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The effect of the potential duration of unemployment benefits on unemployment exits to work and match quality in France

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HIGHLIGHTS

- We evaluate the impact of a large increase in the max duration of unemployment benefits.
- We find no significant effects on post-unemployment wages or job stability.
- We find significant positive effects on unemployment and non-employment duration.

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ABSTRACT

Recent empirical literature finds very limited average effects of generous unemployment benefits on match quality. This study examines those effects in a setting where they could be large. We focus on workers with low employability and evaluate the impact of a large increase in potential benefit duration from 7 to 15 months. Our regression discontinuity design does not elicit significant short-term or medium-term effects on either employment duration or wages, whereas we find large positive effects on unemployment and non-employment duration.

1. Introduction

There is a large body of empirical evidence on the impact of unemployment insurance (UI) generosity. Apart from insurance provision, the empirical literature mostly focuses on impacts on labor market transitions from unemployment to employment. When unemployment benefits (UB) are more generous, reservation wages may increase and/or the search effort may be lower. This leads to a decrease in the unemployment exit rate to jobs. At the same time, unemployment benefits may affect the match quality, either in a positive way as it encourages jobseekers to wait for higher productivity jobs (see Marimon and Zilibotti, 1999; Acemoglu and Shimer, 2000) or in a negative way if human capital depreciates over the unemployment spell or if employers discriminate against candidates on the basis of unemployment duration. Effects on match quality are far less documented than effects on labor market transitions (see the review in Addison and Blackburn, 2000). Most recent studies, such as Card et al. (2007), Lalive (2007), van Ours and Vodopivec (2008), Centeno and Novo (2009), Caliendo et al. (2013) and Schmieder et al. (2012b), do not find any average effects on match quality.[1] This paper provides additional evidence that effects on match quality are also limited for workers with low employability, even though we find strong effects on unemployment duration. Compared with previous studies, this evidence is all the stronger since it concerns workers whose employability is particularly low (they have worked at most one year over the two pre-unemployment years). This is at odds with the idea that those workers should in principle improve their match quality when UB is more generous, for at least two reasons. Low-employability workers typically lack productive or job search skills that they could acquire thanks to extended potential benefit duration (PBD). They are also likely to be financially constrained so that more generous UB could greatly change the value they attach to unemployment and increase their reservation wage.

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[1] There are two very recent papers that find negative effects of the UI extensions on subsequent wages: Degen and Lalive (2013) and Schmieder et al. (2016).

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Our evidence is also all the stronger because we estimate the effect of a large increase in UB generosity. In a regression discontinuity design (RDD) similar to Card et al. (2007), we estimate the impact of an increase from 7 to 15 months in potential benefit duration (PBD). In the 2000–2002 French UI system, when workers have been employed for more than 8 months during the year before job separation, they are entitled to an extra 8 months of UB: their PBD is more than doubled.[2] This large increase makes our design very instructive: effects are expected to be large. However this large increase could also undermine the exogeneity assumption of our RDD, because workers have large incentives to work over the 8-month threshold. If they did so and actually accumulated just after the threshold, then selection into treatment would be endogenous. Yet we do not find any mass point in the distribution of workers just after the threshold. The absence of selection could be explained by our sample features - young unemployed with no previous experience with the UI rules- and by the French institutions - social partners discuss the UI rules every 3 years and may change the thresholds. We can thus be confident in the validity of our RDD.

Our result is robust to different measures of match quality: employment duration and hourly wage of the first job after unemployment exit. We complement those two standard indicators with the wage two years after unemployment registration. This enables us to compare short and extended PBD claimants at the same horizon, whatever the effect of PBD on unemployment duration. These medium-term effects are particularly relevant because accepting a stepping-stone job could ultimately be as efficient as a longer job search for productive jobs.

The absence of match quality effects is all the more striking in that the extension of PBD actually slows down job finding. Jobseekers with extended PBD wait longer before exiting registered unemployment to take a job (roughly 2.5 months). In addition, we verify that effects on registered unemployment duration are not only driven by the claimants' obligation to be registered as unemployed, since PBD extension also increases the duration of non-employment by roughly 1.5 months. Yet jobseekers with extended PBD do not find better jobs.

Our paper starts with a review of existing estimations of the effects of UI generosity. Then we give background information on the institutional environment of jobseekers in the French labor market. We present our data and describe our sample. In the next section, we justify our regression discontinuity design. In the fifth part, we show that extended PBD slows down unemployment exits. Finally, we show that extended PBD does not have any significant effects on match quality.

2. Related literature

Empirical evidence of the negative effect of UI generosity on the unemployment exit rate is abundant. In his seminal work, Meyer (1990) identifies the effect of UI generosity in the US through variations across states. Since the adoption of more generous UI is potentially endogenous at the state level, Card and Levine (2000) focus on exogenous variations in UI generosity due to targeted unanticipated policy change. Using the same identifying method, positive effects of potential benefit duration (PBD) on unemployment duration[3] are found in European countries, such as Germany (Hunt, 1995), Austria (Winter-Ebmer, 1998; Lalive and Zweimuller, 2004; Lalive et al., 2006), Poland (Puhani, 2000), Slovenia (van Ours and Vodopivec, 2006), Finland (Kyyrä and Ollikainen, 2008) and Portugal (Addison and Portugal, 2008). Other authors rely on discontinuities in the UI system to identify the effects. Those discontinuities are usually age thresholds, as in Lalive (2008), Caliendo et al. (2013) and Schmieder et al. (2012a). One exception is Card et al. (2007), who use discontinuities based on past employment thresholds. We follow their strategy.

By contrast, empirical evidence of the effect of UI generosity on match quality is scarce and mixed (see the review in Addison and Blackburn (2000)). Using a structural model, Belzil (2001) finds that increasing the PBD by one week leads to an increase in subsequent employment duration of between 0.5 and 0.8 days. Jurajda (2002) and Tatsiramos (2009) compare UB claimants with unemployed people not eligible for benefits and find large positive effects of eligibility on employment duration. Centeno (2004) estimates that a 10% increase in UI generosity translates into a 3% increase in subsequent job tenure. In more recent studies, authors focus on identifying causal effects using difference-in-difference methods (van Ours and Vodopivec, 2008) or regression discontinuity methods (Card et al., 2007; Lalive, 2007; Centeno and Novo, 2009; Caliendo et al., 2013; Schmieder et al., 2012b). They do not find any average effects of PBD on subsequent wage or employment duration. However, Centeno and Novo (2009) and Caliendo et al. (2013) show that match quality effects are heterogeneous. Centeno and Novo (2009) find that unemployed who are more financially constrained experience an increase by 3 to 8% in their earnings when PDB increases by 6 months. Caliendo et al. (2013) find that unemployed people who find jobs just before their unemployment benefits run out accept less stable jobs than comparable claimants with longer entitlements.[4]

As in Card et al. (2007) and Centeno and Novo (2009), we estimate the effect of UI generosity on younger unemployed people than in most existing RDD, which usually take age threshold late in the worker's career (greater than 40 years old). Part of the analysis of Centeno and Novo (2009) is based on a discontinuity at 30 years old. In the sample of Card et al. (2007), the average age is 31 years old. We specifically compare our results to theirs.

Our paper extends this empirical literature by estimating the effect of unemployment generosity on French workers with low employability.[5] In contrast to existing RDD studies, we focus on workers with low employability (that have unstable work history). They have been employed at most twelve months during the previous two years. In Card et al. (2007), workers have been employed for about 2.5 years during the 5-year period before unemployment; in Lalive (2007), individuals have worked over 9 years; in Schmieder et al. (2012b), the work history requirement amounts to 5 years; in Centeno and Novo (2009), eligible Portuguese workers have worked one year and a half over the two pre-unemployment years. Low-employability workers should in principle improve their match quality when UB is more generous, for at least two specific reasons. They typically lack productive or job search skills that they could acquire thanks to extended PBD. They are also likely to be financially constrained so that more generous UB could greatly change the value they attach to unemployment and increase their reservation wage. However, the match quality effect on low employability workers could be limited by the fact that they face very narrow hourly wage distribution (typically constrained by the high minimum wage in France). To address this issue, we consider a broad set of match quality indicators which encompasses employment duration and medium-run outcomes (and not only hourly wage).

[2] All recent RDD studies, except Card et al. (2007), rely on discontinuities in age (usually late in the workers' career) to estimate the impact of UI extensions. Our paper is the second study to rely on discontinuities in past employment duration. One contribution of this paper is thus to estimate the effect of UB generosity on younger individuals.

[3] Positive effects of replacement ratios are also found using difference-in-difference methods in Sweden (Carling et al., 2001 or Bennmarker et al., 2007) and Finland (Uusitalo and Verho, 2010).

[4] Those positive effects of UI extensions on specific subpopulation contrast with the results from two very recent papers: Degen and Lalive (2013) and Schmieder et al. (2016). Degen and Lalive (2013) focus on workers older than 50 years old and find that medium-run earnings are reduced by UI extension (using a difference-in-difference method). Schmieder et al. (2016) use a regression discontinuity design around the 42 and 45 years old threshold and find that subsequent wages are lower for workers with longer PBD.

[5] Dormont et al. (2001) study the introduction of decreasing replacement rates during the unemployment spell in France. Because the policy affected all the unemployed, they do not compute difference-in-difference estimates, nor do they implement regression discontinuity on the date of policy introduction. To our knowledge, this paper is therefore the first evidence in the French case on both unemployment exits and match quality. Fremigacci (2010) also applies a RDD method in the context of a French reform, but it focuses on senior jobseekers and only estimates effects on registered unemployment.

3. Institutional background

In France from 2000 to 2002, unemployed people aged less than 50 years old might have been eligible for one of four different potential benefit durations (PBDs), depending on past employment duration over a reference period. Jobseekers with a very long employment history could receive their unemployment benefits (UB) for up to 30 months, while PBD was only 4 months for those with the shortest past employment duration (i.e. 4 months over the last 18 months). In our paper, we focus on the 2 intermediate categories. These intermediate categories share the same reference period, one year before job separation, so they can be easily compared in a sharp regression discontinuity design.[6] Jobseekers whose past employment duration was between 6 and 8 months are entitled to 7 months of UB; they will be referred to as short PBD jobseekers. Jobseekers whose past employment duration exceeded 8 months over the previous year will be referred to as extended PBD jobseekers; they are entitled to 15 months of benefits.

UB levels are set according to the same rule in all PBD categories. The replacement rate, i.e. the ratio of UB to the former average wage, decreases with the "reference" wage, i.e. the average wage over the year preceding the job loss. For a reference wage around the legal minimum wage, the replacement rate is around 66% gross.[7] For a reference wage twice as large as the minimum wage, the replacement ratio is 57.4% gross. UB levels are capped at 5400 euros gross per month, one of the highest maximum levels in the OECD. The replacement rate rule changed in July 2001. Between January 2000 and June 2001, the replacement rate was smoothly decreasing in unemployment duration.[8] Since July 2001, the replacement rate is constant during the whole PBD.

The French UI system is one of the most generous in the OECD (though less generous than the Danish and Dutch systems).[9] According to the 2005 OECD summary table, the median maximum PBD among OECD countries is 12 months (24 months in France 2005); the median replacement rate[10] in the OECD is 58% net (67% in France); the median maximum monthly benefits payment is around 3300 euros (66% higher in France).

UI claimants have to register with the Employment Agency (*Anpe*) and respect certain rules to receive their benefits. They have to update their registration with the Employment Agency every month and, since July 2001, they have to meet a caseworker every six months. Monitoring is the same across all PBD categories; thus the comparison between short and extended PBD cannot reflect differences in monitoring practices. Active labor market programs (ALMPs), such as counseling, training or skill assessment, are also available whatever the PBD category.

UI claimants are allowed to work in side jobs and combine their wages with part of their UB. The fraction of UB saved in this way can be received later on, so that the theoretical expiration date of benefits is extended. Claimants working in side jobs remain registered with the Employment Agency, indicating they are still looking for a better job. We therefore consider them as *unemployed* in our main analysis. When UB expire, unemployed people can receive means-tested social assistance, called *Revenu Minimum d'Insertion*.[11] The amount received depends on family composition and earnings; in the early 2000s, a single adult could receive around 400 euros per month, around 33% of the gross minimum wage.

In addition to UI rules, two features of the French labor market should be highlighted. First, France ranks in the high-middle range in OECD indicators of employment protection strictness.[12] While strict, there is no discontinuity in employment protection around the 8 month threshold. Thus there is no EPL driven incentive for firms to fire workers just before they reach 8 months of service. Otherwise, there would be concerns about the validity of our regression discontinuity design. Second, the wage distribution is conditioned by a binding minimum wage. In 2000–2003, around 14% of French workers are paid at the minimum wage. Given that we focus on low-qualified workers, the share of unemployed people who face a rigid wage setting is higher and we expect match quality to be affected through employment duration, rather than wages.

4. Data

Our sample is drawn from a matched data set of French unemployment and employment registers (a complete description can be found in Appendix A). These data give information on the previous and subsequent employment spells of UB claimants, which is crucial to implementing our regression discontinuity design and to inspecting post-unemployment match quality.

We select a flow of new UB claimants who enter the Employment Agency between 2000 and 2002.[13] To avoid identification problems caused by the specific policies aimed at senior jobseekers, we exclude from our sample people aged 50 years old or more at registration. We also exclude jobseekers subject to very specific UI rules, such as recurrent temporary workers (in temp agencies), artists, and technicians working in the culture sector.

We select short and extended PBD jobseekers (intermediate categories) and exclude jobseekers in the least and most generous categories. Short and extended PBD jobseekers represent 28% of all new UB claimants. The majority (63%) of claimants can receive UB payments for 30 months. Those claimants were employed for at least 14 months before registering as unemployed, i.e. a longer period than claimants in our sample of interest. Thus, we identify the impact of PBD on claimants with relatively low employability. Table 6 in Appendix A shows the jobseekers' characteristics for different PBD categories. Jobseekers in the intermediate categories (first column) are younger and have lower education and qualification than those in the most generous category (second column). The proportions of women and foreigners are higher in the intermediate categories. Their previous job positions were less stable and less rewarding: only 14% had a permanent contract before job separation, and their hourly wage was 25% lower than in the broader group. Lastly, they had spent, on average, almost a year unemployed during the last 3 years.

To implement our identification strategy, we need to observe the past employment duration, which conditions eligibility. This information is not precisely recorded in the unemployment registers. At their first interview, claimants present administrative certificates delivered by their former employers, job counselors verify their UI eligibility and usually record in the unemployment registers the minimal past employment duration of their corresponding PBD categories, not the actual employment duration. We therefore use the employment registers to compute past employment duration. Although it is better, information from the employment registers is not perfect: there is still some measurement error. First, around one third of UB claimants have no

[6] When two categories have different reference period, the design becomes fuzzy.

[7] The monthly gross minimum wage was around 1100 euros in the early 2000s.

[8] After 4 months, claimants were to lose around 15% of their benefits; after 10 months, there was a further 15% decrease. Decreasing replacement rates make the difference in generosity between categories less important before than after July 2001. This change turns out to be minor and does not induce any differences in estimated effects.

[9] Note that there are no specific rules for seasonal workers except that their replacement rate has been reduced for income earned since December 2002.

[10] For a single unemployed person at the mean wage.

[11] Claimants having worked at least 5 of the last 10 years are eligible for *Allocation de solidarité Spécifique*, which has the same base amount as RMI, but is less restrictive in terms of family earnings.

[12] See OECD employment outlook 2004.

[13] We only retain new unemployment benefit claimants, meaning they do not have any residual benefits left from a former unemployment spell. Therefore, their potential benefit durations are directly linked to their employment spells since their last unemployment spell. When benefit claimants have residual benefits, a complicated rule extends those benefits according to their last employment spells.

employment spells recorded in the employment registers before unemployment. Second, around 20% of them have a past employment duration recorded in the employment registers which is not consistent with their PBD recorded in the unemployment registers. As displayed in Table 6 in Appendix A, “consistent” jobseekers with consistent records are more attached to the labor market than the unrestricted sample: they are more often men with high levels of education and qualification, they have higher former wages and longer past tenure, and they were less often registered as unemployed during the past three years. This is expected, as firms are known to report stable jobs more carefully in the employment registers. We also verify that unemployed people looking for a job in agriculture or care sectors are more likely to have inconsistent unemployment-employment records. Their former employers, probably in the same sector, are not included in the employment registers. Measurement error would be a detrimental issue if it had smoothed out any discontinuity in the share of extended PBD claimants at the threshold (Davezies and Le Barbanchon, 2014). Fig. 7 in Appendix B shows that this is not the case. Then, following Battistin et al. (2009), we can infer that our measurement error issue arises because of *contaminated* data, where a positive fraction of observations is measured without error. With *contaminated* data, one strategy is to apply a fuzzy regression discontinuity design whose results are reported in Appendix B and confirm our main analysis. In the main text, we exclude “inconsistent” workers from our sample. This strategy may introduce some bias, leading to underestimation in the case of negative selection and overestimation in the case of positive selection.[14] Because we obtain a large positive effect on unemployment duration, where selection is negative, and a zero effect on match quality, where selection is positive, removing any potential bias would actually make our results stronger. Our strategy, which removes inconsistent observations, turns out to be rather conservative.

Despite these inconsistencies, combining information from employment registers and unemployment registers has the clear advantage to increase the quality of measurement of exits to work in the unemployment registers. In our sample, 35% of the unemployed leave the Employment Agency reporting they have found a job. However, 29% of the unemployed leave without reporting their new situation to their caseworkers, and the Employment Agency removes from the unemployment registers 9% of the unemployed for administrative reasons (not showing up to interviews, for example). Those benefit claimants (29% + 9%) may have found a job. Indeed, 41% of the unemployed leaving the Employment Agency start a job recorded in the employment registers around their exit date.[15] Measuring the destination of exits in the employment registers not only increases the level of exits to work, it also affects its timing (a fact already highlighted by Card et al. (2008) and Boone and van Ours (2012)). It displaces the usual exit spikes from before UB exhaustion to after exhaustion (see Appendix A). We use the employment registers to measure destination of exits and compute the unemployment exit rate to jobs.

As expected, the raw comparison of both PBD categories shows that unemployment exits slow down when PBD is extended (see the left-hand panel in Fig. 1). Thus, the median registered unemployment duration is greater when PBD is extended (507 vs 306 days).

Adding the information from employment registers also enables us to consider non-employment duration, rather than registered unemployment duration. One advantage of non-employment duration is that it does not depend directly on registration behavior, which could be affected mechanically by the timing of claims, especially around UB exhaustion date, or by the Employment Agency monitoring rules. Actually, exhaustion spikes are smoothed when duration is measured as non-employment (compare short PBD non-employment exits in the right-hand panel and register exits in the left-hand panel in Fig. 1). However, one disadvantage is that non-employment duration does not take into account the fact that newly-hired individuals may still search for a job. In that case, non-employment duration may underestimate unemployment duration. This bias may be particularly important in the French context because claimants have strong incentives to accept side jobs and stay registered with the Employment Agency (see the previous section on the institutional background). Consequently, we present results for both outcomes: registered unemployment duration and non-employment duration.[16]

Finally, the unemployment registers do not contain any information about the exit jobs of benefit claimants. The employment registers enable us to describe the employment duration of newly-employed workers, their wages, and thus the part of their former wages they are able to recover.

In our sample, the median duration of the first job after leaving the unemployment registers is 6 months.[17] The monthly job separation rate shows spikes at the usual durations of temporary contracts: 6, 12 and 24 months (see the first panel in Fig. 2). Former jobseekers with extended PBD stay longer in their new jobs than those with short PBD: the median of employment duration increases by 1 month between the two groups.

Half of jobseekers gain 2% more than their former real hourly wages when they start a new job.[18] The wage gain is higher for the extended PBD jobseekers (see the second graph in Fig. 2): whereas more than one half of workers from the short PBD category do not recover their previous wage, the median wage gain is more than 3% among extended PBD jobseekers.

The previous descriptive statistics show that jobseekers entitled to longer benefits take more time to find a new job. Their new jobs last longer and are more rewarding. These differences shed some light on the link between UI generosity on the one hand and job-finding and match quality on the other. In the following, we use a regression discontinuity design to address potential selection issues in extended PBD.

5. Identification strategy

Our identification strategy is based on a regression discontinuity design (Hahn et al., 2001; Imbens and Lemieux, 2008). In the RDD framework, assignment to the extended PBD can be considered locally random around the threshold of one forcing variable, here the past employment duration. Then any differences in outcomes between claimants who are just below and just above the threshold can be attributed to the causal effect of extended PBD. The randomness assumption is impossible to test. However, there are ways to evaluate its credibility. First, precise manipulation of past employment duration is unlikely owing to the French institutional environment and the composition of our sample. Second, if there were precise manipulation of past employment duration, we should see some discontinuities in the distributions of the forcing variable and other covariates around the threshold. In this section, we present those discontinuity tests.

[14] Excluding inconsistent observations leads to selecting observations without measurement error and with positive measurement error (low true forcing variable) below the threshold. Above the threshold, it keeps observations without measurement error and with negative measurement error (large true forcing variable). Depending on the relation between potential outcomes and the forcing variable, this leads to underestimation or overestimation.

[15] The corresponding employment spell should begin at most sixty days before or after the actual exit date and it should not end before it.

[16] Note that registered unemployment duration is different from benefit duration. Workers may remain registered as unemployed at the Employment Agency (*Anpe*) even after their UI benefits lapse and the UI agency (*Assedics*) do not send them checks any more. Thus they can benefit from the placement services of the Employment Agency and from social assistance.

[17] Note that 14% of new job spells are censored at the end of the data set (December 2004).

[18] Wage loss is computed as the ratio of starting wage over pre-unemployment wage as computed in the employment registers.

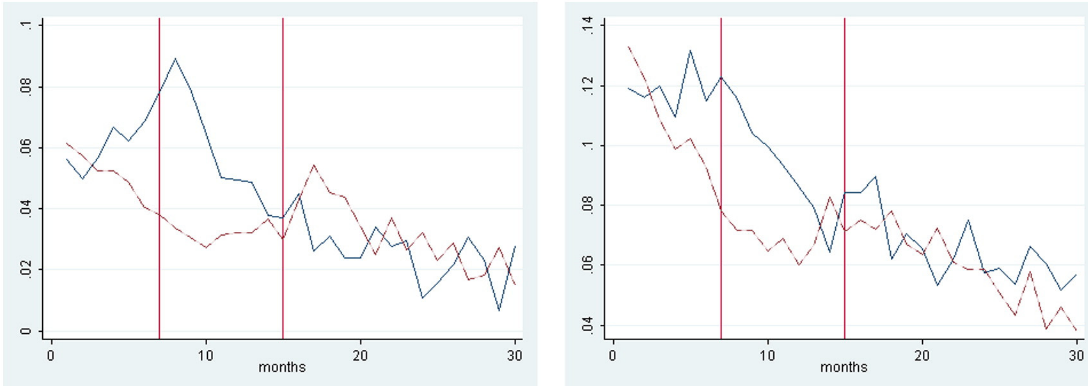

Fig. 1. Monthly unemployment register exit rate to work (on the left) and monthly non-employment exit rate (on the right).
Note: vertical lines represent dates of UB exhaustion for the short and extended PBD categories. The blue curve represents the hazard rate for short PBD, the red curve (dashed) for extended PBD. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)
Source: FH-DADS (Dares, Insee, Pole emploi).

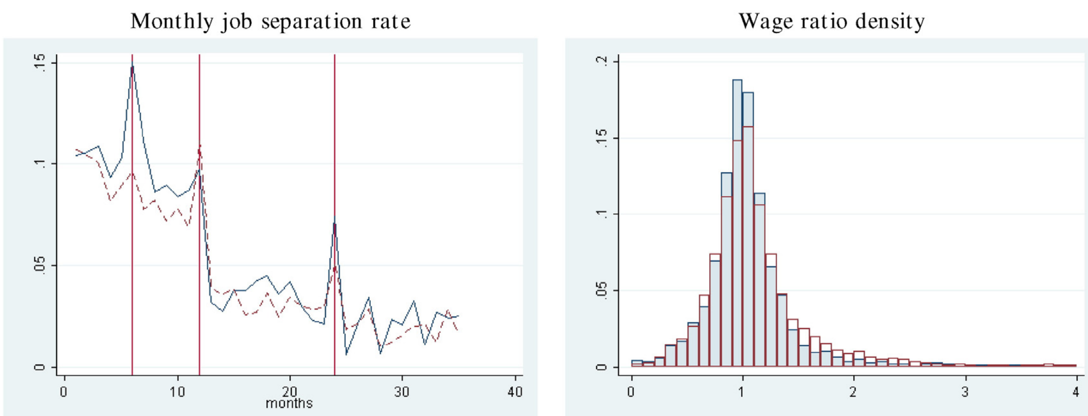

Fig. 2. Subsequent match quality.
Note: on the left-hand graph, the blue curve represents the separation rate for short PBD, the red curve (dashed) for extended PBD. Vertical lines represent typical durations of temporary contracts (6 months, 1 year, 2 years). On the right-hand graph, the blue histogram is drawn for short PBD, the red one for extended PBD. The wage ratio is the ratio of the new real hourly wage over the pre-unemployment real hourly wage. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)
Source: FH-DADS (Dares, Insee, Pole emploi).

5.1. Sample features that argue against precise manipulation

Local randomness of the forcing variable would not be verified if some benefit claimants were able to precisely manipulate their employment duration. If that were the case, those individuals who manipulate employment duration would be just above the threshold, and the comparison of benefits claimants just below and just above the threshold would be biased, because individuals who manipulate their employment duration are likely to have special characteristics highly correlated with unemployment exit rate, subsequent employment duration and wage.

Manipulation could occur at different stages: at benefit registration, when employer and employee separate, or when they first meet. Our measure of past employment is robust to fraud at benefit registration. We observe past employment from an external source, not from administrative recordings at benefit registration, and we drop observations with inconsistent past employment history. Our sample excludes recurrent temporary workers and technicians working in the culture sector whose past employment certificates shown at benefit registration are more often erroneous than those of others (see the 2010 annual report of the French *Cour des comptes*). Because most of the jobseekers in our sample separate from temporary contracts, we believe that manipulation at job separation is less a concern than in the general case. The use of temporary contracts, and their extension, is highly regulated in France. However we cannot exclude the possibility that employer and employee collude when they first meet, and set the contract duration so that it exactly extends the worker's past employment duration to meet the eligibility criteria for extended PBD. One argument which could limit the prevalence of collusion is that the employment prospects of our sample are structurally limited. They are less educated and less qualified than the typical French worker. This should limit their ability to bargain.

Finally, manipulation behaviors require knowledge of the Unemployment Insurance (UI) rules before registering as unemployed. Those rules are updated frequently in France (every three years), which could limit their salience.[19] UI rules are also quite complicated, as past employment duration can be computed on different reference periods. Moreover we focus on workers who are less likely to know UI rules, as they register as unemployed for the first time and they could not inherit knowledge of UI rules from previous unemployment spells.

[19] Trade unions and employers' association negotiate every three years the UI rules. There is no indication that the thresholds are chosen by the parties so that to target specific groups. Indeed the thresholds regularly change, as the number of categories evolves. In 2003, one PBD category was suppressed and the 8-month threshold disappeared. In 2006, the intermediate category threshold was decreased from 14 months to 12 months.

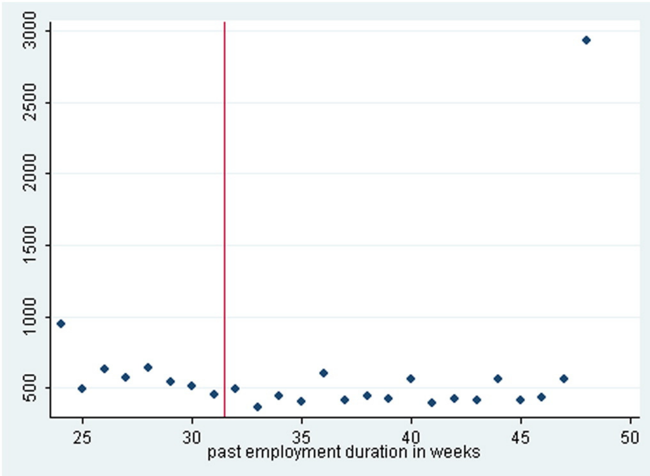

Fig. 3. Frequency of unemployment inflows by past employment duration.
Note: on the x axis, we report past employment duration in "weeks" (more precisely in quarters of a month); it starts at 6 months (24 "weeks"), this is the minimum employment duration for eligibility to the short PBD category. The vertical line represents the threshold between short and extended PBD categories. Mass points are found at typical contract durations (6 and 12 months).
Source: FH-DADS (Dares, Insee, Pole emploi).

5.2. Testing discontinuities in the forcing variable density

Turning to a statistical argument, forcing variable manipulation can be checked by inspecting its density around the eligibility threshold. If employment duration were precisely manipulated, claimants would accumulate just above 8 months. Fig. 3 shows the forcing variable distribution around the threshold. The graph shows that there is no mass point just above the threshold.

To test formally for discontinuity in the population density (McCrary, 2008), we estimate the following model:

$$N_d = \alpha + \delta I\left(d \geq \overline{d}\right) + \left(d - \overline{d}\right)\left(\beta_{-1} I\left(d < \overline{d}\right) + \beta_1 I\left(d \geq \overline{d}\right)\right) + v \qquad (1)$$

where d is pre-unemployment employment duration,[20] i.e. the forcing variable, N_d the population size of claimants with past employment duration d, $\overline{d}$ the threshold above which PBD is extended (8 months) and v the error term. Thus $I(d \geq \overline{d})$ indicates whether individuals benefit from extended PBD. β_{-1} and β_1 capture linear dependencies between the forcing variable and the population size (allowed to be different below and above the threshold). Then, the parameter δ captures the discontinuity in the population density at the threshold. We estimate model (1) on our sample excluding jobseekers who worked during the whole year before their last job separation (the last point in Fig. 3 is clearly an outlier). We cannot reject the null hypothesis that δ is equal to 0 ($\delta = 34$ with standard error 83). The result of the test is robust when unemployed people with one-year past employment duration are included. It is also robust when controlling for any "entire month" effect and for polynomials of past employment duration with higher degrees (in the estimation above, the relation is assumed linear).

5.3. Testing discontinuities in covariate distributions around the threshold

Further evidence of the exogeneity of the forcing variable can be found by inspecting claimants' characteristics around the threshold. There should be no discontinuities in the proportion of men, of low qualified workers, etc. around the threshold. Otherwise it would tend to prove that a certain subpopulation manages to manipulate its past employment duration to gain longer benefits. To test for discontinuity, we run several regression discontinuity estimations on different bandwidths around the threshold. The basic model we estimate has the same form as model (1) with the dependent variable being replaced by our characteristics of interest.

In Table 1, the estimates of δ are reported for different subpopulations around the threshold. In column 1, there are no restrictions on the estimation sample. It includes unemployed people who worked during the whole year before unemployment registration. These claimants may thus have quite different characteristics from the short PBD group who worked between 6 and 8 months over the previous year. Indeed, when all extended PBD claimants are included in the

[20] In this regression, d is expressed in "weeks", more precisely in quarters of a month. In all subsequent regressions, d is expressed in days.

Table 1
Covariate discontinuity test on different bandwidths around the threshold.
Source: FH-DADS (Dares, Insee, Pole emploi).

	Bandwidth around the threshold			
	All	4 months	2 months	1 month
	(1)	(2)	(3)	(4)
Man	−0.074***	−0.062***	−0.054*	−0.042
	(0.018)	(0.022)	(0.032)	(0.044)
Foreigner	0.001	−0.00006	−0.019	−0.013
	(0.009)	(0.011)	(0.016)	(0.022)
Age (log)	0.031***	0.020*	0.009	−0.0008
	(0.009)	(0.012)	(0.017)	(0.023)
Lower secondary education	0.065***	0.057***	0.024	0.039
	(0.013)	(0.016)	(0.022)	(0.030)
Vocational degree	0.005	0.016	0.055*	0.080*
	(0.018)	(0.022)	(0.031)	(0.043)
Upper secondary education	−0.022	−0.014	−0.016	−0.014
	(0.015)	(0.019)	(0.026)	(0.036)
Higher education	−0.042***	−0.058***	−0.055**	−0.088**
	(0.015)	(0.019)	(0.027)	(0.037)
Parent	0.055***	0.050**	0.016	0.021
	(0.016)	(0.020)	(0.029)	(0.039)
Married	0.028*	0.015	−0.001	−0.021
	(0.016)	(0.020)	(0.029)	(0.040)
Residence in great Paris region	−0.022*	−0.025	−0.020	0.016
	(0.013)	(0.016)	(0.023)	(0.030)
No qualification	0.050***	0.043**	0.015	0.033
	(0.016)	(0.020)	(0.029)	(0.039)
Low qualification	−0.032*	−0.004	0.041	0.018
	(0.018)	(0.023)	(0.032)	(0.044)
Intermediate profession	−0.024**	−0.027**	−0.017	−0.026
	(0.010)	(0.013)	(0.018)	(0.026)
Management	−0.012	−0.015*	−0.016	−0.016
	(0.008)	(0.009)	(0.013)	(0.018)
Previous hourly real wage	−0.519	−0.836	−1.070	−2.499
	(0.955)	(0.952)	(1.218)	(1.767)
Days unemployed during the last 3 years	48.513***	30.094**	19.879	12.293
	(11.166)	(13.817)	(19.748)	(27.572)
Previous work in service sector	0.003	0.013	0.011	0.012
	(0.016)	(0.020)	(0.029)	(0.040)
Looking for temporary contracts	−0.013	−0.016	−0.012	−0.020
	(0.010)	(0.012)	(0.017)	(0.024)
Previously on permanent contract	0.025**	0.010	0.031	0.048*
	(0.012)	(0.015)	(0.020)	(0.025)
Job separation during 1st quarter	−0.015	−0.033*	−0.020	−0.098***
	(0.015)	(0.019)	(0.027)	(0.037)
Job separation during 2nd quarter	−0.024	−0.006	−0.048*	0.003
	(0.015)	(0.018)	(0.026)	(0.035)
Job separation during 3rd quarter	0.062***	0.082***	0.091***	0.073*
	(0.016)	(0.020)	(0.028)	(0.038)
Job separation during 4th quarter	−0.023	−0.043**	−0.024	0.022
	(0.017)	(0.021)	(0.029)	(0.040)
Job separation before July 2001	0.067***	0.054**	0.037	−0.002
	(0.018)	(0.023)	(0.032)	(0.044)
Obs.	16,692	8352	3837	1817

Note: OLS estimation. Standard errors are robust to White heteroskedasticity. "Regression discontinuity" polynomials for the distance to the threshold of the forcing variable are first-order.
*** significant at 1%.
** significant at 5%.
* significant at 1%.

estimation, discontinuities are found in many characteristics: gender, age, education, marital status, qualification, unemployment history, previous contract type and quarter of job separation. These discontinuities highlight the fact that, in our design, estimations have to be local to be relevant.[21] Discontinuities persist when the estimation is restricted to a 4-month bandwidth around the threshold (column 2). But most of them vanish in the 2-month and 1-month bandwidth estimation (columns 3 and 4). In column 3, the estimation is restricted to unemployed people who have worked between 7 and 9 months over the last year. It excludes individuals who worked exactly 6 months, a typical temporary contract duration (see the mass point in Fig. 2). The workers whose contract was exactly 6 months may be quite different from claimants closer to the threshold and drive the discontinuities estimated on larger bandwidths.

On bandwidths smaller than 2 months, we find significant discontinuities (at the 5% level) for only two covariates out of the 15 independent covariates tested. We can therefore have confidence in our "no manipulation" assumption on those samples, which are close to the optimal bandwidth of 19.3 days we can estimate with the method of Imbens and Kalyanaraman (2012). In particular, we do not find strong evidence that there are more seasonal workers in the short PBD category.[22] There are no significant discontinuities in the share of unemployed people looking for a temporary contract. This variable can be considered a proxy for seasonal workers.

6. The effect of potential benefit duration on unemployment exits to work

The raw statistics in Section 4 show that hazard rates out of unemployment are lower in the extended PBD category. Our regression discontinuity design gives formal evidence of the causal impact of PBD on exits to work.

In Fig. 4, we illustrate the discontinuity we estimate afterwards. To draw this graph, we estimate the following Cox model of the hazard rate of unemployment exit to work,[23] noted θ_t:

$$\theta_t = \theta_t^0 \exp\left(\sum_{j\in 24..40} h_j I(d=j)\right) \qquad (2)$$

where θ_t^0 is the baseline hazard rate (t is the time in weeks since the jobseeker started claiming benefits) and d is again the past employment duration (expressed in quarters of a month). h_j is the hazard ratio between UB claimants with past employment duration j and claimants just below the extension threshold (h_{31} is set to 1). Fig. 4 displays the estimates of parameters $\exp(h_j)$ against past employment duration j on the sample restricted to the 4-month bandwidth around the threshold. There is a clear jump when crossing the threshold ($j=32$ weeks, i.e. 8 months). In the following, we estimate the size of the effect and its timing within the unemployment spell.

6.1. Estimating an overall effect of potential benefit duration on exit rates

We first estimate the overall effect of PBD extension in the following RDD Cox model of the unemployment exit to employment:

$$\theta_t = \theta_t^0 \exp\left(\delta I\left(d \geq \overline{d}\right) + \left(d - \overline{d}\right)\left(\beta_{-1} I\left(d < \overline{d}\right) + \beta_1 I\left(d \geq \overline{d}\right)\right) + \gamma X\right) \qquad (3)$$

[21] Another implication could have been that the linear assumption leads to misspecification. However, taking that view is somewhat less conservative (results with higher order polynomials available on demand).

[22] Had we found discontinuities in the share of seasonal workers, this would have been of serious concern as their unemployment exits are determined by calendar season and could bias our estimate of PBD extension.

[23] Recall that we measure unemployment duration in the unemployment registers and destination of exits in the employment registers.

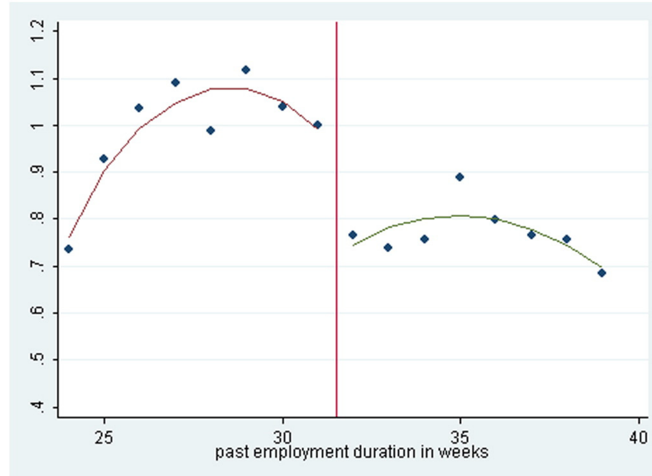

Fig. 4. Effect of potential benefit duration on the hazard of unemployment exit to work. Note: on the x axis, we report past employment duration in "weeks"; it starts at 6 months (24 "weeks"), this is the minimum employment duration for eligibility to the short PBD category. The vertical line represents the threshold between short and extended PBD categories. Here we compare unemployed people in the 4-month bandwidth around the threshold. On the y-axis, we report the coefficients $\exp h_j$ estimated in Eq. (2). Those capture the ratio between the average hazard rate of unemployed people with past employment duration j to that of unemployed people just below the 8-month past employment duration threshold (31 "weeks").
Source: FH-DADS (Dares, Insee, Pole emploi).

where d is the past employment duration expressed in days and all other variables have already been defined. X is a set of covariates, similar to those in Table 1. It comprises gender, nationality, age, education, parental and marital status, residence in the Paris region, qualification, past wage, separation reason, seasonal work dummy, past employment history, preferred sector, seasonality at registry and year dummies. The model is estimated on the full sample and on subpopulations around the threshold. Given the evidence from the tests on covariates, we prefer the estimations on the 2- or 1-month bandwidths where there are no significant discontinuities in covariate distribution around the threshold. Moreover these samples are close to the optimal bandwidth of 19.3 days estimated with the method of Imbens and Kalyanaraman (2012). The estimates of

Table 2
Effect of extending potential benefit duration on unemployment exit rate to employment.
Source: FH-DADS (Dares, Insee, Pole emploi).

	Bandwidth around the threshold			
	All	4 months	2 months	1 month
	(1)	(2)	(3)	(4)
Average effect				
Extending PBD	−0.295***	−0.283***	−0.324***	−0.225*
	(0.053)	(0.066)	(0.094)	(0.129)
Dynamic effect				
During the first 7 months	−0.332***	−0.228***	−0.247**	−0.184
	(0.054)	(0.070)	(0.100)	(0.139)
Between 8 and 15 months	−0.956***	−0.883***	−0.947***	−0.763***
	(0.063)	(0.084)	(0.121)	(0.169)
After 16 months	−0.509***	−0.053	0.029	−0.038
	(0.077)	(0.103)	(0.156)	(0.219)
Obs.	16,692	8352	3837	1817

Note: Cox model estimation. Model (2) in the upper part and model (5) in the lower part. "Regression discontinuity" polynomials for the distance to the threshold of the forcing variable are first-order. All covariates tested in Table 1 are included: gender, nationality, age, education, parental and marital status, residence in the Paris region, qualification, past wage, separation reason, seasonal work dummy, past employment history, preferred sector, seasonality at registry and year dummies.
*** significant at 1%.
** significant at 5%.
* significant at 1%.

parameter δ are reported in the upper part of Table 2. They are all significantly different from 0 at the 10% level whatever the sample. The estimate on the 1-month bandwidth is somewhat less precisely estimated and smaller in magnitude than estimates on wider bandwidths. The estimate on the 1-month bandwidth shows a 20% (exp(−.23)) decrease in the exit rate, compared with 28% (exp(−.32)) on the 2-month bandwidth.

Table 8 in Appendix C shows some robustness checks. The results are robust, in strength and significance, when there are no covariate controls or when we control for entire month effects (unemployed have worked exactly N months before entering unemployment). Estimating the model with higher degree polynomials, however, alters the results. Estimates are smaller in magnitude and no longer significant on the 2-month bandwidth.

One concern with the previous estimation could be that there are more seasonal workers in the short PDB category, so that the increase in the exit rate of the short PDB category only reflects calendar effects. We have already partially addressed this issue, as we do not find evidence of discontinuities around the threshold in proxies for the share of seasonal workers. We also include a dummy for seasonal work in the set of controls X. Another way to identify seasonal workers is to control for repeat employment: seasonal workers typically return to their last employer. The last line of Table 8 in Appendix C shows that the effect is robust when recalled claimants are excluded from the sample.

In the previous model (model (3)), the parameter δ captures the effect of benefiting from extended PBD rather than short PBD on the exits to work at any time in the unemployment spell. The model assumes that the effect does not depend on the timing of UB, and in particular that the effect does not change close to the date of benefit exhaustion. However Fig. 1 shows some evidence of exit rate increases at benefit exhaustion, reflecting the fact that the finite duration of UB makes job search nonstationary. We next document the effect on the dynamics of exit rates.

6.2. Estimating the effect of potential benefit duration on the dynamics of exits to work

During the first 7 months of unemployment, unemployed people in both the short and the extended PBD categories can receive their benefits. Extended PBD claimants are then better off because they anticipate future benefits. Between the two expiration dates (7 and 15 months), short PBD benefits are no longer paid.[24] After 15 months, neither group receives UB. In the following Cox model, the effect is allowed to vary along the unemployment spell:

$$\theta_t = \theta_t^0 \exp\Big(I\big(d \geq \overline{d}\big)\big(\delta_0 I(t<t_0) + \delta_1 I(t_0 \leq t<t_1) + \delta_2 I(t_1 \leq t)\big)\Big)\cdots \tag{4}$$

$$\exp\Big(\big(d-\overline{d}\big)\Big(\beta_{-1} I\big(d<\overline{d}\big) + \beta_1 I\big(d \geq \overline{d}\big)\Big) + \gamma X\Big) \tag{5}$$

where all notations are already defined, except t_0 and t_1 which are the theoretical exhaustion dates of short and extended PBD (equal to 7 and 15 months). Estimations on different bandwidths around the threshold are presented in the lower part of Table 2. We find that UI generosity has a negative effect on exits to jobs in the first 7 months, when all the unemployed receive benefits (see line 1). This corroborates the results of Card et al. (2007), who find that UI generosity has an effect on exits to jobs before the exhaustion of short PBD benefits. However, we also find that UI generosity has a higher and robust effect on exits to work between 7 and 15 months when short PBD benefits have expired and extended PBD benefits have not. This "contemporaneous" effect is very strong in magnitude. In the 2-month bandwidth, it induces a 60% decrease in the exit rate when PBD is extended. After 15 months, there is no significant difference between the hazard rates of unemployed people in the two categories, as all benefits have expired.

Table 3
Effect of extending potential benefit duration on non-employment exit rate.
Source: FH-DADS (Dares, Insee, Pole emploi).

	Bandwidth around the threshold			
	All	4 months	2 months	1 month
	(1)	(2)	(3)	(4)
Average effect				
Extending PBD	−0.116***	−0.167***	−0.141**	−0.137
	(0.039)	(0.049)	(0.069)	(0.095)
Dynamic effect				
During the first 7 months	−0.045	−0.096*	−0.048	−0.062
	(0.041)	(0.052)	(0.074)	(0.101)
Between 8 and 15 months	−0.380***	−0.401***	−0.481***	−0.334**
	(0.052)	(0.066)	(0.097)	(0.138)
After 16 months	−0.080	−0.135*	−0.062	−0.235
	(0.062)	(0.076)	(0.112)	(0.160)
Obs.	16,692	8352	3837	1817

Note: Cox model estimation. Model (2) in the upper part and model (5) in the lower part. "Regression discontinuity" polynomials for the distance to the threshold of the forcing variable are first-order. All covariates tested in Table 1 are included: gender, nationality, age, education, parental and marital status, residence in the Paris region, qualification, past wage, separation reason, seasonal work dummy, past employment history, preferred sector, seasonality at registry and year dummies.
*** significant at 1%.
** significant at 5%.
* significant at 1%.

Table 9 in Appendix C presents some robustness checks: we test whether the estimates and their significance are robust to the introduction of higher degree polynomials and to the exclusion of covariates. Effects after 7 months of unemployment are robust. Effects before 7 months are not robust to controls with higher degree polynomials.

Although this estimation gives some insights into the effects on the dynamics of exits to work, it is subject to dynamic selection bias[25] and should therefore be interpreted with caution. The regression discontinuity design ensures that, when entering unemployment, individuals just below and above the threshold are identical. However, if extended PBD affects two groups of unemployed people differently, say that it reduces unemployment exits for group A, but not for group B, then the composition of the unemployed population at any point later in the unemployment spell will be different between short and extended PBD categories. Group A will be over-represented in the extended PBD category. When contrasting hazard rates, we mix two effects, one direct extended PBD effect and one composition effect.

6.3. Estimating the effect of potential benefit duration on non-employment duration

We also estimate models (3) and (5) with the non-employment exit rate as the dependent hazard. Table 3 displays average effects in its upper part (model (3)) and dynamic effects in its lower part (model (5)). The average effects on non-employment exits are half the size of those on unemployment register exits. The slow-down in non-employment exits due to PBD extension is still significant at the 5% level on the 2-month estimation bandwidth. The effects on non-employment exits are not significant before short PBD exhaustion and after long PBD exhaustion: they are concentrated between 7 and 15 months. This differs from the effects on unemployment register exits, which appear before 7 months. The lower effects on the non-employment exit rate than on the unemployment exit rate suggest that some workers react to extended PBD by searching on-the-job. Extending PBD does not postpone their job finding, but induces them to

[24] These theoretical expiration dates slightly underestimate the true expiration dates, as some unemployed people may take up side jobs while registered as unemployed and delay the time when their benefits expire.

[25] See for example Ridder and Vikstrom (2011).

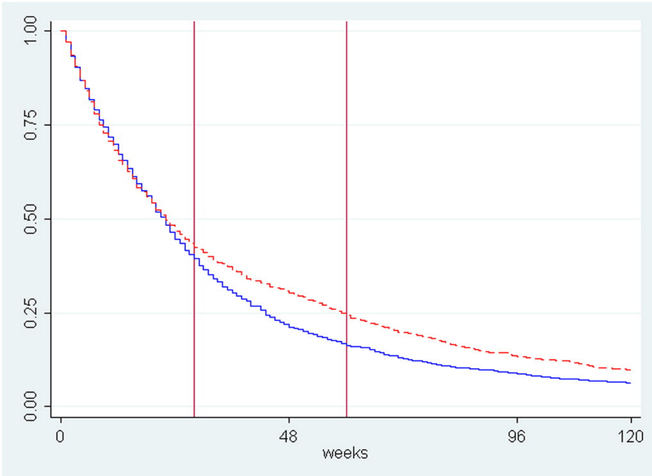

Fig. 5. Survival rates in non-employment by PBD category.
Note: the blue curve represents the survival rate for short PBD, the red curve (dashed) for extended PBD. The sample is restricted to jobseekers whose past employment duration is within a one month window around the RDD cutoff. Vertical lines represent dates of UB exhaustion for the short and extended PBD categories. On the x axis, non-employment duration is measured in "weeks".
Source: FH-DADS (Dares, Insee, Pole emploi).

declare that they are still searching for another job and to stay registered at the PES.[26]

In the current case of non-employment duration, we complement the analysis of hazard rates by plotting the survival rates of job seekers, whose past tenure is close to the RDD threshold (Fig. 5). This helps us to assess how severe is the dynamic selection issue discussed in the section above. Reassuringly, the timing of the effect in Fig. 5 is similar to the one obtained with hazard rates. The two survival curves mostly diverge between the two exhaustion dates of the short and extended PBD categories.

All these elements tend to prove that UI generosity has a causal and negative impact on unemployment exits to employment. We now estimate the impact of PBD on match quality.

7. The effect of potential benefit duration on match quality

We first consider match quality of the first job after leaving the unemployment registers. Match quality is captured by two components: hourly wage and employment duration. Wage is a classical proxy for match productivity, as it represents a fraction of the match surplus. However, as already mentioned, wage setting is quite rigid in France, so that the wage distribution is concentrated around the minimum wage. Therefore we do not expect any strong wage effect of UI generosity. This first proxy can then be fruitfully complemented by employment duration. Considering that employment is an experience good (Jovanovic, 1979), match quality is revealed as time goes by and signalled by continuing employment. We also consider a third proxy: hourly wage at a fixed horizon after unemployment registration (namely two years). This enables us to analyze the medium-term effects of UI generosity, which abstract from the differences in the unemployment exit timing induced by PBD extension. In this section, we first discuss bias arising from selection into employment, and then we present the results for our three proxies of match quality.

7.1. Selection into employment

To estimate the effects of PBD on match quality, we compare outcomes for job finders with short and extended PBD. This comparison

Table 4
Effect of extending potential benefit duration on survival in unemployment.
Source: FH-DADS (Dares, Insee, Pole emploi).

	Bandwidth around the threshold			
	All	4 months	2 months	1 month
	(1)	(2)	(3)	(4)
Survival in unemp. (unconditional)	−0.008	0.011	0.017	0.047
	(0.018)	(0.022)	(0.031)	(0.043)
Still unemp. 2 years after registration	−0.018	−0.005	0.015	0.052
	(0.016)	(0.020)	(0.029)	(0.039)
Obs.	16,692	8352	3837	1817

Note: OLS estimation. Standard errors are robust to White heteroskedasticity. "Regression discontinuity" polynomials for the distance to the threshold of the forcing variable are first-order. All covariates tested in Table 1 are included: gender, nationality, age, education, parental and marital status, residence in the Paris region, qualification, past wage, separation reason, seasonal work dummy, past employment history, preferred sector, seasonality at registry and year dummies.

may suffer from a well-known bias due to different selection in employment across PBD categories (Ham and LaLonde, 1996). Jobseekers induced to leave unemployment because of shorter PBD may be a very specific population with intrinsic characteristics that make them work in different jobs. In that case, comparing characteristics of jobs found after short and extended PBD unemployment spells may amount to comparing individual characteristics rather than measuring the causal impact of PBD. As an assessment of the extent of the potential bias, we estimate a RD model, similar to model (1), with unemployment survival as the outcome. The difference in survival rates informs us about potential selection bias under a monotonicity assumption. The results in Table 4 show that there is no significant discontinuity at the threshold of the forcing variable.[27] In Table 4, we check for discontinuity in survival both before the last date in our data (unconditionally) and 2 years after unemployment registration.[28] These survival outcomes are relevant for our analysis of match quality of the first job after unemployment exit and of match quality 2 years after unemployment registration.

7.2. Effects on the first job when leaving the unemployment register

In this subsection, we restrict the sample to jobseekers who find a job when leaving the unemployment register. The effect on the starting wage is estimated using the following regression discontinuity model:

$$Y = \alpha + \delta I\left(d \geq \overline{d}\right) + \left(d - \overline{d}\right)\left(\beta_{-1} I\left(d < \overline{d}\right) + \beta_1 I\left(d \geq \overline{d}\right)\right) + \gamma X + \varepsilon \qquad (6)$$

It has the same structure as model (1). In addition, we expand the set of covariates with respect to previous estimations to account for labor market conditions at the time of unemployment exit and include quarter dummies. Although these controls are potentially endogenous (unemployed people with longer PBD may select into employment when the labor market is tight), they account for the fact that, due to longer unemployment spells, labor market conditions are systematically different for unemployed people with short and extended PBD. More precisely, claimants with extended PBD tend to exit later than those with short PBD (see graph 1). We also include dummies for calendar month of exit to control for seasonal labor market conditions. These dummies are in addition to the seasonal worker dummy already included in the previous analysis. Note that we also control for returns to the same employer.

[26] The lower effects on the non-employment exit rate than on the unemployment exit rate are also probably related to the absence of spikes at benefit exhaustion (Fig. 1).

[27] Even if the difference is not statistically significant, it could matter quantitatively. We have applied a bounding approach as in Lee (2009). As expected, estimate sets are quite large: 9 points on the wage equation estimated below. They are not very informative.

[28] We consider that a jobseeker has not survived *unconditionally*, if he exits the unemployment register at least once between the first date of registration and the last date of our data (end of December 2004).

Table 5
Effect of extending potential benefit duration on match quality.
Source: FH-DADS (Dares, Insee, Pole emploi).

	Bandwidth around the threshold			
	All	4 months	2 months	1 month
	(1)	(2)	(3)	(4)
Match quality of the first job after unemployment exit				
Hourly wage ratio	0.0009	−0.009	−0.030	0.010
	(0.024)	(0.028)	(0.040)	(0.056)
Employment survival after 8 months	0.034	0.064**	0.012	0.020
	(0.025)	(0.032)	(0.045)	(0.062)
Obs.	7391	3797	1803	830
Match quality of the job 2 years after unemployment registration				
Hourly wage ratio	0.007	0.043	−0.002	−0.004
	(0.029)	(0.035)	(0.051)	(0.065)
Obs.	4546	2229	1058	489

Note: OLS estimation (model (6)). Standard errors are robust to White heteroskedasticity. "Regression discontinuity" polynomials in the distance between the threshold and the forcing variable are first-order. All covariates tested in Table 1 are included: gender, nationality, age, education, parental and marital status, residence in the Paris region, qualification, past wage, separation reason, seasonal work dummy, past employment history, preferred sector, seasonality at registry and year dummies. Covariates capturing the seasonality and the business cycle at the exit date are also included.
*** significant at 1%
** significant at 5%.
* significant at 1%

In model (6), we normalize the starting wage by the past employment wage. Our outcome of interest is thus the logarithm of the ratio between the real hourly starting wage and the real hourly past employment wage. Differences highlighted by the descriptive statistics in Fig. 2 are not confirmed in the regression discontinuity estimation (line 1 in Table 5). There are no significant effects of extended PBD, and parameter estimates vary across different bandwidths. There are no significant effects when covariate controls are excluded, when polynomials of higher orders are used, or when the wage is specified in levels (see Table 10 in Appendix C).

The effect on employment duration is estimated using a RD model equivalent to model (6). We concisely measure employment duration with a dummy variable indicating whether employment spells following unemployment exits last more than 8 months with the same firm.[29] We thus distinguish between typical short temporary contracts and stable employment relations (see the typical pattern of employment duration in Fig. 2). 8 months is an interesting threshold: it is the extended PBD eligibility threshold. Then former jobseekers who find a job lasting more than 8 months are entitled to extended PBD. If they were already in this category, 8 months can be taken as a renewal threshold.

The results are presented in Table 5 (line 2). In the 4-month bandwidth around the threshold, extended PBD seems to have a positive effect on employment duration. Extending PBD increases the proportion of jobs lasting more than 8 months by 6 points. However, when the bandwidth is less than 2 months, the effect of extended PBD on employment duration is lower and not significant (columns 3 and 4). Consequently, there is no evidence of any extended PBD impact on employment duration. This conclusion is robust, when covariates are excluded (see Table 11 in Appendix C) or when a Cox model of the hazard out of employment is estimated (see Table 12 in Appendix C).

7.3. *Effects on the job two years after unemployment entry*

We now turn to medium-term effects.[30] They are interesting for at least two reasons. First, as match quality is an experience good, it may be revealed by hourly wage progression as time goes by. Second, while unemployed people with extended PBD may delay their unemployment exit, former unemployed people with short PBD may gain experience and move to other jobs, and this process may also improve match quality. Medium-term match quality is captured by the hourly wage 2 years after unemployment entry. Again, we do not consider as employed jobseekers who work in side jobs but are still registered at the Employment Agency. As in the previous section, our analysis may suffer from a selection bias into employment. However, we verify that the share of workers 2 years after registration is not affected by extended PBD (see line 2 in Table 4). The results of the regression discontinuity model are presented in the lower part of Table 5. There are no significant discontinuities in the wage ratio. The results are robust when covariates are excluded or higher order polynomials are used as controls (see Table 13 in Appendix C). The results are also robust when the model is estimated on earnings (Table 14 in Appendix C). Then we impute zero earnings to individuals not employed two years after registry. This somehow relives us from our result of no selection bias obtained under a monotonicity assumption.

8. Conclusion

In a regression discontinuity design (RDD) inspired by (Card et al., 2007), we find that potential unemployment benefit duration (PBD) has a significant and large impact on unemployment exits to work, but no impact on subsequent match quality. When jobseekers are entitled to 15 months of benefits (extended PBD) instead of 7 months (short PBD), simply because they cross the 8-month past-employment threshold, their job finding rate slows down, leading to an increase of 2.5 months in registered unemployment duration and of 1.5 month in non-employment duration. This effect is in the range of the consensus estimate put forward by Tatsiramos and van Ours (2014).[31] However, it is twice as large as the RDD estimate in Centeno and Novo (2009), where a 6-month increase in PBD induces jobseekers aged 30 years old to increase their subsidized-unemployment duration by around 1.5 months longer. Early in the unemployment spell, before the short PBD exhaustion date, knowing that PDB will be extended induces a decrease of 11% to 22% in the job finding rate. Again, this effect is larger than the comparable RDD estimate in Card et al. (2007).

In line with the recent literature using RDD or difference-in-difference estimates, we do not find any average improvement in subsequent match quality. Compared with the recent literature, our result can be seen as even stronger evidence that there is no gain in match quality because our estimate of unemployment exit effects is twice as large as usual. Our result is all the stronger because our sample is made up of workers with low employability, who could be greatly affected by unemployment insurance generosity.

Our results - large effects on unemployment duration, but no significant effects on match quality - seem difficult to reconcile with the canonical job search model (as described for example in van den Berg (1990)). However we can propose three interpretations. First, our results may suggest that unemployed individuals face a very narrow and stationary match quality distribution of job offers and that they react to generous unemployment benefits (UB) by lowering their search effort. A second interpretation could be that the reservation wage is not binding on a significant time interval of the unemployment spell. Then, more generous UB does not necessarily induce unemployed individuals to raise their reservation wages, but induce them to lower their search efforts (this interpretation is detailed in Schmieder et al. (2016)). According to a third interpretation, job search is fundamentally non-stationary. For example, the quality of job offered to unemployed individuals decreases as their unemployment spell lengthen. In such a world,

[29] We can ignore censoring issues: there are virtually no employment spells censored before 8 months.

[30] We cannot consider long-term effects due to data availability: the last cohort entering in our sample is observed over two years.

[31] "An extension of potential benefit duration leads to an increase in actual unemployment duration of about 20% of the original benefit duration extension." p.299 in Tatsiramos and van Ours (2014).

unemployed individuals with more generous UB over raise their reservation wages at the beginning of their unemployment spell, stay too long unemployed and are forced to accept low quality jobs later on. Then the effect of generous UB on the average accepted wage is ambiguous and can even be negative for workers whose skills depreciate at a high rate, as in Degen and Lalive (2013). In the same vein, non-stationarity can also arise from decreasing flow value of unemployment with search time on top of decreasing unemployment benefits (Brown et al., 2011). Further work, namely on the timing of the match quality effect along the unemployment spell, is needed to discriminate between those different interpretations.

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Appendix A. Employment-unemployment registers

Our data set is based on the matching, at the individual level, of:

- the *Fichier Historique* (FH) of the French Public Employment Agency (ANPE), which records unemployment spells on a daily basis,
- and the *Déclarations Administratives de Données Sociales* (DADS) built by the French Statistical Institute (Insee) from firms' fiscal declarations. Firms declare employment spells for each worker on a daily basis.

The employment registers cover around 85% of French wage earners. Civil servants from the French central administration (ministries) and workers from the care sector or employed by a private person do not appear in the employment registers.

Due to legal restrictions (protection of private information), the matching only concerns a subpopulation of initial registers. Unemployed people and workers have to satisfy two conditions to be included in the new data set:

- to be born in October of an even year,
- to be registered at least once in one or the other register between 1999 and 2004.

For individuals in the matched sample, we observe all their unemployment and employment spells from January 1999 to December 2004. Spells are censored in December 2004. For individuals who appear at least once in the employment (resp. unemployment) registers between 1999 and 2004, employment (resp. unemployment) spells before 1999 are included (the employment registers start in 1976 and the unemployment registers in 1994).

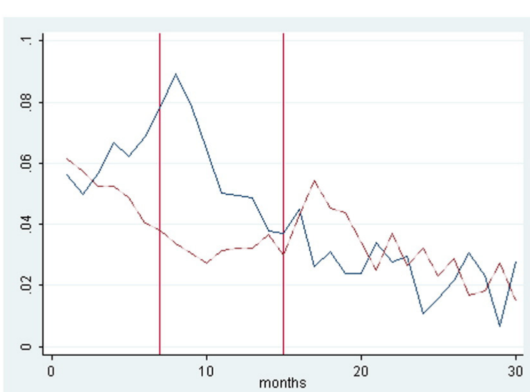
All employment information (wage, duration and sector before or after unemployment) is taken from the employment registers except the type of previous contract (or separation reason) recorded in the unemployment registers. Each employment spell is within the same firm.

All unemployment information (namely duration, UB, desired type of contract) is taken from the unemployment registers. Sociodemographic characteristics (gender, nationality, age, education, parental and marital status, place of residence, qualification) are recorded in the unemployment registers at the beginning of the unemployment spell.

We also consider non-employment duration, which is the time elapsed between two employment spells.

Despite some inconsistencies in the employment-unemployment history of individuals (discussed in the data section), the overall quality of the match is good. For example, the UB take-up rate, i.e. the fraction of unemployed claimants among eligible workers who separate from their employers, is similar when measured in the matched sample and in external sources.

Measuring jobs in employment registers not only increases the levels of exits to jobs (as explained in the data section), it also affects their timing (a fact already highlighted by Card et al. (2008) and Boone and van Ours, 2012). The lack of information due to missing jobseekers' exit reports usually blurs the variations of exit rates to employment at benefit exhaustion and casts doubt on the existence of spikes at that time. The exit rate to jobs, as reported to the Employment Agency, does indeed rise and decline before benefit exhaustion (see the second graph in Fig. 6). However the exit rate to jobs, as recorded in the employment registers, also rises before the end of benefit exhaustion, but it reaches a spike just after it (see the first graph in Fig. 6). This certainly highlights a change in the reporting behavior of jobseekers at benefit exhaustion.



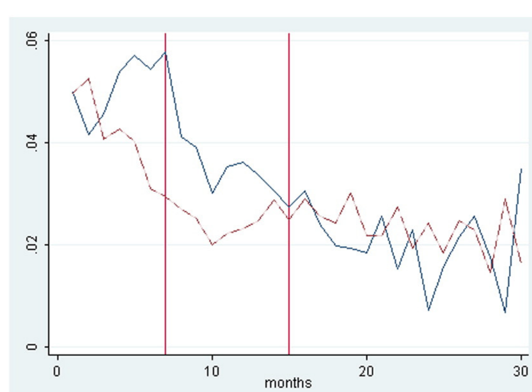


Fig. 6. Monthly unemployment register exit rates to jobs recorded in employment registers (on the left) and to jobs reported in unemployment registers (on the right).
Note: vertical lines represent dates of UB exhaustion for short and extended PBD categories. The blue curve represents hazard rates for short PBD, the red curve (dashed) for extended PBD. (For interpretation of the references to color in this figure legend, the reader is referred to the web version of this article.)
Source: FH-DADS (Dares, Insee, Pole emploi).

Table 6
Effects of sample selection on covariates.

	Short or extended PBD	30-month PBD	Short or extended PBD final sample
Man	0.46	0.49	0.48
Foreigner	0.09	0.06	0.07
Age (log)	29.58	32.28	28.75
Lower secondary education	0.21	0.14	0.15
Vocational degree	0.38	0.42	0.37
Upper secondary education	0.19	0.18	0.21
Higher education	0.19	0.24	0.24
Parent	0.34	0.43	0.28
Married	0.33	0.46	0.29
Residence in greater Paris region	0.16	0.20	0.17
No qualification	0.31	0.20	0.27
Low qualification	0.47	0.50	0.49
Intermediate profession	0.07	0.10	0.09
Management	0.04	0.08	0.05
Previous hourly real wage	6.42	8.88	7.95
Days unemployed during the last 3 years	311.67	106.19	286.40
Attached to the service sector	0.71	0.72	0.72
Looking for temporary contracts	0.08	0.08	0.08
Previously on permanent contract	0.14	0.40	0.15
Observations	31,945	71,184	16,692

Appendix B. Fuzzy design

To test the influence of dropping “inconsistent” workers, we estimate the effect of PBD using a fuzzy RDD on the whole population and compare the results with the effects of PDB estimated in a sharp RDD on the “consistent” population. In the fuzzy RDD, the treatment is instrumented by a prediction using the forcing variable. We estimate effects on unemployment and non-employment duration.

Fig. 7 shows the evolution of the fraction of extended PBD claimants with past employment duration as recorded in the employment registers. As already mentioned, past employment duration is measured with error. Before the 7-month threshold, some unemployed people are entitled to extended PBD, whereas they should not be. After the threshold, some workers are entitled to short PBD, whereas they should benefit from extended PBD. Note that the error is symmetric.

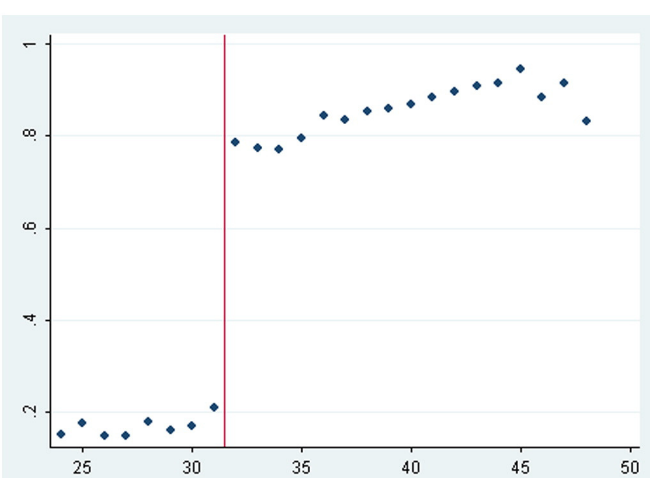


Fig. 7. Actual extended PBD category.
Note: on the x axis, we report past employment duration in “weeks” (more precisely in quarters of a month); it starts at 6 months (24 “weeks”), this is the minimum employment duration to enter the short PBD category. The vertical line represents the threshold between short and extended PBD categories.
Source: FH-DADS (Dares, Insee, Pole emploi).

Table 7 displays estimation results in the fuzzy (upper part) and sharp (lower part) design. Estimates in the fuzzy design are similar in magnitude to those in the sharp design. They are, however, less precisely estimated and significantly different from zero for both durations only in the 2-month bandwidth. Broadly speaking, these results confirm that excluding “inconsistent” workers does not severely bias our analysis.

Table 7
Fuzzy and sharp designs: effects on unemployment and non-employment duration.
Source: FH-DADS (Dares, Insee, Pole emploi).

	All	4 months	2 months	1 month
	(1)	(2)	(3)	(4)
Fuzzy design				
Unemployment	61.732***	75.584***	83.693***	50.947
	(13.616)	(18.189)	(25.493)	(37.246)
Non-employment	23.722	57.640***	44.336	17.293
	(14.901)	(19.677)	(27.972)	(41.332)
Obs.	17,794	10,107	4774	2280
Sharp design				
Unemployment	63.380***	76.821***	85.131***	80.454***
	(9.286)	(11.719)	(16.550)	(22.749)
Non-employment	29.162***	52.934***	50.015***	50.233**
	(10.204)	(12.759)	(18.121)	(25.218)
Obs.	15,039	8352	3837	1817

Note: OLS estimation. Standard errors are robust to White heteroskedasticity. All covariates tested in the previous section are included: gender, nationality, age, education, parental and marital status, residence in the Paris region, qualification, past wage, separation reason, seasonal work dummy, past employment history, preferred sector, seasonality at registry and year dummies.
*** significant at 1%.
** significant at 5%.
* significant at 1%.

Appendix C. Robustness

Table 8
Effect of extending potential benefit duration on unemployment exit rate.
Source: FH-DADS (Dares, Insee, Pole emploi).

	All	4 months	2 months	1 month
	(1)	(2)	(3)	(4)
Without covariates	−0.446***	−0.383***	−0.373***	−0.246*
	(0.052)	(0.065)	(0.092)	(0.126)
2nd order polynomials	−0.130*	−0.246**	−0.084	−0.062
	(0.079)	(0.097)	(0.132)	(0.189)
3rd order polynomials	−0.153	−0.249**	−0.127	−0.178
	(0.106)	(0.126)	(0.175)	(0.257)
Excluding recalls	−0.261***	−0.250***	−0.273***	−0.178
	(0.057)	(0.071)	(0.102)	(0.141)
Controlling for entire month effects	−0.293***	−0.285***	−0.344***	−0.198
	(0.054)	(0.069)	(0.102)	(0.149)
Obs.	16,692	8352	3837	1817

Note: Cox model estimation. All covariates tested in the previous section are included (except in the first line): gender, nationality, age, education, parental and marital status, residence in the Paris region, qualification, past wage, separation reason, seasonal work dummy, past employment history, preferred sector, seasonality at registry and year dummies.
*** significant at 1%.
** significant at 5%.
* significant at 1%.

Table 9
Dynamic effect of extending potential benefit duration on unemployment exit rate.
Source: FH-DADS (Dares, Insee, Pole emploi).

	All	4 months	2 months	1 month
	(1)	(2)	(3)	(4)
Without covariates				
During the first 7 months	−0.472***	−0.330***	−0.290***	−0.208
	(0.053)	(0.069)	(0.099)	(0.137)
Between 8 and 15 months	−1.084***	−0.966***	−0.980***	−0.754***
	(0.063)	(0.083)	(0.120)	(0.167)
After 16 months	−0.643***	−0.119	−0.036	−0.057
	(0.077)	(0.103)	(0.155)	(0.216)
2nd order polynomials				
During the first 7 months	−0.403***	−0.301***	−0.086	−0.142
	(0.073)	(0.098)	(0.134)	(0.189)
Between 8 and 15 months	−1.021***	−0.952***	−0.796***	−0.723***
	(0.078)	(0.106)	(0.147)	(0.208)
After 16 months	−0.568***	−0.120	0.172	−0.007
	(0.087)	(0.119)	(0.174)	(0.243)
3rd order polynomials				
During the first 7 months	−0.688***	−0.423***	−0.253	−0.400*
	(0.089)	(0.120)	(0.167)	(0.240)
Between 8 and 15 months	−1.288***	−1.068***	−0.957***	−0.966***
	(0.091)	(0.125)	(0.176)	(0.251)
After 16 months	−0.815***	−0.231*	0.024	−0.226
	(0.098)	(0.135)	(0.196)	(0.275)
Obs.	16,692	8352	3837	1817

Note: Cox model estimation. All covariates tested in the previous section are included (except in the first line): gender, nationality, age, education, parental and marital status, residence in the Paris region, qualification, past wage, separation reason, seasonal work dummy, past employment history, preferred sector, seasonality at registry and year dummies.
*** significant at 1%.
** significant at 5%
* significant at 1%.

Table 10
Effect of extending potential benefit duration on hourly wage.
Source: FH-DADS (Dares, Insee, Pole emploi).

	All	4 months	2 months	1 month
	(1)	(2)	(3)	(4)
Without covariates	−0.011	−0.012	−0.031	0.028
	(0.023)	(0.028)	(0.040)	(0.056)
Wage in level	−0.004	−0.017	−0.023	−0.029
	(0.015)	(0.018)	(0.025)	(0.035)
2nd order polynomials	−0.034	−0.023	0.063	−0.005
	(0.036)	(0.041)	(0.059)	(0.083)
3rd order polynomials	0.016	0.032	0.023	0.022
	(0.051)	(0.055)	(0.078)	(0.111)
Obs.	7391	3797	1803	830

Note: OLS estimation. Standard errors are robust to White heteroskedasticity. All covariates tested in the previous section are included (except in line 1): gender, nationality, age, education, parental and marital status, residence in the Paris region, qualification, past wage, separation reason, seasonal work dummy, past employment history, preferred sector, seasonality at registry and year dummies. Covariates capturing the seasonality and the business cycle at the exit date are also included.

Table 11
Effect of extending potential benefit duration on employment survival at 8 months.
Source: FH-DADS (Dares, Insee, Pole emploi).

	All	4 months	2 months	1 month
	(1)	(2)	(3)	(4)
Without covariates	0.043*	0.073**	0.019	0.014
	(0.026)	(0.033)	(0.045)	(0.063)
2nd order polynomials	0.017	−0.006	−0.016	0.058
	(0.038)	(0.046)	(0.064)	(0.096)
3rd order polynomials	0.048	0.012	0.062	−0.106
	(0.052)	(0.060)	(0.086)	(0.137)
Obs.	7617	3913	1858	854

Note: OLS estimation. Standard errors are robust to White heteroskedasticity. All covariates tested in the previous section are included (except in line 1): gender, nationality, age, education, parental and marital status, residence in the Paris region, qualification, past wage, separation reason, seasonal work dummy, past employment history, preferred sector, seasonality at registry and year dummies. Covariates capturing the seasonality and the business cycle at the exit date are also included.
*** significant at 1%
** significant at 5%.
* significant at 1%.

Table 12
Effect of extending potential benefit duration on employment duration.
Source: FH-DADS (Dares, Insee, Pole emploi).

	All	4 months	2 months	1 month
	(1)	(2)	(3)	(4)
With covariates	−0.032	−0.047	0.039	−0.076
	(0.057)	(0.073)	(0.101)	(0.143)
Without covariates	−0.051	−0.055	0.032	−0.082
	(0.056)	(0.070)	(0.098)	(0.135)
2nd order polynomials	−0.022	0.057	−0.030	−0.037
	(0.085)	(0.104)	(0.144)	(0.203)
3rd order polynomials	−0.100	−0.017	−0.182	0.457
	(0.118)	(0.137)	(0.188)	(0.293)
Obs.	6966	3563	1689	777

Note: Cox model estimation. “Regression discontinuity” polynomials in the distance between the threshold and the forcing variable are first-order. All covariates tested in the previous section are included: gender, nationality, age, education, parental and marital status, residence in the Paris region, qualification, past wage, separation reason, seasonal work dummy, past employment history, preferred sector, seasonality at registry and year dummies. Covariates capturing the seasonality and the business cycle at the exit date are also included.

Table 13
Effect of extending potential benefit duration on match quality 2 years after registry.
Source: FH-DADS (Dares, Insee, Pole emploi).

	All	4 months	2 months	1 month
	(1)	(2)	(3)	(4)
Without covariates	−0.010	0.032	−0.019	0.007
	(0.029)	(0.034)	(0.050)	(0.064)
2nd order polynomials	−0.040	−0.019	0.050	−0.108
	(0.045)	(0.052)	(0.069)	(0.080)
3rd order polynomials	0.033	0.037	−0.043	−0.154
	(0.060)	(0.066)	(0.083)	(0.124)
Obs.	4546	2229	1058	489

Note: OLS estimation. Standard errors are robust to White heteroskedasticity. “Regression discontinuity” polynomials in the distance between the threshold and the forcing variable are first-order polynomials in the first line. All covariates tested in the previous section are included: gender, nationality, age, education, parental and marital status, residence in the Paris region, qualification, past wage, separation reason, seasonal work dummy, past employment history, preferred sector, seasonality at registry and year dummies.

Table 14
Effect of extending potential benefit duration on earnings 2 years after registry.
Source: FH-DADS (Dares, Insee, Pole emploi).

	All	4 months	2 months	Optimal	1 month
	(1)	(2)	(3)	(4)	(5)
Extending PBD	−0.403**	−0.286	−0.371	−0.363	−0.301
	(0.163)	(0.202)	(0.299)	(0.364)	(0.423)
Obs.	16,692	8352	3837	2514	1817

Note: the earnings outcome is the hourly wage for workers employed 2 years after registry and set to zero for unemployed individuals. OLS estimation (model (6)). Standard errors are robust to White heteroskedasticity. “Regression discontinuity” polynomials in the distance between the threshold and the forcing variable are first-order. All covariates tested in Table 1 are included: gender, nationality, age, education, parental and marital status, residence in the Paris region, qualification, past wage, separation reason, seasonal work dummy, past employment history, preferred sector, seasonality at registry and year dummies. Covariates capturing the seasonality and the business cycle at the exit date are also included.
*** significant at 1%
** significant at 5%.
* significant at 1%

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