NBER WORKING PAPER SERIES

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Working Paper 30301
http://www.nber.org/papers/w30301

NATIONAL BUREAU OF ECONOMIC RESEARCH
1050 Massachusetts Avenue
Cambridge, MA 02138
July 2022

We thank Daniele Paserman and Johannes Schmieder for their helpful comments and guidance on this project and throughout our PhD. We also want to thank Samuel Bentolila, Arindrajit Dube, Ray Fisman, Rafael Lalive, Camille Landais, Kevin Lang, Vicenzo Scrutinio, Linh Tô, Han Ye, and seminar participants at Boston University, IAB, Universitat de Barcelona & IEB and EALE 2019 for helpful comments. We thank the Spanish Social Security administration for granting us data access. Franco Maldonado provided excellent research assistance. Gerard Domènech-Arumí acknowledges financial support from “la Caixa” Foundation (ID 100010434) under agreement LCF/BQ/AN14/10340020. All errors are our own. The views expressed herein are those of the authors and do not necessarily reflect the views of the National Bureau of Economic Research.

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NBER Working Paper No. 30301
July 2022
JEL No. J31,J64,J65

ABSTRACT

We exploit an unanticipated labor market reform in 2012 Spain to estimate the effects of pro-cyclical changes in long-term unemployment assistance (UA). The reform 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 re-employment wages. The reform induced moderate government savings. Our results highlight how considering the interplay with labor market conditions is crucial when designing long-term benefit schedules affecting workers close to retirement.

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1 Introduction

Countries may expand unemployment benefits during economic downturns to cushion the adverse effects of the business cycle for job losers. However, expanding benefits is costly, and benefit expansions are often debt-financed. When recessions affect highly indebted countries, expanding benefits is not always feasible. That was the case in several European countries during the Great Recession (2008-2013), where the interplay between contractions in government spending and the economic crisis frequently resulted 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 increased benefits might be particularly high during recessions, as recipients are likely to exhaust standard benefits, face credit constraints, and experience consumption drops. If labor markets are slack, increased search effort could be unsuccessful and push workers out of the labor force or toward alternative welfare programs (Borghans et al. 2014, Mueller et al. 2016). That is especially true for workers who are more likely to exhaust unemployment benefits and to be liquidity constrained, as the marginal value of benefits is higher for them.

Several recent papers have studied the effects of countercyclical changes in short-term unemployment benefits during the Great Recession (Chodorow-Reich et al. 2019, Farber et al. 2015, Johnston and Mas 2018, Kroft et al. 2013, Rothstein 2011, Schmieder and von Wachter 2016, Valletta 2014). Much less is known about the consequences of changes in long-term benefits, particularly during economic downturns.

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 (typically) means-tested transfers that unemployed individuals can apply upon exhaustion of short-term unemployment insurance (UI) benefits. We deem this change to be of particular interest as older workers are likely to respond to changes to UI or UA benefits differently for two reasons.

First, they are more likely to use welfare programs as a vehicle to exit the labor force and bridge into retirement. Second, they are more likely to respond on the extensive margin if employers prefer to fire older workers who might be less productive and have more attractive outside options (Inderbitzin et al. 2016, Kyyrä and Ollikainen 2008, Lalive 2008).

We exploit a labor market reform adopted in Spain in July 2012, at the peak of the country's recession, that significantly reduced unemployment assistance (UA) duration for older workers. The reform increased the minimum eligibility age to receive the most generous UA benefits available in the country – with unlimited duration – from 52 to 55. After its announcement, the reform was immediately implemented, giving us an ideal quasi-experimental setting in which to study the causal effects of a reduction in potential benefit duration (PBD) by exploiting the cross and within-cohort heterogeneity induced by the reform. Benefits eligibility is determined by the worker's age at UI exhaustion, giving rise to idiosyncratic variation in workers' exposure to the reform related to both the moment of entry into unemployment and the UI entitlement. Exploiting rich administrative data on the complete work histories and detailed demographics of Spanish workers, we apply a difference-in-differences design and compare changes in outcomes over time between affected and unaffected workers based on their age at UI exhaustion (52-55 and 55-58, respectively).

The reform was well enforced, as the probability of accessing unlimited UA benefits decreased by 27 percentage points (pp) for the treated group, essentially going to zero. Consistent with predictions of standard search models, cuts in benefit availability also induced significant labor supply responses, as affected unemployed workers were 9pp (11.5%) more likely to find re-employment and had a significantly shorter non-employment duration (33%, about 150 days). Given a reduction of potential long-term benefit duration of 36 months for the average affected worker, this effect corresponds to a decrease of 0.14 months spent in non-employment for each additional month of benefit reduction, a magnitude that is very close, but of the opposite sign, to the one documented in Schmieder et al. (2016) for UI extensions, and slightly below the one found by Johnston and Mas (2018). While benefit reductions increase exits to employment, we also document a sizeable increase in transitions to other (shorter) UA programs (19 pp, doubling relative to the pre-treatment mean). These findings suggest that UA benefits still play an essential consumption-smoothing role and are in line with previous literature documenting program substitution (Borghans et al. 2014, Inderbitzin et al. 2016). Finally, while the reform successfully brought some individuals back to work, it also pushed others out of the labor force and to other welfare programs. We document a significant increase of 5pp (50%) in exits toward non-participation and welfare programs.

We next investigate the reform's impact on subsequent re-employment wages and job quality. As previous research has highlighted, identification is complicated because benefit cuts may impact non-employment duration, which might in turn influence re-employment wages. Furthermore, the direction of the effect is theoretically ambiguous. On the one hand, benefit cuts reduce workers' outside options, thus inducing them to lower their reservation wage and consider accepting lower-paid jobs. On the other hand, if benefit cuts induce workers to leave unemployment earlier, this might reduce the skill depreciation and stigma usually leading to worse job opportunities (a phenomenon typically referred to as duration dependence) and could improve future job quality (Schmieder et al. 2016). Despite the significant decrease in non-employment duration, we find that affected workers who eventually became re-employed experienced a significantly larger drop 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).[1] Despite the large and significant negative wage effects, we do not detect significant changes in other aspects of

[1] A notable exception is the work by Arni et al. (2013), finding a significant reduction in post-unemployment job quality for Swiss workers who experienced a reduction in UI benefits as punishment for noncompliance with eligibility requirements.

job quality, such as tenure at the new job, the probability of changing industry, or finding a temporary job. Combined, our results on non-employment duration and re-employment wages are consistent with a post-reform reduction in workers' reservation wages and a limited role played by duration dependence and skill depreciation. Our results are unlikely to be driven by adverse dynamic selection, as we do not observe any reform-induced change in the observable characteristics of workers who exit at different moments in their non-employment spell.

Heterogeneity analyses reveal that workers of different ages respond along different margins. Relatively younger workers – those closer to the lower age cutoff (52) – are more likely to find new employment (10%), but in lower-quality jobs – with lower re-employment wages and shorter tenure. On the other hand, older workers (those closer to age 55) are more likely to leave the labor force or go on welfare (9pp).

Exploiting the richness of our data, we investigate workers' behavior along the non-employment spell by estimating flexible hazard models. We detect a clear spike in workers' exits to employment and also out of the labor force at the moment of benefit exhaustion and afterward, but no significant differences in the months preceding exhaustion. These results are confirmed by non-parametric difference-in-differences estimates of the probability of exiting employment by time relative to UI exhaustion, where we fully exploit the within-cohort heterogeneity in UI entitlement. The absence of a significant effect on exits to employment before UI exhaustion suggests no forward-looking behavior by affected workers. This is in contrast with results found for UI extensions by Le Barbanchon et al. (2019), Schmieder et al. (2012b) or Schmieder et al. (2016), and for UI reductions by 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). Our hazard analysis and the lack of evidence for forward-looking behavior are consistent with a reference-dependence model (DellaVigna et al. 2017), workers' discouragement (Kroft et al. 2013), lumpy search effort or over-optimistic beliefs (Mueller et al. 2018).

The combination of shorter non-employment duration, lower wages, and substitution toward other forms of benefits makes the reform's overall impact on the public budget ambiguous. In the last part of the paper, we quantify the reform's fiscal effects and find it induced savings in the order of €11,500 per affected individual. Given an (approximate) total of 52,000 individuals affected by the reform, this translated into 600 million euros (MEUR) in savings, accounting for 12% of the fiscal adjustment required for Spain by the European Union (EU) 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. Considering these, we estimate the actual overall fiscal savings to be closer to 140 MEUR.

This paper contributes to the several strands of the literature that collectively aim at understanding how social insurance systems affect search behavior and labor market outcomes of workers and how behavioral responses change depending on economic circumstances. We make three main contributions.

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 focus on counter-cyclical expansions to short-term UI benefits during recessions and usually find positive and moderate effects.[2] A notable exception is Johnston and Mas (2018), which studied the effects of short-term UI benefits cuts in Missouri in 2011.

[2]Several recent studies focus on UI benefit extensions, in connection with the fact that many countries extended UI benefits during the Great Recession. In the US, benefits were extended from 26 up to 99 weeks. Though the results are not all directly comparable, studies tend to find a significant negative but moderate impact of extended benefits on hazard rates to employment (Farber et al. 2015, Kroft et al. 2013, Rothstein 2011, Valletta 2014). In the European context, most studies focus on less recent reforms and tend to find limited labor supply effects of extended UI benefits (Card et al. 2007, Lalive 2008, Lalive et al. 2006, Landais 2015). In the Spanish context, some papers have investigated other aspects of the same reform, 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). In their review, Schmieder et al. (2016) report that the median estimated effect implies that for a one-month increase in UI duration, non-employment duration rises by about four days.

Our paper differs from theirs in two key dimensions. First, we study significantly larger benefit cuts (on average of 36 months in our case, in contrast to 16 weeks in Missouri); second, we focus on cuts to long-term benefits, which affect a more specific and smaller group of workers. Thus, the possibility of aggregate market-level externalities is much less of a concern in our case. Despite these differences, our findings on non-employment duration are largely comparable in magnitudes, pointing toward a similar shift in structural parameters. Consistent with their study, we also find limited evidence of moral hazard for long-term unemployed benefit exhaustees.

The second key contribution of this paper is its focus on long-term unemployment assistance (UA) benefits. While two-tier unemployment insurance systems – where long-term need-based UA benefits follow short-term tenure-based UI – are relatively common in OECD countries, very little is known about policies targeting the second tier of benefits.[3] Despite receiving little attention, long-term benefits are of great importance for at least two reasons. First, from a welfare point of view, they are more likely to have a high social insurance value, as they target workers who have eroded job prospects and are more likely to be liquidity constrained. Second, long-term benefits are essential policy tools during recessions, when workers are more likely to resort to these types of benefits as they become long-term unemployed. 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.[4] Our results highlight the high insurance value of long-term benefits programs that target particularly disadvantaged workers. In this sense, we provide further evidence complementing papers that discuss the optimal

[3]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.

[4]Some recent papers have documented the unprecedented surge of LTU during the Great Recession in the US (Elsby et al. 2011) and sought to understand the role of various factors, including duration dependence or labor force non-participation, in explaining the phenomenon (Kroft et al. 2019). Similar to previous papers, our non-parametric hazard estimates provide clear evidence of duration dependence in the job-finding rate in Spain, as we document that the probability of exiting employment declines steeply over the first few months of the non-employment spell.

design of UI systems and the importance of allowing UI to vary with labor market and individual characteristics (Kolsrud et al. 2018, Kroft et al. 2016, Schmieder et al. 2012a).

Finally, by studying changes to a program frequently used as a bridge to early retirement, our work is informative about the interplay among different types of social insurance programs, labor supply, and retirement decisions. While previous research has studied the interplay between unemployment insurance and other complementary programs, such as active labor market policies, less is known about how changes in unemployment benefits affect potential substitutes, such as cash transfers or disability insurance programs. Some recent papers study the relationship between short-term UI benefits and disability insurance (DI), finding mixed results. Inderbitzin et al. (2016) investigate the effects of UI extensions for older workers in Austria in 1988, showing how extending benefits generates both "program complementarity" (more DI take-up in the future) as well as "program substitution" (less DI take-up in the present). On the other hand, Mueller et al. (2016) investigates the relationship between UI and DI benefits in the US. Exploiting UI extensions during the Great Recession, they do not find evidence that extended UI benefits reduce DI applications. Similarly, Staubli (2011) shows that stricter DI requirements reduce DI take-up but do not translate into higher employment. Another set of recent papers has investigated 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), mostly focusing on short-term benefit *extensions* and finding evidence of significant disincentive effects.

We reconcile previous findings by showing that different types of workers respond along different margins when facing benefit cuts. Slightly younger workers, who are probably easier to re-employ, find jobs. In contrast, older ones, who might struggle more to find re-employment (especially in recessions), are pushed out of the labor force or into other welfare programs (including DI). Relative to previous papers, we highlight the importance of program similarity in explaining the degree of substitution across programs: we find evidence of a strong, almost one-for-one, program substitution between very similar UA programs, while we detect much smaller effects when looking at other programs with different – and stricter – requirements, such as DI. Overall, our analysis highlights the importance of considering the entire welfare system when considering the effects of a policy change and its budgetary impacts.

2 Institutional background

2.1 The unemployment insurance system in Spain

As in many OECD countries, the Spanish welfare system provides workers with two types of unemployment benefits: unemployment insurance (UI) and unemployment assistance (UA). Both programs 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.[5]

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 duration of UI granted depends on the days worked in the six years before losing her job.[6] In terms of benefit level, UI has a wage replacement rate of 70% during the first six months and 60% from the seventh month onward, subject to minimum and maximum caps revised annually.[7]

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.[8][9] UA benefits are means-tested. The "lack of income" (*carencia de rentas*)

[5] Although anecdotal evidence suggests that enforcement is rare.

[6] See Table A1.

[7] After 2012, the replacement rate after the sixth month dropped to 50%. Rebollo-Sanz and Rodríguez-Planas (2020) study the effects of the change in replacement rates on UI exits. Due to grandfathering (i.e., replacement rates did not change after the sixth month for workers already in UI before the reform), we do not need to worry about this affecting our results.

[8] This maximum waiting time (*mes de espera*) is strictly enforced and workers cannot strategically wait more than that.

[9] Workers without UI entitlement can apply for UA after job separation, provided they worked at least

requirement precludes access to individuals with average monthly incomes above a threshold proportional (75%) to the minimum wage. For reference, the eligibility threshold in 2012 was €480, or about $520. Workers in UA received approximately €430 ($460) per month in 2012.[10] Potential benefit duration (PBD) varies by workers' characteristics, such as age, past employment history, and household composition. The shortest PBD is three months, the most common PBD is six months. The maximum PBD for workers younger than 55 is 30 months.[11] Workers older than 55 (52 until 2012) can receive the "subsidy for individuals older than 52/55" (*subsidio para mayores de 52/55 años*), with unlimited PBD. Eligible workers can apply for this program once they have exhausted their UI/other UA benefits. A worker can stay in this program until she reaches the minimum legal retirement age (61). At that point, she will start receiving a public pension.[12] In addition to unlimited PBD, this particular UA program provides workers with state-sponsored pension contributions, while other UA programs do not.[13] Henceforth, we will refer to this UA program as "unlimited UA," "52/55yo Subsidy" or, simply, "the Subsidy." We will bundle together the rest of UA programs, with limited PBD and no state-sponsored pension contributions, under the label "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 any UA program. RAI's PBD is 11 months. Regional welfare programs are the last resort for

three months. See Table A2 for more details.

[10] The amount is adjusted annually. UA benefits paid €460 ($500) in 2022.

[11] This applies to individuals with long UI entitlement and family dependents. 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.

[12] The legal retirement age in Spain is 65, even though workers can retire at 61 (with a penalization in the pension amount).

[13] The amount corresponds to the prevailing minimum contribution, €750 in 2012. Before the reform, it paid 125% of that amount (€937 in 2012).

workers having exhausted all benefits (including the RAI).[14] We cannot observe workers in the RAI or regional welfare, as the Social Security Administration does not sponsor these programs.

Figure A1 summarizes the key features of the Spanish unemployment benefit system and Table A2 summarizes the different UA programs (including the RAI) in Spain. Figure A1 shows the distribution of average UI/UA entitlements in our sample.

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 A2.

Re-employability of older workers became a first-order concern. The bottom panel in Figure A2 illustrates this phenomenon. Many workers relied heavily 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 terms of their access to UA. Both groups used UA more frequently after 2008, but while the usage growth halted around 2010 for the younger cohorts, it continued until 2014 for the older cohorts. At that time, roughly 40% of the active population in that age group received UA benefits. Such a high share of workers receiving benefits posed a significant burden on public finances.

Growing budget deficits (up to 11% of the GDP) and the Eurozone instability after the Greece bailout episode eroded the confidence of international investors in Spain's ability to repay its debt. The European Commission (EC) urged the new government to move swiftly to revert the situation through labor market reforms. The new government complied and implemented a major reform in February 2012 (today known as *reforma laboral*) using a law-

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

decree.[15] 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 modify the design of 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 insurance (and assistance) system. This new reform increased the 52/55yo Subsidy eligibility entry age from 52 to 55. It also was adopted using a law-decree, passed on July 13, two days after its announcement. It became effective on Sunday, July 15. The reform was implemented with grandfathering, meaning that all the 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. The rest of the paper refers to the July 2012 reform as "the 2012 reform" or "the reform."

Figure 1 illustrates the change in benefits profiles following the reform and offers a hint at our identification strategy. Consider two hypothetical individuals, identical in any observable characteristic (including the date of birth), who worked all their adult lives in the same firm earning a monthly wage of €1,000. Suppose that both lose their jobs during the crisis. One of them lost her job in June 2010 (right after turning 50). The other lost her job in August 2010. They both have two years of UI, as they had worked uninterruptedly during the previous six years. During the first six months in UI, they earn 70% of their last wage (i.e., €700). Beginning with the seventh month and until UI exhaustion, the replacement rate drops to 60% (i.e., €600). If they cannot find a job before UI exhaustion, they can apply to a UA program after waiting one month (*mes de espera*). However, one of them is eligible to receive the 52/55yo Subsidy (with unlimited PBD) as she exhausts her benefits in June 2012, at age 52. The second worker exhausts her UI benefits in August 2012, when the reform is now in place. She is too young to apply for the Subsidy under the new rules.

[15]A law-decree is a law that can be effective the following day after its publication in the state official bulletin. It does not require immediate approval from Congress and Senate, the two legislative chambers of Spain.

Nonetheless, she can still apply to Other UA and receive benefits with a PBD between six and 30 months. The PBD will depend on her family responsibilities. After exhausting those, she cannot receive more UA benefits. Small differences in UI exhaustion's calendar time induced significantly different benefit profiles. Our identification will hinge on this variation.

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.[16] Information is recorded 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 detailed information on earnings obtained from social security, the household structure (gender and age of co-habitants, dependents), and tax records. We use the MCVL for reference years 2004 through 2017 and apply some sample restrictions. The raw data contains approximately 1.67 million individuals. Our research question concerns older workers who experienced non-employment, so we first restrict the sample to individuals born before January 1973 and unemployed at least once since 2006. This brings down the sample size to 245,000 individuals.

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

[16]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.[17] We use slightly older individuals (55-58) as a control group, as they were always eligible to receive unlimited UA based on their age.[18] 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. We keep the first non-employment spell of each worker exhausting UI in this two-year window.[19] Note that all key assignment variables are defined relative to UI exhaustion, which is the moment that determines eligibility to access a particular UA program. There are approximately 11,500 individuals ages 52-58 who would exhaust UI between July 2011 and July 2013. They constitute our core sample.

We thus estimate the model below with ordinary least squares:

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

where Y_i is an outcome of interest for individual i (e.g., receiving the Subsidy during the non-

[17] 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 almost identical if we include these individuals in the sample or expand the donut-hole.

[18] We do not use slightly younger individuals in the control group as they could be indirectly affected by the reform. In the pre-reform period, some of them could have bridged into the Subsidy through Other UA, so they are also indirectly treated.

[19] This restriction ensures that the same individual is not both treated and untreated in our data.

employment spell).[20] $Treated$ 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, as well as province fixed effects). It also includes a constant and indicators for *Treat* 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' changes in outcomes would have behaved similarly in the absence of the reform. The parallel trends assumption is fundamentally untestable, but we provide two pieces of evidence suggesting it holds.

First, treated and control individuals are balanced in terms of levels of a large set of observable characteristics in the pre-reform period, as it can be seen in Panel B of Table A3, which provides summary statistics for our different subsamples by treatment status, before and after the reform. Panel A shows some of the main outcomes explored in our DD setup, providing suggestive evidence of the effectiveness of the reform. For example, while in the pre-reform period the average take-up of the 52/55yo Subsidy was similar between treatment and control group (around 27%) in the post period, for the treatment group, it drops to 3%.

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

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

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

[20] 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 from the day following the last day in which the worker is observed in an employment spell, and ends the day before the start of a new employment spell.

(the last 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, for any outcome, there are no significant differences between groups in the four quarters preceding the reform implementation, supporting the counterfactual parallel trends assumption.

Hazard analysis: We complement our DD framework by estimating a flexible non-parametric hazard model.[21] Estimating hazard rates allows us to characterize and visualize the typical non-employment spell in our sample. That approach does not yield causal estimates,[22] but it is helpful to visualize non-employment spells, think about search effort, and try to 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. The variable $After$ is an indicator taking the value of one if i exhausts UI after the reform. We follow individuals for 36 months.[23]

[21]We also estimated hazards semi-parametrically and obtained similar results with a Cox proportional hazards model. The proportional hazards assumption was violated in some instances, possibly due to the reform changing UI/UA benefit profiles, thus affecting the underlying hazards.

[22]Not having exited non-employment in a given month is an outcome (i.e., selection changes throughout the spell). Also, we are not considering differential time-trends or business cycle effects.

[23]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). We do not have many individuals exiting in the latter stages of the non-employment spell. Thus, our power to capture significant differences there is limited.

4 Main results

4.1 Difference-in-differences

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

We first verify that 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 27 percentage points (pp), essentially 100% relative to the pre-reform period for that group, suggesting effective enforcement of the reform.[24] 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 PBDs. Columns 3 and 4 show a 19pp 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. This is probably related to the fact that, in our context, the two programs are very similar in terms of requirements and application procedures.[25] When we look at program substitution with disability insurance (DI), we find magnitudes that are much closer to the ones of Inderbitzin et al. (2016). Columns 1 and 2 of Table A4 show that workers exhausting benefits after the reform are 1.4pp (40%) more likely to claim a disability pension at some point after entering non-employment ($p < 0.1$). In this sense, our paper highlights the importance of considering program similarity when assessing the degree of program substitution induced by policy changes.

Treated workers become 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

[24] A small fraction of individuals in this group (see Table A3) bridged to the Subsidy through Other UA.

[25] On the other hand, 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.

she "disappears" from our data for at least one year. If that happens, we know that the individual is not working and is not receiving UI, UA, or a retirement pension.[26] 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 suggest that exits out of the labor force or to welfare became 5pp (50%) more likely.[27] We provide further evidence of the robustness of this result in Columns 3 and 4 of Appendix Table A4, where we adopt a stricter definition of "leaving the labor force."[28] Columns 3-4 suggest that, with this stricter definition, exits out of the labor force are still 3pp (30%) higher in the treated group after the reform ($p < 0.05$). Given that eligibility conditions for the RAI program are almost identical to those from UA, we believe a significant fraction of workers "leaving the labor force" are, in fact, benefiting from it.[29]

Non-employment duration of treated individuals decreased by 150 days (33%). Given an average reduction of potential long-term benefit duration of 36 months, this effect corresponds to a decrease of 0.14 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 the figures 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.

Finally, we investigate the characteristics of jobs for the subset of treated individuals

[26] This individual could still receive some other benefit not sponsored by the Social Security Administration – thus, not observable in our data – such as the RAI (see Table A2) or some regional welfare program. She could also be working in the informal market.

[27] Increases in exits to employment or out of the labor force go hand in hand with decreases in censoring – i.e., the non-employment spell is not completed after 50 months (generally because the individual is in a subsidy with unlimited PBD). Columns 3 and 5 from Table A3 provide strong suggestive evidence in favor of this channel (censoring decreases by 60% in the treated group).

[28] There, we consider exits out of the labor force cases where individuals are not observed working and not receiving UI/UA for more than one year after exhausting benefits *and* not finding a job within 50 months (at the censoring date) after entering non-employment.

[29] We thus see results from Columns 7-8 as further evidence of program substitution.

who transition back to employment, and we find evidence of a worsening in job quality. Most papers in the literature exclusively focus on wages as a proxy for job quality. Here we follow this approach by studying (daily) wages,[30] but we 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,[31] and changing job industries.[32] Our estimates show that re-employment wages in the treated group decreased by 8%. We do not find statistically significant differences in the other dimensions of job quality. However, re-employment tenure is slightly shorter (30 days), and new jobs are 2pp more likely to be under a temporary contract. New jobs have lower wages but are not significantly worse along other dimensions.

It is difficult to argue that wage effects are entirely driven by benefit cuts. As highlighted in Nekoei and Weber (2017), the overall impact of benefit cuts on wages is a combination of 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 impedes them from waiting 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, and it potentially reduces their non-employment duration. In the presence of duration dependence,[33] 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 esti-

[30]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.

[31]A temporary job is a job with a fixed-term contract.

[32]We consider 21 aggregate industries as defined by standard SIC codes.

[33]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).

mates to previous research in Figure A3. 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.

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.[34]

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 stay 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

[34]See Figure A4. As discussed in Section 2, individuals can apply to a UA program immediately after exhausting UI. If no UI entitlement is available to them but have worked on their last job for at least three months, they can apply to Other UA immediately after job separation.

rise again at specific points in the spell – most notably, month 24. The declining shape of the hazard is consistent with negative duration dependence.[35] The spike at month 24 is apparent, particularly in the older age group (hazard of 0.1). It 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).

Both treated and control groups saw a reduction in their hazards to employment in the first months of the spell. The treated group experienced increases in the hazards around month 11 (+3pp). Hazards out of the labor force also increased in treatment and control groups the year after the reform, but they increased significantly more in the former. The most significant spike is again around month 11 (+8pp in the treated group). Observed effects in the control group can reflect the business cycle.[36]

5 Mechanisms

5.1 Heterogeneity by UI entitlement

Combined UI and UA PBD varies widely in our sample (see Figures A4 and A5). Given that UA PBD largely depends on previous UI entitlement, we expect average treatment effects to vary significantly on that margin. We study heterogeneity on this dimension in Table 2, where we replicate our DD analysis for different subsamples based on pre-determined covariates.[37] On the UI dimension, we separately study individuals with 0 (*No UI*), 1-23

[35]Unobserved heterogeneity and the possibility of dynamic negative selection along the spell preclude us from giving our estimates a causal interpretation. Ideally, we would need to randomly assign different non-employment durations to workers so that duration is orthogonal to workers' unobserved characteristics. Then we could look at their re-employment probabilities and wages (as done in some experimental work, like Kroft et al. (2013). We do not have this experimental setting. Instead, we provide an empirical test for the presence of dynamic selection in Figure A11. There, we do not detect any significant changes in observable characteristics of workers along the non-employment spell, suggesting a limited role for negative dynamic selection.

[36]To back up the effects of the reform, netting out the business cycle component would require overlaying the two groups in the before and after periods (similar to our DD framework).

[37]Except for the *UI exhaustion* split. Exhausting UI is an outcome.

(*Some UI*), and 24 (*Full UI)* months of UI.

Workers with little or (especially) no UI are responsible for most of the effects documented in Table 1. That is true for almost every one of the outcomes we study. Table 2 shows that, relative to the control group, Subsidy take-up for workers without UI decreases by 50pp. Other UA increases by 37pp. Exits to employment increase by 18pp, and exits out of the labor force and to welfare by 3pp (not significant). Non-employment duration decreases by 330 days. The *Some UI* group experiences smaller effects going in the same direction. That is also true in the *Full UI* group, which sees the smallest reduction in duration (-70 days). The bottom panel shows that wage effects are primarily driven by workers with no UI (18% reduction, $p < 0.01$). Wages from workers with Some or Full UI decrease by up to 6%, but effects are not statistically distinguishable from 0.

This analysis suggests that wage effects are most consistent with a change in reservation wages. Workers with little or no UI also have significantly fewer months of UA (from a baseline of unlimited). That is a significant worsening in their outside options that should be reflected in a reduction of reservation wages. Given that they also spend significantly less time in non-employment, duration dependence cannot be the mechanism behind 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 A6 shows the results.

A large fraction of workers finds jobs only after exhausting UI benefits. The pattern is stronger in the older sample of workers. That is best illustrated by the big spike in the hazard estimates exactly at month 24 (hazard of 0.16 for the younger sample and 0.3 for the older). Similar patterns have been documented in other contexts (e.g., Boone and van Ours 2012, Ganong and Noel 2019). Consistently with results from Table 2, workers with full UI see only mild increases in exits to employment relative to the pre-period. Effects are 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 follow the same pattern and become less likely to find jobs in the first months of their spell. They both experience a significant increase in exits around the 11th month. That effect is larger in the treated group.

Exits out of the labor force and welfare also coincide with the termination of contributive UI benefits. This pattern is exacerbated in the treated group after the reform. That group also experiences significant increases in the hazards after the 30th month, again coinciding with the end of the six months of Other UA that workers meeting the income eligibility conditions have. For those without UI, increases in exits out of the labor force coincide (again) with the 11th month of non-employment. There are significant increases in that specific month after the reform.[38]

5.2 Dynamic effects by time of UI exhaustion

We previously used Figure 2 to show the lack of pre-trends on any of the outcomes analyzed as supporting evidence for the parallel trends assumption.[39] We now go back to it to study heterogeneity in treatment effects depending 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 experience an immediate drop of approximately 20pp in the likelihood of receiving the Subsidy. The counterpart is 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 seem slightly less stable. They oscillate

[38]No UA program has a PBD of 11 months (Table A2). The RAI does. The clear spike precisely on that month and the similarity in eligibility conditions between the RAI and UA make us think that these workers could be on the RAI program.

[39]The outcome *Separation in 6mo* is not included in the figure for presentation purposes and because the outcome is essentially a particular case of the variable *Tenure*.

between 0 and 8pp, with 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 last quarter (with a coefficient of −50 days).

The worsening in re-employment quality is primarily driven by workers exhausting their UI months after the reform. Re-employment wages initially drop by 4% in the first quarter following the reform and decrease to 10% in 2013. Similarly, new jobs are not significantly more likely to be temporary in the last quarters of 2012, but treated workers end up being 10pp more likely ($p < 0.05$) to take these jobs, with consequently shorter re-employment duration (−200 days in 2013q2), in 2013. Results are consistent with individuals exhibiting little or no forward-looking behavior, as workers exhausting benefits in 2013 are 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 a larger impact on younger cohorts (those closer to age 52) for at least two reasons. First, the cut in potential benefit duration is larger for them. Bridging to the Subsidy through Other UA became practically impossible for them, while for workers close to age 55 that was still a viable path. 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.[40] Table 2 replicates our baseline results with these two subsamples. Figures A7 and A8 replicate the event-study analysis.

Exits out of the labor force and welfare are driven by workers close to the age 55 cutoff. Older treated workers are 9pp more likely to complete their non-employment spells following that path. The effect on the younger treated is below 6pp. Also, older workers are three times

[40] 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.

more likely to claim DI insurance, as shown in Panel B and C of Table A4, suggesting that some of them might be trying to follow that path to early retirement. On the other hand, the younger cohorts drive exits to employment (+10pp) and have a shorter non-employment duration (150 vs. 110 days). Figures A7 and A8 suggest these effects are driven by workers exhausting UI in the immediate months following the reform.

There are only minor differences between the younger and older cohorts regarding re-employment outcomes. Still, younger workers experience a slight decrease in tenure (15 days on average, not significant), while the older cohorts stay longer in their following jobs (28 days). A similar pattern occurs in the type of contract, with younger workers 5pp more likely to take temporary jobs ($p > 0.1$).[41] Wage drops are also slightly larger in the younger cohorts (7.4% vs. 6.4%), 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 be substituting Other UA for some regional welfare program or (most likely) RAI. Many would be 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 react, with the caveat that exiting (or not) non-employment is an endogenous decision. We estimate the regression below

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

[41] Figure A7 shows larger effects for this outcome and tenure in 2013.

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.[42] The rest of the variables are defined as before. 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 A9 shows the results for exits to employment and out of the labor force and welfare.

Treatment effects come from 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 is, the treatment and control groups do not exhibit differential exit behavior until the moment of UI exhaustion.

Exits to employment in the treated group steadily increase from $t = 0$ until $t = 7$ and decrease afterward. Exits out of the labor force and welfare increase sharply in month 0 (+0.01, $p < 0.1$) and fluctuate 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 workers can claim after regular UI, thus suggesting that many of them wait 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 are driven by UI exhaustees. Conditional on exhausting UI,[43] treated workers are 46pp less likely to receive the Subsidy, 39pp more likely to go into Other UA, 13pp more likely to exit employment, and 10pp more likely to leave the labor force or go to welfare. Their durations are 215 days shorter. Their re-employment wages are 21% lower, and they are 7pp more likely to end up in a temporary job.

[42] The difference with respect to 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.

[43] Again – an outcome.

6 Robustness

6.1 Selection into non-employment and extensive margin effects

The reform could have impacted the extensive margin – i.e., inflows into non-employment – as it significantly reduced the value of unemployment for workers ages 52-55. Figure A10 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 ($p = 0.052$).

We further check whether these small extensive margin effects matter for our baseline results by replicating our analysis, but restricting the sample to workers already in non-employment on July 15, 2012. Table A5 shows the results of the exercise. The magnitudes are slightly different, but all signs and significance levels remain the same. With the restricted sample, we find that Subsidy take-up decreases by 19 (vs. 27pp). Other UA take-up increases by 14 (vs. 19pp). Non-employment duration is 114 (vs. 150 days) shorter. Re-employment wages are 7 (vs. 8%) lower. 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

Selection is a concern when discussing re-employment outcomes (e.g., wages) as the pool of individuals finding a job is not random.

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, structure of the household, and labor relation in the last job. Figure A11 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 do a bounding exercise. We follow Oster (2017) to assess how important unobservables ought to be to fully drive our results on re-employment wages (i.e., to bring down the coefficient from Table 1 to zero). Figure A12 shows the results. The x-axis shows R_{max}, the hypothetical R-squared of a regression with the full set of observables and unobservables.[44] The y-axis shows the value of δ as defined in Oster (2017), indicating the required relative importance of unobservables to bring down the wage effects to zero. Finally, the red-dashed line in the figure indicates the R^2 of our baseline regression with controls from Table 1 (Panel B, Column 2).

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.[45] Even in the extreme scenario where $R^2 = 1$ could be achieved, unobservables

[44] As Oster (2017) indicates, even when controlling for all the relevant variables, R_{max} does not necessarily go up to 1 – for instance, if some variables are observed with noise.

[45] A negative delta indicates that if observables are positively correlated with the treatment, then unobservables have to be negatively correlated with it to obtain the zero β estimate.

would have to be as important as observables to eliminate the wage effect.

7 Fiscal evaluation of the reform

The combination of shorter non-employment duration, lower wages, and substitution towards other forms of benefits makes the reform's overall impact on the public budget unclear. We follow Inderbitzin et al. (2016) to quantify the fiscal impact of the reform on the government budget.

We first quantify the impact on public finances of the behavioral effects. 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 is cheaper as it does not involve state-sponsored pension contributions. The state chips in approximately €750 per month toward 52/55yo Subsidy recipients' retirement pension. 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 52/55yo Subsidy and Other UA, the pension contributions subsidized by the state, and the total amount of UI and UA payments throughout the 50 months in which we follow individuals. We convert UA and subsidy days in amounts 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 €11,500 per treated individual. That is the combination of four aspects. First, the reform reduced the average individual 52/55yo Subsidy outlay by roughly €4,100. Second, Other UA spending increased by €1,200, 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.[46] Panel B shows that

[46] This estimate is possibly an upper bound, as we cannot measure the increase in spending derived from shifts to other programs (e.g., RAI).

tax revenue decreased by €300 ($p > 0.1$), primarily due to lower re-employment wages. It also shows that most savings, €8,800, come 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 €11,500 per person.

The reform saved the state approximately 600 million euros (MEUR), or about 12% of the European Commission target for Spain. The reform was motivated by the necessity to cut government spending and comply with the fiscal consolidation efforts required by the European Union during the sovereign debt crisis. Their target was 5,000 MEUR.[47] We use that target as a benchmark to get a better sense of the size of our estimates. 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 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 155 MEUR from direct savings in UA payments and over 455 MEUR in pension contributions savings. Taking taxes into account (15 MEUR) yields total savings of approximately 600 MEUR or about 12% of the European Commission's target.[48]

Savings in pension contributions were an accounting artifact to some extent. The previous figure is subject to 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 time in Other UA

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

[48] A caveat is that our saving estimates are for 50 months. The European Commission target was for one year. With evenly distributed savings over time, yearly savings would be 144 MEUR or 2.8% of the target.

(that do not benefit from state-sponsored contributions) receive lower pensions.[49] While in an accounting sense the reform saved the government approximately 600 MEUR, the actual figure was more likely in the order of 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-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 shocks, such as health shocks. Turning on the second piece of information, we can use estimates from previous 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 0.14 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.

[49] 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.

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 (150 days). However, we also documented sizable program substitution as workers became 19pp more likely to enroll in shorter duration UA programs and 5pp 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. Effects were primarily driven by workers with little or no UI exhausting 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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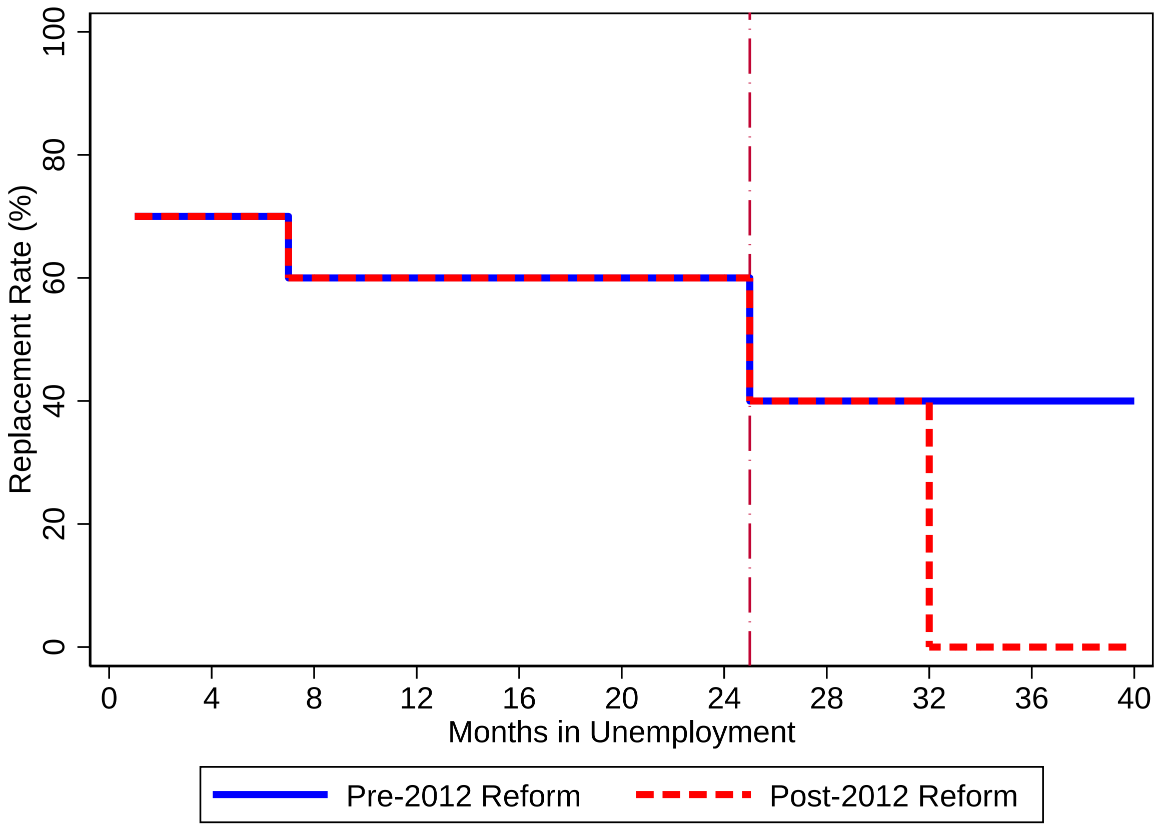


Figure 1: Benefit profiles before and after the reform

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.

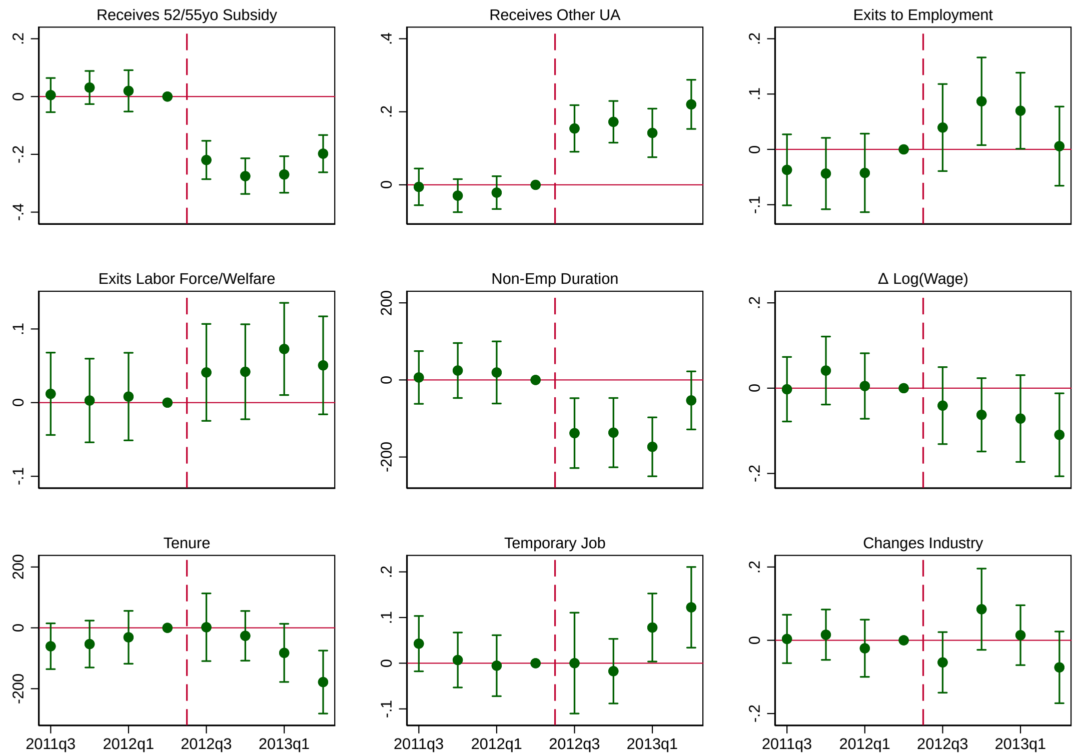


Figure 2: Pre-trends and event study analysis

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.

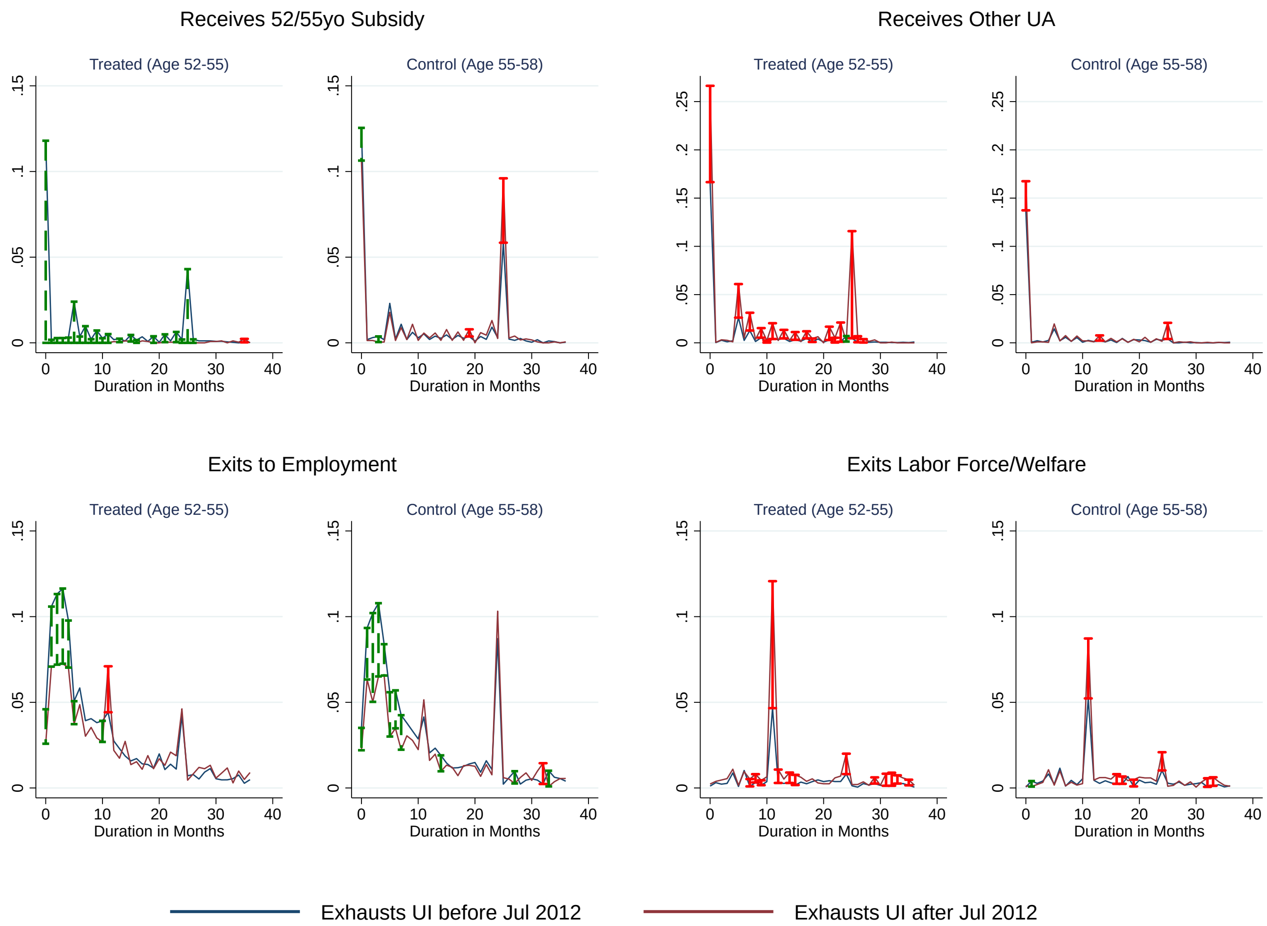


Figure 3: The effects of benefit cuts on non-employment spells — hazard analysis

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 blue line represents hazards in the pre-period. The red line represents those in the post-period. Red vertical bars indicate significant (95% level) increases in the hazards at a given month. Green dashed lines indicate significant reductions.

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	-0.278***	-0.268***	0.182***	0.191***	0.086***	0.086***	0.053***	0.053***	-149.918***	-149.771***
	(0.016)	(0.016)	(0.016)	(0.016)	(0.018)	(0.017)	(0.015)	(0.014)	(21.039)	(19.769)
DV Mean (Pre-T)	0.266	0.266	0.188	0.188	0.743	0.743	0.101	0.101	460.618	460.618
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
R-sq	0.057	0.171	0.050	0.155	0.008	0.097	0.013	0.065	0.013	0.129
Observations	11348	11348	11348	11348	11348	11348	11348	11348	11348	11348

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	-0.037	-0.083***	-33.120	-26.392	0.013	0.022	0.032	0.020	-0.021	-0.015
	(0.023)	(0.021)	(24.441)	(21.593)	(0.023)	(0.022)	(0.023)	(0.019)	(0.025)	(0.024)
DV Mean (Pre-T)	0.026	0.026	254.751	254.751	0.577	0.577	0.838	0.838	0.286	0.286
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
R-sq	0.001	0.249	0.007	0.143	0.000	0.120	0.011	0.235	0.010	0.121
Observations	7450	7450	7450	7450	7450	7450	7450	7450	7450	7450

Notes: 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. We follow individuals for 50 months after entering non-employment. Longer spells are censored. Heteroskedasticity-robust 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	-0.278***	-0.268***	0.182***	0.191***	0.086***	0.086***	0.053***	0.053***	-149.918***	-149.771***
	(0.016)	(0.016)	(0.016)	(0.016)	(0.018)	(0.017)	(0.015)	(0.014)	(21.039)	(19.769)
UI entitlement										
No UI	-0.565***	-0.536***	0.408***	0.373***	0.165***	0.175***	0.044	0.025	-324.644***	-327.894***
	(0.037)	(0.034)	(0.039)	(0.041)	(0.040)	(0.039)	(0.032)	(0.033)	(45.020)	(43.589)
Some UI	-0.200***	-0.200***	0.169***	0.162***	0.078***	0.077***	0.029	0.030	-119.163***	-119.491***
	(0.021)	(0.021)	(0.023)	(0.024)	(0.025)	(0.023)	(0.021)	(0.020)	(28.830)	(27.672)
Full UI	-0.213***	-0.223***	0.183***	0.182***	0.060*	0.067**	0.069***	0.072***	-64.552	-69.122*
	(0.029)	(0.029)	(0.021)	(0.020)	(0.033)	(0.032)	(0.025)	(0.025)	(39.860)	(38.294)
Age										
52 vs 55	-0.294***	-0.290***	0.177***	0.189***	0.112***	0.103***	0.052**	0.057***	-155.491***	-152.045***
	(0.024)	(0.024)	(0.026)	(0.026)	(0.029)	(0.028)	(0.021)	(0.020)	(32.761)	(31.031)
54 vs 55	-0.264***	-0.265***	0.199***	0.223***	0.055**	0.052*	0.087***	0.091***	-117.918***	-110.382***
	(0.028)	(0.027)	(0.025)	(0.024)	(0.028)	(0.027)	(0.022)	(0.022)	(33.230)	(31.950)
UI exhaustion										
Does not exhaust UI	NA	NA	NA	NA	-0.002	-0.008	0.002	0.008	-0.424	6.257
					(0.018)	(0.018)	(0.018)	(0.018)	(9.351)	(8.759)
Exhausts UI	-0.447***	-0.460***	0.390***	0.391***	0.098***	0.129***	0.115***	0.105***	-176.716***	-216.415***
	(0.025)	(0.023)	(0.022)	(0.022)	(0.025)	(0.025)	(0.021)	(0.022)	(28.660)	(23.088)
	Δ Log(Wage)		Tenure		Separation in 6mo		Temporary Job		Changes Industry	
Baseline	-0.037	-0.083***	-33.120	-26.392	0.013	0.022	0.032	0.020	-0.021	-0.015
	(0.023)	(0.021)	(24.441)	(21.593)	(0.023)	(0.022)	(0.023)	(0.019)	(0.025)	(0.024)
UI entitlement										
No UI	-0.114**	-0.181***	25.569	20.151	-0.108**	-0.054	0.025	0.003	-0.008	0.021
	(0.052)	(0.048)	(30.524)	(29.358)	(0.047)	(0.044)	(0.033)	(0.030)	(0.056)	(0.055)
Some UI	0.004	-0.019	-21.259	-21.798	0.042	0.046	-0.003	-0.001	0.021	0.018
	(0.035)	(0.033)	(27.590)	(29.163)	(0.032)	(0.034)	(0.027)	(0.027)	(0.030)	(0.031)
Full UI	-0.042	-0.061	-90.418	-51.743	0.068	0.060	0.053	0.015	-0.025	-0.009
	(0.040)	(0.037)	(54.892)	(55.210)	(0.047)	(0.051)	(0.047)	(0.044)	(0.046)	(0.047)
Age										
52 vs 55	-0.059	-0.074**	-20.765	-15.410	0.047	0.048	-0.016	-0.015	0.004	-0.000
	(0.039)	(0.037)	(33.737)	(31.808)	(0.037)	(0.034)	(0.032)	(0.029)	(0.039)	(0.038)
54 vs 55	-0.061	-0.064*	31.160	28.344	-0.020	0.000	-0.036	-0.032	0.018	0.032
	(0.040)	(0.039)	(37.031)	(37.792)	(0.040)	(0.040)	(0.037)	(0.035)	(0.040)	(0.039)
UI exhaustion										
Does not exhaust UI	0.021	-0.006	-34.041	-35.647	0.061**	0.063**	-0.001	-0.001	-0.005	-0.006
	(0.030)	(0.025)	(30.429)	(29.402)	(0.030)	(0.031)	(0.025)	(0.023)	(0.029)	(0.028)
Exhausts UI	-0.134***	-0.214***	-45.068	-22.637	-0.040	-0.030	0.087**	0.065**	-0.013	0.007
	(0.040)	(0.036)	(37.403)	(28.178)	(0.035)	(0.033)	(0.038)	(0.032)	(0.042)	(0.036)
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	-4226.58***	-4125.45***	1180.47***	1178.49***
	(294.69)	(290.02)	(101.49)	(99.36)
Dv Mean (Pre-T)	3958.66	3958.66	743.32	743.32
Controls	No	Yes	No	Yes
R-sq	0.038	0.109	0.046	0.105
Observations	11314	11314	11314	11314

Panel B. Pension contribution savings and taxes raised

	Pension Contributions Savings		Taxes Raised	
	(1)	(2)	(3)	(4)
Age 52-55 × After	8990.85***	8775.65***	-419.24	-262.28
	(633.72)	(623.80)	(378.22)	(354.86)
Dv Mean (Pre-T)	-8394.12	-8394.12	2200.34	2200.34
Controls	No	Yes	No	Yes
R-sq	0.037	0.108	0.003	0.151
Observations	11314	11314	11314	11314

Panel C. Total effect

	Total Savings	
	(1)	(2)
Age 52-55 × After	11617.72***	11460.33***
	(1128.24)	(1086.73)
Dv Mean (Pre-T)	-10895.77	-10895.77
Controls	No	Yes
R-sq	0.026	0.120
Observations	11314	11314

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. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

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

Gerard Domènech-Arumí and Silvia Vannutelli

A Additional figures and tables

This appendix includes figures and tables complementing the main text.

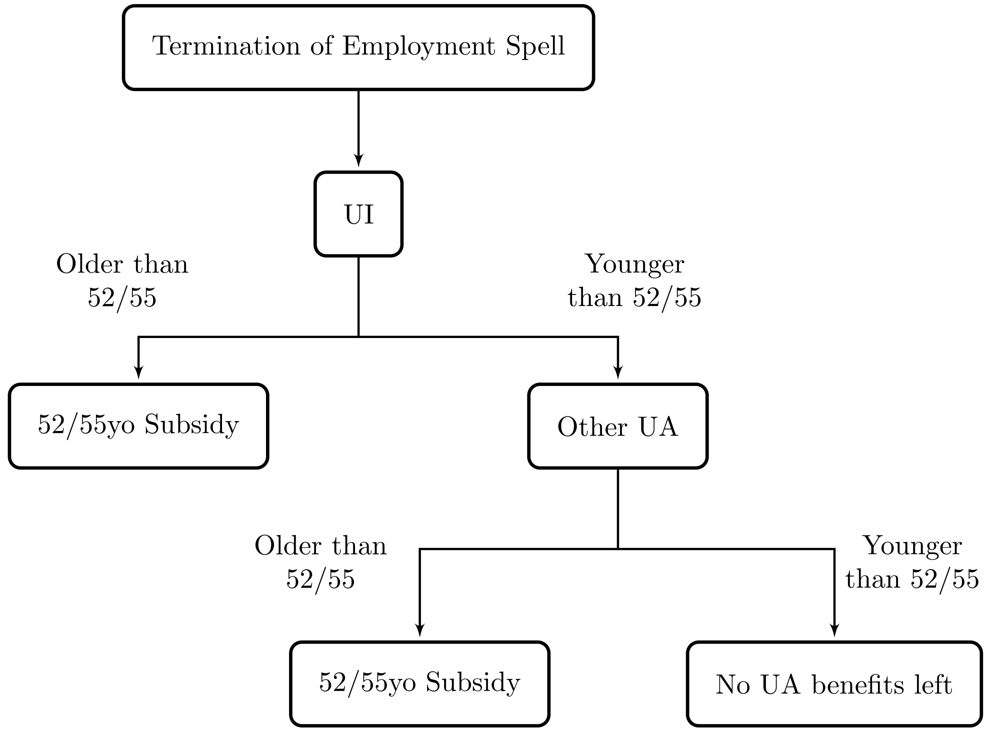
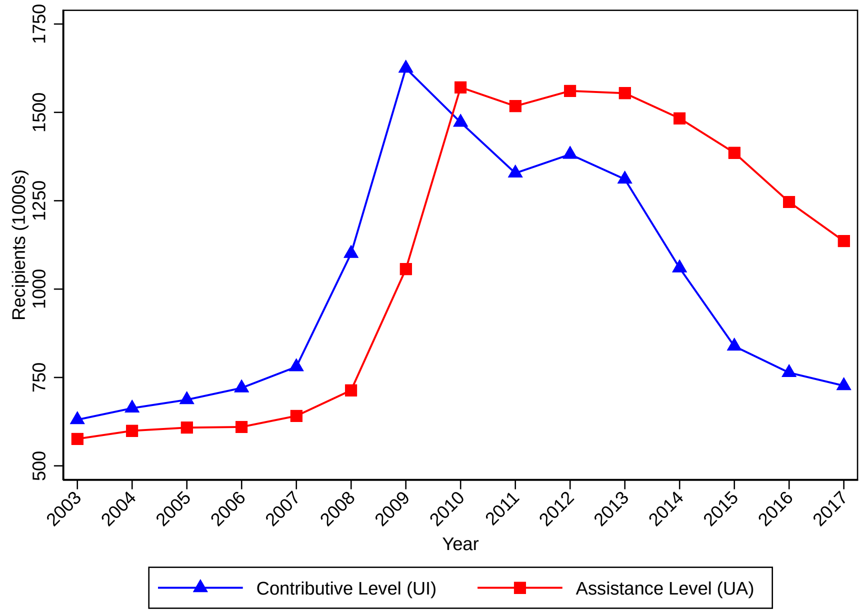
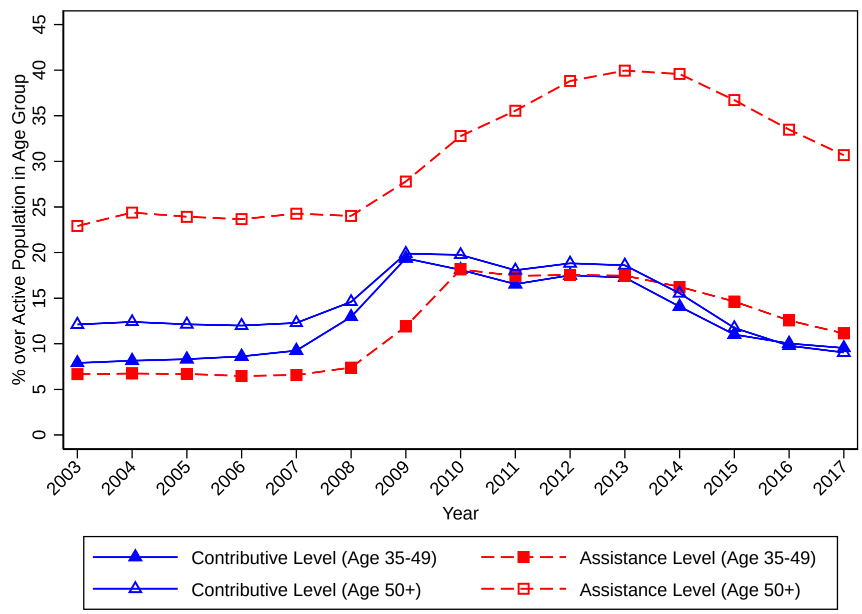


Figure A1: The unemployment insurance system in Spain

Notes: This figure summarizes the unemployment insurance system in Spain. After a non-voluntary termination of an employment spell, individuals that have worked enough days are eligible to UI, with a PBD of up to 24 months (See Table A1 for details). After UI exhaustion, they have one month and 15 days to claim UA benefits. The specific UA program they receive (provided they satisfy the eligibility conditions; see Table A2) depends on their age at the time of UI exhaustion. If older than 52/55 years old, they receive the 52/55yo Subsidy (with unlimited PBD). If younger, they receive Other UA, with a PBD between 3 and 30 months. After exhausting Other UA, individuals have a second chance to claim the 52/55yo Subsidy, provided they are age-eligible. They, again, have one month and 15 days to claim benefits. If not age-eligible, they cannot claim additional UA benefits, but they can apply to other programs not sponsored by Social Security (such as the RAI). Source: *Servicio Estatal Público de Empleo* (SEPE).



(a) Unemployment levels



(b) Unemployment rates

Figure A2: Unemployment evolution in Spain (2003-17) — by benefit type and age group

Notes: This figure shows the evolution of unemployment in Spain (2003-17) by type of benefits (top) and age group (bottom). The category "contributive level" includes regular UI benefits, which generosity and duration depend on the previous work history. The category "assistance level" lumps UA and RAI together. UA paid €430 per month in 2012 and had a PBD ranging between three months and unlimited, depending on the previous work history and individual characteristics. For reference, in 2013, 19% of the individuals in the UA-age 50+ category were in RAI. Source: *Servicio Estatal de Empleo (SEPE).*

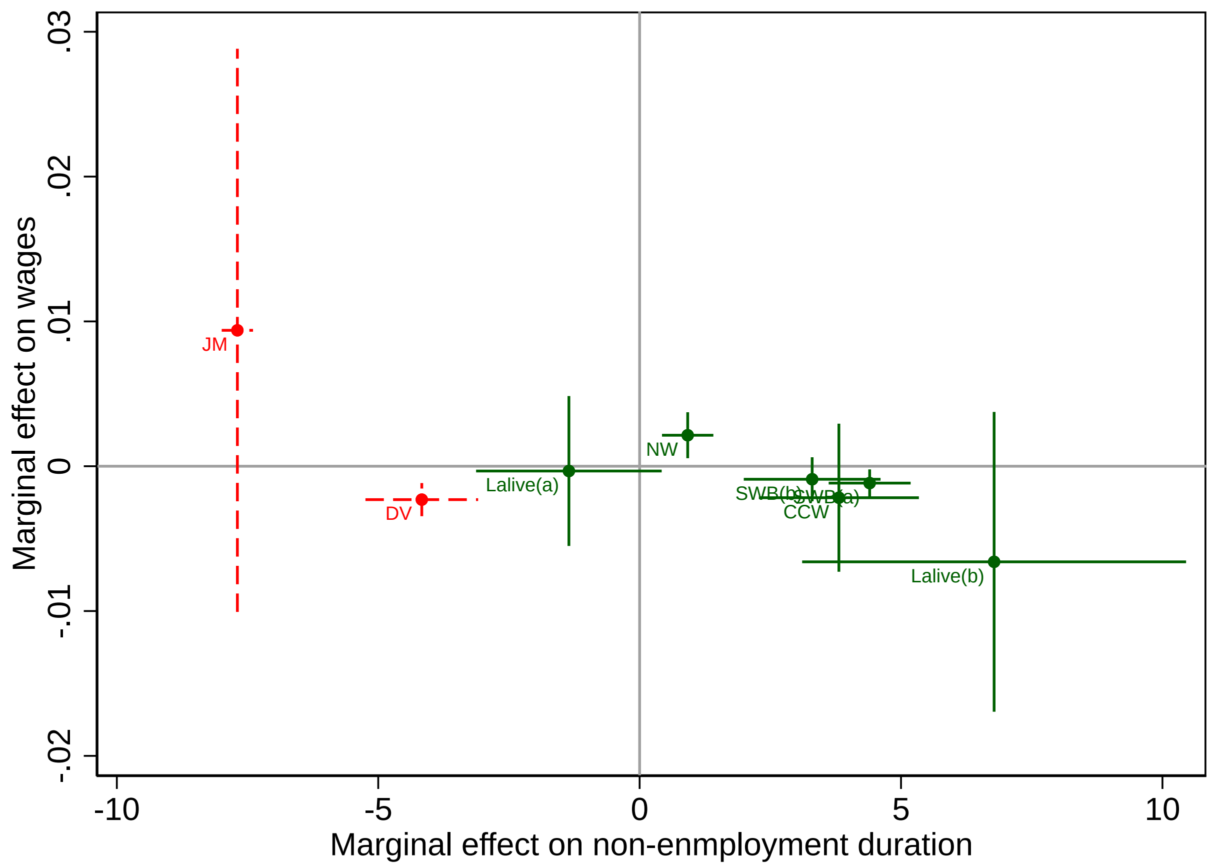


Figure A3: The effects on non-employment duration and re-employment wages in relation to the previous literature

Notes: We follow Nekoei and Weber (2017) to produce a figure comparing our estimates to some of the leading papers in the literature. We use Figure 4 (Panel A) in Nekoei and Weber (2017) and include the estimates from Johnston and Mas (2018). The coefficient with the DV label shows our estimates of the reform's effect on non-employment duration (x-axis) and the re-employment wages (y-axis). They are obtained from Table 1, Column 10 in Panel A (duration), and Column 2 in Panel B (change in wages). The vertical and horizontal lines around the coefficient show 95% confidence intervals. Red coefficients and lines denote studies analyzing benefit reductions. Green coefficients and lines indicate studies investigating benefit extensions.

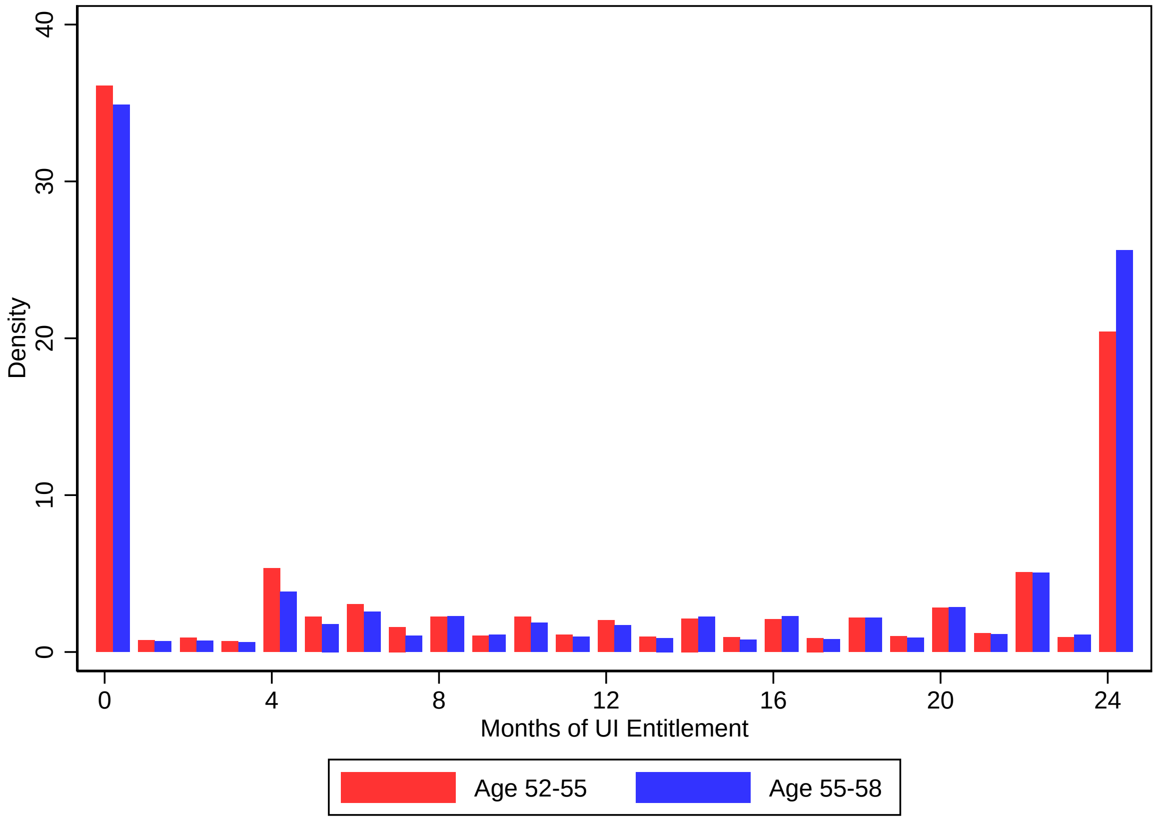


Figure A4: Distribution of UI entitlement across age groups

Notes: This figure shows the distribution of UI entitlement (in months) across age groups in the baseline sample. The maximum UI entitlement is 24 months, obtainable after working for six years uninterruptedly (see Table A1 for details).

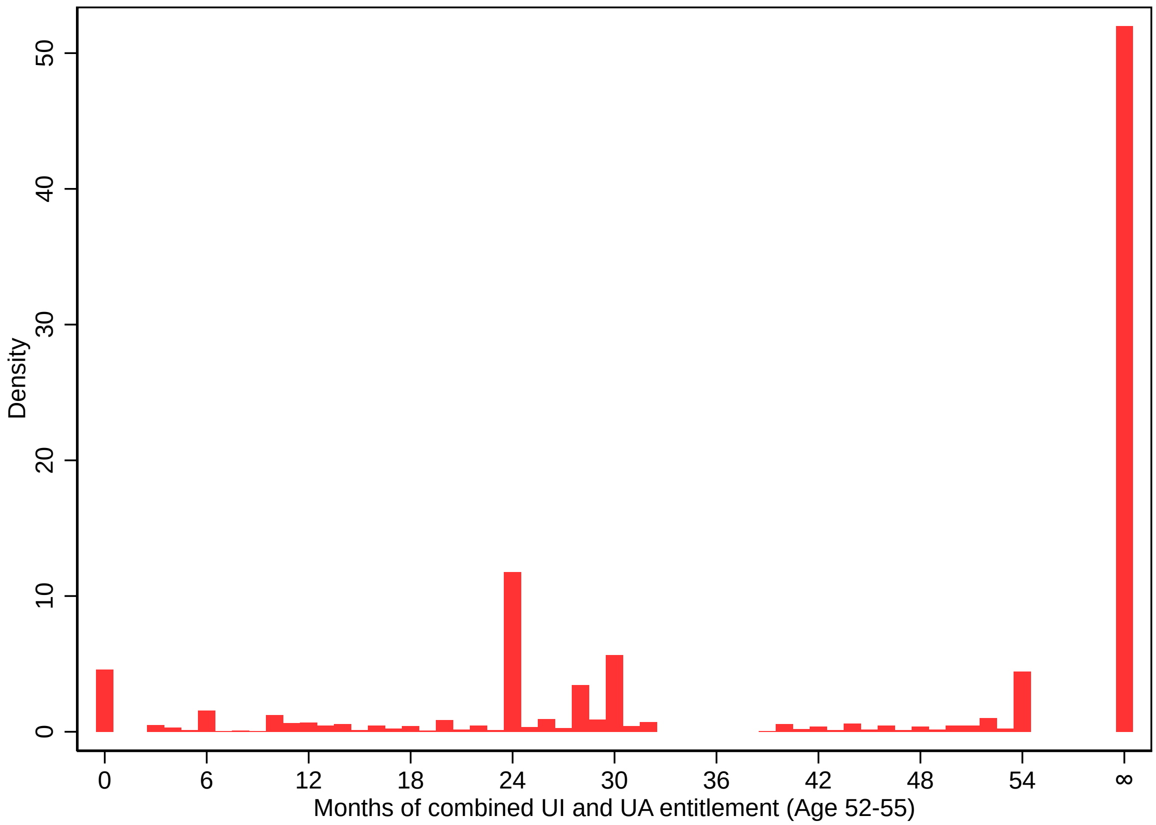


Figure A5: Distribution of combined UI and UA entitlement for the treated group after the reform

Notes: This figure shows the distribution of combined UI and UA entitlement (in months) for the treated group (Age 52-55) after the reform. We estimated UA entitlement following the rules outlined in Table A2, assuming workers satisfy all eligibility conditions for UA other than the age at UI exhaustion. Estimates take into account potential bridging between Other UA and the Subsidy. Estimates are upper bounds, as we assigned workers with more than one co-habitant in the "family" category.

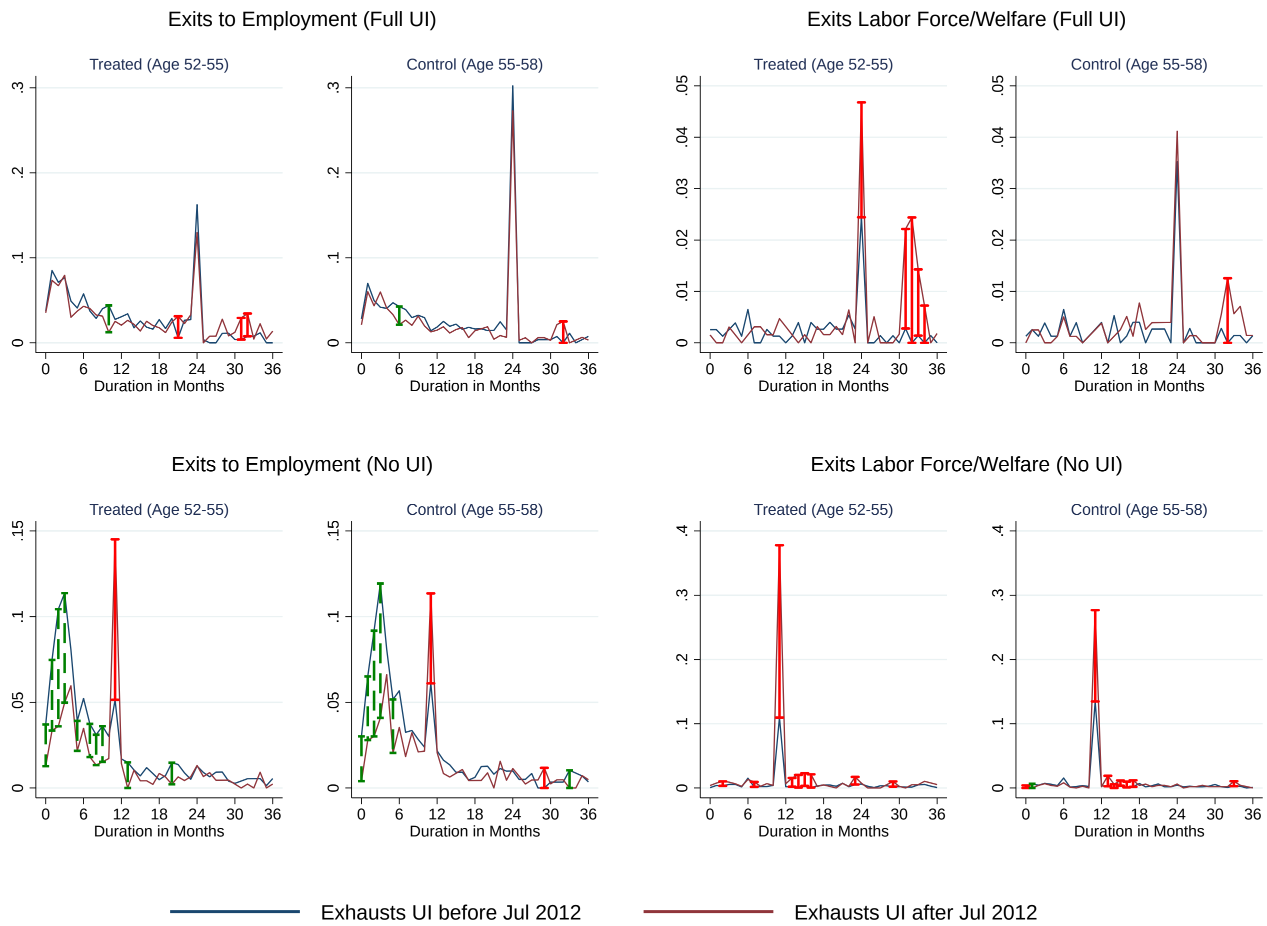


Figure A6: The effects of benefit cuts on the hazards to employment and out of the labor force - heterogeneity in UI entitlement

Notes: Hazard rate estimates to employment and out of the labor force throughout the non-employment spell for the subsample of individuals with full UI entitlement (top) and no UI (bottom). Hazards are estimated non-parametrically using all individuals exhausting UI within one year of the reform, separately by age groups. The blue line represents hazards in the pre-period. The red line represents those in the post-period. Red vertical bars indicate significant (95% level) increases in the hazards at a given month. Green dashed lines indicate significant reductions.

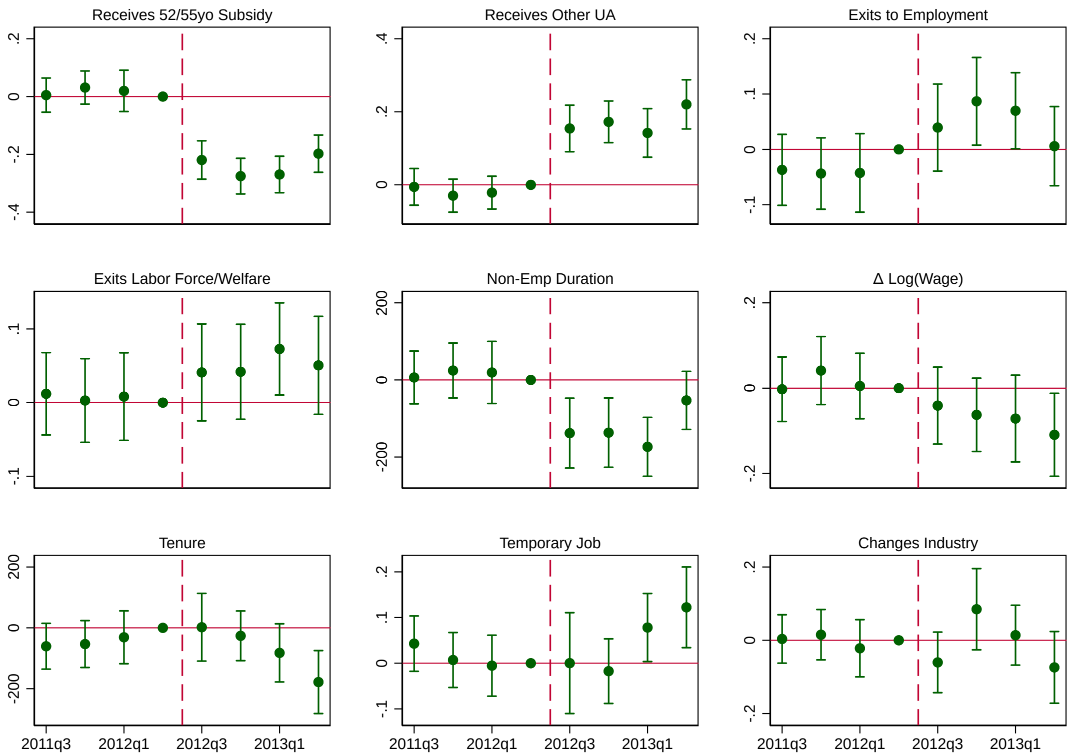


Figure A7: Pre-trends and event study analysis (younger cohorts comparison)

Notes: Each coefficient corresponds to a β_t estimate from equation 2. Vertical lines identify 95% confidence intervals. Treated individuals are those aged 52-53 at UI exhaustion. Control individuals are those aged 55-56 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.

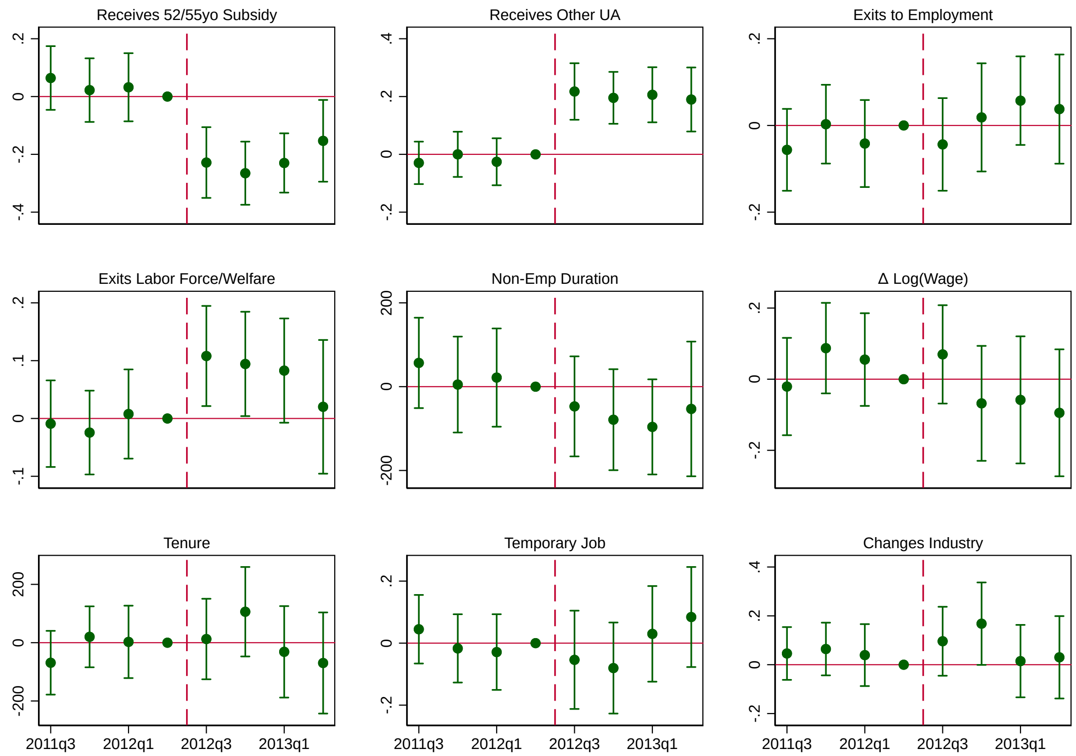


Figure A8: Pre-trends and event study analysis (older cohorts comparison)

Notes: Each coefficient corresponds to a β_t estimate from equation 2. Vertical lines identify 95% confidence intervals. Treated individuals are those aged 54-55 at UI exhaustion. Control individuals are those aged 55-56 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.

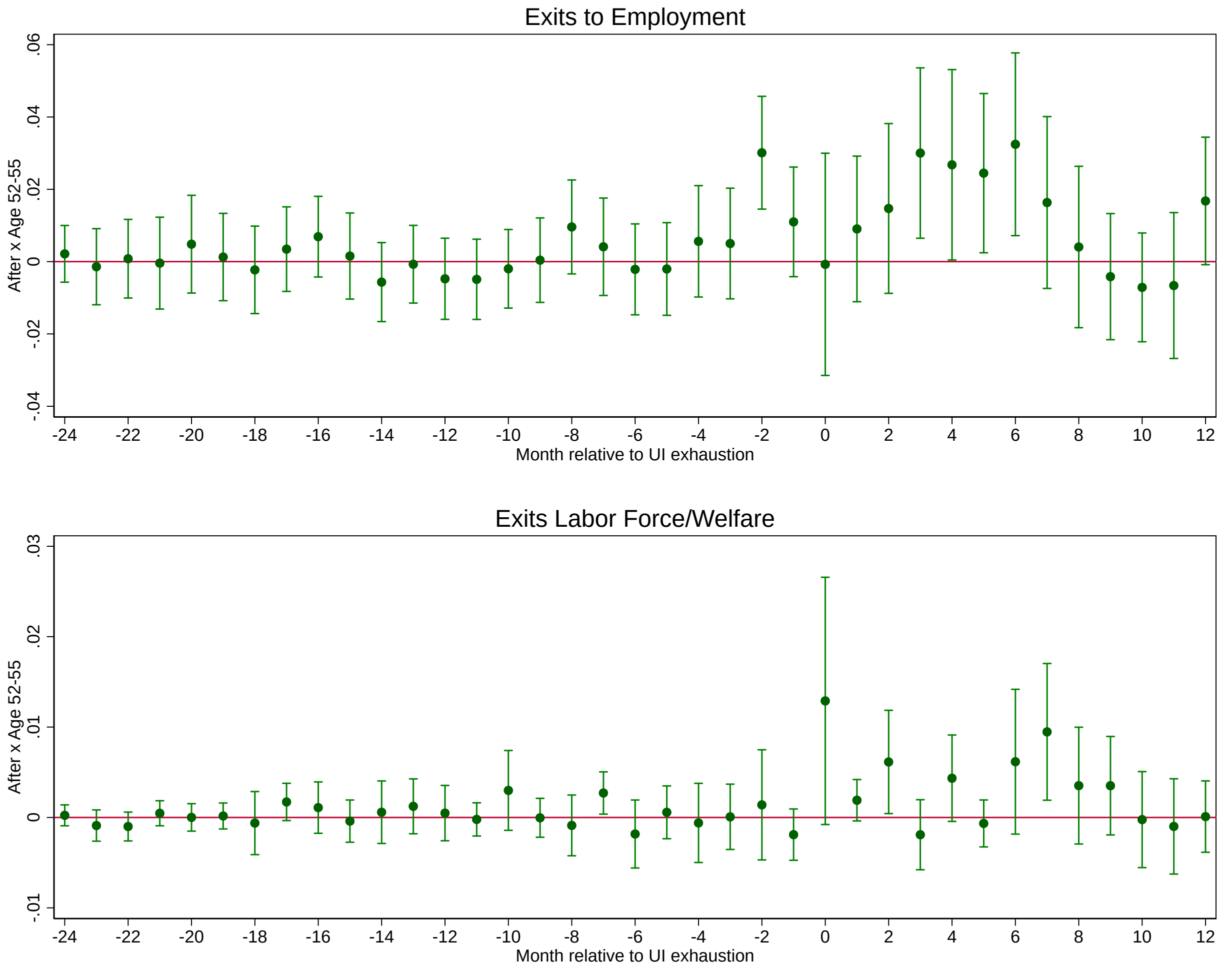


Figure A9: Heterogeneity with respect to UI exhaustion timing

Notes: Each coefficient in the figure estimates the differential probability of exiting to employment (top) or out of the labor force and welfare (bottom) in the treatment group after the reform in a particular month. Individual coefficients are β_t estimates from Equation 4 obtained after restricting the sample at risk of exiting non-employment at a given month relative to their specific UI exhaustion date. In other words, it excludes workers that already completed their non-employment spells at that stage. All regressions are estimated with the same battery of controls as in the baseline, with robust standard errors clustered at the province-quarter level. The green vertical lines depict 95% confidence intervals.

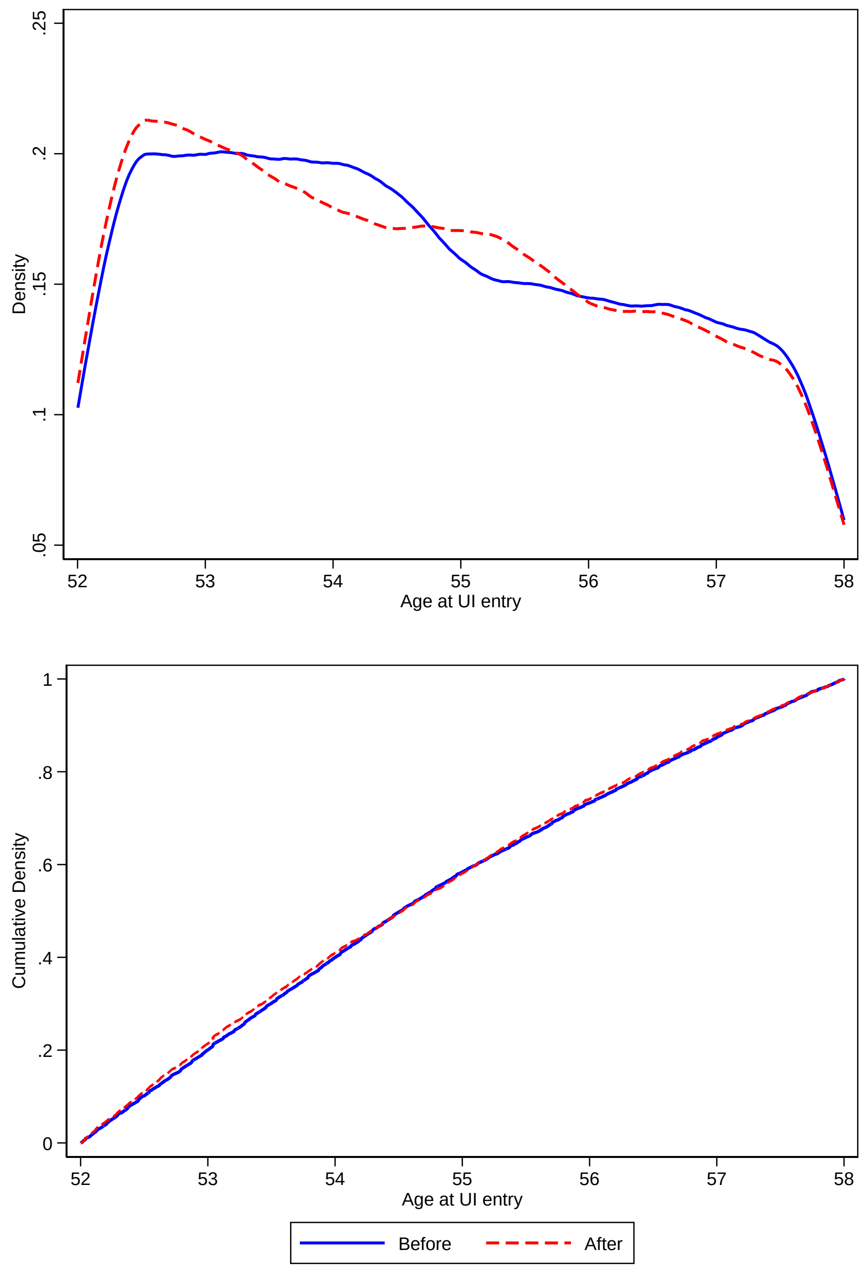


Figure A10: Age composition of inflows into non-employment before and after the reform

Notes: The top plot shows the Kernel density of inflows into non-employment by age at entry. The bottom plot shows the cumulative distribution. The sample is restricted to individuals entering non-employment within a one-year window of the reform (i.e., from July 2011 to July 2013). A Kolmogorov-Smirnov test marginally rejects the hypothesis of the two distributions being drawn from different populations at the 5% level ($p = 0.052$).

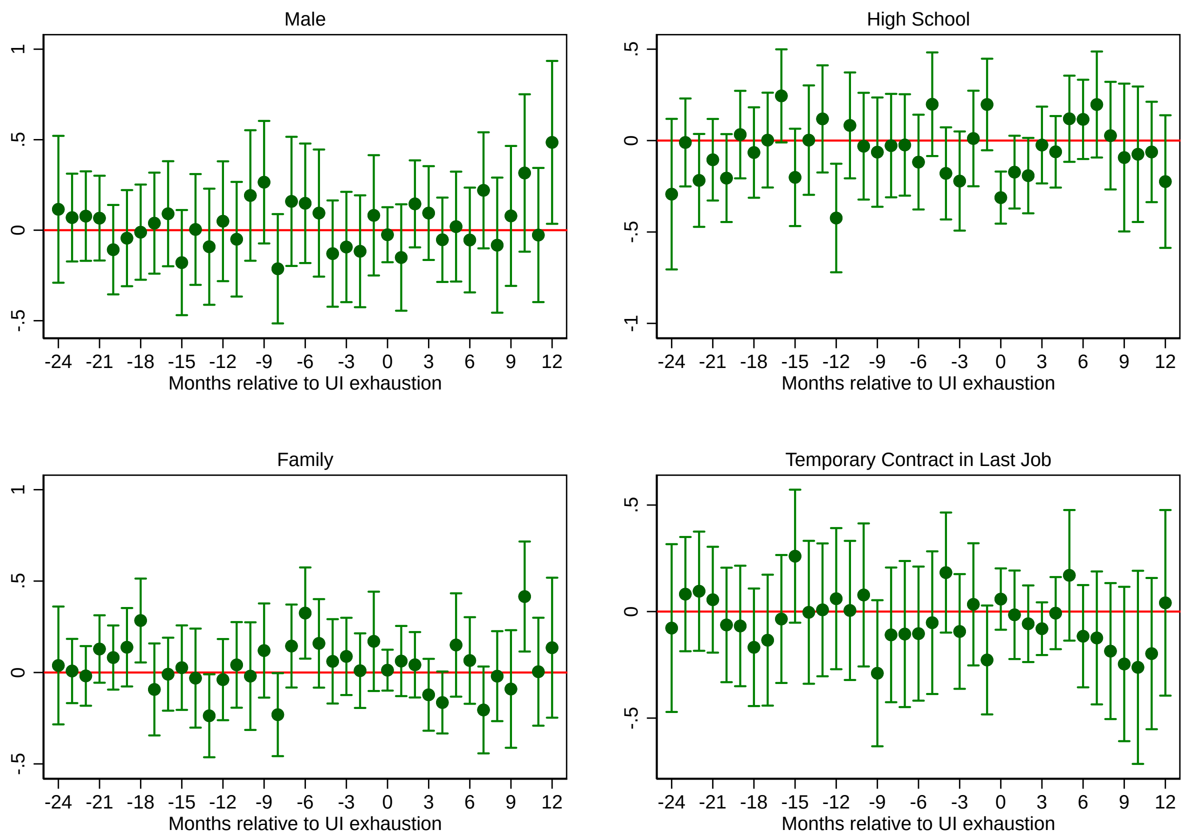


Figure A11: Dynamic selection on observables along the non-employment spell

Notes: Each coefficient in the figure estimates the differential probability that a treated individual exiting non-employment in a specific month (relative to the date of UI exhaustion) belongs to a particular group (e.g., male). *Male* is an indicator identifying males. High School is an indicator identifying workers with at least a high school education. *Family* is an indicator identifying workers with at least one co-habitant in the household. *Temporary Contract in Last Job* is an indicator identifying workers that had a temporary contract in their last job. Individual coefficients are $\beta_{1,t}$ estimates from Equation 5, where the sample is restricted to workers exiting in a specific month. The green vertical lines depict 95% confidence intervals.

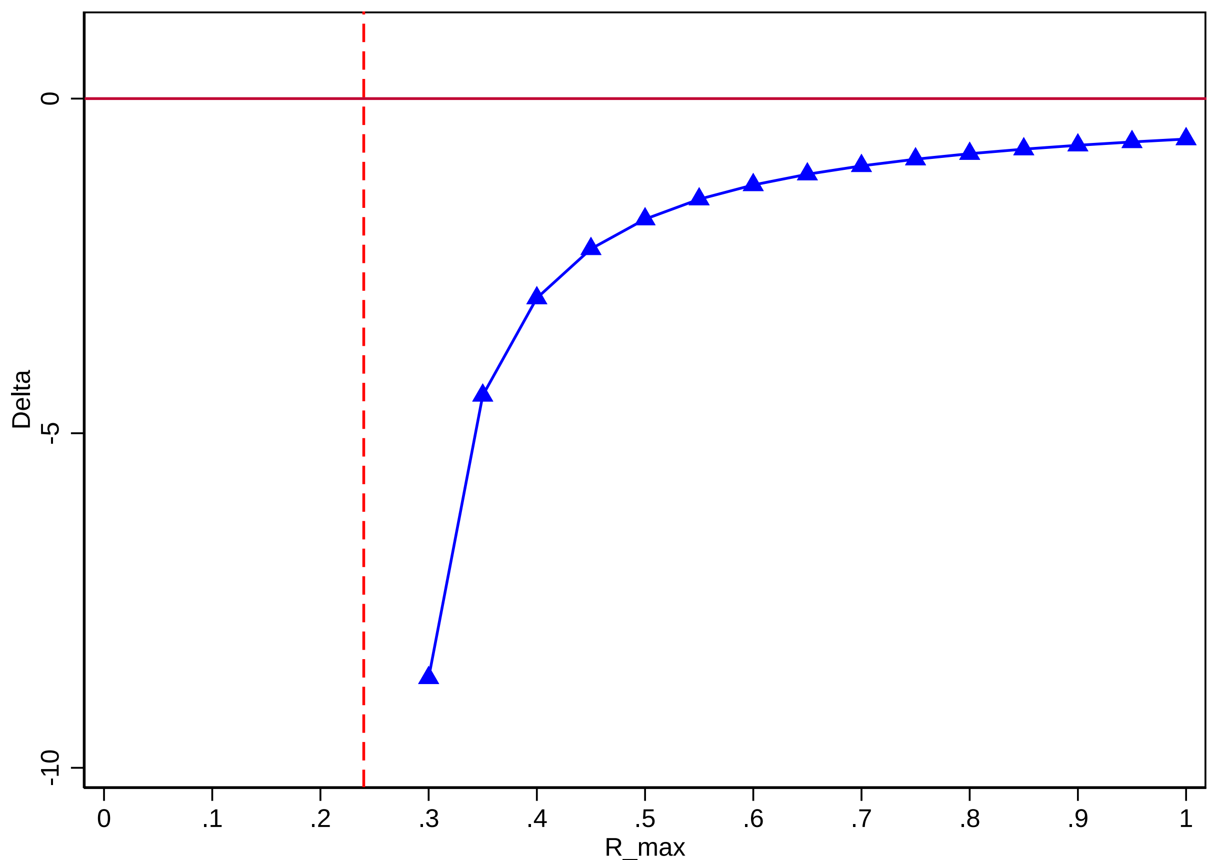


Figure A12: Wage effects and selection on unobservables

Notes: We follow Oster (2017) to assess the likely importance of unobservables in driving the baseline results on re-employment wages. δ quantifies the required relative importance of unobservables to reduce the wage effect to zero. For example, $\delta = 1$ would imply that unobservables should be as relevant as observables. R_{max} denotes the theoretical R^2 achieved in a regression with the theoretically relevant set of observables and unobservables. A negative δ implies that unobservables ought to be negatively correlated with the treatment. The vertical red-dashed line shows the R^2 in our baseline specification (Column 2 in Table 1).

Table A1: Unemployment Insurance (UI) entitlement as a function of contributions

Contributions (days)	UI entitlement (months)
less than 360	0
360 to 539	4
540 to 719	6
720 to 899	8
900 to 1079	10
1080 to 1259	12
1260 to 1439	14
1440 to 1619	16
1620 to 1799	18
1800 to 1979	20
1980 to 2159	22
2160 or more	24

Notes: This table shows the number of months of unemployment insurance (UI) entitlement an individual is eligible to receive as a function of their days of contribution to the Social Security Administration during the previous six years. Source: *Servicio Estatal de Empleo* (SEPE).

Table A2: Unemployment assistance (UA) programs in Spain

Subsidy	**Eligibility**	**Potential Benefit Duration (PBD)**	
		Without Family	**With Family**
52/55yo Subsidy	Age $\geq$ 52/55 at UI Exhaustion	Unlimited	Unlimited
Other UA: *Agotamiento de la prestación contributiva* (UI Exhaustion)	Having exhausted UI $\geq$ 4 months	6 months	24 months (UI $<$ 9 months) 30 months (UI $\geq$ 9 months)
Other UA: *Subsidio por insuficiencia de cotización* (Lack of Contribution)	No UI entitlement at layoff, but at least 3 months of contributions (6 months if no family responsabilities)	6 months	x months, if x $\in$ [3,6] 21 months if x $>$ 6 (x = months of contributions)
RAI: *Renta Activa de Inserción* (Active Insertion Income)	Not being eligible to receive any of the previous unemployment benefits	11 months	11 months

Notes: This table describes the different unemployment subsidies available to workers that exhausted their contributive UI in Spain. The "UI Exhaustion" and "Lack of contribution" unemployment assistance (UA) programs are lumped together under the name "Other UA" throughout the paper. All UA programs require at least three months of contributions at the time of job separation. The "52/55yo Subsidy" requires at least six years of contributions (throughout the entire employment history). The RAI is not a UA program and is not administered by the Social Security administration — thus not observable in the MCVL. In addition to the eligibility conditions specific to each program stated in the table, the following conditions always need to be satisfied: (1) actively look for a job; (2) do not refuse an "adequate" job offer; (3) lack of income (earnings below €480 in 2012 – 75% of the statutory minimum wage). "Family responsibilities" is defined as having children under the age of 26 or a dependent relative, in both instances living in the same dwelling. All subsidies pay the same monthly amount, equivalent to 80% of the IPREM (*Indicador Público de Renta de Efectos Múltiples*). They paid €426 in 2012. They pay €463 in 2022. Source: *Servicio Estatal de Empleo* (SEPE).

Table A3: Summary statistics

	(1) Full Sample	(2) Control Before	(3) Treatment Before	(4) Control After	(5) Treatment After
Panel A: Outcomes					
Receives 52/55yo Sub	0.25	0.30	0.27	0.34	0.030
	(0.43)	(0.46)	(0.44)	(0.47)	(0.17)
Receives Other UA	0.19	0.13	0.19	0.14	0.38
	(0.40)	(0.33)	(0.39)	(0.34)	(0.49)
Exits to Employment	0.72	0.72	0.74	0.63	0.74
	(0.45)	(0.45)	(0.44)	(0.48)	(0.44)
Exits Labor Force/Welfare	0.14	0.12	0.10	0.17	0.20
	(0.34)	(0.33)	(0.30)	(0.38)	(0.40)
Spell is Censored	0.15	0.16	0.16	0.20	0.055
	(0.35)	(0.37)	(0.36)	(0.40)	(0.23)
Subsidy after UA	0.024	0.017	0.030	0.019	0.029
	(0.15)	(0.13)	(0.17)	(0.14)	(0.17)
Non-empl Duration (Days)	492.7	485.9	460.6	615.9	440.6
	(524.8)	(534.3)	(533.9)	(548.5)	(439.5)
Daily wage difference (EUR)	2.29	2.18	2.71	2.88	1.03
	(29.9)	(29.2)	(29.5)	(30.9)	(30.8)
Tenure (Next Job)	281.8	263.5	254.8	350.2	308.3
	(417.8)	(390.5)	(381.2)	(493.9)	(452.6)
Separation in 6mo	0.58	0.58	0.58	0.57	0.58
	(0.49)	(0.49)	(0.49)	(0.49)	(0.49)
Temp Contract (Next Job)	0.80	0.81	0.84	0.72	0.78
	(0.40)	(0.40)	(0.37)	(0.45)	(0.42)
Changes Industry	0.32	0.30	0.29	0.41	0.37
	(0.47)	(0.46)	(0.45)	(0.49)	(0.48)
Disability after Unemp	0.041	0.052	0.034	0.041	0.037
	(0.20)	(0.22)	(0.18)	(0.20)	(0.19)
Panel B: Indiv Characteristics					
Male	0.59	0.60	0.55	0.62	0.59
	(0.49)	(0.49)	(0.50)	(0.49)	(0.49)
High School	0.23	0.20	0.23	0.26	0.24
	(0.42)	(0.40)	(0.42)	(0.44)	(0.43)
Family	0.84	0.84	0.85	0.82	0.82
	(0.37)	(0.37)	(0.36)	(0.38)	(0.38)
Urban	0.51	0.50	0.51	0.55	0.51
	(0.50)	(0.50)	(0.50)	(0.50)	(0.50)
Temp Contract (Last Job)	0.61	0.66	0.69	0.48	0.52
	(0.49)	(0.47)	(0.46)	(0.50)	(0.50)
Last daily wage (EUR)	56.8	57.0	55.3	60.1	56.3
	(25.2)	(24.9)	(24.7)	(27.1)	(24.6)
UI Entitlement (Months)	12.0	11.1	10.1	14.7	14.6
	(9.95)	(9.99)	(9.81)	(9.80)	(9.15)
Number of observations	11348	3308	3982	2062	1996

Notes: Summary statistics by age group and UI exhaustion timing. Before includes the subsample expecting to exhaust UI from July 15, 2011, to July 14, 2012. After includes the subsample expecting to exhaust UI from July 15, 2012, to July 14, 2013. Standard deviations in parenthesis.

Table A4: Disability Insurance and exits out of the labor force

Panel A. Baseline sample

	DI after unemployment		Exits LF/Welfare and does not work again	
	(1)	(2)	(3)	(4)
Age 52-55 × After	0.014*	0.014*	0.030**	0.030**
	(0.007)	(0.008)	(0.014)	(0.013)
DV Mean (Pre-T)	0.034	0.034	0.087	0.087
Controls	No	Yes	No	Yes
R-sq	0.001	0.018	0.010	0.058
Observations	11348	11348	11348	11348

Panel B. Younger cohorts comparison

	DI after unemployment		Exits LF/Welfare and does not work again	
	(1)	(2)	(3)	(4)
Age 52-53 × After	0.015	0.011	0.038*	0.042**
	(0.012)	(0.012)	(0.019)	(0.019)
DV Mean (Pre-T)	0.032	0.032	0.080	0.080
Controls	No	Yes	No	Yes
R-sq	0.004	0.030	0.009	0.077
Observations	4567	4567	4567	4567

Panel C. Older cohorts comparison

	DI after unemployment		Exits LF/Welfare and does not work again	
	(1)	(2)	(3)	(4)
Age 54-55 × After	0.037***	0.033**	0.054***	0.056***
	(0.013)	(0.013)	(0.020)	(0.020)
DV Mean (Pre-T)	0.036	0.036	0.094	0.094
Controls	No	Yes	No	Yes
R-sq	0.002	0.031	0.010	0.077
Observations	4135	4135	4135	4135

Notes: In Panel A, treated individuals are those aged 52-55 at UI exhaustion. Control individuals are those aged 55-58 at UI exhaustion. In Panel B, treated individuals are aged 52-53. Control individuals are aged 55-56. In Panel C, treated individuals are aged 54-55. Control individuals are aged 55-56. *DI after unemployment* is an indicator taking the value of 1 if the worker claims Disability Insurance (DI) at some point after entering non-employment. *Exits LF and does not work again* is an indicator taking the value of 1 if workers exiting the labor force or going into welfare (as previously defined) do not find a job within 50 months of entering 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. We follow individuals for 50 months after entering non-employment. Longer spells are censored. Heteroskedasticity-robust standard errors clustered at the province-quarter level in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table A5: Results excluding workers entering non-employment after July 15, 2012

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	-0.199***	-0.193***	0.138***	0.142***	0.077***	0.074***	0.060***	0.058***	-133.563***	-114.242***
	(0.016)	(0.015)	(0.017)	(0.017)	(0.019)	(0.019)	(0.016)	(0.016)	(22.723)	(21.090)
DV Mean (Pre-T)	0.266	0.266	0.188	0.188	0.743	0.743	0.101	0.101	460.490	460.490
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
R-sq	0.044	0.156	0.027	0.118	0.007	0.100	0.013	0.069	0.011	0.135
Observations	10367	10367	10367	10367	10367	10367	10367	10367	10367	10367

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	-0.074***	-54.885**	-24.635	0.035	0.020	0.058**	0.026	-0.041	-0.023
	(0.024)	(0.023)	(27.660)	(26.061)	(0.026)	(0.026)	(0.025)	(0.023)	(0.027)	(0.026)
DV Mean (Pre-T)	0.026	0.026	254.460	254.460	0.577	0.577	0.838	0.838	0.286	0.286
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
R-sq	0.001	0.247	0.017	0.144	0.001	0.117	0.022	0.239	0.013	0.127
Observations	6863	6863	6863	6863	6863	6863	6863	6863	6863	6863

Notes: Sample is restricted to workers entering non-employment before the reform (July 15, 2012). 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. We follow individuals for 50 months after entering non-employment. Longer spells are censored. Heteroskedasticity-robust standard errors clustered at the province-quarter level in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

B Additional results and robustness

This appendix contains additional results and robustness checks not included in the main text.

B.1 Other heterogeneity

Table B1 explores heterogeneity on four additional dimensions: gender, education, family status, and previous wage.

Women are less likely to enroll into Other UA (15 vs. 22pp) and are more likely to complete their non-employment spell by finding a job (11 vs. 6pp), and experience larger drops in durations (161 vs. 134 days). On the other hand, men seem to drive the drops in re-employment wages (12 vs. 2%).

Workers without a high school education experience larger effects. They are less likely to receive the Subsidy (30 vs. 18pp), more likely to exit to employment (11 vs. 1pp), and to experience larger drops in durations (175 vs. 57 days). There are no significant differences in re-employment outcomes, but low-educated workers experience slightly larger drops in wages (9 vs. 7%).

Treated workers without a family (no cohabitants in the household) are more likely to go into Other UA (27 vs. 18pp), less likely to complete their non-employment spells by finding a job (0 vs. 11pp), and more likely to leave the labor force or go into welfare (13 vs. 3pp). They experience smaller drops in duration (55 vs. 170 days). There are no significant differences in re-employment outcomes.

Treated workers with a low wage in their last job (below €47 per day) are more likely to find jobs (15 vs. 4pp), and slightly less likely to leave the labor force (4 vs. 7pp). They also experience significantly larger drops in duration (190 vs. 110 days). Workers with previously higher wages experience larger drops in re-employment wages (11 vs. 2%).

B.2 Changes in paths to non-employment spell completion

We investigate changes in paths to non-employment spell completion after the reform. Workers can complete their spells by finding a job or leaving the labor force or going into welfare (as defined throughout the paper). If they have not found a job within 50 months after entering non-employment, we consider their spells censored. The potential paths to these exits are through UI without receiving UA, through the Subsidy, Other UA, or Other UA and then the Subsidy. Table B2 shows the results.

Treated workers are 5.6pp more likely to find jobs without going into UA and 13pp more likely after receiving Other UA. If they bridge to the Subsidy, directly or through Other UA, they are up to 9pp less likely to find jobs. Similarly, they are 2-5pp more likely to leave the labor force or go into welfare directly after exhausting their UI or receiving Other UA. Conditional on receiving the Subsidy, they are up to 2pp less likely to leave the labor force. In terms of censoring, we observe that censored spells become 16pp less prevalent in treated workers going into the Subsidy. Censoring through Other UA becomes 1.6pp more likely in the treated group.[1]

B.3 General equilibrium effects

General equilibrium effects (e.g., job displacements) could confound our results. That is a concern highlighted, for example, by Lalive et al. (2015). We believe these are unlikely to be a concern in our setting for several reasons. First, the number of workers in the Subsidy is low. According to Ministerio de Empleo y Seguridad Social (2013), 367,339 workers benefitted from the 52/55yo Subsidy in 2012. That is less than 1% of the working-age population in that year. Second, despite some geographic variation across provinces (see Figure B1), recipients never represent more than 1.8% of the working-age population. Finally, the reform only affected new inflows into the Subsidy. It did not affect the stock due to grandfathering. Based on back-of-the-envelope calculations, we estimate that the reform affected (at most) 52,000 workers (less than 0.15% of the working-age population). Given the magnitudes, significant general equilibrium effects seem unlikely.

B.4 Regression Discontinuity Design (RDD)

We use a Regression Discontinuity Design (RDD) as an alternative identification strategy. We use workers' date of UI exhaustion as the running variable and July 15, 2012, as the cutoff. We estimate the equation below by weighted least squares

$$Y_i = \alpha + \tau After_i + X_i^{'}\gamma + f(T_i) + \epsilon_i \tag{6}$$

where Y, $After$, and X are defined as before. $f(T_i)$ are flexible trends in both sides of the cutoff. Observations are weighted, with weights linearly decaying with the observation's distance to c (Triangular Kernel). We select optimal bandwidths following Calonico et al. (2014a;b). τ is the main parameter of interest, as it captures the average causal effect of the reform at the cutoff if identification assumptions hold.

[1] These results concerning censoring are expected. The only possibility for a worker to still receive benefits after 50 months is when she receives the Subsidy (with unlimited PBD) or when she had 24 months of UI and then 30 months of Other UA. After these 54 months, she should be able to bridge to the Subsidy.

Identification for the RDD requires that the running variable (date of UI exhaustion) is continuous at the cutoff (no manipulation). Figure B2 provides evidence on that direction. Each panel in the figure plots the density of observations within one year of the reform for the two age groups. The numbers within each figure show the discontinuity and standard error estimates obtained from running the McCrary test (McCrary 2008). There are no significant differences (at the 5% level) in the densities around the cutoffs, suggesting no manipulation of the running variable.[2]

Table 1 shows τ estimates from Equation 6. We complement the discussion with RDD plots in Figures B3 (for the treated group) and B4 (control group).

RDD results confirm the reduction in Subsidy take-up in the treated group (-25pp). They suggest slightly larger effects in exits to employment (+12 instead of +9pp) and non-employment duration (-260 days instead of -150). Other UA and exits out of the labor force also increase (by 8 and 4pp, respectively), but coefficients are not significant. Re-employment effects seem to go in the opposite direction, as wages do not change and tenure increases by 112 days ($p < 0.05$). There are no significant effects in the control group.

The discrepancy between the RDD and our baseline DD estimates responds to the local nature of the RDD. Comparing the RDD plots in Figure B3 with the event-study estimates in Figure 2 is helpful for the following discussion.[3] Figure B3 shows how Other UA take-up substantially increases after the reform, from a mean level of 30% to about 50%. This 20pp difference coincides with our baseline DD estimate (+19pp). However, the increase in Other UA is not immediate (it is steep, but it takes 60-90 days to reach the high point). The RDD uses a bandwidth below 60 days. Thus, the small bandwidth and the smaller sample size prevent the RDD from capturing significant effects. The same reasoning applies to non-employment duration. There is a very steep decrease in durations immediately after the reform. The effect then partly reverts, but the RDD puts less weight on observations farther away from the reform cutoff. The RDD plots also show a mild decrease in re-employment wages by accounting for the entire year after the reform. But again, the effect is not immediate. Figure 2 also showed progressively larger drops in wages in the quarters following the reform (the effect of the first quarter is not distinguishable from the zero estimate from the RDD). Therefore, discrepancies can be reconciled after considering the different bandwidths used in the two approaches.

We confirm this point by replicating our RDD analysis with a manually chosen 365-day bandwidth. Table B4 shows the results. With that bandwidth, we estimate a 26pp reduction in Subsidy take-up (vs. 27pp in the DD baseline). Other UA take-up increases by 17pp (vs.

[2] The estimate for the 52-55 age group is significant at the 10% level ($p = 0.07$).

[3] Note that RDD estimates from Table 6 are best compared with DD estimates for 2012q3 in Figure 2.

19pp). Exits to employment increase by 7pp (vs. 9pp). Exits out of the labor force increase by 7pp (vs. 5pp). Non-employment duration decreases by 184 days (vs. 150). All these magnitudes are not statistically different from each other. Re-employment wages decrease by 4% ($p > 0.1$) (vs. 8%). The wage effect is weaker and less precisely estimated in the RDD specification (we also have less power due to the smaller sample size). We do not detect any effects in the control group.

The main difference between the RDD and the DD is in the re-employment tenure results. Figures 2 and (especially) B3 show a decreasing pattern in tenures in the 52-55 age group following an initial jump. On the other hand, the bottom-left plot in Figure B4 shows an increase in re-employment tenures after 2013 in the 55-58 age group. Thus, our DD estimates (that compare averages across groups in the year following the reform) result in slightly shorter tenures. In contrast, the RDD results (that estimate the jump at the cutoff) say the opposite.

Applying the RDD methodology, we estimate fiscal savings of €16,500 per treated worker (instead of €11,500), as shown in Table B5. The main difference comes from higher tax revenue (due to longer re-employment tenures) and less spending in Other UA (with the optimal bandwidth).[4]

The RDD offers a clean way to identify the effects of the reform. It has the disadvantage of estimating a shorter-run impact at a specific point of the year (in the middle of the Summer) and having less statistical power due to smaller sample sizes. The DD captures a longer-run effect and has more power (due to the larger sample size). Its disadvantage is that the parallel trends assumption is not directly testable. However, parallel pre-trends in the outcomes analyzed provide supporting evidence of its plausibility.

B.5 Younger cohorts

Workers younger than 52 are indirectly affected by the reform. As Geyer and Welteke (2021) suggests, shifting the "unemployment tunnel" should impact cohorts younger than 52 as well, as completely shutting down this path forces them to stay in employment longer. We investigate whether the reform shifted the behavior of younger (age 49-52) cohorts using the previous RDD setting with the date of UI exhaustion as the running variable. Figure B5 plots the densities around the cutoff and suggests no manipulation in the running variable. Table B6 shows the RDD results.

Younger workers respond to the new policy environment. No worker in this group can access the subsidy after the reform, while before, 1% of the workers did so by bridging

[4] With a 365-day bandwidth, we estimate fiscal savings of approximately €13,500.

through Other UA. They become 21pp less likely to take up Other UA and stay 108 days less in non-employment. They last longer in their new jobs (45 days, not significant), are 13pp more likely to remain at their new position for at least six months ($p < 0.05$), and are 14pp less likely to change industries ($p < 0.01$). They are not trying to bridge to unlimited UA and find (better) jobs sooner.

B.6 Placebo reform

We replicate our baseline analysis by studying a placebo reform on July 15, 2008 – exactly four years before the actual reform. Panels A and B from Table B7 show the results.

Results suggest no substantial differences in the treated group following the placebo reform. We find slightly smaller subsidy take-up and a lower probability of finding temporary jobs in the placebo-treated group ($p < 0.05$). These represent 3 out of 20 coefficients (15%) significant at the 5% level. Ideally, we should not find more than 1 or 2 statistically significant coefficients. However, these should still be false positives, as nothing changed differentially between groups on July 15, 2008.

B.7 Donut size

We test the sensitivity of our baseline results to different sizes of the donut, from 0 months (no donut) to one year. Our baseline treated group includes individuals aged 52-54.5 (donut of six months). Figure B6 plots the estimated coefficients for the main outcomes analyzed. Results are not very sensitive to the donut size choice.

B.8 Additional robustness on re-employment wages

We provide four additional robustness on our wage effects to further address non-random selection into re-employment. Table B8 presents the results.

Re-estimating wage effects by assuming re-employment wages do not change for the unemployed: We assign a 0% wage change to workers not exiting non-employment to include them in our analysis. This approach attempts to tackle non-random selection in finding a job. Columns 1-2 in the top panel show that re-employment wages in the treated group decrease by 3% after the reform ($p < 0.05$).

Part-time jobs: We test whether workers exit differently to part-time jobs in the treated group. Columns 3-4 in the top panel do not provide evidence in that direction.

Measurement after employment convergence: This exercise aims to re-examine wages when employment levels in the two groups may have converged. We re-estimate wage effects for the subsample of individuals with a job in July of a particular year (2013-2016). Columns 1-4 in Panel B show the results. We find lower re-employment wages, ranging between -9 and -11%. Wage effects could be even larger because employment levels never converge. Many workers in the control group never leave the Subsidy (which has unlimited PBD) to find a job.

Lee bounds: We set bounds to our main wage effects by applying the methodology developed in Lee (2009). The bottom panel in Table B8 shows the results. Without tightening (adding covariates) the bounds (Column 1), the wage effect has a lower bound of -13% and an upper bound of $+7\%$ ($p < 0.05$). By considering workers' previous wage, type of contract in the last job, or education level, the lower bound decreases to approximately -14% ($p < 0.01$), while the upper bound becomes statistically indistinguishable from zero.

B.9 Change in UA eligibility conditions after March 2013

On March 14, 2013, the Spanish government unexpectedly announced a new reform toughening the eligibility conditions to access UA. The new rules modified the "lack of income" condition to include the incomes of all members of the household for its computation. Before that reform, a worker would be eligible to receive UA if her income was below 75% of the minimum wage (a condition usually satisfied when unemployed). After the reform, the *average* income in the household would have to be below 75% of the minimum wage. Similar to the reform we study in this paper, the 2013 new reform was applied with grandfathering – meaning that current beneficiaries would not be affected. Also, the reform was quickly approved (March 15) and implemented (March 17) shortly after its announcement.

To ensure this new reform does not drive our main results, we replicate our baseline specification by excluding all workers with at least one cohabitant in the household expected to exhaust UI after the reform implementation date (March 17, 2013). Table B9 shows the results. Results on that table are not significantly different from those in our baseline (Table 1).

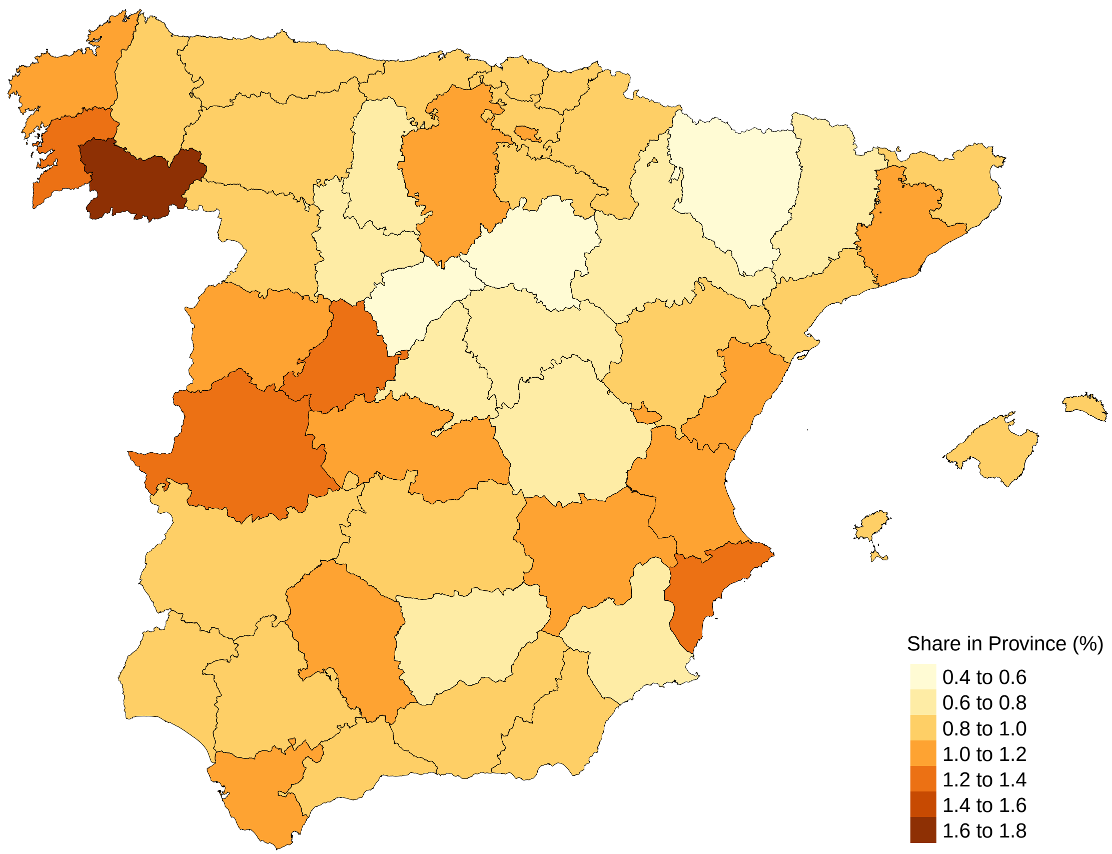


Figure B1: Geographic distribution of 52/55yo Subsidy recipients as a fraction of the working-age population in 2012

Notes: This map shows the distribution of 52/55yo Subsidy recipients as a share of the working-age population (16-65) across Spanish provinces in 2012. Darker colors indicate a larger percentage of subsidy recipients in the province. According to Ministerio de Empleo y Seguridad Social (2013), in 2012, there were 367,339 Subsidy recipients in the country. Source: SEPE and MCVL.

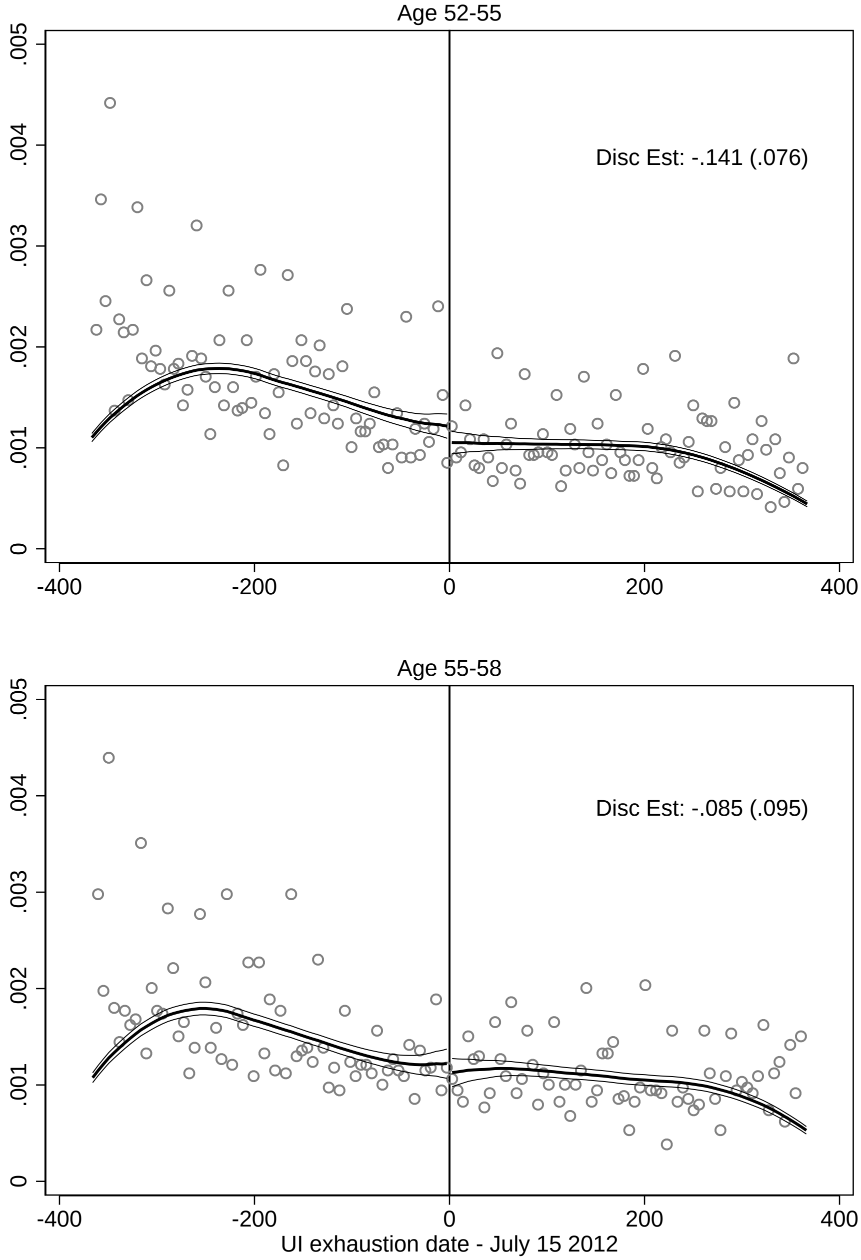


Figure B2: Density of individuals exhausting UI around July 15, 2012

Notes: Density of individuals exhausting UI within a one-year window of the reform by age group. Each subfigure includes the discontinuity and standard error estimate obtained from running the McCrary test (McCrary 2008). Figures produced with the *DCdensity* Stata command.

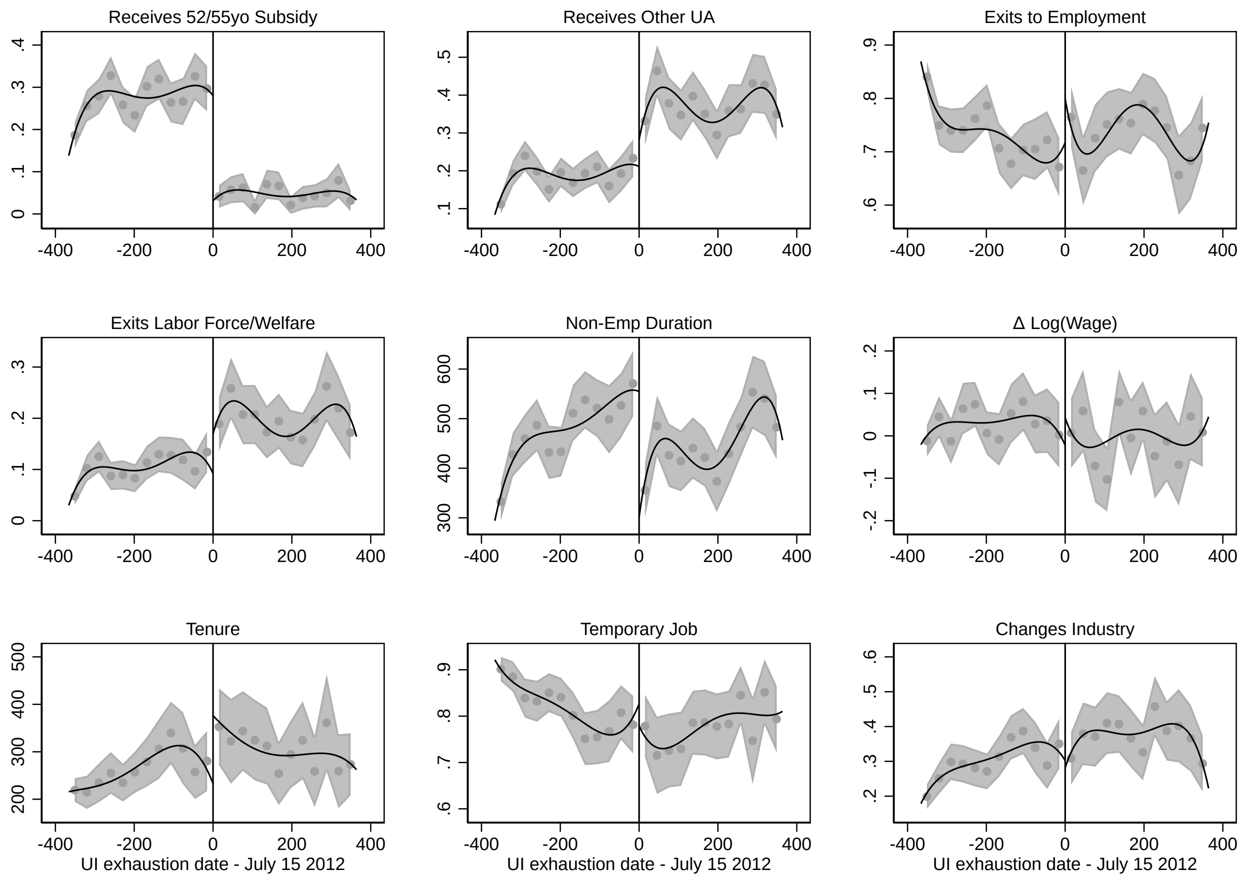


Figure B3: The effects of benefit cuts on non-employment spells and re-employment outcomes (Age 52-55)

Notes: Data-driven RDD plots produced with the Stata command *rdplot* by Calonico et al. (2017). Observations are grouped in 24 evenly spaced bins. Population conditional expectation function is approximated with a 4th order polynomial (solid line). Gray areas represent 95% confidence intervals. All outcomes as previously defined.

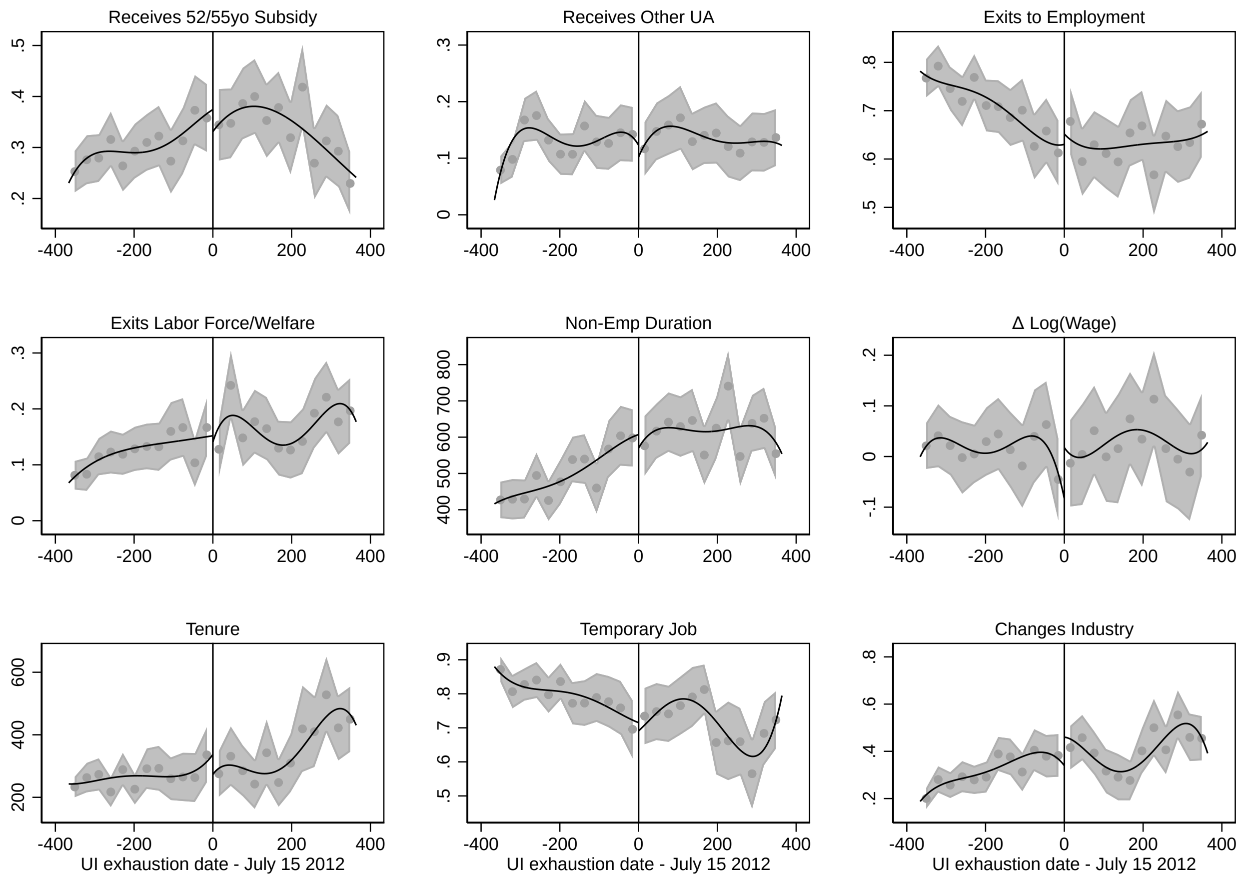


Figure B4: The effects of benefit cuts on non-employment spells and re-employment outcomes (Age 55-58)

Notes: Data-driven RDD plots produced with the Stata command *rdplot* by Calonico et al. (2017). Observations are grouped in 24 evenly spaced bins. Population conditional expectation function is approximated with a 4th order polynomial (solid line). Gray areas represent 95% confidence intervals. All outcomes as previously defined.

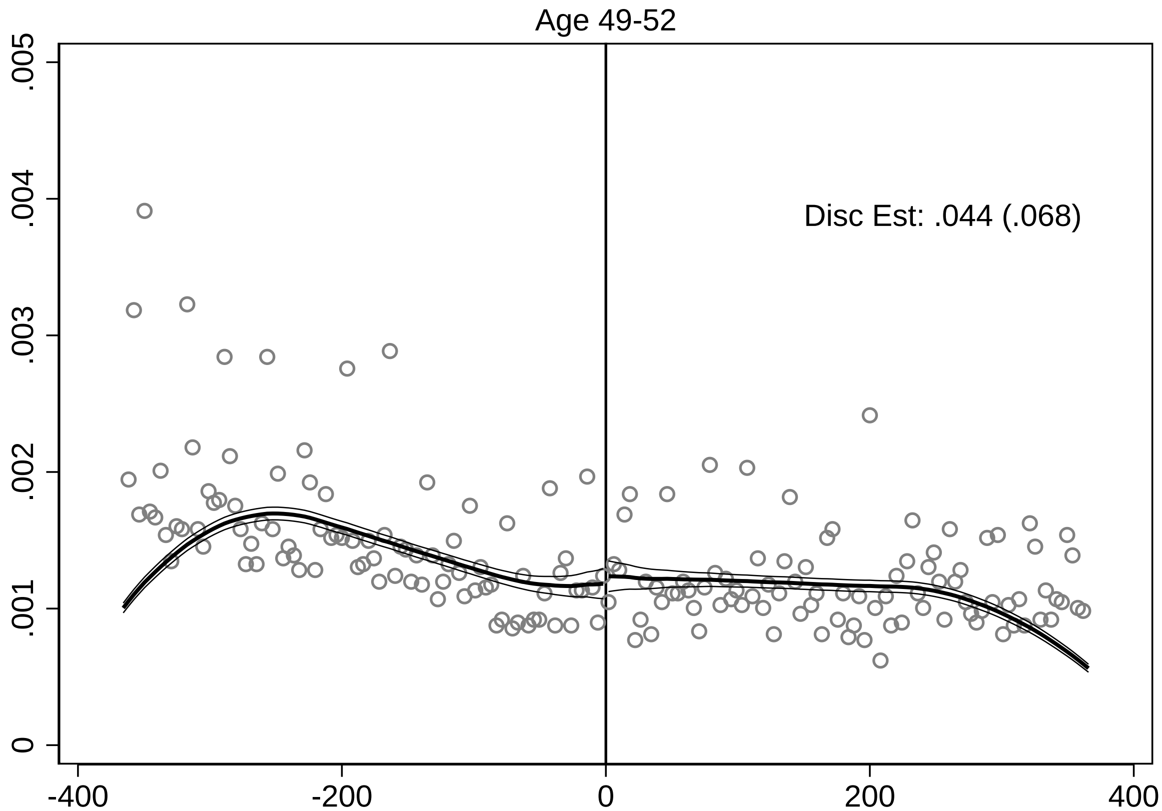


Figure B5: Density of individuals (Ages 49-52) exhausting UI around July 15, 2012

Notes: Density of individuals aged 49-52 exhausting UI within a one-year window of the reform. The figure reports the discontinuity and standard error estimate obtained from running the McCrary test (McCrary 2008). Figure produced with the *DCdensity* Stata command.

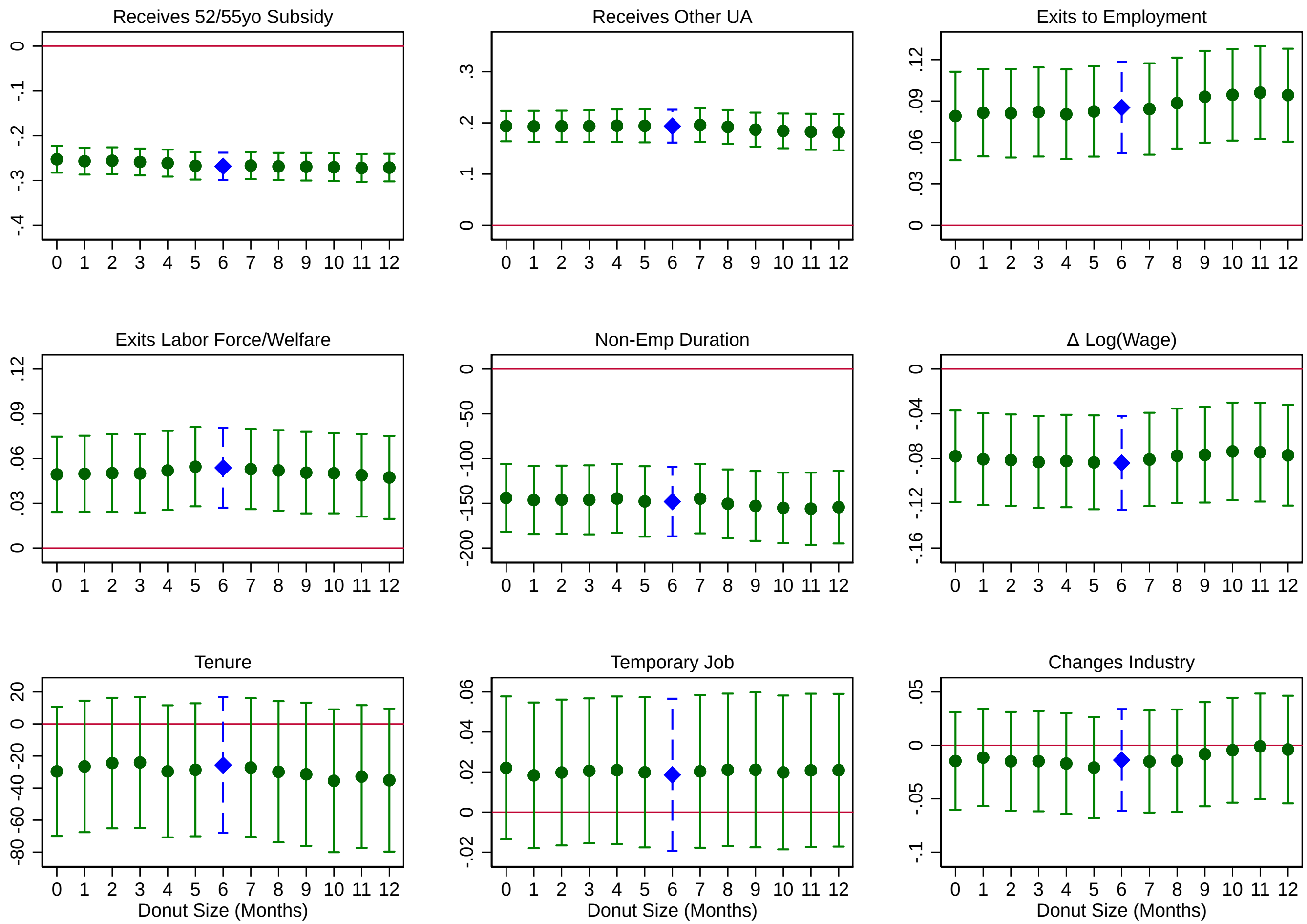


Figure B6: DD with different donut sizes

Notes: Difference-in-difference results with different donut sizes, from 0 (no donut) to 12 months (one year). Baseline results (blue diamond in the figure) include a donut size of six months. Vertical lines denote 95% confidence intervals.

Table B1: Additional heterogeneity analysis

	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)
Baseline	-0.278***	-0.268***	0.182***	0.191***	0.086***	0.086***	0.053***	0.053***	-149.918***	-149.771***
	(0.016)	(0.016)	(0.016)	(0.016)	(0.018)	(0.017)	(0.015)	(0.014)	(21.039)	(19.769)
Gender										
Female	-0.270***	-0.252***	0.135***	0.152***	0.127***	0.109***	0.063**	0.070***	-182.166***	-161.318***
	(0.022)	(0.022)	(0.027)	(0.026)	(0.028)	(0.026)	(0.026)	(0.025)	(30.698)	(27.880)
Male	-0.284***	-0.271***	0.212***	0.217***	0.058**	0.064***	0.044**	0.045**	-130.463***	-133.558***
	(0.022)	(0.020)	(0.019)	(0.019)	(0.023)	(0.022)	(0.019)	(0.018)	(26.877)	(26.029)
Education										
No High School	-0.306***	-0.296***	0.179***	0.194***	0.107***	0.109***	0.044***	0.046***	-166.758***	-174.670***
	(0.018)	(0.018)	(0.019)	(0.019)	(0.019)	(0.018)	(0.016)	(0.015)	(23.676)	(22.442)
High School	-0.208***	-0.181***	0.188***	0.206***	0.022	0.013	0.083***	0.072**	-100.918**	-56.859
	(0.032)	(0.031)	(0.029)	(0.030)	(0.038)	(0.036)	(0.031)	(0.032)	(39.860)	(38.906)
Family										
No Family	-0.264***	-0.256***	0.238***	0.271***	-0.016	-0.002	0.141***	0.131***	-52.397	-55.103
	(0.036)	(0.036)	(0.038)	(0.036)	(0.044)	(0.042)	(0.039)	(0.037)	(47.429)	(48.319)
Family	-0.280***	-0.273***	0.170***	0.175***	0.107***	0.110***	0.034**	0.033**	-170.460***	-171.013***
	(0.018)	(0.017)	(0.018)	(0.018)	(0.019)	(0.018)	(0.015)	(0.015)	(23.276)	(22.449)
Previous wage										
Low Wage	-0.295***	-0.280***	0.168***	0.189***	0.153***	0.122***	0.024	0.041*	-212.632***	-188.143***
	(0.024)	(0.023)	(0.025)	(0.024)	(0.025)	(0.023)	(0.024)	(0.023)	(29.390)	(27.528)
High Wage	-0.263***	-0.254***	0.188***	0.196***	0.036	0.047*	0.072***	0.069***	-96.666***	-109.888***
	(0.021)	(0.020)	(0.020)	(0.022)	(0.026)	(0.025)	(0.020)	(0.020)	(27.663)	(25.687)
	Δ Log(Wage)		Tenure		Separation in 6mo		Temporary Job		Changes Industry	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Baseline	-0.037	-0.083***	-33.120	-26.392	0.013	0.022	0.032	0.020	-0.021	-0.015
	(0.023)	(0.021)	(24.441)	(21.593)	(0.023)	(0.022)	(0.023)	(0.019)	(0.025)	(0.024)
Gender										
Female	0.007	-0.022	-38.042	-38.265	0.019	0.031	0.021	0.010	-0.000	0.001
	(0.038)	(0.034)	(33.964)	(32.260)	(0.039)	(0.038)	(0.033)	(0.028)	(0.034)	(0.032)
Male	-0.064**	-0.124***	-27.274	-16.709	0.009	0.017	0.037	0.022	-0.035	-0.020
	(0.030)	(0.029)	(31.572)	(29.218)	(0.031)	(0.030)	(0.028)	(0.026)	(0.032)	(0.032)
Education										
No High School	-0.044	-0.085***	-0.273	-16.118	-0.004	0.022	0.000	0.014	0.004	-0.001
	(0.027)	(0.024)	(23.058)	(22.269)	(0.027)	(0.026)	(0.022)	(0.020)	(0.027)	(0.026)
High School	-0.047	-0.068	-68.169	-48.224	0.025	0.019	0.056	0.024	-0.066	-0.077
	(0.049)	(0.045)	(55.654)	(53.137)	(0.047)	(0.048)	(0.053)	(0.046)	(0.054)	(0.049)
Family										
No Family	-0.048	-0.087	-17.352	-45.838	-0.020	-0.006	0.025	0.057	-0.007	-0.011
	(0.062)	(0.057)	(58.670)	(58.262)	(0.064)	(0.065)	(0.054)	(0.052)	(0.059)	(0.060)
Family	-0.034	-0.081***	-36.724	-23.990	0.020	0.025	0.033	0.016	-0.023	-0.009
	(0.025)	(0.023)	(26.693)	(23.750)	(0.026)	(0.025)	(0.025)	(0.021)	(0.026)	(0.026)
Previous wage										
Low Wage	-0.039	-0.022	-14.369	-23.010	0.042	0.046	0.038	0.040	-0.005	-0.009
	(0.036)	(0.035)	(29.637)	(29.392)	(0.037)	(0.036)	(0.028)	(0.027)	(0.035)	(0.035)
High Wage	-0.082***	-0.105***	-34.709	-31.956	-0.006	-0.002	0.007	0.001	-0.017	-0.016
	(0.027)	(0.027)	(34.603)	(30.441)	(0.032)	(0.031)	(0.032)	(0.026)	(0.033)	(0.032)
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes

Notes: No High School includes individuals without a high school degree. *High School* includes individuals with at least a high school degree. *No Family* includes individuals without cohabitants. *Family* includes individuals with at least one cohabitant. *Low Wage* and *High Wage* are individuals earning less or more than the median wage in their last job (€47 per day). Robust standard errors clustered at the province-quarter level in parentheses. *$p < 0.1$, **$p < 0.05$, ***$p < 0.01$.

Table B2: Changes in paths to non-employment spell completion

Panel A. Spells completed by finding a job (E)

	UI-E		UI-SUB-E		UI-UA-E		UI-UA-SUB-E	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age 52-55 × After	0.072***	0.056***	-0.098***	-0.092***	0.117***	0.126***	-0.005**	-0.005*
	(0.020)	(0.019)	(0.012)	(0.011)	(0.014)	(0.015)	(0.003)	(0.002)
DV Mean (Pre-T)	0.533	0.533	0.099	0.099	0.104	0.104	0.007	0.007
R-sq	0.004	0.195	0.021	0.138	0.030	0.116	0.001	0.017

Panel B. Spells completed by leaving the labor force (O)

	UI-O		UI-SUB-O		UI-UA-O		UI-UA-SUB-O	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age 52-55 × After	0.021**	0.019*	-0.021***	-0.019***	0.051***	0.052***	0.001	0.002
	(0.011)	(0.010)	(0.005)	(0.004)	(0.010)	(0.010)	(0.001)	(0.001)
DV Mean (Pre-T)	0.042	0.042	0.012	0.012	0.046	0.046	0.001	0.001
R-sq	0.008	0.057	0.006	0.030	0.011	0.058	0.000	0.010

Panel C. Censored spells (C)

	UI-C		UI-SUB-C		UI-UA-C		UI-UA-SUB-C	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Age 52-55 × After	-0.000	-0.001	-0.156***	-0.155***	0.017***	0.016***	0.001	0.000
	(0.000)	(0.001)	(0.012)	(0.012)	(0.005)	(0.005)	(0.005)	(0.005)
DV Mean (Pre-T)	0.000	0.000	0.125	0.125	0.008	0.008	0.023	0.023
R-sq	0.000	0.028	0.031	0.082	0.008	0.025	0.001	0.017
Controls	No	Yes	No	Yes	No	Yes	No	Yes
Observations	11348	11348	11348	11348	11348	11348	11348	11348

Notes: This table explores changes in workers' paths to non-employment spell completions. Spells are completed by finding a job (E) or leaving the labor force (O). They are not completed if the worker receives some unemployment benefit and has not found a job in 50 months after entering non-employment, in which case they are censored (C). Possible transitions to completion are directly from UI (Columns 1-2), through the Subsidy (Columns 3-4), through Other UA (Columns 5-6), or through Other UA and then the Subsidy (Columns 7-8). 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 at UI exhaustion, and province fixed effects. Heteroskedasticity-robust standard errors clustered at the province-quarter level in parentheses. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$.

Table B3: Regression-Discontinuity Design (RDD) results

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)
After	-0.245***	0.010	0.079	0.007	0.118*	0.013	0.039	0.026	-262.058***	2.694
	(0.035)	(0.049)	(0.055)	(0.040)	(0.054)	(0.047)	(0.042)	(0.038)	(57.070)	(63.175)
DV Mean (Pre)	0.270	0.296	0.184	0.128	0.743	0.720	0.100	0.120	463.994	485.942
Robust p-value	0.000	0.886	0.357	0.821	0.028	0.769	0.608	0.623	0.000	0.973
Bandwidth	99.77	125.62	63.64	104.63	64.60	146.35	69.76	140.77	68.96	113.42
Obs (Left)	854	810	562	676	568	988	599	948	595	740
Obs (Right)	670	747	453	631	459	882	482	853	478	698
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age Group	52-55	55-58	52-55	55-58	52-55	55-58	52-55	55-58	52-55	55-58

Panel B. Re-employment outcomes

	Δ Log(Wage)		Tenure		Separation in 6mo		Temporary Job		Changes Industry	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
After	0.007	0.053	111.988**	-98.934	-0.147**	-0.049	-0.032	0.023	-0.025	0.060
	(0.056)	(0.055)	(54.354)	(72.234)	(0.068)	(0.068)	(0.047)	(0.056)	(0.055)	(0.078)
DV Mean (Pre)	0.027	0.019	256.490	263.489	0.576	0.580	0.832	0.807	0.289	0.303
Robust p-value	0.824	0.320	0.046	0.156	0.029	0.649	0.647	0.652	0.560	0.648
Bandwidth	103.63	128.84	121.84	88.91	79.50	119.77	124.95	124.45	122.71	87.60
Obs (Left)	571	505	715	334	440	469	735	493	722	323
Obs (Right)	437	444	511	330	345	428	523	437	518	321
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age Group	52-55	55-58	52-55	55-58	52-55	55-58	52-55	55-58	52-55	55-58

Notes: RDD estimates obtained using the Stata command *rdrobust* by (Calonico et al. 2014a;b; 2017). The running variable is the date of UI exhaustion. The cutoff is July 15, 2012. Observations are weighted with a triangular kernel. All outcomes as previously defined. Controls include gender, family (at least one cohabitant), high school, urban, UI entitlement, tenure and log wage in the last job, province unemployment rate, and variation in the unemployment rate at the start of the spell and UI exhaustion. Conventional heteroskedasticity-robust standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B4: RDD results applying a 1-year bandwidth

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)
After	-0.262***	0.043	0.170***	-0.015	0.069**	0.003	0.073***	-0.012	-183.927***	25.474
	(0.017)	(0.025)	(0.022)	(0.019)	(0.023)	(0.027)	(0.019)	(0.021)	(24.046)	(30.683)
DV Mean (Pre)	0.270	0.296	0.184	0.128	0.743	0.720	0.100	0.120	463.994	485.942
Robust p-value	0.000	0.754	0.000	0.295	0.768	0.823	0.000	0.123	0.000	0.857
Bandwidth	365	365	365	365	365	365	365	365	365	365
Obs (Left)	4637	3296	4637	3296	4637	3296	4637	3296	4637	3296
Obs (Right)	2339	2062	2339	2062	2339	2062	2339	2062	2339	2062
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age Group	52-55	55-58	52-55	55-58	52-55	55-58	52-55	55-58	52-55	55-58

Panel B. Re-employment outcomes

	Δ Log(Wage)		Tenure		Separation in 6mo		Temporary Job		Changes Industry	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
After	-0.042	0.021	32.625	-39.825	-0.053*	-0.029	-0.011	0.012	-0.004	-0.042
	(0.028)	(0.031)	(27.842)	(30.778)	(0.030)	(0.035)	(0.025)	(0.028)	(0.030)	(0.035)
DV Mean (Pre)	0.027	0.019	256.490	263.489	0.576	0.580	0.832	0.807	0.289	0.303
Robust p-value	0.28	0.93	0.07	0.87	0.03	0.19	0.19	0.61	0.73	0.22
Bandwidth	365	365	365	365	365	365	365	365	365	365
Obs (Left)	3198	2227	3198	2227	3198	2227	3198	2227	3198	2227
Obs (Right)	1489	1191	1489	1191	1489	1191	1489	1191	1489	1191
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age Group	52-55	55-58	52-55	55-58	52-55	55-58	52-55	55-58	52-55	55-58

Notes: RDD estimates obtained using the Stata command *rdrobust* by (Calonico et al. 2014a;b; 2017). The running variable is the date of UI exhaustion. The cutoff is July 15, 2012. The bandwidth is manually set at 365 days. Observations are weighted with a uniform kernel. All outcomes as previously defined. Controls include gender, family (at least one cohabitant), high school, urban, UI entitlement, tenure and log wage in the last job, province unemployment rate, and variation in the unemployment rate at the start of the spell and UI exhaustion. Conventional heteroskedasticity-robust standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B5: RDD results – fiscal effects

Panel A. Spending in unemployment benefits

	Spending in 52/55yo Sub		Spending in Other UA	
	(1)	(2)	(3)	(4)
After	-4752.748***	-244.055	384.743	182.340
	(669.931)	(967.272)	(353.135)	(202.588)
Robust p-value	0.000	0.882	0.485	0.384
Bandwidth	102.93	120.47	84.59	145.56
Obs (Left)	759	775	629	973
Obs (Right)	591	728	494	876
Controls	Yes	Yes	Yes	Yes
Age Group	52-55	55-58	52-55	55-58

Panel B. Pension contribution savings and taxes raised

	Pension Contributions Savings		Taxes Raised	
	(1)	(2)	(3)	(4)
After	10120.869***	520.503	2006.267**	-608.669
	(1435.973)	(2057.753)	(998.089)	(570.573)
Robust p-value	0.000	0.874	0.041	0.331
Bandwidth	102.39	121.35	76.35	126.27
Obs (Left)	759	784	585	814
Obs (Right)	591	733	433	750
Controls	Yes	Yes	Yes	Yes
Age Group	52-55	55-58	52-55	55-58

Panel C. Total effect

	Total Savings	
	(1)	(2)
After	16420.645***	49.238
	(2561.908)	(3106.651)
Robust p-value	0.000	0.969
Bandwidth	80.74	122.48
Obs (Left)	600	787
Obs (Right)	471	737
Controls	Yes	Yes
Age Group	52-55	55-58

Notes: RDD estimates obtained using the Stata command *rdrobust* by (Calonico et al. 2017). The running variable is the date of UI exhaustion. The cutoff is July 15, 2012. Observations are weighted with a triangular kernel. *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. RDD estimates for *Spending in 52/55yo Sub* and *Pension Contribution Savings* cannot be obtained for the 49-52 age group due to lack of variability (no individuals exhausting UI after the reform receive the 52/55yo Subsidy). 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 (more than one cohabitant), high school, urban, UI entitlement, tenure and log wage in the last job, province unemployment rate, and variation in the unemployment rate at the start of the spell and UI exhaustion. Conventional heteroskedasticity-robust standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B6: RDD results – Ages 49-52

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)
After	NA	NA	-0.215***	-0.213***	0.055	0.049	-0.031	-0.032	-138.847***	-107.995***
			(0.048)	(0.044)	(0.034)	(0.032)	(0.030)	(0.029)	(36.094)	(31.661)
DV Mean (Pre)	0.012	0.012	0.383	0.383	0.841	0.841	0.125	0.125	302.336	302.336
Robust p-value	0.871	0.946	0.000	0.000	0.125	0.145	0.352	0.347	0.001	0.004
Bandwidth	62.45	64.43	74.83	70.46	96.10	99.10	108.48	108.47	68.16	76.21
Obs (Left)	715	741	833	791	1088	1111	1247	1247	779	881
Obs (Right)	729	748	831	802	1108	1135	1238	1238	787	858
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Age Group	49-52	49-52	49-52	49-52	49-52	49-52	49-52	49-52	49-52	49-52

Panel B. Re-employment outcomes

	Δ Log(Wage)		Tenure		Separation in 6mo		Temporary Job		Changes Industry	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
After	0.061	0.015	45.603	45.825	-0.185***	-0.130**	0.142***	0.046	-0.215***	-0.135***
	(0.041)	(0.036)	(34.094)	(34.495)	(0.058)	(0.053)	(0.050)	(0.035)	(0.064)	(0.051)
DV Mean (Pre)	-0.000	-0.000	236.438	236.438	0.603	0.603	0.846	0.846	0.298	0.298
Robust p-value	0.14	0.73	0.21	0.24	0.01	0.04	0.00	0.12	0.00	0.01
Bandwidth	139	135	126	112	79	83	55	89	57	79
Obs (Left)	1192	1162	1070	928	646	665	449	717	463	646
Obs (Right)	1139	1110	1049	932	692	715	477	770	492	692
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
Age Group	49-52	49-52	49-52	49-52	49-52	49-52	49-52	49-52	49-52	49-52

Notes: RDD estimates obtained using *rdrobust* Stata package (Calonico et al. 2014a;b; 2017). The running variable is the date of UI exhaustion. The cutoff is July 15, 2012. Observations are weighted with a triangular kernel. All outcomes as previously defined. Controls include gender, family (at least one cohabitant), high school, urban, UI entitlement, tenure and log wage in the last job, province unemployment rate, and variation in the unemployment rate at the start of the spell and UI exhaustion. Conventional heteroskedasticity-robust standard errors in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B7: Placebo reform

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	-0.040**	-0.045**	0.008	-0.002	0.022	0.029	-0.010	-0.016	-30.839	-32.314
	(0.020)	(0.018)	(0.018)	(0.016)	(0.022)	(0.020)	(0.016)	(0.016)	(25.499)	(22.911)
DV Mean (Pre-T)	0.196	0.196	0.157	0.157	0.771	0.771	0.104	0.104	378.449	378.449
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
R-sq	0.007	0.168	0.001	0.185	0.012	0.182	0.005	0.084	0.024	0.180
Observations	7220	7220	7220	7220	7220	7220	7220	7220	7220	7220

Panel B. Re-employment outcomes

	Δ Log(Wage)		Tenure		Separation in 6mo		Temporary Job		Changes Industry	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Age 52-55 × After	0.019	0.013	-9.210	8.490	0.029	0.016	-0.033	-0.047**	0.028	0.035
	(0.028)	(0.025)	(24.849)	(22.519)	(0.030)	(0.029)	(0.024)	(0.021)	(0.028)	(0.025)
DV Mean (Pre-T)	0.057	0.057	298.457	298.457	0.536	0.536	0.822	0.822	0.350	0.350
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
R-sq	0.000	0.291	0.007	0.186	0.000	0.103	0.015	0.257	0.011	0.191
Observations	4655	4655	4655	4655	4655	4655	4655	4655	4655	4655

Notes: The table provides β estimates from equation 1, where $After$ is now an indicator identifying individuals exhausting UI from July 15, 2008, to July 14, 2009. The sample is restricted to workers exhausting UI in a one-year window around July 15, 2008, the date of the placebo reform. Treated individuals are those aged 52-55 at UI exhaustion. Control individuals are those aged 55-58 at UI exhaustion. All outcomes as previously defined. 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. We follow individuals for 50 months after entering non-employment. Longer spells are censored. Heteroskedasticity-robust standard errors clustered at the province-quarter level in parentheses. $*p < 0.1$, $**p < 0.05$, $***p < 0.01$.

Table B8: Additional wage robustness

Panel A. No wage change for the unemployed and part-time jobs

	Δ Log(Wage) (Δ=0 if U)		Part-time Job	
	(1)	(2)	(3)	(4)
Age 52-55 × After	-0.023	-0.029**	0.004	-0.001
	(0.015)	(0.014)	(0.016)	(0.017)
Observations	11348	11348	8124	8124
Controls	No	Yes	No	Yes

Panel B. Re-employment wages as of July of a given year

	Δ Log(Wage)			
	(1)	(2)	(3)	(4)
Age 52-55 × After	-0.103**	-0.112***	-0.094**	-0.089**
	(0.041)	(0.039)	(0.038)	(0.037)
Observations	3077	3393	3645	3653
Controls	Yes	Yes	Yes	Yes
Year	2013	2014	2015	2016

Panel C. Lee bounds

	Δ Log(Wage)			
	(1)	(2)	(3)	(4)
Lower Bound	-0.134***	-0.155***	-0.146***	-0.138***
	(0.028)	(0.028)	(0.029)	(0.036)
Upper Bound	0.065**	0.012	0.004	0.019
	(0.027)	(0.029)	(0.031)	(0.044)
Observations	4058	4058	4058	4058
Tightening:				
Low Wage	No	Yes	Yes	Yes
Last contract	No	No	Yes	Yes
Education	No	No	No	Yes

Notes: $\Delta(Wage)$ *($\Delta = 0$ if non-employed)* measures changes in daily log re-employment wages assigning a change of 0 to workers still non-employed at the end of 50 months in our sample. *Part-time Job* is an indicator taking the value of 1 if the worker works less than 30 hours in the new job. Δ*Log(Wage)* is the change in (log) wage from the individual's last job. 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. Lee bounds are estimated with Stata command *leebounds* (Lee 2009, Tauchmann 2014). $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

Table B9: DD results excluding workers potentially affected by the 2013 change in UA eligibility conditions

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	-0.301***	-0.288***	0.181***	0.195***	0.098***	0.095***	0.059***	0.062***	-175.183***	-176.028***
	(0.017)	(0.017)	(0.018)	(0.018)	(0.021)	(0.019)	(0.016)	(0.015)	(23.824)	(21.595)
DV Mean (Pre-T)	0.266	0.266	0.188	0.188	0.743	0.743	0.101	0.101	460.490	460.490
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
R-sq	0.050	0.168	0.045	0.154	0.008	0.099	0.012	0.066	0.013	0.129
Observations	10317	10317	10317	10317	10317	10317	10317	10317	10317	10317

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	-0.041	-0.081***	17.651	6.171	-0.019	-0.001	-0.004	0.001	0.008	0.007
	(0.026)	(0.024)	(24.328)	(22.939)	(0.025)	(0.024)	(0.023)	(0.021)	(0.027)	(0.027)
DV Mean (Pre-T)	0.026	0.026	254.460	254.460	0.577	0.577	0.838	0.838	0.286	0.286
Controls	No	Yes	No	Yes	No	Yes	No	Yes	No	Yes
R-sq	0.001	0.251	0.004	0.135	0.000	0.126	0.008	0.235	0.007	0.116
Observations	6815	6815	6815	6815	6815	6815	6815	6815	6815	6815

Notes: This table replicates baseline DD results excluding workers with at least one cohabitant with expected UI exhaustion date after March 17, 2013. Treated individuals are those aged 52-55 at UI exhaustion. Control individuals are those aged 55-58 at UI exhaustion. All outcomes and control variables as previously defined. Robust standard errors clustered at the province-quarter level in parentheses. $^{*}p < 0.1$, $^{**}p < 0.05$, $^{***}p < 0.01$.

C Data appendix

C.1 Structure of the MCVL

The *Muestra Continua de Vidas Laborales* (MCVL) records the entire employment histories for a 4% random sample of the Spanish population. The data is divided into six tables. They can all be linked using an anonymized individual identifier. Table 1 records demographic information (gender, birth date, province of origin, education). Table 2 records the employment history in spell format, with exact start and end dates. Table 3 records the history of social security employment contributions. Contributions generally correspond to wages and are used to calculate UI amounts and pensions. Table 4 contains information about pensions (retirement and disability). Table 5 contains information of cohabitants. Table 6 contains tax information. A more detailed description of the data is available on the Spanish Social Security website (in Spanish only).

C.2 Data cleaning

Gaps: Sometimes there are gaps between the spells recorded in MCVL Table 2. For example, between the time when an individual exhausts UI and the date of finding the next job. We are certain when individuals work. Therefore, we treat all gaps as non-employment. On any given date, an individual is either in employment or non-employment.

Termination of a non-employment spell: A non-employment spell can terminate with either: (1) the individual finding a job; (2) the individual exiting the labor force (as defined below or with retirement); (3) the spell being censored (after 50 months of non-employment, or in December 2017). These are mutually-exclusive categories.

Overlapping spells: If individuals work more than one job at a time, MCVL Table 2 will record an entry for every job. These situations could pose complications when, for example, computing UI entitlement (risk of double-counting days). Since our focus is on non-employment spells, we impose that employment spells are non-overlapping by modifying the start and end dates accordingly. In these modifications, the longest spell takes priority. Thus, a completely overlapped spell (e.g., an individual working a one-week job while having another job before and after that week) would be disregarded.

Working while receiving unemployment benefits: A worker in UI can work part-time and still receive a proportional amount of UI (e.g., 50% if working half of the regular working hours). A worker can also receive the Subsidy or Other UA if she satisfies the "the lack of income" condition (e.g., earnings below €480 in 2012). These situations would show in the raw data as "overlapped" employed spells within an unemployment spell (see the previous discussion). We treat individuals in this situation as non-employed. We observe that 3.8% of the spells from July 2010 to July 2012 are overlapped.

C.3 Variables description

Receives the 52/55yo Subsidy: The MCVL records 52/55yo Subsidy spells when the variable "type of labor relation" (MCVL Table 2) takes the values of 753 or 754. A complication is that individuals in a "permanent discontinuous" labor relation (*fijos discontinuos*) are lumped in these codes,[5] but we identified them following the instructions we received from the social security administration. Most notably, the "type of contract" in the work spell before the non-employment spell has a code ranging from 300 to 389. The variable "Receives 52/55yo Sub" is an indicator taking the value of 1 if the individual receives the subsidy during the non-employment spell.

Receives Other UA: The MCVL records Other UA spells with the variable "type of labor relation" (MCVL Table 2) takes the values of 755 or 756. It is not possible to distinguish between the different types of UA programs described in Table A2 (except for the 52/55yo Subsidy). The variable "Receives Other UA" is an indicator taking the value of 1 if the individual receives a UA subsidy (except the 52/55yo Subsidy) during the non-employment spell.

Exits to Employment: An indicator taking the value of 1 if the non-employment spell terminates with the individual finding a job.

Exits labor force/Welfare: We say that an individual exits the labor force if she has not worked or received any observed subsidy (UI, the 52/55yo Subsidy, or Other UA) for more than 365 days. A caveat is that an individual could be receiving an unobserved subsidy (e.g., the RAI), during which she is officially registered as an active employment seeker. The variable "Exits Labor Force" is an indicator taking the value of 1 if a non-employment spell terminates with the individual exiting the labor force as defined above.

Non-employment duration: We construct the variable non-employment duration by subtracting the start date of an employment spell from the end date of the previous employment

[5]These are workers with stable but discontinuous jobs in terms of hours worked. School bus drivers, who only work certain days during the school year, are an example.

spell. During a non-employment spell, an individual can be receiving UI, UA, some unobserved benefit (e.g., the RAI), or nothing.

Last (daily) wage: We construct the daily wage in the last job by taking the monthly contribution (MCVL Table 3) in the month before entering non-employment (e.g., April if the individual lost her job in May) and dividing it by 30. We scale that number with the part-time coefficient (MCVL Table 2) to consider workers in part-time employment (e.g., if a worker works 50% of a regular job, we multiply earnings by two to get full-time equivalent earnings). We assign the contribution in the month before separation to avoid underestimating the wage, for example, due to a contract terminating in the middle of the month. If the worker did not work the entire month before losing the job, then we compare the days worked in that month and the month of job separation, and we rescale earnings appropriately by the number of days worked. Monthly contributions are bottom and top-coded. The minimum contribution from 2010 to 2017 increased from 738.90 to 825.60 euros.[6] The maximum contribution in the same period increased from 3,198 to 3,751.20 euros. Given our sample's relatively high proportion of low-wage workers (the median monthly wage is approximately €1,400), we are not concerned about top-coding being a problem. We do not take any specific action to address it.

Next (daily) wage: We follow an analogous approach as before. By default, we take the monthly contributions of the first month following re-employment, divide them by 30, and scale them by the part-time coefficient. If the worker does not complete an entire month in her new job, then we take the earnings of the month with more days worked and rescale appropriately by the number of days worked. This variable is non-missing conditional on finding a job. The previous discussion on wage censoring applies.

Log wage difference: We subtract the logarithm of "next wage" from the logarithm of "last wage."

Tenure: We define tenure (in the next job) by counting the days between the start and end date of the first employment spell following non-employment. "Tenure in the last job" is defined analogously.

Separation in 6mo: An indicator taking the value of 1 if tenure in the next job is below 180 days.

[6]These amounts are very close to the current minimum wage of the time.

Temporary job: An indicator taking the value of 1 if the "type of contract" (MCVL Table 2) in the first employment spell following non-employment ranges from 400 to 600. In the Spanish context, a temporary job is a job with a fixed-term contract.

Industry change: An indicator taking the value of 1 if the industry of the first and last employment spell following non-employment does not coincide. Industries are grouped into 21 broad categories (CNAE-2009).

DI after unemployment: An indicator taking the value of 1 if the worker claims a disability pension at some point after entering non-employment. We obtain the date of disability concession from Table 4 in the MCVL (Pensions).

Exits LF/Welfare and does not work again: An indicator taking the value of 1 if the worker leaves the labor force (as previously defined) *and* does not find a job in the 50 months we follow her.

UI entitlement: We estimate the amount of UI entitlement at the start of every spell following the current legislation (from 1992). We first count the number of days of employment in the past six years (2160 days). Then we assign the theoretical amount of UI entitlement following Table A1. A complication is that UI is "stored" (for up to six years) if not fully exhausted during a UI spell. Therefore, we have to keep track of the unused UI throughout the individuals' employment history. At the start of every spell, we say that an individual's UI entitlement is the maximum between the amount resulting from the work history in the previous six years, and the unused UI from previous UI spells.

Family: An indicator taking the value of 1 if the individual has at least one cohabitant.

High school: An indicator taking the value of 1 if the individual has at least a high school (or equivalent) degree (codes 40 to 98 in the variable "education" from MCVL Table 1).

Urban: An indicator taking the value of 1 if the individual resides in a municipality with at least 40,000 inhabitants. We choose this threshold because the MCVL only records the exact municipality of residence if that is larger than that amount. If smaller, the MCVL only records the province of residence.

Quarterly unemployment rates: We estimate quarterly unemployment rates by dividing the number of unemployed individuals (in UI or UA) over those employed in the week before the start of the quarter.

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