NBER WORKING PAPER SERIES

POTENTIAL UNEMPLOYMENT INSURANCE DURATION AND LABOR SUPPLY: THE INDIVIDUAL AND MARKET-LEVEL RESPONSE TO A BENEFIT CUT

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Working Paper 22411
http://www.nber.org/papers/w22411

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
July 2016

We are grateful to David Card, Mark Duggan, Henry Farber, Robert Jensen, Pauline Leung, Olivia S. Mitchell, Kurt Mitman, Ulrich Müller, Zhuan Pei, Jesse Rothstein, Johannes Schmieder, Steven Woodbury, workshop participants at the ABL conference, Georgetown, New York Federal Reserve, Princeton University, UC Berkeley, and University of Wisconsin. Elijah De La Campa, Kevin DeLuca, Disa Hynsjo, Samsun Knight, Dan Van Deusen, and Sophie Zhu provided excellent research assistance. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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NBER Working Paper No. 22411
July 2016
JEL No. E24,H0,J6,J64,J65

ABSTRACT

We examine how a 16-week cut in potential unemployment insurance (UI) duration in Missouri affected search behavior of UI recipients and the aggregate labor market. Using a regression discontinuity design (RDD), we estimate a marginal effect of maximum duration on UI and nonemployment spells of approximately 0.5 and 0.3 respectively. We use RDD estimates to simulate the unemployment rate assuming no market-level externalities. The simulated response closely approximates the estimated change in the unemployment rate following the benefit cut, suggesting that even in a period of high unemployment the labor market absorbed this influx of workers without crowding-out other jobseekers.

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I. INTRODUCTION

How do recipients respond to the maximum duration of unemployment insurance (UI) benefits, and how do these responses affect the broader labor market? These questions are important for the analysis of UI programs, and relevant for understanding the performance of labor markets over the business cycle. A large literature has estimated the relationship between maximum UI duration and the behavior of UI recipients, and it has been hypothesized that extended benefits may have contributed to the trend of slow job market recoveries (Mitman and Rabinovich 2014). However, evidence on the relationship between potential UI duration and labor market ourtcomes, especially after the mid-1990's in the United States is thin. An additional consideration for evaluating how UI extensions affect the labor market is that the aggregate effects of these policies may differ from those implied by the micro response if there are general equilibrium effects or spillovers, as would be the case if search among UI recipients crowded out other jobseekers. With a few notable exceptions (Levine 1993, Valleta 2014 and Lalive, Landais, and Zweimuller 2015) we know relatively little about the relationship between the micro and macro responses to UI extensions.[1]

In this paper we study the micro and macro effects of a large cut in benefit duration Missouri that occurred in 2011 using newly available administrative data and regression

[1] Levine (1993) estimates the relationship between state and year variation in UI replacement rates and unemployment durations for uninsured workers. Using data from the CPS and NLYS for 1979-1987 he finds evidence of displacement. Valleta (2014) uses linked CPS data to examine the relationship between potential benefit duration by state and exit to unemployment for workers who are likely UI ineligible. On average he finds no relationship, but he finds that ineligible workers in higher unemployment states have higher exit rates when potential duration is higher. Lalive et al. (2013) find evidence of displacement by comparing regions in Austria with longer and shorter potential duration for older workers. Kroft and Notowidigdo (2015) conclude that there is potential crowding out during recessions using variation in benefit levels across states and over time. There is also a literature testing for externalities from job search assistance programs in Western Europe. These include Blundell et al. (2004), Crépon et al. (2013), Ferracci, Jolivet, and van den Berg (2010), and Gautier, Muller, van der Klaauw, Rosholm, and Svarer (2012). Davidson and Woodbury (1993) consider displacement effects from reemployment bonuses in the United States. General equilibrium estimates in Hagedorn, Karahan, Manovskii, and Mitman (2013), Hagedorn, Manovskii, and Mitman (2015), and Marinescu (2014) are also related to tests for the presence of externalities.

discontinuity and differences-in-differences designs. Following the 2007-2009 recession, eight US states reduced regular UI durations, partly in response to diminished reserves in state UI trust funds as well as a changing political environment. While there is a precedent for cutting UI benefit levels, to our knowledge this was the first time states cut maximum UI benefit durations. These eight states (Arkansas, Florida, Georgia, Kansas, North Carolina, Missouri, Michigan, and South Carolina) cut the duration of UI benefits to below 26 weeks of maximum benefits, which had been the standard maximum level in place for over half a century.[2]

We examine the effect of potential UI benefit duration on the duration of UI receipt, reemployment, wages, and the unemployment rate by examining the cut in UI benefit weeks implemented in Missouri in April 2011. This reduction, which occurred while Emergency Unemployment Compensation (EUC) was in effect, resulted in dislocated workers receiving up to 16 fewer weeks of UI eligibility than they would have received if they had applied previously.[3] The policy change was sudden and unanticipated; only five days passed between when the legislation was first proposed and when the law applied to UI claimants.[4] The timing was such that there was almost no opportunity for claimants to shift the timing of their claims.

We use rich unemployment insurance administrative data and wage records from Missouri and a regression discontinuity design (RDD) to estimate the effects of this policy, where the running variable is calendar time and the threshold of interest is the exact week the law was enacted.[5] The administrative data we use not only allows us to measure UI receipt, but

[2] In 2010 all states had a maximum duration of benefit eligibility of at least 26 weeks.
[3] Specifically, the maximum UI duration was cut by 16 weeks for UI recipients previously eligible for 26 weeks of regular state UI and eligible to participate in the EUC program.
[4] The legislation was a compromise aimed at breaking a Republican filibuster in the Missouri State Senate.
[5] More precisely, this is an interrupted time-series design, but we use RDD methods and for convenience refer to the design as a RDD throughout.

also re-entry into employment and wages which has not been possible in the vast majority of papers investigating UI in the United States, particularly in the post-2000 period.

Our findings indicate economically and statistically significant higher rates of exit from UI for claimants subject to the shorter benefit duration relative to claimants with the longer duration at the cutoff, resulting in an estimated sensitivity of unemployment duration to potential UI duration that is at the upper end of the literature. As found in Card, Chetty, and Weber (2007), Schmieder, von Wachter, and Bender (2012), and Le Barbanchon (2012), we find evidence that some UI recipients are forward-looking. For example, UI recipients subject to the benefit cuts had 57 weeks of eligibility, but were 12 percentage points less likely to be receiving UI by week 20 of their spell, from a base of 46 percent. We estimate that a one-month reduction of UI duration reduces the duration of UI receipt by 15 days, on average, and that approximately 54 percent of this change is through changes in exit rates occurring prior to benefit exhaustion.

Analysis of earnings records for the universe of legally employed Missouri workers indicate that those exiting early from UI largely enter employment. The estimates imply that a one-month cut in potential duration resulted in a reduction of nonemployment duration of approximately 10 days. The findings suggest that the benefit cut increased job search intensity. However, we find more limited effects of shorter benefit durations on the UI exit hazard rate after 20 weeks of UI, and for the long-term unemployed we find no evidence that lower potential duration leads to higher employment rates after exhaustion.

As in Card, Chetty, and Weber (2007), we find no significant differences in the average quarterly earnings for the first job of recipients, conditional on employment relative to the comparison group, suggesting that those induced to exit unemployment earlier are not penalized with lower wages.

The effects of extended UI on other job seekers is theoretically ambiguous. If there is job rationing, which can arise in search models with diminishing returns to labor and sticky wages (Michaillat 2012), increased search effort leads to negative externalities on other workers. However, there are no externalities in models with constant marginal returns to labor and perfectly elastic labor demand (Landais, Michaillat, and Saez 2010; Hall 2005). In models of Nash bargaining (such as Pissarides 2000) the macro elasticity of UI benefits is larger than the micro elasticity as a result of the "wage externality".

To assess spillovers, we calculate the change in the predicted path of the unemployment rate from the policy, using the shift in the survivor function estimated from the RDD, and the flow of initial UI claims. In the simulation we assume that jobseekers not affected by the UI cut are not displaced from employment by UI recipients who were exposed to the policy, or other spillovers. We compare this predicted path to the actual path of the unemployment rate from a difference-in-difference (DiD) estimate of the cut. We find that the simulated and estimated paths of the macro effect closely match. The predicted and estimated paths are approximately the same in levels, and follow a similar kinked pattern, peaking at approximately a one percentage point drop in the state unemployment rate. This effect was driven by the change in the number of unemployed. The analysis suggests the labor market absorbed jobseekers without displacement, even though the unemployment rate was high at the time of the cut at 8.6 percent. The findings are more consistent with a labor market characterized by a flat labor demand curve in the framework of Landais, Michaillat, and Saez (2010).

Our study also speaks to the question of the labor market effects of UI extensions during the Great Recession. During this period, UI benefits increased from the near-universal length of 26 weeks to up to 99 weeks in some states. Subsequently, declining unemployment led to reductions in extended benefits, and benefit duration largely returned to pre-recession levels following the expiration of the EUC program in December 2013. The labor market effects from these changes in benefit duration are a central question for labor market policy and have been the focus of a number of studies. Notably, recent papers studying this period in the United States have used state level variation in benefit lengths to estimate the effects of increases and decreases in UI potential duration over the 2007 recession period and its aftermath. The findings from these studies are mixed. Rothstein (2011), Farber and Valleta (2013), and Farber, Rothstein, and Valleta (2015) find limited effects of the UI extensions on job finding rates. Hagedorn et al. (2013) find small effects on jobseekers, but large macro effects on wages, job vacancies, labor force participation and employment. Hagedorn et al. (2015) provide evidence of possibly very large effects of cuts in UI duration in 2013 on unemployment. Our paper contributes to this literature by using a design-based approach with administrative micro data covering UI receipt, employment, and wages to study the labor market effects of changes in maximum duration in this period. While we find little evidence of moral hazard for the long-term unemployed who exhaust their benefits, we identify a fairly large response to the benefit cut for a subset of participants earlier in the spell.

II. Institutional Background

In the United States, UI is administered by state governments but is overseen and regulated by the federal government. Before 2011, eligible laid-off workers received up to 26 weeks of regular unemployment insurance benefits if they were not reemployed before their benefits were exhausted. During periods of unusually high unemployment, state and federal governments have extended potential benefit duration, to support the long-term unemployed after regular benefits are exhausted. In the 2007-2009 recession, two programs provided these extended benefits: the Extended Benefit (EB) program and the Emergency Unemployment Compensation (EUC) program.

EB is a permanent program that provides extended benefits in states with high unemployment to unemployed workers who exhaust their regular state benefits. Until recently, the federal government split the cost of EB with state governments. Through the Recovery Act passed in February 2009, Congress temporarily suspended cost sharing and the federal government bore all the cost of EB through December 2013. EB extended benefits are triggered as a function of a state's total and insured unemployment rate, and triggering thresholds vary by state. When the federal government took on all of the costs of EB, Missouri temporarily enacted legislation to implement an additional trigger that would increase EB duration from 13 to 20 weeks.[6]

Congress occasionally extends unemployment duration through federal legislation when unemployment is high. During the period of the 2007-2009 recession, the EUC program was active from June 2008 through December 2013. In the version in place at the time of the Missouri policy change, federal benefits provided longer extensions for states with higher rates of insured unemployment.[7]

The benefit cut in Missouri was the byproduct of a Republican filibuster, led by four lawmakers in the Missouri State Senate who objected to legislation that would accept federal money to extend UI benefits under the EB program. The bill would have allowed for the

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[6] If the total unemployment rate (TUR) was at least 8 percent and 110 percent of the TUR for the same 3-month period in either of the two previous years, the duration of EB would increase from 13 to 20 weeks (http://www.cbpp.org/cms/index.cfm?fa=view&id=1466).

[7] At that time EUC had four "tiers": tier 1 = 20 additional weeks, tier 2 = 14 additional weeks, tier 3 = 13 additional weeks, and tier 4 = 6 additional weeks. To move into a new tier recipieints had to exhaust the previous tier and the next tier had to be available to state residents. The availability of tiers depended on whether the three month average of the seasonally adjusted state unemployment rate exceeded a threshold set for that tier. At the time of the policy change Missouri recipients were eligible for all four tiers. However, recipients who claimed UI around the time of the policy change in April 2011 were only ever able to claim the first three tiers because the state unemployment rate fell below the tier 4 threshold in February 2012, prior to tier 3 exhaustion.

continuation of 20 additional weeks of benefits to unemployed workers who exhausted their EUC and regular benefits at no cost to Missouri.[8] The extension had already passed the Missouri State House by a margin of 123 to 14. The first news reports of the filibuster were on March 4, 2011 (Wing 2011). On April 6 a report indicated that the lawmakers had agreed to end their filibuster, though the article did not specify terms (Associated Press 2011). On April 8 the *St. Louis Post Dispatch* published the first article detailing the possible compromise. Under the compromise, regular benefits would be cut from 26 to 20 weeks in exchange for Missouri accepting federal dollars and maintaining EB benefits for the long-term unemployed (Young 2011). In effect, the agreement traded-off longer UI durations in the short run (for the long-term unemployed) in exchange for shorter UI durations in the long run. We found no press reports prior to April 8 regarding the possibility of cutting the duration of regular benefits as a possible compromise for the filibuster. This legislation appears to have been unanticipated. On April 13 the Missouri House of Representatives passed the bill which Jay Nixon, the Democratic governor, signed into law on the same day (Selway 2011). All new claims submitted after that date were subject to the abbreviated benefits (Mannies 2011).

Federal regulations calculate EUC weeks eligible in proportion to regular state UI benefits. Thus, the cut in regular state UI benefits triggered an additional 10-week reduction in EUC, and the maximum UI duration fell from 73 weeks for claimants approved by April 13, to 57 weeks for claimants approved afterwards resulting in a total change in potential duration of 16 weeks. EB did not materially affect new claimants at this time (with or without the benefit cut) because EB phased out by the time they were eligible to receive these benefits.

[8] The lawmakers leading the filibuster argued that accepting these funds would increase the federal deficit unnecessarily.

The change in potential UI duration was the only change in Missouri's UI system in the legislation. We corresponded with Missouri UI program administrators who told us that there were no changes in the administration of the program, including search requirements or communications with UI recipients. For example, they did not send additional notices informing UI recipients affected by the policy change.

For convenience, we label recipients applying for UI after the policy change the "treatment group" and recipients applying before the policy change the "control group."

III. DATA

Our analysis utilizes administrative data from the state of Missouri covering workers, firms, and UI recipients from 2003 to 2013. We use three data files for the analysis. The first is a worker-wage file detailing quarterly earnings for each worker with unique (but de-identified) employee and employer IDs. The second is an unemployment claims file that contains the same worker and employer IDs as the wage file. For each claim, we observe the date the claim was filed, the weekly benefit amount, the maximum benefit amount over the entire claim, the dates weekly benefits were issued, the wage history used to calculate benefits and duration, and the benefit regime (i.e. regular benefits, EB, or EUC). For every claim, we link the records for regular benefits, EB, and EUC claims to construct a single continuous history associated with each claim. The third dataset reports a limited set of employer characteristics including detailed industry categories. The raw data contains 1,635,993 initial UI claims over 2003-2013 and 184,191 in 2011. We remove claims ineligible for UI, including unemployed workers who were fired for cause or quit voluntarily, observations with missing claim types (regular, EB, or EUC) or missing base-period earnings, and EB or EUC claims that could not be traced to an initial regular claim. To aid in interpreting the effects, we also limit the sample to those workers who, based on their earnings histories, would have been eligible for the full 26 weeks of regular UI benefits without the policy change. Specifically, the formula for maximum potential duration of regular benefits is:

$$\text{Regular Potential Duration} = \min\left(X, \left(\frac{E}{3}\right)\left(\frac{1}{B}\right)\right)$$

where E is a measure of total base period earnings, B is the average weekly benefit, and X is 26 weeks on or before April 13, 2011 and 20 weeks after this date. Because we want to focus on workers who are affected by the cut in maximum duration we select recipients for whom $\frac{E}{3B} \geq 26$. This procedure does not induce any mechanical change in the characteristics of workers across the policy change threshold. These "full eligibility" claimants represent 72 percent of all claimants in 2011 and 67 percent of all claimants for the entire 2003-2013 period. After these screens we have 1,064,652 claims over the 2003-2013 period and 127,710 claims in 2011.

Descriptive statistics for the administrative data appear in Table 1. Column (1) reports summary statistics for the full 2003-2011 period and column (2) for 2011. The average weekly benefit in 2011 in the sample was $260. UI recipients eligible for the maximum benefit duration had an average of 14.5 quarters of tenure in their previous employer and their earnings in the last complete quarter of employment prior to collecting UI benefits was $8,259. Earnings in the first complete quarter of employment after the UI spell average $7,240. On average, recipients claiming benefits in 2011 received 29.3 weeks of unemployment benefits.

For the aggregate analysis we use data from the Local Area Unemployment Statistics (LAUS) program of the Bureau of Labor Statistics. For outcomes we use the state-by-calendar month unemployment rate, the natural log of number of unemployed, and the natural log of the size of the labor force. We deseasonalize these variables by regressing each outcome on state × month dummies over the 2001-2005 period and then deviating each outcome in 2005-2013 from the predicted value of this regression. We also use these variables derived from the Current Population Survey (CPS) to assess robustness.

IV. Empirical Design

To identify the causal effect of longer UI duration, we utilize the discrete change in the maximum UI duration resulting from a rapid and unexpected policy change: claimants who applied just before April 13, 2011 were eligible for 73 weeks of benefits and those who applied after were eligible for 57 weeks. We use this discontinuity to compare similar displaced workers entering the same labor market who experienced very different UI benefit durations. This quasi-experiment implicitly controls for labor-market conditions that may be affected by the reform.

We model the outcome variable Y_i as a continuous function of the running variable, the claim week, and estimate the outcome discontinuity that occurs at the threshold, the date of the policy change:

$$Y_i = \beta T_i + f(x_i - x') + u_i, \tag{1}$$

where x_i is the calendar week of the UI claim for person i, x' is the week of the policy change, and T_i equals one if worker i applied after the policy change and zero if she applied before.[9] Thus, $f(x_i - x')$ is a continuous function of the running variable which captures the continuous relationship between the application date and the outcome of interest. Because we control flexibly for the running variable, the model can accommodate smooth seasonal and secular changes in the labor market, allowing for unbiased estimation of the effect of the discrete policy change. To expand on this point, the unemployment rate in Missouri began to decline in the months before the policy was enacted. If our model is correctly specified, a smooth improvement in labor market conditions would be captured by the term $f(x_i - x')$. A threat to validity would

[9] We use the claim week because the data can be sparse when using the claim application calendar date, and there are days with no claims, such as administrative holidays and weekends.

be if there was a discrete change in the labor market from one week to the next at the time of the policy.

In practice we first collapse the data to the claim week level and weight the observations by the number of claims in the week, a process that yields identical point estimates to the micro data. As shown by Lee and Card (2008), heteroskedasticity-consistent inference with collapsed data is asymptotically equivalent to clustering on the running variable. We estimate the model using local linear regression (Hahn, Todd and Van der Klaauw 2001) with the Imbens and Kalyanaraman (2012) (IK) optimal bandwidth and a triangular kernel. We consider a range of alternative bandwidths to assess robustness, as well as estimation of a local quadratic using the Calonico, Cattaneo, and Titiunik (2014) (CCT) optimal bandwidth.

V. RESULTS

Diagnostics

We begin by testing for manipulation of the running variable, which would occur if claimants could strategically time their applications around the policy change. Figure 1 plots the frequency distribution of the number of UI claims by week, over the 2009–2012 period. The solid vertical line denotes the time of the policy change, and the dashed vertical lines denote the same date in the previous years. It is evident in Figure 1 that there is a great deal of seasonality in claims, with a large spike in claims around the new year. The policy change occurred after the large seasonal increase, in April, and by this time claims were at moderate levels. There is no visual evidence of an abnormal spike in claims before the policy change, as would be the case if claimants could time their applications apply for longer-lasting UI benefits. Column 1 of Table 2 formally tests for a discontinuity in claims (McCrary 2008). Estimating a local quadratic model to fit the curvature in the distribution, we find no significant discontinuity in the relative frequency of claims.[10]

Inspection of the frequency distribution does reveal a moderate jump in claims two weeks after the change in policy. As will be seen, this applicant cohort looks different in a number of dimensions from recipients who applied before or after this group, and in particular they appear to have characteristics correlated with being lower duration claimants. This outlier might be random noise, or it might reflect a failed attempt to time claims to obtain UI before the cut. To keep the analysis as transparent as possible, we keep this group in the main sample. However, we have also estimated all models excluding this cohort. Estimates presented in the Online Appendix show precise but somewhat smaller estimates on UI receipt and nonemployment when this cohort is excluded.

As a second examination of design validity, we test for discontinuities in pre-determined covariates of UI applicants around the policy change. Because there are numerous predetermined variables from which we can select, we construct an index of predicted log initial UI duration using all covariates available in the data set following the same procedure as Card et al. (2015). To construct the index, we regress log UI duration on a fourth-order polynomial of earnings in the quarter preceding job loss, indicators for four-digit industry, and previous job tenure quintiles. Figure 2 plots the mean values of the covariate index over 2009–2012 by claim week. The continuity in the index around the threshold is borne out visually, and the RDD estimate of this predicted value at the cutoff is small and statistically insignificant (column (2) of Table 2). The lack of evidence of sorting and differences in pre-determined characteristics around the threshold reinforces the claim that the policy change was unanticipated and difficult or

[10] Appendix Figure 1 displays the fitted quadratic in the frequency distribution.

impossible to game.[11]

Duration of UI Receipt

Figure 3 exhibits the mean duration of realized UI spells by application week. There is a clear drop in the number of weeks claimed as a function of the claim week. Column (1) of Panel A in Table 3 shows that the benefit reduction of 16 weeks is associated with 8.7 fewer weeks of UI benefits claimed (s.e. = 1.4), on average. Appendix Figure 2 shows the point-estimate estimated over a range of alternative bandwidths. The estimate is stable for a wide range of bandwidths, including bandwidths smaller than the IK bandwidth and up to twice as large as the IK bandwidth.[12]

We evaluate the chances of estimating a coefficient as large as ours by implementing a permutation test in which we estimate model (1) using every week outside of the winter holiday season as a placebo treatment.[13] The procedure generates 443 placebo estimates, none of which are larger than our RD estimate of the treatment week. The distribution of these placebo estimates is shown in Figure 4. The estimate for the actual treatment week is denoted by the vertical line, which is in the extreme left tail of the placebo distribution.

To evaluate whether our estimates could be driven by seasonal changes, we hone in to the estimated placebo discontinuities at the policy-change week in each of the other nine years for which we have data. While our estimate in the treatment year is -8.7, the nine placebo estimates range from -1.2 to 2.0 (Appendix Table 3).

In the Online Appendix we show that these estimates are robust to a variety of methods

[11] As previously discussed, in Figure 2 we see that the cohort receiving claims two weeks after the duration cut has substantially lower predicted durations. This pattern can be seen in all subsequent analyses.
[12] Appendix Table 1 reports the estimate excluding the negative outlier cohort two weeks after the policy change. The estimate is somewhat smaller but remains highly significant. Appendix Table 2 reports the estimate using a local quadratic model with the CCT optimal bandwidth. The estimated effect is somewhat larger than the local linear case, and statistically significant.
[13] We exclude the holiday season in November and December because of the extreme variation in average UI durations in the period due to seasonal hiring. This procedure generates 443 placebo estimates from 2003-2012.

for dealing with seasonality. Appendix Table 4 shows estimates using deseasonalized initial claims data. Appendix Table 5 removes claimants from the 25 percent of most seasonal industries as well as manufacturing. We also demonstrate that a similar decline in weeks-received does not occur in Utah, the only other state for which we have identical administrative data (Appendix Table 6).

The reduction in weeks of UI receipt is a possible combination of "mechanical" effect of earlier exhaustion for the treatment group and pre-exhaustion UI exit. We decompose the overall change in weeks of UI receipt into two parts: the part due to changes in behavior prior to exhaustion and the part due to pre-exhaustion exit. The estimated effect of the treatment on unemployment duration conditional on duration being less than 58, that is, excluding anyone exhausting, is 6.4 weeks. Because E[Duration]= E[Duration|Duration < 58] * Pr(Duration<58) + E[Duration|Duration ≥58] * Pr(Duration≥58), and Pr(Duration<58) ≈ 0.74 in the control group, approximately 54 percent (=100*(6.4*0.74)/8.7) of the change in the overall duration of UI receipt comes from changes in the response to the cut before exhaustion.

Timing of UI Receipt

To examine the timing of UI receipt in greater detail we estimate the probability that an individual remains on UI through each of the first 73 weeks of the spell. Figure 5 presents binned scatterplots of the probability that claimants remained on UI in weeks 20, 40, 55, and 60 as a function of their initial claim week. The figure shows that there is a response to the cut in maximum duration fairly early in the spell. In weeks 20, 40, and 55, before the treatment group exhausted benefits, it can be seen visually that the duration cut is associated with a lower probability of receipt. By week 60, the probability of remaining in UI for the treated group falls to about zero, consistent with all remaining claimants in the treatment group exhausting their benefits, while 26 percent of the comparison group was still receiving UI at that point. In none of these series do we see a similar break one year prior to the policy change (denoted by the dashed vertical line).

Table 3 columns (2)–(5) report the point estimates for the probability that the UI spell lasted until weeks 20, 40, 55, and 60. The RDD estimate for UI receipt is -12.3 percentage points in week 20, -11.8 percentage points in week 40, -10.1 percentage points in week 55, and -23.6 percentage points in week 60. All estimates are highly significant. These shifts are not seen in the corresponding placebo estimates in Panel B. Placebo estimates are indistinguishable from 0 in all cases except for the probability of receiving benefits in week 20, which is positive.[14] As with unemployment duration, the estimates are somewhat smaller excluding the outlier two weeks after the policy change (Appendix Table 1), and somewhat larger when estimating a local quadratic with a CCT bandwidth (Appendix Table 2) but significant in both cases.

To estimate the timing of the effects over the whole period, we fit variants of equation (1) where, in each specification, Y_i is the probability that the claimant received at least T weeks of benefits, where T spans 1 to 73. These estimates give the relative survival probabilities between the two groups by week. Figure 6 plots each of these RDD estimates with the associated confidence intervals. Figure 6 shows that the survival function diverges between the two groups starting early on in the UI spells, until around week 20 of the UI spell.

Note that there is a sharp drop in the survivor rate for the treatment group in week 20 and a similar drop for the comparison group in week 26. These drops represent individuals who did not receive benefits beyond the regular state benefits, either because they were ineligible since

[14] Because Easter was on April 24, 2011, we also estimated a placebo specification setting the policy change just prior to Easter 2010. We found no significant effects for this placebo as well suggesting that our estimates are not being driven by this holiday.

the federal government automatically enrolls the eligible, or did not enroll for other reasons.[15] Because of these drops in the survivor rate at regular benefit exhaustion date, we do not interpret the 20–26 week span because any differences over this term reflect a combination of eligibility and behavioral effects.

Excluding this 20–26 week period, the treatment-control differences in the survivor rate are relatively stable from week 20 of the UI spell through week 57, at which point there is a significant drop in the relative survivor rates as the treatment group exhausts EUC benefits while the control group continues to receive EUC benefits until week 73. The error bands in Figure 6 show that the first significant difference between the two groups occurs in week 14, and the differences remain significant for all subsequent weeks. These estimates indicate claimants respond in a forward-looking way to UI exhaustion, and much of the response to the duration cut occurs fairly early in the spell, within the first three months. This time pattern of exit is robust to alternative bandwidths. Appendix Figure 3 shows the same plots with double the IK bandwidth and the pattern persists.

We can use the estimated survival functions to estimate the average change in the hazard rate. In Panel A of Figure 7 we show the level of the survival rate for the control and treatment groups which underlie Figure 6. A point in the survivor curve for the control is the constant in the local linear regression used to estimate a weekly estimate in Figure 6. The treatment series is the corresponding intercept for the treatment group. The difference in these two series is Figure 6. Panel B shows the survivor functions in logs. The slope of these functions times -1 is the

[15] It is also possible that this dip could be the result of unmatched administrative claims data. The raw administrative data has a separate record for each type of claim (regular benefits, different EUC tiers, extended benefits). We matched the records to form a continuous history. To the extent that we couldn't match regular benefits to EUC records this pattern would emerge. However, we believe that it is unlikely that this slippage plays a major role in this pattern since the different tiers of EUC are also separate records, and we would therefore expect to see similar step patterns at all points where these transitions occur, which we do not.

hazard rate. To compute the hazard rate we first smooth the survivor functions separately over weeks 1-20 and 26-57 for the treatment group and weeks 1-26 and 26-73 for the control group. We use these separate segments so as to not have the function be influenced by the drop in the survivor function due to regular UI recipients not claiming EUC. We then numerically differentiate these smoothed functions. The derivatives times -1 are plotted in Panel C. The difference in the estimated hazard rates are shown in Panel D.

This exercise reveals several features about the response of recipeints to the cut in benefits. As can be seen in Figure 6, the largest response ocurrs in the first 20 weeks of the spell where the hazard is approximately 1 percentage point higher in the treatment than the control. However, the exit hazard in the treatment remains elevated after 26 weeks, by about half a percentage point, something that is not necessarily apparent when looking at raw survivor functions in Panel A. On average, the treatment group has a 30% higher exit hazard than the control over the first 57 weeks of the UI spell. This translates into a large elasticity of exit hazard with respect to the cut of 1.36.[16] A second interesting feature is that, consistent with Meyer (1990), there are spikes in the exit hazard prior to exhaustion. This can be seen both for the treatment and control groups approaching the EUC exhaustion weeks.[17] There is also some evidence of a slightly elevated hazard rate in the treatment group approaching the 20 week mark. This might indicate that some recipients mistakenly believed that they were not going to receive more than 20 weeks of UI.

Employment

Using the quarterly wage files we can measure the employment rate for the treatment and control groups following the policy change. Figure 8 plots the employment rate by UI

[16] The policy resulted in a 22% change in potential UI duration (16 weeks from a base of 73).
[17] We have also estimated hazard rates of UI exit for other period and have found consistent evidence of a spike approaching exhaustion.

application week for four quarters after the benefit cut. Consistent with the pattern seen for UI exits, in 2011 Q3—the first full quarter after the cut—there is a noticeable jump in the employment rate for applicants claiming after the duration cut. The elevated employment rate for the treated group can also be seen in 2011 Q4, 2012 Q1 and 2012 Q2.

Figure 9 presents the RDD estimates and associated 95 percent confidence intervals for employment rates by quarter, starting in the quarter the policy went into effect (the second quarter of 2011) through the second quarter of 2013. The RDD estimate for employment is insignificant in 2011 Q2, the quarter of the policy change. In 2011 Q3—the first complete quarter after the duration cut—the treated group has a 11.9 percentage point higher employment rate than the comparison group. The difference in employment rates is similar to the 10-12 percentage point difference in the probability of receipt in the early part of the UI spells over the relevant range, suggesting that those individuals who leave UI before exhaustion tend to enter employment. The employment effect fades out by 2012 Q4 at which point both treatment and control have exhausted their benefits. The point estimates and standard errors for the employment RDD are presented in Table 4.[18, 19]

Conveniently, the 16-week period when the treated group had exhausted benefits and the control group was still eligible for benefits covers the entire third quarter of 2012 (as well as part of the second quarter of 2012). Therefore, to assess the effects of benefit exhaustion for the long-term unemployed in the treatment group, relative to the control who still received benefits, we can look at the change in the relative employment rate between the two groups in 2012 Q3 relative to earlier quarters. If exhausting benefits results in people scrambling and successfully

[18] Appendix Table 7 reports local linear estimates excluding the outlier cohort two weeks after the policy change. Appendix Table 8 reports local quadratic estimates with the CCT optimal bandwidth. We continue to see significant employment effects in both cases.

[19] Appendix Figure 4 shows the same charts using twice the IK bandwidth.

finding employment, we would expect to see an increase in the RDD estimate for employment relative to the estimate in the previous quarter and the subsequent quarter. This is not what we find; rather, the relative employment rates in the treatment and control groups fell over the period. This suggests that, for the long-term unemployed who did not respond to the policy early, exhausting UI benefits did not hasten reemployment relative to the control. Instead, the positive employment effects we observe come from the group of UI recipients who responded to the changing weeks of eligibility well before exhaustion. A caveat to this conclusion is that at the time the treatment group exhausts UI benefits the composition of the two groups differs since there were more exits from UI in the treated group among the "forward-looking" subset of participants. It is possible that an increase in the exit rate from this group in the control masks any positive effect of exhaustion on employment in the treatment group.

Figure 10 shows the "placebo" estimate for the employment effect of the benefit cut. Specifically, we estimate the same model with quarterly employment outcomes for quarters starting one year prior to the duration cut, setting the placebo duration cut to April 2010. There are no significant employment estimates over this period. Appendix Figure 5 shows the placebo distribution for employment probabilities in Q3 2011, Q4 2011, and Q1 2012 for placebo weeks that range from one month prior to the actual policy change to six months after. This was a period of improving labor market conditions for Missouri. The unemployment insurance spell duration outcome is included for reference. The figure shows that the estimates for the real policy change week are at the extreme tail of the placebo distribution, demonstrating that our estimates are not simply capturing smooth improvements in the labor market.

We can use the estimates corresponding to the relative nonemployment probabilities by quarter (shown in Figure 9) to calculate the expected difference in the duration of mean nonemployment between the two groups. If we assume that the relative employment probabilities between the two groups are the same after the third quarter of 2012, after which point all recipients have exhausted their benefits, summing the estimates in Figure 9 from the quarter of the policy change through 2012 Q3 implies that a one-month reduction in unemployment duration reduces the number of days of nonemployment by an average of 10.4 days, with a 95 percent confidence interval of (6.7,14.1).[20] This confidence interval implies an approximate elasticity of nonemployment with respect to potential unemployment duration in the range of 0.37-0.78. This elasticity is only an approximation because we are assuming that UI exit prior to exhaustion is into employment, as well as a particular exit hazard rate into employment for UI exhaustees. Both of these assumptions are required to compute an average nonemployment duration in the baseline.[21]

Reemployment Earnings

A class of job search models predict that longer provision of unemployment benefits allows workers to increase their reservation wage and find a more desirable job match. Longer UI duration could also depreciate human capital resulting in lower wages. The literature has mixed findings on the relationship between UI benefit duration and reemployment wages. Card Chetty, and Weber (2007) found no significant effect of delay while Schmieder, Von Wachter, and Bender (2013) find that workers with longer potential UI spells have lower wages. We find that post-employment earnings do not change significantly following the cut in duration. Figure

[20] The confidence interval, which is constructed from the standard errors for each quarterly estimate, assumes no covariance term between the RDD estimates of employment by quarter.

[21] This range is calculated as follows: the percent change in potential unemployment duration was 22%. The confidence interval implies that the policy increased the number of days of nonemployment by 26.8-56.4 days, or 3.8-8.1 weeks. 80% of the control group exited before UI exhaustion and their average duration was 27.6 weeks. We assume that these recipients entered employment We do not have a nonemployemnt spells for exhaustees. If we assume a hazard rate into employment of 2% at the times of exhaustion, which is roughly what Figure 7 implies, this implies a mean duration of 73+1/.02=123 weeks for exhaustees and an overall average duration of 46.7 weeks. This yields a nonemployment elasticity in the range of 0.37-0.78.

11 shows mean log reemployment earnings for the first complete quarter after the individual has been reemployed, by application week.[22] There is no evidence of a break at the threshold, a finding that is confirmed by the positive and insignificant estimate on the log reemployment wage outcome in column (5) of Table 4.

VI. Reconciling the Individual and Market-Level Effect of the Policy

We have documented fairly large responses of the duration of UI receipt and nonemployment to changes in potential duration. In this section we ask how the cut affected the aggregate unemployment rate and, further, what the relative magnitude of the change in the unemployment rate and the change implied by the RDD estimates implies about possible spillovers, particularly displacement effects from the treated group crowding out other jobseekers. To this end, we estimate DiD models comparing the unemployment rate in Missouri to a comparison group of states.[23] We then compare the estimated change in the Missouri unemployment rate over the period to the change in the unemployment rate predicted by the estimated change in the survivor function from the RDD models, assuming no market-level spillovers. A comparison of the two series is informative about the degree of spillovers.[24]

The challenge for estimating the effect of the policy change in Missouri is in constructing a reasonable counterfactual. The policy change occurred during the recovery of the 2007-2009 recession, and it is well known that states differed in the shocks they experienced and the strength and speed of the labor market recoveries. Over the period there were shocks to housing (Mian and Sufi 2012), manufacturing (Charles, Hurst and Notowidigdo (2016)), and credit

[22] Our data contains information on quarterly earnings.
[23] Hagedorn et al. (2014) conduct a similar analysis for a UI duration cut in North Carolina.
[24] Our design is best suited for capturing the "crowding" general equilibrium effects emphasized by Landais et al. (2010). A caveat is that there are general equilibrium effects that are likely not detected by this research design. For example, we may not be able to detect the effects of changes do to gradual firm adjustment to UI policy.

(Chodorow-Reich (2014), Greenstone, Mas and Nguyen (2015)). These shocks had different regional distributions, and it has been found that the labor market recovery varied by region (Yagan 2016). For this reason, we experiment with a number of approaches for estimating counterfactuals in order to match Missouri to similar states with respect to the labor market dynamics, as well as to assess robustness.

In Figure 12 we plot the raw difference between the deseasonalized unemployment rates in Missouri and the average of all other states by month. The figure shows what appears to be a decline in the unemployment rate in Missouri coinciding with the duration cut as we see a relative reduction in the Missouri unemployment rate, peaking at just over 1 percentage point, following the April 2011 cut.[25]

In Figure 13 we compare Missouri to a synthetic control using the method of Abadie and Gardeazabal (2003) and Abadie, Diamond, and Hainmueller (2010) which assigns weights to states as to minimize the mean squared prediction error between the treatment and control states in the pre-intervention period for a set of outcomes. To construct weights for the comparison group, we use as predictors the unemployment rate for each quarter from January 2009 – March 2011, the percent of employment in agriculture, mining, utilities and construction, the percent of employment in manufacturing, the percent of employment in retail and wholesale trade, the percent change in housing values from 1999-2006, the percent change in housing values from 2007-2010, and the percent of the state population that is living in rural areas.[26] We exclude from the donor pool any state that cut UI duration prior to 2013.[27] The figure plots the Missouri

[25] Appendix Figures 6 and 7 show the raw unemployment rates for Missouri and the comparison groups, without seasonal adjustment, using LAUS and CPS data respectively. The comparison groups used are all states, neighboring states, and a weighted average of the unemployment rate using the synthetic controls described below.
[26] The housing values are annual state-level indices for the value of single-family homes from the Federal Housing Finance Agency. The percent of population that is rural is from the 2010 decennial census.
[27] The procedure assigns weights of 10.5% to Arizona, 21.6% to Connecticut, 13.1% to Delaware, 42.2% to Kentucky, 1.2% to Minnesota, 10.7% to North Dakota, and 0.8% to Oklahoma, and 0 to all other states.

unemployment rate against the weighted unemployment rate for the synthetic control. The figure shows a similar drop as when we use the unweighted comparison group of states, with the relative unemployment rate declining, peaking at almost a one-percentage point decline, and then gradually reverting back to the control.

Figure 14 uses the simple average unemployment rate of states that border Missouri as the control. The motivation for this comparison is evidence that there are important regional patterns in the cyclical pattern of unemployment (Yagan (2016)). The disadvantage relative to the synthetic control approach is that we lose the ability to compare states with similar characteristics that are not necessarily regionally concentrated, such as industry and housing price dynamics. The drop in the unemployment rate also has a similar pattern as when we use all states as the control group, though the fall in the unemployment rate appears even more pronounced and more persistent in this comparison.[28]

Note that in these figures there appears to be some decline in the unemployment rate in Missouri relative to the control states a few months *before* the policy change. While this decline is not large, a reasonable concern is that we are detecting a pre-treatment change in trend in the Missouri unemployment rate. In Figures 12-14 the month of April is marked with a dotted vertical line for all years. It can be seen that even with seasonally adjusted data there is a somewhat different seasonal pattern between Missouri and the comparison groups, with Missouri exhibiting a pattern of sharper declines in unemployment from December through April. It is therefore very difficult to distinguish the small decline in the unemployment rate we observe prior to the policy change between a typical seasonal fluctuation and a secular change in trend. Given the evidence from the micro analysis which point to large changes in unemployment

[28] Appendix Figure 8 shows the synthetic control approach just on border states. This figure also shows a similar pattern of falling unemployment after the policy change, though with more of a positive trend between the treated and control groups prior to the policy change.

durations, we believe it is reasonable to conclude that the patterns in these figures are driven by these changes in policies.

Next we compare these relative changes in the state unemployment rate to the changes in the unemployment rate predicted by the RDD estimates assuming no spillovers. For every week τ relative to the week of the benefit cut (τ=0), we compute the predicted change in the number of unemployed ($\Delta\hat{n}_\tau$) due to the policy as:

$$\Delta\hat{n}_\tau = \sum_{t=0}^{57}(\hat{p}_t^T - \hat{p}_t^C) * c_{\tau-t} + \sum_{t=58}^{73}(-0.05) * c_{\tau-t},$$

where $c_{\tau-t}$ is the number of initial UI claims in week $\tau - t$ if $\tau - t \geq 0$, $c_{\tau-t} = 0$ if $\tau - t < 0$, and $\hat{p}_t^T$ and $\hat{p}_t^C$ are the estimated probabilities that UI recipients are receiving benefits t weeks into the spell for the treatment and control groups respectively. An underlying assumption, which the analysis above supports, is that pre-exhaustion exits out of UI represent moves out of unemployment and into employment. For UI recipients who first received benefits 58–73 weeks prior to the week of April 13, we make the assumption that the relative difference in the relative exit rate out of unemployment between treatment and control is the RDD estimate for the employment probability outcome in 2012 Q3. We assume that after 73 weeks, beyond the duration of the program in the control period, there are no differences in relative unemployment exit rates, an assumption that is consistent with the insignificant employment probabilities between the two groups after they both exhaust. We then compute the predicted change in the unemployment rate in each week after April 13, 2011 as $\Delta\hat{n}_\tau / l_\tau$, where l_τ is labor force participation.

Figure 15 plots the predicted change in the state unemployment rate by week against the DiD estimates (by month) of the change in the Missouri unemployment rate expressed relative to the value in March 2011, the month before the cut. The DiD estimates not only line up closely to the predicted change, but the series exhibits a similar kinked pattern with the unemployment rate declining, peaking at close to 1 percentage point and kinking up at approximately same time as the predicted change. It appears that the assumption of no spillovers used to form the predicted response is appropriate as the increased exit rate of the UI applicants translated into a lower unemployment rate. Appendix Figure 9 shows the predicted change using estimates without the outlier cohort. Here the drop in the unemployment rate exceeds the predicted amount.

Table 5 reports the estimates for the DiD models fit over the 2009–2013 period and with the intervention period defined as April 2011 through December 2013. The unit of observation is at the month-by-state level, and we estimate all models with state fixed effects, calendar month dummies, interaction of time (calendar month) with the same set of state characteristics used in the synthetic control match, and with and without a Missouri-specific trend.[29]

Computing standard errors is complicated in cases where there is only one intervention unit. The primary concern when using grouped data in a DiD analysis is how to account for possible serial correlation (Bertrand, Duflo, and Mullainathan 2004). Though we use data from all 50 states and the District of Columbia, we cannot cluster on state because the relevant degrees of freedom is the number of intervention units (Imbens and Kolesar 2012), which in this case is a single state. As an alternative, we employ a number of different approaches for inference. For the unweighted DiD estimates we report OLS standard errors, panel-corrected standard errors, confidence intervals from a wild bootstrap using the empirical t-distribution (Cameron, Gelbach, and Miller 2008), and the percentile rank of the coefficient from a permutation exercise where we estimate a placebo effect of the cut for every state for the post-April 2011 period. We also employ tests from Ibragimov and Müller (2014), which are discussed below. For the synthetic

[29] We have also estimated models with state-specific trends, which yield almost the same point estimates. However, these models are not well suited for bootstrapping so we opted for the more parsimonious model.

control estimates, we report the percentile rank from the permutation exercise. Specifically, for every state we form its state-specific synthetic control and compute the mean difference in the outcome between the state and the state-specific control as if the state were treated. Table 5 also includes the average post-intervention predicted change in the unemployment rate from the RDD estimates, which can be compared to the DiD estimates to assess the degree of spillovers. We show these both for the main estimates and the estimates excluding the outlier cohort.

In Panel A the DiD estimate using the unweighted control is -0.89 percentage points (column 1), and -0.80 percentage points with a Missouri-specific trend. These estimates are interpretable as the difference in the Missouri unemployment rate in the period April 2011-August 2013 relative to January 2009-March 2011 and relative to the average change in all other states. The estimates are statistically different from 0 as well as from the predicted change in the unemployment rate, in both models using OLS standard errors, panel corrected standard errors, and the wild bootstrap confidence intervals. The percentile ranks are 5.9% (column 1) and 2.0% (column 2) meaning that in specification 1, 5.9 percent of states have more negative estimated effects while in specification 2, 2 percent of states have more negative estimated effects. Column (3) presents the synthetic control estimates. The DiD point-estimate is -0.85, which has an associated percentile rank of 3.9%. These estimated average changes in the unemployment rate are larger than the predicted change in the unemployment rate, possibly twice as large if we use the predicted change excluding the outlier cohort.

We estimate these models in Panel B using the Missouri neighbors comparison group. In these models we do not control for state characteristics interacted with time since there are too few degrees of freedom to identify these effects, but otherwise the models are the same as in Panel A. The estimates are similar in magnitude to when using all states. There is an estimated decline in the unemployment rate of 1.0, 0.76 and 0.70 percentage points without trends, with Missouri-specific trends, and with the synthetic control respectively.[30] All of these estimates are significant and are the largest estimated effects when permuting the treatment through this set of states (the percentile rank is 0).

Next we separately look at the numerator and denominator of the unemployment rate. In Table 5 columns (4)–(6) we estimate the same models using the log of the number of unemployed as the dependent variable. Across specifications, we see large and significant declines in the number of unemployed, in the range of 10–12 percent depending on the specification. These estimates are close to the predicted change in the number of unemployment from the RDD estimates of 10 percent, and about 30% larger than the predicted value when excluding the outlier cohort. Columns (7)–(9) report the estimates for log size of the labor force. The estimates tend to be small and insignificant negative estimates, with the exception of the synthetic control estimate that uses neighboring states that is fairly large at -3 percentage points and significant. This last estimate suggests that there may have been some negative impact on labor force participation, though the specifications with log of the number of unemployed imply that a large portion of the effect on the unemployment rate is through changes in the number of unemployed, consistent with the micro evidence.[31]

We have also computed p-values for the DiD estimate of the effect of the policy change on the unemployment rate based on the approach of Ibragimov and Müller (2014). To implement this test we limit the sample to 28 months on each side of the policy change, and collapse the

[30] The synthetic control is constructed using the same matching variables described in Figure 13 but using only neighboring states. We exclude Arkansas from the donor pool because it changed benefit durations over the same period. The control group consists of the following weighted average of states: 38.7% Illinois, 5.6% Nebraska, and 55.7% Kentucky.

[31] In Appendix Table 9 we reproduce this analysis using these measures derived from the Current Population Survey. The magnitudes are close to those from LAUS, and while noisier they are still reasonably precise in most specifications. This analysis shows that our estimates are not driven by how the LAUS data are constructed.

monthly difference between the Missouri and the average of the comparison group unemployment rates (denoted for convenience $U_{MO\text{-}CO,t}$) into blocks of months of varying sizes (28, 14, 7, 4, 3, and 2 blocks in each of the pre and post periods). We then conduct a two-sample t-test of equality of $U_{MO\text{-}CO}$ in the pre and post periods using the collapsed data and N-2 degrees of freedom. In these tests the sampling variances are estimated from variation in $U_{MO\text{-}CO}$ across blocks of months, and in doing so we assume independence of $U_{MO\text{-}CO}$ across blocks of months, but allow for arbitrary correlation within blocks. Under the conventional assumption of weak dependence in time series data, observations that are far apart will be less correlated to each other than those close together, and we would therefore expect less auto-correlation when grouping more months together into larger blocks than smaller blocks. By comparing p-values across block groups we can assess the degree to which the inference is serially robust. Looking across the columns of Table 6, this indeed appears to be the case. For the unweighted and synthetic controls we can reject equality of the pre and post period values of $U_{MO\text{-}CO}$ for all block groupings, even when we collapse the sample to just two blocks on either side of the cut-off, where auto-correlation should be minimal. Appendix Table 10 shows the same test for the CPS derived sample.

In Table 7 we further control for regional shocks by narrowing the estimation to those counties that straddle the Missouri state line. These border estimates look very similar to those from the state-level analyses, with estimated changes in unemployment rates of approximately 0.8 percentage points, 9 percent declines in the number of unemployed, and no detectable changes in labor force participation.

Our conclusion from the cumulative findings is that there is reasonably strong evidence that the increase in exit rates translated into a lower unemployment rate. Moreover, while an important caveat is that in a single unit intervention it is not straightforward to compute correct standard errors, the point-estimates suggest that there were limited displacement effects due to the higher employment rates from the treated group. This analysis also supports another assumption: that the behavioral response is not local to the time of the policy change. If the effect were transitory, we would not expect to see a pronounced and growing change in the state unemployment rate.

VII. DISCUSSION

The UI estimates imply that a one-month reduction in potential UI duration leads to a 0.5 month reduction in compensated UI spells and a 0.3 month reduction in nonemployment. The implied elasticity of the UI exit hazard with respect to the cut in potential duration is approximately 1.4, and we estimate an elasticity of non-employment in the range of 0.37-0.78. These estimates are towards the upper-end of the literature.

To situate the magnitudes, we discuss a subset of papers in the literature that are representative of the range of estimates. Among European studies, the marginal effect for nonemployment is close to van Ours and Vodopivec (2008) (me ≈0.4), women in Lalive (2007) (me ≈ 0.4), women in Lalive (2008) (me ≈ 0.28), Le Barbachon (2012) (me ≈0.3), Landais, Lalive, and Zweimuller (2015) (marginal effect = 0.3), and Centeno and Novo (2009) (me ≈ 0.25), but higher than Card, Chetty, and Weber (2007) (me ≈ 0.1) and Schmieder, Von Wachter, and Bender (2012) (me ≈ 0.1). The elasticity of nonemployment is close to Lalive (2008) (elasticity ≈ 0.37 for men and 0.56 for women), Centeno and Novo (2009) (elasticity ≈ 0.45), but smaller than Card, Chetty, and Weber (2007) (elasticity ≈ 0.1) and Schmieder, Von Wachter, and Bender (2012) (me ≈ 0.12). U.S. studies include Leung and O'Leary (2015) (me ≈ 0.12) (for a population of workers who are on the margin of UI eligibility), Landais (elasticity ≈ 0.34), and

Solon (1979) (elasticity for UI repeaters ≈ 1 , insignificant for non-repeaters).

We are able to make better comparisons to US studies by comparing estimates of the effect of potential duration changes on UI spells, and UI exit hazard rates. Our estimated marginal effect of UI spell duration of 0.5 is higher than Katz and Meyer (1990) (me ≈ 0.2) and Card and Levine (1990) (me ≈ 0.08) and closer to Landais (2015) (me ≈ 0.2-0.4). Our estimated elasticity of UI exit hazard with respect to potential duration is substantially higher than Moffit (1985) (elasticity ≈ 0.16), Card and Levine (1990) (hazard elasticity ≈ 0.34) and Katz and Meyer (1990) (hazard elasticity ≈ 0.3) but closer to Landais (2015) (hazard elasticity ≈ 1.35) and Solon (1979) (weeks unemployed elasticity ≈ 1).

It is not surprising that estimates from some of these studies differ from the estimates reported here since they tend to be from the 1980's and early-1990's in the US, or European countries where the labor market institutions are quite different. In many European countries baseline durations are long, and recipients have access to means-tested welfare programs after UI exhaustion. Given the range of estimates, there is no reason that this relationship is necessarily stable. It is also possible that the response to a potential duration cut is larger than an increase. Few studies have examined cuts to potential duration, but one study that does, van Ours and Vodopivec (2005) in Slovenia, also finds large effects on UI exit and job finding rates (elasticity of exit rate with repect to potential benefit duration ≈ 0.9 – 1).

In a related study that uses nationally representative data from the same period, Hagedorn, Manovskii, and Mitman (2015) estimate the labor market effects of the expiration of Emergency Unemployment Compensation in December 2013. They estimate that the EUC expiration resulted in 954,000 fewer unemployed.[32] Our estimates applied to the national data imply an effect of close to this magnitude. At the time of the expiration there were

[32] They also estimate that another 1.1 million people entered the labor force as a result of the failure to extend EUC.

approximately 4.7 million UI recipients who either had expiring benefits or were going to face expiring benefits over the first half of 2014 (Council of Economic Advisors and Department of Labor 2013). The average reduction in UI duration due to this expiration was 53%. Our estimates imply that a 22% reduction in benefits (16 weeks from a base of 73) led to a 10 percent reduction in the number of unemployed. Applying our estimates directly, a 53% cut in benefit duration implies a 24% reduction in the number of unemployed. This translates to 1.1 million fewer unemployed from a base of 4.7 million. This is very close to the Hagedorn, Manovskii, and Mitman (2015) estimate.[33]

Our findings on the relationship between UI extensions and unemployment differ from Rothstein (2011) and Farber and Valletta (2013) who find only a small relationship between UI extensions and unemployment, most of which is the result of changing labor force status. One explanation, of course, is that they use national data and are looking at changes in extended benefits while we use data for one state and we look at changes in both regular and extended benefits. It is also possible that our findings diverge for statistical reasons. These papers rely on CPS data where unemployment durations and eligibility have to be estimated. This may result in measurement and misclassification error that could attenuate their estimates.

Another finding in our paper is that the increased hazard rate out of unemployment insurance occurs in the first twenty weeks of the UI spell and then stabilizes. There is evidence in the literature of this kind of anticipatory effects (Schmieder, Von Wachter, and Bender (2012); Card, Chetty, and Weber (2007); Le Barbachon (2012), Landais (2015)). It is possible that the media attention following the policy made the duration cut more salient in the minds of some UI recipients, resulting in increased search intensity. However, this explanation would imply that

[33] Our estimated macro effect of the cut is also larger than Marinescu (2014) who estimates that a 10 percent increase in benefits corresponds to a 0.7 percent decline in the unemployment rate. See also Coglianese (2015) and Chodorow-Reich and Karabarbounis (2016).

the change in behavior is mainly local to the time of the cut, and less pronounced for subsequent cohorts of UI recipients. As discussed, since the path of the unemployment rate tracks the predicted path, which is based on the assumption that the change in the survivor function is permanent, this explanation is less compelling.

Another explanation for the forward-looking behavior is that recipients were confused by the policy change, believing that the cut would give them only 20 weeks of benefits and not the federal benefits which were an additional 37 weeks. This explanation is attractive because it would imply smaller UI hazard elasticities, since some of the recipients would have then believed the cuts to be substantially larger than those implemented. It is possible that recipients interpreted the law in this way, but our review of media reports and Missouri communications to UI recipients provide no evidence that the information disseminated would lead to this kind of confusion. The media coverage at the time emphasized that the reduction was a compromise to *preserve* extended benefits (e.g Young 2011). The initial packet sent to claimants before and after the law change was identical and did not explicitly state the number of weeks of eligibility for regular UI. Rather, the report states the maximum benefit and the weekly benefit. The number of weeks of eligibility would be derived from the ratio of these two numbers (see Appendix Figure 10 for an example of this document). No other wording was changed and no information about extended benefits was provided in the initial packet for either the treatment or control group. Instead, the claimants were informed whether extended benefits were in effect when they logged into Missouri's UI website (MODES) and they also received a call informing them that extended benefits are available. When the claimant exhausted their benefits they were reminded in correspondence that EUC was available and eligible claimants were automatically enrolled. These procedures did not change with the law. Because the policy change was clearly described even in the headlines, and the information regarding regular and extended benefits were continuous at the time of the policy change, we find it difficult to sustain an argument that policy understanding was affected discontinuously at the threshold. However, the presence of a small spike in the UI exit hazard prior to 20 weeks in the treatment group might indicate some confusion. If this was the case, it is interesting that some exiting recipients responded well before the 20-week mark and were largely able to find employment.

We find that the long-term unemployed who exhausted their benefits did not have higher rates of reemployment relative to the group that remained on UI. This can be seen most clearly in the comparison of employment rates during the period that the treated group had no benefits remaining while the comparison group remained eligible. There is no evidence that the employment rate rose for the group exhausting benefits during this period (with the caveat that the control group at this point has a different composition near exhaustion as it contains a subset of the “forward-looking” types). This finding suggests that the benefit cut increased reemployment rates for a subset of individuals who responded early in the spell, but for the remaining recipients UI continued to serve an insurance function with limited moral hazard response. As the optimal UI literature suggests, our results suggest that policymakers have to tradeoff between moral hazard and insurance when determining the duration of UI.

Finally, we provide direct evidence on the relative magnitudes of the micro and macro elasticities with respect to potential UI duration. Unlike Lalive et al. (2015), we find that the macro elasticity is at least as large as the micro elasticity. Within the framework of Landais et al. (2010), this finding is consistent with a horizontal aggregate labor demand curve. The finding suggests that the assumptions of the Baily-Chetty model of optimal UI [Baily (1978), Chetty (2006)], which assume no spillovers, are appropriate in this setting, providing empirical support for the theoretical predictions of Kroft and Notowidigdo (2015). The "micro" marginal effect of potential duration in Lalive et al. (2015) is close to the one we find (≈0.3), but the "macro" response differs. While we cannot pin down why this finding is different there are several important differences in the programs and the settings. Lalive et al. study a policy that took place 23 years prior to the one we study, in a country with different institutions and economics circumstances.[34] In terms of the policies, there are also differences: they examine a benefit increase rather than benefit cut, the Austrian program was intended to be an early-retirement program and was targeted to a region that experienced restructuring in the steel sector. Given these differences it is not necessarily surprising that the relationships differ, and it suggests that the relationship between the micro and macro elasticity may depend on the particular setting.

An important caveat for interpretation of our findings is that this is a single state study. Appropriate caution should be taken when extrapolating these estimates to other settings. At the same time Missouri is a fairly typical state in many respects. Appendix Table 11 compares the characteristics of Missouri to the rest of the US. Missouri's demographic and labor market characteristics look fairly similar to the average of the other states in many, though not all dimensions. Using the characteristics in the table we investigate Missouri's "representativeness" by summing each state's rank-distance from the national median for each variable. Using this criterion, Missouri is fifth closest to the median in these characteristics across all states.

We also note that while the seasonally-adjusted Missouri unemployment rate was high at the time of the benefit cut, at 8.6 percent, the labor market nationally was mending, and the finding that the market largely absorbed the larger number of workers exiting UI without

[34] Institutional differences include the availability of means-tested benefits after unemployment benefit exhaustion, availability of benefits for voluntary quitters after a waiting period and near universal collective bargaining coverage. The Austrian labor market was also stronger, by at least some measures. The unemployment rate in Austria at the time of the policy was only 3.6% (OECD) as compared to 8.6% in Missouri.

displacement may not hold when the unemployment rate is even higher or on an upward trajectory.

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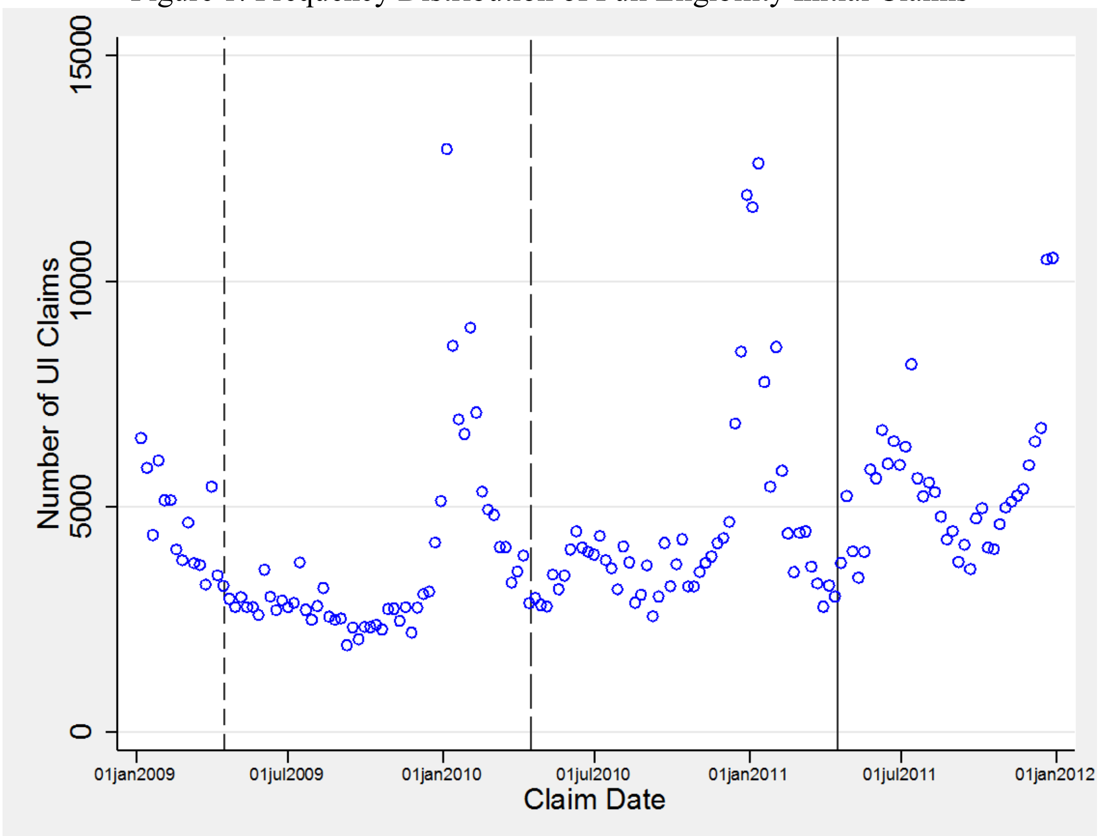
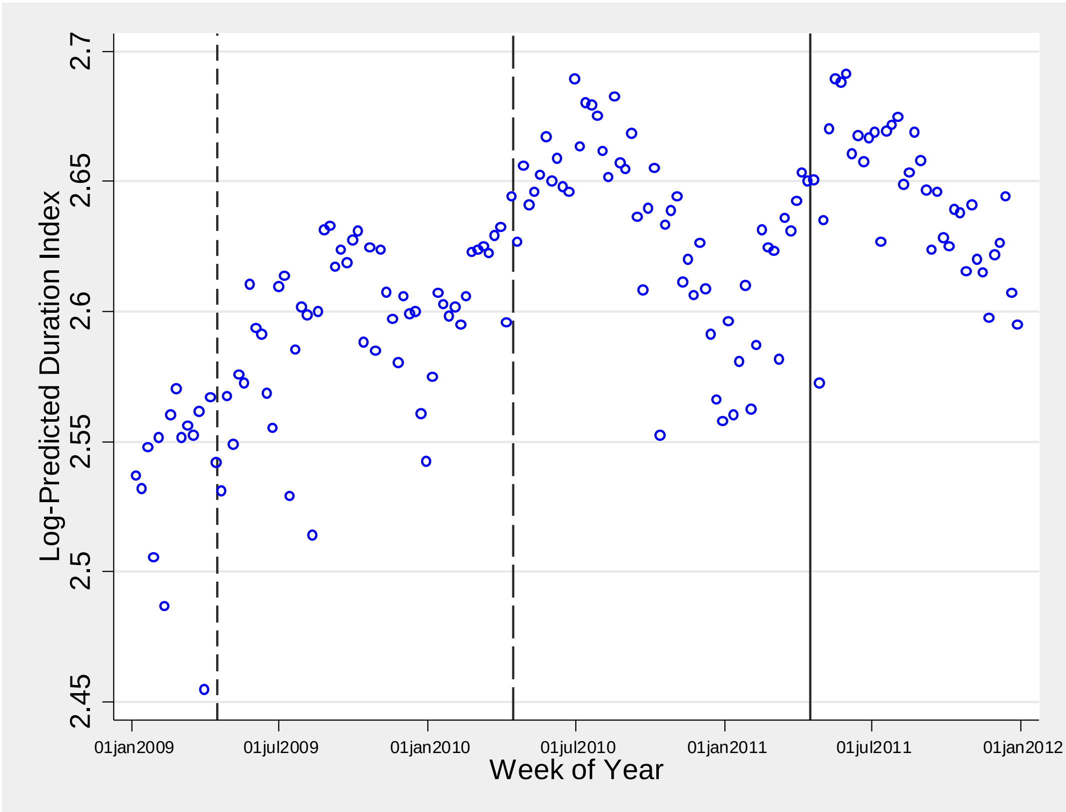
Figure 1: Frequency Distribution of Full Eligibility Initial Claims



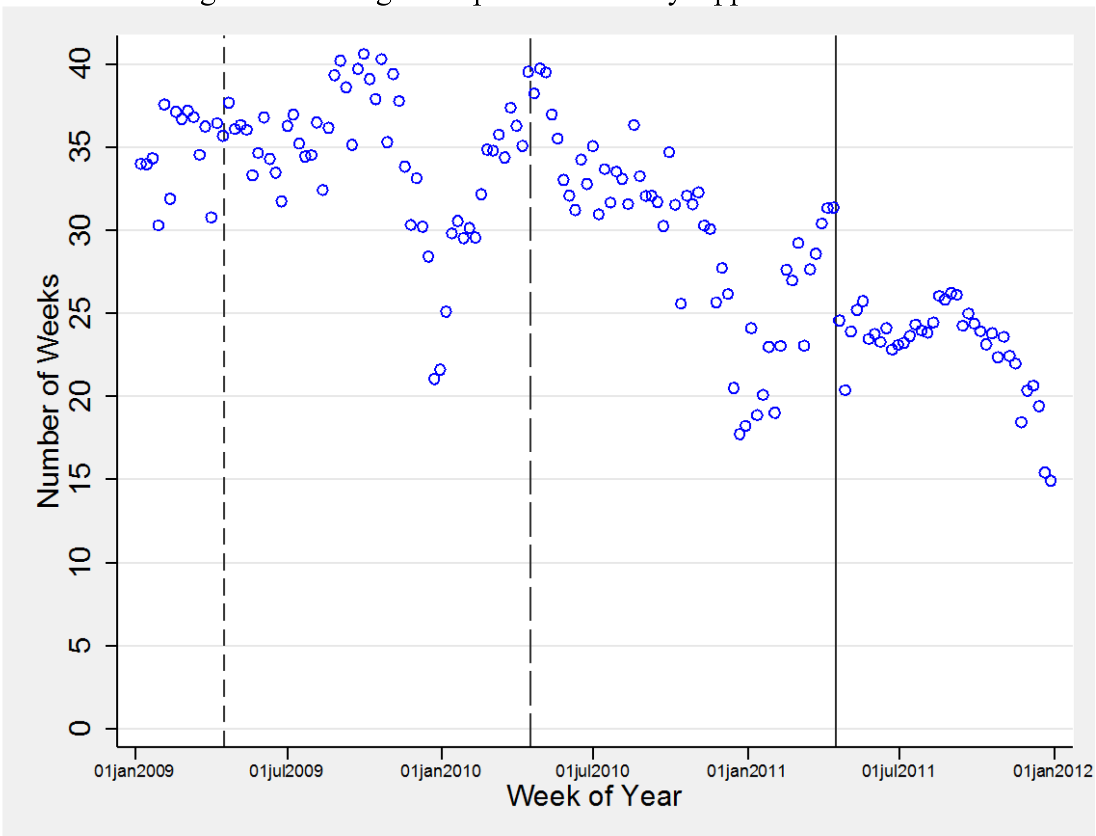
Notes: This figure plots the number of initial UI claims for workers eligible for the maximum duration of regular benefits (26 weeks before the cut and 20 weeks after the cut) by claim week.

Figure 2: Predicted Log Initial UI Spell Duration



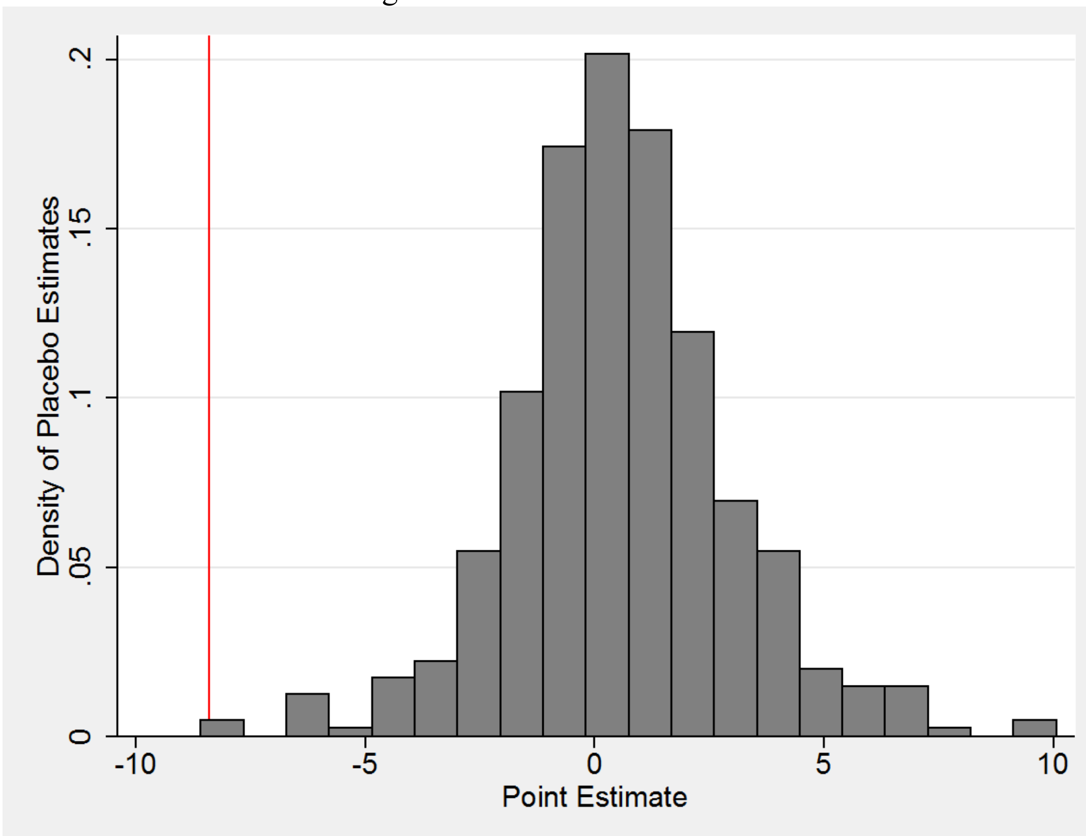
Notes: The figure plots the mean value of the covariates index by claim week. The covariates index is the predicted log initial UI duration using a fourth-order polynomial of earnings in the quarter preceding job loss, indicators for four-digit industry, and previous job tenure quintiles. See text for additional details.

Figure 3: Average UI Spell Duration by Application Week



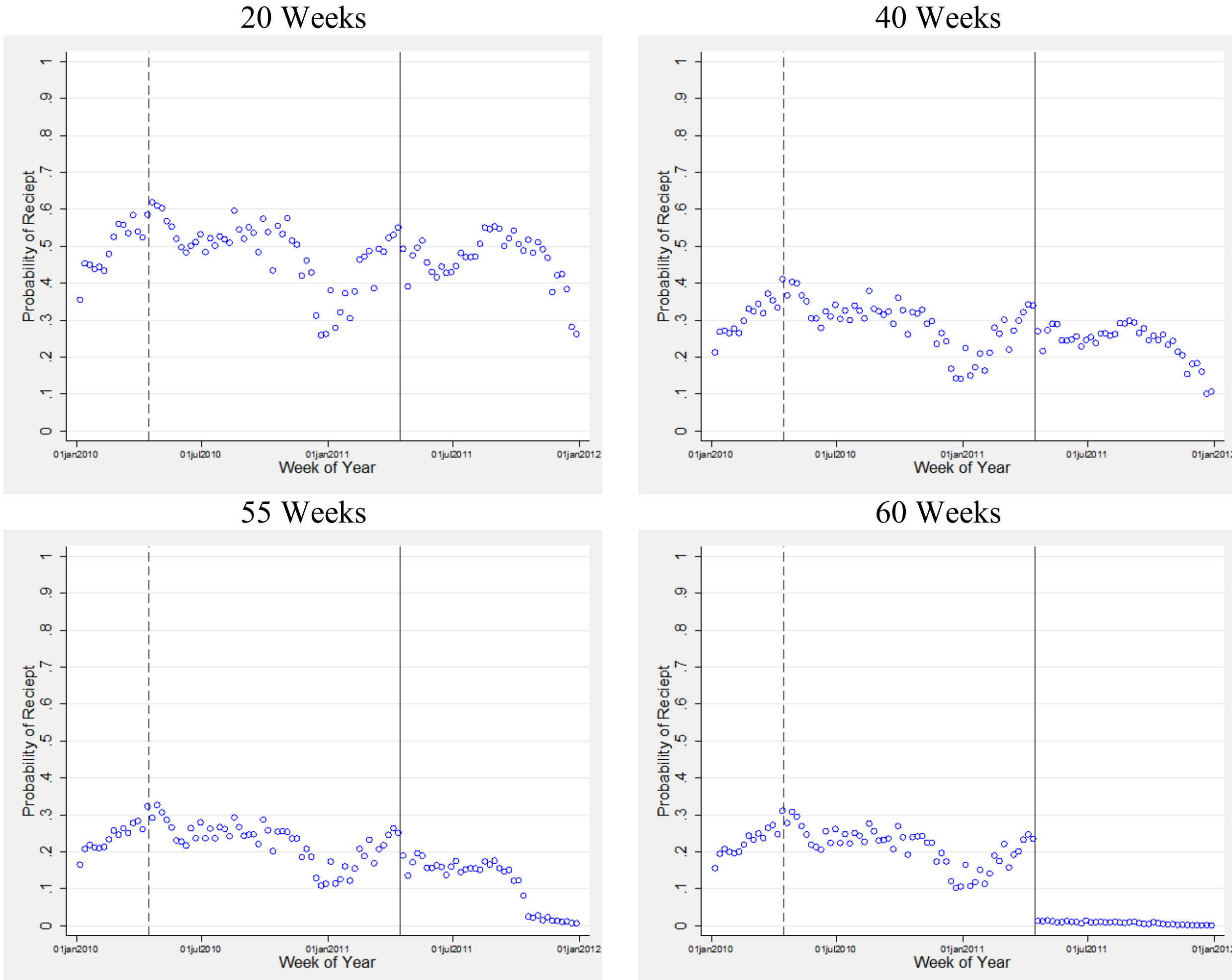
Notes: This figure plots the mean UI spell Duration by week of initial claim. The solid vertical line denotes the week of the cut in potential UI. The dashed vertical lines denote the same week in 2010 and 2009.

Figure 4: Placebo Distribution



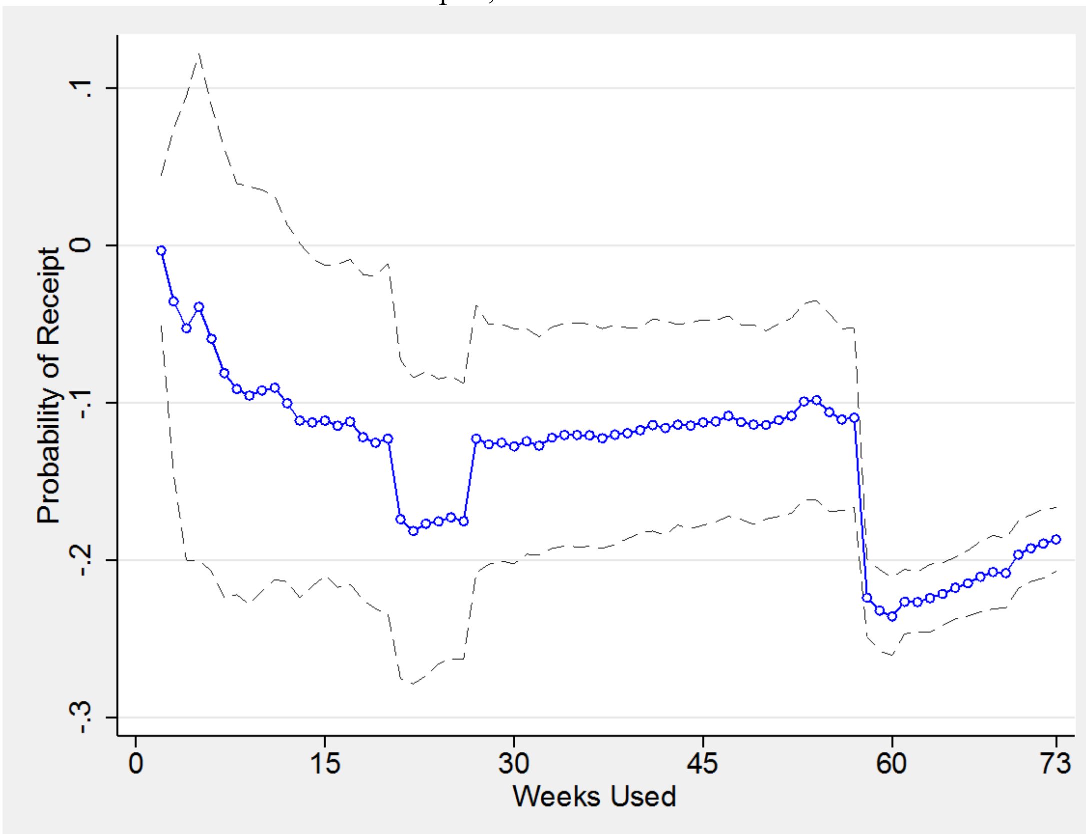
Note: This figure shows the distribution of placebo RDD estimates for unemployment insurance spell durations, where we vary the placebo treatment date over all weeks from January-October for years 2003-2012. Vertical line indicates the real treatment.

Figure 5: Probability UI Spell Duration Exceeds Threshold



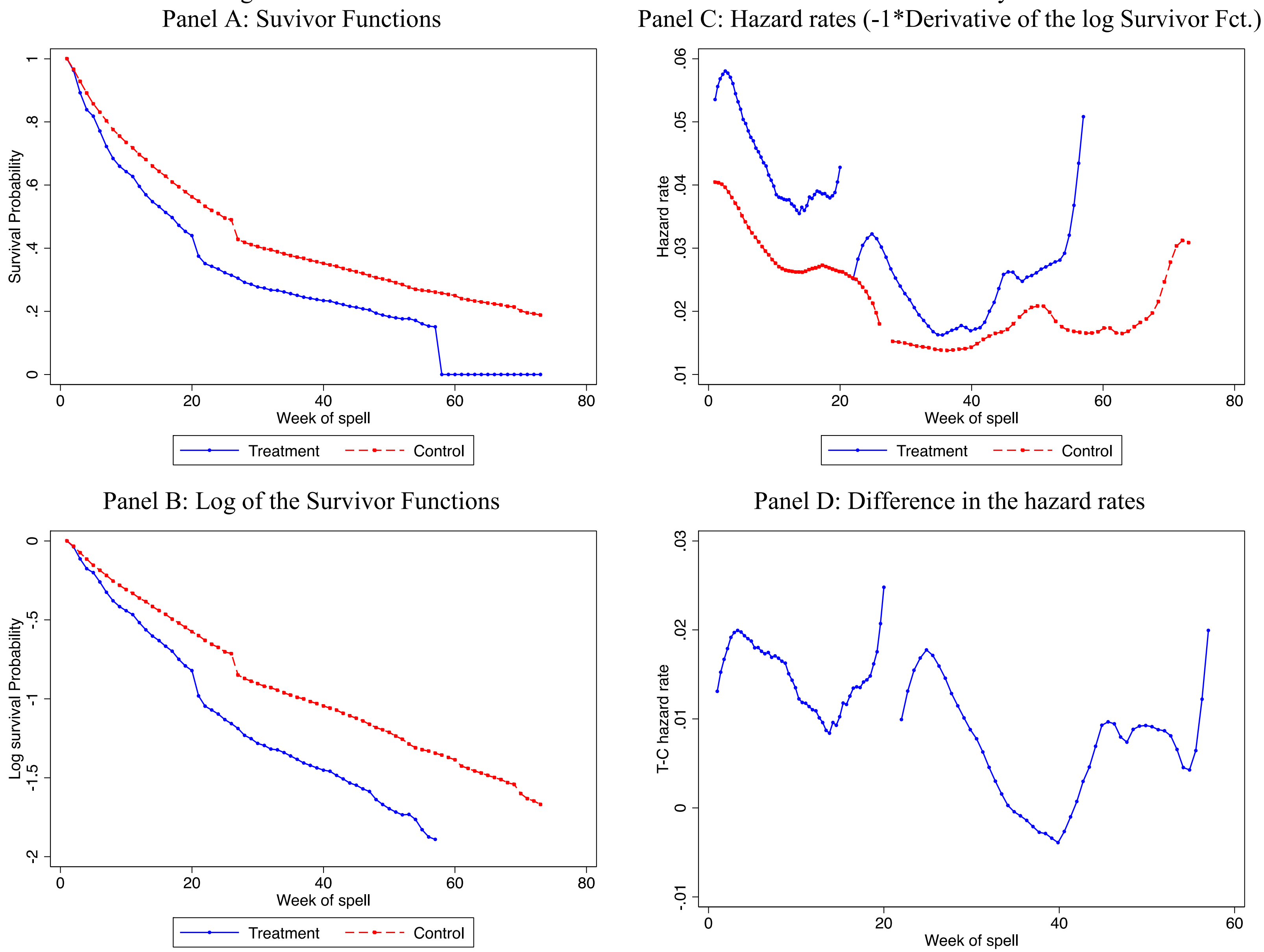
Notes: The figures plot the probability that UI spell durations exceed 20, 40, 55, and 60 weeks, by initial claim week. The solid vertical lines denote the week of the UI potential duration cut. The dashed vertical lines represent the same week in 2010.

Figure 6. RDD Estimates of the Differential Probability of Claiming UI for Weeks 1-73 of the Spell, Treatment - Control



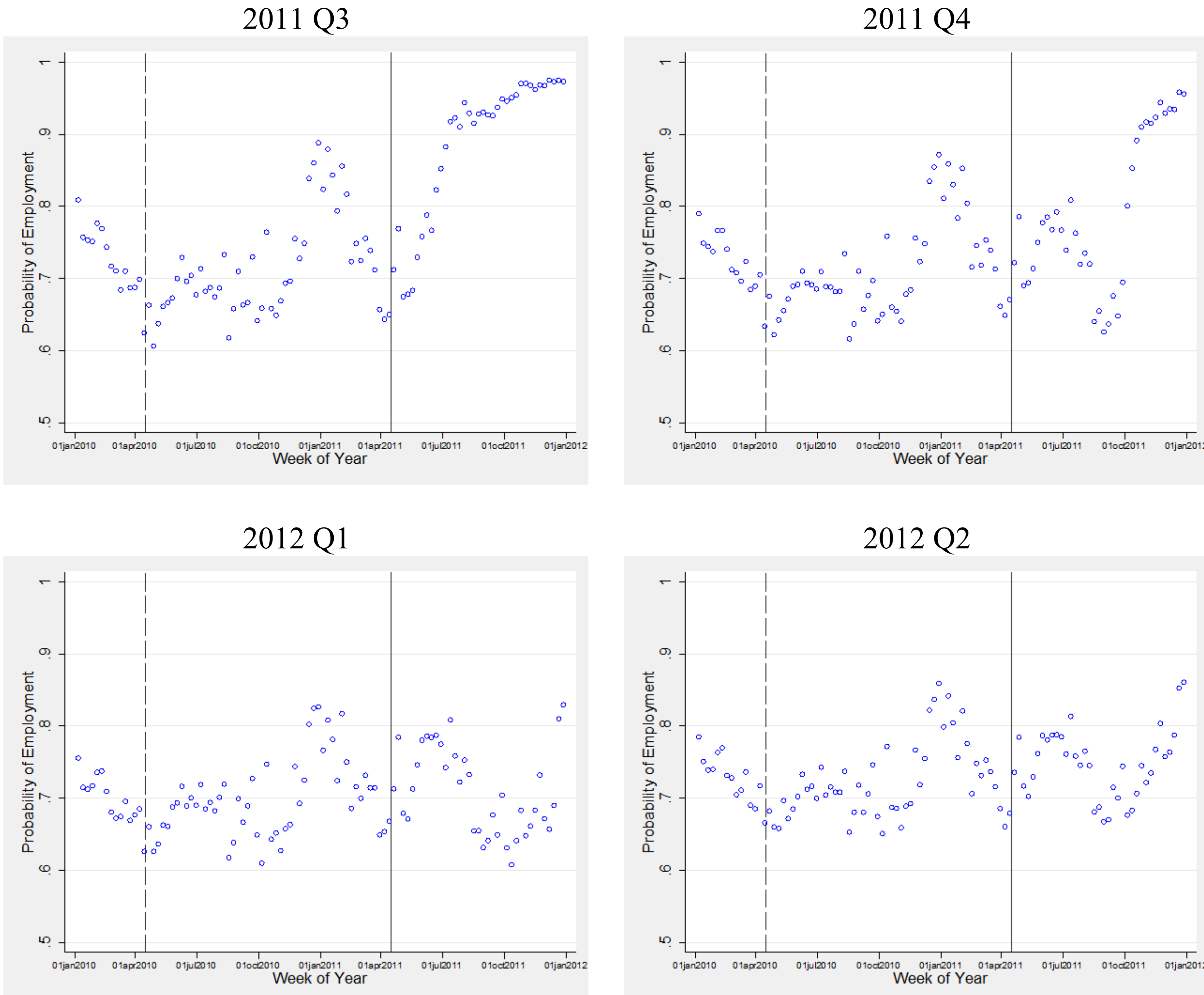
Notes: Each point is an RDD estimate (local linear regression with IK optimal bandwidth with triangular kernel) for the probability that a recipient claims X weeks of UI, for X spanning 1 to 73. The dashed lines are the 95% confidence interval.

Figure 7. Treatment and Control Survivor and Hazards Functions at the Policy Threshold



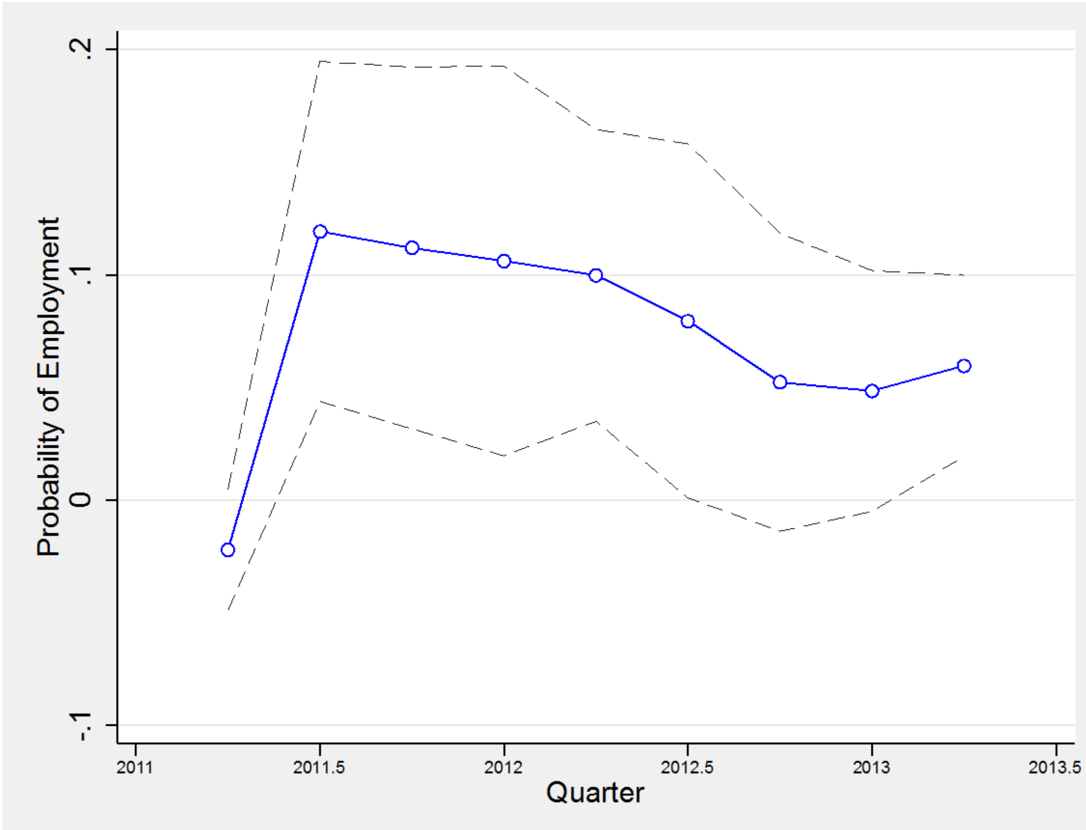
Notes: Panel A plots the RDD estimate of the survivor function. Each point in the control series is the estimated intercept for the control group in the local linear regression used to estimate the RDD probabilities of survival up to a given week, shown in in Figure 6. Each point in the treatment is the corresponding estimate for the treatment. The difference in these two series are the RDD estimates shown in Figure 6. Panel B plots the natural log of the survivor functions. Panel C plots -1 times the numerical derivative of the smoothed survivor functions. To smooth the survivor functions we estimate a local quadratic regression with a bandwidth of 4 seperately for the 1-20 week and the 21-57 week segments for the treatment group and the 1-26 week and 26-73 week segments for the control group. The segments are split this way to avoid the discontinuous drop in enrollment from recipients not enrolling into the EUC program. Panel D shows the difference in the estimated hazards in Panel C.

Figure 8. Probability Claimant Had Positive Earnings, by Quarters:



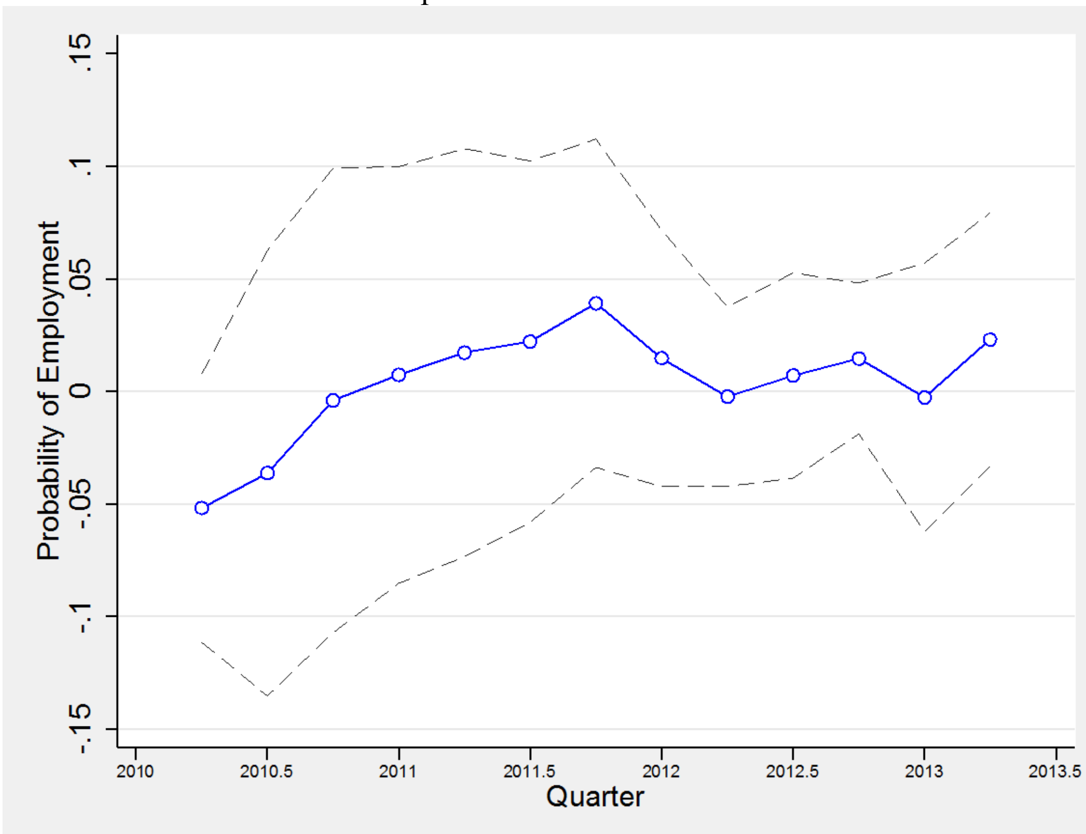
Notes: The figures plot the probability that a UI claimant has positive earnings in 2011 Q3, 2011 Q4, 2012 Q1, and 2012 Q2, by week of initial claim. The solid vertical line denotes the week of the cut in UI potential duration, and the dashed vertical line denotes the same week in 2010.

Figure 9. RDD Estimates of the Probability of Positive Earnings by Quarter following April 2011 UI Duration Cut



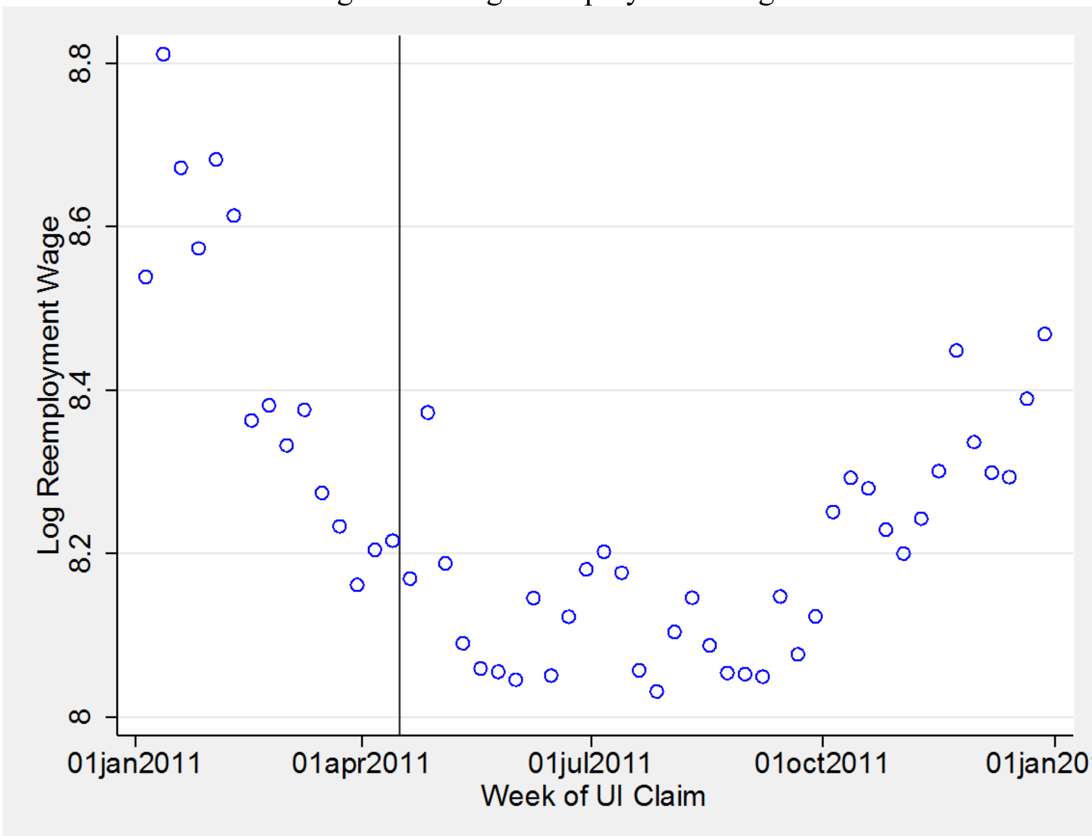
Notes: Each point is the RDD estimate (local linear regression with IK optimal bandwidth with triangular kernel) for the probability that a UI claimant has positive earnings in each quarter subsequent to the cut in potential UI duration. The dashed lines are the 95% confidence interval.

Figure 10: RDD Estimates of the Probability of Positive Earnings by Quarter Subsequent to April 2010 Placebo Cut



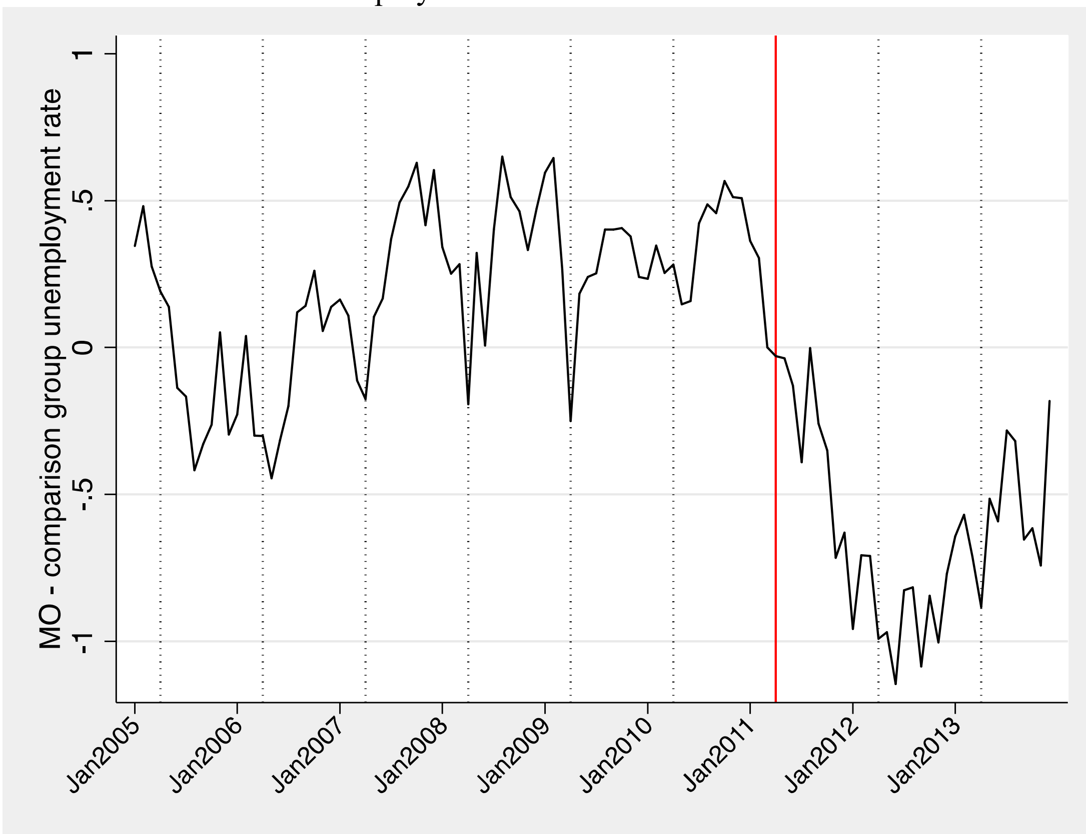
Notes: Each point is the RDD estimate (local linear regression with IK optimal bandwidth with triangular kernel) for the probability that a UI claimant has positive earnings setting the UI benefit cut threshold to April 2010, one year prior to the actual cut in UI duration. The dashed line is the 95% confidence interval.

Figure 11: Log Reemployment Wage



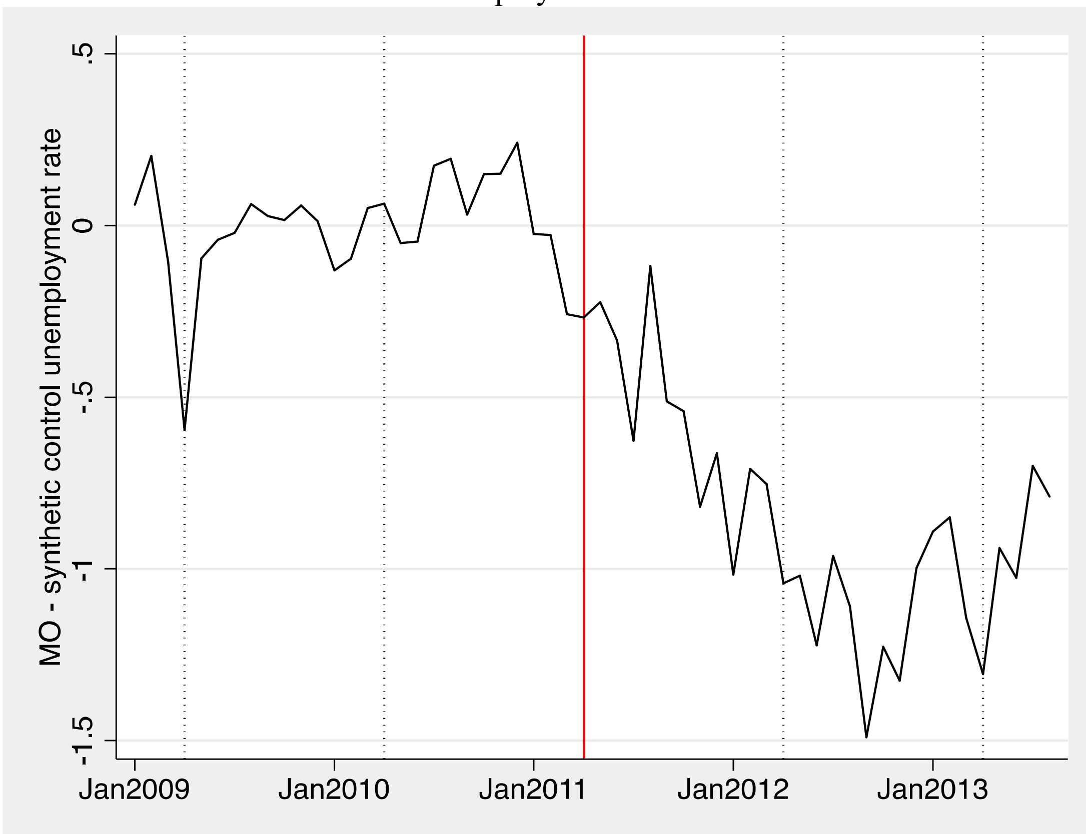
Notes: The figure plots the mean of log earnings for the first complete quarter of earnings after a UI claim.

Figure 12: Difference between the Missouri Unemployment Rate and the Average Unemployment Rate of all Other States



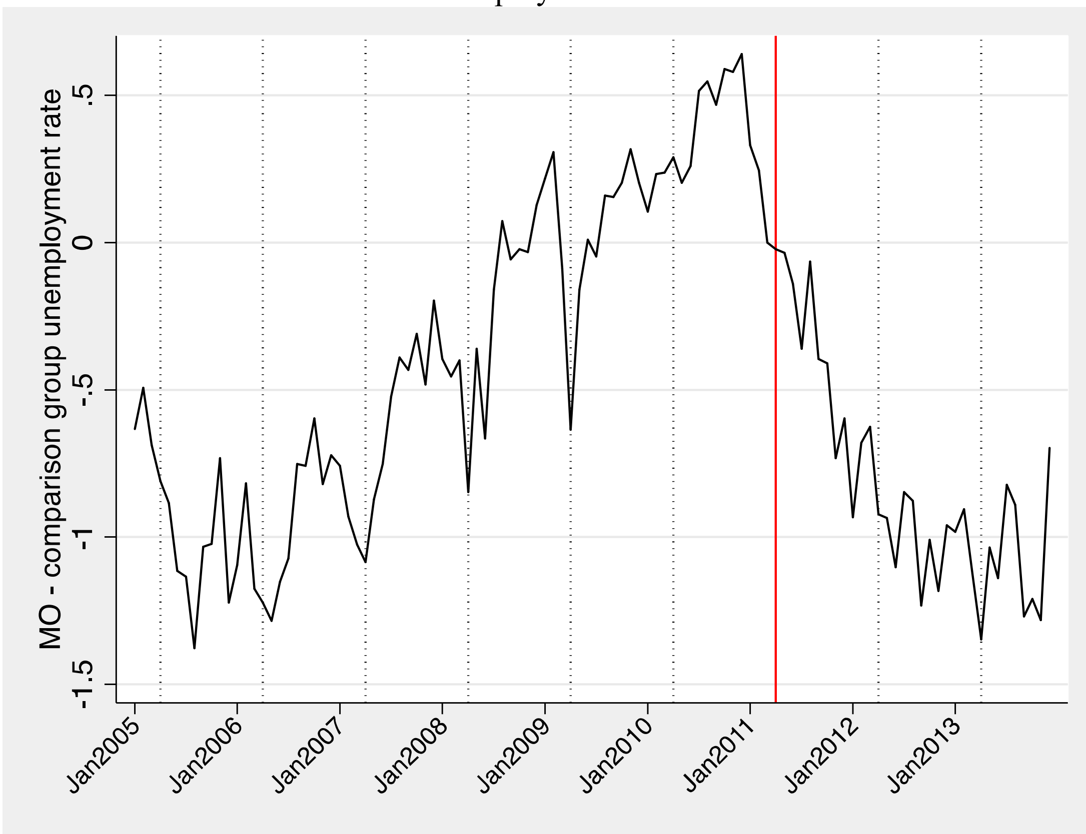
Notes: The figure plots the difference between the deseasonalized monthly Missouri unemployment rate and the average deseasonalized unemployment rate for all other 49 states and the District of Columbia. The series is normalized to 0 in March 2011. The vertical solid line denotes the month of the cut in potential UI duration. The vertical dotted lines denote the month of April in other years.

Figure 13: Difference Between the Missouri Unemployment Rate and the Synthetic Control Unemployment Rate



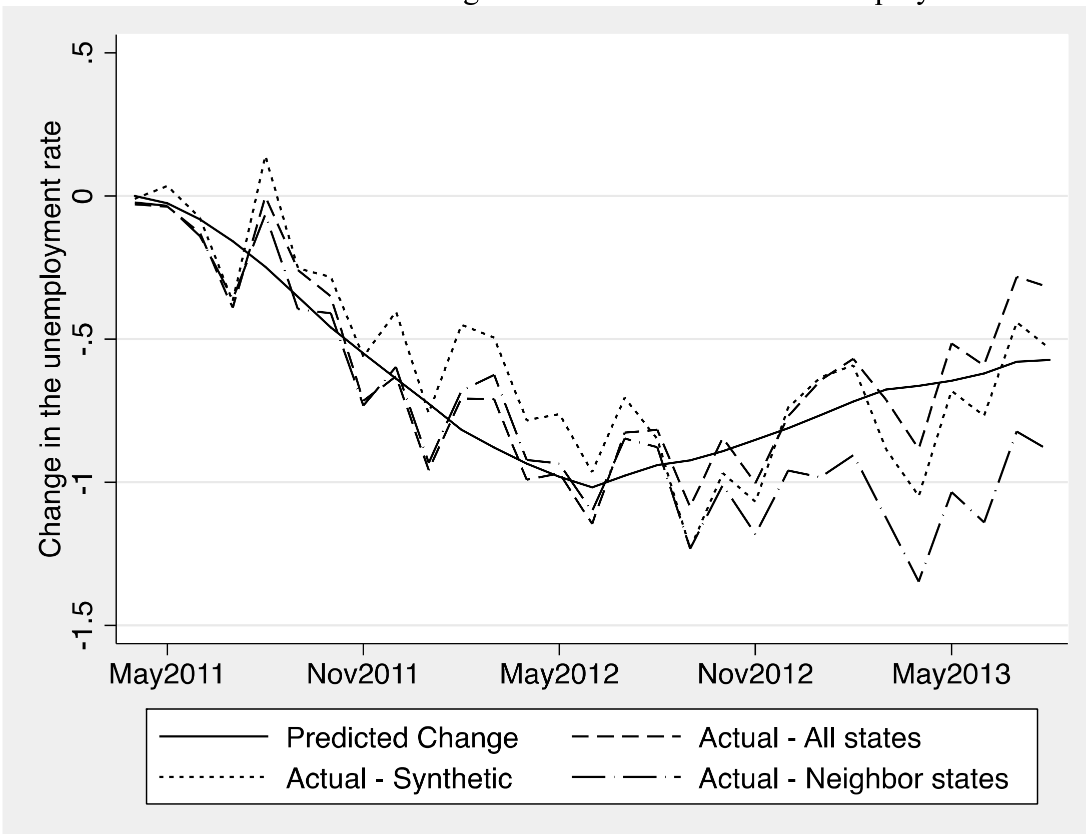
Notes: The figure plots the difference between the monthly deseasonalized Missouri unemployment rate and the deseasonalized unemployment rate of the synthetic control. See text for details on the construction of the synthetic control. The vertical solid line denotes the month of the cut in potential UI duration. The vertical dotted lines denote the month of April in other years.

Figure 14: Difference Between the Missouri Unemployment Rate and Neighboring States Unemployment Rate



Notes: The figure plots the difference between the deseasonalized monthly Missouri unemployment rate and the average deseasonalized unemployment rate for neighboring states. The series is normalized to 0 in March 2011. The vertical solid line denotes the month of the cut in potential UI duration. The vertical dotted lines denote the month of April in other years.

Figure 15: Predicted Change in the Missouri Unemployment Rate versus Difference-in-Difference Estimates of the Change in the Actual Missouri Unemployment Rate



Notes: The "Predicted Change" is the change in the Missouri unemployment rate that is predicted by the estimated RDD change in the survivor function assuming no spillover effects. "Actual – All states" is the difference between the Missouri unemployment rate and the unweighted average of the unemployment rate in all other states relative to March 2011. "Actual – Synthetic" is the difference between the Missouri unemployment rate and the synthetic control unemployment rate. "Actual – Neighbor states" is the difference between the Missouri unemployment rate and the unemployment rate of neighboring states. See text for details on the construction of the synthetic control.

Table 1. Summary Statistics

	2003-2013	2011
Weekly benefit	260.4	259.6
	[65.62]	[74.19]
Maximum benefit	6321	6328
	[1976]	[2727]
Total benefits	3563	4234
	[2769]	[3429]
Reemployment quarterly wage	7720	7240
	[6901]	[5703]
Previous employer quarterly wage	9021	8259
	[8072]	[6891]
Previous employment tenure	12.1	14.5
	[9.50]	[11.18]
Jobless quarters	1.9	1.7
	[5.23]	[3.02]
Weeks received	22.0	29.3
	[18.92]	[23.22]

Notes: Standard deviations in brackets. Maximum benefit is the maximum dollars of regular state benefits available to the UI recipient. Total benefit is the total amount of UI benefits received in the spell. Weekly, maximum and total benefits pertain only to regular UI benefits and not EUC and EB. Reemployment quarterly wage is earnings for the first complete quarter of employment after the UI claim. Previous employer quarterly wage is earnings for the last complete quarter of employment before the unemployment claim. Previous employment tenure is in quarters. Weeks received refers to both regular and extended benefits.

Table 2. RDD Diagnostics

	Claim Frequency (1)	Log Predicted Duration Index (2)
Estimated Discontinuity	3.13	-0.025
	(824.2)	(0.045)
Observations	525	525
Bandwidth	9.64	3.91
Mean of Dependent Variable	5396.76	2.56

Notes: Local quadratic (column 1) and local linear (column 2) RDD estimates with a triangular kernel. Observations are at the claim week level. Models are estimated using weekly averages of the dependent variable, weighting observations by the number of observations in the cell. Column (1) reports the RDD estimate for the number of full eligibility initial UI claims. Column (2) reports the RDD estimate for the index of predicted log initial UI duration which is constructed by regressing log UI duration on a fourth-order polynomial of earnings in the quarter preceding job loss, indicators for four-digit industry, and previous job tenure quintiles.

Table 3. RDD Estimates of the Effect of the Cut in UI Potential Duration on Weeks of UI Received

	Weeks Received (1)	Received at least 20 Weeks (2)	Received at least 40 Weeks (3)	Received at least 55 Weeks (4)	Received at least 60 Weeks (5)
Panel A. Main Estimates					
Estimated Discontinuity	-8.697	-0.123	-0.118	-0.101	-0.236
	(1.424)	(0.057)	(0.035)	(0.035)	(0.013)
Observations	525	525	525	525	525
Bandwidth	14.94	6.15	6.08	5.17	4.92
Mean of Dependent Variable	25.45	0.46	0.25	0.16	0.11
Panel B. Placebo Estimates					
Estimated Discontinuity	-0.747	0.075	-0.009	0.001	0.008
	(1.281)	(0.029)	(0.040)	(0.032)	(0.032)
Observations	525	525	525	525	525
Bandwidth	18.59	6.27	5.32	5.47	6.34
Mean of Dependent Variable	32.1	0.50	0.31	0.24	0.23

Notes: Local linear RDD estimates using the IK optimal bandwidth and a triangular kernel. Observations are at the claim week level. Models are estimated using weekly averages of the dependent variable, weighting observations by the number of observations in the cell. Placebo estimates are from estimating the same specification with a threshold set to one year prior to the April 2011 cut in benefits duration.

Table 4. RDD Estimates of the Effect of the Cut in UI Maximum Duration on Employment and Reemployment Wages

	Pr(Earnings>0 in Q=0) (1)	Pr(Earnings>0 in Q=1) (2)	Pr(Earnings>0 in Q=2) (3)	Pr(Earnings>0 in Q=3) (4)	First complete quarter log reemployment wage (5)
Panel A. Main Estimates					
Estimated Discontinuity	-0.022	0.119	0.112	0.106	0.121
	(0.014)	(0.039)	(0.041)	(0.044)	(0.118)
Observations	104	104	104	104	525
Bandwidth	5.21	6.08	5.97	5.78	7.38
Mean of Dependent Variable	0.84	0.80	0.75	0.71	8.60
Panel A. Placebo Estimates					
Estimated Discontinuity	-0.052	-0.036	0.003	0.0007	-0.029
	(0.030)	(0.050)	(0.052)	(0.047)	(0.033)
Observations	104	104	104	104	525
Bandwidth	5.95	7.52	5.19	4.67	5.80
Mean of Dependent Variable	0.91	0.84	0.77	0.68	8.63

Notes: Local linear RDD estimates using the IK optimal bandwidth and a triangular kernel. Observations are at the claim week level. Models are estimated using weekly averages of the dependent variable, weighting observations by the number of observations in the cell. Q=0 is 2011 Q2 for the main estimates and 2010 Q2 for the placebo estimates. Placebo estimates are from estimating the same specification with a threshold of one year prior to the April 2011 cut in benefits duration.

Table 5. DiD Estimates of the Change in the Missouri Unemployment Rate, Log Number of Unemployed, and Log Size of the Labor Force following the April 2011 UI Maximum Duration Cut

	UR (1)	UR (2)	UR (3)	ln(U) (4)	ln(U) (5)	ln(U) (6)	ln(LF) (7)	ln(LF) (8)	ln(LF) (9)
Panel A. All States									
Missouri * Post	-0.890	-0.801	-0.847	-0.118	-0.101	-0.10	-0.01	-0.006	-0.002
SE	(0.122)	(0.246)		(0.016)	(0.033)		(0.003)	(0.006)	
PCSE	(0.19)	(0.214)		(0.018)	(0.028)		(0.004)	(0.004)	
Wild Bootstrap C.I.	(-1.1, -0.6)	(-0.9, -0.7)		(-0.15, -0.09)	(-0.12, -0.08)		(-0.02, -0.01)	(-0.01, 0.00)	
%-tile rank	0.059	0.020	0.039	0.078	0.020	0.039	0.255	0.137	0.353
Observations	2856	2856	2576	2856	2856	2576	2856	2856	2576
Panel B. Neighbors									
Missouri * Post	-1.01	-0.755	-0.699	-0.113	-0.083	-0.11	-0.008	-0.005	-0.029
SE	(0.108)	(0.215)		(0.013)	(0.026)		(0.003)	(0.005)	
PCSE	(0.203)	(0.189)		(0.028)	(0.028)		(0.004)	(0.003)	
Wild Bootstrap C.I.	(-1.3, -0.7)	(-0.9, -0.6)		(-0.15, -0.07)	(-0.11, -0.06)		(-0.02, 0.00)	(-0.01, 0.00)	
%-tile rank	0.000	0.000	0.000	0.000	0.000	0.000	0.333	0.111	0.000
Observations	504	504	448	504	504	448	504	504	448
Predicted change	-0.64	-0.64	-0.64	-0.10	-0.10	-0.10			
Pred. chg. no outlier	-0.45	-0.45	-0.45	-0.07	-0.07	-0.07			
MO*trend		X			X			X	
Synthetic control			X			X			X

Notes: Observations are state by month units. UR is the unemployment rate, ln(U) is the natural log of the number of unemployed, and ln(LF) is the natural log of the size of the labor force. Variables are derived from the BLS Local Area Unemployment Statistics and deseasonalized as described in the text. The sample spans January 2009 to August 2013. SE is the OLS standard error, PCSE is the panel corrected standard error, and the permutation %-tile rank is the percentage of states that have a more negative "effect" when estimating the same model 51 times, assigning each state to be the "treated" state in each permutation. MO*trend allows for a Missouri specific trend. The synthetic control uses weights from the synthetic control method described in the text to form a control group. Predicted change is the change in UR and ln(U) that is predicted by the RDD estimates of the change in the survivor function assuming no spillover effects. "Pred. chg. no outlier" is the same prediction without the outlier cohort.

Table 6. Ibragimov and Müller p-values by Block Sizes

	(1) 56 blocks	(2) 28 blocks	(3) 14 blocks	(4) 8 blocks	(5) 6 blocks	(6) 4 blocks
Panel A. Unweighted Control; All States (Estimate = -0.95)						
t-statistic	13.50	9.94	7.95	6.62	6.34	14.26
Two-tail p-value (N-2 DOF)	0.00	0.00	0.00	0.01	0.02	0.04
N	56	28	14	8	6	4
Panel B. Synthetic Control; All States (Estimate = -0.86)						
t-statistic	12.20	9.13	6.98	5.74	5.19	4.58
Two-tail p-value (N-2 DOF)	0.00	0.00	0.00	0.01	0.04	0.14
N	56	28	14	8	6	4
Panel C. Unweighted Control; Neighbors (Estimate = -1.01)						
t-statistic	12.01	8.72	6.53	5.15	4.54	3.75
Two-tail p-value (N-2 DOF)	0.00	0.00	0.00	0.00	0.02	0.09
N	56	28	14	8	6	4
Panel D. Synthetic Control; Neighbors (Estimate = -0.73)						
t-statistic	8.68	6.38	4.85	3.82	3.30	2.34
Two-tail p-value (N-2 DOF)	0.00	0.00	0.00	0.01	0.03	0.15
N	56	28	14	8	6	4

Notes: Each column reports the t-statistic and corresponding two-tail p-value with N-2 degrees of freedom for the two sample t-test of equality of the difference between the Missouri and comparison group unemployment rate before and after the potential duration cut, where the monthly Missouri – comparison group unemployment rate differences have been collapsed into the specified number of blocks. In Panel A the comparison group is the equally weighted average of the monthly unemployment rate for all states and the District of Columbia excluding Missouri. In Panel B the comparison group is the synthetic control discussed in the text. We limit the sample to 28 months on each side of the policy change. Unemployment rates are derived from BLS LAUS. See Appendix Table 10 for the same tests using BLS CPS data.

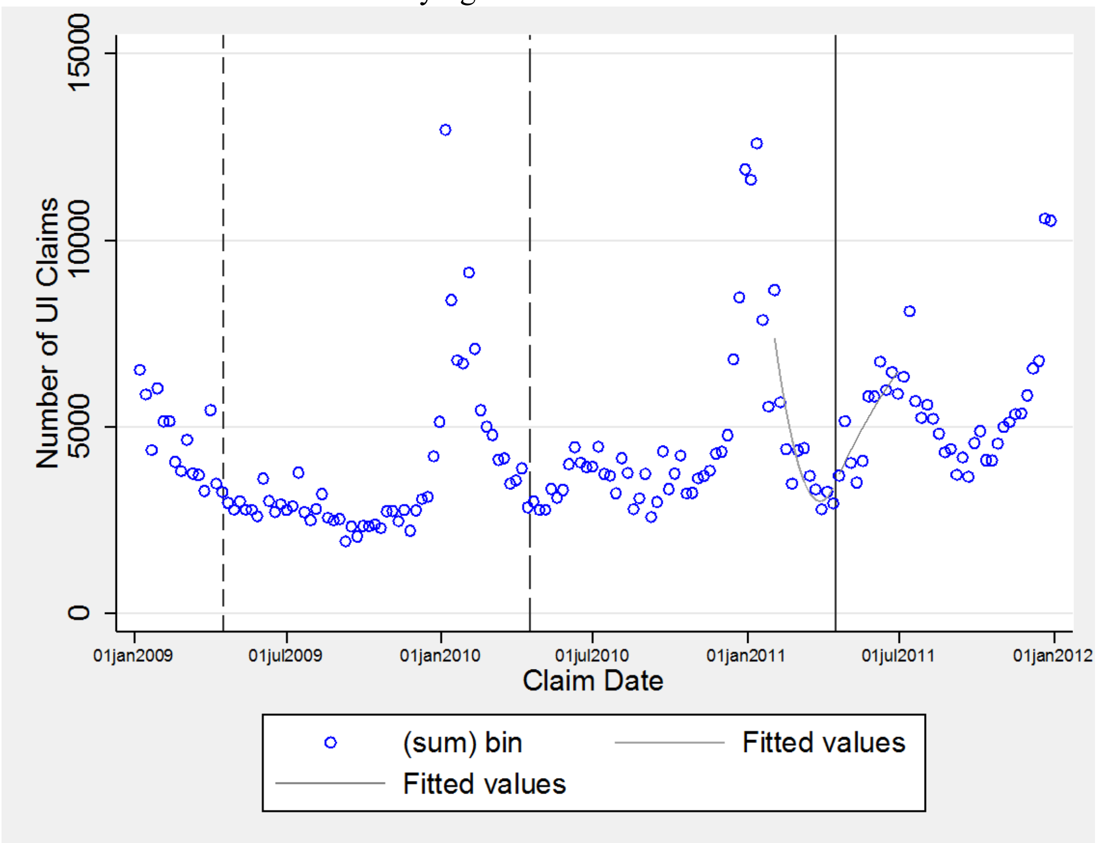
Table 7. Comparison of Bordering Counties

	UR (1)	UR (2)	ln(U) (3)	ln(U) (4)	ln(LF) (5)	ln(LF) (6)
Missouri * Post	-0.774	-0.882	-0.084	-0.089	0.003	0.000
SE	(0.075)	(0.133)	(0.007)	(0.011)	(0.023)	(0.035)
County Cluster SE	(0.165)	(0.094)	(0.022)	(0.011)	(0.007)	(0.002)
Observations	4620	4620	4620	4620	4620	4620
State F.E.	X	X	X	X	X	X
Time F.E.	X	X	X	X	X	X
County-Pair FE	X	X	X	X	X	X
MO*trend		X		X		X

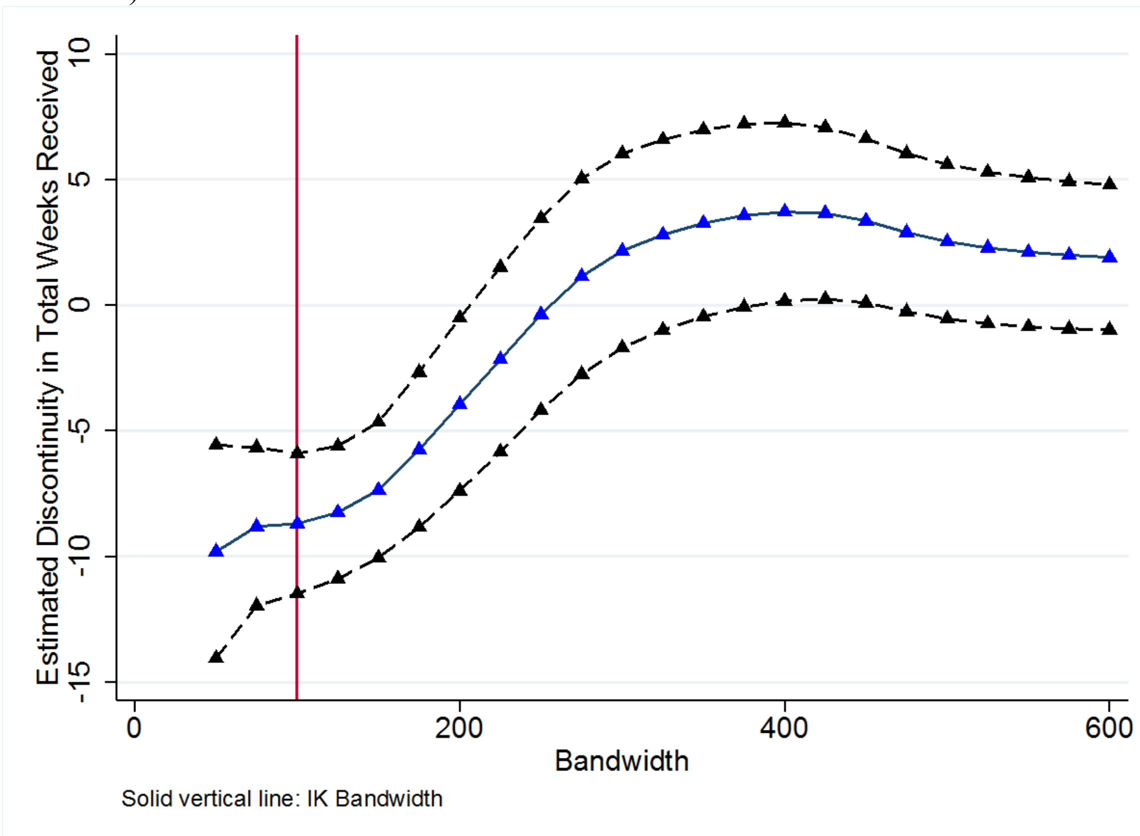
Notes: This table uses LAUS unemployment data from 2009 through 2013 where an observation is the unemployment rate in a county-month. We match each treatment county on Missouri's border to a neighboring untreated county in the adjoining state which we call county pairs. UR is the unemployment rate, ln(U) is the natural log of the number of unemployed, and ln(LF) is the natural log of the size of the labor force. MO*trend allows for a Missouri specific trend.

Online Appendix

Appendix Figure 1. Local Quadratic Fit in the Frequency Distribution of Full Eligibility Claims underlying Column 1 of Table 2.

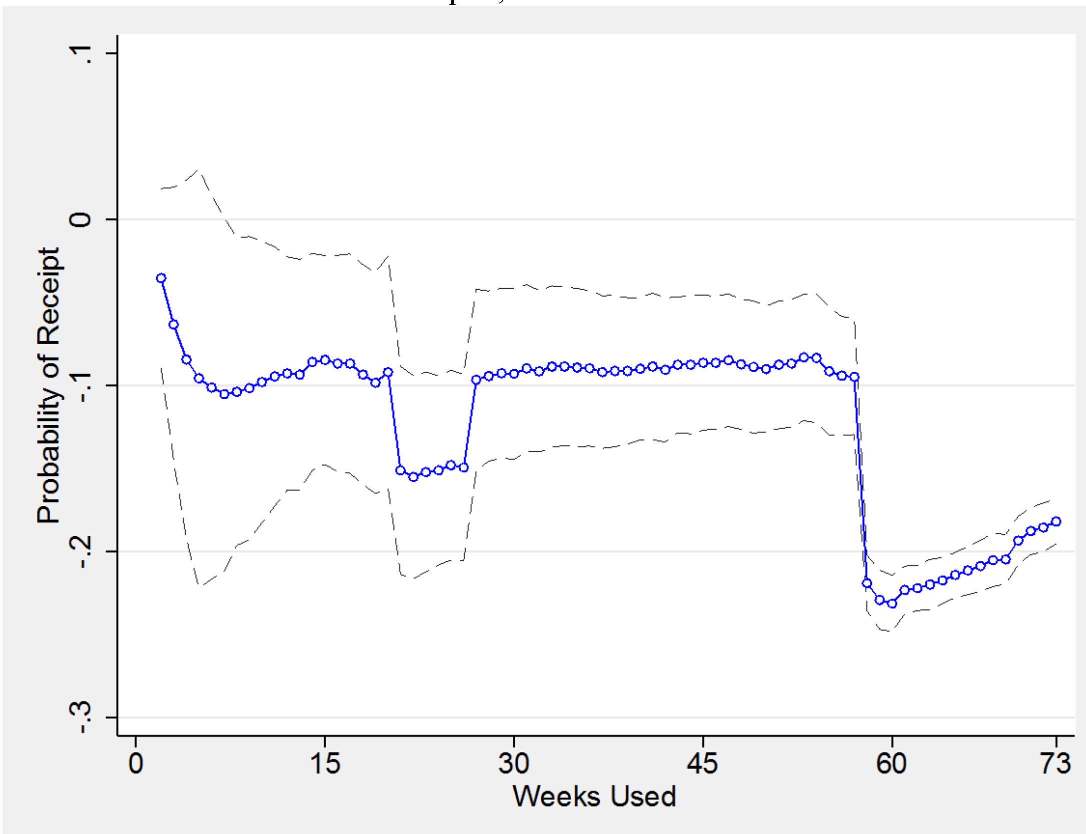


Appendix Figure 2. RDD Estimate of Total Weeks Received by Bandwidth (multiple of the IK bandwidth)

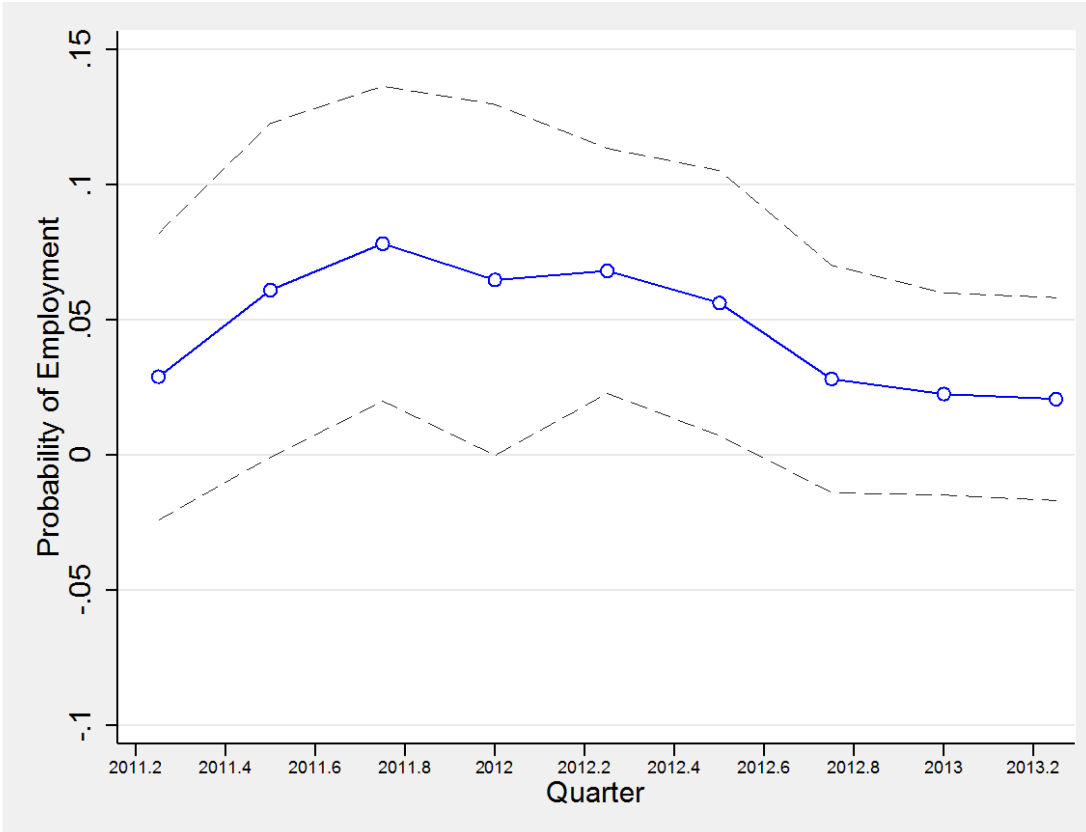


Notes: Each point represents the local linear RDD estimate with bandwidth as a multiple of the IK bandwidth, along with the 95% confidence interval.

Appendix Figure 3. RDD Estimates of the Probability of Claiming UI for Weeks 1-73 of the Potential UI Spell; Twice the IK Bandwidth

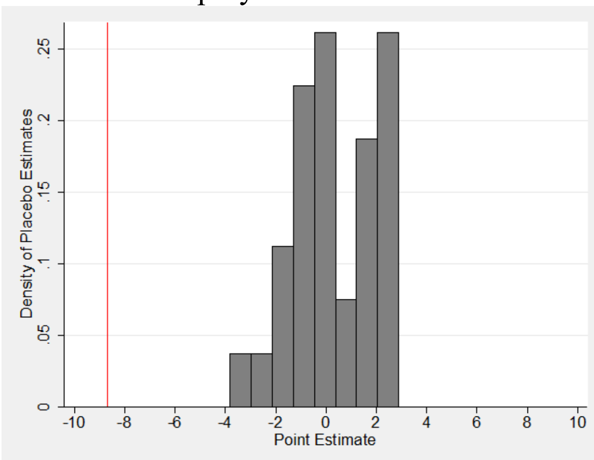


Appendix Figure 4. RDD Estimates of the Probability of Positive Earnings by Quarter; Twice the IK Bandwidth

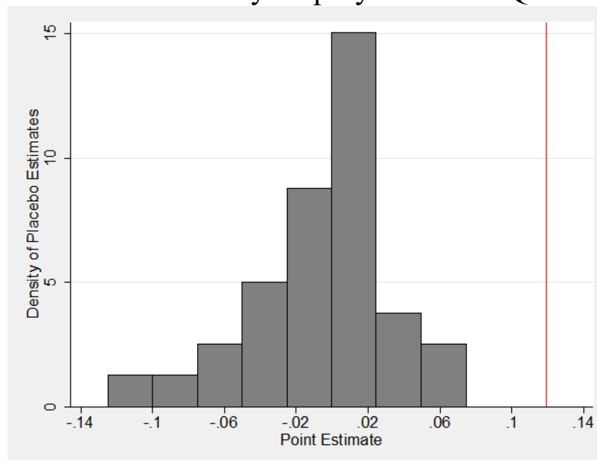


Appendix Figure 5. Distribution of placebo estimates for unemployment insurance duration and employment for the March 2011-October 2011 period of placebo dates

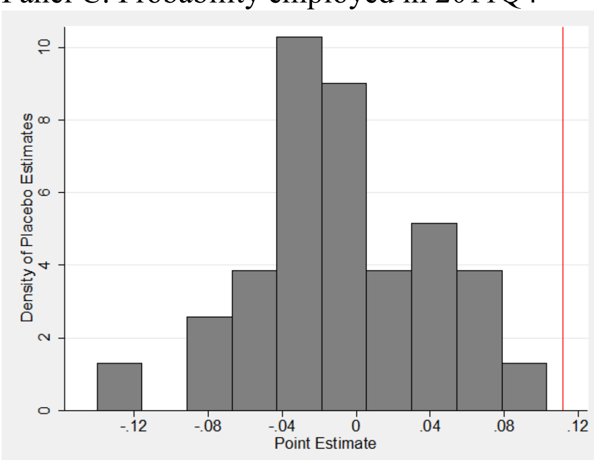
Panel A. Unemployment insurance duration



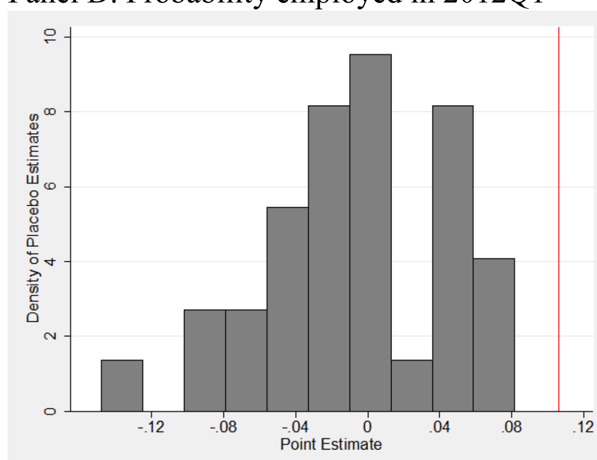
Panel B. Probability employed in 2011Q3



Panel C. Probability employed in 2011Q4

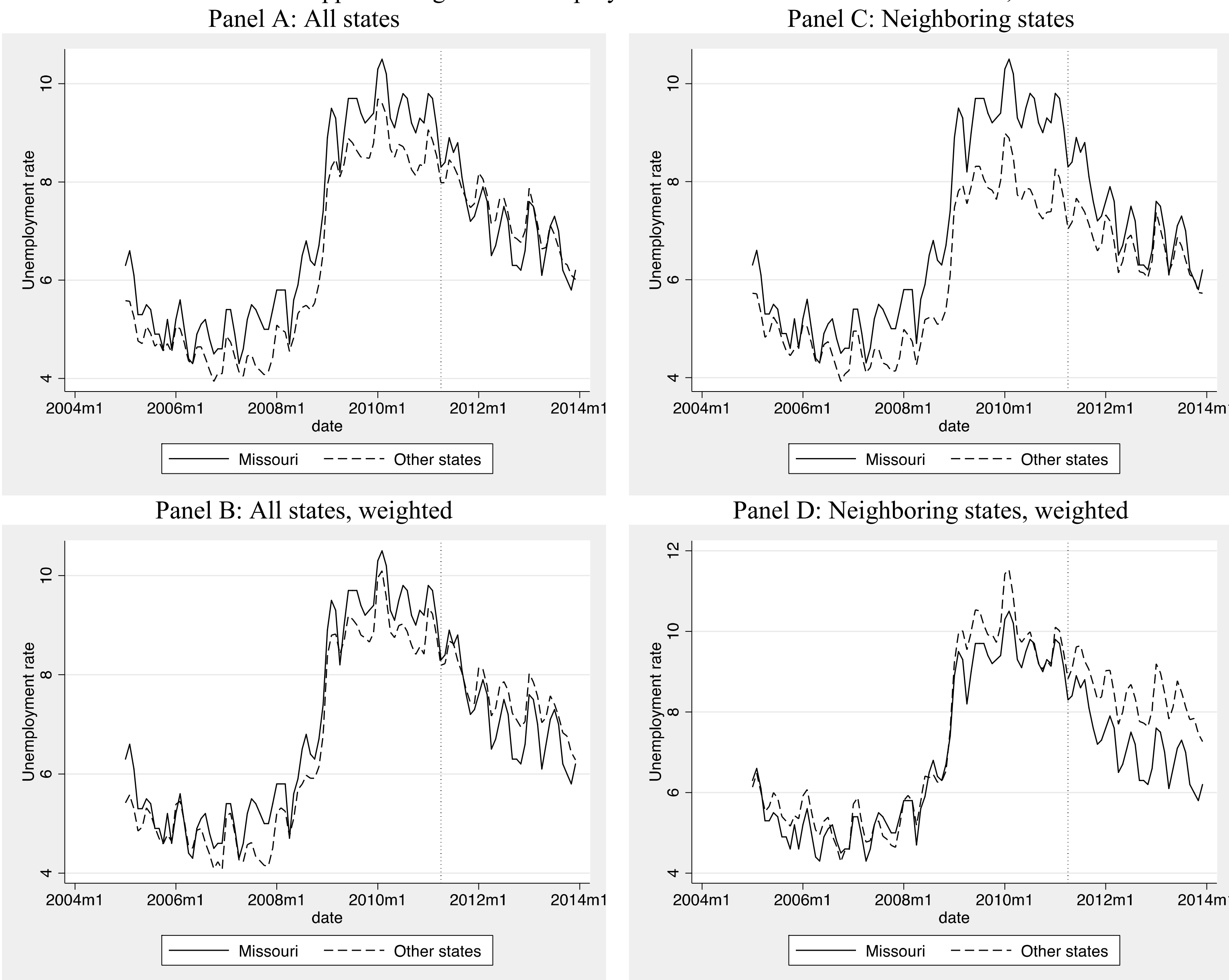


Panel D. Probability employed in 2012Q1



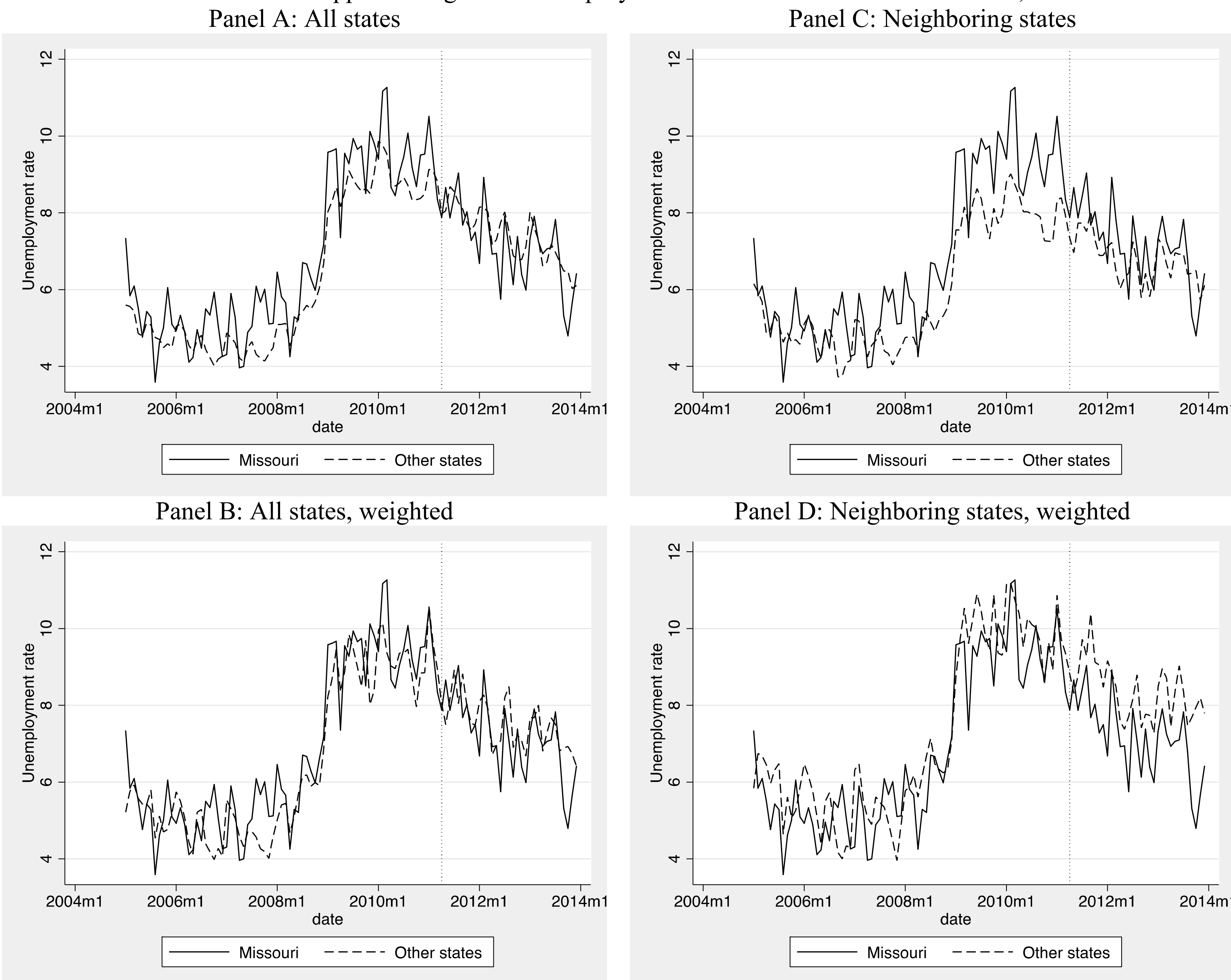
Notes: This figure shows the placebo distribution of estimates for three outcomes where we vary the placebo treatments for each week, starting one month prior to the real policy change through six months after the policy change. All estimates use an IK bandwidth. The purpose of the figure is to show placebo estimates for a period when the labor market in Missouri was improving. The RDD estimate for the real policy change is denoted by the vertical line.

Appendix Figure 6: Unemployment rate in Missouri and other states; LAUS



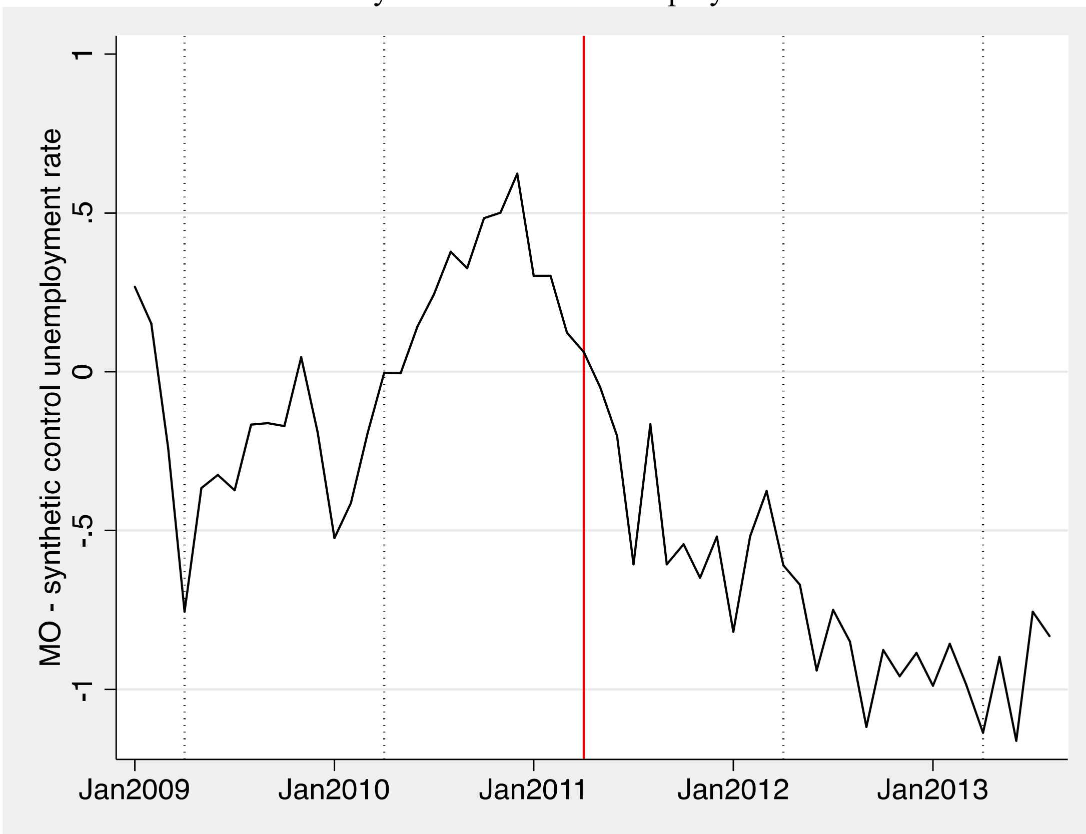
Notes: Data are seasonally unadjusted. Weights are the synthetic weights described in the text. Vertical bar is the month of the policy change.

Appendix Figure 7: Unemployment rate in Missouri and other states; CPS



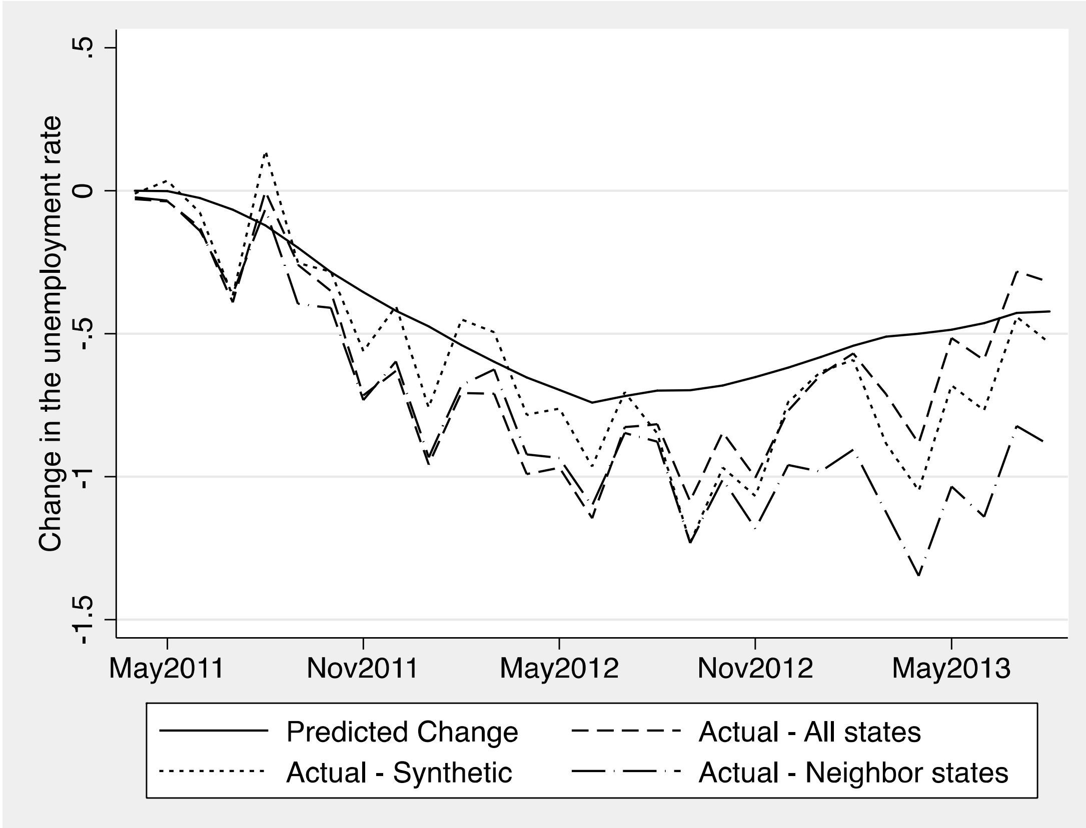
Notes: Data are seasonally unadjusted. Weights are the synthetic weights described in the text. Vertical bar is the month of the policy change.

Appendix Figure 8. Difference Between the Missouri Unemployment Rate and the Neighbors-Derived Synthetic Control Unemployment Rate



Notes: The figure plots the difference between the monthly deseasonalized Missouri unemployment rate and the deseasonalized unemployment rate of the synthetic control derived from neighboring states. The donor pool excludes Arkansas because it changed UI benefit duration over the same period. See text for details on the construction of the synthetic control. The control group consists of the following weighted average of states: 38.7% Illinois, 5.6% Nebraska, and 55.7% Kentucky. The vertical solid line denotes the month of the cut in potential UI duration. The vertical dotted lines denote the month of April in other years.

Appendix Figure 9. Predicted Change in the Missouri Unemployment Rate versus Difference-in-Difference Estimates of the Change in the Actual Missouri Unemployment Rate; No outlier



Notes: The "Predicted Change" is the change in the Missouri unemployment rate that is predicted by the estimated RDD change in the survivor function assuming no spillover effects and excluding the outlier cohort. "Actual – All states" is the difference between the Missouri unemployment rate and the unweighted average of the unemployment rate in all other states relative to March 2011. "Actual – Synthetic" is the difference between the Missouri unemployment rate and the synthetic control unemployment rate. "Actual – Neighbor states" is the difference between the Missouri unemployment rate and the unemployment rate of neighboring states. See text for details on the construction of the synthetic control.

Appendix Figure 10. Example of a Missouri Division of Employment Security Notice of Initial Determination of UI Status

DIVISION OF EMPLOYMENT SECURITY

MISSOURI DEPARTMENT OF LABOR AND INDUSTRIAL RELATIONS
NOTICE OF INITIAL DETERMINATION OF STATUS AS AN INSURED WORKER

John Doe
123 Main St
Springfield MO 65807

Date Mailed: 09/05/14
Social Security No.. 123-45-6789
Benefit Year Begins: 08/31/14

YOU ARE AN INSURED WORKER. YOUR WEEKLY BENEFIT AMOUNT IS.........$301.00
YOUR MAXIMUM BENEFIT AMOUNT IS...... $6020.00

Your unemployment claim is computed on wages paid from 04/01/13 through 03/31/14 .
Our record of wages is as follows:

Employer's Number	Employer's Name	Qtr	Year	Wages
123456 0 999	ABC Company, Inc.	2	13	7138.47
123456 0 999	ABC Company, Inc.	3	13	7532.52
654321 1 777	XYZ, LLC	4	13	5715.78
123456 0 999	ABC Company, Inc.	4	13	1817.39
654321 1 777	XYZ, LLC	1	14	6762.52

IMPORTANT – Being an insured worker does not guarantee payment of unemployment insurance (UI) benefits. Payment of UI benefits is subject to meeting all eligibility requirements. Review this notice carefully to ensure that your address, Social Security Number, and the Division of Employment Security's (DES) record of wages are correct. If there is an error or omission, follow the instructions on the reverse side for filing an appeal. If you have questions or need additional information, you can contact a Regional Claims Center by telephone. It should be noted, however, that contacting the DES by telephone does not preserve your appeal rights. Read the "What You Need to Know About Unemployment Insurance in Missouri" pamphlet for information on the steps to take now that your initial claim has been filed.

(See Reverse Side for Important Messages)

MODES-B-9 (08-12)
DES-BIC103A U.I.Prg.

Appendix Table 1. RDD Estimates of the Effect of the Cut in UI Potential Duration on Weeks of UI Received; Local Linear with IK Bandwidth Excluding Outlier Cohort

	Weeks Received (1)	Received at least 20 Weeks (2)	Received at least 40 Weeks (3)	Received at least 55 Weeks (4)	Received at least 60 Weeks (5)
Estimated Discontinuity	-7.19	-0.075	-0.091	-0.079	-0.235
	(0.818)	(0.013)	(0.011)	(0.014)	(0.013)
Observations	524	524	524	524	524
Bandwidth	15.31	6.18	5.58	5.20	4.89
Mean of Dependent Variable	25.52	0.46	0.25	0.16	0.11

Notes: Local linear RDD estimates using the IK optimal bandwidth and a triangular kernel. Observations are at the claim week level. Models are estimated using weekly averages of the dependent variable, weighting observations by the number of observations in the cell.

Appendix Table 2. RDD Estimates of the Effect of the Cut in UI Potential Duration on Weeks of UI Received; Local Quadratic with CCT Bandwidth

	Weeks Received (1)	Received at least 20 Weeks (2)	Received at least 40 Weeks (3)	Received at least 55 Weeks (4)	Received at least 60 Weeks (5)
Estimated Discontinuity	-11.47	-0.148	-0.129	-0.114	-0.245
	(1.650)	(0.039)	(0.022)	(0.021)	(0.010)
Observations	525	525	525	525	525
Bandwidth	25.00	23.04	25.65	22.25	29.93
Mean of Dependent Variable	25.45	0.46	0.25	0.16	0.11

Notes: Local quadratic RDD estimates using the CCT optimal bandwidth and a triangular kernel. Observations are at the claim week level. Models are estimated using weekly averages of the dependent variable, weighting observations by the number of observations in the cell.

Appendix Table 3. Estimated Effects at Placebo Discontinuities

	Estimate	SE	N
	(1)	(2)	(3)
2012	1.638	1.822	527
2011	-8.697	1.424	527
2010	-0.524	1.276	527
2009	-0.159	1.595	527
2008	1.974	2.167	527
2007	-1.196	0.763	527
2006	-0.926	0.515	527
2005	-1.100	0.724	527
2004	0.372	1.721	527

Notes: This table presents the specification in column (1) of Table 3 for the treatment week in all available years. The year of the actual policy change is 2011.

Appendix Table 4. Estimating Column (1) of Table 3 with Deseasonalizing Data

	(1)	(2)	(3)	(4)
Estimated Discontinuity	-9.66	-8.67	-9.95	-9.762
	(1.84)	(1.49)	(2.51)	(1.77)
Observations	51	51	51	51
Bandwidth	13.09	12.16	11.74	14.13
Mean of Dependent Variable (2010)	33.15	33.15	33.15	33.15
Recession Era Control	X			
All Years Control		X		
2010 Control			X	
2012 Control				X

Notes: To deseasonalize UI spell duration we regress this variable on week-specific fixed effects in non-treatment years and subtract out the resulting seasonal effects in the treatment-year data. We present several estimates using alternative years to estimate seasonality (Recession Era Control includes 2008-2011, All Years Control estimates the seasonal variation using all years other than the treatment year, 2010).

Appendix Table 5. Excluding Seasonal Industries

	Omit Seasonal Industries (1)	Omit Manufacturing (2)
Panel A. Main Estimates		
Estimated Discontinuity	-7.583	-8.077
	(1.560)	(1.672)
Observations	8213	10199
Bandwidth	14.1	13.3
Mean of Dependent Variable	29.46	28.77
Panel B. Placebo Estimates		
Estimated Discontinuity	0.407	0.106
	(1.484)	(2.754)
Observations	8213	10199
Bandwidth	13.7	11.0
Mean of Dependent Variable	35.68	34.9

Notes: We test whether the estimates we obtained in column (1) of Table 3 are robust to the exclusion of the more seasonal industries. To this end, we first estimate seasonality by regressing claim quantities on month dummies and calculating the variance in the month dummies for each two-digit NAICS industry. We then re-estimate our main effect while excluding the most seasonal 25% of industries (column 1). We also test whether the estimate is robust to excluding manufacturing claims (column 2).

Appendix Table 6. Utah Placebo

	Weeks Received (1)
Panel A. Main Estimates	
Estimated Discontinuity	-0.227
	(0.327)
Observations	359
Bandwidth	12.26
Mean of Dependent Variable	10.43
Panel B. Placebo Estimates	
Estimated Discontinuity	0.494
	(0.300)
Observations	359
Bandwidth	11.72
Mean of Dependent Variable	11.67

Notes: As a placebo, we use administrative data from the state of Utah to estimate the same RD as in Missouri in a state where UI parameters were unchanged. The structure of the data and specification is identical to column (1) of Table 3.

Appendix Table 7. RDD Estimates of the Effect of the Cut in UI Maximum Duration on Employment and Reemployment Wages; Local Linear with IK Bandwidth and Excuding Outlier Cohort

	Employed 2011Q2 (1)	Employed 2011Q3 (2)	Employed 2011Q4 (3)	Employed 2012Q1 (4)	First complete quarter log reemployment wage (5)
Estimated Discontinuity	-0.029	0.085	0.082	0.072	0.035
	(0.010)	(0.020)	(0.024)	(0.022)	(0.037)
Observations	103	103	103	103	524
Bandwidth	5.12	6.10	5.53	5.53	6.66
Mean of Dependent Variable	0.71	0.72	0.72	0.70	8.73

Notes: Local linear RDD estimates using the IK optimal bandwidth and a triangular kernel. Observations are at the claim week level. Models are estimated using weekly averages of the dependent variable, weighting observations by the number of observations in the cell. Placebo estimates are from estimating the same specification with a threshold of one year prior to the April 2011 cut in benefits duration.

Appendix Table 8. RDD Estimates of the Effect of the Cut in UI Maximum Duration on Employment and Reemployment Wages; Local Polynomial with CCT Bandwidth

	Employed 2011Q2 (1)	Employed 2011Q3 (2)	Employed 2011Q4 (3)	Employed 2012Q1 (4)	First complete quarter log reemployment wage (5)
Estimated discontinuity	-0.015	0.094	0.079	0.058	0.20
	(0.019)	(0.037)	(0.038)	(0.040)	(0.11)
Observations	104	104	104	104	525
Bandwidth	13.49	11.20	10.53	11.11	16.61
Mean of Dependent Variable	0.84	0.80	0.75	0.71	8.60

Notes: Local quadratic RDD estimates using the CCT bandwidth and a triangular kernel. Observations are at the claim week level. Models are estimated using weekly averages of the dependent variable, weighting observations by the number of observations in the cell. Placebo estimates are from estimating the same specification with a threshold of one year prior to the April 2011 cut in benefits duration.

Appendix Table 9. DiD Estimates of the Change in the Missouri Unemployment Rate, Log Number of Unemployed, and Log Size of the Labor Force following the April 2011 UI Maximum Duration Cut; Current Population Survey Sample

	UR	UR	UR	ln(U)	ln(U)	ln(U)	ln(LF)	ln(LF)	ln(LF)
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Panel A. All States									
Missouri * Post	-0.805	-0.826	-1.07	-0.111	-0.117	-0.14	-0.017	-0.010	-0.002
SE	(0.279)	(0.558)		(0.039)	(0.079)		(0.007)	(0.013)	
PCSE	(0.897)	(1.24)		(0.031)	(0.086)		(0.009)	(0.017)	
Wild Bootstrap C.I.	(-1.1, -0.6)	(-1.0, -0.6)		(-0.14, -0.08)	(-0.14, -0.09)		(-0.02, -0.01)	(-0.02, -0.01)	
%-tile rank	0.078	0.098	0.059	0.118	0.039	0.078	0.216	0.196	0.451
Observations	2856	2856	2576	2856	2856	2576	2856	2856	2576
Panel B. Neighbors									
Missouri * Post	-0.900	-0.656	-0.745	-0.107	-0.078	-0.125	-0.015	-0.009	-0.027
SE	(0.263)	(0.526)		(0.038)	(0.077)		(0.007)	(0.013)	
PCSE	(0.332)	(0.425)		(0.038)	(0.058)		(0.008)	(0.016)	
Wild Bootstrap C.I.	(-1.3, -0.4)	(-0.9, -0.4)		(-0.15, -0.06)	(-0.12, -0.03)		(-0.03, 0.00)	(-0.02, 0.00)	
%-tile rank	0.111	0.111	0.000	0.000	0.111	0.000	0.222	0.222	0.000
Observations	504	504	448	504	504	448	504	504	448
Predicted change	-0.64	-0.64	-0.64	-0.10	-0.10	-0.10			
Pred. chg. no outlier	-0.45	-0.45	-0.45	-0.07	-0.07	-0.07			
MO*trend		X			X			X	
Synthetic control			X			X			X

Notes: Observations are state by month units. UR is the unemployment rate, ln(U) is the natural log of the number of unemployed, and ln(LF) is the natural log of the size of the labor force. Variables are derived from the BLS Current Population Survey and deseasonalized as described in the text. The sample spans January 2009 to August 2013. SE is the OLS standard error, PCSE is the panel corrected standard error, and the permutation %-tile rank is the percentage of states that have a more negative "effect" when estimating the same model 51 times, assigning each state to be the "treated" state in each permutation. MO*trend allows for a Missouri specific trend. The synthetic control uses weights from the synthetic control method described in the text to form a control group. Predicted change is the change in UR and ln(U) that is predicted by the RDD estimates of the change in the survivor function assuming no spillover effects. "Pred. chg. no outlier" is the same prediction without the outlier cohort.

Appendix Table 10. Ibragimov and Müller p-values by Block Sizes; Variables Derived from Current Population Survey

	(1) 56 blocks	(2) 28 blocks	(3) 14 blocks	(4) 8 blocks	(5) 6 blocks	(6) 4 blocks
Panel A. Unweighted Control; All States (Estimate = -0.86)						
t-statistic	3.85	4.60	5.68	4.38	9.72	6.29
Two-tail p-value (N-2 DOF)	0.00	0.00	0.00	0.02	0.01	0.10
N	56	28	14	8	6	4
Panel B. Synthetic Control; All States (Estimate = -1.03)						
t-statistic	4.22	4.55	5.07	4.19	9.83	46.96
Two-tail p-value (N-2 DOF)	0.00	0.00	0.00	0.02	0.01	0.01
N	56	28	14	8	6	4
Panel C. Unweighted Control; Neighbors (Estimate = -0.900)						
t-statistic	4.05	4.75	5.34	6.79	7.02	9.53
Two-tail p-value (N-2 DOF)	0.00	0.00	0.00	0.01	0.02	0.07
N	56	28	14	8	6	4
Panel D. Synthetic Control; Neighbors (Estimate = -0.75)						
t-statistic	3.07	3.71	4.87	8.08	3.70	18.72
Two-tail p-value (N-2 DOF)	0.00	0.00	0.00	0.00	0.07	0.03
N	56	28	14	8	6	4

Notes: Each column reports the t-statistic and corresponding two-tail p-value with N-2 degrees of freedom for the two sample t-test of equality of the difference between the Missouri and comparison group unemployment rate before and after the potential duration cut, where the monthly Missouri – comparison group unemployment rate differences have been collapsed into the specified number of blocks. In Panel A the comparison group is the equally weighted average of the monthly unemployment rate for all states and the District of Columbia excluding Missouri. In Panel B the comparison group is the synthetic control discussed in the text. We limit the sample to 28 months on each side of the policy change. Unemployment rates are derived from BLS CPS data.

Appendix Table 11. Comparison of Characteristics of Missouri and All Other States

	Full sample	Full sample	Unemployed sample	Unemployed sample
	Missouri	All other states	Missouri	All other states
	(1)	(2)	(3)	(4)
High School Degree or Lower	40.7	37.7	60.8	57.1
	[49.1]	[48.4]	[48.7]	[49.4]
Bachelors Degree+	15.4	20.7	6.0	15.7
	[36.1]	[40.5]	[23.8]	[36.4]
Married	40.7	40.3	37.9	38.8
	[49.1]	[49.1]	[48.5]	[48.7]
Never Married	21.4	23.6	40.3	40.1
	[41.0]	[41.5]	[49.0]	[49.0]
Working Age	51.8	51.9	69.7	75.5
	[50.0]	[50.0]	[46.0]	[43.0]
Seniors	12.7	12.9	1.2	3.2
	[33.3]	[33.5]	[10.9]	[17.5]
Makes <30k	44.3	23.6	45.6	38.3
	[44.3]	[42.5]	[49.8]	[48.6]
Makes <50k	43.8	41.6	62	59.9
	[49.6]	[49.3]	[48.5]	[49.0]
Makes <75k	48.4	59	77.8	76.9
	[48.4]	[49.2]	[41.6]	[42.2]
Black	11.7	12.5	17.5	18.2
	[32.1]	[33.1]	[38.0]	[38.6]
Hispanic	3.4	15.8	3.8	18.6
	[18.1]	[36.5]	[19.0]	[38.8]
Non-White	20.3	15.2	21.1	25.2
	[40.2]	[35.9]	[40.8]	[43.4]
NILF	49.5	49.5	0.0	0.0
	[50.0]	[50.0]	[0.0]	[0.0]
Unemployed	5.6	4.8	100	100
	[22.9]	[21.3]	[0.0]	[0.0]
Observations	2,369	133,109	130	6,088

Notes: Variables derived from the March 2010 Current Population Survey. All descriptive statistics are sample weighted by the CPS household weight. Columns (1) and (2) compare the demographics of all Missourians to Americans that do not live in Missouri. In Columns (3) and (4), we compare the demographics of Missouri's unemployed to unemployed Americans that do not live in Missouri. Unemployed is as a percentage of the population.