# Identifying unemployment insurance income effects with a quasi-natural experiment*


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**Abstract**

The negative impact of unemployment insurance (UI) on labor supply has been primarily attributed to the distortion in the relative price of leisure. This paper acknowledges that UI has also a non-distortionary income effect generated by easying the liquidity constraints of the unemployed. Using an exogenous increase in the entitlement period as a quasi-experimental setting, we find evidence of an important income effect. For individuals with the same replacement ratio, the impact is larger for those with pre-unemployment income around and below the median, with the exception of those at the bottom of the distribution (the first quintile). The fact that the most constrained do not profit much from the additional entitlement period conforms to the nonstationarity of the job search process. These results point to the importance of setting the entitlement period as function of pre-unemployment income, as it is already the case with the financial generosity of the UI system.



*Keywords*: Unemployment insurance; Unemployment duration; Liquidity constraints; Income effect.

*JEL Codes*: J65, J64, J23.



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# 1 Introduction

The impact of the unemployment insurance system on labor supply decisions has been extensively studied in the public finance and labor economics literature, as reviewed in Krueger and Meyer (2002). The identified reductions in labor supply induced by unemployment insurance (UI) are attributed primarily to the distortion of the relative price of leisure caused by the benefits. In line with Chetty (2005), we argue that UI can also have an income effect that varies with the degree of liquidity constraints faced by the unemployed, generating an heterogeneous impact of UI on unemployment duration. In this paper, we study the impact of an extension to the benefits entitlement period on subsidized unemployment duration. The legislative change was introduced in July 1999 in the Portuguese UI system, when the economic outlook was positive. This policy exogeneity, together with the fact that the reform only affected particular age groups, generated a privileged quasi-natural experimental setting for evaluation. The availability of data from before and after the change in generosity allows us to confidently identify its impact on unemployment duration (Meyer 1995). To evaluate the heterogeneous impact of UI, we use the quantile treatment effects methodology (Koenker 2005).

The UI literature has emphazised the link between generosity and unemployment duration as the result of a substitution effect between leisure and work, with UI acting primarily as a subsidy to unproductive leisure. However, the total effect of UI on unemployment duration is the sum of this distortionary substitution effect and a non-distortionary income effect – through the marginal utility of wealth, as in the Mortensen (1986) nonstationary job search model. In this model, the marginal utility of wealth becomes sensitive to the level of UI benefits for liquidity constrained individuals (those who find it harder to smooth consumption over labor market states), creating an income effect on unemployment duration. This liquidity constraint is introduced in the model assuming that the unemployed have a limited capacity to self-finance their costs of search. Thus, because unemployed workers differ in terms of their degree of constraints, UI generates a heterogenous impact on unemployment duration. Chetty (2005) emphasizes this in a model where constrained individuals alter their job search behavior more strongly than unconstrained unemployed in reaction to an increase in UI generosity. This type of heterogeneity is also the focus of the literature on the benefits of UI as a consumption smoothing device. In fact, Gruber (1997) and Browning and Crossley (2001) showed that such benefits are concentrated in the sub-group of unemployed workers that face tighter liquidity constraints.

Nonstationary job search theory can be used to predict that responses to increased UI generosity will differ throughout the length of the unemployment spell (Mortensen (1986) and van den Berg (1990)). In nonstationary environments the optimal search strategy delivers a declining reservation wage. In this context, every exogenous variable faced by the unemployed (namely the level of UI, the arrival rate of job offers and the wage offers distribution) can cause nonstationarity because its value is dependent on unemployment durations. Additionally, the different parameters of the UI system (replacement rate and duration of the entitlement period) have distinct impacts on the profile of the exit rate from unemployment. While both parameters increase the reservation wage throughout the entire unemployment spell, an extension in the entitlement period will have a smaller impact at short durations than around the time when benefits expire. Indeed, as the increase in the reservation wage is the result of the increased probability of finding a job before the benefit expires, it would have a smaller impact early in the unemployment spell. Lalive, van Ours and Zweimueller (2006) present empirical evidence of this behavior. In a nonstationary job search environment, the stronger impact of UI generosity at longer durations creates another source of heterogeneity between individuals with different degrees of liquidity. If the distribution of wage offers (or the arrival rate of job offers) deteriorates more significantly for those with tighter constraints (usually at the bottom of the income distribution), then they would be less able to adjust their reservation wage in response to the increased generosity and, subsequently, less able to extend their unemployment spells at all durations, but particularly at longer ones.

If the benefits were to increase at the beginning of the unemployment spell, say in the form of severance payments, then we would unequivocally expect the more constrained unemployed to have a larger reaction in terms of unemployment duration, as shown in the empirical results of Chetty (2005). However, since the benefits of an extended entitlement period only accrue after the previous entitlement limit, generating a delayed response, the more constrained may find it difficult to adjust their behavior to the increased generosity. As Cahuc and Zylberberg (2006) put it, high-income individuals enjoy a wider margin of manoeuver, allowing them to take greater advantage of the increased generosity from UI.

We use a quasi-experimental setting, generated by an exogenous increase in UI generosity, to identify the causal effect of an extension of the entitlement period in the behavior of subsidized unemployment. We acknowledge the possibility of heterogeneous effects at two, not independent, levels. First, the impact on duration will differ with the degree of liquidity constraints, which is proxied by different levels of pre-unemployment income. Secondly, for the same level of liquidity constraints, the impact of a UI extension may vary at distinct locations of the distribution of subsidized unemployment durations. To fully capture the nonstationarity of the job search environment, we use the quantile treatment effects methodology.

Our identification strategy rests on the exogenous variation in UI generosity introduced by the July 1999 reform of the Portuguese UI system. The new law increased substantially the entitlement period for all individuals aged 30-34 years, the treatment group, for whom the benefit period changed from 15 to 18 months. For those aged 35-39, the control group, the entitlement period was left at 18 months. These features result in a privileged quasi-experimental setting, not only because the reform benefited prime-aged individuals, but also because thereafter treatment and control have the same entitlement periods. In addition, the good economic conditions prevailing at the moment of the reform are favorable for our empirical strategy, as the policy change was not motivated by the evolution of the labor market.

Using Social Security administrative data which cover UI related social transfers, our results confirm the idea that, when facing longer entitlement periods, unemployed individuals take them up, remaining in subsidized unemployment for longer periods. These results are in line with the previous evidence for the American labor market (Katz and Meyer 1990, Card and Levine 2000) and some European ones (van Ours and Vodopivec 2006, Lalive et al. 2006).

Our results point to a significant heterogeneous impact across pre-unemployment income levels, which we associate with an important income effect. Indeed, the extension of the entitlement period seems to prolong unemployment spells but its effect is generally decreasing with the quintiles of the income distribution, with the exception of the first income quintile. Also, as predicted by the job search model, the impact increases over the distribution of subsidized unemployment. The evidence of the regression duration models points towards a larger impact of the entitlement extension for unemployed in the second and third quintiles (with an impact at median duration of 128 days). Interestingly, the impact for those in the bottom (1st quintile) and upper (4th and 5th) income quintiles is lower (close to 90 days). Whereas this result is expected in terms of the income effect for the upper quintiles, the result for the bottom quintile may reflect the mitigated adjustment in reservation wages, especially at longer durations, which follows from the nonstationary job search model.

We are also able to test what type of shifts were imposed on the distribution of subsidized unemployment spells by the new law. The hypothesis testing suggests that the July 1999 extension of the entitlement period resulted in longer spells of subsidized unemployment (a location shift) and also in larger variance (a scale shift). These impacts are the ones predicted by economic theory: more generous unemployment benefits result in longer unemployment spells (larger mean) and extensions of entitlement periods tend to have larger impacts at longer durations (larger dispersion).

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

# 2 Literature: Theory and empirical evidence

## 2.1 Theory

Program administrators face important trade-offs when setting up an (optimal) UI system. The trade-offs can be seen to happen between the undesired distortion to job search intensity caused by the provision of benefits, and the possible positive impact on post-unemployment outcomes arising from more generous benefits, particularly in terms of increased match quality, as in Marimon and Zilibotti (1999) and Acemoglu and Shimer (2000).

From a theoretical point of view, most results can be derived from the standard Mortensen (1986) nonstationary job search model. The simple result of observing longer unemployment spells as a response to increased UI generosity (usually interpreted as a distortionary substitution effect) does not preclude the existence of a non-distortionary income effect for agents who face liquidity constraints. The income effect introduces heterogeneity in the UI impact on duration for constrained and unconstrained individuals. In the presence of the income effect, the search behavior of constrained individuals reacts more strongly to changes in UI generosity. If the income effect is important, it can mitigate the disincentive created through the substitution effect, and the total effect of UI becomes less distortionary than previously thought.

To add intuition for these outcomes, we first think of the workers' liquidity constraints as in Mortensen (1986). The liquidity constraint is introduced in the model with the assumption that the worker is able to self-finance the search costs only for a finite time. This implies that constrained workers find it more difficult to smooth consumption over labor market states. In this setting, for constrained workers, UI might create an income effect that occurs in addition to and independently of the usual substitution effect. When a constrained worker relies on UI benefits to maintain consumption, increasing the benefit generosity would reduce the pressure to find a job in order to smooth consumption. On the contrary, if workers are unconstrained, the income effect channel is less relevant, since UI benefits would be a small portion of lifetime income/wealth. In the context of a job search model with a finite benefit entitlement period, these effects are mediated through the unemployed decision variables: the search intensity and the reservation wage. This generates the simple nonstationary environment of Mortensen (1986), further extended in van den Berg (1990). In such models, every exogenous variable, namely, the UI level, the arrival rate of job offers and the wage offers distribution, can cause nonstationarity because its value is dependent on the unemployment duration. At the beginning of the unemployment spell, the search intensity is low (and the reservation wage is high) because the probability of finding a job before UI expires is large. As unemployment duration increases the two decision variables move in opposite direction.

Furthermore, an increase in the entitlement period induces only small disincentive effects early in the unemployment spell because the entitlement extension does not strongly affect the risk of running out of benefits. However, when unemployment duration approaches the limit prevailing before the reform, the difference in search intensities before and after reaches its maximum. However, in a nonstationarity environment the other exogenous variables are also a function of time and their value is dependent on unemployment duration. Indeed, not only the level of UI varies over time in most countries, but, as shown in Addison, Centeno and Portugal (2004), the two remaining exogenous variables present a significant duration dependence in most European countries. The arrival rate of job offers declines over the unemployment spell, and the wage offer distribution also deteriorates as unemployment progresses. This reduction is likely to be stronger for those at the bottom of the income distribution. They face poorer labor market prospects and, thus, will have a more limited ability to adjust their strategy to the increased generosity.

As a result of these two opposite effects, the overall impact of extending the entitlement period remains an empirical question. If the income effect prevails, raising duration significantly for the more constrained, we can expect a larger shift in the distribution for constrained workers, as illustrated by the curves $C^{IE}$ and $U^{IE}$ in Figure 1. However, if nonstationarity plays an important role and the adjustment in the reservation wage is mitigated by either a declining arrival rate of job offers and/or a wage offer distribution that deteriorates with time, we might observe a weaker reaction by constrained unemployed. Graphically, this effect would cause a downward shift in curve to $C^{IE'}$.

FIGURE 1

## 2.2 Previous empirical evidence

There is a large empirical literature estimating the effects of UI on labor supply, starting with the seminal studies by Ehrenberg and Oaxaca in the 1970's. Nickell (1979) and Lancaster (1979) showed that higher benefits are associated with longer unemployment spells, and these findings were followed by a wealth of new results that showed how this effect operates, with due attention paid to other aspects of the UI system. The papers by Meyer (1990) and Katz and Meyer (1990) were the first to show that the hazard from unemployment is highly affected by the approximation of the UI exhaustion date, pointing to the effect of UI on a decreasing reservation wage. Most studies on the US labor market rest on differences in UI legislation across states to identify the impact of UI generosity. Two exception are the paper by Card and Levine (2000) and the more recent study by Meyer and Mok (2007) that explores a quasi-experimental setting generated by UI reforms. Both find a fall in the hazard of leaving UI that coincides with the increase in benefits.

Recently, several studies have explored quasi-experimental settings generated by reforms in European countries' regulations and apply new developments in the treatment effects literature. However, most of the existing literature on labor supply disincentives of UI programs has assumed homogeneous responses, as in van Ours and Vodopivec (2006) and Lalive et al. (2006). Quantile regression techniques are applied by Kyyra and Wilke (2007) to the study of a UI reform in Finland and by Fitzenberger and Wilke (2007) to the characterization of unemployment duration in Germany. All these studies show that unemployed workers have larger exit rates from unemployment the less generous the UI system is. These papers also present evidence of an increasing exit rate of unemployment as UI approaches the expiration date.

The evidence on heterogeneity of UI impact is more scant. Gruber (1997) and Browning and Crossley (2001) show evidence that more liquidity constrained individuals benefit the most from UI generosity in terms of consumption changes in the unemployment state. These results are related with those in Chetty (2005), which suggest that UI raises durations primarily because of an income effect, induced by the inability to save, rather than by moral hazard motives resulting from distorted incentives. Chetty (2005) analyzes a sample of American households divided into groups of liquidity constrained and unconstrained agents. He finds that unemployment benefits generosity has a large effect on unemployment spells of the former group, but only a small effect on the latter group. Furthermore, severance payments awarded to constrained households strongly increase subsequent unemployment spells. Centeno and Novo (2006) show strong heterogeneous impact of UI generosity on post-unemployment tenure for the US labor market. With the exception of Browning and Crossley (2001), who focus on the Canadian UI system, the other studies use the variation of UI generosity across US states to identify its impact on different outcomes.

# 3 Methodology

In the context of a nonstationary job search model, we expect UI to increase the length of unemployment spells by raising the reservation wage. Also, for extensions of the entitlement period, theory predicts a larger impact around the previous entitlement period limit. If this is the case, then the predominant effect of extension should be felt in the upper part of the distribution of unemployment durations. In other words, we expect differentiated impacts at different locations of the distribution, which can be estimated with quantile regression.

## 3.1 Quantile regression

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

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

Furthermore, quantile regression is very well suited for the specific duration-related questions arising in the context of nonstationary job search models described in van den Berg (1990) and that we would like to address in this paper. Quantile regression overcomes the two main limitations of mean regression-type models for the study of duration data, namely the need to assume a parametric form for the duration distribution, and the fact that only the conditional mean depends on the covariates. Indeed, Chaudhuri, Doksum and Samarov (1997) argue that quantile regression is a unifying concept for a plethora of duration models, such as the proportional hazards and accelerated failure time models. Recent applications of quantile regression to duration models can be found in Koenker and Bilias (2001), Machado and Portugal (2002), Centeno and Novo (2006), Fitzenberger and Wilke (2007) and Kyyra and Wilke (2007).

## 3.2 Quantile treatment effects

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

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

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

The connection between quantile treatment responses and quantile regression is obvious from the work of Doksum (1974). Doksum defines $\Delta(y)$ as the "horizontal distance" between the cumulative distributions $F$ and $G$ measured at $y$ so that $F(y) = G(y + \Delta(y))$. Then,

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

$\Delta(y) = G^{-1}(F(y)) - y$. Thus, changing notation, $\tau = F(y)$, to conform with the quantile regression notation introduced above, we can define the Quantile Treatment Effect (QTE) as:

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

In the two-sample case, the QTE is simply estimated by the sample analogs of equation (1), namely,

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

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

The identification hypotheses of the average treatment effect on the treated and the QTE are similar, in that both arise from the fundamental problem of causal inference – the non-observation of the counterfactual. Thus, the analogous identification hypothesis in QTE is that the distribution of potential outcomes in the absence of the treatment ($y_0$) for treated ($D = 1$), $G_{y0|D=1}$, would be the same as that of the control units, $F_{y0|D=0}$. 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,

$$G^{-1}_{y0(t')|D=1}(\tau) - G^{-1}_{y0(t)|D=1}(\tau) = F^{-1}_{y0(t')|D=0}(\tau) - F^{-1}_{y0(t)|D=0}(\tau), \quad \forall\tau. \tag{2}$$

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

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

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

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

### 3.3 Quantile regression inference on distributional shifts

The work of Koenker and Xiao (2002) on statistical inference for the entire quantile regression process offers extremely attractive tools in the present context. It allows for testing two ways in which two distributions may differ, namely, by a *location* shift and by a *location* and *scale* shift. The description of the QTE has already justified the importance of testing for such shifts. Anticipating a little what we will do in the empirical section, a simple regression of (log) duration on a constant and the UI generosity indicator variable together with the inference framework allow us to test the hypothesis that the distribution under a "more generous UI", $G$, differs from the distribution arising in a "less generous UI", $F$, either by a pure location shift

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

or by a location-scale shift

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

where $F^{-1}$ and $G^{-1}$ are as above. In other words, equation (4) tells us that all $\tau$-th quantiles of $F$ and $G$ differ by a constant, $\delta_0$; a pure location change model, which corresponds to the classical homoskedastic linear regression model. On the other hand, equation (5) transforms all $\tau$-th quantiles of $F$ into the respective $\tau$-th quantiles of $G$ by an affine transformation – a location change, $\delta_0$, and a scale change $\delta_1$.

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

# 4 Unemployment insurance system reform and the economy

## 4.1 The extension of some entitlement periods

The Portuguese UI legislation establishes only one eligibility criterion based on recent employment history with social contributions, requiring a minimum of 540 days of contributions in the 24 months before unemployment. Benefits are then set as a percentage of the monthly average of the previous wage. Figure 2 illustrates graphically the financial generosity of the system expressed in terms of the gross replacement rate (GRR).

[FIGURE 2]

Our analysis will focus on the unemployed with GRRs of 65 percent, which translates roughly into average monthly earnings ranging from 1.5 to 4.5 minimum wages.[2] This choice, while still allowing for substantial wage variability, aims at guaranteeing a similar impact of the substitution effect of UI, therefore eliminating a possible source of differentiated behavior among individuals.[3]

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

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

[2] In the data, some ratios of benefits to previous wages are not exactly equal to 65 percent. Therefore, we keep observations with GRR $\in [63, 67]$.

[3] Fitzenberger and Wilke (2007) reports evidence of the large disincentive effects on labor supply arising from high replacement rates.

[TABLE 1]

The characteristics of the reform result in two natural pairs of treatment and control groups, namely, $([15, 24], [25, 29])$ and $([30, 34], [35, 39])$. One of the main advantages of these comparison pairs, beside their proximity in terms of age, is the fact that after the reform they share exactly the same entitlement period. To further guarantee the comparability between treatment and control, we chose the latter pair. Indeed, for the younger cohort the results are likely to be contaminated by factors other than labor market attachment (e.g. education choices), making the treatment and control groups less comparable. On the other hand, the $[30, 34]$ treatment group is likely to share similar labor market characteristcs with the $[35, 39]$ control group, for instance in terms of schooling, marital status and child-bearing decisions, among others. In our case, this ex-ante comparability gains additional importance as a result of the limited information on workers characteristics available in the data set.

## 4.2 Economic conditions

At the moment of the reform, the Portuguese labor market and the economy were buoyant (see Table 2). In the period just prior the reform, real GDP growth was above 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 situation.

[TABLE 2]

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

It is worth noting that the good economic conditions prevailing at the moment of the reform are favorable for our empirical strategy. Indeed, they suggest that the policy change was not driven by the evolution of the labor market. Furthermore, the groups studied, prime-age workers, usually suffer less with labor market swings and do not face the type of retirement decisions common to older workers. This makes our comparison of pre- and post-reform outcomes more convincing, as it is not driven by a specific trend in the labor market or to questions related with population ageing.

## 5 Data

Our study is based on administrative data collected by the Portuguese government's agency *Instituto de Informática e Estatística da Segurança Social* (IIESS). The dataset recorded *all* unemployment-related social transfers that took place between 1998 and 2004. It contains very detailed and reliable information on the type, amount and duration of benefits, the previous wage, and, where applicable, the first re-employment wage and starting date of the job. The socio-demographic variables available are limited to gender, age, nationality and place of residence. However, the availability of the previous wage allows us to partially overcome the problem set by the lack of more detailed individual characteristics. Table 3 contains descriptive summary statistics of the key variables.

[TABLE 3]

With the aforementioned restriction of GRRs to the interval $[63\%, 67\%]$ and considering only complete spells, we have a total of 40,982 subsidized unemployment spells. The treatment group comprises 23,226 observations, of which 3,145 are observed before July 1999. The control group has 3,631 observations in the before period and 14,125 in the following period. Given the limits imposed on the GRRs, the differences in the average values of real previous wages between treatment and control groups are minor. Also, since we use pre-unemployment wages as a proxy for liquidity constraints, it is important to emphasize that we use a 12-month average. We will return to this point when we analyze the constrained and unconstrained groups. Figure 3 plots the histogram of the length (in days) of the subsidized unemployment spells. Although these are administrative data, we still observe some heaping around whole months.[4] Furthermore, due to the maximum entitlement periods, the data show two large heaping points, namely, at 450 days, the before July 1999 limit and at 540 days, the new entitlement period. A simple difference-indifferences (D-inD) estimate gives us an impact on subsidized unemployment duration for the treated group of approximately 83 days (see Table 3, Panel A).

[4] The heaping phenomenon is typical of retrospective questions. Such issues and a methodology are discussed in Torelli and Trivellato (1993) in the context of job-search duration data.

[FIGURE 3]

We take a first look at the impact on subsidized unemployment survival rates, using Kaplan-Meyer estimates (Figure 4). The before-after difference between the two curves drawn for the treatment group suggests that the reform significantly increased the survival rates in unemployment. The same exercise for the control group shows almost no reduction in the survival rates. Using this difference to adjust for aggregate conditions, we compute a simple D-in-D estimator from these Kaplan-Meyer survival rates. The D-in-D estimates show a positive impact of the reform on subsidized unemployment duration of the treated group. In view of the wealth of previous empirical evidence, these results are nothing but expected. Notice, also, that, as predicted by theory for the case of an extension in the entitlement period, the impact is larger at longer durations (closer to the previous entitlement period limit).

[FIGURE 4]

# 6 Income effect: Causal inference evidence

In order to establish the heterogeneous impact of the increased generosity of the UI system and, in particular, to identify the income effect, we now explore our data in a different fashion. First, we describe the process of splitting the sample in order to generate variation in terms of the degrees of liquidity of the unemployed. Then, we use quantile regression tools that do not impose restrictive (homogenous) responses on the conditioning variables to capture the nonstationary nature of the duration process. We use these results to assess the financial costs of the reform. Finally, using inference tools on the quantile regression process, we test for distributional shifts on the duration of subsidized unemployment spells induced by the new legislation.

## 6.1 Measuring liquidity constraints

To identify the income effect generated by the legislative reform, we divide the sample into subsamples reflecting as far as possible different degrees of liquidity constraints faced by unemployed workers. We do not have data on asset holdings for the Portuguese unemployed to directly measure their degree of liquidity constraints. Instead, we use a 12-month average of pre-unemployment wages as an indicator for the distribution of their constraints. This is a good proxy, given the distribution of savings in the Portuguese economy. In 2000, Farinha and Noorali (2004) show that the median level of financial assets held by individuals earning 500 euros or less per month represents 7.7 percent of the median level of financial assets held by Portuguese households. This value increases to 23.1 and 46.2 percent, respectively, for those earning (€500; €1,000] and (€1,000; €1,500]. The results are, if anything, more striking for unemployed workers. Thus, the average pre-unemployment income ends up being a good approximation to the degree of constraints faced in the event of a job loss. We choose to create 5 subsamples based on the pre-unemployment income quintiles. The real wages quintiles are reported in Table 3, Panel B.[5]

### 6.2 Quantile Treatment Effects

Despite the quasi-experimental setting of our analysis, there are possible confounding factors that can be controlled for with regression analysis and, in particular, with quantile regression. The appropriateness of quantile regression has been discussed at length in the methodological section, but still it is worth reiterating that our primary reason for using this method is to unveil potential heterogenous responses to changes in the entitlement generosity of the UI system over the unemployment duration distribution.

The quantile regression model used hypothesizes that the logarithm of subsidized unemployment days, $\log(T)$, has linear conditional quantile functions, $Q$, of the form:

$$Q_{\log(T)}(\tau) = \beta_0(\tau) + \beta_1(\tau)After + \beta_2(\tau)Treat + \beta_3(\tau)After \times Treat + x'\lambda(\tau), \tag{6}$$

where $After$ is an indicator variable for the post-July 1999 period, $Treat$ indicates the age group affected by the new legislation, and, therefore, the coefficient on $After \times Treat$ identifies the impact of the legislation. Additionally, the vector $x$ includes the following list of variables: (i) logarithm of the pre-unemployment income; logarithm of the individual's age at the beginning of the unemployment spell; a gender (female) indicator; regional (22 districts) dummies; and indicators of the month in which the unemployment spell started. This model is estimated for each of the 5 income-based subsamples.

The estimation results are presented in a concise format in Figure 5. Each column of

[5] Notice that the range of wages reported in Farinha and Noorali (2004) is in line with the values observed in our sample.

panels presents the quantile regression estimates for each of the 5 subsamples (from most to least constrained).[6] Each panel depicts the point estimates of the coefficient associated with the respective variable for each quantile. We chose to limit our attention to the quantiles $\tau \in [0.15, 0.70]$, ignoring, in practice, the very short duration (less than 2 months) and the longer durations (more than 470 days, around the previous entitlement period).[7] The shaded areas represent 90 percent confidence intervals.

[FIGURE 5]

Before discussing at length the impact of the reform, we touch upon some of the other variables included in the specification. We start with the logarithm of pre-unemployment wages (4th row). For the least and most constrained individuals, higher pre-unemployment wages are associated with longer unemployment spells. For the 2nd through 4th quintiles, the statistical impact is null, which might be explained by the smaller variability of pre-unemployment wages within each quintile. Despite the short range of ages considered, older individuals tend to spend a longer time unemployed. Finally, the last row of panels tells us that women spend longer periods unemployed, with the exception of the highest paid, who have spells comparable to men's.

We consider now the treatment impact. It is evident that the policy induced longer unemployment spells (positive point estimates shown in the 1st row of plots). Although different in the 5 subsamples, the policy impact is statistically significant, as all 90 percent confidence intervals lay short of zero. The most constrained reacted the least at all durations, although the impact increases over the unemployment spell (duration dependence). For the following two quintiles the impact is typically higher, with point estimates hovering 0.4. Finally, the top two quintiles have impacts larger than that observed for the most constrained, but lower than for the intermediate quintiles. To highlight the differences in the treatment effect across the degrees of liquidity constraints we aggregate the 2nd and 3rd in one group, and the top quintiles in another. Then, we recompute the impacts for these two sets of degrees of liquidity and present them together with the first quintile in Figure 6. The graph confirms the existence of two levels of heterogeneity, between degrees of liquidity constraints and within each group along the distribution of subsidized unemployment spells.

[6] To preserve space, we omitted from this plot the results on the month and region indicator variables.

[7] Despite the omitted quantiles in the plots, all observations are used in the estimation process.

[FIGURE 6]

First, notice that there is evidence of differentiated behavior between the subsamples of pre-unemployment income below and around the median – the group formed by the 2nd and 3rd quintiles – and above it – the top two quintiles. At all durations of unemployment, and in response to the same incentive, the impact on the more constrained is larger than for the least constrained. This conforms to the idea that there is an important income effect dimension to the UI system.

The second result worth highlighting in Figure 6 is the behavior of the 1st quintile. Two interesting features emerge. First, it has a smaller reaction to the increased generosity at all durations. However, it also has the steepest increase until the median duration. Both results can be explained in the context of the nonstationary job search model. These workers are the least able to anticipate the effect of a benefit extension, but given their degree of liquidity constraint, they should remain quite responsive as the unemployment spell progresses. This brings us to another key feature of the results.

For all the subsamples, it is possible to identify an increasing impact over the unemployment spell, which conforms with the theoretical prediction of more pronounced reactions at longer durations than early in the unemployment spells. However, towards the right tail of the distribution of durations, the impact is much flatter for all groups, except the unconstrained. The theoretical foundations for this result have been laid out earlier and rest on the nonstationarity of the job search process. They revolve around the idea that the materialization of the additional benefit is felt heterogenously at different levels of liquidity constraint over the unemployment spell.

Finally, we can look at the differences over time between the curves in Figure 6, in particular, between the top curves. At very short durations, there is a quite small difference between the two groups, but as unemployment duration increases the gap widens in a sustainable way up until the (conditional) median unemployment duration. Afterwards, the gap closes again rather rapidly and is almost inexistent around the 70th quantile. This behavior is also motivated by the nonstationary job search model. Early on the unconstrained are more able to adjust their behavior in anticipation of the increased generosity relative to the constrained unemployed. As the unemployment spell matures, the income effect becomes clearer, but, at later states, nonstationarity can be expected to gain relative importance and this explains both the flattening out of the constrained reaction and the stronger relative reaction of the unconstrained, less affected by the deterioration in the exogenous variables of the search model considered (namely, the arrival rate of job offers and the distribution of wage offers).

### 6.3 Estimating the impact in days and associated financial costs

Assessing the financial cost of the reform is of great economic interest. Ultimately, for the country's public finances, longer unemployment spells increase the financial burden of the system. In order to assess this impact, it is necessary to first express the impact in terms of additional days subsidized. This can be adequately done using the equivariance to monotone transformations of quantiles, $Q_{h(y)}(\tau) = h(Q_y(\tau))$, for non-decreasing functions $h$ in $\mathbb{R}$, which allows us to transform back into days the estimated impacts in log(days). Thus, the QTE estimator of equation (3) becomes

$$\delta(\tau) = \{h(G^{-1}_{y_1(t')|D=1}(\tau)) - h(G^{-1}_{y_0(t)|D=1}(\tau))\} - \{h(F^{-1}_{y_0(t')|D=0}(\tau)) - h(F^{-1}_{y_0(t)|D=0}(\tau))\}. \quad (7)$$

Given the model specification of equation (6), the QTE for quantile $\tau$ expressed in days is given by $\exp(\beta_0(\tau) + \bar{x}'\lambda(\tau))\{\exp(\beta_1(\tau) + \beta_2(\tau) + \beta_3(\tau)) - \exp(\beta_1(\tau)) - \exp(\beta_2(\tau)) + 1\}$. Figure 7 presents in days the QTE for the same quantiles shown before. The median duration increased by slightly over 90 days, close to the entitlement extension, for the bottom and top two quintiles, but by almost 130 days for the 2nd and 3rd quintiles. Again, two interesting results emerge from Figure 7. First, the ranking generated by these curves reproduces the one presented in Figure 6 and is evidence of the important income effect generated by the increased generosity. Secondly, the nonstationarity of the model is revealed by the behavior of the curves at longer durations. Indeed, not only do individuals at the bottom quintile react the least (only 27 days more between the 60th and 70th quantiles), but they also decouple from the other two curves. On the contrary, the unconstrained show the largest increase at long durations (a 43 days increase in the last decile plotted).

[FIGURE 7]

It is now possible to approximate the additional financial burden to the public UI system. To do that we first compute the average daily UI received by the unemployed, per unemployment duration and for each of the quintiles. Then, we multiply the daily UI by the QTE expressed in days. The results are summarized in Table 4. For the median duration, the financial impact is 1,014.45, 1,830.61 and 1,907.33 euros (in 1998), respectively, for the bottom, 2nd-3rd and top two quintiles. This represents a substantial increase in cost for the system, which expressed in terms of the average UI paid to the unemployed in the 1st quintile represents, respectively, 45.7, 82.4 and 85.9 percent. Not surprising, Table 4 also reveals that most of the financial resources additionally spent by the public system were directed to the unemployed in the top two quintiles.

[TABLE 4]

### 6.4 Robustness: Anticipation effects and an alternative ***after*** period

We now check the robustness of our results to different definitions of the sample. In particular, we will consider the following situations that may have biased the estimates or hidden idiosyncratic behaviors:

i) As with all pre-announced legislative reforms, there is the possibility of anticipation effects (Ashenfelter's dip). To address this issue, we excluded from the sample all individuals that claimed benefits during the time window of 6 months centered around July 1999. This excludes individuals who claimed benefits in the final 3 months under the previous law, and may have exited earlier to re-enter the system afterwards. Those that claimed in the first 3 months of the new law were also excluded because they may have been waiting (self-selecting into) the more generous system.

ii) We also consider an alternative, shorter, after period, namely, the period covering July, 1999 to December, 2000. Both the before (January 1998 to June 1999) and the after period span $1\,{}^1\!/_2$ years.

The results in the top panel of Figure 8 show a remarkable similarity with the results discussed hitherto, both in percentual terms (log days), the left panel, and in days, the right panel. This suggests that there were no anticipation effects.

[FIGURE 8]

The results with a shorter after period preserve the ranking, but the bottom quintile and top two quintiles are now slightly more apart. For instance, the quantile treatment effect for the median (conditional) duration is now slightly below 90 days for the bottom quintile and around 100 days for the top two income quintiles.

Overall, our results are robust to the sampling definitions.

### 6.5 Distributional shifts: Location and Location-Scale

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

Table 5 reports test statistics for the distributional shifts. In the upper panel, the contribution of each variable to the distributional shift is tested. The lower panel reports the statistics for the joint hypothesis. The latter reveals that the distribution shift of log durations imposed by the entire set of covariates does not conform to either of the null hypotheses, that is, all null hypotheses are rejected both for the full sample and for all the subsamples analyzed. It is, however, possible that individually a covariate induces distributional shifts of the type being tested. For the current exercise, we focus our attention on the variable identifying the quantile treatment effect, $After \times Treat$, which holds the most interest. For the full sample, both hypotheses are rejected. However, the location and scale hypothesis is only marginally rejected at the 10 percent level, contrarily to the location hypothesis that is unequivocally rejected. Turning to the subsamples, the analysis reveals that the change in (log) unemployment durations for the most constrained unemployed conforms to the location shift hypothesis. That is, the (log) durations shift to the right, but the dispersion of durations did not increase. On the other hand, the middle group (2nd and 3rd quintiles) have their (log) durations affected by the policy in a location and scale shift fashion, resulting in longer and more disperse durations. Finally, for individuals in the top two quintiles, the tests slightly favor the location and scale shift.

[TABLE 5]

In conclusion, the July 1999 extension to the entitlement period resulted in longer spells of subsidized unemployment (location shift) and also in larger variance (scale shift) for the upper

4 quintiles of pre-unemployment income distribution. Only the most constrained increased homogenously (location) their subsidized unemployment spells. Overall, these impacts are the ones predicted by economic theory: more generous unemployment benefits results in longer unemployment spells and extensions of entitlement periods tend to have larger impacts at longer durations (larger dispersion).

# 7 Conclusions

This paper addresses the question of how the generosity of the entitlement period affects the duration of subsidized unemployment. The agenda for unemployment insurance reform points, without exception, towards a significant reduction of its generosity in order to limit moral hazard problems, which ultimately lead to longer unemployment spells. However, the non-distortionary income effect of UI has been neglected. This income effect generates significant heterogeneneity in the UI impact over the income distribution, associated with differences in the degree of liquidity constraints faced by workers. We stress that these effects operate in a nonstationary job search environment, which ultimately strongly influences the observed behavior of quite constrained individuals.

We have relied for our anaysis on a reform of the Portuguese UI system introduced in July 1999. This reform extended significantly the entitlement periods for some age groups of the population, while maintaining the same benefit limit for other (adjacent) age groups. This generated a quasi-experimental setting that has allowed us to use standard program evaluation methodologies. The treatment group is composed of individuals in the age group that benefited from the extension (30-34 years old, from 15 to 18 months) and the control group by individuals aged 35-39 years, whose entitlement remained constant (exactly at 18 months). Furthermore, the reform was not endogenously motivated by labor market conditions. Indeed, it is implemented in a period of strong economic growth and favorable labor market conditions, which contribute to the exogeneity and quality of the experiment.

We present evidence of a heterogeneous impact on the duration of subsidized unemployment. The results point towards the existence of an important income effect, identified by a stronger reaction to generosity of individuals below (and slightly above) the median income distribution. The exception to this result are the individuals in the bottom income quintile, a result we associate with the nonstationarity of the job search process. These results point to the importance of setting the entitlement period as function of pre-unemployment income, as it is already the case with the financial generosity of the UI system.

We focus our study on prime-aged unemployed, those whose labor market decisions are least affected by non-market phenomena (e.g. schooling and retirement decisions). If this can be viewed as a good setting to identify the income effect, it also means that we have to be careful in terms of the external validity of the results. Indeed, governments are often tempted to increase generosity of the UI system in good economic times and, as we showed, this resulted in a substantial burden to the Portuguese UI public system. This kind of change is then difficult to undo, when budgetary pressures appear in recessions. In the light of this, there is scope for an important research topic - to evaluate the empirical relevance of the income effect on younger and older workers.

A final note for another research avenue is worth a mention here: an evaluation of how 'productive' these additional search periods were, that is, what impact did they have on job match quality as proxied by post-unemployment outcomes.

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

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

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

| | Treatment | | Control | |
|---|---|---|---|---|
| | Before | After | Before | After |
| Panel A | | | | |
| Spell duration (in months) | 210.58 | 291.16 | 321.95 | 319.68 |
| Differences | 80.57 | | -2.27 | |
| D-in-D | 82.84 | | | |
| Panel B | | | | |
| Age | 31.88 | | 36.94 | |
| Females | 0.34 | | 0.35 | |
| Previous real wages$^{(1)}$ | | | | |
| Average | 699.81 | | 729.99 | |
| Minimum | 353.10 | | 350.10 | |
| 20th percentile | 511.94 | | 521.70 | |
| 40th percentile | 582.70 | | 591.60 | |
| 60th percentile | 683.05 | | 707.75 | |
| 80th percentile | 860.59 | | 929.99 | |
| Maximum | 1,490.89 | | 1,561.98 | |
| No. of observations | 3,145 | 20,081 | 3,631 | 14,125 |

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

Table 4: The financial impact on the UI system

| | Pre-unemployment income quintiles | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| | 1st | | | 2nd and 3rd | | | 4th and 5th | | |
| $\tau$ | Daily UI$^{(1)}$ | $\Delta$ UI$^{(2)}$ | In %$^{(3)}$ | Daily UI$^{(1)}$ | $\Delta$ UI$^{(2)}$ | In %$^{(3)}$ | Daily UI$^{(1)}$ | $\Delta$ UI$^{(2)}$ | In %$^{(3)}$ |
| 0.15 | 10.77 | 104.03 | 4.7 | 13.47 | 378.99 | 17.1 | 21.84 | 426.61 | 19.2 |
| 0.30 | 11.09 | 361.88 | 16.3 | 13.92 | 877.11 | 39.5 | 25.71 | 939.13 | 42.3 |
| 0.40 | 11.61 | 618.18 | 27.8 | 13.44 | 1,355.01 | 61.0 | 21.64 | 1,482.62 | 66.8 |
| 0.50 | 10.91 | 1,014.45 | 45.7 | 14.28 | 1,830.61 | 82.4 | 20.93 | 1,907.33 | 85.9 |
| 0.60 | 11.30 | 1,155.08 | 52.0 | 13.89 | 2,195.73 | 98.9 | 19.27 | 2,398.19 | 108.0 |
| 0.70 | 11.18 | 1,449.60 | 65.3 | 13.36 | 2,413.73 | 108.7 | 22.70 | 3,792.27 | 170.8 |

Notes: As before, $\tau$ stands for (estimated) quantile; (1) Daily UI is computed as the average daily UI paid to individuals in the $\tau$-th duration quantile in the age group $[30 - 34]$ during the before period; (2) The $\Delta$ UI is the product of the daily UI by the $\tau$-th QTE expressed in days; (3) The percentage impact is given by the ratio of $\Delta$ UI to the average benefits paid in the 1st quintile in the before period.

Table 5: Quantile regression process: Location shift and location-scale shift test statistics

| | Full sample | | Subsamples by quintile of pre-unemployment wages | | | | | |
|---|---|---|---|---|---|---|---|---|
| | | | 1st | | 2nd & 3rd | | 4th & 5th | |
| Individual hypothesis | L | LS | L | LS | L | LS | L | LS |
| After × Treat | 8.958 | 2.627 | 1.175 | 3.457 | 1.685 | 0.793 | 1.507 | 0.772 |
| After | 1.321 | 7.198 | 4.052 | 0.919 | 11.257 | 1.042 | 5.057 | 3.085 |
| Treat | 10.253 | 4.322 | 1.525 | 4.542 | 3.882 | 2.046 | 1.355 | 1.512 |
| log(Wage) | 22.123 | 10.545 | 4.966 | 3.772 | 9.625 | 2.982 | 19.358 | 2.450 |
| log(Age) | 12.561 | 3.095 | 1.283 | 2.329 | 13.622 | 1.363 | 17.702 | 2.684 |
| Female | 26.766 | 1.312 | 4.287 | 0.936 | 13.855 | 1.910 | 13.912 | 3.287 |
| Joint hypothesis | L | LS | L | LS | L | LS | L | LS |
| Statistic | 547.94 | 175.09 | 271.57 | 115.88 | 499.78 | 258.29 | 1,698.63 | 549.61 |

Notes: (1) 'L' and 'LS' stand for the null hypotheses of a location shift and a location-scale shift, respectively. (2) The individual test statistic critical values are 2.420, 1.923 and 1.664 at the 1, 5 and 10 percent levels, respectively. The critical values for the joint hypothesis are 20.14, 18.30 and 17.38 for the same levels. (3) The regional and seasonal indicator variables were included in the specification, but omitted here.

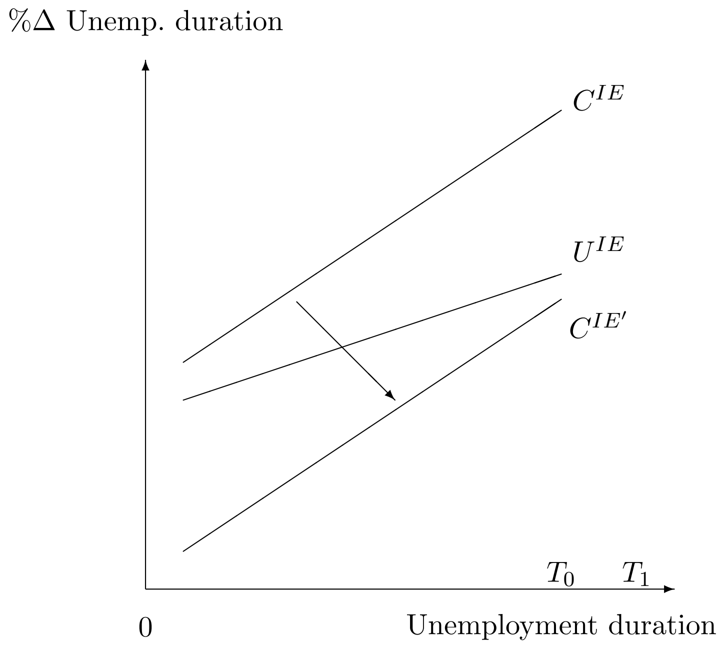


Figure 1: Illustration of the percentage change in unemployment duration following an increase in the benefit entitlement period. Constrained individuals' income effect, $C^{IE}$, is larger, at all durations, than unconstrained individuals', $U^{IE}$. However, the nonstationarity of the job search process may mitigate the overall impact, particularly, for constrained individuals, $C^{IE'}$.

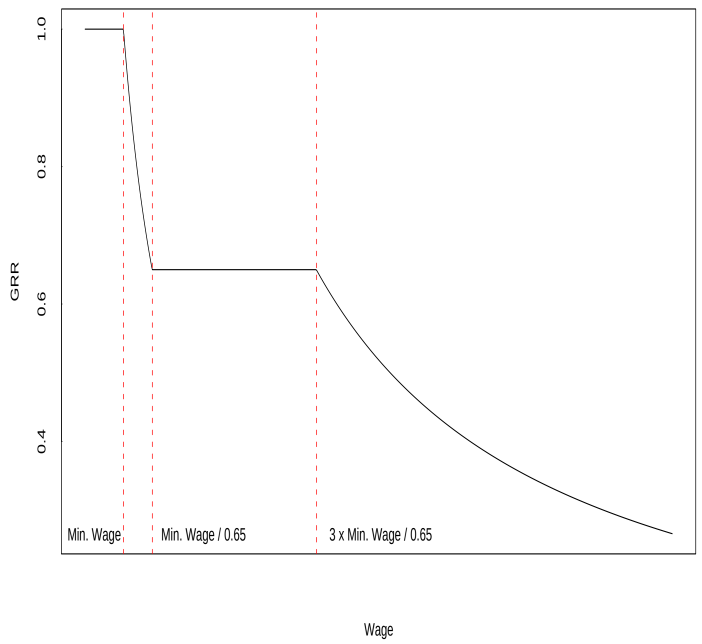


Figure 2: Financial generosity of the Portuguese UI system: Gross Replacement Rates (GRR)

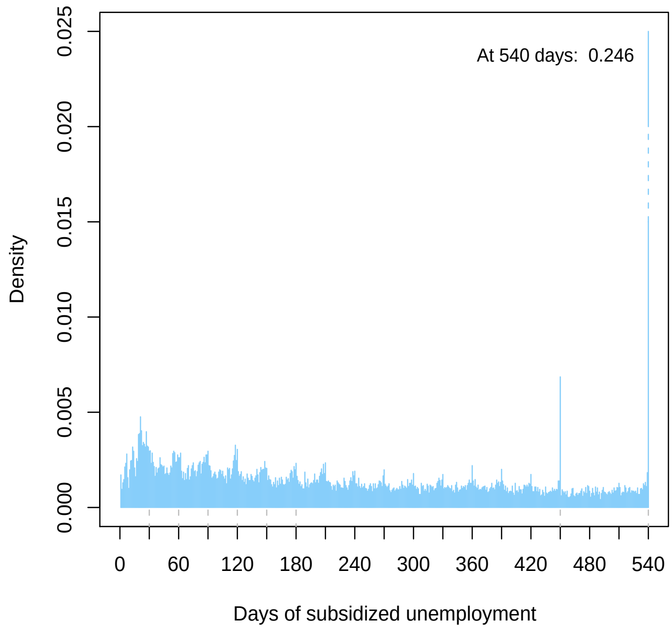


Figure 3: Histogram: Days of subsidized unemployment

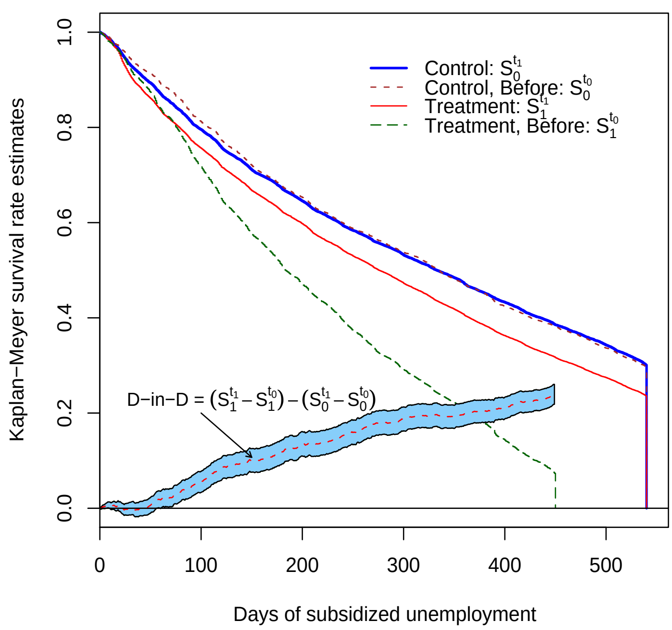


Figure 4: Kaplan-Meyer estimates: Survival rates and D-in-D treatment effect on survival rates

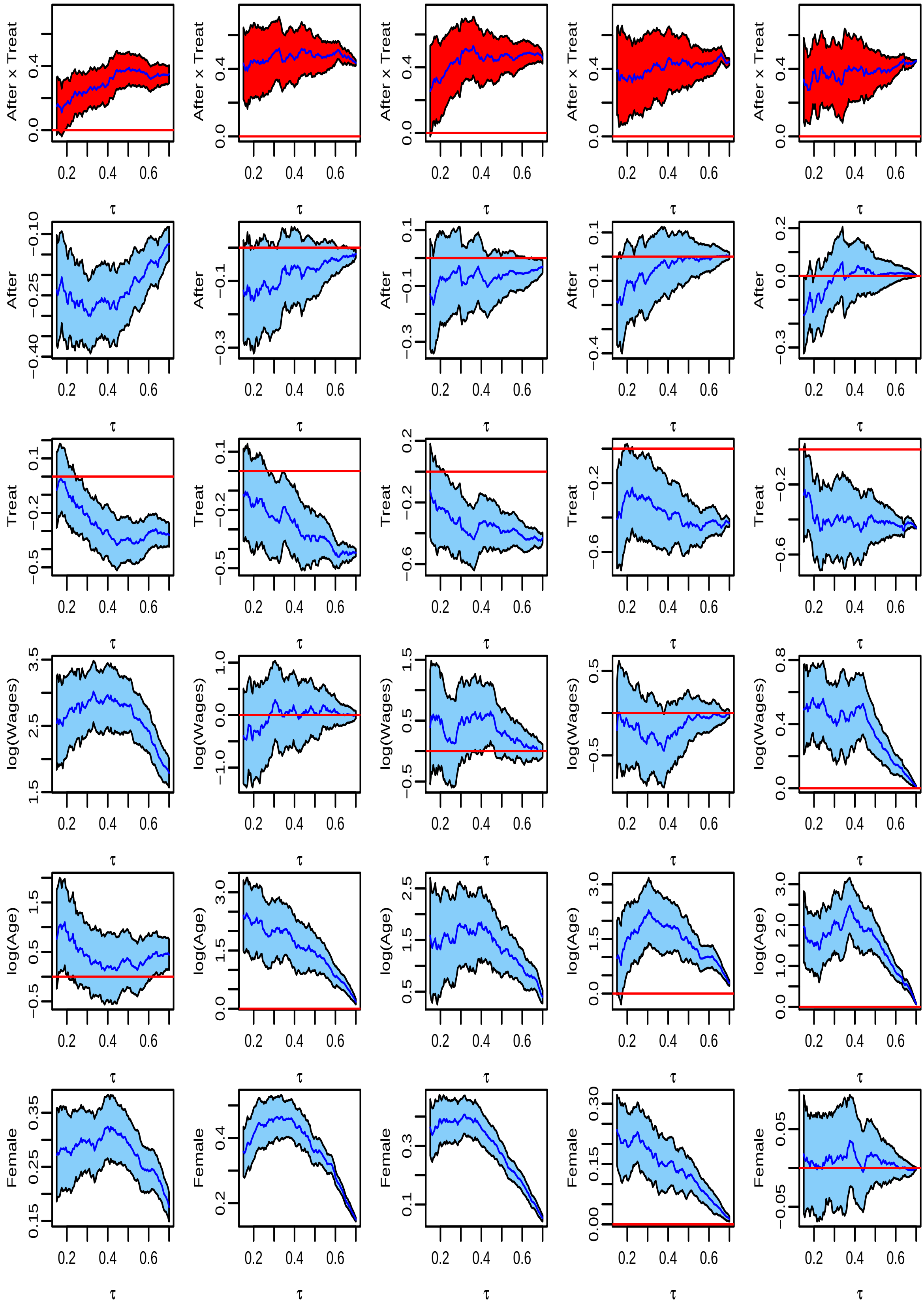


Figure 5: Quantile regression estimates: Log(duration) models by degree of liquidity constraints proxied by quintiles of pre-unemployment average income (1st quintile in the 1st column and so on)

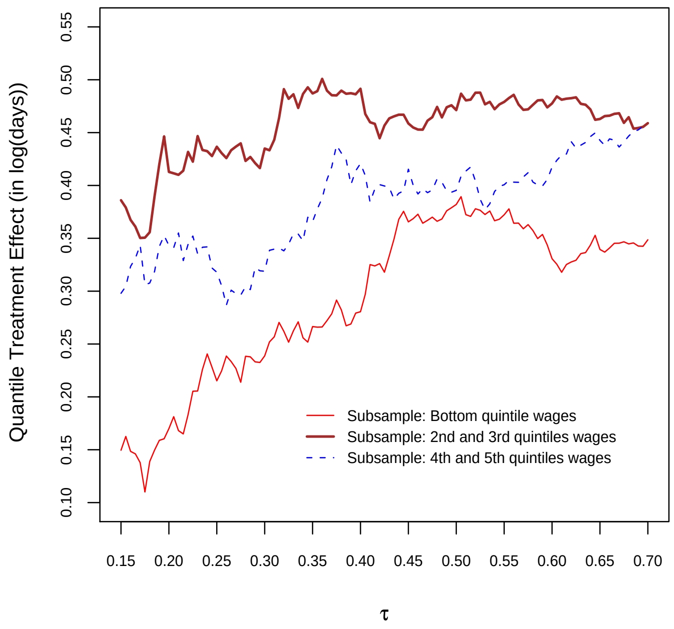


Figure 6: Quantile Treatment Effect estimates by degree of liquidity constraints proxied by quintiles of pre-unemployment average income

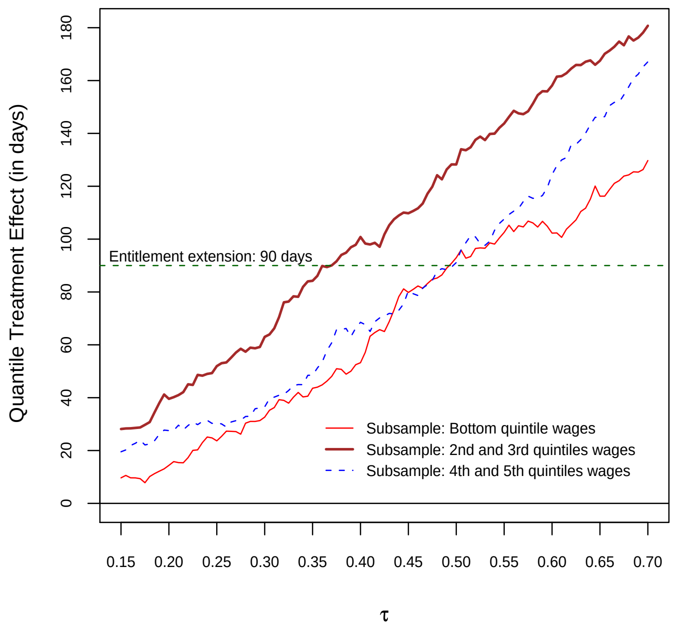


Figure 7: Quantile Treatment Effect estimates expressed in days by degree of liquidity constraints proxied by quintiles of pre-unemployment average income

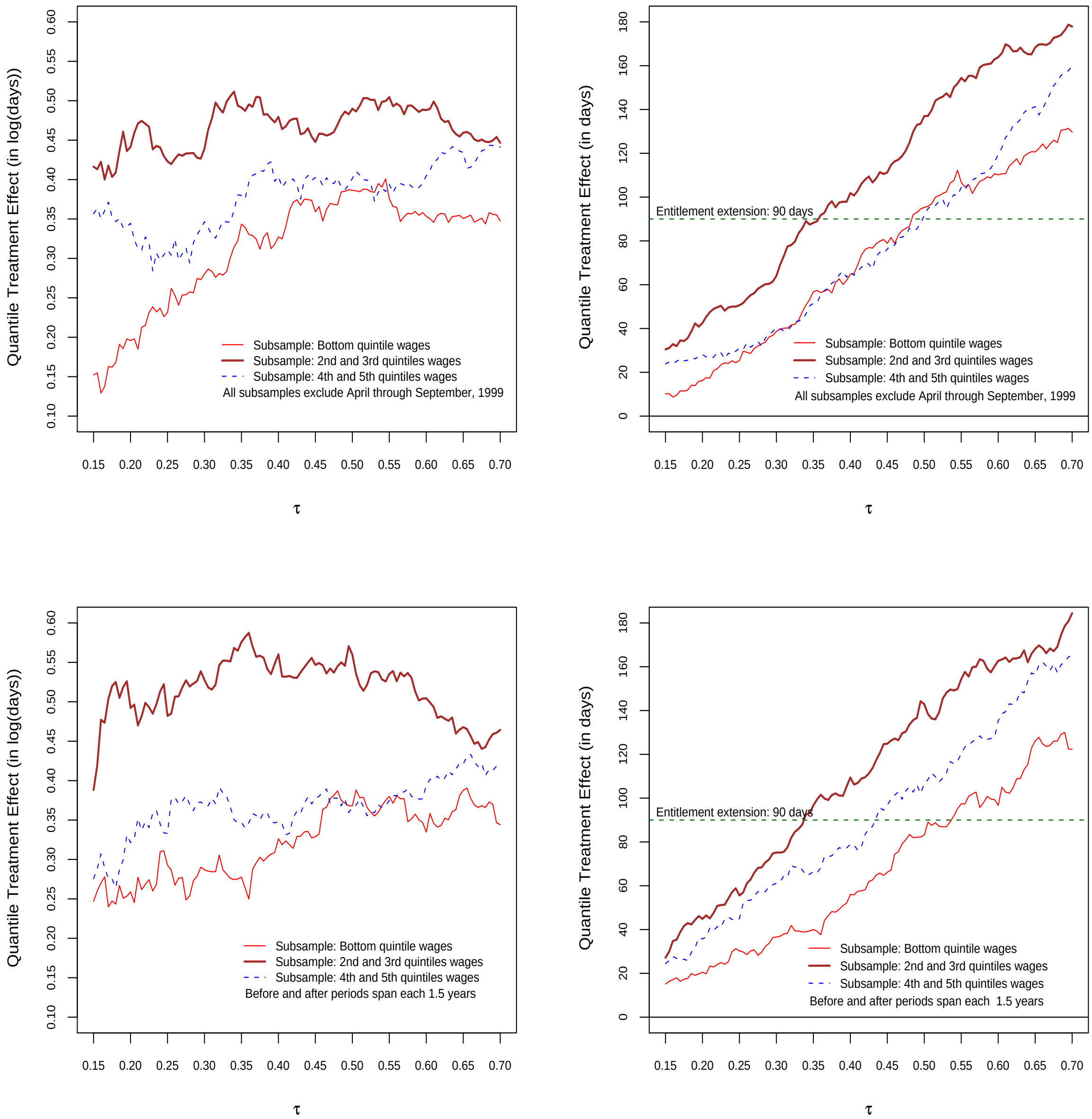


Figure 8: Quantile Treatment Effect estimates by degree of liquidity constraints proxied by quintiles of pre-unemployment average income computed for 2 subsamples: top panel excludes observations in a 6 month time window around July 1999, and the bottom panel restricts the after period to 1.5 years after July 1999