3-28-2018

Working Paper Number 16002

Revisiting the Effects of Unemployment Insurance Extensions on Unemployment: A Measurement Error-Corrected RD Approach

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Abstract

We reassess the literature using county border pair identification strategies to examine the effects of UI benefit duration on labor market conditions. We extend these previous approaches using a regression discontinuity-based approach that controls for changes in unobservables by distance to the border and accounts for measurement error induced by using county-level aggregates. Our new results provide no evidence of a large change in unemployment induced by differences in UI generosity across state boundaries, bringing the evidence from border-based identification strategies in line with estimates from other strands of the UI benefit duration literature.

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

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February 1, 2018

Abstract

We document two potential biases in recent analyses of UI benefit extensions using boundary-based identification: from using county-level aggregates and from across-border policy spillovers. To examine the first bias, we use a regression discontinuity (RD) approach that accounts for measurement error in county-level aggregates. Our results suggest much smaller effects than previous studies, casting doubt on the applicability of border-based designs. We then document substantial spillover effects of UI benefit duration in the form of across-border work patterns that are consistent with increased tightness in high benefit states, providing evidence against a dominant vacancy reduction response to UI extensions.

1 Introduction

While the extension of Unemployment Insurance (UI) benefit duration from 26 weeks to as many as 99 weeks represents one of the key policy responses to rising unemployment in the U.S. during the Great Recession, the total effect of these UI benefit extensions is theoretically ambiguous. UI benefit extensions may impact labor markets through a host of labor supply and demand channels, and the labor demand effects may either reinforce or offset the supply response. Therefore, the net effect of UI extensions is an empirical question. Moreover, while there is a long history of studies on the labor supply effect of reduced search effort in response to such extensions (Solon, 1979; Moffitt, 1985; Katz and Meyer, 1990; Meyer, 1990; Rothstein, 2011), there is a sparser, more recent literature that estimates the total, or macro, effect of extensions inclusive of the labor demand response (Hagedorn et al., 2015; Hagedorn, Manovskii, and Mitman, 2016; Lalive, Landais, and Zweimüller, 2015; Marinescu, 2014; Coglianese, 2015; Chodorow-Reich and Karabarbounis, 2016; Boone et al., 2017; Johnston and Mas, 2015).

The recent literature estimating the total effect of UI extensions on labor market conditions across the U.S. during the Great Recession provides mixed results. An influential paper by Hagedorn, Karahan, Manovskii, and Mitman (2015) (henceforth HKMM) finds large negative effects,[1] while others find much smaller total effects (Chodorow-Reich and Karabarbounis, 2016; Coglianese, 2015; Boone et al., 2017). The papers differ along several dimensions, in particular the choice of identification strategy and how labor market outcomes are measured. HKMM use a boundary-based approach exploiting differences at state boundaries separating different UI regimes.[2] Boone et al. (2017) use

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[1] The large results found by HKMM have led to a great deal of academic and media attention. See Hall's (2013) comment on an earlier version of HKMM and coverage in the *Wall Street Journal* (Wall Street Journal, 2013) and *Washington Post* (Plumer, 2014).

[2] Differences in policy environments at state boundaries have also been used in other literatures. For examples, see Holmes (1998), Dube, Lester, and Reich (2010), or Dube, Lester, and Reich (2016).

a modified boundary design while others take advantage of differences in UI due to sampling error in the measures used to determine state UI benefit levels (Chodorow-Reich and Karabarbounis, 2016; Coglianese, 2015). The divergent results may stem from a number of issues including bias in the different estimation approaches — an issue being debated in the literature (Hall, 2013; Coglianese, 2015; Hagedorn, Manovskii, and Mitman, 2016).

We re-examine the state boundary-based evidence by documenting the fundamental tradeoff between two sources of bias when using such approaches. Boundary-based approaches require that the areas being compared on either side of the border would experience similar labor market conditions in the absence of a difference in UI duration. Effectively, this requires similar industry structure, labor productivity, and agglomeration effects on either side of the border. In the current context, it also means that the economic shocks that triggered UI extensions in one state must evolve over space in such a way that areas on either side of the border are affected similarly. Border-based approaches also require that the effect of the policy is concentrated on one side of the border. If these two conditions hold, then differences in outcomes at state boundaries can be attributed to the the longer UI available in one state and not the other. However, there may be a tension between the two conditions since the same factors that make one side of the border an appealing control group for the other— a degree of similarity and labor market connectedness— may also make policy spillovers more likely.

Conceptually, the first requirement that we compare very similar areas is more likely to hold when we focus on a very narrow area around state borders separating different UI regimes. Note, however, that the county-level labor market data available and used in prior work may not satisfy this requirement— counties at state borders may be too large to ensure that the measured outcomes would be on average the same across border counties without the policy. This could lead to a discontinuous jump in county-level aggregate outcomes at boundaries even when there are no differences for smaller areas closer to the

border. Ideally, we would use data at a smaller level of aggregation to better approximate the thought experiment behind the identification approach. Lacking reliable sub-county level data, we propose a measurement error corrected Regression Discontinuity (RD) approach that can approximate the results from a hypothetical regression using more granular data by controlling for the moments of the within-county population distribution relative to the border.

Ignoring aggregation issues and comparing county-level outcomes at boundaries, we estimate large negative effects of the UI extensions on unemployment. These uncorrected estimates suggest that a permanent extension of UI benefits to 99 weeks would raise unemployment from a baseline of 5 percent to 9.9 percent, closely matching the results from HKMM. However, correcting for the use of county level aggregates, we find much smaller effects. For example, controlling for a linear function of distance to the border, we find that permanently extending UI benefits to 99 weeks would raise unemployment from a baseline of 5 to 5.5 percent. We also find suggestive, but imprecise, evidence that wages and earnings did not change discontinuously at state boundaries.

While focusing attention on smaller areas closer to the border helps match the experimental ideal of comparing similar areas facing different UI duration, that proximity may also increase the possibility that the policy affects outcomes on both sides of the border, violating the second requirement for identifying policy effects at boundaries. In a small area with a connected labor market, the effect of UI extensions may spillover the border since workers and firms have easy access to potential employment matches in the other state. By analyzing patterns in cross-border employment by individuals, we find new evidence that workers who live near the border respond to changes in benefits in the neighboring state by shifting employment, not necessarily residence, from the low- to the high-benefit state. This response is strongest close to the border— within a few kilometers— and fades as you look farther from the border becoming negligible roughly twenty-five kilometers away.

Combining the two sets of results highlights the tension between the two

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sources of bias— while focusing on areas closer to state borders will likely reduce the upward endogeneity bias from the shocks that triggered UI extensions, it will simultaneously increase the importance of the attenuation bias from the treatment spillovers. As both of these effects go in the same direction— toward smaller point estimates when considering areas closer to the border— it is impossible to disentangle the two without imposing strong assumptions on how they differentially evolve over space. Together this tradeoff suggests that the boundary-based estimation strategies are ill-suited for directly identifying the macro effects of UI extensions on unemployment rates.

Despite casting doubt on the applicability of border-based identification approaches in this setting, the observed employment spillovers do have implications for the macro response to UI extensions. In particular, the increase in the proportion of workers employed on the high-benefit side of state borders is consistent with a relative increase in labor market tightness in the high benefit state. Such an increase runs counter to the main mechanism for a large macro effect through reduced vacancies in a standard search model (Pissarides, 2000; Hagedorn et al., 2015). Instead, it is more consistent with the Job Rationing model of Michaillat (2012) which would predict a macro effect that is smaller than the micro effect (Landais, Michaillat, and Saez, 2016).

Finally, our measurement error-corrected RD approach provides an alternative to other boundary-based approaches when using aggregate data. It is particularly useful in cases where the main threat to validity is contemporaneous factors driving policy adoption that are likely to evolve through space, rather than more systematic differences across regions.

The paper proceeds as follows: Section 2 discusses the literature on UI benefits, provides a background on the institutional details of UI extensions in the Great Recession and discusses the implications for boundary-based approaches; Section 3 provides a brief description of the data sources; Section 4 presents the measurement error-corrected RD approach and the results; Section 5 provides evidence of treatment spillovers at the border and discusses the implications

for the macro effect of UI extensions; and Section 6 concludes.

2   Background and RD Motivation

In the absence of any extensions, UI benefits are typically available for a maximum of 26 weeks in most states.[3] The Great Recession brought about a series of UI benefit extensions that were in many ways unprecedented in the United States.[4] In particular, UI extensions were implemented through two separate programs: Extended Benefits (EB) and Emergency Unemployment Compensation (EUC). Prior to the Great Recession, the EB program provided either 13 or 20 additional weeks and was voluntary for states with only partial funding from the federal government. In 2009, the federal government provided full funding, which led many states to take up the EB program. The EUC program was introduced in 2008 and provides 13 to 53 additional weeks of benefits. Altogether, the EB and EUC programs led to an extension of UI benefit duration from 26 to as many as 99 weeks. Importantly for the identification strategies discussed here, the realized levels of EB and EUC benefits were determined by state-level labor market indicators passing pre-specified trigger levels. This implies that the key endogeneity concern stems from the contemporaneous, transitory shocks that triggered a UI extension for a state at a particular point in time, rather than systematic social, economic, or political factors. Importantly, such contemporaneous factors would not be captured by including fixed effects in a regression.

The potential negative effects of increased UI generosity through reduced search effort has been well established in the literature (see Mortensen (1976); Solon (1979); Moffitt (1985); Katz and Meyer (1990); Meyer (1990)). Here we

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[3]Note that following states have a standard duration different from 26 weeks: Massachusetts, Montana, Arkansas, Michigan, South Carolina, Missouri, Kansas, Florida, Georgia, and North Carolina (see `http://www.cbpp.org/research/economy/policy-basics-how-many-weeks-of-unemployment-compensation-are-available`).

[4]The following discussion on the institutional details of UI extensions during the Great Recession follows closely from the more detailed description found in Rothstein (2011).

will focus on the directly related literature studying the total effect of EB and EUC extensions during the Great Recession. A key challenge in estimating the total effect is accounting for the potential endogeneity of the UI extensions. Given that the UI extensions were triggered by negative economic shocks to each state, these extensions will be correlated by construction with the adverse labor market effects of those same shocks. When comparing labor market outcomes across states with different UI regimes during the Great Recession, we risk attributing the deteriorated labor market conditions to UI differences, rather than the economic shocks that triggered them. The result will be an upward bias in the estimated magnitude of UI effects.

HKMM address the endogeneity concern by adapting the Boundary Pair Fixed Effects (BPFE) approach used by Holmes (1998) and Dube, Lester, and Reich (2010, 2016). The BPFE approach focuses on contiguous counties at state borders that, due to being in different states, are subject to different UI regimes. As implemented, the BPFE approach tries to control for two types of confounding factors. First to control for contemporaneous shocks that may affect both counties in a pair at a given time— including those that triggered the UI extensions— HKMM regress the difference in labor market outcomes across contiguous county pairs at state borders in a given quarter on the difference in UI benefit duration for each pair. Further differences between county pairs are then controlled for using the interactive effects framework from Bai (2009). See Online Appendix A for a detailed discussion of the prior estimation strategy.

Identification is based on the assumption that unobserved factors driving labor market outcomes, including the economic shocks that triggered UI extensions, are on average the same within county pairs. Conditional on these pair-level factors, the UI benefit effect is then identified off of differences in unemployment between states with different UI benefit durations. Intuitively, the BPFE approach attempts to approximate the experimental ideal of comparing outcomes for two areas that are identical in terms of the factors influencing labor market outcomes, but happen to operate under different UI regimes by

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chance. Using this approach, HKMM find that the total effect of UI benefit duration is quite large. Their estimates suggest that permanently increasing benefits to 99 weeks would increase unemployment from 5 to 10.5 percent, an effect that would account for all of the rise in unemployment during the Great Recession.[5]

The practical implementation of the BPFE approach using county-level measures of outcomes rather than data from smaller areas raises concerns that the unobservable factors driving labor market outcomes may in fact differ across border pairs given the substantial heterogeneity in county size and economic makeup. For instance, if the population centers of the two counties in a pair are far from the border, then a comparison of county-level measures of labor market outcomes may not reflect outcomes based on areas with similar fundamental factors and responses to the shocks that triggered the UI extensions. In this sense, our concern is that within-county differences may generate differences in the measured outcome across county pairs when no difference exists for smaller, more comparable, areas closer to the border.

Given this concern, we propose using an alternative identification strategy based on the same thought experiment. Specifically, we wish to use the UI policy discontinuity at state boundaries within a geographic RD framework to identify the jump in unemployment at state boundaries with different UI availability.[6] Rather than directly compare county pairs at a border, the RD approach controls for distance to the border to compare the average difference in outcomes precisely at the border.

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[5]In a replication study, Amaral and Ice (2014) rely on the same identification approach, but differ in the sample used. Amaral and Ice (2014) exclude counties with unemployment rates above 10 percent and extend the sample to include years without large UI extensions. Doing so leads to smaller estimates, but they are still larger than comparable estimates in the literature. We focus on the original HKMM results and sample since we prefer not to remove counties with high levels of unemployment and because our identification approach will be based only on periods with differences in UI benefits.

[6]Lalive (2008) uses a geographic RD to study a substantial regional UI extension from 30 to 209 weeks for workers over the age of 50 in Austria in the early 1990s. He finds large labor supply responses for this age group. Our analysis is not only for a different setting- extensions in the US for all age groups during the Great Recession- but it also differs in the focus on the total response through labor supply and demand.

While switching to an RD setting allows us to use comparison groups close to the border, it does introduce other differences relative to the border-pair approach. The BPFE approach explicitly allows for different unobservables at different points along a border segment- i.e. different county pairs may have different fixed effects along the same state border reflecting different industries, agglomeration, or transportation effects. It is important to note that an RD approach will implicitly account for these same factors. RD in this setting is based on the estimated jump at state boundaries in the conditional expectation of labor market outcomes— where the conditioning is on distance to the border. By averaging across areas at the same distance, it does not require that unobservables are evolving in the exact same way along the entire border segment. Any factors that are particular to areas along the border segment that affect both sides- for instance factors captured by county-pair fixed effects- will contribute similarly to the conditional expectations on both sides of the border and will cancel out in the RD estimate.

To highlight the potential problem of using comparisons in county-level measures across borders without controlling for distance in an RD setting, Figure 2.1 plots the county-level log unemployment rate by the population-weighted mean distance to the border in kilometers for several sets of border counties where the marker size is proportional to county population. We weight by county population and use the population weighted mean distance to match our main analysis discussed in Section 4.1. In each case, we plot the data for the low-benefit state to the left of zero and present the length of available UI in each state. The average log unemployment rate among border counties in each state is depicted by the gray dashed line. In each case, the difference in this average across borders is large and consistent with increased unemployment in the high UI state. However, fitting a linear trend in distance to the border weighting by county population— the solid black line— on either side of the boundary leads to a much smaller gap in every case.

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Figure 2.1: Log Unemployment at State Borders: Examples

(a)

Florida-Georgia Border Counties: 2010 Q1
Florida UI=79 Weeks and Georgia UI=99 Weeks

(b)

Iowa-Illinois Border Counties: 2011 Q2
Iowa UI=73 Weeks and Illinois UI=99 Weeks

(c)

Oklahoma-Texas Border Counties: 2010 Q4
Oklahoma UI=62 Weeks and Texas UI=79 Weeks

(d)

Virginia-WestVirginia Border Counties: 2011 Q3
Virginia UI=73 Weeks and WestVirginia UI=98 Weeks

(e)

NorthDakota-Montana Border Counties: 2010 Q2
NorthDakota UI=43 Weeks and Montana UI=63 Weeks

(f)

Tennesse-Kentucky Border Counties: 2011 Q2
Tennesse UI=88 Weeks and Kentucky UI=99 Weeks

Data Sources: BLS LAUS, TIGER geographic shapefiles, EUC and EB trigger reports

The key point in Figure 2.1 is that the average difference across state bound-

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aries among border counties—reflecting a major source of the identifying variation used by the BPFE approach on county aggregates— can be quite large, but the labor market conditions seem to converge near the border— reflecting the identifying variation used by the measurement error corrected RD approach.

While Figure 2.1 pertains only to a few sets of border counties that were chosen to illustrate the potential issues, it suggests that it may be important to test the robustness of the large effect of UI benefits on unemployment using the alternative RD identification approach. Importantly, our empirical analysis will not be based only on a handful of cases, but will pool together over 600 such episodes. By pooling across so many separate RD designs, we avoid issues that would surface when relying on any one case, including small samples, over- or under-fitting the specification of the running variable, or idiosyncratic factors that happen to impact particular counties at particular points in time. Indeed, such factors would be expected to average out across the many cases considered. Appendix C provides evidence based on placebo tests supporting this possibility.

3   Data

In order to highlight the importance of our methodological approach, we match the data sources in prior work as closely as possible. we consider the period from 2005 to the end of 2011. The data for the unemployment rate come from the Local Area Unemployment Statistics (LAUS) compiled by the Bureau of Labor Statistics (BLS). The LAUS provide estimates of county-by-month unemployment counts.[7] We aggregate this data to the quarterly level to match the prior work as well. When comparing to past work using quasi-differenced

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[7]Note that the LAUS based measures are derived, in part, by using state-level unemployment to help overcome sampling issues at such a low level of aggregation thereby introducing error in the measures, an issue discussed in Hall (2013) and Hagedorn et al. (2015). Once again, we choose to use the LAUS data to maintain comparability with past work. The placebo tests presented in Appendix C provide evidence that using these measures does not generate an artificial jump in measured labor market conditions at boundaries. We use LAUS data from before the 2015 redesign to maintain comparability to past work (Boone et al., 2017).

log unemployment, we use the monthly separation rate from the BLS's Job Openings and Labor Turnover Survey (JOLTS).

We also consider employment, earnings, and wage outcomes from the U.S. Census's Quarterly Workforce Indicators (QWI) and the BLS's Quarterly Census of Employment and Wages (QCEW). For earnings, we use the average monthly earnings for all jobs in a county recorded at the beginning of the subsequent quarter. For wages, we use the average weekly wage from the QCEW.

We rely on two sources for the UI benefit duration variables. First, we use the data compiled by Rothstein (2011) for the period from 2005-2010, and we code the final year from the trigger reports for both the EUC and EB programs available online.[8] Together this yields weekly data for each state on the number of weeks of UI insurance currently available. We again aggregate this data to the quarterly level, weighting by the number of days a particular duration was in effect.

To implement our measurement error-corrected RD estimation, we require data on the geographic population distributions within counties. We use population counts by census block from the 2010 Census found in the TIGER geographic shapefiles. The TIGER shapefiles provide precise location data for the census block borders allowing us to calculate the distance from the center of each census block to the state boundary. The very small geographic scope of census blocks allows us to approximate a continuous measure of distance to the border. Finally, we also use the county-level 2010 Census population counts as the weights for our weighted regressions.

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[8]See `http://ows.doleta.gov/unemploy/trigger/` and `http://ows.doleta.gov/unemploy/euc_trigger/`.

4 Regression Discontinuity: Controlling for Endogeneity of UI Extensions

A key issue in the current setting is that measures of labor market outcomes are available (or reliable) only at an aggregate level, making direct implementation of the RD approach difficult. In order to highlight the implications of using aggregate measures and our methodological approach to dealing with these issues, we start with an ideal RD setting with hypothetical disaggregate data and show how it relates to the feasible estimation using aggregate data. Note that the BPFE approach effectively assumes any aggregation issues are similar in paired counties by removing only the unobservable factors common to both counties in the pair. By confronting the aggregation issues we are relaxing this restriction.

4.1 Measurement Error Corrected Geographic RD

To start, consider the basic RD setup where we are interested in estimating the average treatment effect (τ) of a program or policy where treatment is determined by a continuous "running variable" crossing a particular cutoff. Denote the outcome of interest by y, the running variable by x and without loss of generality assume the cutoff occurs at $x = 0$. If the unobservable factors influencing the outcome evolve continuously at the cutoff then the average treatment effect at the boundary is identified nonparametrically by comparing the conditional expectation of y on either side of the cutoff:

$$\tau = \lim_{h \downarrow 0} E\left[y_i | x_i = h\right] - \lim_{h \uparrow 0} E\left[y_i | x_i = h\right]. \tag{4.1}$$

In our setting we would like to use distance to the state border separating UI regimes as the running variable in an RD setup to capture how the unobservable factors evolve on average as we approach the boundary. However, this is complicated by the fact that the available data is aggregated at the county

level. To help illustrate the problems posed by using aggregate measures, we begin by considering an ideal, but hypothetical, data setting that would allow for straightforward estimation by RD.

For simplicity, consider a single state boundary shared by two states in a single quarter. Denote our hypothetical, ideal sub-county-level observations by:

$y_{i,c,s} = \ln(u_{i,c,s})$:log unemployment rate

$x_{i,c,s}$: distance to the border

where

i indexes subregions within a county at a given distance with population $= n_i$

c indexes the county within the state

$s = 0, 1$ indexes the state with 0 the low-benefit state

First note that our hypothetical units of observation, denoted by i, are geographic subregions within a county that are at a given distance to the border. Also note that we have observations on multiple counties within each state. Each of these subregions has their own unemployment rate. If we had data on these subregions, we could estimate $E[y|x, s]$ for each state separately and obtain the estimated Average Treatment Effect (ATE) at the boundary nonparametrically as the difference across the border in the mean log unemployment rate for economic subregions right at the boundary as in Equation (4.1).

4.1.1 Measurement Error in the Running Variable

We now consider what can be estimated when we move from our ideal RD set-up with county sub-regions to one using county-level data. The first issue that arises is that county-level aggregates allow for only one distance measure per county, so that the observed running variable will take on one value for all subregions in a county. The resulting discrete measure of distance implies we must move from nonparametric to parametric RD estimation (Lee and Card,

2008). For state s, assume that among the set of border counties we can approximate the conditional expectation of y by:

$$E[y|x,s] = m_s(x) \qquad (4.2)$$

where $m_s(x)$ is a polynomial of order J

For ease of exposition, let $J = 2$ so that we can write our subregion-level regression equation for state s as

$$y_{i,c,s} = b_{0,s} + b_{1,s}x_{i,c,s} + b_{2,s}x_{i,c,s}^2 + \varepsilon_{i,c,s} \qquad (4.3)$$

The parametric ATE estimate with our hypothetical, disaggregated data will simply be $\hat{\tau} = \hat{b}_{0,1} - \hat{b}_{0,0}$.

One way to move from the subregion-level regression to one based on county-level data is to use the fact that when data has a grouped structure, OLS on the individual data yields identical estimates to an appropriately weighted regression on the group mean data. This leads to the following group-mean level regression estimated with weights proportional to the number of sub-groups within a county:[9]

$$\overline{y}_{c,s} = b_{0,s} + b_{1,s}\overline{x}_{c,s} + b_{2,s}\overline{x^2}_{c,s} + \nu_{c,s} \qquad (4.4)$$

Here, $\overline{y}_{c,s}$ is the mean of $y_{i,c,s}$ within a county and $\overline{x^r}_{c,s}$ is effectively the rth uncentered moment of the distance to the border over the population distribution for each county. Most importantly, these county-level distance moments do not need to come from the same data set as the information on the outcomes of interest. Note that controlling for these moments differs from the common

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[9]Note, how we define our subregions will determine the appropriate weights. Here, a reasonable choice would be to consider each subregion to have the same population, $n_i = \overline{n} \,\forall i$, so that the appropriate weights will be proportional to county-level population (more populous counties have more $\overline{n}$ sized regions). This corresponds to using sub-county areas with the same population as the relevant unit of observation in the hypothetical regression.

practice of using the distance from the geographic centroid of an area as the running variable (Chen et al., 2013; Dachis, Duranton, and Turner, 2011; Dell, 2010; Falk, Gold, and Heblich, 2014; Lalive, 2008; Rathelot and Sillard, 2008). This corresponds to a special case of the procedure in Bartalotti, Brummet, and Dieterle (2017), who show that using the moments will eliminate the asymptotic bias present when using the centroid based distance.

4.1.2 Aggregation-Induced Error in the Outcome Variable

Given the nonlinearity in the log unemployment measure, we do not actually observe $\overline{y}_{c,s}$, the average log unemployment rate across the subregions in a county. Instead we observe $y_{c,s}$, the log unemployment rate for the county as a whole, where generally $y_{c,s} \neq \overline{y}_{c,s}$ due to the nonlinearity in $y_{i,c,s}$. The fact that the two aggregate outcome measures differ is important because it could generate bias in our estimates by introducing a wedge between what we can estimate with available county-level data and what we would estimate with access to the ideal, but hypothetical, subregion-level data. Note that this is also a potential problem for the BPFE approach as it is directly linked to our motivating concern of within county heterogeneity in outcomes relative to the border being linked to the shocks that triggered UI extensions.

We choose to cast this as a measurement error problem and write $y_{c,s} = \overline{y}_{c,s} + \epsilon_{c,s}$, where $\epsilon_{c,s}$ is the measurement error from aggregating the dependent variable. Plugging into Equation (4.4), our estimating equation becomes:

$$y_{c,s} - \epsilon_{c,s} = b_{0,s} + b_{1,s}\overline{x}_c + b_{2,s}\overline{x^2}_c + \nu_{c,s}$$

$$y_{c,s} = b_{0,s} + \epsilon_{c,s} + b_{1,s}\overline{x}_c + b_{2,s}\overline{x^2}_c + \nu_{c,s} \qquad (4.5)$$

Equation (4.5) is now based on county-level observations of variables we actually observe in the data. Our estimate of the intercept will identify:

$$\hat{b}_{0,s} = b_{0,s} + E[\tilde{\epsilon}_{c,s}]$$

where $\tilde{\epsilon}_{c,s}$ is the residual from the linear projection of the measurement error on the distance moments. Finally, this yields the following, potentially biased, estimate of the treatment effect:

$$\hat{\tau} = \tau + E[\tilde{\epsilon}_{c,1}] - E[\tilde{\epsilon}_{c,0}] \tag{4.6}$$

Our ability to uncover the average treatment effect at the boundary depends on the difference in the average residual aggregation error across the border. More generally, it depends on whether the average aggregation error is systematically different for high and low-benefit states.

In Appendix A.1 we show that the aggregation error for a county can be approximated by:

$$\epsilon_c \approx \frac{\sigma_{U,c}^2}{2\overline{U}_c^2} - \frac{\sigma_{L,c}^2}{2\overline{L}_c^2} \tag{4.7}$$

where $\sigma_{U,c}^2$ and σ_L^2 are the variances of unemployed and labor force counts across the subregions within a county and $\overline{U}_c$ and $\overline{L}_c$ are the mean unemployed and labor force counts for the subregions within a county. From Equation (4.7), it is clear that the measurement error is larger when there is more within-county variation in labor market outcomes across subregions and when this variation differs between unemployment and labor force counts.

Using auxiliary data from a restricted-use version of the American Community Survey (ACS), we provide suggestive evidence in Appendix A.1 that this approximation error is not systematically related to UI extensions. Across specifications, the implied bias in our estimates varies from 0.0104 to 0.0833 (in absolute value), which is only 2 to 16 percent of the baseline estimates in Section 4.2 and suggests that this error is not a major source of bias in this setting. Once again, this is important because it suggests that by controlling for the distance moments we have some confidence that we are approximating the ideal, but infeasible, RD in Equation (4.3).

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4.2 Main Specifications

Our application differs from the basic RD setup in that we have a separate geographic RD for each state-boundary in each quarter when the two states have different UI benefits.[10] We pool these separate RDs together, and include boundary-by-quarter fixed effects in order to account for the mean differences across these separate RD designs. By pooling across many— over 600— separate cases, idiosyncratic factors particular to any given boundary at a point in time will average out.[11] We also estimate the effect of a continuous treatment variable, weeks of available UI, rather than a binary high versus low UI treatment as in Section 4.1. Moving from the binary treatment setting to estimating the effect of the duration of UI benefits in weeks simply requires rescaling the treatment effect by the difference in UI benefits across a border at a particular time.

In our baseline RD setup in Equation (4.8) we do not control for distance to a border:

$$y_{c,s,g,t} = \pi + \gamma b_{s,g,t} + \delta_{g,t} + \rho_{c,s,g,t} \tag{4.8}$$

where c indexes counties, s indexes states,

g indexes state boundaries, and t indexes quarters

$b_{s,g,t}$ is the log benefit duration

$\delta_{g,t}$ are boundary-by-quarter fixed effects

In Appendix B we discuss in detail how this approach relates to the BPFE regressions. Intuitively, not controlling for distance in the RD is akin to not allowing for within county heterogeneity in the BPFE approach as both assume that the county-level aggregate is an appropriate measure of the labor market

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[10]See Keele and Titiunik (2014) and Cattaneo et al. (2016) for a discussion on interpreting the estimates with heterogeneous responses.

[11]In Appendix C, we provide placebo tests using data from boundaries in quarters where there was no difference in UI benefits to help illustrate this point. Across different ways of generating placebo treatment variables, we find no evidence of a discontinuity at the border.

conditions at the border.

We then estimate regressions controlling for distance to the border using the measurement error correction from Section 4.1:

$$y_{c,s,g,t} = \pi + \gamma b_{s,g,t} + \delta_{g,t} \tag{4.9}$$

$$+ \mathbf{D}_{g,t} \left[(1 - T_{s,g,t}) \left(\sum_{r=1}^{R_{0,g,t}} \theta_{g,t}^0 \overline{x^r}_c \right) + (T_{s,g,t}) \left(\sum_{r=1}^{R_{1,g,t}} \theta_{g,t}^1 \overline{x^r}_c \right) \right] + u_{c,s,g,t}$$

where

$\mathbf{D}_{g,t}$ is a vector of indicators for each boundary-by-quarter

$T_{s,g,t} = \mathbf{1} \left[b_{s,g,t} > b_{-s,g,t} \right]$ are indicators for being on the high-benefits side

$\overline{x^r}_c$ is the rth uncentered moment of the distance to the border distribution

$R_{T,g,t}$ is the order of the polynomial in distance for group T

The expression in square brackets allows for different polynomial orders and for the effect of distance to differ across state-boundary-quarter groups. In practice, we consider separate linear functions in distance for each side of the border in each quarter. In other specifications, we allow for different polynomial orders for each side of the border in each quarter. We obtain $\overline{x^r}_c$ from census block-level population counts from the 2010 U.S. Census. We first calculate the distance from the center of that census block to the nearest state boundary.[12] Note that for counties close to several state borders, not all census blocks in the county share the same nearest neighbor state. Fortunately, this affects less than 7 percent of census blocks within border counties. We therefore determine the modal nearest state boundary among the census blocks within each county and use the distance to that modal boundary for all census blocks in the county. Once we have the census block-level distances, we can easily cal-

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[12]We use the -nearstat- package in Stata to calculate the distances (Jeanty, 2010). We alter the ado file slightly to generate distances using Stata's "double" precision storage type. Given the small size of census blocks, less precise storage types lead to problematic rounding errors in identifying the center of a census block.

culate the necessary population-weighted uncentered distance moments. Note that census blocks are very small levels of aggregation, allowing us to approximate a continuous distance measure. For instance, the fifteen counties at the Texas-Louisiana border contain over 49,000 census blocks.

4.3   RD Results

4.3.1   Unemployment Rate

Table 4.1 displays our main RD results for unemployment using the same set of border counties as was used in the previous literature to estimate the effects by BPFE. We present results weighting by county population. Following HKMM, we also present calculations based on the estimates for the implied unemployment rate starting from a base rate of 5 percent under two counterfactuals: one based on the average increase in benefits and one assuming a change to the maximum benefit duration. We cluster standard errors at the state-by-boundary level as this is the level at which treatment varies— accounting for both contemporaneous spatial correlation and serial correlation over time.

In Column (1) of Table 4.1 we present RD results not controlling for distance. The implied unemployment rates from the policy counterfactuals— 9.1 percent for change to the average benefit duration and 9.9 percent for a change to the maximum duration— are quite close to the corresponding BPFE estimates from HKMM of 8.6 percent and 10.5 percent. Controlling for a linear function in distance in Column (2) (captured here by including the population-weighted mean distance from the border for a county) produces considerably smaller estimates that are no longer statistically significant. The implied unemployment rate starting from a baseline of 5 percent for the two policy counterfactuals drops to 5.5 percent.

We also estimate the difference in labor market outcomes using higher-order polynomials in distance. Following Lee and Lemieux (2010), we use a cross-validation procedure to choose the length of the polynomial, opting for

Table 4.1

RD Estimates: Log Unemployment			
	(1)	(2)	(3)
		RD Polynomial Order	
Dependent Variable	*0*	*1*	*AICc*
Log Unemployment Rate	0.5100	0.0765	0.0333
	(0.1003)	(0.1574)	(0.1685)
	Implied Unemployment Rate from Base of 5%		
Average Benefits	9.0%	5.5%	5.2%
	[7.0%, 11.1%]	[3.5%, 7.4%]	[3.2%, 7.2%]
Max Benefits	9.9%	5.5%	5.2%
	[7.3%, 12.5%]	[3.3%, 7.8%]	[2.9%, 7.5%]
Observations	8,435	8,435	8,435

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).
Data Sources: BLS LAUS, TIGER geographic shapefiles, EUC and EB trigger reports.

the small sample-corrected version of the Akaike Information Criteria (AICc). For each state-by-boundary-by-quarter we progressively add higher-order terms (quadratic, then cubic, and so on) until the AICc no longer suggests an improvement in fit. Across all state-boundary-by-states-by-quarters, the largest polynomial suggested by this procedure is a cubic. Allowing for different polynomials across quarters for the same state-by-boundary accounts for the possibility that different shocks will propagate through space in different ways. Using the AICc-selected polynomials, the magnitude of the estimate in Column (3) falls slightly and now implies an unemployment rate of 5.2 percent for the two policy counterfactuals. Note that even with the AICc-chosen polynomial the estimates are still precise. The baseline estimate in Column (1) is statistically different from that in Column (3) with a p-value of 0.0134.

4.3.2 Employment, Earnings, and Wages

We also use our measurement error-corrected RD approach to estimate the change in the employment to population ratio, earnings, and wages at state boundaries. Unfortunately, the estimates are not very precise, leading to large confidence intervals and suggesting caution in interpreting the results. The first row of Table 4.2 presents results for the employment to population ratio. Not controlling for distance, we see a fairly large negative elasticity in response to extended benefits. Controlling for distance reduces the estimate substantially, however the new RD estimates are fairly imprecise. Table 4.2 displays results using log earnings from the QWI and the log of weekly wages from the QCEW as the outcome of interest. For both measures we estimate negative elasticities when not controlling for distance that become positive, but very imprecise when including a linear function in distance to the border. Finally, when allowing for a higher order polynomial, both estimates are smaller in magnitude, but still imprecisely estimated. Despite the imprecision, the pattern of estimates is consistent with our motivating story of estimates using county level measures being biased by the negative shocks triggering the UI extensions.

4.4   Comparison to Past Work

Taken at face value, the results above suggest much smaller effects of UI extensions on unemployment during the Great Recession than found in HKMM. Nevertheless, these results differ from HKMM in two important ways. First our choice of using a measurement error-corrected RD instead of the BPFE approach. In addition, we use the log unemployment rate as our main outcome of interest whereas HKMM develop and use the "quasi-differenced" log unemployment rate as an alternative measure of unemployment. Here we will check the sensitivity of our results to using the quasi-differenced unemployment measure and applying the BPFE methodology. See Online Appendix A for a detailed replication of the original HKMM results.

Table 4.2

RD Estimates: Employment, Earnings, and Wages

	(1)	(2)	(3)
			RD Polynomial Order
Dependent Variable	*0*	*1*	*AICc*
Log Emp/Pop: QWI	-0.3065	0.0023	-0.0867
	(0.1227)	(0.3803)	(0.3988)
Observations	8,379	8,379	8,379
Log Earnings: QWI	-0.0291	0.1004	0.0030
	(0.1153)	(0.1515)	(0.1648)
Observations	8,380	8,380	8,380
Log Weekly Wages: QCEW	-0.0567	0.1456	-0.0359
	(0.1156)	(0.1471)	(0.1583)
Observations	8,444	8,444	8,444

Standard errors clustered at the state-by-boundary level in parentheses. 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).
Data Sources: QWI, QCEW, TIGER geographic shapefiles, EUC and EB trigger reports.

HKMM note that regressing log unemployment on UI benefits may not identify the contemporaneous effect of UI benefits. Rather, such an approach will identify the combined effect of current benefits and expectations over future benefits— an effect that is difficult to interpret. Intuitively, today's undifferenced unemployment rate may reflect changes in vacancy creation decisions by firms due to expectations over future UI benefit duration. Since future benefits determine the reservation wage of workers and, in turn, expected future profits to the firm from a current job match, they directly affect the value of that filled job today. HKMM show that under some assumptions the quasi-differencing will cancel out the portion due to future benefits leaving just the portion due to current benefit duration.

In Table 4.3 we present results using the quasi-differenced measure and the BPFE estimation approach, as well as applying our measurement error corrected RD approach using the quasi-differenced outcome measure. The BPFE results are estimated from the following equation after creating a county-pair

level data set from our baseline county-level panel:

$$\Delta y_{p,t} = \alpha \Delta b_{p,t} + \psi_p + \phi_t + v_{p,t} \qquad (4.10)$$

$$\text{where } p \text{ indexes county pairs}$$

$$t \text{ indexes quarters}$$

where $\Delta y_{p,t}$ is the difference between two border counties in a given quarter in the quasi-differenced log unemployment rate and $\Delta b_{p,t}$ is the corresponding difference in log unemployment benefit duration. The quasi-differencing is given by the following:

$$y_t = \log(u_t) - \beta(1 - d_t)\log(u_{t+1})$$

$$\text{where}$$

$$u_t \text{ is the unemployment rate}$$

$$\beta \text{ is the discount rate}$$

$$d \text{ is the job separation rate}$$

The BPFE estimates in Column (1) are quite close to the original HKMM estimate. Since the quasi-differenced measure takes account of expectations over future benefits, we now need to account for both the size of the benefit extension and the duration for which the extension is in effect when calculating the counterfactual unemployment rates. The implied counterfactual unemployment rates in Column (1) are very close to the BPFE estimates from HKMM of 8.6 percent and 10.5 percent. Importantly, when we switch from the BPFE estimation to the RD estimation not controlling for distance to the border in Column (2) we also find similar estimates. However, just as with our main specification, the estimates fall and become small and statistically insignificant when we add controls for distance in Columns (2) and (3). This suggests that our divergent results are due to the choice of methods rather than our choice

of dependent variable.

Table 4.3

BPFE versus RD Estimates: Quasi-Differenced Log Unemployment

	(1)	(2)	(3)	(4)
	BPFE		RD Polynomial Order	
Population Weighted		*0*	*1*	*AICc*
Coefficient	0.0604	0.0660	0.0238	-0.0107
	(0.009)	(0.0113)	(0.0202)	(0.0227)
	Implied Unemployment Rate from Base of 5%			
Actual Duration and Benefits	8.6%	9.0%	6.1%	4.5%
	[7.2%, 9.9%]	[7.2%, 10.8%]	[4.0%, 8.4%]	[2.7%, 6.4%]
Permanent and Max Benefits	10.5%	11.2%	6.7%	4.4%
	[8.2%, 12.7%]	[8.2%, 14.3%]	[3.4%, 9.9%]	[2.0%, 6.8%]
Observations	32,972	8,446	8,446	8,446

Standard errors clustered at the state-by-boundary level for RD and pair level for BPFE in parentheses. 95% Confidence Intervals in square brackets. BPFE indicates Boundary Pair Fixed Effects estimates. 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). Data Sources: BLS LAUS, TIGER geographic shapefiles, EUC and EB trigger reports.

These patterns highlight the concerns with using county-level aggregates in this setting. Specifically, the fact that the estimates of large labor market effects disappear once we control for distance to the border is consistent with the story that the negative shocks that triggered UI extensions disproportionately affect areas in the high benefit states— even when considering only border counties.

5 Policy Spillover Evidence

We now turn our attention to the second potential source of bias when using boundary-based identification approaches – across border policy spillovers. In particular, when people face low costs of working or searching in either state at a boundary, changes to UI benefits in one state may affect workers and firms in both states. This will bias the resulting estimates towards zero, and this bias is expected to be more important closer to the border where labor markets are more integrated. Given the importance of both the distance to the border and the potential problems from using highly aggregated data uncovered by our

RD approach, we look for evidence of policy spillovers close to state borders within small geographic areas in border counties. HKMM look for evidence of across border spillovers by considering subsample sensitivity checks— focusing on counties within 30 miles of a border or only considering border pairs within the same Core Based Statistical Area— and by looking for evidence of out-of-state search using the county-level ACS commuting data available for counties with population greater than 100,000. Across these sensitivity checks and additional analyses, HKMM find no statistically significant differences from their main estimates or any evidence of changes in across border search.

We explore the possibility of workers in one state altering their job search patterns in response to UI benefits in neighboring states using data from the Longitudinal Employer-Household Dynamics (LEHD) Origin-Destination Employment Statistics (LODES). The LODES provide annual counts of employed workers for pairs of census blocks with one census block indicating the location of residence and one indicating where they work.[13] This very precise location information allows us to identify changes in the number of people working across the state border in response to differences in UI benefits.[14]

5.1 Event History Approach

To test the worker search response to UI differences across borders in a clean manner, we consider an event history approach based on a subsample of state boundaries. We first identify 57 of the 106 state boundaries where there is a difference between the two states in UI benefit duration, such that one state is

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[13]For our application, we use the count of all jobs presented in the JT00 LODES files.

[14]As a caveat, producing the LODES requires the Census Bureau to multiply impute data in the case of missing information and to protect confidentiality in small areas through use of noise infusion and synthetic data methods. See Graham, Kutzbach, and McKenzie (2014) for a discussion of the imputation process. Importantly, even before imputation 97 percent of jobs are located with sub-county-level precision and 96 percent of worker residences have at least county-level precision. The imputation ensures, at least, census tract level precision for all residences. The method used for protecting workers' residence is based on census tract level residence distributions for 90 percent of home-to-work flows, with only long distance commutes based on more aggregate geography. For our purposes, this helps ensure that we have properly identified the state of employment and captured the distance to the border at a fairly small disaggregated level.

always the high-benefit state and one is always the low-benefit state— allowing the states to have the same benefit duration in some quarters.[15] This sample selection creates two clear "treatment" groups, avoiding difficulties in modeling the dynamic changes in job search and vacancy creation behavior as the relative generosity of benefits between the states changes over time. In particular, if UI benefit differences are most important when searching for a job or if search frictions result in the measured fraction working across the border to respond slowly, then we might suspect that current employment outcomes depend on past UI differences. In our restricted sample, workers and firms have more time to respond to the UI differences and the responses in one period will not be offset by responses in other periods when the relative ranking of UI benefit duration at a boundary is switched. Importantly, this choice of sample includes a fairly representative group of boundaries, as shown in Appendix Table E.1.

Figure 5.1 depicts the mean UI differences over time for our high- and low-benefit states. Since the LODES employment data we use are based on records from the beginning of the second quarter in each year, we label the axis at April of each year. The vertical dashed lines mark the beginning of April 2008 and the beginning of April 2009. Note that prior to April 2008, there were no benefit differences for the previous four years in this subsample. However, between April 2008 and April 2009, we see a steady increase in average benefit differences that corresponds to the introduction and subsequent expansion of the EUC program. By April 2009, we see an average difference between the high- and low-benefit states of over 10 weeks. For the most part, this difference in mean benefit duration persists until April 2011.

We now consider how the fraction of individuals working across the border evolves over time for our high-benefit and low-benefit subsamples. We calculate the fraction of employed individuals who work in the neighboring state by home census block. Table 5.1 provides summary statistics for the fraction and number

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[15]Generally, not all states have data available in all years in the LODES; however, each state in this subsample is present in all years used.

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Figure 5.1

Mean Difference in Maximum UI Benefits Over Time
Clean Sample
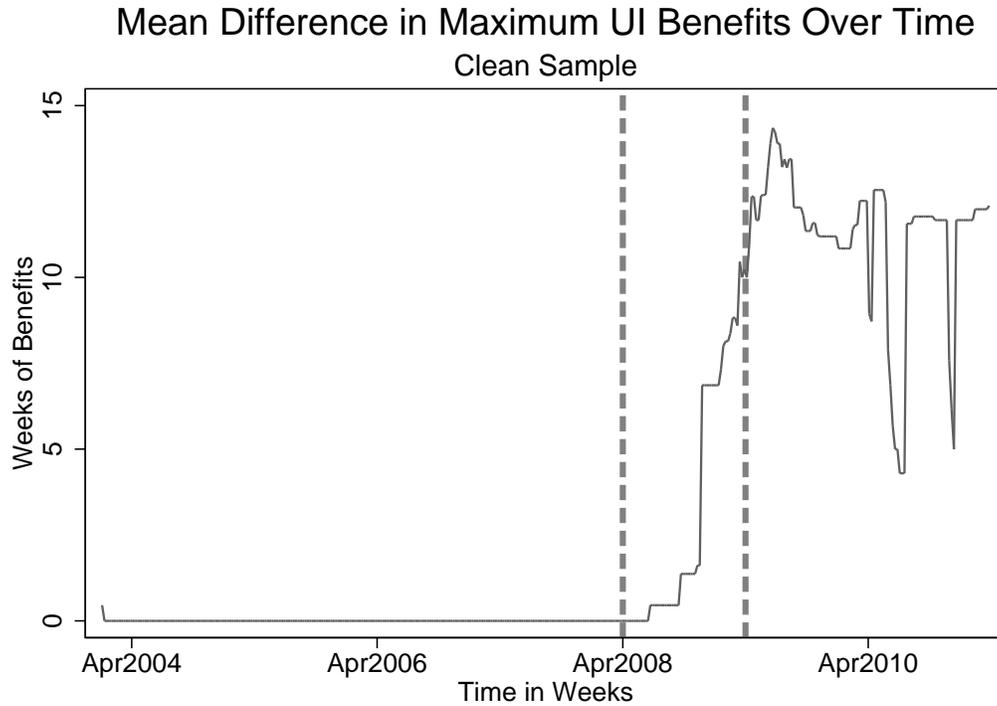
Source: EB and EUC trigger reports. Average difference in available benefits between states identified as always high and always low-benefit states at a given boundary by week.

of workers employed across the border along with the total number employed for the high- and low-benefit event history samples, focusing on census blocks within five kilometers of a state border in the final pre-policy period, 2008.

In Panel (a) of Figure 5.2 we display trends in the average fraction working across the border by year for census blocks within 5km of the state border in our two groups— similar results are found when using 1km and 10km bandwidths. We normalize the fraction to be zero in 2008 for both groups to focus attention on the before/after comparison. Again we mark the transition period between April 2008 and April 2009 with vertical dotted lines. We see a relatively flat profile for the fraction working across the border before 2009 for both the high

Table 5.1

Summary Statistics: Event History Sample Census Blocks within Five km in 2008

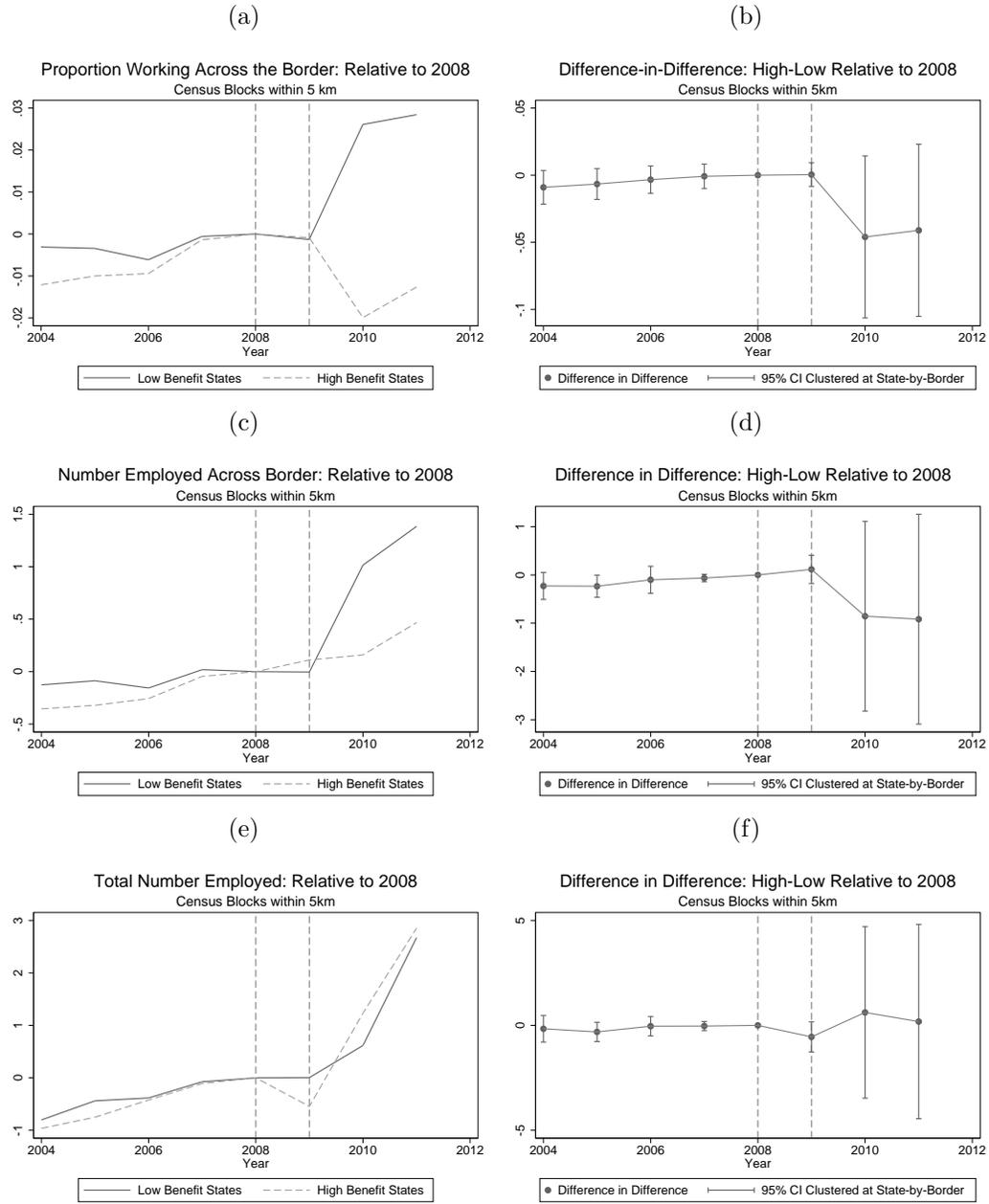|	Low-Benefit		High-Benefit	
	Mean	Std. Dev.	Mean	Std. Dev.
Fraction Working Across Border	0.15	0.24	0.20	0.26
Number Working Across Border	2.45	8.30	3.62	11.29
Total Employed	18.64	37.96	17.59	35.65
Observations	82,508		68,223	

Data Source: LODES

and low-benefit states when there were no differences in UI benefits. However, by April of 2010, after over one year of sustained mean benefit differences, we see a decline in the proportion working across the border for those living in high-benefit states and an increase for those living in low-benefit states. This is consistent with workers targeting their job search in high-benefit states in connected border area labor markets. Given a base rate of 14.8 percent in the low-benefit states in 2008, the nearly three percentage point increase in the employed population working across the border is substantial.

In Panel (b) of Figure 5.2, we show the difference between the two lines in Panel (a), yielding difference-in-differences estimates, along with ninety-five percent confidence intervals using standard errors clustered at the state-by-border level as in our main analysis. Note that these confidence intervals are likely to be conservative as they allow for arbitrary correlation between census blocks along the border both contemporaneously and over time. For instance, we are allowing for correlation in the error term for a census block on the western edge of a state border in 2004 with a census block on the eastern edge in 2011. While the ninety-five percent confidence intervals overlap zero for both the 2010 and 2011 differences, the joint test of whether the difference between the high- and low-benefit states was the same in 2010 and 2011 compared to 2008 is rejected with a p-value of 0.0408. As a point of comparison, the joint test for the two years prior— 2005 and 2006— does not reject the null with a p-value of 0.7985. This suggests that census blocks within five kilometers of

state borders in the high- and low-benefit states experienced different trends in the proportion working across the border once benefits differed between the two groups.

We also show the evolution of the number of workers employed across the border in Panels (c) and (d) and the total number employed in either state within the census block in Panels (e) and (f)— reflecting the numerator and denominator of the proportion variable used in Panels (a) and (b). In terms of the level of across-border employment, we see a slight increase for the high-benefit states but a much larger increase for the low-benefit states. The difference-in-differences estimate suggests nearly one fewer worker working across the border for the high-benefit states. Given mean employment across the border of 2.45 workers for high-benefit states in 2008, this is a large response. However, the estimates in levels are less precise, likely due to increased residual variation in levels of across border employment compared to the fraction. Finally we see that for both sets of states, the total number of residents per census block who were employed in either state was increasing after 2009, likely reflecting the economic recovery.

Figure 5.2

(a)

(b)

Proportion Working Across the Border: Relative to 2008
Census Blocks within 5 km

Difference-in-Difference: High-Low Relative to 2008
Census Blocks within 5km

Low Benefit States — High Benefit States

● Difference in Difference — 95% CI Clustered at State-by-Border

(c)

(d)

Number Employed Across Border: Relative to 2008
Census Blocks within 5km

Difference in Difference: High-Low Relative to 2008
Census Blocks within 5km

Low Benefit States — High Benefit States

● Difference in Difference — 95% CI Clustered at State-by-Border

(e)

(f)

Total Number Employed: Relative to 2008
Census Blocks within 5km

Difference in Difference: High-Low Relative to 2008
Census Blocks within 5km

Low Benefit States — High Benefit States

● Difference in Difference — 95% CI Clustered at State-by-Border

Data Source: LODES and TIGER Geographic Shape Files. Average proportion working across the state border relative to 2008 for census blocks within 5km of a state boundary for states identified as always high and always low-benefit at a given boundary.

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We next consider how this response to benefit changes varies at different distances to the border. Given the stark difference between 2007-08 and 2010-11 for the sample of census blocks within five kilometers shown in Figure 5.2, we calculate the average proportion working across the border in 2010-11 and subtract the average from 2007-08 for each census block in a border county. Figure 5.3 displays a nonparametric regression of this difference in the proportion working across the border on the distance to the border separately for the sample of low and high-benefit states. Consistent with the previous results, we see an increase in the fraction working across the border from low-benefit states and a decrease for those in high-benefit states very close to state borders. Importantly, this difference falls as one moves farther from the border. For those in low-benefit states, the difference falls to near zero by around 25 kilometers from the border. For the high-benefit states, it approaches zero by 10 kilometers to the border, but shows some small variation up to 50 kilometers.

Figure 5.3

Change in Proportion Working Across the Border
Census Blocks: 2010-11 vs 2007-08
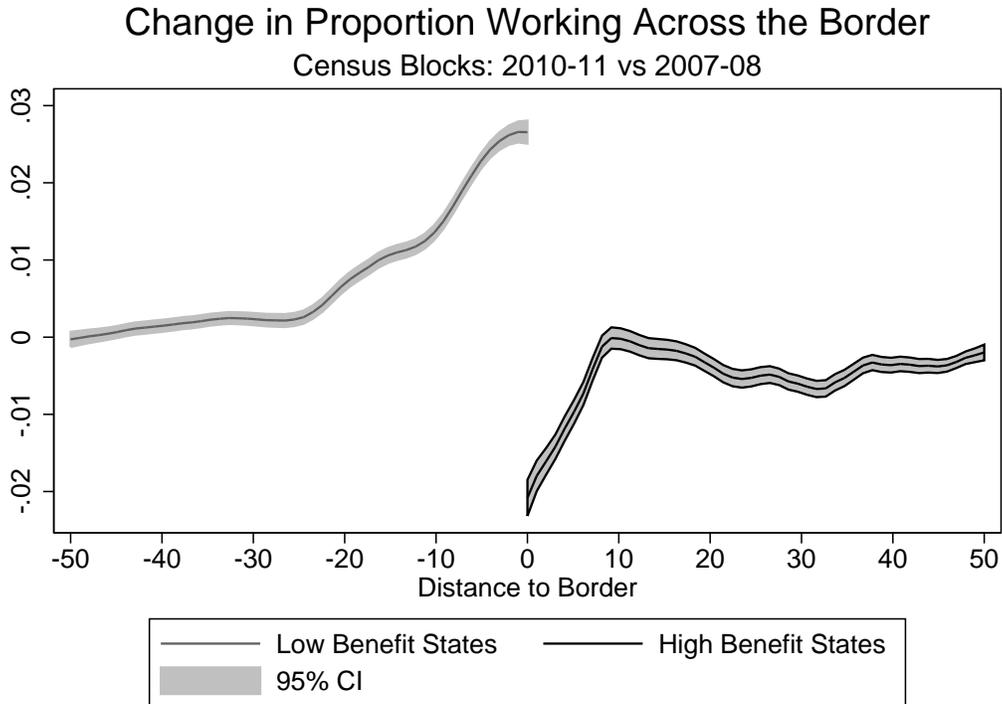
Data Source: LODES and TIGER Geographic Shape Files. Local polynomial smoothing regression of the change in proportion working across the border by distance to the border.

5.2 Implications for Boundary-based Identification Approaches

The pattern in Figure 5.3 is consistent with the spillover effects falling as one moves further away from the border. Of course, focusing on areas thirty or forty kilometers from the border may reduce concerns over the associated attenuation bias, but at the same time it will raise concerns over the endogeneity bias from the shocks that triggered UI extensions. This highlights the fundamental issue for boundary-based identification approaches, such as BPFE or geographic RD:

these approaches trade off between two sources of bias and there is no clear or straightforward way to distinguish between the two. Note that among the set of border counties used here, the population weighted mean distance to the border has an average of 23 kilometers and a maximum of 261 kilometers, implying that the BPFE approach using county-level aggregates will be affected by a mix of these two biases. The geographic scope of the spillovers presented in Figure 5.3 is also consistent with the fact that HKMM find no statistically significant differences when limiting the sample to counties within 30 miles (approximately 48km) since the spillovers appear to die out within this window around state borders

5.3 Full Sample: Census Block Fixed Effects

Here, we expand our sample to include all state boundaries in order to check whether the previous results were driven by the choice of our clean event history sample. We use the full sample of census blocks by year that are within 5km of the border and estimate the following:

$$f_{i,s,g,t} = \alpha + \beta \Delta \bar{b}_{s,g,t} + \delta_i + \phi_t + \varepsilon_{i,s,g,t} \tag{5.1}$$

where i, s, g, t index census blocks, states, boundaries, and years

f is the fraction of workers employed in the neighboring state

$\Delta \bar{b}_{s,g,t} = \bar{b}_{s,g,t} - \bar{b}_{-s,g,t}$ is the yearly average difference in benefit weeks

δ_i are census block fixed effects

ϕ_t are year fixed effects

Table 5.2 presents the estimates of Equation (5.1) with standard errors clustered at the county level to allow for arbitrary spatial and serial correlation. Consistent with the story above, we estimate a negative relationship between the relative generosity of UI benefits in your home state and the fraction working in the other state. That is, as the UI benefits in your state of residence

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increase relative to those across the border, workers are less likely to work in the neighboring state. Likewise, as the benefits in their home state fall further behind those across the border, workers are more likely to work in the neighboring state. To provide a sense of scale, the estimates suggest that having benefits 15 weeks longer in your state of residence, roughly the mean benefit difference in periods with a difference, would lower the fraction working across the border by 3.0 percentage points. This is 19 percent of the mean fraction working across the border for census blocks within 5km.

While the census block fixed effects help control for time constant heterogeneity related to both the propensity for individuals to work in the neighboring state and the UI benefits differences, it does require a strict exogeneity assumption that $E[\varepsilon_{i,s,g,t}|\Delta\bar{b}_{s,g,1}, ..., \Delta\bar{b}_{s,g,T}, \delta_i] = 0, t = 1, ..., T$. We might be concerned that the fraction working across the border in one year may be the result of job matches made in previous years when the relative generosity of UI benefits differed. One way to account for this potential feedback and violation of strict exogeneity is to directly control for lagged values of the UI benefit difference. In Table 5.2, we see that the controlling for the lagged benefit difference or up to three years of lagged differences has little effect on the estimated contemporaneous effect. In addition, the effects of previous benefit differences become smaller as we look at further and further lags.

5.4   Implications for Macro Effects of UI Extensions

The evidence above suggests that residents of one state may respond to UI benefit changes in the neighboring state. This analysis by itself cannot be used to directly evaluate the the effect of UI extensions more generally. For instance, it tells us nothing of the aggregate importance of shifting job creation or employment between areas facing different UI regimes. However, the particular nature of the spillovers presented here does provide insight into the equilibrium response to UI extensions. To that end, we consider the observed flows in light of the job rationing model of Michaillat (2012) to help understand why workers

Table 5.2

Effect of Relative UI Benefit Duration on the Fraction Working Across the Border: Census Block Fixed Effects

Relative UI Difference	-0.0020	-0.0017	-0.0019
	(0.0012)	(0.0010)	(0.0010)
Lagged UI Diff		-0.0008	-0.0007
		(0.0005)	(0.0005)
Second Lag UI Diff			-0.0009
			(0.0004)
Third Lag Diff			0.0003
			(0.0010)
Observations	2,631,856	2,122,461	1,404,106
R^2	0.0044	0.0063	0.0095

Standard errors clustered at the county level reported in parenthesis.
Data Source: LODES and TIGER Geographic Shape Files.

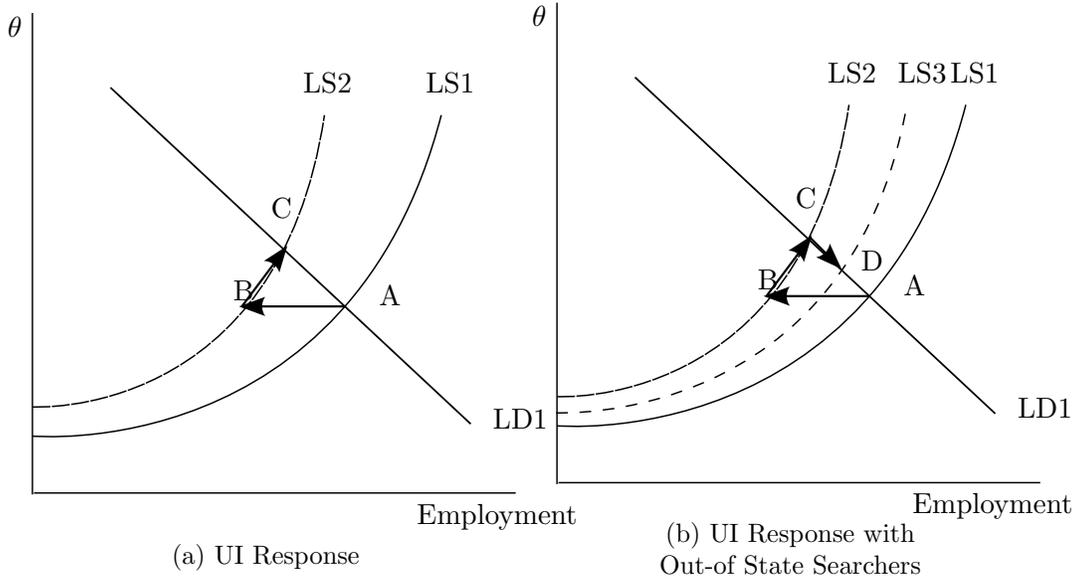might shift their search to the high benefit state following the extension of UI benefits.

To start, we consider the response to an increase in UI in the model, ignoring for the moment the workers who are not eligible for the extended benefits offered in the high benefit state. Following the discussion in Landais, Michaillat, and Saez (2016), we will consider the response in terms of employment and labor market tightness (θ)— the ratio of vacancies to the number of searchers. On the labor supply side, employment is increasing in tightness— as the number of vacancies goes up relative to searchers the probability of finding a match goes up resulting in higher employment. In a simplified version of the model, production is characterized by a concave production function and wages are fixed (Landais, Michaillat, and Saez, 2016). Labor demand is therefore downward sloping in employment-tightness space reflecting the diminishing marginal product of labor at a fixed wage. The equilibrium employment and market tightness before a change in UI is depicted in Panel (a) of Figure 5.4 by the intersection of LS1 and LD1 at point A.

The increase in UI reduces search effort by unemployed workers, thereby

shifting labor supply from LS1 to LS2 in the figure. Holding all else fixed, this results in a reduction in employment from point A to B. This reduction in employment due to reduced search effort is referred to as the micro-elasticity of employment to UI (Landais, Michaillat, and Saez, 2016). Since wages are fixed in this model, labor demand does not shift in response to increased UI benefits. In the figure, we simply move along the new, upward sloping labor supply curve— increasing tightness and employment— until it intersects the original labor demand curve. Here, the macro— or total— effect of the UI increase (from A to C) is smaller than the micro effect (from A to B). Importantly, this runs counter to the motivation for large macro effects of UI in HKMM.

The presence of out-of-state searchers complicates the model. First we need to distinguish between unemployed searchers whose previous job was in the high benefit state who are, therefore, eligible for the extended UI and those from the low benefit state who are not eligible— regardless of their state of residence. Here we will consider out-of-state searchers to be those with access to the lower benefits and consider how they affect the outcome in the high-benefit state. We must assume that firms in the high-benefit state are more affected by the change in UI benefits so that the effects highlighted in Figure 5.4 are stronger in the high benefit state. For instance, if unemployed workers are more likely to search in their home state— or the state of their last job— then firms in the high benefit state would be more likely to be matched with a high UI eligible worker. As simple evidence in support of there being some friction at state boundaries, even within one-tenth of a kilometer of state borders only 22.7 percent of workers are seen working across the border in the LODES data in the period before any UI differences. Further, the fact that we observe any change in work locations in response to UI differences at state boundaries suggests that the policy impacts the two sides differently.

Figure 5.4: Response to UI Increase in Job Rationing Model (Michaillat, 2012)

(a) UI Response

(b) UI Response with
Out-of State Searchers

Since out-of state searchers are not eligible for the higher benefits, they will
not reduce their search effort. However, the change in tightness that may occur
in the high benefit state may alter their choice of which state to search in.
Given the fixed wage, the job rationing model would predict that the increase
in tightness in the high benefit state will make the high benefit state more
desirable since it would generate a wedge in the probability of making a match
between the two states. Therefore, we would expect more people to search
in the high benefit state. In terms of the figure, labor supply in the high-
benefit state would shift to LS3 in Panel (b), moving along the labor demand
curve (from point C to D). Since these new searchers may be residents of either
state— but happen to have previously worked in the low-benefit state— this
would increase the proportion from the low benefit state working across the
border while reducing the proportion from the high benefit state working in
the low-benefit state. This is precisely what we found in Section 4.3.2 and is
an unambiguous implication of the job rationing model in this setting.

In addition to predicting a smaller macro response to extending UI, the

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job rationing model has implications for the optimal UI policy as outlined in Landais, Michaillat, and Saez (2016, 2015). In particular, if tightness rises in response to more generous UI benefits, then Landais, Michaillat, and Saez (2016) provide an argument for the efficient UI policy to be counter-cyclical— namely that UI should be more generous when unemployment rises. Our spillover results provide some suggestive support for the key mechanism underlying this optimal UI response, thereby complementing other recent work (Landais, Michaillat, and Saez, 2016; Lalive, Landais, and Zweimüller, 2015).

Note that the observed pattern of across border employment could also be consistent with a more standard matching model with bargaining over wages as in Pissarides (2000). Unlike the job rationing model, the model predicts that tightness would fall in response to an increase in UI and that the macro response to an increase in UI is larger than the micro response (Hagedorn et al., 2015; Landais, Michaillat, and Saez, 2016) However, such a model would require a substantial increase in wages in response to the UI increase in the high benefit state to offset the fall in tightness in order to explain the observed change in across border employment (see Appendix D). The fact that we find no strong evidence of an increase in earnings in section 4.3.2 provides some suggestive evidence against such a response. Fully distinguishing between the two models is an important area of further research.

The observed flows of employment location from the low- to the high-benefit states are also potentially consistent with an aggregate demand response to UI extensions if the increase in product demand is concentrated in the high benefit state. Since residents of the high-benefit state are more likely to be eligible for the higher benefits, then if consumers are more likely to make purchases in their state of residence we might expect the aggregate demand channel to show up more in the high-benefit state. Like the job rationing model, the aggregate demand story would suggest a smaller macro effect of UI extensions.

6 Conclusion

In this paper, we reanalyze the boundary-based approach to identifying the effect of UI extensions on the labor market. We raise two main issues. The first is partially a data issue— the available county-level labor market measures used may be aggregated at too high level to ensure a clean quasi-experimental comparison across border counties. The second is a conceptual issue— the same factors that make an area a good control group for a neighbor across the border may make it more likely that the policy effect spills over the border contaminating the quasi-experimental control group.

To address the first issue, we develop an RD approach that accounts for this aggregation measurement error by controlling for moments of the underlying population distribution within counties. Using this measurement error-corrected RD produces significantly smaller estimates than either the previously used boundary approach or a comparable RD approach that relies on similar assumptions by omitting controls for distance. These results help to reconcile prior work on the labor market effects of UI benefits, which has focused on two sources of identifying variation: differences in UI benefits at state boundaries and differences due to sampling error in the real time unemployment measures used to trigger the UI extensions. The two sets of papers find mixed results, with the some of the boundary-based results being large and negative, while the sampling-error based approaches find smaller but imprecise effects. We show that results from boundary-based methods are much more similar to those from sampling-error approaches when controlling for distance in an RD setup. Our results also complement those in Boone et al. (2017), who use a modified boundary-based approach on a subset of border counties and find similar small effects.

We also document treatment spillovers at state boundaries with workers close to state boundaries targeting their employment search in the high-benefit states. These findings are compatible with an increase in labor market tightness

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in high benefit states associated with the extension of UI benefits. This also highlights our concern with the policy's effect showing up on both sides of the border, invaldiating the border based approach in this setting. These spillover results complement other recent work on the effect of UI extensions on ineligible searchers (Lalive, Landais, and Zweimüller, 2015; Crépon et al., 2013). On the whole, our results provide evidence against a large vacancy reduction effect of UI extensions and suggest caution in using boundary-based approaches to identify the causal effects of EB and EUC extensions.

Our measurement error-corrected RD also serves as an attractive alternative to the BPFE approach. It highlights the potential for bias from the more common geographic RD approach of calculating distance to the border using geographic centroids,[16] and is particularly useful in the case with the UI extensions during the Great Recession, when policy adoption is drive by contemporaneous factors, rather than more systematic differences between the regions being studied.

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[16]The precise characterization of the bias is derived and explored in detail in Bartalotti, Brummet, and Dieterle (2017).

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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, we have:

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

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 county-level as follows:

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

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

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

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Plugging this into our expression for the aggregation error yields:

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

Recall the bias term for our simplified case with only two states in one time period in Equation (4.6) 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.[17] We readily note that this yearly data may deviate from the quarterly data used in our analysis and that using census tract-level data

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[17]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`.

may yield noisy measures of unemployment and labor force counts. The fact that census tracts have different populations will 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 4.2, replacing the county-level log unemployment rate with the aggregation error as the dependent variable:

$$\epsilon_{c,s,g,t} = \alpha + \eta b_{s,g,t} + \psi_{g,t} + u_{c,s,g,t}$$

where c indexes counties, s indexes states,

g indexes state boundaries, and t indexes quarters

$b_{s,g,t}$ is the log benefit duration

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 4.4, 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

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.

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 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, re-

placing 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 (4.8).

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 arti-

ficial 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 and Karabarbounis (2016), 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

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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 4.3.2 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
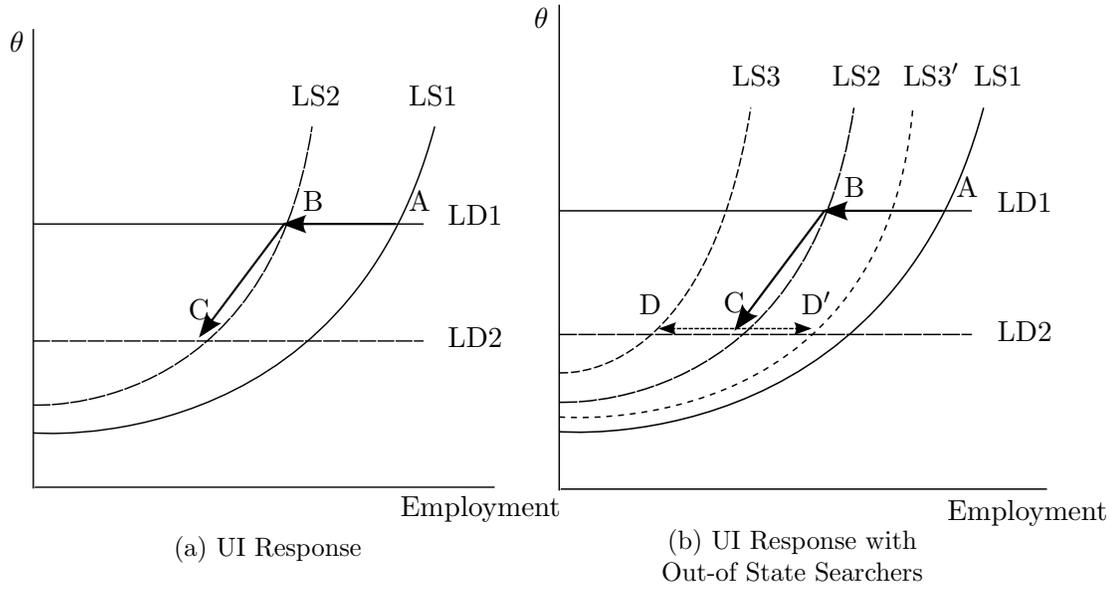
E   Additional Tables

Table C.1

RD Estimates: Placebo Treatments			
	(1)	(2)	(3)
		RD Polynomial Order	
	0	1	AICc
Random Treatment	-0.0110	-0.0378	-0.0259
	(0.0084)	(0.0178)	(0.0232)
	Implied Unemployment Rate from Base of 5%		
	4.9%	4.8%	4.9%
	[4.9%, 5.0%]	[4.6%, 5.0%]	[4.7%, 5.1%]
Observations	18,475	18,475	18,475
UI Difference Treatment	0.0344	-0.0336	-0.0766
	(0.0235)	(0.0523)	(0.0485)
	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
Unemployment 4% Trigger	0.2174	0.1301	0.0577
	(0.0370)	(0.0573)	(0.0426)
	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
Unemployment 5% Trigger	0.1372	0.0739	0.0465
	(0.0264)	(0.0507)	(0.0528)
	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).

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	