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Original

Timed to Say Goodbye: Does Unemployment Benefit Eligibility Affect Worker Layoffs? / Albanese, Andrea; Picchio, Matteo; Ghirelli, Corinna. - In: LABOUR ECONOMICS. - ISSN 0927-5371. - ELETTRONICO. - 65:(2020), pp. 101846.1-101846.20. [10.1016/j.labeco.2020.101846]

Availability:

This version is available at: 11566/264481 since: 2024-09-24T09:01:09Z

Publisher:

Published

DOI:10.1016/j.labeco.2020.101846

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(Article begins on next page)

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

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May 18, 2020

Abstract

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-in-differences estimator, we find an instantaneous increase of about 12% in the layoff probability when unemployment benefit eligibility is attained, which persists for at least 16 weeks. These findings are robust to different identifying assumptions and are mostly driven by jobs started after the onset of the Great Recession and in the South.

Keywords: Unemployment insurance, layoffs, employer–employee moral hazard, difference-in-differences, heterogeneous effects.

JEL classification codes: C31, C41, J21, J63, J65.

*We thank the Ministry of Labour and Social Policies for granting access to administrative individual-level data from the social security registers of the Italian Social Security Institute (LoSai INPS). We are grateful to Ghent University (SHERPPA) for technical support for data management. We thank Bart Cockx and Konstantinos Tatsiramos for their valuable comments. We also thank the participants at the 32nd annual conference of the European Society for Population Economics (ESPE, Antwerp, 2018), the 30th European Association of Labour Economists conference (EALE, Lyon, 2018), the 33rd Italian Association of Labour Economics conference (AIEL, Ancona, 2018), the Counterfactual Methods for Policy Impact Evaluation conference (COMPIE, Berlin, 2018), the seminar of Antwerp University (2018), the seminar of the Bank of Spain (2018), the SemiLux seminar (2018), the International Association for Applied Econometrics conference (IAAE, Nicosia, 2019), the seminar of DiSES (Marche Polytechnic University, 2019), the IZA Workshop on Labor Market Institutions (2019) and the Asian and Australasian Society of Labour Economics Conference AASLE (AASLE, Singapore, 2019). The views expressed in this paper are those of the authors and do not necessarily reflect the views of the Bank of Spain or the European System of Central Banks (ESCB).

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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.¹ 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. 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 twofold.

First, we bring new evidence by exploiting quasi-experimental variation in UB eligibility conditions in Italy. In contrast to the empirical literature on unemployment duration, the effects of UI on layoffs have indeed received little attention.² 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 allows us to carry out a duration analysis in a difference-in-differences (DiD) estimator framework.

Second, 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 the geographical dimension that is related to the puzzling and long-lasting North–South divide in Italy. Our conjecture is that a higher unemployment rate in the South of Italy may drive a different response of the layoff rate to UB eligibility. Furthermore, different social norms (Banfield, 1958; Guiso et al., 2004; Bigoni et al., 2016, 2018) may induce

¹See, among others, Card and Levine (2000), Lalive et al. (2006), van Ours and Vodopivec (2006), Card et al. (2007a,b), Lalive (2008), Schmieder et al. (2012), Tatsiramos and van Ours (2014), and Schmieder and von Wachter (2016).

²Section 2 illustrates the empirical literature on the impact of UI on layoffs.

employees and employers to react in a different way to the same type of moral hazard.

Our results confirm the existence of moral hazard. According to our preferred specification, we find that immediately after reaching UI eligibility, the probability of layoff increases by 12% for at least 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%, 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. We find no evidence of a substitution between quits and layoffs. 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 an RDD. Section 6 presents our preferred model, a DiD design in a duration model, and interprets the results. Section 7 reports a large set of robustness checks of both the RDD estimates and the findings from the DiD duration model. Section 8 concludes. The online Appendix contains further details and estimation results.

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, implicit contract models may explain a boost in dismissals. According to this framework, the job relationship between workers and firms relies on im-

plicit 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 the same line, based on a job search model with UI insurance, Jurajda (2003) shows that, in the presence of firm-specific human capital and training costs, a firm's optimal layoff strategy when facing a cyclical downturn is to fire workers with generous UB entitlements and hire them back once the economy recovers. This is because the firm internalises the fact that these individuals will search less intensively and remain unemployed longer. Once economic conditions improve, the firm may recall them more easily, instead of incurring in training costs for hiring new workers. Furthermore, UB eligibility may attenuate firms' expected separation costs due to possible litigation disputes in countries where judges can have significant discretionary power to determine if a layoff is legitimate. In this context, firms might wait until UI eligibility is attained before firing if they believe that labour judges care about the worker's situation and are more benevolent towards the firm when the worker is eligible for UB. As a matter of fact, Ichino et al. (2003) shows that the decisions of Italian labour courts are biased by local labour market conditions, since judges take into account whether it will be difficult for dismissed workers to find another job.

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 temporary 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 This suggested that firms excessively used layoffs to adjust their workforce when firing workers was less expensive, providing a first evidence that there might be relevant unintended consequences of the set-up of the UI system on job durations and labour market turnover.

Some American empirical studies exploited exogenous changes in the eligibility rules for UBs to study the impact of UB eligibility on employment duration and dismissal rates by means of DiD approaches. 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. Carvalho et al.

³A related literature studied UB provision as an alternative policy to early retirement (Baguelin and Remillon, 2014; Inderbitzin et al., 2016).

⁴In 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.

(2018) and [Van Doornik et al. \(2018\)](#) focused on a Brazilian reform, which in 2015 importantly increased the cumulative tenure for UB eligibility in case of layoff. The former found that UI accounts for 11–13% of the dismissal rates of eligible workers. The latter provided evidence of collusive behaviour between employers and employees to time job turnover with UI benefits, with firms employing workers informally while they receive UB and rehiring them formally at UB exhaustion.

In Europe, important changes in the UB eligibility criteria especially concerned older workers, interacting with early retirement schemes and pathways. [Winter-Ebmer \(2003\)](#) and [Jäger et al. \(2019\)](#) investigated the effect of a 3-year extension of UBs targeted at older workers in Austria during the late 1980s. The former estimated an increase of about 4–11 percentage points in the entry rate into unemployment. The latter found a 28% increase in the job separation rate. Similarly, after the 2003 reduction in older workers' UB duration in the Netherlands, [Tuit and van Ours \(2010\)](#) found that for older workers the spike in the unemployment inflow rate at the UB eligibility attainment disappeared.

Further studies provided evidence on how UI can affect unemployment inflows by looking at the tenure distribution of dismissed workers around the cutoffs for changes in the UI eligibility. Using the dependence on age at the dismissal of the potential benefit duration in Germany, [Schmieder et al. \(2012\)](#) noticed that there were small but significant jumps in the density of UI claim at the age cutoffs. They found evidence suggesting that this was due to firms waiting for a short time until workers are eligible for higher UB. [Khoury \(2019\)](#), exploiting both the introduction of an unemployment program and the discontinuity in the level of UB at a particular value of job tenure in France, found evidence of bunching, i.e. a hole at the left side and a mass at the right side of the job tenure distribution of dismissed workers. The author suggests that the mechanism explaining the bunching mass is employers and employees bargaining over the date of contract end when it is profitable for both parties.

Finally, 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 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.

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 layoff 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 Layoff 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).

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

⁶Examples 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.

- *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 (Bal-
lestrero, 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 (Ar-
ticle 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
than 15 employees dealt with larger expected layoff 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.⁸

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’⁹ 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).

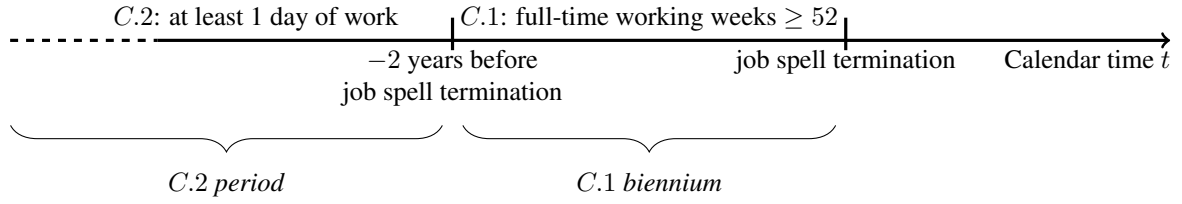
Following termination, the jobless workers satisfying *C.1* and *C.2* had to officially register
their unemployment status at one of the local public employment offices in order to collect UB
payments. UBs were provided for seven months up until 2007 and for eight months after 2007.
The UB amount was related to one’s gross remuneration in the three months before the job loss

⁷On the 19th of July 2012, Law 92/2012 entered into force. This reform modified several aspects of the labour
market by easing the firing costs for larger firms, introducing new unemployment insurance (Aspi and mini-Aspi),
reforming several temporary contracts (e.g. apprenticeships and collaborators), raising employers’ contributions
for temporary jobs, and introducing a firing tax. Most of these changes started to be applied from January 2013.

⁸In 2015, 16.5% of Italians declared having ever been involved in a labour court dispute due to dismissal (20%
among men and 12% among women). This is quite a high number considering that the fraction of those involved
in civil disputes between suppliers and clients is 7.5%, while 5% is the fraction of those involved in bankruptcy
or commercial disputes (ISTAT, 2015).

⁹Examples 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.

Figure 1: Unemployment Benefit (UB) Requirements



and was capped (i.e. €1,014 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. From 2008, the replacement rate was increased by 10 percentage points (Law 247/2007).¹⁰

Workers who did not fully qualify for UBs could be eligible for a reduced version, which was much less generous and 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.

After the exhaustion of the UBs, there is no welfare scheme for the unemployed. The main welfare benefit at the national level is the social allowance (‘assegno sociale’), which is non-contributory, means-tested, and paid to citizens older than 65 falling into poverty.¹¹ However, due to the age requirement, it is not available to the working-age population. Other social inclusion measures may be available at local levels, with their generosity and requirements varying across regions and local authorities.

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 employees¹² working in the private sector. The data contain 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,

¹⁰For 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.

¹¹The allowance is temporary (the requirements are checked every year) and it lasts 13 months.

¹²Semi-subordinate employees are workers with contracts for temporary collaborations that are *de facto* subordinate to the employer but formally self-employed.

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 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.¹³ We denote by Z the random variable indicating the 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.¹⁴

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.

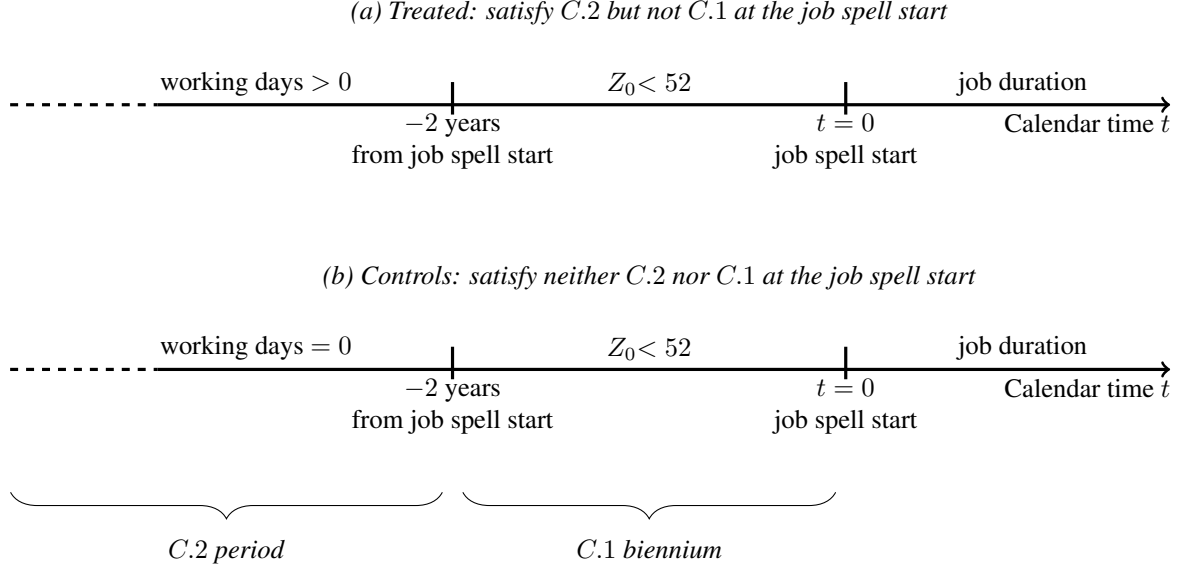
While requirement $C.1$ is standard in UI systems (although the exact numbers vary from country to country), requirement $C.2$ is an Italian peculiarity. If employers and/or employees time an opportunistic behaviour with UI eligibility, we expect to see an increase in the layoff probability when Z_t 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

¹³This 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.

¹⁴In what follows, we express random variables in upper case and their particular realizations in lower case.

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



4.3 Sample selection

We further narrowed our sample as follows. First, we removed the jobs with missing information. Second, we dropped the spells of workers older than 60 at the start of the job. Third, we maintained only full-time jobs. Fourth, 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. Fifth, 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. Finally, we removed spells lying in the bottom or the top percentile of the hourly wage distribution. 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 the summary statistics on completed and incomplete 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%) and for the open-ended contracts (19.0%) than the temporary contracts (9.1%). About 16.2% of the dismissed individuals find a new job within 20 days, compared to 28.0% of the individuals who voluntarily quit their job and 18.5% of workers who reached the termination of their temporary contract.

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 online 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, which is a mass point with higher density for the control group. Table 2 reports summary statistics of the observables that we use as covariates in the econometric analysis. 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).¹⁵ The treated and control groups do not differ in many characteristics (column 8 vs 9). The most notable differences are age (the treated are 3.8 years older on average) and work experience before hiring. These differences are to be expected as the treatment status depends on past employment history. Table 2 also shows how the sample characteristics change while imposing the selection criteria. Compared to the initial sample of 1,766,405 spells (column 1), the treated sample (column 8) is, on average, younger (30.5 vs 35.1 years old), has less frequently used any form of unemployment insurance in the past (12.0% vs 16.2%), and did not have a job in the calendar year before hiring (40.3% vs 26.8%). Other differences are observed for all variables related to labour market experience in the calendar year before hiring.

¹⁵The regional growth rate of the GDP varies on a yearly basis.

Table 2: Summary statistics of the covariates and sample selection

	Initial (1)	No missing (2)	Age < 60 (3)	Full-time (4)	$C:1$ req. $Z_0 < 52$ (5)	$C:2$ req. ≤ 104 (6)	Final sample: Extreme salary (7)	Treated (8)	Control (9)	Standardized bias (10)
<i>Individual characteristics</i>										
Age at the start job	35.1	35.2	34.7	34.9	32.7	28.5	28.4	30.5	26.8	46.7
Woman	0.366	0.363	0.366	0.278	0.307	0.330	0.332	0.320	0.341	-4.4
Ever received income support	0.162	0.163	0.163	0.179	0.120	0.054	0.054	0.120	0.004	49.9
<i>Employment in the calendar year before the start of the job spell</i>										
Blue-collar job	0.525	0.528	0.526	0.558	0.382	0.319	0.320	0.433	0.233	43.3
Open-ended contract	0.352	0.354	0.354	0.377	0.159	0.127	0.127	0.176	0.089	25.7
Temporary contract	0.301	0.300	0.301	0.308	0.301	0.296	0.298	0.359	0.251	23.6
Seasonal employment	0.039	0.039	0.039	0.041	0.049	0.045	0.046	0.063	0.032	14.4
No employment	0.268	0.267	0.267	0.238	0.433	0.482	0.481	0.403	0.628	-46.2
<i>Firm size</i>										
5 employees or less	0.289	0.289	0.288	0.253	0.288	0.275	0.277	0.288	0.268	4.6
Between 6 and 15 employees	0.181	0.182	0.181	0.192	0.192	0.186	0.187	0.189	0.186	1.0
Between 15 and 50 employees	0.161	0.161	0.161	0.179	0.170	0.168	0.168	0.169	0.167	0.6
Between 51 and 100 employees	0.190	0.190	0.191	0.202	0.185	0.192	0.188	0.186	0.190	-0.9
More than 100 employees	0.179	0.178	0.179	0.174	0.166	0.179	0.180	0.167	0.190	-6.0
<i>Type of contract</i>										
Open-ended	0.398	0.400	0.399	0.399	0.358	0.349	0.345	0.350	0.341	1.9
Temporary	0.561	0.560	0.561	0.557	0.589	0.598	0.601	0.591	0.609	-3.5
Seasonal	0.041	0.041	0.040	0.044	0.053	0.053	0.054	0.058	0.050	3.7
<i>Geographical area</i>										
North-West	0.299	0.299	0.301	0.306	0.278	0.288	0.287	0.269	0.302	-7.3
North-East	0.239	0.239	0.240	0.246	0.235	0.245	0.247	0.236	0.255	-4.6
Centre	0.188	0.188	0.187	0.171	0.174	0.180	0.180	0.178	0.182	-1.2
South	0.185	0.186	0.185	0.188	0.211	0.193	0.193	0.211	0.179	8.0
Islands	0.088	0.088	0.088	0.089	0.102	0.093	0.093	0.107	0.082	8.8
<i>Year at the start of the spell</i>										
2005	0.120	0.120	0.120	0.127	0.124	0.128	0.128	0.138	0.121	4.8
2006	0.137	0.137	0.138	0.145	0.142	0.143	0.144	0.155	0.135	5.6
2007	0.165	0.165	0.165	0.169	0.175	0.183	0.184	0.160	0.202	-11.0
2008	0.160	0.160	0.160	0.157	0.155	0.164	0.164	0.135	0.186	-13.8
2009	0.136	0.136	0.136	0.129	0.121	0.121	0.121	0.116	0.124	-2.5
2010	0.144	0.143	0.143	0.138	0.141	0.132	0.131	0.144	0.121	6.9
2011	0.138	0.139	0.138	0.134	0.142	0.129	0.128	0.152	0.110	12.4
<i>Month of the year at the start of the spell</i>										
January–April	0.346	0.347	0.346	0.354	0.310	0.305	0.305	0.305	0.304	0.2
May–August	0.365	0.364	0.364	0.362	0.404	0.408	0.409	0.413	0.406	1.4
September–December	0.289	0.289	0.290	0.283	0.287	0.287	0.287	0.282	0.290	-1.7
Regional GDP growth	0.002	0.002	0.002	0.003	0.002	0.003	0.003	0.004	0.002	6.0
Number of job spells	1,766,405	1,728,401	1,701,906	1,226,779	654,259	433,326	424,473	184,676	239,797	

Notes: Means of the covariates in each sample selection. Columns: (1) initial sample, (2) no spells with missing values in the covariates or the outcomes, (3) no individuals aged more than 60 at hiring, (4) no part-time spells, (5) no spells satisfying $C:1$ criteria at job start ($Z_0 \geq 52$ weeks), (6) no treated units with more than 104 weeks of work experience accumulated during the $C:2$ period at job start, (7) final sample: no first and last percentile of daily salary distribution (< 8 or > 124.1 euros per day), (8) treated, (9) controls, (10) standardized differences of the covariates for treated vs controls ([Rosenbaum and Rubin, 1985](#)).

To get a better idea of how many individuals collect UBs after job termination, 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. First, 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 also 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.¹⁶ Second, for the controls, the UB take-up rate after layoff does not jump at the cutoff (graph b). We can observe a very low fraction of the control group collecting UBs after layoff to the right of the cutoff. In principle, they should not collect UBs because they do not satisfy *C.2*. However, because we evaluated the satisfaction of requirement *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. Third, 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

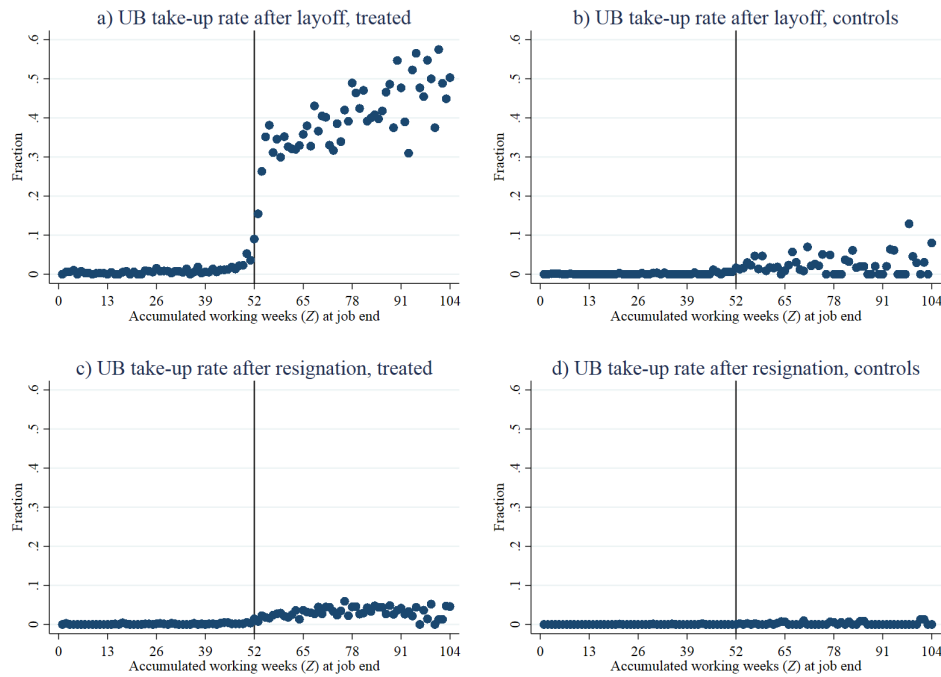
We begin our analysis by providing suggestive evidence of the relationship between UB eligibility and layoff probability. We then estimate the local impact of UB eligibility on the dismissal rate by means of a logit estimator and RDD analysis, which exploits the discontinuity in UB eligibility at the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

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

¹⁶A 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

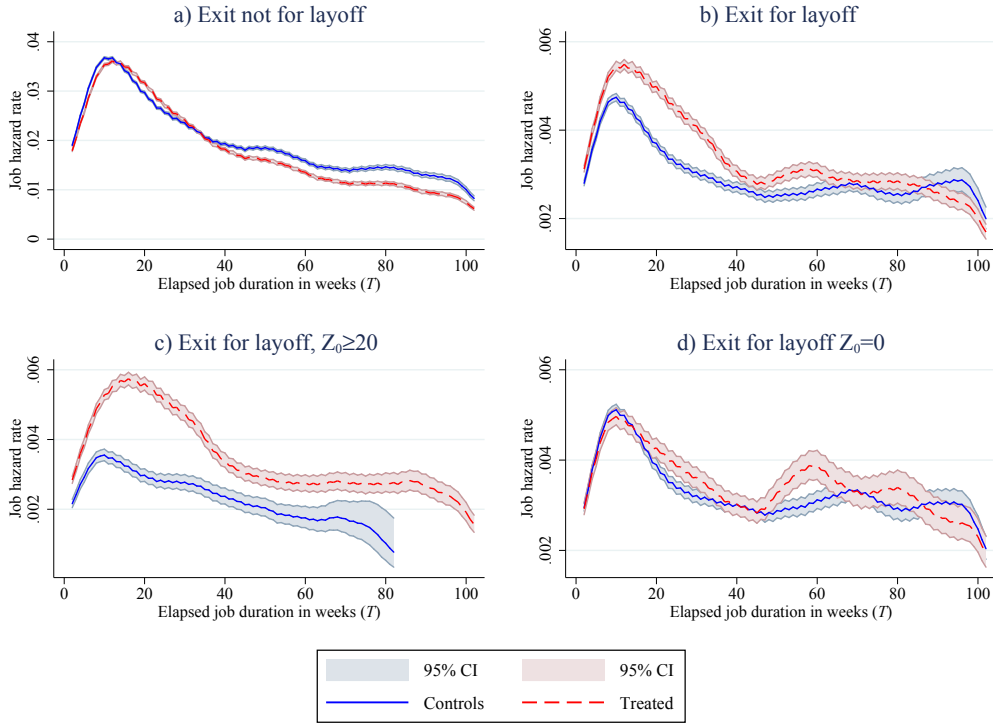


Notes: We plot the fraction of individuals collecting UBs after the end of the job spell by accumulated working weeks (Z) at job termination.

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 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.

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.

5.2 Regression discontinuity design

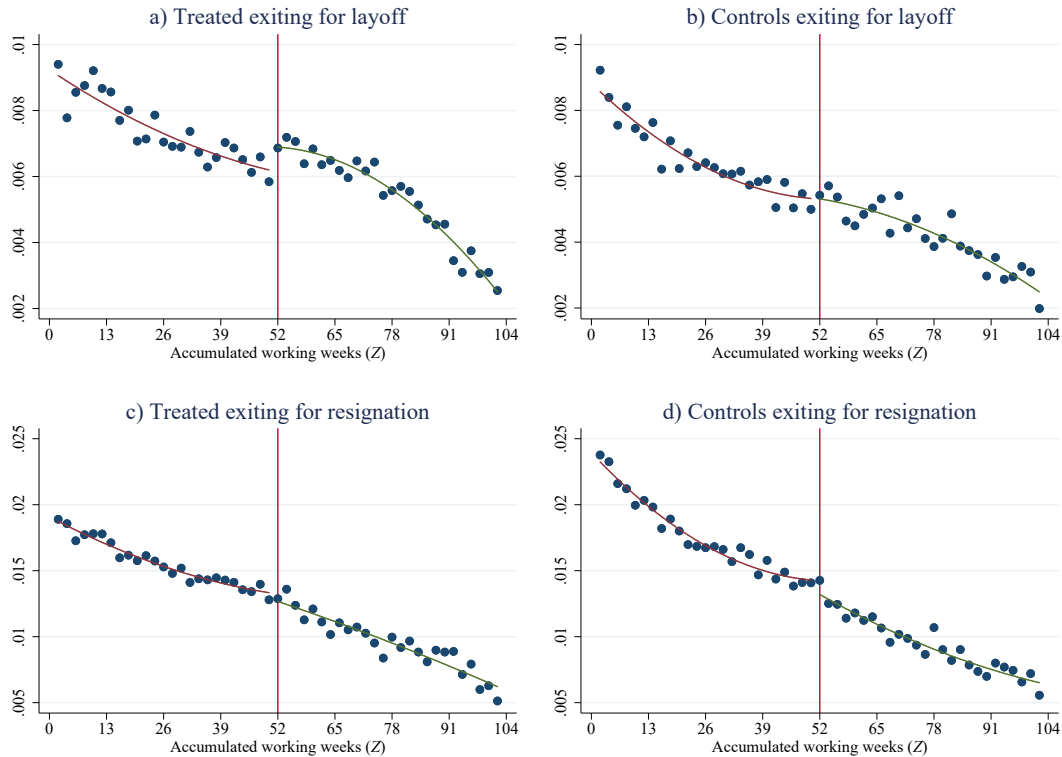
In this subsection, we pooled over t the information on job exit and accumulated working weeks, resulting in 6,110,657 observations. Each job spell contributes to this dataset until either a job exit is observed or the spell is right-censored.

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 that two-week interval because of layoff; and ii) a dummy equal to 1 if the termination in that two-week interval was due to resignation. We regressed these binary responses on the elapsed job duration t ,¹⁷ the set of covariates shown in Table 2 and the working weeks accumulated by the end of each week 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

¹⁷This 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.

effect of the time-varying variable Z_t , as opposed to 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 t , as a function of a full set of 52 dummies for the values of Z accumulated by the end of each t , 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 cutoff.

Figure 5 shows the predicted probabilities of job exit 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 cutoff 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 the working weeks condition $C.1$. Hence, if UB eligibility has an impact on the dismissal 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, unless layoffs become more likely to conceal quits once

the treated become eligible to UB as a result of an agreement between the employer and the employee. 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.¹⁸ The absence of a sudden decrease in the resignation rate for the treated (graph c) is particularly interesting. It suggests indeed that the discontinuity in the layoff probability for the treated is not induced by a simple substitution of quits for layoffs.

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:

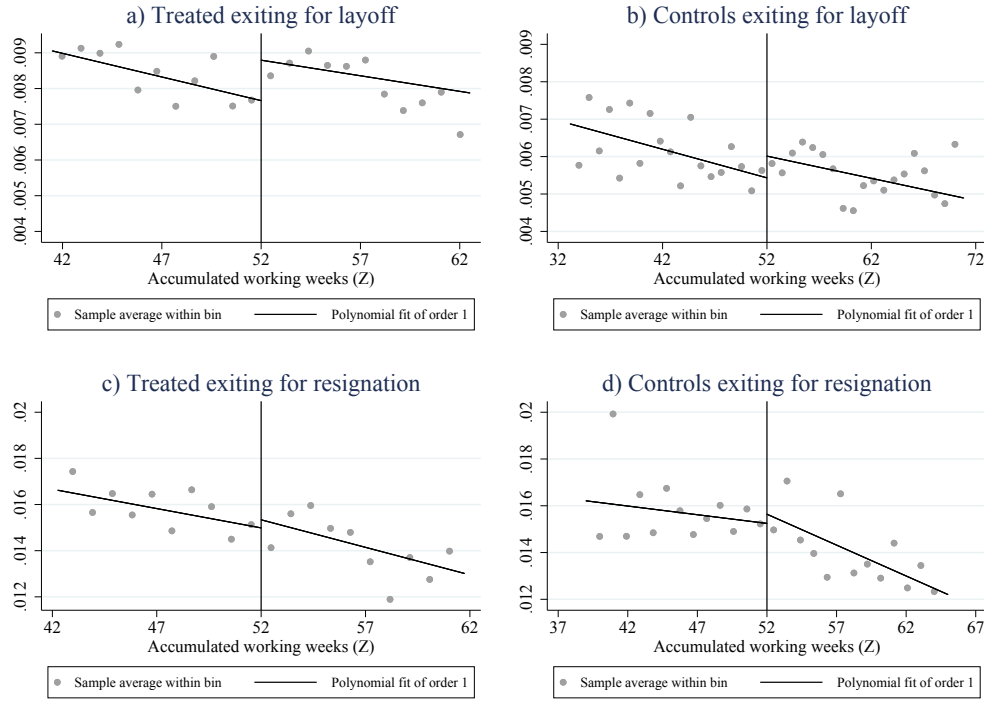
$$y_{it}^k = \alpha^k + \delta^k \cdot \mathbb{1}(z_{it} \geq 52) + \beta^k z_{it} \cdot \mathbb{1}(z_{it} < 52) + \gamma^k z_{it} \cdot \mathbb{1}(z_{it} \geq 52) + \varepsilon_{it}^k, \quad (1)$$

where

- y_{it}^k is equal to 1 if job spell i at elapsed job duration t ends in t 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 week 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 cutoff.
- $\gamma^k z_{it} \cdot \mathbb{1}(z_{it} \geq 52)$ is the linear relationship between the forcing variable and the outcome to the right of the cutoff.
- $\mathbb{1}(z_{it} \geq 52)$ 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 cutoff (LATE) in the relation between the accumulated working weeks and the outcome.
- ε_{it}^k is the idiosyncratic error term (with zero conditional mean).

¹⁸If 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.2–C.4 in online Appendix C.

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



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 t , where Z is the forcing variable with cutoff at 52 accumulated working weeks. We used local linear polynomial 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 online 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.¹⁹

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

¹⁹As 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 = 0.001), in smaller firms (+21%, p -value = 0.019) and in the South (+26%, p -value = 0.007). The LATE in the other dimensions is not significant at the 5% level (see Table C.1 and C.2 in online Appendix C).

relies on the assumption of no manipulation of the forcing variable Z , meaning that firms and workers should not fully control the accumulated working weeks. However, if the agents time the layoff 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 cutoff 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. Finally, as known, the RDD is a local estimator which can have limited external validity to infer the treatment effects on individuals with a different level of labour market experience.

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 dismissal 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 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 cutoff 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.

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:²⁰

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

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.²¹ 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:

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

The 20 coefficients $\gamma_{k,s}$ map the profile of the transition intensity towards risk k .²²

- \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 β_{0k} is the common impact of covariates, whereas β_{1k} captures

²⁰In what follows, we omit the subscript i indicating job spell i for the sake of keeping the notation simple.

²¹To 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.

²²We imposed the innocuous normalization of $\gamma_{k,1}$ to 0 for all $k \in \{l, o\}$.

the deviation from the common impact of the observables for the treated. We later assess the sensitivity of our results to a specification imposing homogeneous effects of \mathbf{x}_t .

- z_t is the time-varying variable measuring the number of working weeks determining the satisfaction of requirement $C.1$. 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 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.²³ 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:

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

- $\mathbf{v}_k \equiv (v_{1k}, v_{0k})$ 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.²⁴ Hence, it 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 online Appendix D, we assume a discrete distribution with four points of support with unknown location of the probability masses.

The parameters $(\lambda_{1l,1}, \dots, \lambda_{1l,10})$ 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 cutoff in the pre-treatment period ($43 < Z \leq 51$).²⁵ Hence, by comparing the periods before and after this cutoff, 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.²⁶

²³The 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.

²⁴Since it is treatment-specific and we do not normalise the means for the treated and the controls to be zero, we cannot include in the specification of the transition intensities the dummy indicator equal to one for the treated usually present in DiD.

²⁵Indeed, 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 $43 < Z \leq 51$.

²⁶Table C.3 in online Appendix C displays the estimation results of the function $\Lambda_l(z_t)$ from an MPH model with

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. 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,²⁷ we can invoke the identification result in [Abbring and van den Berg \(2003a\)](#) for competing risks MPH hazard functions with single-spell data.²⁸

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](#)).²⁹

competing risks estimated using only the treated. 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.

²⁷The 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](#)).

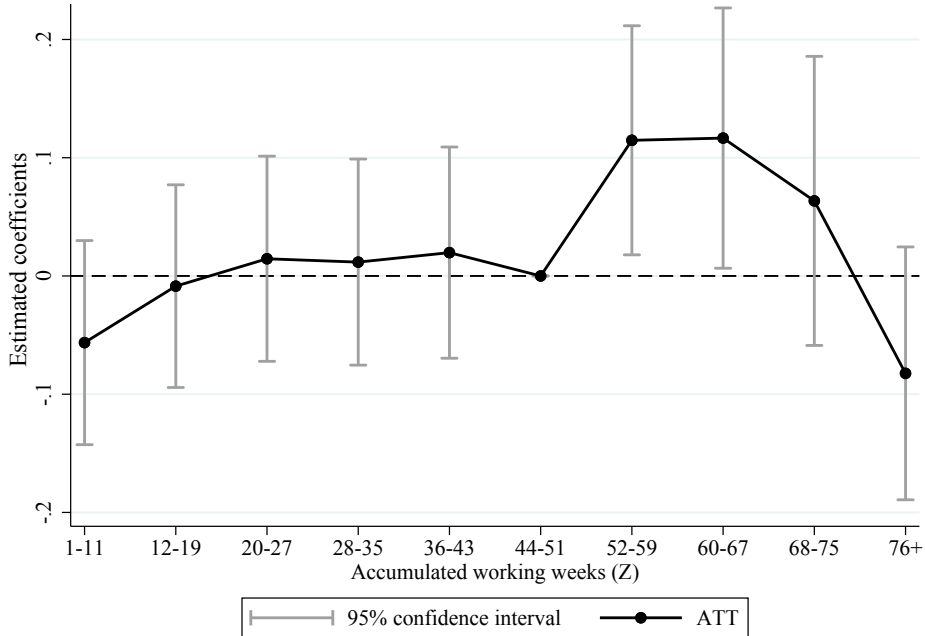
²⁸The identification result in [Abbring and van den Berg \(2003a\)](#) is in continuous time, whereas our durations are grouped into two-week periods. In a large Monte Carlo simulation, [Gaure et al. \(2007\)](#) assessed, however, that time-of-events models à la [Abbring and van den Berg \(2003b\)](#) 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 online Appendix D.

²⁹We 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

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. Online Appendix D provides details on the derivation of the log-likelihood function.

Figure 7 and Table 3 report the estimated coefficients $\hat{\lambda}_{1l,1}, \dots, \hat{\lambda}_{1l,10}$. When $Z \geq 52$ ($\hat{\lambda}_{1l,7}, \dots, \hat{\lambda}_{1l,10}$), the coefficients capture the impact of UB eligibility on the layoff transition intensity. Instead, the coefficients linked to values of $Z < 52$ ($\hat{\lambda}_{1l,1}, \dots, \hat{\lambda}_{1l,5}$) should be seen as a formal ‘placebo test’: their equality to zero shows whether the evolution of the effect of accumulating experience is parallel between treated and controls before the treatment starts. The full set of estimation results of the MPH model are reported in online 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%.³⁰ 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.

Figure 7: Estimated ATTs on the layoff transition intensity



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.4 of online Appendix B.

³⁰12.2% = $[\exp(0.115) - 1] \cdot 100$.

Table 3: Estimated ATTs on the layoff transition intensity

	Coeff.	Std.Err.
<i>Before UB eligibility ($Z < 52$)</i>		
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	-
<i>After UB eligibility ($Z \geq 52$)</i>		
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 $H_0: \lambda_{1l,1} = \dots = \lambda_{1l,5} = 0$	$p\text{-value} = 0.352$	
Wald test $H_0: \lambda_{1l,7} = \dots = \lambda_{1l,10} = 0$	$p\text{-value} = 0.001$	
Number of job spells	424,473	

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

According to the placebo test for the pre-treatment period, none of the coefficients of the pre-treatment dummies $\hat{\lambda}_{1l,1}, \dots, \hat{\lambda}_{1l,5}$ is significantly different from zero. Furthermore, while the Wald statistic for the joint equality to 0 of $\hat{\lambda}_{1l,7}, \dots, \hat{\lambda}_{1l,10}$ confidently rejects the null hypothesis at the 1% level ($p\text{-value} = 0.001$), the coefficients in the pre-treatment period are jointly insignificant ($p\text{-value} = 0.352$). 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.

Finally, although the estimated parameters coming from an MPH hazard model convey a straightforward quantitative interpretation of the impact of UB eligibility on the conditional instantaneous probability of layoff, they are not so informative in terms of job duration or number of jobs ending with a layoff. Hence, we simulate the model over the 104 weeks following job entry and predict job durations and reasons for exit.³¹ We do this for two different cases: 1) for the treated and 2) for the treated but pretending that they enter the sample as controls, and therefore without UB entitlement once the accumulated working weeks exceed 52.³² Simulation 2) serves as a counterfactual for the treated. We can then compare some statistics of interest under simulation 1) and 2) to get an economic interpretation of the effect of interest. Such a comparison of scenario 1) and 2) suggests that, if the UI eligibility was set in place, the median job duration would increase by 3 weeks, whereas the relative and absolute number of layoffs would decrease by 9.2% and 2,664 units, respectively. As our sample represents one-sixth of the population, if we report this number to the larger population we obtain a total of 15,984 excessive layoffs between 2005 and 2012. If we assume that the response is the same for all jobs created in Italy between 2005 and 2006 (excluding the controls), we estimate that a

³¹ At the time at which job spells are right-censored in the original dataset, we stop the simulation. We do not go beyond the point of actual right-censoring to avoid extrapolations beyond the actual observation period.

³² The steps of the simulation procedure are very similar to those used in [Cockx and Picchio \(2012, 2013\)](#) and are described in online Appendix D.

total of 115,393 jobs were dismissed within two years after hiring because of UI eligibility.³³

6.2 Heterogeneous effects

We find a substantial 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.

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 workforce 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.³⁴ 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,³⁵ 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). However, large and small firms differ in 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.³⁶ As shown in panel b) of Table 4, the effect is statistically

³³We multiply the 15,984 excessive layoffs by the relative size of the full sample before any selection, but excluding the controls (1,333,228 jobs) over the selected treated sample (184,676 jobs).

³⁴In 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.

³⁵Conclusions are unchanged if we right-censor the spells still surviving at the end of 2007.

³⁶Since larger firms tend to pay higher salaries, this dimension might also capture differences in replacement rate due to the cap of UBs. However, in our sample this is unlikely to play an important role since we focus

significant only for smaller firms, while for larger firms it is close to zero. Although the point estimates of the ATTs are quite different in size between large and small firms, their difference is not statistically significant. Thus, we refrain from drawing conclusions from this result.

Afterwards, we test whether the effect of the UI is heterogeneous between the South and the rest of Italy. As we found a stronger effect during the financial crisis, we could also detect a larger response in the South, where the unemployment rate is higher and the general economic condition is worse than the rest of Italy.³⁷ At the same time, there is evidence of different social norms between the South and the rest of Italy (Banfield, 1958), which is also reflected in a higher diffusion of undeclared employment.³⁸ 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.³⁹ 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 may suggest that the effect at the national level may mainly be 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 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.

To gain more insight into the relative importance of the role of social norms and the economic condition in driving the more pronounced response in the South, we provide some evidence on the possible transition of the dismissed workers to undeclared employment. 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

on individuals with little work experience, whose salaries tend to be under the cap. This is confirmed by our estimates on the replacement rate, which is 48.1% (47.8%) for individuals working in firms below (above) 15 employees.

³⁷In 2010, the unemployment rate for 15-64 years old people was 13.4% in the South of Italy, against 7.6% in the Center, and 6% in the North (ISTAT, 2018).

³⁸According 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 Table C.8 in online Appendix C for more details on these types of descriptive statistics.

³⁹Southern regions are defined following the European NUTS1 category: Abruzzo, Molise, Campania, Puglia, Basilicata, Calabria, Sicily and Sardinia.

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

	(1)	(2)	(3)	(4)	Significance test of the difference between the ATTs, (1)-(3): <i>p</i> -value ^(a)	
	Coeff.	Std.Err.	Coeff.	Std.Err.		
<i>Panel a)</i>	Before 2008		After 2008			
<i>Before UB eligibility (<i>Z</i> < 52)</i>						
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	–		
<i>After UB eligibility (<i>Z</i> ≥ 52)</i>						
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 : $\lambda_{1l,1} = \dots = \lambda_{1l,5} = 0$	<i>p</i> -value = 0.381		<i>p</i> -value = 0.862			
Wald test H_0 : $\lambda_{1l,7} = \dots = \lambda_{1l,11} = 0$	<i>p</i> -value = 0.520		<i>p</i> -value = 0.000			
Number of job spells	193,616		230,857			
<i>Panel b)</i>	Firms with more than than 15 employees		Firms with fewer than 15 employees			
<i>Before UB eligibility (<i>Z</i> < 52)</i>						
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	–		
<i>After UB eligibility (<i>Z</i> ≥ 52)</i>						
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 : $\lambda_{1l,1} = \dots = \lambda_{1l,5} = 0$	<i>p</i> -value = 0.006		<i>p</i> -value = 0.389			
Wald test H_0 : $\lambda_{1l,7} = \dots = \lambda_{1l,11} = 0$	<i>p</i> -value = 0.372		<i>p</i> -value = 0.003			
Number of job spells	227,526		196,947			
<i>Panel c)</i>	South		Centre-North			
<i>Before UB eligibility (<i>Z</i> < 52)</i>						
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	–		
<i>After UB eligibility (<i>Z</i> ≥ 52)</i>						
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 : $\lambda_{1l,1} = \dots = \lambda_{1l,5} = 0$	<i>p</i> -value = 0.304		<i>p</i> -value = 0.321			
Wald test H_0 : $\lambda_{1l,7} = \dots = \lambda_{1l,10} = 0$	<i>p</i> -value = 0.005		<i>p</i> -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.

^(a) The p -values are for the significance of the equality tests between the coefficients of each pair of independent samples (Clogg et al., 1995; Brame et al., 1998).

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 online Appendix E, the hazard rate of re-hiring by the same firm is similar for workers who at the moment of dismissal accumulated above or below 52 working weeks.⁴⁰ This suggests that the larger effect in the South may not be linked to undeclared work replacing official employment but rather to the worse economic situation.⁴¹

Although we do not have evidence to detect whether our results are driven by employers' or employees' opportunistic behaviour, there are several hints suggesting that the firm side plays a role. First, the boost in the layoff exit rate for workers eligible for UBs after 2008 might have little to do with employees' shirking. The Great Recession should have reduced the willingness of workers to opportunistically exploit the UB system by lowering the value of the outside option. Second, most of the layoffs in our sample are due to economic reasons (91.8%) and not the misconduct of the worker (8.2%).⁴² Third, since voluntary resignation does not generally grant UB eligibility, the absence of a substitution effect between quitting and a layoff might be seen as further evidence that workers cannot easily provoke their dismissals. Overall, this evidence underlines the importance of the demand side in driving our results, but further research is needed to clearly understand the relative importance of the moral hazard coming from both sides of the labour market since it entails very 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).

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 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

⁴⁰The only significant difference is a spike at six months for the treated individuals in the tourism sector in the South, which suggests that UI may be used to cover periods of low economic activity for seasonal workers. This seems unrelated to the effect on the layoff rate because, if we remove the tourism sector, the effects are even larger (results available upon request).

⁴¹We also check whether our results are driven by workers who recursively benefited from unemployment benefits and re-entered into the same firm that previously dismissed them. As our sample of workers has little work experience, only 1% of spells were hirings of workers into a firm where they had already worked and who had also already obtained unemployment benefits. Removing this minority of spells produces very similar estimates.

⁴²If 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, results are very much in line with those from the benchmark analysis (see Table C.9).

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 dismissal and the other covariates. We run the model separately for the treated and the controls. Figure C.8 in online Appendix C displays the predicted probabilities across the working weeks accumulated at dismissal by treatment status. Interestingly, when dismissal 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 may 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 further research and policy interventions.

7 Sensitivity analysis

In this section, we report several robustness checks to test the sensitivity of our estimates, both coming from the RDD and from the duration analysis in a DiD framework.

7.1 Robustness of RDD estimates

We implement the sensitivity tests that are usually run for RDD estimates. First, we move from a local linear polynomial specification to a quadratic one. Second, we include the control variables used in the DiD duration model. Third, we enlarge and shrink by five weeks the bandwidth obtained by the optimal mean squared error criterion of Calonico et al. (2014). Fourth, for both the local linear and the quadratic polynomial specifications we implement placebo tests by testing the effect on false thresholds of Z (i.e. 12, 22, 32, 42, 62, 72, 82). Table 5 reports the results of these sensitivity tests. We run all sensitivity checks not only for the usual grouping of time into two-week intervals (panel b of Table 5), but also when we group time into one-week intervals (panel a of Table 5). Grouping time into one-week intervals should improve the precision of our estimates and avoid eventual heaping-induced bias (Barreca et al., 2016) related to the discretization of time into two-week intervals.

According to the findings in Table 5, the estimation results presented in Subsection 5.2 are very robust. Grouping time into one-week or two-week intervals does not affect the findings. Although the point estimates of the effect in levels differ because we move from the weekly to the biweekly layoff exit rate, the impact in relative terms is rather stable. Using a local quadratic polynomial returns a relative effect of about 8%-15%, which is not far from the one with the local linear polynomial specification (see Figure C.5 in online Appendix C for

a visual inspection). Due to the more flexible local polynomial specification, the effect loses significance at the 5% level. However, after adding the control variables used in the DiD estimator, it again becomes statistically significant in the case of the one-week transition (p -value equal to 0.042) or close to the 10% for the two-week transition (p -value equal to 0.115). Adding the covariates does not affect the point estimates for the one-week transition, while for the two-week transition it makes them more similar to the linear specification (16.1%). Enlarging or shrinking the bandwidth does not change the estimates either, which remains around 12.4%-16.5% in relative terms. Furthermore, the falsification analysis based on placebo cutoffs reveals no significant effect at the false thresholds of Z ,⁴³ neither for the local linear nor the local quadratic polynomial specification (see Table C.4 in online Appendix C).

Table 5: Robustness results for the RDD estimates

	(1) Linear	(2) Linear with Xs	(3) Quadratic	(4) Quadratic with Xs	(5) Linear small bw	(6) Linear large bw	(7) $Z \notin$ [51,52]	(8) $Z \notin$ [50,53]	(9) $Z \notin$ [49,54]
<i>a. One-week transitions</i>									
Effect at cutoff	0.0007***	0.0006***	0.0006*	0.0006**	0.0006**	0.0006***	0.0008***	0.0008**	0.0009***
Robust p -value	0.001	0.001	0.059	0.042	0.012	0.000	0.001	0.002	0.002
Lower 95% CI	0.0003	0.0003	0.0000	0.0000	0.0001	0.0004	0.0004	0.0004	0.0004
Upper 95%	0.0012	0.0011	0.0011	0.0012	0.0011	0.0011	0.0014	0.0016	0.0019
Effect in %	17.69%	16.34%	14.87%	15.51%	15.93%	16.48%	20.30%	19.55%	22.10%
# obs. (left)	1,163,205	1,163,205	1,163,205	1,163,205	777,966	1,546,708	1,161,151	1,158,614	1,157,016
# obs. (right)	797,356	797,356	797,356	797,356	579,078	988,158	759,640	732,277	707,130
<i>b. Two-week transitions</i>									
Effect at cutoff	0.0011**	0.0015***	0.0006	0.0012	0.0010	0.0012***	0.0016***	0.0016**	0.0025**
Robust p -value	0.031	0.001	0.507	0.115	0.109	0.005	0.005	0.011	0.029
Lower 95% CI	0.0001	0.0008	-0.0009	-0.0003	-0.0002	0.0004	0.0005	0.0004	0.0003
Upper 95% CI	0.0022	0.0028	0.0019	0.0025	0.0021	0.0021	0.0030	0.0032	0.0056
Effect in %	14.77%	19.70%	7.97%	16.10%	12.40%	15.48%	21.14%	19.56%	35.31%
# obs. (left)	434,856	434,856	434,856	434,856	355,843	627,777	473,006	469,100	273,562
# obs. (right)	320,549	320,549	320,549	320,549	272,742	428,113	326,167	313,547	189,585

Notes: The dependent binary variable is equal to 1 if the layoff is observed in that week (panel a) or during those two weeks (panel b). Z is the forcing variable with a cutoff at 52 accumulated working weeks. We followed Calonico et al. (2014) with the following options: triangular kernel; variance-covariance matrix estimated using the heteroskedasticity-robust nearest-neighbour variance estimator. Different models: (1) local linear polynomial regression based on the MSE-optimal bandwidth selector (panel a: ± 14.8 & bias ± 29.5 ; panel b: ± 10.5 & bias ± 17.2), (2) local linear polynomial regression adding covariates in Table 2, (3) local quadratic polynomial, (4) local quadratic polynomial adding covariates in Table 2, (5) local linear polynomial regression with a smaller bandwidth (panel a: ± 10 & bias ± 25 ; panel b: ± 8.5 & bias ± 15.2), (6) local linear polynomial regression with a larger bandwidth (panel a: ± 20 & bias ± 35 ; panel b: ± 16 & bias ± 24), (7) donut local linear polynomial regression excluding observations with $Z \in [51,52]$, (8) $Z \in [50,53]$ and (9) $Z \in [49,54]$. * significant at 10% level, ** significant at 5% level, *** significant at 1% level.

The RDD approach relies on the assumption of no manipulation of the forcing variable Z to unveil the local treatment effect. In our framework, firms and employees might adopt an opportunistic behaviour and agree to wait until UB eligibility before the layoff. This would be reflected in a dip in the layoff exit rate, a positive evolution of the density right before UB entitlement, generating an anticipatory effect with the opposite sign than the one in the after-treatment period. To verify the presence of these anticipation effects, which would imply a failure of the no-manipulation assumption, we implement a further set of validity tests.

⁴³We run this falsification analysis by grouping time into one-week intervals to increase precision and therefore increase its power.

First, we drop the observations just around the cutoff, which are most likely to be affected by anticipation, and implement the donut RDD of Barreca et al. (2016). Specifically, we alternatively remove the observations up to 1-, 2-, and 3-week distance from the eligibility and let the local linear polynomial functional form fully extrapolate their values. As shown in columns (7)-(9) of Table 5, the results are even larger.

Second, we check whether the density of Z across the observations in the pooled dataset changes dynamics when approaching the cutoff. In doing this, we need to take into account that we are in a survival framework and each job spell gives as many observations to the pooled dataset used for the RDD as the number of weeks it lasts. Since one of our sample selection criteria is that at the job spell start the accumulated working weeks had to be smaller than 52 ($Z_0 < 52$), the fraction of observations in the pooled dataset with $Z < 52$ will be mechanically larger than the frequency of observations with $Z \geq 52$. Graph a) of Figure C.6 in online Appendix C shows that the frequency of Z across the pooled observations increases somewhat up to 52, followed by a sharp decline. If we had a different sample selection criterion and we selected the treated with a maximum of 40 or 30 accumulated working weeks, we would see that the drop in the profile of the frequency starts at 40 or 30 accumulated working weeks of experience, respectively. Graphs b) and c) of Figure C.6 confirm this and clearly show that at the cutoff of 52, the density of Z is very smooth. If we further restrict the sample to the treated with $Z_0 = 0$ (Graph d) of Figure C.6), we have a further piece of evidence that the density of Z does not change dynamics when approaching the cutoff. This is also confirmed by the density tests of Cattaneo et al. (2019) (see Table C.5), which give results compatible with the visual inspection in Figure C.6.⁴⁴ Finally, as a further check, we rerun the RDD estimates on the subsamples imposing a lower maximum level of Z_0 , which are the groups where we did not observe any jump in the density function. As shown in Table C.6 in online Appendix C, when we focus on these subgroups we obtain an even larger relative effect (ranging from 35% to 20%) and quite similar absolute effects. Due to the smaller sample size, the estimates lose some precision, however.

7.2 Robustness of estimates from DiD duration analysis

First, we assess the robustness of the estimates from the DiD duration analysis to anticipatory effects. Although we did not find evidence of significant anticipatory effects along Z grouped into intervals of (mainly) eight weeks, we now test whether evidence of an anticipatory effect shows up when the worker gets very close to the UB eligibility cutoff. We do this by re-estimating the benchmark model with the piecewise constant specification of $\Lambda_{ek}(Z)$ augmented by a further dummy equal to 1 when $Z = 51$ or $Z \in [50, 51]$ or $Z \in [49, 51]$, for $k \in \{l, o\}$ and $e \in \{0, 1\}$. Model (1) in Table 6 shows that at $Z = 51$ the layoff hazard rate

⁴⁴If we follow the bunching literature (see e.g. Chetty et al., 2011) and test whether the distribution of the density significantly deviates around the threshold, we also obtain compatible results.

decreases by 5.2%. However, this anticipation effect is not significantly different from zero (p -value = 0.608), and all of the other ATTs are close to those of the benchmark model. Furthermore, including $Z = 50$ (Model 2) or also $Z = 49$ (Model 3) in the anticipation dummy completely removes any sign of anticipation. We also test whether changing the reference interval of Z to alternative values, such as $Z \in [11, 19]$ (Model 4) or $Z \in [1, 51]$ (Model 5), affects the estimated ATT. Results are once again very similar to those of the benchmark model.

Second, we test the sensitivity of our findings to alternative specifications of the components of the hazard rates. To begin with, we specify the unobserved heterogeneity term at the individual level, instead of at the job-spell level. There are some gains but also some losses in this swap. The advantage is that exploiting the multi-spell structure of our sample across individuals relaxes the proportionality assumption and the independency between the time-invariant unobserved determinants and the covariates (Abbring and van den Berg, 2003b,a). The loss is that controlling for unobserved determinants at the individual level implies that the unobserved factor is not allowed to vary from job spell to job spell for the same worker. In other words, with unobserved heterogeneity at the job level, we control not only for individual characteristics that are fixed over time but also for all of the unobserved job characteristics that are fixed at the beginning of the job spell.⁴⁵ The results are reported in Table 6, Model (6). They are qualitatively in line with those of the benchmark model but stronger over the full profile of Z , with an immediate jump in the layoff transition intensity of about 18.5%, which persists until the last interval of Z . Afterwards, 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 (7) 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. Finally, in Model (8) we restrict the effect of the control variables and impose homogeneity by treatment group. Estimates are slightly larger but still within the confidence intervals of the estimates of the benchmark model.

Third, we isolate possible anticipation effects of the Law 92/2012 reform by right-censoring the spells that are still ongoing on the 1st of June 2012, a few weeks before the signing of the reform (28 June 2012). Since this right-censoring involves only about 1% of the spells, results are very similar (Model 9). In a second test (available upon request), we bring forward the anticipation period to the 19th of March 2012, a few days before the government announced the reform intention (21 March 2012). We once again get very similar results.⁴⁶

⁴⁵In our sample, we do not have multiple spells for each worker. In fact, for the largest portion of our workers, we only observe one job spell. This is due to the fact that we only consider spells that at entry have not satisfied the *C.1* criterion (i.e. $Z_0 < 52$) and have limited experience in the *C.2* period. In more detail, we observe 424,473 different job spells coming from 253,598 different workers. Out of these 253,598 workers, 149,796 (59.1%) experienced only one job spell, 64,077 (25.3%) have two job spells, and the remaining 39,725 (15.6%) workers have three or more spells (with a maximum of 20).

⁴⁶The RDD findings are also robust to these tests and are available upon request.

Table 6: Robustness checks on the estimated DiD ATTs

	Coeff.			Std.Err.		Coeff.			Std.Err.		Coeff.			Std.Err.																															
	Including dummy for Z = 51 to capture anticipatory effects (1)					Including dummy for Z ∈ [50, 51] to capture anticipatory effects (2)					Including dummy for Z ∈ [49, 51] to capture anticipatory effects (3)																																		
Anticipatory effect	-0.0530					0.1032					-0.0061					0.0792					0.0558					0.0693																			
After UB eligibility (Z ≥ 52)																																													
52–59 accumulated working weeks (λ _{1l,7})	0.1083					**					0.0509					0.1103					**					0.0528					0.1344					**					0.0553				
60–67 accumulated working weeks (λ _{1l,8})	0.1100					**					0.0574					0.1116					*					0.0590					0.1362					**					0.0614				
68–75 accumulated working weeks (λ _{1l,9})	0.0565										0.0636					0.0580										0.0650					0.0829										0.0671				
76+ accumulated working weeks (λ _{1l,10})	-0.0896										0.0559					-0.0882										0.0576					-0.0634										0.0599				
Number of job spells	424,473										424,473										424,473																								
Reference																																													
Z ∈ [11, 19]																																													
(4)																																													
Reference																																													
Z ∈ [1, 51]																																													
(5)																																													
Unobserved heterogeneity																																													
at individual level																																													
(6)																																													
After UB eligibility (Z ≥ 52)																																													
52–59 accumulated working weeks (λ _{1l,7})	0.1171					**					0.0472					0.1060					***					0.0399					0.1697					***					0.0475				
60–67 accumulated working weeks (λ _{1l,8})	0.1185					**					0.0550					0.1040					**					0.0483					0.1977					***					0.0531				
68–75 accumulated working weeks (λ _{1l,9})	0.0640										0.0617					0.0468										0.0555					0.1824					***					0.0582				
76+ accumulated working weeks (λ _{1l,10})	-0.0843										0.0547					-0.1038					**					0.0471					0.1000					**					0.0495				
Number of job spells	424,473										424,473										413,092 ^(a)																								
Parametric baseline																																													
(7)																																													
Homogeneous effects of																																													
control variables X																																													
(8)																																													
Stop on 1st																																													
June 2012																																													
(9)																																													
After UB eligibility (Z ≥ 52)																																													
52–59 accumulated working weeks (λ _{1l,7})	0.0987					**					0.0491					0.1270					**					0.0493					0.1162					**					0.0501				
60–67 accumulated working weeks (λ _{1l,8})	0.0946					*					0.0559					0.1408					**					0.0560					0.1142					**					0.0571				
68–75 accumulated working weeks (λ _{1l,9})	0.0334										0.0621					0.0930										0.0620					0.0620										0.0640				
76+ accumulated working weeks (λ _{1l,10})	-0.1018					*					0.0545					-0.0530										0.0539					-0.0892										0.0560				
Number of job spells	424,473										424,473										424,473																								
Removing seasonal jobs																																													
(10)																																													
Removing all temporary jobs																																													
(11)																																													
Removing all temporary jobs																																													
+ sectors, wage, collar																																													
(12)																																													
After UB eligibility (Z ≥ 52)																																													
52–59 accumulated working weeks (λ _{1l,7})	0.1199					**					0.0505					0.1407					**					0.0658					0.1311					**					0.0656				
60–67 accumulated working weeks (λ _{1l,8})	0.1509					***					0.0574					0.1805					**					0.0737					0.1610					**					0.0731				
68–75 accumulated working weeks (λ _{1l,9})	0.0937										0.0636					0.1504					**					0.0814					0.1237										0.0800				
76+ accumulated working weeks (λ _{1l,10})	-0.0535										0.0565					-0.0006										0.0738					-0.0131										0.0698				
Number of job spells	401,674										146,539										146,539																								
Layoffs to																																													
non-employment																																													
(13)																																													
Layoffs to																																													
employment																																													
(14)																																													
Voluntary																																													
resignation																																													
(15)																																													
After UB eligibility (Z ≥ 52)																																													
52–59 accumulated working weeks (λ _{1l,7})	0.1589					***					0.0557					-0.1174										0.1087					0.0364										0.0339				
60–67 accumulated working weeks (λ _{1l,8})	0.1531					**					0.0631					-0.0648										0.1238					0.0270										0.0390				
68–75 accumulated working weeks (λ _{1l,9})	0.0861										0.0703					-0.0247										0.1342					0.0713										0.0441				
76+ accumulated working weeks (λ _{1l,10})	-0.0944										0.0613					-0.0267										0.1152					-0.1080					***					0.0375				
Number of job spells	424,473										424,473										424,473																								

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

^(a) To reduce the computational burden, we restrict the sample to a maximum of four job spells for each worker (we selected the last four in the observed time window), implying a reduction in the number of job spells by 2.68%.

Fourth, 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 (10) in Table 6). In the same vein, we retain only permanent jobs in Model (11). In Model (12), the estimation is run using only permanent jobs as in Model (11), 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 (11) 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 dismissal than temporary positions, for which the employer typically waits until the end of the contract to get rid of the worker.

Fifth, to ascertain whether the jump in layoffs is related to the UI system, we check whether these layoffs are followed by a spell of unemployment or by a direct transition to another job.⁴⁷ In particular, we distinguish between layoffs followed by a period of unemployment and job-to-job transitions defined as finding a job within three weeks. Operationally, we keep the competing risks structure unchanged, i.e. the number of competing risks is still fixed to two, but now the layoffs not of interest are included in the residual category. We find compatible effects on layoffs followed by unemployment (Model 13) and virtually a zero effect for layoffs followed by a job-to-job transition (Model 14). In a similar way, we estimate the impact of UI eligibility on the (voluntary) resignation exit rate. Because the general rule is that workers resigning lose UB eligibility (see Graph c) of Figure 3), we do not expect significant ATTs for $Z \geq 52$. As expected, the estimated ATTs of Model (15) do not display any evidence of a sudden increase in the job exit rate for voluntarily quitting.

In our benchmark model, those controls that become treated (i.e. satisfaction of C.2) during the job spell are right-censored as of that moment. However, the change of status could be endogenously selective. We check whether the results are sensitive to the imposed right-censoring of the controls becoming treated by modelling transition intensities as in [Rebollo-Sanz \(2012\)](#): we specify the MPH transition intensities as the product of the usual baseline hazard, the systematic part, the unobserved heterogeneity term, and the exponential of a piecewise constant specification of time since eligibility to UBs (both C.1 and C.2 satisfied).⁴⁸ Following the notation of the timing-of-events approach in [Abbring and van den Berg \(2004\)](#), we redefine the

⁴⁷We thank an anonymous reviewer for this suggestion.

⁴⁸In an alternative sensitivity analysis, we instead simply redefined the change in the treatment status occurring during the spell as a job exit towards the residual termination reason. The estimates (available upon request) still show a positive coefficient in the layoff exit rate for the dummy 52–59 weeks.

transition intensities for $k \in \{l, o\}$ as

$$\theta_k(t|\mathbf{x}_t, t_p, \mathbf{v}_k) = \exp \left\{ \Gamma_k(t) + \beta'_k \mathbf{x}_t + v_k + \mathbb{1}(t_p > t) \delta(t|t_p) \right\}, \quad (5)$$

where t_p is the moment of the attainment of UB eligibility. The latter component of Equation (5) captures the differential evolution over time of the hazard rate for those entitled to UBs from the moment at which they get entitled to UBs. The estimated effects are reported in Table 7. Compared to the estimation results reported in Table 3, the effect seems to be longer lasting. However, the estimates reported in Tables 3 and 7 are not directly comparable. In the benchmark model, we report the effect across the value of the accumulated working weeks in the last biennium, whereas in this sensitivity check the effect is over time since UI eligibility. The accumulated working weeks in the last biennium accumulate, on average, at a slower pace than job seniority. For example, for individuals who two years earlier were continuously employed, the accumulated working weeks in the last biennium remain constant over job seniority. This explains why the impact of UB eligibility looks longer-lasting in this sensitivity check.

Table 7: Estimated ATTs on the layoff transition intensity using timing-of-events approach

	Coeff.		Std.Err.
1–8 weeks since UI eligibility	0.067	***	0.026
9–16 weeks since UI eligibility	0.133	***	0.029
17–24 weeks since UI eligibility	0.156	***	0.032
25+ weeks since UI eligibility	0.029		0.026
Number of job spells	424,473		

Notes: *** Significant at the 1% level.

Finally, we implement a regression discontinuity design (RDD) estimator in an MPH model by removing the control units and including a cubic polynomial specification across Z , with different coefficients to the right and left of the cutoff. We find that at the cutoff, the layoff exit rate significantly jumps by 17.3% (p -value 0.001), which is in line with the results of the benchmark model.

7.3 Sensitivity with respect to the definition of the treated group

Our estimates rely on individuals with little work experience (up to 104 working weeks at the moment of job start). In this subsection, we assess the internal and external validity of our results in light of this choice.

First, we enhance the internal validity by imposing a stricter condition on their maximum experience at hiring during the $C.2$ period to make the treated as comparable as possible to the controls. Since the controls have 0 weeks of work experience in period $C.2$ and the treated have instead between 1 day and 104 weeks, they could be too different to compare and show different dynamics in the outcomes along Z_t , which are unrelated to the treatment effect. To assess the

sensitivity of our results to the choice of the treated group, we sequentially restrict the sample of the treated by reducing the maximum number of working weeks in period C.2. Then, in the RDD both in a linear probability model and in an MPH model, we check whether the jump in the layoff probability for the treated at UB eligibility is driven by more experienced workers, who are deemed to be more different from the control units, or also from the treated with low work experience, who are more similar to the controls. Table C.7 in online Appendix C reports the results of this sensitivity analysis. It clearly shows that, even if we are conservative and leave into the treated group only those with at maximum 12 weeks of work experience, the jump in the layoff probability at the moment of UB eligibility is very similar to the one obtained from the baseline sample of the treated. Estimates are much more imprecise, but this is due to the fact that when we restrict the treated to those with at maximum 12 weeks of work experience, their number becomes almost 4 times smaller.⁴⁹

Second, we check the generalizability of our estimates by verifying whether the effect disappears for people with more work experience. In particular, we do not impose the maximum experience requirement during the C.2 period at hiring, which increases the number of treated spells from 184,676 to 401,065 jobs. In principle, enhancing the external validity in this way comes at no cost of internal validity when we use the RDD estimator since in this estimator, we do not directly rely on the control group. On the other hand, this is not true for the DiD estimator as the parallel path assumption between the control and the treated group with greater work experience is more difficult to hold. The pattern is quite similar to the benchmark analysis. In fact, the effect on layoffs appears to be even larger for the treated when individuals with longer work experience are included in the treated group. While the RDD estimator shows an increase in the layoff rate of 30.8% (robust p -value = 0.000), we still do not observe a significant variation in the resignation rate. Similarly, the DiD estimator shows a larger effect (Figure C.7 in online Appendix C). These estimates reassure us regarding the generalisation of the estimated effect, which seems not to be specific only to the analysed population with little work experience.

8 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,

⁴⁹We also run the DiD estimator setting a maximum work experience to 26 weeks, which returns a boost in the layoff exit rate of about 11% for the ATT at 52–59 accumulated working weeks. Because we are losing a large part of the treated group – units decrease from 184,676 to 88,677 – the increase in the standard errors makes the estimated effect not significantly different from zero (p -value equal to 0.102).

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 at least 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. This result holds against a battery of sensitivity analyses and proves to be robust to different empirical frameworks and changes in the definition of the treatment group. We detect heterogeneity in the effects. The results are driven by layoffs of jobs started after 2008 and in the South. A stronger effect during the Great Recession suggests that the demand-side plays a role in driving our results. Indeed, 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. This is speculation, however, and further research is needed to understand the relative importance of the employer-employee moral hazard. Nevertheless, it is in line with the hypothesis that employers exploit the UB system for labour adjustments when negative economic shocks occur ([Zweimüller, 2018](#)).

If this interpretation of the results is correct, our study has important policy implications, and 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 demand-side plays a role in driving the excessive layoffs, the introduction of an experience rating system may be of help to prevent 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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Online 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.	Std.Err.		Coeff.	Std.Err.		Coeff.	Std.Err.		Coeff.	Std.Err.	
<i>Cumulated working weeks (Z)</i>												
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 : $\lambda_{1l,1} = \dots = \lambda_{1l,5} = 0$	$p\text{-value} = 0.352$											
Wald test H_0 : $\lambda_{1l,7} = \dots = \lambda_{1l,10} = 0$	$p\text{-value} = 0.001$											
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 (<i>l</i>)						Transition intensity for other termination reason (<i>o</i>)					
	For everybody			Deviation for the treated			For everybody			Deviation for the treated		
	Coeff.		Std.Err.	Coeff.		Std.Err.	Coeff.		Std.Err.	Coeff.		Std.Err.
(<i>Age</i> – 15)/100	-4.027	***	0.292	3.459	***	0.456	-4.316	***	0.129	2.867	***	0.217
(<i>Age</i> – 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
<i>Employment contract in the calendar year before the start of the job spell - Reference: Temporary contract</i>												
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
<i>Firm size - Reference: 5 or fewer employees</i>												
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
<i>Type of contract - Reference: Open-ended</i>												
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
<i>Geographical area - Reference: North-West</i>												
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
<i>Year at the start of the spell - Reference: 2005</i>												
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
<i>Month of the year at the start of the spell - Reference: January–April</i>												
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.
<i>Unobserved heterogeneity support points</i>								
\mathbf{v}_1	-5.132 ***	0.067	-5.079 ***	0.062	-3.505 ***	0.031	-3.203 ***	0.032
\mathbf{v}_2	-3.028 ***	0.171	-6.356 ***	5.387	-1.658 ***	0.118	-4.411 **	2.166
\mathbf{v}_3	-8.719 ***	0.360	-8.563 ***	0.428	-6.766 ***	0.154	-6.893 ***	0.175
\mathbf{v}_4	-6.294 ***	0.127	-6.469 ***	0.728	-4.375 ***	0.052	-4.512 ***	0.260
<i>Unobserved heterogeneity logistic weights of the probability masses</i>								
λ_1	1.015 ***	0.129						
λ_2	-1.944 ***	0.179						
λ_3	-1.730 ***	0.111						
λ_4	0.000	—						
<i>Resulting unobserved heterogeneity probability masses</i>								
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 (<i>l</i>)		Transition intensity for other termination reason (<i>o</i>)	
	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 (l)				Transition intensity for other termination reason (o)			
	For everybody		Deviation for the treated		For everybody		Deviation for the treated	
	Coeff.	Std.Err.	Coeff.	Std.Err.	Coeff.	Std.Err.	Coeff.	Std.Err.
<i>Accumulated working weeks (Z)</i>								
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 (l)				Transition intensity for other termination reason (o)			
	For everybody		Deviation for the treated		For everybody		Deviation for the treated	
	Coeff.	Std.Err.	Coeff.	Std.Err.	Coeff.	Std.Err.	Coeff.	Std.Err.
$(Age - 15)/100$	-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
<i>Employment contract in the last year before the start of the job spell - Reference: Temporary contract</i>								
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
<i>Firm size - Reference: 5 or fewer employees</i>								
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
<i>Type of contract - Reference: Open-ended</i>								
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
<i>Geographical area - Reference: North-West</i>								
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
<i>Year at the start of the spell - Reference: 2005</i>								
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
<i>Month of the year at the start of the spell - Reference: January–April</i>								
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.

Table B.4: Estimated constant terms of the MPH transition intensities and summary statistics of the estimation, Italy

	Layoff transition intensity (<i>l</i>)						Transition intensity for other termination reason (<i>o</i>)					
	Controls			Treated			Controls			Treated		
	Coeff.	Std. Err.		Coeff.	Std. Err.		Coeff.	Std. Err.		Coeff.	Std. Err.	
<i>Constant terms</i>												
\mathbf{v}_1	-5.082	***	0.054	-5.212	***	0.054	-3.406	***	0.020	-3.369	***	0.022
Log-likelihood							-1,471,798.4					
AIC/ <i>N</i>							6.936					
Number of parameters							186					
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.

C Further tables and figures

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

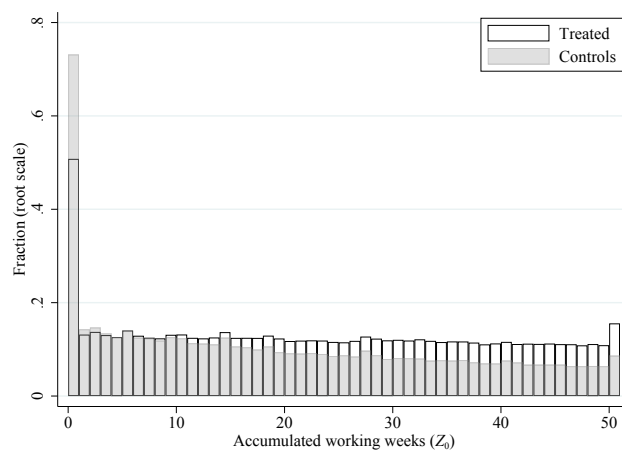


Table C.1: Sharp RDD estimates based on local linear polynomial regressions

<i>Panel a) Outcome: exit due to layoff</i>						
	Treated group			Control group		
	Italy	South	Centre-North	Italy	South	Centre-North
Coefficient (effect at cutoff $Z = 52$)	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
Number of observations (Left)	434,856	154,089	330,294	658,079	139,030	245,014
Number of observations (Right)	320,549	105,381	244,323	448,039	95,866	203,940
<i>Panel b) Outcome: exit due to resignation</i>						
	Treated group			Control group		
	Italy	South	Centre-North	Italy	South	Centre-North
Coefficient (effect at cutoff $Z = 52$)	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
Number of observations (Left)	393,418	178,803	275,797	430,200	120,452	327,803
Number of observations (Right)	296,416	117,431	211,016	332,396	86,881	255,066

Notes: This table reports sharp RDD estimates using local linear polynomial regression. In Panel a (b) the dependent binary variable y_{it}^k is equal to 1 if the layoff (resignation) is observed in t . Z is the forcing variable with cutoff at 52 accumulated working weeks. We used local linear polynomial 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
Coefficient (effect at cutoff $Z = 52$)	0.0021	0.0003	-0.0003	0.0023	-0.0002	0.0008	-0.0003	0.0007
Robust p -value	0.0190	0.4010	0.7510	0.0010	0.8020	0.1980	0.8250	0.3090
Robust lower bound 95% CI	0.0003	-0.0006	-0.0014	0.0009	-0.0024	-0.0005	-0.0023	-0.0008
Robust upper bound 95% CI	0.0036	0.0016	0.0010	0.0037	0.0018	0.0027	0.0018	0.0025
Effect in %	0.2098	0.0633	-0.0332	0.3152	-0.0133	0.0644	-0.0162	0.0624
Number of observations (Left)	207,873	226,983	321,920	235,772	207,873	246,032	216,412	235,772
Number of observations (Right)	149,707	170,842	219,518	169,794	149,707	182,961	160,906	169,794

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 is equal to 1 if the layoff (resignation) is observed in t . Z is the forcing variable with cutoff at 52 accumulated working weeks. We used local linear polynomial 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

	Coeff.	Std.Err.
<i>Before UB eligibility ($Z < 52$)</i>		
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 ($\lambda_{1l,6}$)	–	–
<i>After UB eligibility ($Z \geq 52$)</i>		
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.

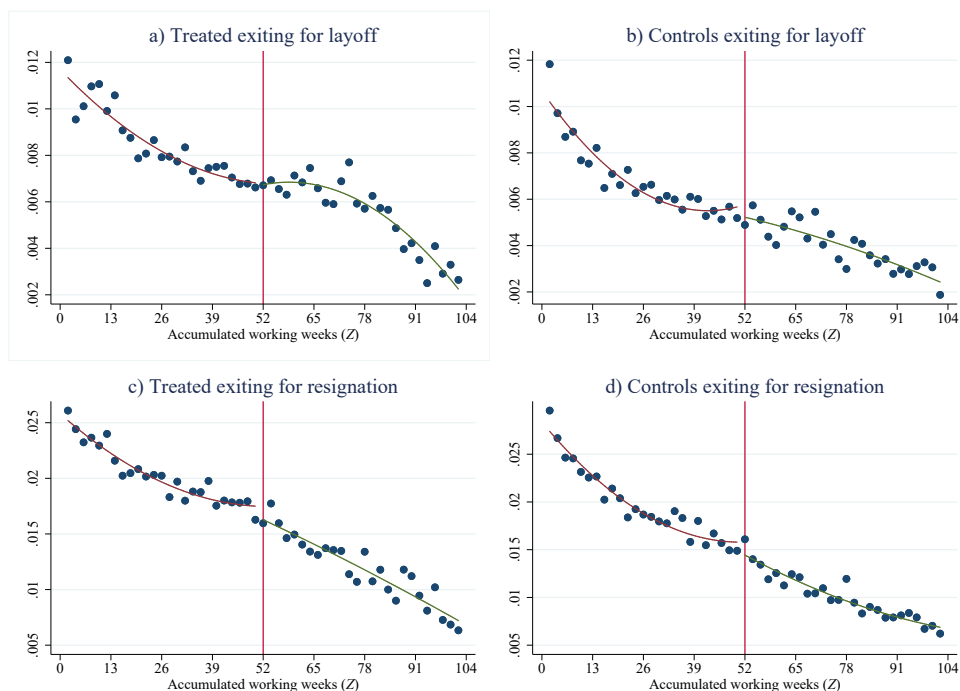
Table C.4: Impact of UB eligibility on the layoff exit rate at false cutoffs (one-week transition)

False cutoffs	12	22	32	42	52	62	72	82
<i>(a) Linear</i>								
Coefficient (effect at cutoff)	0.0001	-0.0001	0.0001	0.0000	0.0007***	-0.0003	0.0001	0.0001
Robust p -value	0.600	0.914	0.521	0.855	0.001	0.165	0.543	0.775
Robust lower bound 95% CI	-0.0008	-0.0005	-0.0003	-0.0005	0.0003	-0.0008	-0.0004	-0.0005
Robust upper bound 95% CI	0.0005	0.0005	0.0006	0.0004	0.0012	0.0001	0.0007	0.0007
Effect in %	1.56%	-1.90%	1.87%	-0.50%	17.69%	-8.57%	4.19%	4.14%
Number of observations (Left)	699,169	1,024,321	1,075,224	1,138,449	1,163,205	970,835	664,673	514,657
Number of observations (Right)	1,044,659	1,108,779	1,154,951	1,101,451	797,356	579,303	461,236	378,019
<i>(b) Quadratic</i>								
Coefficient. (effect at cutoff)	-0.0002	0.0001	0.0003	0.0001	0.0006*	0.0000	0.0000	0.0002
Robust p -value	0.174	0.595	0.377	0.724	0.059	0.840	0.913	0.568
Robust lower bound 95%CI	-0.0003	-0.0005	-0.0003	-0.0005	0.0000	-0.0006	-0.0008	-0.0006
Robust upper bound 95%CI	0.0014	0.0008	0.0009	0.0007	0.0011	0.0008	0.0007	0.0010
Effect in %	-3.07%	2.23%	6.27%	1.30%	14.87%	1.02%	-0.41%	6.67%
Number of observations (Left)	699,169	1,024,321	1,075,224	1,138,449	1,163,205	970,835	664,673	514,657
Number of observations (Right)	1,044,659	1,108,779	1,154,951	1,101,451	797,356	579,303	461,236	378,019

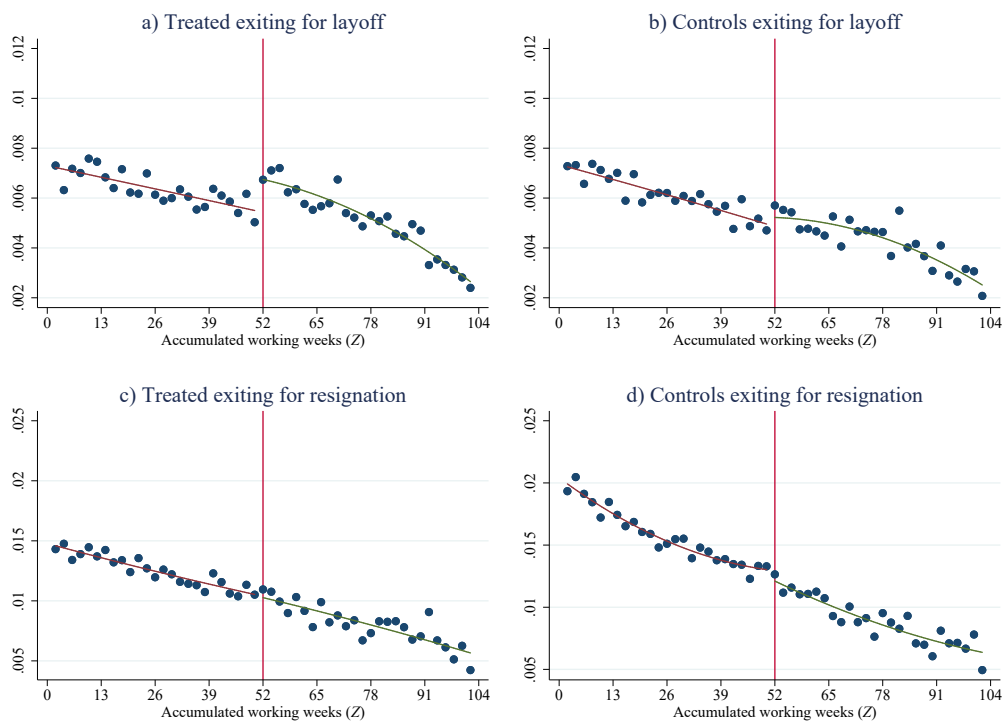
Notes: This table reports sharp RDD estimates using local linear polynomial regression (a) or local quadratic polynomial regression (b). The dependent binary variable is equal to 1 if the layoff is observed in t . Z is the forcing variable with a cutoff at 12, 22, 32, 42, 52, 62, 72, 82 accumulated working weeks. We used local linear polynomial 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. * significant at 10% level, ** significant at 5% level, *** significant at 1% level.)

Figure C.2: 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



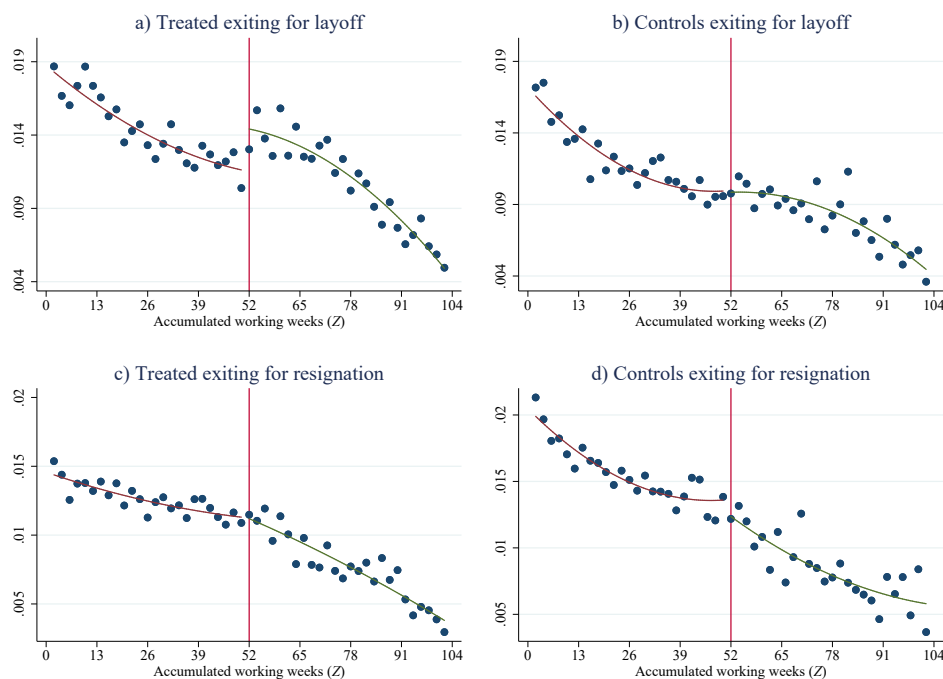
(b) Hiring in 2008 or later



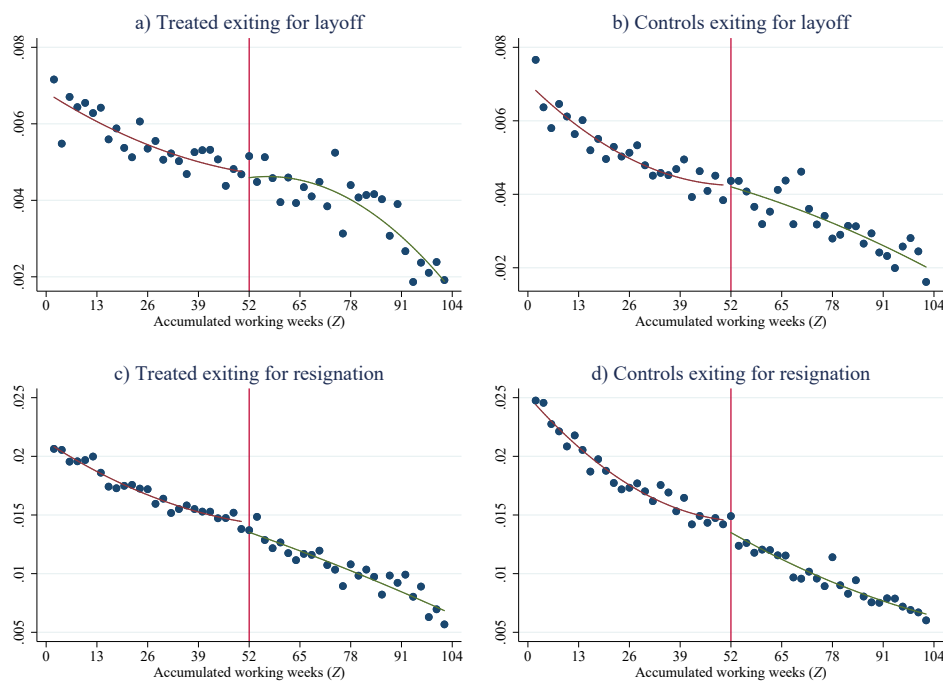
Note: See the footnote of Figure 5.

Figure C.3: 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



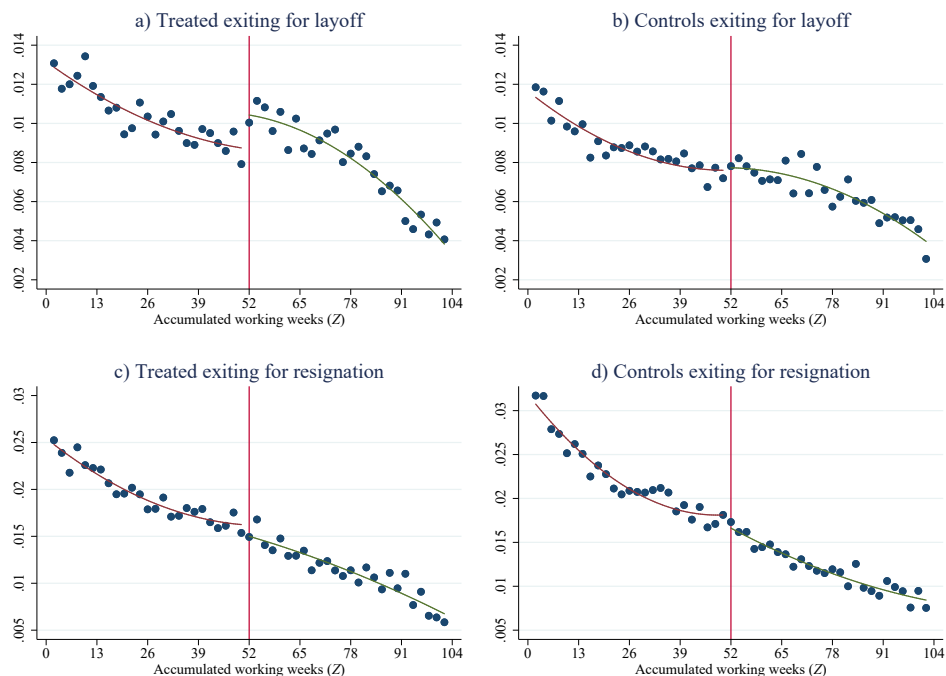
(b) Centre-North



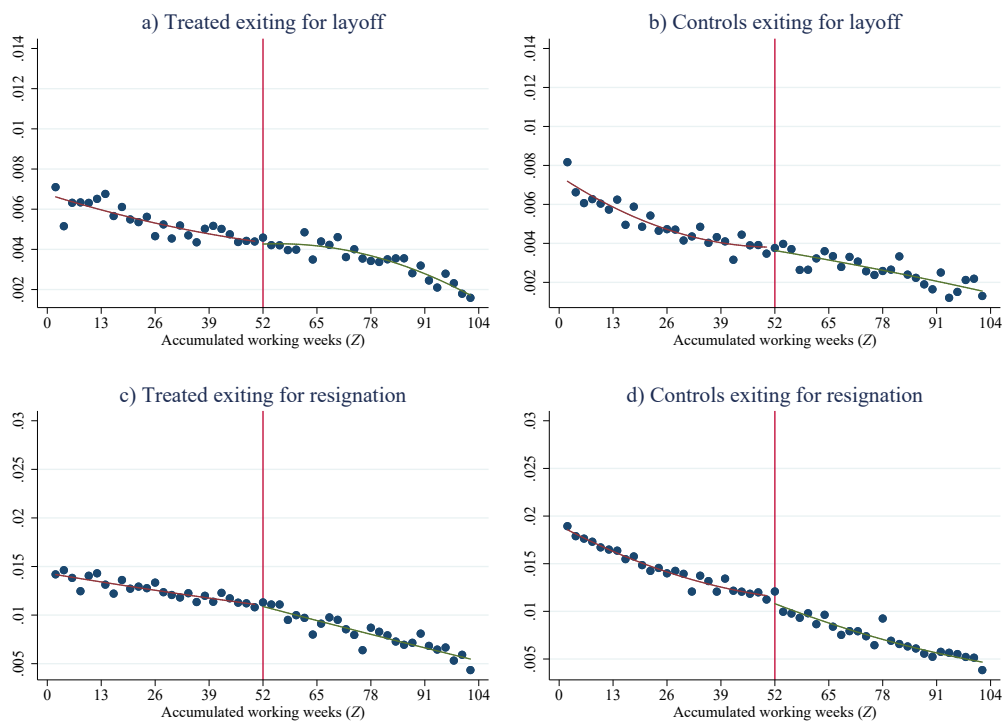
Note: See the footnote of Figure 5.

Figure C.4: 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

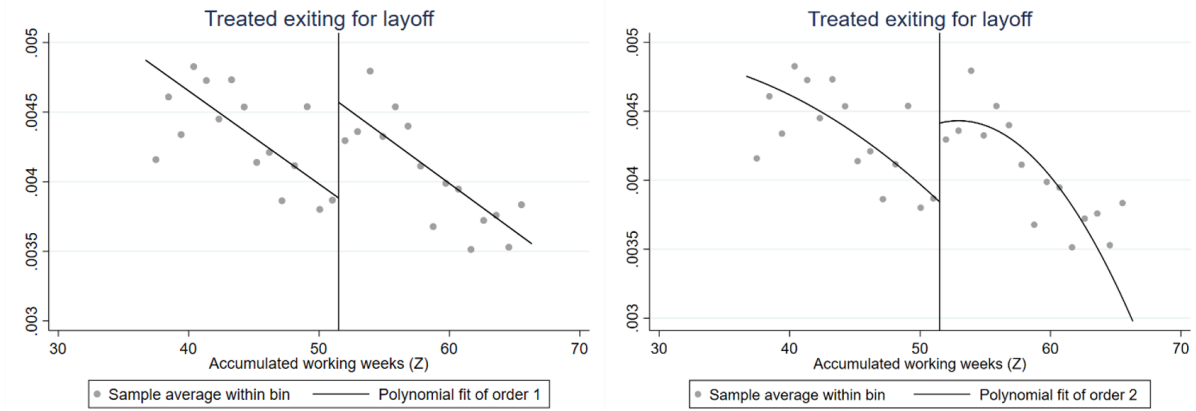


(b) More than 15 employees



Note: See the footnote of Figure 5.

Figure C.5: RDD-predicted probabilities of layoff (one-week transition)



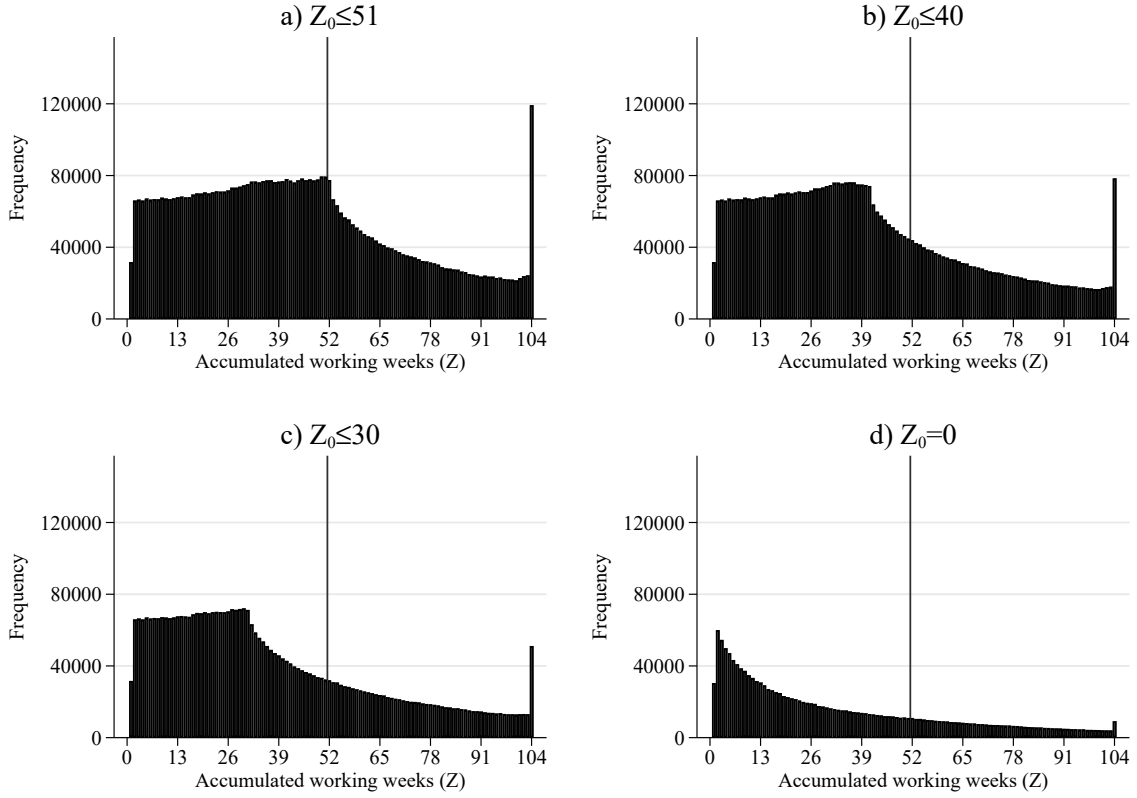
Note: These graphs report RDD plots for the dependent binary variable equal to 1 if there is a layoff in t , where Z is the forcing variable with a cutoff at 52 accumulated working weeks. We used local linear polynomial regression (left graph) or local quadratic polynomial regression (right graph) with triangular weights and bandwidth following the optimal mean squared error criterion in [Calonico et al. \(2014\)](#).

Table C.5: [Cattaneo et al.'s \(2019\)](#) density test

	Optimal bandwidth (± 14.8)	Smaller bandwidth (± 10)	Larger bandwidth (± 20)
	(1)	(2)	(3)
$Z_0 = 0$: p -value	0.515	0.505	0.330
$Z_0 \leq 20$: p -value	0.750	0.815	0.633
$Z_0 \leq 30$: p -value	0.957	0.496	0.088
$Z_0 \leq 51$: p -value	0.000	0.000	0.000

Notes: This table reports the density tests on the cutoff ($Z = 52$) of [Cattaneo et al. \(2019\)](#) for the weekly transitions. Each row shows the p -value when imposing a different maximum experience at hiring (Z_0): 0, 20, 30 or 51 (benchmark). Bandwidth: (1) ± 14.8 weeks (optimal bandwidth of the outcome model), (2) ± 10 weeks (smaller bandwidth), (3) ± 20 weeks (larger bandwidth). Results obtained using the Stata routine *rddensity.ado* using the default options: $q = 3$ ($p = 2$) polynomial for the bias (the estimation), triangular Kernel, density estimation without any restrictions (two-sample, unrestricted inference).

Figure C.6: Density of the treated observations over Z_t for spells with different maximum Z_0



Note: Density of the treated sample (weekly transition) depending on the maximum level of accumulated working weeks over C.1 measured at entry (Z_0).

Table C.6: Impact of UB eligibility on the layoff exit rate by changing Z_0 at entry

(A) One-week transition	$Z_0 = 0$	$Z_0 \leq 30$	$Z_0 \leq 40$	$Z_0 \leq 51$ (benchmark)
Coefficient (effect at cutoff)	0.0009*	0.0008***	0.0007***	0.0007***
Robust p -value	0.087	0.005	0.003	0.001
Robust lower bound 95% CI	-0.0001	0.0002	0.0003	0.0003
Robust upper bound 95% CI	0.0017	0.0013	0.0012	0.0012
Effect in %	34.76%	29.18%	21.46%	17.69%
Number of observations (Left)	187,618	593,319	902,851	1,163,205
Number of observations (Right)	140,832	408,487	546,692	797,356
(B) Two-week transition	$Z_0 = 0$	$Z_0 \leq 30$	$Z_0 \leq 40$	$Z_0 \leq 51$ (benchmark)
Coefficient (effect at cutoff)	0.0012	0.0015**	0.0013**	0.0011**
Robust p -value	0.502	0.023	0.038	0.031
Robust lower bound 95% CI	-0.0015	0.0002	0.0001	0.0001
Robust upper bound 95% CI	0.0030	0.0024	0.0023	0.0022
Effect in %	23.29%	27.32%	20.28%	14.77%
Number of observations (Left)	59,551	276,946	385,097	434,856
Number of observations (Right)	50,250	196,304	248,000	320,549

Notes: This table reports sharp RDD estimates using local linear polynomial regression for the one-week transition (Panel A) or two-week transition analysis (Panel B). The dependent binary variable is equal to 1 if the layoff is observed in that t . Z is the forcing variable with the cutoff at 52 accumulated working weeks. The sample varies according to the number of accumulated working weeks at entry (Z_0). We used local linear polynomial 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.*** Significant at the 1% level; ** significant at the 5% level; * significant at the 10% level.

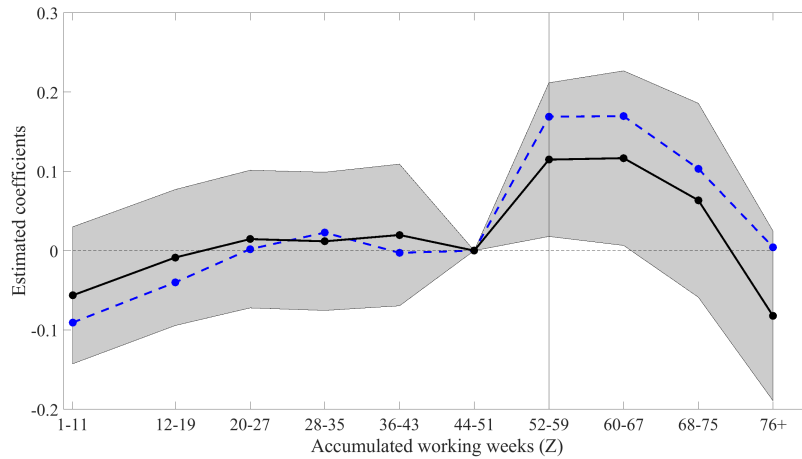
Table C.7: Impact of UB eligibility on layoff exit rate from sharp RDD by changing the maximum employment experience of the treated at hiring in period *C.2*

Maximum work experience at hiring in C.2 period (weeks)	(i)				(ii)			
	RDD in linear probability model				RDD in competing risks MPH model			
	Relative LATE				Relative LATE			
	LATE	(%) ^(a)	p-value	Observ.	LATE	Std.Err.	(%) ^(a)	# job spells
12	0.0011	17.18	0.1579	220,216	0.1390	0.1071	14.91	50,630
18	0.0008	10.99	0.2074	368,092	0.1014	0.0893	10.67	68,664
24	0.0012	17.33	0.0308	472,110	0.1904	0.0800	20.97	82,250
30	0.0016	22.47	0.0035	523,706	0.2397	0.0739	27.09	94,181
36	0.0014	19.68	0.0178	525,014	0.1676	0.0694	18.25	104,713
42	0.0010	13.50	0.0675	579,647	0.1197	0.0658	12.72	114,177
48	0.0011	14.68	0.0530	552,060	0.1202	0.0630	12.77	122,865
54	0.0011	15.78	0.0368	596,886	0.1452	0.0605	15.63	131,730
60	0.0010	13.38	0.0716	638,006	0.1427	0.0582	15.34	139,591
66	0.0009	12.64	0.0845	626,677	0.1269	0.0564	13.53	146,830
72	0.0012	16.07	0.0222	660,321	0.1455	0.0545	15.66	153,824
78	0.0012	15.82	0.0272	642,196	0.1626	0.0530	17.66	160,226
84	0.0012	15.59	0.0240	670,091	0.1609	0.0515	17.46	166,384
90	0.0011	14.21	0.0400	696,103	0.1532	0.0507	16.56	172,004
96	0.0011	15.05	0.0312	657,930	0.1564	0.0496	16.93	177,329
102	0.0011	14.84	0.0329	681,683	0.1623	0.0485	17.62	182,707
104	0.0011	14.77	0.0308	755,405	0.1599	0.0482	17.34	184,676

Notes: This table reports the robustness tests of the LATE for the two-week transitions by varying the maximum employment experience (in weeks) of the treated at hiring in period *C.2*. Results are for the sharp RDD estimates using (i) local linear polynomial regression in a linear probability model for the layoff probability or (ii) a competing risks MPH model for the layoff transition intensity. Only the treated are included in the sample. In the linear model, we use local linear polynomial 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 based on the MSE-optimal bandwidth selector. In the competing risks MPH model, we do not impose a bandwidth and we include in the specification of the transition intensities a cubic polynomial specification across *Z*, with different coefficients to the right and left of the cutoff.

^(a) This column reports the relative effect with respect to the layoff exit rate to the left of the cutoff.

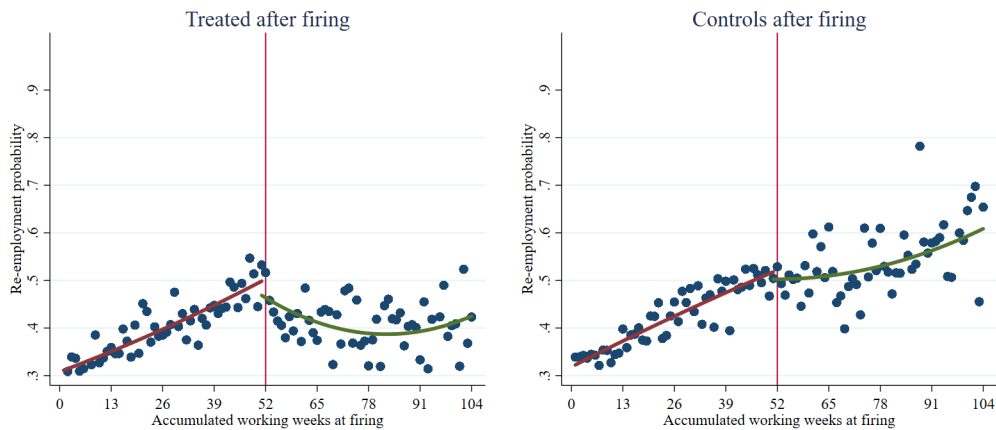
Figure C.7: Estimated ATTs on the layoff transition intensity: including treated with greater work experience



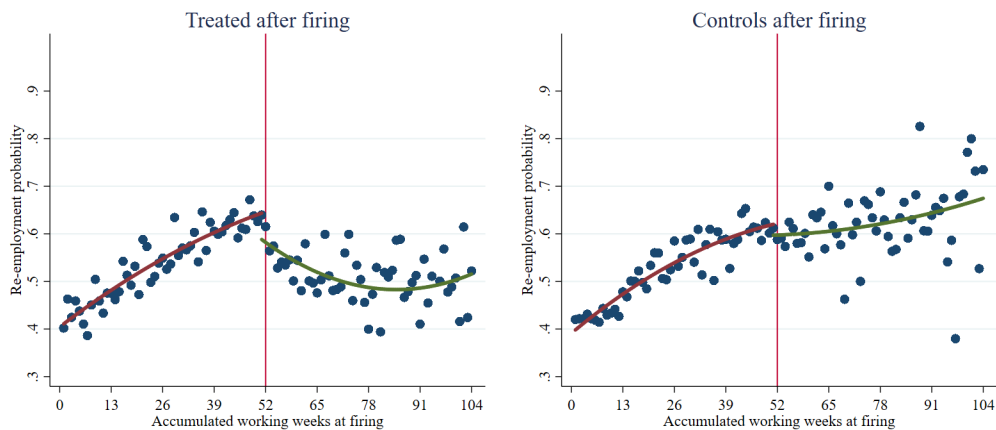
Notes: Full black line: point estimates benchmark model. Grey area: confidence interval at 95%. Blue dashed line: point estimates model keeping without constraints on the experience of the treated spells in the *C.2* period.

Figure C.8: Predicted re-employment probabilities in 6, 9 and 12 months since layoff across the accumulated working weeks (Z) at layoff

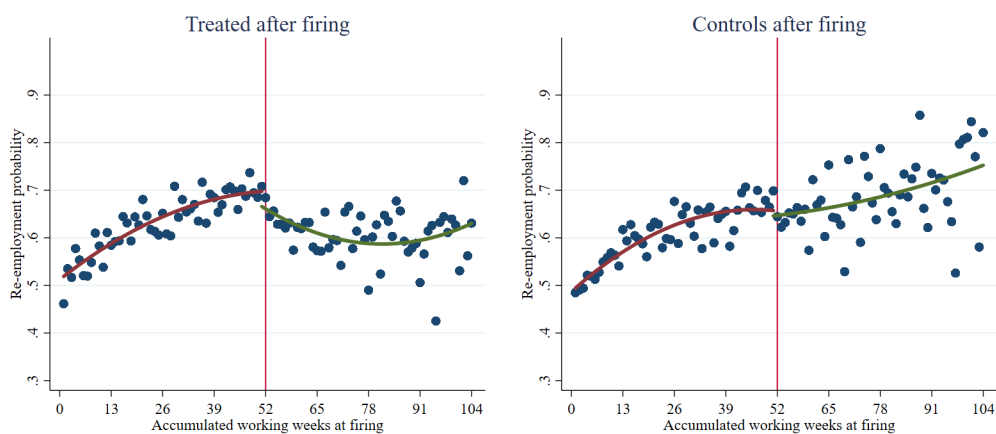
(a) In 6 months after layoff



(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 layoff 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 cutoff of 52 accumulated working weeks.

Table C.8: 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>.

Table C.9: Estimated ATTs on the transition intensity of layoffs for economic reasons

	Coeff.	Std.Err.
<i>After UB eligibility ($Z \geq 52$)</i>		
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 $H_0: \lambda_{1l,1} = \dots = \lambda_{1l,5} = 0$	p -value = 0.265	
Wald test $H_0: \lambda_{1l,7} = \dots = \lambda_{1l,10} = 0$	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.

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:

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

where $\tau \in \mathbb{N}$ and Θ 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:

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

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.⁵⁰ The probabilities associated with the mass points sum to one and, for $m = 1, 2, 3, 4$, are denoted by

$$p_m = \Pr(v_{0l} = v_{0lm}, v_{0o} = v_{0om}, v_{1l} = v_{1lm}, v_{1o} = v_{1om}) \equiv \Pr(\mathbf{v} = \mathbf{v}_m) \quad (\text{D.3})$$

and specified as logistic transforms:

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

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),

⁵⁰ 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:

$$L_i(t_i|\mathbf{x}_{it}, z_{it}, d_i; \Theta, \lambda_1, \lambda_2, \lambda_3, \mathbf{v}_1, \mathbf{v}_2, \mathbf{v}_3, \mathbf{v}_4) = \sum_{m=1}^4 p_m(\lambda_m) [c_i L_i^k(t|\mathbf{x}_{it}, z_{it}, d_i; \Theta, \mathbf{v}_m) + (1 - c_i) L_i^k(t|\mathbf{x}_{it}, z_{it}, d_i; \Theta, \mathbf{v}_m)] . \quad (\text{D.5})$$

The sample log-likelihood function which we maximised with respect to the parameters $(\Theta, \lambda_1, \lambda_2, \lambda_3, \mathbf{v}_1, \mathbf{v}_2, \mathbf{v}_3, \mathbf{v}_4)$ is given by the sum across the job spells of the natural logarithm of Equation (D.5).

After the model estimation, to quantify the ATT in terms of impact on job durations and number of jobs ending in a layoff, we simulated the model for the treated. We used the following algorithm only for the treated:

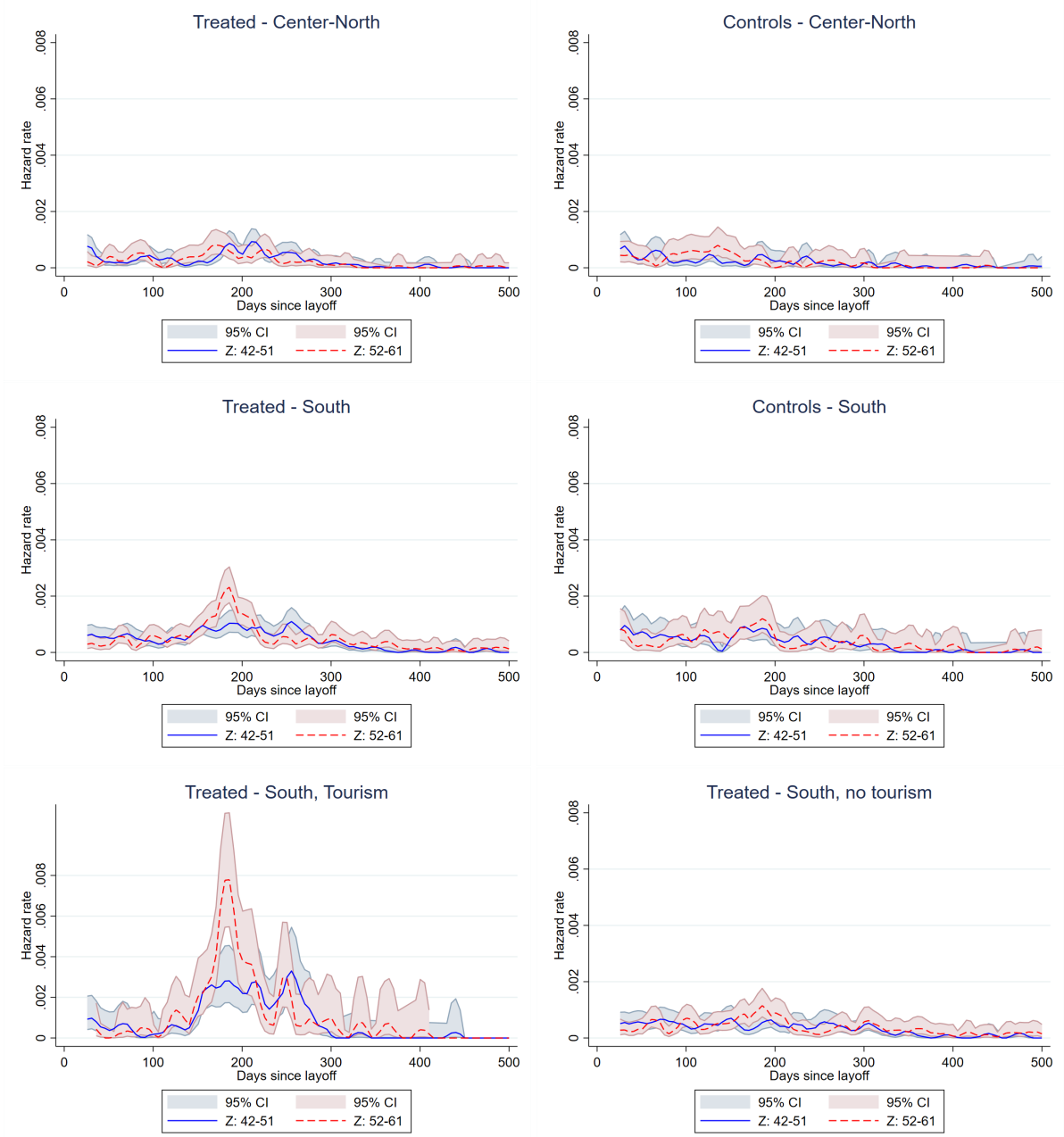
1. We draw a vector of parameter estimates assuming that the estimator is normally distributed around the point estimates with a variance-covariance matrix equal to the estimated one.
2. We assign to each job spell of the treated the observed regressors and unobserved characteristics drawn with the estimated probabilities as described in Equations (D.3) and (D.4).
3. We compute for each job spell and for each t the conditional probabilities of exiting the job for a layoff or for other reasons as Equation (D.2) divided by $S(t - 1|\mathbf{x}_{it-1}, z_{it-1}, d_i, \mathbf{v}_i)$, which is the probability of surviving up to $t - 1$.
4. We simulate the exit due to layoff or due to other reasons by using transition lotteries at each t on the basis of the conditional probabilities at point 3. In this process, the time-varying variables, among which are the value of the accumulated working weeks and the resulting indicators for the distance from UB eligibility, are updated at each t . At the time t at which job spells are right-censored, we stop the simulation to avoid extrapolations beyond the actual observation period. If a transition has not occurred yet, the job spell is predicted to also be right-censored in the simulation.
5. We compute the number of job spells ending due to layoff and their fraction over the total number of treated units. We also store the simulated durations and the simulated reasons of exit.
6. We replicate points 3–5 by pretending that the treated were controls.
7. We compute the absolute and relative variations in the number of layoffs and also the variations in 25th and 50th percentiles of the job durations when we move from the actual scenario (the treated are considered as treated) to the counterfactual scenario (the treated are considered as if they were controls).
8. We replicate points 1–7 999 times to obtain 999 independent realizations for each job

spell of the treated (both in the actual and in the counterfactual scenario) and get rid of randomness by averaging the statistics of interest across these replications.

E 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 that end with a layoff. We focus on individuals who at the moment of dismissal accumulated between 42 and 61 working weeks. This is meant to attenuate the compositional differences between people around the 52 weeks eligibility threshold. We then show the Kaplan-Meier hazard rate of finding a job in the same firm for these 48,636 spells by treatment status and region. To focus on the first transition, we right-censor non-employment spells exiting to other firms, but the estimates also considering later transitions are similar. As shown in Figure E.1, we do not observe large differences in the hazard rate above and below the 52-week threshold either for the control spells or for the treated in the Centre-North. Interestingly, there is a spike after six months for treated spells in the South. However, after removing the highly seasonal sector of tourism, this spike reabsorbs. This evidence suggests that UI may be used to cover periods of unemployment for seasonal jobs. Indeed, alternating six months of unemployment insurance to six months of employment allows the individuals to receive an income and maintain the C.1 requirements for UB eligibility (i.e. 1 year of work over the last 2 years). As after removing the seasonal sectors, we do not observe a significantly different response of the hazard of re-entering into the same firm between different regions; we do not have evidence in favour of the argument that unemployment insurance is linked to undeclared employment.

Figure E.1: Smoothed Kaplan-Meier hazard rate of re-entry into the same firm



Notes: Hazard rate of dismissed individuals re-entering into the same firm given the accumulated working weeks at the moment of layoff. Individuals finding a job in another firm are right-censored. Dashed line: job spells for which $52 \leq Z \leq 61$ at the moment of layoff. Units who satisfy this condition and are in the treated group are also eligible for unemployment benefits (left panels in the first two rows and both panels in the third row), as opposed to those in the control group (right panels in the first two rows). Full line: job spells for which $42 \leq Z \leq 51$ at the moment of layoff. These units are not eligible for unemployment benefits in all panels (i.e. neither in the treated nor in the control group).