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The Medium-Term Effects of Unemployment Benefits*

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Abstract

Although there is extensive literature on the short-term effects of unemployment benefits, little is known about their medium-term implications. In this paper, I use rich and novel administrative data from Italy to study the effects of potential benefit duration on aggregate outcomes over 4 years after layoff. To obtain causal estimates, the identification exploits an age at layoff rule which determines a 4 month increase in potential benefit duration if the worker is fired after turning 50 years old. Workers with longer potential benefit duration spend more time on unemployment benefits and in nonemployment before finding a new job. They are also slightly more likely to find a job with permanent contract. Over the 4 years following layoff, however, the difference in time spent in nonemployment between workers with shorter and longer benefits is substantially reduced. A higher probability of employment in the period following the first reemployment date contributes to explain this discrepancy. These findings are important from a policy perspective as they suggest that classical measures of the cost of unemployment benefits tend to overestimate the negative externalities of potential benefit duration. This, in turn, leads to underestimate the optimal duration of unemployment benefits.

JEL classification: E24; H24; H55; J65

Keywords: Unemployment insurance; post-unemployment wages; regression discontinuity design; medium term outcomes.

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1 Sintesi Non-Tecnica

I sussidi di disoccupazione rappresentano uno degli elementi principali del moderno *Welfare State*. Essi svolgono l'importante funzione di fornire a lavoratori che hanno perso la loro occupazione, un reddito temporaneo con cui sostenere il proprio consumo. Tuttavia, la presenza di questo reddito porta i lavoratori a esercitare minore sforzo nella ricerca di un lavoro, con effetti negativi sulla finanza pubblica. Il bilanciamento di questi effetti è la base per la definizione di una politica economica ottimale come evidenziato dai contributi teorici di Baily (1978) e Chetty (2006).

In genere, la stima dei costi dei sussidi di disoccupazione, in termini di minore livello di occupazione, si basa su quanto tempo in più un lavoratore con sussidi più generosi spende in non occupazione prima di trovare lavoro. Questo tipo di analisi, tuttavia, trascura altri aspetti importanti: da un lato, i lavoratori che spendono più tempo non in occupazione potrebbero avere un rischio maggiore di tornare disoccupati; dal altro, questi lavoratori potrebbero avere una maggiore abilità nel cercare lavoro nel medio-lungo periodo. Non considerare questi effetti può portare a sovrastimare o sottostimare i costi dei sussidi di disoccupazione, con conseguenti ricadute in termini di generosità del sistema.

Questo contributo mira a fornire una stima di medio periodo dei costi in termini di non occupazione di una più lunga durata dei sussidi di disoccupazione. Lo studio si basa sui dati sull'universo di coloro che ricevono il sussidio di disoccupazione ordinaria a requisiti normali tra il 2009 ed il 2012. In modo da ottenere stime causali, questo lavoro sfrutta una discontinuità nella durata del sussidio determinata dall'età del lavoratore al licenziamento: i lavoratori licenziati dopo i loro 50 anni avevano diritto a 12 mesi di sussidio mentre gli altri ne avevano diritto per 8 mesi. Questa variazione esogena nella durata dei sussidi viene utilizzata in un *Regression Discontinuity Design*.

Gli effetti dei sussidi di disoccupazione sono notevoli e i lavoratori che hanno diritto ad un sussidio di più lunga durata ricevono il sussidio per 8 settimane in più e impiegano 6,2 settimane in più prima di trovare un nuovo posto di lavoro. Questa differenza, tuttavia, scende a solo 2 settimane nell'arco di 4 anni. Contrariamente a quanto si attenderebbe, le caratteristiche del primo lavoro dopo la disoccupazione (tipo contratto, durata e salari) non sembrano giocare un ruolo rilevante. Al contrario, due elementi spiegano la differenza tra queste due stime: da un lato, numerosi lavoratori che ricevono un sussidio di disoccupazione perdono nuovamente il lavoro dopo non molto tempo; dall'altro, i lavoratori con un sussidio di maggiore durata hanno una maggiore probabilità di trovare una seconda occupazione dopo il loro primo lavoro post sussidio. Questo secondo effetto appare particolarmente interessante in quanto potrebbe suggerire che questi lavoratori hanno ottenuto vantaggi dal loro più lungo periodo di ricerca di lavoro. Ulteriori analisi sono, tuttavia, necessarie per escludere spiegazioni alternative (per esempio, il fatto che questi lavoratori potrebbero avere una minore possibilità di ricevere nuovamente il sussidio).

In conclusione, questo lavoro porta due contributi principali. In primo luogo, esso mostra che le classiche stime sui costi dei sussidi di disoccupazione tendono a sovrastimare i costi dei sussidi in termini di disoccupazione. Questo suggerisce che potrebbe essere opportuno aumentare la generosità del sussidio con perdite limitate in termini di efficienza. In secondo luogo, le presenti stime forniscono le prime inidcazioni dei costi dei sussidi di disoccupazione con dati concernenti l'universo dei riceventi. In tal senso questo lavoro arricchische un precedente lavoro di Rosolia and Sestito (2012), basato su un campione dei riceventi e su una riforma del 2001 nella struttura dei sussidi. Queste stime appaiono particolarmente importanti alla luce dell'intensa attività legislativa degli ultimi anni che ha fortemente mutato la struttura e la generosità dei sussidi di disoccupazione.

2 Introduction

Unemployment benefits play a central role in the modern welfare state and are present in most developed and developing economies. Their primary role is to support workers' income in periods of unemployment, thus reducing poverty and preventing sharp declines in consumption. However, they also generate fiscal costs and deadweight losses to the extent that workers respond to more generous benefits by spending more time in unemployment. These two components (insurance and negative fiscal externalities) constitute the building block to assess the optimality of the policy (Baily, 1978 and Chetty, 2006). In order to assess the effects of unemployment benefits on the behavior of workers, researchers usually focus on the duration of the nonemployment spell following layoff, but this can provide only a partial picture of the effect of unemployment benefits. On the one hand, human capital depreciation and scarring might make it more difficult for workers to move to a different job or increase the time spent in nonemployment in future transitions between two jobs. On the other hand, workers might exploit the longer period in nonemployment to gain search experience and later move more quickly between jobs. The combined magnitude and sign of these additional effects is an open empirical question which so far has received limited attention.

In this paper, I aim to fill this gap in the literature by investigating the effects of potential benefit duration over an extended period of time. More specifically, I look at the effects of different potential benefit duration on employment, earnings and transfers over a 4 year period after layoff. To this purpose, I use rich and novel administrative data from Italy on the universe of recipients of unemployment benefits and of private sector working histories. The identification of the causal effects relies on a quasi-experimental variation in potential benefit duration induced by age-at-layoff based rule: workers who were fired after turning 50 years of age were eligible to 12 months of potential benefit duration whereas other workers were eligible to 8 months of subsidy. I exploit this variation in a sharp Regression Discontinuity Design strategy. I find that longer potential benefit duration increases the duration of the first nonemployment spell after layoff: longer potential benefit duration leads to longer periods receiving benefits and a longer time to find a new job (by 8 and 6.2 weeks respectively). It also has a positive effect on next job quality with a small, positive and significant increase in the probability of finding a permanent job. The effects on other job characteristics, most notably wages and tenure in the next job, are small, positive but not statistically significant. Over 4 years, however, workers with longer benefits spend in nonemployment only 2 weeks more than workers with shorter benefit duration. Moreover, after their first spell on benefits, they also spend 2 weeks less on unemployment benefits. This discrepancy is determined by two different elements: first, workers with longer benefit duration find a second job quicker than workers with shorter benefit duration; secondly, workers with shorter benefit duration are more affected by cyclical labor market conditions. This second effect represents about three quarters of the total reduction in period spent in nonemployment. Gains in terms of better match between the worker and the first firm do not play a central role: longer potential benefits duration has only a small and not significant effect on the duration of the new job. These results are robust to a wide range of robustness and identification checks. My results also show that effects of potential benefit duration are widely heterogenous across workers, with stronger effects for workers from small firms and with permanent contracts. These results are important from a policy perspective as they hint at an additional channel through which unemployment benefit duration affects workers' career: by inducing search effort at different point of the seasonal demand cycle, potential duration can lead to different employment patterns and outcome which can offset the initial employment gains for workers with shorter benefits.

This paper contributes to an extensive literature on the effects of unemployment benefits. There is a general consensus on the positive effect of potential benefit duration (Card et al., 2007a, Lalive, 2007, Caliendo et al., 2013, Le Barbanchon, 2016, Schmieder et al., 2016, Nekoei and Weber, 2017) on nonemployment duration after layoff, although there is substantial variation in the exact magnitude of this effect: coefficients range from close zero (Lalive, 2007, for small increase in potential benefit duration; Nekoei and Weber, 2017) up to 0.6 (Lalive, 2007 for women with large increase in PBD). Results on the effects on post unemployment job quality are in general mixed with often insignificant effects (Card et al. (2007a) and Van Ours and Vodopivec (2006)) with two main exceptions with contrasting results: Schmieder et al. (2016) find small negative effects on future wages while Nekoei and Weber (2017) identify small positive effects. Several works also estimated the effects of unemployment benefits generosity on nonemployment duration. Lalive et al. (2006) exploit a policy change in Austria in 1989 and investigate the effects of changes in generosity and potential benefit duration. They find the effect of higher generosity to be negligible with respect to the effects of extended benefit duration. More recent contributions, implementing regression kink design, show that higher generosity leads to a longer period receiving unemployment benefits (Card et al., 2015) and to longer nonemployment spells (Britto, 2016, and Landais, 2015). Despite this vast literature, we only have limited evidence on the medium run effects of unemployment benefits. Degen and Lalive (2013) use a difference in difference strategy to assess the effect of a reduction of potential benefit duration for workers with more than 55 years of age at layoff. They find that a reduction in potential negative duration has long lasting positive effects on employment. More recently, Kyyrä and Pesola (2017) use Swedish data and exploit a reform in 2005 which postponed the age at which worker can obtain longer unemployment benefits to workers born after 1950. They find that a postponement in the age at which workers can collect extended benefits leads to 9% increase in months worked and labor income. Due to the direct interaction with early retirement schemes, their results are less informative for workers at different points in the age distribution. Schmieder et al. (2012b) is the closest contribution to my work as they exploit an age based discontinuity in potential benefit duration to study the effects of unemployment benefits in the medium run. They find that the difference in time spent in nonemployment between the two groups is substantially reduced when they consider time spent in nonemployment over 5 years. However, they also find that the discrepancy in time spent receiving subsidies increases over 5 years which makes it difficult to assess the overall effects on negative externalities and public finances. The limited evidence on the effects of unemployment in the medium run (Schmieder and Von Wachter, 2016) makes this topic particularly salient from a research perspective.

This paper contributes to the literature in a twofold direction. First, it provides evidence on the medium term effects of unemployment benefits and shows that workers with longer benefit duration have a higher probability of remaining in employment after finding a job. This finding is crucial from a policy perspective as it suggests that standard measures of the negative externalities of unemployment benefits overestimate the actual costs of longer potential benefit duration. Second, it provides the first causal estimates for the effect of unemployment benefits in the Italian context with large administrative data. In a previous work, Rosolia and Sestito (2012) implement a difference in difference strategy and exploit a policy change in 2001 to evaluate the effect of potential benefit duration and generosity in Italy. However, their sample is limited, and their identification might suffer from changes in business cycle conditions. Interestingly, their estimates are lower but still reasonably close to the ones of the present work.

The rest of paper is structured as follows: Section 4 provides a description of the data and of the sample construction; Section 3 describes the Italian Institutional setting; Section 5 outlines the identification strategy and methodological approach; Section 6 reports the main results and discusses the mechanism at work; Section 7 looks at heterogeneous effects and extensions; Section 8 implements a series of robustness checks and finally Section 9 concludes.

3 Institutional Context

In this study, I use the main subsidy which characterized the Italian Welfare system from before World War II up to 2013, the subsidy for Ordinary Unemployment with Normal Requirement (*Disoccupazione Ordinaria a Requisiti Normali*, OUNR throughout the rest of the paper). This subsidy, introduced at the eve of World War II (*Regio Decreto 14th April 1939*), was progressively extended in both coverage and generosity and, by the start of the new millenium, it covered all employees in the non-agricultural sector.[1] The structure and generosity of the subsidy was modified several times over the years but in the period under study, from 2009 to 2012, it was similar to the one of many other subsidies in European economies (Austria, Nekoei and Weber, 2017; Germany, Caliendo et al., 2013 and Schmieder et al., 2012a). In this period, the potential benefit duration was fully determined by the age at layoff with a threshold mechanism: workers fired before turning 50 were eligible to 8 months of unemployment benefits (34.64 weeks or 241 days)[2] while workers fired after turning 50 received up 12 months of subsidy (52 weeks or 365 days). The amount of the subsidy was proportional to average wages in the 3 months preceding layoff. Workers received 60% of their average wage for the first 6 months of the subsidy, 50% for the following 2 months and 40% for the remaining 4 months, if still eligible. The amount of the subsidy was capped by law and the threshold changed every year.[3]

In terms of eligibility, workers needed to meet two main requirements: the worker should have contributed for the first time to social security at least 2 years before the start of the unemployment benefit spell; the worker should have worked for at least 52 weeks in the last 2 calendar years. Not all workers separating from a firm were entitled to receive the subsidy: differently from other settings, such as the Austrian one (Jäger et al., 2018), workers who quit their job are not entitled to unemployment benefits while workers who are fired for economic reasons, who had to leave the firm due to end of the contract, or who quit for just cause (i.e. harassment or unpaid wage) were eligible. Workers also needed to meet a monthly equivalent minimum wage requirement for each contribution which was proportional to the minimum pension amount (40% or about 192 euro for 2012). The duration and generosity of the subsidy was revised several times over the years. In the paper, I use data for the period between 2009 and 2012 due to data availability and

[1] The *parasubordinati*, workers with usually exclusive contracts for specific tasks and projects with a firm, are categorized as self-employed and they were not eligible to the subsidy. A new subsidy was introduced for them in 2015 (*DIS-COL*).

[2] for the rest of the paper, I follow the convention that one month corresponds to 4.33 weeks.

[3] Over the period considered the maximum amount increased from 1065.26 euros per month in 2009 up to 1119.32 in 2012.

as it allows for a uniform institutional framework.

It should be noted that two additional subsidies, subsidy for Ordinary Unemployment with Reduced Requirement (*Disoccupazione Ordinaria a Requisiti Ridotti*) and the Mobility Subsidy, were available to unemployed workers but they are unlikely to severely bias our estimates. The former was a subsidy with lower requirement (13 weeks in the last year but still 2 years since first contribution) but it was substantially less generous. In addition, the subsidy could be requested only the year after the period of unemployment which made it less attractive with respect to the main one under study. The second one was substantially more generous, but it was also characterized by more stringent access conditions. Indeed, workers needed to have a permanent contract and a minimum tenure at the moment of layoff, and to be fired in a collective dismissal. In addition, the firm had to belong to specific sectors, satisfy some size requirements and the state of economic difficulty, which allowed for collective layoff, had to be certified before workers can apply to receive this subsidy. The strong conditionality and the presence of numerous exogenous constraints made it unlikely that workers self-select into the subsidies. The presence of this subsidy is then unlikely to generate substantial bias due to selection. I provide additional details and discussion about these two subsidies in appendix A.

4 Data and sample

The analysis is based on two main sources: the register for the universe of workers receiving unemployment benefits and the universe of working histories in the private sector.

The former (*SIP*, *Sistema Informativo Percettori*) collects information on unemployment benefits administered by the social security and provides information on the start date, the duration and the amount of the subsidy. The dataset also provides several characteristics of the last employment such as the firm identifier, the type of contract, etc. Due to the reorganization of the social security archives, this data source covers only the period after 2009 which leaves me with a sample period from 2009 to 2012 as the subsidy was later abolished and substituted.[4] The latter (the *Uniemens* dataset) is the archive for the mandatory communications that firms make to Social Security for pension contributions. The dataset is collected at monthly frequency[5] and it contains detailed information on firm workforce composition at worker level, with information on wages, type of contracts, number of days worked, broad occupation classification, and job location at municipality level.

For the analysis, I focus on individuals fired between 46 and 54 years of age and who collect the OUNR. I exclude observations with missing end date for the unemployment benefit and for which it was not possible to match their previous employer with UNIEMENS data. This is related to the presence of workers fired from the public sector and most notably from schools.[6] In addition, I also exclude all the observations concerning workers suspended for a temporary slowdown in the economic activity as, in their case, the subsidy has a different structure and they still keep a close relationship with their previous employer.[7] The final dataset contains 452,888 spells for 353,796 workers.

As the dataset on subsidies does not provide information on the date of first employment after layoff, I derive my measure of time to the next job from the social security records and I define the period of nonemployment as the distance in weeks between the day of layoff and the day of the start of the first contract after the end of the unemployment benefits. This choice aims to overcome possible issues related to short and low paying jobs which might be compatible with unemployment benefits (maximum 5 days of continuous duration). If there is no start of employment after the end of the unemployment benefit, I consider the first start employment date after the end of the last job. This correction involves only a marginal number of observations (about 1,000). A

[4]Data on previous years would provide a relatively small contribution as the structure of the subsidy changed at the start of 2008.This leaves me with a maximum uniform legislative framework from January 2008 to December 2012.

[5]Data is available at monthly level from 2005 but it is available at a yearly frequency from the early 70s. I rely on the annual version for the construction of several variables such as the tenure of the worker in months.

[6]The high share of teachers on temporary contracts creates regular flows towards unemployment benefits in correspondence of the end of the academic year (June) About 50% of the unmatched workers come from the education sector. These workers are unlikely to reflect classical employment dynamics in the private sector and their exclusion should not be problematic.

[7]I provide additional information on the sample definition in appendix B.

limitation of the data is that it does not cover possible transitions towards self-employment or public employment. These transitions are unlikely for workers employed in the private sector in the late stage of their career and their exclusion should not substantially affect my results. I report in the appendix C.2, checks using social security contributions to assess the sensitivity of my results to these transitions. Throughout the paper, I will rely on days of nonemployment in the private sector for the definition of my main dependent variable as information on unemployment is not available in the Social Security archives. Moreover, this can provide an imprecise measure of the labor status of workers as transitions towards outside the labor force are common after the end of the period receiving unemployment benefits (Card et al., 2007b). Finally, if the worker does not find any job up to the end of the observation period (December 2016), I report the time elapsed from the date of layoff up to the end of our sample (this concerns 10% of the sample for temporary contract and 20% for permanent contract, or about 60,000 spells). Throughout the analysis, I will censor duration to 4 years in order to have a common time horizon for all workers in my sample. Table 1 reports summary statistics for my final sample.

Workers spend on average 26 weeks on benefits after layoff but spend much longer (85 weeks with the uncensored measure and 65 with the censored counterpart) in nonemployment before finding a new job. About 60% of the workers find a new job within the first year from layoff, but about one third of them does not find a job after one year and half. Recalls are rather frequent and are more common for workers coming from temporary contracts (about 50%) rather than for workers coming from permanent contracts (20%). This suggests that for workers from temporary contracts periods of nonemployment are, at least in part, a normal component of their employment relation. Most of the recipient are male, full time and blue collar and about half comes from permanent contracts. Workers come from relatively small firms which is consistent with the high share of small firms in the Italian economy. This is also consistent with the presence of alternative subsidies for workers coming from large firms under certain circumstances[8] and with more rigid employment protection legislation for larger firms. Indeed, the possibility not only of

[8]See appendix A.

monetary compensation but also of reintegration for workers fired without just cause (economic or disciplinary) created high level of cost uncertainty for firms. On average workers have 1.28 spells starting between February 2009 and December 2012, mostly due to the frequent transition towards nonemployment of workers with temporary contracts at the end of their contract.

5 Methodology

The identification of the causal effects of treatment relies on the identification of an exogenous variation in the potential benefit duration. I exploit the structure of the potential benefit duration with respect to age at layoff in a regression discontinuity design (RDD) in line with the seminal paper by Lalive (2007) and more recent contributions such as Nekoei and Weber (2017). In practice, as workers who were fired after turning 50 years of age received 4 additional months of potential benefit duration, I will compare individuals fired around the 50 years of age at layoff threshold. Under the identifying assumption that individuals are fired randomly before or after the 50 years threshold, the two groups should have similar characteristics and the strategy should allow to identify the causal effects of longer potential potential benefit duration. I estimate the following equation:

$$y_{ist} = \beta_0 + \beta_1 I(\widetilde{Age}_{it} \geq 0) + \sum_{j=1}^{k} \gamma_j \widetilde{Age}_{it}^{j} + \sum_{j=1}^{k} \delta_j \widetilde{Age}_{it}^{j} X I(\widetilde{Age}_{it} \geq 0) + \\ + X_{it}\pi + \mu_{st} + \epsilon_{ist} \tag{1}$$

where the outcome of interest (y_{ist}) for individual i, who was fired in local labor market s at time t, is regressed on a k^{th} order polynomial for age at layoff in deviation from the 50 years of age threshold ($\widetilde{Age}_{it}$) with different slopes on the two sides of the cutoff and on a dummy for the individual being laid off after turning 50 ($I(Age_{it} \geq 50)$).

Our coefficient of interest is β_1, which identifies the effect of the longer potential benefit duration. Main results are based on a second order polynomial but findings are robust to different parametric choices and estimation as shown in Section 8. The model also includes a rich set of controls for the previous occupation of the worker (X_{it}), such as the share of permanent workers in the previous firm, the age of the last firm, the (log) size of the last firm, if the previous contract was full time or not, gender, the average daily wage in the past 6 months in the firm, market potential experience, tenure in the firm, tenure as a fixed term, the number of months worked in the last seven months, occupation and sector of previous occupation dummies at two digits level (NACE 2007 classification). I also include fixed effects at month and local labor market (μ_{st}) to flexibly control for local economic cycles and seasonality. In the estimation, I will then compare workers who are fired before and after turning 50 in the same month and local labor market.

As mentioned above, this strategy allows to identify the causal effects of an increase in the duration of unemployment benefit under the assumption that workers on the two sides of the cutoff are comparable. To check this assumption, I first check whether workers are able to sort around the cutoff in order to obtain longer benefits and, then, I assess whether observable characteristics show discontinuities at the cutoff.

First, I plot the density of the layoff by age in months in Figure 1. It can be easily seen that workers are indeed able to influence their date at layoff to self-select to the right side of the threshold if their original layoff date was sufficiently close to the threshold. The McCrary test confirms the presence of a discontinuity and strongly rejects the null of continuity of the distribution at the threshold. I explore the determinants and implications of this strategic delay in a related work (Citino et al., 2018). To overcome this issue while keeping the comparison to individuals with similar age, I implement a donut regression discontinuity design in the spirit of Barreca et al. (2011) excluding the first bin before and after the cutoff which are more influenced by manipulation. I also perform several robustness checks in Section 8 and results are largely in line with the main specification.

Then, I check for possible discontinuities in observables at the cutoff. I plot the average of observables by age in months at layoff around the cutoff of 50 years of age in Figure 2. In most of the cases, observables are reasonably continuous at cutoff despite the strategic behavior of workers but there are sizeable jumps in a few instances. To assess the magnitude of these discontinuities and to what extent my strategy can mitigate the problem, I replicate the above analysis in a regression framework: I regress the observables on a square polynomial in age, flexible on the two sides of the cutoff, and on the set of interacted months and LLM fixed effects. Table 2 reports the estimates of this exercise. The simple RDD with the rich set of fixed effects seems unable to capture all the sorting at the cutoff and several variables show highly statistically significant but small jumps: workers on right of the cutoff are more likely to be women, to have a white collar job, a permanent contract, lower tenure in temporary contract, to come from smaller firms, and from firms with a larger share of permanent workers. However, all the discontinuities but one are no longer detectable once the two bins closer to the cutoff are removed. It is worth pointing out that this result is mostly determined by a lower coefficient rather than by increased variance of the estimates which provides evidence in favor of the ability of this strategy to remove the most problematic observations. There is still a small difference in tenure with temporary contract, but the discontinuity is rather limited. These findings seem to suggest that the exclusion of the two bins closest to the cutoff solves several issues concerning strategic sorting around the cutoff.

6 Results

6.1 Effects on benefit and nonemployment

I start by looking at the effects of longer potential benefit on the nonemployment spell immediately following layoff. As a first step, I visually inspect whether the 4 additional months of potential benefit duration lead to a longer period collecting unemployment benefits and nonemployment spell. Both measures are relevant from a policy perspective: the first provides a measure of the direct effects of the potential duration on public expenditure through longer benefit duration; the second characterizes the unemployed behavioral response. Figure 3 plots the average number of weeks receiving the subsidy by age in months at the moment of layoff. The plot shows a clear jump at the cutoff of about 8 weeks. This discontinuity points at an increase in costs for the government due to the longer potential duration, but it is less informative about the overall change in behavior by the workers. Indeed, this effect combines two different components: the mechanical response related to the fact that a longer part of the nonemployment spell is now covered by unemployment benefits; the behavioral response which represents the true change in behavior for individuals eligible for a longer potential benefit duration. In order to identify the latter, I now move to the number of weeks of nonemployment reported in Figure 4. Also in this case, we can observe a neat jump at 50 years of age at layoff although the size is smaller. The average number of weeks in nonemployment increases by about 6.5 weeks with 4 additional months of potential benefit duration.

I verify quantitatively these findings in the regression framework outlined in equation 1. Results are reported in Table 3 and 4. The coefficients confirm that the longer potential benefit duration leads to longer benefit and nonemployment duration. The effects for the duration of the benefit is very stable across specifications and largely confirms the visual inspection: 4 additional months of potential benefit duration lead to 8 additional weeks of benefit or 0.46 additional week per week of potential duration.[9] The baseline model in Column (1), includes a quadratic polynomial in the running variables with different slopes on the two sides of the threshold. Column (2) includes a wide set of controls for the worker and previous job characteristics, Columns (3) and (4) include month fixed effects and local labor market fixed effects[10] and finally Column (5) includes local labor market interacted with monthly fixed effects. This will be the preferred specification for the rest of the paper. This effect represent a 36% increase over the baseline of 22 weeks.[11] Column (6) and (7) report the effect on the total amount of the benefit and point at an increase in

[9] The increase in potential benefit duration by 4 months corresponds to an increase of 17.32 weeks.

[10] The Italian National Institute for Statistics (ISTAT) defines LLM every 10 years. For temporal proximity we use the 2011 definition which identifies 611 local labor markets for the Italian economy.

[11] Throughout the paper the baseline for the RDD estimates is computed as the average value for workers fired between 49 years and 49 years and 11 months.

the expenditure per unemployed by about 1,300 euro (+18%). Results for the number of weeks of nonemployment are slightly less stable but the coefficient in the full specification is well within 2 standard deviation with respect to the baseline model.[12] Workers spend on average 6 additional weeks in nonemployment due to the longer potential benefit duration or 0.358 additional week per week of additional potential duration. The effects are long lasting and, after 4 years since layoff, workers with longer benefits are still 1 percentage point more likely not to have found any job in the private sector (about 6.5% over a baseline of 18%). Estimates for the effect on nonemployment are slightly larger than previous estimates (0.3) on the Italian setting by Rosolia and Sestito (2012), who estimate the effects of benefit potential duration and generosity exploiting a smaller administrative sample and a policy change in 2001.

This effect is driven by three main elements as described by the hazard rates reported in Figure 5: first, unemployed with longer potential benefit duration are less likely to exit from nonemployment since the very beginning of the spell; second, unemployed with shorter potential benefit duration (8 months) tend to have a higher exit rate with respect to unemployed with longer potential benefit duration when they are no longer eligible for benefits; third, after the end of the UB (12^{th} month), unemployed with longer initial duration experience an increase in their exit rate towards employment but this is too small to fully realign the reemployment probability between the two groups. Both groups of workers show a spike in exit rates once they lose eligibility for the subsidy. However, the hazard rate also shows a sizeable jump at 6 months for both groups. This coincides with the first drop in the replacement rate from 60% to 50% but it seems unlikely that the spike is driven by a large response to benefit generosity. Indeed, only a minor change (and in the wrong direction) in the hazard rate is observed for workers with 12 months of eligibility at 8 months of nonemployment (which corresponds to a similar drop from 50% to 40%).[13] As I show in appendix , this pattern is largely driven by recalls and it

[12]Here, I restrict my attention to spells in the private sector and I censor spell at 4 years after layoff. I check the effects of these restrictions in appendix C by trying different censoring and considering transitions to the public sector and self-employment.

[13]The hazard rate for workers with 8 months of eligibility is not informative at 8 months since layoff as the month coincides with the end of their eligibility period.

is related to two main reasons: first, the economic cycle of tourism, which represents an important part of the sample, seems to last about 6 months as workers terminate their contract at the start of November and they are reemployed around April; second, the institutional framework provides strong incentives for workers to be employed at least 6 months per year as they require at least one year of work over 2 years to be eligible for unemployment benefit. This spike could possibly relate to a strong entitlement effect. This is particularly salient for temporary workers who have a reasonably high expectation of experiencing again a job loss. Finally, the hazard rate shows a small increase after 24 months since layoff for both groups. This could be related to a reduction in social security contributions[14] for employers who hire, with permanent contracts, workers who have been unemployed for at least 24 months (L. 407/90). This pattern is indeed more evident for workers coming from permanent contracts who are more likely to be hired with such contracts. As this incentive applies to both treated and controls, it should not affect our results.

These findings are also confirmed in a regression framework with the use of a linear probability model for the probability of not having found a job after t months. In practice, I use as a dependent variable a dummy for not having found a job after t months ($I(t > t*)$) since layoff and iterate it for all the months in the 4 years observation window. Resulting coefficients, which summarize differences in reemployment rates over 4 years, are reported in Figure 6. As described above, the difference in reemployment emerges since the start of the spell and becomes more marked between 8 and 12 months of nonemployment. This corresponds to the periods when workers with longer potential duration are still entitled to their benefits, whereas those fired before turning 50 are not. After the end of the 12 months of subsidy, workers with longer benefits progressively close the gap between them and workers with shorter duration, but this process is slow and, after 4 years, they have still a 1% higher probability of not having found a new job, as shown in the previous regression analysis. Notice that we do not see any particular change in the difference between the two groups at 24 months since layoff, which is comforting about the absence

[14]By 50% of the social security contribution or about 11% of the wage for 3 years.

of heterogenous effects of the social security contribution cut.

6.2 Medium Term Outcomes

The timing and characteristics of the first employment spell provide a partial description of the full effect of unemployment benefits. On the one hand, a longer nonemployment spell could lead to human capital losses and stigma, and influence the future transitions towards other employers or nonemployment of workers with longer benefits. On the other hand, workers with longer benefits might gain search experience, and be able to transition faster across future employers. The sign and the magnitude of the overall effect is an empirical question. These effects might not be fully detectable in the characteristic of the first job in regulated job markets. If contracts and pay are mostly set through sectoral and national level agreements, employers might have limited ability to offer heterogenous contracts thus limiting differences in the new employment characteristics. In addition, as far as the probability of reemployment of the two groups does not fully converge, estimates of the characteristics of the first job might be affected by selection bias.

I provide a more comprehensive view of the overall effects of unemployment benefits by looking at aggregate outcome within 4 years from layoff in the spirit of Schmieder et al. (2012b). I analyze both employment outcomes and earnings, as the they are informative about the medium-term welfare effects of unemployment benefits. I limit my period of observation to 4 years due to data availability as the last individual in my sample is fired in December 2012 and the last available year for the social security records is 2016. Moreover, differently from classical estimates on job quality, this regression is not affected by selection bias as the dependent variable can be defined regardless of reemployment.

As a first step, I plot the overall number of weeks in nonemployment during the 4 years following layoff in Figure 7. First, workers spend a substantial amount of time in nonemployment: over 4 years they spend about 130 weeks in nonemployment over 208 total weeks. This suggests that recurrent nonemployment spells are common in the data. Second, the jump in weeks in nonemployment at 50 years is now substantially reduced. A formal regression, reported in Table 5, confirms these findings: Column (1), which uses my preferred specification for the total number of weeks in nonemployment over 4 years, shows an increase of only 2 weeks. Column (2) looks at differences in total labor income and shows a decline by 800 euro or about 2.4% of the baseline.[15] Columns (3) and (4) add benefits related to the first layoff and show that benefits more than compensate for the labor income losses. These gains are partly mitigated by the inclusion of all benefits[16] received after the first layoff in Column (5) and (6): overall, workers with initial longer potential benefit duration have a 4.8% higher income than workers with shorter benefits. Finally, Columns (7) to (9) provide information on future benefits. Workers with longer benefit duration are less likely to take up new unemployment benefits, they get lower transfers and spend less time on unemployment benefits. These effects directly offset part of the initial higher expenditure through lower future benefit expenditure. It should be noted that these effects are not sufficient to say anything about employment for workers with longer benefits as workers who never gets reemployed cannot access to benefits again. I will provide direct evidence on higher employment stability below. It should also be noted that Schmieder et al. (2012b) provide mixed evidence on this point. If, also in their case, the effect on nonemployment is lower over 5 years, the difference in time spent on unemployment benefits further increases, which makes it more difficult to assess in which direction these result affect efficiency considerations.

To better understand how workers with longer benefits offset their initial employment disadvantage, I then look at the pattern of employment for workers with longer and shorter potential benefit duration. I use a linear probability model at different time horizons since layoff with dependent variable equal to 1 if the worker is employed in the month.[17] Figure 8 reports coefficients over 4 years after layoff. As in the previous case, workers with longer potential benefit duration show a higher probability of nonemployment since the start

[15] Specification in log for taxable income appears to be very sensitive to the correction for zeros and it is not reported. Other income measures are not affected as all individuals collect at least unemployment benefits once by construction. Estimates are available upon request.

[16] Workers in fact experience several unemployment spells over the period and on average get 1.26 additional unemployment benefits over the 4 years period.

[17] A worker is considered employed if she works at least one day in the month.

of the spell. However, the maximum difference in employment between the two groups is lower by about one third (2 percentage points), and it peaks 2 months before the end of the benefit eligibility for workers with longer potential benefit duration. The period of convergence between the two groups is also much shorter: while in Figure 6 the two groups show different reemployment rates up to the very end of the sample, in this case the level of employment is the same after only 18 months. After this period, workers with longer potential benefit duration show slightly higher levels of employment for about 14 months. In the long run, the employment difference among the two groups is close to the long run reemployment difference (about 1%).[18] Figure 9 provides additional evidence on the dynamic effects of longer potential benefit duration. Workers with longer potential benefit duration suffer relatively small income losses which are concentrated in the months between 8 and 12 (Panel A). Even accounting for extensive margins responses, workers with longer potential benefit duration get at most 75 less euros per month. Conditional on employment, there are no differences in monthly earnings (Panel B) and individuals with longer benefits actually get higher monthly wages between 8 and 12 months after layoff. This suggests that workers with shorter duration get worse jobs when they lose eligibility to unemployment benefits. In the case of days worked per month (conditional on employment), there does seem to be at most small differences in favor of workers with longer benefits (Panel C). Finally, workers with longer potential benefit duration are less likely to be on benefits after the end of their eligibility period (Panel D). Also in this case, this is not sufficient to infer employment stability of workers with longer benefits as workers who do not find employment are not able to claim again unemployment benefits.

This discrepancy can be determined by multiple factors which can influence the employment of workers with longer benefit duration. First, workers with longer benefits could find better jobs with expected longer duration. Second, workers with longer benefits could be better at changing employer after the first employment spell. Finally, workers

[18] This dynamic could suggest some cyclical differences across the two groups. To further investigate this issue, I analyze this outcome over a 7 year period using workers fired in 2009 in appendix D.1. This analysis does not show any cyclical dynamic, which suggests that the two employment levels will likely converge in the long run also for the whole sample.

with shorter duration who found a job earlier might lose their job at higher rate, thus closing the employment gap with workers with longer benefit duration. In this section, I will look at these three possible channels to provide evidence on each of these possible explanations.

Quality of the first Job

The assessment of the effects of unemployment benefits on job quality is a crucial and classical part of studies on unemployment benefits. By acting as subsidies to search, longer unemployment benefits can allow workers to search for better jobs, thus improving their labor outcomes and, possibly, productivity in the economy (Acemoglu and Shimer, 1999 and Marimon and Zilibotti, 1999). From a pure policy perspective, positive effects on the quality of the new job could allow to recover part of the costs of the policy through higher taxes and lower future benefits. The presence of large positive effects could make the policy self-financing as in Michalopoulos et al. (2005).

I consider several aspects of the new job and estimate the effects of longer potential benefit duration with my preferred specification. As the model can only be estimated with workers who could find a job, this regression framework is partially affected by selection to the extent that the two groups show different long-term reemployment probability. Although previous results have shown a lower probability for individuals with longer potential benefit duration, it is worth stressing two points: first, the difference in reemployment is overall limited and it should not lead to large biases; then, differences are still informative as it can allow to identify the source of the different employment pattern for the two groups.

Figure 10 reports the effect of longer potential benefit duration on several characteristics of the new job. For the sake of comparison coefficients are standardized by the average in the baseline group[19] and full tables are reported in appendix D.2. Workers with longer potential benefit duration seem to experience small gains in daily wage (a

[19]Workers fired between 49 years and 49 years and 11 months of age.

0.6% increase). Previous studies provided mixed evidence in this regard, with small and statistically insignificant effects (Card et al., 2007a, Van Ours and Vodopivec, 2008). My estimate is also very close to results of Nekoei and Weber (2017) who find that 9 additional weeks of potential benefits lead to a 0.5% increase in daily wage. Workers are however more likely to find a job with a permanent contract (one percentage point over an average of 26% in the baseline group) and more likely to move to older firms (about 1.2 months). Interestingly, this does not translate to longer tenure in the new firm. These workers also have higher probability of having a full-time contract and tend to be hired by smaller firms. Coworkers tend instead to be remarkably similar.

Table 6 further explores characteristics of the new job by looking at mobility of workers in both economic and geographic terms. Longer potential benefit duration slightly promotes mobility with a higher probability of changing firm (Column (1)), a higher probability of changing geographic location but within Local Labor Market and Region[20] (Columns (2)-(4)), and a higher probability of changing sector within broad sector (Columns (5) and (6)). Hence, workers exploit this additional search time to look for jobs locally but over an extended area and in different sectors. I also explore if the new economic or geographic location offers better employment prospects. I check three different outcomes in this direction: first, I look at the growth rate of the number of employees between the year of hiring and year before in the new location; second, I look at retention, defined as the share of workers employed in the firm, sector or municipality in the year before the hiring who are still employed there in the year of hiring; finally, I look more broadly at persistence in employment, defined as the share of workers employed in the firm, sector or municipality in the year before the hiring who are still employed in the private sector in the year of hiring. Although results on growth (Columns (1) to (3)) show that the firm and the new sector are growing faster, the level of retention (Columns (4) to (6)) and persistence (Columns (7) to (9)) in employment does not show any change

[20]Italy is divided in 20 regions which are the intermediate administrative level between municipalities and the central government. They hold relevant legislative powers and can implement local policies concerning both taxation, welfare and labor markets. In this sense, the regions constitute a very relevant administrative dimension in the Italian economy.

in all the three dimensions.

Transitions across firms

Then, I check explicitly if workers who have found a new job after a longer benefit show higher persistence in employment. I consider the first two years after reemployment[21] to have I restrict the sample to all workers who find a job within 3 years since layoff. This restriction causes only small sample losses (5% of workers who find a job). I then implement a regression for the probability of being employed in the months following the first reemployment date by month and plot the resulting coefficients in Figure 11, Panel A. Workers who found a new job after a longer unemployment benefit indeed show consistently higher levels of employment after reemployment. Differences are not significant in the short term, but, after one year, the two groups show a significant divergence in employment which persists for more than an additional year. Panel B restricts the attention to employment in the first firm which hired the worker after reemployment. In this case we do not observe any difference between the two groups. This is consistent with previous findings about tenure in the new firm and, in addition, show that also spells with short breaks in employment can be excluded. Result shows that the decline in the difference for weeks in nonemployment is at least in part driven by higher levels of employment for workers with longer benefits after reemployment. In order to quantify the contribution of this element, I assess the total additional employment over this time span in a regression framework in Table 8. The first two columns report the effect on continuous employment in the new firm (Column (1)) and in all firms (Column (2)). Both coefficients are positive but far from statistical significance. Workers with longer benefits show also a similar probability of job to job transitions (Column (3)).[22] Finally, the last two columns show the effect on months of employment over the two years. There is still no difference concerning employment in the first firm (Column (4)) but a positive and significant difference is observed for employment overall in months and then in weeks

[21]In this section I exploit data on 2017 which have recently been made available. Results for the first year is within my sample of observation for all workers.

[22]These are defined as transitions to a different firm within the next month after layoff.

(Column (5) and Column (6)). The two estimates are consistent and workers with initially longer benefits stay employed for about one additional week after reemployment. In order to map this effect into the whole sample we should consider that the estimates use about 80% of the sample and there are still some differences after 2 years. As these two elements tend to offset each other, the overall contribution of this employment pattern should offset one week of the initial difference in nonemployment. A longer time horizon would help to provide a more precise assessment of the contribution of this element. Finally, it should also be noted that these findings could partly be affected by selection as workers with longer benefits are less likely to find a job within 3 years from layoff. This difference is anyway rather small (about 2 percentage points) and the bias, if any, should be rather limited.

Cyclicality of employment

A third possibility is that workers who find a job after initial shorter benefit duration lose their job in the short run. In this case, workers with shorter benefit duration would go back to nonemployment before workers with longer benefit duration find a job, which might make more difficult to identify these dynamics looking at months after reemployment. Something along these lines is suggested by results presented in Figure 9d: these results indirectly show that jobs found after the end of unemployment benefits for workers with shorter benefits are of lower quality than those found before (in terms of number of days worked per month and of wage). These would also be consistent with previous findings by Caliendo et al. (2013), who find that jobs obtained near the end of unemployment benefits have a higher likelihood to be terminated. I explore this possibility by plotting the employment rate for workers with shorter and longer potential benefit duration over the 4 years after layoff in Figure 12. In this case, I restrict the sample to individuals fired close to the cutoff (between 49 and 6 months up to 50 and 6 months). These results are informative from several perspectives: first, even if about 18% of individuals find a job after layoff, employment rate is never higher than 57%, which shows that many workers who find a job are eventually laid off again; then, there is a pronounced cyclicality in employment which persists even after removing sectors characterized by a marked seasonality[23] (e.g. tourism and constructions). Workers who find a job at the peak of the cycle appear to be more vulnerable to the following contraction and the difference between the two lines is reduced at the trough of the cycle.[24] In addition, workers who are eligible to longer benefits are also faced with a different set of vacancies when they are incentivized to look for jobs. This could help explain part of the reallocation of workers across municipalities and sectors observed in previous sections. Based on previous computations, this component should account for about three quarters of the observed discrepancy between the first spell and medium term nonemployment duration.

7 Heterogeneity and Extensions

7.1 Heterogeneity

The average effects estimated so far may mask underlying strong heterogeneities across workers types. This is a common concern in policy evaluation and Card et al. (2017), for example, find that gender and age of workers play an important role in the effectiveness of labor market policies. In this section, I explore potential heterogeneities across different groups of workers classified according to their last job and personal characteristics. More specifically, I explore geographic, gender, firm, and contract heterogeneity by running my preferred specification across subgroups of workers for my three main variables of interest: duration of nonemployment after the first layoff, total nonemployment duration and log of total labor income and unemployment benefits.

Table 9 reports the results of my estimates. As usual, Column (1) reports the baseline effect for the sake of comparison. First, I explore geographic differences and I

[23] Results not reported

[24] It should be noted that the employment pattern is partly at odds with previous regression evidence presented in Figure **??**. The two are not supposed to match as the present figure is similar in spirit to a non-parametric RDD which provides, in this setting, large estimates as I will show later.

look at the effect of longer potential benefit duration in the Centre-North and in the South of Italy, in Column (2) and (3). The effects on employment and earnings are larger in the South, coherently with more difficulties for workers in this area to find jobs after layoff, but the effect on the total number of weeks of nonemployment is very similar across the two areas. I then explore gender differences in Column (4): women show lower responses to longer potential benefit duration. The dimension of the firm of origin (Column (5) and Column(6)) and the type of the previous contract (Column (7) and Column(8)) are two particularly relevant dimensions. Workers coming from firms with more than 30 employees (the median for my sample) show less than half of the response of workers coming from small firm. In their case there is no effect on the overall time spent in nonemployment over 4 years. Similar differences can be observed for permanent and temporary contracts where permanent workers show much stronger responses. The two sets of finding are related, as a higher share of layoff from larger firms had a previous temporary contract. This difference in composition seems reasonable in light of the Italian Institutional setting as large firms (more than 15 employees) face more stringent regulation with regard to firing workers with permanent contracts (*Article 18* of the Labor Code). In addition, workers from firms undergoing economic restructuring with previous permanent contracts can access a more generous benefit under certain conditions.[25] However, the contract composition is not enough to explain lower effects for workers coming from larger firms as similar discrepancies can also be observed within contract group. The lower effects for workers coming from large firms could be driven by several factors and could provide an additional contribution to the literature on gains from working in larger firms which so far has mostly focused on gains during employment (firm-size wage gap). It is difficult to fully assess the determinants of the lower effects of potential benefit duration for these workers with the data at hand. However, recall, which is more frequent for workers with temporary contracts and in larger firms could contribute to explain these differences (for whom almost 40% of the nonemployment spell end-up being recalled by the same firm). I explore the role and characteristics of recalls more in detail in appendix E. Responses in

[25]These conditions concern the tenure of the worker and the size and sector of the firm. See the Appendix for a more detailed discussion.

terms of gains in labor and benefit income show some heterogeneity but overall this result appears reasonably stable across groups.

The results for this section show that workers exhibit strong differences in their responses to longer potential benefit duration but, in general, the main findings are supported across the whole spectrum of characteristics considered. Other dimensions of heterogeneity such as the year of layoff and the sector are reported in appendix F.

7.2 Interaction with Disability and Pensions

The interaction between unemployment benefits is an important concern from a policy perspective and an active topic of research (Pellizzari, 2006; Zweimüller, 2018). Even if workers spend less time in unemployment benefits, they could have higher take-up rates for other programs. This would then imply higher costs for the government and additional negative externalities. In this section, I tackle this issue by looking at policies which are likely to interact with unemployment benefits according to previous research, that is to say disability benefits and pensions (Inderbitzin et al., 2016; Kyyrä and Pesola, 2017).

In this setting, I will consider the extensive margin for both these policies: I look at the probability of retirement within 4 years since layoff and at the take-up of disability benefits. Again, I only take into account take-up within 4 years to have a common horizon for all the individuals in my sample Results are reported in Table 10. Column (1) reports the effect on retirement while Column (2) the effect on disability benefits. In both these cases, I see a mildly positive effect which is, however, negligible for both programs and not statistically significant for disability benefits. Overall these results point at marginal increase in take-up of other programs, but the effect is small. Hence, these elements should not play an important role in the present analysis.

8 Robustness

Results presented so far are based on a parametric specification of the Regression Discontinuity design with a second order polynomial in the running variable. This parametric assumption could play a role in the estimation results and it should be carefully assessed. To this purpose, I run a series of specification and identification checks to verify the reliability of my estimates: I first start with several robustness tests on the parametrization of the RDD; then, move to placebo tests based on the precise local nature of the treatment; finally; I examine the effects of the choice of the bandwidth and of the donut.

8.1 Polynomial Order

In order to evaluate the sensitivity of my estimates to different strategies, I run a series of checks on the polynomial order and results are reported in Table 11. I first start with different specifications of the polynomial in age using a linear in Column (2) or a third order polynomial in Column (3). Although the estimates seem to be slightly sensitive to this choice, in both cases the point estimate of the new models are always well within a 2 standard deviation distance. In addition, the second order polynomial provides estimates close to the average between the linear and cubic polynomial. This model estimated provides a reasonable result with respect to more extreme values under different parametrization choices. I then estimate my model with a 3 months ray donut, which excludes areas where manipulators are of relatively little importance, in Column (4), but this leads only to a small downward correction in the estimates. Column (5) reports a non-parametric version of the RDD and finally Column (6) implements a non-parametric local linear RDD with triangular kernel and optimal bandwidth with mean square error selection[26] Also in this case, results are consistent with my previous findings although

[26]To perform these estimates, I use the robust estimation by Calonico et al. (2014) and Calonico et al. (2016). Regressions are implemented using the *rdrobust* command by Calonico et al. (2017). As the procedure does not explicitly allow for a donut setting, I adjust the data by reducing (increasing) by one month the age of individuals on the right (left) of the cutoff. This introduces only minimal measurement error. In addition, as the estimation becomes highly time-consuming with a large sample and the inclusion of a rich set of controls, I include in the equation fixed effects only for broad sector (NACE letter), month

slightly larger. Panel B and C replicate the same set of checks for the other two main variables and in all the cases we do not see large discrepancies across different models. The coefficient on total weeks of nonemployment mostly follows the changes in size of the effect on nonemployment in the first spell and the reduction with respect to this effect ranges from 75% of the effect in Column (2) to about 55% in Column (6). In all cases, this effect is substantially smaller than the one on the period of nonemployment following the first layoff presented in Panel A. Finally results in Panel C show that the effects for the log of total labor income and benefits are always positive and tend to be reasonably stable.

Overall, results of this exercise suggest that estimates obtained with my preferred specification are reasonably robust and mediate across the possible range of results obtained with alternative choices. Different parametrizations and estimations provide qualitatively consistent and quantitatively quite similar results.

8.2 Placebo

Another possible issue is that we could obtain comparable estimates at different points of the age distribution due to high variance or to the presence of other policies. This would make our results less reliable and reduce the confidence in the causal interpretation of the estimates. To check if my estimation produces jumps of similar size in other points of the distribution, I run a placebo test by running RDD models with the same specification in other points of the age distribution in the spirit of Kyyrä and Pesola (2017). In practice, I run my preferred specification with fake discontinuities using a 24 months moving window sample centered at the fake cutoff. For the sake of presentation, I report the coefficient every 3 months together with their confidence interval at 95% and do not report the coefficient for one year before and after the real discontinuity. This is done to avoid that spurious effects induced by the true policy. Results are reported in Figure 13. The outcome is reassuring about my identification strategy: the coefficient for the real discontinuity

of layoff and province.

neatly stands out with respect to the others and none of them is statistically significant at 5%. The main coefficient is also reasonably close to the one estimated in the whole sample and it is highly statistically significant. Results are qualitatively similar using a non- parametric approach (see Appendix) although in this case the several coefficients are statistically significant, but my coefficient of interest is almost 4 times larger than the ones for the fake RDD.

These results provide supportive evidence for the causal interpretation of my results.

8.3 Bandwidth and Donut

As the choice of the donut and the selection of the hole for the donut strategy could influence the results of the estimation, I perform several tests to assess the sensitivity of my results to these choices.

I start with the bandwidth choice and I run my preferred specification with a large set of different bandwidths. Resulting coefficients are reported in Figure 14. The specification used in the paper corresponds to the one at 48 months of bandwidth. The estimates appear quite robust to different choices and in no case the coefficient is statistically different from the one obtained with the manual bandwidth. It should also be noted that the use of larger bandwidths leads to substantial improvements in efficiency as they allow for a better estimation of the polynomial. Given the high number of observations and controls, the implementation of the optimal bandwidth estimation is quite time consuming and I report main results with manual bandwidth selection. Estimates with optimal bandwidth are reported for the sake of comparison in Column (6) in Table 11.

As a final check, I also assess the importance of the donut hole selection. I estimate my preferred specification with donut hole from 1 month up to 12 months and then plot the estimates in Figure 15. Coefficients are stable around the main estimate and the increasing size of the hole leads only to small changes up to 5 months from the cutoff. It is also interesting to note that coefficients for donuts for a 4 and 5 months ray around the cutoff are larger than the coefficient using a 3 months donut reported in Table 11, Column (4). This provides further supporting evidence to the estimates obtained with smaller donuts, which show very similar value. In addition, the coefficient for a one-month donut is also very similar to the others which suggest that the rich set of fixed effects and controls is able to mostly capture the determinants of manipulation. Estimates start to differ substantially from the main result only after a 8 months radius and coefficients are not statistically different from 0 for very large donut as the polynomial extrapolation becomes unable to replicate the pattern of the data closer to the cutoff.

All balanced, the evidence in this section shows remarkable resilience for the estimates to changes in bandwidth in the exclusion of bins close to the cutoff.

9 Conclusions

In this paper, I investigate the medium-term impact of longer unemployment benefits on workers employment and earnings. This margin, mostly neglected by previous studies, is crucial from a policy perspective: on the one hand, longer periods in nonemployment could lead to human capital depreciation or scarring and negatively affect workers employment prospects; on the other hand, workers might exploit their higher search experience to look for better jobs or transition faster towards new employment. To estimates these effects, I use rich and novel administrative data from Italy and I implement a Regression Discontinuity Design exploiting exogenous variation in potential benefit duration. The potential benefit duration is fully determined by age at layoff and workers fired before turning 50 years of age are eligible to 8 months of unemployment benefits while workers fired afterwards are eligible to 12 months of unemployment benefits.

Consistently with previous findings in the literature I find that longer potential benefit duration leads to longer periods receiving benefits and to longer time in nonemployment, by 8 and 6.2 weeks respectively. This is determined by three different elements: workers with longer benefit duration have lower exit rate from nonemployment since the start of the spell; the difference in reemployment between the two groups increases sharply between 8 and 12 months after layoff when workers with lower potential benefit duration are no longer eligible for unemployment benefits and workers with longer benefits are still eligible; although workers with longer benefits show a higher exit rate towards employment after 12 months since layoff, they slowly converge to the reemployment probability of workers with shorter benefits and after 4 years they are still 1% less likely have found a job. This longer period spent in nonemployment leads to marginal gains in the quality of the new employment as workers are more likely to find a job with permanent contract and in older firms. The effect on wages, tenure and coworkers' characteristics is positive but not statistically different from zero. Over a 4 years period after layoff, however, workers with longer potential benefit duration show only 2 more weeks in nonemployment. Moreover, they are less likely to get unemployment benefits in the future. They also experience overall small labor income losses (-2.4%) which are more than compensated by the higher benefit transfers and they spend less time on unemployment benefits. Two main components contribute to determine this discrepancy: first, workers who find a job after longer benefits have a higher likelihood of finding a new job sooner after being laid off by the new firm; second, workers with shorter benefits are more vulnerable to cyclical labor demand. The former component explains about 25% of the difference in time in non-employment while the second captures the remaining. These results are robust to a wide range of specification and robustness checks. In addition, the effects are widely heterogeneous across workers. Workers coming from smaller firms and who lost permanent job show the strongest responses to longer potential benefit duration. Also, in their case, however, the overall response in terms of nonemployment over 4 years is substantially lower.

My findings are of crucial importance from a policy perspective from two different angles. If workers are able to spend more time in employment after reemployment and show a lower take up of unemployment benefits in the future, they can at least partially compensate for initial higher expenditure with lower future expenditure and higher contributions. Neglecting these effects leads then to overestimate the negative externalities of longer unemployment and to set inefficiently short (or low) benefits. As the present data does not allow for the measurement of consumption, I remand a discussion of the optimality of the policy to future work. In addition, potential benefit duration interacts with labor demand cyclicality making workers with shorter duration more vulnerable to subsequent job losses. This suggests that potential duration affects workers' job prospects also through the set of vacancies to which they are exposed when they exhaust benefits. Optimal unemployment benefits policies should then also consider this additional channel to fully capture the effects of unemployment benefits on subsequent employment.

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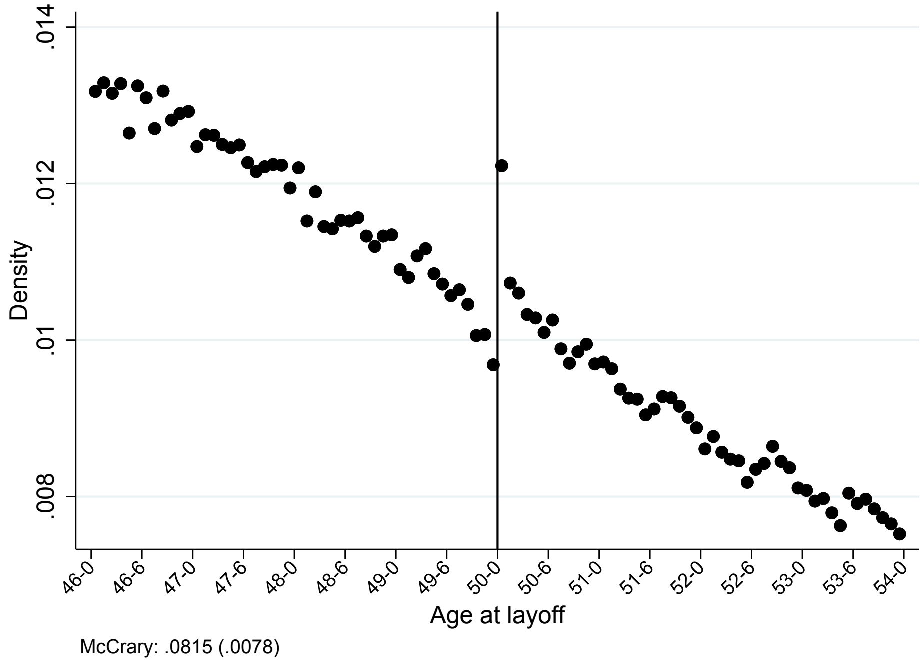
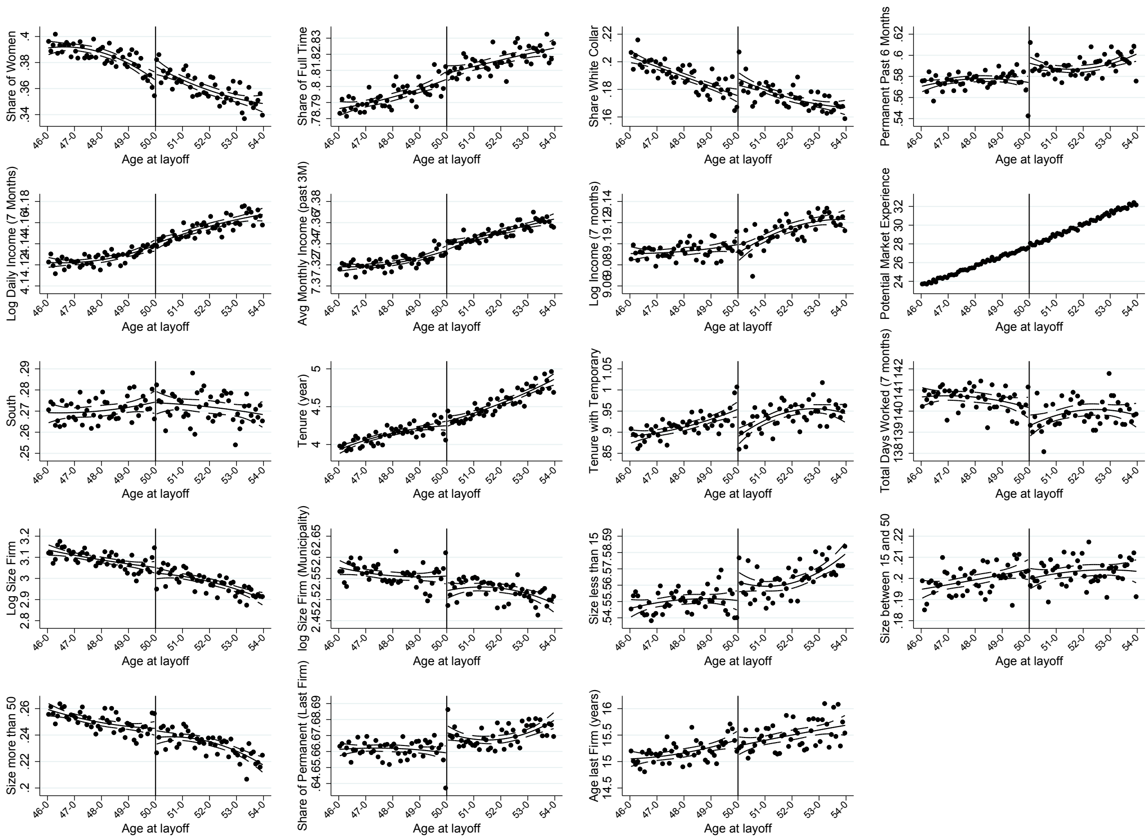
Graphs

Figure 1: Density of Layoff by Age (Monthly)



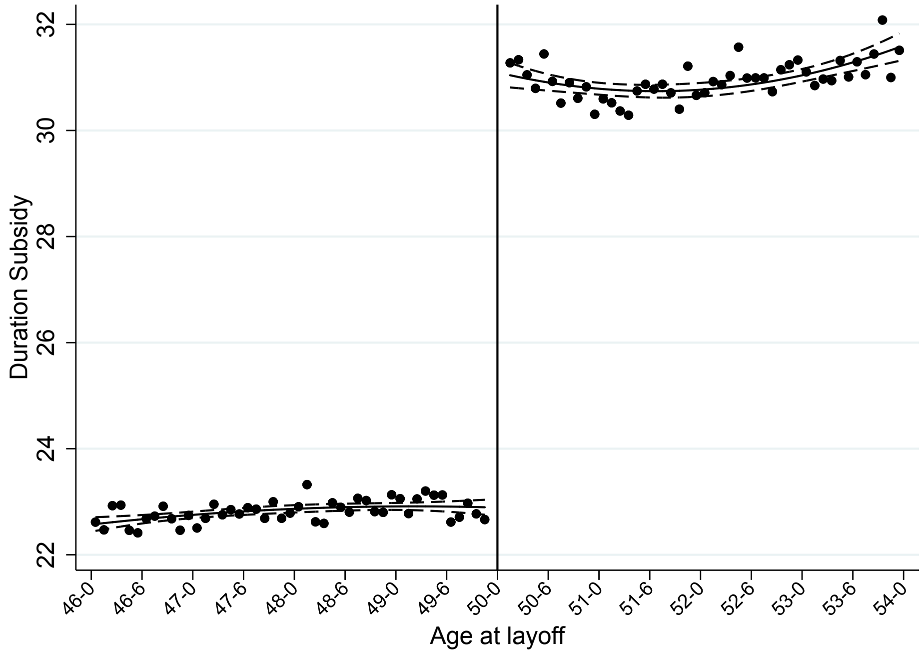
Note: Density of age at layoff for the universe of receipient of unemployment benefits (OUNR) between 2009 and 2012. Number of layoff: 452,888. Result of Mc Crary test for discontinuity at the threshold reported at the bottom of the graph. Corresponding t-stat is 10.44.

Figure 2: Continuity of Observable Characteristics at Cutoff



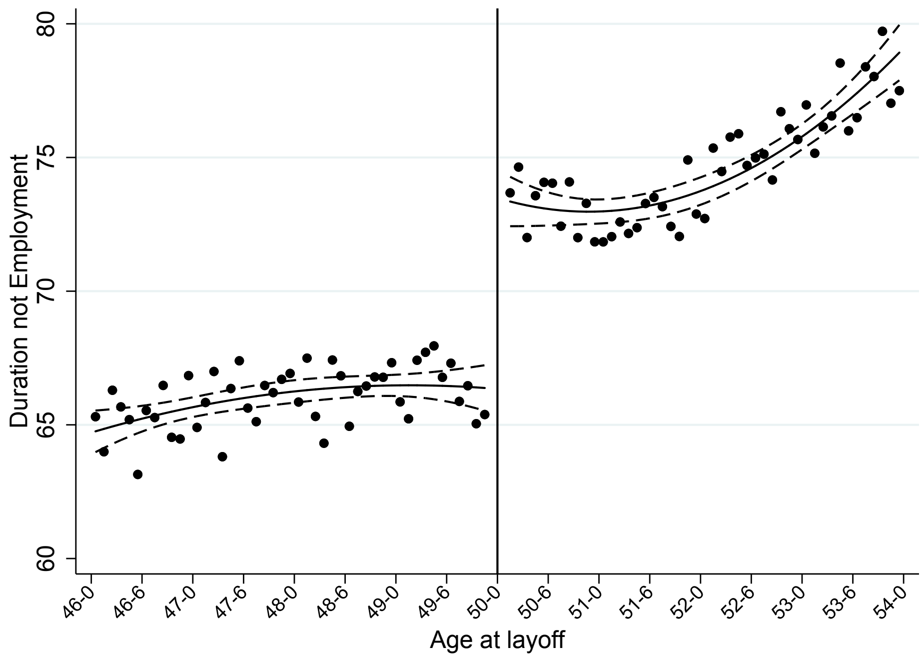
Note: Average of observable characteristics in a 4 year ray around the cutoff. Number of layoff: 452,888; variables reported (from left to right and top to bottom): Female; Full time; White Collar; College or more; Log Daily wage in past 7 months; Average Monthly income (past 2 years); Potential Labor Market experience; Share in South or Island; Tenure with same employer; Tenure with Temporary Contract; Tenure in Spell; Months worked in past 7 Months; Log size firm; Log Size firm in municipality (close to plant definition); Small firm (less than 15 employees); Medium firm (15 to 50); large firm (more than 50 employees); Share of permanent contracts in last firm; Age of last firm in years.

Figure 3: Weeks of Benefit



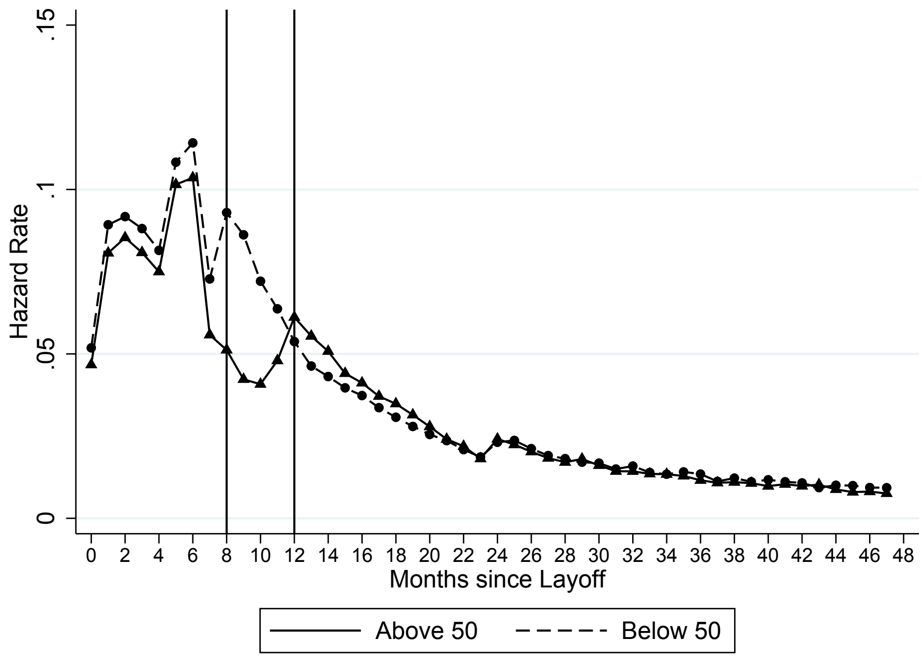
Note: Figure reports the week on benefits in the first spell after layoff. Figure based on 442,964 layoff between February 2009 and December 2012 for workers between 46 and 54 years at layoff excluding workers with 49 years of age and 11 months and 50 years of age. Linear fit estimated by OLS with a square polynomial in age estimated separately on the two sides of the cutoff. Duration of the subsidy reported in weeks. Standard errors of the prediction reported.

Figure 4: Weeks of nonemployment



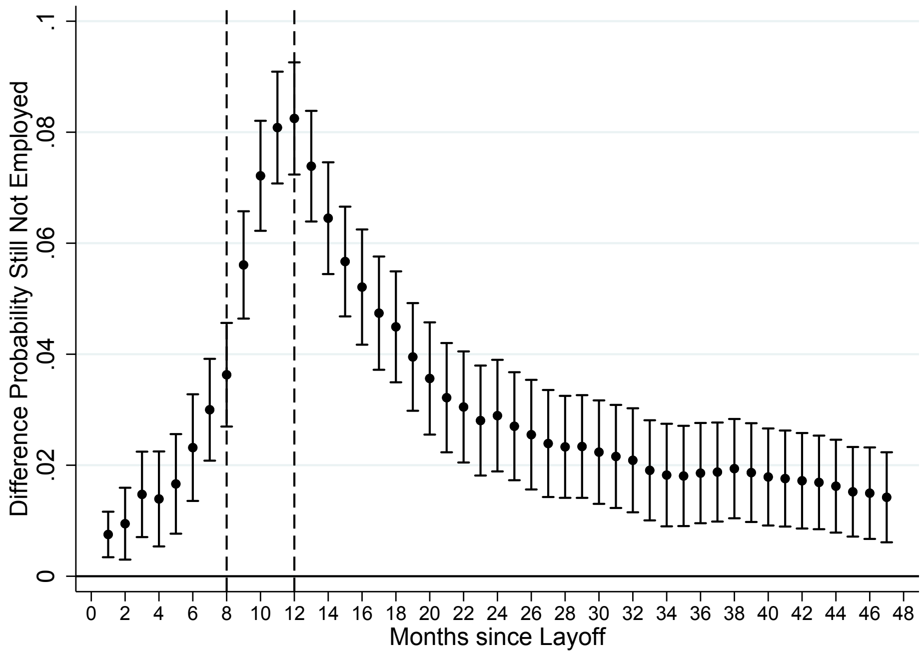
Note: Figure reports the week of nonemployment in the first spell after layoff. Figure based on 442,964 layoff between February 2009 and December 2012 for workers between 46 and 54 years at layoff excluding workers with 49 years of age and 11 months and 50 years of age. Period of nonemployment defined as the number of weeks between the end of the last job and the start of a new job after the end of unemployment benefits. Number of weeks of nonemployment censored at 4 years. Linear fit estimated by OLS with a square polynomial in age estimated separately on the two sides of the cutoff. Standard errors of the prediction reported.

Figure 5: Hazard Rate for Exit from nonemployment



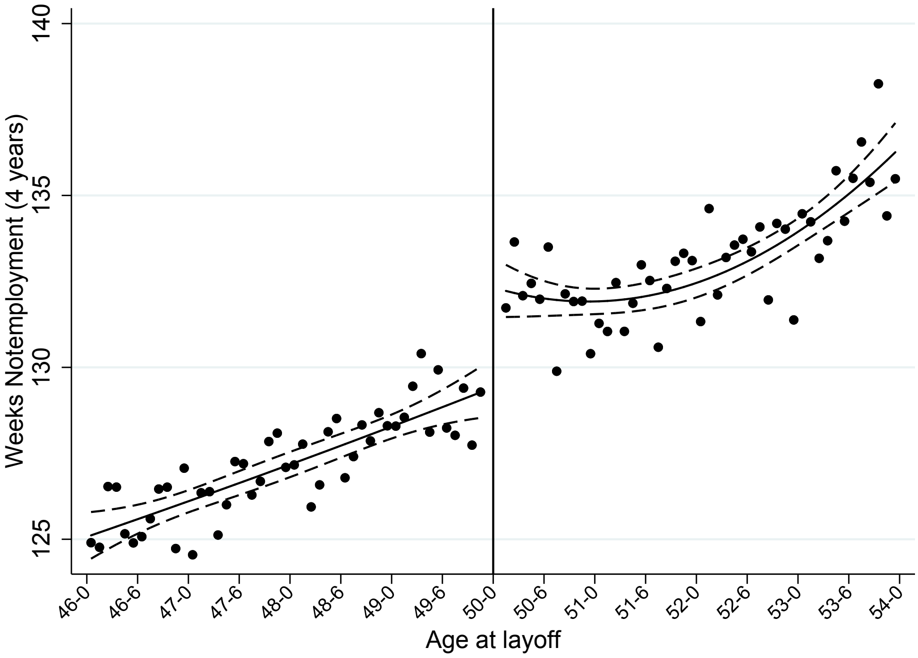
Note: Hazard rate for exit of workers from nonemployment towards employment in the private sector. Hazard rate computed as the share of workers exiting nonemployment in month t over the number of workers still unemployment after t-1 months. Figure based on 442,964 layoff between February 2009 and December 2012 for workers between 46 and 54 years at layoff excluding workers with 49 years of age and 11 months and 50 years of age.

Figure 6: Difference in Reemployment since Layoff



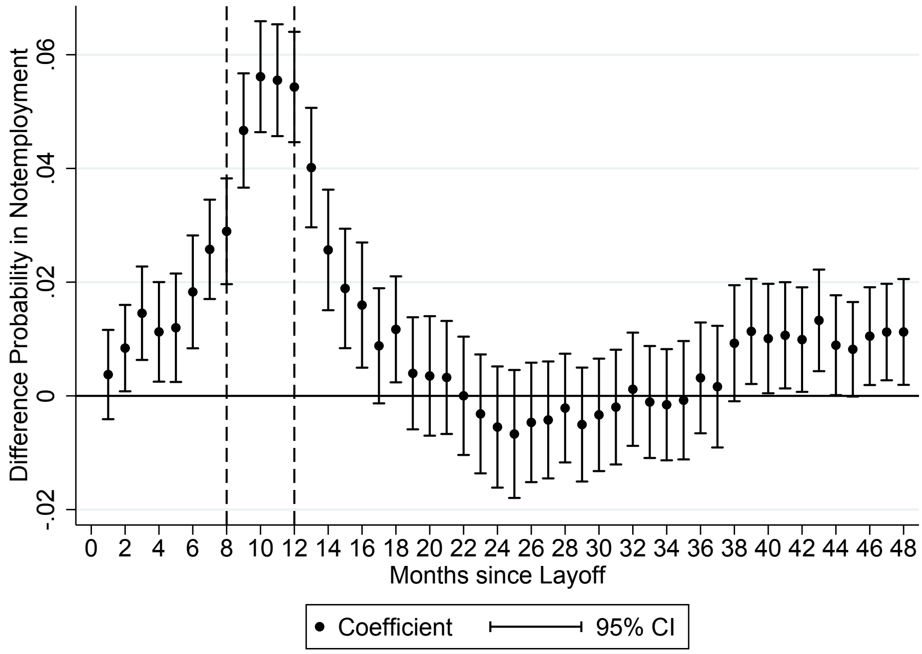
Note: Effect of 4 additional months of potential benefit duration on probability of being still unemployed after t months. Linear probability models with dummy equal to 1 if the worker is still unemployed after t months since layoff. Regressions includes a square polynomial in age with different slopes around the cutoff, controls for the worker and last firm characteristics and local labor market interacted with month of layoff fixed effects. Figure based on 442,964 layoff between February 2009 and December 2012 for workers between 46 and 54 years at layoff excluding workers with 49 years of age and 11 months and 50 years of age. Standard Errors clustered at Local Labor Market level. Confidence interval at 95% reported.

Figure 7: Total Weeks of nonemployment (4 years)



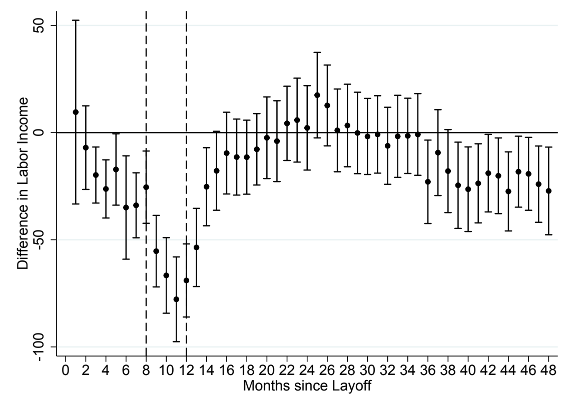
Note: Total weeks of nonemployment within 4 years since layoff. Figure based on 442,964 layoffs between February 2009 and December 2012 for workers between 46 and 54 years at layoff excluding workers with 49 years of age and 11 months and 50 years of age. Linear fit estimated by OLS with a square polynomial in age estimated separately on the two sides of the cutoff. Standard errors of the prediction reported.

Figure 8: Difference in Employment over 4 years following layoff

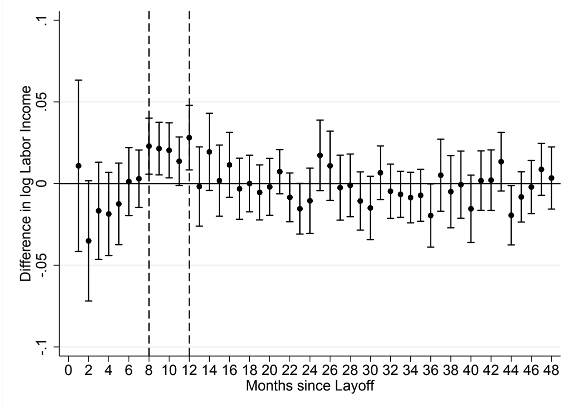


Note: Effect of 4 additional months of potential benefit duration on probability of being employed at t months after layoff. The worker is considered employed if she works at least one day during the corresponding month. Regressions includes a square polynomial in age with different slopes around the cutoff, controls for the worker and last firm characteristics, local labor maket interacted with month fixed effects. Figure based on 442,964 layoffs between February 2009 and December 2012 for workers between 46 and 54 years at layoff excluding workers with 49 years of age and 11 months and 50 years of age. Standard Errors clustered at Local Labor Market level. Confidence interval at 95% reported.

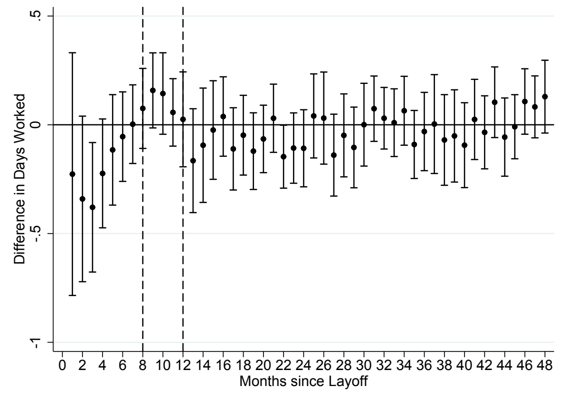
Figure 9: Income, days worked and benefits over 4 years after layoff



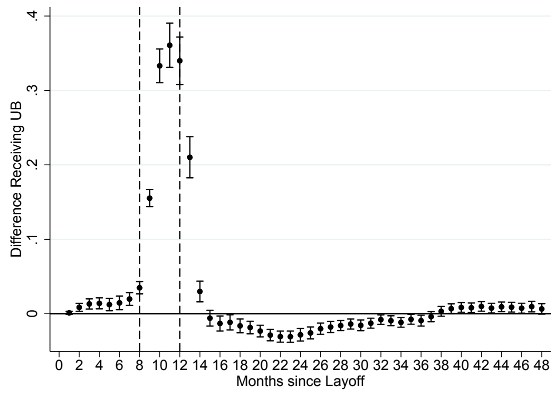
(a) Total Income



(b) Log Total Income (employed)



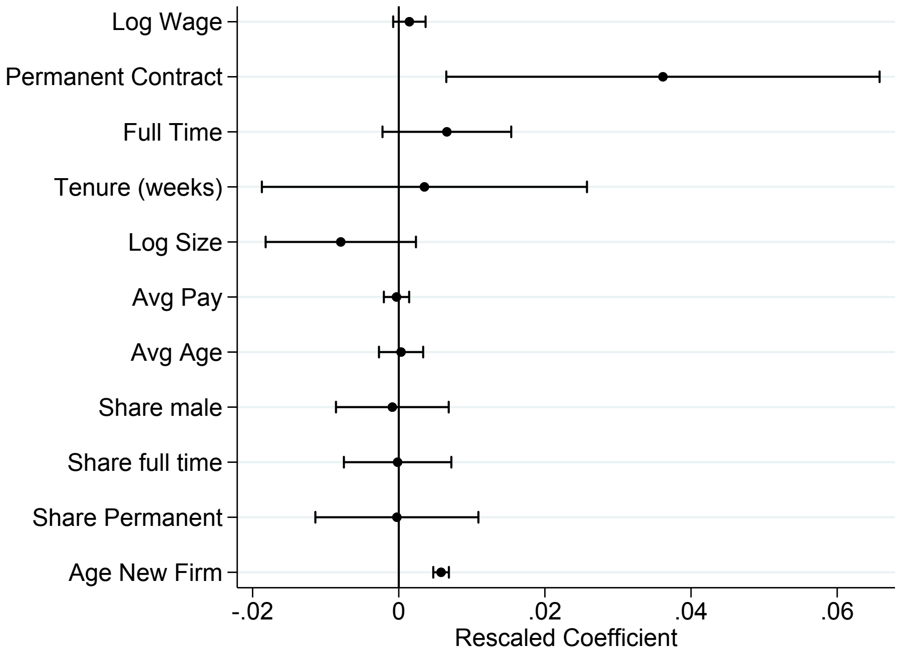
(c) Days Worked (employed)



(d) Receiving UB

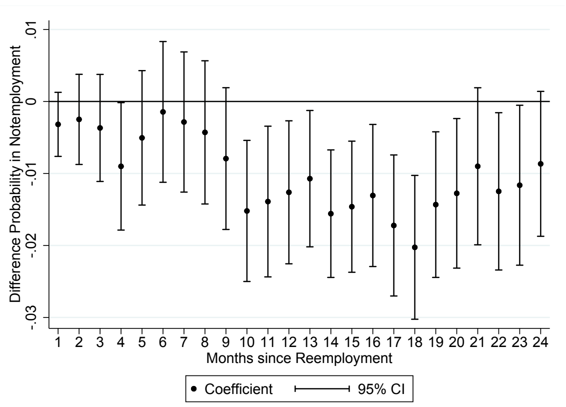
Note: Effect of 4 additional months of potential benefit duration on labor earnings, log labor earnings, days worked and probability of receiving UB. Panel (b) and (c) conditional on employment. Regressions includes a square polynomial in age with different slopes around the cutoff, controls for the worker and last firm characteristics, local labor maket interacted with month fixed effects. Figure based on 442,964 layoffs between February 2009 and December 2012 for workers between 46 and 54 years at layoff excluding workers with 49 years of age and 11 months and 50 years of age. Standard Errors clustered at Local Labor Market level. Confidence interval at 95% reported.

Figure 10: Effect on Post Unemployment Outcomes (first spell)

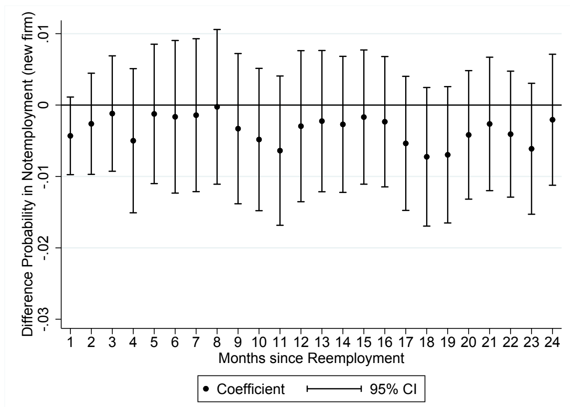


Note: Effect of 4 additional months of potential benefit duration on post unemployment job characteristics. Regressions includes a square polynomial in age with different slopes around the cutoff, controls for the worker and last firm characteristics, local labor market interacted with month fixed effects. Figure based on 356,210 new jobs for subset of layoffs between February 2009 and December 2012 for workers between 46 and 54 years at layoff excluding workers with 49 years of age and 11 months and 50 years of age which end with employment within 4 years. Coefficients standardized by the mean for the baseline group, i.e. workers fired between 49 years of age and 49 years and 11 months of age. Standard Errors clustered at Local Labor Market level. Confidence interval at 95% reported.

Figure 11: Probability of Nonemployment following reemployment



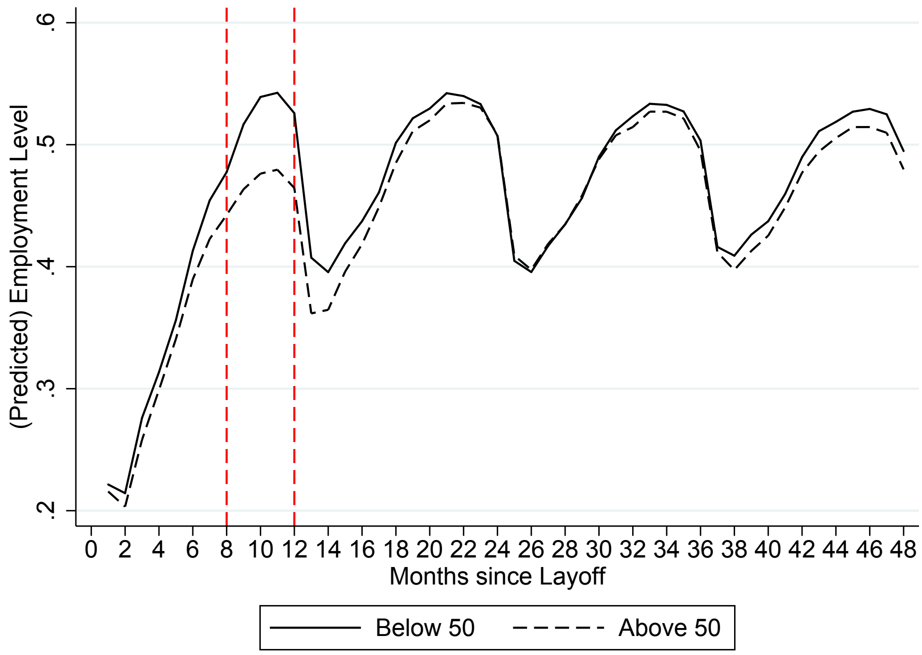
(a) Nonemployment - any firm



(b) Nonemployment - first reemployment firm

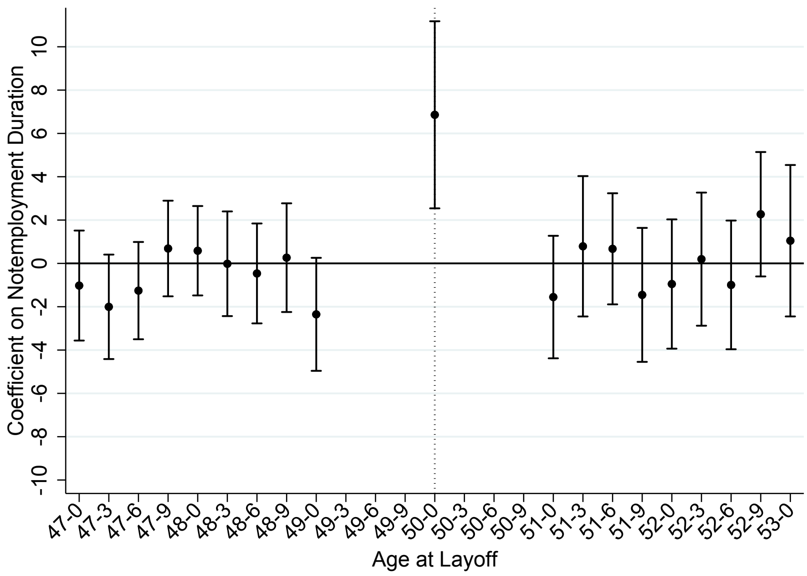
Note: Effect of 4 additional months of potential benefit duration on probability of being employed at t months after reemployment. The worker is considered employed if she works at least one day during the corresponding month. Regressions includes a square polynomial in age with different slopes around the cutoff, controls for the worker and last firm characteristics, local labor market interacted with month fixed effects. Figure based on workers who find an employment within 3 years since layoff and from layoffs between February 2009 and December 2012 for workers between 46 and 54 years at layoff excluding workers with 49 years of age and 11 months and 50 years of age. Standard Errors clustered at Local Labor Market level. Confidence interval at 95% reported.

Figure 12: Employment rate for workers near the cutoff [-6;+6]



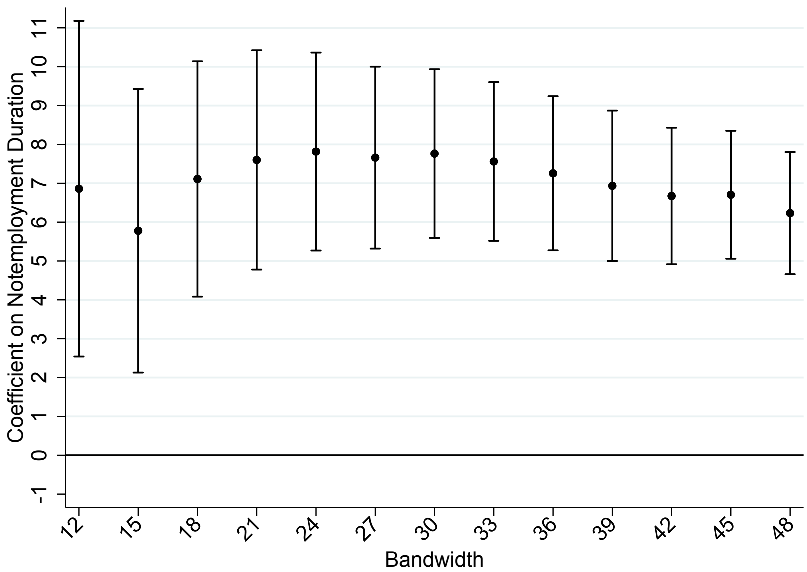
Note: Estimates of share of workers employed at 50 years of age. Estimates based on simple RDD regression with dependent variable a dummy taking value one if the worker is employed at month t and value 0 otherwise. Regression includes second degree polynomial in age with different slopes on the two sides of the cutoff and dummy for workers fired after turning 50 years of age.

Figure 13: Placebo RDD



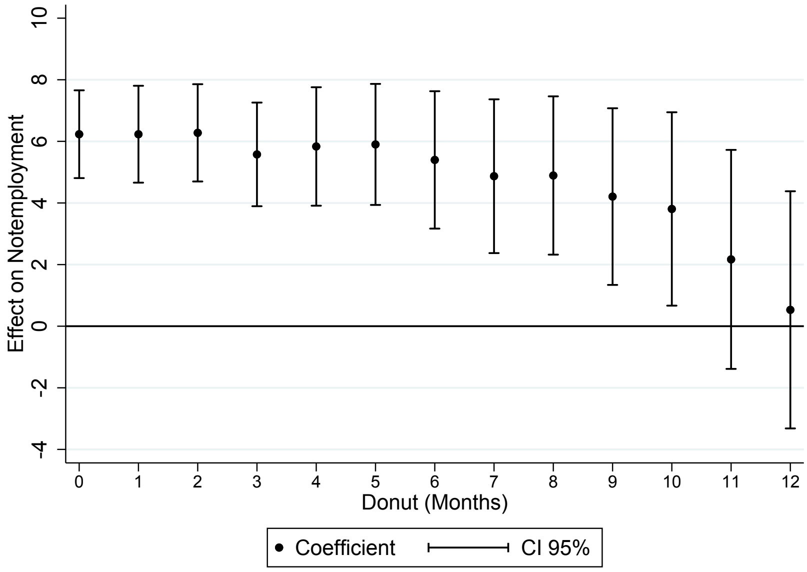
Note: Placebo linear regression for duration of nonemployment in the first spell. Figure based on 442,964 layoffs between February 2009 and December 2012 for workers between 46 and 54 years at layoff excluding workers with 49 years of age and 11 months and 50 years of age. All regressions include a squared flexible polynomial on the two sides of the fake cutoff, controls and monthXlocal labor market fixed effects. Controls include: Full time dummy; gender dummy; log of average daily labor earnings over the six months preceding the month of layoff; dummies for class size of the previous firm; market potential experience; tenure and tenure with fixed term; months worked over the 7 months before layoff; dummies for occupation in previous employment; dummies for age of first contribution to the social security; dummies for ATECO sector at 2 digits in previous employment. Coefficient at 50 years of age corresponds to policy induced change in potential benefit duration. Fake and main RDD regression use a 1 year bandwidth. Standard Errors clustered at Local Labor Market level. Confidence interval at 95% reported.

Figure 14: RDD estimates for the effect on nonemployment in the first spell with different bandwidths.



Note: RDD estimates with different bandwidths around the cutoff. Largest sample includes based on 442,964 layoffs between February 2009 and December 2012 for workers between 46 and 54 years at layoff excluding workers with 49 years of age and 11 months and 50 years of age. All regressions include a squared flexible polynomial on the two sides of the fake cutoff, controls and monthXlocal labor market fixed effects. Controls include: Full time dummy; gender dummy; log of average daily labor earnings over the six months preceding the month of layoff; dummies for class size of the previous firm; market potential experience; tenure and tenure with fixed term; months worked over the 7 months before layoff; dummies for occupation in previous employment; dummies for age of first contribution to the social security; dummies for ATECO sector at 2 digits in previous employment. Coefficient at 48 corresponds to preferred specification. Standard Errors clustered at Local Labor Market level. Confidence interval at 95% reported.

Figure 15: Rdd estimates for the effect on nonemployment in the first spell with different donut.



Note: RDD estimates with different donuts around the cutoff with a 4 years bandwidth. Largest sample includes based on 442,964 layoffs between February 2009 and December 2012 for workers between 46 and 54 years at layoff excluding workers with 49 years of age and 11 months and 50 years of age. All regressions include a squared flexible polynomial on the two sides of the fake cutoff, controls and monthXlocal labor market fixed effects. Controls include: Full time dummy; gender dummy; log of average daily labor earnings over the six months preceding the month of layoff; dummies for class size of the previous firm; market potential experience; tenure and tenure with fixed term; months worked over the 7 months before layoff; dummies for occupation in previous employment; dummies for age of first contribution to the social security; dummies for ATECO sector at 2 digits in previous employment. Coefficient at 1 corresponds to preferred specification. Standard Errors clustered at Local Labor Market level. Confidence interval at 95% reported.

Tables

Table 1: Sample Characteristics

Variable	Average	Standard Deviation
Weeks of Benefit	26.32	15.83
Duration Notemployment	84.90	106.70
Duration Notemployment (censored)	69.63	73.78
% with duration between 0 and 4 months	0.28	0.45
% with duration between 4 and 8 months	0.23	0.42
% with duration between 8 and 12 months	0.12	0.32
% with duration between 12 and 16 months	0.07	0.25
% with duration above 16 months	0.31	0.46
Recall	0.34	0.47
Female	0.38	0.48
Full Time	0.80	0.40
White Collar	0.18	0.39
Permanent Contract	0.54	0.50
(log) Daily Taxable Income	4.14	0.44
Market Potential Employment	27.43	8.85
South	0.27	0.44
Tenure	4.30	5.23
Tenure Temporary	0.92	1.51
Size	2.54	1.55
Size between 0 and 15	0.56	0.50
Size between 15 and 50	0.20	0.40
Size above 50	0.24	0.43
Share Permanent in Last Firm	0.67	0.37
Age last Firm	15.31	13.71
Workers	353796	
Spells	452888	
(Avg) # spells per individual	1.28	

Note: Summary statistics at spell level for individuals receiving the Subsidy for Ordinary Unemployment with Normal Requirement between 46 and 54. The sample excludes individuals coming from the public sector and individuals with seasonal contracts. Weeks of nonemployment defined as the distance between the layoff originating the unemployment benefit and the first hiring date after the end of unemployment benefit. Tenure defined as the number of years, even with breaks, spent with the same employer with any contract (Tenure) or with a specific type of contract (Temporary).

Table 2: Identification Check: Regression Coefficients for Discontinuity of Observables

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Polynomial and FE			Donut radius 1				
Variable	Beta	Standard Deviation	T-stat	Beta	Standard Deviation	T-stat	Average	Relative Effect
Female	0.012	0.004	**2.951**	0.006	0.004	1.378	0.375	0.015
Full Time	0.004	0.003	1.171	0.006	0.004	1.548	0.801	0.007
White Collar	0.011	0.003	**3.367**	0.005	0.004	1.355	0.181	0.029
Permanent Last 7 Months	0.014	0.005	**2.881**	0.006	0.005	1.236	0.577	0.011
(log) Daily Taxable Labor Income	0.001	0.004	0.279	0.003	0.005	0.504	4.134	0.001
(log) Tot Taxable Labor Income	-0.005	0.006	-0.886	-0.004	0.006	-0.553	9.098	0.000
Market Potential Experience	0.112	0.078	1.437	0.022	0.083	0.262	27.168	0.001
Tenure (years)	0.051	0.043	1.178	-0.032	0.048	-0.676	4.233	-0.008
Tenure Temporary (years)	-0.046	0.013	**-3.668**	-0.040	0.013	**-2.962**	0.938	-0.042
Total Paid Days (7 months)	-0.627	0.385	-1.627	-0.537	0.403	-1.333	140.338	-0.004
(log) Size	-0.029	0.020	-1.443	0.008	0.021	0.404	3.058	0.003
(log) Size Municipality	-0.034	0.013	**-2.555**	-0.015	0.014	-1.050	2.551	-0.006
Small Firm (below 15)	0.009	0.005	1.931	0.003	0.005	0.643	0.552	0.006
Medium Firm (Between 15 and 50)	-0.002	0.004	-0.467	-0.001	0.004	-0.368	0.203	-0.007
Large Firm (Above 50)	-0.007	0.004	-1.724	-0.002	0.004	-0.405	0.245	-0.007
Share Permanent in Firm	0.008	0.003	**2.732**	0.003	0.003	0.818	0.661	0.004
Age Last Firm (years)	-0.042	0.112	-0.376	-0.147	0.118	-1.249	15.337	-0.010

Note: Linear regression model with second order polynomial in age with different slopes at two sides of the threshold and LLM and month interacted FE. Column from (1) to (3) include age polynomial and fixed effects. Columns from (4) to (6) the first bin to the right and the left of the cutoff (50 years; 49 years and 11 months). Column (7) reports the average value for the variable for the individuals between 49 years of age and 49 years and 11 months of age. Column (8) reports the ratio between the coefficient in Column (6) and the average in Column (7). Number of spells: 452888. Standard errors are clustered at LLM level.

Table 3: Effect of Potential Benefit Duration on Benefit Duration and Amount

VARIABLES	(1) Weeks	(2) Weeks	(3) Weeks	(4) Weeks	(5) Weeks	(6) Benefits (amount)	(7) Benefits (log)
Above 50 years of age	8.190***	8.085***	8.073***	8.060***	7.963***	1,270.177***	0.180***
	(0.258)	(0.259)	(0.258)	(0.256)	(0.272)	(49.062)	(0.010)
Observations	442,964	442,964	442,964	442,964	442,964	442,964	442,964
Baseline dependent	22.92	22.92	22.92	22.92	22.92	4,761.83	
Controls	NO	YES	YES	YES	YES	YES	YES
Month FE	NO	NO	YES	YES	YES	YES	YES
LLM FE	NO	NO	NO	YES	YES	YES	YES
LLM X Month FE	NO	NO	NO	NO	YES	YES	YES

Note: Linear regression for the duration and amount of the benefit in weeks with a flexible squared polynomial on the two sides of the cutoff (50 years of age). Controls include: Full time dummy; gender dummy; log of average daily labor earnings over the six months preceding the month of layoff; dummies for class size of the previous firm; log of the average size of the firm in the 6 months preceding layoff in the municipality (plant); Age of the last firm in years; share of permanent workers in last firm; market potential experience; tenure and tenure with fixed term; months worked over the 7 months before layoff; dummies for occupation in previous employment; dummies for age of first contribution to the social security; dummies for ATECO sector at 2 digits in previous employment. All regressions include squared age polynomial with different slopes on the two sides of the cutoff. Sample includes all recipients of unemployment benefits (OUNR) between February 2009 and December 2012 excluding workers fired at 50 years and at 49 years and 11 months. Baseline computed as the average nonemployment duration for workers fired between 49 years of age and 49 and 11 months of age. Standard errors clustered at local labor market level. Level of significance: * 10%; ** 5%; *** 1%.

Table 4: Effect of Potential Benefit Duration on nonemployment

VARIABLES	(1) Weeks	(2) Weeks	(3) Weeks	(4) Weeks	(5) Weeks	(6) Not employed (4 years)
Above 50 years of age	7.111***	6.494***	6.431***	6.292***	6.232***	0.012***
	(0.812)	(0.778)	(0.780)	(0.783)	(0.802)	(0.004)
Observations	442,964	442,964	442,964	442,964	442,964	442,964
Baseline dependent	66.48	66.48	66.48	66.48	66.48	.18
Controls	NO	YES	YES	YES	YES	YES
Month FE	NO	NO	YES	YES	YES	YES
LLM FE	NO	NO	NO	YES	YES	YES
LLM X Month FE	NO	NO	NO	NO	YES	YES

Note: Linear regression for the duration of nonemployment in weeks up to the first employment in the private sector after the end of the benefit with a flexible squared polynomial on the two sides of the cutoff (50 years of age). Controls include: Full time dummy; gender dummy; log of average daily labor earnings over the six months preceding the month of layoff; dummies for class size of the previous firm; log of the average size of the firm in the 6 months preceding layoff in the municipality (plant); Age of the last firm in years; share of permanent workers in last firm; market potential experience; tenure and tenure with fixed term; months worked over the 7 months before layoff; dummies for occupation in previous employment; dummies for age of first contribution to the social security; dummies for ATECO sector at 2 digits in previous employment. All regressions include squared age polynomial with different slopes on the two sides of the cutoff. Sample includes all recipients of unemployment benefits (OUNR) between February 2009 and December 2012 excluding workers fired at 50 years and at 49 years and 11 months. Baseline computed as the average nonemployment duration for workers fired between 49 years of age and 49 and 11 months of age. Standard errors clustered at local labor market level. Level of significance: * 10%; ** 5%; *** 1%.

Table 5: Effect of Potential Benefit Duration on Medium Term Outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	Weeks Nonemployment	Inc. (W)	Inc. (W+B)	log Inc. (W+B)	Inc. (W+AB)	log Inc. (W+AB)	N. Benefits	Additional Amount	Additional Weeks
Above 50 years of age	2.044***	-800.311***	469.867*	0.063***	96.437	0.048***	-0.037***	-411.544***	-1.897***
	(0.632)	(291.105)	(268.482)	(0.009)	(278.905)	(0.009)	(0.011)	(51.949)	(0.247)
Observations	442,964	442,964	442,964	442,964	442,964	442,964	442,964	442,964	442,964
Baseline dependent	128.87	33359.56	38121.39		43801.55	1.26	5443.49	26.2	
Controls	YES	YES	YES	YES	YES	YES YES	YES	YES	
Month FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
LLM FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
LLM X Month FE	YES	YES	YES	YES	YES	YES	YES	YES	YES

Note: Linear regression on duration nonemployment over 4 years since layoff and on different measures of total income. Column (2) reports the effect of 4 additional months of potential benefit duration on total taxable labor income; Columns (3) and (4) include benefits collected in the first spell; Columns (5) and (6) include all benefits received by workers after the first layoff. Controls include: Full time dummy; gender dummy; log of average daily labor earnings over the six months preceding the month of layoff; dummies for class size of the previous firm; market potential experience; tenure and tenure with fixed term; months worked over the 7 months before layoff; dummies for occupation in previous employment; dummies for age of first contribution to the social security; dummies for ATECO sector at 2 digits in previous employment. All regressions include squared age polynomial with different slopes on the two sides of the cutoff. Sample includes all recipients of unemployment benefits (OUNR) between February 2009 and December 2012 excluding workers fired at 50 years and at 49 years and 11 months. Baseline computed as the average nonemployment duration for workers fired between 49 years of age and 49 and 11 months of age. Standard errors clustered at local labor market level. Level of significance: * 10%; ** 5%; *** 1%.

Table 6: Effect of Potential Benefit Duration on Sector and Geographic Mobility

VARIABLES	(1) Firm	(2) Municipality	(3) LLM	(4) Region	(5) ATECO Broad	(6) ATECO 2
Above 50 years of age	0.009**	0.013***	0.005	0.001	0.007	0.009**
	(0.004)	(0.005)	(0.005)	(0.003)	(0.004)	(0.005)
Observations	356,210	356,210	356,210	356,210	356,191	356191
Baseline dependent	.58	.41	.26	.09	.25	.34
Controls	YES	YES	YES	YES	YES	YES
Calendar Month FE	YES	YES	YES	YES	YES	YES
LLM FE	YES	YES	YES	YES	YES	YES
LLM X Month FE	YES	YES	YES	YES	YES	YES

Note: Linear regression for the probability of changing firm in Column (1), of changing geographic location (Columns (2)-(4)) or sector (Columns (5)-(6)) with new employment. Controls include: Full time dummy; gender dummy; log of average daily labor earnings over the six months preceding the month of layoff; dummies for class size of the previous firm; market potential experience; tenure and tenure with fixed term; months worked over the 7 months before layoff; dummies for occupation in previous employment; dummies for age of first contribution to the social security; dummies for ATECO sector at 2 digits in previous employment. All regressions include squared age polynomial with different slopes on the two sides of the cutoff. Estimates based on 356,210 new jobs for subset of layoffs between February 2009 and December 2012 for workers between 46 and 54 years at layoff excluding workers with 49 years of age and 11 months and 50 years of age which end with employment within 4 years. Baseline computed as the average nonemployment duration for workers fired between 49 years of age and 49 years and 11 months of age. Standard errors clustered at local labor market level. Level of significance: * 10%; ** 5%; *** 1%.

Table 7: Effect of Potential Benefit Duration on Sector and Geographic Mobility

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Growth Employment			Retention			Persistence		
	Firm	Municipality	Sector	Firm	Municipality	Sector	Firm	Municipality	Sector
Above 50 years of age	0.018***	0.000	0.055***	0.003	-0.000	-0.000	0.002*	0.000	-0.000
	(0.006)	(0.001)	(0.016)	(0.002)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
Observations	317,842	356,205	356,210	317,842	356,205	356,039	317,842	356,205	356,039
Baseline dependent	.14	0.00	0.00	.65	.78	.76	.85	.88	.87
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES
Calendar Month FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
LLM FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
LLM X Month FE	YES	YES	YES	YES	YES	YES	YES	YES	YES

Note: Linear regression for growth and stability of employment in new occupation. Columns (1) to (3) report growth in number of employees in the new job between the hiring year and the year of hiring. Columns (4) to (6) report the effect on retention, defined as the share of workers employed in firm/sector/municipality still employed in the same place between the year before the hiring and the year of hiring. Columns (7) to (9) report the effect on employment persistence, defined as the share of workers employed in firm/sector/municipality still employed between the year before the hiring and the year of hiring. Controls include: Full time dummy; gender dummy; log of average daily labor earnings over the six months preceding the month of layoff; dummies for class size of the previous firm; market potential experience; tenure and tenure with fixed term; months worked over the 7 months before layoff; dummies for occupation in previous employment; dummies for age of first contribution to the social security; dummies for ATECO sector at 2 digits in previous employment. All regressions include squared age polynomial with different slopes on the two sides of the cutoff. Estimates based on 356,210 new jobs for subset of layoffs between February 2009 and December 2012 for workers between 46 and 54 years at layoff excluding workers with 49 years of age and 11 months and 50 years of age which end with employment within 4 years. Baseline computed as the average nonemployment duration for workers fired between 49 years of age and 49 years and 11 months of age. Standard errors clustered at local labor market level. Level of significance: * 10%; ** 5%; *** 1%.

Table 8: Effect of Potential Benefit Duration on Employment after first reemployment

VARIABLES	(1) Months New	(2) Months	(3) Job to Job	(4) Tot months new	(5) Tot months	(6) Tot weeks
Above 50 years of age	0.096	0.112	-0.001	0.084	0.242***	0.911***
	(0.085)	(0.085)	(0.004)	(0.082)	(0.075)	(0.317)
Observations	344,280	344,280	342,438	344,280	344,280	344,280
R-squared	0.166	0.167	0.110	0.152	0.144	0.173
Obs	347454	347454	347454	347454	347454	347454
Controls	YES	YES	YES	YES	YES	YES
Month FE	YES	YES	YES	YES	YES	YES
LLM FE	YES	YES	YES	YES	YES	YES
LLM X Month FE	YES	YES	YES	YES	YES	YES

Note: Linear regression on duration of employment over 2 years after reemployment. Column (2) reports the effect of 4 additional months of potential benefit duration on total taxable labor income; Columns (3) and (4) include benefits collected in the first spell; Columns (5) and (6) include all benefits received by workers after the first layoff. Controls include: Full time dummy; gender dummy; log of average daily labor earnings over the six months preceding the month of layoff; dummies for class size of the previous firm; market potential experience; tenure and tenure with fixed term; months worked over the 7 months before layoff; dummies for occupation in previous employment; dummies for age of first contribution to the social security; dummies for ATECO sector at 2 digits in previous employment. All regressions include squared age polynomial with different slopes on the two sides of the cutoff. Sample includes all recipients of unemployment benefits (OUNR) between February 2009 and December 2012 excluding workers fired at 50 years and at 49 years and 11 months.The sample excludes also all workers withb who fojd a job after 3 years since layoff. Baseline computed as the average nonemployment duration for workers fired between 49 years of age and 49 and 11 months of age. Standard errors clustered at local labor market level. Level of significance: * 10%; ** 5%; *** 1%.

Table 9: Heterogeneous Effects on Nonemployment Duration

	(1) Baseline	(2) Centre-North	(3) South-Island	(4) Female	(5) Less then 30 emp	(6) 30+ emp	(7) Permanent	(8) Temporary
Above 50 years of age	6.232***	5.606***	7.229***	5.672***	8.338***	3.029***	8.112***	4.298***
	(0.802)	(1.162)	(1.044)	(1.257)	(1.020)	(1.145)	(1.258)	(0.901)
Panel B: Notemployment (4 years)								
Above 50 years of age	2.044***	2.192**	2.015***	1.672*	3.301***	0.333	3.722***	0.334
	(0.632)	(0.938)	(0.694)	(0.969)	(0.767)	(1.089)	(0.858)	(0.866)
Panel C: log labor income+ benefits								
Above 50 years of age	0.048***	0.044***	0.0528***	0.044***	0.040***	0.049***	0.062***	0.026**
	(0.009)	(0.012)	(0.0120)	(0.014)	(0.012)	(0.015)	(0.014)	(0.012)
Observations	442,964	267,008	175,956	166,087	303,823	139,141	237,790	205,174
Controls	YES	YES	YES	YES	YES	YES	YES	YES
Calendar Month FE	YES	YES	YES	YES	YES	YES	YES	YES
LLM FE	YES	YES	YES	YES	YES	YES	YES	YES
LLM X Month FE	YES	YES	YES	YES	YES	YES	YES	YES

Note: Linear regression for duration of first nonemployment spell and medium term outcomes. Controls include: Full time dummy; gender dummy; log of average daily labor earnings over the six months preceding the month of layoff; dummies for class size of the previous firm; market potential experience; tenure and tenure with fixed term; months worked over the 7 months before layoff; dummies for occupation in previous employment; dummies for age of first contribution to the social security; dummies for ATECO sector at 2 digits in previous employment. All regressions include squared age polynomial with different slopes on the two sides of the cutoff. Sample includes all recipients of unemployment benefits (OUNR) between February 2009 and December 2012 fired from the private sector excluding workers fired at 50 years and one month of age. Sample restricted to workers with previous temporary contract. Baseline computed as the average for workers fired between 49 years of age and 49 years and 11 months of age. Standard errors clustered at local labor market level. Level of significance: * 10%; ** 5%; *** 1%.

Table 10: Effect of Potential Benefit Duration on Pensions and Disability

VARIABLES	(1) Pensioned 4 Years	(2) Disability 4 Years
Above 50 years of age	0.0029***	0.0019
	(0.0005)	(0.0015)
Observations	442,964	442,964
Baseline dependent	.00	.02
Controls	YES	YES
Month FE	YES	YES
LLM FE	YES	YES
LLM X Month FE	YES	YES

Note: Linear regression for the take up of additional unemployment benefits and other programs (pensions and disability) with a flexible squared polynomial on the two sides of the cutoff (50 years of age). Controls include: Full time dummy; gender dummy; log of average daily labor earnings over the six months preceding the month of layoff; dummies for class size of the previous firm; log of the average size of the firm in the 6 months preceding layoff in the municipality (plant); Age of the last firm in years; share of permanent workers in last firm; market potential experience; tenure and tenure with fixed term; months worked over the 7 months before layoff; dummies for occupation in previous employment; dummies for age of first contribution to the social security; dummies for ATECO sector at 2 digits in previous employment. All regressions include squared age polynomial with different slopes on the two sides of the cutoff. Sample includes all recipients of unemployment benefits (OUNR) between February 2009 and December 2012 excluding workers fired at 50 years and at 49 years and 11 months. Baseline computed as the average nonemployment duration for workers fired between 49 years of age and 49 and 11 months of age. Standard errors clustered at local labor market level. Level of significance: * 10%; ** 5%; *** 1%.

Table 11: Regression estimates under different parametrization and estimation strategies

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Notemployment (First Spell After Layoff)					
Above 50 years of age	6.232***	4.441***	7.764***	5.577***	7.353***	7.328***
	(0.802)	(0.468)	(1.217)	(0.858)	(0.560)	(1.209)
	Panel B: Notemployment (4 years)					
Above 50 years of age	2.044***	1.231***	3.013***	1.929***	3.580***	3.345***
	(0.632)	(0.388)	(0.981)	(0.701)	(0.470)	(1.005)
	Panel C: log labor income + all benefits					
Above 50 years of age	0.048***	0.062***	0.031**	0.054***	0.033***	0.032**
	(0.009)	(0.006)	(0.014)	(0.010)	(0.006)	(0.015)
Observations	442,964	442,964	442,964	424,188	103,568	442,964
Polynomial Degree	2	1	3	2	0	0
Donut	1	1	1	3	1	1
Robust Estimation	NO	NO	NO	NO	NO	YES
Non-Prametric band					1	

Note: Linear regression for duration of first nonemployment spell and medium term outcomes. Controls include: Full time dummy; gender dummy; log of average daily labor earnings over the six months preceding the month of layoff; dummies for class size of the previous firm; market potential experience; tenure and tenure with fixed term; months worked over the 7 months before layoff; dummies for occupation in previous employment; dummies for age of first contribution to the social security; dummies for ATECO sector at 2 digits in previous employment. All regressions include squared age polynomial with different slopes on the two sides of the cutoff. Sample includes all recipients of unemployment benefits (OUNR) between February 2009 and December 2012 fired from the private sector excluding workers fired at 50 years and one month of age. Sample restricted to workers with previous temporary contract. Robust estimation performed using the *rdrobust* STATA command and reducing age for workers older than 50 by one month to accommodate for one month donut. In order to simplify the robust estimation procedure, the equation in Column (6) contains only sector at letter level (ATECO classification), province fixed effects and month of layoff fixed effects. Standard errors clustered at local labor market level. Level of significance: * 10%; ** 5%; *** 1%.

Appendix

A Alternative Subsidies between 2009 and 2012

Two main alternative subsidies were available to unemployed workers depending on their working histories and characteristics of the previous employment. First, individuals who were not eligible for the Subsidy for Ordinary Unemployment with Normal Requirement could apply to receive an alternative benefit with reduced requirements (Subsidy for Ordinary Unemployment with Reduced Requirement). Workers were eligible for the subsidy if they had worked at least 78 days or 13 weeks in the last year but they still needed at least 2 years from the first contribution to the social security system. and no requirement on the number of years from the first contribution to social security. This subsidy granted a monetary transfer to compensate up to 180 days of unemployment. As in the main subsidy, the amount was proportional to past wages and workers were granted 35% of the average daily wage in the previous year for the first 120 days and 40% for the following 60 days. The subsidy was also characterized by a very peculiar payment structure as workers could request it in the solar year following the periods of unemployment up to up to the 31st of March. This measure, while still providing some income support, it is considerably less generous than the previous one and, in addition, the delayed payments made it an imperfect substitute with respect to the one we are studying. It is hence not very likely that workers eligible for the full benefit would prefer this subsidy to the other one also because conditionality conditions, although present for the main benefit, were rarely enforced.

Second, workers fired during firm restructuring and mass layoff could access so called Mobility Benefits (*indennita' di Mobilita'* (law 223/1991).[27] This measure provides a long and generous subsidy which was coupled with meetings and activities to improve the occupational perspectives of the worker. Eligibility to the subsidy was based on two main

[27] Here I will describe only the Mobilita' Ordinaria and neglect other kind of related subsidies which involved a lower number of workers.

Table 1: Benefit duration for Mobility

Age	North and Centre	South and Island
Up to 39	12	24
From 40 to 49	24	36
From 50 onwards	36	48

elements with multiple requirements:

- Worker characteristics: at least 12 months of tenure of which 6 of active work and a permanent contract.
- Firm characteristics:
  - Sector and size: Industrial (at least 15 employees in last 6 months); commercial firms (at least 50 employees); cooperatives (at least 15 employees); artisan firms who supply to eligible firms; tourism (at least 50 employees); security (15 firms); plane transportation (from 2013; no restriction in size).
  - Cause of layoff: economic restructuring closing of the activity.

The duration of the subsidy was based on the age and geographic location of the workers and changed over time. Here I report the values for the period before 2012. Values reported in months.

The amount of the subsidy followed the amount for the maximum salary integration computed yearly by the Social Security and it declined over time. The worker received 94.16% of the amount for the first 12 months and 80% of the amount for the remaining period. As it can be seen, this subsidy is substantially more generous than the other and is very attractive to workers. In the age group, I consider in this paper, about 25% of all workers laid off and receiving a subsidy use this subsidy so it has quantitatively important dimension. However, due to the important conditionalities to access the subsidy both for the firm and on the worker, individuals are highly unlikely to be able to freely self-select into the subsidy which make it less likely that our estimates are contaminated by selection bias. Although they might at least partly affect the external validity of the estimates.

Given the sample composition and the heterogenous effects by workers characteristics, the presence of this subsidy has unclear effects on the estimates. As better workers are fired in collective layoff due to plant closure (in this case the firm is forced to fire also its good quality workers), the effect of potential benefit duration could be lower for them. However, if the firm is closing or substantially restructuring, workers might also be losing more firm specific human capital and they have almost by definition a lower probability of recall (which, as I will show later, plays an important role in the effect of potential benefit duration). This would lead potential benefit duration to have a stronger effect on this group of workers.

B Sample Selection for Recipients UB

I start with data for 4,555,104 unemployment subsidies administered between February 2009 and December 2012. I then remove annulled subsidies, duplications and observations with obvious mistakes (e.g number of days of unemployment implied by end of subsidy less than zero). This reduces the sample to 3,811,687 observations. I also drop suspension subsidies and restrict my attention to workers fired between 46 and 54 years of age. This restriction reduces the sample to 647,888 observations but does not affect the validity of my estimates given the local nature of the estimation strategy. I finally drop workers coming from the public sector (about 147,000 observations): these workers mostly come from the education sector and their hiring and firing periods largely coincides with the Italian academic year (fired in June or July and then hired again in September or October). Due to the specific nature of their occupation, this exclusion should make the results more relevant from a policy perspective. After the exclusion of observations with missing data for my variables of interest, I am left with 452,888 layoffs for 353,796 different individuals.

C Effects on First Spell

C.1 Censoring

Data constraint prevents us from running the analysis over a longer time horizon. Censoring or trimming is something frequent in unemployment studies. Card et al. (2007a), for example, exclude all spells longer than 2 years while Schmieder et al. (2012a) censor their spells of nonemployment at 3 years after layoff. As results in Figure 6 show differences in reemployment rates often extend late in the spell and some workers experience intense difficulties in rejoining the workforce. In this section, I explore different censoring strategies to assess how they can impact estimates of behavioral changes. I repeat my estimation for time to the nexty job and censor the maximum number of weeks at different horizons. Results are reported in the tables below. Censoring has an important effect on estimates and each additional year of observation adds about one week (witrh decreasing effects, which hint at a narrowing of the differences between the two groups as time proceeds) to the effects of longer potential benefit duration as shown in Column (2) and Column (3) (results for the baseline estimation reported for comparison in Column (1)). Column (4) reports the results for the full uncensored duration. This kind of estimation has the disadvantage of allowing for different maximum duration for each of layoff but it also allows to fully exploit the data available as now all nonemployment spells are measured up to December 2016. The overall effects is now close to 7 additional weeks in nonemployment, about 75% larger than the one in Column (3). These results suggest that the effects identified represent, to some extent, a lower bound and the addition of more data could allow to fully estimate the effects.

C.2 Transitions towards Public Sector and Self Employment

A limitation of the data is the possibility to only analyze spells in the private sectors while spells as self-employed or in the public sector cannot be accounted for. In this section I use an alternative source: the social security contribution histories of workers

Table 2: Effect of Potential on nonemployment duration with different censoring

VARIABLES	(1) 4 years	(2) 3 years	(3) 2 years	(4) Uncensored
Above 50 years of age	6.232***	5.344***	4.177***	7.099***
	(0.802)	(0.595)	(0.381)	(1.133)
Observations	442,964	442,964	442,964	442,964
Baseline dependent	66.48	57.29	46.51	80.78
Controls	YES	YES	YES	YES
Month FE	YES	YES	YES	YES
LLM FE	YES	YES	YES	YES
LLM X Month FE	YES	YES	YES	YES

Note: Linear regression for the duration of nonemploymnent up to the next job with a flexible squared polynomial on the two sides of the cutoff (50 years of age). Controls include: Full time dummy; gender dummy; log of average daily labor earnings over the six months preceding the month of layoff; dummies for class size of the previous firm; log of the average size of the firm in the 6 months preceding layoff in the municipality (plant); Age of the last firm in years; share of permanent workers in last firm; market potential experience; tenure and tenure with fixed term; months worked over the 7 months before layoff; dummies for occupation in previous employment; dummies for age of first contribution to the social security; dummies for ATECO sector at 2 digits in previous employment. All regressions include squared age polynomial with different slopes on the two sides of the cutoff. Sample includes all recipients of unemployment benefits (OUNR) between February 2009 and December 2012 excluding workers fired at 50 years and at 49 years and 11 months. Baseline computed as the average nonemployment duration for workers fired between 49 years of age and 49 and 11 months of age. Standard errors clustered at local labor market level. Level of significance: * 10%; ** 5%; *** 1%.

in order to check to what extent these spells might be affecting my results. I obtain the full contribution histories for a subset of my sample (workers laid off between 2010 and 2012) and estimate the time to next job with these new data. Although data is classified at annual level, they still report the start date of each contribution[28] These data contain all the contributions made by the worker throughout her life and they allow to observe spells in self-employment and periods of work in the public sector. I compute several measures of time to the next employment with the original and contribution data and report them in Table 3 Results are overall comforting. Column (2) reports the measure of 4 additional months of potential benefit duration as in the main results (reported in Column (1) for comparison) but restricts the sample to workers for whom I also have contribution histories data. Column (5) reports the same measure with the contribution histories and shows only minimal differences in the effect of the longer subsidy and, by and large, also in the average number of weeks in nonemployment. Column (3) and Column (6) report the effect for the nonemployment duration after correcting the date for the end of the benefit with the maximum duration of the benefit (which takes into account some cases in which the date of the benefit seems mismeasured with respect to the expected theoretical duration). Estimates are very consistent with thei previous method and this is

[28]In this case, a worker who works for 3 years for a firm will have 3 observations, with each of them reporting the original start date of the contributions for the worker with that firm.

comforting about the quality of the data. Finally, Column (4) and Column (7) estimate the effects of nonemployment but do not put any restriction to the first employment spell. This allows us to also take into account spells which might have started when the worker was receiving benefits. This change leads to some changes in the estimates, but the effects are still much in line with the other ones both in terms of the nonemployment effect and of comparison between the two data sources. It should be noted that in all the estimates involving contribution data the average duration of the nonemployment spell is quite close with the average duration without these spells This suggests that the use of spells only in the private sector does constitute a strong limitation for the analysis. It should be noted that contribution histories also suffer from some disadvantages. First, they tend to be updated and recompiled after the worker retires to compute the amount due, this makes them less reliable when used for workers who are not collecting benefits. Second, the comparison between the private sector employees data and contribution histories shows some inconsistencies between the start date of a spell or its continuity. In some cases, contribution histories tend to collect together spells with the same employer or postdate the beginning of the employment relation with respect to the other data source. Although results presented in this section hint at only minor differences when contributions histories are used, the considerations just mentioned should lead to use them with care when interested in fine duration measures.

Table 3: Effect of Potential on nonemployment duration with different definitions of time to next employment

VARIABLES	(1) Baseline	(2) Same Sample	(3) After end UB - Corr	(4) After end UB - No Restr	(5) Estratti Conto	(6) Estratti Conto - Corr	(7) Estratti Conto - No Restr
Above 50 years of age	6.232***	6.045***	6.050***	5.600***	6.059***	6.060***	5.913***
	(0.802)	(0.887)	(0.886)	(0.876)	(0.876)	(0.875)	(0.840)
Observations	440,398	350,036	350,036	350,036	350,036	350,036	350,036
Baseline dependent	66.48	66.67	66.64	65.31	64.12	64.09	61.2
Controls	YES	YES	YES	YES	YES	YES	YES
Month FE	YES	YES	YES	YES	YES	YES	YES
LLM FE	YES	YES	YES	YES	YES	YES	YES
LLM X Month FE	YES	YES	YES	YES	YES	YES	YES

Note: Linear regression for the duration of nonemploymnent up to the next job with a flexible squared polynomial on the two sides of the cutoff (50 years of age). Controls include: Full time dummy; gender dummy; log of average daily labor earnings over the six months preceding the month of layoff; dummies for class size of the previous firm; log of the average size of the firm in the 6 months preceding layoff in the municipality (plant); Age of the last firm in years; share of permanent workers in last firm; market potential experience; tenure and tenure with fixed term; months worked over the 7 months before layoff; dummies for occupation in previous employment; dummies for age of first contribution to the social security; dummies for ATECO sector at 2 digits in previous employment. All regressions include squared age polynomial with different slopes on the two sides of the cutoff. Sample includes all recipients of unemployment benefits (OUNR) between February 2009 and December 2012 excluding workers fired at 50 years and at 49 years and 11 months. Baseline computed as the average nonemployment duration for workers fired between 49 years of age and 49 and 11 months of age. Standard errors clustered at local labor market level. Level of significance: * 10%; ** 5%; *** 1%.

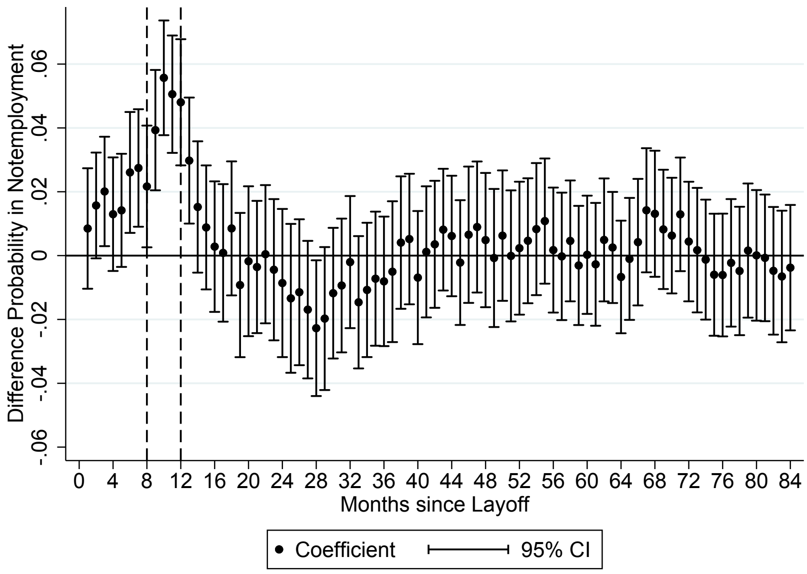
D Medium Term effects and new employment characteristics

D.1 Effect over 7 years

The higher level of nonemployment at the of the 4 years after layoff could suggest that the differences between the two groups are characterized by cycles of employment and nonemployment. This could increase or decrease the effect on aggregate nonemployment depending on the time of observation and amplitude of the cycle. To check if this kind of dynamic affects the results in a substantial way, I focus on workers fired in 2009 who can be observed up to 7 years after layoff and look at the differences between workers with initial longer and shorter unemployment benefits. I look at outcomes in terms of time to the next employment, aggregate total nonemployment over 7 years and aggregate earnings. Results of the estimation are reported in Table 4. In this case, the differences between the estimation on the first spell and overall effect are even more striking. Column (1) shows that the effect on the time to the next job is much larger than the one estimated in aggregate, but the difference with number of weeks in nonemployment, which is reported in Column (2), is even more striking. Column (3) shows that the effect on log of labor income and benefits is similar to what observed before. Column (4), Column (5) and Column (6) report the corresponding effects over a 4 years horizon for the sake of comparison. In all cases, results are in line with previous estimates and suggest that these results are indicative for the remaining part of the sample.[29] It worth noticing that also in this case, the sharp decline in the coefficient for weeks of not employment is not matched by a decline in precision as standard errors remain quite comparable. In addition, to check whether the difference in the two groups follows a cyclical pattern, I also check employment over 7 years, and coefficients for monthly estimates are reported in Figure 1. In this case, convergence is even stronger, and the two groups have the same employment

[29]This year is the first year of the Great Recession and we could have expected fairly different results (Schmieder et al., 2012a; Card et al., 2015)

Figure 1: Employment pattern for workers fired in 2009 for 7 years



Note: Effect of 4 additional months of potential benefit duration on probability of being employed at t months after layoff. The worker is considered employed if she works at least one day during the corresponding month. Regressions includes a square polynomial in age with different slopes around the cutoff, controls for the worker and last firm characteristics, local labor market interacted with month fixed effects. Figure based on 92,928 layoffs between February 2009 and December 2009 for workers between 46 and 54 years at layoff excluding workers with 49 years of age and 11 months and 50 years of age. Standard Errors clustered at Local Labor Market level. Confidence interval at 95% reported.

probability in the long run. The difference follows a pattern similar to the one observed for the full sample: a small anticipation effect, an increase in the divergence between the 8^{th} and the 12^{th} months, and a sharp decline after the 12^{th} month. The two groups fully converge after 36 months and they remain the same for the remaining 4 years. This picture provides suggestive evidence that the two groups converge in the long run and, if anything, the difference in overall employment tends to become smaller. Additional data for the other years would allow us to better understand to what extent these finding can be generalized to the rest of the sample.

Table 4: Effect of Potential Benefit Duration on Medium Term Outcomes

VARIABLES	(1) Weeks	(2) Weeks (7y)	(3) All Income (W+B)	(4) Weeks	(5) Weeks (4y)	(6) Log Inc (W+B)
Above 50 years of age	9.333***	0.880	0.057***	7.001***	1.008	0.054***
	(2.554)	(2.121)	(0.021)	(1.478)	(1.247)	(0.018)
Observations	92,278	92,278	92,275	92,278	92,278	92,278
Baseline dependent	89.07	225.58		65.73	128.05	
Obs	92928	92928	92928	92928	92928	92928
Controls	YES	YES	YES	YES	YES	
Month FE	YES	YES	YES	YES	YES	YES
LLM FE	YES	YES	YES	YES	YES	YES
LLM X Month FE	YES	YES	YES	YES	YES	YES

Note: Linear regression on duration nonemployment with a 7 years horizon for workers fired in 2009. Column (1) reports the effect for the time to the next job; Column (2) reports the effect on the total duration of nonemployment over 7 years; Columns (3) reports the effect of longer potential benefit duration on log of labor income and total benefits; Columns (4), Column (5), and Column (6) perform the same set of regressions but restricting the sample to 4 years after layoff. Controls include: Full time dummy; gender dummy; log of average daily labor earnings over the six months preceding the month of layoff; dummies for class size of the previous firm; market potential experience; tenure and tenure with fixed term; months worked over the 7 months before lay-off; dummies for occupation in previous employment; dummies for age of first contribution to the social security; dummies for ATECO sector at 2 digits in previous employment. All regressions include squared age polynomial with different slopes on the two sides of the cutoff. Sample includes all recipients of unemployment benefits (OUNR) between February 2009 and December 2009 excluding workers fired at 50 years and at 49 years and 11 months. Baseline computed as the average nonemployment duration for workers fired between 49 years of age and 49 and 11 months of age. Standard errors clustered at local labor market level. Level of significance: * 10%; ** 5%; *** 1%.

D.2 Effect on New Job Characteristics

Table 5: Effect of Potential on new job characteristics

VARIABLES	(1) Age New Firm	(2) % permanent	(3) % full time	(4) % male	(5) Average Age	(6) Average Monthly Income	(7) (log) size	(8) Tenure	(9) Full Time	(10) Permanent	(11) (log) Daily Wage
Above 50 years of age	1.130***	-0.000	-0.000	-0.001	0.013	-0.002	-0.021	0.235	0.005	0.010**	0.006
	(0.106)	(0.003)	(0.003)	(0.003)	(0.062)	(0.006)	(0.014)	(0.760)	(0.003)	(0.004)	(0.005)
Observations	356,210	340,504	340,504	340,504	340,483	339,731	354,571	356,210	356,210	356,210	352,241
Baseline dependent	195	.56	.75	.64	40.21	6.96	2.66	66.92	.77	.26	4.07
Se dependent	174.46	.38	.3	.32	6.2	.62	1.61	77.65	.42	.44	.47
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Month FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
LLM FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
LLM X Month FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Note: Linear regression for the duration of nonemploymnent up to the next job with a flexible squared polynomial on the two sides of the cutoff (50 years of age). Controls include: Full time dummy; gender dummy; log of average daily labor earnings over the six months preceding the month of layoff; dummies for class size of the previous firm; log of the average size of the firm in the 6 months preceding layoff in the municipality (plant); Age of the last firm in years; share of permanent workers in last firm; market potential experience; tenure and tenure with fixed term; months worked over the 7 months before layoff; dummies for occupation in previous employment; dummies for age of first contribution to the social security; dummies for ATECO sector at 2 digits in previous employment. All regressions include squared age polynomial with different slopes on the two sides of the cutoff. Sample includes all recipients of unemployment benefits (OUNR) between February 2009 and December 2012 excluding workers fired at 50 years and at 49 years and 11 months. Baseline computed as the average nonemployment duration for workers fired between 49 years of age and 49 and 11 months of age. Standard errors clustered at local labor market level. Level of significance: * 10%; ** 5%; *** 1%.

E Recall

Recall is an important and pervasive phenomenon in the labor market. Workers who have been laid off have a high likelihood of being hired by the firm which laid them off in the first place. Feldstein (1976) underlined the relevance of this phenomenon and tried to build a theoretical framework to conceptualize it in relation with unemployment benefits. More recently several works started to revisit this dynamic using richer and novel administrative data. Nekoei and Weber (2015) underline the role of recall in the observed hazard rate for exit towards employment in Austria while Fujita and Moscarini (2017) provide strong evidence on the relevance of this phenomenon in the US. In addition, they find that the share of recalls is large also for permanently separated workers and rationalize it in a search and matching framework with large employer costs to hire new workers. In my setting, recalls are a pervasive phenomenon and 42% of workers finding a job within 4 years are employed by the same firm. This share is higher for workers coming from temporary contracts with more than 50% being recalled in the same firm. This dynamic is important for the effects of unemployment benefits as workers who have the option to come back to the same firm will experience a different search with respect to other workers. In addition, workers may bargain with the firm to time their hiring with the end of unemployment benefits. As a first step, it is useful to characterize recalls[30] and assess what are the characteristics that make more likely the hiring by the same employer. Table 6 reports a series of regression for workers in our sample with dependent variable equal to 1 if the worker is hired by the same firm and 0 if she is hired by another firm. The regression shows that workers in larger firms, women, and worker for temporary contract have a higher probability of recall. Workers with longer tenure, especially in temporary contracts, have also a higher probability of being recalled.[31] Finally, recall are more frequent for blue collar and apprentices and in cyclical sectors.[32] As shown before,

[30]Information on the expectation of recall available and so here I will focus only on the realized recalls.

[31]This should not be taken for granted as there are legislative limits to the maximum number of years with fixed term contracts with the same firm. In practice, these limits can be easily circumvented by changing a few elements in the contract.

[32]They are the sectors which experience within year changes in the labor force greater than 10% in a panel regression between 2005 and 2008 with quadratic trends and year fixed effects.

being eligible to longer slightly reuces the probability of recall.

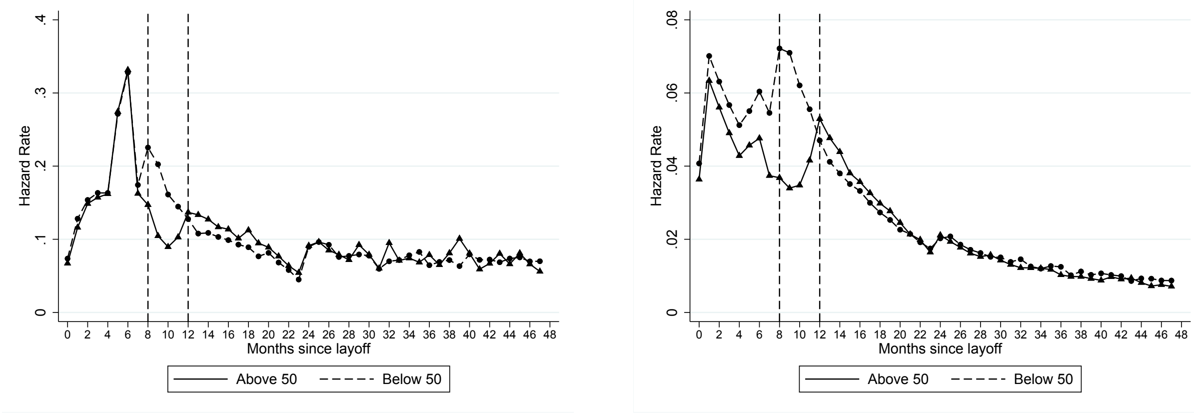
Table 6: Observables and Probability of Recall

	(1)	(2)	(3)	(4)
	Overall	Overall	Permanent	Temporary
Share Permanent in Last Firm	-0.134***	-0.053***	-0.077***	-0.135***
	(0.008)	(0.009)	(0.011)	(0.009)
Age Last Firm	0.000***	0.000***	0.000***	0.000***
	(0.000)	(0.000)	(0.000)	(0.000)
Log Avg. Size Firm	0.010***	0.014***	-0.007***	0.024***
	(0.002)	(0.002)	(0.002)	(0.002)
Full Time	0.006	-0.015***	0.010**	-0.041***
	(0.006)	(0.003)	(0.004)	(0.005)
Female	0.054***	0.064***	0.036***	0.068***
	(0.004)	(0.004)	(0.005)	(0.004)
Permanent last 6 months	-0.037***	-0.092***	-0.140***	0.073***
	(0.007)	(0.005)	(0.035)	(0.009)
Log Daily Tax Income	-0.052***	-0.048***	-0.032***	-0.046***
	(0.008)	(0.004)	(0.004)	(0.007)
Log Total tax income (7 Months)	0.069***	0.068***	0.020***	0.101***
	(0.005)	(0.004)	(0.003)	(0.005)
Market Potential Experience	-0.002***	-0.002***	-0.002***	-0.002***
	(0.000)	(0.000)	(0.000)	(0.000)
Tenure	0.003***	0.004***	0.004***	0.015***
	(0.001)	(0.000)	(0.001)	(0.001)
Tenure Temporary	0.062***	0.052***	0.033***	0.034***
	(0.003)	(0.002)	(0.003)	(0.002)
Months Worked Last (7 Months)	-0.016***	-0.015***	0.003*	-0.022***
	(0.001)	(0.001)	(0.002)	(0.001)
Apprentice	0.243***	0.124**	0.161	0.083
	(0.055)	(0.060)	(0.144)	(0.060)
White Collar	-0.112***	-0.078***	-0.049***	-0.075***
	(0.007)	(0.004)	(0.005)	(0.008)
Manager	-0.255***	-0.138***	-0.042***	-0.190***
	(0.016)	(0.015)	(0.012)	(0.040)
Cyclical Sector	0.148***			
	(0.011)			
Fired after 50	-0.012***	-0.010**	-0.007	-0.011
	(0.004)	(0.004)	(0.006)	(0.006)
Observations	357,227	357,227	173,941	183,286
Baseline dependent	.421	.421	.29	.545
Month FE	NO	YES	YES	YES
LLM FE	NO	YES	YES	YES
LLM X Month FE	NO	YES	YES	YES

Note: Linear probability model for the probability of being recalled. Dependent variable equal to 1 if the worker is hired by the same firm and 0 she is hired by another firm. Controls include: Full time dummy; gender dummy; log of average daily labour earnings over the six months preceding the month of layoff; dummies for class size of the previous firm; market potential experience; tenure and tenure with fixed term; months worked over the 7 months before layoff; dummies for occupation in previous employment; dummies for age of first contribution to the social security; dummies for ATECO sector at 2 digits in previous employment. All regressions include squared age polynomial with different slopes on the two sides of the cutoff. Sample includes all recipients of unemployment benefits (OUNR) between February 2009 and December 2009 excluding workers fired at 50 years and at 49 years and 11 months. Baseline computed as the average not employment duration for workers fired between 49 years of age and 49 and 11 months of age. Standard errors clustered at local labor market level. Level of significance: * 10%; ** 5%; *** 1%.

As a second step, it is interesting to check the role of recalls in the hazard rate towards employment and on the estimates of the effect of unemployment benefits. First, I plot hazard rates for workers recalled and not recalled. Results, reported in Figure 2, show a more prominent negative dependence in the hazard rate of workers not hired by the same firm, consistently with evidence for Austria (Nekoei and Weber (2015)). Hazard rates for these workers are also, in general, much smaller than those for other workers but this is partly mechanical as not recalled workers also include workers who do not find a job after layoff. It is also worth pointing out that the large spike previously observed at 6 months characterizes mostly recalled workers while little can be seen for workers not hired by the

Figure 2: Probability of Employment following reemployment



(a) Hazard Rate for Recalled Workers (b) Hazard Rate for Not Recalled Workers

Note: Hazard rate for exit of workers from nonemployment towards employment in the private sector. Hazard rate computed as the share of workers exiting nonemployment in month t over the number of workers still unemployment after t-1 months.

same firm. This shows neatly how the pattern observed for the overall sample reflects recurrent employment spells which are particularly common in tourism and other seasonal sectors.

Finally, I assess the role of recalls for the estimates of unemployment benefits. Recalls could potentially play an important role: workers could bargain with the employer the time of their recall to match the duration of their unemployment benefit. Hence, they could generate large behavioral responses. However, it is also possible that recalls have to match production needs and workers are not able to fully extract the value of unemployment benefits. In this case, the potential benefit duration would not matter for them. To investigate these effects, I estimate my preferred specification for workers who are recalled and who are not. Note that results in this estimation are not fully comparable to those in the main specification as the sample is restricted only to workers who eventually find a job within the 4 year time horizon. Estimates, reported in table 7, show that recalled workers are not responsive to potential benefit duration and they show insignificant effects for all the variables of interest. Workers who are not recalled show responses very similar to the ones in the main equation. This shows that results are largely driven for workers facing *ex novo* searches in the labor market. This is consistent with the fact that a large share of recalls takes place in the first six months of the nonemployment spells: the share of workers who is recalled is close to 60% among those finding a job at 6 months in the spell whereas the share declines to 40% two months later and to 20 % at 12 months. Even after 4 years of nonemployment still about 10% of the workers are recalled by the same firm.

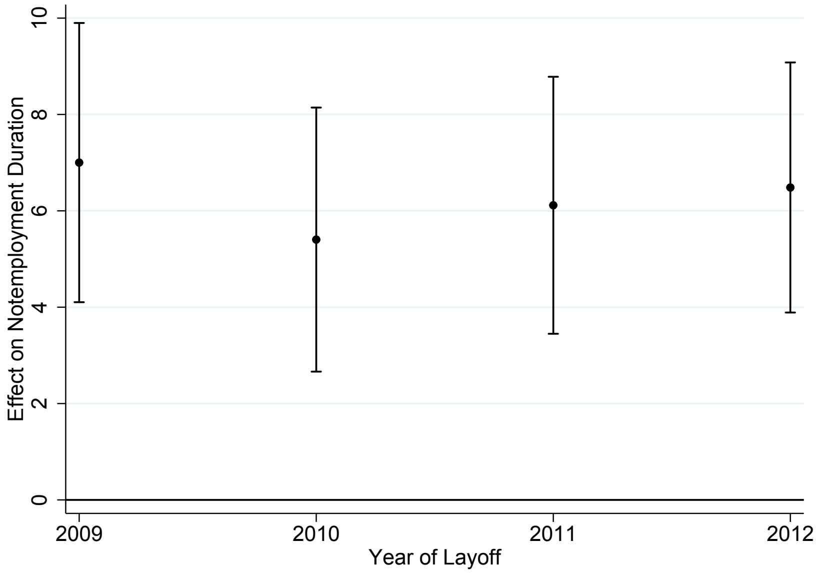
Table 7: Effects of longer potentila benefit duration for workers recalled or changing firm

	Recall			Not Recall		
	(1) Nonempl	(2) Nonemp (4 years)	(3) (log) W+B	(4) Nonempl	(5) Nonempl (4 years)	(6) (log) W+B
Above 50 years of age	1.898	0.0188	0.0118	6.398***	1.948**	0.0624***
	(1.309)	(0.0192)	(0.0181)	(1.238)	(0.925)	(0.0147)
Observations	149,608	149,608	149,608	206,602	206,601	206,601
Baseline dependent	109.35		71.52	133.26		
Controls	YES	YES	YES	YES	YES	YES
Month FE	YES	YES	YES	YES	YES	YES
LLM FE	YES	YES	YES	YES	YES	YES
LLM X Month FE	YES	YES	YES	YES	YES	YES

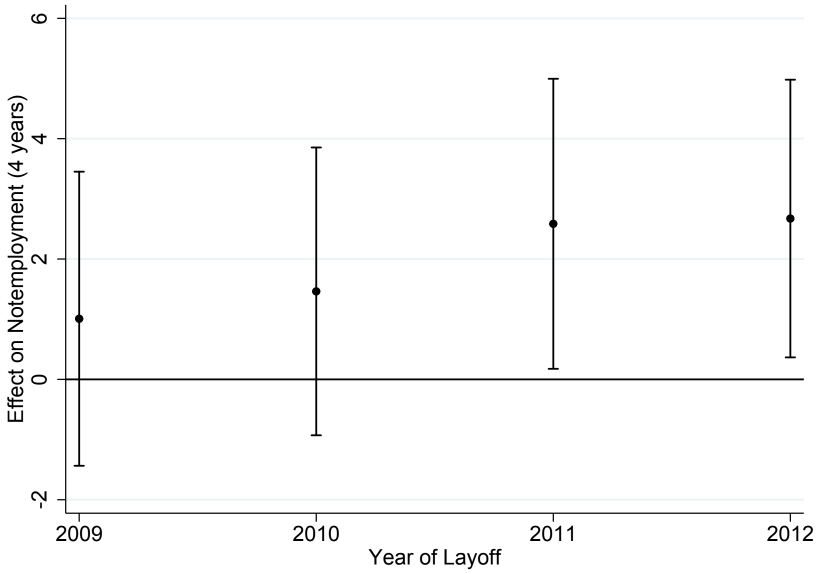
Note: Linear regression for the effects of unemployment benefits on main variables of interest. Sample restricted to workers finding a job within 4 years. Controls include: Full time dummy; gender dummy; log of average daily labor earnings over the six months preceding the month of layoff; dummies for class size of the previous firm; market potential experience; tenure and tenure with fixed term; months worked over the 7 months before layoff; dummies for occupation in previous employment; dummies for age of first contribution to the social security; dummies for ATECO sector at 2 digits in previous employment. All regressions include squared age polynomial with different slopes on the two sides of the cutoff. Columns (1) and (4) report effect on first nonemployment spell after layoff; Columns (2) and (5) report effects on nonemployment over 4 years; Columns (3) and (6) report the effect on labor earnings and benefits over 4 years. Sample includes all recipients of unemployment benefits (OUNR) between February 2009 and December 2009 excluding workers fired at 50 years and at 49 years and 11 months. Baseline computed as the average not employment duration for workers fired between 49 years of age and 49 and 11 months of age. Standard errors clustered at local labor market level. Level of significance: * 10%; ** 5%; *** 1%.

F Heterogenous Effects: by year and sector

Figure 3: Effects on Nonemployment in first spell and over 4 years by year of layoff



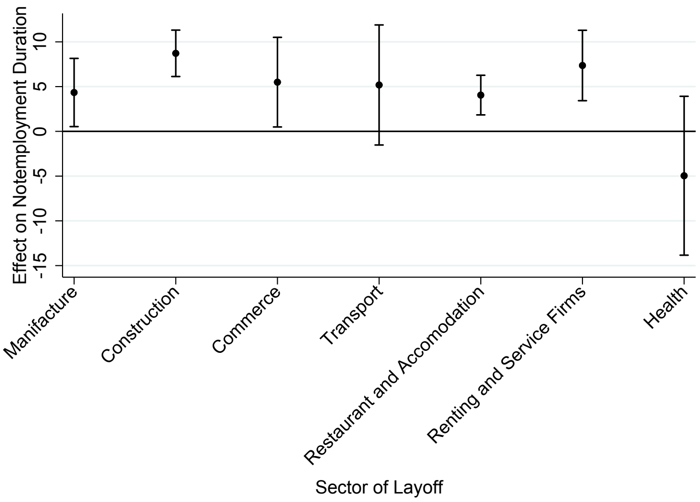
(a) Effect on Nonemployment



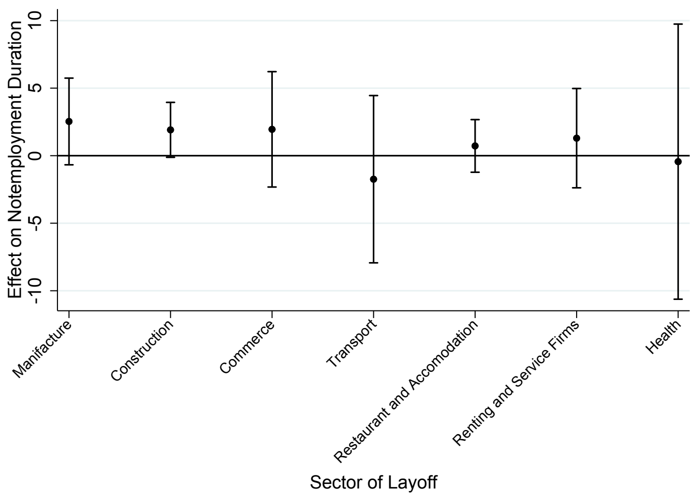
(b) Effect on Nonemployment (4 years)

Note: Effect of 4 additional months of potential benefit duration on nonemployment in the first spell (a) and nonemployment over 4 years (b). Regressions includes a square polynomial in age with different slopes around the cutoff, controls for the worker and last firm characteristics, local labor market interacted with month fixed effects. Figure based on 442,964 layoffs between February 2009 and December 2012 for workers between 46 and 54 years at layoff excluding workers with 49 years of age and 11 months and 50 years of age. Standard Errors clustered at Local Labor Market level. Confidence interval at 95% reported.

Figure 4: Effects on Nonemployment in first spell and over 4 years by broad NACE sector



(a) Effect on Nonemployment

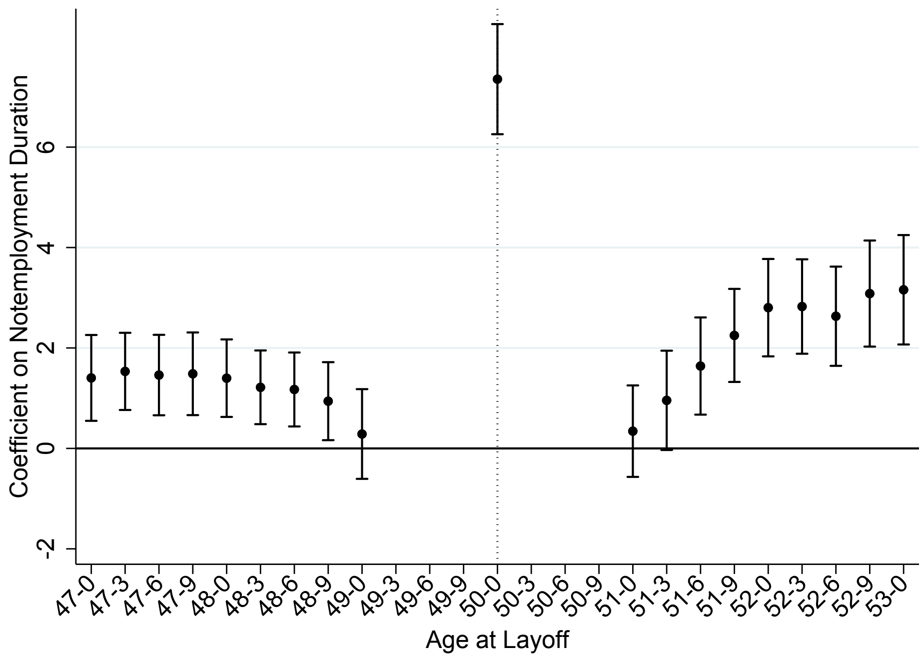


(b) Effect on Nonemployment (4 years)

Note: Effect of 4 additional months of potential benefit duration on nonemployment in the first spell (a) and nonemployment over 4 years (b). Regressions includes a square polynomial in age with different slopes around the cutoff, controls for the worker and last firm characteristics, local labor market interacted with month fixed effects. Figure based on 442,964 layoffs between February 2009 and December 2012 for workers between 46 and 54 years at layoff excluding workers with 49 years of age and 11 months and 50 years of age. Standard Errors clustered at Local Labor Market level. Confidence interval at 95% reported.

G Placebo with Non Parametric RDD

Figure 5: Hazard Rate for Exit from nonemployment



Note: Placebo linear regression for duration of nonemployment in the first spell. Figure based on 442,964 layoffs between February 2009 and December 2012 for workers between 46 and 54 years at layoff excluding workers with 49 years of age and 11 months and 50 years of age. Controls include: Full time dummy; gender dummy; log of average daily labor earnings over the six months preceding the month of layoff; dummies for class size of the previous firm; market potential experience; tenure and tenure with fixed term; months worked over the 7 months before layoff; dummies for occupation in previous employment; dummies for age of first contribution to the social security; dummies for ATECO sector at 2 digits in previous employment. Coefficient at 50 years of age corresponds to polciy induced change in potential benefit duration. Fake and main RDD regression use a 1 year bandwidth. Standard Errors clustered at Local Labor Market level. Confidence interval at 95% reported.

The Medium-Term Effects of Unemployment Benefits

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I WORK*INPS* PAPER

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Pietro Garibaldi