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

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

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Abstract

The extension of Unemployment Insurance (UI) benefits was a key policy response to the Great Recession. However, these benefit extensions may have had detrimental labor market effects. While evidence on the individual labor supply response indicates small effects on unemployment, recent work by Hagedorn et al. (2015) uses a county border pair identification strategy to find that the total effects inclusive of effects on labor demand are substantially larger. By focusing on variation within border county pairs, this identification strategy requires counties in the pairs to be similar in terms of unobservable factors. We explore this assumption using an alternative regression discontinuity approach that controls for changes in unobservables by distance to the border. To do so, we must account for measurement error induced by using county-level aggregates. These new results provide no evidence of a large change in unemployment induced by differences in UI generosity across state boundaries. Further analysis suggests that individuals respond to UI benefit differences across boundaries by targeting job search in high-benefit states, thereby raising concerns of treatment spillovers in this setting. Taken together, these two results suggest that the effect of UI benefit extensions on unemployment remains an open question.

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1 Introduction

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. These benefits provide a safety net to unemployed workers, but are also met by concerns over possible negative labor market effects. These concerns have led many researchers to examine the potential disincentive effects of UI duration on individual workers to seek employment, but have found only small effects. For instance, across a number of specifications Rothstein (2011) finds that labor supply responses to UI extensions account for, at most, 0.5 of the 5.5 percentage point increase in the unemployment rate during the recession.

As Hagedorn, Karahan, Manovskii, and Mitman (2015) (henceforth HKMM) note, this focus on the labor supply response to extended UI benefits only captures part of the picture, and misses a potentially large effect operating through the labor demand response of reduced vacancies. Estimating the total effects of extending UI benefits on unemployment is challenging, however, given the issue of policy endogeneity. Specifically, UI extensions are typically enacted in response to poor economic conditions and are therefore associated with periods of high unemployment regardless of the labor market effects of the policies. HKMM tackle this problem by focusing attention on counties at state boundaries facing different UI policies by adapting the boundary-pair fixed effect (BPFE) approach used by studies such as Holmes (1998) and Dube, Lester, and Reich (2010). This takes each county at a state border and matches it to at least one neighboring county in an adjacent state that was subject to a different UI policy and controls for any unobserved factors that affect the two counties in a pair similarly using pair fixed effects. In effect, identification with BPFE requires there to be no within-pair differences in factors that are systematically related to the difference in policies across the counties in a pair.

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. When considered along with the much smaller labor supply response found in studies such as Rothstein (2011), such a large estimate of the total effect of UI benefit extensions implies a very large labor demand response, and has led to a great deal of academic and media attention focused on HKMM's result.[1]

In this paper, we consider two main concerns that surface when using local variation in

[1] See Hall's (2013) comment on an earlier version of the paper and coverage in the *Wall Street Journal* (Wall Street Journal, 2013) and *Washington Post* (Plumer, 2014).

policies across borders to identify the effects of UI benefits. First, these quasi-experiments rely on the assumption that the identification strategy controls for all relevant unobservable differences on either side of the state borders that separate different UI regimes. Otherwise, the estimates may incorrectly attribute a difference in unemployment to the UI policy differences rather than to other unobservable effects. Second, workers and firms on one side of the border must not respond to UI benefit changes in the neighboring state. If workers and firms do respond to changes across the border, then this treatment spillover is likely to bias the estimates toward zero in the current setting.[2]

To address the first issue, we propose using a regression discontinuity (RD) approach to control more flexibly for unobservable differences across borders. The BPFE strategy assumes that the economic shocks that triggered UI extensions in one state evolve over space in such a way that contiguous border pairs are affected similarly. However, a natural concern is that given heterogeneity in the geographic size, spatial distribution of population, and economic makeup of counties, this may not always hold. Rather than assume that the underlying unobservables are fixed across space within county pair, the RD approach allows the effect of these unobserved factors to evolve continuously across space- both within and across counties. The RD approach accounts for these factors by estimating the difference in mean outcomes at the boundary between the two policy regimes rather than averaging over a larger geographic space as in the BPFE approach. This corresponds more directly to the experimental ideal of the BPFE approach by comparing outcomes for two areas that happen to face different UI policies but are otherwise identical in terms of all other factors driving labor market outcomes.

Moving from BPFE to RD to allow for more general patterns in unobservables across space is made challenging by the data limitations inherent to the study of unemployment and labor market outcomes. The key problem revolves around the need to use data aggregated at large geographic areas in order to capture the effects for a labor market and to avoid using potentially noisy local unemployment data. In order to tackle the problem, we start by considering a hypothetical "ideal" RD setting using disaggregated data and then show that this is equivalent to a county-level RD design with nonclassical measurement error in the running variable.[3] Since the running variable, distance to the border, has only a single measure per county, there will necessarily be measurement error for a given subregion within the county.

[2]For example, if workers are perfectly mobile within labor markets, then a change in benefits in one state will change the reservation wage for all workers in that market and all firms – regardless of the state they are located in – will reduce vacancies.

[3]In addition, the fact that the dependent variable is a nonlinear function generates another form of aggregation measurement error. In Appendix A.1, we provide evidence that the resulting bias from this aggregation is likely small in the current setting.

We implement the correction from Bartalotti, Brummet, and Dieterle (2016) for measurement error in the running variable that is based on controlling for the uncentered moments of the population distribution relative to the state border within each county. Importantly, this differs from the common practice of calculating the distance to the border using the geographic centroid of an area (Chen et al. 2011; Dachis et al. 2011; Dell 2010; Falk et al. 2012; Lalive 2008; Rathelot and Sillard 2008). In fact, this approach not only corrects for measurement error-induced bias in the RD estimates from using the centroid based distance, but also reduces the error variance yielding more precise estimates.

Using this regression discontinuity-based estimation approach, we find much smaller effects than those estimated in HKMM. 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 5 to 6.1 percent. This is considerably smaller than the equivalent calculation based on HKMM's estimates that implies an increase from a baseline of 5 to 10.5 percent. The point estimates when we allow for a more flexible polynomial in distance suggest a statistically and economically insignificant drop in the unemployment rate due to extending UI benefits.[4] Taken at face value, these estimates suggest a much smaller labor demand response than HKMM's results.

These smaller estimates also underscore the importance of our second concern, that UI policy effects may spill over the border and bias the estimates toward zero. As HKMM note, any strategy based on a comparison across the border will be "biased toward zero" if workers are mobile and labor markets are completely integrated at the border. For example, if UI benefit increases in one state raise the reservation wage for all workers in that local labor market, then the firms on both sides of the border may reduce vacancies. Indeed, we find 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.

Viewed from the perspective of HKMM's larger estimates, this potential attenuation bias suggests that the large effects were estimated despite the bias. The smaller estimates found in this paper call into question whether estimation strategies exploiting differences at state borders (both BPFE and geographic RD) can identify the effect of UI extensions. While focusing on smaller areas around state borders will reduce the upward endogeneity bias from the shocks that triggered UI extensions, it will simultaneously increase the attenuation bias from the treatment spillovers. As both of these effects go in the same direction- toward smaller point estimates

[4]We also find similar, mirrored results using employment rate as the outcome measure.

as we get closer to the border- it is impossible to disentangle the two without imposing strong assumptions on how they differentially evolve over space. This leaves the exact effects of UI extensions during the Great Recession an open question.

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 contrasts the BPFE and geographic RD designs; Section 3 presents the measurement error-corrected RD approach; Section 4 provides a brief description of the data sources; Section 5 presents our main estimating equations; Section 6 presents the results; Section 7 explores the possibility of treatment spillovers at the border; and Section 8 concludes.

2 Background and RD Motivation

In the absence of any extensions, UI benefits are available for a maximum of 26 weeks. The Great Recession brought about a series of UI benefit extensions that were in many ways unprecedented in the United States.[5] 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. All together, 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.

The potential negative effects of increased UI generosity on labor market outcomes through reduced search effort and fewer vacancies have been well established in the literature.[6] A number of studies investigate the effects of these expansions during the Great Recession, generally finding small effects (Valletta and Kuang, 2010; Fujita, 2011; Mazumder, 2011; Rothstein, 2011; Grubb, 2011). However, there are a number of challenges in cleanly estimating the effects, including (1) accounting for the potential endogeneity of the UI extensions, (2) capturing the effects on

[5] 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).

[6] For example, see Mortensen (1976); Solon (1979); Moffitt (1985); Katz and Meyer (1990); Meyer (1990).

both labor supply and demand, and (3) accounting for how beliefs over future benefits affect the identification of contemporaneous UI effects.

Here, we follow HKMM in focusing on the combined effect of UI extensions on both labor supply and demand, point (2) above. In particular, HKMM note that studies that focus on the labor supply response by attempting to hold all other labor market conditions fixed will miss any effect operating through a labor demand channel.[7] Regarding point (3), we also use HKMM's novel "quasi-differenced" measure of unemployment and employment to account for expectations over future benefits in order to isolate the effect of current benefits.[8]

The focus of the current paper will be on the first issue: 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 run the risk of attributing the deteriorated labor market conditions to the UI differences, rather than the economic shocks that triggered them. The result will be an upward bias in the estimated magnitude of UI effects.[9]

HKMM attempt to address the endogeneity concern by adapting the BPFE approach used by Holmes (1998) and Dube, Lester, and Reich (2010). The BPFE approach focuses on contiguous counties at state borders that, due to being in different states, are subject to different UI regimes. The BPFE approach tries to control for confounding factors by including fixed effects for each county pair.[10] Identification is based on the assumption that unobserved factors driving labor market outcomes, including the economic shocks that triggered UI extensions, are on average constant within county pairs. Conditional on these pair-level factors, the UI benefit

[7]Specifically, if increasing UI benefits raises wage demands, then it will lower the value of a filled job for firms and lead them to post fewer vacancies.

[8]HKMM note that regressing log unemployment on UI benefits does 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 will 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. This allows for the effect of current benefits on current unemployment to be identified empirically.

[9]This argument is similar to that made by Card and Levine (2000) regarding the literature on UI disincentive effects. They use a benefit expansion in New Jersey to isolate the labor supply disincentive effect, finding that the disincentive effect is still significant, but much smaller than found in the previous studies.

[10]In practice 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 using the interactive effects framework from Bai (2009) to control for further unobserved heterogeneity. In replicating HKMM's results we find that the difference between using the interactive effects framework or more standard additive fixed effects is small. We therefore focus our discussion on the more common additive fixed effects framework. See Appendix B for a detailed discussion of HKMM's estimation strategy.

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 (industry structure, labor productivity, agglomeration effects, population, exposure to specific economic shocks, etc.), but happen to operate under different UI regimes by chance. For very small geographic areas close to the boundary separating the two UI regimes, we might suspect that these factors are more-or-less balanced and within-pair variation in outcomes identifies the policy effect.

Given the substantial heterogeneity in county size and economic makeup, one might be concerned that this assumption is violated (Hall, 2013). For instance, if the population centers of the two counties in a pair are far from each other, the impact of an economic shock that triggered the UI extension in one state and decays over space may affect the two counties differently. In this sense, our concern with controlling for average pair-level factors is that there may yet be within pair heterogeneity in the response to economic shocks that evolves over space and is not captured by the BPFE approach. Importantly, this concern is not simply about within-pair heterogeneity. Rather, it is about the possibility of heterogeneity within and across counties in the potentially large geographic area defined by the set of border counties.

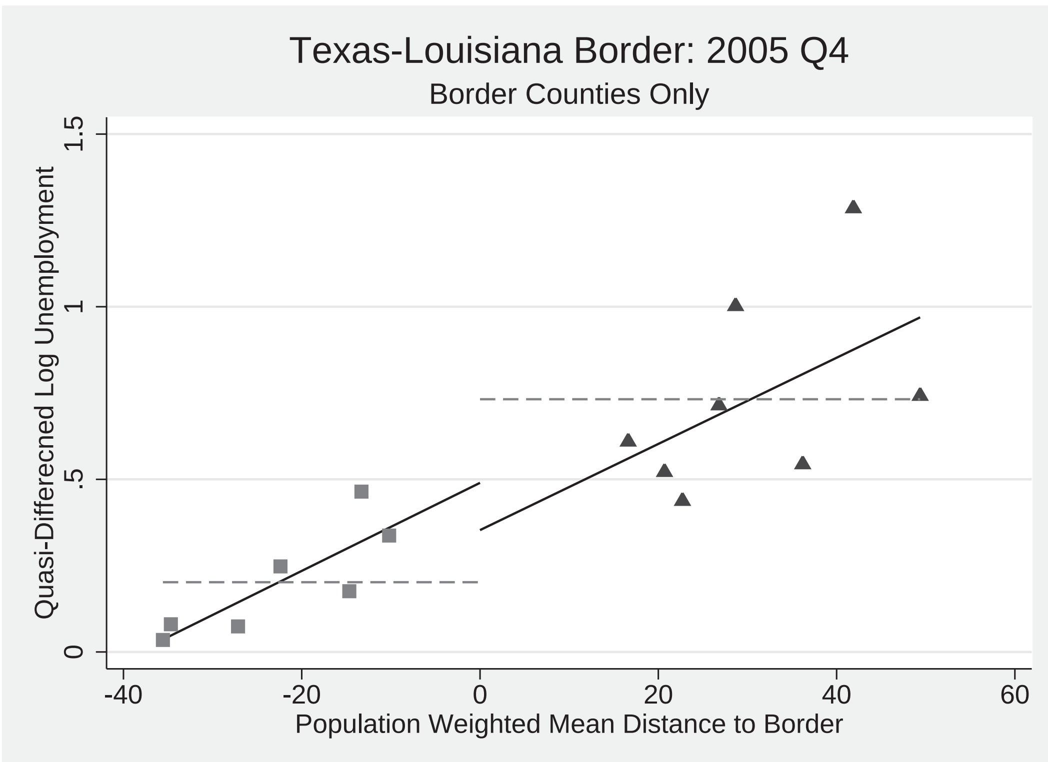
Given this concern, we propose using an alternative identification strategy based on the exact same thought experiment. Specifically, we wish to use the UI policy discontinuity at state boundaries within a geographic RD framework to identify the effect of UI benefit extensions on unemployment.[11] While the BPFE and geographic RD approaches are based on the same experimental ideal, they differ in how they control for the unobservable factors driving unemployment. Geographic RD attempts to estimate the difference in unemployment at the boundary between the two UI policy regimes, rather than average over a larger geographic area as in the BPFE approach. To do so, the geographic RD approach controls for the distance to the state boundary (policy discontinuity) in order to capture how unobservable factors evolve on average as we approach the boundary. It seems plausible that a comparison right at the boundary better balances the effects of economic shocks, and better matches the experimental ideal that both approaches attempt to approximate. Of course, for sufficiently small geographic areas the distinction between the two approaches should not matter.

To motivate the potential importance of controlling for distance to the border, Figure 2.1

[11] 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.

plots the county-level quasi-differenced log unemployment rate by the population-weighted mean distance to the border in kilometers for counties on the Texas-Louisiana border in the fourth quarter of 2005.[12] In 2005 Q4, Louisiana had a longer UI benefit duration (35 weeks) than Texas (26 weeks). The average difference across the border in this outcome (gray dashed line) is large and consistent with increased unemployment as a result of UI benefit extensions. However, fitting a linear trend in population-weighted mean distance to the border on either side of the boundary leads to a much smaller gap of opposite sign and suggests that distance is an important consideration even when the analysis is restricted to border counties.

Figure 2.1: Quasi-Differenced Log Unemployment at the Texas-Louisiana Border



Squares represent averages in Texas counties, triangles represent averages in Louisiana parishes. Dashed lines represent state averages of county means, while solid lines represent linear regressions fitted on county-level data separately for each state.

While Figure 2.1 pertains only to one set of border counties, at the very least 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, RD simply represents an alternative reduced form way of capturing the effect of other unobservable factors that evolve

[12]In Section 3, we show that the appropriate way to control for distance when using county-level measures will be to use moments of the within county population distribution relative to the border. This corresponds to using the population-weighted mean distance in Figure 2.1.

over geographic areas.

3 Measurement Error Corrected Geographic RD

A key issue in the current setting is that aggregate measures of labor market outcomes available make directly applying 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 treating unobservables as fixed within each pair. By confronting the aggregation issues we are relaxing this restriction.

To start, consider the basic RD setup where we are interested in estimating the average treatment effect (τ) of some program or policy where treatment is determined by a continuous "running variable" passing 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:

$$\tau = \lim_{h \downarrow 0} E\left[y_i | x_i = h\right] - \lim_{h \uparrow 0} E\left[y_i | x_i = h\right] \tag{3.1}$$

where h denotes the bandwidth around the cutoff.

In our setting we would like to use distance to the state border separating UI regimes as the running variable in an RD setup in order 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.[13]

Aggregation presents two problems for directly applying the RD approach. First, we will have a single measure of distance to the border for all areas in a county, regardless of their true distance. This introduces measurement error in the running variable. In addition, we only observe the aggregate outcome and do not know how it evolves within a county. In particular, we do not know if the county-level measure of unemployment accurately describes the state of the labor market at any particular distance from the border within the county.

[13] Note that the aggregation allows us to capture effects for a labor market segment and reduce the noise that is inherent in unemployment measures in small geographic areas.

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. We then provide a road map from this clean RD setup to one that can be estimated with available data to give a clear framework for thinking about the potential issues with using county-level aggregates.

We follow HKMM in using the quasi-differenced log unemployment rate to account for expectations over future benefits.[14] Generally for each county c in quarter t, the quasi-differencing is given by:

$$y_{c,t} = \ln(u_{c,t}) - \beta(1 - d_t)\ln(u_{c,t+1}) \tag{3.2}$$

where

$u_{c,t}$ is the unemployment rate for county c in period t

β is the discount rate

d is the job separation rate

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

$y_{i,c,s}$: quasi-differenced 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 quasi-differenced log 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 as

[14] See Footnote 8 for an intuitive discussion of the quasi-differencing.

the following difference:[15]

$$\tau = \lim_{h \downarrow 0} E\left[y_{ics} | x = h, s = 1\right] - \lim_{h \uparrow 0} E\left[y_{ics} | x = h, s = 0\right] \tag{3.3}$$

This is our parameter of interest: the average difference across the border in the quasi-differenced log unemployment rate for economic subregions right at the boundary. Importantly, if we observed y for the units close to the boundary, it would be straightforward to estimate the expectation in this region.

3.1 Measurement Error in the Running Variable

We now consider what can be estimated when we move from our ideal RD set-up with county subregions 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 x 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 describe the conditional expectation of y by:

$$E[y|x, s] = m_s(x) \tag{3.4}$$

$$\text{where } m_s(x) \text{ is a polynomial of order } J$$

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

$$y_{i,c,s} = b_{0,s} + b_{1,s} x_{i,c,s} + b_{2,s} x_{i,c,s}^2 + \varepsilon_{i,c,s} \tag{3.5}$$

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. Therefore, we can

[15] Here we will consider the simplified case of estimating the difference between a high benefit and low-benefit state. This extends easily to our setting where we will estimate the effect of the number of weeks of UI benefits. 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.

estimate $b_{0,s}$ by weighted least squares (WLS) on:[16]

$$\overline{y}_{c,s} = b_{0,s} + b_{1,s}\mu_c^1 + b_{2,s}\mu_c^2 + \nu_{c,s} \tag{3.6}$$

Here, μ_c^r is the r^{th} 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. In practice, we use 2010 Census data at the census block level to estimate these within-county moments. Note that census blocks are very small levels of aggregation, allowing us to approximate a continuous distance measure.[17] Further note that controlling for these moments differs from the common practice of using the distance from the geographic centroid of an area as the running variable. This corresponds to a special case of the procedure in Bartalotti, Brummet, and Dieterle (2016), who show that using the moments will not only eliminate the bias present when using the centroid based distance, but will also reduce the error variance and yield more precise estimates.

3.2 Aggregation-Induced Error in the Outcome Variable

If our outcome of interest were simply the unemployment rate, we could directly estimate Equation (3.6) with the available data. However, given the nonlinearity in the quasi-differenced log unemployment measure, we do not actually observe $\overline{y}_{c,s}$, the average quasi-differenced log unemployment rate across the subregions in a county. Instead we observe $y_{c,s}$, the quasi-differenced 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 $y_{c,s} \neq \overline{y}_{c,s}$ is important because it could introduce 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. To be clear, 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

[16]Note, how we define our subregions will determine the appropriate weights. For instance, if we chose subregions so that all have the same population, $n_i = \overline{n}\,\forall i$, then the appropriate weights will be proportional to county-level population (more populous counties have more $\overline{n}$ sized regions). If instead we were to divide each county into the same number of subregions, then we would want an unweighted regression. Since it is not conceptually clear which division of the counties (and implied weights) is preferred, we will consider both unweighted regressions and regressions weighted by county population.

[17]For instance, the fifteen counties at the Texas-Louisiana border contain over 49,000 census blocks.

$\epsilon_{c,s}$ is the measurement error from aggregating the dependent variable. Plugging into Equation (3.6), our estimating equation becomes:

$$\begin{aligned} y_{c,s} - \epsilon_{c,s} &= b_{0,s} + b_{1,s}\mu_c^1 + b_{2,s}\mu_c^2 + \nu_{c,s} \\ y_{c,s} &= b_{0,s} + \epsilon_{c,s} + b_{1,s}\mu_c^1 + b_{2,s}\mu_c^2 + \nu_{c,s} \end{aligned} \tag{3.7}$$

Equation (3.7) 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:

$$\hat{\tau} = \tau + E[\tilde{\epsilon}_{c,1}] - E[\tilde{\epsilon}_{c,0}] \tag{3.8}$$

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:

$$\epsilon_{c,s,t} \approx \frac{\sigma^2_{U,c,s,t}}{2\overline{U}^2_{c,s,t}} - \frac{\sigma^2_{L,c,s,t}}{2\overline{L}^2_{c,s,t}} - \beta(1-s_t)\left[\frac{\sigma^2_{U,c,s,t+1}}{2\overline{U}^2_{c,s,t+1}} - \frac{\sigma^2_{L,c,s,t+1}}{2\overline{L}^2_{c,s,t+1}}\right] \tag{3.9}$$

where $\sigma^2_{U,c,s,t}$ and $\sigma^2_{L,c,s,t}$ are the variances of unemployed and labor force counts, respectively, across the subregions within a county and $\overline{U}_{c,s,t}$ and $\overline{L}_{c,s,t}$ are the mean unemployed and labor force counts for the subregions within a county. From Equation (3.9), it is clear that the measurement error is larger when there is more within-county variation in labor market outcomes across subregions and when, due to the quasi-differencing, the degree of this variation is changing over time.

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 0.0006 to 0.0046 (in absolute value), which is only 1 to 7.6 percent of the baseline estimate in HKMM and suggests that this error is not a major source of bias in this setting. Once again, this is important because it suggests that the difference between what we can estimate with aggregate data and what we would estimate with ideal data is not large.[18] Therefore, by controlling for the distance moments we have some confidence that we are approximating the ideal, but infeasible, RD in Equation (3.5).

4 Data

In our analysis, we consider the period from 2005 to the end of 2011. The data for our main outcomes of interest, unemployment and employment rates, come from the Local Area Unemployment Statistics (LAUS) compiled by the Bureau of Labor Statistics (BLS).[19] The LAUS provide estimates of county-by-month employment and unemployment counts. We aggregate this data to the quarterly level to match HKMM. To calculate the quasi-differenced rates, we use the monthly separation rate from the BLS's Job Openings and Labor Turnover Survey (JOLTS).

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.[20] 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, note that we also use the county-level 2010 Census population counts as the weights when we estimate by WLS.

[18] Interestingly, this result seems to be driven by the stability over time in the variability of outcomes within county. When not quasi-differencing (i.e., subtracting discounted future values), the implied bias is larger.

[19] In order to tie our results as closely to those in HKMM, we match their data sources as closely as possible. Note that HKMM use employment rates derived from the US Census Bureau's Quarterly Workforce Indicators (QWI). For reasons discussed in Appendix B, we choose to use the LAUS employment numbers due to difficulties in replicating HKMM's employment results using the QWI. Despite relying on different data sources we find very similar effects on the employment rate in our replication exercise.

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

5 Main Specifications

Our application differs from the basic RD setup in that we have multiple discontinuities across time and state boundaries. Intuitively, we have a separate geographic RD for each state-boundary in each quarter when the two states have different UI benefits.[21] We treat these as separate RD designs that we pool together, and include boundary-by-quarter fixed effects in order to account for the mean differences across these separate RD designs. In our baseline RD setup we do not control for distance to a border:[22]

$$y_{c,s,g,t} = \pi + \gamma b_{s,g,t} + \delta_{g,t} + \rho_{c,s,g,t} \tag{5.1}$$

where c indexes counties, s indexes states,

g indexes state boundaries, and t indexes quarters

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

If the data were disaggregated at a small enough level, we could use this expression and simply shrink the bandwidth around the cutoff, focusing only on subregions right at the border. However, this is not possible since we are limited to using county aggregate data, which introduces the measurement error problems discussed previously. Hence, we adopt a parametric model of distance to the border to estimate the expected level of the quasi-differenced log unemployment at each state boundary. The approach described in Section 3.1 overcomes the measurement error in the running variable, and controls for distance to the border by including uncentered moments of the distance to the border over the population distribution in each county. This

[21] See Keele and Titiunik (2014) and Cattaneo et al. (2015) for a discussion on interpreting the estimates with heterogeneous responses.

[22] In Appendix B we discuss how this approach relates to HKMM's BPFE regressions. Intuitively, not controlling for distance in the RD is akin to not allowing for within county heterogeneity in the BPFE approach. Here it is worth noting that our RD estimates not controlling for distance are quite close to HKMM's estimates.

leads to the following regression:

$$\begin{aligned} y_{c,s,g,t} =& \pi + \gamma b_{s,g,t} + \delta_{g,t} \\ &+ \mathbf{D}_{g,t} \left[(1 - T_{s,g,t}) \left(\sum_{r=1}^{R_{0,g,t}} \theta^0_{g,t} \mu^r_c \right) + (T_{s,g,t}) \left(\sum_{r=1}^{R_{1,g,t}} \theta^1_{g,t} \mu^r_c \right) \right] + u_{c,s,g,t} \end{aligned} \tag{5.2}$$

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

μ^r_c is the r^{th} 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 simply allows for different polynomial orders and for the effect of distance to differ across state-boundary-quarter groups.[23] We obtain μ^r_c from census block-level population counts from the 2010 US Census. We first calculate the distance from the center of that census block to the nearest state boundary.[24] Note that for counties close to several state borders, not all census blocks in the county share the same nearest neighbor state.[25] 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 calculate the necessary population-weighted uncentered distance moments.

6 RD Results

Tables 6.1 and 6.2 display our main RD results for both unemployment and employment outcomes using the same set of border counties as HKMM. For comparison, we also report a set of BPFE estimates from Appendix B. In each case we present results both with and without population weights. 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 actual average duration and increase in benefits and one assuming a permanent

[23]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.

[24]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 led to problematic rounding errors in identifying the center of a census block.

[25]Fortunately, this affects less than 7 percent of census blocks within border counties.

change to the maximum benefit duration. For the RD estimates, we cluster standard errors at the state-by-boundary level as this is the level at which treatment varies.[26]

Starting in Table 6.1, we see that both our weighted and unweighted regressions when we do not control for distance to the border (Polynomial Order 0) yield results very similar to the BPFE estimates from Appendix B. In both cases, the implied unemployment rates from the policy counterfactuals are quite close to the corresponding estimates from HKMM of 8.6 percent and 10.5 percent. While the two approaches are not perfectly nested (see Appendix B), conceptually both BPFE and RD without the distance controls would be biased if the shocks that triggered UI extensions evolve over space within the set of border counties. By controlling for distance, we can account for this potential problem. Controlling for a linear function in distance (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. For the population-weighted regressions, the implied unemployment rate starting from a baseline of 5 percent for the two policy counterfactuals drops from 9.0 percent to 6.1 percent and 11.2 percent to 6.7 percent.

We also estimate the treatment effect 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 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.[27] 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 population weighted point estimate falls to a statistically insignificant -0.0107.

The employment rate regressions in Table 6.2 follow a similar pattern with estimates similar to our BPFE estimates and HKMM's results when not controlling for distance and much smaller estimates when we do control for distance. Taken at face value, the results in Tables 6.1 and 6.2 suggest much smaller effects of UI extensions on unemployment and employment during the Great Recession than found in HKMM.

[26] For example, at the Texas-Louisiana border we cluster the counties in Texas and the counties in Louisiana into two separate groups. This clustering accounts for both contemporaneous spatial correlation in the errors as well as serial correlation over time.

[27] Across all state-boundary-by-states-by-quarters, the largest polynomial suggested by this procedure is a cubic.

Table 6.1

Unemployment regression discontinuity and BPFE Results				
		RD Polynomial Order		
Unweighted	*BPFE*	*0*	*1*	*AICc*
Coefficient	0.0521***	0.0465***	0.0245	0.0143
Standard Error	(0.007)	(0.0109)	(0.0185)	(0.0182)
	Implied Unemployment Rate from Base of 5%			
Actual Duration and Benefits	8.0%	7.6%	6.2%	5.7%
95% CI	[6.9%, 9.0%]	[6.1%, 9.0%]	[4.2%, 8.2%]	[3.9%, 7.5%]
Permanent and Max Benefits	9.5%	8.8%	6.8%	6.0%
95% CI	[7.7%, 11.2%]	[6.5%, 11.2%]	[3.7%, 9.8%]	[3.3%, 8.6%]
Observations	32,972	8,446	8,446	8,446
		RD Polynomial Order		
Population Weighted	*BPFE*	*0*	*1*	*AICc*
Coefficient	0.0604***	0.0660***	0.0238	-0.0107
Standard error	(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%
95% CI	[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%
95% CI	[8.2%, 12.7%]	[8.1%, 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: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.
Data Sources: BLS LAUS, TIGER geographic shapefiles, EUC and EB trigger reports.

7 Interpreting our Estimates: No Effect of UI Extensions?

Applying our measurement error-corrected RD approach, we find little evidence of a jump in the unemployment or employment rates when moving across the border from a low benefit to a high-benefit state. The key question then becomes whether these results indicate that there is little or no effect from extending UI benefits on these labor market outcomes.

As noted by HKMM, an identification strategy based on policy differences across borders will be "biased toward zero" when labor markets are completely integrated and workers are fully mobile. Intuitively, with fully mobile workers an increase in UI benefits in one state may increase the wage demands for all workers in that market. All firms hiring in that market, regardless of the state they are located in, will therefore see a reduction in the value of a filled job leading to fewer vacancies created on both sides of the border. Such a story would be consistent with an increase in workers from a low-benefit state directing their search in the high-benefit state while workers in the high-benefit state would search less in the low-benefit state. Note that we would expect the change in worker search to go in the opposite direction if due to either the

Table 6.2

Employment regression discontinuity and BPFE Results				
			Polynomial Order	
Unweighted	*BPFE*	*0*	*1*	AICc
Coefficient	-0.0043***	-0.0031***	-0.0022	-0.0011
Standard Error	(0.001)	(0.0010)	(0.0018)	(0.0020)
	Implied Employment Rate from Base of 95%			
Actual Duration and Benefits	91.4%	92.3%	93.1%	94.1%
95% CI	[90.5%, 92.4%]	[90.8%, 93.9%]	[90.2%, 96.0%]	[90.8%, 97.3%]
Permanent and Max Benefits	90.1%	91.4%	92.4%	93.7%
95% CI	[88.9%, 91.4%]	[89.2%, 93.5%]	[88.4%, 96.4%]	[89.3%, 98.2%]
Observations	32,125	8,264	8,264	8,264
			Polynomial Order	
Population Weighted	*BPFE*	*0*	*1*	AICc
Coefficient	-0.0053***	-0.0030**	-0.0014	0.0011
Standard Error	(0.001)	(0.0014)	(0.0023)	(0.0026)
	Implied Employment Rate from Base of 95%			
Actual Duration and Benefits	90.6%	92.5%	93.8%	96.0%
95% CI	[89.5%, 91.8%]	[90.3%, 94.7%]	[90.1%, 97.6%]	[91.6%, 100.0%]
Permanent and Max Benefits	89.1	91.6%	93.4%	96.4%
95% CI	[87.5%, 90.6%]	[88.6%, 94.6%]	[88.3%, 98.5%]	[90.2%, 102.4%]
Observations	32,125	8,264	8,264	8,264

Standard errors clustered at the state-by-Boundary level for RD and pair level for BPFE in parentheses: * p<0.1, ** p<0.05, *** p<0.01.
Data Sources: BLS LAUS, TIGER geographic shapefiles, EUC and EB trigger reports.

UI differences or the shocks that triggered them, the contraction of vacancies was larger in the high-benefit state. In that case, people may direct their search in the low-benefit state to take advantage of the higher job finding rate due to less rationing of jobs.

Importantly, HKMM's finding of large effects of UI extensions on unemployment when not accounting for distance to the border led this potential source of bias to be treated as a second-order concern. Rather, the majority of their sensitivity analyses were directed at ruling out other non-UI driven explanations for such a large effect. By accounting for distance and finding much smaller effects, our approach changes the focus of discussion.

To explore the possibility of workers in one state altering their job search patterns in response to UI benefits in neighboring states, we use 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. 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.[28]

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.[29] We first identify 57 state boundaries where when there is a difference between the two states in UI benefit duration, one state is always the high-benefit state and one is always the low-benefit state. This sample selection helps make two clear "treatment" groups, avoiding difficulties in modeling the dynamic changes in job search 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.

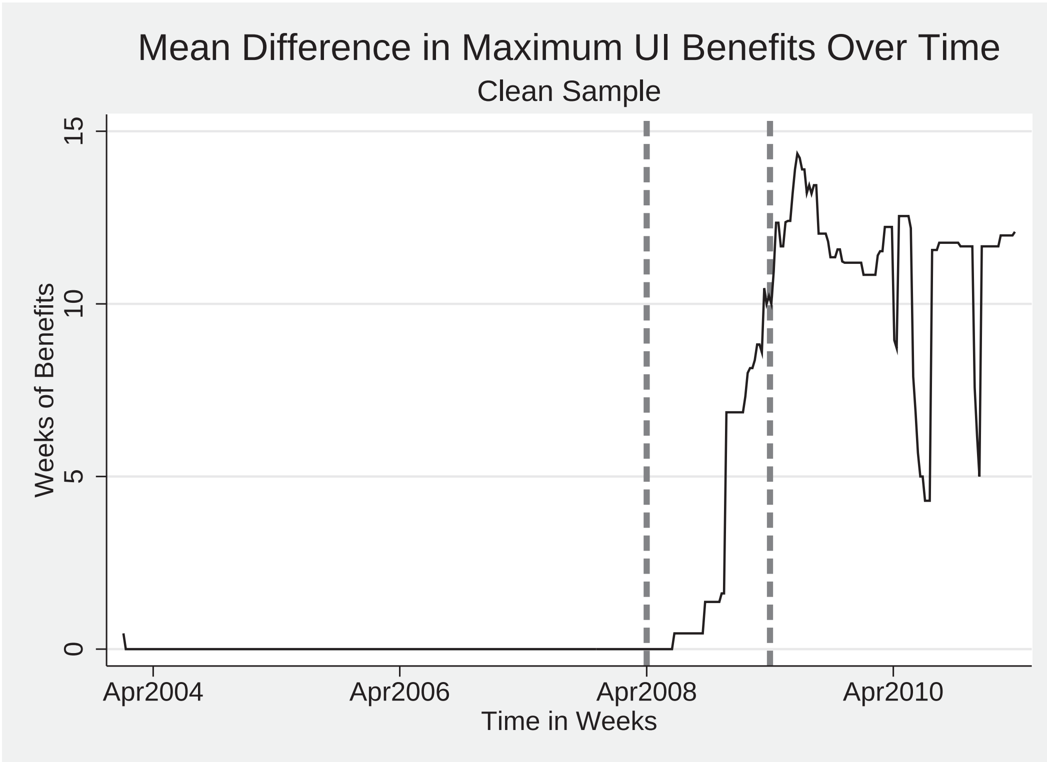
Figure 7.1 depicts the mean UI differences over time for our high and low-benefit states. Since the LODES employment data we use is based on records from the beginning of the second quarter in each year, we label the axis at April of each year. The vertical dashed lines mark the beginning of April 2008 and the beginning of April 2009. Note that prior to April 2008, there were no benefit differences for the previous four years for 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. Again, these counts are based on information from the beginning of the second quarter of each year. In Figure 7.2 we display trends in the average fraction working across the border by year for census blocks within

[28] As a caveat, the LODES data are subject to an imputation and "fuzzification" process in order to preserve anonymity in small areas. 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 "fuzzification" process for data protection at small levels of aggregation is done so that 90 percent of "home to work flows" will not be "coarsened above the census tract level." 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.

[29] In Appendix C we also present the results from census block fixed effects regressions using the full sample, which provide similar results.

Figure 7.1



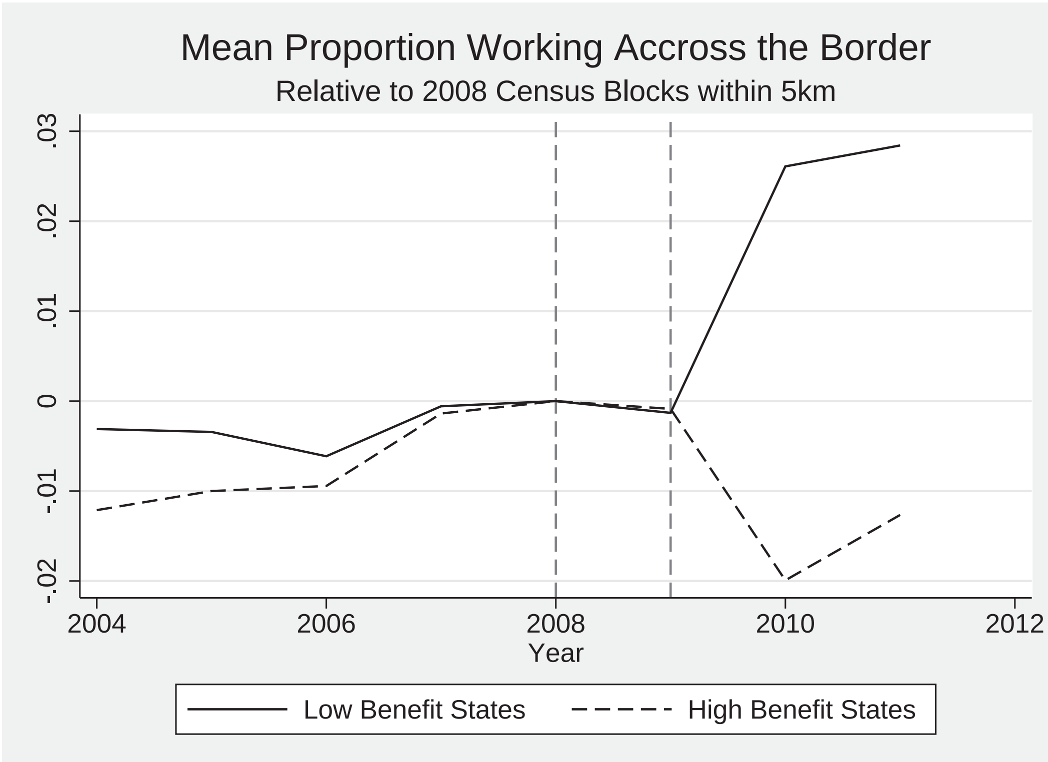
Source: EB and EUC trigger reports. Average difference in available benefits between states identified as always high and always low benefit states at a given boundary by week.

5km in our two groups.[30] 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 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 nontrivial.

Table 7.1 supports the results from Figure 7.2 by presenting the difference between the two groups relative to 2008 by year, effectively providing Difference-in-Difference type estimates. For example, the first row presents the difference between 2004 and 2008 for the high-benefit states minus the same difference for the low-benefit states. In this setting, standard error

[30]Similar results are found when using 1km and 10km bandwidths.

Figure 7.2



Source: LODES. 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.

estimates are sensitive to assumptions regarding the structure of spatial correlation in the error term. We therefore present three sets of standard errors that allow for (1) serial correlation for a census block over time (Census Block Clustering), (2) serial and spatial correlation within census tracts (Census Tract Clustering), and (3) serial and spatial correlation within counties (County Clustering). When allowing for correlation among census blocks in the same county, the relative increase of 4.6 percentage points for the low-benefit states from 2008, the last pre-UI difference year, to 2010, the first year after extended UI differences, is statistically different from zero at the 10 percent level (p-value of 0.069). Furthermore, 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 0.0157 when clustering at the county level. 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.

This evidence suggests that workers in one state may respond to UI benefit changes in the neighboring state, thus changing reservation wages for all workers living near the border

Table 7.1

Proportion Working Across the Border: Difference-in-Difference Estimates
Census Blocks within 5km of Border: Relative to 2008

	Cluster:	*Census Block*		*Census Tract*		*County*	
Year	*Coef.*	*Std. Err.*	*p-value*	*Std. Err.*	*p-value*	*Std. Err.*	*p-value*
2004	-0.0090	(0.0013)	0.000	(0.0021)	0.000	(0.0049)	0.068
2005	-0.0066	(0.0013)	0.000	(0.0021)	0.002	(0.0044)	0.137
2006	-0.0033	(0.0013)	0.012	(0.0020)	0.103	(0.0040)	0.411
2007	-0.0008	(0.0013)	0.544	(0.0018)	0.657	(0.0030)	0.786
2008	-	-	-	-	-	-	-
2009	0.0004	(0.0013)	0.740	(0.0019)	0.816	(0.0031)	0.888
2010	-0.0460	(0.0014)	0.000	(0.0035)	0.000	(0.0252)	0.069
2011	-0.0411	(0.0015)	0.000	(0.0039)	0.000	(0.0268)	0.127

Data Sources: LODES, TIGER geographic shapefiles, EUC and EB trigger reports.

regardless of state of residence.[31] Knowing this, firms in both the high and low-benefit states may reduce vacancies. Therefore, our finding of no jump in unemployment from our measurement error-corrected RD may not reflect the true effect of UI extensions due to treatment spillovers at the border. Again, these changes in worker search go in the opposite direction of what may be expected if firms in high-benefit states actually reduced vacancies more than those in the low-benefit state. This case would also be consistent with finding no jump in unemployment but a jump in employment in our main RD results. The fact that we find no jump in either is consistent with our findings here of increased search in the high-benefit state increasing wage demands and reducing vacancies in both states. Importantly, these current results suggest that considerable care is warranted when trying to exploit the differences in labor market institutions at state borders to identify policy effects. In this particular case, it casts doubt on the large estimated effects found in HKMM and leaves the macro effects of UI extensions as an unanswered question.

8 Conclusion

Increases in UI benefits formed a central part of the policy response to the economic turmoil of the Great Recession, but these increases in UI benefit duration may have deleterious effects on employment. Identifying these effects is difficult due to policy endogeneity issues, however. In this paper, we analyze a recent strategy by Hagedorn et al. (2015) that uses differences in UI

[31] Note that this potential individual labor supply response is of a different nature than was studied in the previous literature. Namely, here we have evidence that people choose new employment opportunities based on expectations over future benefits, whereas the prior work focused on how current benefits affected current job search. To explore this response more fully would require more targeted data on the job search process and subsequent outcomes.

benefit generosity across counties at state borders to identify the effects of UI benefit duration on unemployment. This procedure relies on the comparability of counties across the borders, which might be too strict if one considers that economic shocks and local characteristics that impact labor markets evolve across space. Our results show that accounting for how unobservables evolve across space using a measurement error-corrected RD produces significantly smaller effect estimates. On the surface, our estimates suggest negligible UI benefit effects. However, we also show that these smaller estimates may be driven by previously undocumented treatment spillovers with workers close to state boundaries targeting their employment search in the high benefit states.

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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 quasi-differenced unemployment rate, Equation (3.2), we have:

$$
\begin{aligned}
y_{c,s,t} &= \ln\left(\frac{U_{c,s,t}}{L_{c,s,t}}\right) - \beta(1-s_t)\ln\left(\frac{U_{c,s,t+1}}{L_{c,s,t+1}}\right) \\
&= \ln\left(\overline{U}_{c,s,t}\right) - \ln\left(\overline{L}_{c,s,t}\right) - \beta(1-s_t)\left[\ln\left(\overline{U}_{c,s,t+1}\right) - \ln\left(\overline{L}_{c,s,t+1}\right)\right] \qquad \text{(A.1)}
\end{aligned}
$$

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 quasi-differenced 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 quasi-differenced unemployment rates at the county-level as follows:

$$
\overline{y}_{c,s,t} = \overline{\ln(U)}_{c,s,t} - \overline{\ln(L)}_{c,s,t} - \beta(1-s_t)\left[\overline{\ln(U)}_{c,s,t+1} - \overline{\ln(L)}_{c,s,t+1}\right] \qquad \text{(A.2)}
$$

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

$$
\begin{aligned}
\overline{y}_{c,s,t} \approx\ & \ln\left(\overline{U}_{c,s,t}\right) - \frac{\sigma^2_{U,c,s,t}}{2\overline{U}^2_{c,s,t}} - \ln\left(\overline{L}_{c,s,t}\right) - \frac{\sigma^2_{L,c,s,t}}{2\overline{L}^2_{c,s,t}} \\
& - \beta(1-s_t) \quad \ln\left(\overline{U}_{c,s,t+1}\right) - \frac{\sigma^2_{U,c,s,t+1}}{2\overline{U}^2_{c,s,t+1}} - \ln\left(\overline{L}_{c,s,t+1}\right) - \frac{\sigma^2_{L,c,s,t+1}}{2\overline{L}^2_{c,s,t+1}} \qquad \text{(A.3)}
\end{aligned}
$$

Plugging this into our expression for the aggregation error yields:

$$\begin{aligned}\epsilon_{c,s,t} &= y_{c,s,t} - \overline{y}_{c,s,t} \\ &\approx \frac{\sigma^2_{U,c,s,t}}{2\overline{U}^2_{c,s,t}} - \frac{\sigma^2_{L,c,s,t}}{2\overline{L}^2_{c,s,t}} - \beta(1-s_t)\left(\frac{\sigma^2_{U,c,s,t+1}}{2\overline{U}^2_{c,s,t+1}} - \frac{\sigma^2_{L,c,s,t+1}}{2\overline{L}^2_{c,s,t+1}}\right)\end{aligned} \tag{A.4}$$

Recall the bias term for our simplified case with only two states in one time period in Equation (3.8) 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, 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 quasi-difference log unemployment rate may be a poor measure of the actual variable. However, if this within-county heterogeneity is fairly stable over time, then the quasi-differencing will reduce the bias.

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.[32] We readily note that this yearly data may deviate from the quarterly data used in our analysis and that using census tract level data may yield noisy measures of unemployment and labor force counts. 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 the 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, to be a concern for our RD estimates, this aggregation error must be systematically different for counties in high- versus low-benefit states. When

[32] 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`

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 5, replacing the county-level quasi-differenced 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. For both weighted and unweighted regressions across specifications with different controls for distance the estimated bias (in absolute value) ranges from 0.0006 to 0.0046. As this is only 1 to 7.6 percent of the original HKMM estimates, it suggests that aggregation error in the dependent variable is not a major concern in this setting. Note, that this is due in large part to the quasi-differencing (i.e., subtracting discounted future values). Using just the contemporaneous log unemployment rate leads to much larger aggregation bias terms, suggesting that this particular form of measurement error may prove important in many other settings.

B HKMM Replication

HKMM's baseline specification is written as

$$\Delta y_{p,t} = \alpha \Delta b_{p,t} + \Delta \epsilon_{p,t} \tag{B.1}$$

where p indexes county pairs

t indexes quarters

where $\Delta y_{p,t}$ is the difference between two border counties in a given quarter in what HKMM call the quasi-differenced log unemployment rate (or employment rate) and $\Delta b_{p,t}$ is the corresponding

Table A.1

Aggregation Bias RD Estimates			
	Distance Polynomial Order		
	0	*1*	*AICc*
Unweighted	-0.0006	0.0040	0.0046*
	(0.0009)	(0.0024)	(0.0026)
Population Weighted	-0.0006	0.0033	0.0023
	(0.0008)	(0.0020)	(0.0017)

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 * p<0.1.

difference in log unemployment benefit duration. The quasi-differencing is given by the following:

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

where

u_t is the unemployment rate

β is the discount rate

s is the job separation rate

HKMM estimate this using the interactive effects framework from Bai (2009) setting $\Delta\epsilon_{p,t} = \lambda_p' F_t + \nu_{p,t}$ yielding the following estimating equation:

$$\Delta y_{p,t} = \alpha\Delta b_{p,t} + \lambda_p' F_t + \nu_{p,t} \tag{B.2}$$

where λ_p is a vector of factor loadings and F_t is a vector of factors specific to a time period. HKMM note that this approach "incorporates additive time and county-pair fixed effects as special cases." We therefore consider an additional estimating equation:

$$\Delta y_{p,t} = \alpha\Delta b_{p,t} + \psi_p + \phi_t + v_{p,t} \tag{B.3}$$

By estimating Equations (B.2) and (B.3) we obtain an understanding of the sensitivity of the results to specification of the structure of unobservables.

A key consideration in replicating HKMM is matching their sample as closely as possible. In particular HKMM use unemployment data from the Local Area Unemployment Statistics (LAUS) and employment data from the Quarterly Workforce Indicators (QWI). The QWI does not have universal coverage of all counties. For both sets of regressions (unemployment and employment) HKMM keep only those counties with data on both outcomes.[33] This leaves 1,107 county-pairs. Our replications are based on pooled data for 2005 Q1-2011 Q4. We also run regressions using the full set of county-pairs with unemployment data (1178 county-pairs), not imposing the restrictions based on QWI data availability..

Table B.1 shows HKMM's baseline results and our replication for the unemployment regressions. In order to interpret the estimates, HKMM use their model to calculate the implied change in the unemployment rate starting from a base rate of 5 percent. They consider two sets of parameters: one based on the actual duration of extended benefits during the recession (16 quarters) and the average benefits increase (26 weeks to 82.5 weeks), and a second based on a permanent change to the maximum benefit length (26 to 99 weeks). HKMM's estimate corresponds to an unemployment rate of 8.6 percent based on case one and 10.5 percent with case two. Given that the unemployment rate during the great recession went from 5 percent to about 10 percent (Bureau of Labor Statistics), this represents a large increase due purely to benefits extension.

In Replication (1), we use the QWI based sample restrictions and estimate Equation (B.2) with interactive effects.[34] This matches the reported estimation approach in HKMM. We set the dimension of the interactive effects to be two, matching HKMM as well. Here, we estimate a smaller effect of unemployment benefit duration with implied unemployment rates in the two policy counterfactuals of 7.0 percent and 7.9 percent. In Replication (2), we remove the QWI sample restrictions and obtain a nearly identical estimate. Given this, we proceed to use the full sample in subsequent regressions. For Replication (3), we estimate Equation (B.3) with only additive pair and quarter fixed effects. Interestingly, this estimate is closer to the original HKMM result with implied counterfactual unemployment rates of 8.0 percent and 9.5 percent. Finally, motivated by our RD approach in Section 3, in Replication (4) we also estimate Equation (B.3) weighting by county-pair population. Here, our point estimate is very close to that reported in HKMM implying nearly identical policy effects of 8.6 percent and 10.5 percent.

HKMM also report estimates using the employment rate. There are two main reasons to

[33]The one exception is South Carolina, which has missing counties in the QWI, however to match the sample size in HKMM we include these counties in the unemployment regressions.

[34]We estimate this using the -phtt- package in R.

Table B.1

HKMM Unemployment Replication					
				Replications	
	HKMM	(1)	(2)	(3)	(4)
Coefficient	0.0602***	0.0377***	0.0379***	0.0521***	0.0604***
SE	(p=0.000)	(0.005)	(0.005)	(0.007)	(0.009)
	Implied Unemployment Rate from Base of 5%				
Actual Duration and Benefits	8.6%	7.0%	7.0%	8.0%	8.6%
95% CI				[6.9%, 9.0%]	[7.2%, 9.9%]
Permanent and Maximum Benefits	10.5%	7.9%	8.0%	9.5%	10.5%
95% CI				[7.7%, 11.2%]	[8.2%, 12.7%]
Additive Fixed Effects	No	No	No	Yes	Yes
Interactive Effects	Yes	Yes	Yes	No	No
QWI Sample Restrictions	Yes	Yes	No	No	No
Population Weighted	No	No	No	No	Yes
County Pairs	1,107	1,107	1,178	1,178	1,178
Observations	30,998	30,984	32,972	32,972	32,972

Replications: Standard errors clustered at the County Pair level in parentheses: * p<0.1, ** p<0.05, *** p<0.01.

consider the employment rates in addition to, or instead of, the unemployment rates. First, unemployment rates calculated at small levels of aggregation may be subject to considerable sampling noise. Second, Solon (1979) notes the possibility that individuals may report searching for work despite having no intention of taking a job in order to receive unemployment benefits. Table B.2 displays the employment regression results. Importantly, HKMM use employment data from the QWI. However, the QWI only include employment counts and no measure of the labor force. Using the LAUS labor force numbers as the denominator leads to many implausible employment rates. We choose instead to use the employment numbers from LAUS which are internally consistent and apply the same restriction as HKMM by dropping cases with quarter-to-quarter changes in employment of greater than 15 percent.

Despite using a different data source, our employment results match HKMM closely. As before, they consider the same two counterfactuals, now starting from a base employment rate of 95 percent. HKMM's estimate implies employment rates of 92.1 percent and 91.1 percent. Replication (1), with interactive effects yields estimates of 92.9 percent and 92.1 percent. The fixed effects regression in replication (2) increases the magnitude of the estimates, implying employment rates of 91.4 percent and 90.1 percent. Finally, the additive fixed effects weighted by pair population gives the largest effect with counterfactual employment rate estimates of 90.6 percent and 89.1 percent.

Table B.2

HKMM Employment Replication				
			Replications	
	HKMM	(1)	(2)	(3)
Coefficient	-0.0035*	-0.0025***	-0.0043***	-0.0053***
SE	(p=0.100)	(0.000)	(0.001)	(0.001)
	Implied Employment Rate from Base of 95%			
Actual Duration and Benefits	92.1%	92.9%	91.4%	90.6%
95% CI			[90.5%, 92.4%]	[89.5%, 91.8%]
Permanent and Maximum Benefits	91.1%	92.1%	90.1%	89.1%
95% CI			[88.9%, 91.4%]	[87.5%, 90.6%]
Additive Fixed Effects	No	No	Yes	Yes
Interactive Effects	Yes	Yes	No	No
Population Weighted	No	No	No	Yes
Data Source	QWI	LAUS	LAUS	LAUS
Observations	29,600	32,125	32,125	32,125

Replications: Standard errors clustered at the County Pair level in parentheses: * $p<0.1$, ** $p<0.05$, *** $p<0.01$.

B.1 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, replacing the pair-by-quarter FE with state-boundary-by-quarter FE.[35] This final change gives our baseline RD expression when not controlling for distance found in Equation (5.1).

[35] Replacing pair-by-quarter FE with state-boundary-by-quarter FE does not affect the estimates substantially.

C Working Across the Border: Census Block Fixed Effects

We start by using our full sample of census blocks by year that are within 5km of the border and estimate the following:

$$f_{i,s,g,t} = \alpha + \beta \Delta \bar{b}_{s,g,t} + \delta_i + \phi_t + \varepsilon_{i,s,g,t} \tag{C.1}$$

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

f is the fraction of workers employed in the neighboring state

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

δ_i are census block fixed effects

ϕ_t are year fixed effects

Table C.1 presents the estimates of Equation (C.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 increase relative to those across the border, we see people are less likely to work in the neighboring state. Likewise, as the benefits in your 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 (i.e., when the difference across borders is nonzero), 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, namely 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 C.1, 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

Table C.1

Effect of Relative UI Benefit Duration on the Fraction WorkingAcross 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 County level
*** p<0.01., ** p<0.05, * p<0.1.

differences become smaller as we look at further and further lags.