THE REGRESSIVITY OF UNEMPLOYMENT INSURANCE: IDENTIFICATION OF THE INCOME EFFECT THROUGH THE JULY 1999 LEGISLATION*

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1. INTRODUCTION

Program administrators face important trade-offs when setting up an (optimal) unemployment insurance (UI) system. For instance, they must strike for a balance between the possible positive impacts on consumption smoothing for liquidity constrained individuals and on increased match quality,¹ and the undesired distortion to job search intensity caused by the provision of benefits. The first task is achieved by issuing unemployment insurance, which allows workers to keep a percentage of pre-unemployment income. But, it is precisely this insurance that, by changing the relative price of leisure, has a negative impact on the incentive to search for a job. UI decrease the cost of unemployment or, alternatively, makes employment less attractive in relative terms.² This dimension corresponds to the substitution effect that the literature has highlighted. However, UI can also have an income effect that varies with the degree of liquidity constraints faced by the unemployed, generating a heterogeneous impact on unemployment duration. If this proposition is empirically relevant, than the UI system is fulfilling one of its primary objectives.

In this paper, we take advantage of the exogenous July 1999 legislative extension the UI entitlement period to assess the impact of UI on the duration of unemployment. The analytical advantage of this legal reform is that it allows for the construction of a quasi-experimental setting.

The results show that the UI entitlement period extension prolongs subsidized unemployment spells, but that its effect decreases, typically, with the degree of liquidity constraint (indexed by the pre-unemployment wage quintiles). Thus, we identify a non-distortionary income effect of UI, generated by the reduction in the liquidity constraints of the unemployed. The behavior of individuals in the first quintile is the exception. The fact that individuals with the largest constraints extended the least their unemployment spells is consistent with a nonstationary job search model. Overall, these results suggest that the extension of the entitlement period may introduce regressive elements in the UI system by benefiting the least individuals in the lower part of the income distribution. In normative terms, the results suggest that the UI entitlement period should be shortened, as well as a decreasing function of pre-unemployment income, similarly to the financial generosity of the system.

* We would like to thank the Instituto de Informática da Segurança Social (IISS) for making available the data. The opinions expressed do not necessarily coincide with those of Banco de Portugal or IISS.
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(1) Belzil (2001), Centeno (2004) and Centeno and Novo (2006b) present evidence, respectively, for Canada and the United States, that a financially more generous unemployment benefits result in better jobs, measured in terms of wage gains and job stability.

(2) Yet another less desirable impact of unemployment insurance is the crowding-out effect in other forms of private savings (insurance), but that in practice are difficult to measure.
2. LITERATURE: THEORY AND EMPIRICAL

2.1. Theory

The main theoretical results that motivate the empirical exercise in this paper are derived from the standard nonstationary job search model in Mortensen (1986). 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 (2007).

To add intuition for these outcomes, first we think of the workers’ liquidity constraints as in Mortensen (1986), where the worker is able to self-finance the search costs only for a finite period of time. 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, without changes in the relative prices of leisure and work, therefore, in a non-distortionary form. 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. Chart 1 illustrates this effect in a stylized way. After the increase in the entitlement period, from $T_0$ to $T_1$, the income effect will produce a larger increase in unemployment duration for constrained individuals ($C_{IE}$) than for unconstrained ($UI_{IE}$).

Notice that in Chart 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 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), Mortensen’s model is extended with the inclusion of other exogenous variables, namely, the arrival rate of job offers and the wage offers distribution. All these variables 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.
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 to take full advantage of the additional benefits. Thus, the relative position of the two curves in Chart 1 – the income effect – becomes an empirical question.

2.2. 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.

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.

Note: Illustration of the impact on unemployment duration after an increase in the UI entitlement period. Ceteris paribus, the reaction of individuals with financial constraints, $C^{IE}$, is greater, at all durations, than the reaction of unconstrained individuals, $U^{IE}$. The difference between the two curves identifies the income effect.
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 (2007) 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.

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 non-uniform 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,

\[ Q_{\tau}(x | \beta) = \beta(\tau), \]

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,

\[ E(Y | X = x) = \beta x. \]

Thus, quantile regression has a descriptive advantage over least squares – it provides 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. Thus, if the effect of the entitlement period extension is felt primarily at longer durations, then, for instance, the 75th percentile \( \beta \) will be larger than the 25th percentile \( \beta \).

Furthermore, quantile regression is very well suited for the specific duration-related questions arising in the context of the nonstationary job search model, which we address in this paper. 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 effect

The concept of quantile treatment response was first proposed by Lehmann (1975). In practical terms, Lehmann’s definition is easy to implement. It is heuristically convenient to establish a parallel with the average treatment effect on the duration of subsidized unemployment. This effect is computed as the difference between the average duration of unemployment in the treatment group (those subjected to the policy intervention) and the average for the control group (those not subjected to the policy intervention). In the case of the median treatment effect, for instance, one starts by computing the (empiri-
cal) median unemployment duration for the treatment group; the procedure is repeated for the control group. The difference between the two median durations yields the median treatment effect in the distribution of subsidized unemployment durations. Its interpretation is also rather simple; it tells us, for the case of the median, that it would be \( n \) days higher (smaller if \( n \) is negative) than in the absence of treatment. For other percentiles of the distribution, the procedure and interpretation are the same. Relatively to the average treatment effect, the quantile treatment effect has a descriptive advantage because it allows us to characterize the impact of the policy along the distribution of subsidized unemployment spells.

The observation of individuals belonging to the treatment and control groups in the periods before and after the policy intervention allows us to refine the estimate of the quantile treatment effect. The existence of observations before and after the treatment for the control group provides an estimate of the impact of the macroeconomic environment on the labor market outcomes. If we assume that such environment would affect equally the treatment group in the absence of UI entitlement extension, then we should discount this value to the evolution through time of the treatment group unemployment durations. In other words, we could say that the simple difference of behavior of the treatment group between the before and after period would be contaminated/affected by effects not attributable to the UI extension (macroeconomic effects). Thus, one must subtract the control group difference to the treatment group difference, resulting in the quantile treatment effect difference-in-differences estimate. Formally, the estimate is obtained as: for each time period, the impact of the treatment is computed as described in the previous paragraph and the difference between the estimate for the before period and estimate of the after period gives us the final impact (see Centeno and Novo (2007) and Koenker (2005) for a technical discussion of the quantile treatment effect).

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 540 days 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. Chart 2 illustrates the financial generosity of the system expressed in terms of the gross replacement rate (GRR).

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.\(^3\) 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.\(^4\)

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

\(^{3}\) In the data, some ratios are not exactly equal to 65 per cent, which lead us to preserve all observations with GRR in the interval [63, 67] per cent.

\(^{4}\) Indeed, for Germany, Fitzenberger and Wilke (2007) report evidence of a large disincentive effects on labor supply attributable to high replacement rates.
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.

The methodology used to estimate the impact of the new legislation consists in defining two groups from the population with different exposure to the legislation: (i) the age group [30, 34], whose entitle-
ment period increased from 15 months to 18 months and (ii) the age group [35, 39], whose entitlement period remained unchanged at 18 months. The first group is identified as the treatment group and the second as the control group.

These two groups are particularly comparable given the age proximity and the fact that, after the reform, they share the same entitlement period. Indeed, the treatment group, [30, 34], 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.

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.

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.

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. 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. By the same token, we need to exercise caution when extrapolating the results for the population. The specificity of the studied group and the distinct characteristics of the remaining population are potential external threats to the validity of these results for the population.

<table>
<thead>
<tr>
<th>Year</th>
<th>GDP growth rate</th>
<th>Employment growth rate</th>
<th>Unemployment rate</th>
<th>Long term unemployment (%)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1997</td>
<td>4.2</td>
<td>1.9</td>
<td>5.8</td>
<td>43.6</td>
</tr>
<tr>
<td>1998</td>
<td>4.7</td>
<td>2.3</td>
<td>5.0</td>
<td>45.4</td>
</tr>
<tr>
<td>1999</td>
<td>3.9</td>
<td>1.9</td>
<td>4.4</td>
<td>41.2</td>
</tr>
<tr>
<td>2000</td>
<td>3.9</td>
<td>2.3</td>
<td>3.9</td>
<td>43.8</td>
</tr>
<tr>
<td>2001</td>
<td>2.0</td>
<td>1.5</td>
<td>4.0</td>
<td>40.0</td>
</tr>
<tr>
<td>2002</td>
<td>0.8</td>
<td>0.5</td>
<td>5.0</td>
<td>37.3</td>
</tr>
<tr>
<td>2003</td>
<td>-1.2</td>
<td>-0.4</td>
<td>6.3</td>
<td>37.7</td>
</tr>
<tr>
<td>2004</td>
<td>1.1</td>
<td>0.1</td>
<td>6.7</td>
<td>46.2</td>
</tr>
</tbody>
</table>

Sources: National accounts, INE; Inquérito ao Emprego, INE.
5. DATA

Our study is based on administrative data collected by the Portuguese government’s agency Instituto de Informática da Segurança Social (IISS). The dataset recorded all subsidized unemployment spells initiated between January 1, 1998 and June 30, 2003, which we are able to follow 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.

With the aforementioned restriction of GRRs to the interval [63, 67] per cent, we have a total of 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.

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). The interpretation of this result is straightforward: if there was not an extension of the UI entitlement period, individuals aged 30 to 34, who benefited from a 90 days extension, would, on average, spent 83 days less in subsidized unemployment.

The analysis of survival rates, Kaplan-Meyer estimates (Chart 3), confirms these results and illustrates 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 unemploy-

<table>
<thead>
<tr>
<th>Table 3</th>
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<tbody>
<tr>
<td><strong>SUMMARY STATISTICS: BEFORE JULY, 1999</strong></td>
</tr>
</tbody>
</table>

<table>
<thead>
<tr>
<th></th>
<th>Treatment</th>
<th>Control</th>
</tr>
</thead>
<tbody>
<tr>
<td>Age (in years)</td>
<td>31.88</td>
<td>36.94</td>
</tr>
<tr>
<td>Proportion of women</td>
<td>0.34</td>
<td>0.35</td>
</tr>
<tr>
<td>Real wage(a)</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Full sample</td>
<td>696.27</td>
<td>726.42</td>
</tr>
<tr>
<td>1st wage quintile</td>
<td>496.08</td>
<td>500.55</td>
</tr>
<tr>
<td>2nd wage quintile</td>
<td>583.11</td>
<td>581.83</td>
</tr>
<tr>
<td>3rd wage quintile</td>
<td>681.58</td>
<td>681.69</td>
</tr>
<tr>
<td>4th wage quintile</td>
<td>838.11</td>
<td>842.51</td>
</tr>
<tr>
<td>5th wage quintile</td>
<td>1,160.99</td>
<td>1,191.24</td>
</tr>
<tr>
<td>Minimum</td>
<td>353.10</td>
<td>350.10</td>
</tr>
<tr>
<td>Maximum</td>
<td>1,487.55</td>
<td>1,561.98</td>
</tr>
<tr>
<td>Number of observations</td>
<td>3,145</td>
<td>3,631</td>
</tr>
</tbody>
</table>

Sources: IISS. Authors’ computations.
Note: (a) The pre-unemployment wage for each individual is computed as the 12-month average of wages reported in the period that precedes unemployment in 2 months. Real wages expressed in 1999 euros.
ment. 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).

Table 4

<table>
<thead>
<tr>
<th>IMPACT ON THE DURATION OF SUBSIDIZED UNEMPLOPMENT DURATION: DIFFERENCE-IN-DIFFERENCES ESTIMATES</th>
</tr>
</thead>
<tbody>
<tr>
<td>Treatment</td>
</tr>
<tr>
<td>Before</td>
</tr>
<tr>
<td>Average duration of unemployment (in days)</td>
</tr>
<tr>
<td>Differences</td>
</tr>
<tr>
<td>Difference-in-differences</td>
</tr>
<tr>
<td>Number of observations</td>
</tr>
</tbody>
</table>

Sources: IISS, Authors’ computations.

Chart 3

<table>
<thead>
<tr>
<th>SUBSIDIZED UNEMPLOYMENT SURVIVAL RATES</th>
</tr>
</thead>
</table>

Sources: IISS, Authors’ computations.

Note: Estimates based on the Kaplan-Meyer’s estimator. The impact of the extension is measured by the difference in the survival rates between the 4 groups and it is represented by the “D-in-D” line, at the bottom of the chart, with a 95 per cent interval drawn around it.
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. We split the sample by degrees of liquidity constraints and use quantile regression tools to capture the nonstationary nature of the duration process. 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. To capture such differences, we split the sample into three subsamples, using the 12-month average of pre-unemployment wages 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 *Inquérito ao Património e Endividamento das Famílias (IPEF)* for 2000, a household assets and debt survey.

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). 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 hold by each group, respectively, as (i) a percentage of the average level of financial assets for the IPEF 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.

Table 5

<table>
<thead>
<tr>
<th>MONTHLY WAGES AND LEVEL OF FINANCIAL ASSETS</th>
</tr>
</thead>
<tbody>
<tr>
<td>Wages (in euros, 2000)</td>
</tr>
<tr>
<td>-------------------------</td>
</tr>
<tr>
<td>Group</td>
</tr>
<tr>
<td>1st quintile</td>
</tr>
<tr>
<td>2nd and 3rd quintiles</td>
</tr>
<tr>
<td>4th and 5th quintiles</td>
</tr>
</tbody>
</table>

Sources: IPEF, 2000. Authors’ computations.
Notes: (a) Average level of financial assets expressed in percentage of the average level of financial assets for the sample of IPEF individuals aged 30 to 39. (b) Average level of financial assets expressed as a percentage of the median wage level of each group.
6.2. Quantile treatment effects

The quality of the quasi-experimental setting of was confirmed in the previous analysis. However, there are possible confounding factors that can be controlled for 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 subsidized unemployment days, \( \log(T) \), has linear conditional quantile functions, \( Q \), of the form:

\[
Q_{\log T}(\tau) = \beta_0(\tau) + \beta_1(\tau) \text{After} + \beta_2(\tau) \text{Treat} + \beta_3(\tau) \text{After} \times \text{Treat} + X \lambda(\tau)
\]

where \( \text{After} \) is an indicator variable for the after-July 1999 period, \( \text{Treat} \) indicates the age group affected by the new legislation, and, therefore, the coefficient on \( \text{After} \times \text{Treat} \) 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. This model is estimated for each of the 3 wages-based subsamples.

The estimation results are presented in a concise format in Chart 4. Each column of panels presents the quantile regression estimates for each of the 3 subsamples (from most to least constrained). 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 \([0.15, 0.70]\), ignoring, in practice, the very short duration (less than 2 months) and the longer durations (more than 470 days). The shaded areas represent 90 percent confidence intervals.

To highlight the differences in the treatment effect across the degrees of liquidity constraints, we present these 3 curves together in Chart 5. 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, i.e., an increase in the duration of unemployment of approximately 40 per cent. Finally, the unconstrained group has impacts larger than those observed for the most constrained, but always lower than 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.

These results have multiple interpretations. 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. Secondly, the behavior of individuals at the bottom wages quintile yields two interesting results. 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

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(5) To preserve space, we omitted from this plot the results on the month and region indicator variables.
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.

### 6.3. The financial cost and the redistributive impact

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, which allows us to transform back into days the estimated impacts in \( \log(\text{days}) \).

Chart 6 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.

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. For instance, for the bottom wages group and for individuals who left the UI system at the median duration, the average daily UI received amounted to 10.91 euros. Then, we multiply the daily UI by the QTE expressed in days. For this group the extension was 93 days, therefore, the additional financial costs were 1,014.45 euros. For the remaining groups, the intermediate and top wage groups, the financial impact was 1,830.61 and 1,907.33 euros (in 1999 prices), respectively. 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, 82.4 and 85.9 per cent.\(^6\)

In other words, with the additional financial resources spent with these groups, the UI system could

\(^6\) See Centeno and Novo (2007) for additional details, in particular, for computations at other percentiles of the distribution of subsidized unemployment.
have financed a full spell of unemployment with median duration for an individual in the bottom wages group.

These results show that the majority of the additional financial resources were directed towards unemployed who had higher wages before unemployment. Besides the differences in the amount of UI received, this result is associated with the lowest extension of the period of subsidized unemployment for individuals with the lowest wages. Given that the GRR are the same for all individuals considered in the study, this phenomenon reflects itself in the regressiveness of the UI system. Thus, the UI transfers promoted by the system favor individuals with higher income.

The UI system contains regressive characteristics applicable to the population of UI beneficiaries. In practice, the longer durations of subsidized unemployment are observed for individuals with higher pre-unemployment income. Thus, the fact that these individuals also have longer subsidized unemployment spells accentuates the disparity of UI expenses. Chart 7 plots a Lorenz curve, used to measure inequality; the larger the area between the two curves, the larger the inequality. In the 2000 to 2006 period, this area, measured by the Gini coefficient, is 0.43 (with 0 indicating perfect equality and 1 perfect inequality). Not surprisingly, the current UI system has a degree of inequality that is larger than the wage inequality, which has a value of 0.34. The data show that 10 per cent of the unemployed receive approximately 30 per cent of the outlays with UI, while 35 per cent of the unemployed less subsidized receive only 10 per cent of the total UI outlays.
7. CONCLUSIONS

This paper addresses the question of how the generosity of the UI 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 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 face tight liquidity constraints.

The identification of the 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).
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 by individuals in the intermediate wages group (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 models.

This result shows that a UI system that is very generous in the duration of the benefits can become fiscally regressive, because the extension in the entitlement ends up benefiting the least individuals at the bottom of the wage distribution. Given the smaller reaction of these individuals, who are more affected by the nonstationarity in the labor market environment, the weight of UI expenses with this group of individuals decreases.

The results point towards the importance of changing the UI concession rules. In this context, one proposal would set shorter entitlement periods and set them as a decreasing function of pre-unemployment income. Complementarily, to reach a higher impact of the income effect, it is preferable to increase the financial generosity of the UI towards the most constrained, rather than granting them longer entitlement periods, which they find harder to take up.

REFERENCES


