Is unemployment insurance a life vest of re-employment wages? Treatment effects evidence*

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Abstract

This paper establishes a causal link between the limited time 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 to collect causal evidence. We find that a more generous UI has a small positive impact on re-employment wages, at most 2 percent. Our main contribution is to show that this treatment effect varies widely with the moment of re-employment. The larger impacts are obtained around the pre-reform exhaustion date, in what can be seen as the counterpart of the spike in the job finding rate during the same period. Relatively to a counterfactual without the UI extension, the treatment effect on re-employment wages is in excess of 20 percent for matches formed during the extension period (after 15 to 18 months of unemployment). These gains are larger in the upper quantiles of the distribution of re-employment wages. As predicted by non-stationary job search models, our results highlight the role of UI in shaping the search behavior of the unemployed, working as a life vest of re-employment wages, possibly through its impact on the reservation wage.

Keywords: Unemployment insurance; Re-employment wages; Quasi-natural experiment; Treatment effects

JEL Codes: J64, J65

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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.

The main contribution of the paper is to present causal evidence of the strong impact that the limited duration of UI benefits has on accepted wages. We show that the re-employment wage gains associated with an UI extension are limited to matches formed around the pre-reform exhaustion date (one month before and during the extension period). While previous studies considered already the impact of UI on post-unemployment outcomes (e.g. Belzil 2001, Centeno 2004, Lalive 2007, van Ours & Vodopivec 2008, Arni, Lalive & van Ours 2009) all fell short of showing when potential gains arise during the unemployment spell.

This behavior of re-employment wages is consistent with the decreasing path of reservation wages in nonstationary job search models close to benefit exhaustion (Mortensen 1986, van den Berg 1990), and 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). Indeed, job search theory predicts that the impact of extending UI should be maximum close to the pre-reform exhaustion date, as UI prevents reservation wages from falling, postponing the adjustment to the new entitlement date.

We use Portuguese Social Security administrative data covering *all* subsidized unemployment spells claimed between 1998 and 2000. Individuals are then followed up to September 2004, allowing for the observation of the complete unemployment spell (both the subsidized and unsubsidized periods) and the subsequent transition to a new job. 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 covers both the pre- and post-reform periods with information on the salary and starting date of the first job following unemployment and the 12-month average wage earned prior to entering unemployment.

To evaluate the policy effects, we resort to two methodologies: a difference-in-differences approach (Meyer 1995), and a quantile treatment effects framework (Koenker 2005). The former estimates the average treatment effect of the UI extension, and the latter addresses issues related with the heterogeneity of the UI impact over the distribution of re-employment wages.

The average impact of the UI extension on re-employment wages is small, 2.1 percent, but is weakly non-significant. This result confirms findings from previous research on the impact of benefit extensions, which report little or no impact on re-employment wages for several European countries, such as Lalive (2007), van Ours & Vodopivec (2008), Fitzenberger & Wilke (2007), and Card, Chetty & Weber (2007*a*). However, we go a step further and show that there is a sizeable impact (above 20 percent) in matches formed around the pre-reform exhaustion date (between the 15th and 18th month of unemployment). On the contrary, there are no wage gains in matches formed during the first 14 months of unemployment, when both groups are subsidized, nor after 18 months, when both groups already ran out of benefits. The absence of overall gains and the concentration of gains when treatment and control have differentiated UI coverage, points to the role of UI as a life vest of re-employment wages, postponing the sinking of reservation (and accepted) wages that occurs when the unemployed moves closer, or surpasses, the moment of benefit exhaustion. The absence of match quality effects may be related to the specific labor demand conditions faced by the unemployed, namely the low dispersion of wages, the prevalence of minimum wage jobs, and the low arrival rate of job offers.

The average treatment effect assumes an uniform impact on the distribution of re-employment wages. This hypothesis may not apply if the unemployed face different wage prospects and different groups of workers benefit differently from the increased generosity. The quantile treatment effects show that, for matches formed during the extension period (16 to 18 months), the impact of UI is increasing with the wage quantile.

Altogether, these results point to a strategic use of UI when individuals make their job acceptance decisions. The typical worker reveals a forward looking behavior, postponing re-employment 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. Under unfavorable labor demand conditions, the welfare gains of longer benefits may be due to reduced search costs, without significant improvements in search productivity or post-unemployment outcomes.

The paper is organized as follows. Sections 2 and 3 describe the experiment and the data. Section 4 provides an eyeballing of the results and motivates the more structured approaches of sections 5 and 6, which present the average treatment effect and quantile treatment effects, respectively. We test also for potential sources of selection bias and assess the robustness of our results in Section 7. Concluding remarks are offered in the final section.

2 The unemployment system reform and identification

The extension of some UI entitlement periods

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 only 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). 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 21]

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, such as labor income, schooling, marital status, and child-bearing decisions. Furthermore, labor participation is always very high among prime-age individuals.

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 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 is 65 percent. For individuals earning less than 1.5 minimum wages, the level of UI benefits equals 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 cannot exceed 3 minimum wages, so that the gross replacement rates falls with wages for those earning more than 4.5 minimum wages.

Conditions for identification

The take up of UI, the benefit level, the duration and post-unemployment outcomes are potentially endogenous. Individuals who expect long unemployment spells and a large wage drop may be more likely to claim benefits. Fortunately, our quasi-natural experimental setting, characterized by the availability of suitable treatment and control groups and the observation of individuals in the periods before and after the implementation of the reform, may overcome the selection bias and endogeneity issues usually present when evaluating the impact of UI on search outcomes.

On the subject of identification, the endogeneity of the policy decision to labor market conditions is usually a matter of concern (Card & Levine 2000). However, 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 21]

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, prime-age workers, the core of our treatment and control groups, usually suffer less from labor market swings than younger workers and they do not face the type of retirement decisions common to older workers. Overall, these factors make our comparison of pre- and post-reform outcomes more convincing, as it was not driven by a specific trend in the labor market or to questions related with population aging.

3 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 our attention 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. This time window allows for enough time to observe a re-employment episode. Indeed, even for an individual who starts unemployment in December 2000 and exhausts the 18-month entitlement period in June 2002 without a job, we still have a window of 27 months (up to September 2004) to observe the re-employment outcomes.

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 wages earned in the 12-month period that precede 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 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 problem posed by the lack of more detailed individual characteristics. We restrict our benchmark sample to unemployment spells for which both the pre-unemployment average wage and the re-employment wage are greater than or equal to the minimum wage. Table 3 presents the summary statistics of the key variables for the periods before and after the reform and the 11,503 unemployment spells for the two age groups $[30, 34]$ and $[35, 39]$.

[TABLE 3; see page 22]

The treatment group comprises 5,995 observations, of which 2,403 are from the period before July 1999. The control group has 2,737 observations in the before period and 2,771 in the after period. The percentage of women is similar across treatment and control groups, although it increased in the post-reform period. The differences in the 12-month average pre-unemployment wages between treatment and control groups are, as expected, marginally favorable to older individuals. The gross replacement ratio hovers around 66 percent, a value close to the mode of the system, 65 percent. Overall, although re-employment wages are essentially the same among both groups in both periods, unemployment leads to large wage losses, between 6 and 20 log points. Under nonstationary job search, reservation wages are expected to decline with unemployment duration. Indeed, after 18 months of unemployment, accepted wages are 20 log points below those accepted during the first 15 months. Furthermore, wages accepted while the worker is receiving UI seem to be higher than those accepted after running out of benefits. For instance, before the reform, in matches that follow an unemployment spell that lasted between 16 and 18 months, when the treatment group was no longer entitled to UI, accepted wages by treatment individuals were on average smaller than those accepted by control individuals, a 33 log points difference. Earlier on the unemployment spell, when both groups were entitled to UI, the differences were negligible, 1 to 2 log points.

The reform increased subsidized unemployment duration and shifted the spike in the job finding rate of the treated group from the pre- to the post-reform exhaustion date.[1] 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. 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 27]

Given our focus on re-employment wage gains associated with UI, we take a first look at the distribution of pre- and post-unemployment wages. Figure 2 plots kernel estimates of both distributions (without distinguishing between the treatment and control groups). 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 with gross replacement rates of 65 percent), but it does not preclude re-employment wages to drop below 1.5 minimum wages.[2]

[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 645 euros and the mean re-employment wage is only 553 euros; the difference between median wages is smaller, 529 to 448 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.

[1] In a companion paper, Centeno & Novo (2009) present a full account of the impact of the reform on subsidized unemployment duration.

[2] We base our empirical exercise on the benchmark sample, but in the final section we also use this restricted sample to perform one of the robustness analyses.

4 Eyeballing the UI impact on re-employment wages

The main results of our empirical exercise can be gauged from a simple graphical analysis of the distribution of pre- and post-unemployment wages, while motivating also the more structured approach of the following sections.

The time profile of the job finding rate shown in Figure 1, related to the limited duration of UI, must also have an impact on accepted wages. A preliminary empirical assessment of this claim can be obtained by looking at 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 panels in the first row of Figure 3 refer to the pre-reform period. The distributions of wages of matches formed during the first year of unemployment almost overlap for the treatment and control groups. However, it is quite interesting to note that, in the 3 months after running out of benefits, the proportion of low wages accepted by treated individuals is much higher than in the control group, still entitled to benefits at these durations. The average wage for the treatment group is 394 euros, whereas for the control group is 529 euros.[3] This evidence suggests that, for the same unemployment duration, recipiency status plays a key role in explaining the differences in accepted wages between treated and control individuals.

The panels in the second row of Figure 3 confirm this idea. They refer to the post-reform period, in which both groups share 18 months of UI entitlement. The plots show that during the first year of unemployment the distribution of the quality of matches formed by treatment and control individuals remains similar. Remarkably, this is now also true for the distribution of wages in matches formed after 15 months in unemployment. The difference in average wages of the two groups is only 36 euros, but now higher for the younger group.

Figure 4 presents additional evidence of the impact of the exhaustion date on re-employment wages. It plots the distribution of wages of matches formed during the three-month periods prior to and after the exhaustion date. The data are from the pre-reform period, in which treatment and control groups had different entitlement periods. It is rather striking that,

[3] In 1999, the minimum wage was 306 euros.

in both cases, the exhaustion date plays a critical role in shaping the re-employment wages distribution. Accepted wages by individuals who run out of UI benefits are lower than those accepted by UI recipients (median wages differ by 155 and 153 euros for the treatment and control groups, respectively).

[FIGURE 4; see page 30]

Overall, these results point towards a large impact of the policy on re-employment wages of matches formed around the pre-reform exhaustion date. They are also re-assuring of the quality of our quasi-natural experiment; under similar conditions individuals have akin outcomes.

5 Re-employment wages: Average treatment effects

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:

$$\log(W) = \beta_1 After + \beta_2 Treat + \beta_3 After \times Treat + \sum_{i=1}^{9} \eta_i I_i + x'\lambda, \tag{1}$$

where W is the re-employment wage, $After$ is an indicator variable for the post-July 1999 period, and $Treat$ indicates the age group affected by the new legislation. The I_i are indicator variables for unemployment duration, a piecewise function of the following nine periods (in months): 1-3, 4-6, 7-9, 10-12, 13-14, 15, 16-17, 18, and +19. The vector x includes the pre-unemployment 12-month average wage, and dummy variables for gender regional labor markets, and the quarter of unemployment and of re-employment. The $\beta's, \eta_i$ and the vector λ are coefficients.

Table 4 presents the results from the estimation of equation (1). The average treatment effect on the treated is 2.1 percent, but is weakly non-significant. This evidence is in line with 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). The 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 4; see page 23]

There are other interesting results from the wage regression, but without a causal interpretation. Re-employment wages earned by females are about 2.8 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. The dummies for duration show a declining profile of re-employment wages.

The graphical evidence of Figures 3 and 4 motivate the estimation of the average treatment effect on wages formed at different unemployment durations, in particular around the UI exhaustion date. In order to identify the causal effect of the extended search period on re-employment wages at each level of unemployment duration, we include in equation (1) all possible interactions between the nine duration dummies, I_i, and the three treatment indicators ($After$, $Treat$, and $After \times Treat$). The estimated model is:

$$\log(W) = \sum_{i=1}^{9} (\eta_i + \beta_{1i} After + \beta_{2i} Treat + \beta_{3i} After \times Treat) I_i + x'\lambda. \tag{2}$$

Table 5 reports the D-in-D estimates of the average treatment effect on the treated by level of unemployment duration; it presents only the estimates of the β_{3i}'s. Table A.1, in the Appendix, reports the remaining coefficients associated with the duration dummies.

TABLE 5 [see page 24]

The UI extension does not affect re-employment wages for matches formed within the first 14 months of unemployment. The policy effect kicks in only during the month just prior to the pre-reform exhaustion date, suggesting that wages of treated individuals leaving unemployment in that month are 20 percent above those that would have emerged in a situation without the UI extension. The impact is even slightly higher (around 35 percent) after the 15-month threshold, 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 is 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. 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. 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 effect is non-significant, with the exception of the extension period.

Several features of the Portuguese labor market may help explaining the partial failure of UI in improving search outcomes, namely the high incidence of minimum wage jobs among the re-employed, associated with a lower wage dispersion, and the low arrival rate of job offers. In 1999, the wage distribution of unemployed workers shows a high prevalence of minimum wage jobs among re-employed workers, almost 12 percent earn exactly the minimum wage, which compares with 7 percent among private sector salaried workers. These figures are reflected in the relatively low standard deviation of re-employment wages, 313 euros, which compares with 435 euros for the whole economy at the time. Finally, the low arrival rate of job offers in the Portuguese economy, well documented in Addison, Centeno & Portugal (2009), may be related with the poor outcomes from UI extension. There are not that many job opportunities, and the ones available are not matches with high wages. These characteristics reduce the scope of gains from extended search, and also the attractiveness of longer unemployment spells, in particular for low-wage workers.

Our data does not allow us to sort out all these explanations, but we can use quantile regression to further analyze the role of the wage distribution. Given the specific structure of the labor market (high incidence of minimum wage and segmented access to job offers), the expected impact of the reform may not be homogeneous across 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 be searching in thinner labor markets and/or may not be able to search longer. For example, the incidence of minimum wage jobs is not equally distributed among all workers in the economy.

6 Re-employment wages: Quantile treatment effects

6.1 Methodology

Quantile regression, first introduced by Koenker & 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, β the associated coefficients, and τ is a quantile in the interval $[0, 1]$. Its point estimates, $\beta(\tau)$, characterize and distinguish the effects of covariates, for instance, in the upper and lower quantiles of the distribution.

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.*

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:

$$\delta(\tau) \equiv \Delta(F^{-1}(\tau)) = G^{-1}(\tau) - F^{-1}(\tau). \tag{3}$$

In the two-sample case, the quantile treatment effect is simply estimated by the sample analogous of equation (3), 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. In the Appendix, we extend the quantile treatment effect to a framework resembling the difference-in-differences and discuss at length the identification hypotheses.

6.2 Quantile treatment effects estimates

In the quantile regression analysis, we hypothesize that the logarithm of re-employment wages, $\log(W)$, have linear conditional quantile functions, Q, of the form:

$$Q_{\log(W)}(\tau) = \sum_{i=1}^{9} (\eta_i(\tau) + \beta_{1i}(\tau)After + \beta_{2i}(\tau)Treat + \beta_{3i}(\tau)After \times Treat)I_i + x'\lambda(\tau), \tag{4}$$

where all the variables are defined as above. The results for the 25th, 50th, and 75th quantiles are presented in Table 6.

[TABLE 6; see page 25]

The results confirm the idea that re-employment wage gains associated with the UI extension arise only at unemployment durations in which individuals were previously uninsured. These gains are larger in matches formed with higher (conditional) re-employment wages than in the lower tail of the wage distribution. At shorter durations and after the UI extension, there are no wage gains, neither to high- nor low-wage jobs. This shows that UI fails to improve the allocation of workers across the wage distribution for matches formed during most of the unemployment spell duration.

Figure 5 provides a more complete view of the impact of UI along the distribution of re-employment wages. Each panel represents, for the estimated quantiles, the point estimates of the coefficient associated with the interaction of the $After \times Treat$ variable and the duration indicators. Although, we chose to limit our attention to the quantiles $\tau \in [0.20, 0.80]$, it is worth emphasizing that all observations are used in the estimation process. The dashed lines represent 90 percent confidence intervals based on 500 bootstrapped samples. In order to focus our attention on the more relevant unemployment durations, we aggregate all re-employment events that occurred within the first 14 months of unemployment. We do the same for jobs started during the extension period (16 to 18 months).

FIGURE 5 [see page 31]

The figure reads much like the estimates presented in Table 6. For unemployment duration up to 14 months, the point estimates are close to zero and are statistically non-significant for all quantiles. For matches formed within the last month of benefits for the treatment group in the pre-reform period (15th month), there seems to be signs of potential wage gains, but the estimates are statistically non-significant, probably due to the small number of observations in this range of unemployment duration. The significant impact of UI is observed for matches formed in the extension period, $16 - 18$ months. The gains increase up to the 40th quantile and remain higher thereafter. Once UI benefits are exhausted (after 18 months) individuals are no better off, the point estimates return to values hovering below zero, but statistically non-significant.

7 Selection bias and robustness

Despite the quasi-natural experimental setting, one should be worried about potential sources of selection bias in the data. For example, it is possible that the more generous UI attracted more individuals of the 30-34 age group after the reform. However, this does not seem to be the case during our evaluation period, 1998 to 2000. According to the Labor Force Survey, the share of UI recipients in total unemployment remained fairly stable throughout our sample period, increasing by about 2.5 percentage points in both the treatment and control groups (from 34.1 before the reform to 36.8 percent after the reform for the treatment group and from 40.7 to 43.1 percent for the control group).

We may still be concerned that the more generous UI attracts a less re-employable pool of workers or that the longer unemployment spells have 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. Table 7 reports the estimates of the average treatment effect. In the estimation, to the benchmark sample of full-time re-employed ("successes" in the probit), we add the unemployment spells for which we do not observe subsequent full-time employment ("failures"). We consider two observation periods. One corresponding to the sample used hitherto, which allows re-employment to occur up to September 2004 (column (1)), and a sample with a shorter re-employment observation window: the entitlement period plus one year (column (2)). The first sample gives a considerably long period for re-employment, and more so for those who enter unemployment earlier in the sample period. In order to control for biases arising from differentiated re-employment windows, the second sample gives the same time to re-employment for all individuals after UI exhaustion, regardless of the period of unemployment entry . We focus on the $After \times Treat$ coefficient, which yields the average treatment effect. For both samples and model specifications, the estimated marginal effects are clearly non-significant, leading to the conclusion that the policy change did not affect the probability of re-employment of treated individuals.

[TABLE 7; see page 26]

Our results have highlighted the role that the exhaustion of UI has on determining re-employment wages; matches formed after UI exhaustion have significantly lower wages. However, it is plausible that individuals who exhaust their UI benefits are a selected sample of the whole population of unemployed workers. Admittedly, those that reach the end of the UI entitlement period could represent a worst draw from the wage distribution, reflecting poorer labor market prospects. If this is the case we should have sharp differences in the distribution of wages according to the exhaustion status. To address the possibility of a selection bias, we consider the distribution of pre-unemployment wages of two groups of treated individuals. A first group with workers re-employed in the last three months of UI entitlement (with $13-15$ months of unemployment duration), and a second group re-employed within the three months after UI exhaustion ($16-18$ months). Figure 6 plots kernel density estimates of the pre-unemployment wage distribution for these two groups in the pre-reform period. The two distributions overlap for the most part of their support, even if those re-employed after UI exhaustion had slightly lower median and average pre-unemployment wages, 15 and 11 euros, respectively. This figures are in striking contrast with the differences observed in Figure 4, which is the counterpart figure in terms of re-employment wages. The similarities in terms of pre-unemployment wages allow us to conclude that the pool of workers that exhaust their benefits does not seem to be self-selected, at least no more so than those that exit in the last 3 months of UI. This is reassuring, but we stress that the difference-in-differences methodology further controlled for common unobserved heterogeneity between the treatment and control groups.

FIGURE 6 [see page 32]

Our sample comprises a wide range of wages, including minimum-wage workers. As stated before, these workers have higher replacement rates, to which we might associate a higher disincentive effect of UI. To consider these issues at once, we re-estimate the average treatment effect model for a restricted sample of workers with gross replacement rates between 63 and 67 percent that correspond to pre-unemployment wages ranging from 1.5 to 4.5 minimum wages. The results are reported in Table 8, along side those for the benchmark sample (column (1)). In comparison with the benchmark sample, the average treatment effects are significant for the same unemployment durations, with point estimates that are slightly higher for the restricted sample (column (2)). 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; pre-unemployment wages are at least 1.5 minimum wages, while re-employment wages are bounded from below by the minimum wage. Recall that the quantile treatment effect estimates for the bottom quantiles are smaller than for higher quantiles, which may explain why in the case of the benchmark sample the average treatment effect is smaller than in the restricted sample.

[TABLE 8; see page 26]

As an additional robustness check, but at the cost of a shorter re-employment window, we extend the period of UI claims until the end of 2002. The results in column (3) are quite close to those in the benchmark sample. Also, the quantile treatment effects, presented in Figure A.1 in the Appendix, are consistent with the conclusions eported hitherto.

[FIGURE A.1; see page 35]

Overall, the tests presented are suggestive evidence that the estimates are not driven by selection issues and sampling schemes. This reinforces the causal identification of the results obtained from our quasi-natural experiment.

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. Our estimate of the average treatment effect is also small, 2.1 percent, and weakly non-significant. However, we go a step further and analyze how these gains vary over the unemployment spell. This is important because, if the impact of UI comes about through changes in the reservation wage or search effort, as in the models of Mortensen (1986) and van den Berg (1990), it is expected to be concentrated around the benefit exhaustion. Indeed, the largest estimated impact of the UI extension accrues to matches formed around the pre-reform exhaustion date. Furthermore, these gains are larger for higher re-employment wages as indicated by the quantile treatment effect estimates.

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, the unemployed simply delay the moment of job acceptance, reducing search effort. The unemployed face a wage distribution characterized by the prevalence of low wages, low dispersion and a small arrival rate of job offers, limiting the potential gains from extended search. Indeed, if UI is simply a life vest of re-employment wages, there may not be any true gain 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 shorter UI entitlements and more dispersed wage distributions for the unemployed, 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 (concentrated at quite long unemployment durations and benefiting less low re-employment wages) 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.

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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		
$[50, 54]$	27	$[45, 64]$	30(+8)*
$[55, 64]$	30		

† Age at the beginning of the unemployment spell.
* For those aged 45 or older, 2 months can be added for each 5 years of social contributions during the previous 20 calendar years.

Table 2: The Portuguese economy before and after July 1999

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

Sources: National accounts 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: Average values by treatment status and period

	Before		After	
	Treatment	Control	Treatment	Control
Age	31.9	36.9	31.8	36.8
Females (proportion)	0.37	0.35	0.47	0.43
Log pre-unemployment wages (1999 prices)	6.26	6.39	6.37	6.39
Gross replacement rate	66.9	65.1	66.4	66.2
Log re-employment wages (1999 prices)	6.20	6.19	6.22	6.20
by unemployment duration:				
$1-15$ months	6.24	6.26	6.29	6.28
Treatment − Control	-0.02		0.01	
After − Before		0.03		
$16-18$ months	5.92	6.25	6.26	6.14
Treatment − Control	-0.33		0.12	
After − Before		0.45		
≥ 19 months	5.98	6.06	6.04	6.04
Treatment − Control	-0.08		0.00	
After − Before		0.08		
No. of observations	2 403	2 737	3 592	2 771

Source: Portuguese Social Security records

Notes: Sample uses administrative data covering *all* subsidized unemployment spells claimed between 1998 and 2000. Individuals are then followed up to September 2004, allowing for the observation of the complete unemployment spell (both the subsidized and non-subsidized periods) and the subsequent transition to a new full-time job in the private sector. Treated individuals were aged 30-34 at the moment of unemployment and control individuals were aged 35-39. “Before” refers to spells initiated in the pre-reform period covering January 1998 through June 1999. “After” refers to spells initiated in the post-reform period covering July 1999 to December 2000.

Table 4: Average treatment effects on re-employment wages

Log re-employment wages	D-in-D
Intercept	3.294
	(0.000)
After × Treat	0.021
	(0.137)
Treat	-0.002
	(0.843)
After	-0.019
	(0.070)
Pre-unemployment average wages (1999 prices)	0.481
	(0.000)
Females	-0.021
	(0.060)
Unemployment period:	
$4-6$ months	-0.045
	(0.000)
$7-9$ months	-0.069
	(0.000)
$10-12$ months	-0.111
	(0.000)
$13-14$ months	-0.148
	(0.000)
15 months	-0.210
	(0.000)
$16-17$ months	-0.186
	(0.000)
18 months	-0.343
	(0.000)
≥ 19 months	-0.028
	(0.000)
Other control variable:	
Regional dummies (6 regions)	– Yes –
Quarter of unemployment	– Yes –
Quarter of re-employment	– Yes –
No. of observations	11 503

Notes: *p*-values in parentheses
Sample uses Portuguese Social Security administrative data covering *all* subsidized unemployment spells claimed between 1998 and 2000. Individuals are then followed up to September 2004, allowing for the observation of the complete unemployment spell (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 re-employment period is $1-3$ months.

Table 5: Re-employment wages: Average treatment effects by unemployment duration

Log re-employment wages	D-in-D
Unemployment duration × After × Treat	
$1-3$ months	-0.028
	(0.345)
$4-6$ months	-0.023
	(0.498)
$7-9$ months	0.024
	(0.574)
$10-12$ months	0.004
	(0.933)
$13-14$ months	0.076
	(0.238)
15 months	**0.223**
	(0.027)
$16-17$ months	**0.364**
	(0.000)
18 months	**0.389**
	(0.002)
≥ 19 months	-0.015
	(0.659)
Other control variable	– Yes –
No. of observations	11 503

Notes: p-values in parentheses.
The regression includes a complete set of dummies for the duration of unemployment, and all possible interaction terms with the “Treat” and “After” variables; the full results for these dummies are presented in Table A.1. Additionally, there are dummy variables for gender, region, quarter of unemployment and quarter of re-employment. Pre-unemployment average wages are also included in the set of control variables.

Table 6: Re-employment wages: Quantile treatment effects by unemployment duration

	Quantiles		
Log re-employment wages	25th	50th	75th
Unemployment duration × After × Treat			
$1-3$ months	-0.030	-0.037	0.005
	(0.310)	(0.182)	(0.902)
$4-6$ months	0.008	-0.010	-0.056
	(0.816)	(0.758)	(0.196)
$7-9$ months	-0.033	-0.024	0.028
	(0.493)	(0.604)	(0.637)
$10-12$ months	0.078	-0.010	-0.032
	(0.188)	(0.854)	(0.676)
$13-14$ months	0.102	0.013	0.191
	(0.156)	(0.859)	(0.094)
15 months	0.155	0.144	0.129
	(0.188)	(0.295)	(0.383)
$16-17$ months	**0.281**	**0.360**	**0.278**
	(0.001)	**(0.000)**	**(0.016)**
18 months	**0.274**	**0.304**	**0.426**
	(0.048)	**(0.050)**	**(0.010)**
≥ 19 months	-0.024	-0.048	-0.006
	(0.230)	(0.161)	(0.898)
Other control variable		– Yes –	
No. of observations		11 503	

Notes: p-values in parentheses based on 500 bootstrapped samples. Quantile treatment effects are computed for the 25th, 50th, and 75th 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, quarter of unemployment and quarter of re-employment. Pre-unemployment wages are also included in the set of control variables.

Table 7: Re-employment probability: Probit difference-in-differences estimate (marginal effects)

	Job search period	
	Benchmark	Entitlement + 1 year
Re-employed (full-time job)	(1)	(2)
After × Treat	-0.002	-0.004
	(0.845)	(0.703)
Treat	-0.007	-0.016
	(0.343)	(0.071)
After	-0.023	-0.035
	(0.001)	(0.000)
Log unemployment days	-0.040	-0.117
	(0.000)	(0.000)
Pre-unemployment average wages (1999 prices)	0.077	0.099
	(0.000)	(0.000)
Females	-0.022	-0.016
	(0.000)	(0.005)
Other control variable:		
Regional dummies	– Yes –	
Quarter of unemployment	– Yes –	
Quarter of re-employment	– Yes –	
No. of observations	12 986	12 986
Proportion of full-time re-employment	88.5	82.0

Notes: *p*-values in parentheses.
"Job search period: Benchmark" takes the benchmark sample and considers all transitions that are observed in entire sample period, January 1998 to September 2004. "Job search period: Entitlement + 1 year" considers only transitions to full-time jobs that occurred during the entitlement period or in the year that follows benefits exhaustion. In the latter sample all individuals have the same re-employment window upon UI exhaustion. The marginal effects are computed for a treated male in the post-reform period, earning the average of the 12-month average pre-unemployment wages, with an average (log) unemployment duration.

Table 8: Re-employment wages: Average treatment effects for alternative samples

	Sample composition		
	Benchmark	GRR $\in [63, 67]$	Up to 2002
Log re-employment wages	(1)	(2)	(3)
Unemployment duration × After × Treat			
$1-14$ months	-0.002	-0.012	0.013
	(0.912)	(0.569)	(0.405)
15 months	**0.224**	**0.241**	**0.186**
	(0.027)	**(0.029)**	**(0.025)**
$16-18$ months	**0.377**	**0.390**	**0.302**
	(0.000)	**(0.000)**	**(0.000)**
≥ 19 months	-0.011	0.014	-0.019
	(0.746)	(0.719)	(0.547)
Other control variable		– Yes –	
No. of observations	11 503	8 751	14 479

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 December 2002, this reduces the re-employment observation window. All estimated include the additional control variables mentioned in Table 4.

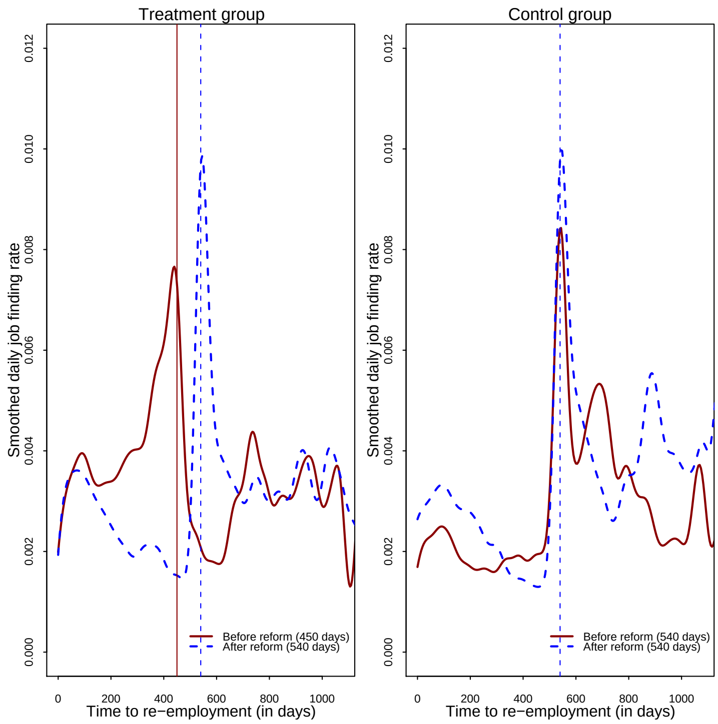

Figure 1: Smoothed non-parametric daily job finding rates. Vertical lines indicate the entitlement periods.

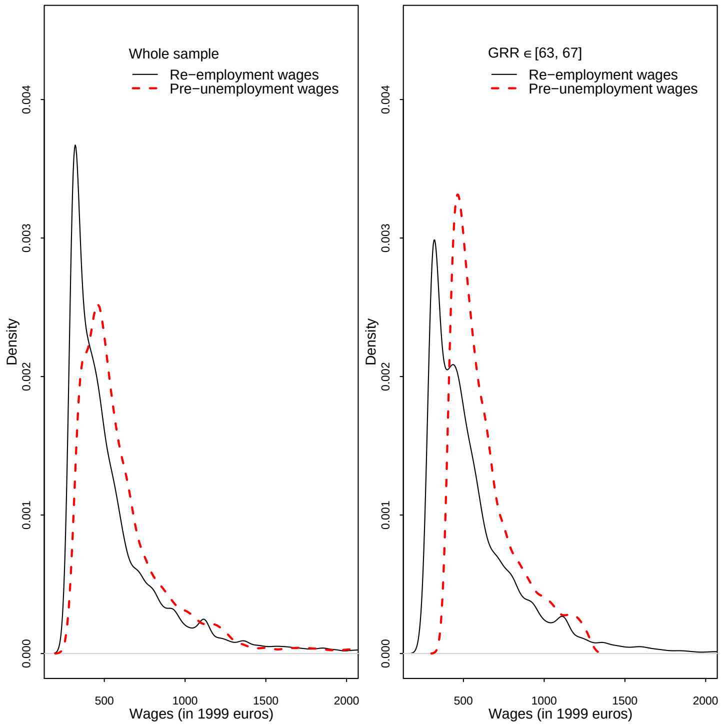

Figure 2: Kernel density estimates of pre-unemployment and re-employment wages. In the right plot, pre-unemployment wages are restricted to the 1.5 to 4.5 minimum wages range (i.e. gross replacement rate $\in [63, 67]$ percent).

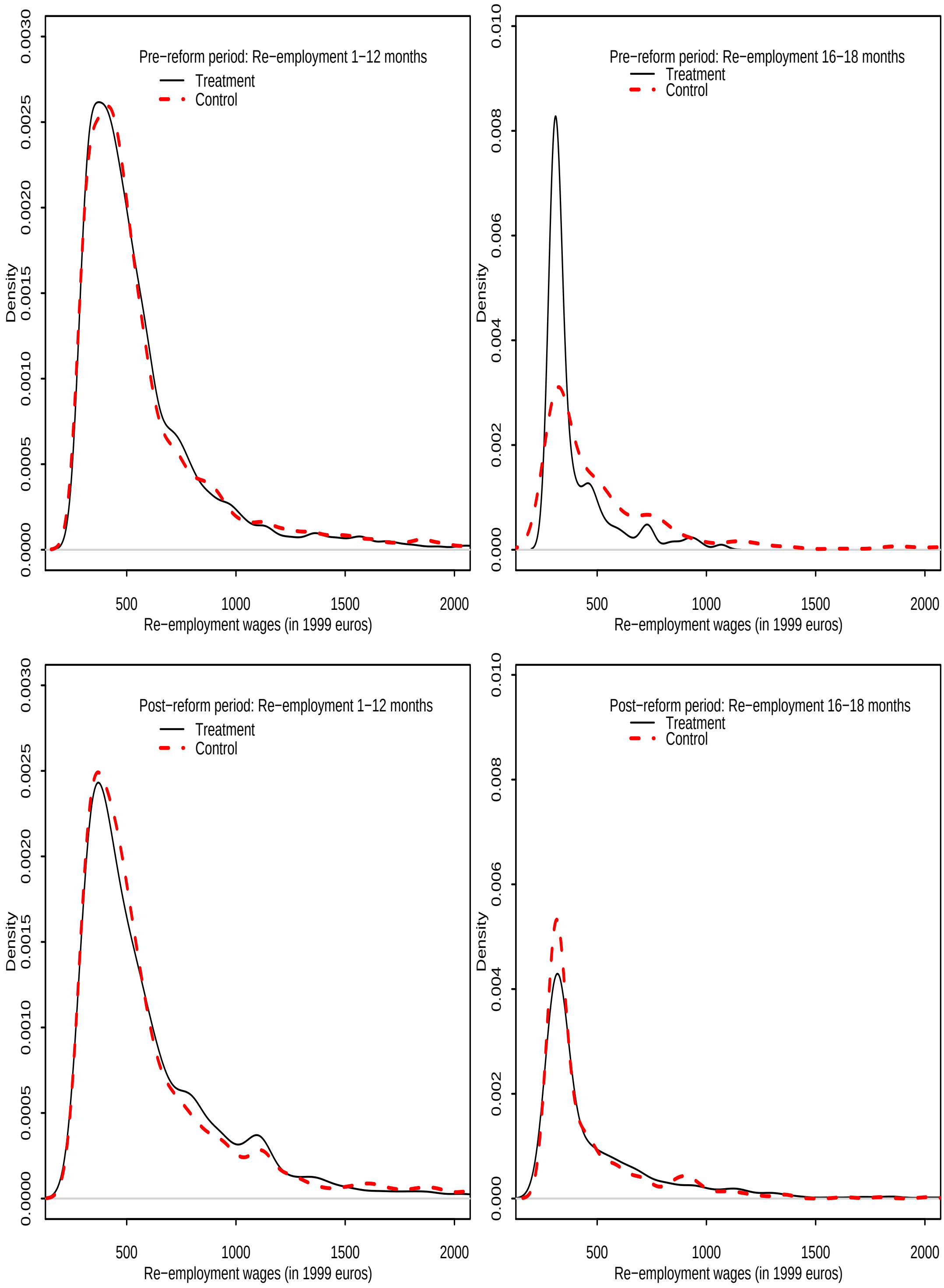

Figure 3: Kernel density estimates of re-employment wages: The four panels compare re-employment wages of treatment and control groups according to the duration of the unemployment spell (up to one year or $16-18$ months), covering the periods before and after the July 1999 reform. Before the reform 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.

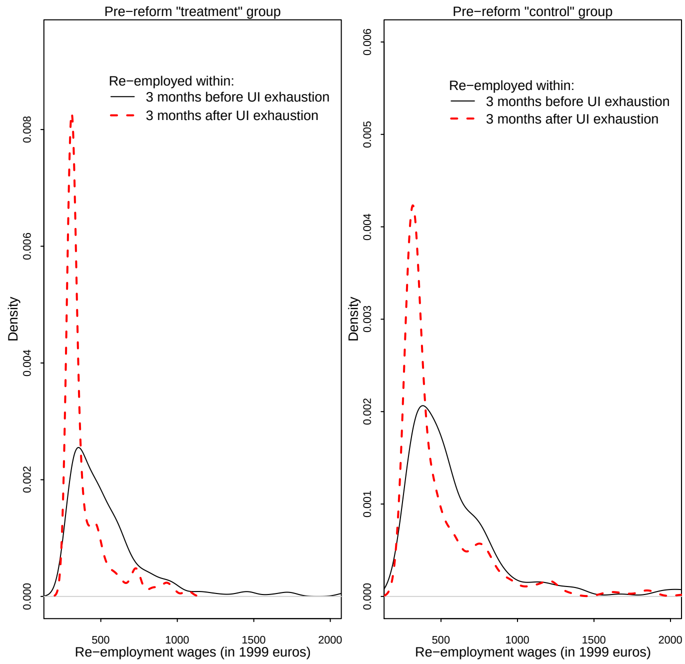

Figure 4: Kernel density estimates of re-employment wages: The two panels compare re-employment wages of treatment and control groups according to the date of re-employment, distinguishing between those that occurred within the three-month period prior to UI exhaustion and those that occurred within the three-month period after UI exhaustion. Both panels refer to the period before the reform, where the treatment group was entitled to 15 months of UI and the control group to 18 months.

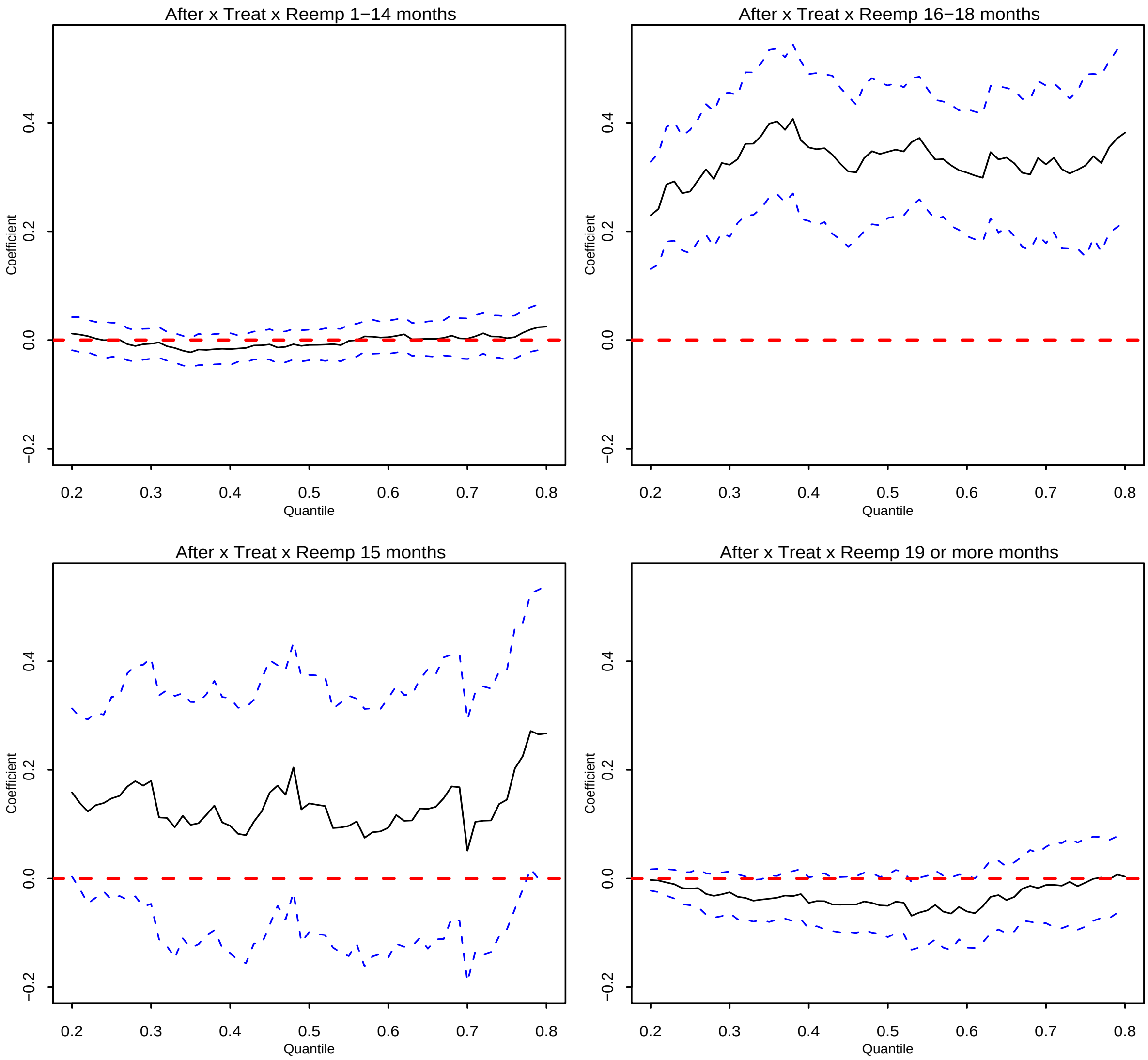

Figure 5: Quantile treatment effects conditional on the duration of subsidized unemployment. This figure plots the impact of receiving an entitlement extension of UI on the τ-th quantile of the re-employment wage distribution conditional on having spent $t_0 - t_1$ months unemployed. For instance, if re-employment occurred in the $16 - 18$ months (top-right plot), re-employment wages of the 30th quantile were about 0.05 log points higher than they would have been in the absence of the extension; for the 70th quantile, the impact is about 0.2 log points and statistically significant. The dashed lines represent 90 percent confidence intervals based on 500 bootstrapped samples.

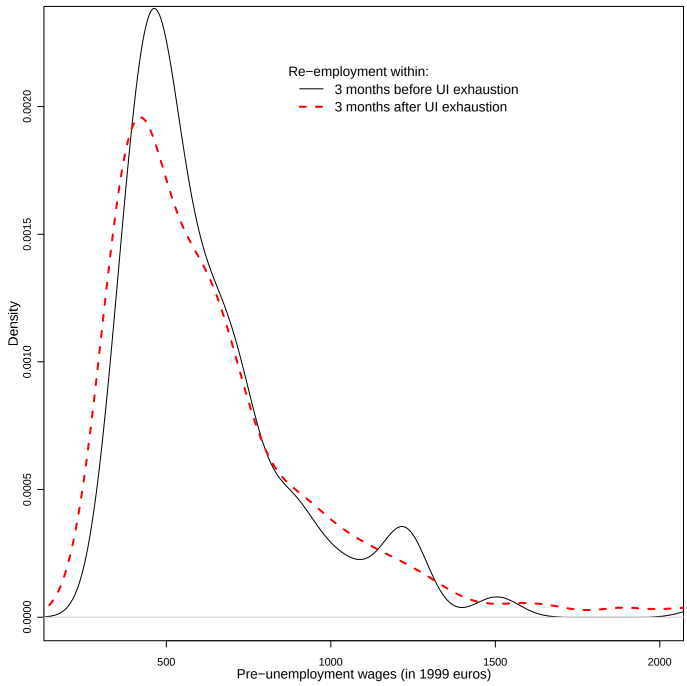

Figure 6: Kernel density estimates of pre-unemployment wages: The two kernel density estimates compare pre-unemployment wages from the pre-reform period of two sets of treatment individuals. The solid line corresponds to individuals re-employed within the three-month period prior to UI exhaustion and the dashed line corresponds to individuals re-employed within the three-month period after UI exhaustion. The treatment group was entitled to 15 months of UI before July 1999.

Appendix

Quantile treatment effect identification

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. The 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,

$$G^{-1}_{y_0(t')|D=1}(\tau) - G^{-1}_{y_0(t)|D=1}(\tau) = F^{-1}_{y_0(t')|D=0}(\tau) - F^{-1}_{y_0(t)|D=0}(\tau), \quad \forall \tau. \tag{5}$$

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 homogeneous 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.

Our identification hypothesis allows us to identify the quantile treatment effect as

$$\begin{aligned}
\delta(\tau) &\equiv G^{-1}_{y_1(t')|D=1}(\tau) - G^{-1}_{y_0(t')|D=1}(\tau) \\
&= G^{-1}_{y_1(t')|D=1}(\tau) - G^{-1}_{y_0(t')|D=1}(\tau) + \{G^{-1}_{y_0(t')|D=1}(\tau) - G^{-1}_{y_0(t)|D=1}(\tau)\} - \\
&\qquad \{F^{-1}_{y_0(t')|D=0}(\tau) - F^{-1}_{y_0(t)|D=0}(\tau)\} \\
&= \{G^{-1}_{y_1(t')|D=1}(\tau) - G^{-1}_{y_0(t)|D=1}(\tau)\} - \{F^{-1}_{y_0(t')|D=0}(\tau) - F^{-1}_{y_0(t)|D=0}(\tau)\}. \qquad (6)
\end{aligned}$$

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.

Table A.1: Average treatment effects on re-employment wages by duration of unemployment

Log re-employment wages	Coefficient	Std. Error	t-value	Pr[$>|t|$]
Previous wage	0.481	0.009	55.686	0.000
Female	-0.028	0.007	-3.814	0.000
Unemployment duration				
$1-3$ months	3.259	0.058	56.226	0.000
$4-6$ months	3.298	0.059	55.502	0.000
$7-9$ months	3.242	0.061	53.246	0.000
$10-12$ months	3.208	0.062	51.660	0.000
$13-14$ months	3.245	0.066	49.475	0.000
15 months	3.189	0.073	43.918	0.000
$16-17$ months	3.147	0.066	47.630	0.000
18 months	3.208	0.072	44.310	0.000
≥ 19 months	2.937	0.059	49.617	0.000
After $\times$ Unemployment duration				
$1-3$ months	0.018	0.022	0.819	0.413
$4-6$ months	-0.048	0.026	-1.855	0.064
$7-9$ months	-0.019	0.031	-0.610	0.542
$10-12$ months	0.022	0.037	0.596	0.551
$13-14$ months	-0.112	0.051	-2.192	0.028
15 months	-0.119	0.076	-1.572	0.116
$16-17$ months	-0.062	0.056	-1.104	0.270
18 months	-0.121	0.091	-1.330	0.183
≥ 19 months	-0.004	0.018	-0.203	0.840
Treat $\times$ Unemployment duration				
$1-3$ months	0.050	0.022	2.255	0.024
$4-6$ months	0.009	0.026	0.339	0.734
$7-9$ months	0.010	0.031	0.321	0.748
$10-12$ months	0.003	0.034	0.084	0.933
$13-14$ months	-0.064	0.041	-1.557	0.119
15 months	-0.088	0.061	-1.446	0.148
$16-17$ months	-0.254	0.050	-5.082	0.000
18 months	-0.341	0.076	-4.467	0.000
≥ 19 months	0.037	0.028	1.315	0.189
After $\times$ Treat $\times$ Unemployment duration				
$1-3$ months	-0.028	0.029	-0.944	0.345
$4-6$ months	-0.023	0.034	-0.678	0.498
$7-9$ months	0.024	0.042	0.563	0.574
$10-12$ months	0.004	0.049	0.084	0.933
$13-14$ months	0.076	0.064	1.181	0.238
15 months	0.223	0.101	2.214	0.027
$16-17$ months	0.364	0.078	4.695	0.000
18 months	0.389	0.126	3.091	0.002
≥ 19 months	-0.015	0.033	-0.441	0.659
Other variables:				
Regional dummies		– Yes –		
Quarter of unemployment		– Yes –		
Quarter of reemployment		– Yes –		

Notes: See Table 4.

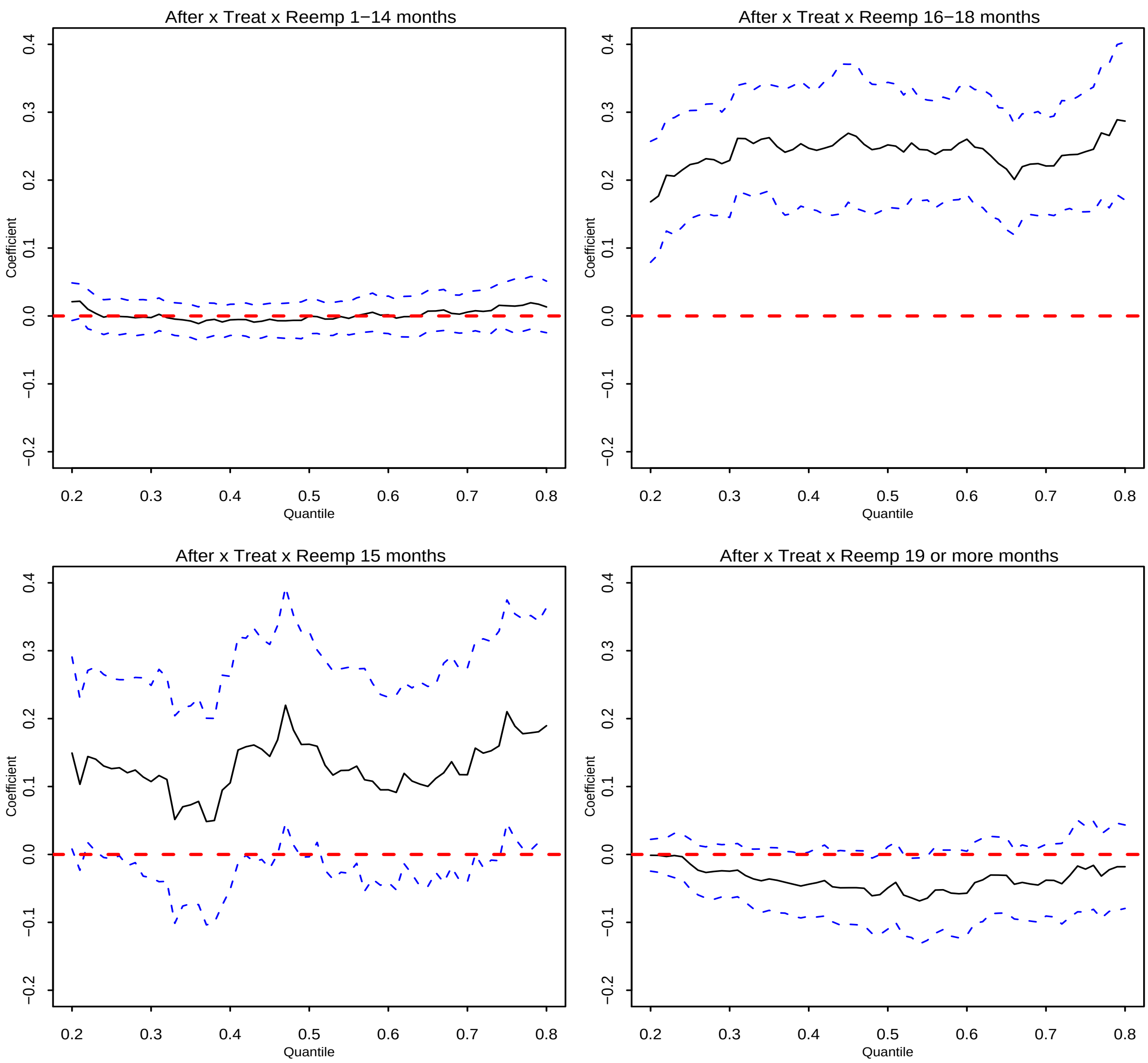

Figure A.1: Quantile treatment effects conditional on the duration of subsidized unemployment. Sample includes UI claims placed between January 1998 and December 2002, with re-employment window until September 2004. The number of observations is 14,479. This figure plots the impact of receiving an entitlement extension of UI on the τ-th quantile of the re-employment wage distribution conditional on having spent $t_0 - t_1$ months unemployed. The dashed lines represent 90 percent bootstrapped (500 samples) confidence intervals.