The Impact of Unemployment Benefit Extensions on Employment: The 2014 Employment Miracle?

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We measure the aggregate effect of unemployment benefit duration on employment and the labor force. We exploit the variation induced by Congress' failure in December 2013 to reauthorize the unprecedented benefit extensions introduced during the Great Recession. Federal benefit extensions that ranged from 0 to 47 weeks across U.S. states were abruptly cut to zero. In sharp contrast to their typical dynamics, labor force and employment growth accelerated sharply in states with larger cuts in benefit duration. These findings are consistent with the equilibrium search framework that assigns an important role to endogenous job creation.
JEL: E24, E62, E65, J65
Keywords: Aggregate Employment, Labor Force, Macroeconomics Stabilization, Unemployment Insurance, Search and Matching

In this paper we measure the response of the labor force and employment to a large and unexpected cut in unemployment benefit duration in the US in December 2013 and find that the evidence is consistent with the basic equilibrium search framework that assigns an important role to endogenous job creation.

We measure the employment and labor force impacts of the December 2013 decision by U.S. Congress to terminate the Emergency Unemployment Compensation Act of 2008 which abruptly lowered benefit duration in all states to their regular duration (typically 26 weeks). This decision terminated an unprecedented extension of unemployment benefit durations adopted by policymakers following the onset of the Great Recession. While benefit durations began declining in some of the states starting in 2011, even by the end of 2013, right before the reform and long after the recession had ended, the average benefit duration across U.S.

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states stood at 53 weeks.

The decision to eliminate benefit extensions at the end of 2013 was quite controversial. Summarizing the conventional wisdom at the time, the Council of Economic Advisers and the Department of Labor (2013) predicted a substantial decline in labor force participation and a loss of 240,000 jobs in 2014. The logic underlying this assessment is familiar. A cut in benefit eligibility leads to a decline in the reservation wage and higher job acceptance by some of the unemployed, while others decide to drop out of the labor force because the value of active search declines relative to the value of non-participation. This unambiguously leads to a decline in the size of the labor force. The effect of the cut in benefits on employment is theoretically ambiguous as those who remain unemployed accept a wider range of jobs while those who drop out of the labor force find jobs at a lower rate. However, employment was predicted to fall because the reduction of transfers through UI benefits to individuals with high marginal propensities to consume implied a large negative impact on aggregate demand.[1]

However, this logic overlooks the potential response of the job-creation channel featured by the equilibrium labor search framework. In such a model, firms increase job creation when the profits from doing so increase. The profits are the difference between the value of a worker's marginal product and the wage. Thus, for a fixed worker productivity, the cut in benefit duration exerts a downward pressure on the wage and leads to an increase in job creation. A higher availability of jobs endogenously increases the job finding rate of active (unemployed) and passive (out-of-the-labor-force) searchers. If this effect is sufficiently strong, it can lead to a rise in employment and labor force participation after a cut in unemployment benefits.

What actually happened to the U.S. labor market in 2014? Employment and the number of job vacancies that employers were trying to fill increased sharply while labor force participation rate suddenly halted its steady secular decline. While suggestive of the importance of the job-creation channel discussed above, based on aggregate data alone, it is difficult to rule out the possibility that some other aggregate shocks (coincidental with the decline in benefit duration) suddenly spurred the decisions of firms to create job vacancies in 2014 and of jobless workers to accept them. To overcome this difficulty, we exploit the fact that, at the end of 2013, federal unemployment benefit extensions available to workers ranged from 0 to 47 weeks across U.S. states. As the decision to abruptly eliminate all federal extensions applied to all states, it was exogenous to economic conditions of individual states. In particular, states did not *choose* to cut benefits based on, e.g. their employment in 2013 or expected employment growth in 2014. This makes it relatively straightforward to exploit the vast heterogeneity in the size of the decline in benefit duration across states to identify the labor market implication of unemployment benefit extensions. However, the benefit durations prior to the

[1]This mechanism is discussed in Lentz (2009), Rendahl (2016), Ravn and Sterk (2017) and Den Haan et al. (2017), among others.

cut, and, consequently, the magnitudes of the cut, likely depended on economic conditions in individual states. Thus, the challenge to measure the effect of the cut in benefit durations on employment and the labor force is the inference on labor market dynamics that states would have experienced without a cut in benefits.

After describing the institutional features of the U.S. unemployment insurance system and the details of the policy change in December 2013, in Section I we document several patterns in the data that will guide our measurement. We first show that states that witnessed larger cuts in benefit duration experienced a significantly stronger acceleration of employment and labor force growth, suggesting that the reform stimulated their labor markets significantly. The abrupt reversal in the relative employment growth trend of high benefit states in December 2013, right at the time when the benefit durations were cut, indicates a sizable effect of the reform as there were no other policy changes at the turn of 2014 that could have differentially affected states depending on their pre-reform benefit duration and had significant labor market implications. We also show that high benefit states did not experience significant relative acceleration in labor force or employment growth in the years leading up to the reform. This implies that state-level employment and labor force follow highly persistent processes. This property of state-level employment was first identified by Blanchard and Katz (1992), who showed that it evolves according to a process statistically indistinguishable from a unit root. Although their 40-year sample period included large booms and recessions, it obviously did not include the Great Recession. Our findings suggest that in this respect it was not very different.[2]

Building on these findings, in Section II we conduct a formal measurement where we allow for a flexible dynamic model of state level employment that permits mean-reversion in the variables of interest (as the data suggest a highly persistent process but not necessarily a random walk). This is important to ensure an unbiased estimate of the effect of the reform that is not contaminated by the acceleration in employment and labor force growth that might have occurred in high benefit states even in the absence of the reform. This and other potential challenges to identification are formalized in Section II.A in which we also propose a methodology that can overcome those challenges.

Our empirical results are presented in Section II.C. An extensive analysis of their sensitivity to alternative modeling choices and data sources reported in Appendix IIISensitivity of Baseline Findingsappendix.C implies that they are quite robust. Specifically, we find that changes in unemployment benefit duration had a large and statistically significant effect on employment: a 1 percent drop in benefit duration led to an increase in employment 4 quarters later by approximately 0.02 log points.[3] Moreover, more than half of the increase in employment

[2] A simple linear regression of state-level employment to population ratio in 2013 Q4 on its value in 1996 Q4 yields a coefficient of 0.934 (s.e. 0.113) and similarly for labor force participation a coefficient of 0.998 (s.e. 0.118), illustrating the high-persistence of labor market variables at the state level.

[3] For example, a one-week cut in benefit duration in December 2013 (from 55 to 54 weeks) would have increased employment by 73,915 by 2014Q4.

attributed to the cut in benefits was due to an increase in the labor force.

To better illustrate the economic magnitude of this effect, we aggregate the state-level estimates to obtain the effect of the nation-wide change in benefit duration. The challenge is that to the extent that economic activity reallocates in response to differences in benefit durations across states, the effects of such a reallocation are reflected in our estimates. However, we document that there was no significant change in individuals' employment status in response to changes in benefits. In addition, we do not find any differential impact of benefit duration changes on employment shares of tradable and non-tradable sectors. In Appendix IIAggregation: A Simple Trade Model of the USappendix.B we use a simple trade model with frictional labor markets and show that these observations allow us to aggregate state-level elasticities to the nation-wide one. Empirically, we find that our estimates imply that the cut in benefit duration accounted for close to 75 percent of aggregate employment growth in 2014. The magnitude of this effect is nearly identical to that implied by the estimates in Johnston and Mas (2018) who study the impact of a smaller abrupt cut of benefit duration in Missouri in 2011. It is slightly larger than the one found by Hagedorn et al. (2013) who impose more structure to exploit multiple potentially anticipated changes in benefit duration over time and space. It is smaller than the one found in Mulligan (2012, 2015) who computes the employment effect of the policy reform based on his measure of the change in implicit marginal tax rates on work associated with the reform. An exhaustive review of the other related but less relevant literature, including Chodorow-Reich et al. (2019), is available in Johnston and Mas (2018) and Hagedorn et al. (2016).

To provide evidence of the mechanisms behind our main findings, we show that other aspects of the data are also consistent with the strong response of the job creation channel to a cut in benefit duration. Specifically, we apply the empirical methodology we developed to data on vacancies as well as wages of all workers and separately to wages of newly-hired workers. We find a large and persistent stimulative effect of the benefit cut on the number of posted vacancies with a 1 percent drop in benefit duration associated with an increase in vacancies by 0.06 log points. Moreover, a 1 percent drop in benefit duration led to a decline 4 quarters later by approximately 0.02 log points for total wages and by 0.05 log points for wages of newly-hired workers. Accounting for the well known composition bias induced by the spike in the number of new hires and the fact that newly hired workers tend to receive lower wages, we find that a 1 percent drop in benefit duration led to a decline in the composition bias-free level of wages 4 quarters later by approximately 0.01 log points. Thus, the key channel through which a cut in benefit triggers an increase in job creation in an equilibrium search model is also supported by the data.[4] Our findings on wages are consistent with

[4]Note that a standard textbook labor demand - labor supply diagram would suggest that higher labor demand would increase employment and labor force participation and lead to an increase in wages. In contrast, we see in the data that wages fall in response to the cut in benefits. It is the fall in wages,

recent evidence from Dahl and Knepper (2022), who study the response of salaries and posted wages across establishments located in different states but belonging to the same firms when some states cut the regular weeks of benefits available and others kept the typical 26 weeks.[5]

In Section III we develop an equilibrium labor search model, calibrate it, and use it to study quantitatively the effects of an unexpected cut in benefit duration. Individuals in the model can be in one of three states – employed, unemployed who are actively engaged in job search, and non-participants who engage in only passive job search. The model can match the gross worker flows observed in the data, including the large observed flows from non-participation into employment. Unemployed individuals are potentially eligible to claim unemployment insurance benefits while non-participants cannot. The probability of an active or a passive searcher to find a job is endogenous and depends on the optimal job creation decisions of employers. We show that in the model the aggregate effect of an unexpected cut in benefits modeled on the U.S. experience in 2013 generates an aggregate effect on employment and the labor force consistent with those that we found in the data. Moreover, we show that when we apply our benchmark empirical methodology to simulated panels of state data generated from the model we correctly recover regression estimates consistent with the implied aggregate effects in the model. Thus, our empirical findings are consistent with the quantitative implications of the equilibrium search framework that assigns an important role to endogenous job creation.

I. Data and the Unemployment Insurance Reform

A. *Policy Environment*

Prior to the onset of the Great Recession, unemployed workers in most states qualified for 26 weeks of unemployment compensation paid by the state in which the lost job was located.[6] In response to the deterioration of labor market conditions, the federal Emergency Unemployment Compensation (EUC08) program was enacted in June 2008. The program started by allowing for an extra 13 weeks of benefits to all states and was gradually expanded to have 4 tiers, providing potentially 53 weeks of federally financed additional benefits. The availability of each tier was dependent on state unemployment rates. The EUC08 program was not originally envisioned to last for many years, but was periodically reauthorized by Congress. The last annual reauthorization took place in December 2012.

holding worker productivity fixed, that induces firms to create jobs in an equilibrium search model. And it is the higher job availability that draws non-participants into the labor market despite the decline in wages they can expect to receive.

[5]Recent work by Jäger et al. (2020) finds little effect of benefits on outside options in Austria, in contrast to the findings of Nekoei and Weber (2017) who find positive effects of benefit extensions on wages, also in Austria. There are well known structural and policy differences that contribute to the diverging labor market experiences on the two sides of the Atlantic (Ljungqvist and Sargent, 2008).

[6]Note that benefit eligibility is based on the location of employment, not the residence of the worker.

In addition, the Extended Benefits (EB) program allows for 13 or 20 weeks of extra benefits in states with elevated unemployment rates. The EB program is a joint state and federal program. The federal government pays for half of the cost, and determines a set of "triggers," related to the state insured and total unemployment rates, that the states can adopt to qualify for extended benefits. At the onset of the recession, many states chose to opt out of the program or only adopt high triggers. The American Recovery and Reinvestment Act of 2009 turned this into a federally funded program. Following this, many states joined the program and several states adopted lower triggers to qualify for the program. Most states wrote their legislation implementing their EB program in a way that provided for their participation only as long as federal government paid for 100 percent of the cost. The provision for federal financing of the EB program was reauthorized together with reauthorizations of the EUC08 program.

An important feature of the EB program is that many triggers available to the states under the federal law contain look-back provisions. In particular, the state under those triggers qualified for federal financing only if state unemployment was 110 or 120 percent (depending on a trigger) higher than in the preceding two years. In other words, the EB program could be made available under those triggers only if unemployment is rising. Consequently, starting in 2011 some states began losing eligibility for the EB program.[7] As total duration of available unemployment benefits began declining so did the unemployment rate resulting in some states also losing eligibility for some of the tiers of the EUC08 program.

As a result, by December 2013 there was substantial heterogeneity in the actual unemployment benefit durations across U.S. states. As Table 1 shows, 3 states had 73 weeks of benefits available, 20 states had 61-63 weeks, 9 states had 54-57 weeks, 18 states had 40-49 weeks, and one state had 19 weeks. These data on unemployment benefit durations in each state is based on trigger reports provided by the Department of Labor. These reports contain detailed information for each of the states regarding the eligibility and activation status of the EB program and different tiers of the EUC08 program.[8]

In December 2013, Congress chose not to reauthorize the EUC08 program. As there was no "phase-out" period for EUC08 payments, all EUC08 payments ceased abruptly in all states when the program ended. Specifically, individuals who exhausted regular state unemployment compensation after December 21, 2013 (December 22, 2013 in NY) were no longer eligible for EUC08. For unemployed individuals already participating in the EUC08 program, the last payable week of EUC08 benefits was the week ending December 28, 2013 (December 29, 2013 in NY)[9]. From the moment the unemployment benefit extensions came to

[7] To mitigate this effect, the federal government temporarily gave states an option of using a three year look-back period.

[8] See http://ows.doleta.gov/unemploy/trigger/ for trigger reports on the EB program and http://ows.doleta.gov/unemploy/euc_trigger/ for reports on the EUC08 program.

[9] All states had triggered off the EB program by the end of 2012, so no states were offering extended benefits under this program in December 2013.

Table 1—: Benefit Duration across States in December 2013

Weeks of Benefits	States
73 weeks	Illinois, Nevada, Rhode Island
63 weeks	Alaska, Arizona, California, Connecticut, Delaware, DC, Indiana, Kentucky, Louisiana, Maryland, Massachusetts, Mississippi, New Jersey, New York, Ohio, Oregon, Pennsylvania, Tennessee, Washington
61 weeks	Arkansas
57 weeks	Michigan
54 weeks	Alabama, Colorado, Idaho, Maine, New Mexico, Texas, West Virginia, Wisconsin
49 weeks	Missouri, South Carolina
44 weeks	Georgia
40 weeks	Florida, Hawaii, Iowa, Kansas, Minnesota, Montana, Nebraska, New Hampshire, North Dakota, Oklahoma, South Dakota, Utah, Vermont, Virginia, Wyoming
19 weeks	North Carolina

an end in December 2013, newly unemployed individuals could only qualify for the regular state unemployment compensation for a duration of 26 weeks in most states.[10]

An important property of the decision not to renew benefit extensions in December 2013 is that it applied to all states, regardless of their economic conditions. In particular, the states could not choose whether to be treated by this reform, for example, based on their employment in 2013 or expected employment growth in 2014. The fact that the policy change was exogenous from the point of view of an individual state, allows for a relatively straightforward identification of its labor market impact. This contrasts sharply with the gradual decline in benefit durations in many states since 2011. While those declines could have had signifi-

[10] Some states had less than 26 weeks available in 2014, including Arkansas (25), Florida (16), Georgia (18), Kansas (20), Michigan (20), Missouri (20), North Carolina (19) and South Carolina (20). Two states – Massachusetts (30) and Montana (28) – offered more generous benefit durations.

cant labor market implications, those policy changes were endogenous to a state's labor market conditions, making the identification of the effects of policies more challenging.

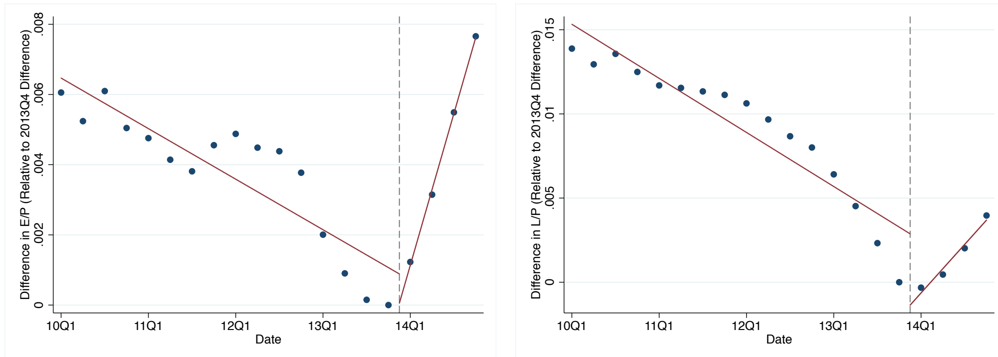
While from the outset, the federal unemployment benefit extension program was understood to be temporary, the decision to stop the program came largely as a surprise. Indeed, by December 2013 the program had been re-authorized a dozen of times. By that time it had paid benefits for a record 66 months, over two years longer than any prior discretionary benefit extension program. However, the U.S. unemployment rate was higher and the long-term unemployment rate was at least twice as high as it was at the expiration of every previous unemployment benefit extension program. Moreover, the Council of Economic Advisors, the Congressional Budget Office and others argued forcefully for the reauthorization on the grounds that EUC08 is among policies with "the largest effects on output and employment per dollar of budgetary cost." In light of this, few expected Congress to terminate the program in December 2013. Even following Congress' decision, there was likely some uncertainty regarding the finality of the program throughout the first half of 2014. For example, on April 7, 2014, the Senate approved a bipartisan bill that would have restored (retroactively to December 2013) federal funding for extended unemployment benefits. The bill faced opposition in the House of Representatives, which refused to hold a vote on it.

B. *A First Look at the Data*

Before proceeding with the formal measurement, we first present the patterns evident in the raw state-level data. This evidence not only highlights the quantitative significance of the changes in the patterns of employment and labor force growth at the time of the reform, but also helps to assuage any fears that subsequent formal results are driven by the choice of the specification, or are significantly influenced by outliers or a few states with benefit duration changes in a particular range.

The main variables of interest are the state-level ratios of employment or labor force to population, henceforth abbreviated EP and LFP, respectively. The only data source in the U.S. that contains long time series of both measures at the state-level at a reasonably high frequency is the Local Area Unemployment Statistics (LAUS) provided by the Bureau of Labor Statistics (Bureau of Labor Statistics, 1990–2014). Conveniently for our purposes, both variables are also consistently defined and represent the counts of individuals at each point in time. Our data is based on the 2015 redesign of LAUS methodology. The measures of employment and the labor-force are seasonally adjusted by the BLS. The data are reported monthly and aggregated to quarterly averages. Quarterly state-level population data are from the Regional Economic Accounts of the Bureau of Economic Analysis (U.S. Bureau of Economic Analysis, 2016).[11] We then construct

[11] The BEA only provides quarterly population estimates from 2010Q1 onwards. Later in



(a) Employment (b) Labor Force

Figure 1. : Difference in EP, Panel 1(a), and LFP, Panel 1(b), between high and low benefit states as defined just prior to the reform in December 2013. The differences are normalized to zero in 2013Q4.

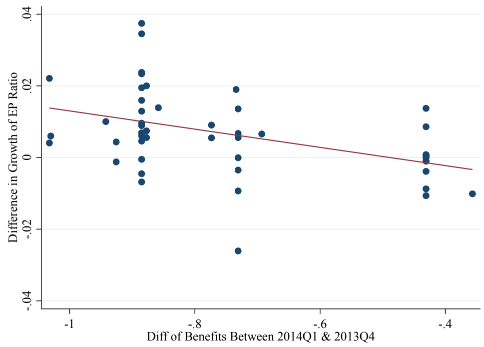
our employment to population ratio and labor force participation measures by dividing the quarterly values. Henceforth, when we refer to employment or labor force as arguments in the descriptive or formal analysis, we mean their ratio to population. Weekly state-level unemployment benefit duration data are also aggregated to a quarterly frequency.[12]

Our first look at the data is at a relatively aggregated level. We partition all states into high and low benefit duration groups depending on whether the cut in benefits experienced by the state in December 2013 was above or below the median. We then plot in Figure 1 the difference in employment and the labor force between the high and low benefit states for a number of quarters before and after the reform. We observe that employment and labor force were persistently declining in high benefit states relative to the low benefit ones prior to reform. The pattern changed abruptly and dramatically immediately following the reform, when previously high benefit states began catching up to their low benefit peers.

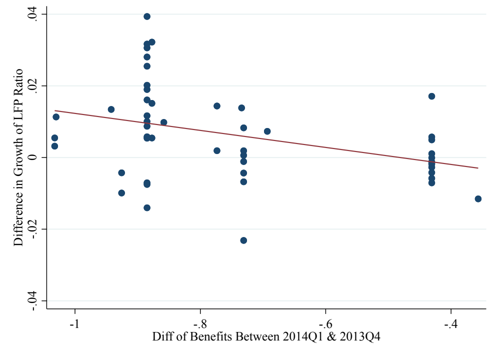
Additional results based on a more disaggregated analysis are reported in Appendix I.1UI Benefit Duration and State Labor Market Performancesubsection.A.1. There we report that U.S. states with a higher level of benefit duration right before the reform also had a worse labor market situation, i.e. lower EP and LFP ratios. Similar to the implication of Figure 1, using state-level data, we confirm that EP and LFP grew *slower* prior to the reform in states with larger cuts (and a higher pre-reform level) of benefit duration. This is important for the subsequent analysis because if employment started to accelerate before the reform in

the paper we will also use quarterly estimates of population for prior years obtained by linearly interpolating (between years) annual state-level population data from the Census Bureau, http://www.census.gov/popest/data/state/totals/2015/index.html, also accessed August 8, 2016.

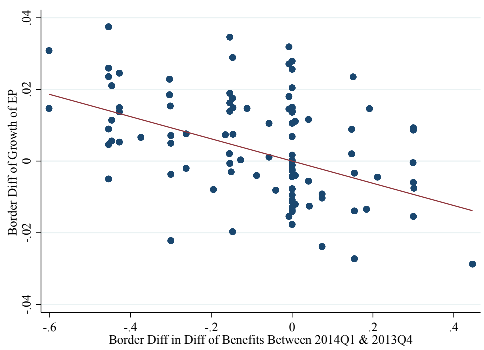
[12]Based on trigger reports described in Footnote 8.



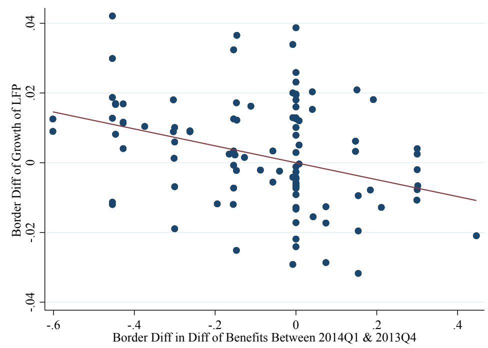
(a) Employment States



(b) Labor Force States



(c) Employment State Borders



(d) Labor Force State Borders

Figure 2. : Difference in growth rates of EP or LFP in 2014 and 2013 vs. the benefit duration cut due to the reform, i.e., the difference in benefit duration between 2014q1 and 2013q4; states and bordering state pairs.

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. Our results imply that this was not the case.

Prior to conducting the formal analysis, it is helpful to establish that there is clearly visible evidence in the data indicating a substantial effect of the reform. In Figure 2(a) we plot the difference in the growth rate of employment to population ratio in 2014 and in 2013 against the cut in benefit duration due to the reform in December 2013 for each state.[13] Similarly, in Figure 2(b) we plot the difference

[13]To focus on the effects of the nationwide cut in benefits in December 2013, we exclude North Carolina from the analysis in this section, since all benefit extensions were eliminated in NC in July 2013 as part of a comprehensive reform of all aspects of the state's UI system. That reform was not exogenous but decided by the state itself. Thus, the nationwide reform in Dec. 2013 did not affect North Carolina but, as our results below will show, caught it in the middle of a transition to higher employment and labor force participation.

in the growth rate of labor force participation in 2014 and in 2013 against the cut in benefit duration due to the reform in Dec. 2013 for each state.[14]

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: -0.0253 (s.e. 0.0074) for EP, and -0.0230 (s.e. 0.0085) 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 in the size of the benefit cut in December 2013 between adjacent states in those years.[15] As neighboring states are expected to have more similar employment 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 December 2013. The negative slope of the linear regression line through these points is slightly larger than in the state-level analysis: -0.0310 (s.e. 0.0056) for EP, and -0.0242 (s.e. 0.0062) for LFP.

What can account for these patterns in the data? One possibility is that the cuts in unemployment benefit duration induce an acceleration of employment and labor force growth. However, these patterns might also be consistent with an explanation based on "mean-reversion" in labor market variables. Such an explanation is based on an idea that shocks to state labor markets tend to revert to the mean. Thus, the lower is, say, a state's employment in some time period, the larger is the increase in employment in the next period. As state unemployment benefit extensions co-move negatively with state employment, this alternative theory predicts an acceleration of employment growth to depend on the level of benefit extensions (and thus the size of the cut), even if benefit durations themselves have no direct impact on employment. While Figure 2 is consistent with both theories, the one based on mean-reversion is largely discredited by the evidence in Figure 3 which summarises the results of a placebo analysis where

[14]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 $(b_{2014Q1} - b_{2013Q4})$.

[15]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 $(\Delta b_{ij,2014Q1} - \Delta b_{ij,2013Q4})$).

we counterfactually assume that the nationwide benefit cut occurred in a given quarter preceding the date of the actual reform. Specifically, Figure 3 summarizes the slopes of the regression lines of the scatter plots such as in Figure 2 constructed in every quarter between 2011Q1 and 2012Q4 by assuming (counterfactually) that benefit extensions were eliminated in that quarter. The figure shows that the size of benefit extensions (i.e., the magnitude of the placebo cut) does not predict the acceleration of employment growth in periods when there was no actual reform eliminating extended benefits. These results show that in the years prior to the benefit cut states with high benefits do not have a "tendency" to experience accelerated growth in employment over the subsequent year, therefore ruling out a significant effect of mean reversion. This evidence suggests that the patterns observed during the actual reform at the end of 2013 are exceptional.

A parallel analysis based on the difference-in-differences specification that replaces the December 2013 cut in benefits with the difference in the growth of benefits in 2014 and 2013 yields very similar results and is presented in Appendix I.2Difference-in-Differences Analysissubsection.A.2.

We now refine this analysis further by considering 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 are all insignificant (see Appendix I.3Do the Effects of a Benefit Cut Vary with Pre-Reform EP or LFP?subsection.A.3), indicating no significant heterogeneity in the effect of treatment.

Figure 4 illustrates that the sharp acceleration of employment and labor force growth in 2014 of high benefit duration states in 2013q4 occurred not only relative to the trend in 2013, but also relative to longer term pre-reform trends. Specifically, for each state i and $\tau = 0, ..., 11$ we define $\Delta_{i,\tau} = (x_{i,2014Q4} - x_{i,2013Q4}) - (x_{i,2013Q4} - x_{i,2012Q4-\tau})$, i.e. the deviation of the 2014 growth of the outcome variable x (EP or LFP) from its growth over $\tau + 4$ preceding quarters. We then regress $\Delta_{i,\tau}$ on the difference in log of benefit duration in state i between 2014q1 and 2013q4. The resulting coefficients are plotted in Panels 4(a) and 4(b) for employment and labor force, respectively. Panels 4(c) and 4(d) report similar coefficients estimated using the state-border specification.[16] The rightmost point in each panel, labeled 12Q4, and the horizontal red line through it refer to the

[16]Recall, $\Delta b_{ijt} = b_{it} - b_{jt}$ and $\Delta x_{ijt} = x_{it} - x_{jt}$ are 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. Panels 4(c) and 4(d) then plot $(\Delta x_{ij,2014Q4} - \Delta x_{ij,2013Q4}) - (\Delta x_{ij,2013Q4} - \Delta x_{ij,2012Q4-\tau})$ against $(\Delta b_{ij,2014Q1} - \Delta b_{ij,2013Q4}))$ for $\tau = 0, ..., 11$.

baseline estimate ($\tau = 0$) based on Figure 2. The horizontal dotted lines indicate the 95% confidence interval of the baseline estimate. As can be seen, in each of the panels, the estimated coefficients remain similar for all values of τ. This confirms that state-specific employment and labor force dynamics are highly persistent.

This finding extends the classic result of Blanchard and Katz (1992), who used forty years of data to document that state-level employment evolves according to a highly persistent process. While their study predated the experience of the Great Recession and subsequent recovery, we find that their conclusions continue to apply during the latter period. In the formal analysis below we will consider richer models of state-level EP and LFP dynamics and confirm that these processes are highly persistent, further ruling out the possibility of the patterns described in this section being driven by mean-reversion of these variables. Additional evidence of EP and LFP persistence at the state level is presented in Appendix I.4Persistence of Labor Force and Employment among U.S. Statessubsection.A.4.

II. Unemployment Benefit Extensions, Employment and Labor Force: Measurement.

In this section we develop and apply the empirical methodology that utilizes cross-state variation to infer the labor market implications of the nationwide cut in benefit duration in December 2013. We begin by describing the empirical specification, its identification, and the construction of the measure of the policy impact. Next, we describe the results of measuring the effects of the reform in the data, document their robustness, and discuss their implications.

A. Empirical Methodology

Our baseline empirical specification, discussed in detail below, is as follows:

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

where $x_{i,t}$ is the labor market outcome (i.e., log of the ratio of employment or labor force and population) in state i at time t, $b_{i,t}$ is the log of the number of weeks of benefits available in state i at time t, the indicator $\mathbf{1}_{t=2014Q\tau}$ equals one in quarter τ in 2014 and zero otherwise, n is the number of lags included, η_i is a state fixed effect, δ_t is an aggregate time effect, and $\nu_t \tilde{x}_{i,2013Q4}$ is a state-specific time trend where $\tilde{x}_{i,2013Q4}$ is the deviation of the outcome variable (EP or LFP) in each state in 2013Q4 from the cross-sectional mean in that quarter and ν_t is a time dummy.[17]

[17]Having access to rich panel data, it is natural to estimate the specification in levels. An alternative approach of estimating it in differences does not affect any of our conclusions but is inferior because

The key object of interest is the estimated cumulative effect of the expiration of the policy on the relevant labor market outcome, $\tilde{\beta}_\tau$ for $\tau = 1, 2, 3, 4$, which takes into account the estimated coefficients β_τ and the dynamic propagation via the estimated lag structure γ_j. For example, the effect in the first quarter is simply summarized by the dummy for the first quarter of 2014:

$$\tilde{\beta}_1 = \beta_1. \tag{2}$$

The cumulative effect in the second quarter is the dummy from the second quarter plus the dynamic effect via the lag from the first one:

$$\tilde{\beta}_2 = \beta_2 + \gamma_1 \tilde{\beta}_1. \tag{3}$$

More generally, we can define the cumulative effects recursively as

$$\tilde{\beta}_m = \beta_m + \sum_{j=1}^{\min\{n,m-1\}} \gamma_j \tilde{\beta}_{m-j}, \tag{4}$$

where, recall, n is the number of estimated lags in the specification. This sequence is the impulse response function, i.e., the response of future labor force or employment to a current change in policy. The effects of the reform are revealed by the magnitude and the statistical significance of the response at various lags. Standard errors of the cumulative effects $\tilde{\beta}_m$ are estimated using the delta method.

Identification. — The identifying assumption is the standard OLS zero conditional mean assumption:

$$E\big[\epsilon_{it} \mid \{\mathbf{1}_{t=2014Q\tau}(b_{i,t} - b_{i,2013Q4})\}_{\tau=1}^{4}, \{x_{i,t-j}\}_{j=1}^{n}, \nu_t \tilde{x}_{i,2013Q4}, \eta_i, \delta_t\big] = 0. \tag{5}$$

To understand what this assumption does and which potential endogeneity problems are addressed, it is instructive to consider a simpler specification,

$$x_{i,t} = \sum_{\tau=1}^{4} \beta_\tau \mathbf{1}_{t=2014Q\tau}(b_{i,t} - b_{i,2013Q4}) + \xi_{i,t}, \tag{6}$$

where this assumption is less likely to be satisfied and which can therefore serve to illustrate the relevant identification issues.

differencing the data exacerbates problems induced by measurement error in quarterly observations and, as explained in Bertrand et al. (2004), this procedure "... is valid only under the very restrictive assumption that changes in the outcome variable over time would have been exactly the same in both treatment and control groups in the absence of the intervention."

For this specification to deliver unbiased results, it is required that the shocks ξ to x are uncorrelated with $b_{i,t} - b_{i,2013Q4}$. This simple specification differs from the benchmark in Eq. (1) in that it does not control for lagged values of x through including $\sum_{j=1}^{n} \gamma_j x_{i,t-j}$, does not control for differences across states through including η_i, does not control for time effects through δ_t and does not include a state-specific time trend $\nu_t \tilde{x}_{i,2013Q4}$. Since these controls are not included, they are captured by ξ. This does not induce a correlation between ξ and $b_{i,t}$ (benefit duration in every quarter of 2014) because the benefit cut affected all states independently of their past employment or unemployment levels or more broadly independently of the economic performance of the state. For the pre-reform duration of benefits $b_{i,2013Q4}$, however, this is not the case and omitting one of the control variables may lead to a bias.

Including the time dummies captures the U.S.-wide evolution of the labor market. A bias may arise if e.g., the US labor market is on a recovery path with employment increasing in all states. When time dummies are not included, this trend in employment would be picked up by the coefficients β_τ, delivering an upward biased estimate of the effects of benefits.

State fixed effects control for permanent differences in employment across states which might be correlated with available benefits. Including them in the specification prevents this correlation from being erroneously attributed to a causal effect of benefit duration.

Another bias arises in the simple specification (6) from the mechanical way benefits are set. In contrast to benefits in 2014, the pre-reform benefit duration in $2013Q4$ depends on the past employment in the state.[18] If employment crosses a certain threshold (from above) then benefits are automatically increased with a short lag, so that

$$b_{i,2013Q4} = G(\{x_{i,2013Q4-j}\}_{j=1}^{k}). \tag{7}$$

If the economy is hit by an adverse shock in the past, employment decreases and then evolves according to the process $x_{i,t} = \sum_{j=1}^{n} \gamma_j x_{i,t-j}$. If employment is mean-reverting, then after a large adverse shock during the Great Recession employment starts recovering so that subsequent employment gains are the results of this recovery process. As this recovery may continue through 2014, employment gains in 2014 might be a result of mean-reversion as well. A bias arises since the initial shock to employment also leads to a rise in benefits which stay at that high level for some time, so that the benefit level is still elevated in 2013Q4. Implementing the simple regression would then suggest a negative effect of benefits on employment even if there were no true causal effect but just because high benefit states are

[18]More precisely, it depends on past unemployment but we will use past employment here as the determinant of benefits to save on switching back and forth between the two.

mean-reverting in 2014. In other words, the identifying assumption of the simple model fails:

$$E[b_{i,2013Q4}\ \xi_{it}] = E[G(\{x_{i,2013Q4-j}\}_{j=1}^{k})\ \xi_{it}] \neq 0. \tag{8}$$

This non-zero correlation is a result of not including past employment levels in the regression as this adds them to the error term ξ_{it}, resulting in a standard endogeneity problem due to omitted variables as past employment levels move both current benefits according to Eq. (7) and the current shock ξ_{it}. Including $\sum_{j=1}^{n} \gamma_j x_{i,t-j}$ into the specification, as in (1), controls for these dynamic adjustments and thus overcomes this bias, as past shocks do not predict current employment or the current shock ξ_{it} conditional on this lag. Benefits in 2013Q4 can still be elevated because of a past negative shock but this does not create a bias since this past shock is not correlated with current employment conditional on the included lags. In the implementation we follow the standard practice and include as many lags such that the shocks ξ_{it} are *i.i.d.*[19]

Finally, the traditional specification in the literature that exploits cross-state variation in economic policies to infer their impact on employment commonly includes state-specific time trends as controls. They are included to control for heterogeneity in the evolution of labor markets within states that might be correlated with the treatment intensity. In our baseline specification, state-specific trends are captured by $\nu_t \tilde{x}_{i,2013Q4}$. The advantage of this flexible specification relative to several alternatives that we will consider below is that (1) it depends on the pre-reform level of the outcome variable only and (2) it directly addresses the concern that the time of the policy reform coincided with the unusual turning point in employment dynamics (as documented above, such turning was not observed in other time periods), whereby employment and labor force growth accelerated more in states experiencing particularly severe lingering effects of the Great Recession by the end of 2013 (and this acceleration would have occurred even in the absence of the reform). As we report below, however, the estimated effects of the policy change are robust to alternative specifications of state-specific trends.

B. *Aggregation of State-Level Employment Effects*

Our baseline estimates reflect the effect of unemployment benefit extensions on the labor force or on employment at the state-level. While of interest on their own, it is also desirable to be able to use the resulting coefficients to predict the effect of a nation-wide extension. A potential concern is that when some states

[19]Note that the coefficients on all controls, including γ_j, are identified from pre-reform data while the effect of the cut in benefits is identified from post-reform data 2013Q4-2014Q4 (conditional on all controls).

cut benefits more than others, economic activity may reallocate to states with, say, the larger benefit cut. This reallocation is picked up by our estimates but will be absent when benefit duration is cut uniformly everywhere. Our results in Section II.D below alleviate such concerns. First, we find large negative effects of unemployment benefit extensions on employment in sectors commonly considered non-tradable and thus not subject to reallocation. Second, we find that, in response to changes in benefits, even unemployed workers living close to state borders do not change the strategy of which state to look for work in. Building on these insights, we show in Appendix IIAggregation: A Simple Trade Model of the USappendix.B that we can use the estimates obtained at the state level to compute the change in U.S. employment due to the cut in benefits in a model where each state is an open economy in the (closed) U.S. economy and 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. The two sectors, producing the tradable and the nontradable good, operate in the same labor market and are subject to the same labor market frictions. We then show that our elasticity for the employment response at the state level can be used at the aggregate level as well.

Specifically, due to the absence of reallocation and mobility caused by a change in benefits, we can estimate Equation (1) using log EP or LFP as x, recover the cumulative effect of interest, e.g. $\tilde{\beta}_4$, and use it to compute the implied increase in aggregate U.S. labor force or employment that is caused by the cancellation of extended benefits. In particular, in a given state s, the drop in benefit duration led to an increase in the ratio of employment or labor force to population by the end of 2014 of

$$\mu_s^x = \tilde{\beta}_4(b_s^{2014Q4} - b_s^{2013Q4})exp(x_s^{2013Q4}). \tag{9}$$

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, and x_s^{2013Q4} is the logarithm of EP or LFP in state s in $2013Q4$. 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

$$\pi^X = \sum_{\text{All U.S. states } s} \mu_s^x P_s^{2014Q4}. \tag{10}$$

C. *Main Empirical Findings.*

Table 2 contains the results of the estimation of the effect of unemployment benefit duration on employment using the specification in Eq. (1) for the period 1990–2014, which is sufficiently long to estimate the coefficients of the dynamic

Table 2—: Unemployment Benefit Extensions, Employment and Labor Force: Findings.

VARIABLES	$\tilde{\beta}_1$	$\tilde{\beta}_2$	$\tilde{\beta}_3$	$\tilde{\beta}_4$
EP	-0.00414***	-0.0106***	-0.0168***	-0.0214***
	(0.000567)	(0.00135)	(0.00208)	(0.00318)
LFP	-0.00314***	-0.00675***	-0.0106***	-0.0145***
	(0.000479)	(0.00102)	(0.00193)	(0.00312)

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

EP Reg.: Adj $R^2 = 0.9989$, $N = 4,947$, the estimated coefficients on the lags: $\hat{\gamma}_1 = 1.85$, $\hat{\gamma}_2 = -1.11$, $\hat{\gamma}_3 = 0.24$

LFP Reg.: Adj $R^2 = 0.9983$, $N = 4,947$, the estimated coefficients on the lags: $\hat{\gamma}_1 = 1.78$, $\hat{\gamma}_2 = -1.04$, $\hat{\gamma}_3 = 0.23$

model without bias.[20] The specification includes three lags of the dependent variable as this is the smallest number of lags needed to ensure that residuals are serially uncorrelated. We report in Appendix III.2Number of Lagssubsection.C.2 that the results are not sensitive to using other criteria or to including many more lags. We find that changes in unemployment benefits have a large and statistically significant effect on the employment-population ratio: a 1 percent drop in benefit duration increases the employment-population by 0.0214 log points after 4 quarters.[21] We also use Eq. (1) with labor force participation on the left hand side to estimate the percentage change in labor force participation attributable to the cancellation of policy. We find that a 1 percent drop in benefit duration increases the labor force participation rate by 0.0145 log points.

These results of evaluating the actual reform in December 2013 stand in sharp contrast to those obtained when using the same empirical specification to assess the impact of placebo reforms in prior time periods. The associated one through four quarters ahead cumulative effects, $\tilde{\beta}_\tau$, on the employment-population ratio and labor force participation are reported in the four panels of Figures 5 and 6. Except for the rightmost point on each panel which corresponds to the actual reform in 2013q4, the estimated effects of the placebo reforms are generally quite small and statistically insignificant.

The internal validity of our baseline empirical specification depends on whether

[20]Econometrically, our setting is best described as a "large T, large N" one. To verify this, in the Appendix we report results of a Monte Carlo study in which we simulate samples with the dimension of the data used in estimation from specification (1) and making sure to preserve the correlation structure between the treatment variable at the time of the reform and the outcome variable. Estimating the benchmark specification on these synthetic data recovers the estimated coefficients, including the ones on lags and fixed effects, and cumulated effects well. In particular, the bias for the fourth quarter cumulant across simulations is less than 5% of the value of the coefficient.

[21]As recommended by Bertrand et al. (2004) and Cameron and Miller (2015), standard errors are clustered by state and time to account for any remaining serial correlation within states and potential correlation across states within a quarter. A one-way clustering by state only has no impact on the significance of any of our estimates.

the parsimonious model of EP and LFP dynamics it uses is sufficient to account for state-level pre-reform trends in these variables. In Appendix Figure A-1Panels 1(a)Subfigure 1(a)subfigure.1.1 and 1(b)Subfigure 1(b)subfigure.1.2: Level of EP or LFP in 2013Q4 and the cut in benefit duration induced by the reform; Panels 1(c)Subfigure 1(c)subfigure.1.3 and 1(d)Subfigure 1(d)subfigure.1.4: 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 II.C where this relationship is not present.figure.caption.1 we show that the size of the benefit cut is related neither to the level in 2013q4 nor to the growth between 2012q4 and 2013q4 of the residuals $\epsilon_{i,t}$ recovered from estimated baseline specification in Eq. (1). Building on this analysis, we now regress the residuals for each state over the 2011Q1-2013Q4 period on a constant and a linear time trend. Each dot in Figure 7 represents the estimated coefficient on the time trend for each state (on the vertical axis) plotted against the future drop in benefits between 2013q4 and 2014q1 induced by the policy reform. We observe no systematic relationship between residual pre-trends and the cut in benefits induced by the reform. To quantify this relationship we also plot a regression line obtained by regressing the residual slope coefficients for each state on the future drop in benefits. We obtain a slope coefficient of 6.4×10^{-6} (s.e. 0.00010) for EP and of 0.00012 (s.e. 0.00013) for LFP. These coefficients are statistically insignificant at conventional levels. The fact that they are positive implies some residual divergence, i.e. a relative deterioration in labor market variables of the states with larger cuts in benefits. This implies that our baseline estimates understate the positive effects of the reform on EP and LFP. Quantitatively, the effect is minuscule, however. To put its magnitude into perspective, we also plot the estimated coefficient $\pm\tilde{\beta}_1$ on the same figure, which highlights that the potential understatement of the effect of the reform is negligible.

D. *Implications for Aggregate Employment and Labor Force*

In the absence of reallocation and mobility caused by a change in benefits, Section II.B shows how our estimates $\tilde{\beta}_4^x$, where x denotes EP or LFP, can be used to compute the implied increase in aggregate U.S. labor force or employment caused by the cancellation of extended benefits. In particular, in a given state i, the drop in benefit duration led to an increase in the ratio of employment or labor force to population by the end of 2014 of

$$\mu_i^x = \tilde{\beta}_4^x (b_i^{2014Q4} - b_i^{2013Q4}) x_i^{2013Q4}, \tag{11}$$

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 $2014Q4$, respectively, and x_i^{2013Q4} is the EP or LFP in state i in $2013Q4$. Denoting the population in state i by P_i, we obtain the increase in the aggregate level of employment or labor force, X, by $2014Q4$ due to the reform:

$$\pi^X = \sum_{\text{All U.S. states } i} \mu_i^x P_i^{2014Q4}. \tag{12}$$

Using our estimates of $\tilde{\beta}_4$ for EP and LFP, this calculation implies that aggregate employment and labor force have increased by the end of 2014 due to the policy reform by

$$\pi^E = 2,542,625 \qquad \text{and} \qquad \pi^{LF} = 1,846,049. \tag{13}$$

Thus, more than half of the increase in employment was due to the increase in the labor force as a result of the reduction of benefit duration. The remaining increase corresponds to a decrease in the number of unemployed.[22]

As highlighted in Section II.B, the degree to which one can rely exclusively on state-level estimates of the effects of unemployment benefit extensions to predict the effects of a nation-wide extensions depends on whether state benefit extensions induce a spatial reallocation of economic activity. We perform two additional exercises to show that there is little evidence for reallocation of economic activity across space induced by the benefit cut. First, if the state-level change in employment was driven to an important degree by reallocation, we would expect that benefit extensions have a larger effect on the tradable sector, which can reallocate, than on the non-tradable, which can reallocate to a much lesser degree. Second, another potential reallocation effect arises because households may live in different states than where they work. In Online Appendix III.1Evidence on Reallocation and Mobilitysubsection.C.1 we show that the benefit cut has no significant effect on the ratio of tradable to non-tradable employment (ruling out the first concern), and nor on job-search behavior across states (ruling out the second concern) using commuting data from the American Community Survey (U.S. Census Bureau, 2016).

E. Robustness of Results

The results of the sensitivity analysis of our findings to changes in the specification and in data sources are reported in Appendix IIISensitivity of Baseline Findingsappendix.C. More specifically,

1) We find that the results are virtually not affected by the choice of the criterion used to select the number of lags included in the specification, or by including many more lags than is suggested by the standard criteria.

[22]This is consistent with the findings for unemployment in e.g., Hartung et al. (2018) for Germany and Fredriksson and Söderström (2020) for Sweden.

2) We search for evidence of heterogeneity in the impact of the cut in benefits across states of different sizes by comparing estimation results that are and are not weighted by state population. The estimates are little affected by the choice of weighting, suggesting that any heterogeneity in the effect of benefits is minimal in the data.
3) We consider alternative specifications for state-specific trends, from basic linear trends to flexible trends that depend on states' economic conditions before the onset of the Great Recession instead or in addition to the benchmark specification where they depend on the conditions right before the reform. None of our conclusions are affected by the specific choice of the model for state-specific trends.
4) We evaluate the consequences of controlling for shocks that jointly affect states that border each other through the inclusion of a border state dummy in the benchmark specification. This does not affect the results, reinforcing the conclusion that the benchmark specification includes an adequate model of employment and labor force dynamics.
5) We control for correlated shocks across states. Specifically, we assume a latent factor model structure for the error term, which flexibly allows for aggregate shocks that have differential impacts across states, and employ the interactive effects estimator in Bai (2009). Our baseline estimate remain essentially unchanged.
6) The aggregate implications of the benefit cut are statistically and economically indistinguishable from the baseline when specify EP and LFP in levels rather than logs.
7) The measure of employment used in our analysis represents the count of all individuals who did any paid market work, regardless of how many jobs those workers might have held. As a complementary analysis, we also consider the effect of the reform on the counts of jobs for which a paycheck subject to a UI tax was issued using the administrative QCEW data (U.S. Bureau of Labor Statistics, 2019). Similar to our results based on the traditional measure of employment, we find that the reform led to a large increase in payroll counts.

F. Unemployment Benefit Extensions, Vacancies, and Wages.

The evidence presented so far reveals that the cut in UI benefit duration led to a substantial increase in employment and the labor force. Moreover, the dominant impact of the benefit cut on employment was not driven by a contraction in the labor force – unemployed dropping out of the labor force because they were no longer entitled to benefits – but instead by those previously not participating in the labor market entering the labor force. As we discussed in the Introduction and will show quantitatively in the next section, the equilibrium labor search framework rationalizes these patterns in the data through a powerful job creation

Table 3—: Unemployment Benefit Extensions and Vacancies

	$\tilde{\beta}_1$	$\tilde{\beta}_2$	$\tilde{\beta}_3$	$\tilde{\beta}_4$
Vacancies	-0.0585***	-0.0464***	-0.0652***	-0.0560***
	(0.0151)	(0.0133)	(0.0136)	(0.0173)

Robust standard errors clustered by state and time in parentheses
*** p<0.01, ** p<0.05, * p<0.1
Adj $R^2 = 0.9258$, $N = 2,754$, the estimated coefficients on the lags: $\hat{\gamma}_1 = 0.74$, $\hat{\gamma}_2 = -0.02$

channel. A cut in benefit duration leads to a decline in wages relative to workers' productivity, which stimulates job creation (measured trough the job vacancy postings). A higher availability of jobs makes it easier for unemployed workers to find employment and draws non-participants back into the labor force. Anticipating this discussion, we now present evidence of the response of vacancies and wages to the cut in benefits in December 2013.

Vacancies. — We obtain data on vacancies from the Job Openings and Labor Turnover Survey (JOLTS) collected by the BLS. The JOLTS data (U.S. Bureau of Labor Statistics, 2000–2014) are available for all 50 states and the District of Columbia beginning in the December 2000. We apply the empirical methodology we developed (Eq. 1) to seasonally-adjusted vacancy rates. The results reported in Table 3 indicate a large, sharp, persistent, and statistically significant increase in vacancies following the cut in benefits. A 1 percent drop in benefit duration increases vacancies by 0.0585 log points on impact and the strength of the effect persists for the following 4 quarters.

Wages. — In an equilibrium search model a cut in unemployment benefits stimulates job creation if wages that firms expect to pay decline relative to workers' productivity. Of course, creating jobs is a long-lasting investment decision based on the full expected value of wage payments relative to workers' output over the life of the match (Mortensen and Pissarides, 1994; Hagedorn and Manovskii, 2008; Mitman and Rabinovich, 2015; Kudlyak, 2014; Basu and House, 2016). On the worker side it is also the total expected value of wage payments that is relevant for the decisions to search for and to accept a job and not necessarily the exact timing of individual payments implementing that expected value. Thus, a complete assessment of this channel would involve measuring the change in the expected present value of wages that firms expect to pay and workers expect to obtain following the reform, which is infeasible in our data. However, many versions of the model specify the wage determination protocol (e.g., continuous re-negotiation) where not only the present value of wages declines following a cut in benefits but the period-by-period value of wages declines as well (Hagedorn and Manovskii,

Table 4—: Benchmark Results: Wages

VARIABLES	$\tilde{\beta}_1$	$\tilde{\beta}_2$	$\tilde{\beta}_3$	$\tilde{\beta}_4$
New Hires' Wages	0.0379***	0.0342***	0.0317***	0.0517***
	(0.00747)	(0.00893)	(0.0109)	(0.0108)
Total Wages	0.0107***	0.0119***	0.0146***	0.0210***
	(0.00261)	(0.00225)	(0.00238)	(0.00377)

Robust standard errors clustered by state and time in parentheses
*** p<0.01, ** p<0.05, * p<0.1
New Hires Reg.: Adj $R^2 = 0.9852$, $N = 3,186$, the estimated coefficients on the lags: $\hat{\gamma}_1 = 0.48$, $\hat{\gamma}_2 = 0.25$, $\hat{\gamma}_3 = 0.19$
Total Reg.: Adj $R^2 = 0.9983$, $N = 3,441$, the estimated coefficients on the lags: $\hat{\gamma}_1 = 0.54$, $\hat{\gamma}_2 = 0.27$, $\hat{\gamma}_3 = 0.15$

2008), potentially with some rigidities (Gertler and Trigari, 2009). Thus, it is informative and fortunately feasible to assess the impact of the cut in benefit duration on wages that can be measured in the data in each quarter following the reform.

We apply the empirical methodology we developed (Eq. 1) to wages of all workers and separately to wages of newly-hired workers that can be measured for each state at quarterly frequency in administrative QWI data (U.S. Census Bureau, 2020). The results are reported in Table 4. We find that changes in unemployment benefit duration had a large and statistically significant effect on wages: a 1 percent drop in benefit duration led to a decline 4 quarters later by approximately 0.02 log points for total wages and by 0.05 log points for wages of the newly-hired workers. Note that this is the equilibrium wage which combines two partially off-setting effects: a decline in the wage induced by the lower workers' outside option and an increase in the wage due to the higher probability of finding a job.

It is well known that wages of new hires are on average lower and more responsive to aggregate labor market conditions than wages of incumbent workers. This is due in part to lower idiosyncratic productivity of many new hires. Thus, although the decline in wages for newly hired workers following the cut in benefit eligibility is consistent with the mechanism of the equilibrium labor search model, it does not provide a fully satisfying test as the productivity distribution of these workers is not known and their idiosyncratic productivity cannot be controlled for without access to detailed panel data at the individual worker level.

A more stringent test of the model is based on the response of the incumbent workers' wages, which reveals the true wage response as it is conditional on workers' productivity and thus controls for the idiosyncratic productivity. To measure this response we can combine the data on total wages and wages of newly hired workers. Specifically, denote the wage of incumbent workers by W^{True}, the wage of newly hired workers by W^{NH}, and the employment share of newly hired workers by S^E.[23] The overall aggregate wage equals

$$W^{Agg} = W^{NH} S^E + W^{True}(1 - S^E). \tag{14}$$

Then, the response of aggregate wages W^{Agg} to the cut in benefit duration $\log b$ can be decomposed as

$$\frac{d\,W^{Agg}}{d\log b} = \frac{d\,W^{NH}}{d\log b} S^E + \frac{d\,W^{True}}{d\log b}(1 - S^E) + (W^{NH} - W^{True})\frac{d\,S^E}{d\log b}. \tag{15}$$

In logarithms,

$$\begin{aligned} \underbrace{\frac{d\log W^{Agg}}{d\log b}}_{0.0209} &= \frac{1}{W^{Agg}}\frac{d\,W^{Agg}}{d\log b} \\ &= \underbrace{(1 - S^W)}_{0.89}\frac{d\log W^{True}}{d\log b} + \underbrace{S^W}_{0.11}\underbrace{\frac{d\log W^{NH}}{d\log b}}_{0.052} + \underbrace{\frac{W^{NH} - W^{True}}{W^{Agg}}}_{-0.284}\underbrace{\frac{d\,S^E}{d\log b}}_{-0.0184}, \end{aligned} \tag{16}$$

where $S^W = S^E W^{NH}/W^{Agg}$ is the wage share of newly hired workers. Measuring all the relevant objects in the QWI data and solving for the incumbent worker's wage response to the cut in benefits we obtain

$$\frac{d\log W^{True}}{d\log b} = 0.011.$$

The fact that the wages of incumbent workers decline following the reform provides a strong testament that the mechanism that gives rise to a strong job creation channel in the model is consistent with the data. This result also makes the increase in labor force participation following the reform all the more striking. The non-participants were not directly affected by the reform as they collected benefits neither before nor after the reform. Yet, they entered the labor market despite the decline in wages they could expect to obtain. A large increase in the probability of finding a job provides a coherent rationalization of this behavior.

[23]Aggregate wages also include the wages of future separators. This decomposition is justified as the wages of incumbents and separators have the same properties. In the data we find that the share of wages of future separators in total wages does not respond to changes in unemployment benefits. For example, $\tilde{\beta}_4$ in the regression of the ratio of wages of separators to non-separations is 0.0023 (s.e. 0.0181), supporting a decomposition that relies only on aggregate wages and those of new hires.

III. Model

In this section we describe a dynamic quantitative model of the labor market, extending the standard Mortensen-Pissarides search-and-matching model. We extend the standard model, where agents can be either employed or unemployed, by adding a labor-force participation margin. The goal of this section is to show that a calibrated model can replicate our empirical findings on the effect of benefits on employment and labor-force participation. In particular, the model shows, maybe surprisingly, that labor force participation increases following a cut of unemployment insurance benefits duration. The reason is that a cut in benefits stimulates job creation which leads to, consistent with the data, an increased flow of workers from out-of-the-labor-force to employment. In what follows, we describe the model environment, how we calibrate the model, and the nature of the quantitative exercise.

A. Preferences and Technology

The state economy is populated by a unit measure of agents and a larger continuum of firms. Time is discrete and agents are infinitely lived, but face a constant probability $1-\mu$ of leaving the economy. The mass of agents that exit is replaced with an identical mass of newborn agents. Thus, the model is one of "perpetual youth" with a constant population. Agents are risk neutral and maximize their expected discounted lifetime utility:

$$\mathcal{U} = \mathbb{E}_\tau \sum_{t=\tau}^{\infty} (\beta\mu)^{t-\tau} c_t,$$

where τ is the period of birth, $\beta \in (0,1)$ is the subjective discount factor, and c_t is the consumption of the agent in period t. Agents can be employed by a firm or non-employed. Non-employed workers produce h, capturing both home production and the leisure value of being non-employed. Non-employed workers can either participate in active labor market search, in which case we denote them as unemployed, or be out-of-the-labor-force and participate in passive search. We abstract from an intensive margin search effort choice and normalize the effective search of the unemployed to 1 and of non-participants to λ. The aggregate search effort in the economy is given by $S_t = u_t + \lambda o_t$, where u_t is the measure of unemployed and o_t the measure of those out of the labor force. Non-employed agents face stochastic costs to active search. We assume that agents currently in the labor force face a distribution of search costs given by Γ^u and agents out of the labor face a distribution of search costs Γ^o. To smooth out the discrete choices, we assume that they are perturbed by type-I extreme value shocks.

Firms are also risk neutral, maximize profits, and discount the future with the same discount factor as workers β. A firm can either be idle, vacant or matched with a worker. Firms can choose to post vacancies v_t at flow cost k. All workers and firms are ex-ante identical. Matched worker-firm pairs produce identical output z_t in period t, where z_t is the state-specific productivity that follows as AR(1) process: $\log z_t = \rho_z \log z_{t-1} + \sigma_z \zeta_t$, where ζ_t are iid standard normal random variables. Firm and worker matches dissolve exogenously at a rate δ.

Non-employed workers and vacancies match in pairs, subject to a constant returns to scale matching function. The number of new matches in period t is given by $M(S_t, v_t)$. Define $\theta_t = v_t/S_t$ as labor market tightness in period t. The job-finding probability per unit of search intensity in period t is given by $f(\theta_t) = M(1, \theta_t)$. Thus, the unemployed find jobs at rate $f(\theta)$ and the nonparticipants at rate $\lambda f(\theta)$. The vacancy-filling probability is given by $q(\theta_t) = M(1/\theta_t, 1)$.

B. *Government*

The government runs an unemployment insurance (UI) system. The UI system is characterized by a level of benefits b and their expiration rate ν_t which is time varying and depends on the unemployment rate in a state. This assumption is made to mimic the actual system of benefit expiration in the US while ensuring the stationarity of the workers' and firms' decision problems. We assume that all employed workers are entitled to benefits as in Mitman and Rabinovich (2015).

C. *Timing*

At the beginning of the period agents can have one of five labor-market statuses: employed (ℓ), non-employed, participating, and eligible for benefits ($n^{P,E}$), non-employed, participating, and ineligible for benefits ($n^{P,I}$), non-employed, out of the labor force, and eligible for benefits ($n^{O,E}$), non-employed, out of he labor force, and ineligible for benefits ($n^{O,I}$). Non-employed agents then make a participation decision

D. *Value Functions*

Here we describe the value functions recursively. Denote the aggregate state by $\Omega = \{z, \nu(\mathtt{u}), \mathtt{u}\}$, where $\mathtt{u} = u/(u+\ell)$ is the unemployment rate in the state.

Workers. — Denoting the wage by w, the employed have the following value function:

$$W(\Omega) = w + \beta\mu(1-\delta)\mathbb{E}W(\Omega') + \beta\mu\delta\mathbb{E}N^{P,E}(\Omega'). \tag{17}$$

Non-employed with benefit eligibility status $e \in \{E, I\}$ for eligible and ineligible, and participation status $j \in \{P, O\}$ for participants and out of labor force, decide each period whether to participate:

$$N^{j,e}(\Omega) = \mathbb{E}\max\{\underbrace{U^e(\Omega) - \gamma^j + \epsilon_U}_{\text{participate}}, \underbrace{O^e(\Omega) + \epsilon_O}_{\text{not participate}}\} \\ = \frac{\gamma^E}{\alpha} + \frac{1}{\alpha}\ln\left(\exp\left(\alpha U^e(\Omega) - \gamma\alpha\right) + \exp\left(\alpha O^e(\Omega)\right)\right), \tag{18}$$

where ϵ_U and ϵ_O are the type-I extreme value shocks with variance α described above, γ^E is Euler's constant, and U^e and O^e are the value functions for eligible unemployed and out of the labor force, respectively, defined below.

The eligible unemployed receive benefits b and home production h, find jobs at rate $f(\theta)$, have benefits expire at rate $\nu(\mathtt{u})$, and have the following value function:

$$U^E(\Omega) = b + h + \beta\mu f(\theta)\mathbb{E}W(\Omega') \\ + \beta\mu(1 - f(\theta))\left((1-\nu(\mathtt{u}))\mathbb{E}N^{P,E}(\Omega') + \nu(\mathtt{u})\mathbb{E}N^{P,I}(\Omega')\right). \tag{19}$$

The unemployed who do not find a job this period start the following period as participants in the labor force. The ineligible unemployed face a similar value function, except they do not receive benefits, and thus do not face the expiration probability:

$$U^I(\Omega) = h + \beta\mu f(\theta)\mathbb{E}W(\Omega') + \beta\mu(1 - f(\theta))\mathbb{E}N^{P,I}(\Omega'). \tag{20}$$

Non-participants who are eligible for benefits do not receive them, because UI rules require active search. However, if non-participants eligible for benefits start participating again they start receiving benefits again. The value function for an agent out of the labor force who is eligible for benefits (but not collecting) is thus given by:

$$O^E(\Omega) = h + \beta\mu\lambda f(\theta)\mathbb{E}W(\Omega') + \beta\mu(1 - \lambda f(\theta))\mathbb{E}N^{O,E}(\Omega'). \tag{21}$$

The corresponding value function for an ineligible non-participants is:

$$O^I(\Omega) = h + \beta\mu\lambda f(\theta)\mathbb{E}W(\Omega') + \beta\mu(1 - \lambda f(\theta))\mathbb{E}N^{O,I}(\Omega'). \tag{22}$$

Firms. — Firms that are matched with a worker produce z and pay a wage w and have the following value function:

$$J(\Omega) = z - w + \beta\mu(1-\delta)\mathbb{E}J(\Omega'). \tag{23}$$

The value of posting a vacancy is given by:

$$V\left(\Omega\right)=-k+\beta q\left(\theta\right)\mathbb{E}J\left(\Omega'\right). \tag{24}$$

We assume that there is free entry of vacant firms, implying that in equilibrium the value of positing a vacancy is equal to 0.

E. Wage Determination

We assume that wages are determined by Nash bargaining. We denote the worker bargaining power by ξ. The wage then solves the following problem:

$$w=\arg\max\left(W(\Omega)-N^{P,E}(\Omega)\right)^{\xi}\left(J(\Omega)-V(\Omega)\right)^{1-\xi}. \tag{25}$$

F. Laws of Motion

A state enters the period with the stocks of employed and non-employed as defined above. First, participation decisions are made to determine which non-employed engage in active search, and are counted as unemployed in the period, and which choose to be out of the labor force, and potentially contact firms through passive search. The measures of unemployed and out of the labor force are then given by:

$$\begin{aligned}
u^{E}&=\mathcal{P}^{P,E}n^{P,E}+\mathcal{P}^{O,E}n^{O,E},\\
u^{I}&=\mathcal{P}^{P,I}n^{P,I}+\mathcal{P}^{O,I}n^{O,I},\\
o^{E}&=(1-\mathcal{P}^{P,E})n^{P,E}+(1-\mathcal{P}^{O,E})n^{O,E},\\
o^{I}&=(1-\mathcal{P}^{P,I})n^{P,I}+(1-\mathcal{P}^{O,I})n^{O,I},
\end{aligned} \tag{26}$$

where $\mathcal{P}^{e,j}$ are the policy functions associated with the discrete participation choices in Eq. (18). The unemployment rate in a period is given by $\mathtt{u}=(u^E+u^I)/(u^E+u^I+\ell)$. Then we have the laws of motion

$$\begin{aligned}
\ell'&=\mu\left[\left(1-\delta\right)\ell+f\left(\theta\right)\left(u^{E}+u^{I}+\lambda\left(o^{E}+o^{I}\right)\right)\right],\\
n^{P,E'}&=\mu\left[\delta\ell+\left(1-f\left(\theta\right)\right)\left(1-\nu(\mathtt{u})\right)u^{E}\right],\\
n^{P,I'}&=\mu\left[\left(1-f\left(\theta\right)\right)u^{I}+\left(1-f\left(\theta\right)\right)\nu(\mathtt{u})u^{E}\right],\\
n^{O,E'}&=\mu\left[\left(1-\lambda f\left(\theta\right)\right)o^{E}\right],\\
n^{O,I'}&=1-\mu+\mu\left[\left(1-\lambda f\left(\theta\right)\right)o^{I}\right].
\end{aligned} \tag{27}$$

G. Stationary Equilibrium

We consider a steady state equilibrium with constant productivity $\sigma_z = 0$. It consists of value functions $\{J, V, W, U^E, U^I, O^E, O^I\}$, participation policy functions $\{\mathcal{P}^{P,E}, \mathcal{P}^{P,I}, \mathcal{P}^{O,E}, \mathcal{P}^{O,I}\}$, and an equilibrium wage w, such that the policy functions for participation solve the participation problem of the workers (18), the wage solves the Nash bargaining problem (25), the value of positing a vacancy is equal to 0, the laws of motion for $\ell, u^E, u^I, o^E, o^I, n^{P,E}, n^{P,I}, n^{O,E}, n^{O,I}$ are consistent with the job-finding rate and participation decisions.

H. Calibration

We set the time period equal to one month. Steady-state productivity is normalized to one. We calibrate some parameters externally. First, we make the following parametric assumptions. We set the matching function to be Cobb-Douglas, $M(\theta) = \chi\theta^{0.4}$ with a 40% share on vacancies, following the evidence described in Hall and Milgrom (2008). Following Krusell et al. (2017), we assume that the shocks to the cost of participation follow discrete iid distributions $\Gamma^u = \{\gamma^{u,h}, \gamma^{u,l}\}$ and $\Gamma^o = \{\gamma^{o,h}, \gamma^{o,l}\}$. We impose that the high cost of participating shocks are identical across labor force statuses: $\gamma^{o,h} = \gamma^{u,h}$. We set the value of benefits to be $b = 0.45$ representing the average effective replacement rate in the U.S. calculated using the Benefit Accuracy Measurement files from the Department of Labor,[24] and set $\nu = 1/6$ such that on average unemployed are eligible for benefits for six months. We choose the value of the vacancy-posting cost $k = 0.58$ as estimated in Hagedorn and Manovskii (2008). We set the discount factor to $0.99^{1/12}$ implying an annual discount rate of 4%.

That leaves the following parameters to be calibrated internally: the value of home production h, the efficiency of the matching function χ, the level of passive search λ, the separation rate δ, the retirement rate $1 - \mu$, bargaining power of workers ξ, the costs of participation γs and the probability of the shocks, and the variance of the extreme value shocks α. We target the following moments in the calibration: the gross flows from E to U, N, the gross flows from U to E, N, the gross flows from N to E, U reported in Krusell et al. (2017),[25] the fraction of unemployed eligible for benefits of 44% (Auray et al., 2019),[26] the elasticity of wages

[24] The Benefit Accuracy Measurement Program shows that in the 2000s the average replacement rate of the unemployed receiving benefits was just over 45%. In other work, Chodorow-Reich and Karabarbounis (2016) estimate $b = 0.041$ of the marginal product of employment by dividing aggregate dollars spent on UI by GDP per capita minus the model estimated stigma of collecting benefits. Abstracting from the stigma estimates, this number is too low because their measurement is not the relevant value of b. The relevant object in the model is b relative to individual productivity of the unemployed which is 45%.

[25] We target the flow rates adjusted for classification error using Abowd-Zellner correction for 1978:I-2012:III reported in Table 2 of Krusell et al. (2017).

[26] Recent work by Birinci and See (2023) highlights the importance of the heterogeneity in replacement rates and take up rate in quantifying the effects of unemployment benefit levels. While we do not model take-up in our framework, we do explicitly model eligibility and we target the fraction of unemployed who are ineligible (56%) as reported by Auray et al. (2019). Thus, we do capture that the majority of the unemployed at any given time are ineligible for benefits.

to benefit duration of 0.01 documented above, the elasticity of unemployment to benefit duration of 0.2 (Katz and Meyer, 1990). Note that we do not explicitly target the response to EP and LFP to benefits. We summarize the calibrated parameters in Table 5. In addition the calibrated values of the preference shocks are $\gamma^{u,h} = \gamma^{o,h} = 10.312$, $\gamma^{u,l} = -0.457$, and $\gamma^{o,l} = -0.318$. The probability of realizing $\gamma^{u,h}$ is 0.132 and the probability of realizing $\gamma^{o,h}$ is 0.023. Table 6 reports gross labor market flows in the model and in the data, validating the calibration strategy and showing that the model can capture well gross labor market flows across all three states. The model also generates a fraction eligible of 44%, the elasticity of wages to productivity of 0.01 and an elasticity of unemployment to benefit duration of 0.2, as targeted. The internally estimated value of passive search $\lambda = 0.112$ is consistent with micro evidence reported by Faberman et al. (2022) on relative probabilities of applying for vacancies among the unemployed and non-participants in the Survey of Consumer Expectations.

Table 5—: Calibrated Parameters

Parameter	Interpretation	Value
h	home production	0.665
ξ	worker bargaining power	0.014
μ	Retirement/exit rate	0.014
δ	separation rate	0.014
λ	passive search	0.112
χ	efficiency of matching	0.203
α	variance EV shock	2.500

Table 6—: Gross Labor Market Flows

	Model				Data		
From		To		From		To	
	E	U	N		E	U	N
E	0.972	0.012	0.016	E	0.972	0.014	0.014
U	0.229	0.656	0.114	U	0.228	0.637	0.135
N	0.026	0.022	0.952	N	0.022	0.021	0.957

Steady state stocks: U 6.5%, EP 62.2%, LFP 67.6%.

I. *Quantitative Analysis*

In order to validate our model and empirical specification we perform the following experiment. We consider a panel of state economies that start in steady state in 1990. We then draw a sequence state-specific of productivity shocks $\{z_t\}$ for each state. We assume that the monthly persistence of productivity is 0.99 and set the standard deviation of innovations to productivity to 0.003 to generate an unconditional standard deviation of productivity consistent with the standard deviation of output per worker in the post-war U.S. time series.

We assume that after 2025 there are no further shocks to productivity and that the economy returns to steady state in 2027. To mimic the Great Recession, when drawing the sequence of state-specific productivity shocks we impose that on average productivity falls 3% in January 2008. Thus, instead of following the individual state Markov chain for the productivity transitions in 2008, we instead draw innovations to state productivity from a uniform distribution between 0% and -6%. After that period, productivity continues to evolve according to the Markov chain. One can interpret this as picking a particular realization of state-specific innovations to generate an aggregate shock to productivity. Thus, the effect of the "Great Recession" in the model will be heterogenous across states, generating a distribution of labor market outcomes across states.

We set the unemployment benefit policy $\nu(\mathtt{u})$ to be consistent with the Extended-Benefits and Emergency Unemployment Compensation (EUC) programs during the Great Recession, starting in 2008, and assume that those extensions were planned to last through October 2014. We then solve for the perfect foresight path of the economy in response to the sequence of productivity shocks and benefits policy $\nu_t(\mathtt{u}_t)$. This amounts for solving for a fixed point in labor-market tightness and the unemployment rate along the entire sequence. This gives us a sequence of labor market variables through 2014. We then perform a second experiment where, at the end of 2013, unemployment benefit extensions expire unexpectedly (an "MIT" shock, Boppart et al. (2018)) and solve for the updated equilibrium path of labor market variables in 2014.

We aggregate the model generated time series of the employment to population ratio and the labor force participation rate to quarterly frequency. We simulate 100 panels of 51 states. We then estimate the same benchmark empirical specification on the model generated data as on actual data for each of the simulated panels. In Table 7, we report the average (across simulations) of estimated regression coefficients and standard error (across simulations). The empirical analysis performed on the model-generated data captures the same effects as in the U.S. data. In particular, the β coefficients are nearly identical when estimated using the same specification in the model-generated data and in the U.S. data, implying the same large impact of a cut in benefits on the labor force and employment. Moreover, in the model, as in the data, it takes time for the effects of benefits to accumulate in employment and the labor force.

Finally, we validate our empirical specification by calculating the elasticity of employment and labor force participation to the cut in benefits in the model and compare it to the elasticity as implied by our empirical specification estimated on model generated data. First, we compute the elasticity of labor market outcomes

Table 7—: Unemployment Benefit Extensions, Employment and Labor Force: Model EUC Experiment

	EP		LFP	
VARIABLES	Model	Data	Model	Data
$\tilde{\beta}_1$	-0.0036	-0.00414	-0.0009	-0.00314
	(0.0003)		(0.0001)	
$\tilde{\beta}_2$	-0.0109	-0.0106	-0.0059	-0.00675
	(0.0007)		(0.0004)	
$\tilde{\beta}_3$	-0.0156	-0.0168	-0.0112	-0.0106
	(0.0012)		(0.0007)	
$\tilde{\beta}_4$	-0.0188	-0.0214	-0.0152	-0.0145
	(0.0018)		(0.0012)	
Simulations	100		100	
Obs/simulation	5,151		5,151	

Standard errors across simulations in parentheses

to benefits in the model by dividing the log difference of employment and the labor force in Q4 2014 in simulations with and without the unexpected expiration of benefit extensions at the end of Q4 2013 by the log difference in benefits, and then taking the average across states and across the 100 simulated panels. We find that the elasticity of EP to benefits of -0.0189 and the elasticity of LFP to benefits of -0.0147. These numbers are nearly identical to the $\tilde{\beta}_4$ estimates of -0.0188 and -0.0152. Thus, our empirical specification captures very well the effect of a cut in benefits in the model.

Thus, our quantitative model can quantitatively account for our empirical estimates and provides validation of our empirical specification. At the heart of the model mechanism is a strong job-creation channel, which pulls workers from non-participation into employment after the cut in benefits.[27]

IV. Conclusion

In this paper we measure the effect of unemployment benefit extensions on employment and the labor force. Following the aftermath of the Great Recession, by December 2013 there was wide heterogeneity of federally-financed durations of benefits across U.S. states, ranging from 0 to 47 weeks on top of the regular state-funded benefits. In December 2013 the U.S. Congress abruptly and immediately eliminated all federal unemployment benefit extensions. The particular usefulness of this policy change for understanding the employment effects of ben-

[27]The focus of the quantitative exercise was motivated by the 2013 cut in benefits, and so focused on the expiration of *temporary* benefit extensions. The model could be fruitfully used to study the employment effects of *permanent* changes. For example, our model implies a permanent cut from 26 to 20 weeks of unemployment benefits would lead to an increase in employment of 1%, consistent with empirical findings from Karahan et al. (2019) who study Missouri's permanent cut in benefit duration in 2011.

efit extensions stems from the fact that the policy change at the national level was exogenous to economic conditions of individual states. The available benefit duration in a given state just prior to the reform, however, was endogenous to the economic conditions of the state. Thus the key challenge to a proper inference of the effects of benefits is to ensure that the effects are not confounded by pre-existing differences in employment or labor force dynamics.

The classic findings in Blanchard and Katz (1992) imply that state-level employment follows a highly persistent process, suggesting that pre-trends induced by mean-reversion in state economic conditions are unlikely to play a very important role. A simple descriptive analysis in Appendix I.4Persistence of Labor Force and Employment among U.S. Statessubsection.A.4 confirms this to be the case in the period we study. Moreover, we document a significant acceleration of EP and LFP growth in states that experienced larger benefit cuts induced by the reform (and thus had higher benefit duration just prior to the reform). The acceleration is quantitatively the same when measured relative to the trend in 2013, or 2012-2013, or 2011-2013, etc. In other words, this implies that pre-reform dynamics are quite stable with little evidence of mean-reversion prior to the reform. Even more reassuringly, using placebo studies we find no evidence of acceleration in EP and LFP growth in high benefit states in pre-reform time periods (when the reform did not actually take place). This once again suggests that the acceleration of EP and LFP growth of high benefit duration states is not a typical feature of the data. Instead, it only happened when benefit durations where exogenously cut in December 2013.

Our formal analysis is based on a rich but parsimonious model of employment and labor force dynamics. Among other things, it allows for mean reversion by measuring the autoregressive components in EP and LFP. Moreover, it allows for the possibility that the time of the policy reform coincided with the unusual turning point in employment dynamics, whereby employment and labor force growth accelerated more in states experiencing particularly severe lingering effects of the Great Recession by the end of 2013. We find that this model is successful empirically and that there are no significant state-level pre-trends in the residuals of this specification in relation to the size of the benefit duration cut due to the reform. Thus, the common trend assumption required for the validity of our analysis based on the regression discontinuity in the time domain is satisfied.

The results of the formal analysis reveal that changes in unemployment benefits have large and statistically significant effects: a 1 percent drop in benefit duration increases four-quarter-ahead state employment by 0.02 log point and state labor force by 0.014 log points. While these state-level estimates are of independent interest, it is also desirable to be able to use them to infer the effects of a nationwide policy change. We document several empirical facts and provide a simple model that guides the aggregation of these effects. We find that the cut in benefit duration accounted for about 75 percent of the aggregate employment growth in 2014. Over half of the aggregate employment growth was due to the increase in the labor force induced by the policy reform. Considering various alternative models of state-level dynamics in Appendix IIISensitivity of Baseline Findingsappendix.C does not substantively affect the conclusion that the benefit duration cut led to significant employment gains and an increase in the labor force. This is in contrast to the estimated effects of placebo reforms which are generally economically small and statistically insignificant.

While we did not impose any theoretical restrictions of a particular labor market model on our empirical analysis, the findings are consistent with the predictions of the standard equilibrium labor market search model featuring an active job creation channel. To further confirm this, we provided direct evidence for the mechanism by investigating the effect of the benefit cut on vacancy creation as well as wages.[28] Consistent with the job-creation channel, vacancies jump significantly in response to the benefit cut, and wages of newly-hired and all workers fall.

Finally, we develop and calibrate an explicit frictional model of the labor market that is consistent with salient features of the U.S. data, including gross flows across between employed, unemployed and out of the labor force. We show that the effects of an unexpected cut in benefits in the model produce quantitatively similar increases in employment and the labor force as measured in the data. Moreover, we use the model to confirm the validity of our empirical specification. In the model, the rise in job creation in response to the benefit cut leads to an increase in equilibrium employment. The increase in job availability draws non-participants (who are not collecting benefits) into the labor market leading to a positive effect on the labor force, outweighing the countervailing effect of some unemployed leaving the labor force after losing eligibility for benefits. Thus, our empirical findings are consistent with the quantitative implications of the equilibrium search framework that assigns an important role to endogenous job creation.

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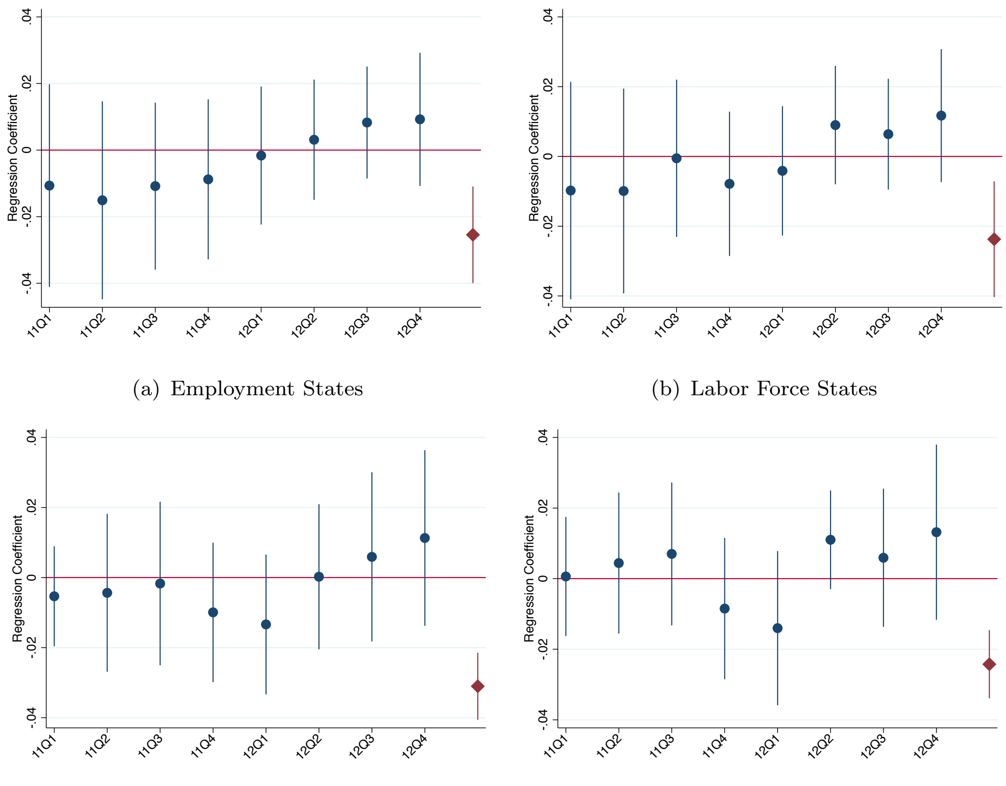


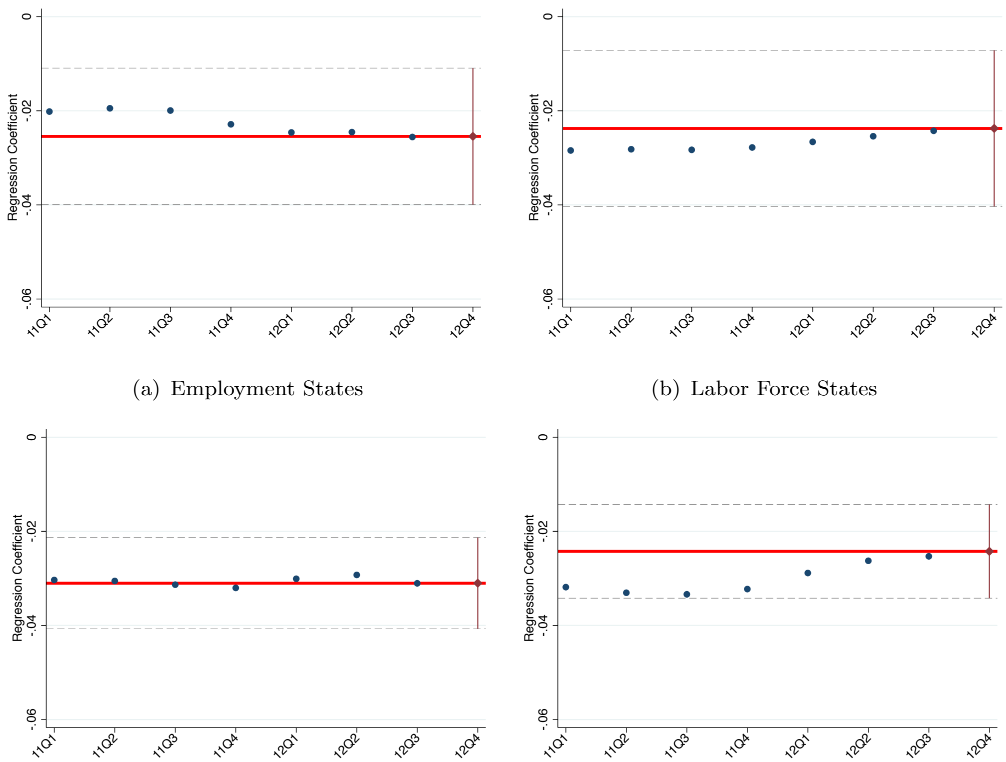
(a) Employment States

(b) Labor Force States

(c) Employment State Borders

(d) Labor Force State Borders

Figure 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 size of placebo benefit cut in the quarter marked on the horizontal axis. States and bordering state pairs. Rightmost point on each panel corresponds to actual reform in 2013q4.



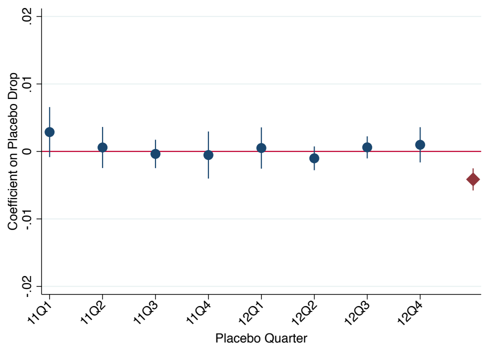
(a) Employment States

(b) Labor Force States

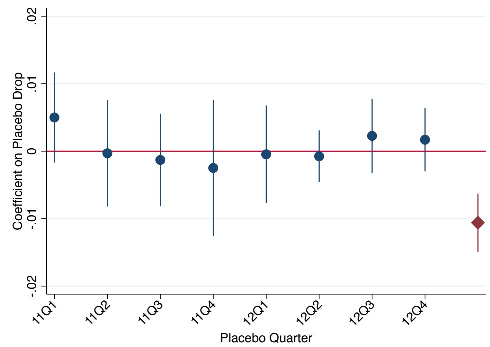
(c) Employment State Borders

(d) Labor Force State Borders

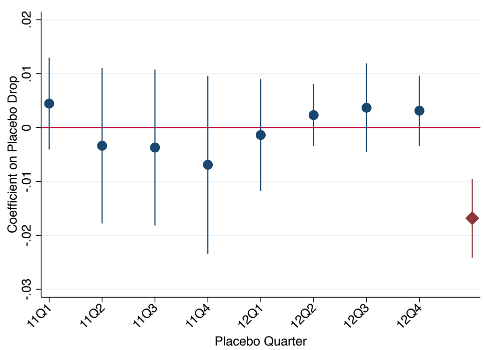
Figure 4. : Slopes of the regression line of the difference in growth rates of EP or LFP in 2014 and between 2013q4 and the quarter marked on the horizontal axis on the benefit duration cut due to the reform, i.e., the difference in benefit duration between 2014q1 and 2013q4. States and bordering state pairs.



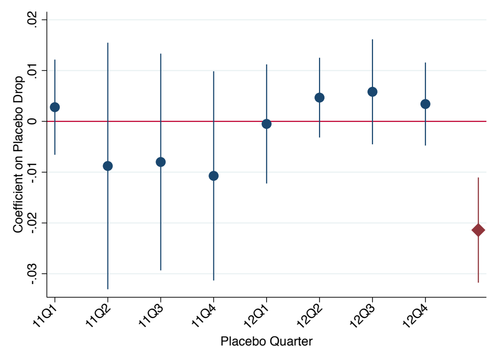
(a) $\tilde{\beta}_1$



(b) $\tilde{\beta}_2$

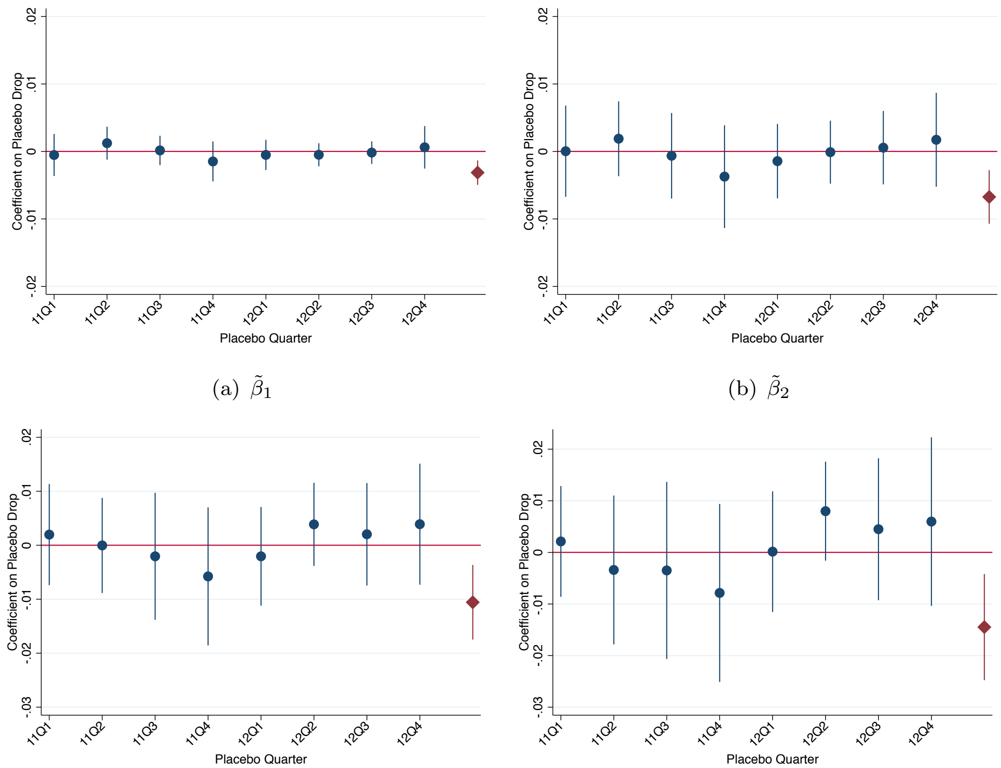


(c) $\tilde{\beta}_3$



(d) $\tilde{\beta}_4$

Figure 5. : Estimated impacts on employment to population ratio, $\tilde{\beta}_\tau$, of a placebo elimination of benefit extensions in the quarter marked on the horizontal axis. Rightmost point on each panel corresponds to actual reform in 2013q4.



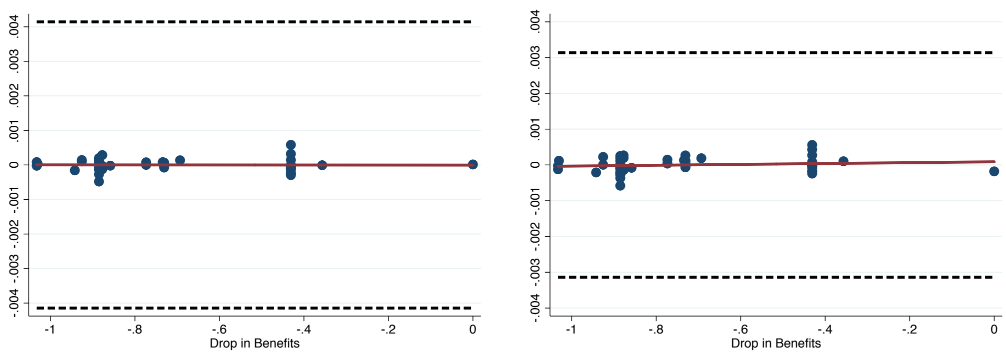
(a) $\tilde{\beta}_1$

(b) $\tilde{\beta}_2$

(c) $\tilde{\beta}_3$

(d) $\tilde{\beta}_4$

Figure 6. : Estimated impacts on labor force to population ratio, $\tilde{\beta}_\tau$, of a placebo elimination of benefit extensions in the quarter marked on the horizontal axis. Rightmost point on each panel corresponds to actual reform in 2013q4.



(a) Employment

(b) Labor Force

Figure 7. : Pre-trends in residuals from baseline specification vs drop in benefit duration between 2013q4 and 2014q1. The dashed lines show the estimated coefficient $\pm\tilde{\beta}_1$.