Unemployment insurance and heterogeneous treatment effects on reemployment wages*

Mário Centeno
mcenteno@bportugal.pt
Banco de Portugal & ISEG - U. Técnica & IZA

Álvaro A. Novo
anovo@bportugal.pt
Banco de Portugal & ISEGI - U. Nova & IZA

November 18, 2009

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. We find that the extension had a positive effect on reemployment wages of matches formed around the pre-reform maximum benefit entitlement. There are no significant gains associated with the initial period of subsidized unemployment. The quantile treatment estimates show that the impact of UI increases with the quantile of reemployment wages; in general, it is not significant for low wages, but for higher reemployment wages the gains are substantial. Unemployed with pre-unemployment wages in the bottom quartile do not gain from long spells. These results highlight the role of UI in shaping the search behavior of the unemployed. Overall, we show that wage gains from longer UI entitlement periods are concentrated at quite long durations and exclusively associated with the periods of steep decline in the reservation wage.

Keywords: Unemployment insurance; Reemployment wages; Liquidity effect; Quasi-natural experiment.

JEL Codes: J38, J65, J64, J68.

*We thank *Instituto de Informática da Segurança Social (II)* 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 *II*.

1 Introduction

The disincentive effect of unemployment insurance (UI) has been analyzed extensively in the unemployment literature. A large body of empirical evidence has been gathered in favor of the hypothesis that more generous UI systems lead to longer subsidized spells. However, UI may have a positive effect on post-unemployment outcomes, for example, by allowing the formation of matches with higher wages or that last longer. In this paper, we associate good matches with higher wages and study the impact on reemployment wages of an increase in UI generosity in Portugal. The exercise takes advantage of a quasi-natural experimental setting generated by a reform of the UI system that increased the entitlement period for some age-groups, while leaving it unchanged for other age-groups.

The impact of 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 Marimon and Zilibotti (1999) and Acemoglu and Shimer (2000) 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 a more generous UI, better job matches emerge.

There is already empirical evidence supporting this effect, with match quality measured in terms of post-unemployment wages and job stability. Centeno (2004), Centeno and Novo (2006*a*) and McCall and Chi (2008) find a 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. Recent studies analyze this issue for the more generous UI systems in several European countries; Fitzenberger and Wilke (2007) and Caliendo, Uhlendorff and Tatsiramo (2009) for Germany, van Ours and Vodopivec (2008), for Slovenia, and Lalive (2007) for Austria report small or no effects in both variables.

The exogenous increase in UI generosity allows us to identify the causal effect of the UI entitlement period on the potential gains in reemployment wages. The exogenous variation in generosity is the result of the July 1999 reform of the Portuguese UI system that increased the entitlement period for those aged 30 to 34 years and, at the same time, left it 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 control for unobserved heterogeneity and common macroeconomic confounding factors. To evaluate the policy we use two methods, the differences-in-differences approach and the quantile treatment effect framework (Koenker 2005). The latter method allows us to address issues related with heterogeneity in the UI impact along the reemployment wage distribution.

The difficulty in identifying the effect of UI arises because the take up of UI, the benefit level, duration and post-unemployment outcomes are potentially endogenous. Individuals who expect a long unemployment spell and a large wage drop may be more likely to claim benefits. The quasi-experimental nature of treatment is explored to overcome the endogeneity between subsidized duration and post-unemployment wages.

We use Portuguese UI administrative data from Social Security covering *all* subsidized unemployment spells claimed between 1998 and 2000; all individuals are followed after exiting UI up to July, 2004. This possibility overcomes one of the main disadvantages of UI administrative data, which is the fact that unemployment duration is usually truncated at the point of maximum benefit entitlement (Moffitt 1985). The dataset has 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.

Centeno and Novo (2009) shows that this UI reform strongly increased the duration of subsidized unemployment. The impact was larger just before the pre-reform exhaustion date, as predicted by non-stationary job search theory (Mortensen 1986, van den Berg 1990).

This paper shows a small, positive, average impact of the UI extension on reemployment wages (2.8 percent). However, this impact is concentrated in matches formed around the pre-reform exhaustion date, where the impact can be sizeable (larger than 20 percent); a result that can be interpreted as an indication of a strong reduction in reservation wages at the moment of benefit exhaustion. In matches formed during the first 420 days of unemployment, one month prior to the pre-reform exhaustion date, there are no wage gains, despite the fall in the exit rate from unemployment reported in Centeno and Novo (2009). Additionally, the quantile treatment effects are increasing with the quantile of the reemployment wages distribution, and are significantly larger in the extension period (451 to 540 days). This points to a more dispersed wage distribution as a result of the UI extension. Finally, at longer unemployment durations, low wage workers do not appear to be the greatest beneficiaries of the UI extension.

Altogether, these results point to the strategic usage of UI to adjust the reservation wage.

Those with a wider ability to postpone employment and benefit from the reform take the most of the extended UI. These results are useful to guide the redesign of UI systems with very long entitlement periods, as is the case of those in place in most European countries. UI is a fundamental component of flexicurity systems and our results show that long UI entitlements are generally not productive for the individuals typically targeted by social insurance policies.

2 Literature: Theory and evidence

There are two alternative views about the way the job matching process evolves. A Diamond-type of model (Diamond 1982) can be used to describe the mechanisms that allow workers to achieve a better job match, usually characterized as a job with a higher wage. In this way, the matching process depends on factors such as outside opportunities and expectations about future wages (Jovanovic 1979). 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 (Akerlof, Rose and Yellen 1988).

The impact of the UI system on productivity and job mismatch has been examined recently 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 both workers to seek higher productivity jobs and firms to create such jobs. In their setting, the UI is more than a search subsidy, and affects the type of jobs that workers look for and accept.

In nonstationary job search models, such as Mortensen (1986) and van den Berg (1990), the unemployed reservation wages change over time. This is the result of the nonstationary job search environment, which arises because the unemployed face a wage offer distribution and an arrival rate of job offers that change over the unemployment spell and, additionally UI benefits are limited in time. These models predict that an increase in UI generosity shifts the duration of subsidized unemployment heterogeneously, as individuals face different search environments.

The ability of constrained individuals to finance the out-of-pocket cost of search improves with UI recipiency, allowing them to smooth consumption between labor market states. In this way, UI generates a liquidity effect, similar to the one described in Chetty (2008). If this liquidity effect is important, the disincentive of UI created through the substitution effect can be mitigated, and becomes less distortionary than previously thought. The non-distortionary nature of the liquidity effect, reducing the pressure of low income workers to accept bad quality matches and allowing them to wait for a better match, can be associated with a greater impact on reemployment wages.

In our setting, the impact on wages works through the reservation wage and longer durations (more time to search and more selective unemployed generate better outcomes). In a nonstationary search environment the impact of UI on the reservation wage is decreasing over the UI spell, which implies that the impact of the entitlement period extension on reemployment wage should be maximum around the pre-reform exhaustion date. The further away the unemployed is from the exhaustion date (both before and after that date) the less sensitive reemployment wages should be from the sudden fall of the reservation wage associated with that moment.

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 (for a survey see Addison and Blackburn 2000). Belzil (2001) looks at job duration by exploring a reduction in the initial entitlement period rule in Canada to study the impact of UI duration on subsequent job duration for young individuals, and reports a weak but positive impact. Centeno (2004) and Centeno and Novo (2006*a*) look at the US system, using UI variation across states with data from the NLSY, and find evidence that more generous UI increases the tenure of reemployment and that this impact is stronger at longer durations. They also show a positive impact on the reemployment wage distribution. More recently, McCall and Chi (2008) using also data from the NLSY, find a positive impact of UI on reemployment wages.

Recently, a number of papers considered the impact of UI on post-unemployment outcomes using data for European countries. Lalive (2008) and Lalive (2007) use Austrian data from an extension of UI benefits and report a significant impact on unemployment duration but no impact on wages. Similar results are obtained in the studies by Fitzenberger and Wilke (2007) and Caliendo et al. (2009) for Germany (in the context of a nonstationary model) and van Ours and Vodopivec (2006) for Slovenia. Card, Chetty and Weber (2007) also use data from Austria and find some impact of severance payments on reemployment job tenure, but no impact on wages.

3 The unemployment system reform and identification

3.1 The extension of some entitlement periods

One peculiar feature of the Portuguese UI system, the time of the reform, was the definition of the entitlement period. Its length was fully determined by the individual's age at the beginning of the unemployment spell. There were eight entitlement levels corresponding to eight age groups. The length of social contributions determine only the eligibility, but not the duration of benefits.

In July, 1999, the reform increased the entitlement period for six of the eight age groups. After the reform, some contiguous age groups share the same entitlement (see Table 1). We focus our evaluation in individuals aged 30-34, whose entitlement period increased from 15 to 18 months and constitutes a natural treatment group. For the contiguous age group, 35-39, the entitlement was left unchanged at 18 months, and we will use it as the control group.

[TABLE 1; see page 22]

One of the main advantages of this pair of age groups is the fact that after the reform they share exactly the same entitlement period, 18 months. Additionally, their age proximity makes it likely that treatment and control groups share similar labor market characteristics, for instance, in terms of labor income, schooling, marital status, and child-bearing decisions.

We could also use the $[15, 24]$ and $[25, 29]$ age groups as treatment and control, respectively. We decided not to do that because the treatment group would be composed of rather young individuals, 15 to 24 years old, with low labor market attachment (for whom, for example, educational and marital choices are still central). Perhaps more importantly, we should note that the income distribution of those aged 15 to 24 has a small overlapping with the older control group, 25-29 (and the remaining population).

In terms of the financial generosity, the value of UI depends on the 12-months average wages earned prior to unemployment. Individuals with wages worth 1.5 to 4.5 minimum wages are entitled to UI worth 65 percent of their previous average wage. For individuals earning less, the UI equals the minimum wage, resulting in higher gross replacement rate that reach 100 percent for minimum wage earners; and UI cannot exceed 3 minimum wages for those earning more than 4.5 minimum wages.

Identification

The identification of the UI effect on reemployment wages is based on the availability of suitable treatment and control groups and the observation of individuals in the periods before and after the implementation of the UI reform. This constitutes a fortunate setting for identification of the UI impact and to overcome selection bias and endogeneity issues usually present when evaluating the impact of UI on search outcomes.

The difficulty in identifying the effect of UI arises because the take up of UI, the benefit level, duration and post-unemployment outcomes are potentially endogenous. Individuals who expect a long unemployment spell and a large wage drop may be more likely to claim benefits.

Our identification rests on the quasi-experimental nature of the implemented reform. The exogenous shift in the UI entitlement period, the availability of a suitable control group of individuals not directly affected by the reform and the analysis of data from before and after the sharp change in UI generosity allows us to isolate the impact of UI on subsidized duration and post-displacement wages.

We checked for the selection issues associated with more generous benefits. One matter of concern would be the possibility that more generous UI are associated with a larger pool of UI claimants. This was clearer not the case in Portugal. The share of subsidized individuals in unemployment remained fairly stable throughout the period in analysis, increasing almost 2.5 percentage points in both the treatment and control groups (from 34.1 before the reform to 36.8 percent and from 40.7 to 43.1, respectively).

Economic conditions

At the moment of the reform, the Portuguese labor market and the economy were buoyant (Table 2). In the period just prior to the reform, real GDP growth exceeded 4 percent and employment was growing consistently above 2 percent. The unemployment rate was at or below 5 percent, showing signs of a tight labor market.

[TABLE 2; see page 22]

It is worth noting that these good economic conditions are favorable to our empirical strategy. Indeed, they suggest that the policy change was not driven endogenously by the evolution of the labor market. There are two exogenous factors that help understand the motivation of the reform. First, in the event of joining the euro area monetary union, the Portuguese public finances benefited significantly from falling interest rates; interest payments decreased by 5 percentage points of GDP (from 8.1 per cent in 1992 to 3.0 per cent in 1999). This budgetary slack was used to expand social and labor market programs. Second, the political cycle may have played also a role since there were scheduled elections for the second half of 1999.

Furthermore, the treatment and control groups, composed of prime-age workers, usually suffer less with labor market swings than younger workers and do not face the type of retirement decisions common to older workers. This makes our comparison of pre- and post-reform outcomes more convincing, as it is not driven by a specific trend in the labor market or to questions related with population ageing.

4 Data

4.1 Description

Our study uses administrative data collected by *Instituto de Informática* of the Portuguese Social Security bureau. The dataset recorded *all subsidized* unemployment spells initiated between January 1, 1998 and December 31, 2000 that ended in a salaried employment position in the private sector. Since the data extends to June 2004, there is a window of at least 24 months after UI exhaustion to observe reemployment, avoiding biases due to truncation. Overall, there are 12,558 reemployment observations for the age group $[30, 39]$. The dataset contains very detailed and reliable information on the type, amount and duration of benefits, and the previous wage. The socio-demographic variables available are limited to gender, age, and place of residence. Fortunately, the availability of the previous wage allows us to partially overcome the problem posed by the lack of more detailed individual characteristics. Table 3 contains descriptive summary statistics of the key variables before and after the reform.

[TABLE 3; see page 23]

The treatment group comprises 6,606 observations, of which 2,702 are from the period before July, 1999. The control group has 2,977 observations in the before period and 2,975 in the after period. The differences in the 12-month average values of real pre-unemployment wages between treatment and control groups, as expected, are favorable to older individuals. The percentage of women is similar across treatment and control groups, although it increased in the after period. The gross replacement ratio hovers around 69 percent, a value close to the mode of the system, 65 percent.

4.2 A bird's-eye view of unemployment and wages distributions

Before discussing the impact of the reform on post-unemployment wages, it is useful to take a quick look at its impact on unemployment duration. Centeno and Novo (2009) present a full account of the impact of the reform on subsidized unemployment duration. A simple difference-in-differences (D-in-D) based on the Kaplan-Meyer survival rates estimates yields the impact on subsidized unemployment duration for the treated group (Figure 1). The before-after difference between the two curves drawn for the treatment group suggests that the reform significantly increased the survival rates in subsidized unemployment. The same exercise for the control group results in virtually imperceptible differences in the survival rates, which reinforces our case for an exogenously driven reform. Using this difference to adjust for aggregate conditions, we compute a simple D-in-D estimator from these Kaplan-Meyer survival rates. The D-in-D estimates show a positive impact of the reform on subsidized unemployment duration of the treated group. Notice that, as predicted by theory for the case of an extension in the entitlement period, the impact is larger at longer durations (closer to the pre-reform entitlement period). These estimates also illustrate the quality of our quasi-natural experiment.

[FIGURE 1; see 25]

The nonstationary job search model stresses the importance of the limited UI periods in the definition of the optimal reservation wage strategy. A preliminary empirical assessment of this claim could be gauged by looking at the distribution of wages before and after the UI reform around the pre-reform UI exhaustion date. We do this in Figure 2, which plots kernel estimates of the distribution of both pre-unemployment and reemployment wages for the treatment and control groups.

[FIGURE 2; see page 26]

The panels in the top row show that reemployment wages are generally lower than pre-unemployment wages. The distribution of reemployment wages lies to the left of the one prevailing before the unemployment experience. In general, an intervening unemployment spell between jobs seems to limit wage progression. This is particularly clear in the right panel that limits pre-unemployment wages to 1.5 and 4.5 minimum wages. This fact is important, when interpreting our results, because we will be only able to identify the differential impact of extra UI time, not on the actual change in wages after unemployment.

The sharp reduction of the reservation wage generates a spike in the exit rate of unemployment close to the exhaustion date (Katz and Meyer 1990, Boone and van Ours 2009), and this drop should have an impact on the distribution of accepted wages. In order to take a first look at the reemployment outcomes the middle and bottom panels of Figure 2 display the distribution of wages of matches formed during the first year of the unemployment spell (well before the benefit extension) and of those formed just after the treatment group's pre-reform entitlement period (450 days).

The results make it quite clear that the policy had a large impact on reemployment wages around that date, and also help in the evaluation of the quality of our quasi-natural experiment, as they show that under similar conditions individuals have akin outcomes.

The plots in the middle row of Figure 2 refer to the pre-reform period. The distributions of reemployment wages of matches formed during the first year of unemployment almost overlap. However, it is quite interesting to note that past the 450 days threshold, the distribution of reemployment wages of treated individuals (those already without UI) is quite different from the one obtained for the control group. Indeed, individuals in the treatment group have much lower wages in the jobs accepted after UI expiration.

Finally, the last row of Figure 2 confirms the quality of the reform; treatment and control have very similar reemployment wage distributions in the period after the reform, this is, in the period in which they share the same UI conditions (540 days of benefits).

5 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 nonstationarity of the job search environment implies decreasing reservation wages over the length of the subsidized unemployment spell. Additionally, the search environment is also a function of the wage offer distribution faced by individuals. Individuals face differentiated payoffs to extend their search period depending on their productive characteristics and type of job they are looking for. The expected impact of the reform is not homogeneous and could vary at different locations of the wage distribution. Some workers can expect larger gains from longer search spells, those with more labor market opportunities, while others may not be able to search longer and/or may be searching in thinner labor markets. Differentiated impacts in the distribution can be estimated with quantile treatment effects.

5.1 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.

5.2 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.

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:

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

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.

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,

$$G^{-1}_{y_0(t')|D=1}(\tau) - G^{-1}_{y_0(t)|D=1}(\tau) = F^{-1}_{y_0(t')|D=0}(\tau) - F^{-1}_{y_0(t)|D=0}(\tau), \quad \forall\tau. \tag{2}$$

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

$$\begin{aligned}
\delta(\tau) &\equiv G^{-1}_{y_1(t')|D=1}(\tau) - G^{-1}_{y_0(t')|D=1}(\tau) \\
&= G^{-1}_{y_1(t')|D=1}(\tau) - G^{-1}_{y_0(t')|D=1}(\tau) + \{G^{-1}_{y_0(t')|D=1}(\tau) - G^{-1}_{y_0(t)|D=1}(\tau)\} - \\
&\qquad \{F^{-1}_{y_0(t')|D=0}(\tau) - F^{-1}_{y_0(t)|D=0}(\tau)\} \\
&= \{G^{-1}_{y_1(t')|D=1}(\tau) - G^{-1}_{y_0(t)|D=1}(\tau)\} - \{F^{-1}_{y_0(t')|D=0}(\tau) - F^{-1}_{y_0(t)|D=0}(\tau)\}. \qquad (3)
\end{aligned}$$

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.

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 liquidity effect generated by more generous UI impacted on the reemployment wages of different levels of individuals with pre-unemployment average income.

6.1 Reemployment wages: Average and quantile treatment effects

In this section, we present the treatment effects estimates of the UI extension. We start by presenting a differences-in-differences (D-in-D) model for the impact of the additional period of subsidized unemployment. The estimated model is:

$$\log(W) = \beta_0 + \beta_1 After + \beta_2 Treat + \beta_3 After \times Treat + x'\lambda, \qquad (4)$$

where $After$ is an indicator variable for the post-July 1999 period, $Treat$ indicates the age group affected by the new legislation. The vector x includes the previous average wage, indicator variables for unemployment duration (piecewise function), a gender variable and dummy variables for regional labor markets and month of unemployment and of reemployment. The indicator variables for unemployment duration consider the following periods (in days): 1-90, 91-180, 181-270, 271-360, 361-420, 421-450, 451-480, 481-540, and +540.

Table 4 presents the results from the estimation of equation (4). The average treatment effect on the treated is 2.8 percent. In other words, without the UI extension wages of treated individuals would be 2.8 percent lower.

There are other interesting results from this wage regression. Reemployment wages earned by females are about 3.4 percent lower than those of males. Also, conditional on all other variables included in the regression, the previous wage has a positive effect on the new wage. This effect is due to unobserved characteristics of the workers that are captured by the previous wage. The elasticity is around 0.4, meaning that if the previous wage was 1 percent higher the current wage would be 0.4 percent higher. The relatively large estimate is due to the omission of important productive characteristics from the regression, for example education and experience. Finally, there are clear signs of duration dependence, in particular after the first year of unemployment. The dummies for duration show a declining profile of reemployment wages. This is directly correlated with the nonstationary nature of the job search environment and implies that post-unemployment outcomes may change along the duration of the unemployment spell (Lalive, van Ours and Zweimueller 2006).

In line with this result, Centeno and Novo (2009) shows that the policy change induced differentiated shifts in unemployment exit rates along the distribution of subsidized unemployment spells duration; larger shifts are observed closer to the pre-reform UI exhaustion date (450 days). These differentiated responses could translate into a different impact of the policy in reemployment wages. We interacted the duration dummies with the treatment indicators ($After$, $Treat$ and $After \times Treat$). The interaction coefficients capture the differentiated treatment effects over the duration of the unemployment spell.

Table 5 reports the D-in-D estimates of the average treatment effect on the treated in the two columns under the label 'D-in-D'; it presents only the estimates for the interactions of the $After \times Treat$ variable and the dummies for subsidized unemployment duration; Table A.1 reports the remaining coefficients associated with the duration dummies. The first column presents the estimates for the whole sample and the second column restricts the sample to workers with gross replacement rates in the 63 to 67 percent range. The shorter GRR range makes the disincentive (substitution) effect of UI more equal for the unemployed, better isolating the impact of extending the entitlement period.

Table 5 [see page 24]

The results show no impact of the UI extension on reemployment wages for matches formed within the first 420 days of unemployment. The policy effect kicks in just prior to the pre-reform exhaustion date, suggesting that wages of treated individuals are 20 percent above those that would have emerged in a situation without the UI extension. This is the point estimate of what was already gauged in Figure 2. The impact is even slightly higher after the 450 days threshold, which can be interpreted as evidence that some workers adjust their reservation wages only after UI termination. The positive impact remains significant for matches formed until termination of the post-reform UI entitlement period (540 day) and drops to zero after that date. Since the results do not differ much among the two samples considered, we keep the GRR restricted sample in the following analyses as it is more homogeneous in face of substitution effects (moral hazard). As an additional robustness check, we extend the period of UI claims until the end of 2002. The results are presented in Table A.2 in the Appendix, and are consistent with the image depicted hitherto.

Next, we consider the possibility of a heterogeneous impact over the distribution of reemployment wages, i.e., whether the gains of UI are distributed among all sort of matches or are concentrated in specific ranges of reemployment wages.

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

$$Q_{\log(W)}(\tau) = \beta_0(\tau) + \beta_1(\tau)After + \beta_2(\tau)Treat + \beta_3(\tau)After \times Treat + x'\lambda(\tau), \qquad (5)$$

and all the variables are defined as above. The results for the 20th, 50th, and 80th quantiles are presented in the last three columns of Table 5, while Figure 3 presents the full range of quantile estimates. The results show that for low reemployment wage matches the gains are concentrated in the period before the pre-reform exhaustion date, whereas for new higher wages (at the median or above) the gains are restricted to matches formed after 450 days of subsidized unemployment. The gains from the extended UI entitlement should reflect the timing of reservation wage adjustments and consequently their impact on the formation of new wages. As before, the gains of higher wages after the UI exhaustion date should reflect a delayed adjustment in reservation wages and job acceptance of these workers.

In Figure 3, each panel represents the point estimates of the coefficient associated with the interaction of the $After \times Treat$ variable and the duration indicators for each of the estimated quantiles. We chose to limit our attention to the quantiles $\tau \in [0.20, 0.80]$.[1] The dashed lines represent 90 percent confidence intervals.

Figure 3 [see page 27]

The quantile treatment effects tell us a story of heterogeneity. First, the point estimates are non significant for all quantiles for durations up to 450 days, with the exception of low reemployment wages (up to the 30th quantile) formed in the last month of benefits (421-450 days). Secondly, for matches formed after 450 days of subsidized unemployment the impact is increasing with the wage quantiles. 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 (2006*b*) and McCall and Chi (2008), shows that the negative 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 UI and pre-unemployment wages

Traditionally, the substitution effect – the increase in the relative price of leisure – was emphasized as the negative outcome of UI. More recently, Chetty (2008) pointed out that UI may have a non-distortionary liquidity effect by easing the worker's liquidity constraints. If indeed there is a liquidity effect – Centeno and Novo (2009) show that constrained individuals reacted differently to this UI reform – then the impact of UI on post-unemployment outcomes may also differ for constrained and unconstrained individuals.

To study the impact of the liquidity effect on reemployment wages, we split the sample by level of pre-unemployment wages. Ziliak (2003) shows that wages are the best predictor for the net worth to permanent income ratio. Thus, like Chetty (2008), we associate the individual's pre-unemployment wage with the degree of financial constraints. We repeat the specifications used earlier, but for simplicity collapse the four dummies for reemployment before one year into one single dummy and also the dummies for one month before and after the pre-reform

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

entitlement into a [421, 480] days dummy. The D-in-D results are presented in Table 6 for each of the four samples splitted according to the pre-unemploymentt wage quartiles.

[Table 6; see page 25]

Like the duration outcomes, reemployment wages are also affected differently across the four groups. Note that the individuals in the interquartile wage range, those who reacted the most in terms of durations, do not gain from the longer UI periods. Their wage elasticities to the benefit along duration are all zero. The behavior of the two tail quartiles is more reactive. Lower-income individuals have wage gains before the previous exhaustion date (450 days), and higher-income individuals have a stronger reaction close to and after the previous exhaustion date. Note also that whenever the impacts are significant, they are larger for unconstrained individuals. In part, the fact that the job options of low-income individuals are scarcer may explain this, and also that less constrained have a wider margin of maneuver to take greater advantage of the extra days of UI.

Finally, we study the liquidity effect along the distribution of reemployment wages. Figure 4 presents quantile treatment effects for the four samples. Overall, these results confirm the D-in-D results, but they show that the average impact comes about essentially through stronger impacts on the upper-tail (higher quantiles) of the reemployment wages distribution. Again, consistent with earlier evidence, the impact on wages after the previous exhaustion date (see plot for [481, 540] days) is clearly stronger for less constrained individuals. A possible interpretation of this result lays on the fact that less constrained individuals prior to the reform where adjusting the most their reservation wage only after running out of UI; they were able to hold on to a higher reservation wage until later in the spell than individuals with higher constraints.

From a policy perspective, the set of results presented are favorable to the views of Marimon and Zilibotti (1999) and Acemoglu and Shimer (2000) that predict more productive job matches. Our estimates suggest that there are non-negligible gains after the previous exhaustion date, without generating losses in the period before. The quantile treatment effects shed some additional insights by showing that the impacts tended to affect more positively the right tail of the reemployment wages distribution. Finally, the fact that the more constrained individuals, those more in need of UI, did not benefit as much as the unconstrained at longer spells suggests that there is room to redesign the UI policy.

Figure 4 [see page 28]

7 Conclusions

The gains from unemployment insurance programs have attracted increased attention from empirical economists. These gains originate in the increased ability of recipients to smooth consumption over labor market states and may also translate into the improvement of post-unemployment outcomes. The purpose of this paper is to analyze the relationship between the quality of job matches (measured by the wage) and UI generosity. We take advantage of a quasi-natural experiment generated by the 1999 reform of the Portuguese UI system that increased entitlement periods for particular age groups. The nature of the reform allows us to identify the causal effect of UI on post-unemployment wages.

We find some evidence that UI generosity increases wages after unemployment. Longer unemployment spells are not particularly helpful for low-income individuals. On the contrary, those with pre-unemployment wages in the top quartile gained the most with the extension in jobs initiated after more than 450 days in unemployment, the pre-reform maximum benefit duration for the treatment group. Additionally, the quantile treatment estimates show that the impact of UI increases with the quintile of reemployment wages. In general it is not significant for low wages, but for high reemployment wages the gains are substantial.

These results, together with those of a companion paper (Centeno and Novo 2009), suggest that the reservation wage is the main channel through which UI affects reemployment wages. This is compatible with a strategic behavior of UI utilization by the unemployed whereby they delay the moment of job acceptance. The absence of gains early on the subsidized spell could be traded-off with gains later on. Indeed, conditional on being unemployed, our results show that workers are better off whenever they are insured. However, given the delayed gains and the non-stationary nature of the search environment, UI extensions may result in an unemployment trap. The decreasing quality and quantity of jobs available after a long period of unemployment may prove particularly harmful for low-wage workers. Thus, UI systems with long entitlement periods may not be optimal to address the needs of this specific group of workers.

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.

Boone, J. and van Ours, J. (2009), Why is there a spike in the job finding rate at benefit exhaustion?, Working paper 4523, IZA.

Caliendo, M., Uhlendorff, A. and Tatsiramo, K. (2009), Benefit duration, unemployment duration and employment stability: A regression-discontinuity approach, mimeo, IZA.

Card, D., Chetty, R. and Weber, A. (2007), 'The spike at benefit exhaustion: Leaving the unemployment system or starting a new job?', *American Economic Review* **97**(2), 113–118.

Centeno, M. (2004), 'The match quality gains from unemployment insurance', *Journal of Human Resources* **39**(3), 839–863.

Centeno, M. and Novo, Á. A. (2006*a*), 'The impact of unemployment insurance generosity on match quality distribution', *Economic Letters* **93**, 235–241.

Centeno, M. and Novo, Á. A. (2006*b*), '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. (2009), Extended unemployment benefits and liquidity effects: Quasi-experimental evidence, mimeo, Banco de Portugal.

Chetty, R. (2008), 'Moral hazard versus liquidity and optimal unemployment insurance', *Journal of Political Economy* **116**(2), 173–234.

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.

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', *The Journal of Political Economy* **87**(5), 972.

Katz, L. F. and Meyer, B. D. (1990), 'Unemployment insurance, recall expectations, and unemployment outcomes', *Quarterly Journal of Economics* **105**, 973–1002.

Koenker, R. (2005), *Quantile regression*, Cambridge University Press, Cambridge.

Koenker, R. and Bassett, G. (1978), 'Regression quantiles', *Econometrica* **46**, 33–50.

Lalive, R. (2007), 'Unemployment benefits, unemployment duration, and post-unemployment jobs: A regression discontinuity approach', *American Economic Review* **97**(2), 108–112.

Lalive, R. (2008), 'How do extended benefits affect unemployment duration? A regression discontinuity approach', *Journal of Econometrics* **142**, 785–806.

Lalive, R., van Ours, J. C. and Zweimueller, J. (2006), 'How changes in financial incentives affect the duration of unemployment', *Review of Economic Studies* **73**, 1009–1038.

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.

McCall, B. and Chi, W. (2008), 'Unemployment insurance, unemployment durations and re-employment wages', *Economics Letters* **99**, 112–115.

Moffitt, R. (1985), 'Unemployment insurance and the distribution of unemployment spells', *Journal of Econometrics* **28**(1), 85–101.

Mortensen, D. (1986), Job search and labor market analysis, *in* O. Ashenfelter and R. Layard, eds, 'Handbook of Labor Economics', Vol. 2, North-Holland, Amsterdam, pp. 849–919.

van den Berg, G. J. (1990), 'Nonstationarity in job search theory', *The Review of Economic Studies* **57**(2), 255–277.

van Ours, J. C. and Vodopivec, M. (2006), '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. (2008), 'Does reducing unemployment insurance generosity reduce job match quality?', *Journal of Public Economics* **92**, 684–695.

Ziliak, J. P. (2003), 'Income transfers and assets of the poor', *Review of Economics and Statistics* **85**(1), 63–76.

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

Before		After	
Age (years)†	Entitlement period	Age (years)†	Entitlement period
[15, 24]	10	[15, 29]	12
[25, 29]	12		
[30, 34]	15	[30, 39]	18
[35, 39]	18		
[40, 44]	21	[40, 44]	24
[45, 49]	24	[45, 64]	30(+8)*
[50, 54]	27		
[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 previous 20 calendar years.

Table 2: The Portuguese economy before and after July 1999

	Real GDP Growth(1)	Employment Growth(2)	Unemployment Rate(2)	Long-term Unemployment (%)(2)	Subsidized Unemployed (thousands)(3)
1997	4.2	1.9	5.8	43.6	172.9
1998	4.7	2.3	5.0	45.4	165.1
1999	3.9	1.9	4.4	41.2	163.1
2000	3.9	2.3	3.9	43.8	166.6
2001	2.0	1.5	4.0	40.0	176.1
2002	0.8	0.5	5.0	37.3	195.2
2003	-1.2	-0.4	6.3	37.7	248.2
2004	1.1	0.1	6.7	46.2	288.4

Sources: (1) National accounts, INE; (2) Employment Survey, INE; (3) Social Security Bureau, MTSS.

Table 3: Summary statistics: Average values by treatment status and period

	Before		After	
	Treatment	Control	Treatment	Control
Unemployment duration (in days)				
Total	265.3	404.4	338.7	343.4
Subsidized	208.8	313.3	249.6	261.4
After UI	56.5	91.1	89.1	82.0
Reemployment period (proportion)				
$[1, 90]$ days	0.27	0.19	0.26	0.24
$[91, 180]$ days	0.19	0.14	0.18	0.17
$[181, 270]$ days	0.14	0.09	0.10	0.12
$[271, 360]$ days	0.14	0.08	0.08	0.07
$[361, 420]$ days	0.07	0.05	0.04	0.03
$[421, 450]$ days	0.05	0.03	0.01	0.02
$[451, 480]$ days	0.01	0.02	0.02	0.01
$[481, 540]$ days	0.02	0.13	0.10	0.12
> 540 days	0.11	0.25	0.21	0.22
Age	31.9	36.9	31.8	36.8
Females	0.41	0.39	0.50	0.46
Pre-unemployment wages (1999 prices)	525.78	623.13	598.76	619.11
Gross replacement rate	71.2	68.8	69.7	69.3
Reemployment wages (1999 prices)	513.33	514.27	532.67	516.52
No. of observations	2 702	2 977	3 904	2 975

Table 4: Average treatment effects on reemployment wages

Log reemployment wages	Coefficient	Std. Error	t-value	$\Pr[> \|t\|]$
Intercept	3.993	0.050	79.818	0.000
After × Treat	0.028	0.013	2.175	0.030
Treat	-0.002	0.010	-0.214	0.830
After	-0.034	0.010	-3.573	0.000
Previous wage	0.373	0.007	52.318	0.000
Female	-0.034	0.007	-5.059	0.000
Unemployment duration				
$[91, 180]$ days	-0.022	0.010	-2.189	0.029
$[181, 270]$ days	-0.033	0.011	-2.858	0.004
$[271, 360]$ days	-0.063	0.013	-5.017	0.000
$[361, 420]$ days	-0.072	0.016	-4.534	0.000
$[421, 450]$ days	-0.180	0.021	-8.593	0.000
$[451, 480]$ days	-0.109	0.026	-4.222	0.000
$[481, 540]$ days	-0.290	0.012	-23.303	0.000
> 540 days	-0.274	0.010	-28.001	0.000
Other variables:				
Regional dummies		– Yes –		
Month of unemployment		– Yes –		
Month of reemployment		– Yes –		
No. of observations		12 558		

Table 5: Reemployment wages: Average and quantile treatment effects by unemployment duration

Log reemployment wages	D-in-D		QTE ($grr \in [63, 67]$)		
	All	$grr \in [63, 67]$	20th	50th	80th
Unemployment duration × After × Treat					
$[1, 90]$ days	-0.008	-0.035	-0.044	-0.024	-0.049
	(0.775)	(0.329)	(0.270)	(0.544)	(0.241)
$[91, 180]$ days	-0.001	-0.038	-0.012	0.009	-0.033
	(0.964)	(0.342)	(0.830)	(0.806)	(0.498)
$[181, 270]$ days	0.015	-0.010	-0.051	-0.041	0.030
	(0.684)	(0.835)	(0.337)	(0.291)	(0.651)
$[271, 360]$ days	0.067	0.056	0.069	0.020	0.094
	(0.120)	(0.299)	(0.364)	(0.709)	(0.284)
$[361, 420]$ days	0.022	0.030	0.016	-0.026	0.144
	(0.708)	(0.678)	(0.894)	(0.767)	(0.121)
$[421, 450]$ days	**0.181**	**0.241**	**0.200**	0.239	0.320
	(0.036)	**(0.019)**	**(0.000)**	(0.166)	(0.185)
$[451, 480]$ days	**0.276**	**0.266**	0.149	**0.288**	0.224
	(0.009)	**(0.041)**	(0.303)	**(0.008)**	(0.118)
$[481, 540]$ days	**0.146**	**0.232**	0.021	**0.167**	**0.435**
	(0.009)	**(0.001)**	(0.407)	**(0.004)**	**(0.000)**
> 540 days	0.013	0.024	-0.003	0.011	0.072
	(0.679)	(0.537)	(0.702)	(0.732)	(0.172)
Other control variable			– Yes –		
No. of observations	12 558	8 664	8 664	8 664	8 664

Notes: p-values in parentheses.
"All" indicates that the sample includes all unemployed whose previous wages where equal or greater than the minimum wage; "$grr \in [63, 67]$" indicates that the sample includes unemployed with gross replacement rates in the 63 to 67 percent range, i.e., whose previous wages ranged from 1.5 to 4.5 minimum wages. "D-in-D" and "QTE" denote, respectively, difference-in-differences and quantile treatment effects. The latter are computed for the 20th, 50th, and 80th quantiles. All regressions include a complete set of dummies for the duration of unemployment, and all possible interaction terms with the "Treat" and "After" variables. Additionally, there are dummy variables for gender, region, month of unemployment and month of reemployment. Pre-unemployment wages are included in the set of control variables.

Table 6: Liquidity effect: Average treatment effects on reemployment wages by level (below and above median) of pre-unemployment wages

Log reemployment wages	D-in-D ($grr \in [63, 67]$) Pre-unemployment wages			
	1st quartile	2nd quartile	3rd quartile	4th quartile
Unemployment duration × After × Treat				
$[1, 360]$ days	0.010	0.053	0.007	**-0.178**
	(0.761)	(0.156)	(0.873)	**(0.003)**
$[361, 420]$ days	**0.201**	-0.056	-0.103	0.118
	(0.095)	(0.66)	(0.501)	(0.488)
$[421, 480]$ days	**0.245**	0.080	0.212	**0.410**
	(0.064)	(0.556)	(0.198)	**(0.023)**
$[481, 540]$ days	**0.232**	0.057	0.174	**0.304**
	(0.026)	(0.709)	(0.191)	**(0.075)**
> 540 days	0.068	0.057	0.123	-0.078
	(0.305)	(0.384)	(0.133)	(0.403)
Other control variable		– Yes –		
No. of observations	2 102	2 101	2 100	2 101

Notes: p-values in parentheses.
"$grr \in [63, 67]$" indicates that the sample includes unemployed with gross replacement rates in the 63 to 67 percent range, i.e., those whose previous wages ranged from 1.5 to 4.5 minimum wages. "D-in-D" denotes difference-in-differences. All regressions include a complete set of dummies for the duration of unemployment and all possible interaction terms with the "Treat" and "After" variables. Additionally, there are dummy variables for gender, region, month of unemployment, and month of reemployment. Pre-unemployment wages are included in the set of control variables.

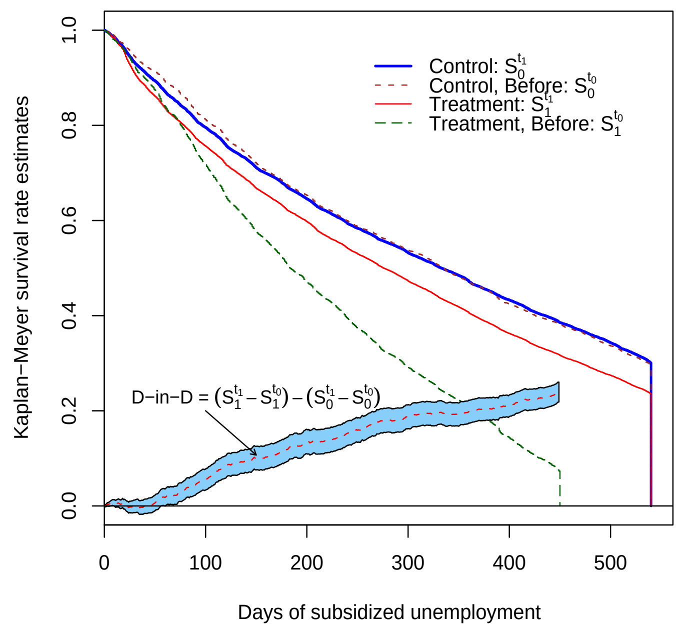

Figure 1: Kaplan-Meier estimates: Non-parametric subsidized unemployment survival rates. The difference-in-differences estimates (bottom curve) are obtained by taking the appropriate differences between the treatment and control curves, namely, $\{$*Treatment, After* $-$ *Treatment, Before*$\}$ $-$ $\{$*Control, After* $-$ *Control, Before*$\}$

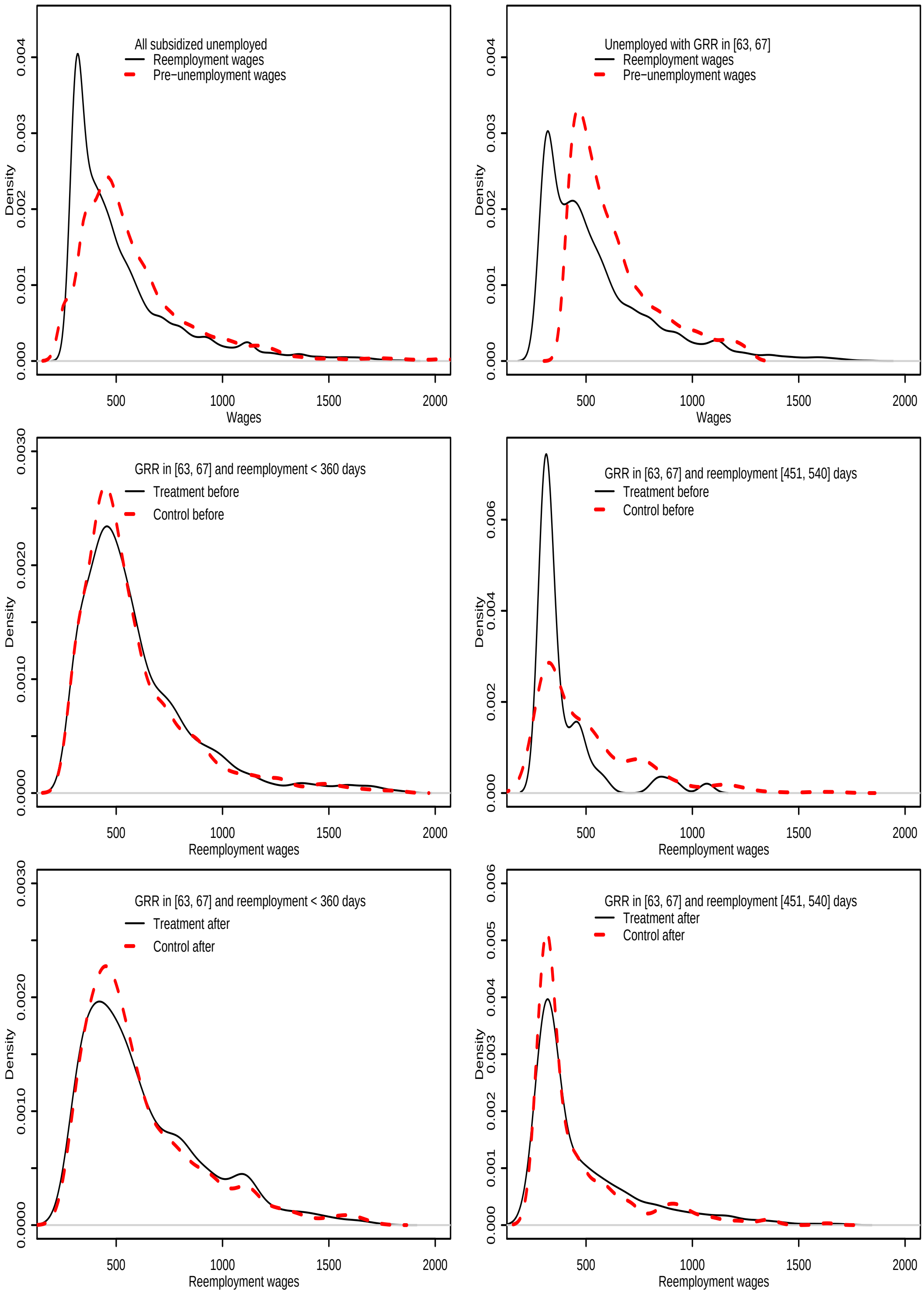

Figure 2: Kernel density estimates: The top row plots kernel density estimates of pre-unemployment and reemployment wages; in the right plot, pre-unemployment wages are restricted to 1.5 to 4.5 minimum wages (i.e. $grr \in [63, 67]$ percent). The four remaining panels compare reemployment wages of treatment and control groups according to the duration of the unemployment spell (up to one year or between 451 and 540 days), covering the periods before and after the reform (July 1999). Before the reform the treatment group individuals were entitled to 450 days of UI; after the reform, all individuals are entitled to 540 days.

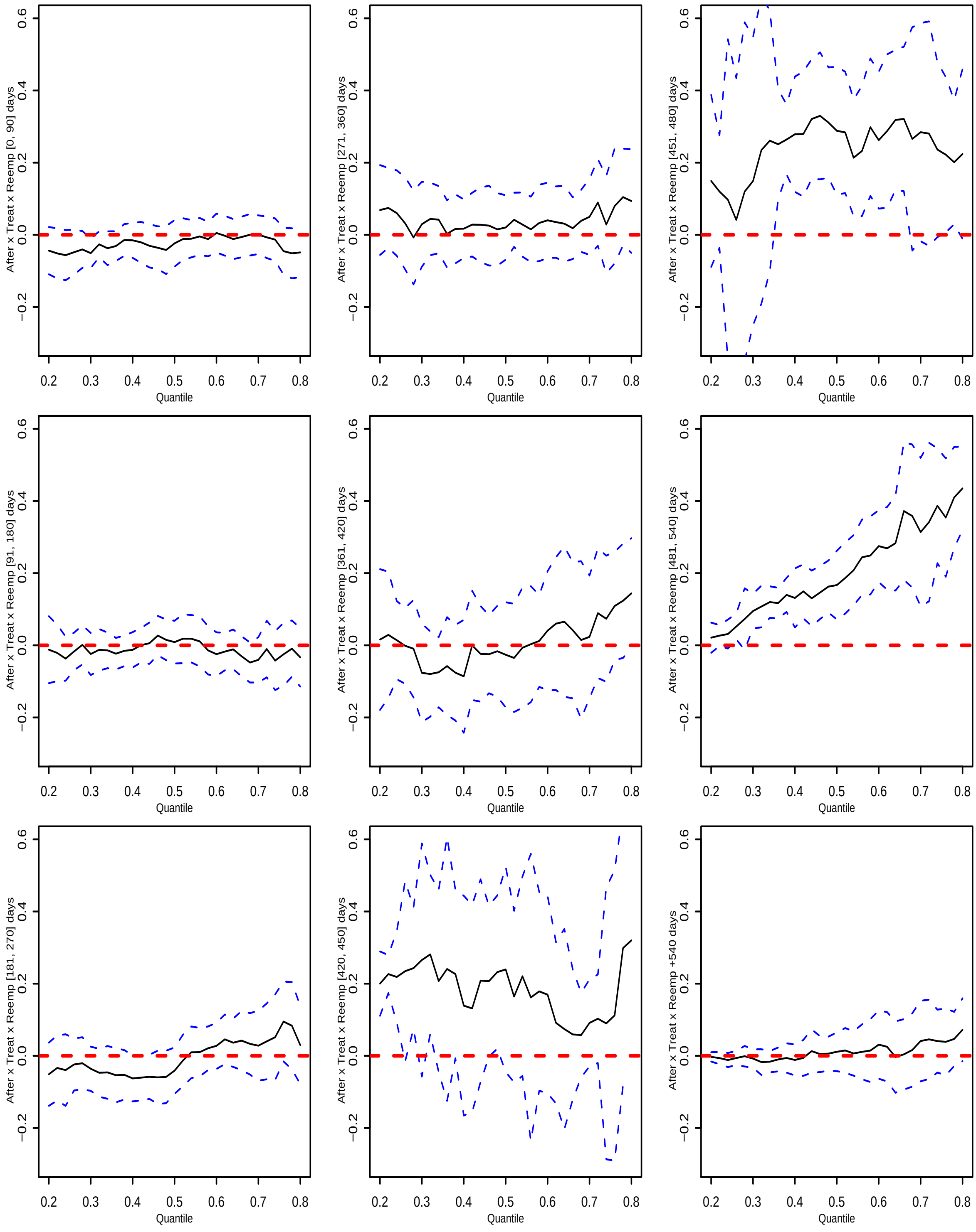

Figure 3: Quantile treatment effects. This figure plots the impact of receiving an entitlement extension of UI valid for the $[451st, 540th]$ days of unemployment on the τ-th quantile of the reemployment wage distribution conditional on having spent $[t_0, t_1]$ days unemployed. For instance, if reemployment occurred between 421 and 450 days, reemployment wages of the 25th quantile were 20 log points higher than would have been in the absence of the extension; for the 75th quantile, the impact is less than 10 log points and statistically insignificant. The dashed lines represent 90 percent confidence intervals.

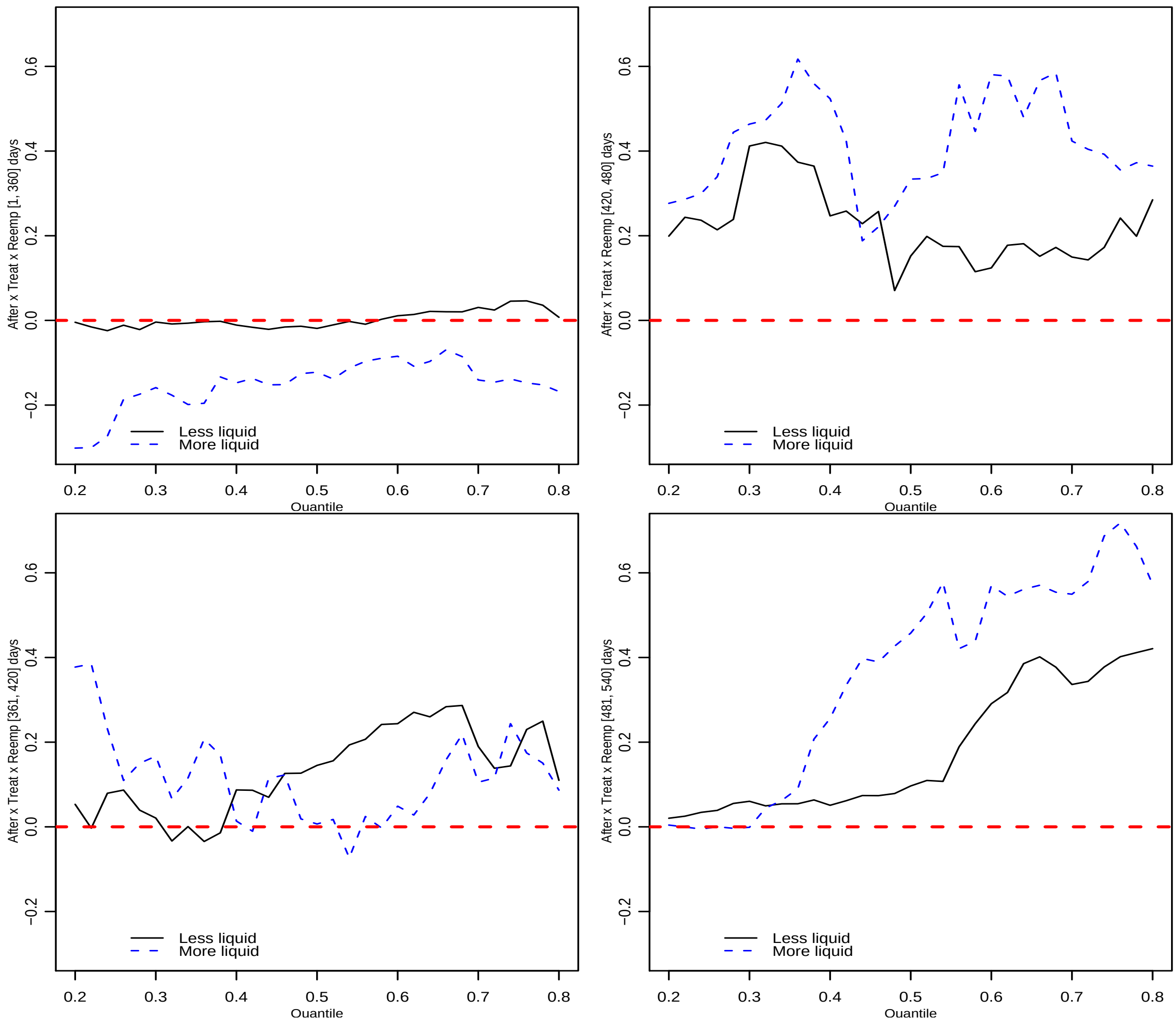

Figure 4: Liquidity effect. The sample was split into pre-unemployment wages quartiles. The quantile treatment effects of the reemployment wages distribution are estimated with quantile regression; for simplicity, dummies for the initial reemployment periods were collapsed into a $[1, 360]$ days dummy, as were dummies for one month before and after the pre-reform entitlement, $[421, 480]$ days. The solid and dashed lines represent the quantile treatment effects for less liquid (first quartile) and more liquid (top quartile) unemployed, respectively. For clarity, confidence intervals are omitted.

Appendix

Table A.1: Average treatment effects on reemployment wages by duration of unemployment

Log reemployment wages	Coefficient	Std. Error	t-value	Pr[$> \|t\|$]
Previous wage	0.373	0.007	52.213	0.000
Female	-0.034	0.007	-5.141	0.000
Unemployment duration				
$[1, 90]$ days	3.965	0.051	77.311	0.000
$[91, 180]$ days	3.984	0.052	76.327	0.000
$[181, 270]$ days	3.950	0.053	73.851	0.000
$[271, 360]$ days	3.946	0.054	72.529	0.000
$[361, 420]$ days	3.950	0.057	69.228	0.000
$[421, 450]$ days	3.862	0.063	61.107	0.000
$[451, 480]$ days	3.910	0.067	58.542	0.000
$[481, 540]$ days	3.740	0.054	69.801	0.000
> 540 days	3.692	0.052	70.983	0.000
After × Unemployment duration				
$[1, 90]$ days	-0.001	0.020	-0.043	0.966
$[91, 180]$ days	-0.060	0.024	-2.532	0.011
$[181, 270]$ days	-0.034	0.028	-1.216	0.224
$[271, 360]$ days	-0.033	0.033	-0.975	0.330
$[361, 420]$ days	-0.087	0.045	-1.933	0.053
$[421, 450]$ days	-0.081	0.066	-1.233	0.217
$[451, 480]$ days	-0.032	0.071	-0.455	0.649
$[481, 540]$ days	-0.096	0.026	-3.682	0.000
> 540 days	0.007	0.019	0.374	0.708
Treat × Unemployment duration				
$[1, 90]$ days	0.032	0.020	1.625	0.104
$[91, 180]$ days	0.016	0.023	0.704	0.482
$[181, 270]$ days	0.022	0.028	0.795	0.427
$[271, 360]$ days	-0.050	0.029	-1.731	0.083
$[361, 420]$ days	-0.014	0.038	-0.372	0.710
$[421, 450]$ days	-0.103	0.049	-2.083	0.037
$[451, 480]$ days	-0.233	0.079	-2.936	0.003
$[481, 540]$ days	-0.105	0.050	-2.112	0.035
> 540 days	0.011	0.025	0.428	0.669
After × Treat × Unemployment duration				
$[1, 90]$ days	-0.008	0.026	-0.286	0.775
$[91, 180]$ days	-0.001	0.031	-0.045	0.964
$[181, 270]$ days	0.015	0.038	0.407	0.684
$[271, 360]$ days	0.067	0.043	1.556	0.120
$[361, 420]$ days	0.022	0.059	0.374	0.708
$[421, 450]$ days	0.180	0.086	2.097	0.036
$[451, 480]$ days	0.276	0.106	2.604	0.009
$[481, 540]$ days	0.146	0.056	2.595	0.009
> 540 days	0.013	0.031	0.414	0.679
Other variables:				
Regional dummies		– Yes –		
Month of unemployment		– Yes –		
Month of reemployment		– Yes –		

Table A.2: Average treatment effects on reemployment wages by duration of unemployment for UI claims placed between January, 1998 and December, 2002

Log reemployment wages	D-in-D		QTE ($grr \in [63, 67]$)		
	All	$grr \in [63, 67]$	20th	50th	80th
Unemployment duration × After × Treat					
$[1, 90]$ days	0.001	-0.025	-0.045	0.003	-0.052
	(0.982)	(0.447)	(0.210)	(0.944)	(0.223)
$[91, 180]$ days	0.004	-0.026	0.018	0.027	-0.024
	(0.886)	(0.498)	(0.750)	(0.420)	(0.622)
$[181, 270]$ days	0.006	-0.004	-0.031	-0.001	0.060
	(0.855)	(0.926)	(0.580)	(0.978)	(0.422)
$[271, 360]$ days	**0.085**	0.072	**0.108**	0.046	0.069
	(0.024)	(0.129)	**(0.033)**	(0.300)	(0.370)
$[361, 420]$ days	0.029	0.048	0.059	0.018	0.095
	(0.567)	(0.443)	(0.632)	(0.765)	(0.168)
$[421, 450]$ days	**0.171**	**0.216**	**0.273**	**0.259**	**0.178**
	(0.014)	**(0.011)**	**(0.014)**	**(0.002)**	**(0.004)**
$[451, 480]$ days	**0.216**	**0.234**	0.004	**0.180**	0.236
	(0.021)	**(0.048)**	(0.972)	**(0.031)**	(0.153)
$[481, 540]$ days	**0.126**	**0.207**	0.020	0.132	**0.311**
	(0.017)	**(0.001)**	(0.343)	(0.147)	**(0.000)**
> 540 days	0.009	0.027	0.005	0.028	0.094
	(0.741)	(0.460)	(0.574)	(0.391)	(0.029)
Other control variable			– Yes –		
No. of observations	15 745	10 739	10 739	10 739	10 739

Notes: p-values in parentheses.

"All" indicates that the sample includes all unemployed whose previous wages where equal or greater than the minimum wages; "$grr \in [63, 67]$" indicates that the sample includes unemployed with gross replacement rates in the 63 to 67 percent range, i.e., whose previous wages ranged from 1.5 to 4.5 minimum wages. "D-in-D" and "QTE" denote, respectively, difference-in-differences and quantile treatment effects. The latter are computed for the 25th, 50th, and 75th quantiles. All regressions include a complete set of dummies for the duration of unemployment, interaction terms with the "Treat" and "After" variables. Additionally, there are dummy variables for gender, region, month of unemployment and month of reemployment. Pre-unemployment wages are including in the set of control variables.