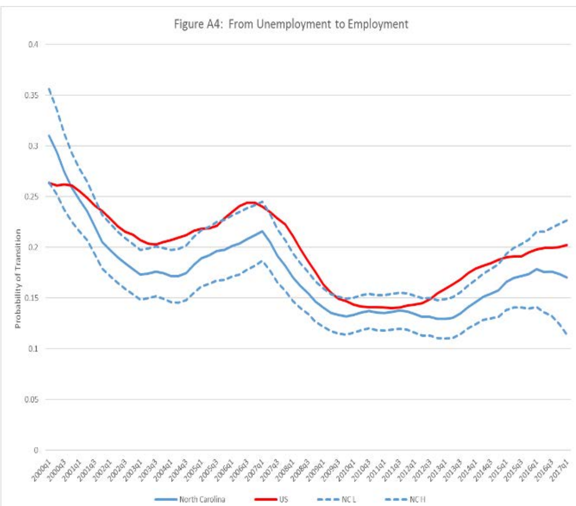
Figure A4: From Unemployment to Employment
Probability of Transition
North Carolina
US
NC L
NC H

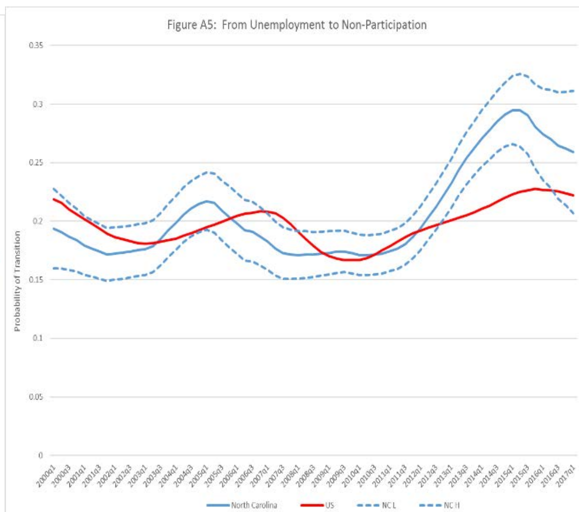
Figure A5: From Unemployment to Non-Participation
Probability of Transition
North Carolina
US
NC L
NC H


Figure A6: From Unemployment to Unemployment
Probability of Transition
North Carolina
US
NC L
NC H

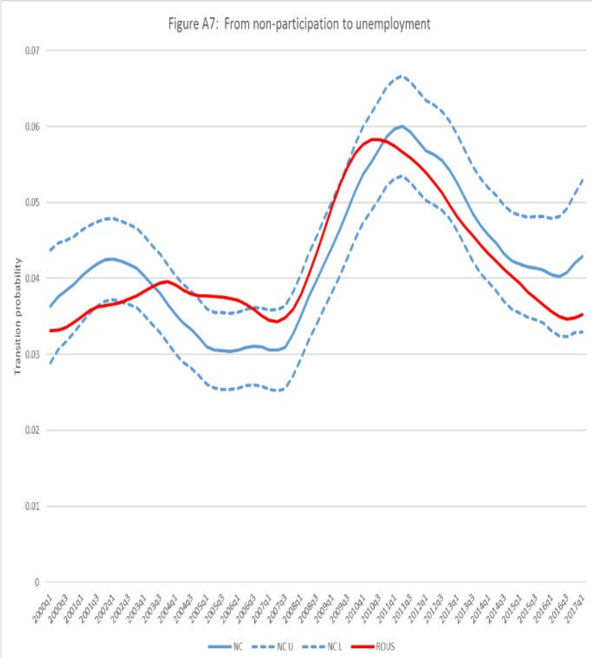

Figure A7: From non-participation to unemployment
Transition probability
NC
NC U
NC L
ROUS

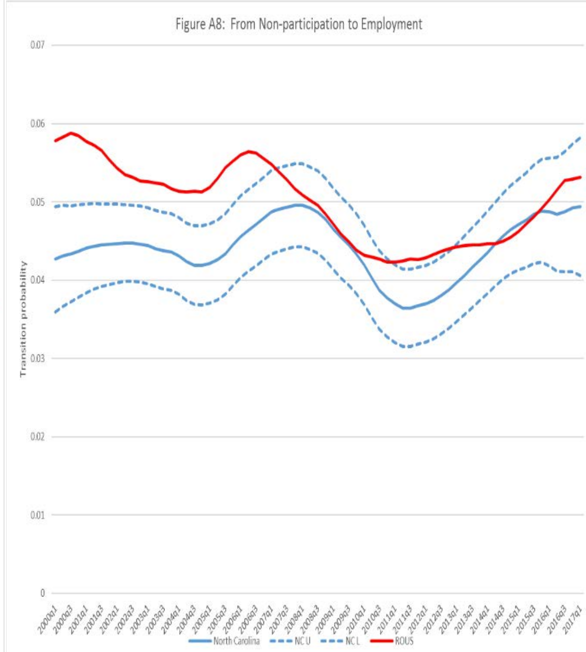

Figure A8: From Non-participation to Employment
Transition probability
North Carolina
NC U
NC L
ROUS

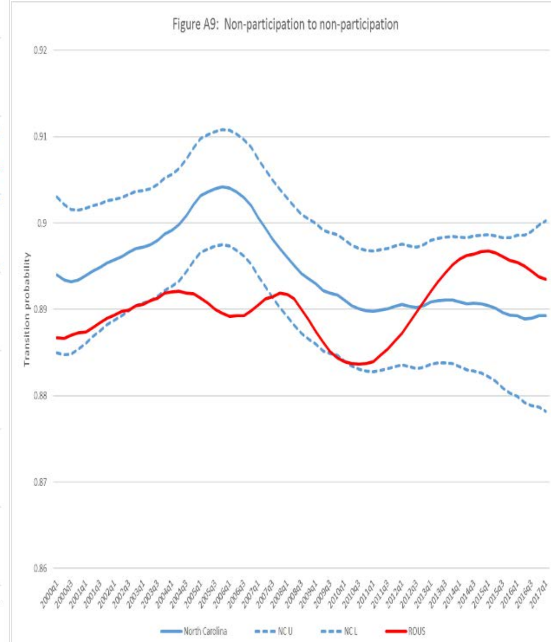

Figure A9: Non-participation to non-participation
Transition probability
North Carolina
NC U
NC L
ROUS

3. Transition from non-participation.

Figures A7 and A8 illustrate the percent of those not participating in the labor force who in the next period are employed or unemployed.

Each of these is a low-probability event, and in both cases the North Carolina experience does not differ significantly from the ROUS experience in the last decade.

As Figure A9 illustrates, the "non-transition" transition probability of remaining a non-participant from month to month is roughly 90 percent both in NC and in the ROUS. The difference between transition probabilities is only statistically significant in the 2004-2007 period; at that time, non-participants in North Carolina were more likely to remain non-participants that those who were non-participants in the ROUS. More generally, the rate at which non-participants remained out of the labor force was higher for NC residents for the period 2000-2012. After 2012, non-participants in ROUS became more likely to remain non-residents.

Appendix B: **Potential Classification Errors in the CPS data**

Gross labor flows between employment (E), unemployment (U) and not in the labor force (N) can be derived from the Current Population Survey through exploiting the fact that each household is interviewed eight times: four consecutive months, followed by eight months out of sample, and then four additional consecutive months. For each individual, there are three potential states in the first month: E, U, or N. There are also three potential states for the second month: E, U, or N, leading to a total of nine labor flow transitions: EE, EU, EN; UE, UU, UN: NE, NU, NN.

Abowd and Zellner (1985) point out two potential pitfalls of using CPS records in this way. First, they report that a respondent's mistake in excluding household identifier, or incorrectly entering the household identifier, leads to the exclusion of as many as 7.5 percent of respondents who in fact participated in the survey over two consecutive years. Second, other response and coding inconsistencies can lead to increased volatility – for example, a household might report one member "out of the labor market" in one period and "unemployed" in the next with no change in the individual's labor-market position. The authors used information from random re-interviewing to identify the likelihood of these types of omission and classification errors and proposed an error-adjustment procedure to control for such. The sample used and re-interviewed was of all individuals age 16 and above in the months between 1976 and 1982 inclusive. The resulting adjustment procedure is time-invariant. See Poterba and Summers (1986) as well.

Elsby et al. (2015) identify another potential classification error: individuals who alternate between U and N from month to month. This could be a "NUN" error – that those out of the labor market will mistakenly report an "unemployed" month sandwiched between "not in the labor force" months – or an "UNU" error – that those who are unemployed report themselves out of the labor market between two "U" responses. They demonstrate that making this correction in a sample of individuals aged 16 and above has very similar effects on the data as the Abowd-Zellner correction. They stress, though, that this is not an unambiguous correction. Classification mistakes may be rectified, but correct responses of NUN or UNU will be incorrectly removed as well.

Shimer (2012) remarked upon the large percentages of observations associated with NE or EN transitions as an artifact of having a month-over-month observation of a continuous phenomenon: the transition from N to E most likely includes an intermediary transition to U that is completed in the month between survey responses. He provides a correction method for the data.

Appendix C: **Calibration.**

The BLS provides the following annual averages of the population between the ages of 25 and 54 inclusive for North Carolina over the years 2012 through 2014. Population here refers to the civilian non-institutional population. NILF is the total of individuals not in the labor force. UR is the unemployment rate (equal to Unemployed/Labor Force) and LFPR is the labor-force participation rate (equal to Labor Force/Population). Unfortunately, monthly totals for this slice of the working-age population are not available.[28]

Table C1: Annual-average measures of Labor Force Participation and Non-participation in North Carolina

	Population	Labor Force	Employed	Unemployed	NILF	UR	LFPR
2012	3839	3133	2896	238	706	7.60	81.61
2013	3825	3130	2924	205	695	6.55	81.83
2014	3740	2998	2850	148	742	4.94	80.16

Source: Bureau of Labor Statistics, LAUS, various years.

The calibration process took place in three recursive steps.

(1) Choose initial starting values of thousands of individuals for the categories Employed, Unemployed and NILF for January 2012.
(2) Use the North Carolina transition probabilities from the regression underlying Table 2 to update the totals, month by month, for 2012, 2013 and 2014.
(3) Calculate the annual averages for Employed, Unemployed and NILF from the resulting monthly totals, and compare to the annual averages from Table A1.

Once the three steps were completed, the transition probabilities were adjusted at the margin until the calibrated series generated the annual averages. The adjustments were generally at the three-digit level of the transitional probabilities. Given that the total population of this age group was shrinking over time, I adjusted the totals during a year by reducing the transition probabilities by a common exponential contraction (e.g., all probabilities raised to 0.9957 in 2014).

Table C2: Comparison of Actual and Calibrated Annual Averages

		Population	Labor Force	Employed	Unemployed	NILF	UR	LFPR
2012	Actual	3839	3133	2896	238	706	7.60	81.61
	Calibrated	3840	3134	2896	238	706	7.59	81.59
2013	Actual	3825	3129	2924	205	695	6.55	81.83
	Calibrated	3824	3129	2925	204	695	6.53	81.83
2014	Actual	3740	2998	2850	148	742	4.94	80.16
	Calibrated	3740	2998	2850	148	742	4.95	80.16

[28] The analysis of CPS data in the last section was undertaken for ages 25-55. This one-year difference in sample group should have no significant effect on the estimated transition probabilities.

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