Unemployment insurance: A life vest of re-employment wages*

Mário Centeno

mcenteno@bportugal.pt

Banco de Portugal & ISEG - U. Técnica & IZA

Álvaro A. Novo

anovo@bportugal.pt

Banco de Portugal & U. Lusófona & IZA

June 13, 2012

Abstract

This paper establishes a link between the limited entitlement period of unemployment benefits recipiency and the behavior of re-employment wages. We use a quasi-natural experiment that originates in an exogenous increase of the unemployment insurance entitlement period in Portugal. Our main contribution is to show the impact that the limited duration of UI benefits has on re-employment wages. We find that the causal effect of more generous UI on re-employment wages is minor, but there are large positive impacts on the wages of matches formed around the pre-reform exhaustion date. As predicted by non-stationary job search models, the limited UI works as a life vest of re-employment wages, possibly through its impact on the reservation wage.

Keywords: Quasi-natural experiment; Difference-in-differences; Non-stationary job search

JEL Codes: J64, J65

*We thank Pierre Cahuc, Kori Kroft, Rafael Lalive, Jan van Ours, Kostas Tatsiramos, and participants at the IZA "Unemployment Insurance and Flexicurity" workshop, Banco de España seminar series and SOLE 2011 for comments on a previous version of the paper. We also 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

An extension of unemployment insurance (UI) may have a positive impact on job search outcomes through an increase in the reservation wage and the larger financial resources available during the search period. UI plays the role of a search subsidy and may improve the allocation of workers to jobs. However, extended benefits lower job search intensity and may result in prolonged unemployment and human capital depreciation, without improving post-unemployment outcomes.

In this paper, we associate good matches with higher wages and study the impact on re-employment wages of an exogenous increase in the UI entitlement period. The exercise takes advantage of a quasi-natural experimental setting generated by a reform of the Portuguese UI system in July 1999. The policy change affected unemployed workers differently: those aged 30-34 experienced an increase in the entitlement period from 15 to 18 months, whereas those aged 35-39 kept an 18-month entitlement period. These two groups define quite naturally the treatment and control groups, respectively. The quasi-experimental nature of the treatment is explored to overcome the standard endogeneity between subsidized unemployment and re-employment wages, using a difference-in-differences methodology (Meyer 1995).

We use Portuguese Social Security administrative data covering *all* subsidized unemployment spells claimed between 1998 and 2000 and estimate that the causal impact of the UI extension on re-employment wages is small and weakly non-significant, -2.8 percent. This result is in line with previous research, which reports little or no impact of UI on re-employment wages for several European countries, such as Lalive (2007), van Ours & Vodopivec (2008), **?**, Card, Chetty & Weber (2007*a*), and Arni, Lalive & van Ours (2009). For Canada and the United States Belzil (2001) and Centeno (2004) report brighter results for the impact of UI on match quality.

In this paper, we go a step further and present evidence of the strong impact that the limited duration of UI benefits has on accepted wages. We show that the UI extension entails wage gains in excess of 20 percent in matches formed around the pre-reform exhaustion date (one month before and during the extension period). As predicted in the model of Lalive, van Ours & Zweimueller (2006), the impact of the UI extension is maximum close to the pre-reform exhaustion date; UI prevents reservation wages from falling, postponing the adjustment until the new entitlement date. Indeed, the UI extension has no impact on wages in matches formed during the first 14 months of unemployment, when both the treatment and control groups are subsidized, nor after 18 months, when both groups already ran out of benefits. Note, however, that the endogeneity of the duration of the unemployment spell may generate a selection bias and hinder the causal interpretation of these effects. We carefully test for possible sources of this selection bias, to conclude that the estimates are economically meaningful and extend our understanding of the impact of UI on job match quality.

The absence of overall gains and the concentration of gains when individuals have differentiated UI coverage points to the role of UI as a life vest of re-employment wages. UI postpones the sinking of reservation (and accepted) wages that occurs when the unemployed moves closer, or surpasses, the moment of benefit exhaustion.

This empirical behavior of re-employment wages is consistent with the decreasing path of reservation wages in nonstationary job search models (Mortensen 1986, van den Berg 1990). In particular, the fall in re-employment wages close to benefit exhaustion can be seen as the counterpart to the spike in the job finding rate observed around that date (Katz & Meyer 1990, Boone & van Ours 2009).

Altogether, these results reveal a strategic use of UI when individuals make their job acceptance decisions. The typical worker has a forward looking behavior, increasing unemployment duration due to the UI extension. However, the absence of overall wage gains and the pattern of accepted wages over the duration of the unemployment spell cast some doubts on the ability of longer entitlement periods to improve the labor market prospects of workers. The welfare gains of longer benefits may be due to reduced search costs, with no significant improvements in search productivity translated into better post-unemployment outcomes.

The paper is organized as follows. Section 2 describes the experiment. Section 3 is devoted to a careful discussion of the identification conditions. The data are presented in following section. Section 5 identifies the causal impact of longer UI on re-employment wages. Section 6 studies the impact of the limited duration of UI on re-employment wages and carefully discusses and tests for selection biases. Section 7 provides a smorgasbord of robustness checks. Concluding remarks are offered in the final section.

2 The unemployment system reform and the economy

The extension of UI entitlement: a quasi-experimental setting

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

Before the July 1999 reform there were eight entitlement levels corresponding to eight age groups. The reform affected these groups differently: it increased the entitlement period for six of the eight age groups and left the entitlement unchanged for the remaining two. As a result of the reform, some contiguous age groups started sharing the same entitlement period (see Table 1). These characteristics of the reform generate a quasi-experimental setting. We focus our evaluation on individuals aged 30-34, whose entitlement period increased from 15 to 18 months, forming a natural treatment group. For individuals in the contiguous age group, 35-39, the entitlement was left unchanged at 18 months, and we will use them as control group.

[TABLE 1; see page 22]

This pair of treatment and control groups has several important features that help in the task of identifying the causal impact. Firstly, after the reform they share exactly the same entitlement period – 18 months. Their age proximity makes it likely that treatment and control groups share similar labor market characteristics, such as labor income, schooling, marital status, and child-bearing decisions. Furthermore, labor participation is always very high among prime-age individuals; they usually suffer less from labor market swings than younger workers and do not face the type of retirement decisions common to older workers. All these features reduce the likelihood of an impact of the UI reform on the employability of benefit recipients.[1] Additionally, but not less important, the availability of data for the periods before and after the implementation of the reform allows the use of the difference-in-differences method, which controls for remaining potential unobserved heterogeneity.

[1]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 income distribution of the treatment has a small overlapping with the older control group, due primarily to different educational and experience levels.

As expected, the reform increased subsidized unemployment duration (Centeno & Novo 2009) and shifted the spike in the job finding rate of the treated group from the pre- to the post-reform exhaustion date. Figure 1 displays the noticeable spikes in the daily job finding rate at benefit exhaustion. In the left panel, in what can be interpreted as quasi-natural evidence, the spike of the treatment group moves in tandem with the shift in the exhaustion dates. The control group has spikes at the 18-month exhaustion date before and after the reform (right panel). These profiles in the job finding rate are in line with what has been found by Katz & Meyer (1990) and Meyer (1990) for the US, and van Ours & Vodopivec (2006) and Boone & van Ours (2009) for Slovenia or Schmieder, von Wachter & Bender (2009) for Germany. For Austria, Card, Chetty & Weber (2007*b*) find a more modest spike in the job finding rate.

[FIGURE 1; see page 28]

Financial generosity

In terms of its financial generosity, the Portuguese system is comparable to the OECD average. The value of UI depends on the average wage earned in the 12 months that precede unemployment by two months. For individuals with pre-unemployment average wages worth 1.5 to 4.5 minimum wages, the gross replacement rate was 65 percent. For individuals earning less than 1.5 minimum wages, the level of UI benefits equalled exactly the minimum wage, resulting in a gross replacement rate that increases for lower wages, reaching 100 percent for minimum wage earners; the level of UI could not exceed 3 minimum wages, so that the gross replacement rates falls with wages for those earning more than 4.5 minimum wages.

Exogeneity of the reform

On the subject of identification, the endogeneity of the policy decision to labor market conditions is usually a matter of concern (Card & Levine 2000). At the moment of the reform, the Portuguese labor market and the economy were buoyant (Table 2). 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]

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. Instead, 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 percent in 1992 to 3.0 percent 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.

3 Identification

More than discussing the intricacies of the estimation methods, this section focus on the question of identification of causal effects.

For easiness of exposition, consider a randomized assignment to the UI system, i.e., treatment status is independent of potential outcomes. Formally, $\{Y_{1i}, Y_{0i}\} \perp D_i$, where $D_i = 1$ if individual i is treated and 0 otherwise, and Y_{1i} and Y_{0i} represent the potential outcome (re-employment wages), respectively, under treatment and no treatment. In such case, the treatment effect on the treated, $E[Y_{1i} - Y_{0i}|D_i = 1]$, can be estimated by the empirical counterparts of $E[Y_i|D_i = 1] - E[Y_i|D_i = 0]$.

What deserves a more careful discussion, and truly motivates this section, is the identification of the causal impact of UI on re-employment wages *conditional* on the duration of the unemployment spell. Can the estimator above be used to identify this conditional causal effect? In short, the theoretical answer is no, even if assignment to UI is random. The fact that unemployment duration is itself susceptible of being affected by UI biases the estimator. In fact, the difference of re-employment wages between treated and non-treated who spent $T_i = \bar{T}$ months unemployed is:

$$\begin{aligned} &E[Y_i|T_i = \bar{T}; D_i = 1] - E[Y_i|T_i = \bar{T}; D_i = 0] \\ &\qquad = E[Y_{1i}|T_{1i} = \bar{T}; D_i = 1] - E[Y_{0i}|T_{0i} = \bar{T}; D_i = 0] \\ &\qquad = E[Y_{1i}|T_{1i} = \bar{T}] - E[Y_{0i}|T_{0i} = \bar{T}], \end{aligned} \tag{1}$$

where the last equality follows from $\{Y_{1i}, Y_{0i}, T_{1i}, T_{0i}\} \perp D_i$. This is a biased estimator of the causal effect. From equation (1), it follows that

$$
\begin{aligned}
&E[Y_i|T_i = \bar{T}; D_i = 1] - E[Y_i|T_i = \bar{T}; D_i = 0] \\
&\qquad = \underbrace{E[Y_{1i}|T_{1i} = \bar{T}] - E[Y_{0i}|T_{1i} = \bar{T}]}_{\text{Causal effect}} + \underbrace{E[Y_{0i}|T_{1i} = \bar{T}] - E[Y_{0i}|T_{0i} = \bar{T}]}_{\text{Selection bias}}, \qquad (2)
\end{aligned}
$$

where the first difference is the causal effect and the last difference represents a selection bias. In the current setting, the selection bias arises if the more generous UI affects the pool of UI receivers that reach $\bar{T}$, i.e., if it generates differences in the potential outcomes of treated and non-treated individuals. If economic theory is able to determine the sign of the selection bias term, it is fair to associate an economic interpretation to the estimates even if not with a causal interpretation.

Job search models predict that, under a more generous UI, liquidity constrained individuals would extend their unemployment longer (Chetty 2008). The stronger postponement of reemployment by treated individuals with higher liquidity constraints implies that they will represent a larger share of the workers finding a job at longer unemployment durations. If, like in Ziliak (2003) and Chetty (2008), constrained individuals have lower wages, it is fair to admit that at these longer durations the selection bias term is negative, $E[Y_{0i}|T_{1i} = \bar{T}] < E[Y_{0i}|T_{0i} = \bar{T}]$. Then, if the causal effect term is positive, our estimate can be interpreted as a lower bound of the true causal effect.

4 Data

Our study uses Social Security administrative data covering the period from January 1998 through September 2004. The dataset has very detailed information on subsidized unemployment spells and the subsequent private sector employment spells. Since we are interested in measuring wage gains through UI, we restrict the benchmark sample to individuals that move between full-time jobs with an intervening subsidized unemployment spell. This is possible because we have information on the pre-unemployment job and are able to follow the unemployed leaving the UI system and entering full-time employment.

We use *all* unemployment spells initiated during the three-year time window centered around the reform date, i.e., between January 1998 and December 2000, and give each unemployed a time window of two years to become re-employed. For example, for individuals who enter unemployment in December 2000 and exhaust the 18-month entitlement period, in June 2002, we still allow for an additional window of 6 months to observe the re-employment outcomes.[2] 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 the amount and duration of benefits and the monthly wage and starting date of the first job following unemployment. We also have information on income prior to unemployment: the average wage earned in the 12-month period that preceded unemployment by two months. This is a better measure of pre-unemployment wage than the last wage received as it is not subject to fluctuations/manipulations just prior to entering unemployment. The socio-demographic variables available are limited to gender, age, and place of residence. Fortunately, the availability of the previous average wage allows us to partially overcome the lack of more detailed individual characteristics. Table 3 presents the summary statistics of the key variables for 10,472 unemployment spells.

[TABLE 3; see page 23]

UI claimers are split between 41 percent of women and 59 percent men. The 12-month average pre-unemployment wages is 625 euros with a standard deviation of 332 euros; the monthly minimum wage in 1999 was 306 euros. Most unemployed are entitled to 65 percent of their previous gross average wage, the mode of the gross replacement rate in our data, which have an average of 66.5 percent, indicating that the distribution is slightly left-skewed in terms of pre-unemployment wages. The regional distribution of UI claimers is similar to the distribution of the unemployed population across the country during the 1998-2000 period. Like unemployment itself, benefit claims have a slight seasonal pattern.

Figure 2 plots kernel estimates of the distributions of pre- and post-unemployment wages. The left plot corresponds to the benchmark sample defined above; the right panel restricts pre-unemployment average wages to the 1.5 to 4.5 minimum wages range (i.e., to all individuals

[2]We will also check the robustness of the results to the inclusion of a longer claiming window, using data up to September, 2004.

with gross replacement rates of 65 percent), but it does not preclude re-employment wages to drop below 1.5 minimum wages.[3]

[FIGURE 2; see page 28]

The figure shows that re-employment wages are generally lower than pre-unemployment wages; the distribution of re-employment wages lies to the left of the one prevailing before the unemployment spell. For the whole sample, the mean pre-unemployment wage is 625 euros and the mean re-employment wage is only 560 euros; the difference between median wages is about the same, 522 and 455 euros, respectively. This fact is important, when interpreting our results, because the empirical exercise identifies the fraction of the re-employment wage that is attributable, in a causal sense, to the extended UI, not the actual change in wages after UI. One should keep in mind that, in general, an intervening unemployment spell between jobs seems to hinder, at least temporarily, wage progression.

5 The causal impact of UI on re-employment wages

In this section, we present the average treatment effect estimates of the UI extension based on a standard difference-in-differences (D-in-D) model. The estimated model is:

$$\log(W) = \beta_0 + \beta_1 After + \beta_2 Treat + \beta_3 After \times Treat + X\lambda + \epsilon, \tag{3}$$

where W is the re-employment wage (1999 prices) and X is a matrix with the following pre-treatment covariates: pre-unemployment 12-month average wage (1999 prices); gross replacement rate; female indicator; region dummies; and dummies of quarter of unemployment entry. $After$ is an indicator variable for the reform period, $Treat$ indicates the age group affected by the new legislation and the coefficient on the interaction term $After \times Treat$ identifies the causal treatment effect on the treated. The β's and the vector λ are coefficients.

Table 6 presents the results from the estimation of equation (3). The average treatment effect on the treated is -2.8 percent, but is weakly non-significant. This evidence is in line with

[3] We base most of our empirical exercise on the benchmark sample, but we also use this restricted sample to limit the potential degree of negative selection bias that a more generous UI may induce, to address the possible impact of the minimum wage, which limits wage losses, and also to eliminate the disincentives arising from the extremely high replacement rates of low wages.

the findings of other studies that looked at the impact of UI extensions in several European countries and report little or no effect on re-employment wages (Lalive 2007, Card et al. 2007*b*, van Ours & Vodopivec 2008). Non-experimental evidence based on variations in the replacement rates reports more significant impacts on post-unemployment wages, for example, the early study for the US by Ehrenberg & Oaxaca (1976) and, more recently, Centeno & Novo (2006) and McCall & Chi (2008).

[TABLE 6; see page 25]

There are other interesting results from the wage regression. Re-employment wages earned by females are 3.0 percent lower than those of males. Also, conditional on all other variables included in the regression, the previous wages are positively correlated with the new wage, with an elasticity of around 0.5. The relatively large estimate captures the effect of unobserved productive characteristics, for example education and experience, that are not in our dataset.

6 The impact on re-employment wages of limited UI

In this section, we explore the impact that the limited duration of UI benefits has on re-employment wages. We start with a simple eyeballing of the potential impact. Then, we carefully scrutinize the data for signs of selection bias. Finally, we present formal estimates of the impact of the limited benefits and discuss the results.

6.1 Eyeballing

A coarse measure of the impact of the limited duration of UI on match quality can be gauged from a simple graphical analysis of the distribution of re-employment wages in matches formed before and after UI exhaustion. Figure 3 plots kernel density estimates of wages in matches formed during the first year of unemployment (panels on the left) and within 16 to 18 months of unemployment – the first three months following the pre-reform exhaustion date for the treatment group (panels on the right).

[FIGURE 3; see page 29]

The two panels in the first column of Figure 3 show that the re-employment wage distribution of matches formed during the first year of the unemployment spell almost overlap for the treatment and control groups. This far away from UI exhaustion date, the differences in the entitlement periods in the pre-reform period do not seem enough to affect distinctly re-employment wages.

The outcome is quite different in the two panels in the left column of Figure 3. In the top panel, treatment individuals ran out of benefits, whereas in the bottom panel they are still entitled to UI benefits. In both panels, control individuals receive UI benefits at the unemployment durations under analysis. These panels suggest that, for the same unemployment duration, recipiency status plays a key role in explaining the differences in accepted wages. Average wage differences between the two groups go from 128 euros with different recipiency status (top panel) to only 30 euros when they share the same entitlement period (bottom panel).

6.2 Unemployment duration and selection bias

As fully acknowledged earlier, a causal interpretation of the evidence above is not awarded due to a potential selection bias. The pool of treated individuals exiting unemployment at a particular duration may have different characteristics due to the variation in UI generosity. They can differ both in terms of unobservable and observable characteristics.

We explore our quasi-experimental setting and use a standard difference-in-differences estimator to study whether the reform changed the pre-treatment observable characteristics of the pool of unemployed exiting at particular unemployment durations. While it is possible to compare the composition of the treatment group before and after the UI extension, macroeconomic factors may contribute to differences over time, confounding the identification. With the difference-in-differences estimator, we can use the control group to account for these (common) trends. Formally, for each individual pre-treatment characteristic and time-to-a-job interval (1-3, 4-6, 7-9, 10-12, 13-15, and 16-18 months), we estimate the following model:

$$x_k = \beta 0 + \beta_1 After + \beta_2 Treat + \beta_3 After \times Treat + X'_{-k}\lambda + \epsilon, \tag{4}$$

where x_k is one of the following pre-treatment covariates: pre-unemployment 12-month average wage (1999 prices); gross replacement rate; female indicator; region dummies; and dummies of quarter of unemployment entry. X_{-k} is a matrix with all these pre-treatment covariates, except the one on the left-hand side of the equation.[4] All other variables are as defined earlier. Any differences in observable characteristics attributable to the selection induced by the UI extension are identified by the coefficient of the $After \times Treat$ variable. Table 4 presents the point estimates of this coefficient. In some sense, we can interpret this as test for the common support or the balancing property of individuals in each duration interval, similar to the matching literature (Imbens 2004).

[TABLE 4; see page 23]

The UI extension did not change the distribution of the covariates across treatment status and periods (before and after the reform); for all variables and re-employment windows, the estimates of the impact of the UI extension on the average value of the treated individual characteristics are statistically non-significant. Consider the pre-unemployment wages (1st row), arguably the most important variable to measure selection. The difference-in-differences estimates range from -18.5 to 13.7 euros, economically and statistically non-significant; the average pre-unemployment wage hovers around 600 euros and the smallest associated p-value is 0.48. Interestingly, the impact is positive for individuals that got re-employed in the first 6 months of unemployment and negative for those individuals that got re-employed after 6 months. Even though these are not potential re-employment wages, the discussion in the context of job search theory suggests that a more generous UI may lead to a negative selection bias term in equation (2) at longer durations. Although not a sufficient condition, the negative sign is necessary to interpret our estimates of the impact of UI on re-employment wages as a lower bound of the causal effect.

An alternative way to tackle the issue of selection is to analyze if, for each particular duration, the increase in the UI generosity changed the probability of observing a treated individual exiting unemployment relatively to a control also exiting. To answer this question, we run a model that resembles a propensity score (Rubin 1974, Imbens 2004), but allows for different coefficients on the observable characteristics in the after period. Formally, we can

[4]In the case of regional and quarter dummies, the regression does not include on the right-hand side the complementary dummies.

test this hypothesis by estimating the following probit model for each time-to-a-job interval:

$$Treat = \beta_0 + X\Psi + \phi After + (X'After)\Theta + \varepsilon, \tag{5}$$

where X includes all the variables mentioned in the context of equation (3) and Ψ and Θ are coefficient vectors. If the more generous UI does not induce selection on observables into the decision to accept a job at a particular unemployment duration, the coefficients in Θ should be statistically non-significant. Table 5 presents the estimates of Θ. Individually, all coefficients are statistically non-significant at the standard 95 percent confidence level. Jointly, there is also no sign that the observable characteristics changed the exit decision of treated individuals with the UI reform; in all time-to-a-job intervals, the joint test statistics (3rd line from bottom) are clearly non-significant. Furthermore, to attest for the quality of the experiment and the lack of evidence of selection on observables, all reported pseudo R^2 for each probit regression are quite low, below 0.07. That is, for each of the time intervals considered, there is not enough variability in the covariates between the treatment and control groups to explain the probability that an individual belongs to the treatment group. This is the type of result that we would expect after successfully performing a matching procedure or with random assignment to treatment (Imbens 2004).

[TABLE 5; see page 24]

These tests boost our confidence that the estimates of the impact of UI on re-employment wages along the unemployment spell are not strongly influenced by the potential selection bias that may arise due to the impact of UI on unemployment duration.

6.3 (Quasi-)treatment effects of limited UI

In order to identify the effect of the extended search period on re-employment wages at different levels of unemployment duration, we include in equation (3) nine duration dummies, I_d, and all possible interactions between them and the three treatment indicators ($After$, $Treat$, and $After \times Treat$). I_d is a piecewise function of the following time-to-a-job periods (in months): 1-3, 4-6, 7-9, 10-12, 13-14, 15, 16-17, 18, and 19-24. The estimated model is:

$$\log(W) = \sum_{d=1}^{9} (\eta_d + \beta_{1d} After + \beta_{2d} Treat + \beta_{3d} After \times Treat) I_d + X\lambda + \varepsilon, \quad (6)$$

where η_d is a coefficients vector and the other variables as defined earlier.

Table 7 reports the D-in-D estimates of the impact of UI by unemployment duration; it presents only the estimates of the β_{3d}'s. We have already discussed extensively the restrictions on the causal interpretation of these coefficients due to the potential selection bias. Despite the fact that our tests showed no signs of selection on observables, the reminder of the results should be read with these caveats in mind.

TABLE 7 [see page 26]

The UI extension does not affect re-employment wages for matches formed within the first 14 months of unemployment. The policy effect kicks in during the month just prior to the pre-reform exhaustion date, suggesting that wages of treated individuals leaving unemployment in that month are 25 percent above those that would have emerged in a situation without the UI extension. The impact is maximum during the extension period (16-18 months), which can be interpreted as evidence that workers adjust their reservation wages more strongly after UI termination. Finally, the impact drops to zero after 18 months of unemployment, when both groups are already without UI. These results conform with what was already gauged in Figure 3 – hardly a 'visual' impact on re-employment wages of matches formed within one year of unemployment and a noticeable impact in the extension period. The strong drop in re-employment wages around the exhaustion date seems to be the counterpart to the behavior in the job finding rate depicted in Figure 1.

The results are in line with the predictions of nonstationary job search models (Mortensen 1986, van den Berg 1990). In these models, as the unemployed gets closer to benefit exhaustion, the value of unemployment drops, since the probability of running out of benefits increases. Consequently, this raises the marginal benefit of search and reduces the reservation wage. An extension of UI leads to higher reservation wages throughout the unemployment spell, but the impact should be stronger around the pre-reform exhaustion date (Lalive et al. 2006). This is exactly what our results indicate. But, as in Burdett (1979), UI can also play the role of a "search subsidy", improving the allocation of resources with an overall positive impact on accepted wages. However, this is not confirmed by our empirical results since the overall causal effect is negligible.

7 Robustness checks

In this section, we verify the sensitiveness of our results to the UI replacement rate, variations in the business cycle, and age effects. We also redefine the unemployment duration variable as time-to-exhaustion to further check the role of UI recipiency status. We finish by testing for other selection effects and the possible anticipation of the reform by the unemployed.

Replacement rate, business cycle, and age proximity

The impact of the benefit extension on post-unemployment outcomes might be associated with the large search disincentive arising from the high replacement rates (up to 100 percent) of low-wage workers. To address this issue, we re-estimate the model for a restricted sample of workers with gross replacement rates between 63 and 67 percent, corresponding to pre-unemployment wages between 1.5 and 4.5 minimum wages. Recall that in the benchmark sample, both pre- and post-unemployment wages are bounded from below by the minimum wage, which could limit wage losses. On the contrary, in the restricted sample there is plenty of room for wage losses. The results are reported in column (2) of Table 8, along side those for the benchmark sample (column (1)). The average treatment effects are almost identical in the two samples. This shows that the results are not driven by the high replacement rate of low-wage workers (those with pre-unemployment wages below 1.5 minimum wages). In addition, the restricted sample limits the scope for the type of negative selection discussed above, since pre-unemployment wages are much more homogeneous.

[TABLE 8; see page 26]

As an additional robustness check, but at the cost of a less uniform macroeconomic business cycle, we extend the period of UI claims until September, 2002. The results in column (3) are only slightly smaller than in the benchmark sample.

Finally, despite controlling for observable differences and using two contiguous age groups, the age range considered implies a 9-year gap (30 to 39). To obviate such potential differences, we restrict our sample to treated individuals aged 33 and 34 years and control individuals aged 35 and 36 years. The results in column (4) are very close to all previous estimates, reinforcing the validate of our main conclusions.

Time-to-exhaustion

Hitherto, the results suggest that, on average, the increase in UI generosity does not have a significant impact on re-employment wages and that any differences in re-employment wages seem to be related with the UI recipiency status. To confirm this interpretation, we use an alternative definition of the moment of re-employment, replacing time-to-a-job by time-to-exhaustion.

Time-to-exhaustion is defined as the difference between the length of the UI entitlement period and the duration of the unemployment spell. Table 9 presents the results of the regression model specified in equation (6), where the I_d variables represent now time-to-exhaustion intervals. In particular, to capture the behavior just prior to and during the extension period, we consider the following time-to-exhaustion intervals: 2 or more months to UI exhaustion, the last month of benefits, 1 to 3 months after UI exhaustion, and 4 or more months without benefits.

[TABLE 9; see page 27]

The impact of the UI extension on re-employment wages by moment of re-employment is given by the last four coefficients in Table 9 (corresponding to the β_{3d}'s in (6)). None is statistically significant, meaning that the impact of time-to-exhaustion did not change with the reform. In other words, benefiting from 15 or 18 months did not improve labor market prospects for most unemployed. This does not mean, however, that time-to-exhaustion itself is not relevant; on the contrary, as revealed by the significance and monotonicity of the coefficients on the time-to-exhaustion variables. This highlights the importance of recipiency status in the determination of re-employment wages. It reinforces also the conclusion that the entitlement extension allowed some workers to keep their reservation wages afloat for 3 additional months.

Endogenous selection and anticipation effects

The evidence presented hitherto does not show signs of self-selection due to the more generous UI. We now check if the reform attracted a pool of less re-employable workers or if the longer unemployment spells had a negative impact on their re-employability. In both cases, it is possible that, after the reform, a smaller fraction of UI recipients will get a job. We test for the impact of the reform on the probability of full-time re-employment by estimating a difference-in-differences probit model, controlling for the same covariates as in equation (3). In the estimation, a "success" corresponds to full-time re-employment and a "failure" is an unemployment spells for which we do not observe subsequent full-time employment. The marginal treatment effect for the average individual indicates a non-significant drop in re-employability of 1.75 percentage points (p-value of 0.1).[5]

Anticipation effects are an additional concern when reforms are announced in advance (van Ours & Vodopivec 2008). The new law was promulgated in April 1999 and came into effect in July, which may have induced individuals in the treatment group to postpone claims. To test whether there is such effect, we run a simple model for the log difference of subsidized unemployment inflows in our data between the treatment and control groups. The model accounts for: (i) changes in the macroeconomic environment (monthly unemployment rate[6]), (ii) the seasonality of unemployment (monthly dummies), and (iii) the possibility of higher inflows in the entire after period (a level shift). Overall, these variables account for 64 percent of the variability of the inflows difference.

Figure 4 plots the residuals of the regression model, which can be considered white noise according to the Ljung-Box and Lagrange multiplier autocorrelation tests (up to 12 lags). In the grey band, April to September 1999, the level of inflows is typically below the estimated ones conditional on the macroeconomic conditions and the higher level of inflows in the after period. Although a reduction in the inflows in the April-June period could be interpreted as a sign of postponing claims, the fact that there is no rebound in the inflows in the July-September period is not consistent with the existence of an anticipation effect.

FIGURE 4 [see page 30]

[5] Results are available from the authors upon request.

[6] Eurostat data available at: http://appsso.eurostat.ec.europa.eu/nui/show.do?dataset=lmhr_m&lang=en

Together, the several robustness checks seem to confirm that there is no significant (change in the) degree of selection on observables that could jeopardize the economic significance of our results. Indeed, it seems fair to conclude that UI acts as a life vest of re-employment wages, a role which is particularly relevant close to the exhaustion date of benefits.

8 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 (Gruber 1997) and may also translate into the improvement of post-unemployment outcomes. This paper analyzes 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 re-employment wages.

Previous evidence of the UI impact on re-employment wages has shown, at best, a small positive impact. We estimate a negligible average treatment effect of a longer UI entitlement. However, we go a step further and analyze how UI shapes re-employment wages over the unemployment spell. We explored the variation in UI recipiency status introduced by the reform and show that the largest estimated impact of the UI extension accrues to matches formed around the pre-reform exhaustion date. Although, in this case, the estimates lose a strict causal interpretation, we have shown through careful analysis that the selection bias ought to be small in our sample.

These results are compatible with a simple strategic use of UI. Extended benefits entail stronger adjustments in the reservation (and the accepted) wage closer to the extension period (around the pre-reform exhaustion date). Otherwise, longer benefits simply delay the moment of job acceptance, reducing search effort. In Portugal, the unemployed face a low-dispersion wage distribution characterized by the prevalence of low wages, which limits the potential gains from extended search. Indeed, if UI is simply a life vest of re-employment wages, there may not be any true gains associated with longer entitlement periods. In this sense, it may not come as a surprise that most studies for the US and Canada, which have less generous UI systems and more dispersed wage distributions, tend to find positive impacts of UI on post-unemployment outcomes, whereas those for Europe fail to do so. From a policy perspective, the pattern of wage gains casts some doubts on the optimality of very long entitlement periods to address the needs of those for whom the insurance motif of UI is more relevant.

References

Arni, P., Lalive, R. & van Ours, J. (2009), How effective are unemployment benefit sanctions? Looking beyond unemployment exit, Working Paper 4509, IZA.

Belzil, C. (2001), 'Unemployment insurance and subsequent job duration: Job matching versus unobserved heterogeneity', *Journal of Applied Econometrics* **16**, 619–636.

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

Burdett, K. (1979), 'Unemployment insurance payments as a search subsidy: A theoretical analysis', *Economic Inquiry* **17**(3), 333–43.

Card, D., Chetty, R. & Weber, A. (2007*a*), 'Cash-on-hand and competing models of intertemporal behavior: New evidence from the labor market', *Quarterly Journal of Economics* **122**(4), 1511–1560.

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

Card, D. & Levine, P. B. (2000), 'Extended benefits and the duration of UI spells: Evidence from the New Jersey extended benefit program', *Journal of Public Economics* **78**, 107–138.

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

Centeno, M. & Novo, Á. A. (2006), 'The impact of unemployment insurance on the job match quality: A quantile regression approach', *Empirical Economics* **31**, 905–919.

Centeno, M. & Novo, Á. A. (2009), Do the poor react less to longer unemployment benefits? 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.

Ehrenberg, R. & Oaxaca, R. (1976), 'Unemployment insurance, duration of unemployment, and subsequent wage gain', *American Economic Review* **66**(5), 754–766.

Gruber, J. (1997), 'The consumption smoothing benefits of unemployment insurance', *American Economic Review* **87**(1), 192–205.

Imbens, G. (2004), 'Nonparametric estimation of average treatment effects under exogeneity: A review', *Review of Economics and Statistics* **86**(1), 4–29.

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

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

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

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

Meyer, B. D. (1990), 'Unemployment insurance and unemployment spells', *Econometrica* **58**(4), 757–782.

Meyer, B. D. (1995), 'Natural and quasi-experiments in economics', *Journal of Business & Economic Statistics* **13**, 151–162.

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 & R. Layard, eds, 'Handbook of Labor Economics', Vol. 2, North-Holland, Amsterdam, pp. 849–919.

Rubin, D. (1974), 'Estimating causal effects of treatment in randomized and non-randomized studies', *Journal of Educational Psychology* **66**, 688–701.

Schmieder, J., von Wachter, T. & Bender, S. (2009), The effects of unemployment insurance on labor supply and search outcomes: Regression discontinuity estimates from Germany, Discussion Paper 0910-08, Columbia University.

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

van Ours, J. C. & 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. & 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 (< July, 99)		After (≥ July, 99)	
Age (years)†	Entitlement months	Age (years)†	Entitlement months
$[15,24]$	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.
* Those aged 45 or older are entitled to 2 more months for each 5 years of contributions in the previous 20 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 and Labor Force Survey, INE.
Long-term unemployment is the share of unemployed workers who have been unemployed for 12 or more months.

Table 3: Summary statistics

	Mean	Std. Deviation
Age	34.18	2.91
Females (proportion)	0.41	0.49
Pre-unemployment wages (1999 prices)	624.71	331.86
Gross replacement rate	66.5	6.80
Re-employment wages (1999 prices)	560.03	313.72
Region		
North	0.40	0.49
Center	0.17	0.38
Lisbon	0.34	0.47
Alentejo	0.03	0.17
Algarve	0.04	0.19
Islands	0.01	0.12
Unemployment entry		
1st quarter	0.31	0.46
2nd quarter	0.24	0.42
3rd quarter	0.25	0.43
4th quarter	0.21	0.41
No. observations		10,473

Source: Portuguese Social Security.
Notes: Sample uses administrative data covering *all* subsidized unemployment spells claimed between 1998 and 2000. Individuals are then followed for up to two years of the unemployment spell, covering the subsidized period and, eventually, the non-subsidized period. Only transitions to full-time jobs in the private sector are considered.

Table 4: Selection bias: Difference-in-differences estimates

	Months to reemployment											
	1-3		4-6		7-9		10-12		13-15		16-18	
Pre-u't wages	2.10	(0.90)	13.73	(0.48)	-5.08	(0.83)	-8.72	(0.76)	-18.48	(0.55)	-9.86	(0.79)
Female	0.05	(0.21)	0.04	(0.39)	0.00	(0.93)	-0.03	(0.62)	0.05	(0.46)	0.04	(0.64)
GRR	-0.25	(0.49)	0.11	(0.80)	-0.99	(0.05)	-0.83	(0.17)	-0.61	(0.35)	-0.96	(0.23)
Region												
North	-0.01	(0.69)	-0.07	(0.13)	0.02	(0.64)	-0.03	(0.58)	0.06	(0.38)	-0.05	(0.55)
Center	-0.02	(0.59)	0.04	(0.22)	0.02	(0.61)	0.08	(0.13)	-0.04	(0.45)	-0.03	(0.70)
Lisbon	0.03	(0.36)	0.02	(0.58)	-0.05	(0.37)	-0.03	(0.68)	-0.04	(0.51)	0.11	(0.15)
Alentejo	0.00	(0.71)	0.00	(0.80)	0.02	(0.25)	-0.02	(0.47)	0.01	(0.61)	-0.01	(0.72)
Algarve	-0.01	(0.64)	-0.01	(0.48)	-0.02	(0.28)	0.00	(1.00)	0.00	(1.00)	0.00	(0.95)
Islands	0.00	(0.91)	0.01	(0.37)	0.00	(0.92)	0.00	(0.90)	0.01	(0.50)	-0.03	(0.16)
U't entry												
1st quarter	-0.05	(0.13)	-0.03	(0.53)	-0.06	(0.22)	-0.08	(0.18)	0.10	(0.15)	0.01	(0.87)
2nd quarter	-0.02	(0.45)	0.06	(0.14)	0.04	(0.34)	-0.02	(0.74)	-0.03	(0.63)	0.02	(0.79)
3rd quarter	0.08	(0.02)	-0.01	(0.88)	0.04	(0.46)	0.01	(0.85)	-0.11	(0.06)	-0.07	(0.34)
4th quarter	0.00	(0.94)	-0.02	(0.50)	-0.02	(0.70)	0.09	(0.09)	0.05	(0.39)	0.04	(0.57)
Exits	3,833		2,000		1,299		932		848		555	

Notes: Difference-in-differences estimates with p-value in parentheses. For each variable, and within each time interval, it is tested the difference in the before-after evolution between the treatment and control groups. If the treatment induced any form of selection throughout the unemployment spell, then the coefficient is expected to be statistically significant (a 'low' p-value).

Table 5: Selection bias: Probit estimates

	Months to reemployment											
	1-3		4-6		7-9		10-12		13-15		16-18	
$After\times$:												
Pre-unemployment wages	0.095	(0.64)	0.393	(0.08)	0.095	(0.73)	0.263	(0.41)	0.096	(0.77)	-0.037	(0.93)
Female	0.121	(0.25)	0.110	(0.37)	-0.013	(0.93)	-0.098	(0.57)	0.135	(0.48)	0.087	(0.71)
GRR	-0.004	(0.69)	0.012	(0.37)	-0.025	(0.13)	-0.015	(0.45)	-0.015	(0.52)	-0.033	(0.22)
Region:												
Center	-0.004	(0.97)	0.199	(0.21)	0.073	(0.73)	0.374	(0.16)	-0.254	(0.40)	-0.107	(0.76)
Lisbon	0.124	(0.33)	0.144	(0.33)	-0.207	(0.25)	-0.002	(0.99)	-0.167	(0.43)	0.335	(0.22)
Alentejo	0.173	(0.52)	0.198	(0.58)	0.522	(0.30)	-0.480	(0.39)	0.335	(0.59)	-0.266	(0.73)
Algarve	-0.043	(0.86)	-0.095	(0.72)	-0.344	(0.32)	-0.043	(0.93)	0.134	(0.79)	0.113	(0.86)
Islands	0.005	(0.99)	0.423	(0.36)	0.038	(0.95)	4.593	(0.96)	0.617	(0.48)	-5.512	(0.96)
Unemployment entry:												
1st quarter	-0.139	(0.34)	0.043	(0.80)	-0.169	(0.43)	-0.509	(0.06)	0.078	(0.79)	0.033	(0.92)
2nd quarter	-0.080	(0.60)	0.237	(0.19)	0.146	(0.53)	-0.537	(0.09)	-0.252	(0.43)	-0.208	(0.57)
3rd quarter	0.133	(0.40)	0.159	(0.41)	0.119	(0.61)	-0.353	(0.22)	-0.549	(0.07)	-0.261	(0.40)
Jointly non-significant	8.875	(0.63)	10.105	(0.52)	13.656	(0.25)	13.713	(0.25)	8.884	(0.63)	6.615	(0.83)
$Pseudo\ R^2$	0.006		0.010		0.020		0.024		0.029		0.067	
Exits	3,833		2,000		1,299		932		848		555	

Notes: The full specification of the probit model is $Treat = Z\Psi + \lambda After + (Z'After)\Theta + \varepsilon$, where Z includes the variables reported above and Ψ and Θ are coefficient vectors. The values reported are Θ coefficients and corresponding p-values. A non-significant individual coefficient in Θ indicates that the probability of observing a treated individuals in a particular employment duration under the more generous UI did not change relatively to the before period (less generous UI) due to that particular observable characteristic. The "Jointly non-significant" line tests the same feature but considering instead the entire set of observables characteristics; the reported values are the χ^2 test statistic and the corresponding p-value. The "$Pseudo\ R^2$" is an indicator of the importance of all the observable characteristics to explain the probability of observing a treated individual rather than a control individual; a low value indicates that there is no sufficient variability in the observable characteristics of the two groups of individuals to explain selection to treatment at a particular duration.

Table 6: Average treatment effect on re-employment wages

Log re-employment wages	D-in-D
Intercept	3.084
	(0.000)
After × Treat	-0.028
	(0.074)
Treat	0.053
	(0.000)
After	-0.004
	(0.723)
Pre-unemployment average wages (1999 prices)	0.485
	(0.000)
Females	-0.030
	(0.000)
Gross replacement rate (GRR)	0.001
	(0.553)
Regional dummies	
Center	0.010
	(0.371)
Lisbon	0.070
	(0.000)
Alentejo	-0.032
	(0.162)
Algarve	0.058
	(0.005)
Islands	0.062
	(0.064)
Quarter of unemployment entry	
1st quarter	0.000
	(0.972)
2nd quarter	-0.005
	(0.699)
3rd quarter	0.023
	(0.047)
Quarter of reemployment	
1st quarter	-0.040
	(0.000)
2nd quarter	-0.007
	(0.542)
3rd quarter	0.003
	(0.821)
No. of observations	10,473

Notes: *p*-values in parentheses. Portuguese Social Security administrative data covering *all* subsidized unemployment spells claimed between 1998 and 2000. Individuals are then followed for up to two years, allowing for the observation of both the subsidized and non-subsidized periods and the subsequent transition to a new full-time job in the private sector. "Treat" refers to individuals aged 30-34. "After" refers to the post-reform period. The omitted regional dummy is "North".

Table 7: Re-employment wages: Difference-in-differences by unemployment duration

Log re-employment wages	D-in-D
Unemployment duration × After × Treat	
$1-3$ months	-0.028
	(0.327)
$4-6$ months	-0.025
	(0.460)
$7-9$ months	0.010
	(0.815)
$10-12$ months	0.000
	(0.994)
$13-14$ months	0.094
	(0.139)
15 months	**0.248**
	(0.013)
$16-17$ months	**0.359**
	(0.000)
18 months	**0.384**
	(0.002)
$19-24$ months	-0.010
	(0.795)
Other control variable	– Yes –
No. of observations	10,473

Notes: p-values in parentheses. In addition to the variables listed in Table 6, the regression includes a complete set of dummies for the duration of the unemployment spell, and all possible interaction terms with the "Treat" and "After" variables. For simplicity and because the quasi average treatment effect on the treated is given by the coefficients on the interaction terms between the duration dummies and the $After \times Treat$, we report only this set of coefficients.

Table 8: Re-employment wages: Robustness checks for alternative samples

	Sample composition			
	Benchmark	GRR $\in [63, 67]$	Up to 2002	Age $\in [33, 36]$
Log re-employment wages	(1)	(2)	(3)	(4)
Unemp. duration × After × Treat				
$1-14$ months	-0.004	-0.012	0.009	-0.026
	(0.826)	(0.554)	(0.551)	(0.335)
15 months	**0.248**	**0.247**	**0.210**	**0.287**
	(0.013)	**(0.025)**	**(0.010)**	**(0.058)**
$16-18$ months	**0.372**	**0.388**	**0.297**	**0.314**
	(0.000)	**(0.000)**	**(0.000)**	**(0.003)**
$19-24$ months	-0.008	0.053	-0.026	-0.015
	(0.845)	(0.253)	(0.482)	(0.821)
Other control variable	– Yes –			
No. of observations	10,473	7,888	13,094	4,060

Notes: p-values in parentheses. "Benchmark" refers to the sample used in all of the previously reported results; "GRR $\in [63, 67]$" is the sample that includes only individuals with gross replacement rates in that range, i.e., individuals whose pre-unemployment 12-month average wages were in the 1.5 to 4.5 minimum wages range; "Up to 2002" includes in the sample UI claims placed up until September 2002; the workers are then followed up to September 2004. "Age $\in [33, 36]$ considers only individuals aged 33 to 36 years old. All estimated include the additional control variables mentioned in Table 7.

Table 9: Re-employment wages: Difference-in-differences by time to UI exhaustion

Log re-employment wages	Coefficient	p-value
Log pre-unemployment wages	0.546	(0.000)
Females	-0.030	(0.000)
Gross replacement rate	0.001	(0.224)
Regional dummies		
Center	-0.024	(0.023)
Lisbon	0.035	(0.000)
Alentejo	-0.036	(0.095)
Algarve	0.004	(0.824)
Islands	0.008	(0.805)
Quarter of unemployment entry		
1st quarter	-0.008	(0.428)
2nd quarter	-0.019	(0.088)
3rd quarter	0.004	(0.717)
Time-to-exhaustion (entitlement $-$ duration):		
≥ 2 months	2.761	(0.000)
1 month	2.493	(0.000)
$[-3, -1]$ months	2.472	(0.000)
≤ -4 months	2.430	(0.000)
After $\times$ Time-to-exhaustion:		
≥ 2 months	-0.006	(0.637)
1 month	-0.097	(0.001)
$[-3, -1]$ months	-0.002	(0.958)
≤ -4 months	0.083	(0.057)
Treat $\times$ Time-to-exhaustion:		
≥ 2 months	0.022	(0.072)
1 month	0.022	(0.575)
$[-3, -1]$ months	-0.058	(0.224)
≤ -4 months	0.040	(0.323)
After $\times$ Treat $\times$ Time-to-exhaustion:		
≥ 2 months	**-0.008**	**(0.638)**
1 month	**0.022**	**(0.649)**
$[-3, -1]$ months	**0.083**	**(0.163)**
≤ -4 months	**-0.031**	**(0.619)**
No. of observations	10,473	

Notes: The difference-in-differences impacts on re-employment wages by moment of re-employment are given by the last four coefficients. The first two refer to individuals re-employed while receiving UI; the last two to individuals re-employed after exhausting UI.

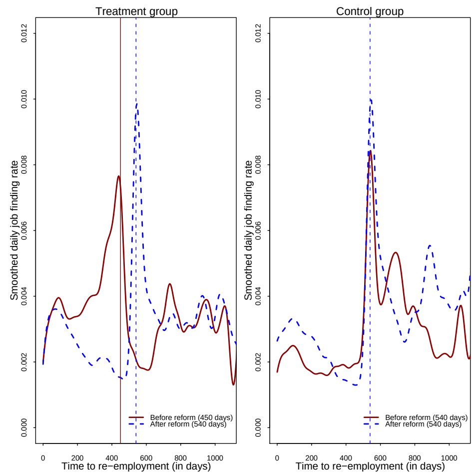

Figure 1: Smoothed non-parametric daily job finding rates. Vertical lines indicate the entitlement periods.

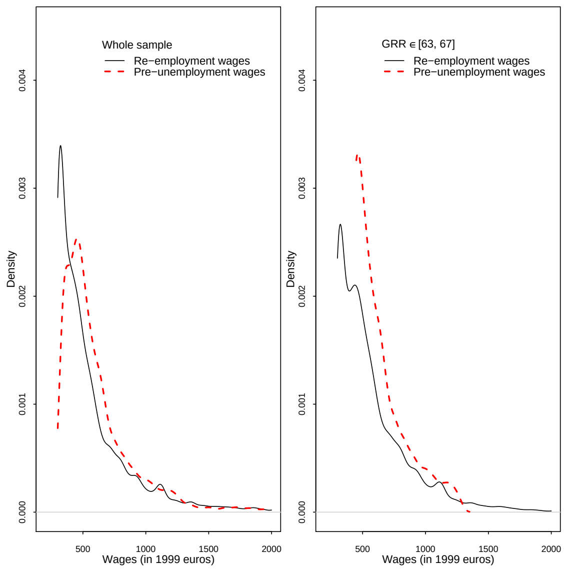

Figure 2: Kernel estimates of pre-unemployment and re-employment wages densities. All densities are truncated at the minimum wage. In the right plot, pre-unemployment wages are restricted to the 1.5 to 4.5 minimum wages range (i.e., gross replacement rate $\in [63, 67]$ percent). The bandwidth parameter is set such that it equals the standard deviation of the smoothing (gaussian) kernel.

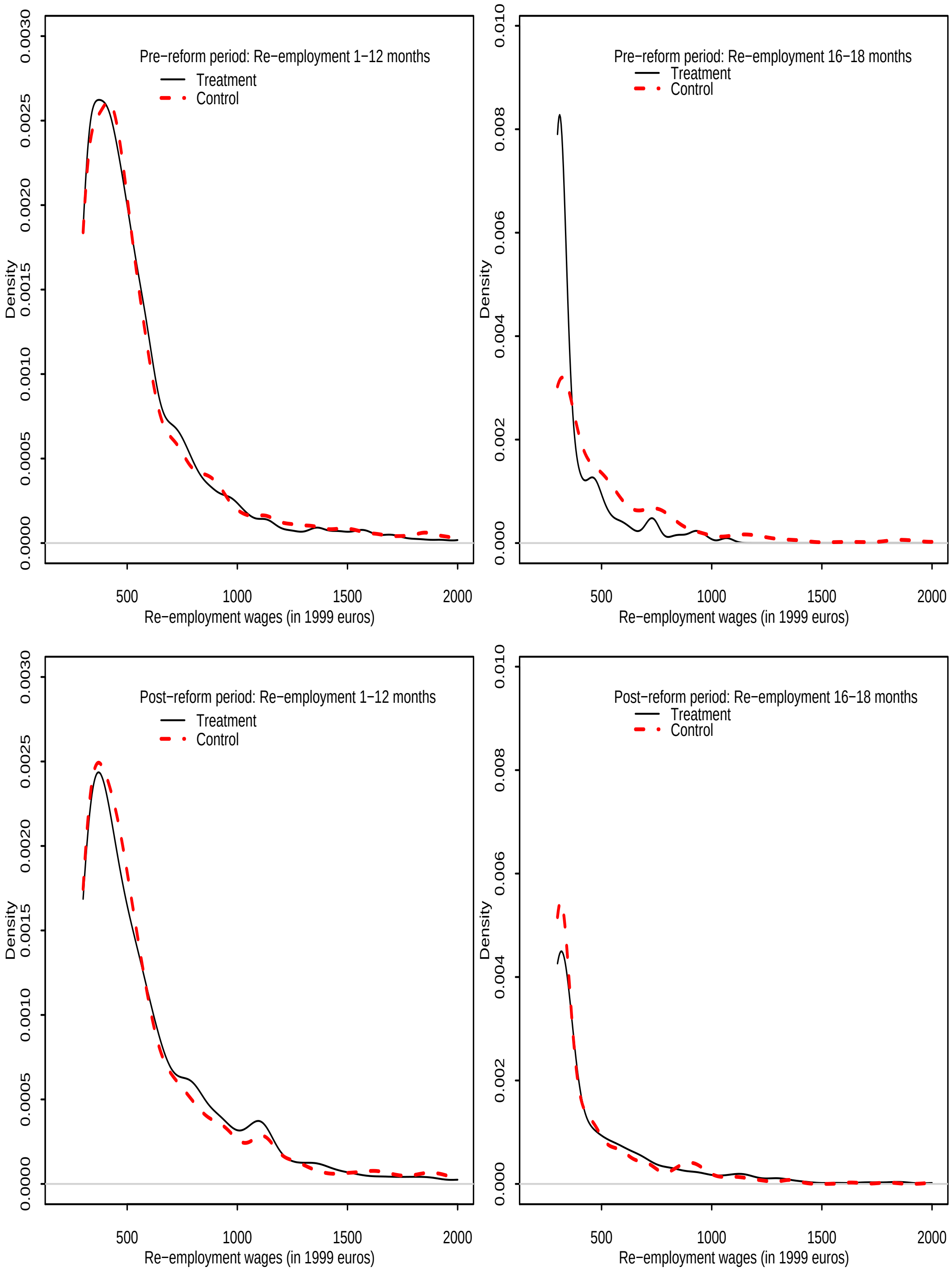

Figure 3: Kernel estimates of re-employment wages densities: The four panels compare re-employment wages of treatment and control groups according to the duration of the unemployment spell (up to one year or $16-18$ months), covering the periods before and after the July 1999 reform. Before the reform the treatment and control group individuals were entitled, respectively, to 15 and 18 months of UI; after the reform, all individuals are entitled to 18 months. See Figure 2 for estimation details.

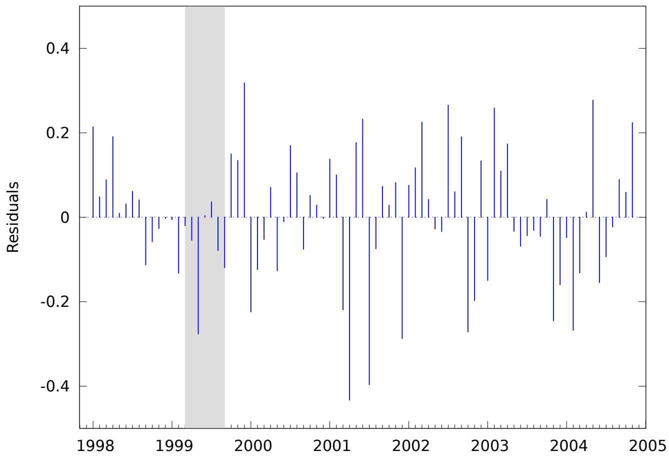

Figure 4: Least squares residuals of the regression model: $\{\log(Inflows_t^{Treat}) - \log(Inflows_t^{Control})\} = \sum_{m=1}^{12} \beta_m D_m + \beta_{13} UR_t + \beta_{14} After + \varepsilon_t$, where $Inflows$ is a monthly measure of UI claims by the treatment and control groups, D_m are monthly dummies, UR_t is the monthly unemployment rate, and $After$ is a dummy variable for the after period. The estimation comprises the 1998-2004 period on a monthly frequency. The R^2 of the regression is 0.64 and the Ljung-Box and the Lagrange Multiplier tests fail to reject clearly the null of zero autocorrelation for up to 12 lags.