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How Large were the Effects of Emergency and Extended Benefits on Unemployment during the Great Recession and its Aftermath?

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Abstract

This paper presents estimates of the effect of unemployment benefit extensions during the Great Recession on unemployment and labor force participation. Unlike many recent studies of this subject, our estimates, following the work of Hagedorn, Karahan, Manovskii, and Mitman (2016), are inclusive of the effects of benefit extensions on employer, as well as, worker behavior. To identify the effect of benefit extensions, we use plausibly exogenous changes in the rules governing benefit extensions and their differential effects on the maximum duration of benefits across states. We find that the effect of benefit extensions is likely modest, with a 90 percent confidence interval of the effect on the unemployment rate ranging from 0 to ½ percentage point.

JEL classifications: J6, E24.

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1. Introduction

This paper presents estimates of the effect of unemployment benefit extensions during the Great Recession on the unemployment rate and the labor force participation rate. In light of Hagedorn, Karahan, Manovskii, and Mitman (2016), henceforth HKMM, our estimates account for the effect of benefit extensions inclusive of general equilibrium responses of employer behavior. In particular, HKMM suggest that prior estimates of the effect of Emergency Unemployment Compensation and Extended Benefits, henceforth EEB, on unemployment are incomplete because they ignore a potentially important general equilibrium effect of extended unemployment benefit durations on labor demand operating through the vacancy posting behavior of employers.[1] HKMM estimate that when this "macro effect" is taken into consideration, EEB raised the unemployment rate by about 2 percentage points in 2011. HKMM's findings highlight the importance of understanding and accurately measuring the "macro effect" of EEB and suggest that in order to estimate the full effect of EEB on unemployment one must take into account employer behavior.

Estimating the effects of EEB is difficult because changes in EEB are correlated with national economic conditions across time, while variation in EEB across states is determined by state unemployment rates and hence is correlated with state-level economic conditions. Our estimation method attempts to capture the causal effect of EEB on the unemployment rate by using exogenous variation in federal benefits across states resulting from rule changes in the EEB program. Rules regarding the relationship between the maximum duration of Unemployment Insurance (UI) benefits and state-level unemployment rates were changed four times from 2008 to 2014. Because these rule changes were plausibly exogenous to changes in *relative* state-level economic conditions, we can use them to instrument for benefits durations.

We estimate effects separately for each of the rule-change episode. Our preferred estimate, which takes the average across episodes with weights inversely proportional to each estimate's variance, implies that EEB caused the unemployment rate to rise by about ¼ percentage point from 2007 to 2011. Confidence bands range from around 0 to about ½ percentage point, encompassing the estimates found in most previous studies, though our estimation method, unlike that in most of these studies captures the response of employer behavior to changes in EEB. Additionally, we estimate the effect of EEB on labor force participation and find no effect.

Interestingly, estimates of the effect on unemployment from the first two rule changes, when firms and households may have expected extended benefits to last a relatively long time (as indeed they did), are larger, averaging almost 1 percentage point, than our estimate for the last episode, when extended benefits may have been expected to terminate soon. This difference points to the potential importance of expectations, as highlighted by HKMM, and suggests that expectations about the persistence of extended benefits may play an important role in determining the size of their effect.

[1] Lalive, Landais, and Zweimuller (2015) point to another channel implicitly ignored by many previous studies: that the ability of workers not eligible for unemployment insurance to find jobs may change in response to a change in UI benefits. This channel would also be included in our estimates of EEB effects.

To put the contribution of this paper in context, the next section summarizes the literature on the effect of benefit extensions on unemployment. In the third section, we present our estimation method, and in the fourth section we present results. The fifth section concludes.

2. Background Literature on EEB Effects

In a standard model of job search, the value of remaining unemployed, as opposed to taking the most recent wage offer or dropping out of the labor force, will be a positive function of the present discounted value of unemployment benefits, B. The value of these benefits is determined by the weekly payment, z, and the duration of benefits. If eligibility for benefits is lost at constant rate δ, then the expected value of benefits is related to the expected duration of benefits times the payout, $B = z(1/\delta)$; the less likely is benefit exhaustion, the higher are expected benefits and the less frequent will be movements into employment and out of the labor force.

Much of the empirical research on the influence of EEB on individual behavior has estimated the effect on some measure of labor force status (e.g. propensity to exit unemployment, fraction of time spent unemployed) of differences in allowable EEB across individuals, where the variable describing EEB could be the maximum duration of EEB benefits, the number of weeks of benefits still available to an individual, or some other measure.

Because EEB benefits are the same across individuals (with comparable work histories) within the same state at the same time, researchers must typically use variation in EEB across time or across states. A typical model is conjectured as follows:

$$y_{it} = f(X_{it}, d_{it}; \theta, \epsilon_{it}). \qquad (1)$$

y_{it} is some measure of labor force status for individual i at time t, X_{it} is a vector of variables intended to capture the influences of factors other than EEB on individual behavior, d_{it} describes the duration of EEB benefits available to individual i, θ is a vector of parameters, and ϵ_{it} is an error term capturing unobserved influences on individual behavior. Typically, X_{it} includes demographic characteristics of the individual (age, gender, education, occupation, duration of current unemployment spell, etc.) as well as variables describing the individual's current economic environment (job growth or the unemployment rate in the individual's state, for example). The identifying assumption is that these control variables capture any influence on individual behavior that is also correlated with EEB benefits.

Of particular concern in this regard is the correlation between EEB benefits and economic conditions. Typically, legislation is passed in the beginning stages of a recession that increases the number of weeks that individuals can receive EEB benefits, and these extensions usually last through the early part of a recovery. Thus, changes in EEB benefits across time are correlated with changes in economic conditions, and this variation must be controlled for to generate unbiased coefficient estimates. It is also typical that enacted legislation grants individuals in states suffering higher levels of total or insured unemployment longer periods of EEB benefit receipt. Thus, differences in EEB benefits across states are also mechanically correlated with adverse economic conditions that must be controlled for.

There is a vast literature on the implications of unemployment benefit duration on the labor market. Among the first was Moffitt and Nicholson (1982), which measured the influence of the total number of weeks of unemployment benefits (regular and EEB) on the fraction of an individual's time spent unemployed. In another early study, Meyer (1990) examined the influence of the number of weeks of benefits remaining for an individual on that individual's propensity to exit UI rolls. Moffitt and Nicholson (1982) used a nationally representative sample of about 1000 individuals who had received Federal Supplementary Benefits during the mid-1970s, while Meyer (1990) studied administrative UI data from 12 states from 1978-1984. Both studies had data on the total weeks of UI benefit eligibility by individual and were thus able to take advantage of differences in benefit availability for individuals with different work histories as well as differences in benefits across states and time. More recently, Schwartz (2009) studied the effect of EEB benefits in the early 1990s recession, while Card and Levine (2000) studied the effect of temporary, politically-motivated (as opposed to economically-motivated) increases in weeks of benefit eligibility in New Jersey in the mid-1990s. Broadly speaking, results of these earlier studies of EEB found that an added week of benefit duration increases unemployment duration by about 0.1 week.

Under additional assumptions, one can translate this estimate into an estimate of the effect of EEB on unemployment in the most recent episode. This is the methodology of Mazumder (2011), who estimates an EEB effect of 0.8 percent.[2] Nakajima (2012) also uses these earlier results to calibrate a model of job search and estimates an effect of EEB on unemployment of 1.4 percentage points. In a similar manner, Mitman and Rabinovich (2014) use a calibrated search model of unemployment and also find estimates of the effect of benefit extensions in the Great Recession consistent with these estimates.

Other studies have attempted to directly estimate the effect of EEB on unemployment using data from the most recent episode. Fujita (2011), Rothstein (2011), Farber and Valletta (2013), Bradbury (2014), Barnichon and Figura (2014), and Farber, Rothstein, and Valetta (2015) all estimate the influence of differences in EEB eligibility on differences in the propensity to exit unemployment. Rothstein (2011) estimates the relationship between the maximum weeks of total benefits available in a state and the propensity of individuals to exit unemployment into employment or out of the labor force. Similarly, Farber and Valletta (2013) estimate the relationship between whether an individual is likely eligible for EEB and unemployment exit rate behavior. To control for the influence of economic conditions on both the propensity to exit and the duration of benefits, both Rothstein (2011) and Farber and Valletta (2013) use functions of a state's unemployment rate and employment growth rate. To bolster their identification strategies, both of these studies also exploit variation across individuals at a point in time within states. In particular, they compare workers who report being job leavers who would not typically be eligible

[2] Multiplying the average increase in UI benefits across states from 2007 to 2010 (69 weeks according to Mazumder (2011)) by 0.1 yields an estimate of an average increase in unemployment duration of 7 weeks, or, assuming an average duration prior to the most recent recession of 17 weeks (as Mazumder (2011) does), about a 40 percent increase. Assuming a take-up rate of unemployment benefits of 40 percent, and given an assumption that the average duration is the inverse of the exit rate from unemployment, this suggests about a 15 percent decline in the exit rate from unemployment. Using a first order approximation to the condition defining the steady state rate of unemployment, one can translate this increase into roughly a 0.8 percentage point increase in the unemployment rate. Under different assumptions, the effect could be considerably larger or considerably smaller.

for unemployment insurance compared to job losers who would be eligible. Additionally, Rothstein (2011) estimates a model which exploits the timing of benefit extensions relative to an individual's date of benefit exhaustion. Generally, estimates of the effect of EEB on unemployment in the most recent episode based on these methods range from ¼ percentage point to ¾ percentage point.

HKKM (2016) point out that EEB may affect the behavior of employers through its effect on the incentives of job searchers and setting. HKMM argue that previous estimation methods that control for state-level unemployment—which embeds the consequences of any changes in employer behavior —implicitly ignore the response of job vacancies and thus, by construction, understate the elasticity of unemployment with respect to the duration of benefits.[3] To capture the full response of labor demand, HKKM estimate the causal effect of UI benefit duration by comparing unemployment rates in counties that share a state border, where on one side of the border, lengthier benefits are available. HKMM assume that any difference in economic conditions between these border counties is uncorrelated with the difference in EEB benefits across the bordering states. With this assumption they are able to use the difference in unemployment rates across the two border counties to estimate the effect of EEB on unemployment.[4] HKMM estimate a much larger effect of EEB, 2 percentage points, than prior studies, and they attribute this difference to their method's ability to capture the effects of EEB on labor demand.[5] Similarly, Johnston and Mas (2015) estimate a large 1 percentage point effect on the unemployment rate from a sudden reduction of 16 weeks in the maximum duration of benefits in Missouri in 2011.[6]

Chodorow-Reich and Karabarbounis (2016a) present evidence of a much more modest aggregate effect of extended benefits on unemployment using a novel method of distinguishing between the effect of benefit increases and changes in economic conditions. They point out that benefit triggers are based on real-time initial estimates of state-level unemployment rates. However, these initial estimates are often revised to incorporate more complete information. If one assumes that the

[3] On the other hand, unemployment benefits may also reduce unemployment by stimulating consumption. A back of the envelope calculation consistent with CBO (2013) methodology suggests this could be substantial. The CBO (2013) assumes a spending multiplier of about 1.5 on unemployment insurance outlays. In 2010, unemployment insurance outlays on extended and emergency benefits were about $100 billion dollars above 2007 levels. Under CBO's assumption, this would imply an increase in GDP of around 1 percent. Using a standard Okun's law relationship between changes in output and unemployment of about one-half, this would imply a reduction in unemployment of around ½ percentage point.

[4] Using the unemployment rate as the dependent variable—as opposed to the exit rate from unemployment used by Rothstein (2011) and Farber and Valletta (2013)—also has other benefits, as the estimating equation itself will provide a direct estimate of the effect of EEB on unemployment, in contrast to previous methods, where alternative assumptions and computations were necessary to map estimated effects on escape rates into estimates of effects of EEB on unemployment.

[5] Dieterle, Bartalotti, and Brummet (2016) suggest that state-border county-pair estimates should be treated with caution due to two biases. The first bias arises from variation in the distance of a county's population center from the state border, which tends to bias up coefficients. On the other hand, if there is variation in how integrated a county pair's labor market is, this bias will serve to attenuate the coefficient estimates. As the two biases are in opposite directions, the net effect is uncertain.

[6] The 16 week reduction resulted from a legislated 6 week reduction in the regular UI benefit from 26 weeks to 20 weeks and an additional 10 week reduction in EUC benefits, as these benefits were calculated in proportion to regular UI benefits. Interestingly, the effect on the behavior of individuals receiving the reduced maximum of 57 weeks of benefits, relative to individuals claiming benefits prior to enactment of the legislation and receiving a maximum of 73 weeks, occurred in the first 20 weeks of individuals' unemployment spells.

revised data most accurately reflect economic conditions, then some states likely received extended benefits by mistake: extended benefit were triggered by the initial, inaccurate estimate of the unemployment rate, but would not have been by the revised estimates. Chodorow-Reich and Karabarbounis (2016a) use these episodes of mistaken triggers to estimate the effect of benefit changes and find the effect to be small and not statistically significant.[7] Most recently, Boone, Dube, Goodman, and Kaplan (2016) use county-level data and also estimate modest EEB effects.

Our approach is guided by the general equilibrium critique laid out in HKMM, and we attempt to capture the full effect of EEB on unemployment by exploiting differences in benefits across states that are uncorrelated with differences in economic conditions. To do this, we use state-level unemployment rate data from the Local Area Unemployment Statistics, or LAUS, program and exploit legislative changes that induce differential increases (and decreases) in EEB at discrete points in time across states.[8] These differential changes are plausibly uncorrelated with differential changes in economic conditions around these rule changes.

3. Estimation method: Using changes in EEB benefit rules to estimate the effect of EEB on unemployment and participation

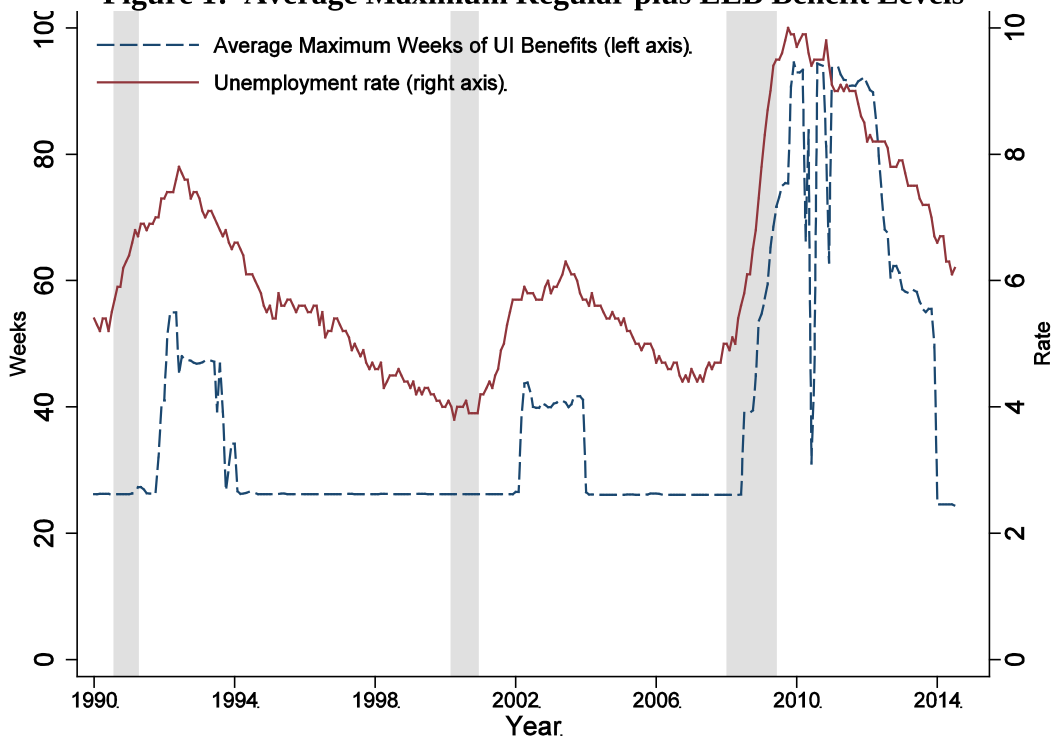
In this section, we develop an instrumental variables estimator of the effect of EEB on unemployment and participation. To ensure that we capture the effects of EEB on both labor demand and labor supply, we use the unemployment rate as our outcome variable, following HKMM. To construct our instrument, we use data from the Employment and Training Administration on rules governing the maximum level of Emergency Unemployment Compensation (EUC) benefits available in a state, which were adjusted four times during the recession and recovery. We can use these rule changes to instrument for benefit durations for the following reason: the differential effect of rules changes on state-level benefits were plausibly exogenous to changes in state-level economic conditions.[9,10]

In the post-WWII period, legislation has been passed in recessions to provide temporary federal unemployment benefits to unemployed workers. In addition, the ongoing extended benefits program provides additional weeks of benefits when state unemployment rates reach certain pre-specified levels. As a result, the maximum duration of unemployment benefits increases noticeably in recessions. Figure 1 shows the evolution of average benefit durations (weighted by a state's labor force) since 1990. Most recently, average benefit levels peaked in 2010 and 2011

[7] As this paper is quite recent, a consensus has not yet formed about the efficacy of this approach. See Hagedorn, Manovskii, and Mitman (2016) and Chodorow-Reich and Karabarbounis (2016b).
[8] The state-level LAUS data are constructed from state-level data from the Current Population Survey (CPS) combined with state-level UI and employment data from the Current Employment Statistics program to filter out high-frequency noise in the state-level CPS data. We also estimated EEB effects using state-level CPS data. However, the results are quite imprecise due to the volatile nature of the state-level estimates. Even so, the point estimates we find using the state-level data are similar to those using LAUS data.
[9] Our empirical method has been labeled IV-DID by Chaisemartin (2012) and has been used most notably by Duflo (2001).
[10] Importantly, for our identification strategy, extended benefits went into effect almost immediately after the relevant legislation was passed. As a result, it is unlikely that individuals and firms significantly changed their behavior prior to actual granting of extended benefits in anticipation of their future availability.

at a little above 90 weeks, up about 65 weeks from their pre-recession levels, and declined to about 25 weeks by the beginning of 2014. The jagged behavior of benefits in 2010 reflects lapses in funding authority for EUC.[11]

Figure 1: Average Maximum Regular plus EEB Benefit Levels



Note. Gray bars represent NBER-dated recessions.

The most recent EUC program was created on June 30, 2008 and was first modified in November of that year. Prior to the November 2008 change, individuals in states with unemployment rates greater than 6 percent were eligible for the same maximum duration of federal benefits as individuals in all other states, but after November 2008, individuals in these states were immediately eligible for 13 more weeks of federal benefits than individuals in other states. The program was modified again in November 2009. Prior to the November 2009 rule change, individuals in states with unemployment rates greater than 8.5 percent were eligible for the same maximum duration of benefits as individuals in states with unemployment rates greater than 6 percent but less than 8.5 percent. But after November 2009, individuals in the former set of states were eligible for greater benefits. Beginning in March 2012, a series of rule changes increased the maximum weeks of benefits available to individuals in the worst-hit states relative individuals in states that were less affected by the recession. Finally, the termination of EUC at the end of 2013

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[11] It is unclear how individuals responded to these benefit lapses. Some individuals may have expected benefits to eventually be reinstated and thus may not have changed their search behavior, while others may have changed their search behavior either because they did not expect an eventual resumption of emergency benefits or because they lacked significant alternative sources of income. The benefit lapses only affect our estimates for the second episode of the rule changes described below. For this episode, we estimated EEB effects both using the actual data on benefit durations and using adjusted durations, which assume that benefit durations remained at the level existing prior to the benefit lapse.

decreased benefit durations by different amounts in different states depending on a state's unemployment rate just prior to the end of EUC. More details on the rule changes are presented in Table A of Appendix A.

While the timing of the benefit extensions in 2008-2012 and their elimination at the end of 2013 were indeed correlated with the aggregate business cycle, the differential effect that these legislative changes had on states in different benefit duration tiers is likely not. Thus, we use these plausibly exogenous (to the state) rule changes to help us solve the identification challenge posed by the endogeneity of EEB. In the following, we lay out a simple model of the determinants of benefit durations and unemployment rates across states to highlight our strategy.

Suppose that the unemployment rate in state *s* at time *t* is a linear function of both the duration of UI benefits available and economic conditions in state *s*, $\alpha_{s,t}$.

$$u_{st} = \beta d_{st} + \alpha_{st} + \epsilon_{st} \tag{2}$$

ϵ_{st} is assumed to be i.i.d and represent all other factors that might influence the unemployment rate not captured by d or α. We would like to estimate β, but because benefit durations are correlated with the state of the economy (both across time and across states) we need to either control for α_s or isolate changes in d that are orthogonal to α_s. Our strategy is to instrument for benefits using the differential effect on state benefit levels of the exogenous rule changes described above. The rule changes specify different benefit levels for different pre-rule-change levels of state unemployment. Thus, states with similar unemployment rates before a rule change will experience similar changes in benefits after the rule change. Our identifying assumption is, thus, that differential changes in benefits, because they occur in response to an exogenous rule change at time *T*, are not correlated with any differential change in economic conditions, described by α. Thus, around a rule change at time *T*, we require only that the difference in economic conditions across the affected and unaffected groups of states is expected to be 0 after the policy is implemented:

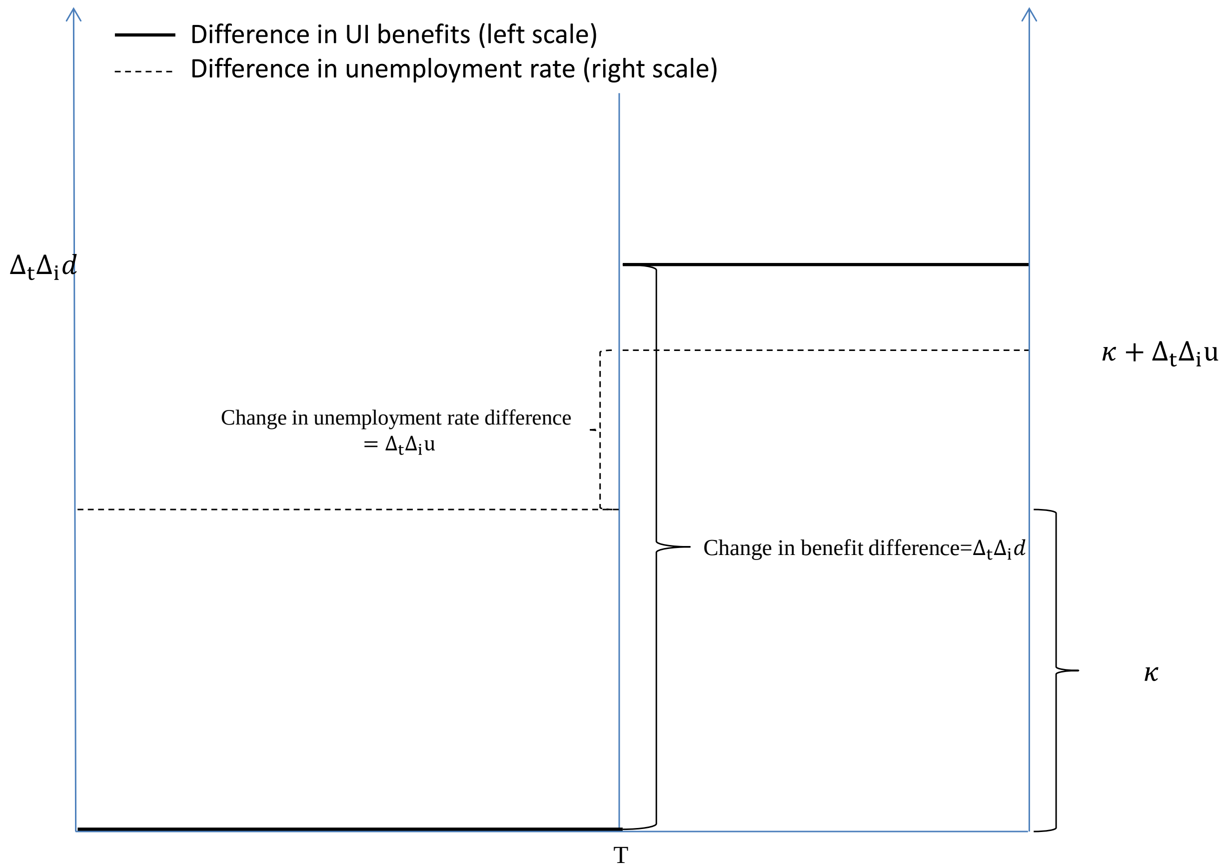
$$E\left[\left(\left(\alpha_{1,T+1} - \alpha_{2,T+1}\right) - \left(\alpha_{1,T-1} - \alpha_{2,T-1}\right)\right)\right] = 0.$$

Further details on our assumptions are available in Appendix B.

Because the variation in benefits that we utilize is both across time (the rule change occurs at a point in time) but also across states (the rule change occurs differentially across states) some intuition for how the effect is identified can be acquired by considering differences in differences in benefits and unemployment rates. For example, prior to a rule change, individuals in states 1 and 2 may receive the same benefits but have different unemployment rates, with $u_{1,t} > u_{2,t}$. At time *T*, a rule change is announced that grants differentially higher benefits to states with higher unemployment, and, as a result, state 1 will receive an increase in benefits relative to state 2. Our estimate of β is identified using the differential change in benefits across states due to the rule change that is plausibly exogenous to differential changes in economic conditions across states 1 and 2.

An idealized depiction of our identification strategy is shown in figure 2. Prior to time *T*, unemployment in state 1 is higher than that in state 2 by κ, the difference between the lower dashed line and the x axis. Benefits increase in state 2 relative to state 1 by $\Delta_t \Delta_i d_{i,t}$, the difference between the solid line and the x axis, after the rule change at time *T*, and unemployment increases in state 2 relative to state 1 by $\Delta_t \Delta_i u_{i,t}$. Thus, the effect of an additional week of benefits in this example is simply $\Delta_t \Delta_i u_{i,t} / \Delta_t \Delta_i d_{i,t}$.

Figure 2. Illustrating the Identification Strategy



Of course, differential changes in benefit levels across time are not completely determined by rule changes. When state unemployment rates change due to changing economic conditions, EEB will also change. To isolate the exogenous variation in benefit levels, we must instrument for differential changes in benefits using rule changes. Our empirical method is, thus, similar to that in Duflo (2001).

Our estimation procedure must also take into account that the effect of the rule change on differences in benefits across states will likely fade over time. This is because of subsequent rule changes and the tendency for some of the cyclical differences in unemployment across states to fade over time. Both of these have the consequence of reducing the correlation between our instrument and benefit differences across states as the length of time from the rule change increases. As a result, we use a limited window of time around each rule change to ensure variation due to the exogenous rule change is significant relative to other types of variation. Specifically, we use a 13 month window around each policy change. If the policy changes in month T, we include months T-5 through T+7 in the sample.[12]

Our first-stage regression specifies benefit durations as a function of state group dummies, I_s^H, where groups H are determined by the unemployment rate specified by the relevant rule change, a dummy for periods following the rule change, I_t^r, the interaction between these two dummies (our instrument), common year effects, T_t, and other controls, denoted by X_t, which includes group-specific time trends:

$$d_{s,t} = \alpha_0 + \alpha_1 I_s^H + \alpha_2 I_t^r + \alpha_3 I_s^H I_t^r + \ \alpha_t^T T_t + X_{s,t} A + \eta_{s,t}. \tag{3}$$

The group-specific time trends control for changes in economic conditions that may be positively correlated across time. If, for example, states with relatively high unemployment rates at the time of a policy change have experienced sharper declines in economic conditions than other states and also experience sharper declines after the policy change, then the relative increase in benefits a state receives at the time of the policy change may be positively correlated with the subsequent change in economic conditions, biasing the absolute value of our estimated effect of EEB upwards. However, time trends should control for the economic trajectory of states before and after the policy change and thus limit the potential for this type of bias.

Our instrument, $I_s^H I_t^r$, captures the differential change in benefits across states following the rule change. We then estimate the following second-stage equation

$$u_{s,t} = \delta_0 + \beta \hat{d}_{s,t} + \delta_1 I_t^r + \delta_1 I_s^H + \delta_t^T T_t + X_{s,t}\Phi + \nu_{s,t}. \tag{4}$$

We also use estimating equations analogous to equations (3) and (4) to estimate the effect of EEB on labor force participation.

Table 1 below describes the groups of states we use for each policy change. In the first three episodes, we identified states that are more likely to receive relative benefit duration boosts after the rule change as those states with unemployment rates in the period preceding the rule change at least ½ percentage point above the threshold unemployment rate for receiving differentially higher benefits following the rule change. This ½ percentage point buffer is used to reduce the probability that states will switch groups after the rule changes.[13] In the group of states unlikely to receive a relative benefit boost, we include states with an unemployment rate at least ½ percentage point below the threshold level. In the last episode, because we distinguish between four groups of states, including ½ percentage point buffers would greatly limit the number of states in our groups. As a result, we don't use buffers for this episode. For the first three episodes, we distinguish between two groups of states and have one instrument; for the final episode, we distinguish between four groups of states, and, thus, have three instruments.

[12] We include a slightly longer post-change window to allow for gradual adjustment to the change in benefits. Estimates using slightly different windows yield similar results.

[13] Consistent with this approach, we find that first-stage F statistics increase as the buffer is increased from 0 to ½ percentage point and then decrease as the buffer is increased further Our results are robust for buffers in the range of 0 to about ¾ percentage point.

When grouping states based on their pre-rule-change unemployment rate, we use the real-time data on unemployment rates, publicly provided by Chorodorow-Reich and Karabarbounis (2016), as this is the data used by the Department of Labor to trigger on or off the different benefit tiers. However, we use the current vintage of unemployment rate data as the dependent variable of equation (4), as these are the most accurate estimates of state-level unemployment. [14]

Table 1: Change in Maximum Duration of Benefits Across Groups of States

Group	Number of States	Three-month unemployment rate immediately prior to rule change	Federal Benefit level prior to rule change	Federal Benefit level after rule change
November 2008				
1	17	>=6.5	13	20
2	25	<=5.5	13	13
November 2009				
1	22	>=9.0	33	53
2	19	<=8.0	33	47
March-September 2012				
1	10	>=9.5	53	47
2	8	<6	34	14
January 2014				
1	2	>=9	53	0
2	20	>=7 & <9	47	0
3	15	>=6 & <7	37	0
4	14	<=6	14	0

At this point, a brief discussion is warranted on the reliability of the LAUS data at the state level. The BLS constructs state-level estimates using a statistical model that relies primarily on state-level data from the CPS, but also incorporates data on UI receipt from state UI agencies and data on employment from the Current Employment Statistics program. In addition, the unemployment (and labor force) estimates are smoothed to filter out the very high frequency oscillations inherent in the small samples available in the CPS. Since the BLS uses UI data to infer the unemployment rate, there may be a mechanical relationship between the duration of UI benefits and the unemployment rate used in our analysis. This relationship should tend to bias the coefficient we estimate up, resulting in larger effects of EEB on unemployment.[15] On the other hand, it is possible that the smoothing methods used in the construction of the LAUS data eliminate some of the true

[14] See Chodorow-Reich and Karabarbounis (2016) for an explanation and analysis of the difference between real-time and current vintage data on unemployment rates.

[15] For example, unemployed individuals in states with longer benefit durations may be more likely to apply for benefits. The resulting higher level of UI claims may, in turn, boost LAUS estimates of unemployment.

effects of the EEB program. Our estimates that use only the state-level CPS data, however, are qualitatively similar to the estimates reported below, although they are quite imprecise.

4. Estimation results

Unemployment rate

Figures 3 through 6 are the empirical analogue of Figure 2. They each show the effect of the policy changes on the difference (across states) in benefit durations and the difference in unemployment rates during these three episodes. For the last episode, all four tiers of EUC were eliminated, allowing us to make three relative comparisons. Thus, Figure 6 compares each of the three highest unemployment rate groups with the lowest unemployment rate group of states. The solid lines in the figures represent the difference in benefit durations between the groups of states in Table 1. Prior to the rule change, this is relatively constant and reflects only changes in unemployment rates at the state level that move a state from one benefit tier to another. The vertical lines mark the month immediately prior to the rule changes. To the right of this line there is a large step up in the relative level of benefits, with the notable exception of the 2012 episode which we discuss further below. The dashed line represents the difference in unemployment rates across states, weighted by the labor force. If benefits have a large effect on unemployment, then after the rule change we should see a noticeable shift in the relative unemployment rate. However, such a shift is not readily apparent in these figures. Regarding the second episode (figure 4), the drop off in benefits at the end of the November 2009 episode is caused by the temporary expiration of EEB.

Figure 3: November 2008 Rule Change (Differences in unemployment rates across high and low unemployment rate groups)

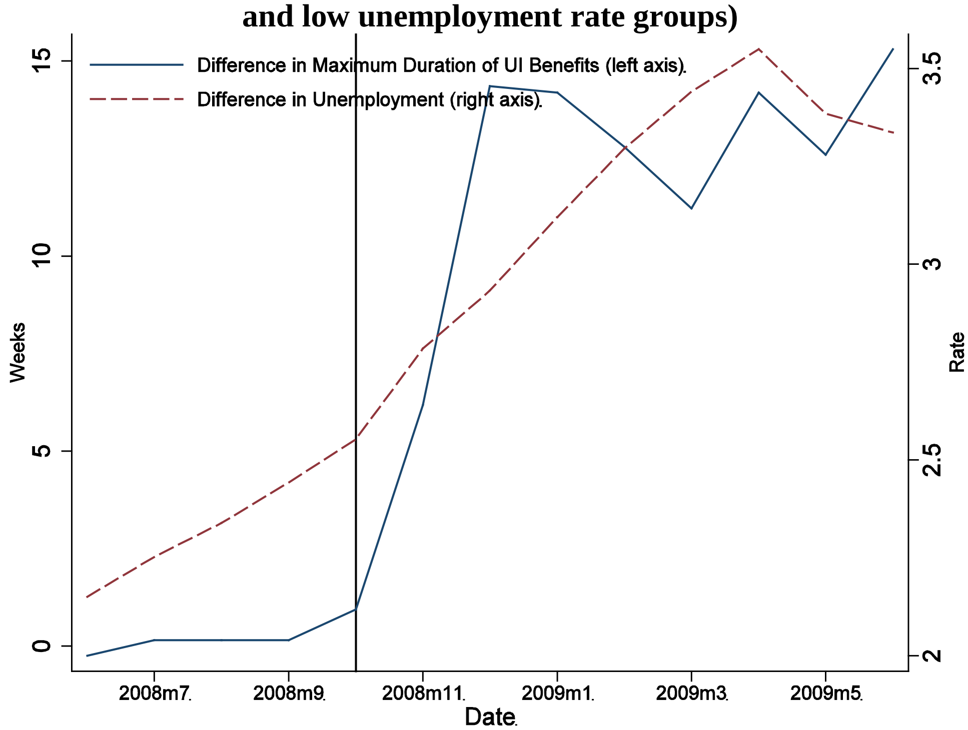


Figure 4: November 2009 Rule Change (Differences in unemployment rates across high and low unemployment rate groups)

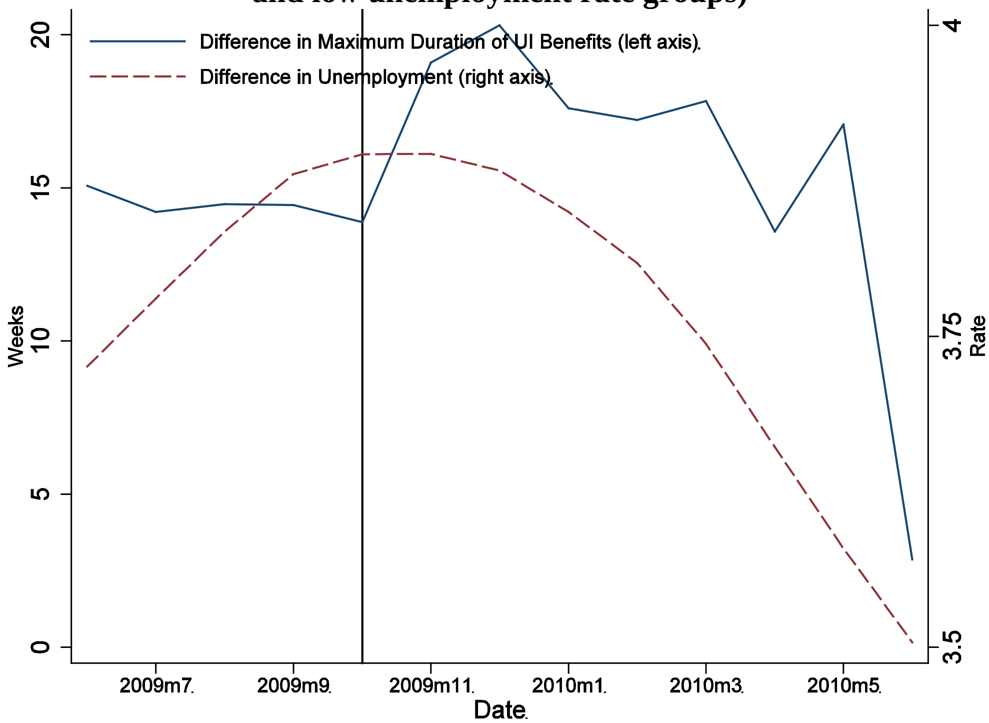
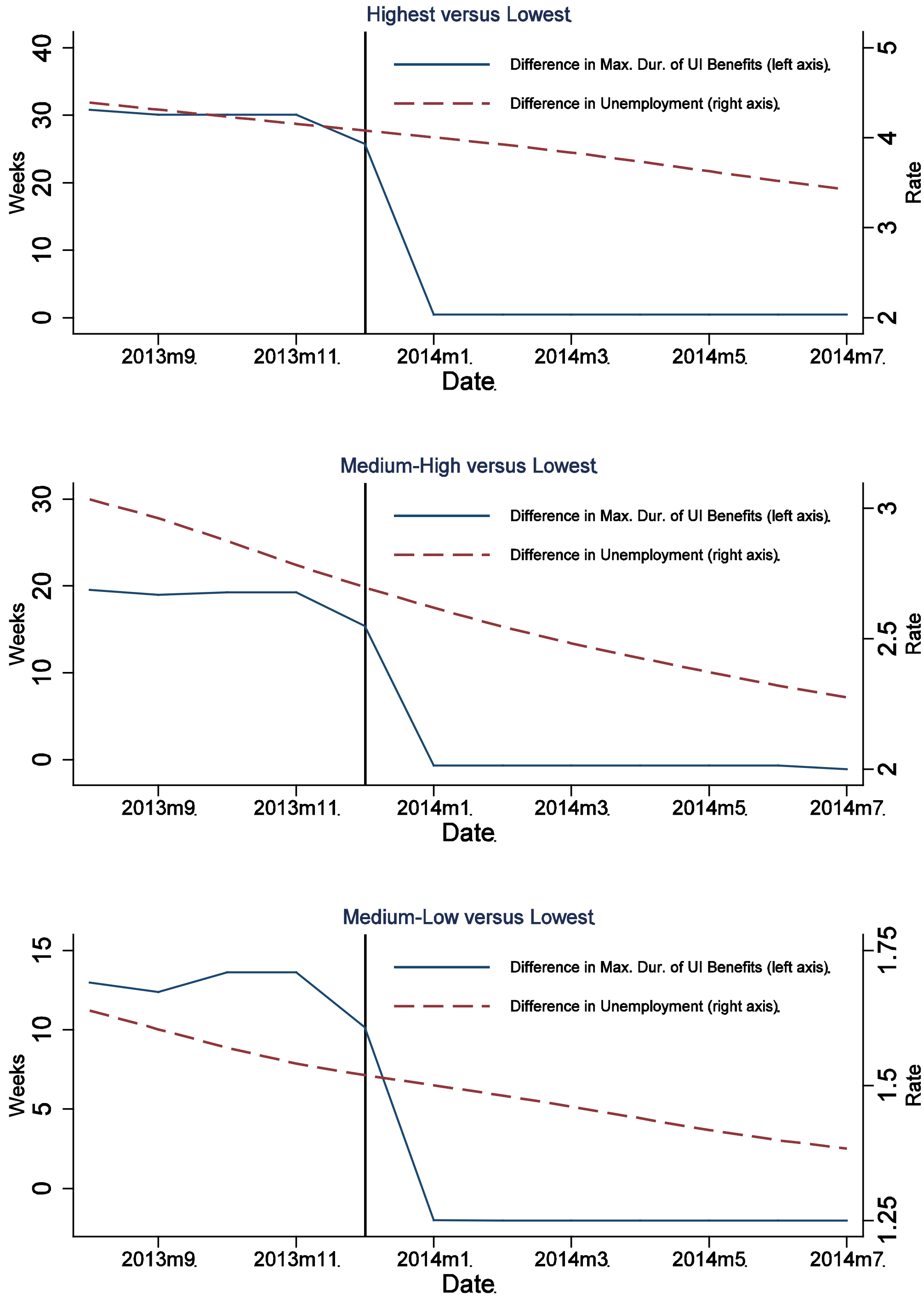


Figure 5: March-September 2012 Rule Change (Differences in unemployment rates across high and low unemployment rate groups)

Figure 6: January 2014 Rule Change



We now turn to estimating equations 3 and 4. For the first-stage regressions described in equation (3), with one exception the fit is quite good, with R^2 statistics all over .7. The exception is the third episode, the six month period from March to September of 2012 over which federal benefits for states with the highest unemployment rates were gradually increased relative to other states. For this episode, the F statistic is essentially zero, as is suggested by Figure 5 above. The problem occurs in this episode because over these months, while unemployment was declining broadly, many states were triggering off of extended benefits. As a result, the maximum duration of total benefits in the high-unemployment states changed little relative to other states and our instrument is not strongly correlated with a rise in total benefits. Consequently, we restrict our analysis below to the other three episodes. In these episodes, F statistics for Wald tests on the coefficients for our instruments are large and above the rule-of-thumb threshold of 10 (Staiger and Stock, 1997), as shown in Table 2.

Table 2: First-Stage F Statistics

	November 2008	November 2009	March-September 2012	January 2014
First-stage F statistic	53.8	15.6	≈0	129.1

Now, we turn to estimates from our second stage regression. Table 3 shows estimates of β from equation (4). The first row shows point estimates of β, which are quite small.

Table 3: Estimation Results for Effect of EEB on the Unemployment Rate

	November 2008	November 2009	January 2014
β	0.0244	0.00806	0.00321
	(0.0285)	(0.00670)	(0.00238)
Implied effect on change in aggregate unemployment rate from 2007 to 2010	1.611 (1.883)	0.532 (0.442)	0.212 (0.157)
90 percent confidence interval	-1.486 - 4.708	-0.195 - 1.260	-0.0467 - 0.471
First-stage F statistic	53.84	15.58	129.1
Linear Trend	Yes	Yes	Yes
R^2	0.882	0.705	0.708
No. of Obs.	546	533	612

Note. Standard errors clustered by state are in parentheses. All regressions include time fixed effects, group fixed effects, and group time trends. Observations are weighted by labor force.

To map these coefficients into estimates of the total effect of increased UI benefit durations on the unemployment rate over the course of the recent recession, TU, we multiply them by the difference in average benefit durations between 2007 and 2011, when average labor-force weighted benefit durations peaked at 92 weeks, $TU = 66\hat{\beta}$.

These estimates are shown in the second row, while the third row shows 90 percent confidence intervals. Figure 7 plots the estimates of the total effect of EEB on the unemployment rate together with 90 percent confidence intervals; the thick black lines represent the estimate of *TU*. At the right of the chart, we show two estimates of the average effect across the three policy changes. The first, labeled "Avg." displays the simple average. The second, labeled "Min. Var. Avg.", displays the weighted average with weights chosen to account for the different variances of the each of the estimates.[16] We construct a confidence interval for both of these averages assuming our estimates are independent of each other. Given that the time spans of the three estimates include only 1 overlapping month, this seems quite plausible.[17] The minimum variance weighted estimate is ¼ percentage point, while the 90 percent confidence interval for the minimum variance estimate ranges from close to 0 to 1/2 percentage point. This includes most of the estimates of the effect of EEB on the unemployment rate from recent studies, but is substantially smaller than the estimate in HKMM. The simple average estimate is larger, at 1.1 percentage points, reflecting the fact that estimates for the first two episodes are noticeably larger than the estimate from the third episode and the fact that the variance of the third estimate is considerably smaller than that for the others. While we prefer the minimum variance estimator as a way to aggregate the estimates, there may be circumstances in which it would be appropriate to give more weight to the first two estimates.

One such circumstance might be a situation in which extended benefits are expected to be very persistent. HKMM emphasize that expectations about the persistence of extended benefits may have an important influence on their effect on unemployment. In particular, if extended benefits are expected to last longer, then the effect on the current unemployment rate will be larger. To the extent that agents have perfect foresight, then the relative magnitudes of the estimated effects across the three episodes provide some support for this notion. In particular, estimated effects for the first two episodes, when agents may have expected extended benefits to be quite persistent, are noticeably larger than in the last episode, when agents may have expected the program to end relatively soon.

We can informally test this hypothesis by changing the time periods used to estimate the effect of the last rule change. The baseline results in table 3 and figure 7 compare the behavior of unemployment rates in the 5 months before the January 1, 2014 rule change to the 7 months after the month of the rule change. During the second half of 2013, agents may not have expected the

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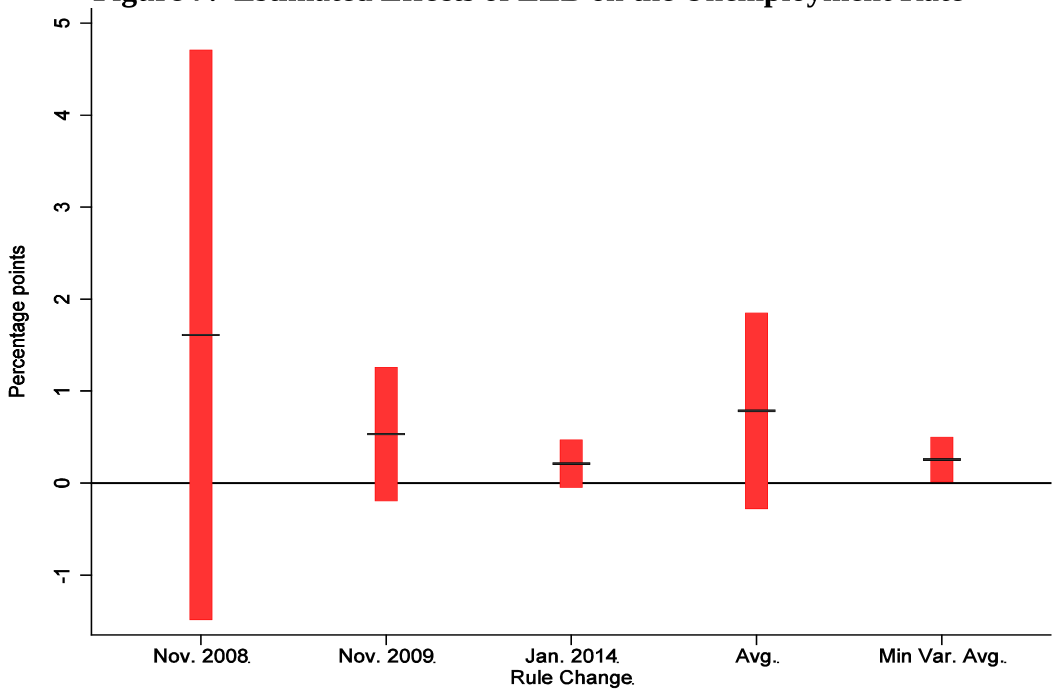
[16] Specifically, the weights are $\omega_i = {}^{1/var_i}\!\big/_{\sum_i 1/var_i}$, where *i* indexes the estimate from rule change *i*.

[17] In addition, the inclusion of time and unemployment-rate-group fixed effects should help to limit any serial correlation in errors across EEB policy-change episodes. The potential for serial correlation is strongest between the first and second policy-change episodes, as they are quite close in time. But, as the weights given the parameter estimates from these episodes by our minimum-variance estimator are already quite small, any increase in the variance of the parameter estimates from these episodes, which would further decrease the weights they receive, would have a minimal effect on the value and estimated confidence interval of our minimum-variance estimate.

program to run much longer. However, earlier on, say in the first half of 2013, agents likely expected extended benefits to persist longer (perfect foresight would suggest 6 months longer) than they expected in the second half of 2013. Thus, if expectations of future extended benefits are important, we should expect that an estimate based on the difference between the first half of 2013 and the first half of 2014 would be larger than an estimate based on the difference between the last half of 2013 and the first half of 2014. In fact, this is the case, with the former estimate about twice as large as the latter.

Figure 7: Estimated Effects of EEB on the Unemployment Rate



Labor Force Participation Rate

Next, we consider the effect of EEB on the labor force participation rate. A simple theoretical model would suggest that the effect of EEB on participation is positive as greater benefits provide an incentive for unemployed workers to remain in the labor force. Our empirical strategy is the same as with the unemployment rate: We estimate equations (3) and (4) with the participation rate substituted for the unemployment rate on the left-hand side of equation (4). Figures analogous to Figures 3-6 are shown in Appendix C. Table 4 and figure 8 present results. The minimum variance estimate is small and not significantly different, either statistically or economically, from 0. This suggests that EEB did not have a discernible effect on the labor force participation rate.

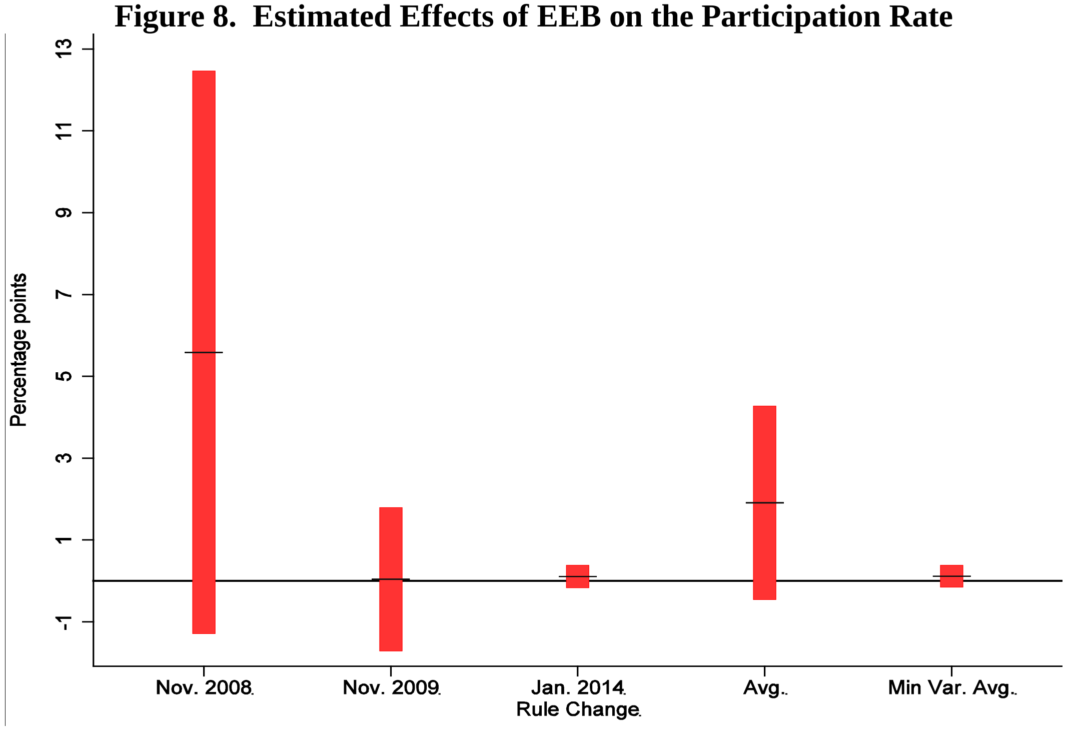
Table 4. Estimation Results for Effect of EEB on the Participation Rate

Table 4: Instrumental Variables Results

	November 2008	November 2009	January 2014
β	0.0846	0.000569	0.00161
	(0.0634)	(0.0161)	(0.00253)
Implied effect on change in aggregate unemployment rate from 2007 to 2010	5.584 (4.182)	0.0375 (1.065)	0.106 (0.167)
90 percent confidence interval	-1.294 - 12.46	-1.714 - 1.789	-0.168 - 0.380
First-stage F statistic	53.67	15.38	132.8
Linear Trend	Yes	Yes	Yes
R^2	0.168	0.238	0.260
No. of Obs.	546	533	612

Note. Standard errors clustered by state are in parentheses. All regressions include time fixed effects, group fixed effects, and group time trends. Observations are weighted by population. First-stage F-stats differ slightly from those in Table 3 since the LFPR regressions are weighted by population instead of labor force.

Figure 8. Estimated Effects of EEB on the Participation Rate



5. Conclusion

HKMM point out that estimates of the effects of EEB on unemployment can be biased downward if they fail to account for a complete response of labor demand to increases in EEB. In response to this insight, we use an estimation method that allows for a complete general equilibrium response. Critical to our ability to identify the effect of EEB is the existence of plausibly exogenous (to the state) enacted rule changes to the EEB program from 2008 to 2013. Our preferred estimate of the EEB effect using this methodology is 1/4 percentage point, suggesting that EEB had a nontrivial, but still relatively modest, effect on the unemployment rate during the Great Recession and its aftermath. However, our estimates of the first two episodes are noticeably larger, suggesting that in some circumstances, such as periods when extended benefits are expected to be quite persistent, the effect may be considerably larger. We also estimate little effect of EEB on the labor force participation rate.

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Appendix A

Table A. The relationship between benefits and unemployment rates under EUC

EUC Benefits	State trigger
June 30, 2008 – Nov. 20, 2008	
13 weeks	None
Nov. 20, 2008 – Nov. 5, 2009	
20 weeks (Tier 1)	None
33 weeks (Tier 2)	States where: • EB has triggered on • State insured unemployment rate over preceding 13 weeks is at least 4 percent, or • State 3 month moving average of seasonally adjusted unemployment rate is at least 6 percent
Nov. 6 2009 – June 1, 2012	
34 weeks (Tiers 1 and 2)	None
47 weeks (Tier 3)	State where: • State insured unemployment rate is at least 4 percent over preceding 13 weeks, or • State 3 month moving average of seasonally adjusted unemployment rate is at least 6 percent
53 weeks (Tier 4)	State where: • State is on extended benefits and state 13-week moving average of insured unemployment rate is at least 6 percent, or • State three month average of seasonally adjusted unemployment rate is at least 8.5 percent
March 1, 2012 – May 31, 2012 (Modification to Tier 4 benefits in states where EB has not triggered on)	
63 weeks (Tier 4)	State where extended benefits had not triggered on and: • State 13-week moving average of insured unemployment rate is at least 6 percent • State three month average of seasonally adjusted unemployment rate is at least 8.5 percent

Table A continued.

EUC Benefits	State trigger
June 1, 2012 – August 31, 2012	
20 weeks	None
34 weeks	State three-month moving average of seasonally adjusted unemployment rate is at least 6 percent
47 weeks	State three-month moving average of seasonally adjusted unemployment rate is at least 7 percent
53 weeks	State three-month moving average of seasonally adjusted unemployment rate is at least 9 percent
September 1, 2012 – December 31, 2013	
14 weeks	None
28 weeks	State three-month moving average of seasonally adjusted unemployment rate is at least 6 percent
37 weeks	State three-month moving average of seasonally adjusted unemployment rate is at least 7 percent
47 weeks	State three-month moving average of seasonally adjusted unemployment rate is at least 9 percent

Appendix B

As shown in Table A, the policy for the duration of UI benefits can be described as a common level of benefits across all states plus an additional increment that depends on a state's unemployment rate. The common level is likely a function of the aggregate unemployment rate, as well as other factors.

$$d_t^s = T_t\left(u_t\right) + \gamma_t\left(u_t^s\right) \tag{B1}$$

Because the policy function can change over time, it is indexed by *t*. When the policy function is constant over some interval of time Ω, it can be expressed as

$$d_\Omega^s = \tau + \sum_{i=1}^{N} \gamma^i I^i \tag{B2}$$

I is an indicator function equal to 1 if the unemployment rate in state *s* at time *t* is in interval *i*. Suppose, for analytical simplicity, that *N=2*, so that states with unemployment rates above some cutoff rate, $\bar{u}$, receive γ extra weeks.

$$\begin{aligned} d_t^1 &= \tau \\ d_t^2 &= \tau + \gamma \end{aligned} \tag{B3}$$

Given equation (2) in the main text, a state will be in the upper tier of benefits if economic conditions in the state are above some threshold defined by $\bar{\alpha} = \bar{u} - \beta(\tau + \gamma)$. (Here worse economic conditions correspond to higher values for $\alpha_{s,t}$.)

If we tried to estimate equation (2) without controlling for economic conditions, we would be estimating

$$\begin{aligned} u_t^s &= \beta d_t^s = \beta(\tau + \gamma I_t^s) \\ I_t^s &= (\alpha_t^s > \bar{\alpha}) \end{aligned} \tag{B4}$$

Given that actual unemployment behavior is described by equation (2), the estimated coefficient on the duration of benefits would be

$$\hat{\beta} = \beta + \frac{E\left(\gamma \widetilde{I_t^s} * \widetilde{\alpha_{st}}\right)}{E\left[\left(\gamma \widetilde{I_t^s}\right)^2\right]} \tag{B5}$$

Tilde superscripts indicate the variable is demeaned. Because the policy function is held fixed, γ is constant. Because I_t^s and α_t^s are positively correlated (from B4), $\hat{\beta}$ will be upward biased.

Suppose, instead, that we estimate the effect of EEB over a period in which the policy function changes. More specifically, suppose that *N=2* and that there is one change in the policy regime such that after period t_0, the policy is the same as that described above, but before t_0, states receive a common level of benefits

$$\begin{aligned} d_t^1 &= \tau^0 \\ d_t^2 &= \tau^0 \end{aligned} \qquad \text{(B6)}$$

Then it may be possible to use IV estimation to isolate changes in benefits that are not correlated with changes in economic conditions.

The requirements for an instrument are that it be correlated with the endogenous right-hand-side variable of interest, d_t^s, and uncorrelated with the left-hand-side dependent variable except through its effect on the endogenous right-hand-side variable.

Relevance
To see the conditions under which the first requirement is satisfied, note that we can write total benefits as

$$d_t^s = (1 - I_t^\tau)\tau^0 + I_t^\tau \tau + I_{t,s}^\gamma I_t^\tau \gamma \qquad \text{(B7)}$$

I_t^τ is an indicator variable equaling 1 if $t \geq t_0$ (and the new policy is in effect) and 0 otherwise. $I_{t,s}^\gamma$ is an indicator variable equal to 1 if the unemployment rate at time *t* in state *s* is above the unemployment threshold, $\bar{u}$.

Our instrument is $I_{t_0,s}^\gamma * I_t^\tau$.[18] $I_{t_0,s}^\gamma$ is equal to 1 if unemployment in state *s* at time t_0 is above $\bar{u}$. Because we will be controlling for I_t^τ and $I_{t_0,s}^\gamma$ in our estimating equation, we are interested in the components of $I_{t_0,s}^\gamma * I_t^\tau$ and d_t^s that are orthogonal to these variables.

$$\begin{aligned} \widetilde{d_t^s} &= d_t^s - \widehat{d_t^s} = d_t^s - \left(\widehat{\alpha_d} + \widehat{\beta_d^\tau} I_t^\tau + \widehat{\beta_d^\gamma} I_{t_0,s}^\gamma\right) \\ \widetilde{I_{t_0,s}^\gamma I_t^\tau} &= I_{t_0,s}^\gamma I_t^\tau - \widehat{I_{t_0,s}^\gamma I_t^\tau} = I_{t_0,s}^\gamma I_t^\tau - \left(\widehat{\alpha_\iota} + \widehat{\beta_\iota^\tau} I_t^\tau + \widehat{\beta_\iota^\gamma} I_{t_0,s}^\gamma\right) \end{aligned} \qquad \text{(B8)}$$

Thus, for our instrument to be relevant, we need $Cov\left(\widetilde{I_{t_0,s}^\gamma I_t^\tau}, \widetilde{d_t^s}\right) > 0$. It is easy to compute this correlation if we first spit the sample space into four quadrants, as shown in the table below.

[18] In the text, this corresponds to $I_s^H I_t^r$.

Table B1. Four quadrants of the sample space

	Before t_0	After t_0
$u < \bar{u}$ at t_0	1	2
$u \geq \bar{u}$ at t_0	3	4

In each quadrant, the instrument is constant. Thus,

$$Cov\left(\widetilde{I^{\gamma}_{t_0,s} I^{\tau}_{t}}, \widetilde{d^s_t}\right) = \sum_{i=1}^{4} \frac{N_i}{N} \overline{I^{\iota}} E_i\, \widetilde{d^s_t}$$

where i indexes the quadrant in the Table B1, $\overline{I^{\iota}}$ is the constant value of our orthogonalized instrument in quadrant i, and N_i is the size of the sample in quadrant i. These constant values are shown in table B2 below.

Table B2. Values of orthogonalized instrument in the four quadrants of the sample space

	Before t_0	After t_0
$u < \bar{u}$ at t_0	$t^{sh}u^{sh}$	$-u^{sh}(1 - t^{sh})$
$u \geq \bar{u}$ at t_0	-$t^{sh}(1 - u^{sh})$	$1 + t^{sh}u^{sh} - u^{sh} - t^{sh}$

t^{sh} is the share of the sample space before date t_0, and u^{sh} is the share of the sample space where $u > \bar{u}$ at t_0.

The values of $\frac{N_i}{N}$ in each of the four quadrants is shown in the table below

Table B3. Share of sample space in the four quadrants

	Before t_0	After t_0
$u < \bar{u}$ at t_0	$(1 - t^{sh})(1 - u^{sh})$	$t^{sh}(1 - u^{sh})$
$u \geq \bar{u}$ at t_0	$u^{sh}(1 - t^{sh})$	$t^{sh}u^{sh}$

As a result,

$$Cov\left(\widetilde{I^{\gamma}_{t_0,s} I^{\tau}_{t}}, \widetilde{d^s_t}\right) =$$
$$\sum_{i=1}^{4} \frac{N_i}{N} \overline{I^{\iota}} E_i\, \widetilde{d^s_t} = (1 - t^{sh})(1 - u^{sh})t^{sh}u^{sh}\left(E_1\widetilde{d^s_t} + E_4\widetilde{d^s_t} - E_2\widetilde{d^s_t} - E_3\widetilde{d^s_t}\right) \qquad \text{(B9)}$$

Next, we need to compute the value of the orthogonalized benefit duration level, $\widetilde{d^s_t}$, in each of the four quadrants. These are shown in Table B4 below.

Table B4. Values of orthogonalized benefit durations

	Before t_0	After t_0
$u < \bar{u}$ at t_0	$\tau^0 - \widehat{\alpha_d}$	$\tau + I_{t,s}^{\gamma}\gamma - \widehat{\alpha_d} - \widehat{\beta_d^{\tau}}$
$u \geq \bar{u}$ at t_0	$\tau^0 - \widehat{\alpha_d} - \widehat{\beta_d^{\gamma}}$	$\tau + I_{t,s}^{\gamma}\gamma - \widehat{\alpha_d} - \widehat{\beta_d^{\tau}} - \widehat{\beta_d^{\gamma}}$

It follows that

$$Cov\left(\widetilde{I_{t_0,s}^{\gamma} I_t^{\tau}}, \widetilde{d_t^s}\right) = (1 - t^{sh})(1 - u^{sh})t^{sh}u^{sh}\left(\gamma^A\left(E_4\left(I_{t,s}^{\gamma}\right) - E_2\left(I_{t,s}^{\gamma}\right)\right)\right)$$

Thus, for our instrument to be relevant, it must be the case that enhanced benefits (γ) are more likely to be available after time t_0 in states where $u \geq \bar{u}$ at t_0 than in states where $u < \bar{u}$ at t_0. In other words, the stronger the correlation between a state's eligibility for enhanced benefits at time t_0 and its eligibility for benefits after t_0, the more relevant our instrument.

In the context of the current model, the events around our third episode of a change in benefits policy can be thought of as follows. A subset of states, *A*, satisfied the unemployment threshold condition at t_0, and thus, all else equal should be more likely to receive enhanced benefits after t_0. However, with the economy improving, all states experienced declines in unemployment following t_0. As a result, many of the states in *A* passed below the threshold level of unemployment after t_0 and were no longer eligible for enhanced benefits. This led to a reduction in the value of $\left(\gamma^A\left(E_4\left(I_{t,s}^{\gamma}\right) - E_2\left(I_{t,s}^{\gamma}\right)\right)\right)$ relative to other episodes.

Uncorrelated with economic conditions

The second condition requires that our instrument is not correlated with α_t^s after conditioning on the other right hand side variables. Thus, we require $Cov\left(\widetilde{I_{t_0,s}^{\gamma} I_t^{\tau}}, \widetilde{\alpha_t^s}\right) = 0$. To understand the circumstances in which this is true, it is again helpful to rewrite the above covariance as

$$Cov\left(\widetilde{I_{t_0,s}^{\gamma} I_t^{\tau}}, \widetilde{\alpha_t^s}\right) = \sum_{i=1}^{4} \frac{N_i}{N} \overline{I^{\iota}} E_i\, \widetilde{\alpha_t^s}$$

From equation (B9), we can express this as

$$(1 - t^{sh})(1 - u^{sh})t^{sh}u^{sh}\left(\left(E_4\widetilde{\alpha_t^s} - E_3\widetilde{\alpha_t^s}\right) - \left(E_2\widetilde{\alpha_t^s} - E_4\widetilde{\alpha_t^s}\right)\right)$$

Thus, for our instrument to be uncorrelated with economic conditions, it must be that around the policy change, the difference in difference in economic conditions equals zero. This justifies the identification condition stated in the text.

Appendix C

Figure C1: November 2008 Rule Change
(Differences in participation rates across high and low unemployment rate groups)

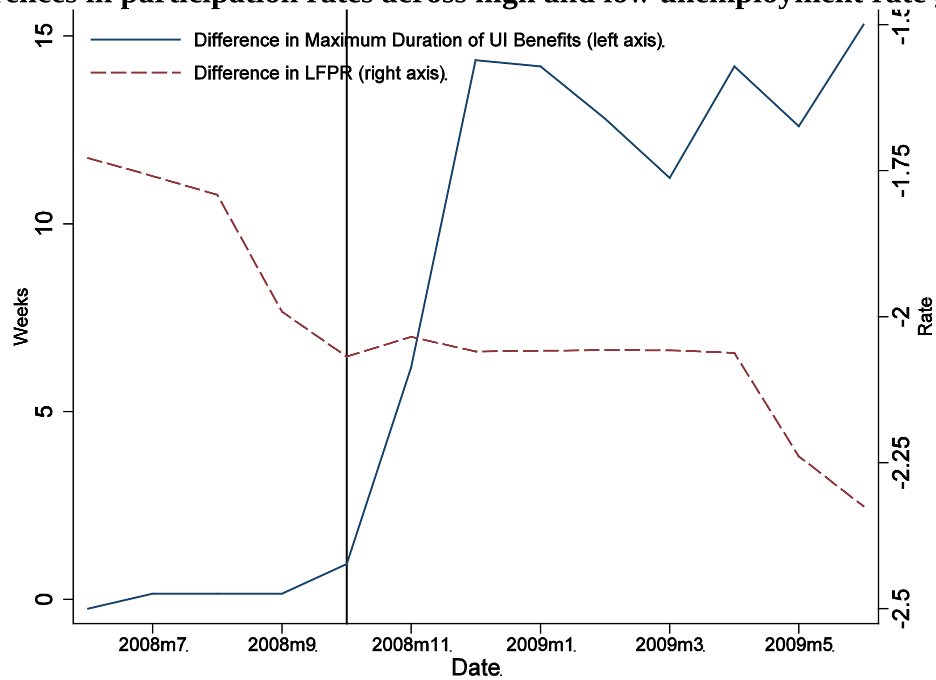

Figure C2: November 2009 Rule Change
(Differences in participation rates across high and low unemployment rate groups)

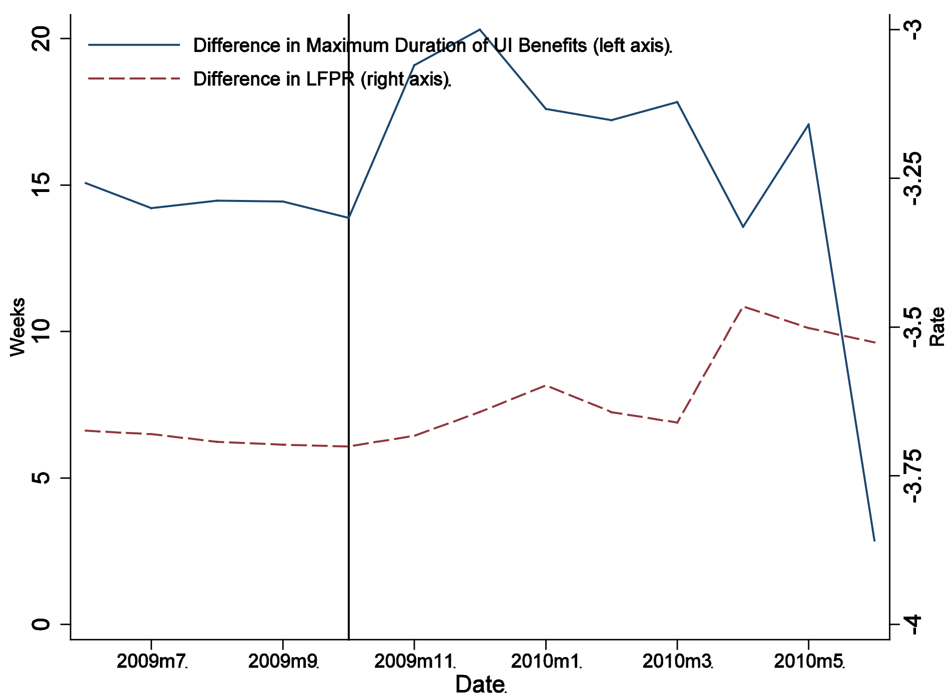

Figure C3: January 2014 Rule Change