When the Going Gets Tough...

Financial Incentives, Duration of Unemployment and Job-Match Quality

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First draft: April 2015; This draft: June 2017

Abstract

In the aftermath of the Great Recession, the Spanish government reduced the replacement rate (RR) from 60% to 50% after 180 days of unemployment for all spells beginning on July 15, 2012. Using Social Security data and a Differences-in-Differences approach, we find that reducing the RR by 10 percentage points (or 17%) increases workers' odds of finding a job by 41% relative to similar workers not affected by the reform. To put it differently, the reform reduced the mean expected unemployment duration by 5.7 weeks (or 14%), implying an elasticity of 0.86. Alternatively, a Regression Discontinuity approach indicates that the reform increased the job finding rate by 26%. We find strong behavioral effects as the reform reduced the expected unemployment duration right from the beginning of the unemployment spell. While the reform had no effect on wages, it did not decrease other measures of post-displacement job-match quality. After 15 months, the reform decreased unemployment insurance expenditures by 16%, about half of which are explained by job seekers' behavioral changes.

Key words: Labor supply, financial incentives, unemployment insurance replacement rate, hazard function models, wages and job-match quality, forward-looking non-employed workers, and longitudinal Social Security data.

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 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, the Netherlands, and Spain, just to name a few countries, have reduced their UI benefits generosity.

In this paper, we analyze the effects of a *reduction* of 10 percentage points (or 16.66%) in the level of UI benefits in relation to expected earnings (the replacement rate, RR hereafter) on the transition to employment (short-run effects), subsequent wage and salary earnings, job stability, and job quality (medium-run effects), and changes to UI expenditures within a context of economic slowdown in Spain. More specifically, 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 reduced from 70% to 50%. Prior to this reform, the reduction (*after* 180 days of unemployment) went from 70% to 60%.[1] Relying on a sudden policy change and using administrative data, this study serves as a valuable addition to the growing literature on how unemployed workers respond to UI generosity.[2] Perhaps more importantly, as the

[1] To the best of our knowledge, *only* Carling *et al.* (2001) analyze the impact of a *reduction* in the RR from 80% to 75% (representing 6.25% decrease) in January 1, 1996 in Sweden at a time of fiscal austerity and economic slowdown.
[2] Using Current Population Survey data and time, state and individual variation, Farber and Valleta (2013) and Rothstein (2011) find small negative effects of expanding UI benefits on the probability that the eligible unemployed exit unemployment, but no effects on the probability of entering employment. These effects are concentrated among the long-term unemployed. Card *et al.* (2015) use a regression kink design to

drop in the RR occurs *not* at the beginning of the unemployment spell, but 26 weeks afterwards, we are able to ascertain whether the reform changed displaced workers' search behavior *before* their UI benefits dropped. Finally, we also measure the effects of the reform on post-displacement job-attributes, including wages, providing evidence on whether the reform affected workers' job-match quality.

Employing Social Security longitudinal data from the *Continuous Sample of Working Histories* (CSWH), our empirical approach uses two alternative identification strategies: a Differences-in-Differences approach (DiD hereafter) and a Regression Discontinuity approach (RD hereafter). In the DiD approach, we compare the non-employment spells of individuals eligible to be affected by the cut in the RR rate (our "treatment group") before and after the reform to those individuals with similar potential UI benefit levels, but who were unaffected by the reform because they were entitled to no more than 180 days of UI benefits (our comparison group). In the RD approach, "treated" individuals are those with entitlement lengths larger than 6 months who became unemployed after the reform, and are compared to individuals with similar length entitlements, but who became unemployed during the first half of 2012. 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 the exhaustion of UI benefits, which allows us to study how the job-finding rate and

estimate the effects of UI benefits on the unemployment spell in Missouri from 2003 to 2013, differentiating before and after the Great Recession. Johnston and Mas (2015) use a regression discontinuity design to estimate the effects of a reduction in the potential duration of UI on job search of UI recipients and the aggregate labor market.

other post-displacement characteristics evolve after the exhaustion of benefits.[3] We observe these workers' employment histories up until March 31, 2014.

Using the DiD approach, we find that reducing the RR by 10 percentage points (or 16.66 percent) increases the workers' job-finding rate by at least 41% relative to similar workers not affected by the reform. To put it differently, the reform reduced the mean expected non-employment duration by 5.7 weeks (or 14%), implying an elasticity of non-employment duration relative to benefit generosity of 0.86.[4] Alternatively, a RD approach indicates that the reform increased the job-finding rate by 26%.

Interestingly, as the effect of the reform is observed *well before* the drop in the RR actually takes place, we find evidence of anticipatory job search behavior. More specifically, we find that the reform increased the probability of finding a new job by 43% during the first 12 weeks of the non-employment spell for treated workers relative to those in the comparison group. During weeks 13 to 26, as the drop in the RR approaches, the effect of the reform is even stronger (with an increase in the job-finding rate of 51%). Importantly, the effect of the reform *after* the drop in the RR is smaller and no longer statistically significant. Hence, we cannot reject the null hypothesis of no effect of the reform *after* 180 days of non-employment, suggesting that most of the effect of the reform takes place *prior* to the actual drop in the RR. This is consistent with forward-looking displaced workers as they increase job search activity from the beginning of the non-employment spell. While this finding is conceptually different from the spikes in the exit rate *shortly* before benefit expiration documented by Katz and Meyer (1990) and Meyer

[3] Card, Chetty, and Weber (2007a), Lalive (2007), Van Ours and Vodopivec (2008), Schmieder, von Wachter, and Bender (2016), and Nekoei and Weber (2015) also exploit Social Security data. They study the effects of an extension of potential UI duration on post-UI job quality.

[4] Interestingly, this estimate is close to the Missouri estimates found by Card *et al.* (2015) during the Great Recession and its aftermath (0.65-0.9).

(1989), it is consistent with the behavioral response to changing potential UI duration found by Card, Chetty, and Weber (2007b) and Nekoei and Weber (2015) in Austria, Johnston and Mas (2015) in Missouri, and Kolsrud *et al.* (2015) in Sweden.[5]

While we find that the reform had no effect on post-non-employment wages (as in Card, Chetty, and Weber, 2007b; and Johnston and Mas, 2015), it did not *decrease* alternative measures of post-displacement job-match quality. More specifically, it increased the probability of exiting to both a fixed-term and permanent contract job (with the effect being larger for the latter), a full-time job (as opposed to a part-time one), or an occupation on par with the pre-displacement one. Our findings on alternative measures of post-displacement job quality are consistent with those of Schmieder, von Wachter, and Bender (2016), but contrast with those of Card, Chetty, and Weber (2007b), and Nekoei and Weber (2015).[6] Note that, in contrast with our study, all of these papers focus on extending UI *duration* as opposed to changing the level of benefits.

Our results are robust to: (1) controlling for seasonality, (2) the use of alternative comparison groups, and (3) alternative specifications. Moreover, placebo tests suggest

[5] Note that our anticipatory effect also differs from that of Carling *et al.* (2001) who estimate the anticipatory effect of the *announcement* of the reform (announced in June 1995, but implemented on January 1996 on all unemployment spells regardless of when they started). Kolsrud *et al.* (2015) provide a general framework to analyze the optimal time profile of benefits during the unemployment spell. Then, using Swedish data and exploiting duration dependence kinks in the RR, they find evidence consistent with individuals being forward looking. More importantly, their paper finds that the response to changes in UI benefits is larger the sooner the change occurs in the UI spell. They also analyze how unemployed workers' expenditures are affected by these UI changes.

[6] Card, Chetty, and Weber (2007b) find no effects of the UI extension on wages and other non-wage measures of job quality. Johnston and Mas (2015) do not find that a cut in UI duration affects re-employment earnings in Missouri. Nekoei and Weber (2015) find *positive* wage effects suggesting that the policy shifted upwards the reservation wage, that is, that in response to higher UI benefits, "*workers became more selective and increased their wage targets*". Yet, Nekoei and Weber (2015) do not find economically significant effects on non-wage measures of job quality. Others have found no statistically significant effects of UI on wages (Lalive, Van Ours, and Zweimuller 2006 and Lalive 2007 in Austria, Van Ours and Vodopivec 2008 in Slovakia, and Centeno and Novo 2009 in Portugal). Degen and Lalive (2013) also find evidence of a positive UI wage effect in Switzerland. As explained by Nekoei and Weber (2015) these different results can be reconciled by the relative importance of the effort versus the selectivity margins in job search across different populations.

that our results are not due to systematic differences in trends between the groups we study.

We estimate that after 15 months, the reform saved the public sector an average of 129,216 euros per 100 displaced workers – a 16% reduction of total UI expenditures. During the first 6 months of unemployment, all of the savings are due to behavioral effects (the indirect component). After 180 days of unemployment, however, we observe a direct effect. Between the 7th and 15th months, the relative weight of the direct component increases from one third to more than half. Nonetheless, by the 15th month, behavioral changes continue to be an important factor driving the reduction in UI expenditures due to the policy change, as they explain close to half of the UI costs reduction. These findings contrast with those of Lalive, Van Ours, and Zweimuller (2006), as these authors find that job seekers' behavioral responses in Austria explain no more than 10% of their policy costs change.

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%) and highly segmented labor market (with about 24% 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, 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 amid low economic activity, soaring government budget deficit, and extreme uncertainty, our analysis is less subject to endogenous policy bias than other studies, as one would have expected policy makers to increase, *not* decrease, the RR.[7]

Our study is similar to that of Carling *et al.* (2001), and Lalive *et al.* (2006), but differs in two important ways.[8] First, since the drop in the RR in Spain takes place after 6 months of unemployment, we can test for "anticipatory" effects of the reform on the job search behavior of workers. Previous papers could not test this because, in their analysis, the RR dropped from the beginning of the unemployment spell. Second, we analyze the effect of the reform on post-displacement wages and job quality.[9]

The paper is organized as follows. Section two reviews the empirical literature. Section three presents a description of the Spanish unemployment insurance system and the Law 20/2012. Sections four and five present the empirical strategy and the data, respectively. Section six presents the results, and section seven concludes.

2. Empirical Literature Review on the Effects of Changing UI Benefit Levels

The effect of economic incentives on individuals' behavior has been widely studied, especially within the context of UI benefits and transitions out of unemployment.[10] In

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[7] 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).*"

[8] Carling *et al.* (2001) analyze the effect of a *decrease* in the RR from 80% to 75% in Sweden in 1996. And Lalive *et al.* (2006) 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.

[9] To the best of our knowledge, only Meyer (1989) has 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%) in the US on post-displacement earnings. As explained earlier, others look at the effects of extending UI duration on post-UI job match (Card, Chetty, and Weber 2007b, Lalive, Van Ours, and Zweimuller 2006, Lalive 2007, Van Ours and Vodopivec 2008, Centeno and Novo 2009, Degen and Lalive 2013, and Nekoei and Weber 2015).

[10] See theoretical analyses by Van den Berg (1990), survey by Atkinson and Micklewright (1991), and discussion by Tatsiramos and Van Ours (2014) on the theoretical and empirical evidence on UI incentives influencing the behavior of UI recipients.

this section, we review studies analyzing the effects of changing levels of UI benefits as opposed to potential benefit duration.[11]

Earlier studies have exploited variation of UI benefits entitlement across time, regions, or age groups, and have found an elasticity of unemployment with respect to the UI benefit level between 0.1 and 1.0, implying that a 10% *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 (Moffit 1985; Katz and Meyer 1990; and Meyer 1989). However, the evidence for Continental Europe is scarcer and finds no significant effects (van den Berg 1990; and Hernæs and Strøm 1996).

To address concerns that variation in UI benefit entitlements is correlated to pre-displacement earnings, which are likely to be correlated with unobserved heterogeneity affecting unemployment duration, several authors have exploited a reform changing the level of UI benefits and used a DiD approach instead. In these cases, the estimated effects are far from negligible in Continental Europe. Lalive *et al.* (2006) found that an *increase* in the RR of 15% in Austria in the late 1980s led to an increase in unemployment duration of 0.38 weeks (or 5%), implying an elasticity of 0.33. Estimates from Carling *et al.* (2001) for Sweden in the mid-1990s are considerably larger. They estimated that a 6% *decrease* in the RR led to a 10% increase in the exit rate to employment (implying an elasticity of 1.6).[12] Uusitalo and Verbo (2010) studied a reform that took place in January 2003 in Finland, where the average benefit increase was 15 percentage points for the first 150

[11] See Hunt (1995), Winter-Ebner (1998), Card and Levine (2000), and Lalive and Zweimüller (2004), for studies using a similar methodology as ours to analyze the effects of changing potential UI benefits duration. As discussed in the Introduction, a recent related literature exploits a regression discontinuity design to estimate the effects of potential UI benefit duration (Nekoei and Weber 2014 and Schmieder, von Wachter and Bender 2012 and 2016, among others).

[12] 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.

days of the unemployment spell. They found that the change in the benefit structure reduced the reemployment hazards by, on average, 17 percentage points.[13]

In the US, Meyer (1989) exploited 16 UI benefit increases during 1979 and 1984 across five states, and found that an average increase in UI benefits of 9% led to an increase of UI receipt duration by about one week. In contrast, Meyer and Mok (2007) found considerably smaller effects than those traditionally found in the US. They exploited an unexpected 36% 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% increase in UI benefits would lower the hazard of ending a UI spell by about 3%. Moreover, the authors found evidence that the reform substantially affected the incidence of claims, introducing incidence bias in their duration estimates. More recently, Card *et al.*, (2015) exploit quasi-experimental variation in the UI benefit schedule in Missouri and find that UI durations are more responsive to benefits during the Great Recession and its aftermath with an elasticity between 0.65 and 0.9 compared to about 0.35 pre-recession.

Using a random-assignment-like variation in unemployment benefit replacement ratios in Norway in the 1990s, Roed and Zhang (2003 and 2005), confirmed that the Continental European estimates are closer to those in the US and the UK, despite the substantial differences in UI institutions. These authors found that the average elasticity of the unemployment hazard rate with respect to unemployment benefits is around 0.95

[13] Note that this result could be interpreted as a lower bound since at the same time that benefits level were increased, the severance pay system was abolished.

for men and 0.35 for women, implying that a 10% *reduction* in benefits may cut a 10-month duration by approximately one month for men and 1 to 2 weeks for women.[14]

In Spain, Bover *et al.* (2002) exploited a 1984 reform to analyze the effects of UI benefits receipt versus non-receipt on unemployment duration between 1987 and 1994.[15] They found 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.*" Most recently, García-Pérez and Rebollo-Sanz (2015) use 2002 to 2007 data and a timing-of-events approach (Abbring and van den Berg 2004) to estimate that the difference in the job-finding probability between workers who receive benefits and those who do not varies between 10 and 20 percentage points during the first months of the unemployment spell in Spain.[16]

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 become unemployed involuntarily are entitled to UI benefits if they have accumulated at

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[14] 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 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.*"

[15] 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 that co-existed with otherwise similar workers enjoying generous benefit entitlements. The authors argue that this "*benefit/non-benefit division is close to a random assignment*". They use Labor Force Survey matched files.

[16] García-Pérez and Rebollo-Sanz (2015) present an assessment of the overall influence of UI entitlement duration on employment stability, simultaneously accounting for the competing effects of benefits on the duration of both unemployment and employment and also considering the occurrence of state dependence. They show that the job-finding rate during the first months of unemployment for those with UI ranged between 10% and 15%.

least 12 months of employment without receiving unemployment benefits within the last 72 months. Individuals receiving full-time disability benefits, voluntary job quitters, and those over the age of 65 are excluded from UI benefits. Benefits end when individuals cease to be unemployed or complete the maximum benefit period.

Benefit duration also depends on the number of accumulated months of employment without receipt of unemployment benefits within the last 72 months. 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.[17] For instance, to be eligible to receive 6 months of UI benefits, workers need to accumulate 18 to 24 months of employment from when they last received UI benefits, whereas to be eligible to receive 8 months of benefits, they need to accumulate 24 to 30 months of employment. This implies that workers with different UI entitlements may well have similar labor market paths.[18]

The UI benefit amount is determined by multiplying the RR by the average basic salary over the 6 months preceding unemployment. The monthly payment is 70% of a worker's average basic pay for the first 180 days of benefits and 60% from the 181st day onwards. UI is also subject to a floor of 75% of the statutory minimum wage (SMW) and a ceiling of between 170% and 220% of the SMW depending on a worker's family circumstances.[19] Esser *et al.* (2013) estimate that within the EU, the Spanish net UI

[17] UI benefit entitlement in Spain is about 30% of the months employed with a maximum of 24 months. To compute the potential duration, one must take into account the most recent employment record since the last time the worker used benefits looking back to a maximum of six years.
[18] For instance, two individuals with identical labor-market experience up until the last 31 months will have different UI entitlement if one became unemployment after 24 months of employment and the other one after 31 months.
[19] 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.

replacement rate ranges in the middle of the RR distribution (see Figure 2 in Esser *et al.*, 2013).

Once UI benefits expire, workers may be entitled to UA. UA is a 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% 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 20/2012. This policy received widespread media attention in newspapers, television, and radio.[20] On July 13, 2012, the vice president, Soroya Saenz de Santamaría, explained the details of the law: all unemployment spells starting on July 15, 2012 would have the RR reduced from 70% to 50% beginning on the 181st day of the unemployment spell. Hence, the RR was reduced from 60% to 50% (16.66%) after 180 days of receiving UI benefits for all workers whose unemployment spell had begun on July 15, 2012 or thereafter. Because the drop in the RR took place *after* 180 days of UI receipt, we are able to study the differential effects of the reform on displaced workers' job search behavior before and after they experienced the RR drop.

[20] A quick search gave us the following links to articles that came out in major newspapers (*El Pais* and *El Mundo*), and in the website of the main Spanish TV channel (TVE) on July 11 2012, the day the Spanish Prime Minister announced the reform:
http://economia.elpais.com/economia/2012/07/11/actualidad/1342000162_261004.html
http://www.elmundo.es/elmundo/2012/07/11/economia/1341993572.html
http://www.rtve.es/noticias/20120711/gobierno-recorta-paro-partir-del-sexto-mes-del-60-50-base-reguladora/545141.shtml

In addition to the media attention that the policy received, the government widely informed the public about the consequences of this reform for UI recipients' current and future benefits. In particular, the Spanish Public Employment Service (INEM) posted a web page on July 16, 2012 explaining the consequences of the reform on UI recipients' benefits.[21] Moreover, individuals have access to a website from the Spanish Department of Labor that estimates his or her UI benefits based on date he or she became unemployed and his or her employment history.[22] As such, UI recipients quickly became aware of the reform and understood the consequences of the policy change for their current and future benefit amounts.

Additionally, because the reform took place two days after being announced, strategic layoffs are unlikely. To address this concern, Figure 1 shows the UI inflows during 2011 and 2012. While there is an increase in UI inflows at the beginning of the summer months, we observe a similar trend of UI inflows in 2011 and 2012 *prior* to July 15, 2012. After the reform, there is 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%.

It is also important to note that in Spain, most of those eligible to receive UI benefits file for benefits. In our sample, the estimated UI take-up rate is over 90%. In addition, as the RR did not change during the first 180 days of UI benefit intake, concerns

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[21] See http://www.citapreviainem.es/real-decreto-ley-20-2012-recortes/ .

[22] See https://sede.sepe.gob.es/dgsimulador/introSimulador.do .

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

On February 10, 2012, a labor market reform that affected collective bargaining agreements at the firm level and reduced dismissal costs for permanent workers was implemented. As our inflows into unemployment span from January 1 to December 31 2012, this other reform affected most of our workers in the same way. Concerns that inflows during January and the first 10 days of February may bias our results are ruled out when we estimate the effects of the decline of the RR on inflows within 3 months of July 15, 2012.

4. The DiD Empirical Strategy and Theoretical Predictions

Identification in our analysis comes from comparing the hazard rate of UI recipients who were displaced between July 15 and December 31, 2012 and whose RR after 180 days of UI receipt dropped from 70% to 50% to similar workers who lost their job between January 1 and July 14, 2012 and whose RR after 180 days dropped from 70% to 60%. To control for any other changes that may have occurred in the Spanish economy at the time, we use as a comparison group UI recipients with similar potential UI benefit levels and who were displaced at the same time, but who were entitled to at least four months of UI benefits, but no more than 180 days of UI receipt. Hence, their RR after 180 days of unemployment was unaffected by the reform. Figure 2 shows the unemployment inflows for these two groups during 2012, and there is no evidence of strategic layoffs prior to the reform. The inflow trends across the two groups are quite similar with minor differences in February and during the last two months of 2012. In the robustness analysis, we show that our main results are robust to only using workers unemployed within 3 months of the reform.

4.1 Basic Specification of the Hazard Model

To estimate how the drop in the RR affects the probability of finding a job, we apply a mixed 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*} for each individual *i* takes the complementary log-log form (Jenkins, 2005). Thus, the general specification of the estimated hazard rate is as follows:

$$h_i(j) = 1 - \exp(-\exp(y_i(j))) \qquad (1)$$

Where, for each individual *i*, $y_i(j)$ is expressed as:

$$y_i(j) = \left(h_{i0}(j) + \alpha_1 D_i^T + \alpha_2 D_i^{post} + \alpha_3 D_i^{post} * D_i^T + \beta X_i(j) + \theta_{iu}\right) \qquad (2)$$

In equation (2), the term D^T is a dummy that takes the value 1 if the worker is entitled to more than 180 days of UI benefits and 0 otherwise; D^{post} is a dummy that takes value 1 if the worker entered unemployment after July 14, 2012 and 0 otherwise. Our coefficient of interest, α_3, measures the effect of the policy on the job-finding rate of UI recipients affected by the reform. *X(j)* is a vector of explanatory variables. $h_0(j)$ captures the duration dependence of the respective hazard, and θ_u is an unobserved heterogeneity term. Because unobserved heterogeneity may affect the estimated pattern of duration dependence (sorting), we control for it by assuming it follows a gamma distribution

(Jenkins, 2004a and 2004b).[23] In the results section, we show that our estimates are robust to alternative assumptions of the unobserved heterogeneity distribution.

Vector, *X(j)*, controls for four different sets of explanatory variables. First, it controls for the quarterly GDP growth, and a set of state and quarter dummies. Note that by using individuals who became unemployed at the same time but with no more than 180 days of UI entitlement, and controlling for the quarter the unemployment spell is observed, we are netting out any seasonality that may occur across quarters. The state dummies and the quarterly GDP growth control for state differences and macroeconomic and business cycle effects, respectively. Second, we add a set of individual characteristics likely to be correlated with finding employment, such as age, gender, nationality, education, pre-displacement labor-market experience, and presence of children in the household. Third, we add information on the individual's UI benefit receipt, such as the potential length of UI entitlement at unemployment entry, and two dummy variables indicating whether the individual is receiving UI or UA. These last two variables (as well as the age of the worker) are time-varying along the unemployment spell.[24] Finally, we control for pre-displacement job characteristics: tenure, blue- versus white-collar job indicator, industry, firm ownership (public versus private), and type of contract (fixed-term versus permanent contract).

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[23] A convenient assumption for the unobserved heterogeneity component used by many authors is that it has a gamma distribution. This distribution has the appropriate range $(0\ \infty)$ and it is mathematically tractable. Abbring and Van den Berg (2007) provide a theoretical justification for using this distribution. This model is estimated using the Stata program pgmhaz8 (Jenkins 2004a and 2004b).

[24] Including time-varying UI variables is standard within the unemployment hazard models literature (see for instance, Meyer, 1989; Narendranathan, and Stewart, 1993 Bover, Arellano and Bentolila, 2002; Lalive and Zweimuller, 2004; Card, Chetty and Weber 2007b). The standard job-search theory provides a framework to understand the proper modeling of benefits. The duration of UI and UA benefits varies according to the individual's past labor-market history. An unemployed individual who is optimizing his or her expected returns to search would be changing his or her behavior over the duration of the unemployment spell as the time of benefit exhaustion approaches. It is, therefore, important to allow for time dependence in the exit probabilities.

We specify the duration dependence of the hazard, $h_0(j)$, as a piecewise constant function of elapsed duration as shown in equation (3) below.

$$h_{i0}(j)=\left(\sum_{l=1}^{15}\left(\lambda_l I_l\left(4l \le j_i < 4(l+1)\right)\right)+\lambda_{16} I_{16}\left(j_i > 64\right)\right) \tag{3}$$

where I_l is an indicator function equal to 1 if j is in the interval I_l, and where $I_{1,\ldots}$ I_{16} is a partition of the range of duration in the data. Hence, the hazard rate shifts at four-week intervals. Because we observe individuals only up until March 31, 2014, we censor the spells at 64 weeks.[25]

To estimate the discrete-time duration model, we construct a panel dataset such that the spell length of any given individual determines a vector of binary responses (Allison, 1982; Jenkins 1995). Let y_i be a binary indicator variable denoting weekly transitions to potential destination states upon exit, that is, y_i=1 if individual i transits to employment and zero otherwise.

4.2 Theoretical Predictions

The standard results from job search models predict that a decrease in the RR will increase the worker's job search intensity, thereby decreasing the average duration of unemployment (Mortensen 1977, and Mortensen 1986). We follow Lalive *et al.* (2006) and assume that an unemployed worker is entitled to unemployment benefits for a fixed duration, and thereafter, he or she is entitled to unemployment assistance, which is lower than his or her unemployment benefits and of infinite duration. Lalive *et al.* (2006) show that such a model, in which a worker balances the marginal costs and benefits of job

[25] Artificially censoring all unemployment spell is standard in this literature to guarantee that the pre-reform data has the same observation period as the post-reform data.

search, predicts that a decrease in the RR will increase the worker's job search intensity *from the beginning* of the unemployment spell as it raises the costs of being unemployed.[26] This effect occurs independently of whether the drop in the RR takes place at the start of the unemployment spell or afterwards because the reform decreases the net present-value of the unemployment spell. Hence, the reduction in job-finding rates should be largest at the beginning of the unemployment spell (regardless of whether the drop in RR occurs at the beginning of the spell or later on) because at that point the change in the value of the remaining future benefits is the highest. This is what we call *the anticipatory effect* in which treated workers will exit unemployment faster, even before the drop in the RR will take place, because their reservation wage decreases (or search intensity increases) from the start of the spell of unemployment.

Because the Spanish reform decreased the RR only after 180 days of UI receipt, we can test whether the reform triggered a strong behavioral response early in the unemployment spell. To do so, we estimate an extended version of the model presented above and add the following term to the previous expression (3):

$$h_{i0,DiD}(j)=\left[\sum_{k=1}^{3}\left(\delta_k I_k\left(12k \le j_i < 12(k+1)\right)\right)+\delta_4 I_4\left(j_i > 64\right)\right]*\left[\alpha_{1k}D_i^T+\alpha_{2k}D_i^{post}+\alpha_{3k}D_i^{post}*D_i^T\right] \tag{4}$$

Equation (4) allows the pattern of duration dependence to change with the reform between 12-week intervals.[27] In this setting, the treatment effect is identified by the set of α_{3k}

[26] As explained by the authors: "*The value of unemployment is determined by the level of the unemployment benefits, the search costs, the situation in the labor market (i.e., the way search intensity translates into job offers), the expected gain from accepting a job, and the risk of not finding a job before unemployment benefits expire.*"

[27] Sample size limitations prevent us from analyzing the effect in 4-week intervals, hence we pool them together to 12-week intervals.

parameters, which are allowed to change between 12-week intervals. Hence, evidence of economically significant effects of the reform prior to the drop in the RR rate would provide strong evidence supportive of non-employed workers anticipating the UI benefit changes. The set of α_{1k} parameters captures ex-ante differences between treated and comparison groups, and the set of α_{2k} parameters captures differences between workers who became unemployed before the reform, and workers who became unemployed after the reform, unrelated to the change in financial incentives.[28]

5. The Data and Descriptive Statistics

The 2013 Continuous Sample of Working Histories (CSWH)

We use the 2012 and 2013 waves of the *Continuous Sample of Working Histories* (hereafter CSWH). This is a 4% non-stratified random sample of the population registered with the Social Security Administration in 2012 or 2013. It includes both wage and salary workers and recipients of Social Security benefits, namely, unemployment benefits, disability, survivor pension, and maternity leave. The CSWH contains workers' full employment histories from the moment they entered the labor market up until March 31, 2014. In addition to age, gender, nationality, state of residence (*Comunidad Autónoma*), education, and presence of children in the household, the CSWH provides detailed information about a 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, and public- versus

[28] Given that we truncate our sample at 64 weeks and our sample includes workers with 104 weeks of entitlement, we are unable to study properly the exhaustion effects, namely, the well-documented spike in job finding probability at the time benefits run out—see Rebollo-Sanz (2012) for a thorough study of the exhaustion effects of unemployment benefits in Spain.

private-sector jobs.[29] We calculate workers' previous work experience as the number of months worked since an employee's first job, and tenure as the number of months a worker has stayed with the same employer. The CSWH also informs us on the reason for the end of the employment spell (resigned versus layoff), and whether an individual receives unemployment benefits and the type (UI versus UA). We compute the duration of each non-employment episode by measuring the time between the end date of a 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.[30] Most importantly, the CSWH allows us to observe individuals after exhaustion of UI benefits, which allows us to study how the job-finding rate and other post-non-employment characteristics evolve after the exhaustion of UI benefits. This post-displacement information is not available in UI claims data. Moreover, the unemployment period is *not* truncated at the date benefits expire, nor at the date a worker finds a new job, as in UI claims data.

We restrict our sample to all 20- to 50-year old wage and salary full-time workers who became unemployed between January 1, 2012 and December 31, 2012. As the reform included other policy changes that affected part-time workers and workers older than 52 years, we excluded from our sample part-time workers and workers 50 years old and older.[31] In addition, we drop individuals who are typically recalled to their prior firm

[29] Earnings are deflated using the Spanish CPI (year 2012).

[30] 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.

[31] Although self-employed workers are also in the CSWH, we exclude them from the analysis as they are not eligible to receive UI benefits. In addition, we restrict the analysis to workers displaced from full-time jobs 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).

(which represents about 15% of the final sample) to exclude temporary layoffs who may not be searching for a job. To ensure that all individuals in our sample are entitled to at least four months of UI benefits, we further restrict our sample to those who have worked for at least 12 months within the last 72-month period. Individuals in the treatment group have worked for a period of at least 24 months within the last 6 years, and their pre-displacement wages ranged between €820 and €1,800 for those without children (or €1,100 and €2,100 for those with children).[32] This implies that, after 180 days of UI entitlement, their RR dropped by 10 percentage points (from 60% to 50%) if they were displaced after July 14, 2012. Individuals in the comparison group have pre-displacement wages within the same range, but have worked for a period of 12 to 24 months within the last 6 years. As their UI entitlement is less than 180 days, they were not affected by the reform.

Our sample has 5,978 non-employment spells in the treatment group, of which 55% ended in a new job during the first 64 weeks of non-employment, and the rest were censored. Of these 5,978 non-employment spells, 3,289 belonged to workers who entered non-employment *before* the reform. Among these, 52.7% found a new job within 64 weeks of losing their job. In contrast, 57.9% of workers who entered non-employment *after* the reform found a new job within 64 weeks of losing their job.

For workers in the comparison group, we observe 1,815 non-employment spells, of which more than 70.9% ended in a new job during the first 64 weeks of non-employment, and the rest were censored. Of the 1,815 non-employment spells, 958 belonged to workers who entered non-employment *before* July 15 2012. Among these,

[32] The mean and median monthly income in Spain in 2012 was €1,893 and €1,587, respectively (Encuesta Estructural Salarial 2012).

about 71.8% found a new job within 64 weeks of losing their job. Among workers who entered non-employment *after* July 14, 70.0% found a new job within 64 weeks of losing their job. This -1.8 percentage-point difference contrasts with the +5.2 percentage-point difference found among workers in the treatment group. Hence, the raw data suggests that the reform increased the share of workers who found jobs by approximately 7 percentage points.

Descriptive Statistics

Panel A in Table 2 presents socio-demographic and pre-displacement job characteristics of UI recipients in the treatment and comparison groups before and after the reform. Two thirds of our sample are women, and about two fifths have a university degree. Regarding pre-displacement job characteristics, close to 60% worked in low-skilled jobs, about 5% worked in high-skilled jobs, and, on average, they earned between €1,400 and €1,500 euros per month. Table 2 also shows several differences between those affected by the reform and those who are not. In particular, individuals affected by the reform are older, more likely to have a family and be natives, have 8.8% higher pre-displacement monthly wages, and are more (less) likely to have been displaced from a permanent contract or a construction (trade services) job than those not affected by the reform (shown in columns 1 to 3). Columns 4 to 6 show that most of these differences existed before the reform, and thus, are "washed out" by our identification strategy, as shown in column 7. The only differences across time that remain are a higher likelihood of losing a private-sector job and a lower likelihood of losing a low-skilled job prior to the reform (although the latter difference is only statistically significant at the 10% level). Subgroup analysis at the end of the paper explores whether results hold across these different groups of displaced workers.

Since the main criterion for eligibility is the length of previous work history, it is not surprising that individuals affected by the reform have 58 more weeks of potential UI benefits entitlement, and 3.75 and 5 more years of pre-displacement experience and tenure, respectively. Again, these differences are washed out by the DiD strategy.

Panel B in Table 2 reports average non-employment spell duration in the first 64 weeks of non-employment by treatment status and time of the displacement. Since we deal with non-employment duration data censored in the first 64 weeks, the average non-employment duration is computed as $\overline{J_u} = \min(J_u, 64)$. Before the reform, the average non-employment duration is 10 weeks longer for individuals in the treatment group than those in the comparison group. The average non-employment duration is 34 weeks for individuals in the comparison group and 44 weeks for individuals in the treatment group. After the reform, the average non-employment duration decreases by 4 weeks for treated individuals and is unaffected for individuals in the comparison group, suggesting that the reform decreased the average non-employment duration by 4 weeks. This difference is statistically significant at the 1 percent level.

Panel A in Figure 3 displays job-finding hazard rates along the spell of non-employment by treatment status and whether the spell began before or after July 15, 2012. Each point is a four-week average. The top graph shows the pre- and post-reform hazard rates for treated workers, and the bottom one shows the pre- and post-reform hazard rates for comparison-group workers. The red vertical line indicates 180 days of UI receipt.

The leading role of tourism and construction sectors in the Spanish economy generates a highly seasonal employment pattern in which jobs are easier to find during the spring and summer months. The bottom graph of Panel A confirms that this is the case, as comparison-group workers displaced during the first half of 2012 find jobs sooner than similar workers displaced during the second half of the year (that is, after the 15th of July). In contrast, the higher job-finding hazard rate for those displaced *before* July 15th is *not always* observed among treated-group workers in 2012 (shown in the top graph of Panel A). Indeed, the job-finding hazard rate is slightly higher during weeks 3 to 8 for those displaced after the reform (relative to those displaced before the reform), suggesting that the reform may have increased the job-finding hazard rates of the treated. Panel B in Figure 3 displays the difference-in-difference in the job-finding hazard rates between treated- and comparison-group workers in 2012, and reveals that there is indeed a positive effect during the first 26 weeks of non-employment. As these are the raw data, in the next section we proceed with the regression analysis.

6. Results

6.1. Average Effect of the Reform on the Job Finding Rate

Table 3 displays the policy effect, α_3, estimated using equations (2) and (3). α_3 captures the effect of the reduction in the RR on the job-finding probability for UI recipients with at least 180 days of UI entitlement relative to their counterparts with less than 180 days of entitlement, net of any changes observed between these two groups before July 15, 2012. Each column presents a different specification. Column 1 presents a hazard model with the post-July 14th dummy, the more than 180 days of entitlement dummy, the interaction of these two dummies, and the 4-week dummies. It shows that reducing the RR by 10 percentage points increases the job-finding rate by 25% for treated workers relative to those in the comparison group. This effect is statistically significant at the 1% level. Column 2 adds a set of state and monthly dummies, and quarterly GDP growth to control for seasonal, regional, and macroeconomic effects. The effect of the reform is now 23% and statistically significant at the 1% level. Adding workers' socio-demographic characteristics slightly raises the reform estimate to 24% (shown in column 3). Interestingly, the effect of the reform becomes stronger (37%) when we move to the specification in column 4, which controls for individual's UI benefit receipt, such as the potential length of UI entitlement at unemployment entry, and two time-varying dummy variables indicating whether the individual is receiving UI or UA. This suggests that not accounting for UI benefit receipt under-estimates the effect of the reform.[33] Column 5 displays our preferred specification, which controls for workers' pre-displacement job characteristics including industry, occupation and type of contract. We find that reducing the RR by 10 percentage points increases the job-finding rate by 41% within the first 64 weeks of non-employment (see the complete list of coefficients in our preferred specification in Appendix Table A.1). The fact that our results are stronger once we add pre-displacement job controls suggests that those most affected by the reform are individuals with better job market opportunities, and hence are most likely to change their behavior pattern.

Column 6 presents estimates from a model that allows the duration dependence term to be different for the treated versus the comparison groups. To do so, we use the following baseline hazard instead of the one in equation (3):

[33] Time-varying unemployment benefits are a key element in many theoretical and empirical models to explain the behavior of unemployed workers along the unemployment spell. The timing of exit from unemployment, unemployed workers' reservation wages and accepted wages are closely related to the design of the UI system (Lalive, Van Ours and Zweimuller, 2006; Van Ours and Vodopivec, 2006; Lalive, 2008; and Akin and Platt, 2012). Note that the estimates for these covariates (shown in Appendix table A.1) are statistically significant and in accordance with the empirical and theoretical UI literature. More specifically, they show that higher UI benefits have a strong negative effect on the probability of leaving unemployment and that this negative effect increases with the length of the entitlement (Meyer, 1989; Tatsiramos, 2009; Rebollo-Sanz, 2012; Caliendo *et al.*, 2013; Tatsiramos and Van Ours, 2014; and Caliendo *et al.*, 2016).

$$h_{i0}(j)=\left(\sum_{l=1}^{15}\left(\lambda_l I_l\left(4l \le j_i < 4(l+1)\right)\right)+\lambda_{16} I_{16}\left(j_i > 64\right)\right)*\left(\alpha_1 D_i^T\right) \qquad (5)$$

Allowing for differential duration dependence between treatment and comparison groups has little effect on the estimated effect of the reform.

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, the arrival rate of job offers, or the wage offer distribution. To assess the existence of differential trends, we take several approaches. First, column 7 in Table 3 adds to our preferred model (shown in column 5) the interaction between state-specific linear trends and the D^T dummy to allow for a differential trend between those in the treatment and comparison groups (as suggested by Meyer, 1995). This specification controls for systematic differences in the behavior between the two groups over time that are unrelated to the change in the RR. As the effect of the reform only decreases by 1 percentage point from 41% (column 5) to 40% (column 7) and remains statistically significant at the 1% level, conditional on the observed heterogeneity considered in the model, differential trends do not seem to be affecting our results. Second, we allow for arbitrarily differential trends by having a third differencing group, in this case workers who became displaced during the year 2011 (shown in column 8). Again, results remain robust to our main estimate: according to the DiDiD estimates, the reform increased the job-finding rate by 36% within the first 64 weeks of non-employment.

The next three columns test the sensitivity of our results to sample criteria. Column 9 in Table 3 re-estimates our preferred specification using *only* those who lost their job within 3 months (instead of 6 months) of July 15, 2012. Even though this reduces the sample size by half, the policy estimate is 40% and remains statistically significant at the 1% level. Column 10 includes the recalls in the sample estimation. In this case, the job-finding rate drops slightly to 27% but remains statistically significant at 1% level. As previous empirical literature has highlighted, this suggests that temporary laid-off workers behave differently than permanent laid-off workers (Feldstein, 1978; Fallick and Ryu, 2007; Rebollo-Sanz, 2012). Concerns that our comparison group may have lower average work experience compared to our treatment group led us to conduct the following robustness check. We re-estimate our preferred specification using an alternative treatment group, namely individuals whose UI entitlement is not longer than 12 months. Column 11 shows that, using this more narrowly defined treatment group, reduces the effect of the reform to a 26% increase in the job-finding rate within the first 64 weeks of non-employment (this effect is statistically significant at the 10% level, as the sample size is now smaller). Note that the smaller impact is consistent with smaller potential losses from the policy change for this treatment group than for the one used in our preferred specification given their shorter entitlements as explained by Lalive *et al.* (2006).

Finally, we estimate two additional DiD models using as comparison groups workers whose UI benefits are either at the min or max. For those with pre-displacement wages below the median of €1,459 euros, we find that reducing the RR by 10 percentage points increases the job-finding rate by 28% within the first 64 weeks of non-employment (shown in column 12). This effect is statistically significant at the 5 percent level. In contrast, we find no effect of the reform for those with pre-displacement wages above the median of 1,459 € (shown in column 13). This finding is consistent with that of Lalive *et al.* (2006), whose reform *only* affected low-income workers.

Parametric Assumptions for the Unobserved Heterogeneity Term. Previous papers have noted the sensitivity of results to different parametric assumptions for the unobserved heterogeneity term (Baker and Melino, 2000; and Abbring and Van den Berg, 2007). To test the robustness of our results to the parametric assumptions, we re-estimate the model using a non-parametric approach, characterizing the frailty distribution with two mass points (as proposed by Heckman and Singer, 1984). The main results hold (shown in Appendix Table A.2).

Placebo Tests. Methodologically, we have relied on the assumption that, in the absence of the reform, the differences in the job-finding rate between the treated and comparison groups would have remained constant. As this assumption is not testable, we carry out three placebo tests, resulting in estimates shown in Table 4. Column 1 in Table 4 presents our preferred specification (also shown in column 5 in Table 3). Column 2 estimates the same DiD model as in our preferred specification but with workers displaced in 2011, hence, one year before the reform took place. In this case, the estimate is close to zero and not statistically significant. This (lack of) effect is robust to using alternative specifications of the placebo tests as those used in columns 1 to 4 in Table 3.[34] Alternatively, column 3 presents a second placebo test to address concerns of differential time/seasonality trends for the treatment and comparison groups. In this case, a different fictitious policy date (April 1 2012) is adopted and only workers displaced between January and June 2012 are used for the analysis. Doing so delivers an estimate that is two-fifths the size of our preferred specification and not statistically significant.[35]

[34] Results available from authors upon request.
[35] Because there was another reform in February 2012 as explained in the Institutional Section, we declined the option of using the fictitious policy-change date of January 2012 and workers who became unemployed between July 2011 and June 2012.

Column 4 presents the third placebo test. In this case, treated workers are those entitled to more than 180 days of UI but who are *not* affected by the reform because they reached either the floor of UI benefits (low-wage workers) or the ceiling (high-wage workers); and the comparison-group workers are those whose benefits also reached the floor or the ceiling *and* have UI entitlements shorter than 6 months. In this case, all workers were displaced in 2012. The placebo estimate is less than one third of our main result and is not statistically significant. It is important to highlight that differences in the length of previous work history between our treatment and comparison groups, and hence, potential UI benefit entitlement, pre-displacement tenure or experience are *not* behind our main results in Table 3. If they were, we would find similar effects of the reform when using as treatment individuals those with entitlements greater than 180 days but *not* affected by the reform because they hit the floor of UI benefit level (low-wage workers) or because they reach the ceiling of the UI benefit level (high-wage workers).

6.2. Regression Discontinuity Approach

In this subsection, we use an alternative identification strategy to estimate the effect of the reform, namely, a regression discontinuity design (RD hereafter) in which the treatment status (being affected by the reform) is a deterministic and discontinuous function of time. The reform created a sharp discontinuity in the date of entry into unemployment with July 15, 2012 being the dividing line. Figure 4 displays the average probability of finding a job during the first 64 weeks of unemployment for different cohorts "c" defined by the time of entrance into unemployment. The horizontal axis shows the running variable (time) with the vertical line describing the date of the implementation of the reform. After this date, unemployment entrants suffer a larger drop in their UI benefits six months after UI receipt than they would have had, had they entered unemployment before that date. The dots in Figure 4 represent $1 - S(T, c)$ where $S(., c)$ denotes the survival function for cohort c that started unemployment during a particular fortnight. Figure 4 reveals a sharp upturn in the job-finding probability following the implementation of the reform. More specifically, it suggests that the reform increased the job finding probability by 15.3% ($15.3\% = \frac{logS(T,1)}{\log S(T,0)} - 1, with\ logS(T, 0) = 0.51\ and\ logS(T, 1) = 0.46$).

We estimate the following RD model using the hazard model specified in equation (1) but replacing equation (2) with equation (6) below:

$$y_i(j) = \left(h_{i0}(j) + \alpha * 1(t_i \geq 0) + \sum_{l=1}^{L} \gamma_{Cl} g(t_i^l) * 1(t_i < 0) + \sum_{l=1}^{L} \gamma_{Tl} g(t_i^l) * 1(t_i \geq 0) + \beta X_i(j) + \theta_{iu} \right) \tag{6}$$

where t_i is the unemployment-entry date of individual i normalized so that $t=0$ at the cut-off date of July 15, 2012. t_i^l is the distance of the individual's date of entry into unemployment from the cut-off date. The relation between t_i^l and the outcome variable y_i is described by the polynomial function $g(.)$. X_i is the vector of observed covariates and θ_u is the unobserved heterogeneity term.[36] The parameters γ_{cl} and γ_{Tl} capture the effects of the assignment variable "date of entry" below and above the threshold on the probability of finding a job. This ensures that α does not capture a general date of entry effect but the causal impact of the discontinuity in the benefit generosity.

[36] The covariates in X_i and the distributional assumptions for the unobserved heterogeneity term (θ_u) in the RD exercise are the same as those in the DiD analysis.

The RD estimation sample includes workers with entitlements longer than 6 months and whose UI benefits levels are within the lower and upper ceilings, leaving us with 3,289 workers who became displaced *before* the reform and 2,689 workers who became unemployed after the reform. In order to keep as close as possible with our DiD exercise, the bandwidth used for the RD is 6 months.

In this context, the key identifying assumption is that *g(.)* is continuous through the cut-off date of July 15, 2012 (that is, observed and unobserved factors are smooth around the cut-off). To put it differently, our main identification assumption is that the assignment into treatment (being entitled to a lower RR after 180 days of UI) is only determined by the date of entry into unemployment and is orthogonal to the remaining observed and unobserved heterogeneity. Assuming this holds for workers displaced in the vicinity of the cut-off date, the coefficient of interest, α, identifies a local treatment effect of a 10 percentage points drop in the RR after 180 days of UI receipt that can only be extended to the population effects with additional assumptions.

Columns 1 to 5 in Table 5 present RD estimates of the reform using equations (1) and (6) above for different specifications. The specification in column 1, which does *not* control for unobserved heterogeneity and *only* controls for duration dependence, indicates that the reform increased the job finding probability by 17%, not far from the 15.3% displayed in Figure 4.

Specification in column 3 adds the full set of covariates used in our preferred DiD model to the specification in column 1. Doing so has little effect on the impact of the reform, which is now estimated to be an 18% increase in the job finding rate. However, as Lancaster (1990), Van Den Berg (1990), Devine and Kiefer (1991), and Jenkins (1995) explain, it is important to control for unobserved heterogeneity in hazard models, especially when unobserved heterogeneity is likely to be correlated with the effects of the reform (in our case, the duration of the unemployment spell). Tatsiramos (2009) shows that this is relevant for many European countries and Bover *et al.* (2002), Rebollo-Sanz (2012), and Garcia-Perez and Rebollo-Sanz (2015) show that it is also relevant for Spain. Hence, our preferred specification is that of column 4, which controls for both observed and unobserved heterogeneity. It suggests that a 10 percentage point decrease in the RR 6 months after UI benefits receipt increases the job-finding rate by 26% within the first 64 weeks of non-employment. This effect increases to 28% if the cohorts of the RD model are defined in terms of weeks instead of fortnights.[37]

One important RD identification assumption is that the assignment to treatment around the threshold is random and that the density of the running variable does not jump around the cutoff. To explore this selectivity issue, Appendix Figure A.1 applies the density test suggested by McCrary (2008). The estimated curve provides little indication of a strong discontinuity near zero. Indeed, the density appears generally quite smooth around the threshold, suggesting that individuals did not manipulate their date of entry into unemployment. It is therefore safe to assume that assignment to treatment near the threshold is randomized.

As the validity of the RD depends on the non-existence of any endogenous sorting, we further test the validity of this assumption by examining whether workers' pre-determined characteristics are smooth around the cut-off date. Intuitively, if the RD is

[37] Because the running variable is discrete, we also checked the robustness of our results to clustering the standard errors on the distinct values of the running variable with a hazard model without controlling for unobserved heterogeneity as proposed by Lee and Card (2008)—results available from authors upon request. Results remain statistically significant at the 1 percent level when we allow for clustering of the regression errors at the fortnight cell level. The precision of the estimates drops to the 10 percent level when we allow for clustering at the month level. Unfortunately, we were unable to cluster standard errors on the distinct values of the running variable as proposed by Lee and Card (2008) when also controlling for unobserved heterogeneity.

valid, the treatment variable cannot influence variables determined *prior* to the realization of the assignment variable. If they do, then the identification assumption does not hold (Lee and Lemieux, 2010). Appendix Table A.3 shows estimates of the RD using pre-displacement characteristics as the outcome variable. All but three coefficients reported in Appendix Table A.3 are small in magnitude and lack statistical significance. The three coefficients that are statistically significant are pre-displacement industry and construction sectors, and pre-displacement tenure. The lack of smoothness around the threshold for the two sector variables indicates a larger amount of unemployed workers from the construction sector and smaller amount from the industry sector during the second half of the year, most likely a reflection of the seasonality of the Spanish labor market.

As selection of unobservables around the discontinuity cannot be tested, we proceed to estimate three different placebo tests, shown in Appendix Table A.4. Column 1 displays RD estimates using workers displaced in 2011. Column 2 displays RD estimates using the fictitious cut-off date of April 1 2012 and individuals displaced between January and June 2012. Column 3 displays RD estimates using workers displaced in 2012 with entitlements shorter than six months and hence not eligible. Neither of the three RD placebo estimates are statistically significantly different from zero. Two of them are relatively small in magnitude and the other has the opposite sign.

It is important to recall that the RD and DiD identification strategies rely on two different control groups, and rest on distinct identification assumptions (Lee and Lemieux, 2010). The DiD approach estimates the average treatment effect on the treated, and uses workers with UI entitlements shorter than 6 months as the counterfactual. In contrast, the RD approach identifies the local average treatment effect, and uses workers with the same length of UI entitlements but who became unemployed during the first half of 2012 as the counterfactual. Hence, it is not surprising that our RD and DiD approaches lead to different estimates of the effects of the reform. Depending on the specification, the DiD estimates range between 23% and 41%, while RD estimates lie between 26% and 32%. However, and most importantly, both identification strategies indicate that the reform increased the job-finding rate.

The intuition for the difference between the DiD and the RD follows. While the DiD estimator nets out the difference in outcomes for the treated group (workers with entitlements lengthier than 6 months) before/after the reform with that of the comparison group (workers with entitlements shorter than six months), the RD estimator evaluates a discontinuity using only the treated individuals (that is, the first difference). Both estimates converge when the difference in outcomes for the comparison group (workers with entitlements shorter than 6 months) before/after the reform approaches zero. Column 3 in Appendix Table A.4 reveals that, with the reform, the probability of finding a job for the control group drops a non-statistically significant 0.195 percentage points, which netted out from our RD estimator, would deliver an estimate of 46%, not far from our DiD estimate.

While the RD approach is generally regarded as having the greatest internal validity of all quasi-experimental methods, it is considered to have less external validity since the estimated treatment effect is local to the discontinuity. As we have discussed, in this case, seasonality patterns in the probability of exiting unemployment may matter, casting greater doubt on the external validity of the RD approach (Percoco, 2014). In what follows, we use the DiD approach.

6.3. Anticipation Effect and Factual Hazard and Survival Functions

To explore whether the reform had a differential effect across time, Table 6 presents heterogeneous effects of the reform along the non-employment spell. Column 1 presents results controlling for unobserved heterogeneity. It shows that the reform increased the probability of finding a new job by 43% during the first 12 weeks of the non-employment spell for treated workers relative to those in the comparison group. This estimate is statistically significant at the 1% level. During weeks 13 to 26, as the drop in the RR approaches, the effect of the reform becomes stronger (with an increase in the job finding rate of 51%). It is also interesting to note that the effect of the reform *after* the actual drop in the RR is no longer statistically significant (despite being positive). Hence, we cannot reject the null hypothesis of no effect of the reform after 180 days of non-employment, suggesting that most of the effect of the reform takes place *prior* to the actual drop in the RR. This is consistent with forward-looking displaced workers as they increase job search activity from the beginning of the non-employment spell.[38] Appendix Tables A.5 and A.6 show parameter estimates of the main model and sensitivity of the results to different parametric assumptions for the unobserved heterogeneity term, respectively.[39] Column 2 in Table 6 shows placebo estimates using only data from 2011. The coefficients are smaller, not statistically significant, and sometimes have opposite signs.

To better illustrate the results, the top panel of Figure 6 displays the factual hazard rate with and without treatment using parameters estimates. To obtain the factual hazard

[38] The higher hazard rate as the RR approaches is also consistent with the predictions of a reference dependence model in which the hazard rate increases as the drop in the RR approaches in anticipation of future loss aversion (Dellavigna et al. 2015).

[39] Results are also robust to alternative specifications, such as DiDiD approach using workers who became displaced during the year 2011 as the third difference.

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 the treatment effect to be 0 and then, again, average across treated individuals. The bottom panel of Figure 4 shows the difference between the two hazard rates, namely, the "average treatment effect on the treated" (ATET).

The left panel of Figure 5 shows that decreasing the RR after 180 days of unemployment increases the non-employment exit rate of treated individuals from the beginning of the non-employment spell as predicted by the job search model. The right panel of Figure 5 shows that the difference in hazard rates peaks at 0.5 percentage points around week 9 of non-employment, and then slowly converges towards 0.2 percentage points thereafter. Note that the ATET is statistically significantly different from zero at the 5% level between week 1 and week 25 of non-employment. During these first 180 days of non-employment, the RR are identical with and without the reform. By 180 days, the hazards of the treatment and comparison groups are still different but this difference is no longer statistically significant. Consequently, from week 26 onward, we cannot reject zero effect of decreasing the RR despite the fact that it is at that point when the actual decrease takes place. These results provide strong evidence of forward-looking displaced workers consistent with the behavioral response to extending the UI duration (as opposed to increasing UI levels) found by Card, Chetty, and Weber (2007b) and by Nekoei and Weber (2015) in Austria.

To analyze the consequences of this reform on the non-employment duration, Figure 6 reports the factual survivor function with and without treatment—shown in the LHS. These survivor functions are calculated in a similar manner as the factual hazard rates: the function is estimated with treatment (or imposing all treatment to be zero if without treatment) for each individual, and then, in a second step, they are averaged with respect to the distribution of individual characteristics in the population receiving treatment in each case. The ATET is reported on the RHS of Figure 6.

Interestingly, the survival functions diverge from the first month of non-employment and this difference persists along the whole non-employment spell. The threat of suffering a drop in the RR by 10 percentage points after 180 days of unemployment spell entails a negative contribution to the change in expected non-employment duration right from the beginning of the non-employment spell. The maximum subtraction arises around week 40, when it reaches an inflection point, and subsequently the subtraction begins to contract.

In order to see how the reform affected the total amount of time spent in non-employment, we estimated the effects of the reforms in terms of non-employment duration.[40] The factual expected non-employment duration with and without treatment for the sample of treated workers is 39.5 and 45.2 weeks, respectively.[41, 42] Hence,

[40] Expected unemployment duration is obtained by integrating the population survivor function with respect to time up to 64 weeks. The expected duration is given by

$$ED = \int \sum_{j=1}^{\infty} j f(j/\theta) dG(\theta)$$

where dG(θ) is the distribution function for the unobserved heterogeneity term (see Eberwein *et al.*, 2002). We compute expected unemployment duration in the first 64 weeks of unemployment because to estimate total expected duration we need to know the survival function until infinity. However, our sample extends only to the first 64 weeks of unemployment. Hence, we calculate a truncated mean $ED = \int \left(\left(\sum_{j=1}^{J} j f(j/\theta) \right) + S(J/\theta) \right) dG(\theta)$.

[41] Non-employment duration for the treated group is computed using the sample characteristics of the treated sample and model parameters estimates. Non-employment duration for the counterfactual is computed using the sample characteristics of the treated sample and model parameters estimates but the policy coefficient is imposed to be 0. A similar approach is used in Lalive *et al.* 2004 and Eberwein *et al.* 2002.

[42] It is important to highlight that the average non-employment duration in the treated group is 40 weeks in the period after July 14 2012 (Table 2, Panel B). The corresponding number implied by the econometric model is 39 weeks, providing solid evidence that our econometric model fits the data relatively well. The results for the comparison group diverge from the ones mentioned above because in that case they were

reducing the RR by 10 percentage points (from 60% to 50%, representing a 16.6% drop in the RR) shortens the non-employment spell by about 5.7 weeks (around 14%). This implies an elasticity of non-employment duration with respect to the RR of around 0.86. This elasticity is higher than the one found by Lalive *et al.* (2006) in Austria in the 1980s (0.33), nearer to the one found by Uusitalo and Verbo (2010) in Finland in 2003 (0.75), and smaller than that found by Carling *et al.* (2001) in Sweden in the 1990s (1.6). Interestingly, it lies within the elasticity found during the Great Recession and its aftermath in Missouri (0.65-0.9) by Card *et al.* (2015).

6.4. Impact on Post-Non-employment Job Characteristics

One concern is that this reform may have lowered workers' reservation wage, making them accept "worse" job offers. For instance, Chetty (2004) interprets the effects of changes in the generosity of benefits in terms of differences in liquidity constraints. He argues that most agents enter unemployment with very low assets and, hence, are highly credit constrained. Such credit constraints make it plausible that income effects play a large role in determining non-employment durations. If this is the case in Spain, after the reform, workers will lower their reservation wages from the onset of the unemployment spell and accept inferior jobs offers. Alternatively, if moral hazard is a concern, this reform may modify individuals' incentives, and reduce moral hazard problems by increasing workers' search intensity. In this case, we would not find evidence of workers accepting lower quality jobs. Ultimately, it is an empirical question.

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computed at sample means for the treated workers. When we estimate the average non-employment duration for the comparison group using the model, we obtain 32 weeks, not far from the 34 weeks average in Panel B of Table 2.

To estimate the effect of the reform on post-non-employment wages is complicated by the fact that we have many right-censored observations for which we do not observe the end of the unemployment spell and the subsequent employment spell. While the reform exogenously assigns some individuals into the treatment and others into the comparison group, there might be dynamic selection among those who become employed based on both observed and unobserved characteristics as explained by Ham and LaLonde (1996). We address the dynamic selection by estimating the discrete-time hazard rate model for the transition from non-employment to employment *jointly* with wages and allowing for potentially correlated unobserved heterogeneity using maximum likelihood estimation methods. The specification of the non-employment hazard rate model is the same as the one used earlier in the paper. The post-non-employment wage equation is specified as a standard log linear DiD model, shown in equation (7) below:

$$\log(w_i) = \alpha_{w0} + \alpha_{w1} D_i^T + \alpha_{w2} D_i^{post} + \alpha_{w3} D_i^{post} * D_i^T + \beta X_i + \theta_{iw} + \varepsilon_i \qquad (7)$$

Overall, the set of covariates in X_i resembles those included in the hazard rate model.[43] The major difference is that we now include the duration of the unemployment spell in the spirit of Caliendo *et al.* (2013), and the length of benefits the worker is entitled to when he or she becomes unemployed. Note that the latter is a variable determined by workers' pre-displacement characteristics.[44] We assume that the error term ε_{it} follows a normal distribution with $\varepsilon_{it} \sim N(0,\sigma^2)$. We compute robust standard errors clustered at the individual level.

[43] The hazard rate model specification correspond to the one used in our preferred model in column 5, Table 3.

[44] In addition, we no longer include the two time-varying dummy variables indicating whether the individual receives UI or UA.

α_{w3} estimates the causal effect of the drop in the RR on post-non-employment wages conditional on time spent non-employed. The specification of post-non-employment wages also includes controls for individual characteristics, pre-displacement job characteristics (including wages), and macroeconomic controls. The post-non-employment wage equation is estimated jointly with the non-employment hazard rate displayed in equations (1) and (2) by maximum likelihood.[45] We follow Heckman and Singer (1984) to model the unobserved heterogeneity distribution. By estimating both equations jointly, we allow the unobservables of the non-employment hazard equation to be correlated with the realized wages.

Column 2 in Table 7 shows the effects of the reform on post-non-employment wages using a DiD specification. Column 4 displays the effect of the reform on post-non-employment wages using a RD specification.[46] In either case, the effect is close to

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[45] The likelihood contribution of an individual *i* with an unemployment spell of j_u intervals, and a subsequent employment spell with wage w_i for given unobserved characteristics θ_{iw}, θ_{iu} for the basic specification is given by:

$$l_i(\theta_i)=\prod_{j_u=1}^{J_u}(1-h(j_u))^{(1-\tau_u)}\left(h(j_u)*\frac{1}{\sqrt{2\pi\sigma^2}}\left(-\frac{\log(w_i)-\log(\hat{w_i})}{2\sigma^2}\right)\right)^{(\tau_u)}$$

We define τ_u as a binary indicator variable denoting a transition to employment when τ_u=1 and zero otherwise. Following Heckman and Singer (1984), the unobserved heterogeneity distribution is defined as a discrete distribution with the support points denoted by ($\theta_i=\theta_{iw}$, θ_{iu}) and the corresponding probability mass term given by $P(\theta_{iw}=\theta_{pw}, \theta_{iu}=\theta_{up})=\pi_p$. Each unobserved factor is assumed to be time invariant and individual specific for the hazard rate and the wage equation. The unobserved component for the wage equation is modeled as follows $\theta_w=\theta_u*\rho$. This allows to define a correlation between the two terms of unobserved heterogeneity and the component ρ acts as a shifter to isolate the specific unobservable factors that affect to wage equation from the non-employment state.
The unobserved factors are assumed to be uncorrelated with observable characteristics X, and the treatment indicator. The sample likelihood is given by :

$$L=\prod_{i=1}^{N}\sum_{p=1}^{P}(\pi_p l_{ip})$$

where the individual likelihood contribution given unobserved characteristics defined in θ is denoted by l_{ip}.
[46] For the RD, the estimated wage equation is:

$$\log(w_i)=\alpha_{w0}+\alpha_{w1}*1(t_i\geq 0)+\sum_{l=1}^{L}\gamma_{Cl}g(t_i^l)*1(t_i<0)+\sum_{l=1}^{L}\gamma_{Tl}g(t_i^l)*1(t_i\geq 0)+\beta X_i(j)+\theta_{iw}+\varepsilon_i$$

where the policy parameter is $\alpha_{w1.}$

zero and not statistically significantly different from zero. Columns 1 and 3 show the effects of the reform on post-non-employment wages using all individuals in our sample but without correcting for the right censoring.[47] In this case, we observe a positive effect of the reform, which is driven by the higher hazard into employment, as those who have not entered employment are assigned a wage of zero. Crucially, Table 7 shows that the reform had little effect on post-non-employment wages, and most importantly, it did not lower them, suggesting that workers are not accessing lower quality job matches.

Schmieder, von Wachter, and Bender (2014) highlight that in countries with wage rigidity due to collective bargaining agreements (such as in Germany or Spain), it may be more appropriate to use multiple post-non-employment job attributes, including—in our paper—, type of contract, job quality or full- versus part-time status, as opposed to only post-non-employment wages, to measure post-displacement job-quality match. Hence, we proceed to present estimates for different outcomes measuring job quality in different dimensions. Using specifications (1) to (4), we estimate the effects of the reform on the exit probability to a permanent contract versus a fixed-term one, the exit probability to a full-time job versus a part-time one, and the exit probability to a new job within the same (or better) occupation versus one that entails a lower occupation.[48] Estimates are shown in Appendix Table A.7 and the respective incidence functions for different outcomes

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[47] Specifications in columns 1 and 3 do not include the duration of the unemployment spell from the vector of covariates.

[48] Occupation downgrading is defined by comparing the skill level of the occupation held prior to the spell of unemployment with the skill level of the occupation observed after the unemployment spell. In our database, skills are ranked from 1 to 10, with 1 being engineer, judge, or doctor; and 10 being unskilled labor (such as administrative assistant). We classify a worker as improving occupations if he or she goes from one job to another one with higher occupation rank.

measuring job-match quality are displayed in Figure 7.[49] Panel A in Figure 7 shows that the reform increased the odds of exiting to non-employment into both a fixed-term and permanent contract, but the effect is slightly stronger for the latter (as shown in Appendix Table A.7), which are jobs in the primary segment of the labor market. We also find that the reform increased the odds of exiting non-employment into a full-time job (shown in Panel B of Figure 7). Fernandez-Kranz and Rodriguez-Planas (2011) show that, in Spain, part-time jobs tend to be "second-best" jobs, offering limited career advances and lower wage growth (for a given level of human capital). Finally, the reform increased the odds of exiting into an occupation as good as the pre-displacement one for those in the treated versus those in the comparison group (shown in Panel C of Figure 7). These findings are suggestive that the reform did not lower the post-non-employment job-match quality.

6.5. Subgroup Analysis

Table 8 presents hazard rate subgroup analysis. Columns 1 and 2 present estimates by gender, columns 3 and 4 by age, columns 5 and 6 by family composition, columns 7 and 8 by pre-displacement job skill level,[50] columns 9 and 10 by pre-displacement contract type, columns 11 and 12 by pre-displacement firm ownership (public versus private), and columns 13 and 14 by size of the pre-displacement firm.

Consistent with Roed and Zhang (2003 and 2005), we find that the effect of the reform is more important for men than women. From the perspective of a job search model, this result informs us that search efforts or reservation wages are more sensitive to the RR for males than females in Spain. This finding is consistent with evidence

[49] In our analysis, the unemployed is subject to competing risks when exiting from unemployment (for instance, temporary versus permanent contract, or full- versus part-time job). The cumulative incidence curve is a proper summary curve showing the cumulative failure rates over time due to a particular cause.
[50] High-skill jobs are those typically requiring a college degree.

showing that, in Spain, labor force attachment is stronger among males than females as they tend to be responsible for contributing to a larger share of the household income than females (Gutierrez-Domenech 2005, and Fernández-Kranz *et al.* 2013). Moving now to columns 3 to 6, we observe that the effect of the reform is driven by middle-aged workers, and workers with children, suggesting that individuals with family responsibilities are more responsive. Columns 7 and 8 show that the effect of the reform affects both skill groups, although the effect is only statistically significantly different from zero for low-skilled workers. Interestingly, our result for low-skilled workers resembles that of Lalive *et al.* (2006), whose reform *only* affected low-income workers. We also observe that the effect of the reform is driven by those with a permanent contract prior to displacement (columns 9 and 10), and displaced from the private sector (columns 11 and 12) or large firms (columns 13 and 14).

7. Conclusion

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 reduced the RR from 60% to 50% for workers who remained unemployed more than 180 days. Using administrative records and quasi-experimental methods, we find that reducing the RR by 10 percentage points (or 17%) increases workers' job-finding probability by at least 41% relative to similar workers not affected by the reform. Interestingly, the reform affected the job-finding probability *before* the drop in the RR actually took place, suggesting an important anticipatory effect, consistent with job search theory. At the same time, we find that the reform did not affect wages, nor did it worsen post-non-employment job quality, suggesting that workers did not settle for inferior job matches.

What were the savings of this policy for the Spanish Government? Using the factual survivor functions with and without treatment (shown in Figure 6), we estimate the cost of monthly UI payments to 100 treated workers and 100 non-treated workers at different points in time in the non-employment spell (shown in columns 6 and 7, Panel A, in Table 9, respectively). Columns 8 and 9 in Panel A estimate the cumulative expenditures and column 10 estimates this reform's savings to the public sector. We assume an average pre-displacement wage of 1,000 euros. We find that, 6 months after displacement, this reform saved the public sector 11.188 euros per 100 displaced workers, the equivalent of 2.84% of total UI payments up until that point (shown in column 11 in Panel A). One year after displacement, this reform saved 83,773 euros per 100 displaced workers (or 12.9% of total UI payments), and 15 months after displacement, it saved 118,110 euros per 100 displaced workers (15.8% of total UI payments).

We can divide these savings in direct and indirect effects of the reform. The reform reduces UI expenditures *directly* as the RR decreases by 16.6% after 180 days of non-employment spell. To estimate this direct effect, we use the factual survivor functions *without treatment,* multiplied by the change in the RR. Direct savings from the reform are estimated in Panel B in Table 9. Since the RR does not change until week 26, the direct effects are zero up until then. Between week 27 and week 52, the total direct savings from this reform increase from 7,987 euros to 42,129 euros per 100 displaced workers, and by month 15, the direct savings from this reform totals 589,909 euros per 100 displaced workers (shown in column 8).

The indirect effect of the reform is the reduction in UI expenditures caused by the behavioral response of UI recipients. To estimate it, we add the savings in the first 6 months due to the lower survivor functions between treated and control groups (at a RR of 70%) to the savings observed thereafter due to the differences in survivor functions between treated and control groups (at a RR of 50%). As the sum of direct and indirect effects add to the total effects, columns 9 and 11 estimate the share of UI savings explained by the direct component (column 9, Panel B) and the indirect component (column 11, Panel B). Column 10 estimates the relative weight of the direct effect in total UI cost reduction.

The relative weight of the direct and the indirect components differ at different points in the non-employment spell. During the first 6 months of unemployment, all of the effects are behavioral effects (indirect component). After 180 days of unemployment, the direct effect begins to kick in, quickly gaining relevance. Within month 7 to month 15, the relative weight of the direct component goes from one third to more than half. Nonetheless, by month 15, behavioral changes continue to be an important factor driving the reduction in UI expenditures due to the policy change, as they explain close to half of the UI costs reduction. These findings contrast with those of Lalive *et al.* (2006) as these authors find that job seekers' behavioral responses explain no more than 10% of their policy costs change, and suggest that similar policies may have different effects due to alternative institutional settings.

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Figures and Tables

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 1: Unemployment Inflows in 2011 and 2012

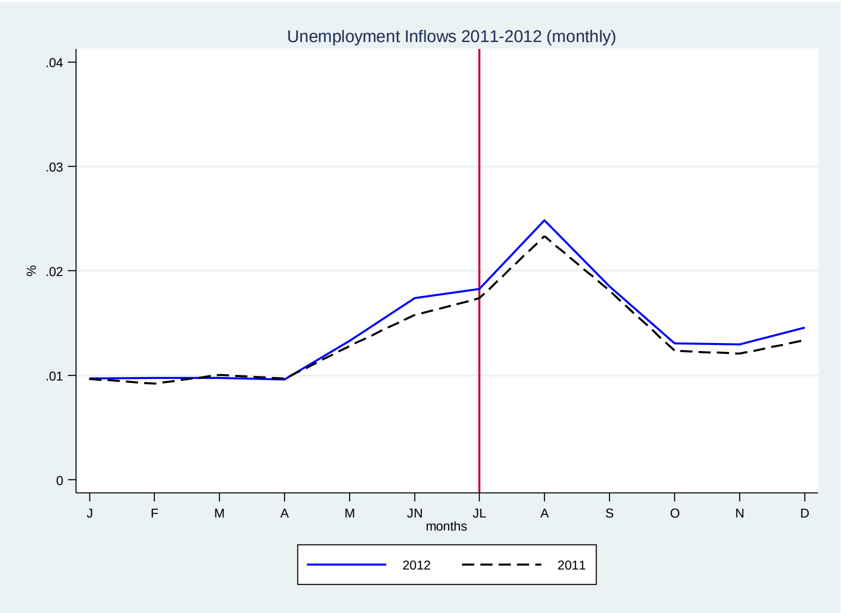


Figure 2. Unemployment Inflows for Treatment and Comparison Groups in 2012

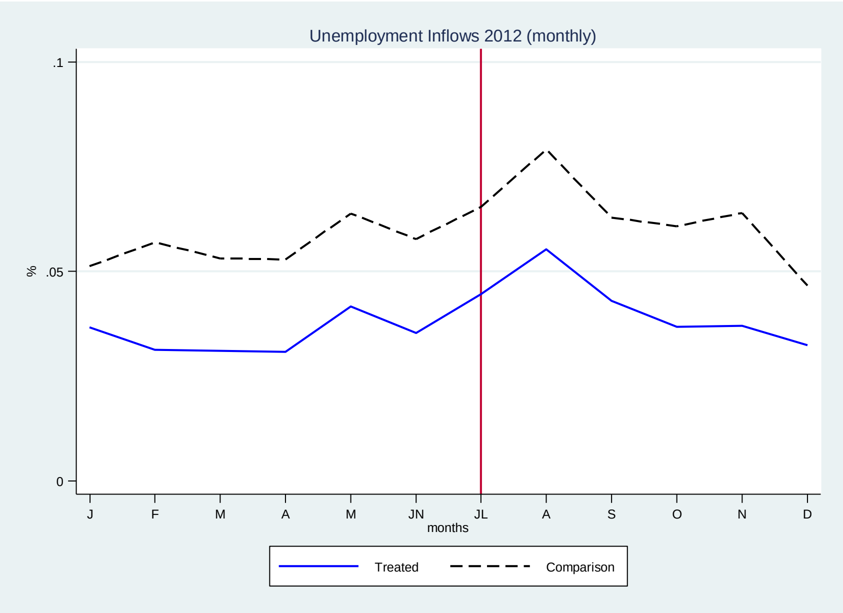


Table 2: Socio-Demographic Descriptive Statistics for Treated and Comparison Groups
(Percent unless stated otherwise)

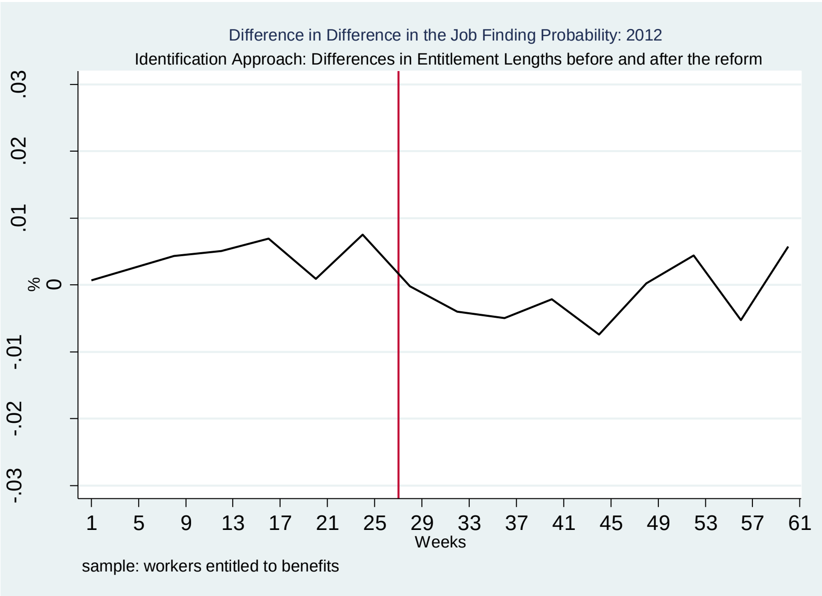
	Post-reform			Pre-reform			DiD
	Treated	Comparison	Diff	Treated	Comparison	Diff	
Panel A: Pre-displacement characteristics							
Female	0.68	0.64	0.034 (0.019)	0.68	0.63	0.049 (0.017)	-0.014 (0.019)
Immigrant	0.09	0.16	-0.075*** (0.01)	0.08	0.16	-0.075*** (0.01)	-0.000 (0.012)
With children	0.57	0.34	0.234*** (0.03)	0.58	0.40	0.189*** (0.02)	0.045 (0.044)
Experience in months	122	76	45.1*** (2.16)	119	71	47.6*** (1.87)	-2.16 (2.81)
Age in years	37	33	3.98*** (0.25)	37	33	3.61*** (0.28)	0.371 (0.41)
University	0.40	0.37	0.028 (0.024)	0.40	0.39	0.009 (0.013)	0.018 (0.027)
Assistance Benefits	0.02	0.07	-0.05*** (0.01)	0.03	0.07	-0.05*** (0.01)	0.008 (0.010)
Length of UI Entitlement in weeks	76	20	58*** (0.94)	77	20	58.5*** (0.87)	-0.459 (1.29)
Tenure in months	85	12	60.6*** (2.15)	82	12	59.5** (1.98)	1.14 (2.9)
Monthly wages in Euros	1,489	1,369	118*** (10.1)	1,494	1,400	94.8** (9.26)	23.3 (14.6)
Permanent contract	0.70	0.17	0.530** (0.018)	0.71	0.22	0.488** (0.017)	0.042 (0.024)
High skill job	0.07	0.04	0.027** (0.010)	0.06	0.05	0.010 (0.009)	0.014 (0.012)
Medium skill job	0.35	0.31	0.048** (0.019)	0.36	0.35	0.005 (0.018)	0.016 (0.013)
Low skill job	0.57	0.64	-0.075** (0.020)	0.57	0.58	-0.015 (0.018)	0.042* (0.019)
Manufacturing	0.17	0.15	0.018 (0.054)	0.18	0.15	0.023 (0.014)	-0.059 (0.021)
Construction	0.22	0.17	0.038* (0.016)	0.23	0.18	0.046** (0.015)	-0.007 (0.023)
Trade services	0.29	0.35	-0.058** (0.018)	0.27	0.30	-0.036* (0.017)	-0.022 (0.025)
Non-trade services	0.31	0.31	0.001 (0.019)	0.31	0.34	-0.032 (0.017)	0.034 (0.026)
Private sector	0.51	0.55	-0.019 (0.015)	0.52	0.45	0.0706 (0.013)	-0.089*** (0.020)
Panel B: Post-displacement employment characteristics							
Avg unemployment	40	34	6.02*** (0.83)	44	34	10.1*** (0.88)	-4.23*** (1.30)
% of exit to: employment	0.57	0.70	-0.13*** (0.03)	0.52	0.71	-0.19*** (0.03)	0.06*** [0.02]
Permanent contract	0.23	0.11	0.12*** (0.02	0.22	0.16	0.06*** (0.016)	0.06*** (0.028)
Full-time contract	0.79	0.73	0.06*** (0.021)	0.77	0.74	0.03*** (0.020)	0.03*** (0.029)
Downgrade (skills)	0.10	0.10	-0.006 (0.15)	0.10	0.09	0.009 (0.014)	-0.014 (0.021)
Wage	1131	1042	99.8** (30.5)	1161	1077	89.2** (34.1)	10.4 (46.5)
Sample size	2,689	857		3,289	958		7,793

Notes: High skill jobs are those requiring a college degree. “Differences” columns display a two-sample t test. Standard errors in parenthesis.
* 10% statistical significance level; ** 5% statistical significance level; *** 1% statistical significance level.

Figure 3.A. Job-Finding Hazard Rates between January 1 and December 31 2012, by Treatment Status and Before and After the Reform

Note: The vertical red line indicates the 180 days of UI receipt. We display monthly as opposed to weekly hazard rates to smooth out the figure

Figure 3.B. Difference-in-Difference in the Job-Finding Hazard Rates Shown in Figure 3.A.



Note: The vertical red line indicates the 180 days of UI receipt. We display monthly as opposed to weekly hazard rates to smooth out the figure

Table 3: Effects of Reducing the RR on the Job-Finding Probability (coefficient estimates, DiD Approach)

	(1) DD	**(2) DD**	**(3) DD**	**(4) DD**	**(5) DD**	**(6) DD**	**(7) DD**	**(8) DDD**	**(9) DD (sample 1)**	**(10) DD (sample 2)**	**(11) DD (sample 3)**	**(12) DD (sample 4)**	**(13) DD (sample 5)**
Reform	0.222***	0.207***	0.218***	0.316***	0.349***	0.351***	0.342***	0.312***	0.340***	0.244***	0.235*	0.247**	-0.069
	[0.07]	[0.07]	[0.07]	[0.08]	[0.08]	[0.07]	[0.08]	[0.13]	[0.10]	[0.09]	[0.13]	[0.11]	[0.10]
Job finding rate	*25%*	*23%*	*24%*	*37%*	*41%*	*41%*	*40%*	*36%*	*40%*	*27%*	*26%*	*28%*	*-6.6%*
	[0.09]	[0.09]	[0.09]	[0.10]	[0.11]	[0.17]	[0.14]	[0.11]	[0.14]	[0.11]	[0.16]	[0.14]	[0.09]
D_i^T, $D_i^{post\_july}$	X	X	X	X	X	X	X	X	X	X	X	X	X
$h_0(j)$ (4-week dummies)	X	X	X	X	X		X	X	X	X	X	X	X
$h_0(j)* D_i^T$						X							
Regional, seasonal and macro controls		X	X	X	X	X	X	X	X	X	X	X	X
Individual characteristics			X	X	X	X	X	X	X	X	X	X	X
UI covariates				X	X	X	X	X	X	X	X	X	X
Job characteristics					X	X	X	X	X	X	X	X	X
$D_i^{treated} *$ *state specific linear trends*							X						
Unobserved heterogeneity	X	X	X	X	X	X	X	X	X	X	X	X	X
N (observations)	344,214	344,214	344,214	344,214	344,214	344,214	344,214	622,005	226,753	320,941	134,599	221,789	217,224
N (individuals)	7,793	7,793	7,793	7,793	7,793	7,793	7,793	15,506	4,300	9,106	3,461	5,096	4,918

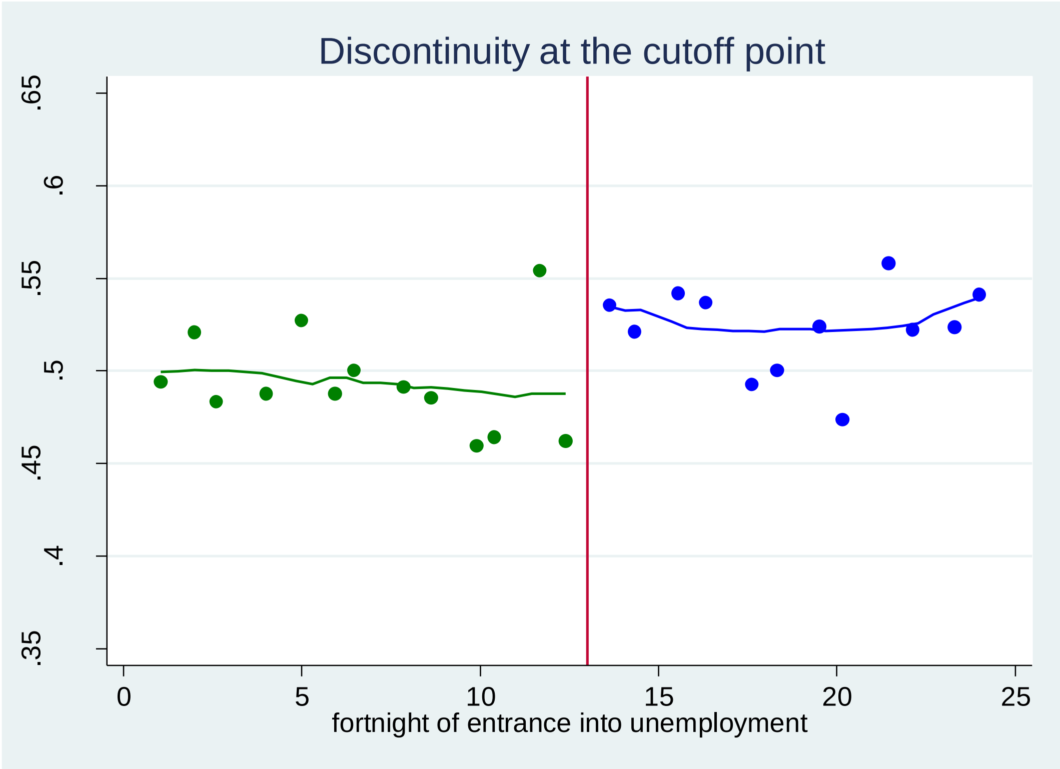
Notes: All models are estimated using a discrete-time hazard model with gamma frailty. Model 5: Preferred specification. Model 6: Allows for duration dependence terms to change for treated versus comparison groups. Model 7: Adds to Model 5 state-specific linear trends interacted by treatment. Model 8: Uses workers displaced during 2011 as the third difference. Model 9: Restricts the sample to workers who became unemployed three months before and after July 15 2012. Model 10: Adds to the reference sample temporary layoffs (that is, workers who returned to the same firm). Model 11: Only uses as treatment group individuals whose UI entitlements are between 8 and 12 months. The control group remains the same as in our preferred specification. Models 12 and 13 use an alternative identification DD strategy. In column 12, the sample is composed by workers with low pre-displacement wages where the comparison-group workers are those with UI entitlements lengthier than 6 months but not affected by the policy because their benefits are too low and treated workers are those affected by the reform but whose pre-displacement wages are below the median. Column 13 uses the same identification strategy as Column 12 but this time the treatment group are workers affected by the reform whose pre-displacement wages are above the median. The control group is composed by workers with UI entitlements lengthier than 6 months but not affected by the policy because their benefits are too high. * 10% statistical significance level; ** 5% statistical significance level; *** 1% statistical significance level.

Table 4: Effects of Reducing the RR on the Job-Finding Probability
Preferred Specification and Placebo Tests (DiD)

	(1) Preferred	(2) Placebo 1	(3) Placebo 2	(4) Placebo 3
Reform	0.349***	0.072	0.138	0.089
	[0.08]	[0.09]	[0.10]	[0.08]
Job finding rate	*41%*	*7.4%*	*14.7%*	*9.3%*
	[0.08]	*[0.08]*	*[0.11]*	*[0.09]*
$D_i^{treated}$, $D_i^{post\_july}$	X	X	X	X
$h_0(j)$ (4-week dummies)	X	X	X	X
Regional, Macro and Seasonal controls	X	X	X	X
Individual, Job and UI covariates	X	X	X	X
N (observations)	344,214	320,679	139,832	197,927
N (Individuals)	7,793	7,713	3,861	4,892

Notes: All models are estimated using discrete-time hazard model with gamma frailty. All models use the same specification as our preferred one shown in column 5, Table 3. Column 1: Preferred specification. Column 2: Placebo 1 uses *only* workers who entered unemployment during 2011 (instead of 2012). Column 3: Placebo 2 uses *only* workers who entered unemployment between January and June 2012 (before the reform under analysis took place) with the fictitious policy-change date of April 1 2012. Column 4: Placebo 3 uses as treated workers those entitled to more than 180 days of UI but who are *not* affected by the reform because they hit the floor of UI entitlement (high-wage workers) or the ceiling (low-wage workers); and the comparison-group workers are those whose benefits also hit the floor or the ceiling *and* have UI entitlements shorter than 6 months. In this case, all workers have been displaced in 2012.

Figure 4. Discontinuity of Job-Finding Rates at the cutoff point



Note: This graph explores whether there is any evidence of a jump in the job-finding probability around the threshold. Bandwidth is 6 months and bins are defined in fortnight.

Table 5: Effects of Reducing the RR on the job Finding Probability
(Coefficient estimates, RD Approach)

	(1)	(2)	(3)	(4)	(5)
$D_i^{post\_july}$	0.157**	0.278**	0.171**	0.232**	0.246**
	[0.06]	[0.11]	[0.06]	[0.08]	[0.08]
Job finding rate	*17%*	*32%*	18%	26%	28%
	[0.07]	*[0.14]*	*[0.07]*	*[0.10]*	*[0.10]*
$h_0(j)$ (4-week dummies)	*X*	*X*	*X*	*X*	*X*
Regional, seasonal, macro, job and individual characteristics and UI			X	X	X
Unobserved heterogeneity		X		X	X
N (Individuals)	5,978	5,978	5,978	5,978	5,978
N (observations)	369,006	369,006	369,006	369,006	369,006

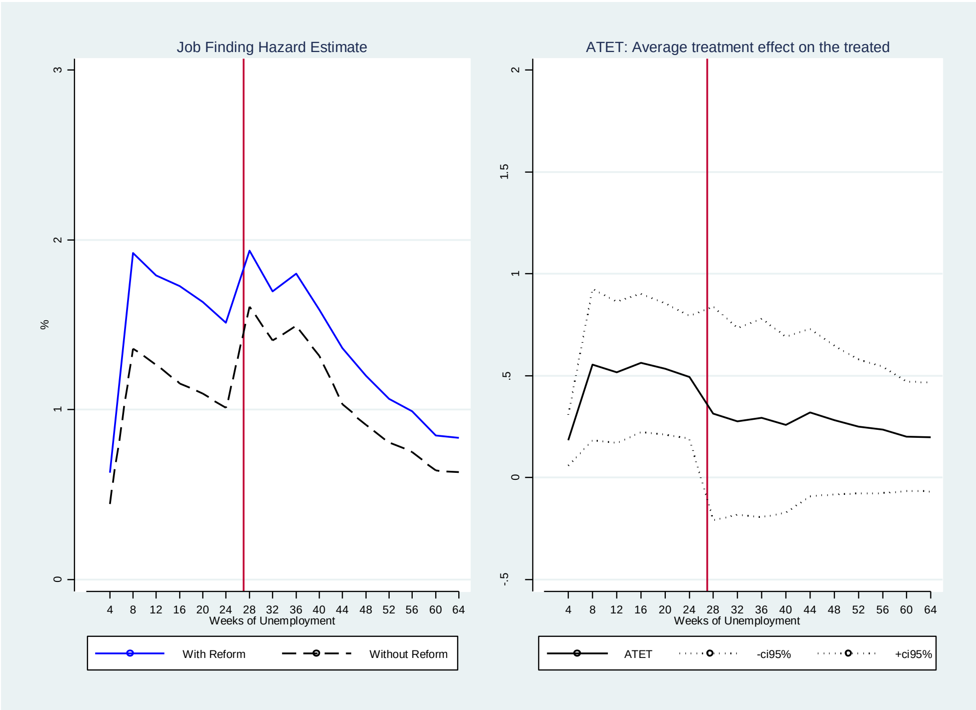
Notes: This table reports the job finding rate of the post-reform indicator controlling for a lineal trend, which allows for different coefficients on each side of the cutoff. In Columns (1)-(5) bandwidth is 6 months. The sample used for the RD exercise is composed by workers whose entitlements are lengthier than 6 months and whose benefits levels do not hit the lower or upper ceilings levels. The treatment group includes those who enter unemployment after July 15 2012. The control group includes those who enter unemployment before July 15 2012. In columns (1) to (4) bins are defined in terms of fortnights. In column (5) bins are defined in terms of weeks.
* 10% statistical significance level; ** 5% statistical significance level; *** 1% statistical significance level.

Table 6. Heterogeneous Effects of the Reform along the Non-Employment Spell (DiD)

	(1)	**(2) Placebo (2011)**
Reform*1-12 Weeks	0.358***	0.132
	[0.12]	[0.11]
Job finding rate	*43%*	*14%*
	[0.17]	*[0.12]*
Reform*13-26 Weeks	0.415***	-0.148
	[0.13]	[0.12]
Job finding rate	*51%*	*-13.7%*
	[0.19]	*[0.10]*
Reform*27-34 Weeks	0. 176	-0. 040
	[0.15]	[0.14]
Job finding rate	*19%*	*-3.9%*
	[0.18]	*[0.13]*
Reform*>34 Weeks	0.281	0.120
	[0.18]	[0.10]
Job finding rate	*32%*	*12.7%*
	[0.23]	*[0.11]*
Regional, Seasonal and macro controls	X	X
Individual and Job Characteristics and UB covariates	X	X
Unobserved Heterogeneity	X	X
N (observations)	344,214	320,679
N (individuals)	7,793	7,713

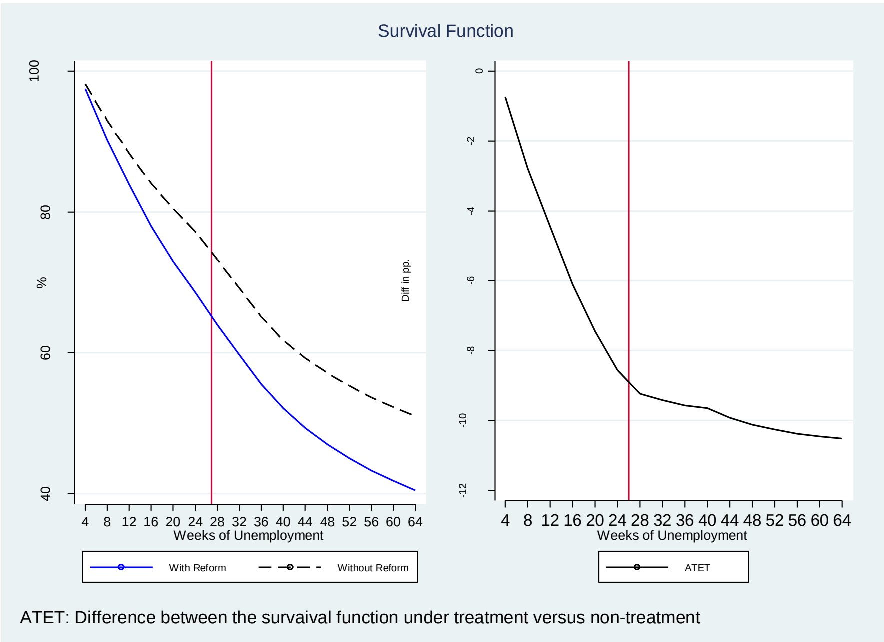
Notes: Hazard Models with gamma frailty. Standard errors in brackets. Sample Characteristics for Columns (1) and (2) are the same as those previously defined in Table 2, column 5 and in Table 4, Column 2, respectively.
* 10% statistical significance level; ** 5% statistical significance level; *** 1% statistical significance level.

Figure 5: Estimated Average Treatment Effect on the Treated: Hazard Rates
(Based on Heterogeneous Effects Model in Table 4)



Note: On the left-hand side we represent the estimated job Finding Probability obtained from model parameters for treated workers with and without the reform. On the right hand side we represent the difference between the job finding probability for treated workers with and without the reform.

Figure 6: Estimated Average Treatment Effect on the Treated: Survivals Rates
(Based on Heterogeneous Model in Table 4)



Note: On the left-hand side we represent the estimated Survival Probability obtained from model parameters for treated workers with and without the reform. On the right hand side we represent the difference between the survival probability for treated workers with and without the reform.

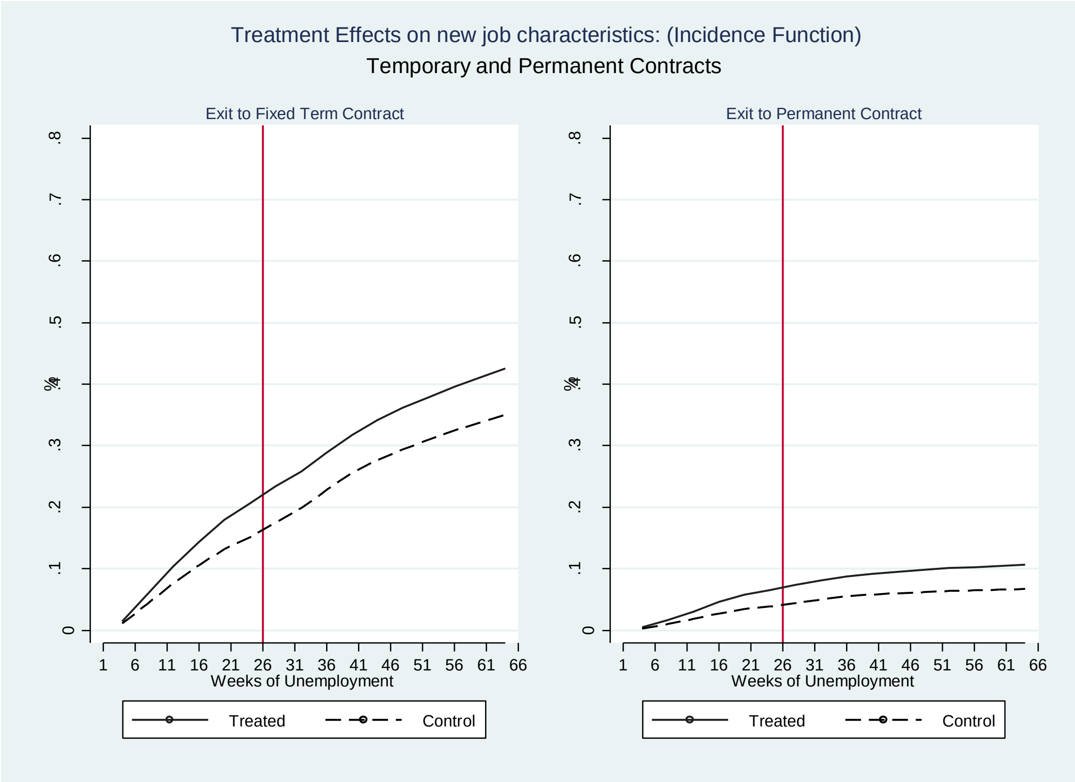
Table 7. Effects of the Reform on Post-Displacement Log Monthly Wages (OLS and Maximum Likelihood Estimation)

	DiD specification		RD specification	
	OLS : wage equation	MLE: Wage equation controlling for dynamic selection	OLS : wage equation	MLE: Wage Equation controlling for dynamic selection
	(1)	(2)	(3)	(4)
Reform	0.436**	-0.015	0.396**	-0.000
	[0.18]	[0.03]	[0.16]	[0.03]
Unemployment Duration (logs)	-	-0.052**	-	-0.085**
		[0.01]		[0.01]
$D_i^{post\_july}$, D_i^{ent6m}	X	X		
Linear Trend			X	X
Macro, UI, Individual and Job Covariates	X	X	X	X
Unobserved Heterogeneity		X		X
N (individuals, wage equation)	6,890	3,876	5,395	2,842

Notes: Robust standard errors clustered at the individual level in brackets. Columns 1 and 3: Wage equation is estimated using all workers. Columns 2 and 4: Using the Maximum Likelihood Estimation (MLE), we jointly estimate the wage equation and the unemployment hazard rate controlling for unobserved heterogeneity using the Heckman-Singer approach (two-mass points and correlated unobserved heterogeneity between the hazard and the wage equation). We only display the results relative to the wage equation.
Sample size is smaller than that of Table 2 because some of the wages were missing. For the RD model, separated linear trends for treated and control observations are used.
* 10% statistical significance level; ** 5% statistical significance level; *** 1% statistical significance level.

Figure 7. Incidence Functions Based on Estimates in Appendix Table A.8[51]

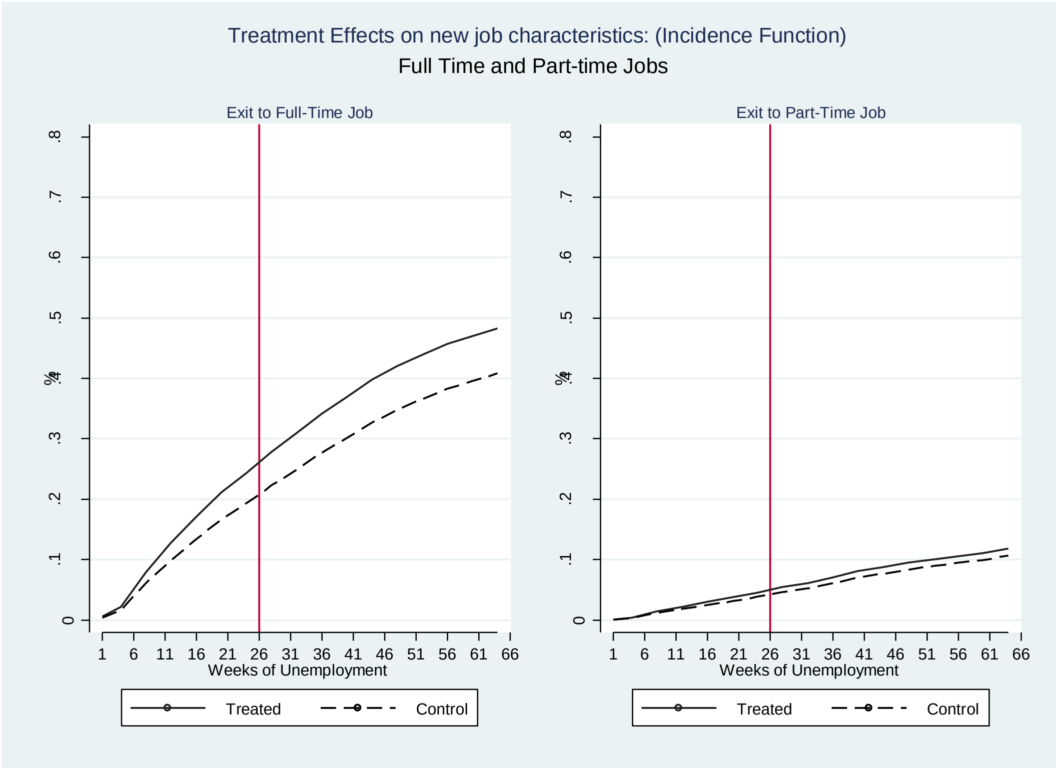
Panel A. Outcome 1: Fixed-term versus permanent contract



Note: Incidence Functions computed using parameters estimated. Table A.7, Columns (1)-(2)

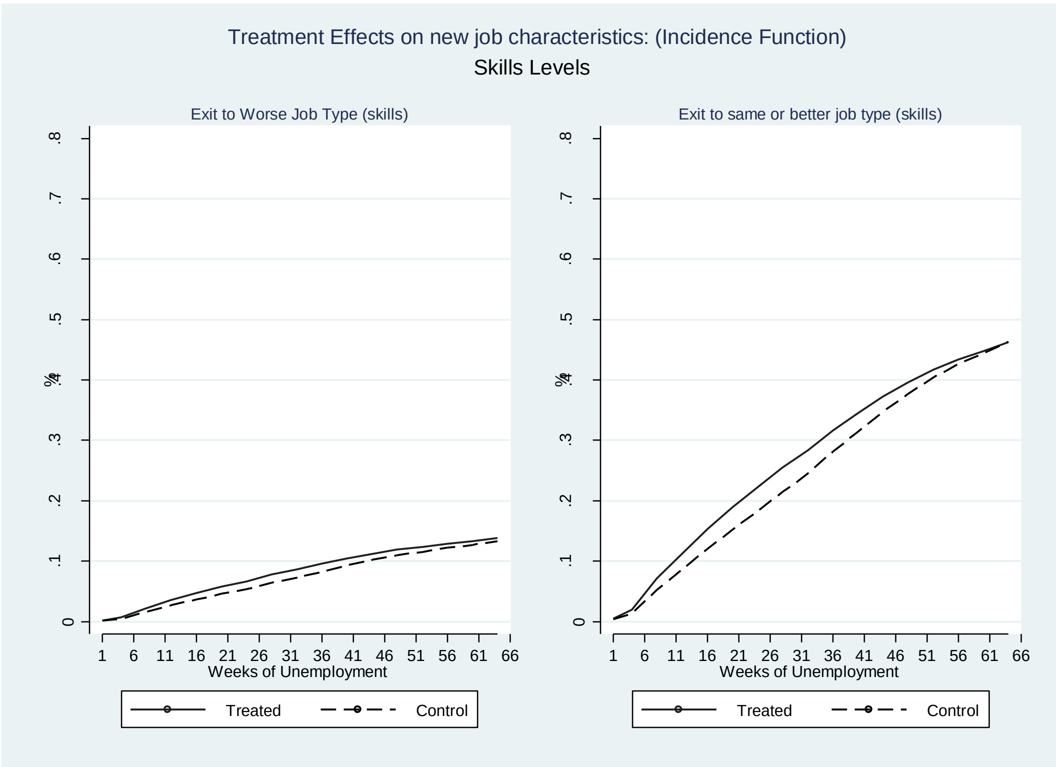
[51] In our analysis, the unemployed is subject to different causes of failure (i.e competing risks). The cumulative incidence curve is a proper summary curve, showing the cumulative failure rates over time due to a particular cause. These incidence functions are computed using parameters estimates shown in Table A.7

Panel B. Outcome 2: Full- versus part-time job



Note: Incidence Functions computed using parameters estimated. Table A.7, Columns (3)-(4)

Panel C. Outcome 3: Upgrading versus downgrading job



Note: Incidence Functions computed using parameters estimated. Table A.7, Columns (5)-(6)

Table 8: Subgroup Analysis of Effects of the Reform

	Gender		Age		Family composition		Skills		Contract type		Private versus public		Firm´s size	
	Females	Males	< =30	>30	No child	At least one child	High and medium	Low	Fixed-term	Permanent	Private firm	Public firm	< 20 employees	>20 employees
	(1)	(2)	(3)	(4)	(13)	(14)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
On hazard rate	0.258**	0.553***	0.069	0.405**	0.177	0.647***	0.272	0.281**	0.225**	0.563***	0.467***	0.261**	0.189	0.527**
	[0.09]	[0.15]	[0.12]	[0.10]	[0.11]	[0.192]	[0.18]	[0.13]	[0.14]	[0.36]	[0.12]	[0.12]	[0.11]	[0.17]
Job finding rate	*28%* *[0.11]*	*73%* *[0.26]*	*7.1%* *[0.13]*	*49%* *[0.15]*	*19%* *[0.13]*	*91%* *[0.36]*	*31%* *[0.23]*	*32%* *[0.17]*	*25%* *[0.17]*	*75%* *[0.63]*	*59%* *[0.19]*	*29%* *[0.15]*	*21%* *[0.13]*	*69%* *[0.29]*
$h_0(j)$ (4-week dummies)	X	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	X	X
Individual, Job and UI covariates	X	X	X	X	X	X	X	X	X	X	X	X	X	X
N (observations)	4,740	3,053	2,550	5,243	4,937	2,856	2,484	5,309	3,498	4,295	4,093	3,700	5,323	2,470
N (individuals)	221,268	122,946	87,229	256,985	224,272	119,942	97,832	246,382	124,278	219,936	172,283	171,931	231,355	112,859

Notes: All Hazard models are estimated with gamma frailty and with the same set of covariates used in our preferred specification. Standard errors in brackets. High-skill jobs are those typically requiring a college degree.

* 10% statistical significance level; ** 5% statistical significance level; *** 1% statistical significance level.

Table 9. Total Unemployment Insurance Expenditures, Model with Heterogeneous Effects
Assuming pre-displacement wage is 1,000 euros, and 100 treated workers and 100 control workers

Panel A. Total Effects of the Reform

Length unemp. (months)	Survival Treated	Survival Control	# treated workers = 100	# control workers = 100	Monthly UI to treated workers (euros)	Monthly UI to control workers (euros)	Cumulative UI received by treated workers (euros)	Cumulative UI received by control workers (euros)	Difference in total UI payments (euros)	Savings in total UI payments (in percent)
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
1	1	1	100	100	70,000	70,000	70,000	70,000	0	0.00%
2	0.993	0.995	99	99	69,535	69,683	139,535	139,683	-148	-0.11%
3	0.974	0.982	97	98	68,157	68,741	207,692	208,424	-732	-0.35%
4	0.898	0.929	90	93	62,848	65,050	270,540	273,474	-2,934	-1.07%
5	0.831	0.882	83	88	58,171	61,715	328,710	335,189	-6,479	-1.93%
6	0.771	0.838	77	84	53,953	58,662	382,663	393,851	-11,188	-2.84%
7	0.717	0.799	72	80	35,872	47,919	418,535	441,770	-23,235	-5.26%
8	0.671	0.763	67	76	33,535	45,799	452,070	487,569	-35,500	-7.28%
9	0.620	0.721	62	72	30,983	43,283	483,053	530,853	-47,800	-9.00%
10	0.573	0.680	57	68	28,660	40,826	511,713	571,679	-59,966	-10.49%
11	0.529	0.641	53	64	26,448	38,439	538,162	610,118	-71,957	-11.79%
12	0.494	0.608	49	61	24,689	36,506	562,851	646,624	-83,773	-12.96%
13	0.466	0.582	47	58	23,278	34,904	586,129	681,528	-95,399	-14.00%
14	0.441	0.558	44	56	22,055	33,497	608,184	715,024	-106,841	-14.94%
15	0.420	0.538	42	54	21,011	32,280	629,194	747,305	-118,110	-15.80%

Panel B. Direct Effects of the Reform

Length unemp. (months)	Survival Control	# control workers = 100	Monthly UI to treated workers (euros)	Monthly UI to control workers (euros)	Cumulative UI received by treated workers (euros)	Cumulative UI received by treated workers (euros)	Difference in total UI payments (euros)	Share of savings in UI payments due to UI system	Direct savings/ total savings (relative weight of direct effects)	Share of savings due to behavior of job seekers
(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
1	1	100	70,000	70,000	70,000	70,000	0.00	0.00%	0.00%	0.00%
2	0.995	98	69,683	69,683	139,683	139,683	0.00	0.00%	0.00%	-0.11%
3	0.982	93	68,741	68,741	208,424	208,424	0.00	0.00%	0.00%	-0.35%
4	0.929	88	65,050	65,050	273,474	273,474	0.00	0.00%	0.00%	-1.07%
5	0.882	84	61,715	61,715	335,189	335,189	0.00	0.00%	0.00%	-1.93%
6	0.838	80	58,662	58,662	393,851	393,851	0.00	0.00%	0.00%	-2.84%
7	0.799	76	39,933	47,919	433,784	441,770	-7,987	-1.81%	34.37%	-3.45%
8	0.763	72	38,166	45,799	471,950	487,569	-15,620	-3.20%	44.00%	-4.08%
9	0.721	68	36,070	43,283	508,019	530,853	-22,834	-4.30%	47.77%	-4.70%
10	0.680	64	34,022	40,826	542,041	571,679	-29,638	-5.18%	49.42%	-5.31%
11	0.641	61	32,033	38,439	574,074	610,118	-36,045	-5.91%	50.09%	-5.88%
12	0.608	58	30,422	36,506	604,495	646,624	-42,129	-6.52%	50.29%	-6.44%
13	0.582	56	29,086	34,904	633,582	681,528	-47,946	-7.04%	50.26%	-6.96%
14	0.558	54	27,914	33,497	661,496	715,024	-53,529	-7.49%	50.10%	-7.45%
15	0.538	52	26,900	32,280	688,396	747,305	-58,909	-7.88%	49.88%	-7.98%

Acknowledgments: We would like to thank the Editor, Judith Hellerstein, and two anonymous referees for helpful comments, which greatly improved this draft of the paper. We would like to thank participants of the SOLE 2015 meetings, Simposio Analisis Economico 2015, and 2015 ESPE meetings. This paper has also benefitted from comments from Bart Cockx, Ignacio García-Pérez, Lawrence Katz, Camille Landais, Arash Nekoei, and Hernan Ruffo. Núria Rodríguez-Planas acknowledges financial support from the PSC-CUNY Research Award. Yolanda Rebollo-Sanz acknowledges financial support from the Spanish Ministry of Science and Innovation (grant No. *ECO2013-43526-R*) and from the Regional Government *P09-SEJ-4546*

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Appendix

Table A.1 Results from the duration model for the Unemployment : Unemployment exit probability to Employment

	With Unobserved Heterogeneity	
	Coef	**Se**
Reform ($D^{T*} D^{post}$)	**0.349**	**0.089**
D^T	-0.103	0.091
D^{post}	-0.242	0.081
$h_0(j)$		
λ_2 (Week5-8)	1.107	0.082
λ_3 (Week9-12)	1.081	0.090
λ_4 (Week13-16)	1.119	0.093
λ_5 (Week17-20)	1.068	0.098
λ_6 (Week21-24)	0.998	0.102
λ_7 (Week25-28)	1.110	0.110
λ_8 (Week29-32)	1.025	0.116
λ_9 (Week33-36)	1.094	0.123
λ_{10}(Week37-40)	1.018	0.131
λ_{11} (Week41-44)	1.019	0.136
λ_{12} (Week45-48)	0.901	0.141
λ_{13} (Week49-52)	0.807	0.152
λ_{14} (Week53-56)	0.794	0.158
λ_{15} (Week57-60)	0.638	0.168
λ_{16} (Week61-64)	0.626	0.175
Individual Characteristics		
Females	0.123	0.045
Experience (in logs)	0.713	0.054
Age (in logs, time varying)	-1.835	0.154
University	0.114	0.044
Children	0.073	0.026
Immigrant	-0.026	0.074
UI covariates		
UI Entitlement Length (in logs)	-0.312	0.069
Receive UB (time-varying)	-1.062	0.076
Receive UA (time varying)	-2.938	0.190
Previous Job Characteristics		
Permanent Contract	-0.132	0.041
Industry	-0.136	0.051
Construction	-0.171	0.048
Commerce and Hotels	-0.025	0.043
High Skill	0.103	0.098
Medium Skills	0.033	0.032
Public Firm	-0.099	0.052
Tenure (in logs)	-0.140	0.034
GDP growth rate (Quarterly)	0.439	0.129
Constant	1.290	0.471
Gamma Var	0.651	0.120

Note: PGM hazard model with gamma frailty. LR test for Gamma=0 (14.45).[52]
Dummy variables for regions and months are used in the estimation but omitted from the Table.
The constant term contains native, low educated male workers, without children, hired with temporary contracts in the service sector with low skills at the first month of unemployment. High skills: Engineering, Judge and so on. Technical engineers, experts and qualified assistants and Administrative and Workshop Managers. Medium Skills: non-qualified assistants, Administrative Officers; Junior staff; Administrative Assistants; Low Skills: First and second class officials and Third order officials.

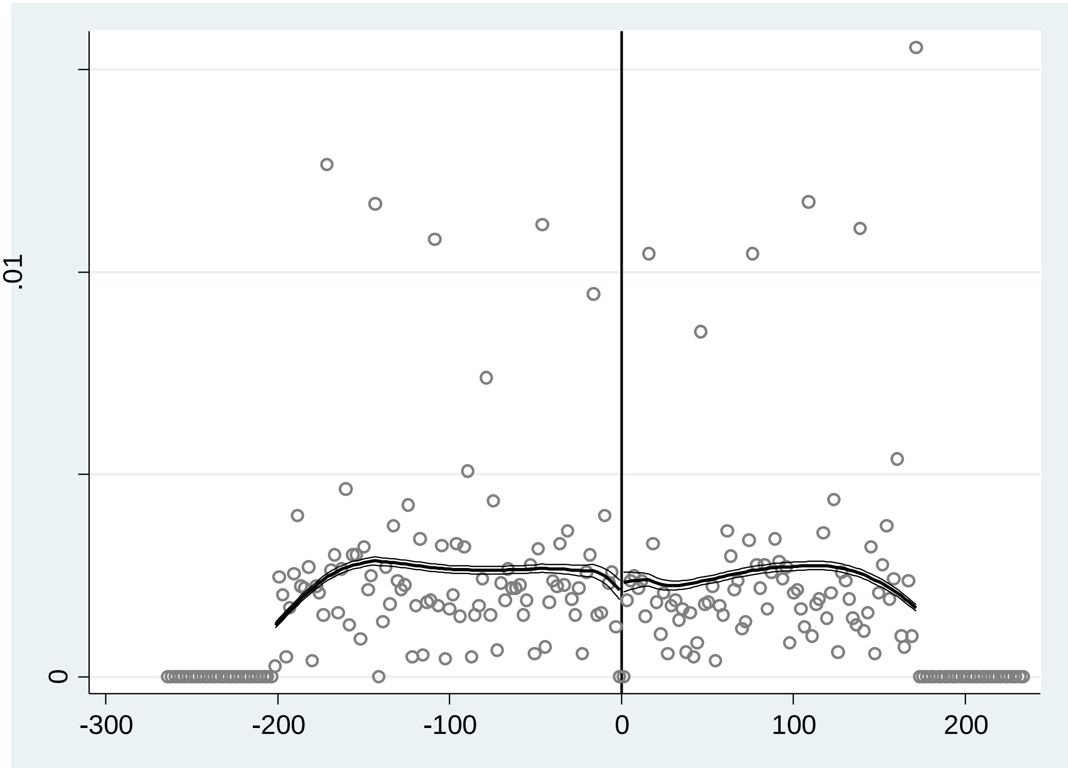
[52] The size of the variance of the gamma mixture distribution relative to its standard error suggests, however, that unobserved heterogeneity is significant in this data set. The likelihood ratio test of the model with versus the model without unobserved heterogeneity, also suggests the same conclusion.

Table A.2: Sensitivity Analysis: Unobserved Heterogeneity

	(1) DiD	(2) DiD	(3) DiD
Reform	0.349*** [0.09]	0.324*** [0.08]	0.262*** [0.06]
D_i^T, $D_i^{post\_july}$	X	X	X
$h_0(j)$ (4-week dummies)	X	X	X
Regional, seasonal and macro controls	X	X	X
Individual, job and UI covariates	X	X	X
Log Likelihood	-23000.505	-23005.505	-23029.665
Unobserved Heterogeneity	Gamma	Heckman Singer	None

Notes: Standard errors in brackets.
Model 2: Heckman-Singer with two mass points. Prob. Type 1=0.23, Prob. Type 2=0.76
*** 1% statistical significance level.

Appendix Figure A.1 Density Test of Manipulation in the Running Variable



Note: Density test of manipulation in the running variable. The density test is based on McCrary (2008) and is implemented using the DCDensity.ado routine in Stata. Discontinuity estimate (standard error in parentheses): 0.106 (0.08)

Table A.3: Regression Discontinuity Design Validity Tests for Main Covariates

	Female	Immigrant	Age	Universit	Children
$D_i^{post\_july}$	0.008 [0.01]	-0.004 [0.01]	-0.001 [0.01]	-0.011 [0.02]	-0.028 [0.03]
	UI entitlement	Industry	Construction	Services	Labor Market Experience
$D_i^{post\_july}$	-0.035 [0.15]	-0.061*** [0.03]	0.034** [0.02]	0.028 [0.02]	-0.023 [0.02]
	Public Sector	Permanent Contract	Previous Tenure	High Skill	Med-Skill
$D_i^{post\_july}$	0.001 [0.02]	-0.012 [0.02]	-0.059** [0.03]	-0.013 [0.02]	-0.008 [0.02]

Note: We test whether there is any discontinuity in observable characteristics around the threshold. Separated treatment and control trends are included in the model. Robust standard errors clustered at the individual level in brackets. Sample sizes are the same as those presented in Table 5.

Table A.4: Effects of Reducing the RR on the job Finding Probability: Placebo Tests (coefficient estimates, RD Approach)

	(1) Placebo 1 (2011)	(2) Placebo 2 (RR)	(3) Placebo 3 (Entitlements<26)
$D_i^{post\_july}$	0.051 [0.23]	0.022 [0.07]	-0.195 [0.13]
Job finding rate	5% [0.24]	2% [0.07]	-17% [0.13]
$h_0(j)$ (4-week dummies)	X	X	X
Regional, Seasonal and macro controls	X	X	X
Individual, job and UI covariates	X	X	X
N (Individuals)	4,163	2,264	771
N (observations)	319,397	165,756	34,742

Notes: Standard errors in brackets. Placebo 1 in Column (1) presents the RD estimate using data from 2011; Placebo 2 in Column (2) presents the RD estimate using *only* workers who entered unemployment between January and June 2012 (before the reform under analysis took place) and applying the fictitious policy-change date of April 1, 2012. Column (3) presents the RD estimate using workers displaced in 2012 with entitlements shorter than six months, and hence not eligible. All models are estimated using linear trend term specific to control and treated groups.
* 10% statistical significance level; ** 5% statistical significance level; *** 1% statistical significance level.

Table A.5: Results from the duration model for the Unemployment: Heterogeneous results by unemployment duration

	With Unobserved Heterogeneity	
$h_0(j)$*DiD parameters	**Coef**	**Se**
λ_l : 1-12 weeks		
Reform* λ_l	**0.358**	**0.121**
D^T* λ_l	-0.021	0.131
D^{post}* λ_l	-0.268	0.142
λ_l	-0.474	0.177
λ_l : 13-26 weeks		
Reform* λ_l	**0.415**	**0.131**
D^{T*} λ_l	-0.230	0.142
D^{post*} λ_l	-0.289	0.139
λ_l :	-0.409	0.212
λ_l : 27-40 weeks		
Reform* λ_l	**0.176**	**0.150**
D^{T*} λ_l	0.137	0.151
D^{post*} λ_l	-0.098	0.176
λ_l :	-0.693	0.242
λ_l >40 weeks		
Reform* λ_l	**0.281**	**0.183**
D^{T*} λ_l	0.087	0.121
D^{post*} λ_l	-0.426	0.201
$h_0(j)$* (weeks)		
λ_2 (Week5-8)	1.116	0.098
λ_3 (Week9-12)	1.064	0.092
λ_4 (Week13-16)	1.150	0.137
λ_5 (Week17-20)	1.142	0.157
λ_6 (Week21-24)	1.080	0.161
λ_7 (Week25-28)	1.156	0.173
λ_8 (Week29-32)	1.063	0.202
λ_9 (Week33-36)	1.093	0.209
λ_{10} (Week37-40)	0.938	0.215
λ_{11} (Week41-44)	0.484	0.140
λ_{12} (Week45-48)	0.397	0.136
λ_{13} (Week49-52)	0.291	0.131
λ_{14} (Week53-56)	0.210	0.132
λ_{15} (Week57-60)	0.062	0.142
Individual Characteristics		
Females	0.127	0.042
Experience (in logs)	0.673	0.040
Age (in logs)	-1.912	0.094
University	0.123	0.048
Children	0.087	0.031
Immigrant	-0.027	0.087
UI covariates		
UI Entitlement Length (in logs)	-3.166	0.088
Receive UI (time varying)	-1.081	0.112
Receive UA (time varying)	-2.851	0.220
Previous Job Characteristics		
Permanent Contract	-0.087	0.039
Industry	-0.100	0.061
Construction	-0.141	0.068
Commerce and Hotels	-0.005	0.053
High Skill	0.011	0.088
Medium Skills	0.046	0.042
Public Firm	-0.102	0.059
Tenure (in logs)	-1.166	0.323
GDP growth rate (quarterly)	0.611	0.180
Constant	2.108	0.596
Gamma var	0.717	0.135

Note: Dummy variables for regions and quarters are used in the estimation but omitted from the table.

Table A.6: Sensitivity Analysis: Unobserved Heterogeneity (DiD)

	(1) Gamma	(2) Heckman Singer	(3) Without
Reform*1-12 weeks	0.358***	0.351***	0.338***
	[0.12]	[0.1	[0.12]
Reform*13-26 weeks	0.415***	0.368***	0.337***
	[0.13]	[0.12]	[0.12]
Reform*27-40 weeks	0.176	0.121	0.095
	[0.15]	[0.15]	[0.14]
Reform*>40 weeks	0.281	0.203	0.163
	[0.18]	[0.18]	[0.16]
$D_i^{treated}$, $D_i^{post\_july}$	X	X	X
Regional, Seasonal and macro controls	X	X	X
Individual, Job and UI Characteristics	X	X	X
Log Likelihood	-22992.175	-22997.505	-23105.39719

Notes: Standard errors in brackets. Model 2: Heckman-Singer with two mass points. Prob. Type 1=0.23, Prob. Type 2=0.76

Appendix Table A.7. Effects of the Reform on Post-Displacement Job Characteristics:

Exit to :	Temporary Contract	Permanent Contract	Part-time Contract	Full-time Contract	Worse job (occupation)	Same/Higher Job (occupation)
Panel A						
Reform	0.328***	0.573**	0.372	0.370**	0.272	0.316**
	[0.10]	[0.21]	[0.14]	[0.10]	[0.18]	[0.14]
D_i^{ent6m}, $D_i^{post\_july}$	*X*	*X*	*X*	*X*	*X*	*X*
$h_0(j)$ (4-week dummies)	*X*	*X*	*X*	*X*	*X*	*X*
Individual, Job and UI covariates	*X*	*X*	*X*	*X*	*X*	*X*
Regional, Macro and Seasonal Controls	*X*	*X*	*X*	*X*	*X*	*X*
Unobserved Heterogeneity	*X*	*X*	*X*	*X*	*X*	*X*
Panel B: Model allowing for heterogeneous effects along the non-employment spell						
Reform (1-12 weeks)	0.325***	0.540**	0.203	0.392**	0.478	0.413**
	[0.15]	[0.30]	[0.28]	[0.14]	[0.13]	[0.15]
Reform (13-26 weeks)	0.411***	0.625**	0.266	0.442**	0.608	0.253**
	[0.15]	[0.32]	[0.26]	[0.15]	[0.13]	[0.18]
Reform (27-40 weeks)	0.176	0.398	0.107	0.182	0.038	0.263**
	[0.18]	[0.42]	[0.26]	[0.18]	[0.13]	[0.08]
Reform (> 40 weeks)	0.317	0.661	0.411	0.232	0.148	0.353**
	[0.20]	[0.53]	[0.31]	[0.21]	[0.36]	[0.21]
D_i^{ent6m}, $D_i^{post\_july}$	*X*	*X*	*X*	*X*	*X*	*X*
$h_0(j)$ (4-week dummies)	*X*	*X*	*X*	*X*	*X*	*X*
Individual, Job and UI covariates	*X*	*X*	*X*	*X*	*X*	*X*
Regional, Macro and Seasonal Controls	*X*	*X*	*X*	*X*	*X*	*X*
Unobserved Heterogeneity	*X*	*X*	*X*	*X*	*X*	*X*

Notes: Standard errors in brackets. Sample size might vary because missing information on new job characteristic.
10% statistical significance level; ** 5% statistical significance level; *** 1% statistical significance level.