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

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Abstract

It has been recently aknowledged that the impact of the unemployment insurance system can be very heterogeneous. We present evidence of this heterogenity for two important labor market outcomes related: unemployment duration and reemployment wages. We exploit a quasi-experimental setting and show that longer spells of unemployment, arise 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 insurance 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 disincentives created by UI along these lines can be found in the seminal paper by Shavell and Weiss (1979) and subsequent work on the optimal design of UI, for example, in Hopenhayn and Nicolini (1997) or Fredriksson and Holmlund (2001). Empirical analyses of the effect of UI on labor supply decisions, namely, on the duration of unemployment spells can be found in Katz and Meyer (1990) and, more recently, in the work of van Ours and Vodopivec (2006).

The impact of the UI system cannot, however, be reduced to its distortionary effect on labor supply decisions. In fact, different theoretical and empirical approaches have considered the benefits of the UI system, both in terms of welfare during the unemployment spell (for example, the consumption smoothing argument) and in terms of post-unemployment outcomes (for example, via higher reemployment wages or longer match duration). Examples of this literature include the models by Acemoglu and Shimer (2000) and Marimon and Zilibotti (1999), who consider the productivity gains obtained from more generous UI, and the empirical analysis by Gruber (1997) on consumption, and Belzil (2001), Centeno (2004) and Centeno and Novo (2006) on employment stability after unemployment.

A different approach looks at heterogenous effects over the income distribution and has been recently explored by Chetty (2005). The basic argument rests on the distinction between income and substitution effects. Indeed, the total effect of UI on unemployment duration includes a substitution effect - through the relative price of leisure - and an income effect - affecting more strongly poor individuals, those who find more difficult to smooth consumption across labor market states. The income effect operates in the same direction as the substitution effect but in an heterogenous way - the higher the income effect the lower the exit rate from unemployment. Chetty's results show that more constrained individuals (those with net wealth below the median) react strongly to an increase in UI generosity, increasing their unemployment survival rates. The effects on financially unconstrained unemployed are non-existent.

In this paper, we use a quasi-experimental setting generated by an exogenous increase in

UI generosity, to identify the causal effect of an extension of the entitlement period on the behavior of subsidized unemployment, both in terms of longer unemployment durations and potential gains in post-unemployment wages. We also acknowledge the possibility of heterogeneous effects along the income distribution and study the differential impact of UI generosity on individuals with income above and below the median. The exogenous variations on the UI system generosity comes from the July-1999 reform of the Portuguese UI system. The reform increased the UI entitlement period for a fraction of the unemployed, while keeping it constant for other groups of the population. This increase in the entitlement period is thought to be equivalent to a pure income effect, maintaining constant the per period value of the unemployment benefit. We interpret the response of unemployed in different parts of the income distribution, but facing roughly constant gross replacement rates, to capture the above mentioned income effect.

We use standard causal effects evaluation techniques to address this problem. We explore the fact that the new UI entitlement period became substantially more generous for a large fraction of workers and it remained the same for workers in two age groups (25 to 30 and 35 to 40). We explore this in a quasi-experimental setting, considering the latter groups as control groups and all other workers as members of the treatment groups. The availability of pre- and post-1999 information allows us to use a difference-in-differences (DinD) methodology. [1]

We use Portuguese administrative data from the Social Security Unemployment Insurance dataset covering *all* subsidized unemployment spells that occurred during the 1998-2004 period. Several characteristics of this data set make it particularly suitable to our study, namely the availability of information on (i) the salary and starting date of the first job following unemployment; (ii) spells initiated both in the period prior to and after the July 1999 reform of the UI system; and (iii) the wage earned prior to entering unemployment.

As mentioned, a large number of papers documented the sensitivity of subsidized unemployment to the generosity of UI. In a recent paper, van Ours and Vodopivec (2006) show that unemployed workers have larger exit rates from unemployment the less generous the UI system is. They also present evidence of an increasing exit rate of unemployment as UI approaches the expiration date. The evidence on heterogeneity of UI impact is more scant. Gruber (1997) and Browning and Crossley (2001) show evidence that the more liquidity constrained individuals

[1] This setting is also a natural one to aplly a regression discontinuity type of analysis. We expect to include this in future versions of this paper.

benefit the most from the UI generosity in terms of consumption changes in the unemployment state. These results are related with those in Chetty (2005), suggesting that UI raises durations primarily because of an income effect, induced by the inability to save, rather than by moral hazard motives resulting from distorted incentives.

Our preliminary results confirm the idea that when facing longer entitlement periods unemployed individuals usually take them up, remaining unemployed for longer periods. The identification of the income effects points towards a significant heterogeneity among age groups and within these groups across pre-unemployment income quartiles. The extension of the entitlement period seems to prolong unemployment spells, but specially for young individuals and the impact is larger at longer durations. Amongst these, those with previous wage income in the upper half of the distribution seem to be the most affected. The quasi-experimental setting allowed us to obtain 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, $[40, 45)$, the three treatment groups.

In terms of reemployment wages, the DinD estimates indicate 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 wage income; the 3rd and 4th quartile for the youngest and only the 4th quartile for the middle group, more in the age range $[30, 35)$.

Taken together, these results seem to indicate that longer subsidized unemployment spells are not associated with higher reemployment wages. This is, in a way an unexpected result, since longer unemployment spells could result in higher wages if they allow workers to look for better matches. A possible explanation for this result can be that the Portuguese UI system induces too long unemployment spells, such that the negative effects of the UI on the transition out of unemployment prevail (for example, one can think of stigma or low attachment to the labor market).

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 using 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 trade-off faced by program administrators: this trade-off 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 theoretical models by Acemoglu and Shimer (2000) and Marimon and Zilibotti (1999) show that more generous unemployment benefits can increase match quality. The two models show that the increase in UI translates into productivity gains.

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 (Belzil (2001), Centeno (2004) and Centeno and Novo (2006)) and on its effect on smoothing consumption during unemployment spells (Gruber 1997).

From a theoretical point of view most results can be derived from a standard Mortensen (1977) type of search-model. The simple result of observing longer unemployment spells as a response to increased generosity (the substitution effect of UI) 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 heterogenous 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 disincentive of UI created

through the substitution effect can be reduced, and become less distortionary than previously thought. We use the quasi-natural experiment to identify this income effect. For that we interpret the extension in the entitlement period as equivalent to a lump-sum increase in the income of the unemployed. They are receiving the same price per leisure (same per period unemployment benefit), but at any given point in time are entitled to a present value of unemployment benefits larger than in the previous regime of the unemployment insurance system. Therefore, given that the relative price of consumption and leisure did not change, we can identify a pure income effect.[2]

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.

Chetty (2005) gathers evidence in favor of such interpretation by analyzing a sample of American households divided into groups of financially constrained and unconstrained agents. He finds that unemployment benefits have large effects on unemployment spells of the former group, but only a small effects on the latter group. Furthermore, severance payments awarded to constrained households increase subsequent unemployment spells.

Acknowledging the presence of this heterogeneity on unemployment duration in response to UI 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.

3 The unemployment system reform and identification

The Portuguese unemployment insurance system was created in 1985. At the time, it was neither very generous nor had a significant take-up rate (see Figure 1). The system has,

[2] Arguably, the relative price might have changed through variation in the price of consumption. But, in our setting, that is irrelevant because it changed equally for the treatment and control groups.

however, been made more generous since that initial period. The subsequent reforms changed the duration of the entitlement period, leaving the level of the benefits almost unchanged. The Portuguese UI system has three main forms of subsidies. The unemployment benefits is the more general one, but it is complemented by a social assistance benefit and a subsequent social assistance benefit that follows a period of unemployment benefit recipiency.

The unemployment benefits (UB) legislation establishes only one eligibility criterium, namely, the employment history with social contributions, requiring a minimum 540 days of contributions in the previous 24 months to unemployment. One peculiar feature of the Portuguese system is the definition of the entitlement period. It is fully determined as a function of the individuals' age at the beginning of the unemployment spell and is orthogonal to the length of past contributions. It was precisely the entitlement period that was changed in July 1999, for several age groups in the population.

Before the reform, 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 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 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 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 security contributions, 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, there with longer contributory spells.

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 pre-reform 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. The entitlement period for individuals in the (40-45) age groups was also increased. These changes make the choice of a treatment and control group very natural We will define as treated all individuals in age groups (1), (3) and (5), respectively aged less than 25, 30-35 and 40-45. The control group would be made of individuals in age groups (2) and (4).We restrict our attention to individuals with less than 45 years to the lack of a suitable control group for older workers and also because the UI legislation induces a set of provisions specific to older workers that are related with pre-retirement schemes, and that make unemployment duration analysis made more difficult.

Another helpful feature of the reform 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 very detailed and reliable 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 un-

[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.

employment benefits only, 378,489 were paid social assistance and 227,752 received unemployment benefits during the complete entitlement period and later on benefited from subsequent social assistance. Table 2 contains summary statistics of the key variables by type of subsidy. At the beginning of the unemployment spell the average individual receiving unemployment benefits is 39 years old and 5 years younger if benefiting from social assistance. 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 market, namely, that unemployment and long-term unemployment incidence is larger among women and that women have shorter contribution careers. Therefore, it is natural that more women benefit from the SSA and social assistance related benefits.

The average unemployment spell lasts one year, both for unemployment benefits and social security recipients. For those benefiting from SSA this average more than duplicates, with an average of 723 days (2 years).

The bottom panels of Table 2 disaggregate the mean values by gender. Males starting a spell of subsidized unemployment 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. The differences between average pre-unemployment income 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 unemployment benefits, then leaving aside social assistance benefits. The reason for this is twofold. Firstly, individuals receiving SA are much more heterogeneous and display a weaker attachment to the labor force. Whereas these characteristics are worth studying, they are not central in our empirical questions here. Secondly, those individuals in SSA have quite long unemployment spells and they experience a decrease

in the benefit amount after the UB spell. Again, looking at these spells has a lot of merits but it is out of the scope of this paper.

5.0.1 A bird's eye view

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

Figure 3 depicts kernel density estimates for the unemployment duration and reemployment wages, by treatment status, before and after the reform. 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). We observe that after the reform a much smaller transition of unemployment spells terminated within the first 1000 day. In the control group (top right panel), we observe a substantial similarity 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 densities. Thus, if only preliminarily, we expect to see substantial effects on the duration of subsidized unemployment whereas the effects on reemployment wages would be governed by the individual distribution of wages on the labor market, and the impact is not noticeable at this level of analysis.

Although rather general, this initial analysis motivates the analysis of heterogeneous effects, in particular along the dimensions of our control variables, namely age and income.

5.1 Unemployment spells duration

we consider all unemployment spells that received UB support and that either ended in reemployment or that exhausted 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).

[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 (see Levine (1993)), but this should also be related with the better economic condition prevailing in late 1999 and 2000.

Plots in Figure 4 present Kaplan-Meyer estimates of the two survival functions for each age group. 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 considered in our analysis are complete.

A general, if preliminary, pattern emerges: while the control groups' survival rates have not been significantly affected, and if anything they decreased, the survival rates of the treated groups have clearly 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 changes in the entitlement period lead, on average, to variations of opposite sign in the duration of the 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, this explains the huge shift is survival for workers in the top age groups.

The period under analysis, 1998 through 2004, 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, as shown in Figure 2. It is, therefore, possible that some of the changes observed within each group between the two periods (legislations) are due to the evolution of the business cycle. We use the difference-in-differences (DinD) estimation strategy to control for such common (and unobserved) characteristics.

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

1. $[15, 25)$, 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;

2. $[30, 35)$ 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;

3. $[40, 45)$, 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, after the reform, is smaller, only 18 months.

Thus, we argue that the first two pairs of treatment and control groups are the best available in terms of age- and entitlement period-comparability.

Figure 5 plots the estimated Kaplan-Meyer survival functions and resulting impact on survival probabilities given by DinD estimates.[6] 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 panels.

For the age groups $[15, 25)$ and $[30, 35)$ there are no naked-eye noticeable difference. Regarding the treatment group in the post-reform period, the pattern is clearly larger and increasing with spell survival probabilities, which become remarkably similar to the control group's. This is confirmed by the DinD estimates. Positive impact on the probability of remaining unemployed and increasing with spell duration. The impact ranges from almost zero at very low durations to close to 20 percent at the upper limit of the entitlement period.[7] On average, the probability of staying unemployed[8] for an individual on the $[15, 25)$ group increases 10.1 percentage points and 8.8 percentage points 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 pre-reform 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 pre and post-reform 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.

[6] 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.

[7] We compute DinD only for the common entitlement periods, although potentially we could extend our 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.

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

5.2 Heterogeneity in UI impact

Next, in the spirit of Chetty (2005), we explore the heterogeneity over the pre-unemployment wage distribution. Thus, we divide our sample by the quartiles of the average wage in the 12 months proceeding 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.[9] There are two possible effects acting in the same direction as we move through the income distribution. One is the liquidity constraint effect described above and associated with a stronger income effect. Accordingly, one would expect a larger impact on unemployment duration at lower quartiles if the income effect is important. 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 existing cap in UI 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 wage quartile, especially for the first and third age groups. Notice, however, that 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 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.

5.3 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 search spells on wages may be either positive or negative. The sign of the effect depends on

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

which of the following effects dominates: longer (and better) search efforts or further depreciated human capital and weaker attachment to the labor market. 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 log of the previous wage;[10] (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[11] and the date of the first job and its quadratic term; (iv) a dummy variable controlling for 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 wage 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 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

[10] 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.

[11] 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.

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 and obtained within a completely different dataset dual exercise.

The raw DinD estimate presented in Table 4, column (1), indicates a statistically significant reduction of re-employment wages, approximately 8.6 %, for the individuals in the age group $[15, 25)$. This estimate is cut in half, -4.4%, 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 although this is not true at the top wages quantile, a result that is also in accordance with other results in the Portuguese economy (see Vieira et al. 2005). 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 wage 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 wage income quartiles).[12]

The same analysis is now conducted with the other two age groups. Table 5 reports the

[12]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).

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. For individuals aged 30-35, 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 top 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 (see Table 6). 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, and get lower wages after reemployment. This may be due to the fact that most of them have just finished school or other type of training, and additional time spent searching for a job depreciates faster their human capital in the context of a weak attachment to the labor market. On the other hand, for older individuals $[30, 35)$ and $[40, 45)$, the impact is either positive or null, in terms of wages, but again with longer unemployment spell which suggest that the effect of more search effort seems to, at least cancel out with 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.

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 specially for young individuals. And, amongst these, those whose previous wage 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 lose 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 wage 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	$[45, 65)$	$30(+8)^*$
(7)	$[50, 55)$	27		
(8)	$[55, 65)$	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

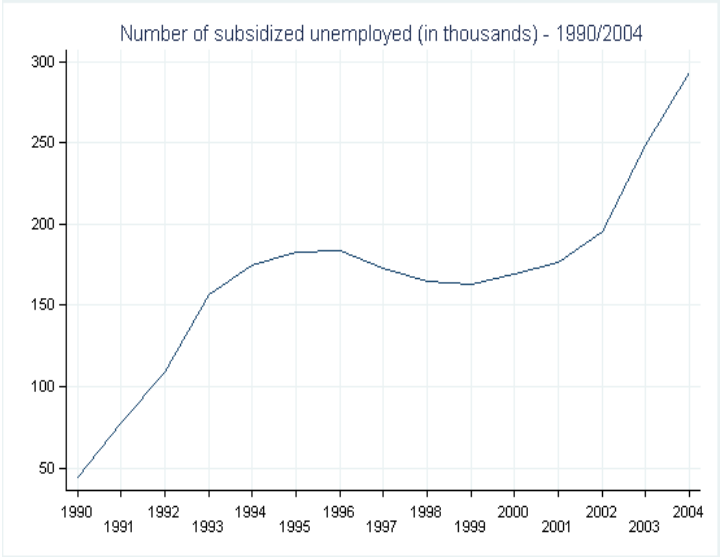


Figure 1: Number of unemployed receiving unemployment insurance, in thousands, 1990-2004

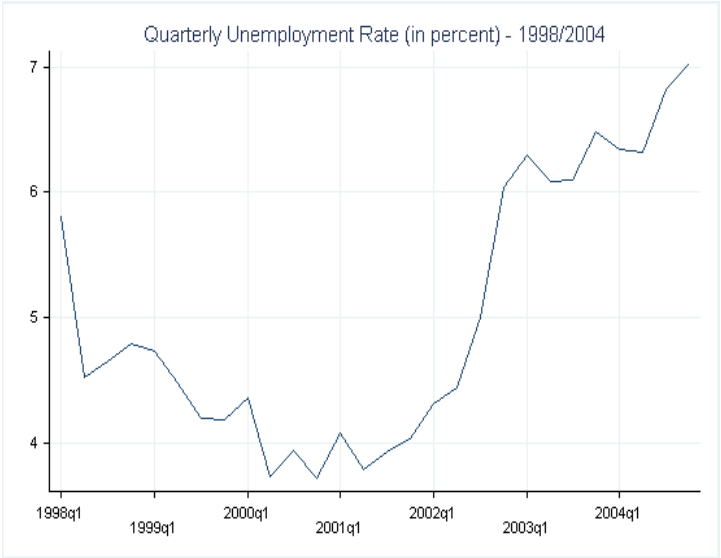


Figure 2: Quarterly unemployment rates (percent), 1998q1-2004q4

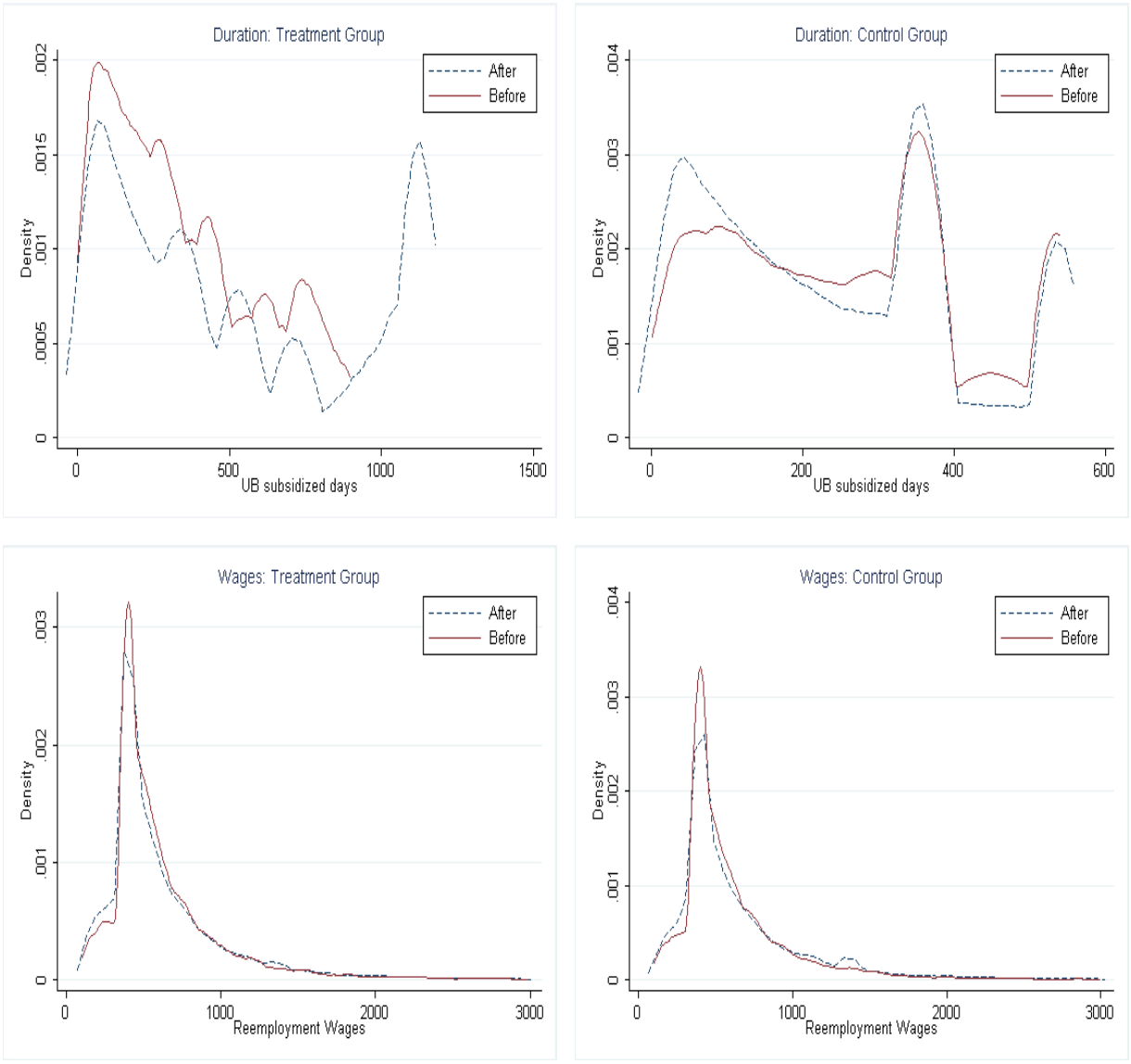


Figure 3: Kernel density estimates: Duration and reemployment wages by treatment and control groups before and after the 1999 law.

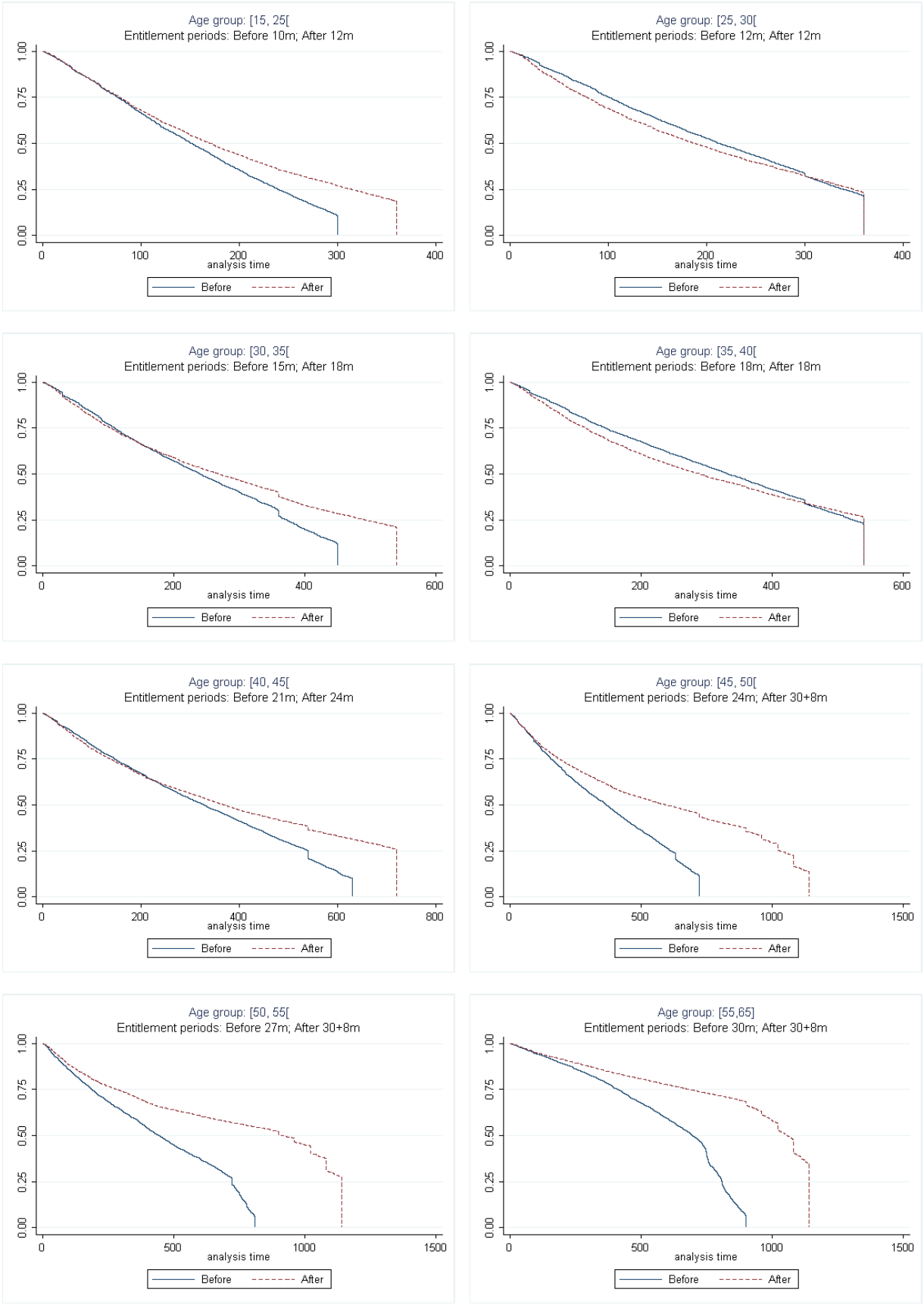


Figure 4: Survival fuctions: Kaplan-Meyer estimates

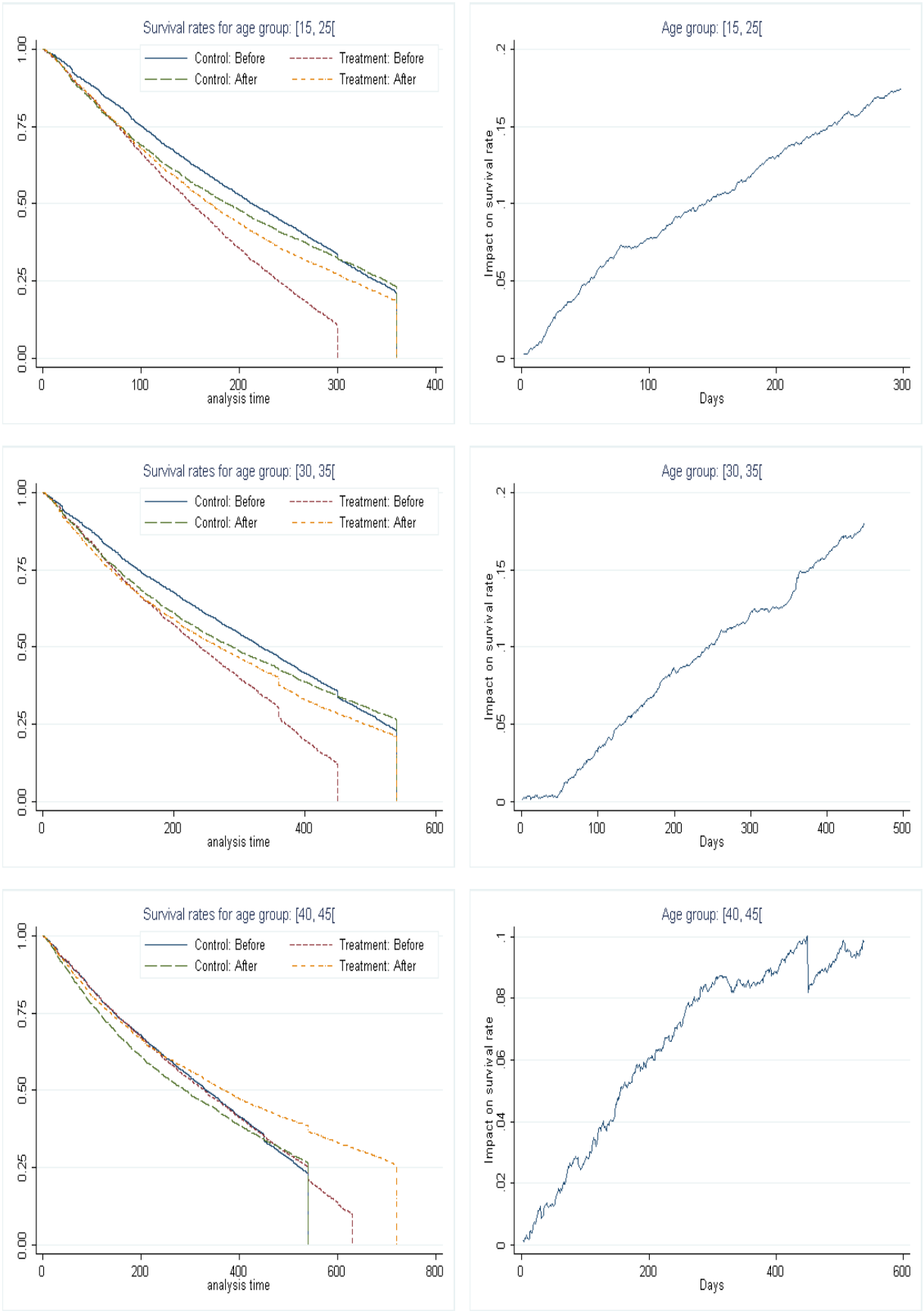
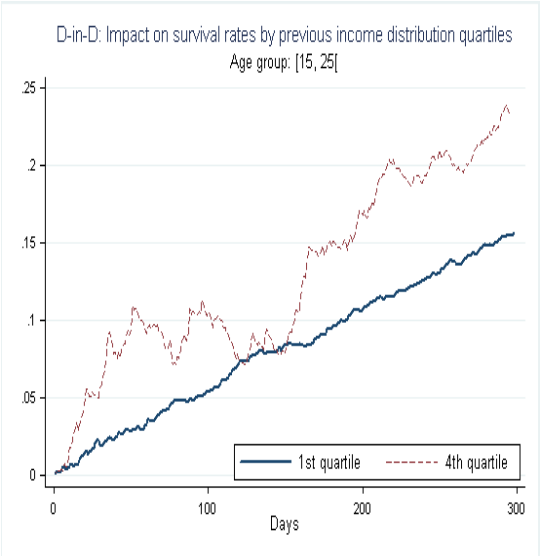
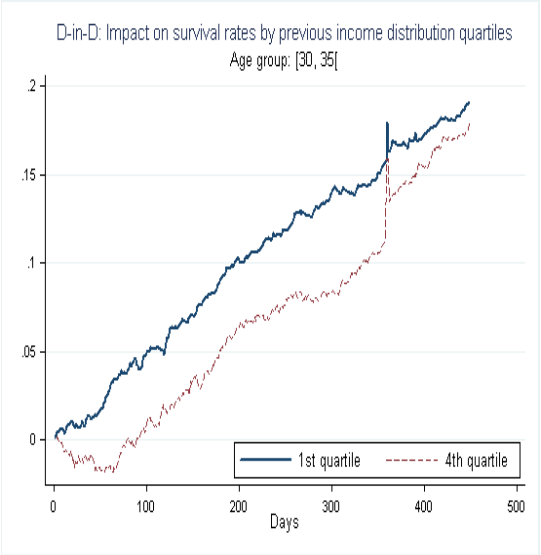


Figure 5: Survival functions (Kaplan-Meyer) and DinD estimates





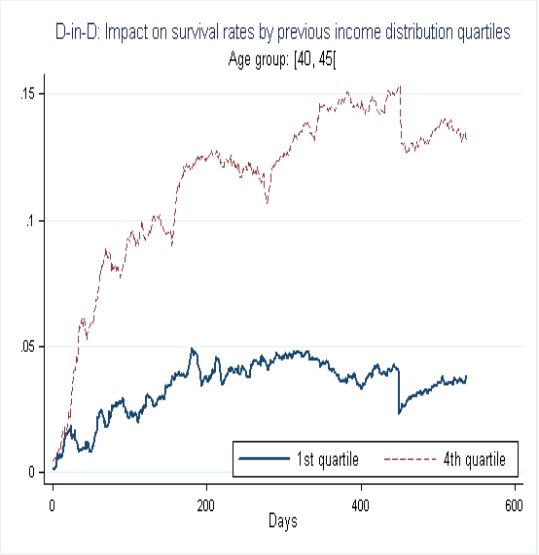


Figure 6: DinD estimates: Impact on survival rates for the treatment (age) groups for 1st and 4th quartiles of previous wages distribution