Unemployment Insurance Generosity and Aggregate Employment

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Online Appendix

A Online Appendix A: EB and EUC programs

Extended Benefits (EB)

Historically, when not in recession, most U.S. states have provided a maximum of 26 weeks of unemployment insurance to job-losers. At the onset of the Great Recession, in 2008, only two states offered more than 26 weeks of regular benefits. Massachusetts had a maximum of 30 weeks of UI benefits and Montana had a maximum of 28 weeks and no states offered less than 26 weeks.[52]

Since Congress created the Extended Benefits (EB) program in 1970, maximum benefit lengths increase automatically when unemployment is high and growing. At a minimum, in states where the Insured Unemployment Rate (IUR) exceeds 5%, and the IUR is at least 1.2 times the IUR in the previous two years, claimants are eligible for 13 additional weeks of UI after the expiration of regular benefits.[53] The same law also provides two optional "triggers," which can be adopted by states at their own discretion. The first trigger provides for 13 weeks of EB for states whose IUR exceeds 6% (regardless of the change in the IUR over time). The other optional trigger is based on the Total Unemployment Rate (TUR): the trigger provides for 13 weeks of EB when both (1) the TUR exceeds 6.5% and (2) the current TUR is at least 1.1 times its value in the prior two years. States adopting this second trigger must provide 20 weeks of EB when (1) the TUR exceeds 8%, subject to the same growth-over-time requirement.[54] States can adopt zero, one, or both optional triggers, but no more than one trigger can be "on" at any point in time, meaning that the number of weeks of EB is capped at 20.

Normally, the costs of EB are shared equally between the federal and state governments. As a result, many states did not have statutes activating the optional EB triggers at the onset of the Great Recession.

[52] Not all claimants are eligible for the maximum number of weeks of benefits. In most states, individuals with relatively weak recent labor force attachment are eligible only for a fraction of the maximum weeks of benefits. Throughout this paper, we abstract from this complication by focusing on the maximum UI duration. Our estimates, therefore, can be seen as an intention to treat effect. Johnston and Mas (2018), using micro-data from Missouri, find that approximately 70% of UI claimants had sufficient labor force attachment to be eligible for the full 26 weeks of regular benefits from 2003-2013.

[53] The Insured Unemployment Rate (IUR) is, roughly, the ratio of current regular UI claimants to the number of UI-covered jobs. The Total Unemployment Rate (TUR) is the usual "unemployment rate": i.e., the ratio of unemployed persons to persons in the labor force.

[54] From December 2010 through the end of 2013 (a period in which the unemployment rate remained high but was generally not growing), states were allowed to apply a three-year lookback period instead of a two-year lookback period for the purpose of determining growth over time.

After the passage of the American Recovery and Reinvestment Act (ARRA), the federal government paid for the full amount of EB extensions. Some states (mostly deeply conservative ones) nonetheless declined to activate the optional triggers. For example, while Mississippi had a TUR of well over 8% continuously from January 2009 through October 2016, peaking at over 11% in 2010, they were never eligible for EB because the IUR never went above 5.6% and the state declined to enact the optional triggers. Thus, different states had different numbers of weeks of EB in part due to differences in the state unemployment rates and in part due to state policy differences. The federal government maintained its full support of EB until the end of 2013 when it returned to the default equal cost sharing rule.

Emergency Unemployment Compensation (EUC)

In response to the first signs of a weakening labor market, on June 30, 2008, Congress and President Bush created the Emergency Unemployment Compensation (EUC) program. At first, EUC provided for 13 additional weeks of benefits for all UI-eligible unemployed workers.[55] The Unemployment Compensation Extension Act of 2008 was then signed into law by President Bush on November 21, 2008. It augmented the EUC program while also creating the first differences across states in their access to the EUC extensions. It authorized 20 weeks of EUC for all states (an increase from 13) and an additional 13 weeks for those with a total unemployment rate exceeding 6%.[56] These additional weeks were organized into "tiers": Tier 1 corresponded to the first 20 weeks of EUC, while Tier 2 corresponded to the baseline 20 weeks plus an additional 13 weeks. During this period, a state with 26 weeks of regular benefits could qualify for up to 79 weeks total of benefits. Then, on November 6, 2009, the Worker, Homeowner, and Business Act of 2009 further increased maximum UI duration. Tier 1 remained in place. However, Tier 2 was increased from 13 to 14 weeks and extended to all 50 states. The law also added Tier 3, providing 13 additional weeks to states with a TUR of greater than 6%, and Tier 4, providing 6 additional weeks for states with a TUR of greater than 8.5%. After the passage of this law, states had access to a maximum of 99 weeks of benefits. This schedule remained in place, with the exception of temporary lapses, until early 2012, when Congress enacted laws that slowly began to phase out EUC.[57]

[55] To be more precise, this legislation—and all subsequent legislation related to EUC—provided for increases in benefit lengths equal to the lesser of (1) a specified number of weeks or (2) a fraction of the number of weeks of regular benefits. For the initial legislation in June 2008, the specified number of weeks was 13 and the fraction of the number of weeks of regular benefits was 50%. For the vast majority of states that had regular benefits greater than or equal to 26, the specified number of weeks was the binding factor. For those states with fewer than 26 weeks of regular benefits, the percentage of regular benefits was always binding. In this paper, we code the weeks available under EUC exactly as specified in the law; however, in the discussion that follows, we discuss only the specified number of weeks, which applies to states with at least 26 weeks of regular benefits.

[56] A state could also have become eligible for 33 weeks with a sufficiently high IUR; in practice, the IUR trigger was never binding.

[57] There were four lapses in EUC that occurred in 2010, arising due to political disagreements regarding the extension of the program. The longest such lapse lasted from May 30, 2010 to July 18, 2010. In each of the lapses, beneficiaries were paid

On February 22, 2012, Congress passed and the President signed The Middle Class Tax Relief and Job Creation Act of 2012 which slightly lowered the generosity of the EUC in a gradual way, first starting on May 27, 2012, and then again on September 2, 2012. By September 2, 2012, Tier 1 had been scaled back to 14 weeks and was still available to all states. Tier 2 remained at 14 weeks but again became available only to states with a TUR of greater than 6%. Tier 3 was scaled back from 13 to 9 weeks and the state TUR threshold was raised to 7%. Finally, Tier 4 was increased to provide 10 extra weeks for states with a TUR of above 9%. The program finally came to an end at the end of December 2013.[58] In total, over the Great Recession, individuals in qualifying states received up to 99 weeks of unemployment insurance. Compared to the baseline of 26 weeks, this is an increase of 73 weeks; so the maximum UI benefit duration in some qualifying states increased by almost 300%.

retroactively for any weeks of missed payments. Furthermore, during these lapses, the funding rules for EB reverted to their pre-ARRA levels, which led many states to suspend EB payments during these lapses as well.

[58]Upon the expiration of EUC at the end of 2013, EUC beneficiaries immediately stopped receiving benefit payments. Prior to the final expiration, however, the phase-out was more gradual. If a state "triggered-off" a certain tier, people who had already qualified for a given tier were allowed to finish that tier but were not allowed to move to the next tier. One exception, discussed in the main text, is North Carolina, which lost access to all EUC money as of July 1, 2013. In our econometric specifications, our duration variable is the maximum duration available in a given month for a new entrant into unemployment. Thus, we do not distinguish between gradual phase-outs and sudden benefit cessations.

B Online Appendix B: Comparison with HKMM and HMM

The results in this paper are quite different than the results in Marcus Hagedorn, Fatih Karahan, Iourii Manovskii and Kurt Mitman (2015) (which studies the effect of UI from 2005 to 2012) and the results in Marcus Hagedorn, Iourii Manovskii and Kurt Mitman (2016) (which studies the effect of EUC expiration at the end of 2013). Similar to this paper, both HKMM and HMM use border county pairs for their estimation. There are differences in data, in econometric specification, and in sample definitions between our paper and these two studies. Some differences are minor, while others are quite important. In this online Appendix, we expand upon the discussion in the main text to offer additional details regarding differences between our methods and results and those of HKMM and HMM.

Comparison to HKMM

In this section, we compare our full sample estimates from the baseline BCP sample to the baseline estimates of HKMM. The HKMM estimation equation is as follows, where data for a given pair p at time t has already been spatially differenced (after taking logs):

$$\ln(u_{pt}) - \beta(1 - s_t)\ln(u_{pt+1}) = \alpha * \ln(D_{pt}) + \lambda_p' F_t + \epsilon_{pt} \tag{B1}$$

Here, u_{pt} is the unemployment rate from LAUS,[59] β is the discount factor (equal to 0.99), s_t is the separation rate, D_{pt} is the same measure of maximum benefit lengths that we use, and $\lambda_p' F_t$ are interactive effects. Thus, the dependent variable is a quasi-forward difference (QFD) of the log of the unemployment rate. They then calculate the total effect of UI on unemployment by considering the steady state ($u_{pt} = u_{pt+1}$) impact of a persistent increase in D_{pt}. In the steady state, $\ln(u_p) = \frac{\alpha}{1-\beta(1-s)} \ln(D_p)$. Therefore, HKMM's headline claim comes from multiplying their main estimate by a factor $\frac{1}{1-\beta(1-s)}$, which is approximately equal to 10. They perform their estimation over the period 2005q1-2012q4.

Our full sample BCP-FE estimation strategy is different from HKMM in six distinct ways. These differences are: (1.) we do not transform our dependent variable using quasi-forward-differencing, (2.) we use employment data from the QCEW rather than unemployment data from LAUS, (3.) we estimate the results using monthly data from 2007m11-2014m12, instead of quarterly data from 2005q1-2012q4, (4.) we control for differences across county pairs using a fixed effects model rather than the Bai (2009) interactive fixed

[59]The LAUS data used by HKMM has been substantially revised since they accessed it. We have estimated the models using both the pre-revision version of the LAUS data used by HKMM and the more recent, revised version of the data. We have found both versions of the data give similar results in the HKMM specifications. We use pre-revision data throughout the discussion of HKMM.

effects model, (5.) we use levels instead of logs, and (6.) we restrict ourselves to a balanced panel, throwing out 10 small counties which did not report county-level employment at least once during our sample period due to disclosure issues.

Appendix Table B1 describes the impact of each of these six steps. Because different specifications have different dependent variables, and because the implied effect is not equal to the coefficient in some specifications, we standardize each specification into an implied effect of the 26-to-99 week expansion on EPOP.[60] We "translate" between implied effects on the unemployment rate and implied effects on EPOP by using the total peak-to-trough impact of the Great Recession. We measure this peak-to-trough impact using the unweighted average of counties in our border-pair sample. In particular, in this sample, EPOP fell from 44.3% to 41.2% and the unemployment rate increased from 4.8% to 9.7%. So, if one estimation suggests that the impact of the 26-to-99 week expansion was 3 percentage points of unemployment, we would convert that specification's estimate into an EPOP effect of $3 \times (\frac{41.2-44.3}{9.7-4.8}) \approx -1.9$ percentage points.

Appendix Table B1 analyzes one-off changes either starting from the HKMM specification (column 1), or moving to our specification (column 2). The first row begins with reporting the estimates: our replication of the HKMM estimates, joint with Dieterle, Bartalotti and Brummet (2020) and discussed in Online Appendix D, suggest that the UI benefit expansion from 26 to 99 weeks has an implied EPOP effect of -2.66, which more than 85% of the decrease in EPOP during the Great Recession within our sample. This corresponds to a coefficient estimate of 0.051, while HKMM report a very similar estimate of 0.049. We find that this estimate is statistically significant, as HKMM do. In contrast, the point estimates for the full sample BCP-FE estimates in this paper suggest that the decline in EPOP would have been about 10% *greater* without the UI expansions, though this is not distinguishable from zero.

The next five rows report the marginal impact of each of the five steps. In column 1, we show what happens when the step reported in the row is added starting with the HKMM specification. In column 2, we show what happens when this step is added to our specification. Finally, in column 3, we consider *all* possible transition paths between HKMM's estimates and our estimates, and report the average marginal contribution of each of the steps, across all of these transition paths.[61]

The key findings are as follows. Quasi-forward differencing, the use of the LAUS unemployment data as opposed to the QCEW employment data, and sample alignment are all consequential choices. In contrast,

[60] Importantly, we scale up the estimates in QFD specifications by $1/[1-\beta(1-s)]$, as HKMM do.

[61] We do not consider the step of switching from logs to levels in column 1, because the quasi-forward-differencing is motivated by theory which requires the data to be in logs. With quasi-forward-differenced data in levels, it is neither clear what we are measuring, nor what the total effect of UI on employment would be. For the same reason, we do not consider adding quasi-forward-differencing to our specification in column 2 (which is in levels). In addition, when calculating the averages in column 3, we discard transition paths that involve using quasi-forward-differenced data in levels. In the end, we estimate 48 models with all allowable combinations of the five sources of differences; we then take 360 paths (equal to 6! paths with 1/2 thrown out because eliminating quasi-forward differencing happens after the logs to levels conversion) between the HKMM and BDGK estimates, and calculate the contribution of each of these six factors averaged across these 360 paths.

the use of interactive fixed effects as opposed to linear fixed effects, the use of logs versus levels, and the use of a balanced as opposed to unbalanced panel of counties are not consequential choices.

Column 1 shows that, starting from the HKMM estimate, switching from the LAUS unemployment rate, or getting rid of quasi-forward differencing, dramatically reduces the HKMM estimates in magnitude towards zero. In particular, just switching from the LAUS unemployment rate to the QCEW EPOP (as shown in Row 4) changes the estimates to $-2.661 + 1.120 = -1.541$, suggesting the UI benefit expansion explained around 50% of the fall in EPOP rather than 85% as implied by HKMM's estimates. Similarly, removing quasi-forward differencing (Row 2) changes the estimates to $-2.661 + 2.618 = -0.043$ percentage points of EPOP. Column 2 shows that use of the LAUS unemployment rate also leads to a (mistaken) suggestion of job loss when we start from our specification, although the impact of this is more modest. Starting from our BCP-FE specification, when we use the LAUS unemployment rate as the outcome, the translated result suggests the UI benefit expansion led to a change in EPOP equal to $0.430 - 1.133 = -0.703$, just under a quarter of the overall change during the Great Recession. When we average the incremental contribution of these two steps across all permissible paths going between the HKMM specification and ours (in column 3), we find that dropping quasi-forward differencing increases the estimates by around 1.30 percentage points of EPOP (about 40% of the change in unemployment rate during the Great Recession), while switching the outcome from LAUS unemployment rate to QCEW based EPOP increases the estimate by about 0.67 percentage points of EPOP.

Aligning our sample period also has a meaningful impact. The HKMM sample of 2005q1-2012q4 starts and ends earlier than our sample of 2007m11-2014m12. Averaged across all sample paths, moving from HKMM's sample to ours adds 0.91 percentage points of EPOP to the estimate. As we showed in **Table 3**, while the baseline BCP-FE approach greatly reduces the pre-existing trend, it does not completely remove it. Use of an earlier start date, as well as an end date prior to the phase-out of differential UI benefits across state borders, can produce a more negative estimate in the presence of such trends. We find that use of this altered sample period leads to somewhat smaller magnitudes of estimates, reducing the impact of the policy by around 0.817, 1.572, and 0.915 percentage points of EPOP in columns 1, 2, and 3, respectively.

In contrast, the use of Bai (2009) interactive effects versus fixed effects, the use of logs versus levels, and the use of a balanced panel make fairly small contributions in explaining the difference between our two sets of estimates.

This analysis shows that (1) changing the sample period (and frequency) from HKMM's specification to ours, (2) eliminating quasi-forward-differencing, and (3) changing the dependent variable from the LAUS unemployment rate to QCEW EPOP all reduce the implied negative impact of UI on employment, by 0.67 to 1.30 percentage points of EPOP when averaged over all possible paths. We next discuss our justification for making the specification choices that we do.

Quasi-Forward Differencing

HKMM derive **Equation (B1)** by considering a search-and-matching framework where the rate of vacancy posting or firm job creation depends on a firm's expectation about future wages. Since unemployment insurance puts upward pressure on wages, an increase in benefits would reduce the expected profits of the firm and lead to a reduction in job creation. Because expectations about *future* benefit changes can affect employment *today*, HKMM make the point that an empirical approach that only relates current employment to current or past policy changes would be misspecified. In order to capture these anticipation effects, HKMM use a quasi-forward-differencing procedure. Their argument is as follows: the value of an employee to an employer is equal to the current-period flow profits, plus $\beta(1-s)$ times the expected value of the employee tomorrow (since the value of a vacant job is driven to zero by free entry). Therefore, HKMM argue, we can isolate the impact of UI on current-period flow profits by considering the quasi-forward difference of the unemployment rate (which they consider to be proportional to current period flow profits, in logs). The theory predicts that, in the case of an increase in generosity that was a surprise and immediately known to be persistent, firms would move from a low-unemployment steady state to a high-unemployment steady state, according to the equation $\Delta \ln(u_p) = \frac{\alpha}{1-\beta(1-s)} \Delta \ln(D_p)$.[62] As we noted above, this choice is quite important—removing forward differencing essentially erases the entirety of their effect even in their sample.

We are generally less favorable toward the use of quasi-forward differencing for several reasons. This model-driven approach relies on strong parametric assumptions—most notably that labor demand is well-characterized by the vacancy-posting problem captured in the model. Unfortunately this results in an empirical approach that is very sensitive to misspecification. For example, if an increase in UI generosity (D_{pt}) tends to be associated with a decrease in future unemployment (u_{pt+1}) in the data, then the estimated coefficient α will be positive. Such a pattern could also be consistent with a Keynesian aggregate demand effect that operates with a small delay. That is, if an increase in benefits in one period leads to increased aggregate demand and lower unemployment in the next period, the HKMM strategy would find that UI *increased* the unemployment rate, when in fact the opposite occurred. This problem is illustrated in **Appendix Figure B1**. Second, as a practical matter, the size of the final estimate is sensitive to assumptions in the model required for translating a flow result to a steady state effect, and in the exact magnitudes of separation and discount rates. Both the heavy dependence on a specific model and the inability to distin-

[62] Here α is the regression coefficient, β is the discount factor, s is the probability that the job ends, u is the unemployment rate, and D is the number of weeks of UI benefits.

guish between alternative explanations make quasi-forward differencing an unattractive strategy from our perspective.

Instead, our preferred strategy is to capture the dynamics in a less model-driven and a more transparent manner using distributed lags. That specification directly estimates employment changes around benefit duration innovations, allowing us to assess possible pre-existing trends, anticipatory effects, and delayed or slow moving response within the window. As we discussed in Section IV.A, we find no evidence of significant anticipation effects in the 12 months prior to benefit changes. The lack of any anticipation effect raises questions about the value of quasi-forward differencing the outcome, especially given the drawbacks discussed above.

LAUS versus QCEW

HKMM predominantly use the LAUS employment data rather than the QCEW employment data to compute county-level measures of employment.[63] Importantly, the LAUS data is partly model-based. In particular, while the LAUS data uses actual movement to unemployment based upon UI claims, they do not observe those entering (or re-entering) the labor force. Therefore, the county-level estimates for unemployment are based on state-level data on labor force entry and re-entry—something BLS states explicitly in their online manual (`http://www.bls.gov/lau/laumthd.htm`):

> “The second category, "new entrants and reentrants into the labor force," cannot be estimated directly from UI statistics, because unemployment for these persons is not immediately preceded by the period of employment required to receive UI benefits. In addition, there is no uniform source of new entrants and reentrants data for States available at the LMA [labor market area] level; the only existing source available is from the CPS at the State level. Separate estimates for new entrants and for reentrants are derived from econometric models based on current and historical state entrants data from the CPS. These model estimates are then allocated to all Labor Market Areas (LMAs) based on the age population distribution of each LMA. For new entrants, the area’s proportion of 16-19 years population group to the State total of 16-19 years old population is used, and for reentrants, the handbook area’s proportion of 20 years and older

[63]They do report results using the log employment from the QCEW and QWI as a robustness check, in columns 3 and 4 of Table 5. The log employment result, -0.03, would imply that the 26-99 week expansion of UI caused a reduction of employment by 3.9%, which would translate to about 1.6 percentage points of EPOP. This is about 40% less than implied EPOP effect of HKMM’s main result, consistent with the average marginal effects reported in **Appendix Table B1**. The log employment results from the QWI are modestly larger.

population to the State total of 20 years and older population is used."

The use of state-level information in estimating county-level unemployment rates is problematic for a border discontinuity design. The border county design attempts to purge reverse causation present at the state level by using more local comparisons. Use of state-level information raises the possibility of finding a (spurious) discontinuity in the measured unemployment rate across the state borders even when there is no such discontinuity in reality.

The QCEW data are based on administrative payroll records provided to the BLS by states, which protects against finding spurious discontinuities. Moreover, the QCEW data includes around 98% of all formal sector workers, making them very close to the true total employment counts in these counties. For these reasons, we consider the QCEW to be the preferred data source for county-level employment. When the results using the QCEW and LAUS data differ non-trivially—which they do in this case—the QCEW findings are much more likely to be accurate.

Sample Alignment

HKMM's sample goes from 2005 through 2012 and uses quarterly data. By contrast, our main specification uses monthly data, starts in 2007m11, and goes through 2014m12. Using quarterly versus monthly data has virtually no impact. For our preferred specification, for example, changing to quarterly data increases the standard errors by a little more than 0.04 and increases the the mean estimate by 0.02 (see **Table 4**). Though that represents a 7% increase, since the baseline estimates are small to start with, the impact is quite small. Switching the time period of estimation from 2005-2012 to 2007m11-2014m12 does make a difference. First of all, as we discussed in **Section IV.C**, the 2007m11-2014m12 sample exhibits a fairly symmetric rise and then fall in treatment intensity, orthogonalizing possible trends. Moving to the 2005-2012 sample makes this less so. As can be seen in **Figure 4**, the 2005-2012 period is largely a period of (1) increasing benefit duration and and (2) decreasing relative employment on the high-treatment side of the border. After 2012, the high-treatment side of the border starts to experience a relative decline in duration, while continuing its relative decline in employment. This is in part due to federal policy changes and in part due to differential changes in unemployment levels. Thus, it is not surprising that adding 2013 and 2014, and removing 2005 to 2007m10, has a noticeable positive impact on the UI duration impact upon employment.

Furthermore, we note that the choice of sample date matters little for the PT-trimmed sample. **Table 3** shows that the estimates in the full sample BCP-FE specification fall from 0.430 to -0.330 when the sample is changed from 2007m11-2014m12 to 2004m11-2014m12, while the estimates in the PT-trimmed sample fall only from 0.18 to -0.06. The IV estimates show a similar pattern, although the range is larger in both samples. This leads us to be confident that the large negative effects seen in full sample specifications with earlier start dates (and/or end dates) reflect endogeneity from pre-existing trends. Furthermore, since the 2007m11-2014m12 sample window effectively orthogonalizes these trends with treatment, we believe that our sample window provides for more reliable estimates than other sample windows, including HKMM's 2005q1-2012q4.

HMM comparison

HMM find that the expiration of EUC at the end of 2013 increased employment, though the implied effect of UI generosity is smaller than that of HKMM. Whereas the latter suggests that approximately 80% of the increase in unemployment during the Great Recession can be explained by the increase in benefit generosity, applying the coefficient estimates of HMM to the 26-to-99 week expansion would imply that UI policy can explain about one third. Scaled another way, HMM finds that the employment effect of the *expiration* is on the same order as total employment gains during 2014. HMM estimate a variety of different empirical models, all of which are motivated by a desire to exploit variation in UI benefits solely coming from the EUC expiration, while at the same time incorporating information over a longer period to formulate a counterfactual for the county-level employment which would have occurred had EUC not expired. Broadly, these specifications can be broken into two groups, which we call the "interaction term" models and the "event study" models.[64] We discuss each of them in turn.

The following is equivalent to HMM's "benchmark" interaction term model, where e_{ct} is log employment, measured either in the QCEW or LAUS:[65]

$$e_{ct} = \kappa[\ln(D_{ct})\mathbb{1}(t \leq 2013q3)] + \alpha[\ln(D_{ct})\mathbb{1}(t \geq 2013q4)] + \mu_c + \nu_{pt} + \gamma_c t + u_{cpt} \tag{B2}$$

That is, the model includes pair-period fixed effects, county fixed effects, as well as a county-specific time trend. The coefficient of interest is α, which measures the effect of duration on employment solely using variation from 2013q4 onward (i.e., from no earlier than the quarter immediately prior to expiration). The other independent variable, the log of benefit duration in periods prior to 2013q4, soaks up the effect of duration up to 2013q3; this ensures that, after taking out county fixed effects and county-specific linear trends, the model is comparing employment differences in 2013q4 to employment differences in all quarters

[64] The former correspond to models discussed in Sections 3 through 5 of HMM and the latter correspond to models discussed in Section 6 of HMM.

[65] We understand that HMM takes the spatial difference across pairs manually; as discussed above, this is equivalent to including a full set of pair-period fixed effects.

in 2014.

The first column of the top panel of **Table B3** shows HMM's estimate of this specification over the 2010q1-2014q4 period, as well as our replication. They estimate a coefficient of -0.0190, with a p-value of zero (to three decimal places) from a block bootstrap procedure. To place this in the context of our other estimates, this would translate into a -1.05 percentage point reduction in EPOP from a 26-to-99 week expansion of duration. While this is smaller than the corresponding estimate in HKMM, it is still substantial, representing about one third of the EPOP drop of the Great Recession; it would also imply that the expiration of EUC was responsible for increasing employment in 2014 by about 2 million jobs. When we estimate this equation using the LAUS data that they use on the county pairs in our sample, we estimate a very similar coefficient of -0.0200, with an analytical standard error (clustered at the state-pair level) of 0.0082,[66] which implies a p-value of about 0.015.[67] Since HMM accessed their data, the entire LAUS series has been redesigned by the BLS, largely to incorporate information from the American Community Survey rather than the Decennial Census.[68] The second column of the first panel shows our estimate from the same specification but with employment derived from the revised data. The coefficient falls in magnitude by three quarters to -0.0048 and becomes statistically indistinguishable from zero. Thus, when using the most recent version of the LAUS employment series, this specification no longer finds that the 2014 EUC expiration caused an employment boom.

HMM also estimate this model using log employment derived from the QCEW and find a modestly negative estimate of -0.0100. In our scale, this would translate to an EPOP effect of -0.558 percentage points from a 26-to-99 week expansion. When we estimate their model we obtain a similar coefficient of -0.0078, corresponding to an EPOP effect of -0.435.[69] While -0.558 is more negative than our 2014 IV specification (-0.024 in the full BCP-FE sample, or -0.214 in the PT-trimmed sample), the difference is at the bottom end of the range of estimates that can be generated using QCEW data from robustness checks on our main specifications. In results available upon request, we re-estimate our baseline 2014 BCP-FE IV specification using all combinations of the following specification choices: (1) using EPOP, log EPOP, or log employment as the dependent variable,[70] (2) using duration in logs or in levels as the independent variable of interest,

[66] In our baseline specifications, we cluster two-way at the state and state-pair level in order to account for any common state-level shocks (including mechanical correlation of errors for those counties that border multiple states). For the sake of this reconciliation exercise, we cluster at the state-pair level. Clustering at the two-way level in this specification increases the standard error to 0.0097.

[67] Our baseline sample includes 1,161 county pairs, and we drop an additional two pairs due to missing data in this specification. While our baseline specification studying the 2014 EUC expiration drops pairs in which either county is in North Carolina, we do not drop such pairs in this reconciliation exercise. HMM report using 1,175 pairs with full data. Such a discrepancy could arise due to reasonable differences in interpretation regarding, e.g., whether counties that touch only on a corner should be included as a "county pair."

[68] See http://www.bls.gov/lau/lauschanges2015.htm for details. We downloaded the current LAUS data on November 10, 2016.

[69] In our baseline specifications in this paper, we seasonally adjust the QCEW data as described in the text. For the sake of this reconciliation exercise, we use not-seasonally-adjusted data.

[70] We do not estimate a specification using employment in levels.

(3) keeping county pairs involving North Carolina or dropping them, (4) defining the instrument based on changes in duration immediately upon the EUC expiration, or defining it based on the change between average duration in 2013q4 and the average duration in 2014, (5) starting the sample in 2013q1 or 2013q4, and (6) using seasonally-adjusted or not-seasonally-adjusted data. After translating each estimate to its implied effect on EPOP in levels, we find that these 96 estimates range between -0.637 and 0.541. The EPOP-equivalent estimate from HMM specification using QCEW data (either -0.558 using their estimate or -0.435 using our replication) is within that range, though at the negative end. Furthermore, as with the LAUS specification, we find a lower level of statistical precision than HMM: our standard error of 0.0068 would mean that HMM's point estimate of -0.0100 would not be statistically distinguishable from zero at conventional levels.

HMM repeat their analysis with two variants of their benchmark model. First, they replace the county fixed effects and linear trends with interactive effects (Bai 2009) and estimate the model over the 2005q1-2014q4 period. Second, they add to the benchmark model county-specific coefficients on three aggregate time series: the price of oil, aggregate construction employment, and reserve balances with the Fed system. We show these estimates in Panels 2 and 3, respectively, of **Table B3**. The first column shows HMM's estimate and our replication using the pre-redesign LAUS data.[71] These estimates are qualitatively similar to the estimates from the benchmark model. And, like the benchmark model, the coefficient estimates come much closer to zero when post-redesign LAUS employment data is used, consistent with the null effect of benefit expansions that we find in our baseline specifications. We have not been able to replicate their results with the QCEW.

Additionally, HMM estimate "event study" specifications, as described in their Section 6. These specifications are designed to compare employment in 2014 to what is predicted to have occurred in the absence of the EUC expiration based on pre-expiration data. These predictions are formed by estimating a model using data solely from 2005q1 to 2013q4, and by using the resulting parameter estimates to project the future path of employment in a given county. To estimate the pre-event model, HMM regress county-level log employment on county fixed effects, date fixed effects, a county-specific cubic in the quarterly date, and four lags of log employment. They then define their dependent variable e^*_{ct} as the difference between actual log employment and predicted log employment based on the model parameters. Finally, they recover the effect of the EUC expiration by estimating the following model using observations only from 2014:

$$e^*_{ct} = \alpha\big(\ln(D_{c,2014}) - \ln(D_{c,2013q4})\big) + \nu_{pt} + \epsilon_{cpt} \tag{B3}$$

[71] We calculate standard errors in Panel 2 via a block bootstrap at the state-pair level. We use four factors, as HMM report using for LAUS employment, throughout Panel 2.

They estimate a coefficient of approximately -0.02, both using employment from LAUS and from the QCEW, meaning that counties which saw larger declines in benefits than their neighbors (i.e., whose independent variable is more negative) experienced higher growth of log employment in 2014, relative to their neighbors, relative to the prediction of their model. As with the estimates found in the "interaction term" models using pre-revision LAUS, this estimate would imply that the 26-to-99 week expansion would explain about one third of the EPOP drop during the Great Recession.

While we have not been able to replicate their results exactly, we do obtain qualitatively similar results. The main result from the event study strategy can be seen immediately in **Appendix Figure B2**, which plots the time series of the average value of log employment, as well as the series of predicted log employment, for high-benefit counties relative to low-benefit counties (where "high" and "low" status is defined by the size of the drop in log duration between 2013q4 and 2014, relative to the county pair partner). The model predicts that employment in high-benefit counties will continue to fall in 2014 relative to their lower-benefit neighbors, when in fact, a modest reversal occurs. The event study approach attributes this to the effect of the EUC expiration. As in the "interaction term" models discussed above, the redesign of the LAUS series affects the results substantially. When we repeat the analysis using the revised data, we find that the coefficient estimate becomes slightly (and insignificantly) positive, as shown in **Appendix Figure B3**. HMM also estimate the event study with QCEW data, and find an estimate of -0.0236, which is larger (in magnitude). When we estimate this model using employment from the QCEW, we find a coefficient of -0.0126 (with a standard error of 0.0113), which is in between our estimates for the specifications with revised and vintage LAUS log employment, respectively.[72] This is shown graphically in **Appendix Figure B4**.

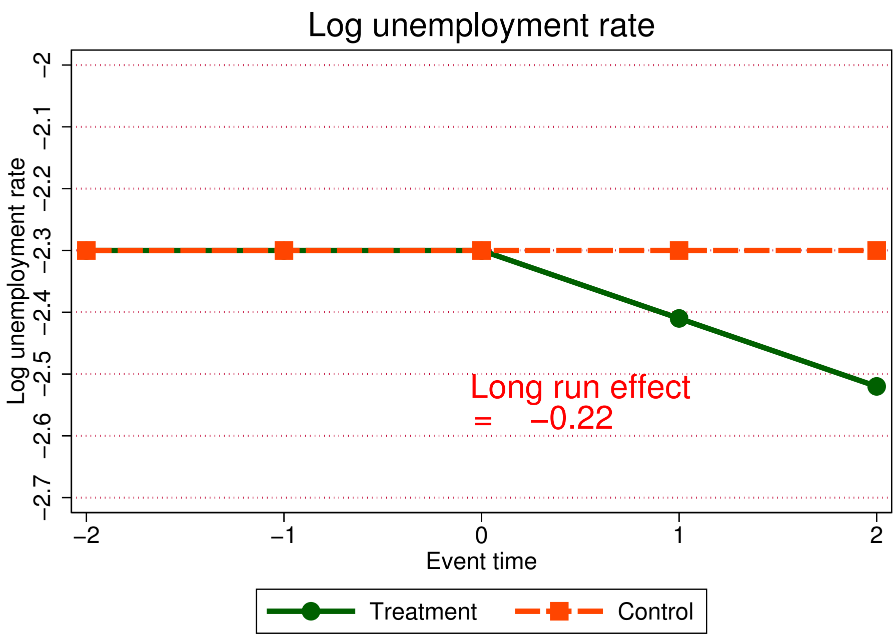
When translated to a change in EPOP, our replication of HMM's event study estimate using the QCEW (-0.703) is substantially more negative than our estimates using EUC expiration, which ranged between -0.024 (full BCP-FE sample) and -0.182 (PT-trimmed sample). HMM's event study strategy estimates a negative effect of EUC expiration using QCEW data because it constructs a counterfactual where the employment differential between the high and low treatment counties is expected to become more negative in 2014. This HMM counterfactual is largely driven by a county-specific polynomial time trend, whose identification is heavily reliant on employment changes that occur up to nine years before the treatment event.[73] As an indication of the type of problem with such a parametric strategy, the employment reversal (both in the QCEW data and, in fact, in the pre-revision LAUS data as well) appears to begin a few quarters prior to the expiration of EUC—a "pre-reversal" which casts doubt on the plausibility of a continuing downward trend as the appropriate counterfactual. In contrast, we take a much more flexible approach by showing whether

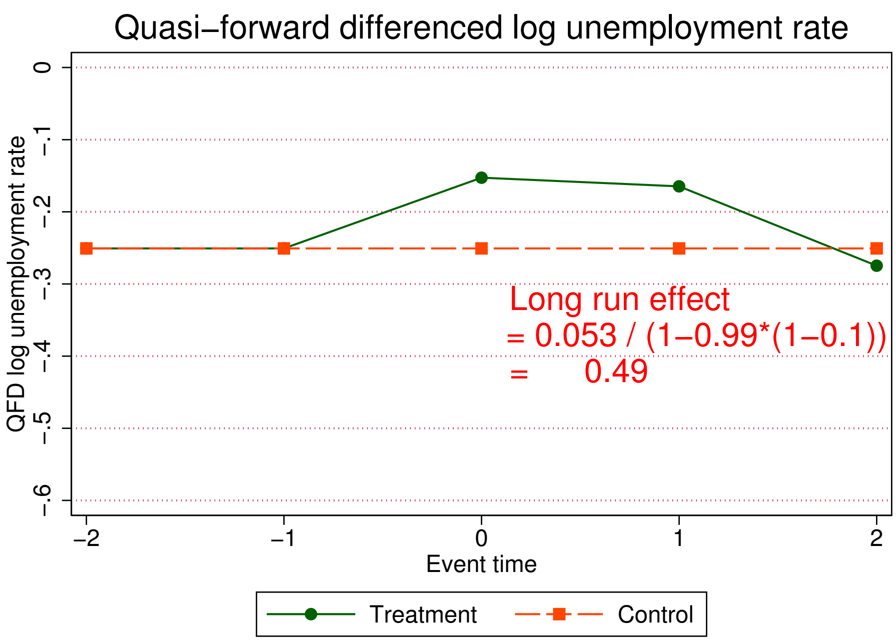
[72] This standard error takes the parameters of the model estimated in the pre-change period as non-random, likely causing us to understate this standard error. HMM use a bootstrapping procedure to construct these standard errors.

[73] The use of a cubic trend, rather than some other degree of polynomial, does not affect these results substantially.

the employment rates were following parallel trends prior to 2014 by treatment status on the two sides of the border in our 2014 expiration IV. We find that they were, indeed, following parallel trends—as shown clearly in **Figure 7** for the full set of border county pairs. And that this employment gap between the two sides of the border remained largely unchanged following the 2014 expiration. We think the more transparent evidence from the 2014 event that we provide in **Figure 7** raises questions about the causal import of the parametric model used by HMM to construct the counterfactual employment path.

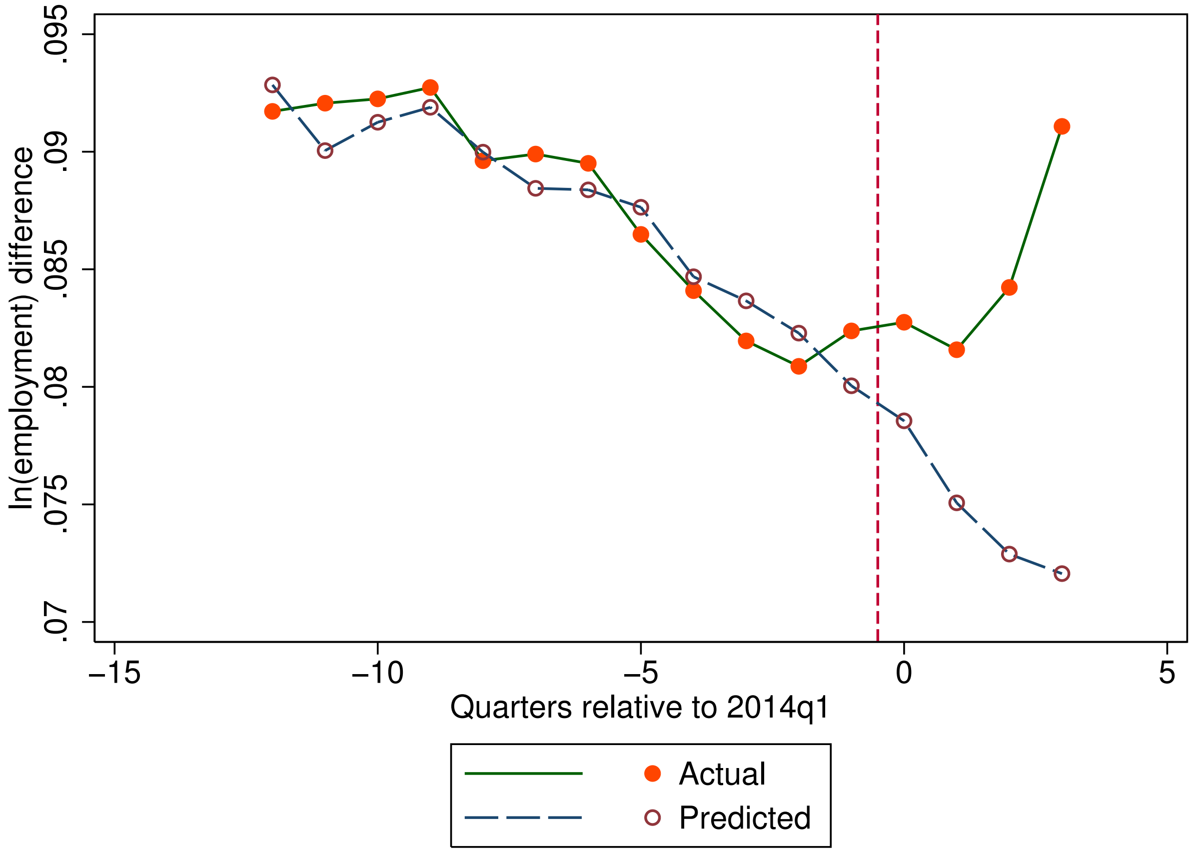
Figure B1: Illustration of wrong-signed estimate using quasi-forward-differenced data





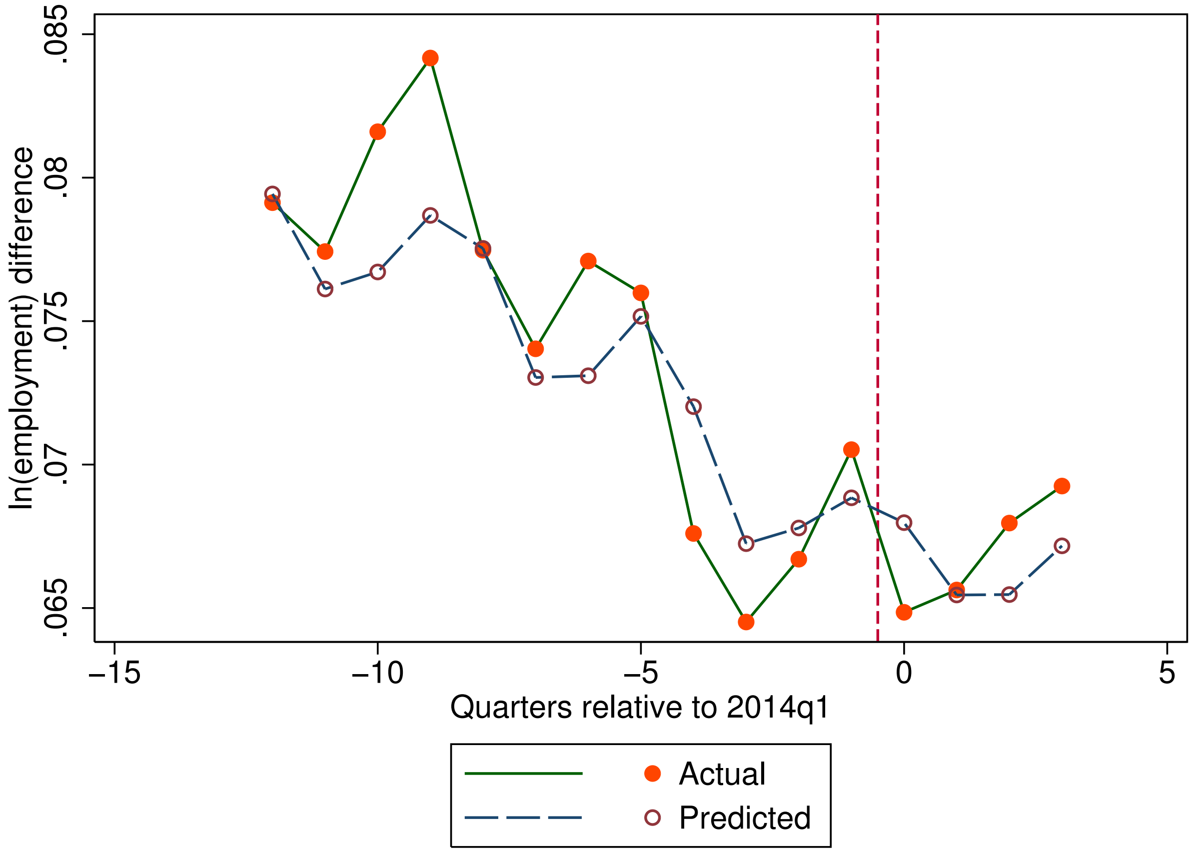
Notes: These two figures illustrate how using a quasi-forward-differenced dependent variable can lead to an estimate that is wrong-signed. In the top panel, the log unemployment rate of a hypothetical treated county is plotted against that of a control county. At event time zero, a one-unit increase in UI duration occurs. The control county unemployment rate is unchanged, but the treatment county log unemployment rate falls by 0.11 at event time 1 and an additional 0.11 at event time 2, where it remains at event time 3 (not plotted). The long-run effect of this change is a reduction of the unemployment rate by 22 log points. In the bottom panel, the data is plotted after taking a quasi-forward-difference (assuming a separation rate of 10 percent). The quasi-forward-differenced unemployment rate increases in the treated county and is unchanged in the control county. The implied coefficient estimate is 0.053, which erroneously implies an increase in the steady state unemployment rate of 49 log points – the wrong sign relative to the true effect.

Figure B2: Replication of HMM event study: Pre-revision LAUS employment



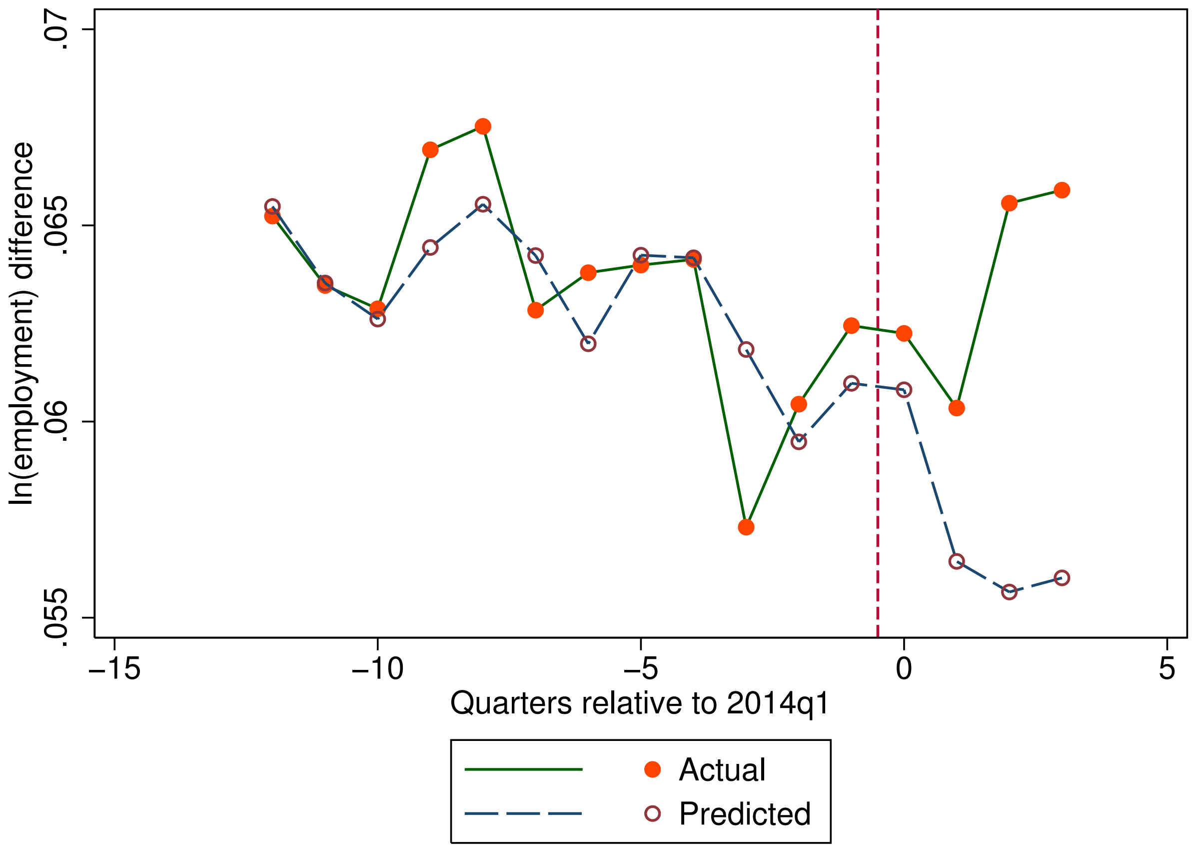
Notes: This figure plots (solid line, solid points) the average difference in log employment between "high" and "low" counties, where a "high" county is defined to have experienced a larger drop in log duration between 2013q4 and 2014 than its neighbor; pairs which experienced identical drops in log duration are not included. The figure also plots (dashed line, hollow points) the average difference in predicted log employment between high and low counties, where the prediction is computed by regressing (on quarterly data from 2005q1 through 2013q4) county log employment on four lags of log employment, time fixed effects, and a county-specific cubic function of the date. Predictions in 2014q1 through 2014q4 are computed recursively. This figure uses employment data from LAUS, prior to the March 2015 redesign.

Figure B3: Replication of HMM event study: Post-revision LAUS employment



Notes: This figure plots (solid line, solid points) the average difference in log employment between "high" and "low" counties, where a "high" county is defined to have experienced a larger drop in log duration between 2013q4 and 2014 than its neighbor; pairs which experienced identical drops in log duration are not included. The figure also plots (dashed line, hollow points) the average difference in predicted log employment between high and low counties, where the prediction is computed by regressing (on quarterly data from 2015q1 through 2013q4) county log employment on four lags of log employment, time fixed effects, and a county-specific cubic function of the date. Predictions in 2014q1 through 2014q4 are computed recursively. This figure uses current LAUS data.

Figure B4: Replication of HMM event study: QCEW employment



Notes: This figure plots (solid line, solid points) the average difference in log employment between "high" and "low" counties, where a "high" county is defined to have experienced a larger drop in log duration between 2013q4 and 2014 than its neighbor; pairs which experienced identical drops in log duration are not included. The figure also plots (dashed line, hollow points) the average difference in predicted log employment between high and low counties, where the prediction is computed by regressing (on quarterly data from 2015q1 through 2013q4) county log(employment) on four lags of log employment, time fixed effects, and a county-specific cubic function of the date. Predictions in 2014q1 through 2014q4 are computed recursively. This figure uses employment data from QCEW.

Table B1: Decomposition of difference between estimates from HKMM and BDGK into contributing factors

Step	From HKMM	To BDGK	Average Marginal Effect
Base Estimate	-2.6612	0.4299	
	(0.6298)	(0.4867)	
No QFD	2.6184		1.3016
	(0.5953)		(0.4197)
Align sample period	0.8165	1.5719	0.9149
	(0.6915)	(0.8585)	(0.3487)
Urate to EPOP	1.1196	1.1334	0.6731
	(1.1841)	(0.4775)	(0.3946)
Bai to FE	0.8241	0.6469	0.1891
	(0.7205)	(0.5576)	(0.2952)
Logs to levels		0.0777	0.0088
		(0.3403)	(0.1416)
Align counties	-0.1162	0.0979	0.0035
	(0.0846)	(0.0658)	(0.0267)

Notes: The first row reports the total effect of the expansion of UI from 26 to 99 weeks, in percentage points of EPOP, implied by the coefficient estimates of HKMM (column 1) and the full sample BCP-FE estimates of this paper (BDGK) (column 2). The remaining estimates in the first column represent the increased total implied effect of UI when one specification change is made from the original HKMM estimate. The remaining estimates in the second column represent the effect of taking each final step to arrive at the BDGK estimate. Because the total implied effect is not well motivated by theory when using quasi-differenced data in levels, we leave two cells blank in these first two columns. The third column represents the average incremental effect of taking each step along all possible transition paths between HKMM and BDGK estimates, except that we discard transition paths that involve estimating models with quasi-differenced data in levels. See text for details regarding each step and the conversion of each coefficient estimate into an effect on EPOP. Standard errors are calculated via a block bootstrap at the state-pair level with 200 replications.

Table B2: Transitioning from HKMM to BDGK estimates: Contribution of factors along three particular paths

	Path 1			Path 2			Path 3	
	Coefficient	EPOP effect		Coefficient	EPOP effect		Coefficient	EPOP effect
HKMM reported result	0.0490	-2.5885						
HKMM replication	0.0510	-2.6612	HKMM replication	0.0510	-2.6612	HKMM replication	0.0510	-2.6612
	(0.0097)	(0.6832)		(0.0097)	(0.6832)		(0.0097)	(0.6832)
Align counties	0.0527	-2.7774	Urate to EPOP	-0.0029	-1.5416	Align sample period	0.0149	-1.8217
	(0.0083)	(0.5905)		(0.0019)	(0.9967)		(0.0032)	(0.4955)
Eliminate QD	0.0104	-0.0428	Elimate QD	-0.0029	-0.1742	Align counties	0.0153	-1.8778
	(0.0332)	(0.1376)		(0.0055)	(0.3247)		(0.0029)	(0.4411)
Bai to FE	0.1291	-0.5759	Logs to levels	-0.0409	-0.0409	Elimate QD	0.0061	-0.0251
	(0.0428)	(0.2077)		(0.2700)	(0.2700)		(0.0203)	(0.0837)
Urate to EPOP	-0.0300	-1.7525	Align sample period	-0.2173	-0.2173	Logs to levels	0.3197	-0.2046
	(0.0125)	(0.7149)		(0.1642)	(0.1642)		(0.1595)	(0.1021)
Align sample period	0.0061	0.3641	Bai to FE	0.4351	0.4351	Bai to FE	1.0995	-0.7035
	(0.0083)	(0.4988)		(0.4802)	(0.4802)		(0.2498)	(0.1599)
Logs to levels (BDGK)	0.4299	0.4299	Align counties (BDGK)	0.4299	0.4299	Urate to EPOP (BDGK)	0.4299	0.4299
	(0.4711)	(0.4711)		(0.4711)	(0.4711)		(0.4711)	(0.4711)

Notes: This table presents three transition paths from HKMM's estimates to the full sample BCP-FE estimates of this paper (BDGK). Each cell presents the coefficient estimate, as well as the implied total effect of the 26-99 week expansion of UI expressed as an implied impact of EPOP, in percentage points. Once a step is made in a given path, it is retained in subsequent specifications in the same path. See text for details regarding each step. Standard errors for specifications involving the Bai (2009) interactive effects estimator are calculated via a block bootstrap at the state-pair level with 200 replications. Standard errors for other specifications are clustered twoway at the state and state-pair level.

Table B3: Estimates using the HMM interaction-term model: Alternative data sets and specifications

	(1) LAUS (orig.)	(2) LAUS (rev.)	(3) QCEW
Benchmark			
HMM's estimate	-0.0190		-0.0100
	[0.000]		[0.050]
Our estimate	-0.0200	-0.0048	-0.0078
	(0.0082)	(0.0060)	(0.0069)
Observations	46440	46440	46440
Interactive Effects			
HMM's estimate	-0.0233		-0.0121
	[0.000]		[0.030]
Our estimate	-0.0231	-0.0050	-0.0031
	(0.0093)	(0.0082)	(0.0092)
Observations	92720	92720	92880
Natural Factors			
HMM's estimate	-0.0144		-0.0141
	[0.000]		[0.020]
Our estimate	-0.0138	-0.0013	-0.0065
	(0.0104)	(0.0070)	(0.0067)
Observations	46440	46440	46440

Notes: This table reports estimates of α from HMM's "interaction-term" model: $e_{ct} = \kappa[\ln(D_{ct})\mathbb{1}(t \leq 2013q3)] + \alpha[\ln(D_{ct})\mathbb{1}(t \geq 2013q4)] + \nu_{pt} + \epsilon_{cpt}$, under different characterizations of the error term ϵ_{cpt}. In each panel, the top row reports the estimates reported by HMM, with p-values (from a block bootstrap at the state-pair level) in brackets. The second row reports our replication, with standard errors in parentheses. The first column uses log employment from LAUS, prior to the 2015 redesign. The second column uses post-redesign LAUS data, downloaded on September 9, 2016. The third column uses (not-seasonally-adjusted) log employment from the QCEW. The first panel represents the "benchmark" specification, in which $\epsilon_{cpt} = \mu_c + \gamma_c t + u_{cpt}$. The second panel replaces the fixed effects and county-specific trends with interactive effects (Bai (2009)): $\epsilon_{cpt} = \lambda_c' F_t + u_{cpt}$. The third panel adds to the benchmark specification county-specific coefficients on three national time series: the price of oil, employment in the construction industry, and reserve balances with the Fed system. Standard errors in the first and third panel are analytical, clustered at the state-pair level. Standard errors in the second panel are derived from a block bootstrap at the state-pair level.

C Online Appendix C: Additional Tables and Figures

Figure C1: Increase in UI benefit duration from the November 2008 expansion of EUC

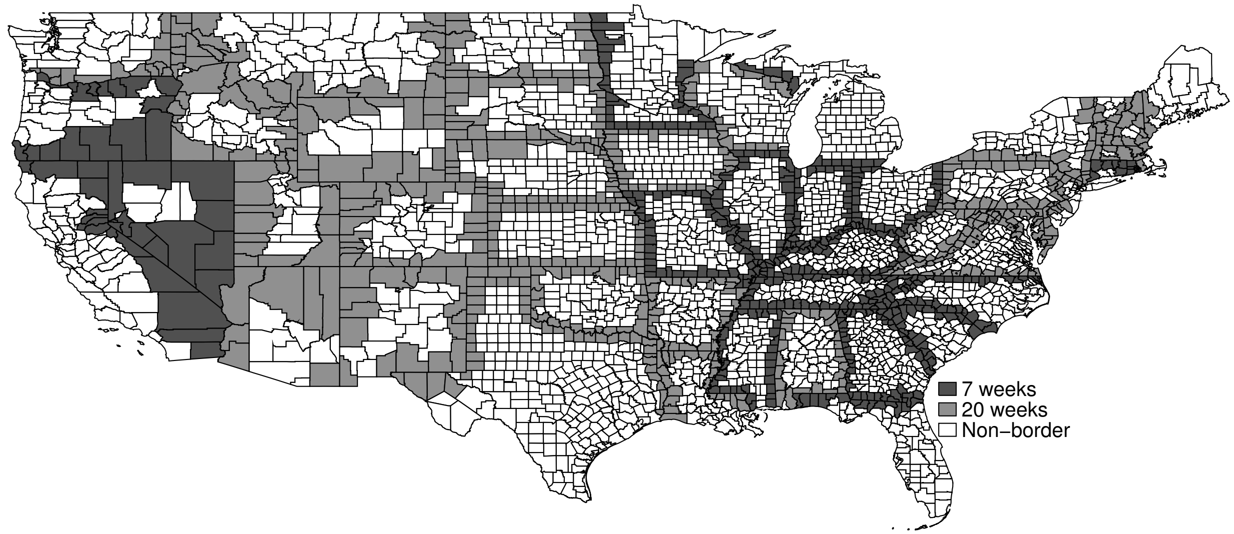
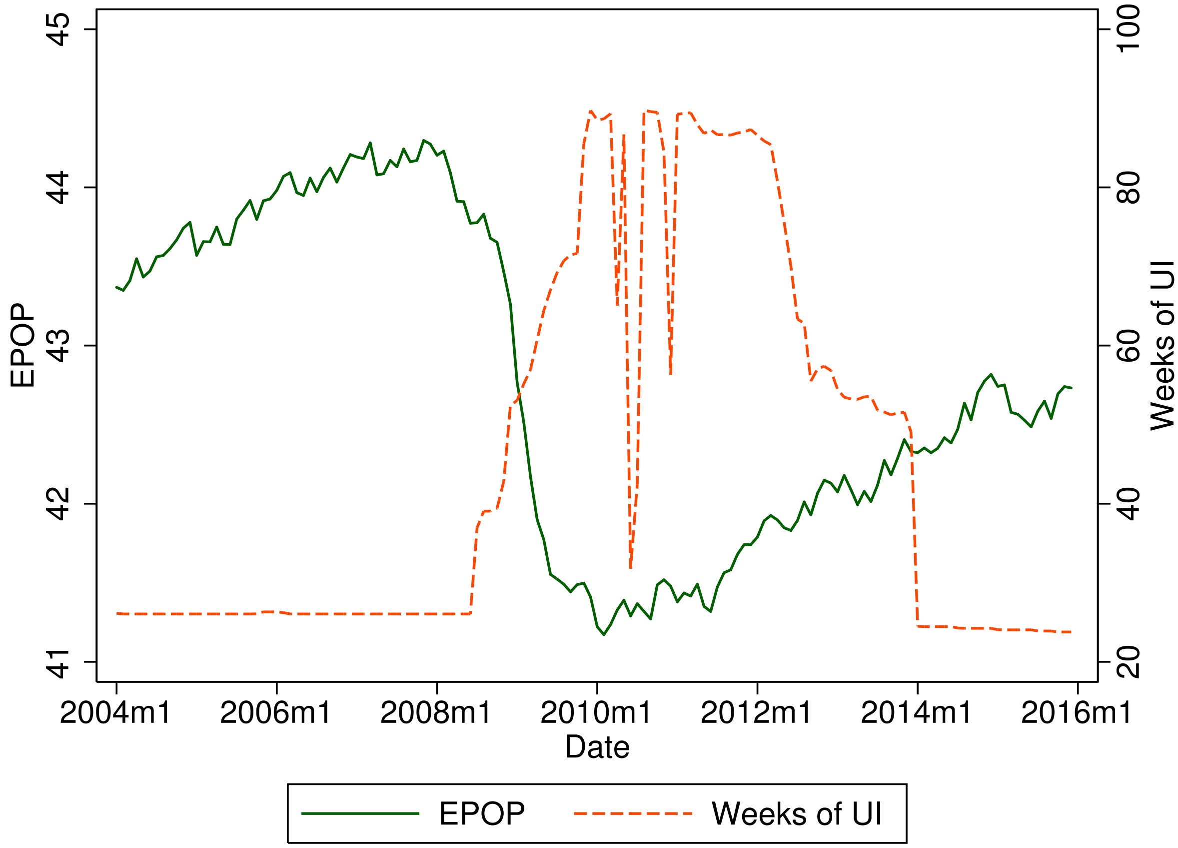
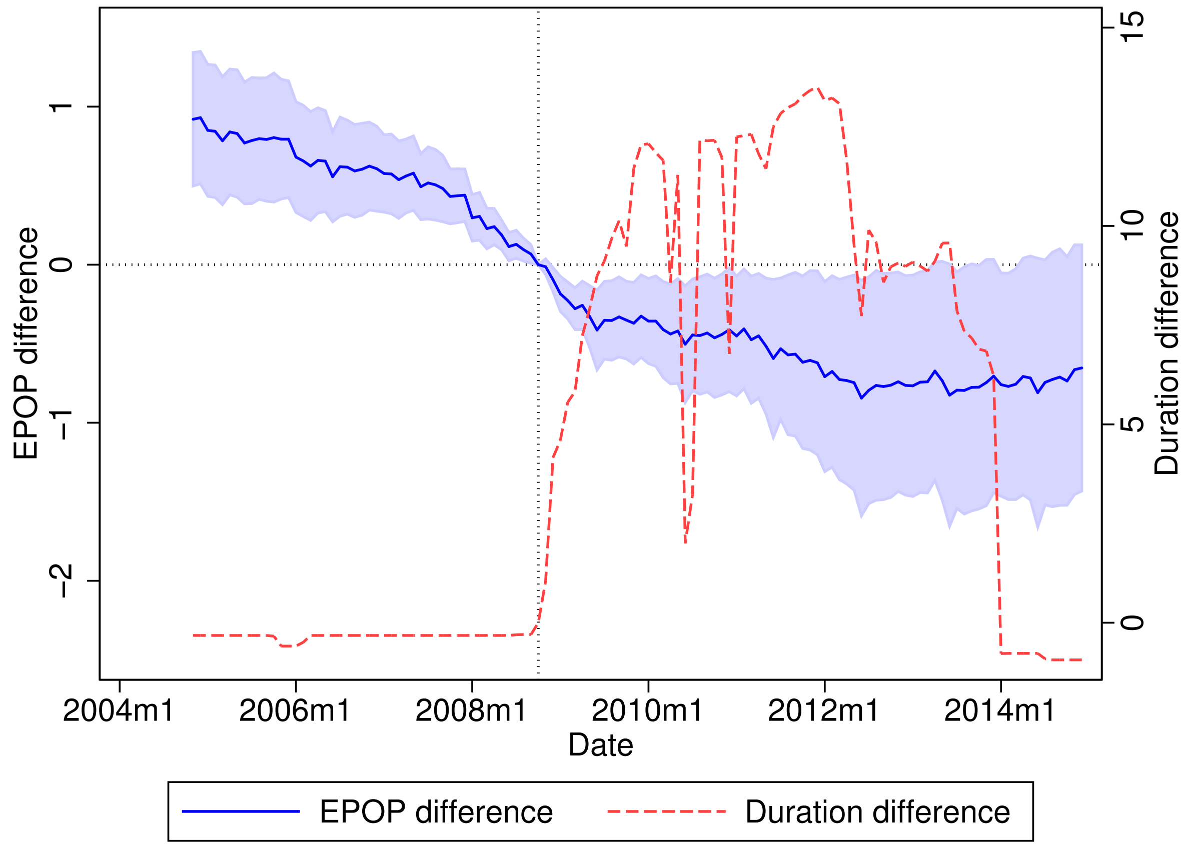


Figure C2: Evolution over time: national QCEW-based EPOP ratio and UI benefit duration



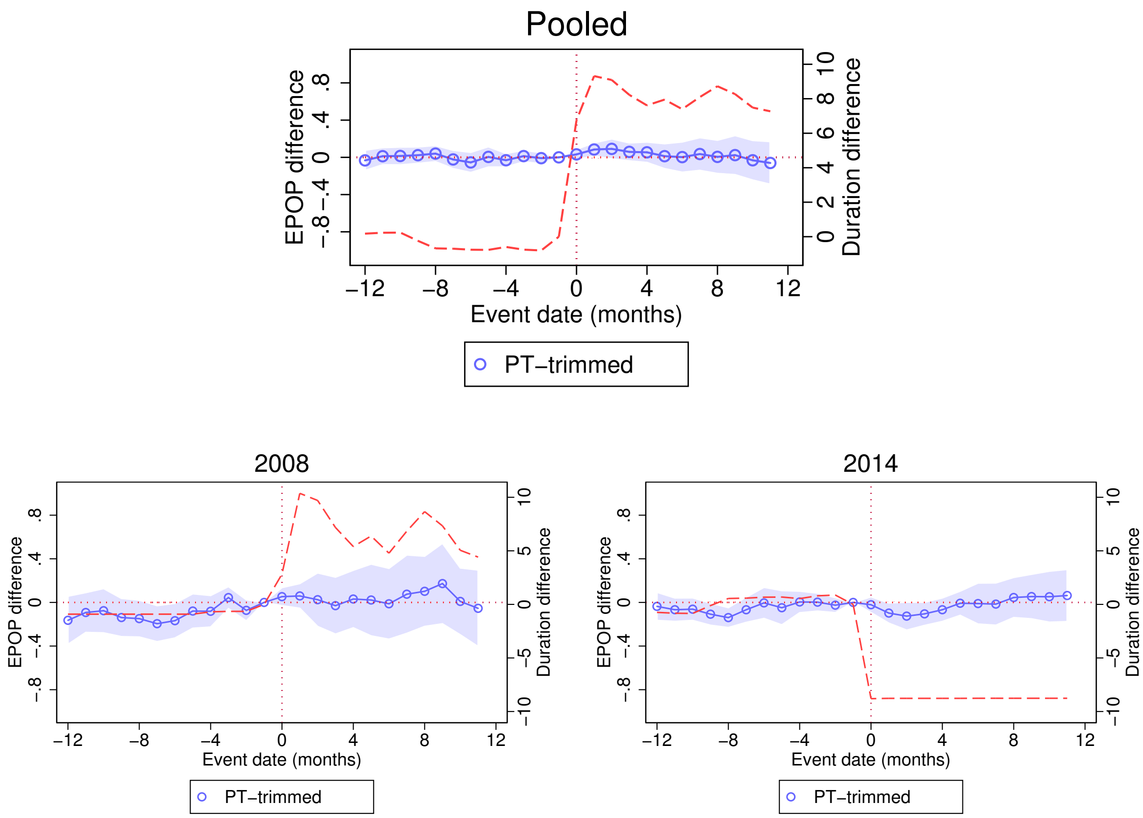
Notes: EPOP is the seasonally-adjusted ratio of employment (from the QCEW) to population age 15+. Weeks of UI represents the maximum number of weeks of UI compensation available. In this figure, both EPOP and weeks of benefits are calculated via an unweighted average of counties.

Figure C3: Evolution of EPOP and UI benefit duration differentials by average treatment intensity: without pair-period fixed effects, using all counties



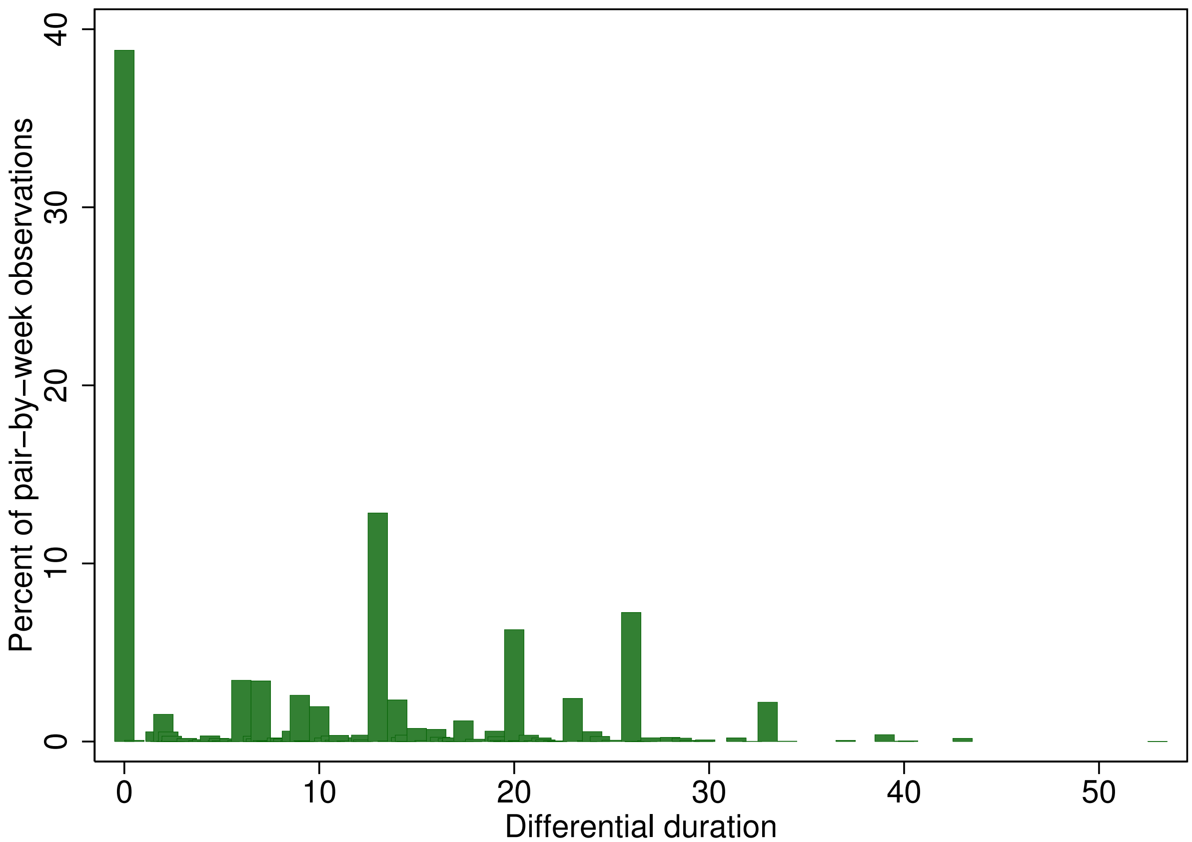
Notes: This figure plots (solid line, left axis) the set of β_s coefficients from the following regression: $E_{ct} = \sum_{s=\tau_A}^{\tau_B} \beta_s treat_c \mathbb{1}\{t = s\} + \lambda_c + \nu_t + \epsilon_{ct}$. E_{ct} is the seasonally-adjusted ratio of total employment to population age 15+, scaled in percentage points. The average treatment intensity, $treat_c$, is a time-invariant, continuous measure defined as the average duration during the treatment period (2008m11-2013m12), minus average duration from the 12 months prior (2007m11-2008m10), divided by 10. The shaded region corresponds to the 95% confidence interval, robust to two-way clustering at the state and state-pair level. The dotted line (right axis) reflects the analogous coefficients with D_{ct} as the dependent variable, where D_{ct} is weeks of benefits. The month 2008m10, the last month prior to the first introduction of differential EUC, is marked with a dotted vertical line. The sample includes 1,161 county pairs.

Figure C4: Evolution of EPOP difference and UI benefit duration difference across state borders: 2008 expansion and 2014 expiration of EUC, using PT-trimmed sample



Notes: This figure reports the monthly cumulative response of EPOP (left axis, hollow circles) around the 2008 expansion and 2014 expiration of cross-state differentials in UI benefits, using the TT-trimmed sample. The top panel uses pooled 2008 and 2014 samples, centered around event date -1 whose cumulative response is defined as zero. The bottom panel separately examines the 2008 and 2014 events. The dependent variable is the first-differenced seasonally adjusted ratio of total employment to population age 15+, scaled in percentage points. The regression includes 11 lags and 12 leads in first-differenced benefit duration: for the 2008 sample, the duration variable is equal to the increase in weeks of UI duration immediately upon the implementation of UCEA, divided by 10; for the 2014 sample, the duration variable is defined as -1 times the weeks of UI duration lost as a result of EUC expiration, divided by 10. The dashed line (right axis) reports the monthly cumulative response of benefit duration around the event; the regression is identical to the EPOP specification except that the dependent variable is the first-differenced benefit duration in weeks. Event date zero is marked with a dotted vertical line; this corresponds to November 2008 for the 2008 sample and January 2014 for the 2014 sample. PT-trimming removes the quartile of county pairs with the highest mean squared error in EPOP between November 2004 and October 2008 (after partialling out a fixed level difference).

Figure C5: Distribution of differences in UI benefit duration across border county pairs



Notes: This figure plots the distribution of duration differences across border county pairs, with each observation at the pair-by-(calendar)-week level. The sample is restricted to weeks between November 23, 2008, and December 22, 2013.

Figure C6: Distribution of EUC differences across border county pairs immediately prior to EUC expiration

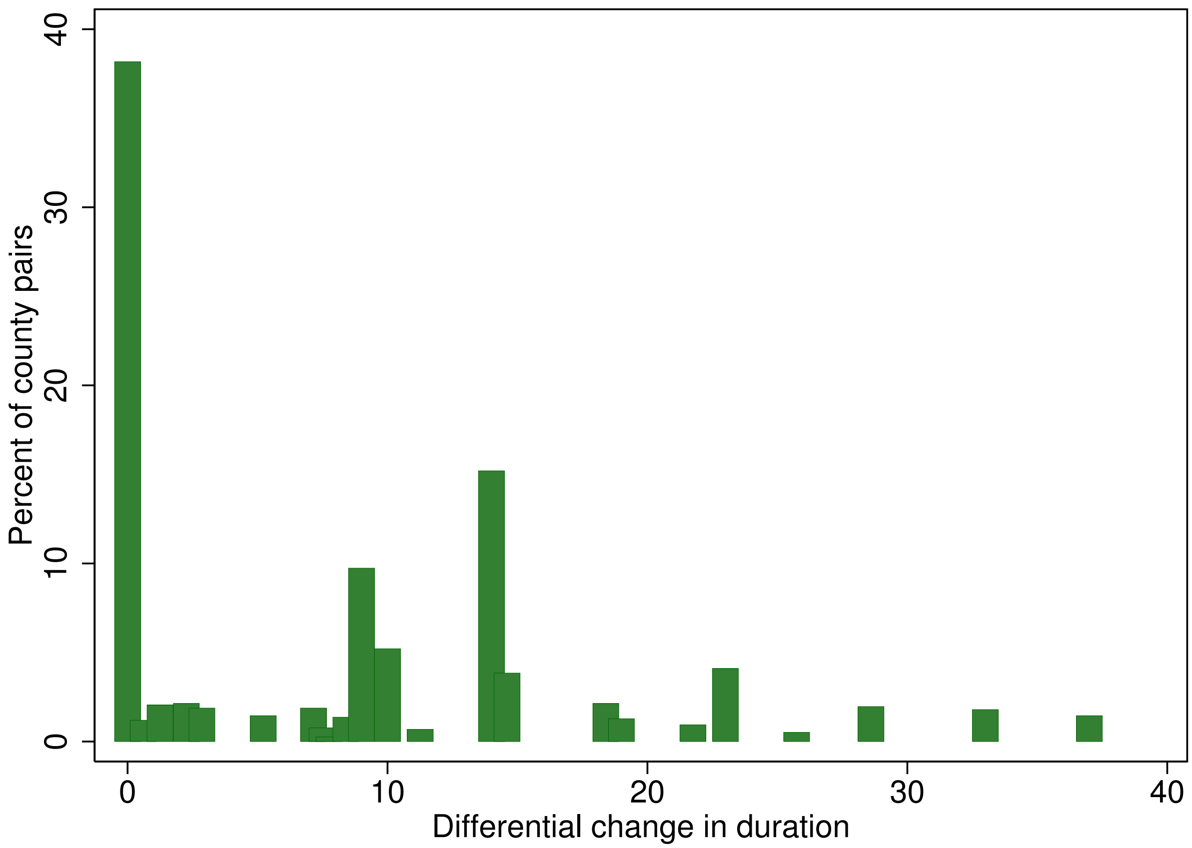
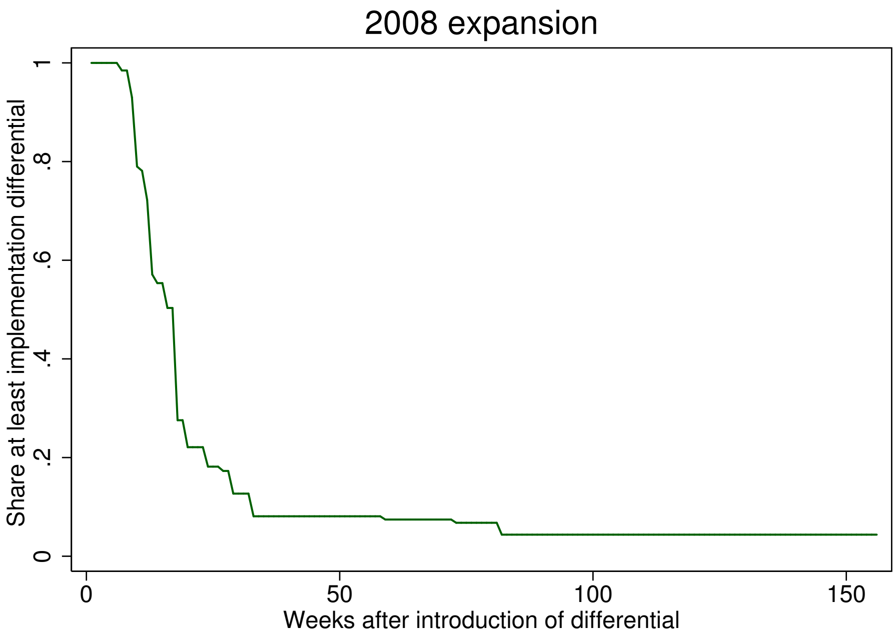
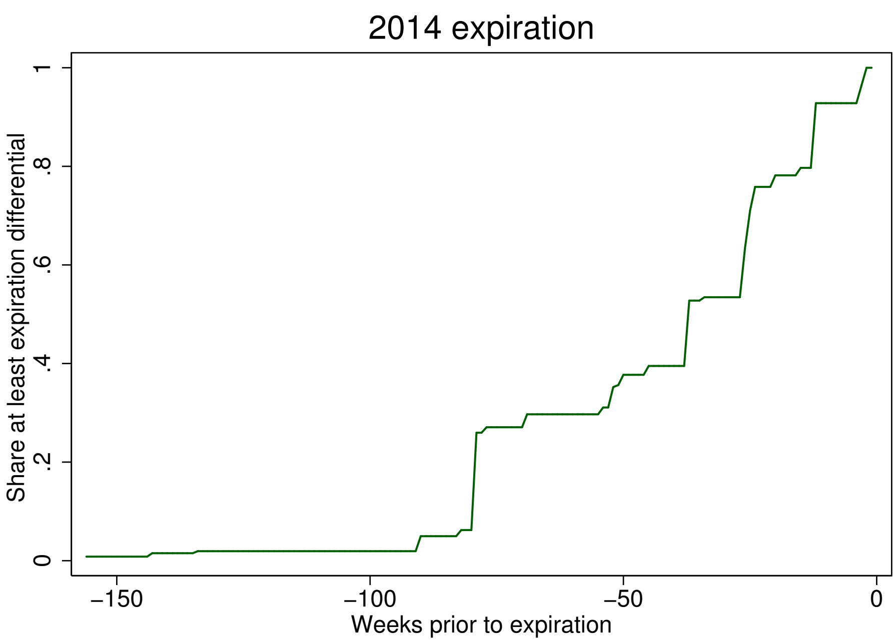


Figure C7: Persistence of duration differences in 2008 and 2014 events





Notes: The top graph plots the share of county pairs that continuously have a duration difference at least as large as immediately after the implementation of UCEA in November 2008. The bottom graph plots the share of county pairs that continuously have a duration difference (moving backward in time) at least as large as immediately prior to the 2014 expiration of EUC. The sample of pairs is restricted to those with differential duration at the time of the event in question.

Table C1: Summary statistics: High-treatment versus low-treatment counties, randomly-matched pairs versus border county pairs

	Randomly-matched				**Border pairs**			
	High: Mean	Sd	Low: Mean	Sd	High: Mean	Sd	Low: Mean	Sd
EPOP (A)	43.520	19.054	44.683	15.672	44.107	17.044	44.621	15.110
Private EPOP (A)	32.374	15.944	33.372	14.469	32.742	14.468	33.485	14.074
LAUS unemp. rate (A)	8.333	2.234	6.749	2.535	7.745	2.358	7.167	2.403
Population age 15+ (A)	100,302	225,744	56,185	133,920	81,415	213,875	71,441	153,060
Share white (B)	0.806	0.179	0.810	0.187	0.811	0.182	0.811	0.177
Share black (B)	0.090	0.145	0.089	0.154	0.085	0.145	0.086	0.147
Share hispanic (B)	0.069	0.105	0.057	0.100	0.067	0.111	0.059	0.092
Share H.S. grad (B)	0.565	0.066	0.571	0.063	0.569	0.064	0.567	0.065
Share college (B)	0.184	0.086	0.186	0.082	0.179	0.078	0.189	0.086
Median h.h. income (B)	43,557	11,842	42,450	11,293	42,645	11,459	43,535	12,127
New mortgage debt p.c. (A)	3.804	3.021	3.086	2.874	3.386	3.092	3.586	2.877
Share in cities 50k+ (C)	0.226	0.355	0.151	0.303	0.190	0.331	0.196	0.331
Min. weeks of UI elig.	24.032	3.876	24.122	3.591	24.470	3.495	24.631	3.199
Max. weeks of UI elig.	97.041	5.829	85.133	13.912	96.105	6.674	86.996	13.320
Pairs w/ different avg treatment	2317		2317		1131		1131	
Pairs w/ identical avg treatment	5		5		30		30	

Notes: The first four columns report summary statistics in county pairs, separately for "high" and "low" treatment counties. A county's assignment to the "high" or "low" group is defined by its average treatment intensity relative to its counterpart within each pair. Average treatment intensity ($treat_c$) is a time-invariant, continuous measure defined as the average duration over the 2008m11-2013m12 period, minus average duration over the 2007m11-2008m10 and 2014m1-2014m12 periods. County pairs with identical treatment are dropped in this table. Columns 1-4 use a set of county pairs formed by randomly matching each county to some other county. Columns 5-8 report analogous statistics for the border county pairs. If a border county appears in j county-pairs, then it appears j times for the purpose of creating the estimates in this table. (A) is from 2007 data, (B) is from the 2005-2009 ACS, and (C) is from the 2010 Census. High school graduates are those who have attained a high school degree but not a bachelor's degree. College graduates are those who have attained a bachelor's degree.

Table C2: Summary statistics for all counties, all county border pairs, and PT-trimmed sample of county border pairs

	All counties		**Border counties**		**PT-trimmed**	
	Mean	St. Dev.	Mean	St. Dev.	Mean	St. Dev.
EPOP (2007)	44.19	18.33	44.51	16.20	43.15	13.96
Private EPOP (2007)	34.58	17.45	34.88	15.47	33.94	13.35
LAUS unemployment rate (2007)	4.857	1.686	4.948	1.777	5.055	1.655
Population age 15+ (2007)	76818.0	243398.5	72692.4	178383.3	84309.4	193927.5
Share white (2005-2009 ACS)	0.796	0.190	0.812	0.181	0.814	0.179
Share black (2005-2009 ACS)	0.0885	0.144	0.0834	0.145	0.0872	0.144
Share hispanic (2005-2009 ACS)	0.0755	0.128	0.0620	0.101	0.0552	0.0922
Share high school grad, less than Bachelor's (2005-2009 ACS)	0.564	0.0665	0.568	0.0640	0.566	0.0635
Share Bachelor's degree or higher (2005-2009 ACS)	0.187	0.0852	0.184	0.0818	0.187	0.0838
Median household income (2005-2009 ACS), 2009 dollars	43299.6	11419.7	42949.1	11725.8	43258.3	12282.6
Newly acquired mortage debt per capita (2007)	3.535	3.216	3.508	3.120	3.604	3.036
Share in cities 50k+ (2010 census)	0.186	0.333	0.188	0.328	0.216	0.344
Minimum weeks of UI eligibility over sample period	23.78	4.365	24.17	4.040	24.26	3.931
Maximum weeks of UI eligibility over sample period	91.37	12.15	90.74	12.38	91.16	11.91

Notes: If a border county appears in j county-pairs in the sample in question, then it appears j times for the purpose of creating estimates in this table. PT-trimming removes the quartile of county pairs with the highest mean squared error in EPOP between November 2004 and October 2008 (after partialling out a fixed level difference).

Table C3: Pre-existing employment trends prior to November 2008 UI benefit expansion

	(1) All counties	(2) Border counties	(3) Border counties	(4) PT-trimmed
Treatment X Date	-0.780 (0.244)	-0.976 (0.206)	-0.241 (0.288)	-0.147 (0.130)
Observations	148896	111456	111456	83520
County fixed effects	X	X	X	X
Pair-period fixed effects			X	X

Notes: In columns 1 and 2, each cell reports the coefficient on $treat_c \times t$ from a regression of the following form: $E_{ct} = \alpha \times treat_c \times t + \lambda_c + \theta_t + \epsilon_{ct}$. In columns 3 and 4, each cell reports the coefficient on $treat_c \times t$ from a regression of the following form: $E_{cpt} = \alpha \times treat_c \times t + \lambda_c + \nu_{pt} + \epsilon_{cpt}$. In all columns, the dependent variable is the seasonally-adjusted ratio of total employment to population age 15+, scaled in percentage points. The regression is estimated over the period 2004m11-2008m10 and t is time measured in months since 2014m11 divided by 48 (representing the 48 month period between the beginning and the end of this sample). The time-invariant variable $treat_c$ is the average treatment intensity for each county, defined as the average duration over the 2008m11-2013m12 period, minus average duration from the 12 months prior (2007m11-2008m10), divided by 10. Thus we interpret the coefficientα as the change in EPOP between 2004m11 and 2008m10 associated with an additional 10 weeks average higher UI duration between 2009m11 and 2014m1. In column 1, standard errors are clustered at the state level. In columns 2, 3, and 4, standard errors are clustered two-way at the state and state-pair level. Columns 4 report the estimates from the set of border county pairs in the PT-trimmed sample. PT-trimming removes the quartile of county pairs with the highest mean squared error in EPOP between November 2004 and October 2008 (after partialling out a fixed level difference).

Table C4: Cumulative response of EPOP from distributed lags specification: full sample regressions

	(1) BCP-FE		(2) PT-Trimmed	
	Leads	Lags	Leads	Lags
Contemp.		-0.006		-0.012
		(0.097)		(0.072)
Lead/lag 1	0	-0.123	0	-0.055
	(0)	(0.129)	(0)	(0.098)
Lead/lag 2	0.118	0.004	-0.020	-0.005
	(0.114)	(0.150)	(0.089)	(0.115)
Lead/lag 3	0.208	0.218	0.092	0.036
	(0.157)	(0.221)	(0.110)	(0.151)
Lead/lag 4	0.263	0.154	0.044	0.002
	(0.198)	(0.185)	(0.146)	(0.149)
Lead/lag 5	0.148	0.220	0.062	0.054
	(0.243)	(0.229)	(0.192)	(0.177)
Lead/lag 6	0.243	0.069	0.077	0.065
	(0.268)	(0.289)	(0.197)	(0.198)
Lead/lag 7	-0.030	0.239	-0.083	0.083
	(0.280)	(0.287)	(0.178)	(0.219)
Lead/lag 8	0.058	0.113	0.043	0.044
	(0.317)	(0.318)	(0.192)	(0.225)
Lead/lag 9	0.056	0.229	0.021	0.025
	(0.322)	(0.337)	(0.202)	(0.253)
Lead/lag 10	0.307	0.165	0.100	0.046
	(0.332)	(0.376)	(0.228)	(0.267)
Lead/lag 11	0.403	0.112	0.132	0.056
	(0.345)	(0.376)	(0.224)	(0.290)
Lead/lag 12	0.228	0.168	0.059	0.050
	(0.338)	(0.394)	(0.231)	(0.304)
Lead/lag 13		0.154		0.107
		(0.403)		(0.322)
Lead/lag 14		0.094		-0.014
		(0.410)		(0.317)
Lead/lag 15		0.193		-0.013
		(0.470)		(0.326)
Lead/lag 16		0.341		-0.010
		(0.445)		(0.304)
Lead/lag 17		0.273		0.021
		(0.468)		(0.309)
Lead/lag 18		-0.003		-0.167
		(0.499)		(0.305)
Lead/lag 19		0.155		-0.068
		(0.510)		(0.323)
Lead/lag 20		0.162		-0.030
		(0.548)		(0.341)
Lead/lag 21		0.168		-0.096
		(0.580)		(0.341)
Lead/lag 22		0.210		0.022
		(0.597)		(0.340)
Lead/lag 23		0.181		-0.075
		(0.583)		(0.351)
Lead/lag 24		0.340		-0.087
		(0.596)		(0.361)

Notes: This table reports cumulative monthly lags and leads estimated on the full sample (2007m11-2014m12), using all border county pairs (BCPs) (column 1) and the subset of BCPs in the PT-trimmed sample (column 2), where all independent variables are divided by 73. The dependent variable is the first-differenced seasonally adjusted ratio of total employment to population age 15+, scaled in percentage points. The regression includes 24 lags and 11 leads and is estimated using EPOP data from 2007m11-2014m12 (and thus duration data from 2005m11-2015m11) in first differences. The zeroth cumulative lag is equal to the estimated coefficient on contemporaneous duration. The jth cumulative lag is equal to the estimated coefficient on contemporaneous duration plus the sum of the estimated coefficient on the 1st through jth lag term. The jth cumulative lead is equal to the sum of the estimated coefficient on the 1st through the $j-1^{th}$ lead term. The 1st cumulative lead is normalized to zero. PT-trimming removes the quartile of county pairs with the highest mean squared error in EPOP between November 2004 and October 2008 (after partialling out a fixed level difference). Standard errors are clustered two-way at the state and state-pair level.

Table C5: Robustness of the effects of UI benefit duration on EPOP: choice of cutoffs for trimming on match quality

	(1) Full	(2) Pooled	(3) 2008	(4) 2014
Baseline	0.430	0.143	0.549	-0.024
	(0.471)	(0.974)	(2.541)	(0.568)
	N = 199692	N = 108000	N = 55728	N = 52272
10th percentile	0.132	0.269	0.844	0.023
	(0.291)	(0.592)	(1.252)	(0.454)
	N = 179568	N = 97056	N = 50112	N = 46944
20th percentile	0.121	0.235	1.182	-0.173
	(0.273)	(0.653)	(1.276)	(0.513)
	N = 159616	N = 86352	N = 44544	N = 41808
25th percentile	0.180	0.253	1.344	-0.214
	(0.268)	(0.650)	(1.253)	(0.523)
	N = 149640	N = 80928	N = 41760	N = 39168
30th percentile	0.217	0.018	0.947	-0.385
	(0.269)	(0.662)	(1.190)	(0.568)
	N = 139664	N = 75456	N = 38976	N = 36480
40th percentile	0.358	-0.007	0.892	-0.388
	(0.303)	(0.635)	(1.151)	(0.576)
	N = 119712	N = 64608	N = 33408	N = 31200
50th percentile	0.240	-0.201	-0.090	-0.247
	(0.315)	(0.542)	(1.194)	(0.456)
	N = 99760	N = 53952	N = 27840	N = 26112

Notes: Each cell reports the baseline coefficient from the full sample, pooled event sample, and 2008 and 2014 subsamples, estimated over a different subsample of border county pairs. The cells in row 1 correspond to the estimates in column 1 of Table 2. In the other rows, the sample of border county pairs (BCPs) is trimmed. First, we calculate the pair-specific mean squared error in EPOP between 2004m11-2008m10 (after partialling out a fixed level difference). We then rank and trim all BCPs according to these MSEs. In the second row, we drop the bottom 10 percent of BCPs with the largest MSE, in the third row, we drop the bottom 20 percent, and so forth. The fourth row (the 25th percentile) corresponds to the estimates in column 2 of Table 2. Standard errors are clustered two-way at the state and state-pair level.

Table C6: Additional robustness checks on the effects of UI benefit duration on EPOP

	2008 sample IV		2014 sample IV	
	(1) BCP-FE	(2) PT-Trimmed	(3) BCP-FE	(4) PT-Trimmed
1. Baseline	0.549	1.344	-0.024	-0.214
	(2.541)	(1.253)	(0.568)	(0.523)
2. Private EPOP	1.097	1.904	-0.164	-0.264
	(2.540)	(1.203)	(0.639)	(0.523)
3. Correlation-trimmed	0.559	1.163	-0.392	-0.327
	(2.895)	(1.198)	(0.728)	(0.581)
4. ISLT	0.237	0.578	1.206	0.830
	(1.398)	(0.715)	(0.878)	(0.392)
5. Quarterly data	0.787	1.560		
	(2.428)	(1.206)		
6. QWI EPOP (quarterly)	0.110	1.875	0.517	0.015
	(1.697)	(1.406)	(0.586)	(0.583)
7. Unbalanced panel	0.511	1.344	-0.002	-0.214
	(2.497)	(1.253)	(0.564)	(0.523)
8. $\ln(EPOP)$	0.032	0.018	-0.001	-0.008
	(0.045)	(0.025)	(0.009)	(0.010)
	[1.886]	[1.048]	[-0.037]	[-0.442]
9. $\ln(emp)$	0.039	0.019	0.006	-0.001
	(0.045)	(0.026)	(0.010)	(0.010)
	[2.327]	[1.138]	[0.320]	[-0.046]
10. Exploit Δ reg. benefits			0.037	-0.194
			(0.561)	(0.502)
11. Drop NC	0.660	1.542		
	(2.838)	(1.388)		
12. Keep NC			-0.437	-0.667
			(0.717)	(0.724)
13. NC: Alt. instrument			-0.037	-0.178
			(0.326)	(0.296)
14. Distance trimming	1.482	2.250	-0.229	-0.354
	(2.592)	(1.126)	(0.712)	(0.640)
15. Hinterland pairs	-0.979	0.600	0.556	-0.800
	(1.367)	(1.532)	(1.726)	(0.671)

Notes: Each cell reports regressions analogous to those reported in Table 2 for the 2008 and 2014 estimates (IV), respcetively. The estimates in the 1st row correspond to the estimates in the top two panels of Table 2. The estimates in the 2nd row replace (total) EPOP with the ratio of private employment to population age 15+. In the 3rd row, we trim the set of border county pairs based on the level of correlation between county EPOP and state EPOP over the period 2004m11-2008m10 (see text for details). The 4th row controls for county-specific linear trends. The 5th row uses quarterly data instead of monthly (and estimates over the 2007q4-2014q4 period). The 6th row uses EPOP derived from the QWI (at the quarterly level) instead of the QCEW. The 7th row includes counties without full EPOP data for each month, which we drop by default. The 8th and 9th row use $\ln(EPOP)$ and $\ln(employment)$, respectively, as dependent variables. The bracketed estimates in these two rows are the level-on-level equivalent, equal to $(\frac{99}{26}^{\hat{\beta}} - 1)\bar{E}$, where $\bar{E}$ is the mean EPOP level in the given sample. The 10th row uses a modified version of the instrument z_{ct} which exploits all changes in benefits, including changes in regular benefits, which occur at the end of December 2013. Rows 11-13 report estimates using alternative strategies for dealing with North Carolina (NC); by default, border county pairs (BCPs) with one neighbor in NC are kept in the full sample OLS and the 2008 subsample and dropped in the 2014 subsample. The 11th row completely drops all NC BCPs. The 12th row keeps all North Carolina BCPs. The 13th row keeps NC BCPs but redefines the instrument for NC counties (see text for details). The 14th row drops county-pairs whose population centroids are greater than 100km apart. The 15th row uses the "hinterland" pairs rather than the border pairs (see text for details). Cells which are not applicable in the given sample, or which provide estimates that are mechanically equal to the baseline estimates, are left blank. Standard errors are clustered two-way at the state and state-pair level.

Table C7: Robustness to inclusion of regression discontinuity controls

	No county fixed effects		**County fixed effects**	
	(1)	(2)	(3)	(4)
Border pairs	0.547	-27.247	0.303	0.612
	(15.454)	(35.819)	(0.300)	(1.155)
Add Hinterland pairs	-3.965	-26.000	0.335	0.553
	(10.123)	(19.193)	(0.277)	(0.659)
Pair-period effects	X	X	X	X
County fixed effects			X	X
Control for distance to border		X		X

Notes: This table reports results from regressions of EPOP on duration, with differing controls. In all specifications, state-pair by period fixed effects are included. In columns 1 and 3, we control for distance to border, with coefficients allowed to vary at the state by state-pair by period level. In columns 3 and 4, we add county fixed effects. To be consistent with Dieterle (2016), we weight the regressions by 2010 population, and do not allow repeated county-month observations (e.g., in cases when a county is a member of more than one pair). In the first row, we use only those counties that are members of a border pair. In the second row, we add counties that are members of a hinterland pair, as discussed in the text. Standard errors are clustered two-way at the state and state-pair level.

Table C8: Robustness to auxiliary specification differences in Dieterle et al. (2018)

	No county fixed effects		**County fixed effects**	
		Add RD controls		Add RD controls
Level-on-level				
Baseline	-3.965	-26.000	0.335	0.553
	(10.123)	(19.193)	(0.277)	(0.659)
Drop Hinterland	0.547	-27.247	0.303	0.612
	(15.454)	(35.819)	(0.300)	(1.155)
Unweighted	-8.721	-9.896	0.600	-1.757
	(4.960)	(15.950)	(0.495)	(1.150)
Original sample	-8.560	-8.870	0.329	-1.398
	(4.436)	(12.052)	(0.474)	(0.977)
Log-on-log				
Baseline	-3.777	-9.940	0.291	-0.142
	(4.773)	(6.769)	(0.177)	(0.419)
Drop Hinterland	-0.459	-7.716	0.122	-0.077
	(7.326)	(10.869)	(0.240)	(0.617)
Unweighted	-8.045	-10.957	0.557	-1.149
	(3.851)	(11.300)	(0.440)	(1.125)
Original sample	-8.570	-10.474	0.278	-0.807
	(3.409)	(9.078)	(0.470)	(1.091)
Pair-period effects	X	X	X	X
County fixed effects			X	X
Control for distance to border		X		X

Notes: This table reports results analogous to **Appendix Table C7**, with additional variations. Rows 1 and 2 of the top panel are identical to rows 2 and 1 in **Appendix Table C7**, respectively. The remainder of the rows modify the specification such that the specification becomes closer to the baseline regression in the top panel of **Table 2**. In the third row, the regression is not weighted by population. In the fourth row, we allow for repeated observations at the county-by-month level (e.g., when a county is in multiple pairs). The bottom panel is analogous to the top panel, except that it regresses log(EPOP) on log(duration); the coefficient estimates and standard errors are translated to the implied level effect at the mean. Standard errors are clustered two-way at the state and state-pair level.

Table C9: Effect of EPOP on duration, interacted with the centroid distance between county pairs

	Border pairs	Hinterland pairs	All pairs
1. D	0.634	1.918	0.634
	(0.548)	(1.236)	(0.548)
	[163]	[378]	[163]
2. $D \times 1(30km \leq dist \leq 40km)$	-0.217		-0.242
	(0.805)		(0.802)
	[222]		[223]
3. $D \times 1(40km \leq dist \leq 50km)$	0.104		0.444
	(0.714)		(0.760)
	[259]		[260]
4. $D \times 1(50km \leq dist \leq 70km)$	-0.930		-0.252
	(0.674)		(0.988)
	[299]		[317]
5. $D \times 1(70km \leq dist \leq 90km)$	-0.142		-0.472
	(1.861)		(1.039)
	[228]		[189]
6. $D \times 1(90km \leq dist \leq 110km)$			0.654
			(0.521)
			[295]
7. $D \times 1(110km \leq dist \leq 135km)$		-0.831	0.607
		(1.231)	(0.535)
		[514]	[549]
8. $D \times 1(135km \leq dist \leq 160km)$		-1.245	-0.070
		(1.328)	(0.739)
		[331]	[350]
9. $D \times 1(160km \leq dist \leq 200km)$		-2.002	-0.497
		(1.481)	(0.925)
		[224]	[241]
10. $D \times 1(200km \leq dist)$		-1.430	-0.193
		(2.321)	(1.838)
		[386]	[417]
P-value for test of joint significance for interaction terms	0.644	0.625	0.576

Notes: This table reports coefficient estimates from a regression of EPOP on duration interacted with the centroid distance between pairs, in bins. The regression uses the 2007m11-2014m12 period and includes county fixed effects and pair-period fixed effects. The first column uses only border pairs. The second column uses only hinterland pairs, as discussed in the text. The third column pools both sets of pairs together. Standard errors are clustered two-way at the state and state-pair level. The number of pairs within each bin is reported in brackets below the estimate. Note that bin 5 in column 1 aggregates bins 5 through 10; likewise, bin 1 in column 2 aggregates bins 1 through 6.

D Online Appendix D: Joint Replication of HKMM

In this joint appendix, we describe our replication of the main estimate in Marcus Hagedorn, Fatih Karahan, Iourii Manovskii and Kurt Mitman (2015), hereafter HKMM. We face two challenges in the replication: (1.) properly constructing the data set and (2.) executing the estimation correctly. Though proper execution of the estimation is potentially challenging due to the non-linearity of the model, replicating the data set is ultimately more difficult. While we are unable to simultaneously reproduce the exact sample size and point estimate, our preferred replication is very close: we come within 0.002 of the estimate and our data set contains about 1% fewer observations (385 out of 37,177). In this description of our replication of HKMM, we discuss not only our final specification but also the choices we made and the reasons for those choices. In particular, we traded off sample size with estimate closeness. We considered specifications with closer estimates where the gap in the sample size compared to HKMM's sample was substantially larger; we also considered samples which matched more closely the sample size of HKMM but where the estimates were not as close.

Estimation Equation and Method

The HKMM estimation equation, which uses data at the pair-by-time level, is as follows. Prior to estimation, the data for a given pair p at time t is spatially differenced.

$$ln(u_{pt}) - \beta(1 - s_t)ln(u_{pt+1}) = \alpha * ln(D_{pt}) + \lambda_p' F_t + \epsilon_{pt}$$

In this expression,u_{pt} is the unemployment rate from LAUS (as calculated prior to the March 2015 redesign of the LAUS program), β is the discount factor equal to 0.99, s_t is the separation rate, andD_{pt} represents weeks of UI benefits available. $\lambda_p' F_t$ represent the interactive effects: F_t is a time-specific vector of length K of common factors, while λ_p (also of length K) represents the pair-specific factor loadings. HKMM determine, by minimizing an Akaike information criterion, that the optimal K is equal to 2. We replicate their minimization, also obtaining two factors as optimal. All of our estimates estimate with two factors in both space and time. We follow HKMM in estimating the model using the method of Bai (2009). In the April 2016 version of HKMM, the authors report a main estimate of 0.049.

Sample

The biggest challenge in replicating this result is determining precisely which pair-time observations were used in the sample. HKMM report using an unbalanced panel of quarterly LAUS unemployment data spanning 32 quarters from 2005q1 to 2012q4 with a sample size of 37,177 county-pair-by-quarter observations in their baseline regression. Dividing the number of observations by the number of quarters indicates that this sample size is similar to a balanced panel of 1,162 county-pairs (37,177/32=1,161.78). Our initial sample of pre-revision LAUS data yields an unbalanced panel for 1,171 county-pairs and a total number of observations of 37,464. This is a nearly balanced panel. It only drops data for the four quarters following Hurricane Katrina (2005q3-2006q2) for the two border pairs that include St. Tammany, LA (paired with Hancock, MS and Pearl River, MS). The missing counties for these quarters range from small to above mean county size. In 2005, these three counties had populations of 217,407, 46,097, and 51,764 respectively, according to the U.S. Census Bureau. Dropping these counties fully to create a balanced panel is essentially inconsequential to our estimates. The estimate in the balanced panel is 0.0527 and the estimate in the unbalanced panel is 0.0529.[74]

Though estimating on the above unbalanced sample yields estimates which are close to those reported in HKMM, in order to more closely match HKMM's reported number of observations, we consider an additional sample restriction to the unbalanced panel. In particular, we note that HKMM draw employment data for auxiliary specifications from the Quarterly Workforce Indicators (QWI). In an earlier draft from October 2013, HKMM report a sample size of 30,988 county-pair-by-quarter observations covering the period from 2005q1 to the beginning of 2012. Over the 28 quarters covered, this sample size would be consistent with a nearly balanced panel of 1,107 county pairs (30,988/28=1,106.71), which aligns with the number of pairs for which HKMM report having "complete data" in the October 2013 and April 2016 drafts. We believe that the phrase "complete data" likely refers to the presence of unemployment data in the LAUS and employment data in the QWI in a given quarter.

So, we consider the possibility that the choice to use the panel of "complete data" in the October 2013 draft may have carried over in some form to the sample used in the April 2016 draft. Specifically, since QWI data only cover Massachusetts beginning in 2010q1,[75] we exclude county pairs that include a Massachusetts county from the sample to generate an unbalanced panel of 1,150 county pairs. This sample restriction leads to a sample size of 36,792 county-pair-by-quarter observations. While this restriction leads to a smaller sample than reported in the April 2016 version of HKMM, it is simpler and yields an estimate closer to HKMM than other QWI-based sample restrictions we considered. In particular, using this sample, we find an estimate of 0.0510 (to four deminal places), compared to the 0.049 (to three decimal places) reported by HKMM.[76]We use this sample for the replication estimate used both by Dieterle, Bartalotti and Brummet

[74]See https://www.bls.gov/katrina/lausquestions.htm for a discussion of the impact of Hurricane Katrina on LAUS.

[75]The QWI includes both beginning-of-quarter and end-of-quarter statistics. Since the beginning-of-quarter statistics are rolled over from the previous quarter's end-of-quarter numbers, some of the QWI data for Massachusetts does not begin until 2010q2.

[76]We estimate this model using the user-written Stata command "regife" (Gomez 2015). We have also written our own simplified version of this command and are able to obtain identical estimates.

(2020) and Boone et al. (2018): an unbalanced panel which (1.) keeps counties which temporarily did not report in the aftermath of Hurricane Katrina and (2.) drops all counties in Massachussetts.

Possible Reasons for Remaining Discrepancy

Lastly, we note that there may be other minor specification choices that prevent us from replicating the results of HKMM. First, we obtain the dependent variable (unemployment rates as estimated by LAUS prior to the March 2015 redesign) through a FRED API. While the original LAUS dataset (which HKMM presumably used) includes the estimates of the raw counts of unemployed persons and the size of the labor force, the FRED API reports only the unemployment rate to the nearest tenth of a percentage point. Thus, HKMM may have been using unrounded unemployment rates while we are using rounded unemployment rates. Second, there may be differences in how we aggregate weeks of benefits from the weekly level (at which they are reported) to quarters. We calculate the weeks of benefits available on a given calendar day, and then aggregate to the month level. We then aggregate to the quarterly level using an unweighted average of the three months within the quarter. It is possible that HKMM performed this aggregation somewhat differently. Third, it is possible that we use a different separation rate than HKMM. We use the non-seasonally-adjusted total separation rate as reported by JOLTS. Other possibilities include the seasonally-adjusted version or the version which includes only private employment. In any case, while these uncertainties might prevent us from replicating HKMM's result exactly, the fact that our replication is within 0.002 (to three significant digits) of HKMM's estimate suggests that these minor differences do not matter qualitatively. [77]

[77] Since the Bai (2009) estimator is non-linear, an additional possibility is that the likelihood function used in the optimization has multiple local optima and that HKMM and our replication of HKMM are at different optima. We do not, however, think this is likely given (1.) that we are able to exactly replicate the optimality of two factors and (2.) that our estimates are so close to those of HKMM.