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 months 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 permanent and full time 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. Frequent transitions between employment and nonemployment, and a faster transition of workers with longer benefits towards new firms 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.

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1 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 spend more time in unemployment when covered by more generous benefits. These two components (insurance and negative fiscal externalities) constitute the building blocks 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 may 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 faster between jobs. In addition, workers can experience frequent transitions between employment and nonemployment and this could contribute to reduce the differences in employment for workers exposed to benefits with different generosity. 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 (PBD) on employment, earnings and transfers over a 4 years 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 of PBD relies on a quasi-experimental source of variation induced by an age-at-layoff rule: workers who were fired after turning 50 years of age were eligible to 12 months of PBD while other workers were eligible to 8 months of PBD. I exploit this variation in a sharp Regression

Discontinuity Design (RDD) strategy. Consistently with previous findings in the literature, I find that longer PBD increases the duration of the nonemployment spell following layoff: workers exposed to longer PBD spend more time on benefits and in nonemployment before finding 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 and full time job. The effects on other job characteristics, most notably wages and tenure, are small, positive but not statistically significant. However, if we look at the full 4 years period after layoff, workers with initially longer benefits spend only 2 more weeks in nonemployment with respect to workers initially eligible to shorter benefits. Consistently, after their first spell on benefits, they also spend 2 weeks less on unemployment benefits. This shows that, over the medium run, workers with longer benefits almost entirely close the gap in terms of nonemployment with respect to other workers. The discrepancy between these effects is determined by two different elements: first, all workers tend to experience again job losses and, as this affects more workers with shorter benefits, it leads to a narrowing in the employment rate gap between the two groups; second, workers with longer benefit duration find a second job quicker than workers with shorter benefit duration. Employment seasonality and temporary peaks in economic activity possibly play an important role as a relevant share of layoff happens close to one year after the first layoff. The first element explains about three quarters of the difference between the first spell and medium run estimates while the remaining quarter is related to a quicker transition to a second job. Gains in terms of better match between the worker and the first firm do not play a central role: longer PBD has only a small and not significant effect on the duration of the first job. These results are robust to a wide range of robustness and identification checks and a rich heterogeneity analysis shows that they apply to a large set of economic conditions and to a wide range of workers. Workers from small firms and permanent contracts are in general the most affected by longer PBD but, also for them, medium term estimates are substantially smaller than the effect on the first nonemployment spell duration. This has implications for the fiscal externalities of unemployment benefits. Indeed, by receiving benefits for longer time and paying less taxes, workers exposed to more generous benefits create costs that have to be financed by general taxation. The present results show that, while the first element can be quite substantial, as workers indeed change their search as a consequence of more generous benefits, the second channel is much less important than what a simple analysis of the time to the next job might suggest. Moreover, I show that workers initially exposed to longer benefits also have a lower probability of getting benefits in the future, which directly offset part of the first element. This, in turn, leads to a downward correction of the costs of longer benefits and could lead to increase the optimal generosity of unemployment benefits. As data on consumption are not available in my setting, I leave a more precise computations on the optimality of the benefits to future research.

This paper contributes to an extensive literature on the effects of unemployment benefits. There is a general consensus on the positive effect of PBD (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 the effect of an additional week of benefit: estimates 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: Schmieder et al. (2016) find small negative effects on future wages whereas Nekoei and Weber (2017) identify small positive effects. Several works also estimated the effects of unemployment benefits generosity. in terms of benefit amount, on nonemployment duration. Lalive et al. (2006) exploit a policy change in Austria in 1989 and investigate the effects of changes in generosity and PBD. 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 a vast literature on the effects of UB, 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 in PBD for workers with more than 55 years of age at layoff. Their results show that this 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 for workers born after 1950. They find that a postponement in this threshold 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 the present work as they exploit an age based discontinuity in PBD 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 unemployment benefits 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 benefits 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 PBD close relatively quickly the gap in employment with respect to workers with shorter benefits. This is related to both frequent transitions between employment and nonemployment and to faster transitions of workers with longer benefits towards a second job afterwards. 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 2 describes the Italian institutional setting; Section 3 provides a description of the data and of the sample construction; Section 4 outlines the identification strategy and methodological approach; Section 5 reports the main results and discusses the mechanism at work; Section 6 looks at heterogeneous effects; Section 7 implements a series of robustness checks; finally Section 8 concludes.

2 Institutional Context

In this study, I focus on the main unemployment benefit which characterized the Italian Welfare system up to 2013: the unemployment benefit for Ordinary Unemployment with Normal Requirement (*Disoccupazione Ordinaria a Requisiti Normali*, OUNR throughout the rest of the paper). This UB was introduced at the eve of World War II (*Regio Decreto 14th April 1939*) and later progressively extended in both coverage and generosity. By the start of the new millennium, all employees in the non-agricultural sector were eligible, conditional on a few requirements.[1] Its structure and generosity were modified several times over the years but in the period under study, from 2009 to 2012, its characteristics were similar to policies in many other European economies (Austria, Nekoei and Weber, 2017; Germany, Caliendo et al., 2013 and Schmieder et al., 2012a). In this period, PBD 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 could receive up 12 months of subsidy (52 weeks or 365 days). The amount was proportional to average wages in the 3 months preceding layoff with a declining schedule over the unemployment spell. Workers received 60% of their average wage for the first 6 months of the subsidy, 50% for the following 2 months

[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 benefit. A new unemployment subsidy was introduced for them in 2015 (*DIS-COL*).

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

and 40% for the remaining 4 months, if still eligible. The transfer was capped by law and the threshold was yearly updated by the social security.[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 layoff, and 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 benefit. Differently from other settings, such as the Austrian one (Jäger et al., 2018), workers who quit their job were generally not entitled to receive unemployment benefits, while workers who were 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 (about 192 euro per month for 2012). The duration and generosity of the benefit were revised several times over the years. In this paper, I use data for the period between 2009 and 2012 as this allows for a uniform institutional framework.

It should be noted that two additional benefits, the benefit for Ordinary Unemployment with Reduced Requirement (*Disoccupazione Ordinaria a Requisiti Ridotti*) and the Mobility Benefit (*Mobilità*), were available to unemployed workers. Their presence is, however, unlikely to generate endogenous selection and hence bias my estimates. The former was a benefit with lower requirements (13 weeks worked in the last year but still 2 years since first contribution) and generosity. In addition, it could be requested only the year after the period of unemployment which made it less attractive with respect to the one under study. The latter was substantially more generous, but it was also characterized by more stringent access conditions. Indeed, workers needed to have a permanent contract, 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 sector specific size requirements and the state of economic distress, which allowed for collective layoff, had to be certified

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

before workers could apply to receive this benefit. While the availability of this benefit will have compositional effects on the sample, the strong conditionality and the presence of numerous exogenous constraints for eligibility made it difficult for workers to self-select into this measure. I provide additional details and discussion about these two policies in appendix A.

3 Data and sample

The analysis is based on two main sources: the register for recipients of unemployment benefits and the 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 benefit. 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 January 2009, which leaves me with a sample period from February 2009 to December 2012 as the benefit 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 worker level data for the workforce composition of firms, 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

[4]Data on previous years would provide a relatively small contribution as the structure of the benefit 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.

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 benefit has a different structure and they still keep a close relationship with their previous employer.[7] The final dataset contains 452,888 spells for 328,835 workers.[8]

As the dataset on benefits 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 benefit. 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 date for employment after the end of the last job. This correction involves only a marginal number of spells (about 1,000). A 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.3 checks using social security contributions histories to assess the sensitivity of my results to these transitions. Results are qualitatively robust and quantitatively close to the main estimates. 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 variable could provide an imprecise measure of the work status as transitions outside the labour 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

[6]The large number 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]These are classified as suspended workers.

[8]I provide additional details on the sample definition in Appendix B.1.

sample (this concerns 10% of the sample for temporary contracts and 20% for permanent contracts, 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 its censored counterpart) in nonemployment before finding a new job. About 60% of the workers finds a new job within the first year from layoff, but about one third of them does not find a job even 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 periods of nonemployment are, at least in part, a normal component of the employment relationship for workers from temporary contracts. Most of the recipients 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 benefits for workers coming from large firms under certain circumstances[9] and with more rigid employment protection legislation for workers in these firms. Indeed, the possibility not only of monetary compensation but also of reintegration for workers fired without just cause (economic or disciplinary) created high level of cost uncertainty for firms firing workers with permanent contracts. On average workers have 1.376 spells starting between February 2009 and December 2012, mostly due to the frequent transition towards nonemployment of workers with temporary contracts.

In terms of sector composition, manufacturing makes up about 20% of the sample while Construction and Tourism (Restaurant and Accommodation) represent about 40% of the sample. Firm Services and Commerce constitute another 20% of the sample while the rest is divided among 15 smaller sectors. A summary for the sector composition of the sample is reported in Figure 1.

[9]See Appendix A.

4 Methodology

The identification of the causal effects of treatment is based on a quasi-experimental variation in PBD. I exploit the structure of the PBD 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). As workers who were fired after turning 50 years of age received 4 additional months of PBD, I compare individuals fired around the 50 years of age at layoff threshold. Under the identifying assumption that individuals are fired randomly around the cutoff, the two groups should have similar characteristics and this strategy allows to identify the causal effects of longer PBD. In practice, I estimate the following equation:

$$
\begin{aligned}
y_{ist} = \beta_0 + \beta_1 I(\widetilde{Age}_{it} \geq 0) + \sum_{j=1}^{k} \gamma_j \widetilde{Age}_{it}^{j} + \sum_{j=1}^{k} \delta_j \widetilde{Age}_{it}^{j} \mathbb{1}(\widetilde{Age}_{it} \geq 0) + \\
+ X_{ist}'\pi + \mu_{st} + \epsilon_{ist} \qquad (1)
\end{aligned}
$$

where the outcome of interest (y_{ist}) for individual i, fired in local labour market s at time t, is regressed on a k^{th} order polynomial in 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 ($\mathbb{1}(Age_{it} \geq 50)$). Our coefficient of interest is β_1, which identifies the effect of longer PBD. Main results are based on a second order polynomial but findings are robust to different parametric choices and estimation as shown in Section 7. The model also includes a rich set of controls for demographics, the previous firm and occupation of the worker (X_{ist}), such as dummies for female, white collar, full time and permanent contract, the log of daily wage and of the average monthly wage in the last three months before layoff, market potential experience, tenure with any contract and with temporary contracts, the share of permanent contracts in the last firm together with the age and the (log) size of the last firm, as well as dummies for the sector of activity of the last firm at 2 digits level (NACE 2007 classification). I also include fixed effects at month and local labour market level (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 labour market. Standard errors are clustered at local labour market (LLM) level but I also experiment with other cluster levels and results are robust to different choices.

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 verify 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 2. 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., 2019). 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): I exclude the first two bins before and the first one after the cutoff which are the ones most affected by manipulation. This is consistent with the analysis developed in Citino et al. (2019). I perform several robustness checks for this choice in Section 7 and results are largely in line with the main specification.

Then, I check for possible discontinuities in observables. I plot the average of characteristics by age at layoff in months in Figure 3. In most of the cases, observables are reasonably continuous at cutoff despite the strategic behaviour of workers but there are sizeable jumps in a few instances. I replicate the above analysis in a regression framework to assess the magnitude of these discontinuities and to what extent my strategy can mitigate this problem: I regress the observables on a square polynomial in age, flexible on the two sides of the cutoff, on a dummy for being laid off after turning 50, and on the set of interacted months and LLM fixed effects. Table 2 reports the results of this exercise. The rich set of fixed effects seems unable to capture all the sorting and several variables show highly statistically significant but quantitatively small jumps: workers on right of the cutoff are more likely to be women, to have a white collar job, to have a permanent contract, to have lower tenure in temporary contract, to come from smaller firms, and from firms with a larger share of permanent workers. However, once the donut region is removed, all the discontinuities but one are no longer detectable. It is worth pointing out that this result is mostly determined by a lower coefficient rather than lower precision of the estimates, which provides evidence in favour 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 size of the jump is limited (2 weeks with respect to an average of 1 year). These findings show that the donut strategy is effective in solving issues concerning strategic sorting.

5 Results

5.1 Effects on Benefits and Nonemployment

First, I look at the effects of longer PBD on the nonemployment spell immediately following layoff and I visually inspect whether the 4 additional months of coverage lead to a longer period collecting unemployment benefits and nonemployment spell. Both measures are relevant from a policy perspective: the former provides a measure of the direct effects of the potential duration on public expenditure through longer benefit duration; the latter characterizes the unemployed behavioural response. Figure 4 plots the average number of weeks of benefits 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 behaviour by the workers. Indeed, this effect combines two different components: the mechanical response, which is related to a better coverage of a possibly long unemployment spell; the behavioural response, which represents the additional time spent in nonemployment by workers as a consequence of changes in their search strategy. In order to identify the latter, I move to the number of weeks of nonemployment reported in Figure 5. Also in this case a clear jump can be detected, although smaller than the one for benefits: 4 additional months of PBD lead to 6.5 additional weeks in nonemployment.

I verify quantitatively these findings in the regression framework outlined in equation 1.[10] Results are reported in Table 3 and 4. Coefficients confirm that the longer PBD leads to longer benefit and nonemployment duration.[11] The effects for the duration of the benefit is very stable across specifications and largely confirms the visual inspection: 4 additional months of PBD lead to 8 additional weeks of benefits or 0.46 additional weeks per week of potential duration.[12] 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 labour market fixed effects[13] and, finally, Column (5) includes local labour market interacted with monthly fixed effects. This will be the preferred specification for the rest of the paper. The effect represents a 36% increase over the baseline of 23 weeks.[14] Column (6) and (7) report the effect on the total amount of the benefit and point at an increase in 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.[15] Workers spend on average 6 additional weeks in

[10]For computational ease, estimation will use a parametric specification with individuals fired between 46 and 54 years of age, excluding individuals in the first two bins on the left and the first bin to the right of the cutoff. Estimates using local polynomials and optimal bandwidth are reported in the Section 7.1

[11]For the sake of comparison, I report in Appendix C.1 results with different levels of clustering for the effect of longer PBD on nonemployment duration up to the next job. The LLM clustering is slightly more conservative than other common choices but results are overall very robust.

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

[13]The Italian National Institute for Statistics (ISTAT) defines LLM every 10 years. For temporal proximity, I use the 2011 definition which identifies 611 LLM.

[14]Throughout the paper, the baseline for the dependent variable is computed as the average value for workers fired between 49 years of age and 49 years and 10 months of age.

[15]Here, I restrict my attention to spells in the private sector and I censor spell at 4 years after layoff. I check the implications of these restrictions in Appendix C.2 by trying different censoring. I consider then transitions to the public sector and self-employment using full contribution histories and restriction to the estimation sample in Appendix C.3.

nonemployment due to the longer potential benefit duration or 0.354 additional weeks 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) for the Italian setting by Rosolia and Sestito (2012), who estimate the effects of benefit potential duration and generosity with 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 6: first, recipients with longer PBD are less likely to exit from nonemployment since the very beginning of the spell; second, unemployed with shorter PBD (8 months) have a much higher exit rate with respect to unemployed with longer PBD 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 overall reemployment probability between the two groups.[16] 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%).[17] As I show in Appendix G, this pattern is largely driven by recalls and it 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

[16] It is worth pointing out that the generosity of the benefits declines after 8 months, with the replacement rate falling from 50% to 40%. This does not seem to have a strong effect on exit rates, as hazard rates have only a very small slowdown in the decline for workers still entitled to benefits. This is consistent with previous results by Rosolia and Sestito (2012).

[17] 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.

at least 6 months per year as they require at least one year of work over two 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 separation. 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[18] for employers who hire workers who have been unemployed for at least 24 months with permanent contracts (L. 407/90). This pattern is indeed more evident for workers coming from permanent contracts who are more likely to be hired again with such contracts. As this incentive applies to both treated and controls, it should not affect the 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 ($\mathbb{1}(t > t^*)$) since layoff and iterate it for all the months in the 4 years observation window. This corresponds to a difference of the survival in nonemployment for the two groups. Resulting coefficients, which summarize differences in reemployment rates over 4 years, are reported in Figure 7. 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 benefit, workers with longer benefits progressively close the gap between them and workers with shorter duration. However, this process is slow and, after 4 years, they have still a 1 percentage point 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 of heterogenous effects of the social security contribution cut.

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

5.2 Medium Term Outcomes

The career of workers could be affected by longer benefits well beyond their first nonemployment spell. 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, workers might not be able to get better contracts but more frequent contract which will improve their overall employment probability and earnings.

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 analyse 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. An advantage of this specification is that it is not affected by selection bias as it can be estimated with the full sample and does not require workers to find a job.

As a first step, I plot the overall number of weeks in nonemployment during the 4 years following layoff in Figure 8. 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 in overall time spent in nonemployment of only 2 weeks. Column (2) looks at the difference in total labour income and shows a decline by 800 euro or about 2.4% of the baseline. Column (3) and Column (4) add benefits related to the first layoff and show that benefits more than compensate for labour income losses. These gains are partly mitigated by the inclusion of all benefits received after the first layoff in Column (5) and (6): overall, workers with initial longer PBD have a 4.8% higher income than workers with shorter benefits. Finally, Columns (7) to Column (9) provide information on future benefits. Workers with longer benefit duration are less likely to take up new 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 transfers. It should 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. Workers might have a lower take up of unemployment benefits due to higher take up of other polices such as disability benefits and pensions. Previous literature, such as Inderbitzin et al. (2016) and Kyyrä and Pesola (2017), underlined the complementarity and substitutability of these benefits with UB. However, they play a very limited role in this setting as shown in Appendix D.

To better understand how workers with longer benefits offset their initial employment disadvantage, I 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.[19] Differently from Figure 7, this specification allows to account for repeated transitions in and out nonemployment. Figure 9 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 of the spell. However, the maximum difference in employment between the two groups is lower by about 25% (2 percentage points), and it peaks 2 months before the end of the benefit eligibility for workers with longer potential

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

benefit duration. The period of convergence between the two groups is also much shorter: while in Figure 7 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%).[20] Figure 10 provides additional evidence on the dynamic effects of longer PBD. Workers initially eligible to longer benefits 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 PBD 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, consistently with past evidence by Caliendo et al. (2013). The same conclusion can be drawn by looking at days worked per month (conditional on employment), there does seem to be at most small differences in favour 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). This is, however, not sufficient to make inference on the employment stability of workers with longer benefits as workers who do not find employment are not able to claim again unemployment benefits.

The discrepancy between results in the first spell and over 4 years can be determined by multiple factors which 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.

[20] This dynamic could suggest some cyclical differences across the two groups. To further investigate this issue, I analyse this outcome over a 7 years period using workers fired in 2009 in Appendix E. 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.

- Finally, workers 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 the following sections, 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 labour 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 PBD, 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 11 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[21] and full table is reported in Appendix F. Workers with longer PBD experience small gains in daily wage (a 0.6% increase). Previous studies

[21] Workers fired between 49 years and 49 years and 10 months of age.

provided mixed evidence in this regard, with small and not statistically significant 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.[22] 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 are, instead, remarkably similar.

Table 6 further explores characteristics of the new job by looking at mobility of workers in both economic and geographic terms. Longer PBD slightly promotes mobility with a higher probability of changing firm (Column (1)), a higher probability of changing geographic location but within LLM and Region[23] (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 related but different sectors. I also explore if the new economic or geographic location offers better employment prospects in Table 7. To this purpose, I check three different outcomes: 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

[22]The effect of an additional week is hence smaller in my study, given the difference in the change in PBD for the two groups of workers.

[23]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 labour markets. In this sense, the regions constitute a very relevant administrative dimension in the Italian economy.

(Columns (7) to (9)) in employment does not show any change in all the three dimensions.

Transitions across firms

Then, I assess whether workers who have found a new job after a longer benefit show higher persistence in employment by transitioning more efficiently across firms. I consider the first two years after reemployment[24] and 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). The difference in reemployment rates between workers with longer and shorter PBD at this horizon is about 2 percentage points.

I 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 12, 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 duration of the job in the new firm and, in addition, show that matches with short breaks do not play an important role. Hence, mobility towards new firms contributes to explain the difference in employment probability previously described.

These results show that the difference between the medium-term estimates and those for the first spell are at least partially explained by later faster transitions for workers with longer benefits. 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. Column (1) reports the effect on the total number of weeks employed in the two years after layoff. It shows that workers initially eligible to longer PBD spend almost a full additional week in employment after reemployment. Column (2) and (3) decompose the effect between

[24]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.

the first firm that hired them and the other firms. Although workers spend actually more time in the first firms, as expected according to the positive tenure effects in previous section, the effect on time spent in other firms is larger and statistically significant at 10%. This does not seem to be explained by faster job to job transitions, which suggests that these workers have still to undergo some search before moving a different firm. One of the main concerns is that this effect comes from an eligibility effect, indeed workers who were eligible to initially longer benefits spend more time in nonemployment before finding a new job and this might lead them not to be eligible for unemployment benefits when they are laid off again. I explicitly control for this in different ways in Columns (5) to (7) by adding a dummy for workers having more than 52 weeks of work in the two years before the new layoff in Column (5) and then implementing a RDD in weeks worked in the last two years with a discontinuity at 52 in Columns (6) and (7). In the last column, I restrict the sample to workers who experienced a second layoff for whom the number of weeks can be computed. Although repeated eligibility seems to play a marginal role, workers with initially longer benefits still show more weeks of employment in other firms than workers with shorter PBD.

In order to map this effect into the whole sample, I consider that the estimates use about 80% of the sample and correct the contribution of this employment margin by this factor. As a result, the overall contribution of this employment pattern represents 0.74 weeks for the overall sample and it explains about 18% of the observed difference between the two set of regressions. The contribution for transitions to new firms accounts for 0.41 weeks (10% of the overall difference) while the longer duration of first job accounts for 0.33 weeks (8% of the difference). This estimate represents a lower bound of the following employment gains for workers originally on more generous benefits as some small differences persist after 2 years.

Repeated layoff and Cyclicality

A third possibility is that workers with short unemployment benefits lose again their job, thus reducing the difference in employment with respect to workers with longer benefits. This is supported by the faster convergence in employment levels in Figure 9 which suggests that employment losses might play an important role.

First, I explore this possibility graphically by plotting the employment rate for workers with shorter and longer potential benefit duration over the 4 years after layoff in Figure 13. I focus on individuals close to threshold and obtain their employment rate by estimating my RDD specification for monthly employment with only the polynomial in age with the jump at the cutoff and a quadratic flexible polynomial in age. I then estimate the share of individuals employed on the right and on the left of the cutoff by predicting the polynomial at the age of 50 on the two sides of the threshold. As expected, the employment pattern mirrors the coefficients in Figure 9 and it highlights how workers who found a job within 12 months experience a large employment drop close to one year after their initial layoff. The drop is sizeable as employment rate for workers with short benefits decline from 53% to 38% and for workers with longer benefits, from 45% to 34%. This leads to a 4 percentage points decline in their relative distance. Results in Figure 7 suggest that, although higher job finding rate for workers with longer benefits might play a role, the difference in job finding rate it is too small to account for a large part of this difference.

Second, Table 9 provides more quantitative perspective on the pattern of hiring and firing within one year from the initial layoff. As expected, workers with shorter benefits have a higher probability of finding a job in the first 12 months after their initial layoff: 68% of them find a job within this time horizon while only 61% find a job among those with higher benefits. A relevant share of workers, however, lose again their job and a large part of these layoffs is concentrated in the months between the eleventh and the thirteenth after the initial layoff. These shares are very similar across the two groups but the overall effect in terms of changes in employment is slightly different. As more workers among those with shorter benefits found a job, the same proportional incidence in layoff leads to a stronger percentage points decline in the number of workers who are employed with a 3 percentage point narrowing in the employment rate gap between the two groups.

These results, which concern all the workers fired before and after 50 years of age, are consistent with previous graphical evidence and suggest that workers are subject to a similar shock. However, as workers with less benefits have higher employment rates, they are also more affected in absolute terms, which, in turn, leads to decline in the difference in total employment between the two groups. This is consistent with the negligible effect on tenure and the characteristics of the new job for the two groups of workers. The dynamic in employment and subsequent job loss is more prevalent among temporary workers but the larger contribution to the decline in the difference in employment comes from workers with permanent contracts.

These results show that transitions between employment and nonemployment are frequent. The pure job finding rate, hence, provides an imperfect proxy for the employment levels of groups subject to different unemployment benefits. Seasonality plays an important role in this sense but these patterns are common for workers with different characteristics as shown in the following section.

6 Heterogeneity and Extensions

6.1 Heterogeneity

Workers 'conditions on the labour market vary considerably and this might lead them to react differently to policies. 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 labour market policies. In this section, I explore the effects of longer PBD across different groups of workers 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 main variables of interest: duration of nonemployment after the first layoff before finding a new job, and total nonemployment.

Table 10 reports the results of my estimates. Panel A reports the effect of longer

PBD on the time spent to the next job in the private sector. As usual, Column (1) reports the baseline effect for the sake of comparison. First, I explore geographic differences and I 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 nonemployment and earnings are larger in the South, coherently with more difficulties for workers in this area to find jobs after layoff. I, then, explore gender differences in Column (4) and Column (5): women show lower responses to longer PBD. Columns (6) to (8) explore the role of size of the firm of origin: being in a large firm generally reduces both the average time that workers spend to find a new job and the additional time they take if they are eligible to longer benefit duration. The possibility to access to a larger set of vacancies within the same firm could play a role in this sense.[25] In relative term, the effect represents about a 10% increase in time to next job with respect to the baseline duration. The stability of the previous contract also plays an important role (Columns (9) and (10)) and workers who lost a permanent contract show more difficulties in transitioning towards a new employer. Workers previously in temporary contracts spend about 50% less time to find a job on average and the effect of longer duration is accordingly rescaled. Several reasons might explain this sizeable difference: workers on permanent contracts might lose more firm specific human capital; they might have less knowledge about vacancies and employment opportunities due to the longer time elapsed since they looked for a new job; they might be more demanding in terms of the characteristics of the new employment. To explore all these possible channels is, however, beyond the scope of the present analysis and I leave it for further research. It is worth pointing out that results for type of contract and firm size are related to some extent as workers from permanent contracts are more likely to be fired from smaller firms. 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 Labour Code). In addition, workers from firms undergoing economic restructuring with previous permanent contracts can access a more generous benefit under certain conditions.[26] However, the contract

[25] Indeed the size of the firm positively affects the probability of recall as discussed in Appendix G.

[26] These conditions concern the tenure of the worker and the size and sector of the firm. See Appendix

composition is not enough to explain lower effects for workers coming from larger firms as similar discrepancies can also be observed within contract group. Then, I assess the effect of longer benefits according to sector cyclicality. A sector is defined cyclical if it experiences quarterly variation in workforce larger than 10%.[27] As expected, workers in cyclical sectors spend less time to find a new job and their response is lower with respect to other workers. Finally, I explore the role of economic conditions at the moment of layoff. I look at individuals who are fired during contractions (-1.5% in the number of employed over the last year in the LLM) and expansions (+3% in the number of employed over the last year in the LLM). I define contractions and expansions based on the growth of employment in the year before the layoff in the labour market and focus on workers laid off in the bottom quartile of the growth distribution and in the top quartile. Interestingly, the effect of longer PBD is stronger during recessions.

Panel B reports the same set of estimates for the total nonemployment over 4 years. In all cases, the difference in overall time spent in nonemployment is smaller with respect to the difference in time before finding a new job: the decline goes from 50% of the effect in the first spell for workers with previous permanent contracts to 93% for workers in temporary contracts. In absolute terms the decline goes from 3.64 weeks for workers in cyclical sectors to 5.27 weeks for workers in the South. These results suggest that the pattern highlighted in previous sections is common to workers from many different backgrounds and conditions and it should be taken into account in general perspective. The stronger relative decline for workers in temporary contracts supports the claim that their employment pattern, such as cyclicality and recall, is particularly important for this phenomenon. It is also interesting to note that the effect on nonemployment over 4 years is remarkably similar across all the different groups of individuals (about 2 weeks) but for workers from permanent contracts for whom the difference in overall time spent in nonemployment is 4.2 weeks. In a few cases, the overall difference in time spent in nonemployment is very close to zero such as in the case of workers from temporary

A for a more detailed discussion.

[27] This is estimated in time series regression between 2005 and 2008 with quadratic trends, year fixed effects and seasonal dummies.

contracts and laid off during an expansion.

7 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. I now test the sensitivity of my estimates to changes in the regression specification and donut. To this purpose, I run a series of specification and identification checks to verify the reliability of the estimated coefficients: I first start with several robustness tests on the parametrization of the RDD; then, I examine the effects of the choice of the bandwidth and of the donut; finally, I move to placebo tests which exploit the precise local nature of the treatment.

7.1 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 from the main estimates. My preferred specification provides estimates close to the average between the two more extreme specifications. I then estimate my model with a 3 months ray donut 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.[28] Similarly to previous cases, results are consistent with main findings although slightly larger. Panel

[28] 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 developed in Calonico et al. (2017). As the procedure does not explicitly allow for a donut setting, I adjust the data by reducing (increasing) by one (two) 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 of layoff and province.

B replicates the same set of checks for the total number of weeks in nonemployment over four years since layoff: estimates are fairly stable and their (slight) changes mirror the variation in the effect for the first nonemployment spell: in all cases, estimates are consistently below the ones for the first nonemployment spell and the decline with respect to this effect ranges from 75% of the effect in Column (2) to about 55% in Column (6).

Overall, results of these checks show that estimates obtained with my preferred specification are reasonably robust and mediate across a range of results obtained with alternative choices. Different parametrizations and estimations provide qualitatively consistent and quantitatively similar results.

7.2 Bandwidth and Donut

The choice of the donut and bandwidth can be crucial for the analysis in RDD setting. In my main settings, I rely on an arbitrary symmetric bandwidth and I use an asymmetric donut region around the cutoff. In this section, I provide additional evidence of the robustness of my results to changes in these two dimensions of my estimation.

I start with the bandwidth choice and I run my preferred specification with a large set of (symmetric) 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. Results with optimal bandwidth were reported in the previous Section in Column (6) in Table 11.

As a final check, I also assess the importance of the donut hole region. I estimate my preferred specification with donut hole from one-month up to twelve 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 five months from the cutoff. It is interesting to note that coefficients for donuts for a four and five months radius around the cutoff are larger than the coefficient using a three 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 without a donut is also very similar to the others which suggests that the rich set of fixed effects and controls is able to capture most of the determinants of manipulation. Estimates start to differ substantially from the main result only after a eight months radius and coefficients are not statistically different from zero 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 a remarkable resilience for the estimates to changes in bandwidth and to the exclusion of different bins close to the cutoff.

7.3 Placebo

Another possible issue is that the regression model could deliver comparable estimates at different points of the age distribution due to high variance in the dependent variable or to the presence of other policies. This would make the results less reliable and reduce the confidence in the causal interpretation. 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 three 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 change. Results are reported in Figure I1. The outcome is reassuring about my identification strategy: the coefficient for the real discontinuity 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 I) although in this case the several coefficients are statistically significant, but the coefficient of interest is almost four times larger than the ones for the fake RDD.

These results provide further supportive evidence for the causal interpretation of the main results.

8 Conclusions

In this paper, I investigate the medium term impact of longer unemployment benefits on workers employment. 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. In addition, a market with frequent transitions between employment and nonemployment might lead to overestimate overall employment differences between workers exposed to longer and shorter benefits and, thus, to overestimate the costs of unemployment benefits in terms of fiscal externalities. To estimates these effects, I use rich and novel administrative data from Italy and I implement a Regression Discontinuity Design exploiting quasi-experimental variation in PBD related to an age at layoff rule. According to this rule, the PBD is fully determined by age at layoff: 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 results in the literature, I find that longer PBD 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 slightly more likely to find a job with permanent and full time 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 PBD show only 2 more weeks in nonemployment. Moreover, they are less likely to get unemployment benefits in the future. Two main elements contribute to determine this discrepancy: first, all workers experience rather frequent transitions between employment and nonemployment and this reduces the employment gap between workers exposed to benefits of different length; second, workers with initially longer benefits experience faster transitions towards a second firm after a new layoff. The former element explains about three weeks in the difference between the two set of estimates while a bit less than one week is explained by a faster transition towards a second firm. these effects contribute to reduce the fiscal externalities generated by longer benefits and could lead to an increase in the optimal generosity of benefits. Although, the most striking differences can be observed for workers with temporary contracts, the discrepancy between the effect on the duration of the first nonemployment spell and the medium term total nonemployment are common to a variety of different settings. 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. Results are robust to a wide range of specification and robustness checks.

These results are of crucial importance from a policy perspective. Indeed, workers with longer PBD generate negative fiscal externalities on other workers to the extent that they change their search behaviour as a consequence of longer benefits: they receive more transfers and pay less taxes. The results in the present work show that employment levels tend to be much similar than expected by looking on the duration of the first spell. As salaries between the two groups of workers are also similar, this suggests a similar level of overall taxation. In addition, a lower probability of getting benefits in the future directly offset part of the initial higher expenditure. Overall, these effects suggest that classical estimates are overestimating the costs of unemployment benefits duration and they could be underestimating the optimal level of generosity.

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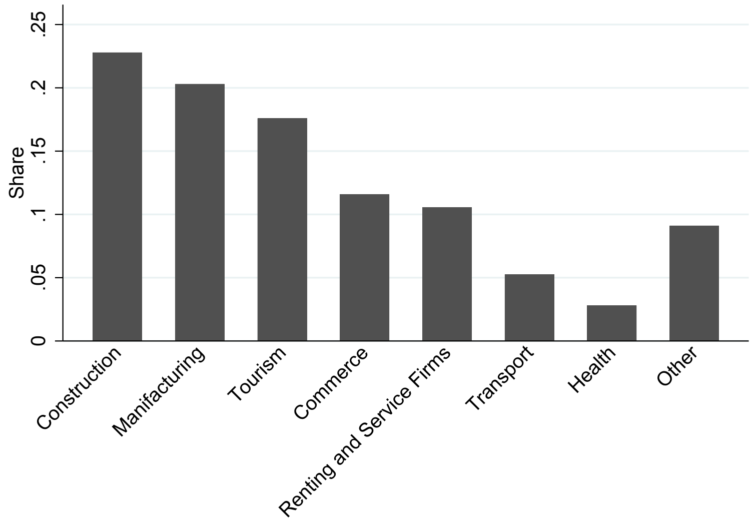
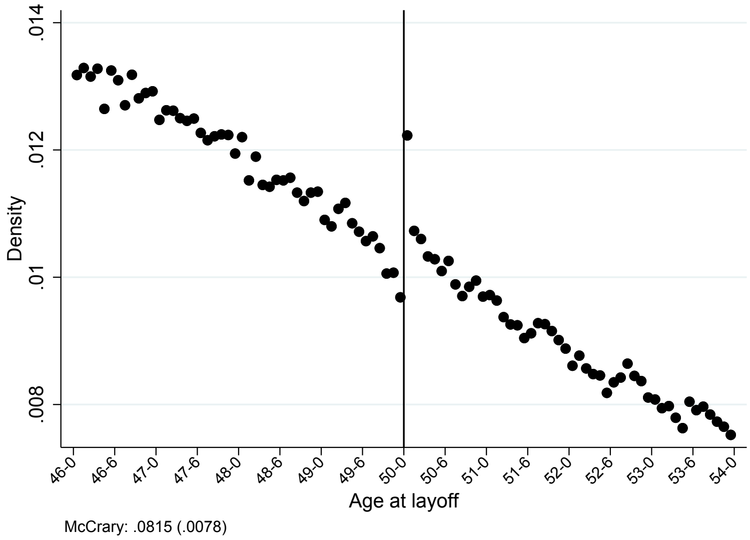
Graphs

Figure 1: Sector Composition for Recipients of Unemployment Benefits



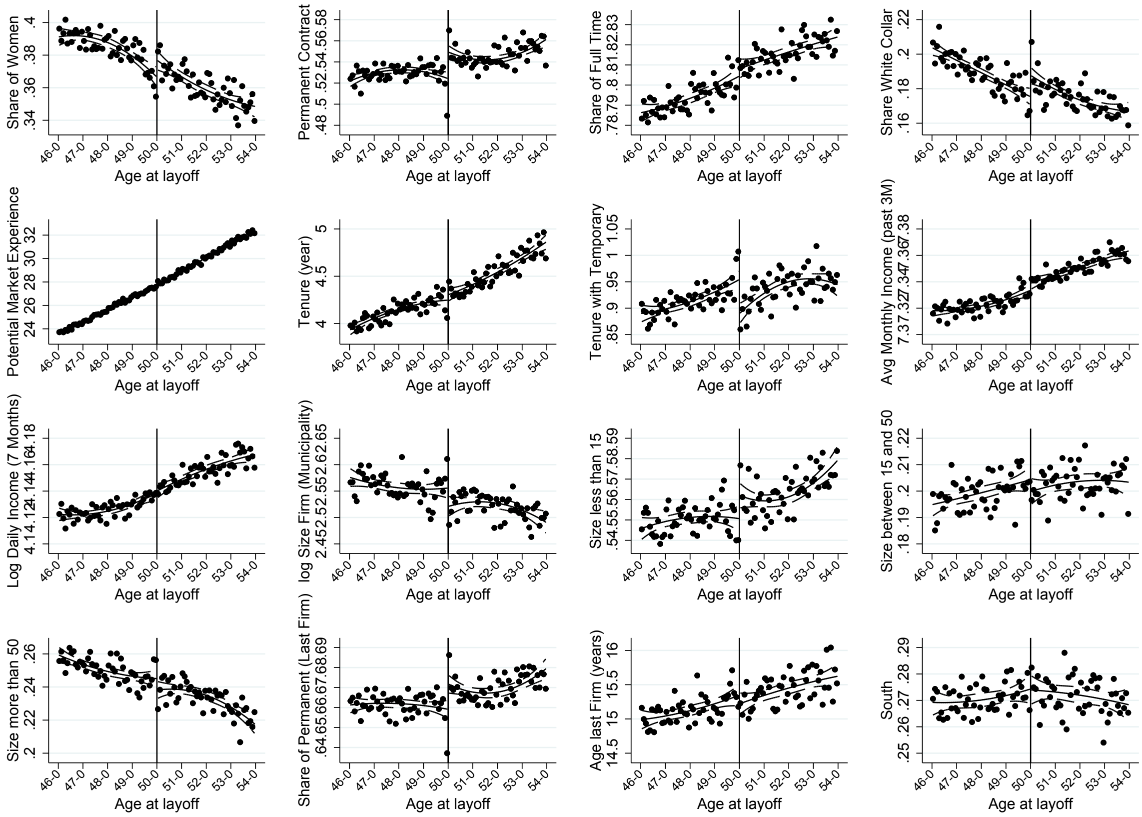
Note: Figure reports sector composition for workers fired between 2009 and 2012 and receiving unemployment benefits. Sample restricted to workers fired between 46 and 54 years of age at the moment of layoff.

Figure 2: Density of Recipient of Unemployment Benefits by Age (Month)



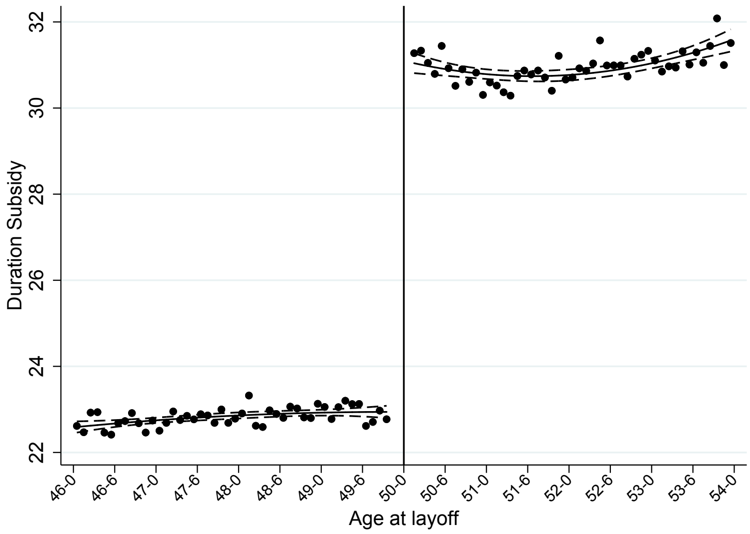
Note: Density of age at layoff for the universe of recipients of unemployment benefits (OUNR) between 2009 and 2012. Number of recipients of unemployment benefits: 452,888. Result of McCrary test for discontinuity at the threshold reported at the bottom of the graph. Corresponding t-stat is 10.45.

Figure 3: Continuity of Observable Characteristics at Cutoff



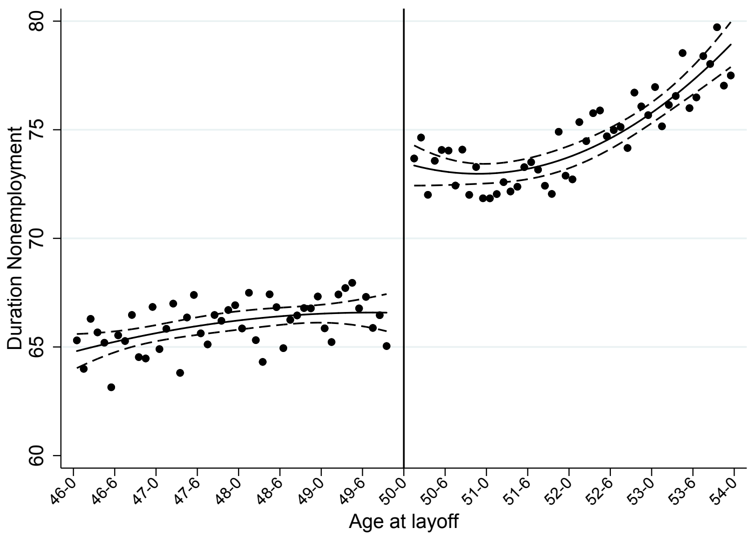
Note: Average of observable characteristics in a 4 year radius around the cutoff. Number of recipients of unemployment benefits: 452,888; variables reported (from left to right and top to bottom): Female; Permanent Contract; Full time; White Collar; Potential Market Experience; Tenure; Tenure with Temporary Contract; Average Monthly Wage in 3 months before layoff; Average daily Wage in 6 months before layoff; Size of the plant (firm-municipality); Small Firm (less than 15 employees); Medium Firm (14-49); Large Firms (more than 49); Share of workers with Permanent Contracts in past firm; Age last firm; Share of workers from Southern Regions. Polynomial fit is estimated by OLS, separately on the two sides of the cutoff, with a square polynomial in age.

Figure 4: Weeks of Benefit



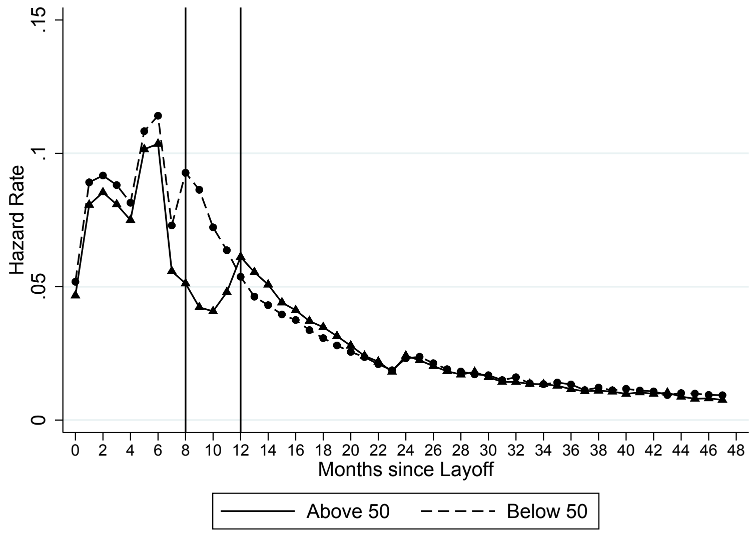
Note: Figure reports weeks on benefits in the first spell after layoff. Figure based on 438,403 layoffs between February 2009 and December 2012 for workers between 46 and 54 years of age at layoff excluding workers from 49 years and 10 months of age to 50 years of age. Polynomial fit is estimated by OLS, separately on the two sides of the cutoff, with a square polynomial in age. Confidence interval at 95% reported.

Figure 5: Weeks of Nonemployment



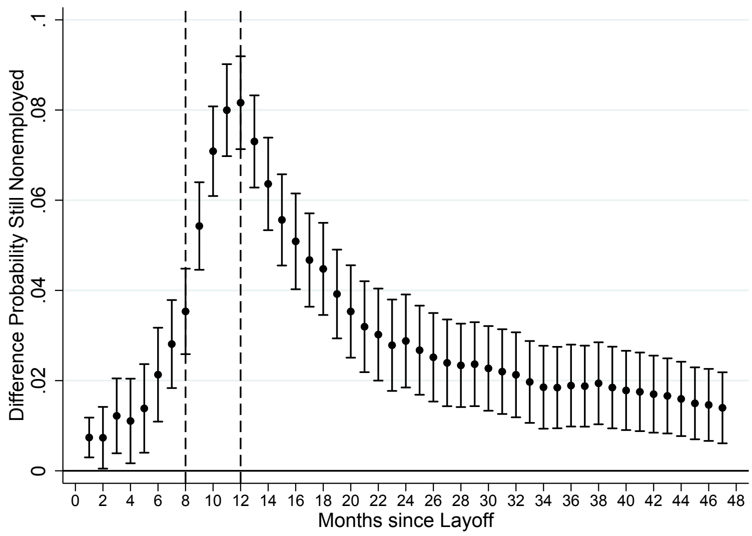
Note: Figure reports the weeks of nonemployment in the first spell after layoff. Figure based on 438,403 layoffs between February 2009 and December 2012 for workers between 46 and 54 years of age at layoff excluding workers from 49 years and 10 months of age to 50 years of age. Period of nonemployment 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. Polynomial fit is estimated by OLS, separately on the two sides of the cutoff, with a square polynomial in age. Confidence interval at 95% reported.

Figure 6: Hazard Rate for Exit from Nonemployment



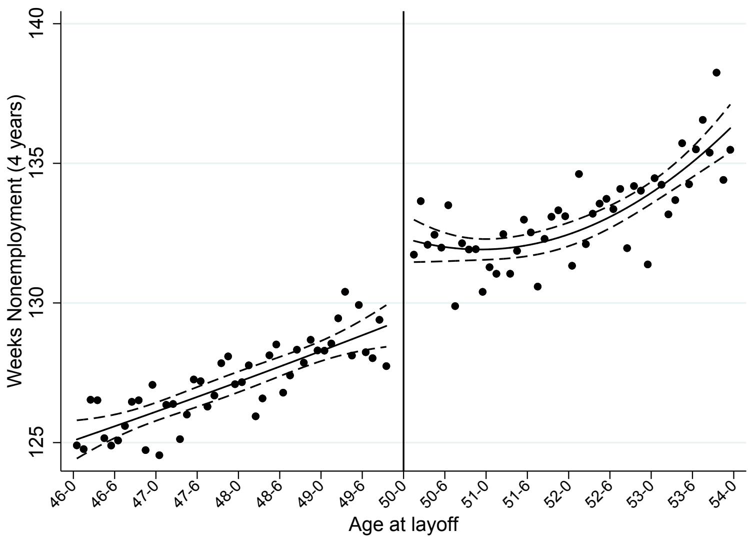
Note: Hazard rate for exit of workers from nonemployment towards employment in the private sector. Hazard rate computed as the share of workers exiting nonemployment in month t over the number of workers still nonemployed after t-1 months. Figure based on 438,403 layoffs between February 2009 and December 2012 for workers between 46 and 54 years of age at layoff excluding workers from 49 years and 10 months of age to 50 years of age.

Figure 7: Difference in Reemployment Probability since Layoff



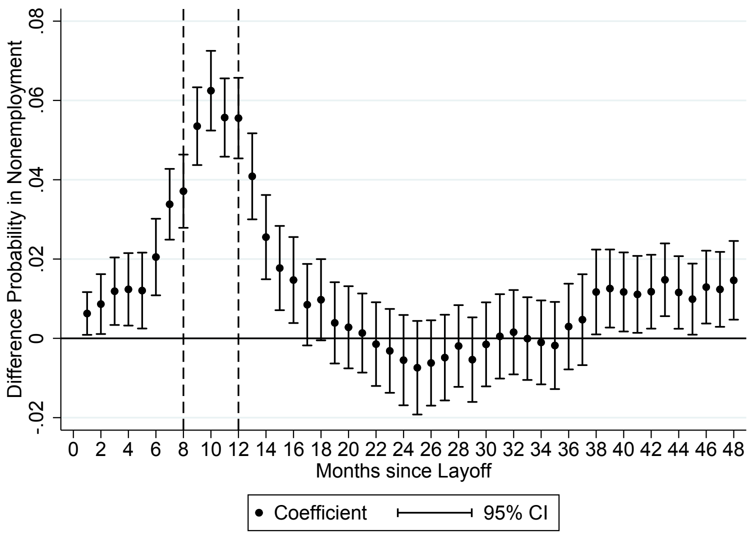
Note: Effect of 4 additional months of potential benefit duration on probability of being still nonemployed after t months. Linear probability models with dummy equal to 1 if the worker is still nonemployed after t months since layoff. Regressions include a square polynomial in age with different slopes around the cutoff, controls for the worker and last firm characteristics and local labour market interacted with month of layoff fixed effects. Figure based on 438,403 layoffs between February 2009 and December 2012 for workers between 46 and 54 years of age at layoff excluding workers from 49 years and 10 months of age to 50 years of age. Standard errors clustered at Local Labour Market level. Confidence interval at 95% reported.

Figure 8: Total Weeks of Nonemployment (4 years)



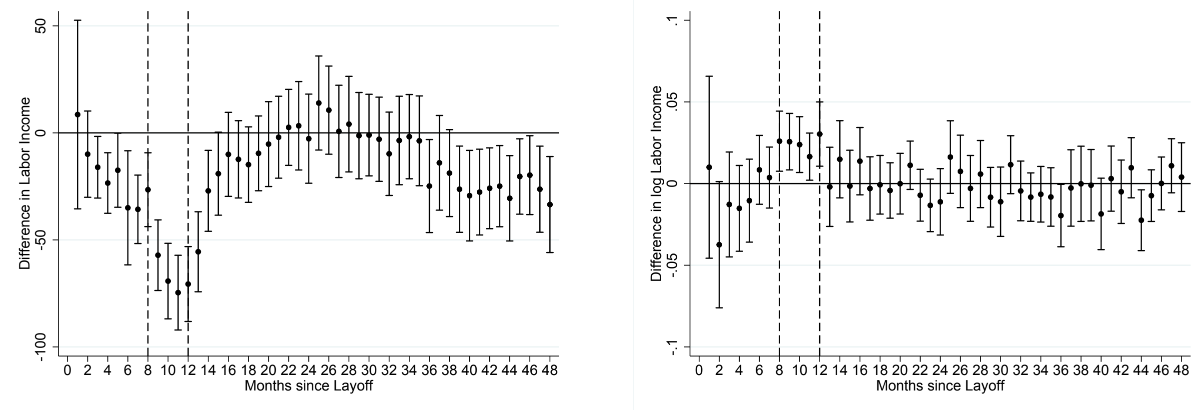
Note: Total weeks of nonemployment within 4 years since layoff. Figure based on 438,403 layoffs between February 2009 and December 2012 for workers between 46 and 54 years of age at layoff excluding workers from 49 years and 10 months of age to 50 years of age. Linear fit estimated by OLS with a square polynomial in age estimated separately on the two sides of the cutoff. Polynomial fit is estimated by OLS, separately on the two sides of the cutoff, with a square polynomial in age. Confidence interval at 95% reported.

Figure 9: Difference in Nonemployment Probability over 4 years since Layoff



Note: Effect of 4 additional months of potential benefit duration on probability of being nonemployed at t months after layoff. The worker is considered employed if she works at least one day during the corresponding month in the private sector. Regressions include a square polynomial in age with different slopes around the cutoff, controls for the worker and last firm characteristics, local labour market interacted with month fixed effects. Figure based on 438,403 layoffs between February 2009 and December 2012 for workers between 46 and 54 years of age at layoff excluding workers from 49 years and 10 months of age to 50 years of age. Standard errors clustered at Local Labour Market level. Confidence interval at 95% reported.

Figure 10: Income, Days Worked and Unemployment Benefits over 4 years after layoff



(a) Total Income

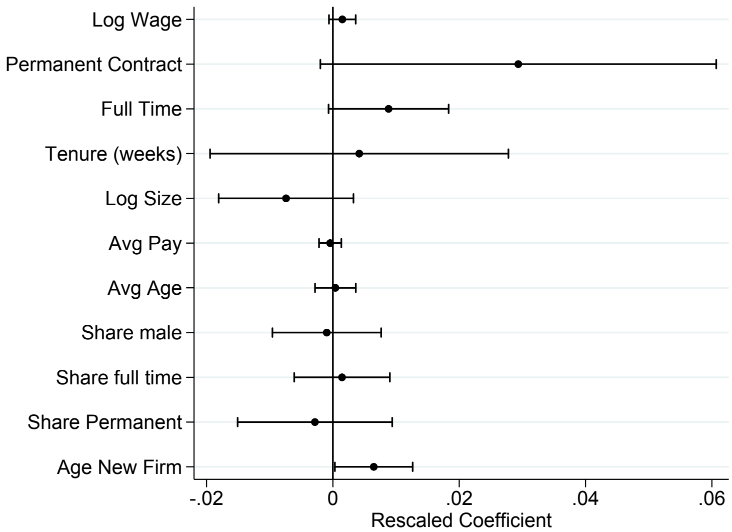
(b) Log Total Income (employed)

(c) Days Worked (employed)

(d) Receiving UB

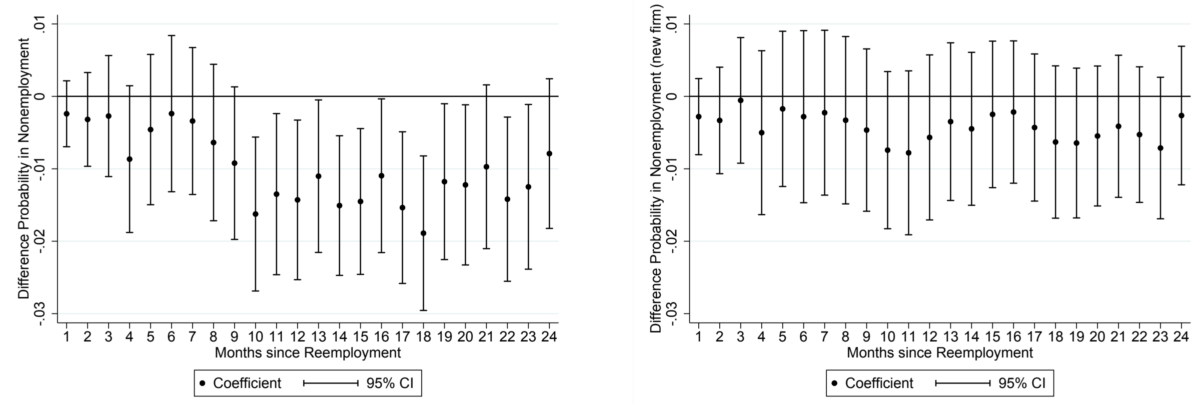
Note: Effect of 4 additional months of potential benefit duration on labour earnings (a), log labor earnings (b), days worked (c) and probability of receiving UB (d). Panel (b) and (c) conditional on employment. Regressions include a square polynomial in age with different slopes around the cutoff, controls for the worker and last firm characteristics, local labour maket interacted with month fixed effects. Figure based on 438,403 layoffs between February 2009 and December 2012 for workers between 46 and 54 years of age at layoff excluding workers from 49 years and 10 months of age to 50 years of age. Standard errors clustered at Local Labour Market level. Confidence interval at 95% reported.

Figure 11: Effect on First Employment Characteristics (first spell)



Note: Effect of 4 additional months of potential benefit duration on post unemployment job characteristics. Regressions include a square polynomial in age with different slopes around the cutoff, controls for the worker and last firm characteristics, local labour market interacted with month fixed effects. Figure based on 352,486 new jobs for subset of layoffs between February 2009 and December 2012. Sample includes workers between 46 and 54 years of age at layoff excluding workers from 49 years and 10 months of age to 50 years of age, and who find a job within 4 years since layoff. Coefficients standardized by the mean for the baseline group, i.e. workers fired between 49 years of age and 49 years and 10 months of age. Standard errors clustered at Local Labour Market level. Confidence interval at 95% reported.

Figure 12: Probability of Nonemployment following reemployment

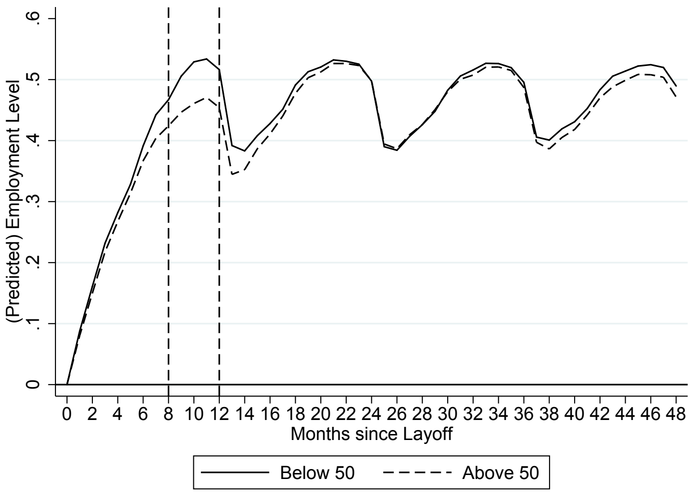


(a) Nonemployment - any firm

(b) Nonemployment - first reemployment firm

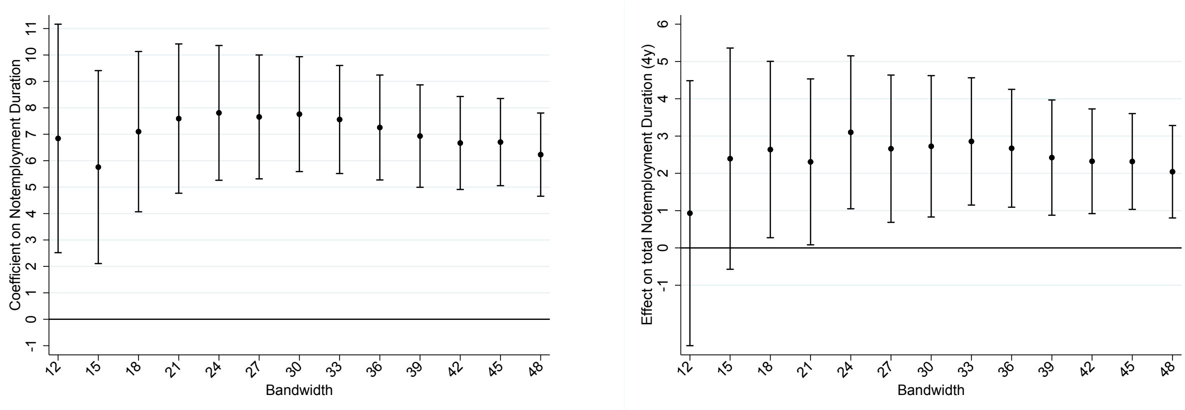
Note: Effect of 4 additional months of potential benefit duration on probability of being employed at t months after reemployment. The worker is considered employed if she works at least one day during the corresponding month in the private sector. Regressions include a square polynomial in age with different slopes around the cutoff, controls for the worker and last firm characteristics, local labour market interacted with month fixed effects. Figure based on workers who find an employment within 3 years since layoff, Subset of all layoffs between February 2009 and December 2012 for workers between 46 and 54 years of age at layoff excluding workers from 49 years and 10 months of age to 50 years of age. Standard errors clustered at Local Labour Market level. Confidence interval at 95% reported.

Figure 13: Employment rate for workers at the cutoff



Note: Share of workers employed at 50 years of age on the two sides of the cutoff. Estimates are based on RDD regressions with dependent variable a dummy taking value one if the worker is employed at month t and value 0 otherwise. Regressions include second order polynomial in age with different slopes on the two sides of the cutoff and a dummy for workers fired after turning 50 years of age.

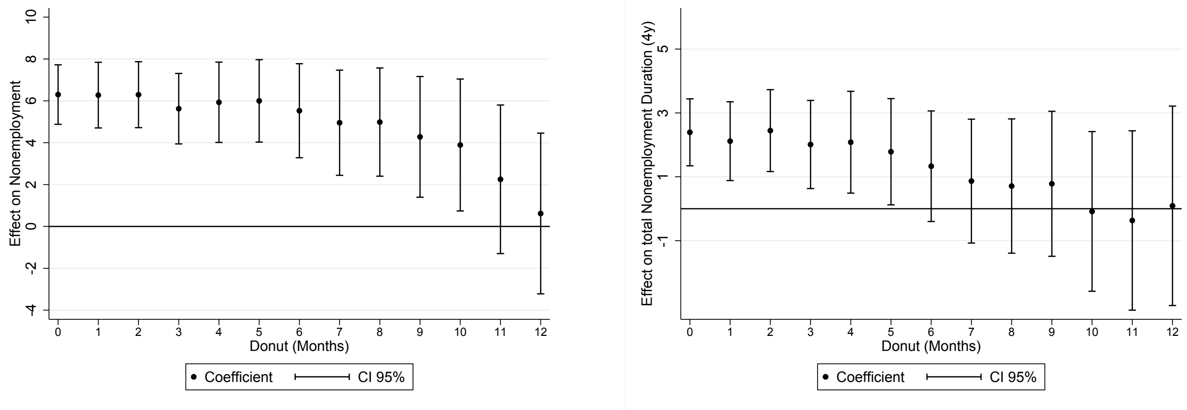
Figure 14: RDD estimates with different bandwidths.



(a) Nonemployment - first spell (b) Nonemployment - 4 years

Note: RDD estimates with different bandwidths around the cutoff. Sample at 48 months corresponds to main sample and it includes 438,403 layoffs between February 2009 and December 2012 for workers between 46 and 54 years of age at layoff excluding workers from 49 years and 10 months of age to 50 years of age. Regressions include a square polynomial in age with different slopes around the cutoff, controls for the worker and last firm characteristics, local labour market interacted with month fixed effects. Standard errors clustered at Local Labour Market level. Confidence interval at 95% reported.

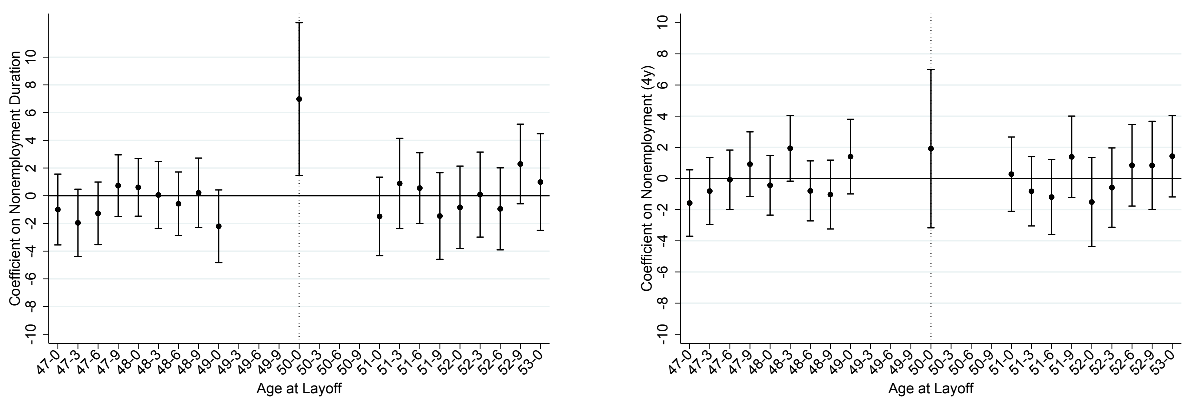
Figure 15: RDD estimates with different donut holes



(a) Nonemployment - first spell (b) Nonemployment - 4 years

Note: RDD estimates with different donuts around the cutoff with a 4 years bandwidth for duration of nonemployment in the first spell and over 4 years since layoff. Regressions include a squared flexible polynomial on the two sides of the fake cutoff, controls for the worker and last firm characteristics and local labor market interacted with month of layoff fixed effects. Coefficient at 1 is the closes to the preferred specification. Standard errors clustered at Local Labour Market level. Confidence interval at 95% reported.

Figure 16: Placebo RDD



(a) Nonemployment - first spell (b) Nonemployment - 4 years

Note: Placebo linear regression for duration of nonemployment in the first spell and over 4 years since layoff. Figure based on 438,403 layoffs between February 2009 and December 2012 for workers between 46 and 54 years at layoff excluding workers with 49 years of age and 10 months and 50 years of age. Regressions include a squared flexible polynomial on the two sides of the fake cutoff, controls for the worker and last firm characteristics and local labour market interacted with month of layoff fixed effects. Coefficient at 50 years of age corresponds to policy induced change in potential benefit duration. Placebo and main RDD regressions use a one year bandwidth. Standard errors clustered at Local Labour Market level. Confidence interval at 95% reported.

Tables

Table 1: Sample Characteristics

Variable	Average	Standard Deviation	Minimum	Maximum
Weeks of Benefit	26.322	15.832	0.143	52
Duration Nonemployment	84.903	106.700	0	413
Duration Nonemployment (Censored)	69.627	73.784	0	208
% with duration between 0 and 4 months	0.275	0.447	0	1
% with duration between 4 and 8 months	0.227	0.419	0	1
% with duration between 8 and 12 months	0.115	0.319	0	1
% with duration between 12 and 16 months	0.070	0.255	0	1
% with duration above 16 months	0.313	0.464	0	1
Recall	0.337	0.473	0	1
Female	0.375	0.484	0	1
Permanent Contract	0.537	0.499	0	1
Full Time	0.803	0.398	0	1
White Collar	0.184	0.387	0	1
Market Potential Experience	27.434	8.853	2.000	50
Tenure	4.303	5.233	0.083	30
Tenure Temporary	0.924	1.507	0	14
log Avg Monthly Wage in last 3 months	7.335	0.376	-1.109	11
log Daily Wage in last 6 months	4.139	0.439	-3.258	10
(log) Avg Size Plant	2.543	1.546	0	10
Small Firm (below 15 employees)	0.556	0.497	0	1
Medium Firm (between 15 and 49 employees)	0.201	0.401	0	1
Large Firm (above 50 employees)	0.243	0.429	0	1
Share Permanent in Last Firm	0.665	0.369	0	1
Age Last Firm	15.258	12.684	0	110
South	0.271	0.445	0	1
Workers	328,835			
Spells	452,888			
(Avg) # spells per individual	1.376			

Note: Summary statistics at spell level for individuals receiving unemployment benefits and fired between 46 and 54 years of age. 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 Contract).

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

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Polynomial and FE			Donut				
Variable	Beta	Standard Deviation	T-stat	Beta	Standard Deviation	T-stat	Average	Relative Effect
Female	0.011	0.004	**2.685**	0.004	0.005	0.859	0.375	1.04%
Permanent Contract	0.025	0.005	**5.196**	0.008	0.005	1.488	0.529	1.43%
Full Time	0.004	0.003	1.042	0.004	0.004	1.012	0.801	0.50%
White Collar	0.012	0.003	**3.634**	0.002	0.004	0.608	0.181	1.36%
Market Potential Experience	0.088	0.076	1.156	-0.015	0.084	-0.180	27.168	-0.06%
Tenure	0.059	0.042	1.400	-0.061	0.054	-1.140	4.233	-1.44%
Tenure Temporary	-0.064	0.013	**-4.862**	-0.029	0.013	**-2.192**	0.938	-3.06%
(Log) Monthly Wage Last 3 Months	0.005	0.003	1.579	0.004	0.003	1.070	7.330	0.05%
(Log) Daily Wage Last 6 Months	0.002	0.004	0.385	0.002	0.005	0.317	4.134	0.04%
(Log) Plant Size (Firm-Municipality)	-0.035	0.014	**-2.443**	-0.009	0.013	-0.690	2.551	-0.36%
Small Firm (<15)	0.011	0.005	**2.406**	0.001	0.005	0.130	0.552	0.11%
Medium Firm (15-49)	-0.004	0.003	-1.316	-0.002	0.004	-0.628	0.203	-1.22%
Large Firm (>50)	-0.007	0.004	-1.610	0.002	0.004	0.416	0.245	0.75%
Share Permanent Contracts Last Firm	0.013	0.003	**3.936**	0.001	0.004	0.366	0.661	0.21%
Age Last Firm	-0.125	0.106	-1.178	-0.170	0.117	-1.450	15.271	-1.11%
South Region	-0.001	0.004	-0.132	0.000	0.000	0.389	0.273	0.03%

Note: Linear regression model with second order polynomial in age with different slopes at two sides of the cutoff and dummy for workers laid off after 50 years of age (coefficient reported in table). Columns from (1) to (3) include age polynomial and fixed effects at Local Labour Market. Columns from (4) to (6) excludes the first two bins to the left and the first bin to the right of the cutoff (from 49 years and 10 months of age to 50 years of age). Column (7) reports the average value for the variable for the individuals between 49 years of age and 49 years and 10 months of age. Column (8) reports the ratio between the coefficient in Column (6) and the average in Column (7). Number of spells: 452,888. Standard errors are clustered at Local Labour Market level.

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

	(1) Weeks	(2) Weeks	(3) Weeks	(4) Weeks	(5) Weeks	(6) Benefits (amount)	(7) Benefits (log)
Above 50 years of age	8.136***	8.056***	8.054***	8.039***	7.947***	1,262.314***	0.177***
	(0.258)	(0.258)	(0.256)	(0.254)	(0.269)	(48.280)	(0.010)
Observations	438,403	438,403	438,403	438,403	438,403	438,403	438,403
Mean dependent	22.94	22.94	22.94	22.94	22.94	4767.21	
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 in weeks and amount of the benefit with a flexible squared polynomial on the two sides of the cutoff. Controls include past job and firm characteristics and fixed effects at month of layoff-local labour market level. List of controls: female, full time contract, past occupation dummies, permanent contract, daily wage in the last 6 months, average monthly wage in the last 3 months, market potential experience, tenure, tenure with temporary contract, log of average firm size, share of permanent contracts in the last firm, age of the last firm and sector dummies. Sample includes all recipients of unemployment benefits (OUNR) between February 2009 and December 2012 excluding workers fired from 49 years and 10 months of age to 50 years of age. Baseline computed as the average of the dependent variable for workers fired between 49 years of age and 49 and 10 months of age. Standard errors clustered at Local Labour Market level. Level of significance: * 10%; ** 5%; *** 1%.

Table 4: Effect of Potential Benefit Duration on Nonemployment

	(1) Weeks	(2) Weeks	(3) Weeks	(4) Weeks	(5) Weeks	(6) Nonemployed (after 4y)
Above 50 years of age	6.879***	6.457***	6.394***	6.249***	6.123***	0.012***
	(0.828)	(0.776)	(0.774)	(0.777)	(0.793)	(0.004)
Observations	438,403	438,403	438,403	438,403	438,403	438,403
Mean dependent	66.58	66.58	66.58	66.58	66.58	.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 UB with a flexible squared polynomial on the two sides of the cutoff. Controls include past job and firm characteristics and fixed effects at month of layoff-local labour market level. List of controls: female, full time contract, past occupation dummies, permanent contract, daily wage in the last 6 months, average monthly wage in the last 3 months, market potential experience, tenure, tenure with temporary contract, log of average firm size, share of permanent contracts in the last firm, age of the last firm and sector dummies. Sample includes all recipients of unemployment benefits (OUNR) between February 2009 and December 2012 excluding workers fired from 49 years and 10 months of age to 50 years of age. Baseline computed as the average for the dependent variable for workers fired from 49 years and 10 months of age to 50 years of age. Standard errors clustered at Local Labour Market level. Level of significance: * 10%; ** 5%; *** 1%.

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

VARIABLES	(1) Weeks Nonemployment	(2) Income (W)	(3) Income (W+B)	(4) log Income (W+B)	(5) Income (W+AB)	(6) log Income (W+AB)	(7) # Other UB	(8) Amount Other UB	(9) Weeks Other UB
Above 50 years of age	2.162***	-883.965***	378.349	0.060***	1.464	0.046***	-0.035***	-418.269***	-1.911***
	(0.646)	(305.085)	(282.046)	(0.009)	(286.657)	(0.009)	(0.012)	(53.089)	(0.256)
Observations	438,403	438,403	438,403	438,403	438,403	438,403	438,403	438,403	438,403
Mean dependent	128.83	33365.46	38132.67		43792.49		1.26	5423.12	26.09
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 nonemployment duration over 4 years since layoff and on different measures of total income. Column (2) reports the effect of 4 additional months of PBD on total taxable labour 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; Columns (7)-(9) report the effect for time on unemployment benefits beyond the first spell of benefits. Controls include past job and firm characteristics and fixed effects at month of layoff-local labour market level. List of controls: female, full time contract, past occupation dummies, permanent contract, daily wage in the last 6 months, average monthly wage in the last 3 months, market potential experience, tenure, tenure with temporary contract, log of average firm size, share of permanent contracts in the last firm, age of the last firm and sector dummies. 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 from 49 years and 10 months of age to 50 years of age. Baseline computed as the average for the dependent variable for workers fired between 49 years of age and 49 and 10 months of age. Standard errors clustered at Local Labour 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	0.008*	0.013**	0.006	-0.000	0.005	0.008*
	(0.004)	(0.005)	(0.005)	(0.003)	(0.005)	(0.005)
Observations	352,486	352,486	352,486	352,486	352,467	352,467
Mean 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)) of changing sector (Columns (5)-(6)) with new employment. Controls include past job and firm characteristics and fixed effects at month of layoff-local labour market level. List of controls: female, full time contract, past occupation dummies, permanent contract, daily wage in the last 6 months, average monthly wage in the last 3 months, market potential experience, tenure, tenure with temporary contract, log of average firm size, share of permanent contracts in the last firm, age of the firm and sector dummies. All regressions include squared age polynomial with different slopes on the two sides of the cutoff. Estimates based on 356,486 new jobs, subset of layoffs between February 2009 and December 2012 for workers between 46 and 54 years at layoff excluding workers from 49 years and 10 months of age to 50 years of age. Baseline computed as the average for the dependent variable for workers fired between 49 years of age and 49 years and 10 months of age. Standard errors clustered at Local Labour Market level. Level of significance: * 10%; ** 5%; *** 1%.

Table 7: Employment Prospects in New Firm and Location

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
		Growth Employment			Retention			Persistence	
VARIABLES	Growth Firm	Growth Municipality	Growth Sector	Firm	Municipality	Sector	Firm	Municipality	Sector
Above 50 years of age	0.016**	0.000	0.058***	0.003	-0.000	-0.000	0.002*	-0.000	-0.000
	(0.007)	(0.001)	(0.017)	(0.002)	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)
Observations	317,842	356,205	356,210	318,436	356,205	356,039	318,436	356,205	356,039
Mean dependent	.14	.00	.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 past job and firm characteristics and fixed effects at month of layoff-local labour market level. List of controls: female, full time contract, past occupation dummies, permanent contract, daily wage in the last 6 months, average monthly wage in the last 3 months, market potential experience, tenure, tenure with temporary contract, log of average firm size, share of permanent contracts in the last firm, age of the last firm and sector dummies. All regressions include squared age polynomial with different slopes on the two sides of the cutoff. Estimates based on 356,486 new jobs, 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 for the dependent variable for workers fired between 49 years of age and 49 years and 11 months of age. Standard errors clustered at Local Labour Market level. Level of significance: * 10%; ** 5%; *** 1%.

Table 8: Effect of Potential Benefit Duration on Employment after First Reemployment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	Weeks	Weeks - first firm	Weeks - other firms	J-t-J	Weeks - other firms	Weeks - other firms	Weeks - other firms
Above 50 years of age	0.925***	0.413	0.512*	-0.000	0.441*	0.612**	0.704**
	(0.348)	(0.366)	(0.266)	(0.004)	(0.268)	(0.261)	(0.285)
Not Eligible again for benefit					1.864***	3.454***	3.091***
					(0.202)	(0.228)	(0.222)
Observations	343,820	343,820	343,820	343,820	343,820	343,820	301,548
Mean dependent	57.4	43.25	14.15	.24	14.15	14.15	15.79
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 on duration of employment over 2 years after reemployment. Columns (1) reports effect of longer initial benefits on total weeks after reemployment. Column (2) and (3) decompose the effect between the first firm and other firms. Column (4) looks at the effect on job to job transitions, where the dependent variable is equal to 1 if the worker finds a job in the same month or in the month after the new layoff. Columns (5) to (7) control for repeated eligibility by looking at how many individuals are still eligible to benefits after the second layoff (i.e. they cumulated at least one year of work in the two years before the new layoff). Column (5) includes a dummy, Column (6) includes a second RDD in the same specification in weeks worked before the second layoff. Column (7) replicates Column (6) but restricts the sample to workers who actually lost their reemployment job. Controls include past job and firm characteristics and fixed effects at month of layoff-local labour market level. List of controls: female, full time contract, past occupation dummies, permanent contract, daily wage in the last 6 months, average monthly wage in the last 3 months, market potential experience, tenure, tenure with temporary contract, log of average firm size, share of permanent contracts in the last firm, age of the last firm and sector dummies. 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 from 49 years and 10 months of age to 50 years of age. The sample excludes also all workers who found a job after 3 years since layoff. Baseline computed as the average for the dependent variable for workers fired between 49 years of age and 49 and 10 months of age. Standard errors clustered at Local Labour Market level. Level of significance: * 10%; ** 5%; *** 1%.

Table 9: Jobs found and lost within 13 months since the first Layoff

	(1) % Found Job	(2) % Lost (again) Job	(3) % Lost Job 11-13	(4) Job Lost and Δpp [(1)*(2)]
Below 50 years of Age				
Perment	0.57	0.41	0.55	0.23
Temporary	0.81	0.62	0.57	0.50
Total	0.68	0.53	0.56	0.36
Above 50 years of Age				
Perment	0.49	0.41	0.58	0.20
Temporary	0.76	0.64	0.59	0.49
Total	0.61	0.54	0.59	0.33

Note: Share of workers finding and losing job within 13 months since initial layoff. Column (1) reports fraction of workers who found a job within 12 months. Column (2) reports the fraction of those who found a job who lost again their job within the same time period of Column (1). Column (3) reports the share of lost job between 11 and 13 months since initial layoff. Column (4) reports the total change in employment by group due to subsequent layoff (Share who found a job again multiplied by the share of those who found a job who lost again their job). Sample includes all workers fired before and after 50 years of age, but those fired in the donut region: 249,162 workers laid off before turning 50 years of age and 189,241 workers fired after turning 50 years of age.

Table 10: Heterogeneous Effects on Nonemployment Duration before the finding a new job and over 4 years.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
VARIABLES	Baseline	Centre-North	South-Island	Female	Male	<15 emp	15-49 emp	>49 emp	Permanent	Temporary	Cyclical	Not Cyclical	Contraction	Expansion
Panel A: Nonemployment (till next job)														
Above 50 years of age	6.123***	5.481***	7.201***	5.701***	6.485***	7.767***	4.923***	4.791***	8.459***	4.081***	5.055***	6.099***	6.596***	4.859***
	(0.793)	(1.132)	(1.084)	(1.271)	(0.906)	(1.072)	(1.629)	(1.410)	(1.260)	(0.918)	(1.228)	(0.855)	(1.382)	(1.390)
Mean dependent	66.58	64.71	69.40	67.41	66.08	74.82	58.12	48.95	84.53	46.04	45.09	71.65	69.66	60.7
Panel B: Nonemployment (4 years)														
Above 50 years of age	2.162***	2.434**	1.926**	1.887*	1.855**	3.218***	1.263	2.209	4.214***	0.275	1.416	1.837**	2.247*	0.410
	(0.646)	(0.944)	(0.747)	(0.997)	(0.809)	(0.844)	(1.387)	(1.580)	(0.886)	(0.885)	(1.050)	(0.736)	(1.176)	(1.234)
Mean dependent	128.83	122.41	138.49	125.99	130.55	134.15	123.63	117.1	137.51	118.89	119.27	131.09	131.35	125.96
Delta	3.961	3.047	5.275	3.814	4.63	4.549	3.66	2.582	4.245	3.806	3.639	4.262	4.349	4.449
Delta over effect first spell	0.65	0.56	0.73	0.67	0.71	0.59	0.74	0.54	0.50	0.93	0.72	0.70	0.66	0.92
Obs	438,403	264,324	174,079	164,441	273,962	266,055	93,251	79,097	235,421	202,982	84,065	354,011	109,507	109,586
Controls	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Calendar Month FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
LLM FE	YES	YES	YES	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	YES	YES	YES

Note: Linear regression on duration of nonemployment before finding a job after the of unemployment benefits (Panel A) and on duration of nonemployment over 4 years (Panel B). Controls include past job and firm characteristics and fixed effects at month of layoff-local labour market level. List of controls: female, full time contract, past occupation dummies, permanent contract, daily wage in the last 6 months, average monthly wage in the last 3 months, market potential experience, tenure, tenure with temporary contract, log of average firm size, share of permanent contracts in the last firm, age of the last firm and sector dummies. 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 from 49 years and 10 months of age to 50 years of age. Sectors defined cyclical if they show a seasonality in employment larger than 10% of the workforce. Contraction and expansion defined based on employment growth in the LLM in the previous year with respect to the layoff. LLM is classified as contracting if employment growth in the past year is lower than -1.5% (bottom quartile of distribution) while LLM is classified as expanding if employment growth in the past year is larger than 3% (top quartile of the distribution). Baseline computed as the average for the dependent variable for workers fired between 49 years of age and 49 and 10 months of age. Standard errors clustered at Local Labour 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: Nonemployment (First Spell After Layoff)					
Above 50 years of age	6.123***	4.369***	7.706***	5.626***	7.678***	7.341***
	(0.793)	(0.463)	(1.297)	(0.859)	(0.553)	(1.204)
	Panel B: Nonemployment (4 years)					
Above 50 years of age	2.162***	1.314***	3.354***	2.011***	4.037***	3.887***
	(0.646)	(0.384)	(1.078)	(0.704)	(0.462)	(1.017)
Observations	438,403	438,403	438,403	424,188	99,007	438,403
Polynomial Degree	2	1	3	2	0	2
Donut	(2,1)	(2,1)	(2,1)	(3,3)	(2,1)	(2,1)
Robust Estimation	NO	NO	NO	NO	NO	YES
Non-Prametric band	NO	NO	NO	NO	1	NO

Note: Linear regression for duration of first nonemployment spell and medium term outcomes. Controls include past job and firm characteristics and fixed effects at month of layoff-local labour market level. List of controls: female, full time contract, past occupation dummies, permanent contract, daily wage in the last 6 months, average monthly wage in the last 3 months, market potential experience, tenure, tenure with temporary contract, log of average firm size, share of permanent contracts in the last firm, age of the last firm and sector dummies. 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. Optimal bandwidth for nonemployment before new spell is 216,307 observatsions. 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 Labour Market level. Level of significance: * 10%; ** 5%; *** 1%.

Appendix

A Alternative Benefits between 2009 and 2012

Two main alternative benefits were available to unemployed workers depending on their working histories and characteristics of the previous employment.

First, individuals who were not eligible for the Benefit for Ordinary Unemployment with Normal Requirement could apply to receive an alternative benefit with reduced requirements (Benefit for Ordinary Unemployment with Reduced Requirements). Workers were eligible for the benefit if they had worked at least 78 days (or 13 weeks) in the last year and if they had contributed for the first to the social security system at least two years before the unemployment period. It granted a monetary transfer for each day worked in the past year up to 180 days. Interestingly, the benefit was more generous for workers who had shorter unemployment spells and aimed at discouraging undocumented work rather than insurance. As with the main policy, 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 benefit 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 31^{st} of March. This measure, while still providing some income support, is considerably less generous than the previous one and, in addition, the delayed payments made it an imperfect substitute with respect to the one under study. Finally, the benefit is not suitable for workers who have long periods of nonemployment during the year, due to its connection with days worked rather than unemployment. It is hence not very likely that workers eligible for the OUNR would prefer the benefit just described.

Second, workers fired during firm restructuring and mass layoff could access so called Mobility Benefit (*Indennita' di Mobilita'*, law 223/1991).[29] This policy provides a long and

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

generous benefit, coupled with active labour market policies such as meetings with consultants and activities to improve the occupational perspectives of the worker. Eligibility to the benefit was based on two main 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 benefit was based on the age at layoff and geographic location of the workers and changed over time. Here, I report the duration in months for workers dismissed for the period before 2012.

Table A1: 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

The amount of the benefit followed the amount for the maximum salary integration computed yearly by the Social Security and it declined over time. The worker received 80% of the salary for the first 12 months and 64% 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 from permanent contracts among recipients of UB use this benefit. However, the important conditionalities to access this benefit both for the firm and for the worker reduce the risk of endogenous selection of workers and selection bias. The exclusion of some of the individuals from the sample could reduce, to some extent, the external validity of the results.

Given the sample composition and the heterogenous effects by workers characteristics, the presence of this benefit 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 PBD 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 by definition a lower probability of recall. This would lead potential benefit duration to have a stronger effect on this group of workers.

B Additional information on Data

B.1 Sample Definition for Recipients UB

I start with data for 4,555,104 unemployment benefits 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 benefit less than zero). This reduces the sample to 3,811,687 observations. I also drop suspension benefits and restrict my attention to workers fired between 46 and 54 years of age. This restriction reduces the sample to 647,888 observations. 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 few remaining observations with missing data for my variables of interest, I am left with 452,888 layoffs for 328,835 different individuals.

B.2 Main Variables Definition

Table B1: Main Variables Definition

Variable	Description
Nonemployment	Number of weeks between layoff of the worker and first job in the private sector. I consider valid jobs only those after the end of unemployment benefits to avoid considering very short spells which might be compatible with UB. Computation is based on UNIEMENS archive.
Nonemployment over 4 years	Number of weeks of nonemployment over 4 years after initial layoff. Number computed as 208 weeks minus the number of days worked in the period considered. The number of days worked is equal to the number of paid days in the month, rescaled by the number of days in the month. Computation is based on UNIEMENS archive.
Female	Indicator for gender of the worker. Variable is based on the worker registry.
Full Time	Indicator equal to 1 if the workers has a full time contract as reported by the SIP and validated with UNIEMENS data.
White Collar	Indicator equal to 1 if the worker has a white collar job. Model also includes dummies for apprentice, manager and few other categories which concern a small minority of the workers. Variable is derived from SIP and validated with UNIEMENS data.
Permanent Contract	Indicator equal to 1 if the worker has a permanent contract. Variable is derived from SIP and validated with UNIEMENS data.
Log Daily Labur Income	Daily labour income for workers in the six months before layoff in the firm laying off the worker. This computation excludes the month of the layoff to have better information on the usual pay of worker without considering possible delayed payments. Variable is derived from the UNIEMENS archive.
Log Average Monthly Income	Monthly average income over the three months before layoff. Information is derived from the SIP archive and it reports the average wage used for the computation of unemployment benefits.
Market Potential Experience	Number of years from the first contribution of the worker to social security as a employee. Variable is derived from the worker registry.

Variable	Description
Tenure	Number of total years spent by the worker in the same firm. This includes discontinous spells. Computation is based on yearly UNIEMENS records from 1982 up to the layoff of the worker.
Tenure Temporary	Number of total years spent by the worker in the same firm with a temporary contract. This includes discontinous spells. Computation is based on yearly UNIEMENS records from 1997 up to the layoff of the worker. Information on the contract of the worker are not available in years before 1997.
Log Average Size Firm	Size of the firm in the municipality (plant) in the six months before the layoff of the worker. Information is derived from the UNIEMENS archive.
Share Permanent Contracts in last firm	Share of workers with permanent contract in the last firm-municipality (plant) of the worker. Information is derived from the UNIEMENS archive.
Age Last Firm	Number of years since the first registration of the firm with the social security. Information is derived from the firm registry.

C Extensions for Effects on the first Spell of Nonemployment.

C.1 Clustering

Table C1: Potential benefit Duration and time to next Job: Cluster

	(1)	(2)	(3)	(4)
VARIABLES	LLM	Month	Age	Robust
Above 50 years of age	6.123***	6.123***	6.123***	6.123***
	(0.793)	(0.629)	(0.657)	(0.711)
Observations	438,403	438,403	438,403	438,403
Mean dependent	66.58	66.58	66.58	66.58
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 in weeks and amount of the benefit with a flexible squared polynomial on the two sides of the cutoff. Controls include past job and firm characteristics and fixed effects at month of layoff-local labour market level. List of controls: female, full time contract, past occupation dummies, permanent contract, daily wage in the last 6 months, average monthly wage in the last 3 months, market potential experience, tenure, tenure with temporary contract, log of average firm size, share of permanent contracts in the last firm, age of the last firm and sector dummies. Sample includes all recipients of unemployment benefits (OUNR) between February 2009 and December 2012 excluding workers fired from 49 years and 10 months of age to 50 years of age. Baseline computed as the average for the dependent variable for workers fired between 49 years of age and 49 and 10 months of age. Standard errors clustered at Local Labour Market level in Column (1), at month of layoff level in Column (2), at running variable level in Column (3) and robust standard errors in Column (4). Level of significance: * 10%; ** 5%; *** 1%.

C.2 Censoring

Data constraint prevents me from running the analysis over a longer time horizon. Censoring or trimming durations is, however, common in unemployment studies. Card et al. (2007a), for example, exclude all nonemployment spells longer than 2 years while Schmieder et al. (2012a) censor their spells of nonemployment at 3 years after layoff. Depending on long term difficulties that workers encounter while looking for a job, these choices might have implications for the estimates of the behavioural responses to longer or more generous benefits. As results in Figure 7 show, differences in reemployment rates persists after a long period of time and some workers experience intense difficulties in rejoining the workforce. In this section, I explore different censoring choices to assess how they can impact estimates of the effect of longer unemployment benefits. To this purpose, I repeat my estimation for time to the next 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. The marginal contribution of an additional year of data is decreasing which is consistent with the narrowing in the difference in reemployment for the two groups of workers as time proceeds. For the sake of comparison my preferred specification is reported in Column (1). The effect for longer benefits is always highly statistically significant and the decline in the coefficient for shorter horizons is accompanied by lower standard errors. Column (4) reports the results for the full uncensored duration. This kind of estimation has the disadvantage of allowing for different maximum duration for workers fired at different point of my reference period but it allows to exploit data more fully as all nonemployment spells are measured up to December 2016. As a consequence, workers will be observed up to 7 years after they first receive unemployment benefits (workers fired in 2009). The effect of longer benefits 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 a more comprehensive assessment. Censoring is, hence, far from innocuous and this particular choice should take into account the long run reemployment probability of workers.

Table C2: Effect of potential benefit duration on nonemployment duration with different censoring

VARIABLES	(1) 4 years	(2) 3 years	(3) 2 years	(4) Uncensored
Above 50 years of age	6.123***	5.251***	4.076***	6.953***
	(0.793)	(0.593)	(0.384)	(1.123)
Observations	438,403	438,403	438,403	438,403
Baseline dependent	66.58	57.38	46.59	80.89
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 in weeks and amount of the benefit with a flexible squared polynomial on the two sides of the cutoff (50 years of age). Controls include past job and firm characteristics and fixed effects at month of layoff-local labour market level. List of controls: female, full time contract, past occupation dummies, permanent contract, daily wage in the last 6 months, average monthly wage in the last 3 months, market potential experience, tenure, tenure with temporary contract, log of average firm size, share of permanent contracts in the last firm, age of the last firm and sector dummies. Sample includes all recipients of unemployment benefits (OUNR) between February 2009 and December 2012 excluding workers fired from 49 years and 10 months of age to 50 years of age. Baseline censoring in Column (1), censoring at 3 years in Column (2), at 2 years in Column (3) and uncensored in Column (4). Baseline computed as the average for the dependent variable for workers fired between 49 years of age and 49 and 10 months of age. Standard errors clustered at Local Labour Market level. Level of significance: * 10%; ** 5%; *** 1%.

C.3 Transitions towards Public Sector and Self Employment

A limitation of the data used in the main analysis is the possibility to only account for spells in the private sectors while spells as self-employed or in the public sector cannot be observed. In this section, I use an alternative source, the *Estratti Conto*, which contains social security contribution histories of workers. 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 compute the time to next job. Although data is reported at annual level, it still reports the start date of each contribution. It should be noted that contribution histories 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 pensions. Second, the comparison between the private sector employees data and contribution histories shows some inconsistencies on 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 durations. I compute several measures of time to the next employment with the original and contribution data and report them in Table C3. Results are 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. The two quantities are very similar and the exclusion of 2009 does not lead to large changes in the effects of longer benefits. Column (5) reports the same measure with the contribution histories and shows only minimal differences in the effect of the longer subsidy. Interestingly, also the average number of weeks to the next employment is very similar and this confirms that transitions towards self-employment in this age group are relatively rare. 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.[30] Estimates are very consistent with the first method. Finally, Column (4) and Column (7) report the effects on nonemployment to the next job without any restriction on the first spell. This allows us to also take into account spells which might have started when the worker was still receiving benefits. This change leads to a small decline in the estimates, but the effects are still much in line with the main ones in terms of both average duration and magnitude of the effects of longer benefits. Overall, results of this exercise do not lead to substantial changes in the estimated effects and in the average duration of the nonemployment spell. This suggests that the use of spells only in the private sector does not constitute a strong limitation for the analysis.

Another possible solution is to abstract from individuals showing any transition towards different forms of self-employment. In this section, I run my main estimates on both nonemployment to the next job and total nonemployment over 4 years by excluding from the sample all workers who ever experienced a self-employment spell in the years following layoff. First, I exclude from the sample all individuals with a *parasubordinato* contract, that is workers who are categorized as self employed but their job shares many characteristics with dependent employees such as stable working hours, unique employer and so on. Then, I exclude all workers with a self- employment spell. It should be noted that, as data for full contribution histories have to be used, these estimates exploit only data for workers fired between 2010 and 2012. I report estimates in Table C4. Panel A reports the effect of longer benefits excluding workers who have at least one *parasubordinato* contract after their layoff. Their exclusion leads only to very small sample losses (about 18,500 spells or 4.2% of the sample) and estimates are very close to the ones in the main sample. Panel B restricts the sample to individuals for whom I have data on possible self-employment spells (those fired between 2010 and 2012) and then excludes all individuals with any spell as self-employed. Changes in the sample are more relevant than before but still limited (about 30,000 or 8.8%). More importantly, the estimated effects are almost unaffected

[30] This takes into account few cases in which the date of the benefit seems misreported with respect to the expected theoretical duration.

with respect to the main sample. These results reinforce the evidence of the previous analysis and show that self-employment plays at best a minor role in the main results.

Table C3: Effect of Potential Benefit Duration on Nonemployment Duration with Different Definitions of time to Next Employment

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
VARIABLES	Baseline	Same Sample	After end UB - Corr	After end UB - No Restr	Estratti Conto	Estratti Conto - Corr	Estratti Conto - No Restr
Above 50 years of age	6.123***	6.022***	6.025***	5.617***	6.094***	6.097***	6.010***
	(0.793)	(0.877)	(0.875)	(0.865)	(0.868)	(0.867)	(0.830)
Observations	438,403	346,421	346,421	346,421	346,421	346,421	346,421
Mean dependent	66.58	66.76	66.73	65.38	64.19	64.16	61.26
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 in weeks and amount of the benefit with a flexible squared polynomial on the two sides of the cutoff (50 years of age). Controls include past job and firm characteristics and fixed effects at month of layoff-local labour market level. List of controls: female, full time contract, past occupation dummies, permanent contract, daily wage in the last 6 months, average monthly wage in the last 3 months, market potential experience, tenure, tenure with temporary contract, log of average firm size, share of permanent contracts in the last firm, age of the last firm and sector dummies. Sample includes all recipients of unemployment benefits (OUNR) between February 2009 and December 2012 excluding workers fired from 49 years and 10 months of age to 50 years of age. Baseline sample in Column (1), sample restricted to individuals whom we can observe in all employment in Column (2), correction for end of unemployment benefits in Column (3), any start of employment for new job in Column (4). Columns (5) to (7) repeat the same analysis for the Estratti Conto, which report all employment spells in private sector, public sector and self employment. Baseline computed as the average for the dependent variable for workers fired between 49 years of age and 49 and 10 months of age. Standard errors clustered at Local Labour Market level. Level of significance: * 10%; ** 5%; *** 1%.

Table C4: Potential Benefit Duration and Exclusion of Self-Employment

VARIABLES	(1) Benefits	(2) Nonemployment	(3) Nonemployment (4y)
	Panel A: Baseline Sample		
Above 50 years of age	7.947***	6.123***	2.162***
	(0.269)	(0.793)	(0.646)
Observations	438,403	438,403	438,403
	Panel B: Exclusion *Parasubordinati*		
Above 50 years of age	7.941***	5.955***	2.300***
	(0.276)	(0.789)	(0.647)
Observations	419,980	419,980	419,980
	Panel C: Exclusion Self-Employed		
Above 50 years of age	7.886***	5.976***	2.476***
	(0.301)	(0.899)	(0.757)
Observations	321,773	321,773	321,773
Controls	YES	YES	YES
Month FE	YES	YES	YES
LLM FE	YES	YES	YES
LLM X Month FE	YES	YES	YES

Note: Linear regression for the duration in weeks and amount of the benefit with a flexible squared polynomial on the two sides of the cutoff. Controls include past job and firm characteristics and fixed effects at month of layoff-local labour market level. List of controls: female, full time contract, past occupation dummies, permanent contract, daily wage in the last 6 months, average monthly wage in the last 3 months, market potential experience, tenure, tenure with temporary contract, log of average firm size, share of permanent contracts in the last firm, age of the last firm and sector dummies. Sample includes all recipients of unemployment benefits (OUNR) between February 2009 and December 2012 excluding workers fired from 49 years and 10 months of age to 50 years of age. Baseline sample is reported in Panel A, sample excluding all workers with at least one spell as *parasubordinati* (self employed with many similarities with employees such as unique firm a which they work) is reported in Panel B and sample excluding any workers with self-employment spell is reported in Panel (C). Sample excluding self-employment is a subsample of all workers for which all spells in private, public and self-employment are observables (Column (2) of Table C3 for baseline). Baseline computed as the average for the dependent variable for workers fired between 49 years of age and 49 and 10 months of age. Standard errors clustered at Local Labour Market level. Level of significance: * 10%; ** 5%; *** 1%.

D Interaction with Disability and Pensions

The interaction between unemployment benefits and other labour market institutions 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, such as disability benefits and pensions (Inderbitzin et al., 2016; Kyyrä and Pesola, 2017).

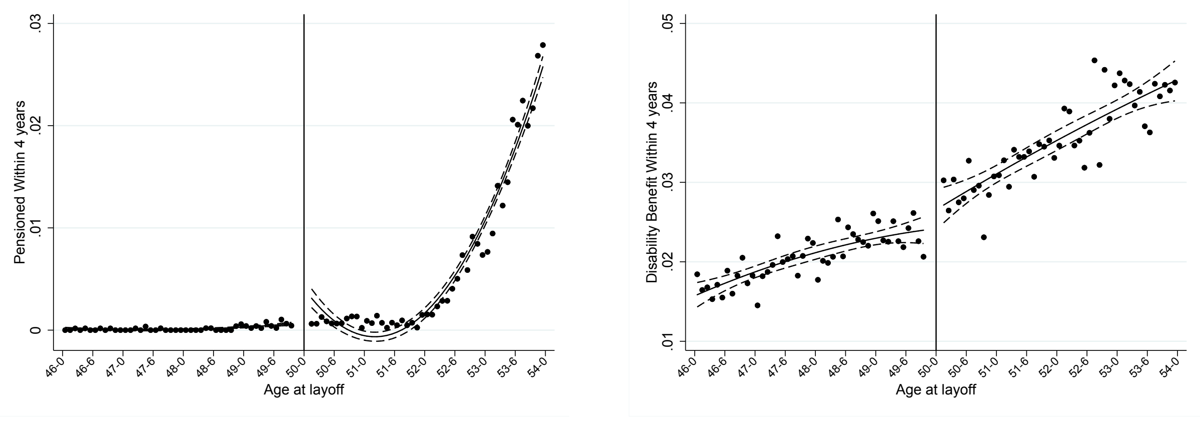
In this setting, I will consider the extensive margin for both these policies: I look at the probability of retirement within 4 years since layoff and at the take-up of disability benefits. Again, I only take into account take-up within 4 years to have a common horizon for all the individuals in my sample Results are reported in Table D1. 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 mildly statistically significant for disability benefits. Further graphical analysis in Figure D1 further confirms the small effect of the longer PBD on Pensions and disability and the effect of pension seem to be related to a poor fit of the polynomial close to the cutoff. Overall these results point at marginal increase in take-up of other programs, but the effect is small. Hence, these elements do not play an important role in the present analysis.

Table D1: Effect of Potential Benefit Duration on Pensions and Disability Benefits

VARIABLES	(1) Pensioned 4 Years	(2) Disability 4 Years
Above 50 years of age	0.003***	0.003*
	(0.000)	(0.002)
Observations	438,403	438,403
Baseline dependent	0.00	.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 past job and firm characteristics and fixed effects at month of layoff-local labour market level. List of controls: female, full time contract, past occupation dummies, permanent contract, daily wage in the last 6 months, average monthly wage in the last 3 months, market potential experience, tenure, tenure with temporary contract, log of average firm size, share of permanent contracts in the last firm, age of the last firm and sector dummies. 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 from 49 years and 10 months of age to 50 years of age. Baseline computed as the average nonemployment duration for workers fired between 49 years of age and 49 and 10 months of age. Standard errors clustered at Local labour Market level. Level of significance: * 10%; ** 5%; *** 1%.

Figure D1: Probability of Pensions and Disability Benefits over 4 years



(a) Pension

(b) Disability

Note: Effect of 4 additional months of potential benefit on probability of receiving pension or disability benefits within 4 years since initial layoff. Sample includes all recipients of unemployment benefits (OUNR) between February 2009 and December 2012 excluding workers from 49 years and 10 months of age to 50 years of age. Baseline computed as the average nonemployment duration for workers fired between 49 years of age and 49 and 10 months of age. Confidence interval at 95% reported.

E Medium Term effects 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 and aggregate total nonemployment over 7 years. Results of the estimation are reported in Table E1. 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 while the estimate for the total number of weeks in nonemployment over 7 years is actually lower, as reported in Column (2). Column (3) and Column (4) report the same effects over a 4 years horizon for the sake of comparison. Results for the effect in the first spell are larger but comparable to previous estimates while the effect over 4 years is smaller. This could be in part related to the role of the Great Recession which induced a strong contraction in the Italian economy and it might have made more difficult to find a job and more likely to lose it afterwards. Results are anyway in line with previous estimates and this suggests that these results are informative about longer horizons for the rest of the sample.[31] In addition, to check whether the difference in the two groups follows a cyclical pattern, I also check the pattern of employment over 7 years. Coefficients for monthly estimates are reported in Figure E1. In this case, convergence is even stronger and the two groups have the same employment 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

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

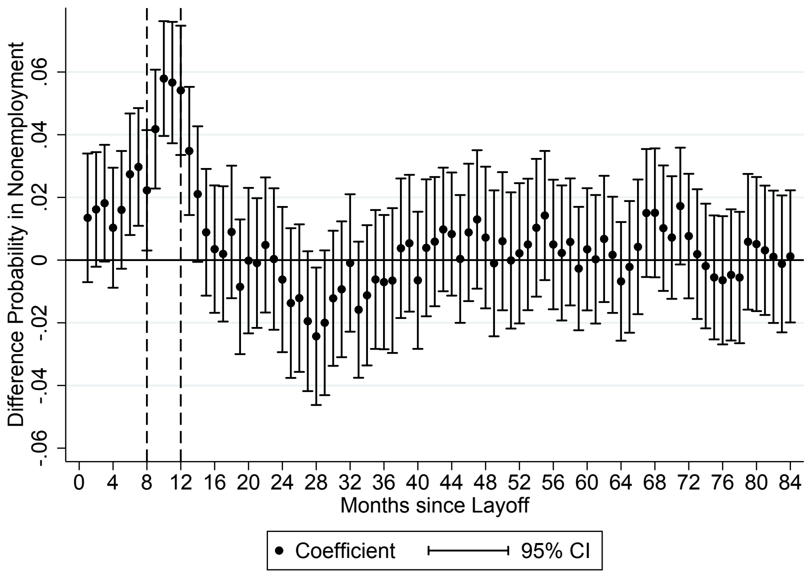
same for the remaining 4 years, although point estimates remain consistently positive but small and not statistically significant. This picture provides suggestive evidence that the two groups converge in the long run. Additional data for the other years would allow us to better understand to what extent these findings can be generalized to the rest of the sample.

Table E1: Effect of Potential Benefit Duration on Medium Term Outcomes

VARIABLES	(1) Nonemployment (7y)	(2) Nonemployment cum (7y)	(3) Nonemployment (4y)	(4) Nonemployment (4y)
Above 50 years of age	8.406***	1.593	6.589***	1.333
	(2.685)	(2.215)	(1.566)	(1.275)
Observations	91,982	91,982	91,982	91,982
Baseline dependent	89.42	225.15	65.89	127.82
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 in weeks and amount of the benefit with a flexible squared polynomial on the two sides of the cutoff (50 years of age). Controls include past job and firm characteristics and fixed effects at month of layoff-local labour market level. List of controls: female, full time contract, past occupation dummies, permanent contract, daily wage in the last 6 months, average monthly wage in the last 3 months, market potential experience, tenure, tenure with temporary contract, log of average firm size, share of permanent contracts in the last firm, age of the last firm and sector dummies. Sample includes all recipients of unemployment benefits (OUNR) between February 2009 and December 2009 excluding workers fired from 49 years and 10 months of age to 50 years of age. Baseline computed as the average nonemployment duration for workers fired between 49 years of age and 49 and 10 months of age. Standard errors clustered at Local Labour Market level. Level of significance: * 10%; ** 5%; *** 1%.

Figure E1: 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. List of controls: female, full time contract, past occupation dummies, permanent contract, daily wage in the last 6 months, average monthly wage in the last 3 months, market potential experience, tenure, tenure with temporary contract, log of average firm size, share of permanent contracts in the last firm, age of the last firm and sector dummies. Figure based on 92,928 layoffs between February 2009 and December 2009 for workers between 46 and 54 years at layoff excluding workers from 49 years and 10 months of age and 50 years of age. Standard errors clustered at Local Labor Market level. Confidence interval at 95% reported.

F New Job Characteristics - Regression

Table F1: Effect of Potential Benefit Duration on New Job Characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
		Coworkers					Job				
VARIABLES	Age New Firm	% permanent	% full time	% male	Average Age	Average Monthly Income	(log) size	Tenure	Full Time	Permanent	(log) Daily Wage
Above 50 years of age	1.262**	-0.002	0.001	-0.001	0.016	-0.003	-0.020	0.279	0.007*	0.008*	0.006
	(0.612)	(0.003)	(0.003)	(0.003)	(0.066)	(0.006)	(0.014)	(0.806)	(0.004)	(0.004)	(0.004)
Observations	352,486	336,944	336,944	336,944	336,923	336,177	350,866	352,486	352,486	352,486	348,562
Mean dependent	194.84	.56	.75	.64	40.2	6.96	2.66	66.93	.77	.26	4.07
Se dependent	175.79	.38	.3	.32	6.21	.62	1.61	77.72	.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: 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 labour market interacted with month fixed effects. List of controls: female, full time contract, past occupation dummies, permanent contract, daily wage in the last 6 months, average monthly wage in the last 3 months, market potential experience, tenure, tenure with temporary contract, log of average firm size, share of permanent contracts in the last firm, age of the last firm and sector dummies. Figure based on 352,486 layoffs between February 2009 and December 2009 for workers between 46 and 54 years at layoff excluding workers from 49 years and 10 months of age and 50 years of age. Sample restricted to individuals who find a job within 4 years since layoff. Standard errors clustered at Local Labour Market level. Level of significance: * 10%; ** 5%; *** 1%.

G Recall

Recall is an important and pervasive phenomenon in the labour market. Indeed, 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 built a theoretical framework to conceptualize it in relation with unemployment benefits. More recently several works started to revisit this employment pattern using richer and novel administrative data. Nekoei and Weber (2015) stress 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 search frictions for employer to find 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 perform a different search and show a different reemployment pattern with respect to other workers. In addition, workers may bargain with the firm to time their hiring with the end of unemployment benefits. So far, we do not have any evidence concerning the relationship between unemployment benefits durations and the pattern of recalls. As a first step, it is useful to characterize recalls[32] and assess what are the characteristics that make more likely the hiring by the same employer. Table G1 reports a series of regression for workers in our sample with dependent variable equal to one if the worker is hired by the same firm and zero 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.[33] Finally, recall are

[32]Information on the expectation of recall are unfortunately not available and, as a consequence, I will only focus on realized recalls.

[33]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.

more frequent for blue collar and apprentices and in cyclical sectors.[34] As shown before, being eligible to longer slightly reduces the probability of recall.

As a second step, I 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 (the latter includes also workers who do not find a job within the time horizon). Results, reported in Figure G1, 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 same firm. This neatly shows how the pattern observed for the overall sample reflects recurrent employment \unemployment 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 G2, 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

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

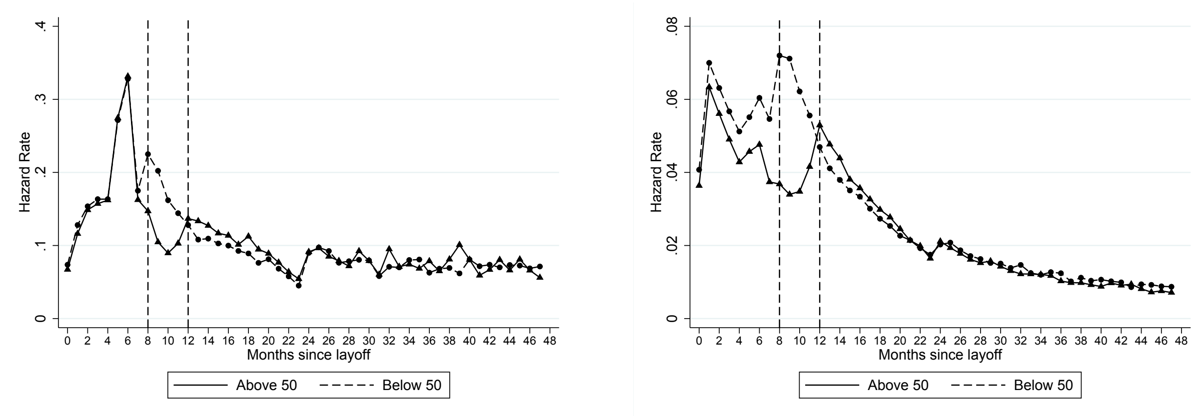
to the ones in the main equation. This shows that results are largely driven for workers facing *ex novo* searches in the labour market. This is consistent with the fact that a large share of recalls takes place in the first six months of the nonemployment spells: the share of workers who is recalled 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 G1: Observables and Probability of Recall

VARIABLES	(1) Overall	(2) Overall	(3) Permanent	(4) Temporary
Female	0.052***	0.060***	0.034***	0.064***
	(0.004)	(0.004)	(0.005)	(0.004)
Full Time	0.015**	-0.005	0.013***	-0.031***
	(0.006)	(0.004)	(0.005)	(0.005)
White Collar	-0.097***	-0.066***	-0.044***	-0.065***
	(0.008)	(0.004)	(0.004)	(0.008)
Apprentice	0.253***	0.139**	0.175	0.112*
	(0.056)	(0.060)	(0.141)	(0.061)
Other	-0.087***	-0.026	0.044	-0.118***
	(0.029)	(0.024)	(0.028)	(0.044)
Manager	-0.213***	-0.102***	-0.030***	-0.151***
	(0.015)	(0.013)	(0.011)	(0.041)
Permanent Contract	-0.037***	-0.090***		
	(0.007)	(0.005)		
Log Daily Income	-0.008	-0.003	-0.019***	0.029***
	(0.007)	(0.004)	(0.004)	(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***	0.016***
	(0.001)	(0.000)	(0.001)	(0.001)
Tenure Temporary	0.066***	0.055***	0.034***	0.040***
	(0.003)	(0.002)	(0.003)	(0.002)
Log Avg. Size Firm	0.009***	0.014***	-0.007***	0.024***
	(0.002)	(0.001)	(0.002)	(0.002)
Share Permanent in Last Firm	-0.123***	-0.042***	-0.083***	-0.123***
	(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)
Cyclical Sector	0.157***			
	(0.011)			
Fired after 50	-0.010**	-0.008*	-0.006	-0.009
	(0.005)	(0.004)	(0.007)	(0.007)
Observations	353,493	353,493	172,166	181,327
Baseline dependent	.419	.419	.288	.543
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. Regressions includes a square polynomial in age with different slopes around the cutoff, controls for the worker and last firm characteristics, local labour market interacted with month fixed effects. List of controls: female, full time contract, past occupation dummies, permanent contract, daily wage in the last 6 months, average monthly wage in the last 3 months, market potential experience, tenure, tenure with temporary contract, log of average firm size, share of permanent contracts in the last firm, age of the last firm and sector dummies. Figure based on 353,493 workers fired between February 2009 and December 2009 for workers between 46 and 54 years at layoff excluding workers from 49 years and 10 months of age and 50 years of age. Sample restricted to individuals who find a job within 4 years since layoff. Standard errors clustered at Local Labour Market level. Level of significance: * 10%; ** 5%; *** 1%.

Figure G1: Hazard rate for Exit towards Employment: Recall vs Not Recall



(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 in nonemployment after t-1 months. Figure based on 438,403 layoff excluding workers fired between 49 years and 10 months of age and 50 years of age. Panel (a) includes all workers who were recalled to their previous firm while Panel (b) includes all workers who move to a new firm or do not find a job in the private sector.

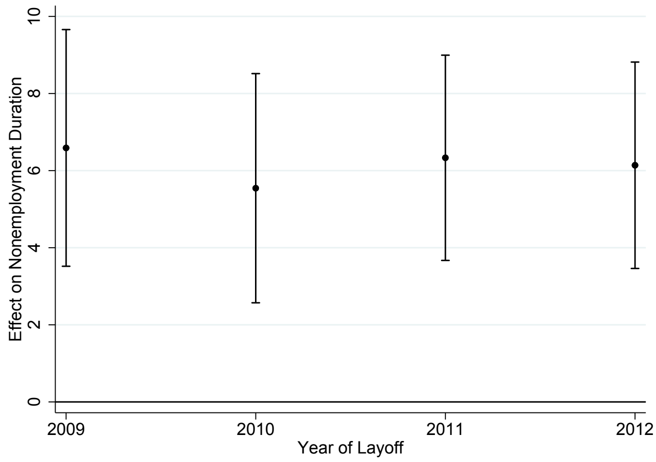
Table G2: Effects of longer PBD for workers Recalled or Changing Firm

	Recall			Not Recall		
	(1)	(2)	(3)	(4)	(5)	(6)
	Nonempl	Nonemp (4 years)	(log) W+AB	Nonempl	Nonempl (4 years)	(log) W+AB
Above 50 years of age	2.318***	-0.651	0.0150*	6.145***	2.027**	0.0258**
	(0.413)	(0.752)	(0.00824)	(0.758)	(0.964)	(0.0107)
Observations	146,894	146,894	146,894	206,599	206,599	206,599
Baseline dependent	25.85	106.67		46.73	117.75	
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

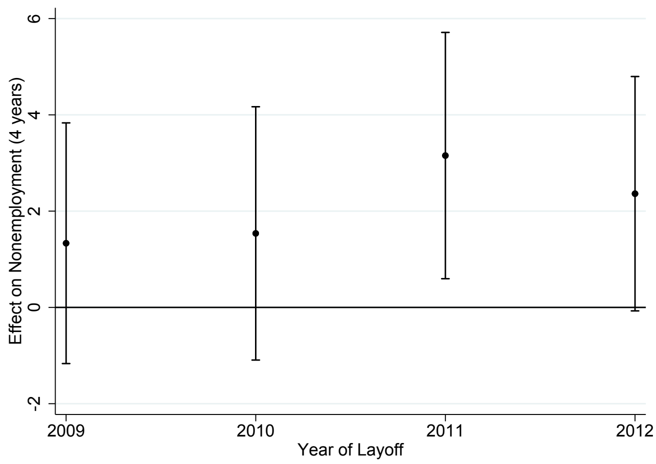
Note: Linear regression for the effects of unemployment benefits on main variables of interest. Regressions includes a square polynomial in age with different slopes around the cutoff, controls for the worker and last firm characteristics and local labour market interacted with month fixed effects. List of controls: female, full time contract, past occupation dummies, permanent contract, daily wage in the last 6 months, average monthly wage in the last 3 months, market potential experience, tenure, tenure with temporary contract, log of average firm size, share of permanent contracts in the last firm, age of the last firm and sector dummies. Sample includes all recipients of unemployment benefits between February 2009 and December 2012 excluding workers fired from 49 years and 10 months of age and 50 years of age. Baseline computed as the average not employment duration for workers fired between 49 years of age and 49 and 10 months of age. Standard errors clustered at Local Labour Market level. Level of significance: * 10%; ** 5%; *** 1%.

H Heterogenous Effects: by year and sector

Figure H1: Effects on Nonemployment in first spell and over 4 years by year of layoff



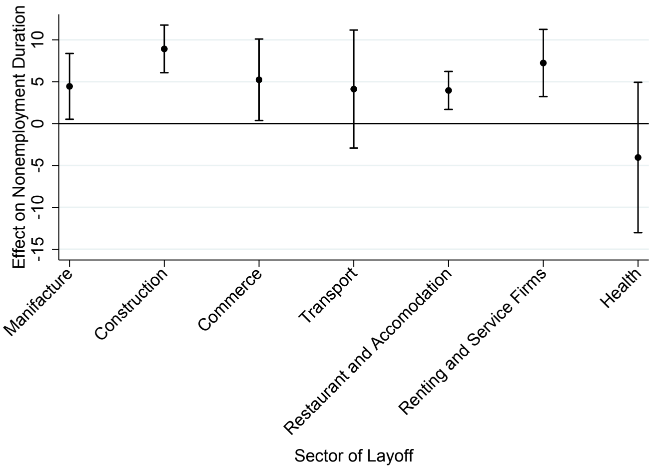
(a) Effect on Nonemployment



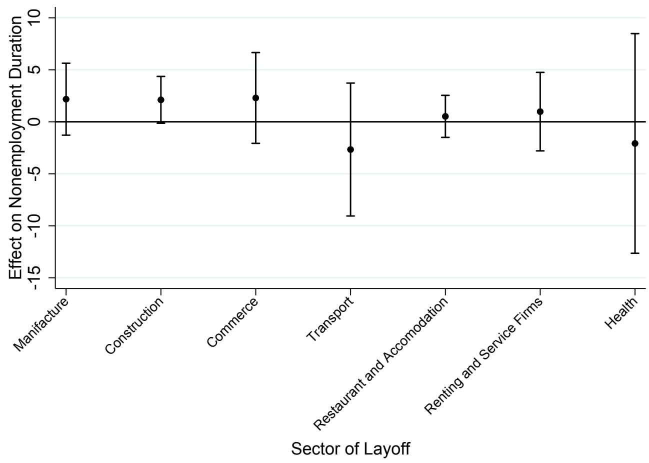
(b) Effect on Nonemployment (4 years)

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, and local labour market interacted with month fixed effects. List of controls: female, full time contract, past occupation dummies, permanent contract, daily wage in the last 6 months, average monthly wage in the last 3 months, market potential experience, tenure, tenure with temporary contract, log of average firm size, share of permanent contracts in the last firm, age of the last firm and sector dummies. Sample includes all recipients of unemployment benefits between February 2009 and December 2012 excluding workers fired from 49 years and 10 months of age and 50 years of age. Standard errors clustered at Local Labour Market level. Confidence interval at 95% reported.

Figure H2: Effects on Nonemployment in first spell and over 4 years by broad NACE sector



(a) Effect on Nonemployment

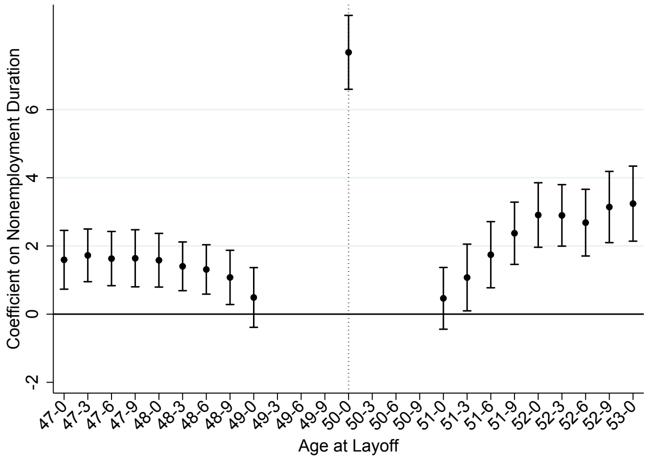


(b) Effect on Nonemployment (4 years)

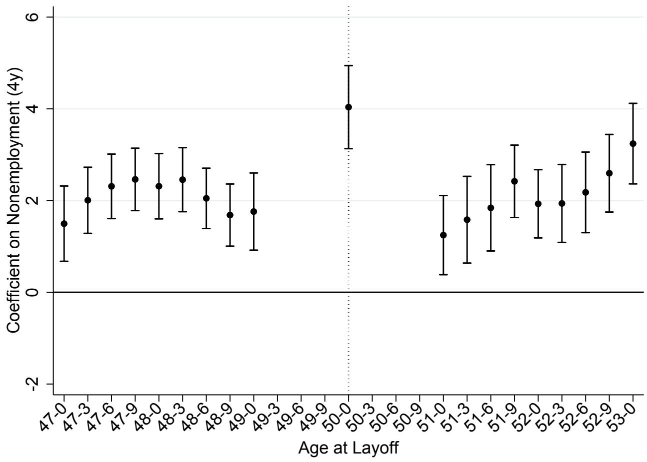
Note: Effect of 4 additional months of potential benefit duration on nonemployment in the first spell (a) and nonemployment over 4 years (b). Regressions includes a square polynomial in age with different slopes around the cutoff, controls for the worker and last firm characteristics, local labour market interacted with month fixed effects. List of controls: female, full time contract, past occupation dummies, permanent contract, daily wage in the last 6 months, average monthly wage in the last 3 months, market potential experience, tenure, tenure with temporary contract, log of average firm size, share of permanent contracts in the last firm, age of the last firm and sector dummies. Sample includes all recipients of unemployment benefits between February 2009 and December 2012 excluding workers fired from 49 years and 10 months of age and 50 years of age. Standard errors clustered at Local Labour Market level. Confidence interval at 95% reported.

I Placebo with Non Parametric RDD

Figure I1: Placebo RDD: Non Parametric



(a) Nonemployment - first spell



(b) Nonemployment - 4 years

Note: Placebo linear regression for duration of nonemployment in the first spell and over 4 years since layoff. Figure based on workers fired between February 2009 and December 2012 for workers between 46 and 54 years at layoff excluding workers with 49 years and 10 months of age and 50 years of age. Regressions includes a square polynomial in age with different slopes around the cutoff, controls for the worker and last firm characteristics, local labour market interacted with month fixed effects. List of controls: female, full time contract, past occupation dummies, permanent contract, daily wage in the last 6 months, average monthly wage in the last 3 months, market potential experience, tenure, tenure with temporary contract, log of average firm size, share of permanent contracts in the last firm, age of the last firm and sector dummies. 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 Labour Market level. Confidence interval at 95% reported.