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WITH A QUASI-NATURAL EXPERIMENT

Mário Centeno

Álvaro Novo

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Please address correspondence to

Mário Centeno

Economics and Research Department

Banco de Portugal, Av. Almirante Reis no. 71, 1150-012 Lisboa, Portugal;

Tel.: 351 21 313 0837, Email: mcenteno@bportugal.pt

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

Mário Centeno

Álvaro A. Novo

Banco de Portugal & ISEG - U. Técnica

Banco de Portugal & ISEGI - U. Nova

mcenteno@bportugal.pt

anovo@bportugal.pt

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Abstract

This paper acknowledges that UI has a non-distortionary income effect generated by easing 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. The extension of the entitlement period prolongs unemployment spells, but its effect is decreasing with the degree of liquidity constraints (indexed by wages quintiles). An exception to this pattern is the behavior of individuals in the first wages quintile. The fact that the most constrained individuals extend the least their unemployment spells conforms to the nonstationarity of the job search process. This result points to the possibility that the UI system may become regressive, benefiting significantly less those at the bottom of the wage distribution, who find it harder to benefit from extended UI entitlements.

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. In Krueger and Meyer (2002) review, 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. However, UI can also have an income effect that varies with the degree of liquidity constraints faced by the unemployed, generating an heterogeneous impact on unemployment duration. In light of the nonstationary job search theory (van den Berg 1990), we study the impact on subsidized unemployment duration of an extension to the benefits entitlement period of the Portuguese UI system. Introduced at a time when the overall 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 (see Meyer 1995). To evaluate the heterogeneous impact of UI over the distribution of unemployment duration, we use the quantile treatment effects methodology (see Koenker 2005).

The UI literature has emphasized 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, as recently emphasized by Chetty (2005), the total effect of UI on unemployment duration is the sum of this distortionary substitution effect and a non-distortionary income effect. The latter is the result of the agents' liquidity constraints, as in Mortensen (1986) and van den Berg (1990), where constrained individuals are able to self-finance their job search costs only for a finite period of time (up to period T). In a model with UI, the benefits represent the only source of income while unemployed for constrained individuals (those with very low levels of financial wealth), and the finite entitlement period establishes a direct link with T . The impact of more generous UI via the income effect operates through the marginal utility of wealth: more constrained individuals will react more to the generosity level of UI, as it represents a larger share of their lifetime wealth.

The nonstationary environment includes other exogenous variables, namely, the job offers arrival rate and the wage offers distribution, which are key to understand the role of non-stationarity in the duration of unemployment. The reservation wage of the unemployed, which

is derived under the assumption of perfect foresight and for a finite entitlement period, changes continuously over time. Indeed, in face of a declining UI level, and a deteriorating wage offers distribution and job offers arrival rate, the reservation wage will decline with elapsed duration of unemployment.

An important conceptual insight from the nonstationary job search theory is that the time dependence of the exogenous variables affects the entire distribution of unemployment durations in a nonuniform way. Indeed, the model predicts that the hazards out of unemployment are likely to change over time, but it remains an empirical question whether the hazard rate will increase or decrease; the deterioration of both the wage offers distribution and the job offers arrival rate may counteract the decline in the reservation wage. By the same token, the nonstationary environment turns also the identification of the income effect into an empirical question, since the time and duration dependence of the exogenous variables varies across individuals according to their degree of liquidity constraints.

The importance of the exogenous variables and their time dependence in the determination of unemployment duration has been well documented in the literature (see the recent survey by Eckstein and van den Berg (2007)). Changes in the search environment at the individual level have an important impact in the hazard rate of unemployment, and, as such, the individuals' optimal search strategy will incorporate this information. Wolpin (1987) identifies an important impact of changes in the job offers arrival rate on unemployment duration, and only minor impacts due to offered wages. In his model, calibrated for a male NLSY sample, increasing the weekly job offers arrival rate from 1 to 5 percent reduces unemployment duration by 60 percent. Additionally, Addison, Centeno and Portugal (2004) present evidence on the time dependence and heterogeneity of the job offers arrival rate and the wage offers distribution for a sample of European households. The results are particularly striking for the job offers arrival rate; an extra month of unemployment reduces the arrival rate by 13 percent. Their results show also a significant impact of age, education and pre-unemployment income on the job offers arrival rate, pointing out to the heterogeneous environment faced by individuals. Portugal is found to be one of the countries with the lowest and more heterogeneous job offers arrival rates.

Altogether, the heterogeneity introduced by the nonstationary environment translates into a pattern of adjustments in the reservation wage that turns the relative magnitude of the impact of increased generosity for individuals with different liquidity characteristics into an empirical question.

We explore a quasi-experimental setting, generated by an exogenous increase in UI generosity, to identify the causal effect on subsidized unemployment duration of an extension of the entitlement period. 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 we proxy by different levels of pre-unemployment wages. Secondly, for the same level of liquidity constraints, the impact of an UI extension may vary at distinct locations of the distribution of subsidized unemployment durations. To capture the wealth of nonuniform impacts on the distribution of unemployment duration, we use the quantile treatment effects methodology.

The exogenous variation in UI generosity was 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 unchanged 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. Also, as predicted by the job search model, the impact increases along the distribution of subsidized unemployment. These results are in line with previous evidence for the American and European labor markets (e.g. Katz and Meyer (1990), Card and Levine (2000), van Ours and Vodopivec (2006), and Lalive, van Ours and Zweimueller (2006)).

Furthermore, our results point to a significant heterogeneous impact across pre-unemployment wage levels, which we associate with the 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 wages distribution, with the exception of the first quintile. The evidence of 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) 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 for low-wage workers, especially at longer durations, which follows from the nonstationary job search environment.

A hypothesis testing following Koenker and Xiao (2002) 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 concluding remarks.

2 Literature: Theory and empirical evidence

2.1 Theory

Program administrators face important trade-offs when setting up an (optimal) UI system. For instance, they must strike a balance between the undesired distortion to job search intensity caused by the provision of benefits and the possible positive impacts on consumption smoothing for liquidity constrained individuals and on increased post-unemployment match quality, as in Marimon and Zilibotti (1999) and Acemoglu and Shimer (2000).

The main theoretical results that motivate the empirical exercise in this paper are derived from the nonstationary job search model in van den Berg (1990). 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 unemployment duration for constrained and unconstrained individuals. If the income effect is important, the total effect of UI becomes less distortionary than previously thought, a result recently emphasized in Chetty (2005).

To add intuition for these outcomes, we first think of the workers' liquidity constraints as a finite period of time where the worker is able to self-finance the job search costs. This implies that constrained workers find it more difficult to smooth consumption over labor market states, and for them, 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, increases in the benefit generosity would reduce the pressure to find a job. On the contrary, if the worker is unconstrained, the income effect channel is less relevant, since UI benefits would be a small portion of the lifetime wealth. Thus, in the event of increased UI generosity, the income effect would predict a larger increase in unemployment duration for constrained individuals than for unconstrained, as illustrated by the relative positions of curves C^{IE} and U^{IE} in Figure 1.

FIGURE 1

Notice that in Figure 1 the impact in unemployment duration is increasing. This also follows from the nonstationary job search. At the beginning of the unemployment spell, an extension of the entitlement period entails only small immediate disincentive effects for workers; most of the action occurs just before the benefit exhaustion in the old system. This is the case because extended benefits delay the spike in the unemployment exit rate that is characteristic of a system with time-limited UI benefits; Katz and Meyer (1990) and Lalive et al. (2006) present evidence of these effects.

In van den Berg (1990), the model exogenous variables, namely the arrival rate of job offers and the wage offers distribution, can cause nonstationarity if their values are dependent on unemployment duration. The literature on the nonstationary job search model, recently reviewed in Eckstein and van den Berg (2007), points out to the importance of these variables in shaping the unemployment duration distribution, through their impact on the reservation wage. The exogenous variables determine the search environment at the individual level and, as shown in Addison et al. (2004) for a sample of European households, this environment has a great deal of heterogeneity among the unemployed. In particular, their results show that low-wage, older and less educated workers have a lower job offers arrival rate. In turn, these individual characteristics are highly correlated with the existence of liquidity constraints. If more constrained individuals face a worse labor market environment, the model predicts that they will react less to the increased generosity. Thus, in this case, there will be an inverted U-

shape relationship between the UI impact on unemployment duration and the degree of liquidity constraints.

In a nutshell, in a nonstationary environment, the most constrained individuals may find it difficult to adjust their behavior to the increased generosity. As Cahuc and Zylberberg (2006) put it, although low-income individuals ought to be more responsive to increased benefits, they enjoy a narrower margin of maneuver, which may prevent them from taking full advantage of the additional benefits. Thus, the relative position of the two curves in Figure 1 – that identifies the income effect – becomes an empirical question.

2.2 Previous empirical evidence

There is a large body of empirical literature estimating the effects of UI on labor supply, starting with the seminal study by Ehrenberg and Oaxaca (1976). 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 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 exceptions are the papers by Card and Levine (2000) and Meyer and Mok (2007) that explore quasi-experimental settings generated by UI reforms. Both studies find a fall in the hazard of leaving UI that coincides with the increase in benefits.

Recently, several studies apply new developments in the treatment effects literature to explore quasi-experimental settings generated by reforms in European countries' regulations. However, most studies assume 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 in less generous UI systems. These papers also present evidence of an increasing exit rate from unemployment as UI approaches the expiration date.

The evidence on the 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. Chetty

(2005) shows that UI raises durations primarily because of an income effect, induced by the inability to save, rather than by moral hazard motives arising 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 constrained group, but only a small effect on the latter group. Furthermore, severance payments awarded to constrained households strongly increase subsequent unemployment spells.

3 Methodology

In the context of a nonstationary job search model, we expect an extension of the UI entitlement period to increase the length of unemployment spells in a nonuniform way, with a larger impact occurring around the previous entitlement period limits. 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 fully captured with quantile regression.

3.1 Quantile regression

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

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

Furthermore, quantile regression is very well suited for the specific duration-related questions arising in the context of the nonstationary job search model described in van den Berg (1990) and that we 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 Biliias (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 exerts a pure location shift on the distribution of the treated. The response may also be a function of the pre-treatment value, for example, $\Delta(y) = \delta_0 y$. While in the former case the two distributions have the same shape, but different locations, in the latter both the location and shape differ. In this case the literature refers to a location and scale shift.

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

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

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 ($D = 1$), $G_{y_0|D=1}$, would be the same as that of the control units, $F_{y_0|D=0}$. To control for time invariant differences between the treatment and control group, we extend the quantile treatment effect in the same fashion as the difference-in-differences literature. Thus, we need an additional identification hypothesis, namely,

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

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

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

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

In the 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 distributions may differ, namely, by a *location* shift and by a *location* and *scale* shift. This has a nice interpretation in the current theoretical setting. If the location hypothesis is accepted all duration shift equally. But, as if shown in Lalive et al. (2006) the impact is larger at longer durations, then the relevant hypothesis is the location-scale shift. Anticipating a little what we will do in the empirical section, a simple regression of unemployment 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

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

or by a location-scale shift

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

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

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

4 The UI reform and the economy

4.1 The extension of some entitlement periods

The Portuguese UI legislation established only one eligibility criterion, namely, a minimum of 18 months of social contributions in the 24 months before unemployment. Benefits are then set as a percentage of the 12-month average of the previous wages. Figure 2 illustrates the financial generosity of the system expressed in terms of the gross replacement rate (GRR).

[FIGURE 2]

One peculiar feature of the Portuguese system is the definition of the entitlement period, which is fully determined by the individual's age at the beginning of the unemployment spell. In July 1999, the entitlement period increased for some age groups in the population.

Before the reform, the Portuguese legislation divided workers into 8 age-groups with different entitlement periods. The reform made this period longer 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 increased to up to 38 months.

[TABLE 1]

The characteristics of the reform result in two natural pairs of treatment and control groups, namely, $([15, 24], [25, 29])$ and $([30, 34], [35, 39])$. 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 pair with older unemployed. Indeed, for the younger cohort the results are more likely to be contaminated by factors other than labor market attachment (e.g. educational and marital choices), making the treatment and control groups less comparable. On the contrary, the $[30, 34]$ treatment group is likely to share similar labor market characteristics with the $[35, 39]$ control group, for instance, in terms of schooling, marital status and child-bearing decisions. In our case, this ex-ante comparability gains additional importance because of the limited information on workers' characteristics available in the dataset.¹

¹The possibility of extending the analysis to older workers is hindered by two factors: (i) there are no obvious control groups, i.e., the entitlement periods changed for all older individuals; and (ii) the same legislative change

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 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 situation.

[TABLE 2]

The business cycle started to change only in the second half of 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 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 decrease 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 increase significantly public employment and expand social and labor market programs, such as the described UI reform and the introduction of a means-tested minimum income scheme. Second, the political cycle may also have played a role since there were scheduled elections for the second half of 1999.

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

introduced generous early retirement schemes for older unemployed, which severely confound the identification of the UI extension impact.

5 Data

Our study is based on administrative data collected by the Portuguese government’s agency *Instituto de Informática e Estatística da Segurança Social* (IESS). The dataset recorded *all subsidized* unemployment spells initiated between January 1, 1998 and June 30, 2003, which amount to 325,825 claims of which 83,436 observations corresponds to the age group [30,39]. From a statistical point of view, it is important to notice that we are able to follow the spells until they are terminated, either before or on the exhaustion date. The dataset contains very detailed and reliable information on the type, amount and duration of benefits and the previous wage. The socio-demographic variables available are limited to gender, age, nationality and place of residence. However, the availability of the previous wage allows us to partially overcome the problem posed by the lack of more detailed individual characteristics. Table 3 contains descriptive summary statistics of the key variables before the reform.

[TABLE 3]

Our analysis will focus on the unemployed with GRRs of 65 percent, which, as can be seen in Figure 2, translates roughly into average monthly earnings ranging from 1.5 to 4.5 minimum wages, that is, it focuses on subsidized unemployed aged 30 to 39 whose previous average wages fall between the 40th and 95th percentiles of the pre-unemployment wages distribution.² This choice, while still allowing for substantial wage variability guarantees a roughly constant fraction of UI on previous wages, eliminating therefore a possible source of differentiated behavior among individuals. Indeed, for Germany, Fitzenberger and Wilke (2007) report evidence of a large disincentive effect on labor supply attributable to high replacement rates associated with lower wages.

With the GRRs in the interval [63%, 67%], we have a final sample with 40,982 subsidized unemployment spells. The treatment group comprises 23,226 observations, of which 3,145 are from the period before July 1999. The control group has 3,631 observations in the before period and 14,125 in the after period. The differences in the 12-month average values of real previous wages between treatment and control groups are minor. Figure 3 plots the histogram of the length (in days) of the subsidized unemployment spells.

[FIGURE 3]

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

A simple difference-in-differences (D-in-D) estimate yields an impact on subsidized unemployment duration for the treated group of approximately 83 days (see Table 4). This estimate is larger than the typical impact reported in the literature (Lalive et al. 2006, van Ours and Vodopivec 2006). As pointed out in Card, Chetty and Weber (2007), these results might be sensitive to different measures of unemployment duration, namely, time to next job. Although we do not restrict our sample to such transitions, since most of the exits occur before the exhaustion date, they are likely to be job transitions. In the following sections, we limit our attention to quantiles in the range [15, 70], further mitigating the differences between unemployment duration measures.³

[TABLE 4]

Kaplan-Meyer survival rates estimates (Figure 4) confirm these results and illustrate the quality of the quasi-natural experiment. 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 results in virtually imperceptible differences in the survival rates, which reinforces our case for an exogenously driven reform. Using this difference to adjust for aggregate conditions, we compute a simple D-in-D estimator from these Kaplan-Meyer survival rates. The D-in-D estimates show a positive impact of the reform on subsidized unemployment duration of the treated group. In view of the wealth of previous empirical evidence, these results are nothing but expected. Notice that, as predicted by theory for the case of an extension in the entitlement period, the impact is larger at longer durations (closer to the previous entitlement period).

[FIGURE 4]

6 Income effect: Causal inference evidence

The model has two empirically testable predictions regarding the impact of UI generosity on unemployment duration: (i) an inverted U-shape relationship with the degree of liquidity constraints and (ii) an increasing impact with unemployment duration. To capture the first of these effects, we split the sample by degrees of liquidity constraints and to capture the second

³Centeno and Novo (2007) use time to next job to measure unemployment duration and obtain quantitatively similar results in terms of the impact of the reform on unemployment duration.

effect, we use quantile regression tools. An assessment of the financial costs of the reform is also provided.

6.1 Measuring liquidity constraints

The identification of the income effect rests on individual differences in the degrees of liquidity constraints. However, the ‘constrained’ status is a latent variable. Thus, it is not feasible to classify, directly from the data, individuals into distinct groups of liquidity constraints. The approach followed to identify these distinct groups was to split the sample according to the 12-month average of pre-unemployment wages, which serve as an index for the distribution of liquidity constraints. We resort to wages because our data lacks the information on asset holdings for the unemployed, a more direct measure of their degree of liquidity constraints.

The quality of pre-unemployment wages as an index for the distribution of savings in the Portuguese economy can be assessed with data from the Consumer Expenditures Survey (CES) for 2000. Table 5 shows information on financial assets holdings for the wage groups defined by the 1st quintile, the 2nd and 3rd quintiles, and the 4th and 5th quintiles for the full sample of unemployed aged 30 to 39 (in 2000 prices).⁴ For each of the 3 subsamples, which we will refer to as bottom, intermediate and top wages subsamples, the last two columns report the average level of financial assets held by each group, respectively, as (i) a percentage of the average level of financial assets for the CES sample aged [30, 39], and (ii) as a percentage of the median wage level of each group. The three groups differ clearly in terms of their financial assets holdings, suggesting that previous wages are a good index for the degree of constraint. For instance, the bottom wages group holds financial assets worth only 2.9 group-median wages, while the remaining groups hold assets worth 4.5 and 7.5 times the respective group-median wage.⁵

TABLE 5

6.2 Quantile treatment effects

The quality of the quasi-experimental setting was confirmed by the simple D-in-D analysis. However, there are possible confounding factors that can be controlled for with regression anal-

⁴The aggregation of the 2nd and 3rd quintiles and the 4th and 5th quintiles will be made clearer in the next subsection.

⁵In the context of our exercise, if the individuals are misallocated to a group in terms of their degree of liquidity constraints that would result in an underestimate of the total income effect associated with the constraint status.

ysis and, in particular, with quantile regression. The primary reason for using this method is to unveil potential heterogeneous responses to changes in the entitlement generosity of the UI system over the unemployment duration distribution, a result that follows from nonstationarity job search theory.

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

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

where *After* is an indicator variable for the after-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: logarithm of the pre-unemployment wages; 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.

The estimation results are presented in a concise format in Figure 5. Each column of panels presents the quantile regression estimates for each of the 3 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).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 (5th row). In all subsamples, wages are positively associated with longer unemployment spells.

⁶In order to have enough variation in the degree of liquidity constraints, we started by splitting the sample into quintiles of the wage distribution. However, as shown in Figure A.1 in the Appendix, the results for the 2nd quintile are similar to the ones for the 3rd quintile, and the same happens with the results for the 4th and 5th quintiles. Thus, in the remaining of the analysis, we present the results for three wage-based subsamples. To preserve space, we omitted from the plots the results on the month and region indicator variables.

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

However, the impact is decreasing with the degree of liquidity constraints (from bottom to top wages subsample). Also, along the distribution of unemployment durations, shorter durations are typically more influenced by the level of pre-unemployment wages than longer duration (the downward profile of the curves). Despite the short range of ages considered, older individuals tend to have longer spells of unemployment. Finally, the last row of panels tells us that women spend longer periods unemployed, although the differences to men decrease at longer unemployment spells.

We consider now the treatment impact. It is evident that the policy induced longer unemployment spells; the policy impact is statistically significant, as all 90 percent confidence intervals lay short of zero (2nd row of plots).

To highlight the differences in the treatment effect across the degrees of liquidity constraints, we present these 3 curves together in Figure 6. The most constrained reacted the least at all durations, although the impact increases over the unemployment spell. For the intermediate group, the impact is the largest, with point estimates hovering 0.4. Finally, the unconstrained group has impacts larger than those observed for the most constrained, but always lower than the ones obtained for the intermediate group. 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.

[FIGURE 6]

First, notice that there is evidence of differentiated behavior between the subsamples of intermediate and top pre-unemployment wages. At all durations of unemployment, and in response to the same incentive, the impact on the more constrained group is larger. 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 bottom quintile. Two interesting results emerge. First, it has the smallest 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 subsamples, it is possible to identify an increasing impact over the unemployment

spell, which conforms with the theoretical prediction of nonuniform impact over the distribution of unemployment spells. However, towards the right tail of the distribution of durations, the curves flatten out 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 heterogeneously at different levels of liquidity constraints over the unemployment spell.

The main novelty of these results is the non-monotonous impact of the extended benefits along the pre-unemployment wages distribution. This result is not motivated by most job search models, but it is evidence in favor of a nonstationary job search environment. The empirical evidence on the income effect is scant. Chetty (2005) studies the US labor market, which is characterized by short unemployment durations and has a UI program with short entitlement periods. Therefore, search conditions in the US are less prone to the type of duration dependence in the exogenous variables arising in nonstationary environments. Nonetheless, some of his results still point towards some nonstationarity; the impact of UI generosity for the bottom quartile of the distribution of net liquid wealth is smaller than the ones obtained for the second and even third quartiles. These results are remarkably similar to the ones we obtained for Portugal, in the context of larger durations of unemployment.

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 evaluate the extra costs, it is necessary to first express the impact in terms of additional subsidized days. This can be adequately done by 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 $\log(\text{days})$. Thus, the QTE estimator of equation (3) becomes

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

Given the model specification of equation (6), the QTE for quantile τ expressed in days is given by $\exp(\beta_0(\tau) + \bar{x}'\lambda(\tau))\{\exp(\beta_1(\tau) + \beta_2(\tau) + \beta_3(\tau)) - \exp(\beta_1(\tau)) - \exp(\beta_2(\tau)) + 1\}$, where \bar{x} indicates the average value of x . Figure 7 presents in days the QTE for the same quantiles

shown before. For the bottom and top subsamples, the median duration increased by slightly over 90 days, close to the entitlement extension, but by almost 130 days for the intermediate subsample. 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 (an increase of only 27 days 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, at each unemployment duration (quantile) and for each subsample, we compute the average daily UI received by the unemployed. Then, we multiply the daily UI by the QTE expressed in days. The results are summarized in Table 6. For the median duration, the financial impact is 1,014.45, 1,830.61 and 1,907.33 euros (in 1999 prices), respectively, for the bottom, intermediate and top wage groups. This represents a substantial increase in cost for the system, which expressed in terms of the average UI paid to the unemployed in the bottom wage quintile represents, respectively, 45.7, 82.4 and 85.9 percent. Not surprising, Table 6 also reveals that most of the additional financial resources spent by the public system were directed to the unemployed in the top wage group.

[TABLE 6]

6.4 Robustness: Falsification test, anticipation effects and an alternative *after* period

We now check the robustness of our results. First, we consider a falsification test by taking the age group [25, 29] as a placebo treatment group. Then, we scrutinize the sensitivity of our findings to different definitions of the sample. We will consider two cases that may bias our estimates or hide idiosyncratic behaviors, namely, anticipation effects and a change in the business cycle.

The leftmost panel of Figure 8 presents the estimates of the falsification test. The placebo treatment group, [25, 29], has a 12-month entitlement period throughout the analysis period,

while the control group has an 18-month entitlement period. The results are reassuring of the appropriateness of our identification strategy. We did not find a significant ‘impact’ on the placebo treatment group; although omitted from the plot, the 90 percent confidence bands include the zero, with the only exception of the quantiles above the median for the top income subsample.

[FIGURE 8]

As it is the case with all pre-announced legislative reforms, there is the possibility of some kind of anticipation effect (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 last 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 for (self-selecting into) the more generous system. The results are plotted in the middle panel of Figure 8, and show a remarkable similarity with the results discussed hitherto, which suggests that there were no anticipation effects.

Finally, we also consider an alternative definition of the *after* period, namely, July, 1999 to December, 2000. This choice yields a more uniform macroeconomic cycle, avoiding the common pitfalls associated with changes in the business cycle. The same conclusion is reached with this exercise; the ranking of the curves is preserved, rightmost panel, but the impacts at the bottom and top subsamples are slightly closer, with the gap to the intermediate group widening.

Overall, all 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 yet established 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 its shape (dispersion)? With a direct interpretation in terms of the nonstationary job search model, which predicts larger impacts at longer durations, Koenker and Xiao (2002) provide us with the inference tools to answer (test) formally these two questions (hypotheses).

Table 7 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, $After \times Treat$. 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 log durations did not increase. On the other hand, the intermediate 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. Notice, however, that accepting a location shift of the log duration distribution implies that in levels the durations at longer spells have increased the most, resulting therefore also in larger unemployment duration variance.

[TABLE 7]

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). Overall, these impacts are the ones predicted by economic theory: more generous unemployment benefits result 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 UI entitlement period affects the duration of subsidized unemployment in a nonstationary job search environment. 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 a significant heterogeneous UI impact over the wages distribution, associated with differences in the degrees 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 individuals with worse labor market prospects, usually those who also face tighter liquidity constraints.

Identification of the income effect relies on a reform of the Portuguese UI system introduced in July 1999, which extended significantly the entitlement periods for some age groups of the population, while maintaining the same benefit limit for other (adjacent) age groups. 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 period remained constant (exactly at 18 months). Furthermore, the reform was not endogenously motivated by labor market conditions. Indeed, it was 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 more constrained individuals (2nd and 3rd wage quintiles). Individuals in the bottom wage quintile increased the least their unemployment spells, which constitutes an interesting result in light of the nonstationary job search model.

These results provide qualitative insights towards designing an optimal UI policy. On the one hand, the nondistortionary nature of the income effect would tend to support more generous UI benefits. However, this policy conclusion is put into question by the fact those who could benefit more from UI end up being the ones who reacted the least to the extension of the entitlement period, as a result of the nonstationary job search process.

We focused 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. This caveat to the generality of our results opens the scope for evaluating the empirical relevance of the income effect on younger and older workers. Additionally, although the income effect is non-distortionary, an evaluation of its positive impact on labor market outcomes requires assessing its impact on the productivity of the job search process, that is, on the job match quality as proxied by post-unemployment

wages and job tenure.

Appendix

[FIGURE A.1 HERE]

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

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

† Age at the beginning of the unemployment spell.

* For those aged 45 or older, 2 months can be added for each 5 years of social contributions during the 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 in the before period

	Treatment	Control
Age	31.88	36.94
Females	0.34	0.35
Previous real wages ⁽¹⁾		
Average computed on:		
Full sample	696.27	726.42
1st wages quintile	496.08	500.55
2nd wages quintile	583.11	581.83
3rd wages quintile	681.58	681.69
4th wages quintile	838.11	842.51
5th wages quintile	1,160.99	1,191.24
Minimum	353.10	350.10
Maximum	1,487.55	1,561.98
No. of observations	3,145	3,631

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: Impact on subsidized unemployment duration: Simple D-in-D estimate

	Treatment		Control	
	Before	After	Before	After
Average spell duration (in months)	210.58	291.16	321.95	319.68
Differences		80.57		-2.27
D-in-D				82.84
No. of observations	3,145	20,081	3,631	14,125

Notes: IIESS dataset with authors' computations.

Table 5: Monthly wage levels and financial assets holdings

Wage level (in 2000 euros)			Financial assets expressed in terms of:	
Minimum	Median	Maximum	Mean level of assets ⁽¹⁾	Median wage ⁽²⁾
€358.15;	€533.00;	€551.73]	0.18	2.90
€551.74;	€634.50;	€757.76]	0.34	4.52
€757.77;	€980.68;	€1,655.10]	0.87	7.51

Notes: Data source: Consumer expenditures survey, 2000. Authors' computations; (1) Average level of financial assets held by individuals in a particular wage group expressed in terms of the average level of financial assets held by individuals aged [30, 39] with wage levels in the same range as that reported for the full sample of unemployed individuals; (2) Average level of financial assets expressed in terms of the median wage reported in the first column.

Table 6: The financial impact on the UI system

τ	Pre-unemployment wages quintiles								
	Daily UI ⁽¹⁾	1st Δ UI ⁽²⁾	In % ⁽³⁾	Daily UI ⁽¹⁾	2nd and 3rd Δ UI ⁽²⁾	In % ⁽³⁾	Daily UI ⁽¹⁾	4th and 5th Δ 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: τ stands for (estimated) quantile; (1) Daily UI is computed as the average daily UI paid to individuals in the τ -th duration quantile in the age group [30, 34] during the before period; (2) The Δ UI is the product of the daily UI by the τ -th QTE expressed in days; (3) The percentage impact is given by the ratio of Δ UI to the average benefits paid in the 1st quintile in the before period.

Table 7: Quantile regression process: Location shift and location-scale shift test statistics

Individual hypothesis	Full sample		Subsamples by quintile of pre-unemployment wages					
	L	LS	1st		2nd & 3rd		4th & 5th	
	L	LS	L	LS	L	LS	L	LS
After \times 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.

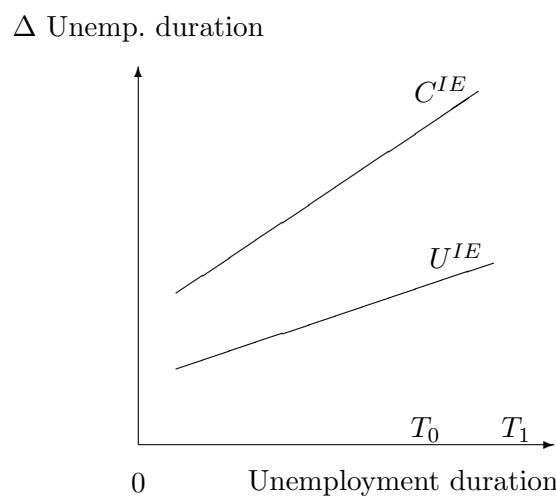


Figure 1: Illustration of the change in unemployment duration following an increase in the benefit entitlement period. *Ceteris paribus*, constrained individuals' reaction, C^{IE} , is larger, at all durations, than unconstrained individuals', U^{IE} . The difference between the two curves identifies the income effect.

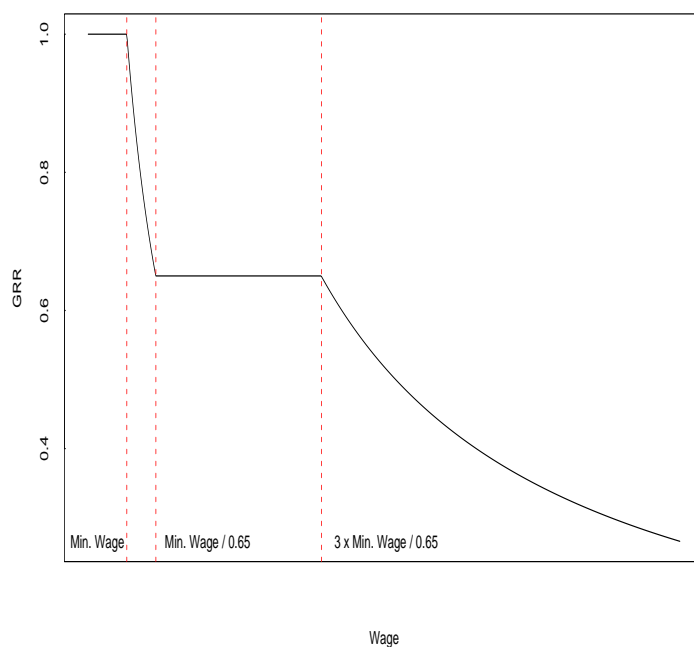


Figure 2: Financial generosity of the Portuguese UI system: Gross Replacement Rates (GRR)

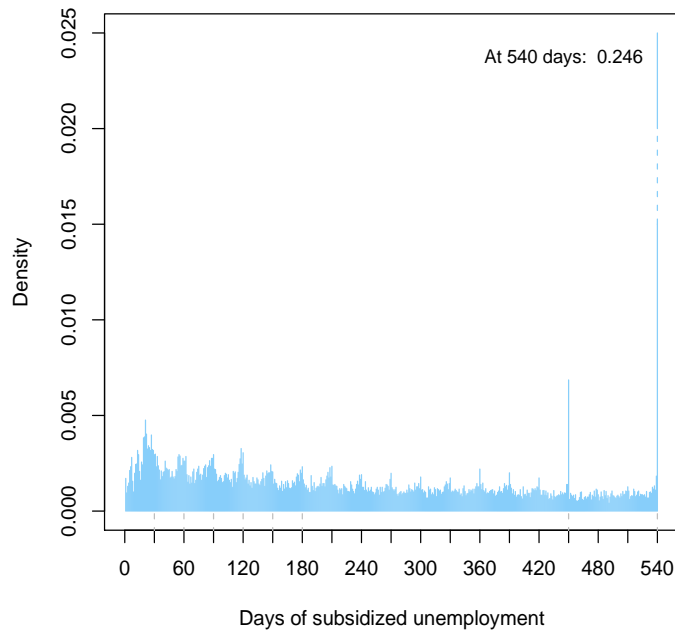


Figure 3: Histogram: Days of subsidized unemployment

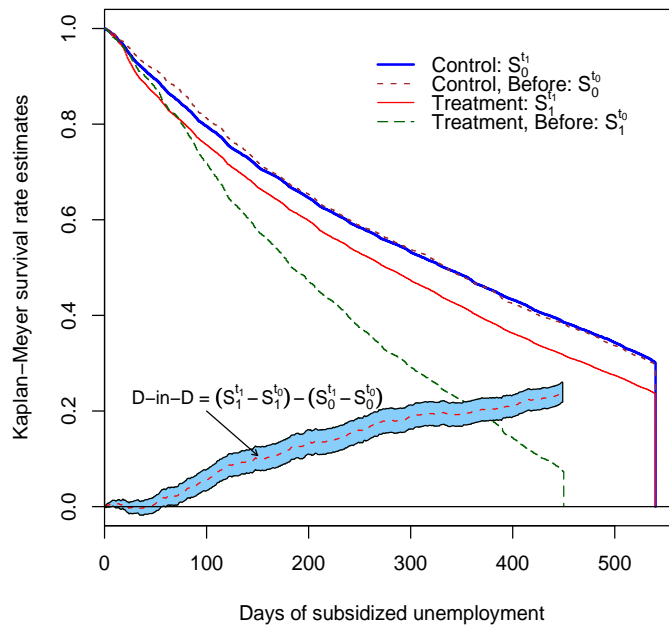


Figure 4: Kaplan-Meier estimates: Survival rates and D-in-D treatment effect on survival rates

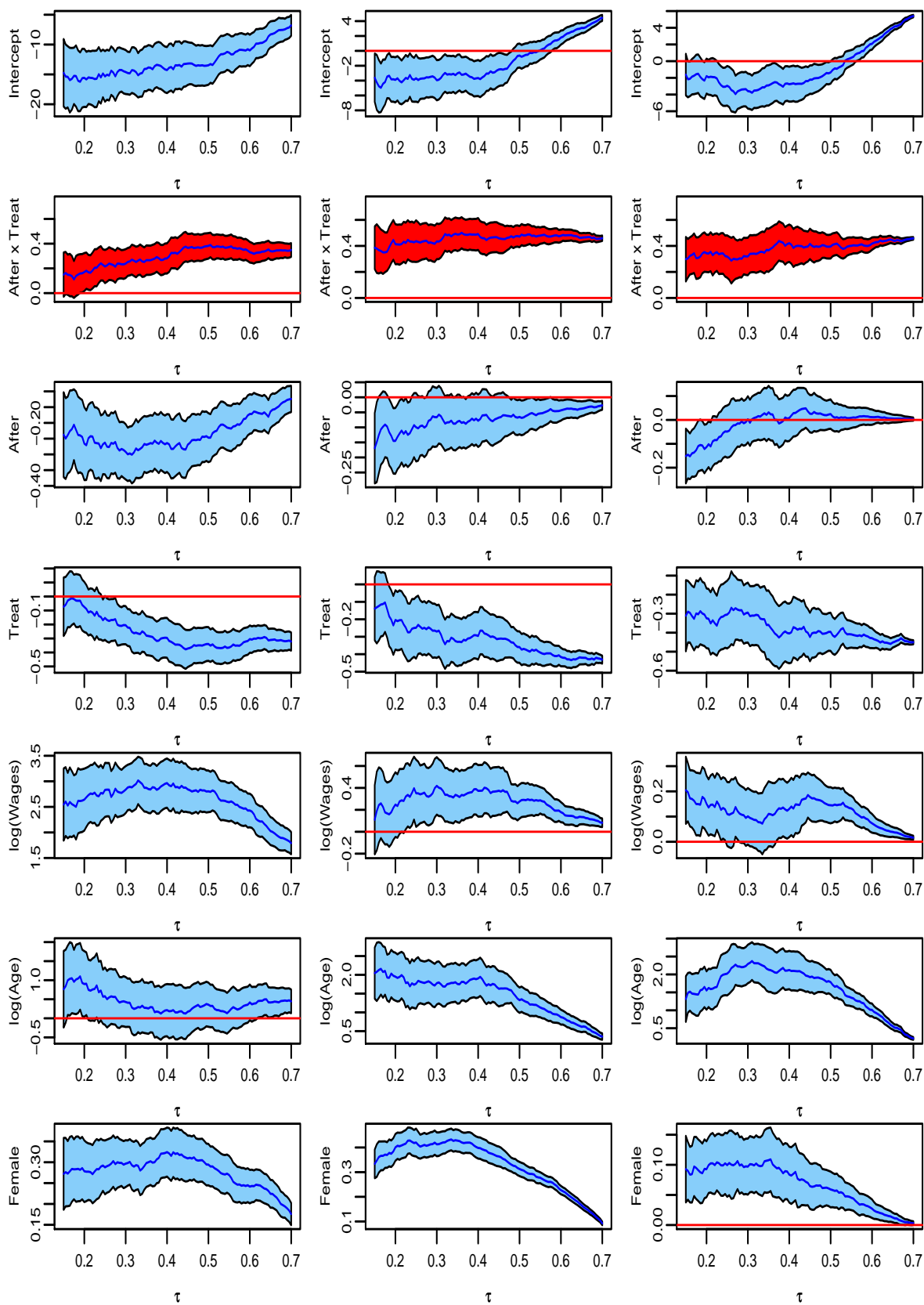


Figure 5: Quantile regression estimates: Log(duration) models by degree of liquidity constraints. The first column presents estimates for the first quintile of pre-unemployment wages; the second column for the 2nd and 3rd wage quintiles and the last column for the top two wage quintiles.

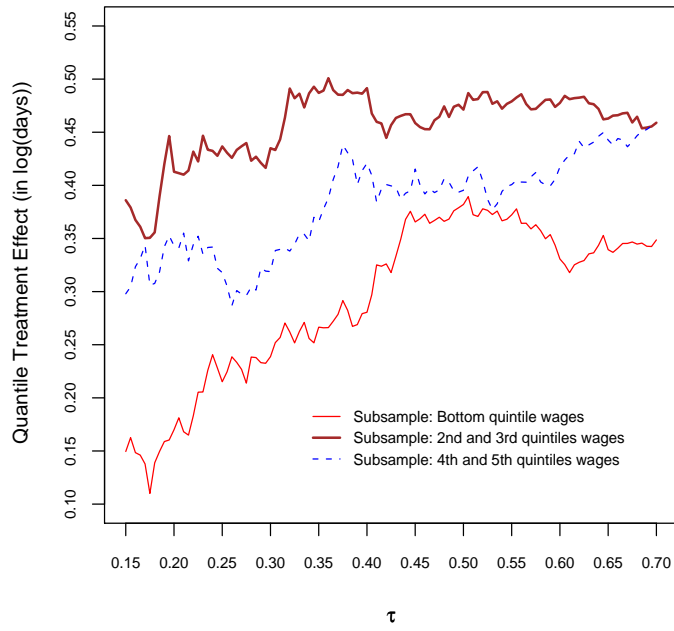


Figure 6: Quantile Treatment Effect estimates by degree of liquidity constraints proxied by quintiles of pre-unemployment average income

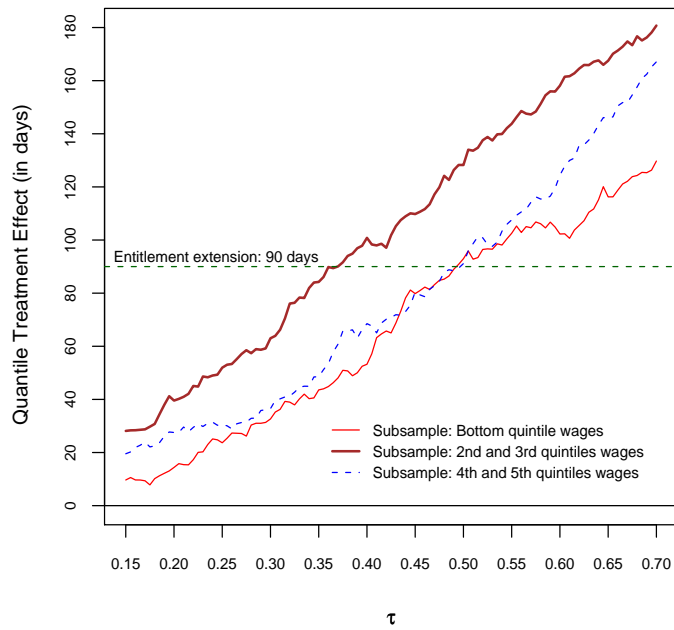


Figure 7: Quantile Treatment Effect estimates expressed in days by degree of liquidity constraints proxied by quintiles of pre-unemployment average income

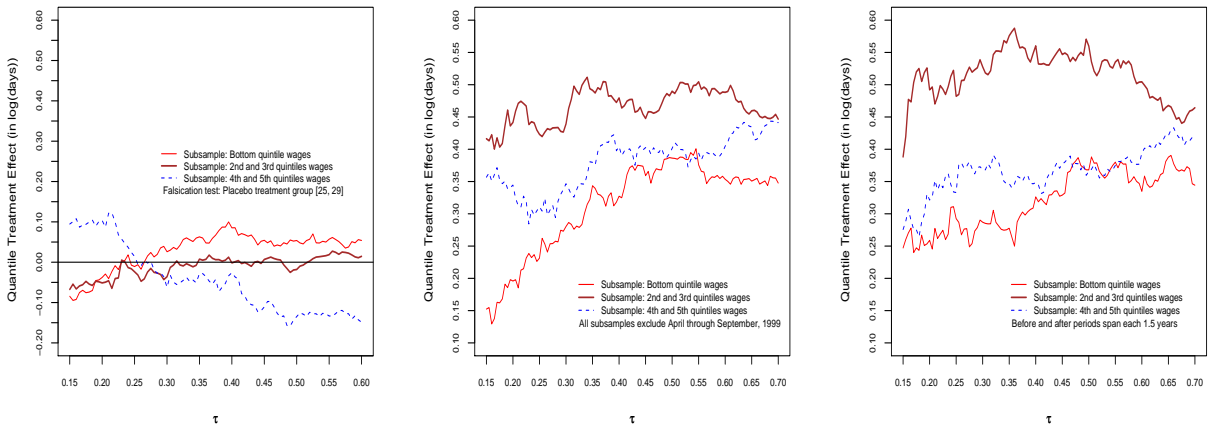


Figure 8: Quantile Treatment Effect estimates by degree of liquidity constraints proxied by quintiles of pre-unemployment average income computed for 3 cases: (i) the falsification test (leftmost panel) uses the age group [25, 29], with entitlement period unchanged at 12 months, as a placebo treatment group; (ii) the central panel excludes observations in a 6 month time window around July 1999, and (iii) the rightmost panel restricts the after period to 1.5 years after July 1999

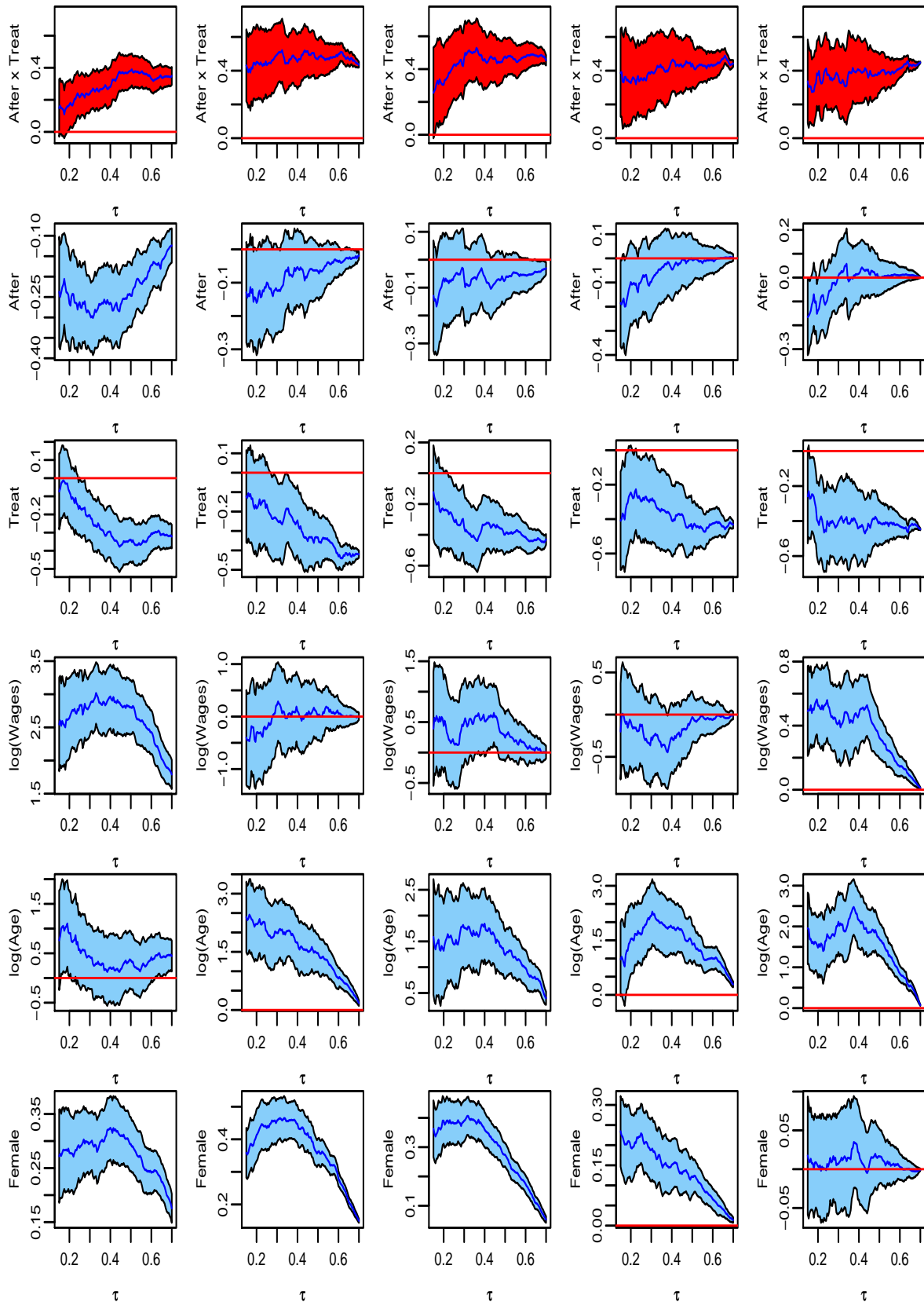


Figure A.1: Quantile regression estimates: Log(duration) models by degree of liquidity constraints. The 1st column presents estimates for the 1st quintile of pre-unemployment wages; the 2nd column for the 2nd quintile and so on. Each panel depicts the point estimates of the coefficient associated with the respective variable for each quantile. The 1st row of plots identifies the quantile treatment effects. The smallest impact occurs in the subsample of 1st quintile wages, while the impact in the 2nd and 3rd quintiles hovers above 0.4 and in the top wage quintiles hovers below 0.4.