

# **When the Going Gets Tough... Reducing Benefits in the Aftermath of the Great Recession**

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## **Abstract**

Using social security data and a Differences-in-Differences approach, we evaluate the impact of a 2012 Spanish reform that reduced the replacement rate from 60 to 50 percent for unemployment spells lengthier than 180 days. We find that reducing the replacement rate by 10 percentage points (or 17 percent) after 6 months of unemployment increases the job finding rate of affected workers by at least 23 percent relative to similar workers who were not affected by the reform. In addition, we find that the effects occur before the RR has dropped, suggesting that displaced workers modified their job search before their benefits were actually reduced. Finally, the evidence does not suggest that a year after losing their job, the reform pushed these workers to accept more precarious jobs. These results are robust to: (1) controlling for the seasonality of the summer months, (2) the use of alternative control groups, (3) alternative specifications (including triple differences), (4) clustering of standard errors, and (5) placebo tests.

**Key words:** Labor supply, incentives, unemployment insurance replacement rate, hazard function models, wages, and longitudinal social security data.

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## 1. Introduction

Traditionally, when labor market conditions are expected to deteriorate, governments expand unemployment insurance (UI) benefits to ease displaced workers' economic pain and maintain their consumption (Moffit 2014). However, in the aftermath of the Great Recession, the fears of the European sovereign-debt crisis led the European Commission to recommend a *decrease* in the generosity of the unemployment insurance (UI) benefits as one of a series of austerity measures aiming at slashing spending and raising taxes (European Commission 2012). Since then, France, Hungary, Ireland, Portugal, Slovenia, Netherlands, and Spain, just to name a few countries, have reduced their UI benefits generosity. Nonetheless, very little is known on what are the causal effects of a *reduction* in the level of UI benefits in relation to expected earnings (the replacement rate, RR hereafter) on: (1) the transition to employment (short-run effects), as well as, (2) subsequent job earnings, job stability and promotion (medium-run effects) within a context of economic slowdown. This is the main objective of this paper.<sup>1</sup>

In comparison to the vast literature on the effects of changing the UI entitlement length, quasi-experimental evidence on the effects of changing the RR is relatively modest and focuses mainly on reforms that took place before the turn of the century.<sup>2,3</sup> To the best of our knowledge, this is the first paper to estimate the causal effect of an unexpected *decrease* in the RR after the Great Recession, a highly policy-timely issue.<sup>4</sup> Perhaps more importantly, as the drop in the RR occurs *not* at the beginning of the unemployment spell but 6 months afterwards, we are able to identify whether it changed displaced workers' search behavior *before* their benefits were actually reduced.

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<sup>1</sup> To the best of our knowledge, *only* Carling et al, 2001, analyze the impact of a *reduction* in the RR from 80 to 75 percent (representing 6.25 percent decrease) in January 1 1996 in Sweden at a time of fiscal austerity and economic slowdown.

<sup>2</sup> See Van Ours and Tatsiramos, 2012, for a recent overview on the effects of changing the UI entitlement length on unemployment and employment duration.

<sup>3</sup> The only exception is Eugster, 2013, that studies a 2003 Swiss reform targeting low-wage earners. All the other studies analyze reforms occurring in the last century. Lalive et al, 2006, and Meyer and Mock, 2007, analyze 1989 reforms in Austria and New York state, respectively. Carling et al 2001 study a reform in Sweden in 1996. Finally, Roed and Zhang, 2003 and 2005, exploit random-assignment-like variation in RR in Norway in the 1990s and Bover et al, 2003, exploit a 1984 reform and compare receivers and non-receivers in Spain during the late 1980s and early 1990s.

<sup>4</sup> Several recent papers have analyzed the effects of dramatically *expanding* UI benefits across the US after the Great Recession. Using Current Population Survey data and time, state and individual variation, Farber and Valletta, 2011, and Rothstein, 2011, find small negative effects of expanding UI benefits on the probability that the eligible unemployed would exit unemployment, but no effects on the probability of entering employment. These effects are concentrated among the long-term unemployed.

We exploit a Spanish policy reform implemented on July 15, 2012 that reduced the RR by 10 percentage points (or 16.66 percent). On July 13, 2012, the Spanish Government announced that all workers whose unemployment spell began on July 15, 2012 would have their RR after 180 days of unemployment spell reduced from 70 percent to 50 percent -- prior to this reform, the reduction went from 70 percent to 60 percent. Our empirical strategy uses Differences-in-Differences approach (DiD hereafter) and Social Security records longitudinal data from the *Continuous Sample of Working Histories* (hereafter CSWH). An important advantage of this dataset over survey data is that non-response bias, recall bias and bunching of the job finding rate at 26 and 52 weeks are not an issue. An additional advantage of this dataset over UI register data is that we continue to observe individuals after exhaustion of UI benefits, which allows to study how the job finding rate and other post-unemployment characteristics evolve after the exhaustion of benefits. In addition to eligibility status and UI receipt, the CSWH provides important socio-economic characteristics, and reports the work history of individuals since they first entered the labor market allowing us to trace workers' employment and unemployment histories over an extended period of time. For our analysis, we compare the inflow to the unemployment registers from January 1, 2012 to July 14, 2012 (before the reform) to the inflow that took place from July 15, 2012 to December 31, 2012 (after the reform) relative to the changes that took place a year earlier, thus obtaining a DiD estimate. We observe these workers' employment histories up until December 31, 2013.<sup>5</sup> Because the reform under study generates exogenous variation in the RR, it is independent of individuals' unobserved heterogeneity, and thus allows to get valid inference about the causal effect of unemployment benefits.

We find that reducing the RR by 10 percentage points (or 17 percent) increases the job finding probability of affected workers by at least 23 percent relative to similar workers who were not affected by the reform. This implies an elasticity of unemployment with respect to the UI benefit level of 0.22, which is close to Lalive et al, 2006, (0.15) but small in relation to the literature discussed in Section 2. We also find that the effect takes place *before* the drop in the RR, suggesting an anticipatory job search behavior among the unemployed in Spain. Finally, the evidence does not

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<sup>5</sup> While we observe individuals up until March 31, 2014 because this is contractual administrative data that is due with a one semester lag, we prefer not using the data after December 31, 2013 as much of the data after this date may be incomplete.

suggest that a year after losing their job, the reform led these workers to accept more precarious jobs. Hence, because of the employment effect, the reform increased, on average, their wages by 22 percent. These results are robust to: controlling for the seasonality of the summer months, (2) the use of alternative control groups, (3) alternative specifications (including triple differences), (4) different clustering of the standard errors, and (5) placebo tests.

The policy change took place in the aftermath of the Great Recession in Spain, a country well known for its high unemployment rate (over 26 percent) and highly segmented labor market (with more than one third of wage and salary workers with fixed-term contracts). The Spanish economy had suffered a major reverse since the Great Recession, with the burst of the real-estate bubble, a failing banking system, a lack of liquidity and loans for firms, and a rigid labor market having driven the economy to a double recession within four years. Because this policy was implemented in the midst of low productivity and soaring government budget deficit, our analysis is less subject to endogenous policy bias than other studies. As explained by Lalive et al, 2006, "*endogenous policy bias arises when more generous unemployment insurance rules are implemented in anticipation of a deteriorating labor market. Such a policy bias has been found important in several recent studies (Card and Levine, 2000; Lalive and Zweimüller, 2004).*" Since the reform under study was implemented at a time in Spain where the future of the Spanish economy was extremely uncertain, one would had expected policy makers to increase, *not* decrease, the RR. If anything, our estimates would represent a lower bound of the true policy effect.

Our study is close to that of Carling et al, 2001, in that authors analyze the effect of a *decrease* in the RR from 80 to 75 percent in Sweden in 1996. However, it differs in at least four ways. First, the size of the benefit cut was almost threefold in Spain relative to Sweden (17 versus 6 percent). Second, the unemployment rate in Sweden was less than half that of Spain (9 versus 23 percent). Third, since in Sweden the reduction of the RR took place on January 1, 1996 regardless of when the unemployment spell had started, identification comes from comparing workers affected by the cut in benefits with those unaffected. This implies that the comparison group are workers with high pre-displacement earnings and who were unaffected because they hit

the maximum benefit level allowed both before and after the reform.<sup>6</sup> In contrast, we use workers with similar potential UI benefit level as a comparison group.<sup>7</sup> Fourth, we also analyze the effects on other outcomes including post-displacement wages and tenure.

Finally, the paper also relates to Lalive et al, 2006. These authors study a 1989 reform in Austria that *increased* the RR for a group of unemployed workers, expanded potential UI duration for another group, increased both the RR and the potential duration for a third group, and had no effect on UI benefits for a fourth group. While the size of the RR change is similar in both studies (15 versus 17 percent), they analyze an increase in the RR during a period of strong GDP and employment growth and low unemployment rate (5 percent). Perhaps more importantly, in their study the increase in the RR affected *only* low-income workers, that is, those earning between 5,000 ATS and 10,000 ATS (or the equivalent of 30 to 62 percent of the median monthly income).<sup>8</sup> In contrast, our reform more broadly affected wage and salary workers in Spain as those displaced from jobs with a monthly salary between €20 and €1800 (if the worker had no children) and €1,100 and €2,100 (if he or she had children) were affected.<sup>9</sup> Additionally, since the drop in the RR takes place after 6 months of unemployment, we can test for “anticipatory” effects of the reform on the job search behavior of workers. This test could not be executed in previous papers because the RR drops from the beginning of the spell of unemployment.<sup>10</sup> Finally, as in Carling et al, 2001, Lalive et al, 2006, do not study post-displacement wages and tenure effects of their reform providing no information on the effects of the reform on job quality.<sup>11</sup>

The paper is organized as follows. Section two reviews the empirical literature. Section three presents a brief description of the Spanish unemployment insurance system and the Law 20/2012. Sections four and five present the empirical strategy and

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<sup>6</sup> The authors present sensitivity analysis that validates their identification strategy.

<sup>7</sup> In the DiDiD approach we also use individuals not affected by the reform because their potential UI spell was less than 180 days.

<sup>8</sup> The median monthly income was about 16,400 ATS in the unemployment inflow from regular jobs in 1988.

<sup>9</sup> The mean and median monthly income in Spain in 2012 was €1,893 and €1,587, respectively (Encuesta Estructural Salarial, 2012).

<sup>10</sup> Carling et al, 2001, estimate the anticipatory effect of a drop in RR given that the reform was announced in June 1995, but implemented on all unemployment spells beginning January 1996.

<sup>11</sup> To the best of our knowledge, only Meyer, 1989, and Eugster, 2013, have analyzed the effects of increasing the RR on post-displacement earnings. In addition, Addison and Blackburn, 2000, look at the effects of receiving UI benefits versus not receiving them (the equivalent to a difference in RR of 44 percent) in the US on post-displacement earnings.

the data, respectively. Sections six presents and discuss the results, respectively. Section seven concludes.

## 2. Empirical Literature Review

The effect of economic incentives on individuals' behavior has been widely studied, in particular within the context of UI benefits and transitions out of unemployment.<sup>12, 13</sup> In this section, we review mainly studies analyzing the effects of changing levels of UI benefits as opposed to potential benefit duration.<sup>14</sup>

Earlier studies have exploited variation of UI benefits entitlement across time, regions or age groups. In the US and the UK, they have found an elasticity of unemployment with respect to the UI benefit level between 0.1 and 1.0, implying that a 10 percent *increase* in the amount of benefits would lengthen average duration by 1 to 1.5 weeks in the US and by 0.5 to 1 week in the UK (Moffitt, 1985; Narendranathan et al, 1985; Katz and Meyer, 1990; and Meyer, 1990). However, the evidence for Continental Europe is scarcer and finds no significant effects (Hujer and Schneider, 1989; Groot, 1990; van den Berg, 1990a; Steiner, 1990; and Hernæs and Strøm, 1996).

To address concerns that variation in UI benefits entitlements is correlated to pre-displacement earnings, which are likely to be correlated with unobserved heterogeneity affecting unemployment duration, several authors have exploited a policy reform changing the level of UI benefits and used a DiD approach instead--see Meyer, 1989; Carling et al, 2001; Lalive et al, 2006; Meyer and Mock, 2007; Uusitalo and Verbo; 2010; and Eugster, 2013. In these cases, the estimated effects are far from negligible in Continental Europe. Lalive et al, 2006, find that an *increase* in the RR of 15 percent in Austria in the late 1980s leads to an increase in unemployment duration of 0.96 weeks (or 5 percent). Estimates from Carling et al, 2001, for Sweden in the mid-1990s are even larger. They estimate an elasticity of 1.6, implying that the 6 percent

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<sup>12</sup> See theoretical analyses by Burdett, 1979; Mortensen, 1977; and Van den Berg, 1990, and early surveys by Atkinson and Micklewright, 1991, Layard et al, 1991, Holmlund, 1998, and Pedersen and Westergaard-Nielsen, 1998.

<sup>13</sup> A related literature analyzes the effects of punitive UI sanctions on the transition rate from unemployment to employment among welfare recipients (Abbring et al 1998, and van der Berg et al 2000).

<sup>14</sup> See Hunt, 1995; Winter-Ebner, 1998, Card and Levine, 2000, Lalive and Zweimüller, 2004, and de Groot and van der Klaauw, 2014, for studies using a similar methodology to analyze the effects of changing potential UI benefits duration.

decrease in the RR leads to a 10 percent increase in the exit rate to employment.<sup>15</sup> Uusitalo and Verbo 2010, study a reform that took place on January 2003, where the average benefit increase was 15 percentage for the first 150 days of the unemployment spell and find that the change in the benefit structure reduced the reemployment hazards by on average 17 percentage.<sup>16</sup> Most recently, Eugster, 2013, estimates that a 10 percentage points increase in the RR for low-wage earners increases unemployment duration for women by 10 percent in Switzerland in 2003. For men, the effect is half the size and not statistically significant.

For the US, Meyer, 1989, exploits 16 UI benefit increases during 1979 and 1984 across five states, and finds that an average increase in UI benefits of 9 percent led to an increase of UI receipt spell by about one week. In contrast, Meyer and Mock, 2007, find considerably smaller effects than those traditionally found in the US. They exploit an unexpected 36 percent increase in the maximum RR on April 1989 in New York state that affected mainly high- (and to a lower extent medium-) earners. Their estimates imply that a 10 percent increase in the benefits would lower the hazard of ending a UI spell by about 3 percent. Moreover, the authors find evidence that the reform substantially affected the incidence of claims, introducing incidence bias in the duration estimates.<sup>17</sup>

Using a random-assignment-like variation in unemployment benefit replacement ratios in Norway in the 1990s, Roed and Zhang, 2003 and 2005, confirm that the Continental European estimates are closer to those in the US and the UK, despite the substantial differences in UI institutions. These authors find that the average elasticity of the unemployment hazard rate with respect to unemployment benefits is around -0.95 for men and -0.35 for women, implying that a 10 percent *reduction* in benefits may cut a 10-month duration by approximately one month for men and 1 to 2 weeks for women.<sup>18</sup> Finally, the empirical research in Spain has focused on the effects of the UI

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<sup>15</sup> They assume that the elasticity of the expected duration is equivalent to the elasticity of the hazard rate only in the absence of duration dependence in the hazard rate. If we make the same assumption, we find that a decrease in the RR of 17 percent led to a 23 percent increase in the hazard rate, implying an elasticity of -1.35.

<sup>16</sup> Note that this results could be interpreted as a lower bound since at the same time that benefits level were increase, the severance pay system was abolished.

<sup>17</sup> While in the US the elasticity of weekly benefit amount on UI take-up rate ranges between 0.4 to nearly 1.0 (Anderson and Meyer, 1997), these estimates are considerably lower in Spain, where we calculate a take-up rate close to 90 percent.

<sup>18</sup> The authors exploit an idiosyncrasy of UI benefit system in Norway, namely that "*UI benefits are calculated on the basis of labor earnings recorded in the previous calendar year, rather than a given*

entitlement length on employment and unemployment duration (see Rebollo-Sanz, 2012, and García-Pérez and Rebollo-Sanz, 2014, for recent empirical evidence). The other Spanish relevant work is that of Bover et al, 2002, where the authors exploit a 1984 reform to analyze the effects of UI benefits receipt versus non-receipt on unemployment duration between 1987 and 1994.<sup>19</sup> They find that "*at an unemployment duration of three months – when the largest effects occur – the hazard rate for workers without benefits doubles the rate for those with benefits.*" In García-Pérez and Rebollo 2014, the estimated differences in the job finding probability between worker who receive benefits and workers who do not receive benefits, during the first months of the unemployment spell, varies between 10-20 percentage points.

### **3. The Spanish Unemployment Insurance Benefit System**

#### ***The UI System Before the Policy Change***

As in most OECD countries, Spain offers two types of unemployment benefits: *Unemployment Insurance* (UI) and *Unemployment Assistance* (UA). All employees who involuntarily become unemployed are entitled to UI benefits, provided that they have been employed for at least 12 months over the 72-month period prior to unemployment. Individuals receiving full-time disability benefits, voluntary job quitters, and anyone over the age of 65 are excluded from UI benefits. Benefits end when individuals cease to be unemployed or complete the maximum benefit period.

Potential UI benefit duration and the amount of UI benefit received depend on the duration and earnings of the previous employment spell, respectively. These benefits last for a period of at least four months extendable in two-monthly periods up to a maximum of two years, depending on the worker's employment record.<sup>20</sup> To be eligible to receive UI for at least 180 days, workers have to have worked at least 18

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*period prior to the entry into unemployment. This rule has no behavioral justification, and it implies that a given income received for a given job in a given period prior to the unemployment spell, entails higher benefits when more of it is concentrated within the last calendar year."*

<sup>19</sup> The 1984 reform legalized the use of fixed-term contracts in Spain and therefore produced a new type of unemployed worker without any UI benefits (workers displaced from a fixed-term contract), that co-existed with otherwise similar workers enjoying generous benefit entitlements (workers displaced from a permanent contract). The authors argue that this "*benefit–non-benefit division is close to a random assignment*". The analysis is done with the matched files of the Spanish Labor Force Survey.

<sup>20</sup> A worker with 12 to 18 months of employment within the last 6 years is entitled to 4 months of UI benefits. If the worker has worked for a period ranging between 19 and 24 months within the last 6 years, he is entitled to 6 months of UI benefits, and so on. This implies that the UI benefit entitlement in Spain is about 30 percent of the months employed during the last 6 years with a maximum of 24 months.



months. The amount of UI benefit is determined by multiplying the RR by the average basic salary over the 12 months preceding unemployment. The monthly payment is 70 percent of the worker's average basic pay for the first 180 days of benefits and 60 percent from the 181<sup>st</sup> day onwards. UI is also subject to a floor of 75 percent of the statutory minimum wage (SMW) and a ceiling of between 170 and 220 percent of the SMW depending on a worker's family circumstances. Hence, the maximum benefit amount is €1,087 for workers without family, €1,242 for workers with one child and €1,397 for workers with two or more children. The minimum benefit amount is €497 for workers without family and €664 for workers with family. This implies that the net RR could be much higher or lower than the rate quoted above. Figure 1 shows that within the EU, the Spanish net UI replacement rate ranges in the middle of the distribution, that is, it is not overly generous, nor is it as low as the RR observed in the UK, Poland, Malta, or Ireland.

Once UI benefits expire, workers are entitled to UA. UA is a non-contributory benefit targeted to those who no-longer qualify for the contributory benefits due to duration of unemployment or lack of contributions. UA payments have no relation with the previous monthly wages. A family income criterion is used whereby per capita family income cannot exceed the SMW. A flat benefit equal to 75 percent of the SMW is paid to all beneficiaries.

### ***The Law 20/2012***

On July 11, 2012, the Spanish Prime Minister, Mariano Rajoy, announced that the Spanish government was going to reform the UI system by law. On July 13, 2012, the vice-president, Soroya Saenz de Santamaria, explained the details of the reform, which were implemented on July 15, 2012 by Law 20/2012. This law established that all unemployment spells starting on July 15, 2012 would have the RR reduced from 70 to 50 percent beginning on the 181<sup>st</sup> day of the unemployment spell. Hence, this implied that the RR was reduced from 60 to 50 percent (16.66 percent) after 180 days of receiving UI benefits for all workers whose unemployment spell had began on July 15 2012 or thereafter. A feature of this Spanish reform worth highlighting is that the drop in the reform took place *after* 180 days of UI receipt, enabling us to study differential effects of the reform on the job search behavior of workers before and after they

experience the RR drop. This contrasts with most of the other studies that exploit a change in the RR implemented at the *beginning* of the UI spell.<sup>21</sup>

Figure 2 shows how the reform affected the RR as a function of workers' pre-displacement earnings and family composition. First, we observe that the reform did not affect the replacement rate of workers' with pre-displacement wages below €820 and above €1,800 without children (or below €1,100 and above €2,100 with one children and above €2,300 with two or more children). Figure 2 also shows that based on pre-displacement wages, the reform broadly affected wage and salary workers, in contrast to other studies in which the reform targeted low-income workers (Lalive et al, 2006 and Eugster, 2013) or high-income workers (Meyer and Mock, 2007).

It is important to note that because the reform took place two days after being announced, strategizing layoffs are unlikely. Indeed, Figure 3 shows no sign of an increase in UI inflows *prior* to the reform. In addition, Figure 3 shows a similar trend of UI inflows between 2011 and 2012 *prior* to July 15. After the reform, we observe a small and transitory increase in UI inflows, suggestive that the reform was not driven by the government anticipating an improvement in the economy. In fact, Table 1 shows that during the year of the reform and afterwards, GDP growth continued to decline in Spain and the unemployment rate continued to grow reaching the highest level in Spanish history: 26.9 percent. Therefore, it is unlikely that our results from comparison of the labor-market experiences of workers displaced prior to the policy change (comparison group) to those displaced after the reform (treatment group) is driven by an improvement of the labor market conditions. If anything, because the economy continued to slow down after the reform, our DiD estimates may be a *lower bound* of the causal effect of the reform. To address this potential concern we will also present a DiDiD estimate of the reform as explained in the Empirical Strategy section.

Finally, it is also important to highlight that, in Spain, practically totality of those who are eligible to receive UI benefits file for benefits. In our sample, the estimated UI take-up rate is over 90 percent. In addition, as the RR did not change during the first 180 days of UI benefit intake, concerns that the reform may have

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<sup>21</sup> The only exception is Carling et al, 2001, who study a drop in the RR implemented in January 1996 in Sweden on all UI recipients, regardless of when their unemployment spell had began. Nonetheless, the authors do not exploit the variation in the timing of the reform. Instead, they compare those affected by the drop in the RR to those not affected because of the UI benefit ceiling.

affected displaced workers' decision to claim their benefits are very unlikely. Nonetheless, we conduct sensitivity analysis in the Results section to evaluate whether compositional bias and heterogeneity of treatment and comparison groups are affecting our results.

#### 4. The Empirical Strategy and Theoretical Predictions

Our preferred specification is a Differences-in-Differences (DiD) model estimated on a sample of UI recipients whose RR after 180 days of UI receipt is around 60 percent and hence were eligible to be affected by the reform. In this case, the identification comes from comparing UI recipients who entered unemployment before and after the reform was implemented on July 15, 2012, to UI recipients who were unemployed during the same calendar period *but a year earlier*.

To estimate how the drop in the RR affects the job finding probability, we apply a proportional hazard model. Given the characteristics of the dataset described in the next section, we use discrete time duration models in which the proportional hazard assumption implies that each hazard  $h(j)$  {j=duration} takes the complementary log-log form (Jenkins, 2005). Thus, the general specification of the hazard rate to be estimated is as follows:

$$h_i(j/x, d) = 1 - \exp\left(-\exp\left(x_i(j)\beta + \alpha_1 D_i^{2012} + \alpha_2 D_i^{postJuly14} + \alpha_3 (D_i^{2012} * D_i^{postJuly14}) + G(j)\delta\right) \lambda_i(j) + UB_i(j)\gamma + Z_i\eta\right) \quad (1)$$

$D_i^{2012}$  is a dummy that takes value 1 if the worker entered unemployment during the year 2012, and 0 if the worker entered unemployment during the year 2011;  $D_i^{postJuly15}$  is a dummy that takes value 1 if the worker entered unemployment after July 14, and 0 otherwise. Our coefficient of interest is captured by  $\alpha_3$ , and measures the effect of the policy on the job finding rate of UI recipients affected by the reform.

In addition, equation (1) controls for four different sets of explanatory variables. First, we control for a set of state and monthly dummies and quarterly GDP growth,  $G(j)$ . Note that by using individuals who became unemployed a year earlier and by controlling for the month the unemployment spell is observed, we are netting out any

seasonality that may occur across months. In addition, to control for regional differences and macroeconomic and business cycle effects, we introduce the state dummies and the quarterly GDP growth. Second, we add a set of individual characteristics,  $X(j)$ , expected to be correlated with finding employment, such as age, gender, nationality, total labor market experience, and presence of children in the household. Third, we add information on the individual's UI benefit receipt,  $UB(j)$ , such as the potential length of UI entitlement and the benefit level the individual is entitled to. Finally, we control for pre-displacement job characteristics,  $Z$ , namely, blue- versus white-collar job, and industry, firm ownership (public versus private), and firm size. Because the benefit level and benefit entitlement length are highly correlated with pre-displacement wages and tenure, respectively, our preferred specification does not include pre-displacement wages. Nonetheless, in our sensitivity analysis we present estimates including both pre-displacement wages and tenure.

Finally,  $\{\lambda(j)\}$  stands for the integrated baseline hazard. We specify the duration dependence of the hazard as a piecewise constant function of elapsed duration as shown in equation (2) below. Thus, the hazard rate shifts in every four-week intervals. Because we observe individuals only up until December 31, 2013, we censor the spells at 52 weeks.

$$\lambda(j) = \exp\left(\sum_{l=0}^{12} \lambda_l I(4l \leq j < 4(l+1)) + \lambda_{13} I(j > 52)\right) \quad (2)$$

where the  $\lambda_l$  ( $l=1,2,\dots,13$ ) measure the duration dependence of the hazard rate.

To estimate this discrete-time duration model, we construct a panel dataset such that the spell length of any given individual determines a vector of binary responses. Let  $y_i$  be a binary indicator variable denoting transitions to potential destination states upon exit, that is,  $y_i=1$  if individual  $i$  transits to employment and zero otherwise, and let  $Y_i$  be the complete set of outcome indicators available for individual  $i$ . The contribution to the likelihood function formed by the event pattern of a particular individual can then be formulated as:

$$L_i(\Omega) = \left[ \prod_{j=1}^J (h_j)^{y_j} (1-h_j)^{1-y_j} \right] \quad (5)$$

We compute robust standard errors clustered at the individual level. In the robustness section, we report results with alternative clustering of the standard errors to account for any dependence of the errors across time and groups.

### *Theoretical Model and Anticipation Effects*

The job search model predicts that a decrease in the RR implies that the worker will search more intensively than before from the start of the unemployment spell, as the costs of being unemployed are higher. The effect exists independently of whether the drop in the RR takes place at the start of the spell of unemployment or afterwards. This is because, the net present value of the unemployment spell will decrease when the worker expects a drop in the replacement rate. Nevertheless, the magnitude of this effect will be higher the sooner the drop in the RR takes place. The reduction in job-finding rates should be largest at the beginning of the unemployment spell because at that point the change in the value of the remaining future benefits is the highest. That is, the typical search model predicts that the policy effect should concentrate at the beginning of the unemployment spell. Treated workers will exit faster even before the drop in the replacement rate will take place because the reservation wage drops (or search intensity increases) since the start of the spell of unemployment.

Because the Spanish reform decreased the RR only after 180 days of UI receipt, it does allow us to test whether there will be a different anticipatory effect, namely the one predicted by job search theory that treated workers will search more intensively and reduce their reservation wage from the beginning of the spell of unemployment in order to exit faster from unemployment and, consequently, avoid the larger drop in income after the 181<sup>st</sup> day of unemployment. We explore whether this anticipation effect occurs by estimating an extended version of the model presented above that interacts the post-July 14 dummy, the year 2012 dummy, and the interaction of these two dummies with a dummy equal to 1 if the individual exits unemployment after 180 days of UI receipt and 0 otherwise. This alternative specification of the model will allow us to identify the presence of anticipation effects.<sup>22</sup>

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<sup>22</sup> While the job search theory also predicts an "entitlement effect", we would not expect this effect to affect workers in the Spanish reform. Since, in the Spanish reform, the RR drop applied to *all* unemployment spells beginning July 15, 2012, the "entitlement effect" -- which implies lower job search

Note that this anticipatory effect differs from that of Carling et al, 2001, namely that workers anticipate the changes in the benefit system *before the reform is implemented*, and hence increase their job search intensity before the law change even occurred -- the reform studied by Carling et al, 2001, was announced 6 months before being implemented. Because the Spanish reform was announced only 2 days before being implemented (and hence unexpected) and it reduced the RR only to those entering unemployment July 15, 2012, the anticipatory effect found by Carling et al 2001 is not of concern in our analysis. Similarly, since the Spanish reform had no effect on the severance payment workers received (as it did in Uusitalo and Verho, 2010), strategic timing of dismissals among those who expect to find jobs quickly is not an concern.

It is important not to confuse this "entitlement effect" with the well documented increase in the job finding probability at the time of benefits exhaustion, the "exhaustion effect".<sup>23</sup> This last effect refers to the spike of job finding probability at the time benefits run out. It could be that the size of the exhaustion effects differs between workers with different replacement rates. Basically, the exhaustion effect could be higher for workers with higher replacement rates. Given that the exiting empirical evidence for Spain (Rebollo-Sanz, 2012 and García-Perez and Rebollo-Sanz, 2014) remarks the importance of the exhaustion effect in our empirical strategy we will try to net out the "entitlement effect" with the "exhaustion effect" by analyzing workers that are not close to the time of benefit exhaustion.

## **5. The Data and Descriptive Statistics**

### ***The 2013 Continuous Sample of Working Histories (CSWH)***

We use administrative longitudinal data from the Spanish Social Security database, the waves 2011 to 2013 of the *Continuous Sample of Working Histories* (hereafter CSWH). The CSWH is compiled annually and every year comprises a 4 percent non-stratified

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intensity as benefit expiration approaches than before because the value of the new job will now be lower as the new job will entitle the worker to a lower RR when he or she becomes unemployed in the future -- does not apply.

<sup>23</sup> Both theoretical and empirical studies have shown that the optimal job search of the worker increases especially around the time benefits run out (Katz and Meyer, 1990). Previous empirical evidence has shown that the spikes in the unemployment exit rate at time of benefit exhaustion are large in Spain (Rebollo-Sanz, 2012, and García-Pérez and Rebollo-Sanz, 2014).

random sample of the population registered with the Social Security Administration. Hence, the initial database includes all individuals who came into contact with the Social Security system -- including both wage and salary workers and recipients of Social Security benefits, namely, unemployment benefits, disability, survivor pension, and maternity leave -- at least once between 2011 and 2013.<sup>24</sup>

In addition to age, gender, nationality, state of residence (*Comunidad Autónoma*), education, and presence of children in the household, the CSWH provides highly detailed information about the worker's previous job. More specifically, we observe the dates the employment spell started and ended, the monthly earnings history, the contract type (permanent versus fixed-term), the occupation and industry, public versus private sector, and the firm size.<sup>25</sup> We calculate the worker's previous work experience as the number of months worked since the employee's first job, and tenure as the number of months the worker has stayed with the same employer. The CSWH also informs us on the reason for the end of the employment spell (quit versus layoff), and whether the worker receives unemployment benefits and the type (UI versus UA). We compute the duration of each unemployment episode by measuring the time between the end date of the worker's previous contract and the start date of the new one. The CSWH also allows us to compute the UI entitlement length and the net RR. We will use the net RR to define our treatment group.<sup>26</sup> Most importantly, the CSWH allows us to observe individuals after exhaustion of UI benefits, which allows to study how the job finding rate and other post-unemployment characteristics evolve after the exhaustion of UI benefits. This contrasts with UI claims data, in which the unemployment period is truncated at the date benefits expire (Card et al, 2007).<sup>27</sup>

From these data we extract a sample that contains all unemployment entrants in the period between 2010-2012. We restrict our sample to 20- to 50-year old wage and salary full-time workers who have experienced an unemployment spell after having

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<sup>24</sup> Although self-employed workers are also in the CSWH, we exclude them from the analysis, as they are not eligible to receive UI benefits.

<sup>25</sup> Earnings are deflated using the Spanish CPI (2011, Base).

<sup>26</sup> We compute the UI entitlement length at each point in time applying the Spanish UI system rules to the worker's labor market history. This is one of the main advantages of the database. We proceed similarly when computing the worker's RR, taking into consideration the ceilings and floors explained in Section 3.

<sup>27</sup> Card et al, 2007, show that the way in which unemployment spells are measured has a significant impact on the magnitude of the spike at exhaustion of benefits. García-Pérez and Rebollo-Sanz, 2014, find similar results for Spain. Hence, censoring the data at the time benefits expire (as is done with UI claims data) would bias the impact of the policy reform we are evaluating.

worked for at least three years and, thus, are entitled to more than one year of benefits.<sup>28</sup> The restriction on the length of the employment spell implies that workers in our sample are strongly attached to the labor force. This restriction allows us to focus on a group of relatively homogenous displaced workers, and to observe individuals for which potential duration of UI benefits lasts for up to *at least* 6 more months *after* the cut in the RR and *before* the time benefits run out, guarantying that the effects of the reform are *not* measured at the time the UI benefits run out. This sample restriction is important as it prevents our results to be influenced by potential exhaustion effects, that is an increase in the job finding probability at the time of benefits exhaustion.

To avoid differential job search behavior by pre-displacement contract type, we restrict our analysis to workers who have been displaced from permanent jobs.<sup>29</sup> Note that close to 90 percent of displaced workers with at least three years of tenure in their former job held a permanent contract in our sample.<sup>30</sup> Finally, we drop those UI recipients whose RR would not have changed due to the reform (regardless of whether they became unemployed before or after the reform). This restriction implies dropping workers with pre-displacement wages above €1,800 (if without children) or €2,100 (if with children) as their RR is below 50 percent due to the UI benefits' ceiling, and thus they would have been unaffected by the reform. Similarly, workers with pre-displacement wages below €20 (if without children) or €1,100 (if with children) will have a RR of 60 percent or higher due to the UI benefits' floor, and thus would have been unaffected by the reform.

Individuals in the treatment group are defined as those UI recipients who became unemployed right after reform was implemented, that is, between July 15, 2012 and December 31, 2012. Those in the comparison group are defined as those UI recipients who became unemployed before the implementation of the reform, that is,

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<sup>28</sup> We restrict the analysis to full-time workers, because the RR for part-time workers depends on the number of hours worked and this information is missing in the sample. Even if we had this information, we would not want to include the part-time workers because the reform changed the way their RR was computed, stating that it would now be the proportion of hours previously worked times the regular RR. As explained by Fernández-Kranz and Rodríguez-Planas, 2011, the fraction of part-time workers in Spain has traditionally been low (below one tenth of the labor force).

<sup>29</sup> Workers with a permanent contract receive severance pay at displacement, whereas those with a temporary contract do not or, in case of receiving severance pay, these payments are much lower. Clearly, the receipt of severance pay affects workers' liquidity constraints, and hence their job search efforts.

<sup>30</sup> Differences between workers under a permanent and a temporary contract in Spain are well beyond the scope of this paper. See Lacuesta et al, 2013, for a thorough explanation on these differences.



between January 1, 2012 and July 14, 2012. To control for potential seasonality in the employment job finding rates in Spain across months, especially during the summer months, we compare the change in the employment finding rate for our treatment and comparison groups relative to the change observed among similar groups of UI recipients who became unemployed during the same calendar time *but* one year earlier, that is, during 2011. Because our data follows workers up until December 31, 2013, we focus our analysis on the first year after becoming unemployed. Hence, we censor unemployment spells at 52 weeks in order to isolate the effects of the policy variable within the first 52 weeks of job search.<sup>31</sup> .

These restrictions imply that for the year 2012, we end up with 8,726 unemployment spells of which more than 27 percent ended in a new job during the first year of unemployment, and 73 percent were censored. Of the 8,726 unemployment spells, 4,621 belong to workers who entered unemployment *before* the reform. Among these, about 24 percent found a new job within a year of losing their job. In contrast, 31 percent of workers who entered unemployment *after* the reform found a new job within a year of losing their job. For the year 2011, we end up with 7,732 unemployment spells of which more than 26 percent ended in a new job during the first year of unemployment, and 74 percent were censored. Of the 7,732 unemployment spells, 3,807 belong to workers who entered unemployment *before* the July 15, 2011. Among these, about 25 percent found a new job within a year of losing their job. Among workers who entered unemployment *after* July 14, 2011, 26 percent of found a new job within a year of losing their job. This one percentage point difference contrasts with the 7 percentage points difference in 2012, and suggest that the reform increased the share of workers who found jobs by 6 percentage points or 25 percent.

### ***Descriptive Statistics***

Table 2 presents descriptive statistics of UI recipients before and after the reform, by whether their unemployment spell began before or after July 15. We observe that about two thirds of our sample are women, a bit over one third have a university degree, and 72 percent live in households with children. On average workers are 40 years old, have

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<sup>31</sup> The idea of artificially censoring all unemployment spell is important since for the pre-reform data we have a longer observation period.

close to 10 years of experience, and are entitled to about one and a half years of UI benefits. Moving now to the job characteristics of their job prior to displacement, Table 2 shows that on average they had 7 years of tenure, earned €1,547 euros per month, had high- and medium-skilled jobs (with less than one tenth working in low-skilled jobs), and worked mostly in services. Table 2 also shows that there are few differences between those who lost their job before and after July 15, 2012, and that these differences are quite small in size. In particular, the only differences statistically significant at the 95 percent level or above are that individuals who lost their job after July 14, 2012 have 5 months (or 4 percent) more experience and 16 weeks (or 4 percent) more tenure than those who lost their job before July 15, 2012. However, column 7 in Table 2 shows that these differences in experience already existed in 2011 and thus are "washed out" by our identification strategy. The only differences across time that remain are a small difference in gender and a 5 weeks difference in potential length of the UI entitlement. In the Results section we will test the sensitivity of our results to this imbalance.

To examine whether the policy is endogenous, Figure 4 shows unemployment outflows to new jobs over the calendar year for the years 2011 and 2012. Although the unemployment outflows are lower during 2012 than 2011 consistent with the economic slowdown observed in Table 1, the seasonal pattern is similar across the two years with outflows increasing up until July 15 and decreasing thereafter. Figure 4 also shows that monthly unemployment outflows to new jobs are around 2.5 percent.

Figure 5 displays monthly empirical job finding rates along the spell of unemployment by the year the unemployment spell began and whether this spell began before or after the 14<sup>th</sup> of July. This figure provides a first crude check on how the policy change may have affected unemployment duration. From basic job search models, if the reform had any effect on the behavior of unemployed workers, we should observe a higher unemployment hazard rate –since the beginning of the unemployment spell–, for workers who entered the unemployment spell after 14<sup>th</sup> in 2012 versus those who entered the unemployment spell before the 14<sup>th</sup> of July 2012. On the contrary, for workers who entered the unemployment spell one year before the reform, there should be no differences by whether they entered unemployment before or after the 14<sup>th</sup> of July 2011. Panel A in Figure 5 shows that the job finding rate peaks at 1.5 percent during week 5 for UI recipients who entered unemployment after the reform and at 1.2 percent

during the same week for UI recipients who entered unemployment before the reform. Thereafter, the job finding rate decreases and fluctuates around 0.8 percent for UI recipients who entered unemployment after the reform. For those who entered before the reform, the job finding rate smoothly declines to 0.5 percent after a year in unemployment. Panel A in Figure 5 also shows that between weeks 21 and 24, just before the worker suffers the drop in the replacement rate, the hazard rate for treated workers is again above the one for non-treated workers. Hence, during the first months of unemployment, the hazard rate for treated workers is above the one for non-treated workers at the beginning of the unemployment spell and right before the drop in the replacement rate. For the subsequent months we observe that between weeks 29 and 49, that is after the drop in the RR, the job finding rate is higher for UI recipients who entered unemployment after the reform than those who entered before the reform. Note that these observed differences could be due to the observed differences in entitlement lengths. Average entitlement length for treated workers is 88 whereas for non-treated workers is 92. Therefore, we can not state that these observed differences are due to the reform. We need to make a conditional analysis to control for these observed differences

Note also that we do not observe such differences between UI recipients who entered unemployment before and after July 15, 2011, that is one year before the reform took place (shown in Panel B). This is suggestive evidence that the reform increased the job finding rate for affected workers.

## **6. Results**

### **6.1. Effects of the Reform on the Job Finding Rate**

Table 3 displays the policy effect,  $\alpha_3$ , which captures the effect of the drop in the RR on UI recipients who became unemployed after July 14, 2012 relative to their counterparts who became unemployed before July 15, 2012, net of any changes observed before and after July 15, 2011. Each column presents a different specification. Column 1 presents a proportional hazard with the post-July 14 dummy, the year 2012 dummy, and the interaction of these two dummies. Column 1 in Table 3 shows that reducing the replacement rate by 10 percentage points after 6 months of unemployment increases the job finding rate by 32.5 percent. This effect is statistically significant at the 99 percent

level. Column 2 adds a set of state and monthly dummies and quarterly GDP growth to control for seasonal, regional and macroeconomic effects. Doing so decreases the effect to 14.7 percent, suggesting that omitting these variables introduces an upward bias in the estimated effect of the reform on the job finding rate. Column 3 adds the individual's UI benefits potential spell and entitlement as additional controls. Doing so, increases the policy effect to 23.1 percent suggesting that not taking into account differences in the UI spell and entitlement leads to a downward bias in the estimated effect of the reform on job finding rates. Adding as additional controls the individuals' characteristics (column 4) and the pre-displacement earnings and tenure (column 5) has little effect on the policy coefficient as  $\alpha_3$  is now 24 and 25.1 percent, respectively. All of these coefficients are statistically significant at the 99 percent level. Column 6 re-estimates the model in column 4 controlling for pre-displacement job characteristics and is our preferred specification. We find that the effect of reducing the RR by 10 percentage points led to an increase in the job finding rate of 23.5 percent within the first year of unemployment. This effect is statistically significant at the 99 percent level.<sup>32</sup> Column 7 adds pre-displacement earnings and tenure to our preferred specification, changing the policy coefficient slightly to 24.8 percent.

## 6.2. Robustness Checks and Sensitivity Analyses

The DiD model may be biased if other shocks (such as changes in state labor-market conditions) coincide with policy changes and affect the behavior of the unemployed workers, leading to changes in workers' reservation wage or the arrival rate of job offers. To assess the existence of differential trends, we take several approaches. First, column 8 adds to our preferred model (shown in column 6) the interaction between the GDP growth and the  $D_i^{postJuly14}$  dummy to allow for a differential trend between those who lose their job in the first or second half of the year (as suggested by Meyer, 1995). This specification controls for systematic differences in the behavior between the two groups over time. As the policy effect only varies slightly from 23.5 percent (column 6) to 21.2 percent (column 8), differential trends do not seem to be affecting much our results. Second, we estimate two Differences-in-Differences-in-Differences models,

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<sup>32</sup> All of the coefficients in the preferred specification are reported in Appendix Table A1. The estimated model contains a large number of parameters, most of which are included solely for control purposes and secondary to the topics discussed in this paper.

shown in columns 9 and 10. In both cases for the third difference we use workers who were only entitled to four and six months of UI benefits and thus, whose RR was not affected by the reform. In column 10, we include the interaction between the GDP growth and the  $D_i^{post_{July14}}$  dummy to allow for a differential trend between those who lose their job in the first or second half of the year. The policy estimate in columns 9 and 10 is 23.7 and 23.3 percent, respectively.

Since we observed in Table 2 that there was a difference of 5 weeks in potential length of the UI entitlement between our treatment and comparison groups in 2012 relative to 2011, column 11 in Table 3 reestimates our preferred specification using *only* individuals with entitlement length of at least 18 months. In addition to eliminating the imbalance, doing so has the advantage that we analyze the effect of the reform among workers who still have one year and a half of UI benefits *after* the drop in RR. The coefficient in column 11 shows that among those who have at least 18 months entitlement, the effect of the reform on their job finding rate is 28 percent. In contrast, our preferred estimate is likely to include some exhaustion effect as some workers are getting close to have their benefits expire after 52 weeks.

It is important to note that our preferred estimates cluster at the individual level. Nonetheless, since we exploit the variation in our policy across workers entering UI at different months over the year we may still be worried about correlation of the error terms within these groups (Cameron and Miller, 2013). To address this concern, Columns 12 and 13 reproduce our preferred estimates under alternative assumptions of correlation in the errors within groups. Column 12 estimates robust standard errors accounting for dependence of errors within month of entry into unemployment, and column 13 estimates robust standard errors accounting for dependence within month of entry into unemployment and state of residence. Doing so has little effect on our main results.

Methodologically, we have relied on the assumption that, in the absence of the reform, the job finding rate differences between the treated and comparison groups would have remained constant. As this assumption is not testable, we proceed to carry out placebo estimates (shown in Table 4). In column 1 of Table 4, we estimate our preferred DiD specification using a period in which no reform was implemented. For this purpose, we only use the years 2010 and 2011. We then define a pre-reform period as the period that begins two years before Law 20/2012 was actually implemented, and

as post-reform period the period that begins one year before Law 20/2012 was implemented. The placebo estimate is not statistically significant. Moreover, the coefficient is considerably smaller in size and has the wrong sign. Column 2 repeats the exercise with the DiDiD specifications in columns 9 in Table 3 but using the period 2010 and 2011, before the reform was implemented. Again, the estimate is small, the wrong sign, and not statistically significant. Columns 3 and 4 in Table 4 present alternative placebo tests, estimated during the 2011-2012 period, when the reform was implemented. Column 3 shows our preferred specification with workers with only 4 or 6 months of UI entitlement as the treated group. Note that since these individuals' UI benefits expired prior to 181 days of UI receipt they were not affected by the policy. Indeed, there is no effect of the reform. Column 4 repeats the exercise with unemployed individuals who did not have any UI benefits. In both cases, we find no effect of the reform. These five placebo tests suggest that our results are not due to systematic differences in trends between the groups we study.

Table 5 presents additional sensitivity analysis. Column 1 presents results when we estimate our preferred specification using only individuals who got displaced within 3 months (instead of 6 months) of July 2015. Here, in the spirit of regression discontinuity analysis, we adjust groups in such a way that they are close to the eligibility threshold and we confine the analysis to the inflow of three months before and after the reform. This reduces the sample size to half. Although we lose precision, the policy estimate is 21.1 percent and remains statistically significant at the 90 percent level.

Columns 2 to 5 address potential concerns that heterogeneity of treatment and comparison groups may be affecting our results by presenting estimates by subgroups. Columns 2 and 3 present estimates by gender, and columns 4 and 5 presents estimates by whether the pre-displacement wage as below or above the median. Consistent with Roed and Zhang, 2003 and 2005, we find that the effect of the reform is larger for males (40.2 percent) than for females (19.5 percent). Both estimates are statistically significant at the 99 percent level. These results show that the elasticity of unemployment duration to RR is notably larger for males than females. From the perspective of a job search models this result informs us that search efforts or reservation wages are more sensitive to the RR for males than females in Spain, consistent with evidence showing that, in Spain, males are more strongly attached to the

labor market and tend to be responsible for contributing to a larger share of the household income than females. Moving now to columns 4 and 5 in Table 5, we observe that the effect of the reform is larger for higher- versus lower-income earners (41.7 versus 17.4 percent). Moreover, the latter is only significant at the 90 percent level. Interestingly, our result for lower income workers is very similar to that of Lalive et al, 2006, whose reform *only* affected low-income workers.

Finally, to address concerns that the small and transitory increase in UI inflows observed in Figure 3 may indicate compositional changes due to the reform, we have estimated whether the reform affected the outflows from employment using a competing risk hazard model in which workers can exit from employment to another job or to unemployment (results shown in Table 6).<sup>33</sup> In this case, we use the same DiD approach as for the unemployment hazard rate. In Table 6 we can see that there is no evidence that the reform affected unemployment inflows.

### **6.3. Predicted Hazard and Survival Functions and Anticipation Effects**

While the earlier section estimated the average effect of the drop in the RR on the new job finding probability, in this section we estimate the effect of reform on the entire re-employment hazard profile. In practise this is done by allowing the effect of the reform to vary across elapsed duration of unemployment as predicted by the search theory. A similar approach has been used in Lalive et al, 2006, Bennmarker et al, 2007, and Uusitalo and Verho, 2010.

Figure 6 presents the factual employment hazard rate with treatment and the counterfactual hazard rate without treatment. To obtain the factual hazard rate with treatment, we calculate the prediction for the individual hazard rate averaging with respect to the distribution of all covariates used in the estimation in the population receiving the treatment. To obtain the counterfactual hazard rate we impose all treatment effects to be 0 to get the individual hazard rate and again average across treated individuals. As explained by Lalive et al, 2006, the difference between the two hazards is equal to the “average treatment effect on the treated”.

Figure 6 shows that decreasing the RR tends to increase the job find rate, as predicted by job search theory. Individuals with less generous benefits tend to leave

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<sup>33</sup> The competing hazard model is explained in the Appendix.

unemployment faster in the covered period. Figure 7 reports the difference in the factual hazard rate with treatment to the counterfactual hazard rate without treatment, namely, “average treatment effect on the treated” (ATET). This figure shows that the ATET is around 0.2 percentage points, implying that if the hazard rate is on average 0.8 percent for the counterfactual, the drop in the RR increased the hazard rate of the treatment to 1 percent, a 25 percent increase. This ATET is statistically significant at the 95 level.

As explained earlier, job search theory would predict an anticipation effect as soon as the unemployment spell begin. To explore this, Table 7 shows policy effect before and after 180 days of UI spell as explained at the end of section 4. Columns 1 and 2 show that most of the effect of the reform is taking place during the first 180 days of UI receipt, that is prior to the actual drop in the RR, suggesting a strong anticipation effect. Column 1 shows that the reform increased the probability of finding a new job by 25.9 percent during the first 180 days of the unemployment spell. This estimate is statistically significant at the 95 percent level. However, we find no effect of the reform on treated workers after day 181, that is when the RR actually dropped. The coefficient is small in size, not statistically significant and negative.

Figures 8 and 9 reestimate the factual and counterfactual employment hazard rates allowing for differential effect of the reform before and after 180 days of UI spell. Figures 8 and 9 show that the reform induced an increase in the job finding rates during the first 27 weeks of the unemployment spell. After the 28th week, there is no longer any difference in the job finding rates between the treated and the comparison groups. Hence, there is evidence of an anticipation effect, and this effect seems to be strong. How do these results compare to those found in the literature? They are consistent. Even though Lalive et al, 2006, are unable to estimate whether the RR led to an anticipatory effect, they do find that most of the effect takes place early in the unemployment spell, consistent with the job search theory. Carling et al, 2001, find evidence of an anticipatory effect. They find that cutting the RR increased the job-finding rates several months before the law change even occurred -- the reform was announced 6 months before being implemented.

While job search theory offers sharp predictions regarding the impact of unemployment insurance parameters on the exit hazard, policy makers are interested in the implied effects on unemployment duration, that is, the effects on the survivor



function. Figure 10 reports the factual survivor function with treatment and the counterfactual survivor function without treatment allowing for heterogenous effects during the length of the unemployment spell. The ATET is reported in Figure 11. Interestingly, the survivor function for treated workers -- and in correspondence with the hazard function -- starts to diverge from the control ones since the beginning of the unemployment spell. Since the hazard rate for treated workers tends to be higher than the counterfactual, the differences widens during the first 6 months of unemployment. From previous Figures, we observe that the first month of unemployment clearly contributes to decrease the expected unemployment duration for treated workers relative to the counterfactual.<sup>34</sup> ..

Table 8 reports the simulated effects on the expected unemployment duration during the first 104 weeks of the unemployment spell.<sup>35</sup> Column 1 in Table 8 reports estimates for the model restricting the effect of the policy to be the same along the spell of unemployment and column 2 reports the estimates for the model that allows heterogeneous effects along the UI spell. We find that reducing the RR by 10 percentage points (from 60 to 50 percent, representing a 17 percent drop) shortens the unemployment spell by about two weeks (around 3.6%). This implies an elasticity of unemployment duration with respect to the replacement rate of around 0.22<sup>36</sup>

Our estimate is only about one fifth the size of the elasticity of unemployment with respect to the UI potential benefit duration measured in Spain, suggesting that a more

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<sup>34</sup> This information is important to understand the “benefits” in terms of payments (savings for the public sector) derived from this policy reform. We can identify there is a direct saving effect due to the change in the UI system. To compute this direct effect we can use the expected unemployment duration without treatment (46 weeks) as well as the drop in the replacement rate which starts at week 25<sup>th</sup>. Given this drop in the RR, the UI system end up transferring 750 Euros instead of 900 Euros (for mean wages) each month. These lower transitions are from the week 25<sup>th</sup> to the week 46<sup>th</sup>. Moreover, the indirect effects of the policy must also be taken into account and they are related with the behavioural changes induced by the reform. That, is the survival function is lower for treated than for control group workers and consequently, new expected unemployment duration drop to 44 weeks. Hence, indirectly, the UI system will save around 375 euros per each worker due to the shortening of the unemployment spell. Summing up the direct savings sum up 750 euros (150 euros each month multiply by 5 months) whereas the indirect savings sump up 375. Hence, two thirds of the savings are due directly to the reform meanwhile one third is due to behavioural effects. This comparably small number reflects the relatively small behavioural changes we have estimated above. In Lalive et al. (2006) the direct component is estimated to be about 90% but mainly because the drop in RR takes place since the start of the spell of unemployment.

<sup>35</sup> Since inference on the survivor function tends to become unreliable as we extend the duration of the unemployment spell, we arbitrarily limit our discussion to the first 104 weeks, which are quite well identified in our large data-set.

<sup>36</sup> The drop in mean unemployment duration is 3.6 percent and the drop in the replacement rate is 17 percent. We divide  $3.6/17 = 0.217$  ..

effective policy to reduce the duration of unemployment would have been to reduce the UI entitlement length.<sup>37</sup>

#### **6.4. Impact on Post-Unemployment Job Characteristics**

In the previous section we estimated the effects of the change in the RR on the actual length of unemployment duration. The typical job search model predicts that unemployed workers will lower their reservation wage and accept “worse” job offers. However, Dolado and Stuchi, 2013, show that, in the presence of generous benefits, individuals are more selective on which jobs to accept and the overall quality of worker-job matches may increase and this can have a positive effect on productivity. In this section, we test this hypothesis by estimating the effects of a drop in the RR on monthly wages, the likelihood of working under a permanent contract and full-time, and number of subsequent jobs. Estimates on wages and the likelihood of working under a permanent contract and full-time are evaluated at the time the workers gets a job (and are set to 0 otherwise). Estimates on the number of subsequent jobs is measured a year after entering unemployment. Table 9 shows that the drop in the RR increased the average monthly wages by 22 percent. This effect is statistically significant at the 99 percent level. Because the policy led to an increase in the likelihood of being employed, the results on monthly wages are most likely driven by the fact that workers found jobs. Indeed, when the analysis is done conditioning on having found a job, the effect is one third smaller and no longer statistically significant.

This result is interesting because it help us to interpret the results relative to the job finding probability in terms of job search models. We have found that the job finding probability for treated workers has increased relative to their counterparts, mainly during the first six months of unemployment. This increase in the job finding probability can be due to higher search effort or to a drop in the reservation wages. Given that, we do not get that treated workers have lower wages, it seems that most of this effect found can be related to differences in search effort between treated and contro group workers.

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<sup>37</sup> García-Pérez and Rebollo-Sanz, 2014, estimate that the elasticity of the unemployment with respect to potential UI duration is close to one in Spain.

We also find that the policy increased the likelihood of working full-time by 59 percent, but had no effects on the likelihood of having a permanent contract. The effect on the likelihood of working full-time is statistically significant at the 99 percent level. This result is quite important, especially because in Spain the evidence shows that part-time jobs tend to be second-best jobs, offering limited career advances and lower wage growth (for a given level of human capital) as shown by Fernández-Kranz and Rodríguez-Planas, 2011. Finally, the lack of result on the likelihood of having a permanent contract is not surprising given that most hires take place through a fixed-term contract (Fernández-Kranz and Rodríguez-Planas, 2014, find that 84 percent of transitions into employment take place through a fixed-term contract).

## ***7. Conclusions***

With the emergence of the Great Recession many governments have passed reforms affecting the design of the UI system. This paper analyzes a July 2012 Spanish reform that led to a drop in the RR from 60 to 50 percent for workers who remained unemployed more than 180 days. Using administrative records and DiD approach we find that the 2012 decrease in unemployment benefit generosity increased the new job finding rate by 23.5 percent. Interestingly, the reform affects the job finding probability before the drop in the replacement rate actually takes place, suggesting a strong anticipatory effect. This is partially in accordance with what the job search theory would predict and results found by Lalive et al, 2006 and Carling et al, 2001. Finally, we also explore the effects of the reform on the type of job workers get after displacement. We do not find evidence indicating that workers were better off in terms of average monthly wages or permanent employment though we do find that the likelihood of working full-time is higher for workers affected the reform

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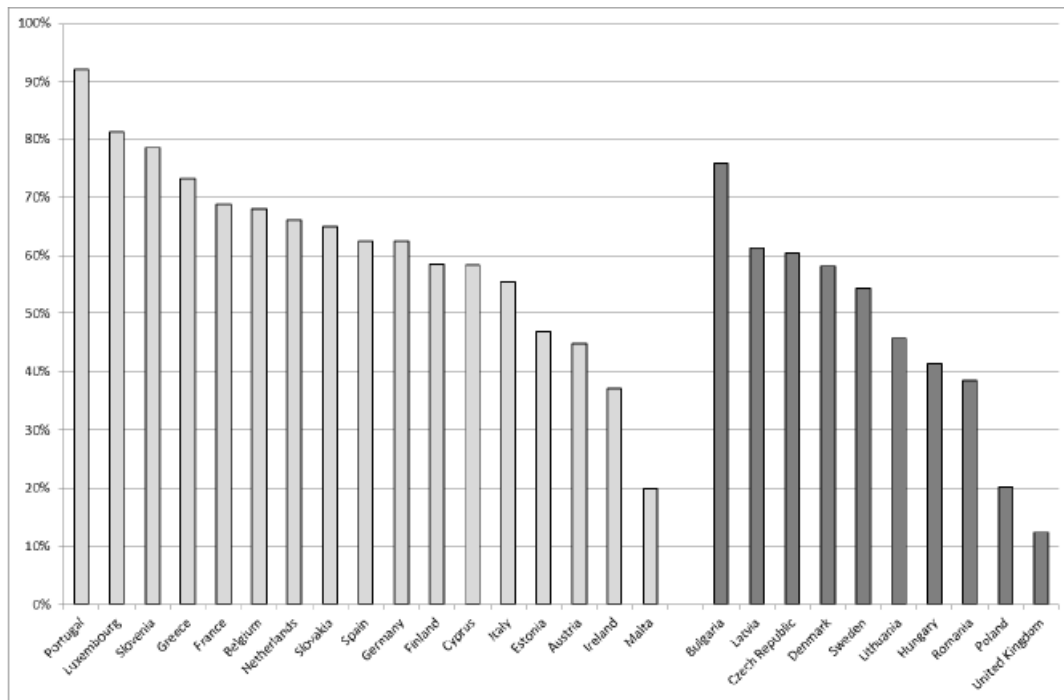
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*Figures and Tables*

Figure 1: UI Net Replacement Rates in 27 EU Member States, 2010



Source: Social Policy Indicator Database (SPIN).

Figure 2: RR before and after the Reform by Family Composition

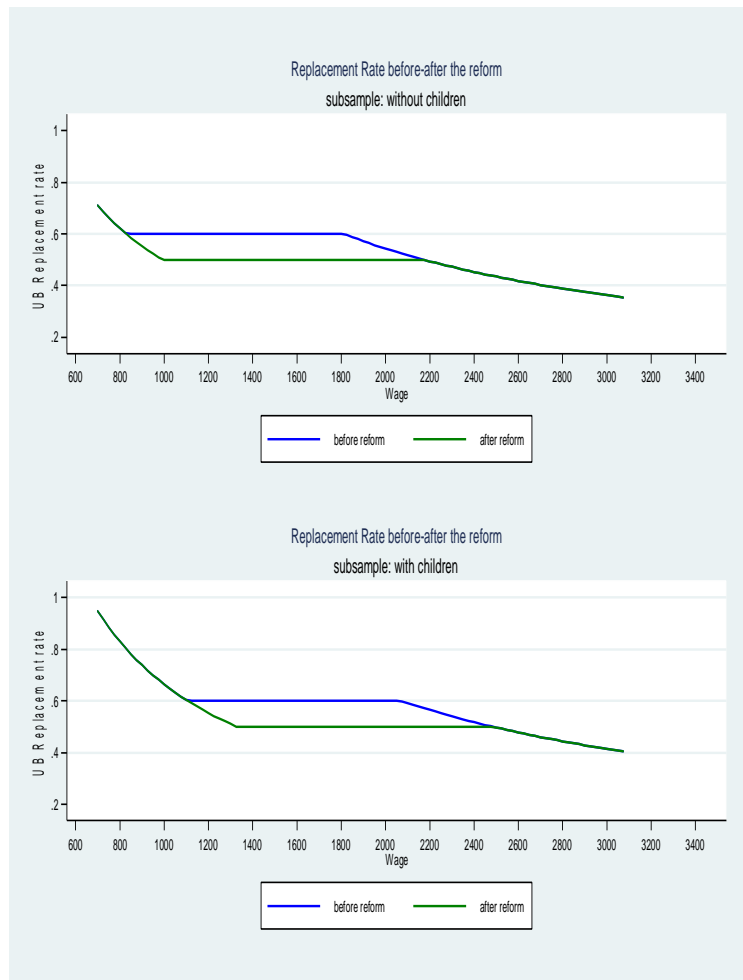


Table 1. Spanish Labor Market 2006 to 2013

	2006	2007	2008	2009	2010	2011	2012	2013
Real GDP growth	4.1	3.5	0.9	-3.8	-0.2	0.1	-1.6	-1.2
Unemployment rate	8.1	8.3	13.9	18.1	20	21.7	24.2	26.9

Note: GDP growth on an annual basis adjusted for inflation and expressed as a percent.

Source: European Commission



Figure 3: Unemployment Inflows from 2011 to 2012 (monthly rates)

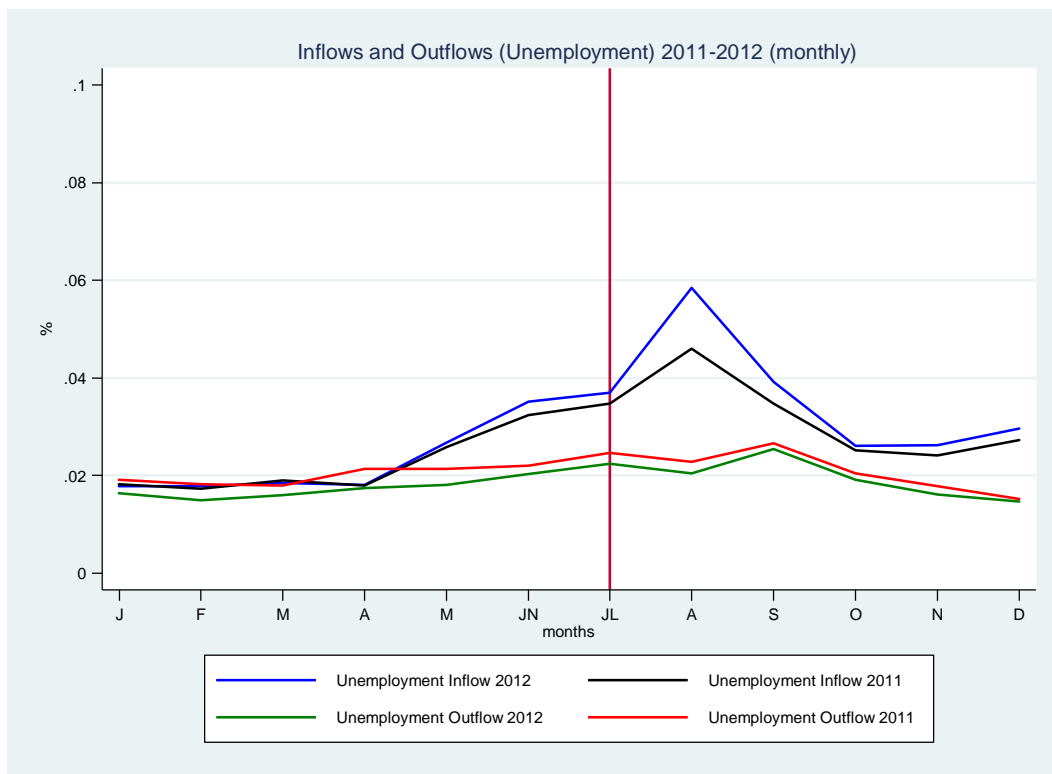


Table 2: Pre-Displacement Descriptive statistics for Treated and Control Groups  
(Percent unless stated otherwise)

	The year of the reform: 2012			One year before the reform: 2011			DiD
	Treatment	Comparator	Diff	Treated	Comparison	Diff	
	July 15 to Jan. 14	Jan.15 to July 14		July 15 to Jan. 14	Jan. 15 to July 14		
Female	0.66	0.68	-0.02* (0.01)	0.65	0.63	0.02* (0.011)	-0.04** (0.014)
Inmigrant	0.07	0.07	0.002 (0.005)	0.07	0.07	0.005 (0.005)	-0.003 (0.008)
With children	0.73	0.72	0.013 (.017)	0.69	0.70	-0.009 (0.018)	0.023 (0.025)
Experience in months	124 [0.83]	118 [0.87]	5.59*** (1.11)	112 [0.92]	107 [0.89]	4.69** (1.18)	0.90 (0.59)
Age in years	41 [0.11]	41 [0.12]	-0.07 (0.20)	41 [0.15]	41 [0.16]	0.23 (0.22)	-0.30 (0.15)
University	0.42	0.41	0.013 (0.010)	0.40	0.38	0.015 (0.011)	-0.002 (0.015)
Length of UI Entitlement in weeks	88 [0.23]	92 [0.27]	-3.95*** (0.36)	92 [0.26]	91 [0.28]	1.05* (0.38)	-5.00*** (.46)
<b>Previous Job Characteristics</b>							
Tenure in weeks	424 [3.0]	408 [3.4]	16*** (5.15)	387 [4.16]	377 [4.13]	9.88 (5.99)	6.12 (7.32)
Monthly wages in Euros	1,547 [6.93]	1,546 [6.36]	0.55 (8.71)	1,527 [10.5]	1,508 [6.9]	19.30** (9.29)	-18.03 (12.32)
Low Skill Job	0.14	0.13	0.010 (0.006)	0.13	0.12	0.005 (0.006)	0.005 (0.01)
Medium-Skill job	0.34	0.31	0.024* (0.005)	0.33	0.34	-0.003 (0.012)	0.027 (0.014)
High Skill job	0.51	0.55	-0.034** (0.010)	0.52	0.52	-0.001 (0.009)	-0.033* (0.015)
Industry	0.21	0.23	-0.201 (0.008)	0.21	0.21	-0.008 (0.009)	-0.012 (0.012)
Construction	0.16	0.19	-0.027*** (0.008)	0.18	0.19	-0.011 (0.009)	-0.016 (0.012)
Services (marketable)	0.40	0.42	-0.010 (0.010)	0.42	0.44	-.025 (0.011)**	0.015 (0.010)
Services (non- marketable)	0.30	0.28	-0.022 (0.009)	0.29	0.27	-0.021	-0.001 (0.014)
Private	.49	.53	-0.038 (0.010)***	0.53	0.55	-0.021 (0.011)**	-0.017 (0.015)
N	4,651	4,075		3,724	3,808		

Differences column displays a two-sample t test.

Standard deviations in brackets and standard errors in parenthesis.

\*90% \*\*95% Significance for the Two-sample t test [Mean(non-treated)-Mean(treated)]. Means are statistically different.

Figure 4. Unemployment Outflows –to jobs (2010-2013)

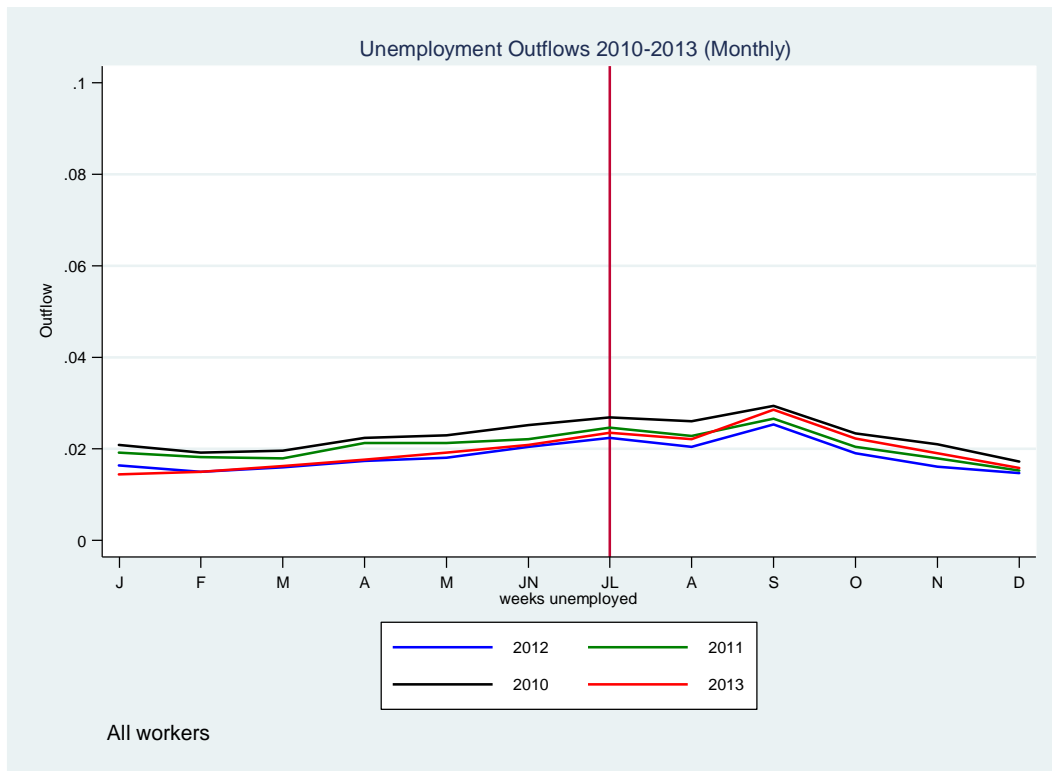
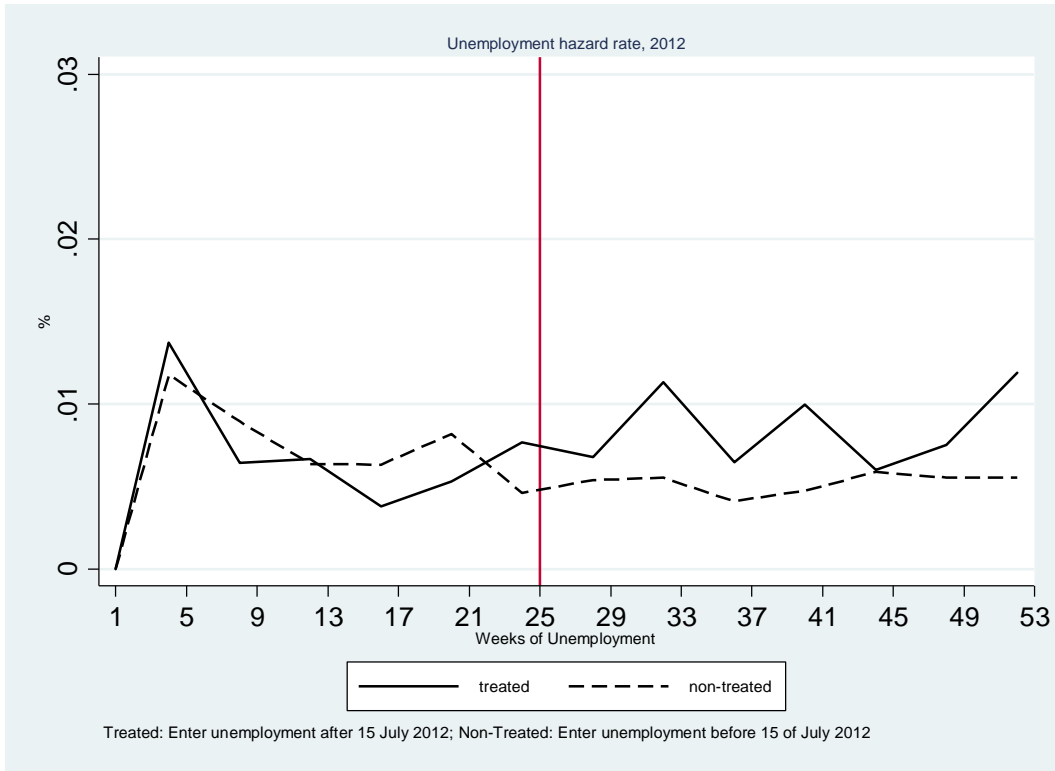


Figure 5. Job Finding Hazard Rates between January 1 and December 31, before and after the Reform  
 Panel A. 2012



Panel B. 2011

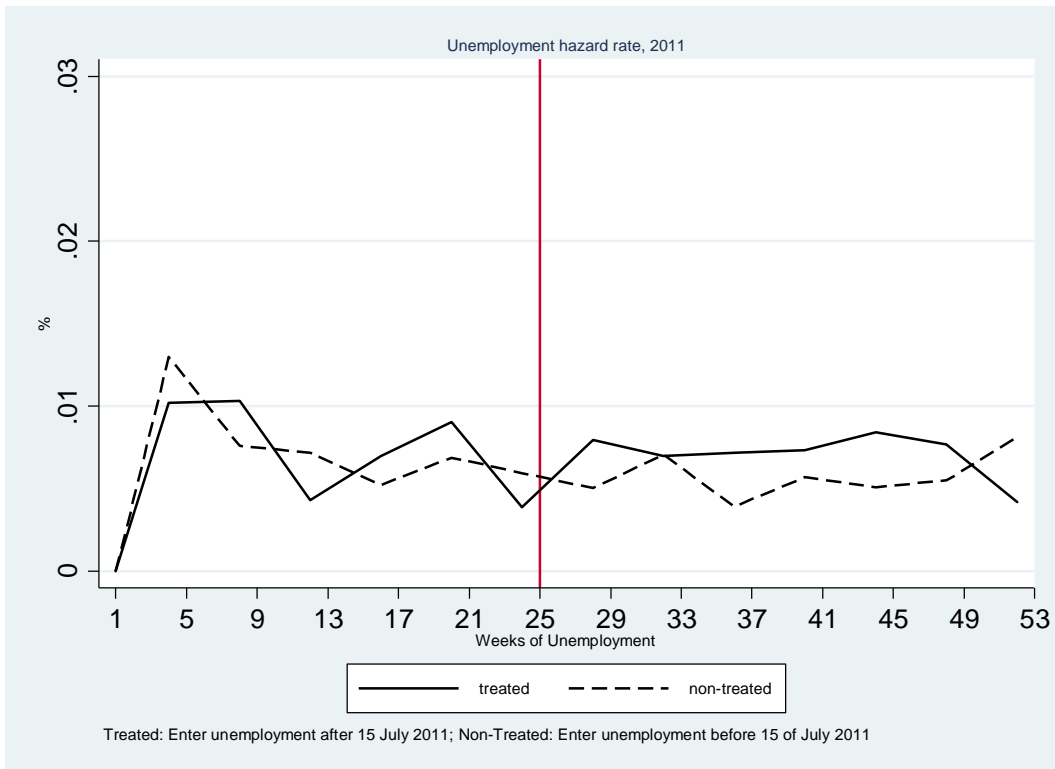


Table 3: Effects of Reducing the RR (odd Ratio, %)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10) DDD	(11) DD	(12) DD-cluster1	(13) DD-cluster2
Reform	0.325** [0.09]	0.147** [0.08]	0.231** [0.09]	0.240** [0.09]	0.251** [0.09]	0.235** [0.09]	0.248** [0.09]	0.212** [0.09]	0.237** [0.09]	0.233** [0.09]	0.283*** [0.11]	0.235* [0.15]	0.235** [0.10]
$D_i^{2012}, D_i^{post\_july}$	X	X	X	X	X	X	X	X	X	X	X	X	X
Weekly dummies	X	X	X	X	X	X	X	X	X	X	X	X	X
Regional, Seasonal and macro controls		X	X	X	X	X	X	X	X	X	X	X	X
UIS covariates			X	X	X	X	X	X	X	X	X	X	X
Individual Characteristics				X	X	X	X	X	X	X	X	X	X
Job Characteristics						X	X	X	X	X	X	X	X
Wages and Tenure					X		X						
GDP*Reform								X		X			

\*\* 95% statistical significance; \*90% statistical significance

Models 9 and 10: In both cases for the third difference, we use workers who were only entitled to four and six months of UI benefits and thus, whose RR was not affected by the reform, as an additional comparison group. In these models sample size increases to 927,811

Model 11: Same model as in 6 but we restrict the analysis to workers entitled to 104 weeks of benefits.

Models 12 and 13: Cluster1 months of entry into unemployment; Cluster 2: month of entry into unemployment interacted with regions

**Table 4: Placebo Estimates of the Effects of Reducing the RR (odd Ratio, %)**

Data from 2011 to 2012 (reform July 15 2012)				
	DD (1)	DDD (2)	DD (3)	DD (4)
Reform	-.093	-.091	-.037	-.052
	[.06]	[.07]	[.12]	[.09]

Note: In DD(1), and DDD(2) we use as placebo data from 2010-2011. That is, we assume that the reform took place in 2011 instead of 2012. T DDD models, column 2 used as the third difference workers with entitlements between 4-6 months and column 3 workers without benefit entitlements.

In models DD(4) and DD(5) we use as placebo the sample of workers who enter into unemployment in 2011-2012 but they were not affected by the reform given they previous employment paths. For these DD models, column 4 used as the third difference workers with entitlements between 4-6 months and column 5 workers without benefit entitlements.

**Table 5: Sensitivity Analysis of Effects of Reducing the RR (odd Ratio, %)**

	Three- months interval	Females	Males	Below median wage	Above median wage
	(1)	(2)	(3)	(4)	(5)
Reform	.211*	.195**	.402***	.174*	.417***
	[.12]	[.10]	[.21]	[.10]	[.20]

Note: Model presented in Column 1 restricts the sample to workers who became unemployed three months before and after the reform; Models in columns 2 and 3 are estimation by gender. Models in columns 4 and 5 are estimation results below and above the median wage (around 1500 Euros)

**Table 6: Composition Effects: Employment Hazard Rate (Competing Risk Approach)**

	Exit to Unemployment (odd ratio)	Job-to-job transition (odd ratio)
Reform	-.0178	.0753
	[.12]	[.06]

Figure 6. Estimated Average Treated and Control Employment Hazard Rates  
(Based on Model in Column 6 in Table 4)

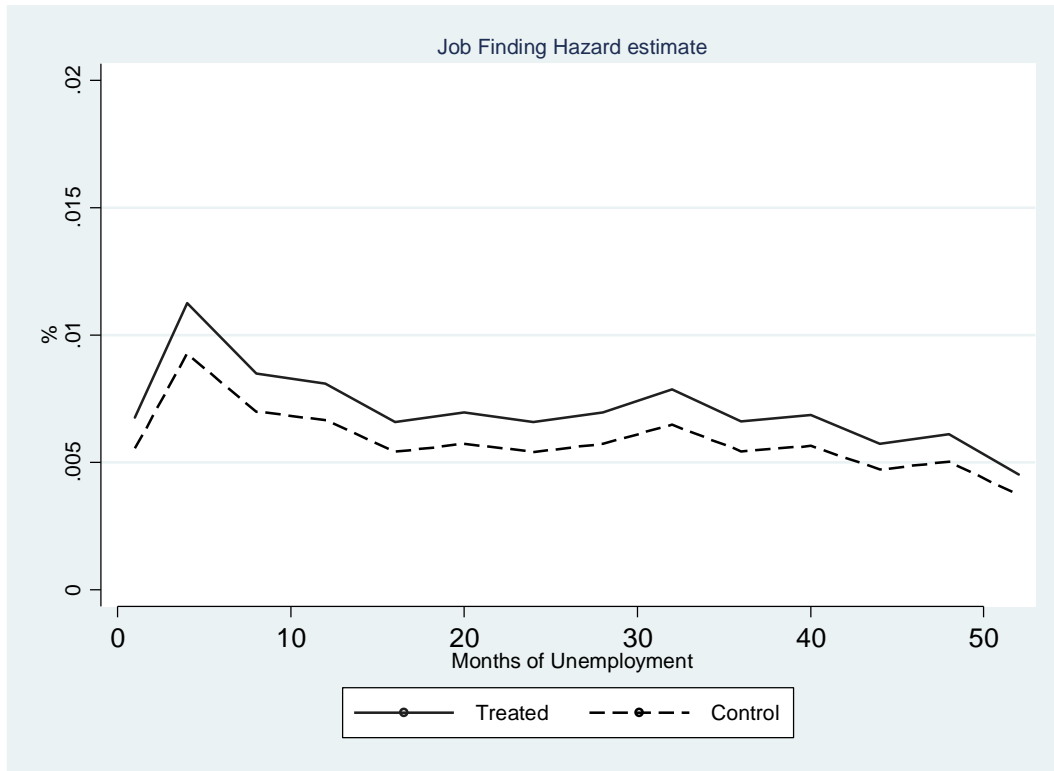


Figure 7. Average Treatment Effect on the Treated in Employment Hazard Rates  
(Based on Model in Column 6 in Table 4)

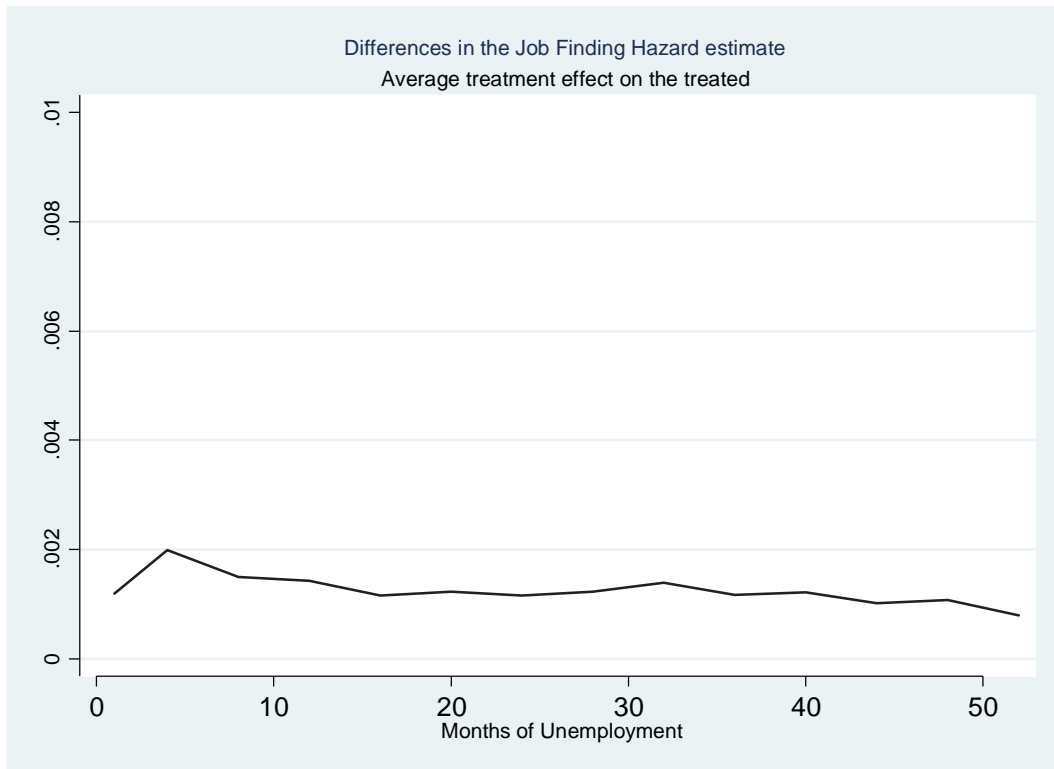


Table 7: Simulated effects on expected duration in first 104 weeks

	Homogeneous effect along the spell of unemployment
Treated	44.6
Counterfactual	46.3
Effect: Change in Expected Unemployment Duration	-1.7 (-3.6%)
Elasticity of Unemployment Duration to Changes in RR	0.22

Table 8. Heterogenous Effects of the Reform Before and After 180 days of Unemployment Spell (odd Ratio, %)

	(1)
Reform*1-6 months of unemployment	0.259** [0.09]
Reform*7*12 months of unemployment	-0.030** [0.13]

\*\* 95% statistical significance; \*90% statistical significance



Figure 8: Estimated Average Treated and Control Employment Hazard Rates  
(Based on Heterogenous Effects Model in Table 6)

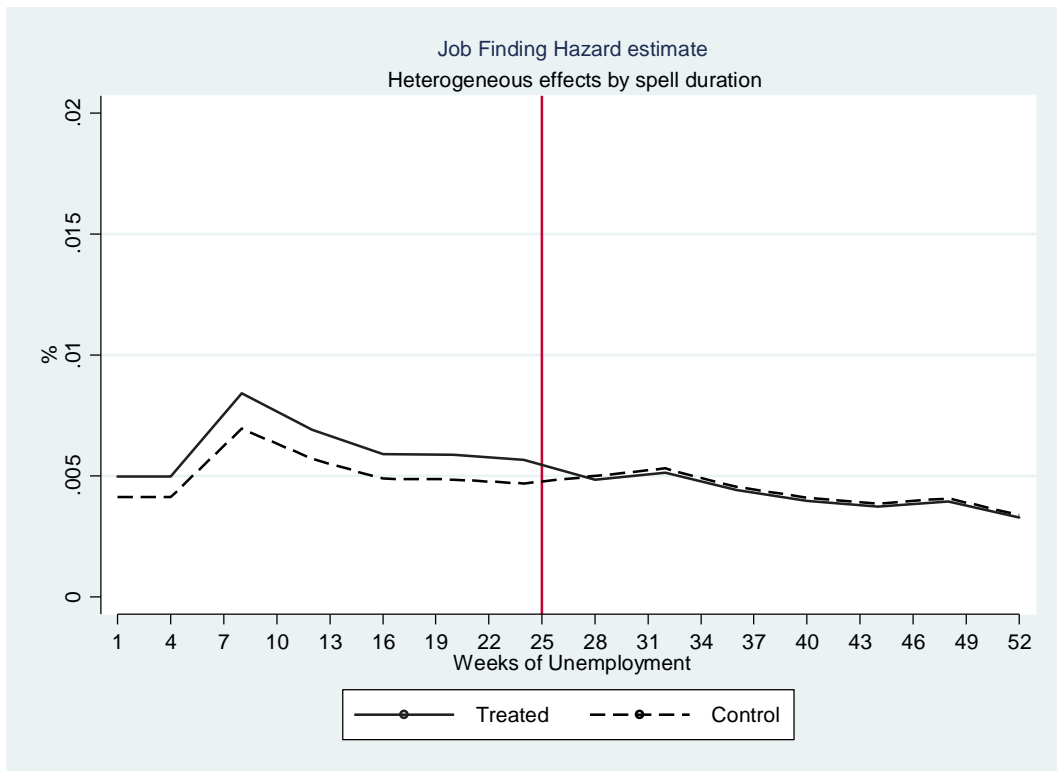


Figure 9: Average Treatment Effect on the Treated in Employment Hazard Rates  
(Based on Heterogenous Model in Table 6)

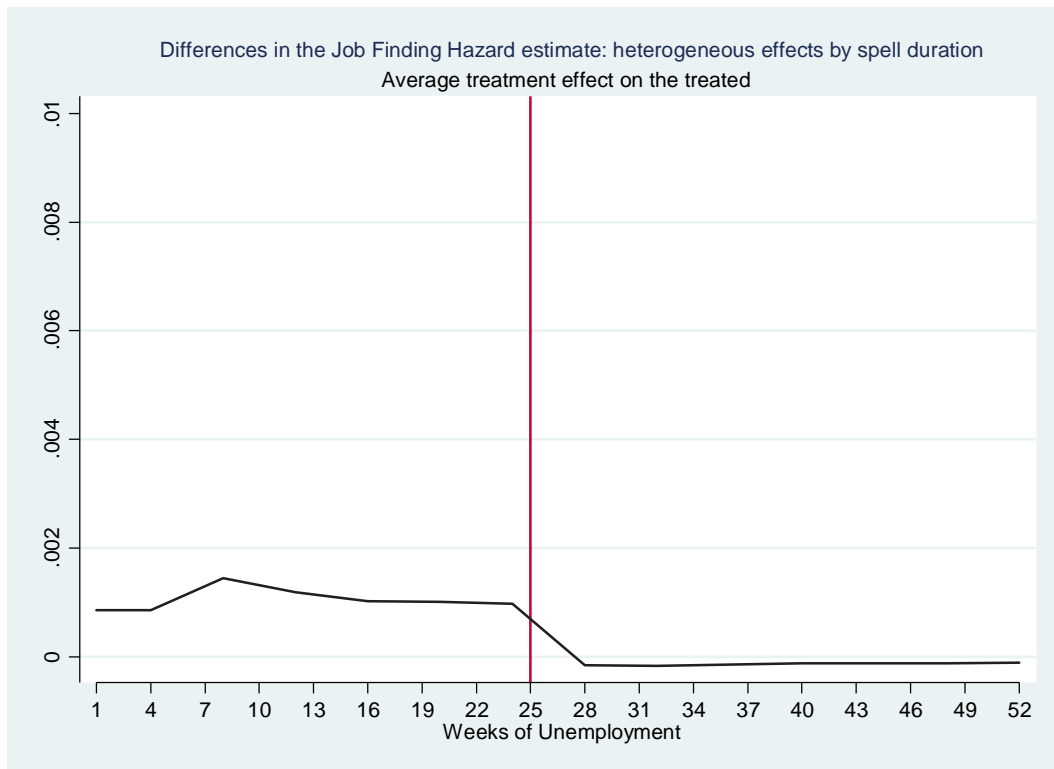


Figure 10: Survival Estimates (Based on the Heterogenous Model in Table 6)

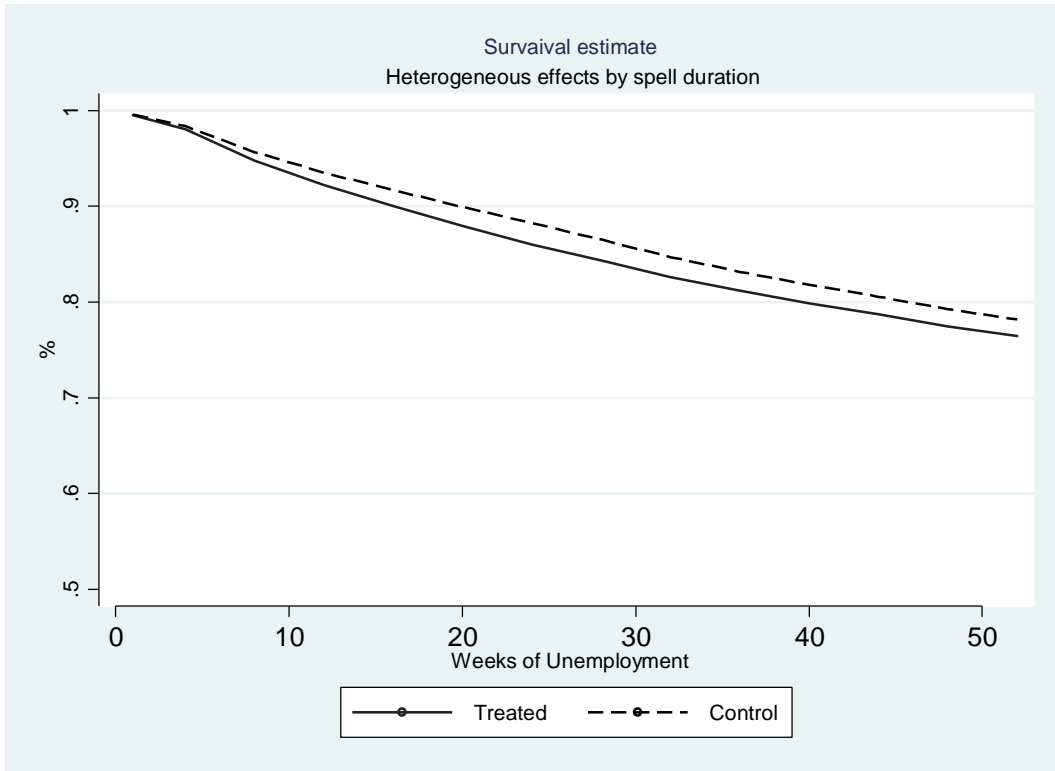


Figure 11 Average Treatment Effect on the Survival Estimates (Based on Heterogenous Model in Table 6)

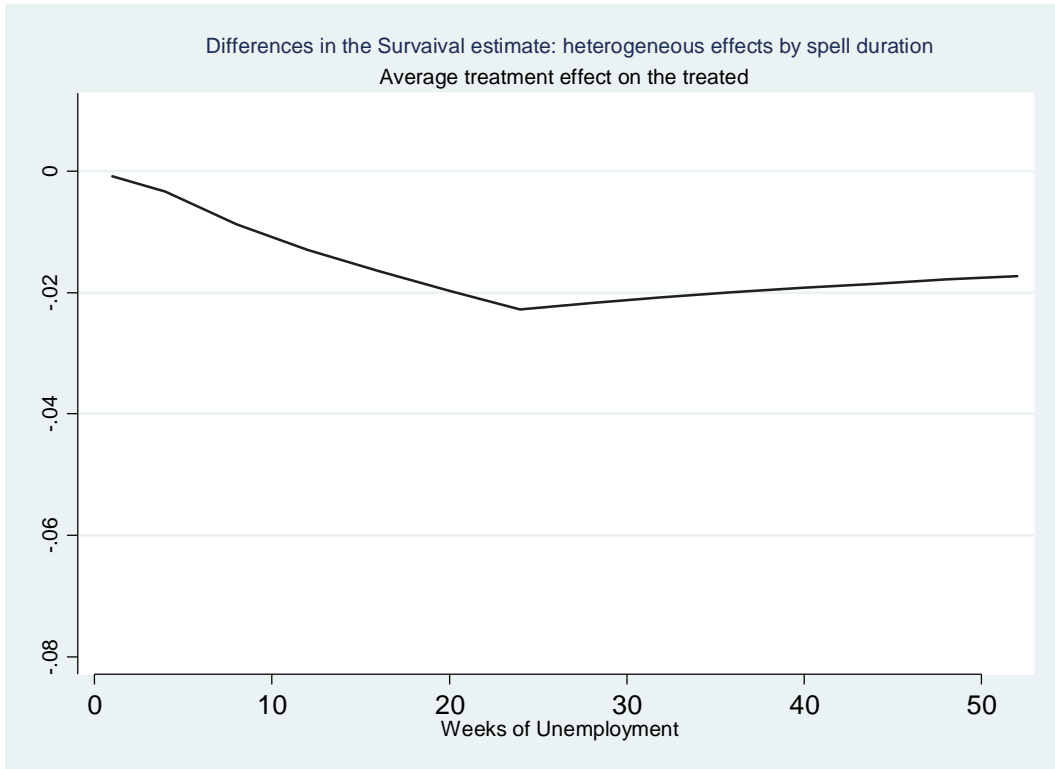


Table 9. Post-Unemployment Effects of the Reform at the Time of the First Job

	Wages (log wages) (coefficient)	Probability of working under a permanent contract (Odd ratio)	Probability of working full-time (Odd ratio)
<b>Full Sample</b>			
Reform	0.22*** [.07]	-0.01 [.01]	0.59*** [.10]
Pre-average means for comparison group	1,601 (median 1,472) 7.38 (logs)	5.63%	17.20%
Placebo (2010-2011)	-0.02 [.12]	0.239 [.23]	-0.026 [.18]
<b>Conditional on job finding</b>			
Reform	0.009 [.09]	-0.126 [.11]	0.519*** [.33]
Pre-average means for comparison group	1586 (median 1455) 7.36 (logs)	27.63%	82.36%
Placebo (2010-2011)	-0.016 [.08]	0.212 [.22]	0.348 [.20]

Appendix

*Table A.1 Results from a the duration model for the Unemployment*

	<b>Coef</b>	<b>Se</b>
<b>Reform</b>	<b>0.244</b>	<b>0.068</b>
Unemployed after the reform	0.100	0.047
Unemployed in 2012	-0.037	0.047
<u>Duration Dependence (weeks)</u>		
$\lambda_1$ (Week1-4)	0.566	0.058
$\lambda_2$ (Week5-8)	0.286	0.067
$\lambda_3$ (Week9-12)	0.225	0.070
$\lambda_4$ (Week13-16)	0.040	0.077
$\lambda_5$ (Week17-20)	0.083	0.077
$\lambda_6$ (Week21-24)	0.009	0.081
$\lambda_7$ (Week25-28)	0.030	0.083
$\lambda_8$ (Week29-32)	0.063	0.082
$\lambda_9$ (Week33-36)	-0.103	0.090
$\lambda_{10}$ (Week37-40)	-0.088	0.090
$\lambda_{11}$ (Week41-44)	-0.200	0.096
$\lambda_{12}$ (Week45-48)	-0.067	0.091
$\lambda_{13}$ (Week49-52)	-0.484	0.112
<u>Individual Characteristics</u>		
Females	0.487	0.038
Experience	0.166	0.050
Age (in logs)	-1.329	0.082
University	0.064	0.036
Children	0.060	0.020
Immigrant	-0.394	0.070
<u>UI covariates</u>		
UI Entitlement Length	-1.278	0.101
Replacement Rate	-1.040	0.275
<u>Previous Job Characteristics</u>		
Industry	-0.167	0.045
Construction	-0.080	0.048
Commerce and Hotels	-0.052	0.043
Firm Size (in logs)	-0.109	0.009
High Skill	0.115	0.056
Medium Skill	-0.154	0.041
Private Firm	-0.097	0.039
GDP growth rate (quarterly)	0.385	0.080
Constant	-7.437	0.508

*Note: Dummy variables for regions and monthly dummies are omitted.*

### A.1- Composition Effects

One could argue that the results are driven for differences in the composition of unemployed workers before and after the reform. To test for composition effects we estimate the employment exit rate taking into account whether the reform could affect it in some way. In particular, one could argue that given that the replacement rate drop after 15<sup>th</sup> July, the population of unemployed after the reform could be “different” in observed and unobserved characteristics, relative to the individuals that become unemployed before the reform. To test for this idea we estimate the employment hazard rate, and we particularly look at whether the probability of remaining in the same job or making a job-to-job transition changes due to the reform. Henceforth, for the employment hazard rate we define two competing risks, conditional on exit from employment. They are a job-to-job transition versus an exit to unemployment. For the identification strategy of the “reform” is also used the DD approach applied for the unemployment hazard. Also, sample restrictions closely resemble the ones imposed for the unemployment analysis. Particularly, for the employment hazard we take employees holding a full-time permanent contract and whose tenure is 36 or lengthier at the time of the reform.

$$h_i(j/x, d) = 1 - \exp\left(-\exp\left(x_i(j)\beta + \alpha_1 D_i^{2012} + \alpha_2 D_i^{postJuly15} + \alpha_3 (D_i^{2012} * D_i^{postJuly15}) + G(j)\delta\right) \lambda_i(j) + V_i(j)\gamma + Z_i\eta\right)$$

Though in this case, the  $h_i(j/x, d)$  represents the employment hazard rate. The set of covariates are the same but in this case job characteristics refer to the current job.

Results relative to the policy variable are displayed in Table 8. From this table we can state we do not find evidence of any composition effects. That is, that the reform did not affect the employment exit rate either to job-to-job transitions or to exit to unemployment. The coefficient associate to the reform variable is not statistically significant for any of the competing exits considered.