

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

June 2015

Andrew C. Johnston

University of Pennsylvania, Wharton

Alexandre Mas

Princeton University, IZA, and NBER

ABSTRACT

In this paper we examine how an unanticipated cut in potential unemployment insurance (UI) duration, which reduced maximum duration in Missouri by 16 weeks, affected the search behavior of UI recipients and the aggregate labor market. Using a regression discontinuity design (RDD), we estimate that a one-month reduction in maximum duration is associated with 15 fewer days of UI receipt and 8.6 fewer days of nonemployment. We use the RDD estimates to simulate the change in the time path of the unemployment rate assuming there are 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 was able to absorb this influx of workers without crowding out other jobseekers.

\*We are grateful to David Card, Mark Duggan, Henry Farber, Robert Jensen, Pauline Leung, Olivia S. Mitchell, Zhuan Pei, Jesse Rothstein, Steven Woodbury, workshop participants at Princeton University and the New York Federal Reserve, and seminar participants at the ABL conference. Elijah De La Campa, Samsun Knight and Dan Van Deusen provided excellent research assistance.

## I. INTRODUCTION

An important question in the analysis of unemployment insurance (UI) programs is how recipients respond to changes in the potential duration of UI, and how these responses affect the labor market as a whole. This question is particularly relevant for understanding the performance of the US labor market in the Great Recession and its aftermath. It is well documented that UI recipients are responsive to changes in maximum UI duration, though the evidence for the recent period is thin.<sup>1</sup> 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 UI recipients crowded out other jobseekers. With the notable exceptions of Levine (1993), Lalive et al. (2013) and Valleta (2014) we still know relatively little about the relationship between the micro and macro responses to UI extensions.<sup>2</sup>

In this paper we study the micro and macro effects of a large benefit duration cut that occurred in 2011 in Missouri using newly available administrative data and regression discontinuity and difference-in-difference designs. Following the 2007 recession, eight US states reduced regular UI durations, partly in response to diminished reserves in state UI trust funds as well as the political environment. While there is a precedent for cutting UI benefit levels, to our

---

<sup>1</sup> Studies that have found this relationship include Card and Levine (2000), Katz and Meyer (1990), Moffitt (1985) and Solon (1979) in the United States and Card, Chetty, Weber (2007), Lalive (2008), Schmieder, Von Wachter, and Bender (2012), and Schmieder, Von Wachter, and Bender (2014) in Western Europe.

<sup>2</sup> 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. There is also a literature testing for externalities from job search assistance programs in Western Europe. These include Blundell et al. (2004), Crepon et al. (2013), Ferracci et al. (2010), and Gautier et al. (2012). Davidson and Woodbury 1993 consider displacement effects from reemployment bonuses in the US. General equilibrium estimates in Hagedorn et al. (2013), Hagedorn et al. (2015), and Marinescu (2014) are also related to tests for the presence of externalities.

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, the standard level in place for over half a century.<sup>3</sup>

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 had received if they had applied previously.<sup>4</sup> The policy change was sudden and unanticipated; only five days passed between when the legislation was first proposed as a compromise in a negotiation aimed at breaking a filibuster in the Missouri State Senate and when it applied to UI claimants. 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.<sup>5</sup>

Our findings indicate economically and statistically significant rates of exit from UI for claimants subject to the shorter benefit duration relative to claimants with the longer duration, 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) in the case of

---

<sup>3</sup> In 2010 all states had a maximum duration of benefit eligibility of at least 26 weeks.

<sup>4</sup> Specifically, the cuts were 16 weeks for UI recipients previously eligible for 26 weeks of regular state UI and eligible to participate in the EUC program.

<sup>5</sup> 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.

Austria, and Schmieder, Von Wachter, and Bender (2012) in the case of Germany, we find evidence that at least some of the UI recipients are forward-looking. For example, UI recipients subject to the benefit cuts had 57 weeks of eligibility, but were 10 percentage points less likely to be receiving UI by week 20 of their spell, from a base of 56 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 47 percent of this change is through changes in exit rates occurring prior to benefit exhaustion.

Analysis of wage records for the universe of Missouri workers whose employers paid UI payroll taxes indicate that the early exit from UI we observe was largely due to individuals entering employment. The estimates imply that a one-month cut in potential duration resulted in a reduction of nonemployment duration of approximately 8.6 days. The findings suggest that the benefit cut increased job search intensity. However, we find 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 wage stickiness (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 et al. 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 predicted change in the path of the unemployment rate from the benefit, using the change in the estimated survivor function 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 then compare this predicted change 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 line up closely. Both the predicted and estimated paths follow a similar U-shaped pattern peaking at just under a 1 percentage point drop in the state unemployment rate. We find no evidence that the cut led to changes in the size of the labor force. The analysis suggests that the labor market absorbed the jobseekers exposed to the policy without displacement, even though the unemployment rate was still 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 et al. (2010).

Our study also speaks to the question of the labor market effects of UI extensions during the Great Recession. Over this period, UI benefits increased from the near-universal length of 26 weeks to up 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 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 (Rothstein 2011; Farber and Valleta 2015) and declines (Farber, Rothstein, and Valleta 2015) in UI potential duration in the

US over the 2007 recession period and its aftermath. These researchers found fairly small effects of changes in benefit lengths on unemployment. Hagedorn et al. (2013) and Hagedorn et al. (2015) find small effects of changes in potential duration on jobseekers, but large macro effects on wages, job vacancies, labor force participation and employment. To our knowledge, ours is the first study to use a design-based approach with administrative micro data to study the labor market effects of changes in maximum duration in this period. In contrast to other studies, we find a fairly large response to the benefit cut for a subset of participants. At the same time, we also find no evidence of moral hazard for the long-term unemployed.

## II. INSTITUTIONAL BACKGROUND

In the United States, UI is administered by state governments but is overseen and regulated by the federal government. In most states, eligible laid-off workers receive up to 26 weeks of regular unemployment insurance benefits if they are not reemployed before their benefits are exhausted. During periods of unusually high unemployment, state and federal governments have extended potential benefit duration, so that the long-term unemployed continue to be supported after regular benefits are exhausted. Two programs provide these extended benefits: the Extended Benefit (EB) and the EUC programs.

EB is a permanent program that provides extended benefits to eligible unemployed workers in states with high unemployment. 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 the cost of EB until December 2013. EB extended benefits are triggered as a function of a state's total and insured

unemployment rate, and the trigger thresholds vary by state.<sup>6</sup> 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.<sup>7</sup>

EUC has been enacted periodically through federal legislation when unemployment is high. During the Great Recession, the EUC program was active from June 2008 through December 2013. In its most recent version, federal benefits provided longer extensions for states with higher insured unemployment.<sup>8</sup>

The benefit cut in Missouri was the byproduct of a Republican filibuster, led by four lawmakers of the Missouri State Senate, of legislation that would have accepted federal money to extend UI benefits under the EB program. The bill would have allowed for the continuation of 20 additional weeks of benefits to unemployed workers who exhausted their EUC and regular benefits at no cost to Missouri.<sup>9</sup> At the time of the filibuster, this bill 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, through the article did not specify the terms (Associated Press 2011). On April 8 there was an article in the *St. Louis Post Dispatch* detailing the compromise. Under the compromise, regular benefits would be cut from 26 to 20 weeks in exchange for Missouri

---

<sup>6</sup> In Missouri the 13-week EB extension can be triggered in two ways. First, EB is triggered if the unemployment rate among insured workers (IUR) is at least 5 percent over the previous 13 weeks and the IUR is 120 percent of the IUR for the same 13-week period in the previous two years. Second, EB can be triggered if the IUR for the previous 13 weeks is at least 6 percent, regardless of the IUR in previous years. If IUR crosses either of these thresholds, the state automatically enrolls unemployed workers in 13 additional weeks of benefits if they exhaust their regular benefits.

<sup>7</sup> 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>).

<sup>8</sup> If a state had less than 6% unemployment, EUC provided 14 additional weeks after regular benefits were exhausted; 28 weeks if less than 7% (but greater than 6%); 37 weeks if less than 9 (but greater than 7%); and 47 weeks if greater than 9%.

<sup>9</sup> The lawmakers leading the filibuster argued that accepting these funds would increase the federal deficit unnecessarily.

accepting federal dollars and maintaining the EB benefits for the long-term unemployed (Young 2011). In effect, the agreement traded-off longer UI durations in the short run (for the currently 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 signed into law by the Democratic governor, Jay Nixon, 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 maximum 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 was a non-factor for new claimants at this time (with or without the benefit cut) because EB phased out by the time they were eligible.

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.

In what follows, for convenience, we label recipients applying after the law 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 base-period earnings, and EB or EUC claims that could not be traced to an initial regular claim. 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 as well as 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

$\frac{E}{3B} \geq 26$ . 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 \$8259. Earnings in the first complete quarter of employment after the UI spell average \$7240. 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  $\times$  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  $\times$  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 the April 2011 legislation: claimants who applied just before April 13, 2011 were eligible for 73 weeks of benefits and those who applied after for 57 weeks. We use this break to compare similar displaced workers graduating into the same labor market who experienced very different UI benefit durations.

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:

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

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.<sup>10</sup>

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. 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.<sup>11</sup> We consider a range of alternative bandwidths to assess robustness.

## 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 for 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

---

<sup>10</sup> 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.

<sup>11</sup> In practice, we use the IK bandwidth without the regularization term, since visual inspection led us to conclude that this gives a better approximation to the data. However, the bandwidths and the estimates are quantitatively and qualitatively close if we include the IK regularization term.

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 to get ahead of the benefit cut. Column 1 of Table 2 presents the corresponding McCrary (2008) test and confirms that there is no significant discontinuity in claims.

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 than recipients who applied before or after this group, and in particular they appear to have characteristics correlated with being lower duration claimants. This blip might be random noise, or it might reflect individuals' timing claims to obtain UI before the cut, but failing due to processing lags. We keep this group in the sample, but we find the estimates are of a similar magnitude and generally become more precise if this cohort is removed.

As a second test 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 by using all covariates available in the data set using the same variables and 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. The RDD estimate of this predicted value at the cutoff is small and statistically insignificant, which we present in column (2) of Table 2. 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. The lack of evidence of sorting and differences in pre-determined characteristics around the threshold reinforces what we know about the policy change, that it was unanticipated and difficult or impossible to game.<sup>12</sup>

### *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 claim 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 8.6 fewer weeks of UI benefits claimed (s.e. = 1.4), on average. Estimating the same model but setting the threshold for the same week one year prior to the cut shows an insignificant difference in the duration of UI receipt between treatment and control (Table 3/Panel B/column 1). Appendix Figure 1 shows the point-estimate estimated over a range of alternative bandwidths. The estimate is stable for bandwidths smaller than the IK bandwidth and up to twice as large as the IK bandwidth.

To examine the timing of UI receipt, we estimate the probability that an individual remains on UI through each of the first 73 weeks of the spell. Figure 4 presents binned scatterplots of the probability that claimants remained on UI in weeks 20, 40, 55, and 60 weeks of UI benefits 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 close to 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

---

<sup>12</sup> 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 will be seen in all subsequent analyses.

(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 approximately -10 percentage points in week 20, -9 in weeks 40 and 55, and -24 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 insignificant from 0 in all cases except for the probability of receiving benefits in week 20, which is positive.

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 5 plots each of these RDD estimates with the associated confidence intervals. Figure 5 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, and then levels out. This pattern implies that the hazard rate for UI exit is larger for the treatment group than for the control group in the first five months of the UI spell, and then stabilizes.

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 to receive benefits beyond the regular state benefits, either because they were not eligible or did not enroll for other reasons. 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 effects and behavioral effects. Nevertheless, this pattern shows that the RDD estimates are detecting expected changes in claiming behavior.

Excluding this 20–26 week period, the treatment/control differences in the survivor rate

looks 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 5 show that the first significant difference between the two groups occurs in week 13, and the differences remain significant for all subsequent weeks. These estimates indicate that there is a forward-looking response to cuts in UI potential duration and that most 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 2 shows the same plots with double and half the IK bandwidth and the patterns are similar.

One way to assess the consistency of the duration and survival RDD estimates is to note that mean UI duration is the integral of the survival function. Using the discrete analog to this relationship, summing the estimated survival probabilities through 73 weeks yields an expected duration of 31.1 weeks in the control group and 22.3 weeks in the treatment group. The 8.8 week difference in the expected duration implied by the survival probabilities is close to the 8.6 week RDD estimate. The consistency of the estimates further reinforces the validity of the design.

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 can use the estimated survival probabilities to 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 survival function implies that the expected duration conditional on duration being less than 58 weeks is 27.6 weeks in the control and 22.1 weeks in the treatment. Because  $E[\text{Duration}] = E[\text{Duration} | \text{Duration} < 58] * \Pr(\text{Duration} < 58) + E[\text{Duration} | \text{Duration} \geq 58] * \Pr(\text{Duration} \geq 58)$ , and  $\Pr(\text{Duration} < 58) \approx 0.74$  in the control group, approximately 47 percent of

the change in the overall duration of UI receipt comes from changes in the response to the cut before exhaustion.

### *Employment*

Using the quarterly wage files we can measure the relative rate of employment for the treatment and control groups following the policy change. Figure 6 plots the employment rate by UI 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 7 presents the RDD estimates and associated 95 percent confidence intervals for employment rates by quarter, starting in the quarter the policy went into effect in 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 9 percentage point higher employment rate than the comparison group, a difference that is significant at conventional levels. The difference in employment rates is similar to the 10 percentage point difference in the probability of receipt in the early part of the UI spells, suggesting that those individuals who leave UI before exhaustion tend to enter employment. The point estimates and standard errors for the employment RDD are presented in Table 4. The employment effect fades out by 2012 Q4 at which point both treatment and control had exhausted their benefits.

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 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 entire 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.<sup>13</sup> A caveat to this conclusion is that at the time the treatment group exhausts 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 8 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.

We can use the estimates corresponding to the relative nonemployment probabilities by quarter (shown in Figure 7) 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 all recipients have exhausted their benefits, summing the estimates in Figure 7 from the quarter of

---

<sup>13</sup> Appendix Figure 3 shows the same charts using twice and half the IK bandwidth.

the policy change through 2012 Q4 implies that a one-month reduction in unemployment duration reduces the number of days of nonemployment by an average of 8.6 days, with a 95 percent confidence interval of (5.5,11.8).<sup>14</sup>

### *Reemployment Wages*

A class of job search models predict that longer periods of unemployment benefits allow workers to increase their reservation wage to find a 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 Webber (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 wages do not change significantly following the cut in duration. Figure 9 shows the mean log reemployment wage by application week. 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. We then compare the estimated change in the Missouri

---

<sup>14</sup> 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.

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 spillovers. A comparison of the two series is informative about the degree of spillovers.

In Figure 10 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.

In Figure 11 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 month from January 2009 – March 2011 and 1-digit NAICS industries.<sup>15</sup> The figure plots the Missouri 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 just above a one-percentage point decline, and then gradually reverting back to the control.

Next we compare these relative changes in the state unemployment rate to the changes in the unemployment rate predicted by the RDD estimates. For every calendar week  $\tau$ , we compute the predicted change in the number of unemployed ( $\Delta\hat{n}_\tau$ ) due to the policy as:

$$\Delta\hat{n}_\tau = \sum_{t=0}^{57} (\hat{p}_t^T - \hat{p}_t^C) * c_{\tau-t} + \sum_{t=58}^{73} (-0.06) * c_{\tau-t},$$

---

<sup>15</sup> The procedure gives 0.2% weight to Alaska, 20.2% weight to Florida, 11.9% weight to Georgia, 1.2% weight to Idaho, 10.5% weight to Michigan, 1.5% weight to Nevada, 18.2% weight to Oklahoma, 13.5% weight to Texas, 22.7% weight to West Virginia, and 0 weight to all other states.

where  $c_{\tau-t}$  is the number of initial UI claims in week  $\tau - t$ , 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  $l_\tau$  is labor force participation.

Figure 12 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 line up fairly closely to the predicted change. In particular, the series exhibit the same U-shaped pattern with the unemployment decline peaking at close to 1 percentage point. The DiD estimates are close to the predicted effects through most of the period, except at the end of 2012 when the DiD series rise somewhat faster than the predicted rate. 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.

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×state level, and we estimate all models with state fixed effects, calendar month dummies, and with and without a Missouri-specific trend.<sup>16</sup> 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 (Kolesar and Imbens 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. For the synthetic control estimates, we report the percentile rank from the permutation exercise. 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 as assess the degree of spillovers.

The DiD estimate using the unweighted control is -0.94 percentage points (column 1), and -0.82 percentage points with a Missouri-specific trend. These estimates are interpretable as the difference in the Missouri unemployment rate in the period April 2011-December 2013 relative to January 2009-March 2011 and relative to the average change in all other states. The estimates are statistically significant 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 9.8% (column 1) and 0.0%

---

<sup>16</sup> 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.

(column 2) meaning that in specification 1, 9.8 percent of states have more negative estimated effects while in specification 2 no states have more negative estimated effects. Column (3) presents the synthetic control estimates, which are somewhat lower than the unweighted control. The DiD point-estimate is -0.54, which has an associated percentile rank of 3.9 percent. The estimate is close but somewhat smaller than the predicted change.

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 9.7–12.3 percent depending on the specification. These estimates are close to the predicted change in the number of unemployment from the RDD estimates of 8.2 percent. Columns (7)–(9) report the estimates for log size of the labor force. The estimates range from a -0.5 percent decline in the unweighted control with a Missouri-specific trend (column 7) to a -0.9 percent decline in the synthetic control (column 9). While the upper wild bootstrap confidence interval is just below 0 in column (7), the permutation percentile ranks of the estimates are large at 24 percent and 22 percent and we therefore conclude that there is little evidence of a statistically significant change in the size of the labor force. The change in the unemployment rate appears to be driven instead by a change in the number of unemployed rather than a change in the size of the labor force.

In Appendix Table 1 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 reasonably precise. This analysis shows that our findings are not driven by how the LAUS data are constructed.

Our conclusion from this analysis 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 receipt estimates imply that a one-month reduction in potential UI duration leads to a 15-day reduction in UI spells and 8.6 fewer days of nonemployment. These estimates are larger than what has been found in the literature using data from earlier decades in the US and in Western Europe. Katz and Meyer (1990) estimate that an extra four weeks of potential UI receipt increases UI spells by 4–6 days, and Card and Levine (2000) who find that the equivalent of four extra weeks of extended benefits in New Jersey increased UI spells by two days. The estimates on nonemployment duration are larger than those of Schmieder, Von Wachter, and Bender (2012) and Lalive (2008), who estimate that a one-month increase in potential duration increases nonemployment by approximately one day in the German and Austrian contexts, respectively.

We find that the increased hazard rate out of unemployment insurance occurs in the first twenty weeks of the UI spell and then stabilizes. While there is previous evidence of this kind of anticipatory effects (Schmieder, Von Wachter, and Bender 2012; Card, Chetty, and Weber 2007), it is perhaps surprising that UI recipients responded so early. 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

the change in behavior is mainly local to the time of the cut, and not as 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.

The findings suggest that there is a forward-looking group of recipients who respond early to changes in potential duration. However, the long-term unemployed who exhausted 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.

The estimated macro effect of the cut is also larger than what other papers have found. Marinescu (2014) estimates that a 10 percent increase in benefits corresponds to a 0.7 percent decline in the unemployment rate and Hagedorn et al.’s (2015) estimates imply that a 10 percent decrease in maximum benefit durations led to a 1.7 percent decrease in the unemployment rate as a result of the decrease in the number of unemployed. In our case, a 10 percent decrease in benefits was associated with approximately a 5 percent decrease in the unemployment rate. Unlike Hagedorn et al. (2015), however, we find no evidence that the benefit cut increased the labor force participation rate.



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. (2013), 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. We note that while the labor market was weak at the time of the benefit cut, with an unemployment rate of 8.6 percent, the labor market nationally was mending, and the market response may have been different if the unemployment rate were even higher or on an upward trajectory.

## REFERENCES

- Abadie, Alberto, and Javier Gardeazabal. 2003. "The economic costs of conflict: A case study of the Basque Country." *American Economic Review*, 93(1): 113-132.
- Abadie, Alberto, Alexis Diamond, and Jens Hainmueller. 2010. "Synthetic control methods for comparative case studies: Estimating the effect of California's tobacco control program." *Journal of the American Statistical Association*, 105(490): 493-505.
- Associated Press. 2011. "Lembke Ends Filibuster Blocking Jobless Benefits." Jefferson, Missouri. *CBS-Saint Louis*. (<http://stlouis.cbslocal.com/2011/04/06/lembke-ends-filibuster-blocking-jobless-benefits/> on January 28, 2015).
- Bertrand, Marianne, Esther Duflo, and Sendhil Mullainathan. 2004. "How Much Should We Trust Differences-In-Differences Estimates?" *The Quarterly Journal of Economics*, 119(1): 249-275.
- Blundell, Richard, Monica Costa Dias, Costas Meghir, and John Van Reenen. 2004. "Evaluating The Employment Impact Of A Mandatory Job Search Program." *Journal Of The European Economic Association* 2, no. 4: 569-606.
- Cameron, A. Colin, Jonah B. Gelbach, and Douglas L. Miller. 2008. "Bootstrap-Based Improvements For Inference With Clustered Errors." *Review of Economics and Statistics* 90, no. 3: 414-427.
- Card, David, Raj Chetty, and Andrea Weber. 2007. "Cash-on-Hand and Competing Models of Intertemporal Behavior: New Evidence from the Labor Market." *The Quarterly Journal of Economics*, 122(4): 1511-1560.
- Card, David, and Phillip B. Levine. 2000. "Extended benefits and the duration of UI spells: evidence from the New Jersey extended benefit program." *Journal of Public Economics*, 78(1): 107-138.
- Card, David, Andrew Johnston, Pauline Leung, Alexandre Mas, and Zhuan Pei. 2015. "The Effect of Unemployment Benefits on the Duration of Unemployment Insurance Receipt: New Evidence from a Regression Kink Design in Missouri, 2003-2013." NBER Working Paper No. 20869.
- Crépon, Bruno, Esther Duflo, Marc Gurgand, Roland Rathelot, and Philippe Zamora. 2013. "Do Labor Market Policies have Displacement Effects? Evidence from a Clustered Randomized Experiment." *The Quarterly Journal of Economics*, 128(2): 531-580.
- Davidson, Carl and Stephen A. Woodbury. 1993. "The displacement effect of reemployment bonus programs." *Journal of Labor Economics*, 11(4): 575-605.
- Farber, Henry S., and Robert G. Valletta. 2013. "Do extended unemployment benefits lengthen unemployment spells? Evidence from recent cycles in the US labor market." NBER Working Paper No. 19048.

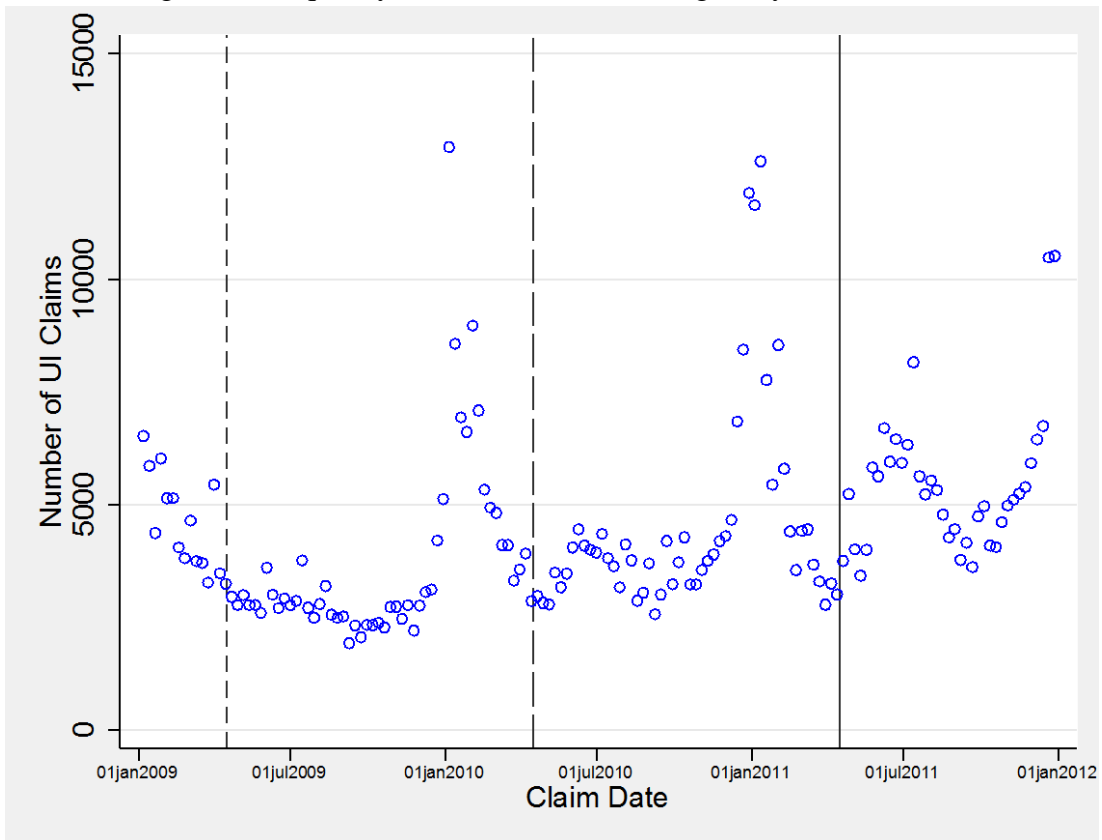
- Farber, Henry S., Jesse Rothstein, and Robert G. Valletta. 2015. "The Effect of Extended Unemployment Insurance Benefits: Evidence from the 2012-2013 Phase-Out." Federal Reserve Bank of San Francisco Working Paper 2015-03.
- Gautier, Pieter A., Paul Muller, Bas van der Klaauw, Michael Rosholm, and Michael Svarer. 2012. "Estimating Equilibrium Effects of Job Search Assistance". Tinbergen Institute No. 12-071/3.
- Grubb, David. 2011. "Assessing the impact of recent unemployment insurance extensions in the United States." OECD Working paper, May 25 2011.
- Hagedorn, Marcus, Fatih Karahan, Iourii Manovskii, and Kurt Mitman. 2013. "Unemployment benefits and unemployment in the great recession: the role of macro effects." NBER Working Paper No. 19499.
- Hagedorn, Marcus, Fatih Karahan, Iourii Manovskii, and Kurt Mitman. 2014. "Case Study of Unemployment Insurance Reform in North Carolina." Unpublished working paper.
- Hagedorn, Marcus, Iourii Manovskii, and Kurt Mitman. 2015. "The Impact of Unemployment Benefit Extensions on Employment: The 2014 Employment Miracle?" NBER Working Paper No. 20884.
- Hahn, Jinyong, Petra Todd, and Wilbert Van der Klaauw. 2001. "Identification and estimation of treatment effects with a regression-discontinuity design." *Econometrica*, 69(1): 201-209.
- Hall, Robert E. 2005. "Employment Fluctuations with Equilibrium Wage Stickiness." *The American Economic Review* 95, no. 1: 50-65.
- Imbens, Guido W., and Karthik Kalyanaraman. 2012. "Optimal Bandwidth Choice For The Regression Discontinuity Estimator." *The Review of Economic Studies*, 79(3): 933-959.
- Imbens, Guido W., and Michal Kolesar. 2012. "Robust Standard Errors in Small Samples: Some Practical Advice." NBER Working Paper No. 18478.
- Katz, Lawrence F., and Bruce D. Meyer. 1990. "The Impact Of The Potential Duration Of Unemployment Benefits On The Duration Of Unemployment." *Journal of Public Economics*, 41(1): 45-72.
- Lalive, Rafael. 2008. "How do Extended Benefits Affect Unemployment Duration? A Regression Discontinuity Approach." *Journal of Econometrics*, 142(2): 785-806.
- Lalive, Rafael, Camille Landais, and Josef Zweimuller. 2013. "Market Externalities of Large Unemployment Insurance Extension Programs." Institute for the Study of Labor (IZA) Discussion Paper 7650.
- Landais, Camille, Pascal Michailat, and Emmanuel Saez. 2010. "Optimal Unemployment Insurance Over The Business Cycle". No. w16526. National Bureau of Economic Research.

- Lee, David S., and David Card. 2008. "Regression Discontinuity Inference With Specification Error." *Journal of Econometrics*, 142(2): 655-674.
- Levine, Phillip B. 1993. "Spillover Effects Between The Insured And Uninsured Unemployed." *Industrial & Labor Relations Review*, 47(1): 73-86.
- Mannies, Jo. 2011. "Missouri Legislators Cut Unemployment Benefits." *The St. Louis American* ([http://www.stlamerican.com/news/community\\_news/article\\_b4dd5a7a-6baa-11e0-88ab-001cc4c002e0.html](http://www.stlamerican.com/news/community_news/article_b4dd5a7a-6baa-11e0-88ab-001cc4c002e0.html) on January 29, 2015)
- Marinescu, Ioana. 2014. "The General Equilibrium Impacts of Unemployment Insurance: Evidence from a Large Online Job Board." Unpublished working paper.
- Meyer, Bruce D. 1990. "Unemployment Insurance and Unemployment Spells." *Econometrica*, 58(4): 757-782.
- McCrary, Justin. 2008. "Manipulation of the running variable in the regression discontinuity design: A density test." *Journal of Econometrics*, 142(2): 698-714.
- Michaillat, Pascal. 2012. "Do Matching Frictions Explain Unemployment? Not In Bad Times." *The American Economic Review* 102, no. 4: 1721-1750.
- Moffitt, Robert. 1985. "Unemployment Insurance And The Distribution Of Unemployment Spells." *Journal of Econometrics* 28, no. 1: 85-101.
- Pissarides, Christopher A. 2000. *Equilibrium Unemployment Theory*. . 2nd ed., Cambridge, MA:MIT Press.
- Rothstein, Jesse. 2011. "Unemployment Insurance and Job Search in the Great Recession." *Brookings Papers on Economic Activity*, 2011(2): 143-213.
- Selway, William. 2011. "Broke U.S. States' \$48 Billion Debt Drives Unemployment Aid Cuts." *Bloomberg Business* (<http://www.bloomberg.com/news/articles/2011-04-15/broke-u-s-states-48-billion-debt-drives-unemployment-assistance-cuts> on January 29, 2015)
- Schmieder, Johannes F., Till von Wachter, and Stefan Bender. 2012. "The Effects of Extended Unemployment Insurance Over the Business Cycle: Evidence from Regression Discontinuity Estimates Over 20 Years." *The Quarterly Journal of Economics*, 127(2): 701-752.
- Schmieder, Johannes F., Till von Wachter, and Stefan Bender. 2013. "The Effect of Unemployment Duration on Wages: Evidence from Unemployment Insurance Extensions." NBER Working Paper No. 19772.
- Solon, Gary. 1979. "Labor Supply Effects of Extended Unemployment Benefits." *Journal of Human Resources*, 14(2): 247-255.
- Valletta, Robert G. 2014. "Recent Extensions Of US Unemployment Benefits: Search Responses In Alternative Labor Market States." *IZA Journal of Labor Policy* 3, no. 1: 18.

Wing, Nick. 2011. "Missouri State Lawmaker: Unemployed Should 'Get Off Their Backsides,' Get Jobs." *The Huffington Post*. ([http://www.huffingtonpost.com/2011/03/03/jim-lembke-missouri-unemployed\\_n\\_830892.html](http://www.huffingtonpost.com/2011/03/03/jim-lembke-missouri-unemployed_n_830892.html) on May 5, 2015).

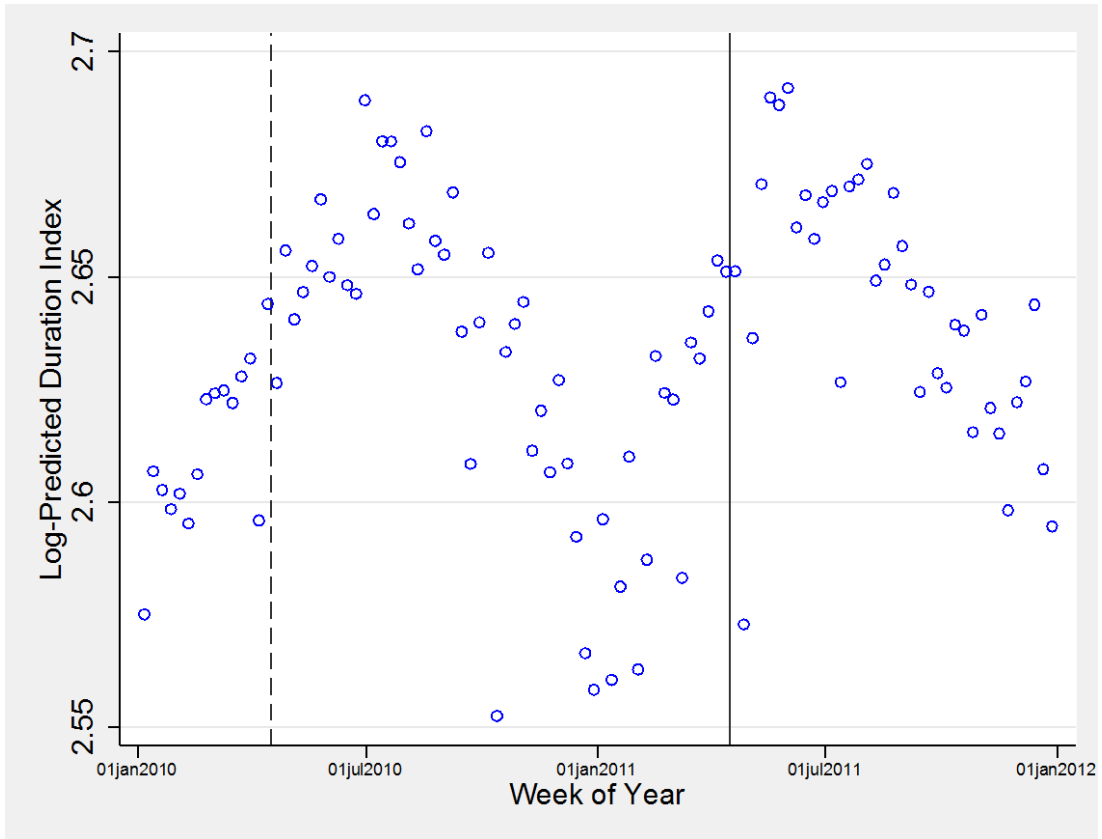
Young, Virginia. 2011. "Senate offers deal on Missouri jobless benefits." *St. Louis Post Dispatch*. ([http://www.stltoday.com/news/local/govt-and-politics/senate-offers-deal-on-missouri-jobless-benefits/article\\_1be0146f-2221-597d-8d9a-1b2ef1a5ca9a.html](http://www.stltoday.com/news/local/govt-and-politics/senate-offers-deal-on-missouri-jobless-benefits/article_1be0146f-2221-597d-8d9a-1b2ef1a5ca9a.html) on May 5, 2015).

Figure 1: Frequency Distribution of Full Eligibility Initial Claims



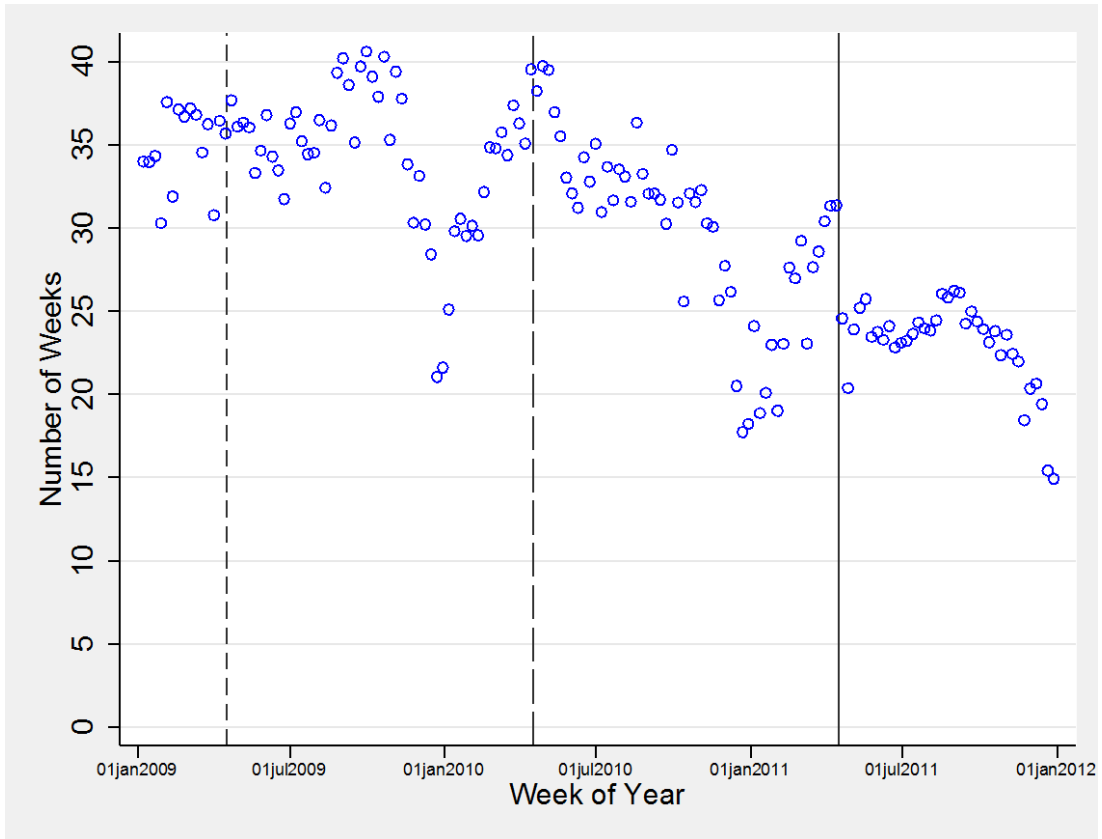
Notes: This figure plots the number of initial UI claims for workers eligible for the maximum duration of regular benefits (26 weeks before the cut and 20 weeks after the cut) by claim week.

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.

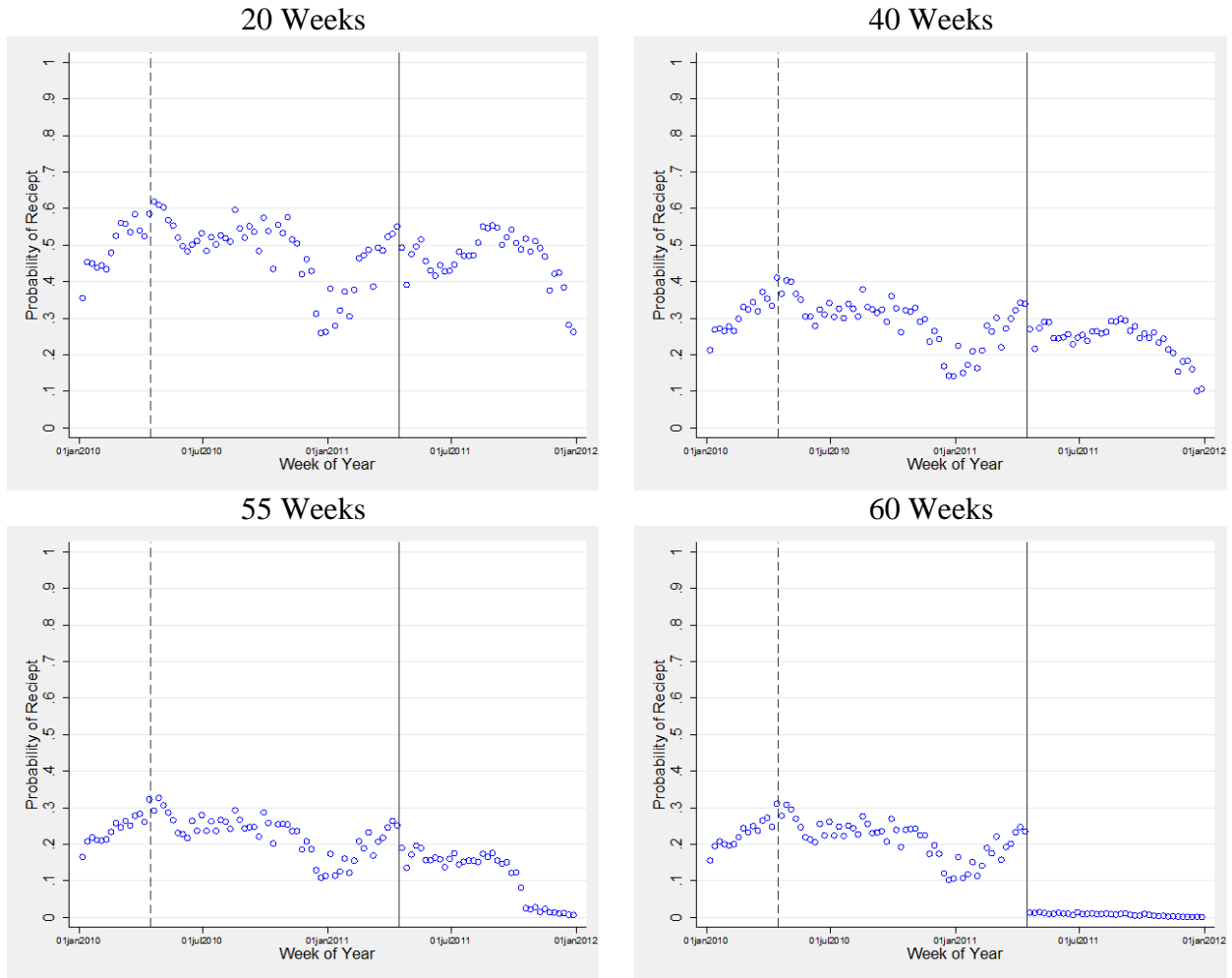
Figure 3: Average UI Spell by Application Week



Notes: This figure plots the mean UI spell 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.

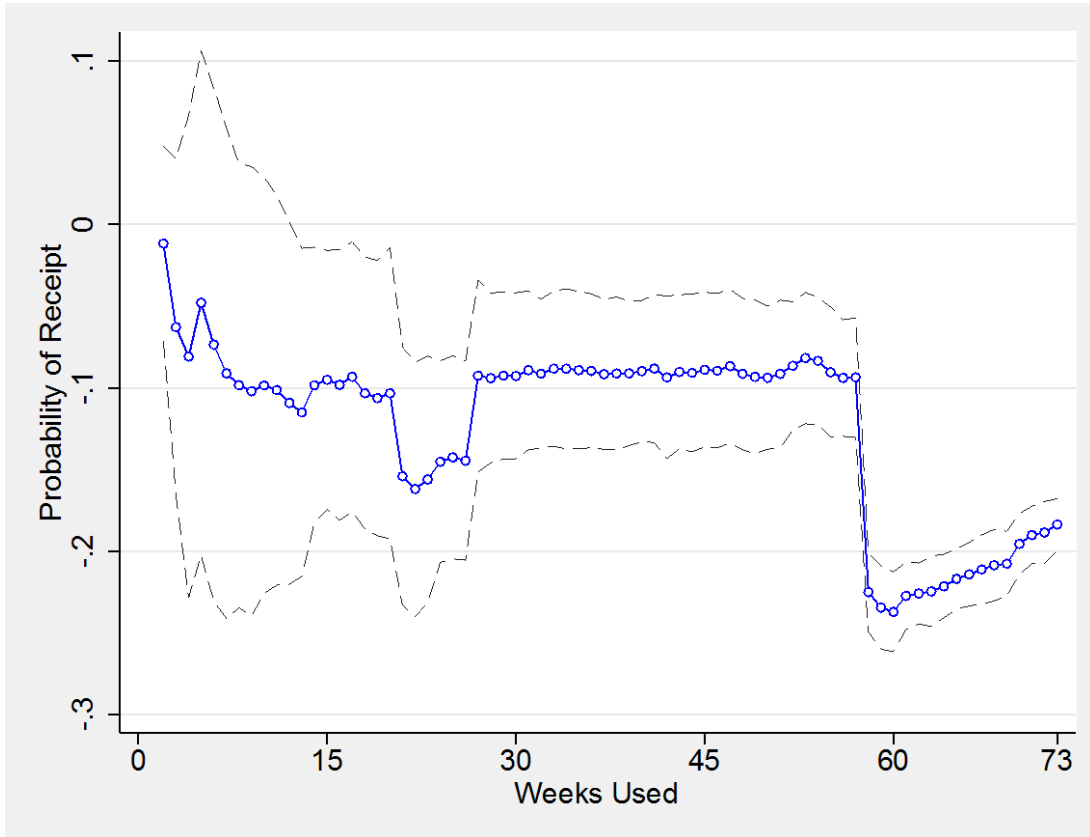


Figure 4: Probability UI Spell Length was at least:



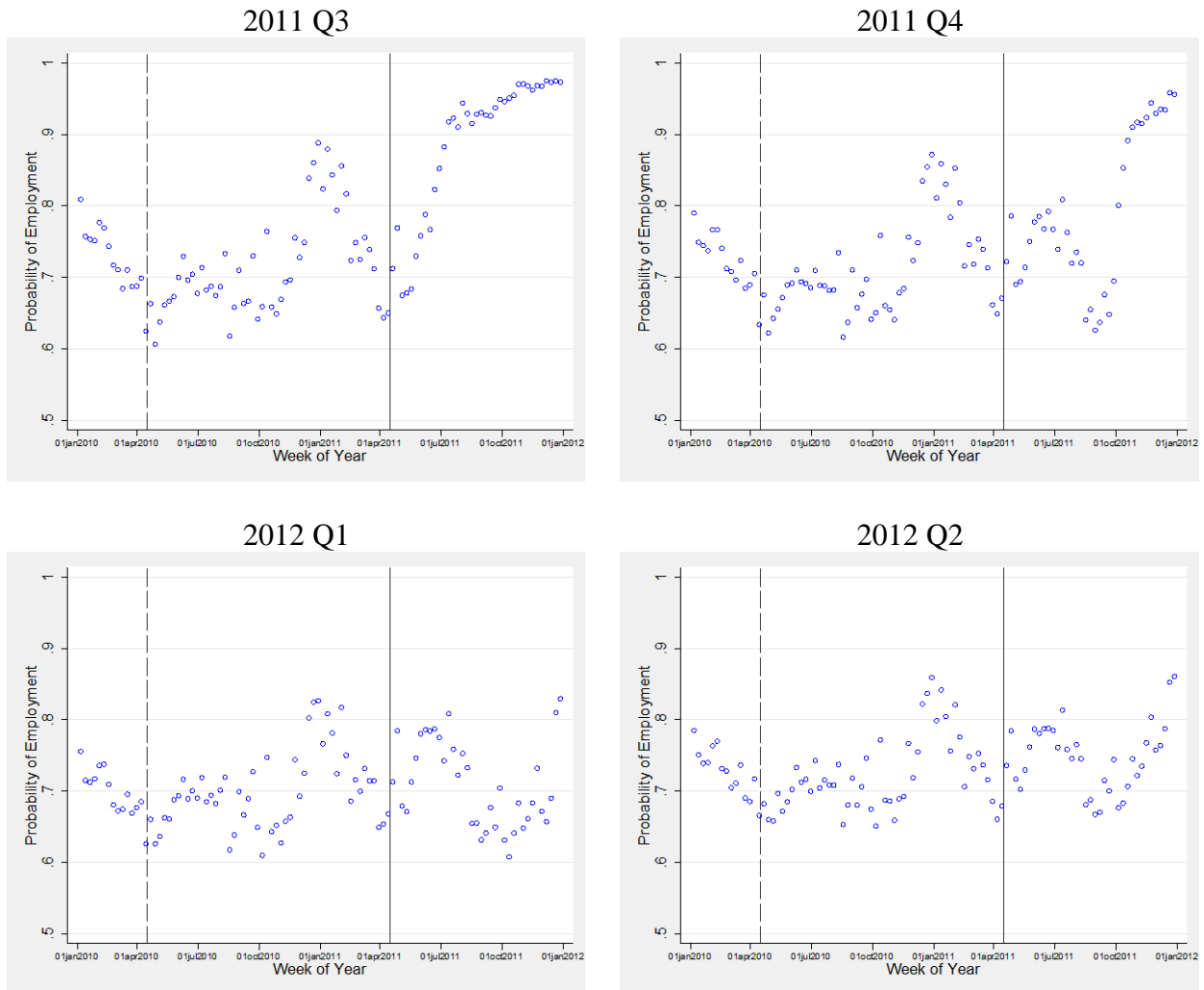
Notes: The figures plot the probability that UI recipients were collecting UI 20, 40, 55, and 60 weeks into their spell, by initial claim week. The solid vertical lines denote the week of the UI potential duration cut. The dashed vertical lines represent the same week in 2010.

Figure 5. RDD Estimates of the Probability of Claiming UI for Weeks 1-73 of the Spell



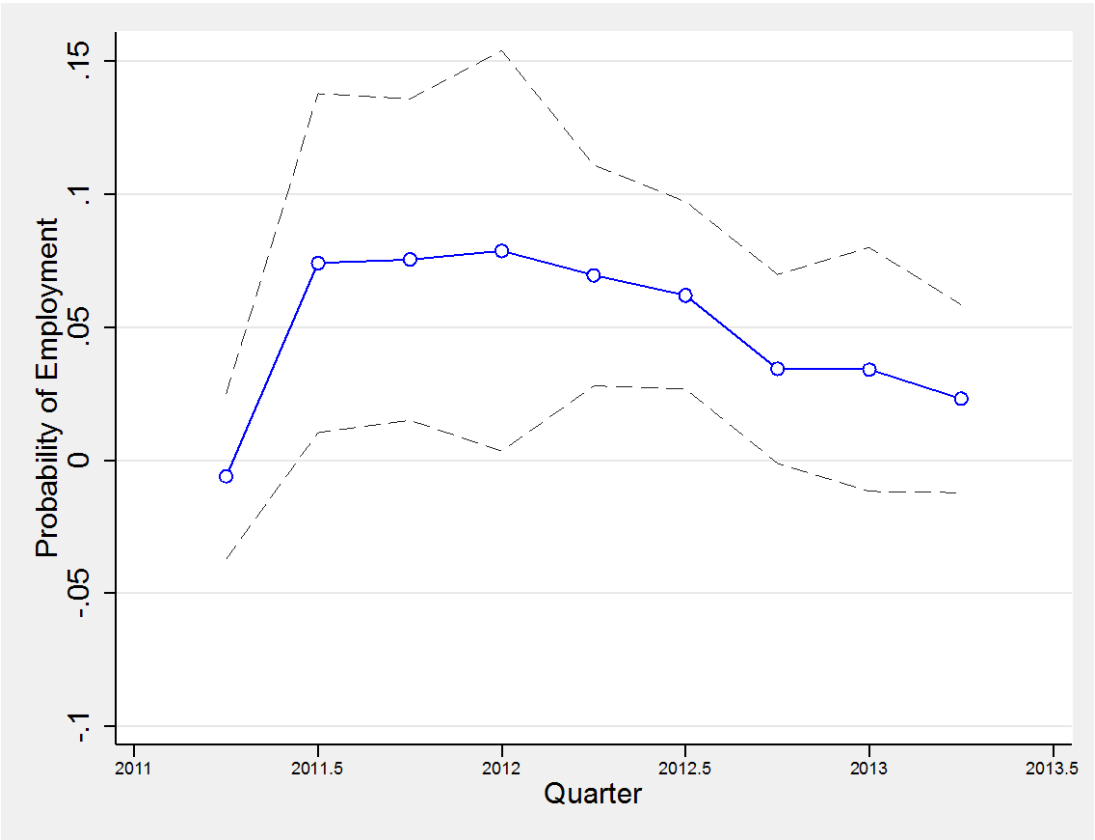
Notes: Each point is an RDD estimate for the probability that a recipient claims X weeks of UI, for X spanning 1 to 73. The dashed lines are the 95% confidence interval.

Figure 6. Probability Claimant Had Positive Earnings in:



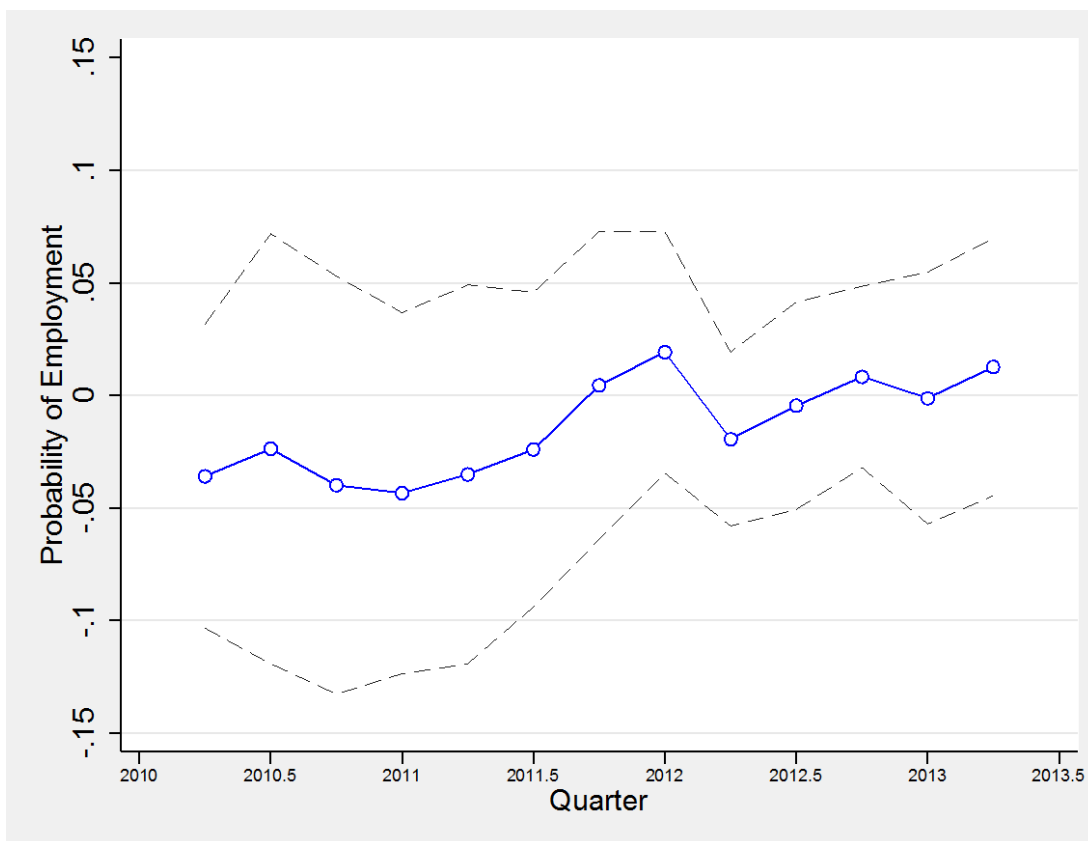
Notes: The figures plot the probability that a UI claimant has positive earnings in 2011 Q3, 2011 Q4, 2012 Q1, and 2012 Q2, by week of initial claim. The solid vertical line denotes the week of the cut in UI potential duration, and the dashed vertical line denotes the same week in 2010.

Figure 7. RDD Estimates of the Probability of Positive Earnings by Quarter following April 2011 UI Duration Cut



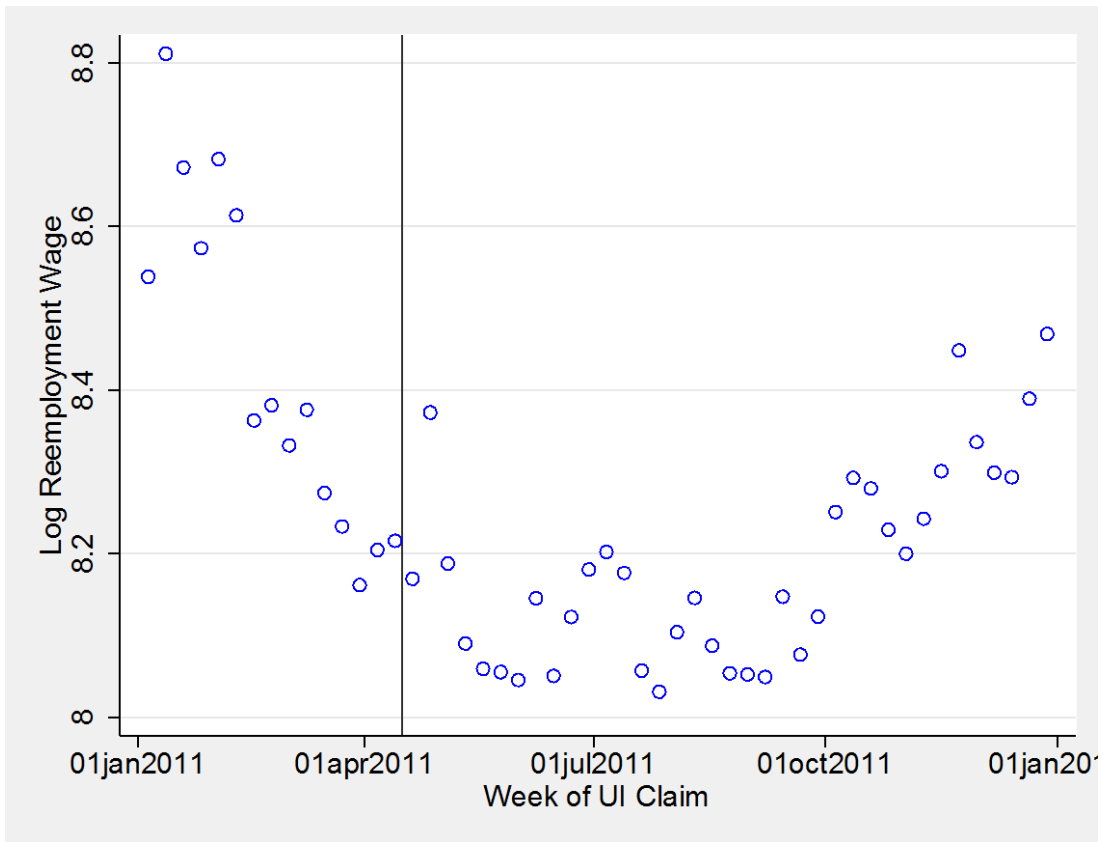
Notes: Each point is the RDD estimate for the probability that a UI claimant has positive earnings in each quarter subsequent to the cut in potential UI duration. The dashed lines are the 95% confidence interval.

Figure 8: RDD Estimates of the Probability of Positive Earnings by Quarter Subsequent to April 2010 Placebo Cut



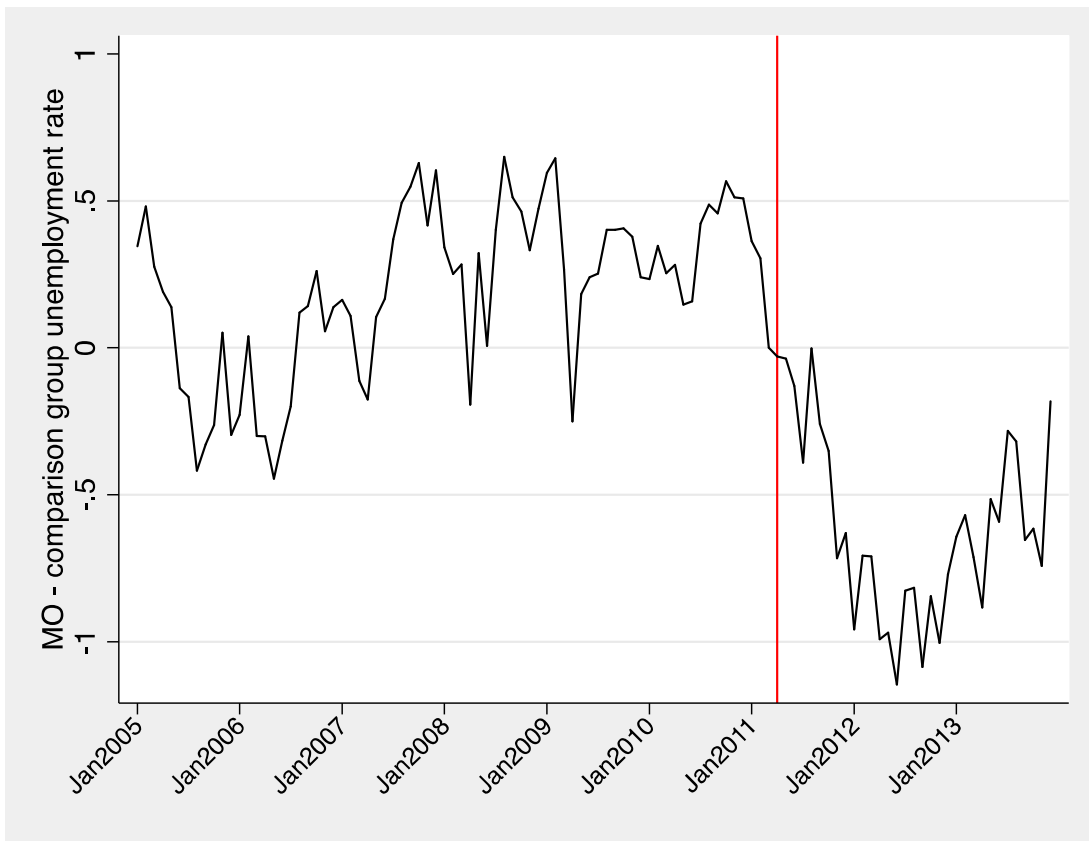
Notes: Placebo estimates. Each point is the RDD estimate for the probability that a UI claimant has positive earnings setting the UI benefit cut threshold to April 2010, one year prior to the actual cut in UI duration. The dashed line is the 95% confidence interval.

Figure 9: Log Reemployment Wage



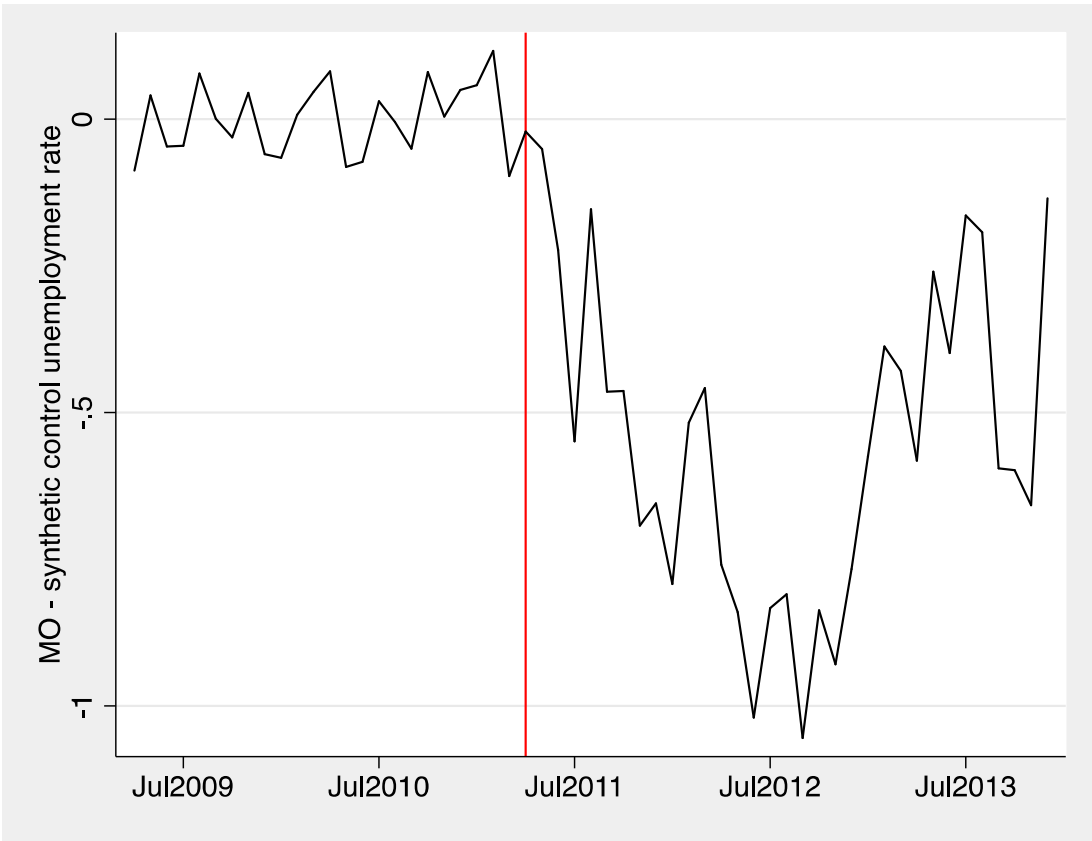
Notes: The figure plots the mean of log earnings for the first complete quarter of earnings after a UI claim.

Figure 10: 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 unemployment rate for all other 49 states and the District of Columbia. The series is normalized to 0 in March 2011. The vertical line denotes the month of the cut in potential UI duration.

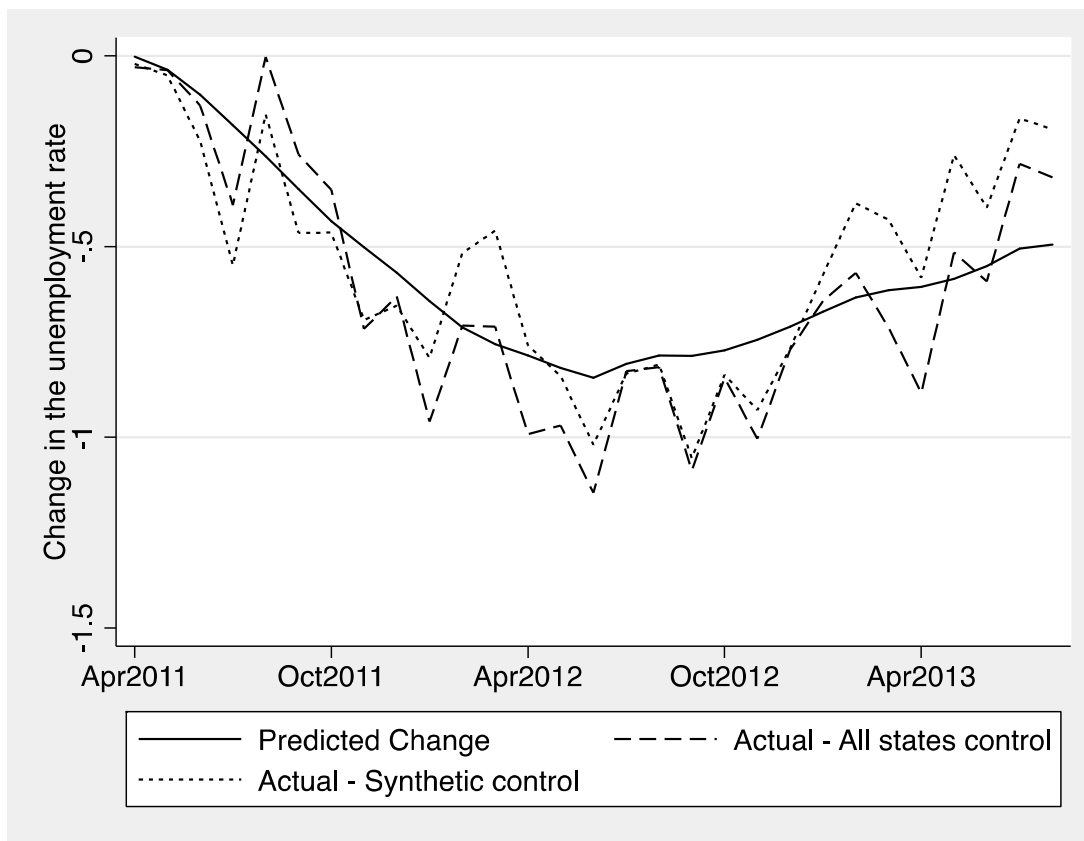
Figure 11: Difference Between the Missouri Unemployment Rate and the Synthetic Control Unemployment Rate



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



Figure 12: 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, plotted by week. “Actual – All states control” is the difference between the Missouri unemployment rate and the unweighted average of the unemployment rate in all other states relative to March 2011, plotted by month. “Actual – Synthetic control” is the difference between the Missouri unemployment rate and the synthetic control unemployment rate. See text for details on the construction of the synthetic control.

Table 1. Summary statistics

	2003-2013	2011
Weekly benefit	260.4 [65.62]	259.6 [74.19]
Maximum benefit	6321 [1976]	6328 [2727]
Total benefits	3563 [2769]	4234 [3429]
Reemployment quarterly wage	7720 [6901]	7240 [5703]
Previous employer quarterly wage	9021 [8072]	8259 [6891]
Previous employment tenure	12.1 [9.50]	14.5 [11.18]
Jobless quarters	1.9 [5.23]	1.7 [3.02]
Weeks received	22.0 [18.92]	29.3 [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. Previous employment tenure is in quarters. Weeks received refers to both regular and extended benefits. Previous employer quarterly wage is earnings for the last complete quarter of employment before the unemployment claim. Reemployment wage is earnings for the first complete quarter of employment after the UI claim.

Table 2. RDD diagnostics

	Claim Frequency (1)	Log Predicted Duration Index (2)
Estimated Discontinuity	-454.40 (933.70)	-0.07 (0.05)
Observations	525	525
Bandwidth	29.34	3.10
Mean of dependent variable	5396.76	2.56

Notes: Local linear RDD estimates using the Imbens and Kalyanaraman (2012) optimal bandwidth omitting the regularization term. 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)
<u>Panel A. Main Estimates</u>					
Estimated discontinuity	-8.550 (1.413)	-0.103 (0.045)	-0.090 (0.022)	-0.090 (0.020)	-0.237 (0.012)
Observations	525	525	525	525	525
Bandwidth	15.60	8.02	11.82	11.14	7.18
Mean of dependent variable	25.39	0.46	0.25	0.16	0.11
<u>Panel B. Placebo Estimates</u>					
Estimated discontinuity	-0.824 (1.318)	0.075 (0.021)	0.009 (0.039)	0.011 (0.033)	0.023 (0.026)
Observations	525	525	525	525	525
Bandwidth	21.75	9.73	6.39	5.99	9.40
Mean of dependent variable	34.9	0.42	0.19	0.13	0.10

Notes: Local linear RDD estimates using the Imbens and Kalyanaraman (2012) optimal bandwidth excluding the regularization term. 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

	Employed 2011Q2 (1)	Employed 2011Q3 (2)	Employed 2012Q4 (3)	Employed 2012Q1 (4)	First complete quarter log reemployment wage (5)
Estimated discontinuity	-0.006 (0.015)	0.079 (0.032)	0.073 (0.032)	0.068 (0.038)	0.045 (0.073)
Observations	104	104	104	104	525
Bandwidth	7.07	8.82	11.77	8.34	18.47
Mean of dependent variable	0.841	0.800	0.752	0.708	8.275

Notes: Local linear RDD estimates using the Imbens and Kalyanaraman (2012) optimal bandwidth excluding the regularization term. 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.

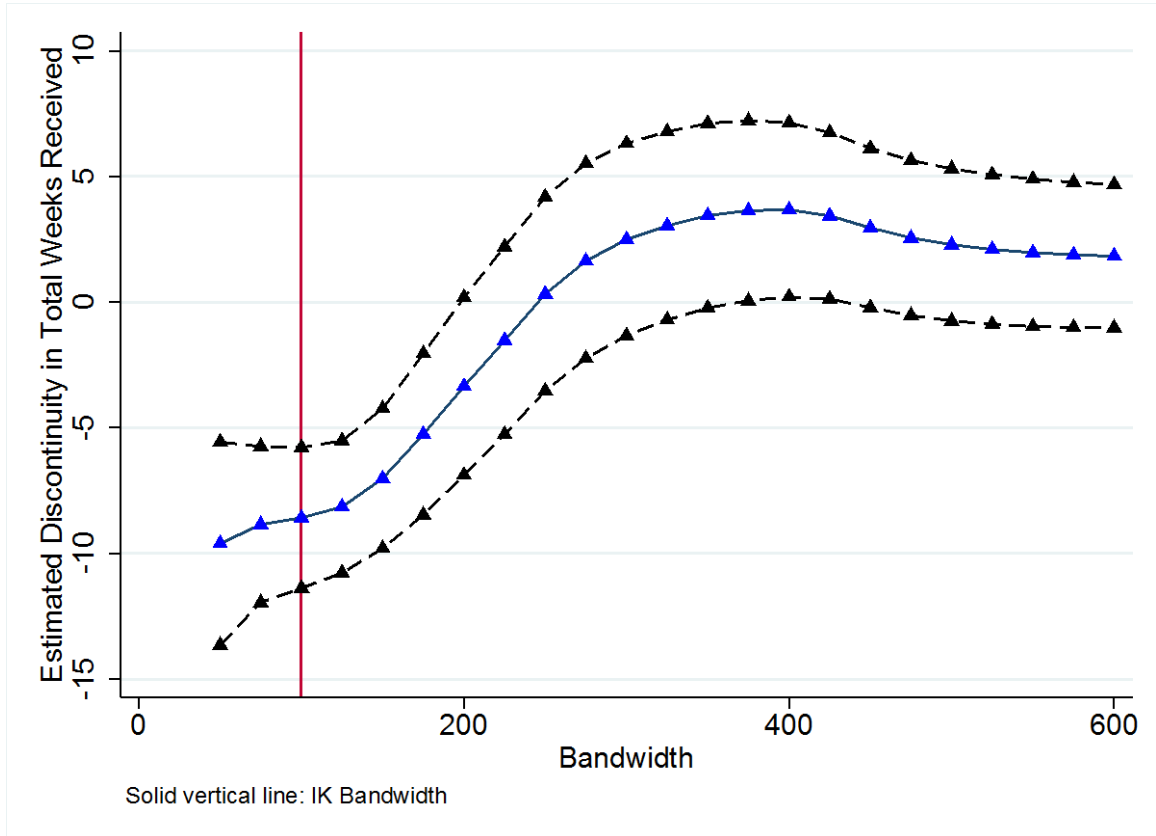
Table 5. DiD Estimates of the change in the Missouri unemployment rate, log number of unemployed, and log size of the labor force following the April 2011 UI maximum duration cut

	UR (1)	UR (2)	UR (3)	ln(U) (4)	ln(U) (5)	ln(U) (6)	ln(LF) (7)	ln(LF) (8)	ln(LF) (9)
Missouri * Post	-0.940	-0.820	-0.54	-0.123	-0.101	-0.097	-0.014	-0.005	-0.009
SE	(0.068)	(0.160)		(0.010)	(0.023)		(0.002)	(0.003)	
PCSE	(0.121)	(0.182)		(0.018)	(0.027)		(0.003)	(0.004)	
Wild Bootstrap C.I.	(-1.2, -0.7)	(-0.9, -0.7)		(-0.15, -0.11)	(-0.12, -0.09)		(-0.02, -0.01)	(-0.01, 0.00)	
Permutation %-tile rank	0.098	0.000	0.039	0.098	0.039	0.020	0.235	0.255	0.216
Predicted change	-0.59	-0.59	-0.59	-0.082	-0.082	-0.082			
Observations	3060	3060	3060	3060	3060	3060	3060	3060	3060
State F.E.	X	X		X	X		X	X	
Time F.E.	X	X		X	X		X	X	
MO*trend		X			X			X	
Unweighted control	X	X		X	X		X	X	
Synthetic control			X			X			X

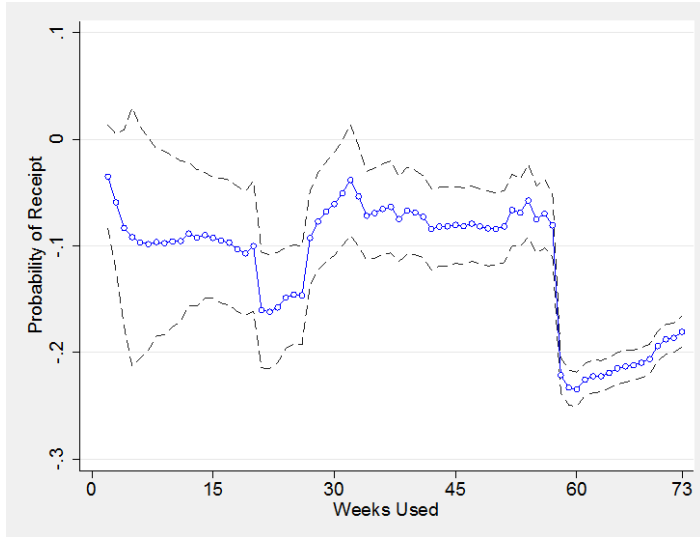
Notes: Observations are state by month units. UR is the unemployment rate, ln(U) is the natural log of the number of unemployed, and ln(LF) is the natural log of the size of the labor force. These variables are derived from the BLS Local Area Unemployment Statistics and deseasonalized as described in the text. The sample spans January 2009 to December 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 permuting the treatment over all states. The unweighted Control uses all equally-weighted states and the District of Columbia as the control group. 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. Time F.E. denote indicators for every time unit in the sample. The predicted change is the change in the outcome variable (excluding ln(LF)) that is predicted by the RDD estimates of the change in the survivor function.

## Online Appendix

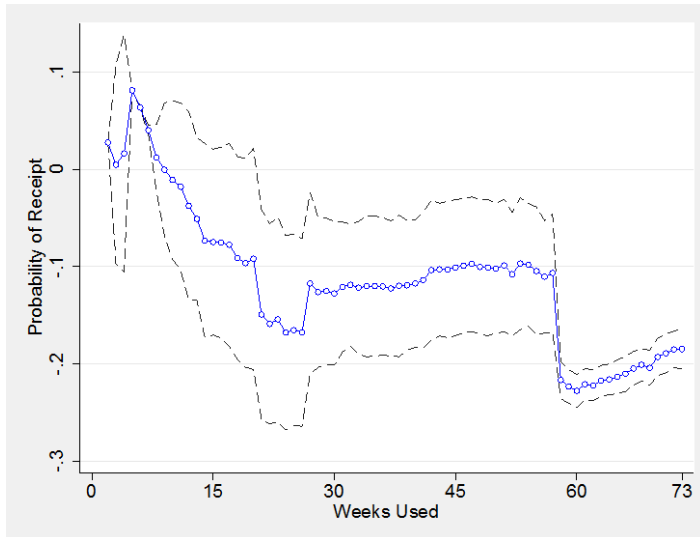
Appendix Figure 1. RDD estimate of total Weeks received by bandwidth (multiple of the IK bandwidth)



Appendix Figure 2a. RDD Estimates of the Probability of Claiming UI for Weeks 1-73 of the Potential UI Spell; Twice the IK Bandwidth

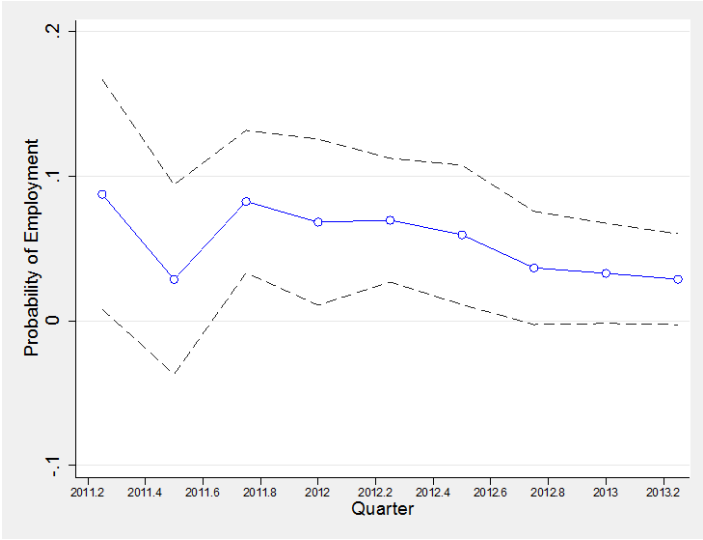


Appendix Figure 2b. RDD Estimates of the Probability of Claiming UI for Weeks 1-73 of the Potential UI Spell; Half the IK Bandwidth

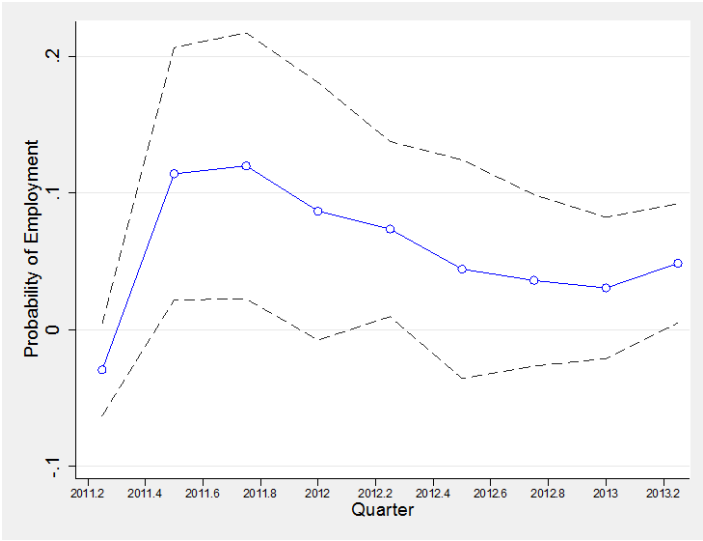




Appendix Figure 3a. RDD Estimates of the Probability of Positive Earnings by Quarter; Twice the IK Bandwidth



Appendix Figure 3b. RDD Estimates of the Probability of Positive Earnings by Quarter; Half the IK Bandwidth



Appendix Table 1. 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; Variables derived from the Current Population Survey

	UR (1)	UR (2)	UR (3)	ln(U) (4)	ln(U) (5)	ln(U) (6)	ln(LF) (7)	ln(LF) (8)	ln(LF) (9)
Missouri * Post	-0.934	-0.711	-1.00	-0.127	-0.091	-0.138	-0.015	-0.013	-0.004
SE	(0.218)	(0.413)		(0.030)	(0.058)		(0.005)	(0.010)	
PCSE	(0.199)	(0.394)		(0.029)	(0.056)		(0.010)	(0.015)	
Wild Bootstrap C.I.	(-1.12, -0.76)	(-0.87, -0.55)		(-0.15, -0.10)	(-0.11, -0.06)		(-0.02, -0.01)	(-0.02, -0.01)	
Permutation %-tile rank	0.098	0.118	0.039	0.059	0.098	0.078	0.157	0.137	0.350
Observations	3060	3060	3060	3060	3060	3060	3060	3060	3060
Predicted change	-0.59	-0.59	-0.59	-0.082	-0.082	-0.082			
State F.E.	X	X		X	X		X	X	
Time F.E.	X	X		X	X		X	X	
State*trend		X			X			X	
Unweighted control	X	X		X	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. These variables are derived from the Current Population Survey and deseasonalized as described in the text. The sample spans January 2009 to December 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 permuting the treatment over all states. The unweighted Control uses all equally-weighted states and the District of Columbia as the control group. 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. Time F.E. denote indicators for every time unit in the sample. The predicted change is the change in the outcome variable (excluding ln(LF)) that is predicted by the RDD estimates of the change in the survivor function.