London School of Economics and Political Science

Essays in Applied Econometrics

Vincenzo Scrutinio

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Declaration

I certify that the thesis I have presented for examination for the PhD degree of the London School of Economics and Political Science is solely my own work other than where I have clearly indicated that it is the work of others (in which case the extent of any work carried out jointly by me and any other person is clearly identified in it).

I declare that my thesis consists of 50,000 words.

Statement of conjoint work

I confirm that Chapter 2 was jointly co-authored with Luca Citino and Kilian Russ, and I contributed 33% of this work.

I confirm that Chapter 3 was jointly co-authored with Shqiponja Telhaj and Steven Gibbons, and I contributed 33% of this work.

Statement of use of third party for editorial help

I can confirm that my Chapter 1 was copy edited for conventions of language, spelling and grammar by Mrs Becca Fuller, a third-party proofreading consultant.

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Abstract

This thesis consists of three chapters.

In the first chapter (The Medium Term Effects of Unemployment Benefits), I explore the effect of longer potential duration of unemployment benefits on workers' employment over 4 years after layoff. To this purpose, I exploit rich and novel administrative data from Italy. The identification is based on an age at layoff rule which determines 4 additional months of benefits for workers who are fired after turning 50 years of age. I use this in a Regression Discontinuity Design with a donut correction to account for strategic delay of layoff in the neighbourhood of the age threshold. I show that workers with longer potential benefits spend more time on benefits and in nonemployment before finding a new job than workers fired before turning 50 years of age. However, I find that the two groups of workers spend a similar amount of time in nonemployment over 4 years since layoff. This shows that classical estimates of nonemployment effects of unemployment benefits, which do not take into account recurrent nonemployment spells, tend to overestimate these negative effects.

In the second chapter (Happy Birthday? Manipulation and Selection in Unemployment Insurance), with Luca Citino (LSE and Bank of Italy) and Kilian Russ (Bonn School of Economics), we study the strategic timing of layoff for workers to gain eligibility to longer benefits. We use rich Italian administrative data and we focus on an age at layoff rule which determined an increase in unemployment benefit potential duration for workers fired after turning 50 years of age. We find that, in a neighbourhood of the threshold, a relevant share of individuals delays the date of layoff in order to be eligible to longer benefits. These workers are more likely to be women, white collar, part time and to be employed in small firms with respect to workers who do not engage in manipulation. Most importantly, these workers show a higher baseline risk of long-term unemployment. Although

manipulation leads to a large increase in benefits, the mechanical component plays a central role and behavioural responses are limited.

In third chapter (Teacher Turnover? Does it Matter for Student Achievement), with Shqiponja Telhaj (University of Sussex) and Steve Gibbons (LSE), we study the effect of teacher turnover in UK secondary schools. By using a rich regression model and large administrative data, we find that teacher turnover has a small negative, but highly statistically significant, effect on pupils' performance. This effect is stronger for pupils at the bottom of grade distribution and from disadvantaged background.

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Chapter 1

The Medium Term Effects of Unemployment Benefits

Vincenzo Scrutinio
IZA, London School of Economics

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.

Keywords: Unemployment insurance; post-unemployment wages; regression discontinuity design; medium term outcomes.
J.E.L. codes: E24; H24; H55; J65.

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1.1 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 behaviour 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 relevant, 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., 2012a, 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 the amount of the benefit, 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 this vast literature, we only have limited evidence on the medium run effects of unemployment benefits. Degen and Lalive [2013] use a difference-in-difference strategy to assess the effect of a reduction 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 labour 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 1.2 describes the Italian institutional setting; Section 1.3 provides a description of the data and of the sample construction;

Section 1.4 outlines the empirical strategy and methodological approach; Section 1.5 reports the main results and discusses the mechanism at work; Section 1.6 looks at heterogeneous effects; Section 1.7 implements a series of robustness checks; finally Section 1.8 concludes.

1.2 Institutional setting

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 14^{th} 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 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,

[1] The *parasubordinati*, workers with usually exclusive contracts for specific tasks and projects with a firm, are categorized as self-employed and they were not eligible to the subsidy. A new unemployment benefit 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.

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

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 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 1.A.

1.3 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 notably, from schools.[6] In addition, I also exclude all the observations concerning workers suspended for a temporary slowdown in the economic activity as, in their case, the subsidy has a different structure and they still keep a close relationship with their previous employer.[7] The final dataset contains 452,888 spells for 328,835

[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.

[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.

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 1.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 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 1.1 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

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

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 1.1.

1.4 Empirical strategy

The identification of the causal effects of treatment is based on a quasi-experimental variation in the 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

[9] See Appendix 1.A.

groups should have similar characteristics and this strategy allows to identify the causal effects of longer PBD. In practice, I estimate the following equation:

$$y_{ist} = \beta_0 + \beta_1 I(\widetilde{Age}_{it} \geq 0) + \sum_{j=1}^{k} \gamma_j \widetilde{Age}_{it}^{j} + \sum_{j=1}^{k} \delta_j \widetilde{Age}_{it}^{j} \mathbb{1}(\widetilde{Age}_{it} \geq 0) + \\ + X'_{ist}\pi + \mu_{st} + \epsilon_{ist} \tag{1.1}$$

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 1.7. 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 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 1.2. 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., 2018). To overcome this issue while keeping the comparison to individuals with similar age, I implement a donut regression discontinuity design in the spirit of Barreca et al. [2011]: 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. [2018]. I perform several robustness checks for this choice in Section 1.7 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 1.3. 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 50 years of age, and on the set of interacted months and LLM fixed effects. Table 1.2 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.

1.5 Results

1.5.1 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 1.4 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 1.5. 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 1.1.[10] Results are reported in Table 1.3 and 1.4. Coefficients confirm that the longer PBD leads to longer benefit and nonemployment duration.[11] The effects for the duration of

[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 1.7.1

[11]For the sake of comparison, I report in Appendix 1.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.

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 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 1.6: 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

[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 1.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 1.C.3.

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 1.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 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

[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.

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

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 1.7. 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.

1.5.2 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 1.8. 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 1.5, 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 1.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 1.7, this specification allows to account for repeated transitions in and out nonemployment. Figure 1.9 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 benefit duration. The period of convergence between the two groups is also much shorter: while in Figure 1.7 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 1.10 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,

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

[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 1.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.

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.
- 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 1.11 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 1.F. Workers with longer PBD experience small gains in daily wage (a 0.6% increase). Previous studies 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 1.6 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

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

[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.

offers better employment prospects in Table 1.7. 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 (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 1.12, 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.

[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.

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 1.8. 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 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 Column (5) by adding a dummy for workers having more than 52 weeks of work in the two years before the new layoff, and then by 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 1.9 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 1.13. 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 1.9 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 1.7 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 1.9 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.

1.6 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 1.10 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

[25] Indeed the size of the firm positively affects the probability of recall as discussed in Appendix 1.G.

a more generous benefit under certain conditions.[26] However, the contract composition is not enough to explain lower effects for workers coming from larger firms as similar discrepancies can also be observed within contract group. 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; bottom quartile of the distribution in the sample) and expansions (+3% in the number of employed over the last year in the LLM; top quartile of the distribution in the sample). 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

[26] These conditions concern the tenure of the worker and the size and sector of the firm. See Appendix 1.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.

nonemployment is very close to zero such as in the case of workers from temporary contracts and laid off during an expansion.

1.7 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.

1.7.1 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 1.11. 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

[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.

main findings although slightly larger. Panel 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.

1.7.2 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 1.14. 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 1.11.

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 1.15. 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 1.11, 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.

1.7.3 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 1.16. 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 1.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.

1.8 Conclusions

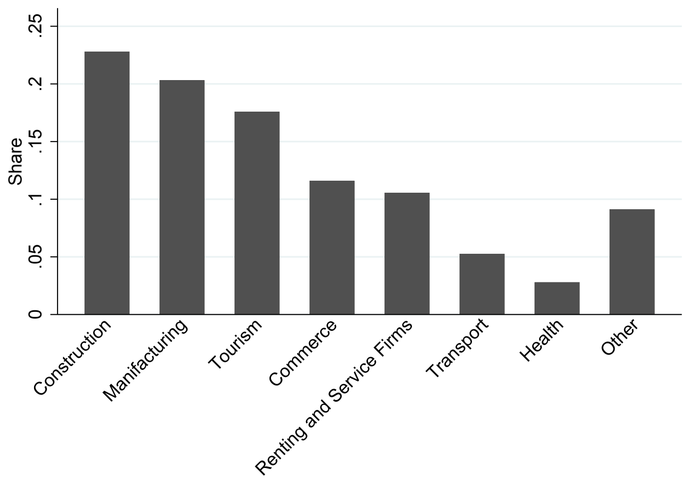
In this paper, I investigate the medium term impact of longer unemployment benefits on workers employment. This margin, mostly neglected by previous studies, is crucial from a policy perspective: on the one hand, longer periods in nonemployment could lead to human capital depreciation or scarring and 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 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.

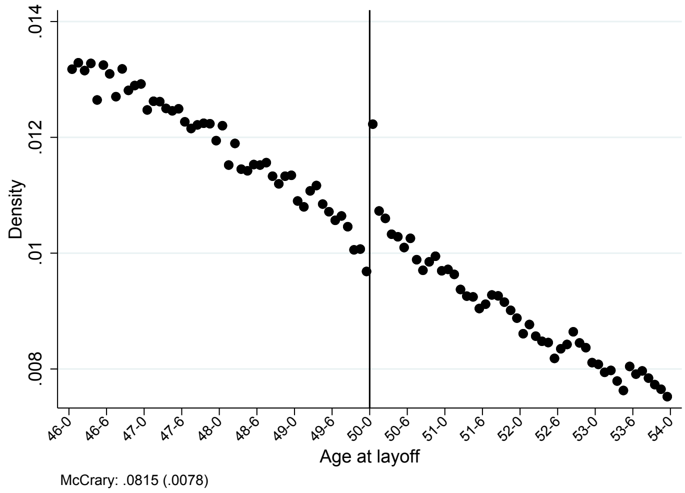
Figures

Figure 1.1: Sector composition for recipients of unemployment benefits



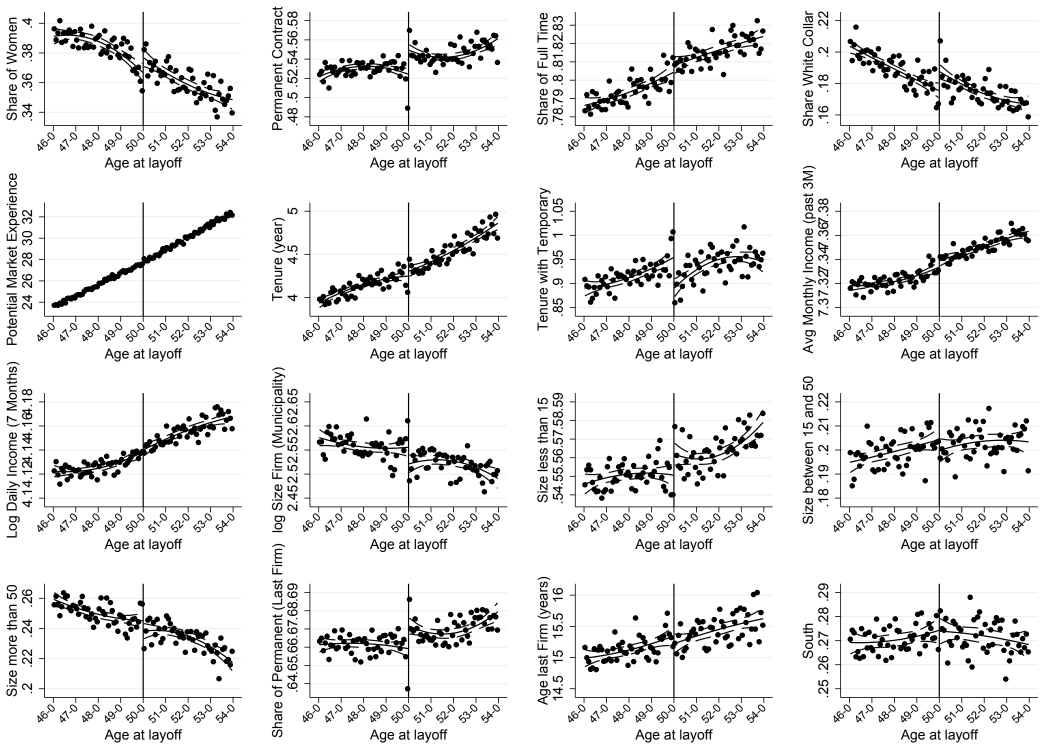
Note: Figure reports sector composition for workers fired between 2009 and 2012 and receiving unemployment benefits. Sample restricted to workers fired between 46 and 54 years of age at the moment of layoff.

Figure 1.2: Density of recipient of unemployment benefits by age (month)



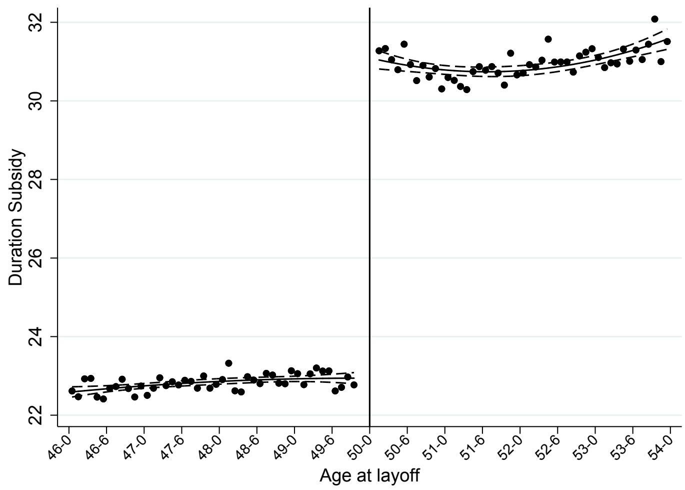
Note: Density of age at layoff for the universe of recipients of unemployment benefits (OUNR) between 2009 and 2012. Number of recipients of unemployment benefits: 452,888. Result of McCrary test for discontinuity at the threshold reported at the bottom of the graph. Corresponding t-stat is 10.45.

Figure 1.3: Continuity of observable characteristics at cutoff



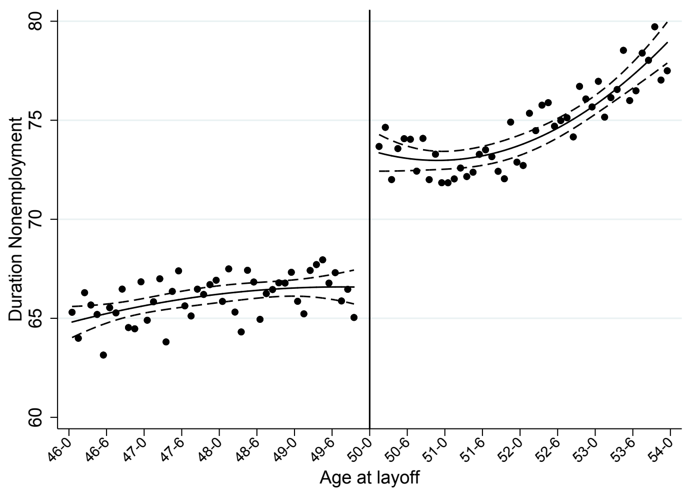
Note: Average of observable characteristics in a 4 year radius around the cutoff. Number of recipients of unemployment benefits: 452,888; variables reported (from left to right and top to bottom): Female; Permanent Contract; Full time; White Collar; Potential Market Experience; Tenure; Tenure with Temporary 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 1.4: Weeks of benefit



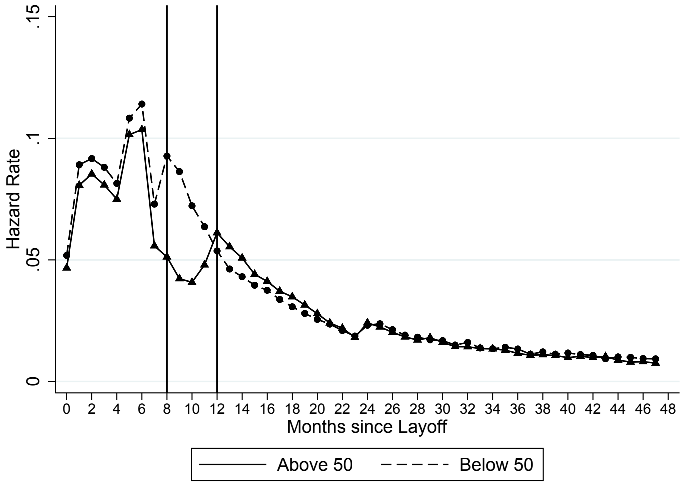
Note: Figure reports weeks on benefits in the first spell after layoff. Figure based on 438,403 layoffs between February 2009 and December 2012 for workers between 46 and 54 years of age at layoff excluding workers from 49 years and 10 months of age to 50 years of age. Polynomial fit is estimated by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

Figure 1.5: Weeks of nonemployment



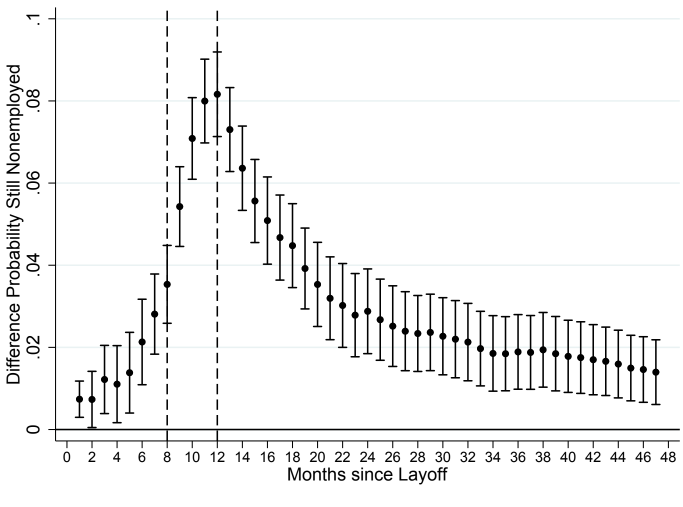
Note: Figure reports the weeks of nonemployment in the first spell after layoff. Figure based on 438,403 layoffs between February 2009 and December 2012 for workers between 46 and 54 years of age at layoff excluding workers from 49 years and 10 months of age to 50 years of age. Period of nonemployment 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 1.6: Hazard rate for exit from nonemployment



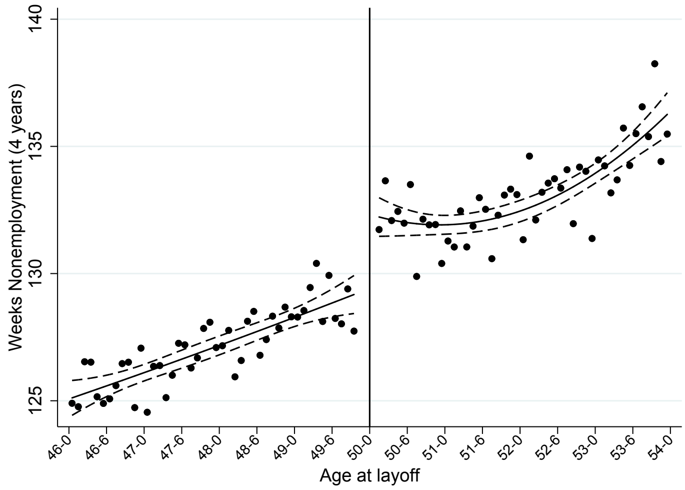
Note: Hazard rate for exit of workers from nonemployment towards employment in the private sector. Hazard rate computed as the share of workers exiting nonemployment in month t over the number of workers still nonemployed after t-1 months. Figure based on 438,403 layoffs between February 2009 and December 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 1.7: Difference in reemployment probability since layoff



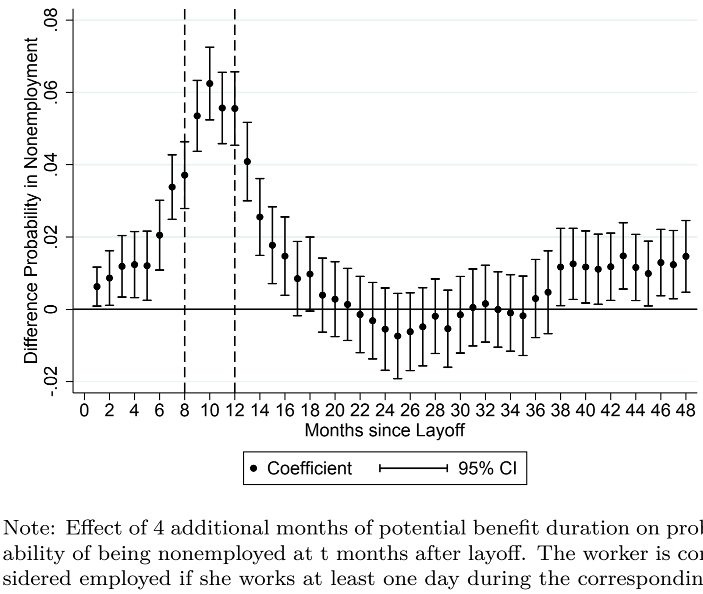
Note: Effect of 4 additional months of potential benefit duration on probability of being still nonemployed after t months. Linear probability models with dummy equal to 1 if the worker is still nonemployed after t months since layoff. Regressions include a square polynomial in age with different slopes 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 1.8: Total weeks of nonemployment (4 years)



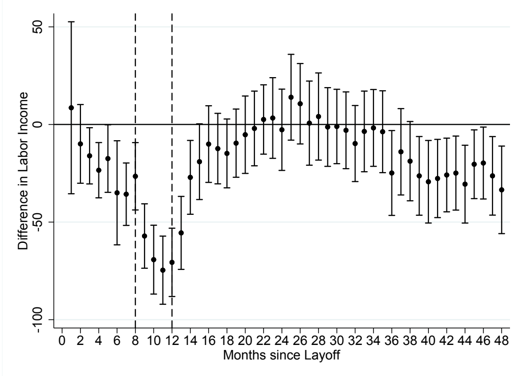
Note: Total weeks of nonemployment within 4 years since layoff. Figure based on 438,403 layoffs between February 2009 and December 2012 for workers between 46 and 54 years of age at layoff excluding workers from 49 years and 10 months of age to 50 years of age. Linear fit estimated by OLS with a square 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 1.9: Difference in nonemployment probability over 4 years since layoff

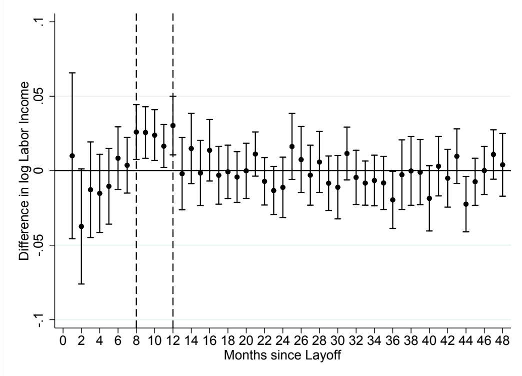


Note: Effect of 4 additional months of potential benefit duration on probability of being nonemployed at t months after layoff. The worker is considered employed if she works at least one day during the corresponding month in the private sector. Regressions include a square polynomial in age with different 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.

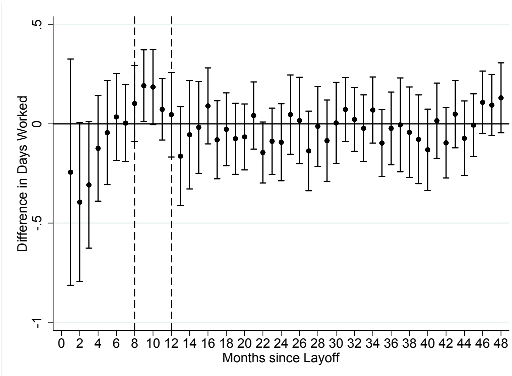
Figure 1.10: Income, days worked and unemployment benefits over 4 years after layoff



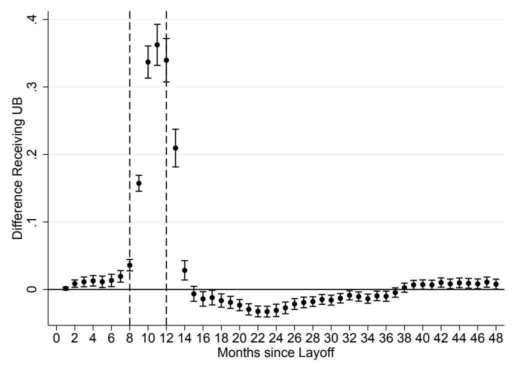
(a) Total Income



(b) Log Total Income (employed)



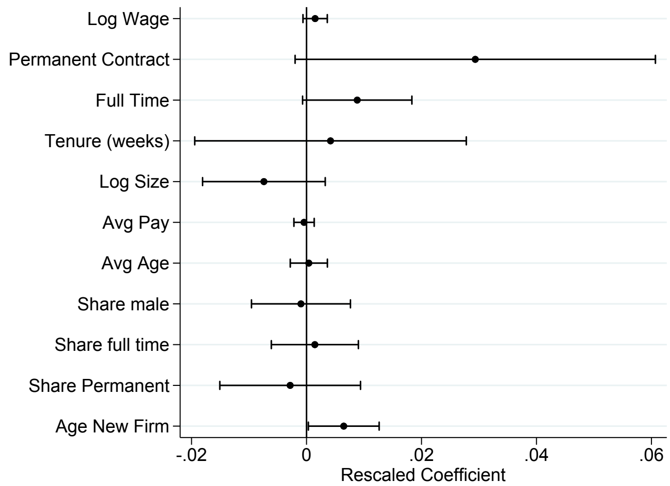
(c) Days Worked (employed)



(d) Receiving UB

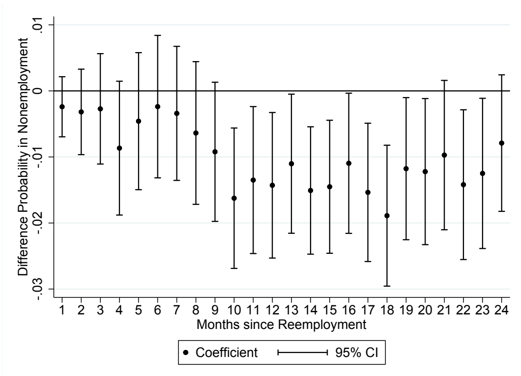
Note: Effect of 4 additional months of potential benefit duration on labour earnings (a), log labor earnings (b), days worked (c) and probability of receiving UB (d). Panel (b) and (c) conditional on employment. Regressions include a square polynomial in age with different slopes around the cutoff, controls 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 1.11: Effect on first employment characteristics (first spell)

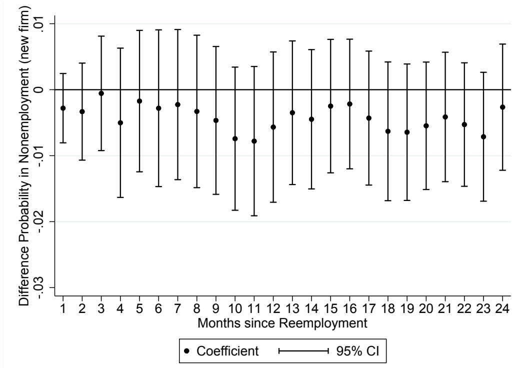


Note: Effect of 4 additional months of potential benefit duration on post unemployment job characteristics. Regressions include a square polynomial in age with different slopes around the cutoff, controls for the worker and last firm characteristics, local labour market interacted with month fixed effects. 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 1.12: Probability of nonemployment following reemployment



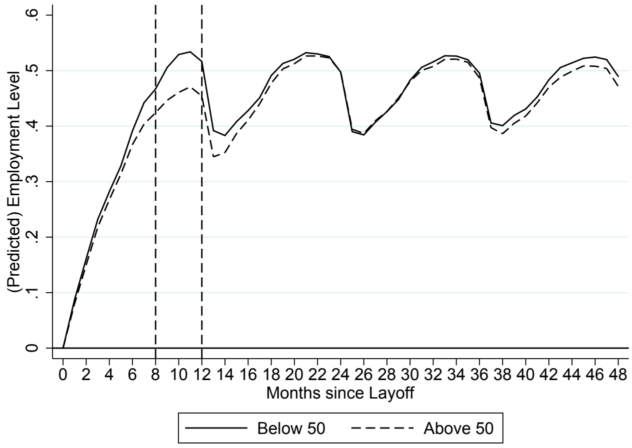
(a) Nonemployment - any firm



(b) Nonemployment - first reemployment firm

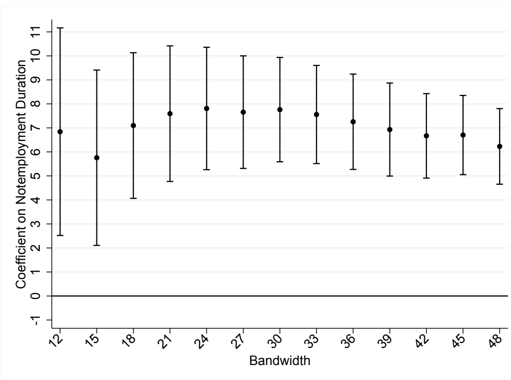
Note: Effect of 4 additional months of potential benefit duration on probability of being employed at t months after reemployment. The worker is considered employed if she works at least one day during the corresponding month in the private sector. Regressions include a square polynomial in age with 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 1.13: Employment rate for workers at the cutoff

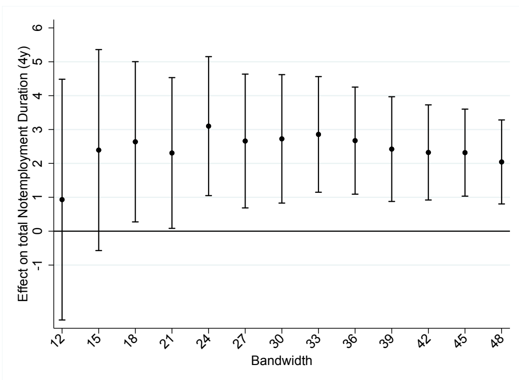


Note: Share of workers employed at 50 years of age on the two sides of the cutoff. Estimates are based on RDD regressions with dependent variable a dummy taking value one if the worker is employed at month t and value 0 otherwise. Regressions include second order polynomial in age with different slopes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

Figure 1.14: RDD estimates with different bandwidths.



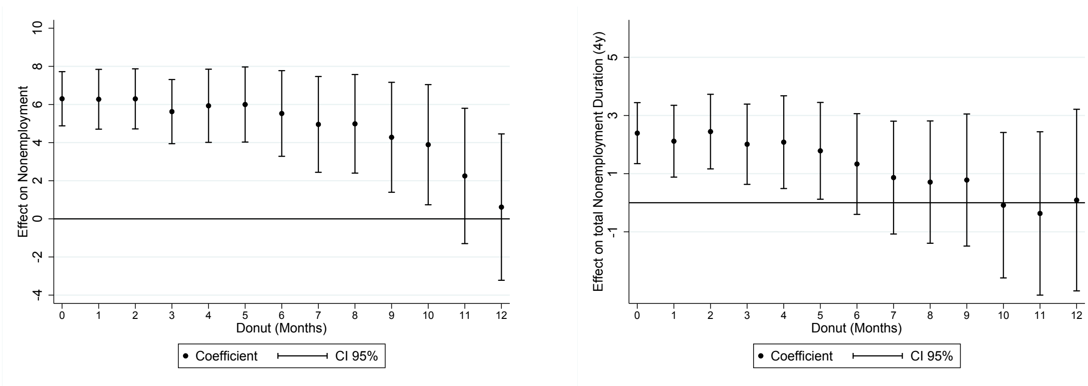
(a) Nonemployment - first spell



(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 1.15: RDD estimates with different donut holes

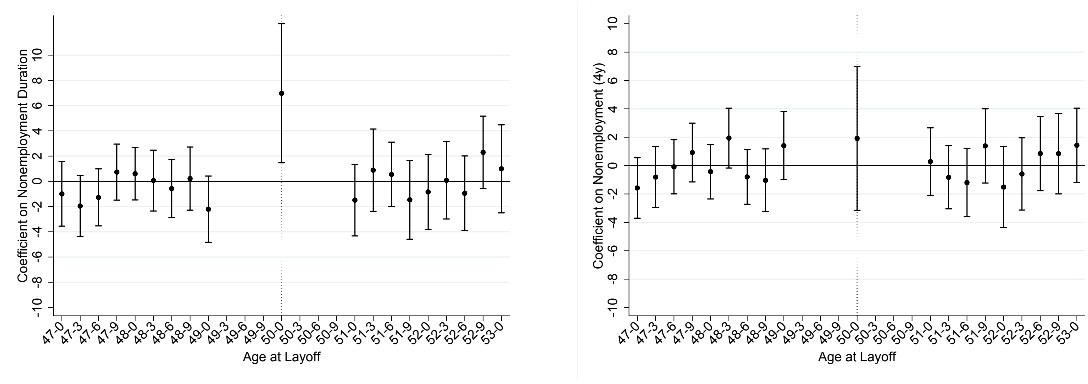


(a) Nonemployment - first spell

(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 1.16: 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 1.1: 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 1.2: 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 1.3: 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 1.4: Effect of potential benefit duration on nonemployment duration

	(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 1.5: Effect of potential benefit duration on medium term outcomes

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	Weeks Nonemployment	Income (W)	Income (W+B)	log Income (W+B)	Income (W+AB)	log Income (W+AB)	# Other UB	Amount Other UB	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 1.6: 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 1.7: 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 1.8: Effect of potential benefit duration on employment after first reemployment

VARIABLES	(1) Weeks	(2) Weeks - first firm	(3) Weeks - other firms	(4) J-t-J	(5) Weeks - other firms	(6) Weeks - other firms	(7) 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 1.9: 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 1.10: Heterogeneous effects on nonemployment duration before the finding a new job and over 4 years

VARIABLES	(1) Baseline	(2) Centre-North	(3) South-Island	(4) Female	(5) Male	(6) < 15 emp	(7) 15-49 emp	(8) > 49 emp	(9) Permanent	(10) Temporary	(11) Cyclical	(12) Not Cyclical	(13) Contraction	(14) 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 1.11: 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%.

Appendices

1.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 31st 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 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 1.A.1: Duration for mobility benefit

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

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

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.

1.B Additional information on data

1.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.

1.B.2 Main variables definition

Table 1.B.1: Main variables definition

Variable	Description
Nonemployment	Number of weeks between lay-off 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 lay-off. 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.

Variable	Description
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.
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.

Variable	Description
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.

1.C Extensions for effects on the first spell of nonemployment

1.C.1 Clustering

Table 1.C.1: Potential benefit duration and time to next job: cluster

VARIABLES	(1) LLM	(2) Month	(3) Age	(4) 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%.

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 1.7 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 table 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. This also shows that censoring is far from innocuous and this particular choice should take into account the long run reemployment probability of workers.

Table 1.C.2: Effect of potential benefit duration on nonemployment duration with different censoring

	(1)	(2)	(3)	(4)
VARIABLES	4 years	3 years	2 years	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%.

1.C.3 Transitions towards public sector and self employment

Data allow to explicilty consider only spells in the private sector 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 1.C.3. 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

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

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 1.C.4. 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 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 1.C.3: Effect of potential benefit duration on nonemployment duration with different definitions of time to next employment

VARIABLES	(1) Baseline	(2) Same Sample	(3) After end UB - Corr	(4) After end UB - No Restr	(5) Estratti Conto	(6) Estratti Conto - Corr	(7) Estratti Conto - No Restr
Above 50 years of age	6.123***	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 1.C.4: Potential benefit duration and exclusion of self-employment

	(1)	(2)	(3)
VARIABLES	Benefits	Nonemployment	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 1.C.3 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%.

1.D Interaction with disability benefits 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).

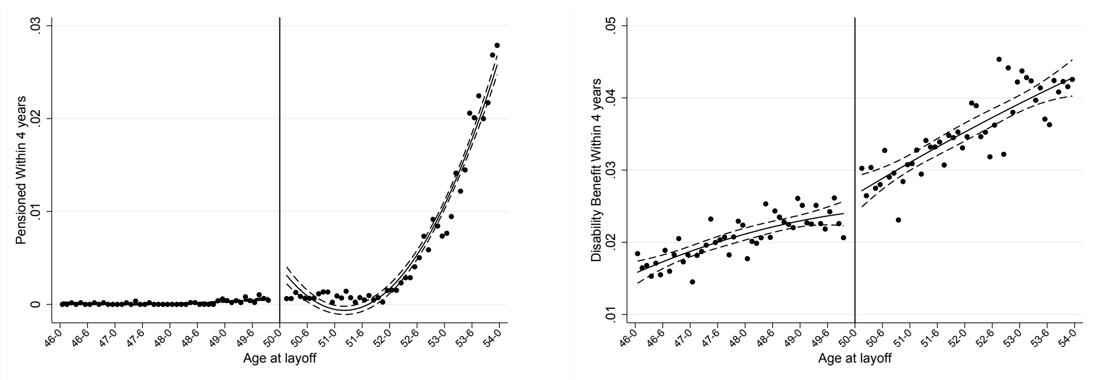
In this setting, I will consider the extensive margin for both these policies: I look at the probability of retirement within 4 years since layoff and at the take-up of disability benefits. Again, I only take into account take-up within 4 years to have a common horizon for all the individuals in my sample Results are reported in Table 1.D.1. 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 1.D.1 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 1.D.1: 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 1.D.1: Probability of pension 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.

1.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 1.E.1. 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 1.E.1. 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$^{\text{th}}$ and the 12$^{\text{th}}$ months, and a sharp decline after the 12$^{\text{th}}$ month. The two groups fully converge after 36 months and they remain the same for the remaining 4 years, although point estimates remain consistently positive but small and not statistically significant.

[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]

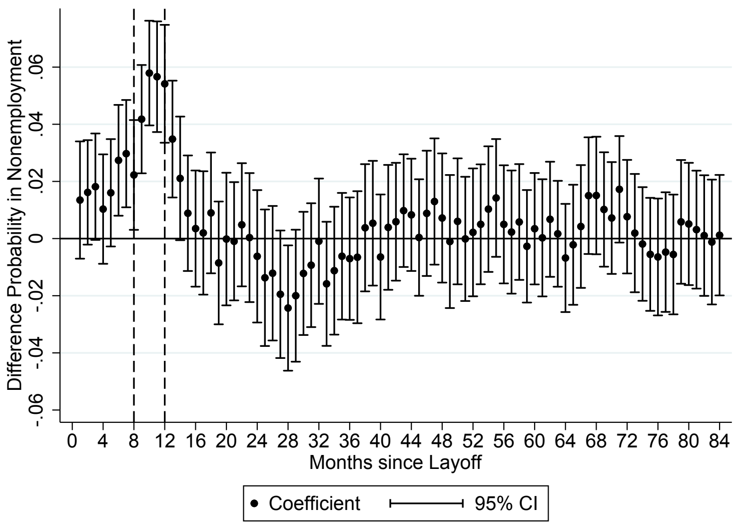
This picture provides suggestive evidence that the two groups converge in the long run. Additional data for the other years would allow us to better understand to what extent these findings can be generalized to the rest of the sample.

Table 1.E.1: 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 1.E.1: Employment pattern for workers fired in 2009 for 7 years



Note: Effect of 4 additional months of potential benefit duration on probability of being employed at t months after layoff. The worker is considered employed if she works at least one day during the corresponding month. Regressions includes a square polynomial in age with different slopes around the cutoff, controls for the worker and last firm characteristics, local labor market interacted with month fixed effects. 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.

1.F New job characteristics - regression

Table 1.F.1: 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%.

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 1.G.1 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 more frequent for

[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.

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 1.G.1, 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 1.G.2, show that recalled workers are not responsive to potential benefit duration and they show insignificant effects for all the variables of interest. Workers who are not recalled show responses very similar to the ones in the main equation. This shows that results are largely driven for workers facing *ex novo* searches in the labour market. This

[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.

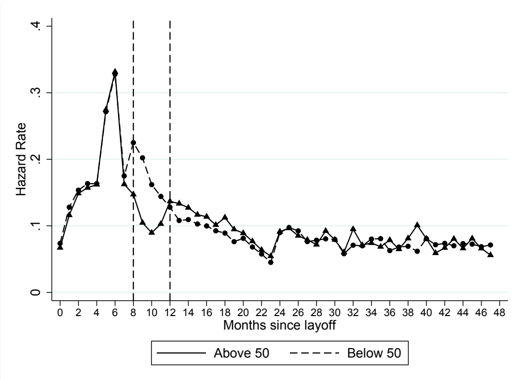
is consistent with the fact that a large share of recalls takes place in the first six months of the nonemployment spells: the share of workers who is recalled is close to 60% among those finding a job at 6 months in the spell whereas the share declines to 40% two months later and to 20 % at 12 months. Even after 4 years of nonemployment still about 10% of the workers are recalled by the same firm.

Table 1.G.1: Observables and probability of recall

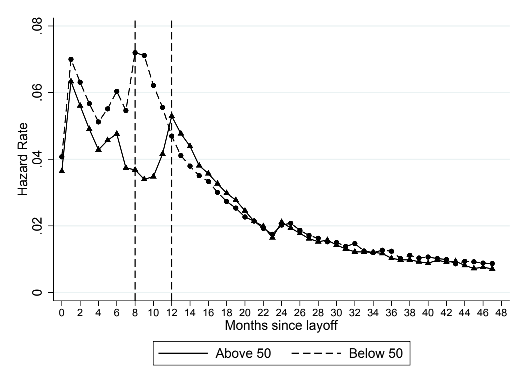
VARIABLES	(1) Overall	(2) Overall	(3) Permanent	(4) Temporary
Female	0.052***	0.060***	0.034***	0.064***
	(0.004)	(0.004)	(0.005)	(0.004)
Full Time	0.015**	-0.005	0.013***	-0.031***
	(0.006)	(0.004)	(0.005)	(0.005)
White Collar	-0.097***	-0.066***	-0.044***	-0.065***
	(0.008)	(0.004)	(0.004)	(0.008)
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 1.G.1: 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.

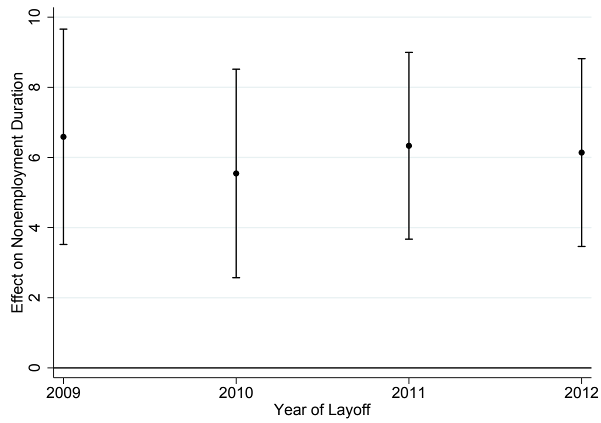
Table 1.G.2: Effects of longer PBD for workers recalled or changing firm

	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

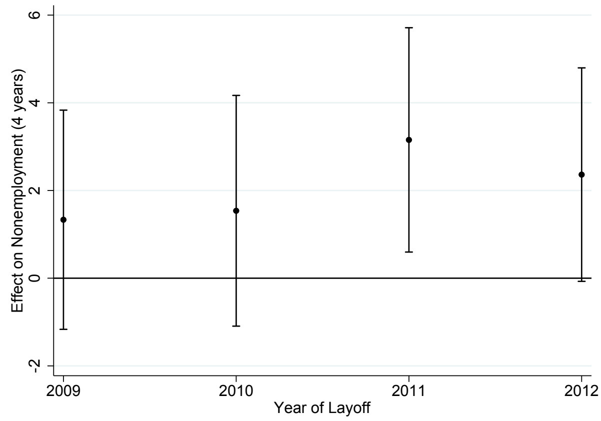
Note: Linear regression for the effects of unemployment benefits on main variables of interest. Regressions includes a square polynomial in age with different slopes around the cutoff, controls for the worker and last firm characteristics and local labour market interacted with month fixed effects. List 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%.

1.H Heterogenous effects: by year and sector

Figure 1.H.1: Effects on nonemployment in first spell and over 4 years by year of layoff



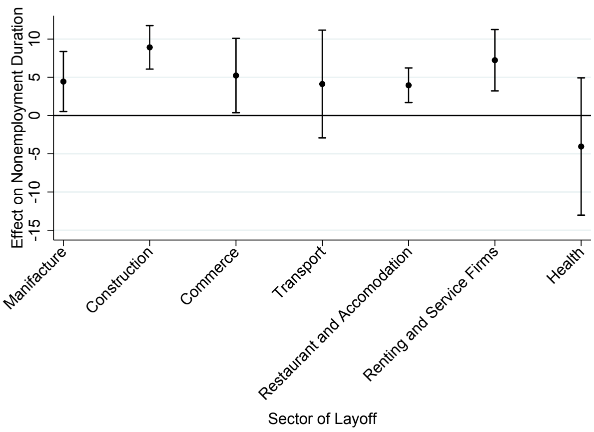
(a) Effect on Nonemployment



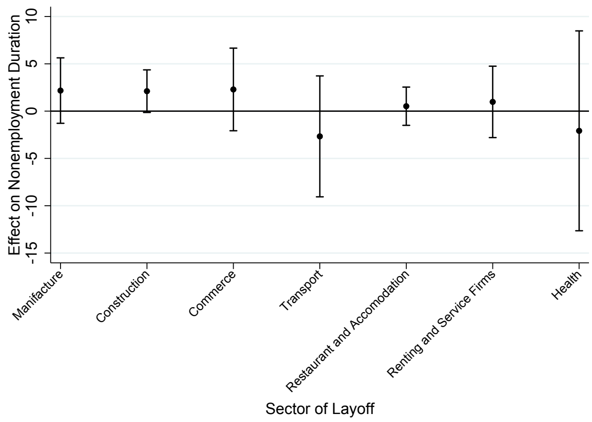
(b) Effect on Nonemployment (4 years)

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 1.H.2: Effects on nonemployment in first spell and over 4 years by broad NACE sector



(a) Effect on Nonemployment

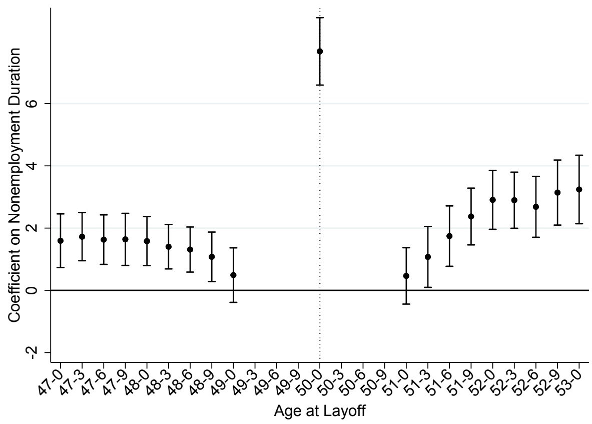


(b) Effect on Nonemployment (4 years)

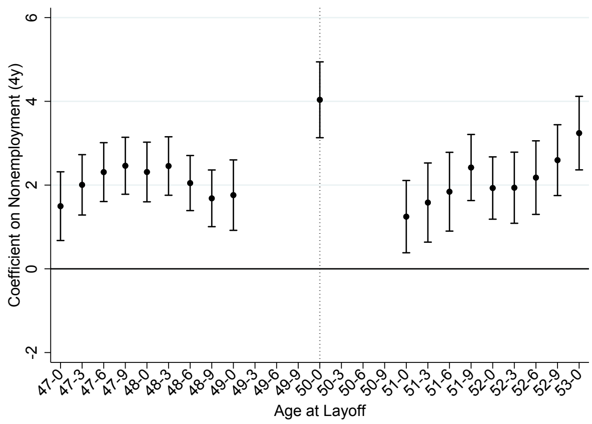
Note: Effect of 4 additional months of potential benefit duration on nonemployment in the first spell (a) and nonemployment over 4 years (b). Regressions includes a square polynomial in age with different slopes around the cutoff, controls for the worker and last firm characteristics, local labour market 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.

1.I Placebo with non parametric RDD

Figure 1.I.1: 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.

Chapter 2

Happy Birthday? Manipulation and Selection in Unemployment Insurance

Luca Citino
Bank of Italy and London School of Economics
Kilian Russ
Bonn Graduate School of Economics
Vincenzo Scrutinio
IZA, London School of Economics

Abstract

This paper documents strategic delays in the timing of layoffs around an age-at-layoff threshold entitling workers to a four months increase in potential unemployment insurance (UI) benefit duration in Italy. Manipulation is quantitatively important with over 10% of layoffs in the two months before workers' fiftieth birthday being delayed. While manipulation unambiguously increases public spending for UI, it may do so both because manipulators are individuals with high long-term nonemployment risk or because they respond to extra UI coverage by decreasing their job search intensity. We build on recently developed bunching estimators to disentangle the two. While in total affected workers lengthen their UI benefit receipt duration by 3.4 months, a survival analysis reveals that 79.6% of this increase is mechanical and due to higher coverage. Only the remaining 20.4% is explained by reductions in job search effort. Consistent with this, we find that even absent manipulation, manipulators face a significantly higher long-term nonemployment risk of 80% after eight months, compared to 60% for non-manipulators. Together our results document pervasive manipulation in UI and identify long-term nonemployment risk as an important motivation to engage in manipulation. Once they qualify, manipulators are only modestly responsive to the additional UI coverage: this mitigates concerns about anticipated moral hazard. These findings highlight the importance of studying the underlying motives for manipulation and might influence how manipulation is perceived. Manipulation is confined to the private sector and permanent contract work arrangements. It is most prevalent among female, white-collar, part-time workers at small firms suggesting that adjustment costs, bargaining power and proximity to superiors play a role for workers' ability to engage in manipulation. Overall, our results highlight the importance to take strategic behaviour into account when designing targeted UI schemes.

Keywords: unemployment benefit, selection, moral hazard.
J.E.L. codes: J65; H55.

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2.1 Introduction

The targeting of public policies on the basis of observable individual characteristics is ubiquitous in OECD countries. Governments tax individuals based on their marital status, provide welfare payments which depend on the number of children in the household, or tie disability insurance to particular medical conditions. The theoretical desirability for targeting based on immutable *tags* has long been recognized (Akerlof, 1978). In practice however, policy makers often rely on imperfect tags, which leave room for strategic manipulation and selection into benefit schemes.

How should we view such manipulation? Typically, the initial inclination is to regard manipulation solely as opportunistic behaviour. Undeserving individuals cheat their way to higher benefits and thrive at the expense of others. While manipulation undeniably increases public spending, this judgment lacks a more comprehensive understanding about the underlying *motivation* for manipulation. Perhaps, individuals who decide to manipulate value the additional benefits tremendously or they manipulate out of necessity. Manipulators might also be relatively less responsive to benefits once they qualify for them. The underlying rationale and subsequent changes in behaviour are important to better understand manipulation and might ultimately shape the way the phenomenon is perceived by policy makers and society at large.

While quantifying additional expenditures is relatively straightforward, providing a comprehensive analysis of the motivation for manipulation is considerably more challenging. Our paper makes progress on this important question by studying a context in which differentiated policies and manipulation are widespread, namely unemployment insurance (UI) (see Spinnewijn, 2019, for a survey, and Doornik et al., 2018, and Khoury, 2018, for recent evidence on manipulation). We study the Italian UI scheme which until 2015 featured a discontinuous jump in potential benefit duration (PBD) depending upon whether the worker was laid off before or after her fiftieth birthday.[1]

We start by providing clear graphical evidence of manipulation in the form of systematic delays in the exact timing of layoffs around the age-at-layoff threshold. Using bunching

[1]Similar policies have been in place in several OECD countries, e.g. Germany, Austria among others.

techniques, we estimate that 10% of all layoffs within two months before workers' fiftieth birthday are strategically delayed. Over the subsequent nonemployment spell affected workers collect an additional 2,239 Euros or 38,5% of total UI benefits on average.

While the above numbers are large, it is important to keep in mind that manipulation provides individuals with additional UI *coverage*. Even without a change in subsequent job search efforts, manipulators would collect additional UI benefits due to the extended coverage from month eight to twelve. More importantly the change in subsequent job search intensity lets us infer something about the underlying motivation for manipulation. To see this point, consider two extreme cases for why individuals might engage in manipulation. First, suppose manipulators are individuals who would have found a job exactly after eight months, but are now staying unemployed for four additional months before taking up their next job. In this case manipulation is motivated by an anticipated moral hazard response. The four additional months of benefits are paid only because individuals change their job search effort. Contrary, suppose manipulators are unemployed for at least twelve months with or without additional UI coverage. In this case, they would also collect four additional months of UI benefits. However, in the latter case, it is individuals' long-term nonemployment risk that drives selection into manipulation. The additional benefits are paid mechanically due to higher coverage. In reality manipulation is likely motivated by a combination of these forces but it is clear that distinguishing between these different motives is crucial for deciding how to view manipulation.

Our survival analysis reveals that manipulators are individuals with significantly higher long-term nonemployment risk. Even absent manipulation, manipulators would face an 80% chance of being still unemployed after eight months, which would lead them to exhaust the less generous UI scheme. We further document that approximately 80% of the increase in UI benefit receipt is mechanically due to higher coverage, while only 20% is explained by a reduction in job search efforts. To put the above numbers into perspective, non-manipulators – individuals who were laid off just before their fiftieth birthday – face a 20 p.p. lower eight months nonemployment survival risk of 60%. We also find no evidence that manipulators are more responsive to additional UI coverage than non-manipulators.

Together our results document pervasive manipulation in unemployment insurance and identify long-term nonemployment risk as an important motivation to engage in manipulation. Manipulators are only modestly responsive to the additional UI coverage once they qualify for longer benefits and this mitigates concerns about anticipated moral hazard. These findings highlight the importance of studying the underlying motives for manipulation and might influence how manipulation is perceived. Our analysis also implies that the type of manipulation we consider has only modest effects on economic efficiency, a conclusion that would not hold if anticipated moral hazard were a prime motive for manipulation. This in turn has implications for the design of optimal differentiated UI policies.

To shed light on the underlying collusion behaviour by which firms and workers agree to postpone the exact date of layoff, we provide evidence by comparing manipulators and non-manipulators based on observable characteristics. Some degree of manipulation is pervasive among all permanent contract workers in private sector firms, with the exception of large firms with more than fifty employees.[2] Manipulation is relatively more prevalent among female, part-time, white-collar workers at small firms. This suggests that lower adjustment costs, higher bargaining power of workers and closer proximity between workers and their supervisors may facilitate manipulation.

Our work relates to several strands of the literature. A large body of work studies the disincentives effect and the effect on post-reemployment outcomes, such as wages, of UI, exploiting similar policy variation, see e.g. Card et al. [2007a], Rosolia and Sestito [2012], Schmieder et al. [2012a], Landais [2015], Nekoei and Weber [2017], Johnston and Mas [2018] among others. Contrary to our setting, these papers rely on the *absence* of manipulation to identify the treatment effects of interest, whereas we study the effect of manipulation in a setting where it does occur. Furthermore, while most previous studies of UI focus on the distortion of job search efforts of the unemployed, we examine strategic behaviour at the point of layoff. Our work closely relates to two recent contributions by Doornik et al. [2018] and Khoury [2018] who exploit manipulation in UI systems around an eligibility and seniority threshold in Brazil and France, respectively. Doornik et al. [2018] provide evidence of strategic collusion between workers and firms who time

[2] We find no evidence of manipulation in public sector firms or among temporary contracts.

layoffs to coincide with workers' eligibility for UI benefits in Brazil. Khoury [2018] exploits a discontinuity in benefit levels for workers laid off for economic reasons and estimates an elasticity of employment spell duration with respect to UI benefits of 0.014. Due to the nature of their policy variation neither of these papers studies the selection patterns we analyse in our work. From a methodological perspective our work is most closely related to the work by Diamond and Persson [2017], who study manipulation in Swedish high-stakes exams. The construction of the manipulation region and of the counterfactual density relies on standard bunching techniques, such as Saez [2010], Chetty et al. [2011] and Kleven and Waseem [2013].

Although the contribution of the paper is empirical, we do relate to the literature on the theoretical desirability of *tagging* (Akerlof, 1978) and ordeals (Nichols and Zeckhauser, 1982). We show that the bargaining over the exact timing of layoffs between workers and firms serves as a screening mechanism for long-term unemployment risk. In recent work Michelacci and Ruffo [2015] argue for higher UI benefits for young workers by analysing the canonical Baily [1978]-Chetty [2006] trade-off from a life-cycle perspective. Age as an useful tag for redistribution has also been studied in the context of taxation by e.g. Weinzierl [2011] and Best and Kleven [2013].

The fact that we find substantial manipulation and positive selection on long-term unemployment risk also speaks to a recent literature studying the role of private information and adverse selection in unemployment insurance, see e.g. Hendren [2017] and Landais et al. [2017]. This literature studies the role of private information about *ex-ante* unemployment risk in shaping the market for UI. Our results indicate that individuals hold information about their expected duration of unemployment at the point of layoff. Understanding to what degree this information is held privately is beyond the scope of this paper.

The remainder of this paper is organized as follows: Section 2.2 introduces the institutional setting and describes the data; Section 2.3 describes our quantities of interest and presents our identification strategy; Section 2.4 goes into more detail in explaining how we implement the latter in practice; Sections 2.5 and 2.6 reports the results of our empirical analysis and robustness checks; Section 2.7 concludes.

2.2 Institutional Setting and Data

2.2.1 Institutional setting

In this paper we focus on *Ordinary Unemployment Benefits* (OUB).[3] This was the main UI scheme active in Italy from the eve of World War II till January 2013.[4]OUB covered all private non-farm and public sector employees who lost their job due to either end of their temporary contract, or an involuntary termination, or quit for just cause (e.g. unpaid wages or harassment). Other types of voluntary quits, together with self-employed and dependent self-employed were not eligible for the benefits.[5] Additional eligibility criteria concerned labour market experience: workers needed to have started their first job spell at least two years before the date of layoff, and to have worked for at least 52 weeks in the previous two years.

Crucially for our purposes, PBD was fully age-dependent and it was not related to other factors (e.g. work experience, gender, family composition, geographic area, etc.). Individuals who were laid off before their fiftieth birthday were entitled to 8 months of benefits (34.7 weeks) while workers laid off after their fiftieth birthday were entitled to 12 months of benefits (52 weeks). Such a *notch* in the PBD schedule generated economic incentives for workers to attempt to have their layoff date delayed, in order to obtain more generous benefits. Benefit level was based on the average monthly wage over the three months preceding the layoff, but the replacement rate was declining over the unemployment spell: 60% of the average wage for the first 6 months; 50% for the following 2 months and 40% for any remaining period. OUB did not involve any form of experience rating. As a consequence, longer PBD was not linked to higher contributions by the separating firm, but for a severance payment which was proportional to tenure (around one monthly wage for every year worked).

During the same time period two other main UI schemes were in place: the Reduced Unemployment Benefits (RUB) and the Mobility Indemnity (MI).[6] On the one hand,

[3] *Indennità di Disoccupazione Ordinaria a Requisiti Normali* in Italian.

[4] OUB was introduced through *Regio Decreto 14^{th}* in April 1939.

[5] For convenience, in the rest of the paper we will use the term "layoff" to indicate all job terminations that are eligible for claiming UI.

[6] Respectively *Indennità di Disoccupazione Ordinaria a Requisiti Ridotti* and *Indennità di*

RUB was directed to the same workers targeted by the OUB but who did not manage to meet minimum contribution requirements. The latter scheme only required 13 weeks (78 days) worked in the last year, instead of 52, but still required at least 2 years from the first contribution to the social security system. It involved a monetary transfer proportional to the days worked in the previous year (up to 180 days) and granted 35% of the average wage earned in the previous year for the first 120 days and 40% for the following 60 days.[7] This measure was substantially less generous than the one under study and, in addition, it was granted during the solar year following the unemployment period. These characteristics made it less attractive to workers, who would prefer the OUB if they met its requirements. On the other hand, MI was active until 2017 and was targeted to workers fired during mass layoffs or business reorganizations. This measure combined a long and generous income support with active labour market policies, to improve workers' occupational perspectives. During the period under study the potential duration of this scheme depended on the worker's age at layoff and the geographical area where she worked, with a maximum PBD of 48 months in southern regions and of 36 months in northern regions. The benefit amounted to 80% of the salary for the first 12 months (with a cap annually set by law) and 64% during the following months. This measure represents a particularly attractive alternative for individuals involved in mass layoffs and could lead to underrepresentation of these types of workers in our sample. What is more relevant for our purposes is that selection for this benefit is mostly beyond the control of the worker: indeed, the firm needed to be undergoing significant economic restructuring and have a minimum size, while workers needed to meet some tenure requirements. Due to these factors, the presence of this benefit might lead to an underrepresentation of some categories of workers in our sample, but selection in and out from this benefit is unlikely to be related to a choice of the worker.

2.2.2 Data

We use confidential administrative data from the Italian Social Security Institute (INPS) on the universe of UI claims in Italy between 2009 and 2012 and combine them with matched employer-employee records covering the universe of working careers in the

Mobilità.

[7]For additional information, please refer to Anastasia et al. [2009].

private sector. Information on UI claims comes from the SIP[8] database, which collects data on the universe of income support measures administered by INPS as a consequence of job separation. For every claim we observe the scheme type, its start date, duration and amount paid. We further observe information related to the job and the firm. This includes details about the type of the contract and a broad occupation category. The SIP database does not report the date of the first job after receiving unemployment benefits, which is crucial for our analysis. We retrieve this information from the matched employer-employee database (UNIEMENS). This provides information on workers' careers in the private sector together with detailed information on wages, type of contract and start date of the job.

In order to construct nonemployment duration after job loss, we count the number of weeks elapsing between the layoff date in the SIP and the first hiring date in UNIEMENS. Crucially, we require the latter to be subsequent to the date of end of UI. This allows us to exclude from the analysis short jobs which might be compatible with UI continuation. Given that we observe all individuals in the sample for a minimum of four years, we censor all nonemployment durations at four years. This allows us to have a common period of observation for all workers.

We restrict our attention to individuals who lost their job between February 2009 and December 2012, were between 46 and 54 years of age at the moment of layoff, and claimed OUB. Unfortunately, our data do not cover the years prior to 2009 and the introduction of a new UI scheme in January 2013 prevents us from including later years. We further restrict our attention to individuals who separate from an employer in the private sector after a permanent contract. This leads to the exclusion of workers from the public sector, regardless of the contract type, and of workers from the private sector with temporary contracts. For the former, the UNIEMENS data does not have information on public sector jobs so it would not be possible to have prior career information on public sector workers. Similarly, if they were to find a job in the same sector, it would not be possible to observe the beginning of their new job. For the latter, instead, we fail to detect manipulation and we decide to focus in the main results on the group most concerned by this behaviour. This is indicative of the fact that individuals holding

[8] *Sistema Informativo Percettori.*

temporary contracts are not able to time the start and duration of their contracts to obtain more generous benefits. We explicitly explore differences across different contract types and sectors in Section 2.5.4. After the exclusion of a few observations, missing key information, we are left with 249,581 separation episodes that lead to a UI claim.

Table 2.1 reports summary statistics for our main sample. The average worker spends about 30 weeks (6.9 months) receiving UI, but 90 weeks in nonemployment before finding a new job. As a consequence, they have a quite high probability of being unemployed after 8 months in the nonemployment spell, with half of the workers still looking for a job after 8 months in the spell. Due to exhaustion of the benefits, quite a few workers find jobs in the following 4 months but still 39% of the worker have to find a job after one year since the layoff. Workers are mostly male, on full time contracts, and employed in blue collars jobs. They have spent about 27.5 years in the labor market since their first job and almost 6 years in their last firm. In terms of geographic distribution, 46% of them are laid off in the South or in the Islands.[9] They earn about 70 euros per day which is equivalent to $70 \times 26 = 1820$ euros per month if working full time. This measure, extracted from UNIEMENS data, is quite consistent with the monthly wage reported by the SIP database, which reports an average monthly wage of 1,735 euros in the three months preceding the layoff. The separating firm is relatively old (14 years) and large (28.16 employees), but this is mostly driven by a few very large firms: indeed, more than 60% of workers come from firms with less than 15 employees while only 18% come from firms with more than 50 employees.

One could be concerned that our sample is composed of workers in the late stage of their career and that some of them might use unemployment benefits to transition towards retirement. This does not seem to be the case as only about 1,500 workers in our full sample claim a pension before the end of our observation window. For these workers, we define the nonemployment spell as the period between the end of the previous employment and the date in which they claim their pension. Finally, our data do not cover transitions towards self-employment, agricultural sector or public employment. This kind of transitions are unlikely for workers employed in the private

[9] This area encompasses the following regions: Abruzzo, Basilicata, Calabria, Molise, Puglia, Sardinia and Sicilia.

sectors at this late stage of their career and their exclusion should not substantially affect our results. We replicated the analysis for a subsample of individuals for whom information on the full contribution history is available and results are qualitatively similar.

2.3 Conceptual framework

2.3.1 The moral hazard cost of extended UI coverage

Manipulation provides individuals with additional UI coverage. As in any insurance context the increase in coverage might cause individuals to change their behaviour by reducing the incentive to avoid adverse states of the world. This change in behaviour, in our context a reduction in job search intensity, constitutes a classical moral hazard response. From an efficiency perspective it is crucial to understand how much of the increase in total insurance payments is driven by changes in behaviour and how much is mechanically due to higher coverage.

Quantifying the relative importance of these effects also leads to potentially different positive views about manipulation and the motivation behind it, which in turn might shape how the phenomenon is perceived both by policy makers and society at large. Consider two extreme cases for why individuals might engage in manipulation in our context. First, suppose manipulators are individuals all of whom would have found a job exactly after eight months, but are now staying unemployed for four additional months before taking up their next job. In this case manipulation would be motivated by an anticipated moral hazard response. The four additional months of benefits are paid only because individuals change their job search effort. Contrary, suppose manipulators are unemployed for at least twelve months with or without additional UI coverage in which case they would also collect four additional months of UI benefits. However, in the latter case, it is individuals' long-term unemployment risk that drives selection into manipulation. The additional benefits are paid mechanically due to higher coverage and do not distort individuals' job search intensities. Of course, in reality manipulation is motivated by a combination of these two forces and we view distinguishing between them as one of this paper's main contributions.

In the following we formalize the above line of reasoning and introduce the relevant quantities of interest. It is constructive to decompose the increase in insurance payments, i.e. UI benefit receipt, under the twelve and eight months scheme as follows:

$$\begin{aligned}\Delta B =& B^{12} - B^{8} = \int_0^{12} S_t^{12} dt - \int_0^{8} S_t^{8} = \\ =& \underbrace{\int_0^{12} (S_t^{12} - S_t^{8}) dt}_{\text{behavioural response } (\Delta B^{MH})} + \underbrace{\int_8^{12} S_t^{8} dt}_{\text{mechanical effect } (\Delta B^{ME})}\end{aligned} \tag{2.1}$$

where B and S denote the average benefit receipt and the survival rate each under the twelve and eight months PBD scheme, respectively. The behavioural moral hazard response, ΔB^{MH}, captures the part of the benefit receipt increase that is due to the outward shift of the survival curve, i.e. the behavioural response. The mechanical effect, ΔB^{ME}, corresponds to the remaining increase in benefit receipt that occurs even absent any behavioural response. Figure 2.1 illustrates decomposition (2.1) graphically by plotting hypothetical manipulators' nonemployment survival under the eight and twelve months PBD scheme. The total increase in benefit receipt corresponds to the sum of the behavioural/moral hazard effect (dark grey area) and mechanical effect (light grey area). As a preview of the results, Table 2.2 reports the decomposition of the total effect on benefit duration for both manipulators and non manipulators in our sample.

While the above quantities capture how manipulators respond to extended UI coverage, they are difficult to compare across groups of individuals, such as manipulators and non-manipulators, or relate to empirical evidence from other studies. To facilitate such cross group comparisons and summarize the extent of moral hazard in one statistic we follow Schmieder and von Wachter [2017] who suggest normalizing the behavioural response by the mechanical effect. Concretely, we calculate the behavioural and mechanical *cost* by multiplying both quantities with their respective unit cost to the government and take their ratio:

$$\frac{BC}{MC} = \frac{b \cdot \Delta B^{MH}}{b \cdot \Delta B^{ME}} \tag{2.2}$$

where b denotes the statutory benefit replacement rate. The BC/MC ratio measures by how many additional euros benefit receipt increases for each euro of mechanical increase. The ratio follows the standard bucket leakage interpretation: if the government wanted to provide one additional dollar of UI transfer, it would have to pay a cost that exceeds one dollar, precisely because there is a behavioural response.

The analysis thus far focused on additional benefit payments and abstracted from the second source of cost to the government: the loss in tax revenues due to longer nonemployment durations. Contrary to the analysis of benefit durations, longer nonemployment durations do not entail a mechanical effect and are solely the result of a behavioural response. Formally, we have:

$$\Delta N = N^{12} - N^{8} = \int_0^\infty S_t^{12} dt - \int_0^\infty S_t^{8} dt = \underbrace{\int_0^\infty (S_t^{12} - S_t^{8}) dt}_{\text{behavioral response } (\Delta N^{MH})}$$

where, as above, N and S denote the average nonemployment duration and the survival rate each under the twelve and eight months PBD scheme, respectively. Since all of the increase in nonemployment duration constitutes a moral hazard response, we add the resulting cost to the behavioural cost and adjust formula 2.2 as follows:

$$\frac{BC}{MC}^{\tau} = \frac{b \cdot \Delta B^{MH} + \tau \cdot \Delta N^{MH}}{b \cdot ME} \tag{2.3}$$

Because there is some disagreement in the literature about what the appropriate tax rate τ in this context is, Table 2.3 reports BC/MC ratios for several tax rates.[10]

2.3.2 Identification strategy

This section provides a self-contained sketch of our estimation strategy and explains the sources of variation in the data that are used to pin down parameters of interest. The main idea is to exploit the local nature of manipulation by extrapolating outcomes from regions that are unaffected by manipulation to learn about what would have

[10] Early studies used a 3% UI tax, however, recent work argues for higher tax wedges (Lawson [2017]).

happened in the manipulation region in the absence of it. We first assess the range of the manipulation region with standard bunching techniques. We then fit polynomials to the unmanipulated part of the data and interpolate to construct a counterfactual layoff frequency and recover the number and share of manipulators. Similarly, we construct counterfactuals of outcomes that are not directly manipulated, such as nonemployment survival probabilities, to learn whether these outcomes respond to manipulation. Intuitively, any unusual change in these outcomes near the cutoff together with an estimate of how many manipulators are causing it, let us recover manipulators' responses. We employ similar reasoning to characterize manipulators based on observable characteristics. Our approach is closely related to that of Diamond and Persson [2017]. In the remainder of this section, we lay out our approach in more detail.

Quantifying manipulation: Consider a hypothetical manipulated layoff density as in Figure 2.2a. Absent any manipulation we would expect the frequency of layoffs to be smooth in the neighbourhood of the cutoff. Manipulation instead causes a sharp drop in the number or layoffs right before and a spike right after age fifty. As in standard bunching techniques, we recover the counterfactual frequency of layoffs by fitting a polynomial to the unmanipulated parts of the data (on the left and right of the cutoff) and interpolate inwards. We determine the lower bound of the missing region by visual inspection, and then iteratively try different upper bounds of the excess region until we are able to balance the missing and excess mass. The difference between the observed frequency and the fitted counterfactual lets us recover missing and excess shares, as well as the number of manipulators in each bin of the missing and excess region.[11]

Effects of manipulation: Equipped with a measure of how many manipulators there are, we then study outcomes which are not directly manipulated but potentially affected by it. Figure 2.2b illustrates the idea for one of our outcomes of interest: nonemployment survival rates. Manipulation provides workers with additional UI coverage from month eight to twelve. Thus, it is likely that nonemployment survival rates respond to the increase in coverage. Consider a hypothetical statistical relationship

[11] This estimation strategy assumes that manipulation takes the form of a pure re-timing of layoffs that would occur in any case. One concern is that the increase in PBD at the age threshold leads to extensive margin effects, see Jäger et al. [2018]. We provide evidence that this is not the case in our setting in Section 2.6.2.

between nonemployment survival and age at layoff, as in Figure 2.2b. In order to estimate how manipulators' survival rate responds, we take the difference between two quantities: manipulators' actual survival probability and manipulators' counterfactual survival probability had they not been able to manipulate. As illustrated in Figure 2.2b, we obtain these quantities by separately studying the missing and excess region. First, we fit a flexible counterfactual on the right side of the threshold and estimate the difference between the observed and predicted survival rates to assess manipulators' actual survival probability. Intuitively, survival rates in the excess region are higher than predicted by the un-manipulated region to the right only due to manipulation. The extent to which observed and predicted nonemployment survival rates differ, together with an estimate of how many manipulators are causing this difference, let us recover manipulators' actual nonemployment survival probability. We use analogous arguments to back out manipulators' counterfactual nonemployment survival probability on the left side of the threshold.

Selection into manipulation: The procedure illustrated in Figure 2.2b also lets us study selection into manipulation by comparing manipulators' counterfactual outcomes to that of non-manipulators, individuals in the missing region who did not manipulate. Figure 2.2b highlights this comparison and would suggest that even absent manipulation, manipulators would have had a higher nonemployment survival rate than non-manipulators due to the drop in the outcome variable to the left of the cutoff. This is indeed what we show in Section 2.5. In light of the selection patterns we document, it is worth bearing in mind that we are estimating the effect of manipulation on individuals who endogenously decide to engage in manipulation, akin to a local average treatment effect.

2.4 Regression Framework

In this section we present the details of how we operationalize our identification strategy in a regression framework.

2.4.1 Estimating the number of manipulators

In order to quantify the amount of manipulation we follow standard bunching techniques (Saez, 2010, Chetty et al., 2011, Kleven and Waseem, 2013). At every age, we estimate a counterfactual layoff frequency by fitting a second order polynomial to the observed frequency, but excluding data from the manipulation region. Concretely, we group all layoffs into two week bins based on the workers' age at layoff and estimate the following specification:

$$c_j = \alpha + \sum_{p=0}^{P} \beta_p \cdot a_j^p + \sum_{k=z_L}^{z_U} \gamma_k \cdot \mathbb{I}[a_j = k] + \nu_j \tag{2.4}$$

where c_j denotes the absolute frequency of layoffs in headcounts in bin j, a_j is the mid-point age in bin j, P denotes the order of the polynomial. Coefficients γ_s control flexibly (bin-by-bin) for differences between the observed data and the counterfactual frequency in the manipulation region $[z_L, z_U]$.[12] The whole counterfactual layoff frequency can be recovered from the fitted values of equation 2.4 omitting the contributions of the missing and excess region dummies, i.e. the counterfactual number of individuals in bin j is given by $\hat{c}_j = \sum_{p=0}^{P} \hat{\beta}_p \cdot a_j^p$.

Crucial to our estimation procedure is a definition of the manipulation region $[z_L, z_U]$. Here we follow the procedure employed in Kleven and Waseem [2013]. We first rely on visual inspection to determine z_L. We set this to be six weeks away from the age fifty cutoff (three bins). Subsequently, we try different specifications that increase z_U by little margins (one bin at the time), until the difference between the missing mass and the excess mass is sufficiently small. If the counterfactual density could be recovered without error by a polynomial, we would stop when $\sum_{k=z_U}^{z_L} \gamma_k \cdot \mathbb{I}[a_j = k] = 0$. In practice we stop when this quantity falls below a critical threshold. This procedure leaves us with a manipulation region of six weeks to the left and four weeks to the right of the cutoff.

The observed layoff frequency and the estimated counterfactual are enough to compute

[12] The inclusion of these dummies is equivalent to estimating the polynomial after excluding observations in the corresponding bins.

the headcount for four groups inside the manipulation region: (1) manipulators in the missing region (2) manipulators in the excess region, (3) non-manipulators in the missing region and (4) non-manipulators in the excess region.[13] These quantities can be computed bin by bin or over the entire region. Expressions are as follows:

$$
\begin{aligned}
N^{\text{missing}}_{\text{mani},k} &= \gamma_k \\
N^{\text{excess}}_{\text{mani},k} &= \gamma_k \\
N^{\text{missing}}_{\text{non mani},k} &= c_k \\
N^{\text{excess}}_{\text{non mani},k} &= c_k - \gamma_k \\
N^{\text{missing}}_{\text{mani}} &= \sum_{k \in \text{missing}} \gamma_k \\
N^{\text{excess}}_{\text{mani}} &= \sum_{k \in \text{excess}} \gamma_k \\
N^{\text{missing}}_{\text{non mani}} &= \sum_{k \in \text{missing}} c_k \\
N^{\text{excess}}_{\text{non mani}} &= \sum_{k \in \text{excess}} c_k - \gamma_k
\end{aligned}
$$

where it must hold that $N^{\text{missing}}_{\text{mani}} = N^{\text{excess}}_{\text{mani}}$, that is manipulators who move from the left-hand side of the manipulation region are to be found in the right-hand side of it. Given the headcount, we compute the relevant shares bin by bin that are useful for the calculation of effects of manipulation in the next section.

$$
\begin{aligned}
s_{\text{missing},k} &= \frac{N^{\text{missing}}_{\text{mani},k}}{N^{\text{missing}}_{\text{mani},k} + N^{\text{missing}}_{\text{non mani},k}} \\
s_{\text{excess},k} &= \frac{N^{\text{excess}}_{\text{mani},k}}{N^{\text{excess}}_{\text{mani},k} + N^{\text{excess}}_{\text{non mani},k}}
\end{aligned}
$$

With all these ingredients, we are now ready to move to the core of our empirical

[13]Here we slightly abuse notation as the group of non-manipulators to the right of the cutoff is composed of individuals who would not have manipulated even under the left side of the cutoff conditions and individuals who would have manipulated if necessary. The latter group did not have any need to actually manipulate as they were fired after their fiftieth birthday at baseline.

analysis.

2.4.2 Estimating the effects of manipulation on manipulators

In the previous section we constructed the number of manipulators and the share they represent in the frequency of layoffs by age bin. We now move to the estimation of the effect of manipulation on manipulators. As outlined in Section 2.3.2, we estimate manipulators' duration response as the difference between manipulators' actual and counterfactual duration. In order to compute both these quantities, we relate differences in observed and predicted durations in the missing and excess region to the missing and excess share of manipulators, respectively. This procedure is readily applicable to all our outcomes of interest. In particular, it lets us study survival probabilities in nonemployment, which allow us to separate the mechanical component of the manipulation cost increase from the behavioural one.

As a first step we run the following regression on individual-level data:

$$\begin{aligned} y_i = \alpha + \sum_{p=1}^{P} \beta_p^{\leq 50} \cdot a_i^p \cdot \mathbb{I}[a_i \leq 50] + \sum_{p=0}^{P} \beta_p^{>50} \cdot a_i^p \cdot \mathbb{I}[a_i > 50] \\ + \sum_{k=z_U}^{z_L} \delta_k \cdot \mathbb{I}[a_i = k] + \xi_i, \end{aligned} \tag{2.5}$$

where y_j the outcome of interest, e.g. weeks of benefit receipt duration or probability of still being nonemployed m months after the layoff, $\beta_p^{\leq 50}$ and $\beta_p^{>50}$ are coefficients of two P^{th} degree polynomials in age, that are constructed based on information from the left-hand side and right-hand side respectively. This specification allows for a treatment effect of longer PBD on duration outcomes, i.e. $\beta_0^{>50}$. We refer to the latter as the "Donut-RD". Under some assumptions this coefficient captures the average treatment effect of four more months of PBD for the average individual in the population, as shown in Barreca et al. [2011].[14] We will use this to benchmark our results for manipulators.[15]

Thanks to the inclusion of $\mathbb{I}[a_i = k]$ indicator variables, the counterfactual polynomial

[14]See Scrutinio [2018] for an application to Italian unemployment benefits.

[15]Intuitively, this coefficient recovers the difference between the two grey dots in Figure 2.2b.

is estimated as if we were excluding observations from the manipulation region $[z_L, z_U]$ (similarly to equation 2.4). The coefficients δ_k capture the difference in average duration between the observed data and the estimated counterfactual in manipulation region. The central idea of our estimation is the re-scaling of these estimated differences by the respective share of manipulators responsible for it. Therefore, for each k in the missing region, we calculate

$$\Delta\bar{Y}_k^{\text{missing}} \equiv \bar{Y}_{\text{non mani},k}^{\text{missing}} - \bar{Y}_{\text{mani},k}^{\text{missing}} = \frac{\delta_k}{s_k^{\text{missing}}}$$

which gives us the difference in durations between manipulators and non-manipulators, in bin k in the missing region, in a world without manipulation. Note that the average duration of non-manipulators in bin k is an observable quantity and given by

$$\bar{Y}_{\text{non mani},k}^{\text{missing}} = \frac{y_k}{c_k},$$

which allows us to recover manipulators' counterfactual duration as

$$\bar{Y}_{\text{mani},k}^{\text{missing}} = \bar{Y}_{\text{non mani},k}^{\text{missing}} - \Delta\bar{Y}_k^{\text{missing}}.$$

The average duration over the *entire* manipulation region is

$$\bar{Y}_{\text{mani}}^{\text{missing}} = \frac{1}{N_{\text{mani}}^{\text{missing}}} \sum_k \gamma_k \cdot \bar{Y}_{\text{mani},k}^{\text{missing}},$$

where the γ_k are the same ones as in Section 2.4.1. Following an analogous argument on the right-hand side, we first re-scale the regression coefficient for bin k to obtain

$$\Delta\bar{Y}_k^{\text{excess}} \equiv \bar{Y}_{\text{mani},k}^{\text{excess}} - \bar{Y}_{\text{non mani},k}^{\text{excess}} = \frac{\delta_k}{s_k^{\text{excess}}}.$$

Now notice that the observable average duration in bin k in the excess region is given by

$$\bar{Y}^{\text{excess}}_{\text{observed},k} = \frac{y_k}{c_k} = \frac{\gamma_k \cdot \bar{Y}^{\text{excess}}_{\text{mani},k} + (c_k - \gamma_k) \cdot \bar{Y}^{\text{excess}}_{\text{non mani},k}}{c_k}$$

Therefore, combining the two expressions above and rearranging terms gives us an estimate of manipulators' actual duration in the form of

$$\bar{Y}^{\text{excess}}_{\text{mani},k} = \bar{Y}^{\text{excess}}_{\text{observed},k} + (1 - s^{\text{excess}}_k) \cdot \Delta\bar{Y}^{\text{excess}}_k,$$

for bin k. As above, manipulators' actual average duration over the *entire* excess region is given by

$$\bar{Y}^{\text{excess}}_{\text{mani}} = \frac{1}{N^{\text{excess}}_{\text{mani}}} \cdot \sum_k \gamma_k \cdot \bar{Y}^{\text{excess}}_{\text{mani},k},$$

which lets us define manipulators' response (or treatment effect) as

$$Y^{TE}_{\text{mani}} \equiv \bar{Y}^{\text{excess}}_{\text{mani}} - \bar{Y}^{\text{missing}}_{\text{mani}}.$$

While this method is general and covers any type of outcome, it is instructive to show how it simplifies when the left-hand-side variable is a survival probability. The survival probability for a given group g in a generic month m is obtained by dividing two headcounts: the number of individuals belonging to g who are still nonemployed in m and the original number of individuals in group g. The latter is already known from the procedure described in subsection 2.4.1. The former is obtained by applying the same estimating equation to the count of people still nonemployed in month m. For every month $m = 1, 2, 3, \ldots$ our estimating equation becomes:[16]

[16]Notice that for $m = 0$ equation 2.6 is identical to 2.4.

$$
\begin{aligned}
c_j^m = \; & \alpha + \sum_{p=1}^{P} \beta_p^{m,\leq 50} \cdot a_j^p \cdot \mathbb{I}[a_j \leq 50] + \sum_{p=0}^{P} \beta_p^{m,>50} \cdot a_j^p \cdot \mathbb{I}[a_j > 50] \\
& + \sum_{k=z_L}^{z_U} \gamma_k^m \cdot \mathbb{I}[a_j = k] + \xi_j^m,
\end{aligned}
\tag{2.6}
$$

where c_j^m is the headcount of individuals still in nonemployment after month m who were laid off in age bin j. Similarly to section 2.4.1, the β_p^m are the coefficients of a P^{th} order polynomial in a_j, where a_j is the mid-point of age bins. The γ^m coefficients recover the difference between the observed and counterfactual headcount and thus the number of manipulators who are still nonemployed after month m in the missing and excess region. For each month m, we compute the number of individuals of each group who are still nonemployed after month m. Formally, we define

$$
\begin{aligned}
N_{\text{mani}}^{\text{missing},m} &= \sum_{s \in \text{missing}} \gamma_s^m \\
N_{\text{mani}}^{\text{excess},m} &= \sum_{s \in \text{excess}} \gamma_s^m \\
N_{\text{non mani}}^{\text{missing},m} &= \sum_{s \in \text{missing}} c_s^m \\
N_{\text{non mani}}^{\text{excess},m} &= \sum_{s \in \text{excess}} c_s^m - \gamma_s^m,
\end{aligned}
$$

as the headcounts for the four groups in each month after layoff.[17] By re-scaling each N value by the same quantities' value in month 0, we obtain an estimate of the probability for staying unemployed for at least m months after layoff. We define

[17] In theory, all Ns must be weakly decreasing in m. However, we do not restrict our estimation procedure and might thus violate this argument due to estimation error. Reassuringly, in practice these instances are rare.

$$
\begin{aligned}
P_{\text{mani}}^{\text{missing},m} &\equiv N_{\text{mani}}^{\text{missing},m} / N_{\text{mani}}^{\text{missing},0} \\
P_{\text{mani}}^{\text{excess},m} &\equiv N_{\text{mani}}^{\text{excess},m} / N_{\text{mani}}^{\text{excess},0} \\
P_{\text{non mani}}^{\text{missing},m} &\equiv N_{\text{non mani}}^{\text{missing},m} / N_{\text{non mani}}^{\text{missing},0} \\
P_{\text{non mani}}^{\text{excess},m} &\equiv N_{\text{non mani}}^{\text{excess},m} / N_{\text{non mani}}^{\text{excess},0},
\end{aligned}
$$

as the survival probability in nonemployment after month m of the respective group. We now turn to our estimation results.

2.5 Results

In this section we examine the main findings. We start by presenting graphical evidence of manipulation in the form of strategic delays in the timing of layoffs around the fiftieth birthday threshold. After quantifying the magnitude of manipulation, we show how manipulators decrease their subsequent job search effort, which is reflected in outward shifts of their nonemployment survival curves and corresponding increases in their average nonemployment durations. We compare the increase in fiscal costs that originates from changes in search intensity to the mechanical increase in cost that would have arisen even absent behavioural changes. As outlined in Section 2.3, this allows us to retrieve the effective moral hazard cost of providing extended UI benefit to manipulators. As a final step we compare non-manipulators' survival curve to that of manipulators in a world where manipulation is not possible. This permits us to assess the degree of selection into manipulation on the basis of underlying long-term unemployment risk.

2.5.1 Evidence of manipulation

To provide graphical evidence of manipulation, Figure 2.3 plots the relative frequency of layoffs against workers' age at layoff. Figure 2.3b covers the entire age range from 26 to 64 years of age, while Figure 2.3a zooms into a narrower, four year window around the age-fifty threshold.[18] Both figures show a clear drop in the frequency of layoffs just before, and a pronounced spike after, the age-fifty threshold.

Following the procedure in Section 2.4.1, we find the manipulation region to consist of all bins from six weeks before (missing region), up to four weeks after the threshold (excess region). We estimate that 13.3% of all layoffs in the missing region or around 10% in the two months prior to the fiftieth birthday threshold are strategically delayed.[19] This amounts to a total of around 490 manipulated layoff dates which corresponds to 17% of all layoffs in the two weeks smaller excess region.[20]

We consider the graphical evidence presented until here as this papers' first main contribution. It documents that incentives generated by the UI system can influence the timing dimension of layoffs and thereby the length of an employment spell. Complementing previous work on the extensive margin response of job separations, we focus on the timing dimension of the layoff decision.[21] Having established sizable manipulation, we now turn to the estimation of its effect on manipulators' subsequent job search efforts.

2.5.2 Survival responses of manipulators

Successful manipulation provides workers with four months of additional potential UI coverage after the eighth month of nonemployment. In this section we make use of the methodology presented in Section 2.3 to study the effects of manipulation on manipulators' subsequent survival probabilities into nonemployment. As we will describe in more detail in section 2.5.3, this exercise is not only interesting from a positive but also relevant from a normative perspective. Intuitively, it's crucial to

[18]By plotting the layoff frequency over the entire age range in Figure 2.3b, we already rule out potential concerns that our findings are caused by other mechanisms like (round-) birthday effects or retirements spillovers. All our estimates for the counterfactual density and counterfactual outcomes will be based on the narrower (46-54) window. Further identification checks are presented in Section 2.6

[19]The counterfactual relationship appears almost perfectly linear and is very robust to the choice of the order of the polynomial. The determination of the manipulation region closely follows the procedure in Kleven and Waseem [2013] and is explained in more detail in Section 2.4.

[20]While the total number of manipulated layoffs might appear small, it is worth bearing in mind that this number is uninformative about the size of the behavioural response. We are currently working on estimating the implied elasticity of the employment spell duration w.r.t. potential benefit duration.

[21]Jäger et al. [2018] and Doornik et al. [2018] both study the extensive margin response of job separations to UI benefits.

understand *when* manipulators respond, as to distinguish between relatively expensive moral hazard responses during months of benefit receipt from those that happen after benefit exhaustion.

We begin by plotting these outcomes against workers' age at layoff in Figure 2.4. The observed pattern in the raw data fits with the model of manipulation we laid out in Section 2.3 and constitutes clear non-parametric evidence that nonemployment survival rates respond to manipulation. We now use methods detailed in Section 2.4.2 to trace out manipulators' and non-manipulators survival curves.

Figure 2.5a shows the estimated nonemployment survival curve of manipulators under the eight and twelve months PBD scheme. Figure 2.5b reports the difference between the two curves at any point, with associated 95% confidence bands.[22] The difference between the two curves reveals the effect of manipulation along manipulators' survival curve. It shows virtually no difference in survival probabilities in the first six to seven months, after which the two curves start diverging. The shift in manipulators' survival curve is substantial with their nonemployment probability after twelve months increasing from 0.55 under the eight months scheme to 0.76 under the twelve months scheme. Perhaps unsurprisingly, the behavioural response is concentrated in the months eight to twelve and coincides with the time of extended UI coverage. However, as pointed out, there is very little evidence of moral hazard in the first eight months of nonemployment. It is noteworthy that manipulation seems to have a very long-lasting effect on individuals' job finding probabilities. The two survival curves in Figure 2.5a show a substantial gap even as far out as thirty-two months after layoff. These shifts in the survival curves for manipulators naturally translate into changes in the *average* benefit and nonemployment durations. Thanks to the methodology in Section 2.4.2 we compute these to be respectively 14.53 weeks for benefit duration and 13.30 weeks for nonemployment duration. We now have all the ingredients that are needed to compute the relative contribution of the mechanical and behavioural effects in determining increases in benefit collection, as outlined in Section 2.3.1

[22] Confidence intervals are obtained by stratified bootstrap, using age-bins as strata.

2.5.3 Selection on risk and moral hazard cost

In this subsection we look at the degree to which manipulators are motivated by their long-term nonemployment risk, compared to an anticipated moral hazard response. The relevant methodology is presented in Section 2.3.1. Table 2.2, panel (a) provides a decomposition of the total increase in weeks on benefits for manipulators into a mechanical and a behavioural effect. In panel (b) we repeat the same exercise for the average individual in the population, using information from the "Donut RD" coefficient, introduced in Section 2.4.2. [23] Two prominent results emerge from this table. First, most of the increase in benefit payments for manipulators stems from a mechanical effect. The latter amounts to 11.57 weeks, which corresponds to 79.6% of the total increase in benefit receipt. Given that the total increase in weeks on benefits equals 14.53, it follows that the remaining behavioural effect amounts to $14.53 - 11.57 = 2.96$ weeks. Second, the relative size of mechanical and behavioural effects for manipulators is remarkably similar to that for the average individual in the population. This implies that manipulators are not adversely selected on their moral hazard response, compared to the average individual.

In order to facilitate cross-group comparisons even further, we follow Schmieder et al. [2012a] and calculate BC/MC ratios, as in equation 2.3. We use the statutory replacement rate of 0.4 for the months eight to twelve and show sensitivity of the results to various values of the UI tax rate.[24] Results for both manipulators and the average individual are reported in Table 2.3. In column 1 we see no difference in the BC/MC ratio across the two groups. This is in line with the result from Table 2.2, and is due to the fact that we are ignoring lost tax revenue in the months after the twelfth. A BC/MC ratio of 0.26 implies that, for one additional dollar of UI transfer (to either a manipulator or average individual), the government would have to spend an additional 26 cents due to the behavioural response that occurs in months 0-12. These numbers become bigger as

[23] As highlighted in Section 2.3.1, the mechanical effect is the integral from month 8 to 12 of the estimated survival curve, under the 8 month PBD scheme. The behavioural effect is the integral from month 0 to 12 of the difference between the estimated survival curves, under the 12 month and 8 month scheme.

[24] There is some disagreement in the literature on what the appropriate tax rate in this context is. Early studies have used a 3% UI tax. However, recent work argues for higher tax wedges (Lawson [2017]).

we increase the tax rates, since we take into account the fact that the government is losing revenues due to longer nonemployment durations. While the two numbers start diverging somewhat due to longer durations after month twelve for manipulators, the figures are still remarkably similar across the two groups. All in all, these results provide evidence that manipulators are not substantially more responsive to the additional UI coverage than the average individual, and thus mitigates concerns about selection on anticipated moral hazard.

The reason why manipulators see large increases in UI benefit receipt duration, although they are not more responsive, is due to selection on long-term nonemployment risk. Figure 2.6a illustrates this by plotting survival rates for manipulators and non-manipulators under the eight month scheme. Even with shorter PBD, the probability of exhausting benefits without finding a new job is 20 p.p. higher for manipulators. The large exhaustion risk makes most of the increase in benefit duration mechanical and thus lowers the BC/MC ratio, *ceteris paribus*.

2.5.4 Selection on observables

Until now we have quantified manipulation and studied its consequences, but we have abstracted from understanding how it occurs. In this section we present a characterization of the manipulators along observable characteristics, in order to provide some suggestive evidence on the economic mechanisms that generate it. In Figure 2.7 we start by visually inspecting the age distribution of layoffs for different types of contracts (permanent and temporary) and sectors (private and public). Workers in the public sector, either with permanent or temporary contracts, show little ability or interest to delay their layoff and the density of layoff does not exhibit any discontinuous pattern for either of these groups. The density for workers laid off from permanent contracts in the public sector also shows substantial variance, due to a smaller number of individuals. Once we move to the private sector, we can observe that workers on permanent contracts are able to manipulate their date of layoff, while the same is not true for workers on temporary contracts. This is consistent with temporary workers having little ability choose a start date for their contracts that positions them on the right-hand-side of the threshold, once laid off. It is also consistent with lower bargaining power with the employer, due to e.g. shorter tenure.[25]

In what follows we focus on the subset of workers who claimed UI after losing a permanent job in the private sector, which was also our sample of interest in the main analysis. We plot age-at-layoff densities for different subgroups in Figure 2.8. We notice that manipulation happens in all subgroups, albeit the size may differ. For example, we notice that the size of the jump at the threshold for females is twice as big as the one we see among males. Similarly, white collars and part-time workers have a higher probability of manipulating compared to blue collars and full-time workers, respectively. The level of the previous wage does not seem to play an important role, as workers show similar patterns of layoff, regardless of whether they earn above or below the median wage. Geographical location has a negligible role at best, since there are small differences in the size of the excess mass in different Italian macro regions. On the contrary, firm size is a key driver of manipulation. Workers in firms with less than 15 employees and between 15 and 50 employees show a strong ability to delay their layoff. This does not happen in larger firms, where the density of layoff does not show any discontinuity. In addition, workers in smaller firms seem to be able to delay their layoff for more weeks, whereas manipulation is concentrated in the month prior to layoff for workers in medium sized firms. We can only speculate as to the reasons behind the firm-size differential in manipulation: the effect may work through personal relationships, workers' (credible) threat to sue the firm for unjust dismissal, or direct bribes paid with part of the extra UI. Our data do not allow us to disentangle these possibilities and leave this question to future research. Overall, these findings suggest that adjustment costs, bargaining power and proximity to managers play a role in workers' ability to engage in manipulation.

Although informative, this graphical analysis does not allow us to precisely quantify to what extent manipulators differ from other individuals who do not engage in manipulation. To provide a more precise assessment, we make use of a procedure developed in [Diamond and Persson, 2017, Section 6.2]. The idea is similar in spirit to the rest of our analysis. Let us say that we want to investigate whether manipulators are more likely to have a given characteristic, e.g. being female. If there are disproportionately more

[25] Although the McCrary test identifies the presence a discontinuity also in this case, this is substantially smaller than the one observed for workers coming from permanent contracts.

(less) women in the excess (missing) region compared to what a fitted counterfactual would predict, then manipulators are more likely to be female. Results are in Table 2.4. Columns (1) and (2) report the estimated mean characteristic for manipulators and non-manipulators, respectively. The difference of the two is reported in column (3), together with bootstrapped 95% confidence intervals. In column (4) we report the estimated mean for yet another group, i.e. all individuals whose unmanipulated age-at-layoff falls in the missing region. The results are in line with what found in the graphical analysis: manipulators are 18 p.p. more likely than non-manipulators to be female, 17 p.p. more likely to be employed in white collar jobs and 7 p.p. less likely to have full-time contracts. We observe that their wages are 6% lower, although estimates are relatively imprecise. No significant difference emerges in terms of tenure and geographic location. As before, we notice that firm size is an important element: manipulators come from firms that are about 40% smaller with respect to firm of non-manipulators. We only see minor and statistically insignificant differences in terms of age of the firm.

2.6 Robustness

2.6.1 Placebo tests

One of the key identifying assumptions of our methodology is that the frequency of layoffs would have been smooth across the threshold, were it not for the sharp PBD increase at the threshold. In this section we present two pieces of evidence in favour of this assumption. We show that manipulation vanishes when there is no discontinuity in benefit generosity at the threshold. In order to do so, we exploit information on two different UI schemes that were introduced *after* 2012: MiniASpI and NASpI.

The first was introduced in January 2013, as part of a broader reform of the Italian UI system. The reform introduced two new benefits that should have replaced both the OUB and RUB, i.e. ASpI and MiniASpI, respectively. ASpI was similar in many respects to OUB and maintained the discontinuity age fifty. MiniASpI, which replaced the RUB, maintained eligibility requirements, but introduced a new formula for PBD calculations: the worker was entitled to a PBD equal to half the weeks worked in the past year. Since this rule did not determine any discontinuity in PBD, it gave no incentive to individuals to manipulate their separation date. The second was introduced in May 2015,[26] together with other labour market reforms. Also the NASpI was not characterized by any discontinuity in PBD at the age-fifty threshold and its PBD formula was similar to the MiniASpI but it was based on the last 4 years before layoff.[27]

Layoff densities are reported in Figure 2.9. In both cases, we focus on workers who were employed on permanent contracts, and are thus more likely to be able to delay their layoff, as shown in Section 2.5.4. We fail to detect any evidence of manipulation. We can observe a small discontinuity in NASpI but it is considerably smaller than the one for the OUB and it is not significant at 5%. This suggests that workers delaying their layoff around 50 years of age in our sample were directly reacting to the policy induced PBD extension.

2.6.2 Extensive margin responses

Manipulation induces a re-timing of existing layoffs from the weeks immediately preceding workers' fiftieth birthday to right after, generating a missing and an excess mass compared to the counterfactual frequency. One of the identifying assumptions of the methods used in this paper is that manipulation is the only reason why we observe these changes in the vicinity of the threshold. However, if longer PBD increases workers' outside option out of employment, it is possible that the number of layoffs discontinuously increases after age fifty, even absent any manipulation. We call this increase an "extensive margin response". This is worrisome for two reasons: first we would be underestimating the upper bound of the manipulation region (z_U), and second, if the extra layoffs are *selected*, we would be altering the composition of jobs in the manipulation region for reasons other than manipulation, introducing a bias.

The nature of the selection is not straightforward. As discussed in Jäger et al. [2018], in a standard Coesean bargaining framework, positive changes in workers' outside options induce separations for those (marginal) jobs that have relatively low joint (firm

[26]Our analysis will focus on workers fired in 2016 to allow for a fuller adjustment of workers to the new rules and legislation.

[27]For additional information on these benefits, see Appendix 2.A.

+ worker) surplus. These could be e.g. the least productive jobs employing the least skilled workers. In other (non-Coasean) settings, changes in outside options induce a higher number of separations among jobs with low workers' surplus. These could be the workers who value leisure relatively more or are employed in physically strenuous occupations, and not necessarily the least productive ones. In both cases this *extensive margin* response on the number of separations would alter the composition of jobs in a way that is potentially correlated with outcomes of interest. These concerns are not purely theoretical: Feldstein [1976], Feldstein [1978] and Topel [1983] provided a theoretical framework and some preliminary evidence on how more generous benefits may generate additional layoffs. Jäger et al. [2018] also finds an effect of extended PBD on job separation rates in Austria. They also find that the job matches of the workers who do not separate are not more resilient in subsequent years, casting doubts on the Coasean framework. Recent work by Albanese et al. [2019] documented an increase in the probability of separation for Italian workers who become eligible to the OUB scheme for the first time. In what follows, we show these concerns find little empirical support in our setting.

In testing for the importance of extensive margin responses, we consider two different scenarios. In the first scenario, all jobs can be hit by random shocks that decrease their value, and whose distribution does not feature any point of discontinuity. Since *all* jobs to the right of the threshold are less resilient due to lower worker surplus, we would expect to see an upward shift in the whole density of layoffs. In the second scenario, there are no shocks, but a limited set of jobs with small and positive surplus will mature into negative surplus as workers' age cross the age-fifty threshold, due to increased outside option of the worker. In this case additional layoffs might be concentrated right after workers' fiftieth birthday, with the following age bins being unaffected. We analyse the former case by checking whether the density of layoff or workers' observable characteristics show a jump at the threshold, even after accounting for the presence of manipulation. We then consider the latter case by comparing the total excess and missing mass on the two sides of the cutoff. Finally, we discuss sample and institutional reasons which might make difficult to apply the results of the more recent contributions on this topic to our setting.

Testing for shifts in the density

Let us now turn to the first check: we look at whether the density of layoffs exhibits an upward shift even after flexibly controlling for the presence of manipulation. After excluding bins belonging to the manipulation region, we fit two separate linear specifications in age to the layoff density, on the left and on the right of the threshold respectively. We then check whether the estimated fit on the left-hand side does a good job in predicting the estimated fit on the right-hand side.[28] Intuitively, if the distance between the two lines is small, this is indicative that extensive margin responses are not so important in our setting. In practice, we run the following regression model separately on the two sides of the cutoff:

$$d_j = \alpha + \lambda a_j + \xi_j \quad \text{for } j \text{ outside of the manipulation region} \tag{2.7}$$

where d_j is the density of layoffs in bin j, a_j is the mid-point age in the bin and ξ_j is an error term. We then take the regression line estimated with left-hand-side data and extrapolate towards the right-hand side of the threshold. In Figure 2.10a and 2.10b we visually inspect whether the extrapolation from the left-hand side of the cutoff is consistently below the right-hand side estimated fit. Figure 2.10a shows results excluding the missing and excess region as defined in the previous sections while Figure 2.10b uses an extended definition of the manipulation region. This includes bins which were excluded in our main procedure but which might possibly contain a small share of manipulators. To identify the extended region, we first run a regression of density of layoff by bin on a first-order age polynomial[29] and on dummies for each age bin between 6 months prior and 6 months after workers' fiftieth birthday. Then we check the sign of the coefficients for the age dummies. We define the extended missing (excess) region as the region characterized by a sequence of negative (positive) coefficient starting from

[28] Note that in this case, we use a linear specification, instead of a quadratic, as higher order polynomial would provide too much weight on extreme observations and might lead to a poorer overall fit outside the sample. Akaike Information Criterion and Bayesian Information Criterion both suggest that the linear and quadratic specification are roughly equivalent. Other measures of goodness of fit such as the R^2 also show substantial equivalence of the two models.

[29] The use of a linear or a quadratic polynomial provides similar results.

the cutoff.[30] This involves a simple assumption of continuity and increasing cost of manipulation in the distance from the threshold and it implies a convex missing mass and excess mass region. As some bins include a very small number of manipulators, they are less interesting from the point of view of our main analysis but their presence might influence the slope of the polynomial. Even limiting to the case in which the usual manipulation region is removed, the two projections show remarkable similarities and the difference in the projected densities can account at most for 5% of the difference between the predicted density based on the left-hand side of the cutoff and the data in the first bin after the cutoff. Once marginal bins are also excluded with the extended manipulation region, the two polynomial overlap almost perfectly. This suggests that it is unlikely that excess margin layoffs influence substantially our density.

Testing for discontinuities in observable characteristics

As a second check, we assess whether workers separating on either side of the cutoff differ systematically, above and beyond what can be explained by manipulation. We run two sets of models, a naive one that does not control for manipulation (and serves as a benchmark) and one that explicitly controls for it. The naive model, ran on the full sample reads:

$$x_i = \alpha + \sum_{p=1}^{P} \lambda_p^{\leq 50} \cdot a_i^p \cdot \mathbb{I}[a_i \leq 50] + \sum_{p=0}^{P} \lambda_p^{>50} \cdot a_i^p \cdot \mathbb{I}[a_i > 50] + \xi_i \qquad (2.8)$$

which is a standard RD model where $\lambda_0^{>50}$ is the jump at the threshold. The other model adds bin-by-bin indicator variables for the manipulation region and is as follows:

[30] To reduce the possible influence of negligible deviations from the polynomial, we consider zeros very small deviations from the polynomial. This makes estimates of the region more stable across specifications. The threshold is set at one thousandth of the average density per bin, roughly equivalent to a deviation of three workers from the polynomial.

$$
\begin{aligned}
x_i = \kappa + \sum_{p=1}^{P} \theta_p^{\leq 50} \cdot a_i^p \cdot \mathbb{I}[a_i \leq 50] + \sum_{p=0}^{P} \theta_p^{>50} \cdot a_i^p \cdot \mathbb{I}[a_i > 50] \\
+ \sum_{k=z_U}^{z_L} \delta_k \cdot \mathbb{I}[a_i = k] + \nu_i,
\end{aligned}
\tag{2.9}
$$

If manipulators are selected on observables, we would expect $\lambda_0^{>50}$ to be different from zero, a point also raised in Section 2.5.4. If however manipulation is the only reason why selection arises, we would expect $\theta_0^{>50}$ to be equal to zero. We reports tests on these two coefficients in Table 2.5. Columns (1) to (3) report estimates from model 2.8. Observable characteristics are indeed different on the two sides of the threshold, because of manipulation, but potentially also because of extensive margin responses. Columns (3)-(5) rule this last channel out. The fact that the distribution of observable characteristics is continuous at the threshold, after accounting for manipulation, makes the presence of additional layoff related to changes in the outside option for the workers less likely. This is very reassuring for the validity of our design, as it seems that changes in PBD do not induce extensive margin changes in the number of layoffs.

Testing for the presence of extra excess mass

So far, these analyses suggest negligible effects of unemployment benefits on layoffs. We now move to testing the second type of extensive margin response, that is the one that emerges only near the threshold. The basic idea behind the test is to see if we can detect additional excess mass to the right of the cutoff, above and beyond what would be predicted by the missing mass. In absence of extensive margin responses, excess and missing mass should be equal, so any difference in favour of the excess mass would make us think PBD is inducing extra layoffs right after the threshold. In order to implement our test, we estimate the following regression model on the layoff density:

$$
c_j = \alpha + \beta a_j + \sum_{k=A}^{50^-} \tilde{\gamma_k} \cdot \mathbb{I}[a_j = k] + \sum_{k=50^+}^{B} \tilde{\delta_k} \cdot \mathbb{I}[a_j = k] + \zeta_j \tag{2.10}
$$

Where $\tilde{\gamma_k}$ and $\tilde{\delta_k}$ are coefficients for dummies corresponding bins of an extended manipulation region, ranging from $A < z_L$ to $B > z_U$. We choose A and B this region under a mild assumption of convexity of the excess and missing mass. In practice, we look at an extended missing (excess) region delimited to the right (left) by the cutoff and to the left (right) by the last bin with a negative (positive) coefficient. The resulting missing region is substantially larger and it goes up to 4 months before the cutoff (9 bins) while the excess region is remarkably similar and it adds only a couple of bins to the one used in our baseline estimates. We then compute the missing and excess mass as the difference between the polynomial and the observed distribution and assess the difference in total excess mass and missing mass. Finally, we rescale it by the excess mass to have a relative magnitude of the share of the excess mass composed by excess layoff. The resulting *excess* excess mass corresponds to only 1.3% of the whole excess mass in the excess region. In addition, this also allows us to quantify the share of the two masses covered by our baseline missing and excess region. The masses computed with our baseline specification represent 67% of the missing mass and 70% of the excess mass. As the regions added in this exercise contain only a small number of manipulators per bin, we use our baseline specification throughout the paper as additional bins would provide little information on manipulators given their limited presence in each extra bin. Overall, results are reassuring about the absence of any additional excess mass coming from additional destruction of marginal jobs and further corroborates our results.

Why are extensive margin responses so small?

In this subsection we discuss why we think it is plausible that we do not detect sizeable extensive margin responses in our setting. Mainly, our benefit changes at the threshold is smaller and less salient compared to other studies and other institutional features limit the scope for big changes. For comparison, we take the results in Jäger et al. [2018] and Albanese et al. [2019] as benchmarks.

First of all, they focus their attention on a policy change in Austria in 1988 that increased PBD from 30 to 209 weeks, a seven-fold increase. In our case the PBD increased by 50%, which is fourteen times smaller. Due to differences in our estimation strategies and setting, it is difficult to map their results in our case as they estimate the additional probability of separation from 49.75 to 55 years (Table A.2 in their paper). However, by assuming linearity in the effects of longer PBD, and applying their estimates to our case, we can still recover what would be the implied extensive margin response in our setting. Jäger et al. [2018] find an increase of separations by 11 percentage points over a baseline of 36%. This would imply an increase in layoff by about 2.5%.[31] This would represent a very small change in our overall density and it unlikely to generate substantial bias.

Secondly, it is worth stressing that two features of our institutional setting make it difficult to extend results from Jäger et al. [2018] to our framework. A relevant aspect that should be taken into account is that the higher separation rate is partly driven by quits rather than layoff. Indeed, in the Austrian system workers who quit their job are eligible to receive unemployment benefits while this is not possible in Italian legislation, unless under particular circumstances. In addition, the longer unemployment benefits under the Austrian REBP could be used by workers to bridge towards retirement after turning 55. This made unemployment more attractive to workers. The Italian pension system was, in the period considered, much less generous as normally workers could retire only after 64 years of age for males and 59 for females. Both these substantial differences make less likely that the extension of potential benefit duration leads to excess layoffs.

We now turn to comparing our work to Albanese et al. [2019], who find a sizable increase in the separation rate for workers who become eligible to the OUB scheme in Italy for the first time. We present four reasons why we think these responses are unlikely to be present in our sample, although we are studying the same benefit program. First of all, individuals in their sample are young and relatively inexperienced, having accumulated at most two years of labor market experience before receiving benefits. We know that workers at the beginning of their careers change firms quite frequently before stabilizing into a permanent position (Topel and Ward, 1992). This would imply that matches in their setting are relatively more fragile and more likely to be terminated after an increase in workers' outside options. Similarly, older workers are more likely to have more resilient matches, which they are less likely to leave, possibly due to accumulated firm-specific human capital or other mobility barriers such as family ties. In addition

[31] This is obtained by deriving an elasticity from Jäger et al. [2018] estimates and then applying it to our change in weeks of unemployment benefits (50%).

to these considerations, it is also worth stressing that the workers in our sample have already experienced a jump their PBD in the past, precisely when they met their eligibility criteria. It follows that the observed matches, which end in a separation in our dataset, have already survived a large increase in their outside option, so they should be less sensitive to further increases in it. Finally, Albanese et al. [2019] exploit variation in UI eligibility rules, which allow workers with no UI to have access to some. We instead study variation at the *intensive* margin, since our workers obtain four extra months of PBD. Whether these two responses should be the same has not been explored so far and it can be argued that the former should be larger than the first case, as the worker gains the coverage immediately after the end of the spell and not in relatively distant period after layoff. To our knowledge there is no explicit analysis of this aspect in existing studies and we leave it to future research. All balanced, all these considerations might explain the discrepancy between our results and the higher probability of separation identified by Albanese et al. [2019].

2.7 Concluding Remarks

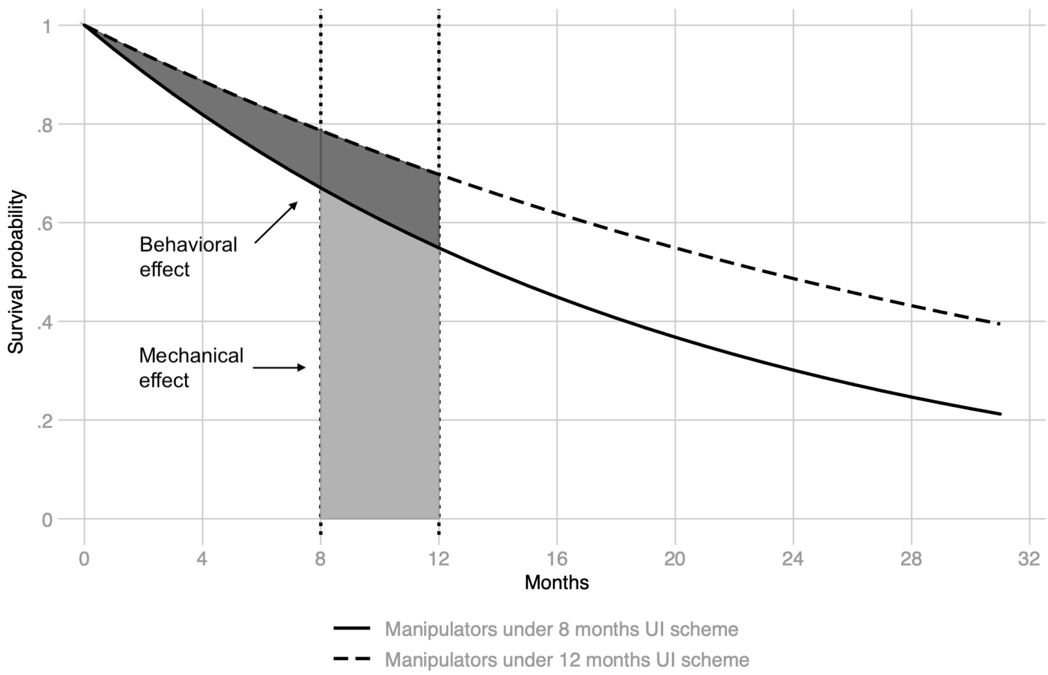
This paper studies manipulation in the context of unemployment insurance. We document substantial manipulation in forms of strategic delays in the timing of layoffs around an age-at- layoff threshold entitling workers to a four months increase in potential UI benefit duration in Italy. Using bunching techniques, we study the selection pattern and moral hazard response of manipulators. We argue that changes in subsequent job search intensities are informative about the underlying motives for manipulation and we identify long-term nonemployment risk as an important factor for selecting into manipulation. Manipulators are only modestly response to the increase in UI coverage mitigating concerns about anticipated moral hazard.

All in all, we illustrate how a more comprehensive understanding of the underlying motivation for manipulation might shape how the phenomenon is perceived. Furthermore, our results highlight the importance to take layoff responses into account when designing differentiated UI schemes and point to potential limits of governments' ability to target UI benefits.

Although a full welfare assessment is beyond the scope of this paper, we deem it a fruitful avenue for future research. So is the more general question of the desirability of differentiated UI policies.

Figures

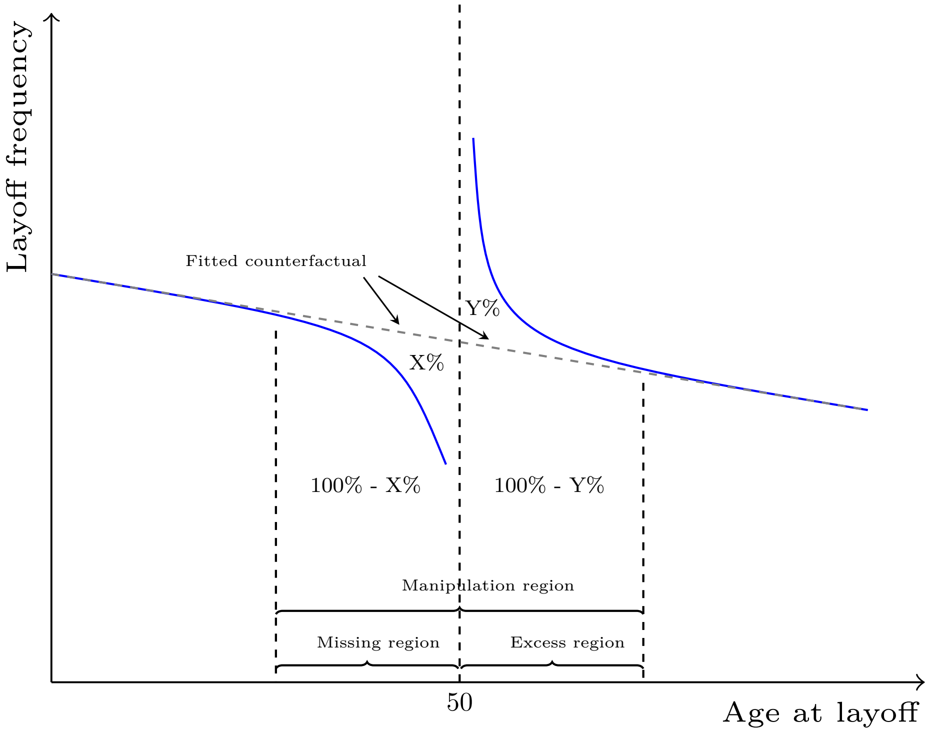
Figure 2.1: Anatomy of the fiscal cost of manipulation



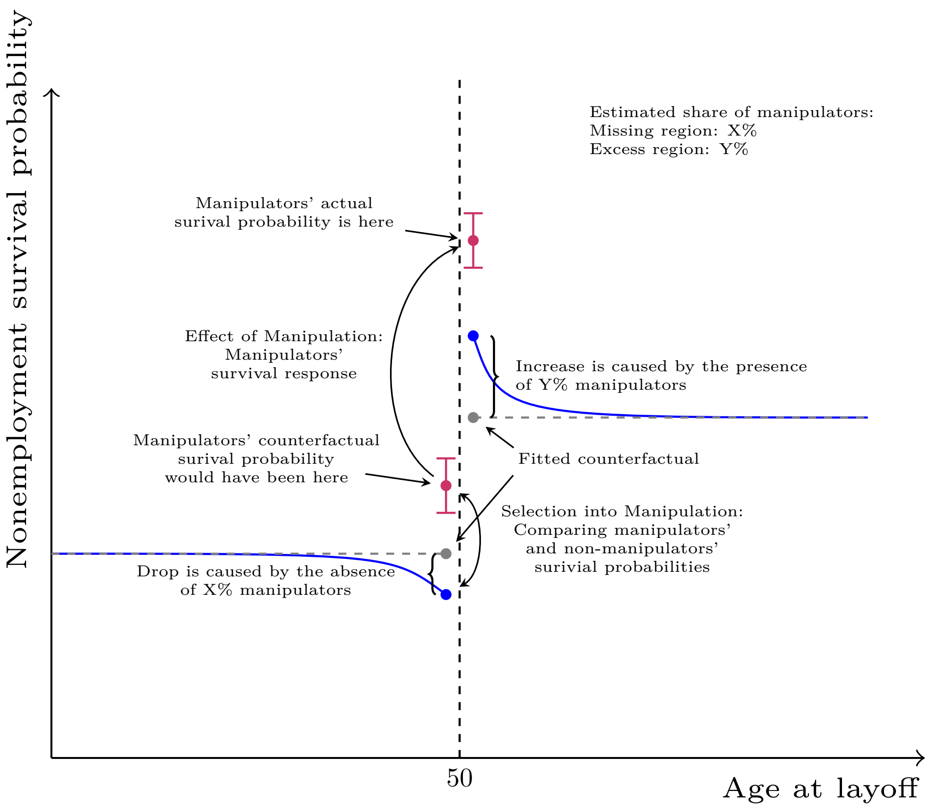
Note: The figure displays manipulators' survival curves in nonemployment under two alternative scenarios: manipulators' potential benefit duration (PBD) is 8 months (solid line), and manipulators' PBD is 12 months (dashed line). The dashed line is above the solid line under the assumption that higher PDB lowers the hazard rate of exit from nonemployment. The curves are simulated as negative exponentials with a constant hazard rate of 5% and 3%, respectively. The increase in the fiscal cost (shaded areas) is due to two components: (1) the mechanical cost (light-shaded area) due to extra UI outlays covering months 8-12, absent any behavioural change; (2) behavioural component (dark-shaded area) due to a shift in the survival curve in months 0-12, induced by the change. The effective moral hazard cost is given by the ratio of (2) and (1).

Figure 2.2: Illustration of identification strategy

(a) Quantifying manipulation



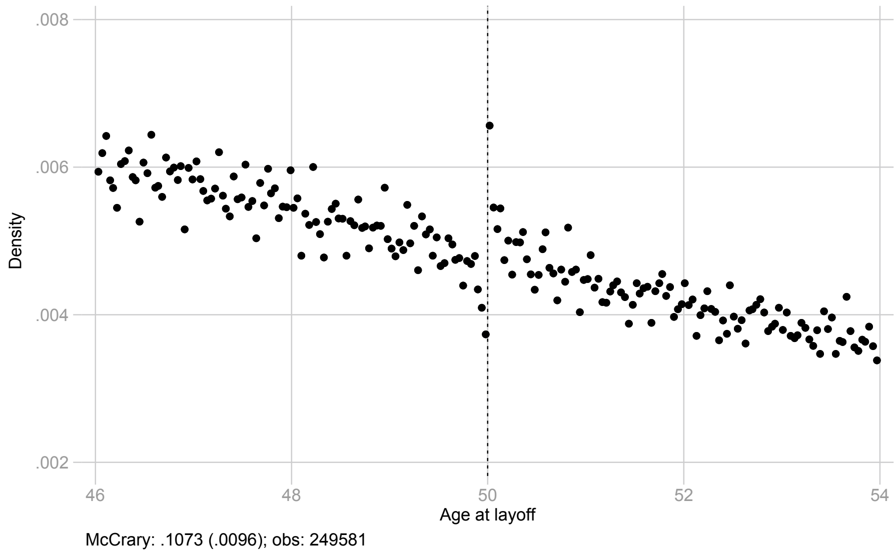
(b) Effect of and selection into manipulation



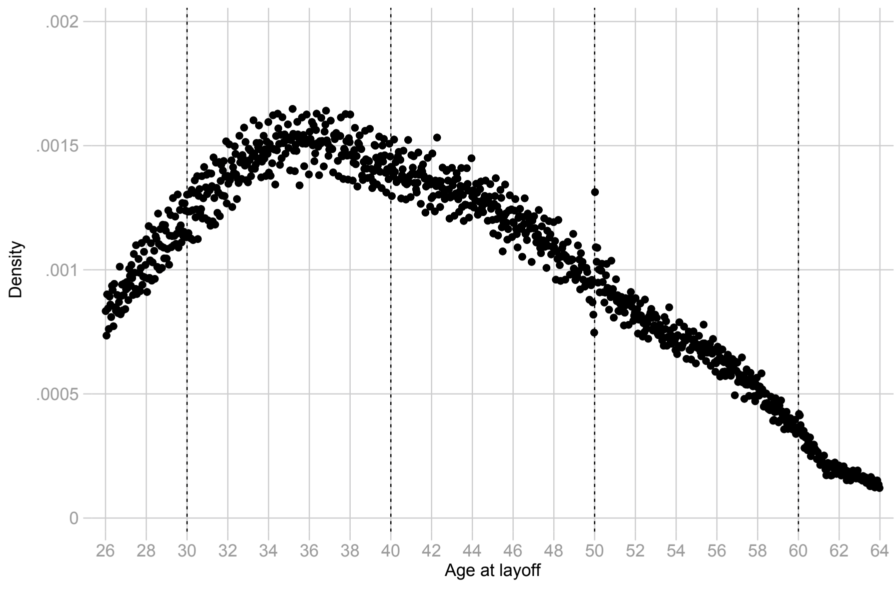
Note: This figure visualizes our identification strategy. Panel (a) illustrates how we estimate the number and respective share of manipulators in both the missing and excess region. Panel (b) constructs manipulators' survival response and illustrates the relevant comparison when studying selection into manipulation. Section 2.4 lays out how we estimate the fitted counterfactuals in practice.

Figure 2.3: Layoff frequency for permanent contract private sector workers

(a) Age-at-layoff between 46 and 54 years

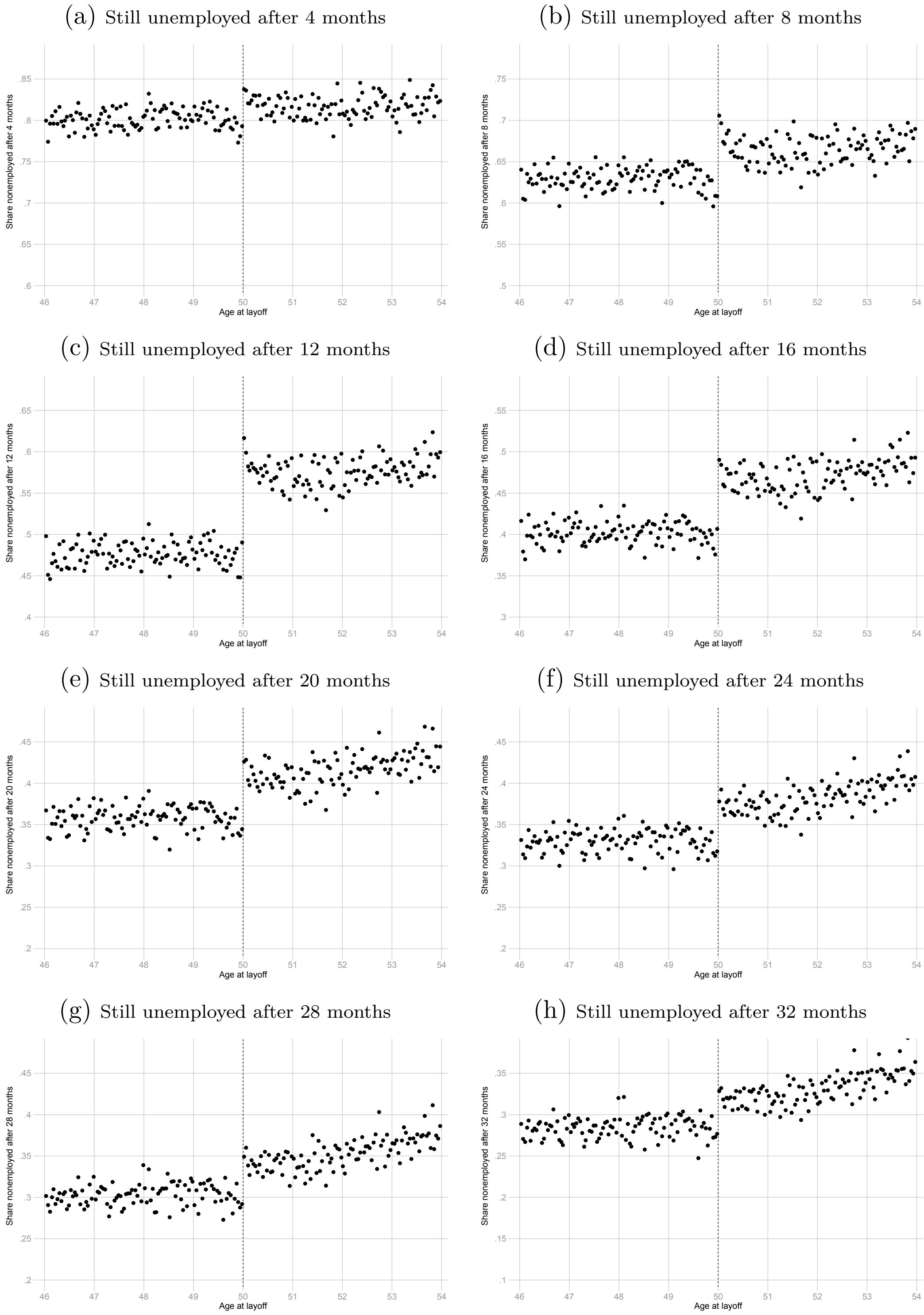


(b) Age-at-layoff between 26 and 64 years



Note: The figure shows the density of layoffs in the private sector, for individuals working on a permanent contract and eligible for regular UI (DORN). The data cover the period February 2009 till December 2012. Panel (a) plots the density for the age range from 46 to 54 years, while Panel (b) does so for the entire age range from 26 to 64 years of age. In both panels each dot represents a two week bin. The underlying data in Panel (a) consists of 249,581 layoffs.

Figure 2.4: Nonemployment survival probabilities

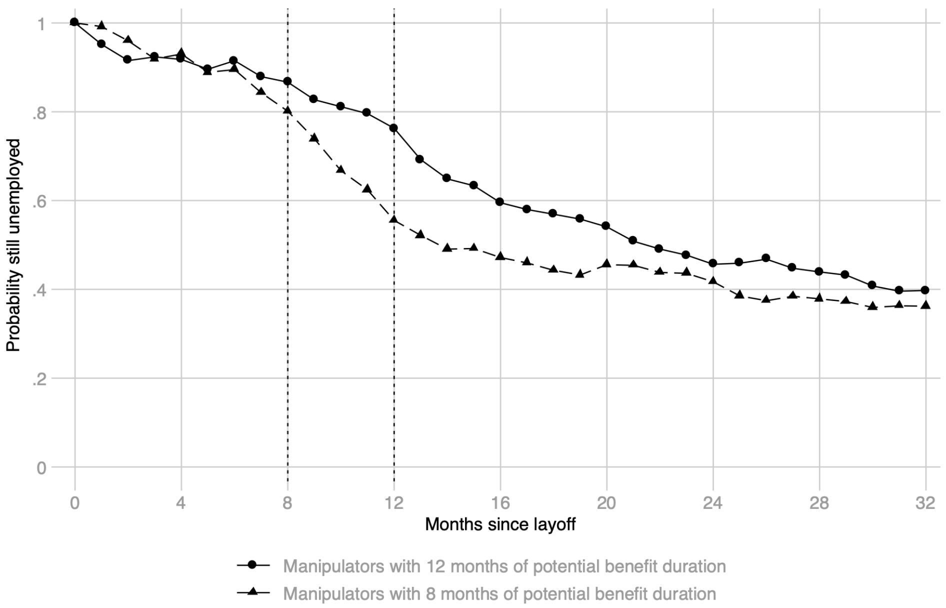


(a) Still unemployed after 4 months (b) Still unemployed after 8 months (c) Still unemployed after 12 months (d) Still unemployed after 16 months (e) Still unemployed after 20 months (f) Still unemployed after 24 months (g) Still unemployed after 28 months (h) Still unemployed after 32 months

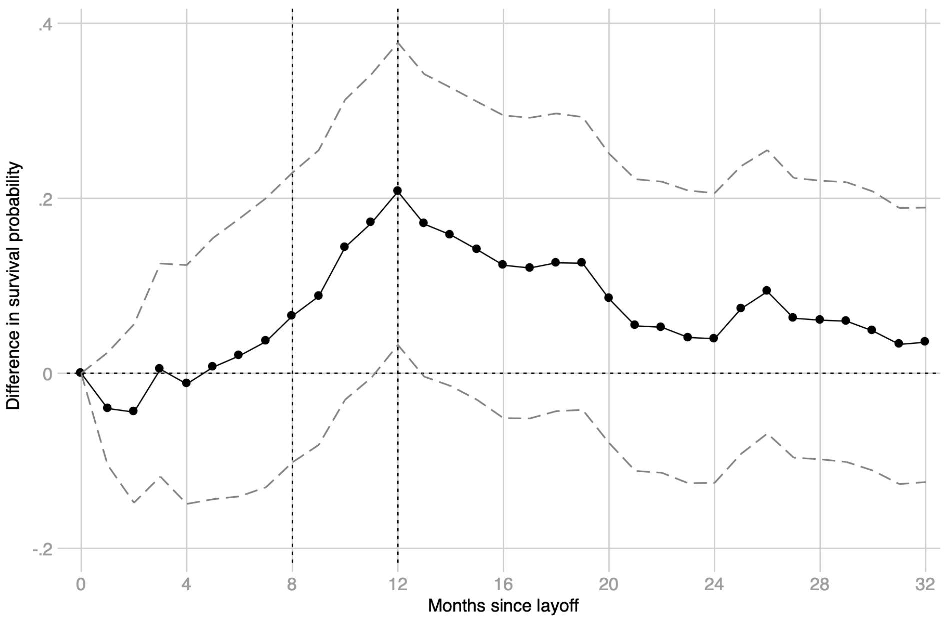
Note: The figures show the share of laid off workers, who are still unemployed after 4, 8, ..., 32 months. In all panels each dot represents a two week bin. The underlying data consists of 249,581 layoffs.

Figure 2.5: Manipulators with 8 and 12 months of potential benefit duration

(a) Survival in nonemployment



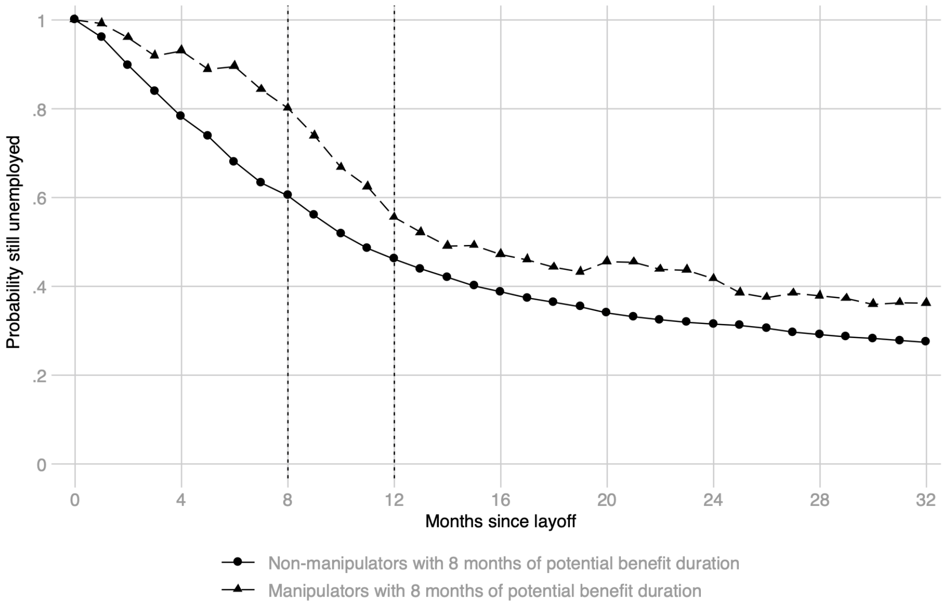
(b) Difference in survival rates of manipulators with 8 and 12 months PBD



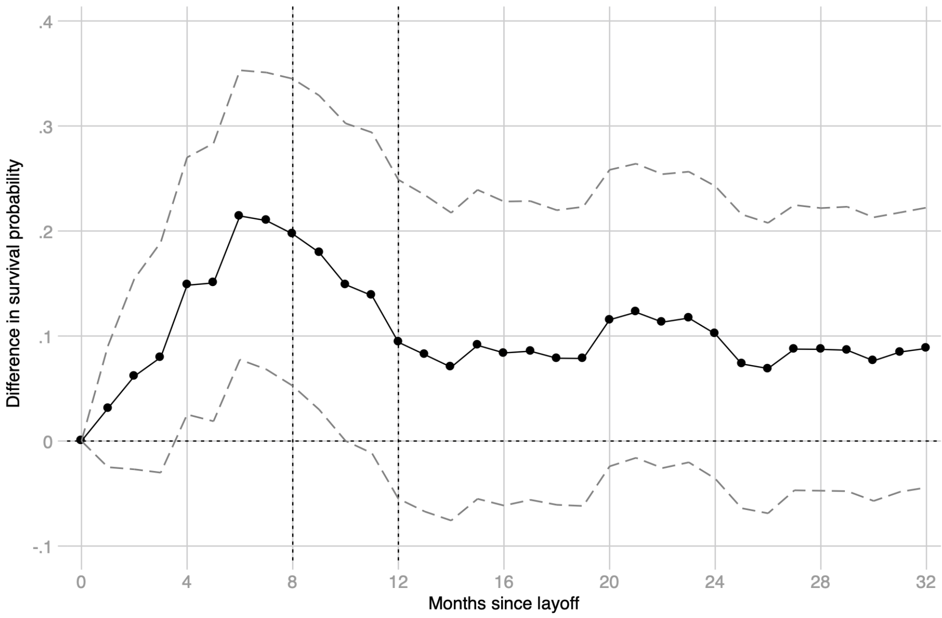
Note: Panel (a) plots point estimates of manipulators' actual and counterfactual nonemployment survival for the first 32 months after layoff. Our estimation strategy is outlined in Section 2.4.2. Panel (b) shows the difference between the two survival curves and contains bootstrapped 95% confidence intervals testing against zero difference.

Figure 2.6: Manipulators and non-manipulators with 8 months of potential benefit duration

(a) Survival in nonemployment



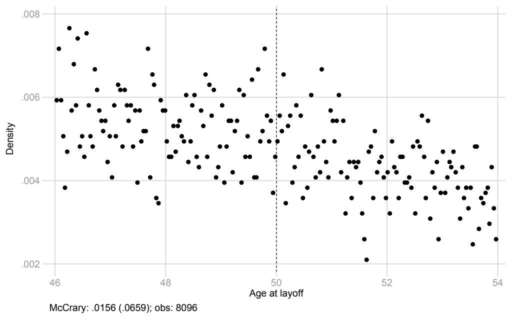
(b) Nonemployment hazard



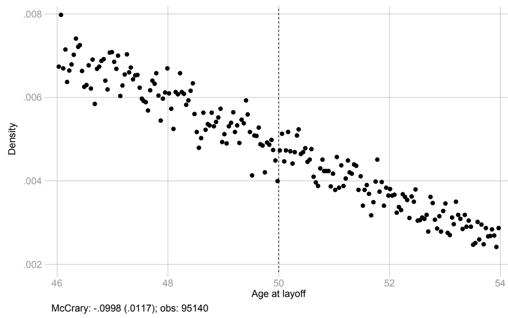
Note: Panel (a) plots point estimates of manipulators' and non-manipulators' nonemployment survival over the first 32 months after layoff under eight months of PBD. The estimation of the former is outlined in Section 2.4.2. The latter represents the observed mean survival rate in the missing region. Panel (b) shows the difference between the two survival curves and contains bootstrapped 95% confidence intervals testing against zero difference.

Figure 2.7: Density of layoff by private and public sector and by contract type

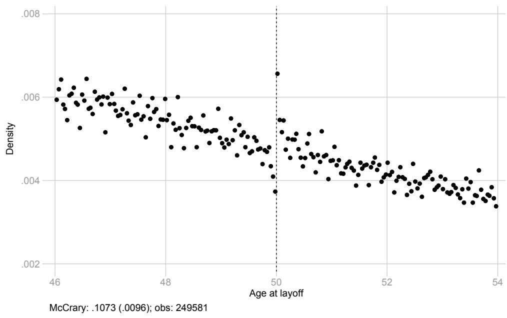
(a) Public Sector: Permanent



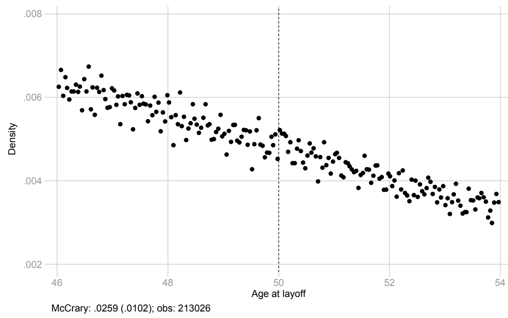
(b) Public Sector: Temporary



(c) Private Sector: Permanent



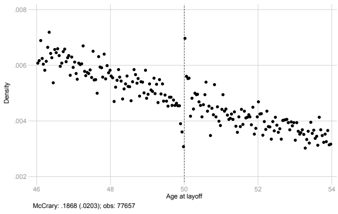
(d) Private Sector: Temporary



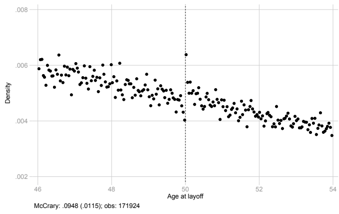
Note: Density by age for recipients of OUB between 46 and 54 years of age who were fired from February 2009 and December 2012. Workers classified from the public sector if their working history could not be observed in the data for universe of workers in the private sector.

Figure 2.8: Layoff frequency for subgroups of workers

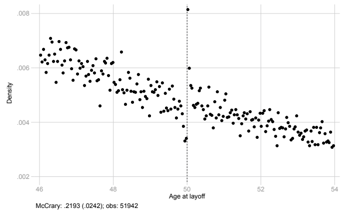
(a) Female



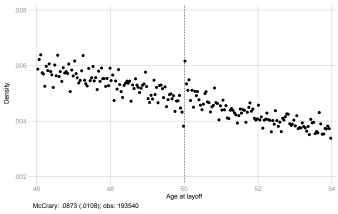
(b) Male



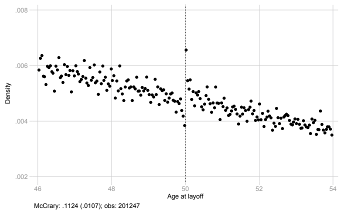
(c) White-collar



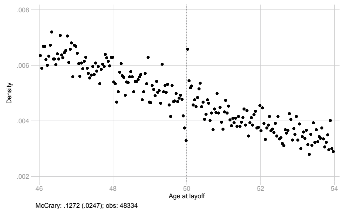
(d) Blue-collar



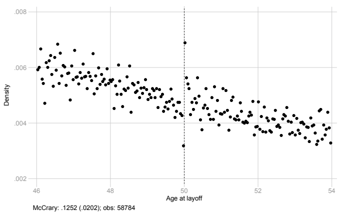
(e) Full-time



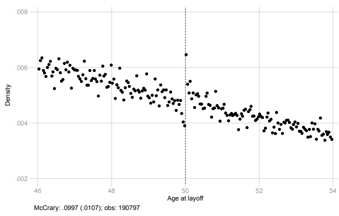
(f) Part-time



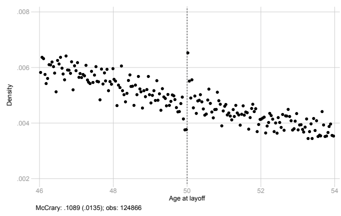
(g) Sector: Manufacturing



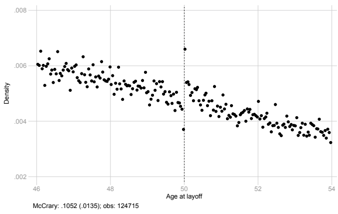
(h) Sector: Services



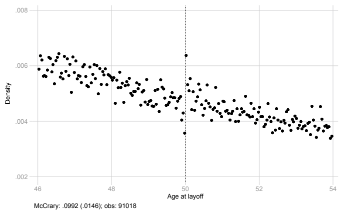
(i) Daily wage above median



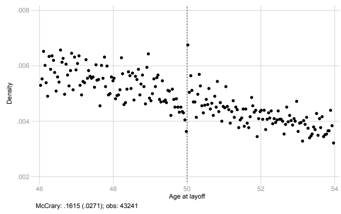
(j) Daily wage below median



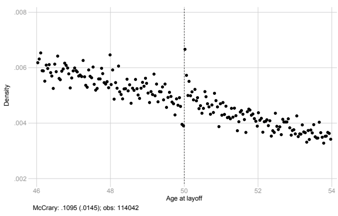
(k) Geographic area: North



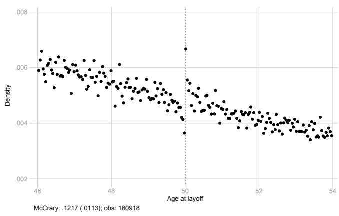
(l) Geographic area: Center



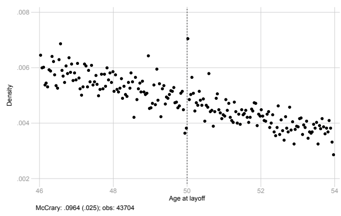
(m) (m) Geographic area: South



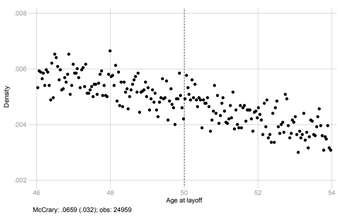
(n) (n) Firm size: 1-14 employees



(o) (o) Firm size: 15-49 employees



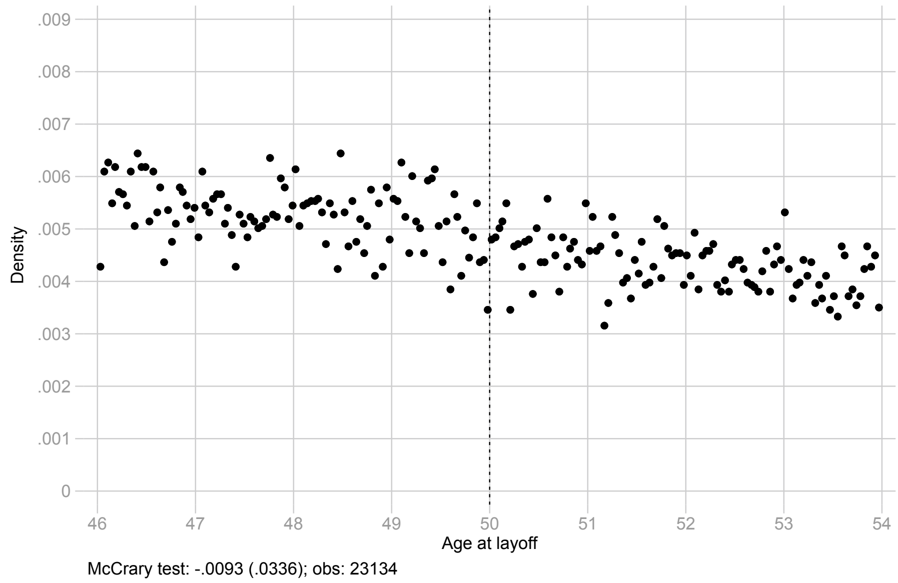
(p) (p) Firm size: 50+ employees



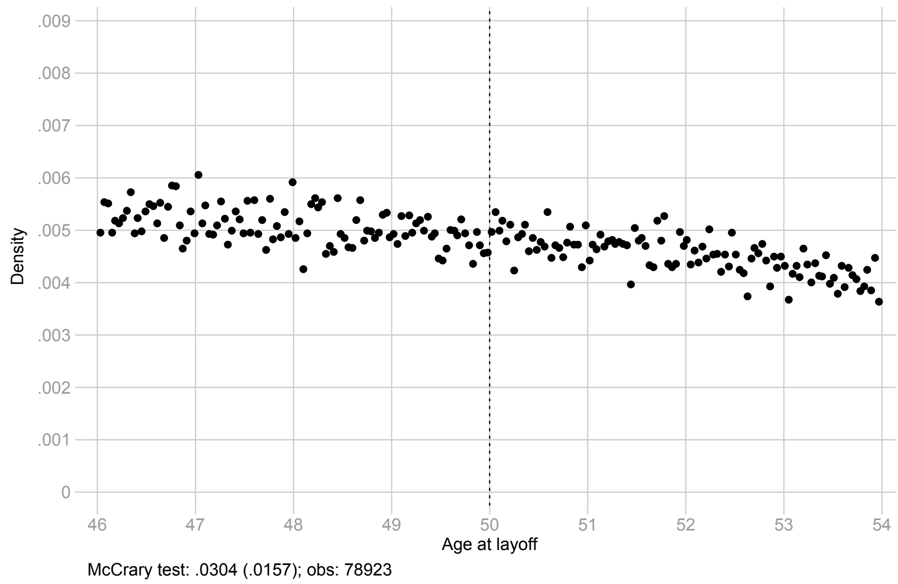
Note: Density by age for recipients of OUB between 46 and 54 years of age who were fired from February 2009 and December 2012. Sample restricted to workers from permanent contracts in the private sector.

Figure 2.9: Placebo checks: MiniASpI and NASpI and density of recipients at 50 years of age

(a) MiniASpI



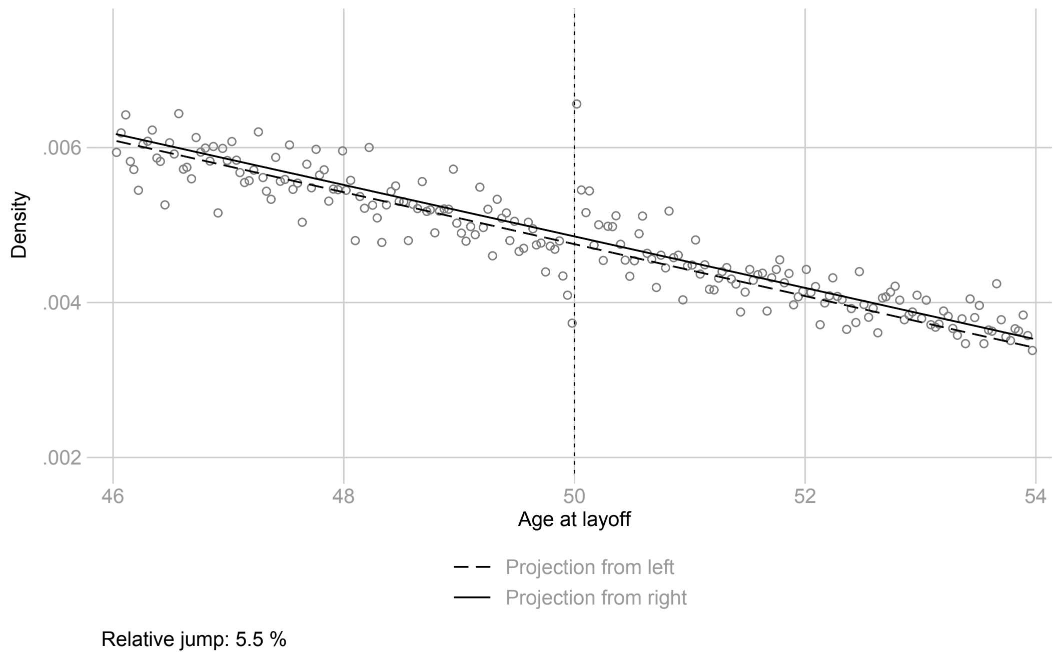
(b) NASpI



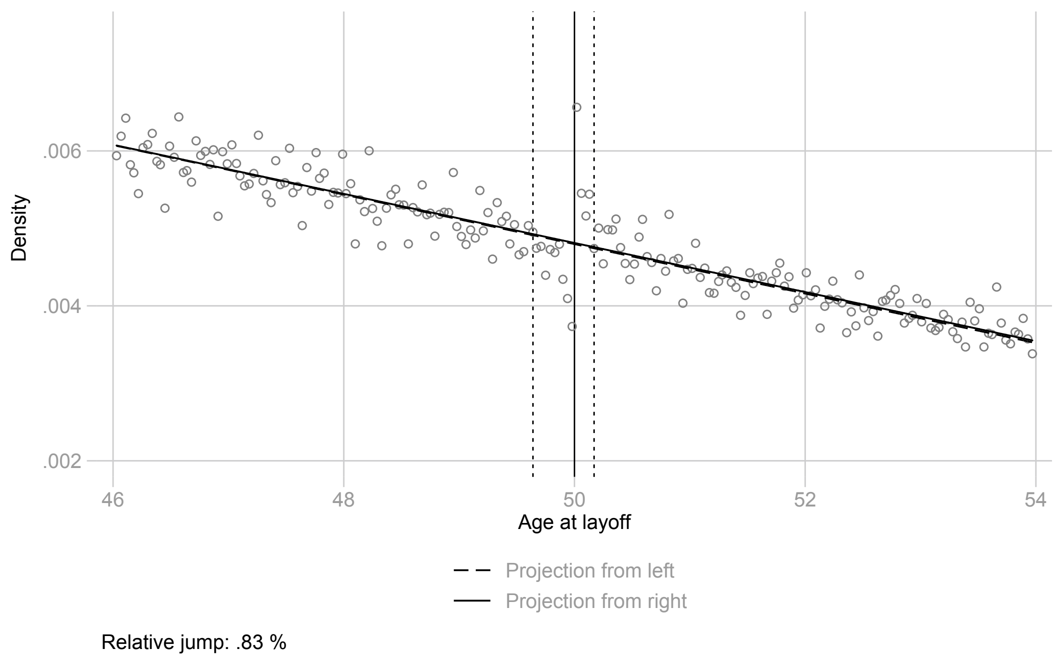
Note: Density of layoff for workers laid off in the private sector and receiving MiniASpI (2013-April 2015) or NASpI (2016). Sample restricted to workers coming from permanent contracts. McCrary test statistics and standard errors are reported below each graph.

Figure 2.10: Linear projection for density of layoff

(a) Baseline



(b) Extended



Note: Density by age for recipients of OUB between 46 and 54 years of age who were fired from February 2009 and December 2012. Sample restricted to workers from permanent contracts in the private sector. Projections derived from OLS estimated separately on the left-hand and right-hand side of the cutoff with a linear polynomial in age and then predicted for the whole age distribution. Dashed line corresponds to prediction based on data on the left -hand side of the cutoff while solid line corresponds to predicted values based on the right-hand side of the cutoff.

Tables

Table 2.1: Summary statistics

Variable	Mean	Std. Dev.	Min.	Max.
Outcomes				
Duration Benefits (Weeks)	29.853	15.923	0.14	52.00
Nonemployment Duration (Weeks)	89.995	79.092	0.00	208.00
Survival 8	0.502	0.500	0	1
Survival 12	0.388	0.487	0	1
Previous job characteristics				
Full Time	0.807	0.395	0.00	1.00
White Collar	0.208	0.406	0.00	1.00
Time since first employment (years)	27.656	8.552	2.00	40.00
Tenure (years)	5.931	6.113	0.08	30.00
South and Islands	0.459	0.498	0.00	1.00
Center	0.174	0.379	0.00	1.00
North	0.367	0.482	0.00	1.00
Daily Income	69.900	70.300	0.038	13,981.01
Female	0.311	0.463	0.00	1.00
Firm age (years)	14.367	12.115	0.00	109.83
Firm size	28.158	259.01	1	14,103
Firm size below 15	0.606	0.489	0	1
Firm size between 15 and 49	0.213	0.409	0	1
Firm size above 49	0.181	0.385	0	1

Notes: This table reports summary statistics on all OUB claims occurring between 2009 and 2012 for individuals aged 46-54 who held a permanent contract in the private sector. The number of spells is 249,581 while the number of workers is 210,041. Weeks of nonemployment is censored at 4 years and is computed as the distance between the layoff and the date of the first hiring that leads to UI termination. Tenure is defined as the number of years, even with breaks, spent with the same employer. South and islands is a dummy indicating that the worker was employed in one of the following regions: Abruzzo, Basilicata, Calabria, Molise, Puglia, Sardinia and Sicilia.

Table 2.2: Decomposition of benefit duration response

	(1) Benefit receipt duration response ΔB in weeks	(2) Behavioral response ΔB^{MH} in weeks	(3) Mechanical effect ΔB^{ME} in weeks
(a) *Manipulators*	14.53	2.96	11.57
	(100%)	(20.4%)	(79.6%)
(b) *Average population*	11.39	2.34	9.05
	(100%)	(20.5%)	(79.5%)

Note: Panel (a) reports the decomposition of manipulators' benefit duration response (in weeks) into a mechanical and a behavioral effect, following the methodology detailed in Section 2.3.1. Panel (b) reports the same decomposition for the average individual in the population, and is based on $\beta_0^{>50}$ coefficients from specification 2.5.

Table 2.3: BC/MC Ratios

	(1) BC/MC ($\tau = 0$)	(2) BC/MC ($\tau = 3\%$)	(3) BC/MC ($\tau = 20\%$)
(a) *Manipulators*	0.26	0.34	0.83
(b) *Average population*	0.26	0.32	0.70

Note: Panel (a) reports BC/MC ratios for manipulators, as defined in equation 2.3 in Section 2.3.1, for different values of the UI tax rate. Panel (b) does the same for the average individual in the population,and is based on $\beta_0^{>50}$ coefficients from specification 2.5.

Table 2.4: Difference in observables between manipulators and other groups

Variable	(1) Manipulators	(2) Non Manipulators	(3) Difference (1)-(2)	(4) Baseline Group	(5) Difference (1)-(4)
Female	0.450	0.270	0.180	0.306	0.144
			[0.100; 0.281]		[0.078; 0.206]
White Collar	0.351	0.180	0.170	0.199	0.152
			[0.101;0.239]		[0.094; 0.208]
Full Time	0.754	0.822	-0.067	0.806	-0.052
			[-0.134; -0.000]		[-0.106; 0.004]
Tenure	6.577	5.718	0.859	5.933	0.644
			[-0.142; 1.853]		[-0.166; 1.449]
Log Daily Wage	4.115	4.176	-0.0610	4.168	-0.053
			[-0.142; 0.023]		[-0.120; 0.015]
South	0.483	0.471	0.012	0.469	0.014
			[-0.072; 0.098]		[-0.056; 0.083]
(Log) Size	1.862	2.258	-0.395	2.207	-0.345
			[-0.640; -0.155]		[-0.546; -0.148]
Age firm (years)	14.546	14.335	0.211	14.482	0.064
			[-1.945; 2.320]		[-1.647; 1.780]

Note: This table reports differences in observable characteristics between manipulators and non-manipulators. The analysis is based on 249,581 spells of individuals laid off from a permanent contract between 2009 and 2012. Column (1) reports estimated means of manipulators' characteristics; column (2) does the same for non-manipulators; Column (4) reports estimated means for baseline group, defined as the set of individuals we would have observed in the missing region, in absence of manipulation. Columns (3) and (5) report the difference between these groups. Bootstrapped confidence interval at 95% are reported in parenthesis. Bootstrap is stratified at the level of age bins.

Table 2.5: Test for discontinuity of observables at cutoff

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Simple RD model			**"Donut" model**			
Variable	$\lambda_0^{>50}$	s.e.	T-stat (1)/(2)	$\theta_0^{>50}$	s.e.	T-stat (4)/(5)	Baseline
Female	0.011	0.005	**2.43**	0.000	0.005	-0.03	0.31
Full Time	0.001	0.005	0.26	0.005	0.005	1.09	0.81
White Collar	0.017	0.005	**3.71**	0.005	0.005	0.86	0.20
Market Potential Experience	0.177	0.095	1.85	0.093	0.107	0.87	27.34
Tenure	-0.040	0.063	-0.63	-0.095	0.078	-1.22	5.85
(Log) Daily Wage	0.000	0.006	0.03	0.005	0.007	0.69	4.17
South	-0.003	0.006	-0.56	-0.005	0.007	-0.74	0.47
(Log) Size	-0.038	0.014	**-2.72**	-0.015	0.016	-0.94	2.02
Age Last Firm (Years)	-.116	.130	-0.89	-.122	.137	-0.89	14.269

Note: This table reports results for the robustness tests described in Section 2.6.2. The analysis based on 249,581 spells of individuals laid off from a permanent contract between 2009 and 2012. $\lambda_0^{>50}$ and $\theta_0^{>50}$ are OLS coefficients from specifications 2.8 and 2.9, respectively. Columns from (1) to (3) report RDD coefficient for dicontinuity of observables at cutoff for whole sample together with standard error and associated t-stat. Columns from (4) to (6) replicates same exercise for sample excluding manipulation region. In both cases, the specification includes a dummy equal to 1 if the worker is fired after turning 50 years of age, a squared polynomial in age in difference from the cutoff and flexible on the two sides. T-stats are bold if coefficients are significantly different from zero at the 5% level. Baseline reports average for the individuals fired between 49 and 50 years of age. Standard Errors clustered at local labour market level.

Appendices

2.A Unemployment benefits in Italy after 2012

The Italian welfare system underwent several reforms after 2012 aimed at rationalizing the improve its homogeneity. In January 2013, both the OUB and the RUB were replaced respectively by the ASPI and MiniASpI. On the one hand, the ASpI mimicked many aspects of the OUB both in terms of requirements and in terms of structure of the benefit. In order to be eligible for the benefit, the worker had to have contributed for the first time to social security at least 2 years before the start of the unemployment spell and needed to have cumulated at least 1 year of work in the last 2 years. Similarly to the OUB, the worker was eligible to 8 months of benefit if she was fired before turning 50 while she was eligible to 12 months if the worker was fired after turning 50 years of age. The duration of the benefit was later modified on several occasions in 2014 and 2015, which makes it more difficult to use it for our analysis. The amount of the benefit was proportional to wages in the last 2 years and the worker received 75% of the average reference wage for the first 6 months and the amount was reduced by 15 percentage points every 6 months (up to 45% after 1 year). On the other hand, the MiniASpI was aimed at workers who did not meet the requirement for the ASpI (which kept most of the structure and requirement of the OUB) but had cumulated at least 13 weeks of work in the last year. The duration of the benefit was equal to half of the weeks worked in the last year and the amount was proportional to past wages: workers received 75% of the average wage in the last 2 years for the whole duration of the benefit.

Following April 2015, both measures were replaced by a unique benefit which provided a homogeneous coverage to workers in case of layoff. The new benefit, the NASpI, was mostly based on the structure of the MiniASpI. Workers were eligible to the benefit if they had worked for at least 78 days in the year before the layoff originating the unemployment spell and the duration of the benefit was equal to half of the weeks worked in the last 4 years. The amount of the benefit was proportional to past average wages and its schedule decreasing. More specifically, the worker was eligible to receive 75% of the average wage in the past 4 years and the amount was reduced by 3 percentage points for every month after the first 4 months of the benefit. This new benefit harmonized the Italian Welfare state and provided uniform coverage to workers previously eligible to different programs. In addition, it removed any discontinuity in the duration of the benefit, thus removing incentives for workers to delay their layoff.

Chapter 3

Teacher Turnover: Does It Matter For Student Achievement?

Stephen Gibbons
London School of Economics
Vincenzo Scrutinio
IZA, London School of Economics
Shqiponja Thelaj
University of Sussex, CEP, London School of Economics

Abstract

This paper contributes to the literature examining whether teacher turnover affects academic achievement. We focus on age-16, state secondary school students and use a unique dataset of linked students and teachers in England. We advance previous work by: a) looking at entry rates and student achievement in subject groups across which there is unlikely to be non-random selective assignment; b) by looking at a context where students study a curriculum for two years during which they will generally be taught by the same teachers. This allows us to estimate the effects of getting a new teacher mid-way through the teaching period. Our identification is based either on a school fixed effects design which exploits year on year variation in turnover in different subject groups, within schools, or a student fixed effect design where identification is based on cross-section variation in turnover in different subjects experienced by the student. Both methods give the same results: a higher teacher entry rate has a small but significant negative effect on students' final qualifications from compulsory-age schooling, despite organisational responses which assign new teachers to less risky grades. This result is robust to wide range of identification and robustness tests. Our findings point to the general disruption and lack of continuity in teaching as the main mechanism through which turnover harms student attainment.

Keywords: teachers, turnover, student attainment, schools.
J.E.L. codes: H4; I2; J24.

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3.1 Introduction

Recent research has established that teachers matter for student achievements, albeit because of dimensions of 'teacher quality' that are largely unexplained. On the basis of this evidence, recent policy in the US has, sometimes controversially, moved towards hiring and firing teachers on the basis of measurable impacts on student test scores (see for example, Thomsen, 2014, and discussion in Hanushek, 2009, Adnot et al., 2017, Rothstein, 2015). These kinds of hiring/firing policies, self-evidently, have limited aggregate implications if the supply of teachers is constrained (Rothstein, 2015). On the one hand, however, turnover could have potential benefits, on aggregate, because it is the mechanism by which: teachers gain a variety of experience; new ideas are brought into schools; and productive teacher-school matches are formed. On the other hand, there are also potential costs for individual students, schools, and on aggregate, from the disruptive effects of turnover. New arrivals take time to assimilate, leavers take school-specific experience with them, different teachers have different teaching styles causing a lack of continuity and turnover absorbs financial and administrative resources. These disruptive effects from teacher turnover could potentially offset any of its advantages, at least in the short run. In the US, England, and elsewhere, there is a presumption amongst policy makers and practitioners that turnover has, on average, adverse impacts. Turnover of teachers is also perennial concern for parents, particularly when it occurs during the period when students are studying for important exams.[1]

Despite the popular importance of this issue, there are relatively few quality studies that investigate it empirically, such as Ronfeldt et al. [2013], Hanushek et al. [2016], Atteberry et al. [2017]. Our paper adds to this existing evidence on the causal impacts of teacher turnover. By causal impacts, we mean the average gap in achievements between students experiencing a high turnover of teachers and students experiencing a low turnover of teachers, in a hypothetical experiment in which teachers, their entry probabilities, their exit probabilities, and their students are all randomly assigned. The analysis is based on a large administrative dataset of teacher workforce records linked, by school and subject categories, to students' achievement records in England over five

[1] A browse of the mumsnet.com website confirms this.

cohorts between 2009 and 2013. For most of the analysis we focus on entry rates as an indicator of turnover – for reasons elaborated later on – though we also look at exit rates.

Our key finding is that students experiencing high teacher turnover do less well in their end-of-school exams. The effects are quite small, though non-negligible relative to other factors that have been found to affect student achievement. A 10 percentage point increase in teacher annual entry rates[2] reduces student point scores (a kind of GPA) by just under 0.5% of one standard deviation. Interestingly, this effect is of a comparable order of magnitude with respect to the effect of other dimensions of turnover that have been investigated by previous research: similar to the externalities from the turnover of students in schools (Gibbons and Telhaj, 2011; Hanushek et al., 2004) and slightly larger than the effects of turnover of students in neighbourhoods (Gibbons et al., 2017). Evidently, teachers entering and leaving matters, but this is no more disruptive to education than turnover amongst a student's peers.

Although our analysis of administrative records is necessarily unable to precisely articulate the behavioural channels through which turnover affects achievement, we say something about the potential mechanisms by looking at heterogeneity across the qualifications, age and experience of teacher who are entering and leaving, and across types of student. The impact is quite general. We also show that the effects are insensitive to a wide range of controls for teacher age, salary and experience, implying that the effects are not due to changes in workforce composition. The results suggest, instead, that the adverse impacts on achievement are due to new teachers disrupting continuity in teaching for students and having no experience specific to the institution they join.

We advance the existing literature in a number of ways. Firstly, in line with arguments in Hanushek et al. [2016], we worry about potential reassignment of new teachers to student groups that are lower or higher performing. Therefore, we focus on the 'intent to treat' impact of teacher entry into subject groups, across all grades in a school in a given year, on the final school qualifications of students taking their exams in that year. These subject-school groups are akin to school departments. There is an advantage of

[2] The corresponding average for entry in the period considered is 14% or about 0.76 new teachers per year.

this approach, over, say comparing the performance of students in a year when they are allocated a new teacher with those who are not (Atteberry et al., 2017), or comparing the performance of students experiencing different rates of teacher entry in specific grades (Ronfeldt et al., 2013). The advantage is that it is hard to reallocate specialised secondary school teachers across subjects. This mitigates concerns about selective allocation of new teachers to lower or higher performing students or student groups within a school. The improvement over using whole school-by-year entry rates without splitting by subject (Hanushek et al., 2016), is that we can control more effectively for school-by-year shocks using fixed effects' estimation. Our research design, therefore, identifies the effects of turnover on achievement from school-subject-year specific shocks in the final year of secondary school. We exploit over time variation in turnover within school-subject groups, controlling for school-year and subject-year shocks. We also present estimates based on within student variation in exposure to turnover across subjects in the same academic year. The similarity of results from this strategy with results for our preferred specification suggests that negative effects of turnover are not driven by unobserved student heterogeneity.

Another disadvantage of studies that analyse the effects of grade-specific variation in turnover is that students are themselves moving between grades and will typically experience a change in teachers regardless of levels of turnover. Therefore, any estimates of turnover based on this type of design will omit effects due to disruption in the continuity of teaching experienced by students – which is one of the main potential channels. Our analysis, in contrast, looks at turnover in subject groups in the middle of a two-year period where students are preparing for their crucial end of school exams, and where disruption is often thought to be particularly important. Typically, students will be taught by the same teachers over this period, and even if they are not, turnover in a department over this period will cause disruption to the organisation of the teaching for students approaching their final exams. We are, therefore, more likely to capture these effects from lack of continuity, alongside any effects related to incoming teachers having no teaching experience specific to that school. Note, this finding is relevant to other contexts in which students experience mid-year disruption due to a change of teacher.

A further refinement of previous work is to demonstrate, through a range of placebo, balancing and robustness tests that we can treat school-subject-year turnover as random, conditional on the various sets of fixed effects. We provide a number of tests to show that our results appear to be causal in that teacher turnover is uncorrelated with student demographics, conditional on our fixed effects design ('balancing'); that we do not observe effects on groups of students who we would not expect to be affected ('placebo'); and that the observed impacts of turnover relate quite precisely to achievement in the years in which we observe the turnover ('event study').

One concern over our 'intent to treat' estimates based on school-subject-year turnover is that they may understate the impacts of teacher entry, if new teachers are assigned to students in grades other than that for which we measure student outcomes (i.e. there is non-compliance with the treatment). We investigate these issue by examining the extent to which schools assign new teachers to grades other than the high-stakes, final exam-taking grade for which we measure student outcomes. Indeed, we find that new teachers, particularly if they are new to the profession, are less likely to teach this grade. Our main estimates are thus potentially a lower bound on the causal impacts of randomly assigning new teachers to students, although further analysis using information on the grade in which a teacher teaches suggests the downward bias is not large. This result is in itself important because it sheds some light on the extent to which school re-organisation may lead to underestimation of the impacts of many types of school intervention or shock. This is a pervasive concern throughout education policy evaluation because it implies that estimates of policy interventions on student outcomes might be lower than what policy makers and researchers might have hoped or expected unless researchers allow for this kind of organisational readjustment. We further show that school organisational quality is an important dimension here, with schools rated 'Outstanding' by the school inspection authorities being less likely to assign new teachers to the grade taking their final qualifications, and experiencing less of an effect from teacher turnover.

The rest of the paper is structured as follows: Section 3.2 briefly summarises previous findings on the topic; Section 3.3 reports our empirical strategy; Section 3.4 describes the education institutional setting in the UK and the data set; Section 3.5 presents our main regression results, with Section 3.6 investigating the robustness of the analysis and Section 3.7 taking a more nuanced look at the variation in the effects across different types of teacher, student and subject; Section 3.8 provides concluding remarks.

3.2 Related literature

Although recently there has been a growing body of research examining patterns of teacher turnover (e.g. Ost and Schiman, 2015, Atteberry et al., 2017), studies investigating the direct link between turnover and student attainment are still thin on the ground. One of the reasons is lack of data, which makes it difficult for researchers to causally identify the direct impact of turnover on student attainment. The first large scale study, directly comparable to ours, is Ronfeldt et al. [2013], which looks at teacher turnover on 4th and 5th grade student performance in New York elementary schools. Their study finds that teacher turnover reduces achievement in both Mathematics and English, particularly for students in schools with a high proportion of low performing and black students. The fixed effects estimation strategy is similar to ours, but exploits within-school variation in turnover between grades and years. Hanushek et al. [2016] highlight the importance of controlling for within-school grade re-assignment of teachers and estimate model specifications that aggregate turnover and grade reassignments at the school-by-year level to address problems introduced by the non-random sorting of teachers among grades. Using data from a Texas district for teachers and students in grades 4 through 8 between 1996/97 and 2000/01, they find that teacher turnover has adverse effects on student academic achievement only in disadvantaged schools. Atteberry et al. [2017] examine how different types of switches (new to profession, district, school, and/or grade re-assignment) affects the attainment of New York City students in grades 3 through to grade 8 in a fixed effects' approach. They find that achievement is the lowest for students of teachers new to profession, followed by teachers who are new to district or school.

Adnot et al. [2017] also study the effects of turnover on achievement, but are interested in the effects of exits in context of a policy environment which encouraged exits of low performing teachers (the IMPACT programme). They find, unsurprisingly, that exits of underperforming teachers raise student achievement, but their study is silent on the impact of disruption caused by new entrants. Similar findings appear in Chetty et al. [2014] who document that: entry of good teachers raises achievement; entry of bad teachers lowers achievement; exit of good teachers lowers achievement; and exit of bad teachers raised achievement (where quality is based on teachers' previous history of generating high test scores).

Our research also relates to a broader literature on teacher turnover which looks into the factors that cause teachers to enter and leave schools and investigating the consequences of sorting for the composition of the teaching workforce (e.g. Ingersoll, 2001; Dolton and Newson, 2003; Allen et al., 2018). The typical finding is that schools serving disadvantaged young people have higher turnover than other schools. From amongst this literature, Hanushek and Rivkin [2010] argue that turnover is potentially beneficial because bad teachers leave and good teachers tend to stay in their sample of schools in Texas, though the aggregate implications are not very clear if teachers are just moving to and from schools elsewhere. They also focus only on the effects attributable to changes in composition, rather than any disruptive impacts.

3.3 Empirical setting

Our aim is to estimate the average causal impact that turnover of teachers in schools has on the academic achievement of their students. Conceptually, the idea is to understand the impact of randomly increasing the rate at which teachers enter and/or leave a school, holding other characteristics of the workforce, school and student body constant.

There are several basic empirical issues. Firstly, there are various ways to define and measure turnover. In line with previous work on student and teacher mobility (Hanushek et al., 2004, Gibbons and Telhaj, 2011, Ronfeldt et al., 2013, Hanushek et al., 2016), we focus the entry rate in a given year to represent turnover. Our design, based on year-to-year shocks to turnover, necessitates short term turnover indicators, rather than long term measures of turnover, churn and instability discussed in Holme et al. [2018]. The reasons for focussing on entry are elaborated at the end of this section, though we also look at exit rates. Secondly, there are obvious potential endogeneity problems. Entry rates (and other measures of turnover) will be, in part, determined by the characteristics of the school, its students and the characteristics of stock of teachers, since these factors will affect the exit rate (and hence the number of vacancies) and how attractive a school is to potential applicants. Moreover, sorting implies that teachers entering a school, the teachers in the stock and the teachers leaving are not likely to be identical, so entry and exit rates can change the composition of the school workforce. All of these factors may have direct effects on achievement and are only partially observed.

We address these endogeneity issues using a fixed effects regression design, in which we regress student exam outcomes in the final year of compulsory schooling (Year 11, age 16) on a rich set of controls for both students and teachers and on teacher entry rates at school-by-subject-by-year level – which measures the entry rate for teachers in a school teaching a particular subject, in a given year for all grades. In our main regression specification, identification comes from year-to-year changes in entry rates within school-subject categories, conditional on school-by-year and subject-by-year fixed effects. In other words, identification comes from year-to-year changes in subject-specific turnover shocks, partialling out year-to-year shocks to turnover across all subjects within the school, and year-to-year shocks to turnover in each subject across all schools. Our preferred specification is thus:

$$Q_{isqt} = \beta Entry_{sqt} + x_i\delta + z_{sqt}\gamma + a_{qt} + b_{st} + c_{qs} + \varepsilon_{isqt} \qquad (3.1)$$

Where Q_{isqt} is an index with mean 0 and unitary standard deviation of student i achievement in age-16 qualifications (in school s, subject q and year t), and $Entry_{sqt}$ is the entry rate (or other turnover measure) in each school-subject-year group. Coefficient β is the expected change in student test scores associated with an exogenous increase in turnover in the year in which a student takes their age-16 exams. The vector of optional student-specific control variables, x_i, includes: prior age-11 primary school test scores; Free School Meal (FSM) eligibility; gender; and ethnicity (white/others). Unobservable factors a_{qt}, b_{st}, c_{qs}, are treated as fixed effects and partialled out during estimation.[3] The vector of control variables at school-subject-year or school-year level, z_{sqt}, includes: the pupil-teacher ratio; proportion of female students; proportion FSM eligible students; proportion of white British students; number of teachers in current and past academic year; average age and experience of teachers; share of female teachers and average log annual salary for teachers. This rich set of control variables allows us to net out time-varying confounders correlated with turnover and, most importantly, the compositional effect of turnover on average characteristics of the school workforce. We cluster standard errors at school level to allow for serial correlation in unobservables over time and heteroscedasticity at school level.

In an extension to this design, we control for student fixed effects and subject-by-year fixed effects, so identification comes purely from variation in entry rates across subjects experienced by a student in a given school and year. The associated equation is:

$$Q_{isqt} = \beta Entry_{sqt} + z_{sqt}\gamma + a_{qt} + b_{st} + c_{qs} + d_i + \varepsilon_{isqt} \tag{3.2}$$

In other words, we examine whether students who face higher teacher mobility in, say, Mathematics than in English have lower academic performance in the former rather than in latter. This between-subject, within-student design has featured in several previous papers, such as Dee [2005], Clotfelter et al. [2010], Slater et al. [2012], Altinok and Kingdon [2012], Lavy et al. [2012], Nicoletti and Rabe [2018]. The key difference between the strategies in 3.1 and 3.2 is that the latter does not exploit the time series variation within school-subject groups, and identification is based purely on cross sectional variation across subjects within students (and schools) in a given year.

[3] We use within-groups estimation, or the numerical procedure of Correia [2014] as implemented in the command reghdfe in Stata.

The identifying assumption underlying these strategies is that teacher entry into a school-subject-year group is determined by the choices of teachers outside the school with only limited information about the characteristics of the specific student cohort. This is especially true because teachers almost always join at the beginning of the school year when they would have no information about the future KS4 performance of the specific student-subject group they will be teaching. Teachers' decisions about entry are, therefore, largely dependent on persistent or time-varying school level and subject level factors. School-subject-year specific entry rates can, therefore, be rendered plausibly exogenous by appropriate conditioning on fixed effects and observable school characteristics. We assess the credibility of this identifying assumption by showing that these subject-school-year specific shocks to turnover rates are largely uncorrelated with observable school, teacher and student characteristics, and by various 'placebo' tests.

The above considerations suggest that entry rates are better than exit rates as measures of turnover. End-of-year exit rates from a school-subject-year group are determined by the choices of teachers inside the school, with good information about the cohort of students they have been teaching. General school cohort quality shocks are taken care of by our school-year fixed effects. However, it is likely that subject-year exit rates, either during year t or t-1 are related to unobserved (to us) student-teacher match quality and, hence, to student attainment in year t. A teacher exit in a specific subject within a school-year group could signal adverse teacher-student match quality that is unobserved to us but observed by the incumbent teacher. The exit of a poorly matched teacher will, in turn, induce the entry of another teacher, but there is no reason to believe that this incoming teacher will share the same characteristics which make the outgoing teacher a poor match for the current student cohort. The entry rate is therefore plausibly exogenous, even if the exit rate isn't. One related situation which might raise concerns is if a shock to a department in year t-1 leads to exits in year t-1, consequent entry in year t, and poor performance in year t. In this case, entry rates in year t are negatively correlated with performance in year t, through the exit rates in t-1. Given a shock to a department in year t-1 would likely cause a fall in performance in year t-1, we would therefore also expect entry rates in year t correlated with performance in year t-1, but we will show through an event study framework that this is not the case.

It is also worth noting at the outset that there is one identification issue which we cannot address when focussing on entry rates: an increase in the entry rate is equivalent to an increase in the share of teachers with zero years of school tenure, so necessarily implies a reduction in average teacher tenure and experience in the school. The effects of entry and the reduction in average school-specific tenure it induces are therefore conceptually equivalent and not separately identified. However, we show that entry effects extend across the range of general teacher experience, so are not primarily a result of entrants being overly represented by new teachers with a lack of teaching experience. In addition, we also control for teacher experience, and the results are insensitive to various parametric and non-parametric specifications of this control, suggesting that the effects of entry are not due to changes in the general teaching experience of the school workforce.

3.4 Institutional setting and data

Our study focuses on the population of secondary school students and teachers in state-maintained secondary schools in England between 2008/09 and 2012/13.[4] Compulsory education in state schools[5] in England is organised into five "Key Stages". The Primary phase, from ages 4-11 spans from the Foundation Stage to Key Stage 2 (Years 1-6, where Years are the English terminology for Grades). At the end of Key Stage 2, when pupils are aged 10/11, children leave the Primary

[4] We base our analysis on 2008/09-2012/13 period. We have exact information on the subjects taught for the period between 2011 and 2013. We extend the analysis to the years 2009 and 2010 by imputing subject taught according to future teaching and qualification, to improve our sample size and to be able to run additional identification checks that require a longer time span. Results are similar if we restrict the analysis to the 2011/2013 period.

[5] State schools in England account for around 93 percent of the population of students.

phase and go on to Secondary school from ages 11-16, where they progress to Key Stage 3 (Years 7-9) and to Key Stage 4 (Year 10-11). At the end of each Key Stage, prior to age-16, pupils are assessed on the basis of standard national tests (though the Key Stage 3 tests stopped in 2008). Our study focuses on students in Year 11, which is their last year of compulsory schooling. During Key Stage 4 (Years 10 and 11), students study for and take assessments in a range of subjects, leading to their final qualifications at age 16. The most common qualification is the General Certificate of Secondary Education (GCSE), and we focus on these GCSE educational outcomes of students by subject. Assessment for GCSEs during our study period was generally carried out by a mixture of coursework during Year 10 and Year 11 and final summer exams in Year 11, with greater weight generally placed on the final exams. However, the structure of assessment varied between subjects, with some subjects such as Art being assessed purely on coursework.

The analysis described in Section 3.3 requires data on student performance and on teachers' career histories. Our main sources are student-level data from the Department for Education's National Pupil Database (NPD) and teacher records from the Schools Workforce Census (SWC), supplemented with the Database of Teacher Records (DTR).

The NPD data contains information on students' socioeconomic characteristics and attainment scores in the Key Stage tests, and Key Stage 4 qualifications. These data come from school returns made in January each year. Student point scores (a form of GPA) at Key Stage 4 – our main outcome measure – are taken from the NPD, along with scores for the Key Stage 2 primary school exam as a measure of general student quality. The national pupil database also reports information on other student characteristics such as age, gender, FSM eligibility and ethnicity.

The School Workforce Census (SWC) has run from 2010/11, and is also based on returns from schools, providing information on teachers, their qualifications, salaries, contract type, number of hours taught, subjects they teach, and other characteristics. We use SWC data up to 2012/13 and supplement it with information from the DTR to extend the data back to 2008/9. The DTR is used in the administration of the national teachers' pension system and also provides a range of information on teachers, such as salaries and their qualifications.[6]

Schools are identified as individual entities that are consistent over time from the "Edubase" dataset, which holds information on basic school characteristics like school phase, type, location in each year. Starting from the universe of secondary schools in UK, we exclude Independent (private) and Special Schools (for children with special needs). We construct unique school identifiers with information available on the Edubase database concerning school conversions. Schools formed from the merger of two or more schools, or schools resulting from the division of a school are treated as new schools. Our data does not permit us to know exactly which teachers teach each student. However, we are able to link students to teachers by the subjects the student takes in a school at Key Stage 4 (Years 10/11) and the subjects a teacher is teaching in that school. The SWC data provides information on the hours a teacher teaches in each subject, from which we derive the main subject taught. In the DTR, this information is unavailable, but we infer their main subject from subjects taught in the later years, and teachers' degree qualification.[7] We form 18 subject groups: Mathematics; English; Science; History; Modern Foreign Languages; Sports; Biology; Chemistry; Physics; Art; IT; Social Science; Design; Business and Economics; Home Economics; Media and Humanities and Engineering. These are aggregated from the 114 original subject codes, in a way that makes it feasible to assign mean teacher characteristics in these subject groups to students, based on which teachers the students are likely to encounter given the subjects they are studying. These subject groups are, in

[6] Our data stops in 2012/13 because after that point there were significant reforms to the GCSE qualifications and their assessment format, which makes comparisons with earlier years potentially problematic.

[7] A comparison of the subject taught and teacher qualification, when both are available, show a high level of concordance (more than 90%), which suggests that this imputation should induce minor measurement error.

effect, approximately equivalent to school teaching departments. The number of teachers by department is reported in Table 3.A.1 of the Appendix. Note that this aggregation does not imply we are introducing measurement error in terms of the entry rates and other measures of mobility: we are over-aggregating our explanatory variable, not introducing noise. We also estimated regressions where we aggregate all the data to form a school-subject-year group panel, but the findings are broadly similar to those reported in the empirical section below and we do not report them.

As discussed in Section 3.3, we use teacher entry rates as the main measure of turnover, but we also look at exit rates. Entry rates are constructed on school-by-subject-by-year groups, and also broken down by teacher characteristics (e.g. gender and salary quintiles). We also determine whether a teacher is moving from one school to another, or appears as a new entrant into the system, or whether they are leaving the system (based on whether we observed them in previous or subsequent years).[8]

The entry rate in a school-subject-year group is computed as the share of teachers present in the school-subject group during the current academic year (t) who were not present in that school-subject group in the previous year (t-1). The exit rate for the current year (t) is the share of stock teachers who are present in the year (t), but are no longer present in the school in next year (t+1). Data from the DTR/SWC for 2007/9 and 2013/14 is used to compute these variables at the beginning and end of our 2008/9 to 2012/13 study period. Entry is necessarily missing for the first year after the school opening.[9] A limitation of this approach to defining entry is that it does not distinguish the year group (i.e. grade) in which teachers are teaching. In practice, most teachers teach in their subject across all grades in England's secondary schools. For example, according to our

[8] To simplify our methodology and decrease the effect of possible misreporting, we do not consider exit from the profession if the teacher is not observed in the data for a few years but eventually is reported again. This concerns 5.6% of the total number of teachers.

[9] We ignore the small proportion (4.5%) of teachers recorded as moving within school across departments.

data, around 90% of teachers teach in both Key Stage 4 (Years 10 and 11) and Key Stage 3 (Years 7-9). In part of our analysis, we use a refined measure of teacher entry based on the share of total hours taught by incoming teachers in Year 11 -the year of students' final qualification exams, and Year 10 - the first year of the Key Stage 4 curriculum phase. In this way, we can say more about the importance of timing of teacher entry relative to the timing of assessments. However, missing data on subject teaching hours reduces the estimation sample size, and, as noted in Section 3.3 and in Hanushek et al. [2016], it could lead to biases if new teachers are selectively assigned to high or low performing grades.

Ultimately, we end up with data on teachers, their characteristics and the turnover variables aggregated to school by subject group by year cells. These school-subject-year variables are then merged with student-level data from the NPD. After cleaning and matching, the final sample spans 5 years, has 18 subject groups, approximately 2,750 schools, 2,305,500 distinct students, 202,500 school-subject-year groups and a dataset with a total of around 12,700,000 student-subject observations.

Descriptive statistics related to this sample are presented in Table 3.A.2 of the Appendix. Annual turnover of teachers is around 12% with entry rates (14%) slightly higher than exit rates (10%). Around 32% of the entry is due to teachers new to the profession (or entering from outside the English state school system), and the rest due to mobility of teachers across schools.

3.5 Main regression results

To begin the empirical analysis of the effect of teacher turnover on students' KS4 (Year 11) attainment, Table 3.1 reports the coefficients and standard errors from baseline regression estimates of equations 3.1 and 3.2, with overall entry rates[10] as turnover measure. As we move left to right across the table, the specifications

[10] Overall entry rate is computed as the share of new teachers over the total number of teachers in the department-school cell.

control for fixed effects at finer levels of granularity, with Columns (1) and (2) controlling only for year dummies, and Columns (7) and (8) controlling for subject-group by year fixed effects and student fixed effects. In order to test and control for other possible confounding factors, each fixed effect specification is reported with and without additional time-varying control variables. Odd-numbered columns have no additional control variables; even-numbered columns include a rich set of control variables for student and teacher characteristics plus average characteristics for students in school-subject-year group cells (see notes of Table 3.1 notes for details). Standard errors are clustered at school level. Entry rates are defined at subject-school-year cells, and, therefore, they represent a Year 11 students' potential exposure to teacher mobility in a school department as a whole in a given academic year, rather than actual exposure to mobility of teachers specifically assigned to teaching in their year group. The coefficients are then best interpreted as an 'intention to treat' effect, which avoids selection issues that could arise through strategic assignment of new teachers into different year groups. We consider alternative definitions of treatment that more closely capture students' actual exposure to teacher entry in Section 3.6.2.

In all specifications in Table 3.1, higher entry rates are associated with lower KS4 scores. With no control variables or fixed effects in Column (1), the coefficient of 0.10 implies that a 10 percentage-point increase in entry (about 60% of a standard deviation) is associated with a 1% standard deviation reduction in KS4 scores. When we add in controls for observable student, teacher and school attributes in Column (2), the coefficient becomes larger in absolute value. It is the inclusion of variables describing the existing teacher stock that leads to this change. However, when we more fully control for unobserved confounders with fixed effects at school-by-year and subject-by-year level in Column (3), the coefficient is reduced again to -0.050 and is now less sensitive to the inclusion of control variables in Column (4) (given the standard errors). The magnitude remains relatively stable with the inclusion of additional fixed effects. In Columns (5) and (6) we control, in addition, for school-by-subject fixed effects, implying that identification is based purely on variation in entry rates over time within these school-by-subject groups, conditional on time-varying factors affecting entry rates at school level and at subject level. In Columns (7) and (8) we introduce student fixed effects. Here identification comes from variation across subjects taken by each student. Note that school-by-year fixed effects are not identified within pupil, and so are omitted. The estimates from this specification are broadly similar to those in Columns (3)-(6) and are again fairly insensitive to the inclusion of time varying control variables. The stability of the estimates to observable characteristics in the specifications that control for school-subject specific unobservables, time-varying school and subject specific shocks, or student specific unobservables, suggests that entry rate variation in these specifications is effectively random with respect to these dimensions. 'Balancing' regressions in which we regress the entry rate on mean student characteristics in school-subject-year cells also demonstrate that the entry rates are uncorrelated with them (see Table 3.A.3 in the Appendix). In the remainder of the empirical analysis, we focus on the more conservative estimates based on year-to-year shocks in mobility in the specification of Column (6). Compositional changes in teacher workforce are a crucial component of the effect of turnover and past research has shown that they can account for a large part of these estimated effects. Hanushek et al. [2016] show that turnover has a negligible effect after controlling flexibly for changes in the experience of teachers and this leads us to consider this aspect more explicitly. In our baseline specification we control linearly for years in teaching profession but non linearities might make this approach insufficient to capture the full effect of experience. To overcome this issue, we assess the reliability of our model by trying several other possible specifications for this crucial control: polynomials (up to third order); share of teachers by experience classes; inclusion of tenure which might capture more fully the loss of school specific human capital. Results of this exercise, reported in Table 3.A.4 of the Appendix, show that the estimates are very robust to all these different choices and experience does not seem to play a very important role once our rich set of fixed effects is included.

Taken together, the estimates in Table 3.1, Columns (5)-(8), suggest that an increase in the entry rate of 10 percentage points reduces attainment by around 0.3-0.5 percent of one standard deviation, with our preferred estimate in Column (6) at just under 0.5 percent of one standard deviation. This implies that a one standard deviation increase in entry rates (16.7 percentage points) in the year of preparation for end of school qualifications reduces attainment by around 0.8 percent of one standard deviation. This is not a huge effect, but it is non-negligible compared to many school interventions and the magnitude is similar to the effects of other turnover-related externalities in schools. The magnitude is close to that from the turnover of students in schools (Gibbons and Telhaj, 2011; Hanushek et al., 2004) and slightly larger than the effects of turnover of students in neighbourhoods (Gibbons et al., 2017). These effects are also comparable in magnitude to estimates for the US such as Hanushek et al. [2016], although in their case the effect of turnover is fully absorbed by compositional changes in the experience of teachers.

3.6 Robustness checks

To assess the robustness of our results, we run a series of checks: 'placebo' treatments; controlling for additional confounding factors; checking the robustness of our definition of exposure to teacher entry; showing the timing of effects in an event study.

3.6.1 Confounding trends and shocks

The estimates of our effect of interest in Table 3.1 appeared robust to the inclusion of a wide range of controls and fixed effects. However, it is still possible that some unobserved pre-existing trends or time varying contemporaneous (to entry) shocks are driving our results. Table 3.2 presents the results of a number of checks related to these threats.

Column (1) reports the coefficient for our preferred baseline specification with school-by-subject, school-by-year, and subject-by-year fixed effects from Table 3.1, Column (6). Column (2) includes two years' lead of the measure of entry (t+2): if entry reflects a general trend of the school/department, then a higher turnover in the future might be associated with lower grades in the current year. As Column (2) shows, the inclusion of this measure of future entry, however, does not have any effect on students' attainment in the current year and our main coefficient of interest is largely unaffected by the inclusion of this measure of future turnover. This suggests that our estimates do not reflect general trends in the school-subject performance. A similar reasoning is applied in Column (3), where we include a measure of turnover in other subject groups within the school in the same year. We exclude other subjects taken by the student in order to avoid any possible spillover across subjects. Again, in this case, the effect of entry is robust and the entry in other subject groups does not have an independent effect on students' scores. Column (4) includes exit rates alongside entry rates. As discussed in Section 3.3, exit rates are more likely to be endogenous than entry rates, due to teacher-student match quality. However, there is no indication of any association here and the effect of entry rates on student performance is almost unchanged. Column (5) includes lagged school-by-subject KS4 achievement, as a proxy for unobservables that are correlated with past performance. Doing so again makes little difference to the magnitude or statistical significance of the effect of teacher entry. Column (6) further checks the robustness of the estimates by controlling for school-subject group specific linear trends to partial out trends in mobility and performance in these groups. This very demanding specification makes little difference to the estimates of the effects of entry rates.

3.6.2 Student exposure to teacher entry

As noted in Section 3.5, our main measure of teacher entry captures entry into school departments as a whole, rather than into the year groups (10 and 11) specifically relevant for KS4 study. This avoids endogeneity issues posed by strategic selection of teachers into 'low-risk' year groups, but masks potentially informative patterns related to timing of entry. To address this issue, Column (7) of Table 3.2 uses, instead, a more refined measure of turnover in which we define entry rates by the share of hours taught by incoming teachers in different year groups (Year 10 or Year 11).[11]

We report three different entry effects based on this hours-based entry rate definition: the effects of entry to Year 11 teaching on the current Year 11 cohort's GCSE results; the effects of entry to current Year 10 teaching on the current Year 11 cohort's GCSE results; and the effects of entry to Year 10 teaching in the previous academic year when the current Year 11 cohort was in Year 10. What matters in these specifications is entry rates in Year 11, when students are in their final examination year. The effect is slightly larger (-0.064, s.e. 0.024) in magnitude than our baseline estimates, although statistically comparable. The sample size is much smaller as data on subject teaching hours is only available for the 2011-2013 period. Note however, that our baseline specification estimated on this smaller sample gives a similar coefficient to that from our main sample, around -0.05. The implication of this result is that there is little loss from using department-wide entry rates, and if anything, our main results are overly conservative.

The zero-insignificant coefficient on Year 10 entry rates reinforces the 'placebo' tests of Columns (2) and (3): new teachers entering in a given academic year have no effect on GCSE results if they are not actually teaching the students taking these exams. The coefficient on entry into Year 10 when students taking GCSEs

[11] This refined measure also takes account of the hours teachers who teach multiple subjects spend teaching each subject so also acts as a test of robustness to misallocation of teachers to subjects.

were actually in Year 10 is negative, but also small and insignificant. This has two possible interpretations: either turnover in Year 10 doesn't impact on students, because it comes at the beginning of teaching on their GCSE course programmes, so it involves no disruption to continuity in teaching; or it has an impact on student performance, but there is little or no persistence in the effects of teacher turnover across years. With no recorded information on Year 10 achievement we are unable to distinguish between these hypotheses.

3.6.3 Event study estimates

Expanding and refining the placebo tests of Column (2) in Table 3.2, we develop an 'event study' style of analysis. In particular, we want to assess whether entry rate in years before of after the year under scrutiny, have an effect on student achievement. While future turnover acts as placebo check, lags of turnover can provide evidence of the persistence of the effect of turnover over time. We implement this analysis by estimating the following equation:

$$Q_{isqt} = \beta_1 Entry_{sqt} + \beta_2 Entry_{s,q,t-j} + x_i\delta + z_{sqt}\gamma + a_{qt} + b_{st} + c_{qs} + \varepsilon_{isqt} \quad (3.3)$$

Where β_2 represents the effect of past or future turnover on the grade in year t with j that goes from -2 to +2. Positive values for j correspond to entry in previous years while negative values correspond to values for future entry. To minimize the loss of observations, we estimate the equation for each lead and lag separately. Figure 3.1 reports the estimates β_1 (circles) and β_2 (triangles) together with their 95% confidence interval. The horizontal axis indicates the lag order. As a consequence, the estimates corresponding to the lead 2 (j=-2) replicate estimates reported in Column (2) of Table 3.2. In line with the results tabulated above, the effect of turnover in the current year is always negative with a magnitude close to -0.05. Evidently, the effects of the lags and leads are never large or significant.

The small size and statistical insignificance of the leads' coefficients show, as we would expect, that teachers entering after a students' KS4 exams have no impact on their exam performance, thus providing a useful placebo test. As discussed in Section 3.3, this finding rules out the possibility that entry rates are explained by a shock to a school department performance in year t-1 causing exits in year t-1, entry in year t and low performance in year t. This would imply that entry in year t would also be correlated with performance in year t-1, whereas the estimated coefficient is only 0.01 and statistically insignificant.

Looking at coefficients of lags, the past entry rates – corresponding to entry rates in the school-subject group when the student was in grades Year 10, Year 9, and so on - also have no effect on KS4 performance. This shows that the effect of entry has a minimal persistence: already in the year following entry, there is no residual effect of the disruption caused by the arrival of new teachers.

These results justify our focus on entry rates in the year of the KS4 qualifications and suggest there is no need to consider cumulative entry over the whole of a student's preceding years of secondary education.

Results in Figure 3.1 also suggest that changes in teachers' quality are not driving our results. If a new teacher was, say, of lower quality, we would expect a persistent negative effect of turnover. However, this is not the case as the figure shows.

3.7 Heterogeneity and mechanism

So far, the analysis focused on the identification of the average effect of turnover on test scores and we have demonstrated that, conditional on our fixed effects, entry rates are exogenous, in the sense that they are uncorrelated with incumbent teacher, school and student characteristics, and school-subject specific shocks. Nevertheless, it is still possible that the effects of entry we have estimated arise because incoming teachers are different from the incumbent teachers, causing changes in the average quality of the workforce at the school-subject level. It is also possible that the magnitude of the disruptive effects of new teachers is heterogeneous along a number of dimensions. Firstly, the amount of disruption may depend directly on incoming teachers' skills and experience, irrespective of whether they differ from the incumbent workforce, or the skills of incomers may interact with those in the incumbent workforce. Allen [2017], for example, highlights the potential costs imposed on students in schools that take on large numbers of newly qualified teachers. Secondly, the magnitudes may depend on school organisation and how incoming teachers are allocated to different year groups. As noted in the Introduction, our 'intent to treat' estimates, based on teacher entry rates into the school-subject as a whole, might underestimate the causal effects of a new teacher on a student if new teachers are allocated to grades other than the one for which we measure KS4 outcomes. In this section we investigate these heterogeneous effects of turnover, and the role of teacher allocation and school organisation in mitigating the effects of turnover.

3.7.1 The role of entrant teacher characteristics

In order to explore the effect of entry of teachers with different characteristics, we repeat our preferred fixed effect specification from Table 3.1, Column (6), but split the entry rate into different components according to incoming teacher characteristics. Table 3.3 presents the results of this regression. The coefficients for the different groups of teachers are all of a similar order of magnitude, indicating that all groups cause disruption. However, the patterns point towards senior teachers causing more disruption: the coefficients increase with age, salary and experience (up to the 3rd category), and the negative effect from being taught by incoming teachers from outside the profession is lower than the effect from those moving between schools. Gender differences also play a role, with entry of male teachers having a much more detrimental effect than female teachers. We also examined whether teachers coming from better/worse schools, based on school-subject specific scores in the KS4 exam in the previous year, had less/more disruptive impacts,[12] but found no difference. Additional regressions in which we interact entry rates with the characteristics of incumbent teachers also revealed no strongly significant interactions or systematic patterns, so we do not report them here.[13]

The finding that less experienced entrants cause less disruption than older and more experienced ones, requires some investigation, as it runs counter to expectations and some previous literature. There are potential behavioural explanations, such as younger teachers being more adaptable, but the results of Table 3.4 point to another explanation. This table reports an analysis based on teacher-level data from the School Workforce Census between 2011-2013, in which we regress an indicator that the teacher teaches Year 11 students on teacher characteristics, a new entrant indicator, and an interaction of the entrant indicator with some specific teacher characteristics. The Table, thus, shows the probability that a new entrant of a specific type teaches Year 11, compared to a baseline incumbent teacher.[14] The Table is organised in a similar fashion to Table 3.3.

The first thing to note from Column 1 is that new entrants are less likely to teach Year 11, than incumbent teachers. This finding explains why, in Table 3.2, the coefficient on Year 11 entry rates was slightly larger than our baseline estimates that use entry in all grades: new teachers are allocated to 'lower risk' grades or do not teach, so our 'intent to treat' estimate underestimates the effect of the treatment on the treated students to whom a teacher is assigned. Note however that a high proportion of new entrants do teach in Year 11: the probability of

[12] A teacher defined as coming from a better school if the average grades in the origin school-subject cell in (t-1) were higher than grades attained in the destination school-subject in (t).

[13] We also looked at the effects of exit rates in these groups. KS4 attainment generally has no association with exit rates, unconditional on entry, though we find positive associations with exit of the lowest paid teachers (bottom quartile) and those with the most experience (10 years +). If we control for both entry and exit, the effects of entry become around 50% bigger and the differences across incoming teacher types less marked. The coefficients on exit, conditional on entry are generally positive, but show no systematic patterns across incoming teacher types. As noted in the text, we do not trust these exit rates results because of their inherent endogeneity.

[14] For this analysis, we use as a baseline a teacher in his/her second year at the school.

teaching Year 11 amongst all teachers is 72%, and incoming teachers are around 6.7 percentage points[15] less likely to teach Year 11 than incumbent teachers.[16]

When we look at differences in assignment to Year 11 across different types of incoming teacher, we see patterns that can also, at least partly, explain the differences seen in Table 3.3. While all entrant groups are less likely to teach Year 11 than incumbents, the least experienced and lower salaried entrants are much less likely to teach Year 11 than more experienced and higher salaried entrants. Male entrants are more likely than incumbents and female entrants to teach Year 11, which may explain why their entry appeared more disruptive to KS4 performance. Similarly, entrants coming from origins other than other schools (which will mean teachers new to the profession, predominantly), are much less likely to teach Year 11 and less likely to affect KS4 scores.

Overall, the results of Table 3.3 and Table 3.4 suggest that schools can and do take steps to mitigate the effects of new teachers on the high-stakes KS4 qualifications by not assigning them to the high-risk grade in which students take these exams.

3.7.2 School quality

The observation that new teachers tend to be re-assigned out of the high-risk grade, Year 11, raises questions about the role of school organisational quality in mitigating the adverse effects of entrants on students. Table 3.5 and Table 3.6 examine this issue, in a similar way to Table 3.3 and Table 3.4, firstly looking at heterogeneity in the effect of entry by indicators of school quality, and secondly investigating how entrant teachers are assigned across grades. Indicators of school quality are based on external inspections by the schools' regulator in England, Ofsted. Ratings are based on a combination of self-evaluation reports by the school and site visits by inspectors, involving meetings with staff, students,

[15] It should be noted that even when new teachers teach, they tend to teach less hours (about 0.5 less on a baseline of 3.8). Results are available upon request.

[16] New entrants are also less likely to teach altogether by 2.2% out of a baseline probability of 86%.

governors and parents. The inspection results in rating of a school's overall performance and organisation as either Outstanding, Good, Requires Improvement, or Inadequate. A school receives the Outstanding rating if it is judged Outstanding on all dimensions that are inspected, including effectiveness of leadership and management.

Looking across Table 3.5, it is evident that students in schools judged as Outstanding are markedly less affected by entrant teachers – the point estimate is half than that for other schools, and statistically insignificant. The point estimate for Good schools is also smaller than those for schools that Require Improvement or are Inadequate, although the differences are not statistically significant. While a number of factors could explain this pattern, Table 3.6 suggests that assignment of entrant teachers to grades other than Year 11 is a contributory factor. Outstanding and Good schools are, somewhat, more likely than schools rated Inadequate or Requires Improvement to assign new teachers to other grades. Other factors are evidently at work though, since Good schools are as more likely to assign new teachers outside Year 11 as are Outstanding schools, and yet their students are much more affected by teacher entry.

3.7.3 Students

Table 3.7 investigates the heterogeneity of the effect of turnover for different groups of students. The regression results are based on our usual preferred fixed effects specification, separately estimated for different groups, with Column (1) repeating the results from Table 3.1, for comparison. As results show, in most of the cases, standard errors are too large to draw definitive conclusions. However, we can see qualitative pattern: students most affected by teacher turnover appear to be the ones from more disadvantaged backgrounds[17] (Columns 2 and 3), male students (Columns 4 and 5), students from ethnic minorities (Columns 6 and 7), and those in the lower quartile of the primary school (KS2) grade distribution[18]

[17]Proxied by free school meal eligibility.
[18]Primary school exam.

(Columns 8 and 9). The difference in the effect between boys and girls is in line with a recurrent theme in the educational literature, where boys generally seem to come off worse (see Gibbons et al., 2017, for example). Disruption from teacher turnover appears to be one contributory factor (albeit a small one) to the gender gap between boys and girls in England's schools. The largest difference is between students in the top quartile (Column 7) and in the bottom quartile (Column 8) of the KS2 grade distribution. More vulnerable students seem to be more affected by disruption induced by teacher turnover as they might be less able to make up for program disruption with additional family resources and independent effort.

These findings suggest that teacher turnover might, at least in part, contribute to the difference in achievement between disadvantaged and not disadvantaged students.

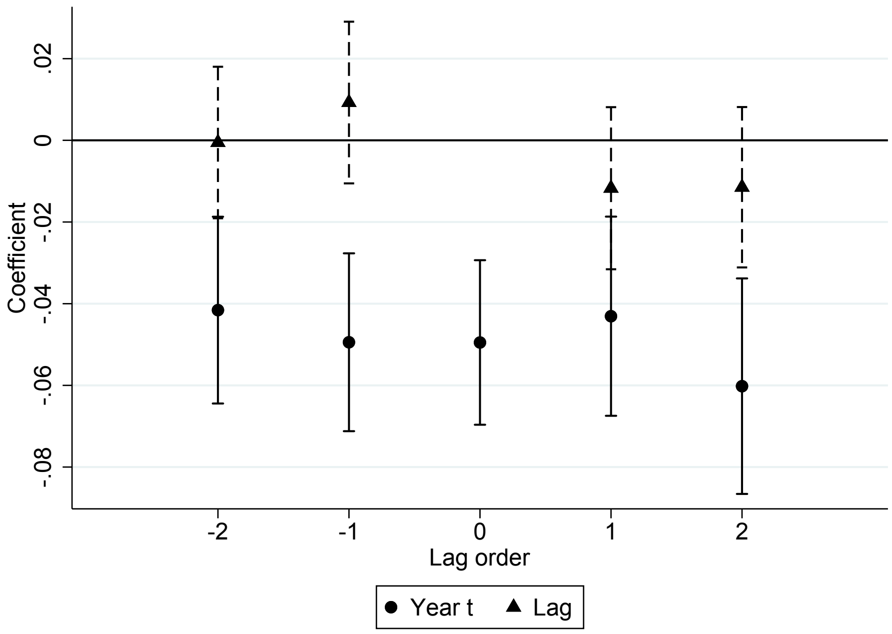
3.8 Conclusion

Our study investigates the impact of teacher entry rates at school-subject-year level on student achievement in England using fixed effects regression designs which control carefully for unobserved school-by-year, subject-by-year shocks and school-by-subject or individual unobservables. The key finding is that students in the final year of compulsory secondary school score less well in their final assessments if they are exposed to higher rates of teacher entry in the subjects they are studying. Entry in the final year in which students take their final GCSE assessments seems crucially important, implying that disruption to final qualifications from new teachers could be minimised by assigning them to year groups with less high-stakes assessment. The magnitudes are, however, quite small, with a 10 percentage point increase in entry rates reducing scores in final qualifications by just under 0.5 percent of a standard deviation. This figure is almost exactly the same as that found for entry of teachers in schools in the US (e.g. Ronfeldt et al., 2013, Hanushek et al., 2016). This suggests that the effects are potentially quite general and not dependent on context. In addition, the magnitude of the impact is economically meaningful compared to many other educational interventions. For instance, the literature on teacher quality suggests that a one standard deviation increase in overall teacher quality – where 'quality' means everything about teachers that is correlated with persistently higher value-added scores – only raises individual student achievement by around 0.11 standard deviations (see for example Hanushek, 2009). Our standardised effect is about 0.8 percent of one standard deviation from a one standard deviation increase in entry rates, so clearly considerably smaller than this, though not negligible, and comparable or larger to the estimates of other forms of educational externality in student groups.

In contrast to Hanushek et al. [2016], we find that the adverse effects of entry do not appear to be related to changes in workforce composition and entry of less experienced teachers. The effects are quite general across entrant teachers with different levels of seniority, in age, experience and salary, and insensitive to controls for workforce composition. The observation that less experienced entrant teachers have no bigger impact than more experienced teachers is partly, but not completely, explained by the fact that schools tend to allocate new teachers outside the high risk grade Year 11 when students take their final exams, and are more likely to do so for younger less senior teachers with less experience and lower salaries. Evidently, schools are able to partly mitigate the impact of turnover by the way they organise teaching, implying that our estimates potentially underestimate (in absolute value) the causal impact in a situation where new teachers were randomly assigned to students. Even so, turnover of teachers matters regardless of teacher seniority and these organisational responses. This implies that the results are likely driven by unavoidable general disruption and lack of continuity in teaching due to new teacher entry.

Figures

Figure 3.1: Effect of teacher turnover on student outcome with leads and lags



Note: Figure plots coefficient of the effect of teacher entry rate on students' standardized KS4 grade with our preferred specification. The graph reports coefficients for Equation 3.3 for values of j between -2 and 2. Each couple of dots and triangles represent an estimation of the equation. Circles report the effect of entry in year t while triangles represent the effect of leads (negative lag order) and lags (positive lag order) of entry. Entry rate is defined as the share of teachers in year t who were not present in the school in year t-1. All equations include controls for teacher, student and school characteristics. Teacher characteristics include: average age of teacher in the department; average experience; share of female and average log salary. Student characteristics include: normalized prior test scores; Free School Meal (FSM) eligibility; gender; ethnicity (white/others). School characteristics include: pupil teacher ratio at department-school-year level; proportion of female students in the department; proportion of FSM eligible in the department; proportion of white students in the department; number of teacher in current and past academic year in the department. Sample includes years from 2009 to 2013. Standard Errors clustered at school level. Equation estimated separately for each lead and lag to minimize sample loss of sample size. Confidence interval at 95% reported.

Tables

Table 3.1: Baseline results for effect of teacher entry rates on KS4 point scores

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Entry rate	-0.100***	-0.243***	-0.050***	-0.075***	-0.031***	-0.049***	-0.029***	-0.041***
	(0.015)	(0.018)	(0.012)	(0.015)	(0.008)	(0.010)	(0.010)	(0.012)
Observations	12,654,691	12,654,691	12,654,691	12,654,691	12,654,691	12,654,691	12,654,691	12,654,691
Year FE	Y	Y	N	N	N	N	N	N
Controls	N	Y	N	Y	N	Y	N	Y
SchoolXYear FE	N	N	Y	Y	Y	Y	N	N
SubjectXYear FE	N	N	Y	Y	Y	Y	N	N
SchoolXSubject FE	N	N	N	N	Y	Y	Y	Y
Student FE	N	N	N	N	N	N	Y	Y

Note: OLS Regression at student level. The dependent variable is the average standardized test score in the KS4 exam by student, subject and year. Entry rate is defined as the share of teachers in year t who were not present in the school in year t-1. Controls include teacher, student and school characteristics. Teacher characteristics include: average age of teacher in the department; average experience; share of female and average log salary. Student characteristics include: normalized prior test scores; Free School Meal (FSM) eligibility; gender; ethnicity (white/others). School characteristics include: pupil teacher ratio at department-school-year level; proportion of female students in the department; proportion of FSM eligible in the department; proportion of white students in the department; number of teacher in current and past academic year in the department. Sample includes years from 2009 to 2013. Standard Errors clustered at school level. Level of significance: *** p<0.01, ** p<0.05, * p<0.1, ***.

Table 3.2: Robustness and placebo tests

	(1) Baseline	(2) Leads	(3) Other Subjects	(4) Exit	(5) Lag Grade	(6) Subject Trends	(7) Year Group Entry
Entry rate	-0.049***	-0.050***	-0.050***	-0.051***	-0.043***	-0.047***	
	(0.010)	(0.010)	(0.010)	(0.011)	(0.010)	(0.010)	
Lead 2 Entry Rate		-0.000					
		(0.010)					
Entry other Subjects			-0.006				
			(0.010)				
Exit Rate				-0.013			
				(0.009)			
Lag Avg KS4 Grade					0.216***		
					(0.008)		
Entry Rate YG11							-0.064***
							(0.025)
Entry Rate YG10							0.001
							(0.022)
Lag Entry Rate YG10							-0.008
							(0.016)
Observations	12,654,691	9,846,958	12,653,579	12,654,691	12,241,679	12,654,691	4,041,094
R-squared	0.465	0.488	0.465	0.464	0.467	0.465	0.467
Year FE	N	N	N	N	N	N	N
Controls	Y	Y	Y	Y	Y	Y	Y
SchoolXYear FE	Y	Y	Y	Y	Y	Y	Y
SubjectXYear FE	Y	Y	Y	Y	Y	Y	Y
SchoolXSubject FE	Y	Y	Y	Y	Y	Y	Y
SubjectXTrends	N	N	N	N	N	Y	N

Note: OLS Regression at student level. The dependent variable is the average standardized test score in the KS4 exam by student, subject and year. Entry rate is defined as the share of teachers in year t who were not present in the school in year t-1. Controls include teacher, student and school characteristics. Teacher characteristics include: average age of teacher in the department; average experience; share of female and average log salary. Student characteristics include: normalized prior test scores; Free School Meal (FSM) eligibility; gender; ethnicity (white/others). School characteristics include: pupil teacher ratio at department-school-year level; proportion of female students in the department; proportion of FSM eligible in the department; proportion of white students in the department; number of teacher in current and past academic year in the department. Column (3) adds entry in subjects not seated by the student. Column (4) adds the exit rate defined as the share of teachers in year t who will not be present in the school in the following year. Column (5) includes the lagged KS4 average score in the previous year in the schoolXsubject. Column (6) adds schoolXsubject linear trends. Column (7) measures turnover in as the share of hours by year group in KS4 taught by teachers not present in the school in year t-1. Sample in Column (7) is limited to years from 2011 to 2013 due to data limitations. Standard Errors clustered at school level. Level of significance: *** p<0.01, ** p<0.05, * p<0.1, ***.

Table 3.3: Heterogenity: effect of entry by composition of entrants

(1) Age		(2) Salary		(3) Experience		(4) Gender		(5) Origin	
20-29	-0.040**	Quartile 1	-0.038***	< 2 years	-0.040***	Female	-0.039***	Other schools	-0.056***
	(0.016)		(0.014)		(0.015)		(0.012)		(0.011)
30-39	-0.044***	Quartile 2	-0.060***	2-5 years	-0.041**	Male	-0.067***	Elsewhere	-0.036**
	(0.015)		(0.017)		(0.021)		(0.016)		(0.016)
40-49	-0.061***	Quartile 3	-0.063***	5-10 years	-0.057***				
	(0.018)		(0.018)		(0.019)				
50+	-0.077***	Quartile 4	-0.057**	> 10 years	-0.063***				
	(0.025)		(0.027)		(0.016)				
Observations	12,654,691		12,654,691		12,654,691		12,654,691		12,654,691
Year FE	N		N		N		N		N
Controls	Y		Y		Y		Y		Y
SchoolXYear FE	Y		Y		Y		Y		Y
SubjectXYear FE	Y		Y		Y		Y		Y
SchoolXSubj FE	Y		Y		Y		Y		Y

Note: OLS Regression at student level. The dependent variable is the average standardized test score in the KS4 exam by student, subject and year. Entry rate is defined as the share of teachers in year t who were not present in the school in year t-1. Controls include teacher, student and school characteristics. Teacher characteristics include: average age of teacher in the department; average experience; share of female and average log salary. Student characteristics include: normalized prior test scores; Free School Meal (FSM) eligibility; gender; ethnicity (white/others). School characteristics include: pupil teacher ratio at department-school-year level; proportion of female students in the department; proportion of FSM eligible in the department; proportion of white students in the department; number of teacher in current and past academic year in the department. Entry by category computed as the number of entrants in that category divided by the number of teachers in the department in t-1. Standard Errors clustered at school level. Level of significance: *** p<0.01, ** p<0.05, * p<0.1, ***.

Table 3.4: Heterogenity: probability of teaching year 11 by incoming teacher characteristics compared to incumbent teachers

(1) Entrant		(2) Age		(3) Salary		(4) Experience		(5) Gender		(6) Origin	
Entrant	-0.066***	20-29	-0.080***	Quartile 1	-0.153***	< 2 years	-0.189***	Female	-0.060***	Other schools	-0.063***
	(0.006)		(0.007)		(0.007)		(0.008)		(0.006)		(0.006)
		30-39	-0.043***	Quartile 2	-0.043***	2-5 years	-0.072***	Male	0.079***	Elsewhere	-0.187***
			(0.006)		(0.007)		(0.007)		(0.006)		(0.008)
		40-49	-0.062***	Quartile 3	-0.031***	5-10 years	-0.026***				
			(0.007)		(0.007)		(0.007)				
		50+	-0.106***	Quartile 4	-0.062***	> 10 years	-0.069***				
			(0.009)		(0.009)		(0.007)				
Observations	412585		412585		412585		412585		412585		412585
Mean Dependent	0.717		0.717		0.717		0.717		0.717		0.717
Year FE	N		N		N		N		N		
Controls	Y		Y		Y		Y		Y		
SchoolXYear FE	Y		Y		Y		Y		Y		
SubjectXYear FE	Y		Y		Y		Y		Y		
SchoolXSubj FE	Y		Y		Y		Y		Y		

Note: Linear probability model at teacher level. The dependent variable equal to one if the teacher teaches positive hours in the school in the academic year in year group 11 (grade of the final exam in secondary school). Controls at teacher and school level. Controls at teacher level include: age, sex, experience, tenure and salary of the teacher. Controls at school or department level include: last OFSTED report grade, ranking in quartile for 5+ A*-C in GCSE in the Performance Tables, dummies for core subjects, average normalized grade in KS2 for students in the department and pupil teacher ratio at department level. Core subjects are: English; Math; Science; History. Sample includes years between 2011 and 2013. Standard Errors clustered at school level. Level of significance: 0.1 *; 0.05 **; 0.01 ***.

Table 3.5: Heterogenity: effect of entry by school quality

	(1) Outstanding	(2) Good	(3) Requires Improvement	(4) Inadequate
Entry Overall	-0.0294	-0.0543***	-0.0610***	-0.0630
	(0.0185)	(0.0165)	(0.0211)	(0.0402)
Observations	2942967	5390165	3114523	986797
Year FE	N	N	N	N
Controls	Y	Y	Y	Y
SchoolXYear FE	Y	Y	Y	Y
SubjectXYear FE	Y	Y	Y	Y
SchoolXSubject FE	Y	Y	Y	Y

Note: OLS Regression at student level. The dependent variable is the average standardized test score in the KS4 exam by student, subject and year. Entry rate is defined as the share of teachers in year t who were not present in the school in year t-1. Controls include teacher, student and school characteristics. Teacher characteristics include: average age of teacher in the department; average experience; share of female and average log salary. Student characteristics include: normalized prior test scores; Free School Meal (FSM) eligibility; gender; ethnicity (white/others). School characteristics include: pupil teacher ratio at department-school-year level; proportion of female students in the department; proportion of FSM eligible in the department; proportion of white students in the department; number of teacher in current and past academic year in the department. Entry by category computed as the number of entrants in that category divided by the number of teachers in the department in t-1. Standard Errors clustered at school level. Level of significance: *** p<0.01, ** p<0.05, * p<0.1, ***.

Table 3.6: Heterogeneity by school quality for probability of teaching: OFSTED report

	(1) Outstanding	(2) Good	(3) Requires Improvement	(4) Inadequate
Entrant	-0.066***	-0.080***	-0.045***	-0.045**
	(0.012)	(0.009)	(0.012)	(0.019)
Observations	106385	176558	95225	34417
Mean	0.724	0.723	0.715	0.722
Year FE	N	N	N	N
Controls	Y	Y	Y	Y
SchoolXYear FE	Y	Y	Y	Y
SubjectXYear FE	Y	Y	Y	Y
SchoolXSubject FE	Y	Y	Y	Y

Note: Linear probability model at teacher level. The dependent variable equal to one if the teacher teaches positive hours in the school in the academic year in year group 11 (grade of the final exam in secondary school). Controls at teacher and school level. Controls at teacher level include: age, sex, experience, tenure and salary of the teacher. Controls at school or department level include: last OFSTED report grade, ranking in quartile for 5+ A*-C in GCSE in the Performance Tables, dummies for core subjects, average normalized grade in KS2 for students in the department and pupil teacher ratio at department level. Core subjects are: English; Math; Science; History. Sample includes years between 2011 and 2013. Standard Errors clustered at school level. Level of significance: 0.1 *; 0.05 **; 0.01 ***.

Table 3.7: Heterogeneity by student characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
VARIABLES	Baseline	FSM eligible	Non-FSM eligible	Female	Male	Not White	White	Top Quality	Bottom Quality
Entry Rate	-0.049***	-0.057***	-0.049***	-0.040***	-0.058***	-0.069***	-0.046***	-0.038***	-0.064***
	(0.010)	(0.019)	(0.010)	(0.011)	(0.012)	(0.018)	(0.011)	(0.010)	(0.014)
Observations	12659601	1376235	11283366	6409915	6249686	2141080	10518521	3543423	2812789
Year FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
SchoolXYear FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
SubjectXYear FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
SubjectXSchool FE	Y	Y	Y	Y	Y	Y	Y	Y	Y
Student FE	N	N	N	N	N	N	N	N	N

Note: OLS Regression at student, department and year level. The dependent variable is the average standardized test score in the KS4 exam by student, subject and year. Entry rate is defined as the share of teachers in year t who were not present in the school in year t-1. Controls include teacher, student and school characteristics. Teacher characteristics include: average age of teacher in the department; average experience; share of female and average log salary. Student characteristics include: normalized prior test scores; Free School Meal (FSM) eligibility; gender; ethnicity (white/others). School characteristics include: pupil teacher ratio at department-school-year level; proportion of female students in the department; proportion of FSM eligible in the department; proportion of white students in the department; number of teacher in current and past academic year in the department. Sample includes years from 2009 to 2013. Standard Errors clustered at school level. Level of significance: *** p<0.01, ** p<0.05, * p<0.1, ***.

Appendices

3.A Tables

Table 3.A.1: Teachers assigned to subjects by year

Subject	2009	2010	2011	2012	2013
Math	17,592	18,651	22,010	22,485	23,041
English	20,065	21,362	24,366	24,843	25,514
History	12,997	13,578	14,926	15,246	15,659
Science	24,757	26,119	28,934	29,321	29,663
Other Foreign Languages	11,599	12,001	13,427	13,582	13,770
Sports	13,593	14,379	15,985	15,917	16,058
Biology	1,341	1,419	1,535	1,639	1,887
Chemistry	1,062	1,132	1,342	1,464	1,611
Physics	1,134	1,202	1,382	1,516	1,585
Art	17,273	18,073	20,253	20,113	20,114
IT	6,645	7,167	8,765	8,544	8,171
Social Science	1,564	1,461	1,404	1,279	1,164
Design	5,690	5,981	6,937	6,749	6,591
Economics	7,242	7,638	8,615	8,437	8,430
Home Economics	3,095	3,254	4,567	4,518	4,390
Media	1,004	1,071	1,443	1,445	1,403
Humanities	2,438	2,623	3,726	3,591	3,439
Engineering	852	895	1,117	1,162	1,132
Total	149,943	158,006	180,734	181,851	183,622

Source: DTR up to 2010 and School Workforce Census from 2011 onwards.

Table 3.A.2: Summary statistics

Variable	Mean	Std. Dev.	Min	Max
Turnover measures at school-subject-year level				
Entry Overall	0.140	0.165	0	1
Exit Overall	0.105	0.151	0	1
Entry School	0.084	0.132	0	1
Exit School	0.077	0.132	0	1
Entry Profession	0.056	0.103	0	1
Exit Profession	0.028	0.078	0	1
Teacher characteristics				
Female	0.622	0.262	0	1
Age	39.904	5.698	20.75	72
Tenure School	6.923	2.848	1	20
Student characteristics				
KS4 Standardized Score	0.009	0.988	-1.694	10.124
KS2 Standardized Score	0.001	0.999	-4.331	2.204
School/school-subject group variables				
# Teachers	70.565	24.171	2	170
% FSM students	0.130	0.109	0	0.757
% Female students	0.500	0.175	0	1
% White students	0.815	0.230	0	1
Pupil Teacher Ratio	16.905	4.204	0.744	316
% Teachers between 20-29	0.197	0.194	0	1
% Teachers between 30-39	0.332	0.229	0	1
% Teachers between 40-49	0.242	0.214	0	1
% Teachers over 50	0.229	0.221	0	1

Note: Summary statistics at student level. Number of observations: 12,654,691.

Table 3.A.3: Teacher turnover and student characteristics

	No Fixed Effects			Fixed Effects		
Variable	Beta	S.E.	T-Stat	Beta	S.E.	T-Stat
	Contemporaneous variables					
Standardized score in KS2	-0.102	0.013	**-8.097**	-0.002	0.003	-0.548
Proportion White	-0.086	0.008	**-10.828**	-0.001	0.001	-1.693
Proportion Female	0.005	0.006	0.888	-0.001	0.001	-1.105
Proportion FSM	0.026	0.004	**7.461**	0.000	0.001	0.775
	Lagged variables					
Lag mean KS2 Standardized Score	-0.183	0.021	**-8.86**	-0.022	0.012	-1.75
Lag Proportion of White Student	-0.160	0.011	**-14.681**	0.000	0.001	0.110
Lag Proportion of Female Students	0.002	0.006	0.330	-0.002	0.001	-1.284
Lag Proportion of FSM Students	0.025	0.004	**7.024**	0.000	0.001	0.026

Note: Regressions of listed variables on entry rate in Columns (1) to (3) and on entry rate and fixed effects at school-year, department-year and school-department in Columns (4) to (6). Fixed effects include school by year, subject by school and subject by year fixed effects. Standard errors clustered at school level. Number of observations: 12,654,691.

Table 3.A.4: Teacher turnover and experience

	(1) Linear	(2) Cubic	(3) Groups	(5) Tenure
Entry rate	-0.049***	-0.051***	-0.048***	-0.051***
	(0.010)	(0.010)	(0.010)	(0.011)
Observations	12,654,691	12,654,691	12,654,691	12,654,691
Year FE	N	N	N	N
Controls	Y	Y	Y	Y
SchoolXYear FE	Y	Y	Y	Y
SubjectXYear FE	Y	Y	Y	Y
SchoolXSubj FE	Y	Y	Y	Y
Student FE	N	N	N	N

Note: OLS Regression at student level. The dependent variable is the average standardized test score in the KS4 exam by student, subject and year. Entry rate is defined as the share of teachers in year t who were not present in the school in year t-1. Controls include teacher, student and school characteristics. Teacher characteristics include: average age of teacher in the department; average experience; share of female and average log salary. Student characteristics include: normalized prior test scores; Free School Meal (FSM) eligibility; gender; ethnicity (white/others). School characteristics include: pupil teacher ratio at department-school-year level; proportion of female students in the department; proportion of FSM eligible in the department; proportion of white students in the department; number of teacher in current and past academic year in the department. Column (1) controls linearly for average experience in the department; Column (2) controls for a cubic polynomial; Column (3) controls for the share of teachers in different experience groups (2 years; between 2 and 5 years; between 5 and 10 years; more than 10 years); Column (4) controls linearly for school tenure. Sample includes years from 2009 to 2013. Standard Errors clustered at school level. Level of significance: *** p<0.01, ** p<0.05, * p<0.1, ***.

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