Heterogeneity of the unemployment insurance system: lazyness or simply income effects?*

Mário Centeno
Banco de Portugal & ISEG - U. Técnica
mcenteno@bportugal.pt

Álvaro Novo
Banco de Portugal & ISEGI - U. Nova
anovo@bportugal.pt

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Abstract

The agenda for unemployment insurance (UI) system 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, in this paper, we show that the impact of the system is very heterogeneous. We show this for the two most important labor market outcomes related with a system of UI benefits: unemployment duration and post-unemployment wages. The results point to longer spells of unemployment, particularly in younger cohorts, if given additional entitlement periods. In terms of reemployment wages, the estimates suggest a negative impact on young individuals' wages, noticeable only at quartiles above the median of the previous income. The effect of added subsidized search time for older individuals is slightly positive and clearly driven by those in the fourth quartile of the previous income distribution. This means that reforms of the system should target these age groups in differentiated ways.

Keywords: Unemployment insurance; Liquidity constraints; Unemployment duration; Reemployment wages.

JEL Codes: J65, J64, J23.

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1 Introduction

The impact of the unemployment system on labor supply decisions has been extensively studied in the public finance and labor economics literature and reviewed recently in Krueger and Meyer (2002). The argument in favor of the estimated large effects of unemployment insurance (UI) in reducing labor supply rests on its impact on the relative price of leisure, thus creating a moral hazard problem. The description of the desincentives created by UI along these lines can be found in the seminal paper by Shavell and Weiss (1979) and has been used more recently in the literature on the optimal design of UI, for example in Hopenhayn and Nicolini (1997) or Fredriksson and Holmlund (2001). This literature as focused on the question of time sequencing of benefits and showed that optimal UI design should include a decreasing UI benefit over the unemployment spell.

The impact of the UI system is not fully captured in these models and a set of both theoretical and empirical papers have consider other, positive, effects of the UI system, both in terms of welfare effects during the unemployment spell and in terms of post-unemployment outcomes (for example, re-employment wages or match duration). These papers include the models by Acemoglu and Shimer (2000) and Marimon and Zilibotti (1999) and the empirical analysis by Gruber (1997), Belzil (2001), and Centeno (2004).

A different approach that looks at heterogenous effects over the income distribution has been recently explored by Chetty (2005). The argument developed in this literature rests on the idea that the UI distortionary effect on the relative price of leisure will depend on the degree of borrowing constraints faced by agents. Their results show that increases in UI benefits have larger effects on more constrained individuals and very small effects on unconstrained unemployed.

In this paper, we consider these three strands of the literature and look at the behavior of subsidized unemployment in terms of longer unemployment durations and potential gains in post-unemployment wages. we do it, in particular, along the pre-unemployment income distribution to capture heterogenous impacts. By taking advantage of the 1999 reform of the Portuguese UI system, we construct a quasi-experimental setting to identify the causal effect that the (extended) entitlement periods have on two main variables: unemployment duration and post-unemployment wages. In both dimensions of the UI impact, we will look carefully at

the differentiated effects over the income distribution.[1]

We use Portuguese administrative data from the Social Security Unemployment Compensation dataset covering *all* subsidized unemployment spells that occured during the 1998-2004 period. Two other distinct characteristics of the dataset are: (i) the information on the salary and starting date of the first job following unemployment and (ii) the information on spells initiated in the period prior to the July 1999 reform of the UI system.

While this reform made the UI entitlement period substantially more generous for a large fraction of workers, it left it unchanged for workers in two age groups. We use this as a quasi-experimental setting, considering the latter groups as a control groups. Together with the pre- and post-1999 information, we are able to use standard causal effects evaluation techniques to this problem.

Our preliminary results suggest that there is heterogeneity among age groups and within these groups across pre-unemployment income quartiles. Thus, the extension of the entitlement period seems to prolong unemployment spells more for young individuals. And, amongst these, those whose previous income fell in the upper two quartiles seem to be the most affected. We take this as evidence in favor of a prevailing substitution effect. The legislation changes allowed us to construct a quasi-experimental setting from which we obtained difference-in-differences (DinD) point estimates. These indicate an increase in the average unemployment survival rate of about 9 to 10 percentage points for individuals in the $[15, 25)$ and $[30, 35)$ age groups, and about $^2/_3$ of that in the older group, $[40, 45)$.

In terms of the other key outcome variable, the DinD estimates indicate even more heterogenous effects. Thus, while the youngest cohort seemed to lose the most from the reform (-4.4%), the oldest group was not affected, but the middle group has apparently benefited (+2.7%). A feature common to these different age groups is that the overall result of each group seems driven by those individuals in the quartiles above the median of the previous income; the 3rd and 4th quartile for the youngest and only the 4th quartile for the middle group, $[30, 35)$.

The paper is organized as follows. In section 2 we present in more detail the theoretical motivation for our analysis. Section 3 sketches the Portuguese UI system and the changes introduced in 1999. We present the data in section 4. The final sections present the results and the conclusions.

[1] ZZZ Isn't this paragraph a bit too repetitive?

2 Literature: Theory and evidence

In this paper, we are interested in the relationship between unemployment insurance generosity and the search behavior of the unemployed. The latter can be observed/measured in different ways, using a number of different labor market outcomes. We will pay attention to two specific measures: the duration of subsidized unemployment and the level of post-unemployment wage gains.

The different measures used in this paper to evaluate the impact of the UI system are two examples of the tradeoff faced by program administrators: this tradeoff can be seen to happen between the undesired distortion to job search intensity caused by the provision of benefits against the possible positive impact on post-unemployment outcomes arising from longer unemployment spells.

The impact of UI on individual behavior has been subject to extensive attention in the labor economics and public finance literature. Since the seminal papers by Nickell (1979) and Lancaster (1979) showing that higher benefits are associated with longer unemployment spells, a wealth of new results has shown how this effect operates and paid attention 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 expiration date, pointing to the effect of UI on a decreasing reservation wage. More recently several papers point to the positive impact of UI on post-unemployment outcomes (Centeno (2004) and Belzil (2001)) and on its effect on smoothing consumption during unemployment spells (Gruber 1997).

From a theorectical point of view most results can be derived from a standard Mortensen (1977) type of search-model. The subsequent theories of optimal UI design and impact can be used as guidance for our empirical tests. We are specially interested in estimating the impact of longer benefits on labor supply decisions in the context of an UI system in which the level of benefits is not constant throughout the unemployment spell.

The simple result of observing longer unemployment spells as a response to increased generosity does not preclude the existence of a large heterogeneity in effects coming, for example, from the importance of income effects. The model in Chetty (2005) can be used to motivate our analysis of heteregenous outcomes over the pre-unemployment income distribution. In Chetty's setting, the impact of UI is differentiated on the basis of the degree of borrowing constraints

faced by unemployed workers. This dimension allows us to add, to the typical substitution effect, the possibility of a, non distortionary, income effect. If this income effect is important, the desincentive effect of UI created through the susbstitution effect can be reduced, and become less distortionary than previously thought.

The intuition for these results is as follows. We first think of workers as being either liquidity constrained or unconstrained, in the sense defined in Zeldes (1989). For liquidity constrained workers, UI might create an income effect that occurs in addition and independently of the usual substitution effect. The intuition is that when a constrained worker relies on UI benefits to maintain consumption, increasing the benefit generosity would have a large effect on consumption while unemployed. This reduces the pressure to find a job in order to generate consumption, creating the potential for an income effect. On the contrary, if workers are unconstrained, the income effect channel is almost absent, since UI benefits are a small portion of lifetime income/wealth.

Aknowledging the presence of this heterogeneity is the first novelty of our approach. However we go a step further and try to shed light on the issue of post-unemployment match quality and the possibility of differentiated effects over the income distribution. From a theoretical point of view, the fact that more generous unemployment benefits can increase match quality has been addressed, for example, by Acemoglu and Shimer (1999) and Marimon and Zilibotti (2000).

3 Brief characterization of the unemployment system reform

The Portuguese unemployment insurance system was created in 1985. At the time, it was neither very generous nor had it a significant take-up rate (see Figure 1). The system has, however, been made more generous since that initial period. The subsequent reforms changed, mainly the duration of the entitlement period, leaving the level of the benefits almost unchanged.

The unemployment benefits (UB) legislation establishes only one eligibility criterium, namely, the employment history with social contributions, requiring a minimum number of monthly contributions. In terms of the concession period, this is legislated as a function of the individuals' age at the beginning of the unemployment spell and orthogonal to the length of past contributions. It was precisely this dimension of the system generosity that was changed in 1999.

Up until July 1999, the Portuguese legislation divided workers in 8 age-groups, all with different entitlement periods. The reform made this period larger for 6 out of the 8 groups and compressed them into 4 age groups (see Table 1).

The new legislation made the duration of UI more generous. The pre-1999 duration of benefits ranged from a minimum of 10 months for those aged less than 25 years old to a maximum of 30 months for those aged 55 or more. The new legislation changed the lower bound to 12 months, while the upper bound can now reach a a maximum of 38 months, depending on the history of social contributions. In particular, individuals aged 45 or more can add 2 months to the entitlement period per each set of 5 years with social contributions to the insurance system, up to a maximum of 8 months. In practice, the upper limit of 38 months applies to a broad proportion of the population aged 45 years old or more.[2]

We will explore the wealth of specific experiences generated by this legislation change. In particular, the fact that two specific groups did not see their entitlement periods changed is particularly helpful to generate a control group.

The Portuguese system, similarly to most European systems, provides the unemployed with two other forms of insurance in the form of social assistance. The main benefit, the Social Assistance (SA), is provided for the unemployed who do not meet the UB-eligibility criterium, i.e., it benefits those who do not have a contributory period long enough to qualify for UB. However, contrary to the UB, the SA is a means-tested benefit and unemployed workers need to prove that their total household income per head does not exceed a threshold (usually indexed to the national minimum wage). The duration of this benefit is exactly the same as the UI benefit, both before and after the reform, but the subsidy amount is smaller. The second form, the subsequent social assistance (SSA), is a benefit that maybe claimed only after the end of the UB entitlement period. Again, the unemployed has to meet the means-test requirement to receive SSA, which is financial less generous and also lasts for shorter periods, half of the UB ones.

In Table 1, we can see that the entitlement periods of groups numbered 2 (those aged 25-30) and 4 (34-40) were left unchanged. On the contrary, groups 1 (less than 25) and 3 (30-35) observed their entitlement periods, not only increasing but also set equal to those in the adjacent age-group. These two facts make the choice of a treatment and control group very

[2]Those who started their working career at the age of 25 and worked 20 straight years are eligible for the 8 months extension period. For earlier starters, even with unemployment periods, it is possible to have 20 years of social contributions.

natural in this setting.

Another helpful feature of the legislation change is that the new rules are applied exclusively to those entering unemployment after the passage of the law. This allows us to use individuals in each group before and after the law as a mean of comparison.

4 Data

Our study is based on administrative data collected by the Portuguese government's agency *Instituto de Informática e Estatística da Segurança Social* (IIES). The dataset registered *all* unemployment related social transfers that took place between 1998 and 2004.[3] It contains information on the type, amount and duration of benefits, the previous wage, i.e., the income that served as reference to compute the amount of UI and, where applicable, the first re-employment wage and starting date of the job. Unfortunately, the socio-demographic variables available are limited to gender, age, nationality and local of residence.

We have a total of 1,205,165 subsidized unemployment spells, of which 598,924 received only UB, 378,489 were paid SA and 227,752 received UB during the complete entitlement period and later on benefited from the subsequent benefit (SSA). Table 2 contains summary statistics of the key variables by type of subsidized unemployment spell. At the beginning of the unemployment spell the average individual is 39 years old if (s)he receives UB and 5 years younger if benefiting from SA. While there is gender balance in unemployment spells subsidized only with UB, the two other categories show a clear unbalance in favor of women. This result is expected given two characteristics of the Portuguese labor markert, namely, that unemployment and long-term unemployment incidence is larger among women. Therefore, it is natural that more women (59%) benefit from the SSA and that a total of 60 percent of the subsidized unemployed are women.

Regarding the length of the unemployment spells, the UB and SA spells have average durations of 347 ($11\,{}^{1}/_{2}$ months) and 354 days (1 year), respectively. However, this average more than duplicates for those benefiting from SSA, with an average of 826 days ($2\,{}^{1}/_{4}$ years). This suggests that the age composition is rather different than the one found in UB-only, because the extension period is half the UB entitlement period, but older individuals have longer entitlement rights. In fact, the age distribution of SSA beneficiaries is more right-

[3]Naturally, spells of subsidized uemployment initiated before 1998 and not concluded by January of 1998 were inlcuded in the starting stock of unemployment-related benefit claimers.

skewed, i.e., with a larger positive difference between the average age and the median age.

The bottom panels of Table 2 disaggregate the mean values by gender. Males are older than females by approximately 4 years, but the difference is much smaller (1 year) if they benefit from SA. In terms of the period spent on subsidized unemployment, males spend slightly larger periods of time than females, about $1\,{}^{1}/_{2}$ months more if only on UB, but clearly larger periods, 4 months more, if they extended their benefit with the subsequent UB. In the case of SA, the lengths are reversed, but not by much: women receive benefits for only 2 more weeks on average. In terms of income, the differences between average values are always in favor of men, which reflects the general distribution of wages in the private sector in Portugal (see Vieira, Cardoso and Portela (2005)).

5 Results

We analyze the implications of the 1999 UI legislation change in terms of two key labor market outcome variables: subsidized unemployment duration and re-employment wages, focusing our attention on spells subsidized only with UB.

A bird's eye view

We start by presenting a simple view of the potential impact of the additional days of subsidized unemployment. For that, we consider one general treatment group, namely those aged $[15, 25)$, $[30, 35)$ and $[40, 45)$, and one control group, namely individuals in the age groups $[25, 30)$ and $[35, 40)$.[4]

Figure 3 depicts kernel density estimates of both the unemployment duration and reemployment wages by treatment and control group in the before and after periods. The differences between the before and after periods in the subsidized unemployment spells are striking in the case of the treatment groups (top left panel). Up until around 1,000 days, we observe that less unemployment spells terminated within this timeframe than in the 'before' period. In the control group plot (top right panel), we observe a substantial similiarity in the before and after periods in the right tail, and even an increase in the weight of the shortest spells of unemployment.[5] The differences in terms of wages are hardly noticeable in the estimated

[4]Table 1 reports the old and new entitlement periods for the treatment group. Naturally, the entitlement period remained unchanged for the control group.

[5]Maybe as a result of longer spells in the treatment group, the control group saw its employment opportunities increase, decreasing the duration of unemployment spells.

densities. Thus, if only preliminarily, we expect to see substantial effects on the duration of subsidized unemployment and tiny effects on reemployment wages.

Although rather general, this initial analysis motivates a age specific analysis, and also a more thorough analysis both in terms of control variables and econometric methodologies, which we carry on in the following subsections.

5.1 Unemployment spells: UB-subsidized

The spells under analysis include all spells that received UB support and that either ended in re-employment or that came to the end of the entitlement period.

As argued earlier, we have a set of "natural" control and treatment groups, but before focusing our attention on such groups, we present evidence of the type *before-after* for the age groups as defined by the legislation in place before 1999 (a total of 8 groups, see Table 1).

Plots in Figure 4 present Kaplan-Meyer estimates of the survival functions. The dashed lines represent estimates based on records initiated between July 1, 1999 and October 31, 2001, and are thus subject to the new law. The solid lines estimates refer to the before-1999 unemployment benefits law and use information from unemployment spells initiated between July 1, 1996 and June 30, 1999. The choice of these periods guarantees that all unemployment spells are observed, if necessary, until the day of legal exhaustion. In this sense, all subsidized spells are complete.

A general, if preliminary, pattern emerges: while the control groups' survival rates have not been significantly affected, the survival rates of the treated groups have increased. These results are expected in view of earlier work (e.g. Katz and Meyer 1990, van Ours and Vodopivec 2006), which showed, for different time periods and countries, that an increase in the entitlement period leads, on average, to longer unemployment spells. One caveat regarding the oldest workers is in order. The 1999 reform also changed the conditions for pre-retirement access for workers aged 55 or more at the moment of unemployment, which is important to understand the behavior of workers in the top age groups.

The period under analysis, 1998 through 2004,[6] is characterized by a change in the business cycle conditions. Low and decreasing unemployment rates and strong economic growth until 2000, followed by stagnation and raising unemployment rates. Figure 2 depicts the quarterly

[6]This is the period where we observe the termination of the unemployment spells initiated between July 1, 1996 and October 31, 2001.

unemployment rates. It is, therefore, possible that some of the changes observed between the two periods (legislations) are due to business cycle related issues. We use the difference-in-differences (DinD) estimation strategy to control for such common characteristics and, of course, unobserved characteristics.

For the reasons stated in section 3, we now focus our attention on the following age groups:

i) $[15, 25)$, which saw the entitlement period increase by 60 days from 10 to 12 months. We use as the control group the individuals aged $[25, 30)$, whose entitlement period remained at 12 months;

ii) $[30, 35)$ with a new entitlement period of 18 months. We study the impact of the potential additional 3 months of benefits by controlling with the age group $[35, 40)$, which entitlement period is also 18 months;

iii) $[40, 45)$, which also benefited from 3 additional months, increasing from 21 to 24 months. While the age group $[35, 40)$ is potentially a good control group in terms of age-related issues, it is not as natural as in ii) because its entitlement is smaller, only 18 months.

Thus, we argue that these pairs of treatment and control groups are the best available in terms of age- and entitlement period-comparability.[7]

Figure 5 plots the estimated Kaplan-Meyer survival functions and resulting impact on survival probabilities given by DinD estimates.[8] On the left handside panels, we plot the survival functions for the treatment and control groups in the before and after periods. The appropriate vertical difference between each set of 4 curves gives the DinD estimates, and they are depicted in the right handside plots.

For the age groups $[15, 25)$ and $[30, 35)$ there are no naked-eye noticeable difference. Regarding the treatment group in the after period, the pattern is clearly of larger and increasing with spell survival probabilities, which become remarkbly similar to the control group's. This is confirmed by the DinD estimates. Positive impact on the probability of remaining unemployed and increasing over time. The impact ranges from a null impact at very low durations to almost 20 percent at the upper limit of the entitlement period.[9] On average, the probability

[7] Table 1 summarizes, in terms of the entitlement periods, the changes that took place between the pre- and post-1999 law.

[8] Throughout this section, in this preliminary version, we omit the standard errors of our point estimates, but we do acknowledge that our estimates have surrounding confidence intervals of positive length.

[9] We compute DinD only for the common entitlement periods, although potentially we could extend our the estimates up to the new exhaustion period by considering zero survival rates for the before-treated group in the time periods after the older exhaustion date.

of staying unemployed[10] for an individual on the $[15, 25)$ group increases 10.1% and 8.8% for the age group $[30, 35)$.

The analysis of the $[40, 45)$ age group is harder due to the different entitlement periods. Nonetheless, notice how the treatment and controls groups are very much alike in the before period – the survival functions almost perfectly overlap. Thus, apart from any changes that affected the behavior of the two groups, which we control for with a before-after difference for the control group, the difference between the before and after periods for the treatment group shall give us a good approximation of the impact of the new law on the survival rates of such individuals. The plot on the right indicates that the impact is smaller than observed for the younger groups, ranging between 0 and 10 percent, with the average impact of 6.5 percent.

Next, in the spirit of Chetty (2005), we explore the heterogeneity over the income distribution. Thus, we divide our sample by the quartiles of the average income in the 12 months preceeding unemployment. Then, for each quartile, we repeat the procedure above to obtain DinD estimates.

In Figure 6, each panel compares de DinD estimates for the 3 treatment groups by pre-unemployment income quartile. The panels plot the results for the 1st and 4th quartiles.[11] There are two possible effects when we move through the income distribution. One is the liquidity constraint effect described below and related with a stronger income effect. Accordingly, one would expect a larger impact on unemployment duration at lower quartiles if the income effect is to dominate over the substitution effect. The second possible effect is the opportunity cost of unemployment in terms of foregone earnings, clearly higher for the highest quartiles. This effect can be magnified by the cap in UB related benefits, that does not allow unemployed to be paid benefits above 3 times the minimum wage.

The results point to an increasing impact with the quartile, especially for the first and third age groups. For these age groups this seems to suggest that the substitution effect dominates the income effect. However, the impact of the additional entitlement period is clearly decreasing over the age distribution. For older workers the impact on the survival probability is smaller than the ones in the younger age group.

Another striking result from Figure 6 is the heterogeneity of the effect over the income distribution across the different age groups. In fact, in the two cases in which the substitution

[10] Given by the simple average of each DinD estimate computed at each time period.

[11] The average over each curve, including the 2nd and 3rd quartiles curves not depicted for presentational motives, are reported in Table 3.

effect seems to dominate the differences in survival probabilities between the bottom and top quartiles are larger than the one observed for the 30-35 age group, in which the results seem to conform more with the expected income effect. The gap between the two survivals is larger for those in the 40-45 age group, specially at longer durations, pointing towards a very strong substitution effect.

5.2 Re-employment wages after longer UB-subsidized periods

Now, we turn our attention to the effect on re-employment wages. In the previous section, we concluded that the new legislation induced longer unemployment spells. The effect of longer spells on wages may be either positive or negative. The sign of the effect depends on which of the following effects dominates: longer (and better) search efforts or further depreciated human capital. It is clearly an empirical issue that we address in a difference-in-differences setting.

Again we use the IIES dataset, which records the wages in the employment spell preceding unemployment and the wage in the first employment experience after unemployment. To address the issue of comparability of wages over time, we inflate all wages to 2004 levels. The estimates are then based on the logarithm of real wages.

We analyze individually each pair of control and treatment groups. For each of them, we begin by computing a DinD estimate without controlling for observable characteristics of the individuals and economic environment. Then, we refine our estimates with the inclusion of the following variables: (i) the previous job log income;[12] (ii) the duration of the subsidized unemployment spell and its quadratic term; (iii) the number of days of non-subsidized unemployment that elapsed between the date the individual stopped receiving UB[13] and the date of the first job and its quadratic term; (iv) a dummy variable controlling for the fact that a larger proportion of individuals has zero days of non-subsidized unemployment; (v) dummy variables for both the year of the job loss and the year of reemployment; (vi) a gender dummy; (vii) age and its square. Finally, we split our sample according to the previous income quartiles and recompute the DinD estimates.

Whenever there is evidence that control and treatment groups differ on observables, the inclusion of pre-treatment variables is recommended. The inclusion of post-treatment variables

[12] It is computed as the average monthly income reported in the 12 months that preceeded the second month before the unemployment spell, following the rule in the UI legislation.

[13] Either because (s)he reached the legal exhaustion date or because the individual fail to meet one of the legal criteria necessary to remain on the subsidy.

is, however, subject to discussion. In our case, the year of reemployment falls into this category. We choose to include it. Although, it is plausible that reemployment wages are affected by the treatment, there are effects arising from the business cycle that cannot be ignored. We argue that the inclusion of such dummy variables removes the 'sheepskin' effects, arising from the differences in the economic cycle as illustrated in Figure 2.

As pointed out earlier, the dataset does not contain detailed socio-demographic information. Thus, for example, we are not able to control for the education level, which carries different returns in the labor market. This is certainly a weakness, but one that we argue is mitigated by (i) the DinD methodology, and (ii) the use of the previous wages, which should combine all the information on productive characteristics of the wage earner, even those not observed by the econometrician, but available in the market. In the regression tables below, the coefficients must, however, be interpreted with this caveat in mind, and not simply as an "autoregressive" parameter. The lack of a structural interpretation does not, however, hinder our objective of identifying a causal relationship between the extended entitlement period and the reemployment wages. Our estimates are in a comparable range with those reported by Carneiro and Portugal (2005) for the Portuguese economy.

The raw DinD estimate presented in Table 4, column (1), indicates a statistically significant reduction of re-employment wages, approximately 8.6 percent, for the individuals in the age group $[15, 25)$. This estimate is cut in half, -4.3%, if we control for observable characteristics (column (2)). Among these variables, we highlight the following results. First, the two 'duration' variables that enter in quadratic form have different implications. While the effect of unemployment duration is concave, the effect of elapsed days after the end of the entitlement period is convex. The first seems to suggest that up to an ideal number of days, the additional search effort pays off, while the latter seems to suggest that jobs obtained after the entitlement period pay lower wages as time progresses (although it eventually slightly reverts; maybe it is a 'rush in' effect triggered by the loss of insurance income that leads to "bad" matches as proxied by wages). The gender effect is the standard one – women have lower (re-employment) wages than men. Experience, as proxied by age, has the standard quadratic effect: additional experience payoffs at a decreasing rate. The business cycle dummy variables are primarily significant and have the expected signs.

The remaining columns of Tables 4 report, by previous income quartile, the DinD estimates. The negative impact on wages seems to be driven by the effect observed in the upper quartiles.

Indeed, there is no significant treatment effect in the samples below the median. The estimates for the 3rd and 4th quartile of the treatment effect are both -4.9 percent. Thus, it seems that the additional subsidized search time affected negatively only those individuals who arguably have lower financial constraints (upper previous income quartiles).[14]

The same analysis is now conducted with the other two age groups. Table 5 reports the estimates for the 30 to 35 years old individuals. The last table presents the same set of results for the oldest individuals, $[40, 45)$.

The results are now somewhat different. In the middle treatment group, the effect is positive, 2.7%, if statistically less significant than the previous result (p-value of 2.8%). The no-controls regression suggested a null treatment impact. The analysis by quartile indicates that the overall results are strongly conditioned by the behavior of the individuals in the last quartile. All estimates in the first 3 quartiles are not significant, and it is only the fourth quartile estimates at 5 percent that is marginally significant with a p-value of 5.9%.

The treatment impact, as estimated by the DinD, is null for the oldest treatment group. This conclusion is valid regardless of the control variables and also for the sub-samples.

Overall, we concluded that younger individuals $[15, 25)$ did not benefit from the additional subsidized search period. This may be due to the fact that most of them have just finished school or other type of formation, and additional time spent searching for a job depreciates faster their human capital. On the other hand, for older individuals $[30, 35)$ and $[40, 45)$, the impact is either positive or null, which suggest that the effect of more search effort seems to dominate the depreciation of human capital.

6 Conclusion

We have shown that the unemployment insurance system has rather heterogenous effect across not only age groups, but also along the distribution of previous income. Inspired by the work of Chetty, Gruber and others, we explored the distribution of previous income to identify effects beyond the typically reported substitution effect of UI, arising from changes in the relative price of leisure and consumption. In particular, the hitherto rather overlooked hypothesis of income effects is entertained.

[14] One could argue that there is some reversion to the mean phenomenon. However, notice that this is not the traditional setting where such misinterpretation of regression has been pointed out (see e.g. Friedman 1992, Hotelling 1933). Furthermore, all estimates by quartiles are negative, while only the upper quartile is statistically significant. To make an analogy with Galton's work, sons of short parents get smaller (not taller).

For the Portuguese case, an UI legislation change that extended for some age groups the entitlement period, while leaving it unchanged for others, provides a quasi-experimental setting for evaluation. We highlight the following results.

More prolonged unemployment spells for young individuals. And, amongst these, those whose previous income fell in the upper two quartiles seem to be the most affected. We take this as evidence in favor of a substitution effect. The DinD estimates indicate an increase in the average unemployment survival rate of about 10 percent for individuals in the $[15, 25)$ and $[30, 35)$ age groups, and about half of that in the older group, $[40, 45)$.

In terms of reemployment wages, the DinD estimates indicate even more heterogenous effects. While the youngest cohort seemed to loose from the reform (-4.4%), the oldest group was not affected, but the middle group has apparently benefited (+2.7%). A feature common to these different age groups is that the overall result of each group seems driven by those individuals in the quartiles above the median of the previous income; the 3rd and 4th quartile for the youngest and only the 4th quartile for the middle group, $[30, 35)$.

Future paths of research include necessarily the assessment of the robustness of the results to our explicit assumptions (e.g. comparability of age groups) and to the assumptions implicit in the econometric methods (whenever testable). One promising path, and quite adequate in the current context, is the use of regression discontinuity around the sharp discontinuity points created by the upper age bounds of the treatment groups and the lower bounds of the control groups, namely, the 25, 30, 35 and 40 years old discontinuity points.

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

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

† Age at the beginning of the unemployment spell.

* For those aged 45 or older, 2 months can be added por each 5 years of social contributions during the past 20 calendar years.

Table 2: Summary statistics

Spell type	Age	Female	Spell in days	Real wages	Reemp. wages
Only unemp. benefits (UB)	598,924	598,924	598,924	120,588	120,588
	39.08	0.51	347.35	541.78	622.40
	37.00	1.00	279.00	436.50	484.07
	12.50	0.50	286.51	374.77	452.50
Only social assistance (SA)	378,489	378,489	378,489	143,381	143,294
	33.83	0.62	354.01	337.14	457.11
	31.00	1.00	327.00	290.70	390.25
	12.07	0.49	258.62	239.76	823.81
UB + Subsequent SA (SSA)	227,752	227,752	227,752	54,965	54,924
	38.47	0.59	723.51	578.97	492.36
	36.00	1.00	630.00	423.00	398.64
	12.31	0.49	314.34	522.68	612.92
Total	1,205,165	1,205,165	1,205,165	318,934	318,806
	37.50	0.60	468.68	456.19	525.70
	35.00	1.00	360.00	362.92	419.07
	12.52	0.50	337.13	371.17	673.09
Males:					
Only UB	41.37	-	369.57	819.62	713.81
Only SA	34.41	-	342.93	506.39	538.38
UB + SSA	41.73	-	787.70	977.89	579.80
Females:					
Only UB	36.93	-	326.39	563.53	533.78
Only SA	33.48	-	360.88	383.26	413.68
UB + SSA	36.23	-	679.42	636.66	440.90

Notes: (i) Summary statistics presented by type of subsidy are: Number of observations; Mean; Median; and standard deviations. The two bottom panels, referring to gender, report only mean values.

Table 3: Average impact on survival rates by quartile and age group

Age group	Mean	Mean s.d.	Min	Max
$[15, 25)$				
1st quartile	0.080	0.003	0.001	0.155
2nd quartile	0.040	0.004	-0.047	0.178
3rd quartile	0.112	0.004	-0.007	0.209
4th quartile	0.128	0.004	0.001	0.240
$[30, 35)$				
1st quartile	0.102	0.003	0.002	0.190
2nd quartile	0.077	0.003	-0.003	0.158
3rd quartile	0.098	0.003	-0.009	0.213
4th quartile	0.066	0.003	-0.018	0.175
$[40, 45)$				
1st quartile	0.034	0.000	0.001	0.049
2nd quartile	0.063	0.003	-0.013	0.193
3rd quartile	0.070	0.002	-0.008	0.132
4th quartile	0.114	0.001	0.004	0.153

Notes: (i) Values computed from series shown in Figure 6; (ii) 'Mean s.d.' stands for mean standard error.

Table 4: Reemployment wages: D-in-D impact estimate for age group $[15, 25)$ with control group $[25, 30)$

Log Reemployment wages	No controls	Controls	Quartiles			
			1st	2nd	3rd	4th
	(1)	(2)	(3)	(4)	(5)	(6)
After x Treat	-.086	-.044	-.010	-.024	-.049	-.049
	(.012)	(.011)	(.023)	(.018)	(.020)	(.025)
After	.045	.023	.036	.020	-.012	-.002
	(.008)	(.011)	(.026)	(.019)	(.020)	(.025)
Treat	-.043	.002	.0008	-.005	.036	-.017
	(.008)	(.011)	(.025)	(.019)	(.021)	(.026)
Previous avg. wage		.346	-.017	.360	.522	.443
		(.007)	(.041)	(.073)	(.052)	(.028)
Days on UB		.0005	.0003	.0002	.0004	.0007
		(.0001)	(.0002)	(.0002)	(.0002)	(.0002)
(Days on UB)^2		-2e-06	-1e-06	-9e-07	-2e-06	-3e-06
		(2e-07)	(5e-07)	(4e-07)	(4e-07)	(5e-07)
Days without UB		-.0002	-.00004	-.0001	-.0002	-.0004
		(.00003)	(.00007)	(.00006)	(.00006)	(.00007)
(Days without UB)^2		8e-08	1e-08	5e-08	1e-07	1e-07
		(2e-08)	(5e-08)	(4e-08)	(4e-08)	(5e-08)
Dummy: zero days without UB		.251	.251	.230	.233	.253
		(.008)	(.018)	(.014)	(.015)	(.018)
Female		-.076	-.115	-.118	-.061	.006
		(.005)	(.012)	(.010)	(.010)	(.012)
Age		.059	.071	.070	.107	.099
		(.013)	(.026)	(.022)	(.027)	(.039)
Age^2		-.001	-.001	-.001	-.002	-.002
		(.0003)	(.0006)	(.0005)	(.0005)	(.0008)
Constant	6.262	3.011	4.983	2.876	1.280	1.866
	(.006)	(.173)	(.396)	(.520)	(.475)	(.543)
Dummies: year of jobloss	No	Yes	Yes	Yes	Yes	Yes
Dummies: year of reemployment	No	Yes	Yes	Yes	Yes	Yes
No. observations	33,338	33,324	5,855	8,787	8,997	8,870

Table 5: Reemployment wages: D-in-D impact estimate for age group $[30, 35)$ with control group $[35, 40)$

Log Reemployment wages	No controls	Controls	Quartiles			
			1st	2nd	3rd	4th
	(1)	(2)	(3)	(4)	(5)	(6)
After x Treat	-.002	.027	.043	-.0003	.015	.050
	(.014)	(.012)	(.027)	(.020)	(.022)	(.027)
After	.006	-.028	-.021	-.014	.014	-.061
	(.010)	(.013)	(.031)	(.021)	(.024)	(.029)
Treat	.0005	-.004	-.007	.026	-.044	-.017
	(.009)	(.013)	(.029)	(.020)	(.023)	(.029)
Previous avg. wage		.413	-.008	.551	.549	.487
		(.006)	(.048)	(.061)	(.052)	(.024)
Days on UB		.0004	.0005	.0001	.0005	-.0001
		(.00007)	(.0002)	(.0001)	(.0001)	(.0002)
(Days on UB)^2		-1e-06	-1e-06	-5e-07	-1e-06	-1e-06
		(1e-07)	(2e-07)	(2e-07)	(2e-07)	(2e-07)
Days without UB		-.0002	-.0001	-.0001	-.0002	-.0004
		(.00004)	(.00007)	(.00006)	(.00007)	(.00008)
(Days without UB)^2		7e-08	7e-08	2e-08	1e-07	1e-07
		(2e-08)	(5e-08)	(4e-08)	(4e-08)	(6e-08)
Dummy: zero days without UB		.210	.188	.194	.192	.208
		(.009)	(.021)	(.015)	(.017)	(.020)
Female		-.071	-.113	-.124	-.055	.026
		(.006)	(.016)	(.010)	(.011)	(.014)
Age		-.029	.030	-.043	-.088	.021
		(.024)	(.054)	(.039)	(.044)	(.053)
Age^2		.0005	-.0004	.0007	.001	-.0002
		(.0003)	(.0008)	(.0006)	(.0006)	(.0008)
Constant	6.291	3.992	5.355	3.302	4.071	2.707
	(.007)	(.428)	(.996)	(.783)	(.835)	(.941)
Dummies: year of jobloss	No	Yes	Yes	Yes	Yes	Yes
Dummies: year of reemployment	No	Yes	Yes	Yes	Yes	Yes
No. observations	28,007	27,996	4,055	7,435	7,743	7,808

Table 6: Reemployment wages: D-in-D impact estimate for age group $[40, 45)$ with control group $[35, 40)$

Log Reemployment wages	No controls	Controls	Quartiles			
			1st	2nd	3rd	4th
	(1)	(2)	(3)	(4)	(5)	(6)
After x Treat	-.020	.009	.019	.010	.040	-.003
	(.015)	(.014)	(.031)	(.022)	(.024)	(.029)
After	.006	-.022	-.043	.010	-.002	-.040
	(.010)	(.014)	(.034)	(.022)	(.025)	(.030)
Treat	.006	-.019	-.010	-.023	-.007	-.026
	(.010)	(.014)	(.032)	(.022)	(.024)	(.029)
Previous avg. wage		.403	.053	.349	.550	.384
		(.006)	(.053)	(.066)	(.056)	(.025)
Days on UB		.00003	.0003	.0001	.0002	-.0005
		(.00006)	(.0001)	(.0001)	(.0001)	(.0001)
(Days on UB)^2		-5e-07	-5e-07	-3e-07	-7e-07	-1e-07
		(9e-08)	(2e-07)	(1e-07)	(1e-07)	(1e-07)
Days without UB		-.0001	-.0001	-.00008	-.0002	-.0003
		(.00004)	(.00009)	(.00007)	(.00007)	(.00008)
(Days without UB)^2		2e-08	5e-08	3e-08	1e-07	4e-08
		(2e-08)	(6e-08)	(4e-08)	(5e-08)	(6e-08)
Dummy: zero days without UB		.219	.186	.195	.198	.229
		(.010)	(.024)	(.017)	(.018)	(.022)
Female		-.084	-.111	-.115	-.066	-.009
		(.007)	(.017)	(.011)	(.012)	(.015)
Age		-.012	.029	.041	.041	-.107
		(.031)	(.070)	(.051)	(.054)	(.067)
Age^2		.0002	-.0004	-.0005	-.0005	.001
		(.0004)	(.0009)	(.0006)	(.0007)	(.0008)
Constant	6.291	3.683	5.045	2.881	1.716	5.918
	(.007)	(.615)	(1.423)	(1.089)	(1.132)	(1.337)
Dummies: year of jobloss	No	Yes	Yes	Yes	Yes	Yes
Dummies: year of reemployment	No	Yes	Yes	Yes	Yes	Yes
No. observations	22,895	22,883	3,159	5,992	6,394	6,391

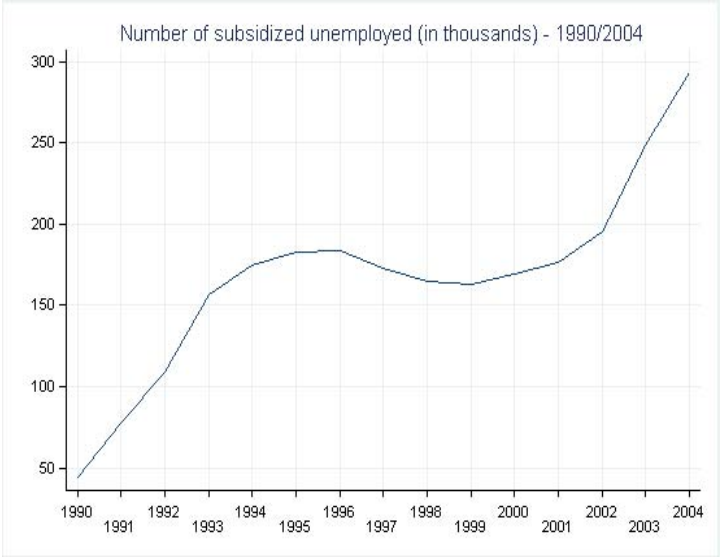

Figure 1: Number of unemployed receiving unemployment insurance, in thousands, 1990-2004

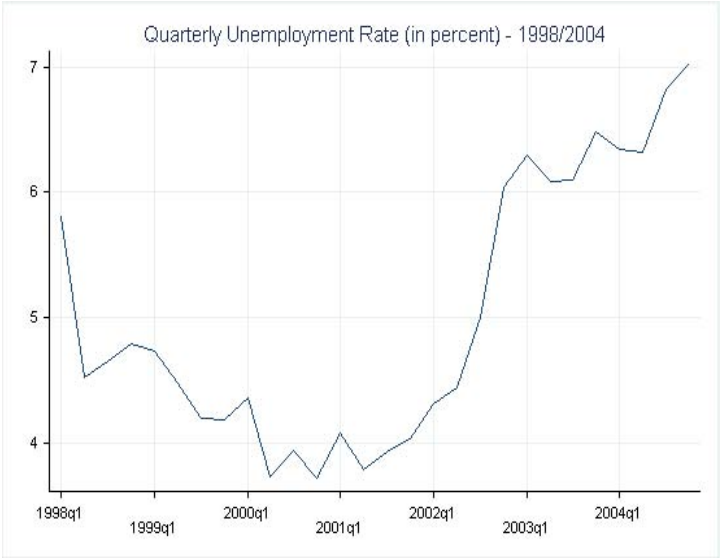

Figure 2: Quarterly unemployment rates (percent), 1998q1-2004q4

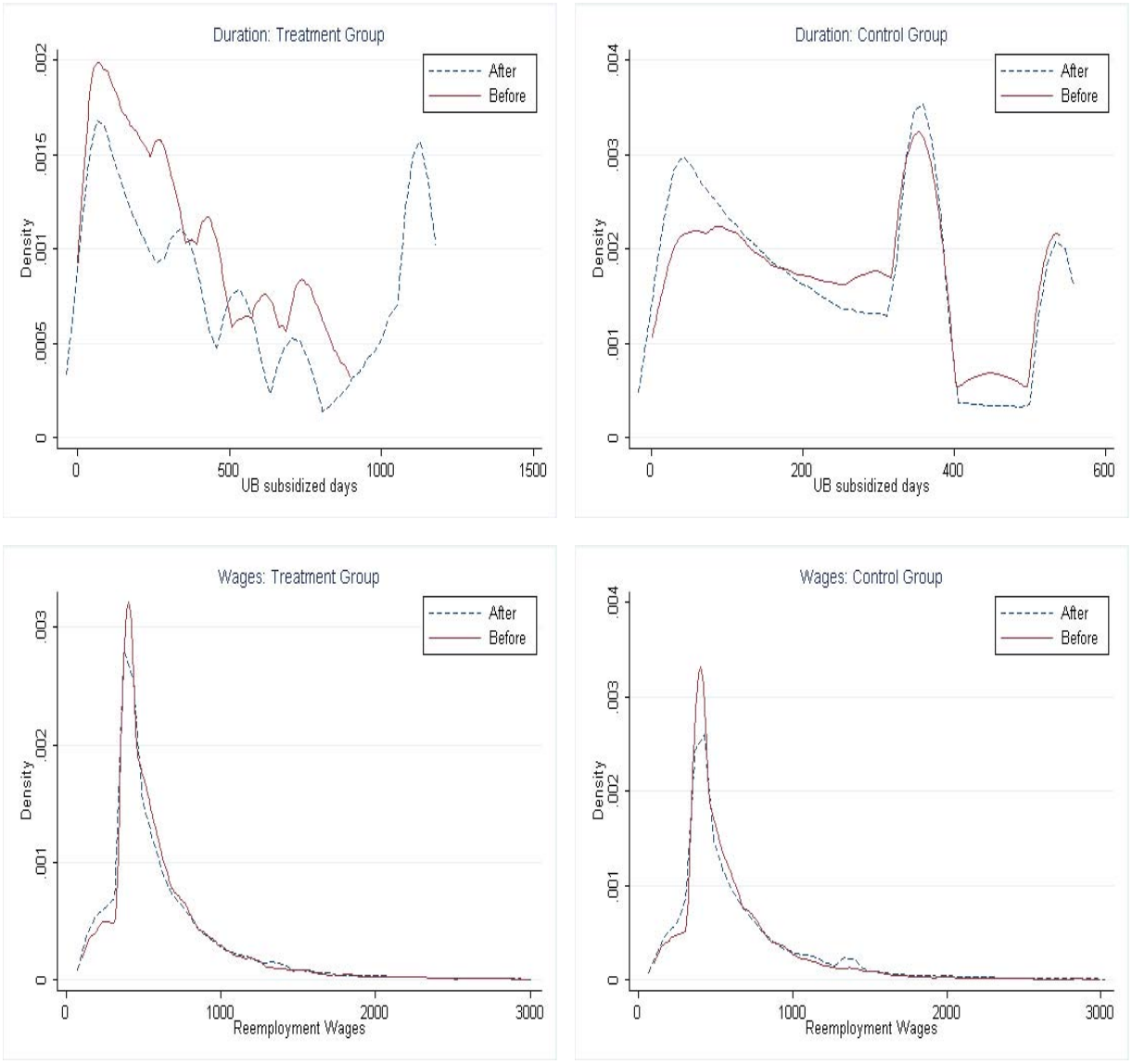

Figure 3: Kernel density estimates: Duration and reemployment wages by treatment and control groups before and after the 1999 law.

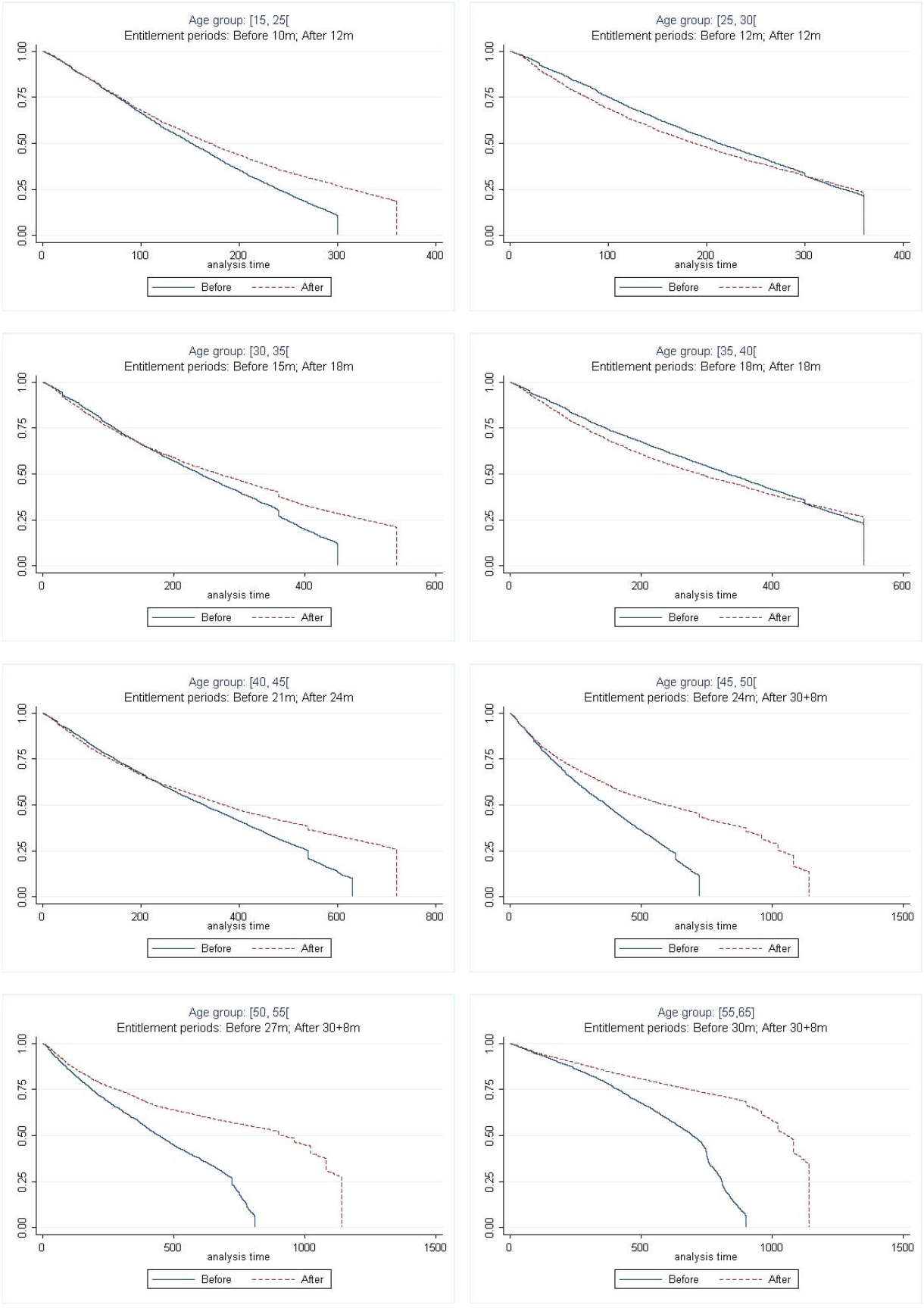

Figure 4: Survival fuctions: Kaplan-Meyer estimates

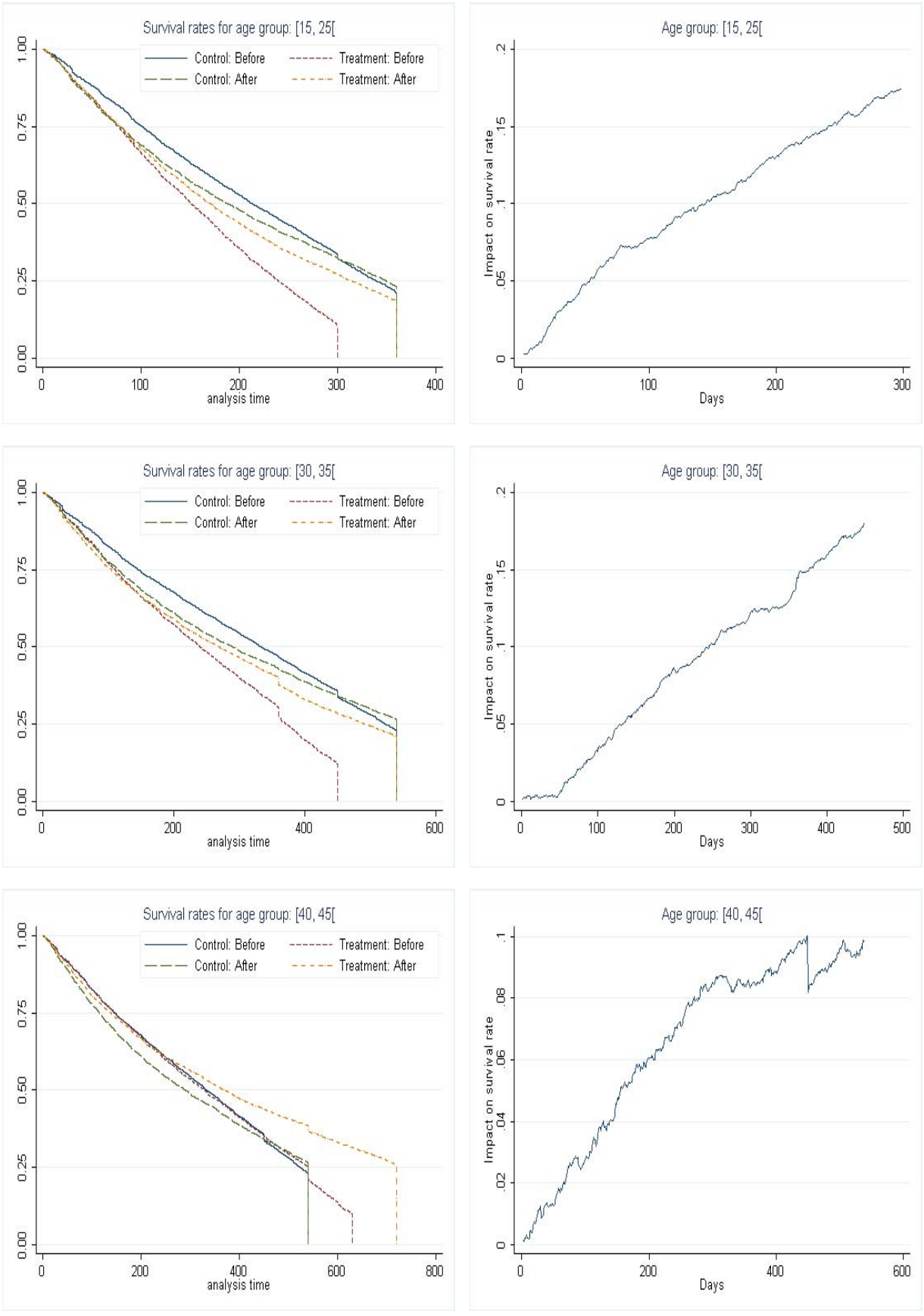

Figure 5: Survival functions (Kaplan-Meyer) and DinD estimates

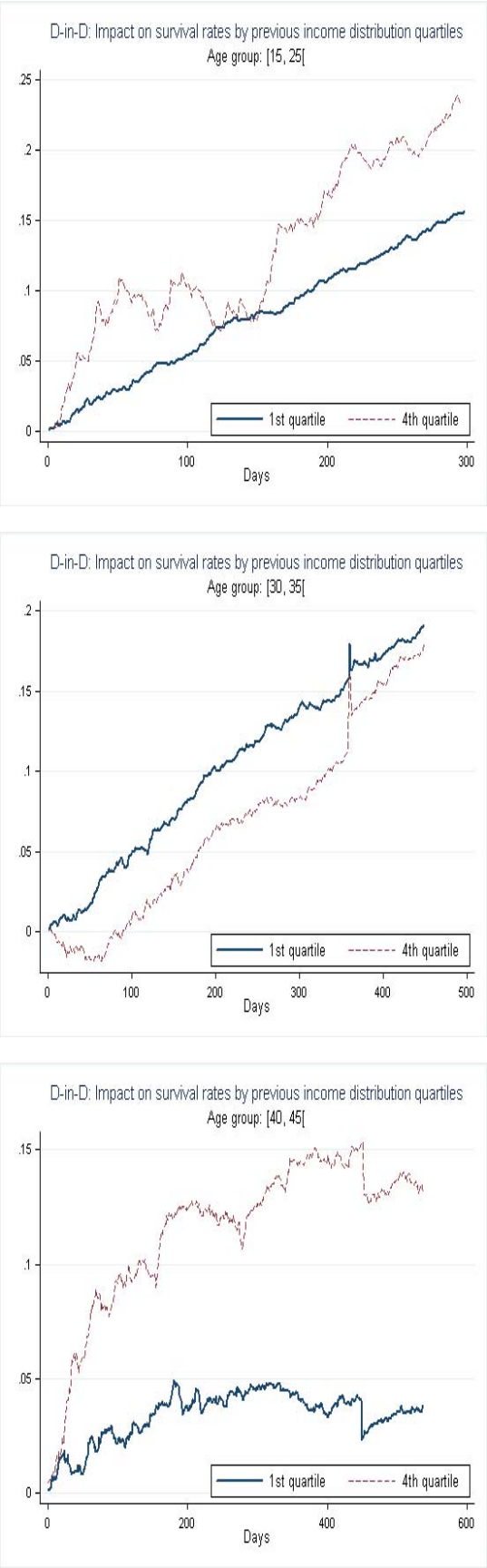

Figure 6: DinD estimates: Impact on survival rates for the treatment (age) groups for 1st and 4th quartiles of previous wages distribution