Supplemental Appendix for The Impact of Unemployment Benefit Extensions on Employment: The 2014 Employment Miracle? by Marcus Hagedorn and Iourii Manovskii and Kurt Mitman

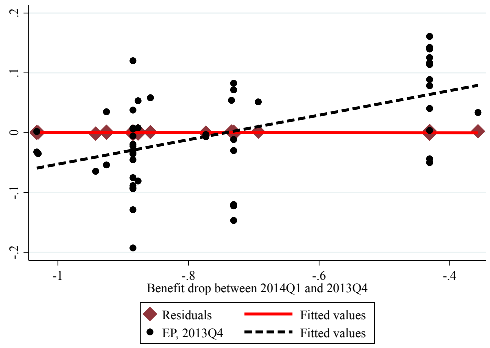
I Additional Descriptive Analysis

I.1 UI Benefit Duration and State Labor Market Performance

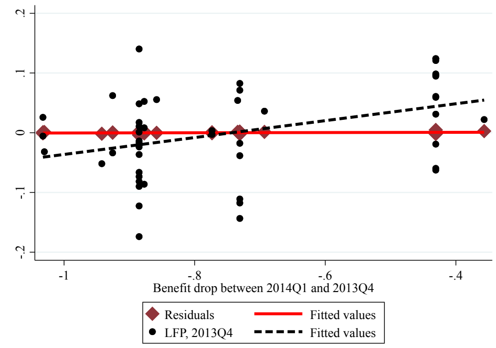
In this section we conduct a descriptive data analysis at a more disaggregated level. We first focus on the relationship between the benefit duration cut and state labor market performance before the reform. To this end, we plot the size of the drop in benefit duration in each state between 2013q4 and 2014q1 against the 2013q4 state's (cross-sectionally demeaned) level of employment in Figure 1(a) and of labor force in Figure 1(b).[1] There is a visible positive relationship, highlighted by the dotted linear regression line, implying that states with a larger drop in benefit duration (and thus generally a higher level of benefit duration at the onset of the reform) also had lower EP and LFP ratios. As expected, this confirms that benefit duration was higher in states with a worse labor market situation.

A more important question for the analysis, however, is how employment evolved across states prior to the reform. If employment started to accelerate before the reform in high benefit states and if this acceleration continued after the reform, then we might erroneously interpret the acceleration of employment as a consequence of the reform. Figures 1(c) and 1(d) illustrate this relationship. They plot each state's employment growth between 2012q4 and 2013q4 against the cut in benefit duration it experienced between 2013q4 and 2014q1. We observe a weaker but still noticeable positive relationship, implying that rather than accelerating, EP and LFP in fact grew *slower* prior to the reform in states with larger cuts (and a higher pre-reform level) of benefit duration.

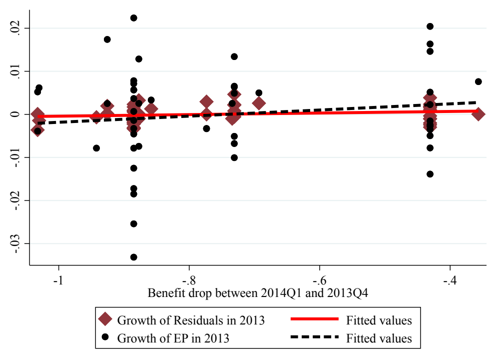
[1] Formally, let b_{it} and x_{it} be the log of benefit duration and the log of EP or LFP in state i in quarter t, respectively. The Figures 1(a) and 1(b) then plot x_{2013Q4} against $(b_{2014Q1} - b_{2013Q4})$, while Figures 1(c) and 1(d) plot $(x_{2013Q4} - x_{2012Q4})$ against $(b_{2014Q1} - b_{2013Q4})$.



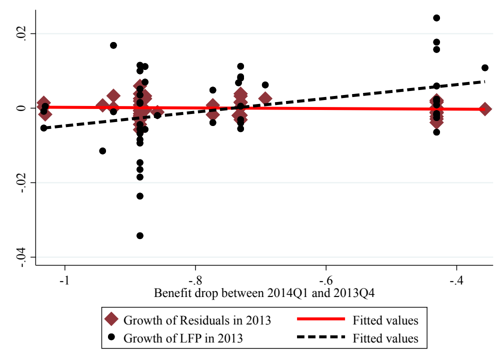
(a) Employment



(b) Labor Force



(c) Employment



(d) Labor Force

Figure A-1: Panels 1(a) and 1(b): Level of EP or LFP in 2013Q4 and the cut in benefit duration induced by the reform; Panels 1(c) and 1(d): Change of EP or LFP between 2012Q4 and 2013Q4 and the cut in benefit duration induced by the reform. EP and LFP show the relationship between the cut in benefit duration and labor market outcomes across states. The residuals are from our formal analysis in Section **??** where this relationship is not present.

In Section **??** we use flexible models of state-level employment trends to make sure that we measure the effects of the reform and not the continuation of the pre-reform trends. The red solid line in every panel of Figure A-1 indicates, that there is no relationship between the level or growth in EP and LFP in 2013 and the cut in benefit duration during the reform after these trends are accounted for.

I.2 Difference-in-Differences Analysis

In this section we apply a simple "difference-in-differences" analysis as the double differencing eliminates potential linear state-specific pre-trends. In Figure 2(a) we plot the difference in the growth rate of employment to population ratio in 2014 and in 2013 against the difference in the growth rate of unemployment benefit duration between those two years for each state. Similarly, in Figure 2(b) we plot the difference in the growth rate of labor force participation in 2014 and in 2013 against the difference in the growth rate of unemployment benefit duration between those two years for each state.[2] The standard logic of "difference-in-differences" ensures that the high growth of employment and labor force in the states with high benefit duration drops in 2014 were not a continuation of trends already present in 2013, ruling out this potential bias.

As evident from the figures, states that saw larger declines in benefit duration in 2014 relative to 2013 also experienced an acceleration in employment and labor force growth. While there is heterogeneity in labor market dynamics across states, the overall pattern is unambiguous with the slope of the linear regression line through these points being negative and highly statistically significant: -0.0284 (s.e. 0.0059) for EP, and -.0241 (s.e. 0.0071) for LFP.

In Figures 2(c) and 2(d), we report the corresponding plots where the unit of analysis is not a state but a border between two adjacent states. Specifically, we first difference employment and labor force growth in a given year between two adjacent states (defined as the difference in EP or LFP in the state with higher benefits at the end of 2013 minus EP or LFP in the state with lower benefits). On the vertical axes of Figures 2(c) and 2(d) we have the difference in these differences in EP and LFP growth, respectively. On the horizontal axes we have the difference between 2014 and 2013 in the differences of benefit duration growth between adjacent states in those years.[3] As neighboring states are expected to have more similar employment

[2]Formally, let b_{it} and x_{it} be the log of benefit duration and the log of EP or LFP in state i in quarter t, respectively. The figure then plots $(x_{2014Q4} - x_{2013Q4}) - (x_{2013Q4} - x_{2012Q4})$ against $(b_{2014Q4} - b_{2013Q4}) - (b_{2013Q4} - b_{2012Q4})$.

[3]Formally, let $\Delta b_{ijt} = b_{it} - b_{jt}$ and $\Delta x_{ijt} = x_{it} - x_{jt}$ be the difference in log of benefit duration and the log of EP or LFP between bordering states i and j in quarter t, respectively. State i is the one with higher benefit duration in 2013Q4 relative to state j. Figures 2(c) and 2(d) then plot $(\Delta x_{ij,2014Q4} - \Delta x_{ij,2013Q4}) - (\Delta x_{ij,2013Q4} - \Delta x_{ij,2012Q4})$ against $(\Delta b_{ij,2014Q4} - \Delta b_{ij,2013Q4}) - (\Delta b_{ij,2013Q4} - \Delta b_{ij,2012Q4})$.

and labor force trends than locations that are further apart geographically, such triple differencing helps to eliminate the potential effect of such trends in addition to eliminating linear state-level pre-trends. The results once again reveal a clear tendency for employment and labor force growth to accelerate in the states experiencing larger benefit declines in 2014 relative to 2013. The negative slope of the linear regression line through these points is slightly larger than in the state-level analysis: -.0329 (s.e. 0.0026) for EP, and -.0260 (s.e. 0.0031) for LFP.

How exceptional are these patterns? In other words, could have we expected the remarkable acceleration of employment and labor force growth in states that experienced an acceleration in the decline in benefit duration in 2014 had benefit duration not been cut? To address this question we perform a placebo analysis where we counterfactually assume that the nationwide benefit cut occurred in some quarter preceding the date of the actual reform. Figure A-3 summarizes the slopes of the regression lines of the scatter plots such as in Figure A-2 constructed in every quarter between 2011Q1 and 2012Q4 by assuming (counterfactually) that benefit extensions were eliminated in that quarter. This evidence suggests that the patterns observed during the actual reform at the end of 2013 are indeed exceptional.

I.3 Do the Effects of a Benefit Cut Vary with Pre-Reform EP or LFP?

We now consider the groups of states that experienced the same cut in benefits due to the reform and ask whether the effects of the reform differed within those groups depending on the state's employment just before the reform. Specifically, we regress the change in the growth rate of labor market outcomes (employment or labor force) between 2014 and 2013 on the change in benefits induced by the reform and the de-meaned level of employment in 2013Q4 interacted with the change in benefits induced by the reform. The interaction term captures how states that had different pre-reform labor market conditions but experienced the same cut in benefits responded differentially to the reform. We find that the coefficient on the interaction terms in both the EP and LFP regressions are statistically insignificant, indicating no significant heterogeneity in the effect of treatment.

Table A-1: Do the Effects of a Benefit Cut Vary with Pre-Reform EP or LFP?

	(1)	(2)	(3)	(4)
VARIABLES	Diff in Growth of EP		Diff in Growth of LFP	
Benefit cut	-0.0254***	-0.0254***	-0.0237***	-0.0237***
	(0.00740)	(0.00752)	(0.00846)	(0.00862)
Group Employment		-0.0364		
		(0.0927)		
Interaction		-0.0640		
		(0.125)		
Group Labor force				0.0514
				(0.111)
Interaction				0.0581
				(0.147)
Observations	50	50	50	50
R-squared	0.198	0.205	0.141	0.146

Standard errors in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

I.4 Persistence of Labor Force and Employment among U.S. States

In the top two panels of Figure A-4 we plot state-level EP and LFP just before the benefit cut 2013Q4 against state-level EP and LFP in 2006Q4, before the Great Recession. In the bottom two panels, we plot EP and LFP in 2013Q4 against their values in 1996Q4. The red dashed line in each panel is the 45 degree line. While both EP and LFP are lower in 2013Q4 than in earlier years, the differences of EP and LFP across states are very persistent over time. Indeed, a simple linear regression of EP in 2013Q4 on EP in 1996Q4 yields a coefficient of 0.934 (s.e. 0.113) and similarly for LFP a coefficient of 0.998 (s.e. 0.118), indicating the high-persistence of labor market variables at the state level.

II Aggregation: A Simple Trade Model of the US

In this section we show that we can use our estimates based on (border) states to derive the implications for the induced employment changes for the aggregate U.S. economy. To this aim, we develop a standard model where we show that our aggregation methodology of Section **??** is exact. In this model each state is an open economy inside the (closed) US economy. The labor market in each state is governed by a Mortensen Pissarides search and matching model. Each state produces (and consumes) a nontradable and a tradable good. Both sectors, the one producing the tradable good and the one producing the nontradable one, operate in the same labor market and are subject to the same labor market frictions as in a standard Mortensen Pissarides model. The evidence provided in Section **??** implies that the unemployed do not change where to search for a job in response to changes in benefits. In the model, which we use to study a policy change in benefits, we therefore assume that unemployed search for jobs in their own state only.

Specifically, each state is described by a discrete time two sector version of the Pissarides (1985, 2000) search and matching model. There is a measure one of infinitely lived workers and a continuum of infinitely lived firms. Workers maximize their expected lifetime utility:

$$\mathbb{E}\sum_{t=0}^{\infty}\delta^t y_t, \tag{A1}$$

where y_t represents income in period t and $\delta \in (0,1)$ is workers' and firms' common discount factor. We denote the sectors producing tradable and non-tradable goods by $\Omega \in \{T, NT\}$. Firms in both the tradable ($\Omega = T$) and the non-tradable ($\Omega = NT$) sector have a constant returns to scale production technology that uses labor as the only input (Pissarides (2000) shows that capital can be added to the model leaving all equations unchanged).

Output of each unit of labor in sector Ω is denoted by A^{Ω}. There is free entry of firms into both sectors. Firms attract unemployed workers by posting a vacancy at the flow cost c. The price of the tradable good is normalized to one ($p^T = 1$) and the price of the non-tradable is denoted p^{NT}. Once matched, workers and firms separate exogenously with probability s per period. Employed workers in sector Ω are paid a wage w^{Ω}, and firms in sector Ω make accounting profits $p^{\Omega}A^{\Omega} - w^{\Omega}$ per worker each period in which they operate. Unemployed workers get flow utility z from leisure/non-market activity. Unemployed workers can search in either one of the two sectors. In equilibrium they are indifferent in which sector to search. Workers and firms split the surplus from a match according to the generalized Nash bargaining solution. The bargaining power of workers is $\beta \in (0, 1)$.

Let u^Ω denote the unemployed searching in sector Ω, e^Ω employment in sector Ω and v^Ω the number of vacancies posted in sector Ω. We refer to $\theta^\Omega = v^\Omega / u^\Omega$ as the market tightness in sector Ω. The number of new matches in each sector is given by a constant returns to scale matching function $m(u^\Omega, v^\Omega)$. Employment in each sector evolves according to the following law of motion:

$$n_{t+1}^\Omega = (1 - s)n_t^\Omega + m(u_t^\Omega, v_t^\Omega). \tag{A2}$$

The probability for an unemployed worker searching in sector Ω to be matched with a vacancy next period equals $f(\theta_t^\Omega) = m(u_t^\Omega, v_t^\Omega)/u_t^\Omega = m(1, \theta_t^\Omega)$. The probability for a vacancy in sector Ω to be filled next period equals $q(\theta_t^\Omega) = m(u_t^\Omega, v_t^\Omega)/v_t^\Omega = m(1/\theta_t^\Omega, 1) = f(\theta_t^\Omega)/\theta_t^\Omega$. We restrict $m(u_t^\Omega, v_t^\Omega) \leq \min(u_t^\Omega, v_t^\Omega)$.

In each sector Ω, denote the firm's value of a job (a filled vacancy) by J^Ω, the firm's value of an unfilled vacancy by V^Ω, the worker's value of having a job by W^Ω, and the worker's value of being unemployed and searching in sector Ω by U^Ω.

$$J^\Omega = p^\Omega A^\Omega - w^\Omega + \delta(1 - s)J^{\Omega'} \tag{A3}$$
$$V^\Omega = -c + \delta q(\theta^\Omega) J^{\Omega'} \tag{A4}$$
$$U^\Omega = z + \delta\{f(\theta^\Omega)W^\Omega + (1 - f(\theta^\Omega))U^{\Omega'}\} \tag{A5}$$
$$W^\Omega = w^\Omega + \delta\{(1 - s)W^{\Omega'} + sU^{\Omega'}\}. \tag{A6}$$

The interpretation is straightforward. Operating firms earn profits $p^\Omega A^\Omega - w^\Omega$ and the matches are exogenously destroyed with probability s. A vacancy costs c and is matched with a worker (becomes productive next period) with probability $q(\theta^\Omega)$. An unemployed worker derives utility z and finds a job next period with probability $f(\theta^\Omega)$. An employed worker earns wage w^Ω but may lose her job with probability s and become unemployed next period.

Nash bargaining with worker bargaining power β implies that a worker and a firm split the surplus $S^\Omega = J^\Omega + W^\Omega - U^\Omega$ such that

$$J^\Omega = (1 - \beta)S \tag{A7}$$
$$W^\Omega - U^\Omega = \beta S^\Omega. \tag{A8}$$

Free entry implies that the value of posting a vacancy is zero: $V^{\Omega} = 0$ and, therefore,

$$c = \delta q(\theta^{\Omega})(1-\beta)S^{\Omega'}. \tag{A9}$$

As shown in Hagedorn and Manovskii (2008), the steady state surplus equals

$$S^{\Omega} = \frac{p^{\Omega}A^{\Omega} - z}{1 - \delta(1-s) + \delta f(\theta^{\Omega})\beta}. \tag{A10}$$

Plugging this into the free entry condition yields:

$$\frac{p^{\Omega}A^{\Omega} - z}{1 - \delta(1-s) + \delta f(\theta^{\Omega})\beta} = \frac{c}{\delta q(\theta^{\Omega})(1-\beta)}, \tag{A11}$$

and, equivalently,

$$\frac{1 - \delta(1-s)}{\delta q(\theta^{\Omega})} + \beta\theta^{\Omega} = \frac{p^{\Omega}A^{\Omega} - z}{c}(1-\beta). \tag{A12}$$

Since unemployed workers are indifferent between searching in the tradable or in the non-tradable sector, $U^T = U^{NT}$, which implies that $A^T = p^{NT}A^{NT}$ (where we used the normalization $p^T = 1$), $\theta^T = \theta^{NT}$ and $w^T = w^{NT}$. To see this, suppose $A^T \neq p^{NT}A^{NT}$, say $A^T > p^{NT}A^{NT}$. Then, the above equations imply that $\theta^T > \theta^{NT}$ and since wages equal

$$w^T = \beta A^T + (1-\beta)z + c\beta\theta^T \tag{A13}$$

$$w^{NT} = \beta p^{NT}A^{NT} + (1-\beta)z + c\beta\theta^{NT}, \tag{A14}$$

we also have $w^T > w^{NT}$. As a a result, $U^T > U^{NT}$, implying by contradiction that $A^T = p^{NT}A^{NT}$, which then implies $\theta^T = \theta^{NT}$ and $w^T = w^{NT}$.

This implies that the employment rate in state i equals $e_i = \frac{f(\theta_i)}{s+f(\theta_i)}$, where $\theta_i = \theta^T = \theta^{NT}$. Employment in the U.S. then equals

$$E = \sum_{\text{All U.S. states } i} e_i L_i, \tag{A15}$$

where L_i is labor force in state i (= population in state i in the simple Mortensen Pissarides model we use here. Below we discuss how to extend the analysis to allow for an endogenous labor force).

In the empirical analysis we compare the employment population ratio e_i for state i before and after the reform, and obtain the difference in log employment $\log(e_i^{after}) - \log(e_i^{before}) + \log(L_i^{after}) - \log(L_i^{before})$, where in this simple model the labor force L_i is fixed. Our regression then delivers an estimate of the change in employment in state i w.r.t. an increase in benefit duration in state i, since employment in state i does not depend on the benefit level in other states. To see this, note that since θ_i solves

$$\frac{1-\delta(1-s)}{\delta q(\theta_i)} + \beta\theta_i = \frac{A^{T,i} - z_i}{c}(1-\beta), \tag{A16}$$

it just responds to changes in z_i but not to changes in z_j in some other state j.[4] The increase in the employment rate (=employment population ratio) in state i is

$$\mu_i^E = \tilde{\beta}_4(b_i^{2014Q4} - b_i^{2013Q4})e_i^{2013Q4}, \tag{A17}$$

where where b_i^{2013Q4} and b_i^{2014Q4} denote the logarithm of the number of weeks of benefits available in state i in $2013Q4$ (just prior to the policy change) and in $2014Q4$ and $\tilde{\beta}_4$ is the estimated cumulative effect.

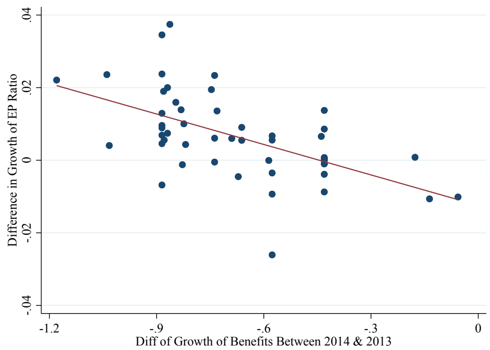
The previous analysis then implies that we can aggregate these employment changes for states i. We compute the policy induced change in employment for the whole U.S. as

$$\pi^E = \sum_{\text{All U.S. states } i} \mu_i^E L_i^{2013Q4}. \tag{A18}$$

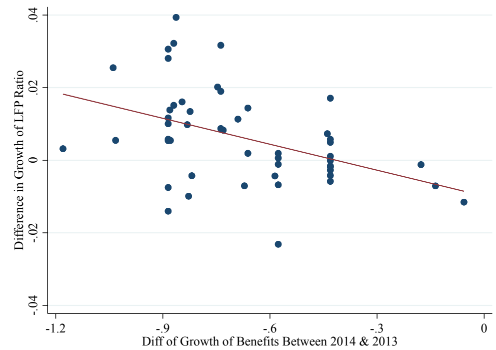
The same derivations for employment can be applied to the labor force as well. Indeed, Pissarides (2000) shows in a more complex model, where heterogenous households take a participation decision, that the labor force can be written as a function of market tightness as well. The same derivations as above in the more elaborated model imply that labor force in a state depends on market tightness in that state only (and not on market tightness in other state). Our regression then delivers again an estimate of the elasticity of the labor force in state i w.r.t. an increase benefit duration in state i (since

[4]In the empirical analysis we also compare two neighboring states i and j. The same arguments apply to this setup as well. The difference in log employment is $\log(e_i) - \log(e_j) + \log(L_i) - \log(L_j)$. Our regression then delivers an estimate of the elasticity of employment in state i w.r.t. an increase benefit duration in state i, since employment in state i does not depend on the benefit duration in other states.

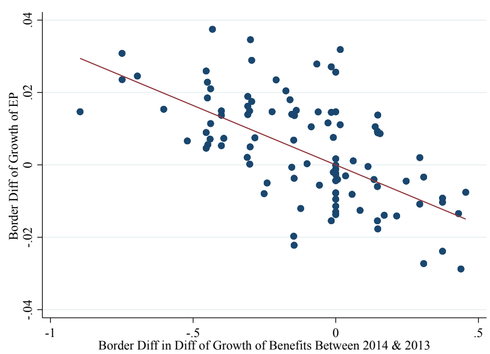
labor force in state i does not depend on the benefit level in other states). And we can again use this estimate at the state level to compute the percentage change of the aggregate labor force as well as the change in the total number of labor force participants.



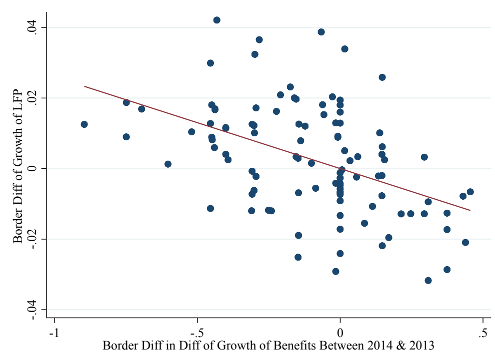
(a) Employment States



(b) Labor Force States

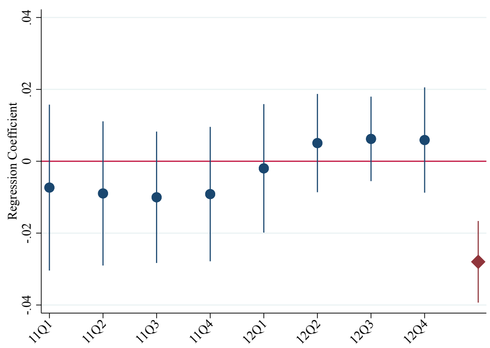


(c) Employment State Borders

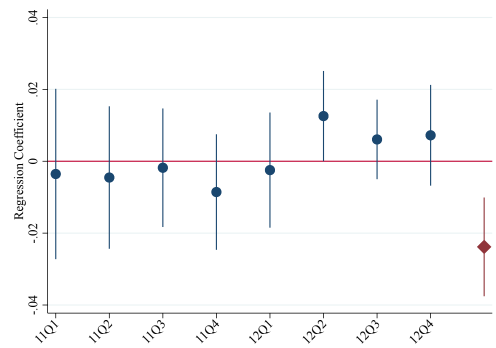


(d) Labor Force State Borders

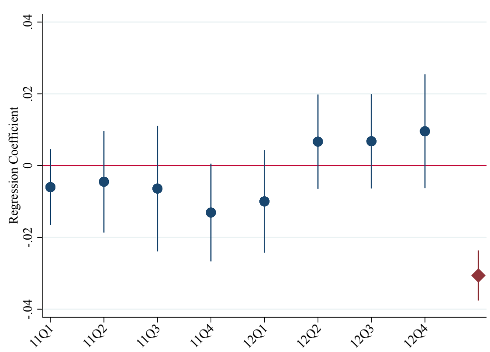
Figure A-2: Difference in growth rates of EP or LFP in 2014 and 2013 vs. the difference in growth rates of benefit duration in 2014 and 2013 across states and bordering state pairs.



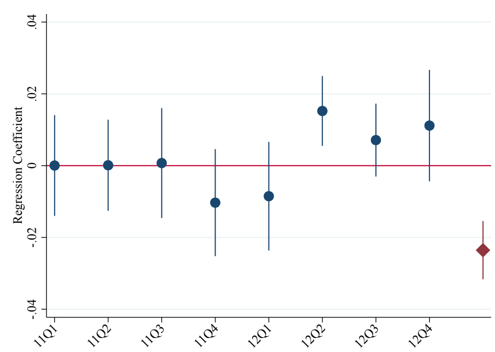
(a) Employment States



(b) Labor Force States

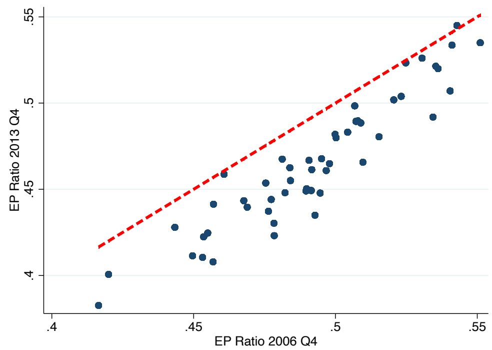


(c) Employment State Borders

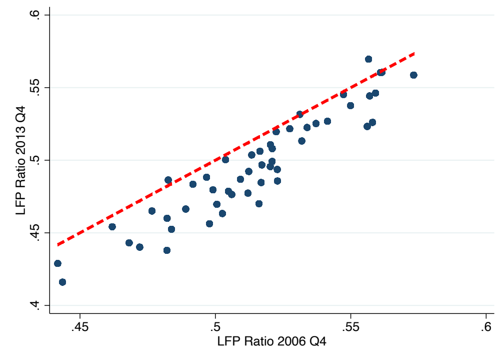


(d) Labor Force State Borders

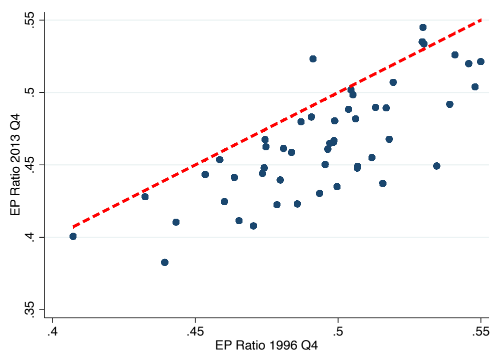
Figure A-3: Slopes of the regression line of the difference in growth rates of EP or LFP over the 4 quarters after and 4 quarters before the quarter marked on the horizontal axis on the corresponding difference in growth rates of benefit duration, where the forward difference in benefit duration counterfactually assumes that benefit extensions were eliminated. States and bordering state pairs. Rightmost point on each panel corresponds to actual reform in 2013q4.



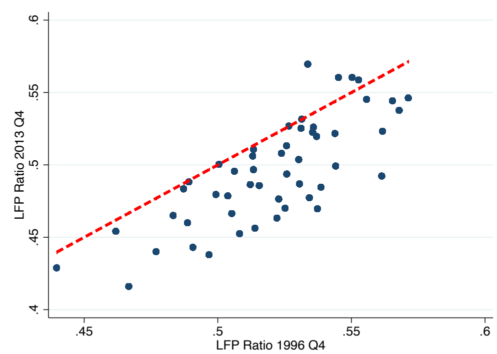
(a) Employment



(b) Labor Force



(c) Employment



(d) Labor Force

Figure A-4: EP and LFP in 2013Q4 against their values in 2006 Q4 (top panels) and 1996 Q4 (bottom panels). The red dashed line in each panel is the 45 degree line.

III Sensitivity of Baseline Findings

III.1 Evidence on Reallocation and Mobility

Our baseline aggregate estimates were based on the assumption of no reallocation or mobility across states. If there is substantial reallocation of economic activity in response to the cut in benefit duration, we would expect to find a decrease in the ratio of employment in non-tradable to tradable sectors in states with the largest cuts in benefits. To assess this possibility, we apply our empirical methodology to measure the change in employment in sectors producing output that is plausibly non-tradable across states, such as leisure and hospitality, to the change in employment in tradable sectors, such as manufacturing. We find that a cut in benefit duration has no significant effect (a coefficient of -0.0124, s.e. 0.0106) on the relative employment in the two sectors, implying that the null hypothesis of no reallocation induced by benefit extensions cannot be rejected in the data. In addition, Hagedorn et al. (2015) use the Nielsen Consumer Panel Data to measure the responsiveness of cross state border shopping to changes in unemployment benefit generosity. Their results indicate that this effect is also negligible.

A second concern is from workers who live and work in different states. This type of worker reallocation would bias even our state-level estimates if the households systematically change their job search behavior in response to changes in unemployment benefits. For example, suppose households search in states with less generous benefits to take advantage of a higher job-finding rate. As employment is measured based on the place of residence and not on the basis of the location of the job, our estimate of the effect of benefit extensions on employment would be biased *downwards*, since some households residing in high benefit states would face a higher job-finding rate, which would translate into higher employment in their state of residence (despite them actually working in the neighboring state). To investigate whether this is the case, we use direct empirical evidence on where people work relative to where they live. Specifically, we use data from the American Community Survey which is an annual 1% survey of households in the United States conducted by the Census Bureau. The survey contains information on the household's state of residence and state of employment. The share of individuals in our sample who worked in a different state from the one they lived in at the time of the reform is 1.68%. Regressing the difference in log share of individuals working in a different state from the one they live in on differences in log benefits duration between 2013 and 2014, we find a very small and statistically insignificant coefficient on weeks of benefits available of -0.038 (s.e. 0.097).[5] This evidence implies that workers' search behavior does not respond significantly to changes in local unemployment benefit duration.

III.2 Number of Lags

Our analysis of the data reveals that residuals become serially uncorrelated if three or more lags of the dependent variable are included in the specification.[6] We selected the specification with three lags as the benchmark but report in Table A-2 that including more lags has little impact on the estimated effects of the cut of benefit duration on employment and the labor force. This indicates that the parsimonious baseline specification is sufficient to control well for the dynamics of the variables of interest.

If we instead rely on the Akaike Information Criterion or the Bayesian (Schwarz) Information Criterion to select the number of lags, we would select only 2 lags. The results corresponding to this specification in Table A-2 suggest only a minor impact on the coefficient of interest.

[5]The interpretation is that in response to a cut in benefits from 53 to 25 weeks the share of workers employed in a different state from the one where they live would increase from 1.68% to 1.72%.

[6]We select this strategy based on the results of a Monte Carlo study in which we simulate data from the specification in (**??**) with residuals modeled as an AR(1) process. We then estimate the benchmark specification on these simulated data by choosing the minimal number of lags sufficient to reject that the residuals are serially correlated. We find that this requires estimating additional lags relative to the true underlying DGP. The estimated cumulative effects of treatment, however, are virtually unaffected despite the different estimated lag structure.

Table A-2: Additional Lags

VARIABLES	$\tilde{\beta}_1$	$\tilde{\beta}_2$	$\tilde{\beta}_3$	$\tilde{\beta}_4$
		Employment to Population Ratio		
2 Lags	-0.00436***	-0.0109***	-0.0170***	-0.0213***
	(0.000451)	(0.00107)	(0.00191)	(0.00312)
4 Lags	-0.00400***	-0.0104***	-0.0165***	-0.0209***
	(0.000547)	(0.00122)	(0.00191)	(0.00301)
5 Lags	-0.00395***	-0.0103***	-0.0163***	-0.0206***
	(0.000564)	(0.00126)	(0.00200)	(0.00314)
6 Lags	-0.00379***	-0.0100***	-0.0159***	-0.0201***
	(0.000588)	(0.00123)	(0.00184)	(0.00302)
12 Lags	-0.00360***	-0.00974***	-0.0160***	-0.0203***
	(0.000674)	(0.00174)	(0.00314)	(0.00454)
24 Lags	-0.00376***	-0.00950***	-0.0154***	-0.0203***
	(0.000841)	(0.00200)	(0.00334)	(0.00497)
		Labor Force to Population Ratio		
2 Lags	-0.00332***	-0.00739***	-0.0117***	-0.0160***
	(0.000422)	(0.00107)	(0.00222)	(0.00348)
4 Lags	-0.00309***	-0.00674***	-0.0108***	-0.0149***
	(0.000462)	(0.00103)	(0.00214)	(0.00346)
5 Lags	-0.00308***	-0.00671***	-0.0107***	-0.0147***
	(0.000464)	(0.00102)	(0.00209)	(0.00341)
6 Lags	-0.00297***	-0.00649***	-0.0104***	-0.0143***
	(0.000464)	(0.00104)	(0.00211)	(0.00349)
12 Lags	-0.00302***	-0.00656***	-0.0106***	-0.0148***
	(0.000520)	(0.00120)	(0.00231)	(0.00374)
24 Lags	-0.00322***	-0.00700***	-0.0110***	-0.0151***
	(0.000642)	(0.00156)	(0.00288)	(0.00434)

Robust standard errors clustered by state and time in parentheses
*** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table A-3: Benchmark Results, Weighted by State Population

VARIABLES	$\tilde{\beta}_1$	$\tilde{\beta}_2$	$\tilde{\beta}_3$	$\tilde{\beta}_4$
EP	-0.00423***	-0.0101***	-0.0154***	-0.0188***
	(0.000692)	(0.00181)	(0.00261)	(0.00327)
LFP	-0.00332***	-0.00626***	-0.00971***	-0.0140***
	(0.000714)	(0.00133)	(0.00179)	(0.00236)

Robust standard errors clustered by state and time in parentheses
*** p<0.01, ** p<0.05, * p<0.1

III.3 Weighting

U.S. states differ in population size. We account for this when aggregating our state-level estimates to obtain an estimate of the nation-wide impact. We do not weight the states by population in estimation as there is no economic or econometric rationale for doing so. It is instructive, however, to compare weighted and unweighted results because this comparison reveals the potential presence of heterogeneity of the impact of the cut in benefits. A comparison of the results of Table A-3 where states are weighted by population size in estimation with the results in Table **??** where they are not weighted, reveals that the heterogeneity in the impact of the cut in benefits is minimal in our data.

III.4 Alternative Specifications of State-Specific Time Trends

Controlling for pre-existing state-specific trends in the labor force and employment is required to obtain unbiased estimates of policy effects.[7] The traditional specification in the literature that exploits cross-state variation in economic policies (e.g., minimum wages) to infer their impact on employment, typically includes linear state-specific time trends as controls. To assess whether this model of state-level employment dynamics affects our in-

[7]However, even downward biased estimates obtained from the specification that does not include any controls for trends, are economically large and statistically significant: $\hat{\tilde{\beta}}_4 = -0.014$, s.e. 0.0018 for employment to population ratio and $\hat{\tilde{\beta}}_4 = -0.0087$, s.e. 0.0036 for labor force to population ratio.

Table A-4: Alternative Specifications of State-Specific Time Trends

VARIABLES	$\tilde{\beta}_1$	$\tilde{\beta}_2$	$\tilde{\beta}_3$	$\tilde{\beta}_4$
		Employment to Population Ratio		
Linear Trend	-0.00336***	-0.00755***	-0.0124***	-0.0174***
	(0.000686)	(0.00174)	(0.00315)	(0.00485)
2006 control	-0.00402***	-0.00989***	-0.0154***	-0.0196***
	(0.000460)	(0.00101)	(0.00163)	(0.00265)
Both 2013 & 2006 controls	-0.00414***	-0.0107***	-0.0170***	-0.0218***
	(0.000773)	(0.00195)	(0.00290)	(0.00416)
		Labor Force to Population Ratio		
Linear Trend	-0.00267***	-0.00477***	-0.00708**	-0.0103**
	(0.000541)	(0.00150)	(0.00286)	(0.00415)
2006 control	-0.00306***	-0.00651***	-0.00994***	-0.0134***
	(0.000465)	(0.000867)	(0.00169)	(0.00288)
Both 2013 & 2006 controls	-0.00300***	-0.00639***	-0.00988***	-0.0135***
	(0.000674)	(0.00145)	(0.00243)	(0.00365)

Robust standard errors clustered by state and time in parentheses
*** p<0.01, ** p<0.05, * p<0.1

ference of the effect of the change in unemployment benefits, we replace the flexible model of state-specific trends in the benchmark specification with linear state-specific trends ζ_i:

$$x_{i,t} = \sum_{\tau=1}^{4} \beta_\tau \mathbf{1}_{t=2014Q\tau}(b_{i,t} - b_{i,2013Q4}) + \sum_{j=1}^{n} \gamma_j x_{i,t-j} + \zeta_i \times t + \eta_i + \delta_t + \epsilon_{i,t}. \quad \text{(A19)}$$

The results of estimating this specification are reported in rows labeled “Linear Trend” in Table A-4. The estimated effects are slightly smaller but are not substantively different from those in the benchmark specification.

The fact that the estimated policy effects estimated using this more rigid specification that also uses post-reform data in the estimation are slightly smaller, is consistent with the recent critique of this specification by e.g., Meer and West (2016). Economic theory implies that employment effects of policy changes are not instantaneous so that policy reforms affect the growth rate of employment (at least during the transition). In this case following the traditional approach and estimating state-specific trends will attenuate the estimated policy treatment effect (and inflate standard errors). Or baseline specification avoids these concerns because the inclusion of lagged variables, $\sum_{j=1}^{n} \gamma_j x_{i,t-j}$, already captures the sluggish adjustment of labor market variables emphasized by these authors. Furthermore, our estimation of a flexible state-specific time trend is based on pre-reform values of the outcome variable only.

In the baseline specification, state-specific trends depend on the level of the outcome variable at the end of 2013, i.e., right before the reform. This directly controls for the possibility that the time of the policy reform coincided with the unusual turning point in employment dynamics whereby employment growth unexpectedly accelerated (for reasons unrelated to the reform) in the the states with low employment the eve of the reform.

An alternative that we consider next models state-specific trends as a function of the outcome variable in 2006, i.e., not only pre-reform, but also pre-recession. This addresses the concern that the recession induced different trends across states, depending on their employment level in 2006 (that reflected, say, the heterogeneous impact of the housing boom). Specifically, we replace $\nu_t \tilde{x}_{i,2013Q4}$ in the baseline specification with $\varsigma_t x_{i,2006}$, i.e., the interaction between the time dummy ς_t with the (cross-sectionally demeaned) average level of the outcome variable in 2006:

$$x_{i,t} = \sum_{\tau=1}^{4} \beta_\tau \mathbf{1}_{t=2014Q\tau}(b_{i,t} - b_{i,2013Q4}) + \sum_{j=1}^{n} \gamma_j x_{i,t-j} + \varsigma_t x_{i,2006} + \eta_i + \delta_t + \epsilon_{i,t}. \tag{A20}$$

The results, summarized in rows labeled “2006 control” of Table A-4, imply that this specification yields very similar estimates to the baseline ones.

Finally, we combine the preceding specification with the baseline one,

$$x_{i,t} = \sum_{\tau=1}^{4} \beta_\tau \mathbf{1}_{t=2014Q\tau}(b_{i,t} - b_{i,2013Q4}) + \sum_{j=1}^{n} \gamma_j x_{i,t-j} + \nu_t x_{i,2013Q4} + \varsigma_t x_{i,2006} + \eta_i + \delta_t + \epsilon_{i,t}. \tag{A21}$$

Despite the added flexibility in this model of state-specific trends in labor force and employment, the estimated coefficients of interest remain little

Table A-5: Unemployment Benefit Extensions, Employment and Labor Force:
Findings from a Specification with a Latent Factor Model.

VARIABLES	$\tilde{\beta}_1$	$\tilde{\beta}_2$	$\tilde{\beta}_3$	$\tilde{\beta}_4$
EP	-0.00414**	-0.0106***	-0.0168***	-0.0214***
	(0.00165)	(0.00350)	(0.00525)	(0.00677)
LFP	-0.00314*	-0.00674**	-0.0106**	-0.0145**
	(0.00163)	(0.00335)	(0.00489)	(0.00616)

Robust standard errors clustered by state and time in parentheses
*** p<0.01, ** p<0.05, * p<0.1

changed, as reported in rows labeled "Both 2013 & 2006 controls" of Table A-4.

III.5 Factor Model

In this robustness check we allow for aggregate shocks that might have heterogeneous impact on different states. To do so, we assume that the error term from our benchmark specification now follows a factor structure $\epsilon_{i,t} = \lambda_i' F_t + \upsilon_{i,t}$, where λ_i is a vector of state-specific loadings, F_t is a vector of aggregate factors in time t, and $\upsilon_{i,t}$ is now the error term. In particular, we now estimate the following regression specification:

$$x_{i,t} = \sum_{\tau=1}^{4} \beta_\tau \mathbf{1}_{t=2014Q\tau}(b_{i,t} - b_{i,2013Q4}) + \sum_{j=1}^{n} \gamma_j x_{i,t-j} + \nu_t \tilde{x}_{i,2013Q4} + \eta_i + \delta_t + \lambda_i' F_t + \upsilon_{i,t}, \tag{A22}$$

where we estimate the vectors of latent aggregate factors F_t and state-specific loadings λ_i following Bai (2009). The factor model allows to flexibly estimate latent aggregate shocks and their differential impact on different states. The estimates from that regression reported in Table A-5 are nearly identical to the benchmark specification.

Table A-6: Unemployment Benefit Extensions, Employment and Labor Force:
Findings from a Specification in Levels.

VARIABLES	(1) $\tilde{\beta}_1$	(2) $\tilde{\beta}_2$	(3) $\tilde{\beta}_3$	(4) $\tilde{\beta}_4$
EP	-0.00188***	-0.00486***	-0.00784***	-0.0101***
	(0.000280)	(0.000647)	(0.000988)	(0.00156)
LFP	-0.00152***	-0.00325***	-0.00517***	-0.00716***
	(0.000256)	(0.000502)	(0.000979)	(0.00163)

Robust standard errors clustered by state and time in parentheses
*** p<0.01, ** p<0.05, * p<0.1

III.6 Specification in Levels rather than Logs

In this section we perform our baseline analysis specifying EP and LFP in levels instead of logs. We continue to find robust negative effects of benefits on labor market outcomes across all four quarters (see Table A-6).

Note that now the estimates represent semi-elasticities, as opposed to elasticities. Thus, in order to compute the implied aggregate effects of the benefit cut we have to modify Eq. (**??**) in the main text:

$$\mu_s^{x,Lev} = \tilde{\beta}_4(b_s^{2014Q4} - b_s^{2013Q4}), \tag{A23}$$

where b_s^{2013Q4} and b_s^{2014Q4} denote the logarithm of the number of weeks of benefits available in state s in 2013Q4 (just prior to the policy change) and in 2014Q4, respectively, Denoting the population in state s by P_s, we obtain the increase in the aggregate level of employment or labor force, X, by 2014Q4 due to the policy reform as

$$\pi^{X,Lev} = \sum_{\text{All U.S. states } s} \mu_s^{x,Lev} P_s^{2014Q4}. \tag{A24}$$

The aggregate implications of these estimates are:

$$\pi^{E,Lev} = 2,610,365 \qquad \text{and} \qquad \pi^{LF,Lev} = 1,850,516. \tag{A25}$$

When we estimated our effects in logs in the main text, we found:

$$\pi^{E} = 2,542,625 \qquad \text{and} \qquad \pi^{LF} = 1,846,049. \tag{A26}$$

Thus, we would conclude that the effects of a cut in benefits on aggregates are statistically and economically indistinguishable regardless of whether they are estimated in levels or logs.

III.7 Analysis at a Level of Adjacent States

A prominent approach in the empirical analysis of the effects of policies is to compare states bordering each other (e.g. New Jersey and Pennsylvania) but having different policies. The idea is that many of the shocks, e.g., weather conditions, affect neighboring states similarly. So far, in our state-based panel analysis, we had to model the impact of such shocks. However, the border state design allows us to control for those common shocks by either differencing between bordering states or using a bordering state time dummy. We now consider such a specification:

$$x_{i,p,t} = \sum_{\tau=1}^{4} \beta_\tau \mathbf{1}_{t=2014Q\tau}(b_{i,t} - b_{i,2013Q4}) + \sum_{j=1}^{n} \gamma_j x_{i,p,t-j} + \nu_t \tilde{x}_{i,p,2013Q4} + \eta_{i,p,t} + \epsilon_{i,p,t}, \tag{A27}$$

where $\eta_{i,p,t}$ is the border-pair by time dummy.[8] The effects of the cut in benefit duration on labor force and employment estimated using this specification are reported in Table A-7. Their similarity to the baseline estimates reinforces the conclusion that the benchmark specification includes an adequate model of employment and labor force dynamics.

III.8 Statistical model validation

We perform a Monte Carlo study to verify the validity of the empirical design in our setting. The Monte Carlo study is designed as follows. We assume a data-generating process for EP and the effect of benefits as follows:

$$\tilde{e}_{i,t} = \sum_{\tau=1}^{4} \beta_\tau \mathbf{1}_{t=2014Q\tau}(b_{i,t} - \tilde{b}_{i,2013Q4}) + G(\{e_{i,\tau}\}_{\tau<t}, \{\tilde{\epsilon}_{i,\tau}\}_{\tau<t}, t, i) + \tilde{\epsilon}_{i,t} \tag{A28}$$

where the β_τ specifies the effect of benefits on EP in quarter τ of 2014. The function $G(\{e_{i,\tau}\}_{\tau<t}, \{\tilde{\epsilon}_{i,\tau}\}_{\tau<t}, t, i)$ allows for the current EP to depend

[8] Alaska and Hawaii are not adjacent to other states and are effectively excluded from this analysis.

Table A-7: Results Based on Border State Specification

VARIABLES	$\tilde{\beta}_1$	$\tilde{\beta}_2$	$\tilde{\beta}_3$	$\tilde{\beta}_4$
EP	-0.00497***	-0.0111***	-0.0154***	-0.0177***
	(0.000631)	(0.00154)	(0.00216)	(0.00261)
LFP	-0.00357***	-0.00720***	-0.00936***	-0.0111***
	(0.000694)	(0.00123)	(0.00143)	(0.00182)

Robust standard errors clustered by state, state pair, and time in parentheses
$N = 20,758$
*** p<0.01, ** p<0.05, * p<0.1

on lags of past values of EP as well as past values of the error term, $\tilde{\epsilon}$, and state- and time-specific components. Thus, G allows us to capture general AR(p), MA(q), and ARMA(p,q) type process for EP. We then perform several experiments to verify the validity of our empirical specification, even under misspecification (when the true DGP differs from our specification).

In the first validation experiment we take the baseline estimates as the true model and then add iid shocks to yield the data-generating process. This is equivalent to specifying $G(\{e_{i,\tau}\}_{\tau<t}, \{\tilde{e}_{i,\tau}\}_{\tau<t}, t, i) = \sum_{j=1}^{3} \hat{\gamma}_j \tilde{e}_{i,t-j} + \hat{\eta}_i + \hat{\delta}_t$. The statistical model is thus of the form:

$$\tilde{e}_{i,t} = \sum_{\tau=1}^{4} \beta_\tau \mathbf{1}_{t=2014Q\tau}(b_{i,t} - \tilde{b}_{i,2013Q4}) + \sum_{j=1}^{3} \hat{\gamma}_j \tilde{e}_{i,t-j} + \hat{\eta}_i + \hat{\delta}_t + \tilde{\epsilon}_{i,t} \tag{A29}$$

where the "hat" variables are the benchmark estimates, the tilde variables are the synthetic data and $\tilde{\epsilon}_{i,t}$ are iid mean zero normal errors with variance equal to the variance of the residuals from the benchmark specification. Note that we also generate synthetic weeks for 2013Q4 because we want to preserve the correlation structure between the treatment variable at the time of the expiration and the outcome variable, to verify that our empirical specification correctly controls for this potential threat to identification. As such we specify a threshold rule for the weeks of benefits that mimics the actual policy in place at the end of 2013[9].

$$\tilde{b}_{i,2013Q4} = \begin{cases} 2.81 \times baseweeks_i & \text{if } \tilde{x}_{i,2013Q4} < \bar{x} - 0.15 + \nu_i \\ 2.24 \times baseweeks_i & \text{if } \tilde{x}_{i,2013Q4} < \bar{x} - 0.10 + \nu_i \\ 2.08 \times baseweeks_i & \text{if } \tilde{x}_{i,2013Q4} < \bar{x} - 0.02 + \nu_i \\ 1.54 \times baseweeks_i & \text{otherwise} \end{cases} \tag{A30}$$

where $\bar{x}$ is the average employment/population over 2005-2007 period, $baseweeks_i$ is the base number of weeks the unemployed are state entitled to in state i, and ν_i are iid mean zero normal errors with variance of 0.01[10]. The thresholds are chosen to match the mean and variance of benefits and the fraction of states with extended benefits in 2013Q3. The variance of ν is chose to match and the correlation between change in employment/population between the 2005-2007 average and benefits in 2013Q4. The statistical model is initialized with the true data values for 1990 Q1-Q3, and then generated synthetically thereafter.

When we estimate the benchmark specification on the synthetic data, we find that the specification recovers the estimated coefficients and cumulated effects well. The average bias for the fourth quarter cumulant across simulations is less than 5% of the value of the coefficient.

Next, to test for whether we are dealing properly with possibly serial correlation in errors we perform a second Monte Carlo study. Now, instead of assumption that $\tilde{\epsilon}_{i,t}$ are iid across time, we assume that the errors have an AR(1) structure, $\tilde{\epsilon}_{i,t} = \rho_\epsilon \tilde{\epsilon}_{i,t-1} + \upsilon_{i,t}$, where $\upsilon_{i,t}$ are assumed to iid normal innovations. We experiment for various values of ρ_ϵ and adjust the variance of $\upsilon_{i,t}$ to keep the unconditional variance of the error equal across specifications (and equal to the unconditional variance of the residuals from the benchmark).

As in the benchmark estimation, we continue to add lags in the Monte Carlo study until we reject that the residuals are serially correlated. We find that this requires estimating additional lags relative to the true underlying DGP. The cumulative effects of treatment, however, are unaffected despite the different estimated lag structure. We find that the fourth quarter cumulant exhibits an average bias of 2-8% of the value of the true underlying

[9] We cannot impose the actual threshold rule for benefits since that depends on the unemployment rate

[10] Consistent with the data, we do not add an extension for North Carolina.

effect, depending on the underlying value for ρ_ϵ. Thus, we are confident that the benchmark specification is robust to serially correlated errors. Further, this suggests that even if $\sum_{j=1}^{n} \gamma_j x_{i,t-j}$ is correlated with the error term and is thus biased, that does not bias the coefficients of interest, the $\tilde{\beta}$s.

Next, to test whether misspecification is an issue, we consider two alternative specifications for $G(\{e_{i,\tau}\}_{\tau<t}, \{\tilde{\epsilon}_{i,\tau}\}_{\tau<t}, t, i)$. In the first, we assume that the true DGP is an ARMA(1,1) process, i.e. $G(\{e_{i,\tau}\}_{\tau<t}, \{\tilde{\epsilon}_{i,\tau}\}_{\tau<t}, t, i) = \gamma \tilde{e}_{i,t-1} + \theta_1 \tilde{\epsilon}_{i,t-1} \hat{\eta}_i + \hat{\delta}_t$. We set $\gamma = 0.9$ and $\theta = 0.9$ and then perform a Monte Carlo study. We again find that the bias in the estimated cumulants for EP are less than 5% of the true values. We have experimented with other values of γ and θ and found that our findings are robust.

In the second, we assume that the true DGP is an MA(q) process, i.e. $G(\{e_{i,\tau}\}_{\tau<t}, \{\tilde{\epsilon}_{i,\tau}\}_{\tau<t}, t, i) = \sum_{j=1}^{q} \hat{\theta}_j \tilde{\epsilon}_{i,t-j} \hat{\eta}_i + \hat{\delta}_t$ with q from 1 to 6. We again perform the Monte Carlo study (we have experimented with values for the θ_js varying between 0.1 and 0.9) and again find a bias in the estimated effects of benefits of less than 5% even though the empirical specification is misspecified (since we estimate an AR process).

III.9 Unemployment Benefit Extensions and QCEW Payroll Employment

The traditional approach, at least in the macroeconomics literature, to measuring the aggregate effects of policies on employment, defines the latter variable as including all individuals who did any work for pay or profit during a given week. For example, when measuring aggregate effects, the literature usually does not draw a distinction whether the increase in employment was due to more individuals becoming employees or starting their own businesses. The object of interest is the change in the total number of individuals supplying labor in the market in response to a policy change. This is the established definition of employment adopted by the Current Population Survey and it corresponds to the measure of employment used so far in this paper. The disadvantage of this measure of employment is that some components of employment have to be measured through surveys that are subject to sampling error.

A more narrow notion of employment can, however, be measured through administrative records. These data are called Quarterly Census of Employment and Wages (QCEW) and represent the count of jobs for which a paycheck subject to a UI tax was issued. Due to the nature of these data, this employment measure counts the number of jobs rather than the number of individuals with at least one job so that the same individual may be counted multiple times if he or she receives payments from multiple employers. Moreover, the data excludes most jobs not subject to the UI tax, such as self employed workers, unpaid family workers or employees of schools affiliated with religious organizations, railroad employees, etc. as well as jobs excluded for other reasons, such as employees of national security agencies.

It is well documented that these two measures of employment often diverge significantly even after accounting for the differences in coverage.[11] Of a particular concern to the period we study is the sharp rise in non-traditional employment, or what has become known as the rise of "1099 economy" (the IRS form 1099-MISC must be submitted by all "employers" who pay someone $600 or more a year in nonemployee compensation). Dourado and Koopman (2015) document a sharp rise in the number of these forms submitted to the IRS in recent years while Abraham et al. (2017) consider additional evidence. For example, an Uber driver would be paid this way. He or she will be classified as being employed according to the CPS definition but will not appear in the QCEW data. An additional complication presented by this rapid ongoing change in the labor market is that Uber drivers may not even classify themselves as being self-employed but to consider themselves as being employed by Uber when replying to the survey. This makes it challenging to interpret the data on self-employment and to use them in conjunction with QCEW data to obtain the picture of total employment changes.

Nevertheless, as QCEW data refer to a well defined segment of employment and are not affected by sampling error, it appears interesting to assess the effects of the cut in benefits on payroll counts as measured by the QCEW. Accordingly, we repeat the analysis above using these data. QCEW data contain non-seasonally adjusted monthly payroll employment counts. We seasonally adjust the monthly series using the X-13 ARIMA procedure. We then aggregate the monthly seasonally adjusted series to quarterly aggregates and divide by the previously constructed population measure.

Table A-8 contains the results. We find that these data also reveal a significant positive impact of the reduction in benefit duration on payroll employment. Specifically, the implied $\pi^E = 1,517,158$ so that the policy re-

[11]See Hagedorn and Manovskii (2011) for a discussion and additional references.

Table A-8: QCEW Results, Payroll Employment to Population Ratio

VARIABLES	$\tilde{\beta}_1$	$\tilde{\beta}_2$	$\tilde{\beta}_3$	$\tilde{\beta}_4$
States	-0.00337***	-0.00730***	-0.00951***	-0.0151***
	(0.00117)	(0.00224)	(0.00331)	(0.00453)
Border States	-0.00427***	-0.00760***	-0.00975***	-0.0138***
	(0.000680)	(0.00102)	(0.00150)	(0.00187)

Robust standard errors clustered by state and time in parentheses
*** p<0.01, ** p<0.05, * p<0.1

form accounted for over 49.2% of the growth in payroll employment in 2014. The magnitude of the effect of the policy reform on payroll employment is somewhat smaller than its impact on total employment, but the foregoing discussion illustrates the difficulties in interpreting this difference. For example, it might be that non-traditional employment was particularly sensitive to the cut in benefits. Alternatively, it might be that some holders of several part-time jobs secured full-time employment as a result of increased job availability following the EUC08 expiration which would be recorded as a decline in the number of jobs in the QCEW (while LAUS employment would not be affected).

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