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Bringing Them In or Pushing Them Out? The Labor Market Effects of Pro-cyclical Unemployment Assistance Changes

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ABSTRACT

We exploit an unanticipated labor market reform to estimate the effects of pro-cyclical changes in long-term unemployment assistance (UA). In July 2012, Spain raised the minimum age to receive unlimited-duration UA from 52 to 55. Using a difference-in-differences design, we document that shorter benefits caused (i) shorter non-employment duration, especially among younger workers; (ii) higher labor force exit and other programs' take-up, especially among older workers; (iii) lower wages upon re-employment. The reform induced moderate government savings. Our results highlight the importance of considering the interplay with labor market conditions when designing long-term benefit schedules that affect workers close to retirement.

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1 Introduction

When recessions affect highly indebted countries, the interplay between contractions in government spending and the economic crisis may result in pro-cyclical changes in social transfers. Cutting benefits during economic downturns can be a double-edged sword. On the one hand, benefit cuts might increase the search effort of affected workers, leading to an increase in re-employment. On the other hand, the social insurance value of benefits is higher during recessions, as recipients are more likely to exhaust them without finding a job, face credit constraints, and experience consumption drops. The latter is particularly true for older workers, who are more likely to become unemployed during recessions and might exploit welfare programs to bridge to retirement (Inderbitzin et al. 2016, Kyyrä and Ollikainen 2008, Lalive 2008).

In this paper, we study the labor market effects of pro-cyclical changes in long-term unemployment assistance (UA) benefits for older workers. UA benefits are means-tested transfers available upon exhaustion of short-term unemployment insurance (UI) benefits. They are common in many European countries. We exploit an unanticipated reform adopted in Spain in July 2012, at the peak of the country's recession, that significantly reduced UA duration for older workers. The reform increased the minimum eligibility age to receive unlimited-duration UA from 52 to 55. Eligibility to UA is determined by age at UI exhaustion, giving rise to idiosyncratic variation within and across workers' exposure to the reform based on the moment of entry into non-employment and UI entitlement. We exploit rich administrative data on complete work histories to causally identify the effects of the reform in a difference-in-differences framework, where we compare outcomes of affected and unaffected workers based on their age at UI exhaustion (ages 52-55 and 55-58, respectively), before and after the reform.

The reform was well enforced and induced significant labor supply responses. Affected workers became 9pp (12%) more likely to find new jobs and exhibited significantly shorter non-employment durations (32%, about 140 days). Given an average reduction of Potential

Benefit Duration (PBD) of 36 months, this effect corresponds to a decrease of 0.13 months spent in non-employment for each additional month of benefit reduction. Our estimates are close in magnitudes to the ones previously obtained for short-term UI benefit changes (Johnston and Mas 2018, Schmieder et al. 2016). At the same time, in line with previous research on program substitution (Borghans et al. 2014, Inderbitzin et al. 2016), there was also a sizeable increase in transitions to other, less generous, UA programs (+18pp, doubling relative to the pre-treatment mean), suggesting that UA benefits still play an essential consumption-smoothing role. Finally, while the reform successfully brought some individuals back to work, it also pushed others towards non-participation and welfare programs (+4pp, 40%).

We next investigate the reform's impact on subsequent re-employment wages and job quality. The direction of the effect is theoretically ambiguous. Benefit cuts reduce workers' outside options, thus lowering their reservation wage. On the other hand, if benefit cuts induce workers to leave unemployment earlier, this might reduce skill depreciation and stigma and improve future job quality (Schmieder et al. 2016). Despite the significant decrease in non-employment duration, we find that affected workers who became re-employed experienced larger drops in re-employment wages (8%). In terms of marginal effects, this corresponds to a wage drop of 0.25% for each month of reduction in UA benefit duration, a magnitude in line with the results from Nekoei and Weber (2017) for UI benefits' expansions, but in contrast with much of the other literature on UI, which typically finds little or no effects on re-employment outcomes (Card et al. 2007, Lalive 2008, Schmieder et al. 2016, van Ours and Vodopivec 2008). Wage effects are unlikely to be driven by adverse dynamic selection, as we do not observe any reform-induced change in the observable characteristics of workers exiting at different moments in their non-employment spell. We do not find significant effects in other dimensions of job quality.

Workers with different UI entitlements and ages responded along different margins. Those with little or no UI and younger (closer to age 52) were more likely to find new employment

(up to 17pp) in lower-paid jobs. Those with more UI and older (closer to age 55) became more likely to leave the labor force or go on welfare (up to 8pp).

Exploiting the cross- and within-cohort heterogeneity in UI entitlement, we investigate workers' behavior along the non-employment spell by estimating flexible hazard models and non-parametric difference-in-differences. We detect a clear spike in workers' exits at UI exhaustion and afterward, but no significant differences in the preceding months. This is in contrast with results in Le Barbanchon et al. (2019), Schmieder et al. (2012b) Schmieder et al. (2016), Johnston and Mas (2018), who all find evidence of forward-looking behavior. The absence of forward-looking responses in our context may be driven by a combination of a "surprise effect" (many of the workers were already in UI) and unobserved heterogeneity (UA benefit cuts affect workers who are likely to exhaust benefits anyway).

We estimate that the reform induced fiscal savings in the order of €11,200 per affected individual. With approximately 52,000 affected individuals, this translated into 580 million euros (MEUR) in savings, accounting for 12% of the fiscal adjustment the European Commission required for Spain at the time of the reform. However, we argue that a significant fraction of this amount was an accounting artifact product of the specificities of the (pay-as-you-go) pension system. The actual figure was probably closer to 140 MEUR.

To the best of our knowledge, we are the first to provide causal estimates of the impact of pro-cyclical changes in long-term unemployment benefits during the Great Recession. Most existing papers in the European or US context focus on counter-cyclical expansions to short-term UI benefits and usually find positive and moderate effects (Card et al. 2007, Farber et al. 2015, Kroft et al. 2013, Lalive 2008, Lalive et al. 2006, Landais 2015, Rothstein 2011, Valletta 2014). A notable exception is Johnston and Mas (2018), which studied the effects of a (much smaller) cut in short-term UI (rather than long-term UA) benefits in Missouri in 2011. In the Spanish context, other papers have also investigated the same reform, but their focus was on other aspects such as cuts in short-term UI benefits or its effects on mental health (Bentolila et al. 2012, García Pérez and Vall Castelló 2021, Rebollo-Sanz and Rodríguez-Planas 2020).

Despite the differences in the settings, our findings on non-employment duration are largely comparable in magnitudes, pointing toward a similar shift in structural parameters.

The second key contribution of this paper is its focus on long-term unemployment assistance benefits. While two-tier unemployment insurance systems are relatively common in OECD countries, very little is known about policies targeting the second tier of benefits.[1] Despite receiving little attention, long-term benefits have a high social insurance value and are particularly important during recessions, when the share of long-term unemployed workers rises. In this sense, our work also contributes to the research on the rise of long-term-unemployment (LTU) during recessions by evaluating the role of unemployment insurance design and its interplay with macroeconomic conditions (Elsby et al. 2011, Kolsrud et al. 2018, Kroft et al. 2016; 2019, Schmieder et al. 2012a).

Finally, our work is informative about the interplay among social insurance programs, labor supply, and retirement decisions. Recent papers study the relationship between short-term UI benefits and disability insurance (DI), finding mixed results. For example, Inderbitzin et al. (2016), in the context of 1988 Austria, show how extending UI benefits generates both "program complementarity" (more DI take-up in the future) as well as "program substitution" (less DI take-up in the present). In the US, Mueller et al. (2016) find no evidence that the UI extensions during the Great Recession reduced DI applications. Another strand of work investigates how social insurance programs affect the retirement decisions of older workers (Inderbitzin et al. 2016, Kyyrä and Ollikainen 2008, Lalive 2008, Staubli and Zweimuller 2013). Their focus is primarily on short-term benefit extensions, and they typically find evidence of significant disincentive effects. We reconcile previous findings by showing that different types of workers respond along different margins when facing benefit

[1] See Schmieder and von Wachter (2016) for a review. In a concurring working paper, Price (2016) analyzes the impact of the 2005 German Hartz reform, which introduced changes in long-term benefits but did not take place during a recession and affected all unemployed workers.

cuts. We also highlight the importance of program similarity in explaining the degree of substitution across programs.

2 Institutional background

2.1 The unemployment insurance system in Spain

There are two tiers of unemployment benefits in Spain: UI and UA. They all require "actively looking for a job" and not refusing "adequate job offers" while receiving the benefits. Violation of these conditions entails potential sanctions and immediate termination of the benefits.

Unemployment insurance: A worker who loses her job and has worked for at least one year is entitled to receive between four and 24 months of UI (*prestación por desempleo*). The maximum potential benefit duration (PBD) of UI depends on the days worked in the six years before losing her job (See Table **??**). UI has a wage replacement rate (RR) of 70% during the first six months and 60% from the seventh month onward, subject to minimum and maximum amounts revised annually.[2]

Unemployment assistance: Unemployed workers who have exhausted their UI benefits can apply for a UA program (*subsidio por desempleo*) within one month and 15 days from UI exhaustion.[3] This maximum waiting time (*mes de espera*) is strictly enforced, and workers cannot strategically wait to apply. UA benefits are means-tested. The "lack of income" (*carencia de rentas*) requirement precludes access to individuals with average monthly incomes above a threshold proportional (75%) to the minimum wage. For reference, the eli-

[2]After July 2012, the RR would drop to 50% after the sixth month. Due to grandfathering (i.e., the RR would not change for workers already in UI before the reform), that policy change does not pose a threat to our identification.

[3]Workers without UI entitlement can apply for UA after job separation, provided they worked at least three months. See Table **??** for more details.

gibility threshold in 2012 was €480, or about $520. Workers in UA received approximately €430 ($460) per month in 2012. PBD varies by workers' characteristics, such as age, past employment history, and household composition. The shortest PBD is three months; the most common is six months; the maximum (for younger workers) is 30 months.[4] Workers older than 55 (52 until 2012) can claim the "subsidy for individuals older than 52/55" (*subsidio para mayores de 52/55 años*), with unlimited PBD. After exhausting their UI/other UA benefits, eligible workers can apply for this program. A worker can stay in this program until she reaches the minimum legal retirement age of 61. At that point, she will start receiving a public pension. In addition to unlimited PBD, this UA program provides workers with state-sponsored pension contributions, while other UA programs do not. Henceforth, we refer to this UA program as "unlimited UA," "52/55yo Subsidy" or, simply, "the Subsidy." We bundle together the rest of UA programs, with limited PBD and no state-sponsored pension contributions, as "Other UA."

Other social programs: Unemployed workers still have options after exhausting all possible UA benefits. These include the RAI (*Renta Activa de Inserción*, or "Active Inclusion Income") program, and region-specific welfare programs. The RAI is administered by the SEPE (*Servicio Estatal de Empleo*) – the same public institution in charge of UI and UA programs – and has the same eligibility conditions and pays the same amount (€430) as all UA programs. RAI's PBD is 11 months. Regional welfare programs are the last resort for workers having exhausted all benefits (including the RAI).[5] In our data, we cannot observe workers in the RAI or regional welfare, as the Social Security Administration does not sponsor these programs.

[4]Workers with high UI entitlement and family dependents can claim 30 months of UA. The social security administration defines having dependents, or "family responsibilities," as living in a household with a descendant below the age of 26 or with an economically dependent (i.e., with "lack of income") relative.

[5]Disability insurance (DI) is independent of workers' employment status and requires a doctor's medical evaluation.

Figure **??** summarizes the key features of the Spanish unemployment benefit system and Table **??** summarizes the different UA programs (including the RAI) in Spain.

2.2 The 2012 labor market reform

The Spanish economy was severely hit by the Great Recession, with unemployment rates reaching an unprecedented 26%. This rise was accompanied by an increase in the incidence of long-term unemployment, as shown in Figure **??**.

Re-employability of older workers became a first-order concern, as illustrated by the bottom panel in Figure **??**. Many workers relied on short-term UI benefits during the crisis (the share of the active population in UI increased from 13 to 20%), but younger (age 35-49) and older (age 50+) groups displayed very different patterns in their access to UA. Both groups used UA more intensively after 2008, but while the usage growth halted around 2010 for the younger workers, it continued until 2014 for the older workers. At that time, roughly 40% of the active population in that age group received UA benefits.

Growing budget deficits (up to 11% of the GDP) and the Eurozone instability after the Greece bailout eroded the confidence of international investors in Spain's ability to repay its debt. The European Commission (EC) urged the new government to swiftly revert the situation through labor market reforms. The new government complied and implemented a first major reform in February 2012 (today known as *reforma laboral*). That reform introduced changes to contractual arrangements geared at making the Spanish labor market more flexible and reducing the high youth unemployment. It did not touch on long-term unemployment benefits.

On July 11, 2012, due to continuing international and financial pressures, the government unexpectedly announced a new reform targeting the unemployment benefits system. This new reform increased the 52/55yo Subsidy eligibility entry age from 52 to 55. It was approved on July 13 and made effective on Sunday, July 15 (four days after the announcement). The reform was implemented with grandfathering, meaning that all existing 52/55yo Subsidy

recipients could keep their benefits under the pre-reform conditions, even when the age requirement was no longer satisfied under the new rules. We refer to this reform as "the 2012 reform" or "the reform" in the rest of the paper.

Figure 1 illustrates the change in benefits profiles following the reform and offers a hint at our identification strategy.

3 Data and research design

3.1 The *Muestra Continua de Vidas Laborales*

We use administrative data on individual work histories drawn from the *Muestra Continua de Vidas Laborales* (MCVL). The MCVL is a large matched employer-employee dataset providing employment, unemployment, disability, and retirement records for a 4% random non-stratified sample of individuals with a relationship with the Spanish Social Security Administration in a reference year.[6] Records are at the spell level, with each spell's exact start and end dates specified. In addition to standard demographic variables (e.g., gender, age, education, place of birth and residence), the MCVL provides information on earnings, the household structure (gender and age of co-habitants), and tax records. We use the MCVL for reference years 2004 through 2017. The raw data contains approximately 1.67 million individuals. Given our focus, we restrict the sample to workers born before January 1973 that experienced unemployment at least once since 2006. We keep 245,000 individuals after applying these restrictions.

3.2 Empirical strategy and identification

Difference-in-differences: We exploit the quasi-experimental variation in long-term unemployment benefits availability induced by the reform and adopt a difference-in-differences

[6]That being understood as either working and contributing to the administration, receiving UI or UA, receiving disability insurance, or receiving a public retirement pension.

(DD) design to identify the effects of long-term unemployment benefit cuts on workers' labor market outcomes.

We compare workers aged 52-55 with those aged 55-58 at the time of exhausting their UI benefits. From July 15, 2012, the age eligibility requirement to access the Subsidy increased from 52 to 55. Individuals in this age range naturally define our treatment group.[7] We use slightly older individuals (55-58) as a control group, as they were always eligible to receive unlimited UA based on their age. As for the time dimension, we consider individuals whose date of potential UI exhaustion falls within a one-year window of the reform implementation date. We allocate an individual into the "before" period if she expects to exhaust UI between July 15, 2011, and July 14, 2012. This sample includes all individuals who could claim UA benefits under the pre-reform rules. Those in the "after" period expect to exhaust UI between July 15, 2012, and July 14, 2013. This sample includes all individuals who claim UA benefits under the new rules. To ensure an individual is not classified as treated and untreated at the same time, we only keep the first non-employment spell of each worker exhausting UI in this two-year window. All key assignment variables are defined relative to UI exhaustion, which is the moment that determines eligibility for a particular UA program. Our core sample contains approximately 11,500 individuals ages 52-58 who expected to exhaust UI between July 2011 and July 2013.

We estimate the model below with ordinary least squares:

$$Y_i = \beta After_i \times Treated_i + X_i^{'}\gamma + \epsilon_i \tag{1}$$

[7] In a spirit of an RDD "donut hole" design, our baseline excludes individuals within six months of the subsidy age eligibility (i.e., those ages 54.5 to 55) to account for the fact that many unemployed workers can receive six months of Other UA after regular UI benefits. Despite the gap, we say "52-55" throughout the paper for expositional reasons. The online appendix shows that results are essentially unaffected if we include these individuals in the sample or expand the donut hole.

where Y_i is an outcome for individual i (e.g., receiving the Subsidy during the non-employment spell).[8] $Treated$ is an indicator taking the value of 1 identifying individuals ages 52-55 at UI exhaustion. $After$ is an indicator identifying individuals exhausting UI after July 15, 2012. X is a vector of covariates capturing demographic characteristics of individuals (e.g., gender, education, UI entitlement) and local economic conditions (e.g., province unemployment rates at the start of the spell and expected UI exhaustion, and province fixed effects). It also includes a constant and indicators for *Treated* and *After*. Standard errors are clustered at the province-quarter level. β is the main parameter of interest, capturing the average treatment effect of the reform.

Identification requires that treatment and control groups' evolution in outcomes would have behaved similarly in the absence of the reform. The parallel trends assumption is untestable, but we provide two pieces of evidence suggesting it holds.

First, treated and control workers have balanced characteristics in the pre-reform period, as shown in Panel B of Table **??**.

Second, treated and control individuals do not exhibit significant differences in pre-trends. We study pre-trends by estimating the event-study specification below:

$$Y_i = \sum_{\substack{t=2011q3 \\ t \neq 2012q2}}^{2013q2} \beta_t I(t_i = t) \times Treated_i + X_i^{'}\gamma + \epsilon_i \tag{2}$$

where I is an indicator identifying individuals exhausting UI benefits in quarter t. β_t captures the average difference in outcomes between groups in a given quarter relative to 2012q2 (the quarter before the reform). Figure 2 shows the β_t estimates for the main outcomes we study. We discuss these results and the precise definition of each outcome later in the paper. For the moment, we only want to note that there are no significant differences between groups

[8]Following previous literature (Card et al. (2007), Nekoei and Weber (2017), Schmieder et al. (2016)), we define non-employment as not having a job. A non-employment spell starts the day after a worker leaves employment. It ends the day before starting a new job.

in the four quarters preceding the reform implementation.

Hazard analysis: We complement our DD framework by estimating a flexible non-parametric hazard model. That approach does not yield causal estimates, as selection in exits out of non-employment might change throughout the spell, and we are not accounting for differential time trends or business cycle effects. However, it is a helpful exercise to visualize non-employment spells, think about search effort, and understand how and at which stage of the spell the reform had the most significant impact. We follow DellaVigna et al. (2017) and estimate hazard rates out of non-employment, separately for each age sub-group, with the linear probability model below:

$$I(t_i^* = t | t_i^* \geq t) = \beta_{0,t} + \beta_{1,t} After_i + \epsilon_{i,t} \tag{3}$$

where I is an indicator for individual i exiting non-employment in month t, conditional on being unemployed at the beginning of that month. $After$ is an indicator taking the value of one if i exhausts UI after the reform. We follow individuals for 36 months.[9]

4 Main results

4.1 Difference-in-differences

Table 1 presents the paper's main results. It shows β estimates from Equation 1 for 10 outcomes, with or without controls (in odd and even columns, respectively).

The reform was well-enforced, as it effectively reduced Subsidy take-up. Columns 1-2 in Panel A show a decrease in Subsidy take-up of approximately 26 percentage points (pp), essentially 100% relative to the pre-reform period for that group, suggesting effective

[9]This approach is significantly more demanding as it involves estimating 72 coefficients for each age subgroup (one for each of the 36 months in which we follow individuals, before and after the reform).

enforcement of the reform. Most workers who became ineligible to claim unlimited UA benefits did not immediately find jobs following UI exhaustion. They instead resorted to alternative UA programs with shorter PBD. Columns 3 and 4 show a 18pp increase in Other UA take-up, doubling the pre-treatment mean. These findings align with previous research documenting welfare program substitution (Borghans et al. 2014, Inderbitzin et al. 2016), but are substantially larger. Inderbitzin et al. (2016) show that UI extensions induced substitution away from DI of around 30%, while in our case, we find an increase close to one-for-one. That is probably related to the fact that, in our context, the two programs are very similar regarding requirements and application procedures.[10] Looking at program substitution with disability insurance (DI), we find magnitudes much more in line with those in Inderbitzin et al. (2016). Columns 1 and 2 of Table **??** show that workers exhausting benefits after the reform were 1.4pp (22%) more likely to claim a disability pension at some point after entering non-employment ($p > 0.1$). That highlights the importance of considering program similarity when assessing the degree of program substitution induced by policy changes.

Treated workers became more likely to find jobs, but also to leave the labor force or go into welfare. We say that an individual exits the labor force or goes into welfare if she "disappears" from our data for at least one year. In that case, we know the individual is not working or receiving UI, UA, or a retirement pension.[11] Columns 5-6 show that the reform effectively induced affected workers to return to work, as transitions to employment increased by 9pp (12%). Not all workers completed their non-employment spells by finding a job. Columns 7-8 show that exits out of the labor force or welfare increased by 4pp (40%). Appendix Table **??**, where we adopt a stricter definition of "leaving the labor force," still

[10]They are different in their benefit provisions. Apart from the extended duration, the 52/55yo Subsidy also provides state-sponsored pension contributions, while Other UA does not.

[11]She could still receive benefits not sponsored by the Social Security – thus, not observable in our data – such as the RAI (see Table **??**) or some regional welfare program.

shows results going in the same direction (2pp, 21%).[12] Given that eligibility conditions for the RAI program are almost identical to those from UA, it is likely that a significant fraction of workers "leaving the labor force" essentially substituted unlimited UA for RAI.

Non-employment duration of treated individuals decreased by 140 days (32%). Given an average reduction of potential long-term benefit duration of 36 months, this effect corresponds to a decrease of 0.13 months spent in non-employment for one additional month of benefit reduction. This magnitude is close, but of the opposite sign, to the ones found by Card et al. (2007) and Schmieder et al. (2016) for UI extensions and slightly smaller than in Johnston and Mas (2018). Some workers might have left the labor force or substituted UA for other programs, but the reform effectively brought a substantial fraction of them (and faster) back to work.

We find evidence of a worsening in re-employment job quality in terms of wages. We look at (daily) wages,[13] but also consider four additional dimensions of quality. These are tenure at the next job, the probability of separation within six months, finding a temporary job – that is, a job with a fixed-term contract – and changing job industries (as defined by 21 aggregate SIC codes). Re-employment wages in the treated group decreased by 8%. We do not find statistically significant differences in the other dimensions of job quality. Although not statistically significant, re-employment tenure is slightly shorter (30 days), and new jobs were 2pp more likely to be temporary. New jobs had lower wages but were not significantly worse on other dimensions.

It is difficult to argue that wage effects were entirely driven by benefit cuts. As highlighted in Nekoei and Weber (2017), the overall impact of benefit cuts on wages is a combination of

[12]There, we consider exits out of the labor force cases in which individuals do not work and do not receive UI/UA for more than one year after exhausting benefits *and* do not find a job within 50 months (at the censoring date).

[13]Similar to Nekoei and Weber (2017), we construct daily wages by looking at earnings in the last month of the previous job and the first month of the next job. We measure changes in daily wages between jobs by computing the log difference of both amounts.

two countervailing forces. On the one hand, the cut in potential long-term benefit duration reduces the value of the outside option for affected workers and disincentivizes them to wait for better matches (Marimon and Zilibotti 1999). It also lowers their bargaining power, so employers might be able to capture a higher share of the match (Cahuc et al. 2006, Jäger et al. 2020, Korenok and Munro 2021). These should negatively impact their reservation wages and, consequently, re-employment wages. On the other hand, a shorter benefit duration induces workers to search more intensively, potentially reducing their non-employment duration. In the presence of duration dependence,[14] this implies that workers would be drawng from potentially better job-offer distributions, with a positive effect on re-employment wages. Ultimately, the impact on re-employment wages is ambiguous.

We follow Lindner and Reizer (2020), Nekoei and Weber (2017) and compare our estimates to previous research in Figure **??**. To make magnitudes comparable, we re-scale all estimates in terms of marginal effects and plot the results for non-employment duration on the horizontal axis and re-employment wages on the vertical axis. Our results align with what Nekoei and Weber (2017) find for short-term UI extensions in Austria, while contrasting with those from Schmieder et al. (2016), which report negative wage effects for UI extensions in Germany. Section 5 focuses on analyzing the mechanisms driving the average effects presented in Table 1. Our analysis suggests that effects are most consistent with a downward shift in reservation wages due to a significant worsening in treated individuals' outside options.

[14]Duration dependence might be induced by multiple factors, such as skill depreciation (Dinerstein et al. 2020), stigma or duration-based employer discrimination (Kroft et al. 2016), behavioral job search (DellaVigna and Paserman 2005, DellaVigna et al. 2017, Paserman 2008), and changing job composition (Nekoei and Weber 2017).

4.2 Hazards

We estimate the non-parametric hazard model from Equation 3. Figure 3 shows the results. Estimated hazard rates are depicted by blue and red solid lines for the pre and post-reform periods, respectively. Vertical red lines represent significant increases in the hazards in a given month. Dashed green lines represent significant decreases.

The top plots show transitions to the 52/55yo Subsidy and Other UA and confirm the findings in Columns 1-4 from Table 1. Transitions to the 52/55yo Subsidy sharply decreased throughout the non-employment spell in the treated group, coupled with a sharp increase in the number of individuals transitioning to Other UA. The largest spikes are in months zero and 24, coinciding with the two most frequent months in which workers in our sample exhaust benefits (Figure **??**).

Hazards to employment are high in the first months of the spell and steadily decrease after. They rise again at UI exhaustion. The bottom-left panel shows that a significant fraction of workers stayed in non-employment for one month or less, as suggested by the peak in month one (hazard of 0.1). After that, hazards to employment steadily decline to only rise again at specific points in the spell – most notably, month 24. The declining shape of the hazard is consistent with negative duration dependence (Kroft et al. 2013). The spike at month 24 is apparent, particularly in the older age group (hazard of 0.1), and reflects an increase in search intensity or a drop in reservation wages at the exhaustion point (or both). It is consistent with models of reference-dependent job search (DellaVigna et al. 2017; 2022) or storable offers (Boone and van Ours 2012).

All workers groups reduced their hazards to employment during the spell's first months. The treated group experienced increases in the hazards around month 11 (+3pp). Hazards out of the labor force also increased in both groups (but more in the treatment group) after the reform. The most significant spike is again around month 11 (+8pp in the treated group). Observed effects in the control group could also reflect the business cycle.

5 Mechanisms

5.1 Heterogeneity by UI entitlement

Combined UI and UA PBD varies widely in our sample (as Figures **??** and **??** show). Given that UA PBD largely depends on previous UI entitlement, we expect average treatment effects to vary significantly on that margin. We study this heterogeneity in Table 2, where we replicate our DD analysis for different subsamples based on pre-determined covariates. On the UI dimension, we separately study individuals with 0 (*No UI*), 1-23 (*Some UI*), and 24 (*Full UI)* months of UI.

Workers with little or (especially) no UI were responsible for most of the effects documented in Table 1. That is true for almost all the outcomes we study. Table 2 shows that, relative to the control group, Subsidy take-up for workers without UI decreased by 50pp. Other UA take-up increased by 34pp. Exits to employment increased by 17pp, and exits out of the labor force and to welfare increased by 1pp ($p > 0.1$). Non-employment duration decreased by 300 days. The *Some UI* group experienced smaller effects going in the same direction. That is also true in the *Full UI* group, which saw the smallest reduction in duration (-80 days). The bottom panel shows that wage effects were primarily driven by workers with no UI (17% reduction, $p < 0.01$). Wages from workers with Some or Full UI decreased by up to 6% ($p > 0.1$).

Wage effects are most consistent with a change in reservation wages. Workers with little or no UI also had significantly fewer months of UA (from a baseline of unlimited). That represented a significant worsening in their outside options that should have impacted reservation wages. Given that these workers also spent significantly less time in non-employment, duration dependence cannot be the mechanism driving wage effects – they should go in the opposite direction (Schmieder et al. 2016).

Comparing individuals with the same amount of UI benefits allows us to understand the reform's effect on the shape of the hazards more cleanly. We thus replicate our hazard

analysis for the subsamples of workers with 24 (Full UI) or 0 (No UI) months of UI. We look at exits to employment and out of the labor force or welfare. Figure **??** shows the results.

A large fraction of workers only found jobs after exhausting UI benefits. As documented in other settings (e.g., Boone and van Ours 2012, Ganong and Noel 2019), That pattern was stronger in the older cohorts. See, for example, the spike in the hazards in the 24th month (0.16 in the younger sample and 0.3 in the older). Consistent with results from Table 2, workers with full UI only saw mild increases in exits to employment relative to the pre-period. These effects were concentrated around the 30th month (coinciding with the end of the six months of Other UA that many workers can claim). The subsamples of workers without UI in the treated and control groups followed the same pattern, exhibiting lower probabilities of finding a job in the first months of the spell. They both experienced a significant increase in exits around the 11th month. That effect was larger in the treated group.

Transitions to non-participation and welfare coincided with the termination of contributive UI benefits. These were exacerbated in the treated group after the reform, who experienced significant increases in the hazards after the 30th month. For those without UI, increases in exits out of the labor force coincided with the 11th month of non-employment. There were significant increases in that specific month after the reform. No UA program has a PBD of 11 months (Table **??**), while the RAI does. Thus, these could reflect transitions from RAI.

5.2 Heterogeneity by time of UI exhaustion

We go back to Figure 2 to study heterogeneity based on the time of UI exhaustion.

There is a clear break in trends after 2012q2. Consistently with our baseline results in Table 1, the break is most apparent in outcomes describing non-employment spell characteristics. Relative to 2012q2, treated individuals experienced an immediate drop of approximately 20pp in the likelihood of receiving the Subsidy. The counterpart was a rapid increase

in Other UA take-up, of about 18pp. Coefficients are stable in the four quarters following the reform implementation. The break in the trend is also evident in exits to employment and out of the labor force, although coefficients on the former are slightly less stable. They oscillate between 0 and 8pp, taking the lowest value in 2013q2. Non-employment duration has a similar pattern, with an immediate large drop (−150 days) and some mean reversal in the final quarter (with a coefficient of −50 days).

The worsening in re-employment quality was primarily driven by workers exhausting their UI months after the reform. The drop in re-employment wages was 4% for workers exhausting UI in 2012q3. It was 10% for those exhausting UI in 2013q2. Similarly, new jobs were not significantly more likely to be temporary in the final quarters of 2012, but treated workers ended up being 10pp more likely ($p < 0.05$) to take these jobs in 2013 – with consequently shorter re-employment duration (−200 days in 2013q2). Results are consistent with individuals exhibiting little or no forward-looking behavior, as workers exhausting benefits in 2013 were the subsample with the most time to adjust to the new policy environment.

5.3 Heterogeneity by age at UI exhaustion

We expect the reform to have had a larger impact on younger cohorts (those closer to age 52) for two reasons. First, the cut in PBD was larger for them. Bridging to the Subsidy through Other UA became practically impossible for the younger cohorts. It was still a viable path for workers close to age 55. Second, younger workers are likely to be more re-employable, particularly in the presence of stigma and discrimination from the side of employers (Kroft et al. 2016). We explore heterogeneity based on age at UI exhaustion by making two comparisons. The younger comparison studies differential effects between workers ages 52-53 (younger treated) and 55-56. The older comparison is between workers ages 54-55 (older treated) and 55-56.[15] Table 2 replicates our baseline results with these two

[15]As before, age is defined at UI exhaustion. We keep our "donut" research design, so the ages in the older treatment group are 53.5-54.5.

subsamples. Figures **??** and **??** replicate the event-study analysis.

Exits out of the labor force and welfare were driven by workers close to the age 55 cutoff. Older treated workers became 8pp more likely to complete their non-employment spells following that path. The effect on the younger treated was below 5pp. Also, older workers became three times more likely to claim DI insurance, as shown in Panel B and C of Table **??**, suggesting that some of them might have tried to use DI as a path to early retirement. On the other hand, younger cohorts drove exits to employment (+10pp) and exhibited shorter non-employment durations (150 vs. 110 days). Figures **??** and **??** suggest these effects were driven by workers exhausting UI in the immediate months following the reform.

There were only minor differences between the younger and older cohorts regarding re-employment outcomes. Still, younger workers experienced a slight decrease in tenure (30 days, $p > 0.1$), while the older cohorts stayed longer in their next jobs (20 days). Wage drops were also slightly larger in the younger cohorts (6.4% vs. 5.6%), although the difference is not statistically significant.

The preceding analysis suggests that exits out of the labor force could, in fact, overwhelmingly be masked program substitution. The older cohort is close to the 55 age cutoff. Workers in that group could plausibly substitute Other UA for some regional welfare program or (most likely) RAI. Many would have been age-eligible to receive unlimited UA after exhausting benefits in these alternative programs.

5.4 Responses along the non-employment spell

We explore within-cohort heterogeneity in UI entitlement and UI exhaustion status by studying the behavior of individuals at the same point of the non-employment spell relative to their specific UI exhaustion date. As with the hazards, these results are helpful to understand at which point of the non-employment spell workers reacted, with the caveat that exiting

non-employment (or not) is an endogenous decision. We estimate the regression below

$$I(t_i^* = t | t_i^* \geq t) = \beta_t After_i \times Treated_i + X_i^{'} \gamma_t + \epsilon_{i,t} \tag{4}$$

where I is an indicator for individual i exiting non-employment in month t, relative to the month of expected UI exhaustion and conditional on being non-employed at the beginning of that month. The key difference with a standard hazard model is that t refers to the month *relative* to the point of UI exhaustion instead of the actual non-employment month. For example, the coefficient for the month $t = -23$ includes only individuals with 23 months of UI and workers with 24 months of UI that did not exit in their first month. The coefficient for $t = 0$ includes all workers that exhaust their UI that month and have not exited non-employment yet. We are interested in β_t, capturing differential effects in a given outcome for individuals completing their non-employment spells at relative time t. Figure **??** shows the results for exits to employment and out of the labor force and welfare.

Treatment effects were driven by workers exhausting their UI benefits. Except for a positive effect in month -2 for exits to employment (+3pp) and in month -7 for exits out of the labor force (+0.02), none of the other 46 coefficients is significantly different from zero. That suggests that treatment and control groups did not exhibit differential exit behavior until the moment of UI exhaustion.

Exits to employment in the treated group steadily increased from $t = 0$ until $t = 7$ and decreased afterward. Exits out of the labor force and welfare increased sharply in month 0 (+0.01, $p < 0.1$) and fluctuated around that level until month 7. Coefficients drop to zero afterward. The peak around months 6-7 coincides with the end of the additional six months of UA benefits that many could claim, thus suggesting that many waited until exhausting all their benefits (UI and UA) before leaving non-employment.

Results in Table 2 confirm that the entirety of the main effects documented in 1 were driven by UI exhaustees. Conditional on exhausting UI, treated workers were 45pp less

likely to receive the Subsidy, 37pp more likely to go into Other UA, 14pp more likely to exit employment, and 9pp more likely to leave the labor force or go to welfare. Their durations were 200 days shorter. Their re-employment wages were 20% lower, and they became 6pp more likely to end up in a temporary job.

6 Robustness

6.1 Selection into non-employment and extensive margin effects

The reform could have impacted the extensive margin as it significantly reduced the value of unemployment for workers ages 52-55. Figure **??** shows the distribution (top) and cumulative distribution (bottom) of inflows into non-employment in a one-year window before and after the reform. The CDFs seem identical, but the density plot at the top shows some minor shifts in the 52-55 range after the reform. A Kolmogorov-Smirnov test only marginally rejects the hypothesis that the two distributions are drawn from different populations ($p = 0.050$).

We check whether these small extensive margin effects drove our results by replicating our analysis while restricting the sample to workers already in non-employment on July 15, 2012. Table **??** shows the results of the exercise. Magnitudes are slightly different, but all signs and significance levels remain the same. With the restricted sample, we find that Subsidy take-up decreased by 19 (vs. 26pp). Other UA take-up increased by 14 (vs. 18pp). Non-employment duration decreased by 110 (vs. 140) days. The rest of the coefficients are not statistically different from those in Table 1. Similarities with baseline results are consistent with a lack of forward-looking behavior.

6.2 Selection into re-employment and unobservables

We first study dynamic selection throughout the non-employment spell by looking at the observable characteristics of those that leave non-employment at specific points of the spell. We estimate the following model

$$I(x_i = 1|t_i^* = t) = \beta_{0,t} + \beta_{1,t} After_i \times Treated_i + \beta_{2,t} After_i + \beta_{3,t} Treated_i + \epsilon_{i,t} \quad (5)$$

where $I(x_i = 1|t_i^* = t)$ is an indicator taking the value of 1 if individual i exits non-employment in relative month t and belongs to a specific category (e.g., is a male). We look at gender, education, the structure of the household, and contract in the last job. Figure **??** plots our $\beta_{1,t}$ estimates for these categories.

There are no substantial differences in observables among non-employment exiters. Some coefficients are significant (e.g., males appear to be more likely to exit in the 12th month after exhausting UI), but most (138 out of 144, 96%) are statistically indistinguishable from zero. Dynamic selection on observables does not seem to drive our findings on re-employment outcomes.

Secondly, we follow Oster (2017) to assess how important unobservables ought to be to fully drive our results on re-employment wages. Figure **??** shows the results.

Unobservables ought to be highly relevant to obtain a zero effect on re-employment wages. If controlling for unobservables were only to increase the regression's explanatory power (in terms of R^2) to 0.3, these should be almost ten times as important as observables to obtain a zero effect. Even in the extreme scenario where $R^2 = 1$ could be achieved, unobservables would have to be as important as observables to eliminate the wage effect.

7 Fiscal evaluation of the reform

We follow Inderbitzin et al. (2016) to quantify the fiscal impact of the reform on the government budget. The first element to consider is program substitution – flows from 52/55yo Subsidy to Other UA. The benefit level in the two cases is the same (about €430 per month), but Other UA has a lower cost as it does not involve state-sponsored pension contributions. The state chips in approximately €750 per month toward Subsidy recipients' retirement pen-

sion. Behavioral responses also affect the public balance by changing the amount of taxes paid (i.e., longer re-employment spells with similar wages imply more taxes paid). Finally, we must consider reform-induced changes in non-employment duration, which critically determine the total cost of UI and UA programs. For each individual, we calculate the total number of days spent in the Subsidy and Other UA, the pension contributions subsidized by the state, and the total amount of UI and UA payments throughout the 50 months we follow individuals. We transform UA and Subsidy days into euros to obtain comparable figures, knowing that each pays around €14.30 per day. We present our estimates in Table 3.

Total fiscal savings amount to €11,200 per treated individual. That is the combination of four aspects. First, the reform reduced the average individual 52/55yo Subsidy outlay by roughly €4,000. Second, Other UA spending increased by €1,100, as a fraction of individuals transitioned to short-PBD UA programs. That amounts to average fiscal savings in unemployment benefits of about €2,900 per affected individual. This estimate is possibly an upper bound, as we cannot measure the increase in spending derived from shifts to other programs (e.g., RAI). Panel B shows that tax revenue decreased by €200 ($p > 0.1$), primarily due to lower re-employment wages. It also shows that most savings, €8,500, came from the state ceasing to contribute to workers' pensions (a unique feature of the Subsidy). Panel C adds up the previous components to obtain a savings estimate of approximately €11,200 per person.

The reform saved the state approximately 580 million euros (MEUR), or about 12% of the EC target for Spain. The reform was motivated by the necessity to cut government spending and comply with the fiscal consolidation efforts required by the EC during the sovereign debt crisis. Their target was 5,000 MEUR,[16] which we use as a benchmark to get a better sense of magnitudes. According to the Ministerio de Empleo y Seguridad Social (2013) and Ministerio de Empleo y Seguridad Social (2014), flows into the 52/55yo Subsidy decreased from 60,000 in 2012 to 8,000 in 2013. Conservatively assuming the flow change

[16]See https://elpais.com/economia/2012/03/12/actualidad/1331589735˙571017.html.

was entirely due to the reform and that affected individuals did not eventually "bridge" into the subsidy, we can back up 52,000 as the approximate number of individuals affected by the reform (in the short-run). Our previous estimates thus translate into 150 MEUR from direct savings in UA payments and over 440 MEUR in pension contributions savings. Taking taxes into account (10 MEUR) yields total savings of approximately 580 MEUR or about 12% of the EC target.

To an extent, savings in pension contributions were an accounting artifact. The previous savings estimate has two important caveats. First, in a pay-as-you-go pension system (such as Spain's), current pension payments are funded with current contributions. Therefore, the substantial contribution reduction we document translated into fewer funds (i.e., larger deficits) to pay pensions by the Social Security Administration (SSA). The SSA is an independent institution, but its deficit is ultimately financed through taxes or debt. Second, long-run savings in pensions are unlikely to be substantial. Due to the formula used to calculate public pensions, it is not necessarily the case that workers spending more time in Other UA will receive lower pensions.[17] While the reform saved the government approximately 580 MEUR in an accounting sense, the actual figure was likely closer to 140 MEUR.

While a full welfare evaluation is beyond the scope of this paper, one could think about evaluating the welfare cost of the policy by comparing a dollar cut of benefit for our treated group with a dollar cut of benefit for a younger group. Two key parameters matter to estimate and compare the marginal value of public funds of the two policies (Hendren and Sprung-Keyser 2020). On the one hand, one needs to consider the behavioral response of younger workers to benefit cuts (i.e., the marginal impact of benefit changes on non-

[17]The formula (in 2012) considered workers' contributions in the 15 years before retirement. Suppose a worker has €0 of contributions in a given month (because she is in Other UA). The formula replaces that zero with the legal minimum contribution – the same amount the state chips in for those in the Subsidy. Because of that, the final pension amount will not be significantly different.

employment duration). On the other hand, we need to know the willingness to pay for an extra dollar of benefits in the two groups of workers. Assuming that both groups of unemployed workers are liquidity constrained (otherwise, they would not qualify for long-term UA benefits), it is reasonable to assume that older workers would value an extra dollar of benefits more. They are more likely to be prolongedly out of employment, and they are also more likely to suffer from other (e.g., health) shocks. Turning on the second piece of information, we can use estimates from the literature to evaluate the expected behavioral response of younger workers to benefit cuts. For example, Schmieder et al. (2012a) estimate the causal effects of extensions in unemployment insurance duration for workers who are 49 years old. They estimate a 0.20-month increase in non-employment duration as a response to a one-month benefit extension. In comparison, our estimates imply a marginal effect of 0.13 months reduction in non-employment duration. Thus, even assuming that younger and older workers value an extra dollar of benefits by the same amount, behavioral responses suggest that the marginal value of public funds would be slightly higher if one would cut benefits for younger workers.

8 Conclusions

We assessed the impact of pro-cyclical long-term unemployment benefit cuts on labor market transitions and re-employment outcomes exploiting variation induced by an unanticipated labor market reform in 2012 Spain.

We found that the reform was well enforced and induced significant increases in re-employment (9pp) and reductions in non-employment duration (140 days). However, we also documented sizable program substitution, as workers became 18pp more likely to enroll in shorter duration UA programs and 4pp more likely to exit the labor force or go into welfare. Older workers drove the latter effect. The subsample of workers who found jobs saw an 8% reduction in wages, an effect primarily driven by workers with little or no UI

who exhausted their benefits. Finally, we estimated that the reform induced fiscal savings of approximately 140 MEUR.

Our results suggest that cutting long-term benefit duration effectively brings workers (that have not spent too much time in non-employment) back into the labor force. Cuts appear less effective for older long-term unemployed workers. These workers are particularly prone to exhaust all the benefits available to them – for example, as suggested by our hazard analysis – and may be trying to delay their re-entry to the labor force as much as possible (potentially *sine die*).

How to bring these workers back in without pushing them out? According to Bentolila et al. (2017), it is crucial to combine changes in benefit durations with effective active labor market programs (ALMP) to "make them more re-employable." Unfortunately, the literature on ALMPs only offers mixed evidence on their effectiveness (Card et al. 2010, Crépon and Van Den Berg 2016); it stresses the importance of well-designed and well-targeted programs (Boone and Van Ours 2004, Caliendo et al. 2011). Nevertheless, without those, older workers may have too many incentives to find their way out to early retirement, particularly in times of crisis.

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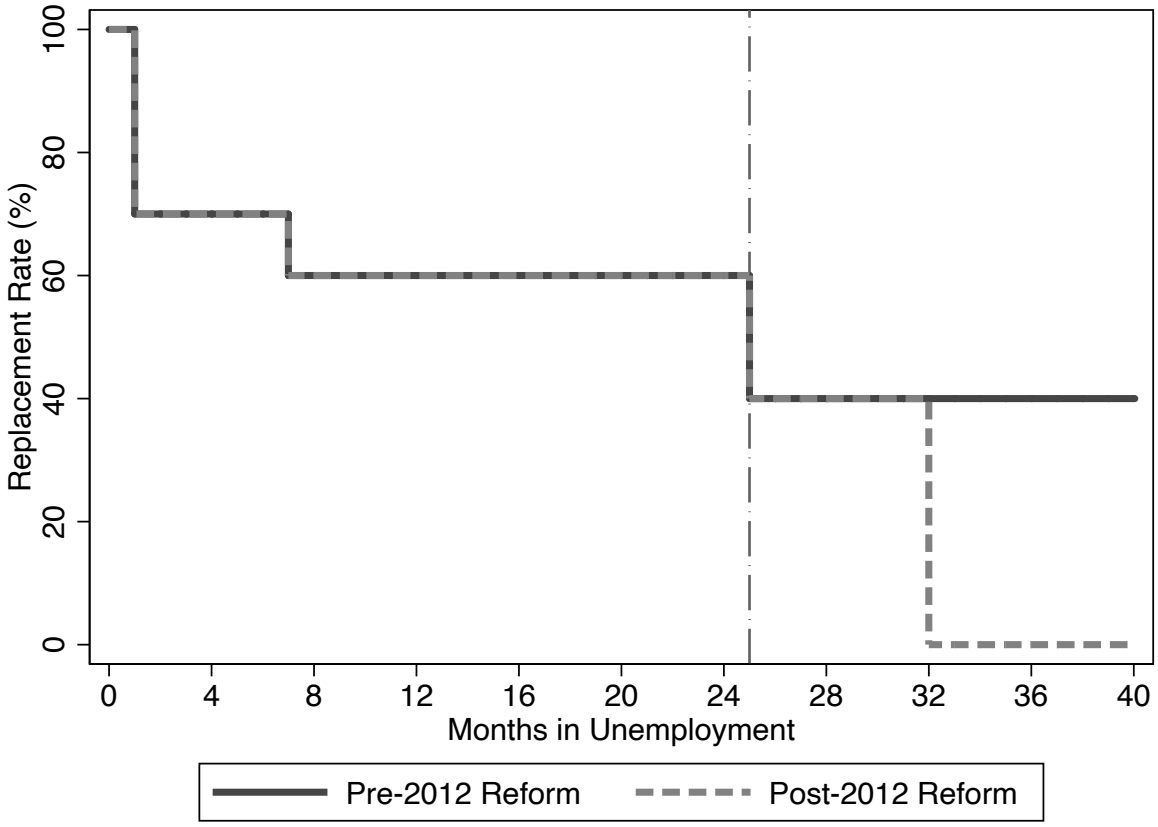
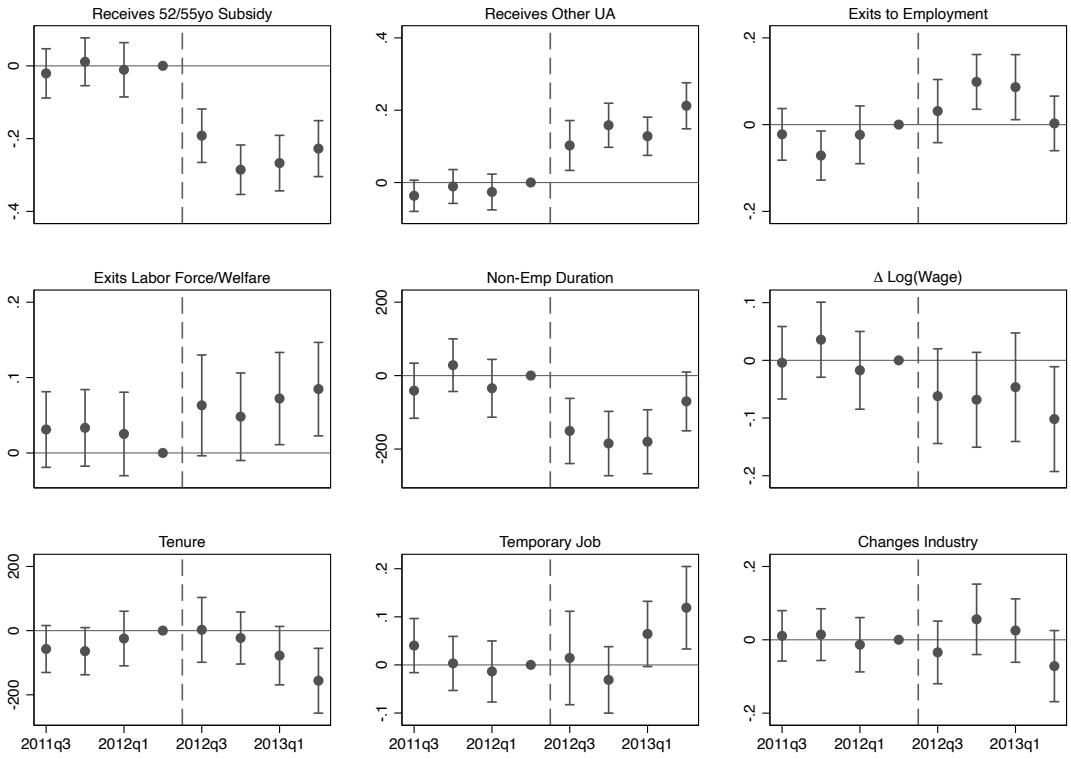
Figure 1: Benefit profiles before and after the reform



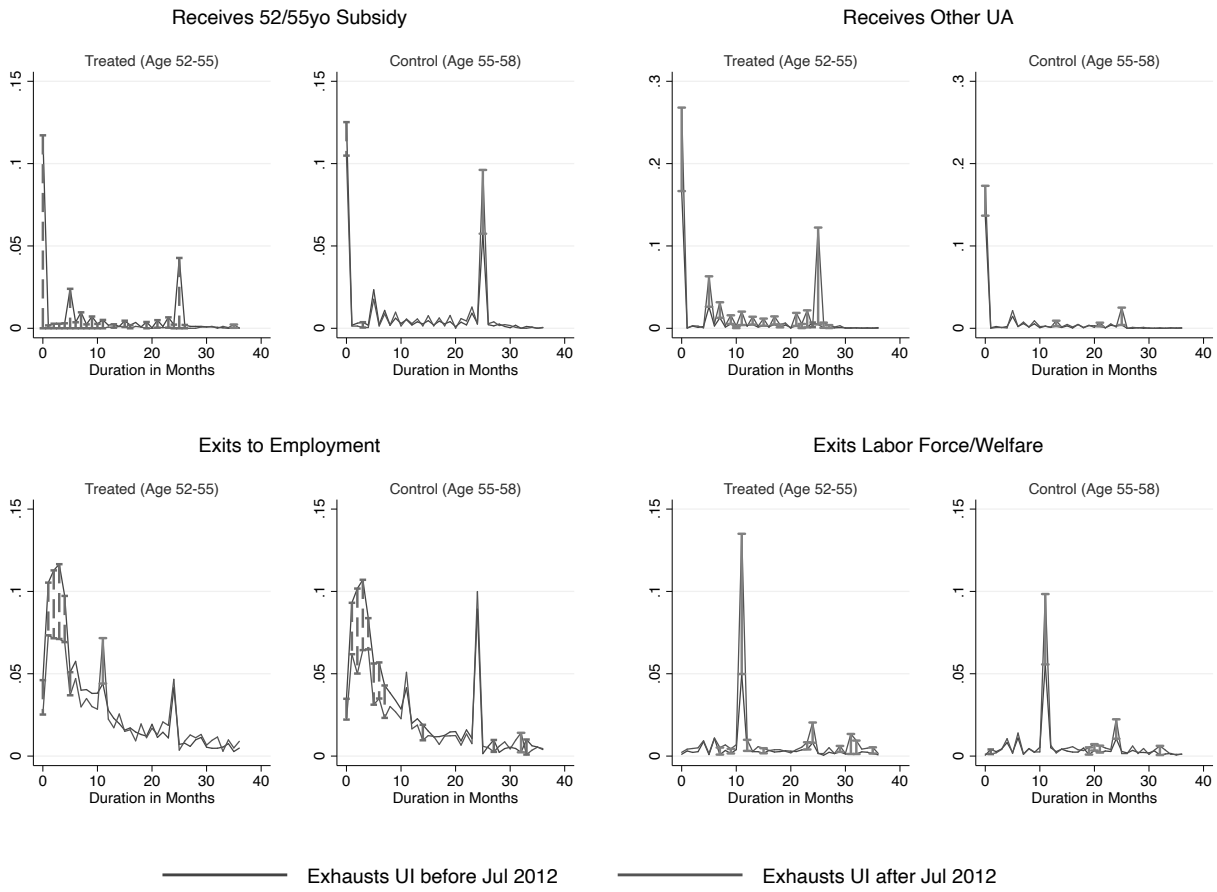
Notes: This figure illustrates the benefits profiles for a hypothetical individual exhausting UI before (blue line) or after (red dashed line) the reform. She earns €1,000 per month and has no family responsibilities. She has 24 months of UI and is in the age range 52-55 at UI exhaustion. Before the reform, she would receive €700 per month during the first six months of UI (replacement rate of 70%). From the seventh month onward, she would receive €600 per month (replacement rate of 60%). After 24 months in UI and waiting one month (*mes de espera*), she would be eligible to receive the 52/55yo Subsidy, with unlimited PBD, based on her age at UI exhaustion. The vertical red dashed line indicates the transition month from UI to UA. The benefit amount is constant at about €430 per month. After the reform, she would only be eligible to receive Other UA for six months following UI exhaustion. Other UA also pays €430 per month. After Other UA, she would not be eligible to receive additional UA benefits unless she had reached the age of 55.

Figure 2: Pre-trends and event study analysis



Notes: Each coefficient corresponds to a β_t estimate from equation 2. Vertical lines identify 95% confidence intervals. Treated individuals are those aged 52-55 at UI exhaustion. Control individuals are those aged 55-58 at UI exhaustion. *Receives 52/55yo Sub* and *Receives Other UA* are indicators taking the value of 1 if the individual benefited from the specific UA program during the non-employment spell. *Exits to Employment* is an indicator taking the value of 1 if the non-employment spell ends with the individual finding a job. *Exits Labor Force/Welfare* is an indicator taking the value of 1 if the individual stops receiving UI or UA and does not find a job within one year after benefits termination. She could be receiving RAI or other welfare. *Non-Emp Duration* is the duration (in days) of the non-employment spell. Δ*Log(Wage)* is the change in (log) wage from the individual's last job. *Tenure* is the next job's duration (in days). *Separation in 6mo*, *Temporary Job* and *Changes Industry* are indicators taking the value of 1 if *Tenure* is below 180 days, individual's next job has a temporary contract or is in a different industry, respectively. Controls include gender, family (at least one cohabitant), high school, urban, UI entitlement, tenure and log wage in the last job, province unemployment rate, variation in the unemployment rate at the start of the spell and UI exhaustion, and province fixed effects.

Figure 3: The effects of benefit cuts on non-employment spells — hazard analysis



Notes: Hazard rate estimates to the 52/55yo Subsidy, Other UA, employment, and out of the labor force throughout the non-employment spell. Hazards are estimated non-parametrically using all individuals exhausting UI within one year of the reform, separately by age groups. The ticker line represents hazards in the pre-period. The lighter line represents those in the post-period. Dashed vertical bars indicate significant (95% level) changes in the hazards at a given month.

Table 1: The effects of benefit cuts on non-employment spells and re-employment outcomes

Panel A. Characteristics of the non-employment spell

	Receives 52/55yo Sub		Receives Other UA		Exits to Employment		Exits Labor Force/Welfare		Non-empl Duration	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Age 52-55 × After	-0.266***	-0.261***	0.173***	0.182***	0.087***	0.091***	0.045***	0.044***	-137.023***	-142.482***
	(0.017)	(0.016)	(0.017)	(0.017)	(0.018)	(0.017)	(0.014)	(0.014)	(19.978)	(19.244)
DV Mean (Pre-T)	0.254	0.254	0.191	0.191	0.751	0.751	0.104	0.104	445.835	445.835
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
R-sq	0.056	0.156	0.052	0.164	0.010	0.090	0.013	0.068	0.014	0.119
Observations	11321	11321	11321	11321	11321	11321	11321	11321	11321	11321

Panel B. Re-employment outcomes

	Δ Log(Wage)		Tenure		Separation in 6mo		Temporary Job		Change Industry	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Age 52-55 × After	-0.029	-0.083***	-33.402	-28.014	0.017	0.028	0.029	0.018	-0.015	-0.010
	(0.023)	(0.021)	(22.822)	(21.037)	(0.022)	(0.021)	(0.022)	(0.019)	(0.024)	(0.023)
DV Mean (Pre-T)	0.028	0.028	254.546	254.546	0.577	0.577	0.837	0.837	0.282	0.282
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
R-sq	0.000	0.248	0.007	0.144	0.000	0.121	0.011	0.237	0.010	0.121
Observations	7506	7506	7506	7506	7506	7506	7506	7506	7506	7506

Notes: Treated (Control) individuals are those aged 52-55 (55-58) at UI exhaustion. *Receives 52/55yo Sub* and *Receives Other UA* are indicator equal to 1 if the individual benefited from the specific UA program. *Exits to Employment* and *Exits Labor Force/Welfare* are indicators equal to 1 if the non-employment spell ends with, respectively, the individual finding a job or not finding a job within one year after benefits' exhaustion. *Non-emp Duration* is the duration (in days) of the non-employment spell. Δ*Log(Wage)* is the change in (log) wage from the individual's last job. *Tenure* is the next job's duration (in days). *Separation in 6mo*, *Temporary Job* and *Changes Industry* are indicators equal to 1 if *Tenure* is below 180 days, individual's next job has a temporary contract or is in a different industry, respectively. Controls include gender, family, high school, urban, UI entitlement, tenure and log wage in the last job, province unemployment rate, variation in the unemployment rate at the start of the spell and UI exhaustion, and province fixed effects. Standard errors clustered at the province-quarter level in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 2: Heterogeneity analysis

	Receives 52/55yo Sub		Receives Other UA		Exits to Employment		Exits LF/Welfare		Non-emp Duration	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Baseline	-0.266***	-0.261***	0.173***	0.182***	0.087***	0.091***	0.045***	0.044***	-137.023***	-142.482***
	(0.017)	(0.016)	(0.017)	(0.017)	(0.018)	(0.017)	(0.014)	(0.014)	(19.978)	(19.244)
UI entitlement										
No UI	-0.530***	-0.508***	0.375***	0.340***	0.157***	0.174***	0.027	0.007	-285.082***	-300.034***
	(0.036)	(0.034)	(0.041)	(0.042)	(0.039)	(0.038)	(0.033)	(0.032)	(42.843)	(43.096)
Some UI	-0.193***	-0.194***	0.160***	0.154***	0.081***	0.079***	0.023	0.024	-111.775***	-111.755***
	(0.021)	(0.021)	(0.023)	(0.024)	(0.025)	(0.024)	(0.021)	(0.020)	(28.604)	(27.220)
Full UI	-0.221***	-0.233***	0.187***	0.185***	0.072**	0.083**	0.065***	0.069***	-73.940*	-82.286**
	(0.028)	(0.028)	(0.022)	(0.021)	(0.035)	(0.034)	(0.025)	(0.025)	(40.285)	(39.429)
Age										
52 vs 55	-0.276***	-0.281***	0.164***	0.171***	0.101***	0.102***	0.049**	0.052**	-140.337***	-148.614***
	(0.025)	(0.024)	(0.027)	(0.026)	(0.028)	(0.027)	(0.021)	(0.020)	(30.728)	(30.176)
54 vs 55	-0.258***	-0.258***	0.181***	0.204***	0.059**	0.057**	0.082***	0.085***	-115.739***	-109.745***
	(0.027)	(0.025)	(0.025)	(0.024)	(0.029)	(0.028)	(0.023)	(0.023)	(33.058)	(31.719)
UI exhaustion										
Does not exhaust UI	NA	NA	NA	NA	-0.003	-0.010	0.003	0.010	2.030	8.509
					(0.018)	(0.019)	(0.018)	(0.019)	(9.388)	(8.724)
Exhausts UI	-0.428***	-0.446***	0.372***	0.374***	0.101***	0.142***	0.101***	0.086***	-159.851***	-206.570***
	(0.026)	(0.023)	(0.022)	(0.023)	(0.025)	(0.025)	(0.020)	(0.020)	(26.325)	(22.907)
	Δ Log(Wage)		Tenure		Separation in 6mo		Temporary Job		Changes Industry	
Baseline	-0.029	-0.083***	-33.402	-28.014	0.017	0.028	0.029	0.018	-0.015	-0.010
	(0.023)	(0.021)	(22.822)	(21.037)	(0.022)	(0.021)	(0.022)	(0.019)	(0.024)	(0.023)
UI entitlement										
No UI	-0.090*	-0.171***	23.232	18.998	-0.098*	-0.034	0.017	-0.012	-0.021	0.012
	(0.053)	(0.047)	(32.345)	(29.667)	(0.053)	(0.049)	(0.033)	(0.032)	(0.051)	(0.049)
Some UI	0.001	-0.027	-23.831	-24.080	0.047	0.052*	-0.007	-0.006	0.031	0.029
	(0.035)	(0.033)	(27.251)	(28.391)	(0.030)	(0.032)	(0.027)	(0.026)	(0.029)	(0.031)
Full UI	-0.031	-0.060	-90.457*	-52.649	0.067	0.060	0.054	0.018	-0.018	-0.005
	(0.039)	(0.037)	(54.568)	(55.372)	(0.046)	(0.050)	(0.046)	(0.042)	(0.046)	(0.046)
Age										
52 vs 55	-0.036	-0.064*	-30.605	-29.797	0.058*	0.061*	-0.005	0.002	0.014	0.008
	(0.040)	(0.037)	(32.734)	(30.788)	(0.034)	(0.032)	(0.031)	(0.028)	(0.037)	(0.036)
54 vs 55	-0.053	-0.056	25.921	16.123	-0.006	0.019	-0.031	-0.021	0.024	0.031
	(0.041)	(0.038)	(34.790)	(35.480)	(0.037)	(0.036)	(0.034)	(0.031)	(0.042)	(0.041)
UI exhaustion										
Does not exhaust UI	0.020	-0.014	-35.581	-37.377	0.065**	0.069**	-0.000	-0.000	-0.001	-0.005
	(0.030)	(0.025)	(29.817)	(28.706)	(0.030)	(0.031)	(0.025)	(0.024)	(0.029)	(0.028)
Exhausts UI	-0.116***	-0.201***	-42.486	-23.632	-0.040	-0.023	0.078**	0.058*	-0.008	0.014
	(0.041)	(0.036)	(33.878)	(29.488)	(0.036)	(0.035)	(0.037)	(0.030)	(0.039)	(0.034)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Notes: *No UI*, *Some UI*, and *Full UI* includes individuals with 0, 1-23, or 24 months of UI, respectively. The age dimension compares individuals aged 52-53 vs. 55-56 (young comparison) or 54-55 vs. 55-56 (old comparison). (*Does not Exhaust UI*) *Exhausts UI* includes the subsample of individuals (not) exhausting their benefits. Controls as previously defined. Robust standard errors clustered at the province-quarter level in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table 3: The effects of benefit cuts on the government budget

Panel A. Spending in unemployment benefits

	Spending in 52/55yo Sub		Spending in Other UA	
	(1)	(2)	(3)	(4)
Age 52-55 × After	-4024.95***	-4010.01***	1135.59***	1135.97***
	(291.28)	(287.56)	(105.02)	(103.89)
DV Mean (Pre-T)	3708.81	3708.81	752.76	752.76
Controls	No	Yes	No	Yes
R-sq	0.037	0.092	0.046	0.107
Observations	11310	11310	11310	11310

Panel B. Pension contribution savings and taxes raised

	Pension Contributions Savings		Taxes Raised	
	(1)	(2)	(3)	(4)
Age 52-55 × After	8559.86***	8528.99***	-418.23	-231.00
	(625.20)	(617.72)	(368.58)	(352.41)
DV Mean (Pre-T)	-7858.71	-7858.71	2219.86	2219.86
Controls	No	Yes	No	Yes
R-sq	0.037	0.090	0.002	0.152
Observations	11310	11310	11310	11310

Panel C. Total effect

	Total Savings	
	(1)	(2)
Age 52-55 × After	11030.99***	11172.03***
	(1070.69)	(1031.33)
DV Mean (Pre-T)	-10100.42	-10100.42
Controls	No	Yes
R-sq	0.025	0.102
Observations	11310	11310

Notes: Estimated savings for a period of 50 months. Treated individuals are those aged 52-55 at UI exhaustion. Control individuals are those aged 55-58 at UI exhaustion. *Spending in 52/55yo Sub* and *Spending in Other UA* reflect state spending on these UA programs. *Pension Contribution Savings* reflect state pension contributions savings from lower 52/55yo Subsidy take-up. *Taxes Raised* are income taxes collected during employment spells. Amounts in 2012 euros discounted at a 2% rate reflect spending made or revenue raised in 50 months following workers' entry in non-employment. Controls include gender, family (at least one cohabitant), high school, urban, UI entitlement, tenure and log wage in the last job, province unemployment rate, variation in the unemployment rate at the start of the spell and UI exhaustion, and province fixed effects. Heteroskedasticity-robust standard errors clustered at the province-quarter level in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.