Evidence of Robustness of Empirical Results

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.[1] For each household, there are at most four of these pairs.[2] 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.

Table D1 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. 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 given in the text.

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.

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

[1] 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.

[2] 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.

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 in the text are conducted on household choices that are not overlapping but which occur in immediate succession.[3] In Table D2 I investigate whether the results are different if I use only the transitions observed one year apart.

The coefficient estimates of Table D2 tell the same story as those of Table 2 in the text, 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 labor-force-participation effect. We also observe a significant negative coefficient in U-to-E transition that is opposite in sign to the moral-hazard hypothesis. For those beginning out of the labor force, there is a significant increase in the N-to-E transition probability with UI reform implementation.

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

Table D1: Number of Observations per Year in CPS Panel (age 25-54)

A. Rest of the US

	Total	Employed	Unemployed	Non-participating
2000	292284	239479	7419	45386
2001	318549	258973	9834	49742
2002	342272	275672	12746	53854
2003	334914	267474	13188	54252
2004	325467	260462	11549	53456
2005	320362	257622	10432	52308
2006	316139	255546	9311	51282
2007	310443	251005	9315	50123
2008	306697	245693	11503	49501
2009	309651	238664	19662	51325
2010	307259	235181	20193	51885
2011	299411	229011	18022	52378
2012	294053	226598	15765	51690
2013	288216	222657	13793	51766
2014	285544	221916	11479	52149
2015	275865	215049	9698	51118
2016	271946	213155	8980	49811
2017	264652	209252	7902	47498
2018	254100	203155	6668	44277
2019	220288	177273	5443	37572

B. North Carolina

	Total	Employed	Unemployed	Non-participating
2000	7669	6353	160	1156
2001	7353	5979	247	1127
2002	7232	5673	319	1240
2003	7235	5681	294	1260
2004	7017	5555	237	1225
2005	6665	5314	206	1145
2006	6350	5107	193	1050
2007	6100	4894	175	1031
2008	6242	4861	286	1095
2009	6242	4664	473	1105
2010	6142	4566	477	1099
2011	5801	4331	430	1040
2012	5965	4551	350	1064
2013	5920	4486	343	1091
2014	5948	4555	234	1159
2015	6313	4793	243	1277

2016	6365	4943	217	1205
2017	6078	4657	184	1237
2018	5896	4676	153	1067
2019	5273	4204	116	953

(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.)[4] 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.[5]

Table D2: 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.

[4] 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.

[5] 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.

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).[6] The results of that set of regressions are reported in Table D3. As is evident there, the results under this formulation for transitions out of unemployment are qualitatively identical to the unadjusted specifications of Tables 2 in the text and D2. 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 value and significance in this formulation, while an N-to-U transition coefficient becomes significant taking the opposite sign.

Table D3: 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.

[6] 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.

(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 D4 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 D4: Considering the sampling weights of the CPS

Transition probability	Unweighted mean	Unweighted standard deviation	Weighted mean	Weighted standard deviation
U_to_N	0.189	0.392	0.192	0.394
U_to_E	0.236	0.425	0.231	0.421
U_to_U	0.574	0.494	0.577	0.494
N_to_N	0.888	0.317	0.887	0.317
N_to_E	0.071	0.258	0.072	0.258
N_to_U	0.040	0.197	0.041	0.199
E_to_N	0.018	0.133	0.019	0.136
E_to_E	0.971	0.168	0.970	0.172
E_to_U	0.011	0.104	0.011	0.107

Redoing the hypothesis tests of this paper using the sampling weights in a weighted-least-squares specification yields the results of Table D5. (For this calculation I return to the original definition of labor transitions used in the text.) 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 moral-hazard 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 D5: 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	0.000	0.004	0.027		-0.018	0.020	0.93		0.038	0.042	0.88
2013q2	-0.007	0.003	1.92		-0.004	0.020	0.20		-0.026	0.040	0.67
2013q3	0.003	0.004	0.63		-0.037	0.024	1.56		**0.136**	0.055	2.45
2013q4	0.003	0.004	0.67		0.004	0.020	0.18		0.095	0.055	1.72
	E to U				N to U				U to U		
2013q1	-0.003	0.003	1.24		0.010	0.014	0.73		-0.088	0.051	1.72
2013q2	**-0.007**	0.002	2.75		0.001	0.014	0.06		0.062	0.050	1.22
2013q3	-0.002	0.003	0.76		**-0.024**	0.010	2.46		-0.086	0.058	1.48
2013q4	-0.004	0.003	1.57		0.003	0.013	0.22		**-0.120**	0.060	2.01
	E to E				N to E				U to E		
2013q1	0.003	0.005	0.69		0.009	0.015	0.59		0.051	0.044	1.15
2013q2	**0.014**	0.004	3.22		0.004	0.016	0.23		-0.035	0.040	0.88
2013q3	-0.000	0.005	0.04		**0.061**	0.022	2.78		-0.050	0.041	1.24
2013q4	0.002	0.005	0.31		-0.006	0.015	0.40		0.025	0.047	0.53
N Obs			4,803,680				1,023,999				238,239
Avg Obs in period			60,046				12.800				2,978
F (159)											
	3.6	E to N			**3.0**	N to N			**5.0**	U to N	
	10.9	E to U			**10.1**	N to U			**21.5**	U to U	
	4.4	E to E			**5.9**	N to E			**17.5**	U to E	

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

(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.[7] The estimation technique is the same as that used in Table 2 of the text. The results are reported in Table D6.[8]

[7] 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.

[8] 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 in the text.

Table D6: 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	0.004	0.44	-0.024	0.019	1.28	0.030	0.043	0.68
2013q2	-0.005	0.004	1.09	0.005	0.019	0.27	-0.050	0.046	1.09
2013q3	0.003	0.004	0.69	**-0.048**	0.020	2.32	**0.122**	0.048	2.52
2013q4	0.003	0.004	0.78	0.002	0.020	0.08	0.058	0.050	1.17
	E to U			N to U			U to U		
2013q1	-0.002	0.003	0.49	0.016	0.012	1.36	-0.085	0.053	1.51
2013q2	**-0.009**	0.003	2.72	-0.003	0.012	0.27	0.051	0.056	0.92
2013q3	-0.003	0.003	0.89	-0.011	0.013	0.89	-0.100	0.059	1.69
2013q4	-0.002	0.003	0.63	0.003	0.012	0.21	-0.103	0.061	1.69
	E to E			N to E			U to E		
2013q1	-0.000	0.005	0.07	0.009	0.015	0.59	0.050	0.044	1.13
2013q2	**0.013**	0.005	2.52	-0.002	0.016	0.12	-0.001	0.047	0.03
2013q3	0.000	0.005	0.05	**0.058**	0.017	3.46	-0.021	0.049	0.43
2013q4	-0.001	0.005	0.18	-0.004	0.016	0.26	0.045	0.051	0.89
N Obs			816,800			194,995			41,114
Avg Obs in period			10,210			2,437			514
Wald (159)									
	313.4	E to N		**266.6**	N to N		**403.4**	U to N	
	621.3	E to U		**590.2**	N to U		**961.2**	U to U	
	336.2	E to E		**334.3**	N to E		**775.8**	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 moral hazard effect is not evident in these estimates, while the "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.[9] 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.

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

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.[10] The explanatory variables for the counterfactual are the average propensity of someone 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.[11] 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 D1.

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.

Figure D1: Evaluating North Carolina's U-to-N propensity by Synthetic Control

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

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

11 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).

mean squared prediction error. I obtain the RMSPE statistic for each US state, imposing the same transition 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.

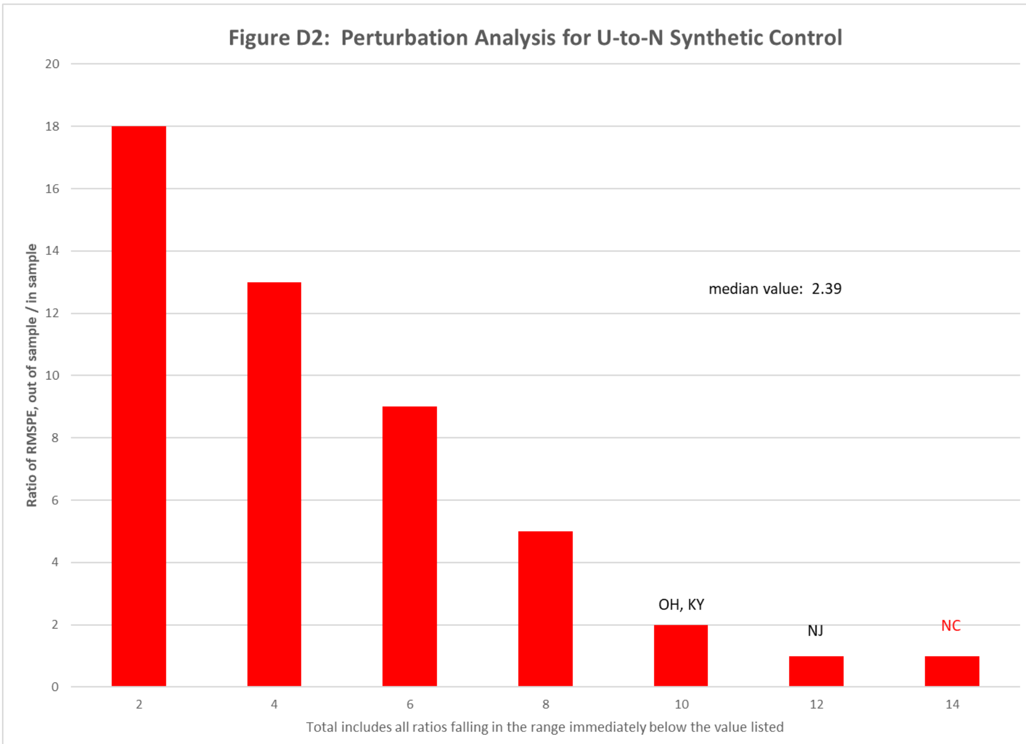


Figure D2 illustrates the result of this analysis.[12] The median value of this ratio for all states is 2.39. The ratio for North Carolina is 12.60 – over five times the median value.[13] This methodology supports the conclusions of the earlier section: the North Carolina reform was followed by an extreme jump in transition from unemployment to non-participation relative to comparator states in the US.

[12] 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).

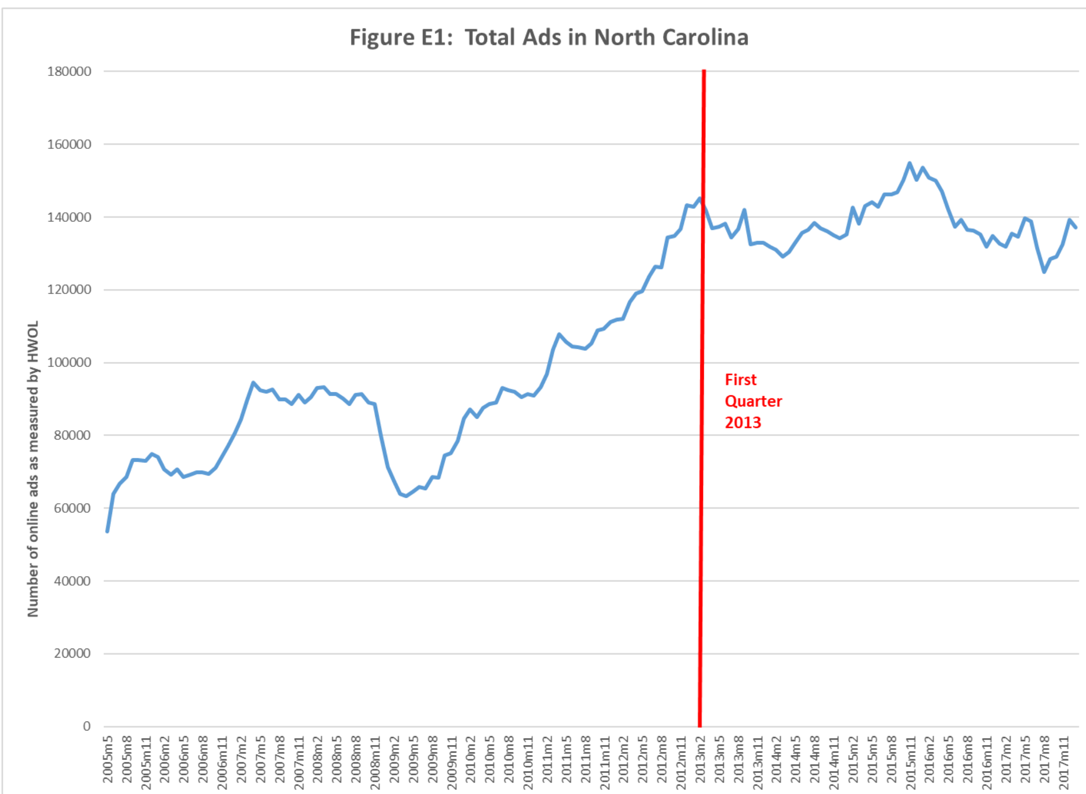
[13] Firpo and Possebom (2017) have begun work on providing confidence sets for statistical tests of the RMSPE statistic in synthetic control analysis. They demonstrate that RMSPE has uniformly greater power than other perturbation test and difference-in-difference statistics.

Appendix E: Was there a spike in hiring?

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.[14] 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.)

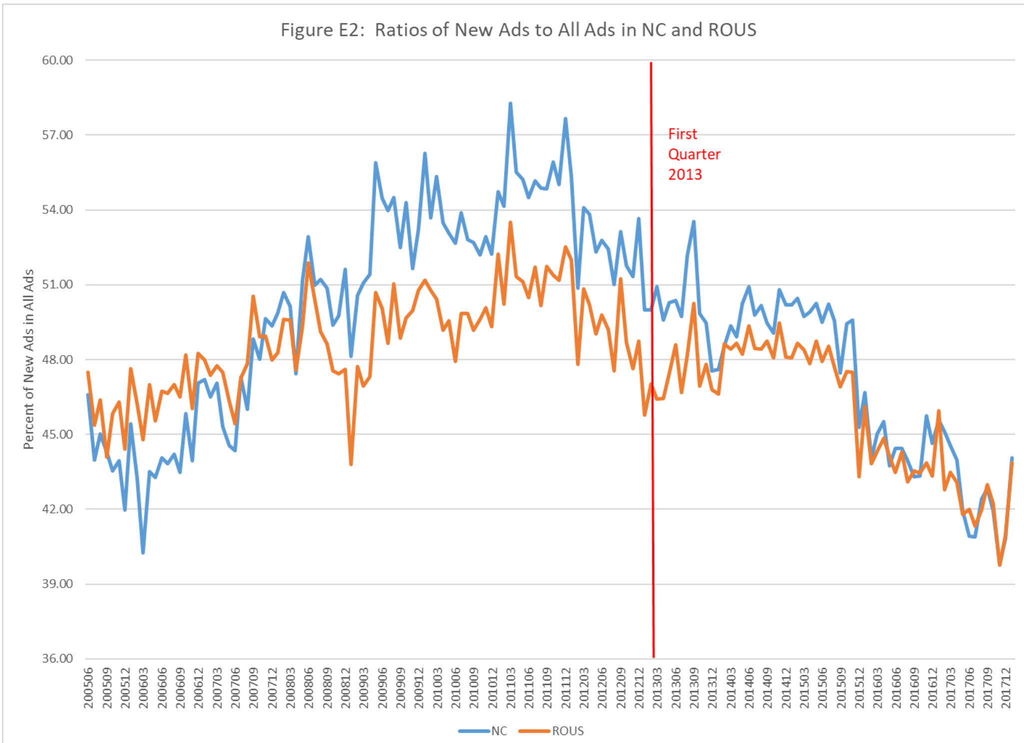
In Figure E1 I report the evolution of that ratio over time. The period between 2005 and 2010 is characterized by rapid growth in both ratios: I interpret this as an indication that firms in North Carolina were relatively slow to turn to online job listings. The peak in these ratios was in March 2013; 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.)



Source: Help Wanted Online Database

[14] 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 follows.

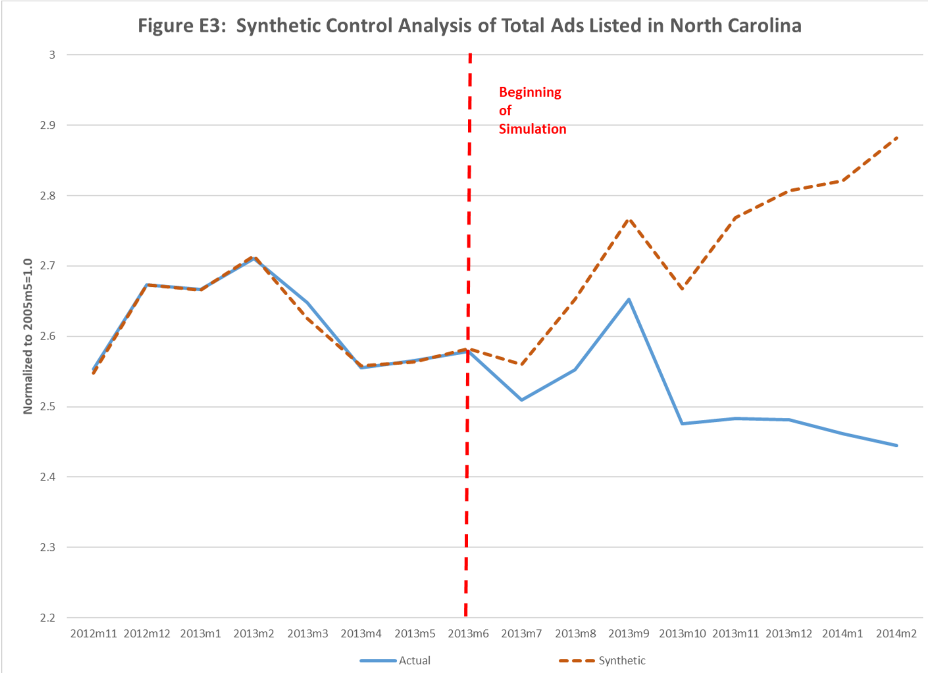
Figure E2 provides an alternative look at this job listing process by taking the ratio of new ads listed to all ads listed for North Carolina and for the rest of the US. If UI reform were to lead to new job creation, then these new jobs will need new ads and the ratio of new ads to all ads will rise in North Carolina while remaining stagnant in the rest of the US. This is not evident in the figure.



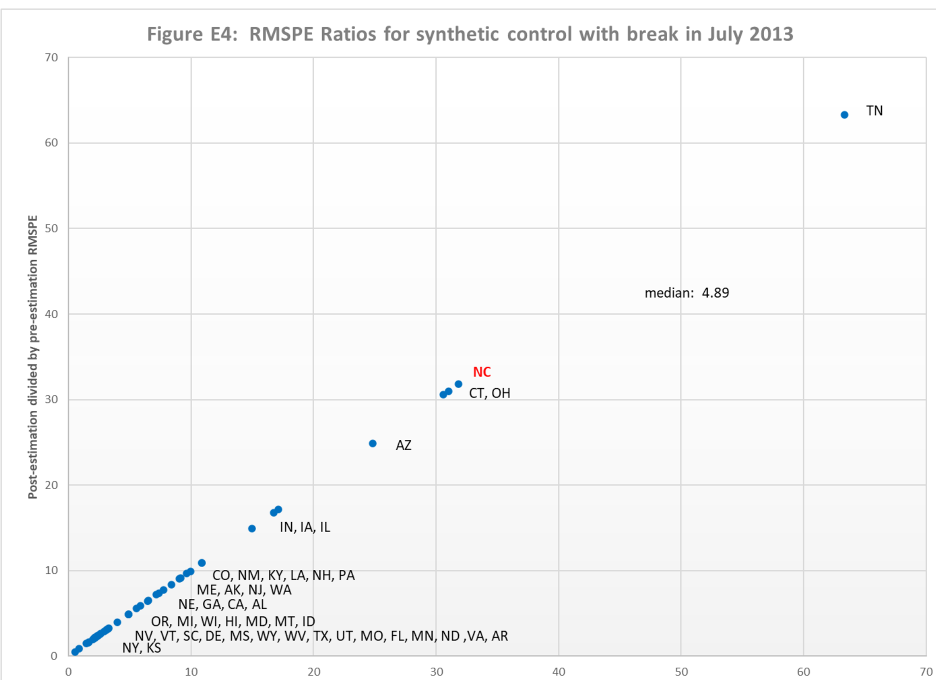
Source: Help Wanted Online Database

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 E3 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 E4 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.[15] The median value of the ratio when all states are considered is 4.89, and the value for North Carolina is 31.85.



[15] 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 E5 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 values. 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 E3 illustrates that North Carolina falls short of the counterfactual. Utah and Maine are also outliers in Figure E4 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.

Figure E5: Distribution of out-of-sample RMSPE from Synthetic Control