Unemployment Insurance and Reservation Wages: Evidence from Administrative Data

Thomas Le Barbanchon Roland Rathelot Alexandra Roulet*

September 2016
PRELIMINARY VERSION

Abstract

Abstract: Although the reservation wage plays a central role in job search models, empirical evidence on the determinants of reservation wages, including key policy variables such as unemployment insurance (UI), is scarce. In France, unemployed people must declare their reservation wage to the Public Employment Service when they register to claim UI benefits. We take advantage of these rich French administrative data and of a reform of UI rules to estimate the effect of the potential benefit duration (PBD) on reservation wages and on other dimensions of job selectivity, using a difference-in-difference strategy. We cannot reject that the elasticity of the reservation wage with respect to PBD is zero. Our results are precise and we can rule out elasticities larger than 0.006. Furthermore, we do not find any significant effects of PBD on the desired number of hours, duration of labor contract and commuting time/distance. The estimated elasticity of actual benefit duration with respect to PBD of 0.3 is in line with the consensus in the literature. Exploiting a regression discontinuity design as an alternative identification strategy, we find similar results. Our findings indicate smaller effects of PBD on reservation wages than predicted by a calibrated non-stationary job-search model with endogenous search effort.

Keywords: unemployment insurance, reservation wage. **JEL Codes:** J64 J65

*Le Barbanchon: Bocconi University, CEPR and IGIER. Rathelot: University of Warwick and CEPR. Roulet: Harvard University. For very helpful comments, we thank Jérôme Adda, Philippe Aghion, Patrick Arni, Gary Chamberlain, Raj Chetty, David Cutler, Simon Jaeger, Xavier Jaravel, Lawrence Katz, Rocco Macchiavello, Andreas Mueller, Nicola Pavoni, Arne Uhlendorff and seminar participants at Bocconi University, CEPR/IZA ALES 2016, CREST, Harvard Labor Lunch, HKS and TSE. We are grateful to Pôle Emploi, and in particular Anita Bonnet, for letting us access their data. The views expressed herein are those of the authors and do not necessarily reflect those of Pôle Emploi. We gratefully acknowledge financial support from la Chaire de Securisation des Parcours Professionnels. All errors are our own.

1 Introduction

In standard job search models, unemployed people receive job offers that they decline or accept depending on the value of the offered job relative to the value of unemployment (McCall, 1970). Their search strategy is then described by one key concept: the reservation wage, i.e. the minimum wage above which unemployed people accept job offers. Despite its crucial role in job search models, the reservation wage is rarely observed and empirical evidence on its determinants remains scarce (Feldstein and Poterba, 1984; Koenig et al., 2014; Krueger and Mueller, 2016). In particular, the existing literature has still no precise estimate of the effect of more generous unemployment benefits on reservation wages. Our paper contributes to filling this gap.

We take advantage of unique administrative data on reservation wages and of a quasi-experimental research design. In France, when individuals register as unemployed at the public employment agency, they have to declare their reservation wage and other information on the job they are looking for, such as commuting time/distance and number of hours and type of labor contract (temporary vs. long-term). We use these data and leverage a reform of the rule used to compute the number of days of unemployment benefits people are entitled to, i.e. the Potential Benefit Duration (PBD). This reform altered the PBD for some claimants groups while leaving it unchanged for some others, depending on their previous work tenure. Using this natural experiment, we compute difference-in-difference estimates of the elasticity of reservation wages with respect to PBD. We also consider an alternative identification strategy taking advantage of a discontinuity in age, at age 50, in the PBD schedule. Both identification strategies deliver converging results, which point to the lack of responsiveness of reservation wages and other dimensions of job selectivity to the potential duration of benefits. In order to interpret this result, we compare our elasticity estimates to theoretical elasticities simulated with a standard non-stationary job search model.

While the previous literature is based on survey data (Feldstein and Poterba, 1984; Koenig et al., 2014; Krueger and Mueller, 2016), we use administrative data on reservation wages. Our data have thus several strengths: large sample size, no missing values due to non-response and precise measure of UI-related policy variables and past labor outcomes, such as past tenure or past wages. On top of these strengths, the data also enable us to follow individuals over multiple claims, so that we observe repeated measures of reservations wages for a given individual. We verify that claimants stating higher reservation wages draw on UI benefits for a longer time period, holding constant the claimants' and the claims' characteristics, and controlling for unobserved time-invariant heterogeneity of claimants (fixed effects models). This first result confirms the meaningfulness of the reservation wage stated by claimants to the UI agency.

Our main identification strategy relies on a UI reform, which occurred in 2009, but was not triggered by the Great Recession. The main objective of the reform was to simplify the rules governing the computation of the potential duration of benefits. In France, PBD is mainly determined by the claimant's previous work duration. Before the 2009 reform, PBD was a step function of past tenure. The 2009 reform simplified the rule and made it linear, so that claimants are now entitled to as many days of benefits as days of work in the previous two years. Some tenure groups benefited from the reform while others lost. Some tenure groups were unaffected and can be used as control groups in a difference-in-difference setting.

Whatever the specification, we cannot reject that the elasticity of reservation wages with respect to PBD is zero. Our results are precise: our estimation of fixed-effect models rules out elasticities greater than 0.006. The elasticity of the *actual* duration of benefits with respect to PBD, estimated at 0.3, is in line with most results of the literature. We do not find any significant effect of PBD on the maximum commuting time/distance that job seekers are willing to accept. Nor do we find any effects of PBD on the number of hours or on the type of contract job seekers are looking for. The absence of responsiveness in all dimensions of job selectivity is a strong result. While the non-responsiveness of reservation wages could have been explained by strong wage rigidity and low mobility across jobs, the fact that the willingness to find open-ended contracts and to ensure job security does not change with PBD suggests that rigid labor markets are unlikely to be the only explanation to our results.

While the elasticity of reservation wages is zero on average, we find that it amounts to a significant 0.01 for job seekers with the lowest past tenure. These job seekers are entitled to short PBD and the date when their benefits could elapse is close to their registration date when they declare their reservation wages. Consistently, we also find that the elasticity of actual benefit duration is higher for these short tenure claimants. We do not find any significant heterogeneity of the PBD elasticity of reservation wages across gender or past wage groups.

We are able to check the robustness of our main results using another identification strategy, a regression discontinuity design. When unemployed are over 50 years old at the separation date from their previous employer, they benefit from more generous PBDs, which are on average 30% longer. We find some manipulation of the separation date around the 50-year-old cutoff. Consequently, we adopt a "Donut" RDD strategy, which excludes observations in a window around the cutoff of the running variable. As with our main difference-in-difference strategy, we cannot reject that the PBD elasticity of reservation wages is equal to zero, while the elasticity of actual benefit duration is around 0.2.

Lastly, we compare our estimated elasticity of reservation wages with the predictions of a canonical job search model. We consider the non-stationary job search model of van den Berg (1990) and add an endogenous decision about search effort. We calibrate the job search model using various moments, including the PBD elasticity of the actual benefit duration, but excluding the elasticity of reservation wages. In qualitative terms, the model predicts that longer PBD are associated with lower elasticities of the reservation wages measured at the beginning of the spell, which is consistent with the empirical heterogeneity we find. Quantitatively, the model predicts that the elasticity of reservation wages declared at the beginning of an unemployment spell is around .03 for a job seeker with 12 months of PBD, which is the median in our sample. This predicted elasticity is too large to be consistent with our empirical results, which rule out average elasticities above 0.006. Even for short PBD groups, the expected magnitude (around 0.04) lies outside the 95% CI interval centered on our estimated elasticity.

Our paper is, to the best of our knowledge, the first one to obtain precise quasi-experimental estimates of the effect of more generous UI on self-reported reservation wages and other dimensions of job selectivity. Most previous contributions could not rely on credible exogenous variations in UI generosity and find mixed results. Feldstein and Poterba (1984) finds a large elasticity of reservation wages to benefit levels, while Krueger and Mueller (2016) cannot reject that this elasticity is equal to zero.[1]

The absence of reservation wage responsiveness sheds lights on the current debate on the effect of UI on accepted wages (Card et al., 2007; Le Barbanchon, 2016; Schmieder et al., 2012b; Nekoei and Weber, 2014). Estimates of the effect on accepted wages cannot disentangle two mechanisms: changes in the job offer distributions along the job search spell and changes in the job seekers' pickiness among this distribution. Our design enables us to hold constant the job offer distribution, as we analyze job seekers at the beginning of their unemployment spell. Our results then show that there are hardly any changes in pickiness/job selectivity at the beginning of the job search spell.

Finally, our results contribute to the debate on the optimality of unemployment insurance. Shimer and Werning (2007) show that the elasticity of reservation wages to unemployment benefits is key to assess the optimality of UI. Our empirical results suggest that the marginal benefits of increasing PBD, captured by the elasticity of the reservation wage, are on average non-significant, while the marginal costs, revealed by the elasticity of the actual benefit duration, are large. This suggests that the average PBD is larger than the optimal level of UI generosity in France.

[1] One recent exception is Lichter (2016) who uses a quasi-experimental setting to analyze the effect of PBD on job search effort, but also on reservation wages. His results on reservation wages are consistent with ours, although less precise because of the small sample size of his survey data.

Section 2 describes the 2009 UI reform and the data. Section 3 presents our DiD strategy and provides a graphical overview of the results. Section 4 presents the main estimates of the effects of PBD on job selectivity and the analysis of their heterogeneity. Section 5 deals with our alternative RDD strategy. Section 6 presents the theory used to interpret our empirical results. Section 7 concludes.

2 Research design and data

2.1 Research design

Our identification strategy relies on a change in UI rules that occurred at the beginning of 2009. The 2009 reform was not triggered by the Great Recession. In France, UI rules are renegotiated by unions and employers' organizations every three years. The renegotiation was thus expected but the nature of the change was not anticipated. The objective of the 2009 change was to simplify the computation of Potential Benefit Duration (PBD), i.e. the number of days of benefits workers are entitled to when they become unemployed. Both before and after the change, PBD depends on the number of days worked in the previous years but in a different way. In the current section, we restrict ourselves to workers less than 50 years old. The UI rules are more generous for workers of more than 50 years old, a group on which we focus in our alternative identification strategy in Section 5. For people less than 50, before the reform, PBD could take three values: i) 213 days if the unemployed had worked between 182 and 365 days (excl.) in the previous 26 months, ii) 365 days if she had worked between 365 and 487 days (excl.); and iii) 700 days if she had worked 487 days or more. After the reform, the unemployed are entitled to as many days of benefits as they worked in the reference period, with a cap at 730 days. Figure 1 shows this shift from a step function to a linear function. Some tenure groups benefited from the change, whereas others lost from it, and within winners and losers, some tenures were associated with larger changes than others. The new UI rule applies to anyone whose contract terminated after March 31st 2009.[2]

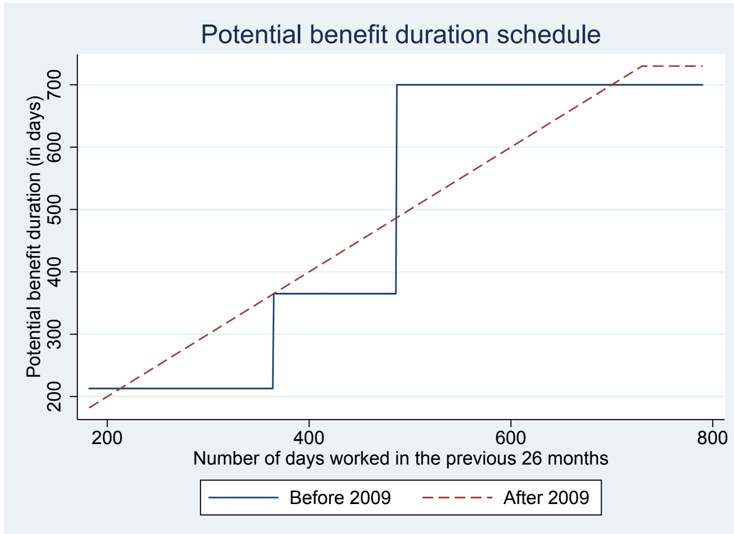
2.2 Sample selection

From the administrative records of the Public Employment Service (PES), we select the inflow of job seekers registering for a new UI claim from 2006 to 2012.[3] We focus on claimants

[2] People whose contract terminated before and who are still unemployed on March 31st 2009 stay entitled to the old rule.

[3] New claims correspond both to first claims ever and to situations when a worker registers again and has no benefits left from her previous claim.

Figure 1: Schedules of PBD, before and after the 2009 reform



of the regular UI rules, excluding senior workers over 50 years old.[4] We restrict the sample to claimants that were previously employed full-time. This resolves any ambiguity about the reservation wage question (see below). Finally, we trim the sample excluding observations with extreme past wages or reservation wages.[5]

We further select unemployed people whose pre-unemployment contract terminated between April 1st 2006 and March 31st 2012 - 3 years before and after the 2009 reform. Last, we focus on claimants with exactly two claims.[6] This enables us to control in our analysis for time-invariant unobserved heterogeneity, esp. in productivity. As will be clear in Section 2.3, focusing on repeated claimants is important in our context and helps to make sense of the link between reservation wages and unemployment duration. Our final sample comprises around 180,000 claims, this represents 9% of claims in our initial sample. Some summary statistics for our sample of interest can be found in Appendix Table A1.

[4]This also means that we exclude workers from temporary help agencies -*interimaires*- and workers from the culture and art sectors - *intermittents du spectacle*.

[5]Observations with reservation wages or past wages below the minimum wages are excluded. We also exclude observations whose past wage is over 3,400 euros, or whose reservation wage is over 3,200 euros (95% percentile). We trim observations with a ratio of the reservation wage over the previous wage that is below 0.4 or above 3 (the first and last percentile).

[6]There are very few individuals with 3 claims or more. We drop them from the longitudinal sample. They account for less than 1% of all claims

2.3 Reservation wage data

When registering as unemployed in France, people are asked the type of job they are looking for and their reservation wage for this job. Figure 2 is a screenshot of the online registration form. The question is stated in these terms: "What minimum gross wage do you ask for?". People indicate an amount and choose the unit in which they are reporting their reservation wage: hourly, monthly or annual. Before answering the reservation wage question, they are asked what occupations they are looking for. These occupations may be different from their previous job and we believe they provide some kind of anchor for the reservation wage question. Job seekers cannot move on to the next page without having reported their reservation wage.

Reservations wages are asked, along with questions about desired hours, type of labor contracts (long-term vs temporary contrats), commuting times/distances, to help the PES caseworkers choose the type of vacancies that will be sent to each job seeker.[7] If browsing through vacancies is costly for job seekers, basic theory suggests that the best response of job seekers is to reveal their true reservation wage to the PES.[8] We are also confident that the monitoring/sanctioning role of the PES does not lead job seekers to lie about their reservation wages. Indeed, when controlling the search effort of job seekers, the legal rule requires to compare the posted wages of vacancies to which job seekers apply, to their *past wage* – and not to their reservation wages.[9] Moreover anecdotal evidence suggest that sanctions for failing to apply to reasonable job vacancies are never implemented.

Given that the reservation wage question is not explicit about whether it relates to full-time or part-time jobs, we focus on job seekers previously working full-time. Then almost all job-seekers in our sample look for a full-time job (see Section 2.4) and the reservation wage data can be interpreted as being in relation to a full-time job.

Figure 3 shows the distribution of the monthly reservation wage.[10] The upper panel displays the reservation wage (in nominal terms). In the intermediate panel, the reservation wage is divided by the minimum wage at the time of registration. The lower panel displays the reservation wage divided by the pre-unemployment wage. Figure 3 shows that 35% of

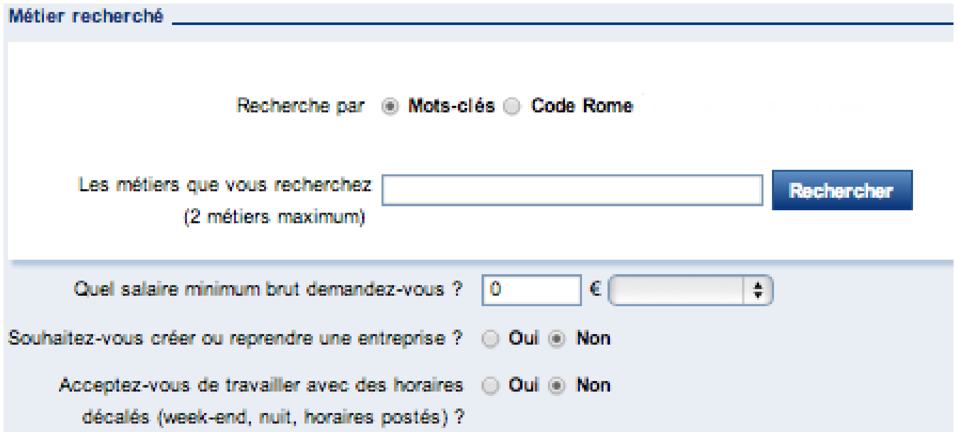
[7] The fact that these variables are used for this purpose is confirmed here `http://www.pole-emploi.fr/candidat/le-projet-personnalise-d-acces-a-l-emploi-@/article.jspz?id=60640`

[8] In case of incomplete information, this conclusion may not hold. Assume for instance that job seekers do not know for certain the distribution of wage offers when they answer the question. In this case, they may be tempted to declare a lower reservation wage than their true one, to learn about the distribution. Interestingly though, shorter PBD would then lead job seekers to declare even lower reservation wages, as they need to learn more quickly. This would amplify the impact of PBD on the observed reservation wage, which is at odds with our results.

[9] See `https://www.legifrance.gouv.fr/eli/loi/2008/8/1/ECEX0812043L/jo/texte` for more details.

[10] We convert in monthly terms wages declared in hours or in annual terms, using the legal number of working hours for full-time employees.

Figure 2: Screenshot of the section dedicated to reservation wage on the PES website at registration



people report the minimum wage as their reservation wage. The minimum wage is high in France[11] and applies to a large share of the workforce: 40% of the unemployment spells of our sample are associated with a previous wage that is inferior or equal to 1.2 times the minimum wage. Dividing the reservation wage by the past wage in the lower panel is a first attempt to control for individual heterogeneity. It captures whether people are being more or less picky, for instance whether they are willing to accept a wage cut. 70% of job seekers would accept a job that pays less than their previous job. The median of the reservation wage rate is 0.93. The distribution of the reservation wage rate in our data, is close to the one obtained by Feldstein and Poterba (1984) or Krueger and Mueller (2016).

One potential concern to address is the extent to which, conditional on the previous wage, the reservation wage conveys some additional information. It could be for instance that the reservation wage is anchored on the previous wage with some noise. We show that the reservation wage carries information. First, the reservation wage is meaningfully correlated with workers' characteristics. Second, we show that the reservation wage helps to predict actual benefit duration, even conditional on past wages. In the remainder of the text, we may also use the term "unemployment duration" for actual benefit duration.

First, Table 1 shows how workers' characteristics are correlated with the reservation wage, controlling non-parametrically for the previous wage.[12] Conditional on the previous wage, women, who might be less confident, tend to have a lower reservation wage. On the contrary, age, experience and education all lead to a higher reservation wage.

Second, we show that higher reservation wages predict longer unemployment duration.

[11] For instance, in 2012, the gross monthly minimum wage was 1400 euros.

[12] We split the sample in 20 equal-sized bins in terms of previous wage and use dummies for these bins

Figure 3: Distribution of reservation wages

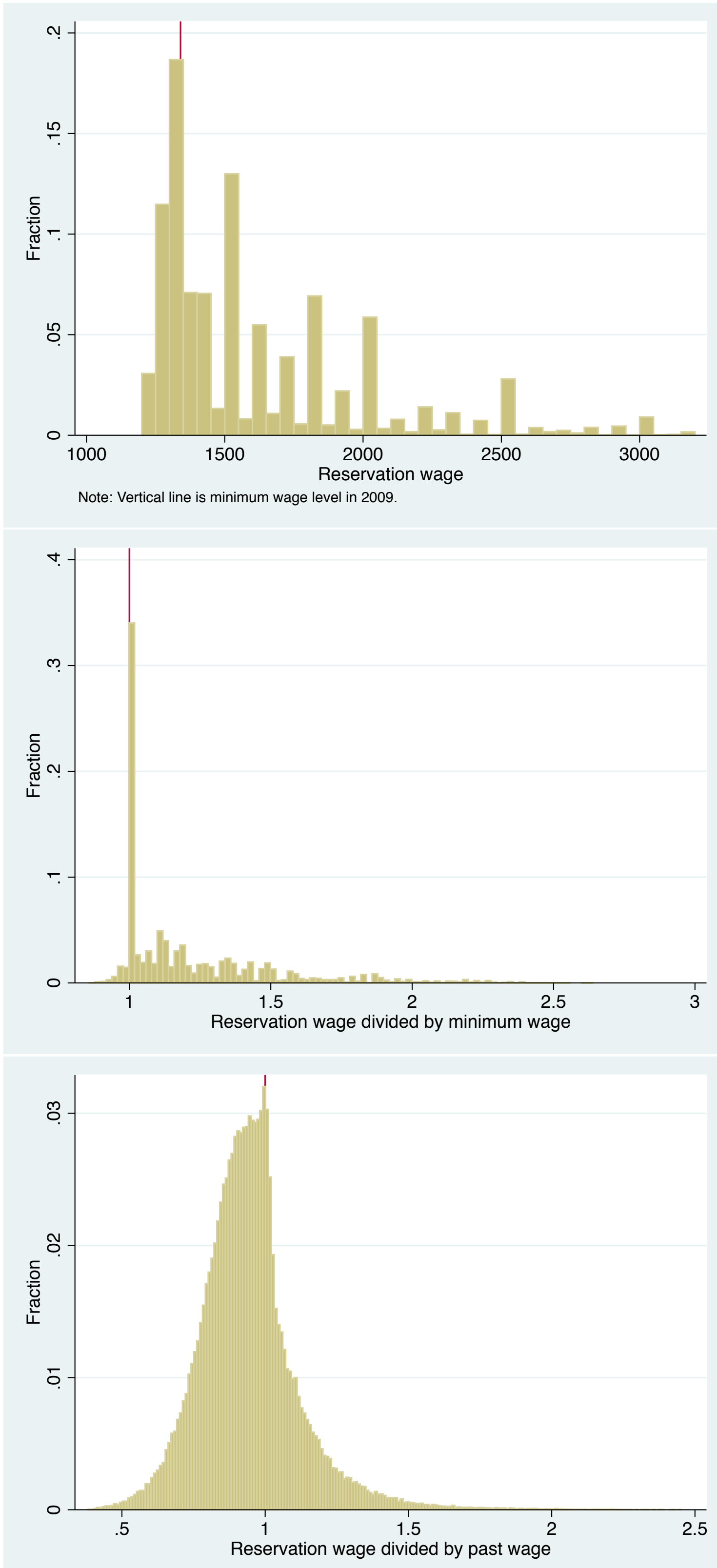


Table 1: Socio-demographic determinants of reservation wages

	Reservation wage	Reservation wage over past wage
Dummies for 20 equal sized bins of past wage	x	x
Female	-44.52***	-0.0289***
	(1.650)	(0.000904)
Married * Female	-20.86***	-0.0129***
	(2.000)	(0.00107)
Married * Male	39.15***	0.0220***
	(2.043)	(0.00111)
Age	2.510***	0.00148***
	(0.101)	(5.53e-05)
Experience	8.131***	0.00456***
	(0.171)	(9.16e-05)
Education	25.99***	0.0141***
	(0.248)	(0.000138)
Obs.	180,637	180,637
R-squared	0.455	0.237

Source: FNA-FH (Pole emploi).
Note: Estimates from an OLS regression. Robust standard errors in parentheses.*** p<0.01, ** p<0.05, * p<0.1.

One concern is that the empirical relationship between the reservation wage and unemployment duration, controlling for observables, is likely to reflect both the causal effect of the reservation wage on unemployment duration and the influence on the reservation wages of unobservables that are also correlated with unemployment duration . In particular, workers who are more productive are likely to have better prospects and thus higher reservation wages. But they are also more likely to stay unemployed for shorter periods. The unobserved heterogeneity in productivity is thus likely to create a negative relationship between reservation wage and unemployment duration, holding observables constant. We test whether this unobserved heterogeneity matters by estimating the following equation with and without individual fixed effects:

$$\log Dur_{i,n} = Indiv.F.E._i + \delta \log resW_{i,n} + \beta X_{i,n} + \epsilon_{i,n} \quad (1)$$

where $Dur_{i,n}$ is the duration of the n-th claim of individual i; $resW_{i,n}$ is the reservation wage declared by individual i at the beginning of her n-th claim; $X_{i,n}$ includes the log of PBD, time fixed effects (quarterly date of registration), 50 past wage bins, age etc. (see footnote of Table 2). First, the OLS coefficient without individual fixed effects (Column (1) in Table 2) is negative and equal to -0.16. This suggests that the unobservable productivity component of reservation wages dominates the correlation. Then we estimate the fixed effects regression and find a positive significant coefficient of 0.28 (Column (2) in Table 2). Once we control for unobserved (time invariant) heterogeneity using the individual fixed effects, the relationship between unemployment duration and reservation wages only reflect the differences between claims in the work v. non-work valuation. The comparison between Column (1) and Column (2) of Table 2 shows the importance of controlling for unobserved (time-invariant) heterogeneity. This justifies our choice of selecting a sample of job seekers with repeated claims, which enables fixed effects estimation. While this selection is admittedly important – recurrent job-seekers are probably less productive than one-time unemployed –, the OLS coefficient of Column (1) in Table 2 does not change much when it is estimated on the whole sample (-0.21 vs. -0.16).

Table 2: Unemployment duration and reservation wage

	Log unemployment duration	
	OLS	FE-OLS
	(1)	(2)
Log reservation wage	-0.155***	0.277***
	(0.0149)	(0.0337)
Obs.	180,637	180,637
R-squared	0.063	0.091

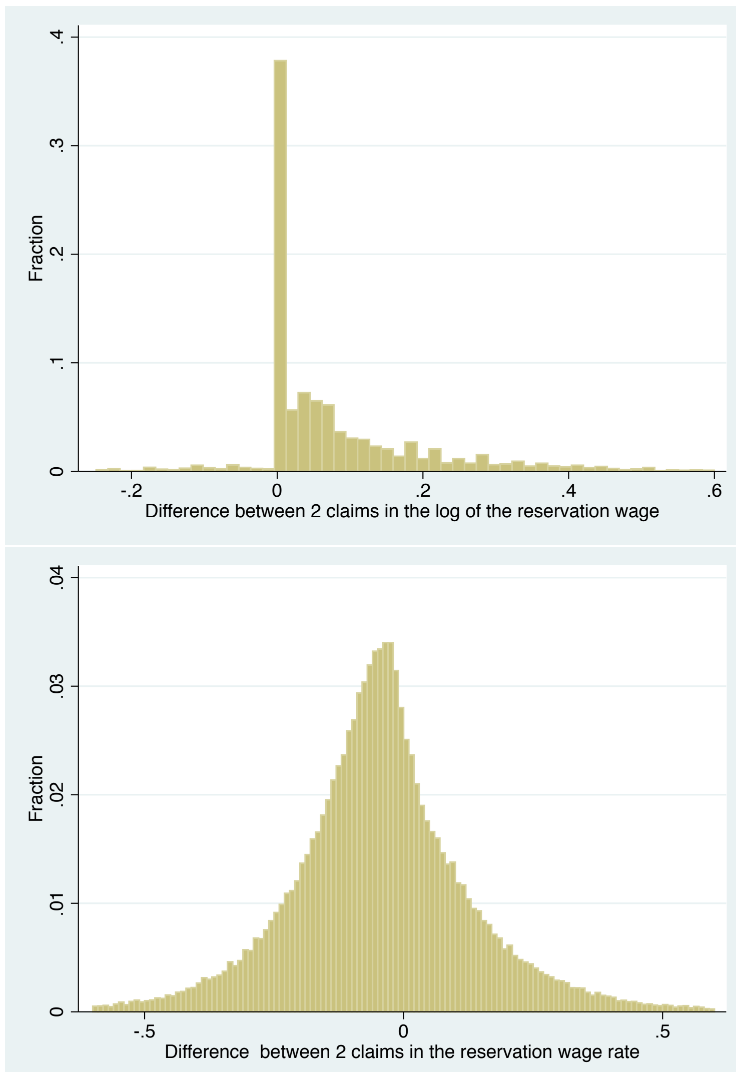
Source: FNA-FH (Pole emploi).
Note: Estimates from an OLS regression. Are included in the specification time fixed-effects as well as individual controls: log of PBD, 50 past wage bins, gender, gender interacted with family status dummies (married, divorced, widow, having kids), foreign born, age and age square, experience and experience square, education, reason of separation from previous job, quarterly inflows of new job seekers and of vacancies in the commuting zone (both in logs). Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$.

Figure 4 shows the distribution of the difference between the reservation wages declared by the same individual at the beginning of his first and second spell over our period of analysis. As the figure shows, although 35% of job seekers declare the same reservation wage for their two unemployment spells, there is still some variability that we relate to exogenous variations in PBD in Sections 3 and 4.

2.4 Data on other dimensions of job selectivity

During the registration process and after stating their reservation wage, job seekers are also asked about other characteristics of the job they are looking for. They are asked the type of contract and the type of hours they would like. In France, there is a strong duality in the labor market between jobs with open-ended contracts and jobs with temporary con-

Figure 4: Within individual variation in the reservation wage



Source: FNA-FH (Pole emploi). Note: The reservation wage rate is the ratio of the reservation wage over the previous wage.

tracts. Open-ended contracts ensure that workers benefit from a high level of employment protection: we call them long-term contracts. Table 3 shows that almost 90% of job seekers declare that they are looking for such contracts. This is in sharp contrast with the share of job seekers separating from a long-term contract, which amounts to 35% (Appendix Table A1). Table 3 also shows that 97% of job seekers are looking for a full-time job. This high share is due to our sample selection: we restrict to job seekers in a full-time job before separation.[13]

The bottom lines of Table 3 reveal to what extent job seekers are mobile. During the registration process, job seekers are asked about the maximum distance/time that they would be willing to commute (one way). Job seekers can answer either in terms of commuting time, or commuting distance in kilometers. The majority choose to give a commuting distance. On average, they declare that they would accept a job that is in a 32 km radius around their home. For those who give a commuting time, on average they would accept a maximum of 44 minutes of commute. Finally, 2% of job seekers declare that they would accept a job anywhere in France. When estimating the effect of UI on mobility, we will abstract from this last category.

Lastly, even if job seekers are asked about the occupation they are looking for, we do not analyze this dimension as we do not have any information about the occupation of the job seeker in their previous job. Such information is essential to compute a meaningful measure of mobility across occupations.

Table 3: Reservation Strategy - all dimensions

Variable	Mean	Std. Dev.	N
Past Wage (gross monthly, in euros)	1721.631	388.383	180,670
Unemployment Benefits (gross monthly, in euros)	1006.869	226.521	180,670
Reservation Wage (gross monthly, in euros)	1599.958	382.129	180,670
Looking for a long-term contract	0.895	0.307	180,670
Looking for a full-time job	0.971	0.167	180,670
Maximum commute time accepted (in minutes)	44.441	19.974	53,880
Maximum commute distance accepted (in kilometers)	31.538	24.4	109,620
No geographical constraint	0.02	0.138	180,670

Source: FNA-FH (Pole emploi).
Note: The question on commuting time/distance is not mandatory, so that the corresponding variable is missing for 7% of our sample.

[13]Recall that this restriction resolves any ambiguity about whether job seekers declare a reservation wage for a full-time or a part-time job.

3 Identification strategy and graphical overview of the effects

Our identification strategy relies on exogenous variations in the Potential Benefit Duration (PBD) triggered by the 2009 reform of the Unemployment Insurance (UI) rules. Whereas Figure 1 displayed the theoretical change in PBD, Figure 5 exhibits the variation in PBD as we observe it in the data. More precisely we plot the α_j coefficients from the following regression

$$PBD_{i,n} = \sum_{j=6}^{26} \alpha_j D(Tenure_{i,n} = j) \times After_{i,n} + \sum_{j=6}^{26} \delta_j D(Tenure_{i,n} = j) + Indiv.F.E. + \nu_{i,n} \quad (2)$$

where i, n denotes the n-th unemployment spell of individual i. $Tenure_{i,n}$ is the number of months worked in the 26 months before the beginning of this spell.[14] $After_{i,n}$ is a dummy variable equal to 1 if the last job before the claim terminated after March 31st 2009.

Figure 5: Treatment: Potential Benefit Duration, in levels (days)

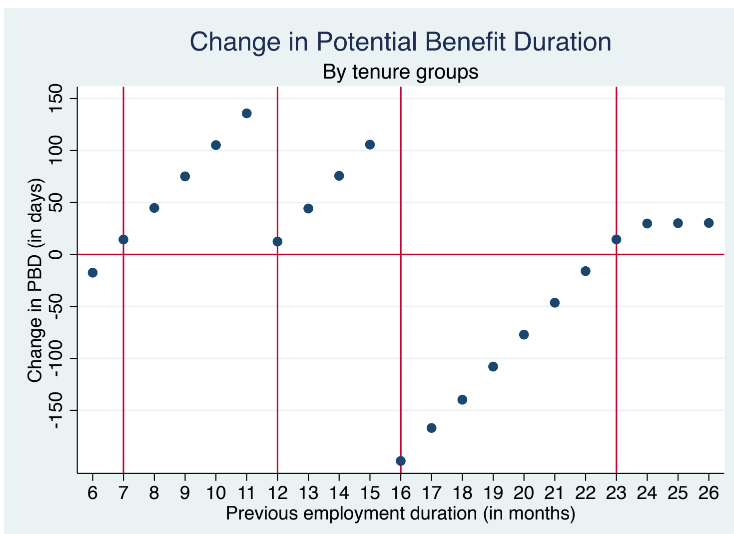


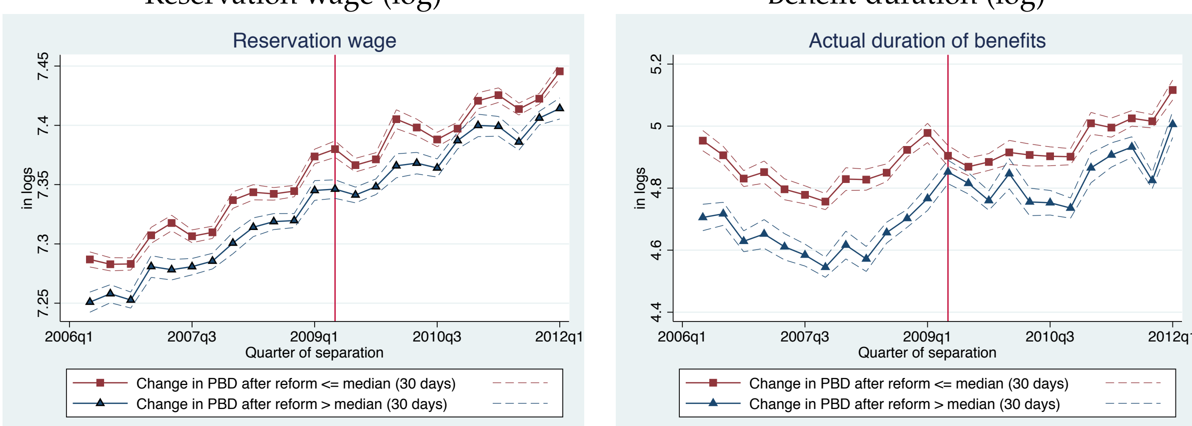
Figure 5 highlights that our identification strategy can be thought of as a difference-in-difference analysis. For job seekers with past tenure equal to 7 months, 12 months and 23 months, PBD according to the 2009 rules is just 15 days over what it would have been under the 2006 rules. These tenure groups constitute our control groups. All other tenure groups experience more substantial changes in PBD. They are treated by the 2009 reform. Several assumptions are needed to ensure that our design actually identifies the effect of PBD.

[14]The past employment duration which is available in days in the data, is rounded down in number of months.

The main assumption is the common trends assumption. Figure 6 presents a graphical check of the validity of this assumption. Because we have a continuous treatment, for the purpose of this test only, we constitute two equal-sized groups based on the size of the change triggered by the reform. The median change is a 30 days increase in PBD. We thus split the sample between tenure groups that are above and below the median. For these two groups, we plot in Figure 6 the evolution over time of the main outcomes of interest: the reservation wage and the *actual* duration of benefits. The red line with squares shows the evolution of these outcomes for workers whose tenure would make them lose from the reform or gain less than 30 days. The blue line with triangles shows the evolution for workers whose tenure would make them gain more than 30 days of PBD. Reassuringly Figure 6 shows roughly parallel trends before the reform, for both outcomes. It actually also previews that the actual duration of benefits seems to have been affected by the reform whereas this is not the case for the reservation wage. We will show this effect more rigorously in what follows.

Figure 6: Common trends

Reservation wage (log) | Benefit duration (log)



Related to the common trends assumption, one may be concerned that the DiD groups are defined according to an endogenous variable: past tenure. We verify in Appendix Figure A1 that the tenure distribution is similar over time and does not seem to be affected by the 2009 reform.[15] In Section 4, we also show results of placebo tests that confirm our identification strategy. Finally, the Stable Unit Treatment Value Assumption (SUTVA) states that each tenure group is not affected by the fact that workers in other tenure groups are treated differently. Lalive et al. (2015) show that UI affects equilibrium conditions on the labor market. Compared to other reforms that have been studied, the 2009 reform is

[15]Appendix Figure A2 shows a further test. We compute from the tenure distributions the average PBD according to both the 2006 and 2009 rules. We then show that the evolution of the average PBD according to both rules is parallel over time.

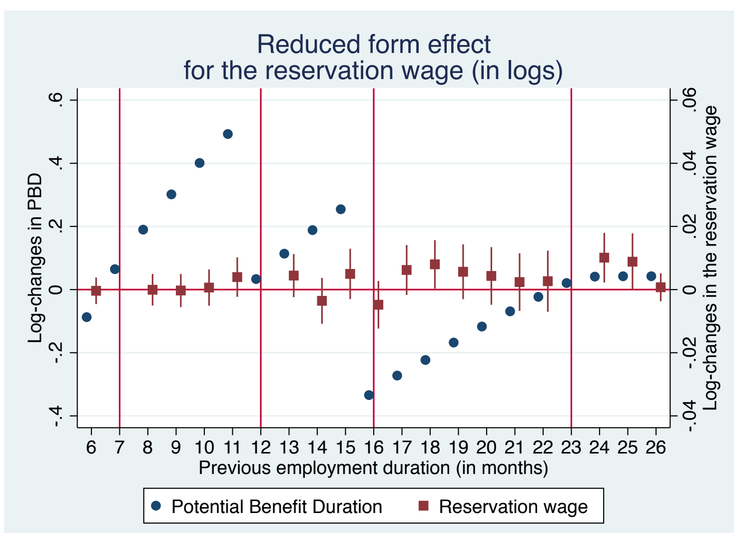
probably less likely to violate the SUTVA as there are both winners and losers, so that the overall generosity of the system is not much affected.

We now turn to explore the reduced form effect of the reform, tenure group by tenure group. Figure 7 shows the reduced-form effect on the reservation wage and Figure 8 on the actual duration of benefits. We plot the β_j coefficients from the following regression.

$$\log Y_{i,n} = \sum_{j=6,excl.7,12,23}^{26} \beta_j D(Tenure_{i,n} = j) \times After_{i,n} + \sum_{j=6,excl.7,12,23}^{26} \delta_j D(Tenure_{i,n} = j) + \gamma X_{i,n} + Year \times QuarterF.E. + Indiv.F.E._i + \nu_{i,n} \quad (3)$$

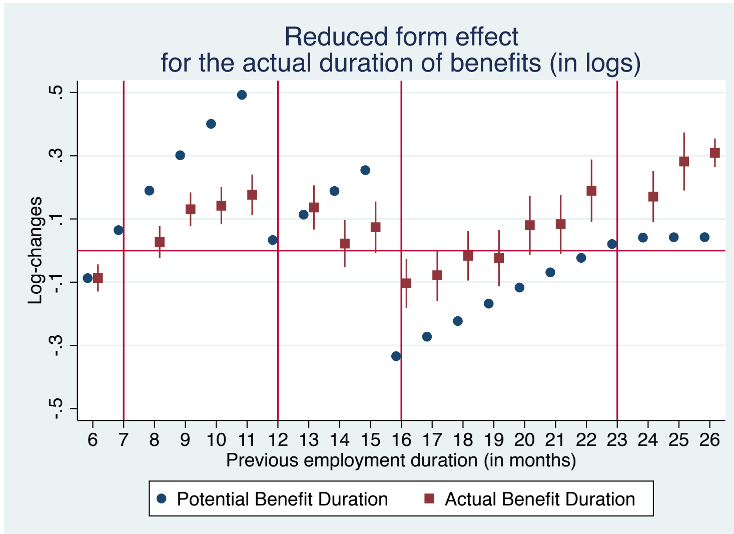
$Y_{i,n}$ is either the reservation wage or the number of days of benefit receipt for the n-th claim of individual i. $X_{i,n}$ controls for the job seeker's observable characteristics. These include gender, age and experience, age square and experience square, number of years of schooling, marital status and number of children, a dummy for being foreign born, and dummies for 20 bins of the previous wage.[16] We also include dummy variables for the year $\times$ quarter at which the previous job ended. We show the results with the individual controls and the year $\times$ quarter fixed effects but the patterns are similar without these individual controls (and replacing the time fixed effects by the time dummy for the reform $After_{i,n}$. Figures 7 and 8 show that whereas the actual benefit duration seems to respond to the change in PBD, the reservation wage is barely affected.

Figure 7: Reduced form effect of the reform on the reservation wage



[16]Obviously, time-invariant characteristics only matter when we run some specifications without individual fixed effects in the next Section.

Figure 8: Reduced form effect of the reform on the actual duration of benefits



4 Main results

In this section, we present our main estimates of the effect of the Potential Benefit Duration (PBD), first on the reservation wage, and on the actual benefit duration, second on the other dimensions of job selectivity. We then present our heterogeneity analysis.

4.1 Effect of PBD on the reservation wage and on the actual benefit duration

We now estimate the elasticity of the reservation wage and of the actual benefit duration with respect to PBD. We consider the following structural model:

$$\log Y_{i,n} = Indiv.F.E._{i} + \alpha \log PBD_{i,n} + \sum_{j=6, excl.7,12,23}^{26} \delta_j D(Tenure_{i,n} = j) + \gamma X_{i,n} + Year \times Quarter F.E. + \epsilon_{i,n} \quad (4)$$

where all notations have been previously defined. Table 4 shows the elasticity estimates (α) of the reservation wage in the upper panel, and of the actual benefit duration in the lower panel. In Column (1), we report the elasticity estimated by OLS without individual fixed effects. In Column (2), we use the 2009 reform as an instrument for PBD. More precisely, we instrument $\log PBD_{i,n}$ by the set of tenure group dummies interacted with the dummy indicating that the reform has taken place: $\forall j \in [6, 26], D(Tenure_{i,n} = j) \times After_{i,n}$. In Columns (3) and (4), we include individual fixed effects, respectively in the OLS and IV estimations. Standard errors are robust and clustered by monthly tenure group in Columns

(1) and (2).

Table 4: Elasticity of the reservation wage and benefit duration with respect to PBD

	OLS (1)	IV (2)	FE (3)	FE,IV (4)
	Log of reservation wage			
log PBD	0.000954	0.00473	-0.000132	-0.000535
	(0.00854)	(0.00691)	(0.00310)	(0.00318)
Obs.	180,637	180,637	180,637	180,637
R-squared	0.474	0.474	0.340	
	Log of actual benefit duration			
log PBD	0.227***	0.232***	0.314***	0.306***
	(0.0274)	(0.0257)	(0.0317)	(0.0325)
Obs.	180,637	180,637	180,637	180,637
R-squared	0.062	0.062	0.095	
Indiv. FE	no	no	yes	yes

Source: FNA-FH (Pole emploi)
Note: Robust standard errors in parentheses. Standard errors clustered by monthly tenure group in Columns (1) and (2). *** p<0.01, ** p<0.05, * p<0.1. Individual controls, time fixed-effects and tenure fixed-effects are included in all specifications.

The estimates of the elasticity of the reservation wage are not statistically different from 0, whatever the specification. The estimation is very precise, especially when individual fixed effects are introduced. The standard errors in Columns (3) and (4) enable us to rule out that the elasticity is greater than 0.006, i.e the upper bound of the 95% confidence interval.

We perform placebo tests, detailed in Appendix Table A2. We consider four placebo reforms dates: end of March 2007, 2008, 2010 and 2011. None of the placebo reforms yields significant effects on the reservation wage. This support the common trend assumption of our identification strategy.

The absence of responsiveness of the reservation wage is in sharp contrast with the estimates of the elasticity of the actual benefit duration. These estimates range from 0.22 in Columns (1) and (2) to 0.33 when individual fixed effects are introduced in Columns (3) and (4). All point estimates are statistically significant. The IV strategy hardly affects the elasticity estimates. Introducing individual fixed effects increases the elasticity estimates by 50%. This points to positive selection into PBD. More productive individuals, who tend to leave fast the UI rolls, also tend to have longer PBD. Overall, our elasticities are in line with the literature: they are lower than the 0.5-0.6 elasticity reported in Schmieder et al. (2012a), but higher than the usual 0.1-0.2 estimates of the elasticity of non-employment duration.

4.2 Effect of PBD on other dimensions of job selectivity

Table 5 shows the effect of PBD on other job characteristics that the job seeker would like. It reports the estimates of α in Equation (4) where we consider as outcome either a dummy indicating whether the job seeker looks for a long-term contract (Column 1), or a dummy indicating whether she looks for a full-time job (Column 2), or a composite measure of the willingness of the job seeker to commute (Column 3). At registration, job seekers can answer the question about mobility in terms of commuting time or commuting distance. We assume that there is a constant speed to commute, we then convert the commuting time in commuting distance and use the log of this composite measure in Column (3), where we also add as a control the declaration unit (kilometers or minutes).

In Table 5, Equation (4) is estimated according to our preferred strategy, using as an instrument the 2009 reform and controlling for individual fixed effects (and covariates). There are no statistically significant coefficients α. We can rule out small effects of PBD on these extra dimensions of job selectivity. For example, a 10% increase in PBD cannot trigger effects larger than 0.08 percentage points on the probability to look for a long-term contract. These results are robust when we estimate Equation (4) by OLS with or without fixed effects (results available upon request).

Table 5: Effect of PBD on other dimensions of job selectivity

	Looking for a		Max. commuting
	long-term contract	full-time job	time/distance (log)
	(1)	(2)	(3)
log PBD	-0.00462	0.000111	-0.000931
	(0.00825)	(0.00496)	(0.0132)
Indiv. FE	yes	yes	yes
Obs.	180,637	180,637	163,192

Source: FNA-FH (Pole emploi)
Note: Robust standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. The Table reports the coefficient α of Eq. 4, where we instrument PBD by the 2009 reform. Individual controls, time fixed-effects and tenure fixed-effects are included. The specification corresponds to that of column (4) in Table 4. There are missing values in Column (3) because the mobility question is not mandatory.

4.3 Heterogeneity analysis

In this section, we analyze the heterogeneity of the effect and we show that low-tenure job seekers, who have a short horizon before benefit exhaustion, react more to changes in PBD.

When the heterogeneity of the effect relates to time-invariant characteristics, we directly use the fixed-effect specification in stratified samples. For characteristics such as tenure or past wage which can vary across spells within individuals, we compute the average within individuals across spells and split the sample at the median of this average value.

Table 6 reports the estimates of the PBD elasticity of the reservation wage (upper panel) and of the actual benefit duration (lower panel) in various sub-samples. The elasticity estimates are obtained according to our preferred estimation strategy, instrumenting PBD with the 2009 reform and controlling for individual fixed effects. Column (1) reports the elasticity estimates for job seekers with an average past tenure below the median, which is 13 months. Their elasticity of the reservation wage is around 0.01, and statistically significant. Their horizon before benefit exhaustion is shorter and they react more to an increase in PBD. Consistently, the elasticity of the actual benefit duration is higher for job seekers with lower tenure (Column 1) than for job seekers with higher tenure (Column 2). This heterogeneity is consistent with the theoretical predictions of the canonical job search model, presented in Section 6.

We do not find any difference in the elasticity of the reservation wage across gender - Column (3) vs. Column (4) in Table 6. In the lower panel, the elasticity of the actual benefit duration is higher for females than for males - a standard result in the literature on labor supply-, although the difference in elasticity is not statistically significant. Lastly we do not find any difference in the elasticity of the reservation wage and of the actual benefit duration, between low-wage and high-wage job-seekers - Column (5) vs. Column (6). This is surprising as we would expect higher wage workers to be less constrained by the minimum wage and thus to respond more to changes in PBD. However, high-wage job seekers probably have higher job finding rates and higher past tenure, which also makes them less responsive to changes in PBD.

Table 6: Heterogeneity analysis

	Tenure		Gender		Past wage level	
	Low tenure	High tenure	Female	Male	Low wage	High wage
	(1)	(2)	(3)	(4)	(5)	(6)
	Log of Reservation wage					
log PBD	0.00964**	-0.00272	0.00156	-0.00245	0.00323	-0.00285
	(0.00379)	(0.00557)	(0.00454)	(0.00435)	(0.00340)	(0.00543)
	Log of Actual Benefit duration					
log PBD	0.514***	0.202***	0.332***	0.292***	0.321***	0.291***
	(0.0399)	(0.0558)	(0.0508)	(0.0423)	(0.0448)	(0.0473)
Obs.	90,364	90,273	72,472	108,165	90,203	90,434
Indiv. F.E.	yes	yes	yes	yes	yes	yes

Source: FNA-FH (Pole emploi)

Note: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. Individual controls, time fixed-effects and tenure fixed-effects are included in the specification. The Table reports the coefficient α of Eq. (4), where we instrument the potential benefit duration by the 2009 Reform.

5 Alternative identification strategy: Regression Discontinuity Design

In this section, we consider an alternative identification strategy based on an age discontinuity in Potential Benefit Duration (PBD).

Workers benefit from a more generous PBD schedule when they are above 50 years old at the time of job separation (before unemployment entry). This is true both before and after the 2009 reform. Before 2009, when senior claimants had worked more than 27 months during the last 36 months, they were entitled to 1,095 days of benefits, i.e. 36 months. If they did not fulfill this criteria, they faced the step function schedule described in Section 2. Starting in 2009, senior workers are entitled to as many days of benefits as days worked within the last 36 months before job separation: the maximum PBD is thus 36 months. Consequently, senior workers with *continuous work history* over the last 3 years before unemployment are entitled to benefits for a period that is 50% longer than that of younger workers. Besides this difference in PBD schedule, UI rules are essentially the same for claimants above and below 50 years old. In particular, the formula used to compute the benefit level as a function of past wages is identical for senior and younger workers.

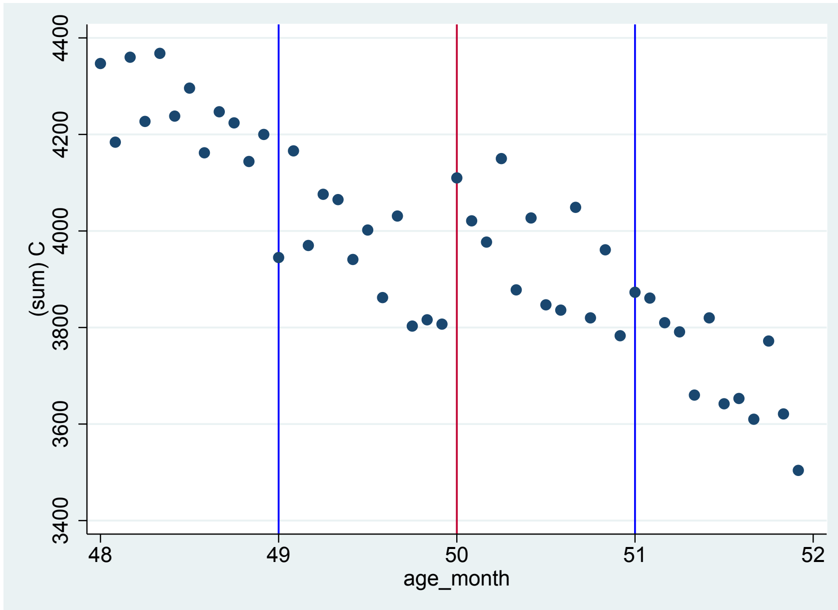
We select a sample of new claims filed between 2006 and 2012. We apply the same sample restrictions as above, except for the fact that we do not restrict the sample to job seekers with multiple claims over the period. We further restrict to claims filed by job seekers aged between 45 and 55 years old, which corresponds to a 10 year window around the RDD cutoff. In our sample, we estimate that PBD is on average around 30% higher for claimants above 50 than below 50.

Figure 9 plots the distribution of the population in age bins which are one month wide. This reveals some missing mass below 50 years old and extra mass just above the cutoff. We perform the usual McCrary test and we estimate that the density is 8% higher just above the cutoff - statistically significant at the 1% level.[17]

We also find some discontinuities in the distribution of covariates around the threshold. Appendix Table A3 shows that workers laid off just after 50 years old are more educated and are paid higher wages. This is consistent with some manipulation of the running variable, i.e. age at the separation date. Manipulation is a serious threat to the RDD identification strategy. This is the reason why we only consider the RDD as an alternative

[17] The bandwidth used to perform the test is around 2 years.

Figure 9: Distribution of the running variable (age in month)



strategy and the next results as suggestive. Yet it is reassuring that they are totally consistent with the main results obtained with the difference-in-difference strategy in Section 4.

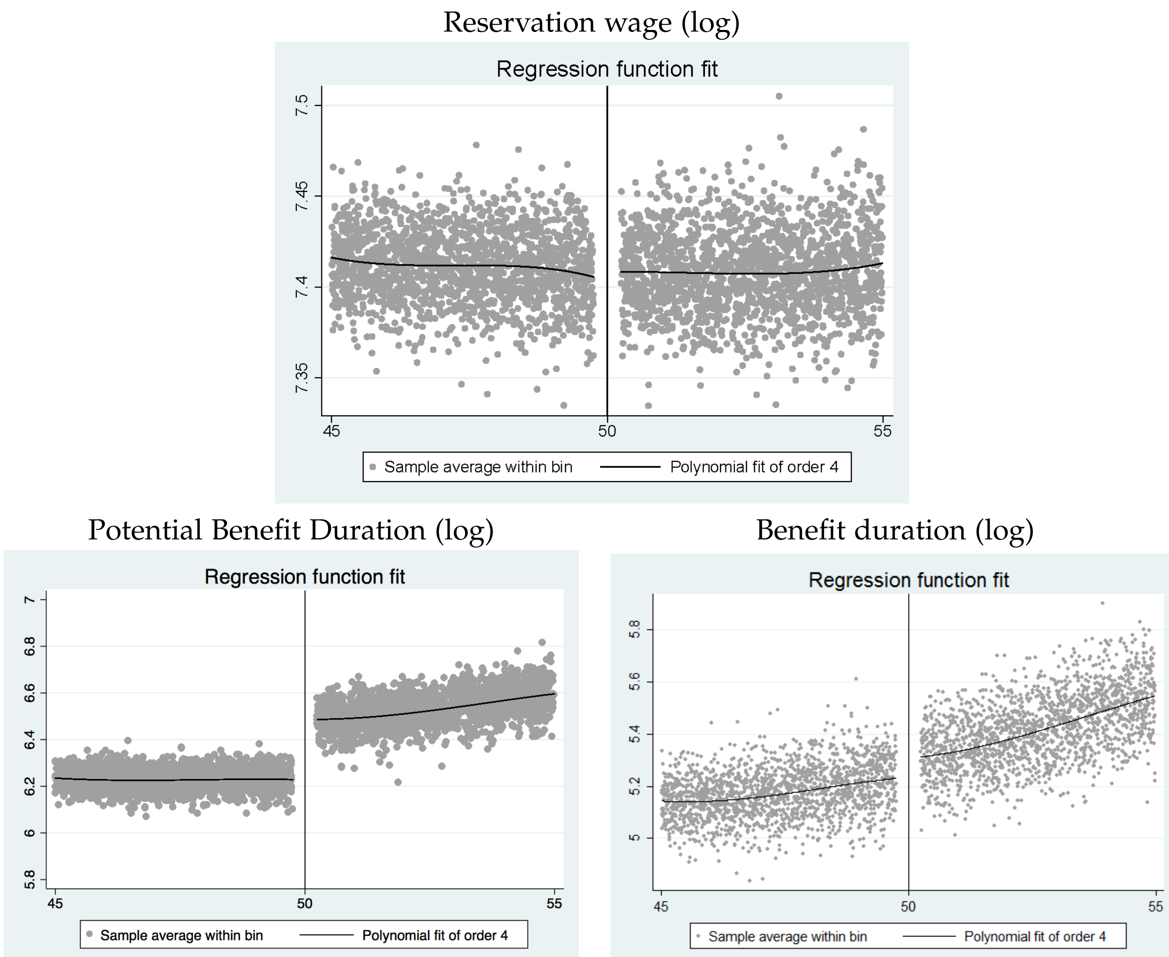
Lalive (2007) and Schmieder et al. (2012a) also report cases of manipulation in the context of UI rules based on age. To address this issue, we follow the same strategy as Schmieder et al. (2012a) and we exclude the observations that are just around the age cutoff.[18] This is a valid correction as long as manipulation is a local matter. There are no theoretical guidelines to choose the size of the window around the cutoff where we exclude observations. Visual inspection of the density of the running variable plotted in Figure 9 suggests that the manipulation window would be between 3 months before and 3 months after the cutoff. We vary the size of the window to test the robustness of our estimates.

Figure 10 plots PBD and the outcomes of interest against age at job separation excluding individuals between 49.75 and 50.25 years old. The upper panel shows that the distribution of reservation wages seems continuous on either side of the cutoff whereas the lower panel shows that the size of the discontinuity in PBD is around 30% as already mentioned above and that there is a discontinuity in the actual duration of benefits at the cutoff.

We now turn to the estimation. We consider the fuzzy RD design where the treatment is the log of PBD. This enables us to directly obtain elasticities with respect to PBD. We estimate

[18]Lalive (2007) finds that only women manipulate their age to benefit from more generous entitlement.

Figure 10: Regression Discontinuity graphics, excluding observations with age between 49.75 and 50.25.



Source: FNA-FH (Pole emploi).

Note: plots are obtained with the stata command rdplot. The data is grouped in age bins, whose size is determined according to Calonico et al. (2014). For each age bin, we plot the averages of the reservation wage, of PBD and of the actual benefit duration. Then a polynomial of degree 4 is fitted on these averages - the solid line.

the following model using as an instrument the dummy indicating that the claimant is over 50 years old (at job separation): $1(age \geq 50)$.

$$\log Y_i = \alpha + \delta \log PBD + P_0(age_i - 50) \times 1(age_i < 50) + P_1(age_i - 50) \times 1(age_i \geq 50) + \epsilon_i \tag{5}$$

where $P_0(.)$ and $P_1(.)$ are polynomials whose coefficients are estimated (without constant). We follow the common practice and we use local polynomial regression. The bandwidth of the local estimation is selected according to Calonico et al. (2014). We also follow the methods they recommend for bias correction and robust standard error correction.

Table 7 reports the estimates of the elasticities δ on samples trimmed in different ways (in columns). The upper panel reports the estimates of the elasticity of the reservation wage, while the lower panel reports those of the elasticity of the actual benefit duration. The elasticity of the reservation wage is not statistically significant, whatever the size of the excluded region around the cutoff. This contrasts with the elasticity of benefit duration, which is around 0.2 and statistically different from 0. This confirms the main results of Section 4: inelastic reservation wage and responsive benefit duration. We also find that the effects of PBD on other dimensions of the reservation strategy, such as the desired type of labor contract or number of hours or the maximum commute accepted, are not statistically different from 0 (results available on request).

Table 7: RDD estimates of elasticities wrt PBD

Age excluded	(1) $[49.9, 50.1]$	(2) $[49.75, 50.25]$	(3) $[49.5, 50.5]$
	Log of Reservation wage		
log PBD	0.0116	0.0172	0.00457
	(0.0149)	(0.0162)	(0.0141)
	Log of Actual benefit duration		
log PBD	0.211***	0.242***	0.175**
	(0.0786)	(0.0669)	(0.0692)
Obs.	470,082	456,280	432,431

Source: FNA-FH (Pole emploi).
Note: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1.
Note: The reported coefficients are the estimated δ from equation (5). The estimation follows Calonico et al. (2014). The kernel used for local polynomial estimation is triangular.

Appendix Table A3 tests for discontinuities in covariates, when observations around the threshold are excluded. Any discontinuities disappear as the bandwidth of the trimmed sample around the cutoff increases. In Appendix Table A4, we further show that including covariates as controls in the RDD estimation hardly affects the estimated elasticities reported in Table 7. Finally, we conduct placebo tests at every age from 44 to 54. Appendix Table A5 shows no significant discontinuities at every placebo age.

6 Theoretical framework

In this section, we compare our empirical findings to the qualitative and quantitative predictions of a standard theoretical framework about job seekers' reservation wage, search decision and hazard rate, along the unemployment spell. We start from van den Berg (1990) and add an endogenous decision about search effort.

6.1 Setting

Job seekers start their unemployment spell at date 0. Until date T, they are entitled to unemployment benefits b. After T, they are only entitled to welfare. At each moment, job seekers choose a search effort e, which is also their probability to receive a job offer. They incur a cost $c(e)$, increasing and convex in e. A job offer is characterized by a wage randomly drawn in a distribution of cdf $F(w)$. Instantaneous utility is denoted as a function $u(.)$ of the revenue received at a given instant. Job seekers choose a reservation wage ϕ_t and will accept an offer if and only if its associated wage is higher than their reservation wage. We assume that, apart from the level of benefits, all parameters are constant over time.

The present intertemporal discounted utility to be unemployed at time t can be written as:

$$\rho U_t = u(vb_t) - c(e_t) + e_t \int_{\phi_t}^{\infty} [W(w) - U_t] dF(w) + \dot{U}_t \tag{6}$$

where v captures the change in utility associated to non-pecuniary aspects of unemployment, ρ the discount rate and $\dot{U}_t$ the first derivative of U_t with respect to time.

The present intertemporal discounted utility of being employed in a job with wage w is:

$$\rho W(w) = u(w) + q(U_0 - W(w)) \tag{7}$$

where q is the probability to become unemployed.

The solution we look for is characterized by van den Berg (1990). For $t > T$, the system is at the stationary equilibrium. When $t < T$, the system move towards the equilibrium. The reservation wage is set such that $W(\phi_t) = U_t$. Search effort is set such that the marginal gain of an additional effort is equal to its marginal cost. We further assume that the instantaneous utility is logarithmic, that $F(w) = \frac{\log(w/\alpha)}{\log(\beta/\alpha)}$ and that the cost of effort is quadratic: $c(e) = \gamma e^2$. We denote $\psi = \log(\phi)$. Combining these assumptions - the proof is reported in the Appendix-, we obtain the following differential equation for the log reservation wage:

$$\dot{\psi}_t = \rho\psi_t + q\psi_0 - (\rho + q)\log(vb_t) - \frac{(\psi_t - \log\beta)^4}{16\gamma(\rho + q)\log^2(\beta/\alpha)}, \tag{8}$$

with $\psi_t \in (\log(\alpha), \log(\beta))$. This differential equation implies that the steady-state value of the reservation wage ψ^* verifies:

$$\psi^* - \log vb - \frac{(\psi^* - \log\beta)^4}{16\gamma(\rho + q)^2\log^2(\beta/\alpha)} = 0, \text{ with } \psi^* \in (\log(\alpha), \log(\beta)) \tag{9}$$

6.2 Calibration

In order to formulate quantitative theoretical predictions, we need to calibrate six parameters: $\alpha, \beta, \gamma, \rho, q, v$ as well as the values of the unemployment and welfare benefits. We take $\alpha = 1$ without loss of generality: wages and benefits will be expressed as a function of this minimum value of the wage. Unemployment benefits are set equal to 80% of the minimum wage while welfare benefits are equal to 50%. We take ρ such that the yearly discount rate is equal to 12%. The maximum wage β is set to 1.5 minimum wages.[19] On the main sample of job-seekers with recurring unemployment spells, the employment spell duration is roughly equal to 12 months and the corresponding monthly separation rate is $q = 1/12$.

γ and v are more difficult to calibrate. We target two moments: the average unemployment spell duration (192 days) and the elasticity of the hazard rate with respect to potential benefit duration (-.33). Targeting these values, we obtain γ = 2.63 and v=1.34.

6.3 Theoretical predictions

Figure 11 shows a simulation of the calibrated model. The value of the reservation wage (as a function of the minimum wage) and the search effort are showed for the 24 months before benefits exhaustion. As expected, reservation wages and search efforts are rather flat 24 months before exhaustion and change faster as exhaustion gets closer. In terms of values, the reservation wage goes from roughly 1.15 minimum wages 24 months before

[19] The 90th percentile of the distribution of wages associated with the employment spell that precedes the second unemployment spell of our repeated claimants is around 1.7 the minimum wage. We take a lower value for β to reflect the fact that part of the heterogeneity is due to individual characteristics.

exhaustion to roughly 1 minimum wage (its lower bound) at exhaustion. Search effort experiences much more variation: it more than doubles between 24 and 0 months before exhaustion.

Figure 11: Evolution of the reservation wage and the search effort along the unemployment spell: Theoretical predictions

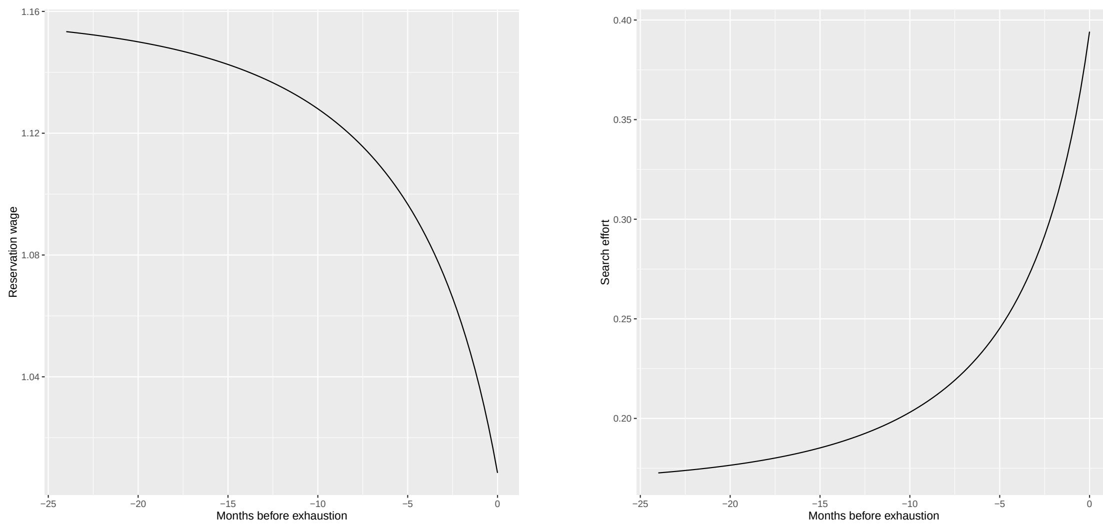
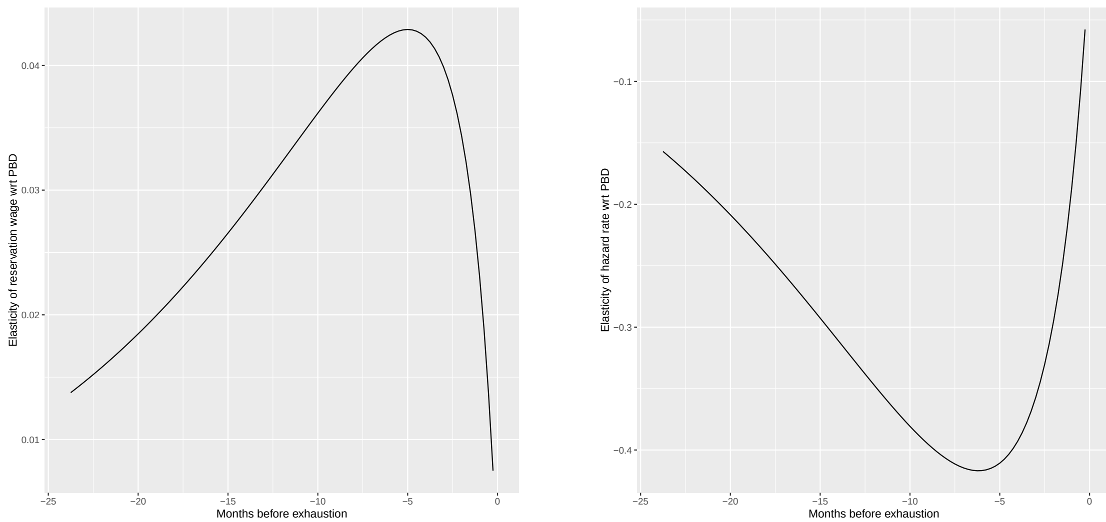


Figure 12 displays how the elasticities of the reservation wage and the hazard rate with respect to PBD change over time. The elasticities vary along the spell: they take higher (absolute) values between 5 and 10 months before exhaustion and lower ones further and closer to exhaustion. Twenty-four months before exhaustion, the elasticity of the reservation wage is lower than .015. Five months before exhaustion, it reaches more than .04. The elasticity of the hazard rate varies between -.15 and -.4 along the spell, consistently with our calibration targets. It is worth noting that the model predicts that the two elasticities can have very different magnitudes, the elasticity of the reservation wage being predicted to be roughly 10 times lower than the elasticity of the hazard rate.

Figure 12 suggests magnitudes for the elasticities of the reservation wage that are much larger than the ones we obtain in Table 4. While the theoretical elasticity is between .02 and .04 for the typical PBDs observed in our data, our 95% confidence interval rule out magnitudes above 0.006. Even for the low tenure group (who are entitled to between 6 and 13 months of PBD), our estimates rule out elasticities above 0.017 whereas the theory predicts that they are between 0.03 and 0.04.

Figure 12: Evolution of the elasticities of the reservation wage and the hazard rate along the unemployment spell: Theoretical predictions



7 Conclusion

Despite a significant and non negligible effect of the Potential Benefit Duration (PBD) on actual benefit duration, there is almost zero effect of PBD on the reservation wage and other dimensions of job selectivity at the start of the unemployment spell. The elasticity of reservation wages is insignificant and we can rule out that it is larger (in absolute value) than 0.006, which means that being entitled to 1 additional month of benefits for a claimant with an initial PBD of 10 months is associated with at most a 0.06% increase in the reservation wage. The lack of responsiveness of reservation wages to changes in UI echoes the results of Krueger and Mueller (2016) who find that reservation wages adjust at a very slow rate along the unemployment spells.

The question of why reservation wages react so little to changes in UI remains open. We see at least two explanations that are worth further explorations. First, job seekers may have reference-dependent behaviors (Koenig et al., 2014; DellaVigna et al., 2016). They may anchor their reservation wages to their past wages, which would limit the responsiveness of reservation wages. Second, job seekers may have biased belief about their employment prospects. Over-optimistic job seekers, who consider that job offers arrive at a high arrival rate, might also react too little to changes in PBD, as they underestimate their likelihood to exhaust benefits (Spinnewijn, 2015). Exploring these possibilities are promising directions for future work.

References

Calonico, S., M. D. Cattaneo, and R. Titiunik (2014): "Robust Nonparametric Confidence Intervals for RegressionDiscontinuity Designs," *Econometrica*, 82, 2295–2326.

Card, D., R. Chetty, and A. Weber (2007): "The Spike at Benefit Exhaustion: Leaving the Unemployment System or Starting a New Job?" *American Economic Review*, 97, 113–118.

DellaVigna, S., A. Lindner, B. Reizer, and J. F. Schmieder (2016): "Reference-Dependent Job Search: Evidence from Hungary," NBER Working Papers 22257, National Bureau of Economic Research, Inc.

Feldstein, M. and J. Poterba (1984): "Unemployment insurance and reservation wages," *Journal of Public Economics*, 23, 141–167.

Koenig, F., A. Manning, and B. Petrongolo (2014): "Reservation Wages and the Wage Flexibility Puzzle," CEP Discussion Papers dp1319, Centre for Economic Performance, LSE.

Krueger, A. B. and A. I. Mueller (2016): "A Contribution to the Empirics of Reservation Wages," *American Economic Journal: Economic Policy*, 8, 142–79.

Lalive, R. (2007): "Unemployment Benefits, Unemployment Duration, and Post-Unemployment Jobs: A Regression Discontinuity Approach," *American Economic Review*, 97, 108–112.

Lalive, R., C. Landais, and J. Zweimueller (2015): "Market Externalities of Large Unemployment Insurance Extension Programs," *American Economic Review*, 105, 3564–96.

Le Barbanchon, T. (2016): "The Effect of the Potential Benefit Duration of Unemployment Benefits on Unemployment Exits to Work and Match Quality in France," *Labor Economics*, 16–29.

Lichter, A. (2016): "Benefit duration and Job search effort: Evidence from a Natural Experiment," unpublished.

McCall, J. J. (1970): "Economics of Information and Job Search," *The Quarterly Journal of Economics*, 84, 113–26.

Nekoei, A. and A. Weber (2014): "Does Extending Unemployment Benefits Improve Job Quality?" NRN working papers 2014-04, The Austrian Center for Labor Economics and the Analysis of the Welfare State, Johannes Kepler University Linz, Austria.

Schmieder, J. F., T. von Wachter, and S. Bender (2012a): "The Effects of Extended Unemployment Insurance Over the Business Cycle: Evidence from Regression Discontinuity Estimates Over 20 Years," *The Quarterly Journal of Economics*, 127, 701–752.

——— (2012b): "The Long-Term Effects of UI Extensions on Employment," *American Economic Review*, 102, 514–19.

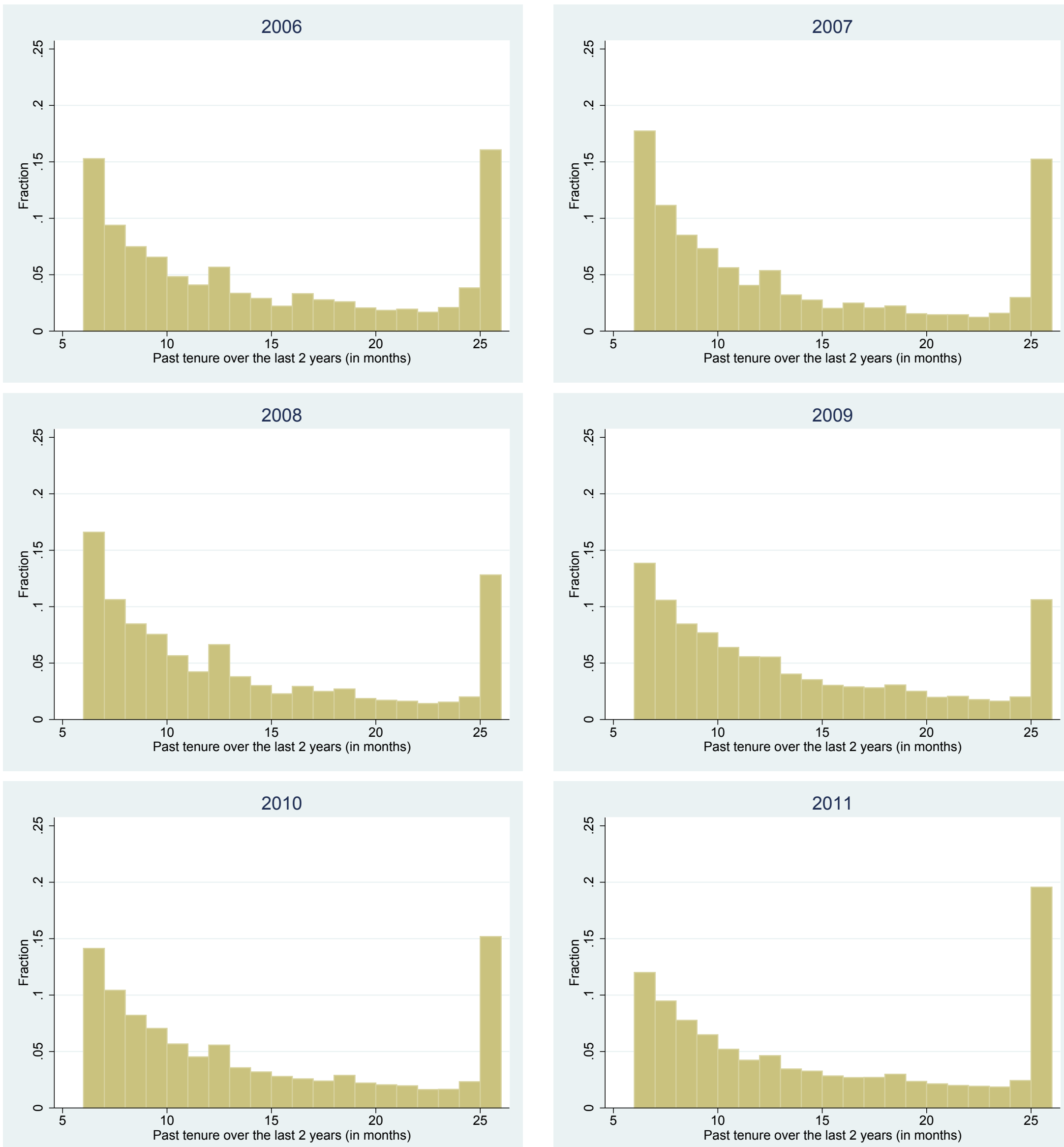
Shimer, R. and I. Werning (2007): "Reservation Wages and Unemployment Insurance," *The Quarterly Journal of Economics*, 122, 1145–1185.

Spinnewijn, J. (2015): "Unemployed But Optimistic: Optimal Insurance Design With Biased Beliefs," *Journal of the European Economic Association*, 13, 130–167.

van den Berg, G. J. (1990): "Nonstationarity in Job Search Theory," *Review of Economic Studies*, 57, 255–277.

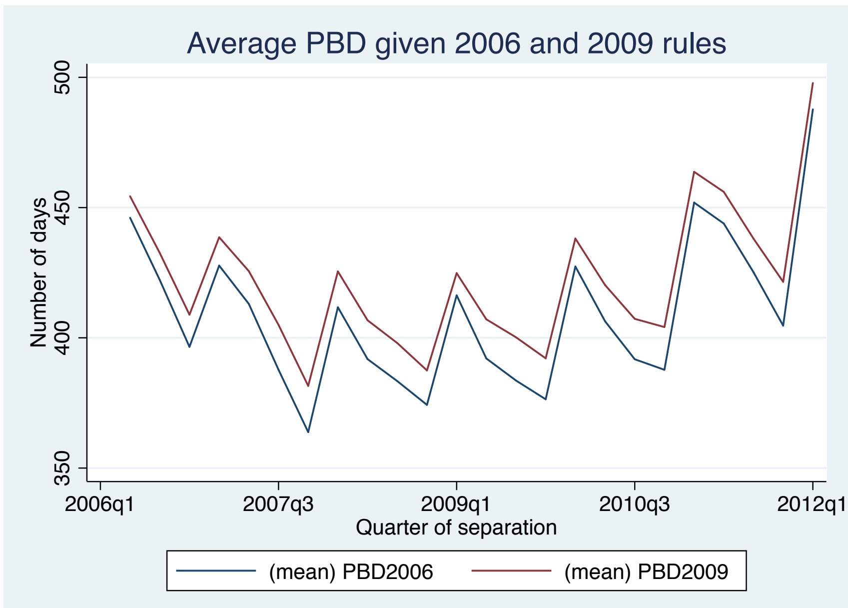
Appendix: Figures

Figure A1: Distribution of the tenure variables



Source: FNA-FH (Pole emploi). Note: Each panel plots the distribution of the past tenure for the inflow of claimants who loose their job between the 1st of April of year N and the 31st of March of Year N+1. Thus the 3 first distributions are pre reform and the 3 last are post reform.

Figure A2: Average PBD given 2006 and 2009 rules



Source: FNA-FH (Pole emploi). Note: for each claimant, we compute, from her tenure, her PBD according to the 2006 rules and the 2009 rules. We then average each PBD over quarter. This translates the actual tenure distribution into potential treatment intensity. The Figure shows that there are no changes in the tenure distribution that would imply a change in treatment intensity on top of the reform shock.

Appendix: Tables

Table A1: Summary statistics

Variable	Mean	Std. Dev.	N
Male	0.599	0.49	180670
Foreign born	0.111	0.314	180670
Age	31.301	7.873	180670
Married	0.353	0.478	180670
Divorced	0.068	0.252	180670
Has a child	0.363	0.481	180670
Education (in years)	11.59	3.272	180637
Occupational Experience (in years)	4.628	5.149	180670
Past Contract is long-term	0.353	0.478	166486
Sum of past tenures over the last 2 years (in days)	427.708	218.351	180670
Past tenure at last employer (in days)	393.648	573.158	180670
Potential Benefit Duration (in days)	413.156	208.855	180670
Actual Benefit Duration (in days)	192.403	163.184	180670
Past Monthly Wage (gross, in euros)	1721.631	388.383	180670
Unemployment Benefits (in euros)	1006.869	226.521	180670

Source: FNA-FH (Pole emploi).

Table A2: Placebo elasticities - DiD strategy

	(1)	(2)	(3)	(4)
	2007	2008	2010	2011
VARIABLES	Log of reservation wage			
Log PBD	0.00979	0.00709	0.00755	0.00512
	(0.00655)	(0.00654)	(0.00582)	(0.00566)
Obs.	30,603	30,603	36,422	36,422
Indiv. F.E.	yes	yes	yes	yes

Source: FNA-FH (Pole emploi).

Note: Robust standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. This Table reports the placebo tests. To avoid contamination, we split our main sample into two subsamples: before and after the 2009 reform. We use the before subsample to test for potential effects of placebo reforms at the end of March 2007 (Column 1) and of March 2008 (Column 2). We use the after subsample in Columns (3) and (4). In each column, we estimate the fixed effects model (2) where we replace actual PBD by a placebo PBD and we instrument the placebo PBD by tenure group dummies interacted with a dummy indicating whether the separation date is after the placebo reform date. For example, in Column (1), the placebo PBD is equal to the PBD according to the 2006 rules for claimants who register before the 31st of March 2007 and it is equal to the PBD according to the 2009 rules for claimants who register after the 1st of April 2007.

Table A3: RDD estimates of discontinuities in covariates

VARIABLES	(1) Log of Past wage	(2) Male	(3) Education	(4) Number of children	(5) Foreign born	(6) Married	(7) Open-ended contract	(8) Experience	(9) Replacement rate
				A. All observations included around the cutoff					
$1(age \geq 50)$	0.0156***	-0.00903	0.258***	-0.0309*	-0.0280***	0.00430	0.0333***	0.212	-0.000770
	(0.00355)	(0.00666)	(0.0574)	(0.0167)	(0.00565)	(0.00611)	(0.00797)	(0.175)	(0.000922)
Obs.	481,871	481,871	481,850	481,871	481,871	481,871	439,678	481,871	481,871
				B. Excluding observations with age in $[49.9, 50.1]$					
$1(age \geq 50)$	0.00957**	-0.00849	0.228***	-0.00680	-0.00731	-9.27e-06	0.00597	-0.339**	-0.000493
	(0.00452)	(0.00859)	(0.0687)	(0.0173)	(0.00490)	(0.00673)	(0.00744)	(0.168)	(0.00112)
Obs.	472,209	472,209	472,188	472,209	472,209	472,209	430,855	472,209	472,209
				C. Excluding observations with age in $[49.75, 50.25]$					
$1(age \geq 50)$	0.00604	-0.00445	0.185**	-0.0109	-0.00151	-0.00523	-0.00120	-0.301*	-0.00123
	(0.00544)	(0.00749)	(0.0767)	(0.0229)	(0.00606)	(0.00769)	(0.00764)	(0.176)	(0.00132)
Obs.	458,337	458,337	458,316	458,337	458,337	458,337	418,204	458,337	458,337
				D. Excluding observations with age in $[49.5, 50.5]$					
$1(age \geq 50)$	-0.00270	0.00539	-0.0833	-0.00718	0.00414	0.00409	-0.00875	0.00497	-0.00110
	(0.00327)	(0.00691)	(0.0537)	(0.0174)	(0.00457)	(0.00689)	(0.00688)	(0.144)	(0.00109)
Obs.	434,387	434,387	434,367	434,387	434,387	434,387	396,322	434,387	434,387

Source: FNA-FH (Pole emploi).

Note: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. This Table reports the estimates of a discontinuity in the distribution of covariates around the 50-year-old age cut-off, i.e coefficient δ of the following equation:

$$X_i = \alpha + \delta 1(age_i \geq 50) + P_0(age_i - 50) \times 1(age_i < 50) + P_1(age_i - 50) \times 1(age_i \geq 50) + \epsilon_i$$

where notations have already been defined in the main text. We follow the estimation strategy of Calonico et al. (2014). The kernel used for the local polynomial estimation is triangular.

Table A4: RDD estimates of elasticities wrt PBD (controlling for covariates)

Age excluded	(1) [49.9, 50.1]	(2) [49.75, 50.25]	(3) [49.5, 50.5]
	Log of Reservation Wage		
log PBD	-0.0141	0.00482	0.0119
	(0.0138)	(0.0128)	(0.0103)
	Log of Benefit duration		
log PBD	0.199**	0.215***	0.161**
	(0.0852)	(0.0797)	(0.0696)
Obs.	470,082	456,280	432,431

Source: FNA-FH (Pole emploi).

Note: Standard errors in parentheses. *** $p<0.01$, ** $p<0.05$, * $p<0.1$. RDD estimates obtained with the corrections of Calonico et al. (2014). The kernel used for the local polynomial estimation is triangular. We control for all covariates listed in Table A3: past wage (log), gender, education (in years), number of children, foreign status, marriage status, type of past labor contract (open-ended), experience in the occupation sought and replacement rate.

Table A5: Estimates of discontinuities in reservation wages at placebo age cutoff

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Placebo Age cutoff	44	45	46	47	48	49	51	52	53	54
$1(age \geq placebo)$	-0.000210	0.00464	-0.00159	0.00194	0.00149	-0.000106	-0.000254	0.0123**	-0.00552	0.0147**
	(0.00302)	(0.00353)	(0.00315)	(0.00327)	(0.00329)	(0.00365)	(0.00396)	(0.00591)	(0.00417)	(0.00692)
Obs.	590,095	564,914	543,235	521,034	499,192	478,334	441,441	427,481	412,624	392,336

Source: FNA-FH (Pole emploi).

Note: Standard errors in parentheses. *** p<0.01, ** p<0.05, * p<0.1. This Table reports the estimates of a discontinuity in reservation wage at placebo age cutoffs, i.e coefficient δ of the following equation:

$$\log resW_i = \alpha + \delta 1(age_i \geq placebo) + P_0(age_i - placebo) \times 1(age_i < placebo) + P_1(age_i - placebo) \times 1(age_i \geq placebo) + \gamma X_i + \epsilon_i$$

where notations have already been defined in the main text. We follow the estimation strategy of Calonico et al. (2014). The kernel used for the local polynomial estimation is triangular. We control for all covariates listed in table A3: past wage (log), gender, education (in years), number of children, foreign status, marriage status, type of past labor contract (open-ended), experience in the occupation sought and replacement rate. Consistently with our preferred specification in the main text, we exclude individuals with an age in a centered 6-month window around the placebo age.

Appendix: theoretical evolution of the reservation wage

In this Appendix, we show how we obtain the differential equation governing the evolution of the reservation wage, i.e. Equation 8.
Proof:
Combining equations (6) and (7), we obtain after some algebra:

$$u'(\phi_t)\phi_t' = \rho u(\phi_t) + qu(\phi_0) - (\rho+q)u(vb_t) + (\rho+q)c(e_t) - e_t \int_{\phi_t}^{\infty} u'(w)\bar{F}(w)dw \tag{10}$$

where $\bar{F}(w) = 1 - F(w)$.
Search effort is set such that the marginal gain of additional effort is equal to its marginal cost. From Equation (6), the first-order condition is:

$$(\rho+q)c'(e_t) = \int_{\phi_t}^{\infty} u'(w)\bar{F}(w)dw \tag{11}$$

We assume that the instantaneous utility is logarithmic, that $\bar{F}(w) = \frac{\log(\beta/w)}{\log(\beta/\alpha)}$ and that the cost of effort is quadratic: $c(e) = \gamma e^2$. We denote $\psi = \log(\phi)$. Equations (10) and (11) become:

$$\psi_t' = \rho\psi_t + q\psi_0 - (\rho+q)\log(vb_t) + (\rho+q)\gamma e_t^2 - \frac{e_t}{2\log(\beta/\alpha)}(\psi_t - \log\beta)^2$$
$$e_t = \frac{(\psi_t - \log\beta)^2}{4\gamma(\rho+q)\log(\beta/\alpha)}$$

Gathering the last two equations to eliminate e_t, the following equation determines the dynamics of the reservation wage for $t < T$.

$$\psi_t' = \rho\psi_t + q\psi_0 - (\rho+q)\log(vb_t) - \frac{(\psi_t - \log\beta)^4}{16\gamma(\rho+q)\log^2(\beta/\alpha)},$$

with $\psi_t \in (\log(\alpha), \log(\beta))$. □