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ABSTRACT

We exploit a policy discontinuity at U.S. state borders to identify the effects of unemployment insurance policies on unemployment. Our estimates imply that most of the persistent increase in unemployment during the Great Recession can be accounted for by the unprecedented extensions of unemployment benefit eligibility. In contrast to the existing recent literature that mainly focused on estimating the effects of benefit duration on job search and acceptance strategies of the unemployed -- the micro effect -- we focus on measuring the general equilibrium macro effect that operates primarily through the response of job creation to unemployment benefit extensions. We find that it is the latter effect that is very important quantitatively.

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1 Introduction

Unemployment in the U.S. rose dramatically during the Great Recession and has remained at an unusually high level for a long time. The policy response involved an unprecedented extension of unemployment benefits with benefit duration rising from the usual 26 weeks to as long as 99 weeks. The motivation for this policy was to provide "income support for a vulnerable group after they have lost their jobs through no fault of their own" as well as "needed support for the fragile economy."[1]

The effectiveness of this policy response was questioned by Barro (2010) and Mulligan (2012), among others. Because unemployment benefit extensions represent an implicit tax on market work, they subsidize unemployment and discourage labor supply. This may offset some of the stimulative effect ascribed to such policies and explain the persistently high unemployment since the end of the Great Recession. Yet, careful microeconomic studies, reviewed below, have found only very small effects of unemployment benefit extensions on labor supply.

These studies, however, did not assess the possibility that extensions of unemployment benefits have a large impact on labor demand. Consider the following stylized decomposition:

$$\text{Job finding rate}_{it} \quad = \quad \underbrace{s_{it}}_{\text{search intensity}} \quad \times \quad \underbrace{f(\theta_t)}_{\text{finding rate per unit of } s} \tag{1}$$

In other words, the probability that an individual i finds a job in a given time period t depends on how hard that individual searches and how selective he is in his acceptance decisions, which is captured by the "search effort" component s_{it}. It also depends on the aggregate labor market conditions θ_t that determine how easy it is to locate jobs by expending a unit of search effort. To use an extreme example, if there are no job vacancies created by employers, $f(\theta_t) = 0$, no amount of search effort by an unemployed worker would yield a positive probability of obtaining a job.

Changes in unemployment benefit policies affect both the search intensity of unemployed workers and the aggregate job finding rate per unit of search effort through general equilibrium effects. Indeed, in the classic equilibrium search framework of Mortensen and Pissarides (1994), the primary analytical device used by economists to study the determination of unemployment, the response of unemployment to changes in benefits is mainly driven by the response of employers' decisions of whether and how many jobs to create and not by the

[1]"Unemployment Insurance Extensions and Reforms in the American Jobs Act," the report by the President's Council of Economic Advisers, the National Economic Council, the Domestic Policy Council, and the Department of Labor, December 2011.

impact on workers' job search and acceptance decisions. The logic of the model is simple. Everything else equal, extending unemployment benefits exerts an upward pressure on the equilibrium wage. This lowers the profits employers receive from filled jobs, leading to a decline in vacancy creation. Lower vacancies imply a lower job finding rate for workers, which leads to an increase in unemployment. Surprisingly, there is little direct empirical evidence on the quantitative magnitude of these effects available in the literature. We attempt to fill this gap in the literature in this paper.

Our empirical strategy exploits a policy discontinuity at state borders to identify the effects of unemployment insurance policies on unemployment. While we discuss the institutional features of the U.S. unemployment insurance system in detail below, its key property is that unemployment insurance policies are determined at the state level and apply to all locations within a state. One cannot infer the effects of benefit extensions by simply relating benefit duration to unemployment in a panel of states because of the potential policy endogeneity: it might be the states that have a large increase in unemployment that expand benefit eligibility as opposed to raises in benefits leading to higher unemployment. We show, however, that the endogeneity problem can be overcome by comparing the evolution of unemployment in counties that border each other but belong to different states.[2] Locations separated by a state border are expected to have similar labor markets due to the same geography, climate, access to transportation, agglomeration benefits, access to specialized labor and supplies, etc. Indeed, we provide direct evidence that economic shocks do not stop at the state border but evolve smoothly across borders. The key feature that sets these locations apart is the difference in policies on the two sides of the border. This policy discontinuity allows to identify its labor market implications. A fundamentally similar identification strategy was used, among others, by Holmes (1998) to identify the impact of right-to-work laws on location of manufacturing industry and by Dube, Lester, and Reich (2010) to identify the effect of minimum wage laws on earnings and employment of low-wage workers. We explicitly control for the effects of other policy changes at the state level (that could be correlated with the expansion of unemployment benefit durations) to ensure that our estimates isolate the effects of unemployment benefit extensions.

In Section 2 of the paper we extend this empirical strategy to accommodate features of the policies that we are interested in evaluating (and verify the successful performance of these extensions in the data generated by an estimated equilibrium search model in Section 5) as follows:

[2] A Map of U.S. state and county borders can be found in Appendix Figure A-2.

1. The decisions of firms to create jobs are forward looking. Thus, they might be affected not only by the existing policy but also by the expectation of possible future policy changes. We derive a quasi-difference estimator of the effect of UI policies on variables such as vacancies and unemployment that controls for the effect of expectations. Among other things, this allows us to generalize our findings and estimate the effect of a temporary or permanent change in unemployment benefit duration.

2. Our estimation is based on a panel of border counties over the period of the Great Recession. Numerous shocks and policy changes have affected the aggregate economy but their impact was likely heterogeneous across county pairs. For example, shocks to and changing regulations of the financial system, while aggregate in nature, might have had a particularly strong impact on the counties on the border of New York and New Jersey, while the auto industry bailout likely had a larger impact on counties surrounding the border between Michigan and Indiana or Ohio. Similarly, the aggregate financial crisis potentially had different impact on the states depending on their different foreclosure laws. To obtain consistent estimates of unemployment benefit extensions despite heterogeneous impacts of the aggregate shocks we follow Bai (2009) and use a flexible interactive effects model.

3. An analysis based on a comparison of border counties belonging to states with different policy regimes must account for the possibility that residents of both counties may direct their job search efforts to the county with better labor market prospects. In Section 6 we will show that these mobility decisions can be measured in the data from the observed labor market flows. The estimates reported in that section imply that individuals do not systematically change their location of employment in response to changes in unemployment benefits across states during the Great Recession. This is perhaps not surprising. Residents of the border counties face a trade-off between receiving higher wages with lower job finding probability in a county belonging to the state with higher benefit eligibility and receiving lower wages with higher job finding probability in the state with lower benefits (note that benefits depend on state of employment, and not on the state of residence). Moreover, the difference in the available duration of benefits across border counties is relatively small and may not justify larger commuting expenses. Thus, while we fully control for the response of the location of employment to changes in benefits in Section 6, this modification of the analysis turns out to be inconsequential. This leads us to work with a simpler and

more transparent specification that ignores mobility decisions in the early parts of the paper.

Following the description of the main data sources we use, in Section 4 we measure the effects of unemployment benefit extensions on unemployment. We find that unemployment rises dramatically in the border counties belonging to the states that expanded unemployment benefit duration as compared to the counties just across the state border. The quantitative magnitude of this effect is so large that our estimates imply that benefit extensions can quantitatively account for much of the unemployment dynamics following the Great Recession.

In Section 5 we assess whether the mechanisms embedded in the standard equilibrium labor market search model can provide a coherent rationalization of the large effect of unemployment benefit extensions on unemployment that we document. The data suggest an affirmative answer. Consistent with implications of the equilibrium search model, we find that border counties with longer benefit extensions have significantly higher wages, lower vacancy rates, and lower employment. The estimated magnitudes of these changes are also quantitatively consistent with the model.

Our estimate of the effect of unemployment benefit extensions on employment is based on the difference across border counties. It is desirable to be able to use the resulting coefficient to predict the effect of a nation-wide extension. A potential concern is that when some states extend benefits more than others, economic activity may reallocate to states with, say, lower benefits. This reallocation is picked up by our estimates but will be absent when the policy is changed everywhere. Our results in Sections 5 and 6 provide evidence against such a concern. First we find large negative effect of unemployment benefit extensions on employment in sectors commonly considered non-tradable and thus not subject to reallocation. Second, we find that unemployed workers do not change the strategy of which county to look for work in response to changes in benefits.

Finally, in Section 7 we briefly consider the implications of our findings for macroeconomic time-series. In particular, we summarize the results in Mitman and Rabinovich (2013), who introduced unemployment benefit extensions into the Mortensen and Pissarides (1994) model calibrated to match the effect of unemployment benefit extensions on unemployment documented in this paper. The model matches nearly perfectly the dynamics of unemployment over the last 60 years. Moreover, the extensions of unemployment benefits generate the apparent shift in the Beveridge curve after the Great Recession that was widely interpreted in the literature as a sign of increased mismatch in the labor market, see Diamond (2013) for a review.

1.1 Brief Overview of the Related Literature

We organize the discussion of the related literature on the effects of unemployment benefit extensions on unemployment around the illustrative decomposition in Equation (1). As is customary in the literature, we label the impact of benefits on the search intensity of an unemployed worker, holding aggregate conditions fixed, the "micro" effect. In contrast, the "macro" effect measures the effect of benefits on the job finding rate per unit of search effort.

1.1.1 Seminal Empirical Contributions

The empirical literature on the effects of unemployment benefit extensions is based on the seminal contributions by Moffitt (1985), Katz and Meyer (1990), Meyer (1990), and Card and Levine (2000).[3] These authors used administrative data on unemployment benefit recipients and exploited the cross-state variation in unemployment benefit extensions to measure the effect of the extensions on the hazard rate of leaving compensated unemployment.[4] These estimates were interpreted using a partial equilibrium search model as measuring how individual search efforts respond to changes in benefits holding labor market conditions constant. As these studies focused on a relatively small subsample of unemployed workers who collect benefits, and the authors could not measure the impact of benefit extensions on the search effort of those who do not receive benefits, they could not assess the impact of benefit extensions on overall unemployment.

1.1.2 Micro Effects

In recent, innovative work, Rothstein (2011) estimates the partial equilibrium effects of the unemployment benefit extensions on labor market outcomes during the Great Recession. Using data from the Current Population Survey (CPS) on individual unemployment duration, he exploits the cross-state variation in unemployment benefit extensions to identify how unemployment benefit durations impact individual search behavior. Importantly, Rothstein (2011) goes to great lengths to "absorb labor demand conditions" – that is, he controls for any changes in job creation to isolate solely the effect on worker search. For example, in one specification he uses unemployed workers who are ineligible for UI benefits as a control

[3]Krueger and Meyer (2002) provide a survey of other important contributions to this literature.

[4]While this hazard was originally interpreted as measuring transitions form unemployment into employment, such an interpretation was recently questioned by Card, Chetty, and Weber (2007).

group. If unemployment benefits have a large effect on job creation, the job finding rate of all unemployed workers would drop significantly, but comparing ineligible to eligible would only capture the difference in behavioral response of search effort between workers, not the possibly much larger macro effect. Rothstein (2011) concludes that the micro elasticity of unemployment duration to unemployment benefits is relatively small, with the estimates implying that only a small fraction of the persistent increase in unemployment after the Great Recession can be attributed to a decline in worker search effort.

In this paper, we aim to exploit the same heterogeneity in policy as in Rothstein (2011), but with the goal of identifying the labor demand or macro elasticity of unemployment benefits that was beyond the scope of his analysis. We see our work as highly complementary and helping provide the complete picture on the effect of benefit extensions.

Another recent paper, Schmieder, Von Wachter, and Bender (2012) estimates the disincentive effect of unemployment benefits over the business cycle. Using detailed administrative data from Germany they exploit a policy discontinuity based on the age of workers on the day they become unemployed. The months of unemployment benefits a worker is eligible for changes discontinuously at two age cutoffs. Using a regression discontinuity design they are able to estimate the change in the behavioral response due to increased benefit eligibility, and how this response varies with business cycle conditions. They find a small disincentive effect overall that does not vary much with business cycle conditions. However, it is important to note that they also hold constant all market-level factors, and identify only the micro elasticity.

1.1.3 Macro Effects

Starting with the pioneering work of Millard and Mortensen (1997), the evidence on the magnitude of the macro effect is predominantly based on the estimation of structural models.[5] Clearly, the firm's vacancy creation decision is based on comparing the cost of creating a job to the profits the firm expects to obtain from hiring the worker. The profit is the difference between a worker's productivity and the wage. Hagedorn and Manovskii (2008) have shown that the fluctuations in aggregate labor productivity of the magnitude observed in the data can account for the observed business cycle fluctuations of aggregate unemployment and vacancies using the Mortensen and Pissarides (1994) model. This implies that the

[5] One line of research, reviewed in e.g., Costain and Reiter (2008), has studied the effects of unemployment benefits on unemployment using cross-country regressions. While this literature typically finds much larger effects than those implied by the micro studies, these estimates are relatively hard to interpret given the endogeneity problems and heterogeneity across countries that is difficult to control for.

amount of job vacancies is highly responsive to the relatively small business cycle frequency changes in productivity. The flip side of this argument is that changes in unemployment benefit policies that affect wages can have a similar impact on profits also implying a large response of vacancies, and, as a consequence, of unemployment. The persuasiveness of these arguments depends, however, on whether one agrees with the parameter values estimated by these authors. Key among them is the flow utility obtained by unemployed workers. This parameter is difficult to measure directly but its value is crucial for the amount of amplification delivered by the search model. Our objective in this paper is to directly measure the impact of unemployment benefits on the labor market variables of interest without having to rely on the estimates of the flow utility of the unemployed and without having to fully specify the model. Our empirical strategy is, however, consistent with a fully specified model.

2 Empirical Methodology

2.1 Identification via Border Counties: Controlling for Expectations

To estimate the macroeconomic effects of unemployment insurance on a variable x_t such as vacancies or unemployment, we first estimate the effect on labor market tightness, θ_t, defined as the ratio of vacancies to unemployment, and therefore look at firms' job creation decision. In the standard Pissarides (2000) model, firms' period t profits from employing a worker are given by the difference between workers' marginal product and the wage. The wage, in turn, is affected by the generosity of unemployment benefits available to the worker. Thus, up to a log-linear approximation with respect to the two state variables of the model, firms' profits from employing a worker are given by:

$$\log(\pi_t) = \gamma_z \log(z_t) - \gamma_b \log(b_t), \tag{2}$$

where z_t is workers' productivity and b_t are benefits. γ_z and γ_b are unknown coefficients which the standard theory implies should both be positive, although we do not impose such a restriction. The value of a filled job for the firm is:

$$J_t = \pi_t + \beta(1 - s_t)E_t J_{t+1}, \tag{3}$$

where β is the discount factor, s_t is the exogenous probability that the job ends and E_t is the expectation operator using information available at time t. Free entry into vacancy posting implies that the expected cost of posting a vacancy is equal to the value of a filled job. The job creation decision is then

$$q(\theta_t)J_t = c, \tag{4}$$

where $q(\theta_t)$ is the probability to fill a vacancy and c is the the cost of maintaining a vacancy. This approximately yields

$$\log(\theta_t) = \tilde{\kappa}\log(J_t). \tag{5}$$

We now approximate $\log(J_t)$ as a function of $\log(\pi_t)$, $\log(J_{t+1})$ and an expectational error $\log(\eta_t)$ around the steady state with a constant $\pi = J(1-\beta(1-s))$, so that the previous equation reads

$$\log(\theta_t) = \tilde{\kappa}\frac{\pi}{J}\log(\pi_t) + \tilde{\kappa}\beta(1-s_t)\log(J_{t+1}) + \log(\eta_t). \tag{6}$$

Using $\pi/J = (1-\beta(1-s))$ and the job creation decision for $t+1$, $\log(\theta_{t+1}) = \tilde{\kappa}\log(J_{t+1})$, yields

$$\log(\theta_t) = \tilde{\kappa}(1-\beta(1-s))\log(\pi_t) + \beta(1-s_t)\log(\theta_{t+1}) + \log(\eta_t). \tag{7}$$

In quarterly data variables such as unemployment are well approximated by a linear function of $\log(\theta)$:[6]

$$\log(x_t) = \lambda_x \log(\theta_t), \tag{8}$$

so that we obtain the quasi-difference

$$\tilde{x}_t := \log(x_t) - \beta(1-s_t)\log(x_{t+1}) = \tilde{\kappa}\lambda_x(1-\beta(1-s))\log(\pi_t) + \lambda_x\log(\eta_t). \tag{9}$$

Now, denote by p the border-county pair. Then, substituting Equation (2) into Equation (9) and differencing between border counties within a pair yields:

$$\Delta\tilde{x}_{p,t} = \alpha\Delta b_{p,t} + \Delta\epsilon_{p,t}, \tag{10}$$

where Δ the difference operator over counties in the same pair. More specifically, if counties i and j are in the same border-county pair p, then $\Delta\tilde{x}_{p,t} = \tilde{x}_{p,i,t} - \tilde{x}_{p,j,t}$, and, with a slight abuse of notation, $\Delta b_{p,t} = \log(b_{p,i,t}) - \log(b_{p,j,t})$.

After we describe the structure of the error term $\Delta\epsilon_{p,t}$ in Section 2.2, Equation (10) can be estimated in the data to recover the coefficient of interest α, which equals, using equations (2) and (9),

$$-\gamma_b\lambda_x\tilde{\kappa}(1-\beta(1-s)). \tag{11}$$

[6]See, e.g., Hall (2005), Shimer (2007). Below we verify that this approximation also performs well in a calibrated equilibrium search model with unemployment benefit extensions.

Dividing this coefficient by the measurable factor $(1-\beta(1-s))$ yields the permanent percentage change of a variable x in response to a permanent one percentage change in the policy variable b, $-\gamma_b \lambda_x \tilde{\kappa}$. More generally, the effect of increasing benefit duration from ω_1 to ω_2 weeks for n time periods is given by

$$\hat{\alpha} \times \frac{1-(\beta(1-s))^n}{1-\beta(1-s)} \times (\log(\omega_2) - \log(\omega_1)) \,. \tag{12}$$

Equation (10), which will form the basis of our empirical strategy, differs from the standard specification in the literature in that the left-hand-side variable is the quasi-difference $\tilde{x}_{p,t}$ as opposed to simply $x_{p,t}$. This is essential in our application because vacancy posting decisions by employers are forward looking and are affected by the expectations of future changes in benefits. Moreover, the expectations of the future path of benefits might depend on the benefit level today. For example, suppose raising benefit levels leads to a rise in unemployment. If the benefit level and the duration are increasing in state unemployment, an increase in benefits today makes it then more likely that benefits would be increased further in the future. Since vacancy creation and, consequently, unemployment respond to this change in expectations, it is clear that the coefficient α in a regression with $x_{p,t}$ on the left-hand side will be a biased estimator of the effect of the current benefit structure on the current variable of interest, such as unemployment.

To clarify how our estimation strategy controls for expectations, recall that our quasi-difference is defined as $\tilde{x}_t := \log(x_t) - \beta(1-s_t)\log(x_{t+1})$. This works because market tightness in period t, θ_t, depends on expected profits J_t and thus on the whole expected sequence of future benefit levels in $t, t+1, t+2, \ldots$ Shifting by one period, market tightness θ_{t+1} depends on expected profits in period $t+1$, J_{t+1}, and thus on the expected sequence of benefit levels in $t+1, t+2, \ldots$ Since profits in periods t and $t+1$ are related by the simple accounting identity, $J_t = \pi_t + \beta(1-s_t)E_t J_{t+1}$, market tightness θ_t depends on current profits π_t (affected by b_t) and on market tightness θ_{t+1} which is linearly related to $E_t J_{t+1}$ and depends on the sequence of benefits $(b_{t+1}, b_{t+2}, \ldots)$. As a result, a change in current benefits b_t affects current profits, current vacancy creation and thus the quasi-differenced market tightness. In contrast, changes in future benefits, say b_{t+1}, affect both θ_t and θ_{t+1}. The effect of b_{t+1} on θ_t is discounted by $\beta(1-s_t)$. The effect of b_{t+1} on θ_{t+1} is not discounted, but is multiplied by $\beta(1-s_t)$ when constructing the quasi-difference. Thus, the effect of a change in b_{t+1} cancels out in the quasi-difference. By the same logic, the quasi-difference eliminates the effect of a change in $b_{t+2}, b_{t+3}, \ldots$ Thus, our specification allows us to obtain an unbiased estimate of the coefficient α - the effect of a current change in benefits on current profits and current market tightness - despite a forward looking nature of the job creation decision.[7]

In order to ascertain the accuracy of our specification, In Section 5.4 we will compare the predicted permanent effect estimated using the proposed method to the actual permanent effect in a calibrated Mortensen and Pissarides (1994) model. We find that our empirical specification is very accurate in model generated data.

2.2 Interactive Effects

The term $\Delta\epsilon_{p,t}$ in Equation (10) contains the expectation error and the permanent differences in $\tilde{x}$ across border counties caused by, e.g., permanent differences in tax policies across states they belong to. Moreover, as we mentioned in the Introduction, various shocks have affected the aggregate economy during the Great Recession. But the same aggregate shocks are likely to have a heterogeneous impact on different border county pairs. In this case, estimating the panel regression in Equation (10), perhaps with a set of county pair and time fixed effects, might be problematic for inference (see Andrews (2005) for the discussion of this problem in a cross-sectional regression). Fortunately, Bai (2009) has shown that consistency and proper inference can be obtained in a panel data context, such as ours, through the use of an interactive-effects estimator. In particular, we decompose the error term in Equation (10) as

$$\Delta\epsilon_{p,t} = \lambda_p' F_t + \nu_{p,t}, \tag{13}$$

where λ_p $(r \times 1)$ is a vector of pair-specific factor loadings and F_t $(r \times 1)$ is a vector of time-specific common factors. Our baseline specification can then be written as

$$\Delta\tilde{x}_{p,t} = \alpha\Delta b_{p,t} + \lambda_p' F_t + \nu_{p,t}. \tag{14}$$

As is shown in Bai (2009), this model incorporates additive time and county pair fixed effects as special cases. It is, however, much more general and allows for a very flexible model of the heterogeneous time trends at the county pair level. The key to estimating α consistently is to treat the unobserved factors and factor loadings as parameters to be estimated. Our implementation is based on an iterative two-stage estimator described in Appendix I.

2.2.1 Estimating the Number of Factors

To implement this estimator, we need to specify the number of factors. Bai and Ng (2002) have shown that the number of factors in pure factor models can be consistently estimated

[7] Obviously, this issue cannot be resolved by including future values of benefits into the regression because they represent a realized path and will bias all the coefficients due to their correlation with today's expectation error.

based on the information criterion approach. Bai (2009) shows that their argument can be adapted to panel data models with interactive fixed effects. Thus, we define our criterion CP as a function of the number of factors k as:

$$CP(k) = \hat{\sigma}^2(k) + \hat{\sigma}^2(\bar{k}) \left[k\left(N+T\right) - k^2\right] \frac{\log\left(NT\right)}{NT},$$

where $\bar{k} \geq r$ is the maximum number of factors, N is the number of pairs, T is the number of time observations, $\hat{\sigma}^2(k)$ is the mean squared error, defined as

$$\hat{\sigma}^2(k) = \frac{1}{NT} \sum_{i=1}^{N} \sum_{t=1}^{T} \left(\Delta\tilde{x}_{p,t} - a\Delta b_{p,t} - \lambda_i'\left(k\right) F_t\left(k\right)\right)^2,$$

and $F_t\left(k\right)$ and $\lambda_i'\left(k\right)$ are the estimated factors and their loadings, respectively, when k factors are estimated. To avoid collinearity, we set $\bar{k}$ to the minimum of seven and $T-1$, one less than the total number of time observations. Our estimator for the number of factors is then given by

$$\hat{k} = \arg\min_{k \leq \bar{k}} CP(k).$$

2.2.2 Standard Errors

To properly compute standard errors, we need to take into account potential correlation in the residuals across counties and over time. There are two possible sources of correlation. First, the outcomes that we are interested in (unemployment, vacancies, wages, etc.) are highly serially correlated. This aspect of the data may cause serial correlation in the errors. Second, the fact that some counties appear in multiple county-pairs results in an almost mechanical correlation across county pairs. To account for these sources of correlation in the residuals, we follow Bertrand, Duflo, and Mullainathan (2004) and use the block-bootstrap to compute standard errors.

3 Data

Data on unemployment among the residents in each county are from the Local Area Unemployment Statistics (LAUS) provided by the Bureau of Labor Statistics.[8] County-level data on private sector employment (the number of jobs located in a county) and wages are from the Quarterly Workforce Indicators (QWI).[9] QWI is derived from the Local Employment

[8] ftp://ftp.bls.gov/pub/time.series/la/
[9] http://lehd.ces.census.gov/datatools/qwiapp.html

Dynamics, which is a partnership between state labor market information agencies and the Census Bureau. QWI supplies data for all counties except those in Massachusetts. Data availability varies substantially across states until 2004 Q4. Thus, for our main empirical analysis we will restrict attention to quarters beginning with 2005 Q1.[10]

To identify the role of unemployment benefit extensions on labor market outcomes, we focus our analysis on a sample of county pairs that are in different states and share a border.[11] There are 1,107 such pairs for which we have complete data through 2012 Q1.

Data on unemployment benefit durations in each state is based on trigger reports provided by the Department of Labor. These reports contain detailed information for each of the states regarding the eligibility and adoption of the two unemployment insurance programs over our primary sample period: Extended Benefits program (EB) and Emergency Unemployment Compensation (EUC08).[12]

The EB program allows for 13 or 20 weeks of extra benefits in states with elevated unemployment rates. The EB program is a joint state and federal program. The federal government pays for half of the cost, and determines a set of "triggers" related to the insured and total unemployment state rates that the states can adopt to qualify for extended benefits. At the onset of the recession, many states chose to opt out of the program or only adopt high triggers.[13] The American Recovery and Reinvestment Act of 2009 turned this into a federally funded program (with 100% Federal funding currently scheduled to expire on December 31, 2013). Following this, many states joined the program and several states adopted lower triggers to qualify for the program.

The EUC08 program enacted in June 2008, on the other hand, has been a federal program since its onset. The program started by allowing for an extra 13 weeks of benefits to all states and was gradually expanded to have 4 tiers, providing potentially 53 weeks of federally financed additional benefits. The availability of each tier is dependent on state unemployment rates.[14] The trigger reports contain the specifics of when each state was eligible and activated the EB program and different tiers of the EUC08 program. We have constructed the data through December 2012.

There is a substantial heterogeneity in the actual unemployment benefit durations across

[10] There are some implausible high frequency changes in county-level employment in QWI data. Thus, when using these data, we restrict the sample to observations where employment changes by no more than 15% from one quarter to the next. All results are robust to the choice of this cut-off.

[11] Data on county pairs are provided by Arindrajit Dube and were used in Dube, Lester, and Reich (2010).

[12] See http://ows.doleta.gov/unemploy/trigger/ for trigger reports on the EB program and http://ows.doleta.gov/unemploy/euc_trigger/ for reports on the EUC08 program.

[13] Wright (1986) studies unemployment benefit extensions in a voting equilibrium.

[14] This discussion is based on Rothstein (2011).

time and across the U.S. states. Appendix Figure A-3 presents some snapshots that illustrate the extent of this variation. Among 1,107 border county pairs used in our analysis, 1,079 have different benefits for at least one quarter. The median county pair has different benefit durations for 11 quarters during 2008-2012. The difference in available benefit duration within a county-pair ranges from 0 to 17 quarters.

Some of the data series used in the analysis are available at a monthly frequency while others are quarterly. Therefore, we aggregate all monthly data to obtain quarterly frequency. Logs are taken after aggregation. When constructing the quasi-differences at the quarterly frequency, we set $\beta = 0.99$ and use the separation rate measured from JOLTS data.[15]

4 Unemployment Benefit Extensions and Unemployment

4.1 Baseline Empirical Results

Column (1) of Table 1 contains the results of the estimation of the effect of unemployment benefit duration on unemployment using the baseline specification in Equation (14). We find that changes in unemployment benefits have large and statistically significant short-run effect on unemployment: a 1% rise in benefit duration for only one quarter increases unemployment rate by 0.06 log points. Equation (12) helps us extrapolate these effects and estimate the effect of a permanent increase in benefit durations. Using the average quarterly separation rate of 10% in JOLTS data, we find that the effect of permanently ($n = \infty$) increasing benefits from $\omega_1 = 26$ to $\omega_2 = 99$ weeks is quite sizable: The effect on unemployment is 110%, meaning that such a permanent increase would increase the long-run average unemployment rate from 5% to 10.5%.

During the Great Recession, unemployment benefits have been on average at 82.5 weeks for approximately 16 quarters. Evaluating Equation (12) at $\omega_1 = 26$, $\omega_2 = 82.5$, and $n = 16$ yields 0.54. Translating this to rates, would predict a rise in unemployment from 5% to 8.6%.[16]

When comparing the magnitude of this effect to the experience in the data, it is important to keep in mind that it is based on the difference across pairs of border counties. Thus, the effects of various other shocks or policies that affect these counties symmetrically are differenced out. For example, the 2% reduction to an employee's share of Social Security payroll taxes implemented in all states in 2011 and 2012 might have had a substantial neg-

[15] http://www.bls.gov/jlt/

[16] $\log(0.05) + 0.54 = \log(0.086)$.

Table 1: Unemployment Benefit Extensions and Unemployment

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Weeks of Benefits	**0.0607** (0.000)	**0.0592** (0.000)	**0.1528** (0.000)	**0.1193** (0.000)	**0.0638** (0.000)	**0.0629** (0.000)	**0.0635** (0.000)	**0.0579** (0.000)
State GDP per Worker		0.0039 (0.690)		**-0.0740** (0.000)				
N. factors	2	2	2	3	2	4	2	3
Observations	30,988	30,988	30,988	30,988	17,312	2,352	30,988	48,611
R-squared	0.4598	0.4601	0.565	0.642	0.490	0.642	0.473	0.405

Note - p-values (in parentheses) calculated via bootstrap. Bold font indicates $p < 0.01$.
Column (1) - Baseline sample,
Column (2) - Baseline sample controlling for State GDP per worker,
Column (3) - Scrambled border county pairs sample,
Column (4) - Scrambled border county pairs sample controlling for State GDP per worker,
Column (5) - Sample of border counties with similar industrial composition,
Column (6) - Sample of border counties within the same Core Based Statistical Areas,
Column (7) - Baseline sample with perfect foresight measure of available benefits,
Column (8) - Baseline results using data from 2001 recession only.

ative impact on unemployment, counteracting some of the effects of unemployment benefit extensions.

4.2 Testing for Endogeneity

In this section we formalize the potential endogeneity problem as well as develop and implement a test to detect its presence. We begin, however, by outlining the origin of the problem informally using an intuitive example. To help fix ideas, the example is stark and imposes stronger conditions than those actually required for identification.

Imagine a border county pair consisting of county a belonging to state A and county b belonging to state B. State A also has some geographic area $\mathcal{A}$ that excludes county a. We now consider two cases.

Case 1. Continuous economic conditions at the state border.
Suppose there is a large shock affecting the economy of $\mathcal{A}$. The economic effects of this shock might spread geographically to reach county a. However, there is no particular reason for them to stop upon reaching the state border. Thus, they will continue spreading and would affect county b similarly to their effect on county a. If this is the case, there is no

endogeneity problem in our baseline specification (14) as the difference in unemployment between counties a and b is due solely to the difference in benefit policies, perhaps triggered by the developments in $\mathcal{A}$. With geographically continuous economic fundamentals, shocks directly to counties a and b also do not create an endogeneity problem even if either one or both counties are large enough to trigger a changes in policies in the corresponding states.

Case 2. Discontinuous economic conditions at the state border.
The endogeneity problem can arise only if shocks to e.g., productivity, stop when reaching a state border. In this case, a shock to $\mathcal{A}$ may affect, say, productivity in county a and trigger a change in unemployment benefit policy in state A. In contrast, this shock stops when reaching the state border so that neither b's productivity nor B's benefit policy is affected. In this case, the difference in unemployment between counties a and b is driven by both the difference in productivities and the difference in benefits, with the latter at least partially induced by the difference in productivities. In this case, the estimate of the effect of benefits would be biased if the difference in state productivities is not controlled for.

As we mentioned, this stark intuitive example helps fix ideas at the cost of imposing stronger conditions than those actually required for identification. For example, an endogeneity problem would not arise even if there are discontinuous idiosyncratic shocks to counties a or b as long as these shocks do not affect the state average conditions and do not trigger changes in benefit policy at the state level. This is not a very strong restriction as the median border county has only one half of one percent of its state's employment.

We now turn to a more formal exposition. The identifying assumption of our empirical strategy is that the error term $\nu_{p,t}$ in estimation equation (14) is uncorrelated with benefits $\Delta b_{p,t}$. The variable x at the county level is driven by benefits b, the time varying factors F and county-specific factors such as county-productivity and demand which are unobserved and are part of the term $\nu_{p,t}$. The assumption that $\nu_{p,t}$ is not correlated with benefits then means that the differences in productivity, demand, etc. across border counties are not correlated with the benefits across the same counties. Since benefits are a function of state level variables, for this assumption to be valid, the difference in county level productivity, demand, etc. has to be uncorrelated with the corresponding differences at the state level, i.e.

$$Corr(\nu_{p,t}, \Delta z_p) = 0, \tag{15}$$

where z is state level productivity and Δz_p is the difference in productivity across states. To test this assumption, we can decompose the term $\nu_{p,t}$ into a part that depends on the state,

Δz_p, and another part that depends on county-specific factors only, $\tilde{\nu}_{p,t}$,

$$\nu_{p,t} = \chi \Delta z_p + \tilde{\nu}_{p,t}, \tag{16}$$

so that we rewrite the empirical specification as

$$\Delta \tilde{x}_{p,t} = \alpha \Delta b_{p,t} + \lambda_p' F_t + \chi \Delta z_p + \tilde{\nu}_{p,t} \tag{17}$$

for a (possibly) nonzero coefficient χ.

The economics behind this specification should by now be clear. Unemployment benefit extensions are determined at the state level and thus depend on a state's economic conditions such as state level productivity z. Thus, a negative state-level shock to z can cause unemployment to increase in all the counties in the state and simultaneously lead to an extension of benefits. When we do not control for z and $\chi \neq 0$, the estimated coefficient α would be biased in specification (17). One way to ensure that $\chi = 0$ would be to assume that the two counties in a pair are identical so that $\nu_{p,t}$ is pure measurement error. Our identifying assumption (15) is weaker than this as we allow counties to be different but only in terms of county-specific factors. State-related factors cancel when we take differences, that is $\chi = 0$. In other words, we allow for county-specific shocks but require that state-shocks affect the two counties symmetrically so that the difference in state-shocks does not affect the difference of x across the two counties.

To test for this type of endogeneity, we implement specification (17). If our empirical methodology suffers from this bias, we would expect the coefficient on Δz_p to be statistically different from zero, $\chi \neq 0$, and, more importantly, the coefficient α on benefit duration to change drastically and perhaps lose its statistical significance.[17] We define state productivity as real gross state product per worker. We obtain data on state real GDP at an annual frequency from the Regional Economic Accounts at the Bureau of Economic Analysis[18] and interpolate it at quarterly frequency. We then divide quarterly state GDP by quarterly state employment. The results are provided in Column (2) of Table 1. Note that including the difference in state productivity has almost no effect on the estimate of the effect of benefit duration on unemployment. These results provide clear evidence that our findings are not driven by a mechanical relationship between the economic conditions at the state level and the duration of unemployment benefits.

[17] We can expect to see some impact on the estimate as there might be at least some correlation between the measured productivities of the county and of the state it belongs to since the number of counties in a state may be too small for the Law of Large Numbers to apply.

[18] http://www.bea.gov/iTable/index_regional.cfm

One may also consider whether the difference in state-level unemployment rates can be used in place of Δz_p when testing for endogeneity. We explain in Appendix II why this would not constitute a valid test.

4.3 Scrambled Border County Pairs

In the previous section we tested for endogeneity by implementing equation (17) and found a negligible effect on the estimated effect of benefit extensions, α and that the effect on difference in state productivities, χ, is not statistically different from zero. The results lent empirical support to our identification assumption (15), implying that our benchmark sample is well described by Case 1 from the example in the preceding section.

Suppose, instead, that we randomly assign counties to pairs. That is, instead of pairing neighboring counties from different states, pairs are formed by randomly matching counties from the original set of the border counties. This mechanically introduces a discontinuity in economic conditions across the constructed "border" county pairs, so that Case 2 described in the preceding section applies with the associated endogeneity bias. Consider again the example of county a from state A being matched to county b from state B. With randomly assigned pairs, however, counties a and b do not border each other so that shocks to, say productivity of area $\mathcal{A}$ of state A affect productivity in county a but not in county b. If these shocks also affect economic conditions in state A, they would also be correlated with the difference in policies between States A and B. This invalidates our identification assumption (15).

Consequently, estimating our benchmark specification (14) on a scrambled border county sample would yield a biased coefficient of interest α because $\nu_{p,t}$ is correlated with $\Delta b_{p,t}$ since both are correlated with Δz_p. The empirical results of the estimation are in Column (3) of Table 1 and show that the estimate of α is indeed substantially upward biased on a sample of randomly paired counties.

Next, we add the difference in state-level productivities to this regression as in specification (17). We expect to find a negative χ because the endogeneity problem induced by the random pairing of counties. Adding state level productivity however alleviates the endogeneity problem and diminishes the bias in estimating α. The bias is not expected to fully disappear when we add state level productivity since we do not control for other state variables, such as state demand, which are also correlated with $\nu_{p,t}$ leading to a bias, albeit a smaller one. Results in Column (4) of Table 1 confirm this logic.

4.4 Border Counties with Similar Industrial Composition

As pointed out by Holmes (1998), the density of manufacturing industry employment varies systematically across counties within border pairs that belong to states with different right-to-work legislation. Manufacturing industries and thus states with a large manufacturing sector have more cyclical unemployment. They may also have a more cyclical unemployment benefit policy, potentially giving rise to the endogeneity problem. If this cyclical heterogeneity across states is sufficiently empirically important, however, our interactive effects estimator picks it up through assigning a higher loading on the cyclical aggregate factor for more cyclical states.

As an additional and more general check, we now investigate whether differences in industrial composition affect our results. To this aim, we repeat the benchmark analysis on a subset of border counties with similar industrial composition. If the industrial composition affected our results, we would expect a different result in the subsample than in the full sample. We obtain data on county employment by industry from the Bureau of Economic Analysis, Regional Economic Information System.[19] Using sample average industry employment shares within each county, we construct the l^2-distance between border counties within each pair. The results, presented in Column (5) of Table 1, are based on the sample of 50% of county pairs with the most similar industrial composition out of all border county pairs. The effect of unemployment benefit extensions on unemployment on this subsample is similar to the one found in our full sample.

4.5 Border Counties within the same CBSAs

The degree of economic integration varies across county border pairs. This is relevant for the following reason. If two border counties have a fully integrated labor market with perfect mobility of workers, the residence and employment decisions are separated. In other words, the decision in which of the two counties to (look for) work is independent of the decision in which of the counties to live. Thus, in response to a change in benefits, say, in one of the states, residents of both counties adopt the same strategy of which county to work in. As unemployment is measured by the place of residence, it will be the same in both counties. Thus, our estimate of the effect of unemployment benefit extensions on unemployment would be severely biased toward zero.

In Section 6 we will present evidence that workers do not change the location of employ-

[19] http://www.bea.gov/regional/

ment in response to changes in benefits and that labor markets in border counties are well approximated as closed economies. Here we explore whether the potential bias is large by restricting attention to a subset of border counties with most integrated labor markets. To do so, we repeat the analysis on a restricted sample of border counties that belong to the same Core Based Statistical Areas (CBSAs). CBSAs represent a geographic entity associated with at least one core of 10,000 or more population, plus adjacent counties that have a high degree of social and economic integration with the core (see Office of Management and Budget (2010) for detailed criteria). The results, presented in Column (6) of Table 1, imply similar effect of unemployment benefit extensions on unemployment to the one found in our full sample.

4.6 Alternative Benefit Duration Measure

Our baseline measure of weeks of benefits available corresponds to the number of weeks a newly unemployed worker can expect to receive if current policies and aggregate conditions remained in force for the duration of the unemployment spell. An alternative, albeit extreme, assumption is that individuals have a perfect foresight of the future path of benefits.

To construct the perfect foresight measure of available benefits, for a worker who becomes unemployed in a given week, we compute the realized maximum number of weeks available to him during the course of his unemployment spell (this takes into account extensions that are enacted after the spell begins).

The following example illustrates the construction of the two measures of benefit duration. Consider October 2009 in California. At the time, up to 26 regular weeks were available, in addition to 20 weeks in Tier 1 and 13 weeks in Tier 2 of EUC08 and 20 weeks in EB. Thus, under our baseline specification the measure of weeks available would be 26+20+13+20=79 weeks. In November of 2009, the weeks available were expanded up to 99 total (two additional tiers were added) and the program continued to be extended at those benefit levels through September of 2012. So the perfect foresight measure would assign 99 weeks available to a worker that became unemployed in 2009.

The results based on the perfect foresight measure of available benefit duration are reported in Columns (7) of Table 1. Similar to the results based on the baseline measure of benefit availability, they continue to imply a quantitatively large impact of unemployment benefit duration on unemployment.

4.7 The 2001 Recession

The Great Recession was unusually severe and accompanied by a financial crisis. This suggests that our findings of the large effect of unemployment benefit extensions on unemployment might be specific to this recession. To assess this hypothesis, we repeated the analysis using the data on benefit extensions during the much milder 2001 recession (using the 1996-2004 sample). In order to extend our analysis to the 2001 recession we need to quantify the difference in benefits during that time period. In addition to EB, the federal government enacted the Temporary Emergency Unemployment Compensation Program (TEUC), which provided up to 26 weeks of additional benefits depending on state conditions. We obtain data on weeks available from BLS trigger reports.[20] As JOLTS data are only available beginning in December 2000, prior to that we set the separation rate equal to its average value in the available JOLTS data. The results of this experiment, reported in Column (8) of Table 1, imply that the effect of unemployment benefit extensions on unemployment is the same in both recessions.

4.8 Controlling for Other State-Level Policies

In this section we control for government tax and transfer policies that might be correlated with unemployment and unemployment benefit extensions at the county or state levels.

4.8.1 Controlling for the Expansion of Food-Stamps Programs

Mulligan (2012) has argued that in addition to unemployment benefit extensions, the Department of Agriculture's food-stamp program, now known as the Supplemental Nutrition Assistance Program, or SNAP, was also expanded considerably following the Great Recession. It is possible that the expansion of this program at the state level was correlated with unemployment benefit extensions so that the results reported above combine the effects of these programs. We now isolate their impacts.

Food-stamps were originally designed as a means-tested program for the poor. During the Great Recession the Federal government has allowed states to adopt broad eligibility criteria that effectively eliminated the asset test and states received waivers from work requirements for the participants in the program. As a result, the participation in the program increased dramatically so that by 2010 half of non-elderly households with an unemployed head or spouse were receiving food stamps, with substantial variation across states.

[20] http://www.ows.doleta.gov/unemploy/teuc/

Table 2: Unemployment Benefit Extensions and Unemployment: Controlling for State SNAP and Foreclosure Policies

VARIABLES	(1)	(2)	(3)	(4)
Weeks of Benefits	**0.0607**	**0.0542**	**0.0586**	**0.0627**
	(0.000)	(0.000)	(0.000)	(0.000)
SNAP Broad Eligibility		**0.0100**		
		(0.000)		
SNAP Spending			0.0056	
			(0.190)	
Foreclosure Policy				0.0025
				(0.470)
Number of Factors	2	2	2	2
Observations	30,988	30,988	30,988	30,988
R-squared	0.460	0.464	0.463	0.465

Note - p-values (in parentheses) calculated via bootstrap. Bold font indicates $p < 0.01$.

To asses the extent to which the effects of unemployment benefit extensions documented above are affected by the expansion of food-stamps program eligibility, we obtained USDA's SNAP Policy Database which documents policy choices of each state at monthly frequency.[21] We construct a dummy variable equal to one during all periods when states use broad-based categorical eligibility to increase or eliminate the asset test and/or to increase the gross income limit for virtually all SNAP applicants. The variable is zero otherwise. We include this variable in our baseline regression and report the results in Column (2) of Table 2. The results confirm the argument in Mulligan (2012) that the expansion of food-stamps eligibility represents a marginal tax on working and thus leads to an increase in unemployment. It is, however, only weakly correlated with unemployment benefit extensions and thus does not significantly affect our estimate of their impact.

In addition, we control for the actual state-level spending on SNAP benefits that we obtained from the Regional accounts of the BEA. The results reported in Column (3) of Table 2 confirm our findings in Column (2) of Table 2 which were based on statutory rule changes.

[21]http://www.ers.usda.gov/data-products/snap-policy-database.aspx

4.8.2 Controlling for Variation in State Foreclosure Policies

The Great Recession has began with a sharp but heterogeneous across states decline in house prices. The government has responded by introducing various mortgage modification programs with the objective of helping underwater mortgagors. Various of these programs were either asset-tested or designed to write down mortgage principle to ensure that housing costs do not exceed a certain proportion of household income. In a series of papers, Mulligan (2008, 2009, 2010) has noted that this represents an implicit subsidy to unemployed workers. Moreover, Herkenhoff and Ohanian (2013) have argued that the duration of the foreclosure process has been extended considerably following the Great Recession and that unemployed mortgagors use their ability to skip payments without being foreclosed upon as an implicit loan subsidy negatively affecting their job search and acceptance decisions.

Cordell, Geng, Goodman, and Yang (2013) use proprietary data to measure the heterogeneity in foreclosure delay following the Great Recession across states. They find that in judicial states, in which state law requires a court action to foreclose, the delay is much larger than in statutory foreclosure states that do not require judicial intervention. Our use of the interactive effects estimator was specifically motivated by the concerns that aggregate shocks, such as shocks to house prices, may have heterogeneous impacts across border-county pairs depending, in part, on their state foreclosure law. To verify the performance of the estimator, we define a dummy variable taking the value of one for border counties belonging to states with judicial foreclosure laws and zero otherwise. We then include in the benchmark specification the difference of the value of this dummy between border counties i and j in pair p. The results reported in Column (4) of Table 2 indicate that this variable (the difference of the two dummies) is not statistically significant and does not affect the estimate of the effect of unemployment benefit extensions. This finding does not imply that foreclosure delay was not an important determinant of unemployment. It only means that our interactive effects estimator accounted for some of this aspect of heterogeneity across states and it did not impart a bias on our estimate of the effect of unemployment benefit extensions.

4.8.3 Controlling for the Effect of Stimulus Spending

In the specification of Column (2) of Table 3 we control for the effects of stimulus spending. We use data on actual county level spending arising from the American Recovery and Reinvestment Act (ARRA) - commonly referred to as the "stimulus package." We obtain an accounting of all stimulus spending at the zip code level under the ARRA.[22] We then

[22]www.recovery.gov.

Table 3: Unemployment Benefit Extensions and Unemployment:
Controlling for State Tax and Spending Policies

VARIABLE	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Weeks of Benefits	**0.0607**	**0.0610**	**0.0552**	**0.0590**	**0.0599**	**0.0613**	**0.0609**	**0.0591**	**0.0606**
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
		Variable in Levels				Variable Relative to GDP			
Stimulus Spending		**0.0008**				0.0007⋆			
		(0.000)				(0.210)			
Total Tax Revenue			**0.0035**				-0.0047		
			(0.005)				(0.140)		
Sales Tax Revenue				0.0004				0.0005	
				(0.245)				(0.720)	
Income Tax Revenue					0.0002				-0.0044
					(0.360)				(0.095)
# Factors	2	2	2	2	2	2	2	2	2
Obs.	30,988	30,988	30,988	30,988	30,988	30,988	30,988	30,988	30,988
R-squared	0.460	0.463	0.464	0.465	0.461	0.465	0.461	0.465	0.460

Note - *p*-values (in parentheses) calculated via bootstrap. Bold font indicates $p < 0.01$. ⋆-Relative to county population

match counties to zip codes. We run our specification both in levels and by dividing the spending by the population in the county, obtained from the Census. We find that that controlling for ARRA spending does not affect our estimate of the effect of unemployment benefit extensions.[23]

4.8.4 Controlling for State Tax Policies

To control for the variation in state-level tax policies we obtained detailed Census Bureau data on quarterly tax revenues for each state.[24] We consider whether effective total or sales tax rates have co-moved systematically with unemployment benefit durations. We find no support for this hypothesis. The results reported in Table 3 imply that directly controlling for these effective tax rates has virtually no impact on our estimates of the effect of unemployment benefit extensions on unemployment.

[23]The coefficient on spending however has to be interpreted with caution. It is conceivable, in contrast to unemployment benefits which depend on economic conditions at the state level, that spending at the county level depends on the economic conditions at the county level. In this case the coefficient on spending will be biased.

[24]http://www.census.gov/govs/qtax/

Our analysis was based on effective tax rates for two reasons. First, the statutory rates have not changed systematically over our sample period. Despite many states having balanced budget laws, expansions of unemployment benefits have not required changes in tax rates as extensions were mostly federally financed. Second, there are numerous state programs targeted to attract businesses that offer tax deductions to individual firms. For competitive reasons details of such policies are rarely disclosed. We can effectively measure them, however, by focusing on actual tax receipts.

4.8.5 Controlling for Other State Policies

While we found no evidence that the effects of unemployment benefit extensions on unemployment are a proxy for changes in other tax policies, we now consider whether they could be driven by other state policies, such as changes in regulatory or litigation environment. For this purpose we obtain data from three prominent indexes of state policies - U.S. State Business Policy Index (SBSI), State Business Tax Climate Index (SBTCI), and BHI State Competitiveness Index (BHI).[25] The construction of these indexes is based on a well-documented methodology, the data is available annually over our sample period, and can be made consistent over time. A more detailed description of these indexes, the analysis of their predictive performance for state economic outcomes, and references to other academic evaluations can be found in Kolko, Neumark, and Mejia (2013).

The motivation for using these broad policy indexes was provided in Holmes (1998), who found that controlling for a similar (but no longer available) index of state policies accounted for the positive relationship between right-to-work laws and manufacturing employment. This suggests that the conclusion about the effects of one policy may be misleading without taking into account other state policies reflected in a broad index. In contrast, the results reported in Table 4 imply that controlling for such indexes does not affect the measured impact of unemployment benefit extensions on unemployment.

[25]www.sbecouncil.org, www.taxfoundation.org, www.beaconhill.org, respectively.

Table 4: Unemployment Benefit Extensions and Unemployment:
Controlling for Other State Policies

VARIABLES	(1)	(2)	(3)	(4)
Weeks of	**0.0607**	**0.0602**	**0.0615**	**0.0588**
Benefits	(0.000)	(0.000)	(0.000)	(0.000)
SBSI		-0.0059		
		(0.105)		
SBTCI			-0.0024	
			(0.260)	
BHI				-0.0039
				(0.175)
Number of Factors	2	2	2	2
Observations	30,988	30,988	30,988	30,988
R-squared	0.460	0.462	0.464	0.462

Note - p-values (in parentheses) calculated via bootstrap.
Bold font indicates $p < 0.01$.

5 The Role of Macro Effects

In equilibrium labor market search models, the dynamics of unemployment over the business cycle and the response of unemployment to changes in policies are primarily driven by employers' vacancy creation decisions. Consider, for example, an increase in unemployment benefit duration. Having access to longer spells of benefits improves the outside option of workers and leads to an increase in the equilibrium wage. This lowers the accounting profits of firms and reduces vacancy posting to restore the equilibrium relationship between the cost of firm entry and the expected profits. Lower vacancy creation leads to a decline in labor market tightness, defined as the ratio of vacancies to unemployment. This lowers the job finding rate of workers and results in an increase in unemployment.

In this section, we present evidence on the empirical relevance of these macro effects. In particular, we document the effect of unemployment benefit extensions on vacancy creation, employment, and wages in the data. We also compare the magnitude of these empirical findings to those in a calibrated equilibrium search model.

Table 5: Unemployment Benefit Extensions and Job Creation

VARIABLES	Vacancies (1)	Tightness (2)	Employment (3)	Wages (4)
Weeks of Benefits	**-0.0631** (0.000)	**-0.1067** (0.000)	**-0.0051** (0.030)	**0.0128** (0.025)
N. factors	2	2	2	2
Observations	29,492	29,492	29,600	29,549
R-squared	0.175	0.178	0.933	0.457

Note - p-values (in parentheses) calculated via bootstrap.
Bold font indicates $p < 0.05$.

5.1 Unemployment Benefit Extensions and Vacancy Creation

We begin by considering the effect of unemployment benefit extensions on vacancy posting by employers and on labor market tightness using the basic specification in Equation (14). We obtain vacancy data from the Help Wanted OnLine (HWOL) dataset provided by The Conference Board (TCB). This dataset is a monthly series that covers the universe of vacancies advertised on around 16,000 online job boards and online newspaper editions. The HWOL database started in May 2005 and replaced the Help-Wanted Advertising Index of print advertising also collected by TCB.[26] For a more detailed description of the data, some of the measurement issues, and a comparison with the well-known JOLTS data, see Sahin, Song, Topa, and Violante (2012).

The results are reported in Columns (1) and (2) of Table 5. We find that changes in unemployment benefits have a large and statistically significant short-run effect on vacancy creation: a 1% rise in benefit duration for only one quarter lowers the number of vacancies by 0.063 log points and labor market tightness by 0.107 log points.

In the standard equilibrium search model, the matching function implies a tight relationship between changes in unemployment, vacancies, and tightness. As we have obtained independent estimates of the effects of benefit extensions on these variables, it is of interest whether their magnitudes are mutually consistent. The following calculation establishes that this is indeed the case.

Assuming that the matching function is of the commonly used Cobb-Douglas type,

$$M(u, v) = \mu v^{1-\gamma} u^{\gamma},$$

[26] For detailed information on survey methodology, coverage, and concepts see the Technical Notes at http://www.conference-board.org/data/helpwantedonline.cfm.

allows us to relate the change in tightness to the change in unemployment. Since the job finding rate is given by

$$f = \mu\theta^{1-\gamma},$$

the implied change in f induced by a change in benefits equals $-(1-\gamma) \times 0.1067$. Since the elasticity of the steady-state unemployment rate u w.r.t f equals $1-u$, the implied change in u (due to the change in tightness induced by the change in benefits) equals

$$(1-u)(1-\gamma) \times 0.1067.$$

For standard values of $\gamma = 0.4, 0.5$, assuming $u = 0.05$, the implied change equals 0.06 ($\gamma = 0.4$) and 0.05 ($\gamma = 0.5$) respectively, values close to the actual change in unemployment reported in Table 1.

5.2 Unemployment Benefit Extensions and Employment

In Column (3) of Table 5 we report the effect of unemployment benefit extensions on employment. We find a large negative effect implying that a rise in unemployment associated with an extension of unemployment benefits is similar in magnitude to the decline in employment. This finding challenges the wisdom of relying on unemployment benefit extensions as a policy to stimulate aggregate demand. The large decline in employment associated with such policies is likely to substantially dampen any potential stimulative effects.

A hypothesis often mentioned in the literature, see, e.g., Solon (1979) and Rothstein (2011), is that the rise in unemployment in response to unemployment benefit extensions might be driven by measurement issues. In particular, workers who collect benefits claim to be actively searching for a job in response to surveys used to determine the unemployment rate, while in reality they are not. In other words, had benefits not been extended, these workers would have reported themselves as being out of the labor force. The decline in the vacancy rates and employment documented here provides evidence against this hypothesis. In fact, if we consider the same calculation as in Section 4.1, we can compute the effect on employment of extending benefits to 82.5 weeks for 16 quarters as:

$$-0.0051 \times \frac{1-(\beta(1-s))^{16}}{1-\beta(1-s)} \times (\log(82.5) - \log(26)) = -0.0455.$$

Translating this into levels, this would predict a drop in the employment rate from 95% to 90.8%.[27] This 4.2 percentage point decrease is slightly larger, but of a comparable magnitude to the 3.6 percentage point increase in the unemployment rate found above.

[27] $\log(0.95) - 0.0455 = \log(0.9077)$.

Note that our estimate of the effect of unemployment benefit extensions on employment is based on the difference across border counties. We then use the resulting coefficient to predict the effect of a nation-wide extension. A potential concern with such a procedure is that when some states extend benefits more than others, economic activity and, thus, employment may reallocate to states with lower benefits. This reallocation is picked up by our estimates but would be absent if the policy was changed nation-wide. We find no empirical justification for such a concern. In particular, we apply our empirical methodology to measure the change in employment in sectors producing output that is plausibly non-tradable across states, such as retail or food services. If the change in employment is driven to an important degree by reallocation, we would not expect benefit extensions to have a large effect on these sectors. Instead, we find that a 1% rise in benefit duration for one quarter leads to a decline of employment by 0.013 and 0.015 log points in retail and food services sectors, respectively. Both effects are statistically significant at 1%.

5.3 Unemployment Benefit Extensions and Wages

We have established that extensions of unemployment benefits lead to a decline in job creation by employers. In a standard equilibrium search model such a response is induced by the fact that longer expected benefit eligibility improves the outside option of workers and leads to an increase in the equilibrium wage. We now assess whether this equilibrium effect is consistent with the data.

Consider the wage of a worker i in county a in pair p which depends on county productivity z^a, county market tightness θ^a, benefits b^a and idiosyncratic productivity ϕ^i:

$$\log(w_t^i) = \beta_0 + \beta_z \log(z_t^a) + \beta_\theta \log(\theta_t^a) + \beta_b \log(b_t^a) + \log(\phi_t^i) + \eta_t^i, \tag{18}$$

where η is a measurement error. Theory predicts that the equilibrium wage, conditional on county productivity, demand, etc, increases when UI becomes more generous. It is important to emphasize that we are referring to the response of the equilibrium wage, which is also negatively affected by a drop in market tightness caused by a negative response of job creation to the policy change. The fact that the equilibrium wage combines the positive direct effect of benefit extensions and the negative effect induced by the equilibrium response of job creation, makes the identification of the net equilibrium effect on wages more demanding on the data.

The crucial issue in studying the dynamics of wages is selection. The idiosyncratic productivity of workers moving from non-employment to employment or from job to job depend on business cycle conditions (Gertler and Trigari (2009), Haefke, Sonntag, and van Rens (2012) and Hagedorn and Manovskii (2013)). Idiosyncratic productivity can be decomposed into permanent ability μ^i, job specific productivity κ^i and a stochastic component ϵ^i:

$$\log(\phi_t^i) = \log(\mu_t^i) + \log(\kappa_t^i) + \log(\epsilon_t^i). \tag{19}$$

The decision of a non-employed to accept a job depends on z_t, μ_t^i, the job-specific productivity $\hat{\kappa}$ as well as on benefits b. The decision of a worker to switch jobs depends on the worker's current job specific productivity κ_t^i and the job-specific productivity in the new job $\hat{\kappa}$. Productivity $\hat{\kappa}$ is a random draw of a distribution F. A worker who has received N offers during a period accepts the highest draw κ, which is distributed according to F^N. Since the F^N are ordered by first-order stochastic dominance, the expected value of κ is increasing in N and is thus increasing in the number of vacancies. A more generous unemployment insurance system leads to a drop in vacancy posting and therefore to fewer offers and a lower expected value of κ. By the Law of Large Numbers, workers starting a new job in a recession or when benefits are high then have a lower average value of κ than workers starting a job when many offers are available such as in a boom or when benefits are low. Thus, if we regress wages on benefits we also pick up the impact of benefits on the average value of κ.[28] To deal with this issue, we follow Hagedorn and Manovskii (2013) and consider job stayers, defined as workers who have the same job in period t and $t+1$ and thus also the same value of κ. Taking differences across time for a job stayer yields

$$\begin{aligned} & \log(w_{t+1}^i) - \log(w_t^i) \\ = \;& \beta_z(\log(z_{t+1}^a) - \log(z_t^a)) + \beta_\theta(\log(\theta_{t+1}^a) - \log(\theta_t^a)) + \beta_b(\log(b_{t+1}^a) - log(b_t^a)) \\ + \;& \log(\epsilon_{t+1}^i) - \log(\epsilon_t^i) + \eta_{t+1}^i - \eta_t^i, \end{aligned} \tag{20}$$

that is the terms μ^i and κ^i drop out. We therefore consider a group of workers who worked in period t and $t+1$ for the same employer with average wages $w_{t,t}^a$ in period t and $w_{t,t+1}^a$ in period $t+1$. Theory then predicts that regressing the difference in wages $\log(w_{t,t+1}^a) - \log(w_{t,t}^a)$ on the difference in benefits, $\log(b_{t+1}^a) - \log(b_t^a)$, yields a positive coefficient. We again have to control for the endogeneity of policy and to this end we again invoke assumption (15) and consider the difference across paired border counties. Taking differences across counties a

[28]Benefits may also affect κ by making liquidity constrained workers more selective in the jobs they accept.

and b in the same pair p of $\log(w^a_{t,t+1}) - \log(w^a_{t,t})$ and $\log(w^b_{t,t+1}) - \log(w^b_{t,t})$ yields

$$\begin{aligned} & (\log(w^a_{t,t+1}) - \log(w^a_{t,t})) - (\log(w^b_{t,t+1}) - \log(w^b_{t,t})) \\ = \; & \beta_\theta((\log(\theta^a_{t+1}) - \log(\theta^a_t)) - (\log(\theta^b_{t+1}) - \log(\theta^b_t))) \\ + \; & \beta_b(\log(b^a_{t+1}) - log(b^a_t)) - (\log(b^b_{t+1}) - log(b^b_t))) + \vartheta_t, \end{aligned} \tag{21}$$

where ϑ_t collects all error terms and stochastic components unrelated to policy. We then regress this double difference of wages on the double difference in benefits. This captures the equilibrium wage response since benefits b are correlated with θ and regressing wages on benefits only captures both the direct effect of benefits on wages as well as the indirect effect of benefits on market tightness θ. We obtain not only the direct effect β_b but the equilibrium response which is a linear combination of β_b and β_θ.

To implement this procedure, we obtain wage data from the QWI that allows us to measure wages of job stayers. The QWI provides a measure of full quarter employment - workers who remained employed at the same firm for the entire quarter - and average wage earnings of full quarter employees. However, in quarter t the measure of full quarter employment also includes workers who will separate in $t+1$, and in quarter t the measure includes new hires from quarter t. Thus, to isolate the wages of stayers we difference out the average wages of $t+1$ separators (also available from QWI) from the average wages in t and difference out the average t new hire wages from the average wages in $t+1$. This yields the true average wages of stayers in quarters t and $t+1$.

Column (4) of Table 5 shows the result. We find that wages statistically significantly increase in response to an increase in benefits. Note that the increase in wages that we document provides strong evidence for the general equilibrium effects. Indeed, if higher unemployment was not caused by unemployment benefit extensions, one would expect wages to be *lower* in counties with higher unemployment.

To assess the quantitative magnitude of this estimate consider a typical county pair in the Great Recession. The estimate implies that a county with 70 weeks of benefits has a 0.3% higher level of wages than a county with 50 weeks of benefits, everything else equal.

5.4 Validation using Model-Generated Data

In this Section we evaluate the performance of our empirical method on data generated by a calibrated equilibrium search model. The model is an extension of Mortensen and Pissarides (1994) to allow for unemployment benefit expiration.

To address the border county design, the model features a nested state-county structure. In particular, there is a stochastic process for state's productivity.[29] The unemployment benefit policy depends on the endogenous unemployment level in the state economy. The county economy takes the endogenously induced joint stochastic process for state unemployment, productivity and benefits as exogenous. The assumption is that counties are "small" relative to the state of which they are apart.

Preferences, technology and frictions are the same across the state and county economies.

Agents. In any given period, a worker can be either employed (matched with a firm) or unemployed. Risk-neutral workers maximize expected lifetime utility

$$U = \mathbb{E}_0 \sum_{t=0}^{\infty} \beta^t c_t,$$

where $\mathbb{E}_0$ is the period-0 expectation operator, $\beta \in (0, 1)$ is the discount factor, c_t denotes consumption in period t. An unemployed worker produces h, which stands for the combined value of leisure and home production. In addition, unemployed workers may be eligible for benefits b. Unemployed workers who are eligible for benefits lose eligibility stochastically at rate $e_t(\cdot)$, which depends on the state unemployment rate as specified below.

Firms are risk-neutral and maximize profits. Workers and firms have the same discount factor β. A firm can be either matched to a worker or vacant. A firm posting a vacancy incurs a flow cost k.

Matching. The number of new matches in period t is given by $M(u_t, v_t)$, where u_t is the number of unemployed in period t, and v_t is the number of vacancies. The matching function is assumed to be constant returns to scale, and strictly increasing and strictly concave in both arguments. We define $\theta_t = v_t/u_t$ as the market tightness in period t. We then define the job-finding probability as $f(\theta_t) = M(u_t, v_t)/u_t = M(1, \theta_t)$ and the probability of filling a vacancy as $q(\theta_t) = M(u_t, v_t)/v_t = M(1/\theta_t, 1)$. By the assumptions on M made above, the function $f(\theta_t)$ is increasing in θ_t and $q(\theta_t)$ is decreasing in θ_t. Existing matches are destroyed with exogenous job separation probability δ.

Production. A matched worker-firm pair produces output z_t, which follows a first order Markov process. Firms pay workers a wage w_t, determined through Nash bargaining with workers' bargaining power ξ. Thus, the period profit of a matched firm is given by $\pi_t = z_t - w_t$.

[29] The literature based on the Mortensen and Pissarides (1994) model typically uses aggregate productivity as the standard stochastic process inducing aggregate fluctuations. Richer stochastic structures can be considered and identified in a more fully specified DSGE model. This can be necessary, depending on the purpose of the analysis. The associated complications, however, appear inessential for our purpose here, which is to assess the performance of our estimator.

5.4.1 State Economy

In the state economy the benefit expiration policy depends on the state unemployment rate, $e_t(u_t^S)$. We assume ineligible workers regain eligibility as soon as they are matched with a firm. The relevant state variables for the state economy are thus the exogenous state productivity z_t^S and the endogenous unemployment rate u_t^S. Let $\Omega_t^S = (z_t^S, u_t^S)$. The state law of motion for employment is therefore:

$$L_{t+1}^S(\Omega_t^S) = (1-\delta)L_t^S + f(\theta_t^S)\left(1 - L_t^S\right) \tag{22}$$

and $u_t^S = 1 - L_t^S$.

Value Functions. The flow value for a firm employing a worker is

$$J_t^S(\Omega_t^S) = z_t^S - w_t^S + \beta\,(1-\delta)\,\mathbb{E}J_{t+1}(\Omega_{t+1}^S) \tag{23}$$

and the flow value of a vacant firm is:

$$V_t^S(\Omega_t^S) = -k + \beta q\left(\theta_t^S\right)\mathbb{E}J_{t+1}(\Omega_{t+1}^S), \tag{24}$$

where k is the flow cost of maintaining a vacancy. The surplus for a firm employing a worker is thus $J_t^S - V_t^S$.

The value functions for workers can be written as:

$$\begin{aligned} W_t^S(\Omega_t^S) &= w_t^S + \beta\,(1-\delta)\,\mathbb{E}W_{t+1}^S + \beta\delta\left(1 - e_t(\Omega_t^S)\right)\mathbb{E}U_{t+1}^{S,E}(\Omega_{t+1}^S) \\ &\quad + \beta\delta e_t(\Omega_t^S)\mathbb{E}U_{t+1}^{S,I}(\Omega_{t+1}^S), \end{aligned} \tag{25}$$

$$\begin{aligned} U_t^{S,E}(\Omega_t^S) &= h + b + \beta f\left(\theta_t^S\right)\mathbb{E}W_{t+1}^S(\Omega_{t+1}^S) + \beta\left(1 - f\left(\theta_t^S\right)\right)\left(1 - e_t(\Omega_t^S)\right)\mathbb{E}U_{t+1}^{S,E}(\Omega_{t+1}^S) \\ &\quad + \beta\left(1 - f\left(\theta_t^S\right)\right)e_t(\Omega_t^S)\mathbb{E}U_{t+1}^{S,I}(\Omega_{t+1}^S), \end{aligned} \tag{26}$$

$$U_t^{S,I}(\Omega_t^S) = h + \beta f\left(\theta_t^S\right)\mathbb{E}W_{t+1}(\Omega_{t+1}^S) + \beta\left(1 - f\left(\theta_t^S\right)\right)\mathbb{E}U_{t+1}^{S,I}(\Omega_{t+1}^S), \tag{27}$$

where W_t^S s the value of a job for a worker, $U_t^{S,E}$ is the value of unemployment for an agent eligible for benefits and $U^{S,I}$ is the value of unemployment for a non-eligible agent. Define the surplus of being employed as $\Delta_t^{S,E} = W_t^S - U_t^{S,E}$. Also define the surplus for an unemployed worker of being eligible: $\Phi_t^S = U_t^{S,E} - U_t^{S,I}$. The laws of motion for these quantities are:

$$\begin{aligned} \Delta_t^{S,E}(\Omega_t^S) &= w_t^S - h - b + \beta\left(1 - \delta - f\left(\theta_t^S\right)\right)\mathbb{E}\Delta_{t+1}^{S,E}(\Omega_{t+1}^S) \\ &\quad + \beta\left(1 - \delta - f\left(\theta_t^S\right)\right)e_t(\Omega_t^S)\mathbb{E}\Phi_{t+1}^S(\Omega_{t+1}^S), \end{aligned} \tag{28}$$

$$\Phi_t^S(\Omega_t^S) = b + \beta\left(1 - f\left(\theta_t^S\right)\right)\left(1 - e_t(\Omega_t^S)\right)\Phi_{t+1}^S(\Omega_{t+1}^S). \tag{29}$$

The wage is chosen to maximize:

$$\left(\Delta_t^{S,E}\left(\Omega_t^S\right)\right)^{\xi}\left(J_t^S\left(\Omega_t^S\right)-V_t^S\left(\Omega_t^S\right)\right)^{1-\xi}. \tag{30}$$

State Equilibrium Definition. Given a policy $(b, e_t(\cdot))$ and an initial condition Ω_0^S an equilibrium is a sequence of Ω_t^S-measurable functions for wages w_t, market tightness θ_t^S, employment L_t^S, and value functions

$$\left\{W_t^S, U_t^{S,E}, U_t^{S,I}, J_t^S, V_t^S, \Delta_t^S\right\}$$

such that:

1. The value functions satisfy the worker and firm Bellman equations (23), (24), (25), (26), (27),
2. Free entry: The value V_t^S of a vacant firm is zero for all Ω_t^S,
3. Nash bargaining: The wage satisfies equation (30),
4. Law of motion for employment: The employment process satisfies (22).

5.4.2 County Economy

The county is assumed to be small with respect to the state of which it is a member. That is, the county unemployment rate is not assumed to affect the state unemployment rate and the county productivity process is orthogonal to the state one. The benefit expiration policy for the county, however, depends on the state unemployment rate. Thus, in addition to exogenous county productivity, z^C, the state productivity and the state unemployment rate will be state variables (since they are jointly sufficient to forecast benefit policy). Thus, denote the vector of states for the county $\Omega_t^C = \left(z_t^C; z_t^S, u_t^S\right)$. All of the equations governing workers and firms are the same as in the state's economy with the appropriately adjusted state variable. The definition of equilibrium is modified to add an additional condition, namely that the joint process for $\left(z_t^S, u_t^S\right)$ is consistent with the state equilibrium. The full equations and definition of the county equilibrium can be found in Appendix III.

5.4.3 Calibration

The calibration strategy we employ is to require the state economy to be consistent with key labor market statistics and to match the effect of unemployment benefit extensions on unemployment estimated in Section 4.1. The model period is taken to be one week. We match the average labor market tightness, the average job finding rate, and the regression

Table 6: Internally Calibrated Parameters

	Parameter	Value	Target	Data	Model
h	Value of non-market activity	0.6246	Regression Coefficient	0.0607	0.0607
ξ	Bargaining power	0.0662	Mean tightness	0.634	0.634
γ	Matching function parameter	0.3995	Mean job finding rate	0.139	0.139

Note - The permanent effect is the average increase in unemployment from increasing unemployment benefit duration by 13 weeks in all states of the world.

coefficient of quasi-differenced unemployment on benefit duration. The calibrated parameters are summarized in Table 6. In order to be consistent with the existing EB program, in the calibration we set benefit expiration policy at 26 weeks when state unemployment is less than 6.5%, 39 weeks when unemployment is between 6.5% and 8% and 46 weeks when greater than 8%. The remainder of the parameters are calibrated externally, using the same values and parametric forms for the matching function as Hagedorn and Manovskii (2008).

5.4.4 Quantitative Evaluation

The goal of the simulation exercise is to generate synthetic data at the county level comparable to the actual data. We simulate two states and one county in each of them. The two states and the two counties each have the same process for productivity. The counties, consistent with our border county assumption, have the same *realized sequence of shocks*. The two states, however, have different realized sequences of productivity shocks. Consequently, the realized exogenous sequences of state unemployment will be different. Thus, the two counties will have a different time series of unemployment benefits.

We simulate the two states and the two counties for 100 years and throw out the first 15 years of data as "burn-in." We then estimate the same regression (with quasi-differenced unemployment on the left-hand side) as we do on the data from the Great Recession. Recall that our calibration strategy ensures that coefficient on the difference in benefits in this regression is the same in the data and in the simulations of the model. Then, we calculate the effect of a permanent 13-week increase in benefits on unemployment, vacancies and tightness. We then compare these true permanent effects from the model to the calculated permanent effects from the data. The results and relevant comparisons are displayed in Table 7. The model generated data confirms the empirical validity of our specification, as our model, calibrated to generate the same regression coefficient on unemployment benefit duration from the data delivers near identical permanent effects on unemployment, vacancies and tightness.

Table 7: Estimated Permanent Effect of a 13 Week Benefit Extension
from Regressions Coefficients in Model Generated Data

VARIABLES	Unemp. (1)	Tightness (2)	Vacancies (3)
Data	0.227	-0.378	-0.231
Model	0.226	-0.388	-0.225

Note that the model does not include endogenous search intensity decisions by unemployed workers. Thus, the micro elasticity is zero, similar to the empirical estimates discussed above. The total response of unemployment is instead driven by the macro effect of benefit extensions on employers' vacancy creation decisions.

6 Change in Location of Employment in Response to Changes in Benefits

A potential concern arises from the observation that households may live in different states than where they work. This would bias our estimates if the households systematically change their job search behavior in response to changes in unemployment benefits. For example, if households search in states with less generous benefits to take advantage of a higher job-finding rate, our estimate of the effect of benefit extensions on unemployment would be biased *downwards*, since those households would face a higher job-finding rate, which would translate into a lower unemployment rate in that county. In this section, we use two different methods to show that our analysis is not affected by such a bias. First, we develop an imputation procedure that allows to estimate the effects of unemployment benefit extensions while fully accounting for mobility. Second, we provide direct empirical evidence of job search behavior. Both approaches confirm that search behavior does not vary systematically with changes in benefits, validating our use of a simple and transparent specification that ignores mobility decisions.

Because integrated labor markets generally contain multiple neighboring counties, instead of focusing on the county pair as the unit of analysis for search behavior we aggregate all counties on both sides of a border segment and perform the imputation on that "border segment" pair. To impute what fraction of workers search in the state where they live, consider the following model. We consider the local economy to consist of a pair of state border segments A, B. The segments are populated by labor forces of size n_t^A and n_t^B (taken as the sum of all the county labor forces in each state on the respective side of the border) and populations p_t^A and p_t^B.

In any given period, a worker can be either employed (matched with a firm), unemployed or not in the labor force. In period t, firms in state A post vacancies in state A, v_t^A. An unemployed worker in state A searches either in state A or in state B. We assume that a fraction ζ of non-labor force participants (observed in the LAUS data) enter the labor force and search for jobs. The number of new matches in state A in period t equals

$$M\left(\tilde{u}_t^A, v_t^A\right),$$

where $\tilde{u}_t^A$ is the measure of individuals in period t searching in state A. The number of matches is the same for state B *mutatis mutandis*. We assume a constant returns to scale matching function M that is strictly increasing and strictly concave in both arguments. We define

$$\tilde{\theta}_t^A = \frac{v_t^A}{\tilde{u}_t^A}$$

to be the market tightness in state A in period t. We define the job-finding and vacancy-filling probabilities as in Section 5.4.

The law of motion for the unemployed who live in states A and B is:

$$u_{t+1}^A = \delta_t\left(n_A - u_t^A\right) + u_t^A\left(1 - x_t^A f\left(\theta_t^A\right) - \left(1 - x_t^A\right) f\left(\theta_t^B\right)\right), \tag{31}$$

$$u_{t+1}^B = \delta_t\left(n_B - u_t^B\right) + u_t^B\left(1 - x_t^B f\left(\theta_t^B\right) - \left(1 - x_t^B\right) f\left(\theta_t^A\right)\right), \tag{32}$$

where u_t^i is the number of unemployed who live in state i, x_t^i is the fraction of the unemployed in state i that searches in state i, and δ_t is the separation probability into unemployment, calculated from the Current Population Survey (CPS) following Shimer (2007).

We can thus write for the number of unemployed searching in state A and B respectively:

$$\tilde{u}_t^A = (u_t^A + \zeta(p_t^A - n_t^A))x_t^A + (1 - x_t^B)(u_t^B + \zeta(p_t^B - n_t^B)), \tag{33}$$

$$\tilde{u}_t^B = (u_t^B + \zeta(p_t^B - n_t^B))x_t^B + (1 - x_t^A)(u_t^A + \zeta(p_t^A - n_t^A)), \tag{34}$$

where we follow Hall (2013) and set ζ to $5/27$ to match the ratio of the job-finding rates of non-participants to the unemployed in the CPS.

We can measure the probabilities for an unemployed worker from states A and B to find a job, ϕ_t^A and ϕ_t^B, in the data:

$$\phi_t^A = \frac{u_t^A - u_{t+1}^A + \delta_t\left(n_t^A - u_t^A\right)}{u_t^A}, \tag{35}$$

$$\phi_t^B = \frac{u_t^B - u_{t+1}^B + \delta_t\left(n_t^B - u_t^B\right)}{u_t^B}, \tag{36}$$

as all right-hand variables are measurable in the data. Using (31), we can then relate the measurable ϕ_t^A and ϕ_t^B to the unobservable variables x_t^A, x_t^B, $f(\theta_t^A)$, $f(\theta_t^B)$:

$$\phi_t^A = x_t^A f\left(\theta_t^A\right) - \left(1 - x_t^A\right) f\left(\theta_t^B\right), \tag{37}$$

$$\phi_t^B = x_t^B f\left(\theta_t^B\right) - \left(1 - x_t^B\right) f\left(\theta_t^A\right). \tag{38}$$

The four equations (33), (34), (37) and (38) have 4 unknowns, $x_t^A, x_t^B, f(\theta_t^A), f(\theta_t^B)$.[30] These equations are not linearly independent and thus do not allow us to recover these 4 unknowns. Instead they give us a set of solutions S.

In order to proceed to identify x_t^A, x_t^B we assume that the matching function is Cobb-Douglas, $\mu u^\gamma v^{1-\gamma}$. Note, however, that we do not necessarily see the true level of vacancies. However, if we assume that we see the same *fraction*, ψ, of total vacancies for both counties in a pair, we can still estimate the effective matching function given our observed vacancies. If we observe $\tilde{v} = \psi v$, then the total number of matches is $\tilde{\mu} u^\gamma \tilde{v}^{1-\gamma}$, where $\tilde{\mu} = \psi^{\gamma-1}\mu$. Thus, we propose to identify $\tilde{\mu}$ and γ in addition to the x's.

We allow $\tilde{\mu}$ to change over time, to capture any possible time trends in the adoption of online vacancies. The algorithm consists of selecting α, $\{\mu_t, x_t^A, x_t^B\}_{t=1}^T$ to minimize the error in the equations (37), (38) and:

$$\frac{q(\theta_t^A)}{q(\theta_t^B)} = \left(\frac{\theta_t^B}{\theta_t^A}\right)^\alpha, \tag{39}$$

where we observe all left hand side variables for all t.[31]

We measure the effect of benefits on search behavior by examining the difference between the imputed fraction of workers searching away from their home states $(1 - x_t^A) - (1 - x_t^B)$. Further, we construct imputed tightness by dividing county level vacancies by the imputed measure of unemployed workers searching in that county ($v_t^A/\tilde{u}_t^A$), corrected for the search behavior along that border segment (we impose the same x's for all counties within a state for each border segment). Then, the job finding rate is constructed using the imputed tightness and the estimated parameters of the matching function. Table 8 Column (1) shows, using the difference-in-difference estimator, that there is only a very small and statistically insignificant response of search behavior, to changes in benefits, so that mobility does not bias our estimates. Further, the effect on imputed tightness, which now fully accounts for changes in mobility in response to changes in benefits, is not statistically significantly different from

[30] We do not directly observe x_t^A, and thus we don't observe $\tilde{u}_t^A$ and θ_t^A, nor the matching function.

[31] The probability to fill a vacancy $q_t = 1 - \frac{v_{t+1} - v_{t+1}^{new}}{v_t}$, where v_t is the stock of vacancies at t and v_t^{new} are newly posted vacancies at t, so that $v_{t+1} - v_{t+1}^{new}$ are not filled vacancies from period t. Both v_t and v_t^{new} are observable in the data.

Table 8: Effect of UI Benefits on Imputed Labor Market Variables

VARIABLES	Out of State Work	Imputed Tightness	Imputed Job-finding
Weeks of	0.0002	**-0.1154**	**-0.0524**
Benefits	(0.510)	(0.000)	(0.000)
Factors	2	2	2
Observations	29,492	29,492	29,492
R-squared	0.066	0.2816	0.2996

Note - p-values (in parentheses) calculated via bootstrap.
Bold font indicates $p < 0.01$.

the baseline estimate. The effect of extending benefits to 82.5 weeks for approximately 16 quarters (the average during the Great Recession) on the quarterly job finding rate would predict a drop from 77.6% to 48.6%.

Next, we look for direct empirical evidence on where people work relative to where they live. We use data from the American Community Survey (ACS) from 2005-2011. The ACS is an annual 1% survey of households in the United States conducted by the Census Bureau. The survey contains information on the county of residence of households and the state of employment. The survey is representative at the Public Use Micro Area level - a statistical area that has roughly 100,000 residents (and thus also for counties with more than 100,000 residents). We compute the share of households in border counties who work in the neighboring state. We can then examine how this share of border state workers responds to changes in benefits across states. We perform our analysis using the quasi-difference estimator derived in the empirical methodology section and using a difference-in-difference estimator:

$$\begin{aligned} \text{Quasi-difference:} \quad \Delta\tilde{e}_{p,t} &= \phi_p + \alpha_e \Delta b_{p,t} + \Delta\nu_{p,t} \\ \text{Diff-in-diff:} \quad \Delta e_{p,t} &= \phi_p + \alpha_e \Delta b_{p,t} + \Delta\nu_{p,t} \end{aligned}$$

where $e_{p,t}$ is the fraction of workers at time t that live in county i and work in the state associated with county j (also in pair p). The results of the regressions are in Table 9. Using both the quasi-difference and difference-in-difference specification the coefficient on weeks of benefits available is statistically insignificant. This direct evidence once again implies that worker search behavior does not respond significantly to changes in unemployment benefits.

Table 9: Regression Estimates of Out of State Employment

VARIABLES	Out of state work	
	Quasi-Difference	Diff-in-Diff
	(1)	(2)
Weeks of Benefits	-0.3560	0.1737
	(1.125)	(1.267)
Pair Fixed Effects	Yes	No
Observations	76	76
R-squared	0.770	0.115

Note - Standard errors in parentheses.
Bold font indicates $p < 0.1$.

7 Implications for Macro Models

Throughout the paper our analysis was motivated by equilibrium search models, such as Mortensen and Pissarides (1994). We found empirical support for the key mechanisms in the model. In particular, extending unemployment benefits puts an upward pressure on equilibrium wages, which induces lower vacancy posting by firms and consequently an increase in unemployment. Using a simple calibrated version of the model we found that these effects are quantitatively consistent with the data.

In this section we briefly comment on the implications of our findings for the business cycle analysis using this class of models. This analysis was carried out in Mitman and Rabinovich (2013), who used a version of the model in Section 5.4, calibrated to match the effect of unemployment benefit extensions on unemployment documented in this paper. They carefully model the history of unemployment benefit extensions in the US. In addition to changing unemployment benefit eligibility over time, the dynamics are driven by fluctuations in aggregate productivity. The endogenously determined dynamics of the unemployment rate in the model together with its evolution in the data are plotted in Figure 1.

The results indicate that the effect of unemployment benefit extensions on unemployment, vacancies, and wages documented in this paper is consistent with the effect of business cycle movements in aggregate productivity on these variables. Interestingly, Mitman and Rabinovich (2013) find that the automatic and discretionary benefit extensions in the recent recessions have substantially amplified the response of unemployment and served as the root cause of the widely documented phenomenon of the jobless recoveries (benefit extensions are triggered when unemployment reached a sufficiently high level so that they effectively kick in

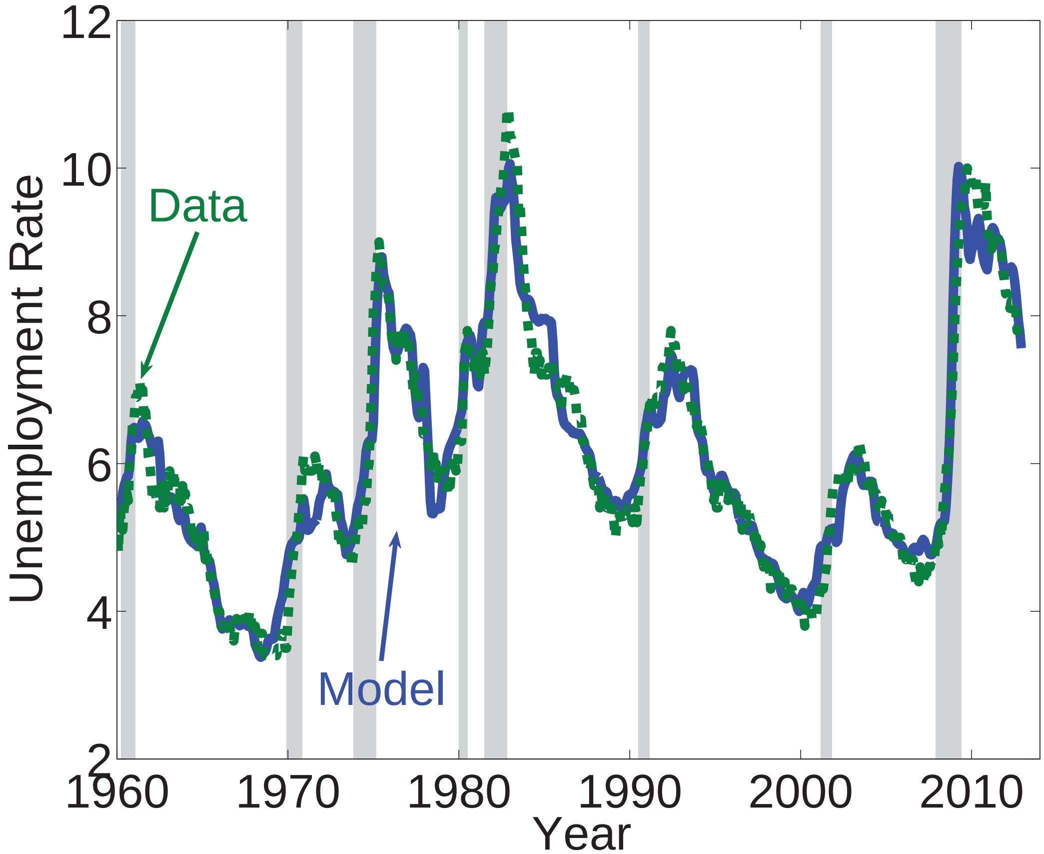

Figure 1: The evolution of the U.S. unemployment rate: data and the prediction of the search model with unemployment benefit extensions in Mitman and Rabinovich (2013).

after productivity is already recovering, inducing a delayed recovery of employment). This is evident in Figure 1.

An important line of research, reviewed in Diamond (2013), that also aims to explain the persistently high unemployment following the great recession focused on the behavior of the Beveridge curve. As the dotted green line in Figure 2 illustrates, the curve appears to have shifted out following the Great Recession. This was interpreted as implying an increase in the "structural" or "mismatch" unemployment because of the apparently high level of vacancies coexisting with high unemployment. As the solid blue line in the same figure illustrates, this behavior of the Beveridge curve arises naturally in the productivity-driven equilibrium search model with the extensions of unemployment benefits as observed in the data during the Great Recession.

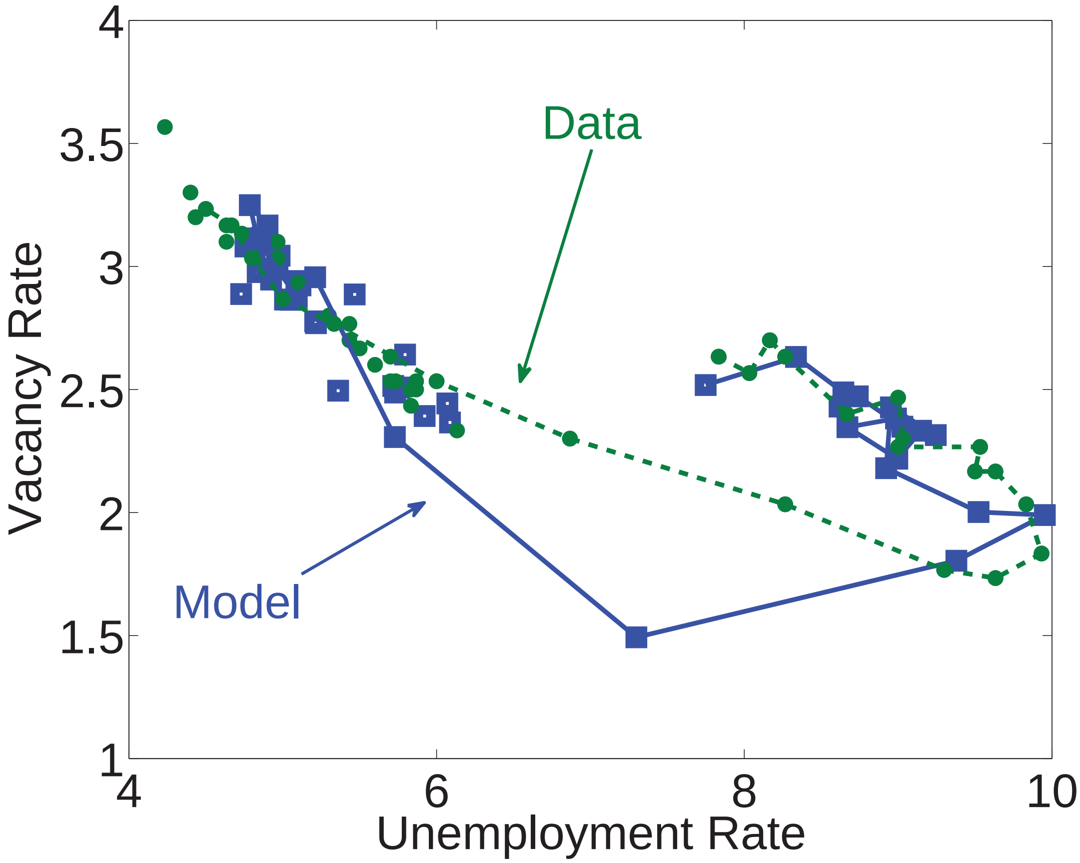

Figure 2: The Beveridge Curve in the Great Recession: data and the prediction of the search model with unemployment benefit extensions in Mitman and Rabinovich (2013).

8 Conclusion

In this paper we employed a state-of-the-art empirical methodology to measure the total effect of unemployment benefit extensions on unemployment. In particular, we exploited the discontinuity of unemployment insurance policies at state borders to identify their impact. Our estimator controls for the effect of expectations of future changes in benefits and has a simple economic interpretation. It is also robust to the heterogeneous impacts of aggregate shocks on local labor markets.

We found that unemployment benefit extensions have a large effect on total unemployment. In particular, our estimates imply that unemployment benefit extensions can account for most of the persistently high unemployment after the Great Recession. Coupled with the robust finding in the recent literature that the "micro" effect of unemployment benefit extensions on worker search effort and job acceptance decisions is small, this finding implies that the "macro" elasticity is quantitatively large, much larger than the micro elasticity. We found direct support for this conclusion by documenting a large negative response of vacancy creation and employment to unemployment benefit extensions.

One motivation for increasing unemployment benefit durations during the Great Recession, in addition to helping unemployed workers smooth their consumption, is to increase employment through its stimulative effect on local demand. Although we cannot do full justice to evaluating this effect given the methodology on which our analysis relies, our results nevertheless offer some insights. To the extent that the unemployed spend a significant fraction of their income in their home counties (in a form of e.g., rent payments or service purchases), the corresponding part of the stimulative effect is fully captured by our analysis. Indeed, we find that border counties with longer benefit durations have much higher unemployment, despite the potential beneficial effects of spending. If, on the other hand, spending by the unemployed was spread uniformly on goods and services provided in all counties, this aggregate component is not captured, as it is differenced out by our estimator. We find, however, that an increase in unemployment due to benefit extensions is similar in magnitude to the decline of employment. Thus, the total effect on spending is ambiguous as extending benefits increase spending by the unemployed but at the same time decrease spending as fewer people are employed. The potential offsetting effect of lower employment due to higher benefits was also recognized by policymakers but considered - based on the micro studies discussed above - to be quantitatively very small. Our results of a sizeable macro effect leads us to expect that the stimulative effect of higher spending by the unemployed is largely offset by the dramatic negative effect on employment from the general equilibrium effect of benefit expansion on vacancy creation. To evaluate this effect more explicitly, especially given the zero lower bound constraints imposed on monetary policy following the Great Recession, it is desirable to asses the effects of unemployment benefit extensions in a richer DSGE model with frictional labor market, such as the one in Christiano, Eichenbaum, and Trabandt (2013). It's interesting to note, however, that we find similar effects of increases in benefits during the Great Recession and during the 2001 recession, despite the fact that the latter featured much higher nominal interest rates.

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APPENDICES

I Implementation of Iterative Two-Stage Estimator

The following is a brief description of the algorithm implementing our iterative two-stage estimator.

1. Start with a guess for α, say α_1.
2. At each iteration ξ, do the following:
   (a) given α_ξ, for each p, construct $\upsilon_{p,t} = \Delta x_{p,t} - \beta(1-s_t)\Delta x_{p,t+1} - \alpha_j \Delta b_{p,t}$. Then, $\upsilon_{p,t} = \lambda_p' F_t$ is a pure factor model and can be estimated consistently using principal components.[32]
   (b) Given the estimates for λ_p and F_t, estimate equation (14) via OLS and update the guess to obtain $\alpha_{\xi+1}$.
3. Repeat 2 until α_ξ converges.[33]

II Inadmissibility of State Unemployment Differences in Testing for Endogeneity

In this section we elucidate why controlling for the difference in state unemployment does not constitute a valid exogeneity test. To illustrate our point, we simulate data from our calibrated model where we impose exogeneity - i.e. we assume the productivity processes at the county and state level are independent. In Figure A-1 we plot the time series for state unemployment, county unemployment and weeks of benefits available. Notice that both state and county level unemployment are smooth moving variables, whereas the weeks of benefits jumps when a benefit extension is triggered on. The correlation between state and county unemployment is significantly higher than between county unemployment and benefits, and controlling for state unemployment completely takes out the effect of benefits. However, it is important to note that the only channel through which the state economy affects the county economy is through the benefit policy (because in this example the productivity processes are orthogonal). Thus, controlling for state unemployment is not a valid test for exogeneity.

[32]The exposition of the estimator assumes that there are no missing observations. We use the generalized procedure described in Bai (2009) and allow for missing observations.

[33]We have conducted a number of Monte Carlo simulations with sample sizes similar to our sample. The estimator described here is found to converge to the true parameter. Results are available upon request.

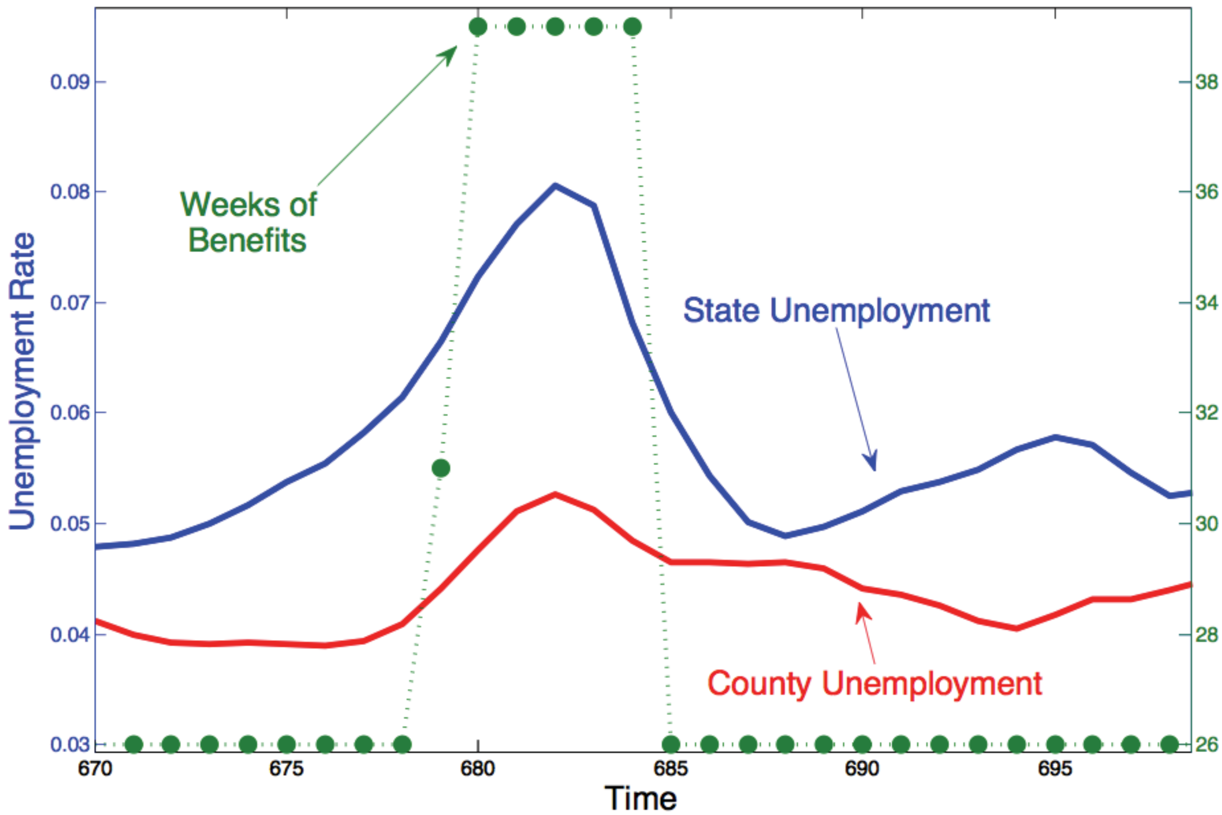


Figure A-1: County and State Unemployment: Model.

III County Economy, Detailed Specification

The law of motion for county employment is:

$$L_{t+1}^C(\Omega_t^C) = (1-\delta)L_t^C + f(\theta_t^C)\left(1 - L_t^C\right). \tag{A1}$$

and $u_t^C = 1 - L_t^C$.

Value Functions. The flow value for a firm employing a worker is

$$J_t^C(\Omega_t^C) = z_t^C - w_t^C + \beta\,(1-\delta)\,\mathbb{E}J_{t+1}(\Omega_{t+1}^C), \tag{A2}$$

and the flow value of a vacant firm is:

$$V_t^C(\Omega_t^C) = -k + \beta q\left(\theta_t^C\right)\mathbb{E}J_{t+1}^C(\Omega_{t+1}^C). \tag{A3}$$

The surplus for a firm employing a worker is thus $J_t^C - V_t^C$.

The value functions for workers can be written as:

$$\begin{aligned}
W_t^C(\Omega_t^C) &= w_t^C + \beta(1-\delta)\mathbb{E}W_{t+1}^C + \beta\delta\left(1-e_t(\Omega_t^C)\right)\mathbb{E}U_{t+1}^{C,E}(\Omega_{t+1}^C) \\
&\quad + \beta\delta e_t(\Omega_t^C)\mathbb{E}U_{t+1}^{C,I}(\Omega_{t+1}^C), \qquad \text{(A4)} \\
U_t^{C,E}(\Omega_t^C) &= h + b + \beta f\left(\theta_t^C\right)\mathbb{E}W_{t+1}^C(\Omega_{t+1}^C) + \beta\left(1-f\left(\theta_t^C\right)\right)\left(1-e_t(\Omega_t^C)\right)\mathbb{E}U_{t+1}^{C,E}(\Omega_{t+1}^C) \\
&\quad + \beta\left(1-f\left(\theta_t^C\right)\right)e_t(\Omega_t^C)\mathbb{E}U_{t+1}^{C,I}(\Omega_{t+1}^C), \qquad \text{(A5)} \\
U_t^{C,I}(\Omega_t^C) &= h + \beta f\left(\theta_t^C\right)\mathbb{E}W_{t+1}(\Omega_{t+1}^C) + \beta\left(1-f\left(\theta_t^C\right)\right)\mathbb{E}U_{t+1}^{C,I}(\Omega_{t+1}^C). \qquad \text{(A6)}
\end{aligned}$$

Define the surplus of being employed as $\Delta_t^{C,E} = W_t^C - U_t^{C,E}$. Also define the surplus for an unemployed worker of being eligible: $\Phi_t^C = U_t^{C,E} - U_t^{C,I}$. The laws of motion for these quantities are:

$$\begin{aligned}
\Delta_t^{C,E}(\Omega_t^C) &= w_t^C - h - b + \beta\left(1-\delta-f\left(\theta_t^C\right)\right)\mathbb{E}\Delta_{t+1}^{C,E}(\Omega_{t+1}^C) \\
&\quad + \beta\left(1-\delta-f\left(\theta_t^C\right)\right)e_t(\Omega_t^C)\mathbb{E}\Phi_{t+1}^C(\Omega_{t+1}^C), \qquad \text{(A7)} \\
\Phi_t^C(\Omega_t^C) &= b + \beta\left(1-f\left(\theta_t^C\right)\right)\left(1-e_t(\Omega_t^C)\right)\Phi_{t+1}^C(\Omega_{t+1}^C). \qquad \text{(A8)}
\end{aligned}$$

The wage is chosen to maximize:

$$\left(\Delta_t^{C,E}\left(\Omega_t^S\right)\right)^\xi \left(J_t^C\left(\Omega_t^S\right) - V_t^C\left(\Omega_t^S\right)\right)^{1-\xi}. \qquad \text{(A9)}$$

County Equilibrium Definition. Taking as given an initial condition Ω_0^C, benefit expiration policy, and the joint stochastic process for state productivity and unemployment, we define an equilibrium given policy:

Definition Given a policy $(b, e_t(\cdot))$ and an initial condition Ω_0^C an equilibrium is a sequence of Ω_t^C-measurable functions for wages w_t, market tightness θ_t^C, employment L_t^C, and value functions

$$\left\{W_t^C, U_t^{C,E}, U_t^{C,I}, J_t^C, V_t^C, \Delta_t^C\right\}$$

such that:

1. The value functions satisfy the worker and firm Bellman equations (A2), (A3), (A4), (A5), (A6);
2. Free entry: The value V_t^C of a vacant firm is zero for all Ω_t^C;
3. Nash bargaining: The wage satisfies equation (A9);
4. Law of motion for employment: The employment process satisfies (A1);
5. The joint process for $\left(z_t^S, u_t^S\right)$ is consistent with the state equilibrium.

IV Appendix Figures

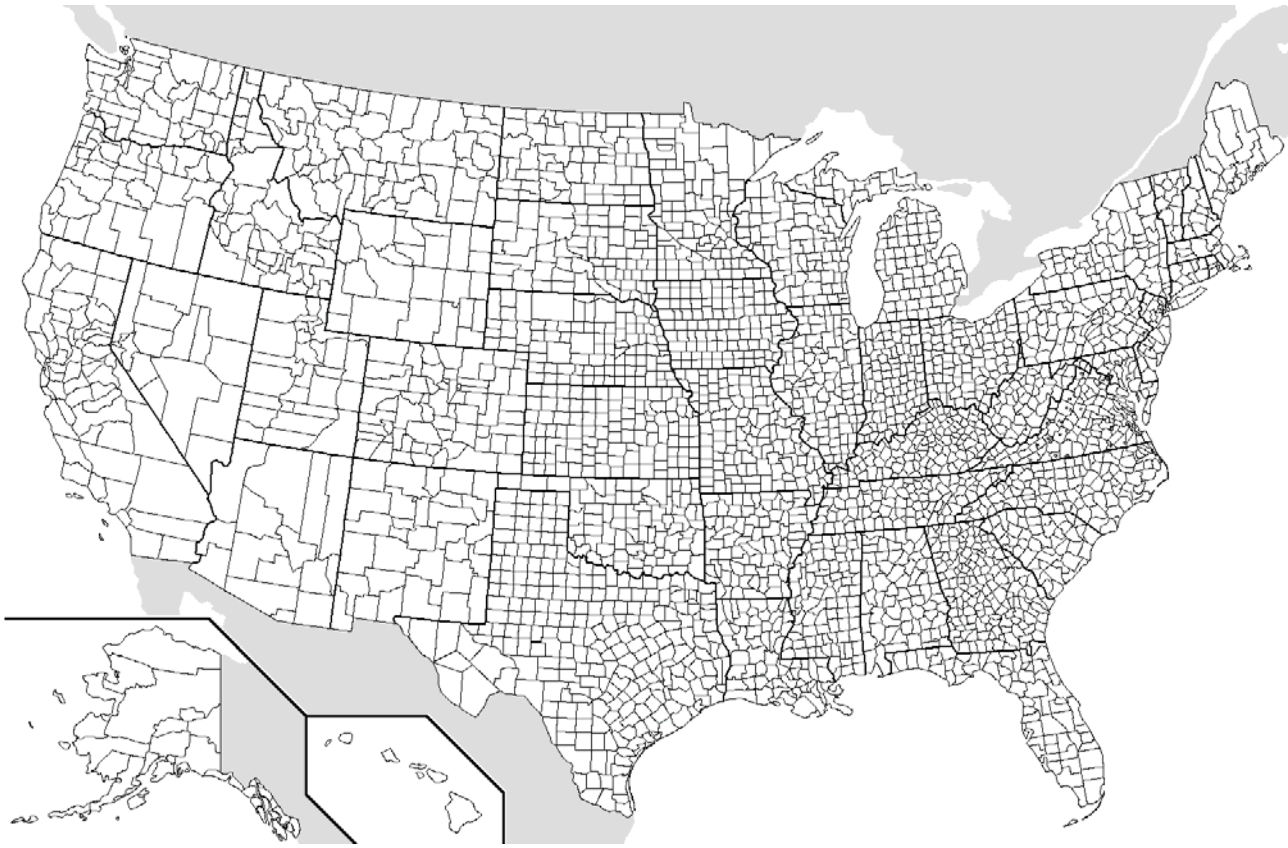
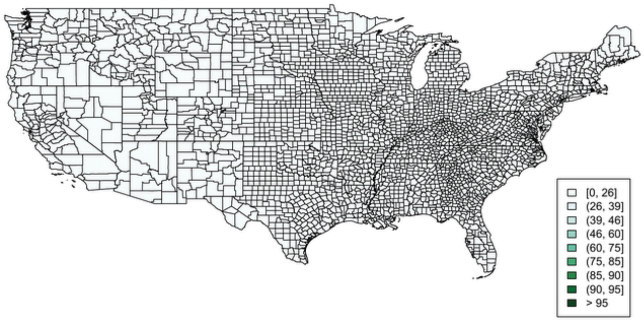


Figure A-2: Map of U.S.A. with state and county outlines.



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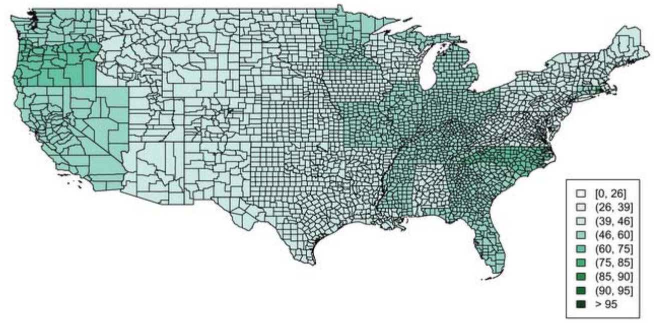

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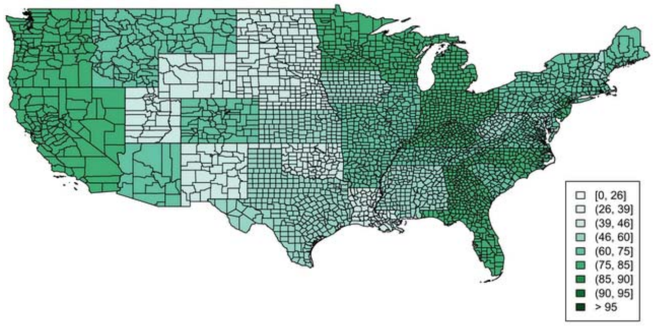

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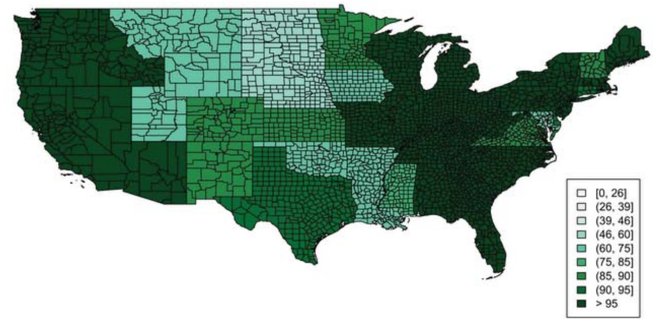

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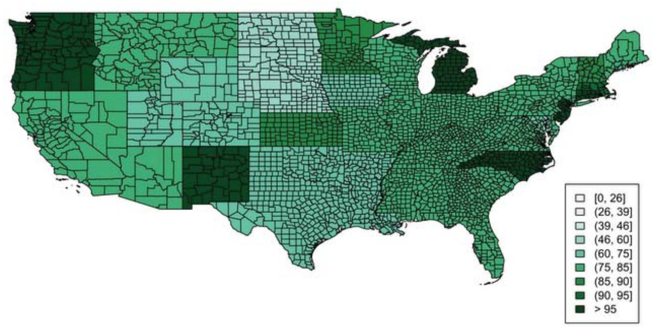
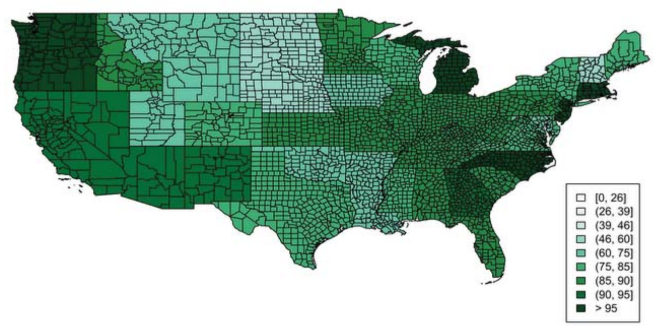


(e) June 2010.



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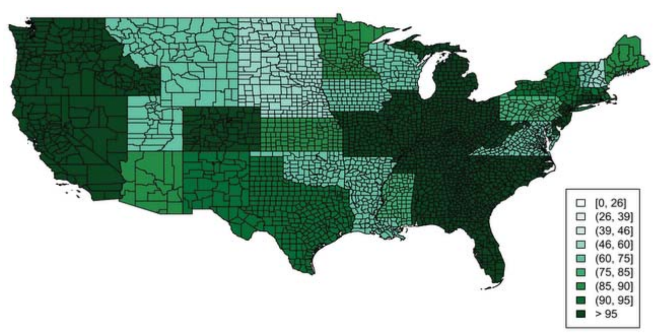
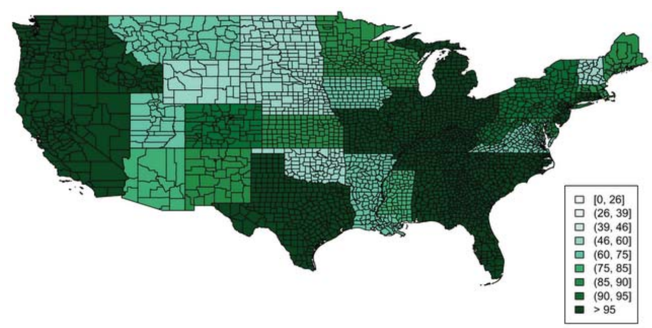
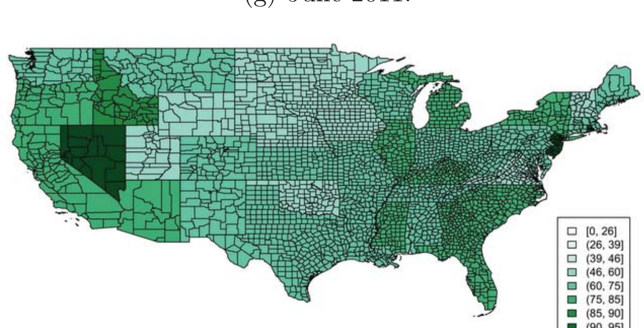


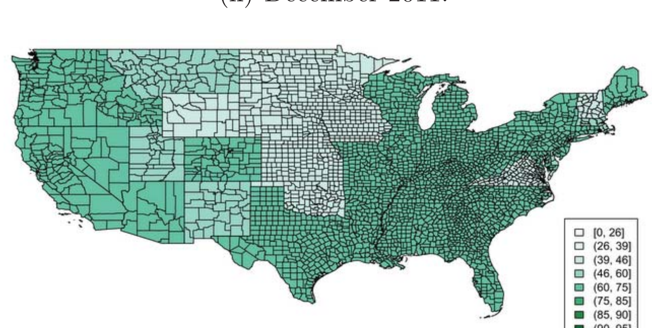
(g) June 2011.



(h) December 2011.



(i) June 2012.



(j) December 2012.

Figure A-3: Unemployment benefit duration across U.S. states during the Great Recession. Selected months.