Revisiting the Effects of Unemployment Insurance Extensions on Unemployment: A Measurement-Error-Corrected Regression Discontinuity Approach*

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Online Appendix

A Approximating the Aggregation Error in the Dependent Variable

To investigate the importance of the aggregation-induced measurement error in our dependent variable, first denote the unemployment and labor force counts in the county as a whole by $U_{c,s,t}$ and $L_{c,s,t}$, then note that these are the sum of the counts in each of our hypothetical subregions (i.e., $U_{c,s,t} = \sum_{i=1}^{N_c} U_{i,c,s,t}$ and $L_{c,s,t} = \sum_{i=1}^{N_c} L_{i,c,s,t}$). Plugging these into the the expression for the observed county-level log unemployment rate gives:

$$y_{c,s,t} = \ln\left(\frac{U_{c,s,t}}{L_{c,s,t}}\right) = \ln\left(\overline{U}_{c,s,t}\right) - \ln\left(\overline{L}_{c,s,t}\right) \tag{A.1}$$

Where $\overline{U}_{c,s,t}$ and $\overline{L}_{c,s,t}$ are the mean unemployment and labor force counts across our hypothetical subregions within a county in period t. Now we can examine how this expression differs from $\overline{y}_{c,s,t}$, the mean log unemployment rate in the county that we would need to implement the ideal RD.

Using the unemployment and labor force counts within our subregions, $U_{i,c,s,t}$ and $L_{i,c,s,t}$, we can write the mean of the subregion log unemployment rates at the

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county-level as follows:

$$\overline{y}_{c,s,t} = \overline{\ln(U)}_{c,s,t} - \overline{\ln(L)}_{c,s,t} \tag{A.2}$$

Comparing Equations (A.1) and (A.2), it becomes clear that we need to characterize the difference between the average of a log and the log of the average. Using a Taylor Series approximation we can approximate $\overline{y}_{c,s,t}$ as

$$\overline{y}_{c,s,t} \approx \left[\ln\left(\overline{U}_{c,s,t}\right) - \frac{\sigma^2_{U,c,s,t}}{2\overline{U}^2_{c,s,t}}\right] - \left[\ln\left(\overline{L}_{c,s,t}\right) - \frac{\sigma^2_{L,c,s,t}}{2\overline{L}^2_{c,s,t}}\right] \tag{A.3}$$

Plugging this into our expression for the aggregation error yields:

$$\epsilon_{c,s,t} = y_{c,s,t} - \overline{y}_{c,s,t} \approx \frac{\sigma^2_{U,c,s,t}}{2\overline{U}^2_{c,s,t}} - \frac{\sigma^2_{L,c,s,t}}{2\overline{L}^2_{c,s,t}} \tag{A.4}$$

Recall the bias term for our simplified case with only two states in one time period in Equation 6 was given by $E[\tilde{\epsilon}_{c,1}] - E[\tilde{\epsilon}_{c,0}]$. Therefore, we need to know how the average aggregation error differs for high-benefit and low-benefit states. Intuitively, the size of the aggregation error for any given county depends on how variable employment outcomes are across our hypothetical subregions within counties. On one extreme, if every subregion has the same unemployment and labor force counts—and therefore, the same unemployment rates, then the county wide measure provides an error-free measure for the subregions at any distance from the border. In terms of the aggregation error, the variance terms σ^2_U and σ^2_L would be zero in this case and the aggregation error would disappear. On the other extreme, if the labor market outcomes are highly variable within counties, then the variance terms will be large and our county-level log unemployment rate may be a poor measure of the actual variable.

With this bias approximation in hand, we can use auxiliary data to obtain evidence on the magnitude of this aggregation bias. Obviously, due to the same data limitations that led us to use county-level data, this issue is difficult to directly assess. Therefore, in order to have reliable subcounty-level unemployment and labor force counts needed to calculate the means and variances in the aggregation error, we need to pool over a longer time period than the quarterly data used in our main analysis. To do this, we utilize restricted use, annual 2005-2011 American Community Survey (ACS) microdata aggregated at the census tract level.[1] We readily note that this yearly data may deviate from the quarterly data used in our analysis and that using census tract-level data may yield noisy measures of unemployment and labor force counts. The fact that census tracts have different populations will

[1]For more detailed information on the ACS methodology, see the ACS Design & Methodology Report at `http://www.census.gov/programs-surveys/acs/methodology/design-and-methodology.html`.

introduce additional variation across subregions relative to our hypothetical data setting. However, as is typical of RD designs, these factors will be less of a concern for our RD estimates if they evolve continuously across state boundaries.

Using this ACS data, we first calculate the variances and means of the unemployment and labor force counts across census tracts for each county-year in order to calculate the annual version of Equation (A.4). Once again, this aggregation error must be systematically different for counties in high- versus low-benefit states to be a concern for our RD estimates. When estimating the effect of UI benefit duration, a continuous measure, the bias will take a classic omitted variables form. Ignoring other controls, the probability limit of our estimate will differ from the true effect by $\eta = Cov(b, \epsilon)/Var(b)$. That is, the extent of the problem depends on the strength of the relationship between the aggregation error and UI duration. Note that η is simply the probability limit of the OLS estimate from a regression of ϵ on b. Therefore, to provide some evidence on the extent of the problem, we repeat our main estimation strategy, discussed in Section III.B, replacing the county-level log unemployment rate with the aggregation error as the dependent variable:

$$
\begin{aligned}
&\epsilon_{c,s,g,t} = \alpha + \eta b_{s,g,t} + \psi_{g,t} + u_{c,s,g,t} \\
&\qquad \text{where } c \text{ indexes counties, } s \text{ indexes states,} \\
&\qquad g \text{ indexes state boundaries, and } t \text{ indexes quarters} \\
&\qquad b_{s,g,t} \text{ is the log benefit duration}
\end{aligned}
$$

The results of this exercise are shown in Table A.1. Across specifications with different controls for distance the estimated bias ranges from -0.0104 to 0.0833. Following the same counterfactual exercise as in Section III.C, if the estimated bias were the only difference across borders, these point estimates would be associated with an implied unemployment rate of 4.9 to 5.4 percent starting from a base rate of 5 percent. As this is considerably smaller than the baseline estimates, it suggests that aggregation error in the dependent variable is not a major concern in this setting.

B From Border Pair-FE to Regression Discontinuity

It is instructive to carefully follow the steps that allow us to compare the estimation strategies based on a border-pair FE estimation to those from an RD setup. First, instead of having the unit of observation be a county pair, RD necessitates units to be each individual county. Note again that $\Delta y_{p,t}$ is the difference within county pair in each quarter. If each county had only one partner, then this would be identical to using the county-level observations and including pair-by-quarter FE. In reality, some counties show up in multiple pairs so the equivalence breaks down. Next, since the treatment effect is only identified when there is a difference in treatment status across the border, we drop all county-quarters where the UI benefits are the same

Table A.1

RD Estimates: Aggregation Bias			
	RD Polynomial Order		
	0	*1*	*AICc*
Population Weighted	-0.0104	0.0833	0.0631
	(0.0177)	(0.0478)	(0.0422)

Source: American Community Survey Census Tract Level Data. For more information, visit `census.gov/acs`.
Coefficients reported from separate regressions of the approximated aggregation error on UI benefit duration with either no control for distance (Polynomial order 0), a linear distance control (Polynomial order 1), or higher order polynomial in distance with the order chosen for each state boundary-by-quarter using a small sample corrected version of the Akaike Information Criteria (Polynomial order AICc). Standard errors clustered at the State-Boundary level in parentheses.

across the border. We effectively have a different geographic RD anytime a state-boundary-by-quarter has a difference in UI benefits across the states. We pool these together, replacing the pair-by-quarter FE with state-boundary-by-quarter FE. Replacing pair-by-quarter FE with state-boundary-by-quarter FE does not affect the estimates substantially. This final change gives our baseline RD expression when not controlling for distance found in Equation (8).

C Placebo Tests

In this section, we check the robustness of our RD procedure by conducting a set of placebo tests. We first consider whether idiosyncratic factors are likely to generate estimated discontinuities in the absence of UI differences. To do so, we use data from state boundaries in quarters when there was no difference in UI benefits and assign states to two separate placebo treatment and control groups. For the first placebo treatment, we randomly select one state at a boundary to be the treated state in each quarter. Using this placebo treatment indicator as the variable of interest in our regressions lets us test whether idiosyncratic factors at state boundaries are likely to lead to biased estimates when we pool across many boundary-by-quarter RDs. For the second placebo treatment, we determine which state was more often the high-benefit state at a particular boundary and set the treatment indicator equal to one for that state with ties broken at random. This second check helps test whether the same idiosyncratic factors at state boundaries are systematically related to areas more or less likely to see UI extensions.

Table C.1 displays the results for these two placebo checks using the quarters with no difference in UI benefits and replacing the available UI duration with the two placebo treatment indicators. Once again, we present the implied unemployment rate starting from a base rate of 5 percent. Across both placebo treatments the estimated treatment effect is never statistically different from zero. Based on the implied unemployment rates, the magnitude of the effect is not economically significant, either. This suggests that idiosyncratic factors— including those that may be related to UI extensions— do not generate artificial jumps or non-smoothness at state boundaries when pooling across many separate RD cases.

The placebo tests also provide evidence that the measurement issues regarding the LAUS unemployment numbers do not generate bias in our estimates. In particular, if the LAUS unemployment measure for a border county partially reflects unemployment in other areas of the state, it could lead to a measured jump in unemployment at state boundaries even if there was no real difference at the border. Again, the lack an estimated discontinuity with our placebo treatments suggests that this is not a first order concern in our setting.

Note that the placebo estimates when not controlling for distance to the border (Polynomial Order 0) are also very close to zero. This is again consistent with the idea that the key endogoneity concern stems from the contemporaneous shocks that triggered UI extensions rather than fixed differences away from state borders. With both of our first two placebo treatments, any misspecification bias from not controlling for distance appears to balance on average across borders when there are no differences in UI and no systematic differences due to the associated shocks. However, when there are real differences in available UI due to shocks in our main analysis, we estimate large effects when not controlling for distance. In this case the misspecification from not controlling for distance is directly related to the negative shocks that triggered the extensions.

Building on this point, we conduct a final placebo check that is similar to one found in Hagedorn, Manovskii and Mitman (2016). Here, we select a sample of state borders in periods when neither state had an UI extension. Using the data on state level unemployment complied by Chodorow-Reich, Coglianese and Karabarbounis (2018), we then mimic the actual policy by creating placebo treatment indicators when the three month average state level unemployment passed a level not associated with an actual policy trigger. We choose artificial triggers of four or five percent since these are not associated with real triggers and yield reasonable sample sizes for the set of counties with differences in our placebo treatment at state boundaries. Note that this placebo treatment is systematically related to state level unemployment, as is the actual policy. In both cases, we estimate positive, and statistically significant increases in unemployment when not controlling for distance. Just as in our main analysis, the estimates become much smaller and statistically insignificant once we control for distance. This suggests that our procedure does a better job of balancing the effect of state-level unemployment shocks at state borders than those that do not account for distance.

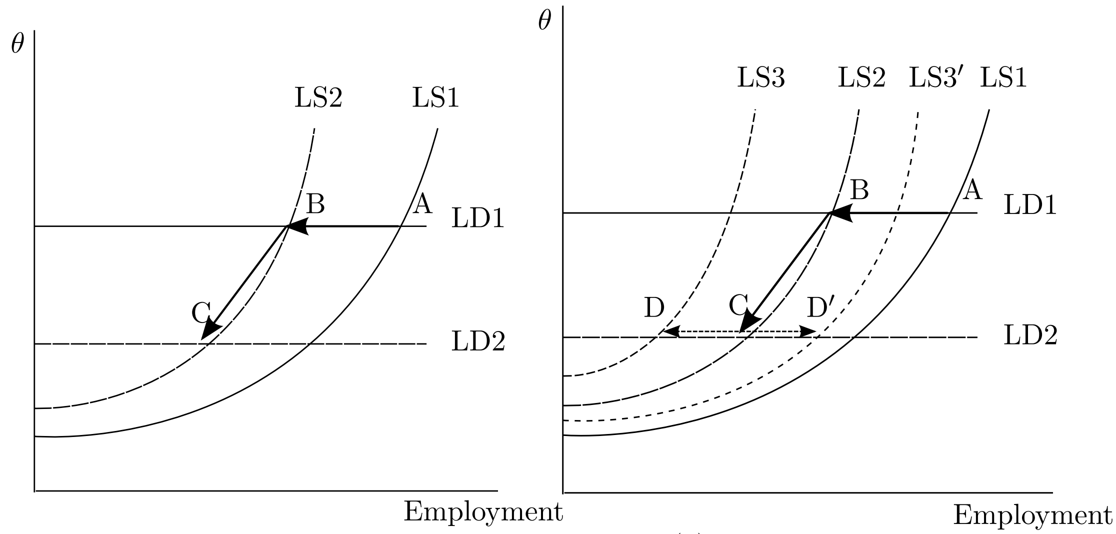
D Employment Spillovers in Standard Matching Model

The standard matching model (Pissarides, 2000) differs from the job rationing model on the labor demand side. For the standard model, production is assumed to be linear in employment implying a constant marginal product of labor. Wages are no longer fixed, but are instead determined by Nash bargaining over the total surplus from making a match for a worker and a firm. In this model, wages are increasing in the generosity of UI benefits as it represents the outside option for the unemployed worker when bargaining over wages. On the other hand, the value to the firm of a filled job falls with an increase in wages. Importantly, labor demand is perfectly elastic with respect to tightness due to the assumption of constant returns to scale in production. The equilibrium employment and market tightness in the standard matching model before a change in UI is depicted in Panel (a) of Figure D.1 by the intersection of LS1 and LD1.

As before, the increase in UI reduces search effort by unemployed workers thereby shifting labor supply in— from LS1 to LS2 in the figure. Holding labor demand fixed, this results in a reduction in employment from point A to B. The increase in UI, and the associated increase in wages, also shifts the labor demand down— from LD1 to LD2 in the figure. This shift in demand moves along the new labor supply curve further reducing employment and lowering tightness— the movement from point B to C. It also reflects a reduction in vacancies in response to the UI increase and further reinforces the unemployment effects of UI. Unlike the job rationing model, the total reduction in employment— or the macro-elasticity (from point A to C)— is larger than the micro-elasticity (from point A to B).

For the standard matching model, the effect of out-of-state searchers is less clear since the response in the high benefit state lowers tightness but raises wages. The lower tightness— and therefore lower probability of being matched to an open vacancy— would make the high-benefit state less attractive for searchers while the higher wage would make it more attractive. Hence, whether unemployed workers with access to the lower benefits would search more in the high-benefit or low-benefit state depends on their preferences and the relative magnitudes of the changes in wages and tightness. In terms of the figure, either fewer people will search in the high benefit state— if the tightness change dominates— shifting labor supply in further to LS3, or more will search in the high benefit state— if the change in the wage dominates— shifting it to LS3′. Therefore, for the standard model to be consistent with the observed across-border employment patterns, the increase in wages in response to the extended UI benefit duration must be sufficiently large to offset the fall in tightness. However, our investigation of earnings at state boundaries in Section III.C found no evidence of differences associated with extended UI benefits.

Figure D.1: Response to UI Increase in Standard Matching Model (Pissarides, 2000)



(a) UI Response

(b) UI Response with Out-of State Searchers

E Additional Tables

Table C.1

RD Estimates: Placebo Treatments			
	(1)	(2)	(3)
	RD Polynomial Order		
	0	*1*	*AICc*
Panel 1A: Coefficient Estimates			
Random Treatment	-0.0086	-0.0393	-0.0244
	(0.0087)	(0.0211)	(0.0249)
Panel 1B: Implied Unemployment Rate from Base of 5%			
	5.0%	4.8%	4.9%
	[4.9%, 5.0%]	[4.6%, 5.0%]	[4.6%, 5.1%]
Observations	18,475	18,475	18,475
Panel 2A: Coefficient Estimates			
UI Difference Treatment	0.0343	-0.0362	-0.0794
	(0.0235)	(0.0522)	(0.0485)
Panel 2B: Implied Unemployment Rate from Base of 5%			
	5.2%	4.8%	4.6%
	[4.9%, 5.4%]	[4.3%, 5.3%]	[4.2%, 5.1%]
Observations	17,635	17.635	17,635
Panel 3A: Coefficient Estimates			
Unemployment 4% Trigger	0.2174	0.1301	0.0577
	(0.0370)	(0.0573)	(0.0426)
Panel 3B: Implied Unemployment Rate from Base of 5%			
	6.2%	5.7%	5.3%
	[5.8%, 6.7%]	[5.1%, 6.3%]	[4.9%, 5.7%]
Observations	4,841	4,841	4,841
Panel 4A: Coefficient Estimates			
Unemployment 5% Trigger	0.1372	0.0739	0.0465
	(0.0264)	(0.0507)	(0.0528)
Panel 4B: Implied Unemployment Rate from Base of 5%			
	5.7%	5.4%	5.2%
	[5.4%, 6.0%]	[4.8%, 5.9%]	[4.7%, 5.8%]
Observations	5,743	5,743	5,743

Standard errors clustered at the state-by-boundary level in parentheses. 95% Confidence Intervals in square brackets. RD Polynomial Order indicates either no control for distance (Polynomial order 0), a linear distance control (Polynomial order 1), or higher order polynomial in distance with the order chosen for each state boundary-by-quarter using a small sample corrected version of the Akaike Information Criteria (Polynomial order AICc). The implied unemployment rates in Panel B are calculated following HKMM as $\exp\left(\ln(0.05) + \hat{\gamma}\right)$.
Data Sources: BLS LAUS, TIGER geographic shapefiles, EUC and EB trigger reports.

Table E.1

Event History Sample: Included and Excluded State Borders			
Included: 57 Borders		*Excluded: 49 Borders*	
Alabama-Georgia	Kentucky-Missouri	Alabama-Florida	Louisiana-Mississippi
Alabama-Tennessee	Kentucky-Tennessee	Alabama-Mississippi	Louisiana-Texas
Arizona-California	Kentucky-West Virginia	Arizona-New Mexico	Maine-New Hampshire
Arizona-Nevada	Maryland-Pennsylvania	Arkansas-Louisiana	Maryland-Virginia
Arizona-Utah	Maryland-West Virginia	Arkansas-Mississippi	Massachusetts-New Hampshire
Arkansas-Oklahoma	Massachusetts-New York	Arkansas-Missouri	Massachusetts-Vermont
California-Oregon	Massachusetts-Rhode Island	Arkansas-Tennessee	Michigan-Ohio
Colorado-Nebraska	Michigan-Wisconsin	Arkansas-Texas	Minnesota-Wisconsin
Colorado-Utah	Minnesota-North Dakota	California-Nevada	Missouri-Tennessee
Colorado-Wyoming	Minnesota-South Dakota	Colorado-Kansas	Nebraska-South Dakota
Connecticut-Rhode Island	Mississippi-Tennessee	Colorado-New Mexico	Nevada-Oregon
Delaware-Maryland	Missouri-Nebraska	Colorado-Oklahoma	New Hampshire-Vermont
DC-Maryland	Missouri-Oklahoma	Connecticut-Massachusetts	New Jersey-Pennsylvania
Florida-Georgia	Montana-North Dakota	Connecticut-New York	New Mexico-Texas
Georgia-North Carolina	Montana-South Dakota	Delaware-New Jersey	New York-Pennsylvania
Georgia-Tennessee	Montana-Wyoming	Delaware-Pennsylvania	New York-Vermont
Idaho-Oregon	Nebraska-Wyoming	DC-Virginia	North Dakota-South Dakota
Idaho-Utah	Nevada-Utah	Georgia-South Carolina	Ohio-Pennsylvania
Idaho-Washington	New Jersey-New York	Idaho-Montana	Tennessee-Virginia
Idaho-Wyoming	North Carolina-South Carolina	Idaho-Nevada	Virginia-West Virginia
Illinois-Iowa	North Carolina-Tennessee	Illinois-Indiana	
Indiana-Michigan	North Carolina-Virginia	Illinois-Kentucky	
Iowa-Minnesota	Ohio-West Virginia	Illinois-Missouri	
Iowa-Missouri	Oklahoma-Texas	Illinois-Wisconsin	
Iowa-Nebraska	Oregon-Washington	Indiana-Kentucky	
Iowa-South Dakota	Pennsylvania-West Virginia	Indiana-Ohio	
Iowa-Wisconsin	South Dakota-Wyoming	Kansas-Missouri	
Kansas-Nebraska	Utah-Wyoming	Kentucky-Ohio	
Kansas-Oklahoma		Kentucky-Virginia	

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