

The Take-Up of Unemployment Benefit Extensions

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Le recours aux extensions d'indemnisation à l'assurance chômage

Résumé

Lorsqu'ils atteignent la fin de leur allocation, les bénéficiaires de l'assurance chômage ne font pas toujours valoir l'intégralité des extensions d'indemnisation auxquelles ils sont éligibles. En droit, au-delà d'un seuil minimum de jours travaillés, chaque emploi effectué contribue à étendre la durée potentielle d'indemnisation d'une durée équivalente, sous réserve qu'une Attestation Employeur a été envoyée par l'allocataire à Pôle Emploi. De fait, jusque mi-2014, plus d'un cinquième des allocataires ne communiquaient pas l'intégralité de leurs attestations. La réforme des Droits Rechargeables a introduit l'envoi automatique d'un courrier à destination des allocataires dont la fin de droits est prévue après le 1er Octobre 2014 afin de les informer de la nécessité d'envoyer ces attestations. Dans cette étude, nous montrons que ce courrier les a effectivement incités à produire ces documents plus systématiquement, permettant ainsi un allongement de leur durée d'indemnisation potentielle. A partir des données du Fichier National des Allocataires (FNA) de l'Unédic, nous estimons par une régression sur discontinuité que ce courrier a réduit de 14 points la part des allocataires ne certifiant pas l'intégralité de leurs emplois passés et ainsi augmenté d'un mois en moyenne la durée d'indemnisation potentielle. Cet effet de l'information est plus marqué parmi les personnes inscrites à l'Unédic pour la première fois, qui sont également les moins familières de ces démarches.

Mots-clés : Recours, Assurance Chômage, Information, Évaluation

The Take-Up of Unemployment Benefit Extensions

Abstract

Claimants do not always take-up the Unemployment Insurance (UI) benefit extension they are eligible for. The French National Employment Agency grants benefit extensions to recipients who reach benefit exhaustion and have worked for a certain period of time since the start of their spell. To this end, claimants are required to send an Employer Certificate for each contract they undertook. Until mid-2014, above one fifth of unemployed workers did not fully certify their work history. In this paper, we show that a simple informational intervention may strongly increase the take-up of potential UI benefit extensions, especially among recipients with little unemployment experience. The "renewal-of-entitlement" reform of October 2014 introduced an informative letter automatically sent to claimants upon benefit exhaustion and emphasizing the role of Employer Certificates. Relying on the administrative file of French UI claimants (FNA) in a regression discontinuity design, we estimate that the letter reduced by 14 points the share of claimants who do not certify their full work history and extended the potential benefit duration by one month on average. This informational mailing narrowed the gap in certification behavior between claimants with different levels of unemployment experience.

Keywords: Take-up, Unemployment Insurance, Information, Policy Evaluation

Classification JEL : C23, D83, J65

1 Introduction

There is a blurred line between employment and unemployment. In France, more than half of unemployed workers take jobs while registered with the National Employment Agency¹. Above a given threshold of days worked, these contracts may contribute to extend the potential benefit duration (PBD), provided that the claimant sends Employer Certificates for each contract of his work history. For eligible workers, the absence of certification of past jobs implies a loss of potential benefits corresponding to (partial or total) non take-up of UI benefits. Between April and September 2014, 21 % of potentially eligible workers did not certify at least one of their work contracts and half of them lost at least two months of potential UI benefits. Part of this non-certification behavior is simply the result of claimants anticipating that they will find a job shortly after benefit exhaustion. For others, generally poorly informed about the UI system, failure to perceive a benefit extension leads to become dependent on social assistance, at best.

The French “renewal-of-entitlement” reform (*Réforme des Droits Rechargeables*) impacted information available to unemployed workers regarding benefit extension. Since its implementation² on October 1st, 2014, a letter has been systematically sent to each worker upon benefit exhaustion, mentioning that Employer Certificates are to be provided in order to claim a renewal of entitlement. We exploit the timing of this reform within a regression discontinuity design to identify the effects of information on the certification behavior of unemployed workers. The French administrative file of UI claimants (*FNA*) provides us with very detailed information about recipients’ unemployment trajectories at a daily level. We directly compare the declared employment history and the employment history actually certified in order to precisely retrace the potential eligibility to a benefit renewal.

A simple plot reveals a sharp drop in the average PBD lost among claimants whose unemployment spell ended on or after October 1st, 2014, and who therefore received the mailing informing them of the necessity to send their Employer Certificates (Figure 1). This simple description of certification patterns depending on the spell exhaustion date must however be considered cautiously, since the renewal-of-entitlement reform induced a change in the probability of reaching benefit exhaustion. In order to circumvent this

¹The French National Employment Agency is *Pôle Emploi*. Statistics about unemployed workers who take jobs while registered are available on the website of *Unédic (Indicateurs de suivi de la Convention d’Assurance Chômage 2014, June 2018)*. During the third quarter of 2014, on average 1.2 million registered claimants are working each month out of a total of 2.4 million registered unemployed workers. This share increased steadily up to 60% in 2017.

²The reform was first enacted on May 15th, 2014.

differential selection issue, we define instead a *Potential Benefit Exhaustion* (PBE) date. This assignment variable is defined as the date of benefit exhaustion expected by the claimant when he has N days of UI benefits left and if he does not take a new job. By construction, it is not impacted by the reform. We show that using this PBE date calls for a fuzzy RD design.

Within this framework, we estimate a strong effect of the information letter on the certification behavior of unemployed workers. The share of eligible claimants who did not fully certify their work history dropped by 14 points with the reform, resulting in a decrease by one month in the total duration of uncertified work contracts. The proportion of claimants who certify none of their past jobs was reduced by 5 points. These results are robust to a wide range of specifications. Effects estimated around two placebo reforms on October 1st 2013 and 2015 are non-significant though precisely estimated, and we provide evidence that the local continuity assumption necessary for identification holds in our framework.

These responses highlight the importance of information regarding the undertaking of administrative procedures and the corresponding take-up of unemployment insurance benefits. They also suggest a non-negligible gain from transparency for claimants close to benefit exhaustion. The letter increased their average PBD by 30 days, their average daily benefits are equal to 35€ and they consume nearly all their benefits. In order to convert our estimate into an impact on the effective duration of compensated unemployment, we consider the elasticity of actual benefit duration with respect to PBD of 0.3 estimated by [Le Barbanchon et al. \[2019\]](#). This elasticity is arguably a lower bound for those unemployed workers who are close to benefit exhaustion without having found a long-term job.³ We suggest that the purely informational component of the reform increased UI benefits by at least 300€ per unemployment spell.

Before the 2014 reform, the duration of the uncertified work history is negatively related with recurring experience in unemployment, measured by the number of unemployment spells the claimant has completed in the past (Figure 2 and 3). This correlation may result from a learning process, whereby workers returning to unemployment know the rules better and certify a higher share of their work history. It could also be the consequence of a selection of knowledgeable claimants into recurring unemployment spells, since sending Employer Certificates increases the likelihood of being granted a new entitlement to UI benefits, or it may reflect the existence of firms actively organizing the intermittency of employment and unemployment spells, simultaneously encouraging employees to fully benefit from the UI. In any case, the informative mailing had het-

³This elasticity is not structural. Because the letter is likely to have increased the salience of UI benefits, we also expect it to have had a positive impact on this elasticity.

erogeneous effects on claimants, ranging from a reduction by nearly 40 days for those unemployed for the first time to a non-significant drop by 10 days for recipients in their fifth spell or above. As illustrated by Figure 3, the reform basically reduced the gap in uncertified work history between claimants with different levels of experience into unemployment. Finally, we find no strong evidence that the informative mailing did reduce disparities in certification behaviors between age groups nor between education levels.

This paper contributes to the literature on the impact of information on the take-up of UI and social benefits. Currie [2006] distinguishes three potential causes for incomplete take-up: stigma, transaction costs and informational barriers.⁴ Here, stigma is not the main concern since the claimants we focus on are already registered with the Employment Agency and receive UI benefits. Furthermore, we analyze a subgroup for which transaction costs and economic incentives (daily benefits and potential duration) remain unchanged by the reform. Therefore, we focus on the role of information on the take-up of UI benefit extensions.

Information provision matters for the take-up of a wide range of programs, among which disability insurance (Armour [2018]), post-secondary education (Barr and Turner [2018]), welfare benefits (Finkelstein and Notowidigdo [2019]), social security (Liebman and Luttmer [2015]), EITC and labor supply (Chetty and Saez [2013]; Guyton et al. [2016]) or retirement savings (Saez [2009]). Information about a program can take many forms and be more or less efficient depending on the underlying issue. On the one hand, reminders prove efficient to increase individuals' attention about a program, but only in the short run (Guyton et al. [2016]; Manoli and Turner [2014]). Experimental mailing to taxpayers who failed to claim the EITC (Bhargava and Manoli [2015]) or aiming to set up a meeting with social welfare recipients (Chareyron et al. [2018]) show that emphasizing the salience of programs' benefits raises the response rate and that framing matters for incentives to be accurately perceived (Saez [2009]). If, on the other hand, incomplete take-up is driven by unawareness or lack of knowledge, then interventions may have long-term effects, but require deeper explanations and eventually personal assistance in order to be efficient (Chetty and Saez [2013]). Both dimensions can be combined when the letter highlights expected benefits, individual eligibility, necessary steps for enrollment and may lead to an intervention of state services (Barr and Turner [2018]). Our natural experiment is closer to the setting of a reminder. The informative mailing (Figure 11 in Appendix A) mainly consists of an advice to send Employer Certificates, without further developments about the French UI system, the 2014 reform or the link between past certified jobs and future benefits' characteristics, nor any individual assessment

⁴Before that, Moffitt [1983] model non-participation in welfare programs as a utility-maximizing decision resulting from a stigma cost only.

of the situation of the claimant. Non-certification is not necessarily due to a lack of knowledge about the UI system, but also likely related to behavioral biases such as procrastination or misperception (of the benefits of certification, of the probability to exit unemployment,...). Therefore, we may interpret the treatment as increasing claimants attention towards certification of their work history.

From a welfare perspective, the optimal level of unemployment insurance is determined by a tradeoff between insuring unemployed workers and providing incentives to search for a new job, an imperfectly monitored action raising a moral hazard issue (Chetty [2006]). If the social planner determines the optimal level of benefits assuming full take-up, any behavior of incomplete take-up would lead to a sub-optimal level of unemployment insurance (Kroft [2008]). Yet, if he takes into account incomplete take-up in his social objective and is able to influence it through information provision, it is not obvious that he should target a full take-up. The optimal policy depends on the correlation between the take-up behavior and individual characteristics. Informational interventions are optimal if incomplete take-up is over-represented among poor populations, for whom it is harder to access relevant information and who may have less attention to dedicate to complex administrative processes (Banerjee and Mullainathan [2008]; Mullainathan and Shafr [2013]). This is especially relevant for unemployed workers who spend time searching for a job. In contrast, Kleven and Kopczuk [2011] argue that imperfect take-up can be the result of an efficient screening, if individuals who achieve to receive benefits are those who are the most in need. Informational frictions lead to a second-best economy where UI takers are those with fewer opportunities. Empirically, we should therefore compare the marginal taker of a program to the average taker before the informational intervention. Finkelstein and Notowidigdo [2019] find a relatively strong impact of information and assistance on the take-up of the Supplemental Nutrition Assistance Program (SNAP), but also estimate that the intervention decreases targeting: marginal enrollees due to the intervention are less needy than average recipients in the control group. For the French case, we find that the treatment effect is slightly—but not significantly—stronger for older and less-educated workers. The relevant dimension of heterogeneity appears to be the experience in unemployment.

Finally, our paper suggests a new angle, considering the take-up of unemployment benefit extensions. The take-up of UI benefits is usually defined as the share of unemployed workers who are registered with the National Employment Agency. The take-up rate ranges from 60% to 80% in OECD countries (Hernanz et al. [2004]), it is equal to 77% in the US (Auray et al. [2019]) and may be even lower in France, around 40% to 60% according to Blasco and Fontaine [2018]. It is a major concern since claiming costs of unemployment insurance may induce large and heterogeneous welfare losses (Fontaine

and Kettemann [2016]).

There are two advantages in considering the take-up of UI benefit *extensions*, in order to isolate the sole effect of information gains but also for measurement reasons. First, this concept seems economically relevant since, if they find no job and get no benefit extensions, these claimants will incur a major loss of income⁵ and their links with the Employment Agency may get more loose. In contrast, workers virtually unemployed during a transition between two long-term contracts won't register if the administrative cost exceeds their marginal gain. The behavior of the latter is mostly driven by transaction costs while the behavior of the former is related to informational issues. Second, eligibility can be accurately assessed because these unemployed workers are already registered with the National Employment Agency. Their take-up may be easily measured as the duration of their certified past employment duration relative to their total declared past employment duration.

The rest of the paper is organized as follows. Section 2 presents features of the French UI benefits system and of the 2014 reform which are of interest to us. Section 3 describes the data we use. Section 4 emphasizes the importance of defining a proper assignment date and presents our fuzzy RD design. Section 5 displays the results, discusses their robustness and investigates heterogeneity of the treatment effect. Section 6 concludes.

2 The French unemployment insurance & the 2014 reform

In this section, we underline several features of the French unemployment insurance system which are of specific interest for our analysis. We first describe the institutional framework surrounding the entitlement to UI benefits, in connection with employment history and the certification of work contracts. We then outline the 2014 reform of UI benefit renewal, and eventually focus on the information environment of benefit recipients as well as the administrative costs that they face.

2.1 Certification of work contracts and benefit entitlement

In France, unemployment insurance benefit duration is determined by past employment history. Nearly all workers from the private sector are affiliated with the unemployment insurance system. One day worked while affiliated to the UI agency ensures one day of benefits, in the limit of 2 years⁶. In order to be granted initial UI entitlement,

⁵Median daily UI benefits is 34€ while daily social benefits for long-term unemployed workers (ASS) is 16.5€ in 2018.

⁶3 years for claimants above 50 years old.

total employment history considered within the last 28 months⁷ should be longer than 4 months. Unemployment should be involuntary. During an ongoing unemployment spell, claimants may accumulate new benefits by taking up new jobs. On top of the 28 months rule, jobs considered for a benefit extension must have taken place after the last work contract which was used for current benefit entitlement⁸ and must end before benefit exhaustion.

Whether considering initial entitlement or benefit extension, work contracts may contribute to benefit duration only to the extent that they have been *certified*. Certification of a past job requires unemployed workers to send their Employment Agency an *Employer Certificate*, which should be handed out by the employer at the end of each contract.⁹ The Employer Certificate is the official legal document characterizing the termination of a work contract. It mentions the employment interval, payroll, termination fees, bonuses and the ground for contract termination, which is necessary to ensure that the required conditions for unemployment insurance (especially the condition of involuntary unemployment) are met. Employer Certificates should be sent by both the employer and the employee in order for the work contract to be labeled as *certified*.¹⁰ Then the EA determines whether the renewal is granted, based on the condition of involuntary unemployment and on certified employment history mentioned in the Employer Certificates. Therefore, the issue of take-up comes into play because affiliation history taken into account for UI benefit extension depends on work contracts certification behavior.

A common difficulty when relying on administrative data to study the take-up of social policies is that eligible non-takers are not observed in the data. One interesting feature of our setting is that, when taking up work contracts while unemployed, workers have to declare them to the Employment agency in order to stop receiving UI benefits temporarily, otherwise facing a penalty.¹¹ The declaration of work contracts delays UI benefits, shifting equally the potential benefit exhaustion (PBE) date. However, this *declaration* process is not sufficient for work contracts to contribute to the total work history: *certification* is mandatory. Therefore, the *declared* employment history provides us information about potential eligibility to an entitlement renewal, while *certified* em-

⁷36 months for claimants over 50 years old.

⁸A job that has not been certified for a previous spell cannot contribute to a new entitlement / extension.

⁹*CT, Art. R1234-9.*

¹⁰Firms failing to send this certificate may be sanctioned by a fine of up to 1500€ (*CT, Art. R1238-7*).

¹¹Legal documents about the French UI can be found on the website of the Unédic. Regarding the declaration of jobs while registered at the EA: *Règlement général (RG) 2014, Art.30*. Pay slips are a supporting document for this declaration of jobs: *Accord d'application n°9 du 14 mai 2014 §1*. Even before 2014, it was mandatory for claimants to send these pay slips in order to keep receiving benefits: *RG 2011 & 2014, Art.25 §4; Code du travail (CT), Art. R5426-3 §3*. Unédic computes the amount of UI benefit extension according to the information on these Certificates: *RG 2014 Art.41 §1*.

ployment history determines the actual eligibility. The comparison of these two variables highlights the take-up of benefit extension.

2.2 The 2014 reform: the renewal of UI entitlement

The reform regarding the renewal of UI entitlements was enacted on May 14, 2014 and implemented on October 1, 2014. It was mostly targeted toward workers returning frequently into unemployment. Table 1 summarizes the main features of the French UI system, whether stable over time or impacted by the 2014 reform.

Before October 1, 2014, benefit extensions were governed by the “comparison-of-benefits” system. In this previous system, workers who went back to unemployment after a work contract and who had accumulated a total affiliation of at least 122 days during their ongoing spell could proceed to a comparison between the remaining entitlement of their current spell, and the potential entitlement triggered by the new work contract(s). The highest daily benefits as well as largest total entitlement were retained, and the duration was calculated as the ratio of the latter to the former. The other potential entitlement was definitively abandoned. A subtlety of this system lied in the moment at which the comparison-of-benefits took place. It could happen as soon as the threshold of 122 days of new work contracts was reached, but it could also be delayed to a certain extent. If unemployed workers de-registered from the EA when they took on a new work contract, then the comparison would be triggered when they got back to unemployment (provided that they had reached the required work affiliation of 122 days). If they did not, they could remain registered and keep resuming their initial entitlement when coming back to unemployment (thus postponing the comparison). In this respect, the 2014 reform simplified the benefit extension mechanism making it independent of the (de-)registration behavior.¹²

The reform of October 1, 2014 enacted the “renewal-of-entitlement” system (*recharge-ment des droits*).¹³ Its stated aim was to “*value all work periods undertaken after the start of the initial compensation period, towards a new entitlement, under certain conditions.*”¹⁴ Several dimensions of the reform served that purpose. First, workers returning into unemployment must now exhaust the remainder of their previous entitlement before starting a new UI compensation period¹⁵. Thus in the new system, there are no more

¹²The implications of this change are important in terms of differential selection before and after the reform (see Section 4).

¹³Another rule was modified by the 1st October 2014 reform, regarding the accumulation of wages and UI benefits for unemployed workers having short-term contracts, which we detail in Appendix B. As we will argue in section 4, this other aspect of the reform cannot have affected the certification behavior that we focus on, which concerns *past* employment contracts that have taken place before the reform.

¹⁴*La Convention d'Assurance Chômage 2014*, documentation juridique.

¹⁵This rule was slightly modified in April 2015 by the extended “*droit d'option*” which allowed recipients

Table 1: Main features of the French UI and 2014 reform

<i>Stable features</i>		
Unemployment	should be involuntary	
PBD	1 day worked = 1 day of benefits	
New UI entitlement	requires at least 122 days worked	
Employer certificate	required for each work contract	
Declared contracts	shift BE by the same duration	
Certified contracts	contribute to UIB entitlement/extension, within 28 month before appliance to UI, after last job of previous spell & before BE	
<i>"Renewal-of-entitlement" reform</i>		
	<i>Before Oct. 2014</i>	<i>After Oct. 2014</i>
Information	\emptyset letter	letter
Min. affiliation	122 days	30 days
Total benefits B	$\max(B_0, B_1)$	first B_0 , then B_1
Daily benefits b	$\max(b_0, b_1)$	first b_0 , then b_1
Remaining benefits	abandoned	available
Timing of extension	re-registration	end of B_0
Comparison of benefits	everyone	available for apprentices
<i>Other components: jobs while unemployed</i>		
	<i>Before Oct. 2014</i>	<i>After Oct. 2014</i>
Max hours p/ month	110	removed
Max wage w_1	$0.7w_0$	removed
Max duration	15 months	removed
Immediate benefits	$b \left(1 - \frac{w_1}{w_0}\right)$	$\max(b - 0.7w_1, 0)$

Note: b_0 and B_0 (resp. b_1 and B_1) refer to daily and total benefits remaining from the previous spell (resp. recently accumulated). In the last part of the table, w_0 refers to the monthly reference wage used to determine benefits b_0 for the previous spell and w_1 is the monthly wage of the job undertaken while unemployed.

"unused" benefits and each work contract may be turned into a corresponding potential benefit entitlement.

Second, any employment history of at least 30 days may now be considered in order to renew one's entitlement to UI benefits, instead of the minimum of 122 days in the pre-reform setting.¹⁶ Third, the reform implemented a new information framework.

with a disadvantageous initial right to opt for the new one instead.

¹⁶The previous threshold of 122 days of affiliation remains for newly unemployed workers without past

Although not the most advertised mechanism of the reform, this information setting also contributed towards valuing all work periods.

2.3 Informational environment and administrative costs

The 2014 reform introduced an information letter for all claimants nearing benefit exhaustion. Until September 30th 2014, claimants had to contact the Employment Agency in order to trigger the procedure for benefit extension. In particular, only claimants who had accumulated more than 122 days of affiliation and had registered to the UI system for the second time (after a de-registration) would be informed of the conditions regarding the “comparison-of-entitlement”. Otherwise, continuously registered claimants could exhaust their entitlement without being informed of the possibility of a new entitlement nor of the fact that handing in their Employer Certificates could lead to a benefit extension.

Since October 1st 2014, claimants are automatically detected and receive a letter one month before their expected benefit exhaustion.¹⁷ The letter mentions the date of expected benefit exhaustion and provides supplementary information depending on the situation of the claimant. The type of letter depends on the information provided beforehand to the Employment Agency by the worker.¹⁸ If he sent pay slips to *declare* some jobs but did not send enough Employer Certificates in order for the Employment Agency to ascertain his eligibility to a benefit extension, the letter indicates that he does not yet fulfill the required conditions and that he should send Employer Certificates in order to be eligible (Figure 11 in Appendix A). In contrast, if he sent enough Employer Certificates in order to claim a renewal, the letter mentions that he is eligible and advises him to send potentially missing Employer Certificates (without indicating which ones).¹⁹ Neither the eligibility rule nor the characteristics of the potential benefit extension are mentioned in these letters.²⁰ After benefit exhaustion, claimants were not able to increase their employment history by taking new jobs, but they could still send certificates for past jobs and they would be paid their benefits retrospectively.

Finally, this informative letter was created in a more global context of dematerialization of Employer Certificates. Since January 1st 2012, Employer Certificates should

unemployment history.

¹⁷See *RG 2014, Art.40 §3* regarding the creation of the information letter.

¹⁸Rapport d’audit sur la mise en oeuvre des droits rechargeables, 9 juillet 2015, Unédic.

¹⁹Since March 23, 2015, 4 new features are included in the letters: *(i)* number of reported hours, *(ii)* the initial date of the employment history, *(iii)* time periods for which employer certificates are missing and *(iv)* deadline before which the claimant has to send supportive documents, as shown in Figure 12 in Appendix A.

²⁰In practice, most claimants whose benefits expired at the end of 2014 received this information after the exhaustion of their UI benefits and the first letters were sent after the implementation of the reform.

in principle be sent electronically by firms with more than 10 employees.²¹ In practice, a large fraction of these firms were still sending Employer Certificates in hard copy by the time of the reform. Dematerialization may improve information processing by the National EA but does not exempt claimants from sending Employer Certificates to the EA.²² Firms with temporary workers already sent electronically the equivalent of Employer Certificates to the EA (called a “monthly statement of work”) and corresponding jobs were automatically considered as *certified*.

3 Data description and sample selection

In order to precisely analyze the change in certification behavior around the 2014 reform, we rely on the National File of UI Claimants (*Fichier National des Allocataires*, FNA), an exhaustive long-run administrative database provided by Unédic²³ which tracks trajectories of all UI recipients while unemployed.

We focus on recipients who have reached a work affiliation of at least 122 days, which allows to compare spells that have not been impacted by the change in minimum affiliation duration. We consider these recipients at the point where their remaining UI benefits fall below a threshold of 61 days. Therefore, we make sure to compare claimants who are close to benefit exhaustion and whose accumulation of UI benefits has not been impacted by the reform (cf. Table 1).²⁴ Claimants aged 50 or above at the time they submit an application for a benefit entitlement are excluded from the sample since they are concerned by a specific set of rules regarding pre-retirement unemployment insurance. We restrict our analysis to claimants affiliated to the general UI regime or temporary work UI regime. This amounts to excluding specific professions, mostly temporary workers of the entertainment industry, whose UI rules are not only different, but also differently modified by the 2014 reform. We also remove childcare workers, for whom affiliation to UI is much more complex to determine. Finally, the sample is further restricted to the baseline type of benefits (ARE - *Allocation de Retour en Emploi*).²⁵

²¹Décret n°2011-138.

²²Circulaire n°2011-09 du 15 février 2011.

²³The National Professional Union for Employment in Industry and Trade, which is responsible for setting the amounts of social contributions and unemployment benefits.

²⁴For workers with more than 730 days of certified work, supplementary work affiliation will be of no use since they have already reached the two-years cap of benefit duration.

²⁵We exclude claimants benefiting from very specific benefit conditions (such as the *Contrat de Sécurisation Professionnelle (CSP)*, a support process to help workers concerned by an economic layoff finding a new job). We also remove individuals who were assigned zero benefits or for whom a spell was registered with zero entitlement duration. These cases most probably correspond to registration errors and are overall quite rare.

The unit of observation is the administrative unemployment spell, defined here as an entitlement period which may include periods of activity. A new spell thus starts with a new UI entitlement. A spell ends either with benefit exhaustion, or if the claimant finds a permanent contract and definitively quits unemployment before having exhausted his benefits²⁶, or—before the reform—if there is a comparison of benefits when the worker gets back to unemployment. An unemployment spell is composed of different sub-periods depending on whether the claimant receives benefits, is working, is on a maternity leave, has been temporarily refused benefits, etc. The dates of these sub-periods and their types are available in the data. In practice, by restricting the estimation to a four-months window around the reform, we observe one spell per claimant. Therefore, we often interpret the results in terms of effects on claimants rather than spells.

We know the exact start and end dates of each terminated work contract. For each one, we also know whether it was only *declared* to the National Employment Agency or if an Employer Certificate has been sent in addition (*certified* work contract). However, the dates when the certificate has been sent and when the status has been updated are not in the data. Our main outcome is the duration of the uncertified employment history, computed as the difference between the declared duration and the certified duration. Alternative outcomes of interest include an index of complete certification (ie. all the work history duration is certified vs. at least part of it is not), and an index of complete non-certification (ie. none of the work contracts are certified vs. at least some are). We only consider work contracts that are over before benefit exhaustion, since those are the only ones taken into account in order to compute affiliation. Among the unemployment spells ending between April 2014 and September 2014 and potentially eligible to a benefit extension, 21.1% included at least one work contract which was not certified: 6.7% certify no work contract at all and 14.4% partially certify their work history. Among spells including at least one uncertified contract, the median “missing” entitlement duration due to these work periods which are not certified is just below two months. When excluding claimants who certify none of their contracts, the median uncertified duration is 27 days.

Table 2 reports some descriptive statistics characterizing the population of interest. Column (1) describes the whole population of claimants with a PBE date in 2014. The average claimant is 29 years old. He is endowed with 34€ of daily benefits for a potential benefit duration (PBD) of 340 days, after which, if he has still found no job, he will either be able to extend his benefits or receive social welfare instead of the UI. Those claimants close to benefit exhaustion consume nearly all their UI benefits. About half of them

²⁶We leave aside these cases, since those claimants have no interest in benefit extension and therefore in the certification of past employment history.

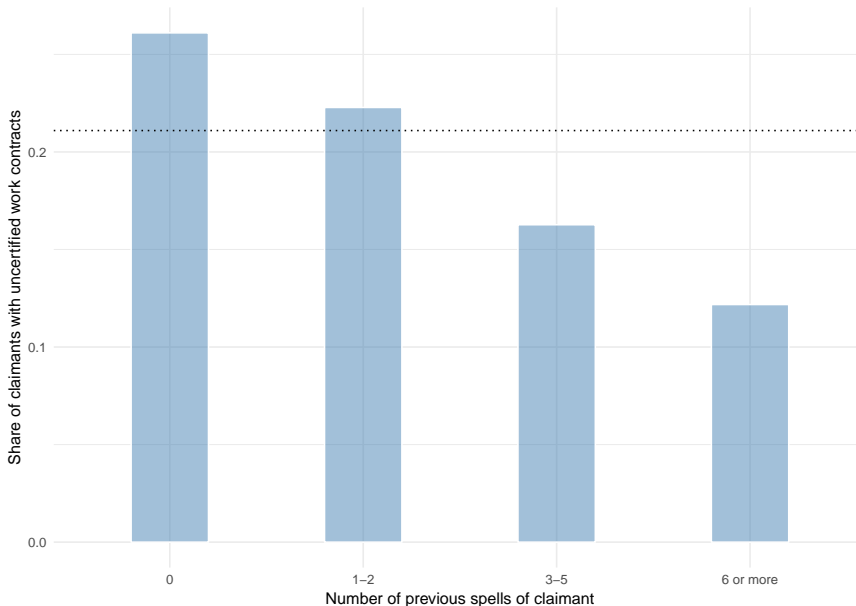
Table 2: Sample description

Period Population	jan-dec.	jan-dec.	aug-sep.	oct-nov.
	all (1)	sample (2)	sample (3)	sample (4)
Age	29.5	30.9	31.1	31.2
Daily wage (€)	55.3	56.8	56.6	57.5
Daily benefits (€)	33.9	34.9	34.8	35.2
PBD (days)	339.6	399.7	407.6	394.8
Actual duration (days)	336.0	392.8	401.2	393.4
<i>Education (%)</i>				
< CAP, BEP	14.7	14.8	15.0	14.9
CAP, BEP	40.5	40.6	40.2	40.5
BAC	25.1	24.6	24.6	24.7
BAC +2	10.8	11.2	11.1	11.6
> BAC +2	8.9	8.8	9.1	8.4
<i># u. spell (%)</i>				
First	31.3	27.0	27.2	25.4
Second	23.9	24.1	25.3	23.2
Third	16.5	17.8	17.6	18.0
Fourth	10.8	11.9	11.7	12.7
≥ Fifth	17.6	19.3	18.2	20.7
N Obs.	294,466	86,458	11,808	18,366

Notes: Claimants below 50 with a Potential Benefit Exhaustion (PBE) date in 2014 and a *declared* affiliation history below 2 years. Claimants from the sample are characterized by a *declared* affiliation history above 122 days when they have two months of benefits left. FNA files (Unédic).

have a high-school degree. The last part of the table highlights the importance of multi-spell unemployment: one third of claimants are in their first unemployment spell, a quarter are in their second spell and more than 17% are at least in their fifth spell. Column (2) displays the same statistics for claimants with a declared affiliation history above 122 days when they have two months of benefits left. They represent nearly 30% of claimants from Column (1). By construction, their PBD is higher than that of the whole population, for they have worked at least 122 days since the start of their unemployment spell. Because they work while unemployed, they are also more likely to start a new unemployment spell after a job and therefore are more often in situations of multi-spell unemployment. Finally, Columns (3) and (4) focuses on the two sub-periods of interest around the 2014 reform and show very similar figures compared to Column (2). The few

Figure 2: Certification behavior depending on unemployment history



Notes: This figure shows the share of claimants who do not entirely certify their employment history, depending on their number of previous unemployment spells. The horizontal dotted line shows the average across all groups (0.211). We focus on claimants with spell end date between April and September 2014, who are eligible to a benefit extension (having accumulated more than 122 days of declared work affiliation). FNA files (Unédic).

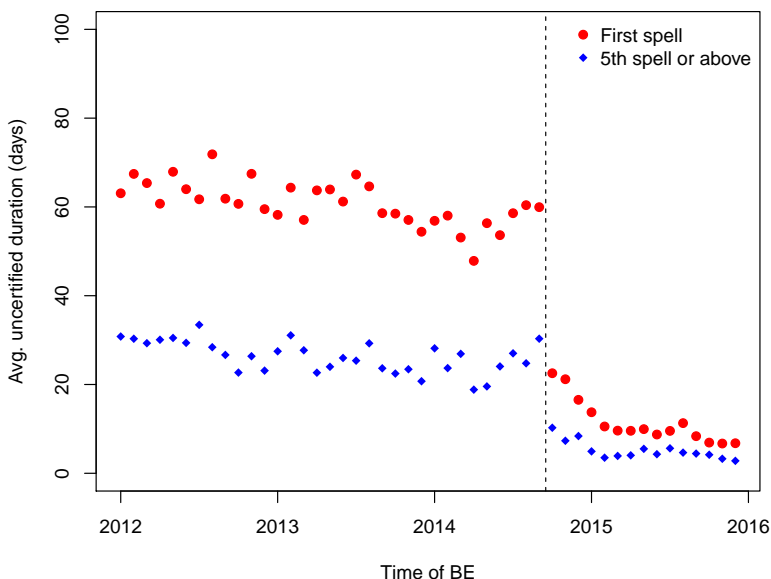
drop in this uncertified duration at the reform appears much smaller than for the less experienced claimants. In the next section, we show that the informative letter increased global knowledge about Employer Certificates and reduced the gap between claimants with different degrees of experience in unemployment.

4 Empirical strategy

The reform impacted unemployment spells for which benefits exhausted on or after October 1st, 2014. We exploit this temporal discontinuity in order to identify the effect of the reform on the certification behavior, in a regression discontinuity (RD) design. Identification in a RD Design rests on the local continuity assumption (LCA) of a continuous and smooth relation between the outcome Y_s and the running variable X_s .

In Subsection 4.1 and 4.2, we consider two potential dates which may serve as assignment variables in our RD design. First we show that the actual *Benefit Exhaustion* (BE) date, which is used in Figure 1 and determines treatment accurately, may jeopardize the local continuity assumption (LCA). In order to solve this issue, we construct a

Figure 3: Experience in unemployment



Notes: This figure shows the average duration of the uncensored affiliation in days among claimants reaching benefit exhaustion a given month for two groups: claimants in their first spell (red dots) and claimants in their fifth spell or above (blue diamonds). We focus on claimants eligible to a benefit extension (having accumulated more than 122 days of declared work affiliation). FNA files (Unédic).

Potential Benefit Exhaustion (PBE) date and use it as an assignment date for the RD analysis, making sure that spells are as good as randomly assigned around the discontinuity according to this assignment date. Solving this issue ensures continuity of the assignment variable density and thus confirms the validity of our RD design (Subsection 4.3). Finally, we show that our sample of interest was not impacted by aspects of the reform other than information (Subsection 4.4).

4.1 Using the *Benefit Exhaustion* (BE) date as the assignment variable: a possible differential selection issue

When a spell comes to an end, whether the *BE* date falls before or after October 1st, 2014 defines actual treatment: if benefit exhaustion falls after October 1st, 2014, the information letter is sent and the new rules of the reform apply. The *BE* date would therefore appear as the natural assignment date in a sharp RD design, since it determines with certainty whether spells are treated or not.

One must however make sure that the chosen assignment date is such that the key identifying assumption of an RD design, the local continuity assumption (LCA) holds.

Under the LCA, observations on each side of the considered threshold may be considered as (locally) randomly assigned. Two types of phenomena in particular may call it into question (Lee and Lemieux [2010]). First, manipulation of the assignment date is problematic in so far as individuals have *precise* control over this date. Second, an issue of differential selection of observations on each side of the discontinuity may arise if the probability of being observed changes sharply at the threshold. In both cases, this may lead to observations on both sides of the threshold not being comparable in terms of unobserved characteristics.

Using the BE date as our assignment variable may raise such a differential selection issue: the process according to which spells reach benefit exhaustion is not the same before and after the reform. By definition, when considering the BE date, we are focusing only on the subsample of individuals who actually exhaust their benefits. This is not the same subsample before and after the reform, as a mechanical result of the switch from the old “comparison-of-benefits” system to the new “renewal-of-entitlement” system. As explained in Section 2, after the reform, recipients who come back to unemployment after a work contract keep on consuming their previous entitlement until they eventually reach benefit exhaustion. By contrast, before the reform, part of the recipients who reached the 122 days of affiliation threshold could proceed to a comparison between remaining benefits and potential new entitlement. In this case, a new spell was initiated, and benefit exhaustion of the previous one was never reached. Considering the *BE* date as our assignment variable would mechanically exclude these claimants from our sample²⁷. This differential selection issue in the RD design has been raised by Lee and Lemieux [2010]. It could jeopardize the validity of our RD design if selection prior to the reform is linked to (unobservable) knowledge of the UI system, which could be the case since the comparison of entitlements was linked to the (de)registration behavior of unemployed workers.

4.2 Defining a valid assignment date: the *Potential Benefit Exhaustion* (PBE) date

In order to overcome this problem, we construct an assignment date which is not subject to this differential selection issue: the *Potential Benefit Exhaustion* (*PBE*) date is defined as the date of benefit exhaustion expected by the claimant when he has N days of UI benefits left and if he does not take a new job. In the baseline estimation, we consider the point in time when a recipient has $N = 61$ days (two months) of remaining UI benefits. We note this date PBE_{-61} and starting from this, we define the PBE date

²⁷ This differential selection issue is made clear by Figure 13 in Appendix C: starting from October 2014 (month 0), there is a strong rise in the frequency of Benefit Exhaustion dates.

as $PBE = PBE_{-61} + 61$ days. The PBE date is therefore defined as the date when benefits would exhaust if the recipient were to consume all her remaining entitlement without any interruption.

Figure 4: Examples of potential benefit exhaustion dates before/after the reform

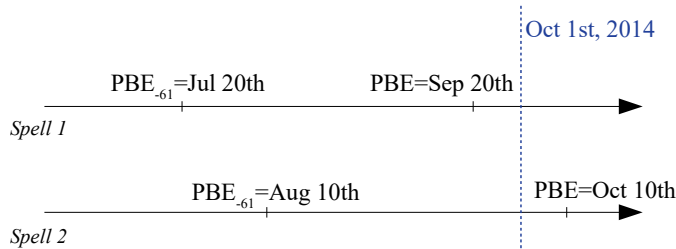


Figure 4 shows an example for two spells exactly identical, except for their initial dates, such that the PBE date falls a few days before the reform for spell 1 and a few days after for spell 2. Two months ahead, from the point of view of date PBE_{-61} , spell 2 is expected to be treated and not spell 1. A BE date only exists insofar as the claimant does reach benefit exhaustion. In contrast, the PBE date can be defined as long as he reaches the PBE_{-61} date. Since for both types of spells, the PBE_{-61} falls before the reform, recipients of spells 1 and 2 have the same probability to reach their PBE_{-61} dates and their PBE dates have the same probability to be observed whether they fall before or after the reform. The differential selection issue is thus solved.²⁸ We consider the PBE date 61 days ahead as a baseline scenario, and we look at alternative scenarios with one or three months in robustness.

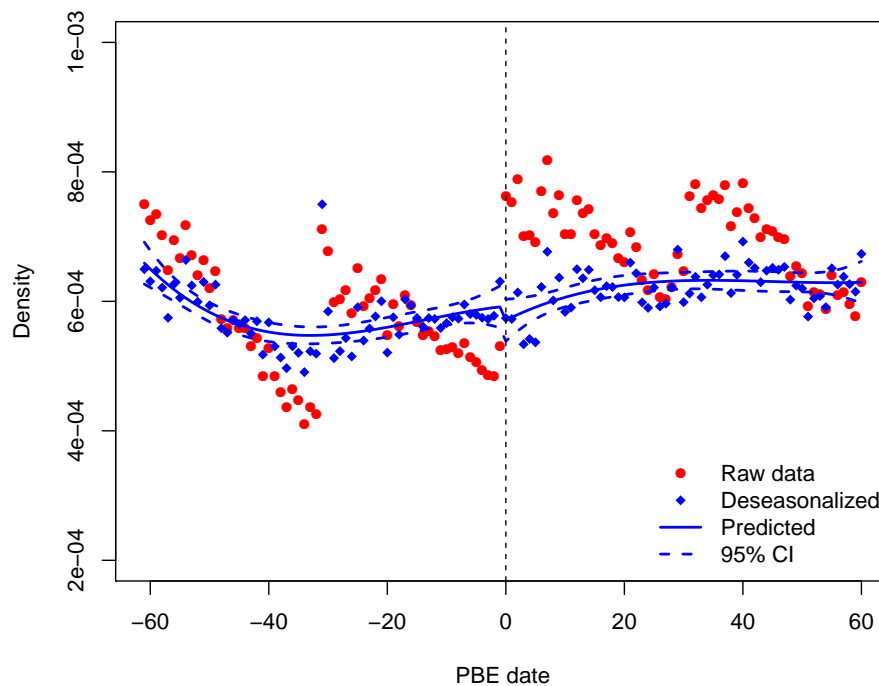
Contrary to the BE date, the PBE is an expectation conditional on information available at date $t(d_0^*)$ when the claimant has d_0^* days of UI benefits left. Since we consider spells within a window around the reform, we can choose d_0^* large enough so that $t(d_0^*)$ always falls before the reform. This PBE date has two interesting properties: (i) first, it only requires claimants to reach the date $PBE_{-d_0^*}$ and not benefit exhaustion, which solves the differential selection issue since treated and control spells have the same probability to reach $PBE_{-d_0^*}$; (ii) second, the set of information available at date $PBE_{-d_0^*}$ depends on work contracts terminated before the reforms, making manipulation virtually impossible.

²⁸The PBE also solves potential issues of manipulation of the assignment date since manipulation would most probably occur close to the date of benefit exhaustion.

4.3 Local continuity assumption and fuzzy RD design

Using the PBE date rather than the actual BE date (and correcting for seasonality, as developed in Appendix D) jointly allow us to test the local continuity assumption by making sure that the density of the assignment variable is continuous at the threshold of interest. When it comes to RD design, the foremost concern is potential manipulation in order to be located on either side of the threshold. Since we are not looking at actual benefit exhaustion but potential benefit exhaustion defined $N = 61$ days earlier (for example), any issue of manipulation close enough to benefit exhaustion is taken care of. However, there may remain a concern about manipulation ahead of the PBE_{-61} date. In the current case, unemployed workers whose PBE_{-61} date lies between the announcement of the reform (Mai 15th, 2014) and its implementation (October 1st, 2014) may have had an interest to take a new job, a sick-day leave or any other way to postpone the end of their spell until after the reform.

Figure 5: Density of the PBE date

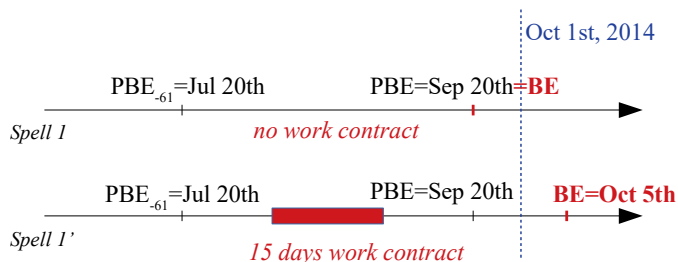


Note: This figure displays the distribution of the Potential Benefit Exhaustion (PBE) date by day, in a four months window around the reform. Red dots display the raw density while blue diamonds show detrended data. The plain blue line represents third order polynomials estimated below and above the threshold and the dashed blue lines represent the 95% confidence interval. The outlier is August 31st 2014. FNA files (Unédic).

Figure 5 displays the distribution of the raw (red dots) and detrended (blue diamonds) PBE date in a four months window around the reform. Once the monthly trend in raw data is removed (Appendix D), the distribution of the PBE date is smooth around the date of the reform. Polynomial approximations on each side of the threshold confirm the absence of local discontinuity in the distribution of the running variable. Claimants are therefore not likely to have manipulated their spells in order to take advantage of the reform. This comforts us regarding the validity of the RD design using the PBE date as an assignment variable.

Using the PBE date as a running variable calls for a Fuzzy RD design. The actual Benefit Exhaustion (BE) date may indeed be postponed by any new work contract happening after PBE_{-61} , as shown on Figure 6. In the case of spell 1, the recipient takes on no work contract between PBE_{-61} and PBE and his Potential Benefit Exhaustion coincides with actual Benefit Exhaustion: $BE = PBE$. In the case of spell 1', the recipient takes a 15 days work contract which postpones actual Benefit Exhaustion by 15 days: $BE > PBE$. Note that the BE date can only be postponed, but it cannot happen earlier in time: $BE \geq PBE$.²⁹

Figure 6: Examples of treated/non-treated observations with the same assignment date



Therefore, for PBE dates after the reform, since actual Benefit Exhaustion necessarily happens after the reform, the probability of treatment is close to 1³⁰. For PBE dates before the reform, spells may actually be treated or non-treated depending on work history posterior to the PBE_{-61} date. For spell 1' of Figure 6, the claimant will ultimately

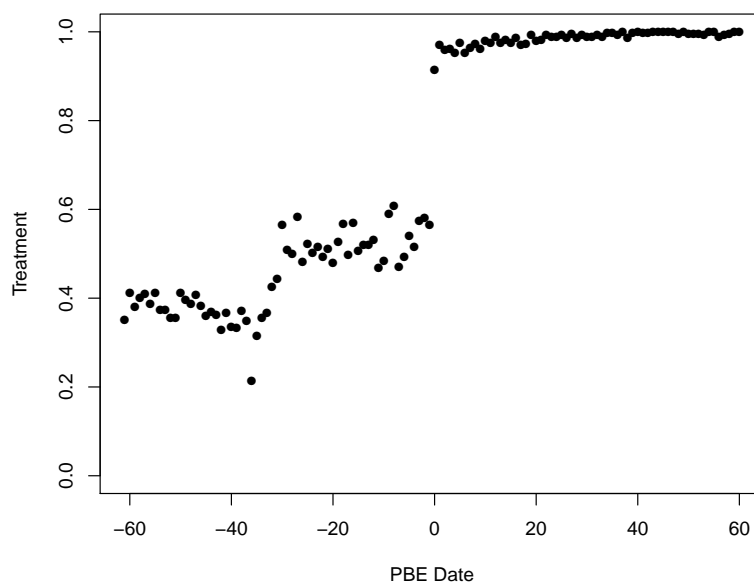
²⁹Except for some of the individuals in our sample who definitively left unemployment. Individuals who left unemployment before the 1st of October 2014 and never reappeared, while their PBE date falls after the threshold, are considered as non-treated as they were never unemployed after the threshold. They constitute a minority of our observations and classifying them as treated or non-treated does not impact our estimations.

³⁰Such a one-sided fuzzy design implies that there are no defiers (apart from the few exceptions mentioned) and that the treatment effect is identified on compliers.

benefit from the reform, but this could not have been anticipated by the econometrician who consider unemployment trajectories from the point of view of the PBE_{-61} date.

Figure 7 displays the treatment D_s as a function of the running variable X_s . Since the effective BE date of the spell can only be postponed compared to the PBE date, observations located above the threshold will be treated. In contrast, some spells that should have terminated before the reform according to the PBE date were extended until after as a result of claimants taking new jobs during these spells. Therefore, the average value for D is positive before the reform.³¹

Figure 7: Treatment as a function of the running variable



Note: This figure displays the proportion of spells ending after the enactment of the reform, October 1st 2014, ($D = 1$) as a function of the day of the PBE date, in a four month window around the reform. FNA files (Unédic).

A direct application of a sharp design using the PBE date as a running variable would lead to estimate an Intent-To-Treat effect, since some spells that we consider as non treated would in fact be treated. In order to estimate a proper Treatment effect, we should rely on the Fuzzy RD design as described by [Lee and Lemieux \[2010\]](#); [Angrist](#)

³¹The fact that the proportion of treated units is not smoothly increasing before the threshold is linked to the seasonality issue mentioned above. When the subpopulation which causes the monthly seasonality is excluded from the sample, the proportion of treated units increases smoothly before the threshold, as shown by Figure 14 in Appendix. Furthermore, notice that the estimations presented in section 5 are not sensitive to the inclusion or exclusion of the subsample of individuals causing the monthly seasonality issue.

and Pischke [2009]; Imbens and Lemieux [2008].³² We therefore instrument treatment with an index of whether the *PBE* date exceeds the date of the reform or not. Taking the notations of Lee and Lemieux [2010], we estimate through 2SLS:

$$\begin{aligned} Y_s &= \alpha + \tau D_s + f(X_s - c) + \epsilon_s \\ D_s &= \gamma + \delta T_s + g(X_s - c) + \nu_s \end{aligned} \tag{1}$$

where Y_s is the outcome of interest for spell s , X_s is the *Potential Benefit Exhaustion* (*PBE*) date of spell s , c is the date of the reform (October 1st, 2014), $T_s = \mathbb{1}[X_s \geq c]$ indicates that benefits are expected to expire after the reform according to the *PBE* date and $D_s = \mathbb{1}[BE_s \geq c]$ indicates whether Benefit Exhaustion occurs after October 1st 2014 and the spell is ultimately treated or not. $f(\cdot)$ and $g(\cdot)$ are polynomials of the *PBE* date centered around the reform and without constant term, such that $f(0) = g(0) = 0$. τ captures the treatment effect and $\delta\tau$ the intent-to-treat (ITT) effect that would be estimated in a direct RDD of the outcome on the assignment variable. Intuitively, δ captures the fuzziness of the RD design measured by the covariance between the treatment and the index of assignment before or after the reform. The lower the δ , the fuzzier the design and the bigger the discrepancy between the ITT and the treatment effects. Parameter $\alpha = \lim_{X_s \rightarrow c^-} \mathbb{E}[Y_s | D_s = 0, X_s]$ is the average initial level of the outcome variable for non treated spells when approaching the date of the reform. Finally, ϵ_s and ν_s are residuals.

4.4 Identifying the effect of the information letter

As explained in Section 2, the “renewal-of-entitlement” reform reduced the minimum affiliation duration required for UI benefit extension. In order to estimate the pure effect of information on the certification behavior, we focus on claimants whose affiliation was always above 122 days and below 730 days, meaning that they could be granted benefit extension before as well as after the reform. Affiliation is defined here as the total work history *declared* by the claimant³³ until the PBE_{-61} date at which we consider his situation.

We can check with Table 1 that these claimants were only impacted by the introduction of the informative letter and not by the other components of the reform. First, they were always eligible to a benefit extension provided they certified their work history, because their affiliation is above 122 days.

³²In the terminology of Cattaneo et al. [2020], our framework is a one-sided fuzzy design since the probability of treatment is close to 1 to the right of the assignment variable threshold.

³³Considering solely *certified* work contracts, which would be endogenous to the outcome of interest.

Second, they expect to receive the allocation $\{b_1, B_1\}$ both before and after the reform. Indeed, we select claimants with low remaining benefits and a potentially high new entitlement. In the comparison-of-benefits system, their best choice was the new entitlement $\{b_1, B_1\}$. Choosing the previous amount of daily benefits would take a disproportionately high b_0 compared to b_1 . Less than 0.1% of spells recipients in our sample who were subject to a comparison-of-benefits chose the remaining benefits from the previous spell rather than the new entitlement. Consequently, each day worked and certified ensures an extra day of entitlement: before the reform because we focus on claimants approaching benefit exhaustion who always choose the new entitlement and after the reform by the definition of the renewal-of-entitlement. The effective incentives to certify work contracts through Employer Certificates remained unchanged by the reform.

Third, the other components of the 2014 reform related to jobs taken while unemployed³⁴ cannot have had any impact on the choice of work contracts or on the certification behavior that we examine. Since we consider jobs finishing at least N days before the PBE date (i.e. terminated before the PBE_{-N} date) and we estimate the treatment effect on spells whose PBE date lies in a $2N$ days window around the reform, the work contracts we analyze necessarily terminated before October 1st, 2014 by construction.

Beyond the pure components of this reform, some changes have occurred during this period, but our design prevents our estimation to be impacted by these trends. First, caseworkers of Employment Agencies might have more systematically asked unemployed workers for their Employer Certificates associated with jobs started after the implementation of the reform. However, our focus on work contracts finished before the reform prevents this trend from jeopardizing our identification of the certification behavior³⁵.

Finally, a potential threat for our interpretation of the letter effect would be for non-certification to result from a strategic behavior rather than actual non-take-up. Certifying contracts impacts not only the duration of the following UI entitlement, but also daily benefits and may therefore imply a trade-off between entitlement duration and daily amount. Since daily benefits are proportional to the average wage of past certified jobs, claimants could have an incentive not to certify low-paid contracts in order to increase their future daily benefits. Although we do not know the wage associated to each job, we know whether the daily benefit chosen in the comparison-of-benefits system is the previous (and non-manipulable) one or the new (manipulable) one. If non-certification resulted from such strategic behavior, it should be predominant for

³⁴ *Cumul allocation-salaire* and *Droit d'option*.

³⁵ This concern is not related to a legal obligation but to a change in practices. The timing of these evolution has been confirmed through a direct exchange with the French National Employment Agency.

recipients who perceive their new daily benefits. However, the measured non-certification behavior is identical whether people chose their previous or new daily benefits.

5 Results

In this Section, we first show graphical evidence of the strong impact of the information letter on the take-up of UI benefit extensions. We then estimate the magnitude of this effect within a Fuzzy Regression Discontinuity Design and discuss robustness of these estimates. Finally, we analyze the heterogeneous effects of the reform.

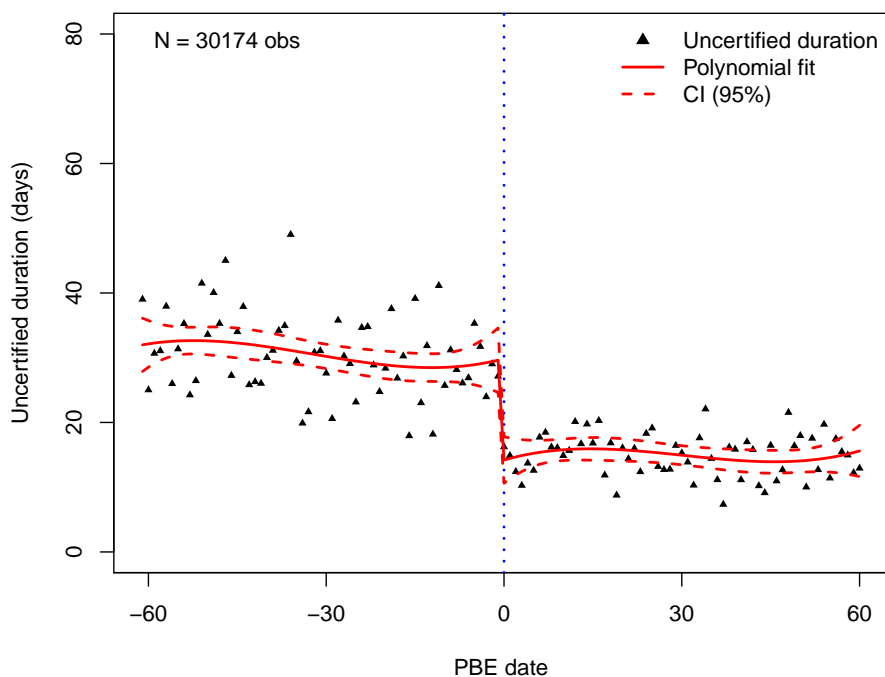
5.1 Graphical evidence

Figure 8 highlights a smooth and nearly constant total duration of uncertified work contacts over time, except at the discontinuity induced by the reform. Until October 1st 2014, uncertified employment history was as high as one month on average, meaning that registered unemployed workers used to loose at least one month of potential benefits and even more if this non-certification implied their affiliation to drop below the 122-days threshold. With the reform, this duration is reduced to 20 days. This pattern is dampened compared to Figure 1 since, in this fuzzy RD design, some spells that actually end up being treated contaminate the pre-reform period.

Figure 9 displays similar drops in the proportion of unemployment spells for which at least one job has not been certified (red dots) and in the proportion of those for which no job has been certified (blue squares). Before the reform, the share of incomplete take-up is already quite low, below 30% of unemployment spells, mainly due to the automatic certification of temporary contracts. The information letter appears as efficient to reduce the share of claimants who never certify any contract and the mass of uncertified days of work. On the extensive margin, some claimants were granted benefit extension while it would not have been the case before the reform. On the intensive margin, others received higher benefit extension than they would have absent any reform. These reduced-form effects correspond to the intent-to-treat effect and a fuzzy design should be developed in order to measure a proper treatment effect.

Looking at explanatory variables around the reform is also helpful to track potential flaws in the RD validity (on top of evidence of the continuity of the assignment date density provided in section 4). Figure 16 in Appendix E displays averages of six variables per PBE day in a four months window around the reform: age, reference wage, replacement rate, number of work contracts during the spell, potential benefit duration, and plant size of the last job before the current unemployment spell. All these variables appear smoothly distributed around the reform.

Figure 8: Duration of uncertified work around the 2014 reform

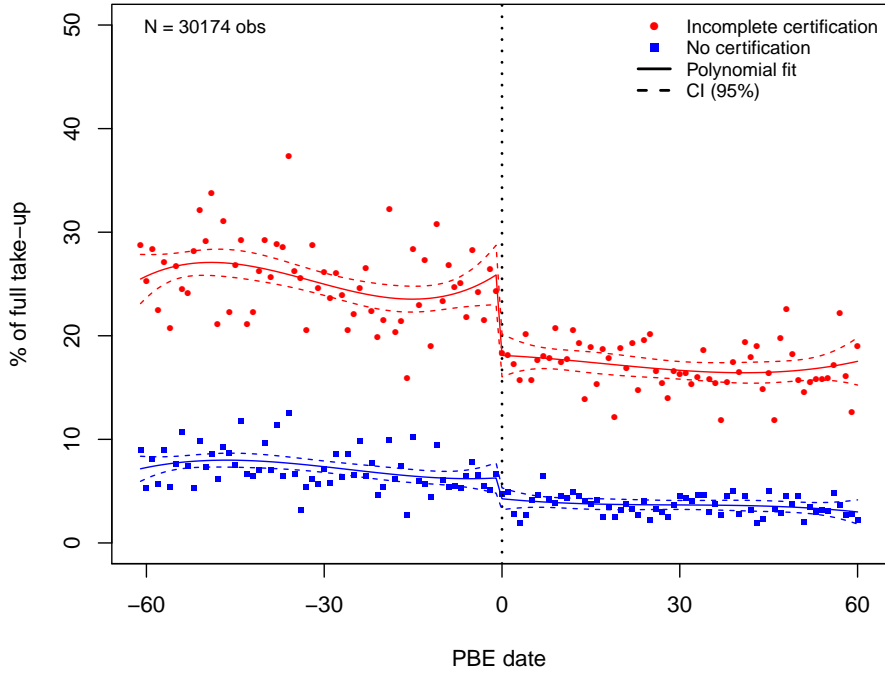


Note: This figure displays the total duration of uncertified work contracts as a function of the day of the potential benefit exhaustion (PBE) date, in a four months window around the reform. Each dot shows the average proportion per day. The red line represents third-order polynomials estimated at the left and at the right of the discontinuity. Red dashed lines represent 95% confidence intervals. FNA files (Unédic).

5.2 Estimates from Fuzzy design

Main regression results. We estimate the Fuzzy RD Design model in order to quantify the incidence of the reform on three outcomes of interest: the share of unemployment spells for which at least one past work contract has not been certified (incomplete take-up), the share of unemployment spells for which no jobs have been certified (non-take-up) and on the total duration of uncertified contracts (uncertified duration). In line with Lee and Lemieux [2010], we favor a local linear regression, which reduces the importance of the bias by an order of magnitude compared to kernel regressions when the slope of the regression line is not equal to zero. Moreover, we show in Appendix E that our results are robust to alternative polynomial estimations and different window widths. In the baseline estimation, the PBE date is defined when remaining benefit duration is equal to two months. Consistently, we estimate the model on a four-months window around the reform, which insures that all jobs taken into account in the past employment history

Figure 9: Incomplete certification of employment history around the 2014 reform



Note: This figure displays the proportion of unemployment spells for which at least one job has not been certified (red dots) and the proportion of those for which no job has been certified (blue squares) as a function of the day of the potential benefit exhaustion (PBE) date, in a four-months window around the reform. Each dot shows the average proportion per day. The plain lines represent third-order polynomials estimated at the left and at the right of the discontinuity. Dashed lines represent 95% confidence intervals. FNA files (Unédic).

end before October 1st 2014 and thus that they are not affected by the reform.

Table 3: Fuzzy RDD by local linear regression

Uncertified Duration	Incomplete Take-up	Non Take-Up
-30.4	-0.144	-0.054
(3.8)	(0.022)	(0.011)

Note: This table shows the treatment effect estimated from model 1 through 2SLS using a local linear regression in a four-months window around October 1st, 2014. Estimates are all significant at 1%. $N = 30,174$ observations. Standard errors are in brackets. FNA files (Unédic).

Table 3 displays the effect of the reform $\hat{\tau}$ on the three outcomes of interest. The informative letter had a positive impact on the certification of past jobs: incomplete take-

up was significantly reduced by more than 14 pts, among which 5 pts are related to a drop in non-take up. Overall, this reduction went on average through the certification of one extra month of past work contracts. Absent any changes in incentives, this automatic mailing highlighted the role of Employer Certificates for UI benefit extensions. The magnitude of this effect is related to the timing of the adjustment: claimants have no deadline to send their employer certificates and can send them even after reaching benefit exhaustion. From the first-stage of the regression of uncertified duration, we estimate $\hat{\delta} = 0.420$ (0.002). Multiplying it by the treatment effect, we find an intent-to-treat effect of 13 days on the uncertified duration, which is consistent with the gap observed at the reform on Figure 8.

5.3 Robustness

In this subsection, we provide with robustness checks as regards: *(i)* the estimation window, *(ii)* two placebo reforms, *(iii)* the choice of the bandwidth, *(iv)* the functional form.

Table 4 provides evidence of treatment effect stability for alternative estimation windows and for two placebo reforms around October 2013 and October 2015. First, in the baseline estimation, we arbitrarily defined the PBE date when the remaining benefit duration is equal to two months. We may set any other value, provided that the PBE date is not too close to benefit exhaustion³⁶, and not too far from it because the design would become extremely fuzzy. Here, we show that the treatment effect remains nearly unchanged if we define the PBE date when the remaining benefit duration is equal to one month (three months) and when we consistently set the estimation window to two months (six months) instead of four months.

Second, the treatment effect could be an unintended consequence of our procedure of data construction. In order to discard this eventuality, we replicate the exact same procedure around two placebo reforms: one on October 1st 2013 and the other on October 1st 2015. We replicate the exact same steps as for the baseline estimation and find no significant treatment effect (only a significantly positive effect for 2015). Figure 15 in Appendix E replicates Figures 8 and 9 around these two placebo reforms. It shows no significant gap at the threshold in the outcome variables, meaning that the certification behavior of claimants remained unchanged.

Third, a choice must be made regarding the bandwidth. Figure 10 presents the treatment effect estimated through local linear regressions for different bandwidths around the date of the reform. Since we rely on exhaustive data and have numerous observa-

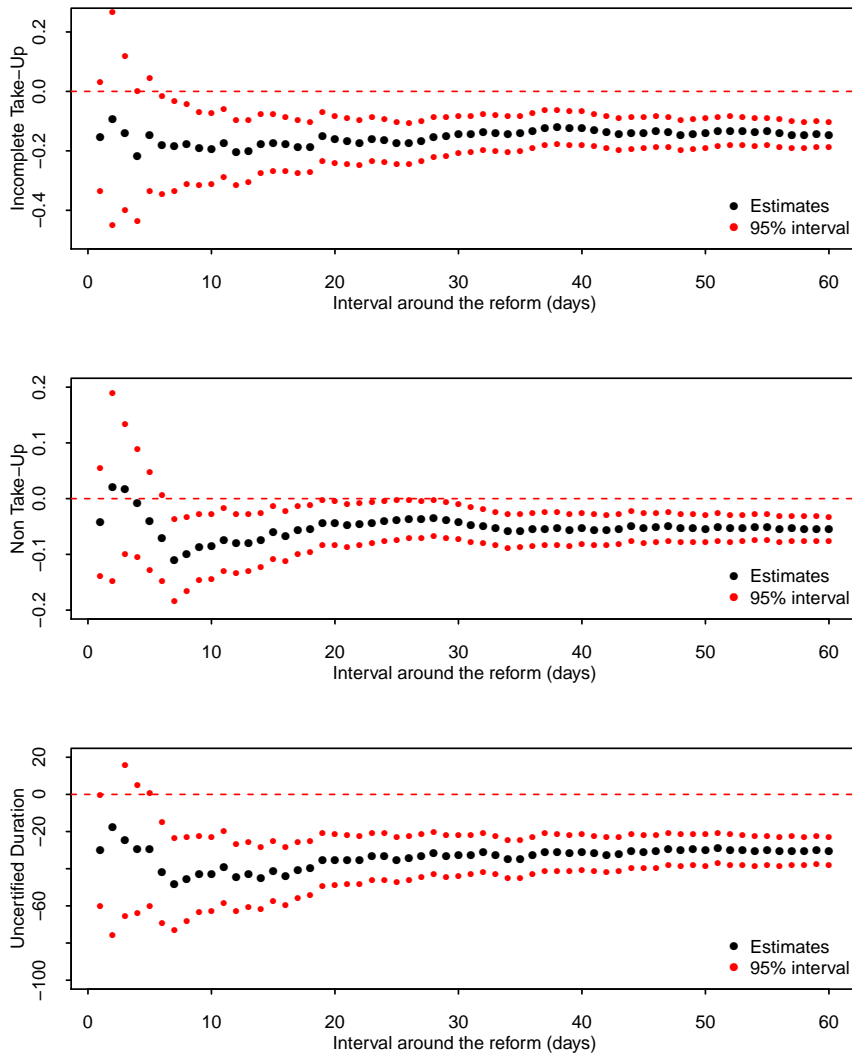
³⁶In which case we would either have to narrow the window within which we are examining the reform or be facing a selection issue.

Table 4: Two placebo reforms and different estimation windows

	Outcome		
	Uncertified Duration	Incomplete Take-up	Non Take-Up
<i>Baseline</i>			
	-30.4 (3.8)	-0.144 (0.022)	-0.054 (0.011)
<i>Four-Months window</i>			
2013	-11.1 (6.9)	-0.027 (0.033)	-0.017 (0.021)
2015	-0.3 (0.9)	0.024 (0.007)	0.025 (0.002)
<i>Two Months window</i>			
2013	13.8 (8.4)	0.069 (0.038)	0.032 (0.025)
2014	-33.6 (5.2)	-0.182 (0.029)	-0.068 (0.015)
2015	-1.8 (1.1)	0.005 (0.008)	0.009 (0.003)
<i>Six Months window</i>			
2013	1.0 (6.4)	0.010 (0.031)	0.033 (0.020)
2014	-21.7 (3.7)	-0.103 (0.023)	-0.041 (0.011)
2015	1.0 (1.0)	0.027 (0.007)	0.021 (0.003)

Note: This table presents the treatment effect estimated from model 1 through 2SLS, using a local linear regression in a four-month window around October 1st 2014. Regressions are run separately for three different outcomes: the share of incomplete takers, the share of non-takers and the uncertified duration of past work contracts. *From the top to the bottom*: the first part shows the baseline estimation (four months window around October 1st 2014), the second shows similar estimations for two placebo reforms around October 1st 2013 and October 1st 2015, the third and the fourth show the same estimations taking respectively a two months and a six months window instead of a four months window around the reform. Standard errors are in brackets. FNA files (Unédic).

Figure 10: Treatment effect for different bandwidths



Note: This figure displays the treatment effect estimated from a local linear regression for different sizes of the bandwidth, from two days up to two months around the reform. The day of the reform (October 1st, 2014) has been removed. Black dots show the value of the estimates and red dots represent the value of the 95% confidence interval. *From the top to the bottom*: incomplete take-up, non-take-up and uncertified duration. FNA files (Unédic).

tions for each *PBE* date, we are able to focus very precisely on the days around the reform. Therefore, we reproduce the regressions of Table 3 for time intervals from two days to 61 days around the reform. Each panel corresponds to an outcome of interest: share of incomplete takers, share of non-takers and uncertified duration from the top to the bottom. When we increase the size of the bandwidth, estimates become quickly significantly positive, from a two week window around the reform. The estimation is very stable over the range of bandwidths.

Fourth, in the local linear estimation, the local continuity assumption takes the form of a polynomial of order 1 of the assignment variable. However, the smooth relation between the outcome and the running variable could take a more general form and the estimated treatment effect should not radically depend on the hypothesis regarding this relation.

Complementary estimates regarding the robustness of the treatment effect are displayed in Appendix E. Table 8 presents results from polynomial estimations, where order of polynomials ranges from 0 to 4 and coefficients of polynomials are constrained to be identical below and above the threshold. For each one of the three outcomes, the treatment effect is very stable across specifications, always significantly negative in 2014, never significant in 2013 (except for $P = 0$) and not significant or slightly positive in 2015.³⁷ Table 10 shows that treatment effects remain unchanged when we allow for the first-degree polynomials $f()$ and $g()$ to have different slopes on each side of the discontinuity in the local linear regression. Table 11 displays treatment effects for different polynomials ranging from order 0 to order 4 and for different bandwidths from 10 days to 120 days around the reform, taking the uncertified duration as the outcome variable. Once again, parameters are significantly negative and stable for bandwidth wider or equal to 20 days around the reform, independently from the order of the polynomial, which confirms the robustness of our estimates to a wide range of specifications.

5.4 Heterogeneous effects of the reform

In this section, we investigate for heterogeneous effects of the informative mailing. For this purpose, we compare estimates for the treatment effect $\hat{\tau}$ and for the average initial level of the outcome variable for non treated spells when approaching the date of the reform, defined as $\hat{\alpha} = \lim_{X_s \rightarrow c^-} \mathbb{E}[Y_s | D_s = 0, X_s]$.

Table 5 relates the certification behavior to the experience in unemployment. Each line displays estimates among claimants who achieved the same number of spells until

³⁷Table 9 in Appendix E presents corresponding ITT effects observed on Figure 9 and estimated from the direct regression of the outcome Y_s on the index T_s . This table confirms that parameters stability is already a feature of the ITT estimation.

Table 5: Uncertified duration and experience in unemployment

		Uncertified duration					
<i>U. spell</i>	#	(1)	(2)		(3)		
		Treat.	Before		After	Obs.	
		$\hat{\tau}_i$	$\hat{\alpha}_i$	$\frac{\hat{\alpha}_i}{\hat{\alpha}_5}$	$\hat{\alpha} + \hat{\tau}$	$\frac{\hat{\alpha}_i + \hat{\tau}_i}{\hat{\alpha}_5 + \hat{\tau}_5}$	
First	$i = 1$	-38.7 (7.8)	58.8 (5.7)	3.06	20.1	2.18	7874
Second	$i = 2$	-33.9 (8.0)	49.9 (5.9)	2.60	16.0	1.74	7247
Third	$i = 3$	-21.1 (8.3)	35.6 (6.2)	1.85	14.5	1.58	5387
Fourth	$i = 4$	-24.6 (10.9)	35.2 (8.4)	1.83	10.6	1.15	3719
Fifth or above	$i = 5$	-10.0 (6.8)	19.2 (5.3)	1	9.2	1	5947

Note: This table shows the intercept α and the treatment effect τ estimated from model 1 through 2SLS, where the outcome is the uncertified duration of past jobs, using a local linear regression in a four-months window around October 1st 2014. Regressions are run for five groups of experience in unemployment. Standard errors are in brackets. FNA files (Unédic).

October 2014. Column (1) shows that the treatment effect is strongly decreasing with the experience in unemployment: the uncertified duration fell by 40 days for claimants in their first spell while it is not significantly reduced for those in their fifth spell or above.³⁸ Column (2) displays this duration before the reform in days ($\hat{\alpha}_i$) and relatively to the high experience group ($\hat{\alpha}_i/\hat{\alpha}_5$). Column (3) presents similar statistics after the reform. Before the reform, claimants in their first spell were missing nearly two months of benefit extension by not certifying past work contracts, which is three times the potential benefit duration forgone by the most experienced claimants. After the reform, this ratio scales down to 2.2. The informative mailing partially compensated for the discrepancy in certification behavior between claimants with different level of experience in unemployment.

Table 6 provides information about heterogeneity of the treatment effect along three dimensions: the reference wage, age and the level of education. The reference wage is an average of the wages the claimant earned during a one year period before the start

³⁸Also, this treatment effect is stronger than simply multiplicative, since it reduces the initial uncertified duration by $\tau/\alpha \approx 2/3$ in the less experienced group and only by 1/2 in the more experienced group.

Table 6: Heterogeneity of treatment on uncertified duration

	Reference wage (quartile)				Age (quartile)				Education (group)			
	Q_1	Q_2	Q_3	Q_4	Q_1	Q_2	Q_3	Q_4	G_1	G_2	G_3	G_4
Initial ($\hat{\alpha}$)	52.0 (5.5)	44.1 (5.9)	30.9 (5.3)	45.5 (5.9)	29.9 (4.1)	47.4 (5.2)	35.4 (5.6)	63.0 (6.8)	51.4 (7.4)	41.1 (4.1)	41.6 (5.6)	48.5 (7.3)
Treat. ($\hat{\tau}$)	-33.9 (7.6)	-29.1 (7.9)	-19.2 (6.9)	-32.3 (7.8)	-20.2 (5.5)	-36.3 (7.0)	-18.6 (7.5)	-44.8 (9.1)	-36.4 (9.7)	-29.1 (5.4)	-30.1 (7.4)	-26.5 (10.0)
Obs.	7544	7543	7539	7548	6477	7986	7760	7951	4500	12181	7436	6054

Note: This table shows the intercept α and the treatment effect τ estimated from model 1 through 2SLS, where the outcome is the uncertified duration of past jobs, using a local linear regression in a four month window around October 1st, 2014. Regressions are run separately for four groups of reference wage (*left*), four age groups (*middle*) and four groups of education level (*right*). The wage and age groups are respectively defined by the quartiles of the wage and age distributions. The groups of education are defined by diploma: G_1 stands for claimants with an education level below the CAP/BEP degree, G_2 for the CAP/BEP degree, G_3 for the BAC degree and G_4 for the BAC+2 level or above. Standard errors are in brackets. FNA files (Unédic).

of his current unemployment spell. It is used to determine the replacement ratio and the daily benefits he will receive. This reference wage can be considered as a proxy for the reservation wage the unemployed worker expects to earn if he finds a permanent job. The reform does not seem to have impacted the four wage groups in significantly different manners. Second, prior to the reform, older claimants were characterized by a higher uncertified duration compared to younger ones, which could result from a selection effect if older unemployed workers are also further away from the labor market and administrative procedures. Through provision of information about Employer Certificates, the letter had a stronger additive impact on older claimants (but a similar multiplicative one). Third, the last part of this table shows estimates for four levels of education, from the lower educated group G_1 to the most educated group G_4 . Here again, the informative mailing seems to compensate for initial differences in certification behavior. The less educated, who were also losing a longer potential duration of benefit extension, experience a stronger impact of the reform than the most educated, although the difference between these treatment effects is not significant.

Through a widespread information about the renewal of entitlement and the role of Employer Certificates, the reform has mostly reduced the gap in certification behaviors between claimants with different levels of experience in unemployment.

6 Conclusion

This paper focuses on the incomplete take-up of unemployment benefit extensions by claimants reaching benefit exhaustion. In principle, each day worked should entitle to one extra day of benefits up to 2 years. However, before the 2014 reform, more than 20% of claimants eligible for an extension of their entitlement did not complete all administrative procedures. They failed sending part or all of their Employer Certificates, a document required by the Employment Agency in order to certify that the duration of a past job entitles for an equivalent extension of Potential Benefit Duration.

We show that the informative mailing automatically sent to claimants nearing benefit exhaustion had a strong impact on their certification behavior. The share of claimants who do not fully certificate their work history decreased by 14 points, which represents a rise in the Potential Benefit Duration by one month. A back-of-the-envelope calculation shows that the average gain from the introduction of this letter is at least 300€ per claimant.

This analysis highlights the effectiveness of informational interventions in order to increase the take-up of UI benefit extensions. In particular, it shows how some claimants, especially those with a low experience in the unemployment insurance system, could not fully benefit from it. The first consequence of this is the inequity generated between low and high-experienced unemployed who are entitled to the same rights. The second is the lack of efficiency it might generate: not benefiting from the full insurance system may make low-experienced unemployed workers less sensitive to the incentives that are associated with the insurance scheme. We show here that such situation might be corrected for. In the case studied here, a simple information letter has considerably compensated the discrepancies between low and experienced workers : it has decreased by 40 days the uncertified duration of unemployment for the low-experienced unemployed, without affecting the one of high-experienced unemployed.

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Appendix

A Letters

This section presents the main letters that are sent to claimants depending on their situation. One month before their Potential Benefit Exhaustion (PBE) date, they receive either letter in Figure 11 if information about past employment history is too scarce or letter in Figure 12 if it is partially known but some Employer Certificates are missing.

Figure 11: Letter if renewal is undetermined

Références à rappeler

numéro identifiant [REDACTED]
numéro de dossier 987 [REDACTED] le 26 avril 2018
numéro d'action 99

KCRH

Objet : **Examen de rechargement des droits**

Madame [REDACTED]

La fin de votre indemnisation est prévue le 26 mai 2018. Dans le cadre des droits rechargeables, nous devons examiner si de nouvelles allocations d'aide au retour à l'emploi peuvent vous être attribuées*.

A ce jour vous ne remplissez pas les conditions requises pour bénéficier d'un rechargement de vos droits.

Si des attestations employeur sont encore en votre possession, merci de nous les retourner pour prise en compte, y compris si vous perdez un ou plusieurs emplois avant la fin de vos droits.

A défaut, nous étudierons la demande d'allocation de solidarité spécifique qui vous a été adressée. Il vous appartient de la compléter, la signer et nous la renvoyer avec les justificatifs demandés.

Merci de nous adresser ces justificatifs par voie dématérialisée à partir de votre espace personnel sur le site www.pole-emploi.fr ou en passant par l'application mobile, rubrique « Mes échanges avec Pôle emploi », service « Transmettre et suivre un document », contexte « Actualisation ».

Vous pouvez également numériser vos justificatifs en vous rendant dans votre agence à l'adresse indiquée en bas de page.

Vous pourrez suivre l'état d'avancement de ces justificatifs depuis votre espace personnel.

Nous vous prions d'agréer, Madame [REDACTED] nos salutations distinguées.

Le Directeur de l'agence

Notes: This letter has been sent on April 26th, one month before the Potential Benefit Exhaustion (PBE) date (May 26th). It mentions that the claimant “does not fulfill the necessary conditions in order to benefit from a renewal of entitlement” and strongly advises him to send any “employer certificate in his possession”.

Figure 12: Letter if some Employer Certificates are missing

Objet : Examen de rechargement des droits

Madame [REDACTED],

La fin de votre indemnisation est prévue le **04 avril 2015**. Dans le cadre des droits rechargeables, nous devons examiner si de nouvelles allocations d'aide au retour à l'emploi peuvent vous être attribuées*.

Depuis le **01 septembre 2011**, vous avez déclaré avoir repris une ou plusieurs activités. Sur l'ensemble des heures déclarées, vous nous avez justifié **909 heures** de travail. Nous n'avons pas reçu toutes les attestations d'employeur.

Si ces activités ont cessé, veuillez nous retourner l'original des attestations d'employeur correspondantes encore en votre possession.

A cette fin, vous trouverez ci-dessous, un récapitulatif des périodes à justifier.

JOIGNEZ LA OU LES ATTESTATIONS D'EMPLOYEUR DES PERIODES CI-DESSOUS	
-mois de septembre 2014	

Afin d'étudier l'éventuel rechargement de vos droits, vous devez nous retourner ce courrier, accompagné des justificatifs nécessaires au plus tard le **21 mai 2015**.

Si avant la fin de vos droits, vous perdez un ou plusieurs emplois, veuillez nous retourner la ou les attestation(s) d'employeur originale(s) sans délai.

Nous vous prions d'agréer, Madame [REDACTED], nos salutations distinguées.

Le Directeur de l'agence

Notes: This letter mentions that 909 hours of work (6 months) have already been certified and that the claimant should send an Employer Certificate for the month of September 2014.

B Additional details on the French UI system

Generalities. Daily unemployment benefits b range from 57% to 75% of a reference wage w determined as the average wage on a 4 to 12 months period before the termination of the last job in the employment history. Benefits are computed as: $b \equiv \min \{0.75w, \max \{28.58\alpha, 0.404w + 11.72\alpha, 0.57w\}\}$ where $\alpha \in [0, 1]$ is the ratio of hours worked over the legal employment duration ($\alpha = 1$ for a full-time job.).

When a worker becomes unemployed, benefit duration is determined by his past employment history. In France, any day worked while affiliated to the UI agency ensures one day of benefits. To be entitled with UI, total employment history should be longer than 4 months within the last 28 months (before 50 years old) or 36 months (above 50). Maximum duration is 2 or 3 years depending on this same age threshold. In order to receive UI benefits, the claimant should not have deliberately quit his job.

For unemployed workers with a previous unemployment spell, the employment history considered for the renewal of UI entitlement begins at the start of the last unemployment period and must be within 28 months from their new unemployment period.

Former “comparison-of-benefits” system. In order to clarify how the comparison was operated, let us note b_0 the daily benefits and d_0 the duration (in days) of remaining entitlement of their current spell: total amount $B_0 = b_0 d_0$. In order to be considered, this previous entitlement should not be older than 3 years plus the duration of the entitlement. Otherwise, the claimants lose their rights. Regarding the potential new entitlement, we note b_1 and d_1 the corresponding daily benefits and duration (b_1 is calculated from the new work contracts). Claimants new entitlement is defined as follows:

$$\left\{ \begin{array}{l} \text{Daily benefits} \equiv \max(b_0, b_1) \\ \text{Total benefits} \equiv \max(B_0, B_1) \\ \text{Duration} \equiv \max(B_0, B_1) / \max(b_0, b_1) \end{array} \right.$$

Another rule was modified by the 1st October 2014 reform, regarding the accumulation of wages and UI benefits for unemployed workers doing short-term contracts. In the comparison-of-benefits system, three upper-bounds were limiting this specific employment status: employment duration should be below 110 hours per month, total duration of this accumulation should not exceed 15 month and the monthly wage should not exceed 70% of the former reference wage. The 2014 reform suppressed these rules. Note that working while unemployed does not necessarily imply the accumulation of wages and UI benefits, since workers above the thresholds may choose to maintain registration at the employment agency.

C Additional information on the dataset

Description

We use exhaustive administrative data from the National File of UI Claimants (FNA, *Fichier National des Allocataires*), a long-run database provided by the Unédic, which tracks trajectories of all UI recipients while unemployed. Besides socio-economic variables (nationality, diploma, zip code, age), this database gathers very detailed information regarding: the cause of unemployment, features of UI benefits (type, daily amount, reference wage, length of entitlement, UI plan), nature of unemployment periods (paid or unpaid – and reason why in the latter case, training, partial unemployment), the previous employer (firm size, type of industry), employment contracts reported for renewal of entitlement (type, length and ground for termination), possible severance payments. In addition, each date is very precisely registered. This is essential for the present purpose, especially the date of first registration and the start and end dates of unemployment spells, of compensation and of each job. Its main drawback is the lack of information about stable employment, since employment periods are observed only if the worker returns to unemployment afterwards. A second issue is that, once the reference wage is computed, the Employment Agency gets rid of the information about the wage of each contract (except in case of reduced activity). Third, some socio-economic characteristics are only registered at the beginning of the first unemployment spell by the Employment Agency, (marital status, number of children, vocational training) and never updated afterwards. Dimensions that are not mentioned in the descriptive statistics of Section 3 have too many missing values to be informative.

Structure

The FNA files contains three main datasets gathering informations on the universe of work contracts and unemployment trajectories. In the first table (PAR), each line correspond to a month of reduced activity for a claimant, but different contracts may be collapsed in one month, one contract may stretch out to many months and we do not have information about the bounds of each contract. The second table (PJC) is a decomposition of the unemployment spell in sub-periods, some spans corresponding to reduced activity contract, but this table brings no more information than the first one. In the third table (PAF), each line corresponds either to one work contract if it has been *certified* by the unemployed workers or to a period of work shorter than one month if it has only be *declared*. Unfortunately, it is not possible to match each period of reduced activity (PJC) with a declared work contract (PAF).

Work affiliation correctly predict future entitlement. In practice, a most important part of our empirical investigation is to construct the affiliation for each spell of each individual. This work history should be carefully measured since it determines both the sample of interest (claimants with a declared work history between 122 and 730 days) as well as the outcome variable (uncertified work history). This affiliation should be structured with caution in order to combine many jobs into a continuous period, especially when they are not registered in the same time unit (hours and days). In particular, since one day of work entitles to one day of benefits, overlapping contracts should not be counted twice.

Table 7 validates the construction of this work affiliation: both before and after the reform, the *certified* work history is relevant to predict benefit extensions, while the *uncertified* work history is not. Here, we focus on the workers who were actually eligible to a benefit extension before as well as after the reform (more than 122 days of certified work affiliation) once they started a new spell, so that we are able to directly compare their new entitlement to their previous work affiliation duration. This table shows that, both before and after the reform, a

one-day increase in the *certified* work history raises the duration of the new entitlement by one day, whereas the *uncertified* work history barely has any impact on the duration of the new UI entitlement.

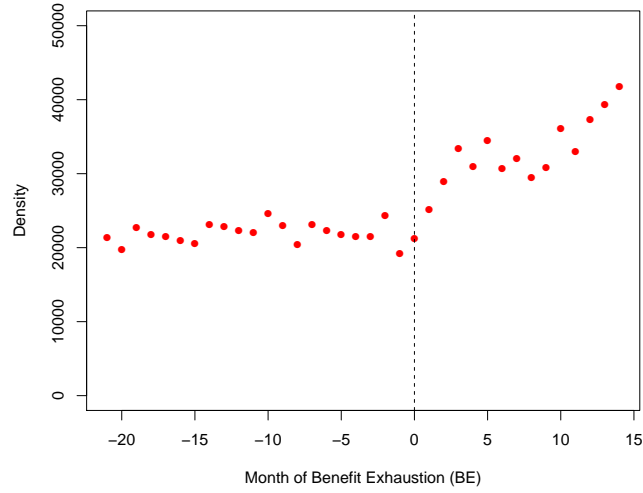
Table 7: Validation of in prediction of the work affiliation

<i>Work affiliation</i>	<i>Duration of new entitlement (days)</i>	
	Before the reform	After the reform
Certified (days)	0.974 ^{***} (0.004)	1.045 ^{***} (0.004)
Uncertified (days)	0.039 [*] (0.020)	0.007 (0.028)
Constant	26.136 ^{***} (1.184)	0.568 (1.286)
Observations	15,671	15,572
R ²	0.889	0.775

Notes: These results show how observed certified or uncertified work contracts empirically impact the new entitlement duration in our data, before and after the reform. The sample is restricted to claimants eligible to a benefit extension before as well as after the reform (more than 122 days of certified work affiliation) once they started a new spell, so that we are able to directly compare their new entitlement to their previous work affiliation duration. Standard errors are in brackets. *p<0.05; **p<0.01; ***p<0.001. FNA files (Unédic).

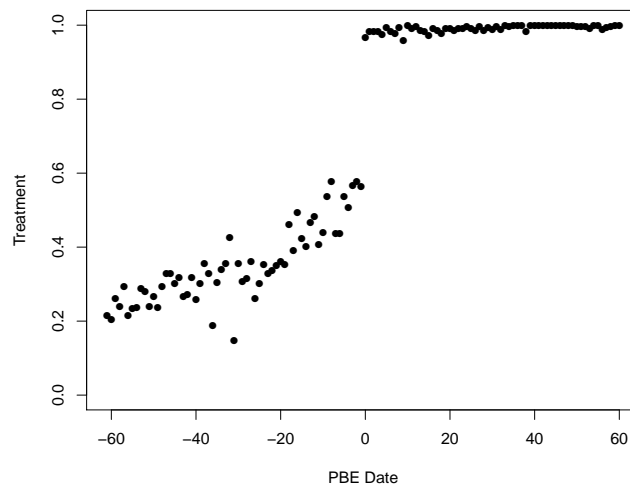
Comparing spells before and after the reform. Switching from the old to the new unemployment insurance system impacted more radically the way unemployment spells are registered. In particular, while claimants could previously finish an unemployment spell with remaining benefits and start a new spell, they now systematically resume their previous spell as they return to unemployment. This changes the definition of the unemployment spell and impact its average duration.

Figure 13: Frequency of benefit exhaustion dates



Note: This figure shows the number of unemployment spells by month of BE between 2013 and 2015, centered around the reform. The mechanical rise starting October 2014 is the consequence of the “renewal-of-entitlement” system, whereby claimants should terminate the current unemployment spell before starting a new one.

Figure 14: Treatment by PBE date, without seasonality



Note: This figure displays the proportion of spells ending after the enactment of the reform ($D = 1$) as a function of the PBE date, in a four-months window around the reform. It replicates Figure 7 on a subsample excluding the subpopulation causing the monthly seasonality issue. FNA files (Unédic).

D Seasonality

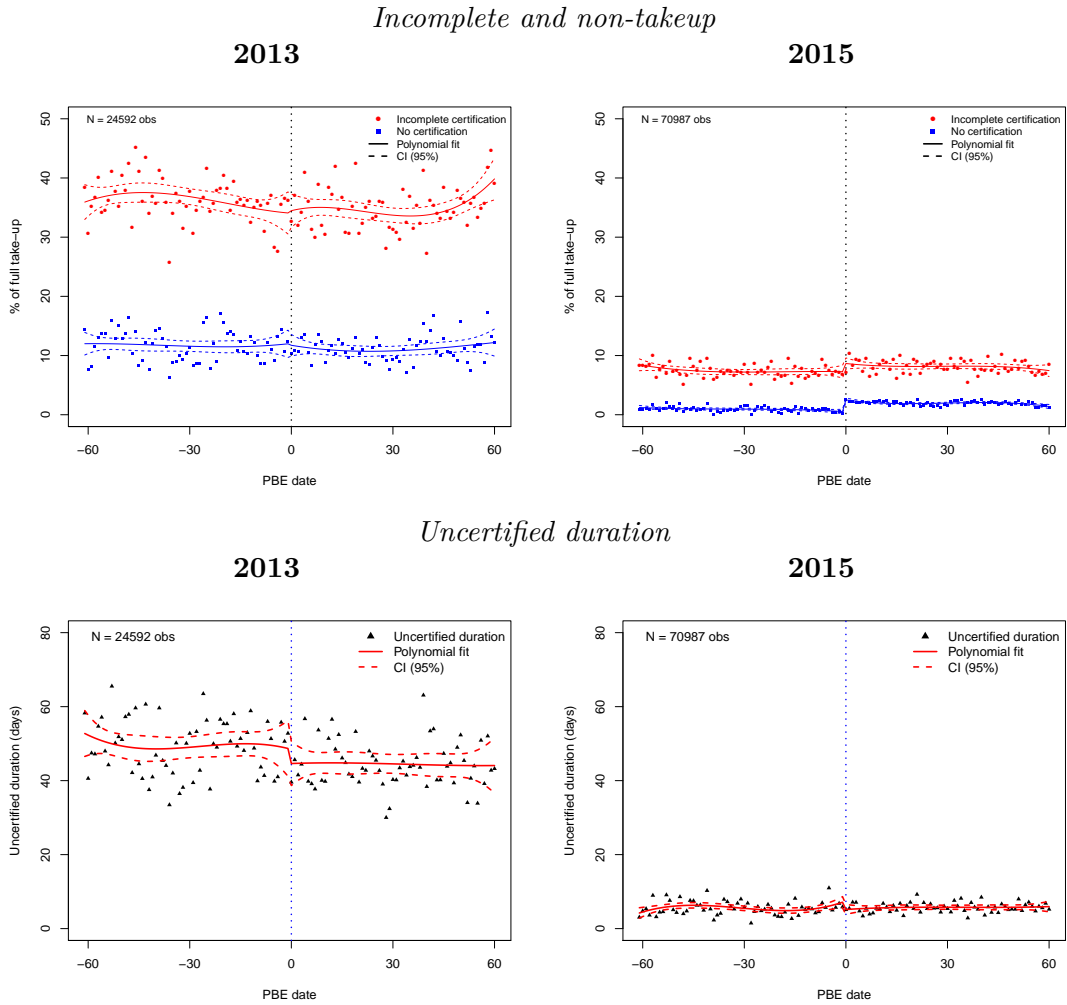
Considering a date as an assignment variable appears very useful when evaluating the impact of a reform on individual trajectories in administrative data, as in Ito [2015]; Kettemann et al. [2017]. Compared to other types of assignment variables, the specific issue of seasonality may arise. A seasonal pattern may be a threat for identification if we were not able to determine whether the jump in the outcome variable at the threshold results from this pattern or from the reform. Consequently, there is a risk of failing the various tests of validity of the design (continuity of the density of the running variable, continuity of other exogenous variables) solely because of the seasonality in the data. However, even if a seasonal pattern induces a discontinuity in the relation between Y_s and X_s , this relation may be predicted as long as the pattern happens periodically. Seasonality can be estimated on a large time span around the reform and used to predict the value of the outcome of treated and non treated units around the reform absent any seasonal pattern.

In the present case, our running variable, the *PBE* date, is mechanically impacted by a monthly pattern. Figure 5 shows a clear downward sloping monthly trend in the distribution of the PBE date by day (red dots), in a four months window around the reform. Indeed, work contracts carried out by claimants registered with the Employment Agency are recorded in the FNA files at the end of each month, and not at their exact start and end dates. Therefore if the PBE_{-61} date of a spell falls within a month which includes a work contract, the *PBE* date of this unemployment spell will have a higher probability to be located at the beginning than at the end of the month. Otherwise, there also appears to be seasonal patterns for given months of the year, as well as specific effects for the last day of each month, when the density may be higher.

In order to remove these trends, we take a quite conservatory stance and estimate a seasonal pattern over the period spanning from January 1st 2012 to December 31th 2015, excluding the window around the reform which is used for estimation. This seasonal pattern features monthly dummies, a common monthly trend and an index for the last day of the month. The deseasonalization equation therefore writes: $Y_s = \gamma_0 + \gamma_1 d_s + \gamma_2 \mathbb{1}_{last} + \sum_m \delta_m \mathbb{1}_{m_s=m} + u_s$ where Y_s is the value of the outcome variable for spell s , d_s is a common monthly trend for the day of the month, $\mathbb{1}_{last}$ is an index that the day of the PBE for spell s is the last of the month and $\mathbb{1}_m$ is a dummy equal to 1 when the PBE month is m and u_s are residuals. Reference is September 15th. The deseasonalized version of Y_s is given by: $\tilde{Y}_s = \gamma_0 + u_s$. This procedure is a simpler case of the “augmented” RD design featured in Hausman and Rapson [2018]. In the rest of the paper, we consider detrended variables and show that they respect the local continuity assumption required by RDD estimations.

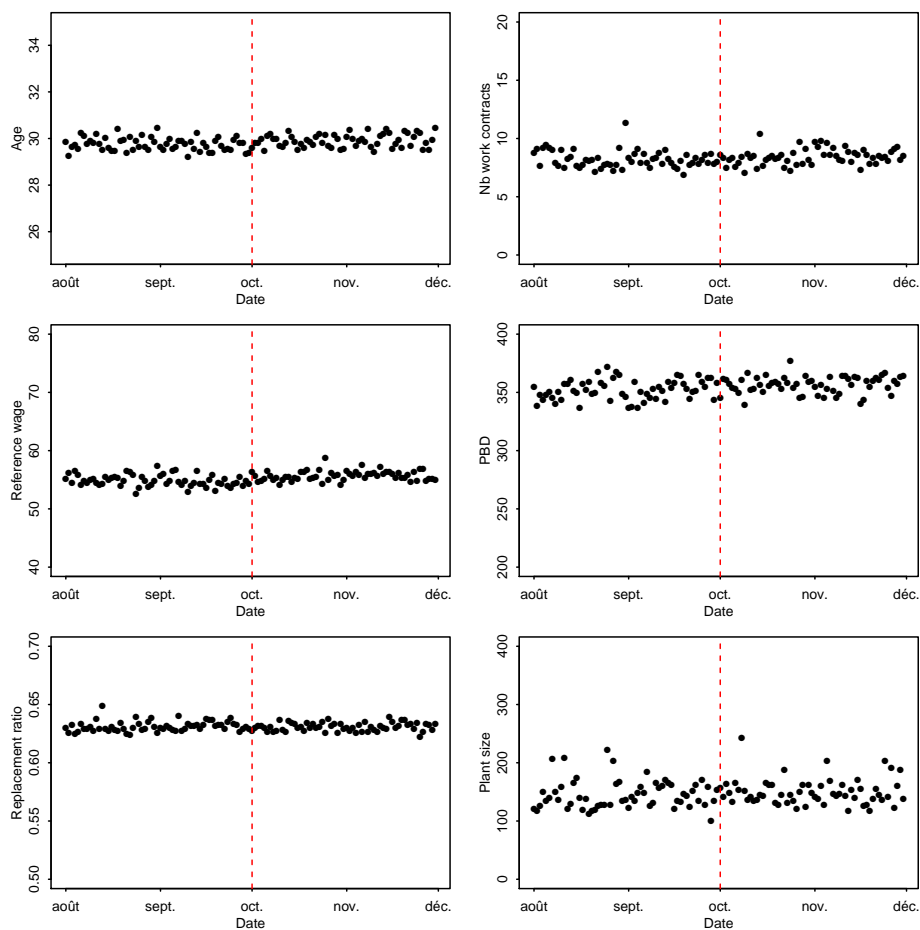
E Additional material on the RD estimations

Figure 15: Some outcomes around two *placebo* reform



Notes: This figure displays a placebo version of Figure 9 as if the reform happened October 1st, 2013 (*left column*) or October 1st, 2015 (*right column*). The y-axis presents the proportion of claimants who fully certified their past employment history as a function of the day of the potential benefit exhaustion (PBE) date, in a four months window around the placebo reform. Each dot shows the average proportion per day. The red line represents third-order polynomials estimated below and above the discontinuity. Red dashed lines represent 95% confidence intervals. In each column, from the top to the bottom, we have respectively the low-, middle- and high-affiliation groups. FNA files (Unédic).

Figure 16: Mean of some observables around the reform



Notes: Each graph of this figure represents the average of an observable variable per day of the PBE date in a four month window around the reform. From the top to the bottom, the variables are: (*left column*) age, reference wage, replacement rate; (*right column*) number of contracts during the spell, Potential Benefit Duration and the size of the plant the claimant were working before the current unemployment spell.

Table 8: Treatment effect (Fuzzy RDD) by polynomial order

	2013			2014			2015		
	Incomp. Take-Up	Non Take-up	Uncert. Duration	Incomp. Take-Up	Non Take-up	Uncert. Duration	Incomp. Take-Up	Non Take-up	Uncert. Duration
0	-0.029 (0.012)	-0.011 (0.008)	-9.886 (2.468)	-0.154 (0.009)	-0.066 (0.004)	-29.182 (1.500)	0.011 (0.003)	0.016 (0.001)	0.189 (0.427)
1	-0.027 (0.033)	-0.017 (0.021)	-11.080 (6.886)	-0.144 (0.022)	-0.054 (0.011)	-30.357 (3.770)	0.024 (0.007)	0.025 (0.002)	-0.278 (0.930)
2	-0.023 (0.033)	-0.016 (0.021)	-11.056 (6.932)	-0.144 (0.023)	-0.054 (0.012)	-30.717 (3.981)	0.025 (0.007)	0.025 (0.002)	-0.261 (0.938)
3	0.061 (0.045)	-0.003 (0.030)	-14.987 (9.678)	-0.134 (0.033)	-0.050 (0.017)	-32.791 (5.649)	0.019 (0.010)	0.027 (0.003)	-0.258 (1.352)
4	0.061 (0.046)	-0.004 (0.030)	-15.700 (9.862)	-0.138 (0.034)	-0.045 (0.018)	-33.734 (5.975)	0.018 (0.010)	0.027 (0.003)	-0.147 (1.375)

Notes: These results correspond to specification 1 where the treatment effect is estimated though 2SLS and $f(X_s - c)$ and $g(X_s - c)$ are polynomials of order P . This table reports the estimate $\hat{\tau}$ of the treatment effect for different order of the polynomial of the running variable. Standard errors are in brackets. FNA files (Unédic).

Table 9: Intent-to-treat effect by polynomial order

	2013			2014			2015		
	Incomp. Take-Up	Non Take-up	Uncert. Duration	Incomp. Take-Up	Non Take-up	Uncert. Duration	Incomp. Take-Up	Non Take-up	Uncert. Duration
0	-0.015 (0.006)	-0.006 (0.004)	-5.176 (1.288)	-0.082 (0.005)	-0.035 (0.002)	-15.637 (0.806)	0.007 (0.002)	0.010 (0.001)	0.123 (0.278)
1	-0.010 (0.012)	-0.006 (0.008)	-4.113 (2.548)	-0.061 (0.009)	-0.023 (0.005)	-12.754 (1.588)	0.014 (0.004)	0.015 (0.001)	-0.164 (0.548)
2	-0.008 (0.012)	-0.006 (0.008)	-4.085 (2.553)	-0.058 (0.009)	-0.022 (0.005)	-12.399 (1.611)	0.014 (0.004)	0.015 (0.001)	-0.153 (0.549)
3	0.022 (0.016)	-0.001 (0.010)	-5.308 (3.411)	-0.051 (0.012)	-0.019 (0.006)	-12.390 (2.139)	0.010 (0.005)	0.014 (0.002)	-0.139 (0.730)
4	0.021 (0.016)	-0.002 (0.011)	-5.490 (3.431)	-0.050 (0.013)	-0.016 (0.006)	-12.307 (2.184)	0.010 (0.005)	0.015 (0.002)	-0.078 (0.732)

Notes: These results correspond to an estimation of the intent-to-treat effect where this effect is estimated though OLS and $f(X_s - c)$ and $g(X_s - c)$ are polynomials of order P . This table reports the estimate of the intent-to-treat effect for different order of the polynomial of the running variable. Standard errors are in brackets. FNA files (Unédic).

Table 10: Fuzzy RDD by LLR with different slopes

	Outcome		
	Incomplete	Non	Uncertified
	Take-up	Take-Up	Duration
2013	-0.023 (0.033)	-0.016 (0.021)	-11.1 (6.9)
2014	-0.145 (0.023)	-0.054 (0.011)	-30.6 (3.9)
2015	0.025 (0.007)	0.025 (0.002)	-0.3 (0.9)

Note: This table presents the treatment effect estimated from model 1 through 2SLS, using a local linear regression in a four month window around the reform, allowing for different slopes at the left and at the right of the discontinuity. Regressions are run separately for three different outcomes: the share of incomplete takers, the share of non-takers and the uncertified duration of past work contracts. Rows display result for the year of the reform (2014) and for two placebo reforms (2013 and 2015). Standard errors are in brackets. FNA files (Unédic).

Table 11: Treatment effect (Fuzzy RDD): polynomial order and bandwidth

Bandwidth (days)	10	20	30	40	50	60	70	80	90	100	110	120
P												
0	-40.9 (6.6)	-32.3 (4.6)	-31.0 (3.7)	-30.5 (3.2)	-29.9 (2.8)	-30.1 (2.5)	-28.3 (2.2)	-29.3 (2.0)	-28.9 (1.8)	-29.6 (1.7)	-29.3 (1.6)	-28.9 (1.5)
1	-29.8 (15.6)	-43.0 (10.1)	-41.4 (8.3)	-35.3 (7.0)	-35.0 (6.3)	-32.9 (5.5)	-34.9 (5.2)	-31.2 (4.9)	-31.0 (4.6)	-30.1 (4.3)	-30.3 (4.0)	-30.7 (3.8)
2	-25.7 (16.8)	-42.0 (10.6)	-38.1 (8.7)	-34.7 (7.2)	-34.4 (6.4)	-33.1 (5.6)	-36.9 (5.6)	-33.6 (5.5)	-33.6 (5.2)	-30.2 (4.7)	-30.6 (4.4)	-31.3 (4.0)
3	-25.3 (24.2)	-42.7 (14.9)	-44.7 (12.0)	-46.9 (10.0)	-38.9 (8.8)	-37.5 (8.0)	-32.9 (7.0)	-36.2 (6.5)	-35.7 (6.2)	-34.4 (6.2)	-33.0 (5.9)	-32.0 (5.8)
4	-31.0 (25.7)	-38.7 (15.8)	-45.4 (12.5)	-44.6 (10.3)	-38.6 (9.1)	-36.5 (8.3)	-30.9 (6.9)	-35.1 (6.4)	-35.4 (6.3)	-37.5 (6.4)	-34.6 (6.3)	-32.4 (6.1)

Notes: These results correspond to specification 1 where the treatment effect is estimated through 2SLS and $f(X_s - c)$ and $g(X_s - c)$ are polynomials of order P . This table reports the estimate $\hat{\tau}$ of the treatment effect for different order of the polynomial ranging from $P = 0$ to $P = 4$ and different bandwidth, from 10 days to 120 days around October 1st 2014. Standard errors are in brackets. FNA files (Unédic).

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