

Unemployment Benefits and Unemployment in the Great Recession: The Role of Macro Effects*

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Abstract

We exploit 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.

Keywords: Unemployment insurance, Unemployment, Employment, Vacancies, Wages, Search, Matching

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"... our citizens are fast returning, from the panic into which they were artfully thrown to the dictates of their own reason; and I believe the delusions they have seen themselves hurried into will be useful as a lesson under similar attempts on them in future... If we can prevent the government from wasting the labors of the people, under the pretence of taking care of them, they must become happy."

Thomas Jefferson, Letter to Thomas Cooper, November 29, 1802

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 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, the 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:

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

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,

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.

$f(\theta_t) = 0$ and 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 driven by the 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 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 the filled jobs. As in equilibrium expected profits from creating a job are driven down to the cost of vacancy creation, the probability of filling a vacancy has to rise, implying that vacancy creation has to decline. 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 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 UI policies are determined at the state level and apply to all locations within a state. To assess the effects of these policies on unemployment, we compare 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.

²A Map of U.S. state and county borders can be found in Appendix Figure A-1.

We extend this successful empirical strategy to accommodate features of the policies that we are interested in evaluating (and verify the good performance of these extensions in the data generated by an estimated equilibrium search model). First, 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 of the effect of a permanent change in nationwide UI policy on unemployment - the effect of particular interest to macro economists. Second, 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 has potentially different impact on the states depending on their foreclosure laws. To obtain consistent estimates of the UI policies we follow Bai (2009) and use a flexible interactive effects model.

Consistent with implications of the equilibrium search model, we find that border counties with longer benefit extensions have significantly higher wages, lower vacancy rates, lower employment, and higher unemployment. Our estimates imply that benefit extensions quantitatively account for much of the unemployment dynamics following the Great Recession. The finding that unemployment benefit extensions have a strong negative impact on labor demand and employment calls into question the effectiveness of using unemployment benefit extensions as a policy tool for stimulating employment. It also has important consequences for the appropriate monetary policy, given the dual mandate of the Fed.

We also consider the implications of our findings for macroeconomic time-series. In particular, we summarize the findings in Mitman and Rabinovich (2013), who introduced benefit extensions into the Mortensen and Pissarides (1994) model and calibrated it 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 Related Literature

We organize the discussion of the related literature 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 benefits extensions on labor market outcomes during the Great Recession. Using data from the Current Population Survey (CPS) on individual unemployment duration, he exploits the variation in benefits available across states 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 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

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

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

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 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 any 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 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

⁵One 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.

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

In this section we develop our empirical methodology.

2.1 Basic Identification via Border Counties

In its most basic form, the identification of policy effects on the sample of contiguous counties on the two sides of a state border is based on the following specification:

$$\left. \begin{aligned} \Delta x_{p,i,t} &= \alpha \Delta b_{p,i,t} + \Delta \epsilon_{p,i,t}, \\ \Delta \epsilon_{p,i,t} &= \phi_p + \nu_{p,i,t}, \end{aligned} \right\} \quad (2)$$

where $x_{p,i,t}$ is the labor market outcome of interest in logs (unemployment rate, vacancy rate, or the ratio of vacancy to unemployment rates, commonly referred to as labor market tightness),⁶ $b_{p,i,t}$ is the logarithm of number of weeks of benefits available, ϕ_p is a pair-specific fixed effect that captures permanent differences in unemployment across border counties caused by, e.g., permanent differences in tax policies across states they belong to, and $\nu_{p,i,t}$ is the error term. The subscript p denotes the county pair, i is the index for county within the pair, and t denotes time. Δ denotes the difference operator over counties in the same pair. More specifically, if counties i and j are in the same county-pair p , then $\Delta x_{p,i,t} = x_{p,i,t} - x_{p,j,t}$.

To make this specification operational, we need to develop a way to control for the anticipation effects of policy changes and specify the structure of the error term. This is the subject of the following two subsections.

⁶These rates are the ratios of the stocks of unemployed workers or vacancies to the labor force, respectively.

2.2 Controlling for Expectations

Since vacancy posting decisions by employers are forward looking, they 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 benefit level and duration is 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 a in regression (2) will be a biased estimator of the effect of current benefit structure on the current variable of interest, such as unemployment. 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. In this section we develop a methodology that allows to obtain an unbiased estimate of the coefficient a despite a forward looking nature of the job creation decisions.

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 Diamond-Mortensen-Pissarides model, firm's period t profits from employing a worker are given by the difference between worker's 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 firm's profits from employing a worker are given by:

$$\log(\pi_t) = \gamma_z \log(z_t) - \gamma_b \log(b_t), \quad (3)$$

where z_t is worker's productivity and b_t are benefits. γ_z and γ_p 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:

$$J_t = \pi_t + \beta(1 - \delta)E_t J_{t+1}, \quad (4)$$

where β is the discount factor, δ is the 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

$$q(\theta_t)J_t = c, \quad (5)$$

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

$$\log(\theta_t) = \tilde{\kappa} \log(J_t). \quad (6)$$

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 $\pi^* = J^*(1 - \beta(1 - \delta))$, so that the previous equation reads

$$\log(\theta_t) = \tilde{\kappa} \frac{\pi^*}{J^*} \log(\pi_t) + \tilde{\kappa} \beta(1 - \delta) \log(J_{t+1}) + \log(\eta_t). \quad (7)$$

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

$$\log(\theta_t) = \tilde{\kappa}(1 - \beta(1 - \delta)) \log(\pi_t) + \beta(1 - \delta) \log(\theta_{t+1}) + \log(\eta_t). \quad (8)$$

In quarterly data variables such as unemployment are well approximated a linear function of $\log(\theta)$:⁷

$$\log(x_t) = \lambda_x \log(\theta_t), \quad (9)$$

so that we obtain the quasi-difference

$$\tilde{x}_t := \log(x_t) - \beta(1 - \delta) \log(x_{t+1}) = \tilde{\kappa} \lambda_x (1 - \beta(1 - \delta)) \log(\pi_t) + \lambda_x \log(\eta_t) \quad (10)$$

Thus, taking into account the effects of expectations, the empirical specification in Equation (2) is modified as follows:

$$\left. \begin{aligned} \Delta \tilde{x}_{p,i,t} &= \alpha \Delta b_{p,i,t} + \Delta \epsilon_{p,i,t}, \\ \Delta \epsilon_{p,i,t} &= \phi_p + \nu_{p,i,t}. \end{aligned} \right\} \quad (11)$$

The coefficient α equals

$$-\gamma_b \lambda_x \tilde{\kappa} (1 - \beta(1 - \delta)), \quad (12)$$

and it relates the permanent percentage change of a variable x in response to a permanent one percentage change in the policy variable b , $-\gamma_b \lambda_x \tilde{\kappa}$, by a measurable factor $(1 - \beta(1 - \delta))$.

In order to ascertain the accuracy of our specification, In Section 5.5 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.

⁷See, 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.

2.3 Interactive Effects

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 Equations (2) or (11), perhaps with the 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 the interactive-effects estimator. In particular, we decompose the error term in Equation (11) as

$$\Delta\epsilon_{p,i,t} = \lambda'_p F_t + \nu_{p,i,t}, \quad (13)$$

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

$$\Delta\tilde{x}_{p,i,t} = \alpha\Delta b_{p,i,t} + \lambda'_p F_t + \nu_{p,i,t}. \quad (14)$$

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.⁸ Our implementation is based on an iterative two-stage estimator described in Appendix I.

2.3.1 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 based on the information criterion approach. Bai (2009) shows that their argument can be modified to the panel data model with interactive fixed effects. Thus, we define:

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

where $\bar{k} \geq r$ and $\hat{\sigma}^2(k)$ is mean squared error, defined as

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

⁸As in all factor models, some normalizations are needed in order to identify the factors and their loadings. We follow the normalizations as in Bai (2009).

and $F_t(k)$ and $\lambda'_i(k)$ are the estimated factors and their loadings, respectively, when k factors are estimated. We set \bar{k} to $T - 1$. Our estimator for the number of factors is then given by

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

2.3.2 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.⁹ More specifically, our blocks are defined as the set of county-pairs that are on the same border segment. A border segment is defined as the border between two states. Two county pairs are defined to be on the same border segment if the counties are in the same states, respectively.

3 Data

Data on employment and unemployment of the residents in each county and state as well as the corresponding labor force are from the Local Area Unemployment Statistics (LAUS) provided by the Bureau of Labor Statistics¹⁰. These data are available starting in 1990 through 2012 Q4.

Data on employment within a county (the number of jobs located in a county) and on new hires in a county are from the Quarterly Workforce Indicators (QWI)¹¹. QWI is derived from the Local Employment 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. The start date from the data varies from 1990 Q1 through 2004 Q4. Thus, for our main empirical analysis we will restrict attention to quarters beginning with 2005 Q1.

⁹We run 250 repetitions and sample, with equal probability, entire border segments.

¹⁰<ftp://ftp.bls.gov/pub/time.series/la/>

¹¹<http://lehd.ces.census.gov/datatools/qwiapp.html>

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.¹² There are 1,118 such pairs for which we have complete data.

To obtain data on unemployment benefit durations in each state, we use 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).¹³

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.¹⁴ The American Recovery and Reinvestment Act of 2009 turned this into a federally funded program. Following this act, 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.¹⁵ 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 time and the U.S. states. Appendix Figure A-2 presents some snapshots that illustrate the extent of this variation. Among 1,181 border county pairs used in our analysis, 1,145 have different benefits for at least one quarter. Median county pair has different benefit durations for 11 quarters during 2008-2012. 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

¹²Data on county pairs are provided by Arindrajit Dube and were used in Dube, Lester, and Reich (2010).

¹³See <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.

¹⁴Wright (1986) studies unemployment benefit extensions in a voting equilibrium.

¹⁵This discussion is based on Rothstein (2011).

others are quarterly. However, separations data 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 job destruction rate observed in the data.

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 5.23 log points. The formula derived in Section 2.2 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 increasing benefits from 26 to 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. This would imply that the effect of this particular extension on unemployment is

$$0.0607 \times \frac{1 - (\beta(1 - \delta))^{16}}{1 - \beta(1 - \delta)} \times (\log(82.5) - \log(26)) = 0.53.$$

Translating this to levels, this would predict a rise in unemployment from 5% to 8.5%.¹⁶

4.2 Testing for Endogeneity

The identifying assumption of our empirical strategy is that the error term $\nu_{p,i,t}$ in our estimation equation

$$\Delta \tilde{x}_{p,i,t} = \alpha \Delta b_{p,i,t} + \lambda'_p F_t + \nu_{p,i,t} \tag{15}$$

is uncorrelated with benefits $\Delta b_{p,i,t}$. 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 $\nu_{p,i,t}$. The assumption that $\nu_{p,i,t}$ is not correlated with benefits then means that the differences in productivity, demand, etc.

¹⁶ $\log(0.05) + 0.53 = \log(0.085)$.

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)	0.0532 (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	38,296	38,296	38,296	38,296	19,148	2,352	38,296	30,718
R-squared	0.4598	0.4601	0.565	0.642	0.490	0.642	0.473	0.5293

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.

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.

$$\text{Corr}(\nu_{p,i,t}, \Delta z_p) = 0, \quad (16)$$

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 $\nu_{p,i,t}$ into a part that depends on the state, Δz_p , and another part that depends on county-specific factors only, $\tilde{\nu}_{p,i,t}$,

$$\nu_{p,i,t} = \chi \Delta z_p + \tilde{\nu}_{p,i,t}, \quad (17)$$

so that we rewrite the empirical specification as

$$\Delta \tilde{x}_{p,i,t} = \alpha \Delta b_{p,i,t} + \lambda'_p F_t + \chi \Delta z_p + \tilde{\nu}_{p,i,t} \quad (18)$$

for a (possibly) nonzero coefficient χ .

The economics behind this specification is simple. Unemployment benefit extensions are determined at the state level and thus depend on state's economic conditions such as state

level productivity z . Thus, a negative state-level shock to z can cause unemployment to increase and vacancies to decrease 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 the specification (18). One way to ensure that $\chi = 0$ would be to assume that the two counties in a pair are identical so that $\nu_{p,i,t}$ is pure measurement error. Our identifying assumption (16) 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 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 the specification (18). 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 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 Analysis¹⁸ 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.

4.3 Scrambled Border County Pairs

In the previous section we tested for endogeneity by implementing equation (18) and found that χ is not statistically different from zero. The sample on which we implemented this regression consisted of the same set of county pairs used in the benchmark estimation. The results lent empirical support to the assumption that these border counties have similar labor markets so that

$$Corr(\nu_{p,i,t}, \Delta z_p) = 0. \tag{19}$$

¹⁷We 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.

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

Suppose instead that we randomly assign counties to pairs. That is, instead of pairing neighboring counties from different states, pairs are formed by two randomly chosen counties. These counties are not expected to have similar labor markets. As a result consider two such counties A and B and the effects of a shock to county A 's state level productivity. Since counties A and B are not paired this shock may affect both counties differently, in the same way that an aggregate shock may affect unrelated county pairs differently. This invalidates our identification assumption (16) and thus

$$Corr(\nu_{p,i,t}, \Delta z_p) \neq 0. \quad (20)$$

Differences in state productivity of two randomly paired counties affect both the difference in county unemployment rates and in benefit levels. The invalidity of our identifying assumption on this scrambled sample implies that in our specification

$$\Delta \tilde{x}_{p,i,t} = \alpha \Delta b_{p,i,t} + \lambda'_p F_t + \nu_{p,i,t} \quad (21)$$

$\nu_{p,i,t}$ is correlated with $\Delta b_{p,i,t}$ since both are correlated with Δz_p . The results of the estimation are in Column (3) of Table 1 and show that the estimate of α is upward biased if we randomly pair counties.

To further explore policy endogeneity we add state-level productivity to this regression as in specification (18). We already established that $\chi = 0$ if we control for endogeneity by pairing neighboring counties and we expect to find a negative χ if we do not properly control for endogeneity by randomly pairing 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 $\nu_{p,i,t}$ 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

Our baseline results are based on the sample of all border county pairs. As pointed out by Holmes (1998) the density of manufacturing industry employment varies systematically across counties within 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 which might potentially affect our results. If this is empirically sufficiently important for unemployment, our interactive effects estimator picks this 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% county pairs with the most similar industrial composition. The effect of unemployment benefit extensions on unemployment on this subsample are similar to the one found in our full sample.

4.5 Border Counties within the same CBSAs

Our baseline results are based on the sample of all border county pairs. It is possible that the degree of economic integration might vary across county border pairs. To evaluate whether this has an impact on our findings, we repeat the analysis on a restricted sample of border counties that belong to the same Core Based Statistical Areas (CBSAa). CBSAa 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

¹⁹<http://www.bea.gov/regional/>

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. 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 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 is to control for the government tax and spending policies that might be correlated with unemployment and unemployment benefit extensions at the county or state levels.

4.8.1 Controlling for the Effect of Stimulus Spending

In the specification of Column (2) of Table 2 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

²⁰<http://www.ows.doleta.gov/unemploy/teuc/>

Table 2: Unemployment Benefit Extensions and Unemployment: Controlling for State Tax and Spending Policies

VARIABLES	(1)	(2)	(3)	(4)
Weeks of Benefits	0.0607*** (0.000)	0.0613*** (0.000)	0.0609*** (0.000)	0.0591*** (0.000)
Stimulus Spending		0.0008 (0.210)		
Tax Revenue per GDP			-0.0047 (0.140)	
Sales Tax Revenue per GDP				0.0005 (0.720)
Number of Factors	2	2	2	2
Observations	38,296	38,296	38,296	38,296
R-squared	0.460	0.465	0.461	0.465

Note: p -values (in parentheses) calculated via bootstrap.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

accounting of all stimulus spending at the zip code under the ARRA from recovery.gov. We then match counties to zip codes and divide the spending by the population in the county, obtained from the Census. We find that ARRA spending had no statistically significant effect on unemployment and that controlling for it does not affect our estimate of the effect of unemployment benefit extensions.

4.8.2 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.²¹ We consider whether effective total or sales tax rates have co-moved systematically with unemployment benefit durations. We find no support for this hypothesis. Directly controlling for these effective tax rates have virtually no impact on our estimates of the effect of unemployment benefit extensions on unemployment.

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

²¹<http://www.census.gov/govs/qtax/>

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.

4.8.3 Controlling for Other State Policies

While we found no evidence that the effects of unemployment benefit extensions on unemployment are 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).²² 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 their 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 conclusion about the effects of one policy may be misleading without taking into account other state policies reflected in a broad index. In contrast, we find that controlling for such indexes does not affect the measured impact of unemployment benefit extensions and unemployment.

4.8.4 Controlling for Other State Policies: Expansion of Food Stamps and variation in Foreclosure policies

Mulligan (2012) has argued that in addition to unemployment benefit extensions other government transfer programs were expanded considerably. He considers the expansion of the Department of Agriculture's food-stamp program, now known as the Supplemental Nutrition Assistance Program, or SNAP and Mortgage modification programs. It is possible that the adoption of these programs 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.

²²www.sbecouncil.org, www.taxfoundation.org, www.beaconhill.org, respectively.

Table 3: Unemployment Benefit Extensions and Unemployment: Controlling for Other State Policies

VARIABLES	(1)	(2)	(3)	(4)
Weeks of Benefits	0.0607*** (0.000)	0.0607*** (0.000)	0.0607*** (0.000)	0.0607*** (0.000)
SBSI		0.0000 (0.000)		
SBTCI			0.000 (0.000)	
BHI				0.0000 (0.000)
Number of Factors	2	2	2	2
Observations	38,296	38,296	38,296	38,296
R-squared	0.460	0.000	0.000	0.000

Note: p -values (in parentheses) calculated via bootstrap.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Expansion of Food Stamps. Food-stamps were originally designed as a means-tested program for the poor. During the Grate 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.

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.²³ We construct a dummy variable equal to one during the periods when states adopt 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 xxx of Table yyy. 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,

²³<http://www.ers.usda.gov/data-products/snap-policy-database.aspx>

however, only weakly correlated with unemployment benefit extensions and thus does not 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 xxx of Table yyy confirm our earlier findings based on the statutory rule changes.

Variation in Foreclosure Policies.

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 the vacancy creation decisions by employers. 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.

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.²⁴ We have access to the data over the period May

²⁴Data collection is handled by Wanted Technologies for TCB. For detailed information on survey methodology, coverage, and concepts see the Technical Notes at <http://www.conference-board.org/data/helpwantedonline.cfm>.

Table 4: Unemployment Benefit Extensions and Job Creation

VARIABLES	Vacancies (1)	Tightness (2)	Employment (3)	Wages (4)
Weeks of Benefits	-0.0619*** (0.000)	-0.1029*** (0.000)	-0.003* (0.080)	0.0086* (0.100)
N. factors	2	2	3	2
Observations	15,860	15,860	38,296	32,080
R-squared	0.318	0.331	0.521	0.438

Note: p -values (in parentheses) calculated via bootstrap.

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

2005 through April 2012. The HWOL methodologies changed during 2005 and 2006 and the series underwent a major expansion in 2007. In addition, the series experienced strong trend growth in the first several years due to the final shift from newspaper and other forms of advertising to online advertising. To prevent these non-economic issues from potentially influencing our estimates we begin with the series after 2007. 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 4. 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 6.2 log points and labor market tightness by 10.3 log points.

5.2 Unemployment Benefit Extensions and Matching Efficiency

Consider a Cobb-Douglas matching function:

$$M(u, v) = \mu v^{1/2} u^{1/2}$$

The job finding rate is given by:

$$f = \mu \theta^{1/2}$$

Total change in unemployment: -0.607 .

Effects on unemployment, Δu , operate entirely through changes in job-finding rate, Δf :

$$\begin{aligned} \Delta u &\approx -\Delta f = \underbrace{-\Delta\mu}_{\text{Matching efficiency}} - \underbrace{1/2\Delta\theta}_{\text{Job creation}} \\ -0.607 &= -\Delta\mu - 1/2 \times (-0.1029) \\ \Rightarrow \Delta\mu &= 0.009 \end{aligned}$$

5.3 Unemployment Benefit Extensions and Employment

In Column (3) of Table 4 we report the effect of unemployment benefit extensions on employment. We find a large negative effect implying that a rise in unemployment associated with extension of unemployment benefits is similar in magnitude to the decline in employment. This finding challenges the wisdom on 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 the 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 been out of the labor force. The decline in the vacancy rates and employment documented here provides strong evidence against this hypothesis.

5.4 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.

To this aim, 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 :

$$\log(w_t^i) = \beta_0 + \beta_z \log(z_t^A) + \beta_\theta \log(\theta_t^A) + \beta_b \log(b_t^A) + \log(\phi_t^i) + \eta_t^i, \quad (22)$$

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 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 :

$$\log(\phi_t^i) = \log(\mu_t^i) + \log(\kappa_t^i) + \log(\epsilon_t^i). \quad (23)$$

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 UI system leads to 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 .²⁵ 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 stayer yields

$$\begin{aligned} & \log(w_{t+1}^i) - \log(w_t^i) \\ = & \beta_z(\log(z_{t+1}^A) - \log(z_t^A)) + \beta_\theta(\log(\theta_{t+1}^A) - \log(\theta_t^A)) + \beta_b(\log(b_{t+1}^A) - \log(b_t^A)) \quad (24) \\ + & \log(\epsilon_{t+1}^i) - \log(\epsilon_t^i) + \eta_{t+1}^i - \eta_t^i, \end{aligned}$$

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 16 and consider the difference across paired counties. Taking differences across counties A and B in

²⁵Benefits may also affect κ by making liquidity constrained workers more selective in the jobs they accept.

the same pair p of $\log(w_{t,t+1}^A) - \log(w_{t,t}^A)$ and $\log(w_{t,t+1}^B) - \log(w_{t,t}^B)$ yields

$$\begin{aligned}
& (\log(w_{t,t+1}^A) - \log(w_{t,t}^A)) - (\log(w_{t,t+1}^B) - \log(w_{t,t}^B)) \\
= & \beta_\theta((\log(\theta_{t+1}^A) - \log(\theta_t^A)) - (\log(\theta_{t+1}^B) - \log(\theta_t^B))) \\
& + \beta_b(\log(b_{t+1}^A) - \log(b_t^A)) - (\log(b_{t+1}^B) - \log(b_t^B)) + \vartheta_t,
\end{aligned} \tag{25}$$

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 b is 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 4 shows the result. We find that wages statistically significantly increase in response to a contemporaneous and to a lagged 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.

5.5 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.

In order to address the border county design, our model will consist of a nested state-county structure. Thus, the state economy will have a stochastic process for its productivity. The unemployment benefit policy will be dependent on the endogenous unemployment level

in the state economy. The county economy will take 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 will be 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 their benefits stochastically at rate $e_t(\cdot)$, which depends on the state unemployment rate.

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

$$\theta_t = \frac{v_t}{u_t}$$

as the market tightness in period t . We then define the functions:

$$f(\theta) = \frac{M(u, v)}{u} = M(1, \theta) \quad \text{and}$$

$$q(\theta) = \frac{M(u, v)}{v} = M\left(\frac{1}{\theta}, 1\right),$$

where $f(\theta)$ is the job-finding probability and $q(\theta)$ is the probability of filling a vacancy. By the assumptions on M made above, the function $f(\theta)$ is increasing in θ and $q(\theta)$ is decreasing in θ .

Existing matches are destroyed with exogenous job separation probability δ . This determines the law of motion for employment, given by

$$L_{t+1} = (1 - \delta)L_t + f(\theta_t)(1 - L_t). \quad (26)$$

Production. A matched worker-firm pair produces output z_t , which follows a first order Markov process. Firms pay workers a wage w_t , which is determined through bargaining, where workers have bargaining power ξ .

Thus, the period profit of a matched firm is given by:

$$\pi_t = z_t - w_t.$$

5.6 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 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:

$$L_{t+1}^S(\Omega_t^S) = (1 - \delta)L_t^S + f(\theta_t^S)(1 - L_t^S). \quad (27)$$

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

5.6.1 Value Functions

The flow value for a firm employing a worker is

$$J_t^S(\Omega_t^S) = z_t^S - w_t^S + \beta(1 - \delta) \mathbb{E}J_{t+1}^S(\Omega_{t+1}^S) \quad (28)$$

and the flow value of a vacant firm is:

$$V_t^S(\Omega_t^S) = -k + \beta q(\theta_t^S) \mathbb{E}J_{t+1}^S(\Omega_{t+1}^S), \quad (29)$$

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:

$$\begin{aligned} W_t^S(\Omega_t^S) &= w_t^S + \beta(1 - \delta) \mathbb{E}W_{t+1}^S + \beta\delta(1 - e_t(\Omega_t^S)) \mathbb{E}U_{t+1}^{S,E}(\Omega_{t+1}^S) \\ &\quad + \beta\delta e_t(\Omega_t^S) \mathbb{E}U_{t+1}^{S,I}(\Omega_{t+1}^S), \end{aligned} \quad (30)$$

$$\begin{aligned} U_t^{S,E}(\Omega_t^S) &= h + b + \beta f(\theta_t^S) \mathbb{E}W_{t+1}^S(\Omega_{t+1}^S) + \beta(1 - f(\theta_t^S))(1 - e_t(\Omega_t^S)) \mathbb{E}U_{t+1}^{S,E}(\Omega_{t+1}^S) \\ &\quad + \beta(1 - f(\theta_t^S)) e_t(\Omega_t^S) \mathbb{E}U_{t+1}^{S,I}(\Omega_{t+1}^S), \end{aligned} \quad (31)$$

$$U_t^{S,I}(\Omega_t^S) = h + \beta f(\theta_t^S) \mathbb{E}W_{t+1}^S(\Omega_{t+1}^S) + \beta(1 - f(\theta_t^S)) \mathbb{E}U_{t+1}^{S,I}(\Omega_{t+1}^S). \quad (32)$$

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:

$$\begin{aligned} \Delta_t^{S,E}(\Omega_t^S) &= w_t^S - h - b + \beta(1 - \delta - f(\theta_t^S)) \mathbb{E}\Delta_{t+1}^{S,E}(\Omega_{t+1}^S) \\ &\quad + \beta(1 - \delta - f(\theta_t^S)) e_t(\Omega_t^S) \mathbb{E}\Phi_{t+1}^S(\Omega_{t+1}^S), \end{aligned} \quad (33)$$

$$\Phi_t^S(\Omega_t^S) = b + \beta(1 - f(\theta_t^S))(1 - e_t(\Omega_t^S)) \Phi_{t+1}^S(\Omega_{t+1}^S). \quad (34)$$

Wages are determined by Nash bargaining. The wage is chosen to maximize:

$$\left(\Delta_t^{S,E}(\Omega_t^S)\right)^\xi \left(J_t^S(\Omega_t^S) - V_t^S(\Omega_t^S)\right)^{1-\xi}. \quad (35)$$

5.6.2 State Equilibrium Definition

Taking as given an initial condition Ω_0^S and benefit expiration policy, we define an equilibrium given policy:

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 (28), (29), (30), (31), (32).

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 (35).
4. Law of motion for employment : Employment and the measure of eligible unemployed workers satisfy (26).
5. Law of motion for employment: The unemployment process satisfies (26).

5.7 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 state productivity, the state productivity and unemployment rate will be state variables (since they are sufficient to forecast benefit policy). Thus, denote the vector of states for the county $\Omega_t^C = (z_t^C; z_t^S, u_t^S)$. All of the equations governing workers and firms are the same with the appropriately adjusted state variable. The definition of equilibrium is modified to add an additional condition, namely that the joint process for (z_t^S, u_t^S) is consistent with the state equilibrium. The full equations and definition of the county equilibrium can be found in the appendix.

5.8 Calibration

The calibration strategy we employ is to calibrate 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 regression coefficient of running quasi-differenced unemployment on benefit duration, the average labor market tightness and the average job finding rate. The calibrated parameters are summarized in the Table 5. 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).

Table 5: Internally Calibrated Parameters

Parameter	Value	Target	Data	Model
h Value of non-market activity	0.6246	Regression Coefficient	0.0609	0.0609
ξ Bargaining power	0.0662	Mean tightness	0.634	0.634
χ Matching 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.

Table 6: Regressions Coefficients in Model Generated Data
Permanent Effect of a 13 Week Benefit Increase

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

5.9 Simulation Exercise

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 counties 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 sequence 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 two counties for 100 years and throw out the first 15 years of data as "burn-in." We then run the same regression as we do on the data from the Great Recession - quasi-differenced unemployment. 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 from the model are displayed in Table 6. The model generated data confirms the empirical validity of our specification, as our model, calibrated to generate the same regression coefficient on unemployment from the data delivers near identical permanent effects on unemployment, vacancies and tightness (compare to the empirical results in Tables 1, 4).

Note that the model does not include endogenous search intensity decision 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 vacancy creation decisions of employers.

6 Implications for Macro Models

Throughout the paper our analysis was motivated by the equilibrium search model, 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 5.5, 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 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 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 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 green line in Figure 2 illustrates, there 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 blue line in the same figure illustrates,

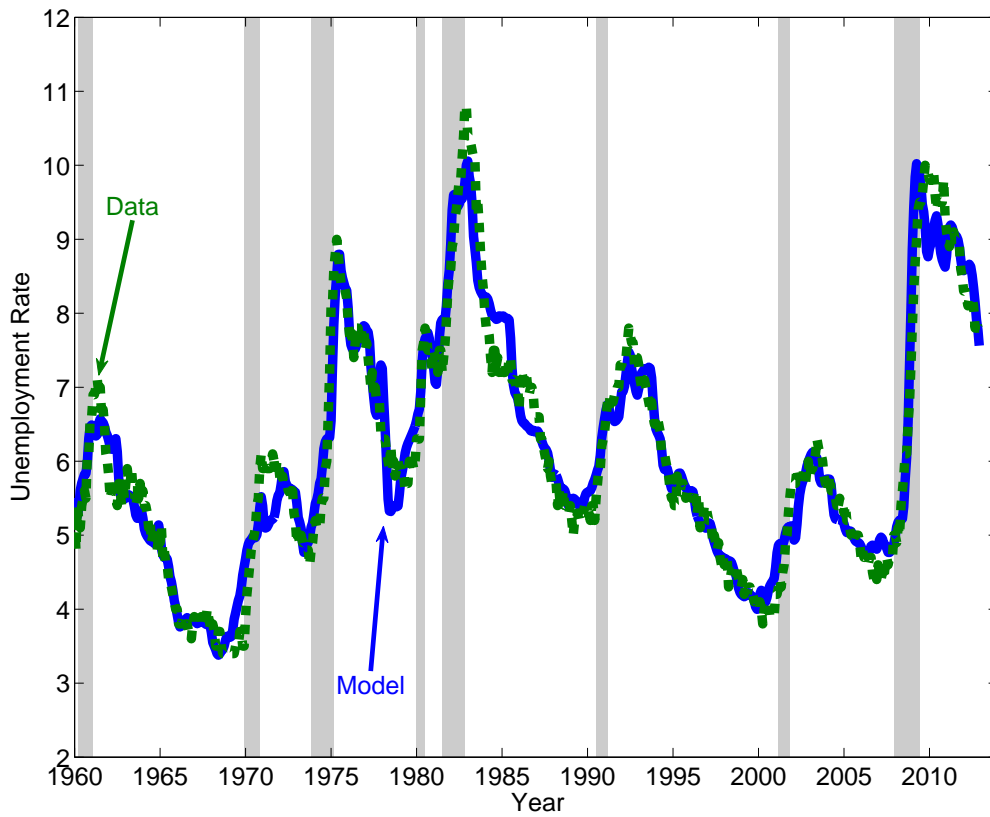


Figure 1: The evolution of the U.S. unemployment rate: data and the prediction of the search model with unemployment benefit extensions in Mitman and Rabinovich (2013).

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.

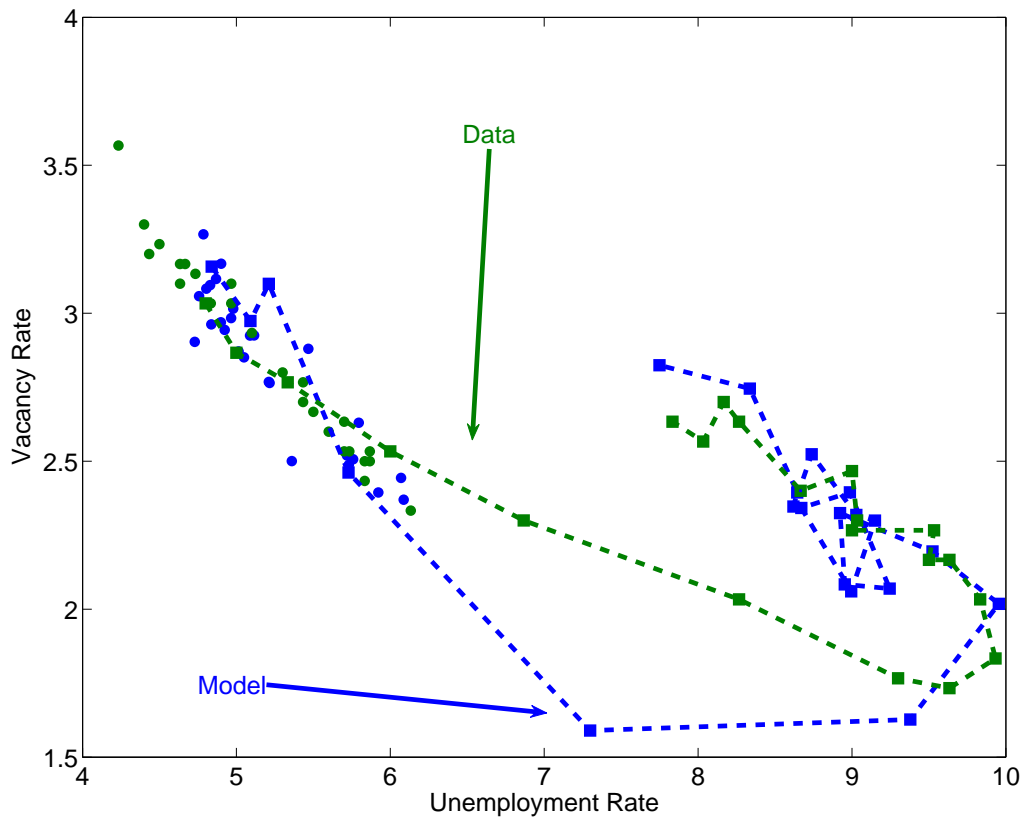


Figure 2: The Beveridge Curve in the Great Recession: data and the prediction of the search model with unemployment benefit extensions in Mitman and Rabinovich (2013).

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

One potential concern is that households may live in different states than where they work. If the households systematically change their job search behavior in response to changes in unemployment benefits, this could bias our estimates. We address this concern in two ways.

First, 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

Table 7: Regression Estimates of Out of State Employment

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

Standard errors in parentheses.

*** p<0.01, ** p<0.05, * p<0.1

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:

$$\text{Quasi-difference: } \Delta \tilde{e}_{p,i,t} = \phi_p + \alpha_e \Delta b_{p,i,t} + \Delta \nu_{p,i,t}$$

$$\text{Diff-in-diff: } \Delta e_{p,i,t} = \phi_p + \alpha_e \Delta b_{p,i,t} + \Delta \nu_{p,i,t}$$

where $e_{p,i,t}$ is the fraction of workers that live in county i that work in the state associated with county j also in pair p at time t . The results of the regressions are in Table 7. Using both the quasi-difference and difference-in-difference specification the coefficient on weeks of benefits available is insignificant. We interpret this as suggesting that worker search behavior does not respond significantly to changes in unemployment benefits.

In addition to the direct empirical measure from the ACS, we attempt to impute the worker search decisions using our county-pair data. Then, we can perform the same statistical analyses on our imputed measure of worker search.

However, because integrated labor markets generally contain multiple neighboring counties, instead of focusing on the county pair as the unit of analysis 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 a 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).

In any given period, a worker can be either employed (matched with a firm) or unemployed. In period t , firms in state A post vacancies in state A , v_t^A . An unemployed worker

in state A can direct his search to either state A or state B . The number of new matches in state A in period t equals

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

where \tilde{u}_t^A is the measure of unemployed workers 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

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

to be the market tightness in state A in period t . We define the job-finding and vacancy-filling probabilities as in Section 5.5.

We calculate the separation probability into unemployment, δ_t from the Current Population survey following Shimer (2007).

Given these assumptions, the law of motion for the unemployed who live in states A and B is:

$$u_{t+1}^A = \delta_t (n_A - u_t^A) + u_t^A (1 - x_t^A f(\theta_t^A) - (1 - x_t^A) f(\theta_t^B)), \quad (36)$$

$$u_{t+1}^B = \delta_t (n_B - u_t^B) + u_t^B (1 - x_t^B f(\theta_t^B) - (1 - x_t^B) f(\theta_t^A)), \quad (37)$$

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 search in states i .

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

$$\tilde{u}_t^A = u_t^A x_t^A + (1 - x_t^B) u_t^B, \quad (38)$$

$$\tilde{u}_t^B = u_t^B x_t^B + (1 - x_t^A) u_t^A. \quad (39)$$

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 . These solve, using (36),

$$\phi_t^A = \frac{u_t^A - u_{t+1}^A + \delta_t (n_t^A - u_t^A)}{u_t^A} = x_t^A f(\theta_t^A) - (1 - x_t^A) f(\theta_t^B), \quad (40)$$

$$\phi_t^B = \frac{u_t^B - u_{t+1}^B + \delta_t (n_t^B - u_t^B)}{u_t^B} = x_t^B f(\theta_t^B) - (1 - x_t^B) f(\theta_t^A). \quad (41)$$

The last four equations have 4 unknowns, $x_t^A, x_t^B, f(\theta_t^A), f(\theta_t^B)$ ²⁶. 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 .

²⁶We 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.

Table 8: Effect of UI Benefits on Imputed Search Location Decisions

VARIABLES	Out of state work	
	Quasi-Diff	Diff-in-Diff
Weeks of Benefits	0.0070 (0.049)	-0.104 (0.0729)
Pair Fixed Effects	Yes	No
Observations	21,465	21,465
R-squared	0.066	0.007

Standard errors in parentheses
 *** p<0.01, ** p<0.05, * p<0.1

In order to proceed to identify x_t^A, x_t^B we assume that the matching function is a Cobb-Douglas function, $\mu u^\alpha v^{1-\alpha}$. Note, however, that we do not see the true level of vacancies. However, if we assume that we see the same *fraction*, γ , of total vacancies for both counties in a pair, we can still estimate the effective matching function given our observed vacancies. If we observe $\tilde{v} = \gamma v$, then the total number of matches is $\tilde{\mu} u^\alpha \tilde{v}^{1-\alpha}$ where $\tilde{\mu} = \gamma^{1-\alpha} \mu$. Thus, we propose to identify $\tilde{\mu}$ and α 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 $\alpha, \{\mu_t, x_t^A, x_t^B\}_{t=1}^T$ to minimize the error in the following equations:

$$\phi_t^A = x_t^A f(\theta_t^A) - (1 - x_t^A) f(\theta_t^B), \quad (42)$$

$$\phi_t^B = x_t^B f(\theta_t^B) - (1 - x_t^B) f(\theta_t^A), \quad (43)$$

$$\frac{q(\theta_t^A)}{q(\theta_t^B)} = \left(\frac{\theta_t^B}{\theta_t^A} \right)^\alpha, \quad (44)$$

where we observe all left hand side variables for all t .

The imputation procedure generates $x_{p,i,t}$ for the duration of our sample. Using the quasi-difference estimator we find that regressing the imputed search decision of households on the difference in weeks of benefits available yields an insignificant coefficient. This result is consistent with the evidence from the ACS, again implying that at least during the Great Recession the variation in benefits across border-county pairs did not induce significant changes in decisions to look for work across state borders.

8 Conclusion

In this paper we employed the 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 account for most of the persistently high unemployment after the Great Recession. Coupled with the robust finding in 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 a direct support for this conclusion by documenting a large negative response of job creation 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 its stimulative effect on the local demand, which is hypothesized to have beneficial employment effects. We cannot separately identify this effect, but, we effectively measure the macro effect on unemployment *net of this effect*. Thus, our estimates imply that any stimulative effect is much too small to overcome the dramatic negative effect on employment from the general equilibrium effect of benefit expansion on vacancy creation.

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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 i and p , construct $v_{p,i,t} = \Delta x_{p,i,t} - \beta(1 - \delta)\Delta x_{p,i,t-1} - \alpha_j \Delta b_{p,i,t}$. Then, $v_{p,i,t} = \lambda'_p F_t$ is a pure factor model and can be estimated consistently using principal components.²⁷
 - (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.²⁸

II County Economy

The law of motion for county employment is therefore:

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

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

II.0.1 Value Functions

The flow value for a firm employing a worker is

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

and the flow value of a vacant firm is:

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

²⁷The 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.

²⁸We 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.

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:

$$\begin{aligned} W_t^C(\Omega_t^C) &= w_t^C + \beta(1 - \delta) \mathbb{E}W_{t+1}^C + \beta\delta(1 - e_t(\Omega_t^C)) \mathbb{E}U_{t+1}^{C,E}(\Omega_{t+1}^C) \\ &\quad + \beta\delta e_t(\Omega_t^C) \mathbb{E}U_{t+1}^{C,I}(\Omega_{t+1}^C) \end{aligned} \quad (\text{A4})$$

$$\begin{aligned} U_t^{C,E}(\Omega_t^C) &= h + b + \beta f(\theta_t^C) \mathbb{E}W_{t+1}^C(\Omega_{t+1}^C) + \beta(1 - f(\theta_t^C))(1 - e_t(\Omega_t^C)) \mathbb{E}U_{t+1}^{C,E}(\Omega_{t+1}^C) \\ &\quad + \beta(1 - f(\theta_t^C)) e_t(\Omega_t^C) \mathbb{E}U_{t+1}^{C,I}(\Omega_{t+1}^C) \end{aligned} \quad (\text{A5})$$

$$U_t^{C,I}(\Omega_t^C) = h + \beta f(\theta_t^C) \mathbb{E}W_{t+1}^C(\Omega_{t+1}^C) + \beta(1 - f(\theta_t^C)) \mathbb{E}U_{t+1}^{C,I}(\Omega_{t+1}^C) \quad (\text{A6})$$

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:

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

$$\Phi_t^C(\Omega_t^C) = b + \beta(1 - f(\theta_t^C))(1 - e_t(\Omega_t^C)) \mathbb{E}\Phi_{t+1}^C(\Omega_{t+1}^C) \quad (\text{A8})$$

Wages are determined by Nash bargaining. The wage is chosen to maximize:

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

II.0.2 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 unemployment process satisfies (26)
5. The joint process for (z_t^S, u_t^S) is consistent with the state equilibrium.

III Appendix Figures

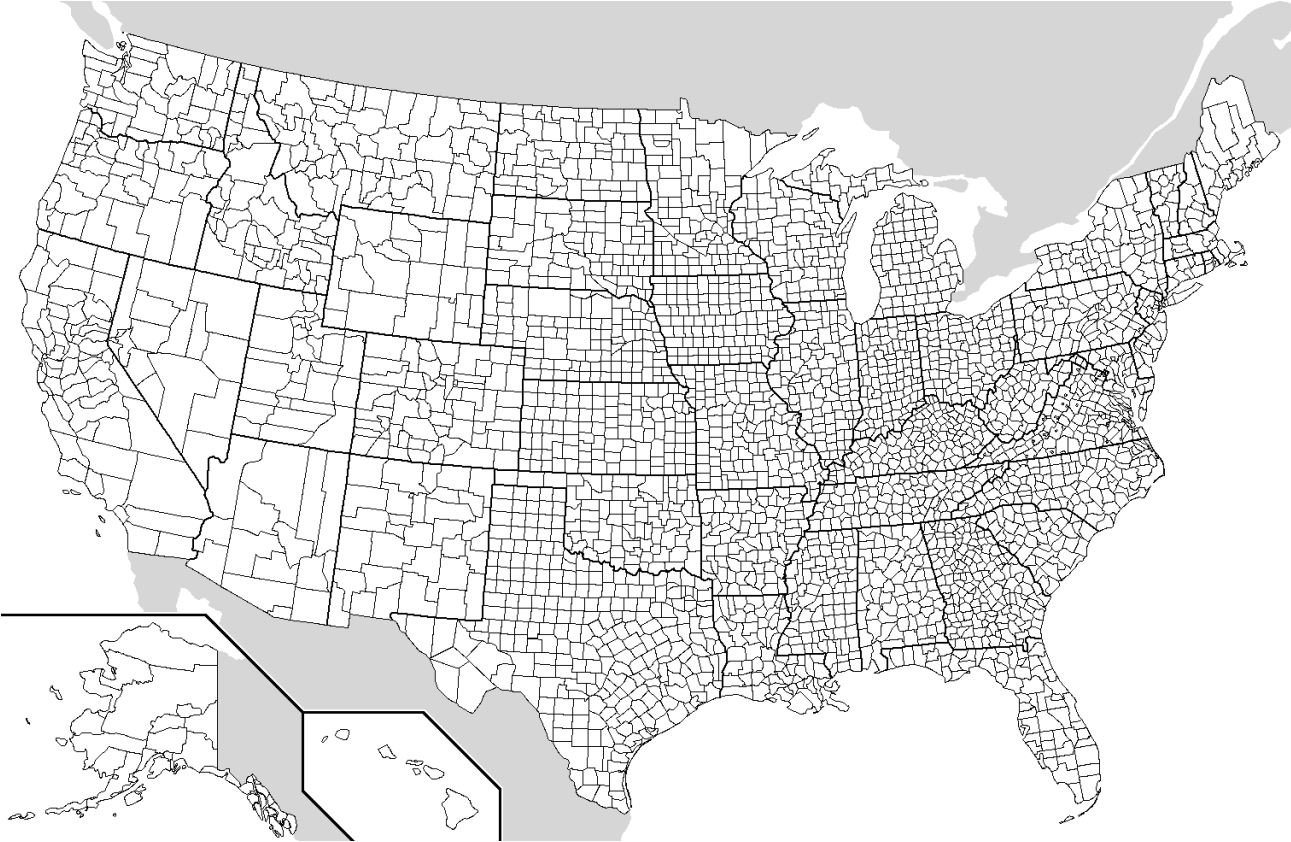
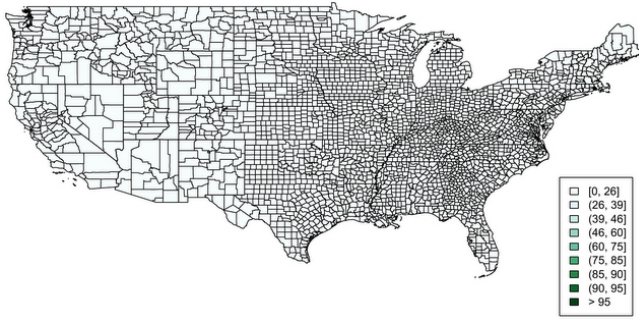
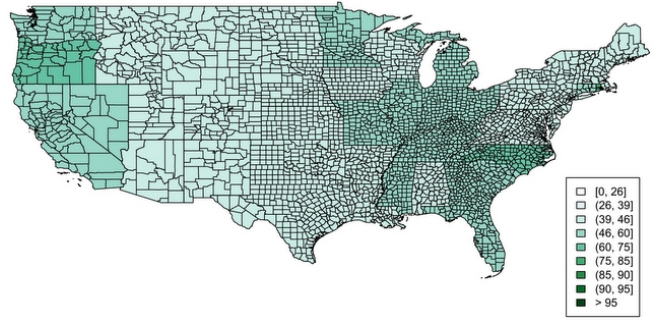


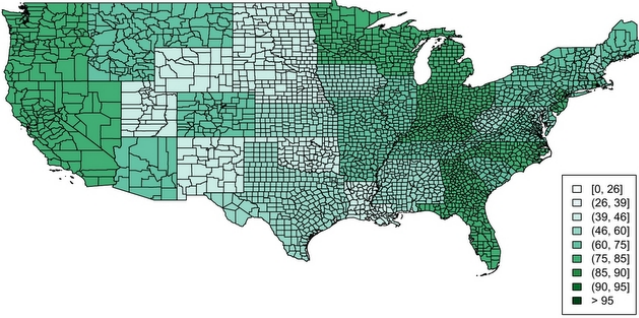
Figure A-1: Map of U.S.A. with state and county outlines.



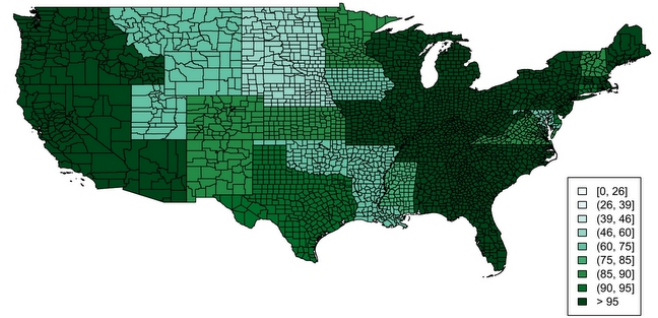
(a) June 2008.



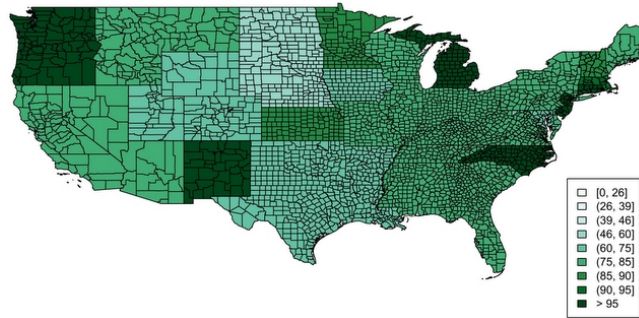
(b) December 2008.



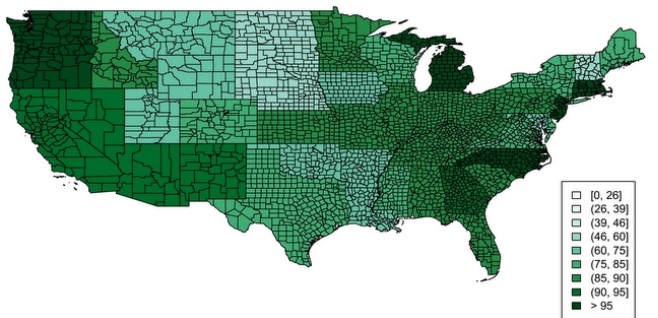
(c) June 2009.



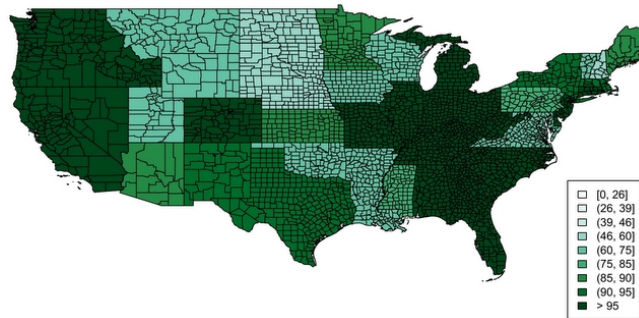
(d) December 2009.



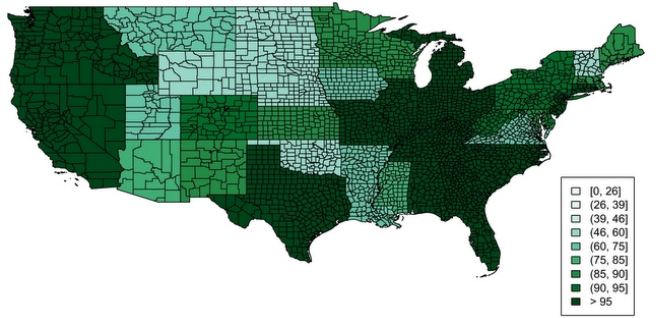
(e) June 2010.



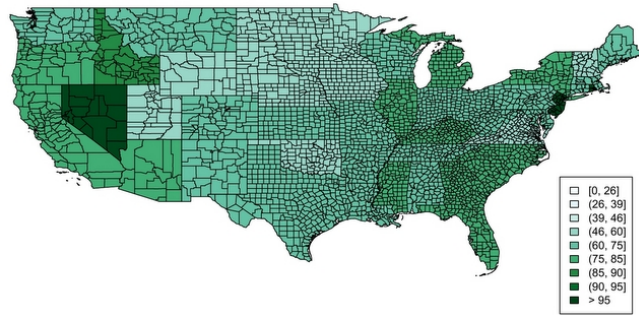
(f) December 2010.



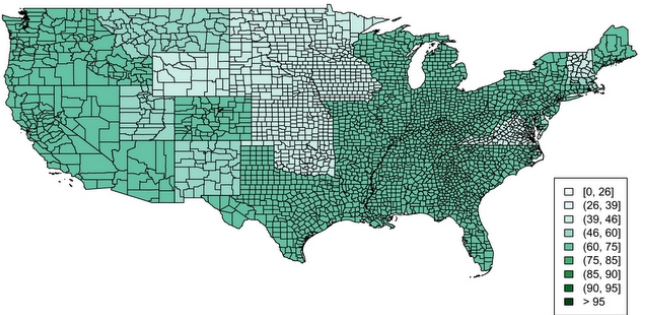
(g) June 2011.



(h) December 2011.



(i) June 2012.



(j) December 2012.

Figure A-2: Unemployment benefit duration across U.S. states during the Great Recession. Selected months.