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Optimal unemployment insurance and on-the-job behaviours

Laura Khoury

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Paris-Jourdan Sciences Économiques

DOCTORAT

Discipline: Analyse et Politique Économiques

Laura KHOURY

Assurance chômage optimale et comportements en emploi

Thèse dirigée par : Luc BEHAGHEL

Date de soutenance : le 26 septembre 2019

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PHD THESIS

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Summary

Social insurances are an important component of welfare states in developed countries. They have been set up to meet the twofold objective of improving efficiency, in a context where information is not available to all parties, and enhancing equity by sharing the cost of insurance across risk types. If this general principle achieves consensus, the question of the optimal level of insurance that should be provided is at the centre of the policy and scientific debate. This dissertation addresses the issue of the optimal design of unemployment insurance (UI), taking into account how UI parameters can affect the behaviour of firms and workers either before, during or after unemployment. It contributes to the literature in three ways: (i) it draws the attention on the interaction between UI design and on-the-job behaviours where the focus has mainly been on the response of job-seekers to UI parameters during unemployment; (ii) using rich administrative data, it empirically reveals the existence and potential costs of such behavioural responses; (iii) it tests the empirical validity of theoretical arguments usually put forward as a motivation for public intervention in the UI market, namely adverse selection and individual optimisation failures.

Chapter 1 assesses the effect of a UI program letting some job-seekers choose between low benefits for a long duration or higher benefits for a shorter duration. I use this uncommon choice feature in nationally-mandated UI schemes to understand the determinants of the choice and its consequences. Using a rich set of covariates, I document the existence of adverse selection, and relate the choice of the high-benefit option to observable characteristics generally associated to a lower risk-aversion or a higher impatience. My results also reveal a high moral hazard cost, as job-seekers opting for higher benefits are predicted to stay unemployed longer. This negative impact that is even higher for job-seekers with a high initial unemployment risk.

Chapter 2 shows that a discontinuous increase in the level of UI benefits at a tenure threshold leads to the strategic scheduling of layoffs in order to maximise the surplus from separation. I use the bunching methodology to quantify the extension of the employment spell in response to the increase in UI benefits. Results suggest that this extension is the result of an individual bargaining process between the worker and the employer. I argue that workers trade higher UI benefits against a reduction in the cost or the risk associated to the layoff, through a lower probability to claim damages, lower severance payment or a lower reputational cost.

Chapter 3 analyses the effect of the UI eligibility criteria imposing a minimum employment record to claim benefits on pre and post-unemployment outcomes. It highlights a separation response through a jump in employment outflows at the eligibility threshold. Exploiting a reform that reduced the minimum employment record, we show that, in some sectors, this separation response translates into a higher number of contracts whose duration exactly coincides with the new work history condition. This suggests that, in some sectors, the change in the UI eligibility criteria contributed to shape a new norm regarding the duration of short contracts. It would imply that UI parameters do not only affect the outcomes of job-seekers before, during and after unemployment, but also the outcomes of workers who do not experience unemployment. The last part of the chapter evaluates the extensive margin effect of UI benefits. Receiving UI benefits as opposed to not receiving any benefit at all has a negative impact on future employment probability, that does not seem to be compensated by an improvement in terms of job quality.

Field: Economics

Keywords: Unemployment insurance, Labour supply, Job quality, Insurance design, Bargaining

Résumé

Les assurances sociales sont une composante importante de l'État-Providence dans les pays développés. Elles ont été mises en place pour répondre au double objectif d'améliorer l'efficacité, dans un contexte où l'information n'est pas disponible pour toutes les parties, et d'accroître l'équité en mutualisant les coûts de l'assurance entre les différents types de risques. Si ce principe de mutualisation fait généralement consensus, la question du niveau optimal d'assurance qui doit être fourni est au centre du débat politique et scientifique. Cette thèse s'intéresse à la question de la conception optimale de l'assurance chômage, en prenant en compte l'influence des paramètres de l'assurance chômage sur le comportement des entreprises et des travailleurs avant, pendant ou après le chômage. Ce travail contribue à la littérature de trois façons : i) il attire l'attention sur les interactions entre l'assurance chômage et les comportements en emploi, là où l'accent a surtout été mis sur la réaction des demandeurs d'emploi aux paramètres de l'assurance chômage pendant l'épisode de chômage ; ii) grâce à de riches données administratives, il révèle empiriquement l'existence et les coûts potentiels de telles réactions comportementales ; iii) il teste la validité empirique des arguments théoriques généralement mis en avant comme motifs d'intervention publique sur le marché de l'assurance chômage, notamment la sélection adverse et les défauts d'optimisation individuelle.

Le chapitre 1 évalue l'effet d'un programme d'assurance chômage qui permet à certains demandeurs d'emploi de choisir entre des prestations faibles pendant une longue période ou des prestations plus élevées pendant une courte période. Nous tirons avantage du fait, peu commun dans les régimes d'assurance chômage mandatés à l'échelle nationale, de pouvoir observer un choix entre plusieurs régimes d'assurance pour en comprendre les déterminants et les conséquences. En nous appuyant sur un riche ensemble de variables, nous documentons l'existence de sélection adverse et nous relierons le choix d'allocations plus élevées à des caractéristiques observables généralement associées à une aversion au risque plus faible ou à une impatience plus forte. Nos résultats révèlent également un aléa moral marqué, qui se traduit par des épisodes de chômage plus longs pour les demandeurs d'emploi qui optent pour des prestations plus importantes. Cet impact négatif est encore plus fort pour les demandeurs d'emploi avec un risque de chômage initial élevé.

Le chapitre 2 montre qu'une augmentation discontinue du niveau des allocations

chômage à un seuil d'ancienneté conduit à une planification stratégique de la date du licenciement économique, afin de maximiser le surplus issu de la fin de contrat. Nous utilisons la méthode dite du *bunching* pour quantifier cette prolongation de l'épisode d'emploi en réponse à l'augmentation des prestations d'assurance chômage. Les résultats suggèrent que cette prolongation est le résultat d'un processus de négociation individuelle entre le travailleur et l'employeur. Dans ce chapitre, nous soutenons l'hypothèse selon laquelle les travailleurs obtiennent des prestations d'assurance chômage plus élevées en échange d'une réduction du coût ou du risque associés au licenciement. Cela peut se traduire par une probabilité moindre de réclamer des dommages-intérêts, une indemnité de départ moins élevée ou un coût réputationnel plus faible.

Le chapitre 3 analyse l'effet du critère d'éligibilité à l'assurance chômage sur différentes caractéristiques des périodes d'emploi avant et après le chômage. Ce critère impose d'accumuler un nombre minimum de jours d'emploi pour faire une demande d'allocation. Ce travail met en évidence une augmentation discontinue dans le taux de sortie de l'emploi au seuil d'éligibilité. En tirant parti d'une réforme qui a réduit le nombre de jours d'emploi minimum nécessaire, nous montrons que, dans certains secteurs, cette augmentation du taux de séparation se traduit par un nombre plus élevé de contrats dont la durée coïncide exactement avec la nouvelle condition d'emploi minimal. Cela suggère que, dans ces secteurs, la modification du critère d'éligibilité à l'assurance chômage a contribué à façonner une nouvelle norme concernant la durée des contrats courts. Cela impliquerait que les paramètres de l'assurance chômage ont un effet non seulement sur les demandeurs d'emploi avant, pendant et après le chômage, mais aussi sur les travailleurs qui ne sont pas directement concernés par le chômage. La dernière partie du chapitre évalue l'impact de la marge extensive des allocations chômage. Le fait de recevoir des prestations d'assurance chômage plutôt que de ne pas en recevoir du tout a une incidence négative sur la probabilité de retour à l'emploi, qui ne semble pas être compensée par une amélioration de la qualité de l'emploi.

Discipline: Économie

Mots-clés: Assurance chômage, Offre de travail, Qualité de l'emploi, Conception des systèmes d'assurance, Négociation

Contents

Acknowledgements	v
Summary	ix
Résumé	xi
General introduction	1
Introduction Générale	18
1 Generosity versus Duration Trade-Off and the Optimisation Ability of the Unemployed	38
1 Introduction	40
2 Legislation and data	43
3 The determinants of the takeup	46
4 Empirical implementation	51
5 Results	56
5.1 Labour market impact of the option right	56
5.2 The optimisation ability of the unemployed	62
5.3 Adverse selection and moral hazard	64
6 Concluding remarks	65
2 Unemployment Benefits and the Timing of Redundancies: Evidence from Bunching	112
1 Introduction	114
2 Institutional Background	118
3 Empirical Evidence of Bunching	121
3.1 Data	121
3.2 Documentation of the Bunching	123
3.3 Underlying Mechanisms: Exploration of the Bargaining Process	126
4 Theoretical Framework of Negotiated Layoff	128
5 Heterogeneity in Bunching	131
5.1 Empirical Bunching Estimation	132
5.1.1 Baseline methodology	132
5.1.2 The Difference-in-Bunching Strategy	137
5.2 Characterisation of the buncher	138
5.3 Interaction Between Ability and Incentives	140
5.3.1 Individual characteristics	141
5.3.2 Firms' characteristics	144
6 Robustness Checks and Extensions	146
6.1 Round-Number Fixed Effects	146

6.2	Response at the two-years cutoff	147
6.3	Extensive margin response	147
7	Welfare implications	148
8	Concluding Remarks	150
1	Institutional context	205
2	Extensions	205
	2.1 Other optimisation mechanisms	205
	2.2 Bunching at the two-year threshold	216
3	Alternative theoretical framework	218
4	Methodological discussion	220
3	Entitled to Leave: the Impact of Unemployment Insurance Eligibility on Employment Duration and Job Quality	224
1	Institutional background	230
2	Data and Descriptive Statistics	231
3	Empirical evidence of a separation response	234
4	Impact of UI eligibility criterion on contract duration	237
5	Extensive margin effect of UI benefits	241
6	Conclusion	249
	Main conclusion	327
	Conclusion générale	331
	References	345
	<i>List of Tables</i>	352
	<i>List of Figures</i>	356

General introduction

Although unemployment insurance (UI) spending represent only 5.13% of social spending, its effects on labour market participation, unemployment, and consumption smoothing are at the centre of thorough academic research with an important echo in the public debate. Social spending is the largest and most rapidly growing component of government expenditure. It includes benefits targeted at low-income households, the elderly, disabled, sick, young or unemployed persons. It has increased from 8% of GDP on average in OECD countries in 1960 to 20% today.

Among these countries, France is characterised by the highest social spending, 31.2%,¹ which makes it a particularly interesting case study (Figure 1). Historically, the welfare state in France has developed following the Bismarckian model, indicating a system characterised by a compulsory funding from both employers and employees, and a protection tied to the working status. More recently, the French welfare state has integrated a Beveridgian component by increasingly relying on income tax funding. This high share of social spending coupled with its dual system motivates why this dissertation concentrates on the study of France.

Public spending on unemployment benefits is a share of social spending, and represents 0.7% of GDP on average in OECD countries, with a maximum at 2.9% in Belgium, and a minimum within Europe at 0.2% in the United Kingdom (Figure 1). France ranks in the 4th position, with 1.6%. As any social insurance, the goal of unemployment insurance is to protect against an adverse event, more specifically, the risk of job loss.

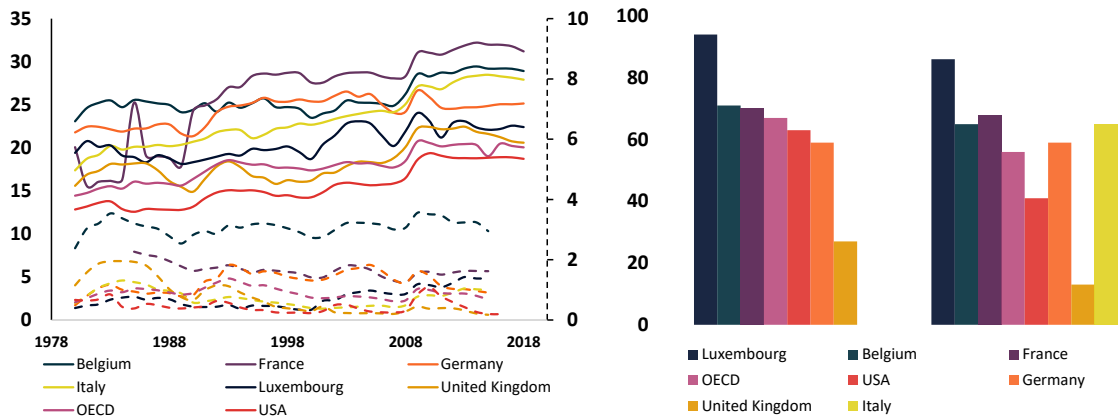
Interestingly, a common yet rarely questioned fact in developed countries is that unemployment insurance schemes are organised at the national level. Although the conditions to enter the scheme and the generosity of the coverage may vary across countries, the general principle of having an insurance covering any private-sector worker with sufficient contributions can be found in most countries. In the academic debate, efficiency-related arguments have been put forward to justify nationally-mandated schemes, whereas the policy debate revolves around equity questions. Indeed, by setting up an insurance at the national level, contributions are pooled across different risk types facing different probabilities of job loss. This is a first channel through which UI schemes entail redistribution, although this is not their

¹Source: [OECD Social Expenditure Database \(SOCX\)](#).

primary objective, as opposed to social safety nets.

The great variation observed in the level of public UI spending translates into an equally wide variation in the generosity of UI benefits across countries. This generosity is usually measured by the net replacement rate, corresponding to the ratio between net UI benefits and net earnings. On the one hand, the average net replacement rate for a single person without children at the average wage, is equal to 56%. Within Europe, the minimum is found in the United Kingdom (13%) and the maximum in Luxembourg (86%). On the other hand, this same rate at the minimum wage goes from 27% (the UK) to 94% (Luxembourg), with an average at 67% in OECD countries (Figure 1).² The difference between the replacement rates at the average and at the minimum wage is another illustration of the redistribution component of UI benefits. We should also keep in mind that UI schemes interact with the whole socio-fiscal system, which explains that net replacement rates are generally higher than gross ones, as benefits are less taxed than labour income.

Figure 1: Public social and UI spending (LHS), and average net replacement rates (RHS) in OECD countries (% GDP)



SOURCE: OECD.

NOTE: The figure at the left-hand side represents the share of social spending (on the left axis) and of unemployment spending (on the right axis) in total GDP. We see that Belgium and France are among the countries at the top of both the social and unemployment spending distributions. Overall, spending is also higher in Europe than in the US.

The figure on the right-hand side plots the net replacement rate, respectively at the level of the minimum wage (left-hand side) and at the level of the average wage (right-hand side). Both rates are computed for a single person without children, at the beginning of the unemployment spell, and does not include housing benefits. The difference between both replacement rates is an illustration of the redistributive component of UI. The most generous benefits are found in Luxembourg, France and Belgium, and the least generous ones are found in the UK. Italy has no data for the replacement rate at the minimum wage, possibly because there is no national minimum wage.

These disparities between countries justify addressing the question of the right level of unemployment benefits to be distributed. This dissertation contributes to this discussion. It provides answers to two research questions: why UI schemes are mandated at the national level, and what is the optimal level of insurance to

²Source: OECD benefits and wages database.

set ? It builds on three empirical studies. These studies investigate the effect of different UI programs on labour supply both in employment and unemployment, on insurance choices and on the structure of contracts. This allows me to infer more general lessons on the optimal design of unemployment insurance. The first chapter addresses both questions. First, it provides evidence that the main motive for public intervention in the UI market – the fact that the most risky individuals are the ones purchasing insurance – is empirically valid. Second, it tackles the question of the optimal level of insurance by investigating how job-seekers react to a change in the level and the duration of their benefits. Chapters 2 and 3 focus on this second question. They take into consideration the behaviour of firms and workers before and after unemployment, to broaden the reflection on the impact of UI on the labour market.

The question of the optimal level of UI has been at the centre of an extensive literature, giving rise to models based on a *sufficient statistics* approach. This type of models does not require to estimate the structural underlying primitives, and focuses on understanding how job-seekers react to the UI parameters in a reduced-form way (Baily, 1978; Chetty, 2006). They aim to bridge the gap between the empirical and theoretical literature on UI, by reducing the model to a small set of parameters that can be easily assessed empirically. The optimal UI formula consists of three parameters: the elasticity of unemployment duration with respect to the level of UI benefits – capturing the cost of UI – and the drop in consumption when entering unemployment multiplied by the risk aversion coefficient to measure the benefit side of UI. Unlike the structural approach, this approach does not require to make strong assumptions, but at the cost of being embedded into a specific policy regime. My dissertation fits into this strand of the literature, using exogenous policy variations to estimate behavioural responses to the UI system credibly, with the ultimate goal of making progress in the design of UI schemes. It contributes to this literature in several respects: (i) I first provide an estimation of one of the key parameters of the optimal UI formula – the sensitivity of unemployment duration to both the level and the duration of UI benefits (Chapters 1 and 3) – in the French context; (ii) I highlight the importance of other parameters, not taken into consideration in the initial model (Chetty, 2006). Indeed, the optimal UI model only accounts for the supply side, but remains silent on the behaviour of the firm. I show that the level of UI benefits influences the decisions of the employer and the employed worker (Chapters 2 and 3) ; (iii) I estimate these new parameters, captured by the sensitivity of employment duration to UI benefits, and use them to understand recent trends on the labour market (Chapters 2 and 3).

In the remainder of this introduction, I will first take a broader perspective by going back to the origin of UI and the reasons why most of the developed countries chose to set up national social insurances. I will then mention what are the current

issues in terms of UI design, both in the policy and academic debates, and how my dissertation relates to them. The last part will describe in more details the different chapters constituting this dissertation.

From unemployment to unemployment insurance

Unemployment represents a harmful and multifaceted adverse event against which individuals may be particularly willing to protect. It does not only affect income ([Jacobson, LaLonde and Sullivan, 1993](#); [Von Wachter, Handwerker and Hildreth, 2009](#)) and wealth, but many other dimensions of the individual welfare. This multidimensionality has long been of interest for sociologists, and has led to the classification of unemployment as an absolute phenomenon. For instance, Dominique Schnapper identifies three different types of unemployment experiences ([Schnapper, 1981](#)): the “inverted unemployment”, usually experienced by young people who just entered the labour market. This type of unemployment is assimilated to leisure time people can use to devote themselves to their passion. The second type is defined as “deferred unemployment”, often encountered among executives, characterised by a very active job search and multiple substitution activities, to delay the true unemployment experience. Finally, the main and most typical type corresponds to the “total unemployment”, which indicates that the income loss comes together with boredom, shame and the break-up of social ties.

Economists have also shown that unemployment encompasses many dimensions. It can have a large detrimental impact on health ([Black, Devereux and Salvanes, 2015](#); [Bergemann, Gronqvist and Gudbjörnsdottir, 2011](#)) and mortality ([Sullivan and Von Wachter, 2009](#); [Eliason and Storrie, 2009](#)), as well as on life satisfaction ([Winkelmann and Winkelmann, 1998](#)), although this negative impact is mitigated in regions with high unemployment rate ([Clark, 2003](#)). These last papers emphasise the importance of non-pecuniary factors such as social relationships and the construction of one’s identity in society, as well as the role of social norms in the unemployment experience. It builds on early sociological work from [Lazarsfeld, Jahoda and Zeisel \(1933\)](#), who portray the decline of a whole city when the main employing plant shuts down in Austria in the 1930’s. The authors describe the feeling of moral uselessness and the loss of time awareness as two forces leading to desocialisation.

In a similar fashion, [Paugam \(2006\)](#) emphasises the complexity of unemployment by asking whether it could be depicted as a “cumulative break-up of social ties”. According to him, the answer depends on the type of institutions in the country, namely the role of the State and the family.

Public intervention in the unemployment insurance market

In keeping with the public economics tradition, the approach taken in this dissertation is not only to focus on the impact of unemployment itself, already largely documented, but mostly on how we can improve the way society takes care of the risk of job loss. The traditional economic motivation for the creation of social insurances lies in two principles: first, setting up an insurance helps reducing the cost of adverse events when individuals are risk-averse. Second, the argument in favour of public intervention originates from the identification of three market failures: (i) asymmetric information, leading to adverse selection; (ii) individual optimisation failures and (iii) externalities.

The first failure refers to the fact that there is heterogeneity in the level of risk, and that individuals know more about their risk type than the insurer does. [Rothschild and Stiglitz \(1992\)](#) have shown that these two conditions can lead to a situation where no equilibrium supports the provision of insurance, which could be solved through mandated insurance. The intuition is that the insurer prices according to the average risk level, as he has no information on individual risk type. The presence of high-risk types makes this average premium too costly for low-risk types, who exit the market. As low-risk types leave, the average premium goes up, driving more and more low-risk types out of the market, and ending up in its collapse ([Akerlof, 1978](#)).

Precisely because UI schemes have always been mandated at the national level, testing empirically the presence of adverse selection in the UI market has proven to be a difficult task. Positive correlation between the level of coverage and realised risk mixes adverse selection with moral hazard, i.e. the fact that individuals may have riskier behaviours when they know they are insured. One notable exception is a recent work by [Landais and Spinnewijn \(2019\)](#) where they use the coexistence of a minimum mandated coverage with private additional insurance that can be purchased on a voluntary basis in Sweden to confirm the presence of adverse selection. Combining a revealed preference with a marginal propensity to consume approach, they also show that the valuation of UI has been largely underestimated in previous work. The difficulty to empirically test the presence of adverse selection is also an issue that I address in Chapter 1 of my dissertation. I take advantage of an unusual setup where I can observe unemployment insurance choices from job-seekers in France, to assess the magnitude of both adverse selection and moral hazard.

A second argument for the introduction of public UI is that individuals may not be able to insure themselves up to the right level unless the government imposes them to do so. It has been shown that individuals systematically underestimate their probability of losing their jobs, and overestimate their probability of finding one, even when they have previous unemployment experience ([Spinnewijn, 2015](#); [Mueller, Spinnewijn and Topa, 2018](#)). Even in nationally-mandated schemes, behavioural

parameters may play a role in affecting effort to keep and look for a job. For instance, impatience may affect job search in both directions: it assigns a low value to the future benefits of searching for a job, but at the same time, it may pull down reservation wages as the individual receiving an offer would prefer accepting it rather than waiting for a potentially better one (DellaVigna and Paserman, 2005; Paserman, 2008). In Chapter 1 of my dissertation, where I observe UI insurance choices, I draw some insights on the insured party preferences and how they correlate with socio-demographics.

Whereas nationally-mandated schemes usually impose all the parameters of the compensation, I use this flexibility given to workers to show that the aforementioned motives for a mandated national insurance may prove real: adverse selection arises, and job-seekers do not choose optimally the way they are compensated in order to go back to employment quickly and in good conditions. It challenges the view of a rational agent trading-off between labour and leisure, in line with the growing literature on the behavioural parameters of the job-seekers.

Although positive externalities of having everyone insured are more salient in the case of health insurance, they have also been raised as an argument for the set-up of a public UI (Hsu, Matsa and Melzer, 2014), as, for instance, being insured may encourage people to take up riskier yet more productive jobs (Acemoglu and Shimer, 2000). Pooling the coverage at the national level may also reduce administrative costs as compared to a situation where many insurance schemes coexist.

The optimal level of unemployment insurance

While the question of having UI mandated at the national level is rather consensual, the issue of the amount of insurance is much more controversial. This topic has sparked off large interest in the policy and academic debate. The optimal UI literature stems from the canonical model of Baily (Baily, 1978), more recently generalised by Chetty (Chetty, 2006). This model is based on a *sufficient statistics* approach, that uses transparent measurement of statistical relationships triggered by exogenous policy variation to derive results on welfare (Chetty, 2009). The objective of this literature is to determine the optimal level of UI that equalises its cost in terms of moral hazard with its benefits in terms of consumption-smoothing. The moral hazard cost could translate, on the supply side, into more on-the-job shirking or less effort to look for a job. The fact that the insurer cannot observe the level of effort of the individual³ leads to a trade-off between reducing market failures and improving efficiency by reducing moral hazard. In the Rothschild-Stiglitz model,

³He can only observe the outcome, that is the job finding rate, but cannot distinguish the case of someone exerting no effort and someone exerting effort but whose characteristics (education, skills, etc.) make it hard for her to find a job.

perfect insurance is optimal as moral hazard is not accounted for. The optimal UI model takes this distortion into account while not taking into consideration the adverse selection problem. In line with the *sufficient statistics* approach, the key parameters of the model can be easily estimated empirically, although this method comes at the cost of being more local.

The literature has then very much focused on the estimation of the cost side, captured by the elasticity of unemployment duration with respect to the level of unemployment benefits. Early papers have used cross-sectional variation. Their main limitation is that the generosity of UI benefits is often a decreasing function of the level of previous earnings. Therefore, the effect of having higher UI benefits on unemployment duration is easily confounded with the effect of having lower previous earnings (and presumably, lower education and skill levels). More recent papers exploit exogenous variations provided by either an age, tenure or time threshold, to isolate the impact of UI generosity using quasi-experimental methods. These studies have mostly focused on the relationship between UI generosity and unemployment or non-employment duration (Lalive, Van Ours and Zweimüller, 2006; Lalive, 2007; Landais, 2015), with elasticity estimates ranging from 0.15 to 2 in Europe, and from 0.10 to 1.21, with a lower average, in the US (Schmieder and Von Wachter, 2016). The initial optimal UI model has then been extended to the study of the optimal benefit duration (Fredriksson and Holmlund, 2001; Le Barbanchon, 2016; Schmieder and Von Wachter, 2016) or time profile (Kolsrud et al., 2018). In this last paper, the authors highlight the fact that the insurance value and the moral hazard cost of UI benefits may not be stable throughout the spell. In particular, they find that the consumption drop is large at the beginning of the spell and further increases after, and that the moral hazard cost is larger earlier in the spell.

However, the very robust relationship between the level of UI benefits and the duration of the unemployment spell is only capturing part of the picture, as longer unemployment is not necessarily detrimental to future employment outcomes. The ultimate question comes down to knowing whether higher UI benefits allow individuals to delay the acceptance of a job and to enjoy more leisure, or whether they subsidise better job search and improve employment opportunities.

It is an established empirical fact that there is negative duration dependence in unemployment, implying that the hazard rate out of unemployment decreases as a function of duration. One challenge is to distinguish between “true duration dependence” due to the depreciation of human capital, or to stigma, from unobserved heterogeneity in hazard rate. In the first case, the probability of finding a job for a given individual decreases with the duration of unemployment, whereas in the second case individuals with the highest hazard rate exit more quickly. As both explanations have the same impact on the distribution of the hazard rate, it is very difficult to distinguish them empirically (see Van den Berg and Van Ours

(1996) for a non-parametric estimation). From a policy perspective, it is crucial to understand which interpretation prevails, to know if the government should focus on long-term unemployment or on the individuals with the lowest hazard rate to begin with. It relates to the question of whether the government should act on individual characteristics driving down employment probability, through training for instance, or on broader phenomena, including by finding ways to reduce the discrimination suffered by the unemployed.

Despite this negative duration dependence, staying unemployed longer as a consequence of generous UI benefits could have positive repercussions beyond the moral hazard cost. Indeed, the literature has also emphasised the potential positive impact of a high level of UI benefits on post-unemployment outcomes, not only focusing on the duration of unemployment. Although the role of UI benefits in encouraging people seeking riskier and more productive activities has been underlined (Acemoglu and Shimer, 2000), the centre of the debate has been the relationship between UI and match quality, with mixed results : Centeno (2004) and Nekoei and Weber (2017) find a positive effect, whereas Schmieder, von Wachter and Bender (2016) find a negative effect and Card, Chetty and Weber (2007); Lalive (2007); Van Ours and Vodopivec (2008) a non-significant one. A reason why it is difficult to detect any significant net effect may stem from the fact that two countervailing mechanisms add up. Higher UI benefits alleviate liquidity constraints and allow to have higher target wages (selectivity channel),⁴ which could be counterbalanced by negative duration-dependence. The standard job search model predicts that higher benefits allow workers to take up jobs with higher wages, as explained by the first channel. However, when duration dependence is integrated to the model, the direction of the effect could go either way (Nekoei and Weber, 2017). Le Barbanchon, Rathelot and Roulet (2017) distinguish both channels and examine the validity of the selectivity one by looking directly at the impact of UI benefits on reservation wages. They report precisely estimated elasticity of reservation wages with respect to potential benefit duration (PBD) close to zero.

Better understanding the relationship between *unemployment insurance* and *pre* and *post*-unemployment outcomes, beyond the only measure of unemployment duration, is a crucial issue. Chapters 2 and 3 of my dissertation contribute to this research agenda. The second one focuses on pre-unemployment outcomes by analysing the scheduling of economic layoffs according to UI benefits, possibly through collusion between firms and workers. Chapter 3, joint with Clément Brébion and Simon Briole, addresses the question of how UI benefits influences both pre and post-unemployment outcomes. Using the eligibility condition to UI benefits that imposes a minimum em-

⁴Nekoei and Weber (2017) distinguish between target and reservation wage, the former being measured by the wage of the last job an individual applied to, while the latter is a theoretical construction that we never observe. Indeed, questions on what would be the lowest acceptable wage for an individual are never asked without referring to a target job.

ployment record, we first show that UI parameters impact the separation behaviour and the duration of offered contracts. We then assess the extensive margin effect of UI benefits on employment probability and post-unemployment outcomes related to job quality.

Current theoretical and policy issues in unemployment insurance design

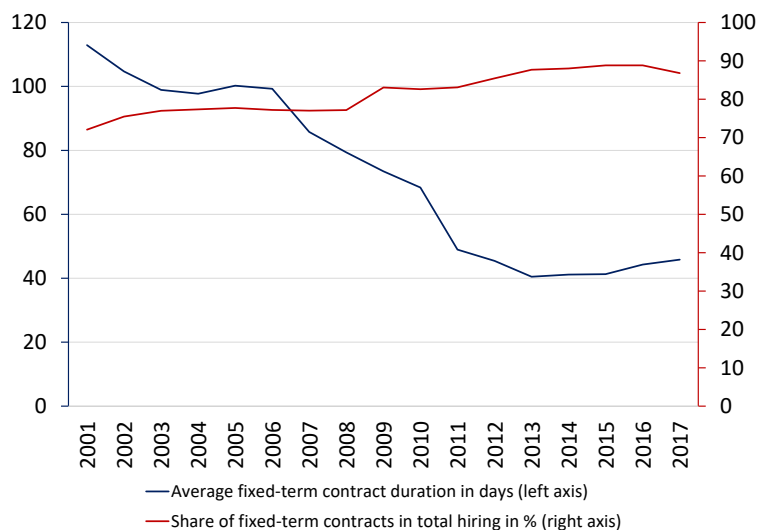
Recent labour market trends have led to a change in focus in the study of the interactions between UI and behaviours during employment. In most European countries, professional paths are increasingly fragmented, with increased transitions between employment and unemployment. The share of temporary employment in total dependent employment has grown from 9.19% on average in OECD countries in 1980 to 11.24% in 2017. This trend hides a great heterogeneity: in Italy or Luxembourg, this figure has more than doubled, while it has stayed almost stable in the UK or in Sweden. Among the most remarkable evolutions, the Netherlands and France had their share multiplied by four. Similarly, the part-time employment rate has increased by four percentage points in OECD countries on average since the mid-1970's, to reach 16.55%. The evolution is particularly substantial in countries where the share of temporary employment has increased the least.

In parallel, the strictness of employment protection legislation on temporary contracts, as computed by the OECD,⁵ has also decreased in many countries since the 1990's, such as Italy, Greece, Belgium, Germany, Portugal, Spain or Sweden. Within this general framework, France is a noteworthy case study not only because of the marked increase in the share of temporary employment in terms of stock, but also because of its increase in total hiring and the reduction of the average duration of fixed-term contracts. The former has grown from 72% in 2001 to 87% in 2017 while the latter has been reduced from 117 days to 43 days (Figure 2).⁶ These trends are partly explained by the increase in the share of contracts under one month, a phenomenon that went along with an increase in the share of workers recalled by a former employer. The share of hiring through a recall has increased from 46% of total recruitments in 1995 to 69% in 2012. This figure goes up to 84% among contracts of one month or less (Benghalem, 2016). The turnover rate has also increased to a large extent, from 29% in 1993 to 96% in 2017, a change partly explained by the increase in very short contracts, as one-day contracts accounted for 30% of all contracts in 2017 (DARES, 2018).

⁵Source: OECD

⁶Source: DARES

Figure 2: Share and average duration of fixed-term contracts in France (2001-2017)



SOURCE: DARES.

NOTE: This figure plots the trend in the share of fixed-term contracts in total hiring (on the right axis), and the average duration of fixed-term contracts (on the left axis) in France. We observe that fixed-term contracts account for an increasing share of the flows to employment, with a decreasing average duration.

These recent trends have put at the centre of the public debate the unemployed persons experiencing short employment and unemployment spells. The new profile of the typical unemployed has diverged from the traditional model of a career in the same firm under a permanent contract, where unemployment would have been a setback. The 2014 UI reform in France was precisely targeted at this type of workers, with a simplification of the rules to combine small jobs with part of the UI benefits, and a possibility to extend the UI rights with short contracts during the unemployment spell.

These changes in the institutional framework and in the structure of the labour market has called for a better consideration of the interactions between UI and employment. This dissertation contributes to this change in focus by devoting the last two chapters to the analysis of how UI partly shapes the hiring and firing decisions of employers in reaction to changes in the workers' outside option.

Recent papers have investigated how UI rules can affect the duration of employment. Some have focused on specific workers, such as senior workers for whom UI could serve as a bridge to retirement. [Baguelin \(2016\)](#) has built a model, confirmed by empirical findings in France ([Baguelin and Remillon, 2014](#)), showing that in some cases, the last exit from employment was timed in such a way that workers would be covered by UI and then by pensions without any interruption in payment. In the Austrian context, [Jäger, Schoefer and Zweimüller \(2019\)](#) also document a marked increase in the separation rate as a response to benefit duration extensions for older workers.

Other papers have analysed the impact of the UI eligibility criterion defined in terms of work history on transitions from employment to unemployment (Martins, 2016; Baker and Rea Jr, 1998; Green and Riddell, 1997; Rebollo-Sanz, 2012; Albanese, Ghirelli and Picchio, 2019) respectively in Portugal, Canada, Spain and Italy. Indeed, in most developed countries, workers need to be employed, and then to contribute to UI a minimum number of months in order to be able to receive benefits. This is linked to the contributory nature of social insurances, which imposes a strong relationship between what is paid and what is received. The common finding in these papers is that there is a discontinuity in the transition rate from employment to non-employment at the eligibility threshold, showing that employers and employees respond to incentives created by UI even during employment. As quits do not entitle to UI benefits in most countries, this jump in exits from employment necessarily comes from layoffs or mutually agreed terminations, and implies that employers are instrumental in this phenomenon.

Several explanations could rationalise this finding: on the supply side, workers may exert less on-the-job effort when their outside option, that is the value of unemployment, increases. On the demand side, employers may be willing to lay-off workers as soon as they are eligible to UI to adjust more flexibly to demand fluctuations, potentially rehiring them in the future. This kind of employers-workers agreement can ease the relationships between the two parties in a context where workers are frequently recalled (Christofides and McKenna, 1995). The standard theoretical framework usually mobilised to rationalise this finding is the implicit contract theory (Feldstein, 1976; Baily, 1977). At the moment of the hiring, employers may agree to separate when the outside option of the worker discontinuously increases at the eligibility threshold. Employers and workers can split the surplus from separation by agreeing on lower wages, for instance.

Van Doornik, Schoenherr and Skrastins (2018) have found in Brazil that not only flows to unemployment increase by 12% at the UI eligibility threshold, but that the whole layoff and hiring pattern is compatible with strategic behaviour related to UI incentives. In particular, employers lay-off workers when they become eligible to UI and rehire them when their benefits exhaust. They also document that most of the entries to unemployment actually hide informal employment. Workers are therefore staying employed while receiving UI benefits. This extreme form of optimising behaviour goes through firm-worker collusion to extract some rent from UI and share it through lower equilibrium wages.

On the contrary, the efficient contract theory is not always verified. Jäger, Schoefer and Zweimüller (2019) use the comparison of the group affected by a UI benefit duration extension policy after it has been abolished with the control group. They reject the efficient separation theory that would imply that UI extensions would destroy low-surplus jobs and make remaining high-surplus more resilient. Matches

destroyed as a result of UI rules are characterised as blue-collar, older and sicker workers in declining firms. The fact that they exhibit similar wages to survivors is justified, according to the authors, by wage frictions and regulations on separations preventing efficient bargaining.

In keeping with this literature, we show in Chapter 3 of this dissertation that the adjustment of contract length to the eligibility requirement may be granted in exchange of flexibility to demand fluctuations, as the effect we measure is more marked in sectors characterised by a high separation rate and a high share of very short contracts in total hiring.

According to [Van Doornik, Schoenherr and Skrastins \(2018\)](#), rent extraction can be reduced by “higher experience rating, tighter eligibility rules, or lower replacement rates. More nuanced policies include tweaks to eligibility rules to prevent repeated temporary layoffs of the same worker.” That is precisely the spirit of a 2019 UI reform in France, that has pushed the eligibility threshold from 4 months within the last 28 months to 6 months within the last 24 months, and introduced a modulation of contributions in an experience rating spirit.⁷

Contributions and Outline of the Dissertation

This dissertation builds on the large literature on optimal UI described above, trying to answer pressing issues that have attracted attention both in the academic and policy debates. It enlarges the scope of existing knowledge on this topic: (i) first, I examine different outcomes, not only focusing on unemployment duration but also studying pre and post-unemployment outcomes. In particular, I bring the role of firms into the reflection on the optimal design of unemployment insurance. (ii) Second, I provide empirical evidence to confirm or refute theoretical priors on the existence of adverse selection in the UI market and that points to failures in the optimisation behaviours of job-seekers.

This dissertation relates to the fundamental question, in public economics, of whether the government should set up social insurances and how much of social insurance is optimal. More precisely, I try to understand how do firms and workers react to changes in UI parameters by taking into account interactions between contract design, labour supply, job search behaviour and insurance choices. As UI benefits are part of the worker’s outside option, it is likely to influence his labour supply choice and work effort not only during unemployment but also when employed. Worker’s utility could also be incorporated by employers and affect their hiring and firing decisions. Analysing the response of each party to modifications of the UI scheme provides an opportunity to learn lessons on the optimal design of UI in the light of recent developments on the labour market. I rely on an in-depth knowledge

⁷[Decree No. 2019-797 of 26 July 2019 on the unemployment insurance scheme](#)

of the French institutional background and on recent empirical methodologies, such as quasi-experimental or bunching methods, to address the endogeneity issue. The use of administrative data coupled with the identification of policy reforms allows to exploit local changes to estimate precisely the relationship between UI and labour market outcomes. Although this “local” strategy may undermine the external validity of the findings, it allows to pinpoint phenomenon that would not necessarily be detectable in survey data and to make progress in the knowledge of UI design.

This dissertation has been carried out on UI data from the *Unédic*, which is the organisation in charge of managing the UI in France. Beside providing the data, the *Unédic* has also hosted me for the entire duration of my PhD, and allowed me to benefit from the insights of UI experts working “on the field”. As such, this thesis is also connected to relevant questions in the public debate on the design of UI in developed countries. France is a particularly suitable country to conduct this type of research. As UI legislation is revised every two-three years under the supervision of employers’ and employees’ representatives, there are several sources of variation to tackle the key questions on the design of optimal UI.

The first chapter of my dissertation looks at the relationship between UI benefit level, potential benefit duration and unemployment duration, exploiting the fact the UI benefit level and duration are traded-off by workers, as a way to understand the determinants and the consequences of their choice. The second chapter investigates the impact of UI benefits on the timing of the separation, in the context of economic layoff, and shows that both firms and workers respond to incentives created by UI during employment. Finally, the third chapter, joint with Clément Brébion and Simon Briole, focuses on the contributory nature of UI that imposes a minimum employment record to be entitled to claim benefits. We look at the impact of this eligibility criterion on the separation rate, the duration of contracts, future employment probability, and job quality.

In Chapter 1, *Generosity versus Duration Trade-Off and the Optimisation Ability of the Unemployed*, I exploit an uncommon setting of the French UI scheme, the fact that the unemployed are given the choice between two UI entitlements when they experience a very large drop in income entering unemployment. This feature introduces a trade-off between the level of UI benefits and the potential benefit duration, which can be used to discern preferences of the unemployed over the level of insurance coverage desired. Using administrative unemployment data, I provide descriptive evidence on the determinants of this choice. I report that workers choosing higher benefits over the duration are on average younger, male, more educated and skilled than eligibles choosing the other option. These characteristics could be associated with better employment prospects, less risk aversion or more impatience, that make them prioritise the level of the benefits received immediately

at the expense of the duration.

The presence of eligibility criteria allows to assess the labour market impact of the chosen compensation contract, through a fuzzy regression discontinuity design. While the impact is not clear *a priori*, as two countervailing forces are at play, the Wald estimate⁸ suggests that opting for higher benefits for a shorter duration is predicted to increase the subsequent paid unemployment spell duration by more than 5 months. This surprising effect can be partly explained by the fact that those opting for the shorter duration are still entitled to a long potential duration in absolute terms. This very large negative impact fades out over time, as the effect on the total number of days on benefits over the whole period under study is low. Workers favouring the level of benefits are also found to work more frequently and for a higher number of hours during the unemployment spell, but for similar wages. The type of job held during the unemployment spell often being temporary, under short-term and/or part-time contracts, it is not clear to what extent they are a stepping-stone to more stable employment.

The choice feature of the policy makes it an interesting opportunity to test empirically the usual assumption that public intervention in the UI market is justified by adverse selection. Although benefiting from a longer compensation duration comes at the expense of the level of benefits earned early in the spell, the total theoretical amount of benefits collected is larger in the long-duration option.⁹ Positive correlation tests between the predicted unemployment duration and the choice of the most comprehensive and longest coverage supports the hypothesis of adverse selection. Worryingly, I also find that workers with the highest predicted unemployment duration, corresponding to less skilled, educated and younger job-seekers are the ones suffering the most from the detrimental impact of the program on unemployment duration. Therefore, on this specific population, the policy giving a degree of flexibility in the choice of the compensation parameters seems to widen initial inequality in unemployment risk, which raises both efficiency and equity questions.

In Chapter 2, entitled *Unemployment Benefits and the Timing of Redundancies: Evidence from Bunching*, I show that a discontinuous increase in the level of UI benefits at a tenure threshold leads to a scheduling of layoffs that maximises the

⁸In a fuzzy regression discontinuity design, the treatment takeup rate is not necessarily 100% for people assigned to treatment, nor 0% for people not assigned to treatment. The Wald estimator quantifies the effect on the population responding to treatment by dividing the measured effect on the population assigned to treatment by the difference in takeup rates between those assigned and those not assigned to treatment.

⁹The long-duration option actually includes the high-benefit one. The difference is that, by choosing the long-duration option, benefits received at the beginning of the spell will be lower, but will eventually converge to the same level than in the high-benefit option. Therefore, the total theoretical amount of benefits is necessarily higher in the long-duration option, but if the worker finds a job before the end of the entitlement, the actual amount of benefits collected could be higher in the short-duration option.

level of UI benefits for workers. I first identify this response using the fact that, at the threshold, there is a jump in the density of tenure at layoff. I use the bunching methodology, which relates the excess mass of workers laid-off right after the tenure threshold to the missing mass of layoffs that did not occur right below the threshold, to derive an elasticity of employment duration with respect to the level of unemployment benefits. This elasticity, which is the counterpart of the elasticity of unemployment duration with respect to the level of UI benefits traditionally used in the optimal UI formula, is informative on the extent to which firms and workers react to incentives created by UI in their layoff decision.

I also introduce a theoretical framework to better understand the underlying mechanism. The hypothesis I put forward is that employers and workers bargain to maximise the surplus from separation, through the extension of the employment spell. By postponing the layoff date, firms and workers can extract some rent from UI and share it through a lower risk and cost associated with the layoff. I model the share of the rent going to the firm as a lower severance package granted to the worker in exchange of the employment spell extension. Although I cannot directly observe this division of the surplus, I get several predictions from the model that I bring to the data. In particular, I use the heterogeneity analysis, drawing on firms and individual characteristics, to provide evidence in favour of this bargaining scenario. Heterogeneity can stem from variation in the level of incentives or in the ability to bargain. The elasticity estimate is used as a metric to neutralise the variation in the jump in replacement rate at the tenure threshold. The typical buncher is a highly educated and skilled male, older than the average, working in small firms in the service sector. While these individuals characteristics are usually associated with a high individual bargaining power, small firms in the service sector are traditionally the ones where collective representation institutions are less present. Confirming a prediction of the model, I show that workers with the highest amount of severance payment absent the extension of the employment spell, who are those who have more resources to compensate the employer, are more prone to bunch. Taken together, these two findings point to an individual bargaining process. Although not large in magnitude, this response comes at the cost of additional benefits paid to bunchers, who, in turn, will stay unemployed longer. Whereas the firm is not integrated into the optimal UI framework (Baily, 1978; Chetty, 2006), this chapter points to a substantial response of employers to the design of UI.

Chapter 3, *Entitled to Leave: the Impact of Unemployment Insurance Eligibility on Employment Duration and Job Quality*, joint work with Clément Brébion and Simon Briole, investigates a little explored dimension of UI schemes, that is the entitlement conditions. In this chapter, we analyse the minimum employment record condition to qualify for UI benefits on transitions in and out of employment.

Using administrative panel data matching employment and unemployment spells, we provide a comprehensive evaluation of the 2009 reform that has changed the eligibility criterion in France.

We first show suggestive evidence of a separation response at the eligibility threshold at the micro level, through a jump in transitions from employment to registered unemployment and non-employment. This response translates into an aggregate effect, through the scheduling of contract duration in line with the eligibility requirements. Indeed, we show that after the reform reducing the minimum employment record, the number of contracts whose duration corresponds to the new condition increases as compared to the number of contracts whose duration corresponds to the old condition. This evolution is concentrated among some sectors which are the ones previously identified as having a high share of very short contracts in total hiring, and a high separation rate. It is also concentrated on fixed-term contracts, which are the ones most easily adjustable, and providing most of the entries to UI. Among the affected sectors, this relative increase in the number of fixed-term contracts corresponding to the new condition affects workers no matter their eligibility status, or the number of months of employment they need to claim UI benefits. This last observation point to the new condition creating a new norm with regard to the duration of short-term contracts in some sectors resorting frequently to them. It suggests that this reform could be one of the driver of the recent trend of contract shortening observed in France since the beginning of the 2000's.

In the second part of the paper, we analyse the extensive margin impact of UI benefits – that is receiving benefits instead of not receiving any benefit – in terms of future labour market outcomes, in a regression discontinuity design. The use of the reform as well as relevant sample restrictions make this analysis possible despite manipulation in the running variable. Our findings point to a large negative impact of UI benefits receipt on employment probability up to 21 months after meeting the eligibility criterion. The results on job quality do not seem to counterbalance the negative employment response. The data allows at the same time to analyse multiple dimensions of job quality – type of contract, tenure at future job, working hours, wage, matching in terms of sector of activity – but is limited in terms of precision. Our findings point to not economically relevant results, consistent with several papers of the literature on UI and job quality.

Introduction Générale

Alors même que les dépenses d'assurance chômage (AC) ne représentent que 5,13 % des dépenses sociales, leurs effets sur la participation au marché du travail, le chômage et le lissage de la consommation sont au centre d'une abondante recherche académique, qui trouve un écho important dans le débat public. Les dépenses sociales constituent la composante la plus importante au sein des dépenses publiques, et celle qui croît le plus rapidement. Elle comprend les prestations destinées aux ménages à faible revenu, aux personnes âgées, aux personnes handicapées, aux personnes malades, aux jeunes ou aux chômeurs. Elle est passée de 8 % du PIB, en moyenne, dans les pays de l'OCDE en 1960, à 20 % aujourd'hui.

Parmi ces pays, la France se caractérise par les dépenses sociales les plus élevées, 31,2 %, ¹⁰ ce qui en fait un cas d'étude particulièrement intéressant (Graphique 3). Historiquement, l'État-providence en France s'est développé selon le modèle bismarckien, qui se caractérise par un financement par prélèvements obligatoires auprès des employeurs et des employés, et une protection liée au statut professionnel. Plus récemment, l'État-providence français a intégré une composante beveridgienne en faisant de plus en plus reposer son financement sur l'impôt. Cette part élevée des dépenses sociales couplée à la dualité de son système explique pourquoi cette thèse se concentre sur l'étude de la France.

Les dépenses publiques consacrées aux allocations chômage sont incluses au sein des dépenses sociales, et représentent 0,7 % du PIB en moyenne dans les pays de l'OCDE, avec un maximum de 2,9 % en Belgique et un minimum en Europe de 0,2 % au Royaume-Uni (Graphique 3). La France se classe en 4^{ième} position, avec une part à 1,6 %. Comme toute assurance sociale, l'assurance chômage a pour but de protéger contre un événement indésirable, plus précisément contre le risque de perte d'emploi.

Il est intéressant de noter que, même si les régimes d'assurance chômage sont organisés au niveau national dans la plupart des pays développés, on s'interroge rarement sur le bien-fondé d'une telle organisation. Si les conditions d'affiliation au régime et la générosité de la couverture varient d'un pays à l'autre, le principe général d'une assurance couvrant tout travailleur du secteur privé ayant suffisamment contribué au régime existe dans la plupart des pays. Dans le débat académique, des arguments liés à l'efficacité ont été avancés pour justifier les régimes nationaux

¹⁰Source : La base de données de l'OCDE sur les dépenses sociales (SOCX)

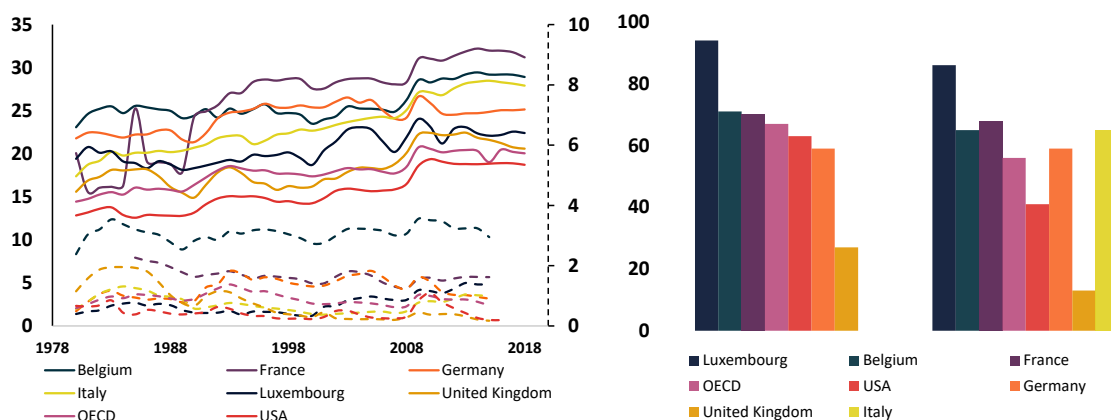
mandatés, alors que le débat politique porte plutôt sur les questions d'équité. En effet, la mise en place d'une assurance au niveau national permet de mutualiser les cotisations entre les différents types de risques confrontés à différentes probabilités de perte d'emploi. Cette mutualisation constitue un premier mécanisme de redistribution au sein des régimes d'assurance chômage, alors même que cela ne constitue pas leur objectif premier, par opposition aux systèmes de solidarité.

La large variation observée dans le niveau des dépenses publiques d'assurance chômage se traduit par une variation tout aussi importante du niveau des allocations chômage entre les pays. Cette générosité se mesure généralement par le taux de remplacement net, qui correspond au rapport entre les prestations nettes d'assurance chômage et le salaire net. D'une part, le taux de remplacement net moyen pour une personne seule sans enfant au salaire moyen, est égal à 56 %. En Europe, le minimum se trouve au Royaume-Uni (13 %) et le maximum au Luxembourg (86 %). D'autre part, ce même taux au salaire minimum passe de 27% (Royaume-Uni) à 94% (Luxembourg), avec une moyenne de 67% dans les pays de l'OCDE (Graphique 3).¹¹ La différence entre les taux de remplacement au salaire moyen et au salaire minimum est une illustration supplémentaire de la composante redistributive des prestations d'assurance chômage. Il convient également de noter que les régimes d'assurance chômage interagissent avec l'ensemble du système socio-fiscal, ce qui se traduit notamment par le fait que les taux de remplacement nets sont généralement plus élevés que les taux bruts, car les allocations sont moins imposées que les revenus du travail.

Ces disparités entre les pays justifient que l'on s'interroge sur le niveau adéquat des allocations chômage. Cette thèse contribue à cette discussion, en apportant des éléments de réponse aux deux questions de recherche suivantes : pourquoi les régimes d'assurance chômage sont-ils mandatés au niveau national ? Quel est le niveau optimal d'assurance à établir ? Ce travail s'appuie sur trois études empiriques, qui examinent l'effet de différents programmes d'assurance chômage sur l'offre de travail, à la fois en emploi et au chômage, sur les choix en matière d'assurance et sur la structure des contrats. Il permet de dégager des enseignements plus généraux sur la conception optimale des systèmes d'assurance chômage. Le premier chapitre aborde ces deux questions. Premièrement, il démontre que le principal motif d'intervention publique sur le marché de l'assurance chômage - le fait que les personnes les plus à risque sont celles qui achètent de l'assurance - est empiriquement valide. Deuxièmement, il s'intéresse à la question du niveau optimal d'assurance chômage en examinant comment les demandeurs d'emploi réagissent à un changement du niveau et de la durée de leurs prestations. Les chapitres 2 et 3 portent sur cette deuxième question. Ils intègrent le comportement des entreprises et des travailleurs avant et après l'épisode de chômage, afin d'élargir la réflexion sur l'impact de l'assurance

¹¹Source : base de données des prestations et des salaires de l'OCDE.

Figure 3: Dépenses sociales et de chômage (gauche), et taux de remplacement nets moyens (droite) dans les pays de l'OCDE (% PIB)



SOURCE: OECD.

NOTE: Le graphique de gauche représente la part des dépenses sociales (sur l'axe de gauche) et des dépenses de chômage (sur l'axe de droite) dans le PIB total. Nous observons que la Belgique et la France sont parmi les pays qui se situent en tête de la distribution des dépenses sociales et de chômage. Globalement, les dépenses sont également plus élevées en Europe qu'aux États-Unis.

Le graphique de droite représente le taux de remplacement net, respectivement au niveau du salaire minimum (à gauche) et au niveau du salaire moyen (à droite). Les deux taux sont calculés pour une personne seule sans enfants, au début de l'épisode de chômage, et n'incluent pas les allocations de logement. La différence entre les deux taux de remplacement est une illustration de la composante redistributive de l'assurance-chômage. Les prestations les plus généreuses se trouvent au Luxembourg, en France et en Belgique, et les moins généreuses au Royaume-Uni. L'Italie ne dispose d'aucune donnée sur le taux de remplacement au salaire minimum, possiblement parce qu'il n'existe pas de salaire minimum national.

chômage sur le marché du travail.

La question du niveau optimal d'assurance chômage a été au centre d'une abondante littérature, donnant lieu à des modèles fondés sur une approche de *statistiques suffisantes*. Ce type de modèle ne requiert pas d'estimer les primitives structurelles sous-jacentes, mais s'attache à comprendre comment les demandeurs d'emploi réagissent aux paramètres de l'assurance chômage en forme réduite (Baily, 1978; Chetty, 2006). Cette approche vise à combler l'écart entre la littérature empirique et théorique sur l'assurance chômage, en réduisant le modèle à un petit ensemble de paramètres qui peuvent être facilement estimés empiriquement. La formule d'assurance chômage optimale est composée de trois paramètres : l'élasticité de la durée de chômage par rapport au niveau des allocations – mesurant le coût de l'assurance chômage – et la baisse de la consommation à l'entrée au chômage, multipliée par le coefficient d'aversion au risque, pour quantifier les bénéfices de l'assurance chômage. Contrairement à l'approche structurelle, cette approche n'exige pas de formuler des hypothèses fortes, avec le désavantage, cependant, d'être insérée dans un régime politique spécifique. Cette thèse s'inscrit dans ce courant de la littérature, en tirant profit de variations de politiques exogènes pour estimer de façon crédible les réactions comportementales au régime d'assurance chômage, dans le but ultime de faire progresser la conception des régimes d'assurance chômage. Elle contribue à cette

littérature à plusieurs égards : (i) nous fournissons d’abord une estimation de l’un des paramètres clés de la formule d’assurance chômage optimale - la sensibilité de la durée du chômage au niveau et à la durée des prestations d’assurance chômage (chapitres 1 et 3) - dans le contexte français ; (ii) nous soulignons l’importance d’autres paramètres non pris en compte dans le modèle initial (Chetty, 2006). En effet, le modèle d’assurance chômage optimal ne prend en considération que le côté de l’offre, sans intégrer le comportement de l’entreprise. Nous montrons que le niveau des allocations chômage influe sur les décisions de l’employeur et du travailleur lorsqu’il est en emploi (chapitres 2 et 3). (iii) Nous estimons ces nouveaux paramètres, mesurés par la sensibilité de la durée d’emploi aux prestations, et les utilisons pour éclairer les évolutions récentes sur le marché du travail (chapitres 2 et 3).

Dans la suite de cette introduction, nous adopterons d’abord une perspective plus large en revenant sur l’origine de l’assurance chômage et sur les raisons pour lesquelles la plupart des pays développés ont choisi de créer des assurances sociales nationales. Nous mentionnerons ensuite les enjeux actuels liés à la conception des systèmes d’assurance chômage, tant sur le plan politique qu’académique, et comment cette thèse s’y rattache. La dernière partie reviendra plus en détail sur les différents chapitres constituant cette thèse.

Du chômage à l’assurance chômage

Le chômage représente une expérience négative complexe aux multiples facettes, contre laquelle les individus sont particulièrement désireux de se protéger. Elle n’affecte pas seulement le revenu (Jacobson, LaLonde and Sullivan, 1993; Von Wachter, Handwerker and Hildreth, 2009) et la richesse, mais de nombreuses autres dimensions du bien-être individuel. Cette multidimensionnalité intéresse depuis longtemps les sociologues et a conduit à classer le chômage comme un phénomène absolu. Dominique Schnapper identifie notamment trois types différents d’expérience du chômage (Schnapper, 1981) : le “chômage inversé”, généralement vécu par des jeunes qui viennent de faire leur entrée sur le marché du travail. Ce type de chômage est assimilé à du temps de loisir, que l’on peut utiliser pour se consacrer à sa passion. Le deuxième type de chômage est défini comme un “chômage différé”, souvent rencontré chez les cadres, caractérisé par une recherche d’emploi très active et de multiples activités de substitution, afin de retarder la véritable expérience du chômage. Enfin, le type le plus répandu et le plus caractéristique correspond au “chômage total”, qui se définit comme une perte de revenu qui s’accompagne d’ennui, d’un sentiment de honte et d’une rupture des liens sociaux.

Les économistes ont également montré que le chômage englobe de nombreuses dimensions. Il peut avoir un impact négatif important sur la santé (Black, Devereux

and Salvanes, 2015; Bergemann, Gronqvist and Gudbjörnsdottir, 2011) et la mortalité (Sullivan and Von Wachter, 2009; Eliason and Storrie, 2009), ainsi que sur le bien-être subjectif (Winkelmann and Winkelmann, 1998), bien que ce dernier effet soit limité dans les régions à taux de chômage élevé (Clark, 2003). Ces derniers travaux mettent en exergue l'importance des facteurs non monétaires tels que les relations sociales et la construction de son identité au sein de la société, ainsi que le rôle des normes sociales dans l'expérience du chômage. Ils s'appuient sur les premiers travaux sociologiques de Lazarsfeld, Jahoda and Zeisel (1933), qui décrivent le déclin d'une ville entière en Autriche, dans les années 1930, lorsque l'usine qui occupe le rôle de principal employeur ferme ses portes. Les auteurs décrivent le sentiment d'inutilité morale et la perte de conscience du temps comme deux forces conduisant à la désocialisation.

De même, Paugam (2006) souligne la complexité du chômage en se demandant s'il peut être défini comme une "rupture cumulative des liens sociaux". Selon lui, la réponse dépend du type d'institutions dans le pays, à savoir le rôle de l'État et de la famille.

Intervention publique sur le marché de l'assurance chômage

Conformément à la tradition de l'économie publique, l'approche adoptée dans cette thèse ne consiste pas seulement à s'intéresser à l'impact du chômage lui-même, déjà largement documenté, mais surtout à comment améliorer la façon dont la société prend en charge le risque de perte d'emploi. La motivation économique traditionnelle soutenant la création d'assurances sociales repose sur deux principes : tout d'abord, la mise en place d'une assurance permet de réduire le coût des événements défavorables lorsque les individus sont averses au risque. En second lieu, l'argument en faveur de l'intervention publique découle de l'identification de trois défaillances du marché : (i) des asymétries d'information, conduisant à une sélection adverse ; (ii) des défauts d'optimisation individuelle et (iii) des externalités.

La première défaillance fait référence au fait que les individus, dans un contexte où les niveaux de risque sont hétérogènes, en savent plus que l'assureur sur leur type de risque. Rothschild and Stiglitz (1992) ont montré que ces deux conditions peuvent conduire à une situation où aucun équilibre n'est compatible avec la mise en place d'une assurance, et qui peut se résoudre par l'instauration d'une assurance nationale obligatoire. L'intuition est que l'assureur établit le prix en fonction du niveau de risque moyen, car il ne dispose d'aucune information sur le type de risque individuel. La présence d'individus à risque élevé rend cette prime moyenne trop coûteuse pour les individus à faible risque, qui quittent le marché. Au fur et à mesure que les individus à faible risque quittent le marché, la prime moyenne augmente, entraînant de plus en plus d'individus à faible risque hors du marché et aboutissant à son effondrement (Akerlof, 1978).

C'est précisément parce que les régimes d'assurance chômage ont toujours été

obligatoires au niveau national, qu'il s'est avéré difficile de vérifier empiriquement la présence d'une sélection adverse sur le marché de l'assurance chômage. La corrélation positive que l'on observe entre le niveau de couverture et le risque réalisé ne permet pas de démêler le rôle de la sélection adverse et celui de l'aléa moral, qui désigne le fait que les individus peuvent avoir des comportements plus risqués dès lors qu'ils sont assurés. Une exception notable est un travail récent de [Landais and Spinnewijn \(2019\)](#) dans lequel ces derniers utilisent la coexistence d'une couverture minimale obligatoire avec une assurance complémentaire privée qui peut être souscrite sur une base volontaire en Suède, pour confirmer la présence de sélection adverse. Combinant une approche par les préférences révélées et par le calcul d'une propension marginale à consommer, ils établissent également que la valorisation de l'assurance chômage a été largement sous-estimée dans les travaux antérieurs. La difficulté de tester empiriquement la présence de sélection adverse est également une problématique que nous traitons dans le chapitre 1 de cette thèse. Nous tirons parti d'un cadre institutionnel rare qui nous permet d'observer, en France, les choix de certains demandeurs d'emploi en matière d'assurance chômage, pour évaluer l'ampleur de la sélection adverse et de l'aléa moral.

Un deuxième argument en faveur de l'introduction d'une assurance chômage publique tient dans le fait que les individus ne sont pas forcément en mesure de s'assurer au bon niveau, à moins que le gouvernement ne les y oblige. Il a été démontré que les individus sous-estiment systématiquement leur probabilité de perdre leur emploi et surestiment leur probabilité d'en trouver un, même lorsqu'ils ont déjà fait l'expérience du chômage ([Spinnewijn, 2015](#); [Mueller, Spinnewijn and Topa, 2018](#)). Même au sein de programmes d'assurance mandatés nationalement, les paramètres comportementaux peuvent jouer un rôle dans les efforts déployés pour conserver et chercher un emploi. L'impatience peut, par exemple, affecter la recherche d'emploi dans les deux directions : elle conduit à attribuer une faible valeur aux bénéfices futurs de la recherche d'emploi, mais peut en même temps encourager les individus à fixer un salaire de réserve plus faible dans la mesure où, quand ils reçoivent une offre, ils préfèrent l'accepter plutôt que d'attendre une offre future potentiellement meilleure ([DellaVigna and Paserman, 2005](#); [Paserman, 2008](#)). Dans le chapitre 1 de cette thèse, dans lequel nous observons les choix des individus en matière d'assurance chômage, nous donnons un aperçu des préférences des assurés en utilisant la manière dont celles-ci sont corrélées avec des données socio-démographiques.

Là où les régimes nationaux imposent généralement tous les paramètres de l'indemnisation, nous utilisons cette flexibilité donnée aux salariés pour montrer que les motifs invoqués pour justifier une assurance nationale obligatoire peuvent s'avérer réels : nous mettons au jour la présence de sélection adverse et nous montrons que les demandeurs d'emploi ne choisissent pas de façon optimale la manière dont ils sont indemnisés dans le but de retrouver rapidement un emploi et dans de bonnes

conditions. Cela remet en cause la conception d'un arbitrage rationnel entre travail et loisir, en accord avec la littérature récente sur les paramètres comportementaux des demandeurs d'emploi.

Si les externalités positives d'une assurance nationale apparaissent de façon plus évidente dans le cas de l'assurance maladie, elles ont également été mobilisées comme un argument en faveur de la création d'une assurance chômage publique (Hsu, Matsa and Melzer, 2014), puisque, par exemple, être assuré peut encourager les individus à accepter des emplois plus risqués et plus productifs (Acemoglu and Shimer, 2000). La mutualisation de la couverture au niveau national peut également réduire les coûts administratifs par rapport à une situation où de nombreux régimes d'assurance coexistent.

Le niveau optimal de l'assurance chômage

Si la question du mandat de l'assurance chômage au niveau national fait plutôt consensus, la question du niveau d'assurance à mutualiser est beaucoup plus controversée. Ce sujet suscite un vif intérêt dans le débat politique et académique. La littérature traitant de l'assurance chômage optimale découle du modèle canonique de Baily (Baily, 1978), plus récemment généralisé par Chetty (Chetty, 2006). Ce modèle est construit sur une approche de *statistiques suffisantes*,¹² mesurant de façon transparente des relations statistiques qui sont le résultat de changements de politiques exogènes, afin d'évaluer l'impact sur le bien-être (Chetty, 2009). L'objectif de cette littérature est de déterminer le niveau optimal d'assurance chômage qui égalise son coût en termes d'aléa moral avec ses bénéfices en termes de lissage de la consommation. Le coût de l'aléa moral pourrait se traduire, du côté de l'offre, par un effort moindre en emploi ou pour rechercher un travail. Le fait que l'assureur ne puisse pas observer le niveau d'effort de l'individu¹³ mène à la recherche d'un compromis entre la réduction de la défaillance de marché et l'amélioration de l'efficacité de l'assurance chômage par la réduction de l'aléa moral. Dans le modèle de Rothschild-Stiglitz, l'assurance totale est optimale car l'aléa moral n'est pas pris en compte. Le modèle d'assurance chômage optimal tient compte de cette distorsion sans intégrer le problème de sélection adverse. Conformément à l'approche de *statistiques suffisantes*, les paramètres clés du modèle peuvent être facilement estimés empiriquement, au prix de conclusions plus locales.

La littérature s'est beaucoup intéressée à l'estimation des coûts, à travers la mesure de l'élasticité de la durée du chômage par rapport au niveau des allocations chômage. Les premiers articles ont exploité la variation transversale au sein d'un

¹²Ce terme fait référence à l'approche de *sufficient statistics*.

¹³Il ne peut qu'observer le résultat, c'est-à-dire le taux de retour à l'emploi, mais ne peut distinguer le cas d'une personne n'exerçant aucun effort et celui d'une personne exerçant un effort mais dont les caractéristiques (éducation, compétences, etc.) rendent l'accès à l'emploi difficile.

échantillon donné, avec comme principale limite le fait que la générosité des allocations chômage est souvent une fonction décroissante du niveau de salaire antérieur. Par conséquent, l'effet de prestations d'assurance chômage plus élevées sur la durée du chômage est facilement confondu avec l'effet d'un revenu antérieur plus faible (et, vraisemblablement, d'un niveau d'éducation et de qualification moins élevé). Des études plus récentes exploitent les variations exogènes fournies par un seuil d'âge, d'ancienneté ou temporel, pour isoler l'impact de la générosité de l'assurance chômage au moyen de méthodes quasi-expérimentales. Ces études ont surtout porté sur la relation entre la générosité de l'assurance chômage et la durée du chômage ou du non-emploi (Lalive, Van Ours and Zweimüller, 2006; Lalive, 2007; Landais, 2015), avec des estimations d'élasticité variant de 0,15 à 2 en Europe et de 0,10 à 1,21, avec une moyenne inférieure, aux États-Unis (Schmieder and Von Wachter, 2016). Le modèle d'assurance chômage optimal initial a ensuite été étendu à l'étude de la durée optimale des prestations (Fredriksson and Holmlund, 2001; Le Barbanchon, 2016; Schmieder and Von Wachter, 2016) ou du profil temporel (Kolsrud et al., 2018). Dans ce dernier article, les auteurs soulignent le fait que la valeur de l'assurance et le coût des prestations d'assurance chômage en termes d'aléa moral peuvent varier au cours de l'épisode de chômage. En particulier, ils révèlent que la baisse de la consommation est importante au début de la période et qu'elle est encore plus marquée par la suite, alors que le coût de l'aléa moral est plus élevé au commencement de l'épisode.

Toutefois, la relation fermement établie entre le niveau des prestations d'assurance chômage et la durée de la période de chômage ne saisit qu'une partie du tableau, car un chômage plus long ne nuit pas nécessairement aux performances futures en matière d'emploi. La question finale revient à déterminer si des allocations chômage plus élevées permettent aux personnes de retarder l'acceptation d'un emploi et de profiter de plus de temps de loisirs, ou si elles subventionnent une meilleure recherche de travail et améliorent les opportunités d'emploi.

C'est un fait empirique démontré que la probabilité de sortir du chômage diminue à mesure que la durée du chômage se prolonge.¹⁴ Économétriquement, il s'avère complexe de distinguer si ce phénomène est dû à la dépréciation du capital humain, ou à la stigmatisation des chômeurs, ou bien à l'hétérogénéité non observée du taux de sortie du chômage. Dans le premier cas, la probabilité de trouver un emploi pour une personne donnée diminue avec la durée du chômage, tandis que dans le second cas, les personnes présentant la probabilité de sortie la plus élevée quittent le chômage plus rapidement. Les deux explications ayant le même impact sur la distribution agrégée du taux de sortie, il est très difficile de les discerner empiriquement (voir Van den Berg and Van Ours (1996) pour une estimation non paramétrique). Du point de vue des politiques publiques, il est crucial de comprendre quelle interprétation prévaut, afin de déterminer si le gouvernement devrait se concentrer sur le chômage

¹⁴Ce phénomène est désigné sous le terme de “*negative duration dependence in unemployment*”.

de longue durée ou sur les personnes dont la probabilité de sortie initiale est la plus faible. Cela touche à la question de savoir si le gouvernement doit agir sur les caractéristiques individuelles qui réduisent la probabilité d'emploi, par la formation par exemple, ou sur des phénomènes plus larges, en trouvant notamment des moyens de réduire la discrimination subie par les chômeurs.

Si la probabilité de sortie décroît avec la durée du chômage, le fait de rester au chômage plus longtemps grâce à des allocations élevées pourrait avoir des répercussions positives, au-delà de l'aléa moral. En effet, la littérature a également souligné l'impact positif potentiel d'un niveau élevé de prestations d'assurance chômage sur les performances postérieures à l'épisode de chômage, au-delà de la seule mesure de la durée du chômage. S'il a été souligné que les allocations chômage pouvaient encourager la recherche d'activités plus risquées et plus productives (Acemoglu and Shimer, 2000), c'est la relation entre l'assurance chômage et la qualité de l'appariement entre offre et demande de travail qui a occupé le centre du débat, avec des résultats variables : Centeno (2004) et Nekoei and Weber (2017) trouvent un effet positif sur la qualité de l'emploi retrouvé, tandis que Schmieder, von Wachter and Bender (2016) mettent au jour un effet négatif et Card, Chetty and Weber (2007); Lalive (2007); Van Ours and Vodopivec (2008) un effet non significatif. L'une des raisons pour lesquelles il est difficile de déceler un effet net significatif peut provenir du fait que deux mécanismes opposés s'additionnent. Des prestations d'assurance chômage plus élevées atténuent les contraintes de liquidité et permettent de viser des salaires plus élevés (canal de sélectivité),¹⁵ ce qui pourrait être contrebalancé par une probabilité de sortie qui décroît avec la durée du chômage. Le modèle standard de recherche d'emploi prévoit que des prestations plus élevées permettent aux travailleurs d'accepter des emplois mieux rémunérés, ce qui fait référence au premier canal mentionné. Cependant, lorsque le lien entre la probabilité de sortie et la durée de chômage déjà écoulée est intégré au modèle, la direction de l'effet devient ambiguë (Nekoei and Weber, 2017). Le Barbanchon, Rathelot and Roulet (2017) distinguent les deux mécanismes et examinent la validité du canal de la sélectivité en analysant directement l'impact des prestations d'assurance chômage sur les salaires de réserve. Ils rapportent une estimation précise de l'élasticité du salaire de réserve par rapport à la durée potentielle des prestations qui est proche de zéro.

Une meilleure compréhension de la relation entre *l'assurance chômage* et les performances sur le marché du travail *avant* et *après* l'épisode du chômage, qui dépasse la seule mesure de la durée du chômage, est un enjeu crucial. Les chapitres 2 et 3 de ma thèse contribuent à cet objectif de recherche. Le chapitre 2 est axé

¹⁵Nekoei and Weber (2017) distinguent entre salaire cible et salaire de réserve. Selon eux, le premier est mesuré par le salaire du dernier emploi auquel un individu a postulé, alors que le second est une construction théorique que nous n'observons jamais. En effet, on ne pose jamais de questions sur le salaire minimal qu'une personne est prête à accepter sans faire mention à un emploi de référence.

sur les périodes antérieures à l'épisode de chômage, et analyse la planification des licenciements en fonction des allocations chômage, par une entente potentielle entre les entreprises et les travailleurs. Le chapitre 3, rédigé conjointement avec Clément Brébion et Simon Briole, traite de l'influence des allocations chômage sur les périodes antérieures et postérieures à l'épisode de chômage. En utilisant la condition d'éligibilité aux prestations d'assurance chômage qui impose une durée d'affiliation minimale, nous montrons d'abord que les paramètres de l'assurance chômage ont une incidence sur le comportement de rupture de contrat et sur la durée des contrats offerts. Nous évaluons ensuite l'effet de la marge extensive des allocations chômage sur la probabilité d'emploi et la qualité de l'emploi retrouvé.

Problématiques théoriques et politiques actuelles sur la conception des systèmes d'assurance chômage

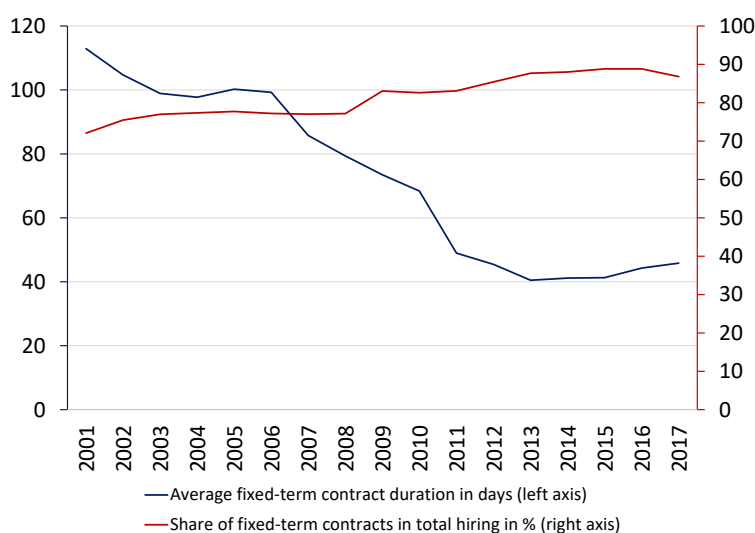
Les récentes évolutions sur le marché du travail ont déplacé le centre de gravité de la littérature vers l'étude des interactions entre l'assurance chômage et les comportements en emploi. Dans la plupart des pays européens, les trajectoires professionnelles sont de plus en plus fragmentées et les transitions entre emploi et chômage se multiplient. La part de l'emploi temporaire dans l'emploi salarié total est passée de 9,19 % en moyenne dans les pays de l'OCDE en 1980 à 11,24 % en 2017. Cette tendance générale dissimule une large hétérogénéité : en Italie ou au Luxembourg, ce chiffre a plus que doublé, alors qu'il est resté presque stable au Royaume-Uni ou en Suède. Parmi les évolutions les plus remarquables, les Pays-Bas et la France ont vu leur part multipliée par quatre. De même, le taux d'emploi à temps partiel a augmenté de quatre points de pourcentage en moyenne dans les pays de l'OCDE depuis le milieu des années 1970, pour atteindre 16,55 %. L'évolution est particulièrement marquée dans les pays où la part de l'emploi temporaire a le moins augmenté.

En parallèle de ces évolutions, la législation en matière de protection de l'emploi concernant les contrats temporaires, telle que calculée par l'OCDE,¹⁶ est devenue moins stricte dans de nombreux pays depuis les années 1990, comme l'Italie, la Grèce, la Belgique, l'Allemagne, le Portugal, l'Espagne ou la Suède. Dans ce cadre général, la France constitue un cas d'étude remarquable, non seulement en raison de l'augmentation sensible de la part de l'emploi temporaire en termes de stock, mais aussi en raison de l'augmentation de la part de l'emploi temporaire dans l'embauche totale, et de la réduction de la durée moyenne des contrats à durée déterminée. La

¹⁶Source : OCDE

première est passée de 72 % en 2001 à 87 % en 2017, tandis que la seconde a été ramenée de 117 à 43 jours (Graphique 4).¹⁷ Ces tendances s'expliquent en partie par l'augmentation de la part des contrats de moins d'un mois, phénomène qui s'est accompagné d'une augmentation de la proportion des travailleurs embauchés par un ancien employeur. La part de la réembauche est ainsi passée de 46 % du total des recrutements en 1995 à 69 % en 2012. Ce chiffre s'élève même à 84 % pour les contrats d'un mois ou moins (Benghalem, 2016). Le taux de rotation a également fortement crû, évoluant de 29 % en 1993 à 96 % en 2017, ce qui s'explique en partie par l'augmentation des contrats très courts, puisque les contrats d'un jour représentaient 30 % de la totalité des contrats en 2017 (DARES, 2018).

Figure 4: Part et durée moyenne des contrats à durée déterminée en France (2001-2017)



SOURCE: DARES.

NOTE: Ce graphique trace l'évolution de la part des contrats à durée déterminée dans l'embauche totale (sur l'axe de droite) et de la durée moyenne des contrats à durée déterminée (sur l'axe de gauche) en France. On constate que les contrats à durée déterminée représentent une part croissante des flux vers l'emploi, avec une durée moyenne décroissante.

Ces tendances récentes ont placé au centre du débat public les chômeurs alternant les courts épisodes d'emploi et de chômage. Le nouveau profil-type du chômeur s'est éloigné du modèle traditionnel d'une carrière dans la même entreprise sous contrat à durée indéterminée, où le chômage n'aurait été qu'un accident de parcours. La réforme de l'assurance chômage de 2014 en France visait précisément ce type de travailleurs, en introduisant une simplification des règles de cumul des revenus de l'emploi et des prestations d'assurance chômage, et la possibilité d'étendre ses droits à l'assurance chômage par des emplois, même de courte durée, occupés pendant l'épisode de chômage.¹⁸

¹⁷Source: DARES

¹⁸Cette dernière mesure porte le nom de droits rechargeables.

Ces changements institutionnels et ceux relatifs à la structure du marché du travail invitent à une meilleure prise en compte des interactions entre l'assurance chômage et l'emploi. La présente thèse contribue à ce changement d'orientation en consacrant les deux derniers chapitres à l'analyse de la façon dont l'assurance chômage influe en partie sur les décisions d'embauche et de licenciement des employeurs, en réaction aux changements dans l'option de sortie des travailleurs.

Plusieurs articles récents ont examiné comment les règles de l'assurance chômage peuvent influencer sur la durée de l'emploi. Certains se sont concentrés sur des populations de travailleurs spécifiques, telles que les seniors, pour qui l'assurance chômage peut parfois servir de pont vers la retraite. [Baguelin \(2016\)](#) a élaboré un modèle, confirmé par des résultats empiriques en France ([Baguelin and Remillon, 2014](#)), démontrant que dans certains cas, la dernière sortie de l'emploi était programmée de telle sorte que les travailleurs étaient couverts successivement par l'assurance chômage, puis par la retraite, sans interruption de paiement. Dans le contexte autrichien, [Jäger, Schoefer and Zweimüller \(2019\)](#) font également état d'une augmentation marquée du taux de départ à un âge stratégique, en réaction à une politique d'augmentation de la durée des prestations pour les travailleurs âgés.

D'autres articles se sont penchés sur l'impact des critères d'éligibilité à l'assurance chômage définis en termes d'historique d'emploi – appelé durée d'affiliation en France – sur les transition de l'emploi vers le chômage ([Martins, 2016](#); [Baker and Rea Jr, 1998](#); [Green and Riddell, 1997](#); [Rebollo-Sanz, 2012](#); [Albanese, Ghirelli and Picchio, 2019](#)), respectivement au Portugal, Canada, Espagne et Italie. En effet, dans la plupart des pays développés, les travailleurs doivent avoir un emploi et cotiser à l'assurance chômage pendant un nombre minimum de mois pour pouvoir recevoir des allocations par la suite. Cette condition est au cœur de la nature contributive des assurances sociales, qui impose un lien fort entre ce qui est payé et ce qui est reçu. La conclusion commune qui ressort de ces articles est que les employeurs et les employés réagissent aux incitations créés par l'assurance chômage, même pendant l'épisode d'emploi. Cela s'illustre par une discontinuité dans le taux de transition de l'emploi vers le non-emploi au seuil d'éligibilité. Étant donné que les départs volontaires n'ouvrent pas droit aux allocations chômage dans la plupart des pays, cette augmentation discontinue des sorties de l'emploi découle nécessairement de licenciements ou de ruptures conventionnelles, révélant le rôle déterminant des employeurs.

Plusieurs explications à ce phénomène peuvent être invoquées : du côté de l'offre de travail, les salariés pourraient être incités à exercer moins d'efforts sur le lieu de travail lorsque leur option de sortie, c'est-à-dire la valeur du chômage, augmente. Du côté de la demande, les employeurs peuvent trouver un intérêt à licencier les travailleurs dès lors qu'ils sont admissibles à l'assurance chômage, afin de s'adapter avec plus de souplesse aux fluctuations de la demande, et avec une éventuelle intention de réembauche future. Ce type d'accord employeurs-employés

peut faciliter les relations entre les deux parties dans un contexte où les travailleurs sont fréquemment rappelés par un ancien employeur ([Christofides and McKenna, 1995](#)). Le cadre théorique standard habituellement mobilisé pour rationaliser ce résultat est la théorie du contrat implicite ([Feldstein, 1976](#); [Baily, 1977](#)). Au moment de l'embauche, les employeurs peuvent accepter de se séparer de leur salarié lorsque l'option extérieure de celui-ci augmente de façon discontinue au seuil d'éligibilité. Les employeurs et les travailleurs peuvent partager le surplus de la fin de contrat à travers des salaires d'équilibre plus faibles, par exemple.

[Van Doornik, Schoenherr and Skrastins \(2018\)](#) ont constaté qu'au Brésil, non seulement les flux vers le chômage augmentent de 12 % au seuil d'éligibilité à l'assurance chômage, mais l'ensemble des licenciements et des embauches suit un schéma compatible avec un comportement stratégique lié aux incitations de l'assurance chômage. Plus précisément, les employeurs licencient les travailleurs lorsqu'ils deviennent éligibles à l'assurance chômage et les réembauchent lorsque leurs prestations sont épuisées. Ils documentent également un phénomène d'emploi informel dissimulé par du chômage. Les travailleurs conservent donc leur emploi de façon informelle, tout en recevant des allocations chômage. Cette forme extrême d'optimisation passe par une entente entre les entreprises et les salariés afin d'extraire une rente de l'assurance chômage et la partager à travers des salaires d'équilibre plus bas.

À l'inverse, la théorie du contrat efficient n'est pas toujours vérifiée. [Jäger, Schoefer and Zweimüller \(2019\)](#) utilisent la comparaison du groupe affecté par une politique de prolongation de la durée des prestations d'assurance chômage, après son abolition, avec le groupe témoin. Ils rejettent la théorie de la séparation efficiente, qui impliquerait que la politique détruise les emplois dégageant un faible surplus et permette aux emplois au surplus élevé de mieux résister. Les emplois détruits à la suite de la politique de prolongation des droits du chômage correspondent à des emplois ouvriers, occupés par des travailleurs plus âgés et plus fréquemment malades, dans des entreprises en déclin. Cependant, ils sont associés à des salaires similaires aux salaires des emplois ayant subsisté, ce qui s'explique, selon les auteurs, par des frictions salariales et par la législation sur les licenciements empêchant une négociation efficiente.

Dans la lignée de cette littérature, nous montrons dans le chapitre 3 de cette thèse que l'ajustement de la durée du contrat au critère d'éligibilité à l'assurance chômage peut être accordé en échange d'une adaptation plus flexible aux fluctuations de la demande. En effet, l'effet que nous mettons en lumière est plus marqué dans les secteurs caractérisés par un taux de séparation élevé et une forte part de contrats très courts dans l'embauche totale.

Selon [Van Doornik, Schoenherr and Skrastins \(2018\)](#), ce phénomène de mise à profit de l'assurance chômage pourrait se trouver réduit grâce à "une modulation des contributions plus forte, des règles d'éligibilité plus strictes ou des taux de remplace-

ment plus bas. Des politiques plus nuancées peuvent passer par des modifications des règles d'éligibilité pour empêcher le licenciement répété d'un même travailleur." C'est précisément dans cet esprit qu'a été décidée la réforme de l'assurance chômage en France en 2019, qui a fait passer le seuil d'éligibilité de 4 mois au cours des 28 derniers mois à 6 mois au cours des 24 derniers mois, et qui a introduit une modulation des cotisations dans une perspective de tarification personnalisée du coût de l'assurance chômage pour les entreprises.¹⁹

Contributions et plan de la thèse

La présente thèse s'appuie sur la vaste littérature sur l'assurance chômage optimale décrite ci-dessus, pour tenter de répondre à des questions au cœur des débats académiques et de politiques publiques. Elle élargit le champ des connaissances existantes sur ce sujet : (i) premièrement, nous examinons l'impact de l'assurance chômage sur des caractéristiques différentes de celles habituellement analysées, associées aux périodes pré et post-chômage, sans seulement se concentrer sur la durée du chômage. En particulier, nous intégrons le rôle des entreprises dans la réflexion sur la conception optimale des systèmes d'assurance-chômage. (ii) Deuxièmement, nous apportons des preuves empiriques afin de confirmer ou réfuter les arguments théoriques avançant l'existence d'une sélection adverse sur le marché de l'assurance chômage, et qui mettent en évidence une optimisation imparfaite des demandeurs d'emploi.

Cette thèse porte sur la question fondamentale, en économie publique, de savoir si le gouvernement doit mettre en place des assurances sociales et quelle est la part optimale des assurances sociales dans la société. Plus précisément, nous essayons de comprendre comment les entreprises et les travailleurs réagissent à l'évolution des paramètres de l'assurance chômage, en tenant compte des interactions entre la façon dont sont élaborés les contrats, l'offre de travail, les comportements de recherche d'emploi et les choix d'assurance. Étant donné que les prestations d'assurance chômage sont une composante de l'option de sortie du travailleur, il est probable qu'elles influencent son offre de travail et son niveau d'effort non seulement pendant le chômage mais aussi lorsqu'il est en emploi. L'utilité des travailleurs peut également être incorporée par les employeurs et influencer sur leurs décisions d'embauche et de licenciement. L'analyse de la réaction de chaque partie aux modifications du régime d'assurance chômage offre l'occasion de tirer des enseignements sur l'élaboration des règles optimales d'assurance chômage, à la lumière des récents développements sur le marché du travail. Nous nous appuyons sur une connaissance approfondie du contexte institutionnel français et sur des méthodologies empiriques récentes, telles que les méthodes quasi-expérimentales ou de *bunching*, pour résoudre le problème

¹⁹Décret n° 2019-797 du 26 juillet 2019 relatif au régime d'assurance chômage

de l'endogénéité. L'utilisation de données administratives couplée à l'identification de réformes permet d'exploiter des changements locaux pour estimer précisément la relation entre l'assurance chômage et le marché du travail. Bien que cette stratégie "locale" puisse affaiblir la validité externe des résultats, elle permet d'identifier des phénomènes qui ne seraient pas nécessairement détectables dans les données d'enquête, et de faire des progrès dans la connaissance de la conception des systèmes d'assurance chômage.

Cette thèse a été réalisée à partir des données d'assurance chômage de l'Unédic, qui est l'organisme chargé de la gestion de l'assurance chômage en France. En plus de fournir les données, l'Unédic m'a également hébergée pendant toute la durée de mon doctorat et m'a permis de bénéficier de l'expertise de spécialistes de l'assurance chômage travaillant au plus près du "terrain". Cette collaboration a permis à cette thèse d'être connectée à des questions importantes dans le débat public sur l'élaboration des règles d'assurance chômage dans les pays développés. La France est un pays particulièrement adapté pour mener ce type de recherche. La législation sur l'assurance chômage étant révisée tous les deux à trois ans, sous l'égide des partenaires sociaux, les sources de variation que l'on peut exploiter pour répondre aux questions clés touchant à l'élaboration des règles de l'assurance chômage sont multiples.

Le premier chapitre de cette thèse porte sur la relation entre le niveau des prestations d'assurance chômage, la durée potentielle des prestations et la durée du chômage. Ce travail tire avantage d'un programme où les travailleurs arbitrent entre le niveau et la durée des allocations chômage, afin de comprendre les déterminants et les conséquences de leur choix. Le deuxième chapitre examine l'incidence des prestations d'assurance chômage sur la date de fin de contrat, dans le contexte des licenciements économiques, et montre que les entreprises et les travailleurs réagissent aux incitations créés par l'assurance chômage quand ils sont en emploi. Enfin, le troisième chapitre, rédigé conjointement avec Clément Brébion et Simon Briole, porte sur la nature contributive de l'assurance chômage, qui impose d'accumuler un nombre minimum de jours d'emploi pour recevoir des allocations. Nous examinons l'incidence de ce critère d'éligibilité sur le taux de sortie de l'emploi, la durée des contrats, la probabilité d'emploi et la qualité de l'emploi futur.

Dans le chapitre 1, *Generosity versus Duration Trade-Off and the Optimisation Ability of the Unemployed*, nous exploitons un cadre peu commun dans le système français d'assurance chômage, dans lequel certains chômeurs ont le choix entre deux régimes d'assurance lorsqu'ils subissent une baisse de revenu très importante au moment où ils entrent au chômage. Ce programme introduit un arbitrage entre le niveau et la durée potentielle des prestations, qui peut être utilisé pour mieux appréhender les préférences des chômeurs concernant le niveau d'assurance souhaité.

À l'aide de données administratives sur le chômage, nous décrivons les déterminants de ce choix. Nous établissons que les travailleurs qui optent pour des allocations plus élevées plutôt qu'une durée plus longue sont en moyenne plus jeunes, de sexe masculin, avec un niveau d'éducation et de qualification plus élevé que les personnes éligibles qui choisissent l'autre option. Ces caractéristiques peuvent être associées à de meilleures perspectives d'emploi, à une plus faible aversion au risque ou à une plus grande impatience, qui les amènent à privilégier le niveau des prestations reçues immédiatement au détriment de la durée.

La présence de critères d'éligibilité permet d'évaluer l'impact sur le marché du travail du contrat d'indemnisation choisi, au moyen d'un modèle de régression sur la discontinuité floue. Bien que l'impact ne soit pas clair *a priori*, puisque deux forces contraires sont présentes, l'estimateur de Wald²⁰ suggère que le fait d'opter pour des prestations plus élevées pour une durée plus courte fait augmenter de plus de cinq mois la durée du prochain épisode de chômage indemnisé. Cet effet surprenant s'explique en partie par le fait que ceux qui optent pour une durée plus courte ont toutefois droit à une longue durée d'indemnisation potentielle en termes absolus. Ce très fort impact négatif s'estompe avec le temps, comme en témoigne le faible effet sur le nombre total de jours indemnisés sur l'ensemble de la période étudiée. Les chômeurs privilégiant le niveau des prestations travaillent aussi plus souvent et plus longtemps pendant leur épisode de chômage, mais pour des salaires similaires. Les personnes cumulant emploi et chômage sont souvent en contrat à durée déterminée, de courte durée et/ou à temps partiel. Savoir dans quelle mesure ce statut constitue un tremplin vers un emploi plus stable est une question ouverte.

Observer un choix entre plusieurs régimes d'assurance est une occasion unique de tester empiriquement la principale hypothèse qui sous-tend l'intervention publique sur le marché de l'assurance chômage, la sélection adverse. Si bénéficiaire d'une plus longue durée d'indemnisation se fait au détriment du niveau des allocations reçues au début de l'épisode de chômage, le montant théorique total des allocations perçues est plus élevé dans le cas de l'option du droit long.²¹ Des tests de corrélation positive entre la durée prédite du chômage et le choix d'une couverture assurantielle plus complète étayaient l'hypothèse de la présence de sélection adverse. Des résultats complémentaires nous amènent au constat inquiétant que les travailleurs dont la durée de chômage prédite est la plus élevée, correspondant aux demandeurs d'emploi

²⁰Dans le cadre d'une régression sur la discontinuité floue, le taux de recours au traitement n'est pas forcément de 100 % chez les personnes assignées au traitement, ni de 0 % chez les personnes non assignées au traitement. L'estimateur de Wald renseigne sur l'effet au sein de la population qui réagit au traitement en divisant l'effet mesuré sur la population assignée au traitement par la différence de taux de recours entre les personnes assignées et non assignées au traitement.

²¹La différence est qu'en choisissant l'option du droit long, les prestations reçues au début de l'épisode seront moins élevées, mais elles finiront par converger vers le même niveau que dans l'option à prestations élevées. Par conséquent, le montant théorique total des prestations est nécessairement plus élevé dans l'option du droit long, mais si le travailleur trouve un emploi avant la fin du droit, le montant réel des allocations perçues pourrait être supérieur dans l'option du droit court.

plus jeunes et au niveau d'éducation et de qualification plus faible, sont ceux qui souffrent le plus de l'impact négatif du programme. Par conséquent, la politique consistant à donner plus de flexibilité dans le choix des paramètres d'indemnisation, sur cette population spécifique, semble creuser les inégalités initiales en termes de risque de chômage, ce qui soulève à la fois des questions d'efficacité et d'équité.

Dans le chapitre 2, intitulé *Unemployment Benefits and the Timing of Redundancies: Evidence from Bunching*, nous montrons qu'une augmentation discontinue du niveau des allocations chômage à un seuil d'ancienneté mène à la planification de la date des licenciements économiques, de sorte à maximiser le niveau des prestations d'assurance chômage touchées par les salariés. Nous identifions d'abord cette réponse en utilisant le fait que l'on observe une discontinuité dans la densité de l'ancienneté au moment du licenciement au niveau du seuil stratégique. Nous utilisons la méthode dite du *bunching*, qui établit un lien entre la concentration de travailleurs licenciés immédiatement après le seuil d'ancienneté et la masse manquante de licenciements qui n'ont pas eu lieu juste en dessous du seuil, pour obtenir une élasticité de la durée d'emploi par rapport au niveau des allocations chômage. Cette élasticité, qui est le pendant de l'élasticité de la durée du chômage par rapport au niveau des allocations chômage traditionnellement utilisée dans la formule d'assurance chômage optimale, indique dans quelle mesure les entreprises et les travailleurs réagissent aux incitations créées par l'assurance chômage dans leur décision de licenciement.

Nous présentons également un cadre théorique permettant de mieux comprendre les mécanismes sous-jacents. L'hypothèse que nous soutenons est que les employeurs et les salariés négocient pour maximiser le surplus issu de la fin de contrat, en prolongeant la période d'emploi. En reportant la date de licenciement, les entreprises et les travailleurs peuvent extraire une rente de l'assurance chômage et la partager à travers une réduction des risques et des coûts associés au licenciement. La part de la rente qui revient à l'entreprise est modélisée par une indemnité de départ moins élevée accordée au salarié en échange de la prolongation de la période d'emploi. Si nous n'observons pas directement cette division du surplus, le modèle nous permet de faire certaines prédictions que nous confrontons aux données. En particulier, nous effectuons une analyse d'hétérogénéité, en nous appuyant sur des données individuelles et d'entreprises, pour étayer le scénario d'une négociation. Les sources de l'hétérogénéité sont à trouver dans la variation du niveau des incitations ou de la capacité de négociation. L'estimation de l'élasticité est utilisée comme métrique pour neutraliser la variation dans l'écart des taux de remplacement au seuil d'ancienneté. Le *buncher* type est un salarié homme, au niveau d'éducation et de qualification élevé, plus âgé que la moyenne, travaillant dans une petite entreprise du secteur des services. Bien que ces attributs individuels soient généralement associés à un pouvoir de négociation individuel élevé, les petites entreprises du secteur des services

sont traditionnellement caractérisées par une faible présence des institutions de représentation du personnel. Confirmant une prédiction du modèle, nous montrons que les salariés dont le montant de l'indemnité de licenciement est le plus élevé en l'absence de prolongation du contrat, c'est-à-dire ceux qui ont le plus de ressources pour compenser leur employeur, sont plus susceptibles d'être licencié juste après le seuil. Ces deux résultats pris conjointement indiquent que le mécanisme à l'œuvre est un processus de négociation individuel. Cette réaction comportementale, bien que d'une ampleur limitée, induit un coût supplémentaire provenant d'allocations additionnelles versées à ces *bunchers* qui, en retour, resteront au chômage plus longtemps. Alors que l'entreprise n'est pas intégrée dans le modèle d'assurance chômage optimale (Baily, 1978; Chetty, 2006), le présent chapitre révèle une réaction importante des employeurs aux règles de l'assurance chômage.

Le chapitre 3, *Entitled to Leave : the Impact of Unemployment Insurance Eligibility on Employment Duration and Job Quality*, rédigé conjointement avec Clément Brébion et Simon Briole, examine une dimension peu explorée des régimes d'assurance chômage, à savoir les conditions d'éligibilité. Dans ce chapitre, nous analysons la condition minimale d'affiliation imposant d'avoir travaillé un minimum de mois dans le passé pour être indemnisé par l'assurance chômage, sur les transitions de et vers l'emploi. À l'aide de données administratives permettant d'apparier les périodes d'emploi et de chômage pour un panel d'individus, nous produisons une évaluation exhaustive de la réforme qui a modifié, en 2009, les critères d'éligibilité en France.

Nous mettons au jour l'existence d'une réaction à la réforme au niveau micro-économique, à travers une augmentation des transitions de l'emploi vers le chômage ou le non-emploi au niveau du seuil d'éligibilité. Cette réponse se traduit, au niveau agrégé, par un ajustement de la durée des contrats à la condition d'éligibilité. En effet, nous montrons qu'après la réforme réduisant la durée d'affiliation minimale, le nombre de contrats dont la durée correspond à la nouvelle condition augmente par rapport au nombre de contrats dont la durée correspond à l'ancienne condition. Cette évolution se concentre dans certains secteurs, précédemment identifiés comme ayant une part élevée de contrats très courts dans l'embauche totale, et un taux de séparation élevé. Elle se concentre également sur les contrats à durée déterminée, qui sont les plus facilement ajustables, et qui constituent la plupart des entrées à l'assurance chômage. Parmi les secteurs touchés, cette augmentation relative du nombre de contrats à durée déterminée correspondant à la nouvelle condition affecte tous les salariés, quel que soit leur statut d'éligibilité ou le nombre de mois d'emploi manquants pour faire une demande d'indemnisation. Ce dernier résultat indique que la nouvelle condition d'éligibilité aurait contribué à façonner une nouvelle norme concernant la durée des contrats courts, dans certains secteurs qui y ont fréquemment recours. Cela suggère que cette réforme pourrait être l'un des facteurs d'explication

de la tendance récente au raccourcissement des contrats observée en France depuis le début des années 2000.

Dans la deuxième partie de ce chapitre, nous analysons l'impact, sur le marché du travail, de la marge extensive des allocations chômage - qui fait référence au fait de recevoir des prestations plutôt que de ne pas en recevoir du tout - en utilisant un modèle de régression sur la discontinuité. Des restrictions d'échantillon pertinentes, couplées à l'utilisation de la réforme, rendent cette analyse possible malgré la manipulation de la variable de date de fin de contrat servant à déterminer l'affectation au traitement. Nous révélons que le fait de recevoir des allocations chômage a une incidence négative importante sur la probabilité d'emploi, jusqu'à 21 mois après avoir atteint le critère d'éligibilité. L'impact sur la qualité de l'emploi ne semble pas suffisant pour compenser l'effet négatif en matière de taux de retour à l'emploi. Si les données permettent d'analyser de multiples dimensions de la qualité de l'emploi - type de contrat, durée de l'emploi futur, temps de travail, salaire, adéquation en termes de secteur d'activité - elles sont limitées en termes de précision. Les résultats mis en lumière sont modestes, voire non significatifs, en adéquation avec la plupart des papiers s'intéressant aux liens entre assurance chômage et qualité de l'emploi.

Chapter 1

Generosity versus Duration Trade-Off and the Optimisation Ability of the Unemployed*

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Abstract

In this chapter, I exploit an uncommon setting of the French unemployment insurance (UI) scheme, the fact that the unemployed are given the choice between two UI entitlements when they experience a very large drop in income entering unemployment. It introduces a trade-off between the level of UI benefits and the potential benefit duration, which can be used to discern preferences of the unemployed over the level of insurance coverage desired. Using administrative unemployment data, I provide descriptive evidence to analyse the determinants of the choice. The presence of eligibility criteria allows to assess the labour market impact of the chosen compensation contract, through a fuzzy regression discontinuity design. The Wald estimate suggests that opting for higher benefits for a shorter duration is predicted to increase the subsequent paid unemployment spell duration by more than five months. Although this very large negative impact has to be qualified by the low effect found in the long-run total number of days on benefits, it raises some questions on the efficiency of a policy targeting a population already at risk on the labour market.

JEL Codes: J08, J65, J68, H31

Keywords: Unemployment, Behavioural response to benefits, Insurance design

1 Introduction

The unemployment insurance (UI) scheme aims at balancing its insurance value in terms of consumption smoothing with its cost in terms of disincentive effect on job search. The proper quantification of the insurance gains associated to UI is therefore crucial to assess the performance of UI schemes. However, the valuation of UI coverage is often not well identified. The main challenge is that, unlike classic private insurance schemes, nationally-mandated UI schemes¹ do not allow the insured party to choose the desired level of coverage. Therefore, it is not possible to infer insurance preferences of individuals from their choices through a revealed-preference approach. The optimal insurance literature (initiated by [Baily \(1978\)](#) and later generalised by [Chetty \(2006\)](#)) makes the insurance benefit depend on the drop in consumption from the employed to the unemployed states and on risk-aversion, whereas both are hardly identifiable.²

I take advantage of an uncommon UI program that let job-seekers, under some eligibility conditions, choose between receiving a low compensation for a long total potential duration and receiving a higher benefit immediately for a shorter total potential duration. This possibility, called the *option right*, has been introduced in France on April, 1st, 2015. Exercising the option right means to choose the high-benefit option. This program allows unemployed workers alternating short employment and unemployment spells with large variation in their earnings to better smooth their income. More precisely, people entering back into unemployment with a remainder from their previous unemployment right that they have not exhausted are given a choice between either (i) benefiting from their full right (starting with the remainder and followed by their new right) or (ii) directly using their new right (computed on the basis of their very last employment spell only). When there is a large gap in benefits associated with the two rights – because last work contract was highly paid – the unemployed person can opt for the new right and give up on the remainder of the previous right.

This trade-off can be considered as a choice between long-term smoothing, allowed by the longer coverage, or short-term smoothing, as a higher benefit will avoid a big drop in income from employment to unemployment. The risk of the second option is that the worker ends up with zero income if he does not find a job by the end of the short compensation period. From an insurance perspective, what makes the worker better off is *a priori* unclear. That is the reason why employers' and employees' representatives who regulate the UI design in France have decided to let the choice in the hands of the unemployed person herself.

¹The main form of UI in developed countries.

²The risk aversion coefficient is, by definition, difficult to estimate, whereas results on the optimal level of UI are highly dependent on it. Similarly, data on consumption are not easily available.

Such a setting is particularly appropriate for the study of the behavioural response of unemployed people to the level of benefits as well as to the potential benefit duration (PBD). In theory, both effects are expected to go in opposite directions, as most of the literature has found a positive relationship between the level of benefits and unemployment duration on the one hand, and potential benefit duration and unemployment duration on the other hand ([Schmieder and Von Wachter \(2016\)](#) for a review). Therefore, the net impact is ambiguous. Although it is not possible to disentangle the two effects in this chapter, I analyse the ultimate impact of opting for the shorter right associated to a higher benefit, and whether taking the option right makes the unemployed person better-off in financial and employment terms. I am therefore able to measure the joint impact of the change in the level and the duration of benefits, potentially different from the addition of the two separate impacts.

This analysis is made possible by taking advantage of eligibility criteria introducing thresholds that can be exploited as part of a regression discontinuity design. This allows for a clean estimation of the effect of the option right, measuring the sensitivity to a simultaneous change in benefits level and potential duration. The behavioural response can be linked to the characteristics of both the dropped and the new rights, to find out whether unemployed people are able to optimise their compensation conditions and to maximise their benefits capital. In particular, I perform a heterogeneity analysis of both the takeup rate and the labour market impact of the option right to know more about the optimisation ability of various groups of workers. Indeed, the choice made by eligible workers relates to their anticipations about their unemployment survival rate and their labour market prospects. These expectations are both influenced by objective parameters based on their past work experience and by subjective beliefs on their probability to find a job. Preferences parameters such as impatience and risk-aversion are also likely to affect the choice.

This work relates to the optimal unemployment insurance literature, as it explores the behavioural response of workers to different parameters of UI, in order to measure its distortion cost on labour supply. The optimal UI model, as first developed by [Baily \(1978\)](#) and [Chetty \(2006\)](#), aims at balancing the cost of UI in terms of moral hazard with the benefits in terms of consumption smoothing. The ultimate formula equalising the cost and the gain of transferring one euro from the employed to the unemployed state, consists in a small number of sufficient statistics that can be estimated empirically. In line with the papers which quantified the distorting effect of UI on unemployment duration or reservation wage ([Meyer, 1988](#); [Feldstein and Poterba, 1984](#); [Le Barbanchon, Rathelot and Roulet, 2017](#)), I provide a new empirical estimation of the impact of higher benefits in the French context.

Some authors ([Landais, 2015](#); [Kolsrud et al., 2018](#)) have built upon the initial framework to study the optimal time profile of UI, contributing to draw the attention

also on the duration of UI entitlements and not only on the replacement rate, and highlighting the links between the two parameters. Both elasticities are crucial to evaluate the cost and welfare effect of UI and to improve its design. This chapter contributes to give more insights into this issue. Often examined separately, the analysis focuses here on the combined effect of both parameters on labour market outcomes.

Contributing to an emerging literature, this chapter also looks at the value side of the optimal UI formula, exploiting the choice feature of the option right. Indeed, the option right trade-off can be considered as a choice between more or less insurance. If the premium paid does not directly vary (the contributions paid during employment spells are the same), retaining the remainder of the former right is equivalent to an extension of the coverage, as the total benefits capital potentially – but not necessarily *effectively* – received will always be larger in this case. In the same spirit, Landais and Spinnewijn (2019) have used the possibility, in Sweden, to add complementary insurance contracts to the public UI to assess the valuation of unemployment insurance, through both a consumption-based and a revealed-preferences approaches.

Observing the choice of the level of insurance is a unique opportunity to empirically test the presence of adverse selection in the UI market. Because of information asymmetry on the level of risk of each individual, there may be no equilibrium supporting the provision of insurance (Rothschild and Stiglitz, 1992). Only the most risky individuals are willing to insure, leading to the exit of the low-risk individuals and the collapse of the market (Akerlof, 1978). Although adverse selection is the main rationale motivating public intervention in the UI market, evidence on its empirical validity is scarce. This chapter contributes to corroborating this theoretical argument.

Observing the terms and conditions of the option right choice and looking at the observed characteristics of takers and non-takers provides some information on the determinants of the option right take-up and can be related, to a certain extent, to risk aversion, present-biased preferences, or optimism. Observed choices of the unemployed can be rationalised by hyperbolic time preferences, with a low short-term discount parameter for takers. High short-run impatience could explain both the decision to exercise the option right – high benefits today are much more valued than the risk of running out of benefits in some months – and poor labour market outcomes. Indeed, it has been shown that unemployed people, especially at low levels of wages, exhibited hyperbolic time preferences (Paserman, 2008) and that impatience associated to hyperbolic time preferences had a negative impact on job search (DellaVigna and Paserman, 2005). A further exploration of how option right choices could be used to estimate time-discounting or risk aversion is left to future work.

The main results of the empirical analysis point to a very large negative impact of the option right on employment: the duration of the following paid unemployment spell is expected to increase by about 5 months. However, the effect seems to fade out over time. Compliers do not differ in terms of total number of days on benefits on a longer time horizon, whereas they are able to collect a larger amount of total benefits on the whole period. They also work more frequently while being unemployed,³ which is often associated to part-time and temporary contracts. It suggests that the additional number of days unemployed in the short run are not used to find better-quality jobs, although I cannot have a definitive answer to this question.⁴ The heterogeneity analysis reveals that those more likely to optimise their compensation – that is to opt for the higher benefits without harming too much their labour market prospects – are in the middle of the age distribution, and highly educated. This last finding raises some questions on the efficiency of UI in helping individuals with low attachment to the labour market going back to work.

The remainder of the chapter proceeds as follows. Section 2 describes the institutional background as well as the data, Section 3 analyses the determinants of the takeup. Section 4 estimates the impact of the option right on labour market outcomes and investigates which types of unemployed people are able to optimise their compensation. Section 5 concludes.

2 Legislation and data

Institutional background – The introduction of the option right has been decided as a modification of the 2014 UI Agreement⁵ which has been unfavourable to some unemployed people. Indeed, the 2014 Agreement introduced two principles: (i) the automatic resumption of the former right, meaning that a person taking a new job before exhausting his UI right automatically benefited from the remainder of his former right when coming back to unemployment; (ii) the recharging of the right, meaning that, at the exhaustion point of his former right, he was allowed to extend his entitlements based on his last employment spells.⁶ This mechanism was a way to use any employment spell – even short ones – to extend UI entitlements and maximise the coverage duration without any interruption in payment. However, this mechanism, meant to be more favourable, turned out to have unintended consequences for workers whose last employment spell was highly paid whereas the remainder of their former

³The French UI scheme offers the possibility to job-seekers to keep on receiving part of their UI benefits when they go back to work if the earnings and working time associated to the new job are below a certain threshold.

⁴I do not observe directly the quality of the job found at the end of the unemployment spell, but only the one of the jobs taken during the unemployment spell.

⁵Amendment N°1 of March, 25th, 2015 modifying the general regulation appended to the Convention of May, 14th 2014 on unemployment insurance.

⁶This holds as long as accumulated at least 150 hours of new employment spells that have never been used to compute any UI entitlements.

right was associated to a low daily benefit (DB). Making the resumption of the former right automatic at the end of the employment spell would cause a high drop in income. This regulation has then been rectified by an amendment to the 2014 Agreement which gave the possibility to these types of workers to choose between benefiting from the remainder of their former right and then recharging with the new one or to directly jump to the new right associated to a higher daily benefit. It is this possibility to choose that has been called the option right.

More precisely, the eligibility criteria to be granted this choice are the following: (i) having a remainder from the former right; (ii) having worked at least 122 days or 610 hours since the opening of the former right (which correspond to the minimum work history to open a UI right); (iii) having a DB associated to the former right lower or equal to 20€ *or* having a DB associated to the new right at least 30% greater than the former one. The last condition is the most crucial one, and will allow to use the thresholds as part of the identification strategy.

If eligible workers choose to exercise their option right, they will directly benefit from their new right, and definitively give up on the remainder of their former right. An illustrative example can be found in Figure 1.1.

Data – I use administrative data from the *Unédic*,⁷ the organisation in charge of UI in France. It gathers all the information needed to compute UI entitlements, on the characteristics of the unemployment spell, as well as socio-demographic variables. It allows to follow the universe of registered unemployed workers with exhaustive information on their successive unemployment spells for the period of interest (October, 2014 - May, 2017). However, two important data limitations have to be noted:

First, if numerous details are available on the characteristics of the unemployment spell, much less is known on what happens to the unemployed people when they leave the unemployment roll. If they interrupt the unemployment spell for any reason – sickness, maternity leave, a new job – while keeping on being registered, we can still follow them in the database even if we do not necessarily know the reason of the interruption. It is notified that the person does not use her right nor receives benefits. If the person does not register anymore, she just disappears from the database, without necessarily providing the motive of the exit. With this information at hand, and knowing that we are interested in employment spells of at least 122 days between two unemployment spells, I define as an unemployment spell any period during which the person is registered as unemployed with interruptions lower than four months. Any interruption of at least four months, even if the person is still registered but does not consume her right and is not paid, means the end of the unemployment

⁷*Union nationale interprofessionnelle pour l'emploi dans l'industrie et le commerce*, National interprofessional union for employment in industry and trade.

spell, except if we know explicitly that it is for another reason than the return to the labour market.

Then, I define as the *paid unemployment spell duration* the addition of all the periods consumed and paid, and *full unemployment spell duration* the addition of all registered periods, paid and unpaid.⁸

Second, for people eligible to the option right and choosing not to take it, nothing is known about the new right they could have opened, unless we observe a recharging in the future. Indeed, not to exercise means they are resuming their former right. Then, the data does not record the opening of the new right they would have benefited from had they exercised, and no information is available on this potential new right. This partial information implies that I am not able to compute the ratio between the new and the former DB, and that I cannot take advantage of the 30% eligibility threshold. This limitation has important consequences as it imposes to restrain the analysis to the 20€ threshold, for which only the information on the former DB is needed. Still, knowing the value of the potential new DB for these people would have been very informative to understand the exact terms of the trade-off faced by eligible people and to better understand the determinants of the takeup.

I build a final sample made of people having begun an unemployment spell from October, 1st, 2014,⁹ who are meeting at least the first two eligibility criteria, that are not having exhausted his former right and having worked at least 122 days between two unemployment spells. Among those, I can only observe if the person is eligible but does not exercise her option right under the 20€ condition, and if she exercises the option right as part of both conditions. Table 1.1 details the sample composition, with some proxies for the takeup rate, as the true one could only be determined if we had the exact number of eligible people. I end up with a sample of 2,209,471 individuals, whose more than 200,000 are eligible under the 20€ condition.

Restricting to the 20€ condition, the takeup rate is equal to 34%, although it may not perfectly reflect the overall takeup rate in the population. This rate may seem small, given the very low amount of daily benefits associated to the former right among this population (under 20€), but reasons not to choose to exercise the option right are numerous, and can stem from both the caseworker and the unemployed person herself. Indeed, survey data from the Unedic has revealed that caseworkers were sometimes reluctant to advertise this choice and to argue in favour of opting for the new right, as they perceived it to be risky for a population of

⁸By definition of the unemployment spells, the unpaid periods within the spell are necessarily lower than four months. These periods are accounted for in the full unemployment spell duration computation only if the person keeps on being registered as unemployed and then still appears in the database.

⁹The amendment was passed on March, 29th, 2015 and started to apply from April, 1st, 2015 retrospectively on unemployment spells starting from October, 1st, 2014 onwards. It means that a person who did an automatic resumption of her former right between October, 1st, 2014 and April, 1st, 2015 could decide, from April, 1st, 2015, to exercise her option right and to switch directly to her new right.

workers generally in difficulty on the labour market. In addition, unemployed people are not necessarily aware of the existence of such a possibility, whereas the default option is not to exercise the option right. According to the law, the application for the option right has to be on the job-seeker's initiative. That is the reason why we observe an ascending trend in the number of takers over the months, with a seasonality component, as displayed on Figure 1.2.

3 The determinants of the takeup

Because of data limitations, the study focuses on a particular unemployed population with very low daily benefit (lower than or equal to 20 euro). Yet, if particular, this population is non negligible, as it accounts for 12% of the flows to compensated unemployment as part of the main UI benefit.¹⁰

This population is also particularly in difficulty on the labour market, as their benefit and therefore their previous earnings are very low, which is often associated with low qualifications and low reemployment probability. Both their weight among all people receiving UI benefits as well as their situation on the labour market justify that we pay a particular attention to their outcomes on the labour market. Then, if the results obtained cannot necessarily be extrapolated to the whole unemployed population, they are of particular policy relevance if we think the State should provide a specific support to the population further away from the labour market.

An exploration of the observable characteristics of eligible workers choosing to exercise the option right brings useful insights on the profile of the takers. If not allowing to conclude on their preferences, it can proxy for their prospects on the labour market, the way they anticipate them and their impatience and risk aversion.

The profile of the taker is quite consistent across observable variables (Table 1.2). When compared to eligible non-takers, he is, on average, younger and more frequently male, which are characteristics generally associated with riskier behaviours or lower loss aversion (Falk et al., 2015; Albert and Duffy, 2012; Gächter, Johnson and Herrmann, 2007; Holt and Laury, 2002; Jianakoplos and Bernasek, 2006). He is also more skilled and more educated, which can explain that he may anticipate a quick return to the labour market, and may be less reluctant to give up on additional days of entitlement. He also works more hours, indicating a stronger attachment to the labour market and more stable jobs, which is also in line with the higher proportion of men. The lower tenure can be explained by the fact that takers are younger and then have less experience on the labour market. Concerning the right's characteristics, the analysis is less straightforward: the initial potential benefit duration (PBD) associated to the former right is lower for takers, which can explain why they are less reluctant to give up on their former right. However, the remaining benefit duration

¹⁰As measured between January, 2014 and March 2015.

at the moment they have to decide whether exercising the option right or not is higher for takers, which may seem surprising. Both indicators taken together mean that they have less consumed of their former right, and then that they have spend less time unemployed as part of their former right, which may explain why they anticipate a quick return to the labour market and choose to exercise their option right despite a high remaining PBD. This is in line with the fact that they have experienced less unemployment spells over the whole period (October, 2014 - May, 2017).

We cannot exclude that the caseworkers also play a role in shaping such a profile: according to their beliefs about which type of people would benefit the most from the option right, they may themselves select the type of unemployed persons they will advice to exercise it, and for whom they will devote more time to explain its terms and conditions. Then, the observed differences between eligible takers and non-takers would not necessarily be due to differences in preferences or risk-aversion, but to caseworkers' beliefs.

Table 1.2 also shows the difference between the population of takers based on the 20€ condition with the whole population of interest. It indicates that, if takers exhibit large differences with eligible non-takers, they also differ to a large extent from the whole population of non-eligible non-takers.¹¹ Overall, the gap in some variables observed between takers and eligible non-takers are observed in the opposite direction between eligible non-takers and non-eligible non-takers: indeed, takers are much more frequently male as compared to eligible non-takers, but more frequently female as compared to non-eligible non-takers, as eligible non-takers are at 69% women. The pattern is less clear in terms of education and qualifications. Takers are much more numerous to have a vocational diploma, go less frequently to higher education, and are often classified as skilled employee or blue collar workers, then located in the middle of the qualification distribution. They also have less tenure, which is again in line with their young age, work less frequently full-time, and have a lower average potential benefit duration.

All in all, what stands out from Table 1.2 is that eligible people under the 20€ condition are a more fragile population, more in difficulty on the labour market as compared to the rest of the population of interest. However, among them, unemployed people who decide to exercise their option right appear to be less precarious, and to perform better on the labour market. Their higher education and qualification can explain why they may be more confident on their labour market prospects, and why they may prioritise the UI generosity over the duration of the coverage.

Table 1.3 describes the compliers characteristics, that is to say the population

¹¹This non-eligible non-taker population refers to the population of people similar to eligible workers except for the 3rd eligibility condition, that is having a former daily benefit lower than 20€. However, it may include workers eligible as part of the ratio criterion that I am not able to identify.

of takers meeting the 20€ but not the 30% condition. Among all takers below the 20€ cutoff, they exhibit a specific profile: they are much older and more frequently female, even as compared to all takers and the whole population. They also work more as employees. Differences in education are less pronounced if we compare with column (2) but compliers have much lower levels of education than all other takers, which is in line with their lower level of daily benefit and then of earnings. They also tend to be recalled more frequently than other takers and eligible non-takers. It could give them confidence in the fact that they will soon be rehired by a previous employer and that they do not need a long coverage, although the difference is small. Other takers have a higher remaining PBD from their former right, which can come from the fact that they are willing to give up more duration coverage than compliers as their financial gain is above 30%, or from the fact that the initial PBD associated to the former right is also longer.

Overall, what Table 1.3 tells us is that other takers are in a better situation on the labour market, both in terms of skills, education, working hours, as compared to compliers and even eligible non-takers. Compliers are also different from other takers: they are almost systematically women, older, employees. The fact that their new right benefit is associated to a gain lower than 30% means that they keep on having low benefits and then low earnings. This can be associated to a profile of women locked into low-paid part-time jobs,¹² whereas other takers would rather correspond to situations of workers at the beginning of their careers, then more likely to have low-paid jobs (for those meeting both criteria), or to be on an increasing wage profile (for those meeting only the 30% criterion).

Tables 1.4 and 1.5 run a multivariate analysis to examine the marginal effect of each variable, as they are potentially correlated. When only looking at predetermined characteristics, what seems to influence the most the probability to take the option right among eligibles under the 20€ criterion is the age, being female – both negatively correlated – and the level of qualification, all else being equal. The addition of characteristics associated to the right makes the marginal impact of predetermined characteristics vary: the effect of age and gender seems to be partly captured by the positive and significant impact of working hours, for example.

Age and being female are still positively correlated with the probability of being a complier conditional on being eligible under the 20€ criterion, but the effect of gender seems to matter more. The coefficients on the right's characteristics are very small, but go in the same direction as in Table 1.3. Interestingly, the number of hours worked has a strong and positive impact on the probability of being a complier. It could be explained by the fact that being a complier implies

¹²Descriptive evidence from the 1/10th sample of the UI data (FNA) computed by the *Unédic* shows that those compliers are very concentrated in the education, health and social action industry, where those precarious jobs often held by women are numerous.

having a financial gain in terms of daily benefit lower than 30%, meaning that it does not correspond to situations where the person went from a part-time job with a few number of hours to a full-time job, making the earnings jump mechanically. It rather seems that compliers are durably in part-time jobs with a medium number of hours worked and less variations in their employment spells characteristics.

To have a more complete picture of the determinants of their choice, we can also look at the exact terms of the trade-off faced by takers. Table 1.6 provides a comparison of the former and new rights takers are entitled to, to better understand their motivation.

Takers are characterised by a new DB which is, on average, more than twice the former DB, which is not surprising as their choice to exercise the option right must be motivated by a high financial gain to compensate the loss in terms of potential benefit duration. This ratio is even higher – close to 3 – for takers having a former DB lower than 20€, which is in line with the fact that, as their former DB is very low, the new one is likely to be much higher. However, compliers, who, by definition, have a ratio between new and former benefit lower than 1.3, gain only 18% in terms of level of benefits, on average.

Row 3 of Table 1.6 indicates that the new PBD the taker is entitled to is 1.35 longer than the PBD he gives up by exercising the option right. This ratio is lower in the case of those taking under the 20€ criterion. It is reasonable to think that, at these very low levels of DB, unemployed people are willing to give up a remaining PBD that, in proportion represents more of their new PBD, if it allows them to earn higher benefits. In other words, in the amount-duration trade-off, they are likely to put more weight on the amount of their DB.

For both types of takers, the total initial PBD associated to both rights is almost the same, which can also motivate their choice. Indeed, they are offered, as part of their new right, a PBD that is equal to what they were entitled to at the beginning of their former right. By definition, if they are eligible to the OR, they are in a situation where they did not exhaust their former right. Then, based on their very last experience, it makes sense for them to anticipate that they will not entirely consume their new right if they take it, and then, that they do not need a longer coverage, and that they should exercise their option right.

Last row indicates however that, by doing so, takers choose to receive benefits for a period of time that is a bit more than half what they could have got had they not exercised the option right. In the case of compliers, they lose less in terms of duration, which is partly explained by a long new right, longer than their former one and much longer than the remaining PBD (row 3). This is also consistent with their choice: as taking the option right for them is only associated with a small increase in the level of benefits, they might be more willing to take only if they are en-

sured of a long coverage despite the withdrawal of the remainder of their former right.

To put numbers into perspective, Figures 1.3, 1.4 and 1.5 show the average daily benefit and PBD among takers and non-takers, as a function of the previous level of daily benefit. UI benefits are higher for takers, as a direct consequence of the option right. The slope is slightly increasing after the 20€ threshold, as takers above this threshold necessarily need to fulfil the 30% criterion. Among takers, the PBD would be almost twice as high as their actual PBD, had they not exercised the option right, with a linear increasing pattern along the daily benefit distribution. Comparing the PBD of takers and non-takers is not straightforward, as we do not have information on the potential new right for eligible non-takers. Figure 1.5 only compares the actual PBD of takers with the remainder of the former right among non-takers, which accounts for only part of the coverage duration they are entitled to. The PBD of takers increases continuously along the previous daily benefit distribution, which indicates that there is a positive relationship between the level and the duration of benefits. It is not surprising as workers with higher benefits have also higher wages and have therefore a stronger attachment to the labour market, which is associated to more work experience and therefore a higher PBD. It could also be the case that, as the previous daily benefit increases, liquidity constraints pushing in favour of the option right are alleviated. Therefore, a higher PBD duration is needed to justify the choice to exercise the option right. Conversely, the remainder of the former right is slightly decreasing in the level of the previous benefit. It can be related to the positive relationship between unemployment duration and the level of UI benefits found in most studies. The higher the level of benefits, the longer the unemployment spell, the lower the remainder of the right when the person finds a job.

If this exploration of individual and benefits' characteristics cannot be entirely conclusive on the determinants of the takeup, it draws a consistent picture of the taker's profile and the characteristics of his right. The choice to exercise the option right can be explained by three types of reason: (i) the objective characteristics in terms of education and qualification levels, and past work experience, that are associated with better prospects on the labour market and a lower need for long UI coverage; (ii) individual characteristics generally associated with higher confidence, impatience and lower risk aversion, such as the younger age and being male (Albert and Duffy, 2012; Gächter, Johnson and Herrmann, 2007; Holt and Laury, 2002; Jianakoplos and Bernasek, 2006), which can only proxy for unobservable preferences; (iii) a trade-off between two rights much more favourable to the new one in terms of benefits generosity, although I am not able to assert that this gap in benefits is greater for takers than for eligible non-takers. A last possible factor is, as underlined earlier, the possible role of caseworkers in selecting the unemployed persons to which

they will provide more information and support in favour of exercising the option right.

4 Empirical implementation

The empirical strategy to assess the impact of the option right on labour market outcomes consists in taking advantage of the existence of a threshold defining the eligibility condition, at 20€ in the daily benefit distribution, as part of a regression discontinuity design. The idea is that people located very close to the threshold are likely to be similar, on average, in all respects but their eligibility status. Therefore, any systematic difference in their outcomes can be imputed to the fact that some are eligible to, and then may exercise the option right. This “quasi experimental design” is closely related to a local randomisation in the neighbourhood of the threshold as on which side any person will be located can be considered random, as long as some assumptions are verified.

Empirical methodology – The estimated equation is the following:

$$Y = \alpha + \tau \mathbb{1}_{DB_p \leq c} + \delta_f f(DB_p - c) + \delta_g g((DB_p - c) \mathbb{1}_{DB_p \leq 20}) \quad (1.1)$$

with Y being the outcome, such as unemployment duration in this case, $\mathbb{1}_{DB_p \leq c}$ an indicator equal to 1 when the previous daily benefit is lower or equal to c , the cutoff value and $f(\cdot)$ and $g(\cdot)$ are flexible functions that we allow to differ on each side of the cutoff.

In this setting, the RD design is qualified as “fuzzy” in the sense that the probability to exercise the option right does not jump from 1 to 0 when crossing the 20€ threshold, for two reasons: (i) all eligible persons below 20€ will not exercise it; (ii) some people above 20€ are eligible under the 30% ratio condition and will choose to exercise the option right.

It follows that:

$$Pr(OR = 1 | DB_p = 20 - \epsilon) < 1 \quad \text{and} \quad Pr(OR = 1 | DB_p = 20 + \epsilon) > 0$$

with OR being a dummy indicating if the person takes the option right.

Both imperfect takeup and the existence of other eligibility criteria take us away from the standard “sharp” RD design. Yet, the identification remains possible as long as we have a jump in the probability of treatment at the cutoff, though lower than one:

$$Pr(OR = 1 | DB_p = 20 - \epsilon) \neq Pr(OR = 1 | DB_p = 20 + \epsilon)$$

The “fuzzy” RDD exploits the discontinuity in the probability of treatment at the threshold. The treatment effect can then be recovered by dividing the jump in the relationship between the outcome and the option right treatment by the jump in the relationship between the option right treatment and the running variable – previous daily benefit – at the cutoff.

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

The identification rests upon two assumptions: (i) monotonicity, *i.e.* the fact that crossing the 20€ cutoff does not cause, at the same time, some units to be treated and others to be excluded from treatment; (ii) excludability, *i.e.* the fact that crossing the 20€ cutoff does not have an impact on Y other than through the option right. If the first assumption is verified by definition of the design of the option right eligibility rules,¹³ the second assumption cannot be ultimately tested, but some elements make it more credible, that will be further developed in the following paragraphs. Theoretically, if the window considered is not too large, there is no reason for being located right below or right above the 20€ cutoff to affect labour market outcomes other than through the eligibility to the option right. If the previous daily benefit level is linked to past employment history and then relates to future labour market performance, this effect has no reasons not to be continuous at the 20€ threshold. To make this excludability assumption more plausible, three types of tests are performed: (i) a check of the continuity of the running variable density at the cutoff to get rid of any manipulation suspicion; (ii) a check of the continuity of observed baseline covariates at the cutoff to confirm the non-selection and comparability of populations at each side of the cutoff; (iii) a check of the existence of a jump in the probability of being treated at the cutoff, a necessary first-stage to detect any effect.

Validity conditions of the RDD – One key assumption to check for the RDD to be valid is that there is no manipulation at the threshold, or strategic sorting of worker at either side of the threshold. If it was the case, we would have a selection bias that would prevent us from comparing the populations at each side of the cutoff. Theoretically, there is no reason to think that unemployed persons may have an interest in reporting lower earnings to have a DB right below the 20€ cutoff, for several reasons: (i) they would receive very low benefits, lower than what they were entitled, would they have reported their true earnings; (ii) the earnings value used to compute DB is reported on a certificate delivered by the employer to open UI rights, making the falsification very unlikely; (iii) manipulating their earnings value

¹³Crossing the 20€ cutoff leads for some units not to be eligible anymore, and for others to stay eligible if they also meet the 30% criterion.

in anticipation of a future option right would require very strong foresighting abilities, as well as a precise knowledge of UI legislation.¹⁴

Although the manipulation scenario seems little plausible, I still perform a McCrary test (McCrary, 2008) to check that the density of the former DB distribution is smooth at the 20€ cutoff (Figure 1.6).

Some regularities in the level of earnings or in the UI parameters tend to create small spikes at different points of the distributions, without threatening the validity of the RDD, as these spikes are not in the neighbourhood of the cutoff. For example, we observe a big jump in density around 32€, at this corresponds to the level of DB for a person who has worked full-time at the minimum wage.

As there is no precise sorting at the threshold, RDD is considered “as good as randomisation” in the neighbourhood of the threshold.

To provide an additional guarantee that the RD design is similar to a local randomisation close to the threshold, we can compare individual characteristics of unemployed people earning a daily benefit of 20€ or less, with those of unemployed people earning more than 20€. I consider a restricted 8€ window around the cutoff. To avoid any bias due to the selection of individuals as soon as the option right is introduced,¹⁵ which would change the composition of the pool of unemployed people around the 20€ cutoff, I compute these descriptive statistics on a sample of unemployed people having entered unemployment between January, 1st, 2013 and December, 31st, 2013, receiving the standard benefits and having a remainder from a former right (Table 1.7).

If some differences are significant, they are of small magnitude, and do not draw a clear pattern in terms of education and qualification. People with daily benefit between 16€ and 20€ are, on average, older and more frequently female, but the gap is not very large. To make sure it does not threaten the validity of the RD design, I check that there is no discontinuous change in the distribution of those variables at the 20€ threshold, and I add them as covariates in the regression to verify the estimated coefficient is left unchanged.

If I chose to focus on one eligibility criterion for data limitation issues, I could still observe which eligibility criterion was binding for eligible workers who chose to exercise the option right. The distribution across eligibility conditions for takers shows that the most decisive criterion is having a new daily benefit greater than the former one by at least 30%. Indeed, 97.5% of takers having a previous DB lower than

¹⁴It would require unemployed people to be aware of the existence of the option right, to anticipate they will find a job and loose it again and then that they might be eligible, and to know very precisely the rules to compute DB from earnings, whereas some parameters are updated every semester.

¹⁵This type of selection would still be a crucial issue for the estimation of the causal impact of the option right on labour market outcomes, as this estimation requires that unemployed people are locally randomised around the threshold, and then that there is no manipulation or selection from the unemployed people. For this reason, it would be carefully checked as part of the conditions for the RDD to be valid.

20€ also fulfil the ratio criterion, and 92.2% of all takers fulfil the ratio criterion (Table 1.8).

This distribution emphasises the fact that having information on both criteria would have helped to capture the option right impact in a more exhaustive way, and that the population of compliers is very specific: it is made of people eligible under the 20€ criterion but not under the 30% criterion. Indeed, the share of people eligible based on the 30% criterion has no reason not to be continuous at the 20€ threshold,¹⁶ meaning that the compliers have a financial gain when exercising the option right necessarily lower than 30%, translating into at most 6€ daily. It implies that compliers are willing to give up on a significant additional coverage duration (336 days on average) for a limited increase in income, demonstrating either particular preferences, very tight financial constraints, or very optimistic anticipations on the return to the labour market.

Table 1.8 also provides an additional rationale for checking that we observe a discontinuity in the takeup rate at the 20 euro threshold – a first-stage relationship necessary to measure the local impact of the option right through a RD design.

Empirically, I estimate Equation 1.1 using either a local linear regression or higher order polynomials. What is crucial, in particular for the local linear regression, is to carefully choose the bandwidth so that it is neither too large, which would create a bias if the relationship between Y and X is not linear within the window, nor too small, which would reduce the precision by lowering the number of observations. To demonstrate the robustness of the effect, results will be shown for a range of different polynomial orders. Table 1.9 and Figure 1.7 reproduce the main results making the size of the bandwidth vary (choosing, in particular, between 0.5 and 3 times the optimal bandwidth value).

Equation 1.1 shows the reduced form of two equations capturing the first-stage relationship between the previous daily benefit level and the option right takeup (Eq. 1.2) and the second-stage relationship between the option right takeup and labour market outcomes (Eq. 1.3).

$$OR = \alpha_f + \tau_f \mathbb{1}_{DB_p \leq 20} + \beta_{f_f} f_f(DB_p - c) + \beta_{g_f} g_f((DB_p - c) \mathbb{1}_{DB_p \leq 20}) + \mu_f \quad (1.2)$$

$$Y = \alpha_s + \tau_s OR + \beta_{f_s} f_s(DB_p - c) + \beta_{g_s} g_s((DB_p - c) OR) + \mu_s \quad (1.3)$$

¹⁶I cannot ultimately test this as I cannot identify the eligible population but the continuity assumption at least holds for the percentage of takers under the 30% criterion. This alleviates the suspicion that takeup could be discontinuous at the threshold also because of people already eligible under the 30% criterion, who exercise the option right, but who would not have exercised it if they would have been above the threshold because of higher salience of the option right possibility under the threshold.

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

Table 1.10 shows that being located at the right hand side of the cutoff makes the probability of taking the option right significantly decrease, by 3 to 7 percentage points, depending on the specification. Although the effect is not very strong, the estimate is highly significant, and the jump in the probability is clear, as depicted on Figure 1.8. The weak first-stage regression could raise some precision issues. However, as the sample size is large, we can be confident in having precisely estimated treatment effects. Table 1.10 provides F-statistics demonstrating the reliability of the first-stage estimation for all specifications.

Graphically (Figure 1.8), we observe a drop of about four percentage points, from an initial probability of around 19%. It means that the drop translates into a 21% increase in the probability of taking the option right when crossing the 20€ cutoff. The decrease around 18€ may be explained by the presence of numerous subsidised jobs paid the minimum wage for a 20-hour weekly working time,¹⁷ translating into a daily benefit around 18. These types of jobs are generally offered to long-term unemployed persons who have been away from the labour market for a long time, and who experienced great difficulties to find a job. It means that, if they could be eligible for the option right, the caseworkers are very unlikely to advice them to take it, and themselves may be reluctant to give up on additional compensation days, given their poor labour market prospects.

To fully conclude that the difference in outcomes we observe between populations at each side of the threshold can be imputed to the difference in option right takeup, we need to rule out the influence of other variables at the threshold. Figures 1.9 to 1.11 do not depict any clear jump in the distribution of covariates at the threshold. Figure 1.12 shows graphically that the differences are not significant at the 1% level in most cases (although they can be significant at lower levels as reported in Table 1.11, but with very small magnitude). In addition, these estimates have to be considered in the light of the whole distribution pattern, with numerous bumps and lumps at other values of the covariates. It would not be surprising to observe that age declines with the value of daily benefit around the 20€ threshold, but there is no theoretical reason – linked to a legislative feature for example – to think that age should change discontinuously at this point. As already underlined in the analysis of the McCrary (2008) test, strategic sorting of people on either side of the threshold is very unlikely as they would need to anticipate, when opening their UI right, that they might exercise the option right in the future if they work for a better paid job (in some cases even before the option right has been implemented), and, in order to be eligible, they should be willing to falsify their work certificate to receive lower benefits immediately. Then, even though several arguments alleviate the concern of

¹⁷20 hours a week is the minimum working time for those types of contracts.

discontinuous covariates at the relevant cutoff, I include them in the regressions as an additional check, and the results are close.

5 Results

5.1 Labour market impact of the option right

Option right impact on unemployment spell duration – Before examining the effect of the option right on labour market outcomes, we first have to determine what are the most relevant outcomes. A first natural indicator to look at is the duration of the unemployment spell. Previous literature has shown that the elasticities of unemployment duration with respect to the level of unemployment benefits as well as the potential benefit duration were positive (Schmieder and Von Wachter (2016) for a review). Then, in this setting, we expect the effect of the option right to go in two opposite directions: the effect of receiving higher benefits for a shorter potential duration is *a priori* unclear.

In practice, the unemployment spell duration can be defined in several ways, considering the data at hand. A first measure – the paid unemployment spell duration – corresponds to the addition of all subperiods during which benefits were paid, within the same spell.¹⁸ The full unemployment spell duration corresponds to all registered subperiods within the same spell, including the unpaid ones, which, by definition of the spell, last less than 4 months.

However, restricting the analysis to the unemployment spell duration may sometimes not be relevant as, if the person keeps going back and forth on the labour market, the unemployment spell may be short without necessarily corresponding to a stable exit to the labour market.¹⁹ That is why this measure will be presented with other complementary outcome variables meant to capture a medium to long-term effect.

Tables 1.12 and 1.13 show, making the polynomial order vary, that taking the option right has a strong and significant effect on unemployment spell duration - both paid and unpaid. If we focus on the quadratic specification, without any controls, of Table 1.12, the option right leads to an increase in the paid unemployment duration of about 161 days. The effect is markedly large, and the option right seems to have

¹⁸An unemployment spell has been defined in Section 2 as any period of registered unemployment with interruptions shorter than 4 months. Then, the paid unemployment spell duration refers to the addition of registered and paid subperiods without counting the time elapsed during the interruptions.

¹⁹Yet, as the unemployment spell are defined so that they are separated by at least 4 months of interruption, we can be pretty confident in the fact that the end of an unemployment spell corresponds to a job of at least several months.

a very detrimental impact on the employment outcomes of a population already in a precarious situation. In particular, if we consider that the average duration of a spell at the cutoff is around 100 days, the effect is equivalent to multiplying the spell duration by 2.6.²⁰ At first sight, evidence would lead to the conclusion that letting the unemployed choose the terms and conditions of their compensation is a very inefficient way of ensuring satisfactory coverage and a quick return to the labour market.

The strong and positive effect on unemployment duration is confirmed by Figures 1.13 and 1.14. The addition of covariates does not change the order of magnitude of the results for any specification, which is reassuring on the validity of the RDD. The results are also very consistent across the local linear regression and the higher-order polynomials.

Placebo tests are performed at different cutoff values randomly chosen along the distribution of daily benefit (Table 1.14), and display no significant estimates.

These different findings indicate that benefiting from a shorter potential duration with a higher level of benefits makes the duration of the unemployment spell increase. In this specific context, the elasticity of unemployment duration to the level of unemployment benefits outweighs the elasticity of unemployment duration with respect to the potential benefit duration. This result is in line with the literature, as elasticities of non employment duration or benefit duration with respect to the benefit level are usually higher than the same elasticities measured with respect to potential benefit duration (see Schmieder and Von Wachter (2016) for a recent review).

The average gain in replacement rate²¹ is equal to 10.2 percentage points, which is equivalent to a 18.5% increase. As indicated by row 4 of Table 1.6, the average loss in PBD for compliers amounts to 35.4%. If we take, based on average values of paid unemployment duration elasticities from a panel of recent studies in Europe (Schmieder and Von Wachter, 2016), an elasticity of unemployment duration with respect to the replacement rate of 1 (noted $\epsilon_{B,RR}$) and an elasticity of unemployment duration with respect to potential benefit duration of 0.4 (noted $\epsilon_{B,PBD}$), we can carry out a simple computation exercise:

$$\begin{aligned} \Delta B &= \Delta B|PBD_{fixed} + \Delta B|RR_{fixed} \\ \Delta B &= \frac{\Delta RR}{RR} \times \epsilon_{B,RR} \times \overline{Duration} + \frac{\Delta PBD}{PBD} \times \epsilon_{B,PBD} \times \overline{Duration} \\ \Delta B &= .185 \times 1 \times 95.65 - .354 \times 0.4 \times 95.65 \\ \Delta B &= 4.15 \end{aligned}$$

²⁰It should be noted that the counterfactual average duration of the spell among compliers in the absence of the option right may be different.

²¹Taken as the difference between $RR_{previous} = \frac{Previous\ DB}{New\ Earnings}$ with $RR_{new} = \frac{New\ DB}{New\ Earnings}$.

with ΔB being the unemployment duration response, $\overline{Duration}$ the average spell duration of unemployed people having a previous daily benefit between 20€ and 22€, $\frac{\Delta RR}{RR}$ the change in replacement rate and $\frac{\Delta PBD}{PBD}$ the change in PBD. The net effect on paid unemployment duration is positive, as confirmed by my results. In terms of magnitude, the predicted increase is of 4.34%, which translates into 4.15 days on average at the threshold. This figure is much lower than my LATE estimate of 161 days for specification (2), emphasising the fact that the population of compliers is likely to be specific in terms of elasticity and time preferences.

Indeed, one should keep in mind that the RD estimate is similar to a local average treatment effect (LATE), weighted by the pre-assignment probability of being located just below the threshold. Then, it is valid for this specific threshold, and informs on the behaviour of this peculiar population of compliers around the 20€ threshold. The fact that those workers are more sensitive to the level of benefits rather than to the potential benefit duration can be explained by their profile: (i) they have so low benefit that they may face sizeable liquidity constraints, then any increase in their income may have a substantial effect; (ii) even when choosing the shortest option, they are still entitled to a long coverage in absolute terms (560 days on average for compliers and 456 for takers below the 20€ threshold); (iii) they are used to go back and forth on the labour market, alternating very short employment and unemployment spells. Then, they are used not to exhaust their right and to use their frequent employment spells to extend it, which can explain why they put less weight on the PBD when optimising their search behaviour.

Longer-term impact on the professional path – Looking only at this first evidence on unemployment duration would induce to conclude that the option right has a negative impact on employment. Its initial goal was to prevent workers from experiencing very large drops in income from employment to unemployment. The direct impact is an increase in the unemployment spell duration that immediately follows the exercise of the option right. However, the ultimate impact on worker's welfare depends on whether this increase in the duration of the subsequent unemployment is driven by the fact that the person can afford to take more time to find a job, and that this job will be more stable and of better quality. If job quality cannot be measured directly with the available data, as we have no information on the job found when the unemployed person leaves the rolls, we can still try to capture a longer-run effect, by measuring the total number of days spent unemployed after the exercise of the option right. If the option right was associated to an increase in job quality, we would observe that, despite a longer immediate unemployment spell, people exercising the option right would be less unemployed over the whole subsequent period. Then, I define two new outcome variables: the total number of days unemployed over the subsequent period, and the total number of days on UI

benefits over the subsequent period.

Figures 1.17 and 1.18 exhibit a drop in the total number of days registered as unemployed over the subsequent period, but not in the total number of days on UI benefits over the subsequent period. Consequently, Tables 1.15 and 1.16 show that the effect on the total number of days registered as unemployed is significant for all specifications, whereas it is never significant for the total number of days on benefits. The estimation is also run on a sample restricted to people beginning a new unemployment spell in the twelve months following the introduction of the option right to avoid right-censoring issues²² (which are less of a problem for short-term outcome variables) as a robustness check. Results are similar, though less significant, potentially because of the smaller sample size (Table 1.17).

Several interpretations of this difference between the effect on the total number of days on benefits and the total number of days registered as unemployed can be put forward: being registered as unemployed without receiving benefits generally corresponds either to (i) legal waiting period before the start of the entitlement, (ii) to periods where the unemployed receives assistance benefits, or (iii) to periods during which the person works while registered as unemployed, because she is still looking for another job or because her contract is short. I examine the plausibility of each motive in the following paragraphs.

The first motive is regulated by some legal criteria, and has no reason to differ from one group to the other.²³

Distinguishing between the second and the third explanation is a key issue. The second scenario would mean that the higher number of days registered as unemployed but not paid corresponds to unemployed people at the exhaustion point of their right, staying registered to keep on benefiting from the support of the caseworker. In particular, to receive assistance benefits or the minimum income, it is required to be registered as unemployed. This would be compatible with the fact that takers have mechanically shorter potential benefit duration than non-takers. It would lead to the conclusion that evidence in Tables 1.15 and 1.16 and Figure 1.17 supports the hypothesis that the option right slows down the return to work, even in the long run, and forces takers to receive assistance benefits as they are not entitled to UI benefits

²²In any case, right-censoring would matter if it would not be random. Figure 1.19 shows that the distribution of the starting date of the spell is rather uniform across groups of workers with former benefit above and below the 20€ cutoff. The difference in entry date, if any, would rather lead to underestimate the results in terms of unemployment duration as those earning more than 20€ enter more frequently at the beginning of the period.

²³The waiting period includes a mandatory minimum 7 days period, that can be extended according to the level of the severance pay. In the case of recharging of the right, however, the recharging occurs generally the day after the exhaustion of the former right, without waiting period. As people exercising the option right give up on the remainder of their former right, they cannot, by definition, recharge the right they have given up (even though they can still recharge their rights in the future). It means that people choosing not to exercise the option right are more likely to recharge, and then to reduce the number of days registered but not paid as part of the waiting period. However, if this mechanism can play a role, it is likely to be marginal.

anymore. If the third scenario prevails, it implies that, in the long run, takers work slightly more, although it can be under temporary and part-time contracts. My data does not contain, at the moment, periods where the unemployed person is under assistance benefits, i.e. these days do not enter the computation of registered but not paid periods. Still, Table 1.18 indicates that compliers are more likely to reach the exhaustion point of their benefits by about 20 to 55 percentage points depending on the specification, from a baseline of around 4% (Figure 1.20). Then, the difference we observe in terms of number of days registered could be partly explained by the fact that takers do run out of benefits more frequently.

The main difference in the number of days registered but not paid may more plausibly come from the last motive. Anecdotal evidence has revealed that caseworkers were advising unemployed persons who found a job under a fixed-term contract to keep on registering at the job center to avoid starting again the whole procedure in case they go back to unemployment. It is also particularly recommended when the job is temporary, part-time or corresponds to qualifications that are not perfectly matching those of the worker, so that the person can keep on looking for a better job and benefiting from support and guidance from the caseworkers. Therefore, if we think that the higher number of days registered but not paid corresponds to trial periods at the beginning of an open-ended contract, when the worker is not sure yet to be permanently hired, we may consider the option right acts as a stepping-stone to more stable job in the long run. Another reason why people would stay registered as unemployed while working is that they earn a wage low enough to be entitled to receive complementary benefits from UI. The benefits received are lower than a full month of complete compensation, and the person would then appear as on benefits for some days in the month, and registered but not receiving benefits for the rest of the month. In other words, these periods during which the person is registered without receiving benefits generally corresponds to employment spells under unstable, temporary and/or part-time contracts. For example, if, in a given month, the person is employed under a part-time contract and is entitled to receive one third of the monthly benefits she would receive with no job at all, she will appear as registered on benefits for 10 days in the month, and registered without benefits for the other 20 days. However, if the person has no job at all, she will appear as registered on benefits for all the 30 days. This scenario is then compatible with takers having a similar total number of days on UI benefits with higher total number of days registered without benefits at the same time. All in all, the evidence suggests that in the medium to long run, the option right does not impact negatively the professional path in terms of unemployment probability - though it may encourage temporary, unstable and part-time contracts.

Next subsection investigates more deeply whether the difference in terms of days registered as unemployed can be explained by takers having more frequent small

employment spells while keeping on being registered.

Impact on partial employment – Unemployment spells have been defined as a period of unemployment without any significant interruption leading to a stable return to the labour market. Unemployment spells, though, may include small employment periods when the person works and may also be registered, and even receiving partial benefits, while accumulating new entitlements. In the main outcome of interest – paid unemployment spell duration – those employment periods are not counted, but are not lasting enough to define the end of the unemployment spell. To have a sense of how erratic was the unemployment spell, I compare the total number of days receiving benefits within the spell with the total length of the spell, including those periods when the person was registered but not receiving benefits, or not even registered, when she potentially worked. If the ratio of those two measures is close to 1, it means that there was almost no interruption in the paid unemployment spell, and that the unemployed person has arguably found a stable job at the end without experiencing small employment periods. On the contrary, if the ratio is close to 0, it means that the paid unemployment spell has been spread out on a long period of time, interrupted by many small contracts. Table 1.19 shows a small and insignificant difference in most specifications, although always negative, which means that takers would experience more interruptions within their spell.

Another insightful outcome is whether the unemployed person has worked while being registered as unemployed. Under some conditions, unemployed people are able to keep on earning benefits while working for a small number of hours or a small wage. The benefits they would earn would be lower than if they would be working. They would be entitled to receive benefits as long as the sum of their labour income and their UI benefits would not exceed their previous earnings. In any case, if an unemployed person works while being registered as unemployed, I am able to track his employment spells and to have information on the number of hours worked and total earnings, no matter whether he kept on earning benefits during this period. These types of employment spells typically include short-term and part-time contracts rather than a stable jobs.²⁴ In the remainder of the chapter, I will refer to these periods of employment while registered as unemployed as *partial employment*.

I observe that people exercising the option right have a probability of experiencing partial employment during the unemployment spell higher by about 55 percentage points (Table 1.21). This finding is consistent with the fact that the impact I measure on the unemployment duration is much higher in terms of number of days registered than in terms of number of days receiving benefits, both in the short and long term.

The fact that compliers do earn more income from labour during the spell

²⁴Survey evidence (Unédic, 2018b) shows that 70% of individuals working while being on UI benefits are under a fixed-term contract.

(Table 1.22), conditional on working at least one hour during the spell, is explained by a higher number of hours worked (Table 1.23), and not by a higher hourly wage (Table 1.24). Although they resort more often to partial employment during the UI spell, the type of job they find are not of better quality, if we assume that wage is a good proxy for job quality.²⁵

This evidence is again confirmed when examining the probability of partial employment at different time horizons (Tables 1.25 and 1.26): compliers do have a higher probability to work 3 months and 6 months after exercising the option right, whereas the effect is not significant on a longer horizon.

Taken together, these findings suggest that people exercising the option right are not only on benefits for a longer time in the short run, but also experience a more unstable path, and alternate more frequently small employment and unemployment spells. It could also be the case that people used to be partially employed are less worry about having a shorter unemployment right and focus more on the generosity of UI benefits, as they know they will find short-term employment contracts to extend the length of their UI entitlements.²⁶

5.2 The optimisation ability of the unemployed

If the impact of the option right on unemployment duration is markedly negative in the short run, and less clear in the long run, the conclusion in terms of unemployed people optimisation ability is not straightforward. Assuming that the the welfare objective is to maximise the total capital of benefits earned, we can look at the effect of the option right on total benefits. The theoretical benefits capital is always higher in case of non-takeup, as it is equal to the theoretical capital in case of takeup, plus the capital from the remainder of the former right. However, since the unemployed person will not necessarily use his full potential benefit period if he finds a job before the end of his entitlement, it is likely that the actual benefit capital received during the unemployment spell does not correspond to the theoretical capital. Indeed, Table 1.27 shows that exercising the option right makes the total amount of benefits received over the subsequent spell increase. The difference is lower in the long run (Table 1.28) though substantial and significant, meaning that those not taking the option right catch up to some extent. Yet, maximising the amount of benefits received does not necessarily makes the worker better-off, as this can result in more

²⁵It is important to keep in mind that results on the intensive margin, that is to say on the number of hours or the level of earnings, are conditional on having worked during the spell. As the option right has itself an effect on the probability to work, we cannot rule out the hypothesis that the difference we observe in terms of number of hours or earnings is not a direct effect of the option right, but a composition effect arising from the fact that people working during the spell right below or right above the 20€ cutoff are different, precisely because of the option right.

²⁶A descriptive analysis of individuals working while being on UI benefits (Unédic, 2016) reveals that their profile is quite consistent with the one of compliers: more frequently female, in the personal care sector.

days of unemployment.

To assess the extent to which workers are able to make the right insurance decision, one important parameter to take into account is whether they reach the exhaustion point of their entitlements. Indeed, if by exercising the option right they loose in terms of benefit duration, they could still be entitled to a long coverage. If they are not at risk of running out of benefits, taking the option right would simply mean having higher benefits, and would be a risk-less way to maximise the amount of benefits collected. Table 1.20 shows that indeed, only a small share of takers, 17%, do exhaust their UI entitlement. However, the strong effect of the option right on the probability to exhaust benefits (Table 1.20) suggests that at least part of the unemployed choosing the option right because they anticipate a quick return to the labour market fail in their prediction.

To better understand if workers are able to optimise their compensation, I perform a heterogeneity analysis, intersecting the propensity to take the option right with the outcome in terms of unemployment spell duration. The idea is to analyse which subpopulations are more likely to take the option right, and whether they are right in doing so. Using age, gender and education categories and comparing the first and second-stage for each category, I observe that younger unemployed people have a higher jump in the probability of taking the option right at the 20€ threshold (Tables 1.29 and 1.30), which is compatible with the less stable professional status that is often experienced in the early years of the career. Very young workers (under 35 years-old) are not perfectly optimising, as both their takeup and the negative impact on unemployment duration are of high magnitude. Older workers have insignificant first and second-stage estimates. Some of them do exercise the option right but they do not seem to resort to it under the 20€ criterion. Similarly, Tables 1.31 and 1.32 point to a high takeup jump for people with a higher level of education, with a limited negative impact. In particular, when comparing the highest two categories, we observe that people with higher education display a significant and substantial jump in takeup while the impact on unemployment duration is not significant, whereas it is negative and of higher magnitude for people who only completed high school despite a higher first-stage. In line with this result on education, we observe that unskilled employees and workers often exercise the option right whereas the impact is markedly negative for them (Tables 1.33 and 1.34). On the reverse, skilled workers is the category where the jump in takeup is the highest with a limited and insignificant effect on unemployment duration. It means that, although they are not at the top of the skill distribution, their better qualifications do play a role in protecting them from the adverse impact of the option right on labour market performance. The option right is also more detrimental to females, as they exhibit a higher discontinuity in takeup, coupled with a stronger negative impact on labour market outcomes (Tables 1.35 and 1.36).

All in all, it seems that male workers in the middle and at the top of the age and skills distribution and highly educated are the ones taking the most advantage of the option right, with a limited impact on the subsequent unemployment spell duration. One interpretation is that they are better at predicting their reemployment probability, and that they have at the same time better objective labour market prospects. More educated workers are more likely to take the risk of exercising the option right without making their unemployment spell duration increase too much, as they are better equipped to find a job rapidly and they may have a more stable professional status.

5.3 Adverse selection and moral hazard

The choice feature of the option right is a unique opportunity to try to measure the extent of adverse selection in the UI market. Indeed, the main rationale for the implementation of a UI mandated at the national level comes from the Rothschild-Stiglitz demonstration (Rothschild and Stiglitz, 1992) that, because of heterogeneity in risk types and asymmetry of information, there is no equilibrium supporting the provision of insurance. Empirically, most of the papers have taken this result as granted without questioning the actual presence of adverse selection, and rather focusing on moral hazard. In this subsection, I try to disentangle the adverse selection from the moral hazard by looking at both the predicted and realised unemployment duration. Unemployment duration is predicted using a sample of similar job-seekers during the two years preceding the implementation of the option right. I use a large set of covariates associated to the worker and to his last employer to capture as accurately as possible all the information that is available to the worker when he has to decide whether to take or not the option right. An alternative group of workers is used for the prediction in Tables 1.41 to 1.44.²⁷

Table 1.37 shows that the predicted unemployment duration is higher for eligible non-takers than for takers. The fifteen-day difference, representing a 10% increase relative to the predicted unemployment duration of takers, is indicative of significant adverse selection. Job-seekers with higher predicted unemployment duration are more likely to choose the longest UI coverage.

This is confirmed by Tables 1.38 and 1.39 where the population is divided into quintiles of predicted unemployment duration. We observe that the takeup rate is a decreasing function of the predicted unemployment duration. It means that the higher the unemployment risk, the more likely it is that the worker will choose the longest coverage. Consistently, Table 1.39 shows that the jump in takeup at

²⁷Results in Tables 1.41 to 1.44 are computed based on the sample of job-seekers above the 20€ threshold (and below 40€), that is the control group. Although it has the advantage of using workers unemployed during the same time period as our sample of interest, the main limitation is that this group also includes eligibles and takers, and that their unemployment duration may be affected by the treatment.

the threshold is the highest in the lowest quintiles. This positive correlation test with the *predicted* unemployment duration, although it may not capture the role of unobservables, is indicative of adverse selection.

We know from previous section that the moral hazard response to the option right is substantial, as measured by the increase in the unemployment spell duration at the eligibility threshold. I try to go further by analysing the response on the different predicted unemployment duration quintiles (Table 1.40). It shows that, as the predicted unemployment duration increases, the negative impact of the option right increases as well. It means that those who are initially predicted to stay unemployed longer will suffer even more from choosing a shorter coverage with higher benefits. These results suggest that the policy consisting in giving a certain degree of flexibility in the UI choice is widening inequalities in terms of employment probability.

6 Concluding remarks

This chapter takes advantage of an uncommon setting where unemployed people can choose the conditions of their compensation, based on their expectations about their labour market outcomes and on their preferences. It is *a priori* unclear what makes the worker better-off between higher benefits for a shorter duration or longer PBD associated to a lower benefit. This chapter precisely looks at the combined effect of a variation in both parameters on labour market outcomes. Opting for higher benefits is predicted to increase dramatically the length of the subsequent unemployment spell. This effect is particularly worrying if we consider that the targeted population is already at risk on the labour market, alternating unemployment and employment spells. However, both the choice for higher benefits and the positive short-run impact on unemployment duration could be rationalised by severe liquidity constraints, especially at this level of earnings. Additional benefits could be used to loosen liquidity constraints and subsidise job search for a better-quality job.

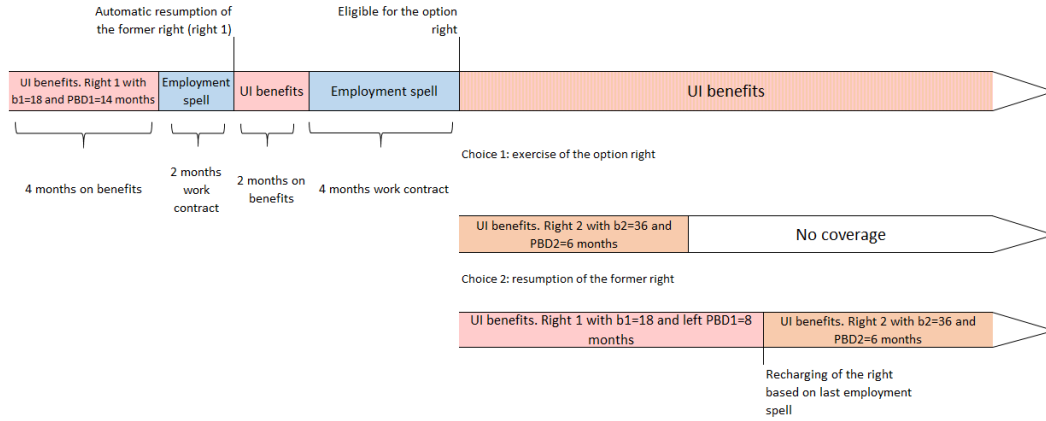
When decomposing the takeup rate and labour market impact by subpopulations, we observe that the female and youngest population has a higher probability to exercise the option right whereas the negative impact for them is particularly large. On the contrary, the highly educated unemployed have also a high probability to exercise the option right while managing to contain the negative impact. This pattern indicates that, for at least part of the takers, their choice is not only driven by liquidity constraints.

However, the option right effect is no longer significant when considering a longer time-horizon. The unemployed choosing to exercise their option right do not experience more days on UI benefits over the whole following period, but they resort more to short-term and part-time work contracts. Overall, compliers are able to receive a higher amount of total benefits, and this difference cannot be entirely

attributed to additional days spent on benefits. The main mechanism explaining both the divergence between the short and long run effects and between the impact on paid and registered unemployment is the more regular use of partial employment. Determining whether experiencing numerous short employment spells has a positive impact on the long run professional path is an open question. However, findings on the duration of unemployment and the use of partial employment taken together indicate that the additional days of unemployment among compliers are not necessarily used to improve the quality of the job.

Tables and Figures

Figure 1.1: Option right trade-off



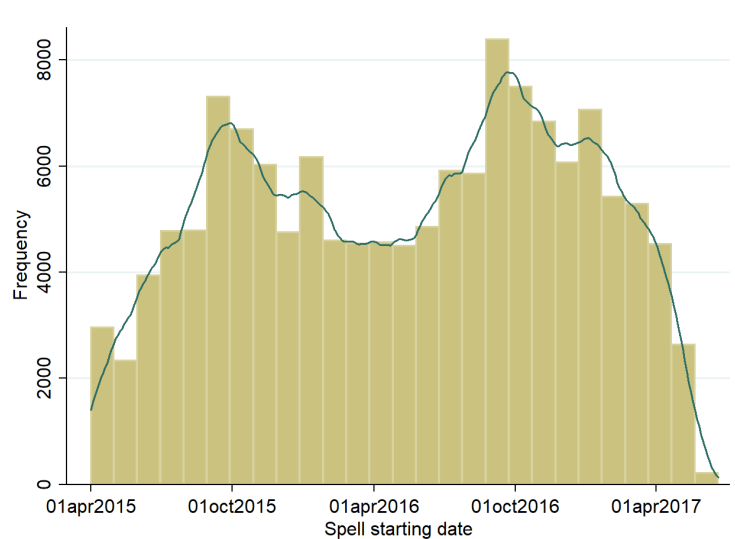
NOTE: This diagram illustrates the different possibilities faced by a worker eligible to the option right. Right 1 refers to the first right he has opened and not entirely consumed. At the end of the first employment spell, the individual has not worked enough to be eligible to the option right, he then automatically resumes right 1. At the end of the second employment spell, he has accumulated 6 months of employment. As the daily benefit associated to right 1 is lower than 20€, he is entitled to exercise the option right. If he does so, he will benefit from a new 6-month right based on his last employment spells. If he does not exercise his option right, he will benefit from the remainder of right 1 (duration = $14 - 4 - 2 = 8$ months). At the end of right 1, he will be able to claim right 2.

Table 1.1: Sample composition

	20€ condition	30% ratio condition	Total
Similar	–	–	2,209,471
Eligible non-takers	210,116	?	210,116
Takers	71,525	128,441	139,254

NOTE: This tables details the composition of the sample I am working on, classified by eligibility criterion. Similar workers refer to workers similar to eligible workers, as they have opened a UI in the past they did not entirely consumed, but with a daily benefit higher than 20€. Eligible non-takers refer to eligible workers who chose not to take the option right. I can only identify them under the 20€ condition, as I have no information on the new potential right for non-takers. Takers refer to the ones choosing the option right, that is choosing the higher level of benefits. We count every unemployment spell meeting the eligibility condition, whereas for takers, we count one taker per right, as the option right can only be exercised at the beginning of the right (exercising it, by definition, leads to the opening of a new right, then we cannot observe a person exercising the option right at the beginning of a spell within a right). The addition of takers meeting the 20€ condition with takers meeting the 30% ratio condition is greater than the totality of takers as both conditions are not mutually exclusive.

Figure 1.2: Number of people exercising the option right over time



SOURCE: UI data (FNA).

NOTE: This graph displays the distribution of the number of people taking the option right over time, since its implementation in April, 2015. We see an overall increase overtime, with a seasonal pattern.

Table 1.2: Individual characteristics of takers and non-takers based on the 20€ criterion

	Eligibles - 20€ criterion		All	Difference		
	Takers	Non takers		(3) - (2)	(3) - (1)	(2) - (1)
<i>Demographics</i>						
Age	30.32	35.25	37.08	1.828*** (0.028)	6.595*** (0.044)	4.932*** (0.057)
Female	0.57	0.69	0.46	-0.234*** (0.001)	-0.094*** (0.002)	0.119*** (0.002)
<i>Qualification</i>						
Executive	0.02	0.00	0.07	0.067*** (0.001)	0.049*** (0.001)	-0.012*** (0.001)
Intermediate occupation	0.02	0.01	0.03	0.024*** (0.001)	0.003*** (0.001)	-0.019*** (0.001)
Skilled employee	0.58	0.51	0.52	0.003 (0.002)	-0.065*** (0.003)	-0.067*** (0.003)
Skilled blue collar worker	0.27	0.22	0.25	0.024*** (0.002)	-0.022*** (0.002)	-0.044*** (0.003)
Unskilled employee	0.09	0.22	0.10	-0.124*** (0.001)	0.018*** (0.002)	0.131*** (0.002)
Unskilled blue collar worker	0.02	0.03	0.04	0.006*** (0.001)	0.017*** (0.001)	0.012*** (0.001)
<i>Level of education</i>						
No education	0.02	0.04	0.03	-0.011*** (0.000)	0.008*** (0.001)	0.018*** (0.001)
Elementary school completed	0.01	0.02	0.01	-0.010*** (0.000)	0.000 (0.000)	0.009*** (0.001)
6 th to 8 th grade	0.01	0.02	0.02	-0.008*** (0.000)	0.003*** (0.001)	0.010*** (0.001)
Middle school completed	0.04	0.06	0.04	-0.021*** (0.001)	-0.000 (0.001)	0.019*** (0.001)
10 th to 11 th grade	0.01	0.02	0.01	-0.003*** (0.000)	0.002*** (0.000)	0.005*** (0.001)
Vocational diploma	0.47	0.41	0.37	-0.038*** (0.001)	-0.098*** (0.002)	-0.063*** (0.002)
High school diploma - Baccalaureate	0.23	0.25	0.25	-0.001 (0.001)	0.025*** (0.002)	0.026*** (0.002)
Two-year Higher education degree	0.11	0.09	0.14	0.046*** (0.001)	0.024*** (0.001)	-0.018*** (0.001)
Three to four-year Higher education degree	0.06	0.06	0.07	0.015*** (0.001)	0.015*** (0.001)	0.001 (0.001)
Five-year and more Higher education degree	0.04	0.03	0.06	0.030*** (0.001)	0.021*** (0.001)	-0.006*** (0.001)
Rank among contracts with the same employer	1.13	1.10	1.20	0.103*** (0.002)	0.056*** (0.003)	-0.034*** (0.002)
<i>Rights' characteristics</i>						
Tenure	393.07	685.69	917.05	231.359*** (6.007)	505.497*** (12.585)	292.619*** (9.407)
Part-time coefficient	0.69	0.65	0.96	0.312*** (0.000)	0.239*** (0.001)	-0.044*** (0.002)
Number of unemployment spells over the period	1.37	2.75	2.48	-0.274*** (0.006)	1.132*** (0.009)	1.381*** (0.011)
PBD of the former right	468.78	550.85	564.43	13.573*** (0.648)	94.381*** (1.653)	82.072*** (1.730)
Remaining PBD from former right	355.74	325.61	330.42	4.810*** (0.597)	-25.753*** (0.947)	-30.125*** (1.067)
Observations	62,397	166,504	1,667,017	1,833,521	1,895,918	228,901

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

NOTE: This table compares the characteristics of eligible workers under the 20€ criterion, decomposed by taker status, with the characteristics of all other workers. Among eligibles, takers are more frequently male, younger, more skilled and educated. As compared to non-eligibles, takers are also younger but the proportion of female is higher, and they are less skilled. The rank among contracts with the same employer refers to the position of the contract among all contracts with a given employer (any number greater than one means that the worker has been recalled by a former employer). However, we do not have information on the universe of contracts, but only on the last one before opening a UI entitlement. Therefore, the measure of the recall probability is underestimated, but plausibly in the same way for takers and non-takers.

Table 1.3: Individual characteristics of compliers

	Compliers	Takers meeting both criteria	All takers	Eligible non takers	(2) - (1)	(3) - (1)	(4) - (1)
<i>Demographics</i>							
Age	39.50	30.09	31.19	35.25	-9.406*** (0.283)	-8.302*** (0.267)	-4.247*** (0.324)
Female	0.85	0.56	0.53	0.69	-0.288*** (0.013)	-0.325*** (0.013)	-0.162*** (0.012)
<i>Qualification</i>							
Executive	0.00	0.02	0.09	0.00	0.012** (0.004)	0.088*** (0.009)	-0.000 (0.002)
Intermediate occupation	0.02	0.02	0.04	0.01	0.009 (0.005)	0.025*** (0.006)	-0.010*** (0.002)
Skilled employee	0.62	0.58	0.55	0.51	-0.041** (0.015)	-0.068*** (0.015)	-0.108*** (0.015)
Skilled blue collar worker	0.21	0.27	0.23	0.22	0.057*** (0.014)	0.017 (0.013)	0.011 (0.013)
Unskilled employee	0.14	0.09	0.07	0.22	-0.047*** (0.009)	-0.068*** (0.008)	0.085*** (0.013)
Unskilled blue collar worker	0.01	0.02	0.02	0.03	0.010* (0.004)	0.006 (0.004)	0.022*** (0.005)
<i>Level of education</i>							
No education	0.04	0.02	0.02	0.04	-0.016*** (0.004)	-0.020*** (0.004)	0.003 (0.005)
Elementary school completed	0.02	0.01	0.01	0.02	-0.011*** (0.003)	-0.014*** (0.002)	-0.002 (0.004)
6 th to 8 th grade	0.02	0.01	0.01	0.02	-0.011*** (0.003)	-0.014*** (0.003)	-0.001 (0.004)
Middle school completed	0.05	0.04	0.03	0.06	-0.006 (0.005)	-0.015** (0.005)	0.013* (0.006)
10 th to 11 th grade	0.01	0.01	0.01	0.02	-0.004 (0.003)	-0.005* (0.002)	0.001 (0.003)
Vocational diploma	0.42	0.47	0.37	0.41	0.046*** (0.013)	-0.053*** (0.013)	-0.019 (0.013)
High school diploma - Baccalaureate	0.25	0.23	0.23	0.25	-0.020 (0.011)	-0.015 (0.011)	0.006 (0.011)
Two-year Higher education degree	0.10	0.11	0.14	0.09	0.011 (0.008)	0.042*** (0.009)	-0.007 (0.008)
Three to four-year Higher education degree	0.06	0.06	0.08	0.06	0.000 (0.006)	0.027*** (0.007)	0.001 (0.006)
Five-year and more Higher education degree	0.02	0.04	0.09	0.03	0.011* (0.005)	0.066*** (0.007)	0.005 (0.004)
Rank among contracts with the same employer	1.23	1.13	1.14	1.10	-0.103*** (0.011)	-0.0955*** (0.011)	-0.134*** (0.010)
<i>Rights' characteristics</i>							
Tenure	395.29	393.02	397.97	685.69	-2.262 (29.728)	2.679 (33.640)	290.400*** (67.251)
Part-time coefficient	0.69	0.69	0.77	0.65	-0.005 (0.015)	0.078*** (0.013)	-0.049*** (0.013)
Number of unemployment spells over the period	1.34	1.37	1.38	2.75	0.036 (0.023)	0.042 (0.023)	1.416*** (0.070)
PBD of the former right	446.45	469.23	474.69	550.85	22.779* (10.985)	28.237** (10.769)	104.399*** (11.587)
Remaining PBD from former right	341.26	356.09	364.38	325.61	14.834* (5.929)	23.123*** (5.923)	-15.645** (5.933)
Observations	1,508	60,889	123,571	166,504	62,397	125,079	168,012

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

NOTE: This table compares the characteristics of compliers, takers meeting both the 20€ and the 30% criteria, all takers and eligible non-takers. Compliers are the ones meeting the 20€ but not the 30% condition, as they would not be treated absent the 20€ criterion. They are older and more frequently female than all other categories. They are also over-represented among skilled employees. They are typically found in preschool assistant occupations, where income fluctuations are common, as wage depends on the number of children cared for. The rank among contracts with the same employer refers to the position of the contract among all contracts with a given employer (any number greater than one means that the worker has been recalled by a former employer). However, we do not have information on the universe of contracts, but only on the last one before opening a UI right. Therefore, the measure of the recall probability is underestimated, but plausibly in the same way for takers and non-takers.

Table 1.4: Characteristics associated to option right takeup - takers

	Probability of taking (20€ criteria) among all similar unemployed		Probability of taking (20€ criteria) among eligibles	
Elementary school completed	0.025160*** (0.005019)	0.003143 (0.002035)	0.030132** (0.013672)	0.007604 (0.010504)
6 th to 8 th grade	0.004410 (0.004170)	-0.000509 (0.001710)	-0.015637 (0.012529)	-0.001600 (0.009558)
Middle school completed	-0.003939 (0.003072)	0.002251* (0.001339)	0.001737 (0.009862)	0.014084* (0.007641)
10 th to 11 th grade	-0.023479*** (0.003840)	0.000604 (0.001839)	-0.030771** (0.013836)	0.021836* (0.011462)
Vocational diploma	0.002534 (0.002517)	0.005713*** (0.001068)	0.077864*** (0.008085)	0.033690*** (0.006193)
High school diploma - Baccalaureate	-0.028487*** (0.002528)	-0.001748 (0.001070)	-0.025753*** (0.008265)	0.003903 (0.006322)
Two-year Higher education degree	-0.010932*** (0.002609)	0.001277 (0.001126)	0.052097*** (0.008961)	0.037404*** (0.007084)
Three to four-year Higher education degree	-0.004330 (0.002742)	0.001781 (0.001187)	-0.004269 (0.009532)	0.019176** (0.007487)
Five-year and more Higher education degree	0.019198*** (0.002920)	0.005710*** (0.001282)	0.005442 (0.010670)	0.016391* (0.008367)
Intermediate occupation	-0.013155*** (0.003124)	0.004629** (0.002125)	-0.014972 (0.018860)	0.046751* (0.028132)
Skilled employee	-0.061633*** (0.002055)	-0.018049*** (0.001391)	-0.307112*** (0.014862)	-0.201210*** (0.022700)
Skilled blue collar worker	-0.071083*** (0.002140)	-0.022053*** (0.001423)	-0.322512*** (0.015045)	-0.210648*** (0.022798)
Unskilled employee	-0.103384*** (0.002184)	-0.031211*** (0.001425)	-0.493636*** (0.015091)	-0.279381*** (0.022770)
Unskilled blue collar worker	-0.114998*** (0.002459)	-0.030667*** (0.001552)	-0.457289*** (0.016778)	-0.268196*** (0.023381)
Age	-0.005622*** (0.000037)	-0.000835*** (0.000019)	-0.010938*** (0.000116)	-0.003284*** (0.000111)
Being a female	0.020972*** (0.000703)	-0.001489*** (0.000377)	-0.085785*** (0.003313)	-0.042449*** (0.002989)
Tenure		-0.000011*** (0.000000)		-0.000028*** (0.000002)
Part-time coefficient		-0.072184*** (0.000800)		0.092338*** (0.004397)
Number of unemployment spells over the period		-0.011231*** (0.000206)		-0.050182*** (0.001252)
PBD of the former right		-0.000052*** (0.000001)		-0.000252*** (0.000010)
Remaining PBD from former right		0.000075*** (0.000001)		0.000305*** (0.000010)
Observations	763,959	701,825	102,123	72,672

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

NOTE: This table regresses the probability of taking the option right, respectively on the whole sample and on the sample of eligible workers, on a set of observable individuals and right's characteristics. The first two columns look at the probability of exercising the option right among the whole sample whereas the last two columns look at the probability of exercising the option right under the 20€ criterion among eligibles based on the same criterion. Among eligible workers, the probability to opt for higher benefits depends mostly on the level of education, skills, gender, and workings hours. The reference category is male, executive, with no education.

Table 1.5: Characteristics associated to option right take-up - compliers

	Probability of being complier among all similar unemployed		Probability of being complier among eligible	
	(1)	(2)	(3)	(4)
Elementary school completed	0.000707 (0.000584)	-0.000260 (0.000186)	0.002863 (0.004070)	-0.002530 (0.001568)
6 th to 8 th grade	0.000304 (0.000508)	-0.000160 (0.000195)	0.000487 (0.003669)	-0.001602 (0.001666)
Middle school completed	-0.000227 (0.000363)	-0.000128 (0.000164)	-0.000928 (0.002901)	-0.001099 (0.001454)
10 th to 11 th grade	-0.000246 (0.000509)	-0.000128 (0.000234)	0.000489 (0.004482)	-0.000853 (0.002221)
Vocational diploma	-0.000150 (0.000301)	-0.000081 (0.000139)	0.004608* (0.002422)	-0.000343 (0.001239)
High school diploma - Baccalaureate	-0.000495 (0.000305)	-0.000097 (0.000143)	0.002344 (0.002507)	0.000156 (0.001311)
Two-year Higher education degree	-0.000906*** (0.000307)	-0.000228 (0.000144)	0.002982 (0.002830)	-0.000496 (0.001424)
Three to four-year Higher education degree	-0.000839*** (0.000320)	-0.000138 (0.000156)	-0.000186 (0.002929)	0.000830 (0.001647)
Five-year and more Higher education degree	-0.001179*** (0.000324)	-0.000346** (0.000148)	-0.004109 (0.003123)	-0.002221 (0.001524)
Intermediate occupation	0.000896*** (0.000252)	0.000353* (0.000209)	0.030555** (0.015441)	-0.009669 (0.007848)
Skilled employee	0.001436*** (0.000091)	0.000317*** (0.000085)	-0.000055 (0.009957)	-0.000263 (0.004824)
Skilled blue collar worker	0.001319*** (0.000118)	0.000248*** (0.000091)	-0.003223 (0.009995)	-0.000409 (0.004846)
Unskilled employee	0.001331*** (0.000139)	0.000233** (0.000096)	-0.010037 (0.009968)	-0.001870 (0.004835)
Unskilled blue collar worker	0.000358** (0.000156)	-0.000002 (0.000096)	-0.013847 (0.010109)	-0.003769 (0.004853)
Age	0.000025*** (0.000004)	0.000009*** (0.000002)	0.000127*** (0.000046)	0.000074*** (0.000023)
Being a female	0.001831*** (0.000086)	0.000247*** (0.000044)	0.007886*** (0.001080)	-0.000519 (0.000581)
Tenure		-0.000000*** (0.000000)		-0.000001** (0.000000)
Part-time coefficient		-0.000968*** (0.000099)		0.007404*** (0.001027)
Number of unemployment spells over the period		-0.000109*** (0.000023)		-0.001088*** (0.000248)
PBD of the former right		-0.000001*** (0.000000)		-0.000006*** (0.000002)
Remaining PBD from former right		0.000000*** (0.000000)		0.000003* (0.000002)
Observations	763,959	701,825	63,289	62,446

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

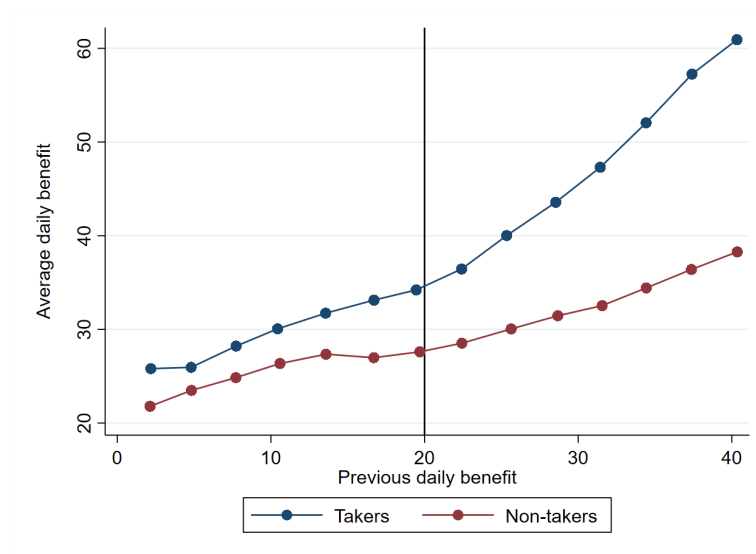
NOTE: This table regresses the probability of taking the option right, respectively on the whole sample and on the sample of eligible workers, on a set of observable individuals and right's characteristics. The first two columns look at the probability of being a complier among the whole sample whereas the last two columns look at the probability of being a complier among eligibles based on the 20€ criterion. Among eligible workers, the probability to be a complier depends mostly on age, workings hours and the number of past unemployment spells. The reference category is male, executive, with no education.

Table 1.6: New and former rights' characteristics of takers

	All takers	Takers based on the 20€ criterion	Compliers
Average ratio between new and former DB	2.25	2.79	1.18
Average ratio between new and former PBD	1.07	1.03	1.38
Average ratio between new PBD and the remaining PBD	1.35	1.27	1.65
Average ratio between total PBD if taking or not taking	.593	.575	.646

NOTE: This table informs on the characteristics associated to the former and the new rights for takers, decomposed by eligibility criterion. Compliers refers to the ones taking the option right under the 20€ condition, but not fulfilling the 30% criterion. By construction, the ratio of the new and the former daily benefits is lower for them, as is it constrained to be under 1.3. However, they loose less in terms of duration, as shown by the last row.

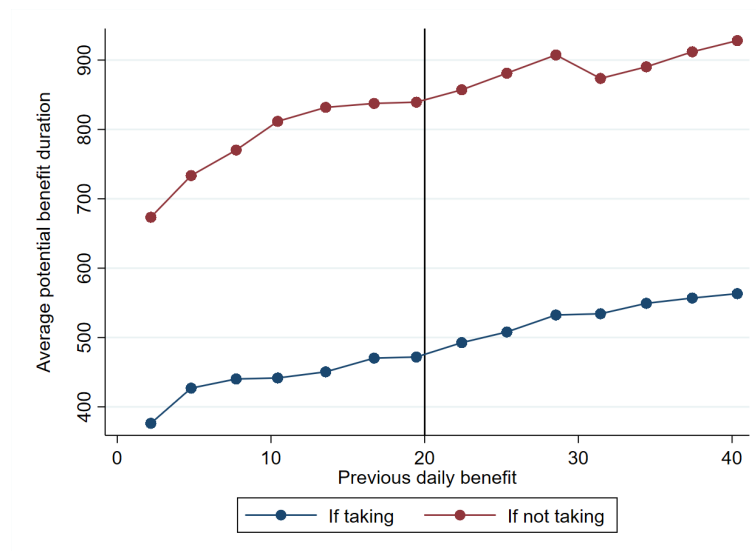
Figure 1.3: Average daily benefit among takers



SOURCE: UI data (FNA)

NOTE: This figure plots the average daily benefit as a function of the daily benefit of the former right for takers and non-takers. Overall, it shows an increasing relationship, as there is a correlation between past and current earnings. Even for non-takers, the current daily benefit is, on average, higher than the past one, especially for very low levels of benefit, as such levels are not very common among all workers. For takers, we observe that they gain significantly in terms of daily benefits by taking the option right, as both the level and the slope of the line are greater. We also notice that the slope slightly increases after the 20€ threshold, as takers necessary fulfil the 30% condition above that point.

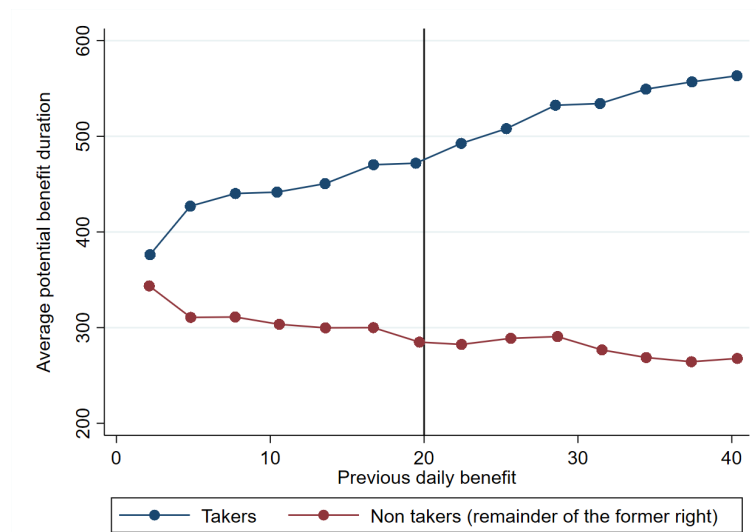
Figure 1.4: Average PBD among takers if taking or not taking



SOURCE: UI data (FNA)

NOTE: This figure plots the average potential benefit duration as a function of the daily benefit of the former right for takers. I compute the potential PBD they would have had they not taken the option right. Both lines follow the same pattern. Although the average PBD duration is much higher in the “no taking” scenario, takers are still entitled to a long duration in absolute terms.

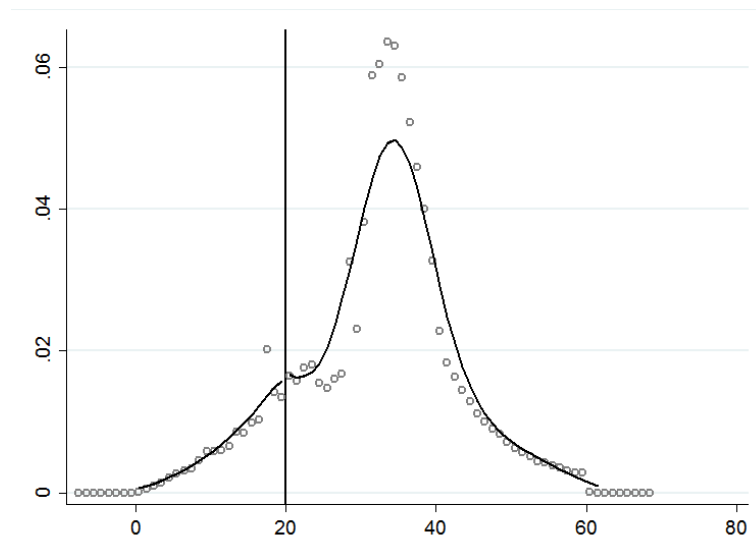
Figure 1.5: Average PBD without taking into account recharging



SOURCE: UI data (FNA)

NOTE: This figure plots the average PBD as a function of the daily benefit of the former right for takers and non-takers. In this graph, the PBD of non-takers corresponds to the duration of the remainder of the former right. The total PBD, which is the duration of the remainder plus the duration of the new potential right, cannot be computed for non-takers. We observe that the remainder of the former right is slightly decreasing with the level of previous benefit. It could be driven by the fact that the unemployment duration is generally positively correlated with the level of benefits. The higher the daily benefit, the more the job-seeker will consume of his right, the less he will have left for future unemployment spells.

Figure 1.6: Mc Crary test on previous DB distribution



SOURCE: UI data (FNA).

NOTE: This graph shows the [McCrary \(2008\)](#) test (binsize = 1, bandwidth=8) performed on the previous daily benefit distribution to test the hypothesis of continuity of the running variable distribution at the threshold. The density exhibits no discontinuity at the cutoff.

Table 1.7: Individual characteristics of unemployed people earning between 16€ and 24€ as daily benefits

	<i>DB</i> ≤ 20	<i>DB</i> > 20	Difference (1) - (2)
<i>Demographics</i>			
Age	35.30	31.66	3.643*** (0.103)
Female	0.73	0.65	0.088*** (0.004)
<i>Level of qualification</i>			
Executive	0.17	0.23	-0.057 (0.072)
Intermediate occupation	0.31	0.28	0.021 (0.083)
Skilled employee	47.29	47.41	-0.124 (0.770)
Skilled blue collar worker	5.31	7.53	-2.213*** (0.396)
Unskilled employee	40.79	37.66	3.123*** (0.749)
Unskilled blue collar worker	6.14	6.89	-0.751 (0.387)
<i>Level of education</i>			
No education	3.63	2.30	1.335*** (0.146)
Elementary school completed	2.36	1.44	0.924*** (0.117)
6 th to 8 th grade	2.31	1.77	0.534*** (0.125)
Middle school completed	5.82	5.42	0.393 (0.211)
10 th to 11 th grade	1.46	1.37	0.088 (0.108)
Vocational diploma	39.76	43.61	-3.854*** (0.457)
High school diploma - Baccalaureate	26.02	26.95	-0.932* (0.409)
Two-year Higher education degree	9.69	9.22	0.463 (0.269)
Three to four-year Higher education degree	5.89	5.55	0.344 (0.213)
Five-year and more Higher education degree	3.07	2.37	0.705*** (0.144)
<i>Tenure</i>	657.09	618.56	38.538** (12.895)
<i>Part-time coefficient</i>	0.67	0.73	-0.062*** (0.002)
<i>PBD</i>	560.23	561.62	-1.391 (2.059)
Observations	14,551	60,103	

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

NOTE: This table compares the characteristics of workers having a daily benefit below and above the 20€ threshold in its close neighbourhood. The whole window ranges between 16€ and 24€. To avoid any bias due to the selection of individuals as soon as the option right is introduced, which would change the composition of the pool of unemployed people around the 20€ cutoff, I compute these descriptive statistics on a sample of unemployed people having entered unemployment between January, 1st, 2013 and December, 31st, 2013, receiving the standard benefits and having a remainder from a former right. Workers below the threshold are older, more frequently female, work less hours, have more tenure, and are more represented at both ends of the education level distribution.

Table 1.8: Eligibility criteria distribution among takers

	Takers based on the 20€ criterion	
	0	1
Takers based on the 30% ratio criterion	0	1,791
	1	69,734

NOTE: This table reports the distribution of takers according to both eligibility criteria. We notice that most of the takers under the 20€ condition also fulfil the 30% one, as it is uncommon to earn these very low levels of benefits for several consecutive unemployment spells.

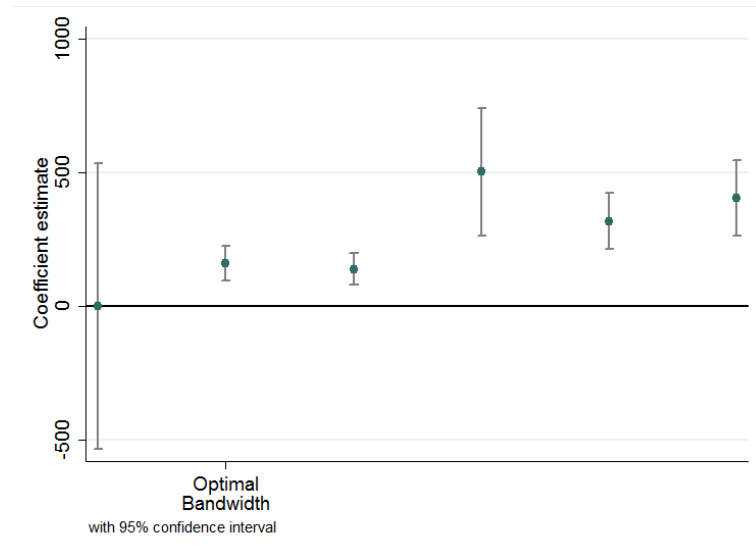
Table 1.9: Impact of paid unemployment spell duration with different bandwidths

	Optimal bandwidth			Optimal bandwidth * 2			Optimal bandwidth * 0.5		
Probability of taking the OR	155.135*** (18.569)	160.747*** (32.834)	180.974*** (39.099)	192.350*** (30.334)	328.577*** (59.406)	197.267*** (41.084)	260.962 (161.983)	-66.386 (378.079)	208.455*** (65.456)
Distance	-2.325*** (0.484)	-4.854*** (0.854)	0.864 (1.869)	0.036 (0.507)	1.527 (1.126)	-4.270*** (0.794)	9.295 (11.054)	-19.230 (37.633)	23.482 (15.606)
Distance × DB > 20	5.894*** (1.208)	10.476*** (1.766)	1.819 (2.949)	3.627*** (0.504)	8.364*** (1.115)	11.553*** (1.465)	-8.364 (7.346)	4.961 (18.382)	-39.547** (15.541)
Distance ²		-1077.933*** (202.147)	1803.835** (867.519)		-264.589*** (60.675)	-1492.782*** (159.116)		-9159.031 (17316.567)	18725.127 (14839.506)
Distance ² × DB > 20		482.099** (227.948)	-912.140 (1002.875)		-379.543*** (142.662)	581.607*** (182.593)		15320.187 (23649.922)	-1433.824 (15484.668)
Distance ³			39087.906*** (11944.296)			-9680.261*** (1135.141)			396759.301 (374743.042)
Distance ³ × DB > 20			-56372.499*** (16033.316)			13868.027*** (1702.476)			-804653.162** (387322.018)
Age				0.767*** (0.115)	1.148*** (0.200)	0.691*** (0.128)	1.077* (0.631)	-0.257 (1.515)	0.830*** (0.258)
Sex				15.966*** (2.008)	23.936*** (3.498)	15.861*** (2.238)	21.268* (11.791)	-1.511 (24.979)	16.906*** (4.453)
Level of education				-0.745* (0.428)	-3.004*** (0.901)	-0.998 (0.611)	-2.087 (2.450)	2.802 (5.437)	-1.042 (0.891)
Constant	66.425*** (4.032)	64.427*** (6.860)	62.245*** (8.110)	28.742*** (8.685)	-3.278 (15.283)	29.477*** (10.389)	11.811 (44.639)	104.144 (106.509)	29.792* (18.005)
Observations	129,090	244,710	287,215	251,244	457,292	564,976	60,563	120,174	155,282

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

NOTE: This table reproduces the main result on the impact of the option right on the duration of the subsequent paid unemployment spell, using different bandwidths. The RD is estimated with the optimal bandwidth (MSE criterion), twice its value and half its value. Results are overall similar, but not necessarily significant when choosing a bandwidth equivalent to half of the optimal one.

Figure 1.7: Impact of paid unemployment spell duration with different bandwidths



SOURCE: UI data (FNA).

NOTE: This graph reproduces the main result on the impact of the option right on the duration of the subsequent paid unemployment spell, using different bandwidths and a 2^{nd} order polynomial. Optimal bandwidth has been multiplied respectively by 0.5, 1.5, 2, 2.5 and 3. Results are overall similar, but not significant when choosing a bandwidth equivalent to half of the optimal one.

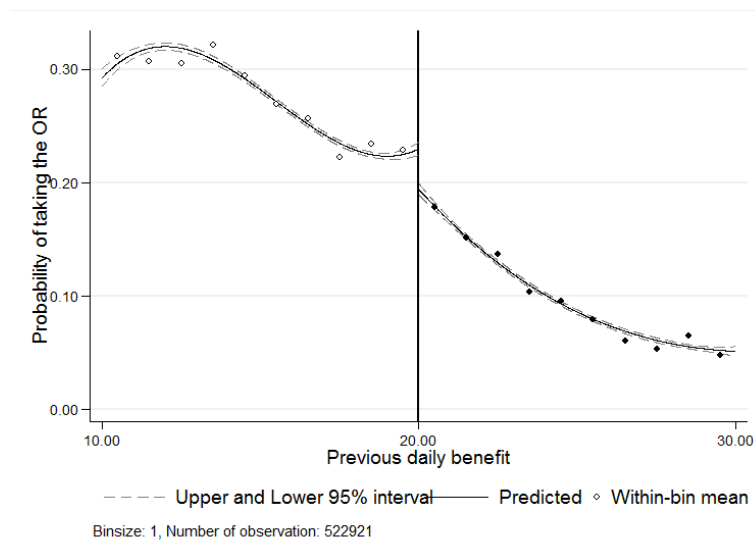
Table 1.10: Impact of having a DB lower or equal to 20€ on OR takeup

	Probability of taking the OR					
<i>DB > 20</i>	-0.067*** (0.005)	-0.042*** (0.005)	-0.045*** (0.006)	-0.028*** (0.004)	-0.035*** (0.005)	-0.041*** (0.006)
<i>Former DB</i>	0.027*** (0.002)	0.012*** (0.004)	0.011 (0.008)	-0.009*** (0.001)	-0.001 (0.003)	-0.001 (0.006)
<i>Distance × DB > 20</i>	-0.045*** (0.003)	-0.036*** (0.005)	-0.027*** (0.010)	-0.014*** (0.002)	-0.023*** (0.004)	-0.012 (0.008)
<i>Distance²</i>		5.086*** (0.894)	5.312 (3.535)		1.797** (0.698)	1.522 (2.598)
<i>Distance² × DB > 20</i>		-4.677*** (1.131)	-9.758** (4.383)		-1.211 (0.872)	-6.041* (3.230)
<i>Distance³</i>			17.335 (45.444)			-6.672 (29.934)
<i>Distance³ × DB > 20</i>			54.689 (54.891)			70.343* (36.278)
<i>Age</i>				-0.004*** (0.000)	-0.004*** (0.000)	-0.004*** (0.000)
<i>Sex</i>				-0.064*** (0.002)	-0.064*** (0.002)	-0.064*** (0.001)
<i>Level of education</i>				0.013*** (0.000)	0.013*** (0.000)	0.014*** (0.000)
Constant	0.251*** (0.003)	0.232*** (0.004)	0.231*** (0.005)	0.304*** (0.005)	0.306*** (0.005)	0.303*** (0.005)
Observations	129,090	244,710	287,215	217,638	264,736	312,161
F-test	225.38***	76.97***	58.45***	68.21***	62.43***	57.02***

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

NOTE: This table reports the first-stage estimation results. It shows that having a daily benefit lower than 20€ is associated to a jump in the option right takeup rate of about four percentage points. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector.

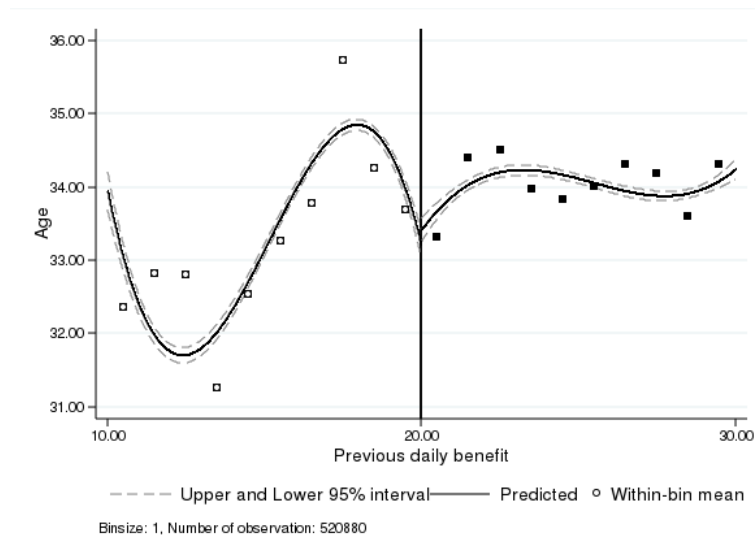
Figure 1.8: Probability of taking the option right



SOURCE: UI data (FNA).

NOTE: This graph reports the first-stage relationship between the running variable, which is the former daily benefit, and the probability to take the option right. It shows that having a daily benefit lower than 20€ is associated to a jump in option right takeup right of about four percentage points.

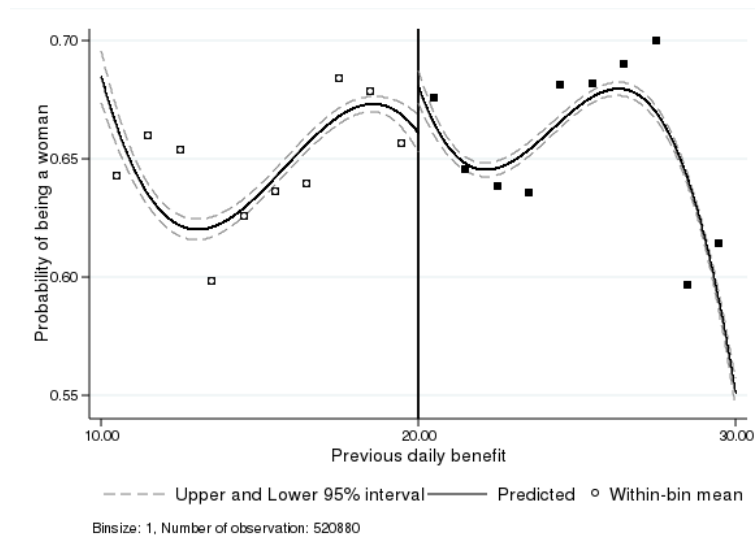
Figure 1.9: Age distribution



SOURCE: UI data (FNA).

NOTE: This graph plots the average age as a function of the level of the former daily benefit (3^{rd} order polynomial). It tests the assumption of continuity in the distribution of covariates at the threshold. Reassuringly, we do not see any discontinuity at the 20€ threshold.

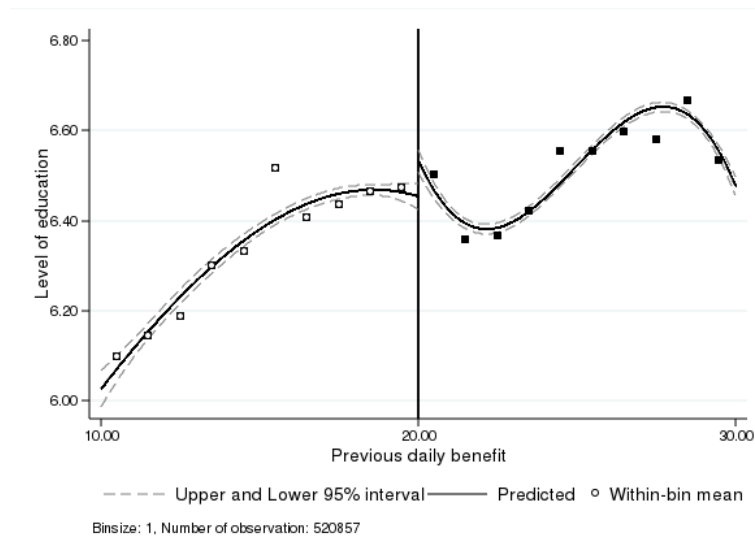
Figure 1.10: Proportion of women distribution



SOURCE: UI data (FNA).

NOTE: This graph plots the proportion of women as a function of the level of the former daily benefit (3^{rd} order polynomial). It tests the assumption of continuity in the distribution of covariates at the threshold. Reassuringly, we do not see any discontinuity at the 20€ threshold.

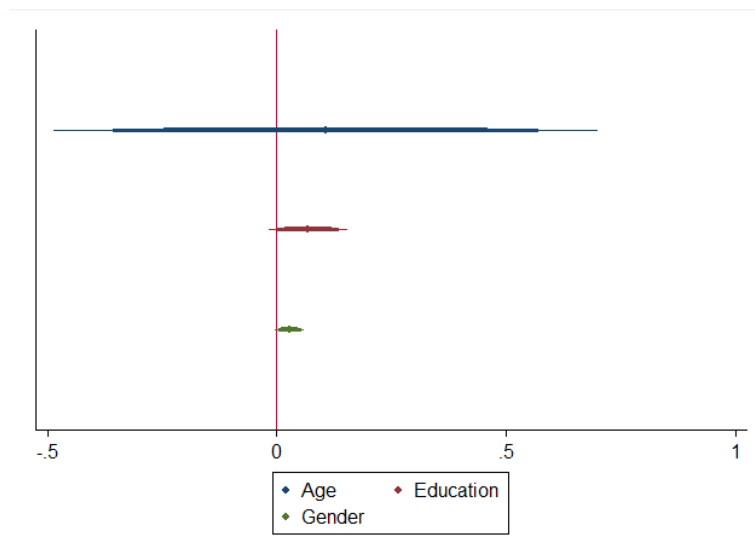
Figure 1.11: Level of education distribution



SOURCE: UI data (FNA).

NOTE: This graph plots the average level of education as a function of the level of the former daily benefit (3^{rd} order polynomial). It tests the assumption of continuity in the distribution of covariates at the threshold. Reassuringly, we do not see any discontinuity at the 20€ threshold.

Figure 1.12: Magnitude of the difference in covariates at the cutoff



NOTE: This figure plots the difference in the average level of each covariate at the threshold, to test the assumption of continuity in the distribution of covariates at the eligibility threshold. The three levels of significance of the confidence intervals depicted are 5%, 1% and 0.1%.

Table 1.11: Test of continuity of covariates distribution

	Age			Gender			Level of education		
<i>DB</i> > 20	0.237*	0.375**	0.107	0.032***	0.031***	0.028***	0.061***	0.071***	0.068***
	(-0.018,0.493)	(0.040,0.709)	(-0.246,0.460)	(0.020,0.044)	(0.016,0.047)	(0.009,0.047)	(0.016,0.106)	(0.024,0.119)	(0.017,0.119)
<i>Distance</i>	-1.139***	-1.765***	-2.109***	-0.014***	0.016	0.110***	0.037**	0.023	0.131***
	(-1.268,-1.010)	(-2.122,-1.408)	(-2.548,-1.670)	(-0.023,-0.006)	(-0.004,0.035)	(0.074,0.146)	(0.005,0.070)	(-0.021,0.067)	(0.074,0.188)
<i>Distance</i> × <i>DB</i> > 20	1.747***	2.381***	3.644***	-0.014**	-0.079***	-0.259***	-0.185***	-0.203***	-0.417***
	(1.558,1.935)	(1.891,2.870)	(3.040,4.248)	(-0.026,-0.002)	(-0.107,-0.051)	(-0.309,-0.209)	(-0.230,-0.141)	(-0.264,-0.142)	(-0.495,-0.338)
<i>Distance</i> ²		-319.439***	-527.209***		13.986***	110.113***		0.442	54.814***
		(-432.268,-206.610)	(-734.652,-319.766)		(6.776,21.197)	(84.446,135.779)		(-11.705,12.590)	(30.381,79.247)
<i>Distance</i> ² × <i>DB</i> > 20		282.757***	23.525		8.050	5.738		37.074***	42.108**
		(131.623,433.891)	(-255.647,302.698)		(-2.303,18.404)	(-29.964,41.440)		(20.505,53.644)	(9.045,75.171)
<i>Distance</i> ³			-1651.661			2610.285***			666.758***
			(-4397.612,1094.290)			(2081.041,3139.529)			(372.418,961.098)
<i>Distance</i> ³ × <i>DB</i> > 20			6206.383***			-5284.637***			-1483.944***
			(2568.055,9844.711)			(-6013.020,-4556.254)			(-1875.656,-1092.233)
Constant	32.997***	32.893***	32.829***	0.652***	0.663***	0.683***	6.498***	6.491***	6.532***
	(32.813,33.182)	(32.650,33.136)	(32.574,33.084)	(0.644,0.661)	(0.652,0.675)	(0.670,0.697)	(6.465,6.530)	(6.457,6.526)	(6.496,6.569)
Observations	127,549	183,185	276,067	97,125	147,575	186,864	94,029	207,402	298,768

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

NOTE: This table tests the assumption of continuity in the distribution of covariates at the eligibility threshold by using as a dependent variable in the RDD regression each covariate. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector. We observe that there are differences in the distribution of covariates at the threshold, although the magnitude is small. However, the graphs do not depict at the threshold any jump that would be more significant than at other points of the distribution. Covariates are also added to the regression, yielding similar results.

Table 1.12: Impact of the option right on paid unemployment duration – 2nd stage

	Paid unemployment spell duration					
Probability of taking the OR	155.135***	160.747***	180.974***	193.507***	182.987***	189.902***
	(18.569)	(32.834)	(39.099)	(36.351)	(37.961)	(40.592)
<i>Distance</i>	-2.325***	-4.854***	0.864	-0.593	-2.861***	0.176
	(0.484)	(0.854)	(1.869)	(0.653)	(0.984)	(1.819)
<i>Distance</i> × <i>DB</i> > 20	5.894***	10.476***	1.819	4.938***	8.744***	3.840
	(1.208)	(1.766)	(2.949)	(0.630)	(1.448)	(2.404)
<i>Distance</i> ²		-1077.933***	1803.835**		-642.648***	874.767
		(202.147)	(867.519)		(165.552)	(692.788)
<i>Distance</i> ² × <i>DB</i> > 20		482.099**	-912.140		110.422	-589.665
		(227.948)	(1002.875)		(223.017)	(784.210)
<i>Distance</i> ³			39087.906***			21071.319**
			(11944.296)			(8406.419)
<i>Distance</i> ³ × <i>DB</i> > 20			-56372.499***			-30035.497***
			(16033.316)			(11256.948)
<i>Age</i>				0.800***	0.723***	0.739***
				(0.141)	(0.142)	(0.150)
<i>Sex</i>				15.571***	15.247***	15.846***
				(2.389)	(2.482)	(2.622)
<i>Level of education</i>				-0.719	-0.616	-0.700
				(0.512)	(0.525)	(0.564)
Constant	66.425***	64.427***	62.245***	26.541**	29.770***	29.151**
	(4.032)	(6.860)	(8.110)	(10.527)	(10.941)	(11.471)
Observations	129,090	244,710	287,215	217,638	264,736	312,161

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

NOTE: This table reports the regression discontinuity regressions on the duration of the subsequent unemployment spell. The dependent variable is the paid unemployment duration, which only includes days the job-seeker receives benefits. Linear, quadratic and cubic specifications are used, with and without controls. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector. The option right is predicted to increase the subsequent paid unemployment spell duration by more than five months.

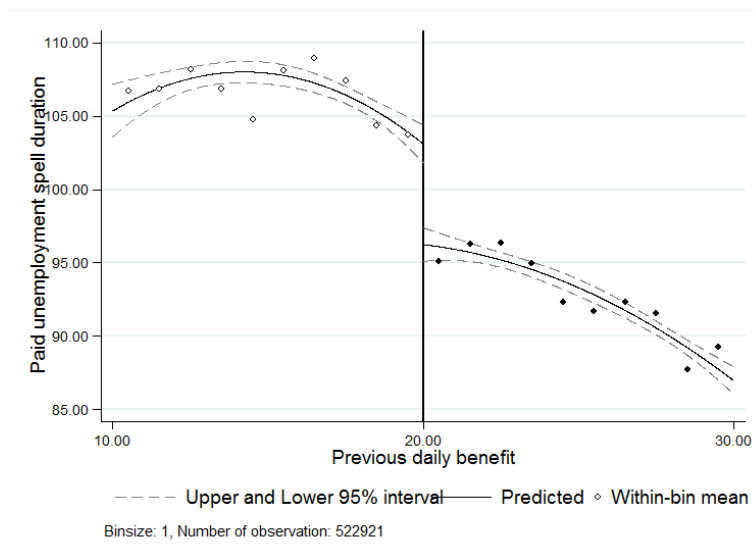
Table 1.13: Impact of the option right on full unemployment duration – 2nd stage

	Full unemployment spell duration					
Probability of taking the OR	370.896***	356.759***	390.788***	405.259***	383.820***	425.527***
	(31.397)	(52.124)	(62.511)	(52.607)	(55.119)	(70.529)
<i>Distance</i>	-1.690**	-4.009***	1.754	1.848**	-0.790	3.558
	(0.681)	(1.070)	(2.712)	(0.901)	(1.196)	(2.593)
<i>Distance</i> × <i>DB</i> > 20	10.868***	13.089***	5.307	6.757***	10.193***	5.316
	(1.720)	(2.448)	(4.384)	(0.897)	(1.992)	(3.361)
<i>Distance</i> ²		-1340.889***	1378.011		-695.939***	859.719
		(223.830)	(1190.241)		(175.172)	(850.775)
<i>Distance</i> ² × <i>DB</i> > 20		944.227***	-534.752		335.934	-728.090
		(239.673)	(1408.821)		(226.304)	(1072.759)
<i>Distance</i> ³			35462.304**			16216.357*
			(15708.817)			(9414.697)
<i>Distance</i> ³ × <i>DB</i> > 20			-50327.902**			-20865.132*
			(20839.025)			(11704.823)
<i>Age</i>				2.220***	2.107***	2.256***
				(0.201)	(0.203)	(0.258)
<i>Sex</i>				36.118***	34.854***	37.260***
				(3.491)	(3.619)	(4.491)
<i>Level of education</i>				-3.962***	-3.691***	-4.330***
				(0.756)	(0.775)	(0.971)
Constant	76.978***	79.985***	74.902***	-1.640	4.625	-4.833
	(6.705)	(10.893)	(12.985)	(15.026)	(15.753)	(19.946)
Observations	141,400	280,795	300,343	235,275	300,026	345,383

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

NOTE: This table reports the regression discontinuity regressions on the duration of the subsequent unemployment spell. The dependent variable is the total unemployment duration, including days the job-seeker is registered as unemployed, no matter whether he receives benefits or not. Linear, quadratic and cubic specifications are used, with and without controls. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector. The option right is predicted to increase the subsequent full unemployment spell duration by more than eleven months.

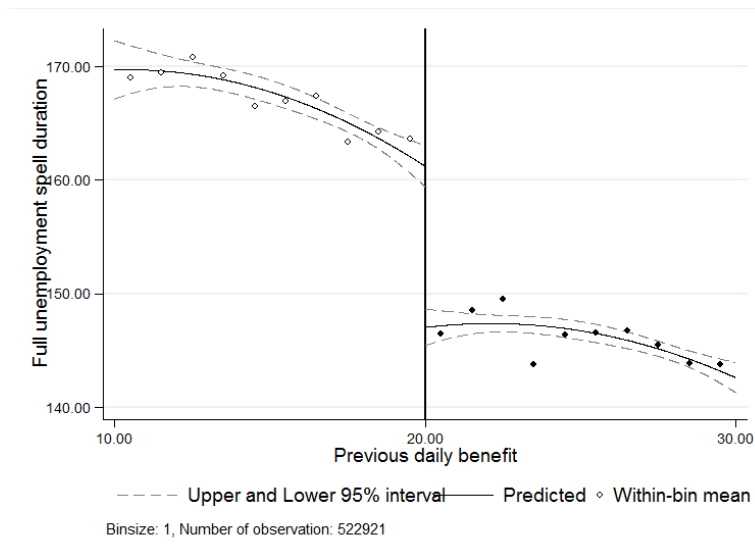
Figure 1.13: Paid unemployment spell duration



SOURCE: UI data (FNA).

NOTE: This graph reports the second-stage relationship between the running variable, which is the former daily benefit, and the duration of the subsequent unemployment spell (2^{nd} order polynomial). The unemployment spell is defined as the paid unemployment spell, i.e. the addition of the days the job-seeker receives benefits. It shows that having a daily benefit lower than 20€ is associated to a jump in the duration of the paid unemployment spell from about 97 days to 104 days.

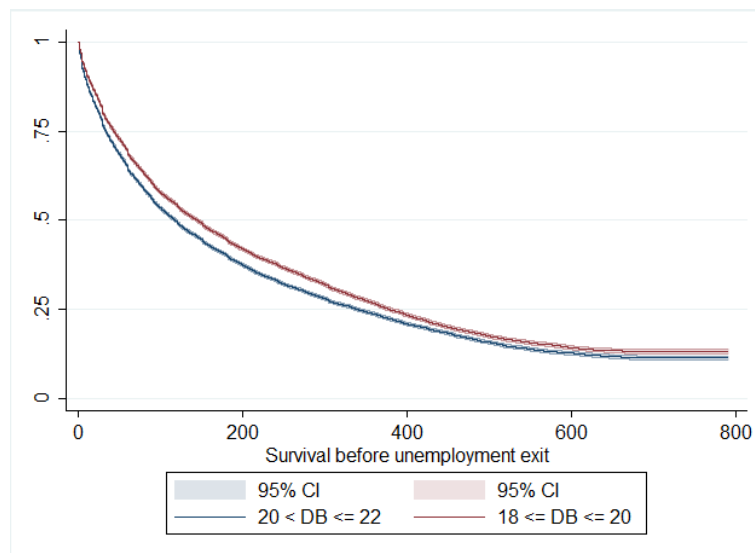
Figure 1.14: Full unemployment spell duration



SOURCE: UI data (FNA).

NOTE: This graph reports the second-stage relationship between the running variable, which is the former daily benefit, and the duration of the subsequent unemployment spell (2^{nd} order polynomial). The unemployment spell is defined as the full unemployment spell, i.e. the addition of all the days the job-seeker is registered as unemployed, not matter whether he receives benefits or not. It shows that having a daily benefit lower than 20€ is associated to a jump in the duration of the full unemployment spell from about 148 days to 161 days.

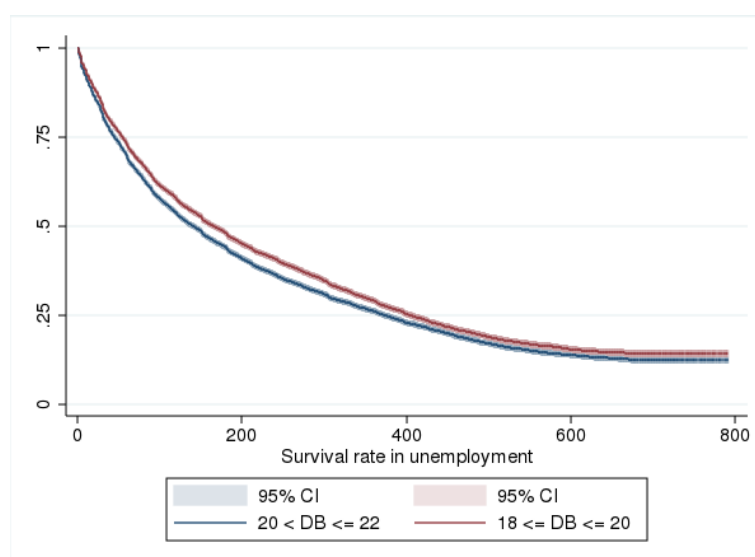
Figure 1.15: Short-term Kaplan-Meier survival function



SOURCE: UI data (FNA).

NOTE: The Kaplan-Meier survival functions have been estimated for unemployed people earning a daily benefit lying between 18 and 20 (included), and 20 (excluded) and 22 separately. Survival duration is defined as the time before exiting unemployment for at least 4 months after the decision to exercise or not the option right. The survival function of unemployed people below the 20€ threshold is always significantly lying above, indicating that their probability of not having exited unemployment at any point in time is always higher than for the other group. This result is in accordance with the RD estimates, and allows to correct for right censoring.

Figure 1.16: Long-term Kaplan-Meier survival function



SOURCE: UI data (FNA).

NOTE: The Kaplan-Meier survival functions have been estimated for unemployed people earning a daily benefit lying between 18 and 20 (included), and 20 (excluded) and 22 separately. Survival duration is defined as the time before exiting unemployment for at least 4 months, taking into account all unemployment spells after the decision to exercise or not the option right. The Kaplan-Meier survival functions in the long run duration measure reveals a higher survival probability at any point in time for the unemployed people below the 20€ threshold, though the difference is a bit lower in magnitude as compared to the short-term outcome.

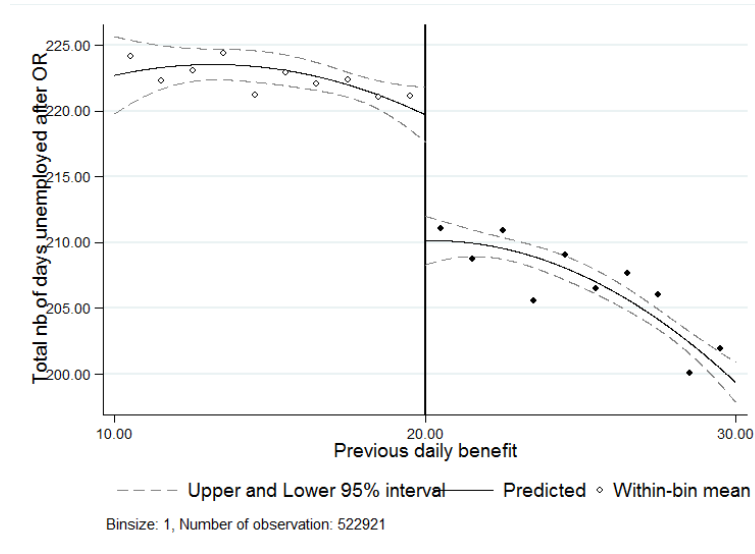
Table 1.14: Placebo tests at random cutoff values

	Paid unemployment spell duration		
	Cutoff at 8	Cutoff at 33	Cutoff at 48
RD_Estimate	-255.836 (509.432)	-13644.033 (918840.218)	-136.885 (372.238)
Observations	1,888,093	1,888,093	1,888,093

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

NOTE: The RD is estimated at different cutoff values using a second order polynomial and optimal bandwidth (MSE criterion allowing for different bandwidths at each side of the cutoff).

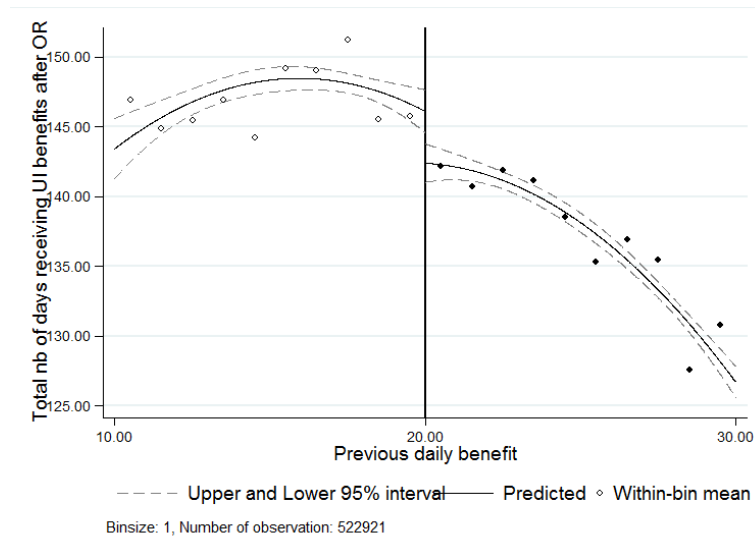
Figure 1.17: Total number of days unemployed after the exercise of the option right



SOURCE: UI data (FNA).

NOTE: This graph reports the second-stage relationship between the running variable, which is the former daily benefit, and the total number of days registered as unemployed over the whole observed period (2nd order polynomial). The observation period is between October, 2014 and May 2017. It shows that having a daily benefit lower than 20€ is associated to a jump in the total number of days registered as unemployed from about 210 days to 220 days.

Figure 1.18: Total number of days on UI benefits after the exercise of the option right



SOURCE: UI data (FNA).

NOTE: This graph reports the second-stage relationship between the running variable, which is the former daily benefit, and the total number of days on UI benefits over the whole observed period (2nd order polynomial). The observation period is between October, 2014 and May 2017. It shows that having a daily benefit lower than 20€ is associated to a jump in the total number of days on UI benefits from about 143 days to 146 days.

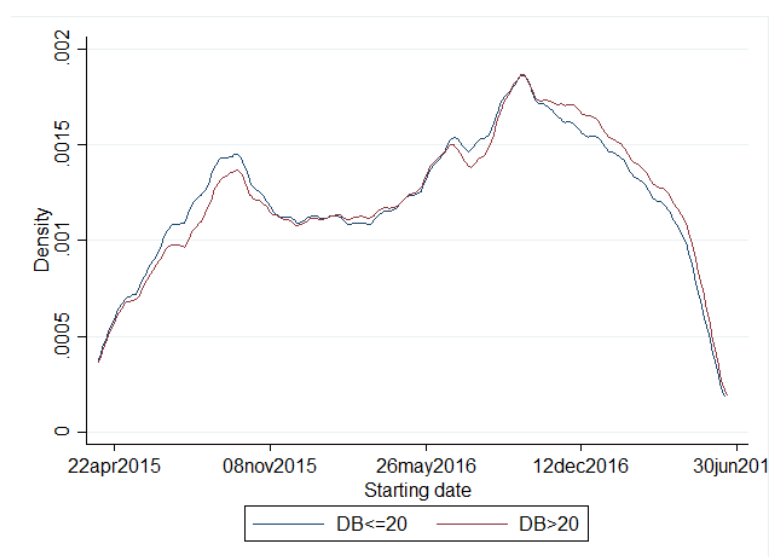
Table 1.15: Impact of the option right on the total number of days on UI benefits – 2nd stage

	Total number of days receiving UI benefits after OR					
Probability of taking the OR	50.906 (36.970)	11.928 (37.462)	-10.246 (42.277)	8.635 (30.421)	34.800 (43.627)	33.968 (48.703)
<i>Distance</i>	1.102 (1.026)	-4.376*** (1.027)	-2.438 (1.637)	-2.952*** (0.648)	-3.428*** (1.253)	-5.321*** (1.729)
<i>Distance</i> × <i>DB</i> > 20	-2.834** (1.389)	4.111* (2.120)	-2.079 (3.069)	2.186** (0.849)	4.316*** (1.648)	8.091*** (2.364)
<i>Distance</i> ²		-728.435*** (251.339)	158.219 (682.050)		-502.543** (219.317)	-1315.643** (559.888)
<i>Distance</i> ² × <i>DB</i> > 20		686.131** (281.490)	1537.423* (812.093)		323.229 (282.492)	298.341 (735.915)
<i>Distance</i> ³			9693.813 (8261.737)			-7701.675 (6154.272)
<i>Distance</i> ³ × <i>DB</i> > 20			-31567.581*** (12019.519)			16483.456** (7887.741)
<i>Age</i>				-0.060 (0.124)	0.051 (0.164)	0.037 (0.178)
<i>Sex</i>				6.766*** (2.197)	8.508*** (2.856)	8.947*** (3.099)
<i>Level of education</i>				2.544*** (0.446)	2.039*** (0.599)	2.135*** (0.671)
Constant	134.843*** (7.624)	140.390*** (7.852)	146.312*** (8.885)	122.252*** (9.109)	115.104*** (12.571)	114.035*** (13.819)
Observations	112208	239336	314281	162003	255812	347309

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

NOTE: This table reports the regression discontinuity regressions on the total number of days on UI benefits over the whole observed period. The observation period is between October, 2014 and May 2017. Linear, quadratic and cubic specifications are used, with and without controls. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector. The option right does not have a significant impact on the total number of days on UI benefits.

Figure 1.19: Starting date distribution



SOURCE: UI data (FNA).

NOTE: This graph plots the distribution of workers with a daily benefit below and above the 20€ threshold (within a window ranging between 16€ and 24€), according to the starting date of the unemployment spell. It shows that control and treated workers are distributed the same way across time, meaning that right censoring should not bias the results.

Table 1.16: Impact of the option right on the total number of days registered as unemployed – 2nd stage

	Total number of days unemployed after OR					
Probability of taking the OR	224.351*** (31.997)	159.162*** (55.618)	135.527** (54.830)	238.441*** (45.877)	247.116*** (59.330)	187.022*** (61.807)
<i>Distance</i>	2.561*** (0.803)	-2.333** (1.159)	1.453 (2.236)	0.866 (0.749)	0.130 (1.080)	0.779 (1.990)
<i>Distance</i> × <i>DB</i> > 20	-0.805 (1.976)	2.859 (2.654)	-7.180* (4.215)	3.432*** (0.758)	5.213*** (1.538)	-0.557 (3.002)
<i>Distance</i> ²		-631.429*** (237.274)	1253.660 (962.451)		-314.859*** (90.295)	95.873 (600.486)
<i>Distance</i> ² × <i>DB</i> > 20		1048.041*** (293.064)	1934.219* (1130.045)		90.714 (140.051)	845.883 (783.234)
<i>Distance</i> ³			23913.535** (11883.703)			3234.808 (6115.500)
<i>Distance</i> ³ × <i>DB</i> > 20			-59988.448*** (17662.232)			-12344.350 (8263.200)
<i>Age</i>				1.537*** (0.173)	1.596*** (0.212)	1.377*** (0.224)
<i>Sex</i>				29.106*** (3.020)	29.516*** (3.766)	25.957*** (3.915)
<i>Level of education</i>				-1.279** (0.648)	-1.450* (0.834)	-0.623 (0.857)
Constant	171.268*** (6.916)	183.120*** (11.659)	190.083*** (11.527)	103.514*** (13.122)	100.275*** (16.511)	118.585*** (17.477)
Observations	126865	270267	307173	261616	387608	369044

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

NOTE: This table reports the regression discontinuity regressions on the total number of days registered as unemployed over the whole observed period. The observation period is between October, 2014 and May 2017. Linear, quadratic and cubic specifications are used, with and without controls. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector. The option right is predicted to increase the total number of days registered as unemployed by about 160 days if we look at the second specification.

Table 1.17: Impact of OR on unemployment duration – 2nd stage

	Total nb of days receiving UI benefits after OR						Total nb of days unemployed after OR					
Probability of taking the OR	59.986 (52.654)	23.676 (64.589)	-9.652 (67.919)	92.280 (63.056)	47.754 (70.655)	27.861 (72.547)	195.481*** (73.603)	102.070 (77.348)	76.607 (85.742)	306.610*** (61.167)	159.413* (85.961)	182.142*** (68.294)
<i>Distance</i>	2.375 (1.656)	-5.039** (1.999)	-3.569 (3.154)	-1.888 (1.414)	-3.817* (2.213)	-6.248** (3.137)	2.775 (2.390)	-6.273*** (2.400)	-7.638** (3.323)	2.860*** (1.102)	-0.949 (2.522)	-1.043 (2.446)
<i>Distance</i> × <i>DB</i> > 20	-6.060*** (2.157)	4.406 (2.880)	-2.472 (4.743)	3.845*** (1.171)	4.322* (2.434)	7.000** (3.562)	-4.777 (2.914)	2.541 (3.844)	0.688 (5.555)	1.908* (1.042)	-1.833 (3.065)	-0.399 (3.353)
<i>Distance</i> ²		-812.381** (372.159)	-31.259 (1221.389)		-580.657* (335.180)	-1616.620* (933.877)		-1670.177*** (479.837)	-2887.594** (1169.154)		-526.012 (365.344)	-959.186 (602.982)
<i>Distance</i> ² × <i>DB</i> > 20		799.723* (460.874)	2278.564 (1425.408)		408.234 (474.869)	1130.579 (1143.492)		2593.550*** (570.247)	5327.473*** (1455.896)		1426.723*** (524.054)	1961.965*** (615.534)
<i>Distance</i> ³			9820.358 (14733.238)			-11078.596 (9520.050)			-25089.789* (12807.653)			-8933.414* (4990.968)
<i>Distance</i> ³ × <i>DB</i> > 20			-39625.894* (20941.552)			13653.772 (12203.760)			1625.752 (19163.597)			393.815 (7320.802)
<i>Age</i>				0.649*** (0.232)	0.438* (0.250)	0.382 (0.248)				2.530*** (0.219)	2.020*** (0.302)	2.165*** (0.223)
<i>Sex</i>				11.379** (4.488)	8.508* (5.052)	8.271* (4.898)				38.413*** (4.447)	27.712*** (6.137)	30.159*** (4.507)
<i>Level of education</i>				0.868 (1.029)	1.694 (1.119)	2.046* (1.146)				-3.579*** (1.010)	-1.276 (1.382)	-1.794 (1.218)
Constant	167.003*** (11.400)	171.196*** (13.994)	179.861*** (14.776)	121.506*** (17.616)	134.927*** (19.687)	138.401*** (19.628)	239.175*** (15.892)	256.684*** (16.835)	262.744*** (18.811)	127.363*** (17.063)	168.894*** (23.825)	160.667*** (17.250)
Observations	53,265	116,540	145,707	89,062	126,341	166,593	52,442	114,775	157,033	126,574	130,083	204,156

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

NOTE: This table reports the regression discontinuity regressions on the total number of days on UI benefits (first six columns) and on the total number of days registered as unemployed over the whole period of observation. The observation period is between April, 2015 and May 2017. Linear, quadratic and cubic specifications are used, with and without controls. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector. The sample has been restricted to unemployment spells starting between April, 2015 and April, 2016 to avoid right-censoring issue and to have a longer time horizon to measure long-run impact. Results are similar than on the whole sample.

Table 1.18: Impact of OR on the probability to exhaust benefits – 2nd stage

	Probability of exhausting the right					
Taking the OR	0.5154***	0.4751***	0.4493***	0.1830***	0.2290***	0.2927***
	(0.0664)	(0.0452)	(0.0239)	(0.0361)	(0.0298)	(0.0215)
<i>Distance</i>	-0.0006	-0.0015	-0.0038**	-0.0010**	-0.0014	-0.0028*
	(0.0008)	(0.0016)	(0.0015)	(0.0004)	(0.0010)	(0.0016)
<i>Distance</i> × <i>DB</i> > 20	0.0043***	0.0061**	0.0077***	0.0008	0.0013	0.0050
	(0.0013)	(0.0030)	(0.0029)	(0.0007)	(0.0020)	(0.0030)
<i>Distance</i> ²		-0.3583	-1.4338***		-0.1731	-0.8583
		(0.3748)	(0.4882)		(0.1998)	(0.5318)
<i>Distance</i> ² × <i>DB</i> > 20		-0.1561	0.9990*		0.3286	0.3993
		(0.4494)	(0.5817)		(0.2369)	(0.6290)
<i>Distance</i> ³			-13.1398***			-7.6026
			(4.2578)			(4.8874)
<i>Distance</i> ³ × <i>DB</i> > 20			15.2395**			12.6127
			(7.3755)			(8.3817)
Age				0.0001	0.0001	0.0001***
				(0.0001)	(0.0001)	(0.0000)
Probability of being a woman				-0.0062***	-0.0053***	-0.0041***
				(0.0008)	(0.0008)	(0.0008)
Level of education				-0.0020***	-0.0025***	-0.0031***
				(0.0004)	(0.0003)	(0.0002)
Observations	168,865	243,811	402,038	232,042	286,540	383,325
<i>Chi</i> ²	35.821***	74.037***	250.726***	4.900**	22.709***	109.083***

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

NOTE: This table reports the regression discontinuity regressions on the probability to exhaust UI benefits. The right here refers to the new right for those exercising the option right, and to the remainder from the former right for those not exercising. If an eligible non-taker has exhausted the remainder and I do not observe that he recharges his right based on his last employment spells, I assume that he is not able to recharge, and has exhausted all his entitlements. If a recharging is observed for non-takers, I take into account the addition of the remainder and the new right. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector. Linear, quadratic and cubic specifications are used, with and without controls. The option right is predicted to increase the probability to exhaust UI benefits by 47 percentage points if we look at the second specification.

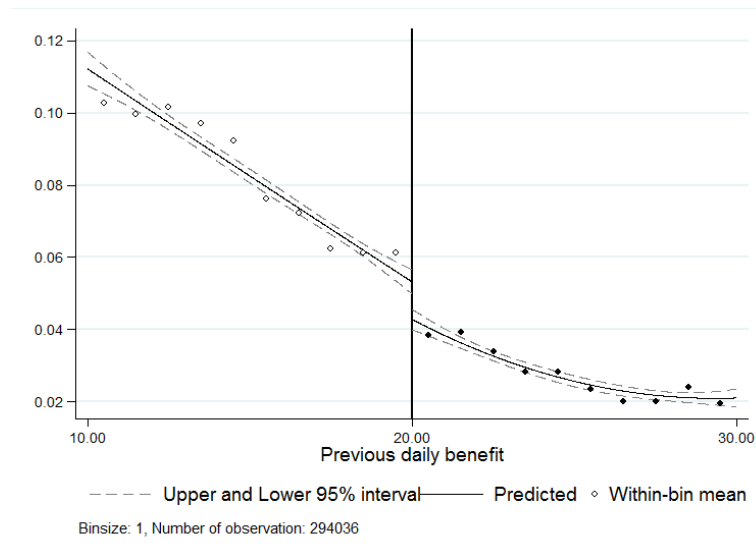
Table 1.19: Impact of OR on the consumption pace of UI benefits – 2nd stage

	Consumption pace of UI benefits					
Taking the OR	-0.0005	-0.0020	-0.0045	-0.0525	-0.0392	-0.0337**
	(0.0035)	(0.0053)	(0.0055)	(0.0474)	(0.0241)	(0.0167)
<i>Distance</i>	-0.0000	0.0001	0.0000	-0.0003	-0.0001	-0.0005
	(0.0002)	(0.0002)	(0.0004)	(0.0007)	(0.0008)	(0.0011)
<i>Distance</i> × <i>DB</i> > 20	0.0002	0.0001	0.0000	0.0001	0.0000	0.0007
	(0.0004)	(0.0004)	(0.0007)	(0.0013)	(0.0014)	(0.0019)
<i>Distance</i> ²		0.0160	-0.0026		0.0103	-0.1613
		(0.0536)	(0.1561)		(0.1669)	(0.4011)
<i>Distance</i> ² × <i>DB</i> > 20		-0.0601	-0.0418		-0.0876	-0.1260
		(0.0605)	(0.1709)		(0.1862)	(0.4399)
<i>Distance</i> ³			-0.3092			-1.9835
			(1.6587)			(4.1936)
<i>Distance</i> ³ × <i>DB</i> > 20			0.7672			5.5196
			(2.5792)			(6.7610)
Age				-0.0003	-0.0002**	-0.0002***
				(0.0002)	(0.0001)	(0.0001)
Probability of being a woman				-0.0034	-0.0028*	-0.0023**
				(0.0028)	(0.0015)	(0.0010)
Level of education				0.0005	0.0004	0.0002
				(0.0005)	(0.0002)	(0.0001)
Observations	113,442	252,017	342,060	133,321	263,414	344,960
<i>Chi</i> ²	.098045	.002337	.250086	7.22367***	13.2424***	18.1381***

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

NOTE: This table reports the regression discontinuity regressions on the consumption pace of the UI right. I measure the consumption pace by taking the ratio of the paid unemployment spell duration over the total duration of the spell, including days the worker was not registered as unemployed. As the unemployment spell is defined as the addition of all days unemployed with interruptions lower than four months, it means that those interruptions are counted in the total duration of the spell (denominator). This measure represents how spread out in time was the paid unemployment spell. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector. Linear, quadratic and cubic specifications are used, with and without controls. The option right does not have a significant impact on the consumption pace.

Figure 1.20: Impact on the probability of exhausting the UI right



SOURCE: UI data (FNA).

NOTE: This graph reports the relationship between the probability to exhaust UI benefits and the running variable, which is the level of the former daily benefit. The right here refers to the new right for those exercising the option right, and to the remainder from the former right for those not exercising. If an eligible non-taker has exhausted the remainder and I do not observe that he recharges his right based on his last employment spells, I assume that he is not able to recharge, and has exhausted all his entitlements. If a recharging is observed for non-takers, I take into account the addition of the remainder and the new right. The probability jumps at the threshold from 4.1% to 5.7%.

Table 1.20: Probability of exhausting the UI right

	% reaching the end of the right at the end of the first U spell	% reaching the end of the right when taking into account all U spells
All takers	12%	13.7%
Takers with previous benefit $\leq 20\text{€}$	15.2%	17.2%

NOTE: The first column shows the probability of exhausting the right at the end of the first unemployment spell, and the second one takes into account all the unemployment spells.

Table 1.21: Impact of the option right on the probability to work within the spell – 2nd stage

	Has worked during the unemployment spell					
Taking the OR	0.586*** (0.019)	0.544*** (0.019)	0.551*** (0.013)	0.560*** (0.018)	0.558*** (0.012)	0.530*** (0.009)
<i>Distance</i>	0.012*** (0.001)	0.022*** (0.003)	0.027*** (0.005)	0.005** (0.002)	0.031*** (0.003)	0.033*** (0.004)
<i>Distance</i> \times <i>DB</i> > 20	-0.004* (0.002)	-0.030*** (0.005)	-0.040*** (0.009)	0.004 (0.004)	-0.042*** (0.005)	-0.047*** (0.006)
<i>Distance</i> ²		3.256***	6.752***		5.635***	8.794***
<i>Distance</i> ² \times <i>DB</i> > 20		1.361* (0.748)	2.057 (2.288)		-0.353 (0.802)	-0.636 (1.330)
<i>Distance</i> ³			55.873** (25.377)			70.558*** (12.051)
<i>Distance</i> ³ \times <i>DB</i> > 20			-138.273*** (38.668)			-137.535*** (17.940)
Age				0.003*** (0.000)	0.003*** (0.000)	0.003*** (0.000)
Probability of being a woman				0.039*** (0.003)	0.045*** (0.002)	0.041*** (0.002)
Level of education				-0.011*** (0.001)	-0.011*** (0.000)	-0.012*** (0.000)
Observations	177,324	249,863	312,648	112,307	242,463	378,615
<i>Chi</i> ²	60.3269***	78.7632***	160.224***	78.1139***	180.387***	310.681***

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

NOTE: This table reports the regression discontinuity regressions on the probability to work within the subsequent unemployment spell, while staying registered as unemployed. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector. Linear, quadratic and cubic specifications are used, with and without controls. The option right is predicted to increase the probability to work during the spell by about 55 percentage points.

Table 1.22: Impact of the option right on the labour income earned over the spell – 2nd stage

	Labour income earned over the UI spell					
Taking the OR	10169.913*** (2942.869)	8482.491*** (2529.745)	7895.241*** (2513.341)	11228.270*** (3010.538)	12082.698*** (3771.801)	9825.407*** (2725.136)
<i>Distance</i>	230.963*** (85.245)	148.822* (76.340)	174.664 (121.675)	261.771*** (80.268)	380.653*** (143.154)	329.383** (130.105)
<i>Distance</i> × <i>DB</i> > 20	229.243*** (56.558)	282.997*** (93.868)	159.010 (138.836)	244.673*** (65.333)	161.756 (106.778)	41.220 (120.415)
<i>Distance</i> ²		-4614.130 (8134.298)	11305.374 (34132.140)		33608.491* (17648.445)	48019.940* (28962.846)
<i>Distance</i> ² × <i>DB</i> > 20		-9256.213 (11361.954)	6970.926 (36801.474)		-46128.454* (22775.662)	-21500.964 (27087.584)
<i>Distance</i> ³			196701.662 (328101.499)			425981.760* (240393.268)
<i>Distance</i> ³ × <i>DB</i> > 20			-540273.331 (422189.706)			-852418.508** (354609.926)
Age				89.079*** (19.651)	93.130*** (23.264)	76.628*** (15.964)
Probability of being a woman				846.325*** (255.012)	893.783*** (319.599)	720.112*** (223.516)
Level of education				-120.564** (50.166)	-141.091** (62.868)	-101.505** (45.770)
Constant	384.974 (889.644)	870.550 (778.021)	1078.750 (767.644)	-2824.658* (1467.460)	-3088.746* (1770.849)	-1951.977 (1234.535)
Observations	75,089	122,926	146,431	80,358	113,464	160,851

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

NOTE: This table reports the regression discontinuity regressions on the labour income earned during the subsequent unemployment spell, while staying registered as unemployed. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector. Linear, quadratic and cubic specifications are used, with and without controls. The option right is predicted to increase the labour income earned during the spell by about 10,000€, depending on the specification. This result has been computed on the labour income conditional on having worker during the spell, which is itself endogenous to the option right. Therefore, we cannot exclude that there is a composition effect mixing with the pure effect on labour income.

Table 1.23: Impact of the option right on the number of hours worked during the spell – 2nd stage

	Number of hours worked over the UI spell					
Taking the OR	542.230*** (182.685)	1156.195** (586.864)	849.777** (329.987)	1161.665*** (284.847)	951.704*** (313.961)	947.692*** (242.135)
<i>Distance</i>	11.571*** (4.483)	27.795* (16.485)	34.998* (18.181)	37.223*** (9.599)	34.378*** (11.561)	39.570*** (11.236)
<i>Distance</i> × <i>DB</i> > 20	10.880*** (3.998)	30.974** (13.716)	-5.123 (14.808)	7.636 (6.464)	1.302 (8.528)	-9.617 (10.514)
<i>Distance</i> ²	798.061 (786.383)	7419.039 (5226.348)		3421.804*** (1284.477)	6697.557*** (2262.546)	
<i>Distance</i> ² × <i>DB</i> > 20	-3377.360 (2258.232)	-5045.137 (5903.856)		-3020.448* (1715.670)	-3027.011 (1995.811)	
<i>Distance</i> ³			80130.482			50814.027***
<i>Distance</i> ³ × <i>DB</i> > 20	-112022.104*	(56628.704)		-98618.918***	(17823.800)	
Age			(66640.554)	9.823*** (1.878)	8.268*** (1.930)	8.080*** (1.405)
Probability of being a woman				123.420*** (25.490)	108.596*** (26.849)	109.398*** (19.990)
Level of education				-23.043*** (5.159)	-19.729*** (5.319)	-19.673*** (4.237)
Constant	155.285*** (55.850)	-36.518 (180.110)	65.716 (99.346)	-302.887** (135.248)	-197.605 (146.394)	-189.012* (107.639)
Observations	91,773	187,140	132,776	58,910	113,238	165,823

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

NOTE: This table reports the regression discontinuity regressions on the number of hours worked during the subsequent unemployment spell, while staying registered as unemployed. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector. Linear, quadratic and cubic specifications are used, with and without controls. The option right is predicted to increase the number of hours worked during the spell by about 1,000 hours, depending on the specification. This result has been computed on the number of hours conditional on having worker during the spell, which is itself endogenous to the option right. Therefore, we cannot exclude that there is a composition effect mixing with the pure effect on the number of hours worked.

Table 1.24: Impact of the option right on hourly wage
from work during the UI spell – 2nd stage

	Hourly wage during the UI spell					
Probability of taking the OR	-46.105 (40.129)	-14.687 (79.195)	-72.557 (86.347)	-40.967 (42.273)	-59.447 (104.612)	-55.283 (105.729)
<i>Distance</i>	-1.303 (1.402)	-0.903 (2.733)	-4.545 (5.172)	-1.312 (1.507)	-3.991 (4.464)	-3.334 (6.169)
<i>Distance</i> × <i>DB</i> > 20	-2.236 (1.713)	-0.450 (2.770)	-0.084 (4.954)	-1.406 (1.503)	1.193 (2.616)	0.559 (4.026)
<i>Distance</i> ²		25.323 (325.333)	-1006.007 (1690.383)		-568.615 (602.902)	-349.371 (1649.230)
<i>Distance</i> ² × <i>DB</i> > 20		136.681 (472.667)	1789.684 (1863.429)		714.784 (752.184)	558.278 (1671.354)
<i>Distance</i> ³			-10019.537 (19721.536)			230.715 (16352.334)
<i>Distance</i> ³ × <i>DB</i> > 20			2185.521 (25557.422)			-1236.630 (19761.673)
Age				-0.212 (0.289)	-0.285 (0.653)	-0.249 (0.642)
Probability of being a woman				-5.172 (3.747)	-7.725 (9.029)	-6.427 (8.832)
Level of education				2.206*** (0.765)	2.912* (1.747)	2.763 (1.738)
Constant	31.380** (12.363)	21.430 (24.289)	38.705 (26.030)	26.169 (20.689)	30.794 (49.119)	28.483 (48.569)
Observations	42,264	108,269	119,543	43,755	102,290	137,647

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

NOTE: This table reports the regression discontinuity regressions on the hourly wage earned during the subsequent unemployment spell, while staying registered as unemployed. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector. Linear, quadratic and cubic specifications are used, with and without controls. Results are not significant, and, if anything, are negative. This result has been computed on the hourly wage conditional on having worker during the spell, which is itself endogenous to the option right. Therefore, we cannot exclude that there is a composition effect mixing with the pure effect on hourly wage.

Table 1.25: Impact on the probability to work during the UI spell (3-6 months horizon) – 2nd stage

	Probability of working while registered as unemployed					
	3-months horizon			6-months horizon		
Taking the OR	0.2206*** (0.0656)	-0.0144 (0.0400)	0.0571** (0.0233)	0.1980*** (0.0662)	-0.0016 (0.0401)	0.0462 (0.0338)
<i>Distance</i>	0.0058*** (0.0014)	0.0103*** (0.0028)	0.0108*** (0.0032)	-0.0004 (0.0017)	-0.0007 (0.0026)	-0.0078 (0.0049)
<i>Distance</i> × <i>DB</i> > 20	-0.0043 (0.0030)	-0.0246*** (0.0053)	-0.0256*** (0.0056)	0.0057 (0.0038)	-0.0091* (0.0047)	0.0051 (0.0088)
<i>Distance</i> ²		2.5772*** (0.6985)	3.2475*** (1.0830)		-0.6831 (0.5770)	-4.9108** (2.2310)
<i>Distance</i> ² × <i>DB</i> > 20		0.4418 (0.7677)	2.0488* (1.1368)		2.6640*** (0.6370)	4.6039* (2.3901)
<i>Distance</i> ³			26.0649*** (10.0207)			-62.6739** (27.2853)
<i>Distance</i> ³ × <i>DB</i> > 20			-68.2110*** (14.8480)			86.3566** (41.2891)
Observations	148,170	244,213	391,378	131,725	261,860	306,410

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

NOTE: This table reports the regression discontinuity regressions on the probability to work within the subsequent unemployment spell, while staying registered as unemployed, at different time horizons. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector. Linear, quadratic and cubic specifications are used. The option right is predicted to increase the probability to work during the spell by about 20 percentage points after 3 or 6 months, when looking at the linear specification.

Table 1.26: Impact on the probability to work during the UI spell (9-12 months horizon) – 2nd stage

	Probability of working while registered as unemployed					
	9-months horizon			12-months horizon		
Taking the OR	0.0882 (0.0766)	0.0038 (0.0428)	0.0470 (0.0352)	0.0143 (0.0790)	-0.0196 (0.0405)	-0.0441 (0.0371)
<i>Distance</i>	-0.0068*** (0.0024)	-0.0071** (0.0029)	-0.0208*** (0.0053)	-0.0095*** (0.0029)	-0.0144*** (0.0030)	-0.0317*** (0.0067)
<i>Distance</i> × <i>DB</i> > 20	0.0102*** (0.0039)	0.0002 (0.0054)	0.0234** (0.0094)	0.0103** (0.0043)	0.0124** (0.0055)	0.0423*** (0.0117)
<i>mphDistance</i> ²		-2.4811*** (0.7204)	-11.2583*** (2.5125)		-4.1991*** (0.8077)	-14.8993*** (3.8015)
<i>Distance</i> ² × <i>DB</i> > 20		3.5915*** (0.7891)	8.2821*** (2.6784)		4.1495*** (0.8847)	6.4790 (4.0488)
<i>Distance</i> ³			-141.6861*** (32.3290)			-178.7830*** (58.8951)
<i>Distance</i> ³ × <i>DB</i> > 20			201.0583*** (48.4002)			321.0733*** (88.7753)
Observations	115,992	242,514	294,999	102,231	226,253	248,942

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

NOTE: This table reports the regression discontinuity regressions on the probability to work within the subsequent unemployment spell, while staying registered as unemployed, at different time horizons. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector. Linear, quadratic and cubic specifications are used. The option right has no significant effect on the probability to work during the spell 9 months and 12 months after the beginning of the spell.

Table 1.27: Impact of OR on the capital of benefits earned over subsequent spell – 2nd stage

	Capital of benefits earned over the subsequent unemployment spell					
Probability of taking the OR	5735.506*** (494.465)	5571.235*** (896.127)	5960.234*** (1015.047)	6321.407*** (878.991)	5273.361*** (891.843)	6176.622*** (992.456)
Former DB	8.898 (11.353)	-47.871** (21.608)	78.612* (44.563)	39.836*** (15.370)	-48.382** (19.739)	15.059 (38.752)
Distance × DB > 20	165.399*** (29.895)	254.158*** (43.016)	56.930 (73.996)	139.837*** (14.907)	239.340*** (33.833)	198.620*** (58.657)
Distance ²		-20218.972*** (4482.165)	44521.812** (20002.448)		-20604.801*** (3002.545)	2004.344 (13835.613)
Distance ² × DB > 20		8124.313 (5563.741)	-21696.354 (24324.699)		10667.197*** (3982.959)	-14966.357 (16439.877)
Distance ³			902122.969*** (268513.586)			248384.981 (156207.638)
Distance ³ × DB > 20			-1309239.146*** (362653.699)			-201103.819 (224791.106)
Age				16.521*** (3.359)	11.803*** (3.294)	14.905*** (3.643)
Sex				240.224*** (58.463)	181.746*** (58.653)	241.034*** (64.032)
Level of education				4.368 (12.698)	21.378* (12.587)	8.893 (13.838)
Constant	1277.687*** (106.956)	1293.260*** (186.476)	1258.026*** (211.605)	430.076* (250.924)	696.945*** (255.417)	465.071* (281.843)
Observations	137,855	255,019	295,751	231,056	292,674	328,051

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

NOTE: This table reports the regression discontinuity regressions on the total amount of benefits earned over the subsequent unemployment spell. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector. Linear, quadratic and cubic specifications are used, with and without controls. The option right is predicted to increase the total amount of benefits earned by about 5,500€. This effect may come from the positive impact of the option right on the duration of the spell, or from the fact that benefits are higher for people exercising the option right.

Table 1.28: Impact of OR on the capital of benefits earned over subsequent period – 2nd stage

	Capital of benefits earned over the whole subsequent period					
Probability of taking the OR	4080.429*** (910.643)	3389.098*** (959.263)	2811.152*** (1062.149)	4712.023*** (992.527)	3261.864*** (1107.996)	3724.973*** (1196.554)
<i>Former DB</i>	87.826*** (24.810)	-30.843 (24.517)	41.174 (40.006)	39.076** (17.154)	-27.484 (27.429)	-57.377 (40.326)
<i>Distance</i> × <i>DB</i> > 20	7.480 (35.197)	176.409*** (53.711)	-25.913 (77.791)	130.191*** (17.491)	155.588*** (43.179)	254.629*** (59.126)
<i>Distance</i> ²		-16593.640*** (5910.029)	16741.330 (16549.398)		-15474.401*** (4510.802)	-32316.935** (12646.022)
<i>Distance</i> ² × <i>DB</i> > 20		11766.664* (6818.868)	30432.780 (20379.285)		15351.187** (6523.144)	8565.134 (17117.230)
<i>Distance</i> ³			386322.632* (198515.429)			-198943.763 (134968.096)
<i>Distance</i> ³ × <i>DB</i> > 20			-989522.292*** (298888.582)			412922.041** (182119.344)
<i>Age</i>				6.514* (3.801)	0.386 (4.147)	1.610 (4.364)
<i>Sex</i>				141.704** (66.039)	57.496 (72.897)	108.527 (76.040)
<i>Level of education</i>				44.547*** (14.262)	66.997*** (15.262)	63.084*** (16.586)
Constant	2612.877*** (188.068)	2696.487*** (201.415)	2860.706*** (223.520)	1838.047*** (284.685)	2242.281*** (320.988)	2078.247*** (339.044)
Observations	113,318	244,213	315,886	227,605	266,875	355,480

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

NOTE: This table reports the regression discontinuity regressions on the total amount of benefits earned over the whole subsequent period (October, 2014-May, 2017). Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector. Linear, quadratic and cubic specifications are used, with and without controls. The option right is predicted to increase the total amount of benefits earned by about 3,400€, when looking at the quadratic specification. This effect may come from the positive impact of the option right on the total number of days unemployed over the subsequent period, or from the fact that benefits are higher for people exercising the option right.

Table 1.29: Impact of having a DB lower or equal to 20€ on OR takeup by age categories

	Probability of taking the OR				
	Less than 25y/o	Between 25 and 34y/o	Between 35 and 44y/o	Between 45 and 54y/o	55y/o and more
<i>DB</i> > 20	-0.068*** (0.009)	-0.041*** (0.009)	-0.003 (0.007)	-0.002 (0.008)	0.007 (0.014)
<i>Distance</i>	0.004 (0.006)	0.007 (0.006)	-0.013*** (0.004)	-0.011*** (0.004)	-0.024*** (0.008)
<i>Distance</i> × <i>DB</i> > 20	-0.028*** (0.007)	-0.042*** (0.007)	-0.011*** (0.004)	-0.009** (0.004)	0.004 (0.010)
<i>Distance</i> ²	3.474*** (1.066)	2.949*** (1.129)	-0.469 (0.458)	-0.476 (0.431)	-2.821** (1.287)
<i>Distance</i> ² × <i>DB</i> > 20	-4.151*** (1.318)	-0.457 (1.356)	1.803*** (0.511)	1.479*** (0.473)	3.869** (1.541)
Constant	0.301*** (0.007)	0.272*** (0.007)	0.142*** (0.006)	0.125*** (0.006)	0.110*** (0.011)
Observations	84,033	88,596	80,529	67,611	19,734

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

NOTE: This table reports estimates from the first-stage regression of the probability to take the option right on a binary variable indicating whether the former daily benefit was lower than 20€. The regression has been run separately on different age groups. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector, with a quadratic specification. The jump in takeup at the threshold is the highest for youngest workers.

Table 1.30: Impact of OR on the paid unemployment duration by age categories – 2nd stage

	Paid unemployment spell duration				
	Less than 25y/o	Between 25 and 34y/o	Between 35 and 44y/o	Between 45 and 54y/o	55y/o and more
Probability of taking the OR	108.708*** (28.688)	172.948** (77.730)	6758.002 (75665.113)	-120.063 (705.040)	-1088.520 (1960.488)
<i>Distance</i>	-2.140 (1.474)	-3.407 (2.181)	96.305 (1119.308)	-8.426 (9.226)	-27.700 (41.441)
<i>Distance</i> × <i>DB</i> > 20	3.236 (2.113)	10.446*** (3.372)	65.690 (717.744)	6.673 (4.876)	5.186 (13.282)
<i>Distance</i> ²	-400.084 (274.238)	-626.804 (436.150)	4678.329 (57734.620)	-1191.924** (482.065)	-3326.945 (4960.384)
<i>Distance</i> ² × <i>DB</i> > 20	730.917** (311.605)	-287.943 (726.547)	-13851.048 (162553.779)	1152.240 (965.247)	4722.389 (7094.764)
Constant	68.928*** (7.524)	59.907*** (19.145)	-847.716 (10572.447)	106.993 (86.230)	232.822 (225.267)
Observations	75,326	75,128	81,378	46,876	19,210

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

NOTE: This table reports estimates from the regression discontinuity regression on the paid unemployment spell duration. The regression has been run separately on different age groups. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector, with a quadratic specification. The positive impact of the option right on unemployment duration is the highest for youngest workers.

Table 1.31: Impact of having a DB lower or equal to 20€ on OR takeup by education categories

	Probability of taking the OR			
	Less than high school completed	Vocational high school diploma	General high school diploma	Higher education
<i>DB</i> > 20	-0.015 (0.010)	-0.014* (0.007)	-0.074*** (0.009)	-0.049*** (0.011)
<i>Distance</i>	-0.017*** (0.006)	-0.003 (0.005)	0.020*** (0.007)	0.012 (0.008)
<i>Distance</i> × <i>DB</i> > 20	-0.005 (0.007)	-0.041*** (0.006)	-0.020** (0.008)	-0.018 (0.011)
<i>Distance</i> ²	-1.223 (0.957)	3.144*** (0.931)	5.243*** (1.421)	2.791 (1.725)
<i>Distance</i> ² × <i>DB</i> > 20	2.684** (1.111)	0.135 (1.127)	-9.547*** (1.752)	-5.688** (2.311)
Constant	0.134*** (0.008)	0.218*** (0.006)	0.240*** (0.007)	0.280*** (0.009)
Observations	41,378	106,990	74,622	53,655

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

NOTE: This table reports estimates from the first-stage regression of the probability to take the option right on a binary variable indicating whether the former daily benefit was lower than 20€. The regression has been run separately on different education groups. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector, with a quadratic specification. The jump in takeup at the threshold is the highest for highly-educated workers.

Table 1.32: Impact of OR on the paid unemployment duration by education categories – 2nd stage

	Paid unemployment spell duration			
	Less than high school completed	Vocational high school diploma	General high school diploma	Higher education
Probability of taking the OR	466.232** (230.608)	422.312 (272.430)	82.913*** (29.544)	69.425 (51.016)
<i>Distance</i>	5.208 (5.348)	-1.685 (3.589)	-5.767*** (1.412)	-5.377*** (1.258)
<i>Distance</i> × <i>DB</i> > 20	2.788 (2.658)	20.239** (9.285)	7.815*** (2.409)	7.408*** (2.855)
<i>Distance</i> ²	209.060 (456.467)	-1562.483*** (456.062)	-1155.649*** (328.933)	-904.847*** (292.659)
<i>Distance</i> ² × <i>DB</i> > 20	-500.012 (723.540)	19.502 (555.030)	946.287*** (352.415)	681.006*** (258.681)
Constant	36.492 (28.471)	8.310 (56.581)	80.660*** (5.984)	86.106*** (13.268)
Observations	48,547	122,221	71,890	62,523

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

NOTE: This table reports estimates from the regression discontinuity regression on the paid unemployment spell duration. The regression has been run separately on different education groups. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector, with a quadratic specification. The positive impact of the option right on unemployment duration is the highest for low-educated workers, while it is much smaller or even insignificant for highly-educated workers.

Table 1.33: Impact of having a DB lower or equal to 20€ on OR takeup by skill categories

	Paid unemployment spell duration			
	Executives and Intermediate occupations	Skilled employees	Skilled blue-collar workers	Unskilled employees and blue collar-workers
<i>DB</i> > 20	-0.075 (0.049)	-0.050*** (0.012)	-0.083*** (0.019)	-0.034*** (0.012)
<i>Distance</i>	0.024 (0.030)	-0.053*** (0.011)	0.063*** (0.015)	0.000 (0.007)
<i>Distance</i> × <i>DB</i> > 20	-0.035 (0.042)	0.036** (0.015)	-0.107*** (0.020)	-0.028*** (0.008)
<i>Distance</i> ²	3.949 (5.651)	-11.222*** (2.867)	21.260*** (3.590)	2.442** (1.000)
<i>Distance</i> ² × <i>DB</i> > 20	-5.663 (7.723)	7.043* (3.691)	-20.276*** (4.692)	-0.790 (1.119)
Constant	0.767*** (0.034)	0.334*** (0.009)	0.379*** (0.014)	0.167*** (0.010)
Observations	3,049	51,112	24,503	32,018

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

NOTE: This table reports estimates from the first-stage regression of the probability to take the option right on a binary variable indicating whether the former daily benefit was lower than 20€. The regression has been run separately on different skill groups. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector, with a quadratic specification. The jump in takeup at the threshold is the highest for skilled workers.

Table 1.34: Impact of OR on the paid unemployment duration by skill categories – 2nd stage

	Paid unemployment spell duration			
	Executives and Intermediate occupations	Skilled employees	Skilled blue-collar workers	Unskilled employees and blue collar-workers
Probability of taking the OR	-116.976 (224.824)	92.475** (39.330)	70.919 (69.631)	243.129* (125.769)
<i>Distance</i>	-3.189 (6.765)	-5.347*** (2.064)	-4.996** (2.028)	0.393 (2.328)
<i>Distance</i> × <i>DB</i> > 20	6.865 (14.199)	7.443*** (1.931)	5.501 (6.937)	4.356 (3.499)
<i>Distance</i> ²	-2.465 (1178.075)	-957.352*** (292.214)	-665.598 (736.808)	-381.651 (238.194)
<i>Distance</i> ² × <i>DB</i> > 20	-1095.082 (1402.311)	755.135** (377.181)	539.180 (421.949)	136.591 (315.879)
Constant	206.954 (164.601)	80.813*** (12.600)	84.753*** (23.085)	65.353*** (18.329)
Observations	3,389	73,836	29,092	33,303

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

NOTE: This table reports estimates from the regression discontinuity regression on the paid unemployment spell duration. The regression has been run separately on different skill groups. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector, with a quadratic specification. The positive impact of the option right on unemployment duration is the highest for low-skilled workers, while it is much smaller or even insignificant for highly-skilled workers.

Table 1.35: Impact of having a DB lower or equal to 20€ on OR take-up by sex categories

	Probability of taking the OR	
	Men	Women
<i>DB</i> > 20	-0.033*** (0.008)	-0.040*** (0.005)
<i>Distance</i>	0.007 (0.006)	0.008** (0.004)
<i>Distance</i> × <i>DB</i> > 20	-0.058*** (0.007)	-0.026*** (0.005)
<i>Distance</i> ²	4.312*** (1.010)	3.380*** (0.756)
<i>Distance</i> ² × <i>DB</i> > 20	0.258 (1.234)	-3.405*** (0.928)
Constant	0.291*** (0.007)	0.199*** (0.004)
Observations	99,766	180,525

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

NOTE: This table reports estimates from the first-stage regression of the probability to take the option right on a binary variable indicating whether the former daily benefit was lower than 20€. The regression has been run separately on different gender groups. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector, with a quadratic specification. The jump in take-up at the threshold is the highest for female workers.

Table 1.36: Impact of OR on the paid unemployment duration by sex categories – 2nd stage

	Paid unemployment spell duration	
	Men	Women
Taking the OR	111.228*	169.213***
	(65.755)	(39.555)
<i>Distance</i>	-4.683***	-4.381***
	(1.222)	(1.126)
<i>Distance</i> × <i>DB</i> > 20	8.288***	9.070***
	(2.939)	(1.845)
<i>Distance</i> ²	-897.015***	-947.606***
	(139.816)	(244.810)
<i>Distance</i> ² × <i>DB</i> > 20	468.853	617.769**
	(296.383)	(273.618)
Constant	66.755***	68.951***
	(17.611)	(6.930)
Observations	111,082	161,614

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

NOTE: This table reports estimates from the regression discontinuity regression on the paid unemployment spell duration. The regression has been run separately on different gender groups. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector, with a quadratic specification. The positive impact of the option right on unemployment duration is the highest for female workers.

Table 1.37: Predicted unemployment duration by takeup

	Controls	Takers	Non takers	Difference (3) - (2)
Predicted paid unemployment spell duration	138.429	152.294	167.861	15.567*** (0.397)
Difference between the actual and predicted U spell duration	-0.000	15.846	-77.080	-92.926*** (0.835)
Observations	224,169	24,136	96,779	120,915

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

NOTE: Unemployment duration has been predicted using information on age, gender, skill level (6 items), education level (10 items), tenure in past job, wage, sought occupation (81 items), working hours, separation motive (8 items), region (31 items), sector of activity (21 items), number of children, marital status, month of separation, occupation in past job (81 items), firm size, plant size, number of previous unemployment rights, daily benefit, potential benefit duration, average benefit level and duration based on previous unemployment rights. The regression has been made on job-seekers similar to those under study unemployed during the two years preceding the implementation of the option right (April 2013-March 2015). The coefficients are reused to compute the predicted unemployment duration for eligible job-seekers. All variables related to the UI right or to past job or firm are the ones corresponding to the previous UI right that was not exhausted, as the information is available for all eligible job-seekers, whereas information on the potential new right is available only for takers, and therefore cannot be used for the prediction.

Table 1.38: Summary statistics by predicted unemployment duration

	1 st quintile	2 nd quintile	3 rd quintile	4 th quintile	5 th quintile
Taking the option right	0.259	0.226	0.211	0.161	0.142
Actual paid unemployment spell duration	81.596	94.598	109.813	115.610	129.499
Observations	24183	24183	24183	24183	24183

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

NOTE: Unemployment duration has been predicted using information on age, gender, skill level (6 items), education level (10 items), tenure in past job, wage, sought occupation (81 items), working hours, separation motive (8 items), region (31 items), sector of activity (21 items), number of children, marital status, month of separation, occupation in past job (81 items), firm size, plant size, number of previous unemployment rights, daily benefit, potential benefit duration, average benefit level and duration based on previous unemployment rights. The regression has been made on job-seekers similar to those under study unemployed during the two years preceding the implementation of the option right (April 2013-March 2015). The coefficients are reused to compute the predicted unemployment duration for eligible job-seekers. All variables related to the UI right or to past job or firm are the ones corresponding to the previous UI right that was not exhausted, as the information is available for all eligible job-seekers, whereas information on the potential new right is available only for takers, and therefore cannot be used for the prediction.

Table 1.39: First-stage regression by predicted unemployment duration quintile

	Taking the option right				
	1 st quintile	2 nd quintile	3 rd quintile	4 th quintile	5 th quintile
$DB > 20$	-0.089*** (0.017)	-0.088*** (0.013)	-0.060*** (0.012)	-0.047*** (0.012)	-0.024** (0.010)
Former DB	0.012 (0.016)	0.038*** (0.011)	0.026** (0.010)	0.030*** (0.010)	0.006 (0.007)
Distance \times DB > 20	-0.014 (0.020)	-0.045*** (0.015)	-0.037*** (0.014)	-0.036*** (0.014)	-0.002 (0.011)
Distance ²	6.572* (3.855)	12.807*** (2.812)	8.726*** (2.612)	10.687*** (2.580)	3.617* (1.890)
Distance ² \times DB > 20	-9.437* (4.983)	-14.618*** (3.710)	-9.636*** (3.528)	-13.604*** (3.457)	-7.874*** (2.690)
Constant	0.228*** (0.013)	0.212*** (0.010)	0.190*** (0.009)	0.167*** (0.009)	0.109*** (0.007)
Observations	16849	28893	31882	30582	38903

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

NOTE: Unemployment duration has been predicted using information on age, gender, skill level (6 items), education level (10 items), tenure in past job, wage, sought occupation (81 items), working hours, separation motive (8 items), region (31 items), sector of activity (21 items), number of children, marital status, month of separation, occupation in past job (81 items), firm size, plant size, number of previous unemployment rights, daily benefit, potential benefit duration, average benefit level and duration based on previous unemployment rights. The regression has been made on job-seekers similar to those under study unemployed during the two years preceding the implementation of the option right (April 2013-March 2015). The coefficients are reused to compute the predicted unemployment duration for eligible job-seekers. All variables related to the UI right or to past job or firm are the ones corresponding to the previous UI right that was not exhausted, as the information is available for all eligible job-seekers, whereas information on the potential new right is available only for takers, and therefore cannot be used for the prediction. The first-stage regression is run separately on the different predicted unemployment duration quintiles. The jump in the option right takeup right decreases as the quintile increases, meaning that the higher the predicted unemployment duration, the more likely the job-seeker will opt for the longest coverage. It is indicative of adverse selection.

Table 1.40: Second-stage regression by predicted unemployment duration quintile

	Actual paid unemployment spell duration				
	1 st quintile	2 nd quintile	3 rd quintile	4 th quintile	5 th quintile
Taking the option right	100.677*** (36.980)	138.729*** (36.220)	262.650*** (71.009)	357.235*** (116.939)	921.300** (377.469)
Former DB	-7.870*** (2.710)	-6.671*** (2.076)	-2.967 (2.703)	-5.255 (3.492)	2.905 (5.985)
Distance × DB > 20	11.400*** (3.841)	10.989*** (3.800)	13.733*** (5.327)	19.829*** (7.271)	4.309 (10.314)
Distance ²	-1988.548*** (634.107)	-1658.623*** (571.038)	-766.851 (761.493)	-887.102 (1175.680)	-327.628 (1581.695)
Distance ² × DB > 20	1504.878** (734.063)	1333.645** (616.710)	-529.251 (832.291)	-406.480 (1153.479)	2652.591 (2042.548)
Constant	47.732*** (6.660)	56.278*** (6.012)	50.560*** (11.319)	50.496*** (16.648)	23.460 (36.626)
Observations	16849	28893	31882	30582	38903

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

NOTE: Unemployment duration has been predicted using information on age, gender, skill level (6 items), education level (10 items), tenure in past job, wage, sought occupation (81 items), working hours, separation motive (8 items), region (31 items), sector of activity (21 items), number of children, marital status, month of separation, occupation in past job (81 items), firm size, plant size, number of previous unemployment rights, daily benefit, potential benefit duration, average benefit level and duration based on previous unemployment rights. The regression has been made on job-seekers similar to those under study unemployed during the two years preceding the implementation of the option right (April 2013-March 2015). The coefficients are reused to compute the predicted unemployment duration for eligible job-seekers. All variables related to the UI right or to past job or firm are the ones corresponding to the previous UI right that was not exhausted, as the information is available for all eligible job-seekers, whereas information on the potential new right is available only for takers, and therefore cannot be used for the prediction. The second-stage regression is run separately on the different predicted unemployment duration quintiles. The positive impact on unemployment duration increases as the quintile increases, meaning that the higher the predicted unemployment duration, the more the job-seeker will suffer from the detrimental impact of the option right.

Table 1.41: Predicted unemployment duration by takeup (using the control group)

	Controls	Takers	Non takers	Difference (3) - (2)
Predicted paid unemployment spell duration	89.291	98.830	100.764	1.934*** (0.190)
Difference between the actual and predicted U spell duration	-0.000	69.311	-9.983	-79.293*** (0.798)
Observations	878679	24136	96779	120915

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

NOTE: Unemployment duration has been predicted using information on age, gender, skill level (6 items), education level (10 items), tenure in past job, wage, sought occupation (81 items), working hours, separation motive (8 items), region (31 items), sector of activity (21 items), number of children, marital status, month of separation, occupation in past job (81 items), firm size, plant size, number of previous unemployment rights, daily benefit, potential benefit duration, average benefit level and duration based on previous unemployment rights. The regression has been made on the control group made of job-seekers above the eligibility threshold (with a previous daily benefit between 20€ and 40€, and the coefficients are reused to compute the predicted unemployment duration for eligible job-seekers. All variables related to the UI right or to past job or firm are the ones corresponding to the previous UI right that was not exhausted, as the information is available for all eligible job-seekers, whereas information on the potential new right is available only for takers, and therefore cannot be used for the prediction.

Table 1.42: Summary statistics by predicted unemployment duration (using the control group)

	1 st quintile	2 nd quintile	3 rd quintile	4 th quintile	5 th quintile
Taking the option right	0.199	0.222	0.212	0.174	0.191
Difference between the actual and predicted U spell duration	9.683	10.186	8.484	3.724	-2.851
Actual paid unemployment spell duration	75.692	94.726	108.567	117.805	134.327
Observations	24183	24183	24183	24183	24183

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

NOTE: Unemployment duration has been predicted using information on age, gender, skill level (6 items), education level (10 items), tenure in past job, wage, sought occupation (81 items), working hours, separation motive (8 items), region (31 items), sector of activity (21 items), number of children, marital status, month of separation, occupation in past job (81 items), firm size, plant size, number of previous unemployment rights, daily benefit, potential benefit duration, average benefit level and duration based on previous unemployment rights. The regression has been made on the control group made of job-seekers above the eligibility threshold (with a previous daily benefit between 20€ and 40€, and the coefficients are reused to compute the predicted unemployment duration for eligible job-seekers. All variables related to the UI right or to past job or firm are the ones corresponding to the previous UI right that was not exhausted, as the information is available for all eligible job-seekers, whereas information on the potential new right is available only for takers, and therefore cannot be used for the prediction.

Table 1.43: First-stage regression by predicted unemployment duration (using the control group) quintile

	Taking the option right				
	1 st quintile	2 nd quintile	3 rd quintile	4 th quintile	5 th quintile
$DB > 20$	-0.063*** (0.016)	-0.067*** (0.012)	-0.059*** (0.013)	-0.062*** (0.012)	-0.055*** (0.010)
<i>Former DB</i>	0.039*** (0.014)	0.043*** (0.010)	0.025** (0.011)	0.033*** (0.010)	0.022*** (0.008)
$Distance \times DB > 20$	-0.067*** (0.018)	-0.054*** (0.014)	-0.046*** (0.015)	-0.032** (0.014)	-0.001 (0.012)
$Distance^2$	11.200*** (3.526)	14.637*** (2.633)	10.008*** (2.684)	9.293*** (2.645)	7.753*** (2.055)
$Distance^2 \times DB > 20$	-8.375* (4.494)	-15.654*** (3.484)	-8.719** (3.636)	-13.406*** (3.548)	-16.065*** (2.924)
Constant	0.201*** (0.012)	0.182*** (0.009)	0.186*** (0.009)	0.171*** (0.009)	0.161*** (0.008)
Observations	17758	29216	29124	28320	42691

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

NOTE: Unemployment duration has been predicted using information on age, gender, skill level (6 items), education level (10 items), tenure in past job, wage, sought occupation (81 items), working hours, separation motive (8 items), region (31 items), sector of activity (21 items), number of children, marital status, month of separation, occupation in past job (81 items), firm size, plant size, number of previous unemployment rights, daily benefit, potential benefit duration, average benefit level and duration based on previous unemployment rights. The regression has been made on the control group made of job-seekers above the eligibility threshold (with a previous daily benefit between 20€ and 40€, and the coefficients are reused to compute the predicted unemployment duration for eligible job-seekers. All variables related to the UI right or to past job or firm are the ones corresponding to the previous UI right that was not exhausted, as the information is available for all eligible job-seekers, whereas information on the potential new right is available only for takers, and therefore cannot be used for the prediction. The first-stage regression is run separately on the different predicted unemployment duration quintiles. The jump in the option right takeup right decreases as the quintile increases, meaning that the higher the predicted unemployment duration, the more likely the job-seeker will opt for the longest coverage. It is indicative of adverse selection.

Table 1.44: Second-stage regression by predicted unemployment duration (using the control group) quintile

	Actual paid unemployment spell duration				
	1 st quintile	2 nd quintile	3 rd quintile	4 th quintile	5 th quintile
Taking the option right	200.613*** (56.950)	218.869*** (51.403)	282.777*** (75.478)	216.078*** (77.970)	428.874*** (99.815)
Former DB	-9.401*** (2.397)	-6.594*** (2.195)	-7.994*** (2.871)	-8.721*** (2.982)	1.082 (3.370)
Distance × DB > 20	19.714*** (5.132)	14.967*** (4.533)	27.594*** (5.983)	17.676*** (5.782)	5.596 (6.236)
Distance ²	-2602.004*** (686.591)	-2095.931*** (697.441)	-1858.901** (834.255)	-1583.282* (875.789)	310.337 (945.696)
Distance ² × DB > 20	1034.359 (746.973)	1100.264* (640.525)	-1108.404 (909.808)	1270.635 (921.661)	166.694 (1155.254)
Constant	27.985*** (9.417)	45.103*** (7.574)	42.399*** (11.783)	68.011*** (10.886)	60.280*** (13.388)
Observations	17758	29216	29124	28320	42691

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

NOTE: Unemployment duration has been predicted using information on age, gender, skill level (6 items), education level (10 items), tenure in past job, wage, sought occupation (81 items), working hours, separation motive (8 items), region (31 items), sector of activity (21 items), number of children, marital status, month of separation, occupation in past job (81 items), firm size, plant size, number of previous unemployment rights, daily benefit, potential benefit duration, average benefit level and duration based on previous unemployment rights. The regression has been made on the control group made of job-seekers above the eligibility threshold (with a previous daily benefit between 20€ and 40€, and the coefficients are reused to compute the predicted unemployment duration for eligible job-seekers. All variables related to the UI right or to past job or firm are the ones corresponding to the previous UI right that was not exhausted, as the information is available for all eligible job-seekers, whereas information on the potential new right is available only for takers, and therefore cannot be used for the prediction. The second-stage regression is run separately on the different predicted unemployment duration quintiles. The positive impact on unemployment duration increases as the quintile increases, meaning that the higher the predicted unemployment duration, the more the job-seeker will suffer from the detrimental impact of the option right.

Table 1.45: Impact on the probability to be on paid unemployment – 2nd stage

	3-months horizon		6-months horizon		9-months horizon		12-months horizon		18-months horizon		24-months horizon	
	Quadratic	Cubic	Quadratic	Cubic	Quadratic	Cubic	Quadratic	Cubic	Quadratic	Cubic	Quadratic	Cubic
Taking the OR	0.2078*** (0.0478)	0.1909*** (0.0323)	0.1561*** (0.0504)	0.1229*** (0.0355)	0.0467 (0.0554)	0.0426* (0.0254)	0.0180 (0.0473)	0.0236 (0.0314)	0.0302 (0.0289)	-0.0085 (0.0190)	-0.0042 (0.0173)	-0.0044 (0.0181)
Distance	-0.0128*** (0.0037)	-0.0177*** (0.0052)	-0.0130*** (0.0032)	-0.0155*** (0.0050)	-0.0089** (0.0036)	-0.0139*** (0.0033)	-0.0077** (0.0030)	-0.0066 (0.0043)	0.0021 (0.0017)	0.0055** (0.0027)	0.0023** (0.0011)	0.0144*** (0.0037)
Distance × DB > 20	0.0299*** (0.0066)	0.0393*** (0.0092)	0.0232*** (0.0058)	0.0305*** (0.0090)	0.0070 (0.0066)	0.0207*** (0.0059)	0.0015 (0.0057)	-0.0011 (0.0077)	-0.0088*** (0.0030)	-0.0186*** (0.0050)	-0.0051** (0.0022)	-0.0250*** (0.0065)
Distance ²	-3.5039*** (0.9083)	-6.6233*** (2.1182)	-2.9937*** (0.7408)	-4.2272** (2.0766)	-2.1084** (0.9629)	-4.6210*** (1.1079)	-1.7617** (0.7872)	-1.7092 (1.8020)	0.5024 (0.3702)	2.6131** (1.1296)	0.6822*** (0.2512)	7.7553*** (2.2677)
Distance ² × DB > 20	0.5733 (0.9977)	0.7701 (2.2791)	1.1363 (0.8236)	-0.5168 (2.2312)	2.9833*** (1.0587)	2.0101* (1.1626)	3.1667*** (0.8649)	4.5455** (1.9497)	0.9357** (0.4138)	1.6609 (1.2345)	-0.2761 (0.2797)	-3.1481 (2.5115)
Distance ³		-54.0021** (23.4800)		-20.9624 (23.3291)		-35.3841*** (10.1010)		-6.1319 (20.6111)		31.1315** (12.6494)		107.2264*** (38.6363)
Distance ³ × DB > 20		98.6190*** (35.5156)		57.8253 (35.3779)		55.4538*** (15.0174)		-26.2095 (31.4515)		-72.7161*** (19.3777)		-170.7322*** (58.3858)
Observations	241,132	332,494	252,703	328,830	226,662	397,092	234,727	323,644	260,915	329,734	259,631	230,602
Chi ²	11.7766***	21.2465***	6.31485**	6.92009***	.006982	.001546	.863782	1.37003	.038178	3.65429*	1.4403	1.3924

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

NOTE: To ensure that results are not polluted by the way unemployment spells are constructed, I also look at the probability of being on paid unemployment at different time horizons after the decision to exercise or not the option right. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector. It shows that the main effect occurs in the short term.

Table 1.46: Impact on the probability to be registered as unemployed – 2nd stage

	3-months horizon		6-months horizon		Probability of being registered as unemployed				18-months horizon		24-months horizon	
	Quadratic	Cubic	Quadratic	Cubic	9-months horizon		12-months horizon		Quadratic	Cubic	Quadratic	Cubic
Taking the OR	0.1696*** (0.0396)	0.1901*** (0.0255)	0.1311** (0.0512)	0.1881*** (0.0380)	0.0668 (0.0579)	0.1452*** (0.0409)	-0.0267 (0.0557)	0.0321 (0.0422)	0.0254 (0.0399)	0.0163 (0.0308)	0.0080 (0.0237)	0.0035 (0.0293)
<i>Distance</i>	-0.0014 (0.0031)	-0.0028 (0.0046)	-0.0115*** (0.0036)	-0.0135** (0.0067)	-0.0174*** (0.0040)	-0.0174*** (0.0065)	-0.0172*** (0.0040)	-0.0208*** (0.0063)	-0.0015 (0.0024)	0.0035 (0.0044)	0.0010 (0.0014)	0.0141*** (0.0052)
<i>Distance</i> × <i>DB</i> > 20	0.0027 (0.0057)	0.0066 (0.0082)	0.0123* (0.0067)	0.0183 (0.0117)	0.0154** (0.0074)	0.0111 (0.0114)	0.0038 (0.0074)	0.0064 (0.0112)	-0.0109** (0.0045)	-0.0255*** (0.0079)	-0.0071*** (0.0026)	-0.0263*** (0.0090)
<i>Distance</i> ²	-0.8480 (0.7038)	-1.7438 (1.8039)	-3.1530*** (0.8990)	-5.3248* (3.1560)	-5.1812*** (1.0567)	-8.0996*** (3.0059)	-4.2910*** (1.0827)	-7.9517*** (2.9723)	-0.4326 (0.5516)	1.6804 (1.9267)	0.2538 (0.2761)	7.7549** (3.1132)
<i>Distance</i> ² × <i>DB</i> > 20	1.3449* (0.7847)	1.8230 (1.9534)	2.8016*** (0.9879)	3.6415 (3.3659)	4.8973*** (1.1600)	11.0649*** (3.2231)	6.9010*** (1.1799)	13.2430*** (3.1800)	2.9243*** (0.6148)	5.9547*** (2.0866)	0.8086*** (0.2965)	-3.9376 (3.3589)
<i>Distance</i> ³		-13.6074 (19.2251)		-39.6306 (40.6041)		-76.6972** (37.9685)		-68.4978* (37.9163)		24.5568 (22.9738)		108.1115** (50.7652)
<i>Distance</i> ³ × <i>DB</i> > 20		10.7796 (28.8773)		62.2969 (60.5933)		41.9751 (56.8020)		13.5053 (56.7292)		-96.8765*** (34.9394)		-147.1404* (76.5966)
Observations	257,301	345,006	243,420	294,999	231,523	299,521	226,662	297,513	258,620	313,022	290,872	238,444
<i>Chi</i> ²	7.66643	23.4778	1.47499	8.82385***	.005819	3.29802*	3.56611*	1.39604	.485599	1.21796	.956557	.901414

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

NOTE: To ensure that results are not polluted by the way unemployment spells are constructed, I also look at the probability of being registered as unemployed at different time horizons after the decision to exercise or not the option right. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector. It shows that the main effect occurs in the short term.

Table 1.47: Impact of the option right on paid UI spell duration before any interruption

Paid UI spell duration before any interruption						
Taking the OR	43.467*** (14.739)	47.275* (27.244)	25.198 (28.025)	54.875*** (18.729)	56.721** (28.244)	52.236 (32.524)
<i>Distance</i>	-3.017*** (0.380)	-4.694*** (0.652)	-4.567*** (1.046)	-2.586*** (0.382)	-4.506*** (0.653)	-5.728*** (1.124)
<i>Distance</i> × <i>DB</i> > 20	3.899*** (0.970)	6.548*** (1.285)	4.026** (2.033)	3.887*** (0.664)	7.097*** (1.131)	8.935*** (1.567)
<i>Distance</i> ²		-860.132*** (132.638)	-879.669** (428.485)		-838.266*** (107.740)	-1561.183*** (356.678)
<i>Distance</i> ² × <i>DB</i> > 20		619.760*** (165.119)	1404.368*** (517.837)		487.457*** (146.847)	780.695 (475.877)
<i>Distance</i> ³			-2144.272 (5057.106)			-10765.506*** (3859.849)
<i>Distance</i> ³ × <i>DB</i> > 20			-5910.606 (7565.569)			16948.210*** (4977.571)
Age				0.201*** (0.077)	0.223** (0.105)	0.203* (0.119)
Probability of being a woman				7.591*** (1.362)	7.494*** (1.855)	7.519*** (2.062)
Level of education				1.714*** (0.281)	1.855*** (0.396)	1.875*** (0.450)
Constant	60.049*** (3.225)	58.891*** (5.675)	64.031*** (5.901)	34.837*** (5.720)	32.242*** (8.150)	33.436*** (9.217)
Observations	131,725	257,301	318,926	152,044	276,512	352,827

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

NOTE: Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector, with a quadratic specification. This table reproduces the main regression on the paid unemployment duration by taking an alternative definition of the unemployment spell. The preferred definition used throughout the paper is the gathering of days registered as unemployed without any interruption of at least 4 months. The 4 months criterion has been chosen to ensure that the interruption reflects a stable return to work, and because it is the minimum working requirement to be able to open a new UI right. The definition chosen should not affect the result to the extent that we are comparing people at the direct neighbourhood of the 20€ cutoff, and that there is, *a priori*, no reason for the 4 months interruptions to be more or less frequent for a person earning 19€ or 21€ daily, apart from the effect of the option right. Still, it could be argued that if people exercising the option right are more prone to experience very short employment spells while still being registered as unemployed, it could inflate the duration of the unemployment spell as defined earlier, even though these small employment periods do not enter the counting of the unemployment spell duration. To alleviate this concern, I perform the same analysis on the duration of the spell before any interruption in payment (i.e. going from subsidised unemployment to non-subsidised unemployment is considered an interruption here). Estimates are smaller – which makes sense as the definition of the spell being more restrictive the average spell duration is lower – although significant in all specifications but the cubic one. Then, the measured impact on unemployment spell duration does not depend on the definition of the spell I choose to adopt.

Chapter 2

Unemployment Benefits and the Timing of Redundancies: Evidence from Bunching*

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Abstract

Most of the empirical literature related to unemployment insurance (UI) has focused on its impact on outflows from unemployment rather than on inflows. In this chapter, I show that workers respond to the design of UI while being employed. I exploit a discontinuity in the level of UI benefits at a particular value of tenure at current job. Using French administrative unemployment data, I analyse the concentration of workers in the tenure distribution at the relevant notch, a phenomenon known as bunching. The bunching mass is used to compute an elasticity of employment spell duration with respect to unemployment benefits. I find an estimate equal to 0.014 in my preferred specification, translating into a 0.5 day of extension for a 10% increase in the replacement rate. This estimate measures strategic behaviours attenuated by optimisation frictions. I identify the underlying mechanism as bargaining between employers and employees who maximise their joint surplus thanks to a state transfer. I find that the elasticity is the highest in the population facing the strongest incentives and in the highest occupations. This heterogeneity can be related to differences either in ability to bargain or in preferences.

JEL Codes: H30, J52, J63, J65, J68

Keywords: Unemployment, Behavioural response to taxation, Bunching, Collective bargaining

1 Introduction

Unemployment insurance (UI) spending accounts for a significant share of social spending in OECD countries, comparable to the share of family cash or incapacity benefits.¹ UI programs aim to reduce risk for individuals, helping them smooth their income and may act as an automatic stabiliser (Maggio and Kermani, 2016; Pareliussen, 2014). Besides their pure monetary cost, UI benefits make the outside option of employed workers more attractive and may influence the design of contracts by employers. The literature has mainly focused on the UI disincentive effect on the job search effort of the unemployed (Schmieder and Von Wachter, 2016). In parallel, many papers have explored the role of employment protection legislation (EPL) in enhancing job creation.² However, little is known about the impact of UI on the behaviour of employed workers and employers. The effect of UI on outcomes during employment is a relevant behavioural response since it can trigger potential costs that are usually not taken into account in the optimal UI framework. In times of high unemployment where the performance of UI schemes is highly scrutinised,³ accurately measuring their costs and benefits turns out to be of crucial importance.

In this chapter, I shed light on this often neglected aspect of UI, investigating how UI design affects the behaviour of employers and employed workers. I focus on its effect on the timing of the redundancy decision, *i.e.* when a firm facing economic difficulties decides to lay off part or the totality of its workforce. I exploit a notch created by a discontinuous jump in the level of UI benefits offered to laid-off workers above a tenure threshold. It allows me to measure whether the official contract termination is postponed when it benefits both employers and employees. The main contribution of this chapter is to highlight the existence of strategic behaviours from employers and workers in response to UI incentives, where UI is used to maximise the surplus from separation. This chapter also contributes to gaining insights into the employer-employee bargaining black box by analysing the individual and firm-level determinants of collusion between the worker and the firm.

I focus on a French unemployment program targeted at workers made redundant. Redundancies have attracted particular public interest as they often end up in mass layoffs.⁴ A UI package targeted at laid-off workers called *Contrat de sécurisation*

¹UI benefits weighed for around 1% of GDP on average in 2015, only including cash benefits.

²Theoretical arguments have been provided to support a relationship between EPL and job creation going on both directions (Addison and Teixeira (2003) for a review). Empirically, no consensus has been reached. The International Labour Organisation points out that “Irrespective of the methodology used, there is a general consensus that the impact of EPL on employment/unemployment level is rather mixed” (ILO, 2012). For instance, Bassanini and Duval (2006) find no significant effect of EPL on employment whereas Micco et al. (2006) find a negative impact.

³In many OECD countries (Spain, Italy, France, etc.), the unemployment rate is still higher than pre-recession levels.

⁴However, they account for a relatively small share of contract terminations (2.6% of all registrations to UI according to the French Unemployment Agency). In France, laid-off workers represent about 7% of flows into paid unemployment. They stay unemployed longer than the

professionnelle (CSP) was introduced in 2005⁵ in addition to the main UI benefits.⁶ It introduced a non-linearity in the compensation amount at a tenure threshold, which went from two years to one year in 2011. Laid-off workers having completed one year (respectively two years before 2011) of tenure are entitled to a specific benefit equivalent to 80% of their previous gross earnings.⁷ Those under this threshold only receive the standard benefit that represents 57.4% to 75% of previous gross earnings, depending on the past wage. This jump in the level of UI benefits incentivises workers to reach the tenure threshold before being laid-off.

This chapter documents the existence of a sizeable concentration of workers laid-off right after one year in the distribution of seniority at layoff, a phenomenon known as bunching. I argue that it is the incentives created by the UI that explain the observed bunching. This result is robust to several tests ruling out alternative hypotheses. I model the underlying mechanism as a bargaining process between employers and employees on the contract termination date. In this setting, I do not consider the potential impact of the UI benefits level on the layoff decision itself, but I focus on the timing of this decision. On the employee side, the expected UI payment can affect whether the employee proceeds with negotiation on the contract termination day. On the firm side, the employer may consider additional UI benefits as a way to offset the psychological and social cost of the redundancy for the worker. A significant financial compensation is likely to deter workers from harming their employer's reputation or from claiming extra-legal severance payment.⁸ This reduction in the layoff cost can compensate for the maintenance of the wage for a few additional days, whereas the employer does not support the direct cost of the CSP.⁹ French legislation defines minimum time periods between each step of the redundancy procedure. The scenario supported by this chapter is that, through these minimum time periods, the employer has some room to strategically extend the length of the procedure, and thus the length of the employment spell.

The particularity of my setting – a two-sided negotiation whereas the financial incentives lie mainly on the workers' side – and the heterogeneity I observe in the magnitude of bunching call for the introduction of a theoretical framework. It clarifies the cost and benefits of extending the employment spell on each side. This setting

average worker, and therefore represent 10% of the stock of unemployed people on benefits at any given time.

⁵Law №2005-32 of January, 18th, 2005 - art. 74 JORF January, 19th 2005

⁶The UI scheme in France is characterised by a main insurance benefit, the *Allocation de retour à l'emploi* (ARE), designed for all workers having lost their jobs unintentionally, and fulfilling very general and nonrestrictive conditions.

⁷It translates into virtually 100% of previous net earnings.

⁸There is a growing literature showing the sensitivity of firms to social pressure (Luo and Bhattacharya, 2006; Baron, 2011; Schmitz and Schrader, 2015; Bénabou and Tirole, 2010).

⁹The employer does not pay an additional contribution to fund the CSP scheme. He may pay an indirect cost if such a strategy leads to an increase in the reservation wage or in the unemployment rate, thereby leading to an increase in employers' contributions. However, given the number of employees concerned, this indirect cost can be considered second order.

motivates the need to take into account the interactions between the different agents, and to cast light on the bargaining process between employers and employees in a contentious context.¹⁰ I investigate the heterogeneity in bunching by a tentative decomposition of the factors explaining bunching into incentives, preferences and ability to negotiate.

To disentangle the role of financial incentives from other explanatory factors, I build upon the bunching methodology to compute a reduced-form estimate of the employer and employee's response to UI benefits in terms of contract length. I exploit the change in the seniority threshold from the first period (September, 2009 - August, 2011) to the second period (October, 2011 - September, 2014) using the first period distribution as a counterfactual. This method, known as the difference-in-bunching strategy (Brown, 2013), isolates the pure effect of the UI program, regardless of the role of other labour regulations or social norms that could trigger some bunching unrelated to the behaviour of interest.¹¹ It provides an estimate of the elasticity of contract extension to the level of UI benefits. This estimate is used as a metric neutralising the variation in financial incentives, and allowing other sources of heterogeneity to be explored.

This study contributes to the literature on the effect of taxes and benefits on labour supply. The behavioural response to non-linearities in the budget set, either through a discontinuity in the slope or in the level of the budget constraint, has been extensively studied in labour and public economics. Tax and transfer policies often lead to the creation of such kinks and notches, exhibiting a bunch at this point of the distribution. I draw on the growing bunching literature (see Kleven (2016) for a review) by identifying strategic behaviours at the level of the employer-employee pair. Important optimisation frictions have already been highlighted by several papers (Chetty et al., 2011; Kleven and Waseem, 2013). They attenuate the magnitude of bunching as predicted by the standard labour supply model, and account for the gap between observed and structural compensated elasticity of income with respect to the net-of-tax wage. I characterise here a new source of frictions, bargaining frictions, that makes the adjustment of contract length not necessarily coincide with what would be optimal for the worker.

Therefore, one contribution of this chapter is to provide insights into the employer-employee bargaining black box, by identifying individual and firm-level determinants of bunching. In particular, I find evidence that the rescheduling is part of an individual more than a collective negotiation process. The positive relationship between the presence of work councils and outcomes such as wages, job satisfaction or employment relationships has been documented (Addison, Schnabel and Wagner, 2004; Addison, Teixeira and Zwick, 2010; Hübler and Jirjahn, 2003; Grund and

¹⁰Redundancies often involve collective lay-offs and as a consequence, are highly covered by the media.

¹¹Round-number or psychological anchoring effect for example.

Schmitt, 2011; Grund, Martin and Schmitt, 2016). Yet, less is known about the relationship between the presence of representatives and more individual outcomes, especially when the layoff decision has already been taken, and the worker is close to leaving the firm. Grund and Martin (2017) show that work councils have a positive impact on the incidence of severance payment in the case of a plant closing, but a negative impact in the case of individual dismissal. My findings point to a non significant impact of representation institutions on the incidence of bunching. This may be explained by the fact that this outcome is highly dependent on individual parameters, and is not relevant for all laid-off workers.

A novelty of this chapter is that it analyses behaviours when employed, at the moment of the layoff. Indeed, up to now, most works have tended to emphasize the effect of UI design on unemployment outflows rather than inflows. A few papers have investigated the impact of UI on unemployment inflows, generally focusing either on the eligibility criteria to UI benefits (Van Doornik, Schoenherr and Skrastins, 2018; Rebollo-Sanz, 2012; Christofides and McKenna, 1995; Green and Riddell, 1997) or on changes in the potential benefit duration of older workers (Jäger, Schoefer and Zweimüller, 2019; Tuit and van Ours, 2010; Baguelin, 2016; Baguelin and Remillon, 2014). They show that the separation rate is positively affected by these two parameters through strategic scheduling of contract termination according to UI entitlements. I build on this existing evidence, widening the scope by not focusing solely on older workers, whose labour supply decision is likely to be particular, and influenced by retirement considerations. I am also able to distinguish between layoffs and resignations, which is crucial to interpret the results, as resignations do not open entitlements to the UI program under study. Observing layoffs allows me to explore the bargaining process between employers and employees and to decompose the driving factors of the extension decision. In my setting, UI does not affect the decision of layoff itself, but its timing, conditional on the decision having been taken. It means that the incentives for the employer to grant the extension of the contract are even less straightforward, as the layoff decision has made it clear that the match was not profitable anymore. In this context, UI may be used as a way to soften the conditions of the redundancy, thereby leading to several types of inefficiencies.¹² I am therefore interested in the conditions that make employers and employees collude to maintain a match that has turned non-productive. By performing a heterogeneity analysis, I describe the characteristics of the retimed matches to gain insights into those conditions.

This chapter also relates to the literature on the optimal unemployment insurance, as the elasticity of unemployment duration to the level of UI benefits is one of the

¹²For example: (i) the maintenance of a poor match some additional days; (ii) the employer using this third-party compensation, whose cost does not enter his utility function, to ease the layoff conditions; (iii) the covering by UI of people who should not have been covered, which increases UI spending mechanically and indirectly through a possible longer unemployment duration.

parameters entering the sufficient statistics formula (Baily, 1978; Chetty, 2006). This optimality condition equalises the moral hazard cost of transferring one euro from the employed to the unemployed state, to the benefits of such a transfer, in terms of consumption smoothing. I focus on the impact of the level of UI benefits in France, where this topic has been rarely studied since the sources of variation are scarce.

A final contribution of the chapter is methodological. It warns against the use of eligibility thresholds in quasi-experimental methods (*e.g.* regression discontinuity design), as they can be manipulated even in some contexts where researchers might not particularly expect it.

The remainder of this article is organised as follows: Section II gives an overview of the legislative framework, Section III presents the data and provides empirical evidence of bunching. Section IV elucidates each party's theoretical costs and incentives, while Section V describes the bunching method and its implementation, and analyses the sources of variation of the bunching intensity. Section VI provides some robustness checks. Section VII derives the welfare implications and Section VIII concludes.

2 Institutional Background

The UI program under study is targeted at laid-off workers. In France, when an employer wants to separate from a worker under a permanent contract after the probation period, he has two main options: dismissal for personal reasons and layoff for economic reasons. The first motive is linked to the behaviour of the person dismissed whereas the second motive is justified by economic difficulties faced by the firm. In both cases, the reason has to be clearly stated and supported by objective and verifiable facts. As the separation motives are very different between the two separation options, the concerned populations are likely to differ as well. The population of laid-off workers is mainly composed of workers in shrinking industries, on average older, more frequently male, less educated but with a higher wage and compensation duration than the other compensated workers (Unedic, 2015a). This is the reason why a specific benefit package called CRP (*Convention de reclassement personnalisée*) was introduced on April, 5th, 2005¹³ for laid-off workers in addition to the main UI benefits. It was in effect during my first period of interest (September, 1st, 2009-August, 31st, 2011). It was then transformed into CSP (*Contrat de sécurisation professionnelle*), in effect during my second period of interest (October, 1st, 2011-September, 30th, 2014). Any changes taking place after this date are not taken into account, in particular the reform enforced on April, 1st, 2015 which introduced many modifications to the scheme.

¹³Law N°2005-32 of January, 18th, 2005 - art. 74 JORF January, 19th 2005

The two schemes have been designed in the same spirit, as a way to secure the professional path of laid-off workers by offering comprehensive and personalised support to help them reintegrate the labour market as soon as possible, and in good conditions. They give access to career coaching, training, assistance in the design of a professional project and even psychological support. Besides these aspects, one of their main characteristics is to introduce a non-linearity in the compensation amount at a tenure threshold. This threshold went from two years to one year in 2011. While all the laid-off workers are entitled to the coaching and training components of the CSP-CRP, laid-off workers having completed one year (respectively 2 years for the CRP) of tenure are entitled to a specific benefit equivalent to 80% of previous gross earnings (which translates into virtually 100% of previous net earnings). This specific benefit is called ASP. Those under this threshold only receive the standard benefit, whose replacement rate lies between 57.4% to 75% of previous gross earnings, depending on the past wage.

In this chapter, I focus on the CSP,¹⁴ and I use the CRP only for comparative purposes. In the following paragraph, the legislative rules regarding the CSP will be detailed, as the rules that apply to the CSP also apply to the CRP (the main change being on the seniority criteria to benefit from the higher compensation). A detailed description of the legal layoff timeline is provided in Figure 2.1.

Scope of the CSP

In firms with less than 1,000 employees or in compulsory liquidation or receivership (whatever the workforce size), employers are bound to offer the CSP to any employee they want to lay-off, during the interview prior to layoff or after the last meeting of employees' representatives. It should be noticed that workers laid-off for economics reasons are necessarily workers in open-ended contracts.

To benefit from the CSP, the worker must also meet the following criteria: (i) having worked at least 122 days or 610 hours within the last 28 months; (ii) not having reached the compulsory retirement age; (iii) to live on the territory where the unemployment insurance is applicable; (iv) being physically able to work. Those criteria also apply to receive the standard benefit.

The seniority condition does not determine the eligibility to the CSP, but to higher benefits (ASP). Under one year of seniority, the worker is only entitled to receive the standard benefit (ARE) and to benefit from the counselling and training dimensions of the CSP.¹⁵

¹⁴This choice is justified by the fact that the legislation has been more stable and with clearer incentives during the period the CSP was in place. Indeed, the CRP benefit has been gradually decreasing for a certain period of time, and the total compensation length has been extended in 2009.

¹⁵If he chooses to accept it. The CSP is offered to any eligible worker but he decides ultimately if he accepts it or if he just gets the standard compensation scheme.

Steps of the legal procedure

A quick description of the different steps is provided, as this information will be useful later to understand whether employers and employees have room to strategically delay layoffs.

The procedure for redundancies implies several steps, whose number depends on the workforce size and the number of people laid-off. It involves meeting and discussing with employees' representatives, when they are present in the firm, and respecting minimum periods of time between each step. The whole procedure is monitored by the Health and Safety Inspection.

In the concerned firms, the employer, after having announced the economic layoff plan and discussed with the employees' representatives, must offer the CSP, individually and in a written way, to any eligible worker, either during the interview prior to layoff, or after the last meeting of the employees' representatives, or after the approval of the redundancy plan, if any. The employee has a 21-day period to take his decision: if he refuses, he gets the standard benefit scheme; if he accepts, the work contract terminates at the end of the 21-day period, without any advance notice.

The CSP is organised, over a maximum period of 12 months, as a path back to employment, through intensive counselling and coaching, and possibly through a career change or the creation of an enterprise. During the whole process, the unemployed worker benefits from regular meetings with his dedicated counsellor, including a skills' assessment, the formulation of a professional project, a social and psychological support, training, advice for job interviews, etc.

If the guidance and counselling dimensions can be seen as either attractive – as additional resources to accelerate the return to employment – or troublesome – felt like ways to monitor the worker too closely – the additional benefit offered to workers eligible to the CSP and having completed at least one year of seniority creates strong financial incentives to go beyond this one-year seniority threshold. However, as the layoff decision and its timing are in the hand of the employer, it is not up to the employee to decide whether he completes his seniority year. In theory, the layoff decision, in the setting we are interested in, is only motivated by the economic difficulty of the firm, and should affect workers within the enterprise, if not randomly,¹⁶ at least not according to some sharp eligibility thresholds.

Two important consequences of the acceptance of the CSP should be noted: First, as soon as the 21-day period ends, the worker starts to be compensated without any

¹⁶In setting up a collective redundancy plan, the employer has to follow some criteria to determine which workers will be laid-off in priority. If the last-in-first-out rule applies, many other determinants are taken into account. Among them, there are the family load, the seniority, social characteristics making the return to work difficult, the professional skills, etc.

waiting period or notice, on the basis of the standard benefit or the ASP, depending on his seniority. Second, for workers accepting the CSP, the breach of the work contract is no longer considered a layoff, but a mutually agreed termination, which may imply less administrative constraints for the employer in the future. The worker is still entitled to receive severance payments and to appeal Labour Court, but is much less likely to do so. This reduction in trial risk can be considered a reduction in the layoff cost.

The different possibilities and their consequences are summed up in Table 2.1.

A more detailed presentation of the consequences of accepting the CSP and how they are valued can be found in Table 2.33 (Appendix 1). Redundancies can be either collective or individual. Collective redundancies are defined as the layoff of more than one employee within a 30-day period. The main steps of the legal procedure are essentially the same in the collective and in the individual case, although some requirements depend on the number of persons laid-off. For instance, if the employer plans to lay-off two to nine employees, he has to organise a meeting with the work council, whereas there is no such an obligation for an individual layoff. As a more general information on the legal framework, it should be mentioned that all laid-off workers eligible for this UI program work under permanent contract. In addition, although there is a probation period for permanent contracts, its maximum duration is equal to eight months (only for executive workers in case it is renewed). It means that a spike in the separation probability at one year could not be rationalised by regularity in fixed-term contract or probation period duration.

3 Empirical Evidence of Bunching

3.1 Data

I use administrative data (*Fichier national des allocataires*, FNA) collected by the organisation in charge of the unemployment insurance in France, the *Union nationale interprofessionnelle pour l'emploi dans l'industrie et le commerce* (Unédic) for the years 2009 to 2014. More precisely, I focus on two sub-periods, for comparative purpose, gathering respectively the contract terminations for economic reasons occurring between September, 1st, 2009 and August, 31st, 2011, and October, 1st, 2011 and September, 30th, 2014. I select only the contract terminations for economic reasons opening entitlements to the CSP, that are contract terminations for economic reasons in firms of less than 1,000 employees, or firms in compulsory liquidation or receivership (whatever the workforce size). Although the total number of workers laid-off as part of a redundancy represents a relatively small share of total separations, it is equivalent to about 10% of entries into subsidised unemployment. My sample is made of all workers eligible to the CSP, accounting for 482,497 observations in the

first period, and 636,350 observations in the second period. The unit of observation is the layoff.

The CSP is offered to any eligible laid-off workers: if he chooses to accept it, he will benefit from the package for a maximum of 12 months. If he has not found a job by the end of the 12 months, he can switch to the standard compensation scheme (ARE), if he was initially entitled to a compensation duration greater than 12 months (meaning that his work history was greater than 12 months before the contract termination). Then, the compensation duration is computed by subtracting the duration of the CSP to the initial compensation duration.

In my analysis, I consider the whole compensated period by reconstructing the unemployment spell: it corresponds either to the addition of the CSP duration and the potential compensation period under the standard benefit that immediately follows, or directly to the standard benefit compensation period if the worker has rejected the CSP. In some cases, the unemployed person has experienced several episodes during the same unemployment spell, either because he found a temporary job and resumed UI compensation after, or because he interrupted compensation for sickness or other motives. As the information on the reason why the unemployed person left the UI register is not entirely reliable, I chose to gather within the same spell the episodes separated by a period shorter than the minimum work history required to open a new entitlement to UI benefits (i.e. four months). I do not, however, add these periods to my computation when counting the total duration of the compensated spell.¹⁷

Another limit of the data is that I do not directly observe the return to work: the only variable I am able to measure is the duration of compensated unemployment, which is an imperfect proxy for the return to work, as leaving unemployment does not necessarily mean that the worker has found a new job (Card, Chetty and Weber, 2007). It means that I cannot have information on the future employment spells after the layoff. I cannot say anything on the quality of the job found after the layoff, or on the probability of being rehired by the same employer for example. However, as the return to work is not my main outcome of interest, I can still use the duration of compensated unemployment to gain insights into the effect of the CSP on labour supply.

My main variable of interest is the tenure variable: I need a precise and reliable measure of the density of the tenure at current job on a daily basis to understand what happens at the one-year threshold. The information on tenure comes from the certificate delivered by the employer either to the employee for him to receive unemployment benefits, or directly to the employment agency (*Pôle Emploi*). As this

¹⁷Although this choice can be discussed, we can argue that these breaks generally correspond to small employment periods – and then should not be counted in the unemployment spell duration – or to periods where the unemployed person was not able to look for a job – for sickness or maternity for example.

certificate is mandatory for the employee to be compensated and the information on tenure determines the way the worker will be compensated, this information is closely monitored by the employment agency and can be deemed reliable. The contract termination occurs at the end of the reflection period granted to the employee eligible to the CSP, no matter his answer. Yet, I find some inconsistency in the data between the end of the contract date and the beginning of the advance notice period: indeed, in case the worker refuses the CSP, he will receive the standard benefit, and will potentially carry out a notice period, that is a period when he knows he is laid-off, but he continues to work and to be paid. This period is a way to give the worker some time to find a new job while continuing to receiving a wage. In some cases, the notice is not carried out but the worker still gets the corresponding earnings. Adjusting for this discrepancy by subtracting the notice duration to the tenure value does not affect the results. I then choose to keep the raw tenure variable for everyone.

A simple descriptive analysis of my sample indicates that the CSP takeup rate is much lower below one year of tenure (26%) than above (56% between one and two years of tenure). Workers accepting the CSP are on average, older, more frequently women, more educated, more skilled, and working in larger firms in the service or manufacturing sector (Table 2.2). The multivariate regression (Table 2.3) reveals that individual characteristics play a bigger role in explaining the decision to accept the CSP than firm characteristics.

3.2 Documentation of the Bunching

First evidence that some manipulation occurs at the one-year seniority threshold is provided by the [McCrary \(2008\)](#) test which analyses the difference in log density between both sides of a specific threshold. This test is traditionally used in regression discontinuity designs to make sure that the running variable has not been manipulated. The [McCrary \(2008\)](#) test on the seniority density highlights the discontinuity at the strategic one-year threshold.

Figure 2.2 provides striking graphical evidence of bunching just above the 365 days threshold for our period of interest. It shows a significant 36% increase in the density at the cutoff, with a hole at the left side and a mass at the right side, two distinctive features of bunching.

Although this graph provides evidence of some concentration of workers at one year, I cannot yet rule out the possibility that this pattern is due to other factors than the CSP. First note that all the contract terminations for economic reasons examined in my sample are open-ended contracts, meaning that this pattern cannot be due to some regularity in the duration of fixed-term contracts. Second, the observed spike cannot be accounted for neither by the existence of renewable trial periods, as the maximum duration that can be reached corresponds to eight months (for executive workers).

We can still distinguish several explanations to the spike in the density that we observe at the cutoff, either through psychological mechanisms (anchoring phenomenon on a reference point) or legislative feature (the 365-days cutoff can serve as a threshold for other policies). I argue that this concentration is indeed due to strategic behaviours in response to the incentives introduced by the CSP, and that it is not just an administrative or psychological reference point. Three pieces of evidence support this scenario.

I exploit the fact that a similar package existed before the introduction of the CSP in September, 2011, the CRP. The main difference with the CSP is that, to qualify for the higher benefit, the requirement is *to have at least two years of tenure* instead of one. I then perform a placebo test on the period *preceding the introduction of the CSP* by plotting the distribution of the seniority density at the same threshold, when the CRP was in effect (Figure 2.3). Reassuringly, we observe a discontinuity in the tenure density at two years in the period September, 2009 - August, 2011, which disappears completely after the CRP has been replaced by the CSP. The density jumps by 32% at the cutoff, and the discontinuity is significantly different from zero. No or a much smaller discontinuity is, however, observed respectively at the one-year and two-year seniority cutoffs for the 2009-2011 and 2011-2014 periods. It means that when no incentives created by the UI benefits schedule exist at some points of the seniority distribution, no bunching is observed. This finding needs to be qualified by the fact that I find some evidence of a small discontinuity at the one-year seniority threshold for the 2009-2011 period that is necessarily explained by some factors unrelated to the CRP/CSP: although much smaller in magnitude (a 16% jump instead of a 36% jump after the introduction of the CSP), it should not be neglected, and it calls for the use of a difference-in-bunching methodology, as I will further explain in sub-section 5.1.2.

This spike at the two-year cutoff can be interpreted as a sign that the bunching is a response to the CSP and CRP packages, as, if it would have been linked to any other feature of the legislation related to the one-year seniority cutoff, it would not have been observed at the two-year cutoff in the 2009-2011 period. Similarly, after the introduction of the CSP, the discontinuity at the two-year cutoff disappears while the discontinuity at the one-year threshold starts appearing (Figure 2.3). All in all, having discontinuities in the seniority density at the threshold corresponding respectively to the CSP and CRP legislation for the relevant period, and, conversely, not observing these discontinuities for the period the CSP or the CRP are not applicable are all elements converging toward the strategic behaviour scenario.

In addition to observing a shift in the discontinuity location at the relevant thresholds respectively for the CRP and the CSP, we notice that this shift occurs rapidly. In the first semester after the introduction of the CSP, we already observe a sharp decrease in the two-year discontinuity and an important increase in the

one-year one (Figure 2.4). In the following semesters, the discontinuity at one year goes on increasing and then stabilises, while the one at 730 days almost disappears. We also see this gradual evolution in Table 2.4 and in Figure 2.5. The share of the bin gathering seniority values within the bunching area¹⁸ relative to the whole distribution considerably rises at the introduction of the CSP.

The immediate translation of the legal scheme into a change in the seniority distribution corroborates the hypothesis that the behavioural response is driven by the CRP-CSP incentives.

To be fully convinced that the bunching we observe is not just linked to another feature of the legislation or to a round-number effect (employers when reporting the date of contract termination choose it so that they have a round-number seniority), I perform another placebo test by plotting the seniority density of all the persons registered as unemployed after the termination of an open-ended contract, excluding economic layoffs. Figure 2.6 does not exhibit any significant discontinuity at the relevant threshold, confirming my hypothesis.¹⁹

A last concern could be that the discontinuity observed in the population of eligible laid-off workers registered for UI would not be observed in the total population of eligible laid-off workers. It means that the bunching would be due to a discontinuity in the UI takeup rate while the separation rate would be smooth at the threshold. It is unlikely, as even those right below the one year threshold are still entitled to a significant level of UI benefits²⁰ for at least 12 months. Still, to rule out this hypothesis, I need to rely on another data source. I use the MMO (*Mouvements de Main-d'oeuvre*) data, which gathers the quarterly forms any employer in a firm of 50 employees or more has to return to the administration. It describes all the employment flows, with information on the starting and ending dates of the contract, on the type of contract, and on the separation motive. I plot the same tenure distribution for laid-off workers in firms of less than 1,000 employees. Reassuringly, Figure 2.8 shows a similar bunching at the one-year cutoff. Although UI registration rate might differ from one side of the threshold to the other, Figure 2.8 ensures that the separation rate is indeed discontinuous at the threshold.

¹⁸The bunching area has been defined visually as the [365;397] days bracket.

¹⁹Not identifying any discontinuity at the one-year threshold among all open-ended contracts while identifying some bunching at the same threshold under the pre-CSP period could seem puzzling. Indeed, if the bunching observed among laid-off workers under the pre-CSP period is unrelated to UI incentives, it should appear similarly among other types of open-ended contracts layoffs. One explanation could be that the pre-CSP bunching is related to a round-number effect that exists in the case of other open-ended contracts but might be attenuated by the fact that severance payment start to be paid at one year of seniority. In case of more individual layoffs where other incentives – like the CSP – do not interfere, employers could be more prone to dismiss right before the one year threshold, potentially reducing the round-number spike at one year.

²⁰With a gross replacement rate between 57% and 75%.

3.3 Underlying Mechanisms: Exploration of the Bargaining Process

My preferred scenario to justify the excess mass in layoffs after one year of tenure is that, conditional on the layoff having been decided, employers and employees bargain over the date of contract termination as they have both incentives and room to do so. On the worker's side, differences in preferences interact with differences in ability to negotiate and in incentives, whereas on the employer's side, the cost of extending the work contract and then of paying employees additional weeks or months while knowing that they would leave the firm eventually is also likely to change among employers.

Profile of the typical buncher

To investigate in further details the type of employees and employers concerned by this bunching phenomenon, I produce two types of graphs (Figures 2.9 to 2.21). The first ones are testing whether there is a discontinuity at the cutoff in the distribution of some observable characteristics, and the second ones are specifying for which values of these observable characteristics the discontinuity in the seniority density is the highest. In other words, the first graphs indicate in which dimensions the populations on each side of the cutoff differ on average. The second graphs reproduce the McCrary (2008) test on the seniority density by sub-populations.

The main insights from these visual elements are that bunchers are typically working more frequently full-time, with a high level of education, are more skilled, with a higher wage as compared to those below the threshold, and are more frequently female (Figures 2.9-2.18). The discontinuity is also slightly more pronounced in small or intermediate firms, whereas no strong pattern emerges in terms of sector (Figures 2.19 to 2.21).

Table 2.5 provides numerical evidence – from the McCrary (2008) test – of the stronger magnitude of the discontinuity for some sub-populations, to refine the profile of the typical buncher. All the characteristics of the typical buncher may be associated with a higher bargaining power, which is compatible with my preferred scenario.

Layoffs order

As explained in the description of the institutional background, layoffs can be either collective or individual. More precisely, I observe in my sample that among all firms laying-off at least one worker during the period of observation, 41.2% of them had laid-off at least two individuals. It corresponds to 76.8% of laid-off workers in the sample.²¹ I can use these collective layoffs to infer what is the underlying

²¹A more detailed description of the distribution of the redundancy size can be found in Table 2.6.

mechanism driving bunching.

If the bargaining scenario was true, we might observe that people laid-off as part of the same collective layoff plan would have different end of the contract dates. In particular, we might have people right above the cutoff laid-off later than those far from the threshold (either above or below) and for which it is useless or too costly to manipulate the seniority.

Spotting firms for which several layoffs are observed for the period October 2011-September 2014, I can count the number of people laid-off and look at the layoff order. All the layoffs observed from the same employer over a period of 30 days (the legal criteria to consider a layoff as collective is to have several layoffs on a 30-day period) are gathered into the same layoff spell to get the order, by date of contract termination, of each layoff in the same spell. Indeed, I find that having a seniority lying between 365 and 380 days (corresponding to the area right above the cutoff) is associated to a lower probability of being laid-off first as part of a collective layoff plan (- 12.8ppts). However, it is also associated with a higher probability of being laid-off second (+10.4ppts). The rest of the distribution is rather similar (as observed on Figure 2.22). As the median of the rank variable is two, being in the second position means, in 50% of the cases, being the last person laid-off in the layoff plan. Thus, it appears that workers close to the cutoff are indeed more frequently laid-off later in the layoff plan. The results are robust even if we make the window above the cutoff vary, as illustrated in Table 2.7.

I also look at the relationship between the proportion of persons whose layoff seems strategically delayed and the value of seniority at the beginning of the layoff spell. To construct Figure 2.23, I identify individuals: (i) laid-off as part of a collective layoff plan; (ii) not laid-off first (which suggest a waiting time); (iii) laid-off just (i.e. within 10 days) after reaching the one year condition (which suggests the waiting time was related to the CSP). Figure 2.23 plots the proportion of people fulfilling these three conditions among people fulfilling conditions (i) and (ii) as a function of tenure at the moment of the first layoff within the same collective redundancy. This proportion indeed goes up as the gap between the seniority value at the moment of the first layoff and the cutoff closes. This result suggests that the cost of waiting strategically increases with the initial distance to the cutoff.

This finding also confirms that the excess mass right above the one-year threshold comes from the area right below the threshold – which is consistent with hole visible at the left-hand side of the cutoff (Figure 2.2) – in line with the hypothesis of strategic bargaining for people just below one year of seniority.²²

Cost of extending the contract

²²Another scenario could be that employers falsify the contract termination date to make workers better-off. But this is not consistent with my finding as, in that case, they would not necessarily choose workers with true seniority just below the cutoff.

In the bargaining scenario, we must consider the trade-off between the benefit for the employee to move up the 365-day cutoff and the cost for the employer to extend the contract. A likely hypothesis is that this cost varies among employers, and might be higher for firms that have economic difficulties but continue their activity than for firms definitively shutting down, as for these firms, paying some workers additional weeks or months will not change the final outcome, whereas it can put in jeopardy firms trying to overcome their difficulties. To have an idea of which firm is shutting down, I compute the difference between the number of people laid-off during the same layoff episode and total workforce size. If I consider "having a difference between the total workforce size and the number of people laid-off during the same layoff episode lower than 5" as a proxy for the firm shutting down, we see that having a seniority lying between 365 and 380 days is associated with a 12.5 percentage points higher probability of shutting down. If I make the definition of "being right above the cutoff" and of the proxy for shutting down vary, the results go all in the same direction, as made clear in Table 2.8.

These results indeed show that employers seem more willing to grant contract extension when the firm is shutting down, and therefore when it represents a negligible cost for them.

To further investigate the bargaining scenario, other workforce adjustment mechanisms have been explored, to understand to what extent bunching firms are also more likely to use other types of contract terminations in the neighbourhood of economic layoff as a potential way to alleviate administrative and financial constraints linked to job separation. Appendix 2 shows that bunching firms seem more willing and potentially more able to use other types of contract termination within the same episode, which could be associated with a better knowledge and use of job separation legislation.

4 Theoretical Framework of Negotiated Layoff

Setup – I develop an illustrative model, motivated by the preceding empirical findings, aiming at organising the analysis of the observed bunching by clarifying each party's incentives and cost.

The model describes a firm hit by a productivity shock that makes the employment contract not profitable anymore. I assume that wage cannot be adjusted downward after the contract has been set up as open-ended contracts entail wage rigidity.

The specificity of the setting under study is that, contrary to the traditional bunching scenario – a change in the marginal tax rate in most cases – the optimisation is not at the worker level: it is the joint optimisation of the employer and the employee that determines the optimal number of extension days from the moment the layoff

is announced. Employer and employee decide whether to proceed or not with the extension of the contract based on the sum of their net utilities from the extension. Conditional on the extension being efficient from a joint point of view, the employee and the employer bargain over the cost of the layoff. The employee trades the extension of the contract against a lower layoff cost, through a reduction in the supra-legal severance payment or in the risk of paying damages.

I do not consider the impact of UI benefits on the design of the contract at the moment of the hiring decision, to focus on the effect on the timing of the layoff once it has been decided.

On the firm's side, the profit has become negative. I describe the payoffs in the case where no extension occurs (indexed by 0), and in the case there is extension :

$$\begin{aligned}\Pi(0) &= -t^0 \\ \Pi(d) &= (P - w)d - t\end{aligned}$$

with t^0 the payment made to the worker at the moment of the layoff when there is no extension, t the payment made to the worker at the moment of the layoff when there is extension, P worker's productivity, w the wage and d the number of days of extension. Note that, after the productivity shock, we have $P < w$.

Similarly, on the worker's side:

$$\begin{aligned}U(0) &= SBd + t^0 + V_u^0 \\ U(d) &= (w - \alpha)d + t + V_u^d\end{aligned}$$

with SB being the standard benefit, α disutility from work, and V_u^0 and V_u^d respectively the utility derived from unemployment in the no extension and in the extension scenario (with $V_u^d > V_u^0$ because of the jump in benefits at the threshold).

Condition for the extension of the contract – There is extension of the contract if and only if the surplus associated with the extension is positive.

$$U(d) - U(0) + \Pi(d) - \Pi(0) \geq 0 \Leftrightarrow (P - SB - \alpha)d + V_u^d - V_u^0 \geq 0 \quad (2.1)$$

Based on the consideration of the surplus, this condition describes the cases where the extension is efficient. We have $d = L_{365} - L_{initial}$, with L_{365} being the threshold at 365 days, and $L_{initial}$ the seniority value at the moment the layoff is announced, before any extension. L_0 is the lowest value of $L_{initial}$ which verifies 2.1, or, in other words, the initial value of seniority of the marginal buncher.

However, we have been silent on the underlying negotiation mechanism that makes this extension happen. To be implemented, negotiation needs to ensure that both parties are at least as well-off as if no extension takes place.

Bargaining on the layoff cost – Once condition 2.1 is verified, a negotiation occurs on the level of the severance package granted to the employee upon separation after the extension, that is the contingent part of the layoff cost.

We are interested in the set of parameters which lead to an efficient outcome, that is, which make each party at least as well-off as in the no extension scenario.²³

It means that the supra-legal severance package in case of extension, t , should be such that:

$$(P - w)d - t + t_0 \geq 0 \tag{2.2}$$

In practice, an additional constraint has to be fulfilled: the negotiated severance package in case of extension cannot be negative. It means that, although the employee is willing to accept a reduced severance package in exchange of the extension of his contract, it is not possible for the employer to make the employee pay at the moment of the separation.

$$t \geq 0 \tag{2.3}$$

Conditions for an agreement – From equations 2.1, 2.2 and 2.3, we end up with two necessary and sufficient conditions for the bunching to occur:

1. Efficiency condition $(P - SB - \alpha)d + V_u^d - V_u^0 \geq 0$
2. Feasibility condition $t^0 \geq d(w - P)$

Those two conditions describe the whole set of parameters that would lead to an efficient extension.

We define L_1 as the lowest value of the initial seniority which verifies both conditions. We observe empirically that those bunching are the ones with initial seniority above $L_{min} = \max(L_0, L_1)$.

Testable predictions – I do not explicitly model here the respective bargaining powers that determine the division of the surplus, as I am primarily interested in

²³Regarding the determination of the solutions for t , a standard Nash bargaining framework could have been considered. I show in Appendix 3 that the Nash solutions are actually a subset of the efficient solutions I am describing in the core of the chapter. I then stick to the more general formulation of the model.

the conditions that make the bunching occur. In addition, I have no information available on the level of t in my data, preventing me from discussing the empirical implications of the surplus division rule.

However, the bargaining power may still influence the likelihood to bunch, as it is related to t^0 which enters the feasibility condition. In this framework, t^0 has been taken as exogenous at the moment of the layoff, but we can imagine that the bargaining power that has determined the division of the surplus from extension is correlated to the bargaining power intervening in the determination of t^0 .

Table 2.9 sums up the correlations that we expect to see between the magnitude of the bunching and different parameters. The difference in utility $V_u^d - V_u^0$ cannot be directly measured. This difference captures both the change in benefits at the threshold, and the fact that if the extension is granted, when V_u is measured, at the moment where the worker reaches one year of tenure, he will be entitled to a compensation duration D . If no extension is granted, the worker is entitled to the same duration. However, at one year, he will have already consumed d days of benefits.

Empirically, as the difference in benefits is large, if d is small, the difference $V_u^d - V_u^0$ can be proxied by the difference in replacement rate at the threshold ΔC .

The challenge of the empirical analysis is to disentangle the different mechanisms that can explain the variation in the bunching magnitude: (i) the level of the incentives, captured by ΔC and SB ; (ii) individual worker's characteristics such as P , α or w ; (iii) the bargaining power captured through t^0 .

5 Heterogeneity in Bunching

Since the standard benefit replacement rate varies according to previous earnings, the jump in expected benefits at the one-year threshold varies accordingly (from 5 to 22.6 percentage points). A first natural heterogeneity analysis involves comparing the behavioural response of subpopulations defined with respect to their potential gain when crossing the cutoff. I therefore split my sample into four gain categories (the distribution can be found in Table 2.10). The McCrary (2008) test computed on these different categories shows that the magnitude of the bunching is positively correlated to the magnitude of the difference in replacement rates, and then to the wage (Table 2.11).

Yet, it is difficult to disentangle the different channels at play: the population having a higher potential gain, and then higher earnings, is also significantly more educated, more skilled, working more frequently full-time, than the rest of the population. It means that, for the moment, we cannot decompose the effect of having higher incentives and of having characteristics associated with more bargaining

power.

What is at stake here is to know whether the higher propensity to bunch as the potential gain increases is due: (i) to higher financial incentives, keeping preferences and the ability to bargain constant; (ii) to different preferences coupled with higher incentives, keeping the ability to bargain constant; (iii) to a higher ability to bargain coupled with higher incentives, keeping preferences constant.

Therefore, we need a metric that neutralises the effect of having higher incentives, to see to what extent it varies with different characteristics. In other words, if we observe differences in L_{min} , we do not know yet if it comes from a higher Δ_C – higher incentives – a higher ability to bargain (through t^0) – or differences in preferences – α – or productivity – P . Next subsections will develop the methodology that uses bunching first to recover a reduced-form elasticity of contract duration with respect to the level of benefits, and second to describe the buncher characteristics as compared to the workers eligible to bunching in a counterfactual distribution that would be observed absent the notch. To separate the effect of Δ_C from the joint effect of α and of the bargaining power, the reduced-form elasticity will be used as a metric measuring bunching for a given level of incentives. This elasticity will be computed for different subpopulations defined in terms of potential gain, individual and firm level characteristics to see which factors have a greater weight in explaining bunching variation.

5.1 Empirical Bunching Estimation

5.1.1 Baseline methodology

Drawing on the bunching literature (Saez, 2010; Chetty et al., 2011; Kleven and Waseem, 2013; Brown, 2013), I exploit the observed hole and spike in the seniority distribution to have an estimate of the elasticity of labour supply, using the relationship between observed bunching and elasticity brought to light by Saez (2010). An additional difficulty here is that we are in the case of an upward notch, as disposable income dramatically increases at the one-year threshold, thanks to a jump in the level of the budget set. Then, I cannot identify an area of strictly dominated choice and use it to estimate the optimisation frictions pointed out by Chetty et al. (2011) and Kleven and Waseem (2013), though these frictions are likely to be important in this case, as the extension of the contract can only occur at some bargaining cost.

The usual methodology rests upon the standard labour supply model where the individual trades off the value of consumption (measured by the disposable income when employed or unemployed) with the cost of work effort (captured by the before-tax-and-benefits income). In my particular setting, the optimisation is,

however, at the level of the joint surplus, which complicates the derivation of a structural elasticity parameter. Moreover, the definition of the structural underlying parameter is not obvious *ex ante*, as the observed behaviour is a mix of the employer's response, the employee's response, and potentially some bargaining frictions. For these reasons, and because I am primarily interested in gaining insights into the differences of behaviours between subgroups rather than having a precise estimate of a structural parameter, I implement a reduced-form strategy to uncover the elasticity of contract extension thanks to an estimation of the bunching. On the one hand, this estimate sheds light on the response in presence of important negotiation frictions, and then, will not give a precise measure of the true workers' optimisation behaviour. On the other hand, it does not rely on any structural assumption. It will be used as a metric to compare subgroups behaviours, neutralising the impact of the differences in incentives.

Figure 2.24 illustrates the implications of introducing a notch in the budget set. It makes all the individuals located in the interval $[L^*, L^* - \Delta L^*)$ in the pre-notch distribution bunch at the notch point, with L^* being L_{365} . $L^* - \Delta L^*$ corresponds to the pre-notch seniority of the marginal buncher, L_{min} , or, in other words, to the lowest pre-notch seniority value of the bunchers. The marginal buncher is the one exactly indifferent between the notch point and his best interior solution after the introduction of the scheme. There is a hole in the post-notch density distribution as no individual is willing to locate between $L^* - \Delta L^*$ and L^* .

Excess bunching at the notch can be expressed as:

$$B = H_0(L^*) - H_0(L^* - \Delta L^*) = \int_{L^* - \Delta L^*}^{L^*} h_0(L) dL \approx h_0(L^*) \Delta L^*$$

where $H_0(L)$ and $h_0(L)$ are respectively the seniority cumulative distribution function and the seniority density function in the absence of the notch. The approximation holds if we assume that the density $h_0(L)$ is roughly constant over the interval $(L^* - \Delta L^*; L^*)$.

The reduced-form approach does not rely on any parametric assumption. As we are in the case of a notch, the extension day response needs to be related to a change in the implicit marginal replacement rate between the notch point L^* and the last bunching point $L^* - \Delta L^*$, as in [Kleven and Waseem \(2013\)](#). The implicit replacement rate, r^* , is given by the following expression:

$$r^* \equiv \frac{U(L^* - \Delta L^*) - U(L^*)}{\Delta L^*}$$

$U(L)$ gives the amount of UI benefits paid at any value of L . If we denote r_0 the standard benefit replacement rate and Δr the change in replacement rate at the

notch, we have:

$$\begin{aligned} r^* &= \frac{L^*(r_0 + \Delta r) - (L^* - \Delta L^*)r_0}{\Delta L^*} \\ r^* &= r_0 + L^* \frac{\Delta r}{\Delta L^*} \end{aligned}$$

The elasticity parameter becomes:

$$\begin{aligned} e_{RF} &= \frac{\Delta L^*/L^*}{\Delta r^*/(1+r^*)} = \frac{\Delta L^*}{L^*} \times \frac{1+r_0+L^*\frac{\Delta r}{\Delta L^*}}{L^*\frac{\Delta r}{\Delta L^*}} \\ e_{RF} &= \left(\frac{\Delta L^*}{L^*}\right)^2 \times \frac{1+r_0+L^*\frac{\Delta r}{\Delta L^*}}{\Delta r} \end{aligned}$$

The formula essentially treats the notch as an hypothetical kink where the replacement rate would jump to r^* . However, the kink schedule includes interior points that are strictly preferred to L^* by the marginal buncher, who then would not become a buncher if faced with this kink. Therefore, the bunching response to the notch overstates the response that would be created by the corresponding kink, making e_{RF} an upper-bound to the true structural parameter.

The reduced-form elasticity depends on policy parameters, Δr , r_0 , L^* , and on the response in terms of extension days, which needs to be estimated. The bunching methodology aims precisely at providing an estimation of the extension day response, through the measure of the bunching mass at the notch point.

The empirical methodology consists in estimating the excess mass of individuals laid-off at a seniority value within the defined bunching area by computing a counterfactual seniority density, and compare it with observed one.

I start by fitting a polynomial to the empirical distribution, excluding an area around the notch point, that I will refer from now on as the *excluded area*. The counterfactual distribution is then estimated using the same coefficients, from a regression of the following form:

$$c_s = \sum_{j=0}^J \beta_j \cdot (L_s)^j + \sum_{i=L_l}^{L_u} \lambda_i \cdot \mathbb{1}_{L_s=i} + \nu_i \quad (2.4)$$

where c_s is the number of individuals in bin s , L_s the seniority value in bin s , J is the order of the polynomial, $[L_l; L_u]$ the excluded area around the notch point. The counterfactual distribution is then computed as the predicted value from equation 2.4, omitting the contribution of the dummies around the notch point. It follows that the counterfactual density is given by:

$$\hat{c}_s = \sum_{j=0}^J \hat{\beta}_j \cdot (L_s)^j \quad (2.5)$$

Empirically, excess bunching \hat{B} is obtained by taking the excess number of individuals located at the notch of the observed distribution as compared to the counterfactual one (Figure 2.25).

$$\hat{B} = \sum_{L^*}^{L_u} c_s - \hat{c}_s$$

The excluded area upper bound can be determined visually without ambiguity, as the spike is typically sharp. Regarding the lower bound, the missing mass is harder to delimit as it is more diffuse: the standard methodology is to set the upper bound, and to determine the lower bound through an iterative process, by making it vary and reestimating the counterfactual density until the bunching mass (\hat{B}) and the missing mass ($\hat{M} = \sum_{L_l}^{L^*} \hat{c}_s - c_s$) equalise.²⁴

Following Chetty et al. (2011), we define b as the excess mass around the notch as a proportion of the average density of the counterfactual distribution in the area around the notch:

$$\hat{b} = \frac{\sum_{L^*}^{L_u} \hat{c}_s - c_s}{\sum_{L^*}^{L_u} \hat{c}_s / (L_u - L^* + 1)}$$

The identification of the elasticity from bunching measurement rests upon two assumptions: (i) the counterfactual distribution is smooth in the bunching area, so that B captures a behavioural response; (ii) Bunchers come from a continuous set $M = B$ below the notch point so that we can identify a marginal buncher.

In the spirit of Goupille-Lebret and Infante (2018), I decompose the contribution to the observed distribution of the points within the excluded area into the part due to points above and the part due to points below the notch. The part below corresponds to the hole created by the postponement of contract termination whereas the mass above captures the concentration of layoffs once the cutoff is passed. By measuring the two components separately, I relate the layoffs located right after the cutoff to the missing ones that have been strategically retimed. Then, I redefine the regression model as :

$$c_s = \sum_{j=0}^J \beta_j \cdot (L_s)^j + \gamma_1 \cdot \mathbb{1}_{L_l \leq L_s < L^*} + \gamma_2 \cdot \mathbb{1}_{L^* \leq L_s \leq L_u} + \nu_i$$

Table 2.12 shows the corresponding regression results: the seniority bin counts are regressed on a 4th order polynomial of the seniority value, with a specific set of dummies for being located in the bunching area above the notch point and another set of dummies for being located in the bunching area below the notch point. This table indicates that for my period of interest, being located in the upper (respectively

²⁴The missing mass must be equal to the bunching mass as all the bunchers come from the left side of the cutoff, creating a hole.

lower) bunching area is always associated with a significant rise (resp. decline) in the number of laid-off workers at this seniority value. It means that we indeed observe a significant hole and spike in the distribution of the seniority around the notch point in the period were the CSP was in force, whereas no such pattern is observed for the pre-CSP period.

Replicating the results highlighted by the [McCrary \(2008\)](#) test, [Figures 2.26 and 2.27](#) show that no significant bunching occurs at the two-year cutoff during the post-CSP period, whereas there is bunching at the strategic two-years cutoff during the pre-CSP period.

I derive elasticity estimates from the observed bunching using a reduced-form approach, relating the bunching mass to the extension day response entering the elasticity formula. Following [Kleven and Waseem \(2013\)](#), I use two methodologies to recover ΔL^* . The first one is called the "convergence method", and uses the point of convergence L_l which makes the missing and excess mass equalise as an estimate of L_{min} . Indeed, if the area delimiting the bunching mass can be determined visually, the lower bound corresponding to L_l , the marginal buncher seniority, is defined so that the missing and excess masses are equal. In my case, as the jump in benefits occurs above the threshold, the missing mass appears below the threshold. L_l rather corresponds to a "point of divergence", where the counterfactual and empirical distributions start to diverge. The distance between L_l and L^* provides a measure of the extension day response ΔL^* . This would give an upper bound of the true behavioural response, as it assumes that all the individuals located between L^* and L_l respond to the incentives by bunching at the notch point. The fact that the missing mass area is not entirely empty would be due to some optimisation frictions.

However, if we allow for heterogeneity in the elasticity, observing some individuals in the missing mass area, between the notch point and the marginal buncher point, could be rationalised by low elasticity values. In this context, bunching could be used to estimate an average extension days response $E[\Delta L_e^*]$. Denoting by $\bar{h}_0(L, e)$ the joint seniority-elasticity distribution in the pre-notch situation, we have:

$$B = \int_e \int_{L^* - \Delta L^*}^L \bar{h}_0(L, e) dLde \approx h_0(L^*) E[\Delta L_e^*]$$

Using the measure of \hat{B} and an estimate \hat{c}_s of $h_0(L^*)$ at the notch, we can retrieve the average extension day response. This second method would give a lower bound to the reduced-form elasticity as it assumes that all the individuals in the missing mass area have not bunched because of a too low elasticity.²⁵

Using both methods, I provide upper and lower bounds to the reduced-form elasticity, and make the estimation window vary to test the robustness of the

²⁵As I am in the case of an upward notch in the level of disposable income, I am not able to identify a strictly dominated region to estimate the optimisation frictions and to rescale $E[\Delta L_e^*]$ as in [Kleven and Waseem \(2013\)](#).

estimation.

Table 2.13 shows consistent reduced-form estimates throughout different estimation windows and excluded area boundaries. Standard errors are computed using a bootstrap procedure generating a large number of distributions and associated estimates of each variable. The standard errors correspond to the standard deviations of the distribution of each variable estimate.

The elasticity's lower bound appears quite low, but we have to keep in mind that it does not account for the optimisation frictions. Using the estimate bounds computed on the one-year window, it means that a 10% increase in UI marginal replacement rate leads, on average, to 0.52 to 4.36 days response, measured at one year.

5.1.2 The Difference-in-Bunching Strategy

The presence of a small discontinuity in the density in the period preceding the introduction of the CSP (referred from now on as the pre-CSP period) justifies the use of a difference-in-bunching strategy (as in Brown (2013) in the case of retirement decisions), to neutralise the effect of other factors unrelated to the behaviour of interest. The methodology consists in measuring the pure CSP-related bunching as the excess mass relative to the counterfactual density, no longer computed by fitting the empirical one excluding an area around the notch point, but by taking the pre-CSP density. Using the pre-CSP density makes it possible to take into account any pattern in the seniority distribution at layoff that would not be a response to UI incentives. The identification relies on the assumption that, absent the notch, the shape of the seniority distribution should be the same in both periods.²⁶ If this assumption holds, taking the difference between the observed distributions before and after the introduction of the CSP isolates the bunching exclusively due to the incentives created by the CSP.

A comparison of the pre and post-CSP densities on the whole distribution (Figure 2.28) and on a tightened 6-month window around the notch point (Figure 2.29) is a first evidence that the period preceding the introduction of the CSP can be convincingly used as a counterfactual. We observe that, when we choose a 6-months window, the two curves cross at the left-hand side of the cutoff around the value 180. The fact that the missing mass is more spread out than the bunching peak can justify the need to take a larger window at the left-hand side of the cutoff, to be sure we are not dropping missing people who have strategically retimed their layoff and who should have had a seniority lower than 180 days. Then, I also use a window from 120 to 540 days, as shown on Figure 2.30.

Figure 2.30 clearly shows that both densities are at the same level and have the

²⁶The raw number of laid-off workers can vary across time, as long as the distribution of seniority at layoff stays unchanged in both periods.

same shape, except around the notch point. The post-CSP density shows a hole before the threshold, and a spike after, as compared to the pre-CSP density. We see in Figure 2.31 that the pre-CSP distribution exhibits a small increase at the one-year threshold but which does not have the same pattern as a bunching mass, and is not preceded by a hole before the cutoff. It provides an additional rationale to use the pre-CSP distribution as part of a difference-in-bunching estimation.

I reproduce the reduced-form methodology, adjusting the formula from Chetty et al. (2011) by measuring b and m as the areas between the before and after densities.

Using the pre-CSP density as counterfactual yields estimates very close to those obtained with the computed density, suggesting that non-CSP factors do not play a major role in explaining the bunching. Most of the observed bunching can then be imputed to a behavioural response to incentives created by the CSP. If we focus on the average response, using a one-year window, the difference-in-bunching estimate is equal to 0.011, which corresponds to 77% of the baseline estimate.

I make the time window vary, to ensure that I am not capturing the effect of the change in the economic context between September, 2009 - August, 2011 and October, 2011 - September, 2014. Thus, I restrict the window to January, 2011 and June, 2012. I get estimates of the same order of magnitude, though slightly higher.²⁷ In any case, it makes us even more confident than the baseline elasticity estimates are not capturing a spurious phenomenon that would not be related to UI incentives.

5.2 Characterisation of the buncher

Drawing on the descriptive statistics provided in section 3.3, a more systematic investigation of the buncher characteristics allows the identification of the difference between the population eligible for bunching and the one actually bunching in terms of observable, pre-determined characteristics. As in Diamond and Persson (2016), summary statistics of the buncher can be recovered by applying the bunching methodology to individual characteristics instead of the seniority density. If we consider *passing the threshold* as a treatment, and my sample in a potential outcome framework, workers whose observed seniority falls into the bunching window and who are missing below the threshold can be considered compliers. Those under the threshold in the observed distribution can be thought as never takers as their seniority value makes them potentially eligible for manipulation while it did not occur. Finally, those whose observed and counterfactual seniority falls into the bunching window are always takers.

For any observable characteristic X , I use workers outside of the manipulation

²⁷It is potentially due to the fact that the economic situation has improved from 2009 to 2014, reducing the number of economic layoffs and the need to optimise unemployment compensation.

region to estimate $E(X|s)$ at any seniority s inside the manipulation region:

$$X_{is} = \sum_{j=0}^J \beta_j \cdot (X_{is})^j + \nu_{is} \quad (2.6)$$

where $s < L_l$ or $s > L_u$.

It provides an estimate of the expected value of any observable for each seniority bin, had there been no bunching. It can then be compared to the actual value of the variable in the bunching area above (respectively below) the threshold, denoted \bar{X}^{up_actual} (respectively \bar{X}^{down_actual}).

Workers located in the bunching area above the threshold includes both “manipulators”, who crossed the cutoff in response to the UI incentives, considered compliers, and workers who would have had the same seniority value absent UI incentives, who are always-takers.

$$\begin{aligned} \bar{X}^{up_actual} &= \frac{1}{N_{up}^{total}} \sum_{i=L^*}^{L_u} X_i \mathbb{1}_{s=i} \\ &= \frac{N_{up_count}}{N_{up_count} + N_{compliers}} \bar{X}^{up_count} + \frac{N_{compliers}}{N_{up_count} + N_{compliers}} \bar{X}^{compliers} \end{aligned}$$

with N_{up_count} being the number of individuals in the bunching area above the threshold in the counterfactual distribution, the always-takers, and \bar{X}^{up_count} their average value of X .

Similarly, I define

$$\begin{aligned} \bar{X}^{down_actual} &= \frac{1}{N_{down}^{total}} \sum_{i=L_l}^{L^*-1} X_i \mathbb{1}_{s=i} \\ &= \frac{N_{down_count}}{N_{down_count} - N_{compliers}} \bar{X}^{down_count} - \frac{N_{compliers}}{N_{down_count} - N_{compliers}} \bar{X}^{compliers} \end{aligned}$$

with N_{down_count} being the number of individuals in the missing mass area below the threshold in the counterfactual distribution, and \bar{X}^{down_count} their average value of X . The never-takers who choose to locate below the threshold even in the presence of UI incentives are given by the actual distribution below the threshold.

We recover \bar{X}^{down_count} and \bar{X}^{up_count} by using extrapolation from equation 2.6 as well as the counterfactual density previously estimated with 2.5.

Equation 2.5 computed for the bunching regions below and above the cutoff allows the estimation of the number of always-takers and never-takers.

We define $N_{down}^{total} = N_{down_count} - N_{compliers}$ and $N_{up}^{total} = N_{up_count} + N_{compliers}$ as the total number of individuals in the bunching area above and below the threshold in the observed distribution.

Finally, using equally information both from the excess and missing masses, it

follows that

$$\bar{X}^{compliers} = 0.5 \cdot \left(\frac{N_{up}^{total}}{N_{up}^{total} - N_{up_count}} \cdot \bar{X}^{up_actual} - \frac{N_{up_count}}{N_{up}^{total} - N_{up_count}} \cdot \bar{X}^{up_count} \right) + 0.5 \cdot \left(\frac{N_{down_count}}{N_{down_count} - N_{down}^{total}} \cdot \bar{X}^{down_count} - \frac{N_{down}^{total}}{N_{down_count} - N_{down}^{total}} \cdot \bar{X}^{down_actual} \right)$$

I can then compare compliers' characteristics with those of workers located right below the cutoff in the absence of UI incentives, and then "eligible" for contract extension.

Compliers are, on average, older, more frequently female, more educated and more skilled, and work in smaller firms (Table 2.15).

Table 2.15 also shows the characteristics of the never-takers, to gain insights into the bargaining frictions. Indeed, the never-takers are the ones for which bargaining frictions are so high that they do not manage to negotiate the contract extension even though they are close to the tenure threshold.²⁸ They are significantly younger, less educated, less skilled and work in bigger firms than compliers.

Using a more systematic approach, this analysis confirms previous evidence on the profile of the typical buncher. Still, those characteristics could be associated with higher bargaining power, higher incentives, or different preferences. Next subsection precisely aims at disentangling the role of the incentives from other channels, by using the elasticity measure derived from bunching (section 5.1).

5.3 Interaction Between Ability and Incentives

The reduced-form elasticity as computed above is a way to measure to what extent the couple employer-employee responds to an increase in the level of unemployment compensation at the moment of the layoff, by retiming the termination of the contract. It scales the observed response in extension days by the magnitude of the financial gain, and provides a metric that should be valid for any value of the gain in UI benefits.

As such, it can be used to compare the behaviours of different categories of the population, precisely delimited by their potential gain when crossing the one-year cutoff.

In Table 2.16, the elasticity parameter shows that – keeping incentives fixed – the last category seems to be much more responsive to a change in UI benefits than the first ones, especially when looking at the average extension days response (lower bound estimate). Overall, the elasticity increases with the category, and is not even significant for the first one, although the pattern is less clear for the two middle categories. For a 10% increase in the replacement rate, workers in the highest gain category would increase the length of their contract by 0.67 days on average, whereas

²⁸Never-takers are identified as those being in the missing mass area in the empirical distribution

worker in the third gain category would increase it by 0.39 days if we stick to the lower bound estimates, and by 5.2 versus 3.6 days if we look at the upper bound. This positive relationship indicates that people in the highest incentives category do negotiate more on contract termination to reach the cutoff, but not only because their gain from unemployment compensation is higher, but also because they have different preferences, and/or different abilities to bargain. An exploration of the observable characteristics of the different groups can help disentangling the role of the different parameters.

5.3.1 Individual characteristics

Descriptive statistics by gain category – Belonging to a higher gain category is associated, on average, to a higher age, a higher level of education, to a higher probability of being executive, to longer working hours, and, mechanically,²⁹ to higher earnings (Tables 2.17 and 2.18). These characteristics are likely to be positively correlated with bargaining power, through the fact that more educated and skilled people are more able to voice their claims or to use the representation resources available, and that they are able to forego a higher amount of severance payment. Indeed, the amount t^0 of supra-legal severance payment the worker is able to negotiate in the no bunching scenario is likely to increase with earnings, which are positively correlated to potential gain.

Those results can be interpreted in two ways: either these characteristics are associated with differences in preferences, for example, an older executive with high earnings may be more willing to extend his employment spell even if he knows he is laid-off, whereas the psychological cost to keep on working in the firm can be greater for other types of workers; or these characteristics put the worker in a better position to negotiate with the employer, and to offer him a high transfer.

Heterogeneity in bunching within a gain category – Previous subsection has shown that elasticity increases with gain category even though incentives are kept constant. Descriptive statistics (Tables 2.17 and 2.18) give information on how the characteristics of workers vary with the gain category. However, to be able to disentangle the role of incentives and of different observable characteristics that may be correlated with bunching, I investigate how the magnitude of bunching varies with several covariates, holding gain category fixed. Graphical evidence can be found in Figures 2.37 to 2.42. We observe the same pattern: within the same gain category, bunching increases with age, education, skills, and being a woman, especially in the highest gain category where estimates are more precise.

²⁹The standard benefit replacement rate increases as earnings decrease, reducing the gap between the two types of benefits replacement rates.

I also take advantage of the fact that above a certain earnings threshold, the replacement rate of the standard benefit is held constant, at 57.4%. Therefore, while wage keeps on increasing, the gap in replacement rate between both types of benefits is fixed, providing an opportunity to study how bunching varies with wage, controlling for variation in financial incentives. I divide the distribution of wage above the threshold where the gap in replacement rates remains constant into two parts, above and below the median, and I compute the log difference in the seniority density at the one-year cutoff. Figure 2.43 shows that bunching still increases slightly with wage, possibly capturing the role of individual ability to bargain or preferences. However, as standard errors are large, I cannot rule out the hypothesis that wage has no influence on the magnitude of bunching, at least in this area of the distribution.

Elasticity by individual characteristics' cell – To perfectly neutralise the influence of financial incentives, the elasticity can be derived for different subpopulations defined in terms of firm size, sector, age, gender and education. In order to keep a sufficiently large sample size by cell and not to compute too many statistics, I design broad categories for each covariate. This method makes it possible, nonetheless, to control for several characteristics at the same time. Tables 2.19 and 2.20 show that there is still variation in the behavioural response after controlling for financial incentives and some individual and firms characteristics. The highest estimates are typically found in small firms among highly educated individuals, in the retail, food and accommodation, and services sectors. This pattern can be rationalised by differences in preferences or individual characteristics between cells, or by differences in ability to negotiate. However, statistics on representation institutions indicate that the presence of representatives is an increasing function of firm size, and that the building sector is where the unionisation rate of representatives is the highest (Breda, 2016). It suggests that the type of firms where the elasticity is the highest are typically not the ones where employees are better represented, arguing for an individual bargaining process.

I focus on a specific variable entering the decision to bunch according to my theoretical framework, that is the supra-legal severance payment received by the worker when there is no extension (t^0). The model predicts that the magnitude of bunching will be positively correlated with the level of t^0 . Unfortunately, I have no direct measure of t^0 in my data for the sample of laid-off workers,³⁰ but I use the information available for dismissed people after an open-ended contract. I restrict the sample to those having a seniority lower than 2 years and I fit a zero-inflated negative

³⁰Supra-legal severance payment is recorded in the UI data as it defines the legal delay between the day the job-seeker registers as unemployed, and the first day he actually receives benefits. However, one of the consequences of accepting the CSP is that there is no such a delay. The information on the supra-legal severance payment is, therefore, not recorded for those accepting the CSP.

binomial model. The regression output and a discussion on the choice of the model can be found in Appendix 4. I use the regression coefficients to predict the value of the supra-legal severance payment that would have been paid to the laid-off workers of my sample. I can then test the model's prediction by computing the elasticity on a high supra-legal severance payment versus a low supra-legal severance payment sub-group. Table 2.21 shows that, in accordance with the model, the high supra-legal severance payment group exhibits a higher elasticity than those who would receive low supra-legal severance payments when there is no extension. These results have to be taken cautiously, first because the value of t^0 is predicted by the zero-inflated negative binomial model, and not directly observed. Second, if the elasticity allows to control for the level of incentives, t^0 is still likely to be correlated with other variables that have in turn an impact on the likelihood to bunch. Still, they suggest that a higher t^0 makes the extension of the contract more likely, through the feasibility condition in the model. It can be argued that a higher t^0 is itself the result of a higher bargaining power, which would imply a positive link between the bargaining power and the propensity to bunch.

A way to summarise the relationship between the propensity to bunch and individual characteristics can be found in the correlation between the predicted unemployment duration and the elasticity of employment duration with respect to the level of UI benefits. Unemployment spell duration is predicted for the sample of interest using a large set of socio-demographic characteristics and covariates associated to the last job. Intuitively, the bargaining model would predict an ambiguous relationship between predicted unemployment duration and the elasticity, as a higher duration is associated to a higher jump in utility at the threshold, as the worker will enjoy the additional benefits longer (up to 12 months). However, a longer duration implies that the worker has characteristics associated to a lower employment probability, such as a lower level of education or skills. These characteristics may negatively correlate with the after shock productivity or the baseline level of severance payment, which themselves positively correlate with the propensity to bunch. Table 2.22 shows that elasticity is the highest in the middle of the predicted unemployment duration distribution. The empirical results confirm the insight from the model that those responding the most to the UI incentives have both a high financial gain – as they anticipate to stay unemployed for a significant period a time – and the bargaining resources to do so.

What we can draw from this analysis is that the propensity to bunch is positively correlated with the magnitude of the financial incentives, but that individual characteristics, including the bargaining power, still play a role in explaining the variation in bunching when keeping potential gain constant. I now turn to the exploration of firms' characteristics, and, in particular, to the forms of employees' representation.

5.3.2 Firms' characteristics

Bunching and employees' representation – I use the aggregate statistics provided by the Statistics department of the Ministry of Labour, Employment, Professional training, and Social Dialogue³¹, computed from the 2011 REPONSE survey on managers, employees and employees' representatives in firms of more than 10 employees, drawing a picture of the state of professional relationships in France. The data, available online, gives the aggregate proportion of employees' representatives and their different forms (unionised or not for example) by workforce size category. It also indicates the proportion of firms having negotiated on a specific topic in the last two years, decomposed by topic, by workforce size, by industry or by type of collective agreement.

I start by imputing the probability of not having any representative institution within the firm, which is a decreasing function of workforce size. I then run a cell analysis by examining how the magnitude of the jump in density varies with the gain and representation category. One caveat has to be made, as the data only covers firms with more than 10 employees, leaving 48.6% of my sample without any information on employees' representation.³² I then focus on this subsample of firms with more than 10 employees, keeping in mind this limitation.

Table 2.23 shows that the gain category seems to matter more than the quality of representation in explaining the magnitude of the bunching. Indeed, as the probability of having no representation within the firm increases, keeping the gain category fixed, the propensity to bunch does not seem to vary significantly, or slightly increases, indicating, if any, a negative correlation between the quality of the representation and the magnitude of bunching. On the contrary, the magnitude of bunching increases almost systematically as the gain category increases, keeping the probability of being represented fixed. The bunching is always significant and of high magnitude in the highest gain category, no matter the forms and intensity of representation in the firm. Focusing on this category, we observe that the magnitude of bunching tends to go up as the probability of having no representation increases. It would suggest that representation structures, within the firm, are not necessarily helpful in negotiating the extension of work contracts, but it is more the way different categories of workers, more or less skilled or educated, are able to mobilise the available resources that seems to matter. The elasticity estimates for the corresponding cells can be found in Table 2.24.

Table 2.23 indicates that individual characteristics, correlated to individual preferences or ability to bargain, have a greater impact on bunching than the quality of representation at the firm level. Yet, it should be noted that the way the quality of representation is measured is very broad – the probability of having no representation

³¹DARES, Directorate of research activity, studies and analyses

³²Firms of less than 10 employees face no legal obligation in terms of employees' representation.

at all decomposed by large workforce size categories – and the forms of representation are likely to vary within each category. In addition, this analysis leaves aside all firms with 10 employees or less, which represents almost half of the sample, and describes only correlations with the magnitude of the jump in density.

To complement this picture, I compute a proxy for bunching at the firm level. For any firm with more than two layoffs in the period of interest, I compute the proportion of workers with a seniority at lay-off lying in a small window above one year – which is an indicator of bunching. It should be noted that 23% of the sample are firms dismissing only one person, and are then excluded from this analysis. Among those firms, the proportion of firms with some of their laid-off workers located just above one year is 4.34% if we take a five days window, indicating that the bunching is concentrated among a small number of firms. Keeping in mind that, as a consequence, the sample size is small, I observe that the propensity to bunch at the firm level is negatively and significantly correlated with the workforce size. In Table 2.25, I regress the propensity to bunch on individual characteristics as well as representation indicators from the REPOSE survey or directly on workforce size. Potential gain, education and sex, always have a positive and significant effect, whereas variables related to the representation structures – *e.g.* probability of having a work council, a unionised delegate, workforce size – are not significant, or have a low-magnitude negative effect. These results go in the same direction as the previous one, suggesting that the representation in the firm, at least in terms of volume, does not seem to favour more bunching.

Using all the firms in my sample, I compute the elasticity estimate for individual and collective layoffs separately. Pointing to the individual bargaining scenario, Table 2.26 shows that the elasticity is much higher for individual layoffs. This result can be rationalised by the fact that one layoff date might be more easily scheduled according to UI incentives than several ones. It also strengthens the idea that the extension is negotiated in a one-to-one framework and that collective representation institutions are not necessarily involved in the process.

Bunching and firm fixed-effects – As I have no precise data on the quality of representation within the firm, I can use a fixed-effect logit model to determine, within a firm, which individual characteristics are associated with a higher propensity to bunch. I define as bunching any individual with a seniority at layoff falling between 365 and 397 days, as it corresponds to the bunching area for most estimations. I consider a logit model as my outcome is a binary variable, and I use firm fixed-effects to neutralise any characteristic specific to the firm, that I am not able to capture with the information at hand. Again, the limitation of this analysis is that I use only the subsample of firms for which I have several observations (*i.e.* several persons laid-off), and with some variation in the bunching dummy. It leaves me with 19,868

observations distributed in 3,396 firms.

Table 2.27 shows that potential gain, age, education level all have a positive effect on the propensity to bunch. It indicates that, keeping all firms characteristics constant – including some unobservable management practices or representation quality on which we have no precise information – the propensity to bunch is positively correlated to the level of the potential gain. When adding some other characteristics – which are themselves correlated to the potential gain – the coefficient of the potential gain decreases. Education level and age are both positively and significantly correlated to the propensity to bunch. It could mean that, keeping firm’s characteristics and incentives constant, more educated and older workers have preferences encouraging bunching, or that they are more able to take advantage of the representation structures that exist in the firm, or even that there is some heterogeneity in management practices (if the employer acts differently according to the type of employee).

All in all, these results suggest that the representation structures are not necessarily helpful in negotiating the extension of the contract, or that, at least, individual characteristics seem to matter more when it comes to explaining bunching. It could also indicate an heterogeneity in the representation quality according to the level of education or skills, within the same firm. This finding is in line with other cases of individual-level bargaining settings, where it has been shown that the level of qualification was a strong determinant of the bargaining outcome. For example, in France we observe that executives are able to negotiate much higher severance payments in case of mutually agreed contract termination, whereas low-skilled workers only get the legal minimum payment in most cases (Bouvier, 2017). A potential explanation put forward in the study is the difference in the level of information and in the level of knowledge of the decision-making process in the firm.

6 Robustness Checks and Extensions

6.1 Round-Number Fixed Effects

Following Kleven and Waseem (2013), I use an alternative strategy to take into account round-number fixed effects. Indeed, it is reasonable to think that the distribution of seniority at layoff will exhibit small peaks at regular intervals, as, for example, employers may lay off the first or last day of the month. This would mechanically lead to higher densities at seniority values around multiples of 30, though it would not be driven by any strategic behaviour. The optimisation behaviour at the one-year threshold, clear of the effect of being at a round month and year value, can be measured by accounting for this phenomenon.

A simple way of doing this is to add to the density regression round-number fixed

effects, that is to say a dummy equal to one for each value of seniority around a multiple of 30. To account for the fact that a month lasts either 28, 29, 30 or 31 days I choose the bandwidth such that the round-number dummy is equal to one for any number i meeting the following condition: $k - 0.1 \leq \frac{i}{30} \leq k + 0.1, k \in \mathbb{N}$.

The density regression becomes:

$$Den_s = \sum_{j=0}^J \beta_j \cdot (L_s)^j + \gamma_1 \cdot \mathbb{1}_{L_l \leq L_s < L^*} + \gamma_2 \cdot \mathbb{1}_{L^* \leq L_s \leq L_u} + \rho \cdot \mathbb{1}_{k-0.1 \leq \frac{L_s}{30} \leq k+0.1, k \in \mathbb{N}} + \nu_i$$

Adding these round-number fixed effects to compute the density does not change much the results (Table 2.28). Not surprisingly, the parameters are a bit lower, as we remove part of the bunching only due to the regularity in hiring and firing dates.

Taking into account the regularity in starting and ending dates of work contracts leaves the results virtually unchanged.

6.2 Response at the two-years cutoff

The existence of a similar setting at a different seniority value under a different period has also been exploited to compute elasticity estimates from the bunching response at the two-year cutoff during the pre-CSP period. This is a way to check whether the response is similar under both periods and cutoffs, and to confirm that the bunching measured is indeed a behavioural response to incentives created by the UI design, no matter the value of the threshold or the time period considered.

Table 2.41 (Appendix 2) exhibits elasticity estimates derived from the average response very close to those from the main estimation, around 0.008, although the missing mass area is more stretched out, leading to a higher upper bound.

The same threshold under the post-CSP period could have been used to measure some optimisation frictions, the idea being that any bunching observed at two years after September, 2011 is the sign of some workers not able to adjust to the new incentives. However, as depicted in Figure 2.3, no significant bunching occurs at this point under the post-CSP period. It follows that it is not possible to determine the missing mass lower bound that equalises the missing and excess masses, and that delimits the area where the behavioural response is observed. Then, it indicates that the main frictions attenuating the bunching of interest come from negotiation frictions that do not allow workers to adjust freely their layoff date.

6.3 Extensive margin response

The bunching methodology allows to measure the intensive margin response to an increase in the level of UI benefits, captured by the extension of the employment spell by a few days, conditional on the layoff. However, although legal constraints make

it unlikely,³³ we cannot rule out the presence of an extensive margin response, i.e. an increase in the total number of layoffs decided for tenure values in the bunching area after the implementation of the CSP. To measure such a response, I perform a difference-in-difference analysis to estimate the excess number of layoffs in the bunching window that would not have happened in the absence of the CSP. I take as a control group workers with a tenure lying between eight and ten months, as there is no reason that the CSP would affect the incentives to layoff in that tenure bracket. The first treatment group is defined as workers with a tenure between twelve and fourteen months, as we expect a higher number of layoffs in that tenure bracket after the implementation of the CSP. Symmetrically, the second treatment group is defined as workers with a tenure lying between ten and twelve months, where we expect less layoffs after the implementation of the CSP due to the postponement of the layoff date. In the absence of any extensive margin response, the excess and missing number of layoffs in the respective treatment groups should be equivalent. I run the regression on an administrative dataset gathering all employment flows in firms of more than 50 employees, allowing not to restrict to workers registering to UI. The dataset is built from mandatory declarations filled by employers each quarter (MMO, *Mouvements de Main d'Oeuvre*, DARES). Using this data instead of UI data alleviates the concern that the CSP may have affected the registration behaviour differently in different tenure brackets, which would artificially affect the total number of layoffs in UI data. Table 2.29 displays the additional variation in the number of layoffs in each treated group after the implementation of the CSP relative to the control group. They show that the relative increase and decrease in layoffs are of similar magnitude, around 0.4 daily layoff on average.

Table 2.30 shows the same regression on UI data, as a robustness check. Results point to a number of excess layoffs slightly higher than the number of missing layoffs. The difference with MMO data could come from a difference in registration behaviour or in the sample of firms covered. If anything, the extensive margin response is likely to be second-order as compared to the intensive margin response.

7 Welfare implications

The ultimate welfare impact of the extension of work contracts depends not only on the impact on the length of the employment spell, but also on the impact on the length of the unemployment spell. It is a well established fact that higher unemployment benefits are associated with longer unemployment duration (Schmieder and Von Wachter, 2016). Although I am not able to disentangle the moral hazard from the selection effect, precisely because there is bunching, Figure 2.44 shows

³³An extensive margin response would imply that new economic layoffs are decided in response to the increase in UI benefits where there would not have occurred had the CSP not been implemented. This is unlikely as economic layoffs have to be heavily motivated as being a last resort solution.

that there is a large jump in subsidised unemployment duration at the one-year cutoff. A further decomposition of the effect by gain category highlights a strong and significant increase in unemployment duration for the highest gain category, whereas no significant jump is observed for the lowest gain category, where no bunching is measured (Figure 2.45). In any case, to have a complete picture of the welfare implications of this delay strategy, this effect on the unemployment spell must be taken into consideration.

Having this in mind, I am able to compute an estimate of the cost and benefits of the extension of the contracts from the point of view of the government. On the cost side, the direct loss due to the payment of higher benefits is doubled-up with an indirect loss from the lengthening of the subsequent unemployment spell.³⁴ Using the same methodology as in section 5.2, I compute the average daily wage (w), unemployment spell duration (D), daily standard benefit (SB) and daily CSP benefit (ASP), as well as the takeup rate ($t_{compliers}^{CSP}$) for the population of compliers and for the population of eligibles. It follows that:

$$\begin{aligned} \text{Individual difference in total benefits} = & \underbrace{(ASP_{compliers} - SB_{compliers}) * \min(D_{compliers}, 365)}_{\text{Direct cost}} \\ & + \underbrace{SB_{compliers} * (D_{compliers} - D_{eligibles})}_{\text{Indirect cost}} \end{aligned}$$

$$\text{Aggregated difference in total benefits} = \text{Individual difference in total benefits} * B * t_{compliers}^{CSP}$$

B computed in section 5.1 gives the excess bunching, that is the number of individual in excess in the bunching area, who come from the area where a hole is observed.

On the other hand, a few more days of employment translates into more contributions paid to fund the UI scheme. Employers also contribute to the funding of the CSP by paying to the State part of the severance payment S they would have paid to the worker for any worker accepting the CSP. It means that the State receives more severance payment contributions only to the extent that there is a difference in takeup rate between compliers and eligibles. With a total contribution rate of 6.4% over the whole period, we have:

$$\text{Individual difference in contributions paid} = w_{compliers} * \Delta L * 0.064 + S * (t_{compliers}^{CSP} - t_{eligibles}^{CSP})$$

$$\text{Aggregated difference in contributions paid} = \text{Individual difference in contributions paid} * B$$

ΔL gives the average response in terms of days of extension, and is derived from the measure of the bunching, as explained in section 5.1.

³⁴This indirect effect has to be taken into account, although the CSP itself is limited to 12 months. After those 12 months, the job-seeker can keep on receiving benefit but at the standard replacement rate.

Using the values found in Table 2.31, the total additional benefits paid amount to 16,640,251€ over the whole 3 years period. With total additional contributions paid equal to 1,870,911€, the net cost for the unemployment insurance is 14,769,340€.

This simple computation highlights the fact that employers and workers use the UI scheme as a way to improve the conditions of the layoff for the employee while alleviating its cost for the firm, at the expense of the government.

To give more sense to these figures, I compare them to the annual amount of CSP-related spending from the UI. UI accounts (Unedic, 2018a) indicates annual net and gross CSP spending, but without taking into account neither the standard benefit that would have been paid instead of the ASP, nor any behavioural response in terms of unemployment duration. Therefore, I also compute net spending without taking into account the counterfactual capital of benefits that would have been paid absent bunching. As workers with tenure value around one year account only for a small share of workers taking the CSP, I also relate my figures to UI spending for workers taking the CSP with a tenure below 2 years.³⁵

Although spending related to bunching do not account for a huge share of total CSP spending, it exemplifies the distortions that can be created by UI design on the labour market.

8 Concluding Remarks

The impact of UI parameters on employment outcomes has drawn some attention in recent years, in particular since we observe that the interaction of UI and the labour market can influence the forms of employment: by taking into consideration the shortening of employment spells and allowing workers to be covered even between two short work contracts, the UI has sometimes been accused of encouraging the development of these precarious forms of work.

The setting under study has an impact on the timing of economic layoffs, and therefore, on the duration of the employment spell. Bunching evidence, identified as being due to the design of UI, is used to quantify the sensitivity of the duration of the contract to UI financial incentives. Bunching behaviour is the result of the bargaining of employer and employee who agree to maximise joint surplus by extending the contract when it is profitable to both parties. These strategic behaviours have several public policy implications. First, it encourages to maintain a poor match while it is not efficient anymore. It also allows employers to soften the conditions of the redundancy plan, reducing the risk of having their reputation harmed or of paying damages, thanks to a transfer from the State. They can use the UI scheme as an

³⁵For this comparison, I am only able to use gross spending as revenues cannot be disaggregated by tenure in the UI accounts.

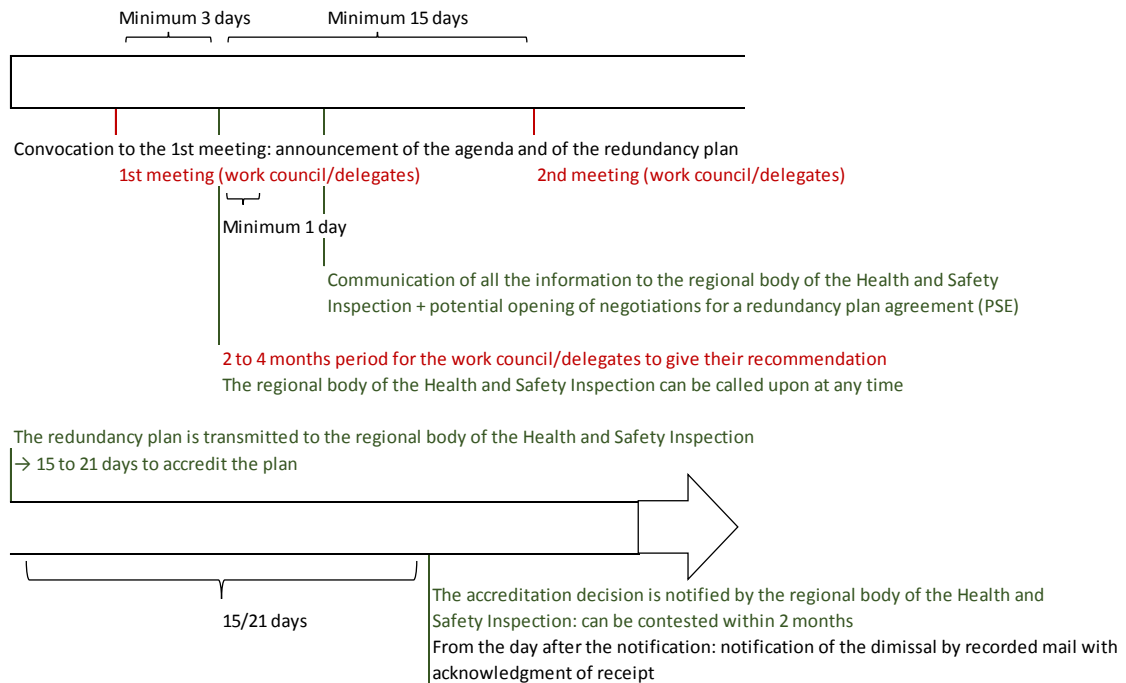
instrument for social peace, without bearing the cost of such a strategy, and without internalising it in the conditions of the breach of the contract.

The direct cost of this behaviour is doubled-up with an indirect cost, as receiving higher benefits itself influences the duration of the subsequent unemployment spell. Measuring the effect of the UI scheme on unemployment duration is challenging, precisely because the bunching observed implies that there is a selection issue when ones wants to compare populations on both sides of the cutoff. Although I am not able to fully distinguish the selection effect from the pure moral hazard effect of the CSP, a first intention-to-treat estimate suggests that the workers who bunch have also longer unemployment spells, triggering again additional cost to the UI. However, an exhaustive welfare analysis would require to analyse the effect on the subsequent job quality, on which I have no information.

The analysis in terms of individual and firms characteristics reveals that, keeping incentives constant, workers more likely to bunch, and then to extract rent from UI, are more educated, more skilled, and better integrated to the labour market (with higher earnings and working hours). Representation structures do not seem to favour bunching. In this setting, I identify a mostly individual bargaining process, where those who are less in difficulty on the labour market are more able to mobilise the bargaining resources and to get a higher compensation. This finding raises some questions on the efficiency of the UI at targeting the population most in need and further away from the labour market.

Tables and Figures

Figure 2.1: Economic layoff procedure



NOTE: This diagram describes the different steps of the economic layoff procedure with the minimum period of time between each step (for firms with more than 10 employees). It indicates that employers have the means to strategically extend the procedure in order to make workers exit at the tenure threshold.

Table 2.1: Entitlements according to worker's decision and seniority

Accepting the CSP		Refusing the CSP
Seniority < 365 days	Seniority ≥ 365 days	Whatever the seniority
Counselling + training	Counselling + training	
57.4% to 75% wage	80% wage	57.4% to 75% wage
+ compensation in lieu of notice	+ no waiting period	compensation in lieu of notice
+ no waiting period		

This table details the consequences of accepting the CSP, depending on the tenure value. Workers accepting the CSP are all entitled to the training and counselling components, but only those with a tenure value at least equal to one year are entitled to a benefit equal to 80% of the wage.

Table 2.2: Characteristics of laid-off workers accepting or refusing the CSP

	Refuse CSP	Accept CSP	Difference (1)-(2)
Age	41.002	42.207	-1.205*** (0.237)
Proportion of female	0.370	0.426	-0.056*** (0.001)
Level of education	5.970	6.636	-0.666*** (0.005)
Proportion of unskilled workers	0.079	0.053	0.026*** (0.001)
Proportion of unskilled employees	0.091	0.057	0.034*** (0.001)
Proportion of skilled workers	0.245	0.183	0.063*** (0.001)
Proportion of skilled employees	0.468	0.545	-0.077*** (0.001)
Proportion of intermediate occupations	0.029	0.041	-0.012*** (0.001)
Proportion of executives	0.088	0.122	-0.033*** (0.001)
Tenure	2576.101	2995.626	-419.525*** (7.799)
Firm size	63.622	91.544	-27.922*** (0.856)
Industry and agriculture	0.205	0.213	-0.009*** (0.001)
Building, Retail, Food and accomodation	0.478	0.440	0.038*** (0.001)
Services and temporary work	0.317	0.347	-0.029*** (0.001)
Observations	280,076	356,274	636,350

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

NOTE: This table reports the characteristics of workers laif-off between October, 2011 and September, 2014 and eligible to the CSP (636,350 observations), according to their take-up status. It indicates that takers are on average older, more frequently female, more educated and skilled, working in bigger firms in the services or the industry.

Education ranges from 1 (no education) to 10 (5 years or more of higher education).

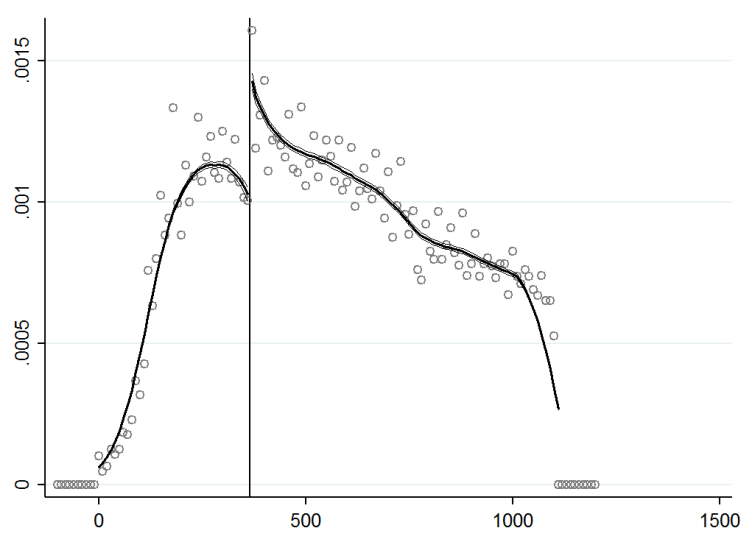
Table 2.3: Determinants of the probability to accept the CSP

	Probability of accepting the CSP
Age	0.00003** (0.00001)
Female	0.0141*** (0.00157)
Level of education	0.0363*** (0.00039)
Unskilled employee	-0.0309*** (0.00394)
Skilled worker	0.0152*** (0.00323)
Skilled employee	0.0824*** (0.00310)
Intermediate occupation	0.0969*** (0.00477)
Executive	0.0516*** (0.00375)
Tenure	0.00001*** (2.44e-07)
Firm size	0.00004*** (2.03e-06)
Building, Retail, Food and accomodation	0.0024 (0.00195)
Services and temporary work	-0.0115*** (0.00210)
Constant	0.2432*** (0.00392)
R^2	0.040
Observations	468,212

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

NOTE: This table reports a multivariate analysis of the determinants of takeup on workers laid-off between October, 2011 and September, 2014 and eligible to the CSP. It indicates that the variable having the highest explanatory power are related to the skill and education level. Education ranges from 1 (no education) to 10 (5 years or more of higher education). The reference category is unskilled worker in the manufacturing or agriculture sector.

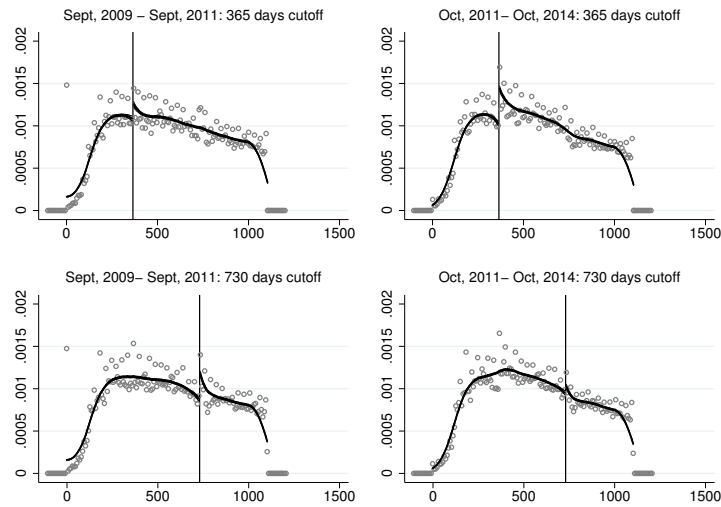
Figure 2.2: McCrary test on the seniority variable at the 365 cutoff (October, 2011 - September 2014)



SOURCE: UI data (FNA).

NOTE: This graph performs the McCrary test (McCrary, 2008) on the density of tenure at layoff at the one-year threshold on the whole population of workers eligible to the CSP entering unemployment between October, 2011 and September, 2014. Binsize: 10, bandwidth: 100.

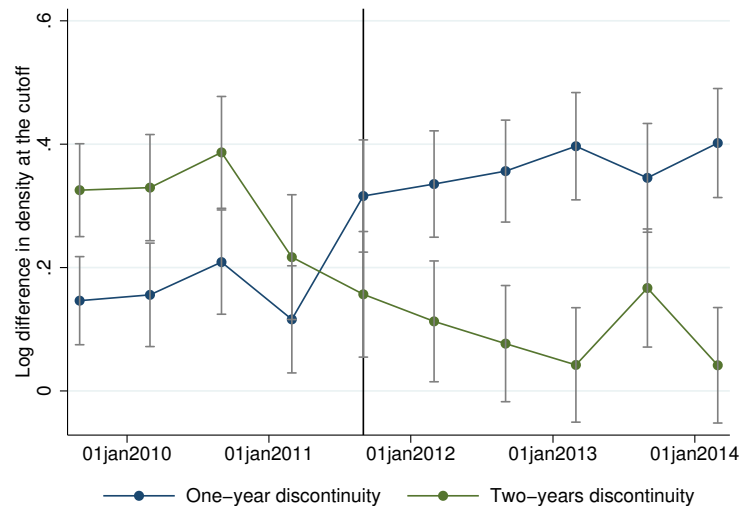
Figure 2.3: McCrary test on the seniority variable at the 365 and 730 days cutoffs for the two periods of interest



SOURCE: UI data (FNA).

NOTE: This graph reproduces the (McCrary, 2008) test on the density of tenure at layoff, respectively at the one and two-year thresholds, on the whole population of workers eligible to the CSP entering unemployment between September, 2009–August, 2011 and October, 2011–September, 2014 (1,118,847 observations). Binsize: 10, bandwidth: 100.

Figure 2.4: Evolution of the log difference in density at one and two years (2009-2014)



SOURCE: UI data (FNA)

NOTE: This graph plots the log difference in density from the McCrary (2008) test performed on the density of tenure at layoff both at the one-year and two-year cutoffs. The vertical line represents the moment when the tenure threshold where the level of benefits jumps has changed from two years to one year.

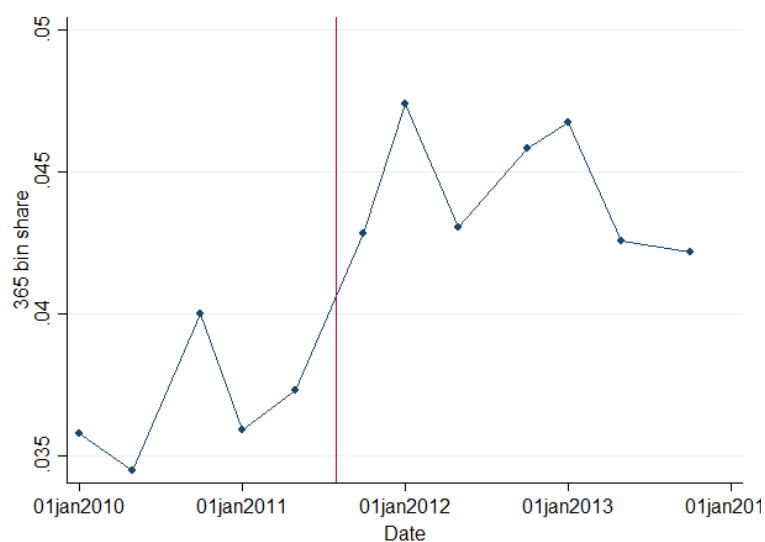
Table 2.4: Log discontinuity estimates

Time period	Threshold	Log difference
September 2009 - September 2011	365 days	.157*** (.021)
	730 days	.319*** (.022)
July 2011 - September 2011	365 days	.067 (.072)
	730 days	.154* (.081)
September 2011 - November 2011	365 days	.315*** (.060)
	730 days	.174*** (.067)
December 2011 - February 2012	365 days	.284*** (.059)
	730 days	.146** (.068)
March 2012 - May 2012	365 days	.350*** (.060)
	730 days	.174** (.069)
June 2012 - August 2012	365 days	.332*** (.063)
	730 days	.040 (.069)

SOURCE: UI data (FNA)

NOTE: This table gives the log difference in density at the one-year and two-year cutoffs for different periods of time from the [McCrary \(2008\)](#) test performed on the density of tenure at layoff for workers eligible to the CSP. It shows that the jump in density evolves in accordance with the legal changes in UI incentives, as the threshold where UI benefits increase discontinuously has moved from two years to one year in September, 2011.

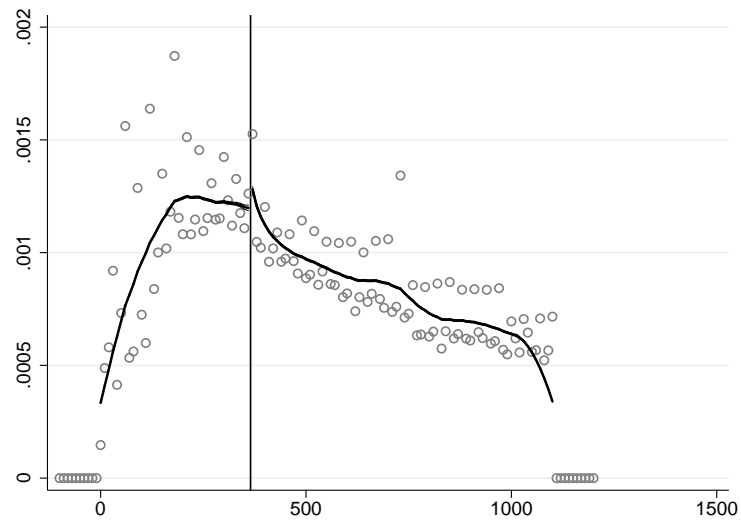
Figure 2.5: Evolution of the share of workers laid-off with a seniority lying in the bunching window (2010-2013)



SOURCE: UI data (FNA)

NOTE: This graph plots the share of workers laid-off with a tenure ranging between 365 and 397 days (in accordance with the estimation of section 5) among all laid-off workers eligible to the CSP, during the 2010-2013 period. The share of workers laid-off within the bunching window increases markedly at the moment when the notch moved from the two-years to the one-year threshold, as represented by the vertical line.

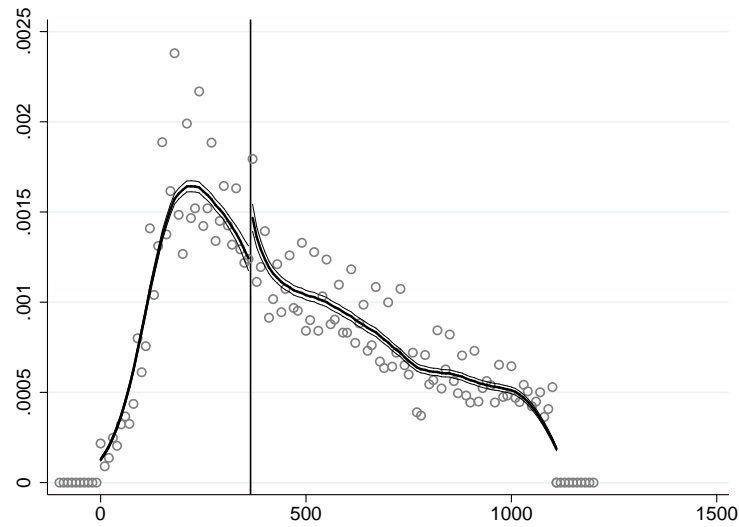
Figure 2.6: McCrary test on the seniority variable at the 365 days cutoff on all open-ended contracts (excluding workers eligible for the CSP) (France, 2011-2014)



SOURCE: UI data (FNA).

NOTE: This graph reproduces the (McCrary, 2008) test performed on the sample of unemployed persons after an open-ended contracts over the period between October 2011 and September 2014, excluding workers eligible for the CSP (binsize: 10, bandwidth: 100). As there is no discontinuity at the one-year threshold, it provides further evidence that the jump observed in the distribution of laid-off workers at the same threshold is driven by the CSP and not by another motive that would concern all workers under a permanent contract.

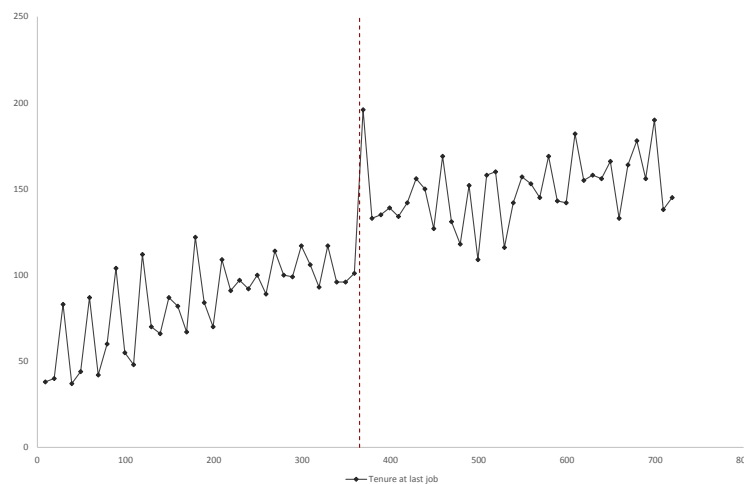
Figure 2.7: Mc Crary test on employers who did not offer the CSP



SOURCE: UI data (FNA)

NOTE: This figure plots the [McCrory \(2008\)](#) test on the subsample of workers eligible to the CSP, laid-off between October, 2011 and September, 2014, who report not having been offered the CSP by their employer. It is reasonable to think that employers who did not offer the CSP – whereas they have the legal obligation to do so – were not involved in bargaining over the extension of the contract for employees to get the higher CSP benefits. For them, we do not observe significant bunching other than related to a round-number effect (no hole before the cutoff or mass after). It provides additional evidence that the bunching is a response to CSP incentives.

Figure 2.8: Tenure distribution of all eligible laid-off workers (France, 2011-2014)



SAMPLE: The whole population of laid-off people in firms of less than 1,000 employees between October, 2011 and September, 2014, on the MMO data. Binsize: 10.

NOTE: This figure plots the density of tenure at layoff for workers eligible to the CSP. The data comes from the mandatory form each employer in a firm of 50 employees or more has to return to the administration to inform on the employment flows. It confirms that the discontinuity in the tenure distribution observed in the UI data does not come from a discontinuity in the UI registration rate, but from a discontinuity in the separation rate.

Table 2.5: Magnitude of the discontinuity at 365 days for different values of observable characteristics

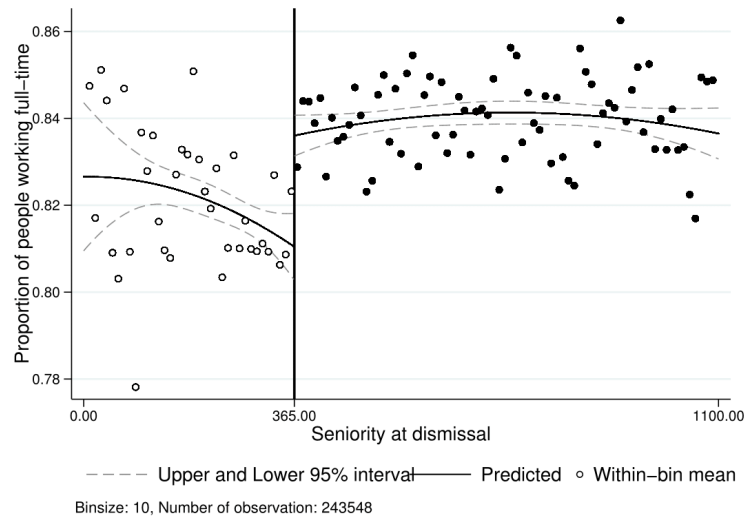
Variable	Log discontinuity estimates
Sex	
Male	0.314*** (0.022)
Female	0.45*** (0.032)
Diploma	
Primary school	0.054 (0.085)
Vocational high school	0.317*** (0.030)
Bac+5 or more	0.62*** (0.071)
Level of qualification	
Executive	0.614*** (0.069)
Skilled employee	0.42*** (0.028)
Unskilled employee	0.148** (0.062)
Working time	
Full time	0.382*** (0.020)
Part time	0.25*** (0.044)

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

SAMPLE: The whole population of workers eligible to the CSP entering unemployment between October, 2011 and September 2014

NOTE: This table reports the log difference in density at the threshold from the (McCrary, 2008) test performed on different subpopulations. We see that the jump at the one-year threshold is the highest for women, with a high level of skills and education, and working full-time.

Figure 2.9: Distribution of the proportion of full-time workers with respect to seniority (France, 2011-2014)

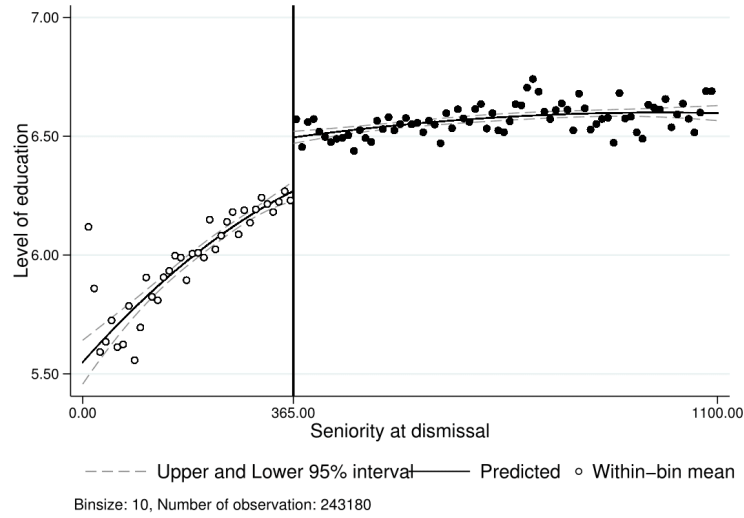


SOURCE: UI data (FNA).

SAMPLE: The whole population of unemployed persons eligible to the CSP entering unemployed between October, 2011 and September, 2014.

NOTE: this graph plots the distribution of the proportion of full-time workers with respect to their value of tenure at layoff (binsize: 10, bandwidth: 100). We observe a jump in the proportion of full-time workers at the one-year threshold, indicating that bunchers work more frequently full-time.

Figure 2.10: Distribution of the education level with respect to seniority (France, 2011-2014)

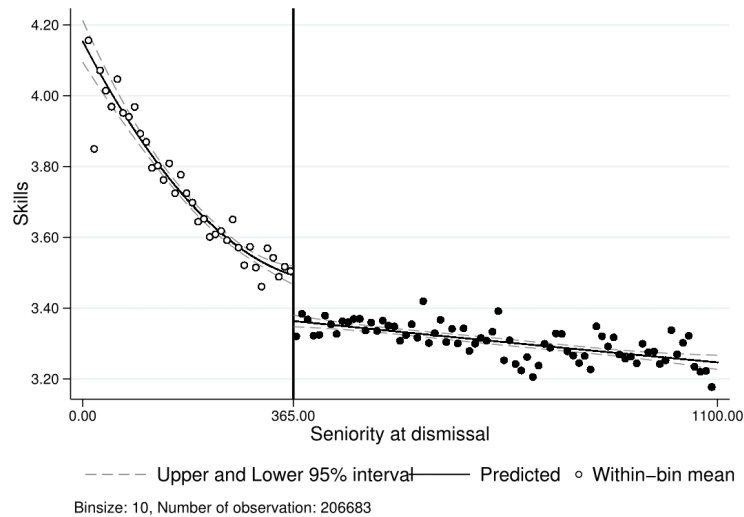


SOURCE: UI data (FNA).

SAMPLE: The whole population of unemployed persons eligible to the CSP entering unemployed between October, 2011 and September, 2014.

NOTE: This graph plots the distribution of the education level of workers with respect to their value of tenure at layoff (binsize: 10, bandwidth: 100). The education variable is coded on a scale from 0 (no education) to 10 (5 years or more of higher education). We observe a jump in the level of education at the one-year threshold, indicating that bunchers are better educated.

Figure 2.11: Distribution of the level of qualification with respect to seniority (France, 2011-2014)

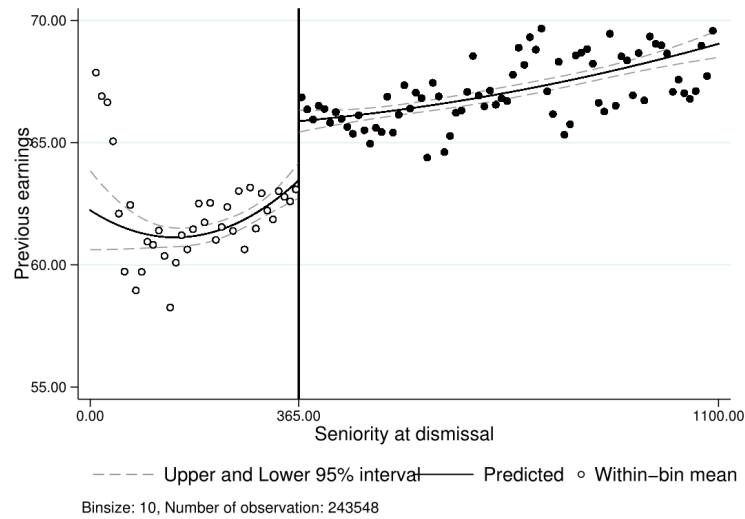


SOURCE: UI data (FNA).

SAMPLE: The whole population of unemployed persons eligible to the CSP entering unemployed between October, 2011 and September, 2014.

NOTE: This graph plots the distribution of the skill level of workers with respect to their value of tenure at layoff (binsize: 10, bandwidth: 100). The skills variable is coded on a scale from 6 (unskilled worker) to 1 (executive). We observe a jump in the level of skills at the one-year threshold, indicating that bunchers have better skills.

Figure 2.12: Distribution of the level of previous earnings with respect to seniority (France, 2011-2014)

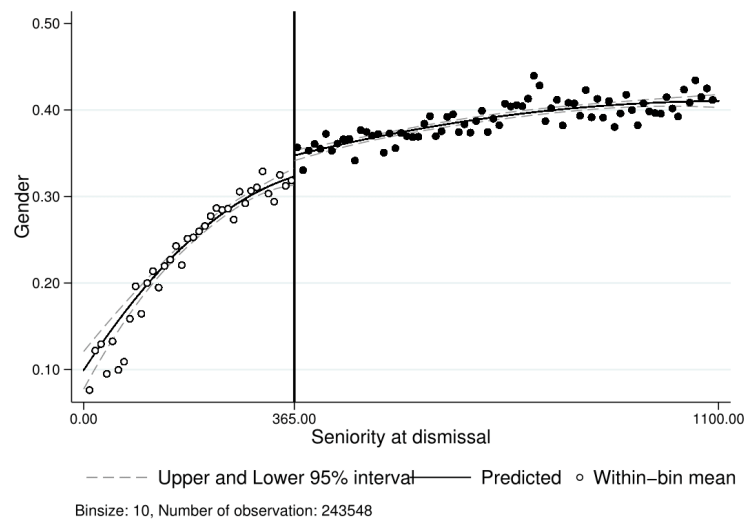


SOURCE: UI data (FNA).

SAMPLE: The whole population of unemployed persons eligible to the CSP entering unemployed between October, 2011 and September, 2014.

NOTE: This graph plots the distribution of the level of earnings of workers with respect to their value of tenure at layoff (binsize: 10, bandwidth: 100). We observe a jump in the level of earnings at the one-year threshold, indicating that bunchers are better paid.

Figure 2.13: Distribution of the proportion of women with respect to seniority (France, 2011-2014)

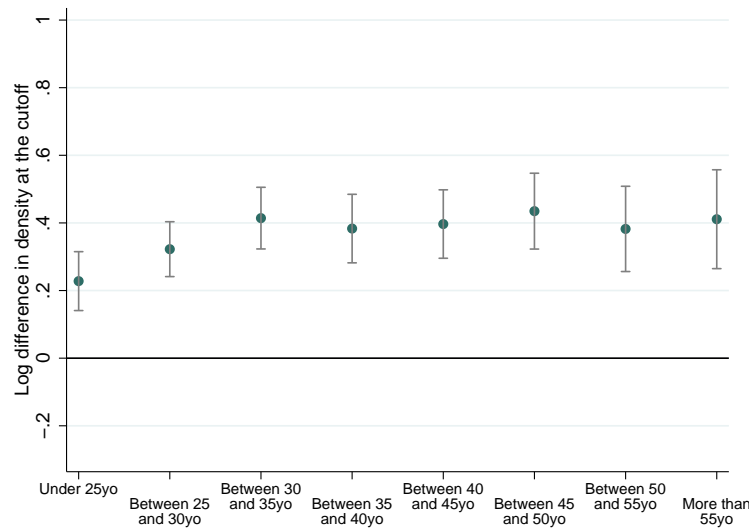


SOURCE: UI data (FNA).

SAMPLE: The whole population of unemployed persons eligible to the CSP entering unemployed between October, 2011 and September, 2014.

NOTE: This graph plots the distribution of the proportion of female workers with respect to their value of tenure at layoff (binsize: 10, bandwidth: 100). We observe a small jump in the proportion of female workers at the one-year threshold, indicating that bunchers are more frequently female.

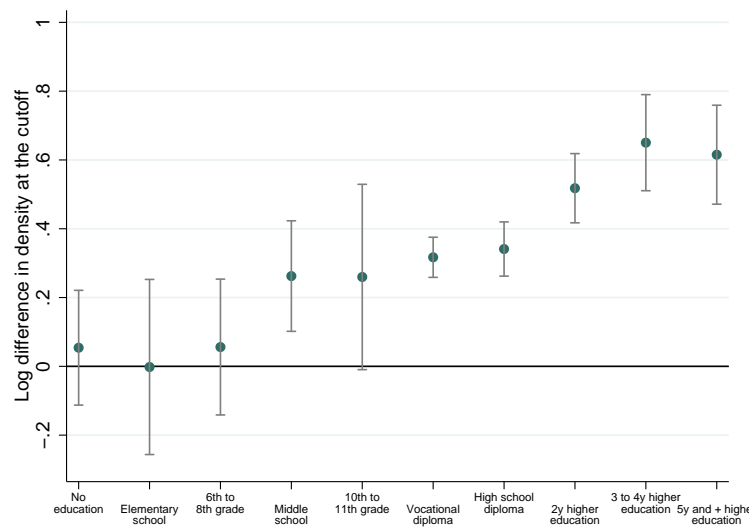
Figure 2.14: McCrary test by age group



SOURCE: UI data (FNA).

NOTE: This figure plots the coefficients from the [McCrary \(2008\)](#) test on workers eligible to the CSP, laid-off between October, 2011 and September, 2014, divided by age categories (binsize: 10, bandwidth: 100). The highest jumps in density at the one-year threshold are found for older workers.

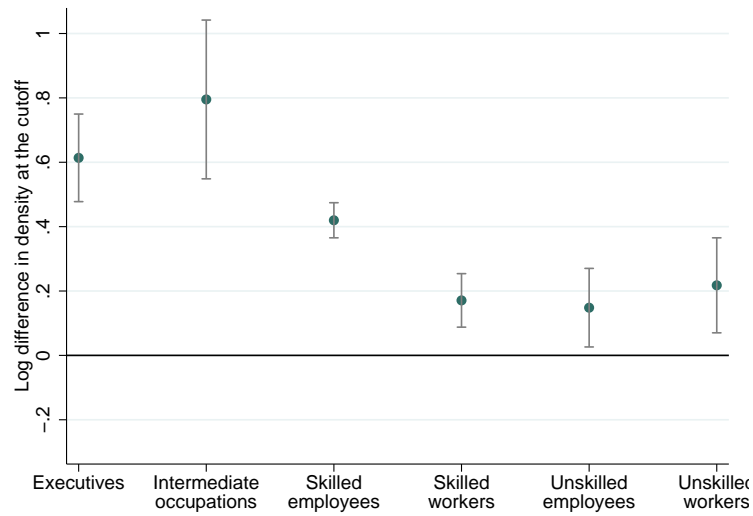
Figure 2.15: McCrary test by education group



SOURCE: UI data (FNA).

NOTE: This figure plots the coefficients from the [McCrary \(2008\)](#) test on workers eligible to the CSP, laid-off between October, 2011 and September, 2014, divided by education categories (binsize: 10, bandwidth: 100). The highest jumps in density at the one-year threshold are found for more educated workers.

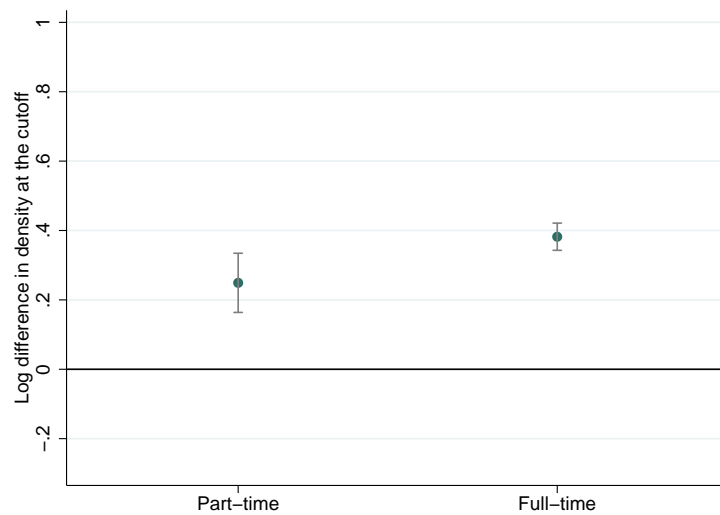
Figure 2.16: McCrary test by qualification group



SOURCE: UI data (FNA).

NOTE: This figure plots the coefficients from the [McCrary \(2008\)](#) test on workers eligible to the CSP, laid-off between October, 2011 and September, 2014, divided by skill categories (binsize: 10, bandwidth: 100). The highest jumps in density at the one-year threshold are found for more skilled workers.

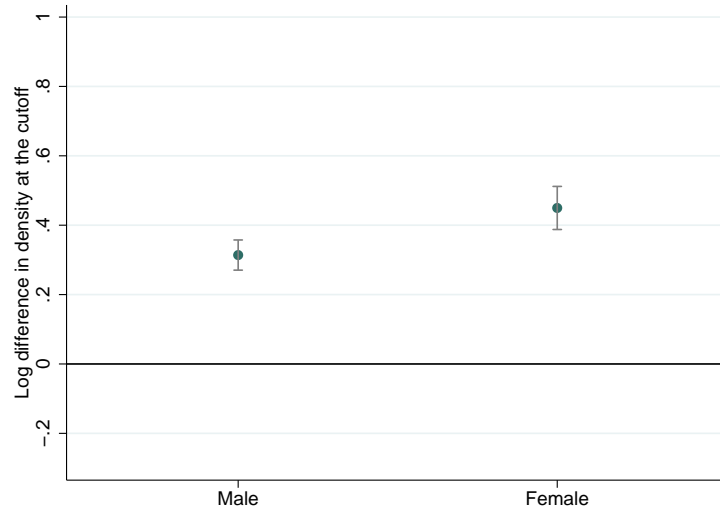
Figure 2.17: McCrary test by working time group



SOURCE: UI data (FNA).

NOTE: This figure plots the coefficients from the [McCrary \(2008\)](#) test on workers eligible to the CSP, laid-off between October, 2011 and September, 2014, divided by working time categories (binsize: 10, bandwidth: 100). The highest jumps in density at the one-year threshold are found for full-time workers.

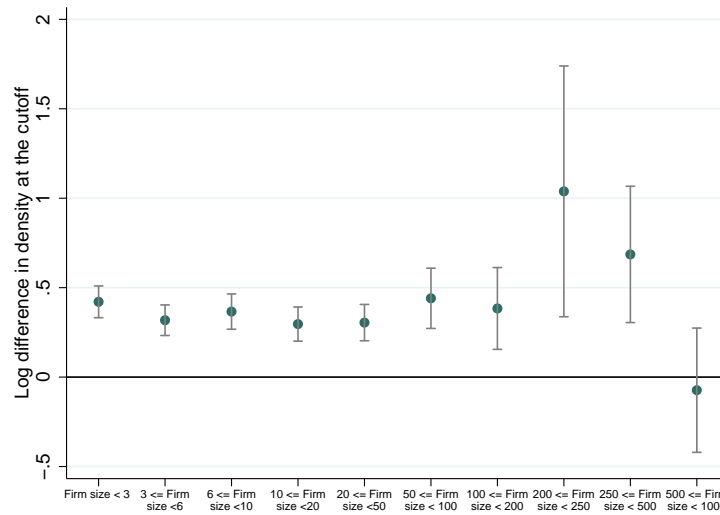
Figure 2.18: McCrary test by gender



SOURCE: UI data (FNA).

NOTE: This figure plots the coefficients from the [McCrary \(2008\)](#) test on workers eligible to the CSP, laid-off between October, 2011 and September, 2014, divided by gender categories (binsize: 10, bandwidth: 100). The highest jumps in density at the one-year threshold are found for female workers.

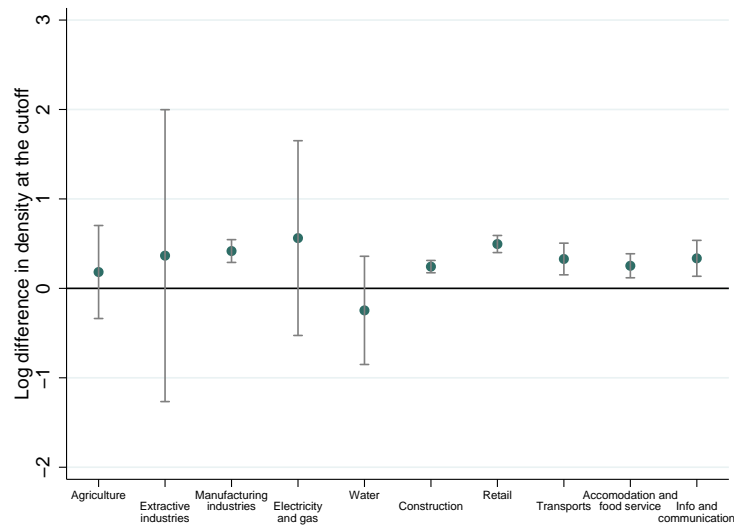
Figure 2.19: McCrary test by firm's size group



SOURCE: UI data (FNA).

NOTE: This figure plots the coefficients from the [McCrary \(2008\)](#) test on workers eligible to the CSP, laid-off between October, 2011 and September, 2014, divided by firm size categories (binsize: 10, bandwidth: 100). The highest jumps in density at the one-year threshold are found for intermediate firms.

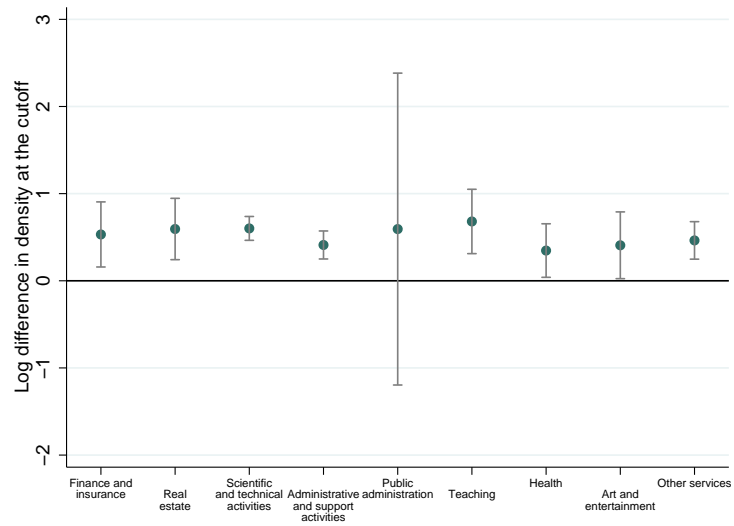
Figure 2.20: McCrary test by sector



SOURCE: UI data (FNA).

NOTE: This figure plots the coefficients from the [McCrary \(2008\)](#) test on workers eligible to the CSP, laid-off between October, 2011 and September, 2014, divided by sector of activity (binsize: 10, bandwidth: 100). There is no strong pattern relating propensity to bunch and sector of activity.

Figure 2.21: McCrary test by sector



SOURCE: UI data (FNA).

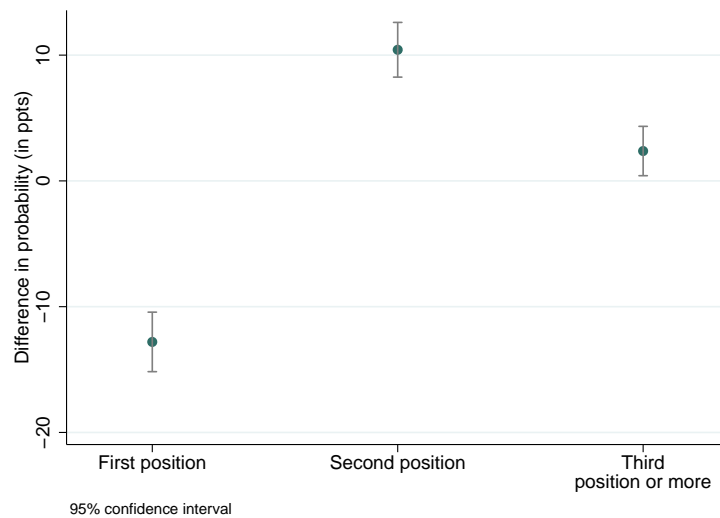
NOTE: This figure plots the coefficients from the [McCrary \(2008\)](#) test on workers eligible to the CSP, laid-off between October, 2011 and September, 2014, divided by sector of activity (binsize: 10, bandwidth: 100). There is no strong pattern relating propensity to bunch and sector of activity.

Table 2.6: Distribution of the number of workers laid-off by the same firm

Number of laid-off workers	Over the same 30-day spell		Over the full period	
	As a share of layoff spells	As a share of workers	As a share of firms	As a share of workers
1	68.49%	34.56%	58.79%	23.18%
2	16.41%	16.56%	18.68%	14.73%
3	6.19%	9.36%	8.23%	9.73%
4	2.95%	5.95%	4.26%	6.72%
5	1.71%	4.30%	2.52%	4.97%
6	1.06%	3.20%	1.68%	3.99%
7	0.69%	2.45%	1.14%	3.15%
8	0.49%	2.00%	0.84%	2.64%
9	0.35%	1.61%	0.60%	2.14%
10 and more	1.66%	20.01%	3.26%	28.76%

NOTE: This table reports the distribution of the number of workers laid-off as part of the same layoff spell, with different definitions of the spell. The sample is made of laid-off workers eligible to the CSP. The full period of observation is October, 2011-September, 2014. A layoff spells gathers all layoffs occurring on the same 30-day period, as this is the legal period to assess whether the redundancy is collective or not. Most of the workers are laid-off as part of individual or two-worker layoffs.

Figure 2.22: Layoff order within the same layoff plan with respect to the distance from the cutoff (October 2011-September 2014)



SOURCE: UI data (FNA).

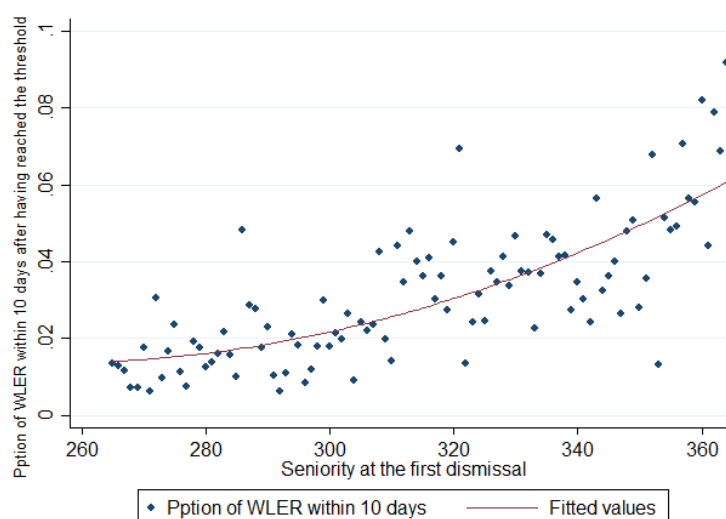
NOTE: The layoff plan gathers all layoffs from the same employer on a 30 days period. Being right above the cutoff means having a seniority lying between 365 and 380 days (included). Workers with a seniority right above the cutoff have a higher probability of being laid-off in the second, third, or higher position within a layoff spell. It suggests that employers have waited for them to cross the cutoff before dismissing them.

Table 2.7: Layoff rank within the same redundancy plan

Layoff rank	Far from the threshold		Right above the threshold	
	$Seniority < 365$ or $Seniority > 380$	$Seniority < 365$ or $Seniority > 390$	$365 \leq Seniority \leq 380$	$365 \leq Seniority \leq 390$
1 st position	47.9%	47.9%	35.1%	35.7%
2 nd position	30.2%	30.1%	40.6%	41%
3 rd position	22%	22%	24.4%	23.3%

NOTE: This table reports the layoff rank of workers eligible to the CSP, laid-off between October, 2011 and September, 2014, as part of collective layoff plans. I consider all the plans gathering more than one layoff at different dates from the same employer within a 30 days period (excluding single layoff plans and layoffs where the employer identifier is missing). I then compute the chronological order of the layoffs and I compare the rank according to the distance of the seniority from the threshold. It shows that workers right above the cutoff are more frequently laid-off in the last position, suggesting that employers wait for them to cross the cutoff and start by dismissing other employees.

Figure 2.23: Proportion of workers laid-off after a first layoff right after one year of tenure among workers laid-off in the same redundancy plan (France, October 2011-September 2014)



SOURCE: UI data (FNA).

NOTE: This graph plots the proportion of workers (i) laid-off as part of a collective layoff plan; (ii) not laid-off first (which suggest a waiting time); (iii) laid-off just (i.e. within 10 days) after reaching the one year condition (which suggests the waiting time was related to the CSP), among workers fulfilling conditions (i) and (ii), as a function of the tenure value at the moment of the first layoff of the redundancy plan. The redundancy plan gathers all layoffs from the same employer on a one year period. This graph suggests that, in collective redundancy plans, employers would wait to layoff workers initially right below the cutoff. They will not be laid-off first in the plan, which would allow them to reach the cutoff.

Table 2.8: Proportion of firms shutting down

Proportion of firms whose workforce = Nb of people laid-off		Proportion of firms whose workforce = Nb of people laid-off ± 5		Proportion of firms whose workforce = Nb of people laid-off ± 10	
<i>Seniority</i> < 365 or <i>Seniority</i> > 380	$365 \geq \textit{Seniority} \leq 380$	<i>Seniority</i> < 365 or <i>Seniority</i> > 380	$365 \geq \textit{Seniority} \leq 380$	<i>Seniority</i> < 365 or <i>Seniority</i> > 380	$365 \geq \textit{Seniority} \leq 380$
6.9%	10.9%	35.3%	47.8%	48.6%	62.8%
<i>Seniority</i> < 365 or <i>Seniority</i> > 390	$365 \geq \textit{Seniority} \leq 390$	<i>Seniority</i> < 365 or <i>Seniority</i> > 390	$365 \geq \textit{Seniority} \leq 390$	<i>Seniority</i> < 365 or <i>Seniority</i> > 390	$365 \geq \textit{Seniority} \leq 390$
6.9%	10.4%	35.3%	47.1%	48.5%	62.2%

NOTE: This table reports the proportion of workers in firms shutting down (using different definitions) among workers eligible to the CSP, laid-off between October, 2011 and September, 2014, and classified according to their tenure at layoff. Firms shutting down are defined as firms with a number of people laid-off approximately equal to the firm size. Workers close to the cutoff are more frequently present in firms shutting down. It is compatible with the hypothesis that employers in firms shutting down have nothing to lose anymore and are therefore more willing to grant the contract extension.

Table 2.9: Predicted correlations with the likelihood to bunch

Parameter	Correlation
Distance to the one-year threshold d	(-)
Baseline supra-legal severance package t^0	(+)
After-shock productivity P	(+)
Jump in utility from unemployment $V_u^d - V_u^0$	(+)
Standard benefit SB	(-)
Wage w	(-)
Disutility from work α	(-)

NOTE: This table describes the predicted correlations derived from the model developed in Section 4. In particular, the likelihood to bunch is positively correlated with the level of severance pay absent the extension of the contract, which represents how much the worker can transfer to the firm, or with the productivity or the jump in benefits at the threshold. On the reverse, it is negatively correlated with the distance to the threshold, as it increases the cost of extension, or the standard benefit, the wage or the disutility from work.

Table 2.10: Gain distribution of the sample under 2 years of tenure

Gain category	Number of observations	Share in total sample
$\textit{Gain} < 10\textit{ppts}$	5,916	3.47%
$10\textit{ppts} \leq \textit{Gain} < 15\textit{ppts}$	20,311	11.9%
$15\textit{ppts} \leq \textit{Gain} < 20\textit{ppts}$	62,288	36.5%
$\textit{Gain} \geq 20\textit{ppts}$	82,189	48.2%

NOTE: This table presents the gain distribution of the sample of workers eligible to the CSP, laid-off between October, 2011 and September, 2014, under two years of tenure. The gain is defined as the difference in replacement rate between the standard benefit and the CSP benefit granted to those with a tenure of one year of more. This difference increases with previous earnings, as the replacement rate associated with the standard benefit decreases with earnings. The majority of the sample has a jump in replacement rate higher than 20 percentage points.

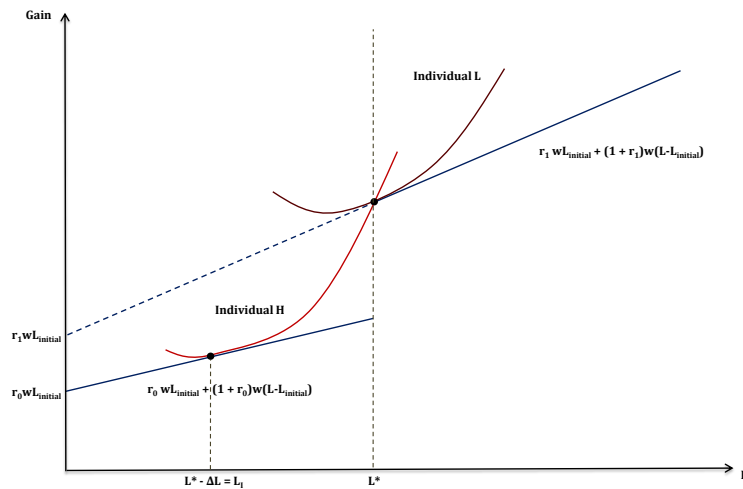
Table 2.11: Log discontinuity estimates according to potential gain

Potential gain from ARE to CSP	Log discontinuity estimates
$Gain < 10ppts$.1074 (.1031)
$10ppts \leq Gain < 15ppts$.2673*** (.0513)
$15ppts \leq Gain < 20ppts$.3012*** (.03023)
$Gain \geq 20ppts$.4640*** (.0265)

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

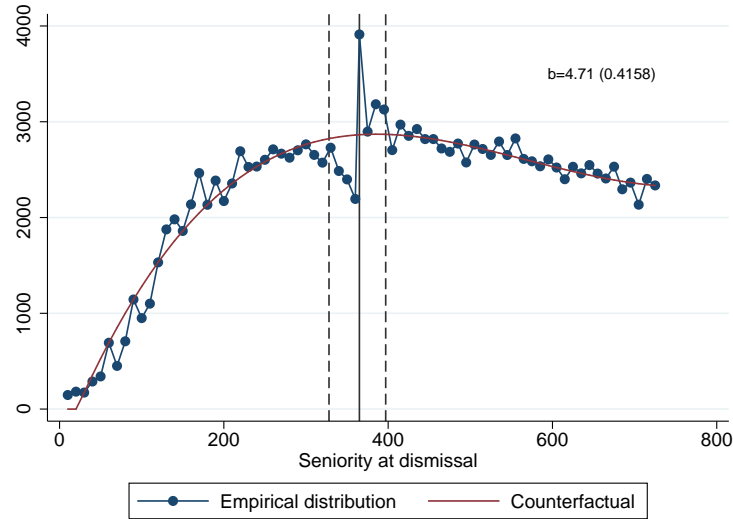
NOTE: This table reports the log difference estimates from the Mc Crary (McCrory, 2008) test performed on the density of tenure at layoff at the one-year threshold. It has been done on the sample of worker laid-off during the October, 2011-September, 2014 period, separately for each potential gain category. The potential gain category is defined according to the difference in replacement rate between the standard benefit and the CSP benefit at the one-year threshold. The table indicates that, as the potential gain increases, the discontinuity increases as well.

Figure 2.24: Notch in the budget set



NOTE: This diagram describes how individuals will bunch at the same point of the distribution of tenure when a notch is introduced in the level of UI benefits that depends on the value of tenure. The replacement rate jumps from r_0 to $r_0 + \Delta r = r_1$ at L^* , making all individuals located between $L^* - \Delta L^*$ and L^* on the pre-notch distribution bunch at the notch point. The marginal buncher is the one who is indifferent between the notch point L^* and the best interior solution L^I with $L^I < L^*$ after the introduction of the notch.

Figure 2.25: Empirical and counterfactual distributions of seniority at layoff (October 2011-September 2014)



SOURCE: UI data (FNA).

NOTE: This graph plots in blue the empirical distribution of seniority at layoff for workers eligible to the CSP, laid-off between October, 2011 and September, 2014 (binsize: 10). The counterfactual distribution has been computed by fitting a 4th order polynomial, excluding an area around the notch, and extrapolating the distribution in the excluded area. The excluded area upper bound has been determined visually where the bunching stops. The lower bound has been found through an iterative process such that the excess and missing masses equalise (see Section 5.1 for more details). The upper and lower bounds are represented by the dashed lines. The solid vertical line represents the one-year cutoff, where there are incentives to bunch during this period. The figure exhibits significant bunching at the notch, other than related to a round-number effect.

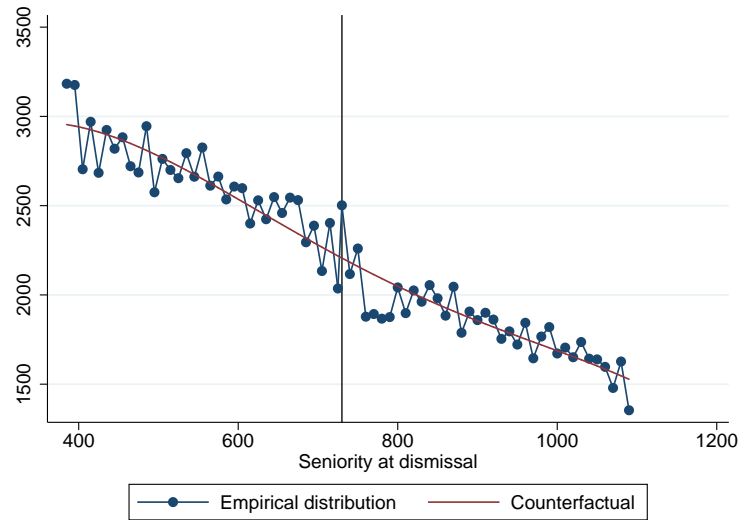
Table 2.12: Seniority count regression

<i>Seniority</i>	2.081*** (.227)	2.105*** (.233)	.598 (.342)	.558 (.350)	2.243*** (.114)	1.308*** (.160)
<i>Seniority</i> ²	-.004** (.001)	-.004** (.001)	.002 (.002)	.002 (.002)	-.005*** (.0004)	-.003*** (.001)
<i>Seniority</i> ³	1.68e-06 (2.66e-06)	1.78e-06 (2.79e-06)	-7.47e-06 (3.99e-06)	-8.25e-06* (4.20e-06)	4.07e-06*** (5.69e-07)	2.01e-06* (7.97e-07)
<i>Seniority</i> ⁴	5.32e-10 (1.81e-09)	5.15e-10 (1.90e-09)	5.85e-09* (2.72e-09)	6.39e-09* (2.86e-09)	-1.20e-09*** (2.56e-10)	-5.36e-10 (3.59e-10)
Bunching area below	-55.060*** (16.304)	-38.832** (11.862)	-24.386 (24.521)	-18.560 (17.842)	-35.893*** (9.909)	-2.424 (13.880)
Bunching area above	76.005*** (19.158)	43.048*** (12.423)	33.0277 (28.814)	5.216 (18.686)	44.572*** (10.876)	20.397 (15.234)
Constant	-38.640** (11.828)	-39.689*** (11.905)	32.551 (17.789)	33.477 (17.907)	-43.985*** (8.976)	7.974 (12.572)
$L_u - L^*$	12	33	12	33	33	33
$L^* - L_l$	17	37	17	37	41	41
Period	Oct, 11 - Oct, 14	Oct, 11 - Oct, 14	Sep, 09 - Sep, 11	Sep, 09 - Sep, 11	Oct, 11 - Oct, 14	Sep, 09 - Sep, 11
Window	< 730	< 730	< 730	< 730	< 1100	< 1100

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

NOTE: This table reports the number of workers laid-off among workers eligible to the CSP from August, 2009 to September, 2014, regressed on a 4th order polynomial of the seniority value, and a dummy for being laid-off within the bunching area below or above the one-year cutoff. The regressions have been made separately on the period where the CSP and the CRP applied. The different columns correspond to different seniority windows considered in the regression. The dummy for being laid-off within the area right below the one-year cutoff is systematically associated to a negative coefficient during the CSP period, and a non significant one during the CRP period. Conversely, the dummy for being laid-off within the area right above the one-year cutoff is systematically associated to a positive coefficient during the CSP period, and a non significant one during the CRP period. It means that when the CSP applies, there is a significantly lower number of layoffs right below the one-year cutoff, and a significantly higher number of layoffs right above the one-year cutoff. The fact that this is not the case during the pre-CSP period indicates that this pattern is due to the CSP.

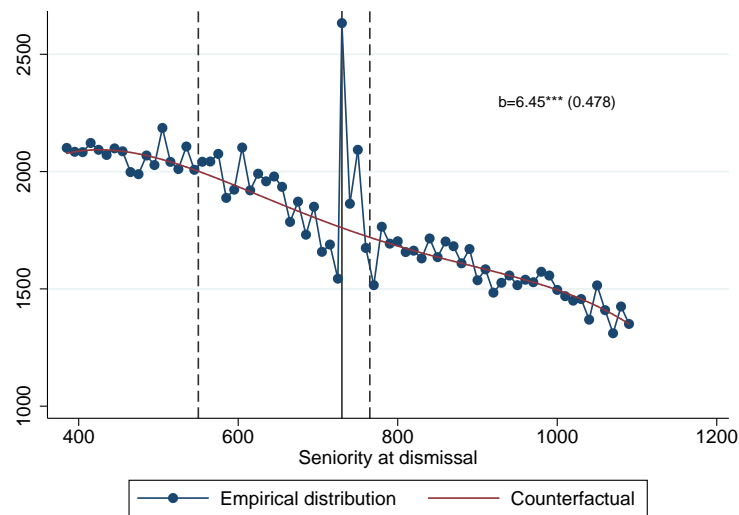
Figure 2.26: Empirical and counterfactual distributions of seniority at layoff, October 2011-September 2014, 2y cutoff



SOURCE: UI data (FNA).

NOTE: This graph plots in blue the empirical distribution of seniority at layoff for workers eligible to the CSP, laid-off between October, 2011 and September, 2014 (binsize: 10). The counterfactual distribution has been computed by fitting a 4th order polynomial, excluding an area around the notch, and extrapolating the distribution in the excluded area. The excluded area upper bound has been determined visually where the bunching stops. The lower bound has been found through an iterative process such that the excess and missing masses equalise (see Section 5.1 for more details). The upper and lower bounds are represented by the dashed lines. The solid vertical line represents the two-year cutoff, where there are no incentives to bunch during this period. The figure exhibits no significant bunching at the notch, other than related to a round-number effect.

Figure 2.27: Empirical and counterfactual distributions of seniority at layoff, August 2009-August 2011, 2y cutoff



SOURCE: UI data (FNA).

NOTE: This graph plots in blue the empirical distribution of seniority at layoff for workers eligible to the CSP, laid-off between August, 2009 and August, 2011 (binsize: 10). The counterfactual distribution has been computed by fitting a 4th order polynomial, excluding an area around the notch, and extrapolating the distribution in the excluded area. The excluded area upper bound has been determined visually where the bunching stops. The lower bound has been found through an iterative process such that the excess and missing masses equalise (see Section 5.1 for more details). The upper and lower bounds are represented by the dashed lines. The solid vertical line represents the two-year cutoff, where there are incentives to bunch during this period. The figure exhibits significant bunching at the notch, similar to the one observed at the one-year cutoff between October, 2011 and September 2014.

Table 2.13: Reduced-form elasticity estimates

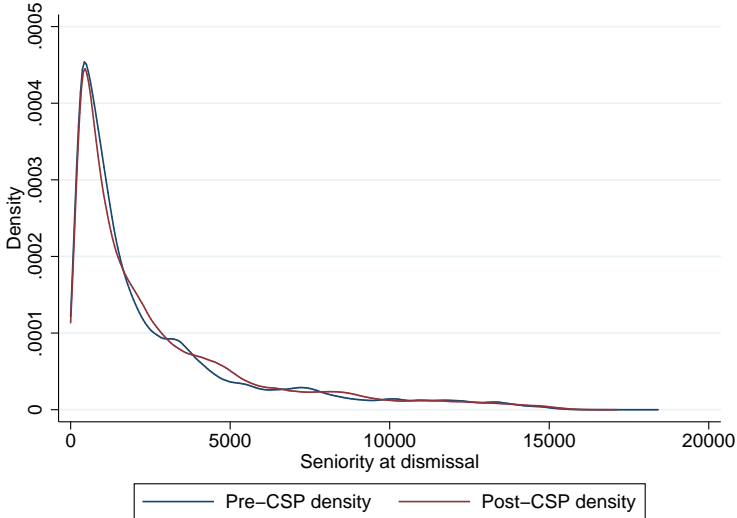
Seniority window	[120;540]	[180;540]	[0;730[[0;1100[
L_u	398	398	398	398				
L_l	309*** (4.13)	323*** (11)	338*** (5.46)	339*** (4.69)				
b	5.55*** (0.3181)	5.01*** (0.3478)	4.72*** (0.3462)	4.56*** (0.3295)				
m	5.59*** (0.342)	5.05*** (0.37)	4.7*** (0.3564)	4.53*** (0.3209)				
% change in replacement rate	12	11.98	11.8	12				
	Lower bound	Upper bound	Lower bound	Upper bound	Lower bound	Upper bound	Lower bound	Upper bound
ϵ_{RF}	0.0173*** (0.0012)	0.3468*** (0.0369)	0.0154*** (0.0012)	0.2312*** (0.0864)	0.0143*** (0.0012)	0.1194*** (.0374)	0.0137*** (0.0011)	0.1136*** (0.0318)

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

NOTE: The elasticity is computed using the same formula as in Chetty et al. (2011) adjusted for the notch $\frac{(m/L^*)^2}{\Delta r / (1+r_0+L^*\Delta r/\Delta L^*)}$ for the lower bound, and using $\frac{(\Delta L^*/L^*)^2}{\Delta r / (1+r_0+L^*\Delta r/\Delta L^*)}$ with ΔL^* measured as the distance between the point of divergence and the notch point for the upper bound. More detailed explanations on the method can be found in Section 5.1. The different columns correspond to different tenure windows considered in the estimation.

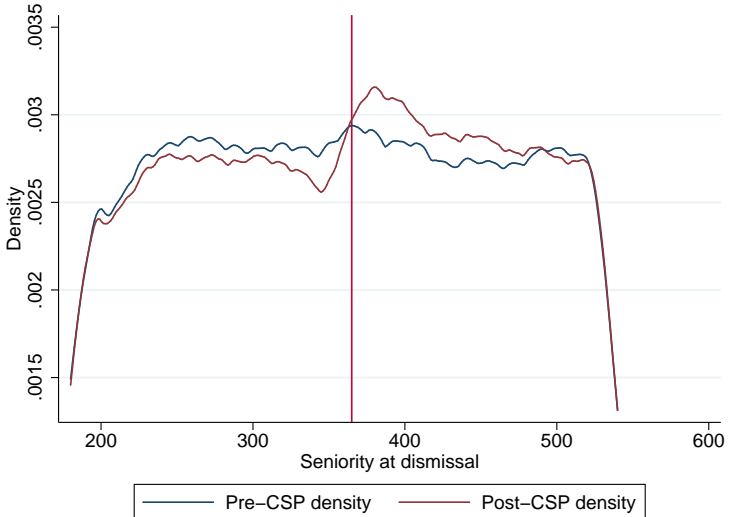
Standard errors are calculated using a bootstrap procedure generating seniority distributions and associated estimates by random resampling. 600 replications.

Figure 2.28: Pre and Post-CSP seniority density on the whole distribution



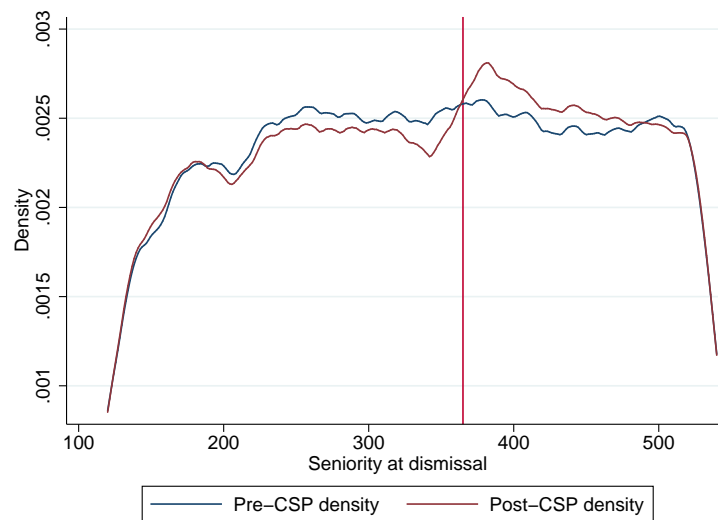
SOURCE: UI data (FNA).
NOTE: This graph plots the density of tenure at layoff for workers eligible to the CSP, respectively for the pre-CSP period (August, 2009-August, 2011) and the post-CSP period (October, 2011-September, 2014) on the whole distribution. It shows that the density in the pre-CSP and post-CSP period look similar, demonstrating that the pre-CSP density provides a good counterfactual distribution to measure the bunching exclusively related to the incentives created by the CSP.

Figure 2.29: Pre and Post-CSP seniority density around the notch (180-540 days)



SOURCE: UI data (FNA). Seniority window: 180-540 days.
NOTE: This graph plots the density of tenure at layoff for workers eligible to the CSP, respectively for the pre-CSP period (August, 2009-August, 2011) and the post-CSP period (October, 2011-September, 2014), between 6 and 18 months of tenure. The vertical line corresponds to the one-year cutoff. It shows that the density in the pre-CSP period is pretty flat in the bunching area, whereas we observe a clear hole before and a mass after the one-year threshold in the post-CSP density. It demonstrates that the pre-CSP density provides a good counterfactual distribution to measure the bunching exclusively related to the incentives created by the CSP.

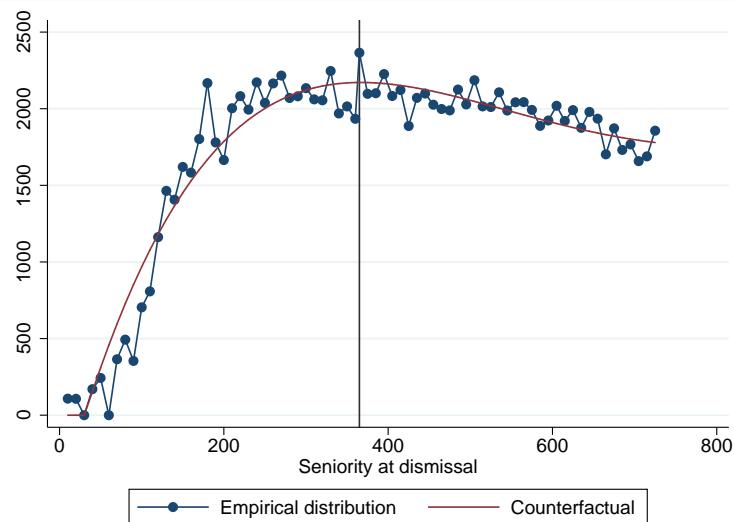
Figure 2.30: Pre and Post-CSP seniority density around the notch (120-540 days)



SOURCE: UI data (FNA). Seniority window: 120-540 days.

NOTE: This graph plots the density of tenure at layoff for workers eligible to the CSP, respectively for the pre-CSP period (August, 2009-August, 2011) and the post-CSP period (October, 2011-September, 2014), between 4 and 18 months of tenure. The vertical line corresponds to the one-year cutoff. It shows that the density in the pre-CSP period is pretty flat in the bunching area, whereas we observe a clear hole before and a mass after the one-year threshold in the post-CSP density. It demonstrates that the pre-CSP density provides a good counterfactual distribution to measure the bunching exclusively related to the incentives created by the CSP.

Figure 2.31: Empirical and counterfactual distributions of seniority at layoff, August 2009-August 2011, 1y cutoff



SOURCE: UI data (FNA).

NOTE: This graph plots in blue the empirical distribution of seniority at layoff for workers eligible to the CSP, laid-off between August, 2009 and August, 2011 (binsize: 10). The counterfactual distribution has been computed by fitting a 4th order polynomial, excluding an area around the notch, and extrapolating the distribution in the excluded area. The excluded area upper bound has been determined visually where the bunching stops. The lower bound has been found through an iterative process such that the excess and missing masses equalise (see Section 5.1 for more details). The upper and lower bounds are represented by the dashed lines. The solid vertical line represents the one-year cutoff, where there are no incentives to bunch during this period. The figure exhibits no significant bunching at the notch, other than related to a round-number effect.

Table 2.14: Difference-in-bunching elasticity estimates

Time period	Sept, 2009 - Sept, 2014			Jan, 2011 - June, 2012	
L_u	395	395	395	395	395
L_l	325*** (11.7)	324*** (12.02)	325*** (13.65)	308*** (11.89)	308*** (11.87)
Seniority window	[120;540]	[180;540]	[0;730[[120;540]	[180;540]
b	3.24*** (0.5525)	3.04*** (0.5043)	3.75*** (0.702)	5.4*** (0.8718)	5.04*** (0.9118)
m	3.18*** (0.5141)	3.03*** (0.4592)	3.7*** (0.6763)	5.37*** (0.8941)	4.93*** (0.8429)
ϵ_{RF}^{ub}	0.2137** (0.0979)	0.223* (0.115)	0.2197* (0.1158)	0.3708*** (0.1176)	0.3694*** (0.1168)
ϵ_{RF}^{lb}	0.0094*** (0.0016)	0.0089*** (0.0014)	0.011*** (0.0022)	0.0166*** (0.003)	0.0151*** (0.0028)

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

NOTE: The elasticity is computed using the same formula as in [Chetty et al. \(2011\)](#) adjusted for the notch $\frac{(m/L^*)^2}{\Delta r / (1 + r_0 + L^* \Delta r / \Delta L^*)}$ for the lower bound, and using $\frac{(\Delta L^* / L^*)^2}{\Delta r / (1 + r_0 + L^* \Delta r / \Delta L^*)}$ with ΔL^* measured as the distance between the point of divergence and the notch point for the upper bound. The counterfactual density is derived from pre-CSP distribution of tenure. The first three columns correspond to the full time period (September, 2009-September, 2014), whereas the last two columns correspond to a restricted 8-month window on each side of the reform, to avoid capturing the effect of different economic conditions. Within each time period, the different columns correspond to different tenure windows considered for the estimation. Standard errors are calculated using a bootstrap procedure generating seniority distributions and associated estimates by random resampling. 200 replications.

Table 2.15: Characteristics of compliers, eligible and never-takers

	Compliers	Never-takers	Eligibles	Difference (2)-(1)	Difference (2)-(3)	Difference (1)-(3)
Age	36.34 (1.801)	35.57 (0.112)	35.58 (0.042)	-0.763*** (0.128)	-0.001 (0.008)	0.762*** (0.127)
Gender	0.41 (0.027)	0.31 (0.004)	0.33 (0.002)	-0.095*** (0.002)	-0.017*** (0.000)	0.079*** (0.002)
Education	7.13 (0.313)	6.22 (0.022)	6.31 (0.008)	-0.908*** (0.022)	-0.084*** (0.002)	0.824*** (0.022)
Skills	2.89 (0.182)	3.52 (0.013)	3.47 (0.005)	0.631*** (0.013)	0.050*** (0.001)	-0.581*** (0.013)
Working time	0.95 (0.046)	0.94 (0.002)	0.94 (0.001)	-0.007* (0.003)	-0.002*** (0.000)	0.005 (0.003)
Firm size	17.32 (4.645)	28.73 (0.938)	29.63 (0.472)	11.411*** (0.335)	-0.899*** (0.074)	-12.310*** (0.330)

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

NOTE: The method described in Section 5.2 is applied to workers eligible to the CSP, laid-off between October, 2011 and September, 2014, to compare the characteristics of workers defined as compliers (bunching), always-takers (in the bunching area in the counterfactual and observed distributions of tenure at layoff) and never-takers (in the area below the cutoff in the counterfactual and observed distributions of tenure at layoff). The bunching boundaries are located at 324 days and 397 days. The area used to estimate the counterfactual is included between 0 and 1100 days. The polynomial fitting the seniority bin count is of order 4. Standard errors are calculated using a bootstrap procedure generating seniority distributions and associated estimates by random resampling (200 replications).

Skills are given from a scale from 1 (high skills) to 6 (low skills). Education ranges from 1 (no education) to 10 (5 years or more of higher education).

Table 2.16: Elasticity estimates by gain categories

Gain category	Average gain in replacement rate (in %)	L_u	L_l	b	m	ϵ_{RF}^{lb}	ϵ_{RF}^{ub}
$Gain < 10ppts$	3.58	369	340*** (14.91)	1.8 (1.2776)	1.69 (1.1987)	0.00545 (0.0044)	0.2181 (0.1968)
$10ppts \leq Gain < 15ppts$	8.66	398	339*** (6.04)	3.77*** (.8895)	3.76*** (.8906)	0.0113*** (0.0026)	0.1311*** (0.0477)
$15ppts \leq Gain < 20ppts$	10.95	398	342*** (5.14)	3.56*** (0.5203)	3.53*** (0.5164)	0.0106*** (0.0017)	0.0993*** (0.0336)
$Gain \geq 20ppts$	14.04	398	334*** (8.07)	5.99*** (0.4764)	6.02*** (0.4704)	0.0184*** (0.0016)	0.1412*** (0.0512)

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

NOTE: The elasticity is computed separately for each gain category, using the same formula as in Chetty et al. (2011) adjusted for the notch $\frac{(m/L^*)^2}{\Delta r / (1+r_0+L^*\Delta r/\Delta L^*)}$ for the lower bound, and using $\frac{(\Delta L^*/L^*)^2}{\Delta r / (1+r_0+L^*\Delta r/\Delta L^*)}$ with ΔL^* measured as the distance between the point of divergence and the notch point for the upper bound. The area used for estimating the counterfactual is included between 0 and 730 days. The polynomial fitting the seniority bin count is of order 4. The potential gain category is defined according to the difference in replacement rate between the standard benefit and the CSP benefit at the one-year threshold. The table indicates that, as the potential gain increases, the elasticity increases as well. Standard errors are calculated using a bootstrap procedure generating seniority distributions and associated estimates by random resampling. 200 replications.

Table 2.17: Descriptive statistics by gain category

	<i>Gain</i> < 10 <i>ppts</i>	10 <i>ppts</i> ≤ <i>Gain</i> < 15 <i>ppts</i>	15 <i>ppts</i> ≤ <i>Gain</i> < 20 <i>ppts</i>	<i>Gain</i> ≥ 20 <i>ppts</i>	Total
Sex					
Male	40.3	46.1	54.7	65.4	59.9
Female	59.7	53.9	45.3	34.6	40.1
Education level					
Not any education	7.7	7.6	5.4	3.6	4.6
Primary school	4.6	3.8	3.1	2.0	2.6
Primary school to 8 th grade	4.7	4.6	3.4	2.1	2.8
9 th grade	8.6	8.0	6.3	4.1	5.2
10 th -11 th grade	2.2	2.1	1.4	0.9	1.2
Vocational diploma (CAP/BEP)	39.2	40.9	45.1	35.8	39.3
High school diploma	18.8	19.9	20.3	18.1	19.0
2 years of higher education	8.1	8.1	9.9	16.6	13.6
3-4 years of higher education	2.2	1.6	1.5	8.8	5.7
5 years or more of higher education	3.9	3.4	3.4	7.9	6.0
Qualification					
Executives	0.6	0.4	1.0	17.9	10.7
Intermediate professions	0.4	0.6	1.2	5.4	3.5
Unskilled employees	22.0	16.7	10.2	3.7	7.2
Skilled employees	52.3	54.5	54.7	48.5	51.1
Unskilled workers	10.3	10.0	8.8	4.5	6.4
Skilled workers	14.5	17.8	24.0	20.1	21.1

NOTE: This table presents descriptive statistics of the sample of workers eligible to the CSP, laid-off between October, 2011 and September, 2014, divided by gain categories. The gain is defined as the difference in replacement rate between the standard benefit and the CSP benefit granted to those with a tenure of one year of more. This difference increases with previous earnings, as the replacement rate associated with the standard benefit decreases with earnings. Proportions of male, of highly educated and of highly skilled individuals increase with gain category.

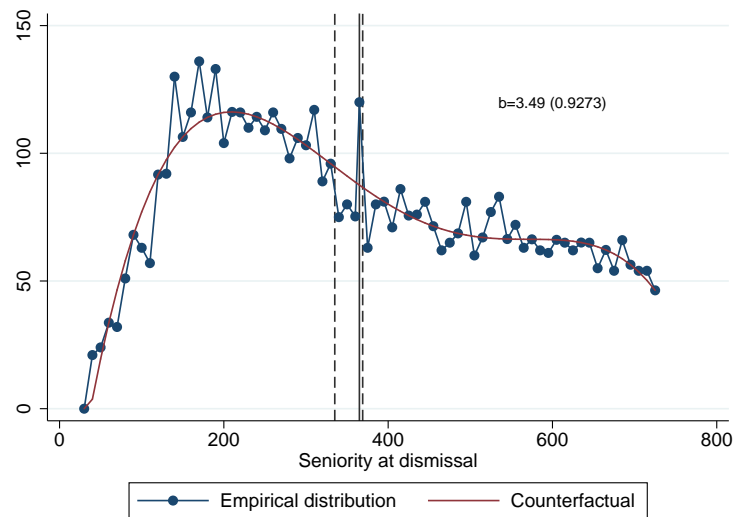
Table 2.18: Differences in observable characteristics by gain categories

	Age	Sex	Education level	Proportion of executives	Proportion of intermediate professions	Proportion of unskilled employees	Proportion of skilled employees	Proportion of unskilled workers	Proportion of skilled workers
2 nd gain category	1.0625*** (.1091)	-.0573*** (.0049)	.0202 (.0203)	-.0013 (.0033)	.0017 (.0021)	-.0526*** (.0029)	.0224*** (.0056)	-.0032 (.0027)	.0329*** (.0046)
3 rd gain category	2.0151*** (.1008)	-.144*** (.0046)	.279*** (.0188)	.0048 (.0031)	.0078*** (.0019)	-.1175*** (.0026)	.0246*** (.0052)	-.015*** (.0025)	.0954*** (.0042)
4 th gain category	5.8161*** (.0996)	-.2507*** (.0045)	1.0104*** (.0186)	.173*** (.0031)	.0496*** (.0019)	-.1827*** (.0026)	-.038*** (.0051)	-.0577*** (.0025)	.0557*** (.0042)
Constant	36.5246*** (.0980)	.5967*** (.0044)	5.6701*** (.01825)	.0055 (.003)	.0041* (.0019)	.2199*** (.0026)	.5228*** (.005)	.1029*** (.0025)	.1448*** (.0041)

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

NOTE: This table presents descriptive statistics of the sample of workers eligible to the CSP, laid-off between October, 2011 and September, 2014, divided by gain categories. The reference category is the 1st gain category. The gain is defined as the difference in replacement rate between the standard benefit and the ASP granted to those with a seniority of one year of more. This difference increases with previous earnings, as the replacement rate associated with the standard benefit decreases with earnings. Proportions of male, of highly educated and of highly skilled individuals increase with gain category.

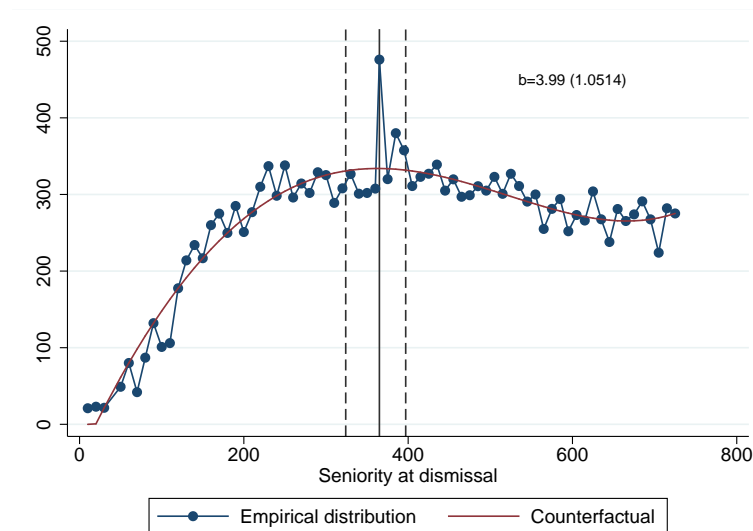
Figure 2.32: Empirical and counterfactual distributions of seniority at layoff, 1st gain category (October 2011-September 2014)



SOURCE: UI data (FNA).

NOTE: This graph plots in blue the empirical distribution of seniority at layoff for workers eligible to the CSP, laid-off between October, 2011 and October, 2014, and part of the 1st gain category (binsize: 10). The counterfactual distribution has been computed by fitting a 4th order polynomial, excluding an area around the notch, and extrapolating the distribution in the excluded area. The excluded area upper bound has been determined visually where the bunching stops, and corresponds to 369 days. The lower bound has been found through an iterative process such that the excess and missing masses equalise (see Section 5.1 for more details), and corresponds to 335 days. The two dashed vertical lines represent the excluded area bounds, and the solid vertical line represents the one-year cutoff. The figure exhibits no significant bunching at the notch, other than related to a round-number effect. The gain is defined as the difference in replacement rate between the standard benefit and the ASP granted to those with a seniority of one year of more. This difference increases with previous earnings, as the replacement rate associated with the standard benefit decreases with earnings. The first gain category corresponds to a gain lower than 10 percentage points.

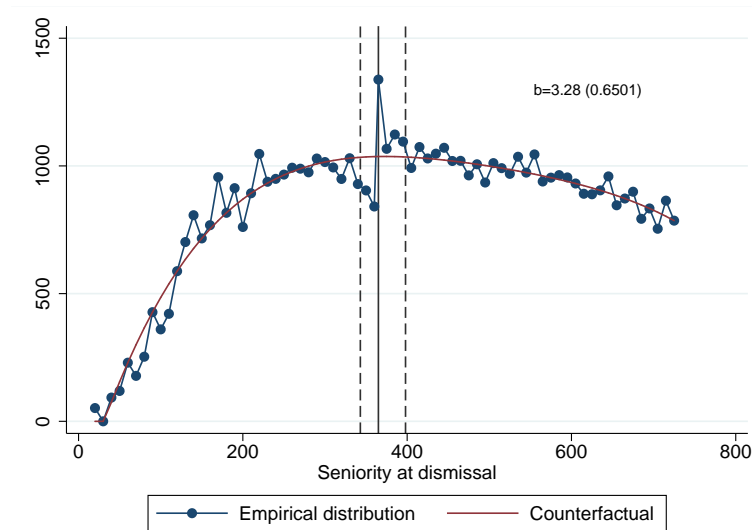
Figure 2.33: Empirical and counterfactual distributions of seniority at layoff, 2nd gain category (October 2011-September 2014)



SOURCE: UI data (FNA).

NOTE: This graph plots in blue the empirical distribution of seniority at layoff for workers eligible to the CSP, laid-off between October, 2011 and October, 2014, and part of the 2nd gain category (binsize: 10). The counterfactual distribution has been computed by fitting a 4th order polynomial, excluding an area around the notch, and extrapolating the distribution in the excluded area. The excluded area upper bound has been determined visually where the bunching stops, and corresponds to 397 days. The lower bound has been found through an iterative process such that the excess and missing masses equalise (see Section 5.1 for more details), and corresponds to 324 days. The gain is defined as the difference in replacement rate between the standard benefit and the ASP granted to those with a seniority of one year of more. This difference increases with previous earnings, as the replacement rate associated with the standard benefit decreases with earnings. The second gain category corresponds to a gain higher or equal to 10 percentage points and lower than 15 percentage points.

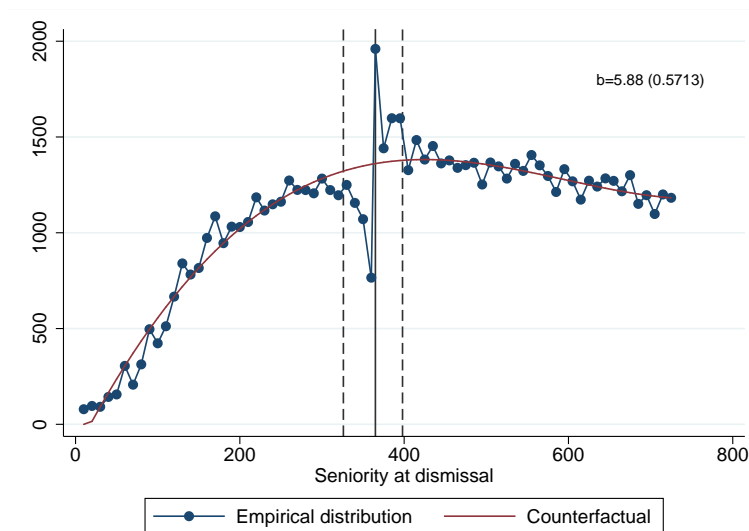
Figure 2.34: Empirical and counterfactual distributions of seniority at layoff, 3rd gain category (October 2011-September 2014)



SOURCE: UI data (FNA).

NOTE: This graph plots in blue the empirical distribution of seniority at layoff for workers eligible to the CSP, laid-off between October, 2011 and October, 2014, and part of the 3rd gain category (binsize: 10). The counterfactual distribution has been computed by fitting a 4th order polynomial, excluding an area around the notch, and extrapolating the distribution in the excluded area. The excluded area upper bound has been determined visually where the bunching stops, and corresponds to 398 days. The lower bound has been found through an iterative process such that the excess and missing masses equalise (see Section 5.1 for more details), and corresponds to 343 days. The two dashed vertical lines represent the excluded area bounds, and the solid vertical line represents the one-year cutoff. The gain is defined as the difference in replacement rate between the standard benefit and the ASP granted to those with a seniority of one year of more. This difference increases with previous earnings, as the replacement rate associated with the standard benefit decreases with earnings. The third gain category corresponds to a gain greater or equal to 15 percentage points and lower than 20 percentage points.

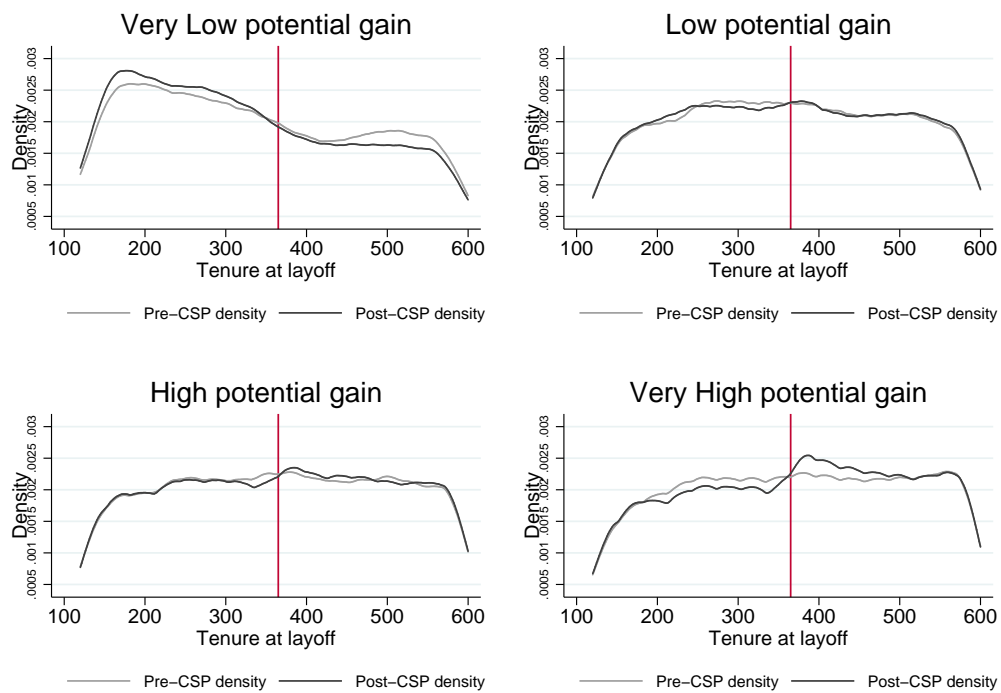
Figure 2.35: Empirical and counterfactual distributions of seniority at layoff, 4th gain category (October 2011-September 2014)



SOURCE: UI data (FNA).

NOTE: This graph plots in blue the empirical distribution of seniority at layoff for workers eligible to the CSP, laid-off between October, 2011 and October, 2014, and part of the 4th gain category (binsize: 10). The counterfactual distribution has been computed by fitting a 4th order polynomial, excluding an area around the notch, and extrapolating the distribution in the excluded area. The excluded area upper bound has been determined visually where the bunching stops, and corresponds to 398 days. The lower bound has been found through an iterative process such that the excess and missing masses equalise (see Section 5.1 for more details), and corresponds to 326 days. The two dashed vertical lines represent the excluded area bounds, and the solid vertical line represents the one-year cutoff. The gain is defined as the difference in replacement rate between the standard benefit and the ASP granted to those with a seniority of one year of more. This difference increases with previous earnings, as the replacement rate associated with the standard benefit decreases with earnings. The fourth gain category corresponds to a gain greater or equal to 20 percentage points.

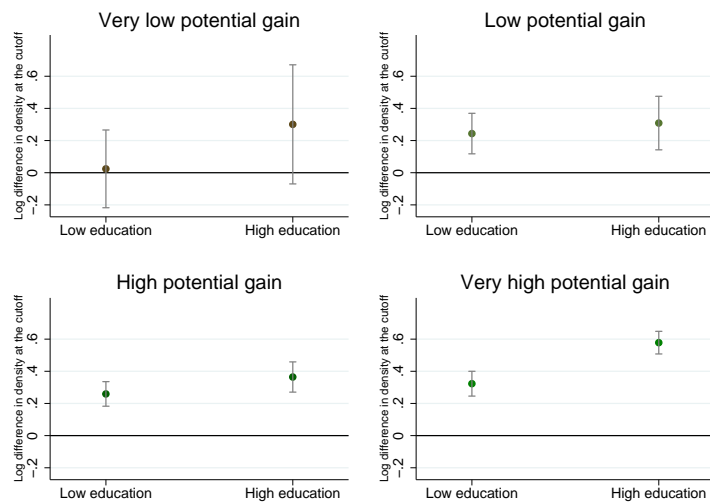
Figure 2.36: Pre and Post-CSP seniority density around the notch by gain category



SOURCE: UI data (FNA). Seniority window: 120-600 days.

NOTE: This graph plots the density of tenure at layoff for workers eligible to the CSP, respectively for the pre-CSP period (August, 2009-August, 2011) and the post-CSP period (October, 2011-September, 2014), decomposed by gain category. The vertical line corresponds to the one-year cutoff. Consistently with elasticity estimates computed by gain category, it shows no bunching for the 1st gain category, and a more and more pronounced bunching as the gain category increases. It means that the heterogeneity in bunching by gain categories highlighted by the elasticity estimates reflects a true heterogeneity in response, and not a heterogeneity in the counterfactual distribution.

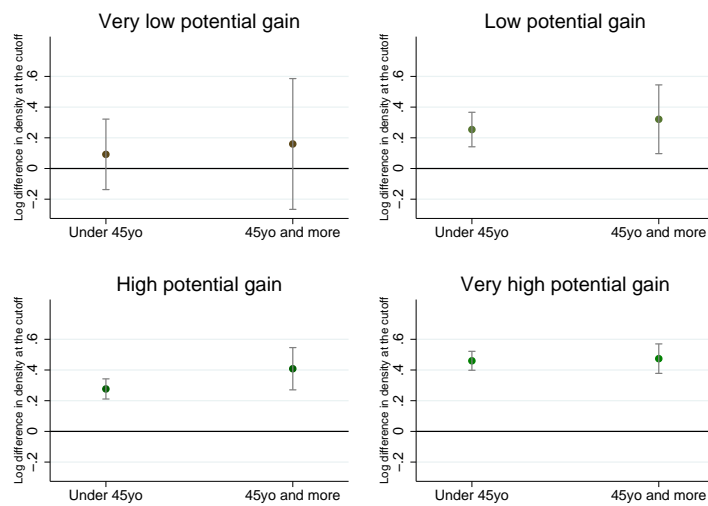
Figure 2.37: Magnitude of the bunching by gain \times education categories



SOURCE: UI data (FNA).

NOTE: This graph reports the coefficient from the McCrary (2008) test performed on workers eligible to the CSP, laid-off between October, 2011 and September, 2014, divided into gain and education categories. Low education means less than the high school diploma. The gain is defined as the difference in replacement rate between the standard benefit and the ASP granted to those with a seniority of one year of more. This difference increases with previous earnings, as the replacement rate associated with the standard benefit decreases with earnings. Very low potential gain stands for a gain lower than 10 percentage points, low potential gain for a gain between 10 and 15 percentage points, high potential gain for a gain between 15 and 20 percentage points, and very high potential gain for a gain higher than 20 percentage points.

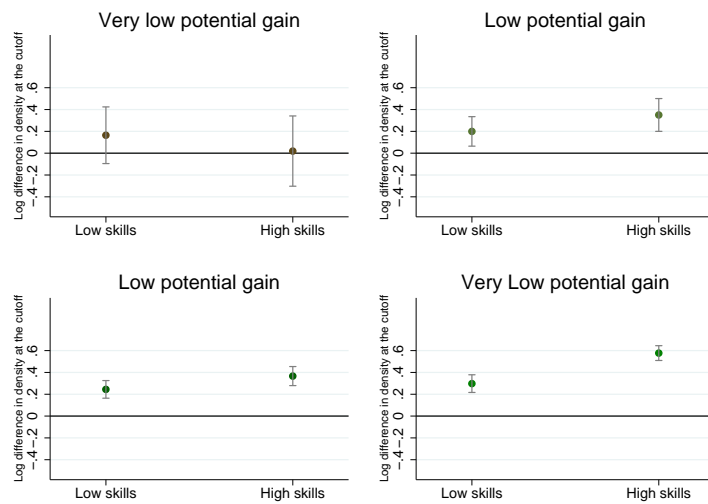
Figure 2.38: Magnitude of the bunching by gain \times age categories



SOURCE: UI data (FNA).

NOTE: This graph reports the coefficient from the McCrary (2008) test performed on workers eligible to the CSP, laid-off between October, 2011 and September, 2014, divided into gain and age categories. The gain is defined as the difference in replacement rate between the standard benefit and the ASP granted to those with a seniority of one year of more. This difference increases with previous earnings, as the replacement rate associated with the standard benefit decreases with earnings. Very low potential gain stands for a gain lower than 10 percentage points, low potential gain for a gain between 10 and 15 percentage points, high potential gain for a gain between 15 and 20 percentage points, and very high potential gain for a gain higher than 20 percentage points.

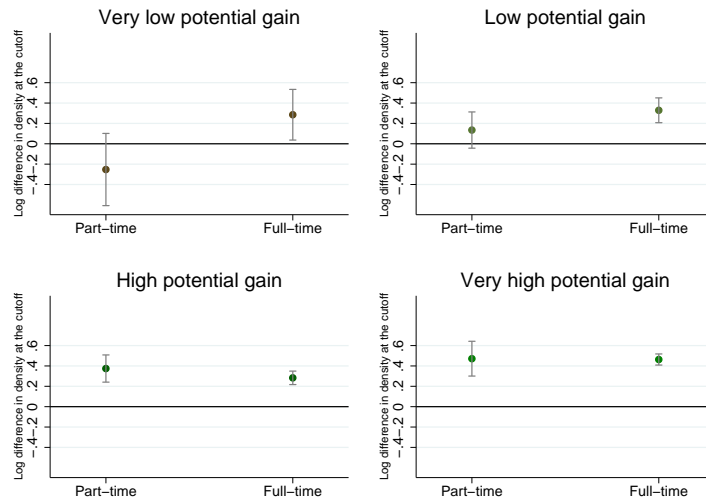
Figure 2.39: Magnitude of the bunching by gain \times skills categories



SOURCE: UI data (FNA).

NOTE: This graph reports the coefficient from the McCrary (2008) test performed on workers eligible to the CSP, laid-off between October, 2011 and September, 2014, divided into gain and skills categories. High skills include executives, intermediate occupations and skilled employees. The gain is defined as the difference in replacement rate between the standard benefit and the ASP granted to those with a seniority of one year or more. This difference increases with previous earnings, as the replacement rate associated with the standard benefit decreases with earnings. Very low potential gain stands for a gain lower than 10 percentage points, low potential gain for a gain between 10 and 15 percentage points, high potential gain for a gain between 15 and 20 percentage points, and very high potential gain for a gain higher than 20 percentage points.

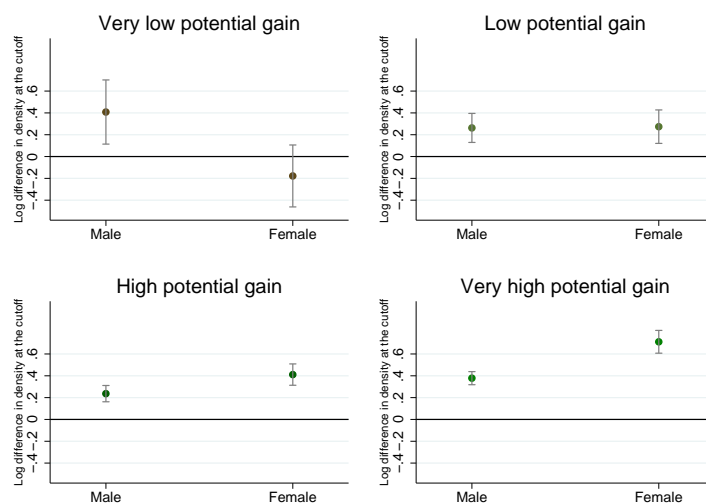
Figure 2.40: Magnitude of the bunching by gain \times working times categories



SOURCE: UI data (FNA).

NOTE: This graph reports the coefficient from the McCrary (2008) test performed on workers eligible to the CSP, laid-off between October, 2011 and September, 2014, divided into gain and working time categories. The gain is defined as the difference in replacement rate between the standard benefit and the ASP granted to those with a seniority of one year of more. This difference increases with previous earnings, as the replacement rate associated with the standard benefit decreases with earnings. Very low potential gain stands for a gain lower than 10 percentage points, low potential gain for a gain between 10 and 15 percentage points, high potential gain for a gain between 15 and 20 percentage points, and very high potential gain for a gain higher than 20 percentage points.

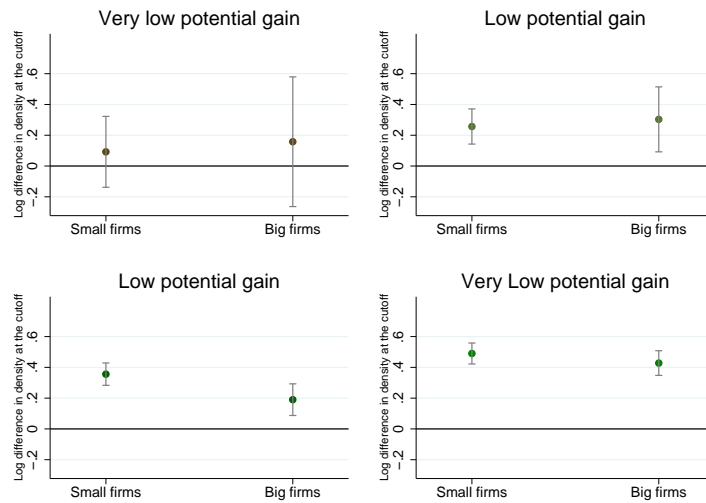
Figure 2.41: Magnitude of the bunching by gain \times gender categories



SOURCE: UI data (FNA).

NOTE: This graph reports the coefficient from the McCrary (2008) test performed on workers eligible to the CSP, laid-off between October, 2011 and September, 2014, divided into gain and gender categories. The gain is defined as the difference in replacement rate between the standard benefit and the ASP granted to those with a seniority of one year of more. This difference increases with previous earnings, as the replacement rate associated with the standard benefit decreases with earnings. Very low potential gain stands for a gain lower than 10 percentage points, low potential gain for a gain between 10 and 15 percentage points, high potential gain for a gain between 15 and 20 percentage points, and very high potential gain for a gain higher than 20 percentage points.

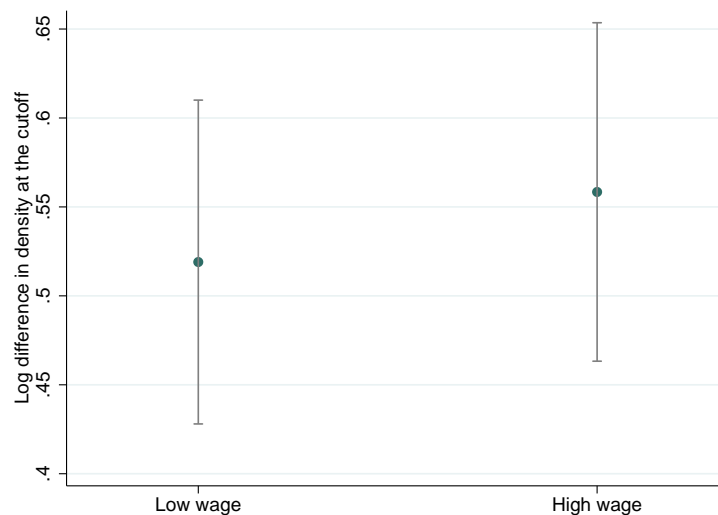
Figure 2.42: Magnitude of the bunching by gain \times firm size categories



SOURCE: UI data (FNA).

NOTE: This graph reports the coefficient from the McCrary (2008) test performed on workers eligible to the CSP, laid-off between October, 2011 and September, 2014, divided into gain and firm size categories. Small firms include firms with less than 10 employees. The gain is defined as the difference in replacement rate between the standard benefit and the ASP granted to those with a seniority of one year of more. This difference increases with previous earnings, as the replacement rate associated with the standard benefit decreases with earnings. Very low potential gain stands for a gain lower than 10 percentage points, low potential gain for a gain between 10 and 15 percentage points, high potential gain for a gain between 15 and 20 percentage points, and very high potential gain for a gain higher than 20 percentage points.

Figure 2.43: Magnitude of the bunching by wage half



SOURCE: UI data (FNA).

NOTE: This graph reports the coefficient from the McCrary (2008) test performed on workers eligible to the CSP, laid-off between October, 2011 and September, 2014, divided into wage half, and restricted to workers with a wage above the threshold where the gap in replacement rates between the two types of benefits does not vary anymore. The log difference in the seniority density is computed for each half. It indicates that the bunching response increases as the wage increases with no variation in the level of incentives, although standard errors are large.

Table 2.19: Elasticity estimates by cell (small firms)

			Change in replacement rate (in %)	L_l	b	m	ϵ_{ub}	ϵ_{lb}
Industry, Agriculture, Building	Less than 45yo	Male	11.39*** 0.08	324*** 21	3.26*** 0.98	3.26*** 0.89	0.2531 0.1690	0.0097*** 0.0028
		Female	11.94*** 0.12	345*** 9	3.39*** 1.26	3.63** 1.42	0.0843* 0.0487	0.0109** 0.0045
	45yo and more	Male	11.97*** 0.13	322*** 20	3.50** 1.56	3.44** 1.58	0.2551* 0.1540	0.0103** 0.0051
		Female	11.58*** 0.19	331*** 19	4.32** 2.03	4.80** 2.45	0.1893 0.1395	0.0150* 0.0084
	Less than 45yo	Male	11.68*** 0.10	322*** 17	4.33*** 1.34	4.54*** 1.40	0.2540* 0.1407	0.0139*** 0.0047
		Female	12.39*** 0.08	310*** 13	7.44*** 1.20	6.89*** 1.24	0.3462*** 0.1078	0.0218*** 0.0044
	45yo and more	Male	12.29*** 0.13	314*** 15	5.53*** 2.08	5.34*** 1.82	0.3164*** 0.1224	0.0166*** 0.0062
		Female	10.40*** 0.11	321*** 20	4.09*** 1.40	4.39*** 1.44	0.2871* 0.1734	0.0136*** 0.0049
	Retail, Food and accommodation, Services	Male	11.87*** 0.08	315*** 10	6.86*** 1.06	8.12*** 1.24	0.2973*** 0.0939	0.0265*** 0.0047
		Female	12.47*** 0.17	324*** 14	7.79*** 2.27	9.36*** 2.76	0.2224*** 0.1136	0.0314*** 0.0107

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors are below the estimates.

NOTE: This table displays elasticity estimates by cell, with their standard errors below. Elasticity estimates are computed using the same formula as in Chetty et al. (2011) adjusted for the notch $\frac{(m/L^*)^2}{\Delta r / (1+r_0+L^*\Delta r/\Delta L^*)}$ for the lower bound, and using $\frac{(\Delta L^*/L^*)^2}{\Delta r / (1+r_0+L^*\Delta r/\Delta L^*)}$ with ΔL^* measured as the distance between the point of divergence and the notch point for the upper bound. The area used for estimating the counterfactual is included between 0 and 730 days. The polynomial fitting the seniority bin count is of order 4. Standard errors are calculated using a bootstrap procedure generating seniority distributions and associated estimates by random resampling. 100 replications. Small firms include firms with less than 10 employees. Low education corresponds to people having less than the high school diploma or having at most a vocational high school degree.

Table 2.20: Elasticity estimates by cell (large firms)

			Change in replacement rate (in %)	L_l	b	m	ϵ_{ub}	ϵ_{lb}	
Industry, Agriculture, Building	Less than 45yo	Male	Low education	11.72***	311***	2.06**	1.98**	0.3398***	0.0057**
				0.09	11	0.91	0.92	0.0957	0.0028
		High education		12.93***	307***	8.83***	8.71***	0.3594***	0.0285***
				0.11	11	2.22	2.03	0.0914	0.0077
	45yo and more	Female	Low education	11.33***	323***	2.51	2.50	0.2486*	0.0074
				0.34	18	1.54	1.53	0.1393	0.0048
		High education		12.40***	331***	3.18*	3.57*	0.1880	0.0108
				0.16	22	1.85	2.03	0.1535	0.0066
	Less than 45yo	Male	Low education	13.05***	345***	2.71***	2.79**	0.0951	0.0081**
				0.10	17	1.01	1.09	0.1033	0.0034
		High education		13.93***	326***	2.37*	2.37*	0.2110	0.0069
				0.17	21	1.42	1.43	0.1446	0.0044
Retail, Food and accommodation, Services	Female	Low education	11.75***	322***	1.28	1.39	0.2702	0.0040	
			0.82	25	1.14	1.41	0.2075	0.0043	
	High education		13.26***	322***	1.37*	1.41	0.2366*	0.0040	
			0.27	17	0.79	0.88	0.1272	0.0026	
45yo and more	Male	Low education	12.18***	320***	4.10***	3.86***	0.2677**	0.0116***	
			0.15	16	1.48	1.32	0.1269	0.0043	
	High education		12.92***	307***	6.29***	7.00***	0.3615***	0.0222***	
			0.09	11	1.54	1.55	0.0902	0.0056	
	Female	Low education	11.04***	318***	3.79*	3.85**	0.3007**	0.0118*	
			0.19	19	1.94	1.90	0.1497	0.0064	
	High education		12.32***	312***	7.11***	8.13***	0.3272***	0.0265***	
			0.10	14	1.55	1.70	0.1215	0.0063	

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Standard errors are below the estimates.

NOTE: This table displays elasticity estimates by cell, with their standard errors below. Elasticity estimates are computed using the same formula as in Chetty et al. (2011) adjusted for the notch $\frac{(m/L^*)^2}{\Delta\tau/(1+r_0+L^*\Delta\tau/\Delta L^*)}$ for the lower bound, and using $\frac{(\Delta L^*/L^*)^2}{\Delta\tau/(1+r_0+L^*\Delta\tau/\Delta L^*)}$ with ΔL^* measured as the distance between the point of divergence and the notch point for the upper bound. The area used for estimating the counterfactual is included between 0 and 730 days. The polynomial fitting the seniority bin count is of order 4. Standard errors are calculated using a bootstrap procedure generating seniority distributions and associated estimates by random resampling. 100 replications. Large firms include firms with 10 employees or more. Low education corresponds to people having less than the high school diploma or having at most a vocational high school degree.

Table 2.21: Elasticity estimates by baseline supra-legal severance payment

Supra-legal severance payment category	Average gain in replacement rate (in %)	L_u	L_l	b	m	ϵ_{RF}^{lb}	ϵ_{RF}^{ub}
Low gain	11.27*** (0.0004)	398	339*** (8.03)	4.43*** (0.5665)	4.39*** (0.5587)	0.0133*** (0.0018)	0.1215** (0.0553)
High gain	12.79*** (0.0004)	398	329*** (9.76)	5.54*** (0.5669)	5.58*** (0.5832)	0.0171*** (0.002)	0.1809*** (0.0701)

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

NOTE: This table displays elasticity estimates by category of supra-legal severance payment. Elasticity estimates are computed using the same formula as in Chetty et al. (2011) adjusted for the notch $\frac{(m/L^*)^2}{\Delta r / (1+r_0+L^*\Delta r/\Delta L^*)}$ for the lower bound, and using $\frac{(\Delta L^*/L^*)^2}{\Delta r / (1+r_0+L^*\Delta r/\Delta L^*)}$ with ΔL^* measured as the distance between the point of divergence and the notch point for the upper bound. The area used for estimating the counterfactual is included between 0 and 1100 days. The polynomial fitting the seniority bin count is of order 4. Standard errors are calculated using a bootstrap procedure generating seniority distributions and associated estimates by random resampling. 100 replications. Supra-legal severance payment has been predicted using a sample of similar dismissed workers with tenure lower than two years. Low (respectively high) severance payment category corresponds to a supra-legal severance payment below (respectively above) the median. The fact that the elasticity estimate is higher for the high-payment category confirms a prediction from the model, i.e. workers with more resources to compensate the employer have a higher chance to bunch.

Table 2.22: Elasticity estimates by predicted unemployment duration quintile

	1 st quintile	2 nd quintile	3 rd quintile	4 th quintile	5 th quintile
Elasticity	.0096999*** (.0018414)	.0204535*** (.0026108)	.0150138*** (.003055)	.0187388*** (.002979)	.0156698*** (.0036916)

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

NOTE: This table displays elasticity estimates by quintile of predicted unemployment duration. Elasticity estimates are computed using the same formula as in Chetty et al. (2011) adjusted for the notch $\frac{(m/L^*)^2}{\Delta r / (1+r_0+L^*\Delta r/\Delta L^*)}$. The area used for estimating the counterfactual is included between 0 and 730 days. The polynomial fitting the seniority bin count is of order 4. Standard errors are calculated using a bootstrap procedure generating seniority distributions and associated estimates by random resampling. 200 replications. Unemployment duration has been predicted using information on age, gender, skill level (6 items), education level (10 items), tenure in past job, wage, sought occupation (81 items), working hours, separation motive (8 items), region (31 items), sector of activity (21 items), number of children, marital status, month of separation, occupation in past job (81 items), firm size, plant size, number of previous unemployment rights, daily benefit, potential benefit duration, average benefit level and duration based on previous unemployment rights. The regression has been made on a sample of laid-off workers with similar tenure value in the pre-CSP period (September 2009-September 2011). Consistent with the mode, the highest elasticity estimates are found in the middle of the predicted unemployment duration distribution, where workers have both high incentives with sufficient resources to get the employment spell extension.

Table 2.23: Log discontinuity estimates by gain and representation categories

		Gain category			
		$gain < 10ppts$	$10ppts \leq gain < 15ppts$	$15ppts \leq gain < 20ppts$	$20ppts \geq gain$
Probability of having no representative within the firm	$p \leq 0.05$.09568 (.37148)	.09744 (.18412)	.16337* (.08929)	.39973*** (.07152)
	$0.05 < p \leq 0.1$.25452 (.468713)	.33265* (.18518)	.18852** (.08880)	.39394*** (.06927)
	$0.1 < p \leq 0.5$	-.06454 (.45927)	.32650 (.28568)	.19934 (.15456)	.65682*** (.11770)
	$p > 0.5$.14372 (.62489)	.58716* (.31582)	.20545 (.14480)	.41178*** (.10643)

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

NOTE: This table reports the log difference in density from the McCrary (2008) test performed on the density of tenure at layoff for different gain and representation categories, at the one-year threshold. The probability of having no representative at all within the firm has been imputed based on data from the REPOSE survey (Statistics department of the Ministry of Labour, DARES), according to firm size. The gain is defined as the difference in replacement rate between the standard benefit and the ASP granted to those with a seniority of one year or more. This difference increases with previous earnings, as the replacement rate associated with the standard benefit decreases with earnings. Estimates increase with potential gain but no systematic pattern is found concerning the relationship between representation and the magnitude of the discontinuity.

Table 2.24: Elasticity estimates by gain and representation categories

		Gain category			
		$gain < 0.1$	$0.1 \leq gain < 0.15$	$0.15 \leq gain < 0.2$	$0.2 \geq gain$
Probability of having no representative within the firm	$p \leq 0.05$.0154 (.3487)	.185 (2.1677)	.0355 (.18)	.0495 (.2526)
	$0.05 < p \leq 0.1$	–	.011 (.1294)	.0062 (.015)	.1074* (.0642)
	$0.1 < p \leq 0.5$	–	–	.072 (.0568)	.0612* (.0329)
	$p > 0.5$	–	–	.0289 (.0316)	.0682* (.0359)

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

NOTE: The elasticity estimates are computed on the population of workers eligible to the CSP, laid-off between October, 2011 and September, 2014, divided into gain and representation categories. Representation is proxied using aggregate data from the REPOSE survey (DARES). The methodology used to compute the elasticity estimates is the one described in Section 5.1. The bunching boundaries change with gain category. The area used for estimating the counterfactual is included between 180 and 540 days. The polynomial fitting the seniority bin count is of order 4. Standard errors are calculated using a bootstrap procedure generating seniority distributions and associated estimates by random resampling. 200 replications. The empty cells are those for which I could not find a value for M and B to converge.

Table 2.25: Logit model on propensity to bunch

	Propensity to bunch	Propensity to bunch	Propensity to bunch
Potential gain	1.403638*** (.2514381)	1.397635*** (.2529513)	1.249442* (.4913836)
Education level	.0567225*** (.0055986)	.0574443*** (.0056078)	.0531391*** (.01009)
Age	.0042388*** (.0009846)	.0042643*** (.0009859)	.0018906 (.0017692)
Gender	.1165235*** (.0221166)	.116092*** (.0221874)	.115917** (.0400252)
Being an executive	.1716293*** (.0402472)	.1730862*** (.040391)	.1103645 (.0617812)
Workforce size	-.0004814*** (.0000876)		
Workforce size category		-.0117347** (.0044994)	
Proba of having a unionised representative			-.1132072 (1.504067)
Proba of having a work council			-.6683384 (5.085477)
Proba of having at least one staff representative			-.2669206 (1.284029)
Proba of having at least one unique representative body			.2559616 (2.397695)
Proba of having a health and safety committee			.470253 (3.34815)
Constant	-3.06808*** (.0646402)	-3.055762*** (.065114)	-2.800797*** (.4052817)
Observations	115,203	114,840	38,550

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

NOTE: This tables reports the regression of the propensity to bunch on different individual and firm's characteristics, on the sample of workers eligible to the CSP, laid-off between October, 2011 and September, 2014. Propensity to bunch is defined as the probability of having a seniority between 365 and 397 days, relative to having a seniority lower than 365 or between 397 and 540 days. The different representation probabilities have been imputed based on data from the REPONSE survey (Statistics department of the Ministry of Labour, DARES), according to firm size. We see that individuals characteristics seem to have a greater impact on the propensity to bunch than representation probabilities.

Table 2.26: Elasticity estimates by type of layoff

	Collective	Individual
Elasticity	.0071487*** (.0011237)	.0235715*** (.00212)

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

NOTE: This table displays elasticity estimates by type of layoff. Elasticity estimates are computed using the same formula as in [Chetty et al. \(2011\)](#) adjusted for the notch $\frac{(m/L^*)^2}{\Delta r / (1+r_0 + L^* \Delta r / \Delta L^*)}$. The area used for estimating the counterfactual is included between 0 and 730 days. The polynomial fitting the seniority bin count is of order 4. Standard errors are calculated using a bootstrap procedure generating seniority distributions and associated estimates by random resampling. 200 replications. Collective layoffs are defined as more than 2 persons laid-off within the same 30-day-period within the same firm. The elasticity estimate is the highest for individual layoffs, presumably because the scheduling of a single layoff date is easier than the scheduling of multiple dates with multiple tenure values.

Table 2.27: Fixed-effect logit of the propensity to bunch (odds ratio)

	Propensity to bunch	Propensity to bunch	Propensity to bunch	Propensity to bunch
Potential gain from CSP	2.471491*** (0.850876)	5.540569*** (2.280127)	2.171878** (0.698587)	4.701598*** (1.929778)
Potential gain ²		1.195319*** (0.062900)		1.171815*** (0.061894)
Age			1.004217** (0.002019)	1.003589* (0.002031)
Level of education			1.031145*** (0.012229)	1.029404** (0.012221)
Gender			0.932627 (0.048924)	0.940633 (0.049424)
Being an executive			0.919180 (0.080635)	0.911186 (0.079979)
Observations	20,106	20,106	20,061	20,061

* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. Standard errors in parentheses.

NOTE: This table reports results from a firm fixed-effect model run on firms for which I observe several layoffs over the period. The propensity to bunch within a firm is regressed on individual characteristics and the level of the potential gain, measured by the gap in replacement rate at the one-year threshold. Bunching is defined as having a tenure at layoff between 365 and 398 days. Standard errors are clustered at the firm level. I restrict the sample to seniority values between 6 and 18 months, to compare those bunching – having a seniority between 365 and 397 days – with those having a seniority close but outside the bunching window. Neutralising the effect of time invariant firms characteristics, we still observe a positive impact of age and education, even after controlling for financial incentives.

Table 2.28: Reduced-form elasticity estimates with round-number fixed effect

	398	398	398
L_u	312***	323***	342***
L_l	(8.3)	(7.92)	(3.43)
Seniority window	[120;540]	[180;540]	[0;730]
b	5.19*** (0.3307)	4.86*** (0.3135)	4.1*** (0.3314)
m	5.25*** (0.3668)	4.92*** (0.3494)	4.15*** (0.3593)
Average change in replacement rate (in %)	11.98*** (0.0003)	11.98*** (0.0003)	11.98*** (0.0002)
ϵ_{RF}^{ub}	0.3275*** (0.0768)	0.2306*** (0.0659)	0.097*** (0.0205)
ϵ_{RF}^{lb}	0.0161*** (0.0012)	0.015*** (0.0012)	0.0124*** (0.0012)

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

NOTE: Elasticity estimates are computed using the same formula as in Chetty et al. (2011) adjusted for the notch $\frac{(m/L^*)^2}{\Delta r / (1+r_0+L^*\Delta r/\Delta L^*)}$ for the lower bound, and using $\frac{(\Delta L^*/L^*)^2}{\Delta r / (1+r_0+L^*\Delta r/\Delta L^*)}$ with ΔL^* measured as the distance between the point of divergence and the notch point for the upper bound. The computation is adjusted with round-number fixed-effects to get rid of the artificial bunching at each month interval, due to the regularity in contract starting and ending dates (see Section 6.1 for more details). The different columns represent different tenure windows considered in the estimation. Estimates are very close from the ones in the main estimation, ensuring that most of the bunching is due to the response to the CSP.

Table 2.29: Difference-in-difference estimation of the number of excess layoffs (MMO data)

	Number of layoffs	
	Excess layoffs	Missing layoffs
Post-CSP	0.638*** (0.1824)	0.615*** (0.1956)
12 to 14-m tenure <i>vs</i> 8 to 10-m	0.031 (0.0921)	
12 to 14-m tenure <i>vs</i> 8 to 10-m × Post-CSP	0.405*** (0.1264)	
10 to 12-m tenure <i>vs</i> 8 to 10-m		0.560*** (0.0910)
10 to 12-m tenure <i>vs</i> 8 to 10-m × Post-CSP		-0.463*** (0.1249)
Constant	1.149*** (0.1070)	1.238*** (0.1122)
Observations	2094	2018

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

SOURCE: MMO data (DARES). NOTE: This table reports the difference-in-difference estimate of the change in the number of layoffs in different tenure brackets before and after the implementation of the CSP. The first column compares the change in the number of layoffs in the 8 to 10-month tenure bracket (control group) and in the 12 to 14-month tenure bracket (treated group), where we expect excess layoffs. The second column compares the change in the number of layoffs in the 8 to 10-month tenure bracket (control group) and in the 10 to 12-month tenure bracket (treated group), where we expect missing layoffs. The number of layoffs is computed on a daily basis. Month and year fixed-effects have been added. The idea of this regression is that, if the number of excess and missing layoffs coincides, the whole bunching response comes from a postponement behaviour. We observe that there is approximately the same number of excess and missing layoffs after the implementation of the CSP, indicating that if there is any extensive margin response, it is limited in magnitude.

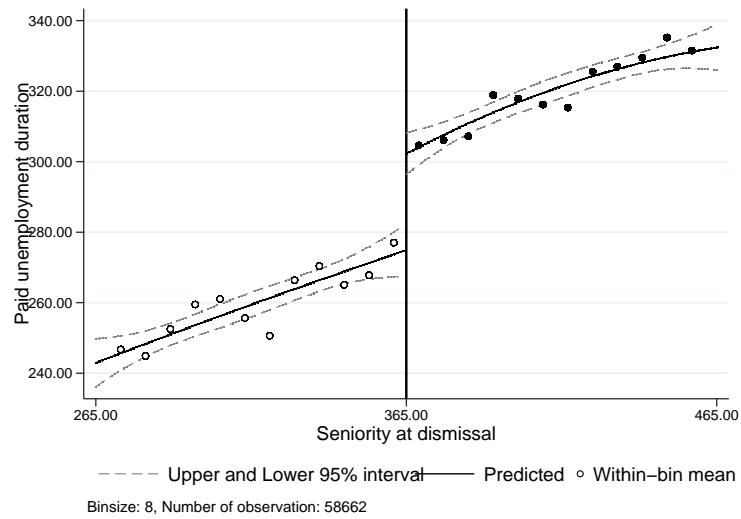
Table 2.30: Difference-in-difference estimate of the number of layoff (UI data)

	Number of layoffs	
	Excess layoffs	Missing layoffs
Post-CSP	1.365502 (1.439060)	3.003807** (1.265823)
12 to 14-m tenure <i>vs</i> 8 to 10m	2.360795*** (0.715558)	
Post-CSP × 12 to 14-m tenure <i>vs</i> 8 to 10m	3.894232*** (0.960826)	
10 to 12-m tenure <i>vs</i> 8 to 10m		0.675285 (0.628794)
Post-CSP × 10 to 12-m tenure <i>vs</i> 8 to 10m		-0.880327 (0.844887)
Constant	16.356402*** (0.866471)	15.447411*** (0.761540)
Observations	4365	4360
R^2	0.446	0.488

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

SOURCE: UI data (FNA). NOTE: This table reports the difference-in-difference estimate of the change in the number of layoffs in different tenure brackets before and after the implementation of the CSP. The first column compares the change in the number of layoffs in the 8 to 10-month tenure bracket (control group) and in the 12 to 14-month tenure bracket (treated group), where we expect excess layoffs. The second column compares the change in the number of layoffs in the 8 to 10-month tenure bracket (control group) and in the 10 to 12-month tenure bracket (treated group), where we expect missing layoffs. The number of layoffs is computed on a daily basis. Day, month and year fixed-effects have been added. The idea of this regression is that if the number of excess and missing layoffs coincides, the whole bunching response comes from a postponement behaviour. We observe that there is slightly more excess than missing layoffs after the implementation of the CSP, indicating that there may have been additional layoffs that would not have taken place absent the program. However, the extensive margin response is limited in magnitude.

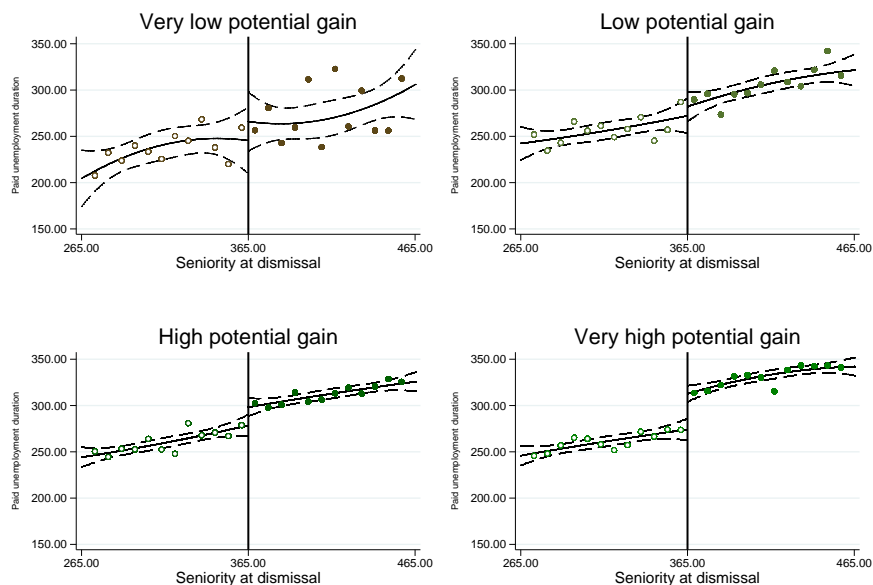
Figure 2.44: Effect of CSP on paid unemployment spell duration



SOURCE: UI data (FNA).

NOTE: This figure shows the average unemployment spell duration with respect to seniority at layoff, with a sharp jump at the one-year threshold. It indicates that taking the CSP is associated with a higher unemployment spell duration, although the selection effect underlined by the existence of bunching cannot be distinguished from pure moral hazard. An unemployment spell has been defined as the addition of days on benefits with no interruption of more than 4 months, as 4 months is the minimum working time necessary to open a new UI entitlement.

Figure 2.45: Effect of CSP on paid unemployment duration by gain category



SOURCE: UI data (FNA).

NOTE: This figure shows the average unemployment spell duration with respect to seniority at layoff by gain category. An unemployment spell has been defined as the addition of days on benefits with no interruption of more than 4 months, as 4 months is the minimum working time necessary to open a new UI entitlement. As we observe bunching at the threshold, we cannot disentangle the selection effect from pure moral hazard when analysing the relationship between seniority and unemployment duration. The gain is defined as the difference in replacement rate between the standard benefit and the ASP granted to those with a seniority of one year or more. This difference increases with previous earnings, as the replacement rate associated with the standard benefit decreases with earnings. Very low potential gain stands for a gain lower than 10 percentage points, low potential gain for a gain lying between 10 and 15 percentage points, high potential gain for a gain lying between 15 and 20 percentage points, and very high potential gain for a gain higher than 20 percentage points.

The decomposition by gain category suggests that when no significant bunching is observed (1st gain category), no significant jump in unemployment is observed neither. On the reverse, the 4th gain category where bunching is the most sizeable also shows the sharpest jump in unemployment.

Table 2.31: Parameters used in the cost-benefit computation

w	77.33 (3.858)
$ASP_{compliers}$	69.20 (2.431)
$D_{compliers}$	441.32 (26.965)
$D_{eligibles}$	405.54 (1.326)
$SB_{compliers}$	45.66 (2.259)
$t_{compliers}^{CSP}$	0.95 (0.028)
$t_{eligibles}^{CSP}$	0.42 (0.002)
ΔL	4.94 (0.422)
B	1475.45 (112.074)

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

NOTE: This table reports the parameters used for the cost-benefit computation of the bunching related to the CSP. Standard errors have been obtained by bootstrapping (100 replications). The compliers' and eligibles' characteristics are computed using the methodology in [Diamond and Persson \(2016\)](#), as in section 5.2. I compare the cost of bunching in terms of higher daily benefits and longer unemployment duration with the benefits in terms of additional contributions.

Table 2.32: Relative annual cost of bunching without taking into account the counterfactual benefits

	Due to bunching	Overall	Ratio (in %)
Net spending	12,773,967	1,050,750,000	1.2%
Gross spending for CSP workers below 2 years of tenure	13,397,603	242,000,000	5.54%

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

NOTE: Those figures do not take into account what would have been paid to the workers absent the bunching. I have subtracted from the Unedic spending those related to the counselling and guidance part of the CSP, as well as those related to additional CSP-related benefits that I am not able to compute. I am taking the average values over the 2014-2017 period.

Appendices

1 Institutional context

Table 2.33: Consequences of accepting the CSP and their valuation

All workers accepting the CSP	Guidance and counselling	+ / - : depending on preferences
	No waiting period	+
	Less contributions on CSP benefits with respect to ARE	+
	Right to accept very short-term contracts limited	-
If seniority \geq 365 days	No advance notice or compensation in lieu of notice	+ : can be felt difficult to keep on working in the firm while knowing you are laid-off - : If the return to the labour market anticipated as quick, the worker can use hours of job-search provided for in most of the collective agreements during his notice
	80% replacement rate VS 57.4% to 75%	+++

NOTE: This table describes the detailed consequences of accepting the CSP, and how they are generally valued by workers, from survey data (Unedic, 2015b).

2 Extensions

2.1 Other optimisation mechanisms

Other channels of optimisation could possibly be found on the employer's side when looking at substitutions between types of contract termination. As part of the economic layoff procedure, employers are constrained by some requirements based on workforce size and number of people laid-off. To escape those requirements, they could substitute economic layoffs with other forms of contract termination less administratively demanding, such as contract termination by mutual agreement for example. Interacting both channels of optimisation – economic layoff right after the one-year threshold and substitutions between types of contract termination – two hypothesis could be rationalised: (i) First, we could observe that in these firms with a high propensity to bunch, employers and human resources managers have an accurate enough knowledge of the legislation and bargaining practices to use all potential channels of optimisation of the conditions of contract termination, among which substitution; (ii) on the other hand, it could also be the case that in firms with a high propensity to bunch, having a high proportion of laid-off workers accepting the CSP leads to a high reduction in the number of people considered as laid-off when

examining the different requirements in terms of workforce size and number of laid-off workers.³⁶ Then, firms resorting to contract extension to avoid the economic layoff procedure requirements do not necessarily need to take advantage of other channels of optimisation.³⁷ In other words, there could be some degree of complementarity and substitution between the two channels of optimisation.

A descriptive analysis performed on the sample of firms having at least one laid-off workers between October, 2011 and September, 2014 provides some information on the validity of both hypotheses.

Table 2.34 shows the proportion of each contract termination type over the 30 days preceding and the 30 days following any layoff for economic reason, as 30 days is the relevant period to assess the administrative constraints the employer has to abide by.³⁸ Contrasting the results by propensity to bunch indicates that being a buncher³⁹ is predicted to decrease the proportion of laid-off workers and to increase other types of contract termination within the same 30-day spell. Adopting a definition of bunching at the firm level (having at least one buncher laid-off for economic reasons over the whole sample period) does not change the results (Table 2.35).

³⁶Economic layoff plans involve some requirements that depend on total workforce size and on the number of people dismissed for economic reason on a 30 days period. Once a laid-off workers has accepted the CSP, he is not considered anymore as laid-off for economic reasons with regard to those requirements. Contract termination is considered as the result of a mutual agreement, then the worker does not enter the counting of the number of people laid-off. This is an additional motive for accepting the extension of the work contracts for workers right below the threshold, although not the only one, as bunching is particularly strong in small firms (under 50 employees) not targeted by those requirements.

³⁷In addition to this counting agreement, it is also much less likely that workers benefiting from training, support for a career change and financial compensation would sue their employers. Creating the conditions for the workers to benefit from the CSP is then a way to reduce the layoff cost.

³⁸To assess whether a layoff is collective and the number of laid-off workers within the same spell, it is a 30-day period that is considered.

³⁹The propensity to bunch here is defined as having a seniority lying in the bunching area.

Table 2.34: Proportion of different contract termination types within the same episode for bunching and non bunching individuals

	Buncher	Non buncher	Difference
End of fixed-term contract	0.03	0.02	-0.002** (0.001)
End of temporary contract	0.00	0.00	-0.000 (0.000)
End of trial period on employer's initiative	0.00	0.00	-0.001*** (0.000)
End of trial period on employee's initiative	0.00	0.00	0.000 (0.000)
Economic layoff	0.94	0.95	0.011*** (0.002)
Mutually agreed termination	0.01	0.01	-0.002*** (0.000)
Other dismissals	0.02	0.02	-0.006*** (0.001)
Quits	0.00	0.00	0.000 (0.000)
Other motives	0.00	0.00	0.000 (0.000)

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

NOTE: This tables reports, for each economic layoff, the number of workers separating by type of separation motive within the same spell. The spell is defined as the 30 days preceding and the 30 days following the economic layoff. The number of separations by type is compared for bunchers and non-bunchers. The sample is drawn from people having registered to UI and may not be representative of the distribution of contract termination motives in general. For example, people quitting are usually not entitled to receive UI benefits and may be underrepresented in this sample. Being a buncher is defined as being laid-off for economic reasons with a seniority lying between 365 and 397 days.

Table 2.35: Proportion of different contract termination types within the same episode for bunching and non bunching firms

	Buncher	Non buncher	Difference
End of fixed-term contract	0.33	0.25	-0.002** (0.001)
End of temporary contract	0.13	0.01	-0.000 (0.000)
End of trial period on employer's initiative	0.01	0.01	-0.001*** (0.000)
End of trial period on employee's initiative	0.00	0.00	0.000 (0.000)
Economic layoff	0.14	0.43	0.011*** (0.002)
Mutually agreed termination	0.04	0.09	-0.002*** (0.000)
Other dismissals	0.30	0.19	-0.006*** (0.001)
Quits	0.02	0.02	0.000 (0.000)
Other motives	0.03	0.01	0.000 (0.000)

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

NOTE: This tables reports, for each economic layoff, the number of workers separating by type of separation motive within the same spell. The spell is defined as the 30 days preceding and the 30 days following the economic layoff. The number of separations by type is compared for bunching firms and non-bunching firms. The sample is drawn from people having registered to UI and may not be representative of the distribution of contract termination motives in general. For example, people quitting are usually not entitled to receive UI benefits and may be underrepresented in this sample. Being a bunching firm is defined as having at least one layoff for economic reason with a seniority lying between 365 and 397 days over the whole period.

This descriptive evidence would be in favour of more complementarity than substitution between the two channels of optimisation. In firms used to negotiate the contract termination date, and then more prone to bunching, employers resort more to other types of contract termination, in particular “end of fixed-term contract” and “other dismissals”, that are typically contract terminations motives under the control of the employer.

This evidence is only suggestive, as we cannot rule out the possibility that the higher proportion of other types of contract termination is not due to strategic behaviour to escape administrative constraints related to economic layoffs, but to other factors.

An additional piece of evidence in line with previous results is that this pattern is also pronounced when restricting to firms with a workforce size above 50 and having done at least one economic layoff over the period, as this is the threshold defining the obligation of setting up a redundancy scheme (Table 2.36). More precisely, firms with 50 employees or more are more likely to use “end of fixed-term contract” and “mutually agreed termination” as motives for the breach of the contract, two motives that are even more controllable by employers.

Table 2.36: Proportion of different contract termination types within the same episode depending on workforce size

	Firms with workforce size ≥ 50	Firms with workforce size < 50	Difference
End of fixed-term contract	0.33	0.21	-0.115*** (0.001)
End of temporary contract	0.02	0.01	-0.010*** (0.000)
End of trial period on employer's initiative	0.02	0.01	-0.006*** (0.000)
End of trial period on employee's initiative	0.00	0.00	-0.000*** (0.000)
Economic layoff	0.34	0.41	0.073*** (0.001)
Mutually agreed termination	0.11	0.07	-0.034*** (0.000)
Other dismissals	0.16	0.26	0.099*** (0.001)
Quits	0.02	0.01	-0.005*** (0.000)
Other motives	0.01	0.01	-0.001*** (0.000)

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

NOTE: This tables reports, for each economic layoff, the number of workers separating by type of separation motive within the same spell. The spell is defined as the 30 days preceding and the 30 days following the economic layoff. The number of separations by type is compared for firms with more or less than 50 employees, as it determines part of the administrative constraints they face in case of economic layoff. The sample is drawn from people having registered to UI and may not be representative of the distribution of contract termination motives in general. For example, people quitting are usually not entitled to receive UI benefits and may be underrepresented in this sample.

To better capture the pure effect of the propensity to bunch, I regress the share of each termination type on a variable indicating that the firm is a bunching type

– that is to say has at least one laid-off workers bunching over the whole period – controlling for workforce size and the total number of workers laid-off for economic reasons within the same redundancy plan. Being in a bunching firm has a significant impact on the distribution of types of contract termination – although coefficients are rather small – and is predicted to increase the total number of terminations within the same spell by 15, translating into a 50% rise (Table 2.37). In particular, being in a bunching firm makes the proportions of end fixed-term contracts, of other dismissals and of end of temporary contracts increase respectively by 0.5, 0.9 and 0.1 percentage points, while the proportion of workers laid-off for economic reasons is predicted to decrease by 1.3 percentage points.

Table 2.37: Proportion of different contract termination types within the same episode

	End of fixed-term contracts	End of temporary contracts	End of trial period (employer's decision)	End of trial period (employee's decision)	Mutually agreed termination	Other dismissals	Quits	Economic layoff	Total number of terminations
Propensity to bunch	0.005*** (0.000)	0.001*** (0.000)	0.000*** (0.000)	-0.000 (0.000)	-0.001*** (0.000)	0.009*** (0.000)	0.000*** (0.000)	-0.013*** (0.000)	15.242*** (0.013)
Workforce size	0.000*** (0.000)	0.000*** (0.000)	0.000*** (0.000)	0.000** (0.000)	0.000*** (0.000)	-0.000*** (0.000)	0.000*** (0.000)	-0.000*** (0.000)	-0.033*** (0.000)
Total number of layoffs within the same episode	-0.002*** (0.000)	-0.000*** (0.000)	-0.000*** (0.000)	-0.000*** (0.000)	-0.001*** (0.000)	-0.001*** (0.000)	-0.000*** (0.000)	0.004*** (0.000)	1.017*** (0.003)
Constant	0.234*** (0.000)	0.010*** (0.000)	0.014*** (0.000)	0.000*** (0.000)	0.084*** (0.000)	0.240*** (0.000)	0.015*** (0.000)	0.396*** (0.000)	29.664*** (0.079)

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

NOTE: The number of separations is counted within the same 60-day spell. The number of separation within each motive is regressed on the propensity to bunch defined at the firm level, and on the workforce size and number of economic layoffs within the same spell. Being a bunching firm is defined as having at least one layoff for economic reason with a seniority lying between 365 and 397 days over the whole period. The sample is drawn from people having registered to UI and may not be representative of the distribution of contract termination motives in general. For example, people quitting are usually not entitled to receive UI benefits and may be underrepresented in this sample.

It means that being in a firm that has some of its employees bunching – and then potentially used to negotiate the terms and conditions of contract terminations – keeping the workforce size and the total number of laid-off workers in the same episode fixed, is associated to larger scale termination spells, where the additional persons leaving the firm separate through other motives that may be easier to implement and less administratively demanding than the economic layoff. These results may indicate that some firms concentrate the use of several complementary mechanisms to escape administrative constraints and make separation easier.⁴⁰ In the specific context of economic layoffs, negotiation over tenure at layoff appears as

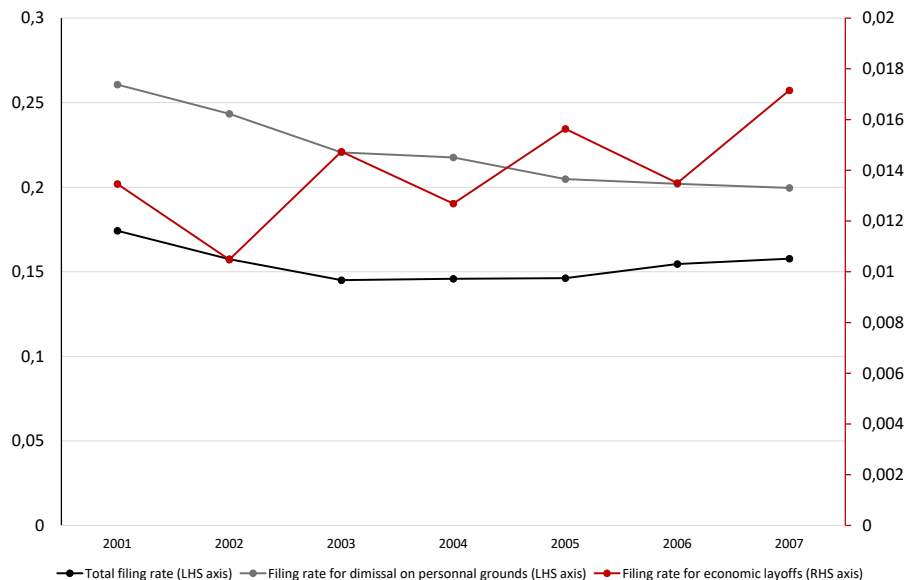
⁴⁰Again, it has to be emphasised here that those findings are only suggestive. Indeed, it could

the main mechanism, but is often coupled with the use of various types of contract terminations.

The same findings could also be interpreted another way: if other types of dismissals are often less administratively constraining, they are associated to a higher risk of trial to the Labour Court (Oyer and Schaefer, 2000; Serverin and Valentin, 2009). The filing rate – although not perfectly estimated – amounts to 2% for economic layoffs, versus more than 20% for other types of dismissals (Figures 2.46 and 2.47).

Then, firms with a high share of dismissals for personal motive could use the extension of the contract for those laid-off for economic reasons within the same dismissal spell as a way to reduce total dismissal cost. Indeed, granting the contract extension for laid-off workers deters them from appealing Labour Court as they receive an immediate compensation. We can even think that employers would choose, among the workers they want to dismiss, those with the highest damages potential to dismiss them under the economic motive, while granting them the extension of the contract, in order to limit the risk of paying high damages (Fraisie, Kramarz and Prost, 2011). If the correlation highlighted by Tables 2.34 to 2.37 does not allow to decide between those two mechanisms, it suggests a link between the optimisation through substitution of separation motives and through extension of work contracts.

Figure 2.46: Evolution of the rate of labour disputes brought to court

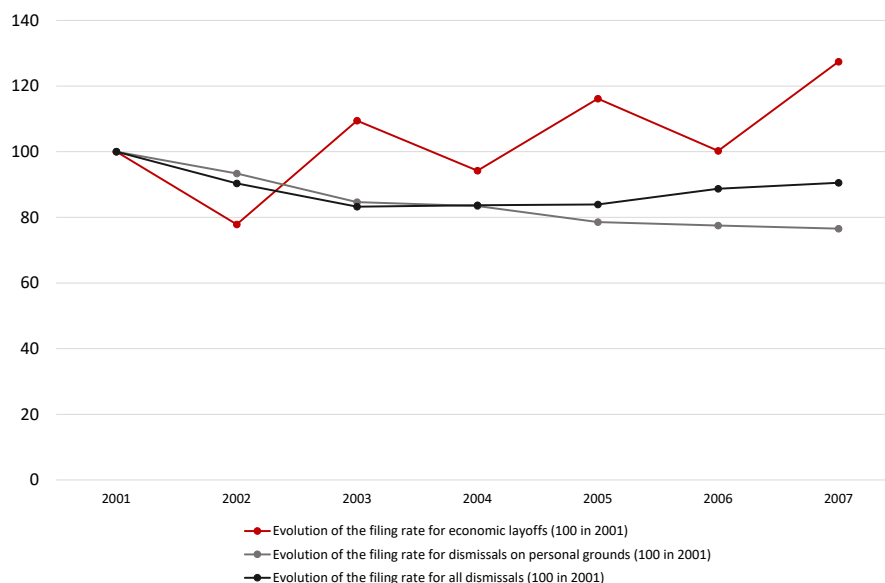


SOURCE: Computation based on figures in Serverin and Valentin (2009)

This graph plots the trend in the rate of labour disputes brought to court by type of separation.

be the case that for example bunching firms are also employing more fixed-term contracts and have a higher turnover, without the purpose of releasing the administrative and financial constraints related to job separation.

Figure 2.47: Evolution of the rate of labour disputes brought to court (Base 100)



SOURCE: Computation based on figures in [Serverin and Valentin \(2009\)](#)

This graph plots the relative trend in the rate of labour disputes brought to court by type of separation. The reference point corresponds to 2001.

One concern could be that findings on complementarity of optimisation mechanisms are due to some mechanical difference in the probability of laying-off a worker for economic reason for firms having or not having already laid-off a worker for economic reason within the bunching window in the same 60 days period.

The use of this restricted period 30 days before and 30 days after each separation is justified by the legal criteria using the number of people laid-off in the same 30 days period to assess the legal requirements the employer has to bear. However, one could argue that a firm laying-off a worker for economic reason within the bunching window could be mechanically more prone to lay-off a worker for the same reason in the following days as this type of layoff is often collective, or less prone as it has just laid-off a worker for the same motive. It could then induce a difference in either way with firms who did not lay-off a worker for economic reasons in the same period in the proportion of layoffs for economic reasons. This concern is partly alleviated when adopting a definition of bunching at the firm level – that is having one worker laid-off for economic reason within the bunching window – as the bunching worker has not been necessarily laid-off within the 30 days period we are looking at.

Nonetheless, to solve any potential issue related to this, another method has been applied to determine whether a bunching firm displays a different pattern in terms of separation motives, even beyond this 30-day period. The pre-CSP period (September, 2009 - September, 2011) has been used to draw a sample of bunching firms, based on all layoffs for economic reasons that has occurred over the whole pre-CSP period.

A firm is defined as bunching when it has laid-off at least one worker for economic reason within the 730-765 days seniority window, as the strategic threshold is at 2 years under the pre-CSP regime. The control group is made of firms having laid-off at least one worker for economic reason within a 694-729 days seniority window over the same period.

Those same treated and control firms are studied over the post-CSP period (October, 2011 - October, 2014) to determine the impact of bunching on the probability to bunch again in the future, and on the distribution of separation motives.

Table 2.38: Impact of having bunched in the past on the probability of bunching in the future

	Bunching in the 2011- 2014 period	
Having bunched	0.01340**	0.01272*
	0.00673	0.00671
Firm's workforce size		0.00005**
		0.00002
Observations	5,866	5,865

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

NOTE: having bunched is defined as having laid-off at least one worker for economic reason within the 730-765 days seniority window, as opposed to having laid-off at least one worker for economic reason within a 694-729 days seniority window over the September, 2009 - September, 2011 period. We look at the probability to bunch for the same firms over the post-CSP period (October, 2011-September, 2014), depending on their bunching status in the past. To maintain a reasonable sample size, bunching in the future has been defined in a less restrictive way as bunching in the past. It corresponds to laying-off a worker for economic reason within the bunching window as opposed to laying-off for economic reason anywhere outside the bunching window (and not only for seniority values right below the bunching cutoff). We observe that firms having bunched in the past are much more likely to bunch in the future, indicating a concentration of the optimisation behaviour within a small set of firms.

Having been a bunching firm in the past is associated to a 1.3 percentage point increase in the probability of bunching in the future, from a baseline probability of 6% (Table 2.38). Therefore, it represents a substantial and significant 22% increase, even after controlling for firm size. It suggests that optimisation is concentrated among a pool of strategic firms that keep on finding ways of optimising the conditions of the separation in the future.

Regarding the separation motives, firms that have bunched in the past are predicted to use more end of fixed-term contract, layoff for economic reasons and dismissal for personal reasons (Table 2.39). It seems that bunching firms use other forms of flexible contracts or separations that are under the employer control than non bunching firms, although some can be administratively constraining and with a substantial risk of judicial proceedings such as the dismissal for personal motive. However, we also notice that bunching firms experience more separations in total (18 more on average on the 2011-2014 period), which could also influence positively

the total number of separations for each motive.

Table 2.39: Impact of having bunched in the past on the number of different contract termination types

	Having bunched
Economic layoffs	1.7089*** (0.4885)
End of fixed-term contract	3.7980** (1.4892)
End of temporary contract	0.1531 (0.1192)
Mutually agreed terminations	0.5322** (0.2260)
Dismissals for personal motive	10.3763*** (3.4218)
End of trial period (employer's initiative)	0.1657** (0.0698)
End of trial period (employee's initiative)	0.0030* (0.0016)
Quits	0.2522** (0.1071)
Other motives	1.4820 (1.5633)
Total number of separations	18.4512*** (5.5667)
Observations	12,719

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

NOTE: having bunched is defined as having laid-off at least one worker for economic reason within the 730-765 days seniority window, as opposed to having laid-off at least one worker for economic reason within a 694-729 days seniority window over the September, 2009 - September, 2011 period. We look at the impact of having bunched in the past on the number of separations for each motive for the same firms over the post-CSP period (October, 2011-September, 2014). The sample is drawn from people having registered to UI and may not be representative of the distribution of contract termination motives in general. For example, people quitting are usually not entitled to receive UI benefits and may be underrepresented in this sample.

Therefore, I focus on proportions of each motive relative to total separations in Table 2.40. Bunching firms resort more, proportionally, to end of temporary contract of trial periods, which are little costly forms of separations. They also exhibit a higher proportion of workers laid-off for economic reasons, which is in line with the justification of bunching as a way to escape administrative constraints related to the number of economic layoffs. Indeed, in firms with a higher number of workers laid-off for economic reasons in both absolute and relative terms, employers may choose to extend the work contract strategically to increase the probability of the workers choosing the CSP, therefore excluding them from the counting of the total number of workers laid-off for economic reasons.

Table 2.40: Impact of having bunched in the past on the proportion of different contract termination types

	Economic layoff	End of fixed-term contract	End of temporary contract	Mutually agreed termination	End of trial period (employer's initiative)	End of trial period (employee's initiative)	Quits	Other motives
Having bunched	0.0201** (0.0089)	0.0040 (0.0070)	0.0022** (0.0011)	0.0098 (0.0061)	0.0149*** (0.0041)	0.0027** (0.0012)	0.0095** (0.0041)	0.0084*** (0.0033)
Observations	12,719	12,719	12,719	12,719	12,719	12,719	12,719	12,719

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

NOTE: having bunched is defined as having laid-off at least one worker for economic reason within the 730-765 days seniority window, as opposed to having laid-off at least one worker for economic reason within a 694-729 days seniority window over the September, 2009 - September, 2011 period. We look at the impact of having bunched in the past on the proportion of each separation motive for the same firms over the post-CSP period (October, 2011-September, 2014). The sample is drawn from people having registered to UI and may not be representative of the distribution of contract termination motives in general. For example, people quitting are usually not entitled to receive UI benefits and may be underrepresented in this sample.

Those results, which are in line with previous ones focusing on the pattern of separation within the same 60-day episode, point again to some interactions between different optimisation mechanisms.

2.2 Bunching at the two-year threshold

As a robustness check, I use the existence of bunching at the two-year threshold during the pre-CSP period to measure similar elasticities and to determine whether I get consistent estimates of the behavioural response over time.

Table 2.41: Reduced-form elasticity estimates at the two-year threshold during the pre-CSP period

L_u	765
L_l	586*** (9.4)
Seniority window	[0;1090]
b	6.38*** (0.4535)
m	5.61*** (0.4494)
Average change in replacement rate	11.99*** (0.0003)
ϵ_{RF}^{ub}	0.5229*** (0.0522)
ϵ_{RF}^{lb}	0.0082*** (0.0007)

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

NOTE: Elasticity estimates are computed using the same formula as in [Chetty et al. \(2011\)](#) adjusted for the notch ($\frac{(m/L^*)^2}{\Delta r / (1+r_0 + L^* \Delta r / \Delta L^*)}$) for the lower bound, and using $\frac{(\Delta L^* / L^*)^2}{\Delta r / (1+r_0 + L^* \Delta r / \Delta L^*)}$ with ΔL^* measured as the distance between the point of divergence and the notch point for the upper bound. Standard errors of L_l are calculated using a bootstrap procedure generating seniority distributions and associated estimates by random resampling. 100 replications.

Table 2.41 shows that the elasticity estimates based on the average response are similar in both periods, though the bunching is more fuzzy at the two-year threshold, which makes the missing mass area larger and the upper bound elasticity higher.

These findings give more strength to the idea that the bunching I measure during the post-CSP period is not related to some specificity at the one-year threshold or to something happening only from September, 2011. The fact that it appears with the same magnitude at both thresholds and both periods suggests that it indeed captures a behavioural response to UI incentives.

This setting with two different thresholds under two different periods could possibly be used to measure optimisation frictions noticeable at the two-year threshold under the post-CSP period. Indeed, the presence of bunching - although very small - at seniority values that used to be relevant for optimisation when incentives were different is a sign of imperfect optimisation, and can serve as a measure of optimisation frictions.

Figure 2.3 shows however that no significant bunching is present at the two-year

cutoff after September, 2011. This is confirmed by the fact that, when trying to estimate bunching at this cutoff, it is not possible to find any value for the missing mass lower bound that equalises the missing and excess areas. It highlights the fact that most frictions in the bunching I observe come from negotiation frictions – seniority at layoff is the result of a bargaining process and is not entirely under the control of the worker.

3 Alternative theoretical framework

I consider the standard Nash bargaining framework where employers and employees maximise the product of their net utilities weighted by their respective bargaining power over t .

As a result, the total surplus is split so that the firm gets a share β (capturing its bargaining power) and the worker a share $1 - \beta$.

This can be written as

$$t = t^0 - \beta[(P - SB - \alpha)d + \Delta C] + d(P - w) \quad (2.7)$$

$$\Leftrightarrow t^0 - t = d(w - P) + \underbrace{\beta[(P - SB - \alpha) + \Delta C]}_{\text{Total surplus}}$$

The last equation indicates that the severance payment the employee foregoes against the extension is made of two elements: the compensation for the wage cost triggered by the extension as the contract is not profitable anymore $((w - P)d)$, and the employer's part of the surplus from the extension $(\beta[(P - SB - \alpha) + \Delta C])$.

Similarly, we restrict the set of possible solutions to those such that $t \geq 0$. If this condition is fulfilled, as well as condition (2.1), the job is extended and equation (2.7) describes how the surplus is split.

To illustrate how t varies with the bargaining power, I consider the two polar cases:

If $\beta = 1$, $t = d(SB + \alpha - w) - \Delta C + t^0$. The employer has a full bargaining power and gets the whole surplus, whereas the employee is as well off as in the no extension scenario.

If $\beta = 0$, $t = (P - w)d + t^0$. The employee gets the whole surplus and the employer is as well off as in the no extension scenario.

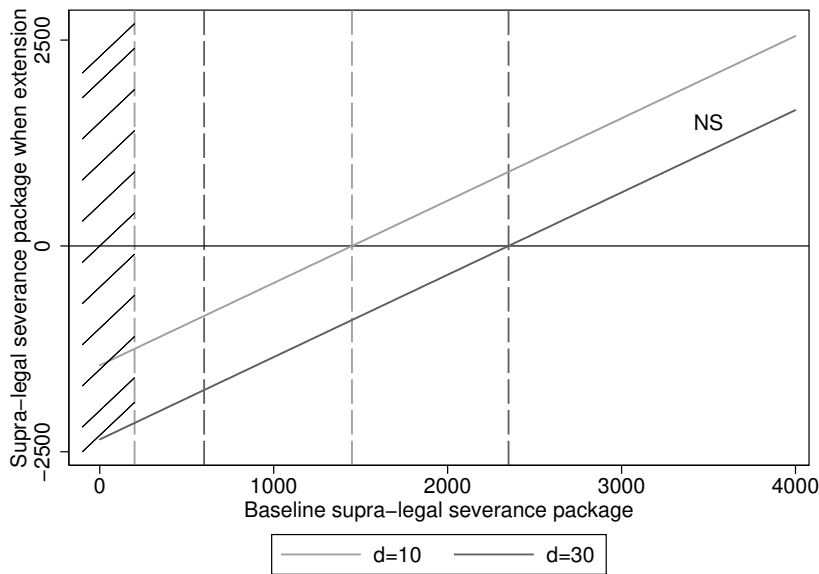
Conditions 2.1 and $t \geq 0$ imply that we necessarily have $t^0 + (P - w)d \geq 0$. It allows to characterise the boundary value of t^0 under which the extension is not possible for any value of β , and to highlight the role of t^0 in determining the propensity to bunch.

This boundary value writes:

$$t^0 = (w - P)d$$

Graph 3 illustrates the restricted Nash bargaining solution such that $t \geq 0$. If we look at the light-grey lines ($d = 10$), we see that the dashed area corresponds to the area for which there is no solution such that $t \geq 0$, no matter the value of β . It corresponds to the cases where $t^0 < (w - P)d$. The area between the two light-grey dashed lines corresponds to cases where we could find a $t \geq 0$ but for lower values of β . The Nash solution cannot be implemented, whereas we can find a $t \geq 0$ such that the two parties will be at least as well-off with the extension than in the no extension scenario. In that sense, the model described in the core of the chapter encompasses the Nash bargaining solutions. The light-grey solid line describes the Nash solution.

I reproduce the same lines for the case where $d = 30$, all other parameters being equal. It illustrates that, the higher the number of days of extension needed, the less likely it is that the employer and the employee settle an agreement.



4 Methodological discussion

The choice of a zero-inflated negative binomial model has been guided by the structure of the data. Indeed, supra-legal severance payment is a necessarily positive amount, with a high frequency on the value 0,⁴¹ and a variance much higher than the average. The [Vuong \(1989\)](#) test as well as the likelihood ratio test of $\alpha = 0$ both yield a significant output, indicating that the zero-inflated negative binomial model is better suited than the standard negative binomial model and the zero-inflated poisson model.

Table 2.42: Zero-inflated negative binomial regression tests

Likelihood-ratio test of $\alpha = 0$	7.1e+09 (0.000)
Vuong test	250.54 (0.000)

p-value in parenthesis

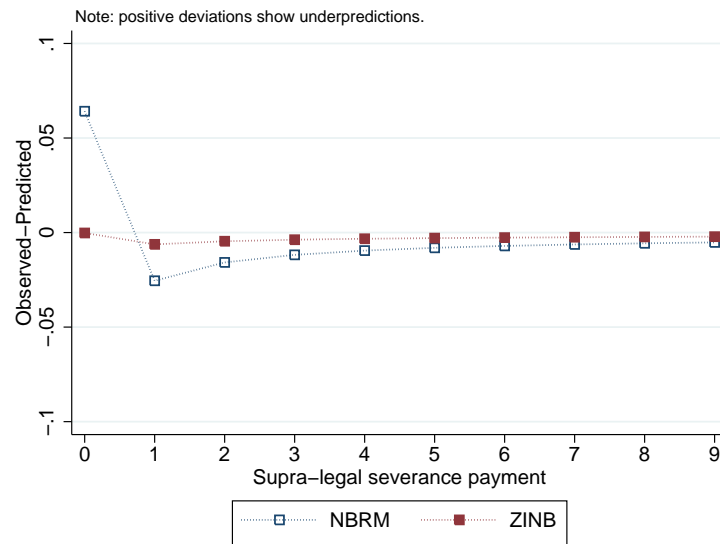
NOTE: This table displays results from the [Vuong \(1989\)](#) and the likelihood-ratio test of $\alpha = 0$, showing that the zero-inflated negative binomial model is better suited than the standard negative binomial model and the zero-inflated poisson model.

Figure 2.48 shows that, at least for the first 10 values, the zero-inflated negative binomial model does better at predicting the value of the supra-legal severance payment than its standard equivalent.

Table 2.43 displays the regression output.

⁴¹I am only considering here the *extra* amount paid to the worker, in addition to the legal minimum severance payment.

Figure 2.48: Comparison of the zero-inflated and standard negative binomial models



Source: UI data (FNA).

NOTE: The graph plots the difference between the observed and predicted values for the zero-inflated and the standard negative binomial models, for the values from 0 to 10. It shows, especially for the value zero, that the zero-inflated model better fits the data.

Table 2.43: Zero-inflated negative binomial regression on supra-legal severance payment

Supra-legal severance payment	
Gender	0.1781 (0.1578)
Age	0.4132* (0.1941)
Age^2	-0.0099 (0.0053)
Age^3	0.0001 (0.0000)
Level of education	0.3612 (0.4766)
$Levelofeducation^2$	-0.1136 (0.0940)
$Levelofeducation^3$	0.0080 (0.0054)
Sector of activity	0.0061 (0.0171)
Unskilled employee	0.6187* (0.2756)
Skilled worker	1.0540*** (0.2502)
Skilled employee	0.5086** (0.1901)
Intermediate occupation	0.0783 (0.2008)
Executive	0.7196* (0.3139)
Daily wage	-0.0002 (0.0101)
$Dailywage^2$	0.0001 (0.0001)
$Dailywage^3$	-0.0000 (0.0000)
Firm size	-0.0001*** (0.0000)
$Firmsize^2$	0.0000*** (0.0000)
$Firmsize^3$	-0.0000*** (0.0000)
Constant	2.0343 (2.3626)
Inflation equation	
Level of education	0.0853* (0.0405)
$Levelofeducation^2$	-0.0293*** (0.0082)
$Levelofeducation^3$	0.0015** (0.0005)
Unskilled employee	-1.1926*** (0.0298)
Skilled worker	-0.2547*** (0.0255)
Skilled employee	-1.2791*** (0.0268)
Intermediate occupation	-1.3648*** (0.0545)
Executive	-1.5476*** (0.0400)
Age	0.1852*** (0.0236)
Age^2	-0.0038*** (0.0006)
Age^3	0.0000*** (0.0000)
Constant	-3.2129*** (0.2943)
$\ln\alpha$	0.7568*** (0.0601)
Observations	271,230

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

NOTE: This table displays results from the regression of supra-legal severance payment on several covariates. The model used is a zero-inflated negative binomial regression to accommodate the specificity of the dependent variable, which takes only positive values with a large number of zeros. The regression is estimated on a sample of workers laid-off for non economic reasons after an open-ended contract, with seniority below 2 years. Reference category for qualification is unskilled worker.

Chapter 3

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

*This chapter is jointly written with Clément Brébion and Simon Briole. We are grateful to Luc Behaghel, Philippe Askénazy, Christine Erhel, Eric Maurin, Antoine Bozio, Andrea Weber, Thomas Le Barbanchon, Camille Landais for their help and comments, as well as to the participants of the Labour Chair workshop at the Paris School of Economics. We would also like to thank the *Unédic* and the *CASD* for providing us access to the data. We benefited from the *LABEX OSE - ouvrir la Science Économique* funding for data access.

Abstract

A little explored dimension of unemployment insurance (UI) schemes is the entitlement conditions. In this chapter, we analyse the minimum employment record condition to qualify for UI benefits on transitions in and out of employment. Using administrative panel data matching employment and unemployment spells, we provide a comprehensive evaluation of the 2009 reform that has changed the eligibility criterion in France. We show suggestive evidence of a separation response at the eligibility threshold both at the micro level – through a jump in transitions from employment to unemployment – and at the macro level – through the scheduling of contract duration in line with the eligibility requirements. In a regression discontinuity design, we also analyse the extensive margin impact of UI benefits – i.e. receiving benefits instead of not receiving any benefit – in terms of future labour market outcomes. The use of the reform as well as relevant sample restrictions make this analysis possible despite manipulation in the running variable. Our findings point to a large negative impact of UI benefits receipt on employment probability up to 21 months after meeting the eligibility criterion. Although less precisely estimated, the results on job quality do not seem to counterbalance the negative employment response.

JEL Codes: J08, J65, J68, H31

Keywords: Unemployment, Behavioural response to benefits

Introduction

Unemployment insurance (UI) schemes are often compared across countries and over time in terms of level of benefits or coverage duration. A less explored dimension – also less quantifiable – is the ease of access of those schemes. Indeed, UI schemes are often characterised by some rules determining eligibility to claim benefits, and obligations to keep on receiving them once the person has opened an entitlement. Regarding this aspect, the literature has mostly focused on the role of monitoring and sanctions (McVicar, 2008; Van den Berg, Van der Klaauw and Van Ours, 2004; Fredriksson and Holmlund, 2001; Lalive, Van Ours and Zweimüller, 2005), generally finding a positive effect on the number of days employed. However, the eligibility criterion to UI benefits has received much less attention, whereas it is at the core of the insurance dimension of unemployment schemes. It requires that the individual must have contributed to the funding of the scheme through a minimum employment record to be entitled to claim benefits. These eligibility conditions have been heavily discussed in the policy debate, as highlighted by the construction of an indicator of the strictness of eligibility criterion by the OECD (Venn, 2012).¹ This indicator is made up of several items, including the minimum employment record,² but also job-search and availability requirements, monitoring and sanctions.

This chapter precisely focuses on the impact of this minimum employment record requirement on transitions both in and out of employment. We take advantage of a reform in France that has moved the work history threshold from 6 months over the past 22 months to 4 months over the past 28 months in April, 2009. We use administrative data linking a matched employer-employee dataset to UI data for a nationally-representative panel of individuals³ between 2003 and 2012. We are therefore able to follow individuals over their employment and unemployment spells and to precisely measure the transitions and characteristics associated to each spell.

In most OECD countries, the level of benefits does not increase with work history. It implies that the eligibility condition is often defined in a binary way since, when reaching the threshold, the worker experiences a jump in the level of benefits, going from zero to the maximum amount he could be entitled to, given the level of his past earnings. Therefore, it introduces a discontinuity in the outside option of the worker that may influence his labour supply decision. This increase in the unemployment value may also be internalised by employers, and have an effect on the design of work contracts, in particular in terms of duration. Indeed, employers may rely on UI to provide a replacement income to workers between two contracts when activity slows

¹In France, for example, the current government has put on the agenda the possibility to give access to UI to the persons having resigned from their job. It questions the characterisation of the unemployment scheme as an insurance, as opposed to a safety net.

²This item weights for 1/8 of the total index.

³The panel includes workers born in October of each year.

down, all the more so if they know eligibility criterion are little restrictive. Such mechanisms are often not taken into account when assessing the efficiency of UI and the response of employed workers and employers to UI incentives is often ignored.

The 2009 French reform has to be considered in the context of a dramatic increase in the share of short-term contracts in total hiring since the beginning of the 2000's.⁴ Many factors could drive this trend, and UI rules have sometimes been put forward as a potential contributor. Indeed, to take into account evolutions on the labour market, and the growing importance of more flexible forms of employment, UI rules have been revised to give more incentives to go back to work, even for short employment spells.⁵ One objective of this chapter is therefore to shed light on the potential role of UI design in the increase of short-term contracts. We find evidence that the eligibility condition induces a separation response through an increase in the transition rate from employment to registered unemployment from almost 0 to 1% at the threshold. We interpret our findings as being driven both by more individuals separating from their job to claim UI benefits when crossing the threshold, and by individuals deciding to stay unemployed conditional on having a job separation. Similarly, the transition rate from employment to non-employment computed on a subsample of workers for which we have a better precision in the measure of work history jumps from 7% to 10% at the threshold.

We also check whether this separation response translates into an effect on the structure of contract duration at the aggregate level. We observe that the number of fixed-term 4-month contracts relatively to fixed-term 6-month contracts increased by about 30% (i.e. about 1.5 contract per month within firm) after the reform. This marked increase is more specifically concentrated in sectors previously identified as frequently hiring on very-short contracts and with a high separation rate (DARES, 2018). These trends suggest that the 4-month contract seems to become a new norm after the reform, indicating a potential role of UI design in explaining the recent increase in short-term contracts. It points to an impact of UI not only on job-seekers' pre and post-unemployment outcomes, but also on outcomes of workers who do not experience unemployment. This kind of unintended consequences going beyond the population of targeted workers have previously been underlined in the German context. The "minijobs", in Germany, i.e. the jobs associated with gross monthly earnings below 400€, have grown massively since the 2003 reform which exempted them from Social Security Contributions and income tax. It could have contributed to frame a norm in terms of number of hours of work in some sectors, especially for women (Weinkopf, 2014). This kind of norms on the number of hours worked can act

⁴The share of contracts of less than one month has increased by 165% from 2000 to 2017 (Unedic, 2018a), as depicted in Figure 3.1.

⁵In particular, in 2014, conditions to keep on benefiting from part of UI benefits in case of take up of a small job have been relaxed. All employment spells have also been revalued by taking them into account to extend the entitlement duration.

as constraint on the free adjustment of labour supply for people who would like to work a different number of hours (Müller, Neumann and Wrohlich, 2018). Similarly, a new norm on the duration of short contracts could restrain the labour supply of workers in some sectors, even of those not seeking UI eligibility.

The 2009 reform has also changed the composition of the pool of benefits claimants, giving the possibility to receive benefits from the 4th month of employment, and extending the base reference period. This could have an impact on the labour market outcomes of these workers who were not eligible to UI benefits and had no substitution income in case of no employment before the reform, and who were able to open a UI entitlement after the reform. The underlying question relates to whether being covered by UI has an impact on the probability to find a job, and on the characteristics associated to this job.

In the absence of any insurance, individuals may be forced to take a job quickly, not necessarily matching with their skills or potentially of low quality. They might also be constrained to resort to minimum social benefits. In a regression discontinuity design around the reform, we show that claiming benefits is associated with a reduction in the probability to find a job at different time horizons, without any clear positive impact on the subsequent job quality. The negative effect on employment is still present more than one year and a half after the end of the contract that made workers reach the eligibility threshold. We also document a negative impact on daily wage, more pronounced on the mid to long-term. Because of the employment response, it is difficult to disentangle the negative impact on earnings due to a reduction in the probability of employment from having lower paid jobs, conditional on being employed. However, complementary results suggest that there is no significant increase in any dimension of job quality that may counterbalance this negative impact on employment.

Although the impact of UI parameters on the behaviour of the unemployed has been extensively studied in the literature (Schmieder and Von Wachter (2016) for a review),⁶ the potential impact of UI on behaviours of employed workers and of employers is usually not incorporated in the optimal UI framework. Better understanding the interaction between UI and on-the-job outcomes would allow to enrich the traditional optimal UI framework. Theoretical mechanisms have already been highlighted (Ortega and Rioux, 2010; Zhang and Faig, 2012; Pan and Zhang, 2012; Andersen, Kristoffersen and Svarer, 2015; Hopenhayn and Nicolini, 2009), but the empirical evidence on this topic remains scarce, quite old and usually based on survey data.⁷ One contribution of this chapter is therefore to shed light on a little

⁶Many papers have highlighted the impact of UI generosity and potential benefit duration on unemployment duration (Lalive, Van Ours and Zweimüller, 2006; Lalive, 2007; Landais, 2015) or reservation wages (Feldstein and Poterba, 1984; ?; Le Barbanchon, Rathelot and Roulet, 2017) although the last two papers cannot reject the null of a zero effect.

⁷See Baker and Rea Jr (1998); Green and Riddell (1997); Christofides and McKenna (1995) for example, with the exception of Martins (2016); Rebollo-Sanz (2012); Albanese, Ghirelli and

studied question, looking at how UI can affect transitions out of employment and influence separation decisions. This aspect has never been studied in France, whereas it is likely to be highly influenced by the institutional context.⁸ We also go further by analysing the consequences of this separation response at the aggregate level, providing suggestive evidence that the overall duration of work contracts responds to UI incentives.

Our second contribution speaks to the much larger literature on UI benefits and unemployment duration. It is now a well-known empirical fact that the duration of unemployment is positively affected by the level of UI benefits (Chetty, 2008; Landais, 2015; Lalive, Van Ours and Zweimüller, 2006). However, the extensive margin effect of UI benefits on unemployment duration - i.e. the effect of having some benefits as opposed to no benefit at all - has been much less documented. One of the reason for this lack of evidence is that papers looking at this margin have essentially focused on the response in terms of exit from employment.⁹ It is this response that precisely makes the analysis of the consequences in terms of future employment prospects difficult. In particular, quasi-experimental methods become hardly usable as the endogenous employment response entails sorting at the eligibility threshold. A notable exception is Davezies and Le Barbanchon (2017), who analyse the consequences of receiving UI benefits in terms of match quality in France, as an application of the method they develop to correct for measurement error. We add to this previous analysis in two ways: (i) we are able to get rid of the endogenous employment response issue by taking advantage of the 2009 reform and making useful sample restrictions; (ii) we look at a wider range of outcomes to draw a more complete picture of the effect of receiving UI benefits. In particular, our work relates to the empirical debate on the effect of UI benefits on job quality.¹⁰ Looking only at the probability of employment could be misleading, as UI benefits could affect other dimensions of labour market outcomes. Being able to measure the impact on job quality is therefore crucial to better assess the welfare impact of UI benefits. Finally, another advantage of our setup is that we can combine the analysis of UI benefits

Picchio (2019); Van Doornik, Schoenherr and Skrastins (2018); Jäger, Schoefer and Zweimüller (2019); Baguelin and Remillon (2014). The first four papers focus on the impact of the UI eligibility criterion on flows to unemployment, and show that they are strategically timed to coincide with UI eligibility. The last two papers focus on the population of older-workers. They document that the last exit from employment is scheduled according to UI as a bridge to early retirement, and that it responds to changes in the potential benefit duration.

⁸For instance, the effect is likely to differ according to the presence or not of experience rating.

⁹Martins (2016); Rebollo-Sanz (2012); Albanese, Ghirelli and Picchio (2019).

¹⁰Whereas the standard job search model predicts a positive impact of UI benefits on future job quality, through higher reservation wages, the effect becomes ambiguous when negative duration dependence is accounted for (Nekoei and Weber, 2017). Empirically, Nekoei and Weber (2017) find a positive effect, whereas Schmieder, von Wachter and Bender (2016) find a negative effect and Card, Chetty and Weber (2007); Lalive (2007); Van Ours and Vodopivec (2008) a non-significant one. Focusing on the reservation wage channel in France, Le Barbanchon, Rathelot and Roulet (2017) find a precisely estimated zero elasticity of reservation wages with respect to the level of UI benefits.

eligibility criterion on both the transitions in and out of employment. It allows a comprehensive evaluation of the 2009 reform. One drawback of this exhaustiveness is that, although we find meaningful results on many dimensions, we are under-powered on some aspects related to job quality. Further analysis on this topic is left for future research.

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

1 Institutional background

UI in France, as in many developed countries, is made of two components: one insurance part, and one solidarity part. What characterises the insurance part is a strong contributory link. It means that what is paid to the claimants is tightly linked to their contribution to the scheme. This general principle translates into different rules: (i) the amount of benefits and social security contributions are proportional to past earnings; (ii) the potential benefit duration (PBD) is proportional to work history; (iii) the main eligibility criterion also depends on a minimum employment record. The third point is at the core of this chapter, and will be used to study the extensive margin impact of UI benefits on different labour market outcomes. We take advantage of a reform that has changed, on April 1st, 2009,¹¹ this minimum work history condition from 6 months over the last 22 months to 4 months over the last 28 months. The pre-reform period was characterised by different paths that linked a work history duration to a potential benefit duration. For example, the minimum working condition required to have worked at least 6 months over the last 22 months, and was giving right to 7 months of potential benefit duration. After the reform, a simpler rule was adopted introducing a one-to-one relationship between the number of days worked over the last 28 months and the benefit duration. The minimum work history to be eligible is equal to 4 months, and the potential benefit duration cannot exceed 2 years even when work history is longer.¹² We are going to exploit both the presence of a work history threshold in the post-reform period as well as the fact that this threshold has changed over time. Once eligible, the worker is entitled to claim benefits equal to a proportion of his past earnings, between 57% and 75%. The level of benefits stays stable along the unemployment spell.

¹¹cf. *Arrêté du 30 mars 2009 portant agrément de la convention du 19 février 2009 relative à l'indemnisation du chômage et de son règlement général annexé* (Decree of March, 30th, 2009).

¹²For workers of 50 years old or more, the maximum PBD is equal to 3 years.

2 Data and Descriptive Statistics

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

However, several caveats have to be made: First, the unit of observation in the *DADS* is defined at the firm \times individual \times year level. It means that if an individual has several contracts within the same firm the same year, they will be gathered into the same observation. In the remainder of the chapter, we will call this unit of observation a *position*. The starting date and ending dates will correspond to the first entry in the firm and the last exit from the firm within a year, meaning that the individual is not necessarily continuously employed in the firm in between. This could lead to an overestimation of the contract duration, and of the number of one-year contracts, as the *positions* starting and ending dates are often recorded as January, 1st and December, 31st although the actual contracts did not last 1 year, because it corresponds to the first day of the first contract and the last day of the last one within the same firm, or because it has not been filled properly by the employer as it does not have any consequences for the payment of contributions. As a result, we may potentially end up with measurement error when computing work history, in particular for workers often working under short-term contracts and being recalled by former employers. Second, the *DADS* does not contain information on the exit motive, preventing us from distinguishing layoffs from resignations. This information is present in the UI data, but it is then not available for those separating and not registering to UI.¹³

Keeping in mind these limitations, we can still perform our analysis both on the separation response and the consequences of UI eligibility on future labour market outcomes. The first part of the analysis focuses on whether employers and employees react to the change in the unemployment value at the eligibility cutoff by looking at

¹³Most of people resigning are not eligible to UI benefits and therefore do not register for UI.

the transitions out of employment. We first take out from our sample people who experienced particular forms of employment such as home employment for private employers, or in the public sector, as they are subjected to different rules in terms of UI. Our broad sample includes 2,690,114 individuals accounting for 18,114,742 *positions* ending between 2004 and 2012 and 3,071,283 corresponding unemployment spells. Depending on the analysis we perform, we further restrict the sample, as detailed in each section.

The second part of the analysis examines the impact of receiving UI benefits on future labour market prospects, using a regression discontinuity design. A potential separation response would make workers separating at 4 months after the reform different (i) from the ones separating at 4 months before the reform, and (ii) from the ones separating right before 4 months after the reform. Therefore, to perform our RDD without biasing the results, we make some sample restrictions. We select workers having accumulated between 4 and 6 months of work history in the neighbourhood of the reform, who separated after a fixed-term contract that has started before the reform. Restricting the analysis to this type of separation makes the assumption that the separation is not an endogenous response to the reform more plausible, as the contract has been designed before the reform. A fixed-term contract could still be breached before its expected term, but it is quite costly. We provide further empirical evidence that such a response did not occur on this restricted sample in Section 5. We also change the unit of analysis by gathering the contracts that occurred without any interruption for the same individual within the same establishment. Indeed, as the dataset is built on observations within a year, a 2-year contract is automatically split into two lines although it corresponds to a continuous employment spell, for instance. We gathered those observations into a single one, except when the type of contract has changed, to be able to capture transitions from fixed-term to permanent contracts within the same firm.

We are left with 23,559 observations. To implement our RDD, we need a precise measure of the work history for each individual, no matter his eligibility status. Therefore, we cannot use the information already available in the UI data, but we need to recompute it from the *DADS*. Because of the first limitation underlined in the previous paragraph, our measure will suffer from some measurement error. This is an issue to the extent that it will undermine the precision of our estimation. However, it should not bias our results as there is no reason why the measurement error should differ from one side of the cutoff to the other, at the close neighbourhood of the threshold. For the subsample for which we have the information in both datasets (for workers registering as unemployed), we can cross-check our computed variable with the one from UI agency. We get approximately a 75% match rate if we take a rather slack definition of a match.¹⁴

¹⁴More precisely, we define the two values as matching when they are equal ± 30 days, as there

We use a second database called the *MMO*,¹⁵ from the Ministry of Labour. This last database captures accurately every job transition with information on ending and starting dates at the contract level, and on the separation motive. However, it is a cross-section and therefore does not allow to track individuals. A second limitation is that it does not measure job-to-job transitions with the same employer: for instance, a fixed-term contract extended with a new fixed-term contract or converted into a permanent one without any interruption will appear in a single spell. We use this data to analyse the aggregate response to the change in eligibility criterion in terms of contract duration (Section 4). Indeed, the measure of the aggregate response does not impose to have a panel structure, whereas the *MMO* provides a more accurate information on contract duration and separation motive than the *DADS*. If we assume that the conversion and renewal behaviours stay unchanged before and after the reform, the last limitation should not affect the results. Gathering all contracts ending between 2005 and 2015, and disaggregating by type of separation, we end up with the sample described in Table 3.1.

The different analyses developed in this chapter, and especially the second part, focus on a particular population of workers with short work history. Tables 3.2 to 3.4 describe its characteristics as well as those of their jobs. Their short work history can be explained both by their shorter experience on the labour market, but also by their weaker attachment to the labour market, as indicated by the lower hourly wage, and their lower chance to have a permanent contract or to work full-time. They are also more frequently male, work in smaller plants, and have a higher probability of holding multiple jobs at the same time.¹⁶ This last characteristic is consistent with their higher share of temporary and part-time contracts.

Workers with short work history are typically found in sectors such as the agriculture, food and accommodation, administrative services, arts and entertainment, and much less in the manufacturing or the construction sectors. This pattern coincides with the one underlined by studies describing which sectors were frequently hiring under very short contracts (DARES, 2019). In terms of types of occupations, workers with short work history are less likely to be in a higher-ranked occupation such as civil servant executive, administrative and business executive or intermediate

is also a difference in way of counting days in the two bases that could lead to a small mismatch without implying any measurement error.

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

¹⁶When a person holds multiple jobs at the same time and loses one of them, she is entitled to receive UI benefits as long as she fulfils the eligibility conditions. The benefits will be computed based on this lost activity, and potentially previous ones that have been lost within the last twelve months. The person can receive her benefits in addition to earnings from the other jobs she did not lose. In the event that she loses one of her jobs again, the UI entitlement will be revised to take into account this new job loss.

profession, head of a company, engineers, and more likely to be commercial or service employee, unskilled or agricultural worker.

3 Empirical evidence of a separation response

The eligibility criterion to UI benefits may influence the labour supply decision of workers as it introduces a sharp discontinuity into the value of unemployment. Indeed, when crossing the work history threshold, workers experience an increase in their outside option value, as they will be entitled to receive a replacement income in case they stop working.¹⁷

One piece of evidence that workers and/or employers react to incentives created by the UI eligibility criterion is provided by the distribution of the probability to transit from employment to unemployment with respect to previous work history. Figure 3.2 shows that conditional on ending a contract, the probability to register for UI jumps at the eligibility threshold. It suggests that, as they are eligible to UI benefits, workers will not try to find another job but choose to stay unemployed. Figure 3.2 also shows a spike at 6 months, that may be due to regularity in contract duration, as 6 months is a reference point. However, we do not observe any break in the trend, whereas we do observe a discontinuity during the pre-reform period, when the eligibility criterion was at 6 months within the last 22 months (Figure 3.3). On the reverse, we do not observe any discontinuity in the probability to transit from employment to unemployment at 4 months within the last 28 months during the pre-reform period, or at 6 months within the last 22 months during the post-reform period (Figures 3.4 and 3.5). It indicates that discontinuities observed at eligibility thresholds in Figures 3.2 and 3.3 cannot be explained by those thresholds being reference points for reasons outside UI eligibility.

Part of the jumps we observe in Figures 3.2 and 3.3 may be mechanical: since workers are not eligible below the thresholds, it implies that the probability is necessarily equal to 0 at the left hand side of the threshold. However, those graphs are still informative as they suggest that, conditional on ending a contract, workers choose not to look for another job but to take their UI benefits.

Indeed, the behavioural response we would like to measure is twofold: (i) we are first interested in knowing whether workers and/or employers will deliberately choose to end the work contract as soon as workers become eligible; (ii) but we also want to know whether, conditional on the contract ending, they will choose to stay unemployed once they are eligible whereas they would have taken another job in the absence of UI. Figures 3.2 and 3.3 suggest that the second mechanism is at play,¹⁸

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

¹⁸However, we cannot totally rule out the hypothesis that previously, workers would have chosen to stay unemployed to the same extent, without registering for UI as they were not eligible to claim

whereas we cannot be that assertive concerning the first one.

That is the reason why we turn to the measure of the biweekly transition from employment to registered unemployment (as defined as being registered for UI) and from employment to a broader definition of unemployment that includes not working and not being registered for UI. We convert our data defined at the *position* level into a dataset where each observation represents two weeks of each worker's career path, with information on the type and date of transition, and the work history at the end of the two weeks. For our main period of interest, the starting point is defined at the worker level, as the first contract starting after the reform. We then observe what happens at a biweekly rate. The different types of transitions are defined as follows: from employment to employment; from employment to registered unemployment; from employment to non-employment; from non-employment to employment. The second type is included in the third one. We can then measure whether the transition rate jumps at particular values, on the sample of 864,534 individuals that we observe from their first contract after the reform up to two years after.

Figure 3.7 shows a clear jump in the probability of transiting from employment to unemployment at the eligibility threshold. It complements previous evidence by suggesting that not only individuals decide to stay unemployed and claim benefits conditional on separating, but they also choose to separate precisely to start receiving UI benefits. However, we do not observe such a pattern if we look at the transitions from employment to non-employment (Figure 3.6). Those two results mean that we cannot rule out the hypothesis that the jump in transitions from employment to registered unemployment may only reflect a change in registration behaviour. Substitution from non-registered non-employment to registered unemployment could rationalise our findings.

We still have several reasons to think that the jump in the transition rate from employment to registered unemployment partly arises from the separation response due to UI eligibility. First, one reason might be that, because separations at this exact threshold are motivated by eligibility to UI, and as transitions to UI are a small share of total transitions to non-employment, the effect is attenuated when we pool together all transitions out of employment. Second, the measurement error in the computation of work history previously mentioned might affect the precision of the results and hide the discontinuity. One way to limit this concern is to focus on workers whose number of hours worked during the *position* corresponds to the number of hours worked for a person employed full-time all the days covered by the *position*. This restriction ensures that the *position* corresponds to one single contract, and alleviate measurement issues. One drawback is that we focus on a population of individuals working full-time, which may not be the population reacting the most to incentives to exit employment. Still, Figure 3.8 shows that there is a

benefits.

small discontinuity in the transition rate from employment to non-employment at the eligibility threshold, confirming that part of the difference in pattern between transition rate to registered unemployment and non-employment may be due to precision issues. This is confirmed by Table 3.5 which measures, in a regression discontinuity design spirit, the discontinuity in the transition rate at the threshold on this specific sample. According to this regression, the transition rate increases by about three percentage points at the cutoff, which represents a 43% increase relative to the rate right below the cutoff.

Another limitation is that we cannot distinguish layoffs from quits in the *DADS*, whereas only layoffs allow to qualify for UI benefits. Evidence from other countries (Rebollo-Sanz (2012) in Spain and Albanese, Ghirelli and Picchio (2019) in Italy) point to a significant separation response at the eligibility thresholds in comparable institutional settings.¹⁹ In the French context, Khoury (2019) highlights strategic separation scheduling in the case of economic layoffs to qualify workers for higher UI benefits. These different findings suggest that a response to UI incentives in terms of transition from employment to non-employment is a likely phenomenon. In addition, this mechanism would be consistent with the aggregate results described in next Section.

Table 3.6 confirms previous graphical evidence. It shows that having a work history right above the eligibility threshold is associated to a 0.8 percentage point increase in the probability to transit from employment to registered unemployment, from an almost 0 probability at the left-hand side of the cutoff. The low transition rate in absolute terms is explained by the fact that we analyse only the opening of new UI rights. Indeed, the first time a person registers to UI, she has to open a right based on her work history over the last 28 months. If she finds a job before the end of her entitlement, and then returns to unemployment, she can either resume her former right, open a new right based on her very last employment spell, or take a mix of both rights. The important point here is that she does not need to fulfil the minimum employment history condition again to be able to receive UI benefits as long as the former right is not exhausted. That is the reason why we focus on the opening of new UI rights to better capture the effect of the eligibility criterion. We then only measure a subsample of all transitions to registered UI. Both Tables 3.6 and 3.5 and Figures 3.7 and 3.8 are hinting at a response from firms and workers to the increase in the value of unemployment through separation at the eligibility threshold. As most of the resignations do not allow workers to claim UI benefits, it implies that employers do contribute to this separation response.

¹⁹Both Rebollo-Sanz (2012) and Albanese, Ghirelli and Picchio (2019) can make the distinction between quits and layoffs. They show that there is a jump in the hazard rate out of employment when looking at layoffs, but not when looking at quits.

4 Impact of UI eligibility criterion on contract duration

The analysis developed in Section 3 indicates that workers respond to the change in the value of unemployment at the eligibility threshold through a higher probability to transit to unemployment, especially registered unemployment. However, to be eligible, workers also need to prove that job loss was involuntary. It means that they are entitled to receive benefits only after having been dismissed or laid-off, or having mutually agreed on contract termination with their employer. Only few cases of resignation are considered legitimate and open entitlements to UI (to follow a spouse who has been transferred for example). This rule implies that employers are instrumental in this strategic job separation, by accepting to dismiss the worker, by mutually agreeing on contract termination, or, more likely, by designing shorter work contracts.²⁰

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

The relaxation of the UI eligibility criterion is contemporaneous with a sharp increase in the share of short-term contracts in total hiring, and in the share of employees rehired by the same employer. Those two trends are compatible with employment relationships where employers would offer a contract just long enough so that workers would be eligible to UI benefits at the end of the contract, then rely on UI to compensate workers, and then potentially rehire them. This type of employment relationship has the triple advantage to: (i) allow a more flexible adjustment of the workforce to the variation of the firm activity with shorter commitment periods,²¹ as underlined in [Christofides and McKenna \(1995\)](#); (ii) help employees accept shorter contracts as it is counterbalanced by UI benefits and recall behaviour; (iii) allow employers to retain workers and to have them investing in firm-specific human capital without bearing the cost of high employment protection. Indeed, the share of recalled workers has increased along with the share of short-term contracts in recent years ([Benghalem, 2016](#); [Journeau, 2019](#)).

One of the research questions is therefore to investigate whether the change in eligibility criterion has contributed to the decrease in the duration of contracts

²⁰Most of the entries to UI are through end of fixed-term or temporary contracts (about 24% between May 2016 and May 2017 for example, according to UI figures).

²¹The commitment period here refers to the duration of the fixed-term contract. Although it is possible to break a fixed-term contract before its end, it entails an economic and administrative cost.

offered by employers. Section 3 has highlighted, at the micro level, that workers respond to the change in eligibility by transiting more frequently from employment to unemployment at the eligibility threshold. As the threshold has moved from 6 to 4 months, it would lead to transitions out of employment sooner in the work history. We now turn to the analysis at the more aggregate level of the evolution of the duration of contracts. The data used in this analysis corresponds to the MMO (*Mouvements de Main d'Oeuvre*) coming from administrative records employers have to fill each quarter on inflows and outflows from the firm. It informs on the date and type of flows and on some characteristics of the worker and the firm. We perform a difference-in-difference comparing the evolution of the number of 4-month and 6-month contracts, before and after the reform. We first focus on fixed-term contracts. The idea is to capture the difference in the before-after difference in the number of contracts of 4-month and 6-month duration. This is measured by the *4 – month (versus 6m) contracts* \times *Post* coefficient of Table 3.7. The fourth specification, including firm, sector, year and month fixed-effects, indicates that the reform in the UI eligibility criterion seems to have induced 1.53 additional contracts ending between 4 and 5 months as compared to 6 and 7 months on average within a firm within a month. It represents a 17.4% increase. When restricting to the sample of firms present in both periods and having at least one 4-month and one 6-month contract ending during each period, the increase is equivalent to 17.9%. This evidence is confirmed by Figure 3.9 which decomposes the increase into yearly variation. It depicts a clear jump after the reform, that slightly decreases the following years, whereas no significant difference is observed before the reform. The year after the reform, there is, on average, more than two additional fixed-term contracts ending between 4 and 5 months rather than between 6 and 7 months as compared to the year before, within a firm within a month. As the average within firm number of fixed-term contracts ending each month with a duration between 4 and 5 months is equal to 4.8 in 2008, it represents a 45% increase.²² Figure 3.9 suggests that employers substitute 6-month contracts with 4-month contracts when making their hiring decisions as a response to the reform. This response indicates that the 4-month contracts tends to become a new norm among short-term contracts after the reform.

First measuring an overall increase in the relative number of 4-month contracts, we push the analysis further by looking at the within-sector decomposition of the change. Figure 3.10 shows the difference-in-difference estimate using the fourth specification of Table 3.7 within each of the thirty-five sectors defined using a 2-digit classification. We observe that thirteen sectors out of thirty-five experience a significant increase in the relative number of 4-month contracts after the reform. More importantly,

²²However, we should keep in mind that the difference-in-difference coefficient measures the relative increase of 4-month contracts, which may not be equivalent to the change in the number of 4-month contracts in absolute terms.

those sectors are mainly sectors identified as having a high separation rate,²³ or having a high-share of very short contracts in total hiring (DARES, 2018). Out of the thirteen sectors identified as having a high separation rate or a high share of very short contracts, ten experience a positive change in the number of 4-month contracts relative to 6-month contracts after the reform. This picture suggests that employers in sectors used to hire repeatedly under very short contracts with a high turnover may be more aware of UI eligibility rules and more willing to minimise contract duration and maximise flexibility.²⁴ They may then particularly react to a change in the eligibility criterion.

This evidence is confirmed by a firm-level analysis, where we compute the change in the number of 4-month contracts relative to 6-month contracts using the same difference-in-difference approach within each firm. We keep only firms that are observed in both period and with at least one contract ending at four months and one ending at six months before and after the reform. We then examine the distribution of sectors among firms with a high difference-in-difference coefficient, defined as a coefficient above the 90th percentile of the coefficient distribution. These firms are firms where the relative increase in 4-month contracts has been substantial. Table 3.8 compares the distribution of sectors among these firms and all the other firms, while Figure 3.11 reports the difference in the share of firms belonging to each sector between these two groups when this difference is positive. It means that we plot only the difference in share for sectors that are over-represented among high-coefficient firms. Sectors identified as having either a high separation rate or a high share of very short contracts are again highly represented among firms with a high increase in 4-month contracts. This is illustrated by the fact that most of the points on Figure 3.11 are red, i.e. most of the over-represented sectors among firms with a high relative increase are sectors with a high turnover, and that most of the red points are located where the difference is the highest. It provides further evidence that some sectors are particularly reacting to the change in the UI eligibility criterion.

Although we expect the response to be particularly high for fixed-term contracts, as they represent the majority of the flows and the easiest way to adjust the workforce to economic fluctuations, we also examine other types of contract separation. Overall, we do not see any positive response in terms of economic layoffs or dismissals on personal grounds (Figures 3.12 and 3.13 and Tables 3.9 and 3.10). However, we also observe that there is a positive response concentrated in the same sectors as for fixed-term contracts, where the separation rate and the share of very-short contracts are high (Figures 3.15 and 3.16), especially for economic layoffs. Reassuringly, we

²³The 2019 unemployment insurance reform in France has introduced a contribution scheme similar to experience rating, targeting seven sectors whose high turnover was making UI bear a substantial cost. The criterion to identify those sectors is to have a separation rate higher than 150% (Decree No. 2019-797 of 26 July 2019 on the unemployment insurance scheme).

²⁴In particular, temporary employment agencies are included in these sectors characterised by a strong positive response in the number of 4-month contracts.

do not observe any similar pattern for voluntary resignation, as this motive does not open entitlements to UI, and should not be influenced by UI eligibility rules (Figures 3.14 and 3.17). The evolution for other separation motives can be seen in Tables 3.11 to 3.14 and Figure 3.18. In particular, we observe a small positive response when looking at pre-retirement separations. This is in line with previous findings (Baguelin and Remillon, 2014) highlighting a strategic scheduling of retirement and pre-retirement dates in order for workers to be covered by UI first and then by pension schemes without any interruption in payment. Those motives may then be particularly sensitive to a change in UI eligibility rules.

The increase in the relative number of 4-month fixed-term contracts is the most substantial in the transportation, human health activities, legal, accounting, management, architecture, engineering, technical testing and analysis activities, and real estate activities sectors. Figures 3.19 to 3.22 depict the yearly variation in those sectors, and confirm that the parallel trend hypothesis holds.

In Table 3.15 and Figure 3.23, we corroborate previous finding using UI data providing information on the set of the last contracts used to open UI entitlements (that are the contracts just preceding the UI spell). It does not represent the universe of contracts, but has the advantage of being a panel and including work history information. As it is UI data, one may be concerned about a potential increase in the share of 4-month contracts being driven by the fact that 4-month contracts are sufficient to open a UI entitlement after the reform whereas it was not the case before. This would mechanically lead to less 4-month contracts being recorded before the reform. That is why we exclude workers registering with a work history between 4 and 6 months, as they would have been invisible before the reform.²⁵ Figure 3.23 depicts the yearly evolution of the share of each type of contracts among contracts of 1 year or less, according to their ending date.²⁶ The vertical line separates the pre and post-reform periods. While the trends are parallel during the pre-reform period, we observe that after the relaxation of the eligibility criterion, the share of 4-month contract has dramatically increased whereas the share of 6-month contracts has slightly decreased.²⁷ The response in terms of number of 4-month contracts already measured in the MMO data seems to be exacerbated in the UI data. It may be explained by the fact that (i) UI data includes information only on the last employment spell used to open a UI entitlement, which may be particularly subjected to optimisation, and (ii) that sectors with a high turnover, where the response as measured by the MMO is the highest, may be over-represented in UI data as they

²⁵Including them in the sample indeed yields higher estimates of the increase and decrease in 4 and 6-month contracts shares, respectively of +2ppts and -1.5ppts.

²⁶The ending date of the contract is used to determine under which UI rules the worker falls.

²⁷We also represent the share of 1-month and 10-month contracts as a way to control for the trend, which may be particularly crucial at a time of economic crisis.

send a high share of workers to UI. Figures 3.24 to ?? decompose the change into separation motives, drawing a similar picture to the one when using the MMO: the response is the highest for fixed-term contracts.

The fact that we observe this pattern even if we excluded workers with a work history between 4 and 6 months is an argument in favour of the hypothesis that the 4-month contract has become a new norm after the reform, at least in some sectors familiar with short-term employment, no matter if the worker is already eligible to UI or not. If we believe this hypothesis is true, it means that UI design not only affects job-seekers before unemployment, when they are employed, but also potentially workers who did not and will not experience unemployment, through a change in contract duration practices.

5 Extensive margin effect of UI benefits

Although the effect of higher UI benefits on employment outcomes has been largely documented, much less is known on the effect of UI at the extensive margin. Several reasons can be put forward: (i) such an analysis requires to have data on employment and unemployment spells of all workers, not only those registering to UI, to be able to compare eligible and non eligible workers; (ii) the effect on transitions out of employment highlighted in the Sections 3 and 4 can hinder the analysis of the effect of UI receipt on future employment outcomes. Indeed, if the eligibility to UI benefits has an impact on the probability to transit from employment to unemployment, it becomes difficult to compare eligible to non eligible workers. For example, comparing workers at both sides of the eligibility threshold in a regression discontinuity design spirit may lead to biased results if there is sorting into unemployment on one side.

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

To be fully convinced that such restrictions of our sample ensures that no separation response biases the analysis, we can check whether there was not a disproportionately high number of fixed-term contracts of 4 months or less that have started before the reform and have ended right after. Figures 3.29 to 3.33 depict that share of contracts (i) having started before the reform; (ii) of a certain duration among contracts of 1 year and less, according to their ending date. For 4-month contracts, for example (Figure 3.33), we see that there is indeed an increase for contracts ending in April. However, this increase is likely to be driven by the high number of contracts starting on January, 1st, and mechanically ending at the end of April.²⁸ Indeed, we observe the same increase for years different from the one of the reform, indicating that this pattern is driven by seasonality.

We implement a fuzzy regression discontinuity design on this restricted sample made of 23,559 observations. The treated group is composed of workers having started a fixed-term contract before April, 1st, 2009 that ended after the reform and made them reach a work history lying between 4 and 6 months. They are eligible to unemployment benefits. The control group is made of workers reaching the same work history interval after a fixed-term contract ending before the reform, and who are then not eligible to UI benefits. By construction, the ending date of the contract cannot exceed 6 months after the reform. We then take a similar 6-month time window before the reform.

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

Table 3.16 provides some descriptive statistics on treated and control workers. Control workers have, on average, a slightly higher daily wage, which seems almost entirely driven by the fact that they work more frequently full-time. They also work in smaller establishments. Differences are not big and only concern a few covariates, and thus do not seem to challenge the validity of the RDD. More importantly, we will check in next subsection that these covariates do not differ discontinuously at the time threshold. Among treated workers, takers and non-takers do not differ in terms of socio-demographic characteristics (Table 3.17). Not surprisingly, takers have a higher wage and a higher work history,²⁹ which is associated to a higher benefit and a longer potential benefit duration.

²⁸This mechanism is also partly at play for contracts ending in May as we have selected the share of contracts between 4 (included) and 5 (excluded) months.

²⁹By construction, both takers' and non takers' work history lies between 120 and 180 days.

Empirical methodology – The equation we estimate is the following:

$$Y = \alpha + \tau \mathbb{1}_{Z \geq c} + \delta_f f(Z) + \delta_g g(Z) \mathbb{1}_{Z \geq c} \quad (3.1)$$

with Y being the outcome, such as the employment probability in this case, $\mathbb{1}_{Z \geq c}$ an indicator equal to 1 when the running variable – the ending date of the contract – is greater or equal to c , the time cutoff, equal to April, 1st, 2009. $f(\cdot)$ and $g(\cdot)$ are flexible functions that we allow to differ on each side of the cutoff.

In this setting, the RD design is qualified as “fuzzy” in the sense that the probability to receive UI benefits does not jump from 0 to 1 for workers with more than 4 months of work history right after the reform. Indeed, having accumulated 4 months of work history at the end of a contract does not mean the person will immediately open an unemployment right as (i) she may very quickly transit to another job; (ii) she may not be informed about her eligibility; (iii) she may be informed but not be willing to take her benefits for many reasons, such as stigma for example.³⁰

It follows that:

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

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

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

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

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

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

The identification rests upon two assumption: (i) monotonicity, *i.e.* the fact that crossing the cutoff does not cause, at the same time, some units to be treated and

³⁰According to figures from UI in September 2016 (Insee, 2018), about 75% of individuals eligible to UI benefits actually claim them.

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

Validity conditions of the RDD – One key assumption to check for the RDD to be valid is that there is no manipulation at the threshold, or strategic sorting of worker at either side of the threshold. If it was the case, we would have a selection bias that would prevent us from comparing the populations at each side of the cutoff. In particular, we want to check if there is no separation response to the eligibility that would make the density of contract terminations jump at the threshold. We perform a [McCrary \(2008\)](#) test to check that the density of the contract ending date is smooth at the cutoff (Figure 3.34). The density exhibits some spikes at each month interval – including at the cutoff – due to the regularity in the starting and ending dates of the contract. However, the histogram (Figure 3.35) indicates that the spike is of the same magnitude as for other months of the year. We also perform the same Mc Crary test at the same time threshold one year before, that confirms that the spike is only due to regularity in contract dates (Figure 3.36). These different tests demonstrate that there is no precise sorting at the threshold, and that RDD can be considered “as good as randomization” in the neighbourhood of the threshold.

First stage estimation – Equation 3.1 shows the reduced form of two equations capturing the first stage relationship between the ending date of the contract and the opening of a UI right (Eq. 3.2) and the second stage relationship between the opening of a UI right and labour market outcomes (Eq. 3.3).

$$UI = \alpha_f + \tau_f \mathbb{1}_{Z \geq c} + \beta_{f_f} f_f(Z) + \beta_{g_f} g_f(Z) \mathbb{1}_{Z \geq c} + \mu_f \quad (3.2)$$

$$Y = \alpha_s + \tau_s UI + \beta_{f_s} f_s(Z) + \beta_{g_s} g_s((Z)UI) + \mu_s \quad (3.3)$$

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

Table 3.18 shows that being located at the right hand side of the cutoff makes the probability of opening a UI right significantly increase, from 5.8 to 6.9 percentage points, depending on the specification. Although the effect is not very strong, the estimate is highly significant, and the jump in the probability is clear, as depicted on Figure 3.37. The weak first-stage regression could raise some precision issues. Table 3.18 provides F-statistics demonstrating the reliability of the first-stage estimation for all specifications. As underlined in previous subsection, many reasons could explain an imperfect takeup of UI rights, the main one being that there are many job-to-job transitions with small interruptions in between. Therefore, people who know they will be employed again in the very short-run will plausibly not undertake the administrative burden of registering as unemployed.

To fully conclude that the difference in outcomes we observe between populations at each side of the threshold can be imputed to the difference UI takeup, we need to rule out the influence of other variables at the threshold. Figures 3.38 to 3.42 do not depict any clear jump in the distribution of covariates at the threshold. The small decrease in the proportion of full-time workers that we observe graphically does not translate into a significant change in the regression, as confirmed by Table 3.19. Figure 3.43 shows graphically that the differences are not significant at the 5% level. As already underlined in the analysis of the Mc Crary test, strategic sorting of people on either side of the threshold is very unlikely as the eligibility requirements are closely checked by the unemployment insurance.³¹

Second stage estimation – Empirically, we estimate Equation 3.1 using a local polynomial regression. The bandwidth has been chosen using an optimal bandwidth selection method minimising the mean squared error (Calonico, Cattaneo and Titiunik, 2014; Calonico, Cattaneo and Farrell, 2018; Calonico et al., 2018). The main specification uses the local linear regression, but Tables 3.26 to 3.31 show the same regression using a quadratic specification. Results are quantitatively similar, but the higher-order specification being more demanding in terms of number of observations, coefficients are not always significant. Indeed, the sample restrictions that have been made to ensure that the analysis will not be polluted by a separation response imply that the sample size is smaller, and the coefficients less precisely estimated. However, the fact that the coefficients are of the same magnitude is reassuring on the validity of the effect.

Tables 3.20 to 3.25 show that there is a strong negative response on employment probability even in the long-run (up to 21 months after contract separation). Receiv-

³¹In order to open a UI right, a worker must justify his work experience based on employers' certificate delivered at the end of each contract or on payslip.

ing UI benefits as opposed to not receiving any benefit at all is predicted to decrease employment probability in the future to a large extent.³² Results are surprising in the sense that we would expect a negative impact in the very short-run and a positive one in the longer-run, as workers in the control group are incentivised to accumulate work history to become eligible in the future. However, we observe that coefficients are not significant in the first three months, and then become negative and significant. A potential explanation is that the population of controls ending a contract before the reform with a work history between 4 and 6 months may only need to work a few hours or a few days after the reform to become eligible to UI benefits, as they would have a work history between 4 and 6 months with their last contract ending after the reform. This could explain why the very short-term effect is not negative. On the other hand, because they need to work a positive number of hours or days to qualify for UI benefits as compared to the treated, they would end up being eligible with a longer average work history than the treated. This would entitle them to a longer benefit duration. If we believe that a longer benefit duration is helpful in finding a more stable job, it could explain that they are less unemployed on the longer-run. In any case, the interpretation of the RDD estimate is not straightforward as it captures not only the effect of being eligible to UI benefits, but also the effect, for control workers, of having incentives to find a job quickly to increase work history.

Figure 3.44 illustrates the effect graphically on the probability to be employed 6 months after the end of the contract determining the eligibility status. Similar graphs plotting the probability to be unemployed and to be in the labour force, and the corresponding regressions, can be found in Figures 3.45 and 3.46.

Consistently, we find a positive impact of receiving UI benefits on the time to the next employment spell, indicating that workers receiving UI benefits take more time to find a new job (Table 3.32). This result is in line with what has been previously found on the impact of UI benefits at the intensive margin on the unemployment duration (Card and Levine, 2000; Chetty, 2008; Card et al., 2015; Kroft and Notowidigdo, 2016; Landais, 2015; Le Barbanchon, 2016). Indeed, it has been shown that more generous UI benefits were associated with longer unemployment spells. However, much less has been said on the extensive margin impact of UI benefits, that is the effect of receiving any benefit at all. In this chapter, we show that there is a negative and long-lasting impact of receiving UI benefits on the probability to be employed.

To complement this picture, we then look at other dimensions of employment related to job quality. The literature on this topic draws less clear-cut conclusions (Nekoei and Weber, 2017; Card, Chetty and Weber, 2007; Schmieder, von Wachter

³²Results are quantitatively difficult to interpret as coefficients are often greater than one whereas the outcome variable is a probability. A bivariate probit model has been used as a complement to accommodate the binary nature of the outcome variable. Results displayed in Tables 3.104 to 3.109 indicate a 60 percentage points drop in the probability of being employed in the short-run.

and Bender, 2016; Lalive, 2007; Van Ours and Vodopivec, 2008). The standard job search model predicts that receiving UI benefits would enhance job quality as reservation wages are set higher. However, negative duration dependence could counterbalance this effect. Tables 3.45 to 3.47 show a negative impact on daily wage, that starts to appear in the medium-run and is still present in the long-run. However, for the assumption of the RDD to hold, we need to take into account in the estimation all the workers of the sample without restricting to those being employed. It means that we impute a zero value to earnings of workers not employed at the different time horizons. Not considering them in the estimation would mean to condition on an endogenous variable, that is being employed. Then, the negative relationship can arise both from the negative impact on employment or from a negative impact on daily earnings, conditional on being employed.

To better disentangle both channels, we look at cumulative earnings over a 2-year period. This measure allows not to condition on endogenous employment status but still to capture a potential effect on job quality. The idea is that, over a 2-year horizon, if the receipt of UI benefits would have a positive and significant impact on daily wage, the positive effect on wage would have the time to offset the negative effect on employment probability. Table 3.48 shows negative coefficients, although standard errors are large. Compliers forego about 30,000€ over two years, which amounts to an average monthly loss of 1,250€ in gross terms. Compared to the average daily wage of control workers reported in Table 3.16, it represents a 76% reduction, although the difference in cumulative earnings partly comes from a lower number of days employed, and not only from lower daily wages. This sizeable effect indicates that compliers do not seem to catch up on their lower employment probability through higher earnings.

Not to restrict the analysis only to the monetary dimension of job quality, we look at other outcomes associated to the next employment spell.³³ Tables 3.49 to 3.52 show the effect on the probability to have a permanent contract, to work full-time, to work in the same 2-digit industry as in the previous job and on the duration of the next employment spell. The effect seems negative on working hours, but positive on the probability to have a permanent contract, although the duration of the next employment spell is negatively impacted. However, none of the coefficients associated to these qualitative dimension of the next employment spell are significant.

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

³³By construction, individuals who do not find a job within the observed period will have those variables coded as missing.

benefits: (i) the first method consists in measuring the probability of having a job with a given characteristic at different time horizons, putting a 0 value both to those not having a job and to those having a job not meeting the criterion under study. Similar to the analysis of daily wage, it yields an unbiased estimate of the causal impact of the reform, but it does not allow to distinguish between the employment probability channel and the quality channel conditional on having a job; (ii) we use the technique developed by [Lee \(2009\)](#) to get bounds on the treatment effect on different dimensions of job quality, taking into account the selection into employment. Results yielded by both techniques can be found from [Tables 3.53 to 3.92](#). All in all, quality dimensions measured unconditionally seem negatively impacted, which is not surprising given the negative effect on employment probability. Bounds on treatment effects are generally not very informative as they are almost centred around zero, except for the probability to have a permanent contract ([Tables 3.77 and 3.78](#)). The intervals still include 0 but are to a large extent positive in the short-run. This result combined with positive (but insignificant) coefficients on the conditional probability that the next employment spell is under a permanent contract suggest that there might be a positive impact of UI benefits receipt on this dimension of job quality. The fact that we do not find any significant positive impact of UI benefits receipt on the quality of the match - as measured by the probability to work in the same 2-digit industry as before - is not in line with the findings in [Davezies and Le Barbanchon \(2017\)](#). However, they are examining a different sample (before the 2009 reform) at a different threshold. They look at the 6-month eligibility threshold whereas we use the time threshold of the reform. Potential sorting at the eligibility threshold caused by the eligibility status could affect their results.³⁴

To have a clearer idea on the ultimate impact on career path, we try to have a measure of the stability of the working status over a long-term horizon – 2 years. Therefore, we look at the number and duration of the employment spells over the following two years. The idea is to capture whether the individual does a lot of transitions in and out of employment, and whether his employment spells are short or not. Indeed, having a higher probability of being employed at different points in time could still be associated with a highly fragmented path, if the person goes very often from one job to the other but with short breaks in between. [Table 3.101](#) indicates that receiving UI benefits is associated to a lower number of employment spells over the following two years. This could either be interpreted positively – a more stable path – or negatively – more time unemployed. The impact on the number of unemployment spells is positive but insignificant, whereas the impact on the total number of days employed is negative and significant in the linear specification

³⁴As mentioned at the beginning of the section, the assumptions of a regression discontinuity design around the eligibility threshold may not hold if there is a separation response at the threshold because the worker becomes eligible to UI benefits.

(Tables 3.102 and 3.103). Those results taken together suggest that receiving UI benefits does not seem to contribute to a more secured career path.

To be more able to interpret the result, we complement the analysis by looking at the employment response using a bivariate probit specification. Indeed, the regression discontinuity design relying on local polynomial estimation on each side of the cutoff may not be suitable for binary outcome variables such as probabilities, as they yield coefficients outside the feasible range. However, bivariate probit relies on a parametric specification and distributional assumptions,³⁵ which may explain why they may not be preferred to linear models. We provide the estimation results using a bivariate probit on the main outcome variable, the employment probability. Tables 3.104 to 3.109 display marginal effects that are qualitatively similar to results obtained using local polynomial regressions. Receiving UI benefits is predicted to decrease employment probability six months after the end of the contract by 61 percentage points. The effect remains strong around 53 percentage points up to 21 months after the end of the contract.

6 Conclusion

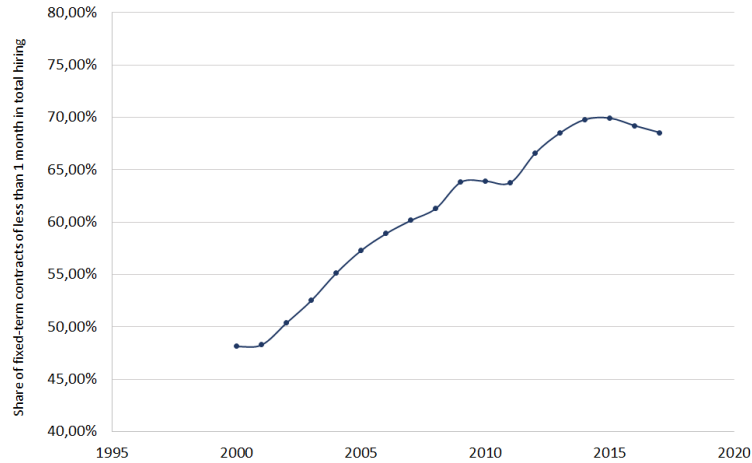
In this chapter, we look at the impact of the eligibility criterion to UI benefits that imposes a minimum employment history. We complement previous analyses of the impact of the *level* of UI benefits at the intensive margin on *unemployment* duration by looking at the impact of *receiving any UI benefits* at the extensive margin both on transitions *in and out* of employment. The first part of the analysis reveals that individuals react to their eligibility status by increasing their transition rate from employment to unemployment. At the aggregate level, we also observe an increase in the share of 4-month fixed-term contracts, corresponding to the new threshold, after the reform, partly at the expense of fixed-term 6-month contracts, which correspond to the former duration to be eligible to UI benefits. Those results are indicative of a response both from employers and employees to incentives generated by UI in the way they design contracts and trade-off between employment and unemployment. It suggests that workers and firms collude to extract rent from UI: workers get benefits while employers gain flexibility. In the second part of the chapter, we find that claiming benefits is predicted to decrease future employment probability, even in the medium-term, without any significant improvement in terms of job quality. However, these results should not necessarily be interpreted as an argument in favour of the repeal or the decrease of UI benefits, for several reasons: (i) some of our estimates on job quality suffer from a lack of precision and results must be interpreted

³⁵Notably regarding homoskedasticity, homogenous treatment effect and normally distributed error terms.

cautiously; (ii) the fuzzy RDD allows the estimation of a local average treatment effect on the compliers; (iii) we are exploiting a change in the eligibility criterion from 6 to 4 months. The last two reasons make it hard to extrapolate the results to any unemployed worker. A more careful public policy implication that we can draw from this analysis is that we should pay close attention to potential detrimental side effects of new UI rules that may have been created in order to help unemployed workers securing their situation.

Tables and Figures

Figure 3.1: Share of fixed-term contracts of less than one month in total hiring (France, 2000-2017)



SOURCE: DPAE (ACOSS), DSN (DARES).

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

Table 3.1: Sample composition (MMO data)

Separation motive	Number of observations	Number of contracts	Number of firms
End of fixed-term contract	22,208,669	67,075,786	181,162
Personal dismissal	1,417,836	4,335,334	115,134
Economic layoff	340,448	1,426,004	35,984
Quits	3,212,904	13,205,768	166,798
Retirement	820,051	2,126,980	85,982
Pre-retirement	16,440	45,036	5,148
End of trial period	934,844	4,247,370	87,806

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

Table 3.2: Descriptive statistics by work history

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

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

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

Table 3.3: Descriptive statistics by work history - sectors of activity

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

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

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

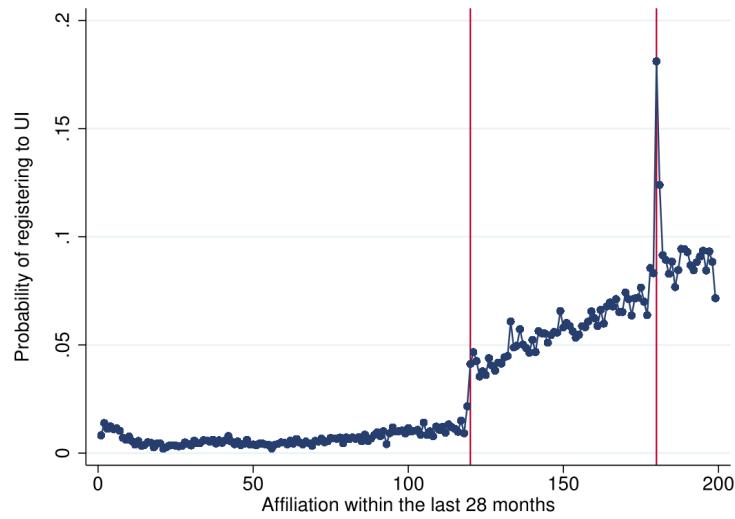
Table 3.4: Descriptive statistics by work history – Occupation type

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

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

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

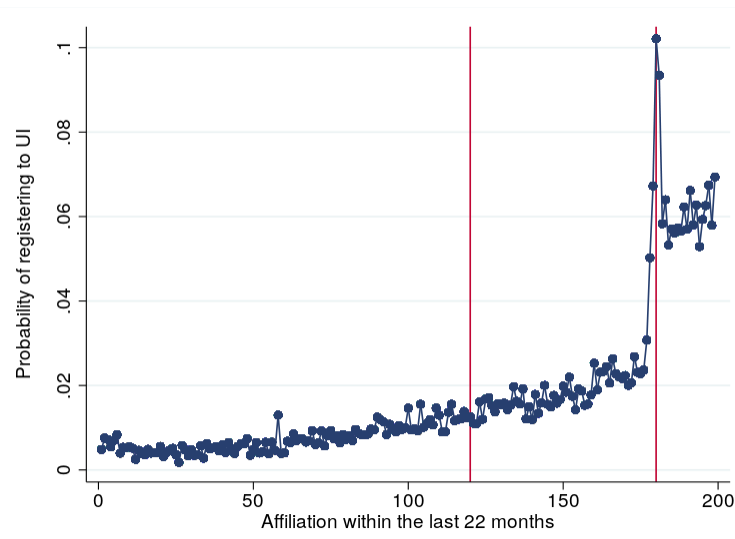
Figure 3.2: Probability of UI registration conditional on separation (Post-reform, 28-months base reference period)



SOURCE: FH-DADS.

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

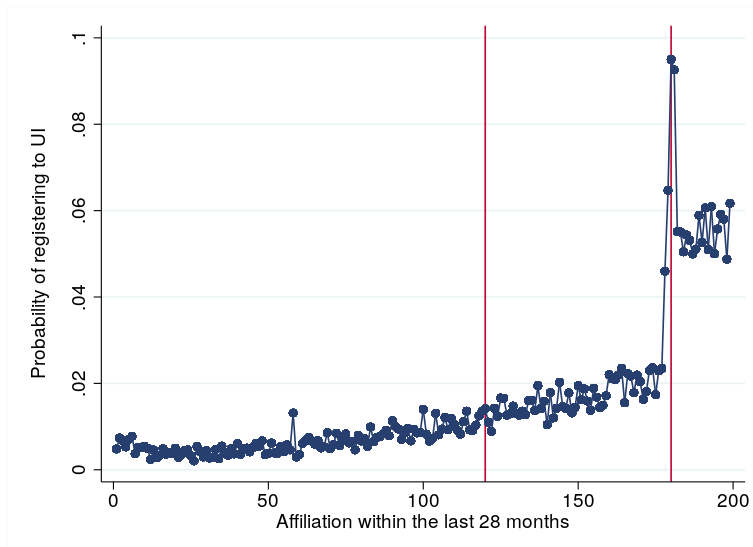
Figure 3.3: Probability of UI registration conditional on separation (Pre-reform, 22-months base reference period)



SOURCE: FH-DADS.

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

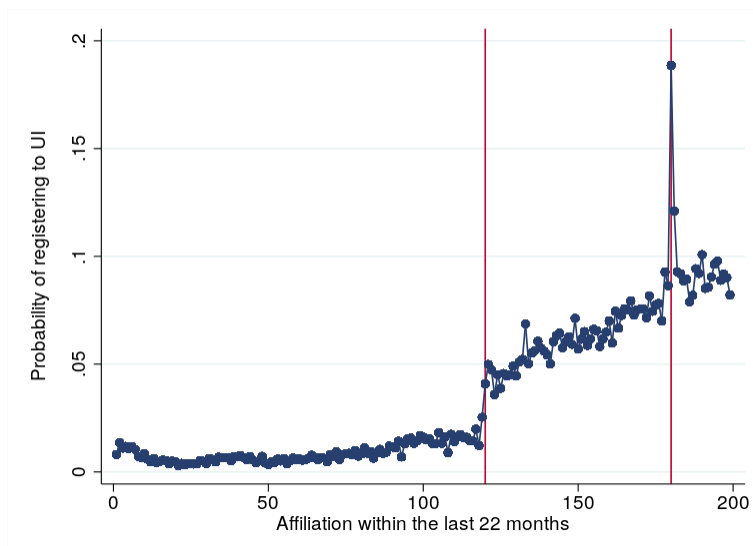
Figure 3.4: Probability of UI registration conditional on separation (Pre-reform, 28-months base reference period)



SOURCE: FH-DADS.

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

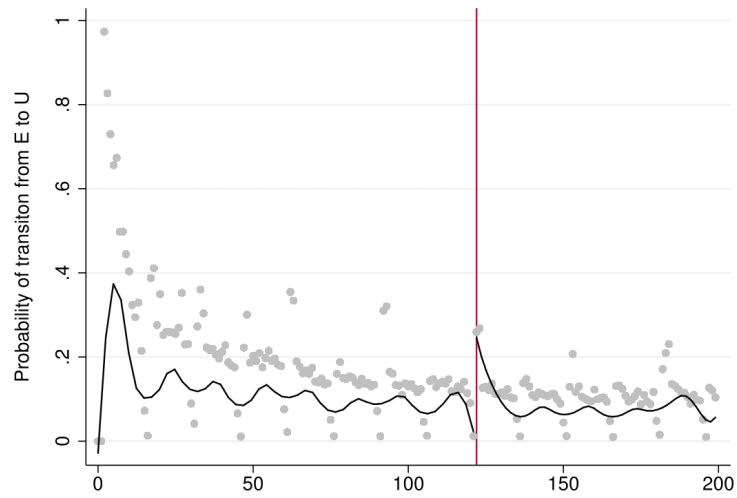
Figure 3.5: Probability of UI registration conditional on separation (Post-reform, 22-months base reference period)



SOURCE: FH-DADS.

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

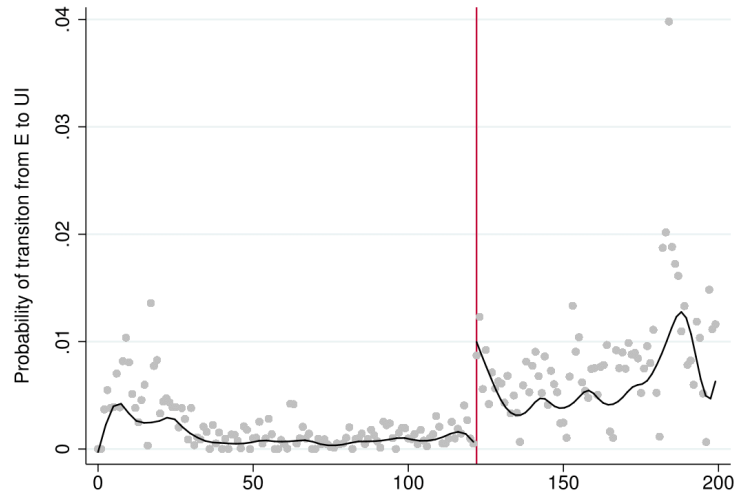
Figure 3.6: Probability to go from employment to non-employment



SOURCE: FH-DADS.

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

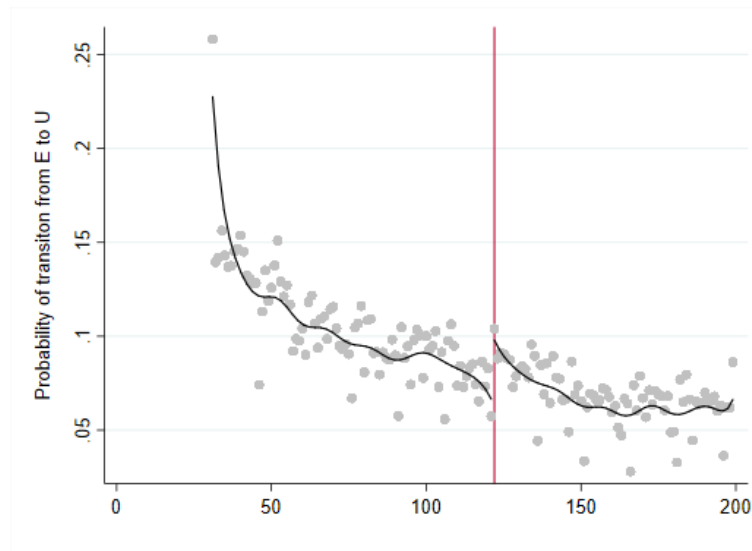
Figure 3.7: Probability to go from employment to registered unemployment



SOURCE: FH-DADS.

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

Figure 3.8: Transition probability from employment to non-employment, restricted to full-time workers



SOURCE: UI data (FNA)

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

Table 3.5: Discontinuity in the transition rate from employment to non-employment on full-time workers

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

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

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

Table 3.6: Discontinuity in the transition rate from employment to registered unemployment

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

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

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

Table 3.7: Difference-in-difference estimate of the number of 4-month contracts relative to 6-month contracts (End of fixed-term contracts)

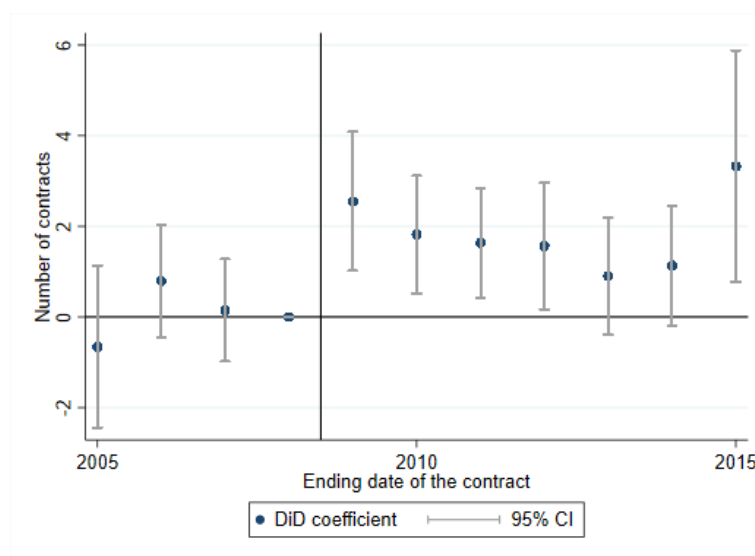
	Within firm monthly number of contracts				
4-month (versus 6m) contracts	7.44499***	4.88344***	3.67946	-0.59904	-0.86041
	(1.104302)	(0.136075)	(2.540706)	(0.420878)	(0.607546)
Post-reform	-0.13257	1.29470***	1.25968	0.48156	0.37090
	(0.281080)	(0.336468)	(0.898371)	(0.567464)	(0.814415)
4-month (versus 6m) contracts × Post-reform	0.49209	0.25898	0.24836	1.53001**	2.38411***
	(1.396799)	(0.161821)	(0.455498)	(0.605193)	(0.912708)
Constant	3.75650***	4.17694***	4.83235***	8.91393***	13.82090***
	(0.256077)	(0.241221)	(1.724860)	(0.433443)	(0.603916)
Observations	549208	549208	549208	517695	352660

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

SOURCE: MMO.

NOTE: The table shows the difference-in-difference estimate of the number of fixed-term 4-month contracts relative to fixed-term 6-month contracts before and after the reform, computed at the firm × month level and aggregated at the national level. The first column displays the raw regression, and month, year, sector and firm fixed-effects are progressively added. The last column includes all fixed-effects, and has been computed on firms observed in both period with at least one 4-month and one 6-month contracts ending in each period. Standard errors are clustered at the sector level for the 3rd specification, and at the firm level for the last two ones.

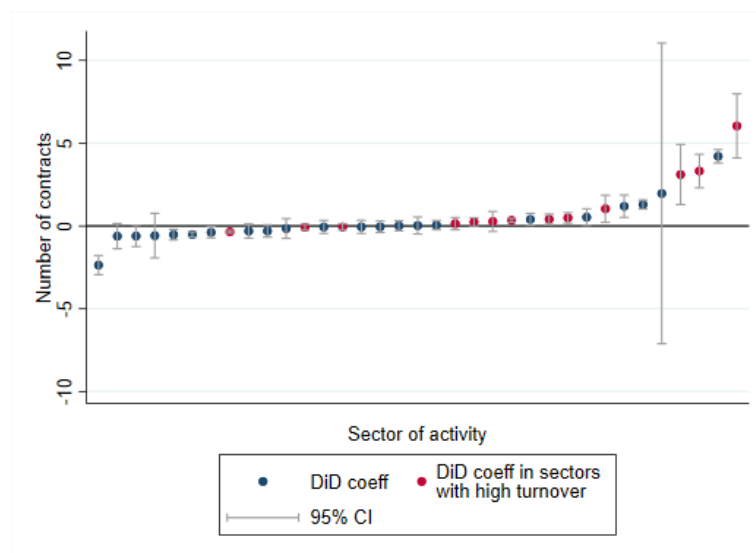
Figure 3.9: Yearly evolution of the number of 4-month contracts relative to 6-month contracts (end of fixed-term contracts)



SOURCE: MMO

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

Figure 3.10: Within sector change in the number of 4-month contracts relative to 6-month contracts (end of fixed-term contracts)



SOURCE: MMO

NOTE: This graph plots the difference-in-difference estimate of the change in the number of fixed-term 4-month contracts relative to fixed-term 6-month contracts by sector. The number of contracts has been computed at the firm \times month level, and then aggregated at the national level, with month, year, and firm fixed-effects. The regression has been run separately in each sector. Standard errors are clustered at the firm level. Sectors in red are the ones identified as having a high separation rate or a high share of very short contracts (DARES, 2018).

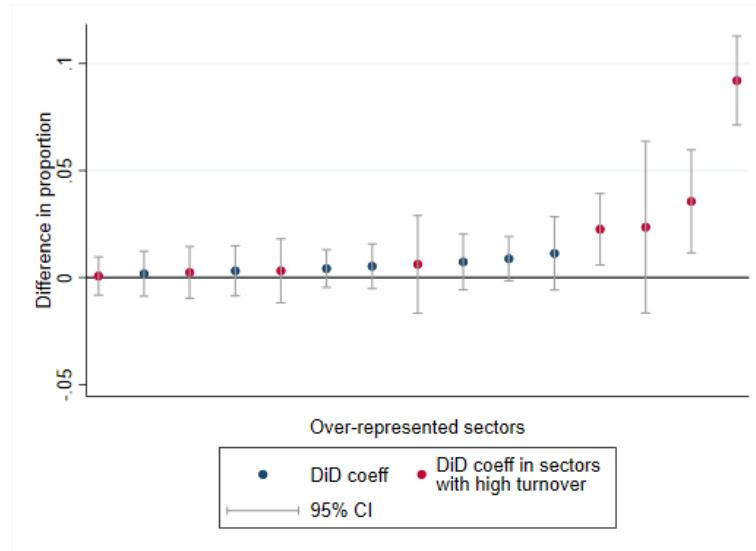
Table 3.8: Sectorial distribution of firms with a high relative increase in 4-month contracts (End of fixed-term contracts)

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

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

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

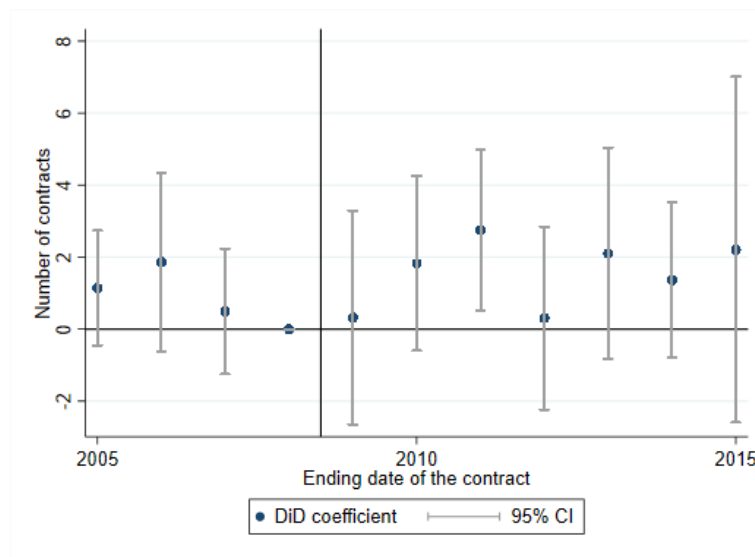
Figure 3.11: Difference in the share of each sector between firms with a high relative increase in 4-month contracts and all other firms (end of fixed-term contracts)



SOURCE: MMO

NOTE: This graph plots the difference in the share of each sector between firms with a high relative increase in 4-month contracts after the reform and all other firms. We keep only the positive points, i.e. sectors that are over-represented among high-increase firms. The relative increase in the number of 4-month contracts is measured by a difference-in-difference regression of the number of fixed-term 4-month contracts relative to fixed-term 6-month contracts before and after the reform, computed within each firm, with sector, month and year fixed-effects. We restricted to firms observed in the pre and post period, with at least one 4-month and one 6-month contract in each period. Having a high relative increase in the number of 4-month contracts is defined as having a within firm difference-in-difference coefficient above the 90th percentile in the distribution of coefficients. Sectors in red are the ones identified as having a high separation rate or a high share of very short contracts (DARES, 2018).

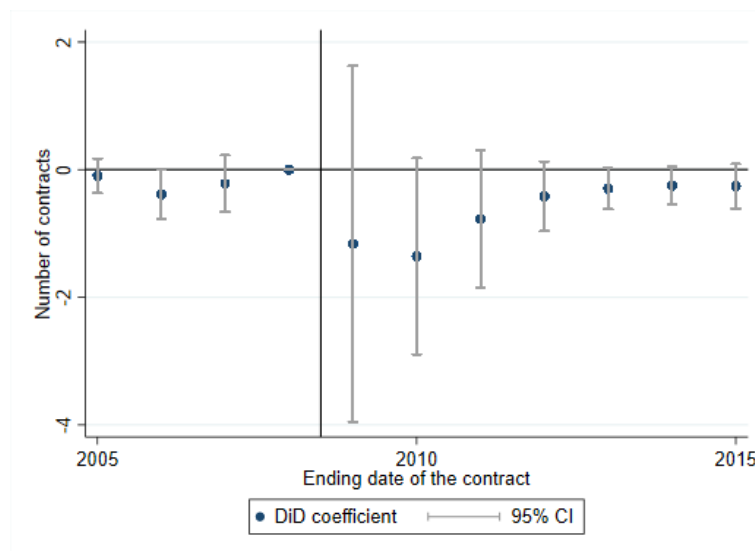
Figure 3.12: Yearly evolution of the number of 4-month contracts relative to 6-month contracts (economic layoffs)



SOURCE: MMO

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

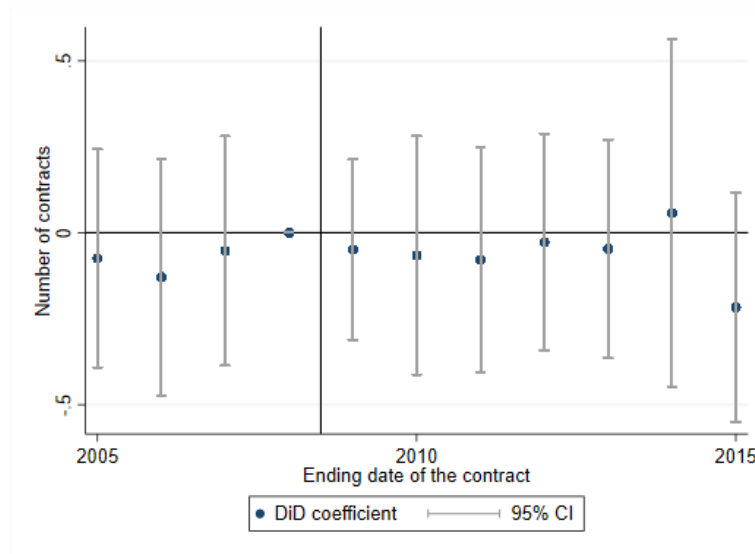
Figure 3.13: Yearly evolution of the number of 4-month contracts relative to 6-month contracts (personal dismissals)



SOURCE: MMO

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

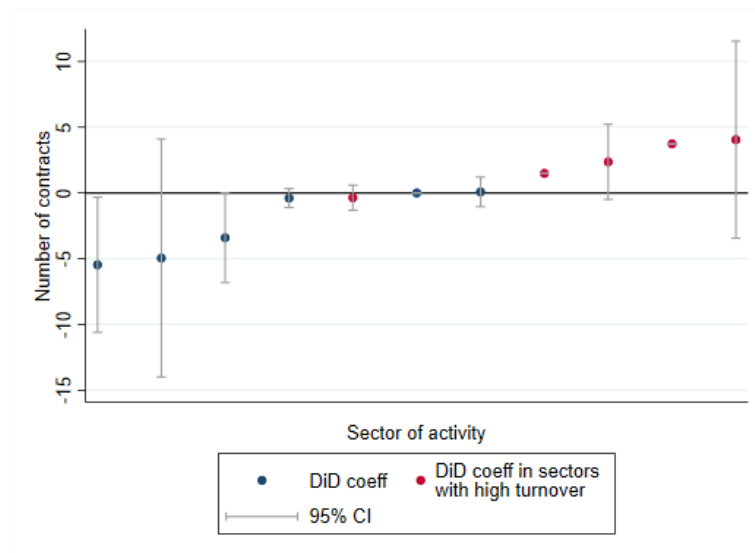
Figure 3.14: Yearly evolution of the number of 4-month contracts relative to 6-month contracts (resignations)



SOURCE: MMO

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

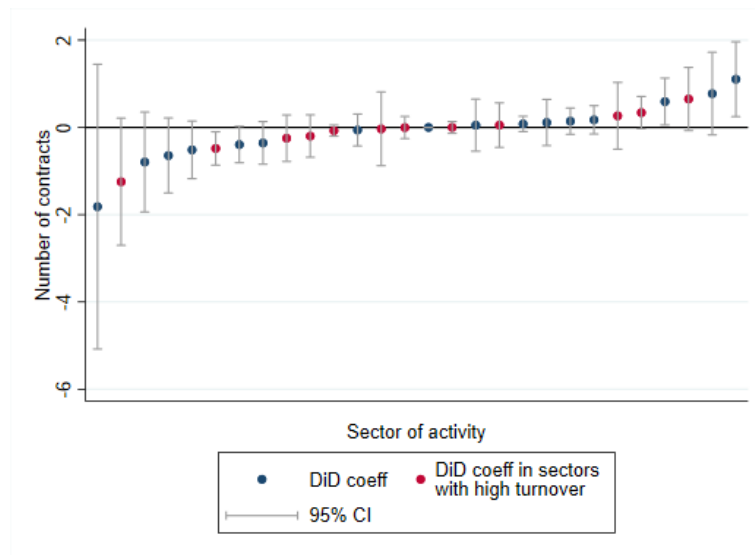
Figure 3.15: Within sector change in the number of 4-month contracts relative to 6-month contracts (economic layoffs)



SOURCE: MMO

NOTE: This graph plots the difference-in-difference estimate of the change in the number of 4-month contracts relative to 6-month contracts ending as an economic layoff by sector. The number of contracts has been computed at the firm \times month level, and then aggregated at the national level, with month, year, sector and firm fixed-effects. The regression has been run separately in each sector. Standard errors are clustered at the firm level. Not all 35 sectors are represented as the number of observations was not sufficient in some sectors. Sectors in red are the ones identified as having a high separation rate or a high share of very short contracts (DARES, 2018).

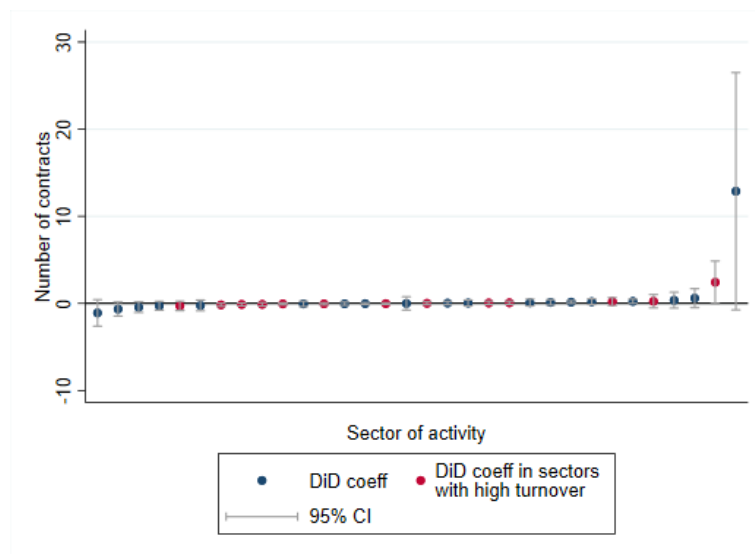
Figure 3.16: Within sector change in the number of 4-month contracts relative to 6-month contracts (personal dismissals)



SOURCE: MMO

NOTE: This graph plots the difference-in-difference estimate of the change in the number of 4-month contracts relative to 6-month contracts ending as a personal dismissal by sector. The number of contracts has been computed at the firm \times month level, and then aggregated at the national level, with month, year, sector and firm fixed-effects. The regression has been run separately in each sector. Standard errors are clustered at the firm level. Sectors in red are the ones identified as having a high separation rate or a high share of very short contracts (DARES, 2018).

Figure 3.17: Within sector change in the number of 4-month contracts relative to 6-month contracts (resignations)



SOURCE: MMO

NOTE: This graph plots the difference-in-difference estimate of the change in the number of 4-month contracts relative to 6-month contracts ending as a resignation by sector. The number of contracts has been computed at the firm \times month level, and then aggregated at the national level, with month, year, sector and firm fixed-effects. The regression has been run separately in each sector. Standard errors are clustered at the firm level. Sectors in red are the ones identified as having a high separation rate or a high share of very short contracts (DARES, 2018).

Table 3.9: Difference-in-difference estimate of the number of 4-month contracts relative to 6-month contracts (Economic layoffs)

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

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

SOURCE: MMO.

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

Table 3.10: Difference-in-difference estimate of the number of 4-month contracts relative to 6-month contracts (Personal dismissals)

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

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

SOURCE: MMO.

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

Table 3.11: Difference-in-difference estimate of the number of 4-month contracts relative to 6-month contracts (End of trial period)

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

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

SOURCE: MMO.

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

Table 3.12: Difference-in-difference estimate of the number of 4-month contracts relative to 6-month contracts (Voluntary resignation)

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

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

SOURCE: MMO.

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

Table 3.13: Difference-in-difference estimate of the number of 4-month contracts relative to 6-month contracts (Retirement)

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

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

SOURCE: MMO.

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

Table 3.14: Difference-in-difference estimate of the number of 4-month contracts relative to 6-month contracts (Pre-retirement)

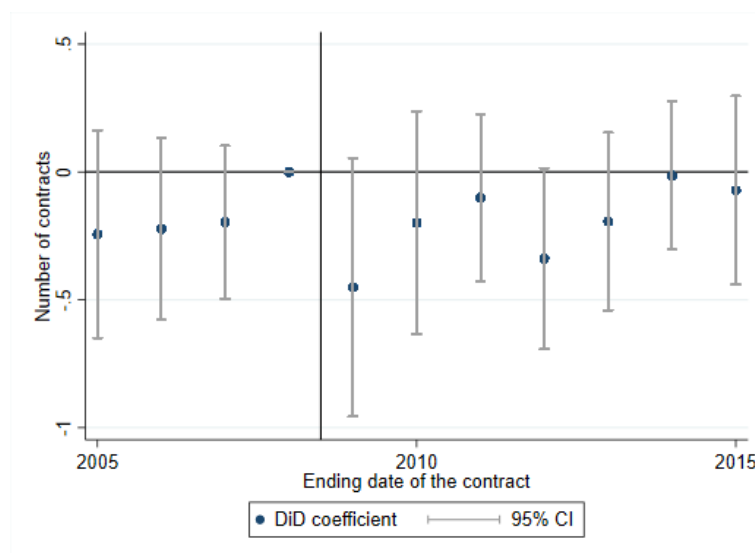
	Within firm monthly number of contracts		
4-month (versus 6m) contracts	3.09464* (1.684594)	1.45730*** (0.525373)	-0.24507 (0.435111)
Post-reform	-0.14827 (0.228724)	-0.87115 (1.218646)	-0.85619* (0.429935)
4-month (versus 6m) contracts × Post-reform	-1.57978 (1.966535)	-1.08176* (0.574451)	0.87408**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

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

SOURCE: MMO.

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

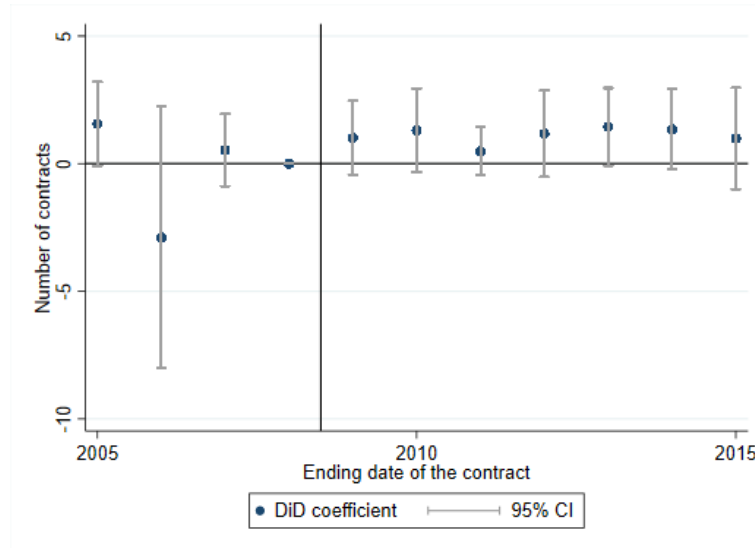
Figure 3.18: Yearly evolution of the number of 4-month contracts relative to 6-month contracts (end of trial period)



SOURCE: MMO

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

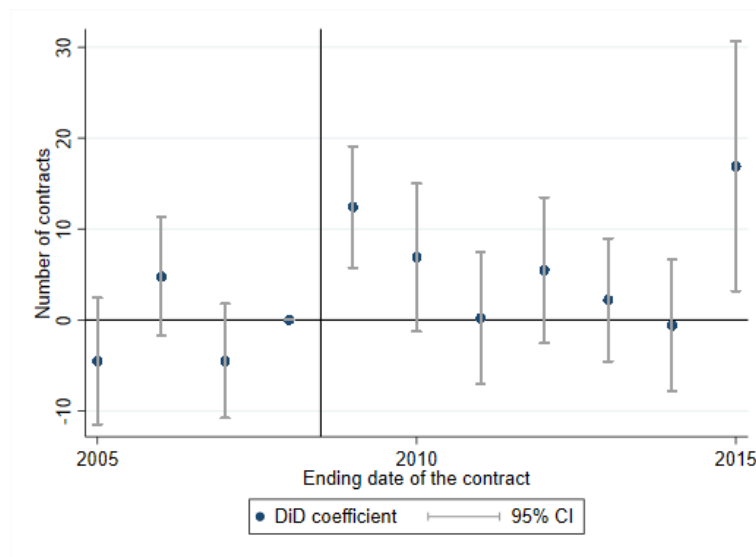
Figure 3.19: Yearly evolution of the number of 4-month fixed-term contracts relative to 6-month fixed contracts for legal, accounting, management, architecture, engineering, technical testing and analysis activities



SOURCE: MMO

NOTE: This graph plots the yearly evolution in the number of fixed-term 4-month contracts relative to fixed-term 6-month contracts for legal, accounting, management, architecture, engineering, technical testing and analysis activities. The regression includes month, year and firm fixed-effects. The reference year is 2008, the last pre-reform year. The number of contracts has been computed at the firm \times month level, and then aggregated at the sectorial level. Standard errors are clustered at the firm level.

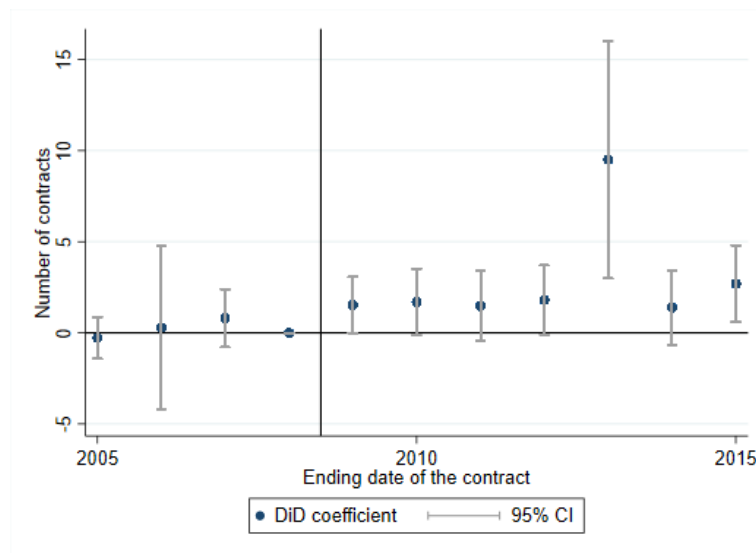
Figure 3.20: Yearly evolution of the number of 4-month fixed-term contracts relative to 6-month fixed contracts for transportation activities



SOURCE: MMO

NOTE: This graph plots the yearly evolution in the number of fixed-term 4-month contracts relative to fixed-term 6-month contracts for transportation activities. The regression includes month, year and firm fixed-effects. The reference year is 2008, the last pre-reform year. The number of contracts has been computed at the firm \times month level, and then aggregated at the sectorial level. Standard errors are clustered at the firm level.

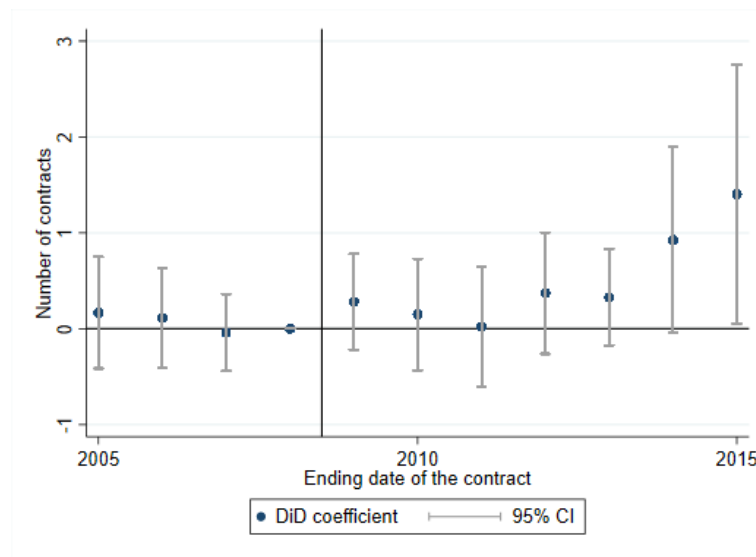
Figure 3.21: Yearly evolution of the number of 4-month fixed-term contracts relative to 6-month fixed contracts for real estate activities



SOURCE: MMO

NOTE: This graph plots the yearly evolution in the number of fixed-term 4-month contracts relative to fixed-term 6-month contracts for real estate activities. The regression includes month, year and firm fixed-effects. The reference year is 2008, the last pre-reform year. The number of contracts has been computed at the firm \times month level, and then aggregated at the sectorial level. Standard errors are clustered at the firm level.

Figure 3.22: Yearly evolution of the number of 4-month fixed-term contracts relative to 6-month fixed contracts for human health activities



SOURCE: MMO

NOTE: This graph plots the yearly evolution in the number of fixed-term 4-month contracts relative to fixed-term 6-month contracts for human health activities. The regression includes month, year and firm fixed-effects. The reference year is 2008, the last pre-reform year. The number of contracts has been computed at the firm \times month level, and then aggregated at the sectorial level. Standard errors are clustered at the firm level.

Table 3.15: Post-reform evolution of 4 to 5-month contracts (6-month and 1 to 3-month control group)

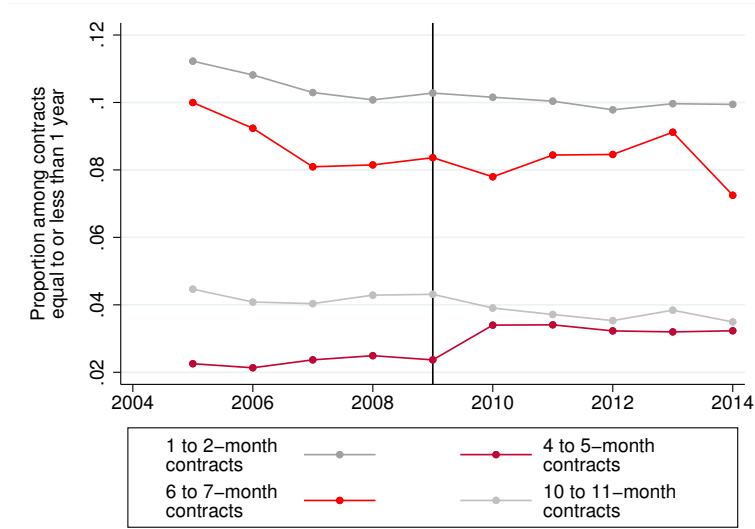
	Proportion among contracts ≤ 1 year	Proportion among contracts ≤ 1 year
Post-reform	-0.00515*** (0.0000395)	-0.00500*** (0.0000549)
4 to 5-month contracts	-0.06441*** (0.0000649)	-0.06436*** (0.0001418)
Post-reform \times 4 to 5-month contracts	0.01470*** (0.0000811)	0.01451*** (0.0001751)
Constant	0.08778***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

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

SOURCE: UI data (FNA). The data comes from UI records, and gathers all contracts that were immediately followed by a UI right. To keep the same sample definition throughout the years, we excluded workers who registered with a work history between four and six months as they would not be registered in UI data before the reform.

NOTE: The first specification shows the difference in the evolution of the shares of 4-month and 6-month contracts. The second specification compares 4-month contracts to contracts between 1 and 3 months. Shares have been computed among contracts of one year and less. All types of separation are considered.

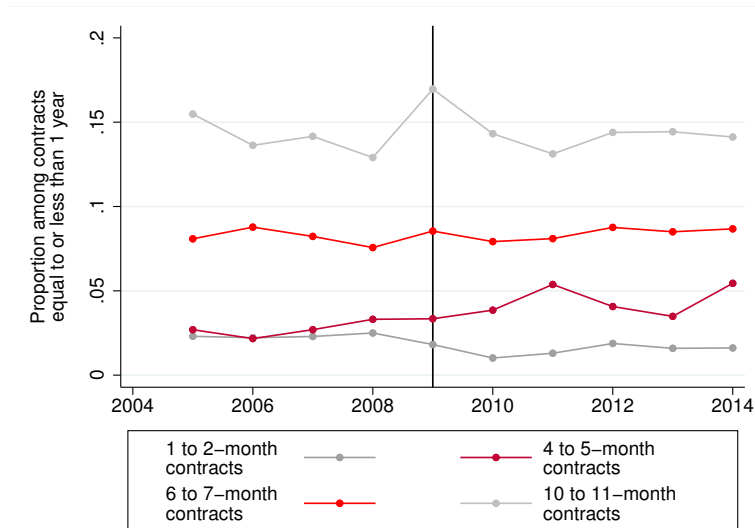
Figure 3.23: Share of contracts among contracts of 1 year or less (all separations)



SOURCE: UI data (FNA).

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

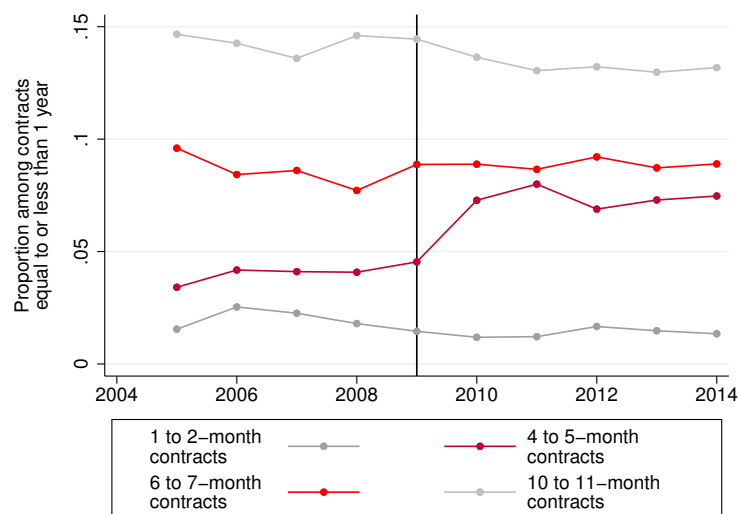
Figure 3.24: Share of contracts among contracts of 1 year or less (economic layoffs)



SOURCE: UI data (FNA).

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

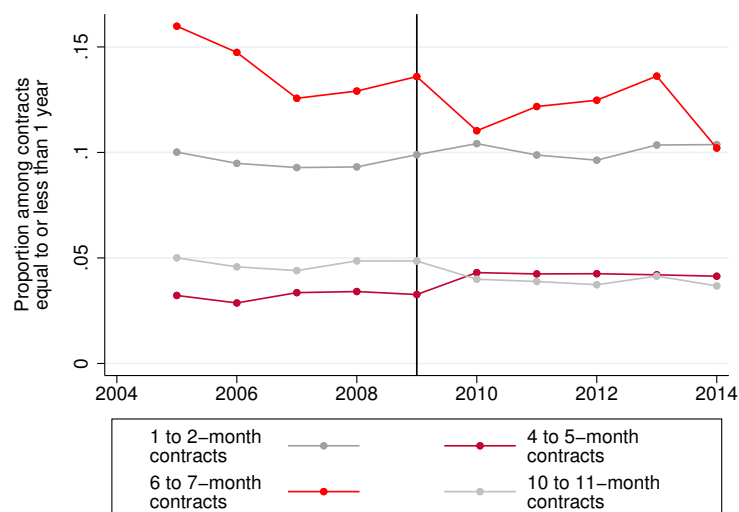
Figure 3.25: Share of contracts among contracts of 1 year or less (other dismissals)



SOURCE: UI data (FNA).

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

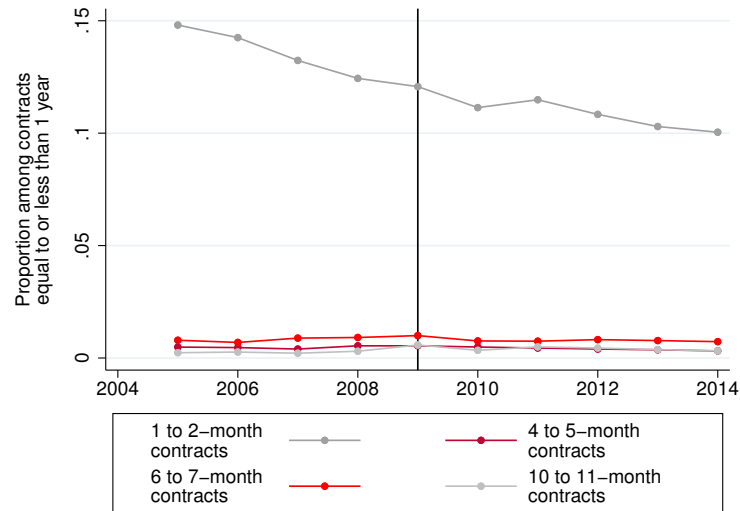
Figure 3.26: Share of contracts among contracts of 1 year or less (end of fixed-term contract)



SOURCE: UI data (FNA).

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

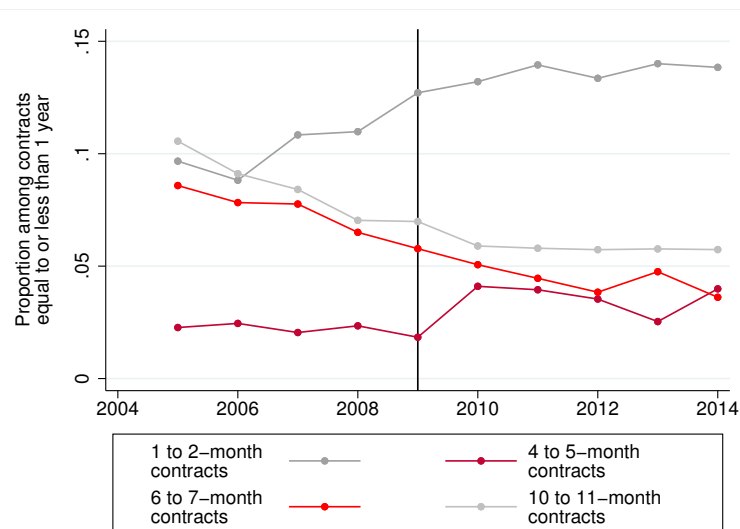
Figure 3.27: Share of contracts among contracts of 1 year or less (end of temporary job)



SOURCE: UI data (FNA).

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

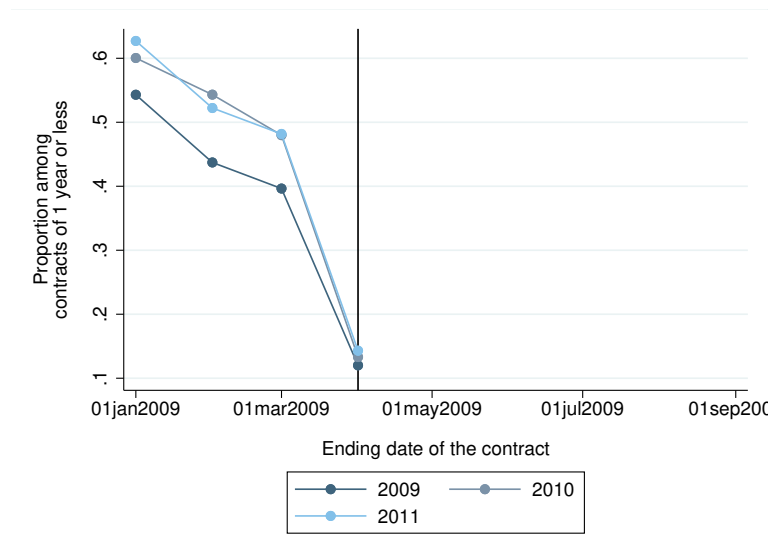
Figure 3.28: Share of contracts among contracts of 1 year or less (voluntary resignations)



SOURCE: UI data (FNA).

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

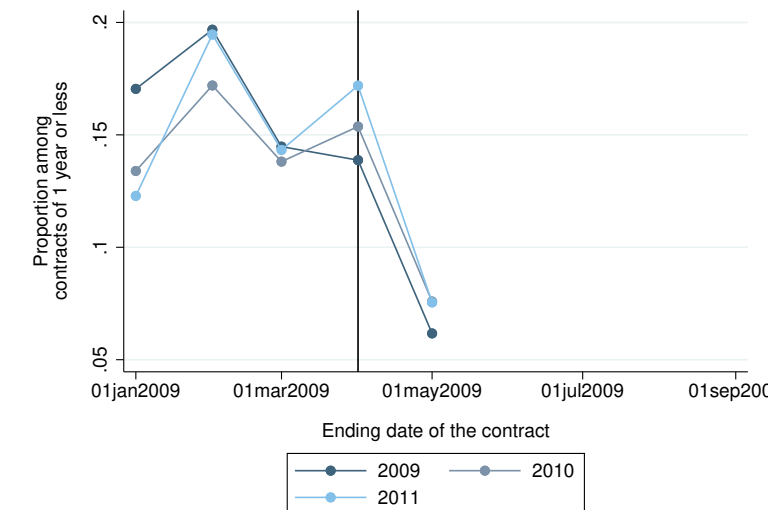
Figure 3.29: Share of 1 to 30-day contracts among contracts of 1 year or less



SOURCE: UI data (FNA).

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

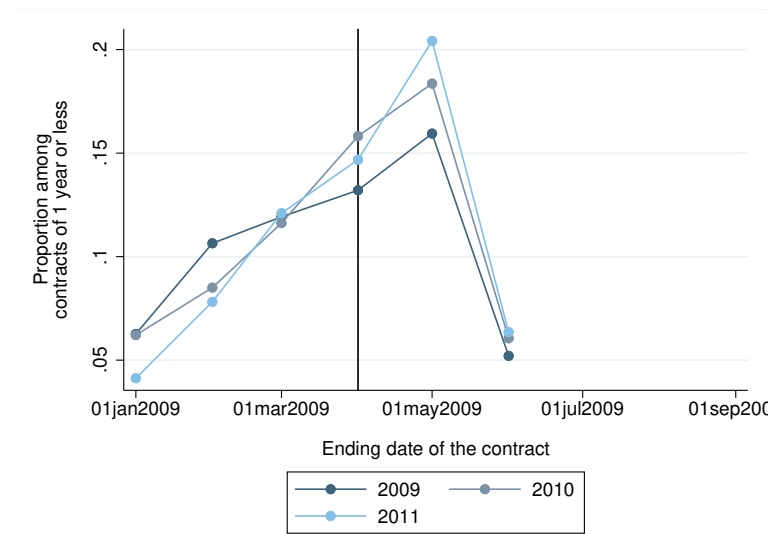
Figure 3.30: Share of 31 to 60-day contracts among contracts of 1 year or less



SOURCE: UI data (FNA).

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

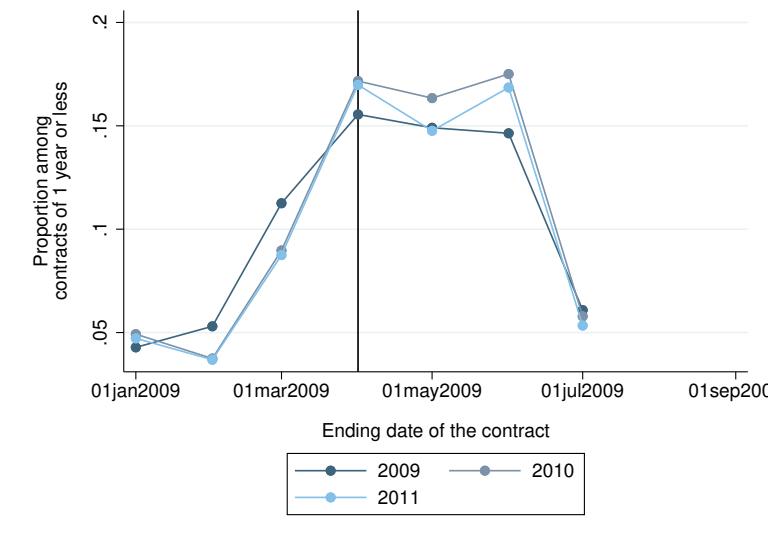
Figure 3.31: Share of 61 to 90-day contracts among contracts of 1 year or less



SOURCE: UI data (FNA).

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

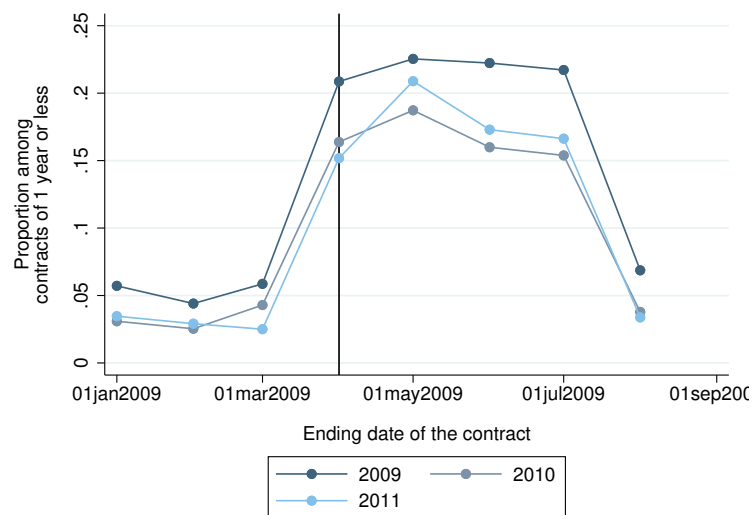
Figure 3.32: Share of 91 to 120-day contracts among contracts of 1 year or less



SOURCE: UI data (FNA).

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

Figure 3.33: Share of 121 to 150-days contracts among contracts of 1 year or less



SOURCE: UI data (FNA).

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

Table 3.16: Descriptive statistics on treated and control workers

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

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

SOURCE: FH-DADS.

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

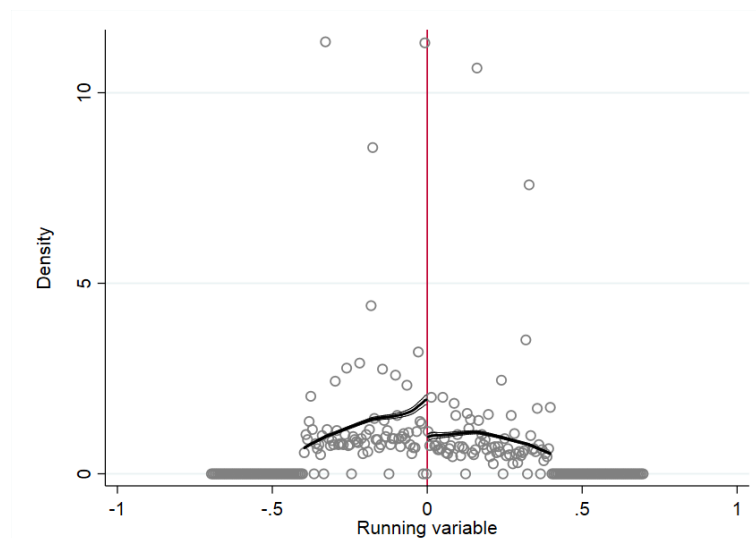
Table 3.17: Descriptive statistics on takers and non-takers

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

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

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

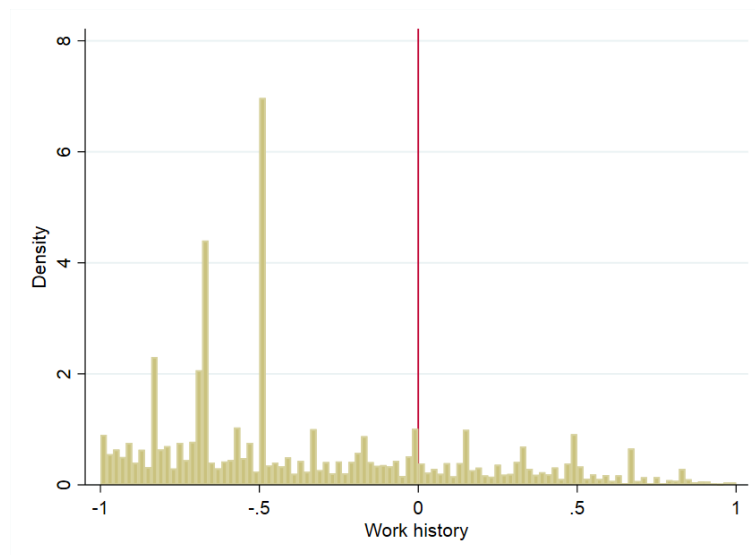
Figure 3.34: Mc Crary test contract ending date distribution



SOURCE: FH-DADS.

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

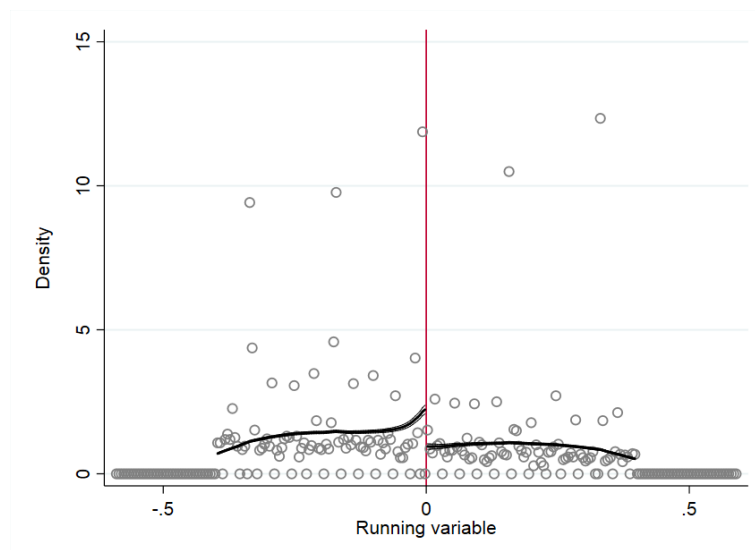
Figure 3.35: Histogram of the contract ending date frequencies



SOURCE: FH-DADS.

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

Figure 3.36: Mc Crary test contract ending date distribution one year before the reform (2008)



SOURCE: FH-DADS.

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

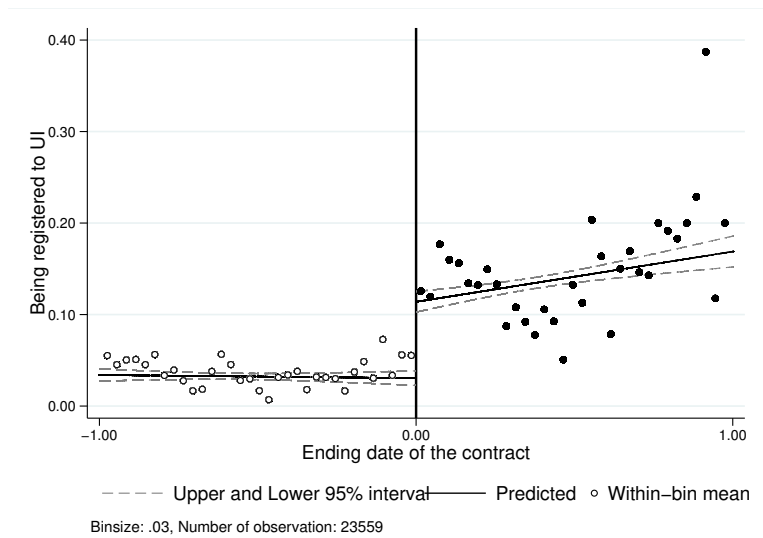
Table 3.18: Impact of separating after the reform on UI
takeup

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

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

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

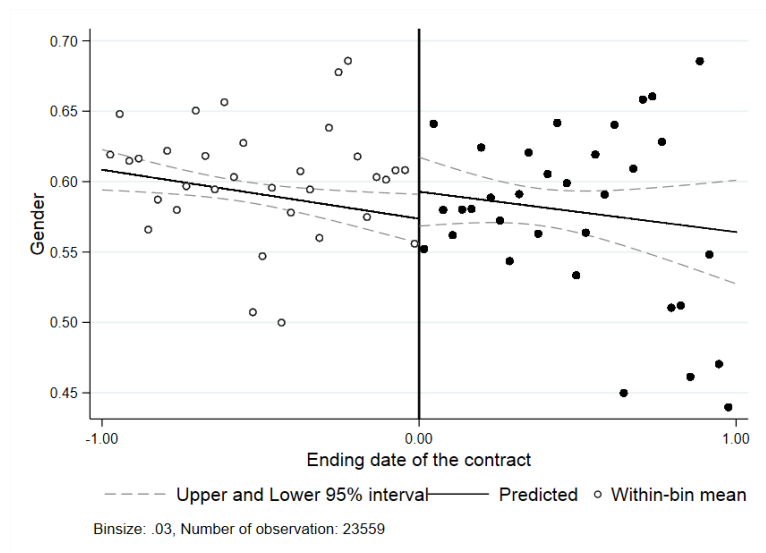
Figure 3.37: Probability of opening a UI right



SOURCE: FH-DADS.

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

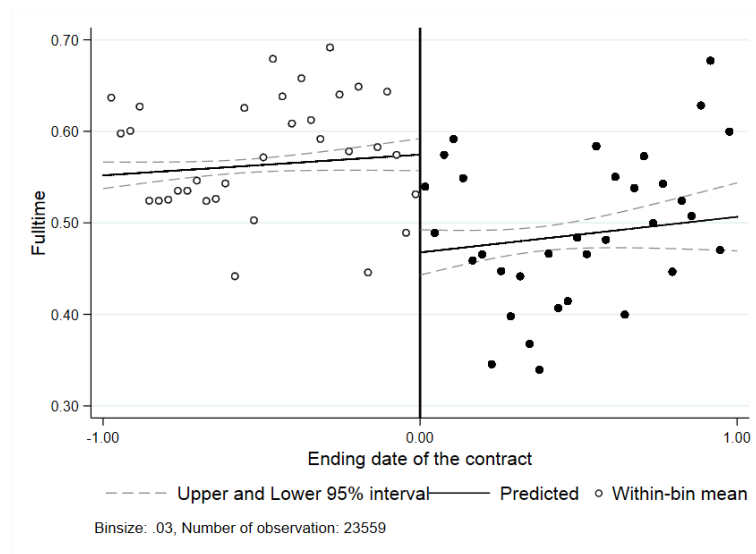
Figure 3.38: Distribution of the female proportion with respect to contract ending date



SOURCE: FH-DADS.

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

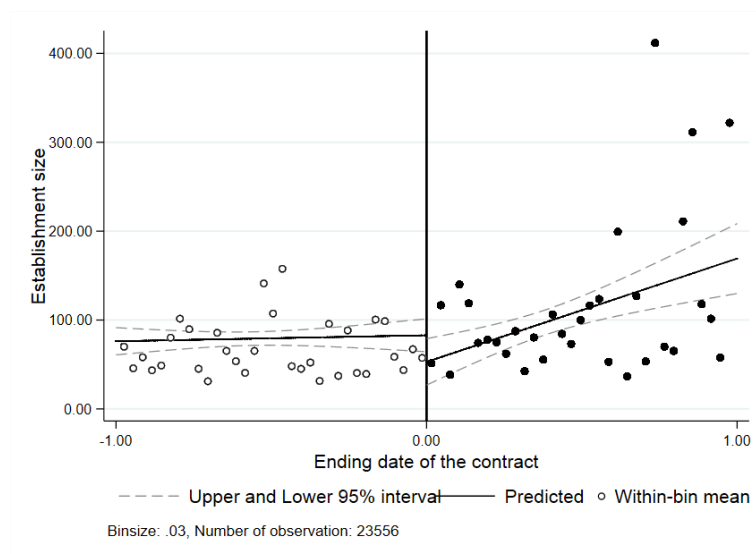
Figure 3.39: Distribution of the proportion of fulltime workers with respect to contract ending date



SOURCE: FH-DADS.

NOTE: This figure shows the distribution of the proportion of full-time workers with respect to the contract ending date. The vertical line corresponds to the reform. There is a discontinuity at the threshold, but it seems that it is rather driven by the non-linearity of the relationship between the proportion of full-time workers and the ending date of the contract. Table 3.19 shows that the more demanding RDD regression on the full-time variable does not yield a significant coefficient, confirming that workers are statistically similar at both sides of the threshold.

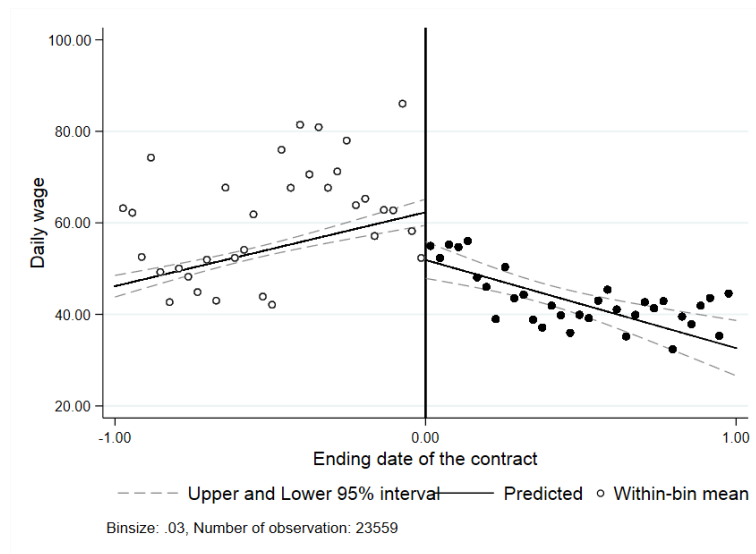
Figure 3.40: Distribution of the establishment size with respect to contract ending date



SOURCE: FH-DADS.

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

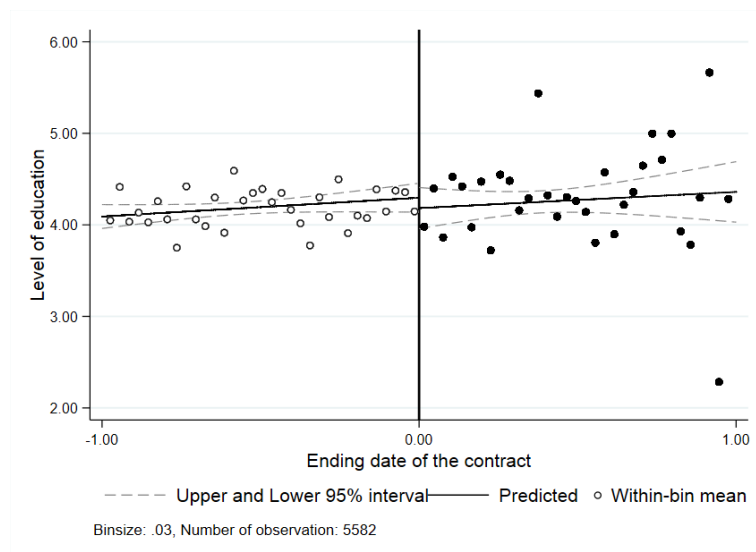
Figure 3.41: Distribution of the daily wage with respect to contract ending date



SOURCE: FH-DADS.

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

Figure 3.42: Distribution of the education level with respect to contract ending date



SOURCE: FH-DADS.

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

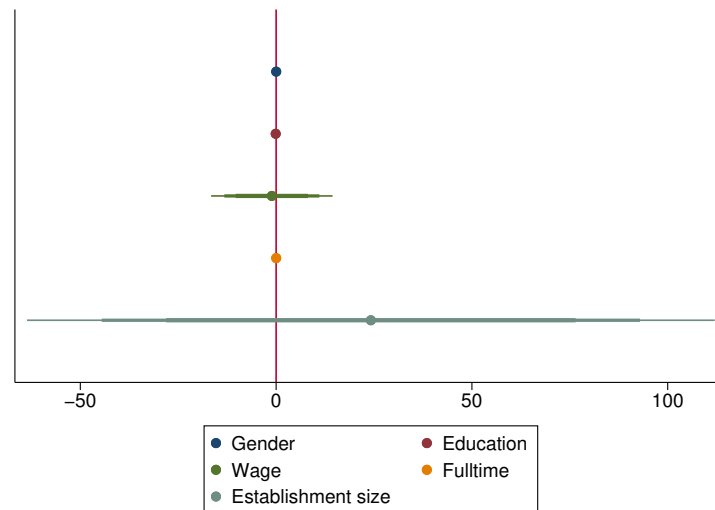
Table 3.19: Check of no discontinuity in the distribution of covariates

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

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

NOTE: This table reports the regression discontinuity estimates, using each covariate as the dependent variable, to test the assumption of continuity of the covariates distribution at the threshold. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification.

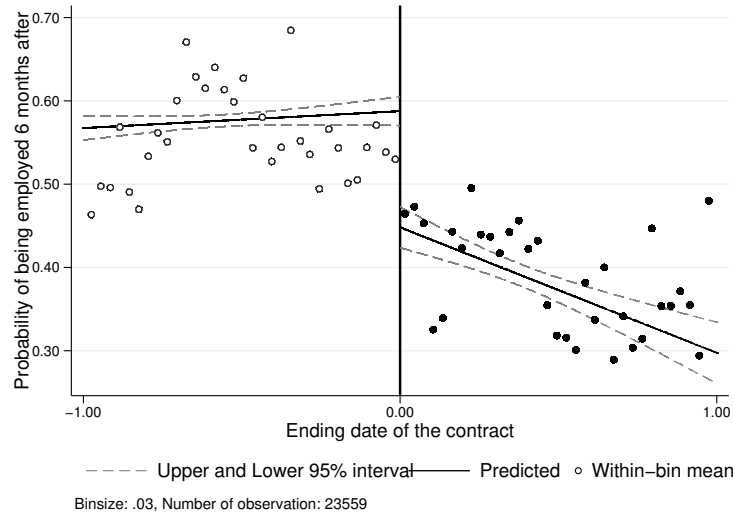
Figure 3.43: Magnitude of the difference in covariates at the cutoff



SOURCE: FH-DADS.

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

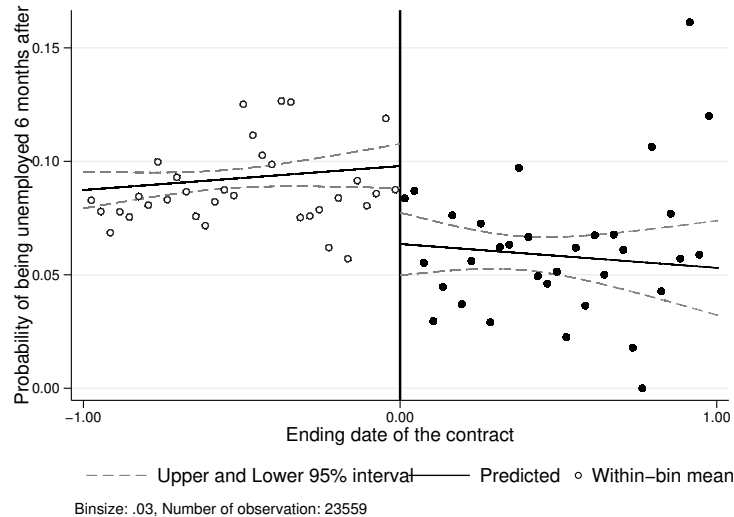
Figure 3.44: Impact of UI eligibility on employment probability 6 months after



SOURCE: FH-DADS.

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

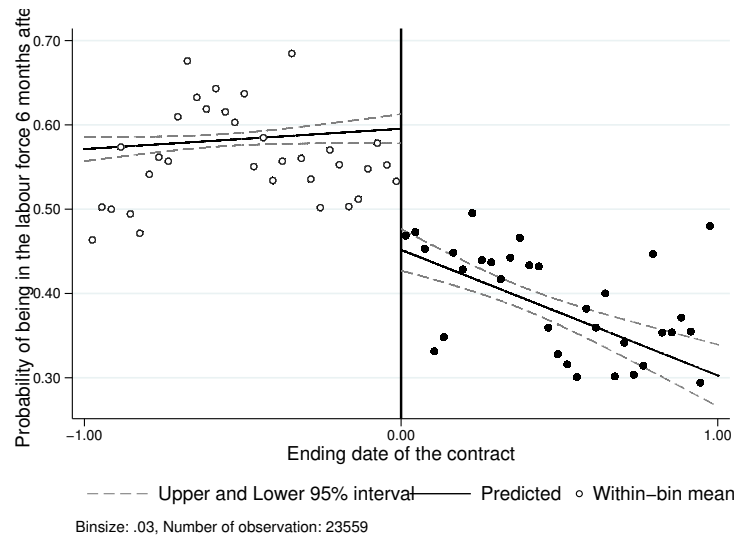
Figure 3.45: Impact of UI eligibility on unemployment probability 6 months after



SOURCE: FH-DADS.

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

Figure 3.46: Impact of UI eligibility on labour market participation probability 6 months after



SOURCE: FH-DADS.

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

Table 3.20: Impact of UI benefits receipt on employment probability (1-4 months)

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

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

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

Table 3.21: Impact of UI benefits receipt on employment probability (5-8 months)

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

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

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

Table 3.22: Impact of UI benefits receipt on employment probability (9-12 months)

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

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

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

Table 3.23: Impact of UI benefits receipt on employment probability (13-16 months)

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

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

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

Table 3.24: Impact of UI benefits receipt on employment probability (17-20 months)

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

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

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

Table 3.25: Impact of UI benefits receipt on employment probability (21-24 months)

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

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

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

Table 3.26: Impact of UI benefits receipt on employment probability (1-4 months)

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

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

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

Table 3.27: Impact of UI benefits receipt on employment probability (5-8 months)

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

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

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

Table 3.28: Impact of UI benefits receipt on employment probability (9-12 months)

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

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

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

Table 3.29: Impact of UI benefits receipt on employment probability (13-16 months)

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

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

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

Table 3.30: Impact of UI benefits receipt on employment probability (17-20 months)

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

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

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

Table 3.31: Impact of UI benefits receipt on employment probability (21-24 months)

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

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

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

Table 3.32: Impact of UI benefits receipt on the duration to next job

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

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

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

Table 3.33: Impact of UI benefits receipt on unemployment probability (1-4 months)

	Probability of being unemployed 1 months after	Probability of being unemployed 2 months after	Probability of being unemployed 3 months after	Probability of being unemployed 4 months after
RD_Estimate	-0.141 (0.187)	-0.206 (0.215)	-0.058 (0.251)	-0.274 (0.251)
Observations	23559	23559	23559	23559

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

NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of being unemployed, i.e. registered for UI, at different time horizons after the end of the contract that defines the treatment status.

Table 3.34: Impact of UI benefits receipt on unemployment probability (5-8 months)

	Probability of being unemployed 5 months after	Probability of being unemployed 6 months after	Probability of being unemployed 7 months after	Probability of being unemployed 8 months after
RD_Estimate	-0.136 (0.249)	-0.220 (0.243)	-0.197 (0.225)	-0.028 (0.223)
Observations	23559	23559	23559	23559

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

NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of being unemployed, i.e. registered for UI, at different time horizons after the end of the contract that defines the treatment status.

Table 3.35: Impact of UI benefits receipt on unemployment probability (9-12 months)

	Probability of being unemployed 9 months after	Probability of being unemployed 10 months after	Probability of being unemployed 11 months after	Probability of being unemployed 12 months after
RD_Estimate	-0.017 (0.227)	0.094 (0.222)	0.003 (0.223)	-0.109 (0.217)
Observations	23559	23559	23559	23559

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

NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of being unemployed, i.e. registered for UI, at different time horizons after the end of the contract that defines the treatment status.

Table 3.36: Impact of UI benefits receipt on unemployment probability (13-16 months)

	Probability of being unemployed 13 months after	Probability of being unemployed 14 months after	Probability of being unemployed 15 months after	Probability of being unemployed 16 months after
RD_Estimate	0.193 (0.248)	-0.066 (0.239)	0.172 (0.250)	0.012 (0.244)
Observations	23559	23559	23559	23559

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

NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of being unemployed, i.e. registered for UI, at different time horizons after the end of the contract that defines the treatment status.

Table 3.37: Impact of UI benefits receipt on unemployment probability (17-20 months)

	Probability of being unemployed 17 months after	Probability of being unemployed 18 months after	Probability of being unemployed 19 months after	Probability of being unemployed 20 months after
RD_Estimate	0.108 (0.248)	0.053 (0.234)	-0.003 (0.214)	0.147 (0.211)
Observations	23559	23559	23559	23559

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

NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of being unemployed, i.e. registered for UI, at different time horizons after the end of the contract that defines the treatment status.

Table 3.38: Impact of UI benefits receipt on unemployment probability (21-24 months)

	Probability of being unemployed 21 months after	Probability of being unemployed 22 months after	Probability of being unemployed 23 months after	Probability of being unemployed 24 months after
RD_Estimate	-0.096 (0.213)	0.148 (0.202)	-0.002 (0.203)	-0.039 (0.210)
Observations	23559	23559	23559	23559

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

NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of being unemployed, i.e. registered for UI, at different time horizons after the end of the contract that defines the treatment status.

Table 3.39: Impact of UI benefits receipt on labour market participation probability (1-4 months)

	Probability of being in the labour force 1 months after	Probability of being in the labour force 2 months after	Probability of being in the labour force 3 months after	Probability of being in the labour force 4 months after
RD_Estimate	0.197 (0.606)	0.030 (0.493)	-0.540 (0.428)	-0.945* (0.494)
Observations	23559	23559	23559	23559

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

NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of being part of the labour force at different time horizons after the end of the contract that defines the treatment status.

Table 3.40: Impact of UI benefits receipt on labour market participation probability (5-8 months)

	Probability of being in the labour force 5 months after	Probability of being in the labour force 6 months after	Probability of being in the labour force 7 months after	Probability of being in the labour force 8 months after
RD_Estimate	-0.863* (0.494)	-1.118** (0.500)	-0.996** (0.469)	-1.104*** (0.292)
Observations	23559	23559	23559	23559

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

NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of being part of the labour force at different time horizons after the end of the contract that defines the treatment status.

Table 3.41: Impact of UI benefits receipt on labour market participation probability (9-12 months)

	Probability of being in the labour force 9 months after	Probability of being in the labour force 10 months after	Probability of being in the labour force 11 months after	Probability of being in the labour force 12 months after
RD_Estimate	-0.402 (0.410)	-0.908** (0.459)	-0.928** (0.450)	-0.670 (0.438)
Observations	23559	23559	23559	23559

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

NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of being part of the labour force at different time horizons after the end of the contract that defines the treatment status.

Table 3.42: Impact of UI benefits receipt on labour market participation probability (13-16 months)

	Probability of being in the labour force 13 months after	Probability of being in the labour force 14 months after	Probability of being in the labour force 15 months after	Probability of being in the labour force 16 months after
RD_Estimate	-0.576 (0.447)	-0.725 (0.444)	-0.668 (0.433)	-0.745* (0.435)
Observations	23559	23559	23559	23559

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

NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of being part of the labour force at different time horizons after the end of the contract that defines the treatment status.

Table 3.43: Impact of UI benefits receipt on labour market participation probability (17-20 months)

	Probability of being in the labour force 17 months after	Probability of being in the labour force 18 months after	Probability of being in the labour force 19 months after	Probability of being in the labour force 20 months after
RD_Estimate	-0.717 (0.450)	-1.027** (0.483)	-0.945** (0.449)	-1.009** (0.474)
Observations	23559	23559	23559	23559

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

NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of being part of the labour force at different time horizons after the end of the contract that defines the treatment status.

Table 3.44: Impact of UI benefits receipt on labour market participation probability (21-24 months)

	Probability of being in the labour force 21 months after	Probability of being in the labour force 22 months after	Probability of being in the labour force 23 months after	Probability of being in the labour force 24 months after
RD_Estimate	-1.012*** (0.390)	0.066 (0.433)	0.002 (0.423)	0.042 (0.423)
Observations	23559	23559	23559	23559

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

NOTE: This table reports the regressions discontinuity estimates of the impact of UI benefits receipt. The bandwidth has been computed using the MSE optimal bandwidth selector. Linear specification. The dependent variable corresponds to the probability of being part of the labour force at different time horizons after the end of the contract that defines the treatment status.

Table 3.45: Impact of UI benefits receipt on daily earnings (1-4 months)

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

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

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

Table 3.46: Impact of UI benefits receipt on daily earnings (5-8 months)

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

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

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

Table 3.47: Impact of UI benefits receipt on daily earnings (9-12 months)

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

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

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

Table 3.48: Impact of UI benefit receipt on cumulative earnings over two years

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

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

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

Table 3.49: Impact of UI benefit receipt on the probability to have a permanent contract

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

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

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

Table 3.50: Impact of UI benefit receipt on the probability to work full-time

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

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

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

Table 3.51: Impact of UI benefit receipt on the probability to work in the same 2-digit industry

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

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

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

Table 3.52: Impact of UI benefit receipt on the duration of the contract

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

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

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

Table 3.53: Impact of UI benefits receipt on probability of having a permanent contract (1-4 months)

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

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

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

Table 3.54: Impact of UI benefits receipt on probability of having a permanent contract (5-8 months)

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

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

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

Table 3.55: Impact of UI benefits receipt on probability of having a permanent contract (9-12 months)

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

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

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

Table 3.56: Impact of UI benefits receipt on probability of having a permanent contract (13-16 months)

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

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

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

Table 3.57: Impact of UI benefits receipt on probability of having a permanent contract (17-20 months)

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

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

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

Table 3.58: Impact of UI benefits receipt on probability of having a permanent contract (21-24 months)

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

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

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

Table 3.59: Impact of UI benefits receipt on the probability of working in the same 2-digit industry (1-4 months)

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

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

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

Table 3.60: Impact of UI benefits receipt on the probability of working in the same 2-digit industry (5-8 months)

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

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

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

Table 3.61: Impact of UI benefits receipt on the probability of working in the same 2-digit industry (9-12 months)

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

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

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

Table 3.62: Impact of UI benefits receipt on the probability of working in the same 2-digit industry (13-16 months)

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

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

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

Table 3.63: Impact of UI benefits receipt on the probability of working in the same 2-digit industry (17-20 months)

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

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

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

Table 3.64: Impact of UI benefits receipt on the probability of working in the same 2-digit industry (21-24 months)

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

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

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

Table 3.65: Impact of UI benefits receipt on the probability of working full-time (1-4 months)

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

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

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

Table 3.66: Impact of UI benefits receipt on the probability of working full-time (5-8 months)

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

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

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

Table 3.67: Impact of UI benefits receipt on the probability of working full-time (9-12 months)

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

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

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

Table 3.68: Impact of UI benefits receipt on the probability of working full-time (13-16 months)

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

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

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

Table 3.69: Impact of UI benefits receipt on the probability of working full-time (17-20 months)

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

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

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

Table 3.70: Impact of UI benefits receipt on the probability of working full-time (21-24 months)

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

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

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

Table 3.71: Impact of UI benefits receipt on contract duration (1-4 months)

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

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

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

Table 3.72: Impact of UI benefits receipt on contract duration (5-8 months)

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

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

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

Table 3.73: Impact of UI benefits receipt on contract duration (9-12 months)

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

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

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

Table 3.74: Impact of UI benefits receipt on contract duration (13-16 months)

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

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

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

Table 3.75: Impact of UI benefits receipt on contract duration (17-20 months)

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

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

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

Table 3.76: Impact of UI benefits receipt on contract duration (21-24 months)

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

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

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

Table 3.77: Bounds on the treatment effect on the probability of having a permanent contract (1-4 months)

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

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

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

Table 3.78: Bounds on the treatment effect on the probability of having a permanent contract (5-8 months)

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

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

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

Table 3.79: Bounds on the treatment effect on the probability of having a permanent contract (9-12 months)

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

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

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

Table 3.80: Bounds on the treatment effect on the probability of having a permanent contract (13-16 months)

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

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

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

Table 3.81: Bounds on the treatment effect on the probability of having a permanent contract (17-20 months)

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

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

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

Table 3.82: Bounds on the treatment effect on the probability of having a permanent contract (21-24 months)

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

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

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

Table 3.83: Bounds on the treatment effect on the probability of working in the same 2-digit industry (1-4 months)

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

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

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

Table 3.84: Bounds on the treatment effect on the probability of working in the same 2-digit industry (5-8 months)

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

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

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

Table 3.85: Bounds on the treatment effect on the probability of working in the same 2-digit industry (9-12 months)

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

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

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

Table 3.86: Bounds on the treatment effect on the probability of working in the same 2-digit industry (13-16 months)

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

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

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

Table 3.87: Bounds on the treatment effect on the probability of working in the same 2-digit industry (17-20 months)

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

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

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

Table 3.88: Bounds on the treatment effect on the probability of working in the same 2-digit industry (21-24 months)

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

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

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

Table 3.89: Bounds on the treatment effect on the probability of working full-time (1-4 months)

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

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

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

Table 3.90: Bounds on the treatment effect on the probability of working full-time (5-8 months)

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

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

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

Table 3.91: Bounds on the treatment effect on the probability of working full-time (9-12 months)

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

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

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

Table 3.92: Bounds on the treatment effect on the probability of working full-time (13-16 months)

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

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

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

Table 3.93: Bounds on the treatment effect on the probability of working full-time (17-20 months)

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

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

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

Table 3.94: Bounds on the treatment effect on the probability of working full-time (21-24 months)

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

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

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

Table 3.95: Bounds on the treatment effect on contract duration (1-4 months)

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

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

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

Table 3.96: Bounds on the treatment effect on contract duration (5-8 months)

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

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

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

Table 3.97: Bounds on the treatment effect on contract duration (9-12 months)

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

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

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

Table 3.98: Bounds on the treatment effect on contract duration (13-16 months)

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

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

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

Table 3.99: Bounds on the treatment effect on contract duration (17-20 months)

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

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

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

Table 3.100: Bounds on the treatment effect on contract duration (21-24 months)

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

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

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

Table 3.101: Impact of UI benefit receipt on the number of employment spells over the following two years

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

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

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

Table 3.102: Impact of UI benefit receipt on the number of unemployment spells over the following two years

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

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

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

Table 3.103: Impact of UI benefit receipt on the total number of days employed over the following two years

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

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

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

Table 3.104: Impact of UI receipt on employment probability (1-4 months) - Bivariate probit

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

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

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

Table 3.105: Impact of UI receipt on employment probability (5-8 months) - Bivariate probit

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

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

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

Table 3.106: Impact of UI receipt on employment probability (9-12 months) - Bivariate probit

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

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

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

Table 3.107: Impact of UI receipt on employment probability (13-16 months) - Bivariate probit

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

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

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

Table 3.108: Impact of UI receipt on employment probability (17-20 months) - Bivariate probit

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

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

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

Table 3.109: Impact of UI receipt on employment probability (21-24 months) - Bivariate probit

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

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

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

Main conclusion

This dissertation proposes an empirical investigation of the impact of unemployment insurance (UI) programs on the behaviour of firms and workers. At the crossroads between labour and public economics, it relates to the issues of why and how much UI we should set up at the national level. From the comprehensive study of the French case, I intend to provide more general results on the knowledge of the labour market effects of UI.

Contributions and policy implications

While evidence on the existence of adverse selection in the UI market is scarce, chapter 1 takes advantage of an original framework to verify this hypothesis empirically. It also highlights the existence of significant moral hazard and imperfect individual optimisation in the UI market. I evaluate a UI program offering some job-seekers the choice between a low level of benefits for a long duration or a higher level of benefits for a shorter duration. Whereas most of the papers estimating the cost of UI within the optimal UI framework focus either on the duration or the level of benefits, I am able to look at a simultaneous and opposite change in both parameters. The choice is related to the predicted unemployment risk to demonstrate that riskier individuals opt for the highest level of coverage. I also show that taking the higher level of benefits is associated to a strong moral hazard cost through a longer unemployment spell duration. Combining the study of the takeup choice and its consequences, I document a marked heterogeneity: younger workers with a low level of education and skills, who are the ones with a high initial unemployment risk, experience a higher increase in their unemployment duration. It suggests that workers who are the most in difficulty do not manage to optimise their compensation.

In Chapter 2, I study the relationship between UI benefits and layoff decisions. More precisely, I show that workers and firms agree to schedule the layoff date in response to a discontinuous increase in the level of benefits at a tenure threshold. This extension of the employment spell is quantified by the computation of an elasticity of employment duration with respect to the level of UI benefits. This parameter can be seen as the counterpart of the elasticity of unemployment duration with respect to the level of UI benefits, which is at the core of the optimal UI formula. I provide suggestive evidence that this extension goes through an individual bargaining

process between the worker and the firm. To gain insight on this bargaining black-box, I try to decompose the heterogeneity in bunching into the part due to the variation in incentives and the part due to the variation in the ability to bargain. All in all, individual characteristics related to the bargaining resources of the worker seem to play a major role. This finding is in line with what has been revealed in other contexts of individual bargaining, on the severance pay granted after a mutually-agreed termination for instance (Bouvier, 2017).

Chapter 3 is in keeping with the second one, in the sense that it investigates the influence of UI on pre-unemployment outcomes. The existence of a minimum employment history condition to claim UI benefits and the fact that this condition has changed overtime are used to show that workers and employers respond to the increase in the outside option of the worker. This response takes the form of a jump in the separation rate at the eligibility threshold. This result translates, in some sectors, into a post-reform increase in the number of temporary contracts whose duration corresponds to the new employment record condition. The fact that this increase affects workers irrespective of their work history suggests the emergence of a new norm with regard to contract duration. Therefore, UI design seems to influence the structure of contracts, at least for part of the workforce. We then complement the recent literature on the impact of UI on *inflows* to unemployment by showing that besides this impact, UI may influence the employment spells of workers not concerned by unemployment. We also report findings on the impact of receiving UI benefits on post-unemployment outcomes. The negative effect on the job-finding probability does not seem to be counterbalanced by a positive impact on job quality. However, measurement issues in the work history variable could raise some concerns in terms of precision.

The consistency of those three different chapters allow me to draw some policy implications, following the efficiency-equity twofold objective. The first one would be to pay more attention to the effect of UI design on the hiring and firing decisions of firms or on the effort of the worker while employed. This is an aspect that has started to be integrated into the reflection on UI design in France, as illustrated by the introduction of some degree of experience rating in 2019. The second one stems from the fact that all three chapters point to substantial heterogeneity in the effect that is measured, generally at the expense of workers with the highest unemployment risk. As all unemployed workers do not have the same level of information, are not able to grasp the rules the same way, or do not face the same liquidity constraints, attention should be devoted to design programs that do not widen these initial inequalities.

Limitations and future extensions

The dissertation intends to bring new answers on optimal UI design, but suffers from several limitations. I describe some of those limitations, and provide potential

ideas to tackle them.

In chapter 1, I exploit the fact that I observe workers choosing their level of insurance. However, I believe that I could go one step further and use this asset to inform on job-seekers' preferences. Although I am limited by the fact that I do not observe all the characteristics of the different options for part of my sample, I think this extension could take one of the three following forms: (i) a first approach relates to the one developed by Landais and Spinnewijn (2019), who also use insurance choices in Sweden to assess the valuation of UI by workers. It is a crucial issue in public economics, as it captures the benefits of UI that should equalise its cost in the optimal UI formula. With a few exceptions (Gruber, 1997; Landais and Spinnewijn, 2019), no accurate evaluation of the value of UI is available; (ii) a second possibility would be to use the trade-off between the level and the duration of UI benefits to derive a marginal rate of substitution between those two parameters. It would complement previous research that has integrated those two aspects into a single model (Landais, 2015; Schmieder, von Wachter and Bender, 2016). (iii) A third way to go relates to the behavioural literature. As the choice I observe comes down to choosing high benefits sooner or later in the spell, it could be used to measure impatience among job-seekers. According to previous papers (DellaVigna and Paserman, 2005; Paserman, 2008), job search is negatively impacted by impatience when hyperbolic discounting is introduced. In my setting, hyperbolic discounting could both rationalise the choice and the impact on unemployment duration, as the ones choosing higher benefits immediately are also the ones staying unemployed longer.

A potential extension that was part of the initial project on this topic would be to set up an experiment on job-seekers eligible to the option right, to explore one of the mechanisms driving their choice. Anecdotal evidence has reported that information could be a possible channel. The idea would be to give an information shock to a random subsample of eligible workers, by sending them a letter mentioning their eligibility, and the terms and conditions of the choice. This extension would require the cooperation of the *Unédic*, and could be done joint with Luc Behaghel. Another worthy channel to explore would be the role of caseworkers in shaping the choice of job-seekers. Additional data from the unemployment agency (*Pôle emploi*) could possibly be requested to further study this mechanism.

Chapter 2 uses the bunching methodology to measure a reduced-form elasticity of employment duration with respect to the level of UI benefits. A potential extension would be to provide a structural estimation of this parameter. The structural estimation turns out to be complex in my setting as the optimisation behaviour is at joint level of the worker and the firm. It is therefore difficult to build a model disentangling the role of each party. However, it could be very informative to do out-of-sample predictions of the behavioural response to different benefit policies. Another limitation is that, for the moment, arguments supporting the bargaining

scenario may be convincing but are only indirect (provided by the heterogeneity analysis or the study of collective dismissals). Having more information on the division of the surplus between firms and workers could be an interesting extension. To my knowledge, no individual data on industrial tribunal proceedings or damages can be matched with UI data, and I find no evidence of bunchers receiving lower wages, as it would be predicted by the efficient contract theory. Further exploration of potential data could be done, or qualitative evidence could be collected.

In chapter 3, the results on contract duration are particularly interesting in the light of recent trends on the French labour market. However, to be fully convincing, the analysis should ideally be done on vacancy data. Indeed, to support the hypothesis that the reform in the UI eligibility criteria has shaped a new norm regarding the duration of short contracts offered by employers, we would like to observe the posted duration rather than the effective one. I believe this complementary analysis could easily be done on data from the unemployment agency (*Pôle emploi*), although the data request requires some time.

The second part of the chapter reveals surprising results on employment probability, and mostly insignificant ones on job quality. This section of the chapter may be flawed by precision issues, as the work history is measured with error. The regression discontinuity design being very demanding in terms of precision, it could explain part of our results. A potential solution is provided by the methodology developed by [Davezies and Le Barbanchon \(2017\)](#) to correct for measurement error in the running variable. We already established that this method was suitable for our data, and we are in the process of applying it.

More generally, I am very interested in pursuing the path I have started with this dissertation, by further analysing the relationship between on-the-job behaviours and UI design. In particular, I believe that the integration of the elasticity of employment duration with respect to the level of UI benefits into the optimal UI model could be a substantial addition to the original framework. A better consideration of the interactions between UI design and employment protection would also be an important step to understand how firms, to some degree, adapt their decisions to this institutional setting. This objective will partly drive my future research agenda.

Conclusion générale

La présente thèse propose une étude empirique de l'impact des programmes d'assurance chômage sur le comportement des entreprises et des travailleurs. À la croisée de l'économie du travail et de l'économie publique, elle traite de la question de pourquoi et dans quelle mesure nous devons mettre en place une assurance chômage au niveau national. À partir de l'étude détaillée du cas français, l'objectif est de fournir des résultats plus généraux alimentant la connaissance des effets de l'assurance chômage sur le marché du travail.

Contributions et implications de politiques publiques

Alors que les preuves de l'existence d'une sélection adverse sur le marché de l'assurance chômage sont rares, le chapitre 1 tire parti d'un cadre original pour vérifier la validité empirique de cette hypothèse. Il met également en évidence l'existence d'un aléa moral important et d'une optimisation individuelle imparfaite sur le marché de l'assurance chômage. Nous évaluons un programme d'assurance chômage qui offre à certains demandeurs d'emploi le choix entre un faible niveau de prestations pour une longue période ou un niveau plus élevé de prestations pour une plus courte période. Alors que la plupart des études qui évaluent le coût de l'assurance chômage dans le cadre du modèle d'assurance chômage optimale portent sur la durée ou le niveau des prestations, nous sommes en mesure d'examiner le changement simultané et contraire des deux paramètres. Le choix observé est rapproché du risque de chômage prédit pour démontrer que les personnes les plus à risque optent pour le niveau de couverture le plus élevé. Nous montrons également que le fait de toucher des prestations plus élevées est associé à un fort aléa moral qui se traduit par une durée plus longue de l'épisode de chômage. En combinant l'étude du choix du niveau d'assurance et de ses conséquences, nous constatons une hétérogénéité marquée : les jeunes travailleurs ayant un faible niveau d'éducation et de qualification, qui sont ceux qui présentent un risque initial de chômage élevé, voient leur durée de chômage augmenter davantage. Ce résultat suggère que les travailleurs les plus en difficulté ne parviennent pas à optimiser les conditions de leur indemnisation.

Dans le chapitre 2, nous nous concentrons sur l'étude de la relation entre les prestations d'assurance chômage et les décisions de licenciement. Plus précisément, nous montrons que les travailleurs et les employeurs s'entendent pour fixer la date de

licenciement en réponse à une augmentation discontinue du niveau des allocations à un seuil d'ancienneté. Cette prolongation de la période d'emploi est quantifiée par le calcul d'une élasticité de la durée d'emploi par rapport au niveau des allocations chômage. Ce paramètre peut être considéré comme le pendant de l'élasticité de la durée du chômage par rapport au niveau des allocations chômage, qui est au cœur de la formule d'assurance chômage optimale. Nos résultats suggèrent que cette prolongation passe par un processus de négociation individuelle entre le travailleur et l'employeur. Pour mieux comprendre ce processus de négociation, nous tentons de décomposer l'hétérogénéité du *bunching*, qui renvoie au phénomène de regroupement des licenciements juste au-dessus du seuil d'un an d'ancienneté, en une partie due à la variation des incitations financières et une autre partie due à la variation dans la capacité de négociation. Dans l'ensemble, les caractéristiques individuelles associées au pouvoir de négociation du travailleur semblent jouer un rôle majeur. Ce résultat est en conformité avec ce qui a été constaté dans d'autres contextes de négociation individuelle, par exemple en ce qui concerne l'indemnité de départ accordée après une rupture conventionnelle (Bouvier, 2017).

Le chapitre 3 se place dans la même lignée que le second, en ce sens qu'il examine l'influence de l'assurance chômage sur les résultats pré-chômage. L'existence d'une condition d'historique d'emploi minimal pour faire une demande d'allocations chômage et le fait que cette même condition a changé dans le temps sont utilisés pour montrer que les travailleurs et les employeurs réagissent à l'augmentation de l'option de sortie du travailleur. Cette réponse prend la forme d'un accroissement discontinu dans le taux de séparation au seuil d'éligibilité. Ce résultat se traduit, dans certains secteurs, par une augmentation, après la réforme, du nombre de contrats à durée déterminée dont la durée correspond à la nouvelle condition d'emploi minimal. Le fait que cette augmentation affecte les travailleurs indépendamment de leur historique d'emploi suggère l'émergence d'une nouvelle norme relative à la durée des contrats. Par conséquent, la façon dont est conçue l'assurance chômage semble influencer sur la structure des contrats, au moins pour une partie de la main-d'œuvre. Nous enrichissons la récente littérature sur l'impact de l'assurance chômage sur les *entrées* au chômage en montrant qu'outre cet impact, l'assurance chômage peut influencer les épisodes d'emploi des travailleurs non concernés par le chômage. Nous présentons également des résultats sur l'incidence du versement de prestations d'assurance chômage sur les résultats postérieurs à l'épisode de chômage. L'effet négatif sur la probabilité de trouver un emploi ne semble pas être compensé par un effet positif sur la qualité de l'emploi. Toutefois, les problèmes de mesure de la variable d'historique d'emploi compromettent la précision de ces derniers résultats.

La cohérence de ces trois chapitres me permet de dégager certaines pistes de réflexion sur des politiques publiques qui répondraient au double objectif d'efficience et d'équité. La première implication consisterait à accorder plus d'attention à l'effet

de l'assurance chômage sur les décisions d'embauche et de licenciement des entreprises ou sur l'effort du travailleur lorsqu'il est en emploi. C'est un aspect qui commence à être intégré dans la réflexion sur la conception de l'assurance chômage en France, comme l'illustre l'introduction, en 2019, d'une modulation des contributions. La deuxième implication tient au fait que les trois chapitres mettent en évidence une forte hétérogénéité dans l'effet mesuré, généralement au détriment des travailleurs les plus en difficulté sur le marché du travail. Etant donné que tous les chômeurs n'ont pas le même niveau d'information, ne sont pas capables d'appréhender les règles de la même façon ou ne sont pas confrontés aux mêmes contraintes de liquidité, il convient de s'attacher à concevoir des programmes qui ne creusent pas ces inégalités initiales.

Limites et développements futures

Cette thèse vise à apporter de nouvelles réponses sur la conception optimale de l'assurance chômage, mais se heurte à plusieurs limites. Nous décrivons certaines de ces limites, et proposons de potentielles façons de les dépasser.

Dans le chapitre 1, nous exploitons le fait que nous observons les chômeurs choisir leur niveau d'assurance. Cependant, nous pourrions sans doute aller plus loin et utiliser cette étude pour approfondir nos connaissances sur les préférences des demandeurs d'emploi. Malgré le fait que nous soyons limités par une connaissance partielle des caractéristiques des différentes options pour une partie de notre échantillon, cette extension pourrait prendre l'une des trois formes suivantes : i) une première stratégie se rapproche de celle développée par [Landais and Spinnewijn \(2019\)](#), qui utilisent également l'observation du choix d'assurance en Suède pour évaluer la façon dont les individus valorisent l'assurance chômage. Il s'agit d'une question cruciale en économie publique, car cette valorisation correspond aux bénéfices de l'assurance chômage qui doivent équilibrer ses coûts dans la formule d'assurance chômage optimale. A quelques exceptions près ([Gruber, 1997](#); [Landais and Spinnewijn, 2019](#)), aucune évaluation précise de la valeur de l'assurance chômage n'est à ce jour disponible ; ii) une deuxième possibilité consisterait à utiliser l'arbitrage entre le niveau et la durée des prestations pour obtenir un taux marginal de substitution entre ces deux paramètres. Cette extension compléterait les recherches antérieures qui ont intégré ces deux aspects dans un modèle unique ([Landais, 2015](#); [Schmieder and Von Wachter, 2016](#)). (iii) Une troisième approche est liée à la littérature sur les paramètres comportementaux. Etant donné que le choix que nous observons revient à opter pour des prestations plus élevées plus ou moins tôt dans l'épisode de chômage, il pourrait être utilisé pour mesurer l'impatience des demandeurs d'emploi. Selon des articles antérieurs ([DellaVigna and Paserman, 2005](#); [Paserman, 2008](#)), la recherche d'emploi est négativement affectée par l'impatience lorsque des préférences hyperboliques en matière d'escompte sont introduites. Dans le contexte de cette étude, un taux

d'escompte hyperbolique pourrait à la fois rationaliser le choix observé et l'impact sur la durée du chômage, puisque ceux qui choisissent des allocations plus élevées immédiatement sont aussi ceux qui restent au chômage plus longtemps.

Une extension potentielle, qui faisait partie du projet initial sur ce sujet, serait de mettre en place une expérience sur les demandeurs d'emploi éligibles au droit d'option, afin d'explorer un des mécanismes motivant leur choix. Des témoignages ont rapporté que l'information pourrait être un canal possible. L'idée serait de donner un choc d'information à un sous-échantillon aléatoire de travailleurs éligibles, par l'envoi d'un courrier mentionnant leur éligibilité et les modalités du choix. Cette expérimentation nécessiterait la coopération de l'*Unédic*, et pourrait être faite conjointement avec Luc Behaghel. Un autre canal intéressant à explorer serait le rôle des conseillers dans le choix des demandeurs d'emploi. Des données supplémentaires de *Pôle emploi* pourraient éventuellement être demandées pour étudier plus avant ce mécanisme.

Le chapitre 2 utilise la méthode du *bunching* pour mesurer une élasticité en forme réduite de la durée d'emploi par rapport au niveau des allocations chômage. Une extension possible consisterait à réaliser une estimation structurelle de ce paramètre. L'estimation structurelle s'avère complexe dans le contexte de cette étude car le comportement d'optimisation se situe au niveau conjoint du travailleur et de l'entreprise. Il est donc difficile de construire un modèle démêlant le rôle de chaque partie. Toutefois, ce paramètre pourrait être très utile pour faire des prédictions hors échantillon de la réaction comportementale à différentes politiques affectant les prestations. Une autre limite de ce chapitre est que, pour le moment, les preuves soutenant le scénario de négociation sont potentiellement convaincantes mais restent indirectes (fournies par l'analyse de l'hétérogénéité ou l'étude des licenciements collectifs). Obtenir plus d'information sur la répartition du surplus entre les employeurs et les travailleurs pourrait se révéler intéressant. A notre connaissance, aucune données individuelles sur les procès ou les dommages et intérêts aux prud'hommes ne peuvent être appariées aux données de l'assurance chômage, et aucun de nos résultats ne montrent que les *bunchers* touchent des rémunérations plus faibles, comme le prévoit la théorie du contrat efficient. Une exploration plus approfondie de potentielles données pourrait être faite, ou des données qualitatives pourraient être recueillies.

Le chapitre 3 présente des résultats sur la durée des contrats, qui prennent toute leur importance à la lumière des évolutions récentes sur le marché du travail français. Toutefois, pour être pleinement convaincante, l'analyse devrait idéalement porter sur des données renseignant sur les postes vacants. En effet, pour étayer l'hypothèse selon laquelle la réforme du critère d'éligibilité à l'assurance chômage a façonné une nouvelle norme concernant la durée des contrats de courte durée offerts par les employeurs, nous aimerions observer la durée affichée plutôt que la durée effective des contrats. Cette analyse complémentaire pourrait facilement être réalisée à partir

des données de *Pôle emploi*, bien que le processus d'accès aux données soit long.

La deuxième partie du chapitre met en lumière des résultats surprenants sur la probabilité de retour à l'emploi, et insignifiants pour la plupart sur la qualité de l'emploi. Cette section du chapitre est affaiblie par des problèmes de précision, car l'historique d'emploi est mesuré avec une marge d'erreur. La méthode de la régression sur discontinuité étant très exigeante en termes de précision, ce problème pourrait expliquer en partie nos résultats. Une solution potentielle est fournie par la méthodologie développée par [Davezies and Le Barbanchon \(2017\)](#) pour corriger les erreurs de mesure dans la variable déterminant l'affectation au traitement. Nous avons déjà vérifié que cette méthode était adaptée à nos données, et son application est en cours.

De façon plus générale, je suis très enthousiaste à l'idée de poursuivre dans la voie que j'ai entamée avec cette thèse, en analysant plus en profondeur la relation entre les comportements en emploi et la conception de l'assurance chômage. En particulier, l'intégration de l'élasticité de la durée de l'emploi par rapport au niveau des allocations chômage dans le modèle d'assurance chômage optimal pourrait constituer un ajout important au cadre initial. Une meilleure prise en compte des interactions entre l'élaboration des règles de l'assurance-chômage et de la protection de l'emploi serait également une étape importante pour comprendre comment les entreprises adaptent, dans une certaine mesure, leurs décisions à ce cadre institutionnel. Cet objectif est ce qui orientera, en partie, mes futures problématiques de recherche.

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List of Tables

1.1	Sample composition	67
1.2	Individual characteristics of takers and non-takers based on the 20€ criterion	69
1.3	Individual characteristics of compliers	70
1.4	Characteristics associated to option right takeup - takers	71
1.5	Characteristics associated to option right takeup - compliers	72
1.6	New and former rights' characteristics of takers	72
1.7	Individual characteristics of unemployed people earning between 16€ and 24€ as daily benefits	75
1.8	Eligibility criteria distribution among takers	76
1.9	Impact of paid unemployment spell duration with different bandwidths	77
1.10	Impact of having a DB lower or equal to 20€ on OR takeup	78
1.11	Test of continuity of covariates distribution	82
1.12	Impact of the option right on paid unemployment duration – 2 nd stage	82
1.13	Impact of the option right on full unemployment duration – 2 nd stage	83
1.14	Placebo tests at random cutoff values	86
1.15	Impact of the option right on the total number of days on UI benefits – 2 nd stage	88
1.16	Impact of the option right on the total number of days registered as unemployed – 2 nd stage	89
1.17	Impact of OR on unemployment duration – 2 nd stage	90
1.18	Impact of OR on the probability to exhaust benefits – 2 nd stage	91
1.19	Impact of OR on the consumption pace of UI benefits – 2 nd stage	92
1.20	Probability of exhausting the UI right	94
1.21	Impact of the option right on the probability to work within the spell – 2 nd stage	94
1.22	Impact of the option right on the labour income earned over the spell – 2 nd stage	95
1.23	Impact of the option right on the number of hours worked during the spell – 2 nd stage	96
1.24	Impact of the option right on hourly wage from work during the UI spell – 2 nd stage	97
1.25	Impact on the probability to work during the UI spell (3-6 months horizon) – 2 nd stage	98
1.26	Impact on the probability to work during the UI spell (9-12 months horizon) – 2 nd stage	98

1.27	Impact of OR on the capital of benefits earned over subsequent spell – 2 nd stage	99
1.28	Impact of OR on the capital of benefits earned over subsequent period – 2 nd stage	100
1.29	Impact of having a DB lower or equal to 20€ on OR takeup by age categories	100
1.30	Impact of OR on the paid unemployment duration by age categories – 2 nd stage	101
1.31	Impact of having a DB lower or equal to 20€ on OR takeup by education categories	101
1.32	Impact of OR on the paid unemployment duration by education categories – 2 nd stage	102
1.33	Impact of having a DB lower or equal to 20€ on OR takeup by skill categories	102
1.34	Impact of OR on the paid unemployment duration by skill categories – 2 nd stage	103
1.35	Impact of having a DB lower or equal to 20€ on OR takeup by sex categories	103
1.36	Impact of OR on the paid unemployment duration by sex categories – 2 nd stage	104
1.37	Predicted unemployment duration by takeup	104
1.38	Summary statistics by predicted unemployment duration	105
1.39	First-stage regression by predicted unemployment duration quintile	105
1.40	Second-stage regression by predicted unemployment duration quintile	106
1.41	Predicted unemployment duration by takeup (using the control group)	106
1.42	Summary statistics by predicted unemployment duration (using the control group)	107
1.43	First-stage regression by predicted unemployment duration (using the control group) quintile	107
1.44	Second-stage regression by predicted unemployment duration (using the control group) quintile	108
1.45	Impact on the probability to be on paid unemployment – 2 nd stage	108
1.46	Impact on the probability to be registered as unemployed – 2 nd stage	109
1.47	Impact of the option right on paid UI spell duration before any interruption	110
2.1	Entitlements according to worker’s decision and seniority	152
2.2	Characteristics of laid-off workers accepting or refusing the CSP	153
2.3	Determinants of the probability to accept the CSP	154
2.4	Log discontinuity estimates	157
2.5	Magnitude of the discontinuity at 365 days for different values of observable characteristics	161
2.6	Distribution of the number of workers laid-off by the same firm	169
2.7	Layoff rank within the same redundancy plan	170
2.8	Proportion of firms shutting down	171
2.9	Predicted correlations with the likelihood to bunch	171
2.10	Gain distribution of the sample under 2 years of tenure	171
2.11	Log discontinuity estimates according to potential gain	172

2.12	Seniority count regression	173
2.13	Reduced-form elasticity estimates	176
2.14	Difference-in-bunching elasticity estimates	180
2.15	Characteristics of compliers, eligible and never-takers	181
2.16	Elasticity estimates by gain categories	181
2.17	Descriptive statistics by gain category	182
2.18	Differences in observable characteristics by gain categories	182
2.19	Elasticity estimates by cell (small firms)	193
2.20	Elasticity estimates by cell (large firms)	194
2.21	Elasticity estimates by baseline supra-legal severance payment	195
2.22	Elasticity estimates by predicted unemployment duration quintile	195
2.23	Log discontinuity estimates by gain and representation categories	196
2.24	Elasticity estimates by gain and representation categories	196
2.25	Logit model on propensity to bunch	197
2.26	Elasticity estimates by type of layoff	197
2.27	Fixed-effect logit of the propensity to bunch (odds ratio)	198
2.28	Reduced-form elasticity estimates with round-number fixed effect	198
2.29	Difference-in-difference estimation of the number of excess layoffs (MMO data)	199
2.30	Difference-in-difference estimate of the number of layoff (UI data)	200
2.31	Parameters used in the cost-benefit computation	203
2.32	Relative annual cost of bunching without taking into account the counterfactual benefits	204
2.33	Consequences of accepting the CSP and their valuation	205
2.34	Proportion of different contract termination types within the same episode for bunching and non bunching individuals	207
2.35	Proportion of different contract termination types within the same episode for bunching and non bunching firms	208
2.36	Proportion of different contract termination types within the same episode depending on workforce size	209
2.37	Proportion of different contract termination types within the same episode	210
2.38	Impact of having bunched in the past on the probability of bunching in the future	213
2.39	Impact of having bunched in the past on the number of different contract termination types	214
2.40	Impact of having bunched in the past on the proportion of different contract termination types	215
2.41	Reduced-form elasticity estimates at the two-year threshold during the pre-CSP period	216
2.42	Zero-inflated negative binomial regression tests	220
2.43	Zero-inflated negative binomial regression on supra-legal severance payment	222
3.1	Sample composition (MMO data)	251
3.2	Descriptive statistics by work history	252
3.3	Descriptive statistics by work history - sectors of activity	253
3.4	Descriptive statistics by work history – Occupation type	254
3.5	Discontinuity in the transition rate from employment to non- employment on full-time workers	259

3.6	Discontinuity in the transition rate from employment to registered unemployment	259
3.7	Difference-in-difference estimate of the number of 4-month contracts relative to 6-month contracts (End of fixed-term contracts)	260
3.8	Sectorial distribution of firms with a high relative increase in 4-month contracts (End of fixed-term contracts)	262
3.9	Difference-in-difference estimate of the number of 4-month contracts relative to 6-month contracts (Economic layoffs)	267
3.10	Difference-in-difference estimate of the number of 4-month contracts relative to 6-month contracts (Personal dismissals)	267
3.11	Difference-in-difference estimate of the number of 4-month contracts relative to 6-month contracts (End of trial period)	268
3.12	Difference-in-difference estimate of the number of 4-month contracts relative to 6-month contracts (Voluntary resignation)	268
3.13	Difference-in-difference estimate of the number of 4-month contracts relative to 6-month contracts (Retirement)	269
3.14	Difference-in-difference estimate of the number of 4-month contracts relative to 6-month contracts (Pre-retirement)	269
3.15	Post-reform evolution of 4 to 5-month contracts (6-month and 1 to 3-month control group)	273
3.16	Descriptive statistics on treated and control workers	280
3.17	Descriptive statistics on takers and non-takers	281
3.18	Impact of separating after the reform on UI takeup	283
3.19	Check of no discontinuity in the distribution of covariates	287
3.20	Impact of UI benefits receipt on employment probability (1-4 months)	289
3.21	Impact of UI benefits receipt on employment probability (5-8 months)	290
3.22	Impact of UI benefits receipt on employment probability (9-12 months)	290
3.23	Impact of UI benefits receipt on employment probability (13-16 months)	290
3.24	Impact of UI benefits receipt on employment probability (17-20 months)	291
3.25	Impact of UI benefits receipt on employment probability (21-24 months)	291
3.26	Impact of UI benefits receipt on employment probability (1-4 months)	291
3.27	Impact of UI benefits receipt on employment probability (5-8 months)	292
3.28	Impact of UI benefits receipt on employment probability (9-12 months)	292
3.29	Impact of UI benefits receipt on employment probability (13-16 months)	292
3.30	Impact of UI benefits receipt on employment probability (17-20 months)	293
3.31	Impact of UI benefits receipt on employment probability (21-24 months)	293
3.32	Impact of UI benefits receipt on the duration to next job	293

3.33	Impact of UI benefits receipt on unemployment probability (1-4 months)	294
3.34	Impact of UI benefits receipt on unemployment probability (5-8 months)	294
3.35	Impact of UI benefits receipt on unemployment probability (9-12 months)	294
3.36	Impact of UI benefits receipt on unemployment probability (13-16 months)	295
3.37	Impact of UI benefits receipt on unemployment probability (17-20 months)	295
3.38	Impact of UI benefits receipt on unemployment probability (21-24 months)	295
3.39	Impact of UI benefits receipt on labour market participation probability (1-4 months)	296
3.40	Impact of UI benefits receipt on labour market participation probability (5-8 months)	296
3.41	Impact of UI benefits receipt on labour market participation probability (9-12 months)	296
3.42	Impact of UI benefits receipt on labour market participation probability (13-16 months)	297
3.43	Impact of UI benefits receipt on labour market participation probability (17-20 months)	297
3.44	Impact of UI benefits receipt on labour market participation probability (21-24 months)	297
3.45	Impact of UI benefits receipt on daily earnings (1-4 months)	298
3.46	Impact of UI benefits receipt on daily earnings (5-8 months)	298
3.47	Impact of UI benefits receipt on daily earnings (9-12 months)	298
3.48	Impact of UI benefit receipt on cumulative earnings over two years	299
3.49	Impact of UI benefit receipt on the probability to have a permanent contract	299
3.50	Impact of UI benefit receipt on the probability to work full-time	299
3.51	Impact of UI benefit receipt on the probability to work in the same 2-digit industry	300
3.52	Impact of UI benefit receipt on the duration of the contract	300
3.53	Impact of UI benefits receipt on probability of having a permanent contract (1-4 months)	301
3.54	Impact of UI benefits receipt on probability of having a permanent contract (5-8 months)	301
3.55	Impact of UI benefits receipt on probability of having a permanent contract (9-12 months)	302
3.56	Impact of UI benefits receipt on probability of having a permanent contract (13-16 months)	302
3.57	Impact of UI benefits receipt on probability of having a permanent contract (17-20 months)	303
3.58	Impact of UI benefits receipt on probability of having a permanent contract (21-24 months)	303
3.59	Impact of UI benefits receipt on the probability of working in the same 2-digit industry (1-4 months)	304
3.60	Impact of UI benefits receipt on the probability of working in the same 2-digit industry (5-8 months)	304

3.61	Impact of UI benefits receipt on the probability of working in the same 2-digit industry (9-12 months)	305
3.62	Impact of UI benefits receipt on the probability of working in the same 2-digit industry (13-16 months)	305
3.63	Impact of UI benefits receipt on the probability of working in the same 2-digit industry (17-20 months)	306
3.64	Impact of UI benefits receipt on the probability of working in the same 2-digit industry (21-24 months)	306
3.65	Impact of UI benefits receipt on the probability of working full-time (1-4 months)	307
3.66	Impact of UI benefits receipt on the probability of working full-time (5-8 months)	307
3.67	Impact of UI benefits receipt on the probability of working full-time (9-12 months)	307
3.68	Impact of UI benefits receipt on the probability of working full-time (13-16 months)	308
3.69	Impact of UI benefits receipt on the probability of working full-time (17-20 months)	308
3.70	Impact of UI benefits receipt on the probability of working full-time (21-24 months)	308
3.71	Impact of UI benefits receipt on contract duration (1-4 months)	309
3.72	Impact of UI benefits receipt on contract duration (5-8 months)	309
3.73	Impact of UI benefits receipt on contract duration (9-12 months)	309
3.74	Impact of UI benefits receipt on contract duration (13-16 months)	310
3.75	Impact of UI benefits receipt on contract duration (17-20 months)	310
3.76	Impact of UI benefits receipt on contract duration (21-24 months)	310
3.77	Bounds on the treatment effect on the probability of having a permanent contract (1-4 months)	311
3.78	Bounds on the treatment effect on the probability of having a permanent contract (5-8 months)	311
3.79	Bounds on the treatment effect on the probability of having a permanent contract (9-12 months)	312
3.80	Bounds on the treatment effect on the probability of having a permanent contract (13-16 months)	312
3.81	Bounds on the treatment effect on the probability of having a permanent contract (17-20 months)	313
3.82	Bounds on the treatment effect on the probability of having a permanent contract (21-24 months)	313
3.83	Bounds on the treatment effect on the probability of working in the same 2-digit industry (1-4 months)	314
3.84	Bounds on the treatment effect on the probability of working in the same 2-digit industry (5-8 months)	314
3.85	Bounds on the treatment effect on the probability of working in the same 2-digit industry (9-12 months)	315
3.86	Bounds on the treatment effect on the probability of working in the same 2-digit industry (13-16 months)	315
3.87	Bounds on the treatment effect on the probability of working in the same 2-digit industry (17-20 months)	316
3.88	Bounds on the treatment effect on the probability of working in the same 2-digit industry (21-24 months)	316

3.89	Bounds on the treatment effect on the probability of working full-time (1-4 months)	317
3.90	Bounds on the treatment effect on the probability of working full-time (5-8 months)	317
3.91	Bounds on the treatment effect on the probability of working full-time (9-12 months)	318
3.92	Bounds on the treatment effect on the probability of working full-time (13-16 months)	318
3.93	Bounds on the treatment effect on the probability of working full-time (17-20 months)	319
3.94	Bounds on the treatment effect on the probability of working full-time (21-24 months)	319
3.95	Bounds on the treatment effect on contract duration (1-4 months)	320
3.96	Bounds on the treatment effect on contract duration (5-8 months)	320
3.97	Bounds on the treatment effect on contract duration (9-12 months)	321
3.98	Bounds on the treatment effect on contract duration (13-16 months)	321
3.99	Bounds on the treatment effect on contract duration (17-20 months)	322
3.100	Bounds on the treatment effect on contract duration (21-24 months)	322
3.101	Impact of UI benefit receipt on the number of employment spells over the following two years	323
3.102	Impact of UI benefit receipt on the number of unemployment spells over the following two years	323
3.103	Impact of UI benefit receipt on the total number of days employed over the following two years	323
3.104	Impact of UI receipt on employment probability (1-4 months) - Bivariate probit	324
3.105	Impact of UI receipt on employment probability (5-8 months) - Bivariate probit	324
3.106	Impact of UI receipt on employment probability (9-12 months) - Bivariate probit	324
3.107	Impact of UI receipt on employment probability (13-16 months) - Bivariate probit	325
3.108	Impact of UI receipt on employment probability (17-20 months) - Bivariate probit	325
3.109	Impact of UI receipt on employment probability (21-24 months) - Bivariate probit	325

List of Figures

1	Public social and UI spending (LHS), and average net replacement rates (RHS) in OECD countries (% GDP)	2
2	Share and average duration of fixed-term contracts in France (2001-2017)	10
3	Dépenses sociales et de chômage (gauche), et taux de remplacement nets moyens (droite) dans les pays de l'OCDE (% PIB)	20
4	Part et durée moyenne des contrats à durée déterminée en France (2001-2017)	28
1.1	Option right trade-off	67
1.2	Number of people exercising the option right over time	68
1.3	Average daily benefit among takers	73
1.4	Average PBD among takers if taking or not taking	73
1.5	Average PBD without taking into account recharging	74
1.6	Mc Crary test on previous DB distribution	74
1.7	Impact of paid unemployment spell duration with different bandwidths	77
1.8	Probability of taking the option right	79
1.9	Age distribution	80
1.10	Proportion of women distribution	80
1.11	Level of education distribution	81
1.12	Magnitude of the difference in covariates at the cutoff	81
1.13	Paid unemployment spell duration	84
1.14	Full unemployment spell duration	85
1.15	Short-term Kaplan-Meier survival function	85
1.16	Long-term Kaplan-Meier survival function	86
1.17	Total number of days unemployed after the exercise of the option right	87
1.18	Total number of days on UI benefits after the exercise of the option right	87
1.19	Starting date distribution	88
1.20	Impact on the probability of exhausting the UI right	93
2.1	Economic layoff procedure	152
2.2	McCrary test on the seniority variable at the 365 cutoff (October, 2011 - September 2014)	155
2.3	McCrary test on the seniority variable at the 365 and 730 days cutoffs for the two periods of interest	156
2.4	Evolution of the log difference in density at one and two years (2009-2014)	156

2.5	Evolution of the share of workers laid-off with a seniority lying in the bunching window (2010-2013)	158
2.6	McCrary test on the seniority variable at the 365 days cutoff on all open-ended contracts (excluding workers eligible for the CSP) (France, 2011-2014)	159
2.7	McCrary test on employers who did not offer the CSP	160
2.8	Tenure distribution of all eligible laid-off workers (France, 2011-2014)	160
2.9	Distribution of the proportion of full-time workers with respect to seniority (France, 2011-2014)	162
2.10	Distribution of the education level with respect to seniority (France, 2011-2014)	163
2.11	Distribution of the level of qualification with respect to seniority (France, 2011-2014)	163
2.12	Distribution of the level of previous earnings with respect to seniority (France, 2011-2014)	164
2.13	Distribution of the proportion of women with respect to seniority (France, 2011-2014)	164
2.14	McCrary test by age group	165
2.15	McCrary test by education group	165
2.16	McCrary test by qualification group	166
2.17	McCrary test by working time group	166
2.18	McCrary test by gender	167
2.19	McCrary test by firm's size group	167
2.20	McCrary test by sector	168
2.21	McCrary test by sector	168
2.22	Layoff order within the same layoff plan with respect to the distance from the cutoff (October 2011-September 2014)	169
2.23	Proportion of workers laid-off after a first layoff right after one year of tenure among workers laid-off in the same redundancy plan (France, October 2011-September 2014)	170
2.24	Notch in the budget set	172
2.25	Empirical and counterfactual distributions of seniority at layoff (October 2011-September 2014)	173
2.26	Empirical and counterfactual distributions of seniority at layoff, October 2011-September 2014, 2y cutoff	174
2.27	Empirical and counterfactual distributions of seniority at layoff, August 2009-August 2011, 2y cutoff	175
2.28	Pre and Post-CSP seniority density on the whole distribution . . .	177
2.29	Pre and Post-CSP seniority density around the notch (180-540 days)	177
2.30	Pre and Post-CSP seniority density around the notch (120-540 days)	178
2.31	Empirical and counterfactual distributions of seniority at layoff, August 2009-August 2011, 1y cutoff	179
2.32	Empirical and counterfactual distributions of seniority at layoff, 1 st gain category (October 2011-September 2014)	183
2.33	Empirical and counterfactual distributions of seniority at layoff, 2 nd gain category (October 2011-September 2014)	184
2.34	Empirical and counterfactual distributions of seniority at layoff, 3 rd gain category (October 2011-September 2014)	185

2.35	Empirical and counterfactual distributions of seniority at layoff, 4 th gain category (October 2011-September 2014)	186
2.36	Pre and Post-CSP seniority density around the notch by gain category	187
2.37	Magnitude of the bunching by gain × education categories	188
2.38	Magnitude of the bunching by gain × age categories	189
2.39	Magnitude of the bunching by gain × skills categories	190
2.40	Magnitude of the bunching by gain × working times categories	191
2.41	Magnitude of the bunching by gain × gender categories	191
2.42	Magnitude of the bunching by gain × firm size categories	192
2.43	Magnitude of the bunching by wage half	192
2.44	Effet of CSP on paid unemployment spell duration	201
2.45	Effect of CSP on paid unemployment duration by gain category	202
2.46	Evolution of the rate of labour disputes brought to court	211
2.47	Evolution of the rate of labour disputes brought to court (Base 100)	212
2.48	Comparison of the zero-inflated and standard negative binomial models	221
3.1	Share of fixed-term contracts of less than one month in total hiring (France, 2000-2017)	251
3.2	Probability of UI registration conditional on separation (Post-reform, 28-months base reference period)	255
3.3	Probability of UI registration conditional on separation (Pre-reform, 22-months base reference period)	255
3.4	Probability of UI registration conditional on separation (Pre-reform, 28-months base reference period)	256
3.5	Probability of UI registration conditional on separation (Post-reform, 22-months base reference period)	256
3.6	Probability to go from employment to non-employment	257
3.7	Probability to go from employment to registered unemployment	258
3.8	Transition probability from employment to non-employment, restricted to full-time workers	258
3.9	Yearly evolution of the number of 4-month contracts relative to 6-month contracts (end of fixed-term contracts)	260
3.10	Within sector change in the number of 4-month contracts relative to 6-month contracts (end of fixed-term contracts)	261
3.11	Difference in the share of each sector between firms with a high relative increase in 4-month contracts and all other firms (end of fixed-term contracts)	263
3.12	Yearly evolution of the number of 4-month contracts relative to 6-month contracts (economic layoffs)	264
3.13	Yearly evolution of the number of 4-month contracts relative to 6-month contracts (personal dismissals)	264
3.14	Yearly evolution of the number of 4-month contracts relative to 6-month contracts (resignations)	265
3.15	Within sector change in the number of 4-month contracts relative to 6-month contracts (economic layoffs)	265
3.16	Within sector change in the number of 4-month contracts relative to 6-month contracts (personal dismissals)	266

3.17	Within sector change in the number of 4-month contracts relative to 6-month contracts (resignations)	266
3.18	Yearly evolution of the number of 4-month contracts relative to 6-month contracts (end of trial period)	270
3.19	Yearly evolution of the number of 4-month fixed-term contracts relative to 6-month fixed contracts for legal, accounting, management, architecture, engineering, technical testing and analysis activities .	271
3.20	Yearly evolution of the number of 4-month fixed-term contracts relative to 6-month fixed contracts for transportation activities . .	272
3.21	Yearly evolution of the number of 4-month fixed-term contracts relative to 6-month fixed contracts for real estate activities	272
3.22	Yearly evolution of the number of 4-month fixed-term contracts relative to 6-month fixed contracts for human health activities . .	273
3.23	Share of contracts among contracts of 1 year or less (all separations)	274
3.24	Share of contracts among contracts of 1 year or less (economic layoffs)	274
3.25	Share of contracts among contracts of 1 year or less (other dismissals)	275
3.26	Share of contracts among contracts of 1 year or less (end of fixed-term contract)	275
3.27	Share of contracts among contracts of 1 year or less (end of temporary job)	276
3.28	Share of contracts among contracts of 1 year or less (voluntary resignations)	276
3.29	Share of 1 to 30-day contracts among contracts of 1 year or less .	277
3.30	Share of 31 to 60-day contracts among contracts of 1 year or less .	277
3.31	Share of 61 to 90-day contracts among contracts of 1 year or less .	278
3.32	Share of 91 to 120-day contracts among contracts of 1 year or less	278
3.33	Share of 121 to 150-days contracts among contracts of 1 year or less	279
3.34	Mc Crary test contract ending date distribution	281
3.35	Histogram of the contract ending date frequencies	282
3.36	Mc Crary test contract ending date distribution one year before the reform (2008)	282
3.37	Probability of opening a UI right	284
3.38	Distribution of the female proportion with respect to contract ending date	284
3.39	Distribution of the proportion of fulltime workers with respect to contract ending date	285
3.40	Distribution of the establishment size with respect to contract ending date	285
3.41	Distribution of the daily wage with respect to contract ending date	286
3.42	Distribution of the education level with respect to contract ending date	286
3.43	Magnitude of the difference in covariates at the cutoff	287
3.44	Impact of UI eligibility on employment probability 6 months after	288
3.45	Impact of UI eligibility on unemployment probability 6 months after	288
3.46	Impact of UI eligibility on labour market participation probability 6 months after	289

