Entitled to Leave: the Impact of Unemployment Insurance Eligibility on Employment Duration and Job Quality*

Laura Khoury†, Clément Brébion‡ and Simon Briole§

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Abstract

Entitlement conditions are a little explored dimension of unemployment insurance (UI) schemes. In this paper, we provide a comprehensive evaluation of a reform that softened the minimum employment record condition to qualify for UI benefits in France after 2009. Using administrative panel data matching employment and unemployment spells, we first provide clear evidence that the reform induced a separation response at the eligibility threshold. It appears both at the micro level – through a jump in transitions from employment to unemployment – and at the macro level – through the scheduling of shorter contracts, in line with the new eligibility requirements. Exploiting the reform as well as relevant sample restrictions, we then estimate the effects of receiving UI benefits on subsequent labour market outcomes using a regression discontinuity design. Our findings point to a large negative impact of UI benefits receipt on employment probability up to 21 months after meeting the eligibility criterion, which is not counterbalanced by an increase in job quality.

JEL Codes: J08, J65, J68, H31

Keywords: Unemployment, Employment duration, Behavioural response, Entitlement conditions, Job quality

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†Norwegian School of Economics. Email: laura.khoury@nhh.no

‡CEET-CNAM and Paris School of Economics. Email: clement.brebion@psemail.eu

§Paris School of Economics and JPAL Europe. Email: simon.briole@psemail.eu

Introduction

Unemployment insurance (UI) schemes are often compared across countries and over time in terms of level of benefits or coverage duration. A less explored dimension – also less quantifiable – is the ease of access to those schemes. UI schemes are characterised by rules determining eligibility to claim benefits, and obligations to keep on receiving them once an entitlement is opened. The existing literature mostly focuses on this second aspect and finds that monitoring and sanctions have a positive effect on the number of days employed (Fredriksson and Holmlund, 2001; Van den Berg et al., 2004; Lalive et al., 2005; McVicar, 2008). By contrast, eligibility conditions to receive UI benefits have attracted much less attention, although they are at the core of the insurance dimension of unemployment schemes. These conditions determine how much individuals should have contributed to the funding of the scheme through a minimum employment record to be entitled to claim benefits. As highlighted by the construction of an indicator of the strictness of eligibility criterion by the OECD, they vary a lot across countries (Venn, 2012).[1] As such, they have been heavily discussed in the policy debate over recent years.Yet, evidence on their effects is still scarce.

This paper precisely evaluates the impact of the minimum employment record requirement on transitions both in and out of employment. It takes advantage of a French reform that softened this requirement, moving the work history threshold from 6 months over the past 22 months to 4 months over the past 28 months in April, 2009. Exploiting administrative data linking a matched employer-employee dataset to UI data for a nationally-representative panel of individuals[2] between 2003 and 2012, we are able to follow individuals over their employment and unemployment spells and to precisely measure the transitions and characteristics associated to each spell.

Like in most OECD countries, eligibility conditions are defined in a binary way in France: workers experience a jump in the level of benefits when they reach the threshold,

[1] This indicator is made up of several items, including the minimum employment record as well as job-search and availability requirements, monitoring and sanctions. The item related to minimum employment record weights for 1/8 of the total index.

[2] The panel includes workers born in October of each year.

going from zero to the maximum amount they could be entitled to given the level of their past earnings.[3] This discontinuity in the outside option of workers may not only influence their labour supply decision but may also be internalised by employers, who may align work contracts duration with the minimum record employment conditions. Indeed, employers may rely on UI to provide a replacement income to workers between two contracts when activity slows down, all the more so if they know eligibility conditions are little restrictive.

In this paper, we first measure a separation response to the eligibility condition: the transition rate from employment to registered unemployment increases from almost 0 to 1% at the threshold. We interpret our findings as being driven both by more individuals separating from their job and claiming UI benefits when crossing the threshold, and by more individuals remaining unemployed conditional on having a job separation. Focusing on the subsample of individuals working full-time, for which we have a better precision in the measure of work history, we further show that the reform induced a jump in the biweekly transition rate from employment to non-employment from 7% to 10% at the threshold for these workers.

We then investigate whether the increase in separation rates induced by the reform translates into an effect on the structure of contract duration at the aggregate level. We show that the number of fixed-term 4-month contracts relatively to fixed-term 6-month contracts increased by about 30% (i.e. about 1.5 contract per month within firm) after the reform. This marked increase is more specifically concentrated in sectors previously identified as frequently hiring on very short-term contracts and with a high separation rate.[4] These trends suggest that the 4-month contract tend to become a new norm after the reform. As a consequence, our results point to an impact of UI not only on job-seekers' pre and post-unemployment outcomes, but also on outcomes of workers who do not experience unemployment. These effects are consistent with Müller et al. (2018) who show, in the German context, that such norms can act as a constraint on the free

[3] Put differently, the level of benefits does not increase with work history once the threshold is reached.
[4] Decree No. 2019-797 of 26 July 2019 on the unemployment insurance scheme, DARES (2018).

adjustment of labour supply for people who would like to work a different number of hours than what is offered by firms. Similarly, a new norm on the duration of short contracts could restrain the labour supply of workers in some sectors, including those not seeking UI eligibility.

Finally, we investigate whether receiving UI benefits *because* of the reform has an impact on individuals' subsequent labour market outcomes. The 2009 reform changed the composition of the pool of benefits claimants, giving the possibility to receive benefits from the 4^{th} month of employment, and extending the base reference period. This could have an impact on the labour market outcomes of these workers who were not eligible to UI benefits and had no substitution income in case of no employment before the reform, and who were able to open a UI entitlement after the reform. In the absence of any insurance, individuals are expected to take a job quickly, which doesn't necessarily match with their skills or which is of low quality. We therefore ask whether being covered by UI has an impact on the probability to find a job, and on the characteristics associated to this job.

Using a regression discontinuity design, we show that claiming benefits is associated with a reduction in the probability to find a job at different time horizons, without any clear positive impact on the subsequent job quality. The negative effect on employment is still observed more than one year and a half after the end of the contract that made workers reach the eligibility threshold. We also document a negative impact on daily wages, more pronounced in the mid to long-term. Because of the employment response, it is difficult to disentangle the negative impact on earnings due to a reduction in the probability of employment from an increase in the probability to have lower paid jobs, conditional on being employed. However, complementary results suggest that there is no significant increase in several dimensions of job quality that may counterbalance this negative impact on employment.

These results bring two main contributions to the literature. First, although the impact of UI parameters on the behaviour of the unemployed has been extensively studied in the literature (see Schmieder and Von Wachter (2016) for a review),[5] the potential impact of UI on behaviours of employed workers and of employers is usually not incorporated in the optimal UI framework. While some theoretical mechanisms have already been highlighted (Feldstein, 1976; Baily, 1977; Ortega and Rioux, 2010; Zhang and Faig, 2012; Pan and Zhang, 2012; Andersen et al., 2015; Hopenhayn and Nicolini, 2009), the empirical literature on this topic remains scarce, quite old and usually based on survey data.[6] In particular, this aspect has never been studied in France, whereas it is likely to be highly influenced by the institutional context.[7] The first contribution of this paper is therefore to shed light on a little studied question, looking at how UI can affect transitions out of employment and influence separation decisions in the country. We also go further by analysing the consequences of this separation response at the aggregate level, providing suggestive evidence that the overall duration of work contracts responds to UI incentives.

Our second contribution speaks to the much larger literature on UI benefits and unemployment duration. It is now a well-known empirical fact that the duration of unemployment is positively affected by the level of UI benefits (Chetty, 2008; Landais, 2015; Lalive et al., 2006). However, the extensive margin effect of UI benefits on unemployment duration - i.e. the effect of having some benefits as opposed to no benefit at all - has been much less documented. One of the reason for this lack of evidence is that papers looking at this margin have essentially focused on the response in terms of exit from employment (Martins, 2016; Rebollo-Sanz, 2012; Albanese et al., 2019). It is this response that precisely makes the analysis of the consequences in terms of future employment prospects difficult. In particular, quasi-experimental methods become hardly usable as

[5]Many papers have highlighted the impact of UI generosity and potential benefit duration on unemployment duration (Lalive et al., 2006; Lalive, 2007; Landais, 2015) or reservation wages (Feldstein and Poterba, 1984; Krueger and Mueller, 2016; Le Barbanchon et al., 2017) although the last two papers cannot reject the null hypothesis of a zero effect.

[6]See Baker and Rea Jr (1998); Green and Riddell (1997); Christofides and McKenna (1995) for example, with the exception of Martins (2016); Rebollo-Sanz (2012); Albanese et al. (2019); Van Doornik et al. (2018); Jäger et al. (2019); Baguelin and Remillon (2014). The first four papers focus on the impact of the UI eligibility criterion on flows to unemployment, and show that they are strategically timed to coincide with UI eligibility. The last two papers focus on the population of older-workers. They document that the last exit from employment is scheduled according to UI as a bridge to early retirement, and that it responds to changes in the potential benefit duration.

[7]For instance, the effect is likely to differ according to the presence or not of experience rating.

the endogenous employment response entails sorting at the eligibility threshold. A notable exception is Davezies and Le Barbanchon (2017), who analyse the consequences of receiving UI benefits in terms of match quality in France, as an application of the method they develop to correct for measurement error. We add to this previous analysis in two ways: (i) we are able to get rid of the endogenous employment response issue by taking advantage of the 2009 reform and making useful sample restrictions; (ii) we look at a wider range of outcomes to draw a more complete picture of the effect of receiving UI benefits. In particular, our work relates to the empirical debate on the effect of UI benefits on job quality.[8] Looking only at the probability of employment could be misleading, as UI benefits could affect other dimensions of labour market outcomes. Being able to measure the impact on job quality is therefore crucial to better assess the welfare impact of UI benefits. Finally, another advantage of our setup it that we can combine the analysis of UI benefits eligibility criterion on both the transitions in and out of employment. It allows a comprehensive evaluation of the 2009 reform. One drawback of this exhaustiveness is that, although we find meaningful results on many dimensions, we are under-powered on some aspects related to job quality. Further analysis on this topic is left for future research.

The remainder of the paper proceeds as follows: Section I and II describe the institutional background and the data. The following two sections provide evidence of a separation response at the micro level (Section III), translating into a contract duration response at the aggregate level (Section IV). Section V presents the methodology and the results of the regression discontinuity design, and Section VI concludes.

[8]Whereas the standard job search model predicts a positive impact of UI benefits on future job quality, through higher reservation wages, the effect becomes ambiguous when negative duration dependence is accounted for (Nekoei and Weber, 2017) Empirically, Nekoei and Weber (2017) find a positive effect, whereas Schmieder et al. (2016) find a negative effect and Card et al. (2007); Lalive (2007); Van Ours and Vodopivec (2008) a non-significant one. Focusing on the reservation wage channel in France, Le Barbanchon et al. (2017) find a precisely estimated zero elasticity of reservation wages with respect to the level of UI benefits.

1 Institutional background

As in many developed countries, UI is made of two components in France: an insurance part and a solidarity part. What characterises the insurance part is a strong contributory link. It means that what is paid to the claimants is tightly linked to their contribution to the scheme. This general principle translates into different rules: (i) the amount of benefits and social security contributions are proportional to past earnings; (ii) the potential benefit duration (PBD) is proportional to work history; (iii) the main eligibility criterion also depends on a minimum employment record.

This paper focuses on the third rule in order to study the extensive margin impact of UI benefits on different labour market outcomes. We take advantage of a reform implemented in April 2009[9] that changed the minimum work history condition from 6 months over the last 22 months to 4 months over the last 28 months. The pre-reform period was characterised by different paths that linked a work history duration to a PBD. For example, the pre-reform minimum working condition was requiring individuals to have worked at least 6 months over the last 22 months and was giving them right to 7 months of potential benefit duration. A simpler rule was introduced by the reform, leading to a one-to-one relationship between the number of days worked over the last 28 months and the PBD. After the reform, the minimum work history to be eligible is equal to 4 months, and the PBD cannot exceed 2 years even when work history is longer.[10] Our main empirical strategy exploits both the existence of a work history threshold in the post-reform period and the evolution of this threshold over time due to the reform.

The other main features of the UI system have not been affected by the reform. To receive benefits, unemployed workers have to fulfill the following requirements: (i) be younger than the compulsory retirement age; (ii) live on the territory where the unemployment insurance is applicable; (iii) be physically able to work; (iv) the job loss must be involuntary;[11] (v) be actively looking for a job and be available to work. Once

[9] cf. *Arrêté du 30 mars 2009 portant agrément de la convention du 19 février 2009 relative à l'indemnisation du chômage et de son règlement général annexé* (Decree of March, 30^{th}, 2009).

[10] For workers of 50 years old or more, the maximum PBD is equal to 3 years.

[11] Some exceptional cases of resignation open entitlements to UI, but they are very marginal.

eligible, workers are entitled to claim benefits equal to a proportion of their past earnings. The replacement rate is a decreasing function of the level of previous wage and can vary from 57.4% to 75%. The level of benefits stays stable along the unemployment spell. Once unemployed workers run out of benefits or if they are not eligible to UI benefits, they may receive solidarity benefits or the minimum income.

2 Data and Descriptive Statistics

Our main data source is administrative data that links a matched employer-employee dataset (called DADS hereafter) to UI data (FH). The matched dataset is referred to as the FH-DADS. The first dataset comes from employer records filled by firms each year on each of their employees and that are used to compute social security contributions. It contains detailed information on earnings, number of days worked, type of job, firm size, industry, occupation. Most importantly, it includes identifiers for both the individual and the employing firm. The second dataset comes from the French Unemployment Agency (*Pôle Emploi*) and gathers information on individuals' level and duration of benefits for each unemployment spell and on their work history. It also provides some details on their last work contract (firm size, industry, type of contract, separation motive, tenure, etc.). These two datasets have been matched together for a subsample of the French population ($1/12^{th}$) from 2003 to 2012, resulting in an individual panel which allows us to track individual career path and transitions from employment to unemployment.

Two main caveats of this dataset should be mentioned. First, the DADS does not contain information on the exit motive, preventing us from distinguishing layoffs from resignations. This information is present in the UI data, but it is then not available for those separating and not registering to UI.[12] Second, the unit of observation in the DADS is defined at the firm × individual × year level. It means that if an individual has several contracts within the same firm the same year, they will be gathered into the same observation. In the remainder of the paper, we will call this unit of observation a *position*.

[12] As previously mentioned, most individuals who resign from a working contract are not eligible to UI benefits and therefore do not register to UI.

The starting date and ending dates will correspond to the first entry in the firm and the last exit from the firm within a year, meaning that the individual is not necessarily continuously employed in the firm in between. This could lead to an overestimation of the contract duration, and in particular of the number of one-year contracts, as *positions* starting and ending dates are often recorded as January, 1^{st} and December, 31^{st} even when the actual contracts did not last 1 year.[13] As a result, we may potentially end up with measurement error when computing work history, in particular for workers often working under short-term contracts and being recalled by former employers. For the subsample for which we have the information in both datasets (for workers registering as unemployed), we can cross-check the variable we compute using the DADS with the one from the UI agency. We get approximately a 75% match rate if we take a rather slack definition of a match.[14]

The first part of the analysis investigates how employers and employees react to the change in the unemployment value at the eligibility cutoff by looking at the transitions out of employment. We first remove from our sample people who experienced particular forms of employment and are subject to different rules in terms of UI, such as home employees for private employers or public sector workers. Our broad sample includes 2,690,114 individuals accounting for 18,114,742 *positions* ending between 2004 and 2012 and 3,071,283 corresponding unemployment spells. Depending on the analysis we perform, we further restrict the sample, as detailed in each section.

We exploit a second database to analyse the aggregate response to the change in eligibility criterion in terms of contract duration (Section 4). This database is called the MMO[15] and is provided by the French Ministry of Labour. This is a repeated cross-section, but the measure of the aggregate response does not require a panel structure.

[13]It may be the case because it corresponds to the first day of the first contract and the last day of the last one within the same firm or because it has not been properly filled by the employer as it does not have any consequences for the payment of contributions.

[14]More precisely, we define the two values as matching when they are equal ± 30 days, as there is also a difference in way of counting days in the two bases that could lead to a small mismatch without implying any measurement error.

[15]The MMO comes from a monthly return filled by any employer from the private and public sector, including the self-employed, the unions and the associations if they employ 50 workers or more. Firms of less than 50 employees are surveyed. It informs on all hiring and separation flows.

The MMO has the advantage to capture every job transition more accurately than the DADS, as it provides information on ending and starting dates and on the separation motive at the contract level. However, job-to-job transitions with the same employer cannot be measured in the dataset: for instance, a fixed-term contract extended with a new fixed-term contract or converted into a permanent one without any interruption will appear in a single employment spell. Assuming that the conversion and renewal behaviours stay unchanged before and after the reform, this limitation should not affect our results. Gathering all contracts ending between 2005 and 2015, and disaggregating by type of separation, we end up with the sample described in Table A1.

The different analyses developed in this paper, and especially the second part, focus on a particular population of workers with short work history. Using the DADS, Tables A2 to A4 show how their individual and job characteristics compare to workers on longer-term contracts. Their short work history can be explained both by their shorter experience and by their weaker attachment to the labour market, as indicated by a lower hourly wage and by a lower chance to have a permanent contract or to work full-time. They are also more frequently male, work in smaller plants, and have a higher probability of holding multiple jobs at the same time.[16] This last characteristic is consistent with their higher share of temporary and part-time contracts. Furthermore, workers with short work history are typically found in sectors such as the agriculture, food and accommodation, administrative services, arts and entertainment, and much less in the manufacturing or the construction sectors. This pattern coincides with the one underlined by studies describing which sectors are frequently hiring workers under very short-term contracts (DARES, 2019). In terms of types of occupations, workers with short work history are more likely to be in a low-ranked occupation.

[16] When a person holds multiple jobs at the same time and looses one of them, she is entitled to receive UI benefits as long as she fulfils the eligibility conditions. The benefits will be computed based on this lost activity, and potentially previous ones that have been lost within the last twelve months. The person can receive her benefits in addition to earnings from the other jobs she kept. In the event that she loses one of her jobs again, the UI entitlement will be revised to take into account this new job loss.

3 Empirical evidence of a separation response

The eligibility criterion to UI benefits may influence the labour supply decision of workers as it introduces a sharp discontinuity into the value of unemployment. Indeed, when crossing the work history threshold, workers experience an increase in their outside option value, as they will be entitled to receive a replacement income in case they stop working.[17] It may also affect labour demand if employers rely on the UI to provide workers a replacement income in low phases of the cycle. The behavioural response we want to measure is twofold: (i) we are first interested in knowing whether workers and/or employers deliberately choose to schedule the end of contracts so that it coincides with workers' eligibility; (ii) but we also study whether, conditional on the contract ending, workers choose to stay unemployed once eligible while, absent the UI, they would take another job.

The first step of our analysis consists in studying the distribution of the probability to transit from employment to unemployment with respect to previous work history. Figure 1 shows that, conditional on ending a contract, the probability to register for UI jumps at the eligibility threshold. A spike can also be seen at 6 months that may be due to regularity in contract duration, as 6 months is a reference point. However, we do not observe any break in the trend, whereas we do observe a discontinuity during the pre-reform period, when the eligibility criterion was at 6 months within the last 22 months (Figure 2). On the reverse, we do not observe any discontinuity in the probability to transit from employment to unemployment at 4 months within the last 28 months during the pre-reform period, or at 6 months within the last 22 months during the post-reform period (Figures A1 and A2). It indicates that discontinuities observed at eligibility thresholds in Figures 1 and 2 cannot be explained by those thresholds being reference points for reasons outside UI eligibility.

To complete the analysis, we also investigate whether the probability to end a contract is affected by the eligibility criterion. To do so, we turn to the measure of the biweekly

[17]Provided that they meet the other eligibility criteria, and especially the one specifying that the job loss must be involuntary.

transition from employment to registered unemployment (as defined as being registered to UI) and from employment to a broader definition of non-employment that includes not working and not being registered for UI. We convert our data defined at the *position* level into a dataset where each observation represents two weeks of each worker's career path, with information on the type and date of transition, and the work history at the end of the two weeks. For our main period of interest, the starting point is defined at the worker level as the first contract starting after the reform. We define four types of transitions which may happen at a biweekly rate: (1) from employment to employment; (2) from employment to registered unemployment; (2') from employment to non-employment; (3) from non-employment to employment.[18] We then examine whether the transition rate jumps at particular values, on the sample of 864,534 individuals that we observe from their first contract after the reform up to two years after. Figure 3 shows a clear jump in the probability of transiting from employment to unemployment at the eligibility threshold. In line with this result, Table 1 shows that having a work history right above the eligibility threshold is associated to a 0.8 percentage point increase in the probability to transit from employment to registered unemployment, from an almost 0 probability at the left-hand side of the cutoff.[19] However, we do not observe such a pattern if we look at the transitions from employment to non-employment (Figure 4).

Taken together with Figures 1 and 2 and Figures A1 and A2, these findings are compatible with two interpretations. First, a separation response of both (or either) workers and employers could drive the results. However, at that stage, we cannot exclude that our findings are fully mechanical. Non-employed individuals with an affiliation lying between 4 and 6 months could simply take advantage of their new right without any other change in the behaviour of economic agents. In other words, substitution from non-registered

[18]Note that (2) is included in (2').

[19]The low transition rate in absolute terms is explained by the fact that we only analyse the opening of new UI rights. We do so because, if a unemployed individual finds a job before the end of her entitlement and then returns to unemployment, she can either resume her former right, open a new right based on her very last employment spell, or take a mix of both rights. This means that she doesn't need to fulfil the minimum employment history condition again to be able to receive UI benefits as long as the former right is not exhausted. That is the reason why we focus on the opening of new UI rights to better capture the effect of the eligibility criterion. We then only measure a subsample of all transitions to registered UI.

non-employment to registered unemployment could rationalise our findings.

We still have several reasons to think that the jump in the transition rate from employment to registered unemployment partly arises from the separation response due to UI eligibility. First, the literature suggests that a response to UI incentives in terms of transition from employment to non-employment is a likely phenomenon. Thus, evidence from other countries (Rebollo-Sanz (2012) in Spain and Albanese et al. (2019) in Italy) point to a significant separation response at the eligibility thresholds in comparable institutional settings.[20] In the French context, Khoury (2019) highlights strategic separation scheduling in the case of economic layoffs to qualify workers for higher UI benefits.

Second, several elements could blur our previous result on the transition from employment to non-employment. Transitions to UI are a small share of total transitions to non-employment. As a result, the effect may be attenuated when we pool together all transitions out of employment. Further, the measurement error in the computation of work history mentioned in section 2 might affect the precision of the results and hide the discontinuity. To overcome this issue, we focus on workers whose number of hours worked during the *position* corresponds to the number of hours worked for a person employed full-time all the days covered by the *position*. This restriction alleviates measurement issues by ensuring that the *position* corresponds to one single contract.[21] Figure 5 shows that there is a clear discontinuity in the transition rate from employment to non-employment at the eligibility threshold, confirming that part of the difference in pattern between transition rate to registered unemployment and non-employment may be due to precision issues. This is confirmed by Table 2 which measures, in a regression discontinuity design spirit, the discontinuity in the transition rate at the threshold on this specific sample. According to this regression, the transition rate increases by about three percentage points at the cutoff, which represents a 43% increase relative to the rate right below the cutoff.

[20]Both Rebollo-Sanz (2012) and Albanese et al. (2019) can make the distinction between quits and layoffs. They show that there is a jump in the hazard rate out of employment when looking at layoffs, but not when looking at quits.

[21]One drawback of this restriction is that we focus on a population of individuals working full-time, which may not be the population reacting the most to incentives to exit employment.

Overall, Tables 1 and 2 and Figures 1, 3 and 5 are therefore hinting at a response from firms and workers to the increase in the value of unemployment through separation at the eligibility threshold. As most of the resignations do not allow workers to claim UI benefits, it implies that employers do contribute to this separation response.

4 Impact of UI eligibility criterion on contract duration

The analysis developed in Section 3 indicates that the change in the value of unemployment at the eligibility threshold leads to a higher probability to transit to unemployment, especially registered unemployment, at the threshold. To be eligible, workers need to prove that job loss was involuntary. It means that they are entitled to receive benefits only after having been dismissed or laid-off, or having mutually agreed on contract termination with their employer. Only few cases of resignation are considered legitimate and open entitlements to UI.[22] This rule implies that employers are instrumental in this strategic job separation, by accepting to dismiss the worker, by mutually agreeing on contract termination, or, more likely, by designing shorter work contracts.[23]

Indeed, the relaxation of the UI eligibility criterion introduced by the 2009 reform may have influenced the duration of fixed-term contracts offered by employers, as they may internalise the jump in the value of unemployment at the new eligibility threshold at the moment of the hiring. The reform thus allows them to have more flexibility and to commit on a shorter period, as they are able to offer 4-month contracts while guaranteeing to the worker the maintenance of their income through UI benefits between two short contracts.

The 2009 reform is concomitant with a sharp increase in the share of short-term contracts in total hiring, and in the share of employees rehired by the same employer, as depicted in Figure A3 in the appendix. Those two trends are compatible with employment

[22] To follow a spouse who has been transferred is one of them.

[23] Most of the entries to UI are through the end of fixed-term or temporary contracts (about 24% between May 2016 and May 2017 for example, according to UI figures).

relationships where employers would offer a contract just long enough so that workers would be eligible to UI benefits at the end of the contract, then rely on UI to compensate workers, and then potentially rehire them. This type of employment relationship has the triple advantage to: (i) allow a more flexible adjustment of the workforce to the variation of the firm activity with shorter commitment periods,[24] as underlined in Christofides and McKenna (1995); (ii) help employees accept shorter contracts as it is counterbalanced by UI benefits and recall behaviour; (iii) allow employers to retain workers and to have them investing in firm-specific human capital without bearing the cost of high employment protection. Indeed, the share of recalled workers has increased along with the share of short-term contracts in recent years (Benghalem, 2016; Journeau, 2019).

We therefore ask whether the change in eligibility criterion introduced by the 2009 reform contributed to the decrease in the duration of contracts offered by employers. To do so, we analyse the evolution of contracts duration at the aggregate level, exploiting administrative records that employers have to fill each quarter on inflows and outflows from the firm. These records are gathered in the MMO (*Mouvements de Main d'Oeuvre*) database, which provides information on the date and type of flows and on some characteristics of the worker and the firm.

We first focus on fixed-term contracts and we perform a difference-in-difference estimation, comparing the evolution of the number of 4-month and 6-month contracts, before and after the reform. To do so, we restrict our sample to 4-month and 6-month fixed-term contracts and we aggregate all observations at the firm x month x contract duration level.[25] We then estimate the following equation:

$$Y_{imt}^{d} = \alpha + \beta_1.post_{mt} + \beta_2.\mathbb{1}_{d=4} + \beta_3.post_{mt} * \mathbb{1}_{d=4} + \mu_i + \kappa_m + \delta_t + \epsilon_{imt} \quad (1)$$

where Y_{imt}^{d} is equal to the number of contracts of duration $d \in \{4;6\}$ ending in firm i, on month m of year t. $post_{mt}$ is a dummy variable that equals 0 before the reform was implemented in April 2009 and 1 for all subsequent periods, and $\mathbb{1}_{d=4}$ is a dummy variable

[24] The commitment period here refers to the duration of the fixed-term contract. Although it is possible to break a fixed-term contract before its end, it entails an economic and administrative cost.

[25] Contract duration is rounded to the nearest month.

indicating 4-month contracts. The parameter of interest, β_3, captures the evolution of the average number of 4-month contracts ending every month in each firm before and after the reform, relative to the evolution of 6-month contracts. We complete the model with the full set of firm, month and year fixed effects.[26] Results are displayed in Table 3. The fourth specification, which includes firm, sector, year and month fixed-effects, indicates that the reform of the UI eligibility criterion have induced 1.53 additional 4-month contracts ending each month in a given firm as compared to 6-month contracts. It represents a 17.4% increase. When restricting to the sample of firms present in both periods and having at least one 4-month and one 6-month contract ending during each period, the increase is equivalent to 17.9% (see column 5).

This evidence is further confirmed by the estimation of a model in which we interact the dummy variable for 4-month contracts in equation (1) with yearly dummies rather than a pre-post variable. The resulting estimates are displayed in Figure 6. It depicts a clear jump after the reform, with the number of 4-months contracts (relative to 6-months contracts) remaining at higher levels on all subsequent years, whereas no significant difference is observed before the reform. The year after the reform, there are on average two additional 4-months contracts ending every month in each firm as compared to the year before, relative to the same evolution for 6-months contracts. Compared to a 4.8 average number of 4-months contracts ending each month in each firm in 2008, this represents a 45% increase.[27] Results outlined in Table 3 and Figure 6 indicate that employers substitute 6-month contracts with 4-month contracts when making their hiring decisions as a response to the reform, suggesting that the 4-month contracts tends to become a new norm among short-term contracts after the reform.

We push the analysis one step further by looking at the within-sector decomposition of the change. Figure 7 shows the difference-in-difference estimate using the fourth specifi-

[26]We also include sector dummies to account for the very few cases of firms switching across sectors over the period.

[27]However, we should keep in mind that the difference-in-difference coefficient measures the relative increase of 4-month contracts, which may not be equivalent to the change in the number of 4-month contracts in absolute terms.

cation of Table 3 within each of the thirty-five sectors defined using a 2-digit classification. We observe that about a third of them experience a significant increase in the relative number of 4-month contracts after the reform. Those sectors are mainly sectors identified as having a high separation rate,[28] or having a high-share of very short contracts in total hiring (DARES, 2018). Out of the thirteen sectors identified by the administration[29] as having a high separation rate or a high share of very short contracts, ten experience a positive change in the number of 4-month contracts relative to 6-month contracts after the reform. This picture suggests that employers in sectors that are used to repeatedly hiring under very short contracts with a high turnover may be more aware of UI eligibility rules and more willing to minimise contract duration and maximise flexibility.[30] They may then particularly react to a change in the eligibility criterion.

This evidence is further confirmed by a firm-level analysis, where we compute the change in the number of 4-month contracts relative to 6-month contracts using the same difference-in-difference approach within each firm. We keep only firms that are observed in both periods and with at least one contract ending at four months and one ending at six months before and after the reform. We then examine the distribution of sectors among firms with a high difference-in-difference coefficient, defined as a coefficient above the 90^{th} percentile of the coefficient distribution. Table A5 compares the distribution of sectors among these firms and all the other firms, while Figure A4 reports the difference in the share of firms belonging to each sector between these two groups when this difference is positive (i.e., for sectors that are over-represented among high-coefficient firms). Sectors identified as having either a high separation rate or a high share of very short contracts are again more likely to include firms with a high increase in 4-month contracts. This is illustrated by the fact that most of the points on Figure A4 are red, i.e. most of the over-represented sectors among firms with a high relative increase are sectors with a high

[28]The 2019 unemployment insurance reform in France introduced a contribution scheme similar to experience rating, targeting seven sectors whose high turnover was making UI bear a substantial cost. The criterion to identify those sectors is to have a separation rate higher than 150% (Decree No. 2019-797 of 26 July 2019 on the unemployment insurance scheme).

[29]We pool together sectors specified in DARES (2018) and in the Decree No. 2019-797 of 26 July 2019 on the unemployment insurance scheme.

[30]In particular, temporary employment agencies are included in these sectors characterised by a strong positive response in the number of 4-month contracts.

turnover, and that most of the red points are located where the difference is the highest. It provides further evidence that some sectors are particularly reacting to the change in the UI eligibility criterion.

Although we expect the response to be particularly high for fixed-term contracts, as they represent the majority of the flows and the easiest way to adjust the workforce to economic fluctuations, we also examine other types of contract separation. Overall, we do not see any positive response in terms of economic layoffs or dismissals on personal grounds (Figures A5 and A6 and Tables A6 and A7). However, if we focus on the sectors where the separation rate and the share of very-short contracts are high, we also observe a positive response, and especially for economic layoffs (Figures A7 and A8). Reassuringly, we do not observe any similar pattern for voluntary resignation, as this motive does not open entitlements to UI, and should not be influenced by UI eligibility rules (Figures A9 and A10). The evolution for other separation motives can be seen in Tables A8 to A11. In particular, we observe a small positive response when looking at pre-retirement separations. This is in line with previous findings (Baguelin and Remillon, 2014) highlighting a strategic scheduling of retirement and pre-retirement dates in order for workers to be covered by UI first and then by pension schemes without any interruption in payment. Those motives may then be particularly sensitive to a change in UI eligibility rules.

In Table A12 and Figure A11, we corroborate previous findings using UI data providing information on the set of the last contracts used to open UI entitlements (that are the contracts just preceding the UI spell). While the dataset does not represent the universe of contracts, it has the advantage of being a panel and including work history information. As it is UI data, one may be concerned about a potential increase in the share of 4-month contracts being driven by the fact that 4-month contracts are sufficient to open a UI entitlement after the reform whereas it was not the case before. This would mechanically lead to less 4-month contracts being recorded before the reform. That is why we exclude workers registering with a work history between 4 and 6 months, as they were invisible before the reform.[31] Figure A11 depicts the yearly evolution of the share of each type of contracts among contracts of 1 year or less, according to their ending date.[32] The vertical line separates the pre and post-reform periods. While the trends are parallel during the pre-reform period, we observe that after the relaxation of the eligibility criterion, the share of 4-month contract has dramatically increased whereas the share of 6-month contracts has slightly decreased.[33] The response in terms of number of 4-month contracts already measured in the MMO data seems to be exacerbated in the UI data. It may be explained by the fact that (i) UI data includes information only on the last employment spell used to open a UI entitlement, which may be particularly subject to optimisation, and that (ii) sectors with a high turnover, where the response as measured by the MMO is the highest, may be over-represented in UI data as they send a high share of workers to UI.[34]

The fact that we observe this pattern even if we excluded workers with a work history between 4 and 6 months suggests that the 4-month contract has become a new norm after the reform, at least in some sectors familiar with short-term employment, no matter if the worker is already eligible to UI or not. This means that UI design not only affects employed workers who will experience unemployment, but it may also affect workers who did not and will not experience unemployment, through a change in contract duration practices.

5 Extensive margin effect of UI benefits

Although the effect of higher UI benefits on employment outcomes has been largely documented, much less is known on the effect of UI at the extensive margin. Several reasons can be put forward: (i) such an analysis requires to have data on employment

[31] Including them in the sample indeed yields higher estimates of the increase and decrease in 4 and 6-month contracts shares, respectively of +2ppts and -1.5ppts.

[32] The ending date of the contract is used to determine under which UI rules the worker falls.

[33] We also represent the share of 1-month and 10-month contracts as a way to control for the trend, which may be particularly crucial at a time of economic crisis.

[34] In addition to this, when we decompose the change by separation motives, it provides us with a similar picture as when using the MMO: the response is the highest for fixed-term contracts. These graphs are available upon request.

and unemployment spells of all workers, not only those registering to UI, to be able to compare eligible and non eligible workers; (ii) the effect on transitions out of employment highlighted in the Sections 3 and 4 can hinder the analysis of the effect of UI receipt on future employment outcomes. Indeed, if the eligibility to UI benefits has an impact on the probability to transit from employment to unemployment, it becomes difficult to compare eligible to non eligible workers. For example, comparing workers at both sides of the eligibility threshold in a regression discontinuity design spirit may lead to biased results if there is sorting into unemployment on one side.

We work around this issue by using a reform in the eligibility criterion to UI benefits that came into effect on April, 1^{st}, 2009. The work history requirement has been relaxed from 6 months over the last 22 months to 4 months over the last 28 months. We can therefore compare people with work history between 4 and 6 months before and after the reform, as, under some conditions, they would not react in terms of employment exit in the close neighbourhood of the reform. This assumption is plausible if we make the following restrictions: we select workers reaching 4 to 6 months of cumulative work history at the end of the their contract (i) only if they have started their last contract *before* the reform has passed and (ii) only if their last contract was a *fixed-term* contract. Indeed, if there is a separation response after the reform has passed, it may be much more limited for fixed-term contracts that have started before the reform, as it is quite costly to separate ahead of the expected end of the contract. The separation response is likely to go mainly through dismissals of workers under permanent contracts or design of new shorter fixed-term contracts. Both channels are muted after the sample restrictions.

To be fully convinced that such restrictions of our sample ensures that no separation response biases the analysis, we can check whether there was not a disproportionately high number of fixed-term contracts of 4 months or less that have started before the reform and have ended right after. Figures A12 to A16 depict that share of contracts (i) having started before the reform; (ii) of a certain duration among contracts of 1 year and less, according to their ending date. For 4-month contracts, for example (Figure A16), we see that there is indeed an increase for contracts ending in April. However, this increase is likely to be driven by the high number of contracts starting on January, 1^{st}, and mechanically ending at the end of April.[35] Indeed, we observe the same increase for years different from the one of the reform, indicating that this pattern is driven by seasonality.

We implement a fuzzy regression discontinuity design on this restricted sample. For this part of the analysis, we gather into a single observation all the contracts that occurred without any interruption for the same individual within the same establishment, so as to be able to capture transitions from fixed-term to permanent contracts within the same firm.[36] We end up with a sample made of 23,559 observations. The treated group is composed of workers having started a fixed-term contract before April, 1^{st}, 2009 that ended after the reform and made them reach a work history lying between 4 and 6 months. They are eligible to unemployment benefits. The control group is made of workers reaching the same work history interval after a fixed-term contract ending before the reform, and who are then not eligible to UI benefits. By construction, the ending date of the contract cannot exceed 6 months after the reform. We then take a similar 6-month time window before the reform.

The idea is that people located very close to the time threshold are likely to be similar, on average, in all respects but their eligibility status. Therefore, any systematic difference in their outcomes can be imputed to the fact that some are eligible to, and then may receive UI benefits. This "quasi experimental design" is closely related to a local randomisation in the neighbourhood of the threshold as on which side any person will be located can be considered random, as long as some assumptions are verified.

Table A13 provides some descriptive statistics on treated and control workers. Control workers have, on average, a slightly higher daily wage, which seems almost entirely driven by the fact that they work more frequently full-time. They also work in smaller establishments. Differences are not big and only concern a few covariates, and thus do not seem

[35]This mechanism is also partly at play for contracts ending in May as we have selected the share of contracts between 4 (included) and 5 (excluded) months.

[36]We do so to correct for the fact that, as the dataset is built on observations within a year, a contract which duration is greater than 1 year is automatically split into two lines although it corresponds to a continuous employment spell.

to challenge the validity of the RDD. More importantly, we will check in next subsection that these covariates do not differ discontinuously at the time threshold. Among treated workers, takers and non-takers do not differ in terms of socio-demographic characteristics (Table A14). Not surprisingly, takers have a higher wage and a higher work history,[37] which is associated to a higher benefit and a longer potential benefit duration.

Empirical methodology – The equation we estimate is the following:

$$Y = \alpha + \tau \mathbb{1}_{Z \geq c} + \delta_f f(Z) + \delta_g g(Z) \mathbb{1}_{Z \geq c} \tag{2}$$

with Y being the outcome, such as the employment probability in this case, $\mathbb{1}_{Z \geq c}$ an indicator equal to 1 when the running variable – the ending date of the contract – is greater or equal to c, the time cutoff, equal to April, 1^{st}, 2009. $f(.)$ and $g(.)$ are flexible functions that we allow to differ on each side of the cutoff. Recall that the variable accounting for work history may suffer some measurement error because we compute it from the DADS which is originally organised by *positions*. This is an issue to the extent that it will undermine the precision of our estimation. However, it should not bias our results as there is no reason why the measurement error should differ from one side of the cutoff to the other, at the close neighbourhood of the threshold.

In this setting, the RD design is qualified as "fuzzy" in the sense that the probability to receive UI benefits does not jump from 0 to 1 for workers with more than 4 months of work history right after the reform. Indeed, having accumulated 4 months of work history at the end of a contract does not mean the person will immediately open an unemployment right as (i) she may very quickly transit to another job; (ii) she may not be informed about her eligibility; (iii) she may be informed but not be willing to take her benefits for many reasons, such as stigma for example.[38]

[37] By construction, both takers' and non takers' work history lies between 120 and 180 days.

[38] According to figures from UI in September 2016 (Insee, 2018), about 75% of individuals eligible to UI benefits actually claim them.

It follows that:

$$Pr(UI = 1|Z = c - \epsilon) = 0 \text{ and } Pr(UI = 1|Z = c + \epsilon) < 1$$

with UI being a dummy indicating if the person receives UI benefits.

Imperfect takeup takes us away from the standard "sharp" RD design. Yet, the identification remains possible as long as we have a jump in the probability of treatment at the cutoff, though lower than one:

$$Pr(UI = 1|Z = c - \epsilon) \neq Pr(UI = 1|Z = c + \epsilon)$$

The "fuzzy" RDD exploits the discontinuity in the probability of treatment at the threshold. The treatment effect can then be recovered by dividing the jump in the relationship between the outcome and the ending date of the contract by the jump in the relationship between the UI benefit receipt and the ending date of the contract at the cutoff.

The estimand can be interpreted as a weighted local average treatment effect, as it is computed on the population of compliers, where the weight represents the ex ante probability of being around the threshold.

The identification rests upon two assumption: (i) monotonicity, *i.e.* the fact that crossing the cutoff does not cause, at the same time, some units to be treated and others to be excluded from treatment; (ii) excludability, *i.e.* the fact that crossing the cutoff does not have an impact on Y other than through the receipt of UI benefits. If the first assumption is verified by definition of the design of the UI eligibility rules, the second assumption cannot be ultimately tested, but some elements make it more credible, that will be further developed in the following paragraphs. Theoretically, if the window considered is not too large, there is no reason for ending the contract right before or right after the April, 1^{st}, 2009 cutoff to affect labour market outcomes other than through the eligibility to UI benefits. To make this excludability assumption more plausible, 3 types of tests are performed: (i) a check of the continuity of the running variable density at the cutoff to get rid of any manipulation suspicion; (ii) a check of the continuity of observed baseline covariates at the cutoff to confirm the non selection and comparability of populations at each side of the cutoff; (iii) a check of the existence of a jump in the probability of being treated at the cutoff, a necessary first-stage to detect any effect.

Validity conditions of the RDD – One key assumption to check for the RDD to be valid is that there is no manipulation at the threshold, or strategic sorting of worker at either side of the threshold. If it was the case, we would have a selection bias that would prevent us from comparing the populations at each side of the cutoff. In particular, we want to check if there is no separation response to the eligibility that would make the density of contract terminations jump at the threshold. We perform a McCrary (2008) test to check that the density of the contract ending date is smooth at the cutoff (Figure 8). The density exhibits some spikes at each month interval – including at the cutoff – due to the regularity in the starting and ending dates of the contract. However, the histogram (Figure 9) indicates that the spike is of the same magnitude as for other months of the year. We also perform the same Mc Crary test at the same time threshold one year before, that confirms that the spike is only due to regularity in contract dates (Figure 10). These different tests demonstrate that there is no precise sorting at the threshold, and that RDD can be considered "as good as randomization" in the neighbourhood of the threshold.

First stage estimation – Equation 2 shows the reduced form of two equations capturing the first stage relationship between the ending date of the contract and the opening of a UI right (Eq. 3) and the second stage relationship between the opening of a UI right and labour market outcomes (Eq. 4).

$$UI = \alpha_f + \tau_f \mathbb{1}_{Z \geq c} + \beta_{f_f} f_f(Z) + \beta_{g_f} g_f(Z) \mathbb{1}_{Z \geq c} + \mu_f \tag{3}$$

$$Y = \alpha_s + \tau_s UI + \beta_{f_s} f_s(Z) + \beta_{g_s} g_s((Z)UI) + \mu_s \tag{4}$$

The estimate τ_s from the two stage least square corresponds to a local average treatment effect.

Table 4 shows that being located at the right hand side of the cutoff makes the probability of opening a UI right significantly increase, from 5.8 to 6.9 percentage points, depending on the specification. Although the effect is not very strong, the estimate is highly significant, and the jump in the probability is clear, as depicted on Figure 11. The weak first-stage regression could raise some precision issues. Table 4 provides F-statistics demonstrating the reliability of the first-stage estimation for all specifications. As underlined in previous subsection, many reasons could explain an imperfect takeup of UI rights, the main one being that there are many job-to-job transitions with small interruptions in between. Therefore, people who know they will be employed again in the very short-run will plausibly not undertake the administrative burden of registering as unemployed.

To fully conclude that the difference in outcomes we observe between populations at each side of the threshold can be imputed to the difference UI takeup, we need to rule out the influence of other variables at the threshold. Figures A17 to A21 do not depict any clear jump in the distribution of covariates at the threshold. The small decrease in the proportion of full-time workers that we observe graphically does not translate into a significant change in the regression, as confirmed by Table 5. Figure A22 shows graphically that the differences are not significant at the 5% level. As already underlined in the analysis of the Mc Crary test, strategic sorting of people on either side of the threshold is very unlikely as the eligibility requirements are closely checked by the unemployment insurance.[39]

Second stage estimation – Empirically, we estimate Equation 2 using a local polynomial regression. The bandwidth has been chosen using an optimal bandwidth selection

[39] In order to open a UI right, a worker must justify his work experience based on employers' certificate delivered at the end of each contract or on payslip.

method minimising the mean squared error (Calonico et al., 2014, 2018a,b). The main specification uses the local linear regression, but Tables A15 to A20 show the same regression using a quadratic specification. Results are quantitatively similar, but the higher-order specification being more demanding in terms of number of observations, coefficients are not always significant. Indeed, the sample restrictions that have been made to ensure that the analysis will not be polluted by a separation response imply that the sample size is smaller, and the coefficients less precisely estimated. However, the fact that the coefficients are of the same magnitude is reassuring on the validity of the effect.

As for the main specification, Tables A21 to A26 show that there is a strong negative response on employment probability even in the long-run (up to 21 months after contract separation). Receiving UI benefits as opposed to not receiving any benefit at all is predicted to decrease employment probability in the future to a large extent.[40] Results are surprising in the sense that we would expect a negative impact in the very short-run and a positive one in the longer-run, as workers in the control group are incentivised to accumulate work history to become eligible in the future. However, we observe that coefficients are not significant in the first three months, and then become negative and significant. A potential explanation is that the population of controls ending a contract before the reform with a work history between 4 and 6 months may only need to work a few hours or a few days after the reform to become eligible to UI benefits, as they would have a work history between 4 and 6 months with their last contract ending after the reform. This could explain why the very short-term effect is not negative. On the other hand, because they need to work a positive number of hours or days to qualify for UI benefits as compared to the treated, they would end up being eligible with a longer average work history than the treated. This would entitle them to a longer benefit duration. If we believe that a longer benefit duration is helpful in finding a more stable job, it could explain that they are less unemployed on the longer-run. In any case, the interpretation of the RDD estimate is not straightforward as it captures not only the

[40]Results are quantitatively difficult to interpret as coefficients are often greater than one whereas the outcome variable is a probability. A bivariate probit model has been used as a complement to accommodate the binary nature of the outcome variable. Results displayed in Tables A27 to A32 indicate a 60 percentage points drop in the probability of being employed in the short-run.

effect of being eligible to UI benefits, but also the effect, for control workers, of having incentives to find a job quickly to increase work history.

Figure 12 illustrates the effect graphically on the probability to be employed 6 months after the end of the contract determining the eligibility status. Similar graphs plotting the probability to be unemployed and to be in the labour force can be found in Figures 13 and 14.[41] They indicate that the lower employment probability of compliers transits through a higher probability to be non-employed but not registered for UI.

Consistently, we find a positive impact of receiving UI benefits on the time to the next employment spell, indicating that workers receiving UI benefits take more time to find a new job (Table A33). This result is in line with what has been previously found on the impact of UI benefits at the intensive margin on the unemployment duration (Card and Levine, 2000; Chetty, 2008; Card et al., 2015; Kroft and Notowidigdo, 2016; Landais, 2015; Le Barbanchon, 2016). Indeed, it has been shown that more generous UI benefits were associated with longer unemployment spells. However, much less has been said on the extensive margin impact of UI benefits, that is the effect of receiving any benefit at all. In this paper, we show that there is a negative and long-lasting impact of receiving UI benefits on the probability to be employed.

To complement this picture, we then look at other dimensions of employment related to job quality. The literature on this topic draws less clear-cut conclusions (Nekoei and Weber, 2017; Card et al., 2007; Schmieder et al., 2016; Lalive, 2007; Van Ours and Vodopivec, 2008). The standard job search model predicts that receiving UI benefits would enhance job quality as reservation wages are set higher. However, negative duration dependence could counterbalance this effect. Tables A34 to A36 show a negative impact on daily wage, that starts to appear in the medium-run and is still present in the long-run. However, for the assumption of the RDD to hold, we need to take into account in the estimation all the workers of the sample without restricting to those being employed. It means that we impute a zero value to earnings of workers not employed at the different time horizons. Not considering them in the estimation would mean to condition on

[41] The corresponding regressions at different time horizons are available upon request.

an endogenous variable, that is being employed. Then, the negative relationship can arise both from the negative impact on employment or from a negative impact on daily earnings, conditional on being employed.

To better disentangle both channels, we look at cumulative earnings over a 2-year period. This measure allows not to condition on endogenous employment status but still to capture a potential effect on job quality. The idea is that, over a 2-year horizon, if the receipt of UI benefits would have a positive and significant impact on daily wage, the positive effect on wage would have the time to offset the negative effect on employment probability. Table 6 shows negative coefficients, although standard errors are large. Compliers forego about 30,000€ over two years, which amounts to an average monthly loss of 1,250€ in gross terms.[42] Compared to the average daily wage of control workers reported in Table A13, it represents a 76% reduction, although the difference in cumulative earnings partly comes from a lower number of days employed, and not necessarily from lower daily wages. This sizeable effect suggests that, if any, the potential increase in compliers' daily wage is not large enough to compensate their lower employment probability.

Not to restrict the analysis only to the monetary dimension of job quality, we look at other outcomes associated to the next employment spell.[43] Tables A37 to A40 show the effect on the probability to have a permanent contract, to work full-time, to work in the same 2-digit industry as in the previous job and on the duration of the next employment spell. The effect seems negative on working hours, but positive on the probability to have a permanent contract, although the duration of the next employment spell is negatively impacted. However, none of the coefficients associated to these qualitative dimension of the next employment spell are significant.

Analysing the impact on characteristics of the next employment spell is informative on the type of jobs found by treated and controls after the reform, but forces to condition on having a job by the end of the observation period. Any impact we could measure would then be a mix of treatment and selection effects. We then perform two comple-

[42] We do not impute values of unemployment benefits in this calculation.

[43] By construction, individuals who do not find a job within the observed period will have those variables coded as missing.

mentary analyses to capture the unconditional impact of receiving UI benefits. The first method consists in measuring the probability of having a job with a given characteristic at different time horizons, putting a 0 value both to those not having a job and to those having a job not meeting the criterion under study. Similar to the analysis of daily wage, it yields an unbiased estimate of the causal impact of the reform, but it does not allow to distinguish between the employment probability channel and the quality channel conditional on having a job. In the second method, we use the technique developed by Lee (2009) to get bounds on the treatment effect on different dimensions of job quality, taking into account the selection into employment. Results yielded by both techniques can be found from Tables A41 to A88. All in all, quality dimensions measured unconditionally seem negatively impacted, which is not surprising given the negative effect on employment probability. Bounds on treatment effects are generally not very informative as they are almost centred around zero, except for the probability to have a permanent contract (Tables A65 and A66). The intervals still include 0 but are to a large extent positive in the short-run. This result combined with positive (but insignificant) coefficients on the conditional probability that the next employment spell is under a permanent contract suggest that there might be a positive impact of UI benefits receipt on this dimension of job quality. The fact that we do not find any significant positive impact of UI benefits receipt on the quality of the match - as measured by the probability to work in the same 2-digit industry as before - is not in line with the findings in Davezies and Le Barbanchon (2017). However, they are examining a different sample (before the 2009 reform) at a different threshold. They look at the 6-month eligibility threshold whereas we use the time threshold of the reform. Potential sorting at the eligibility threshold caused by the eligibility status could affect their results.[44]

To clarify on the ultimate impact on career path, we propose a measure of stability in employment over a long-term horizon – 2 years. Therefore, we look at the number and

[44]As mentioned at the beginning of the section, the assumptions of a regression discontinuity design around the eligibility threshold may not hold if there is a separation response at the threshold because the worker becomes eligible to UI benefits.

duration of the employment spells over the following two years. The idea is to capture whether the individual does a lot of transitions in and out of employment, and whether his employment spells are short or not. Indeed, having a higher probability of being employed at different points in time could still be associated with a highly fragmented path, if the person goes very often from one job to the other but with short breaks in between. Table A89 indicates that receiving UI benefits is associated to a lower number of employment spells over the following two years. This could either be interpreted positively – a more stable path – or negatively – more time unemployed. The impact on the number of unemployment spells is positive but insignificant, whereas the impact on the total number of days employed is negative and significant in the linear specification (Tables A90 and A91). Those results taken together suggest that receiving UI benefits does not seem to contribute to a more secured career path.

To clarify the interpretation of our results, we finally complement the analysis by looking at the employment response using a bivariate probit specification. Indeed, the regression discontinuity design relying on local polynomial estimation on each side of the cutoff may not be suitable for binary outcome variables such as probabilities, as they yield coefficients outside the feasible range. However, bivariate probit relies on a parametric specification and distributional assumptions,[45] which may explain why they may not be preferred to linear models. We provide the estimation results using a bivariate probit on the main outcome variable, the employment probability. Tables A27 to A32 display marginal effects that are qualitatively similar to results obtained using local polynomial regressions. Receiving UI benefits is predicted to decrease employment probability six months after the end of the contract by 61 percentage points. The effect remains strong around 53 percentage points up to 21 months after the end of the contract.

[45]Notably regarding homoskedasticity, homogenous treatment effect and normally distributed error terms.

6 Conclusion

In this paper, we analyse the impact of the eligibility criterion that imposes a minimum employment history to access UI benefits. We complement previous analyses of the impact of the *level* of UI benefits (i.e., at the intensive margin) on *unemployment* duration by looking at the impact of *receiving any UI benefits* (i.e., at the extensive margin) both on transitions *in and out* of employment. To do so, we take advantage of a French reform that changed the work history threshold from 6 months over the past 22 months to 4 months over the past 28 months in April, 2009. The first part of the analysis reveals a jump in the transition rate from employment to unemployment at the UI eligibility threshold. At the aggregate level, we also observe a change in contract duration that is consistent with the evolution of the threshold. After the reform, the number of 4-month fixed-term contracts increased relative to the number of 6-month fixed-term contracts.

Taken together, those results are indicative of a response from both employers and employees to incentives generated by UI. While employers gain in flexibility through the opportunity to reduce the length of contracts more easily, workers may be more prone to accept shorter contracts due to the reform. However, we find that receiving benefits is predicted to decrease future employment probability, even in the medium term, without any significant improvement in terms of job quality. In addition to this, the new norm introduced by the reform regarding the length of working contracts may incur negative spillovers for some employed workers who are offered shorter contracts. Overall, our results shed light on the potential detrimental effects of UI rules created to help unemployed workers securing their situation.

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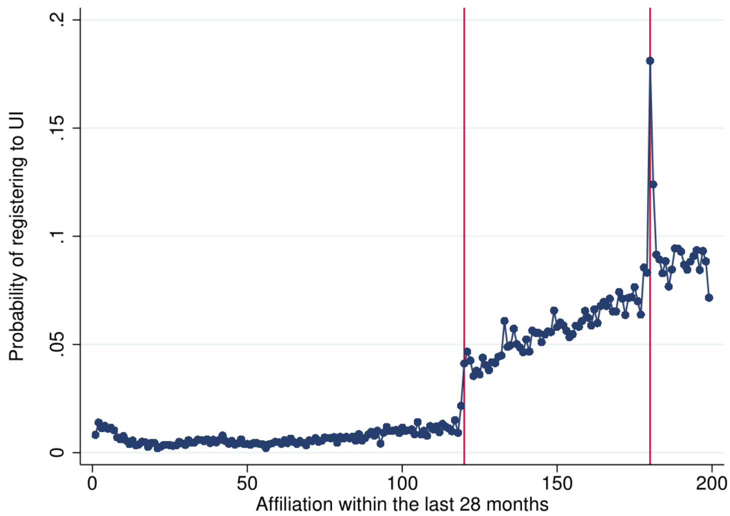
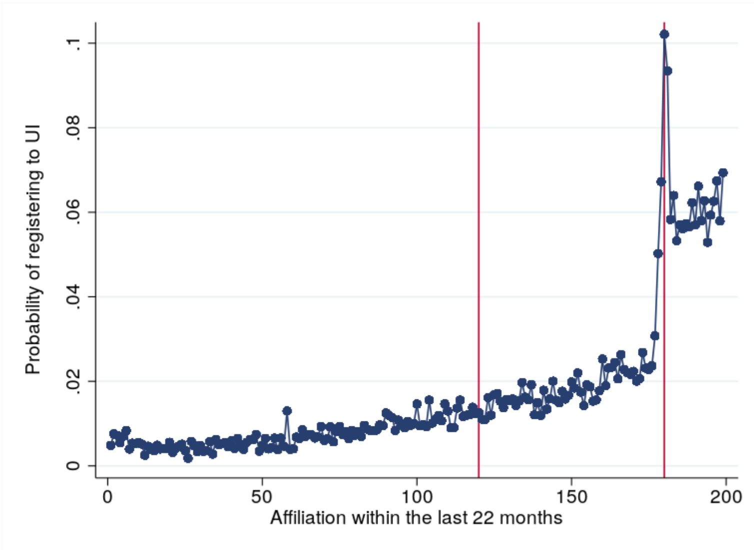
Main Tables and Figures

Figure 1: Probability of UI registration conditional on separation (Post-reform, 28-months base reference period)



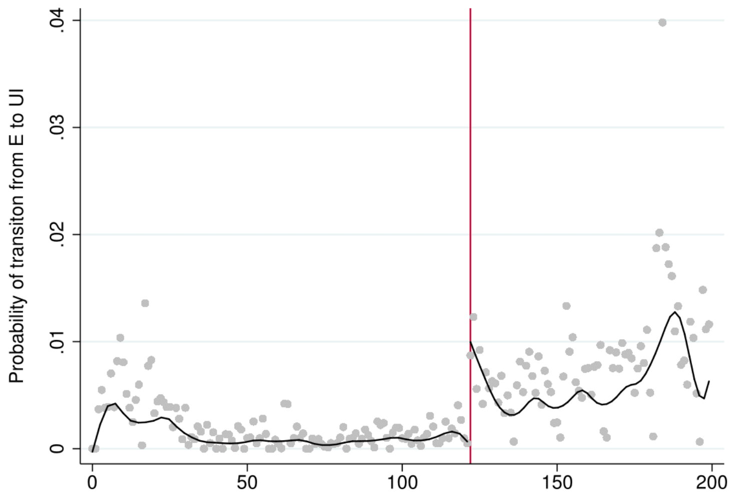
SOURCE: FH-DADS.
NOTE: This graph plots the probability to transit from employment to registered unemployment conditional on ending a contract, with respect to work history computed within the last 28 months. The two vertical lines represent, respectively, 4 months and 6 months of work history. We restrict the sample to contracts ending between April, 1^{st}, 2009 and December, 31^{st}, 2012, which corresponds to the post-reform period.

Figure 2: Probability of UI registration conditional on separation (Pre-reform, 22-months base reference period)



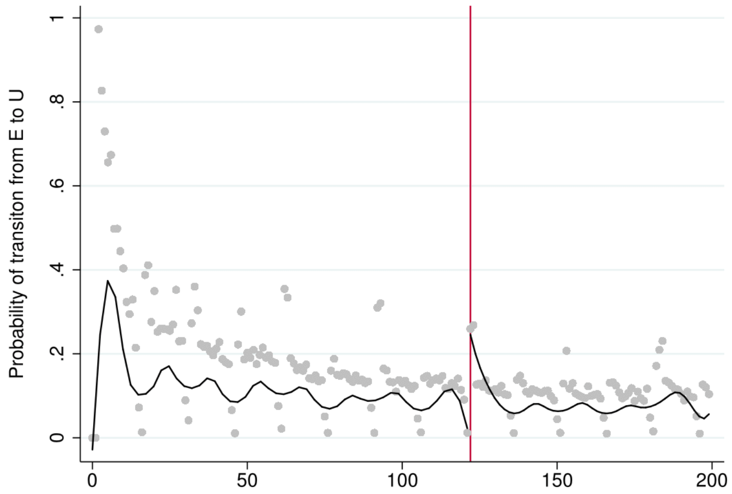
SOURCE: FH-DADS.
NOTE: This graph plots the probability to transit from employment to registered unemployment conditional on ending a contract, with respect to work history computed within the last 22 months. The two vertical lines represent, respectively, 4 months and 6 months of work history. We restrict the sample to contracts ending between January, 1^{st}, 2004 and March, 30^{th}, 2009, which corresponds to the pre-reform period.

Figure 3: Probability to go from employment to registered unemployment



SOURCE: FH-DADS.
NOTE: This graph plots the biweekly transition rate from employment to registered unemployment, with respect to work history computed within the last 28 months. We restrict the sample to contracts beginning after April, 1^{st}, 2009 and we track every transition in and out of employment over the following two years (post-reform period). A transition rate around 0.01 at exactly 4 months of work history means that 1% of employed workers at the beginning of the two-week spell had transited from employment to registered unemployment within the last 15 days with a work history equal to 4 months at the end of the two-week spell.

Figure 4: Probability to go from employment to non-employment



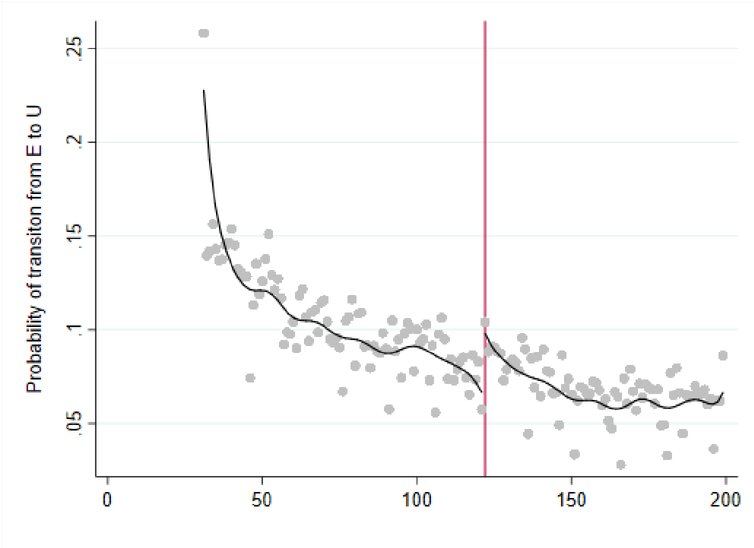
SOURCE: FH-DADS.
NOTE: This graph plots the biweekly transition rate from employment to non-employment, with respect to work history computed within the last 28 months. We restrict the sample to contracts beginning after April, 1^{st}, 2009 and we track every transition in and out of employment over the following two years (post-reform period). A transition rate of 0.27 at exactly 4 months of work history means that 27% of employed workers at the beginning of the two-week spell had transited from employment to non-employment within the last 15 days with a work history equal to 4 months at the end of the two-week spell.

Table 1: Discontinuity in the transition rate from employment to registered unemployment

	Probability of transiting from employment to registered unemployment		
RD_Estimate	0.008***	0.007***	0.010***
	(0.001)	(0.001)	(0.001)
Observations	1270880	1270880	1270880

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The regression shows in a regression discontinuity design spirit the discontinuity in the biweekly transition rate from employment to UI. The running variable is the work history over the last 28 months and the cutoff value is 4 months. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector with a linear specification.

Figure 5: Transition probability from employment to non-employment, restricted to full-time workers



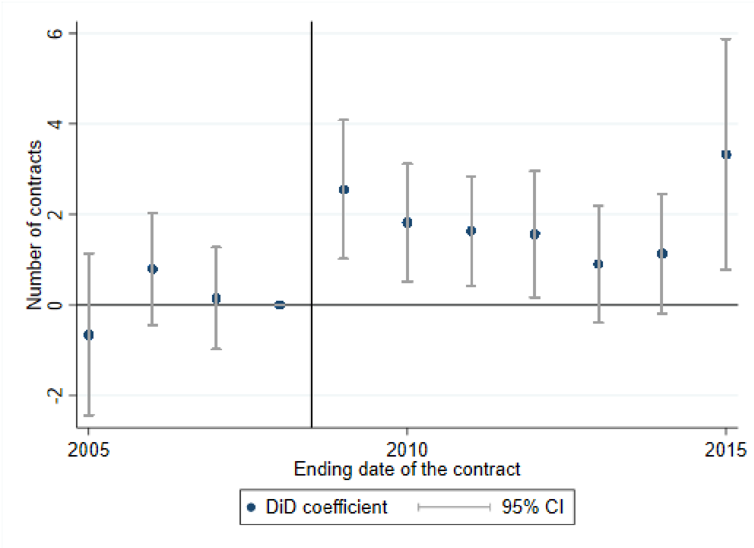
SOURCE: UI data (FNA)
NOTE: This graph plots the biweekly transition rate from employment to non-employment, with respect to work history computed within the last 28 months. We first restrict the sample to contracts beginning after April, 1^{st}, 2009 and we track every transition in and out of employment over the following two years (post-reform period). We then further restrict the sample to workers with a number of hours corresponding to the working time of a full-time employee working every day covered by the *position*, to get rid of some of the measurement error. A transition rate of 0.1 at exactly 4 months of work history means that 10% of employed workers at the beginning of the two-week spell had transited from employment to non-employment within the last 15 days with a work history equal to 4 months at the end of the two-week spell.

Table 2: Discontinuity in the transition rate from employment to non-employment on full-time workers

	Probability of transiting from employment to non-employment		
RD_Estimate	0.027***	0.032***	0.036***
	(0.006)	(0.008)	(0.010)
Observations	436350	436350	436350

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The regression shows in a regression discontinuity design spirit the discontinuity in the biweekly transition rate from employment to non-employment. The running variable is the work history over the last 28 months and the cutoff value is 4 months. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector with a linear specification. The sample has been restricted to workers whose number of hours corresponds to a daily full working time multiplied by the number of days covered by the *position* to reduce the probability that the *position* does not correspond to an uninterrupted employment spell.

Figure 6: Yearly evolution of the number of 4-month contracts relative to 6-month contracts (end of fixed-term contracts)



SOURCE: MMO
NOTE: This graph plots the yearly evolution in the number of fixed-term 4-month contracts relative to fixed-term 6-month contracts, with month, year, sector and firm fixed-effects. The reference year is 2008, the last pre-reform year. The number of contracts has been computed at the firm × month level, and then aggregated at the national level. Standard errors are clustered at the firm level.

Table 3: Difference-in-difference estimate of the number of 4-month contracts relative to 6-month contracts (End of fixed-term contracts)

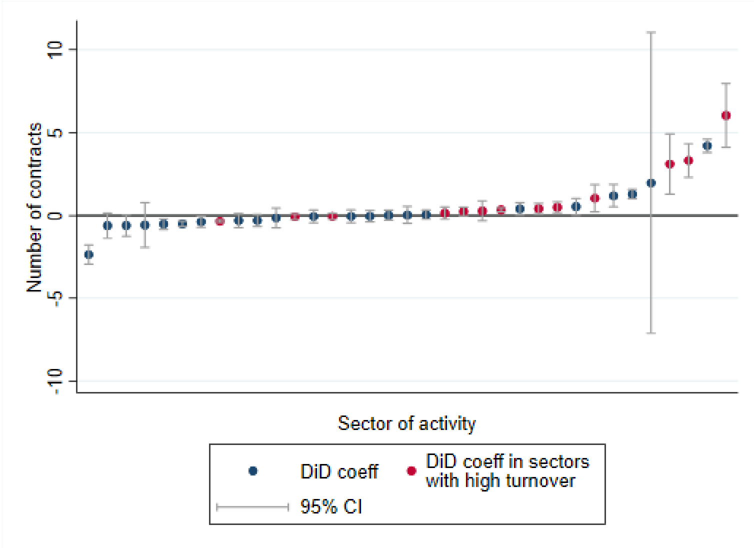
	Within firm monthly number of contracts				
4-month (versus 6m) contracts	7.44499***	4.88344***	3.67946	-0.59904	-0.86041
	(1.104302)	(0.136075)	(2.540706)	(0.420878)	(0.607546)
Post-reform	-0.13257	1.29470***	1.25968	0.48156	0.37090
	(0.281080)	(0.336468)	(0.898371)	(0.567464)	(0.814415)
4-month (versus 6m) contracts × Post-reform	0.49209	0.25898	0.24836	1.53001**	2.38411***
	(1.396799)	(0.161821)	(0.455498)	(0.605193)	(0.912708)
Constant	3.75650***	4.17694***	4.83235***	8.91393***	13.82090***
	(0.256077)	(0.241221)	(1.724860)	(0.433443)	(0.603916)
Month fixed-effect	No	Yes	Yes	Yes	Yes
Year fixed-effect	No	Yes	Yes	Yes	Yes
Sector fixed-effect	No	No	Yes	Yes	Yes
Firm fixed-effect	No	No	No	Yes	Yes
Sample	Sample 1	Sample 1	Sample 1	Sample 1	Sample 2
Observations	549208	549208	549208	517695	352660

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*

SOURCE: MMO.

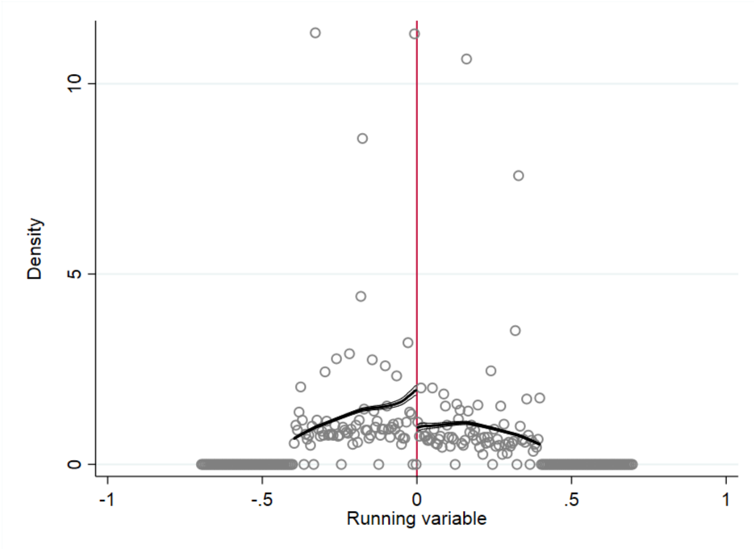
NOTE: The table shows the difference-in-difference estimate of the number of fixed-term 4-month contracts relative to fixed-term 6-month contracts before and after the reform, computed at the firm × month level and aggregated at the national level. The first column displays the raw regression, and month, year, sector and firm fixed-effects are progressively added. The two last column include all fixed-effects. Construction of Sample 1 is detailed in the paper. Sample 2 is a restriction to firms observed in both period with at least one 4-month and one 6-month contracts ending in each period. Standard errors are clustered at the sector level for the 3^{rd} specification, and at the firm level for the last two ones.

Figure 7: Within sector change in the number of 4-month contracts relative to 6-month contracts (end of fixed-term contracts)



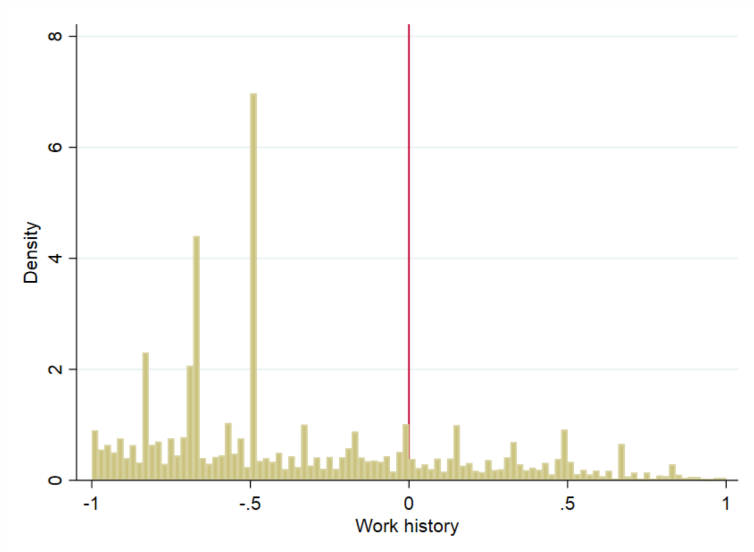
SOURCE: MMO
NOTE: This graph plots the difference-in-difference estimate of the change in the number of fixed-term 4-month contracts relative to fixed-term 6-month contracts by sector. The number of contracts has been computed at the firm × month level, and then aggregated at the national level, with month, year, and firm fixed-effects. The regression has been run separately in each sector. Standard errors are clustered at the firm level. Sectors in red are the ones identified as having a high separation rate or a high share of very short contracts (Decree No. 2019-797 of 26 July 2019 on the unemployment insurance scheme, DARES, 2018).

Figure 8: Mc Crary test contract ending date distribution



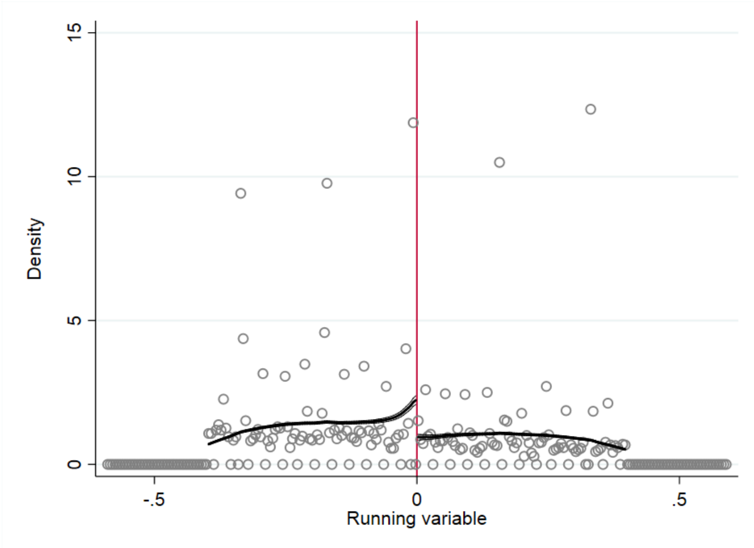
SOURCE: FH-DADS.
NOTE: This figure is the McCrary (2008) test performed on the sample defined using the methodology detailed in Section 5 at the reform threshold. The running variable, ending date of the contract, has been normalised around the time threshold to be equal to 0 at the threshold, -1 six months before, and +1 six months after. We observe a small discontinuity at the threshold, but that is driven by regularity in starting and ending date of contracts.

Figure 9: Histogram of the contract ending date frequencies



SOURCE: FH-DADS.
NOTE: The running variable, ending date of the contract, has been normalised around the time threshold to be equal to 0 at the threshold, -1 six months before, and +1 six months after. The bin size is equal to 0.02.

Figure 10: Mc Crary test contract ending date distribution one year before the reform (2008)



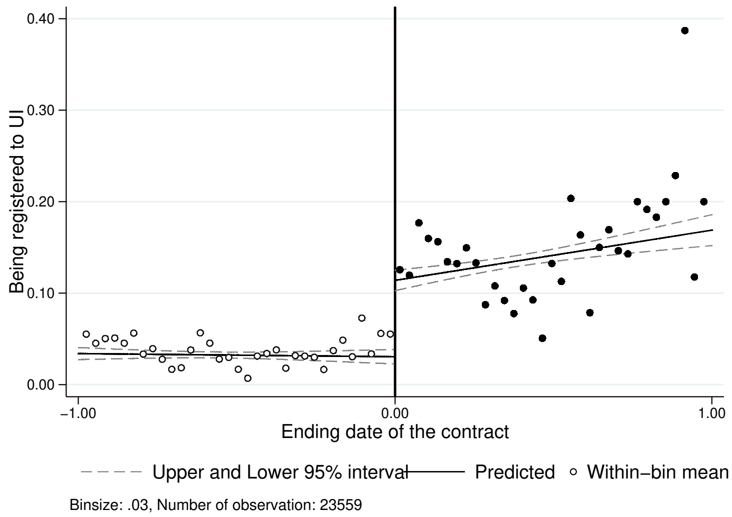
SOURCE: FH-DADS.
NOTE: This figure is the McCrary (2008) test performed on the sample defined using the methodology detailed in Section 5, using 2008 as the reform year. The running variable, ending date of the contract, has been normalised around the time threshold to be equal to 0 at the threshold, -1 six months before, and +1 six months after. This placebo tests aims at showing that the small discontinuity at the time threshold is only driven by regularity in contract starting and ending dates, as it is the same the year of the reform and the year before.

Table 4: Impact of separating after the reform on UI takeup

	Register as unemployed		
	Linear	Quadratic	Cubic
RD_Estimate	0.069***	0.061**	0.058**
	(0.021)	(0.024)	(0.024)
F-stat	72.64	46.38	21.14
Observations	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table shows the first-stage regression from the fuzzy regression discontinuity design. It regresses the probability to open a new UI right on an assignment variable indicating whether the workers has ended his contract before or after the reform. It shows that ending a contract after the reform, in its close neighbourhood, is associated to a strong increase in the probability of opening a UI right. The bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector.

Figure 11: Probability of opening a UI right



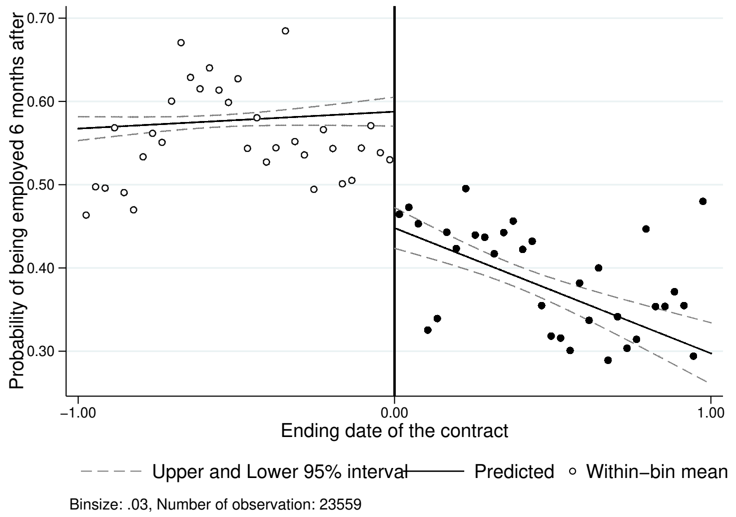
SOURCE: FH-DADS.
NOTE: This graph shows the relationship between the probability to open a new UI right and the ending date of the contract. The running variable, ending date of the contract, has been normalised around the time threshold to be equal to 0 at the threshold, -1 six months before, and +1 six months after. The vertical line corresponds to the reform. It shows that ending a contract after the reform, in its close neighbourhood, is associated to a strong increase in the probability of opening a UI right.

Table 5: Check of no discontinuity in the distribution of covariates

	Gender	Level of education	Daily wage	Fulltime	Establishment size
Treated	0.025	-0.104	-1.094	0.020	24.201
	(0.029)	(0.237)	(4.710)	(0.032)	(26.674)
Ending date of the contract	-0.322***	-0.199	-35.248*	-0.112	-15.210
	(0.122)	(0.829)	(20.636)	(0.150)	(95.425)
Treated=1 × Ending date of the contract	0.308	0.824	-13.588	-0.311	-34.776
	(0.200)	(1.327)	(35.658)	(0.253)	(161.536)
Constant	0.560***	4.185***	58.154***	0.539***	64.249***
	(0.017)	(0.141)	(2.695)	(0.018)	(15.767)
Observations	4739	1341	4384	4104	5388

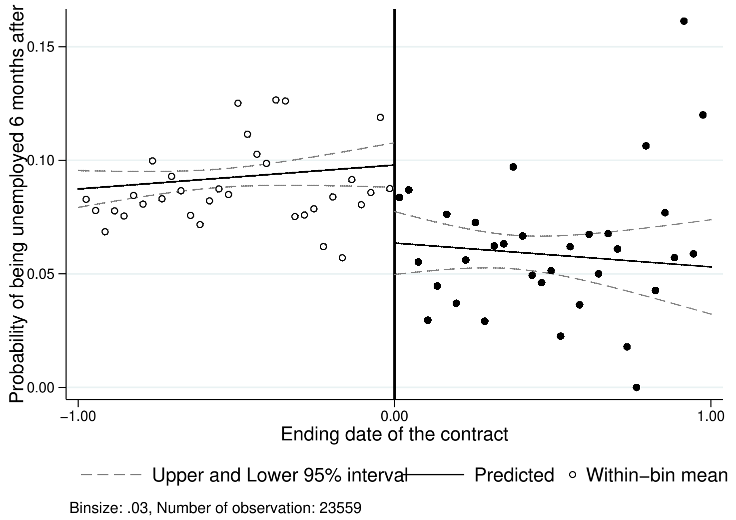
** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regression discontinuity estimates, using each covariate as the dependent variable, to test the assumption of continuity of the covariates distribution at the threshold. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification.

Figure 12: Impact of UI eligibility on employment probability 6 months after



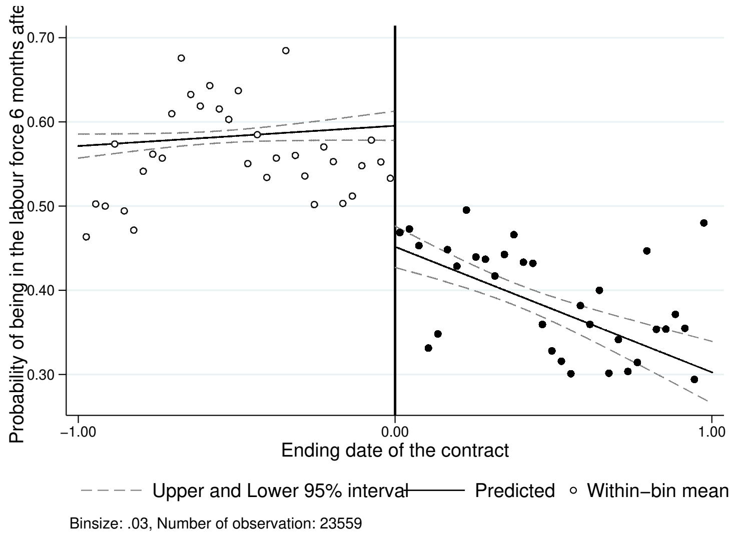
SOURCE: FH-DADS.
NOTE: The running variable, ending date of the contract, has been normalised around the time threshold to be equal to be equal to 0 at the threshold, -1 six months before, and +1 six months after. Points have been fitted using a local linear regression with a bandwidth equal to 0.03. The graph plots the probability of being employed six months after the end of the contract.

Figure 13: Impact of UI eligibility on unemployment probability 6 months after



SOURCE: FH-DADS.
NOTE: The running variable, ending date of the contract, has been normalised around the time threshold to be equal to be equal to 0 at the threshold, -1 six months before, and +1 six months after. Points have been fitted using a local linear regression with a bandwidth equal to 0.03. The graph plots the probability of being unemployed defined as being registered for UI, six months after the end of the contract.

Figure 14: Impact of UI eligibility on labour market participation probability 6 months after



SOURCE: FH-DADS.
NOTE: The running variable, ending date of the contract, has been normalised around the time threshold to be equal to be equal to 0 at the threshold, -1 six months before, and +1 six months after. Points have been fitted using a local linear regression with a bandwidth equal to 0.03. The graphs plots the probability of being in the labour force defined as being either employed or in registered unemployment, six months after the end of the contract.

Table 6: Impact of UI benefit receipt on cumulative earnings over two years

	Cumulative earnings over 2 years		
	Linear	Quadratic	Cubic
RD_Estimate	-34790.054*	-29929.143	-37389.971
	(17979.466)	(25519.166)	(30919.655)
Observations	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear, quadratic and cubic specifications. The dependent variable corresponds to earnings accumulated over two years after the end of the contract that defines the treatment status.

Appendix - additional Tables and Figures

Table A1: Sample composition (MMO data)

Separation motive	Number of observations	Number of contracts	Number of firms
End of fixed-term contract	22,208,669	67,075,786	181,162
Personal dismissal	1,417,836	4,335,334	115,134
Economic layoff	340,448	1,426,004	35,984
Quits	3,212,904	13,205,768	166,798
Retirement	820,051	2,126,980	85,982
Pre-retirement	16,440	45036	5,148
End of trial period	934,844	4,247,370	87,806

NOTE: This table details the sample composition of the data used to measure the response to the change in the UI eligibility criterion on contract duration (MMO data, DARES). The number of contracts corresponds to the weighted number of observations, as some firms are surveyed, and weights also adjust for under-declaration.

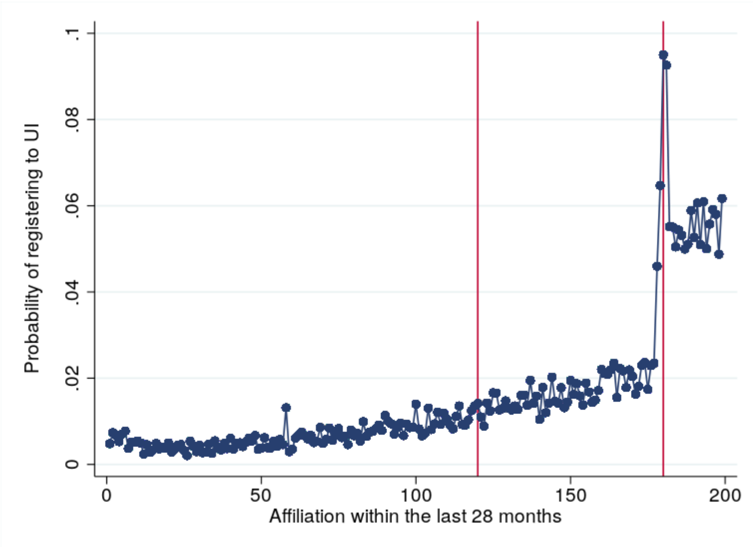
Table A2: Descriptive statistics by work history

	Work history < 6 months	Work history ≥ 6 months	Difference (2) - (1)
Gender	0.587	0.612	0.025***
			(0.0004)
Level of education	4.213	4.488	0.275***
			(0.0038)
Hourly wage	13.046	15.586	2.540***
			(0.1297)
Permanent contract	0.162	0.463	0.301***
			(0.0004)
Fulltime	0.602	0.692	0.090***
			(0.0004)
Establishment size	113.390	258.550	145.160***
			(0.8760)
Experience on the labour market (years)	5.526	10.794	5.267***
			(0.0081)
Daily number of hours worked	4.075	4.187	0.112***
			(0.0026)
Probability to hold multiple jobs in a given month	0.054	0.040	-0.014***
			(0.0002)
Average number of simultaneous jobs in a given month	1.067	1.049	-0.018***
			(0.0001)
Observations	1942608	6491757	8434365

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*

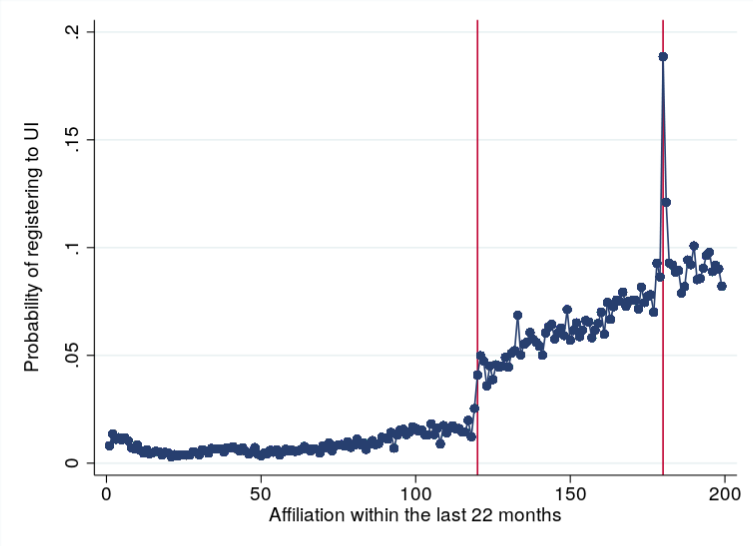
NOTE: This table displays descriptive statistics comparing workers with an employment record of more or less than 6 months over the last 28 months. These statistics have been computed on the sample of workers employed during the 2004-2012 period using the DADS. Work history has been computed by the authors.

Figure A1: Probability of UI registration conditional on separation (Pre-reform, 28-months base reference period)



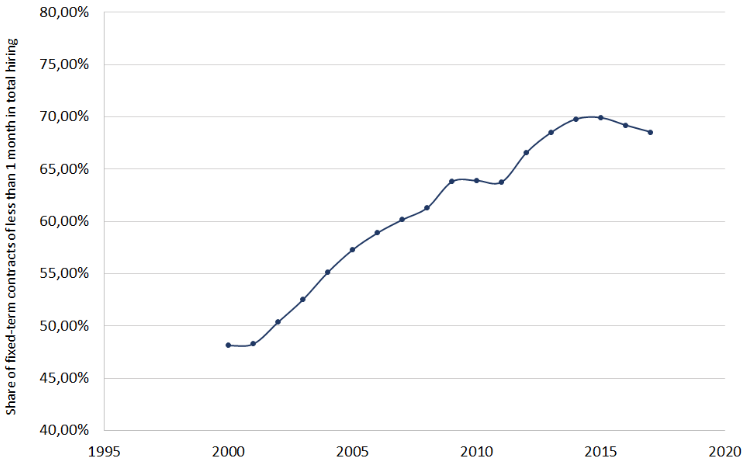
SOURCE: FH-DADS.
NOTE: This graph plots the probability to transit from employment to registered unemployment with respect to work history computed within the last 28 months. The two vertical lines represent, respectively, 4 months and 6 months of work history. We restrict the sample to contracts ending between January, 1^{st}, 2004 and March, 30^{th}, 2009, which corresponds to the pre-reform period. Unlike in the post-reform period, we do not observe any discontinuity at the 4-month threshold.

Figure A2: Probability of UI registration conditional on separation (Post-reform, 22-months base reference period)



SOURCE: FH-DADS.
NOTE: This graph plots the probability to transit from employment to registered unemployment with respect to work history computed within the last 22 months. The two vertical lines represent, respectively, 4 months and 6 months of work history. We restrict the sample to contracts ending between April, 1^{st}, 2009 and December, 31^{st}, 2012, which corresponds to the post-reform period. Unlike in the pre-reform period, we do not observe any discontinuity at the 6-month threshold.

Figure A3: Share of fixed-term contracts of less than one month in total hiring (France, 2000-2017)



SOURCE: DPAE (ACOSS), DSN (DARES).
NOTE: This figure plots the share of fixed-term contracts of less than one month in total hiring in France, from 2000 to 2017. It shows that very short contracts account for most of the employment flows, and that their share has massively increased throughout the last two decades.

Table A3: Descriptive statistics by work history - sectors of activity

	Work history < 6 months	Work history ≥ 6 months	Difference (2) - (1)
Agriculture, Forestry and Fishing	0.0320	0.0148	-0.0172***
			(0.00014)
Extractive industry	0.0002	0.0009	0.0006***
			(0.00003)
Manufacturing industry	0.0543	0.1159	0.0616***
			(0.00031)
Gas and electricity	0.0011	0.0067	0.0056***
			(0.00008)
Water supply, Sanitation, Waste management	0.0020	0.0056	0.0036***
			(0.00007)
Construction	0.0483	0.0762	0.0279***
			(0.00027)
Retail and wholesale trade; Car repair	0.1344	0.1517	0.0173***
			(0.00037)
Transportation and storage	0.0253	0.0635	0.0382***
			(0.00024)
Food and accommodation	0.1088	0.0782	-0.0307***
			(0.00029)
Information and Communication	0.0378	0.0475	0.0097***
			(0.00022)
Financial and Insurance activities	0.0177	0.0364	0.0187***
			(0.00018)
Real estate	0.0090	0.0135	0.0045***
			(0.00012)
Specialised, scientific and technical activities	0.0425	0.0645	0.0220***
			(0.00025)
Administrative services and support activities	0.3772	0.2065	-0.1707***
			(0.00044)
Public administration	0.0029	0.0097	0.0068***
			(0.00009)
Teaching	0.0093	0.0122	0.0029***
			(0.00011)
Health and Social action	0.0367	0.0487	0.0120***
			(0.00022)
Art and entertainment	0.0341	0.0221	-0.0120***
			(0.00016)
Other services	0.0261	0.0254	-0.0007***
			(0.00016)
Extraterritorial activities	0.0000	0.0000	0.0000*
			(0.00001)
Observations	1188815	3968959	5157774

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*

NOTE: This table displays descriptive statistics comparing workers with an employment record of more or less than 6 months over the last 28 months. These statistics have been computed on the sample of workers employed during the 2004-2012 period using the DADS. Work history has been computed by the authors.

Table A4: Descriptive statistics by work history – Occupation type

	Work history < 6 months	Work history ≥ 6 months	Difference (2) - (1)
Farmer	0.0001	0.0000	-0.0000***
			(0.00001)
Craftsperson	0.0001	0.0007	0.0006***
			(0.00002)
Retail trader	0.0004	0.0015	0.0011***
			(0.00003)
Head of a company of 10 employees or more	0.0008	0.0047	0.0039***
			(0.00005)
Professional activity (doctor, architect, etc.) under a salaried status	0.0005	0.0010	0.0004***
			(0.00002)
Civil-servant executives	0.0001	0.0027	0.0026***
			(0.00004)
Professors, Scientific occupations	0.0025	0.0048	0.0024***
			(0.00005)
Information, art and entertainment	0.0350	0.0208	-0.0142***
			(0.00013)
Administration and business executives	0.0122	0.0536	0.0415***
			(0.00017)
Specialised executives and engineers	0.0083	0.0414	0.0331***
			(0.00015)
Primary school teachers	0.0080	0.0092	0.0013***
			(0.00008)
Social work and health intermediate professions	0.0174	0.0192	0.0018***
			(0.00011)
Clergy	0.0000	0.0001	0.0000***
			(0.00001)
Administrative intermediate professions of the public sector	0.0008	0.0047	0.0038***
			(0.00005)
Administrative and business intermediate professions of the private sector	0.0450	0.0829	0.0379***
			(0.00021)
Technicians	0.0150	0.0357	0.0207***
			(0.00014)
Foreman	0.0050	0.0180	0.0130***
			(0.00010)
Civil-servants	0.0174	0.0230	0.0056***
			(0.00012)
Supervising officer	0.0115	0.0133	0.0018***
			(0.00009)
Administrative employees in firms	0.0874	0.0984	0.0111***
			(0.00024)
Commercial employee	0.1201	0.0804	-0.0398***
			(0.00023)
Employees providing services to individuals	0.1097	0.0729	-0.0368***
			(0.00022)
Skilled worker in the industry	0.0522	0.0721	0.0198***
			(0.00021)
Skilled worker in the arts and crafts	0.0789	0.0853	0.0064***
			(0.00023)
Driver	0.0322	0.0436	0.0115***
			(0.00016)
Skilled worker in retail handling, stocking and transportation	0.0244	0.0262	0.0018***
			(0.00013)
Unskilled worker in the industry	0.1529	0.0787	-0.0742***
			(0.00024)
Unskilled worker in the arts and crafts	0.0785	0.0570	-0.0215***
			(0.00020)
Agricultural worker	0.0331	0.0123	-0.0208***
			(0.00011)
Observations	1942564	6491702	8434266

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*

NOTE: This table displays descriptive statistics comparing workers with an employment record of more or less than 6 months over the last 28 months. These statistics have been computed on the sample of workers employed during the 2004-2012 period using the DADS. Work history has been computed by the authors.

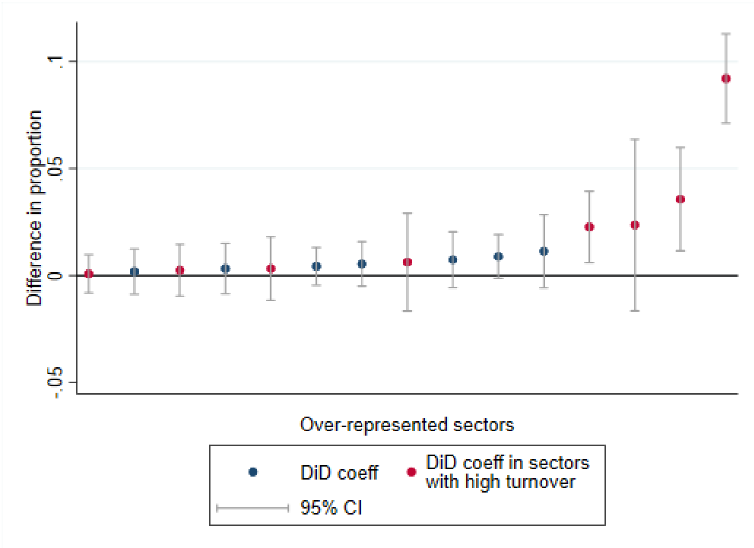
Table A5: Sectorial distribution of firms with a high relative increase in 4-month contracts (End of fixed-term contracts)

	High response	All other firms	Difference (2)-(1)
Manufacture of food, beverages and tobacco products	0.1263	0.0342	-0.0921***
			(0.01062)
Manufacture of textiles, wearing apparel, leather and related products	0.0171	0.0083	-0.0088
			(0.00529)
Manufacture of wood except furniture, paper, printing	0.0034	0.0072	0.0038
			(0.00494)
Manufacture of chemicals and chemical products	0.0137	0.0084	-0.0053
			(0.00532)
Manufacture of rubber, plastic products, and other metallic non mineral products	0.0068	0.0096	0.0027
			(0.00569)
Manufacture of basic and fabricated metal products, except machinery and equipment	0.0137	0.0105	-0.0032
			(0.00596)
Manufacture of machinery and equipment	0.0034	0.0067	0.0033
			(0.00476)
Manufacture of motor vehicles, trailers, semi-trailers and other transport equipment	0.0068	0.0077	0.0008
			(0.00510)
Water supply; sewerage, waste management and remediation activities	0.0068	0.0061	-0.0007
			(0.00455)
Wholesale and retail trade; repair of motor vehicles and motorcycles	0.1502	0.1919	0.0417
			(0.02301)
Transportation and storage	0.0478	0.0416	-0.0062
			(0.01167)
Accommodation and food service activities	0.0819	0.0463	-0.0356**
			(0.01228)
Publishing, programming and broadcasting activities	0.0205	0.0173	-0.0032
			(0.00762)
Telecommunications	0.0102	0.0060	-0.0042
			(0.00451)
Computer programming, consultancy, information service activities	0.0102	0.0085	-0.0018
			(0.00535)
Financial and insurance activities	0.0273	0.0316	0.0043
			(0.01022)
Real estate activities	0.0068	0.0091	0.0023
			(0.00555)
Legal, accounting, management consultancy, architectural and engineering activities; technical testing and analysis	0.0068	0.0269	0.0201*
			(0.00945)
Scientific research and development	0.0034	0.0068	0.0034
			(0.00479)
Advertising and market research, veterinary, other professional, scientific and technical activities	0.0137	0.0113	-0.0024
			(0.00616)
Administrative and support service activities	0.1672	0.1437	-0.0236
			(0.02049)
Public administration and defense; compulsory social security	0.0205	0.0132	-0.0073
			(0.00666)
Education	0.0341	0.0229	-0.0113
			(0.00873)
Human health activities	0.0683	0.0723	0.0040
			(0.01513)
Residential care, social work without accommodation activities	0.0614	0.1574	0.0959***
			(0.02127)
Arts, entertainment and recreation	0.0444	0.0218	-0.0226**
			(0.00853)
Other service activities	0.0273	0.0282	0.0009
			(0.00967)
Total	100	100	
Observations	293	4955367	4955660

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*

NOTE: The table compares the distribution of sectors in firms with a high relative increase in the number of 4-month contracts after the reform with all other firms. The relative increase is measured by a difference-in-difference regression of the number of fixed-term 4-month contracts relative to fixed-term 6-month contracts before and after the reform, computed within each firm, with sector, month and year fixed-effects. We restricted to firms observed in the pre and post period, with at least one 4-month and one 6-month contract in each period. Having a high relative increase in the number of 4-month contracts is defined as having a within firm difference-in-difference coefficient above the 90^{th} percentile in the distribution of coefficients.

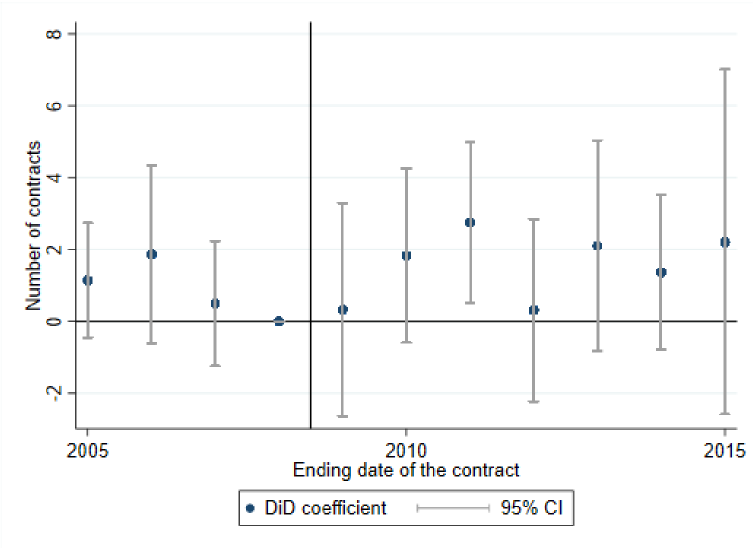
Figure A4: Difference in the share of each sector between firms with a high relative increase in 4-month contracts and all other firms (end of fixed-term contracts)



SOURCE: MMO

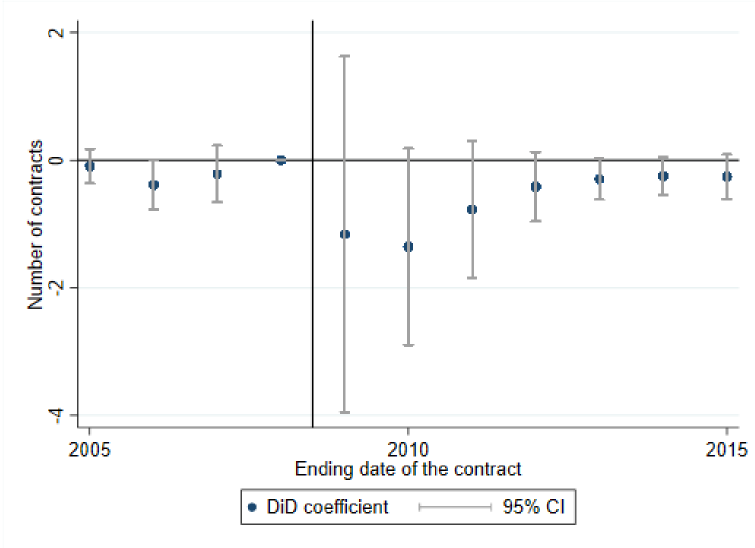
NOTE: This graph plots the difference in the share of each sector between firms with a high relative increase in 4-month contracts after the reform and all other firms. We keep only the positive points, i.e. sectors that are over-represented among high-increase firms. The relative increase in the number of 4-month contracts is measured by a difference-in-difference regression of the number of fixed-term 4-month contracts relative to fixed-term 6-month contracts before and after the reform, computed within each firm, with sector, month and year fixed-effects. We restricted to firms observed in the pre and post period, with at least one 4-month and one 6-month contract in each period. Having a high relative increase in the number of 4-month contracts is defined as having a within firm difference-in-difference coefficient above the 90^{th} percentile in the distribution of coefficients. Sectors in red are the ones identified as having a high separation rate or a high share of very short contracts (Decree No. 2019-797 of 26 July 2019 on the unemployment insurance scheme, DARES, 2018).

Figure A5: Yearly evolution of the number of 4-month contracts relative to 6-month contracts (economic layoffs)



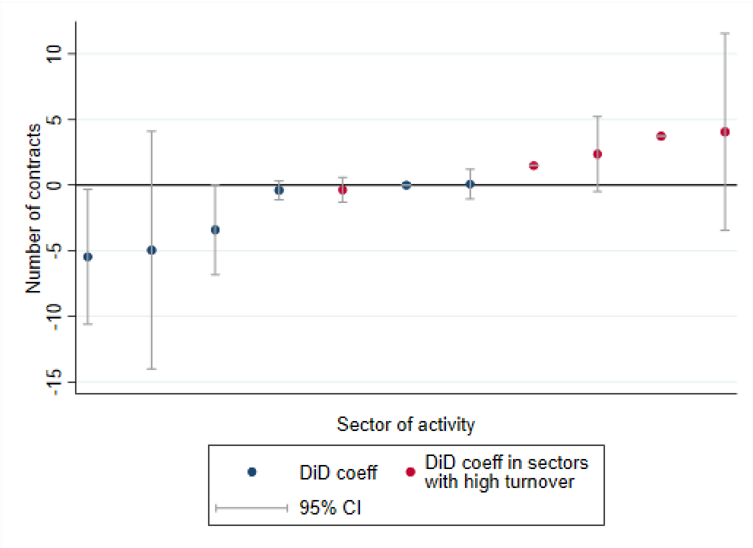
SOURCE: MMO
NOTE: This graph plots the yearly evolution in the number of 4-month contracts relative to 6-month contracts ending as an economic layoff, with month, year, sector and firm fixed-effects. The number of contracts has been computed at the firm × month level, and then aggregated at the national level. Standard errors are clustered at the firm level.

Figure A6: Yearly evolution of the number of 4-month contracts relative to 6-month contracts (personal dismissals)



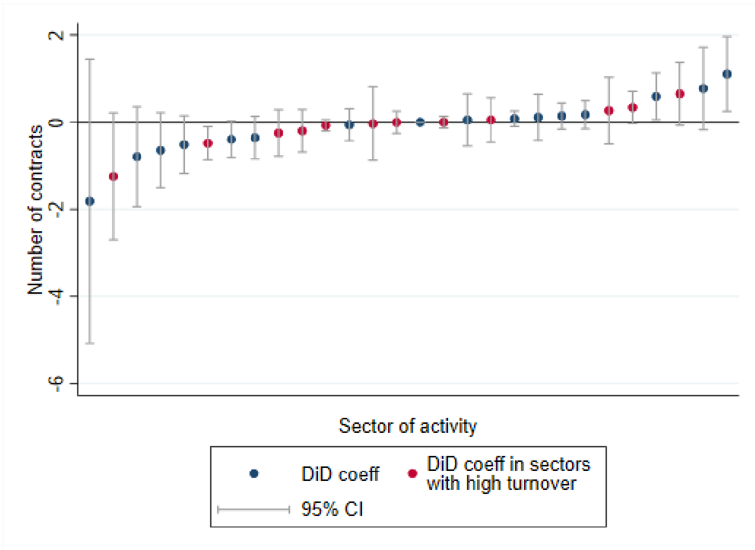
SOURCE: MMO
NOTE: This graph plots the yearly evolution in the number of 4-month contracts relative to 6-month contracts ending as a personal dismissal, with month, year, sector and firm fixed-effects. The reference year is 2008, the last pre-reform year. The number of contracts has been computed at the firm × month level, and then aggregated at the national level. Standard errors are clustered at the firm level.

Figure A7: Within sector change in the number of 4-month contracts relative to 6-month contracts (economic layoffs)



SOURCE: MMO
NOTE: This graph plots the difference-in-difference estimate of the change in the number of 4-month contracts relative to 6-month contracts ending as an economic layoff by sector. The number of contracts has been computed at the firm × month level, and then aggregated at the national level, with month, year, sector and firm fixed-effects. The regression has been run separately in each sector. Standard errors are clustered at the firm level. Not all 35 sectors are represented as the number of observations was not sufficient in some sectors. Sectors in red are the ones identified as having a high separation rate or a high share of very short contracts (Decree No. 2019-797 of 26 July 2019 on the unemployment insurance scheme, DARES, 2018).

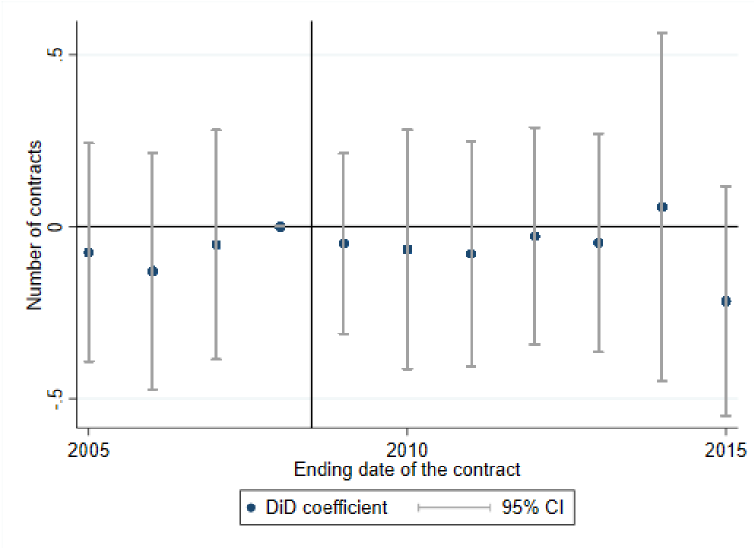
Figure A8: Within sector change in the number of 4-month contracts relative to 6-month contracts (personal dismissals)



SOURCE: MMO

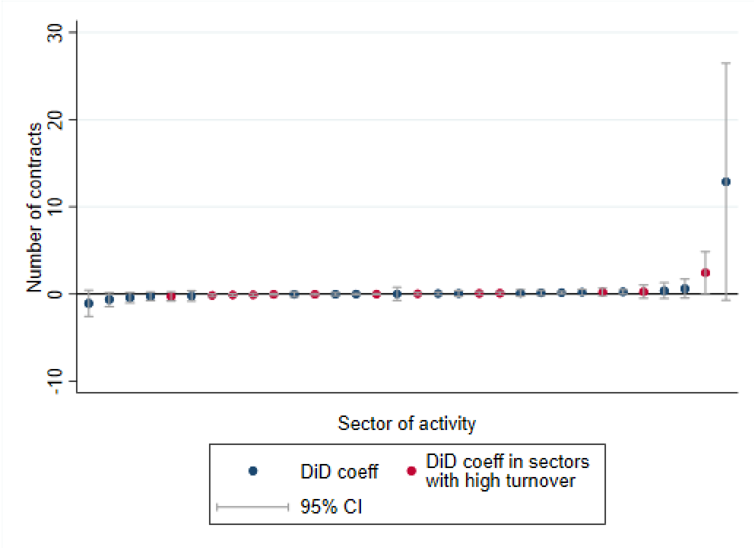
NOTE: This graph plots the difference-in-difference estimate of the change in the number of 4-month contracts relative to 6-month contracts ending as a personal dismissal by sector. The number of contracts has been computed at the firm × month level, and then aggregated at the national level, with month, year, sector and firm fixed-effects. The regression has been run separately in each sector. Standard errors are clustered at the firm level. Sectors in red are the ones identified as having a high separation rate or a high share of very short contracts (Decree No. 2019-797 of 26 July 2019 on the unemployment insurance scheme, DARES, 2018).

Figure A9: Yearly evolution of the number of 4-month contracts relative to 6-month contracts (resignations)



SOURCE: MMO
NOTE: This graph plots the yearly evolution in the number of 4-month contracts relative to 6-month contracts ending as a resignation, with month, year, sector and firm fixed-effects. The reference year is 2008, the last pre-reform year. The number of contracts has been computed at the firm × month level, and then aggregated at the national level. Standard errors are clustered at the firm level.

Figure A10: Within sector change in the number of 4-month contracts relative to 6-month contracts (resignations)



SOURCE: MMO
NOTE: This graph plots the difference-in-difference estimate of the change in the number of 4-month contracts relative to 6-month contracts ending as a resignation by sector. The number of contracts has been computed at the firm × month level, and then aggregated at the national level, with month, year, sector and firm fixed-effects. The regression has been run separately in each sector. Standard errors are clustered at the firm level. Sectors in red are the ones identified as having a high separation rate or a high share of very short contracts (Decree No. 2019-797 of 26 July 2019 on the unemployment insurance scheme, DARES, 2018).

Table A6: Difference-in-difference estimate of the number of 4-month contracts relative to 6-month contracts (Economic layoffs)

	Within firm monthly number of contracts			
4-month (versus 6m) contracts	0.30040	0.57754	0.42187	0.20505
	(0.310263)	(0.398772)	(0.308982)	(0.423318)
Post-reform	2.11316	-1.38976*	1.35330	-1.75507
	(2.188388)	(0.717226)	(1.997292)	(1.646634)
4-month (versus 6m) contracts × Post-reform	-1.57979	-1.60371***	-0.75299	0.26891
	(2.277347)	(0.518521)	(1.374629)	(0.562155)
Constant	1.94096***	3.86586***	2.08531**	7.22421***
	(0.174215)	(0.449624)	(0.914280)	(0.823544)
Observations	2563	2563	2563	904

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
SOURCE: MMO.
NOTE: The table shows difference-in-difference estimate of the number of 4-month contracts relative to 6-month contracts before and after the reform ending as an economic layoff, computed at the firm × month level and aggregated at the national level. The first column displays the raw regression, and month, year, sector and firm fixed-effects are progressively added. Standard errors are clustered at the sector level for the 3^{rd} specification, and at the firm level for the last one.

Table A7: Difference-in-difference estimate of the number of 4-month contracts relative to 6-month contracts (Personal dismissals)

	Within firm monthly number of contracts			
4-month (versus 6m) contracts	-0.11788	-0.12293***	-0.11791	0.10557
	(0.121538)	(0.045261)	(0.143453)	(0.083567)
Post-reform	0.31098	1.53765***	1.47727	1.46555
	(0.265350)	(0.109400)	(0.928783)	(1.014531)
4-month (versus 6m) contracts × Post-reform	-0.28015	-0.28728***	-0.29325	-0.55718*
	(0.279306)	(0.059218)	(0.398947)	(0.338505)
Constant	1.77571***	1.06971***	1.10386**	1.34979**
	(0.099820)	(0.066753)	(0.442160)	(0.540093)
Observations	54520	54520	54520	41251

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
SOURCE: MMO.
NOTE: The table shows difference-in-difference estimate of the number of 4-month contracts relative to 6-month contracts before and after the reform ending as a personal dismissal, computed at the firm × month level and aggregated at the national level. The first column displays the raw regression, and month, year, sector and firm fixed-effects are progressively added. Standard errors are clustered at the sector level for the 3^{rd} specification, and at the firm level for the last one.

Table A8: Difference-in-difference estimate of the number of 4-month contracts relative to 6-month contracts (End of trial period)

	Within firm monthly number of contracts			
4-month (versus 6m) contracts	0.00637	0.01186	0.03827	0.05307
	(0.048083)	(0.018094)	(0.044819)	(0.075234)
Post-reform	0.01134	-0.20961***	-0.16306	-0.26112
	(0.038568)	(0.039613)	(0.230984)	(0.305089)
4-month (versus 6m) contracts × Post-reform	-0.04266	-0.03998*	-0.01815	-0.05891
	(0.059314)	(0.022073)	(0.057726)	(0.083605)
Constant	1.64216***	1.79143***	1.73302***	1.96070***
	(0.023687)	(0.028345)	(0.140446)	(0.191079)
Observations	41258	41258	41258	28982

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
SOURCE: MMO.
NOTE: The table shows difference-in-difference estimate of the number of 4-month contracts relative to 6-month contracts before and after the reform ending during the trial period, computed at the firm × month level and aggregated at the national level. The first column displays the raw regression, and month, year, sector and firm fixed-effects are progressively added. Standard errors are clustered at the sector level for the 3^{rd} specification, and at the firm level for the last one.

Table A9: Difference-in-difference estimate of the number of 4-month contracts relative to 6-month contracts (Voluntary resignation)

	Within firm monthly number of contracts			
4-month (versus 6m) contracts	0.37476**	0.33881***	0.24660	0.06009
	(0.155234)	(0.026209)	(0.206972)	(0.044961)
Post-reform	-0.02598	-1.10184***	-0.97563	-1.69439
	(0.040873)	(0.067227)	(1.235164)	(1.618877)
4-month (versus 6m) contracts ×Post-reform	-0.28319*	-0.24381***	-0.18222	0.01393
	(0.165990)	(0.034037)	(0.176098)	(0.061971)
Constant	1.62898***	2.26422***	2.22125***	2.91428***
	(0.027401)	(0.041236)	(0.671168)	(0.931143)
Observations	168664	168664	168664	142416

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
SOURCE: MMO.
NOTE: The table shows difference-in-difference estimate of the number of 4-month contracts relative to 6-month contracts before and after the reform ending as a resignation, computed at the firm × month level and aggregated at the national level. The first column displays the raw regression, and month, year, sector and firm fixed-effects are progressively added. Standard errors are clustered at the sector level for the 3^{rd} specification, and at the firm level for the last one.

Table A10: Difference-in-difference estimate of the number of 4-month contracts relative to 6-month contracts (Retirement)

	Within firm monthly number of contracts			
4-month (versus 6m) contracts	-0.17310	-0.07902	-0.05755	-0.11393
	(0.131355)	(0.061660)	(0.096664)	(0.137914)
Post-reform	-0.14139	-0.31085	-0.31163*	-0.66726*
	(0.129628)	(0.205566)	(0.157997)	(0.370642)
4-month (versus 6m) contracts × Post-reform	0.29047	0.04742	-0.00482	0.16945
	(0.238324)	(0.080463)	(0.182940)	(0.194480)
Constant	1.71333***	1.83491***	1.83975***	2.27758***
	(0.075188)	(0.122873)	(0.085550)	(0.199458)
Observations	3614	3614	3614	1214

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
SOURCE: MMO.
NOTE: The table shows difference-in-difference estimate of the number of 4-month contracts relative to 6-month contracts before and after the reform ending as a retirement, computed at the firm × month level and aggregated at the national level. The first column displays the raw regression, and month, year, sector and firm fixed-effects are progressively added. Standard errors are clustered at the sector level for the 3^{rd} specification, and at the firm level for the last one.

Table A11: Difference-in-difference estimate of the number of 4-month contracts relative to 6-month contracts (Pre-retirement)

	Within firm monthly number of contracts		
4-month (versus 6m) contracts	3.09464*	1.45730***	-0.24507
	(1.684594)	(0.525373)	(0.435111)
Post-reform	-0.14827	-0.87115	-0.85619*
	(0.228724)	(1.218646)	(0.429935)
4-month (versus 6m) contracts × Post-reform	-1.57978	-1.08176*	0.87408**
	(1.966535)	(0.574451)	(0.369557)
Constant	1.27309***	2.80715**	2.57142***
	(0.220178)	(1.070311)	(0.410223)
Observations	79	79	72

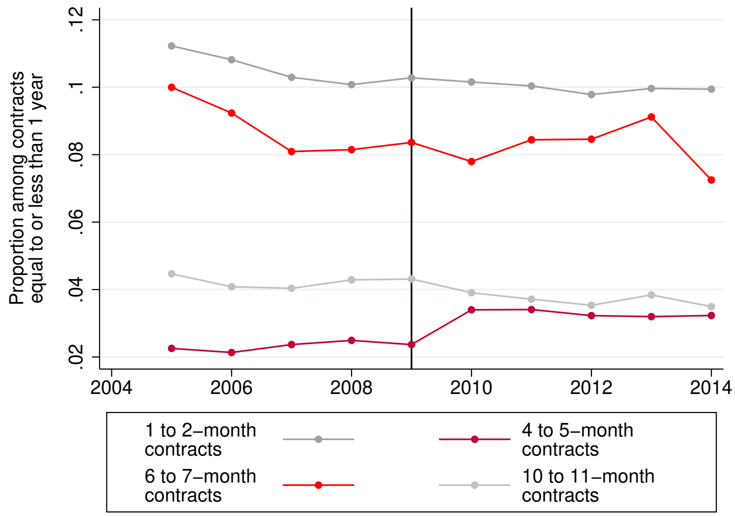
** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
SOURCE: MMO.
NOTE: The table shows difference-in-difference estimate of the number of 4-month contracts relative to 6-month contracts before and after the reform ending as a pre-retirement, computed at the firm × month level and aggregated at the national level. The first column displays the raw regression, and month, year and sector fixed-effects are progressively added. Firm fixed-effects cannot be added due to the small number of observations. Standard errors are clustered at the sector level for the 3^{rd} specification, and at the firm level for the last one.

Table A12: Post-reform evolution of 4 to 5-month contracts (6-month and 1 to 3-month control group)

	Proportion among contracts ≤1 year	Proportion among contracts ≤1 year
Post-reform	-0.00515***	-0.00500***
	(0.0000395)	(0.0000549)
4 to 5-month contracts	-0.06441***	-0.06436***
	(0.0000649)	(0.0001418)
Post-reform × 4 to 5-month contracts	0.01470***	0.01451***
	(0.0000811)	(0.0001751)
Constant	0.08778***	0.08758***
	(0.0000298)	(0.0000414)
Observations	122867	299939

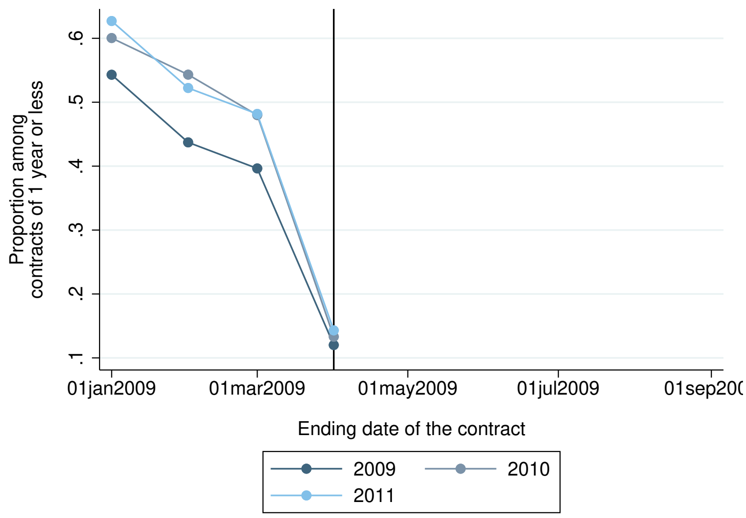
** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
SOURCE: UI data (FNA). The data comes from UI records, and gathers all contracts that were immediately followed by a UI right. To keep the same sample definition throughout the years, we excluded workers who registered with a work history between four and six months as they would not be registered in UI data before the reform.
NOTE: The first specification shows the difference in the evolution of the shares of 4-month and 6-month contracts. The second specification compares 4-month contracts to contracts between 1 and 3 months. Shares have been computed among contracts of one year and less. All types of separation are considered.

Figure A11: Share of contracts among contracts of 1 year or less (all separations)



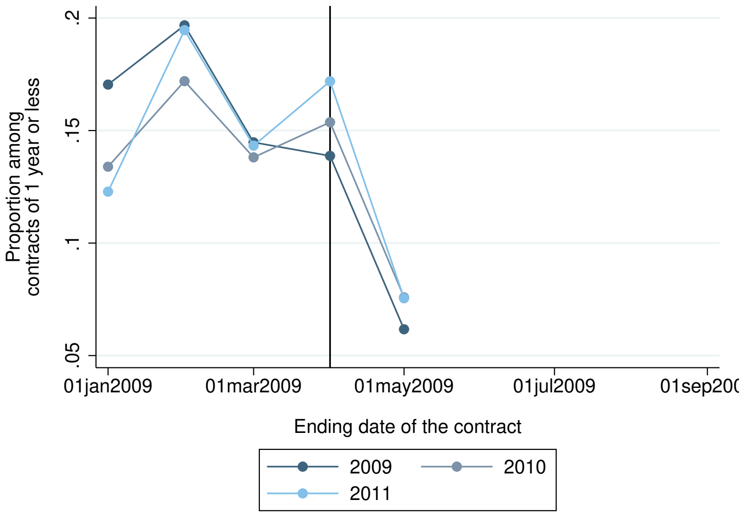
SOURCE: UI data (FNA).
NOTE: This figure plots the share of contracts of respectively one, four, six, and ten-month duration among contracts of one year and less, for all types of separations, between 2005 and 2014 in France. The vertical line corresponds to the 2009 reform that shortened the UI eligibility criterion in terms of employment record from four months to six months. The data comes from UI records, and gathers all contracts that were immediately followed by a UI right. To keep the same sample definition throughout the years, we excluded workers who registered with a work history between four and six months as they would not be registered in UI data before the reform.

Figure A12: Share of 1 to 30-day contracts among contracts of 1 year or less



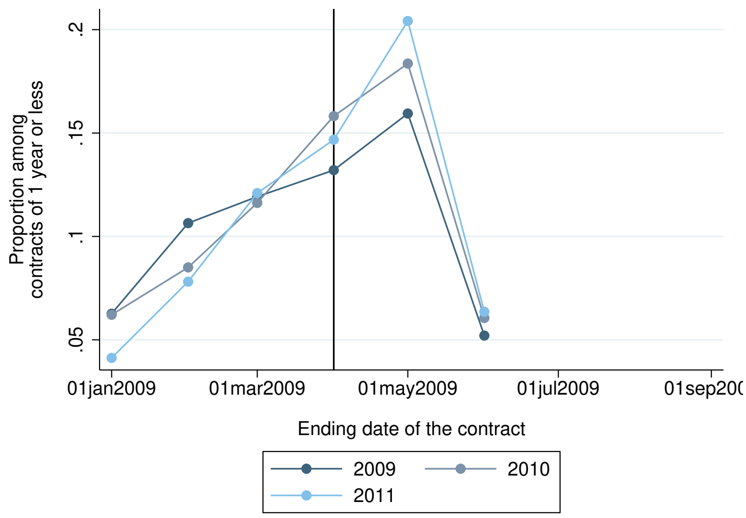
SOURCE: UI data (FNA).
NOTE: This graph plots the share of contracts between 1 and 30 days that have started before April, 2009, 2010 and 2011 respectively, and ended after that time threshold, among all contracts of one year and less. The objective is to test the hypothesis that there was not an increase in the share of 1 to 30-day contract signed before the reform but ending after, as an anticipation of the reform. The fact that the three lines look similar is evidence that such anticipation did not occur.

Figure A13: Share of 31 to 60-day contracts among contracts of 1 year or less



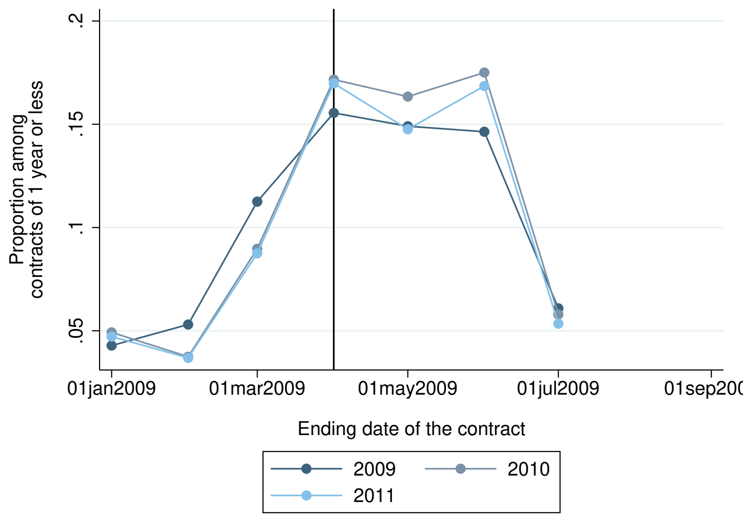
SOURCE: UI data (FNA).
NOTE: This graph plots the share of contracts between 31 and 60 days that have started before April, 2009, 2010 and 2011 respectively, and ended after that time threshold, among all contracts of one year and less. The objective is to test the hypothesis that there was not an increase in the share of 31 to 60-day contract signed before the reform but ending after, as an anticipation of the reform. The fact that the three lines look similar is evidence that such anticipation did not occur.

Figure A14: Share of 61 to 90-day contracts among contracts of 1 year or less



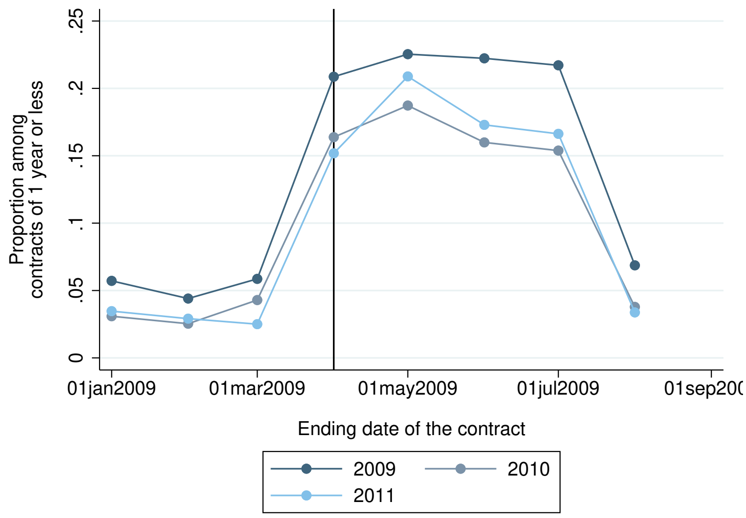
SOURCE: UI data (FNA).
NOTE: This graph plots the share of contracts between 61 and 90 days that have started before April, 2009, 2010 and 2011 respectively, and ended after that time threshold, among all contracts of one year and less. The objective is to test the hypothesis that there was not an increase in the share of 61 to 90-day contract signed before the reform but ending after, as an anticipation of the reform. The fact that the three lines look similar is evidence that such anticipation did not occur.

Figure A15: Share of 91 to 120-day contracts among contracts of 1 year or less



SOURCE: UI data (FNA).
NOTE: This graph plots the share of contracts between 91 and 120 days that have started before April, 2009, 2010 and 2011 respectively, and ended after that time threshold, among all contracts of one year and less. The objective is to test the hypothesis that there was not an increase in the share of 91 to 120-day contract signed before the reform but ending after, as an anticipation of the reform. The fact that the three lines look similar is evidence that such anticipation did not occur.

Figure A16: Share of 121 to 150-days contracts among contracts of 1 year or less



SOURCE: UI data (FNA).
NOTE: This graph plots the share of contracts between 121 and 150 days that have started before April, 2009, 2010 and 2011 respectively, and ended after that time threshold, among all contracts of one year and less. The objective is to test the hypothesis that there was not an increase in the share of 121 to 150-day contract signed before the reform but ending after, as an anticipation of the reform. The fact that the three lines look similar is evidence that such anticipation did not occur.

Table A13: Descriptive statistics on treated and control workers

	Treated	Control	Difference (2)-(1)
Gender	0.58	0.59	0.01
			(0.008)
Level of education	4.25	4.18	-0.07
			(0.070)
Daily wage	44.66	53.28	8.61***
			(1.254)
Fulltime	0.48	0.56	0.08***
			(0.008)
Establishment size	96.59	79.11	-17.48*
			(8.145)
Observations	5401	18158	23559

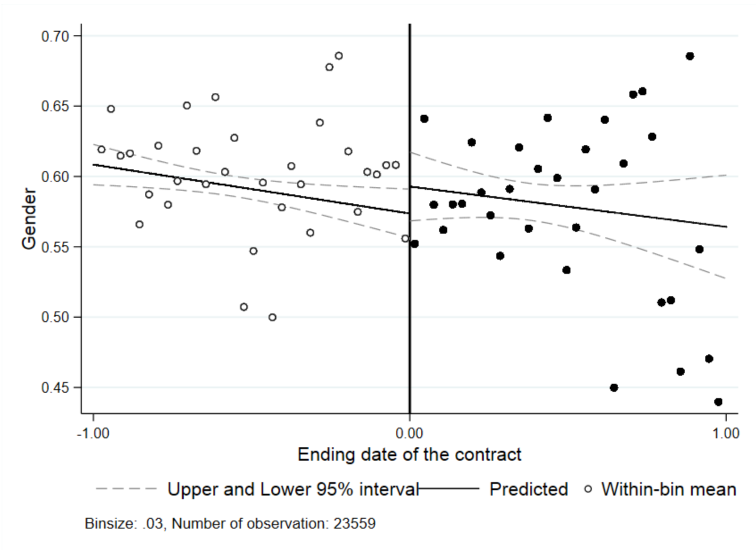
* *p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
SOURCE: FH-DADS.
NOTE: This table compares the observable characteristics of treated and control workers. Treated workers are defined as workers ending a fixed-term contract after the reform that has started before the reform, and with a work history between four and six months. Control workers are defined the same way, except that they end their contract before the reform. In total, they account for 23,559 observations. The reform, enacted in April, 1^{st}, 2009, has reduced the employment record condition from six months over the last twenty-two months to four months over the last twenty-eight months. The time period considered corresponds to October, 2008-September, 2009. Control workers have, on average, a higher daily wage, and work in smaller firms.

Table A14: Descriptive statistics on takers and non-takers

	Takers	Treated non takers	Difference
Gender	0.57	0.58	0.02
			(0.020)
Level of education	4.36	4.23	-0.13
			(0.171)
Daily wage	62.30	41.92	-20.38***
			(1.422)
Fulltime	0.76	0.44	-0.32***
			(0.019)
Establishment size	99.58	96.12	-3.46
			(24.431)
Work history over the last 28 months	157.15	152.32	-4.83***
			(0.821)
Observations	727	4674	5401

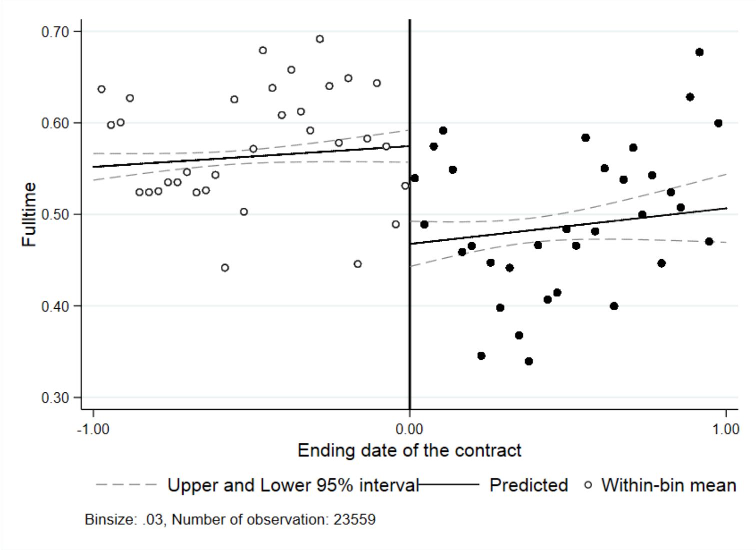
** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The two columns compare the characteristics of treated workers eligible to UI benefits and deciding to claim or not to claim them. Treated workers are defined as workers ending a fixed-term contract after the reform that has started before the reform, and with a work history between four and six months. In total, they account for 5,401 observations. The reform, enacted in April, 1^{st}, 2009, has reduced the employment record condition from six months over the last twenty-two months to four months over the last twenty-eight months. The time period considered corresponds to October, 2008-September, 2009. Takers have, on average, a higher daily wage, and work more hours.

Figure A17: Distribution of the female proportion with respect to contract ending date



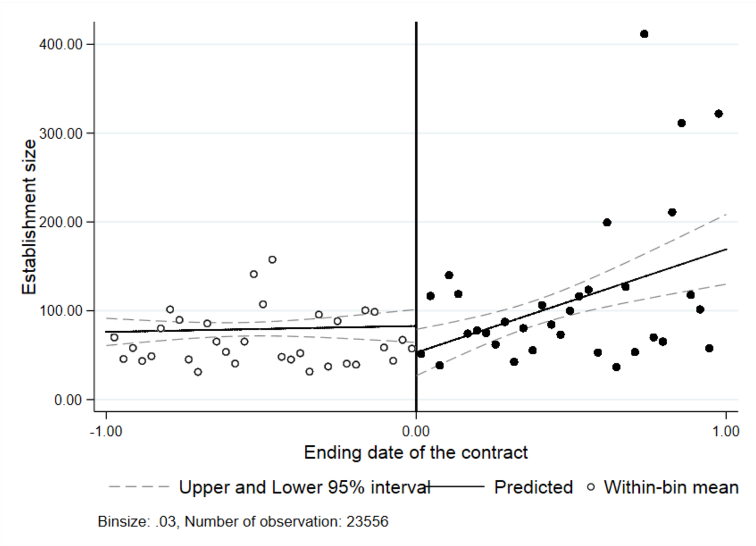
SOURCE: FH-DADS.
NOTE: This figure shows the distribution of the proportion of female workers with respect to the contract ending date. The vertical line corresponds to the reform. It shows that there is no significant discontinuity at the threshold, confirming that workers are statistically similar at both sides of the threshold.

Figure A18: Distribution of the proportion of fulltime workers with respect to contract ending date



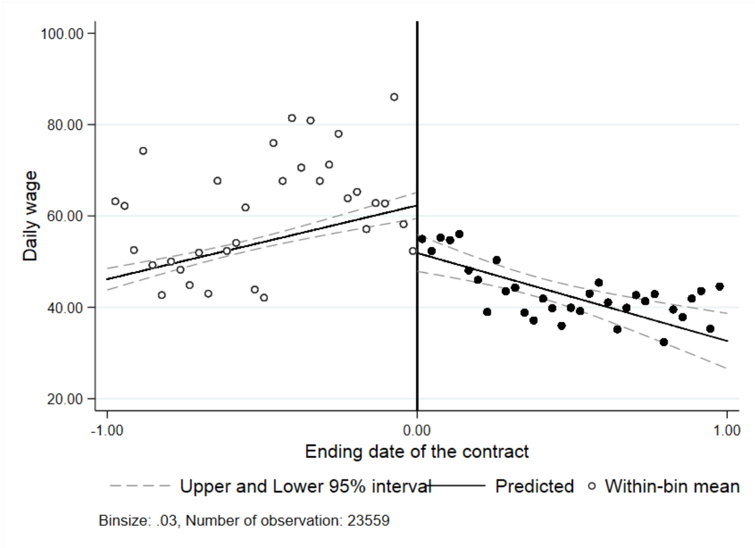
SOURCE: FH-DADS.
NOTE: This figure shows the distribution of the proportion of full-time workers with respect to the contract ending date. The vertical line corresponds to the reform. There is a discontinuity at the threshold, but it seems that it is rather driven by the non-linearity of the relationship between the proportion of full-time workers and the ending date of the contract. Table 5 shows that the more demanding RDD regression on the full-time variable does not yield a significant coefficient, confirming that workers are statistically similar at both sides of the threshold.

Figure A19: Distribution of the establishment size with respect to contract ending date



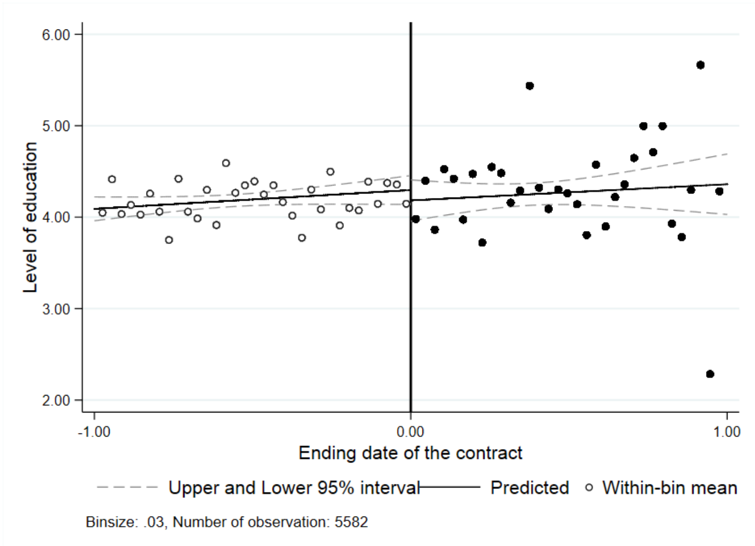
SOURCE: FH-DADS.
NOTE: This figure shows the distribution of the establishment size with respect to the contract ending date. The vertical line corresponds to the reform. It shows that there is no significant discontinuity at the threshold, confirming that workers are statistically similar at both sides of the threshold.

Figure A20: Distribution of the daily wage with respect to contract ending date



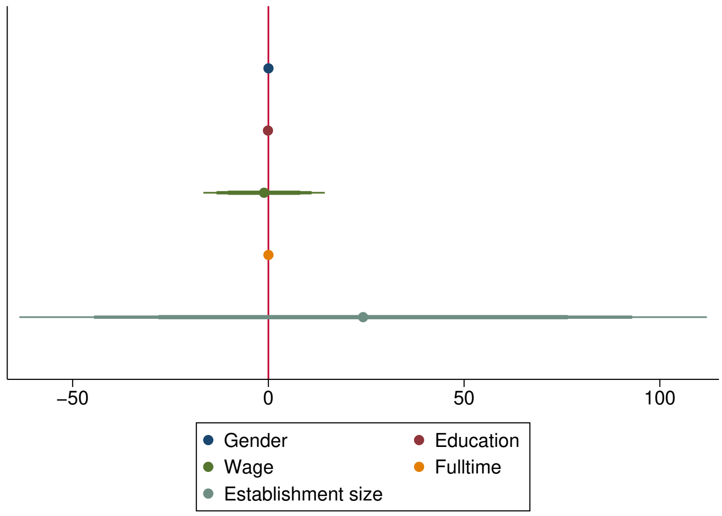
SOURCE: FH-DADS.
NOTE: This figure shows the distribution of the wage with respect to the contract ending date. The vertical line corresponds to the reform. It shows that there is no significant discontinuity at the threshold, confirming that workers are statistically similar at both sides of the threshold.

Figure A21: Distribution of the education level with respect to contract ending date



SOURCE: FH-DADS.
NOTE: This figure shows the distribution of the education level with respect to the contract ending date. The vertical line corresponds to the reform. It shows that there is no significant discontinuity at the threshold, confirming that workers are statistically similar at both sides of the threshold.

Figure A22: Magnitude of the difference in covariates at the cutoff



SOURCE: FH-DADS.
NOTE: This graph shows the difference in the distribution of several covariates between workers located at each side of the threshold in its close neighborhood. Coefficients and standard errors have been obtained from the RDD estimates using each covariate as the dependent variable. The three levels of significance of the confidence intervals depicted are 5%, 1% and 0.1%. It shows that none of the tested variables significantly differ from one side of the threshold to the other in a discontinuous way, ensuring that there is no sorting of workers at the threshold.

Table A15: Impact of UI benefits receipt on employment probability (1-4 months) – Quadratic specification

	Probability of being employed 1 months after	Probability of being employed 2 months after	Probability of being employed 3 months after	Probability of being employed 4 months after
RD_Estimate	0.326	0.681	-0.589	-0.742
	(0.687)	(0.774)	(0.548)	(0.663)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Quadratic specification. The dependent variable corresponds to the probability of being employed at different time horizons after the end of the contract that defines the treatment status.

Table A16: Impact of UI benefits receipt on employment probability (5-8 months) – Quadratic specification

	Probability of being employed 5 months after	Probability of being employed 6 months after	Probability of being employed 7 months after	Probability of being employed 8 months after
RD_Estimate	-0.710	-0.795	-1.068*	-1.337**
	(0.662)	(0.708)	(0.612)	(0.654)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Quadratic specification. The dependent variable corresponds to the probability of being employed at different time horizons after the end of the contract that defines the treatment status.

Table A17: Impact of UI benefits receipt on employment probability (9-12 months) – Quadratic specification

	Probability of being employed 9 months after	Probability of being employed 10 months after	Probability of being employed 11 months after	Probability of being employed 12 months after
RD_Estimate	-0.481	-1.361*	-1.198*	-0.653
	(0.564)	(0.707)	(0.667)	(0.653)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Quadratic specification. The dependent variable corresponds to the probability of being employed at different time horizons after the end of the contract that defines the treatment status.

Table A18: Impact of UI benefits receipt on employment probability (13-16 months) – Quadratic specification

	Probability of being employed 13 months after	Probability of being employed 14 months after	Probability of being employed 15 months after	Probability of being employed 16 months after
RD_Estimate	-0.665 (0.632)	-0.663 (0.654)	-0.826 (0.656)	-0.819 (0.616)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Quadratic specification. The dependent variable corresponds to the probability of being employed at different time horizons after the end of the contract that defines the treatment status.

Table A19: Impact of UI benefits receipt on employment probability (17-20 months) – Quadratic specification

	Probability of being employed 17 months after	Probability of being employed 18 months after	Probability of being employed 19 months after	Probability of being employed 20 months after
RD_Estimate	-0.766 (0.663)	-0.929 (0.703)	-1.064 (0.732)	-1.128 (0.749)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Quadratic specification. The dependent variable corresponds to the probability of being employed at different time horizons after the end of the contract that defines the treatment status.

Table A20: Impact of UI benefits receipt on employment probability (21-24 months) – Quadratic specification

	Probability of being employed 21 months after	Probability of being employed 22 months after	Probability of being employed 23 months after	Probability of being employed 24 months after
RD_Estimate	-1.510** (0.719)	-0.145 (0.583)	0.244 (0.646)	0.490 (0.682)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Quadratic specification. The dependent variable corresponds to the probability of being employed at different time horizons after the end of the contract that defines the treatment status.

Table A21: Impact of UI benefits receipt on employment probability (1-4 months)

	Probability of being employed 1 months after	Probability of being employed 2 months after	Probability of being employed 3 months after	Probability of being employed 4 months after
RD_Estimate	0.023 (0.556)	0.019 (0.495)	-0.536 (0.426)	-0.902* (0.488)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of being employed at different time horizons after the end of the contract that defines the treatment status.

Table A22: Impact of UI benefits receipt on employment probability (5-8 months)

	Probability of being employed 5 months after	Probability of being employed 6 months after	Probability of being employed 7 months after	Probability of being employed 8 months after
RD_Estimate	-0.895* (0.492)	-1.068** (0.494)	-1.020** (0.469)	-1.141*** (0.342)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of being employed at different time horizons after the end of the contract that defines the treatment status.

Table A23: Impact of UI benefits receipt on employment probability (9-12 months)

	Probability of being employed 9 months after	Probability of being employed 10 months after	Probability of being employed 11 months after	Probability of being employed 12 months after
RD_Estimate	-0.376	-0.949**	-0.978**	-0.695
	(0.381)	(0.453)	(0.455)	(0.438)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of being employed at different time horizons after the end of the contract that defines the treatment status.

Table A24: Impact of UI benefits receipt on employment probability (13-16 months)

	Probability of being employed 13 months after	Probability of being employed 14 months after	Probability of being employed 15 months after	Probability of being employed 16 months after
RD_Estimate	-0.641	-0.719	-0.684	-0.772*
	(0.453)	(0.446)	(0.435)	(0.438)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of being employed at different time horizons after the end of the contract that defines the treatment status.

Table A25: Impact of UI benefits receipt on employment probability (17-20 months)

	Probability of being employed 17 months after	Probability of being employed 18 months after	Probability of being employed 19 months after	Probability of being employed 20 months after
RD_Estimate	-0.779*	-0.979**	-0.940**	-1.064**
	(0.461)	(0.480)	(0.453)	(0.486)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of being employed at different time horizons after the end of the contract that defines the treatment status.

Table A26: Impact of UI benefits receipt on employment probability (21-24 months)

	Probability of being employed 21 months after	Probability of being employed 22 months after	Probability of being employed 23 months after	Probability of being employed 24 months after
RD_Estimate	-1.108**	0.069	0.079	0.048
	(0.444)	(0.437)	(0.428)	(0.424)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of being employed at different time horizons after the end of the contract that defines the treatment status.

Table A27: Impact of UI receipt on employment probability (1-4 months) - Bivariate probit

	Probability of being employed 1 month after	Probability of being employed 2 months after	Probability of being employed 3 months after	Probability of being employed 4 months after
Being registered to UI	0.282	0.350	-0.370	-0.608***
	(0.849)	(0.416)	(0.410)	(0.144)
Ending date of the contract	-0.737***	-0.498***	-0.048	-0.116
	(0.121)	(0.089)	(0.146)	(0.090)
Contract ending date × Treated	0.201	0.385	0.634*	0.610**
	(1.268)	(0.858)	(0.356)	(0.267)
Observations	3189	3913	4693	4150

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The bivariate probit has been computed on the sample restricted to a window around the threshold defined by the same MSE-optimal bandwidth selector as for the RDD, and using triangular kernel weights. Linear specification. The dependent variable corresponds to the probability of being employed at different time horizons after the end of the contract that defines the treatment status.

Table A28: Impact of UI receipt on employment probability (5-8 months) - Bivariate probit

	Probability of being employed 5 months after	Probability of being employed 6 months after	Probability of being employed 7 months after	Probability of being employed 8 months after
Being registered to UI	-0.524**	-0.614***	-0.639***	-0.619***
	(0.217)	(0.132)	(0.126)	(0.082)
Ending date of the contract	-0.290**	-0.229***	-0.183**	-0.132***
	(0.122)	(0.087)	(0.078)	(0.040)
Contract ending date × Treated	0.677**	0.570**	0.538**	0.546***
	(0.313)	(0.254)	(0.233)	(0.129)
Observations	4150	4384	4693	6833

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The bivariate probit has been computed on the sample restricted to a window around the threshold defined by the same MSE-optimal bandwidth selector as for the RDD, and using triangular kernel weights. Linear specification. The dependent variable corresponds to the probability of being employed at different time horizons after the end of the contract that defines the treatment status.

Table A29: Impact of UI receipt on employment probability (9-12 months) - Bivariate probit

	Probability of being employed 9 months after	Probability of being employed 10 months after	Probability of being employed 11 months after	Probability of being employed 12 months after
Being registered to UI	0.065	-0.437**	-0.496***	-0.336
	(1.102)	(0.177)	(0.178)	(0.301)
Ending date of the contract	-0.133	0.263***	0.175**	0.053
	(0.267)	(0.069)	(0.072)	(0.105)
Contract ending date × Treated	0.231	0.743***	0.760***	0.659**
	(0.848)	(0.227)	(0.216)	(0.266)
Observations	5388	5014	5014	4927

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The bivariate probit has been computed on the sample restricted to a window around the threshold defined by the same MSE-optimal bandwidth selector as for the RDD, and using triangular kernel weights. Linear specification. The dependent variable corresponds to the probability of being employed at different time horizons after the end of the contract that defines the treatment status.

Table A30: Impact of UI receipt on employment probability (13-16 months) - Bivariate probit

	Probability of being employed 13 months after	Probability of being employed 14 months after	Probability of being employed 15 months after	Probability of being employed 16 months after
Being registered to UI	-0.270	-0.305	-0.313	-0.352*
	(0.324)	(0.297)	(0.267)	(0.212)
Ending date of the contract	-0.064	-0.041	-0.028	-0.088
	(0.123)	(0.109)	(0.096)	(0.084)
Contract ending date × Treated	0.657**	0.415	0.300	0.387
	(0.312)	(0.293)	(0.274)	(0.248)
Observations	4499	4739	4927	4927

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The bivariate probit has been computed on the sample restricted to a window around the threshold defined by the same MSE-optimal bandwidth selector as for the RDD, and using triangular kernel weights. Linear specification. The dependent variable corresponds to the probability of being employed at different time horizons after the end of the contract that defines the treatment status.

Table A31: Impact of UI receipt on employment probability (17-20 months) - Bivariate probit

	Probability of being employed 17 months after	Probability of being employed 18 months after	Probability of being employed 19 months after	Probability of being employed 20 months after
Being registered to UI	-0.361	-0.535***	-0.542***	-0.532***
	(0.222)	(0.142)	(0.144)	(0.127)
Ending date of the contract	-0.209**	-0.155**	-0.127*	-0.206***
	(0.101)	(0.076)	(0.068)	(0.071)
Contract ending date × Treated	0.669**	0.609***	0.448**	0.649***
	(0.265)	(0.220)	(0.213)	(0.200)
Observations	4443	4499	5014	4869

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The bivariate probit has been computed on the sample restricted to a window around the threshold defined by the same MSE-optimal bandwidth selector as for the RDD, and using triangular kernel weights. Linear specification. The dependent variable corresponds to the probability of being employed at different time horizons after the end of the contract that defines the treatment status.

Table A32: Impact of UI receipt on employment probability (21-24 months) - Bivariate probit

	Probability of being employed 21 months after	Probability of being employed 22 months after	Probability of being employed 23 months after	Probability of being employed 24 months after
Being registered to UI	-0.622***	-0.038	0.107	0.025
	(0.085)	(0.375)	(0.372)	(0.369)
Ending date of the contract	0.023	0.186	-0.024	-0.045
	(0.051)	(0.130)	(0.129)	(0.126)
Contract ending date × Treated	0.621***	0.191	0.233	0.249
	(0.149)	(0.356)	(0.362)	(0.341)
Observations	5227	4499	4739	4739

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The bivariate probit has been computed on the sample restricted to a window around the threshold defined by the same MSE-optimal bandwidth selector as for the RDD, and using triangular kernel weights. Linear specification. The dependent variable corresponds to the probability of being employed at different time horizons after the end of the contract that defines the treatment status.

Table A33: Impact of UI benefits receipt on the duration to next job

	Duration until next job		
	Linear	Quadratic	Cubic
RD_Estimate	410.259*	586.974	750.566
	(238.507)	(417.329)	(546.979)
Observations	21291	21291	21291

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Quadratic specification. The dependent variable corresponds to the probability of being employed at different time horizons after the end of the contract that defines the treatment status.

Table A34: Impact of UI benefits receipt on daily earnings (1-4 months)

	Daily gross earnings							
	1 months after	2 months after	3 months after	4 months after	5 months after	6 months after	7 months after	8 months after
RD_Estimate	13.897	24.483	-31.527	-54.548	-49.795	-38.757	-39.499	-51.429
	(49.214)	(49.405)	(34.263)	(33.992)	(34.323)	(35.528)	(39.134)	(32.737)
Observations	23559	23559	23559	23559	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to daily earnings at different time horizons after the end of the contract that defines the treatment status. Workers not employed at the considered date are included in the regression with earnings equal to zero

Table A35: Impact of UI benefits receipt on daily earnings (5-8 months)

	Daily gross earnings							
	9 months after	10 months after	11 months after	12 months after	13 months after	14 months after	15 months after	16 months after
RD_Estimate	-12.723	-52.716*	-62.717**	-40.706	-19.179	-46.364	-70.580**	-72.175**
	(28.145)	(31.908)	(30.623)	(32.084)	(37.119)	(37.597)	(35.968)	(35.160)
Observations	23559	23559	23559	23559	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to daily earnings at different time horizons after the end of the contract that defines the treatment status. Workers not employed at the considered date are included in the regression with earnings equal to zero

Table A36: Impact of UI benefits receipt on daily earnings (9-12 months)

	Daily gross earnings							
	17 months after	18 months after	19 months after	20 months after	21 months after	22 months after	23 months after	24 months after
RD_Estimate	-65.922*	-68.088*	-47.716	-66.245*	-80.080**	-17.829	-26.430	-40.600
	(36.111)	(36.053)	(38.250)	(35.396)	(33.910)	(31.402)	(32.600)	(35.723)
Observations	23559	23559	23559	23559	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to daily earnings at different time horizons after the end of the contract that defines the treatment status. Workers not employed at the considered date are included in the regression with earnings equal to zero

Table A37: Impact of UI benefit receipt on the probability to have a permanent contract

	Permanent contract		
	Linear	Quadratic	Cubic
RD_Estimate	0.287	0.364	0.623
	(0.359)	(0.513)	(0.849)
Observations	20898	20898	20898

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear, quadratic and cubic specifications. The dependent variable corresponds to the probability that the contract following the end of the contract that defines the treatment status is permanent. Workers who do not find a job by the end of the observed period are treated as missing. The probability is then conditional on having found a job by the end of the observed period

Table A38: Impact of UI benefit receipt on the probability to work full-time

	Full-time job		
	Linear	Quadratic	Cubic
RD_Estimate	-0.330	-1.034	-1.673
	(0.410)	(0.714)	(1.104)
Observations	21243	21243	21243

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear, quadratic and cubic specifications. The dependent variable corresponds to the probability that the contract following the end of the contract that defines the treatment status is full-time. Workers who do not find a job by the end of the observed period are treated as missing. The probability is then conditional on having found a job by the end of the observed period

Table A39: Impact of UI benefit receipt on the probability to work in the same 2-digit industry

	Same industry		
	Linear	Quadratic	Cubic
RD_Estimate	-0.578	0.284	0.072
	(0.501)	(1.128)	(1.079)
Observations	21180	21180	21180

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear, quadratic and cubic specifications. The dependent variable corresponds to the probability that the contract following the end of the contract that defines the treatment status is in the same 2-digit industry as the previous one. Workers who do not find a job by the end of the observed period are treated as missing. The probability is then conditional on having found a job by the end of the observed period

Table A40: Impact of UI benefit receipt on the duration of the contract

	Duration of the next employment spell		
	Linear	Quadratic	Cubic
RD_Estimate	-244.861 (269.854)	-373.511 (376.109)	-631.482 (599.540)
Observations	21291	21291	21291

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear, quadratic and cubic specifications. The dependent variable corresponds to the probability that the contract following the end of the contract that defines the treatment status is permanent. Workers who do not find a job by the end of the observed period are treated as missing. The probability is then conditional on having found a job by the end of the observed period

Table A41: Impact of UI benefits receipt on probability of having a permanent contract (1-4 months)

	Probability of having a permanent contract 1 months after	Probability of having a permanent contract 2 months after	Probability of having a permanent contract 3 months after	Probability of having a permanent contract 4 months after
RD_Estimate	-0.094 (0.309)	0.108 (0.341)	-0.118 (0.313)	-0.254 (0.331)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of working under a permanent contract at different time horizons after the end of the contract that defines the treatment status. The probability of having a permanent contract is unconditional, in the sense that it is set to zero if the person is not employed.

Table A42: Impact of UI benefits receipt on probability of having a permanent contract (5-8 months)

	Probability of having a permanent contract 5 months after	Probability of having a permanent contract 6 months after	Probability of having a permanent contract 7 months after	Probability of having a permanent contract 8 months after
RD_Estimate	-0.376	-0.385	-0.274	-0.500*
	(0.333)	(0.310)	(0.317)	(0.289)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of working under a permanent contract at different time horizons after the end of the contract that defines the treatment status. The probability of having a permanent contract is unconditional, in the sense that it is set to zero if the person is not employed.

Table A43: Impact of UI benefits receipt on probability of having a permanent contract (9-12 months)

	Probability of having a permanent contract 9 months after	Probability of having a permanent contract 10 months after	Probability of having a permanent contract 11 months after	Probability of having a permanent contract 12 months after
RD_Estimate	-0.254	-0.679**	-0.533	-0.469
	(0.303)	(0.339)	(0.343)	(0.320)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of working under a permanent contract at different time horizons after the end of the contract that defines the treatment status. The probability of having a permanent contract is unconditional, in the sense that it is set to zero if the person is not employed.

Table A44: Impact of UI benefits receipt on probability of having a permanent contract (13-16 months)

	Probability of having a permanent contract 13 months after	Probability of having a permanent contract 14 months after	Probability of having a permanent contract 15 months after	Probability of having a permanent contract 16 months after
RD_Estimate	-0.499	-0.509	-0.694**	-0.667*
	(0.366)	(0.336)	(0.343)	(0.346)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of working under a permanent contract at different time horizons after the end of the contract that defines the treatment status. The probability of having a permanent contract is unconditional, in the sense that it is set to zero if the person is not employed.

Table A45: Impact of UI benefits receipt on probability of having a permanent contract (17-20 months)

	Probability of having a permanent contract 17 months after	Probability of having a permanent contract 18 months after	Probability of having a permanent contract 19 months after	Probability of having a permanent contract 20 months after
RD_Estimate	-0.408	-0.448	-0.476	-0.754**
	(0.337)	(0.333)	(0.343)	(0.362)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of working under a permanent contract at different time horizons after the end of the contract that defines the treatment status. The probability of having a permanent contract is unconditional, in the sense that it is set to zero if the person is not employed.

Table A46: Impact of UI benefits receipt on probability of having a permanent contract (21-24 months)

	Probability of having a permanent contract 21 months after	Probability of having a permanent contract 22 months after	Probability of having a permanent contract 23 months after	Probability of having a permanent contract 24 months after
RD_Estimate	-0.243	-0.412	-0.486	-0.616
	(0.203)	(0.358)	(0.357)	(0.395)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of working under a permanent contract at different time horizons after the end of the contract that defines the treatment status. The probability of having a permanent contract is unconditional, in the sense that it is set to zero if the person is not employed.

Table A47: Impact of UI benefits receipt on the probability of working in the same 2-digit industry (1-4 months)

	Probability of being in the same 2-digit industry 1 months after	Probability of being in the same 2-digit industry 2 months after	Probability of being in the same 2-digit industry 3 months after	Probability of being in the same 2-digit industry 4 months after
RD_Estimate	-0.255	-0.500	-0.565	-0.729*
	(0.379)	(0.380)	(0.378)	(0.375)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of working in the same 2-digit industry at different time horizons after the end of the contract that defines the treatment status. The probability of working in the same 2-digit industry is unconditional, in the sense that it is set to zero if the person is not employed.

Table A48: Impact of UI benefits receipt on the probability of working in the same 2-digit industry (5-8 months)

	Probability of being in the same 2-digit industry 5 months after	Probability of being in the same 2-digit industry 6 months after	Probability of being in the same 2-digit industry 7 months after	Probability of being in the same 2-digit industry 8 months after
RD_Estimate	-0.727**	-0.724**	-0.418	-0.535
	(0.352)	(0.346)	(0.337)	(0.339)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of working in the same 2-digit industry at different time horizons after the end of the contract that defines the treatment status. The probability of working in the same 2-digit industry is unconditional, in the sense that it is set to zero if the person is not employed.

Table A49: Impact of UI benefits receipt on the probability of working in the same 2-digit industry (9-12 months)

	Probability of being in the same 2-digit industry 9 months after	Probability of being in the same 2-digit industry 10 months after	Probability of being in the same 2-digit industry 11 months after	Probability of being in the same 2-digit industry 12 months after
RD_Estimate	-0.360	-0.247	0.040	0.108
	(0.301)	(0.309)	(0.350)	(0.361)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of working in the same 2-digit industry at different time horizons after the end of the contract that defines the treatment status. The probability of working in the same 2-digit industry is unconditional, in the sense that it is set to zero if the person is not employed.

Table A50: Impact of UI benefits receipt on the probability of working in the same 2-digit industry (13-16 months)

	Probability of being in the same 2-digit industry 13 months after	Probability of being in the same 2-digit industry 14 months after	Probability of being in the same 2-digit industry 15 months after	Probability of being in the same 2-digit industry 16 months after
RD_Estimate	-0.270	-0.117	-0.032	-0.070
	(0.354)	(0.358)	(0.344)	(0.347)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of working in the same 2-digit industry at different time horizons after the end of the contract that defines the treatment status. The probability of working in the same 2-digit industry is unconditional, in the sense that it is set to zero if the person is not employed.

Table A51: Impact of UI benefits receipt on the probability of working in the same 2-digit industry (17-20 months)

	Probability of being in the same 2-digit industry 17 months after	Probability of being in the same 2-digit industry 18 months after	Probability of being in the same 2-digit industry 19 months after	Probability of being in the same 2-digit industry 20 months after
RD_Estimate	-0.078	-0.303	-0.525	-0.329
	(0.345)	(0.354)	(0.347)	(0.362)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of working in the same 2-digit industry at different time horizons after the end of the contract that defines the treatment status. The probability of working in the same 2-digit industry is unconditional, in the sense that it is set to zero if the person is not employed.

Table A52: Impact of UI benefits receipt on the probability of working in the same 2-digit industry (21-24 months)

	Probability of being in the same 2-digit industry 21 months after	Probability of being in the same 2-digit industry 22 months after	Probability of being in the same 2-digit industry 23 months after	Probability of being in the same 2-digit industry 24 months after
RD_Estimate	-0.552*	0.146	0.299	0.505
	(0.335)	(0.291)	(0.405)	(0.424)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of working in the same 2-digit industry at different time horizons after the end of the contract that defines the treatment status. The probability of working in the same 2-digit industry is unconditional, in the sense that it is set to zero if the person is not employed.

Table A53: Impact of UI benefits receipt on the probability of working full-time (1-4 months)

	Probability of having a full-time job 1 months after	Probability of having a full-time job 2 months after	Probability of having a full-time job 3 months after	Probability of having a full-time job 4 months after
RD_Estimate	-0.129	-0.016	0.027	-0.567
	(0.384)	(0.370)	(0.377)	(0.387)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of working full-time at different time horizons after the end of the contract that defines the treatment status. The probability of working full-time is unconditional, in the sense that it is set to zero if the person is not employed.

Table A54: Impact of UI benefits receipt on the probability of working full-time (5-8 months)

	Probability of having a full-time job 5 months after	Probability of having a full-time job 6 months after	Probability of having a full-time job 7 months after	Probability of having a full-time job 8 months after
RD_Estimate	-0.541	-0.759*	-0.728*	-0.947**
	(0.397)	(0.409)	(0.389)	(0.383)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of working full-time at different time horizons after the end of the contract that defines the treatment status. The probability of working full-time is unconditional, in the sense that it is set to zero if the person is not employed.

Table A55: Impact of UI benefits receipt on the probability of working full-time (9-12 months)

	Probability of having a full-time job 9 months after	Probability of having a full-time job 10 months after	Probability of having a full-time job 11 months after	Probability of having a full-time job 12 months after
RD_Estimate	-0.355	-0.224	-0.279	-0.209
	(0.376)	(0.331)	(0.328)	(0.337)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of working full-time at different time horizons after the end of the contract that defines the treatment status. The probability of working full-time is unconditional, in the sense that it is set to zero if the person is not employed.

Table A56: Impact of UI benefits receipt on the probability of working full-time (13-16 months)

	Probability of having a full-time job 13 months after	Probability of having a full-time job 14 months after	Probability of having a full-time job 15 months after	Probability of having a full-time job 16 months after
RD_Estimate	-0.083	-0.318	-0.517	-0.159
	(0.354)	(0.411)	(0.426)	(0.373)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of working full-time at different time horizons after the end of the contract that defines the treatment status. The probability of working full-time is unconditional, in the sense that it is set to zero if the person is not employed.

Table A57: Impact of UI benefits receipt on the probability of working full-time (17-20 months)

	Probability of having a full-time job 17 months after	Probability of having a full-time job 18 months after	Probability of having a full-time job 19 months after	Probability of having a full-time job 20 months after
RD_Estimate	-0.249	-0.453	-0.253	-0.312
	(0.418)	(0.434)	(0.395)	(0.274)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of working full-time at different time horizons after the end of the contract that defines the treatment status. The probability of working full-time is unconditional, in the sense that it is set to zero if the person is not employed.

Table A58: Impact of UI benefits receipt on the probability of working full-time (21-24 months)

	Probability of having a full-time job 21 months after	Probability of having a full-time job 22 months after	Probability of having a full-time job 23 months after	Probability of having a full-time job 24 months after
RD_Estimate	-0.566	0.004	0.129	0.030
	(0.402)	(0.413)	(0.393)	(0.373)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of working full-time at different time horizons after the end of the contract that defines the treatment status. The probability of working full-time is unconditional, in the sense that it is set to zero if the person is not employed.

Table A59: Impact of UI benefits receipt on contract duration (1-4 months)

	Duration of the employment spell 1 months after	Duration of the employment spell 2 months after	Duration of the employment spell 3 months after	Duration of the employment spell 4 months after
RD_Estimate	-390.527	-504.597*	-576.516*	-672.300*
	(351.933)	(293.801)	(336.191)	(362.457)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the duration of the employment spell at different time horizons after the end of the contract that defines the treatment status. The contract duration is unconditional, in the sense that it is set to zero if the person is not employed.

Table A60: Impact of UI benefits receipt on contract duration (5-8 months)

	Duration of the employment spell 5 months after	Duration of the employment spell 6 months after	Duration of the employment spell 7 months after	Duration of the employment spell 8 months after
RD_Estimate	-707.630*	-821.084**	-668.136*	-876.174**
	(366.177)	(355.791)	(359.051)	(383.444)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the duration of the employment spell at different time horizons after the end of the contract that defines the treatment status. The contract duration is unconditional, in the sense that it is set to zero if the person is not employed.

Table A61: Impact of UI benefits receipt on contract duration (9-12 months)

	Duration of the employment spell 9 months after	Duration of the employment spell 10 months after	Duration of the employment spell 11 months after	Duration of the employment spell 12 months after
RD_Estimate	-463.129	-746.594*	-658.973*	-634.606*
	(305.095)	(403.763)	(388.678)	(374.647)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the duration of the employment spell at different time horizons after the end of the contract that defines the treatment status. The contract duration is unconditional, in the sense that it is set to zero if the person is not employed.

Table A62: Impact of UI benefits receipt on contract duration (13-16 months)

	Duration of the employment spell 13 months after	Duration of the employment spell 14 months after	Duration of the employment spell 15 months after	Duration of the employment spell 16 months after
RD_Estimate	-609.118	-618.413*	-732.174*	-786.672**
	(395.255)	(375.210)	(378.387)	(383.681)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the duration of the employment spell at different time horizons after the end of the contract that defines the treatment status. The contract duration is unconditional, in the sense that it is set to zero if the person is not employed.

Table A63: Impact of UI benefits receipt on contract duration (17-20 months)

	Duration of the employment spell 17 months after	Duration of the employment spell 18 months after	Duration of the employment spell 19 months after	Duration of the employment spell 20 months after
RD_Estimate	-731.153*	-733.036*	-745.598**	-839.462**
	(380.131)	(379.900)	(370.343)	(392.360)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the duration of the employment spell at different time horizons after the end of the contract that defines the treatment status. The contract duration is unconditional, in the sense that it is set to zero if the person is not employed.

Table A64: Impact of UI benefits receipt on contract duration (21-24 months)

	Duration of the employment spell 21 months after	Duration of the employment spell 22 months after	Duration of the employment spell 23 months after	Duration of the employment spell 24 months after
RD_Estimate	-830.318**	-342.242	-347.639	-375.121
	(359.420)	(351.752)	(372.287)	(359.088)
Observations	23559	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the duration of the employment spell at different time horizons after the end of the contract that defines the treatment status. The contract duration is unconditional, in the sense that it is set to zero if the person is not employed.

Table A65: Bounds on the treatment effect on the probability of having a permanent contract (1-4 months)

	Probability to have a permanent contract 1 months after	Probability to have a permanent contract 2 months after	Probability to have a permanent contract 3 months after	Probability to have a permanent contract 4 months after
lower	-0.018	-0.001	-0.028	-0.053**
	(0.026)	(0.024)	(0.023)	(0.022)
upper	0.159***	0.113***	0.068*	0.126***
	(0.042)	(0.037)	(0.035)	(0.035)
Effect ci lower bound	-0.0610	-0.0402	-0.0659	-0.0900
Effect ci upper bound	0.2283	0.1746	0.1267	0.1838
Observations	4104	3913	3639	4150

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of working under a permanent contract at different time horizons after the end of the contract that defines the treatment status. The method developed by Lee (2009) allows to derive bounds on the treatment effect taking into account the selection into employment. The implementation has been done using the package "leebounds" developed by Tauchmann (2013) on Stata.

Table A66: Bounds on the treatment effect on the probability of having a permanent contract (5-8 months)

	Probability to have a permanent contract 5 months after	Probability to have a permanent contract 6 months after	Probability to have a permanent contract 7 months after	Probability to have a permanent contract 8 months after
lower	-0.052**	-0.047*	-0.045*	-0.064**
	(0.023)	(0.025)	(0.026)	(0.028)
upper	0.161***	0.198***	0.196***	0.194***
	(0.035)	(0.037)	(0.038)	(0.040)
Effect ci lower bound	-0.0899	-0.0882	-0.0878	-0.1106
Effect ci upper bound	0.2187	0.2594	0.2576	0.2588
Observations	4384	4443	4443	4332

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of working under a permanent contract at different time horizons after the end of the contract that defines the treatment status. The method developed by Lee (2009) allows to derive bounds on the treatment effect taking into account the selection into employment. The implementation has been done using the package "leebounds" developed by Tauchmann (2013) on Stata.

Table A67: Bounds on the treatment effect on the probability of having a permanent contract (9-12 months)

	Probability to have a permanent contract 9 months after	Probability to have a permanent contract 10 months after	Probability to have a permanent contract 11 months after	Probability to have a permanent contract 12 months after
lower	0.004	-0.049*	-0.047*	-0.044
	(0.024)	(0.026)	(0.026)	(0.027)
upper	0.052	-0.017	0.015	0.027
	(0.032)	(0.031)	(0.031)	(0.033)
Effect ci lower bound	-0.0357	-0.0932	-0.0889	-0.0891
Effect ci upper bound	0.1050	0.0345	0.0657	0.0813
Observations	4739	4150	4150	3639

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of working under a permanent contract at different time horizons after the end of the contract that defines the treatment status. The method developed by Lee (2009) allows to derive bounds on the treatment effect taking into account the selection into employment. The implementation has been done using the package "leebounds" developed by Tauchmann (2013) on Stata.

Table A68: Bounds on the treatment effect on the probability of having a permanent contract (13-16 months)

	Probability to have a permanent contract 13 months after	Probability to have a permanent contract 14 months after	Probability to have a permanent contract 15 months after	Probability to have a permanent contract 16 months after
lower	-0.045	-0.039	-0.059**	-0.058**
	(0.029)	(0.023)	(0.024)	(0.023)
upper	0.038	0.038	0.016	0.042
	(0.035)	(0.029)	(0.028)	(0.028)
Effect ci lower bound	-0.0928	-0.0772	-0.0982	-0.0946
Effect ci upper bound	0.0966	0.0854	0.0624	0.0877
Observations	3102	4332	4150	4332

* *p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of working under a permanent contract at different time horizons after the end of the contract that defines the treatment status. The method developed by Lee (2009) allows to derive bounds on the treatment effect taking into account the selection into employment. The implementation has been done using the package "leebounds" developed by Tauchmann (2013) on Stata.

Table A69: Bounds on the treatment effect on the probabilityof having a permanent contract (17-20 months)

	Probability to have a permanent contract 17 months after	Probability to have a permanent contract 18 months after	Probability to have a permanent contract 19 months after	Probability to have a permanent contract 20 months after
lower	-0.033	-0.032	-0.032	-0.065**
	(0.023)	(0.024)	(0.024)	(0.026)
upper	0.094***	0.119***	0.122***	0.125***
	(0.029)	(0.030)	(0.029)	(0.030)
Effect ci lower bound	-0.0714	-0.0721	-0.0714	-0.1073
Effect ci upper bound	0.1414	0.1676	0.1692	0.1747
Observations	4332	4443	4869	4739

* *p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of working under a permanent contract at different time horizons after the end of the contract that defines the treatment status. The method developed by Lee (2009) allows to derive bounds on the treatment effect taking into account the selection into employment. The implementation has been done using the package "leebounds" developed by Tauchmann (2013) on Stata.

Table A70: Bounds on the treatment effect on the probability of having a permanent contract (21-24 months)

	Probability to have a permanent contract 21 months after	Probability to have a permanent contract 22 months after	Probability to have a permanent contract 23 months after	Probability to have a permanent contract 24 months after
lower	-0.030	-0.071***	-0.052*	-0.054*
	(0.027)	(0.027)	(0.028)	(0.028)
upper	0.081***	-0.012	-0.039	-0.048*
	(0.030)	(0.025)	(0.026)	(0.029)
Effect ci lower bound	-0.0743	-0.1149	-0.1014	-0.1067
Effect ci upper bound	0.1308	0.0298	0.0061	0.0057
Observations	4332	4629	3959	3276

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of working under a permanent contract at different time horizons after the end of the contract that defines the treatment status. The method developed by Lee (2009) allows to derive bounds on the treatment effect taking into account the selection into employment. The implementation has been done using the package "leebounds" developed by Tauchmann (2013) on Stata.

Table A71: Bounds on the treatment effect on the probability of working in the same 2-digit industry (1-4 months)

	Probability of being in the same 2-digit industry 1 month after	Probability of being in the same 2-digit industry 2 months after	Probability of being in the same 2-digit industry 3 months after	Probability of being in the same 2-digit industry 4 months after
lower	-0.088***	-0.090***	-0.082***	-0.096***
	(0.031)	(0.029)	(0.027)	(0.027)
upper	0.059*	0.018	0.003	0.058*
	(0.035)	(0.031)	(0.030)	(0.031)
Effect ci lower bound	-0.1395	-0.1378	-0.1264	-0.1397
Effect ci upper bound	0.1169	0.0700	0.0527	0.1088
Observations	5054	4927	4693	5054

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of working in the same 2-digit industry at different time horizons after the end of the contract that defines the treatment status. The method developed by Lee (2009) allows to derive bounds on the treatment effect taking into account the selection into employment. The implementation has been done using the package "leebounds" developed by Tauchmann (2013) on Stata.

Table A72: Bounds on the treatment effect on the probability of working in the same 2-digit industry (5-8 months)

	Probability of being in the same 2-digit industry 5 months after	Probability of being in the same 2-digit industry 6 months after	Probability of being in the same 2-digit industry 7 months after	Probability of being in the same 2-digit industry 8 months after
lower	-0.112***	-0.138***	-0.138***	-0.118***
	(0.028)	(0.028)	(0.028)	(0.029)
upper	0.078**	0.080**	0.072**	0.101***
	(0.033)	(0.033)	(0.034)	(0.036)
Effect ci lower bound	-0.1577	-0.1845	-0.1847	-0.1652
Effect ci upper bound	0.1320	0.1344	0.1285	0.1603
Observations	4869	5227	5054	4927

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to theprobability of working in the same 2-digit industry at different time horizons after the end of the contract that defines the treatment status. The method developed by Lee (2009) allows to derive bounds on the treatment effect taking into account the selection into employment. The implementation has been done using the package "leebounds" developed by Tauchmann (2013) on Stata.

Table A73: Bounds on the treatment effect on the probability of working in the same 2-digit industry (9-12 months)

	Probability of being in the same 2-digit industry 9 months after	Probability of being in the same 2-digit industry 10 months after	Probability of being in the same 2-digit industry 11 months after	Probability of being in the same 2-digit industry 12 months after
lower	-0.000	0.003	0.017	-0.002
	(0.024)	(0.028)	(0.029)	(0.031)
upper	0.003	0.012	0.020	0.019
	(0.030)	(0.033)	(0.025)	(0.036)
Effect ci lower bound	-0.0463	-0.0478	-0.0389	-0.0554
Effect ci upper bound	0.0604	0.0740	0.0677	0.0827
Observations	5227	3913	4693	3189

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of working in the same 2-digit industry at different time horizons after the end of the contract that defines the treatment status. The method developed by Lee (2009) allows to derive bounds on the treatment effect taking into account the selection into employment. The implementation has been done using the package "leebounds" developed by Tauchmann (2013) on Stata.

Table A74: Bounds on the treatment effect on the probability of working in the same 2-digit industry (13-16 months)

	Probability of being in the same 2-digit industry 13 months after	Probability of being in the same 2-digit industry 14 months after	Probability of being in the same 2-digit industry 15 months after	Probability of being in the same 2-digit industry 16 months after
lower	-0.042*	-0.030	-0.019	-0.025
	(0.025)	(0.025)	(0.026)	(0.025)
upper	0.001	0.010	0.010	0.022
	(0.029)	(0.029)	(0.032)	(0.030)
Effect ci lower bound	-0.0832	-0.0708	-0.0640	-0.0669
Effect ci upper bound	0.0484	0.0580	0.0638	0.0718
Observations	4693	4384	3639	3835

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of working in the same 2-digit industry at different time horizons after the end of the contract that defines the treatment status. The method developed by Lee (2009) allows to derive bounds on the treatment effect taking into account the selection into employment. The implementation has been done using the package "leebounds" developed by Tauchmann (2013) on Stata.

Table A75: Bounds on the treatment effect on the probability of working in the same 2-digit industry (17-20 months)

	Probability of being in the same 2-digit industry 17 months after	Probability of being in the same 2-digit industry 18 months after	Probability of being in the same 2-digit industry 19 months after	Probability of being in the same 2-digit industry 20 months after
lower	-0.028	-0.048*	-0.067***	-0.071***
	(0.026)	(0.026)	(0.025)	(0.027)
upper	0.056*	0.075**	0.065**	0.082**
	(0.031)	(0.032)	(0.030)	(0.034)
Effect ci lower bound	-0.0699	-0.0914	-0.1071	-0.1156
Effect ci upper bound	0.1073	0.1269	0.1142	0.1384
Observations	3959	4150	4869	4104

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of working in the same 2-digit industry at different time horizons after the end of the contract that defines the treatment status. The method developed by Lee (2009) allows to derive bounds on the treatment effect taking into account the selection into employment. The implementation has been done using the package "leebounds" developed by Tauchmann (2013) on Stata.

Table A76: Bounds on the treatment effect on the probability of working in the same 2-digit industry (21-24 months)

	Probability of being in the same 2-digit industry 21 months after	Probability of being in the same 2-digit industry 22 months after	Probability of being in the same 2-digit industry 23 months after	Probability of being in the same 2-digit industry 24 months after
lower	-0.021	-0.016	-0.015	0.006
	(0.024)	(0.032)	(0.031)	(0.037)
upper	0.032	0.050*	0.024	0.028
	(0.030)	(0.027)	(0.025)	(0.031)
Effect ci lower bound	-0.0607	-0.0687	-0.0659	-0.0579
Effect ci upper bound	0.0821	0.0938	0.0655	0.0817
Observations	4629	3835	3913	2615

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of working in the same 2-digit industry at different time horizons after the end of the contract that defines the treatment status. The method developed by Lee (2009) allows to derive bounds on the treatment effect taking into account the selection into employment. The implementation has been done using the package "leebounds" developed by Tauchmann (2013) on Stata.

Table A77: Bounds on the treatment effect on the probability of working full-time (1-4 months)

	Probability to work full-time 1 month after	Probability to work full-time 2 months after	Probability to work full-time 3 months after	Probability to work full-time 4 months after
lower	-0.038	-0.008	0.000	-0.082**
	(0.037)	(0.043)	(0.038)	(0.036)
upper	0.142***	0.069*	0.094***	0.098***
	(0.038)	(0.040)	(0.034)	(0.032)
Effect ci lower bound	-0.0989	-0.0780	-0.0621	-0.1405
Effect ci upper bound	0.2049	0.1348	0.1507	0.1514
Observations	3639	2253	2665	3276

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of working full-time at different time horizons after the end of the contract that defines the treatment status. The method developed by Lee (2009) allows to derive bounds on the treatment effect taking into account the selection into employment. The implementation has been done using the package "leebounds" developed by Tauchmann (2013) on Stata.

Table A78: Bounds on the treatment effect on the probability of working full-time (5-8 months)

	Probability to work full-time 5 months after	Probability to work full-time 6 months after	Probability to work full-time 7 months after	Probability to work full-time 8 months after
lower	-0.085**	-0.120***	-0.120***	-0.135***
	(0.038)	(0.034)	(0.033)	(0.036)
upper	0.110***	0.125***	0.123***	0.123***
	(0.034)	(0.031)	(0.029)	(0.032)
Effect ci lower bound	-0.1475	-0.1759	-0.1733	-0.1952
Effect ci upper bound	0.1658	0.1757	0.1716	0.1760
Observations	3102	4332	4869	4332

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to theprobability of working full-time at different time horizons after the end of the contract that defines the treatment status. The method developed by Lee (2009) allows to derive bounds on the treatment effect taking into account the selection into employment. The implementation has been done using the package "leebounds" developed by Tauchmann (2013) on Stata.

Table A79: Bounds on the treatment effect on the probability of working full-time (9-12 months)

	Probability to work full-time 9 months after	Probability to work full-time 10 months after	Probability to work full-time 11 months after	Probability to work full-time 12 months after
lower	-0.005	0.028	0.027	0.009
	(0.031)	(0.029)	(0.023)	(0.027)
upper	0.043	0.041	0.031	0.048**
	(0.027)	(0.025)	(0.020)	(0.024)
Effect ci lower bound	-0.0560	-0.0239	-0.0152	-0.0366
Effect ci upper bound	0.0877	0.0861	0.0686	0.0879
Observations	4629	4499	6833	4739

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of working full-time at different time horizons after the end of the contract that defines the treatment status. The method developed by Lee (2009) allows to derive bounds on the treatment effect taking into account the selection into employment. The implementation has been done using the package "leebounds" developed by Tauchmann (2013) on Stata.

Table A80: Bounds on the treatment effect on the probability of working full-time (13-16 months)

	Probability to work full-time 13 months after	Probability to work full-time 14 months after	Probability to work full-time 15 months after	Probability to work full-time 16 months after
lower	0.004	0.003	-0.011	-0.016
	(0.026)	(0.022)	(0.028)	(0.021)
upper	0.066***	0.065***	0.065***	0.080***
	(0.023)	(0.020)	(0.024)	(0.018)
Effect ci lower bound	-0.0387	-0.0335	-0.0572	-0.0504
Effect ci upper bound	0.1035	0.0974	0.1052	0.1103
Observations	5054	6521	4104	6923

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of working full-time at different time horizons after the end of the contract that defines the treatment status. The method developed by Lee (2009) allows to derive bounds on the treatment effect taking into account the selection into employment. The implementation has been done using the package "leebounds" developed by Tauchmann (2013) on Stata.

Table A81: Bounds on the treatment effect on the probability of working full-time (17-20 months)

	Probability to work full-time 17 months after	Probability to work full-time 18 months after	Probability to work full-time 19 months after	Probability to work full-time 20 months after
lower	-0.046**	-0.063**	-0.063***	-0.074***
	(0.022)	(0.026)	(0.025)	(0.024)
upper	0.080***	0.086***	0.098***	0.119***
	(0.019)	(0.022)	(0.021)	(0.020)
Effect ci lower bound	-0.0829	-0.1060	-0.1036	-0.1138
Effect ci upper bound	0.1108	0.1221	0.1320	0.1521
Observations	6765	5442	6421	7329

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of working full-time at different time horizons after the end of the contract that defines the treatment status. The method developed by Lee (2009) allows to derive bounds on the treatment effect taking into account the selection into employment. The implementation has been done using the package "leebounds" developed by Tauchmann (2013) on Stata.

Table A82: Bounds on the treatment effect on the probability of working full-time (21-24 months)

	Probability to work full-time 21 months after	Probability to work full-time 22 months after	Probability to work full-time 23 months after	Probability to work full-time 24 months after
lower	-0.030	-0.013	0.019	0.008
	(0.023)	(0.022)	(0.027)	(0.025)
upper	0.061***	0.050*	0.033	0.013
	(0.019)	(0.027)	(0.032)	(0.021)
Effect ci lower bound	-0.0678	-0.0497	-0.0285	-0.0392
Effect ci upper bound	0.0928	0.0948	0.0908	0.0527
Observations	7197	5014	3189	4693

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of working full-time at different time horizons after the end of the contract that defines the treatment status. The method developed by Lee (2009) allows to derive bounds on the treatment effect taking into account the selection into employment. The implementation has been done using the package "leebounds" developed by Tauchmann (2013) on Stata.

Table A83: Bounds on the treatment effect on contract duration (1-4 months)

	Duration of the employment spell 1 months after	Duration of the employment spell 2 months after	Duration of the employment spell 3 months after	Duration of the employment spell 4 months after
lower	-89.458***	-75.050***	-91.003***	-103.098***
	(30.005)	(26.080)	(31.850)	(25.164)
upper	162.700***	104.299***	55.467	138.637***
	(28.829)	(38.229)	(50.010)	(24.803)
Effect ci lower bound	-138.8119	-117.9474	-143.3921	-144.4899
Effect ci upper bound	210.1185	167.1804	137.7273	179.4340
Observations	3959	4150	2442	3959

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the duration of the employment spell at different time horizons after the end of the contract that defines the treatment status. The method developed by Lee (2009) allows to derive bounds on the treatment effect taking into account the selection into employment. The implementation has been done using the package "leebounds" developed by Tauchmann (2013) on Stata.

Table A84: Bounds on the treatment effect on contract duration (5-8 months)

	Duration of the employment spell 5 months after	Duration of the employment spell 6 months after	Duration of the employment spell 7 months after	Duration of the employment spell 8 months after
lower	-83.174***	-89.186***	-98.983***	-112.802***
	(24.848)	(26.193)	(28.496)	(30.591)
upper	184.910***	209.690***	205.383***	215.224***
	(22.579)	(23.212)	(26.525)	(28.095)
Effect ci lower bound	-124.0459	-132.2693	-145.8545	-163.1204
Effect ci upper bound	222.0498	247.8710	249.0132	261.4365
Observations	4384	4499	4150	4150

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the duration of the employment spell at different time horizons after the end of the contract that defines the treatment status. The method developed by Lee (2009) allows to derive bounds on the treatment effect taking into account the selection into employment. The implementation has been done using the package "leebounds" developed by Tauchmann (2013) on Stata.

Table A85: Bounds on the treatment effect on contract duration (9-12 months)

	Duration of the employment spell 9 months after	Duration of the employment spell 10 months after	Duration of the employment spell 11 months after	Duration of the employment spell 12 months after
lower	-9.781	-85.947**	-85.320***	-69.013**
	(27.884)	(38.153)	(33.072)	(28.244)
upper	84.087**	68.302	68.487*	46.002
	(37.421)	(46.045)	(39.703)	(34.663)
Effect ci lower bound	-55.6459	-148.7036	-139.7188	-115.4708
Effect ci upper bound	145.6388	144.0382	133.7918	103.0172
Observations	4869	2567	3102	3835

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the duration of the employment spell at different time horizons after the end of the contract that defines the treatment status. The method developed by Lee (2009) allows to derive bounds on the treatment effect taking into account the selection into employment. The implementation has been done using the package "leebounds" developed by Tauchmann (2013) on Stata.

Table A86: Bounds on the treatment effect on contract duration (13-16 months)

	Duration of the employment spell 13 months after	Duration of the employment spell 14 months after	Duration of the employment spell 15 months after	Duration of the employment spell 16 months after
lower	-74.305***	-67.056***	-78.091***	-79.384***
	(26.415)	(25.145)	(25.091)	(24.245)
upper	54.469	65.758**	49.305	75.626***
	(33.193)	(31.019)	(31.098)	(28.424)
Effect ci lower bound	-117.7535	-108.4169	-119.3618	-119.2634
Effect ci upper bound	109.0664	116.7791	100.4571	122.3803
Observations	3913	4057	3835	3959

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the duration of the employment spell at different time horizons after the end of the contract that defines the treatment status. The method developed by Lee (2009) allows to derive bounds on the treatment effect taking into account the selection into employment. The implementation has been done using the package "leebounds" developed by Tauchmann (2013) on Stata.

Table A87: Bounds on the treatment effect on contract duration (17-20 months)

	Duration of the employment spell 17 months after	Duration of the employment spell 18 months after	Duration of the employment spell 19 months after	Duration of the employment spell 20 months after
lower	-71.239***	-73.267***	-84.344***	-89.722***
	(23.405)	(24.636)	(25.884)	(26.183)
upper	109.733***	133.107***	125.101***	156.466***
	(25.886)	(25.590)	(26.532)	(26.705)
Effect ci lower bound	-109.7366	-113.7895	-126.9184	-132.7897
Effect ci upper bound	152.3111	175.1985	168.7424	200.3920
Observations	4332	4195	4104	4384

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the duration of the employment spell at different time horizons after the end of the contract that defines the treatment status. The method developed by Lee (2009) allows to derive bounds on the treatment effect taking into account the selection into employment. The implementation has been done using the package "leebounds" developed by Tauchmann (2013) on Stata.

Table A88: Bounds on the treatment effect on contract duration (21-24 months)

	Duration of the employment spell 21 months after	Duration of the employment spell 22 months after	Duration of the employment spell 23 months after	Duration of the employment spell 24 months after
lower	-71.541**	-94.057***	-62.264*	-39.740
	(28.791)	(34.426)	(31.821)	(25.143)
upper	105.512***	-27.733	-37.908	-18.249
	(30.310)	(29.994)	(25.357)	(56.198)
Effect ci lower bound	-118.8980	-150.7517	-116.7682	-85.0931
Effect ci upper bound	155.3676	21.6636	5.5259	83.1241
Observations	4104	3189	3959	4104

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*

NOTE: The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the duration of the employment spell at different time horizons after the end of the contract that defines the treatment status. The method developed by Lee (2009) allows to derive bounds on the treatment effect taking into account the selection into employment. The implementation has been done using the package "leebounds" developed by Tauchmann (2013) on Stata.

Table A89: Impact of UI benefit receipt on the number of employment spells over the following two years

	Number of employment spells over the following 2 years		
	Linear	Quadratic	Cubic
RD_Estimate	-9.795**	-9.298*	-9.336
	(4.076)	(5.059)	(6.296)
Observations	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear, quadratic and cubic specifications. The dependent variable corresponds to the number of employment spells over the two years following the end of the contract that defines the treatment status.

Table A90: Impact of UI benefit receipt on the number of unemployment spells over the following two years

	Number of unemployment spells over the following 2 years		
	Linear	Quadratic	Cubic
RD_Estimate	0.957	0.604	0.120
	(1.038)	(1.556)	(2.213)
Observations	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear, quadratic and cubic specifications. The dependent variable corresponds to the number of unemployment spells over the two years following the end of the contract that defines the treatment status.

Table A91: Impact of UI benefit receipt on the total number of days employed over the following two years

	Total number of days employed over the following 2 years		
	Linear	Quadratic	Cubic
RD_Estimate	-1027.132*	-737.503	-1084.775
	(566.452)	(739.947)	(837.905)
Observations	23559	23559	23559

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear, quadratic and cubic specifications. The dependent variable corresponds to the total number of days employed over the two years following the end of the contract that defines the treatment status.