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ScienceDirect
ELSEVIER Journal of Economic Dynamics & Control 32 (2008) 1120–1155
JOURNAL OF Economic Dynamics & Control
www.elsevier.com/locate/jedc

Business cycles, unemployment insurance, and the calibration of matching models

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Received 20 April 2005; accepted 30 April 2007
Available online 29 May 2007

Abstract

This paper theoretically and empirically documents a puzzle that arises when an RBC economy with a job matching function is used to model unemployment. The standard model can generate sufficiently large cyclical fluctuations in unemployment, or a sufficiently small response of unemployment to labor market policies, but it cannot do both. Variable search and separation, finite UI benefit duration, efficiency wages, and capital all fail to resolve this puzzle. However, either sticky wages or match-specific productivity shocks can improve the model's performance by making the firm's flow of surplus more procyclical, which makes hiring more procyclical too.

JEL classification: C78; E24; E32; I38; J64

Keywords: Real business cycles; Matching function; Unemployment insurance

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doi:10.1016/j.jedc.2007.04.008

1. Introduction

A model of real business cycles with matching (RBCM) is a natural candidate for exploring many dynamic policy issues. Postulating a job matching function helps us give a coherent analysis of unemployment and its response to labor market policies (see Rogerson et al., 2005 for a recent survey of matching models). Moreover, Merz (1995), Andolfatto (1996), and den Haan et al. (2000) have claimed that endogenizing unemployment by means of a matching function improves the fit of real business cycle models. Thus it is tempting to use the RBCM framework to measure the costs of business cycles or the purported benefits of output stabilization, or to ask whether unemployment benefits should vary with the cycle, among other issues.

These questions interest us. But when we tried to build a model to address them, we quickly encountered problems with the RBCM framework which existing literature had not pointed out. For our purposes, we needed a model consistent both with business cycle facts and with the effects of labor market policies. We found it easy to choose parameters to make the cyclical variation in unemployment as large in the model as it is in the data, or to make the response of unemployment to a change in the unemployment insurance (UI) benefit as small in the model as it is in the data. But no calibration permits the standard RBCM model to reproduce both these features: improving the fit over the cycle makes the fit worse with respect to policy, and *vice versa*. Similar problems occur with employment, vacancies, tightness, and the probability of job finding.

These findings are related to a prominent recent controversy. Shimer (2004, 2005) and Hall (2003, 2005a, b) studied the cyclical dynamics of calibrated RBCM models and obtained fluctuations of unemployment and vacancies an order of magnitude smaller than those in the data.[1] The reason is that in their models, productivity shocks cause strong wage movements that offset the incentive to vary hiring, thus eliminating most fluctuations in unemployment and vacancies. As a corollary, they also found that a model with sticky wages, instead of the more traditional Nash wage bargaining framework, does a better job of reproducing labor market fluctuations.

While our observations are related to those of Shimer and Hall, we feel that an important element is missing in their argument, because their claim that unemployment is insufficiently variable in the RBCM model is not true in general. In fact, it is specific to their particular calibration: Shimer and Hall both assume that workers' cost of working is low compared to their productivity, so that the match surplus is large. When this restriction is removed, it is easy to make unemployment volatile. If the surplus is small on average, then a small fall in labor productivity may eat up a large proportion of the surplus, so that realistic productivity fluctuations generate arbitrarily high variability in vacancies, unemployment, and tightness. Stated differently, if the cost of working is acyclical, and is on average only slightly less than after-tax labor productivity, then wages will be relatively rigid and profits and hiring incentives will be strongly procyclical.

[1] An early paper anticipating Shimer and Hall's results is Millard et al. (1997).

The observation that employment is volatile in the RBCM model if the surplus is small has been made again more recently by Hagedorn and Manovskii (2006) in a sharp critique of Shimer and Hall's claims. However, our main point here is that such a calibration only creates another problem. The hiring margin is affected by productivity, taxes, and workers' disutility costs and opportunity costs of labor. If we blow up the impact of productivity by making the surplus small on average, then hiring becomes extremely sensitive to taxes and labor market policies too. We demonstrate analytically in a simple benchmark RBCM model that the responses of unemployment to productivity shocks and to policy variables cannot be simultaneously reconciled with the data. We go on to show numerically that this problem remains when the model is extended in several ways not considered by Shimer and Hall, and is also present but undiagnosed in previous papers.

The recent survey of Hornstein et al. (2005a) concludes, like us, that solving the unemployment fluctuation problem by making the surplus small is likely to exaggerate the model's policy effects. However, while cyclical labor market dynamics have been extensively documented in recent papers, the policy effects that underlie the other half of our argument are more controversial. Therefore, we also perform a detailed robustness analysis of the best-known cross-country policy regressions. We conclude that the effects of UI benefits and taxes are quite robustly identified by both cross-sectional and time series evidence, and are approximately equal, as our model implies. Our coefficients are around twice as large as those of Nickell and Layard (1999), and imply that benefits and taxes have economically important effects, but are still much too small to be reconciled with the cyclical volatility of unemployment in the standard RBCM model.

Finally, our paper also discusses two possible solutions of our 'puzzle'. Shimer (2004) and Hall's (2005a, b) argument that sticky wages help by making firms' share of surplus more procyclical also helps resolve our puzzle, as long as wages eventually adjust to long run policy changes. However, we also identify a real mechanism that can reconcile the cyclical and policy-related variation in unemployment. Embodied (that is, match-specific) technological change also increases the cyclicality of the match surplus, especially for the firm, without changing long run policy impacts. While we focus on one particular puzzle for the RBCM model, and propose one new solution, several other recent papers make related points. Other empirical criticisms of the RBCM model include Cole and Rogerson (1999), Fujita (2004), and Ravn (2006). Other papers offering ways of improving the model's fit include Mortensen and Nagypál (2006), Silva and Toledo (2005), and Hall and Milgrom (2005).

The next section states our general model. In Section 3, we analytically calculate the relationship between the cyclical variability of unemployment and the effects of UI on unemployment in a tractable special case. In Section 4, we briefly discuss cyclical stylized facts and then carefully study the robustness of cross-country evidence on policy effects, concluding that these two sets of evidence jointly reject our baseline model. Section 5 shows that neither variable search, variable separation, finite UI benefit duration, nor efficiency wages suffice to make the model fit the data, but that sticky wages or match-specific productivity shocks might. In Section 6, we discuss some earlier RBCM papers that are not nested in our analysis (mainly because they allow for physical capital), and show that they are subject to the same critique. Section 7 concludes.

2. The model

Our general model is a version of the standard RBCM model, as spelled out in Pissarides (2000) and elsewhere. We simplify by ignoring physical capital; including it would be likely to reinforce our 'puzzle', since capital can more easily adjust to long term policy changes than to short term business cycle fluctuations.[2] In hopes of finding a successful version of the model, we generalize in several ways: we allow productivity to vary across matches, and we allow separation rates and bargaining power to vary too.

2.1. Values and surpluses

Let Z be a shock to the productivity of the economy, and let z be the value of this shock at the time when a given job was formed. We consider a labor productivity process y that allows the output of a match to depend on its vintage:

$$y(z, Z) = 1 + \alpha_Z Z + \zeta(1 - \alpha_Z)z. \tag{1}$$

In the usual RBC specification ($\alpha_Z = 1$), aggregate productivity fluctuates because technology shocks immediately affect all matches. But alternatively, technological progress could require the creation of new jobs. In that case, productivity would have a match-specific or cohort-specific component, which would be consistent with Devereux's (2003) evidence that workers tend to find persistently better matches in booms. Setting $\alpha_Z = 0$ attributes all fluctuations in aggregate productivity to this cohort-specific component. The parameter ζ allows us to adjust the impact of the match-specific shock z relative to the aggregate shock Z.

It is well known that in matching models without a capital stock, surpluses and most decision variables are independent of the unemployment rate. Without mentioning unemployment, we can write transition probabilities in terms of labor market tightness, which in turn depends on productivity. To save on notation, we immediately impose these restrictions by writing the value and policy functions in terms of their appropriate state variables. Later we point out why these restrictions are valid.

If the after-tax wage is $w(z, Z)$, and the discount rate is β, then an employed worker's value, $W^E(z, Z)$, is

$$W^E(z, Z) = w(z, Z) + \beta E_{Z'|Z}[(1 - \delta(z, Z))W^E(z, Z') + \delta(z, Z)W^U(Z')]. \tag{2}$$

[2]See Section 6. We also simplify by ignoring two other generalizations that are unlikely to resolve the dilemma at hand. One might want to consider procyclical labor market distortions (since both UI benefits and taxes are typically increasing in the wage) or procyclical hiring costs (since the cost of hiring may consist mostly of labor time). However, these factors would only make firms' hiring incentives *less* procyclical, so they are not likely to help resolve the puzzle that concerns us.

We generalize by allowing the separation rate δ to depend on productivity. We will see that the probability of finding a job can be written as $p(s,\theta)$, where s is search effort and θ is labor market tightness. Therefore the value $W^U(Z)$ of unemployment is

$$W^U(Z) = \max_s \{b - h(s) + \beta E_{Z'|Z}[p(s,\theta(Z)) W^E(Z',Z') + (1 - p(s,\theta(Z))) W^U(Z')]\}. \tag{3}$$

Here b represents the UI benefit, though in general it should also be understood to capture other costs of working, such as disutility costs. The term $h(s)$ represents the costs of searching. Most of the time we will fix $s \equiv 1$ and $h(1) \equiv 0$. But we will also consider the variable search case, in which the following first-order condition holds:

$$h'(s) = \beta \frac{\partial p(s,\theta(Z))}{\partial s} E_{Z'|Z} \Sigma^W(Z',Z'), \tag{4}$$

where Σ^W is a worker's surplus from being employed. We assume h' is increasing in s and $\partial p/\partial s$ is weakly decreasing in s, so that (4) has a unique solution for any Z.

Workers' surplus is defined as the difference between the values of employment and unemployment; it satisfies

$$\begin{aligned}\Sigma^W(z,Z) &= W^E(z,Z) - W^U(Z) \\ &= w(z,Z) - b + h(S(Z)) + \beta E_{Z'|Z}[(1-\delta(z,Z))\Sigma^W(z,Z') \\ &\quad - p(S(Z),\theta(Z))\Sigma^W(Z',Z')],\end{aligned} \tag{5}$$

where $S(Z)$ either represents the optimal search intensity defined by (4), or equals one if search is exogenous.

The value to the firm of a filled job, $J(z,Z)$, satisfies the recursive equation

$$J(z,Z) = \Sigma^F(z,Z) = y(z,Z) - w(z,Z) - \tau + \beta(1-\delta(z,Z)) E_{Z'|Z} J(z,Z'), \tag{6}$$

where τ represents total labor taxes on the worker and the firm. Unlike a worker's job acceptance decision, filling a job is assumed (as usual) to have no opportunity cost in terms of lost hiring opportunities, so that the surplus $\Sigma^F(z,Z)$ associated with a filled job is the same as the value of that job. In other words, firms offer new jobs until the expected profits associated with a vacancy are zero. If the probability of filling a job is $p^F(S,\theta)$, where S is average search intensity, then the zero profits condition is

$$\kappa = \beta p^F(S,\theta(Z)) E_{Z'|Z} \Sigma^F(Z',Z'), \tag{7}$$

where κ is the flow cost of maintaining a vacancy.

The wage is determined by the Nash bargaining condition

$$\frac{\Sigma^W(z,Z)}{\Sigma^F(z,Z)} = \frac{\mu(Z)}{1-\mu(Z)}. \tag{8}$$

Here we generalize again, by letting workers' bargaining power μ vary with the aggregate state.

2.2. The labor market

We assume that total matches M are given by

$$M = \gamma V^{1-\lambda} U^{\lambda} S, \tag{9}$$

where V is total vacancies, and U is unemployment. Tightness is defined as $\theta \equiv V/U$, so that it depends on unemployment U instead of total search effort US, which is not observable. Matching probabilities can then be written in terms of tightness and search:

$$p^F(S,\theta) \equiv \frac{M}{V} = \gamma\theta^{-\lambda} S \tag{10}$$

and

$$p(s,\theta) \equiv \frac{M}{US} s = \gamma\theta^{1-\lambda} s. \tag{11}$$

Eq. (11) implicitly provides a metric for search effort, saying that the individual probability of finding a job is proportional to search.

In equilibrium $s = S = S(Z)$, and we can use (10) and (11) to eliminate p^F and p. We are then left with the five equations (4)–(8) to determine the five functions $S(Z)$, $\Sigma^W(z,Z)$, $\Sigma^F(z,Z)$, $\theta(Z)$, and $w(z,Z)$, without reference to unemployment U. Thus it is reasonable to look for a solution of these equations that is independent of U.

As we define the labor market dynamics of our model, we must note that $\alpha_Z < 1$ implies a distribution of match productivities. To deal with this effect in the simplest possible way, in Section 4 where we allow $\alpha_Z < 1$ we will assume that productivity follows a two-state Markov process, taking a low value Z^{LO} or a high value Z^{HI}. We then distinguish between the fraction of the labor force in matches with low productivity, N_t^{LO}, and the fraction matched with high productivity, N_t^{HI}. Total employment plus unemployment must sum to one:

$$N_t + U_t \equiv N_t^{\mathrm{HI}} + N_t^{\mathrm{LO}} + U_t = 1. \tag{12}$$

If we write total matches at time t as $M_t \equiv \gamma\theta(Z_t)^{1-\lambda} S(Z_t) U_t$, then the three labor market state variables follow the dynamics:

$$N_{t+1}^{\mathrm{HI}} = (1 - \delta(Z^{\mathrm{HI}}, Z_t)) N_t^{\mathrm{HI}} + M_t \mathbf{1}(Z_{t+1} = Z^{\mathrm{HI}}), \tag{13}$$

$$N_{t+1}^{\mathrm{LO}} = (1 - \delta(Z^{\mathrm{LO}}, Z_t)) N_t^{\mathrm{LO}} + M_t \mathbf{1}(Z_{t+1} = Z^{\mathrm{LO}}), \tag{14}$$

$$U_{t+1} = \delta(Z^{\mathrm{LO}}, Z_t) N_t^{\mathrm{LO}} + \delta(Z^{\mathrm{HI}}, Z_t) N_t^{\mathrm{HI}} - M_t + U_t, \tag{15}$$

where $\mathbf{1}(x)$ is an indicator function equalling 1 if statement x is true, and 0 if x is false. In these equations, total job destruction is $D_t = \delta(Z^{\mathrm{HI}}, Z_t) N_t^{\mathrm{HI}} + \delta(Z^{\mathrm{LO}}, Z_t) N_t^{\mathrm{LO}}$. Finally, since there is no capital stock, aggregate output Q_t is

$$Q_t = (1 - U_t)(1 + \alpha_Z Z_t) + \zeta(1 - \alpha_Z)(N_t^{\mathrm{HI}} Z^{\mathrm{HI}} + N_t^{\mathrm{LO}} Z^{\mathrm{LO}}). \tag{16}$$

3. Unemployment volatility: cycles and policies

We now consider the simplest and most standard version of this model, in which productivity is disembodied ($y = 1 + Z$), and the separation rate δ and bargaining power μ are constant.[3] For this special case, we can characterize the dynamics explicitly, and calculate how the cyclical variability of labor market aggregates relates to their response to UI policy.

Define total surplus as $\Sigma_t \equiv \Sigma_t^F + \Sigma_t^W$. Summing Eqs. (5) and (6), and using the fact that the worker's share of surplus is μ, we see that Σ must satisfy

$$\begin{aligned}\Sigma_t &= y_t - b - \tau + h(S_t) + \beta(1-\delta)E_t\Sigma_{t+1}^F + \beta(1-\delta-p_t)E_t\Sigma_{t+1}^W \\ &= y_t - b - \tau + h(S_t) + \beta(1-\delta-\mu p_t)E_t\Sigma_{t+1},\end{aligned} \tag{17}$$

where $h(S) = 0$ if search is exogenous. We already see that productivity, UI benefits, and labor taxes will affect match surplus in closely related ways, which is the key to our results. In addition, we have the zero profit condition

$$\kappa = \beta p_t^F E_t J_{t+1} = p_t^F (1-\mu) E_t \Sigma_{t+1}. \tag{18}$$

In Eqs. (17) and (18), $p_t = \gamma S_t \theta_t^{1-\lambda}$ and $p_t^F = \gamma S_t \theta_t^{-\lambda}$ depend only on tightness θ_t and search effort S_t. Thus when search is exogenous, (17) and (18) suffice to determine total surplus Σ_t and tightness θ_t.

In the endogenous search case, the first-order condition (4) plus the zero profit condition (18) allow us to eliminate search in favor of tightness:

$$\frac{\kappa\theta_t}{(1-\mu)} = \frac{h'(S_t)S_t}{\mu}. \tag{19}$$

Since $h(S)$ is convex, (19) implies a positive relation $S(\theta)$ between search and tightness: people search harder when jobs are easier to find. We write the elasticity of S as $\eta_\theta^S(\theta) \equiv (1 + h''(S(\theta))S(\theta)/h'(S(\theta)))^{-1}$. In what follows, we will assume that search costs $h(S)$ are small on average, but are sufficiently convex so that job finding responds relatively inelastically to θ. This guarantees existence of a unique equilibrium, and as we will see shortly, large search costs or highly elastic search effort would have counterfactual implications.

3.1. Steady state

In the nonstochastic steady state (indicated by dropping the subscript t), Eqs. (17) and (18) give two different expressions for Σ. Substituting for p and p^F, we have

$$\Sigma = \frac{\beta\kappa\theta^\lambda}{\gamma S(1-\mu)} = \frac{y - b - \tau + h(S)}{1 - \beta(1 - \delta - \mu\gamma S\theta^{1-\lambda})}. \tag{20}$$

If S is exogenous, then the left-hand side is increasing in θ, and the right-hand side is decreasing in θ, so there exists a unique steady state for θ and Σ. In the case of

[3] To simplify notation, we now use the time subscript t to denote dependence on the aggregate state Z_t (and also on U_t where appropriate).

endogenous search, we assume S is sufficiently inelastic so that the same conclusions hold. In particular, we assume the left side of (20) is increasing in θ, which requires

$$\lambda^* \equiv \lambda - \eta_\theta^S(\theta) > 0. \tag{21}$$

We can use (20) to derive the comparative statics of θ in terms of UI and taxes. To keep the results unit-free, it will be helpful to do our calculations in terms of the unitless variable $\xi \equiv b/y$, which we will call the 'replacement ratio', though more precisely it is the steady state ratio of UI benefits to labor productivity.[4] Likewise, we will calculate the effects of taxes in terms of the unitless variable τ/y, which we call the 'tax wedge'. Now, let hats represent changes in the log of the steady state. Eq. (20) implies

$$\begin{aligned}\lambda\hat{\theta} - \hat{S} = &-\frac{b}{y - b - \tau + h(S)}\hat{b} - \left(\frac{\beta\mu p}{1 - \beta(1 - \delta - \mu p)}\right)[(1-\lambda)\hat{\theta} + \hat{S}] \\ &+ \frac{h(S)}{y - b - \tau + h(S)}\eta_S^h(S)\hat{S},\end{aligned} \tag{22}$$

where $\eta_S^h(S) \equiv h'(S)S/h(S)$. We simplify, using (20) again, and writing the equations in terms of $\hat{p} = (1 - \lambda^*)\hat{\theta}$. Then the elasticity of the job finding probability with respect to the replacement ratio, $\eta_\xi^p \equiv \hat{p}/\hat{\xi} = \hat{p}/\hat{b}$, is

$$\eta_\xi^p = -\frac{1-\lambda^*}{\lambda^*}\left(\frac{b}{y - b - \tau + h}\right)\left(\frac{1 - \beta + \beta\delta + \beta\mu p}{1 - \beta + \beta\delta + \beta\mu p/\lambda^* - h\eta_S^h\eta_\theta^S/(\lambda^*\Sigma)}\right) < 0. \tag{23}$$

The steady state effect of the replacement ratio on unemployment is approximately the opposite of its effect on the job finding probability p. In steady state, unemployment satisfies $\delta(1 - U) = pU$, which implies

$$\eta_\xi^U \equiv \frac{\hat{U}}{\hat{\xi}} = -(1 - U)\frac{\hat{p}}{\hat{\xi}} = -(1 - U)\eta_\xi^p > 0. \tag{24}$$

Eqs. (23) and (24) show that $\lambda^* > 0$ is necessary for UI to affect unemployment positively, as observed in the data; this justifies assumption (21).

3.2. Dynamics

Now consider the dynamics. Suppose that $y_t = 1 + Z_t$ is AR1 in logs:

$$\tilde{y}_{t+1} = \rho\tilde{y}_t + \varepsilon_{t+1}, \tag{25}$$

where ε is *i.i.d.* with $E_t\varepsilon_{t+1} = 0$, and $\rho \in (0, 1)$. (Now tildes signify log deviations from steady state, and unadorned variables are steady state values or constants.) If we linearize the surplus dynamics (17) and the zero profit condition (18) and impose

[4]In steady state, the difference between our 'replacement ratio' $\xi \equiv b/y$ and the true UI replacement ratio b/w is small; we have verified numerically that the quantitative impact of using b/y instead of b/w is trivial.

saddle path stability, we find an explicit formula for the dynamics of the job-finding probability, in terms of the productivity shock:

$$\frac{\tilde{p}_t}{\tilde{y}_t} = \frac{1-\lambda^*}{\lambda^*}\left(\frac{y}{y-b-\tau+h}\right)\left(\frac{1-\beta+\beta\delta+\beta\mu p}{1/\rho-\beta+\beta\delta+\beta\mu p/\lambda^*-h\eta_S^h\eta_\theta^S/(\lambda^*\Sigma)}\right). \tag{26}$$

The close resemblance between (23) and (26) will help us test the model. Intuitively, the model says that a permanent increase in UI or taxes should have exactly the same effect on the surplus process, and therefore on hiring, as a permanent decrease in productivity by the same amount. Since equivalent changes in b, τ, and y mean changes by the same absolute amount (instead of equal percentage changes), the clearest way to express our results will be in terms of semielasticities (instead of elasticities).[5] Writing the semielasticity of job finding with respect to the replacement ratio as $\varepsilon_\xi^p \equiv \eta_\xi^p/\xi$, we can use (23) and (26) to obtain:

Proposition 1. *The dynamic elasticity of the probability of job finding with respect to productivity, and the long-run semielasticity of the probability of job finding with respect to the replacement ratio* ξ, *have the following ratio in absolute value*:

$$\left|\frac{\tilde{p}_t/\tilde{y}_t}{\varepsilon_\xi^p}\right| = \left(\frac{1-\beta+\beta\delta+\beta\mu p/\lambda^*-h\eta_S^h\eta_\theta^S/(\lambda^*\Sigma)}{1/\rho-\beta+\beta\delta+\beta\mu p/\lambda^*-h\eta_S^h\eta_\theta^S/(\lambda^*\Sigma)}\right) \leqslant 1. \tag{27}$$

This ratio equals one if and only if $\rho = 1$, and is strictly less than one if $\rho < 1$. That is, a *permanent* increase in labor productivity has the same effect on hiring as a permanent decrease in UI benefits by the same absolute amount, while a *temporary* rise in labor productivity would have a smaller impact. Endogenous search leaves this ratio unchanged if $\rho = 1$, and makes it smaller if $\rho < 1$, because the search term $h\eta_S^h\eta_\theta^S/(\lambda^*\Sigma)$ decreases the numerator proportionally more than the denominator. Also, it is easy to verify that exactly the same formula can be derived if we replace η_ξ^p by the semielasticity of job finding with respect to the tax wedge.

For comparison with the data it is helpful to translate Proposition 1 into a statement about unemployment. Turning to the dynamics of U, we have

$$U_{t+1} = U_t + \delta(1-U_t) - \gamma S_t\theta_t^{1-\lambda}U_t. \tag{28}$$

In the Appendix we calculate the ratio of the standard deviations of the logs (the usual business cycle volatility measure) of unemployment and the technology shock, which we can then compare to the semielasticity $\varepsilon_\xi^U \equiv \partial \log U/\partial\xi$ of unemployment with respect to the replacement ratio. Using the notation $\sigma_x \equiv \sqrt{\mathrm{Var}(\tilde{x}_t)}$, we obtain:

Proposition 2. *The relative standard deviation of log unemployment to log output, and the long-run semielasticity of unemployment with respect to the replacement ratio* ξ,

[5]Another crucial reason to state our results in terms of semielasticities is that our model's b should actually be interpreted as the sum of the UI benefit (observed) and the disutility of working (unobserved). The semielasticity of unemployment with respect to the total cost of working b is the same as the semielasticity with respect to observed UI benefits. In contrast, the *elasticity* with respect to b cannot be directly estimated without assumptions about the size of the unobserved disutility component.

have the following ratio:

$$\frac{\sigma_U/\sigma_Q}{\varepsilon_\xi^U} = \frac{(\sigma_y/\sigma_Q)(\sigma_U/\sigma_y)}{\varepsilon_\xi^U} = \left|\frac{\tilde{p}_t/\tilde{y}_t}{\varepsilon_\xi^p}\right| \left(\frac{\delta(U+\rho(U-\delta))}{(2U-\delta)(U+\rho(\delta-U))}\right)^{1/2} \frac{\sigma_y}{\sigma_Q}. \tag{29}$$

The left side of Proposition 2 is easily observable. The relative volatility σ_U/σ_Q can be calculated from standard macroeconomic data; and following Nickell and Layard (1999), we will regress log unemployment on the replacement ratio across countries to estimate ε_ξ^U. On the right side, the first term is strictly less than one unless technology shocks are permanent. The second term is less than or equal to one if $U > \delta$, which is true if and only if $\delta + p < 1$. Thus this restriction is satisfied unless we choose an inappropriately long period (a Cobb–Douglas matching model like this is not well behaved if periods are so long that transition probabilities are near one). The last term is less than one in the data, and it cannot exceed one in our model except in the irrelevant case of a large positive correlation between y and U. Thus for any sensible parameters, all three terms on the right-hand side are weakly less than one, strictly so in the case of the last term.[6]

4. Empirical evidence

We have shown that the RBCM framework implies a tight relationship between cyclical and policy-related variation in unemployment and other labor market variables. Next, we briefly discuss labor market fluctuations (which have been extensively reviewed elsewhere recently), and then explore the effects of labor market policies in greater detail.

4.1. Unemployment over the business cycle

For evidence on cyclical fluctuations, we consider US data from 1951:1 to 2006:2 from the St. Louis Fed's FRED database, either using quarterly series, or monthly series aggregated to quarterly frequency. We use series GDPC1 for our measure of real output, UNEMPLOY for the number of unemployed workers, the advertising index HELPWANT for vacancies, and UEMPMED for median unemployment duration. All series discussed below are seasonally adjusted, logged, and detrended with the HP filter, unless otherwise specified. Following Shimer (2005), we set the HP smoothing parameter to 100 000, because otherwise the implied HP unemployment trend comoves strongly with the NBER-identified business cycle. Thus, let σ_X denote the standard deviation of the HP cyclical component of the log of variable X. In our sample, the volatility of log GDP, Q, is $\sigma_Q = 0.0252$. By contrast, log unemployment U fluctuates almost eight times as much: $\sigma_U = 0.1933$, giving the

[6]We should emphasize that this result is independent of the mean unemployment rate U. In the numerator, σ_U is approximately the standard deviation of unemployment divided by U. In the denominator, $\varepsilon_\xi^U \approx U^{-1}\partial U/\partial\xi$. So U^{-1} cancels, meaning our results do not depend on how we calibrate mean U, and also do not depend on using logs rather than levels of U.

ratio $\sigma_U/\sigma_Q = 7.66$.[7] Similarly, the log of the median unemployment spell duration has a standard deviation of 0.1732 after HP filtering. Vacancies V are also highly volatile: $\sigma_V = 0.1974$.

Another striking labor market fact is the robust negative correlation between the cyclical components of log unemployment and log vacancies, -0.884 in our data. Given this correlation, the tightness ratio $\theta = V/U$ is even more volatile than the two series separately: $\sigma_\theta = 0.3736$. By contrast, employment N, wages w, and labor productivity y are even smoother than GDP: $\sigma_N = 0.0137$, $\sigma_w = 0.0140$, and $\sigma_y = 0.0164$ using FRED series CE16OV, COMPRNFB, and OPHNFB.

The key point here is the high volatility of unemployment (and vacancies and tightness) relative to its own mean.[8] This robust finding has been discussed in many other studies; see for example Merz (1995), Cole and Rogerson (1999), and Gomes et al. (2001) for similar second moments. Shimer (2005) reports that workers' job finding probability p is also volatile, with $\sigma_p = 0.118$.

4.2. Literature on labor market policy and unemployment

Our model's predictions about labor market policy are general equilibrium results, like its implications for business cycles. So to test them, we need to see whether policy differences cause aggregate changes in unemployment and vacancies. One way to do this might be to look at policy changes over time in a single country. But since large policy changes are rare, in practice this strategy boils down to case studies of major reforms that act as 'natural experiments'.[9] Unfortunately, such studies are few and far between, and their reliance on unique events makes them hard to interpret. Alternatively, it might seem useful to estimate how different policy treatments affect individual labor market outcomes in microdata. Studies of this kind are plentiful but are not directly relevant since they only identify the partial equilibrium effect of policy on workers' choices. Layard et al. (1991) survey argues that the consensus range of estimates for the elasticity of unemployment duration with respect to UI benefits is 0.2–0.9; this is only relevant for us insofar as it is smaller than the general equilibrium estimates we will see below.

Thus, by a process of elimination, we believe that the best evidence on these issues comes from international cross-sectional or panel-data studies. Adequate data for this purpose have been compiled by the OECD for many of its member countries, often going back to 1960. For purposes of comparison, we will take as our benchmark the methodology and estimates of Nickell and Layard (1999, henceforth

[7] Setting the HP smoothing parameter to 1600 only strengthens our results, raising σ_U/σ_Q to 7.87, because lowering the parameter decreases σ_Q more than σ_U.

[8] It is quantitatively unimportant whether we make this point in terms of the log of the number unemployed, the log of the unemployment rate, or the mean and standard deviation of the unemployment rate. Studying the log or the coefficient of variation of unemployment might seem strange to those accustomed to frictionless models where unemployment is just a residual. But the log-linear matching technology in the RBCM framework places strong restrictions on the fluctuation of unemployment, relative to its mean, which are central to calibrating the model.

[9] See for example Solon (1985), Hunt (1995), and Bennmarker et al. (2007).

LN99), who used OECD data to estimate regressions of the form

$$\ln U_{it} = \alpha_0 + \alpha_i + \alpha_t + x_{it}'\beta^x + b_{it}\beta^b + \tau_{it}\beta^\tau + v_{it} \tag{30}$$

over countries i and time periods t. They find that the semielasticity β^b of unemployment U_{it} with respect to the UI benefit replacement ratio b_{it} is 1.3, with a standard error of 0.5.[10] Similar results, for related data sets, are reported by Layard et al. (1991), Scarpetta (1996) and Disney (2000). While some studies have argued that the effects of UI are smaller (Baker et al., 2003 claim that the UI effects in OECD data are not significant), the largest cross-country estimates of UI effects in these papers are still of the same order of magnitude as those in LN99.

Some recent studies are more ambitious, addressing higher-frequency data and attempting to identify interaction terms. Blanchard and Wolfers (2000) study how institutions interact with shocks; Belot and van Ours (2004) interact different institutions; Nickell et al. (2005) use annual data to identify both institutional interactions and country-specific trends. Baker et al. (2003, 2004) provide an excellent overview and critique of these and other recent studies (Elmeskov et al., 1998; Fitoussi et al., 2000; Bertola et al., 2001). While these papers' estimates vary widely, the important point for our purposes is that none of them find substantially larger effects of unemployment benefits than those we estimate.[11]

4.3. Possible problems with cross-country estimates

Cross-country regressions to measure the impact of policy are frequently criticized,[12] for at least three good reasons. First, the number of countries and periods is often small, and data on institutions and policy variables may be of poor quality. More precisely, since each country has its own statistical agency, data definitions may differ, and some observations may be missing. Second, a fully structural estimate of the effects of policy and institutions would probably require additional variables on which no data exist at all. Therefore, cross-country studies estimate reduced form relationships, leaving reasonable doubts about robustness to econometric variations. Third, results may be biased due to endogeneity. Rather than identifying the effects of policy on unemployment, regressions could capture reverse causation from unemployment to changes in policy. Our empirical work aims to investigate how the results of LN99 and related papers hold up in the face of these three lines of criticism.

[10]Here x_{it} is a vector of labor market policy variables and other controls in a panel of OECD countries. LN99 run GLS in order to allow for random country effects α_i. They treat 1983–1988 and 1989–1994 averages as two observations, and include a fixed time dummy α_t for the time period 1989–1994 only, so that the constant α_0 represents the 1983–1988 mean.

[11]The largest estimate obtained in these papers is that of Elmeskov et al. (1998), who find that a 10 percentage point rise in the replacement ratio would raise unemployment by 1.29 percentage points. See the summary table Baker et al. (2003, p. 36).

[12]E.g. Levine and Renelt (1992) and Durlauf and Quah (1999) in the growth literature. Hagedorn and Manovskii (2006) reject out of hand the use of cross-country data to evaluate policy effects.

The issues of data inconsistency across countries and of omitted cross-sectional regressors motivate us to include country effects α_i. Omitted influences on global economic conditions motivate time effects α_t. Unfortunately, estimating both time and country effects is quite demanding on our relatively small data set, so our estimated coefficients sometimes become unstable when both α_i and α_t are treated as fixed effects. Thus, both to avoid estimating so many coefficients and to explore robustness, we consider several alternative ways of controlling for time and country effects. Besides country dummies, we also try random effects or cluster corrections for the cross-sectional dimension; besides time dummies, we also try controlling for the aggregate business cycle in the time dimension.

Many additional checks mitigate our remaining concerns about data quality and robustness. We use a longer time sample than LN99, and we compare regressions for two different data sets. We also try different sample periods, exclusion of possible outliers, and the exclusion of several combinations of regressors. Also, we exploit our model's prediction that the tax and benefit coefficients should be equal ($\beta^b = \beta^\tau$), which lets us estimate the coefficient on their sum $\tau_{it} + b_{it}$ for increased efficiency. As for the endogeneity problem, the main concern is that budgetary pressures could force countries with persistently high unemployment to lower benefits and raise taxes. Since these pressures go in opposite directions, the coefficient on the sum $\tau + b$ is less likely to be biassed than those on taxes and benefits separately (another good reason to regress on the sum). However, we also try controlling for endogeneity by running IV and GMM specifications that instrument labor market policies by their lags. The results are largely robust to this and other specification changes.

4.4. Impact of benefits and taxes on unemployment

We now report a variety of cross-country regressions to estimate how the UI replacement ratio and the tax wedge affect log unemployment. We base our regressions on those of LN99, estimating variants of (30). Like LN99, we try to avoid complex time series methods through time aggregation: we average all variables over five year periods before estimation. But we also extend their results in several ways. First, by studying a long sample, from 1960 to 1999, we compare the implications of the cross-sectional and time series variation in our data. Second, we run many robustness checks, especially with respect to sample period, possible outliers, and endogeneity. Third, we test and then impose our model's restriction that the coefficients on taxes and benefits should be equal, thus improving the stability of our estimates.

We use two data sources: the Labor Market Institutions Database of Nickell and Nunziata (2001), and an expansion of this dataset, extended to 1999 and including more series, constructed by the IMF (2003) and by Baker et al. (2003). Except where otherwise noted, the results are based on the latter. The dependent variable is the natural log of the five-year average of the unemployment rate U_{it}. The regressors either include the tax wedge τ_{it} and the UI benefit replacement ratio b_{it} separately, or the sum of the two. We usually also include indices of benefit duration, employment protection, union density, and bargaining coordination, and the percent of households who are owner-occupiers. Some regressions also include an active labor market policy index (ALMP), or the cross-country mean of the output gap. Data sources and definitions are described in Appendix B, which also provides a web address where our data are available.

Our benchmark estimates are reported in Table 1, which compares fixed effects and random effects specifications. When possible, we use the same variables as LN99, scaling them all (except benefit duration, which is defined differently in their paper) to make our coefficients directly comparable to those reported in their Table 15. The first column shows an OLS estimate that includes country dummies; the tax coefficient (semielasticity 2.09) is smaller than that of LN99, while that on UI benefits (semielasticity 3.09) is slightly over twice their estimate. As in LN99 and some bargaining models, nationwide bargaining coordination has a negative effect on unemployment; and the percentage of households that are owner occupiers has a significant positive effect on unemployment, which makes sense if countries with more rental housing attain greater labor market flexibility. One notable difference between this regression and those of LN99 is that we do not obtain significant effects of benefit duration, employment protection, or union density.

Of course, we should not only control for unobserved country-specific factors α_i, but also for global economic conditions α_t. Unfortunately, the least bias-prone way of doing this – by including both time and country dummies in the regression – causes the coefficients to become unstable, as we see from the strong negative effect of taxes on unemployment in Table 1, column 3. Even though this sign is significant, we believe it is spurious, reflecting the difficulty of estimating so many coefficients in a panel of only 19 countries. Therefore, we also try controlling for time and country effects in other ways that discard less of the explanatory power in the data. First, in the country dimension, we try random effects estimation (last four columns of the table) instead of fixed effects, so that information on cross-country means is taken into account.[13] The coefficient on taxes rises under random effects, and that on benefits falls, but the differences are mostly insignificant, suggesting that the random effects model is consistent with the data. Second, in the time dimension, we regress on the cross-country mean of the output gap (columns 5, 6, 9, and 10) instead of including time dummies; this gives us reasonable coefficients on both taxes and benefits and on most other variables.

Another way to get more power from our data is to impose our model's restriction that the semielasticities of unemployment with respect to the tax wedge and the UI benefit should be equal. When we test this prediction in Table 1, only one of the five specifications considered rejects it. Estimates that impose this restriction by regressing on the sum $\tau + b$ (even columns of Table 1) yield significant semielasticities from 1.30 (for OLS with time and country dummies) to 2.85 (OLS with country dummies only). Next, Table 2 compares the coefficient on $\tau + b$ across many specifications, starting from pooled OLS and then controlling for

[13] Space constraints oblige us to place LN99's specification (random country effects and time dummies) in a footnote to the table. The results resemble the case with both country and time dummies shown in column 3, except that the negative tax coefficient becomes insignificant.

Table 1
Explaining log unemployment (comparing fixed and random country effects)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable: log of average unemployment rate, 5-year period[a]										
Tax wedge (percent)	0.0209		**−0.0437**		0.0078		**0.0326**		**0.0242**	
	(0.0145)		(0.0161)		(0.0143)		(0.0098)		(0.0098)	
UI replacement ratio (percent)	**0.0309**[b]		**0.0226**[c]		**0.0285**[b]		**0.0227**[b]		**0.0199**[b]	
	(0.0062)		(0.0052)		(0.0060)		(0.0051)		(0.0049)	
Tax wedge plus		**0.0285**		**0.0130**		**0.0239**		**0.0257**		**0.0211**
UI replacement ratio (percent)		(0.0045)		(0.0047)		(0.0045)		(0.0035)		(0.0036)
Benefit duration (fraction)	−0.3653	−0.3855	−0.4065	−0.5062	−0.3854	−0.4249	−0.0081	−0.0246	−0.0640	−0.0634
	(0.4652)	(0.4624)	(0.3799)	(0.3985)	(0.4435)	(0.4429)	(0.3160)	(0.3217)	(0.3029)	(0.2996)
Employment protection (index 0–20)	−0.0239	−0.0258	−0.0243	−0.0372	−0.0226	−0.0266	−0.0124	−0.0094	−0.0111	−0.0090
	(0.0265)	(0.0261)	(0.0230)	(0.0240)	(0.0252)	(0.0250)	(0.0205)	(0.0201)	(0.0197)	(0.0189)
Union density (percent)	−0.0191	**−0.0201**	−0.0023	−0.0146	−0.0154	−0.0175	−0.0075	−0.0063	−0.0051	−0.0041
	(0.0100)	(0.0098)	(0.0093)	(0.0092)	(0.0096)	(0.0094)	(0.0068)	(0.0065)	(0.0065)	(0.0060)
Bargaining coordination (index 0–6)	**−0.2083**	**−0.1953**	**−0.2403**	−0.1656	**−0.2152**	**−0.1887**	−0.1619	**−0.1802**	−0.1575	**−0.1651**
	(0.1008)	(0.0977)	(0.0844)	(0.0861)	(0.0961)	(0.0936)	(0.0857)	(0.0830)	(0.0829)	(0.0794)
Owner occupancy rate (percent)	**0.0573**	**0.0543**	−0.0030	−0.0042	**0.0438**	**0.0383**	**0.0524**	**0.0519**	**0.0456**	**0.0453**
	(0.0170)	(0.0160)	(0.0157)	(0.0165)	(0.0166)	(0.0160)	(0.0090)	(0.0092)	(0.0089)	(0.0087)
Mean output gap (percent)					**−0.2440**	**−0.2309**			**−0.2039**	**−0.2068**
					(0.0668)	(0.0660)			(0.0657)	(0.0647)
Country dummies	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No	No
Time dummies	No	No	Yes	Yes	No	No	No	No	No	No
Random country effects	No	No	No	No	No	No	Yes	Yes	Yes	Yes

Columns (1)–(6): OLS.
Columns (7)–(10): GLS random country effects model.
Note: GLS with random country effects and time dummies implies: −0.0015 (0.0121) on τ; **0.0165** (0.0049) on b; and **0.0133** (0.0041) on $\tau + b$.

[a]Panel of 19 OECD countries (Portugal excluded) aggregated over eight 5-year periods, 1960–1999. Dependent variable is log of 5-year average of unemployment rate. Standard errors are given in parentheses; bold type indicates significance at 5% level.

[b]Coefficient on UI replacement ratio *not* significantly different from that on tax wedge at 5% level.

[c]Coefficient on UI replacement ratio significantly different from that on tax wedge at 5% level.

country-specific and/or time-specific factors in different ways.[14] Estimates driven mostly by the time series variation in the data[15] tend to imply larger coefficients than those based mostly on cross-country variation,[16] but these differences are usually insignificant. Moreover, when we regress on the sum $\tau + b$, we obtain a robustly significant semielasticity even if we control for country and time effects simultaneously. Both in random effects regressions that include the output gap (column 9 of Table 2, where we try to avoid estimating too many coefficients), and in regressions with both time and country dummies (column 5 of Table 2, where we try to avoid biases), the estimates cluster around two.

Table 2 also considers many other robustness issues. First, we try eliminating the pre-1975 data, which may be less reliable. The coefficient on $\tau + b$ decreases somewhat; the most interesting effect (not shown) is that benefit duration usually has a significant positive coefficient in post-1975 estimates. Splitting the sample at 1980 or 1985 yields similar results; the coefficient on $\tau + b$ is larger but less significant in the early subsample, and smaller in the later one, when much of the impact of benefits goes through duration instead. In row 3, we replace the BGHS data (available to 1999) with the LMIDB dataset (available to 1995); the coefficient on $\tau + b$ decreases moderately unless both time and country dummies are included, in which case it becomes insignificant. Next, we try excluding Scandinavia, where unemployment has remained generally low despite large benefit increases over our sample period, perhaps due to generous retraining and reemployment spending. There is a nontrivial increase in the coefficient on $\tau + b$, to around 3.5. As an alternative control for this type of social spending, the regressions in row 5 include an indicator of 'active labor market policies' (ALMP); this series is limited to 1985–1999 and requires an IV treatment, as in LN99. The coefficient falls to roughly 1.5 (but is higher than in a 1985–1999 regression without ALMP). In the last two rows of results, we check how the other regressors influence the coefficient on $\tau + b$. Eliminating employment protection, union density, and benefit duration, which are insignificant in the full 1960–1999 sample, has little effect on the estimate; eliminating bargaining coordination and the owner occupancy rate (which are usually significant) tends to decrease the coefficient and its significance.

The possible endogeneity of policy is addressed in Table 3. The issue is that high unemployment could worsen the government's finances and force it to raise taxes (or lower benefits). Thus, unusually high unemployment (high v_{it}) would be associated with higher τ_{it} and lower b_{it}, biassing up the coefficient on taxes (and biassing down that on benefits). In the first six columns, we report IV regressions in which we instrument all current policy variables by their lagged values. We also include country dummies, because otherwise lagged policies cannot be valid instruments, as they would be correlated with the fixed effects. In these estimates,

[14]The regressors are the same as in Table 1, unless stated otherwise, but only the coefficient on taxes plus benefits is shown.

[15]In particular, estimates which allow for fixed country effects α_i, like column 2 of Table 1 and column 4 of Table 2.

[16]In particular, estimates which allow for fixed time effects α_t, like column 2 of Table 2.

Table 2
Robustness: coefficient on $\tau + b$

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent variable: *log of average unemployment rate, 5-year period*[a]									
Benchmark specification[b]	**0.0245**	0.0198	**0.0221**	**0.0285**	**0.0130**	**0.0239**	**0.0257**	**0.0133**	**0.0211**
	(0.0084)	(0.0106)	(0.0089)	(0.0045)	(0.0047)	(0.0045)	(0.0035)	(0.0041)	(0.0036)
Excluding pre-1975 data	**0.0133**	**0.0115**	**0.0126**	**0.0204**	**0.0150**	**0.0189**	**0.0167**	**0.0120**	**0.0152**
	(0.0049)	(0.0054)	(0.0050)	(0.0044)	(0.0048)	(0.0043)	(0.0030)	(0.0036)	(0.0030)
LMIDB data (excludes 1995–1999)	**0.0158**	**0.0100**	**0.0132**	**0.0152**	0.0011	**0.0110**	**0.0169**	0.0050	**0.0129**
	(0.0039)	(0.0045)	(0.0038)	(0.0035)	(0.0033)	(0.0034)	(0.0026)	(0.0028)	(0.0025)
Excluding Scandinavia	**0.0355**	**0.0313**	**0.0333**	**0.0467**	**0.0327**	**0.0418**	**0.0386**	**0.0279**	**0.0340**
	(0.0103)	(0.0116)	(0.0107)	(0.0053)	(0.0057)	(0.0054)	(0.0044)	(0.0050)	(0.0045)
IV estimate with ALMP,	**0.0117**	**0.0119**	**0.0116**	**0.0161**	**0.0154**	**0.0160**	**0.0190**	**0.0132**	**0.0130**
1985–1999	(0.0055)	(0.0055)	(0.0055)	(0.0065)	(0.0063)	(0.0061)	(0.0079)	(0.0057)	(0.0056)
Excluding ep, ud, and bd	**0.0253**	0.0203	**0.0228**	**0.0236**	0.0081	**0.0190**	**0.0243**	**0.0118**	**0.0199**
	(0.0081)	(0.0105)	(0.0087)	(0.0038)	(0.0041)	(0.0039)	(0.0029)	(0.0037)	(0.0031)
Including tw + brr only	0.0189	0.0097	0.0137	**0.0321**	**0.0087**	**0.0235**	**0.0297**	**0.0086**	**0.0218**
	(0.0094)	(0.0106)	(0.0094)	(0.0034)	(0.0042)	(0.0037)	(0.0033)	(0.0039)	(0.0035)
Cluster-corrected by country	Yes	Yes	Yes	No	No	No	No	No	No
Country dummies	No	No	No	Yes	Yes	Yes	No	No	No
Random country effects	No	No	No	No	No	No	Yes	Yes	Yes
Time dummies	No	Yes	No	No	Yes	No	No	Yes	No
Including mean output gap	No	No	Yes	No	No	Yes	No	No	Yes

Columns (1)–(6): OLS with cluster correction by country or with country dummies.

Columns (7)–(9): GLS random country effects model.

[a]Panel of 19 OECD countries (Portugal excluded) aggregated over eight 5-year periods, 1960–1999. Dependent variable is log of 5-year average of unemployment rate. Standard errors in parentheses; bold type indicates significance at 5% level.

[b]Benchmark regressors are those in column 2 of Table 1. Table only shows coefficient on tax wedge plus UI benefit.

Table 3
Robustness: controlling for endogeneity by instrumenting with lagged policy

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Dependent variable: *log of average unemployment rate, 5-year period*[a]										
Tax wedge (percent)	0.0300		−0.0639		0.0263		**0.0271**		**0.0407**	
	(0.0271)		(0.0365)		(0.0260)		(0.0104)		(0.0092)	
UI replacement ratio (percent)	**0.0406**[b]		**0.0297**[c]		**0.0369**[b]		0.0088[b]		0.0100[c]	
	(0.0091)		(0.0079)		(0.0088)		(0.0073)		(0.0064)	
Tax wedge plus		**0.0386**		**0.0235**		**0.0349**		0.0132		**0.0180**
UI replacement ratio (percent)		(0.0070)		(0.0079)		(0.0068)		(0.0070)		(0.0060)
Benefit duration (fraction)	−0.8206	−0.8065	−0.6505	−0.5221	−0.8557	−0.8413	0.2690	0.5203	0.4060	0.4830
	(0.8195)	(0.8222)	(0.6899)	(0.7076)	(0.7821)	(0.7856)	(0.4198)	(0.5851)	(0.4657)	(0.4091)
Employment protection (index 0–20)	−0.0208	−0.0231	−0.0380	−0.0603	−0.0246	−0.0269	0.0146	0.0273	0.0112	0.0259
	(0.0410)	(0.0401)	(0.0388)	(0.0376)	(0.0389)	(0.0381)	(0.0302)	(0.0389)	(0.0271)	(0.0367)
Union density (percent)	**−0.0304**	**−0.0316**	−0.0168	**−0.0317**	**−0.0317**	**−0.0330**	−0.0085	−0.0014	−0.0051	−0.0001
	(0.0148)	(0.0140)	(0.0164)	(0.0136)	(0.0141)	(0.0134)	(0.0042)	(0.0053)	(0.0056)	(0.0036)
Bargaining coordination (index 0–6)	−0.2229	−0.2010	**−0.4052**	−0.2492	−0.2206	−0.1986	−0.0813	−0.0872	−0.1296	−0.1972
	(0.1824)	(0.1670)	(0.1657)	(0.1515)	(0.1742)	(0.1597)	(0.1252)	(0.1324)	(0.1232)	(0.1382)
Owner occupancy rate (percent)	**0.0495**	**0.0454**	−0.0119	−0.0162	0.0350	0.0311	**0.0462**	**0.0439**	**0.0532**	**0.0459**
	(0.0246)	(0.0223)	(0.0226)	(0.0230)	(0.0239)	(0.0219)	(0.0093)	(0.0118)	(0.0085)	(0.0094)
Mean output gap (percent)					**−0.2263**	**−0.2244**			**−0.1408**	**−0.1716**
					(0.0754)	(0.0751)			(0.0446)	(0.0488)
Country dummies	Yes	Yes	Yes	Yes	Yes	Yes	No	No	No	No
Time dummies	No	No	Yes	Yes	No	No	Yes	Yes	No	No
System GMM	No	No	No	No	No	No	Yes	Yes	Yes	Yes

Columns (1)–(6): IV regression, instrumenting all policy variables on their lags.

Columns (7)–(10): Blundell–Bond system GMM, instrumenting all policy variables by their lags, with robust standard errors. Instruments in differences equation are second lag of levels; instruments in levels equation are first lag of differences; deeper lags are not used.

[a]Panel of 19 OECD countries (Portugal excluded) aggregated over eight 5-year periods, 1960–1999. Dependent variable is log of 5-year average of unemployment rate. Standard errors are given in parentheses; bold type indicates significance at 5% level.

[b]Coefficient on UI replacement ratio *not* significantly different from that on tax wedge at 5% level.

[c]Coefficient on UI replacement ratio significantly different from that on tax wedge at 5% level.

the coefficient on UI rises substantially, consistent with the form of endogeneity described above; the effects on the tax coefficient are less clearcut. The coefficient on the sum $\tau + b$ also rises, to 2.35 in the case with country and time dummies, and to 3.49 with country dummies and the mean output gap included.

As an alternative way of allowing for endogeneity, we also run 'system GMM' estimates, shown in the last four columns of Table 3. Like the last columns of Table 1, this specification is motivated by the greater efficiency achievable when the information in the levels of the data is not discarded (Blundell and Bond, 1998). In order to avoid overfitting, we do not impose all the moment conditions associated with multiple lags.[17] System GMM is less suggestive of endogeneity than our IV results; coefficients are broadly similar to those in Table 1. Several aspects of the results indicate that system GMM works better than our IV estimates: the benefit duration coefficient has the expected sign, and there is not a significant negative effect of union density. Also, even when we include time dummies (in addition to the control for country fixed effects implied by system GMM), we find reasonable coefficients on τ and b separately, not significantly different from each other, both lying near 2.5.

In summary, we find that the semielasticity of unemployment with respect to benefits is substantially larger than LN99's estimate of 1.3. With or without controlling for policy endogeneity, we usually estimate a semielasticity around two, though an IV treatment with country dummies and the output gap raises the coefficient to 3.49. At least two factors help explain our higher estimate. LN99 used data from 1984 onwards, and in this period part of the impact on unemployment is attributed to benefit duration rather than to benefit levels *per se*. Also, LN99 ran a GLS regression with random country effects and fixed time effects, which we find yields a lower coefficient (Table 2, column 8). However, while our results imply that the impact of UI benefits is economically important, it is still far too small to reconcile our model with business cycle data. In our model, the ratio $(\sigma_U/\sigma_Q)/\varepsilon^U_\xi$ between the relative cyclical volatility of unemployment and the semielasticity of unemployment to UI benefits should be substantially less than one. But in our data, (σ_U/σ_Q) is over seven, and ε^U_ξ is near two or three. The standard errors on our policy estimates are far from sufficient to reconcile these observations with Proposition 2.

5. Variations on the standard model

The analytical version of our model from Section 3 is strongly rejected by the data we just reviewed. Tinkering with parameters will not help, since the upper bound in Proposition 2 is independent of calibration. However, some generalization of the

[17]We run system GMM using the STATA add-on xtabond2 (Roodman, 2006), reporting robust standard errors. We treat the mean output gap as strictly exogenous. We treat all other variables as endogenous by instrumenting the differences equation with second lags and the levels equation with first lags; moment conditions relating to higher lags are left out to avoid overfitting. Arellano–Bond tests do not indicate AR(2) errors, and Sargan tests are consistent with instrument exogeneity (regardless of whether robust or nonrobust standard errors are used).

setup might fit better, so we turn next to numerical simulations of the general model from Section 2. Our first calibration is chosen to match our summary estimate of the semielasticity of unemployment with respect to UI benefits, $\varepsilon_{\xi}^{U} \approx 2$.

5.1. Benchmark parameters

Our benchmark numerical calibration is as follows. All matches have equal productivity ($\alpha_Z = 1$). The productivity shock Z is a two-state Markov process, taking values $Z^{\mathrm{LO}} = -0.018$ and $Z^{\mathrm{HI}} = 0.018$, which remains unchanged from one period to the next with probability ρ_Z. We simulate the model at weekly frequency, but report results aggregated to quarterly frequency. We impose an approximate yearly persistence of $\bar{\rho}_Z \equiv \frac{2}{3}$, implying six-year cycles, by assuming that Z remains unchanged from one week to the next with probability $\rho_Z \equiv \bar{\rho}_Z^{1/52} \approx 0.9922$.

Search intensity is exogenous: $S = 1$, $h = 0$ and $\eta_S^h = \infty$. The elasticity of total matches to unemployment is $\lambda = 0.5$, consistent with Blanchard and Diamond (1989). We assume an efficient benchmark equilibrium, setting $\mu = 0.5$ (Hosios, 1990). We calibrate an annual job loss rate of $\bar{\delta} \equiv 25\%$ by setting the weekly probability of job loss to $\delta \equiv \bar{\delta}/52$. This is reasonable for the US, though separation rates are higher for the least stable classes of jobs and workers. To get an annual discount factor of $\bar{\beta} \equiv 95\%$, we set weekly discounting to $\beta \equiv \bar{\beta}^{1/52}$. The matching efficiency and vacancy cost parameters γ and κ are reset in each simulation so that steady state unemployment is always $U = 0.06$ (again, a US calibration) and so that a vacancy lasts two weeks on average. Vacancy duration is just a normalization: doubling it would mean doubling vacancies, reducing κ by half, and adjusting γ to keep total matches, total vacancy costs, and job finding probabilities unchanged.

On average, the Markov process spends equal time in good and bad states, so mean productivity y is 1. Next, choosing $b + \tau$ is crucial for labor market volatility, because a higher b or τ implies a smaller and more variable surplus. In fact, (26) shows that the cyclical variance of job finding goes to infinity as $b + \tau$ approaches $y + h$. We set $b = 0.745$ in our numerical benchmark, which implies $\varepsilon_{\xi}^{U} = 2$, consistent with our estimates of policy effects. Shimer (2005) instead calibrates $b = 0.4$, which implies, roughly, that the only cost of working is the loss of the UI benefit. But considering our model, b must also include the utility costs (or any other costs) of working, which are presumably nontrivial. We set $\tau = 0$ for consistency with related papers, but it is really only the sum $b + \tau$ that matters, so everything we will show about the effects of varying b is also applicable to changes in τ.

5.1.1. Benchmark results: importance of the size of the of surplus

Table 4 shows the simulation results, with the numerical benchmark calibration in line 1. All relative standard deviations and correlations refer to data aggregated to quarterly frequency, and results are HP-filtered with smoothing parameter 100 000.[18]

[18]The filter has a mild effect on the level of fluctuations σ_Q, but has virtually no effect on the relative fluctuations σ_U/σ_Q, which are our focus. The ratio σ_U/σ_Q only changes from 1.40 (with filtering) to 1.42 (without). HP filtering would be slightly more relevant if we chose a higher persistence $\bar{\rho}_Z$.

Table 4
Simulation results

	Parameters	Results								
		$\rho_{Q_{-1}}$	$\frac{\sigma_y}{\sigma_Q}$	$\frac{\sigma_w}{\sigma_Q}$	$\frac{\sigma_p}{\sigma_Q}$	$\frac{\sigma_U}{\sigma_Q}$	$\rho_{U,V}$	ε_ξ^U	$\frac{\sigma_U}{\sigma_Q}\frac{1}{\varepsilon_\xi^U}$	$\frac{\sigma_U}{\sigma_Q}\frac{1}{\varepsilon_D^U}$
(1)	Benchmark	0.84	0.92	0.91	1.61	1.40	−0.80	2.00	0.70	–
(2)	$b = 0.955$	0.88	0.64	0.93	6.63	5.71	−0.79	14.29	0.40	–
(3)	$b = 0.4$	0.84	0.96	0.93	0.72	0.62	−0.80	0.82	0.76	–
(4)	$\bar{\rho}_Z = 0.75$	0.87	0.91	0.93	1.68	1.48	−0.83	2.07	0.71	–
(5)	$\bar{\delta} = 0.4$	0.84	0.91	0.94	1.69	1.52	−0.89	2.00	0.76	–
(6)	$\lambda = 0.3$	0.85	0.88	0.95	2.37	2.06	−0.66	2.89	0.71	–
(7)	$\mu = 0.3$	0.84	0.92	0.82	1.54	1.33	−0.80	2.04	0.65	–
(8)	δ varies with Z by $\pm 15\%$	0.88	0.64	1.00	1.40	5.89	0.95	2.11	2.79	–
(9)	$\eta_S^h = 4$	0.85	0.83	0.95	3.18	2.75	−0.60	4.14	0.66	–
(10)	$\eta_S^h = 2$	0.87	0.67	1.01	6.14	5.31	−0.17	8.98	0.59	–
(11)	Benefits last 6 months, $b = 0.87$	0.84	0.94	0.94	1.60	1.45	−0.86	1.83	0.79	7.33
(12)	Benefits last 2 years, $b = 0.80$	0.84	0.92	0.92	1.64	1.44	−0.82	1.96	0.73	11.27
(13)	μ varies with Z by $\pm 15\%$	0.87	0.65	0.59	6.55	5.67	−0.80	2.08	2.73	–
(14)	Efficiency wages	0.85	0.84	0.82	2.97	2.58	−0.80	4.00	0.64	–
(15)	Cohort-specific benchmark	0.93	0.54	3.64	11.32	9.66	−0.77	1.79	5.40	–
(16)	Cohort-specific, $\zeta = 1.6$, $\alpha_Z = 0.5$	0.88	0.67	2.29	6.28	5.36	0.77	1.77	3.02	–
(17)	Cohort-specific, $\zeta = 1.6$, $\alpha_Z = 0.5$, δ varies with z by $\pm 10\%$	0.89	0.60	2.43	6.79	6.43	−0.60	1.80	3.58	–

Notes: Benchmark: $\alpha_Z = 1$, $Z = \pm 0.018$, $\bar{\rho}_Z = 2/3$, $\bar{\beta} = 0.95$, $\bar{\delta} = 0.25$, $\lambda = \mu = 0.5$, $b = 0.745$, $\eta_S^h = \infty$.
Cohort-specific benchmark: $\alpha_Z = 0$, $Z = \pm 0.018$, $\zeta = 1$, $\bar{\rho}_Z = 0.6$, $\bar{\beta} = 0.95$, $\bar{\delta} = 0.25$, $\lambda = \mu = 0.5$, $b = 0.7$, $\eta_S^h = \infty$.
σ_x is the standard deviation of $\log x$ (quarterly); $\rho_{U,V}$ the correlation between $\log U$ and $\log V$ (quarterly); $\rho_{Q_{-1}}$ the annual first order serial correlation of $\log Q$; η_x^y the elasticity of y w.r.t. x; ε_x^y the semielasticity of y w.r.t x.

By construction, the long run semielasticity of unemployment with respect to the replacement ratio is $\varepsilon_{\xi}^{U} = 2.00$ in our numerical benchmark. But this calibration yields insufficient cyclical variation in log unemployment, with $\sigma_U/\sigma_Q = 1.40$, when this ratio is over seven in the data. The punchline is that $(\sigma_U/\sigma_Q)/\varepsilon_{\xi}^{U}$ equals 0.70, far lower than in the data, and also well below our analytical upper bound of one. Similar results hold for the job finding probability p: its cyclical variability is $\sigma_p/\sigma_Q = 1.61$ (too low), while the semielasticity ε_{ξ}^{p} is -2.13 (about right; not shown in table). The cyclical variability of vacancies $\sigma_V/\sigma_Q = 3.23$ is also too low.

As we mentioned above, a higher b can increase unemployment variability, by making the surplus smaller and proportionally more volatile. With the benchmark value $b = 0.745$, total surplus Σ is 45.2% of the mean quarterly output of a matched worker. In line 2 we set $b = 0.955$ (95.5% of mean y), as assumed by Hagedorn and Manovskii (2006), which shrinks Σ to just 8.0% of mean quarterly labor productivity. The relative volatility of unemployment rises to $\sigma_U/\sigma_Q = 5.71$, almost as high as in US data. However, unemployment also becomes more responsive to UI benefits, with $\varepsilon_{\xi}^{U} = 14.29$, which drastically exceeds our estimates. Intuitively, such a large b means firms own a highly leveraged claim on the productivity process y, so that small variations in y or b motivate big changes in hiring.

In line 2, we go in the opposite direction and decrease b to 0.4, as in Shimer (2005).[19] Total surplus Σ is now 106.3% of mean quarterly labor productivity. The unemployment semielasticity ε_{ξ}^{U} falls to 0.82, and the cyclical volatility of unemployment falls to $\sigma_U/\sigma_Q = 0.62$. Thus this calibration not only produces insufficient cyclical volatility: it is even too inelastic to match the estimated impact of UI. Thus we see the main tradeoff: we can make the model more volatile to better match cyclical data, or less volatile to better match policy effects, but the two goals are at odds with each other. In relative terms, the tradeoff is worse when b is large: $(\sigma_U/\sigma_Q)/\varepsilon_{\xi}^{U} = 0.40$ on line 2, compared with $(\sigma_U/\sigma_Q)/\varepsilon_{\xi}^{U} = 0.76$ on line 3.

Before moving to other model specifications, we consider several other parameters. In line 4, we set $\bar{\rho}_Z = 75\%$, so that a full cycle lasts roughly eight years. In line 5, we increase the separation rate to $\bar{\delta} = 40\%$ annually, a reasonable US calibration if we prefer to focus on relatively unstable jobs and workers. Line 6 lowers the elasticity of matching with respect to unemployment to $\lambda = 0.3$, while maintaining $\mu = 0.5$; and line 7 lowers μ to 0.3, with $\lambda = 0.5$. While there are mild changes in some statistics, the ratio $(\sigma_U/\sigma_Q)/\varepsilon_{\xi}^{U}$ is robust, staying close to 0.7 in all these experiments.

5.2. Variable separation and variable search

Davis et al. (1996) argue that job destruction is strongly countercyclical. Therefore, we need to ask whether variation in separation rates might change our results. The usual model of variable separation (Mortensen and Pissarides, 1994) posits match-specific productivity shocks, so that worker–firm pairs separate when

[19] Hall (2005b) sets b even lower, to 35% of the firm's flow of surplus in a new match.

their joint surplus becomes negative. For simplicity, we instead impose an exogenous separation rate that depends negatively on the aggregate technology shock, which is essentially what Mortensen and Pissarides' model implies. We set $\bar{\delta}(Z^{\mathrm{LO}}) = 0.25 * 1.15 = 0.2875$ and $\bar{\delta}(Z^{\mathrm{HI}}) = 0.25/1.15 \approx 0.2174$, so that $\bar{\delta}$ varies with Z by $\pm 15\%$. Line 8 shows that this brings the cyclicality of unemployment closer to the data: σ_U/σ_Q rises to 5.89. The semielasticity of unemployment with respect to ξ changes only slightly, so the ratio $(\sigma_U/\sigma_Q)/\varepsilon_\xi^U$ improves, rising to 2.79.

The problem is that this way of resolving the conflict destroys the Beveridge curve: the correlation between unemployment and vacancies switches sign to $\rho_{U,V} = 0.95$. The fact that variable separation makes unemployment more volatile, but eliminates the Beveridge curve, has also been noted by Cole and Rogerson (1999) and Shimer (2005). Second, although unemployment variability rises, the probability of job finding now varies less: the ratio σ_p/σ_Q falls from 1.61 in the numerical benchmark to 1.40 with variable separation. This contradicts Shimer's (2005) calculation that job finding is almost as variable as unemployment. Third, the amount of variation in the separation probability needed here is too large. The relative standard deviation of job destruction to employment is now $\sigma_D/\sigma_N = 13.51$, well above Cole and Rogerson's (1999) figure of six. (In the numerical benchmark, it is exactly one by construction.)

Lines 9 and 19 allow for variable search effort, first considering relatively inelastic search ($\eta_S^h = 4$) and then higher elasticity ($\eta_S^h = 2$). Variable search effort makes unemployment more cyclical because (as in Section 2) search rises when productivity is high. With $\eta_S^h = 4$, we have $\sigma_U/\sigma_Q = 2.75$, while $\eta_S^h = 2$ matches cyclical volatility quite well, reaching $\sigma_U/\sigma_Q = 5.31$. However, the response of unemployment to benefits rises even more, so that the key ratio $(\sigma_U/\sigma_Q)/\varepsilon_\xi^U$ falls. That is, as Propositions 1 and 2 indicated, endogenous search only makes the tradeoff worse. Also, sufficiently elastic search effort again destroys the Beveridge curve: with $\eta_S^h = 2$, we have $\rho(U, V) = -0.17$.[20]

5.3. Finite UI benefit duration

Another issue that might matter for our results is our assumption that UI benefits continue as long as unemployment lasts. Intuitively, UI benefits might affect unemployment less if they eventually expired. The easiest way to model finite benefit duration is to assume benefits expire with probability ϕ per period, implying expected duration $D \equiv 1/\phi$. Then there are three labor market states: employed, unemployed with benefits, and unemployed without benefits. The employed workers' Bellman equation (2) is unchanged.[21] Restricting ourselves to exogenous search

[20] Merz (1995) also finds that variable search effort acts against the Beveridge curve.

[21] This assumes workers become eligible for UI from the moment of matching. Otherwise there would be a fourth labor market state, employed without benefits, with a lower outside option and thus lower wages. As Coles and Masters (2005) point out, this would drive down wages of new jobs in recessions (when more workers run out of benefits), making unemployment even less volatile.

effort, the value (3) of unemployment with benefits is replaced by

$$W^U(Z) = b + \beta E_{Z'|Z}\{p(1,\theta(Z))W^E(Z',Z') + (1-p(1,\theta(Z))) \times[(1-\phi)W^U(Z') + \phi W^X(Z')]\}, \tag{31}$$

where $W^X(Z)$ is the value of unemployment without benefits, given by

$$W^X(Z) = b_0 + \beta E_{Z'|Z}\{p(1,\theta(Z))W^E(Z',Z') + (1-p(1,\theta(Z)))W^X(Z')\}. \tag{32}$$

Here, for the first time, we must distinguish the actual UI benefit $b-b_0$ (which expires at rate ϕ) from work disutility b_0. For consistency with the US, we set $b-b_0 = 0.4$.

We first consider a mean benefit duration of six months, which is the US norm. Finite expected UI duration increases the employment surplus $\Sigma^W = W^E - W^U$ (this is still the relevant surplus for the Nash wage equation), so with $b = 0.745$ the cyclical volatility of unemployment is greatly decreased. Therefore, in line 11 of Table 4 we also raise b to 0.87, which sets the surplus to the same level as in the numerical benchmark of line 1. The results resemble those in line 1: benefits have a reasonable effect on unemployment, but the cyclical variability of unemployment is much too low, so the key ratio $(\sigma_U/\sigma_Q)/\varepsilon_\xi^U$ increases only slightly, to 0.79. In line 12, we instead assume benefits last two years (the median duration reported in LN99 for European countries). This time we adjust b to 0.80 to keep the level of surplus in line with that of the benchmark model. Results are again similar.

Thus finite benefit duration does not affect our main results. However, it does imply another way to test the model, using LN99's estimate of the semielasticity of unemployment with respect to benefit duration, $\varepsilon_D^U = 0.1$.[22] Since the relative volatility of unemployment is $\sigma_U/\sigma_Q \approx 7$, the ratio $(\sigma_U/\sigma_Q)/\varepsilon_D^U$ should be around 70. Instead, as the final column of lines 11 and 12 shows, this ratio is 7.33 or 11.27 in our simulations. In this way, considering finite benefit duration reinforces the evidence that the standard RBCM framework understates cyclical volatility relative to policy effects.

5.4. Sticky wages

We have seen that higher b means proportionally higher variation in the firm's surplus over the cycle, increasing the variability of hiring and unemployment. Another obvious way to make the firm's surplus volatile would be to impose some form of wage stickiness, as Shimer (2004) and Hall (2005a, b) also advocate. Furthermore, it seems natural to assume that sticky wages are only a short run phenomenon, so that they should have no influence on the long run impact of the UI benefit.

[22]See their Table 15. Here we refer to LN99's estimate because their duration variable, the number of years benefits last, can be interpreted as $D = 1/\phi$ in our model. Our own duration regressor is the fraction of benefits remaining after the first year, which is harder to interpret in terms of (31)–(32). But since it is insignificant in our regressions, the conclusion remains that UI duration has stronger effects in the model than in the data.

Again, we choose an easy *ad hoc* way of making wages sticky. We assume that workers' bargaining power varies negatively with the technology shock, so that workers get a larger share of surplus in recessions. This stabilizes the wage over the cycle, and thus destabilizes the firm's hiring incentives. In line 13 we assume that the worker's bargaining power increases (decreases) by 15% when the aggregate technology shock is low (high). This raises σ_U/σ_Q to 5.67, roughly consistent with the data. The semielasticity ε^U_ξ hardly changes, so that $(\sigma_U/\sigma_Q)/\varepsilon^U_\xi$ increases to 2.73.

This does not seem like an unreasonable degree of wage stickiness: the ratio of the standard deviations of log wages and log output is now $\sigma_w/\sigma_Q = 0.59$. This is better than the figure of 0.91 in the baseline model, though still not as low as in the data; for example, Merz (1995) reports $\sigma_w/\sigma_Q = 0.37$ for the US. Therefore, sticky wages seem a potentially promising way of improving the model's fit. But obviously they are controversial, and debate goes on about possible justifications for wage stickiness.

One possible microfoundation for wage stickiness is an 'efficiency wage'. Here, if we follow Shapiro and Stiglitz (1984) by assuming a constant probability of catching shirkers, firms should offer workers a constant surplus just sufficient to prevent shirking.[23] Thus in line 14 we report a version of our model where the Nash bargaining condition (8) is replaced by an equation that fixes a constant surplus for the worker at all times (equal to the average surplus in the numerical benchmark of line 1). While cyclical unemployment volatility rises, the semielasticity of U with respect to ξ increases even more, so that $(\sigma_U/\sigma_Q)/\varepsilon^U_\xi$ falls to 0.64. The problem is that the moral hazard problem alters wages not only in the short run, but also in the long run. Fixing a constant surplus for the worker makes hiring incentives fall sharply with the replacement ratio, so that our efficiency wage model fits less well than our *ad hoc* sticky wage model, in which wages adjust flexibly to long run changes in UI.

5.5. Cohort-specific technology shocks

Finally, we show that a form of embodied technological change could also help solve the puzzle that concerns us. If it is cheaper to start using a new technology by hiring new workers with different skills instead of retraining existing employees, then technology shocks should affect new matches without changing the productivity of old ones.[24] One reason to prefer such a specification is that, in contrast to the model of Section 3, it makes wages of new hires more procyclical than those of continuing jobs. This is a well-established empirical fact (see for example Bils, 1985; Bowlus, 1995). There is also direct evidence that workers find higher-quality jobs in booms than in recessions, from data on movements across sectors (Heckman and Sedlacek,

[23] Costain and Jansen (2006) provide a more complete analysis of the cyclical dynamics of a matching model with efficiency wages.

[24] Mortensen and Pissarides (1998) and Hornstein et al. (2005b) study trend growth in models where employers can implement new technology by retraining or by rehiring.

1985), job tenure (Bowlus, 1995), and worker and job characteristics (Devereux, 2003). Again, this suggests that productivity should have a match-specific or cohort-specific component.

So we next set $\alpha_Z = 0$, making the productivity of each match specific to its time of creation. Since shocks no longer affect all matches equally, the persistence of aggregate output increases, so we decrease the persistence parameter $\bar{\rho}_Z$ from 0.67 to 0.6 annually. We also initially set $\zeta = 1$, so that the cohort-specific shock has the same impact as the aggregate shock did; and we lower b to 0.7 to keep ε_{ξ}^{U} near its target level of two. This simple change of specification, called the 'cohort-specific benchmark' in the table, more than suffices to reconcile cyclical unemployment volatility with the effect of UI. In line 15, we find that $\sigma_U/\sigma_Q = 9.66$, even higher than in the data, while $\varepsilon_{\xi}^{U} = 1.79$ is slightly decreased, so that the ratio $(\sigma_U/\sigma_Q)/\varepsilon_{\xi}^{U}$ rises to 5.40.[25] The job finding probability also varies more: $\sigma_p/\sigma_Q = 11.32$.

Why does this cohort-specific productivity specification increase cyclical volatility so much? Note that when technology shocks are disembodied ($\alpha_Z = 1$) and thus immediately affect all matches, workers and firms know that a high match productivity Z may fall before separation, and a low Z may rise. In contrast, in the embodied ($\alpha_Z = 0$) case, a match's productivity z will be unchanged until separation; other things equal, this increases the difference in value between high and low productivity matches, making hiring respond more strongly to the aggregate state. Also, since employment now varies more relative to output, and high and low productivity matches coexist, we now find that aggregate productivity varies less relative to output than it did with disembodied productivity: σ_y/σ_Q falls from 0.92 in the model of line 1 to 0.54 in the cohort-specific benchmark of line 15. This also improves the model's fit, though it goes somewhat too far, overshooting the ratio $\sigma_y/\sigma_Q = 0.65$ we calculate from the FRED data.

A potential problem with the embodied technology specification is that wages are too volatile: σ_w/σ_Q more than triples from its benchmark value in line 1, which is already too high. The reason is that even though a technological improvement leaves the productivity of existing matches unchanged, it nonetheless raises all workers' outside options, and thereby their wages. We should emphasize here that matching models do not actually tie down the wage process. These models only specify how the surplus is split between the firm and worker, and more than one wage process (including, for example, implicit contracts that keep the wages of *existing* matches fixed) is consistent with the implied behavior of the surplus.[26] Therefore we may not want to reject this model on the basis of its wage implications. However, those who wish to take wage data literally may prefer the sticky wage model of line 13.

Since assuming entirely embodied technology exaggerates unemployment fluctuations and also understates aggregate productivity fluctuations, we can now afford to

[25] Since fluctuations are more persistent under this specification, the results are now more sensitive to the HP filter. Without filtering, we have $\sigma_U/\sigma_Q = 7.86$ instead of 9.66, but this still suffices to match the data.

[26] Rudanko (2006) explores implicit contracts, as does Reiter (2006), which studies our embodied technology specification in greater depth. In both papers, aggregate wages become even smoother than they are in US data.

go to the intermediate case $\alpha_Z = 0.5$, so that technology shocks have both aggregate and cohort-specific effects. To succeed on both these margins simultaneously, it also helps to raise ζ to 1.6, making embodied technology shocks 60% stronger than disembodied shocks. This parameterization is shown in line 16, with labor market cyclicality ($\sigma_U/\sigma_Q = 5.36$) and policy effects ($\varepsilon^U_\xi = 1.77$) both close to the data.

Finally, since output differs across matches, it now seems especially important to consider variable separation. Thus in line 17 we vary the separation rate by $\pm 10\%$ with the match-specific shock z, so that less productive matches are always more likely to separate. This specification yields our most successful simulation. The ratio σ_U/σ_Q rises to 6.43, and the semielasticity of unemployment with respect to UI is nearly unchanged, at $\varepsilon^U_\xi = 1.80$, so that $(\sigma_U/\sigma_Q)/\varepsilon^U_\xi = 3.58$ is consistent with the data. While line 8 showed that generating labor market volatility through variable separation alone reverses the sign of the Beveridge correlation, combining variable separation with another source of labor market volatility only mildly reduces the correlation between unemployment and vacancies, to -0.60. This version of the model is also fairly successful with the volatility of labor productivity: σ_y/σ_Q equals 0.60 in our model and 0.65 in our data; the job finding probability: σ_p equals 10.5 in our model and 11.8 in the data of Shimer (2005); and job destruction: σ_{JD}/σ_N equals 3.36 in our model and 6.57 in the data of Cole and Rogerson (1999). The biggest problem with this specification is again excessive wage variability, with $\sigma_w/\sigma_Q = 2.43$.

6. Matching in business cycle models with capital

We have argued that our model's lack of physical capital is probably inessential for our results. But to be sure, we finish by reexamining the models of Merz (1995) and Andolfatto (1996), which include capital. While these papers reported some success in modeling labor market fluctuations, when we recalculate their steady states to measure the effects of UI benefits, we find that they suffer from the same problem as our benchmark model: insufficient cyclical volatility compared with the impact of policy.

6.1. The model of Andolfatto (1996)

To understand both models it is helpful to start by looking at the surplus. In Andolfatto's case, we calculate that the match surplus is only 17.3% of mean quarterly labor productivity – much lower than that of our numerical benchmark.[27] This suggests that his labor market should be quite volatile.

At first glance, Andolfatto's labor market appears to work well. His Table 1 shows that employment is 0.51 times as variable as output in his model, compared with 0.67

[27] In Andolfatto's notation, from $qJ = \kappa$ and $J = \alpha\Sigma$ we get the total surplus as $\Sigma = \kappa/(q\alpha) = 0.105/(0.9 * 0.6) = 0.194$ in units of quarterly output. (This equals μ, the shadow value of a match, divided by the marginal utility of consumption.) Labor productivity is $(1-\theta)y/n = 0.64/0.57 = 1.123$, so match surplus is $0.194/1.123 = 17.3\%$ of quarterly productivity.

in his data. However, this hides a surprising failure to explain unemployment, because of an unusual calibration. Andolfatto sets the mean employment rate to 57%, so that the mean unemployment rate is 43%, thus treating any nonworking person over age 16 as unemployed. This goes far beyond some authors, such as Cole and Rogerson (1999) and den Haan et al. (2000), who have argued for a broader definition of unemployment, by including all pensioners, students, and homemakers as inputs to the matching function. Any given standard deviation of log employment therefore corresponds to a smaller standard deviation of log unemployment in Andolfatto's calibration than it would if baseline unemployment were lower. With his numbers, we find

$$\frac{\sigma_U}{\sigma_Q} = \frac{1-U}{U}\frac{\sigma_N}{\sigma_Q} = \left(\frac{0.57}{0.43}\right)0.51 = 0.68 \tag{33}$$

less than one-tenth of the volatility we calculate from US data, based on the standard definition of unemployment.

Furthermore, even if we choose to ignore unemployment, Andolfatto's model also understates the volatility of other labor market variables. The coefficient of variation of vacancies in his model is about 4.4%, so that $\sigma_V/\sigma_Q = 3.2$, compared to 9 in his data. Since unemployment hardly varies, the coefficient of variation of tightness is only slightly higher (4.6%), compared with 37% in our data. Using $1-\lambda = 0.6$, workers' job finding probability has coefficient of variation $0.6 * 4.6\% = 2.8\%$ in Andolfatto's model, about one-fourth of the volatility Shimer (2005) finds in US data.

Andolfatto's model has no UI benefits, but in his setup they would be equivalent to work disutility. Thus, to mimic a one percentage point increase in the UI replacement ratio, we raise the utility of the nonemployed by one percent of mean labor productivity, scaled by the marginal utility of consumption. We calculate that the semielasticity of unemployment with respect to UI is 2.41 in Andolfatto's model, which might seem consistent with our cross-country estimate. But it is not: given Andolfatto's interpretation of unemployment, each 1% increase in log U represents a 0.43 *percentage point* increase in unemployment. That is, a one percentage point rise in the replacement ratio increases unemployment by $2.41 * 0.43 = 1.04$ percentage points, whereas Layard et al. (1991) estimate that this coefficient is 0.17.[28] Seen in this way, Andolfatto's labor market both exhibits insufficient cyclical fluctuation, and overreacts to UI; the punchline for his paper is $(\sigma_U/\sigma_Q)/\varepsilon_\xi^U = 0.68/2.41 = 0.28$.

6.2. The model of Merz (1995)

Merz (1995) comes close to fitting the variability of unemployment and the job finding probability in US data. With her benchmark specification, $\sigma_U/\sigma_Q = 4.77$,[29] and $\sigma_p/\sigma_Q = 5.41$. However, if we back out the effect of UI in the same way we did

[28] Here, for comparability, we refer to a slope estimate instead of a semielasticity estimate.

[29] This is the result of our own calculation and differs slightly from the number in Merz' Table 2.

for Andolfatto, we find that the model exaggerates the sensitivity of unemployment to benefits yielding a semielasticity of 6.54. The statistic $(\sigma_U/\sigma_Q)(1/\varepsilon_\xi^U)$ is therefore 0.73, so Merz' model fails by roughly the same factor as our benchmark model in Section 4.1.

When we calculate the match surplus in Merz' model, it turns out to be only 1.69% of mean quarterly labor productivity – five times smaller than anything we have seen so far. Thus Merz achieves sufficient cyclical volatility only by assuming an almost negligible surplus, and in doing so exaggerates the response to UI benefits.

The reason Merz' labor market fluctuates so little, in relation to the tiny surplus she assumes, is that she defines the surplus differently from all the other models we have discussed. Most matching models assume that the marginal disutility of work is constant along the extensive margin (increases in employment) even if it is increasing along the intensive margin (increasing marginal disutility in hours per job, as in Andolfatto's model). In contrast, in Merz' paper the surplus accrues to a family with a continuum of members, with increasing marginal disutility of work as more family members find jobs. At the margin, the disutility from one more job almost equals the wage income from that job, so the surplus is extremely small. To us, the usual formulation seems more appropriate, since typical households contain only one or two earners, each of whom may have a large inframarginal gain when they find a job.

6.3. Other models with capital

den Haan et al. (2000) study an RBCM model with endogenous separations. They are successful in explaining variations in job creation and destruction, and find that the interaction between job destruction and investment helps amplify shocks. This is consistent with our finding that variable separation can make matching volatile. However, our calculations suggest that their model will fail to generate a Beveridge curve. Their paper does not report the correlation between vacancies and unemployment.[30]

Gomes et al. (2001) simulate a business cycle model in which individuals search for jobs. It is not an RBCM model, because it has no matching function. Instead, it has an exogenous distribution of job offers, making it a dynamic extension of McCall's (1970) partial equilibrium search model. They successfully reproduce the cyclical fluctuations of unemployment. However, they state that raising the replacement ratio from 0.5 to 0.7 increases unemployment from 6.1% to 13.9%, which is a semielasticity of 6.49, exceeding our estimate by a factor of two or three. Thus their model suffers from the same problem as the RBCM models we have addressed.

7. Conclusions

A model of real business cycles and matching implies that job creation depends on the surplus available to the matched pair. Procyclical employment fluctuations occur

[30] Fujita (2003) explores ways of extending the den Haan et al. model to generate a Beveridge curve.

if surplus rises in booms, and raising UI benefits drives down employment by decreasing the surplus. The standard RBCM model implies a close relationship between these two aspects of employment variability, which is strongly at odds with data. To fit business cycle data, the surplus must be small enough so that productivity shocks have a big effect on vacancies; but to reproduce the observed effects of policies, the surplus must be large enough so that UI benefits have a small effect on vacancies. We have shown analytically that these two requirements cannot be reconciled in a baseline version of the model. We have shown numerically that this result is robust to endogenous search, endogenous separation, finite benefit duration, and efficiency wages; we have also argued that capital, variable benefits, and variable hiring costs are unlikely to resolve the puzzle; and we have argued that the HP filter is not crucial for our results.

Match-embodied technological change can help reconcile these two implications of the model (see also Hornstein et al., 2005a), because it makes the surplus accruing to the firm substantially more procyclical, so that hiring, unemployment, and the worker's job-finding probability all fluctuate more. Sticky wages have a similar effect on the firm's surplus, so they also help increase cyclical variability without exaggerating the impact of labor market policy (see also Shimer, 2004; Hall, 2005b; Gertler and Trigari, 2006; Menzio, 2005).

Our findings suggest that modeling labor market fluctuations by calibrating a very small match surplus, as Hagedorn and Manovskii (2006) advocate, is unhelpful because it is inconsistent with robust observations about labor market policy effects. There is endless scope for debating cross-country regressions, but we find that the small surplus calibration needs to stray far from any available evidence on policy effects in order to reproduce cyclical fluctuations. While sticky wages or embodied productivity shocks may prove to be fruitful explanations of labor market dynamics, many other ways of improving the fit of the matching model have also been suggested recently, including alternative specifications of the bargaining game, hiring and training costs, and shocks to job destruction (Hall and Milgrom, 2005; Silva and Toledo, 2005; Mortensen and Nagypál, 2006). In the long run we expect economists to learn a lot about labor markets and business cycles by asking which of these alternatives are consistent with a wide range of empirical facts. More generally, we believe policy studies may often provide useful tests for business cycle models: measuring the impact of observable policy shocks may help impose discipline on business cycle models where shocks might otherwise have to be treated as unobservables. This sort of discipline seems especially important if the models in question are intended for use in policy analysis.

Acknowledgments

We gratefully acknowledge research grants from the Spanish Ministry of Education (PB98-1065 and PB98-1066) and the Ministry of Science and Technology (SEC2002-01601 and SEJ2004-03619) and from the Bank of Spain. Many people have given us helpful comments, including seminar participants at Pompeu Fabra, the University of Cyprus, FEDEA, Carlos III, Queen Mary, and EEA-ESEM 2004. We are especially thankful for data, programs, and advice from Wouter den Haan, Monika Merz, Robert Shimer, Giulio Fella, Christian Haefke, Victor Ríos Rull, Marcus Hagedorn, Steve Nickell, Andrew Glyn, John Schmitt, Ernesto Villanueva, Thijs van Rens, Olympia Bover, David Roodman, and two anonymous referees. The opinions and analysis in this paper are those of the authors, and do not necessarily coincide with those of the Bank of Spain or the Eurosystem. Errors are the responsibility of the authors.

Appendix A. Linearized dynamics

First we linearize the zero profit condition (18) and the dynamics of the surplus (17):

$$\lambda\tilde{\theta}_t - \tilde{S}_t = \lambda^*\tilde{\theta}_t = E_t\tilde{\Sigma}_{t+1}, \tag{34}$$

$$\tilde{\Sigma}_t = \frac{y}{\Sigma}\tilde{y}_t + \beta(1-\delta-\mu p)E_t\tilde{\Sigma}_{t+1} - \beta\mu p\tilde{p}_t + \frac{h}{\Sigma}\eta^h_S\tilde{S}_t. \tag{35}$$

These equations can be simplified by writing $\tilde{p}_t$ and $\tilde{S}_t$ in terms of $\tilde{\theta}_t$ and $E_t\tilde{\Sigma}_{t+1}$, as follows: $\tilde{p}_t = (1-\lambda^*)\tilde{\theta}_t$, and $\tilde{S}_t = \eta^S_\theta\tilde{\theta}_t$, and $\tilde{\theta}_t = (1/\lambda^*)E_t\tilde{\Sigma}_{t+1}$. The following matrix system summarizes the dynamics of $\tilde{y}$ and $\tilde{\Sigma}$:

$$\begin{pmatrix} E_t\tilde{y}_{t+1} \\ E_t\tilde{\Sigma}_{t+1} \end{pmatrix} = \begin{pmatrix} \rho & 0 \\ -\frac{y}{\Sigma}\left[\beta\left(1-\delta-\frac{\mu p}{\lambda^*}\right) + \frac{h\eta^h_S\eta^S_\theta}{\Sigma\lambda^*}\right]^{-1} & \left[\beta\left(1-\delta-\frac{\mu p}{\lambda^*}\right) + \frac{h\eta^h_S\eta^S_\theta}{\Sigma\lambda^*}\right]^{-1} \end{pmatrix} \times \begin{pmatrix} \tilde{y}_t \\ \tilde{\Sigma}_t \end{pmatrix}. \tag{36}$$

The eigenvalues are $0<\rho<1$ and $[\beta(1-\delta-\mu p/\lambda^*) + h\eta^h_S\eta^S_\theta/\Sigma\lambda^*]^{-1}$. We assume search is sufficiently inelastic so that the second eigenvalue is greater than one (this is automatically true if search is exogenous).[31] Thus the system is saddle-path stable, and has a unique equilibrium. The eigenvector associated with the stable eigenvalue can be written as $(1\ x)'$, where

$$x \equiv \frac{y}{\Sigma\left(1-\rho\left[\beta\left(1-\delta-\mu\frac{p}{\lambda^*}\right) + h\eta^h_S\eta^S_\theta/\Sigma\lambda^*\right]\right)}. \tag{37}$$

Using the steady state surplus equation (20), this can be written as

$$x = \left(\frac{y}{y-b-\tau+h}\right)\left[\frac{1-\beta+\beta\delta+\beta\mu p}{1-\rho\beta+\rho\beta\delta+\rho\beta\mu p/\lambda^* - \rho h\eta^h_S\eta^S_\theta/(\Sigma\lambda^*)}\right]. \tag{38}$$

[31]We assume periods are short enough so that p is small, which means this eigenvalue is positive.

Saddle path stability implies that x is the elasticity $\tilde{\Sigma}_t/\tilde{y}_t$. Thus, in terms of the observable variable $\tilde{p}$, we have

$$\tilde{p}_t = (1-\lambda^*)\tilde{\theta}_t = \frac{1-\lambda^*}{\lambda^*}E_t\tilde{\Sigma}_{t+1} = \frac{1-\lambda^*}{\lambda^*}\rho x \tilde{y}_t. \tag{39}$$

Again we see that sufficiently inelastic search ($\lambda^*>0$) is essential for matching the data: (39) shows that the job finding probability is decreasing in productivity if $\lambda^*<0$.

Now using formula (38) for x, we obtain Eq. (26), which is used to derive Proposition 1. For Proposition 2, we linearize the dynamics (28) of unemployment:

$$\tilde{U}_{t+1} = (1-\delta)\tilde{U}_t - \delta\left(\frac{1-U}{U}\right)(1-\lambda^*)\tilde{\theta}_t - \delta\left(\frac{1-U}{U}\right)\tilde{U}_t. \tag{40}$$

On the saddle path, we have

$$\tilde{\theta}_t = \frac{1}{\lambda^*}E_t\tilde{\Sigma}_{t+1} = \frac{1}{\lambda^*}\rho\tilde{\Sigma}_t = \frac{1}{\lambda^*}\rho x\tilde{y}_t \tag{41}$$

so the dynamics of U become $\tilde{U}_{t+1} = A\tilde{U}_t - B\tilde{y}_t$, where we define $A \equiv (U-\delta)/U$ and $B \equiv \delta((1-U)(1-\lambda^*)/(U\lambda^*))\rho x$. This implies

$$\mathrm{Var}(\tilde{U}_t) = \frac{B^2(1+\rho A)}{(1-A^2)(1-\rho A)}\mathrm{Var}(\tilde{y}_t), \tag{42}$$

which simplifies to

$$\frac{\sigma_U}{\sigma_y} \equiv \sqrt{\frac{\mathrm{Var}(\tilde{U}_t)}{\mathrm{Var}(\tilde{y}_t)}} = \rho x(1-U)\left(\frac{1-\lambda^*}{\lambda^*}\right)\sqrt{\frac{\delta(U+\rho(U-\delta))}{(2U-\delta)(U+\rho(\delta-U))}}. \tag{43}$$

This equation, together with the formula (38) for x, and the formula (23) for the steady state comparative statics, gives us Proposition 2.

Appendix B. Data

Data sources. We study two data sets. First, we obtained the Labor Market Institutions Database (LMIDB), compiled by Steven Nickell and Luca Nunziata, from the webpage of the Centre for Economic Performance (it is an appendix to CEP Discussion Paper #502). The LMIDB database is constructed from OECD data on institutional and labor market characteristics of 20 countries for 1960–1994.

Our second dataset includes extensions of the LMIDB database added first by the IMF and then by Baker, Glyn, Howard, and Schmitt (BGHS). This dataset, available on the webpage of John Schmitt at the CEPR, extends most series to 1999 and includes some additional or alternative variables. When the BGHS data we need only exist up to 1997 or 1998, we extend the data to 1999 using the last available value.

Sample. The LMIDB and BGHS data sets include annual data from 1960 to 1994 and 1999, respectively, for Australia, Austria, Belgium, Canada, Denmark, Finland, France, Germany (including East from 1989 on), Ireland, Italy, Japan, Netherlands, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland, the UK, and the US. Before running our regressions, we average each variable, for each country, over five-year periods from 1960–1964 to 1995–1999. We exclude Portugal because of missing data, so in most regressions our panel contains 19 countries and 8 time periods.

Variable definitions. Except where stated otherwise, the following variables are identical to those in Nickell and Nunziata (2001), which can be consulted for further details.

Log unemployment rate: To construct our dependent variable, we first average the unemployment rate over 5-year periods, and then take the log.

Tax wedge: This is the sum of three tax rates: employers' contributions as a fraction of payments to labor, plus direct taxes as a fraction of household income, plus indirect taxes as a fraction of private expenditures. In our regressions we multiply this fraction by 100 to convert it to a percentage, for comparability with LN99, Table 15.

UI benefit replacement ratio: Initial UI benefits as a fraction of gross wage income. We multiply it by 100 to convert it to a percentage, for comparability with LN99. In some regressions, instead of including the tax wedge and UI benefit replacement ratio separately, we include their sum.

Benefit duration: This expresses UI benefits after the first year as a fraction of initial benefits b. If b_{23} is the benefit level in the second and third years of unemployment, and b_{45} is the level in the fourth and fifth years, benefit duration is $b^{-1}(0.6b_{23} + 0.4b_{45})$.

Employment protection: This is an index of the extent of legal impediments to firing, taking values from 0 (weak employment protection laws) to 2 (strong). We multiply it by 10 so that it is an index from 0 to 20, as in LN99.

Union density: This is the fraction of workers who are union members. We multiply it by 100 to convert it to a percentage, as in LN99.

Bargaining coordination: This is an index representing the extent to which wage bargaining is coordinated at a nationwide level, taking values from 0 (no coordination) to 3. We multiply it by 2, making it an index from 0 to 6, as in LN99.

Owner occupancy rate: This represents the fraction of households who own their homes. We multiply it by 100 to convert it to a percentage, as in LN99.

Active labor market policies (*ALMP*): This variable, available in the BGHS data but not the LMIDB, represents the fraction of GDP, per unemployed worker, spent by the government on job training and job matching. Since dividing by the unemployment rate causes an endogeneity problem, we also construct an instrument in which the fraction of GDP spent on ALMP is divided by the unemployment rate in the previous five-year period. ALMP is only available from 1985 to 1999, with some missing values which we have filled in by interpolation.

Mean output gap: This variable, available in the BGHS data but not the LMIDB, is an OECD estimate of the output gap as a fraction of GDP (positive when output is above potential). We never use this as a country-specific variable; instead, we include its (unweighted) cross-country mean as an alternative to including time dummies.

Web page: Our data files can be downloaded from the following web page: http://www.econ.upf.edu/~reiter/webbcui/bcui.html

This page also includes step-by-step information about the construction of our data set, STATA commands for running our regressions, and many regression results.

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