

The bright side of the moon: Unemployment insurance generosity and post-unemployment wages*

Mário Centeno

Álvaro A. Novo

Banco de Portugal & ISEG - U. Técnica

Banco de Portugal & ISEGI - U. Nova

mcenteno@bportugal.pt

anovo@bportugal.pt

June 6, 2007

Abstract

This paper assesses the gains from unemployment insurance (UI) by measuring its impact on post-unemployment wages. It takes advantage of a quasi-natural experimental setting generated by a reform of the Portuguese UI system that increased the entitlement period for some age-groups, while leaving it unchanged for other age-groups. This reform strongly increased the observed duration of subsidized unemployment. However, its impact was quite heterogeneous as a result of the income effect of UI: the impact was stronger for low income individuals. This paper finds that the new law had a small positive effect on reemployment wages, resulting in a wage distribution with higher mean and variance. Furthermore, there is evidence that the income effect of UI generated a stronger impact on reemployment wages of unemployed at the bottom of the pre-unemployment wage distribution. This evidence reinforces the interpretation of the UI income effect as a non-distortionary impact on the search behavior of the unemployed.

Keywords: Unemployment insurance; Reemployment wages; Income effect.

JEL Codes: J38, J65, J64, J68.

*We thank Instituto de Informática e Estatística da Segurança Social (IEESS) for making available to us the data, in particular, João Morgado for insightful discussions. Opinions expressed herein do not necessarily reflect the views of the *Banco de Portugal* and *IEESS*. All remaining errors are of our responsibility.

1 Introduction

The impact of unemployment insurance (UI) generosity on the quality of post-unemployment matches has been the subject of increased attention in the labor economics literature. The theoretical approaches of Acemoglu and Shimer (2000) and Marimon and Zilibotti (1999) predict a positive impact of UI generosity on the quality of job matches: without UI, workers will avoid the risk of unemployment by taking low productivity jobs that are easier to obtain, and firms will offer them insurance in the form of jobs with low unemployment risk, but with a premium in the form of lower wages. As a result of more generous UI better job matches emerge. There is some empirical evidence supporting this effect, with match quality measured in terms of post-unemployment wages and job stability. Centeno (2004) and Centeno and Novo (2006*b*) find some positive effect both in wages and job tenure for the United States, whereas Belzil (2001) reports positive but weaker evidence for job duration in Canada. Two recent studies analyze this issue for European countries. Fitzenberger and Wilke (2007), for Germany, and van Ours and Vodopivec (2006*a*), for Slovenia, report small or no effects in both variables. In this paper we associate good matches with high wages and study the impact of increased UI generosity in Portugal, a country with rigid labor market institutions, where a high employment rate coexists with a low unemployment rate. It takes advantage of a quasi-natural experimental setting generated by a reform of the Portuguese UI system that increased the entitlement period for some age-groups, while leaving it unchanged for other age-groups.

Centeno and Novo (2007) shows that this reform strongly increased the duration of subsidized unemployment, and that this impact was quite heterogeneous over the distribution of pre-unemployment income. While typically the impact was stronger for individuals at the bottom of the income distribution, there were different responses throughout the distribution of subsidized unemployment spells. Indeed, the unemployed at the top of the income distribution were more responsive at short spells, while the impact at longer spells was stronger for unemployed with lower income levels. These results can be interpreted as a sign of a significant income effect of UI – the increased generosity affected mostly the individuals with tighter budget constraints. With these results in mind, we study the impact of extended unemployment durations on wages. We ask if the provision of more generous UI allowed unemployed to find better matches, and how the impact differs depending on the level of (pre-unemployment) income. The latter captures the impact of generosity via an income effect.

In this paper, we use a quasi-natural experimental setting, generated by an exogenous increase in UI generosity, to identify the causal effect of an extension of the UI entitlement period on the potential gains in post-unemployment wages. The exogenous variation in generosity is the result of the July 1999 reform of the Portuguese UI system, which increased the entitlement period for those aged 30 to 34 years and, at the same time, left it remained unchanged for workers aged 35 to 39 years old. These two groups constitute our treatment and control groups, respectively. The availability of pre- and post-1999 information allows us to use a difference-in-differences methodology to control for common (macroeconomic) confounding factors. The heterogeneity in the impact is addressed using a quantile treatment effect framework (Koenker 2005).

We use Portuguese administrative data from the Social Security UI dataset covering *all* subsidized unemployment spells that ended during the 1998-2002 period. Several characteristics of this dataset make it particularly suitable for our study, namely, the availability of information on (i) the salary and starting date of the first job following unemployment; (ii) spells initiated both in the period prior to and after the July 1999 reform; and (iii) the wage earned prior to entering unemployment.

Our results show a small, but statistically significant, positive impact of the extended entitlement period with important heterogeneous effects throughout the distribution of reemployment wages. Quantile treatment effects suggest that the treatment group benefited from the reform with an estimated gain on post-unemployment wages lying around 3 percent. However, the impact is larger at upper wage quantiles (around 4 percent) and lower for wage quantiles below the median (around 2 percent). We also collected evidence that the extended period not only resulted in wage gains for the treatment group, but also that the dispersion of wages increased due to the policy.

These results seem to indicate that longer subsidized unemployment spells are associated with higher reemployment wages. In view of Marimon and Zilibotti (1999) and Acemoglu and Shimer (2000), this is an expected result, since longer subsidized unemployment spells allow workers to be more selective and look for better matches. This result comes about even for an UI system that induces rather long unemployment spells.

To further explore this issue, we compare the reemployment wages of unemployed from different parts of the pre-unemployment income distribution. This allows us to investigate whether the income effect of UI had any impact on post-unemployment match quality. Our

results show that workers from the lowest quartile of the pre-unemployment income distribution benefited the most from the increased generosity. Indeed, the difference-in-differences estimated impact on reemployment wages falls as we move towards the highest quartiles of the pre-unemployment income distribution. This evidence reinforces the interpretation of the UI income effect as a non-distortionary impact on the search behavior of the unemployed.

The paper is organized as follows. In section 2, we review the theoretical motivation for our analysis and previous empirical evidence. The econometric methodology is reviewed in section 3. Section 4 sketches the Portuguese UI system and the changes introduced in 1999. We present the data in section 5. The final sections present the results and the conclusions.

2 Literature: Theory and evidence

There are basically two alternative views about the way the job matching process evolves. We can see the job matching process as one of search for new employment positions. A Diamond-type model Diamond (1982) or Jovanovic (1979) model can be used to describe the mechanisms that allow workers to achieve a better job match, usually characterized as a job with higher wage. In this way, the matching process depends on factors such as outside opportunities and expectations about future wages. Alternatively, one can see the process of job matching in the context of a non-market clearing model of the labor market, in which wages are fixed above the equilibrium level, jobs are rationed, and the main force behind the process of job changes is the so called vacancy chain. In this kind of model quits are procyclical because vacancy chains are longer when unemployment is low (see Akerlof, Rose and Yellen (1988)).

The impact of labor market policies, particularly the UI system, on productivity and job mismatch has recently been examined in several theoretical papers. Marimon and Zilibotti (1999) present a model of the role of UI on mismatch and unemployment and show the positive impact of the UI system on the reduction of job mismatch. In a related paper, Acemoglu and Shimer (2000) analyze the productivity gains from more generous UI systems. Considering risk averse workers, they show that UI increases labor productivity by encouraging workers to seek higher productivity jobs and by encouraging firms to create these jobs. In their setting, the UI is more than a search subsidy, and affects the type of jobs that workers look for and accept.

Additionally, we need to take into account that benefits do not last forever and that the effect of UI in the reservation wage might not be constant over time. UI benefits are received

for a fixed period of time. If someone unemployed reaches the end of this period without finding an "acceptable match", we might expect the reservation wage to gradually fall over time. Following Mortensen (1977), the escape rate from unemployment decreases for a newly laid-off worker eligible for UI benefits with both the benefit duration and the benefit amount. This result implies that the congestion pressure will decrease with both UI parameters. For a non-eligible or exhaustee worker the opposite result applies, given that the value of being unemployed and eligible for UI benefits following an employment spell increases with the benefit level. The model in Chetty (2005) can be used to motivate our analysis of heterogeneous outcomes over the pre-unemployment income distribution. In Chetty's setting, the impact of UI is differentiated on the basis of the degree of borrowing constraints faced by unemployed workers. This dimension allows us to add to the typical substitution effect, the possibility of a non-distortionary income effect. If this income effect is important, the disincentive of UI created through the substitution effect can be reduced, and become less distortionary than previously thought. The non-distortionary nature of the income effect, reducing the pressure of low income workers to accept bad quality matches and allowing them to wait for a better match, should be associated with a greater impact on post-unemployment wages.

The impact of UI on match quality remains, nonetheless, an empirical issue. There are only a limited number of studies addressing the impact of UI on post-unemployment outcomes, and they have concentrated almost exclusively on the wage dimension of match quality. Belzil (2001) looks at job duration exploring a reduction in the initial entitlement period rule in Canada to study the impact of UI duration on subsequent job duration, and reports a weak but positive impact of the maximum benefit duration on subsequent job duration, for a sample of young Canadian male workers. Addison and Blackburn (2000) analyze the impact of UI on post unemployment job stability using a sample of displaced workers taken from the Current Population Survey. They find weak positive evidence that UI recipients are more likely to have had only one job following displacement (rather than holding more than one job). More recently, Centeno (2004) and Centeno and Novo (2006*b*) look at the US system, using variation across states, and find some evidence that more generous UI increases the tenure of post-unemployment and that this impact is stronger at long duration.

The older literature on the impact of UI on post-unemployment earnings is not completely conclusive. While most studies found a positive (frequently small) impact; some studies found it to be statistically non-significant. The best-known paper is probably the one by Ehrenberg

and Oaxaca (1976). They found evidence that higher UI benefits make the process of job search more productive, resulting in longer unemployment spells and higher post-unemployment wages. Their results indicate that an increase in UI benefits has a significantly greater impact in the older male sample, than in the female and younger male samples. A number of other studies are reviewed in Burtless (1990), and Cox and Oaxaca (1990), which draw seemingly opposite conclusions from the available evidence. More recently, Addison and Blackburn (2000) conclude for the existence of, at most, a weak effect of UI on post-unemployment earnings. Their results point to a positive, but statistically non-significant, effect of UI for a sample taken from the 1988, 1990 and 1992 Displaced Workers Supplement from the January Current Population Survey. The main conclusion from this literature is that studies using samples of UI claimants found little beneficial effects of UI on wages, while studies comparing recipients with nonrecipients usually found more significant impact estimates.

3 Methodology

In the context of a job-search model, we expect UI to increase the length of unemployment spells by raising the reservation wage. However, the capacity to adjust the reservation wage is arguably not homogenous across all individuals. In other words, we expect differentiated impacts at different locations of the distribution, which can be estimated with quantile regression.

3.1 Quantile regression

Quantile regression, first introduced by Koenker and Bassett (1978), specifies and estimates a family of conditional quantile functions, $Q_{y|x}(\tau|x) = x\beta(\tau)$, where Q is the conditional quantile function of Y given X , a vector of conditioning variables, and τ is a quantile in the interval $[0, 1]$. In this respect, quantile regression is similar to the rather more ubiquitous mean regression method. The least squares estimator also specifies a linear function of conditioning variables, namely, the conditional mean function, $E[Y|X = x] = x\beta$.

Thus, quantile regression has a descriptive advantage over least squares by providing several summary statistics of the conditional distribution function, rather than just one characteristic, namely, the mean. Ultimately, with point estimates of $\beta(\tau)$, quantile regression allows us to characterize and distinguish the effects of covariates on the upper and lower quantiles of the

distribution.

3.2 Quantile treatment effects

The concept of quantile treatment response was first proposed by Lehmann (1975) as:

Suppose the treatment adds the amount $\Delta(y)$ when the response of the untreated subject would be y . Then the distribution G of the treatment responses is that of the random variable $Y + \Delta(Y)$ where Y is distributed according to F .

In this structure, the treatment may be, for instance, equally beneficial (prejudicial) to all subject, in which case the two distributions will differ by a constant, $\Delta(Y) = \delta_0 > 0$ ($\Delta(Y) = \delta_0 < 0$). In this case, the quantile treatment response does not differ from the standard average treatment response. The treatment exerts a pure location shift on the distribution of the treated. The response may also be a function of the pre-treatment value, for example, $\Delta(y) = \delta_0 y$. While in the former case the two distributions have the same shape, but different locations, in the latter both the location and shape differ. In this case the literature refers to a location and scale shift.¹

The connection between quantile treatment responses and quantile regression is obvious from the work of Doksum (1974). Doksum defines $\Delta(y)$ as the “horizontal distance” between the cumulative distributions F and G measured at y so that $F(y) = G(y + \Delta(y))$. Then, $\Delta(y) = G^{-1}(F(y)) - y$. Thus, changing notation, $\tau = F(y)$, to conform with the quantile regression notation introduced above, we have that the Quantile Treatment Effect (QTE) is defined as:

$$\delta(\tau) = \Delta(F^{-1}(\tau)) = G^{-1}(\tau) - F^{-1}(\tau). \quad (1)$$

In the two-sample case, the quantile treatment effect (QTE) is simply estimated by the sample analogous of equation (1), namely,

$$\hat{\delta}(\tau) = \hat{G}_n^{-1}(\tau) - \hat{F}_m^{-1}(\tau),$$

where G_n and F_m denote the empirical distribution functions of the treatment and control groups, respectively.

¹We will address distributional shifts hypothesis testing in the following subsection.

The identification hypotheses of the average treatment effect on the treated and the QTE are similar, in which both arise from the fundamental problem of causal inference – the non-observation of the counterfactual. Thus, the analogous identification hypothesis in QTE is that the distribution of potential outcomes in the absence of the treatment (y_0) for treated ($D = 1$), $G_{y_0|D=1}$, would be the same as that of the control units, $F_{y_0|D=0}$. To control for time invariant differences between the treatment and control group, we extend the quantile treatment effect in the same fashion as the difference-in-differences literature. Thus, we need an additional identification hypothesis, namely,

$$G_{y_0(t')|D=1}^{-1}(\tau) - G_{y_0(t)|D=1}^{-1}(\tau) = F_{y_0(t')|D=0}^{-1}(\tau) - F_{y_0(t)|D=0}^{-1}(\tau), \quad \forall \tau. \quad (2)$$

This hypothesis expresses the condition that the difference over time (from t to t') between the distributions of potential outcomes in the absence of the treatment would have been the same for treated and non-treated subjects. Contrary to the D-in-D hypothesis, which assumes an homogenous difference throughout the entire distribution, this hypothesis allows for distinct differences across quantiles. The only restriction is that the differences for a quantile remain the same over time.

Thus, our identification hypothesis allows us to identify the quantile treatment effect as

$$\begin{aligned} \delta(\tau) &\equiv G_{y_1(t')|D=1}^{-1}(\tau) - G_{y_0(t')|D=1}^{-1}(\tau) \\ &= G_{y_1(t')|D=1}^{-1}(\tau) - G_{y_0(t')|D=1}^{-1}(\tau) + \{G_{y_0(t')|D=1}^{-1}(\tau) - G_{y_0(t)|D=1}^{-1}(\tau)\} - \\ &\quad \{F_{y_0(t')|D=0}^{-1}(\tau) - F_{y_0(t)|D=0}^{-1}(\tau)\} \\ &= \{G_{y_1(t')|D=1}^{-1}(\tau) - G_{y_0(t)|D=1}^{-1}(\tau)\} - \{F_{y_0(t')|D=0}^{-1}(\tau) - F_{y_0(t)|D=0}^{-1}(\tau)\}. \end{aligned} \quad (3)$$

In the 4-sample case, this is estimable by the sample quantiles. Extensions to account for differences in observable characteristics of the subjects are estimated with quantile regression, in a similar fashion to the estimation of the difference-in-differences estimator with least squares. See Koenker (2005) for a thorough discussion and illustrations of quantile treatment effects.

3.3 Quantile regression inference on distributional shifts

The work of Koenker and Xiao (2002) on statistical inference for the entire quantile regression process offers extremely attractive tools in the present context. It allows for testing two ways

in which two distributions may differ, namely, by a *location* shift and by a *location* and *scale* shift. The description of the QTE has already motivated the importance of testing for such shifts. Anticipating a little what we will do in the empirical section, a simple regression of (log) reemployment wages on a constant and the UI generosity indicator variable together with the inference framework allow us to test the hypothesis that the distribution under a “more generous UI”, G , differs from the distribution arising in a “less generous UI”, F , either by a pure location shift

$$G^{-1}(\tau) = F^{-1}(\tau) + \delta_0, \quad \forall \tau \in [0, 1], \quad \delta_0 \in \mathbb{R}, \quad (4)$$

or by a location-scale shift

$$G^{-1}(\tau) = \delta_1 F^{-1}(\tau) + \delta_0, \quad \forall \tau \in [0, 1], \quad \delta_0, \delta_1 \in \mathbb{R}, \quad (5)$$

where F^{-1} and G^{-1} are as above. In other words, equation (4) tells us that all τ -th quantiles of F and G differ by a constant, δ_0 ; a pure location change model, which corresponds to the classical homoskedastic linear regression model. On the other hand, equation (5) transforms all τ -th quantiles of F into the respective τ -th quantiles of G by an affine transformation – a location change, δ_0 , and a scale change δ_1 .

A full description of the technical procedures, as well as, an empirical application into the effects of a reemployment financial bonus on the duration of subsidized unemployment spells can be found in Koenker and Xiao (2002).

4 The unemployment system reform and identification

4.1 The extension of some entitlement periods

The Portuguese UB legislation establishes only one eligibility criterium, namely, the employment history with social contributions, requiring a minimum 540 days of contributions in the 24 months before unemployment. Benefits are then set as a percentage of the monthly average of the previous wages. Figure 1 illustrates graphically the financial generosity of the system expressed in terms of the gross replacement rate (GRR). The amount of UB (and GRR) paid is defined in accordance with the level of pre-unemployment average earnings:

1. For individuals with average earnings below the minimum wage (W^{min}), for example,

- part-timers, the GRR is 100 percent;
2. If the average earnings fall in the interval $[W^{min}, W^{min}/0.65)$, then the UB will equal the national minimum wage, which, in 1999, stood at 61,300 escudos/month (305.76 euros);
 3. In the next interval, $[W^{min}/0.65, 3W^{min}/0.65)$, the UB will represent 65% of the previous earnings;
 4. Finally, for individuals with average monthly earnings above $3W^{min}/0.65$, the UB is set to its maximum of 3 minimum wages.

Figure 1

The analysis will be focused on the unemployed with GRRs of 65 percent, which translates roughly into average monthly earnings ranging from 1.5 to 4.5 minimum wages. This choice, while still allowing for a substantial wage variability, aims at guaranteeing similar substitution effects, therefore, eliminating a possible source of differentiated behavior among individuals.²

One peculiar feature of the Portuguese system is the definition of the entitlement period. It is fully determined by the individuals' age at the beginning of the unemployment spell. It was precisely the entitlement period that was changed, in July 1999, for some age groups in the population.

Before the reform, the Portuguese legislation divided workers into 8 age-groups with different entitlement periods. The reform made this period larger for 6 out of the 8 groups, leaving the remaining two groups unchanged (see Table 1). The pre-1999 duration of benefits ranged from a minimum of 10 months for those aged less than 25 years old to a maximum of 30 months for those aged 55 or more. The new legislation changed the lower bound to 12 months, while the upper bound can now reach 38 months.

Table 1

The characteristics of the reform result in two natural pairs of treatment and control groups, namely, $([15, 24], [25, 29])$ and $([30, 34], [35, 39])$. For comparability reasons, we chose the latter. For the younger cohort the results are likely to be contaminated by factors other than labor market attachment (e.g. education choices), making the treatment and control groups less

²In the data, some ratios of benefits to previous wages are not exactly equal to 65 percent. Therefore, for sample size reasons, we keep observations with $GRR \in [63, 65]$.

comparable. On the contrary, the treatment group, [30, 34], is likely to share similar labor market characteristics with the control group, [35, 39], for instance in terms of schooling, marital status, child bearing, among others. In our case, this ex-ante comparability gains additional importance due to some data limitations.

4.2 Economic conditions

At the moment of the reform, the Portuguese labor market and the economy were in a strong condition (see Table 2). In the period just prior the reform, real GDP growth was above 4 per cent and employment growing consistently above 2 per cent. The unemployment rate was at or below 5 per cent, showing signs of a tight labor market situation.

Table 2

The business cycle started to change only after mid-2001, with both GDP and employment growth rates declining. It was also visible in the turning point in unemployment, after the all-time low rate in 2000. The large share of long-term unemployment, a characteristic of the Portuguese labor market, remained above 40 per cent until 2002. After that, the surge in the separation rate associated with the recession led to a fable employment growth and a significant hike in the unemployment rate.

It is worth noting that the good conditions prevailing at the moment of the reform are favorable for our empirical strategy. Indeed, they show that the policy change was not driven by the evolution of the labor market. Furthermore, its application to prime-age workers, who usually suffer less with labor market swings, makes our comparison of pre- and post-reform outcomes more convincing, as it is not driven by a specific trend in the labor market.

5 Data

Our study is based on administrative data collected by the Portuguese government's agency *Instituto de Informática e Estatística da Segurança Social* (IEESS). The dataset registered *all* unemployment related social transfers that took place between 1998 and 2004. It contains very detailed and reliable information on the type, amount and duration of benefits, the previous wage, i.e., the income that served as reference to compute the amount of UB and, where applicable, the first reemployment wage and starting date of the job. Unfortunately, the socio-

demographic variables available are limited to gender, age, nationality and local of residence. Table 3 contains summary statistics of the key variables.

Table 3

With the aforementioned restriction of GRRs in the interval $[63\%, 67\%]$ and considering only spells terminated with reemployment, we have a total of 9,675 subsidized unemployment spells. The treatment group is comprised of 4,901 observations, of which 2,232 are observed before July 1999. The control group has 2,725 observations in the before period and 2,049 in the following period. Figure 2 plots the histogram of the real reemployment wages (at 1999 prices). We impose the restriction that wages should fall in the interval bounded below by the minimum wage and above by 1.5 the range of wages observed before the unemployment spell. The latter were in turn limited by having chosen the GRR of 65 percent. Given the low level of wage prevailing in the Portuguese economy, and the stylized facts of unemployment insurance, the bulk of observations is concentrated on the left tail of the distribution. From Table 3 is also clear that the average reemployment wage is below the average wage before the unemployment spell. There are 3 moments in the subsidized unemployment spell where we observed more reemployment experience, namely, in the first 60 days or then later between the 4 months and 1 year. For the control group between 23 and 29 of the reemployment occurred after the end of the benefits. A similar value is observed for the treatment group in the after period, 26 percent, but notice that it used to represent only 12 percent of the individuals.

Figure 2

6 Results

We analyze the implications of the 1999 UI legislation change in terms of a key post-unemployment variable – reemployment wages. First, we study the determination of the distribution of the post-unemployment wages. Then, we explore how the income effect generated by the more generous UI impacted on the reemployment wages of different levels of pre-unemployment average income.

6.1 Reemployment wages: Average and quantile treatment effects

We start by presenting a simple view of the impact of the additional period of subsidized unemployment. Table 4 reports difference-in-differences (D-in-D) estimates of the average treatment effect on the treated. The top panel reports the unrestricted D-in-D estimates, while the bottom panel conditions the estimates on a set of variables, including previous average income, indicator variables for unemployment duration (piecewise function) and a dummy variable that captures the event of benefits exhaustion. A gender variable and dummy variables for regional labor markets and month of unemployment and of reemployment were also included.

Table 4

The unrestricted D-in-D estimate suggests that the extension of the entitlement period did not on average impacted (-0.35 percent) on the reemployment wages of the individuals aged 30 to 34 years old. The point estimate of the $After \times Treat$ variable in the restricted D-in-D specification indicates that treated individuals benefited marginally from the new policy. Relatively to the no treatment counterfactual, wages increased 2.76 percent.

We acknowledge, however, that this type of policies is likely to result in heterogeneous responses. Indeed, Centeno and Novo (2007) have already shown that this policy change induced differentiated responses of the unemployed along the distribution of subsidized unemployment spells. Thus, we now turn to estimates of the quantile treatment effects on reemployment wages.

The quantile regression analysis hypothesizes that the logarithm of reemployment wages, $\log(W)$, have linear conditional quantile functions, Q , of the form:

$$Q_{\log(W)}(\tau) = \beta_0(\tau) + \beta_1(\tau)After + \beta_2(\tau)Treat + \beta_3(\tau)After \times Treat + x'\lambda(\tau), \quad (6)$$

where $After$ is an indicator variable for the post-July 1999 period, $Treat$ indicates the age group affected by the new legislation. Additionally, the vector x includes the variables already mentioned in the restricted D-in-D specification (see Table 4).

Figure 3 presents the estimation results in a concise format. We include the same set of conditioning variables that were included in the D-in-D estimation, although we omit from the figure the regional, seasonal and gender dummies. Each panel represents the point estimates

of the coefficient associated with the respective variable for each of the estimated quantiles. We chose to limit our attention to the quantiles $\tau \in [0.10, 0.90]$.³ The shaded areas represent 90 percent confidence intervals.

Figure 3

The quantile treatment effects tell us a story of heterogeneity. First, the point estimates are positive for all quantiles. Secondly, the impact is different for quantiles below and above the median. In particular, for reemployment wages below the median the impact is around 2 percent, although statistically the impact is marginally non-significant. Above the median reemployment wage the effect jumps up to values of around 4 percent and statistically significant. From an economic point of view, the impacts generated by the longer entitlement periods are sizeable. This evidence, taken together with the results for the United States in Centeno and Novo (2006a), shows that the typical moral hazard interpretation of the impact of unemployment insurance system on the duration of unemployment might be mitigated by the positive impact that the system has on job match quality as proxied by reemployment wages.

6.2 Reemployment wages: Distribution shifts

Hitherto, it is obvious that the new legislation impacted on the distribution of reemployment wages. What we have not answered yet is how the distribution changed. Was it a simple location shift, increasing homogeneously all wages? Or, was it a location and scale shift, affecting not only the location of the distribution (mean), but also its shape (dispersion)? Koenker and Xiao (2002) provide us with the inference tools to answer (test) formally these two questions (hypotheses).

Table 4 reports test statistics for the distributional shifts. On the top panel, the contribution of each variable to the distributional shift is tested. The bottom panel reports the statistics for the joint hypothesis. The latter reveal that the distribution shift of log durations imposed by the entire set of covariates does not conform to neither of the null hypotheses, that is, both null hypotheses are rejected. It is, however, possible that individually a covariate induces distributional shifts of the type being tested. For the current exercise, we focus our attention on the variable identifying the quantile treatment effect, $After \times Treat$, which carries the most interest.

³It is worth emphasizing that all observations are used in the estimation process, despite the omitted quantiles in the plots.

Koenker and Xiao (2002) warn us to the usual statistical objections that arise in analyzing individual coordinates that are not independent. Nonetheless, with this caveat in mind, we now learn about the individual contribution of each variable to the distributional shifts. Intuitively, if a variable exerts a location shift on the conditional distribution then graphically the estimated coefficients along the quantiles should be constant (horizontal line). If the plot of the coefficient resembles the intercept up to an affine transformation, then it is more likely that it affects the location and scale of the conditional distribution. With this in mind, and looking back at Figure 3 it is not surprising that the test statistic fails to reject both the location and location-scale shift hypotheses for the impact of the policy change. The flat profile of the coefficient estimates for the variable $\text{After} \times \text{Treat}$ would suggest that a location shift, but the slight increase over the quantile could also point towards a location-scale shift. Irrespective of the hypothesis, notice that we are testing the effect on the logarithm of reemployment wages, and therefore even if we accept a location hypothesis we are indeed implying that the conditional distribution of reemployment wages shifted disproportionately more to the right. That is, not only did the location change, but also the variance of the untransformed variable. The two variables that most resemble the intercept are the log previous average income and the indicator variable for reemployment after UI has exhausted. The test statistics confirm this visual inspection and the location shift hypothesis is rejected for both variable, while the location-scale hypothesis is not rejected.

In conclusion, the July 1999 extension of the entitlement period resulted in higher reemployment wages (location shift) and also in larger variance (scale shift). These impacts mitigate the moral hazard problems associated with the unemployment insurance system and support the models of Marimon and Zilibotti (1999) and Acemoglu and Shimer (2000).

6.3 UI income effect and reemployment wages

The increase in the generosity of the UI system generates a non-distortionary income effect that should be felt more strongly by individuals facing financial constraints (Chetty 2005). Using the same experiment, Centeno and Novo (2007) shows that the extension of the entitlement period resulted in longer spells of unemployment for all individuals, but, in particular, for the set of individuals with the lower levels of pre-unemployment average income.⁴

⁴As argued in Centeno and Novo (2007), average income is a good proxy for financial constraints in the Portuguese economy due to the low level of financial assets hold by the households.

We now follow a similar strategy to assess the impact that the non-distortionary income effect might have had on job match quality as proxied by reemployment wages. If the impact is stronger for those at the bottom of the pre-unemployment wage distribution we can interpret it as evidence in favor of an income effect of UI. If the increased UI generosity has a larger impact on unemployment duration of these individuals and if this translates into better post-unemployment outcomes for the same group of individuals, then the UI impact can be viewed as non-distortionary of their search behavior.

We divide the sample in 4 subsamples by quartiles of pre-unemployment average income. Then, we study the impact of the extension on reemployment wages by specifying the linear model considered earlier.⁵ Table 6 presents the restricted D-in-D point estimates. First, there is some marginal evidence that the group of financially more constrained individuals was the only one to benefit from the new policy; an average treatment effect on reemployment wages of 4.9 percent. Secondly, the treatment effect decreases monotonically with the quartile of previous average income. Indeed, although none of the point estimates is statistically significant, the impact decreases and is negative for 4th quartile.

Table 6

This small positive impact on reemployment wages for the group of financially constrained individuals, those who are more prone to benefit from the UI income effect, is taken as a positive outcome of the unemployment insurance system. Again, the system seems to benefit those that it intends in the first place to help – those who find it harder to smooth consumption when a negative shock hits their labor market stance.

7 Conclusions

The impact of social insurance programs, particularly of unemployment insurance programs, has increasingly attracted the attention of empirical economics. However, this attention has primarily been directed towards estimating the disincentive effects of these programs. The purpose of this paper is to analyze the relationship between the quality of job matches (measured by the wage) and UI system generosity. We found some evidence that UI generosity

⁵Relatively to the earlier specification, we actually drop the variable log previous average income from the right hand side because we are selecting samples based on it. Including this variable does not change the qualitative results.

increases the wages after unemployment and some support in favor of an income effect of UI in match quality, as increased generosity seems to benefit more those at the bottom of the wage distribution.

We take advantage of a quasi-natural experiment in Portugal generated by the 1999 reform of the UI system to identify the impact of unemployment benefits. The paper investigates if the extended entitlement period, which increased subsidized unemployment duration, also allowed workers to be more selective and implied higher post-unemployment wages. Indeed, if the increase in the entitlement period altered the search behavior of unemployed workers, by increasing their reservation wages, it would be more likely that the post-unemployment jobs would have a higher wage. We find supporting evidence.

These results, together with those of a companion paper (Centeno and Novo (2007)) suggest that the altered behavior of the unemployed delivered a more productive search period. The companion paper shows that the increase in the entitlement period was taken up by the workers in the form of longer unemployment spells. This result is somehow at odds with the one reported in van Ours and Vodopivec (2006b), that finds a null impact on wages upon a reduction in the entitlement period in Slovenia. It is, however, possible that the impact of variations in UI generosity is not symmetric. Complementarily, our preferred interpretation comes from the observed strong income effect generated by UI generosity in the Portuguese labor market. The treatment groups wage gains in Portugal were concentrated on the unemployed with the lowest pre-unemployment income. These were also the individuals that showed the largest increase in subsidized unemployment duration. If the impact of UI is mediated through this non-distortionary income effect, we should expect its positive effect to be larger. In that sense, the impact of increased generosity allowed a fraction of Portuguese unemployed to wait for the bright side of the moon.

References

- Acemoglu, D. and Shimer, R. (2000), 'Productivity gains from unemployment insurance', *European Economic Review* **44**, 1195–1224.
- Addison, J. and Blackburn, M. (2000), 'The effects of unemployment insurance on postunemployment earnings', *Labour Economics* **7**, 21–53.
- Akerlof, G., Rose, A. and Yellen, J. (1988), 'Job switching and job satisfaction in the U.S. labor market', *Brooking Papers on Economic Activity* pp. 495–582.
- Belzil, C. (2001), 'Unemployment insurance and subsequent job duration: Job matching versus unobserved heterogeneity', *Journal of Applied Econometrics* **16**, 619–636.
- Burtless, G. (1990), Unemployment insurance and labor supply: a review, *in* W. Hansen and J. Byearnsand, eds, 'Unemployment insurance: The second half-century', University of Wisconsin Press, Madison, WI, pp. 69–107.
- Centeno, M. (2004), 'The match quality gains from unemployment insurance', *Journal of Human Resources* **39**(3), 839–863.
- Centeno, M. and Novo, Á. A. (2006a), 'The impact of unemployment insurance generosity on match quality distribution', *Economic Letters* **93**, 235–241.
- Centeno, M. and Novo, Á. A. (2006b), 'The impact of unemployment insurance on the job match quality: A quantile regression approach', *Empirical Economics* **31**, 905–919.
- Centeno, M. and Novo, Á. A. (2007), Heterogeneity of the unemployment insurance system: Lazyness or simply income effects?, mimeo, Banco de Portugal.
- Chetty, R. (2005), 'Why do unemployment benefits raise unemployment durations? The role of borrowing constraints and income effects', *NBER* **11760**.
- Cox, J. C. and Oaxaca, R. L. (1990), Unemployment insurance and job search, *in* L. Bassi and D. Crawford, eds, 'Research in Labor Economics', Vol. 11, JAI Press, pp. 223–240.
- Diamond, P. (1982), 'Aggregate demand management in search equilibrium', *Journal of Political Economy* **90**(5), 798–812.

- Doksum, K. (1974), ‘Empirical probability plots and statistical inference for nonlinear models in the two-sample case’, *Annals of Statistics* **2**, 267–277.
- Ehrenberg, R. and Oaxaca, R. (1976), ‘Unemployment insurance, duration of unemployment, and subsequent wage gain’, *American Economic Review* **66**(5), 754–766.
- Fitzenberger, B. and Wilke, R. (2007), ‘New insights on unemployment duration and post unemployment earnings in germany: Censored Box-Cox quantile regression at work’, *IZA* **2609**.
- Jovanovic, B. (1979), ‘Job matching and the theory of turnover’, *Journal of Political Economy* **87**, 972–990.
- Koenker, R. (2005), *Quantile regression*, Cambridge University Press, Cambridge.
- Koenker, R. and Bassett, G. (1978), ‘Regression quantiles’, *Econometrica* **46**, 33–50.
- Koenker, R. and Xiao, Z. (2002), ‘Inference on the quantile regression process’, *Econometrica* **70**, 1583–1612.
- Lehmann, E. (1975), *Nonparametrics: Statistical Methods Based on Ranks*, Holden-Day, San Francisco.
- Marimon, R. and Zilibotti, F. (1999), ‘Unemployment vs. mismatch of talents: Reconsidering unemployment benefits’, *The Economic Journal* **109**, 266–291.
- Mortensen, D. (1977), ‘Unemployment insurance and job search decisions’, *Industrial and Labor Relations Review* **30**(4), 505–517.
- van Ours, J. C. and Vodopivec, M. (2006a), ‘How changes in benefits entitlement affect job-finding: Lessons from the Slovenian “Experiment”’, *Journal of Labor Economics* **24**(2), 351–378.
- van Ours, J. C. and Vodopivec, M. (2006b), ‘Shortening the potential duration of unemployment benefits does not affect the quality of post-unemployment jobs: Evidence from a natural experiment’, *CEPR* **5741**.

Table 1: Entitlement periods (in months): Before and after July, 1999

Group	Before		After	
	Age (years)†	Entitlement period	Age (years)†	Entitlement period
(1)	[15, 24]	10	[15, 29]	12
(2)	[25, 29]	12		
(3)	[30, 34]	15	[30, 39]	18
(4)	[35, 39]	18		
(5)	[40, 44]	21	[40, 44]	24
(6)	[45, 49]	24		
(7)	[50, 54]	27	[45, 64]	30(+8)*
(8)	[55, 64]	30		

† Age at the beginning of the unemployment spell.

* For those aged 45 or older, 2 months can be added for each 5 years of social contributions during the past 20 calendar years.

Table 2: The Portuguese economy before and after July 1999

	Real GDP Growth	Employment Growth	Unemployment Rate	Long-term Unemployment (%)
1997	4.2	1.9	5.8	43.6
1998	4.7	2.3	5.0	45.4
1999	3.9	1.9	4.4	41.2
2000	3.9	2.3	3.9	43.8
2001	2.0	1.5	4.0	40.0
2002	0.8	0.5	5.0	37.3
2003	-1.2	-0.4	6.3	37.7
2004	1.1	0.1	6.7	46.2

Sources: National accounts, INE; Employment Survey, INE

Table 3: Summary statistics: Mean values and number of observations

	Treatment		Control	
	Before	After	Before	After
Age	32.2	32.2	37.5	37.1
Female	0.33	0.45	0.32	0.38
Spell duration (in days)	227.6	254.0	324.9	274.5
Percentage reemployed within:				
[1, 60] days	0.15	0.21	0.11	0.17
[61, 90] days	0.08	0.09	0.05	0.08
[91, 120] days	0.08	0.08	0.05	0.08
[121, 240] days	0.24	0.19	0.16	0.21
[241, 360] days	0.20	0.11	0.15	0.10
[361, 449] days	0.16	0.05	0.11	0.04
[450, 539] days	-	0.02	0.11	0.02
Reemployed on UI limit	0.02	0.11	0.08	0.12
Reemployed after UI	0.07	0.14	0.18	0.17
Wages (1999 prices):				
Previous	684.01	744.01	714.89	743.82
Reemployment	650.38	708.89	623.14	682.84
GRR	0.65	0.65	0.65	0.65
No. observations	2,232	2,669	2,725	2,049

Notes: IIES dataset with authors' computations. (1) The previous wage of each individual is computed as the average of reported wages over the period of 12 months that preceded the job loss in 2 months. (2) Real wages are expressed in 1999 euros.

Table 4: Average treatment effect on (log) reemployment wages: D-in-D estimates

	Estimate	Std. Error	t value	Pr(> t)
<i>Unrestricted D-in-D</i>				
Intercept	6.3229	0.0091	692.93	0.0000
After	0.0728	0.0139	5.2374	0.0000
Treat	0.0386	0.0136	2.8382	0.0046
After × Treat	-0.0035	0.0195	-0.1795	0.8587
<i>Restricted D-in-D</i>				
Intercept	3.0417	0.1028	29.5900	0.0000
After	0.0027	0.0134	0.2000	0.8427
Treat	-0.0105	0.0125	-0.8400	0.4011
After × Treat	0.0276	0.0176	1.5700	0.1168
Log(Prev. avg. wage)	0.4891	0.0151	32.4400	0.0000
Reemployed within:				
[1, 60] days	0.1395	0.0242	5.7700	0.0000
[61, 90] days	0.1279	0.0269	4.7600	0.0000
[91, 120] days	0.1065	0.0267	3.9800	0.0001
[121, 240] days	0.0864	0.0236	3.6600	0.0003
[241, 360] days	0.0600	0.0242	2.4700	0.0134
[361, 449] days	0.0103	0.0257	0.4000	0.6883
Reemployed on UI limit	-0.3250	0.0261	-12.4521	0.0000
Reemployed after UI	-0.2898	0.0241	-12.0249	0.0000
Female	0.0038	0.0091	0.4200	0.6733
Dummies:				
Regional			- Yes -	
Month of unemployment			- Yes -	
Month of reemployment			- Yes -	
No. of observations	9,675			
R ²	0.226			

Table 5: Reemployment wages: Location shift and location-scale shift test statistics

	Location	Location-scale
Individual hypothesis		
After x Treat	0.7936	1.0157
After	1.1168	0.8736
Treat	0.8783	0.7799
log(Previous average income)	6.3944	2.6715
Reemployed after UI	2.8045	2.2346
Joint hypothesis		
Statistic	189.48	98.95

Notes: (1) The individual test statistic critical values are 2.420, 1.923 and 1.664 at the 1, 5 and 10 percent levels, respectively. The critical values for the joint hypothesis are 20.14, 18.30 and 17.38 for the same levels. (2) The reemployment period, gender, regional and seasonal indicator variables were included in the specification, but omitted here.

Table 6: Average treatment effect on (log) reemployment wages: D-in-D estimates by quartile of previous average income

	Previous (12 months) average wages quartiles							
	1st		2nd		3rd		4th	
	Coef.	Pr(> t)	Coef.	Pr(> t)	Coef.	Pr(> t)	Coef.	Pr(> t)
Intercept	6.1592	0.000	6.2427	0.000	6.1993	0.000	6.4138	0.000
After	0.0127	0.582	-0.0086	0.725	-0.0264	0.346	0.0281	0.375
Treat	-0.0273	0.152	-0.0237	0.291	-0.0218	0.411	0.0469	0.158
After × Treat	0.0496	0.096	0.0377	0.238	0.0323	0.369	-0.0425	0.329
Dummies:								
Reemployment period				- Yes -				
Reemployed after UI				- Yes -				
Female				- Yes -				
Regional				- Yes -				
Month of unemployment				- Yes -				
Month of reemployment				- Yes -				
Degrees of freedom	2,363		2,363		2,362		2,363	
R ²	0.170		0.167		0.193		0.197	

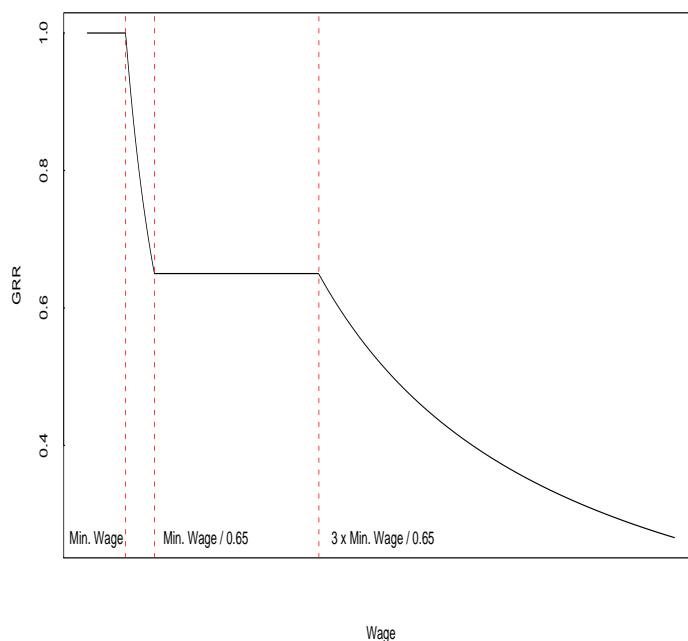


Figure 1: Financial generosity of the Portuguese UI system: Gross Replacement Rates (GRR) = Monthly UB / Average pre-unemployment earnings

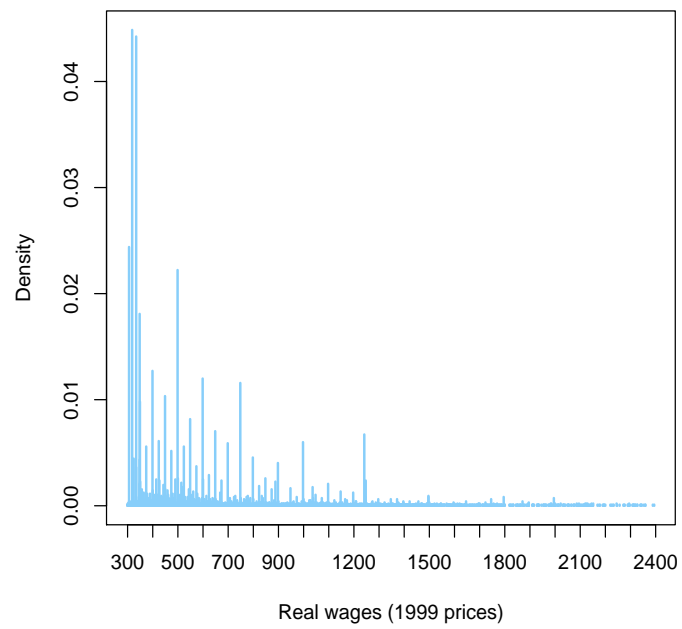


Figure 2: Histogram: Real reemployment wages (1999 prices)

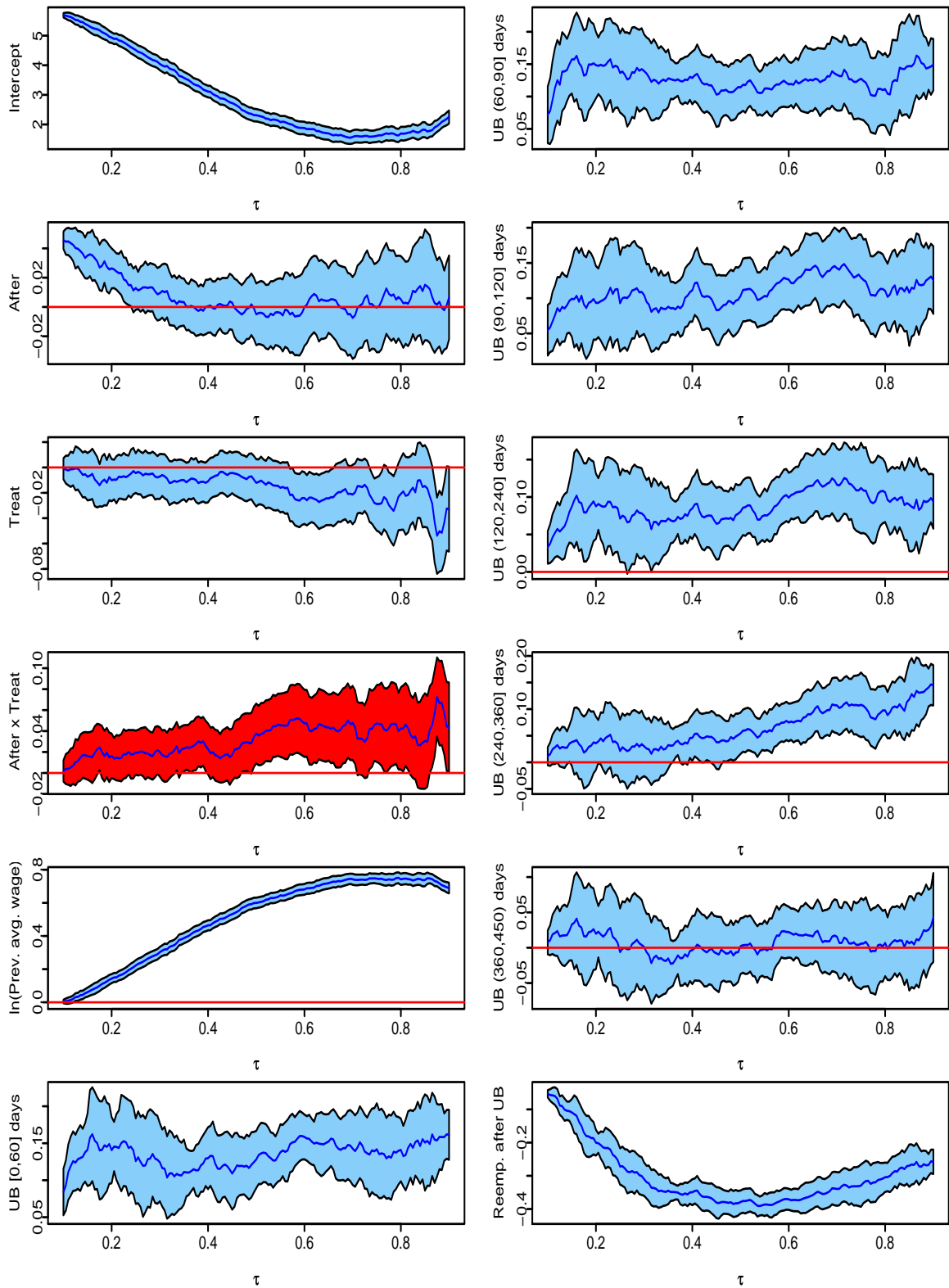


Figure 3: Quantile regression for (log) reemployment wages (1999 prices)