DISCUSSION PAPER SERIES

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ABSTRACT

Timed to Say Goodbye: Does Unemployment Benefit Eligibility Affect Worker Layoffs?*

We study how unemployment benefit eligibility affects the layoff exit rate by exploiting quasi-experimental variation in eligibility rules in Italy. By using a difference-indifferences estimator, we find an instantaneous increase of about 12% in the layoff probability when unemployment benefit eligibility is attained, which persists for about 16 weeks. These findings are robust to different identifying assumptions and are mostly driven by jobs started after the onset of the Great Recession, in the South and for small firms. We argue that the moral hazard from the employer's side is the main force driving these layoffs.

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1 Introduction

The main purpose of unemployment insurance (UI) is to provide income support during unemployment in case of job loss. The design of UI, however, entails a trade-off between insurance and incentives. This may lead to a moral hazard problem, which induces the insured unemployed to search less intensively. The empirical literature has extensively analysed the effect of unemployment benefits (UBs) on unemployment spell duration (Card and Levine, 2000; Lalive et al., 2006; van Ours and Vodopivec, 2006; Lalive, 2008; Tatsiramos and van Ours, 2014; Schmieder and von Wachter, 2016). The results generally show that the generosity of UB prolongs unemployment duration, suggesting that the insured unemployed may behave opportunistically while searching for jobs.

Beyond its well-known effects during unemployment, UI may induce a moral hazard behaviour in both employers and employees, which can alter job separation rates. On the demand side, firms may have an incentive to exploit the UI system to adjust their workforce in case of negative demand shocks (Feldstein, 1976). On the supply side, workers may have a preference for leisure combined with UB compensation. In this case, they have an incentive to work just long enough to attain UB eligibility and then exit employment. Understanding the relative importance of these two behaviours is fundamental as it entails different policy implications. While employees' moral hazard may imply reduced UI generosity, employers' moral hazard could support the introduction of firing taxes to prevent excessive layoffs (Zweimüller, 2018).

In this paper, we aim to estimate the causal effect of UB eligibility on layoff probability and to provide some insight on the role of the employer and employee's moral hazard. Our contribution to the literature is threefold.

First, we bring new evidence by exploiting quasi-experimental variation in UB eligibility conditions in Italy.[1] The analysis relies on an inflow sample of more than 400,000 new jobs drawn from administrative registries covering the period of 2005 to 2012, which we follow until job separation. We identify the impact of attaining UB eligibility by exploiting two eligibility conditions in the Italian UI system: i) at least 52 working weeks in the last two calendar years and ii) at least one day of work *before* this two-year horizon. Identifying the effect of UI eligibility is challenging since it is confounded by the effect of work experience accumulated along the job spell. To control for this confounder, we add a control group of workers who cannot attain UB eligibility despite accumulating the same level of work experience over the last two years. This extends the standard before-and-after analysis previously implemented in the literature in the same spirit of a difference-in-differences (DiD) estimator.

Second, we provide new insights on the relative importance of moral hazard on the firm's

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[1] In contrast to the empirical literature on unemployment duration, the effects of UI on layoffs have received little attention. An old literature focused on the impact of the UI system on the layoff rate in the United States (Feldstein, 1976, 1978; Saffer, 1983; Topel, 1984, 1983; Anderson and Meyer, 1993). Within the more recent empirical literature, only two papers have studied the impact of UI on the probability of layoff: Rebollo-Sanz (2012) for Spain and Light and Omori (2004) for the United States. See Section 2 for more details.

and the worker's side by exploiting the large sample of new jobs in two ways. First, we estimate heterogeneous effects before and after 2008, the onset of the Great Recession, when the forces at work determining the incentives to engage in opportunistic behaviour may have changed. On the one hand, in downturns, employers may be more tempted to exploit the UI system in order to reduce their firing costs. On the other hand, workers should have lower incentives to shirk because in bad times it is more difficult to find a new job in case of layoff. Second, we try to understand the role of employers' firing costs. In Italy during the period of the analysis, the employment protection legislation (EPL) on individual layoffs was more stringent for firms with more than 15 employees. Hence, smaller firms could more easily offer job packages that included the wage and a probability of being laid off (Zweimüller, 2018), thereby taking advantage of the UI system to adjust their workforce. From these contrasting expected behavioural changes, we shed new light on the two sides of moral hazard.

As a third contribution, we provide evidence that the impact of UB eligibility on layoffs in Italy is not homogeneous across regions, despite these being characterised by the same labour market institutions. We focus on a particular geographical dimension that is related to the puzzling and long-lasting North–South divide in Italy (e.g. Manacorda and Petrongolo, 2006) and is reflected in differences in socio-economic measures, social norms and the ability to cooperate (Banfield, 1958; Guiso et al., 2004; Bigoni et al., 2016, 2018). Our conjecture is that a low level of trust in the south of Italy may induce employees and employers to easily feel justified in adopting opportunistic behaviour and in being part of an implicit contract affected by moral hazard.[2]

Our results confirm the existence of moral hazard, which we argue to be demand-induced. According to our preferred specification, we find that immediately after reaching UI eligibility, the probability of layoff increases by 12% for about 16 weeks. The impact is significantly larger after the Great Recession, when the instantaneous increase is of 21%, while no effect is found before 2008. In the South, the effect peaks at 24% between nine and 16 weeks from UI eligibility, whereas in the rest of Italy, the overall effect is not significantly different from zero. The layoff rates in smaller firms also show a more pronounced reaction. Similar conclusions are reached if we rely on a regression discontinuity design (RDD) estimator identifying the effect around the 52 weeks eligibility threshold.

The paper is structured as follows. In Section 2, we present the theoretical setting and review the existing empirical literature. Section 3 presents the Italian institutional framework for the period under analysis. Section 4 describes the data and the sample. Section 5 shows some descriptive evidence and results based on a RDD framework. Section 6 presents our preferred model, a DiD design in a duration model, and interprets the results. Section 7 concludes.

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[2]Eugster et al. (2017) exploited the cultural differences across the Swiss language areas to study the impact of culture on job search behaviour. They found that unemployment duration is longer in Latin language areas compared to German-speaking ones. Eugster et al. (2011) found that Latin language areas have a higher preference for redistributive social insurance compared to German language areas.

2 Theoretical framework and existing evidence

In the standard Diamond–Mortensen–Pissarides model with endogenous job destruction, jobs are destroyed when an idiosyncratic shock decreases job productivity below a reservation level (Mortensen and Pissarides, 1994). According to this model, UBs improve the employee's outside option, which raises the reservation productivity threshold of the job and, therefore, the job separation probability. Specific supply and demand factors may affect the behaviour of the agents and create a moral hazard to exploit the UI system.

On the labour supply side, the positive shock on the outside option may induce the worker to reduce their exerted effort. Therefore, the more generous the UB compensation, the higher the probability for the worker of being fired or of inducing a layoff (Shapiro and Stiglitz, 1984; Jurajda, 2002). Furthermore, there may be cases in which a worker may prefer an intermittent working pattern in which periods of work, long enough to reach UB eligibility, are alternated with periods under UB compensation. Knowing this preference, it might be convenient for a firm to have a policy of only firing workers who qualify for UI. More workers will apply to this firm, knowing its reputation for timing its layoffs with UI eligibility. The firm will therefore reduce the costs of filling the vacancy and be able to choose candidates from a larger pool (Christofides and McKenna, 1996; Green and Sargent, 1998).

On the labour demand side, at least two arguments may explain a boost in dismissals. First, UB eligibility may attenuate firms' expected separation costs due to possible litigation disputes. In some countries, like Italy, judges can have significant discretionary power to determine if a layoff is legitimate. In this context, they might more often rule in favour of the firm if the worker is eligible for UB, lowering the expected firing costs for employees entitled to UB and creating an incentive to wait until UI eligibility is attained before firing. Second, according to implicit contract models, the job relationship between workers and firms relies on implicit contracts that take into account the wage and a positive probability of layoff due to future macroeconomic uncertainty. In the presence of a UI system, firms use temporary layoffs and the availability of UB compensation to adjust the workforce to macroeconomic conditions (Feldstein, 1976; Baily, 1977). In addition, based on a job search model with UI insurance, Jurajda (2003) shows that a firm's optimal layoff strategy when facing a cyclical downturn is to fire workers with generous UB entitlements. This is because the firm internalises the fact that these individuals will search less intensively and remain unemployed longer. Therefore, once economic conditions improve, the firms may recall them more easily.

Finally, UBs may generate collusive behaviour between employers and employees, in which they share the surplus of UBs by officially terminating the employment relationship but maintaining it off the books.

The empirical literature focusing on the impact of UB eligibility on employment duration has been quite limited.[3] Earlier studies for the United States in general showed that tempo-

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[3] A related literature studied UB provision as an alternative policy to early retirement (Baguelin and Remillon,

rary layoffs were more common when UI was not fully funded by experience rating (Feldstein, 1976, 1978; Saffer, 1983; Topel, 1984, 1983; Anderson and Meyer, 1993).[4] A few North American empirical studies exploited exogenous changes in the eligibility rules for UBs to study the impact of UB eligibility on employment duration by means of a DiD approach. Solon (1984) used the fact that since 1983, voluntary resignation conferred the right to UBs in some US states. Although his results were imprecise, they were compatible with the hypothesis that more stringent conditions for UBs reduce job quitting, supporting the moral hazard hypothesis. Green and Riddell (1997) and Baker and Rea (1998) studied the impact of a reform that changed the UB eligibility conditions in Canada in 1990. They found that UB eligibility increased the hazard rate out of employment. Finally, other studies investigated the effect of a 3-year extension of UBs targeted at older workers in Austria during the late 1980s. Based on DiD approaches, the authors estimated an increase of about 4–11 percentage points in the entry rate into unemployment (Winter-Ebmer, 2003) and a 28% increase in the job separation rate (Lalive et al., 2015).

Another strand of the literature estimated duration models to identify the impact of UB eligibility on employment duration without exploiting exogenous reforms of the UI system. For the United States, Jurajda (2002) considered the period of 1974–1979 (i.e. before the UI reform) and showed that the attaining UB eligibility decreased the duration of employment. Furthermore, by exploiting cross-state and cross-year variation in UB calculations, Light and Omori (2004) found that more generous UBs deterred workers from voluntary job quitting. For Canada, Christofides and McKenna (1995, 1996) and Green and Sargent (1998) showed that in the late 1980s, the employment exit rate increased after attaining UB eligibility, suggesting moral hazard as an explanation. A more recent paper focused on Europe. Rebollo-Sanz (2012) studied the effect of reaching UB eligibility on employment duration in Spain. The author compared the outcomes before and after reaching UB eligibility and found a positive effect of UB eligibility on layoffs, but not on job quitting. She focused on job episodes that had not yet met UB requirements before the start of the spell and that had different levels of accumulated working weeks at the beginning of the spell. The author controlled for total past experience, a baseline hazard rate for duration dependence and UB eligibility dummies. However, as UB eligibility is attained by accumulating work experience along the spell, the author could not separate the effect of UI from the experience effect, which remained a confounding factor. As explained in Section 6, compared to this evaluation strategy, we make a step forward and, in a DiD setting, disentangle the effect of work experience accumulated along the spell from the job duration dependence.

2014; Inderbitzin et al., 2016).

[4]In an experience-rated UI system, firms pay UI taxes that are proportional to their use of the UI system, which is an implicit tax on firing aiming to reduce excess layoffs.

3 Institutional set-up

In this section, we briefly describe the Italian institutional set-up of the UI system and the EPL. Both institutions can indeed interact and affect layoffs. Both the labour market institutions regulating the UI system and firing practices have changed in the last two decades. In what follows, we focus the discussion on the period under analysis, which goes from 2005 to 2012.

3.1 Firing system

In terms of the legislation of layoffs, Italy was historically characterised by significant rigidities. The EPL for open-ended contracts was, for example, one of the strictest among the OECD countries.[5] The highest source of rigidity was related to the dismissal of individual workers: the employer could only fire an employee if there was a fair reason. An individual dismissal was considered to be fair when it was motivated by situations referred to as *just cause*, *just objective motive* or *just subjective motive*.

- *Just cause* referred to cases of serious worker misconduct that impeded the development of a trustful labour relationship between both parties. Dismissal for just cause was a last resort solution and occurred without notice.[6]

- *Just objective motive* referred to economic reasons for termination. The employer had to prove that, due to economic reasons, (i) the company had to be reorganised and (ii) the employee could not be transferred to other functions within the company (not necessarily the functions the worker was hired for) or to other companies in the same group (Law 604/66).

- *Just subjective motive* referred to employee misconduct. This is similar to *just cause* but for less serious misconduct, and the layoff had to occur with notice.

In all cases, the burden of proof of the fairness of the layoff laid with the employer (Ballestrero, 2012). If the court declared that the dismissal was unfair, then workers in firms with more than 15 employees could be reinstated in the workplace and receive compensation equal to the remuneration foregone until reintegration with a minimum of five months of salary (Article 18, Law 300/1970). Workers in firms with less than 15 employees were not entitled to reintegration and the compensation was between 2.5 and six months. Hence, firms with more

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[5]According to the 2012 OECD indicator on the strictness of EPL, Italy ranked fourth after Portugal, the Czech Republic and the Netherlands.

[6]Examples of misconduct leading to a dismissal for just cause include abandoning the workplace if this harms the safety of people or the plant, unjustified absence from the workplace for multiple days, a false medical certificate, refusal to take up work again after sick leave, insubordination, having a second job whose interests are in conflict with the company's activities, defamation of the company, having committed a crime not related to the company but that could harm the company's reputation, theft of company holdings of substantial value and badge falsification.

than 15 employees dealt with larger expected firing costs.[7]

Labour disputes in Italy could take years before settlement. This implied uncertain and substantial costs for firms in case of loss. In 2006, the average duration of disputes for layoffs was 718 days for first instance trials and 646 days for second instance trials. Workers won in 2/3 and 1/3 of cases in first and second instance trials, respectively (ISTAT, 2008). High expected costs in case of loss and the discretionary power of labour courts to determine whether a dismissal was fair were a strong deterrent for employers looking to fire workers.

3.2 Unemployment insurance system

The Italian UI system sheltered private sector salaried workers who incurred an involuntary job loss or resigned for 'just cause'[8] and satisfied the following eligibility conditions related to one's previous employment history. Figure 1 provides a visual representation of the UB eligibility rules:

C.1. *'weeks requirement'*: the worker needs at least 52 full-time working weeks during the biennium before the end of the job spell (i.e. C.1 *biennium* in Figure 1);

C.2. *'experience requirement'*: the worker needs at least one day of work in the period *before* the C.1 *biennium* (C.2 *period* in Figure 1).

Figure 1: Unemployment Benefit (UB) Requirements
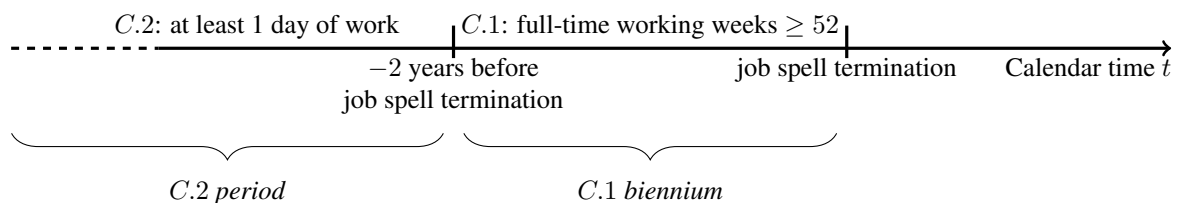
Following termination, the jobless workers satisfying C.1 and C.2 had to officially register their unemployment status at one of the local public employment offices in order to collect UB payments. UBs were provided for seven months up until 2007 and for eight months after 2007. The UB amount was related to one's gross remuneration in the three months before the job loss and was capped (i.e. €1014 in 2007). The replacement rate decreased along the unemployment spell. Until 2007, it was 50% during the first 6 months and dropped to 40% afterwards. After 2007, the replacement rate was increased by 10 percentage points (Law 247/2007).[9]

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[7]The regulation on dismissals was changed in January 2013 by Law 92/2012.

[8]Examples of resignation for 'just cause' are: mobbing, having suffered sexual harassment in the workplace, delayed or missed wage payments, deterioration in work tasks and being moved to a different establishment without organizational or technical reasons.

[9]For workers older than 50 years of age at job loss, before (after) 2008 the maximum duration was 10 (12) months, with a replacement rate of 30% (40%) from the 10th (9th) month.

Workers who did not fully qualify for UBs could be eligible for a reduced version, which were much less generous but were subject to looser criteria. These reduced UBs were paid all at once in the calendar year after the job loss. Until 2007, they covered the same number of days worked during the year before the job loss, with a maximum of five months and a (capped) replacement rate of 30%. From 2008, the maximum number of months was raised to six and the replacement rate increased to 35% for the first 120 days and 40% afterwards.

4   Data and sample

4.1   Data

We use administrative data from the social security registers of the Italian Social Security Institute (LoSai INPS). The overall sample available for research has a longitudinal structure up to 2015 and covers 6.5% of all salaried and semi-subordinate employees[10] working in the private sector. The data contains individual employment histories since 1985, unemployment benefit receipts from 1999 and other information on assimilated working weeks (e.g. sickness, maternity leave, military service, short-term compensation). The unit of observation is the single job contract. For each contract, the dataset provides information on the start and termination date, termination reason, location, firm sector, firm size, qualification and type of contract. It also contains worker characteristics such as gender and year of birth.

We selected a sample of fresh job spells starting between January 1st 2005 and December 31st 2011. We excluded job spells beginning prior to 2005 because the job starting date and the job termination reason were unavailable. We followed job spells until the end of 2012 because in 2013, labour market regulations changed. We excluded apprenticeships from our sample because apprentices were eligible for UBs only in special cases. We also exclude, contracts in agriculture due to the high seasonality of job relations in this sector and the specific UB rules. This selection resulted in an initial sample of 1,766,405 fresh job spells. The main outcome variable of interest in the analysis is job duration until separation, T, which is measured every two weeks from hiring. Job spells can terminate for different reasons. From the information in LoSai, we can distinguish among the three main causes of job exit: i) firm layoff; ii) employee's voluntary resignation; and iii) end of a temporary or seasonal job. If an individual experienced a job interruption of less than three weeks and then restarted working in the same firm, we consider the two jobs as the same uninterrupted spell. By doing so, contract transformations are considered as unique job spells.[11]   We denote by Z the random variable indicating the

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[10]Semi-subordinate employees are workers with contracts for temporary collaborations that are *de facto* subordinate to the employer but formally self-employed.

[11]This is especially important for the renewing of temporary contracts because in Italy, multiple renewals of temporary contracts were allowed if there was a waiting time of 20 (10) days between the end of the old and the beginning of the new contract when the duration of the old contract was (less) more than six months.

accumulated full-time working weeks in the $C.1$ biennium. This random variable is indexed as Z_t to indicate the accumulated working weeks in a particular elapsed duration t of the job spell.[12]

4.2 Treatment definition

At the start of the job spell ($t = 0$), we calculate the accumulated working weeks $Z_{t=0}$ during the initial $C.1$ biennium. Then, we update the value of Z_t at the end of each t until the end of the spell, with t ticking two weeks. The calculation of Z_t is based on the mobile biennium $C.1$, which moves along the spell duration. For a given t, the end of the mobile biennium coincides with the calendar time t periods after the start of the job. The beginning of the mobile biennium is calculated going back two years.

While requirement $C.1$ is standard in UI systems (although the exact numbers vary from country to country), requirement $C.2$ is an Italian peculiarity. If employers and/or employees time an opportunistic behaviour with UI eligibility, we expect to see an increase in the layoff probability when Z_t reaches 52 (i.e. when $C.1$ is satisfied) only for spells that also satisfy criterion $C.2$. We define these units as 'treated', while those not satisfying criterion $C.2$ are our 'controls', who cannot claim UB eligibility even when Z_t turns equal to 52. We define the treatment status at the beginning of the spell, i.e. when $t = 0$. Figure 2 clarifies the differences between the two groups by way of a calendar timeline: the only difference is in the employment history during the $C.2$ period: the treated group worked at least one day, whereas the controls had no days of work.

Figure 2: Treated and control group definitions

(a) Treated: satisfy $C.2$ but not $C.1$ at the job spell start

(b) Controls: satisfy neither $C.2$ nor $C.1$ at the job spell start

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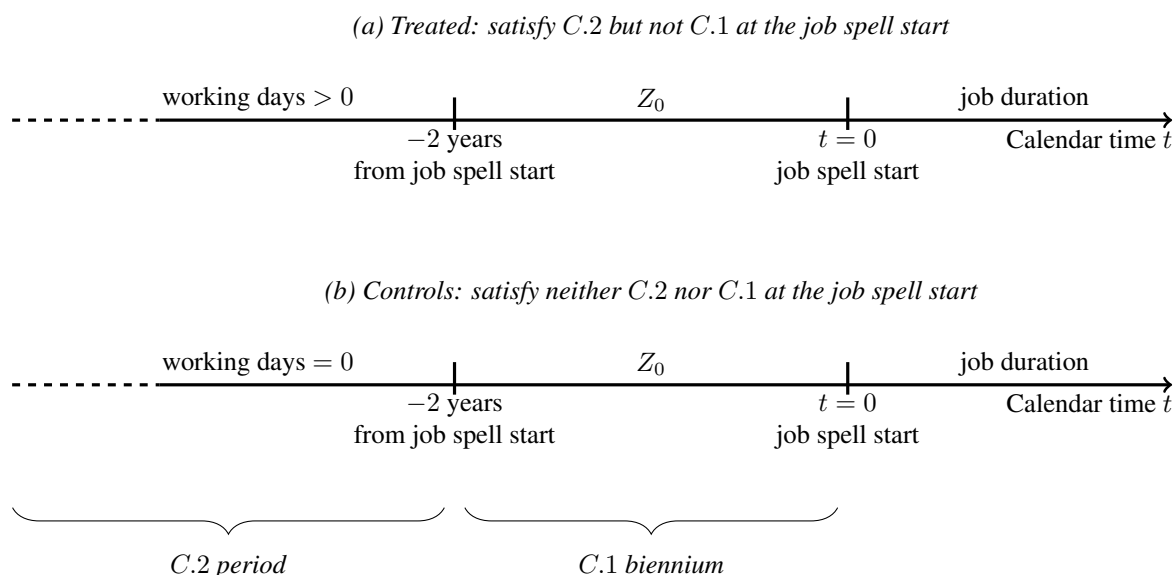
[12]In what follows, we express random variables in upper case and their particular realizations in lower case.

4.3 Sample selection

We further narrowed our sample as follows. First, we removed the jobs that satisfied UB eligibility requirement $C.1$ from the beginning of the spell (i.e. $Z_0 \geq 52$). This is because we want to observe how the job separation rate evolves before and after attaining UB eligibility. Second, we dropped the spells of individuals who, at the moment of hiring, had more than two years of past employment experience in the $C.2$ period. This selection criterion is to ensure that the treated and the controls are not too different in terms of past employment histories. Third, we dropped spells of workers older than 60 at the start of the job. Finally, we removed spells lying in the bottom or the top percentile of the hourly wage distribution and part-time jobs. The final sample is made up of 424,473 fresh job spells, translating into 6,110,657 observations (i.e. potential job terminations every two weeks). A total of 184,676 spells belong to treated group (43.5%), while 239,797 are controls.

Table 1 reports summary statistics on completed and uncompleted spells. We right-censored all of the spells still ongoing at the end of 2012 or surviving after 104 weeks of tenure (28,568 spells). The first right-censoring is due to the end of the observed time window. The second was applied because both $C.1$ and $C.2$ were always satisfied after 104 weeks of elapsed job duration and all spells would move to the treated group. Similarly, we right-censored a further 30,290 spells belonging to the control units as soon as they satisfied $C.2$, since otherwise they would shift to the treated group from that moment onwards. About 11.5% of the job spells ended because of layoff. This fraction is larger for the treated (13.2%) than for the controls (10.2%).

Table 1: Summary statistics of job spell durations by treatment status

	Total	Treated	Controls
Number of job spells			
Total	424,473	184,676	239,797
Completed due to layoff	48,636	24,293	24,343
Completed due to resignation or end of temporary contract	316,979	141,417	175,562
Right-censored on 31/12/2012 or at 104 weeks	28,568	18,966	9,602
Right-censored when controls become treated	30,290	0	30,290
Fraction of right-censored spells	0.139	0.103	0.166
Fraction of completed spells due to layoff	0.115	0.132	0.102
Fraction of completed spells due to resignation or end of temporary contract	0.747	0.766	0.732
Average job duration (weeks)	28.844	30.932	27.235
Duration percentiles (weeks)			
10th	4	4	4
25th	8	8	8
50th	18	18	16
75th	40	42	38

Figure C.1 in Appendix C shows the distribution of Z_0 for the treated and control groups. Both groups share very similar absolute frequencies at all values of Z_0 apart from zero, when it is higher for the control group. Table 2 reports summary statistics of the observables that we use

as covariates in the following analyses. We control for individual characteristics (age at spell start and gender), variables capturing past employment history (whether one already benefited of income support in the past and some employment features of the last year), characteristics of the job spell under analysis (contract type, firm size, location and calendar time of the job spell start) and the regional GDP growth rate (which varies over the spell).[13] The treated and control groups do not differ in many characteristics. The most notable differences are age (the treated are 3.8 years older, on average) and a variable work experience before hiring. These differences are to be expected as the treatment status depends on past employment history.

Table 2: Summary statistics of the covariates by treatment status

	Whole sample				Treated	Controls
	Mean	Std. Dev.	Min.	Max.	Mean	Mean
Individual characteristics						
Age at the start of the job spell (years)	28.428	8.274	15.000	60.000	30.558	26.787
Woman	0.332	0.471	0.000	1.000	0.320	0.341
Ever received income support of any type	0.054	0.227	0.000	1.000	0.120	0.004
Blue-collar job in calendar year before the start of the job spell	0.320	0.467	0.000	1.000	0.433	0.233
Employment contract in the calendar year before the start of the job spell						
Open-ended contract	0.127	0.333	0.000	1.000	0.176	0.089
Temporary contract	0.298	0.457	0.000	1.000	0.359	0.251
Seasonal employment	0.046	0.209	0.000	1.000	0.063	0.032
No employment	0.530	0.499	0.000	1.000	0.403	0.628
Characteristics of the job spell						
Firm size						
5 employees or less	0.277	0.447	0.000	1.000	0.288	0.268
Between 6 and 15 employees	0.187	0.390	0.000	1.000	0.189	0.186
Between 15 and 50 employees	0.168	0.374	0.000	1.000	0.169	0.167
Between 51 and 100 employees	0.188	0.391	0.000	1.000	0.186	0.190
More than 100 employees	0.180	0.384	0.000	1.000	0.167	0.190
Type of contract						
Open-ended	0.345	0.475	0.000	1.000	0.350	0.341
Temporary	0.601	0.490	0.000	1.000	0.591	0.609
Seasonal	0.054	0.225	0.000	1.000	0.058	0.050
Geographical area						
North-West	0.287	0.453	0.000	1.000	0.269	0.302
North-East	0.247	0.431	0.000	1.000	0.236	0.255
Centre	0.180	0.384	0.000	1.000	0.178	0.182
South	0.193	0.395	0.000	1.000	0.211	0.179
Islands	0.093	0.290	0.000	1.000	0.107	0.082
Year at the start of the spell						
2005	0.128	0.335	0.000	1.000	0.138	0.121
2006	0.144	0.351	0.000	1.000	0.155	0.135
2007	0.184	0.387	0.000	1.000	0.160	0.202
2008	0.164	0.370	0.000	1.000	0.135	0.186
2009	0.121	0.326	0.000	1.000	0.116	0.124
2010	0.131	0.337	0.000	1.000	0.144	0.121
2011	0.128	0.334	0.000	1.000	0.152	0.110
Month of the year at the start of the spell						
January–April	0.304	0.460	0.000	1.000	0.305	0.303
May–August	0.409	0.492	0.000	1.000	0.412	0.406
September–December	0.287	0.453	0.000	1.000	0.283	0.291
Time-varying covariate						
Regional yearly GDP growth rate at job spell start	0.003	0.041	-0.071	0.180	0.004	0.002
Number of job spells		424,473			184,676	239,797

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[13]The regional growth rate of the GDP varies on a yearly basis.

To get a better idea of how many individuals collected UBs, in Figure 3 we report the take-up rate of the standard UBs after the end of a job spell across the value of Z measured at the termination date. Graphs a) and c) focus on the treated, while graphs b) and d) are for the controls. The graphs at the top (a and b) are for job spells ending with a layoff. The graphs at the bottom (c and d) refer to job spells ending due to worker resignation. Three features are worth mentioning:

1. Graph a) shows a clear discontinuity in the UB take-up rate once the treated attain 52 weeks of working weeks in the last two years. The UB take-up rate is almost but not always exactly zero when $Z < 52$. This might be due to measurement error induced, for example, by an underestimation of Z for individuals with multiple jobs. In addition, as has also been observed in other countries (see e.g. Anderson and Meyer, 1997 for the US), the UB take-up rate does not jump to 1 once eligibility is attained. This might be due to, for example, the individual having moved to another job, self-employment, education, or not having officially registered the unemployment status at the public employment office.[14]

2. For the controls, the UB take-up rate after layoff does not jump at the cut-off (graph b). We can observe a very low fraction of the control group collecting UBs after layoff to the right of the cut-off. In principle, they should not collect UBs because they do not satisfy $C.2$. However, because we evaluated satisfaction of $C.2$ on the basis of data on past employment history, there may have been some marginal measurement errors in the construction of the administrative data.

3. Graphs c) and d) show that when job termination is due to the worker's voluntary resignation, it is very unlikely that the worker will collect UBs. For the treated group, very few job spells are followed by UBs. In some special cases, workers can indeed collect UBs to the extent that $C.1$ and $C.2$ are met and if the resignation was for 'just cause'; as mentioned in Section 3.

5   First evidence

We begin our analysis by providing suggestive evidence of the relationship between UB eligibility and firing probability. We then estimate the local impact of UB eligibility on the firing rate by means of a logit estimator and RDD analysis, which exploits the discontinuity in UB eligibility at the 52nd accumulated working week during the $C.1$ biennium.

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[14]A similar pattern is observed if we focus on temporary jobs terminated due to contract expiration (graphs available upon request).

Figure 3: UB take-up rate after job termination by treatment status and termination reasons
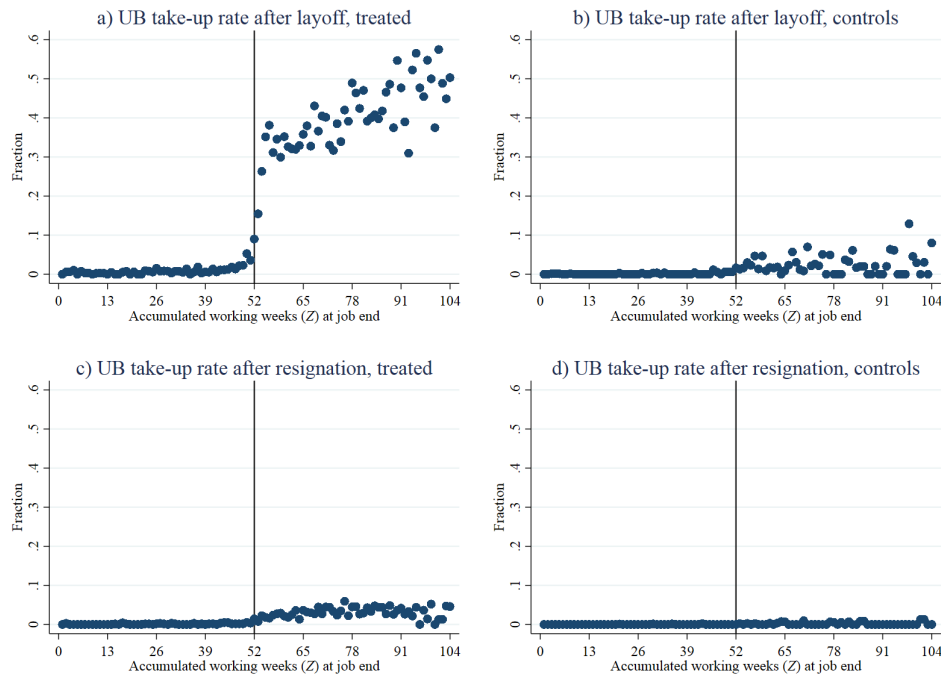
Notes: We plot the fraction of individuals collecting UBs after the end of the job spell by accumulated working weeks (Z) at job termination.

5.1 Descriptive evidence

Figure 4 reports the Kaplan–Meier hazard rate of job separation for the 424,473 spells by treatment status and reason of exit. Graph a) shows that both the treated and the controls have a very similar profile in the job separation rate when we do not consider exits due to layoff: at the beginning, the probability of job termination due to resignation or end of temporary/seasonal contract is quite large and peaks at about four months of job seniority, with the probability of leaving the job in a given interval of two weeks being about 3.6%. The peak is explained by the probationary periods and the typical duration of many temporary contracts. Afterward, the job hazard rate quickly decreases, declining to about 1%–1.4% at the 80th week of job seniority.

Several reasons can explain the decline in the hazard rate along the elapsed job duration: bad job matches are dissolved quickly, temporary contracts are typically not long-lasting and further heterogeneity generates selection over time and leaves only good matches in the sample. The treated group shows a similar job exit rate at the beginning of a spell but a much lower probability of exit once the first year has passed. Because treated individuals have more past work experience, a longer job duration is expected. However, the difference in the job hazard rate between the treated and the controls is reverted once we focus on the exits due to layoffs only. Graph b) of Figure 4 shows that the layoff hazard rate of the treated is well above that of the controls for most of the time, converging only when approaching the second year of job seniority. Furthermore, the difference in the layoff rate is more substantial for individuals

Figure 4: Smoothed Kaplan–Meier job hazard rate
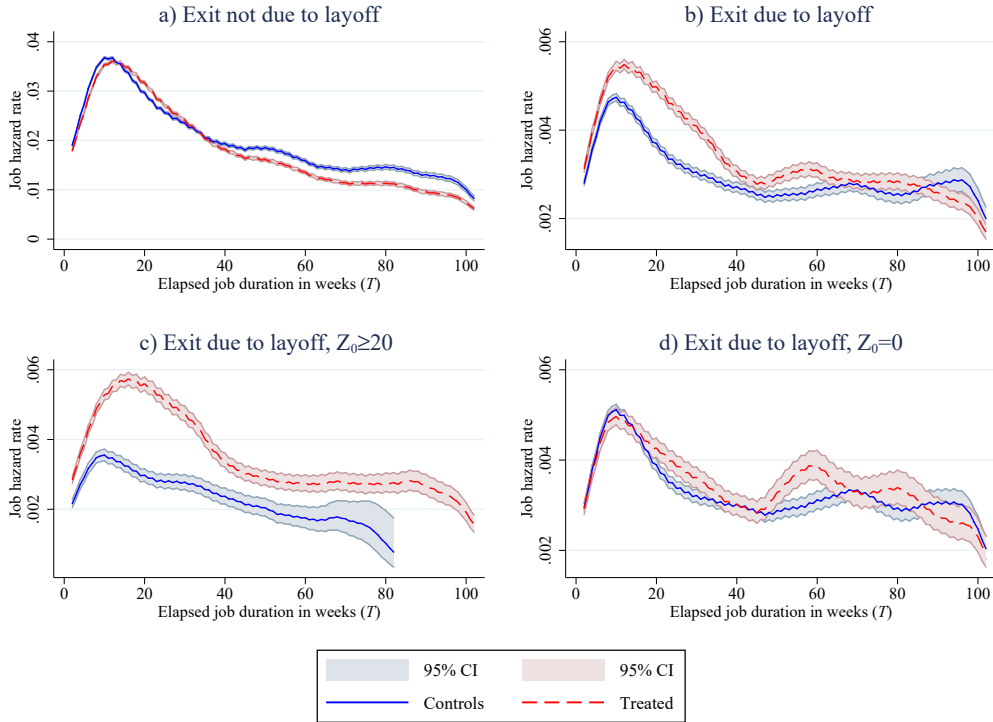
Notes: The elapsed job durations (T) are grouped into intervals of two weeks. The reported job hazard rates are therefore probabilities of leaving a job in a two-week interval, conditional on surviving until the beginning of that interval. The graphs are based on a weighted kernel smoothing of the estimated hazard rates (Epanechnikov kernel function with a half-width of four). In graph a), the focus is on jobs terminated because of resignation or the end of temporary/seasonal contract. Graphs b), c) and d) focus on layoffs. Graph c) considers only spells with $Z_0 \geq 20$, while graph d) includes only spells with $Z_0 = 0$. In all of the graphs, the job spells ending for the other reason are right-censored when this happens.

starting the spell with a high level of Z_0 (graph c), who are expected to attain UB eligibility earlier. Treated units with $Z_0 = 0$ (graph d) show a spike only after the 52nd week of elapsed job duration, which corresponds to the moment when they attain UI eligibility. This suggests that the eligibility attained by the treated during the spell might increase the chances of layoff.

5.2 Regression discontinuity design

In this subsection, we pooled over t the information on job exit and accumulated working weeks, resulting in 6,110,657 observations. Each job spell contributes to this dataset until either a job exit is observed or the spell is right-censored.

In a first analysis, we estimated pooled logit models with two different dependent variables: i) a dummy equal to 1 if the spell ended in the subsequent two weeks because of layoff; and ii) a dummy equal to 1 if the termination in the subsequent two weeks was due to resignation. We regressed these binary responses on the elapsed job duration t,[15] the set of covariates

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[15]This is specified with a set of dummies that are grouped every four weeks until the 64th week, every eight weeks until the 88th week and a unique dummy from the 89th week onwards. This is the same specification used for the baseline hazard in the duration models of Section 6.

shown in Table 2 and the working weeks accumulated by the end of each t (Z_t). The latter non-parametrically enters the linear index of the logit model by a piecewise constant specification with the accumulated working weeks grouped into two-week intervals. We estimated the pooled logit models separately for the treated and the control groups. Since the job spells started with different values of Z_0, we can disentangle the duration dependence (t) from the effect of the time-varying variable Z_t, as per the previous literature (e.g. Rebollo-Sanz, 2012). In our case, there is also a second identifying source that is of help in disentangling these: Z_t does not necessarily evolve along the job spell at the same rate as t (or does not evolve at all over some t). This happens if, in the initial part of the $C.1$ biennium, the worker accumulated work experience that is lost as the biennium moves during the spell.

Figure 5: Predicted probabilities of job exit across the accumulated working weeks from logit model estimates
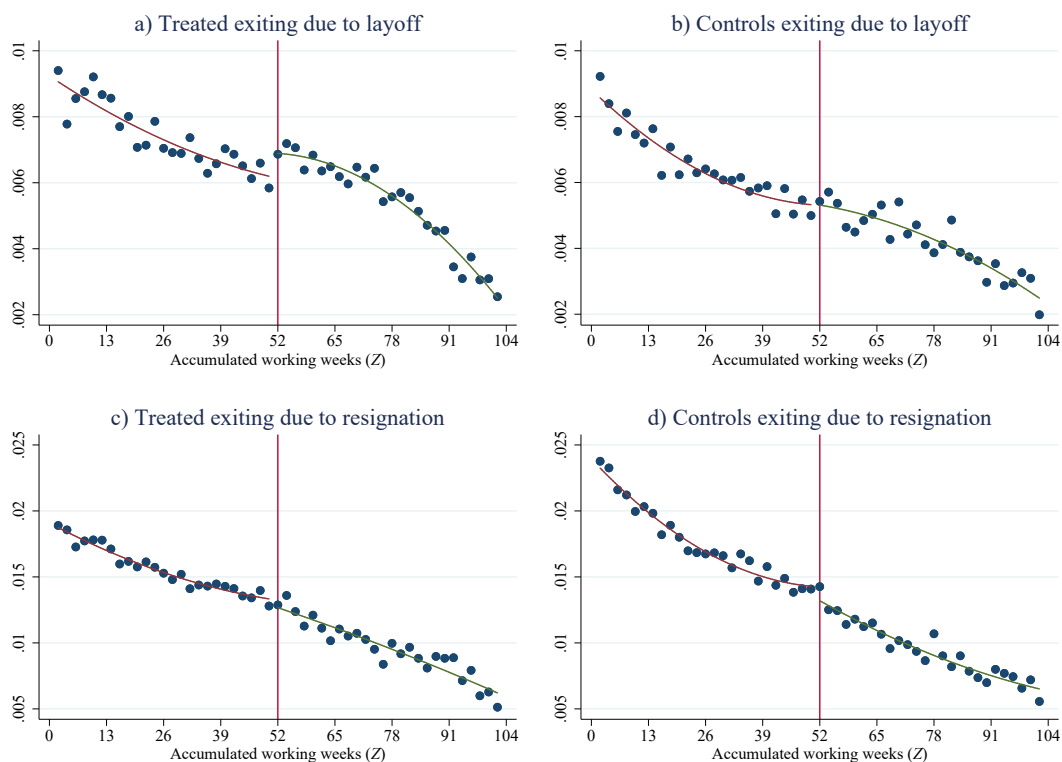
Notes: To draw these graphs, we: i) estimated logit models with the dependent binary variable equal to 1 if the layoff (graphs a and b) or resignation (graphs c and d) is observed in the subsequent two weeks, as a function of a full set of 52 dummies for the values of Z accumulated by the end of each t period grouped into two-week intervals and a set of elapsed duration dummies and covariates; ii) estimated the predicted probabilities of job exit for each Z at the mean of the other covariates; iii) plotted the predicted probabilities along with their quadratic fit to the left and the right of the 52 working weeks cut-off.

Figure 5 shows the predicted probabilities of job exit in the subsequent two weeks across the accumulated working weeks (at the mean of the other regressors) along with their quadratic fit to the left and the right of the cut-off of the 52nd accumulated working week. Recall that our treated group is composed of spells that already satisfied the experience condition $C.2$ at the start of the job spell. These spells became eligible for UBs as soon as they satisfied

14

the working weeks condition $C.1$. Hence, if UB eligibility has an impact on the firing rate, we expect to observe a jump in the probability of layoff for the treated group as soon as the accumulated working weeks exceed 52. In contrast, we should not observe any jump: i) for the control group since for this group, satisfying the working weeks requirement is not sufficient for UB entitlement; ii) when we model the probability of resignation. All of these intuitions are confirmed by the graphs in Figure 5. Indeed, graph a) shows that the probability of layoff suddenly grows from 0.6% to 0.7% when the treated reach the 52nd accumulated working week, which is a substantial relative increase of about 17%. No jump at $Z = 52$ can be detected in the remaining plots.[16]

We then formally exploit the discontinuity in UB eligibility and implement a sharp RDD to estimate the local average treatment effect (LATE) of satisfying condition $C.1$. In this RDD setting, the accumulated working weeks z_{it} of job spell i at elapsed duration t represent the forcing variable. We compare the job separation rate just before and after the 52nd working week. For simplicity, covariates are not included. We use local linear regressions with triangular weights and choose the bandwidth following the optimal mean squared error criterion as in Calonico et al. (2014). The underlying linear probability model in error term form estimated by RDD separately for the treated and the controls is:

$$y_{it}^k = \alpha^k + \delta^k \cdot \mathbb{1}(z_{it} \geq 52) + \beta^k z_{it} \cdot \mathbb{1}(z_{it} < 52) + \gamma^k z_{it} \cdot \mathbb{1}(z_{it} \geq 52) + \varepsilon_{it}^k, \quad (1)$$

where

- y_{it}^k is equal to 1 if job spell i at elapsed job duration t will end within two weeks due to reason k.

- $\mathbb{1}(\cdot)$ is the indicator function, which is equal to 1 if the argument is true.

- α^k is the constant.

- z_{it} is the accumulated working weeks at the end of the elapsed job duration t.

- $\beta^k z_{it} \cdot \mathbb{1}(z_{it} < 52)$ is the linear relationship between the forcing variable and the outcome to the left of the cut-off.

- $\gamma^k z_{it} \cdot \mathbb{1}(z_{it} \geq 52)$ is the linear relationship between the forcing variable and the outcome to the right of the cut-off.

- $\mathbb{1}(z_{it} \geq 52)$ is a dummy indicator equal to 1 once the worker has accumulated at least 52 working weeks in the last biennium. The associated parameter δ is the discontinuity at the cut-off (LATE) in the relation between the accumulated working weeks and the outcome.

- ε_{it}^k is the idiosyncratic error term (with zero conditional mean).

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[16]If we split the sample by year of hiring (before and after 2008), firm dimension (above and below 15 employees) and geographical area (South and rest of Italy), we observe similar but more pronounced jumps after 2008, in smaller firms and in the South. No discontinuity is visible for the other dimensions, however. These results are in figures C.3–C.5 in Appendix C.

Figure 6: RDD-predicted probabilities of job exit across the accumulated working weeks

a) Treated exiting due to layoff

b) Controls exiting due to layoff

c) Treated exiting due to resignation

d) Controls exiting due to resignation
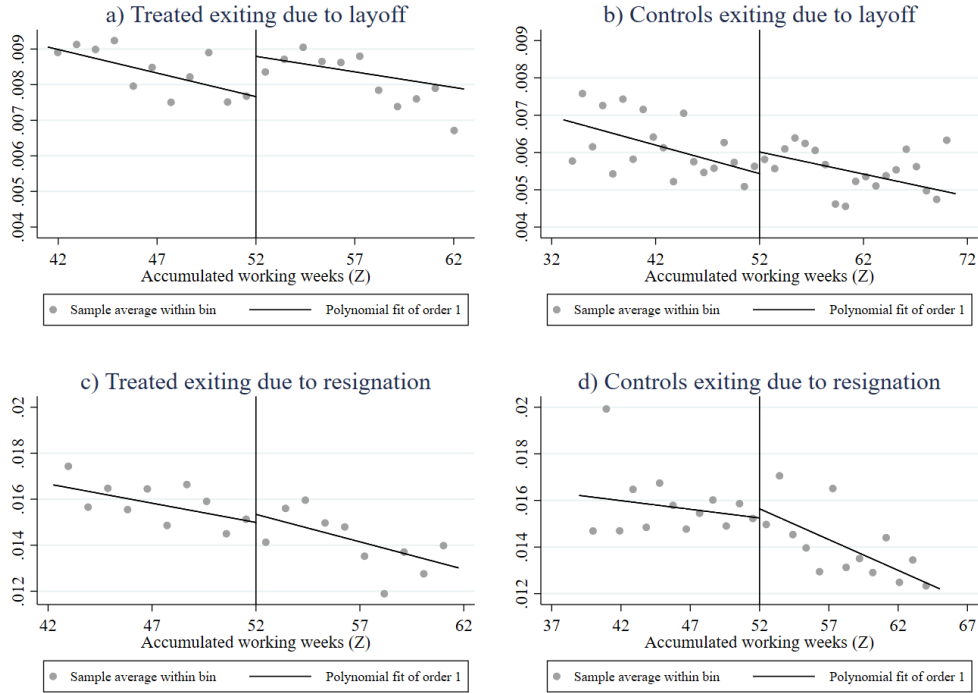
Notes: These graphs report RDD plots for the dependent binary variable equal to 1 if the layoff (graphs a and b) or resignation (graphs c and d) is observed in two weeks, where Z is the forcing variable with cut-off at 52 accumulated working weeks. We used local linear regression with triangular weights and bandwidth following the optimal mean squared error criterion in Calonico et al. (2014).

Figure 6 graphically summarises the RDD estimation results. The RDD point estimates along with further estimation details are in Appendix C, Table C.1. For the treated (graph a), the RDD estimator yields an effect of 0.11 percentage points on the layoff probability (with robust bias-corrected p-value equal to 0.031), which amounts to a 15% increase in relative terms. The LATE for the controls is equal to 0.06 percentage points, which is insignificant at the 10% level. Similarly, the effect on the resignation rate of the treated and the controls is 0.03 (p-value 0.681) and 0.04 (p-value 0.563) percentage points, respectively. These estimates are very much in line with the predicted probabilities from the logit model estimates.[17]

6   Main estimation results

The empirical evidence in Section 5 suggests that UB eligibility boosts the probability of job termination due to layoff. However, the assumptions to be satisfied in order to assign a causal interpretation to the RDD estimates might be considered as too strong. First, the RDD approach relies on the assumption of no manipulation of the forcing variable Z, meaning that firms and

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[17]As found in the logit model, if we split the sample by year of hiring, geographical area and firm size, we estimate a larger effect after 2008 (+31%, p-value = 0.000), in smaller firms (+22%, p-value = 0.003) and in the South (+26%, p-value = 0.007). The LATE in the other dimensions is not significant at the 5% level (see tables C.1 and C.2 in Appendix C).

workers should not fully control the accumulated working weeks. However, if the agents time the firing according to UB eligibility requirements, we might expect a reduction in layoffs for values of Z just below the 52 week eligibility threshold, which might upwardly bias the estimates. Furthermore, agents manipulating the forcing variable may not be a random sample and could have particular characteristics related to the probability of job termination. This would imply that factors determining the outcome process do not evolve smoothly with respect to the forcing variable, generating the failure of the their local continuity restriction (Hahn et al., 2001; Lee and Lemieux, 2010). Second, in Section 5, when analysing the probability of job termination due to one reason, we simply right-censor job spells that were terminated for other competing reasons, as if these exits were exogenously driven. However, the same observed and unobserved determinants of one exit, including UB eligibility, could also affect the job termination rate for other reasons, generating endogenous attrition in the longitudinal dimension of our pooled dataset. Failure to appropriately account for attrition can bias the estimation results. Third, for many workers, the cut-off at 52 accumulated working weeks might coincide with the moment at which they reach a particular moment in their career. For example, for the treated starting their job spell with $Z_0 = 0$, the 52nd working week is likely to be attained at one year of job seniority. If one year of job seniority is a milestone for the future development of a career within the firm, for example, as an informal probationary period, a confounding component would bias the RDD estimate.

In this section, we propose a mixed proportional hazard (MPH) model for duration outcomes designed to overcome these problems and credibly claim that we identify the UB effect on the firing rate. This is not free of cost. Modelling job exits using the MPH specification imposes a parametric structure on the job duration distribution, since the hazard rate fully characterises the corresponding duration distribution. This parametric structure, which is not required in the linear probability model estimated by RDD, is the price that we pay.

First, we design our duration model so as to take into account the presence of unobserved heterogeneity, which affects the sample composition over time and will allow us to compare spells with different amounts of accumulated working weeks at hiring. Second, we model competing risks of exit, jointly determined by observed and unobserved characteristics. Third, we use both controls and treated spells and focus on their differential evolution in the job separation rates both before and after the accumulation of 52 working weeks, as if in a DiD design. The control group allows us to isolate the common impact of experience. By doing so, we are able to net out confounding components related to work experience and job seniority that might induce a spurious jump in the job separation rates for reasons other than UB eligibility. We are also able to test whether there are anticipatory effects before the cut-off that would violate the RDD assumption of no manipulation of the forcing variable Z. Finally, it allows us to move from a local identification of the effect to a more general one across $Z \geq 52$, avoiding the sensitivity to manipulations just around the 52 weeks eligibility threshold.

17

6.1 DiD hazard rate specification and results

The dependent variable is the job spell duration until either layoff or other type of termination (resignation or end of temporary/seasonal contract). Therefore, we model two competing risks of exiting a job: layoff l and other reasons o. The observed durations are grouped into time intervals of two weeks. The time unit $t \in \mathbb{N}_0$ is therefore a two-week period. To avoid the dependency of parameters to the time unit of observation (Flinn and Heckman, 1982), we model the discrete-time process as in a grouped continuous-time model (van den Berg and van der Klaauw, 2001). The transition intensity of a job spell to $k \in \{l, o\}$ is specified with the following MPH form:[18]

$$
\begin{aligned}
\theta_k(t|\mathbf{x}_t, z_t, d, \mathbf{v}_k) &= \exp\Big\{\Gamma_k(t) + \Lambda_{0k}(z_t) + \boldsymbol{\beta}'_{0k}\mathbf{x}_t + d \cdot \big[\Lambda_{1k}(z_t) + \boldsymbol{\beta}'_{1k}\mathbf{x}_t\big] \\
&\quad + d \cdot v_{1k} + (1-d) \cdot v_{0k}\Big\},
\end{aligned}
\tag{2}
$$

where

- d is an indicator variable equal to 1 if the job spell belongs to the treated and 0 otherwise.

- $\exp[\Gamma_k(\cdot)]$ is the piecewise constant baseline hazard common to all job spells. We use a piecewise constant function since parametric assumptions that are too strict are a possible sources of bias. The discrete time axis of the job spells is cut into 20 intervals.[19] Let the cut-points of the time axis be $0 = c_0 < c_1 \cdots < c_{19} = \infty$. The assumed piecewise constant specification of the function $\Gamma_k(\cdot)$ for $k \in \{l, o\}$ is:

$$
\Gamma_k(t) = \sum_{s=1}^{20} \mathbb{1}(c_{s-1} < t \leq c_s)\gamma_{k,s}.
\tag{3}
$$

  The 20 coefficients $\gamma_{k,s}$ map the profile of the transition intensity towards risk k.[20]

- $\mathbf{x}_t$ is the vector of covariates controlling for observed heterogeneity. Table 2 lists the full set of covariates included in $\mathbf{x}_t$. Apart from the regional yearly GDP growth rate, all other controls are time-invariant and measured at the beginning of the job spell. The conformable parameter vector $\boldsymbol{\beta}_{0k}$ is the common impact of covariates, whereas $\boldsymbol{\beta}_{1k}$ captures the deviation from the common impact of the observables for the treated.

- z_t is the time-varying variable measuring the number of working weeks determining the satisfaction of $C.1$. It takes values of positive integers up to a maximum of 104. Its value is updated at the end of each t. $\exp[\Lambda_{0k}(z_t)]$ is a piecewise constant function so

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[18] In what follows, we omit the subscript i indicating job spell i for the sake of keeping the notation simple.

[19] To reduce the number of parameters to estimate, we assume that the profile changes every two time units (i.e. every four weeks) until the 64th week. The profile is then allowed to change every four time units (i.e. every eight weeks) until the 88th week. From week 89 onward, the baseline hazard is assumed to be constant.

[20] We imposed the innocuous normalization of $\gamma_{k,1}$ to 0 for all $k \in \{l, o\}$.

as to flexibly retrieve the impact, common to everybody, of the accumulated working weeks on the transition intensities. $\exp[\Lambda_{1k}(z_t)]$ is also a piecewise constant function measuring the deviation from the common effect for the treated. To increase precision, we regroup the support of Z into 10 intervals.[21] Let the cut-points of the support of Z be $0 = q_0 < q_1 \cdots < q_{10} = 104$. The assumed piecewise constant specification of the function $\exp[\Lambda_{ek}(z_t)]$ for $k \in \{l, o\}$ and $e \in \{0, 1\}$ is:

$$\Lambda_{ek}(z_t) = \sum_{s=1}^{10} \mathbb{1}(q_{s-1} < z_t \le q_s)\lambda_{ek,s}. \tag{4}$$

- $\mathbf{v}_k \equiv (v_{1k}, v_{0k})$ captures unobserved heterogeneity for the treated (v_{1k}) and the controls (v_{0k}). The impact of unobserved heterogeneity on the transition intensities is treatment-specific and therefore takes into account that the treated and the control group might systematically differ in unobservables. We denote by G the mixing joint distribution of $\mathbf{V} \equiv (V_{0l}, V_{0r}, V_{1l}, V_{1r})$ with finite first moments. As explained in Appendix D, we assume a discrete distribution with four points of support with unknown location of the probability masses.

The parameters $(\lambda_{1l,1}, \ldots, \lambda_{1l,10})$ characterise the different evolution of the effect of accumulated work experience on the layoff transition intensity of the treated with respect to the control group. They are therefore the parameters of primary interest. If employers and/or employees time layoffs with UB eligibility, then we expect the function $\Lambda_{1l}(Z)$ to display a sudden profile change after $Z = 52$ (when only the treated attain UB eligibility) with respect to the baseline interval of Z. We consider as the baseline interval the band of Z that is closest to the cut-off in the pre-treatment period ($43 < Z \le 51$).[22] Hence, by comparing the periods before and after this cut-off, both for the treated and the control group, we measure the impact of UB eligibility on the log transition intensity as in the usual DiD set-up for linear models of the conditional mean of the outcome variable.[23]

The MPH specification of the transition intensities in Equation (2) is such that the systematic part, the impact of Z and the unobserved heterogeneity depend on the treatment status.

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[21]The eight central intervals are equally spaced (eight weeks), the first is 12 weeks long (up to and including week 11), the last goes from week 76 onwards to increase precision, as fewer individuals reach this level of accumulated experience.

[22]Indeed, we normalised $\lambda_{ek,6}$ to 0 for each $e \in \{0, 1\}$ (i.e. controls and treated) and $k \in \{l, o\}$, which is the impact of Z on the transition intensity when $43 < Z \le 51$.

[23]Table C.3 in Appendix C displays the estimation results of the function $\Lambda_l(z_t)$ from an MPH model with competing risks estimated using only the treated, similarly to Rebollo-Sanz (2012). If we use only the treated, we cannot disentangle the impact of accumulated work experience from that of UB eligibility. Interestingly, however, we find that the layoff transition intensity clearly decreases in the accumulated working weeks up to the moment of UB eligibility, when it suddenly jumps and then flattens out. After the 76th accumulated working week, it regains its decreasing profile. This suggests a positive impact of UB eligibility on the layoff transition intensity. However, the effect is likely to be downward-biased due to the observed negative relationship between work experience and layoffs.

19

The baseline hazard of each transition intensity is instead common to both the treated and the controls. If the baseline hazards depend on the treatment status, then we could eventually recover the effect of UI entitlement on the job layoff rate by separately estimating the job hazard rate of the treated and the controls and taking the difference in the functions $\Lambda_{ek}(Z)$ for the two groups ($e = 0, 1$). As such, under the MPH specification, regularity conditions on the MPH components, the finiteness of the first moment of the mixing distribution G and the orthogonality between the observed and unobserved determinants,[24] we can invoke the identification result in Abbring and van den Berg (2003a) for competing risks MPH hazard functions with single-spell data.[25]

Similarly to the usual DiD set-up, some assumptions must be satisfied to recover the treatment effect on the treated (ATT) of UB eligibility on the layoff hazard rate from the difference between $\Lambda_{1l}(Z)$ and $\Lambda_{0l}(Z)$, for $Z \geq 52$. First, conditional on $(\mathbf{v}, \mathbf{x}, z_t)$, there should be no time-varying unobservables determining the time to UB eligibility. If there were time-varying omitted variables affecting both UB eligibility and the job hazard rate, then the impact of Z on the layoff transition intensity for the treated and control groups would be different for spurious reasons, not solely because of UB eligibility. In a standard DiD approach for linear conditional means, this translates to the common trend assumption, which, in our case, means that the accumulation of work experience Z should have a common effect for both groups in the absence of UB eligibility. Second, no anticipation over the baseline interval of the pre-treatment period ($44 \leq Z < 52$) should hold. If the treated reacted to the treatment during this reference period, then the estimated effects were biased; see e.g. Ashenfelter's dip (Ashenfelter and Card, 1985). In our model, we flexibly estimate the function $\Lambda_{1l}(Z|Z < 52)$ in the pre-treatment period so as to provide evidence that the data support the parallel trend and no-anticipation assumptions. This is similar to the strategy that is often used in standard DiD design, consisting of including leads of the indicator for the treated in the treatment period (see e.g. Autor, 2003).[26]

The transition intensities fully characterise the duration distribution. Hence, once we opt for the MPH specification in Equation (2) and assign a particular distribution to the unobserved heterogeneity, we can write down the sample log-likelihood as a function of a finite set of parameters and maximise it with respect to these. Appendix D provides details on the derivation of the log-likelihood function.

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[24]The failure of the orthogonality condition between observed and unobserved determinants does not necessarily imply a bias in the estimation of the effects of interest. We lose the possibility of giving a structural interpretation to the coefficients of the observables (Cockx et al., 2013; Cockx and Picchio, 2013).

[25]The identification result in Abbring and van den Berg (2003a) is in continuous time, whereas our durations are grouped into two-week periods. In a large Monte Carlo simulation, Gaure et al. (2007) assessed, however, that time-of-events models a là Abbring and van den Berg (2003b) with time-grouped data are estimated without bias when the time-grouping is incorporated in the derivation of the likelihood function. We therefore explicitly took into account the time-grouping in the derivation of the likelihood function, as described in Appendix D.

[26]We also take into account that the treated and the control groups might differ in time-invariant unobserved heterogeneity. If not controlled for, this could also invalidate the DiD identification strategy as the selection effects operating on individuals over the elapsed job duration might modify the sample composition of the two groups in different ways. The results without unobserved heterogeneity are in tables B.1–B.3 of Appendix B.

Table [3] reports the estimated coefficients $\widehat{\lambda}_{1l,1}, \ldots, \widehat{\lambda}_{1l,10}$, which capture the impact of UB eligibility on the layoff transition intensity. The full set of estimation results of the MPH model are reported in Appendix [A], Tables [A.1]–[A.4]. As soon as the UB eligibility kicks in, the layoff exit rate of the treated jumps significantly by 12.2%.[27] The increase in the job layoff transition intensity stays at a similar level for about 16 weeks of work experience. Then, for $Z \geq 68$, the treated and control groups have similar layoff exit rates. The sudden but temporary increase in the layoff transition intensity after UB eligibility suggests that the job matches that are meant to be dissolved with an improvement in the employees' outside option immediately take advantage of the opportunity, as if the job mismatches were prearranged. Jobs surviving beyond the 76th week are probably higher quality matches and the workers' improved outside option is not large enough to generate job destruction.

The placebo test for the pre-treatment period passes, as none of the coefficients of the pre-treatment dummies $\widehat{\lambda}_{1l,1}, \ldots, \widehat{\lambda}_{1l,5}$ are significantly different from zero. Furthermore, while the Wald test of the joint insignificance of $\widehat{\lambda}_{1l,7}, \ldots, \widehat{\lambda}_{1l,10}$ is confidently rejected at the 1% level (p-value $= 0.001$), the coefficients in the pre-treatment period are jointly insignificant (p-value $= 0.352$). Hence, when both the treated and the controls do not satisfy the eligibility requirement for UB, their layoff exit rate evolves in a parallel way. This supports the validity of our identifying assumptions.

Table 3: Estimated ATTs on the layoff transition intensity

	Coeff.		S.E.
Before UB eligibility ($Z < 52$)			
1–11 accumulated working weeks ($\lambda_{1k,e}$)	-0.056		0.044
12–19 accumulated working weeks ($\lambda_{2k,e}$)	-0.009		0.044
20–27 accumulated working weeks ($\lambda_{3k,e}$)	0.015		0.044
28–35 accumulated working weeks ($\lambda_{4k,e}$)	0.012		0.044
36–43 accumulated working weeks ($\lambda_{5k,e}$)	0.020		0.046
44–51 accumulated working weeks ($\lambda_{6k,e}$)	0.000		-
After UB eligibility ($Z \geq 52$)			
52–59 accumulated working weeks ($\lambda_{7k,e}$)	0.115	**	0.049
60–67 accumulated working weeks ($\lambda_{8k,e}$)	0.117	**	0.056
68–75 accumulated working weeks ($\lambda_{9k,e}$)	0.063		0.062
76+ accumulated working weeks ($\lambda_{10k,e}$)	-0.082		0.055
Wald test H_0: $\lambda_{1l,1} = \cdots = \lambda_{1l,5} = 0$		p-value $= 0.352$	
Wald test H_0: $\lambda_{1l,7} = \cdots = \lambda_{1l,10} = 0$		p-value $= 0.001$	
Number of job spells		424,473	

Notes: *** Significant at the 1% level; ** significant at the 5% level; * significant at the 10% level.

6.2 Sensitivity analysis

In Appendix [E], we report several robustness checks to test the sensitivity of our DiD estimation results of the ATTs. We re-estimate the MPH model after deleting seasonal jobs and after keeping only open-ended contracts. We parametrically specify the baseline hazard to avoid

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[27]12.2% $= [\exp(0.115) - 1] \cdot 100$.

possible biases coming from too flexible specifications in the baseline hazard and unobserved heterogeneity (Baker and Melino, 2000). We also change the definition of the treatment group. According to our baseline definition, the treatment group is made up of the spells of workers with at least one day and no more than two years of work experience during the $C.2$ period. We test whether the results are sensitive to this choice by re-estimating the benchmark model after modifying the maximum work experience to 26 weeks and 156 weeks. We check whether the results are sensitive to the right-censoring of the controls becoming treated. We implement a RDD estimator in the competing risks MPH model on the treated units only and including a cubic polynomial specification across Z, with different coefficients to the right and left of the cut-off. In all of these cases, the estimated ATTs were very close to those from the benchmark model, and the RDD estimator delivers very similar results to the LATE estimates obtained on pooled data in Section 5 (+15-17%).

Although we did not find evidence of significant anticipatory effects along Z grouped into intervals of (mainly) eight weeks, in a further check, we test whether evidence of an anticipatory effect shows up when the worker gets very close to the UB eligibility threshold of 52 accumulated working weeks. We do this by re-estimating the benchmark model with the piece-wise constant specification of $\Lambda_{ek}(Z)$ augmented by a further dummy equal to 1 when $Z = 51$, for $k \in \{l, o\}$ and $e \in \{0, 1\}$. If, indeed, firms and employees adopt an opportunistic behaviour and agree to wait until UB eligibility before firing, this should be reflected in a dip in the layoff exit rate just before UB entitlement, generating an anticipatory effect with the opposite sign than the one found in the after-treatment period. Model (11) in Table E.1 of Appendix E shows that this extra dummy for $Z = 51$ has the expected negative sign, pointing to a reduction of 5.2% in the layoff exit rate just before UB eligibility, which may explain the different effect estimated by implementing the RDD estimators (+15-17%) and the DiD estimator close to the threshold (+12%). However, this anticipation effect is not significantly different from zero and all of the other ATTs are close to those of the benchmark model.

Finally, we run a validation test, in the spirit of a placebo test, by estimating the impact of UB eligibility on the (voluntary) resignation exit rate. Because the general rule is that workers voluntarily resigning lose UB eligibility (see graph c in Figure 3), we do not expect significant ATTs for $Z \geq 52$. Operationally, we keep the competing risks structure unchanged, i.e. the number of competing risks is still fixed to two, but now, one risk of exit is voluntary resignation and the second risk is the residual category including all of the other exit risks (e.g. layoff and the end of temporary/seasonal job). As expected, the estimated ATTs from this modified model do not display any evidence of a sudden increase in the job exit rate for voluntary quitting.

6.3   Mechanisms

We find a sizable impact on the rate at which workers are laid off as soon as they acquire UB eligibility. In this section, we aim to gain a better understanding of the mechanisms at play with

the help of the information coming from possible heterogeneous effects. To get more insight into the role of the economic situation, social norms and firing costs, we divide the population into subgroups along geographical dimension, firm size and year of hiring, and re-estimate the benchmark DiD model. The ATTs are reported in Table 4 along with the p-values from the equality tests between the coefficients of each pair of independent samples (Clogg et al., 1995; Brame et al., 1998).

The first dimension of heterogeneity is related to the economic situation, which changed significantly after 2008. The Great Recession should have modified the incentives for moral hazard of employers and employees in different directions. On the one hand, in economic downturns or in periods of higher demand uncertainty, employers need to adjust their work-force and exploiting UB eligibility may be a way to reduce the expected firing costs. On the other hand, deteriorated economic conditions should make workers less willing to agree on job packages that involve a certain probability of layoff once UB eligibility is attained: because the probability of finding a new job decreases in downturns, the outside option is less valuable and more risky.[28] Panel a) shows that when splitting the job spells into those started before and after January 2008, it seems that the latter are driving the findings for the whole sample. No effect on the layoff transition intensity is detected among spells started before the economic crisis, whereas UB eligibility boosts the layoff exit rate of job spells started after 2008 by 21.2% in the eight weeks following UB eligibility and by 14.4% in the subsequent eight.

Second, we investigate the role of employers' firing costs by dividing the sample between firms with more and with fewer than 15 employees, a threshold that also coincides with different EPL on layoffs. As explained in Section 3, firms with more than 15 employees face large expected firing costs. By contrast, employers with less than 15 employees are subject to much looser EPL and lower expected firing costs. Hence, smaller firms might more easily offer job packages including, as a feature of the agreement, a probability of being laid off at UB eligibility (Zweimüller, 2018). As shown in panel b) of Table 4, the effect is statistically significant only for smaller firms, while for larger firms it is very close to zero. Because our estimates are driven by firms with fewer than 15 employees, the results might suggest that low rigidity is a driver of the effect. However, large and small firms differ on other dimensions besides different EPL regimes. For example, stronger interpersonal relations and trust between employees and their employer might play a more significant role in smaller firms and facilitate collusion to exploit the UB system. Nonetheless, we should be careful in interpreting these results; although the point estimates of the ATTs are quite different in size between large and small firms, their difference is not statistically significant.

Another driving force might be related to trust in society and social norms. Differences in social norms could lead to heterogeneous responses to the same moral hazard incentive.

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[28]In 2008, the UB system also became more generous to a certain extent, which might have increased the incentive to exploit the UI system. The raise in the replacement rate was mild, however, and also applied to the 'reduced' UB for workers with very few accumulated working weeks. Therefore, it is unlikely to play a major role.

Table 4: Heterogeneity of estimated ATTs on the layoff transition intensity by selected dimensions

	(1) Coeff.		(2) S.E.	(3) Coeff.		(4) S.E.	Significance test of the difference between the ATTs, (1)-(3): p-value
Panel a)	Before 2008			After 2008			
Before UB eligibility ($Z < 52$)							
1–11 accumulated working weeks ($\lambda_{1l,1}$)	-0.093		0.065	-0.028		0.060	
12–19 accumulated working weeks ($\lambda_{1l,2}$)	-0.029		0.065	-0.005		0.060	
20–27 accumulated working weeks ($\lambda_{1l,3}$)	-0.023		0.065	0.034		0.061	
28–35 accumulated working weeks ($\lambda_{1l,4}$)	0.028		0.065	-0.017		0.061	
36–43 accumulated working weeks ($\lambda_{1l,5}$)	0.006		0.067	0.018		0.063	
44–51 accumulated working weeks ($\lambda_{1l,6}$)	0.000		–	0.000		–	
After UB eligibility ($Z \geq 52$)							
52–59 accumulated working weeks ($\lambda_{1l,7}$)	0.005		0.074	0.192	***	0.067	0.061
60–67 accumulated working weeks ($\lambda_{1l,8}$)	0.088		0.082	0.135	*	0.078	0.673
68–75 accumulated working weeks ($\lambda_{1l,9}$)	0.032		0.091	0.100		0.086	0.584
76 or more accumulated working weeks ($\lambda_{1l,10}$)	-0.057		0.079	-0.113		0.076	0.610
Wald test H_0: $\lambda_{1l,1} = \cdots = \lambda_{1l,5} = 0$	p-value $= 0.381$			p-value $= 0.862$			
Wald test H_0: $\lambda_{1l,7} = \cdots = \lambda_{1l,11} = 0$	p-value $= 0.520$			p-value $= 0.000$			
Number of job spells	193,616			230,857			
Panel b)	Firms with more than 15 employees			Firms with fewer than 15 employees			
Before UB eligibility ($Z < 52$)							
1–11 accumulated working weeks ($\lambda_{1l,1}$)	-0.155	**	0.070	0.041		0.056	
12–19 accumulated working weeks ($\lambda_{1l,2}$)	-0.064		0.070	0.072		0.056	
20–27 accumulated working weeks ($\lambda_{1l,3}$)	-0.028		0.071	0.079		0.056	
28–35 accumulated working weeks ($\lambda_{1l,4}$)	-0.002		0.071	0.049		0.057	
36–43 accumulated working weeks ($\lambda_{1l,5}$)	0.108		0.074	-0.020		0.058	
44–51 accumulated working weeks ($\lambda_{1l,6}$)	0.000		–	0.000		–	
After UB eligibility ($Z \geq 52$)							
52–59 accumulated working weeks ($\lambda_{1l,7}$)	0.044		0.081	0.131	**	0.063	0.395
60–67 accumulated working weeks ($\lambda_{1l,8}$)	0.062		0.094	0.112		0.070	0.671
68–75 accumulated working weeks ($\lambda_{1l,9}$)	0.021		0.104	0.040		0.078	0.881
76 or more accumulated working weeks ($\lambda_{1l,10}$)	-0.115		0.089	-0.110		0.070	0.962
Wald test H_0: $\lambda_{1l,1} = \cdots = \lambda_{1l,5} = 0$	p-value $= 0.006$			p-value $= 0.389$			
Wald test H_0: $\lambda_{1l,7} = \cdots = \lambda_{1l,11} = 0$	p-value $= 0.372$			p-value $= 0.003$			
Number of job spells	227,526			196,947			
Panel c)	South			Centre-North			
Before UB eligibility ($Z < 52$)							
1–11 accumulated working weeks ($\lambda_{1l,1}$)	0.002		0.064	-0.103	*	0.060	
12–19 accumulated working weeks ($\lambda_{1l,2}$)	0.096		0.064	-0.097		0.061	
20–27 accumulated working weeks ($\lambda_{1l,3}$)	0.079		0.065	-0.034		0.061	
28–35 accumulated working weeks ($\lambda_{1l,4}$)	0.067		0.066	-0.031		0.061	
36–43 accumulated working weeks ($\lambda_{1l,5}$)	0.053		0.068	0.001		0.062	
44–51 accumulated working weeks ($\lambda_{1l,6}$)	0.000		–	0.000		–	
After UB eligibility ($Z \geq 52$)							
52–59 accumulated working weeks ($\lambda_{1l,7}$)	0.150	**	0.073	0.053		0.068	0.331
60–67 accumulated working weeks ($\lambda_{1l,8}$)	0.216	***	0.082	-0.024		0.079	0.035
68–75 accumulated working weeks ($\lambda_{1l,9}$)	0.125		0.092	0.000		0.087	0.319
76 or more accumulated working weeks ($\lambda_{1l,10}$)	-0.046		0.084	-0.089		0.075	0.707
Wald test H_0: $\lambda_{1l,1} = \cdots = \lambda_{1l,5} = 0$	p-value $= 0.304$			p-value $= 0.321$			
Wald test H_0: $\lambda_{1l,7} = \cdots = \lambda_{1l,10} = 0$	p-value $= 0.005$			p-value $= 0.463$			
Number of job spells	121,301			303,172			

Notes: *** Significant at the 1% level; ** significant at the 5% level; * significant at the 10% level.

There is evidence of different social norms between the South and the rest of Italy (Banfield, 1958). For example, according to the laboratory-in-the-field experiment in Bigoni et al. (2016, 2018) and the 2008 European Values Survey, people in the south of Italy have a much lower level of trust in people and in society. With this general sentiment of distrust towards the others, employers and employees may find it easier to feel justified in behaving opportunistically and exploiting the UI system.[29] Related to this, undeclared employment also occurs more in the South than in the rest of Italy.[30] In a society where undertaking undeclared work is more widespread, firms and workers might more easily agree, once UB eligibility is attained, to officially terminate the official employment relationship but maintain it off the books. The surplus coming from the UB could then be shared between both sides of the job relation, depending on their bargaining power. Panel c) of Table 4 reports the estimation results of interest when we split Italy into South and Centre-North.[31] In the South, the layoff transition intensity increases by 16.2% when UB eligibility is attained. The increase peaks at 24% between 60 and 67 weeks of accumulated work experience and then fades away. In the rest of Italy, the effect of UB eligibility is instead not significantly different from zero and the joint significance test of the overall effect for $Z \geq 52$ reported at the bottom of panel c) of Table 4 cannot reject the null hypothesis. This suggests that the effect at the national level is mainly driven by the labour market in the South. When testing whether the ATTs in the South are different from those in the Centre-North, we find that only the difference in the effect for weeks 60–67 is significant at the 5% level. The other ATTs, although quite different in size between the South and the Centre-North, do not formally show a significant difference, due to the increasing standard errors when we split the sample into subgroups. Although the sample size seems large, because the duration increases at a similar rate as the accumulated working weeks, we start having a problem of lack of precision when looking at the heterogeneity of ATTs.

In our administrative dataset, we cannot find information on job relations that are off the books. However, if workers and firms agree on officially terminating the job relation to maintain it off the books once UB eligibility is attained, displaced workers who are entitled to UB should display a higher probability of officially re-entering the same firm after UB exhaustion. As shown in Appendix F, the probability of re-hiring by the same firm does not display discontinuities at the eligibility cut-off measured at firing. This suggests that the larger effect detected in the South is not linked to undeclared work replacing official employment. However, it does

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[29]Guiso et al. (2004) exploit these geographical differences in Italy and find evidence that low trust in society leads to lower financial development. In addition, low-trust areas are associated with a greater use of loans from relatives or friends, as opposed to bank loans.

[30]According to the estimates in De Gregorio and Giordano (2015), irregular employment was 15.7% of total employment in the South, compared with 9.8% for the whole country. Moreover, according to data collected by the Italian Labour Inspectorate, each audit finds, on average, about 19% more undeclared jobs in the South than in the rest of Italy, albeit the audits per firm in the South are more than twice as many as those in the Centre-North (6.2% versus 2.9%). See C.4 in Appendix C for more details on these types of descriptive statistics.

[31]Southern regions are defined following the European NUTS1 category: Abruzzo, Molise, Campania, Puglia, Basilicata, Calabria, Sicily and Sardinia.

not completely rule it out because the undeclared job relation could go on for many years.

Deriving clear-cut policy implications from our estimated ATTs and their heterogeneity is not straightforward. The main difficulty is obtaining data to credibly disentangle the detected opportunistic behaviour in the moral hazard component on the firms' side from that on the employees' side. If we could do this, then we would be able to suggest to what extent the policy maker should intervene to reduce the opportunistic behaviour of firms, by introducing an experience rating system to reduce layoffs, for example, and/or the moral hazard behaviour of workers, such as by making the UB system less generous.

Although we do not have evidence regarding which side of the labour market mostly determines our empirical findings, there are several hints suggesting that the dominant moral hazard is on the firm side. First, lower firing costs should not increase employees' incentives to exploit the UI system, but rather, employers'. Similarly, the boost in the layoff exit rate for workers eligible for UB after 2008 might have little to do with the moral hazard of employees. The Great Recession should have reduced the willingness of workers to opportunistically exploit the UB system by lowering the value of the outside option. Furthermore, most of the layoffs in our sample are due to economic reasons (91.8%) and not the misconduct of the worker (8.2%). To gauge the importance of the first type of layoff in driving our findings, we redefine the two competing risks by isolating layoffs for economic reasons in one of these and by including all of the other reasons for job exit into the second risk. Table 5 reports the estimated ATTs on the exit rate due to layoffs for economic reasons. They are very much in line with those from the benchmark analysis.[32] This corroborates the hypothesis that it might mostly be employers who take the initiative for the layoff and, therefore, who drive our findings.[33] Hence, we argue that: i) the main driver of the estimated ATTs is likely to be employers' moral hazard behaviour; and ii) policy interventions targeted at reducing the moral hazard of firms, especially small firms, should effectively contribute to reducing excessive layoffs.[34]

The importance of limiting excessive layoffs is not only related to the issue of the optimal use of the UB system in protecting dismissed workers, but it also has indirect and long-term implications that are linked to the time that the excess displaced workers take to find a new job. First, excessive layoffs may increase the proportion of unemployed affected by stigma, with lower re-employment possibilities (Canziani and Petrongolo, 2001). Second, if there is an excessive number of layoffs entitled to UB, then there will be an excessive number of unemployed workers with a reduced job-search effort and/or higher reservation wage. The larger the

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[32]An analysis with worker misconduct as the outcome of interest gives compatible point estimates, though these are rather imprecise given the limited number of layoffs attributed to such a reason (results available upon request).

[33]We refrain from assigning to the observed reason for job termination the actual revealed initiative of one of the two parties to terminate the job relationship, as they may have agreed on the formal reason for the job termination.

[34]In this spirit, a 2013 labour market reform introduced a firing tax in Italy (Law 92/2012). Although this may be a step in the right direction, the introduction of firing taxes may have unintended consequences in a dual labour market such as the Italian market: permanent contracts risk becoming even less attractive than temporary jobs.

Table 5: Estimated ATTs on the transition intensity of layoffs for economic reasons

	Coeff.		S.E.
After UB eligibility ($Z \geq 52$)			
52–59 accumulated working weeks ($\lambda_{1l,7}$)	0.124	**	0.052
60–67 accumulated working weeks ($\lambda_{1l,8}$)	0.107	*	0.059
68–75 accumulated working weeks ($\lambda_{1l,9}$)	0.052		0.066
76 or more accumulated working weeks ($\lambda_{1l,10}$)	-0.066		0.057
Wald test H_0: $\lambda_{1l,1} = \cdots = \lambda_{1l,5} = 0$	p-value $= 0.265$		
Wald test H_0: $\lambda_{1l,7} = \cdots = \lambda_{1l,10} = 0$	p-value $= 0.004$		
Log-likelihood	-1,463,163.6		
Number of job spells	424,473		

Notes: *** Significant at the 1% level; ** significant at the 5% level; * significant at the 10% level.

actual disincentive effects in looking for and accepting a new job, the more relevant this indirect effect is for policy implications. In the economics literature, many studies link UB generosity with an increase in unemployment duration (see, for example, the survey by Schmieder and von Wachter, 2016). To gauge the extent to which this indirect implication could be relevant in Italy, from our sample we select workers who were laid off and follow them over time from that moment. We then estimate logit models for the probability of re-entering employment in six, nine and twelve months from dismissal as a function of a full set of dummies for the value of the working weeks accumulated at firing and the other covariates. We run the model separately for the treated and the controls. Figure C.2 in Appendix C displays the predicted probabilities across the working weeks accumulated at firing by treatment status. Interestingly, when firing occurs at $Z < 52$, we find that both groups share a similar probability of finding a new job in six, nine, and twelve months. Above 52 weeks, the re-employment probability suddenly drops but only for the treated. These profiles indicate that the excessive layoffs detected in the baseline model also have indirect and long-lasting consequences for the re-integration of the unemployed entitled to UB into the labour market. This reinforces the need for a policy intervention.

7 Conclusions

Unemployment insurance (UI) protects workers in the event of job loss and grants earnings stability. Previous literature has shown that it introduces a moral hazard on the job-seeking behaviour of the unemployed. In this paper, we show that this moral hazard is not only limited to unemployment spells but also affects employment spells. As in most countries the eligibility for unemployment benefits (UBs) depends on the amount of contributions to social security, both workers and firms may behave strategically to satisfy such conditions and appropriate the surplus coming from the UI system. UB eligibility may therefore distort firms and workers' behaviour and affect the duration of existing jobs. The relative importance of moral hazard on the firm's and the worker's side is an open question.

In this paper, we investigated whether and to what extent layoffs are affected by the attainment of UB eligibility during the job spell. In our empirical analysis we focus on Italy, a country characterised by the lack of an experience rating system, like many countries in Europe. Wide economic and cultural differences across regions and different levels of employment protection legislation between firms make Italy an interesting case to study heterogeneity in response to UB eligibility. We rely on an inflow sample of more than 400,000 new jobs drawn from administrative registries covering the period of 2005 to 2012. To identify the impact of UB eligibility on the layoff transition intensity, we exploit the peculiarity in the eligibility conditions of the Italian UI system. Our identification strategy is based on a difference-in-differences estimator that compares the layoff probability of individuals before and after the attainment of UI eligibility to a control group that cannot claim UBs.

We find robust evidence that UB eligibility increases the layoff exit rate by 12% as soon as eligibility is attained. The effect persists for about 16 weeks. The sudden boost suggests that the workers' improved outside option can dissolve the most fragile job matches, while it is not enough to terminate longer-lasting jobs. We detect significant heterogeneity in the effects. The results are driven by layoffs of jobs started after 2008, in small firms and in the South. A stronger effect during the Great Recession and in smaller firms is compatible with an induced moral hazard on the employer's side. First, in downturns, employees should have lower incentives to shirk because the outside option is less valuable due to the greater difficulty in finding a new job. Second, in Italy smaller firms have lower firing costs and therefore can more easily take advantage of the UI system to adjust their workforce. Finally, only a small minority of the layoffs observed in our sample are due to the misconduct of the worker. The results are in line with the hypothesis that employers exploit the UB system for labour adjustments when negative economic shocks occur (Zweimüller, 2018).

From a policy perspective, our study has important implications, not only for Italy. In contrast to the US, most European countries have not adopted experience rating systems which require firms to pay unemployment taxes based on their use of UBs. Some countries have introduced limited interventions targeting specific groups such as older workers (e.g. Delalande tax in France and Arbeitslosengeld I in Germany), but they involve a small minority of jobs. Since our findings suggest that the excessive layoffs might be due to employers' moral hazard, the introduction of an experience rating system may be of help to prevent firms from misusing the UB system. Furthermore, its introduction should make jobs longer lasting, which may have indirect positive consequences on productivity in the economy if longer-lasting jobs are associated with gains in general and firm-specific human capital. Finally, as we found evidence that the mass of excessive layoffs results in an excessive number of insured unemployed workers with longer-lasting unemployment spells, re-aligning firms' incentives is even more fundamental.

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Appendix

A Full set of estimation results of the DiD MPH competing risk model with unobserved heterogeneity

Table A.1: Impact of accumulated working weeks (Z) on transition intensities, Italy

	Layoff transition intensity (l)				Transition intensity for other termination reason (o)			
	For everybody		Deviation for the treated		For everybody		Deviation for the treated	
	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.
Cumulated working weeks (Z)								
1–11 accumulated working weeks ($\lambda_{1k,e}$)	0.636 ***	0.035	-0.056	0.044	0.995 ***	0.014	-0.067 ***	0.018
12–19 accumulated working weeks ($\lambda_{2k,e}$)	0.454 ***	0.035	-0.009	0.044	0.646 ***	0.014	-0.058 ***	0.018
20–27 accumulated working weeks ($\lambda_{3k,e}$)	0.271 ***	0.035	0.015	0.044	0.403 ***	0.014	-0.014	0.018
28–35 accumulated working weeks ($\lambda_{4k,e}$)	0.179 ***	0.036	0.012	0.044	0.237 ***	0.014	0.004	0.018
36–43 accumulated working weeks ($\lambda_{5k,e}$)	0.084 **	0.037	0.020	0.046	0.121 ***	0.014	0.019	0.019
44–51 accumulated working weeks ($\lambda_{6k,e}$)	0.000	–	0.000	–	0.000	–	0.000	–
52–59 accumulated working weeks ($\lambda_{7k,e}$)	-0.068 *	0.040	0.115 **	0.049	-0.110 ***	0.016	-0.018	0.021
60–67 accumulated working weeks ($\lambda_{8k,e}$)	-0.207 ***	0.045	0.117 **	0.056	-0.307 ***	0.018	-0.019	0.024
68–75 accumulated working weeks ($\lambda_{9k,e}$)	-0.300 ***	0.051	0.063	0.062	-0.523 ***	0.021	-0.040	0.028
76+ accumulated working weeks ($\lambda_{10k,e}$)	-0.684 ***	0.049	-0.082	0.055	-0.796 ***	0.021	-0.203 ***	0.024
Wald test H_0: $\lambda_{1l,1} = \cdots = \lambda_{1l,5} = 0$			p-value = 0.352					
Wald test H_0: $\lambda_{1l,7} = \cdots = \lambda_{1l,10} = 0$			p-value = 0.001					
Number of job spells			424,473					

Notes: *** Significant at the 1% level; ** significant at the 5% level; * significant at the 10% level.

Table A.2: Impact of covariates on transition intensities, Italy

	Layoff transition intensity (l)						Transition intensity for other termination reason (o)					
	For everybody			Deviation for the treated			For everybody			Deviation for the treated		
	Coeff.		S.E.	Coeff.		S.E.	Coeff.		S.E.	Coeff.		S.E.
$(Age-15)/100$	-4.027	***	0.292	3.459	***	0.456	-4.316	***	0.129	2.867	***	0.217
$(Age-15)^2/1000$	1.177	***	0.079	-0.853	***	0.115	1.090	***	0.036	-0.656	***	0.055
Woman	-0.208	***	0.015	-0.075	***	0.022	-0.065	***	0.006	-0.029	***	0.010
Ever received income support	0.254	**	0.113	0.008		0.115	-0.011		0.054	0.088		0.056
Blue collar job in year before spell start	0.322	***	0.028	0.087	**	0.037	0.393	***	0.011	0.042	***	0.016
Employment contract in the calendar year before the start of the job spell - Reference: Temporary contract												
Open-ended contract	0.181	***	0.030	0.036		0.037	0.106	***	0.013	-0.050	***	0.017
Seasonal employment	-0.135	***	0.046	0.037		0.058	-0.096	***	0.021	-0.002		0.027
No employment	0.073	***	0.023	0.031		0.033	0.080	***	0.009	-0.078	***	0.014
Firm size - Reference: 5 or fewer employees												
Between 6 and 15	-0.187	***	0.019	-0.028		0.026	-0.125	***	0.009	-0.003		0.014
Between 15 and 50	-0.335	***	0.021	-0.028		0.029	-0.069	***	0.009	-0.005		0.014
Between 51 and 100	-0.678	***	0.022	-0.106	***	0.032	-0.085	***	0.009	0.023	*	0.014
More than 100	-0.641	***	0.027	0.001		0.040	0.058	***	0.009	0.039	***	0.015
Type of contract - Reference: Open-ended												
Temporary	0.022		0.016	0.031		0.023	-0.497	***	0.007	-0.161	***	0.011
Seasonal	0.869	***	0.033	-0.307	***	0.048	0.430	***	0.017	-0.110	***	0.026
Geographical area - Reference: North-West												
North-East	0.066	***	0.023	-0.080	**	0.034	0.035	***	0.008	-0.063	***	0.012
Centre	0.211	***	0.023	-0.067	**	0.034	-0.033	***	0.009	-0.074	***	0.014
South	0.824	***	0.021	-0.003		0.031	-0.044	***	0.009	-0.042	***	0.014
Islands	0.888	***	0.026	-0.055		0.036	0.045	***	0.012	-0.102	***	0.018
Year at the start of the spell - Reference: 2005												
2006	-0.169	***	0.031	0.058		0.043	-0.070	***	0.014	0.017		0.021
2007	-0.192	***	0.036	-0.029		0.052	-0.079	***	0.016	-0.028		0.025
2008	-0.015		0.037	-0.060		0.053	-0.029	*	0.017	-0.100	***	0.027
2009	-0.236	***	0.044	-0.038		0.063	0.036	*	0.020	-0.108	***	0.031
2010	-0.319	***	0.036	-0.011		0.051	-0.027	*	0.016	-0.131	***	0.024
2011	-0.552	***	0.040	0.058		0.055	-0.174	***	0.017	-0.102	***	0.026
Month of the year at the start of the spell - Reference: January–April												
May–August	0.390	***	0.018	-0.021		0.024	0.436	***	0.008	-0.058	***	0.011
September–December	0.303	***	0.019	0.005		0.026	0.363	***	0.008	0.000		0.012
Regional yearly GDP growth rate	-1.149	***	0.294	0.017		0.432	-0.171		0.129	0.188		0.205

Notes: *** Significant at the 1% level; ** significant at the 5% level; * significant at the 10% level.

Table A.3: Estimation results of the baseline transition intensities, Italy

	Layoff transition intensity (l)			Transition intensity for other termination reason (o)		
	Coeff.		Std. Err.	Coeff.		Std. Err.
Elapsed job spell (weeks)						
[1, 4]	0.000		–	0.000		–
[5, 8]	0.189	***	0.019	0.401	***	0.008
[9, 12]	0.285	***	0.023	0.625	***	0.010
[13, 16]	0.538	***	0.026	0.917	***	0.012
[17, 20]	0.446	***	0.030	0.806	***	0.014
[21, 24]	0.400	***	0.034	0.779	***	0.017
[25, 28]	0.652	***	0.036	1.078	***	0.018
[29, 32]	0.553	***	0.041	0.905	***	0.020
[33, 36]	0.517	***	0.044	0.915	***	0.022
[37, 40]	0.643	***	0.047	1.020	***	0.024
[41, 44]	0.625	***	0.051	1.033	***	0.025
[45, 48]	0.657	***	0.054	1.076	***	0.027
[49, 52]	0.656	***	0.058	1.144	***	0.028
[53, 56]	1.032	***	0.059	1.453	***	0.029
[57, 60]	0.957	***	0.064	1.243	***	0.032
[61, 64]	1.077	***	0.068	1.330	***	0.033
[65, 72]	1.262	***	0.066	1.534	***	0.033
[73, 80]	1.582	***	0.075	1.855	***	0.037
[81, 88]	1.665	***	0.081	1.847	***	0.040
[89, 104]	1.773	***	0.082	1.884	***	0.041

Notes: *** Significant at the 1% level; ** significant at the 5% level; * significant at the 10% level.

Table A.4: Estimated unobserved heterogeneity distribution of the MPH transition intensities and summary statistics of the estimation, Italy

	Layoff transition intensity (l)				Transition intensity for other termination reason (o)			
	Controls		Treated		Controls		Treated	
	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.	Coeff.	Std. Err.
Unobserved heterogeneity support points								
v_1	-5.132 ***	0.067	-5.079 ***	0.062	-3.505 ***	0.031	-3.203 ***	0.032
v_2	-3.028 ***	0.171	-6.356	5.387	-1.658 ***	0.118	-4.411 **	2.166
v_3	-8.719 ***	0.360	-8.563 ***	0.428	-6.766 ***	0.154	-6.893 ***	0.175
v_4	-6.294 ***	0.127	-6.469 ***	0.728	-4.375 ***	0.052	-4.512 ***	0.260
Unobserved heterogeneity logistic weights of the probability masses								
λ_1	1.015 ***	0.129						
λ_2	-1.944 ***	0.179						
λ_3	-1.730 ***	0.111						
λ_4	0.000	–						
Resulting unobserved heterogeneity probability masses								
p_1					1.189			
p_2					0.062			
p_3					0.076			
p_4					0.431			
Log-likelihood					-1,469,772.3			
AIC/N					6.926			
Number of parameters					201			
Number of job spells					424,473			
Number of time-spell observations					6,110,657			

Notes: *** Significant at the 1% level; ** significant at the 5% level; * significant at the 10% level.

B Full set of estimation results of the DiD MPH competing risk model without unobserved heterogeneity

Table B.1: Estimation results of the baseline transition intensities, Italy

	Layoff transition intensity		Transition intensity for other termination reason	
	Coeff.	Std. Err.	Coeff.	Std. Err.
	Elapsed job spell (weeks)			
[1, 4]	0.000	–	0.000	–
[5, 8]	0.078 ***	0.016	0.304 ***	0.007
[9, 12]	0.081 ***	0.018	0.444 ***	0.007
[13, 16]	0.242 ***	0.020	0.647 ***	0.007
[17, 20]	0.057 ***	0.022	0.449 ***	0.008
[21, 24]	-0.067 ***	0.025	0.345 ***	0.009
[25, 28]	0.104 ***	0.025	0.566 ***	0.009
[29, 32]	-0.079 ***	0.028	0.313 ***	0.011
[33, 36]	-0.190 ***	0.031	0.250 ***	0.012
[37, 40]	-0.133 ***	0.033	0.287 ***	0.013
[41, 44]	-0.225 ***	0.035	0.226 ***	0.014
[45, 48]	-0.258 ***	0.038	0.203 ***	0.015
[49, 52]	-0.337 ***	0.040	0.194 ***	0.015
[53, 56]	-0.036	0.039	0.425 ***	0.015
[57, 60]	-0.198 ***	0.044	0.129 ***	0.018
[61, 64]	-0.148 ***	0.047	0.146 ***	0.020
[65, 72]	-0.086	0.040	0.223 ***	0.016
[73, 80]	0.042	0.045	0.353 ***	0.018
[81, 88]	-0.002	0.050	0.216 ***	0.021
[89, 104]	-0.044	0.046	0.105 ***	0.019

Notes: *** Significant at the 1% level; ** significant at the 5% level; * significant at the 10% level.

Table B.2: Estimation results of the impact of accumulated working weeks (Z) on transition intensities, Italy

	Layoff transition intensity				Transition intensity for other termination reason			
	For everybody		Deviation for the treated		For everybody		Deviation for the treated	
	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.
Accumulated working weeks (Z)								
1–11	0.409 ***	0.033	-0.038	0.040	0.740 ***	0.012	0.020	0.016
12–19	0.247 ***	0.033	0.053	0.042	0.426 ***	0.012	0.039 **	0.017
20–27	0.125 ***	0.034	0.060	0.043	0.247 ***	0.013	0.056 ***	0.017
28–35	0.087 **	0.035	0.049	0.044	0.137 ***	0.014	0.054 ***	0.018
36–43	0.040	0.037	0.033	0.045	0.072 ***	0.014	0.042 **	0.018
44–51	0.000	–	0.000	–	0.000	–	0.000	–
52–59	-0.003	0.040	0.095 *	0.049	-0.044 ***	0.015	-0.048 **	0.020
60–67	-0.062	0.045	0.076	0.055	-0.162 ***	0.018	-0.075 ***	0.023
68–75	-0.063	0.049	-0.006	0.060	-0.278 ***	0.020	-0.133 ***	0.026
76 or more	-0.287 *	0.045	-0.123 **	0.049	-0.370 ***	0.017	-0.282 ***	0.021

Notes: *** Significant at the 1% level; ** significant at the 5% level; * significant at the 10% level.

Table B.3: Estimation results of the impact of covariates on transition intensities, Italy

	Layoff transition intensity				Transition intensity for other termination reason			
	For everybody		Deviation for the treated		For everybody		Deviation for the treated	
	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.	Coeff.	S.E.
$(Age - 15)/100$	-2.692 ***	0.266	2.628 ***	0.418	-3.253 ***	0.101	2.409 ***	0.164
$(Age - 15)^2/1000$	0.853 ***	0.072	-0.658 ***	0.105	0.835 ***	0.028	-0.544 ***	0.042
Woman	-0.196 ***	0.014	-0.046 **	0.021	-0.050 ***	0.005	-0.015 **	0.008
Ever received income support	0.242 **	0.107	0.037	0.109	-0.022	0.046	0.134 ***	0.047
Blue–collar job in year before spell start	0.230 ***	0.027	0.066 *	0.035	0.295 ***	0.009	0.033 ***	0.012
Employment contract in the last year before the start of the job spell - Reference: Temporary contract								
Temporary contract	0.127 ***	0.028	0.048	0.034	0.055 ***	0.011	-0.043 ***	0.014
Seasonal employment	-0.083 *	0.044	0.034	0.055	-0.042 **	0.016	-0.006	0.021
No employment	0.076 ***	0.022	0.036	0.031	0.094 ***	0.008	-0.090 ***	0.011
Firm size - Reference: 5 or fewer employees								
Between 6 and 15	-0.147 ***	0.017	-0.021	0.024	-0.094 ***	0.007	0.003	0.011
Between 15 and 50	-0.295 ***	0.019	-0.013	0.027	-0.041 ***	0.008	0.004	0.011
Between 51 and 100	-0.617 ***	0.021	-0.100 ***	0.031	-0.041 ***	0.007	0.017	0.011
More than 100	-0.617 ***	0.027	-0.037	0.039	0.084 ***	0.008	0.011	0.011
Type of contract - Reference: Open-ended								
Temporary	0.125 ***	0.015	0.059 ***	0.021	-0.394 ***	0.006	-0.110 ***	0.009
Seasonal	0.844 ***	0.030	-0.219 ***	0.045	0.400 ***	0.012	-0.021	0.018
Geographical area - Reference: North-West								
North-East	0.048 **	0.022	-0.064	0.033	0.020 ***	0.006	-0.043 ***	0.010
Center	0.218 ***	0.023	-0.042	0.033	-0.031 ***	0.007	-0.037 ***	0.011
South	0.799 ***	0.020	-0.003	0.029	-0.058 ***	0.007	-0.024 **	0.011
Islands	0.850 ***	0.024	-0.042	0.034	0.025 **	0.010	-0.063 ***	0.014
Year at the start of the spell - Reference: 2005								
2006	-0.137 ***	0.029	0.047	0.040	-0.049 ***	0.011	0.005	0.017
2007	-0.146 ***	0.034	-0.028	0.049	-0.045 ***	0.013	-0.027	0.020
2008	0.002	0.034	-0.046	0.049	-0.016	0.014	-0.086 ***	0.021
2009	-0.212 ***	0.041	-0.037	0.058	0.043 ***	0.016	-0.113 ***	0.024
2010	-0.278 ***	0.034	-0.001	0.047	-0.004	0.013	-0.130 ***	0.018
2011	-0.447 ***	0.038	0.073	0.052	-0.095 ***	0.014	-0.088 ***	0.020
Month of the year at the start of the spell - Reference: January–April								
May–August	0.271 ***	0.016	-0.022	0.022	0.333 ***	0.006	-0.064 ***	0.009
September–December	0.169 ***	0.017	-0.063 ***	0.024	0.238 ***	0.006	-0.077 ***	0.009
Regional yearly GDP growth rate	-1.042 ***	0.278	0.119	0.404	-0.136	0.105	0.258	0.160

Notes: *** Significant at the 1% level; ** significant at the 5% level; * significant at the 10% level.

37

C   Further tables and figures

Table C.1: Sharp RDD estimates based on local linear regressions

Panel a) Outcome: exit due to layoff

| | Treated group | | | Control group | | |
	Italy	South	Centre-North	Italy	South	Centre-North
Coeff. (effect at cut-off $Z = 52$)	0.0011	0.0034	0.0004	0.0006	0.0014	0.0006
Robust p-value	0.0310	0.0070	0.3080	0.1010	0.2950	0.1200
Robust lower bound 95% CI	0.0001	0.0009	-0.0004	-0.0001	-0.0010	-0.0002
Robust upper bound 95% CI	0.0022	0.0054	0.0014	0.0013	0.0033	0.0017
Effect in %	0.1477	0.2596	0.0705	0.1070	0.1374	0.1493
Eff. number of obs. (Left)	434,856	154,089	330,294	658,079	139,030	245,014
Eff. number of obs. (Right)	320,549	105,381	244,323	448,039	95,866	203,940

Panel b) Outcome: exit due to resignation

| | Treated group | | | Control group | | |
	Italy	South	Centre-North	Italy	South	Centre-North
Coeff. (effect at cut-off $Z = 52$)	0.0004	0.0010	0.0002	0.0004	0.0005	0.0003
Robust p-value	0.6810	0.2940	0.8400	0.5630	0.6020	0.7040
Robust lower bound 95% CI	-0.0011	-0.0009	-0.0016	-0.0010	-0.0018	-0.0013
Robust upper bound 95% CI	0.0017	0.0030	0.0020	0.0018	0.0032	0.0020
Effect in %	0.0234	0.0948	0.0113	0.0258	0.0378	0.0197
Eff. number of obs. (Left)	393,418	178,803	275,797	430,200	120,452	327,803
Eff. number of obs. (Right)	296,416	117,431	211,016	332,396	86,881	255,066

Notes: This table reports sharp RDD estimates using local linear regression. In Panel a (b) the dependent binary variable y_{it}^{k} is equal to 1 if the layoff (resignation) is observed in two weeks. Z is the forcing variable with cut-off at 52 accumulated working weeks. We used local linear regressions as in Calonico et al. (2014) with the following options: triangular kernel; variance–covariance matrix estimated using the heteroskedasticity-robust nearest-neighbour variance estimator; bandwidth selected based on the MSE-optimal bandwidth selector.

Table C.2: Sharp RDD estimates: heterogeneous effects

| | a) Treated group - layoff | | | | b) Treated group - resignation | | | |
	Smaller	Larger	<2008	≥2008	Smaller	Larger	<2008	≥2008
Coeff. (effect at cut-off $Z = 52$)	0.0022	0.0003	-0.0002	0.0023	-0.0001	0.0008	-0.0001	0.0004
Robust p-value	0.0090	0.5160	0.8130	0.0000	0.9040	0.2430	0.9160	0.3860
Robust lower bound 95% CI	0.0005	-0.0008	-0.0014	0.0013	-0.0023	-0.0007	-0.0024	-0.0007
Robust upper bound 95% CI	0.0039	0.0016	0.0011	0.0037	0.0021	0.0027	0.0023	0.0017
Effect in %	0.2218	0.0513	-0.0233	0.3152	-0.0078	0.0602	-0.0058	0.2530
Eff. number of obs. (Left)	207,873	205,942	270,149	407,058	169,298	226,983	163,001	449,965
Eff. number of obs. (Right)	160,133	170,842	200,841	267,739	138,501	182,961	139,014	286,513

Notes: This table reports sharp RDD estimates using local linear regression by firm size ($\leq$ or $>$ 15 employees) and year of hiring ($<$ or $\geq$ 2008). In column a (b) the dependent binary variable y_{it}^{k} is equal to 1 if the layoff (resignation) is observed in two weeks. Z is the forcing variable with cut-off at 52 accumulated working weeks. We used local linear regressions as in Calonico et al. (2014) with the following options: triangular kernel; variance–covariance matrix estimated using the heteroskedasticity-robust nearest-neighbour variance estimator; bandwidth selected based on the MSE-optimal bandwidth selector.

Table C.3: The impact of accumulated working weeks on the layoff transition intensity with unobserved heterogeneity for the treated

Before UB eligibility (Z < 52)			
1–11 cumulated working weeks ($\lambda_{1l,1}$)	0.482	***	0.032
12–19 cumulated working weeks ($\lambda_{1l,2}$)	0.378	***	0.031
20–27 cumulated working weeks ($\lambda_{1l,3}$)	0.221	***	0.030
28–35 cumulated working weeks ($\lambda_{1l,4}$)	0.147	***	0.028
36–43 cumulated working weeks ($\lambda_{1l,5}$)	0.085	***	0.028
44–51 cumulated working weeks ($\lambda_{1l,6}$)	–		–
After UB eligibility (Z ≥ 52)			
52–59 cumulated working weeks ($\lambda_{1l,7}$)	0.069	**	0.031
60–67 cumulated working weeks ($\lambda_{1l,8}$)	0.024		0.038
68–75 cumulated working weeks ($\lambda_{1l,9}$)	0.096	**	0.045
76 or more cumulated working weeks ($\lambda_{1l,10}$)	-0.500	***	0.049
Log-likelihood			-672,615.7
Number of job spells			184,676
Number of parameters			121

Notes: *** Significant at the 1% level; ** significant at the 5% level; * significant at the 10% level.

Table C.4: Data on frequency of labour inspections across regions

	Number of firms (2015)	Number of audits (2016)	Undeclared jobs found (2016)	Ratio audits/firms (%)	Ratio undeclared jobs/audits
Piemonte	316,258	9,768	2,552	3.1	0.261
Valle d'Aosta	11,223	155	56	1.4	0.361
Liguria	120,647	5,137	1,156	4.3	0.225
Lombardia	786,798	14,758	3,985	1.9	0.270
Trentino Alto Adige	83,418	–	–	–	–
Veneto	384,164	7,985	2,305	2.1	0.289
Fiuli-Venezia Giulia	81,566	3,446	609	4.2	0.177
Emilia-Romagna	360,034	10,406	3,322	2.9	0.319
Tuscany	314,456	10,854	3,502	3.5	0.323
Umbria	65,261	3,935	498	6.0	0.127
Marche	124,092	5,096	1,112	4.1	0.218
Lazio	417,132	11,990	4,526	2.9	0.377
Abruzzo	95,791	5,017	1,211	5.2	0.241
Molise	20,360	2,361	562	11.6	0.238
Campania	330,569	14,043	6,698	4.2	0.477
Puglia	245,374	15,164	5,164	6.2	0.341
Basilicata	34,215	6,849	949	20.0	0.139
Calabria	104,153	8,133	2,812	7.8	0.346
Sicilia	259,346	–	–	–	–
Sardinia	100,816	6,826	2,030	6.8	0.297
Italy	4,339,091	141,920	43,048	3.3	0.303
South (no Sicily)	830,462	51,567	17,396	6.2	0.337
Centre-North (no Trentino Alto Adige)	3,082,447	90,356	25,653	2.9	0.284

Sources: The number of firms comes from ISTAT, *Risultati economici delle imprese*, retrieved from http://dati.istat.it/Index.aspx?DataSetCode=DCSP_SBSREG. The number of audits and the number of undeclared jobs come from National Labour Inspectorate, *Monitoraggio gennaio-dicembre 2016*, retrieved from https://www.ispettorato.gov.it/it-it/studiestatistiche/Pagine/Monitoraggio-trimestrale-attivita-di-vigilanza.aspx.

Figure C.1: Distribution of accumulated working weeks Z_0 at job spell start ($t = 0$)
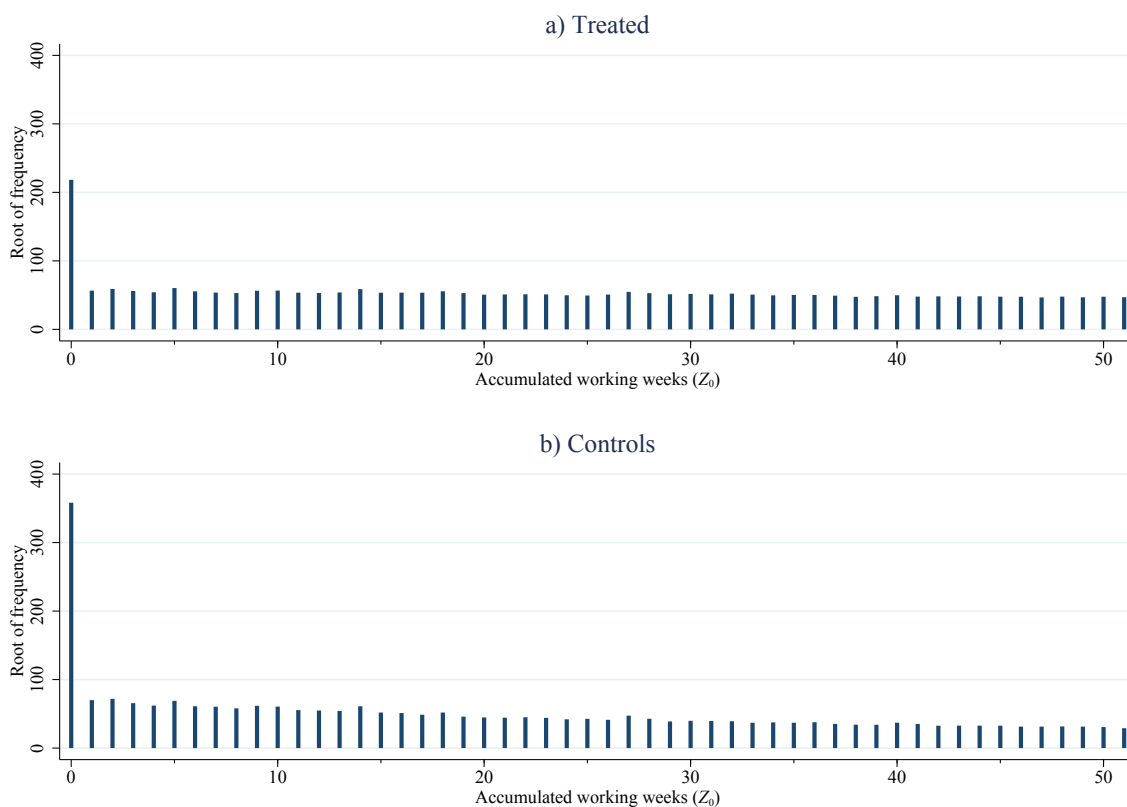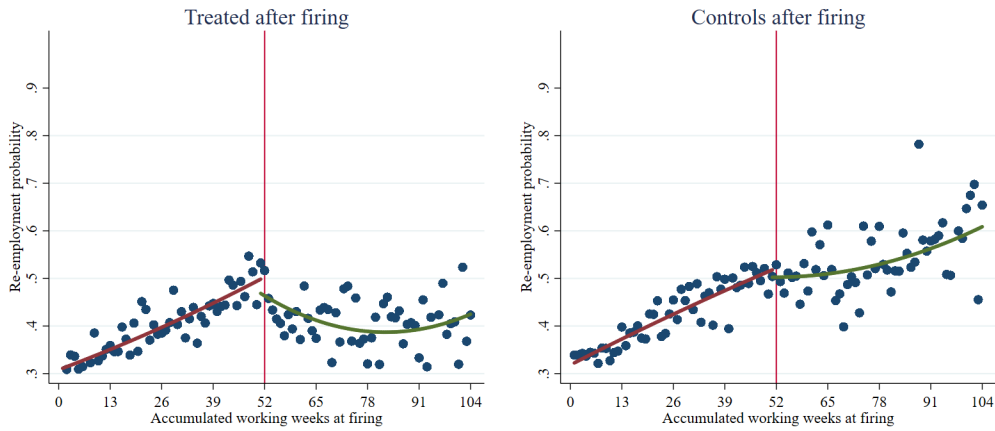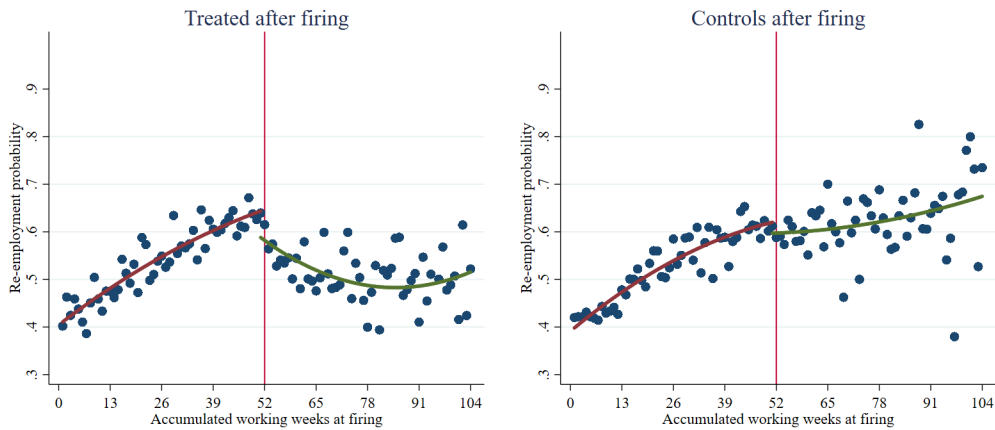
a) Treated

b) Controls

Figure C.2: Predicted re-employment probabilities in 6, 9 and 12 months since firing across the accumulated working weeks (Z) at layoff
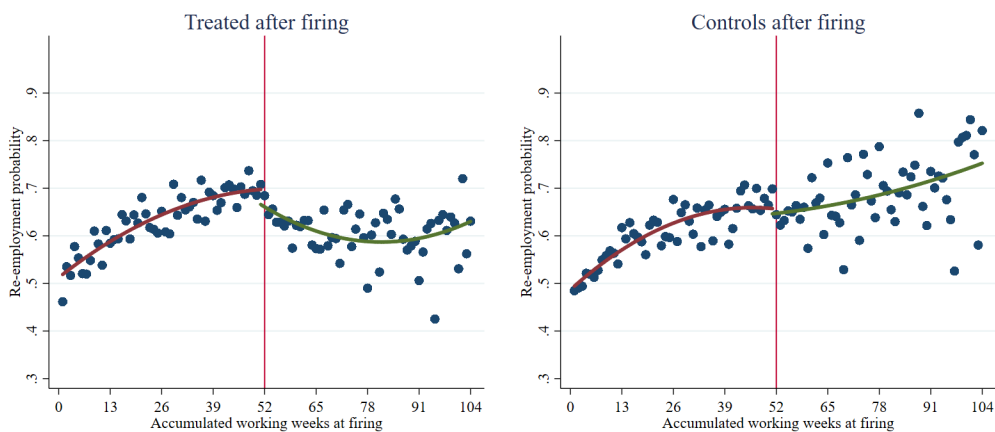
(a) In 6 months after layoff

(b) In 9 months after layoff

(c) In 12 months after layoff

Notes: The re-employment probabilities are predicted at the sample mean of the covariates. They are calculated after the estimation of logit models for the probability of re-entering employment in 6, 9 and 12 months from dismissal, as a function of a full set of dummies for the value of the accumulated working weeks at firing and the other covariates and separately for the treated and the controls. The solid curves are quadratic fits of the predicted probabilities, separately computed to the left and to the right of the cut-off of 52 accumulated working weeks.

41

Figure C.3: Logit estimates of the relation between the accumulated working weeks (Z) and the probability of job exit in 2 weeks by year of hiring
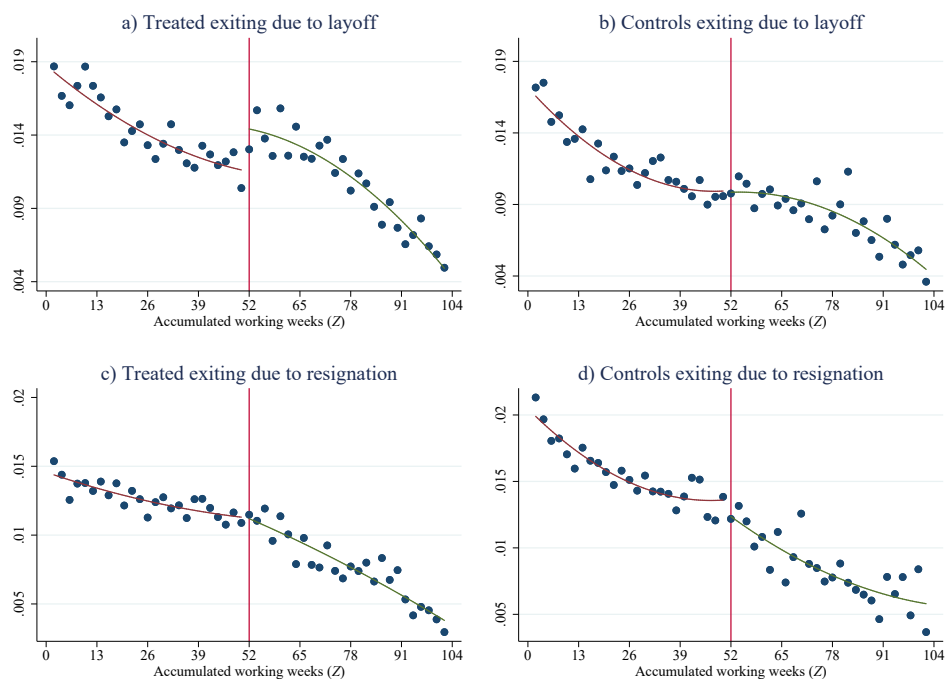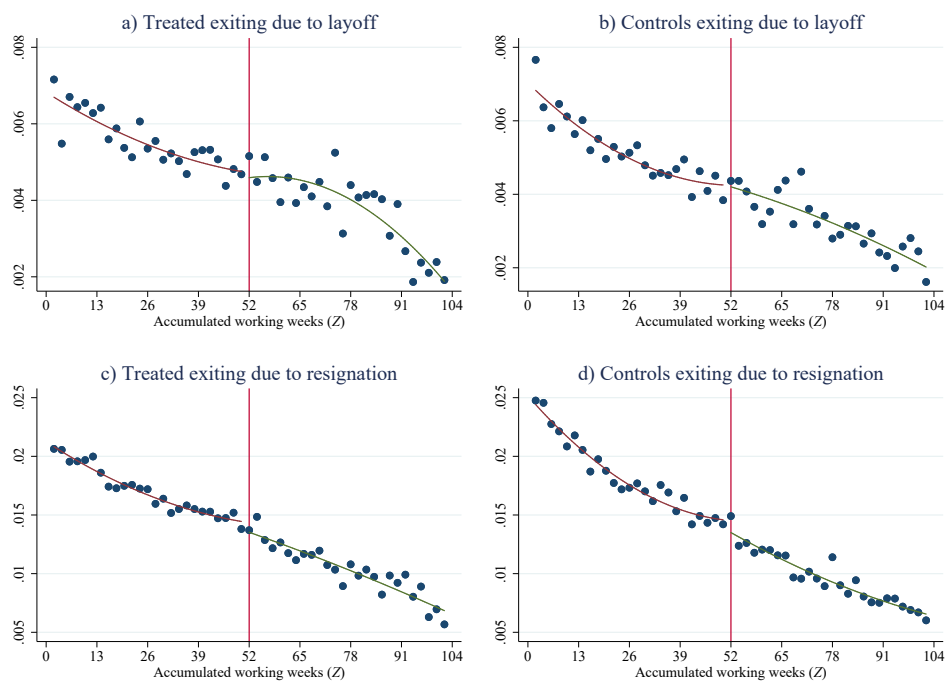
(a) Hiring before 2008
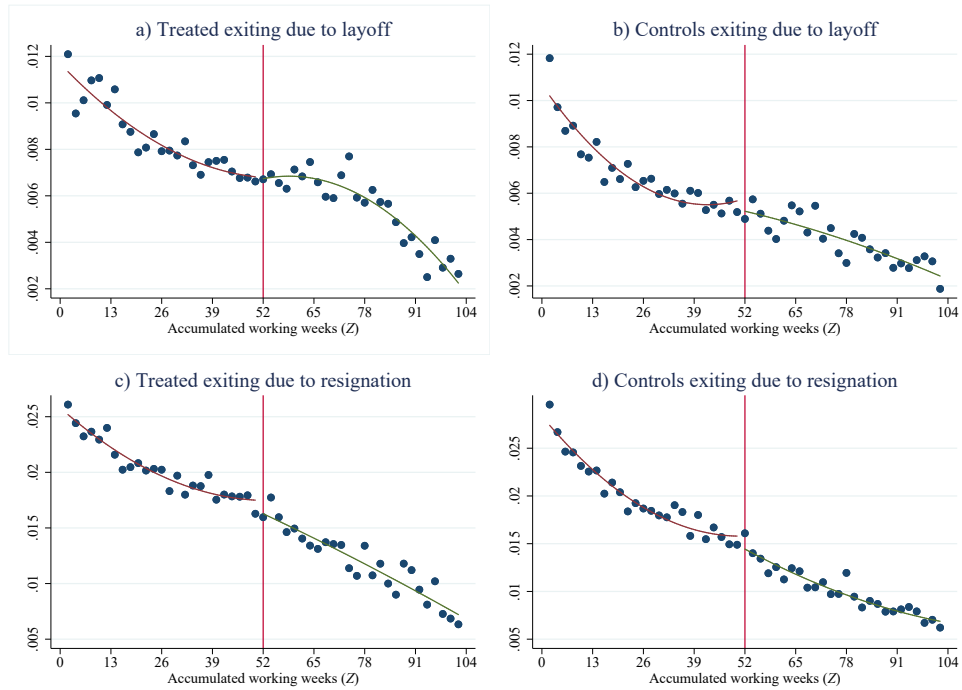
(b) Hiring in 2008 or later
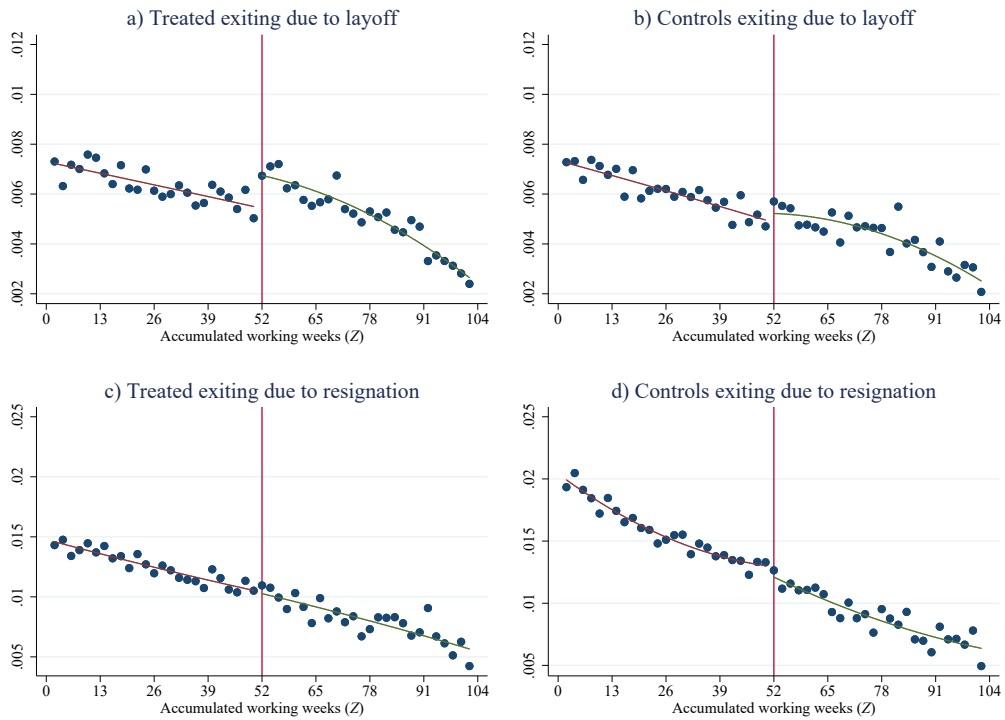
Note: See the footnote of Figure 5.

Figure C.4: Logit estimates of the relation between the accumulated working weeks (Z) and the probability of job exit in two weeks by geographical area
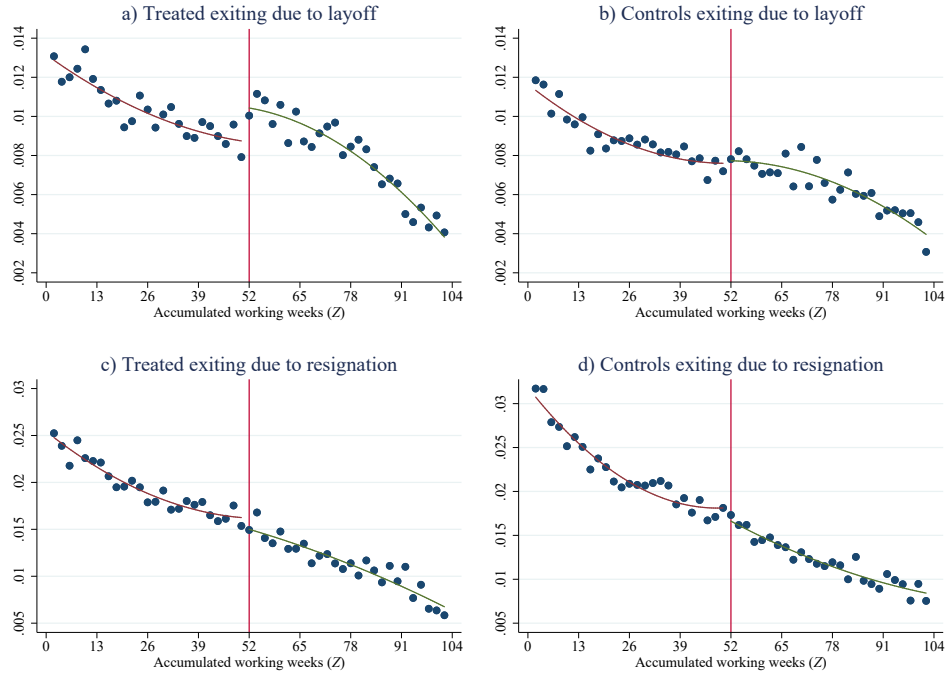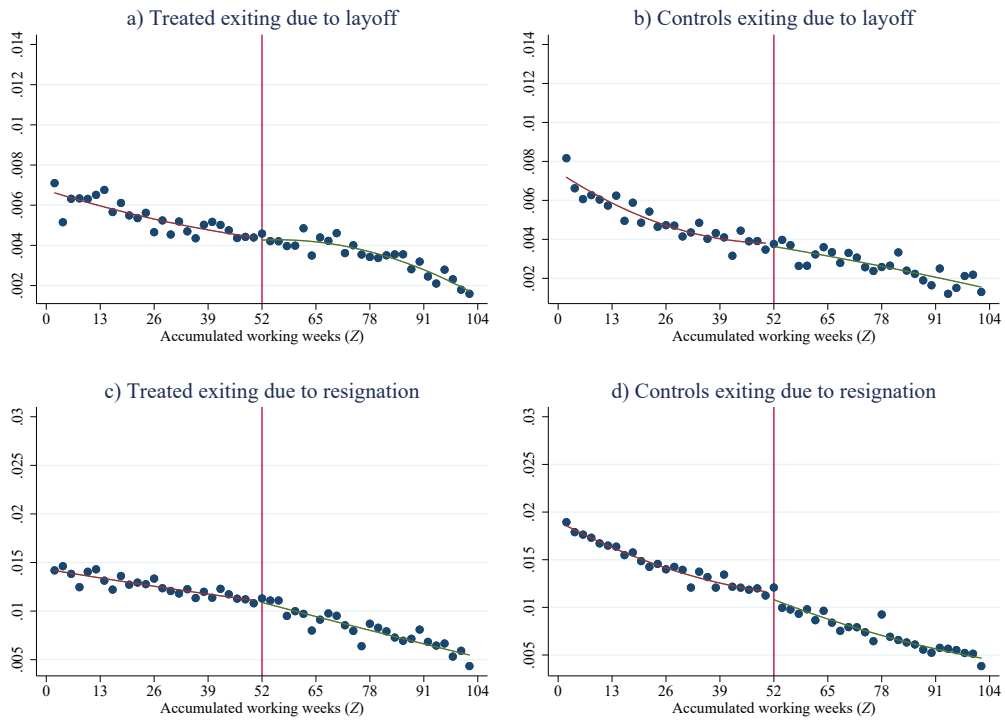
(a) South

(b) Centre-North

Note: See the footnote of Figure 5.

Figure C.5: Logit estimates of the relation between the accumulated working weeks (Z) and the probability of job exit in two weeks by firm size

(a) 15 employees or less

(b) More than 15 employees

Note: See the footnote of Figure 5.

D Likelihood function and estimation of the DiD MPH competing risk model

In this appendix, we describe the likelihood function and its derivation for the DiD MPH model in Subsection 6.1. The contribution to the likelihood function of job spell i is fully characterised by the transition intensities specified in Equation (2). If the job spell is right-censored after t periods, then its contribution is given by the survivor function until the end of the t-th time unit:

$$L_i^c(t|\mathbf{x}_{it}, z_{it}, d_i, \mathbf{v}_i; \mathbf{\Theta}) \equiv S(t|\mathbf{x}_{it}, z_{it}, d_i, \mathbf{v}_i) = \prod_{\tau=1}^{t} \exp\left\{ -\sum_{k\in\{l,o\}} \theta_k(\tau|\mathbf{x}_{i\tau}, z_{i\tau}, d_i, v_{ik}) \right\}, \tag{D.1}$$

where $\tau \in \mathbb{N}$ and $\mathbf{\Theta}$ is the set of unknown parameters. If the job spell i is complete and ends in k, then its contribution to the likelihood function, which is derived below, is as follows:

$$\begin{aligned}
L_i^k(t|\mathbf{x}_{it}, z_{it}, d_i, \mathbf{v}_i; \mathbf{\Theta}) &= \frac{\theta_k(t|\mathbf{x}_{it}, z_{it}, d_i, v_{ik})}{\sum_{r\in\{l,o\}} \theta_o(t|\mathbf{x}_{it}, z_{it}, d_i, v_{ik})} \\
&\times \ [S(t-1|\mathbf{x}_{it-1}, z_{it-1}, d_i, \mathbf{v}_i) - S(t|\mathbf{x}_{it}, z_{it}, d_i, \mathbf{v}_i)]. \tag{D.2}
\end{aligned}$$

Because the likelihood contribution is conditional on the unobservables in $\mathbf{v}_i$, we need to integrate them out after imposing an assumption on their distribution G. To avoid parametric assumptions that are too strict, we follow Heckman and Singer (1984) and assume that the vector $\mathbf{v}_i \equiv (v_{i0l}, v_{i0o}, v_{i1l}, v_{i1o})$ is a random draw from a discrete distribution function with four points of support.[35] The probabilities associated with the mass points sum to one and, for $m = 1, 2, 3, 4$, are denoted by

$$p_m = \Pr(v_{0l} = v_{0lm}, v_{0o} = v_{0om}, v_{1l} = v_{1lm}, v_{1o} = v_{1om}) \equiv \Pr(\mathbf{v} = \mathbf{v}_m)$$

and specified as logistic transforms:

$$p_m = \exp(\lambda_m)\bigg/\sum_{g=1}^{4} \exp(\lambda_g) \quad \text{with} \quad m = 1, \dots, 4 \quad \text{and} \quad \lambda_4 = 0.$$

By defining c_i as the dummy indicator equal to 1 if spell i is censored and 0 if it is complete, the contribution to the likelihood function of spell i with duration t_i (complete or incomplete),

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[35] Although Gaure et al. (2007) suggested choosing the number of support points that minimises the Akaike Information Criterion (AIC), we had to limit their number to four for computational reasons, given that we have to process more than 6 million job-time observations in the benchmark model. When progressively increasing the number of support points up to four, the AIC showed decreasing values.

unconditional on unobserved heterogeneity is:

$$L_i(t_i|\mathbf{x}_{it}, z_{it}, d_i; \boldsymbol{\Theta}, \lambda_1, \lambda_2, \lambda_3, \mathbf{v}_1, \mathbf{v}_2, \mathbf{v}_3, \mathbf{v}_4) =$$
$$\sum_{m=1}^{4} p_m(\lambda_m) \left[c_i L_i^k(t|\mathbf{x}_{it}, z_{it}, d_i; \boldsymbol{\Theta}, \mathbf{v}_m) + (1 - c_i) L_i^k(t|\mathbf{x}_{it}, z_{it}, d_i; \boldsymbol{\Theta}, \mathbf{v}_m) \right] . \quad \text{(D.3)}$$

The sample log-likelihood function which we maximised with respect to the parameters ($\boldsymbol{\Theta}, \lambda_1, \lambda_2, \lambda_3, \mathbf{v}_1, \mathbf{v}_2, \mathbf{v}_3, \mathbf{v}_4$) is given by the sum across the job spells of the natural logarithm of Equation (D.3).

E   Robustness checks

First, we investigate whether our findings are sensitive to the removal of seasonal jobs from the sample, as these are likely to be quite a different type of contract, serving different technological purposes, than open-ended or fixed-term contracts (Model (1) in Table E.1). In the same vein, we retain only permanent jobs in Model (2). In Model (3), the estimation is run using only permanent jobs as in Model (2), but we add additional covariates such as sector, daily salary and collar type in the specification of the transition intensities. These variables are potentially endogenous as forward-looking agents could respond on these margins by anticipating future UI eligibility. Consequently, we excluded them from the benchmark analysis. The estimated ATTs from these three alternative sample definitions and/or model specifications are very much in line with those of the benchmark model, but somewhat larger. For example, when in Model (2) we keep only permanent workers in the sample, the increase in the layoff exit rate amounts to 15.1% and 19.8% at 52–59 and 60–67 accumulated working weeks, respectively. A more substantial effect on permanent jobs was expected because these jobs are more likely to result in firing than temporary positions, for which the employer typically waits until the end of the contract to get rid of the worker.

Second, we parametrically specify the baseline hazards to avoid possible biases coming from too flexible specifications in the baseline hazard and unobserved heterogeneity (Baker and Melino, 2000). As in Baker and Melino's (2000) simulations, in Model (4) we use a cubic polynomial in durations for the baseline hazards of both competing risks. The estimated ATTs are very much in line with those of the benchmark model.

Third, because the right-censoring of the controls becoming treated (i.e. eligible for $C.2$) could be endogenously selective, in Model (5) we consider the change in the treatment status occurring during the spell as a job exit towards the residual termination reason. By doing so, we endogenously take into account the eventual compositional change in the control group. In this check, the point estimates of the ATTs are also close to those of the benchmark model. In Model (6), instead of right-censoring the controls that become treated during the job spell, we retain them and model their job hazard rate while keeping them in the control group. By

doing so, we contaminate the control group, because now they satisfy $C.2$ and in the moment at which they accumulate 52 working weeks over the previous two years, they will be entitled to UB. The estimated effects are therefore expected to be biased towards zero. They can be interpreted as intention-to-treat (ITT) effects or lower bounds of the true effect. Model (7) is like (6) but focuses only on job spells in the South.[36] As expected, the estimated ITT effects are biased towards zero but still significant and sizable in the South.

Fourth, we check the sensitivity of our results to the definition of the treatment group. According to our definition, the treatment group is made up of spells of workers with at least one day and no more than two years of work experience during the $C.2$ period. We limited the work experience to a maximum of two years to enhance comparability with the controls, who have no work experience during the $C.2$ period. In this robustness check, we test whether the results are sensitive to this choice by re-estimating the benchmark model after modifying the maximum work experience to 26 weeks in Model (8) and 156 weeks in Model (9). The latter reproduces results very similar to those of the benchmark model. Model (8) also returns a point estimate of the ATT at 52–59 accumulated working weeks very close to that of the benchmark model, translating into a boost in the layoff exit rate of about 11%. Because we are losing a large part of the treated group,[37] the increase in the standard errors makes the estimated effect not significantly different from zero (p-value 0.102). The point estimate of the ATT for the subsequent interval is instead halved with respect to the benchmark model. However, the standard error increases by almost 30% with respect to the baseline model. Therefore, the resulting 95% confidence interval largely encompasses the corresponding estimate from the benchmark model.

The fifth robustness analysis is a validation test in the spirit of a placebo test. We estimate the impact of UI eligibility on the (voluntary) resignation exit rate. Because the general rule is that workers voluntarily resigning lose UB eligibility (see graph c) in Figure 3), we do not expect significant ATTs for $Z \geq 52$. Operationally, in Model (10) we keep the competing risks structure unchanged, i.e. the number of competing risks is still fixed to two, but now one risk of exit is voluntary resignation and the second risk is the residual category including all other risks of exit (e.g. layoff and the end of a temporary/seasonal job). As expected, the estimated ATTs of Model (10) do not display any evidence of a sudden increase in the job exit rate for voluntarily quitting.

Although we did not find evidence of significant anticipatory effects along Z grouped into intervals of (mainly) eight weeks, in a sixth robustness check, we test if evidence for an anticipatory effect shows up when the worker gets very close to the UB eligibility threshold of 52 accumulated working weeks. We did this in Model (11) by re-estimating the benchmark model with the piecewise constant specification of $\Lambda_{ek}(Z)$ augmented with a further dummy equal to

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[36]The ITT effects in the Centre-North are also close to zero. They are available upon request.
[37]They decrease from 184,676 to 88,677.

1 when $Z = 51$, for $k \in \{l, o\}$ and $e \in \{0, 1\}$. If indeed firms and employees opt for an opportunistic behaviour and agree to wait until UB eligibility before firing, this should be reflected in a dip in the layoff exit rate right before UB entitlement, generating an anticipatory effect with the opposite sign from the one found in the after-treatment period. Model (11) in Table E.1 shows that this extra dummy for $Z = 51$ has the expected negative sign, pointing to a reduction of 5.2% in the layoff exit rate right before UB eligibility. This coefficient is not significantly different from zero, however, and all of the other treatment effects are close to those of the benchmark model. If we instead repeat the same robustness check only in the South (Model (12)), which is where we found the strongest effect on layoffs, then the anticipatory dummy implies a decrease in the layoff exit rate of 25.2%, with a p-value of 0.054.[38]

Finally, as an alternative identification strategy, we implement a regression discontinuity design (RDD) estimator in the duration setting by removing the control units and including a cubic polynomial specification across Z, with different coefficients to the right and left of the cut-off (available upon request). We find that at the cut-off, the layoff exit rate significantly jumps by 17.5% (p-value 0.001). This effect is very much in line with the results in the benchmark model, especially if one considers the slight overestimation coming from the dip right before the cut-off (–5.2%) detected in Model (11).

To conclude, it is worth noting that in some specifications, the ATT at 76 or more accumulated working weeks is significantly negative. However, this is not robust across models and is also present in the validation test on resignations. As Z increases, control units become more and more selective because they represent spells of workers with no previous experience during the $C.2$ period but who survive for at least 76 weeks in the same job. While our model extensively controls for (observed and unobserved) differences between treated and controls, we acknowledge that the comparison may become harder for the very last values of Z. Hence, we refrain from interpreting the coefficient of the differential exit rate for the treated for $Z \geq 76$. For the other coefficients, the robustness checks confirm the reliability of the estimates.

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[38]The coefficients of the other dummies for the pre-treatment period are not significant even at the 10% level, suggesting that an anticipatory effect is present only very close to the eligibility threshold. In the Centre-North, we do not observe the anticipatory effect at the 51st week, which is in line with the lack of effect above the 52nd week (results are available upon request).

Table E.1: Robustness checks on the estimated DiD ATTs

	Coeff.		S.E.	Coeff.		S.E.	Coeff.		S.E.
	Removing seasonal jobs			*Removing all temporary jobs*			*Removing all temporary jobs + sectors, wage, collar*		
	(1)			(2)			(3)		
After UB eligibility (Z ≥ 52)									
52–59 accumulated working weeks ($\lambda_{1l,7}$)	0.1199	**	0.0505	0.1407	**	0.0658	0.1311	**	0.0656
60–67 accumulated working weeks ($\lambda_{1l,8}$)	0.1509	***	0.0574	0.1805	**	0.0737	0.1610	**	0.0731
68–75 accumulated working weeks ($\lambda_{1l,9}$)	0.0937		0.0636	0.1504	**	0.0814	0.1237		0.0800
76+ accumulated working weeks ($\lambda_{1l,10}$)	-0.0535		0.0565	-0.0006		0.0738	-0.0131		0.0698
Number of job spells	401,674			146,539			146,539		
	Parametric baseline			*Modelling the exit of controls that become treated*			*Retaining controls that become treated (ITT)*		
	(4)			(5)			(6)		
After UB eligibility (Z ≥ 52)									
52–59 accumulated working weeks ($\lambda_{1l,7}$)	0.0987	**	0.0491	0.1053	**	0.0494	0.0769		0.0473
60–67 accumulated working weeks ($\lambda_{1l,8}$)	0.0946	*	0.0559	0.0981		0.0562	0.0640		0.0533
68–75 accumulated working weeks ($\lambda_{1l,9}$)	0.0334		0.0621	0.0271		0.0625	0.0312		0.0587
76+ accumulated working weeks ($\lambda_{1l,10}$)	-0.1018	*	0.0545	-0.1711	***	0.0563	0.0176		0.0501
Number of job spells	424,473			424,473			424,473		
	South: retaining controls that become treated (ITT)			*Treated: less than 26 weeks of experience in the biennium before hiring*			*Treated: less than 156 weeks of experience before the biennium at hiring*		
	(7)			(8)			(9)		
After UB eligibility (Z ≥ 52)									
52–59 accumulated working weeks ($\lambda_{1l,7}$)	0.1445	**	0.0690	0.1015		0.0622	0.0945	**	0.0447
60–67 accumulated working weeks ($\lambda_{1l,8}$)	0.1739	**	0.0770	0.0643		0.0717	0.0811		0.0503
68–75 accumulated working weeks ($\lambda_{1l,9}$)	0.1253		0.0858	0.0795		0.0792	0.0345		0.0554
76+ accumulated working weeks ($\lambda_{1l,10}$)	0.0384		0.0772	-0.1883	***	0.0701	0.0180		0.0472
Number of job spells	121,301			328,474			467,127		
	ATTs on resignation (Validation test)			*Including dummy for Z = 51 to capture anticipatory effects*			*South: including dummy for Z = 51 to capture anticipatory effects*		
	(10)			(11)			(12)		
Anticipatory effect at Z = 51				-0.0530		0.1032	-0.2909	*	0.1514
After UB eligibility (Z ≥ 52)									
52–59 accumulated working weeks ($\lambda_{1l,7}$)	0.0364		0.0339	0.1083	**	0.0509	0.1151		0.0752
60–67 accumulated working weeks ($\lambda_{1l,8}$)	0.0269		0.0390	0.1100	*	0.0574	0.1800	**	0.0839
68–75 accumulated working weeks ($\lambda_{1l,9}$)	0.0713		0.0441	0.0565		0.0636	0.0893		0.0934
76+ accumulated working weeks ($\lambda_{1l,10}$)	-0.1079	***	0.0374	-0.0896		0.0559	-0.0831		0.0856
Number of job spells	424,473			424,473			121,301		

Notes: *** Significant at the 1% level; ** significant at the 5% level; * significant at the 10% level.

F   Re-hiring in the same firm

In this appendix, we empirically check whether the probability of being re-hired in the same firm changes around the UI eligibility threshold. First, we select from our sample job spells in the South that end with layoff. We then estimate the probability of re-entering the same firm within 18 months from the layoff with a logit model, as a function of a full set of dummies for each value of Z measured at firing and conditional on the same covariates used in the specification of the benchmark duration model. Figure F.1 displays the predicted probabilities, at the means of the covariates, for each value of Z at firing by treatment status in the South. For both the treated and the control groups, the predicted probability of re-entering the same firm within 18 months is quite flat across the accumulated working weeks and, for the treated group, does not jump at 52 accumulated working weeks. Finally, we obtain very similar findings if we focus on the probability of re-entering the same firm within nine months (available upon request).

Figure F.1: Predicted re-employment probabilities in the 18 months since firing across the accumulated working weeks (Z) at layoff in the South
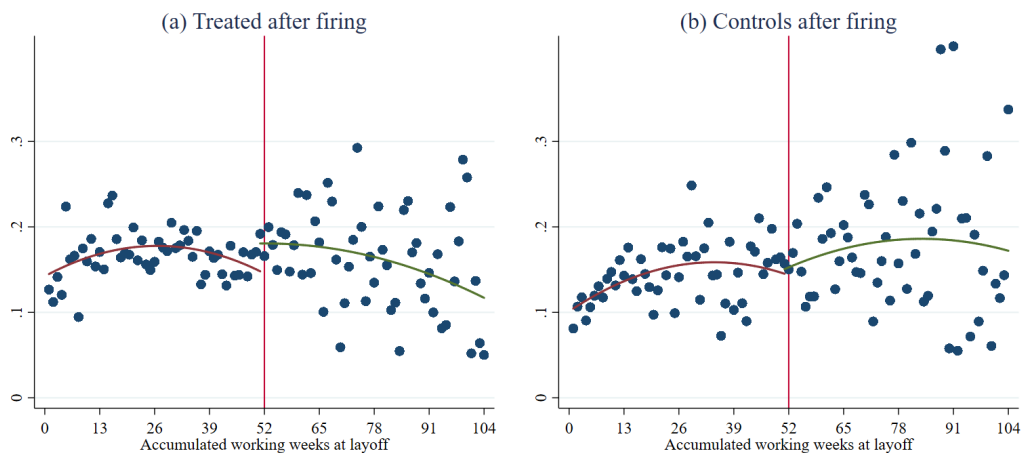
Notes: The re-employment probabilities are predicted at the sample mean of the covariates. They are calculated after the estimation of a logit model for the probability of re-entering employment in the 18 months since dismissal as a function of a full set of dummies for the value of the accumulated working weeks at firing and the other covariates and separately for the treated and the controls. The solid curves are quadratic fits of the predicted probabilities, separately computed to the left and to the right of the cut-off of 52 accumulated working weeks.