London School of Economics and Political Science

Essays in Unemployment Insurance

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London, May 2022

A thesis submitted to the Department of Economics of the London School of Economics for the degree of Doctor of Philosophy.

Declaration

I certify that the thesis I have presented for examination for the PhD degree of the London School of Economics and Political Science is solely my own work.

I declare that my thesis consists of 50,000 words.

Statement of Inclusion of Previous Work

I confirm that Chapter 3 is a revised version of previous work I submitted for obtaining a Masters of Research from the London School of Economics and Political Science in 2018.

Acknowledgments

I am extremely grateful to my supervisor Camille Landais for his invaluable support, guidance, and for his insightful comments and suggestions throughout the PhD. I would also like to thank my advisors, Johannes Spinnewijn and Xavier Jaravel for their time, their advice and their precious comments. They played a key role in the development of this thesis.
I would also like to thank the whole Public and Labour Economics groups at the London School of Economics and my peers, Bilal Tabti, Daniel Albuquerque, Thomas Brzustowski, Alice Lapeyre, Tillman Höening, Heidi Thysen, Nicolas Chanut, Akash Raja, Edoardo Leonardi, Jack Fisher, Tiernan Evans, Gabriel Leite-Mariante, William Parker and many others for their help, their time and their recommendations.

I am also grateful to the ESRC and the STICERD for funding my research, and to Pôle Emploi and to the Spanish Dirección General de Ordenación de la Seguridad Social for providing me with the datasets that made this whole thesis possible.

Abstract

This thesis consists of three chapters.

In the first chapter (Optimal Unemployment Insurance with Liquidity Provision), I exploit the joint provision, in many countries, of Unemployment Benefits (UB) and a Severance Package (SP) post redundancy to suggest a new and welfare enhancing organisation of Unemployment Insurance (UI). To do so, I use French administrative data and a combination of regression kink and discontinuity designs. While the SP and UB paid early in the unemployment spell should have roughly the same consumption smoothing value, I show that UB are a lot more distortive. All of my results point in the same direction and indicate that the SP would be a good substitute for UB paid early in the unemployment spell. As a consequence, the optimal UI organisation should be made of a SP at the onset of unemployment followed by a waiting period with no UB, and by the payment of UB in case of long-term unemployment.

In the second chapter (Can Unemployment Insurance Reduce Job Stability?) I analyse the impact of a specific eligibility condition on unemployed individuals' incentives to accept short-time contracts. The analysis relies on a reform in France that reduced the necessary number of days worked to open eligibility to UI from 122 to 30 days. By exploiting administrative data and a regression kink design strategy, I show that this reform made unemployed more likely to accept short-time contracts, and to repeat the unemployment experience after their end date. I estimate this increased likelihood to become unemployed again to generate an additional cost per unemployed of 500 to 700€.

In the third chapter (There is Only One First Time: Behavioural Responses and Unemployment Experience) I explore the impact of one dimension of heterogeneity, namely unemployment experience, on unemployed individuals' behavioural

response to UB. This paper leverages a key advantage of the MCVL, a Spanish administrative dataset: it tracks the full employment history of a 4% randomly selected sample, *i.e.* that it provides full details about every job held by these individuals since their very first employment contract. I then analyse, through a regression kink design strategy, a discontinuity in the UB schedule, and differentiate this approach as a function of individuals' unemployment experience. Only individuals in their very first unemployment spell significantly respond to the exogenous change in the UB level. This finding is robust to a large set of validity checks and to multiple specifications.

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Chapter 1

Optimal Unemployment Insurance with Liquidity Provision

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Abstract

Many countries simultaneously provide Unemployment Insurance (UI) and a Severance Package (SP) upon redundancy. This paper is the first to leverage this feature and analyse the interactions between this SP payment at the onset of unemployment and the optimal unemployment benefits (UB) profile. Using French administrative data and a combination of regression discontinuity and kink designs, I show that the optimal UI should be made of a SP only at first, and then provide UB to insure individuals in case of long-term unemployment, thus leading to an increasing benefits profile through the presence of a waiting period. This conclusion results from a combination of findings. First, UB paid early in the unemployment spell have a significantly larger moral hazard cost compared to the SP, while these two instruments have similar consumption smoothing values. Secondly, providing less UB early in the spell affects the dynamic selection into long-term unemployment. It screens out the *high-types*, therefore raising the value of benefits paid later through a better targeting. Finally, I show suggestive evidence indicating that the planner should push the steepness of the optimal UB profile all the way to a corner solution. The optimal design of UI should then be made of a SP at the onset of unemployment and of UB only in case of long-term unemployment.

Keywords: Unemployment insurance; Dynamic Policy; Regression Discontinuity Design; Sufficient Statistics.
J.E.L. codes: H20; J64; J65.

1.1 Introduction

Many countries simultaneously provide Unemployment Insurance (UI) and a Severance Package (SP) upon redundancy. While impacting one another, these two programs have been used and developed separately by policy-makers. The payment of a SP early in the unemployment spell might nevertheless exert a deep influence on the optimal unemployment benefits (UB) profile.

To analyse the interactions between these two programs, this paper exploits the traditional Baily-Chetty model for social insurance and augments it with a new instrument, namely the provision of liquidity through a SP. I leverage this evidence-based framework to identify the optimal combination of SP and UB over time. In doing so, this paper aims to bridge two different strands of the literature. The first one analyses the optimality of key UI parameters – UB and Potential Benefits Duration (PBD) - in a stylised framework with a flat benefits profile (Gruber [1997], Chetty [2006], Chetty [2008], Landais [2015], Lalive et al. [2006], Schmieder et al. [2012b]). Some papers within this branch even use the provision of a SP as a source of identification to back-out some theoretical moments (Chetty [2008], Card et al. [2007]). However none of them draw any conclusion regarding the impact of this SP provision on the optimal evolution of benefits over time. The second branch focuses on optimal dynamic policies (Shavell and Weiss [1979], Hopenhayn and Nicolini [1997], Werning [2002], Shimer and Werning [2008], Hopenhayn and Nicolini [2009], Pavoni [2009], Kolsrud et al. [2018], Lindner and Reizer [2020]). To the best of my knowledge, it does not – in the context of UI – incorporate and empirically evaluate the impact of a SP provision at the onset of an unemployment spell on the optimal benefits profile. Within this branch, the closest papers to my approach are the Shimer and Werning [2008] and the Kolsrud et al. [2018]. Based on a sequential job search model *a la* McCall [1970], Shimer and Werning [2008] study two distinct components of the optimal UI, insurance and liquidity. In a general case with a CRRA utility function, they conclude that the optimal benefits profile is slightly increasing. Their results are nevertheless hard to connect to the data and require numerical simulations. In addition, their stationary approach cannot capture one simple but key fact. The characteristics of the pool of unemployed evolve over an unemployment spell, a force that will influence the consumption smoothing value of UB and therefore directly impact the optimal steepness of the benefits profile. Such dynamic selection plays a decisive role in the targeting of benefits, not only in the context of UI but for social insurance in general (Deshpande and Li [2019], Homonoff and Somerville [2021]). The work by Kolsrud et al. [2018] does not suffer from these drawbacks. They develop a state of the art general framework that takes into account multiple dimensions of heterogeneity and allows for the identification of clear sufficient statistics that can be empirically quantified. They nevertheless do not explicitly take into account the co-existence of SP and UB. My results are in line with these two papers and in favour of an increasing benefits profile.

The approach in this paper is based on the rich dynamic model of unemployment developed in Kolsrud et al. [2018] and based on Baily [1978] and Chetty [2006]. Compared to these papers, the planner is provided with an additional instrument - the payment of a SP at the onset of unemployment. The optimal mix between the SP and UB over the unemployment spell is captured through a simple set of optimality conditions. In the spirit of the Baily-Chetty formula, these optimality conditions capture a simple trade-off between the relative Consumption Smoothing (CS) gains and Moral Hazard (MH) costs of the corresponding instruments.
The consumption smoothing side captures how effective the different instruments are at maintaining a certain consumption level when individuals face an unemployment shock. The higher the concavity of the utility function, *i.e.* the more risk averse individuals are, the more valuable this consumption smoothing will be at the optimum. The moral hazard component captures individuals' behavioural response to a given instrument. By possibly changing a set of relative prices, an instrument will affect individuals' search effort which in turn will impact the government budget constraint through a so-called fiscal externality. The intuition underlying the SP versus UB trade-off is straightforward. The SP is provided to individuals made redundant, no matter how long they stay unemployed. It is, in that sense, unconditional. Conversely, UB keep being provided to individuals only if they stay unemployed. This implies that the former should generate a smaller moral hazard cost, but should also not perform as well as UB in terms of consumption smoothing (for example because unemployed tend to be too optimistic

(Spinnewijn [2015])).

The empirical part of this paper not only quantifies these sufficient statistics for the different instruments but also analyses how they evolve as a function of the level of the other instruments. By doing so, it offers an attempt to push the sufficient statistics approach one step further and to go beyond local recommendations. The sufficient statistics are identified through a combination of regression discontinuity and kink designs. They provide new insights about how UB should evolve over the unemployment spell when explicitly taking into account the payment of a SP. The effect of this SP is captured by exploiting a tenure-related rule. For individuals made redundant from a permanent contract, every additional year of tenure increases the amount of the SP by a fifth of a monthly wage[1]. This generates a discontinuity in the level of the severance package that can be leveraged as a source of identification through a Regression Discontinuity Design (RDD). The effect of UB provided over the unemployment spell is captured through a Regression kink Design (RKD) strategy exploiting kinks in the schedule of UB. Despite the presence of a flat benefits profile in France, the existence of a waiting period implicitly generates a kink that allows to separate the effect of short and long term UB.

This paper provides the following main results.

First, I obtain a clear ranking between the moral hazard cost of the different instruments. The MH cost of early UB is about 40% larger compared to the MH cost of the SP, while - over a *short enough* period - these two instruments should have similar consumption smoothing properties. This suggests that the planner should provide more SP and less UB at the beginning of an unemployment spell, thus leading to an increasing benefits profile.

The MH cost of UB is also shown to decrease over the unemployment spell, in line with Kolsrud et al. [2018]. While surprising, this result is partly explained by the fact that unemployed individuals do not seem to be forward looking. A higher level of benefits paid later in the spell is indeed shown to have no effect on the duration spent in short time unemployment.

[1]This holds for every year of tenure from one to ten. Every year above 10 raises the amount of the severance package by an additional 2/15 of a monthly wage.

Secondly, I show that changing the level of UB paid at the beginning of the unemployment spell will have a significant effect of the dynamic selection into long-term unemployment. Less UB early in the spell will screen-out relatively more the *high-types*, therefore increasing the consumption smoothing value of UB paid later on. This pushes in favour of an even steeper benefits profile.

Finally, this paper explores how the different MH costs evolve as a function of the level of the other instruments. It is shown that an increase in the level of the SP or in the level of benefits insuring against the risk of long term unemployment would both affect the relative moral hazard cost of the other instruments in favour of a steeper benefits profile. In other words, all the mechanisms point in the same direction and suggest that a corner solution would be optimal. The planner should provide a SP only at first, and then provide unemployment benefits to insure individuals in case of long-term unemployment. This amounts to implementing a schedule with a waiting period.

This paper contributes to several literatures. With the development of the sufficient statistics approach and the increasing access to high quality administrative data, a large body of the literature focused on the optimal UB level in a stylised framework with a flat benefits profile (Chetty [2008], Landais [2015], Card et al. [2015a]). The findings of this literature often are that UB are too generous as the MH cost outweighs the CS gains. The optimal duration of benefits has also been the subject of a lot of attention (Lalive et al. [2006], Schmieder et al. [2012a]). Interestingly Schmieder et al. [2012b] show, based on German administrative data, that the (CS) gains from extending UB during a recession more than compensate the possible additional (MH) cost. Despite sometimes directly using severance packages as a source of identification (Card et al. [2007], Chetty [2008]), this literature never analysed how such liquidity provision interacts with the profile of unemployment benefits over the unemployment spell.
Another branch, which was first purely theoretical and has only recently become more empirical, studies the optimal UB profile (Shavell and Weiss [1979], Hopenhayn and Nicolini [1997], Werning [2002], Pavoni [2009], Kolsrud et al. [2018], Lindner and Reizer [2020]). The estimates of the behavioural responses to benefits contained in this paper are well in line with those two literatures. Explicitly taking into account a SP provision at the onset of unemployment allows to bring new and interesting insights regarding

the optimal organisation of benefits over time, filling a gap between those two sides of the UI literature.

Note that this paper will be focused on the workers' perspective, and therefore will not speak to the literature on UI and experience rating (Feldstein [1976], Feldstein [1978], Millard and Mortensen [1997], Wang and Williamson [2002], Cahuc and Malherbet [2004] Mongrain and Roberts [2005]).

The remainder of this paper proceeds as follows. Section 1.2 briefly presents the theoretical framework, and provides details about the optimality conditions characterising the optimal combinations between the different instruments over the unemployment spell. It also provides insights about how UB might affect the dynamic selection into long-term unemployment. Section 1.3 describes the data and the UI system in the French context. Section 1.4 explains the empirical strategies - the RDD and RKDs - developed to measure the policy relevant moments, and presents the estimates for the MH costs. Section 1.5 focuses on the dynamic selection channel, while section 1.6 challenges the sufficient statistics approach and tries to go beyond local recommendations. Section 1.7 presents validity checks, and section 1.8 concludes.

1.2 Model

This section briefly presents a the dynamic model of unemployment leveraged in this paper. This model incorporates multiple dimensions of heterogeneity, for example in terms of preferences or assets. Its dynamic aspect allows to identify the optimal profile of unemployment benefits in a non stationary environment.

1.2.1 Setup & Dynamic Sufficient Statistics

The model relies on the seminal work of Baily [1978] later on generalised by Chetty [2006] and Kolsrud et al. [2018]. It is a partial equilibrium model with a *continuum* of heterogeneous agents of mass 1. Time is discrete and goes from t=0...T-1. Agents start unemployed at time 0. They face a time-consistent policy $\mathscr{P}$ that includes both UB b_t at time t and the payment of a SP upon redundancy A_0. Having such a SP as an instrument for the planner is a key addition compared to Kolsrud et al. [2018].

The planner maximises the sum of utilities across heterogeneous individuals subject to a budget constraint $G(\mathscr{P})$. This latter is made of the sum of taxes τ collected on employed individuals, the SP paid at the onset of unemployment A_0, and of the UB paid throughout unemployment b_t, s.t. $G(\mathscr{P}) = (T-D)\tau - A_0 - \sum_{t=0}^{T-1} b_t S_t$. D represents the duration spent in unemployment, and corresponds to the sum of the survival rates (S_t) at each duration, such that $D = \sum_{t=0}^{T-1} S_t$. Her optimisation program therefore looks as follows:

$$\underset{\boldsymbol{b},A_0}{max}\ W(\mathscr{P}) = \int V_i(\mathscr{P})di + \lambda[G(\mathscr{P}) - \bar{G})] \tag{1.1}$$

Where $\boldsymbol{b} = (b_0, b_1, ..., b_{T-1})$ corresponds to a vector of all the benefits possibly paid during an unemployment spell, $\bar{G}$ is an exogenous revenue constraint and λ represents the Lagrange multiplier on the government budget constraint.

In practice most UI systems implement a two-tier benefits profile. This can easily be captured within the above model by defining benefits b_1 (resp. b_2) for a duration D_1 (resp. D_2[2]) - which I will refer to as short (resp. long) term unemployment in what follows. Note that the case of a flat benefits profile can also be captured by setting $b_1 = b_2 = \bar{b}$. A_0 does not depend on the length of the unemployment spell and is, in that sense, unconditional. Conversely, individuals will keep receiving UB only if they stay in the same state of the world, *i.e.* unemployed. By being unconditional, severance packages generate less behavioural distortions and therefore perform better in terms of moral hazard. This unconditional aspect nevertheless implies that they are not as good at insuring individuals against the risk of staying unemployed, and hence do not perform as well as UB in terms of consumption smoothing. This trade-off is captured in the following proposition.

[2] $D_1 = \sum_{t=0}^{B_1} S_t$ and $D_2 = \sum_{t=B_1+1}^{T-1}$

Proposition 1: *Consider an unemployment policy $\mathscr{P}$ where the planner can use three distinct instruments (A_0, b_1, b_2). Assuming differentiability and an interior solution for every instrument, the optimal combinations between the instruments are captured by the following optimality conditions:*

$$\frac{CS_i(A_0, b_1, b_2)}{CS_j(A_0, b_1, b_2)} = \frac{MH_i(A_0, b_1, b_2)}{MH_j(A_0, b_1, b_2)} \qquad \forall\ i, j \in \{A_0, b_1, b_2\} \tag{1.2}$$

Where CS_i and MH_i respectively represent the Consumption Smoothing gains and Moral Hazard cost arising from a small increase in the level of instrument i. These two components are fully defined in Appendix A.

Proof: Simply combine the FOCs from equation 1.1 ■

The CS term captures how good a given instrument is at helping individuals to maintain a certain consumption level, *i.e.* to limit the consumption drop, despite the unemployment shock and the corresponding income loss. The MH term captures the consequences of using such instrument. By changing a set of relative prices, the instrument will affect individuals' behaviour - their search effort - which in turn will impact the planner's budget constraint through a fiscal externality. This is the source of the MH cost. The optimum will be reached when these relative effects are equalised across the different instruments. If A_0 has similar CS properties as b_1 - which is likely to be the case if D_1 is *short enough* - and has a smaller MH cost, its relative level should be increased at the optimum. Note that very often in the UI literature, the CS term is re-written - through a first order Taylor approximation - as a consumption drop weighted by a coefficient of relative risk aversion (CRRA). Having a ratio of CS terms, the LHS of equation 1.2 in proposition 1 could easily be re-written as a ratio of consumption drops since the CRRA coefficients would cancel out.

While I do not have access to the necessary data to directly evaluate the left hand side of equation 1.2, a growing literature relying on very detailed bank account data (Kolsrud et al. [2018], Ganong and Noel [2019]) provides a good sense of the CS gains provided by UB. Despite such limitation, note that the model developed in this section can be used to identify a simple set of conditions indicating how the left hand side would evolve as a function of certain parameters, as explained in the following subsection.

1.2.2 Benefits paid early in the unemployment spell and dynamic selection

Most of the literature on UI so far has focused either on the optimal level of UB or on their optimal evolution over the unemployment spell. The dynamic UI models nevertheless very often consider the case of a stationary environment which does not allow to take into account the effect UB can have on the dynamic selection into long-term unemployment. Absent this channel, Shimer and Werning [2008] conclude that the optimal unemployment insurance should include a slightly increasing UB profile over time. They further highlight that the gains arising from such schedule compared to a simple flat profile would be marginal. But if providing less UB early in the unemployment spell were to screen-out of long-term unemployment individuals valuing UB relatively less, this would increase the CS value of benefits paid later on and provide a strong incentive to implement an increasing benefits profile. This dynamic selection channel is - by nature - completely missing from models displaying a stationary environment. The next proposition further illustrates the potential importance of such channel.

Proposition 2: *Consider an initial policy $\mathscr{P}$ made of three instruments: $\mathscr{P} = (A_0, b_1, b_2)$.*

Imagine a reform decreasing the level of unemployment benefits paid early in the unemployment spell: $db_1 < 0$. Call $\mathscr{P}' = (A_0, b_1^-, b_2)$ this new policy, with $b_1^- = b_1 + db_1$.

In a case where utility is separable in consumption and search effort, a sufficient condition for such reform to increase the consumption smoothing value of benefits paid later in the unemployment spell is that it increases the proportion of low-types, i.e. the proportion of individuals with a large marginal utility from consumption, in long-term unemployment.

Proof: See the Appendix A ■

The intuition underlying this proposition is straightforward. A decrease in UB paid early in the unemployment spell will (weakly) reduce the consumption level of every unemployed individual. This first effect will necessarily increase the CS value of future UB. Such reform would also change the composition of the pool of individuals receiving these benefits. If it pushes away from unemployment individuals with a low marginal utility from consumption (high types), the resulting pool of unemployed will, on average, value UB even more. These two effects would therefore go in the same direction, increasing the consumption smoothing value of future benefits.

The above proposition is very powerful. It allows to focus on the effect of a change in b_1 on the survival rate of different well defined groups to know how the consumption smoothing value of b_2 will evolve. The empirical analysis contained in the next section will make use of this property.

1.3 Context and Data

To implement the above formulae and measure the relative incentive costs of severance packages and unemployment benefits, this paper exploits multiple sources of identification offered by the french UI system. These variations and the data used are presented in this section.

1.3.1 Institutional Background

France has a rich institutional context with a multiplicity of rules that can be exploited to reach causal identification. The key advantage is that these rules can be leveraged to identify the effect of the SP and of UB, both for short and long-term unemployment.

Individuals made redundant from a permanent contract and with at least a year of tenure are for example entitled to the payment of a SP. Such payment is mandated by the state. Its level corresponds to a fifth of a monthly wage per year of tenure[3].

[3] This is true up to 9 years of tenure. From 10 years onwards, 2/15th of a monthly wage are added to the amount of the severance package per additional year of tenure.

An individual made redundant after working for five years within a company under a permanent contract would hence receive a SP representing a monthly wage. The level of this SP is a discontinuous function of tenure (see figure 1.B.2 in Appendix B), a feature that can be leveraged through a Regression Discontinuity Design (RDD) strategy. The next section provides further details about the different estimation strategies.

A fifth of a monthly wage corresponds to a legal minimum. Workers can nevertheless receive more than this level, either through some bargaining at the individual level or due to the existence of more generous collective agreements. The amount of the SP above and beyond the legal minimum ($\tilde{A}$) would lead to a Waiting Period (WP), *i.e.* a certain number of days at the beginning of the unemployment spell over which the individual would not receive any UB. The length of this WP (B_1) is determined by the ratio between the amount of the SP above and beyond the legal minimum and the individual's daily wage. This WP has a maximum length of 75 days, therefore creating a kink in its schedule that can be leveraged as a source of identification through a Regression Kink Design (RKD) strategy (see figure 1.B.4 in Appendix B). Despite the presence of a flat UB profile in France, this kink in the WP can be used to back-out the effect of UB paid early in the unemployment spell (b_1 - see figure 1.B.6 in Appendix B).

Finally, the level of UB is determined through a complex formula that contains three distinct kinks - each of which could potentially be leveraged through a RKD strategy to identify the effect of UB on unemployment length. Figure 1.B.1 in Appendix B illustrates how the replacement rate evolves as a function of the daily wage prior unemployment. It varies between 57.4 and 75%, and displays three distinct kinks. The empirical analysis will make use of the kink on the right hand side since it is also where the vast majority of the observations are located. This later will be used to identify the effect of the UB paid after the end of the WP - b_2 - on the unemployment length.

The UI system is subject to a reform every three years. Some of them are mostly incremental, adjusting some parameters at the margin, while others implement significant changes. To maintain a constant environment, I will consider the case of France between 2009 and 2013[4].

[4]For the year 2009, I consider unemployment spells that started after the 1st of April 2009. I exclude the unemployment spell that started in 2014, even though the 2014 reform occurred

1.3.2 Data

This article relies on an administrative dataset called the FHS-D3. It corresponds to a representative panel of 10% of the unemployed population between 2008 and 2017. The key strength of this dataset is that is contains very precise information about both the level of UB and of the SP.

The D3 contains all the details about the unemployment spell, e.g. when it starts and ends, information about the entitlement length etc. It also provides information about the exact level of the replacement rate and therefore about the level of UB received by individuals. Importantly, it also contains details about the level of the SP received by individuals if this later exceeds the legal minimum. It therefore allows to identify the length of the corresponding waiting period, a key element to back-out the effect of UB at the beginning of the unemployment spell. It nevertheless does not provide any information about the level of the SP if this later corresponds to the legal minimum. I could nevertheless combine this dataset with the information about all the employment contracts used by individuals to register as unemployed and prove their eligibility to UI. I therefore know for how long and under which contract(s) individuals worked prior unemployment. This allows to know whether individuals were entitled to the payment of a SP, and if so, to compute its level.

The FHS (*Fichier Historique Statistique*) contains a large set of demographic details. It provides information about individuals' age, their education, marital status, number of children, geographic location etc.
Combined together, these datasets provide very precise information about individuals, their unemployment spell and the level of the SP they received at the onset of unemployment. These details will be used in the next section to identify the effect of the SP (A_0) and UB (b_1 and b_2) on individuals' behaviour over their unemployment spell. Compared to other datasets, the key advantage of the FHS-D3 results from the multiple sources of identification it includes. Not only does it contain multiple kinks allowing to identify the effect of UB paid at different times during the unemployment spell on unemployment length, but the discontinuities in the level of the SP provided also allow

later in the year to avoid any anticipatory effect that could have affected individuals' behaviour.

to measure the effect of this liquidity provision early in the unemployment spell on unemployment duration.

Unemployment will be defined as the time between registration and de-registration from the Public Employment Office (*Pôle Emploi*). Using the day of the last employment contract is an alternative that does not affect the main results. Table 1.B.1 in the Appendix B provides summary statistics on unemployment and demographic variables. The average individual in my sample is a man of 36 years old, married, with children and with a high school degree. He receives a SP of almost €4,000, faces a WP of almost two months and spends almost a year and a half unemployed.

1.4 Duration Responses

This section analyses unemployment responses to changes in the SP and UB paid at different times during the unemployment spell. It starts by providing further details about the identification strategies and then presents the main results.

1.4.1 Identification Strategies

Regression Discontinuity Design and Severance Packages

As explained previously, the level of the severance package jumps by a fifth of a monthly wage for every additional year of tenure: $A_0 = \frac{W_m}{5} * T$, where W_m represents the monthly wage and T is the tenure, expressed in years ($T \in \mathbb{N}$). This can be exploited through a RDD strategy. In order to increase the power in the different regressions, a common measure of distance (R) with respect to every additional year of tenure, *i.e.* every jump can be defined. With such measure individuals with say 300, 665 and 1395 days of tenure will all be located at -65 days of a discontinuity. This allows to group together all the jumps displayed in figure 1.B.2 in Appendix B, leading to figure 1.B.3. The following specification can then be used to estimate the effect of the liquidity provision on unemployment length:

$$\mathbb{E}[Y|_{R=r}] = \alpha_{A_0} + \sum_{p=1}^{\bar{p}} \Big[\gamma_{p,A_0} r^p + \tau_{p,A_0} * \mathbb{1}(r \geq 0)\Big] \quad \text{Where } |r| \leq \mathscr{B}_{A_0} \tag{1.3}$$

Where R - for Running variable - corresponds to the aforementioned measure of distance, and $\mathscr{B}_{A_0}$ is a given bandwidth for the instrument A_0. Y represents the outcome of interest. In the analysis, Y will be the duration of either short or long term unemployment (resp. D_1 and D_2). The coefficient of interest - τ_{p,A_0} - measures by how much Y jumps at the discontinuity. The elasticity of Y with respect to the SP can then be defined as $\varepsilon_{Y,A_0} = \frac{\hat{\tau}_{1,A_0}}{\tilde{\tau}} * \frac{\bar{A}_0}{\bar{Y}}$, where $\tilde{\tau}$ is the estimated coefficient from the same regression where the level of the SP is the outcome variable, and $\bar{A}_0$ and $\bar{Y}$ represent respectively the average of the SP and of the outcome of interest in a bandwidth $\mathscr{B}_{A_0}$ around the discontinuity. Of course, while located at the same distance to a jump, individuals with tenures of say, 300 and 1395 days, will potentially be very different. Regressions will therefore contain a '*years of tenure*'[5] fixed effect in order to take such heterogeneity into account.

Regression Kink Design and Unemployment Benefits

This subsection will explain how I identify the effect of benefits covering both short and long term unemployment. It provides details about how a WP can be leveraged to separate the effect of b_1 and b_2 despite the presence of a flat benefits profile.

Insurance against long term unemployment The kinks displayed in figure 1.B.1 can be exploited in a RKD to identify the effect of UB on unemployment length. I will use the kink located on the right hand side (dashed line) as most of the observations in my dataset are located around this kink. Such change in slope can be leveraged in a RKD by using the following specification:

$$\mathbb{E}[Y|_{R=r}] = \alpha_{b_i} + \sum_{p=1}^{\bar{p}} \Big[\gamma_{p,b_i} r^p + \tau_{p,b_i} r^p * \mathbb{1}(r \geq 0)\Big] \quad \text{Where } |r| \leq \mathscr{B}_{b_i} \tag{1.4}$$

Where the notations are similar to equation 1.3 but adjusted for the different scenarii. b_i with $i \in \{1, 2\}$ represents either short or long term unemployment. Moreover note that the coefficient τ_{p,b_i} now identifies a change in slope and no longer a jump. Elasticities

[5] This year of tenure fixed effect will lead to compare individuals on both side of the same jump, and will therefore avoid comparisons across jumps. Perhaps a more precise denomination would be an across year of tenure fixed effect, as in practice it will lead to compare individuals between 1.49 and 2.5 years of tenure, individuals between 2.51 ans 3.49 years of tenure etc.

can be obtained using the same method explained for severance packages. The change in slope for the benefits level is now deterministic and therefore does not need to be estimated (Card et al. [2015b]). Such a source of variation is very often used in the UI literature. The main innovation comes from the method used to differentiate the effect of benefits provided for short and long term unemployment.

Insurance against short term unemployment Despite the presence of a flat benefits profile, the existence of a WP can be leveraged to separate the effect of benefits covering short versus long term unemployment. A WP indeed creates the equivalent of a two tier benefits profile, with first no benefits at all ($b_1 = 0$) and then a positive level of UB ($b_2 > 0$, see figure 1.B.5 in the Appendix B). A shorter WP can be seen geometrically as similar to a higher level of benefits insuring against short term unemployment - in the spirit of Landais [2015]. Figure 1.B.6 in the Appendix B illustrates this reasoning. This implies that the very existence of a WP provides a source of variation that can be leveraged to separately identify the effects of b_1 and b_2 on some key outcomes of interest. In practice a change in the duration of the WP will also affect the exhaustion date for benefits. This effect can nevertheless be neglected for two reasons. First, the next section will show that individuals do not seem to be forward looking. Second, table 1.B.1 in the Appendix B shows that the average entitlement length is of about two years. Combined, these two elements indicate that ignoring the effect of the WP on the exhaustion date is perfectly reasonable. Such WP can therefore be used to back-out the effect of UB covering short term unemployment. It has a maximum length of 75 days, creating a kink in its schedule that provides the source of identification needed (see figure 1.B.4 in the Appendix B). This later can also be exploited through a RKD.

1.4.2 Moral Hazard Cost Estimates

The rest of this section presents the main empirical results and analyse their consequences for the optimal UB profile.

Table 1.1 below provides the estimates of the MH costs for the three instruments (A_0, b_1 and b_2) and the corresponding elasticities. The associated figures are relayed to the Appendix B, see figures 1.B.7, 1.B.8 and 1.B.9. Table 1.B.2 in the Appendix B also varies the set of controls used in every regressions and shows that the coefficients of interest are stable, even to the inclusion of a very strict set of controls and fixed effects.

Table 1.1: Main Results - Full Sample Analysis

Source of variation	**MH**	$\varepsilon_{D_1,\cdot}$	$\varepsilon_{D_2,\cdot}$
$\boldsymbol{b_1}$	1.343***	1.270***	0.166***
	(0.294)	(0.021)	(0.036)
N	21,409	21,409	21,409
$\boldsymbol{b_2}$	1.187**	-0.0770	1.185**
	(0.536)	(0.149)	(0.535)
N	23,152	23,152	23,152
$\boldsymbol{A_0}$	0.958**	-0.0050	0.250**
	(0.454)	(0.106)	(0.120)
N	15,936	15,936	15,936

Notes: This table contains the point estimates obtained from RKDs (b_1 and b_2) and RDD (A_0). See equations 1.3 and 1.4. The regressions include a large set of controls and fixed effects. The controls take into account the age, gender, the marital status, a dummy for the presence of children, education, a dummy for whether the individual previously worked part-time, a dummy for whether the individual has already been unemployed in the past. The fixed effects control for the year, geographic location, the reason for the redundancy and the year of tenure. The link between the MH cost and the elasticities is explained in Appendix A. Standard errors are obtained through a bootstrap procedures with 50 replications.
*,** and *** denote significance at the 10%, 5% and 1% levels, respectively.

Various key results should be emphasised.

First, a clear ranking emerges between the three costs, with $MH_{b_1} > MH_{b_2} > MH_{A_0}$. While surprising, the result that $MH_{b_1} > MH_{b_2}$ is explained by the fact that unemployed do not seem to be forward looking. Indeed, a change in b_2 has no effect on the duration spent in short-time unemployment. This result is in line with the findings in Kolsrud et al. [2018] and suggests that an increasing benefits profile would be optimal. Under a set of assumptions[6] and as explained in section 2, the LHS of equation 1.2 can be-rewritten as a ratio of consumption drops. It is well established that the average

[6] More precisely, assuming that preferences over consumption are separable from leisure, that a first order Taylor expansion provides a reasonable approximation of the utility function (*i.e.* that higher order terms can be neglected), and that preferences are homogeneous.

consumption level falls over the unemployment spell (Kolsrud et al. [2018], Ganong and Noel [2019]), implying that $\frac{CS_{b_1}}{CS_{b_2}}$<1. On the other hand, $\frac{MH_{b_1}}{MH_{b_2}}$ is larger that one. Proposition 1 therefore confirms that benefits insuring against the risk of long term unemployment should see their level increased compared to benefits covering short time unemployment, leading to an increasing benefits profile.

Secondly, the introduction of a liquidity provision through a SP at the onset of unemployment reinforces the incentive to implement an increasing benefits profile, and provides a new margin to implement it. For a short enough period - D_1 has a maximum length of 75 days and an average length of a month and a half - the consumption smoothing gains from the SP and UB should be similar, implying that $\frac{CS_{b_1}}{CS_{A_0}} \approx 1$. On the other hand the relative cost of these instruments - $\frac{MH_{b_1}}{MH_{A_0}}$ - is significantly larger than one (1.4). Proposition 1 therefore indicates that the mix between SP and UB at the beginning of the unemployment spell is not optimal and should be modified in favour of relatively more SP and less UB, leading to a further increasing benefits profile. Note that the elasticities of unemployment duration with respect to benefits levels estimated in this article, despite being slightly smaller than the ones in Kolsrud et al. [2018], are still on the high end of the range of existing estimates (see Schmieder and Von Wachter [2016]).

Third, the conclusion about the relative levels of A_0 and b_2 is relatively less clear cut as I do not have information about the insurance value of UB. In the context of Kolsrud et al. [2018], the consumption smoothing ratio ($\frac{CS_{b_2}}{CS_{A_0}}$) would be of about 2, which would be much larger than the corresponding relative MH cost of about 1.24, suggesting that the (A_0, b_2) mix should evolve in favour of more b_2 and less A_0. For the case of the US, using the data from Ganong and Noel [2019] would lead to a similar conclusion.
All these mechanisms point in the exact same direction. The relative level of b_2, both compared to A_0 and b_1, should be increased. Similarly, the relative level if A_0 compared to b_1 should be raised, leading to an increasing benefits profile. Introducing a new instrument - a SP - therefore leads to the interesting finding that the planner would want to implement an even steeper profile compared to the case where she would only have two instruments available.

In a non-stationary environment, a change in the mix between UB paid early in the unemployment spell and SP could affect the dynamic selection into long-term unemployment. This channel is explored in the next section.

1.5 Dynamic selection into long-term unemployment and early Unemployment Benefits

The previous section demonstrated that at the optimum, the planner should provide relatively more SP and relatively less UB early in the spell (b_1). This section analyses whether a decrease in the level of UB paid early in the unemployment spell would affect the pool of individuals selecting into long-term unemployment. Proposition 2 shows that if such a change increases the proportion of low-types selecting into long-term unemployment, it will increase the CS value of benefits paid later in the spell. In order to analyse this dynamic selection mechanism, this section splits the population into two groups, high school drop-outs (low-types) versus individuals with at least a high school degree (high-types). The level of education is known to correlate with earnings, which in turn correlate with the marginal utility from consumption. In short, low-types will consume relatively less and therefore have a higher marginal utility from consumption, *i.e.* that they will value UB relatively more compared to high-types. Table 1.B.3 in Appendix B illustrates various key differences between these two groups. High school drop-outs are older, made much less while working and were more likely to work part time.

The objective is to identify how a change in b_1 would affect the proportion of low-types in long term unemployment. To analyse such effect, I will exploit the kink in the WP in a linear probability model (LPM). The estimated equation is similar to equation 1.4, but the outcome variable corresponds to a dummy variable taking a value of 1 if the individual is a high school drop-out and zero otherwise. The coefficient of interest is $-\tau_{1,b_1}$. It measures how a decrease in the level of b_1 affects the probability to be a low-type in long term unemployment, and therefore how it affects the corresponding proportion of low-types. The sample used in this section consists of all the individuals surviving to the WP. Low-types represent about 9% of this population.

Table 1.2 delivers a consistent message. A decrease in the level of benefits paid early in the unemployment spell will increase the proportion of low-types in long term unemployment. Note that the estimated coefficients measure how a decrease in one euro of b_1 would affect the aforementioned probability. If one were to decrease b_1 by 10% (about €150), this would increase the proportion of low-types by about 3.9 percentage points, from about 9 to 12.9% of the unemployed population. Early UB therefore significantly affect the dynamic selection into-long term unemployment. The response of the probability to be a low-type in long-term unemployment to the kink in b_1 is represented in figure 1.B.10, Appendix B.

Table 1.2: Dynamic Selection into Long-Term Unemployment

	No controls	Baseline controls	Full controls
$-\tau_{1,b_1}$	0.000301**	0.000265**	0.000260**
	(0.000134)	(0.000129)	(0.000128)
Year FE	✗	✗	✓
Departement FE	✗	✗	✓
End of contract FE	✗	✗	✓
Observations	37,884	32,369	32,369

Notes: This table contains the point estimates obtained from estimating equation 1.4, where the outcome of interest corresponds to a dummy variable taking a value of 1 if the individual is a high school drop-out and zero otherwise. The first column includes no controls. The second one takes into account the age, gender, the marital status, a dummy for the presence of children, a dummy for whether the individual previously worked part-time, a dummy for whether the individual has already been unemployed in the past. It also controls for the possible presence of a kink in b_2 and for a possible jump in the level of the SP. The last column adds to this list of controls year, geographic and end of contract fixed effects.
*,** and *** denote significance at the 10%, 5% and 1% levels, respectively.

By implementing an increasing benefits profile through a substitution of early UB in favour of more SP, the planner would - through a dynamic selection mechanism - increase the CS value of benefits paid later in the spell. This improved targeting of benefits would incentivise the planner to implement an even steeper UB profile. One may wonder how far should the planner push the steepness of the UB profile. The sufficient statistics approach is nevertheless not suited to answer this type of questions as it only - by design - provides local recommendations. One can nevertheless try and push this approach one step further. To do so, one has to realise that all the MH costs measured so far are functions of the level of the other instruments. It is therefore possible to analyse, for example, how the MH cost of both early benefits and the SP would evolve if the planner were to increase the level of benefits provided in case of long term unemployment. The next section offers to analyse these mechanisms.

1.6 Beyond local recommendations

The previous sections showed that the planner should, at the optimum, provide more SP and relatively less UB early in the unemployment spell. By implementing such an increasing profile, the planner would increase the CS value of benefits paid later in the spell, which would provide incentives to make the benefits profile even steeper. This section explores how far the planner should push the steepness of the benefits profile at the optimum. To do so, it analyses how the MH cost of different instruments varies when the level of other instruments is changed. In a first time, it analyses how the MH cost of the SP evolves in comparison to the MH of early benefits when the level of UB for long term unemployment is changed. In a second time, it explores how the MH cost of early benefits evolves compared to the MH of benefits paid later in the spell when the level of the SP changes. Note that this section provides suggestive evidence as the different point estimates cannot be statistically differentiated from one another. Despite this caveat, all the estimates point in the same direction.

1.6.1 Benefits insuring against long-term unemployment and the relative MH cost of early benefits compared to SP

All the MH costs estimated so far are functions of the level of the other instruments. The level of insurance against long-term unemployment (b_2) might for example impact how the planner will want to mix SP and UB early in the unemployment spell. A higher b_2 could for instance raise the relative cost of the SP, which might put a break on how much to increase A_0 and decrease b_1. To explore this question, this subsection splits the sample at the median of b_2's distribution. Individuals above the median will be considered as receiving a high b_2, while individuals below the median will be considered as receiving a low b_2. The resulting groups will of course have different characteristics. Weights are identified through a probit procedure in order to make both groups similar based on observables (see Appendix B, table 1.B.4 for further details) and will be used in all of the regressions. The comparison of the MH costs across these two groups will indicate how these costs would evolve if the planner were to provide relatively more UB in case of long-term unemployment. Table 1.3 contains the estimated values of the MH costs generated for these two groups.

Table 1.3: Insurance against long term unemployment and MH costs

Source of variation	**Low b_2**			**High b_2**		
	$MH_.$	$\varepsilon_{D_1,.}$	$\varepsilon_{D_2,.}$	$MH_.$	$\varepsilon_{D_1,.}$	$\varepsilon_{D_2,.}$
b_1	1.292**	1.189***	0.154**	1.419**	1.294***	0.163**
	(0.616)	(0.046)	(0.071)	(0.616)	(0.041)	(0.071)
% of total MH_{b_1}	47.67			52.35		
N	11,015			11,015		
A_0	1.109***	0.066	0.159**	0.851***	-0.008	0.327***
	(0.215)	(0.098)	(0.079)	(0.215)	(0.080)	(0.082)
% of total MH_{A_0}	61.4			38.6		
N	10,531			10,531		
$\frac{MH_{b_1}}{MH_A}$	1.165			1.667		

Notes: This table contains the point estimates obtained from RKD (b_1) and RDD (A_0). Regressions are weighted to make both groups alike based on observables (see Appendix B for further details) and include a large set of controls and fixed effects. The controls take into account the age, gender, the marital status, a dummy for the presence of children, education, a dummy for whether the individual previously worked part-time, a dummy for whether the individual has already been unemployed in the past. The fixed effects control for the year, geographic location, the reason for the redundancy and the year of tenure. The link between the MH costs and the elasticities is explained in the Appendix. Standard errors are obtained through a bootstrap procedures with 50 replications.
*,** and *** denote significance at the 10%, 5% and 1% levels, respectively.

First of all, note that since the sample has been splitted at the median, the average of the MH costs estimated in table 1.3 across the two groups simply corresponds to the MH costs estimated in table 1.1 for the whole sample. While the MH cost of early benefits is larger for individuals with a high b_2, the MH cost of the SP is much smaller. As a consequence, the ratio of the MH cost between early UB (b_1) and the SP (A_0) is significantly larger for individuals with a high b_2. This suggests that after an increase in b_2, the MH cost of early benefits with respect to the SP will become even larger. As a consequence, as the planner increases the level of UB paid for long-term unemployment, she will want to provide even more SP instead of early UB, leading to an even steeper UB profile.

1.6.2 Severance Package and the relative MH cost of benefits

The previous subsection suggested that as the planner increases the level of UB insuring against the risk of long term unemployment, she should provide relatively more SP and relatively less UB early in the spell. It is therefore natural to study how the MH cost of UB - provided both early and later in the unemployment spell - would evolve if the planner were to provide more SP. To analyse this question, this subsection proceeds in a similar fashion compared to the previous one. Two groups are created depending on whether individuals are above or below the median of the SP's distribution. Weights are identified through a probit procedure and used in the regressions to make sure that both groups are similar based on observables (see Appendix B, table 1.B.5). Since the population has been splitted at the median, the average of the MH costs across the groups contained in table 1.4 simply corresponds to the MH cost for the full sample contained in table 1.1.

Table 1.4: Moral Hazard Cost of Unemployment Benefits as a Function of Liquidity

Source of variation	**Low liquidity**			**High liquidity**		
	$MH_{.}$	$\varepsilon_{D_1,.}$	$\varepsilon_{D_2,.}$	$MH_{.}$	$\varepsilon_{D_1,.}$	$\varepsilon_{D_2,.}$
b_1	1.070*	0.454***	0.142**	1.601***	1.372***	0.177***
	(0.573)	(0.080)	(0.063)	(0.573)	(0.045)	(0.063)
% of total MH_{b_1}	40.07			59.95		
N	11,319			11,319		
b_2	1.055**	-1.112	1.021*	1.357**	0.463	1.355**
	(0.537)	(1.964)	(0.536)	(0.537)	(0.781)	(0.536)
% of total MH_{b_2}	43.74			56.26		
N	7,799			7,799		
$\frac{MH_{b_1}}{MH_{b_2}}$	1.014			1.180		

Notes: This table contains the point estimates obtained from RKDs (see equation 1.4). Regressions are weighted using a probit process described in Appendix B to make the two groups similar based on observables. The regressions include a large set of controls and fixed effects. The controls take into account the age, gender, the marital status, a dummy for the presence of children, education, a dummy for whether the individual previously worked part-time, a dummy for whether the individual has already been unemployed in the past. The fixed effects control for the year, geographic location, the reason for the redundancy and the year of tenure. The link between the MH cost and the elasticities is explained in the Appendix. Standard errors are obtained through a bootstrap procedures with 50 replications.
*,** and *** denote significance at the 10%, 5% and 1% levels, respectively.

Two results in table 1.4 are worth emphasising. First the point estimates for the behavioural responses, both to short and long term benefits, are higher for individuals with a larger SP. Second, the ratio of MH costs ($\frac{MH_{b_1}}{MH_{b_2}}$) is also larger for individuals above the median of the SP. This means that both the absolute and the relative behavioural distortions created by benefits received early in the unemployment spell are stronger for individuals receiving more SP. This suggests that as the planner increases the level of the SP, she will want to provide relatively less early UB compared to UB paid later in the spell as the relative cost of the former increases with the level of the SP provided.

All these effects taken together create a self-sustained process pushing in favour of more SP and less UB for short time unemployment. At first, the relative level of the SP compared to early UB should be increased. By providing relatively less insurance in case of short time unemployment, the planner would induce a dynamic selection into long-term unemployment increasing the value of benefits paid later in the spell. This would provide an incentive to implement an even steeper UB profile. After increasing the level of UB insuring against long-term unemployment, the cost of the SP compared to early UB would drop. The planner should therefore decrease even further the level of early UB compared to the level of the SP. By doing so, it would increase the cost of early UB compared to benefits paid later in the spell, calling again for an even steeper UB profile. Since all these forces point in the same direction, this suggests that the planner should go all the way to a corner solution, with a benefits profile made of a SP, no benefits for short time unemployment, and UB covering individuals only in case of long term unemployment.

1.7 Validity

The empirical strategies in the previous sections rely on well known identifying assumptions that need to be fulfilled for our estimates to have a causal interpretation. In short, individuals need to not be able to perfectly manipulate the running variable, *i.e.* the distance to the kink or jump, and the distribution of unobserved heterogeneity needs to be smooth around the jump and kinks. These assumptions are made to ensure that any change exploited by the RDD and RKDs is due to the exogenous discontinuities/non-linearities themselves and not to some potential manipulation from individuals. There exist multiple tests for these assumptions, and this section presents the most important ones. Considering that the empirical analysis in this paper uses multiple outcome variables, sources of identification and a decomposition into different groups, only tests for the full sample will be presented in the main text. Further details and validity checks about the different subgroups are relayed in the Appendix C.

Manipulation of the running variable McCrary [2008] developed an intuitive test for the first assumption. Consider the case of severance packages. If individuals could decide when to be made redundant, we would expect to see a hole in the density of redundancies just before the jump in the level of the severance package and an excess mass right after, *i.e.* some bunching. Conversely if employers could perfectly manipulate the redundancy date, one would expect the opposite to happen with an excess mass prior the jump and a hole right after. This would be a key threat to identification. The idea of the McCrary test is to analyse whether such behaviours exist by looking, both visually and formally, for possible discontinuities in the density around the discontinuities. Figure 1.C.1 in Appendix C illustrates these two points, by plotting the densities for the jump and kinks exploited in the empirical analysis. Every figure contains two tests. The first one corresponds to the traditional McCrary test and evaluates the presence of a jump in the density. The second one tests for the presence of a change in the slope of the density, and is particularly informative for the RKDs (Landais [2015]). These tests are also summarised in table 1.C.1.
These tests lead to reject the presence of any discontinuity or change in slope of the pdf for all of the sources of identification exploited in this article. This confirms that neither the employees nor the employer can perfectly control the different running variables.

Distribution of heterogeneity The second key identifying assumption requires heterogeneity to be smoothly distributed around the discontinuities. Consider once again the case of severance packages. If older individuals were better at making sure to be made redundant right after the jump in their severance package, one could not say whether any impact on an outcome of interest would be coming from the severance package or individuals' age. To ensure that the effect identified with the RDD comes from the additional liquidity provision, one needs to make sure that, on average, individuals on the both sides of the jump are similar, *i.e.* that the source of variation is as good as random in a neighbourhood of the discontinuity. While one can never guarantee that unobserved heterogeneity is smoothly distributed, the evolution of different demographic characteristics provides a good indication of whether the second identifying assumption is respected. Instead of looking at different demographic characteristics individually for every source of identification, Landais et al. [2021] suggested to use a covariate index. This index is created by regressing the different outcomes of interest on a large set of characteristics, among which the age, education level, gender, a dummy for the presence of children, the reason for the redundancy, the region. Table 1.C.1 in the Appendix C summarises the results from these covariates tests for our three instruments and two outcomes of interest. None of the six covariate tests is significant. This confirms that demographic characteristics are smoothly distributed around the discontinuities for all of our three instruments and two outcome variables.

Bandwidth Selection There exist different methods for selecting the optimal bandwidth when implementing a RDD or a RKD (Imbens and Kalyanaraman [2012], Calonico et al. [2014] [CCT] hereafter). The bandwidth used in the empirical analysis in the previous subsections have been determined through the CCT criterion. In theory one would want to consider only a very small interval around the discontinuity that is leveraged either in a RDD or in a RKD. In practice such a small interval would nevertheless often not contain enough observations, despite the use of administrative data. One can therefore wonder how the point estimates would evolve if the selected bandwidth in the regressions were to be different. A very simple way to answer such question is to repeat the estimation procedures for different bandwidths, and to plot the corresponding coefficients and confidence intervals. Figures 1.C.2, 1.C.3 and 1.C.4 in the Appendix C show how the estimated moral hazard costs evolve as a function of the bandwidth considered. The costs remain very stable and precisely estimated.

Placebo test For the case of short-time benefits, a reform implemented in 2014 provides the opportunity to run a placebo test. The 2014 reform modified the formula that identifies the length of the WP. Importantly, it increased its maximum duration from 75 to 180 days[7]. By using the available data in the FHS-D3 from the end of 2014 until 2016, I can repeat the previous analysis by completely ignoring this reform. It is possible to compute, over this period, the length of the WP individuals would have faced had the rules not changed. If the variation exploited in the previous analysis indeed comes from the existence of a kink at 75 days, one should see no variation whatsoever after 2014 around that duration. Figure 1.C.5 in Appendix C confirms that neither short nor long term unemployment display a kink around 75 days post 2014. Figure 1.C.6 on the other hand confirms that the kink in the WP formula indeed is the driver underling the variation in unemployment duration. Despite being more noisy due to a much smaller number of observations, this figure clearly displays kinks on both short and long term unemployment around the new discontinuity located at 180 days.

1.8 Conclusion

By introducing a new instrument in the traditional UI framework, this paper recommends implementing an increasing benefits profile through the use of a new mechanism. It identifies and measures a set of sufficient statistics which ultimately rationalise the use of waiting periods. The optimal benefits profile should be made of a SP provision at first, and UB should only insure individuals facing long term unemployment. The key advantage conveyed by the SP is a smaller behavioural distortion, and therefore a lower moral hazard cost. In addition, such organisation would lead to a better targeting of benefits through a screening-out property. Providing less UB early in the unemployment spell indeed affects relatively more negatively the survival function of the *high-types* - individuals with a higher level of education. This improved targeting would increase the consumption smoothing gains from benefits paid later in the unemployment spell, which in turn would encourage the provision of relatively more benefits to long-term unemployed. A higher level of benefits for long-term unemployment would itself decrease the relative moral hazard cost of the SP compared to early UB. Additionally, a higher

[7]Such maximum duration was later on reduced to 150 days, see RG. 14/05/2014, art. 21 §2 and art. 21 §1 Décret 2021-346.

level of SP would increase the relative moral hazard cost of early UB compared to benefits paid later in the unemployment spell. These mechanisms create a self-sustained process leading all the way to a corner solution with a SP provision only at first, no benefits for short term unemployment, and UB only in case of long-term unemployment. Most UI systems already incorporate waiting periods, mostly for administrative reasons. Relying on the aforementioned organisation would therefore come at relatively small cost. It nevertheless requires Public Employment Services (PES) to be provided with details about SP and individuals' financial situation, a direction recently taken by the French PES *Pôle* emploi.

Appendices

1.A Appendix A - Technical Appendix

1.A.1 Model

Set-up For a complete presentation of the set-up, the reader is referred to Chetty [2006] and Kolsrud et al. [2018].

Key Moments To obtain optimality conditions similar to equation 1.2, the reader can simply replace indexes with the corresponding instruments. The Moral hazard costs correspond to a weighted sum of different elasticities. They are described below, along with the consumption smoothing gains of the different instruments:

$$
\begin{aligned}
MH_{A_0} &\equiv \frac{(\tau + b_1)D_1}{A_0}\varepsilon_{D_1,A_0} + \frac{(\tau + b_2)D_2}{A_0}\varepsilon_{D_2,A_0} \\
MH_{b_1} &\equiv \frac{(\tau + b_1)}{b_1}\varepsilon_{D_1,b_1} + \frac{(\tau + b_2)D_2}{D_1 b_1}\varepsilon_{D_2,b_1} \\
MH_{b_2} &\equiv \frac{(\tau + b_1)D_1}{b_2 D_2}\varepsilon_{D_1,b_2} + \frac{(\tau + b_2)}{b_2}\varepsilon_{D_2,b_2} \\
CS_{A_0} &\equiv \frac{\mathbb{E}_0^u[\frac{\partial v_i^u}{\partial c_{i,0}^u}] - \lambda}{\lambda} \\
CS_{b_1} &\equiv \frac{\mathbb{E}_1^u[\frac{\partial v_i^u}{\partial c_{i,1}^u}] - \lambda}{\lambda} \\
CS_{b_2} &\equiv \frac{\mathbb{E}_2^u[\frac{\partial v_i^u}{\partial c_{i,2}^u}] - \lambda}{\lambda}
\end{aligned}
$$

Where λ corresponds to the lagrange multiplier from the Planner's optimisation program. It can be interpreted as the shadow cost of the government's budget constraint, and represents the cost of a given instrument relative to an unconditional transfer.

$\mathbb{E}_t^u$ takes the weighted average across all individuals' marginal utility of consumption in the t-th period of the unemployment spell, the weight being given by $\frac{S_{i,t}}{S_t}$. S_t corresponds to the average survival rate, across individuals, into unemployment up to time t, such that $S_t \equiv \int_i \prod_{k=0}^{t-1}(1-s_{i,k})di$

1.A.2 Proofs

Proof of Proposition 2 To simplify the proof and without loss of generality, consider the presence of two distinct types: low types (L), in proportion $(1-\alpha)$ at the beginning of the unemployment spell, and high types (H) in proportion α. These two types are represented by the utility functions $v^L(c_t^L, s_t^L)$ and $v^H(c_t^H, s_t^H)$ respectively. Further assume that utility is separable in consumption and search effort. We have that $\frac{\partial v^L(c_t^L,s_t^L)}{\partial c_t^L} > \frac{\partial v^H(c_t^H,s_t^H)}{\partial c_t^H}$. The consumption smoothing value of UB paid at time t is then given by:

$$CS_{b_t} = \frac{(1-\alpha)\frac{S_t^L}{S_t}\frac{\partial v^L(c_t^L,s_t^L)}{\partial c_t^L} + \alpha\frac{S_t^H}{S_t}\frac{\partial v^H(c_t^H,s_t^H)}{\partial c_t^H} - \lambda}{\lambda}$$

Where S_t^i represents the survival probability of an individual of type i at time t, *i.e.* the probability that an individual of type i is still unemployed at time t.

Define the proportion of low-types in the total unemployed population at time t as $p_t^L \equiv (1-\alpha)\frac{S_t^L}{S_t}$. Consequently $\alpha\frac{S_t^H}{S_t} = 1 - p_t^L$.

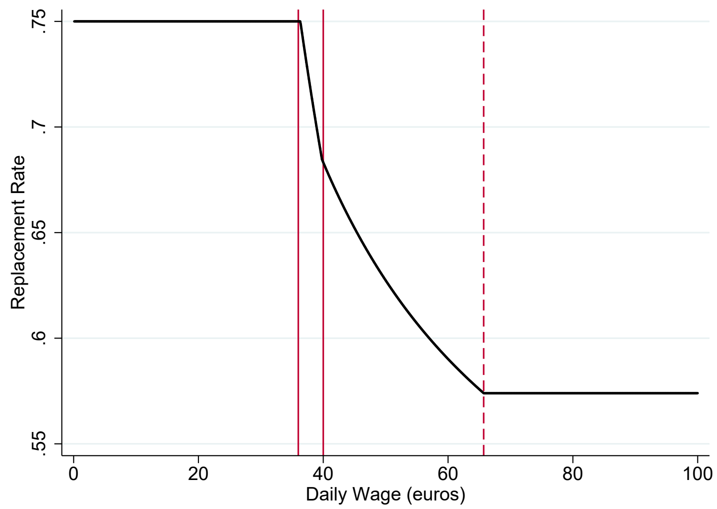
Let us now analyse how the consumption value of benefits paid at time t would respond to a change in the level of benefits paid earlier in the spell. For t>1:

$$\frac{\partial CS_{b_t}}{\partial b_1} = \underbrace{\frac{\partial p_t^L}{\partial b_1}}_{?}\left\{\underbrace{\frac{\partial v^L(c_t^L,s_t^L)}{\partial c_t} - \frac{\partial v^H(c_t^H,s_t^H)}{\partial c_t}}_{>0}\right\} + \underbrace{\underbrace{\frac{\partial^2 v^L(c_t^L,s_t^L)}{\partial {c_t^L}^2}}_{\leq 0}\underbrace{\frac{\partial c_t^L}{\partial b_1}}_{\geq 0} + \underbrace{\frac{\partial^2 v^H(c_t^H,s_t^H)}{\partial {c_t^H}^2}}_{\leq 0}\underbrace{\frac{\partial c_t^H}{\partial b_1}}_{\geq 0}}_{\leq 0}$$

If more benefits early on in the unemployment spell screen-out relatively more the low types ($\frac{\partial p_t^L}{\partial b_1} < 0$), it will necessarily decrease the consumption smoothing value of benefits paid later on. Conversely, a decrease in b_1 will necessarily increase CS_{b_t} if it increases the proportion of low-types. ■

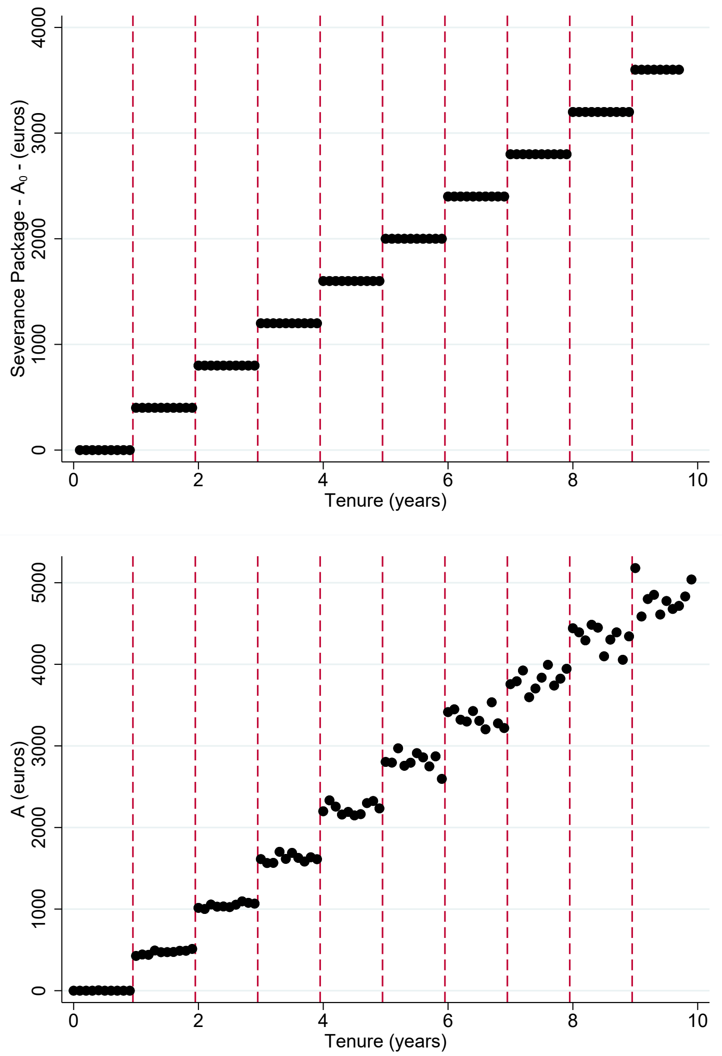
1.B Appendix B - Additional Figures and Tables

Figure 1.B.1: Replacement Rate



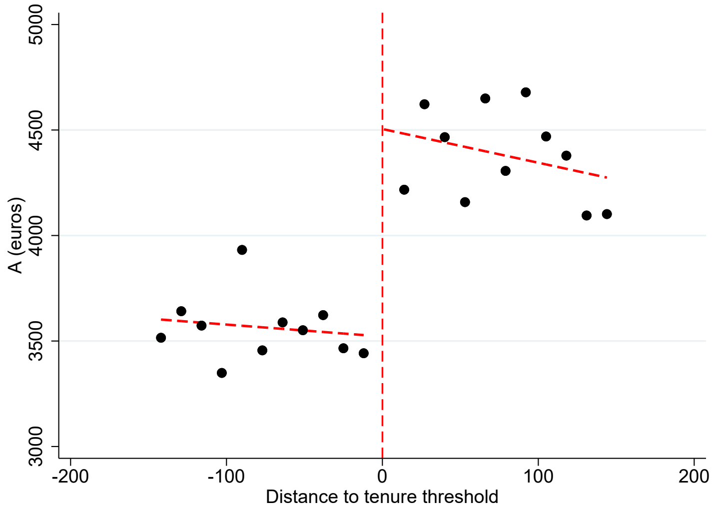
Notes: This figure illustrates the formula used to compute the replacement rate. It displays three kinks. The empirical analysis contained in this article relies on the kink with the dashed red vertical line as most of the observations are located around this kink.

Figure 1.B.2: Tenure and Severance Package



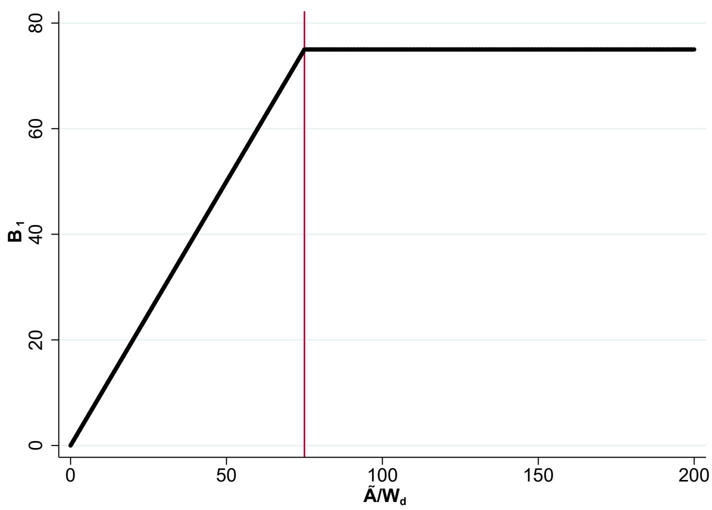
Notes: The first figure displays how the severance package evolves as a function of tenure for a given individual earning €2,000 a month with a permanent contract. The second figure displays the jumps in the estimated severance packages for the whole sample.

Figure 1.B.3: Jump in the Severance Package



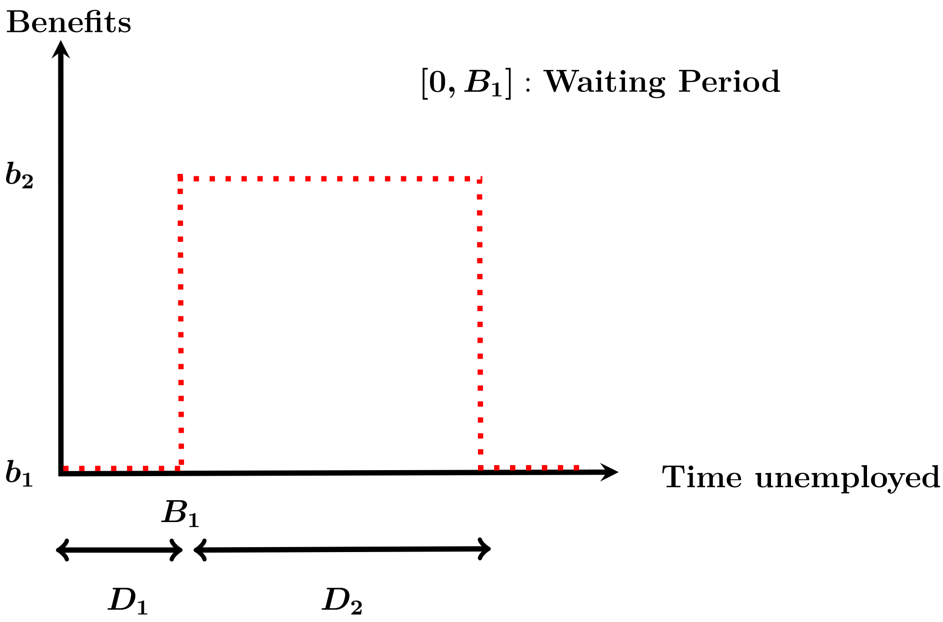
Notes: This figure combines all the discontinuities contained in figure 1.B.2 together by defining a measure of distance with respect to the jumps common across all discontinuities. Individuals with 300, 630 and 1360 days of tenure are then all located at -65 days of a discontinuity.

Figure 1.B.4: Waiting Period Schedule



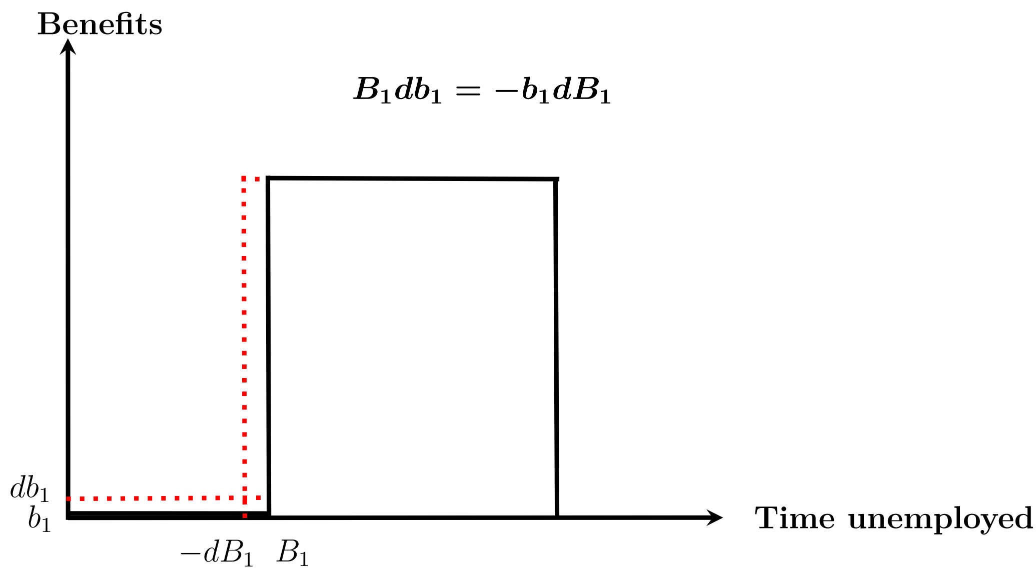
Notes: The Waiting Period's length (B_1) corresponds to the minimum between a ratio between the amount of the SP above and beyond the legal minimum ($\tilde{A}$) and the daily wage (W_d), and 75 days: $B_1 = min\{\frac{\tilde{A}}{W_d}, 75\}$.

Figure 1.B.5: Similarities between a WP and a Two-Tier Benefits Profile



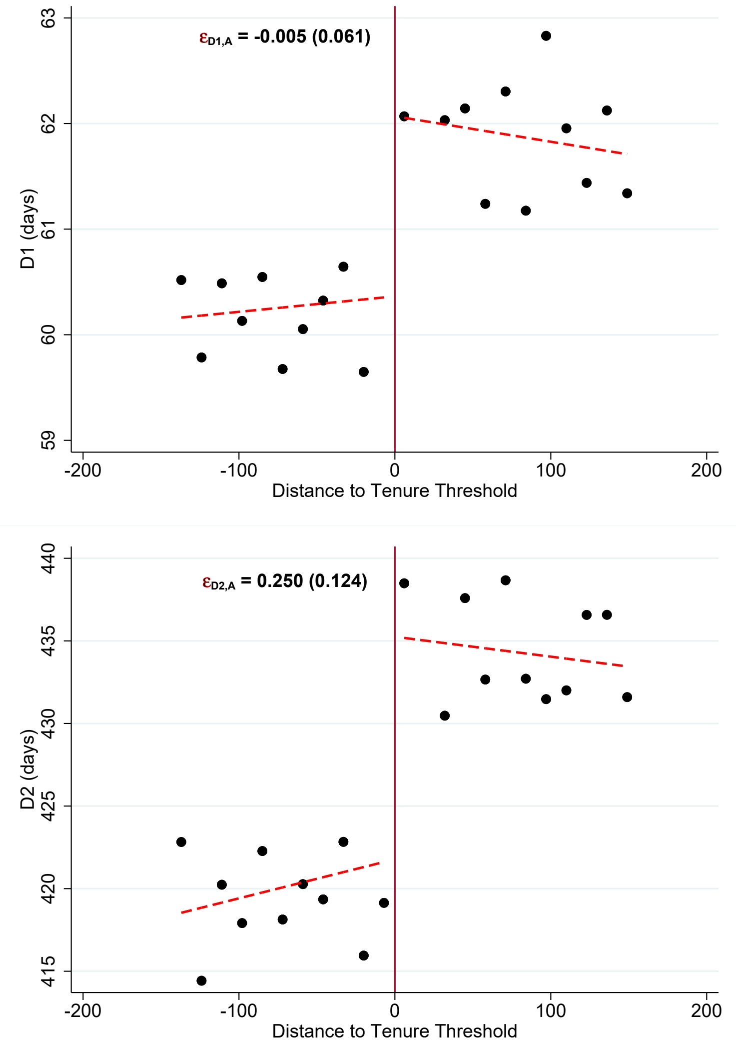
Notes: This figure illustrates the similarities between a two tier benefits profile and a system with a flat benefits profile and a waiting period. With a WP, individuals do not receive any unemployment benefits ($b_1 = 0$) for a duration B_1, and then start receiving UB ($b_2 > 0$), therefore creating a two tier (increasing) benefits profile.

Figure 1.B.6: Waiting Period and UB for Short-Term Unemployment (b_1)



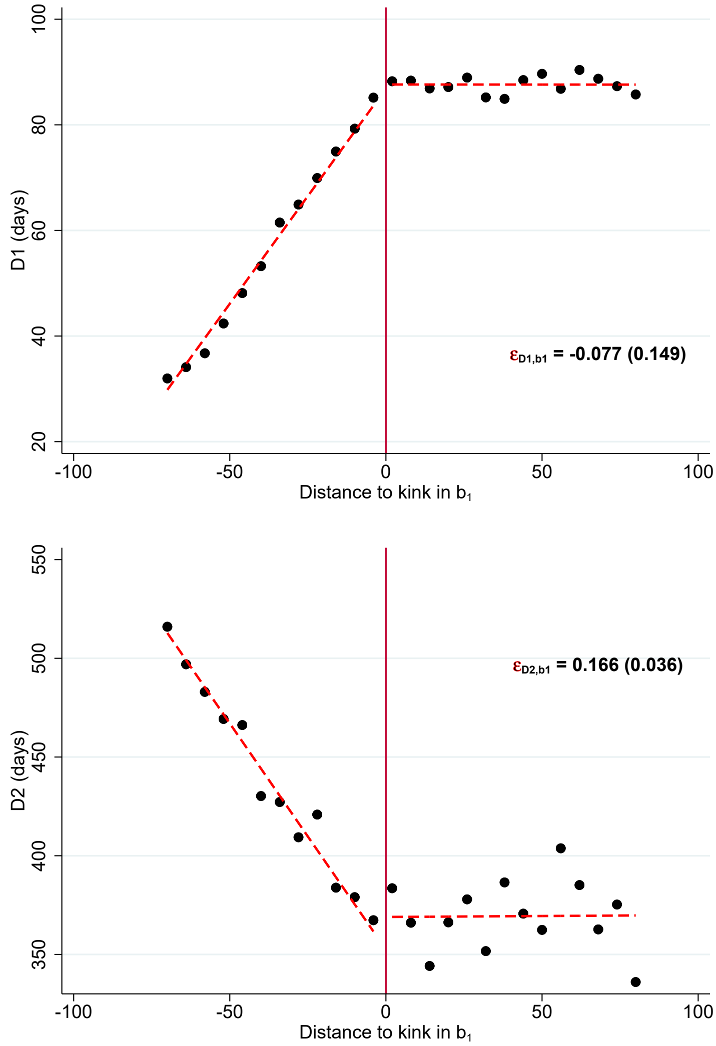
Notes: This figure illustrates the fact that in a schedule with a flat benefits profile, the presence of a waiting period can be used to disentangle the effect of unemployment benefits covering short and long term unemployment (b_1 and b_2). Note that such figure is not completely accurate as a shorter waiting period would bring forward the exhaustion date too. Table 1.1 in the main text nevertheless shows that individuals do not seem to be forwards looking. A higher b_2 indeed does not have any effect on the duration of short time unemployment. Moreover table 1.B.1 below shows that individuals on average are entitled to about two years of UI. It is therefore reasonable to neglect the effect of the WP on the exhaustion date.

Figure 1.B.7: Severance Package (A_0) and Unemployment Length (D_1, D_2)



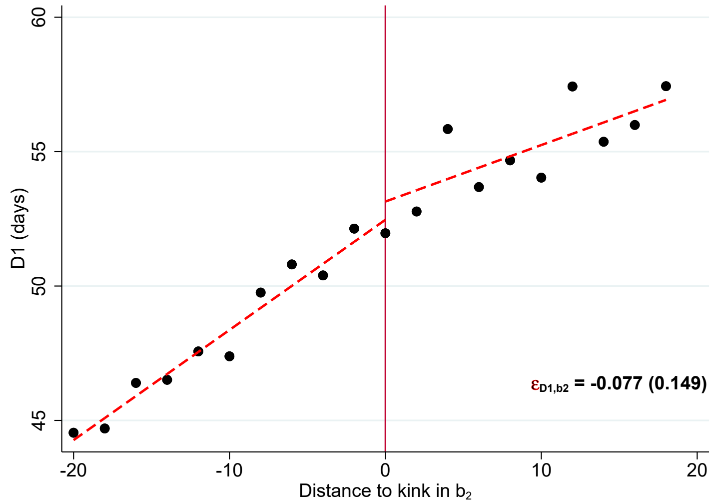
Notes: This figure represents the effect of the severance package on the duration spent both in short and long term unemployment. It represents the raw data, while the computed elasticities include a set of controls mentioned in table 1.1. The elasticities are obtained by using equation 1.3 and the following formula: $\varepsilon_{Y,A_0} = \frac{\hat{\tau}_{1,A_0}}{\tilde{\tau}} * \frac{\bar{A}_0}{\bar{Y}}$, where $\tilde{\tau}$ is the estimated coefficient from the same regression where the level of the severance package is the outcome variable, and $\bar{A}_0$ and $\bar{Y}$ represent respectively the average of the severance package and of the outcome of interest in a bandwidth $\mathscr{B}_{A_0}$ around the discontinuity. Standard errors are obtained through a bootstrap procedure with 50 replications.

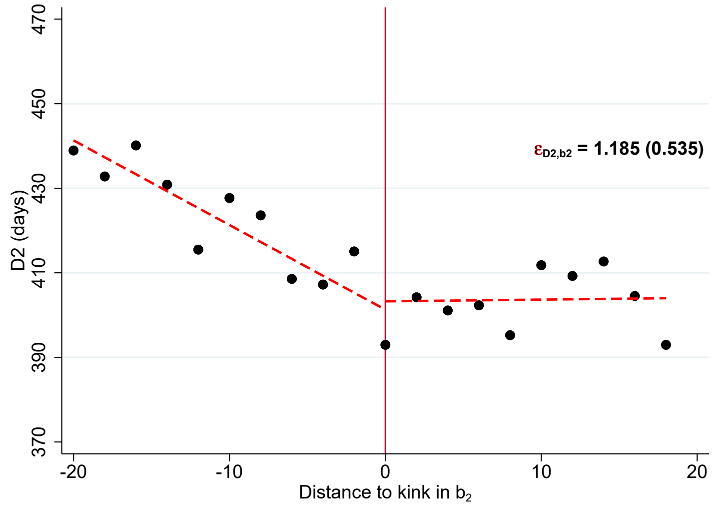
Figure 1.B.8: Benefits for Short-Term Unemployment (b_1) and Unemployment Length (D_1, D_2)



Notes: This figure represents the effect of benefits for short-time unemployment on the duration spent both in short and long term unemployment. The Waiting Period is exploited to back-out the effect of b_1 using a method described in the main text and explained in figure 1.B.6. The elasticities are defined as $\varepsilon_{X,b_1} = \frac{\partial X}{\partial b_1}\frac{b_1}{X}$, where the estimated coefficients come from equation 1.4. Standard errors are obtained through a bootstrap procedure with 50 replications.

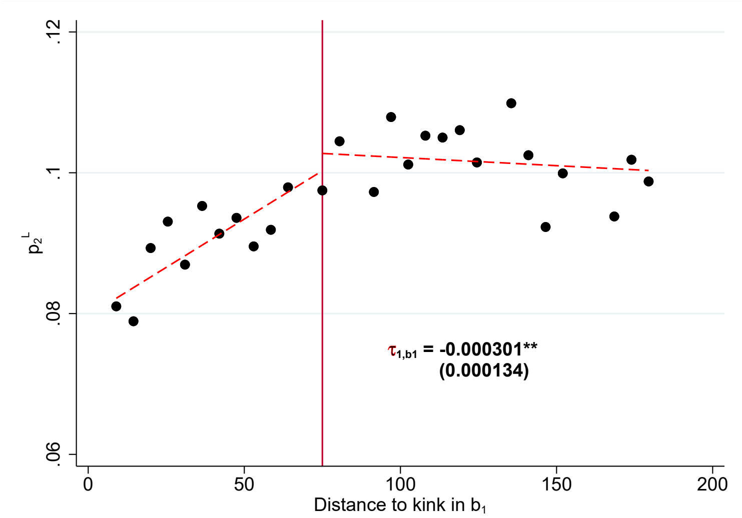
Figure 1.B.9: Benefits for Short-Term Unemployment (b_2) and Unemployment Length (D_1, D_2)





Notes: This figure represents the effect of benefits for long-time unemployment on the duration spent both in short and long term unemployment. The elasticities are defined as $\varepsilon_{X,b_1} = \frac{\partial X}{\partial b_1}\frac{b_1}{X}$, where the estimated coefficients come from equation 1.4. Standard errors are obtained through a bootstrap procedure with 50 replications.

Figure 1.B.10: Probability to be a Low-type (p_2^L) in Long-Term Unemployment and Kink in b_1



Notes: This figure represents the response of the probability to be a low-type in long term unemployment (p_2^L) to the kink in b_1. The estimated coefficients are obtained from an equation similar to equation 1.4 where the outcome variable is a binary variable taking a value of 1 if the individual is a high school drop-out and zero otherwise.

Table 1.B.1: Descriptive Statistics - Full Sample

	Mean	p10	p50	p90
	Demographics			
Age	34	23	32	48
Fraction women	0.46	0	0	1
Fraction College Drop-Out	0.10	0	0	1
Fraction High-School Graduates	0.71	0	1	1
Fraction Bachelor or More	0.19	0	0	1
Fraction Married	0.48	0	0	1
Fraction with kids	0.46	0	0	1
Frac engaged in partial unemp.	0.61	0	1	1
	Unemployment characteristics			
D1 (days)	36	0	31	75
D2 (days)	414	11	356	843
b1 (euros)	0	0	0	0
b2 (euros)	37	20.93	33.62	56.15
Eligibility (days)	655	383	730	730
Replacement rate (gross)	0.60	0.57	0.59	0.65
Receives UB at least once	0.94	1	1	1
	Severance Package Details			
Amount A (euros)	3,152	333.9	2,023	7,713
Amount $\tilde{A}$ (euros)	2,052	225.3	725.3	6,100
B_1 (days)	45	15	38	75
Observations	55,866			

Notes: This table contains detailed descriptive statistics for the main sample used in the analysis.

Table 1.B.2: RDD and RKDs with different controls

Variable	Baseline Controls $\boldsymbol{MH_{.}}$	Baseline Controls $\boldsymbol{\varepsilon_{D_1,.}}$	Baseline Controls $\boldsymbol{\varepsilon_{D_2,.}}$	Full Controls $\boldsymbol{MH_{.}}$	Full Controls $\boldsymbol{\varepsilon_{D_1,.}}$	Full Controls $\boldsymbol{\varepsilon_{D_2,.}}$
$\boldsymbol{A}$	0.803**	-0.007	0.210**	0.958**	-0.005	0.250**
	(0.376)	(0.038)	(0.098)	(0.473)	(0.061)	(0.098)
N	15,936			15,936		
Bandwidth	+/-90			+/-90		
$\boldsymbol{b_1}$	1.316***	1.271***	0.162***	1.343***	1.270***	0.166***
	(0.300)	(0.022)	(0.037)	(0.294)	(0.021)	(0.036)
N	21,409			21,409		
Bandwidth	+/-60			+/-60		
$\boldsymbol{b_2}$	1.146**	-0.098	1.144**	1.187**	-0.077	1.185**
	(0.547)	(0.152)	(0.546)	(0.536)	(0.149)	(0.535)
N	23,152			23,152		
Bandwidth	+/-15			+/-15		
End on contract FE	✗	✗	✗	✓	✓	✓
Year FE	✗	✗	✗	✓	✓	✓
Departement FE	✗	✗	✗	✓	✓	✓
Tenure group FE	✗	✗	✗	✓	✓	✓

Notes: This table contains the point estimates from both RDD (A_0) and RKD (b_1 and b_2) with different sets of controls. The baseline controls include basic demographics such as the age, level of education, gender, marital status and a dummy indicating whether or not the individual has children. It also takes into account the number of days of entitlement to UI and the existence of at least one prior unemployment spell. Full controls incorporate on top of these controls a large set of fixed effects.
*,** and *** denote significance at the 10%, 5% and 1% levels, respectively.

Split by Level of Education for Individuals Surviving the WP

The following table provides brief descriptive statistics for two distinct groups surviving the waiting period, high school drop-outs and individuals with at least a high-school degree. High school drop-outs had a much smaller wage prior unemployment and were more likely to work part-time. They therefore receive a smaller level of UB. These characteristics suggest that they should have a larger marginal utility from consumption.

Table 1.B.3: Descriptive Statistics - Split by Level of Education

	High School Drop-Out	High School Graduates and +
Age	40.09	33.85
	(10.909)	(9.534)
Fraction women	0.427	0.481
	(0.495)	(0.500)
Fraction married	0.559	0.483
	(0.497)	(0.500)
Fraction with children	0.541	0.459
	(0.498)	(0.498)
Tenure (years)	4.497	3.350
	(7.226)	(5.067)
Fraction working PT prior unemp.	0.324	0.213
	(0.468)	(0.410)
Gross daily wage (euros)	53.17	67.19
	(22.387)	(37.408)
Entitlement length to UI (days)	696.5	661.7
	(219.831)	(168.375)
UB level (euros)	31.33	38.56
	(13.104)	(21.692)
Observations	4,107	46,007

Notes: This table contains detailed descriptive statistics for the individuals that survive the waiting period. The sample is split into two distinct groups, with high school drop-outs and individuals that at least graduated from high school. The former group represents roughly 9% of the aforementioned population.

Split by level of insurance for long term unemployed

Table 1.B.4 below provide details about different individual characteristics by groups. The weights are defined using the same method as for the split by liquidity.

Table 1.B.4: Descriptive Statistics - Split by level of b_2

	Low b_2	**High b_2**	**High b_2 - Weighted**
Age	36.30	40.16	36.50
	(10.055)	(8.606)	(8.615)
Fraction women	0.522	0.374	0.475
	(0.500)	(0.484)	(0.499)
Education	5.646	7.138	5.417
	(1.767)	(1.767)	(1.913)
Fraction Married	0.538	0.646	0.509
	(0.499)	(0.478)	(0.500)
Fraction with kids	0.505	0.578	0.484
	(0.500)	(0.494)	(0.500)
Fraction worked PT	0.198	0.0450	0.251
	(0.399)	(0.206)	(0.434)
Frac engaged in partial unemp.	0.596	0.527	0.563
	(0.491)	(0.499)	(0.496)
Eligibility (days)	726.0	765.7	706.9
	(177.506)	(161.262)	(172.363)
Observations	27,933	27,933	25,987

Notes: This table contains details about demographic characteristics across two groups - individuals below and above the median of the distribution of unemployment benefits in long-term unemployment (resp. low and high b_2). A set of weights is identified through a probit regression that is then used to re-weight observations in the last column.

Split by level of SP

The following table provides descriptive statistics to emphasise the differences between individuals above and below the median of the severance packages' distribution. In order to re-weight the sample, I define a dummy variable taking a value of 1 if individuals are below the median. I then run a probit regression of this dummy on a large set of demographic characteristics including the age, education, marital status, the presence of children. Based on this regression, I predict a probability $\hat{p}$ that I then use to create a weight $w = \frac{\hat{p}}{1-\hat{p}}$. This weight is then used to obtain the results contained in table 1.4.

Table 1.B.5: Descriptive Statistics - Split by Liquidity

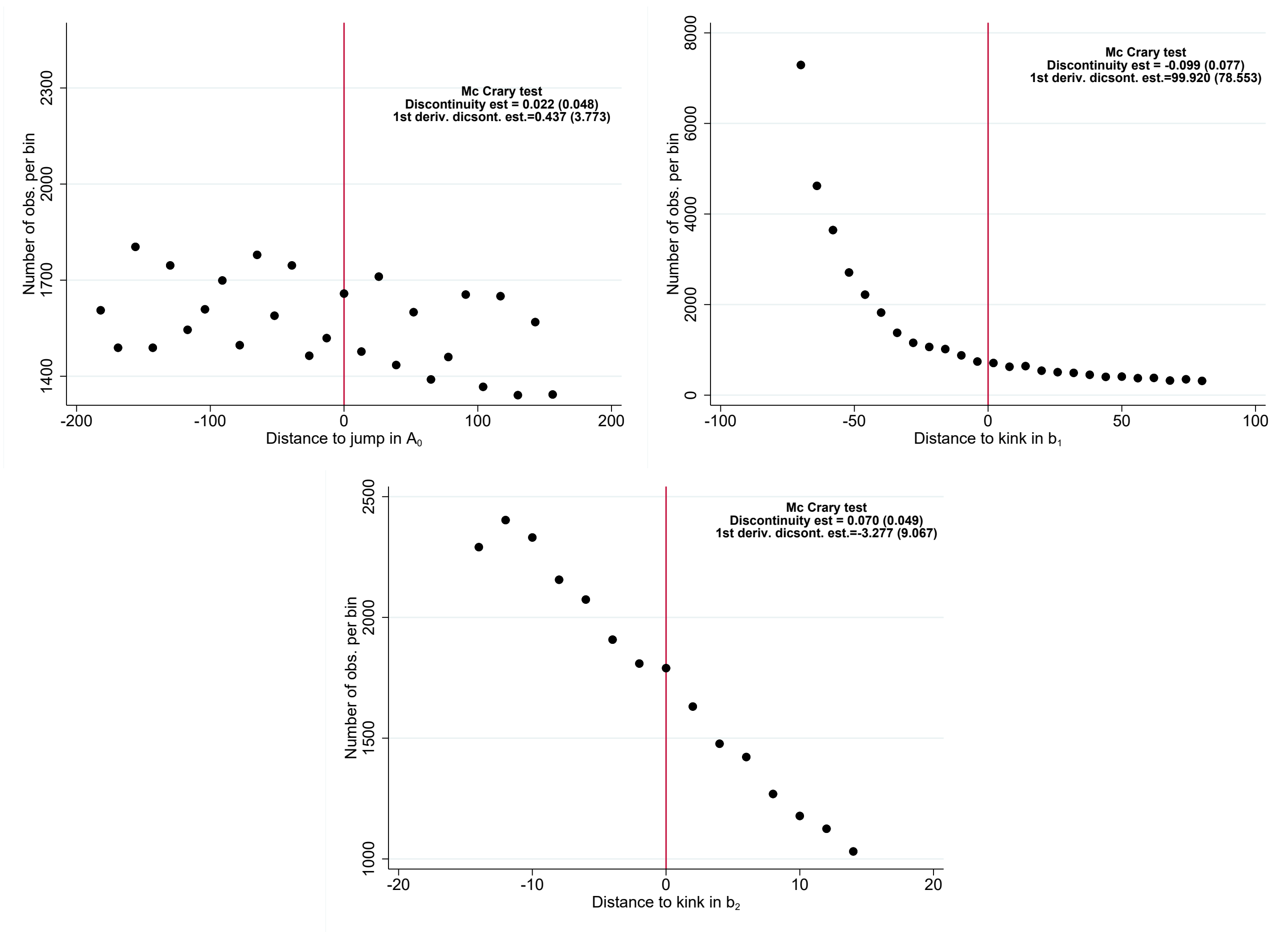
	Low Liquidity	**High Liquidity**	**High Liquidity - Weighted**
Age	34.55	41.91	35.86
	(9.171)	(8.451)	(8.351)
Fraction women	0.485	0.411	0.469
	(0.500)	(0.492)	(0.499)
Education	6.154	6.630	6.123
	(1.801)	(2.000)	(1.832)
Fraction Married	0.502	0.679	0.516
	(0.500)	(0.467)	(0.500)
Fraction with kids	0.482	0.600	0.504
	(0.500)	(0.490)	(0.500)
Fraction worked PT	0.161	0.0820	0.148
	(0.367)	(0.275)	(0.355)
Frac engaged in partial unemp.	0.587	0.536	0.586
	(0.492)	(0.499)	(0.492)
Eligibility (days)	695.1	796.6	719.6
	(164.167)	(161.857)	(128.894)
Observations	27,933	27,933	26,389

Notes: This table contains details about demographic characteristics across two groups - individuals below and above the median of the severance packages' distribution (resp. low and high liquidity). A set of weights is identified through a probit regression that is then used to re-weight observations in the last column.

1.C Appendix C - Validity Checks

1.C.1 Full Sample Analysis

Figure 1.C.1: McCrary Tests



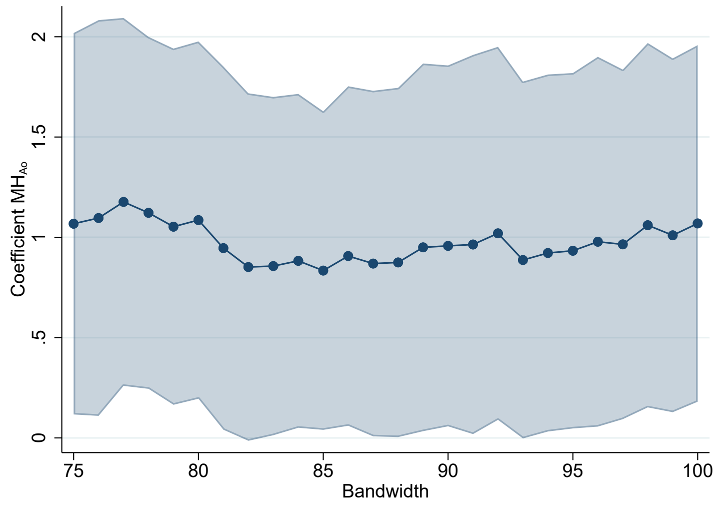
Notes: This figure represents the McCrary tests for the three main sources of identification exploited in this article. Each figure contains two tests: the baseline McCrary test and an additional test for the presence of a change in the slope of the density.

Table 1.C.1: Robustness Checks - Full Sample

	$\boldsymbol{A_0}$	$\boldsymbol{b_1}$	$\boldsymbol{b_2}$
	Density Tests		
McCrary Test	0.022	-0.099	0.070
	(0.048)	(0.077)	(0.049)
McCrary 1st deriv.	0.437	99.920	-3.277
	(3.773)	(78.553)	(9.067)
	Covariate Tests		
$\boldsymbol{D_1}$	0.448	0.001	0.074
	(0.304)	(0.011)	(0.256)
$\boldsymbol{D_2}$	2.257	-0.182	0.988
	(2.860)	(0.136)	(1.640)

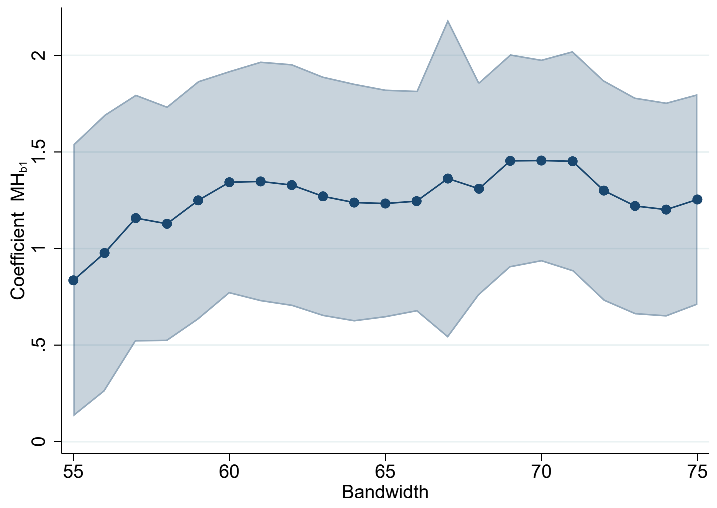
Notes: This table contains the main robustness checks for the validity of the RDD and RKD strategies when estimating the moral hazard cost of the three different instruments (A_0, b_1, b_2). See the main text for further explanations about the different tests. Standard errors are obtained through a bootstrap procedures with 50 replications.
*,** and *** denote significance at the 10%, 5% and 1% levels, respectively.

Figure 1.C.2: Robustness to Bandwidth Choice - MH_{A_0}



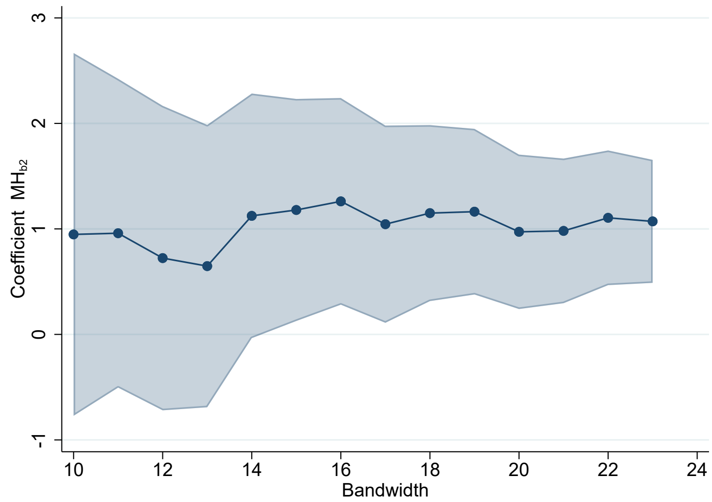
Notes: This figure represents the estimated value for the moral hazard cost of providing a severance package (A_0) for different bandwidths. It pictures both the point estimates (connected dots) and the confidence interval (light blue area).

Figure 1.C.3: Robustness to Bandwidth Choice - MH_{b_1}



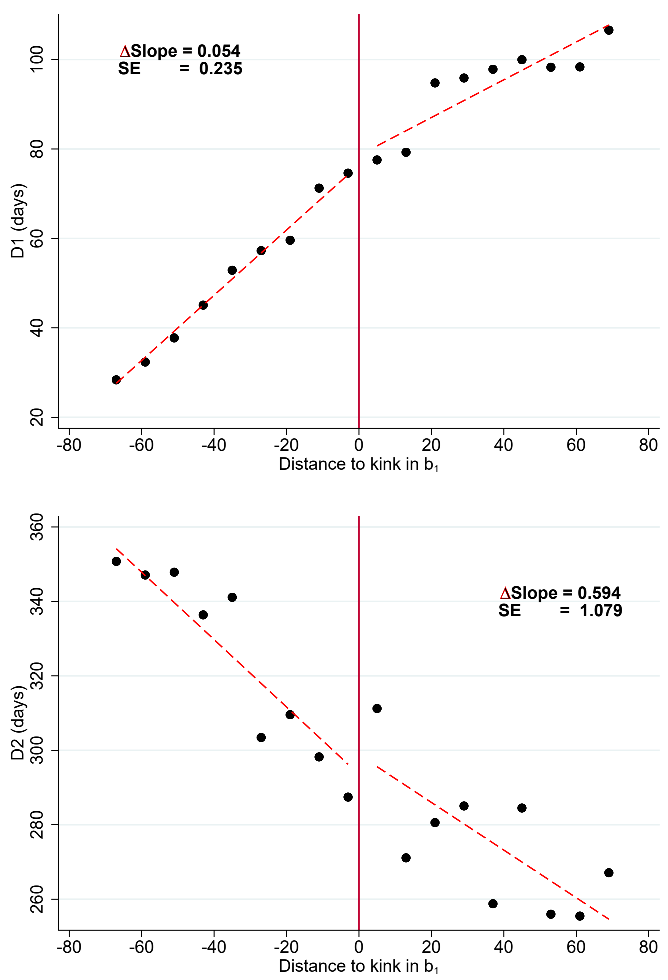
Notes: This figure represents the estimated value for the moral hazard cost of providing unemployment benefits early on in the spell (b_1) for different bandwidths. It pictures both the point estimates (connected dots) and the confidence interval (light blue area).

Figure 1.C.4: Robustness to Bandwidth Choice - MH_{b_2}



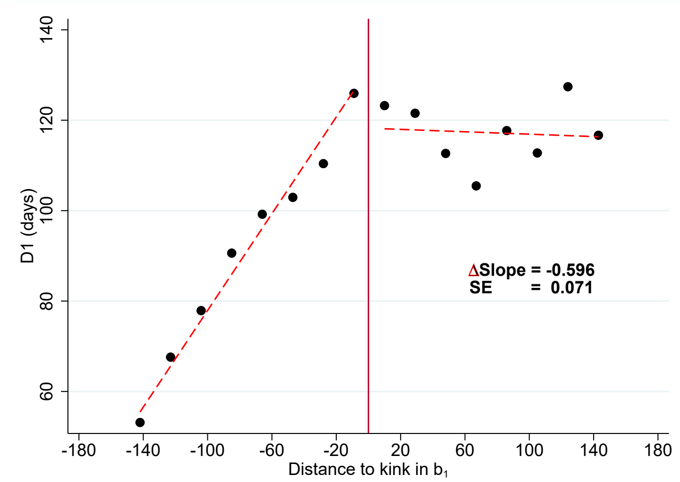
Notes: This figure represents the estimated value for the moral hazard cost of providing unemployment benefits covering long-tern unemployment (b_2) for different bandwidths. It pictures both the point estimates (connected dots) and the confidence interval (light blue area).

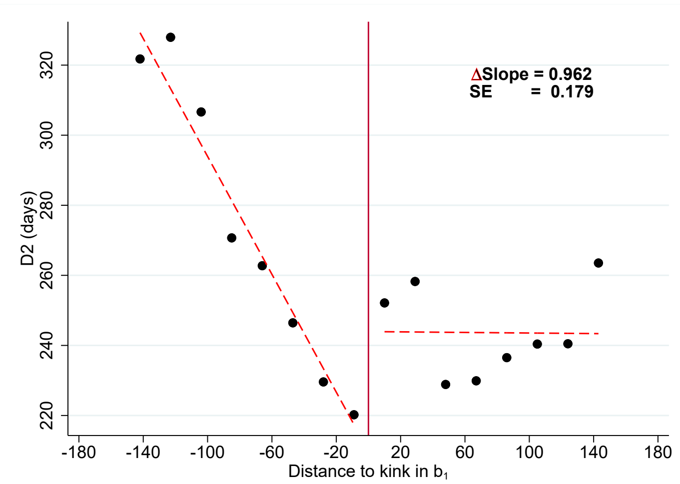
Figure 1.C.5: Placebo Test - Short-Time Unemployment benefits (b_1)



Notes: The above figure represents the evolution of respectively short and long term unemployment as a function of the distance to the kink in the waiting period. This distance is computed using the pre 2014 rules applied to data from 2014 until 2016. The change in slope and standard errors are obtained by estimating an equation similar to equation 1.4.

Figure 1.C.6: Kink in Short-Time Unemployment benefits (b_1) post the 2014 Reform





Notes: The above figures illustrate the fact that the kink arising with the presence of the waiting period did move from 75 to 180 days after the 2014 reform. The change in slope and the corresponding standard errors are obtained using an equation similar to equation 1.4.

Polynomial order A key risk, especially for RKD, is to assimilate a simple non-linearity to a discontinuity. Various tests exist to identify the optimal polynomial order that should be used in equations 1.3 and 1.4. A simple option is to base such choice on the BIC criterion. The table below contains the BIC from different regressions when I increase the polynomial order from p=1 to p=3.

Table 1.C.2: Optimal Polynomial Order

	D_1			D_2		
Polynomial order	1	2	3	1	2	3
A_0	**269945**	269955	269966	330602	**330601**	330609
b_1	**178066**	178086	178102	**304222**	304238	304257
b_2	**189255**	189273	189292	**328720**	328739	328758

Note: This table contains the BIC obtained after estimating equations 1.3 and 1.4 and varying the polynomial order from p=1 to p=3. In bold are the smallest BIC values, *i.e.* the specifications that should be preferred.

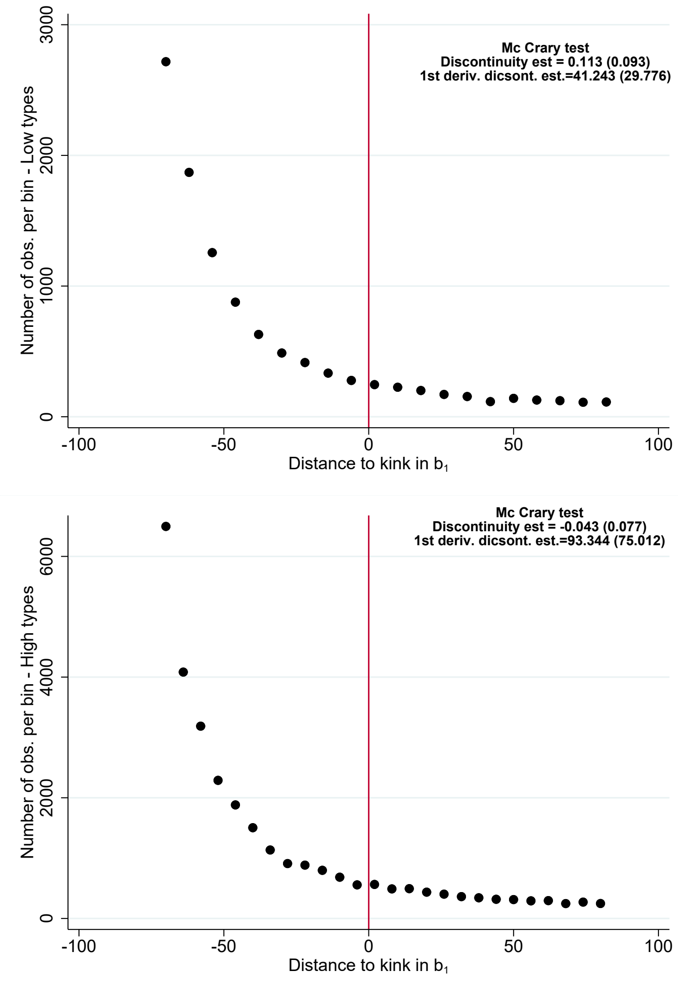
Table 1.C.2 indicates that in every but one case, a simple polynomial of order one should be preferred. Only for the case of the severance package with long-term unemployment should a polynomial of order 2 be preferred. The difference between the BIC with p=1 and p=2 is nevertheless marginal, and the analysis therefore - for simplicity - focuses on the case of a polynomial of order one for every regression and instrument.

1.C.2 Validity checks by groups

Dynamic selection mechanism and split by level of education

This subsection focuses on the split between high school drop-outs (low types) versus high school graduates and more (high types). While a McCrary test cannot be implemented directly for the proportion of low types in long term unemployment, one can verify that each group cannot independently perfectly manipulate the running variable. This amounts to applying a McCrary test for each group.

Figure 1.C.7: McCrary test for Low and High Types



Notes: The above figures represent the results of a McCrary test for both the low-types (high school drop-outs) and high-types (individuals with a high school degree or more). Two tests are displayed on each figures: a standard McCrary test for the discontinuity of the pdf, and a test assessing whether the slope of the pdf changes discontinuously around the kink.

1.C.3 Split by level of A and b_2

This subsection reproduces the validity checks presented in the main text for the different group decompositions (by liquidity and level of insurance in case of long-term unemployment.)

Table 1.C.3: Robustness Checks - MH_{b_1}

	Low Liquidity	**High Liquidity**
	Density Tests	
McCrary Test	-0.065	-0.08
	(0.131)	(0.075)
McCrary - 1st deriv.	45.012	3.607
	(109.691)	(4.023)
	Covariate Tests	
$\boldsymbol{D_1}$	-0.063	0.018
	(0.071)	(0.048)
$\boldsymbol{D_2}$	-0.155	0.398
	(0.577)	(0.581)

	Low b_2	**High b_2**
	Density Tests	
McCrary Test	-0.177	0.002
	(0.109)	(0.093)
McCrary - 1st deriv.	26.576	17.186
	(37.812)	(16.083)
	Covariate Tests	
$\boldsymbol{D_1}$	-0.105	0.122*
	(0.085)	(0.063)
$\boldsymbol{D_2}$	-0.776	0.559
	(0.566)	(0.533)

Notes: This table contains the main robustness checks for the validity of the RKD strategy when estimating the moral hazard cost of b_1 for different groups. See the main text for the precise definitions of the groups and further explanations about the different tests. Standard errors are obtained through a bootstrap procedures with 50 replications.
*,** and *** denote significance at the 10%, 5% and 1% levels, respectively.

Table 1.C.4: Robustness checks - MH_{b_2}

	Low Liquidity	**High Liquidity**
	Density Tests	
McCrary Test	0.082	0.071
	(0.061)	(0.076)
McCrary - 1st deriv.	-3.979	-6.312
	(13.604)	(5.592)
	Covariate Tests	
$\boldsymbol{D_1}$	-0.104	0.253
	(0.200)	(0.297)
$\boldsymbol{D_2}$	0.587	2.523
	(2.393)	(2.352)

Notes: This table contains the main robustness checks for the validity of the RKD strategy when estimating the moral hazard cost of b_2 for different groups. See the main text for the precise definition of the groups and further explanation about the different tests. Standard errors are obtained through a bootstrap procedures with 50 replications.
*,** and *** denote significance at the 10%, 5% and 1% levels, respectively.

Table 1.C.5: Robustness checks - MH_{A_0}

	Low b_2	**High b_2**
	Density Tests	
McCrary Test	-0.056	0.093
	(0.071)	(0.066)
McCrary - 1st deriv.	0.701	-0.310
	(1.797)	(2.188)
	Covariate Tests	
$\boldsymbol{D_1}$	0.372	0.436
	(0.345)	(0.329)
$\boldsymbol{D_2}$	1.341	6.932
	(4.548)	(5.188)

Notes: This table contains the main robustness checks for the validity of the RKD strategy when estimating the moral hazard cost of b_2 for different groups. See the main text for the precise definition of the groups and further explanation about the different tests. Standard errors are obtained through a bootstrap procedures with 50 replications.
*,** and *** denote significance at the 10%, 5% and 1% levels, respectively.

Chapter 2

Can Unemployment Insurance Reduce Job Stability ?

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Abstract

With a rising share of temporary contracts with shorter durations in new hirings, the number of transitions between employment and unemployment significantly increased in developed countries over the last decades.
To take into account such evolution and improve Unemployment Insurance (UI) coverage, the French UI reform of 2014 introduced an *top-up of entitlement* mechanism. For unemployed individuals receiving benefits, it reduced by more than four - from 122 to 30 - the minimum number of working days necessary to top-up one's entitlement to UI. Short temporary contracts therefore saw their potential value increase.
The effects of this top-up of entitlement mechanism are analysed using a fuzzy regression kink design based on the timing of the reform. Relying on administrative data from the French National Employment Agency, I show that the reform indeed incentivised unemployed to accept short temporary contracts, decreased their reservation wage, and increased their probability to become unemployed again. A heterogeneity analysis shows that this last effect is stronger for experienced unemployed, *i.e.* individuals that already experienced at least one unemployment spell in the past, further reducing their attachment to the labour market. It also underlines that the effects of such mechanism are not evenly distributed. Finally this paper provides the first cost estimate of the top-up mechanism. The rise in the probability to repeat unemployment is estimated to generate an additional cost of 500 to 700 euros per unemployed topping-up their entitlement.

Keywords: Unemployment insurance; Job Stability; Regression Discontinuity Design; Sufficient Statistics.
J.E.L. codes: H20; J64; J65.

2.1 Introduction

Labour markets in most developed countries experienced a combination of two trends in the last few decades: a strong increase in the share of temporary contracts in new hirings accompanied with a decrease in the average duration of these contracts.
Such evolutions resulted in an upsurge in the number of transitions between employment and unemployment, and challenged the traditional organisation of Unemployment Insurance (UI) systems. These latter were indeed developed in a very different context where permanent contracts characterised by long average durations were the dominating form of employment relationship.
UI schemes can often be summarised through a common set of parameters, namely the replacement rate, its evolution over the unemployment spell, and the potential benefit duration (PBD). While the effect(s) of these parameters have been largely explored by the literature, the impact of eligibility conditions remains mostly unknown. Among those conditions, a key factor determining whether individuals will be entitled to UI is the number of days they have worked over a given *base* or *reference/qualifying* period. Along such dimension, there exist significant variations, both across countries and within countries over time. Germany, Switzerland and Portugal require that individuals have worked for at least 12 months over the last two years to be eligible to Unemployment Benefits (UB). While seemingly identical, such condition is actually less demanding in Spain as unemployed must have worked for at least 12 months over the last 6 years. Iceland requires 3 months of work the year prior unemployment while in Finland individuals must have worked for at least 26 weeks, with a minimum of 18 hours of work per week, over the last 28 months. In the US, eligibility conditions are State dependent and can be based on cumulated wages perceived and/or on the number of days worked. In California for example UB eligibility depends on the cumulated value of wages prior unemployment while Illinois requires that individuals have worked for at least 12 months and have earned a minimum amount of cumulated wages. Ultimately these conditions still amount to some constraint on the number of days worked, as the longer the contract(s) duration, the higher the cumulated wage(s). These different

examples clearly illustrate the existence of substantial variation between countries[1]. Eligibility criteria also vary within countries over time. Italy reformed its UI system in 2015. For individuals losing their job up until the 30th of April 2015, 12 months of social security contributions in the two years prior the unemployment spell were necessary to open entitlement to UI. After this date, such level dropped to 13 weeks in the four years before unemployment, with at least 30 working days the year preceding the termination of employment. Despite having one of the least demanding eligibility thresholds among OECD countries – 122 days worked over the last 28 months - France implemented a reform on the 1st of October 2014 made to expand UI coverage to short contracts. This reform introduced a *top-up of entitlement* mechanism. For unemployed individuals receiving benefits, it reduced by more than four - from 122 to 30 - the minimum number of working days necessary to generate a new entitlement to UI. Such new entitlement can start straight after the previous one ended. This mechanically increased the potential value of short contracts, and could have further reinforced their development. Imagine a short time temporary contract for precisely 30 days paid at the minimum wage - 1445.38€ - in 2014. Prior the reform, such contract had a value represented by its wage. After the reform, it also, for unemployed individuals, opens a new entitlement to UI, with a daily UB level of about 31€ for 30 days. Ignoring aspects related to discounting and preference for the present, the reform increased the value of this contract by more than 64% - 930€ - therefore making it a lot more valuable[2].

UI systems are often seen as being in favour of job stability. UB represent a safety net, and provide unemployed with more time to find a good match, *i.e.* a job that would be a good fit considering their skills and experience. This paper challenges such perspective and asks whether, through their eligibility requirements, UI systems could foster job instability. While highly policy relevant, such question remains mostly unexplored by the literature. It will be analysed by exploiting the fourfold reduction in eligibility requirements introduced by the top-up of entitlement reform. If unemployed individuals are highly sensitive to such change and become more willing to accept short contracts, UI could become an important factor explaining the expansion of short temporary

[1] See table 2 in Tatsiramos and Van Ours [2014] for further examples.

[2] Figure 2.B.1 in the Appendix illustrates the differential, in terms of value, for contracts of less than 122 days between prior and after the 2014 reform.

contracts.

By using a regression kink design exploiting the timing of the reform, I show that the use of this mechanism significantly increased the probability to repeat unemployment within a short period of time. More precisely, after topping-up their entitlement, individuals realise the additional value the reform provided to short temporary contracts, revise downward their reservation wage and become more likely to accept such contracts. The hourly wage accepted by unemployed drops by about 0.3-0.5€ (3-4%). A heterogeneity analysis shows that most of the increase in the probability to repeat unemployment is driven by individuals that already experienced at least one unemployment spell in the past, further reducing their attachment to the labour market. Besides, individuals with the maximum entitlement, and therefore presumably the strongest attachment to the labour market, are unaffected.

The analysis is based on a French administrative dataset called FHS-D3. It is a panel of 10% of the unemployed population covering the period 2008-2017. While the FHS (*Fichier Historique Statistique*) mostly contains demographic characteristics at the individual level, the D3 has all the information about the unemployment spell, among which the start and end date of unemployment, the replacement rate, and number of days of entitlement to UB. It also contains details about individuals engaging in partial unemployment. These later indeed have to declare the number of hours worked and the corresponding wage for the Public Employment Agency (*Pôle Emploi*) to be able to adjust their UB level.

This paper is at the intersection of three different, while complementary, strands of the literature.

A first branch offers to analyse the effect of UI parameters. From a theoretical perspective, this literature expanded after the seminal work of Baily [1978], later generalised by Chetty [2006][3]. The so-called Baily Chetty formula highlights a key tradeoff between the gains from UI (consumption smoothing [CS]) and the corresponding cost (moral hazard [MH]). This formula relies on a set of key parameters that a large empirical literature then tried to identify, fostering the development of the sufficient statistics approach.

[3]Hopenhayn and Nicolini [1997], Hopenhayn and Nicolini [2009] and Shimer and Werning [2007] also provided key theoretical developments in the UI literature.

This empirical literature started with the work of Meyer [1990]. He first highlighted the existence of a spike in the exit rate out of unemployment shortly before benefits' exhaustion. The focus of this literature then shifted towards two key parameters of UI, namely the UB level (b) and the Potential Benefit Duration (PBD). While some papers exploited discontinuities in the former, mostly through regression kink design strategies (Lalive et al. [2006], Card et al. [2015a], Landais [2015], Kolsrud et al. [2018]), others analysed the effect of the latter (Lalive [2007], Schmieder et al. [2012b], Le Barbanchon [2016a]).

Among this branch of the literature, further attention has recently been devoted to the effect of UI on jobs' quality post unemployment. While prolonged UB may give the unemployed more time to find a good match, a longer unemployment spell may send a bad signal to employers, and lead to some further human capital depreciation (Pavoni [2009]). These effects, going in opposite directions, imply an ambiguous theoretical prediction. The literature has not reached any clear consensus on the question (Lalive [2007], Nekoei and Weber [2017], Le Barbanchon et al. [2017]).

Another branch of the literature focuses on partial unemployment – namely the possibility to receive a wage and UB at the same time (Le Barbanchon [2016b], Le Barbanchon [2015], Fremigacci and Terracol [2013], Fontaine and Rochut [2014], Kyyrä [2010], Gonthier and Le Barbanchon (2016)). While designed to incentivise unemployed to maintain some form of contact with the labour market, such mechanism could act as a lock-in and keep unemployed in a precarious situation, alternating between short contracts and unemployment. No clear results emerged from this literature. Fremigacci and Terracol [2013] for instance underline that in a first time a lock-in effect dominates, "*but that the overall effect eventually becomes positive*".

Finally, a third branch tries to understand the surge in the share of temporary contracts in new hirings. In its early stage, this literature did not really provide any explanation as to why firms would prefer to use temporary against permanent contracts. It simply assumed that while temporary contracts could be terminated at no cost, it was costly to fire an employee with a permanent contract (Blanchard and Landier [2002], Cahuc and Postel-Vinay [2002]). Such assumption certainly simplifies the theoretical analysis, but is at odds with the empirics. In many countries like France, ending a temporary

contract before its termination date is as costly as firing an employee with a permanent contract. Recent papers put forward the role of job protection legislation (Bassanini and Garnero [2013]), on the job search (Cao et al. [2010]), or of heterogeneity in the arrival rate of idiosyncratic productivity shocks (Cahuc et al. [2016]).
To the best of my knowledge, the closest article to this one is Baker and Rea Jr [1998]. They nevertheless exploit a different source of variation, namely an increase in eligibility requirements from 10 to 14 weeks of work in Canada, based on survey data. Their source of identification is also not so clear as eligibility conditions can vary across regions and unemployment rates.

This paper is organised as follows. The institutional details are presented in section 2.2. Section 2.3 presents the data and provides some descriptive statistics about the sample, while section 2.4 focuses on the empirical strategy and the key estimates from the regression kink design strategy. Section 2.5 explores two dimensions of heterogeneity. Section 2.6 discusses different validity checks, section 2.7 provides an estimate of the cost of the reform and section 2.8 concludes.

2.2 Institutional background and the French 2014 *top-up of entitlement* reform

UI schemes can often be summarised by a set of key parameters: the replacement rate, the PBD and eligibility conditions. This section provides further details about the french UI system and about the changes implemented by the 2014 reform.

2.2.1 Key parameters of the UI system

This paper will focus on the so-called *general regime*, which represents the large majority of unemployed and unemployment spells[4].
The replacement rate is computed based on an average wage identified over a period of a year prior the unemployment spell. It varies between 57 and 75 %[5]. Contrarily to

[4]The french UI system is made of 10 special regimes.

[5]This level used to be 57.4% prior the 2014 reform. The way the daily level of UB is determined is fairly complex:
$UB_d = min\Big[0.75 * \bar{w}, \max\{\gamma^* UB_{d,min},\ 40.4\%\ \bar{w} + \gamma^*\mathrm{F},\ 57.4\%\ \bar{w}\}\Big]$ where $\bar{w}$ corresponds to the

countries like Spain, where this replacement rate drops after a certain period of time[6], it is constant throughout the unemployment spell in France. UB are paid monthly, and individuals have to renew their registration to the Public Employment Agency every month to receive them.

Eligibility and length of entitlement to UI are a function of the number of days worked over the 28 months prior the unemployment spell. To be eligible, individuals need to have worked for at least 122 days. Above this threshold, a simple principle applies: 1 day worked equals 1 additional day of UI. This holds up to a maximum of 730 days for individuals below 50 years old, it goes up to 1095 days otherwise.

As in the US, this system also allows for partial unemployment. Unemployed can cumulate a wage with a fraction of their UB, under certain conditions. The main objective is to incentivise unemployed to maintain a link with the labour market. Jobs accepted while unemployed can indeed be a stepping stone towards a stable job (Fremigacci and Terracol [2013]). Further details about partial unemployment are provided in the Appendix.

2.2.2 The *top-up of entitlement* mechanism

Implemented on the 1st of October 2014, the top-up of entitlement reform introduced a new mechanism: the possibility for unemployed to top-up the duration of their entitlement[7]. The main idea is relatively straightforward. For an unemployed individual that would accept temporary contracts, any contract (or accumulation of contracts) of more than 30 days can, after the 2014 reform, open a new entitlement to UI. Prior the reform at least 122 days of work were necessary. This reform therefore increased the potential value of short contracts.

It applied from the 1st of October onward, universally, and retroactively. Consider the following example. Imagine an individual entitled to about a year (360 days) of UI. After consuming half of her entitlement, this individual accepts a 2 months contract, then goes back to unemployment and consumes her remaining entitlement to UI. The

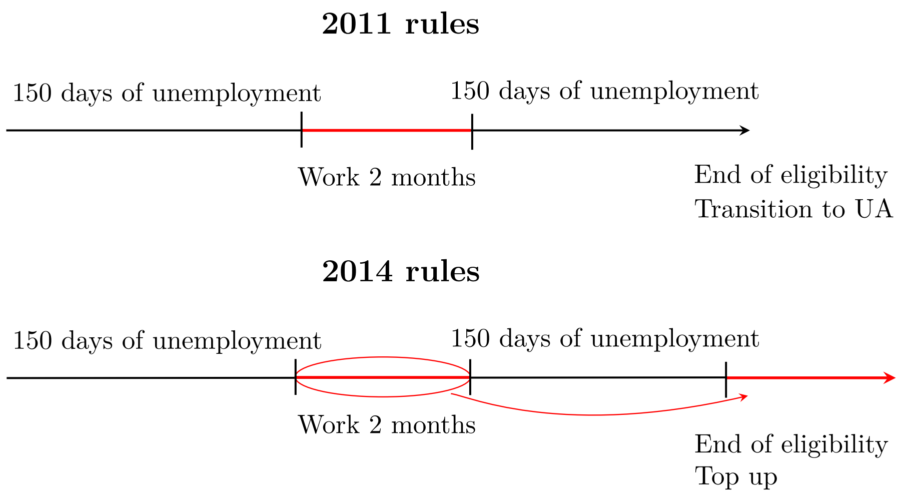
average wage the year prior unemployment, and F corresponds to a fixed amount of about 11€ that can be adjusted based on inflation. γ is a part time coefficient and $UB_{d,min}$ is a minimum level of daily UB.

[6]It drops from 70 to 50% after 6 months of unemployment

[7]In this paper, I will use interchangeably the words renewal and top up of entitlement

figure below illustrates the differential treatment of such situation depending on whether the unemployment spell ended before of after the reform.

Figure 2.1: 2014 reform and top-up of an entitlement



Notes: This figure illustrates the effect of the introduction of the top-up of entitlement mechanism. While the 2 months contract did not create any entitlement to UI under the 2011 rules, it does so after the 2014 reform. Post 2014 reform, these 2 months of work generate two additional months of entitlement to UI. Instead of transitioning to Unemployment Assistance (UA), the individual tops-up her entitlement and benefits from two additional months of UI.

As can be seen with Figure 2.1, while under the 2011 rules the 2 months contract did not open any entitlement to UI in the future, it did with the 2014 reform. Instead of transitioning from UI to Unemployment Assistance (UA), the individual benefits from 2 additional months of eligibility coming from the contract she accepted halfway through the unemployment spell. Consequently, this reform increased the potential value of short temporary contracts. Once aware of this, unemployed should revise downward their reservation wage and be more likely to accept such short contracts. If these contracts are then not renewed, unemployed accepting them will also be more likely to repeat the unemployment experience.

2.3 Data and Sample Selection

This section first provides detailed information about the data used, and then offers descriptive statistics along with further details about the sample.

2.3.1 Data

This paper relies on French administrative data from the Public Employment Agency called the FHS-D3. This dataset contains a representative panel of 10% of the unemployed population, from 2008 until 2017. I have the full unemployment history of the individuals selected over this 10 years period. The FHS-D3 is made of two core datasets, the FHS (*Fichier Historique Statistique*) and the D3.

The FHS contains demographic characteristics measured at the time individuals register as unemployed. It contains details about their gender, age, level of education, marital status, number of children etc. Interestingly, it also contains some questions regarding the type of job individuals are looking for, whether is it a full time or part time job, and whether they would prefer a permanent or a temporary contract.

The D3 contains details about the unemployment spells' characteristics[8]: when it started and (eventually) ended, the length of the spell, the number of days of entitlement to UI, the level of UB, the replacement rate, the wage level prior unemployment etc. Importantly, since this dataset is a panel, if an individual repeats the unemployment experience multiple times, I observe the details about the characteristics of every spell. This dataset also provides information about partial unemployment. More precisely, individuals have to declare to the Public Employment Agency how many hours they worked during a given month and how much they were paid, so that their level of UB can be adjusted. The panel dimension of the data, combined with the information on partial unemployment, will play a key role when it comes to estimating the effects of this reform on the evolution of the hourly wage individuals are willing to accept to work. It will also be a key feature in the estimation of the effect of the reform on individuals' probability to repeat unemployment.

[8]To be precise, the D3 is made of four datasets, each containing some specific information, that can then be combined together.

2.3.2 Sample

The core sample in this paper is made of individuals aged 18 to 49 years old, registered within the general regime, that have not been registered within a specific regime before, and that will not experience more than 5 unemployment spells over the period where I observe them. These restrictions are made to try and capture the effect of the reform on individuals that had a relatively strong attachment to the labour market. Individuals registered within one of the 10 specific regimes are indeed a lot more likely to repeat unemployment a large number of times. I also restrict the sample to individuals younger than 50 years old as rules about the entitlement length become different after this age. Some early retirement schemes may also become available. The sample is also restricted to individuals that stay unemployed for less than 1100 days, the 99th percentile of unemployment length distribution, therefore minimising problems related to long term unemployment.

The key characteristics of the sample are displayed in Table 2.1 below. The first column provides details for the full population. The average individual in the sample is a woman aged 29, that used to earn about 56€ a day (about 1,680€ a month for a full time job), and used on average 3 different employment contracts to open an eligibility to UI. The next columns provide the same details for three groups having distinct length of entitlement to UI. The largest group has the maximum entitlement length, 730 days. Such decomposition highlights the presence of heterogeneity among the population. Individuals with a smaller entitlement to UI are on average younger and less educated. They used to work for a lower wage, were more likely to have a temporary contract and to work part-time prior unemployment. But the larger this entitlement to UI, the older the individuals, the higher their wage and the more likely they were to have a single, full-time and permanent contract prior unemployment. The length of entitlement to UI therefore seems to capture various dimensions of heterogeneity, both in terms of demographic characteristics and labour market history.

Table 2.1: Descriptive Statistics - Main sample

	Full population	[0,365]	[366,729]	[730,730]
Demographic characteristics				
Age	29.44	27.37	28.45	32.20
	(8.272)	(7.892)	(7.766)	(8.245)
Fraction of women	0.527	0.561	0.537	0.486
	(0.499)	(0.496)	(0.499)	(0.500)
Education	6.091	5.944	6.143	6.195
	(1.920)	(2.001)	(1.911)	(1.835)
Fraction married	0.385	0.294	0.343	0.496
	(0.485)	(0.453)	(0.473)	(0.499)
Fraction with kids	0.364	0.276	0.329	0.467
	(0.481)	(0.447)	(0.470)	(0.499)
Labour market/unemployment characteristics				
Frac. engaging in partial unemp.	0.640	0.614	0.656	0.652
	(0.480)	(0.487)	(0.475)	(0.476)
Daily Wage (euros)	56.18	49.79	52.68	65.00
	(26.246)	(19.175)	(23.845)	(31.040)
Fraction worked PT	0.311	0.420	0.327	0.195
	(0.463)	(0.494)	(0.469)	(0.396)
PT intensity	0.908	0.880	0.899	0.943
	(0.175)	(0.192)	(0.181)	(0.144)
Frac. worked temporary contract	0.499	0.766	0.528	0.219
	(0.500)	(0.423)	(0.499)	(0.413)
Frac. used top-up	0.0710	0.114	0.0590	0.0380
	(0.257)	(0.318)	(0.236)	(0.192)
Eligibility (days)	491	214	530	730
	(232.677)	(75.491)	(112.091)	(0.000)
Replacement rate (gross)	0.621	0.626	0.629	0.611
	(0.053)	(0.047)	(0.052)	(0.057)
Unemployment length (days)	380	254	408	481
	(248.609)	(155.642)	(231.199)	(279.873)
Nb. Emp. Contract	3.182	4.379	3.474	1.797
	(4.288)	(5.155)	(4.532)	(2.325)
Avg length Emp contract (days)	397.7	143.9	356.8	675.8
	(319.595)	(168.107)	(252.812)	(248.375)
Median length Emp contract (days)	386	136	339	666
	(330.496)	(172.615)	(271.270)	(269.125)
Observations	193,023	68,698	53,327	70,998

Notes: This table reports average values and standard deviations between parenthesis from the FHS-D3. Column 1 provides detailed information about the characteristics of the main sample. Columns 2 to 4 split this sample by length of entitlement to UI. 730 days corresponds to the largest possible entitlement. It also constitutes the largest group. PT refers to Part-Time.

2.4 Empirical Analysis

This section presents the empirical analysis. It first explains the source of identification that will be used in this article, defines the corresponding theoretical concepts and provides evidence in favour of the strategy developed. It then presents the main estimates and later discusses the effects on wages.

2.4.1 Source of Identification

To identify the effects of this change in terms of eligibility condition, this paper will use a strategy based on the timing of the reform. The new rules applied to every unemployment spell, whether it started after the 1st of October 2014, or before and was still ongoing after this date.

One can use the fact that the spells that started before this date were more or less likely to last long enough to be exposed to the new rules. More precisely, I will exploit a strategy based on the combination of two elements. First, I will use the starting date of the unemployment spells. This variable can nevertheless be manipulated, and is thus very likely to be endogenous. I will therefore supplement it with the length of entitlement to UI, and use the combination of these two variables as an instrument. This paper will rely on two key theoretical variables, first the Theoretical Exhaustion Date (TED), and then the distance (R) between the date of the reform and this TED.

$$\textbf{TED} = \text{Starting date of Unemployment} + \#\text{ days of entitlement to UI} \tag{2.1}$$

$$\textbf{R} = \text{Reform Date - TED} \tag{2.2}$$

The TED indicates when an unemployed would have exhausted her UB had she consumed her entitlement on a monthly basis. It is key to understand that this variable is unaffected by the decisions an unemployed makes during her unemployment spell. For example, the TED will remain unchanged, whether this individual chooses to engage in partial unemployment or not, whether she leaves unemployment after one month, two months etc. While intuitive, the relationship between the TED and the probability to top-up one's entitlement has nothing entirely mechanical. Despite having a TED occurring after the reform, individuals could perfectly leave unemployment earlier and find a permanent job, or leave unemployment after exhausting their entitlement without topping up.

Pros and cons of the TED

Remember that the length of entitlement to UI is computed based on the number of days worked in the last 28 months prior unemployment. Consequently, the TED is a variable that could hardly be manipulated by individuals. It would require them to *(i)* remember how many days they worked in the last two years and a half, *(ii)* to manipulate the end date of their contract, *(iii)* to know a few months ahead when the reform would be implemented *(iv)* and to understand it very well.
Since the main analysis will be focused on a window around the time of the reform, the TED presents another advantage. It will lead the empirical analysis to be mostly focused on individuals that registered as unemployed prior the reform, therefore limiting concerns about adverse selection into unemployment. It is indeed possible that, once the reform implemented, individuals will register as unemployed whereas they would have chosen not to do so absent the possibility of topping-up their entitlement.

The main inconvenient that comes with such variable is illustrated in Table 2.1 below. As can be seen from this table, while individuals A, B and C have very different lengths of entitlement to UI, and are therefore likely to have very different characteristics and attachment to the labour market, they share the same TED. These individuals would, in a regression analysis, and despite their different characteristics, be directly compared to one another based on their use or not of the top-up mechanism. Pooling them altogether would hence lead to measure an average effect across very different sub-populations. Such issue can nevertheless be taken into account in the empirical analysis by grouping individuals in bins of entitlement length.

Table 2.1: TED and eligibility length

Individual	Starting date unemployment	Entitlement length (days)	TED
A	1st Oct 2012	730	1st Oct 2014
B	1st Oct 2013	365	1st Oct 2014
C	1st May 2014	153	1st Oct 2014

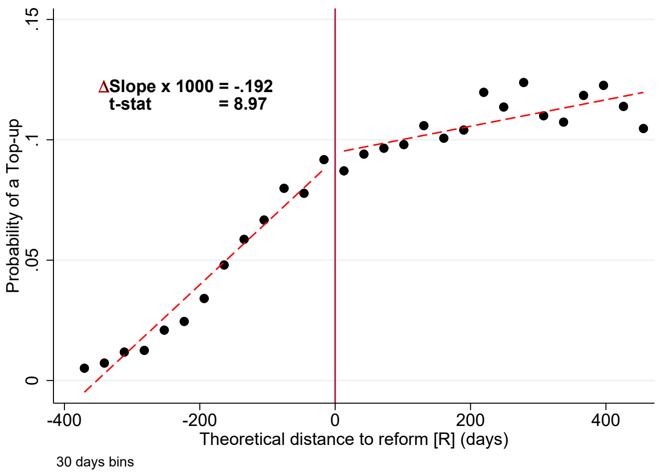
Notes: This table illustrates the main downside of the TED. Individuals with different length of entitlement to UI, and possibly very different characteristics, can share the same TED, as illustrated with individuals A,B and C.

This discussion suggests an interesting dimension of heterogeneity that should be exploited. As underlined by Table 2.1, the length of entitlement to UI is a variable that captures many aspects of heterogeneity, both in terms of demographic characteristics and attachment to the labour market. The section devoted to the heterogeneity analysis will therefore split the sample into different entitlement groups in line with Table 2.1, and analyse whether individuals with smaller entitlement to UI, and presumably smaller attachment to the labour market, are more affected by the reform. If anything, Table 2.1 shows that they top-up their entitlement relatively more, which suggests that the reform did affect relatively more the targeted populations.

2.4.2 Graphical Evidence

The question is now to know whether the distance between the date of the reform and the TED - R - captures some information regarding individuals' probability to top-up their entitlement. Figure 2.1 below provides clear evidence that this is indeed the case.

Figure 2.1: Probability to top-up one's entitlement and R

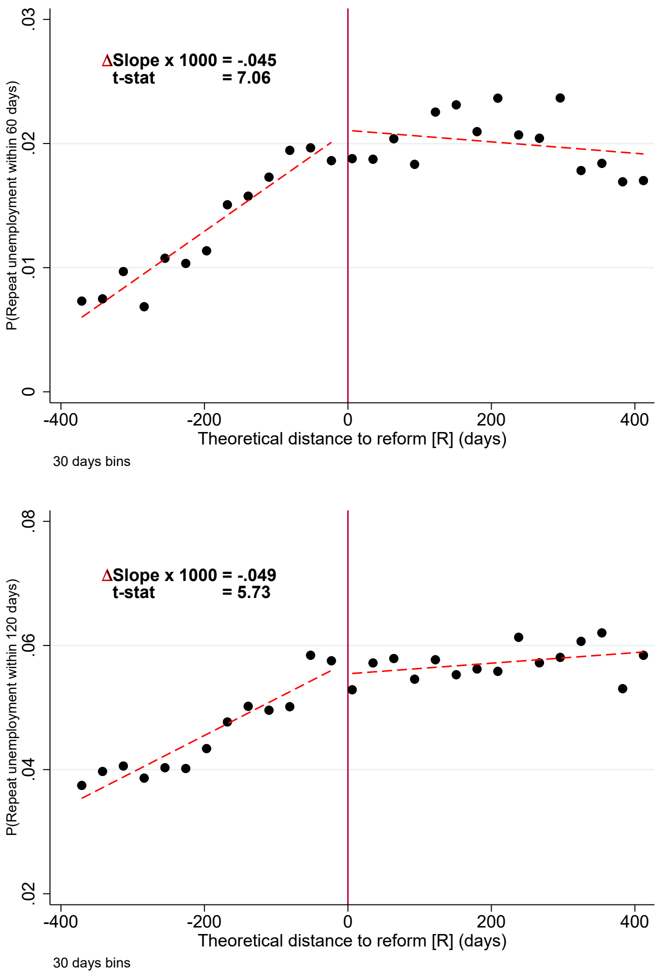


Notes: This figure illustrates the relationship between R and the probability that a given unemployed uses the top-up of entitlement mechanism. The reported change in slope corresponds to the coefficient β_2 in the following regression: $Y = \alpha + \beta_1 * R + \beta_2 * R * Post + \varepsilon$, where Post corresponds to a dummy variable taking a value of 1 if R>0.

As can be seen from this figure, the smaller the distance between the TED and the date of the reform, the more likely individuals are to top-up their entitlement. The relationship between these two variables displays a clear kink at the time of the reform. One now has to analyse whether this kink in the relationship between the probability to top-up one's entitlement and the distance to the reform has any effect on different outcomes of interest. This paper will focus on individuals' probability to repeat unemployment within # days. Figure 2.2 clearly suggest that the use of the top-up of entitlement mechanism does affect the probability to repeat unemployment.

Identifying which empirical strategy to use before analysing these figures was not obvious. The reform could have generated a jump in the probability to repeat unemployment, or simply no effect at all. These figures nevertheless made it clear that the relevant empirical method to use is a Regression Kink Design (RKD).

Figure 2.2: $\mathbb{P}$(repeat unemployment within # days) and distance to the reform



Notes: This figure plots by bins of 30 days the average probability to repeat unemployment within 60 and 120 days as a function of the distance between the TED and the time of the reform. The reported change in slope corresponds to the coefficient β_2 in the following regression: $Y = \alpha + \beta_1 * R + \beta_2 * R * Post + \varepsilon$, where Post corresponds to a dummy variable taking a value of 1 if R>0.

2.4.3 Regression Kink Design (RKD)

The RKD is a method that has been largely used to measure the effect of different social insurance programs, particularly for UI (Card et al. [2015a], Landais [2015], Kolsrud et al. [2018], among others).

It relies on two key assumptions. First of all, individuals must be unable to perfectly manipulate the running variable (R). Secondly, the distribution of heterogeneity must be smooth around the kink[9].

The core idea is very similar to an IV strategy, where the change in slope in Figure 2.1

[9]For further details about this method, see Card et al. [2012], Card et al. [2015b] and Card et al. [2016].

would give the coefficient of the first stage, whereas the changes in slopes in figures 2.2 would give the coefficient of the reduced form. The RKD estimate then corresponds to the ratio of these two coefficients:

$$\tau_{RKD} = \frac{\lim\limits_{r_0 \to 0^+} \frac{d\mathbb{E}(Y|_{R=r})}{dr}\Big|_{r=r_0} - \lim\limits_{r_0 \to 0^-} \frac{d\mathbb{E}(Y|_{R=r})}{dr}\Big|_{r=r_0}}{\lim\limits_{r_0 \to 0^+} \frac{d\mathbb{E}(T|_{R=r})}{dr}\Big|_{r=r_0} - \lim\limits_{r_0 \to 0^-} \frac{d\mathbb{E}(T|_{R=r})}{dr}\Big|_{r=r_0}} \tag{2.3}$$

Where the running variable, R, corresponds to the distance between the reform date and the TED, Y corresponds to the outcome of interest, T the fact of topping-up one's entitlement, and r_0 refers to a case where $R = 0$ (*i.e.* TED=Reform Date=1st of October 2014).

2.4.4 Estimates

The next table summarises the results of the RKD, for different horizons regarding the outcome of interest, and for different sets of controls. To minimise space, it only displays the elasticities (ε) obtained based on the RKD estimates. These elasticities measure by how much the probability to repeat unemployment within # days would increase if the fraction of the unemployed population topping-up its entitlement to UI were to increase by 1%. Note that the number of observations decreases with the time horizon considered. This comes from the fact that, while an individual finishing her unemployment spell in September 2017 would still be taken into account when measuring the effect of a top-up on the probability to repeat unemployment in 60 days, this same individual would not be taken into account when it comes to the probability to repeat unemployment in 120 days or more as the data stop in December 2017.

The different columns of Tables 2.2 and 2.3 introduce an increasingly conservative set of controls. The first column incorporates no controls, and is therefore subject to the problem highlighted via Table 2.1. The second one takes into account such concern, and controls for bins of entitlement length. On top of this, column three takes into account the effect of time and controls for seasonality by introducing both year and quarter fixed effects. Finally, the fourth column adds a large set of controls, both in terms of demographic, unemployment and labour market characteristics[10].

First of all, note that the elasticities remain very similar across the different columns. The discrepancy nevertheless increases with the time horizon considered. Independently of the set of controls, the elasticities are clearly decreasing over the time horizon considered. This directly suggests that after benefiting from a top-up of their entitlement, individuals realise the additional value short temporary contracts have. They then become more likely to accept such contracts. Nevertheless, once these later end and are not renewed, individuals register as unemployed again. The fact that these elasticities are strongly positive for durations of 30 to 90 days, and become much smaller thereafter confirms that the duration of contracts accepted by individuals to leave unemployment was short.

So far the analysis suggests that after experiencing a top-up of their entitlement, individuals are more willing to take on short temporary contracts, and then become more likely to repeat the unemployment experience. One can push the analysis one step further and ask whether such reform had an effect on the hourly wage individuals were willing to accept to engage in partial unemployment. If individuals realise that the reform provided short temporary contracts with an additional insurance value, they should be willing to accept them for a smaller wage. The next subsection offers to explore such question.

[10] This set of controls includes variables for the effect of age, gender, education, marital status, presence or not of children, a fixed effect for the departement, whether the individual was working part time prior unemployment, whether the contract was a temporary contract, the average wage, the number of contract used to open eligibility to UI and their average length.

Table 2.2: RKD estimates I

	No controls	Eligibility control	Elig + Time FE	Full controls
		$\mathbb{P}$(Repeat unemployment within 30 days)		
ε	.771***	.809***	.726***	.683***
	(.140)	(.120)	(.101)	(.111)
N	106,386	106,386	106,386	102,229
		$\mathbb{P}$(Repeat unemployment within 60 days)		
ε	.697***	.742***	.661***	.625***
	(.140)	(.119)	(.100)	(.111)
N	106,386	106,386	106,386	102,229
		$\mathbb{P}$(Repeat unemployment within 90 days)		
ε	.630***	.666***	.592***	.568***
	(.129)	(.112)	(.095)	(.105)
N	106,305	106,305	106,305	102,150
		$\mathbb{P}$(Repeat unemployment within 120 days)		
ε	.290***	.260***	.227***	.229***
	(.089)	(.074)	(.060)	(.064)
N	106,124	106,124	106,124	101,975
Year FE	✗	✗	✓	✓
Quarter FE	✗	✗	✓	✓
Departement FE	✗	✗	✗	✓
Covariates	✗	✗	✗	✓
Bandwidth	[350,350]	[350,350]	[350,350]	[350,350]

Notes: Standard errors between parenthesis. They are clustered by departement and year. Elasticities are obtained as follows: $\varepsilon = \tau_{RKD} * \frac{\bar{\mathbb{P}}(TopUp)}{\bar{\mathbb{P}}(Rep\ within\ \#d)}$. where $\bar{\mathbb{P}}()$ corresponds to the average empirical probability of the corresponding event at the kink. Covariates include variables for the effect of age, gender, education, marital status, presence or not of kids, a dummy indicating whether the individual was working part time prior unemployment, one indicating whether the contract was a temporary contract, the average wage, the number of contract used to open eligibility to UI and their average length.

*,** and *** denote significance at the 10%, 5% and 1% levels, respectively.

Table 2.3: RKD estimates I

	No controls	Eligibility control	Elig + Time FE	Full controls
	$\mathbb{P}$(Repeat unemployment within 180 days)			
ε	.199***	.178***	.139***	.130***
	(.063)	(.053)	(.042)	(.047)
N	105,906	105,906	105,906	101,767
	$\mathbb{P}$(Repeat unemployment within 365 days)			
ε	.113***	.110***	.065***	.053**
	(.035)	(.031)	(.025)	(.027)
N	105,023	105,023	105,023	100,916
Year FE	✗	✗	✓	✓
Quarter FE	✗	✗	✓	✓
Departement FE	✗	✗	✗	✓
Covariates	✗	✗	✗	✓
Bandwidth	[350,350]	[350,350]	[350,350]	[350,350]

Notes: Standard errors between parenthesis. They are clustered by departement and year. Elasticities are obtained as follows: $\varepsilon = \tau_{RKD} * \frac{\bar{\mathbb{P}}(TopUp)}{\bar{\mathbb{P}}(Rep\ within\ \#d)}$. where $\bar{\mathbb{P}}()$ corresponds to the average empirical probability of the corresponding event at the kink. Covariates include variables for the effect of age, gender, education, marital status, presence or not of kids, a dummy indicating whether the individual was working part time prior unemployment, one indicating whether the contract was a temporary contract, the average wage, the number of contract used to open eligibility to UI and their average length.
*,** and *** denote significance at the 10%, 5% and 1% levels, respectively.

2.4.5 Effects on wages

This subsection analyses whether individuals accepted a drop in their hourly wage in order to top-up their entitlement for the first time. If this happens to be the case, this would suggest that individuals understood the effect of the reform and re-adjusted their reservation wage accordingly. It also analyses whether such re-evaluation only happened after individuals experienced their first top-up, which would *(i)* suggest the presence of informational issues and *(ii)* rule out manipulation concerns.
The key specification this subsection relies on is as follows:

$$\Delta hwage_{idt} = \alpha + \beta Topup_{idt} + \delta Post\ Topup_{idt} + \gamma \Delta X_{idt} + \varepsilon_{idt} \tag{2.4}$$

Where i corresponds to the individual, d the "departement"[11] and t to the year. X_{idt} corresponds to a set of controls that includes bins for the entitlement length, the age,

[11] A French geographical unit. France has 101 departements.

the number of employment contract used to open eligibility to UI, their average length, the average wage prior unemployment, and a control for whether individuals worked part-time or not. Topup and Post Topup are the two key variables of interest in this equation. They correspond to dummy variables. The first one takes a value of 1 if the spell was made possible *via* the top-up mechanism. It therefore captures whether individuals accepted a change in their hourly wage in order to be able to top-up their entitlement to UI. The second one takes a value of one if the individual repeats the unemployment experience after her first top-up. One can reasonably assume that after experiencing a first top-up, individuals became familiar with the mechanism. Note that the analysis in this section requires to restrict the sample to individuals that will repeat the unemployment experience at least twice, and that will engage in partial unemployment.

To estimate such equation, I will leverage two aspects of the data. First of all, I will use details about partial unemployment. As explained previously, individuals working while unemployed have to declare to the Public Employment Agency how many hours they worked in the month, and how much they were paid as these details are then used to adjust their UB level. I can then use the panel structure of the data. Some individuals will indeed repeat the unemployment experience, and will engage in partial unemployment for consecutive unemployment spells. I can therefore analyse the variation in the hourly wage they worked for across unemployment spells. Different sources of variation can then be exploited to estimate equation 2.4.

First of all, I can proceed to a simple comparison across individuals (Column 1 in table 2.4 below), between those that repeat unemployment and top-up their entitlement, and those that repeat unemployment without using this mechanism. I can then refine the previous analysis and try to reduce concerns about selection into the top-up mechanism by using a nearest neighbour matching (column 2 of Table 2.4). The selection concern would nevertheless remain valid as these two empirical strategies exploit between individual variations across treated and non treated.

To deal with such selection into the top-up mechanism, a solution is to rely only on the within individual variation by fully exploiting the panel dimension of the data. I can

indeed focus on individuals that will *(i)* use the top up mechanism, and *(ii)* experience at least three unemployment spells, one before the top-up takes place, and one after. I can then explore whether to use this mechanism, unemployed accepted a drop in their hourly wage compared to the one they worked for during their previous unemployment spell. If this is indeed the case, then the coefficient β should be negative. And if they repeat the unemployment experience after using it, I can also analyse the variation in their hourly wage post top-up. Such variation will be given by the coefficient δ in equation 2.4. If the hourly wage only drops after using this mechanism ($\beta \approx 0$ and $\delta < 0$), this would *(i)* tend to confirm the absence of strategic behaviour from the unemployed, and *(ii)* suggest the presence of some informational issues. This would also, by revealed preference, tell us something about individuals' valuation of UI.
The last column of table 2.4 proceeds to a simple test. Among the group of unemployed that repeat unemployment at least 3 times, but never use the top up mechanism, I allocate a random use of the top-up of entitlement, and ask whether the hourly wage changed, before or after using such mechanism. I repeat such experiment 200 times. The coefficients in column 4 (Placebo) correspond to the average and standard deviations of these 200 estimates.

Table 2.4 below contains point estimates for these different empirical strategies. The main message from this table is very clear, and consistent across the different strategies. While individuals do not seem to accept any drop in their hourly wage in order to top-up their entitlement for the first time, they clearly do so after using this mechanism. This confirms the presence of an informational issue. Only once individuals experience a top-up do they realise the additional value the reform provided to short contracts, and only then do they revise downward their reservation wage and accept to work for a smaller hourly wage. Note that on average, individuals engage in partial unemployment for an hourly wage of about 12€. A drop of 0.35-0.5€ therefore represents a decrease of about 3 to 4%.

Table 2.4: Top up and change in the hourly wage

Δ hwage	Full Sample	NN	Within	Placebo
No controls				
Top Up	-0.004	0.058	0.133	0.189
	(0.066)	(0.065)	(0.103)	(0.377)
Post Top Up	-0.603***	-0.564***	-0.495***	-0.188
	(0.065)	(0.069)	(0.105)	(0.385)
Observations	19,843	10,928	6,424	10,036
With controls				
Top Up	-0.031	0.057	0.127	0.178
	(0.069)	(0.070)	(0.110)	(0.362)
Post Top Up	-0.467***	-0.393***	-0.337***	-0.180
	(0.070)	(0.079)	(0.109)	(0.381)
Observations	18,994	10,433	6,152	9,614

Notes: Standard errors are clustered by departement and year.
Top up corresponds to a dummy variable with a value of 1 if the unemployment spell was opened using the top-up mechanism. Post Top Up is a dummy variable taking a value of one is the spell started after the individual topped-up her entitlement for the first time. The different columns exploit different sources of variation detailed in the subsection prior this table. The controls include the variation in the entitlement length, in the wage prior unemployment, in a variable controlling for whether the individuals worked full-time or not, in the number of contracts to open eligibility, in the average length of these contracts and in the age.
*,** and *** denote significance at the 10%, 5% and 1% levels, respectively.

2.5 Heterogeneity analysis

This section explores two dimensions of heterogeneity.

First, it asks whether individuals with relatively more unemployment experience, and therefore a relatively smaller attachment to the labour market, are more affected by this reform. To answer this question, it repeats the previous analysis by splitting the unemployed population into two categories, experienced and inexperienced unemployed. The second dimension this section explores directly comes from the very nature of the running variable. Indeed, as discussed in subsection 2.4.1, since R is a direct function of the entitlement length, it seems natural, instead of controlling for such variable, to split the unemployed population into groups with different length of entitlement to UI.

2.5.1 Effect of unemployment experience

This subsection explores an interesting dimension of heterogeneity, namely unemployment experience. It repeats the previous analysis by splitting the population into inexperienced and experienced unemployed.

The first category, inexperienced unemployed, is made of unemployed who used the top-up mechanism by the end of their very first unemployment spell. One could argue that such category is actually made of the most strategic individuals who decided to register as unemployed because they had a very good understanding of the reform. This is nevertheless unlikely to be the case since my sample is mostly made of individuals who registered as unemployed before the reform was implemented[12]. The second category, experienced unemployed, will be made of individuals who already completed at least one unemployment spell before they topped-up their entitlement. Note that experienced unemployed are more likely to be familiar with the parameters of the UI system, and to potentially use the 2014 reform to their advantage. If this indeed happened, the reform may well have reduced even further these individuals' attachment to the labour market. Tables 2.1 and 2.2 below contain the estimates with such decomposition.

These tables make it very clear that the effect of the reform mostly comes from the experienced unemployed. For horizons below 90 days, both categories are affected.

[12] Also see the discussion in section 2.4.1

The effect of the top-up mechanism on the probability to repeat unemployment is nevertheless generally two to three times larger for the experienced unemployed. Beyond this horizon, the effect of the reform is no longer significant for the inexperienced unemployed, whereas it persists and remains relatively strong, though a lot weaker compared to shorter horizons, for experienced unemployed. This suggests that the reform decreased even further the attachment to the labour market of individuals who already alternated between employment and unemployment in the past.

Table 2.1: Heterogeneity in Unemployment Experience - I

	No controls	Eligibility control	Elig + Time FE	Full controls
	$\mathbb{P}$(Repeat unemployment within 30 days)			
Inexperienced unemployed				
ε	.524***	.567***	.531***	.512***
	(.182)	(.159)	(.139)	(.158)
N	65,755	65,755	65,755	62,980
Experienced unemployed				
ε	1.04***	1.04***	.937***	.851***
	(.191)	(.168)	(.147)	(.161)
N	40,631	40,631	40,631	39,249
	$\mathbb{P}$(Repeat unemployment within 60 days)			
Inexperienced unemployed				
ε	.428**	.479***	.442***	.432***
	(.186)	(.159)	(.141)	(.158)
N	65,755	65,755	65,755	62,980
Experienced unemployed				
ε	1.00***	1.01***	.907***	.830***
	(.188)	(.166)	(.145)	(.158)
N	40,631	40,631	40,631	39,249
	$\mathbb{P}$(Repeat unemployment within 90 days)			
Inexperienced unemployed				
ε	.322*	.368**	.344***	.331**
	(.165)	(.143)	(.126)	(.143)
N	65,726	65,726	65,726	62,952
Experienced unemployed				
ε	1.02***	1.01***	.905***	.863***
	(.189)	(.166)	(.143)	(.156)
N	40,579	40,579	40,579	39,198
Year FE	✗	✗	✓	✓
Quarter FE	✗	✗	✓	✓
Departement FE	✗	✗	✗	✓
Covariates	✗	✗	✗	✓
Bandwidth	[350,350]	[350,350]	[350,350]	[350,350]

Notes: Standard errors between parenthesis. Standard errors are clustered by departement and year. Elasticities are obtained as follows: $\varepsilon = \tau_{RKD} * \frac{\bar{\mathbb{P}}(TopUp)}{\bar{\mathbb{P}}(Rep\ within\ \#d)}$. where $\bar{\mathbb{P}}()$ corresponds to the average empirical probability of the corresponding event at the kink. Covariates include variables for the effect of age, gender, education, marital status, presence or not of kids, a dummy indicating whether the individual was working part time prior unemployment, one indicating whether the contract was a temporary contract, the average wage, the number of contract used to open eligibility to UI and their average length.
*,** and *** denote significance at the 10%, 5% and 1% levels, respectively.

Table 2.2: Heterogeneity in Unemployment Experience - II

	No controls	Eligibility control	Elig + Time FE	Full controls
	$\mathbb{P}$(Repeat unemployment within 120 days)			
Inexperienced unemployed				
ε	0.101	0.092	0.089	0.085
	(.088)	(.081)	(.068)	(.076)
N	65,647	65,647	65,647	62,877
Experienced unemployed				
ε	.657***	.606***	.487***	.497***
	(.148)	(.121)	(.101)	(.109)
N	40,477	40,477	40,477	39,098
	$\mathbb{P}$(Repeat unemployment within 180 days)			
Inexperienced unemployed				
ε	0.031	0.027	0.019	0
	(.064)	(.060)	(.049)	(.057)
N	65,570	65,570	65,570	62,805
Experienced unemployed				
ε	.567***	.523***	.388***	.418***
	(.116)	(.095)	(.079)	(.085)
N	40,336	40,336	40,336	38,962
	$\mathbb{P}$(Repeat unemployment within 365 days)			
Inexperienced unemployed				
ε	0.032	0.033	0.003	-0.010
	(.037)	(.035)	(.028)	(.031)
N	65,206	65,206	65,206	62,456
Experienced unemployed				
ε	.292***	.284***	.183***	.182***
	(.061)	(.055)	(.047)	(.050)
N	39,817	39,817	39,817	38,460
Year FE	✗	✗	✓	✓
Quarter FE	✗	✗	✓	✓
Departement FE	✗	✗	✗	✓
Covariates	✗	✗	✗	✓
Bandwidth	[350,350]	[350,350]	[350,350]	[350,350]

Notes: Standard errors between parenthesis. Standard errors are clustered by departement and year. Elasticities are obtained as follows: $\varepsilon = \tau_{RKD} * \frac{\bar{\mathbb{P}}(TopUp)}{\bar{\mathbb{P}}(Rep\ within\ \#d)}$. where $\bar{\mathbb{P}}()$ corresponds to the average empirical probability of the corresponding event at the kink. Covariates include variables for the effect of age, gender, education, marital status, presence or not of kids, a dummy indicating whether the individual was working part time prior unemployment, one indicating whether the contract was a temporary contract, the average wage, the number of contract used to open eligibility to UI and their average length.
*,** and *** denote significance at the 10%, 5% and 1% levels, respectively.

2.5.2 Effect of the length of entitlement to UI

This subsection offers to explore another dimension of heterogeneity, namely the length of entitlement to UI. As illustrated by Table 2.1, individuals with a longer entitlement to UI are usually older, more educated, have a higher wage and a stronger attachment to the labour market. On the other hand, individuals in the smallest eligibility groups are more likely to be young women with a low level of education, alternating between multiple temporary part time jobs. It therefore seems that the length of entitlement to UI captures many dimensions of heterogeneity, both in terms of demographic characteristics and labour market history. Moreover, the very nature of the TED invites to proceed to such a decomposition of the sample.

In what follows, I will decompose the main sample into three eligibility groups, in line with Table 2.1. The first group is made of all the unemployed who, once they registered at the Public Employment Agency, had a length of entitlement to UI inferior or equal to 365 days. This group most likely contains individuals with the smallest attachment to the labour market as it contains individuals who worked for a maximum of a year over the last two years and a half. The second group contains individuals who had an entitlement between 366 and 729 days while the last one comprises individuals with the maximum entitlement to UI, 730 days. The characteristics of these three groups are provided in Table 2.1. Tables 2.3 and 2.4 below display the RKD estimates where I control for time fixed effects[13]. These tables enlighten the presence of a strong heterogeneity in terms of the effect of the reform. While the possibility to top-up one's entitlement significantly increases the probability to repeat unemployment for individuals with an entitlement to UI smaller than the maximum, individuals with the strongest attachment to the labour market were unaffected. Interestingly, the effect of a top-up are not the strongest for the first group, but for the one with an intermediate entitlement to UI. This therefore suggests that the reform decreased attachment to the labour market for individuals that were in intermediate situations.

[13]Specifications with no controls of the full set of controls are available upon request.

Table 2.3: Heterogeneity by entitlement length - I

	[0,365]	[366,729]	[730,730]
	$\mathbb{P}$(Repeat unemp. within 30 days)		
ε	.682***	1.23***	0.098
	(.128)	(.207)	(.456)
N	37,142	29,984	40,183
	$\mathbb{P}$(Repeat unemp. within 60 days)		
ε	.590***	1.19***	0.115
	(.128)	(.203)	(.448)
N	37,142	29,984	40,183
	$\mathbb{P}$(Repeat unemp. within 90 days)		
ε	.532***	1.16***	-0.050
	(.127)	(.206)	(.345)
N	37,092	29,964	40,171
	$\mathbb{P}$(Repeat unemp. within 120 days)		
ε	.257***	.462***	-0.070
	(.099)	(.111)	(.129)
N	36,952	29,935	40,159
Year FE	✓	✓	✓
Qtr FE	✓	✓	✓
Bandwidth	[350,350]	[350,350]	[350,350]

Notes: Standard errors between parenthesis. Standard errors are clustered by departement and year. Elasticities are obtained as follows: $\varepsilon = \tau_{RKD} * \frac{\bar{\mathbb{P}}(TopUp)}{\bar{\mathbb{P}}(Rep\ within\ \#d)}$. where $\bar{\mathbb{P}}()$ corresponds to the average empirical probability of the corresponding event at the kink.
*,** and *** denote significance at the 10%, 5% and 1% levels, respectively.

Table 2.4: Heterogeneity by entitlement length - II

	[0,365]	[366,729]	[730,730]
	$\mathbb{P}$(Repeat unemp. within 180 days)		
ε	.136*	.303***	0.000
	(.081)	(.077)	(.094)
N	36,789	29,900	40,138
	$\mathbb{P}$(Repeat unemp. within 365 days)		
ε	0.058	.094*	0.060
	(.046)	(.051)	(.053)
N	36,143	29,734	40,062
Year FE	✓	✓	✓
Quarter FE	✓	✓	✓
Bandwidth	[350,350]	[350,350]	[350,350]

Notes: Standard errors between parenthesis. Standard errors are clustered by departement and year. Elasticities are obtained as follows: $\varepsilon = \tau_{RKD} * \frac{\bar{\mathbb{P}}(TopUp)}{\bar{\mathbb{P}}(Rep\ within\ \#d)}$. where $\bar{\mathbb{P}}()$ corresponds to the average empirical probability of the corresponding event at the kink.
*,** and *** denote significance at the 10%, 5% and 1% levels, respectively.

2.6 Validity

With the rapidly expanding empirical literature exploiting regression discontinuity and kink designs, an increasing number of validity checks have also been developed. Among these tests, this section will focus on the McCrary [2008] test, tests related to the smoothness of the distribution of the covariates around the kink, and on the permutation test (Ganong and Jäger [2018]). Variations in terms of polynomial order and bandwidth size are relayed in a technical appendix available upon request.

2.6.1 McCrary [2008] test and smoothness of the covariates

The McCrary [2008] test has been developed to detect potential manipulation of the running variable. In the present case it assesses whether the distribution of the number of individuals evolves smoothly around R: the distance between the TED and the time of the reform. If such distribution were to exhibit a jump or a kink around R=0, this would indicate the presence of a strong manipulation of the running variable, preventing any causal interpretation of the RKD estimates. One could for example imagine that individuals that would not have registered as unemployed decided to do so after learning about the reform and in anticipation of its implementation. If this were to be the case, the number of unemployed should be larger prior the reform.

Since the running variable in this article ultimately corresponds to a date, I face an additional issue. The evolution of the unemployment rate is indeed highly seasonal, leading to significant variations in the number of unemployed across months (see figure 2.B.2 in the Appendix). Every year, the unemployment rate tends to be higher after summer, *i.e.* in September-October, which coincides with the time of the reform. A McCrary test on the raw data would therefore necessarily lead to the conclusion of a non smoothness of the distribution of the number of unemployed at the time of the reform, even absent any manipulation of the running variable. The problem will be the same when trying to estimate the smoothness of the distribution of the covariates around the kink. Indeed, consider the following situation. Imagine that individuals that register as unemployed right after summer are a few years younger compared to other individuals that normally register as unemployed. This implies that on average, the age of the unemployed population in September-October will decrease. If I compare this age to the one of the unemployed population before this period, the averages may well be different, not for reasons related to the reform, but due to seasonality effects. I will therefore apply all the tests in this section on deseasonalised data.

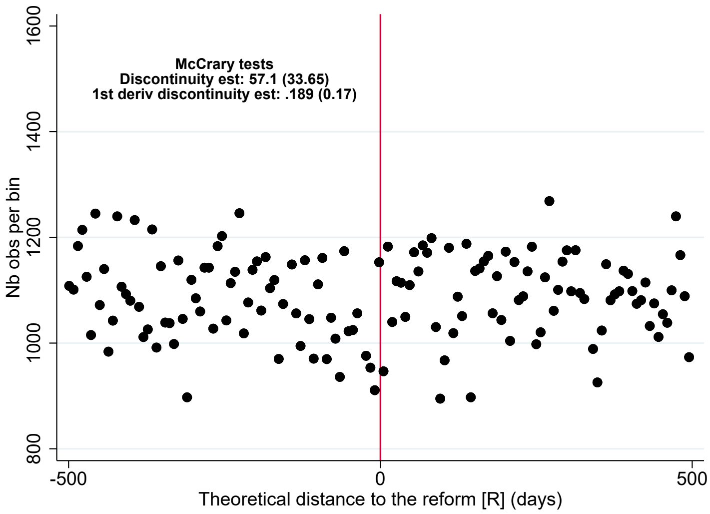
The key equation of interest is as follows:

$$\mathbb{E}[\tilde{Y}|_{R=r}] = \alpha + [\sum_{p=1}^{N} \beta_p (r - r_0)^p + \delta_p (r - r_0)^p * Post] + \gamma * Post \quad \text{where } |r - r_0| \leq \text{h} \tag{2.5}$$

Where $\tilde{Y}$ corresponds to the deseasonalised outcome of interest. Post corresponds to a dummy variable taking a value of one if the TED takes place after the date of the reform ($Post = \mathbb{1}[TED \geq Reform\ Date]$), r_0 is the reform date and h corresponds to the bandwidth size. In what follows I will test whether δ_1 and γ are equal to zero or not.

Figure 2.1 below summarizes the result of the McCrary test while Table 2.1 provides further details about the tests related to the smoothness of the covariates.

Figure 2.1: Pseudo McCrary test



Note: This figure displays the deseasonalised number of observation per bins of 7 days. It also contains the results of a McCrary test, and of a test of the discontinuity of the first derivative of the pdf, both non significant at the 5% level. These tests have been done on $\tilde{Y}$, where this variable corresponds to the deseasonalised number of observation.

This figure clearly suggests an absence of manipulation of the running variable, a key element for the validity of the RKD.

Table 2.1 below confirms that all of the covariates are smoothly distributed around the kink once seasonality effects are controlled for. It tests for both the presence of a jump and of a kink. Only one case turns out to be significant at the 10% level, the presence of a jump in the fraction of women.

Table 2.1: Pseudo McCrary and Smoothness of the Covariates

	Test for a jump		**Test for a kink**	
Variable	Coefficient	SE	Coefficient	SE
		McCrary test		
Density	24.197	123.561	-0.318	0.622
		Demographic Characteristics		
Fraction women	-0.009	0.005	0.000	0.000
Age	0.007	0.185	-0.000	0.001
Education	0.012	0.034	0.000	0.000
Fraction married	0.003	0.008	-0.000	0.000
Fraction with kids	-0.000	0.008	-0.000	0.000
		Unemployment Characteristics		
Nb emp. contract	0.003	0.084	-0.000	0.000
Frac. Partial Unemp.	-0.005	0.007	-0.000	0.000
Frac. temp ctrct	-0.004	0.021	0.000	0.000
Mean emp. length	0.769	17.210	-0.004	0.081
Past avg wage	0.632	0.685	-0.001	0.003
Past avg. wage FT-adj	0.389	0.572	-0.000	0.003
Fraction PT	-0.008	0.009	0.000	0.000

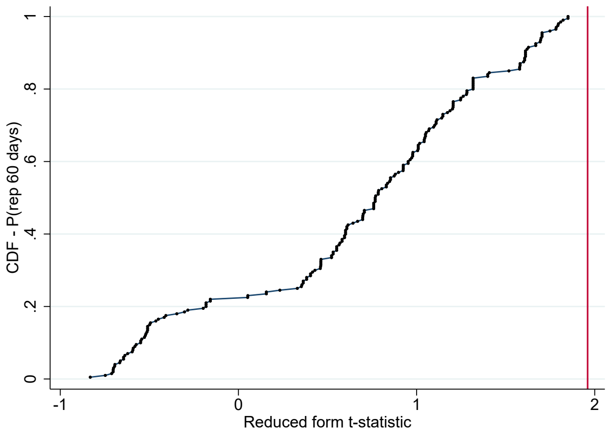
Notes: PT means Part-Time, FT Full-Time. Past avg. wage FT-adj corresponds to a full-time equivalent of the wage for individuals that used to work part-time. This distinction allows to disentangle between a possible change in composition of the pool of unemployed between former part time and full time workers, and a change in the wage levels for a fixed composition of the unemployed population.

2.6.2 Permutation test - Ganong and Jäger [2018]

The key idea with the permutation test is to ensure that the empirical strategy indeed captures the consequences of the exogenous variation in UI rules coming from the reform rather than some other variation, for example due to seasonality. In the context of this article, the permutation test amounts to *(i)* simulate pseudo reforms at different points in time where no reform happened, *(ii)* to then run the same empirical specification and analyse whether the point estimates are significant or not. If these estimates are significant, this would cast doubt on a causal interpretation of the RKD results.

To apply this permutation test, I simulate 200 reform dates. In order to ensure that my point estimates are not driven by seasonality, these 200 pseudo reform dates are randomly selected in an interval of 150 days centred around the 1st of October 2012 and the 1st of October 2016. In line with Card et al. [2015b], these estimates are graphically summarised by representing the CDF of their tstats. This simplifies the analysis, and allows to observe directly whether a significant fraction of these 200 point estimates turns out to be significant. This test will be repeated for the different horizons I consider in Table 2.2. Figure 2.2 below provides such graphical representation for the point estimates of the probability to repeat unemployment within 60 days, with no controls:

Figure 2.2: Permutation test - t stats - $\mathbb{P}$(rep unemp within 60 days)



Notes: This figure contains a permutation test *a la* Ganong and Jäger [2018]. I randomly draw 200 placebo dates in a bandwidth of 150 days around the 1st of October 2012 and 2016. This figure contains the CDF of the t-statistics of the RKD estimates based on these 200 placebo reform dates. The red vertical line is located at the 5% significance level, 1.96.

Figure 2.2 shows that among the 200 estimates randomly drawn around the 1st of October 2012 and the 1st of October 2016, none were significant. This clearly rules out the possibility that the main estimates in this article are driven by some random variation, or seasonality.

The different validity checks presented in this section uniformly lead to confirm that the key identifying assumptions of a RKD hold. The McCrary test indeed leads to reject the possibility of a manipulation of the running variable, and the numerous covariates taken into account are all smoothly distributed around the kink. Additionally, the permutation test confirms that the 2014 reform is the exogenous source of variation driving the results. The estimates presented in this article can therefore legitimately receive a causal interpretation.

2.7 Cost Estimate

This section offers to estimate what is, to the best of my knowledge, the first complete evaluation of the cost of the top-up of entitlement mechanism. It indeed provides measures for both the mechanical and behavioural costs of the reform. It first presents a direct cost estimate from an accounting perspective by measuring for how long individuals stay unemployed after a first top-up of their entitlement and how much UB they receive on average. A french public institute called UNEDIC (*Union Nationale interprofessionnelle pour l'Emploi Dans l'Industrie et le Commerce*) already provided such accounting perspective, though for a different sample and time period - which can provide a source of comparison. It then uses the RKD estimates to measure the cost generated by the behavioural response to this reform. Remember that after topping-up their entitlement, unemployed also become more likely to repeat the unemployment experience. This leads to an additional cost through two channels, a direct and an indirect one, that are analysed in subsection 2.7.2.

2.7.1 Mechanical cost of a top-up

In a document published in October 2019[14] the UNEDIC analysed the cost arising from the possibility to top-up one's entitlement. The UNEDIC estimated that, for individuals topping up their entitlement, the average potential benefit duration in 2017 is of 10.3 months with an average daily UB level of about 35€[15]. These number do not say how much these unemployed indeed received, but instead how much they could have received had they consumed the entirety of their entitlement.

My sample differs from the one used by UNEDIC along various dimensions. First of all, I mostly focus on individuals who registered as unemployed prior the reform. Secondly, I only take into account individuals registered under the general regime, and who did not register under any special regime beforehand. I also only take into account the first time a given individual uses the top-up mechanism, which reduces the possibility

[14]See *Les droits rechargeables*, UNEDIC

[15]Among these individuals topping up, 37% have a PBD of less than 6 months, 27% have PBD between 6 months and a year.

of strategic behaviour arising from repeated interactions with the Public Employment Agency under the new rules. Lastly, I restrict the sample to individuals younger than 50 years old (for further details regarding the sample, see subsection 2.3.2.).

Within my sample, the average individual using the top-up of entitlement mechanism for the first time is a woman of about 30 years old, that has an education below the *Baccalaureat* and that worked under a temporary contract prior unemployment. This person will, after topping-up her entitlement, stay unemployed for an average of 205 days, and receive a total amount of UB representing 3,937€. Table 2.C.1 in the Appendix provides further details. It contains information about the values for different variables of interest, between prior, during, and after the top-up of one's entitlement. This table highlights the fact that individuals topping-up their entitlement then stay unemployed for a relatively shorter duration (221 days versus 549). These lengths may seem a lot longer than the average duration of an unemployment spell. It is indeed the case, but remember that a top-up first requires to consume all of the existing entitlement to UI. After such top-up, unemployed receive a total amount of UB about three times lower compared to the spell that lead to it. Based on this table, the average mechanical cost coming from the top-up of a given entitlement in my sample is given by :

$$\overline{\text{Mechanical Cost Top up}} \approx 3,937\text{€} \tag{2.6}$$

The next subsection identifies the additional cost generated by the behavioural response to this reform.

2.7.2 Behavioural cost of a top-up

The behavioural cost generated by this reform will occur through two distinct channels. A first channel comes from the fact that, by increasing the probability to repeat unemployment, further individuals will become unemployed again. This generates an immediate cost which depends on the total value of the UB they will receive.

A second channel comes from the fact that, by being unemployed again, individuals will not work and therefore not pay taxes contributing to the financing of the UI system. Over the period I analyse, the contribution rate to UI stayed constant, at 6.40%, with

4% being paid by firms and the remaining 2.40% being paid by the employee. In what follows and to simplify, I will only consider the contributions paid by the employees, therefore under-estimating the reduction in resources arising from the larger probability to become unemployed again after using the top-up of entitlement mechanism.

The following formula summarises these two channels:

$$\overline{\textbf{Behavioural Cost per Topup}} = \hat{\tau}^D_{RKD} * [\underbrace{\overline{UB}}_{Direct\ Cost} + \underbrace{\overline{T}}_{Indirect\ Cost}] \tag{2.7}$$

Where all the costs are measured based on the spell (eventually) happening after the top-up of one's entitlement. This behavioural cost is measured by using the RKD estimates. The direct cost is relatively straightforward and comes from the product between the increase in the probability to repeat unemployment at horizon D ($\hat{\tau}^D_{RKD}$) and the average cost per individual of an unemployment spell happening after a top-up. The indirect cost is slightly more complicated due to the possibility of partial unemployment. It takes into account the average length of the unemployment spell happening after a top-up ($\overline{UL}$), but also the fact that only a fraction α of the unemployed engages in partial unemployment for an average duration of $\overline{PU}$ days. This indirect cost therefore takes the following form:

$$\overline{T} = \tau_{tax}\overline{w} * [\overline{UL} - \alpha * \overline{PU}]$$

All the variables in this equation are measured during the spell happening after a top-up. Table 2.C.2 in the Appendix provides the full details about the estimated cost, for different horizons and different set of controls. The last column of this table expresses the behavioural cost as a percentage of the mechanical one measured in subsection 2.7.1. This table leads to the conclusion that the behavioural cost generated by the reform is between 500 and 700€. This brings the total cost of the reform to about 4500-4700€ per top-up.

2.8 Conclusion

By exploiting the specific characteristics of a reform introduced in France on the 1st of October 2014, this paper studies a highly policy relevant yet mostly unexplored question. It analyses if, instead of improving job stability, UI could actually foster job instability. More precisely it analyses whether a given eligibility condition to UI, namely the number of days necessary to open a new entitlement, can affect individuals' incentives to accept short temporary contracts. I show by using a RKD strategy based on the timing of the reform that after experiencing their first entitlement top-up, individuals are willing to work for lower hourly wages and also become more likely to repeat unemployment within a short period of time. This indicates that after a top-up these individuals become more likely to accept temporary contracts with short durations. Once these contracts end and are not renewed, individuals register as unemployed again.

The drop in their hourly wage is in between 0.3 to 0.5€, which represents a reduction of between 3-4%. By revealed preference, such decrease tells us something about how much individuals value UI. Interestingly, such drop only happens after individuals experienced their first top-up, and not before. This suggests the presence of informational issues, as once individuals experience and understand what a top-up is, they re-evaluate downward their reservation wage. It also minimises concerns regarding a potential strategic behaviour from the unemployed.

A heterogeneity analysis reveals that the effects of this reform are not concentrated among individuals with the shortest entitlement to UI, and therefore presumably on individuals with the smallest attachment to the labour market. They are nevertheless a lot stronger on experienced unemployed, which suggests that the reform decreased an already weak attachment to the labour market. While such reform was introduced to enlarge the coverage of UI to short contracts, it may well have fostered their development.

These findings are robust to a large battery of tests, and to the introduction of a very large set of controls. Concerns about seasonality are dealt with by introducing both year and quarter fixed effects, and validity checks are ran on deseasonalised data. Additionaly, placebo reforms simulated at the same time of the year, in 2012 and 2016 show that seasonality does not drive the results.

This article also offers, to the best of my knowledge, the first complete evaluation of the cost of this top-up of entitlement mechanism. It measures both the mechanical and behavioural costs of this later, and concludes that every top-up generates a cost of between 4,500 and 4,700€. Considering that, in September 2018, 750,000 entitlement to UI were open through a top-up, this cost is far from negligible.

Appendices

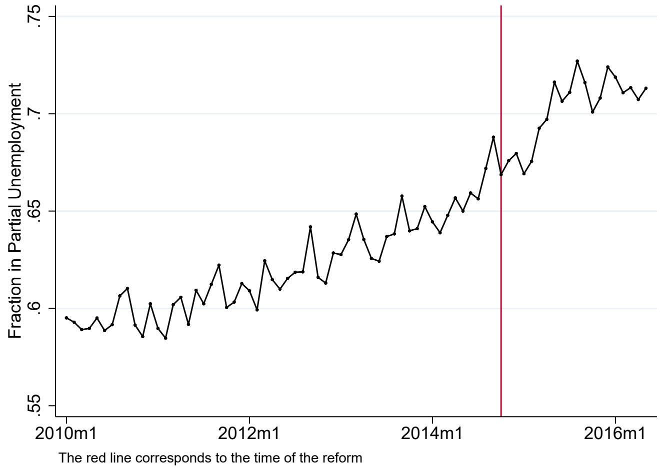
2.A On the effect of the reform on partial unemployment

Before the 2014 reform, partial unemployment was possible under three conditions: *(i)* working for less than 110 hours a month, *(ii)* receiving a wage representing less that 70% of the average wage prior unemployment, and *(iii)* being in a situation of partial unemployment for less than 15 months. All of these conditions were removed by the 2014 reform. Post reform, the only remaining constraint is that the sum of unemployment benefits plus the wage received while unemployed represents less than the wage prior unemployment. If the cumulated earnings and UB while unemployed exceed the past wage, unemployed cannot keep receiving UB. This nevertheless did not affect unemployed's behaviour, neither along the intensive margin (number of hours worked on average) nor in terms of their average wage under partial unemployment.

Remember that before the reform, unemployed could not receive a salary representing more that 70% of their wage prior unemployment. As underlined by Gonthier and Le Barbanchon (2016), one should therefore expect a missing mass in individuals' distribution for wages slightly above 70% of the past wage, this region being strictly dominated. They nevertheless show, by using a bunching strategy, that instead of having an additional mass prior the 70% threshold and a missing mass right after, the distribution is smooth across this cutoff value. Since this 70% threshold got removed by the reform, along with the two other aforementioned conditions, one may have a few concerns.

First, individuals that preferred not to engage in some partial unemployment prior the reform could re-evaluate such option and start working. The figure below alleviates this concern. As can be seen, the fraction of unemployed engaging some form of partial unemployment evolved smoothly at the time of the reform.

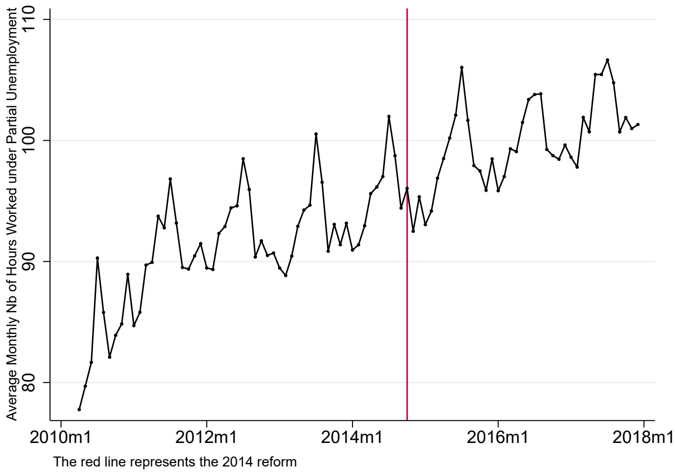
Figure 2.A.1: Fraction of Unemployed in Partial Unemployment



Notes: This figure represents the evolution of the fraction of unemployed engaged in partial unemployment, *i.e.* working part time while staying unemployed.

Secondly, one may be concerned that individuals could respond along the intensive margin. Since both the 70% threshold and the limit in terms of number of hours worked were removed by the 2014 reform, one has to take into account such margin as well. Figure 2.A.2 below represents the average number of hours worked under partial unemployment over time. Once again, no significant change can be observed at the time of the reform.

Figure 2.A.2: Average Number of Hours Worked under Partial Unemployment



Notes: This figure represents the evolution of the average number of hours worked per month by unemployed engaged in partial unemployment.

Finally, due to the change of the 70% threshold, one can analyse the evolution of the distribution of the ratio between wages under partial unemployment and prior unemployment. This allows to capture simultaneously two margins, one in terms of number of hours worked, and one in terms of wage per hour. If unemployed really took advantage of the reform, then one should expect the distribution of the ratio of the wage under partial unemployment and past wage to shift from below 70% to above this level. The figure below rules out such concern.

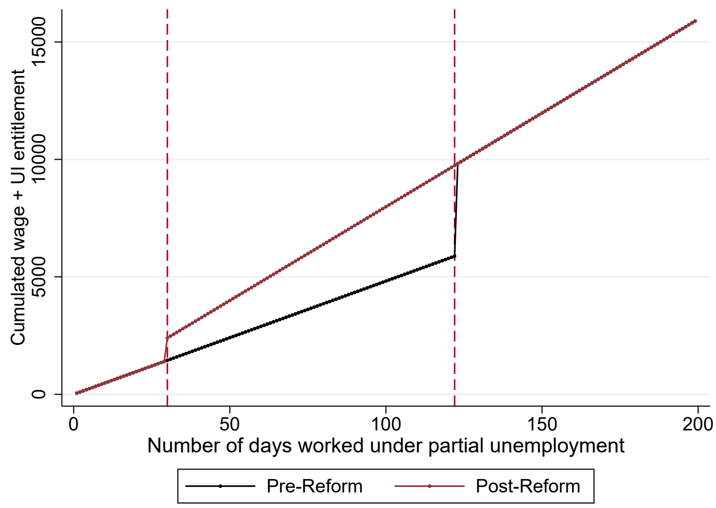
Figure 2.A.3: Evolution of the Wage as Fraction of Past Wage under Partial Unemployment (PU)

Notes: This figure represents distribution of the ratio between the wage under partial unemployment and the wage prior unemployment, both in 2014 and in 2015.

2.B Additional figures

Figure 2.B.1 below illustrates the influence the reform had on the value of contracts with a length between 30 and 122 days. While prior the reform, such contracts could not open any entitlement to UI, they do after the reform on a one day worked one day of entitlement to UI basis. This means that a contract of say, 30 days, opens entitlement to UI for 30 days after the reform. This type of contract therefore saw its value increase through the insurance it now provides.

Figure 2.B.1: Effect of the reform on the value of short contracts



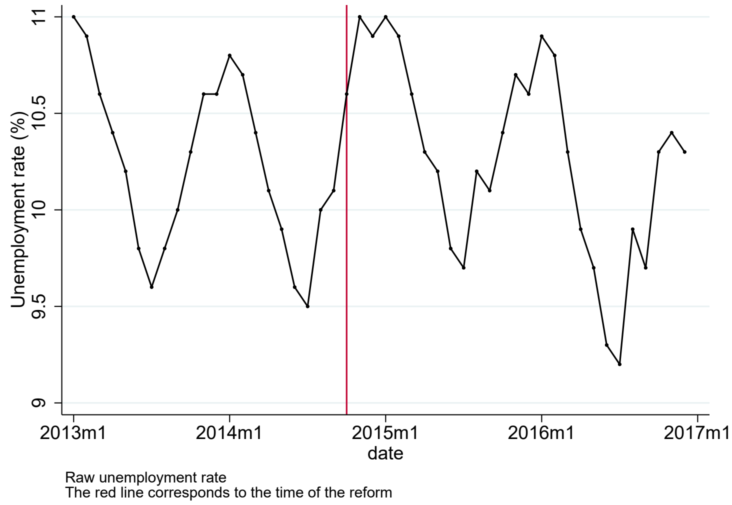
Note: This Figure considers the case of an individual that would accept an employment contract under partial unemployment paid at the minimum wage in 2014 (1445.38€ for a full-time job). The length of such contract is represented on the x-axis, while its total value is on the y-axis. This total value corresponds to the cumulated value of the wage plus the potential value of the corresponding entitlement to UI, absent any discounting/preference for the present. It assumes that every day worked is worked full-time. The two vertical red-dashed lines are located at 30 and 122 days.

The increasing wedge between the red and the black line correspond to the additional value the reform provided to short temporary contracts through the potential entitlement to UI they can now open. This figures ignores the fact that by working for X days under partial unemployment, the individual also postpones the end of her eligibility by X days, which can be valuable in itself.

Figure 2.B.2 and illustrates the seasonality in the unemployment rate in France between

2013 and 2017. The vertical red line represents the time of the reform. A McCrary test applied around this area would lead to reject the null of no manipulation of the running variable, even for a placebo reform in October 2012, simply due to seasonality. I will therefore deseasonalise before running the McCrary test.

Figure 2.B.2: Seasonality in the unemployment rate



Note: This figure represents the evolution of the unemployment rate in France over time.

2.C Cost estimate - Details about the core sample

The table below provides further information regarding the average characteristics of the sample made of individuals topping-up their entitlement for the first time.

Note that in my data, the *education* variable is categorical, and coded as follows:

- 1: Aucune formation scholaire (no diploma)
- 2: Classe de 6eme, 5eme, 4eme (Stopped before year 10)
- 3: College (year 10)
- 4: Certificat d'etude primaire (year 11)
- 5: CAP/BEP (equivalent to year 12-13)
- 6: Baccalaureat (Secondary school)
- 7: Bac+2 (Secondary school + 2 years)

- 8: Bac +3 et Bac +4 (Bachelor and bachelor +1 year)
- 9: Bac +5 et plus (Masters and more)

Table 2.C.1: Cost estimate - top-up of entitlement

Variables	Pre Top-up	Top-up	Post Top-up
	Demographic characteristics		
Education	5.656	5.645	5.567
	(1.923)	(1.916)	(1.904)
Fraction women	0.565	0.562	0.596
	(0.496)	(0.496)	(0.491)
Age	29.769	29.868	29.395
	(8.729)	(8.144)	(7.616)
Fraction married	0.347	0.335	0.315
	(0.474)	(0.470)	(0.461)
Fraction with kids	0.364	0.326	0.319
	(0.481)	(0.469)	(0.466)
	Unemployment characteristics		
Frac engaged in partial unemployment	0.992	0.664	0.715
	(0.089)	(0.472)	(0.451)
Nb days partial unemployment	131.389	105.972	94.691
	(108.511)	(117.501)	(99.904)
Hourly wage partial unemployment	12.02	11.87	11.89
	(2,335)	(2.438)	(2,406)
Nb days received UB	370.552	127.509	115.594
	(214.608)	(100.243)	(74.685)
Eligibility (days)	371.216	175.530	151.541
	(214.347)	(138.618)	(99.092)
Daily UB	31.866	31.464	31.631
	(10.957)	(9.917)	(9.139)
Replacement Rate	0.624	0.623	0.623
	(0.047)	(0.043)	(0.040)
Fraction Minimal Condition	-	0.452	0.346
	-	(0.498)	(0.476)
Fraction Renewing Entitlement	1	0.218	0.246
	(0)	(0.413)	(0.431)
Total amount UB paid (€)	12,250.09	3,937.50	3,603.75
	(9682.009)	(3453.475)	(2540.314)
Unemployment length (days)	549.194	221.342	205.426
	(230.168)	(171.894)	(135.233)
	Prior Employment characteristics		
Average past wage	52.046	51.312	51.494
	(20.384)	(18.411)	(17.083)
FT coefficient	0.891	0.884	0.894
	(0.178)	(0.190)	(0.175)
Average past wage, PT adjusted	58.91	58.78	58.60
	(23.336)	(20.568)	(22.070)
Fraction worked PT	0.411	0.435	0.428
	(0.492)	(0.496)	(0.495)
Fraction worked on temporary contract	0.645	0.773	0.814
	(0.479)	(0.419)	(0.389)
Number of employment contracts	4.686	4.776	4.305
	(5.618)	(5.192)	(4.822)
Average length of employment contracts (days)	274	76	78
	(301.485)	(92.007)	(80.163)
Observations	18,588	9,590	1,415

Notes: This table provides detailed information about individuals that will use the top-up mechanism and repeat unemployment after using it. It decomposes such information in between prior the top-up, during the spell that happened through the use of this mechanism and after using it. FT refers to Full-Time.

Table 2.C.2: Behavioural Cost Estimates

Horizon in days (D)	τ^{D}_{RKD}	Direct Cost	Indirect Cost	Tot. Behavioural Cost	% Mechanical Cost
			No controls		
30	0.182***	3,279.38	48.211	605.62	15.38
60	0.169***	3,279.38	48.211	562.36	14.28
90	0.179***	3,279.38	48.211	595.63	15.13
120	0.239***	3,279.38	48.211	795.29	20.20
180	0.287***	3,279.38	48.211	955.02	24.26
250	0.349***	3,279.38	48.211	1,161.33	29.50
365	0.354***	3,279.38	48.211	1,177.96	29.92
			Eligibility control		
30	0.191***	3,279.38	48.211	635.57	16.10
60	0.180***	3,279.38	48.211	598.96	15.21
90	0.189***	3,279.38	48.211	628.91	15.97
120	0.214***	3,279.38	48.211	712.10	18.09
180	0.256***	3,279.38	48.211	851.86	21.64
250	0.325***	3,279.38	48.211	1,081.46	27.47
365	0.344***	3,279.38	48.211	1,144.69	29.08
			Eligibility + Time FE		
30	0.171***	3,279.38	48.211	569.01	14.45
60	0.160***	3,279.38	48.211	532.41	13.52
90	0.168***	3,279.38	48.211	559.03	14.20
120	0.187***	3,279.38	48.211	622.26	15.81
180	0.200***	3,279.38	48.211	665.51	16.90
250	0.210***	3,279.38	48.211	698.79	17.75
365	0.205***	3,279.38	48.211	682.15	17.33
			Full controls		
30	0.161***	3,279.38	48.211	535.74	13.61
60	0.152***	3,279.38	48.211	505.79	12.85
90	0.161***	3,279.38	48.211	535.74	13.61
120	0.189***	3,279.38	48.211	628.91	15.97
180	0.188***	3,279.38	48.211	625.58	15.89
250	0.181**	3,279.38	48.211	602.29	15.30
365	0.168**	3,279.38	48.211	559.03	14.20

Notes: This table contains the different components necessary to measure the behavioural cost of the reform. These components are decomposed by time horizon, from 30 to 365 days, and as a function of the controls taken into account in the RKD estimates, from an absence of control to the introduction of the full set of controls. The full controls include variables for the effect of age, gender, education, marital status, presence or not of kids, a departement fixed effect, a dummy indicating whether the individual was working part time prior unemployment, one indicating whether the contract was a temporary contract, the average wage, the number of contract used to open eligibility to UI and their average length. All these variables are based on a sample of individuals repeating unemployment at least three times, one of the spells happening after the use of the top-up mechanism.
*,** and *** denote the significance at the 10%, 5% and 1% respectively.

Chapter 3

There is Only One First Time: Behavioural Responses and Unemployment Experience

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Abstract

This paper analyses the impact of unemployment experience on the behavioural response to unemployment benefits (UB). It shows, in the Spanish context, that the moral hazard cost generated by unemployment insurance is significantly positive only for the individuals in their first unemployment spell, the *inexperienced unemployed.* The elasticity of unemployment duration with respect to unemployment benefits is consistently negligible and non significant for individuals that already experienced at least one unemployment spell, the *experienced unemployed.* I show that introducing such feature in a Baily-Chetty type of framework could, under reasonable assumptions, legitimate heterogeneous unemployment benefits depending on the unemployment experience, even more so if experienced unemployed are poorer and more risk averse.

The empirical analysis relies on Spanish administrative data, the MCVL, that provide exhaustive details about individuals' labour market experience since their very first job. Using a regression kink design, I show that only first time unemployed significantly respond to a kink in the Spanish unemployment insurance system. This finding is robust to the introduction of a large set of controls, multiple specifications, and resists to bandwidth selection and permutation tests.

Keywords: Unemployment insurance; Job Stability; Regression Discontinuity Design; Sufficient Statistics.

J.E.L. codes: H20; J64; J65.

3.1 Introduction

In line with a growing number of countries like Sweden and Belgium, while facing a significant increase of its unemployment rate, Spain decided to reform its Unemployment Insurance (UI) system. This country indeed experienced a large rise of its unemployment rate, from 8.5% in 2006 to almost 25% in 2012 (OECD). The main idea underlying those reforms is often to decrease the cost of UI. By receiving relatively smaller unemployment benefits (UB), unemployed should reinforce their search effort, which in turn should reduce the average unemployment duration. These implications rely on the idea that unemployed individuals respond to exogenous shocks on the level of benefits they receive.

This paper analyses the impact of unemployment history on the moral hazard (MH) cost of UI, one of the key parameters of the so called Baily-Chetty formula (Baily [1978], Chetty [2006]). While unemployment experience could lead individuals to learn about the UI parameters and become more strategic, it could also leave a scar (Pavoni [2009]) limiting unemployed's response to UB. Individuals' unemployment history is therefore likely to affect the optimal design of UI. If the consumption smoothing (CS) benefits of UI increase with the individuals' number of unemployment spells, whereas the MH cost decreases with such number, a Baily-Chetty type of formula would be in favour of an increasing profile of UB with unemployment experience. By introducing heterogeneity in a Baily-Chetty framework, I show that such increasing profile could well, under reasonable assumptions, be optimal, even more so if agents in their second, third or even fourth unemployment spell - the *experienced unemployed* - are poorer and more risk averse.

I am not aware of any paper analysing the MH cost of UI depending on individuals' unemployment history. The main reason is certainly that such analysis is highly demanding in terms of data. It indeed requires to know the complete labour market history of every individual since their very first job. One needs to be able to identify whether a given unemployed agent is in her first, second or n^{th} unemployment spell. The identification of the MH cost also requires an exogenous source of variation that would affect every unemployed individual similarly and independently of their unemployment history.

The analysis in this paper relies on the *Continuous Sample of Working Life* (MCVL, Muestra Continua de las Vidas Laborales), a Spanish administrative dataset that fulfils both of these conditions. For a 4% non-stratified sample of the Spanish population, it provides details about the full labour market experience of every individual, since their very first job until 2016 if they did not retire yet. The institutional context provides the exogenous source of variation required for the analysis. The UB level is indeed capped at 70% of the past wage, up to a maximum level. This creates a kink that I exploit using a Regression Kink Design (RKD), in line with Card et al. [2012], Landais [2015] and Lalive [2007]. This allows to precisely measure the elasticity of unemployment duration with respect to the UB level. Differentiating such RKD depending on individuals' unemployment experience yields the key result of this paper. The MH cost of UI is significantly positive only for the first time unemployed. The extent of unemployment experience, *i.e.* whether individuals are in their second, third or even fourth unemployment spell does not affect such result.

The optimal level of UB is a research question that has already been deeply explored by the literature. The (static) Baily-Chetty formula identifies two key parameters determining such optimal level. The first one corresponds to the CS benefits of UI. By transferring some resources from a good (employment) to a bad (unemployment) state, UI allows to smooth consumption in case of an adverse event, namely the occurrence of an unemployment spell. This CS is even more valuable the larger the curvature of the utility function. The second parameter is the one that the aforementioned reforms try to influence. It corresponds to the elasticity of unemployment duration with respect to UB and is often referred to as the MH cost of UI. If such elasticity is high, then reducing UB could substantially reduce the unemployment length.

Such simple and intuitive formula, however, due to its consumption smoothing component, is not easy to take to the data, a problem that holds as well with the reservation wage approach proposed by Shimer and Werning [2007]. Based on a sufficient statistics approach, Chetty [2008] offers a formulation that allows to analyse UI only using data about unemployment durations. He disentangles two effects, a liquidity and a moral hazard effect, and underlines that by neglecting the former, any analysis would necessarily conclude that UB are too high with respect to their optimal level, and hence that

UI strictly reduces welfare. One therefore needs to be particularly careful concerning the modelling assumptions used to analyse the optimal UB level.

In addition to the optimal level, the literature also addressed the question of the optimal time profile of UB. Shavell and Weiss [1979], and Hopenhayn and Nicolini [1997], among others, underline that UB should be decreasing over time. Intuitively, as UB decrease, individuals should put more and more effort to find a job. These analyses however do not take into account certain elements such as human capital depreciation over the unemployment spell, or the negative signal long or repeated unemployment episodes could send to employers. This first element was for instance modelled by Pavoni [2009], but does not change the optimality of a decreasing profile. Interestingly, Pavoni [2009] shows that, if rapid enough, human capital depreciation can lead to the emergence of a *minimal assistance*, *i.e.* that UB can be optimally bounded below by a minimal level. Lehr [2017] introduced negative duration dependence in hiring rates into a Baily-Chetty type of formula. By considering the cases of employer screening and human capital depreciation, he shows that the effect of UI on hiring rates completely depends on assumptions about firms' behaviour.
Hopenhayn and Nicolini [2009] is the only contribution I am aware of that focuses on the influence of unemployment history. They underline that, if quits cannot be distinguished from layoffs, then coverage should be an increasing function of the previous employment spell's length. Their model can however hardly be taken to the data.

In a recent and thorough analysis, Kolsrud et al. [2018] combined these two strands of the literature, namely the optimal level and path of UB over time, into a single insightful framework. They show that the intuition of the basic Baily-Chetty formula can be generalised to the case of dynamic unemployment policies. The optimal level of UB is such that the associated insurance value balances the implied moral hazard cost at every period t. Using an extremely comprehensive set of Swedish data, they then evaluate this dynamic Baily-Chetty formula empirically. They can indeed, by using both survey and tax register data on income and wealth, measure individuals' consumption over time, and therefore cover precisely every aspect of this formula.

In terms of method, this paper relies on the same Regression Kink Design (RKD) as in Card et al. [2012], Card et al. [2015a] and Landais [2015], and Kolsrud et al. [2018]. Despite the various technical challenges it implies (Gelman and Imbens [2019], Imbens and Kalyanaraman [2012], Imbens and Lemieux [2008]), this method recently became increasingly popular. Our empirical analysis exploits the MCVL data that allow to match longitudinal details from social security, income tax and census records. These data have already been extensively used to analyse unemployment-related questions (Arranz et al. [2013], Bentolila et al. [2017], Arranz and Garcia-Serrano [2013]). However, to the best of my knowledge, this paper is the first to fully exploit one of its key advantages: it allows to know the complete labour market history of the individuals since their very first job. I can hence identify, with a daily precision, the length of every single spell of employment, unemployment and non-employment, for every individual in the sample. Individuals can therefore be distinguished depending on their unemployment experience, and I can test whether their behavioural response to an exogenous shock on the UB level is the same, conditional on such experience.

By analysing the whole set of unemployed, I show that an analyst would conclude that the MH cost of UI is negligible. Such approach would however mask significant heterogeneity. Decomposing the pool of unemployed depending on individuals' unemployment history yields interesting results. Only the individuals in their first unemployment spell - the *inexperienced unemployed* - significantly respond to the kink in the UB level. This finding is robust to the introduction of a broad set of controls and resists to a large set of bandwidth selection and permutation tests.

The remaining of this paper is organised as follows. Section 3.2 presents a basic Baily-Chetty framework. It briefly reviews the case of homogeneous agents, to then incorporate and analyse the effect of heterogeneity in terms of preferences and unemployment history on the optimal UB level. Section 3.3 presents the data and the institutional context. It provides precise details about the Spanish UI system. Section 3.4 describes the regression framework and presents the empirical analysis. It also incorporates extensive validity checks. Finally, section 3.5 concludes.

3.2 Conceptual Framework

The analysis in this paper relies on a key question: depending on their unemployment experience, do all unemployed react the same way to changes in the level of UB? If the answer to such question is no, differentiating unemployment benefits as a function of individuals' unemployment history may be welfare improving.

Answering such question requires to take into account heterogeneity among the pool of unemployed individuals. In what follows, I will start by presenting a basic Baily-Chetty framework with homogeneous agents. I will then introduce two dimensions of heterogeneity, in terms of preferences and unemployment experience. Under some reasonable conditions, I show that these sources of heterogeneity can push in favour of an inclining profile of UB with unemployment experience. By unemployment experience, I refer to the fact that among the population of unemployed, some may be experiencing their first unemployment spell, and hence be inexperienced, while some others may be experienced, *i.e.* be in their second, third ... unemployment spell. More precisely this section shows that with heterogeneous agents, differentiating UB depending on individuals' unemployment experience, an observable characteristic, may well be welfare enhancing.

3.2.1 Baily-Chetty with homogeneous agents

This subsection builds on the intuition introduced by Baily [1978] and extended by Chetty [2006]. I will use the two period framework presented in Chetty [2008]. Consider a pool of unemployed individuals in period 0. In a first time, assume that all the unemployed have the same characteristics. In t=0, they determine the optimal level of search effort, e. The higher this effort, the higher the probability to find a job, $\pi(e)$, but the higher the corresponding utility cost of effort, $\psi(e)$. The $\psi(.)$ function is assumed to be increasing and convex. In period t=1, a fraction $1 - \pi(e)$ will still be unemployed while $\pi(e)$ individuals will be employed. The former will receive UB b, while the latter will receive a wage w and pay a tax τ. The consumption of employed agents (e) is therefore $c_e = A + w - \tau$, while the one of unemployed (u) is $c_u = A + b$, where A represents a given level of assets.

The social planner solves the following optimisation program:

$$\begin{cases} \underset{b}{max} & W(b) = \pi(e(b))u(A+w-\tau(b)) + (1-\pi(e(b)))u(A+b) - \psi(e(b)) \\ s.t \quad \tau(b) * \pi(e(b)) = & (1-\pi(e(b)))b \end{cases} \tag{3.1}$$

Using the envelop theorem, and the following money-metric $\frac{d\tilde{W}}{db} = (\frac{dW}{db})/\pi u'(c_e)$, we obtain:

$$\frac{d\tilde{W}}{db} = \underbrace{\frac{1-\pi}{\pi}}_{\text{Unemp. rate}} \Big[\underbrace{\frac{u'(c_u) - u'(c_e)}{u'(c_e)}}_{\text{CS}} - \underbrace{\frac{\varepsilon_{1-\pi,b}}{\pi}}_{\text{MH}} \Big]$$

Where $\varepsilon_{1-\pi,b}$ corresponds to the elasticity of the probability of remaining unemployed with respect to UB, which in our context can also be interpreted as the elasticity of unemployment duration with respect UB. CS correspond to the Consumption Smoothing effect of UI, and MH to the Moral Hazard cost. The optimal UI has to balance these two elements. This is the well known Baily-Chetty formula that underlines the existence of a tradeoff between the insurance value of UB and the corresponding behavioural cost. By receiving UB, unemployed can smooth the effect of an adverse shock - losing one's job - but it could on the other hand lead them to reduce their search effort.

3.2.2 Baily-Chetty with heterogeneity

This subsection introduces two dimensions of heterogeneity, in terms of unemployment history and preferences. While the former is observable, the latter is not. There is no reason, *a priori*, to consider as homogeneous the preferences, and hence the risk aversion, of individuals with different experiences on the labour market.

In what follows, I will focus on the case of an actuarially fair UI. From an optimal perspective, this choice seems reasonable since in an ideal case, UI should depend on both individuals' characteristics and job finding rates. This allows to rule out the implicit across agents transfer implied by a uniform financing of UI.

I will denote by $\mathcal{U}$ the pool of unemployed. Consider that such pool is composed of two groups, in proportion α and $1-\alpha$[1]. The first group corresponds to the first time/inex-

[1] The size of the unemployed population is implicitely normalized to 1.

perienced unemployed, that I will index by f, whereas the second one corresponds to the individuals that repeat the unemployment experience (r), *i.e.* to all the agents that experienced at least one unemployment spell in the past - the experienced unemployed. For a given individual $i \in \mathcal{U}$, independently of the group she belongs to, the welfare change coming from a one euro increase in b would be given by:

$$\frac{dW^i}{db} = (1-\pi_i)u_i^{'}(c_u^i) - \pi_i u_i^{'}(c_e^i)\frac{d\tau^i}{db}$$

The aggregation issue The challenge is now to identify such marginal welfare change for the whole population of unemployed individuals, and not just for individual i. I cannot simply sum over the previous expression. Indeed, Von-Neumann Morgenstern utilities are uniquely defined only up to an affine transformation. Andrews and Miller [2013] analyse three different strategies to proceed to such aggregation. Whether one normalises individual utilities to '*rule out problematic [affine] transformation*', selects welfare weights leading to invariant measure of aggregate welfare, or directly relies on a money metric, results are similar.

In what follows, I will rely on their first strategy, and normalise individual utilies. Morevoer, I will consider the case of a utilitarian welfare metric, defined as $\hat{W} = \mathbb{E}(\frac{\tilde{W}^i}{\bar{\pi}})$, which I express per employed agents. In general, these assumptions lead to the following result:

$$\frac{d\hat{W}}{db} = (\frac{1-\bar{\pi}}{\bar{\pi}})\Big[\mathbb{E}^u\big[\frac{u_i'(c_i^u) - u_i'(c_i^e)}{u_i'(c_i^e)}\big] - \mathbb{E}^e\big[\frac{\varepsilon_{1-\pi,b}}{\bar{\pi}}\big]\Big] \tag{3.2}$$

Where I used the fact that $\frac{d\tau_i}{db} = \frac{1-\pi_i}{\pi_i}(1 + \frac{\varepsilon^i_{1-\pi,b}}{\pi})$.

Using the two aforementioned groups, and allowing for heterogeneity in terms of preferences, formula 3.2 gives:

$$\begin{aligned}\frac{d\hat{W}}{db} = &\quad \alpha\frac{d\hat{W}^f}{db} + (1-\alpha)\frac{d\hat{W}^r}{db} && (3.3)\\ \approx &\quad \mu_f\Big\{\bar{\gamma}^{u,f}\bar{\Delta}^{u,f} + cov^{u,f}(\gamma_i,\Delta_i) - \mathbb{E}^{u,f}\big[\frac{\varepsilon^{i,f}_{1-\pi_f,b}}{\pi_{i,f}}\big]\Big\} + \\ &\quad \mu_r\Big\{\bar{\gamma}^{u,r}\bar{\Delta}^{u,r} + cov^{u,r}(\gamma_i,\Delta_i) - \mathbb{E}^{u,r}\big[\frac{\varepsilon^{i,r}_{1-\pi_r,b}}{\pi_{i,r}}\big]\Big\} && (3.4)\end{aligned}$$

Proof: For each group, just use a first order Taylor expansion: $u'(c_u) - u'(c_e) \approx u''(c_e)(c_u - c_e)$, denote by $\gamma \equiv -\frac{u''(c_e)}{u'(c_e)} * c_e$ the coefficient of relative risk aversion, and by $\Delta \equiv \frac{c_e - c_u}{c_u}$. The μs represent weighted unemployment rate among each group. More precisely, $\mu_f \equiv \alpha(\frac{1-\bar{\pi}_f}{\bar{\pi}})$, and $\mu_r \equiv (1-\alpha)(\frac{1-\bar{\pi}_r}{\bar{\pi}})$. For further details, see the Appendix and Andrews and Miller [2013]. ■

This formula contains highly interesting insights. First of all, note that each group is weighted by its relative unemployment rate. For each group, the formula contains three components. Two of them are identical to the basic Baily-Chetty formula, but incorporate heterogeneity. The first one, $\bar{\gamma}^{u,\cdot} * \bar{\Delta}^{u,\cdot}$ corresponds to the product between (group-specific) risk aversion and the consumption smoothing gain of UI. Indeed, Δ captures the consumption drop associated with an unemployment shock. The last term represents the MH cost of UI. The second one directly comes from the potential preference heterogeneity. Andrews and Miller [2013] call it the *covariance effect.* Importantly, by ruling out preference heterogeneity and not taking into account heterogeneity in unemployment history, formula 3.4 simplifies to 3.2.

Discussion While formula 3.4 seems quite complex, it corresponds to a straightforward extension of equation 3.2. The two terms $\mathbb{E}^{u,f}(.)$ and $\mathbb{E}^{u,r}(.)$ depend on the joint distribution of individual level elasticities and job finding rates. They are difficult to measure in practice, which is why the literature generally does not focus much on individual level heterogeneity and considers the case of uniform taxes to finance UI.

Since the decomposition proposed in formula 3.4 relies on an observable characteristic, namely the unemployment experience, a social planner could avoid any inter-group transfer by equalising to zero each group-specific component of formula 3.3. Interestingly in such a case, one can directly see that there is no reason to provide the same UI to individuals with different unemployment experience.

Consider a case where risk aversion would be declining in wealth, so that poorer workers would be more risk averse. Moreover, imagine that individuals repeating the unemployment experience have already depleted most of their assets during their first unemployment spell. In line with the literature about the scarring effect of unemployment (Arulampalam et al. [2000], Arulampalam et al. [2001]), consider that these latter are also more likely to be relatively poorer. This would imply that $cov^{u,f} \geq 0$, $cov^{u,r} \geq 0$ and $cov^{u,r} \geq cov^{u,f}$. This would also mean that experienced unemployed are less likely to be able to smooth their consumption when facing a new unemployment spell. In other words, the consumption smoothing gains of UI would be larger for these latter. If in addition, they exhibit a smaller elasticity of unemployment duration with respect to unemployment benefits, all the elements would push in favor of an inclining profile of UB with unemployment experience.

This whole reasoning however relies on the idea that the observable characteristics, unemployment experience, remains exogenous to the parameters of UI system. If UB were to be more generous for experienced unemployed, this could introduce a new margin of moral hazard. By receiving more generous UB after their first unemployment spell, individuals could indeed have an incentive to repeat the unemployment experience again and again.

In the empirical analysis presented in section 3.4, I will remain agnostic about preference heterogeneity and consumption smoothing, and will focus on the MH cost of UI. I will show that UB provided to experienced unemployed generate a much smaller moral hazard cost. One of the condition for an inclining profile of UB with unemployment experience is therefore fulfilled.

3.3 Data and Institutional Background

This section provides details about the dataset used, the Spanish institutional context, and summarises important characteristics of the sample used for the empirical analysis.

3.3.1 The MCVL

The analysis relies on the *Continuous Sample of Working Life* (*Muestra Continua de Vidas Laborales* [MCVL]). This dataset contains details about all the employment, unemployment and non-employment spells for a 4% non stratified random sample of the Spanish population who had any sort of relationship with the social security in any year between 2006 and 2016. This dataset has a key advantage: it allows to trace the complete labour market history of the corresponding individuals since the 1980s[2]. The MCVL is composed of many different datasets that can be linked through a unique individual identifier.

The PERSONAL or PERSANON files contain a large range of individual level characteristics. Each of these files provides details about the education level, gender, date of birth and nationality. By using the individual specific identifier, each individual can be related to the AFILIAD (or AFILIANON or AFILIA) files that contain very detailed information about labour market characteristics. These files provide information about the length of every employment / unemployment / non-employment spell for every individual since the beginning of their experience on the labour market. It allows to know, for every single day, whether the individual was working, if so in which firm, if she was unemployed, and in such a case whether or not she was receiving unemployment benefits, or finally if she was not employed. It also contains information about the contract type, whether the individual was working part time or full time, the occupation, the regime she was contributing to, the sector of activity, and the firm size.

The identification strategy I intend to use is the well known RKD. Its implementation requires extremely precise data about individuals' earnings. However, earnings data from the social security are top and bottom coded. This is nevertheless not the case of the tax data that are available between 2006 and 2016. I explain in the Appendix, section 3.B how earnings data from the tax and social security records can be combined to precisely identify monthly earnings at the individual level, and as a consequence the level of UB. This section also contains further details about sample selection. In addition to earnings, the fiscal data incorporate individual level information such as the nature of the earnings, a firm fiscal identifier, the date of birth, the amount of income tax paid, or whether the agent received disability transfers. Tax data also contain a variable related to individuals' marital status. Unfortunately providing such information

[2]Individual level information are available starting in 1967, but earnings data are only available starting in 1980.

is not mandatory and this latter variable is most of the time missing.

A weakness of the MCVL for the analysis in this paper concerns the absence of any variable providing direct information about the number of children. The CONVIVIENTES files only provide details about how many individuals live in the same place over a given year. If the agent lives with N different individuals, it provides the gender and date of birth of each of them. I chose to rely on the following simple rule. If the age gap between the individual and the people she lives with is positive, and in between 18 and 43 years, I will consider the corresponding individuals as her children. If the age gap is outside this range, I will not consider these individuals as her children. They could be housemates or flatmates, relatives, parents etc.

Table 3.E.1 in the Appendix provides summary statistics about the core sample. The average individual in my sample is a man of 29 years old that used to work for a monthly wage of 1,655€. The sample incorporates about 44% of woman, and 25% of individuals with a tertiary level of education. The average duration of unemployment is always larger than the median one, reflecting the influence of long term unemployment. The average time between unemployment spell is rather long, more than a year and a half. Women seem to repeat relatively less the unemployment experience since the fraction of women decreases with the number of spells. The average education level also decreases with unemployment experience, and so does the average wage, in line with the idea of a wage scarring (Arulampalam et al. [2001]).

3.3.2 Institutional background

Prior the reform implemented on the 15th of July 2012, the Spanish UI system was made of a constant UB level capped at a maximum level. The next subsections provide further details about the eligibility conditions, the level and potential length of UB.

Eligibility, level and duration of UB

To be eligible to UB, an individual needs to have worked, not necessarily continuously, at least 360 days over the last six years before becoming unemployed. Provided she has accumulated such employment experience, she is eligible to UB. In case an individual is not eligible to UB, she can still receive UA provided she fulfils more specific criteria. Those latter are detailed in the following document: UA.

The duration of such eligibility depends on the cumulated labour market experience over the last 6 years. It is summarised in table 3.1 below[3].

Table 3.1: UB duration

In Days		**In Month**	
Contribution period	UB length	Contribution period	UB length
From 360 to 539	120	From 12 to 17	4
From 540 to 719	180	From 18 to 23	6
From 720 to 899	240	From 24 to 29	8
From 900 to 1079	300	From 30 to 35	10
From 1080 to 1259	360	From 36 to 41	12
From 1260 to 1439	420	From 42 to 47	14
From 1440 to 1619	480	From 48 to 53	16
From 1620 to 1799	540	From 54 to 59	18
From 1800 to 1979	600	From 60 to 65	20
From 1980 to 2159	660	From 66 to 71	22
Over 2159	720	72 and above	24

Notes: This table represents the entitlement length as a function of the number of days or months worked over the reference period.

Provided that the individual is eligible to receive UB, the level of benefits can differ depending on various factors.

First, this level will be based on the average daily wage over the last 180 days of employment.

Secondly, maximum and minimum levels will depend on the family structure, and on an index called IPREM (*Indicador Pùblico de Renta de Efectos Mùltiples*) fixed every year by the Spanish government[4]. Further details about this index are provided in the Appendix, section institutional background.

The replacement rate corresponds to 70% of the average wage prior unemployment. UB are computed as follows:

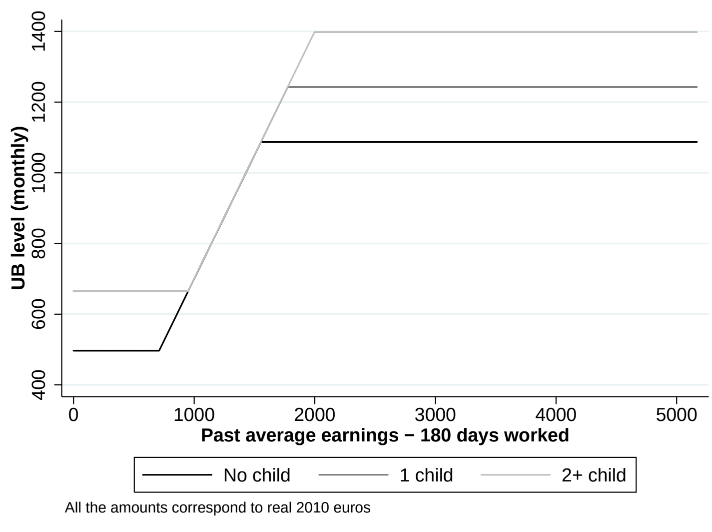
[3] A graphical representation of this duration based on the contribution length is provided in the appendix. See figure (3.C.1), section additional figures.

[4] This index replaced the minimum wage (‘*Salario Minimal Interprofesional*’) in 2004.

$$b_i = \begin{cases} b^F_{min} & if \quad \bar{w_i} * r \leq \quad b^F_{min} \\ b^{F_i} & if \quad b^F_{min} \leq r * \bar{w_i} \leq \quad b^F_{max} \\ b^F_{max} & if \quad r * \bar{w_i} > \quad b^F_{max} \end{cases} \tag{3.5}$$

Where $\bar{w_i}$ represents the average wage earned by individual i over the last 180 days worked, and r corresponds to the replacement rate. F_i refers to the family situation, with $F_i \in \{No\ dependent,\ One\ dependent,\ Two\ or\ more\ dependent\}$ and b^F_{min}, b^F_{max} are the minimum and maximum UB levels that apply depending on the family situation[5]. UB are capped at a maximum level, therefore creating a kink that can be exploited to identify the effect of UB on unemployment length. This kink is represented graphically in figure 3.1 below.

Figure 3.1: UB kink depending on family situation



Notes: This figure represents the evolution of UB as a function of the average wage prior unemployment for three different family types.

[5]According to the spanish UI laws, '*a worker is considered to have dependent children when these children are under 26 years old or older than 26 with a disability that is equal to or greater than 33%, have no income of any kind equal to or higher than the minimum wage excluding the proportional part of two extraordinary payments, and live with the recipient*'.

3.4 Empirical Analysis

As underlined previously, the core question that motivates this article is to know whether unemployment experience affects the elasticity of unemployment duration with respect to the UB level.

To answer to such question, I exploit a kink in the schedule of UB created by the existence of a maximum level *via* a RKD design strategy (Card et al. [2012], Landais [2015], Lalive [2007]).

I will estimate the following key equation:

$$\mathbb{E}[D|_{W=w,\ \mathcal{U}}] = \alpha_0 + [\sum_{p=1}^{\bar{p}} \gamma_p (w-k)^p + \beta_p (w-k)^p * A] \quad where\ |w-k| \leq h \tag{3.6}$$

Where A=$\mathbb{1}[w \geq k]$ is a dummy variable that takes a value of one if the agent is above the kink, w represents the average wage over the last 180 days worked, and k the kink level. $\mathcal{U}$ is a vector referring to all the possible unemployment histories of the individuals. More precisely:

$$\mathcal{U} = \{ \underbrace{(1^{st}\ unemp\ spell)}_{Inexperienced\ unemployed}, \underbrace{(2^{nd}\ unemp\ spell),\ (3^{rd}\ unemp\ spell\ ),\ (4^{th}\ unemp\ spell\ )\}}_{Experienced\ unemployed} \tag{3.7}$$

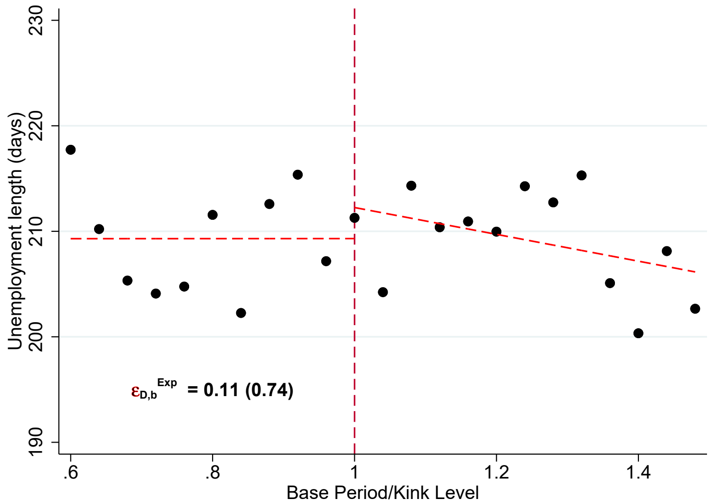
In what follows, I will be interested in the β coefficients, and will split them for different subsets of $\mathcal{U}$. Intuitively, these coefficients indicate whether being above the kink generates a differential change in the relationship between unemployment length and unemployment benefits. The implicit idea is that as UB represent a decreasing fraction of the previous wage, which is the case on the right hand side of the kink, unemployment length should decrease.

3.4.1 Graphical Evidence

I start by providing some graphical evidence and analyse how the whole population of unemployed responds to an exogenous change in the UB schedule.

Figure 3.1 illustrates such relationship and does not exhibit any clear kink. Note that such relationship might nevertheless hide significant heterogeneity.

Figure 3.1: Full Sample

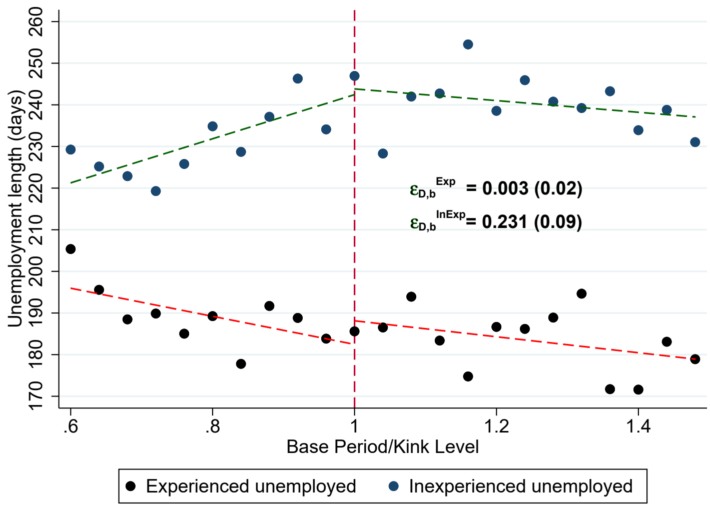


Notes: This figure represents the evolution of unemployment length around the kink for the full sample. The elasticity is obtained by estimating equation 3.6 and through the following transformation: $\varepsilon_{D,b} = \hat{\beta}_1 * \frac{\bar{b}}{\bar{D}}$, where $\bar{b}$ and $\bar{D}$ represent respectively the average level of UB and average unemployment length around the kink.

Figure 3.2 therefore offers to decompose $\mathcal{U}$ into two distinct groups: individuals in their first unemployment spell - the inexperienced unemployed - and individuals that already experienced at least one unemployment spell - the experienced unemployed.

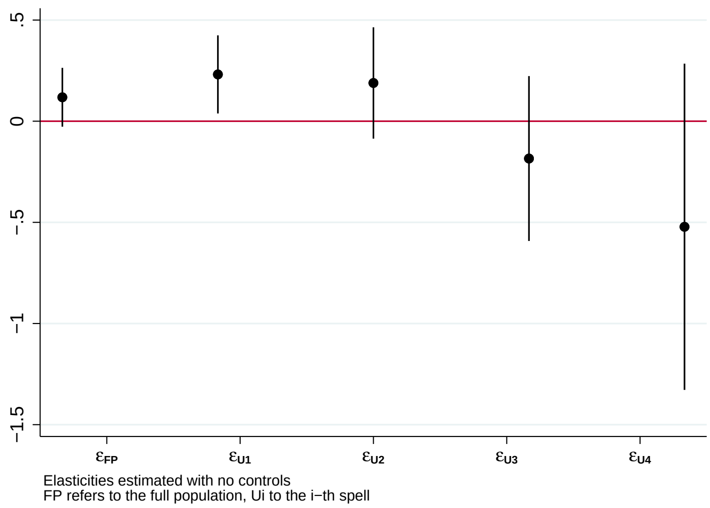
Pooling all unemployed individuals together as in figure 3.1 hence leads to confound highly heterogeneous effects depending on the unemployment history. Figure 3.2 confirms that individuals react highly differently depending on whether they already experienced an unemployment spell in the past or not. While inexperienced unemployed respond to the kink, experienced unemployed do not. Figure 3.3 below provides a spell by spell decomposition of the behavioural response. It confirms that only first time unemployed stay unemployed for a longer duration when receiving more UB.

Figure 3.2: Experienced Vs Inexperienced Unemployed



Notes: This figure represents the evolution of unemployment length around the kink with a split between inexperienced and experienced unemployed. Inexperienced unemployed are individuals in their very first unemployment spell, whereas experienced unemployed have already gone through at least one unemployment spell in the past. The elasticity is obtained, for both groups, by estimating equation 3.6 and through the following transformation: $\varepsilon_{D,b} = \hat{\beta}_1 * \frac{\bar{b}}{\bar{D}}$, where $\bar{b}$ and $\bar{D}$ represent respectively the average level of UB and average unemployment length around the kink.

Figure 3.3: Across spell behavioural response

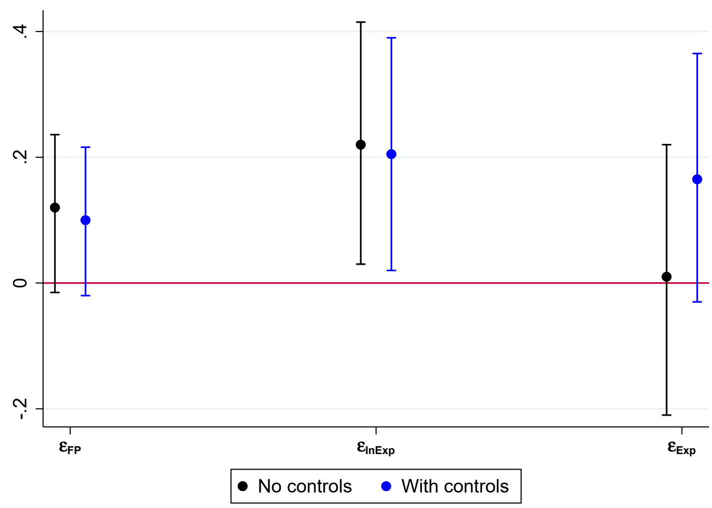


Notes: This figure represents the point estimate and confidence interval for the elasticity of unemployment duration with respect to the unemployment benefits level ($\varepsilon_{D,b}$), with a split by unemployment spell. The elasticity is obtained, for each group, by estimating equation 3.6 and through the following transformation: $\varepsilon_{D,b} = \hat{\beta}_1 * \frac{\bar{b}}{\bar{D}}$, where $\bar{b}$ and $\bar{D}$ represent respectively the average level of UB and average unemployment length around the kink.

Note that as the number of unemployment spells increases, the number of observations decreases, which explains the broader and broader confidence intervals[6]. As a consequence and to maximise the power in the remaining statistical analysis, I will focus on the two aforementioned groups: inexperienced Vs experienced unemployed.

So far, one could raise a few concerns about our analysis. First, all the elasticities I have shown are identified with no controls. If covariates were to be non smoothly distributed around the kink, this would be a threat to identification. I will therefore both control for covariates, and show that they are smoothly distributed around the kink.
Secondly, the split between experienced and inexperienced unemployed is mixing two distinct sources of variations: within and between individuals. Part of our sample is indeed observed in the first, and then second (eventually third and fourth) unemployment spell, while some other individuals will only be observed in a specific unemployment spell, and not necessarily the first one. The next figure addresses the first concern by introducing a large set of controls.

Figure 3.4: Behavioural Response - Experienced Vs Inexperienced Unemployed



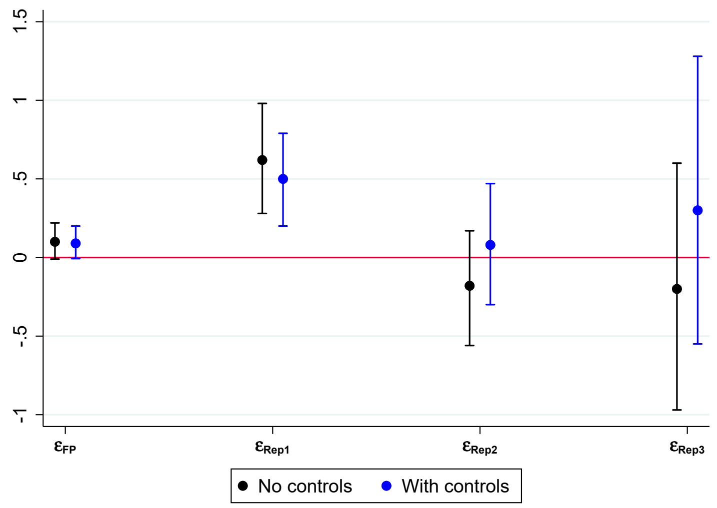
Notes: This figure represents the point estimate and confidence interval for the elasticity of unemployment duration with respect to the unemployment benefits level for three different groups, the whole population (ε_{FP}), the *inexperienced unemployed* (ε_{InExp}), and the *experienced unemployed* (ε_{Exp}). For each group, the elasticity is obtained by estimating equation 3.6 and through the following transformation: $\varepsilon_{D,b} = \hat{\beta}_1 * \frac{\bar{b}}{\bar{D}}$, where $\bar{b}$ and $\bar{D}$ represent respectively the average level of UB and average unemployment length around the kink.

[6]This can also be seen in table 3.E.1

As figure 3.4 shows, introducing controls slightly reduces the estimated behavioural response for the inexperienced unemployed. The corresponding coefficient nevertheless remains statistically significant.

Having a significant behavioural response for the first spell only could still be driven by individual level unobserved heterogeneity not captured by our broad set of controls. In order to address this concern, I introduce a new group, namely the *repeaters*. This group is only made of individuals that are observed during both their first and second unemployment spell. This way, I can make sure that the results are not driven by individual level unobserved heterogeneity, as long as this later is constant over time. If anything, the previous finding is made stronger and the key conclusion remains unchanged. Only the first unemployment spell generates a significant behavioural response.

Figure 3.5: Behavioural Response - Repeaters



Notes: This figure represents the point estimate and confidence interval for the elasticity of unemployment duration with respect to the unemployment benefits level for four different groups, the whole population (ε_{FP}), and the *repeaters* in their first, second and third unemployment spell (ε_{Repi}, i=1,2,3). For each group, the elasticity is obtained by estimating equation 3.6 and through the following transformation: $\varepsilon_{D,b} = \hat{\beta}_1 * \frac{\bar{b}}{\bar{D}}$, where $\bar{b}$ and $\bar{D}$ represent respectively the average level of UB and average unemployment length around the kink.

3.4.2 Estimation results

I now estimate equation 3.6 more formally, introducing additional controls and the aforementioned groups.

Experienced Vs Inexperienced Unemployed, and Repeaters

Table 3.1 below provides our key estimates.

The same table with a decomposition by unemployment spell is provided in the Appendix (table 3.E.3, section additional statistics). The set of controls used in these regressions incorporates the age, a dummy variable for whether the individual has a tertiary level of education, dummy variables for the sector, the occupation, the contract type and the job relationship, the number of dependants, tenure, the length of entitlement to UI, dummies for the municipality, all evaluated at the start of the spell, and year FE. This table reflects our key findings. Independently of the group definition, only the first unemployment spell generates a significant behavioural response. Even when focusing on the repeaters, which alleviates concerns related to individual unobserved heterogeneity, this result still holds, and is even reinforced. The estimated elasticity when focusing on the repeaters is indeed much larger. However, by focusing on such group, we might be selecting a very specific subsample among the population. Table 3.E.2 in the Appendix alleviates such concern. The repeaters indeed have similar characteristics compared to the rest of our sample. They are slightly less educated, but have on average the same age (28 years old), the same number of dependants and include about 44% of woman.

Table 3.1: Behavioural response - Elasticity estimates

	Experienced Vs Inexperienced				**Repeaters**			
	Inexperienced No controls	Inexperienced With controls	Experienced No controls	Experienced With controls	1st unemp spell No controls	1st unemp spell With controls	2nd unemp spell No controls	2nd unemp spell With controls
β	0.0559**	0.0492**	0.00294	0.0299	0.0992***	0.0801***	-0.0349	0.0110
	(0.0238)	(0.0218)	(0.0234)	(0.0214)	(0.0303)	(0.0307)	(0.0348)	(0.0340)
$\varepsilon_{D,b}$	0.231**	0.204**	0.0141	0.143	0.613***	0.495***	-0.203	0.0639
	(0.0985)	(0.0904)	(0.112)	(0.102)	(0.188)	(0.190)	(0.202)	(0.198)
Observations	16,210	15,841	14,822	13,949	4,519	4,519	4,518	4,508

Notes: The (duration) outcome is expressed in days. Controls include age, gender, a dummy variable for whether the individual has a tertiary level of education, dummy variables for the presence of children, sector, the occupation, the contract type, and the job relationship, the number of dependants, tenure, the entitlement period, dummies for the municipality, all evaluated at the start of the spell, and year FE. The elasticity is estimated as $\varepsilon_{D,b} = \beta * \frac{\bar{b}}{\bar{D}}$, where $\bar{D}$ is the average unemployment duration at the kink, and $\bar{b}$ is the average UB level at the kink. Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1.

Potential explanations

The previous results all suggest that their is something unique about the first unemployment spell. But why would unemployed individuals significantly react to exogenous variations in the UB level only during their first unemployment spell?

Knowledge of the UI system After being unemployed once, individuals should know more about the UI rules, and hence be more able to take advantage of them. This intuition however contradicts our previous result. Indeed if anything, having individuals taking advantage of a better knowledge of the UI system should lead the elasticity of unemployment duration with respect to UB to increase with the unemployment experience. My results exhibit the exact opposite trend. This does not mean that unemployed individuals do not behave strategically with a given knowledge of UI rules, but that they do not become increasingly strategic as their knowledge of these rules accumulates.

Liquidity constrained individuals One of the key insights from Chetty [2008] is that, depending on their ability to smooth exogenous shocks, unemployed individuals may react differently. He offers a key distinction between liquidity constrained and unconstrained individuals. He underlines that about 60% of the increase in unemployment duration caused by UI is due to a liquidity effect. To measure such effect, he uses severance payments received by a (non-random) subpopulation of unemployed. The MCVL unfortunately neither provide access to an equivalent type of information, nor to saving details that would allow us to precisely identify the liquidity constrained households.

Using the tax data nevertheless allows to go one step further in the exploration of the liquidity channel. The amount of income tax (IT) paid by individuals can indeed be used as a proxy to identify liquidity constrained individuals. The associated intuition is straightforward. If an agent paid a significant amount of IT the year before becoming unemployed, this later is likely to have some savings allowing to smooth temporary negative income shocks. The following table provides further details about the IT paid by the different groups.

Table 3.2: Income Tax By Groups

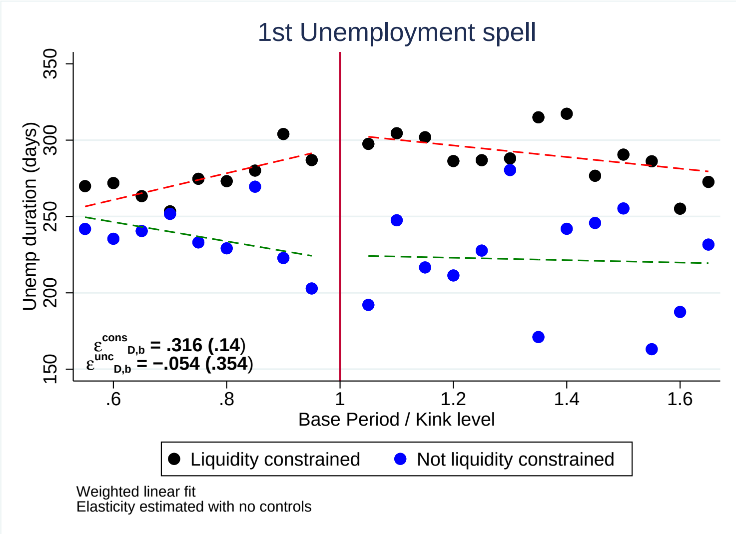
	Unemp1	Unemp2	Unemp3	Experienced	Repeat1	Repeat2
	Including agents not paying IT					
Avg IT tax paid	259.7	249.6	245.7	245.9	288.2	233
Median IT paid	0	0	0	0	0	0
75th pctile IT paid	96.62	97.01	107.1	97.68	122.4	95.23
85th pctile IT paid, Inc	350.9	333.8	341.8	329.4	418.8	268.8
95th pctile IT paid	1,549	1,520	1,449	1,485	1,580	1,459
	Excluding agents not paying IT					
Avg IT tax paid	665.2	603.1	589.5	594.5	679	534.5
Median IT paid	187.5	153.3	160.6	153.3	179.8	131.3
75th pctile IT paid	752.6	672	681.1	669.2	711.9	558.7
85th pctile IT paid, Exc	1,330	1,267	1,213	1,236	1,274	1,141
95th pctile IT paid	2,839	2,734	2,711	2,722	2,705	2,501
Fraction not paying IT (%)	59.45	57.50	57.55	57.68	55.48	55.03
Number of spells	19,578	11,197	4,992	17,604	5,351	5,352

Notes: Every amount is expressed in 2010 real euros. Unempi refers to individuals in their i-th unemployment spell. Repeati refers to repeaters in their i-th unemployment spell. Experienced are experienced unemployed, *i.e.* individuals that already faced one unemployment spell in the past. IT refers to the Income Tax.

Every group incorporates significant heterogeneity concerning the amount of IT paid, which will provide enough variation to analyse the influence of liquidity constraints. The definition of the liquidity constraint I will adopt, based on the amount of IT paid, however faces a constraint. Being too strict and only considering as not constrained the individuals above the 95th percentile would significantly reduce the number of observations. I will hence consider as not liquidity constrained all the individuals above the 85th percentile (computed among individuals paying a positive IT) concerning the amount of IT paid. Conversely, individuals not paying any IT will be considered as liquidity constrained. Due to sample size constraints, I cannot apply such decomposition to the repeaters and will hence focus on the experienced versus inexperienced. Interestingly for the case of inexperienced unemployed such distinction provides results perfectly in line with Chetty [2008]. Figure 3.6 below indeed shows that, while liquidity constrained individuals react strongly to the kink, unconstrained individuals are essentially unaffected. The estimated elasticity for the liquidity constrained individuals is 36% larger than the one I identified for the first unemployment spell (0.316 Vs 0.231).

Such distinction between liquidity constrained and unconstrained individuals however

Figure 3.6: Liquidity channel - First Unemployment spell



Notes: This figure represents the evolution of unemployment length around the kink with a split depending on liquidity constraints. Liquidity constrained individuals are individuals who pay an amount of IT below the 85th percentile of IT paid, computed among individuals paying a positive amount of IT. The elasticity is obtained, for both groups, by estimating equation 3.6 and through the following transformation: $\varepsilon_{D,b} = \hat{\beta}_1 * \frac{\bar{b}}{\bar{D}}$, where $\bar{b}$ and $\bar{D}$ represent respectively the average level of UB and average unemployment length around the kink.

does not yield any difference in terms of moral hazard cost of UI when analysing the second, third or fourth unemployment spell. Figure 3.D.1 in the Appendix shows that experienced unemployed do not respond to the kink, independently of the liquidity constraint.

A simple distinction between liquidity constrained and unconstrained individuals, while insightful, is therefore not sufficient to explain my results.

Behavioural aspects Another element that could explain the difference in terms of behavioural response between the first and subsequent spells is related to behavioural economics. It could indeed well be that, while experiencing their first unemployment spell, individuals pay a lot of attention to every information they receive. Facing such an adverse shock, it would make sense for them to make sure they will receive enough UB to maintain their consumption level, or at least to minimize the consumption drop (Δ). After having experienced their first unemployment spell, it could be that individuals become relatively more inattentive. This could explain at least part of the smaller behavioural responses with the increasing unemployment experience.

Unfortunately, no information in the MCVL allows to test this assumption. Such combination of behavioural economics with unemployment theory is a fruitful research area. In a recent article, Spinnewijn [2015] for instance underlined that unemployed suffer from biased beliefs concerning their employment prospects. They are indeed too optimistic about the time they will need to find a job while unemployed, which can induce non optimal savings behaviours.

3.4.3 Validity of the RKD

All the results presented so far are subject to the validity of the empirical strategy. The RKD approach relies on two key identifying assumptions.

First, individuals should not be able to manipulate the assignment variable. In more technical terms, this means that the pdf of the assignment variable should be smooth around the kink. The direct marginal effect of the wage on unemployment duration should hence be smooth around the kink.

Secondly, unobserved heterogeneity should also evolve smoothly around the kink.

These two assumptions mean that the conditional density of the wage and its first derivative have to evolve smoothly around the kink. They are made to ensure that the discontinuity indeed comes from an exogenous source of variation, namely the cap concerning the maximum UB level that can be received.

The McCrary [2008] test

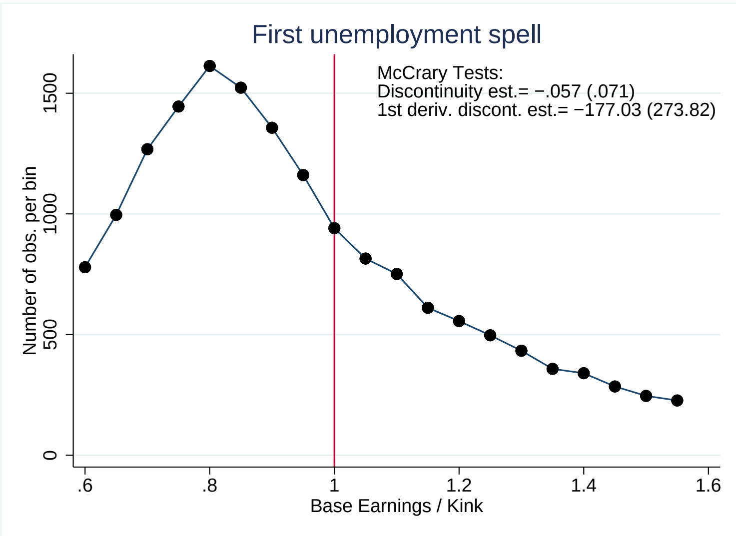
One way to check the validity of the first assumption is to group individuals by bins as a function of their reference wage, and to see whether the number of individuals per bin evolves discontinuously around the kink. This is the intuition underlying the well known McCrary [2008] test.

The next figures evaluate this first element and report the result of a McCrary test. Based on Card et al. [2012] and Landais [2015], I extend McCrary's intuition and test the continuity of the first derivative of the pdf. These elements are reported below, both for the individuals in their first unemployment spell, and for the experienced unemployed. Figures for the other groups are relayed in the Appendix.

Figures 3.7 and 3.8 and the tests they contain point in the same direction and confirm that the first assumption for the RKD's validity is respected. These figures indeed allow us to reject the presence of any discontinuity around the kink, both in levels and slopes.

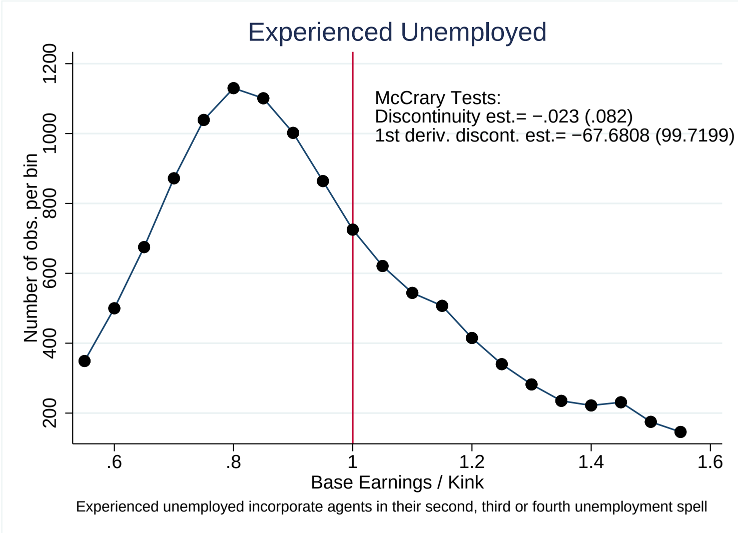
Interestingly, the wage distribution is shifted downward for experienced unemployed compared to the first time unemployed, in line with the dense literature about the *scarring effect* of unemployment (Arulampalam et al. [2000], Arulampalam et al. [2001]). The second assumption has to be tested as well. To do so, I plot the evolution of some key variables such as the age or education level as a function of the wage/kink prior the unemployment spell.

Figure 3.7: McCrary test - Inexperienced unemployed



Notes: This figure represents the evolution of the number of individuals on their first unemployment spell as a function of the running variable. It displays two tests: a baseline McCrary (2008) test, and a test for the discontinuity of the first derivative of the pdf. See main text for further details.

Figure 3.8: McCrary test - Experienced unemployed



Notes: This figure represents the evolution of the number of experienced unemployed as a function of the running variable. It displays two tests: a baseline McCrary (2008) test, and a test for the discontinuity of the first derivative of the pdf. See main text for further details.

Covariates

This subsection presents two figures that summarise the evolution of different relevant covariates for two of our key groups, the inexperienced unemployed, *i.e.* the individuals in their first unemployment spell, and the experienced ones, *i.e.* those that already experienced at least one unemployment spell. Similar figures for the other groups are provided in the Appendix, section additional figures.

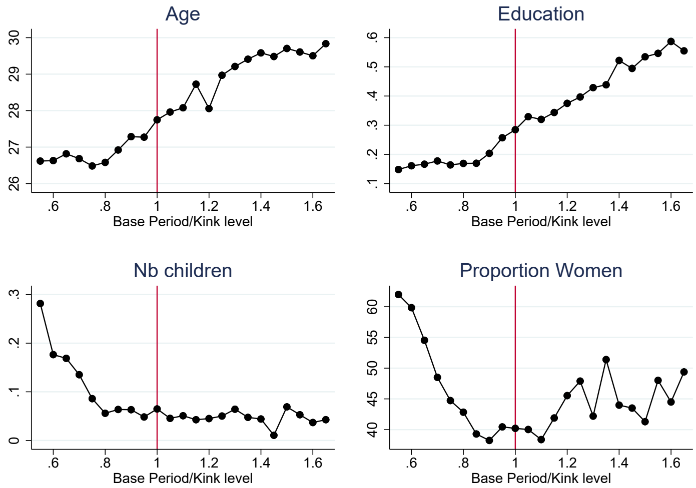
First striking element, these figures underline important non-linearities between the covariates and the assignment variable. However, these non linearities were to be expected, and are highly similar to those in Card et al. [2015b][7]. The wage for instance increases both with age and education. The fraction of women is higher at the bottom of the wage distribution as well. One potential concern could be that those non-linearities will drive the results. I do not believe that such reasoning holds for at least two reasons. First of all, these non linearities are almost identical for the first and subsequent unemployment spells (all incorporated into the experienced group). If they were to drive our results, then experienced unemployed would exhibit significant behavioural

[7]See the working paper version, appendix figure 3.

responses as well, which is not the case.

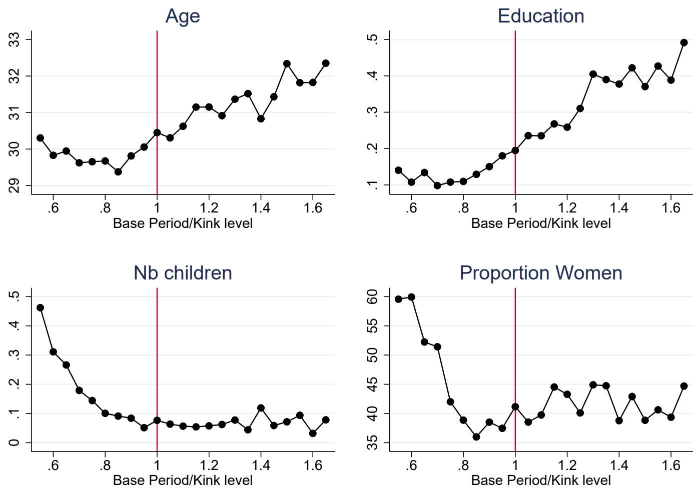
Secondly, as shown in the regression analysis, once controlling for those covariates, our key results still hold.

Figure 3.9: Covariates - Inexperienced unemployed



Notes: This figure represents, for the inexperienced unemployed, the evolution of four covariates as a function of the running variable.

Figure 3.10: Covariates - Experienced unemployed



Notes: This figure represents, for the experienced unemployed, the evolution of four covariates as a function of the running variable.

As can be seen from these two figures, the key covariates of interest do not display noticeable jump at the kink. Note that the second key assumption for the validity of the RKD does not imply that covariates must be distributed uniformly on both sides of the kink. The identified non-linearities do not imply violation of this assumption, as long as the covariates are smoothly distributed around the kink.

This second set of empirical evidence confirms the validity of our empirical strategy. These non-linearities could nevertheless raise different concerns that are addressed in the next subsection with permutation tests.

Permutation test

In this section, I implement a permutation test based on Ganong and Jäger [2018] and Card et al. [2015b]. Ganong and Jäger [2018] underline that the existence of non linearities between the assignment and the outcome variables can lead the RKD estimates to be spurious. In the Appendix (section additional figures), figures 3.E.6 to 3.E.9 represent the relationship between the outcome and the assignment variables for a much broader range of wages. The idea with the permutation test is to randomly allocate kink locations and to construct a distribution of placebo estimates in regions without policy changes. If these latter happen to be often significant, this would cast serious doubt about the validity of the RKD approach.

As pointed out by Card et al. [2015b], the presence of non linearities between the outcome variable and observable characteristics cannot be neglected. Curvature heterogeneity indeed happens to be a key element in the permutation test. Our figures 3.E.4 and 3.E.5 clearly underlined the presence of non linearities between the outcome and some key covariates. To assess the role of compositional changes in observables along the distribution of wages, I follow the method proposed by Card et al. [2015b]. I first regress the outcome variables on the four observable characteristics displayed in figures 3.E.4 and 3.E.5 using a 10% (randomly selected) of the sample[8]. I then predict the residuals from this regression for the remaining 90% of the sample, and use this latter variable as a new dependent variable[9] when estimating the effect of UB on unemployment duration at the placebo kinks.

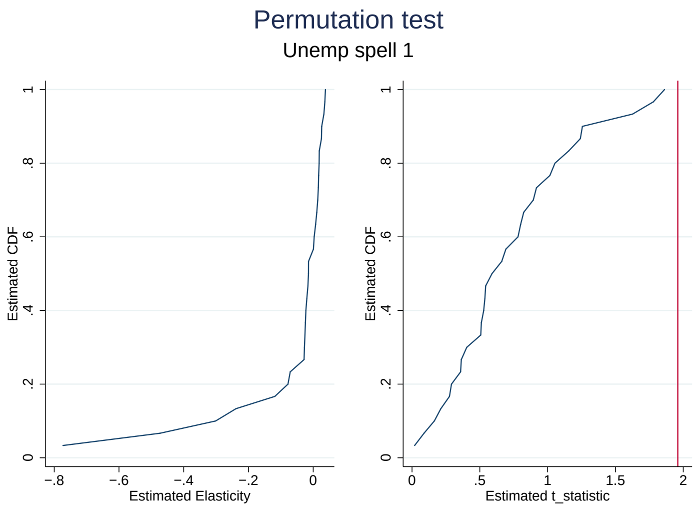
In the case of the first unemployment spell, figure 3.11 below reports the distribution of

[8]Card et al. [2015b] use a sample representing 5% of their population. Since my sample size is smaller, I prefer to use 10%.

[9]This ‘new’ outcome variable is therefore net from the non-linearities coming from observable characteristics.

both the estimated elasticity and the corresponding t-statistic for all the thirty placebo kinks I simulated.

Figure 3.11: Permutation test - First Unemployment spell



Notes: These figures contain the result of a permutation test *a la* Card et al. [2015b]. First, I randomly select 10% of the sample and regress unemployment length on the four covariates contained in figures 3.9 and 3.10. I then predict the residuals from that regression on the remaining 90%, and use this later variable as a new outcome. I finally randomly create placebo kinks, and test for discontinuity in the slope of the new outcome variable around those kinks.

First of all, note that all the estimated elasticities have an opposite sign compared to our reference estimates. None of them happen to be significant. This test therefore confirms that our findings are not driven by spurious correlations generated by non-linearities coming from observable characteristics. In the Appendix, I report this same test for the population of repeaters in their first unemployment spell, and the conclusion remains unchanged (see figure 3.E.10).

Bandwidth selection

Finally, a traditional concern about the RKD approach is related to the bandwidth choice. Results can indeed significantly differ depending on the chosen bandwidth.

In what follows, I proceed to a simple test, and ask: what would have been the results had the selected bandwidth been different? Table 3.3 below reports the estimated elasticities for individuals in their first unemployment spell. Tables for all other configurations are relayed in the Appendix.

Table 3.3: Bandwidth selection test - First unemployment spell

	No Controls		**With Controls**	
Bandwidth	N	$\varepsilon_{D,b}$	N	$\varepsilon_{D,b}$
$[0.9, 1.1]$	4,274	-0.549	4,211	-0.925
$[0.8, 1.2]$	8,774	-0.0443	8,674	-0.0279
$[0.7, 1.3]$	12,545	0.309	12,415	0.220
$[0.6, 1.4]$	15,112	0.149	14,962	0.182
$[0.5, 1.5]$	16,654	0.0658	16,354	0.162*
$[0.5, 1.6]$	17,127	0.138	16,827	0.164**
$\mathbf{[0.6, 1.6]}$	**16,210**	**0.231*** **	**15,841**	**0.204** **
$[0.5, 1.7]$	17,509	0.223***	17,099	0.184***
$[0.5, 1.8]$	17,830	0.314***	17,330	0.229***
$[0.5, 1.9]$	18,079	0.317***	17,679	0.222***
$[0.5, 2]$	18,302	0.301***	17,802	0.236***

Notes: This table contains results obtained after estimating equation 3.6 for different bandwidths and for individuals in their first unemployment spell. In bold, the main estimates of this article. *,**,*** denote significance at the 10%, 5% and 1% levels respectively.

In bold, the main point estimates used in this article. Lower bounds do not go below 50% of the kink level for the lower bandwidth as the kink level is already relatively low. For instance in 2010, an individual with an average daily wage larger than 51 euros, *i.e.* an average monthly wage larger than about 1,500 euros, would be above the kink. We hence only have a few observations below that threshold.

As can be seen, the elasticities remain similar across the different bandwidths. With really narrow bandwidths, observations are so close to the kink that any trend cannot be captured by our regression. The larger the bandwidth, the easier it becomes to capture any existing trend. This is reflected through the increasing relationship between the estimated elasticities and the selected interval's width. The elasticity lies between 0.16 and 0.3.

This bandwidth robustness check confirms the validity of our findings. Tables 3.E.5 and 3.E.6 reported in the Appendix, section Additional statistics, respectively for the repeaters in their second unemployment spell and the experienced unemployed, only contain non significant behavioural responses for all the bandwidths considered. This confirms that unemployed do respond differently to exogenous variations depending on their unemployment history. More precisely, unemployment benefits do generate a behavioural response only for the first unemployment spell, *i.e.* that the behaviour of the unemployed changes depending on their unemployment experience.

3.5 Conclusion

This article provides robust evidence that the moral hazard cost generated by unemployment insurance is significantly positive for the first unemployment spell, and the first one only. Unemployed individuals indeed respond to the exogenous kink in the Spanish UB schedule only in their first unemployment spell. Such heterogeneity could not be identified when analysing simultaneously the whole pool of unemployed.

Even when controlling for individual level and time constant unobserved heterogeneity, only the first unemployment spell leads to a positive and significant elasticity of unemployment duration with respect to UB. In the second, third and even fourth unemployment spell, unemployed do not respond any longer to the same exogenous shock. This finding is robust to the introduction of a broad range of controls, and still holds when proceeding to extensive permutation and bandwidth selection tests.

When reviewing different theoretical channels, I end up with a puzzle, as none of them can rationalise such finding. The idea of strategic unemployed and the distinction between liquidity constrained and unconstrained individuals both fail to explain the specificity of the first unemployment spell. If anything, they would indeed lead to conclude that unemployed should become more responsive to exogenous shocks as their unemployment experience increases, which is in contradiction with the empirical evidence.

Only the combination of unemployment theory with some of the insights from behavioural economics could potentially rationalise my findings.

This paper calls for further research about the heterogeneity among the pool of unemployed, as this latter could well affect the optimal design of UI. Introducing heterogeneity in terms of unemployment history and preferences in a basic Baily-Chetty framework could indeed, under some reasonable assumptions, justify the introduction of an increasing profile of UB with the number of unemployment spells experienced by individuals. My analysis however focuses on one specific component of the Baily-Chetty formula. Further research differentiating the consumption smoothing gain of unemployment insurance depending on the unemployment experience, as well as the risk aversion, would be useful to complete the picture, and potentially improve the design of UI systems.

Appendices

3.A Conceptual Framework - Details

3.A.1 Baily-Chetty formula with heterogeneous agents

Allowing for heterogeneity with two distinct groups within the unemployed population, formula (3.2) gives:

$$\begin{aligned}
\frac{d\hat{W}}{db} = \quad & \alpha\frac{d\hat{W}^f}{db} + (1-\alpha)\frac{d\hat{W}^r}{db} \\
= \quad & \alpha(\frac{1-\bar{\pi}_f}{\bar{\pi}})\Big\{\mathbb{E}^{u,f}\big[\frac{u'_{i,f}(c_i^{u,f}) - u'_{i,f}(c_i^{e,f})}{u'_{i,f}(c_i^{e,f})}\big] - \mathbb{E}^{e,f}\big[\frac{\varepsilon^{i,f}_{1-\pi_f,b}}{\pi_{i,f}}\big]\Big\}+ \\
& (1-\alpha)(\frac{1-\bar{\pi}_r}{\bar{\pi}})\Big\{\mathbb{E}^{u,r}\big[\frac{u'_{i,r}(c_i^{u,r}) - u'_{i,r}(c_i^{e,r})}{u'_{i,r}(c_i^{e,r})}\big] - \mathbb{E}^{e,r}\big[\frac{\varepsilon^{i,r}_{1-\pi_r,b}}{\pi_{i,r}}\big]\Big\} \\
\approx \quad & \mu_f\Big\{\bar{\gamma}^{u,f}\bar{\Delta}^{u,f} + cov^{u,f}(\gamma_i,\Delta_i) - \mathbb{E}^{u,f}\big[\frac{\varepsilon^{i,f}_{1-\pi_f,b}}{\pi_{i,f}}\big]\Big\}+ \\
& \mu_r\Big\{\bar{\gamma}^{u,r}\bar{\Delta}^{u,r} + cov^{u,r}(\gamma_i,\Delta_i) - \mathbb{E}^{u,r}\big[\frac{\varepsilon^{i,r}_{1-\pi_r,b}}{\pi_{i,r}}\big]\Big\}
\end{aligned}$$

Details: For each group, just use a first order Taylor expansion: $u'(c_u) - u'(c_e) \approx u''(c_e)(c_u - c_e)$, denote by $\gamma \equiv -\frac{u''(c_e)}{u'(c_e)} * c_e$ the coefficient of relative risk aversion, and by $\Delta \equiv \frac{c_e - c_u}{c_u}$. The μs represent weighted unemployment rate among each group. More precisely, $\mu_f \equiv \alpha(\frac{1-\bar{\pi}_f}{\bar{\pi}})$, and $\mu_r \equiv (1-\alpha)(\frac{1-\bar{\pi}_r}{\bar{\pi}})$.

3.B Key variables and sample restrictions

Wage identification

As underlined in equation 3.5, the average wage earned over the last 180 days of employment is a key component in UB identification. The RKD strategy requires high precision in the measure of this latter. The monthly individual-firm specific social security earnings are however not precise enough considering they are top and bottom coded. I hence follow the method proposed by Roca and Puga [2017] and allocate annual tax earnings across the different firms the individual worked for in a given year in proportion to social security earnings. This allows to obtain uncensored earnings for every working individual in the sample. In all the analysis, monetary amounts will be expressed in 2010 real euros.

UB identification

One of the difficulty in the analysis is to identify individuals' eligibity to unemployment benefits, and the level of UB they are entitled to.

One can use the tax data in order to identify the amount of UB individuals indeed received over a given year. The decision to build an algorithm that identifies *(i)* individuals' eligibility and *(ii)* the amount of UB they are entitled to over a given unemployment spell, is both theoretically and empirically motivated.

From a theoretical perspective, as underlined by Gruber [1997], '*receipt of unemployment insurance, and the amount of UI received, is endogenous*'. This is true in the Spanish case since the receipt of UB is not automatic. Agents have to fill a form to claim the benefits they are potentially entitled to. They could however decide not to do so, due to potential stigma effects, or if they for instance believe that they will find a new job rapidly (Spinnewijn [2015]).

From an empirical perspective, remember that tax data are provided on a yearly basis. In case the individual experienced multiple unemployment spells over a given year, I cannot precisely identify the amount she received over each spell. Conceptually a simple proportionnality rule could be applied. However, it is not possible to know whether the agent received benefits right at the beginning of the spell or a few weeks after. Even in case the individual experienced only one spell over a given year, it could be that she collected only part of the benefits she was entitled to.

The first step in this algorithm requires to precisely analyse the labour market experience of the individual prior the unemployment spell. One indeed needs to compute the cumulated number of days worked before the unemployment experience (and after the previous one, if one occurred before). The longitudinal structure of the MCVL allows a very precise identification of this information. This part is however not as trivial at it might seem since individuals can experience multiple unemployment/non-employment spells. Let us go through some examples.
Imagine an individual that started working at 22 years old with a permanent contract. Suppose this agent worked continuously, and was made redundant at age 28 for economic reasons. This individual cumulated more than 3 years of work experience, and will hence be entitled to the maximum duration of unemployment benefits (2 years).
However, individuals can experience multiple unemployment spells (even within the same year), which makes the identification of their eligibility more complex.
Imagine that this same individual after her first unemployment spell found a job, and experienced a new unemployment spell. Identifying whether she is entitled to unemployment benefits for this second spell requires careful analysis.

If the first unemployment spell had a length of 100 days the individual will have used 100 days over the 720 she was entitled to. From the first entitlement, she will still be eligible to unemployment benefits for the days she did not use. In this context, independently of the length of the employment spell after her first unemployment spell, she will be entitled to unemployment benefits during her second unemployment spell

Now imagine that this individual used her complete entitlement by the end of the first unemployment spell, *i.e.* that she stayed unemployed for at least two years. In this case, her eligibility during the second unemployment spell will completely be determined by the length of the employment spell between the two unemployment spells. If she worked for more than 180 days (full time), she will create a new eligibility, but if not, she will not be eligible to UB. However, she could still be eligible to unemployment assistance. My algorithm identifies these configurations as well, but the analysis will focus on contributory UB[10].

Finally, imagine that the agent has not used her complete entitlement during the first unemployment spell, and created a new one in between the two unemployment spells. Denote by E_1 the first entitlement generated, by E_2 the second one, and by D_1 the duration of the first unemployment spell. The algorithm relies on the following rule: $E = max(E_1 - D_1, E_2)$.

The second step in this algorithm is to identify the last 180 days the agent has worked before the start of the unemployment spell. Note that employment experience can be discontinuous, and individuals can alternate between employment and non employment before becoming unemployed. Once those 180 days identified, one can compute the average daily wage, and then apply the 70% replacement rate. The estimated UB amount will then be capped above or below depending on the family situation (see table 3.1 above and formula 3.5).

Sample restrictions

I define unemployment spells based on the *tipo de relacion laboral* variable contained in the AFILIAD files. This latter allows to differentiate between unemployment and non employment. Indeed, when taking a value between 750 and 760, this variable clearly identifies the individual as receiving or at least claiming unemployment benefits.

The sample contains all the individuals that experienced at least one unemployment spell between 2006 and mid-2012. I further restrict unemployment spells as periods of at least 2 weeks where the individuals claimed unemployment benefits. In line with Kolsrud et al. [2018] and with Chetty [2008], I censor unemployment spells with duration longer that 730 days. This allows to deal with some spells without any end date, and to reduce the influence of outliers. To reduce the effect of seasonal unemployment, I drop all the individuals observed experiencing strictly more than 4 unemployment spells. Note

[10]For further details about contributory unemployment benefits and unemployment assistance in Spain, see the following documents: contributory UB and UA

that individuals are potentially observed from the early 1980s until 2016, and that the aforementioned restriction applies over this whole period. Consequently, an individual experiencing two unemployment spells, the first one in 1990 and the second one in 2008 would be included in the sample. However, an agent experiencing unemployment spells only before 2006 or after mid-2012 would not, neither would an individual experiencing 5 or more unemployment spells.

I also drop individuals that receive unemployment benefits while still working. Employed individuals can indeed receive UB in case they faced a decrease in their number of hours worked.

The analysis also focuses on individuals between 20 and 50 years old, the reason being that after 50, individuals can receive pre-retirement subsidies potentially distorting their search effort/incentive to find a job.

Using the fiscal data, agents receiving disability benefits are excluded as well. The reason of this exclusion is that such configurations could potentially distort labour supply decisions/search effort while unemployed. If one spell starts during the same month the previous one ended, they are aggregated, their duration is hence summed over. This assumption is innocuous and does not affect our results. It simply makes the algorithm that identifies UB much faster.

Part time workers are excluded from the analysis. The reason is that, For this type of contract, UB and eligibility identification would require to know the precise number of hours worked, an information not directly available in the MCVL. Voluntary quitters are excluded as well. The decision to quit could be motivated by job offers from other firms or industries, which could in turn affect the individuals' job search effort. Self employed are excluded as well for similar motives, and due to well known to self-reporting issues.

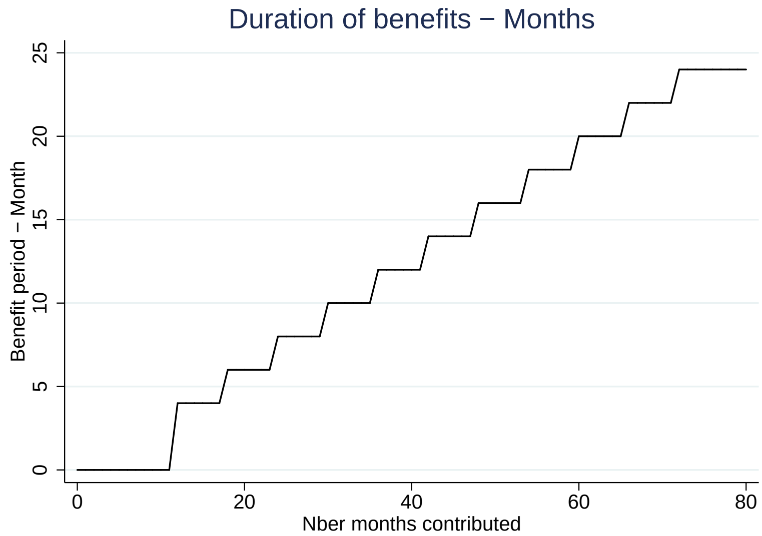
I focus on individuals that contributed within the general regime ('*REGIMEN GENERAL*') since some regimes can have specific rules.

Finally, observations from Ceuta and Melilla are excluded as well given their special enclave status in Africa (Roca and Puga [2017]).

3.C Institutional background

3.C.1 Benefit duration

Figure 3.C.1: Benefits duration



Notes: This figure represents the length of entitlement to UI, as a function of the duration worked over the last 6 years, both expressed in months.

3.C.2 IPREM

The following table provides further details about the IPREM evolution over time. Note that prior 2003, the data refer to the SMI and not the IPREM.

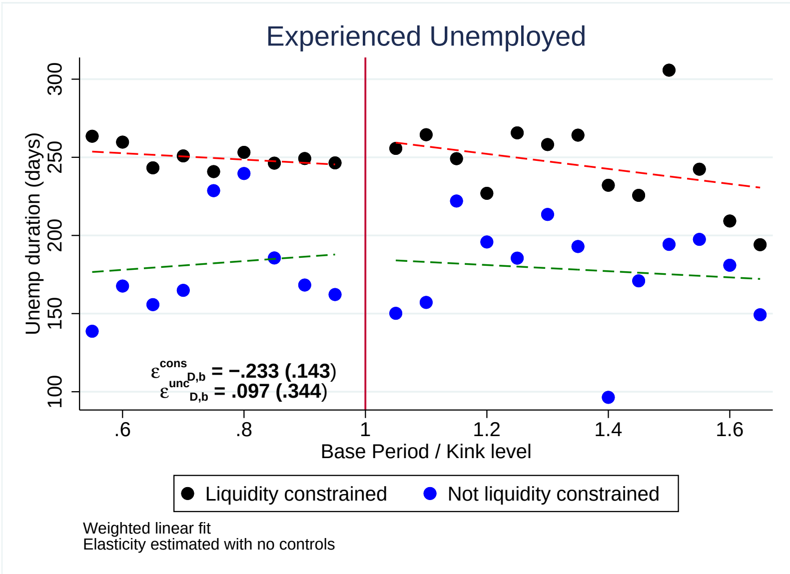
Table 3.C.1: IPREM

Year	Daily IPREM	Monthly IPREM (30days)	Yearly IPREM (12 payments)	Yearly IPREM + 1/6 (14 payments)
1992	11.276	338.280	4059.360	4735.920
1993	11.726	351.770	4221.240	4924.780
1994	12.134	364.030	4368.360	5096.420
1995	12.561	376.830	4521.960	5275.620
1996	13.006	390.180	4682.160	5462.520
1997	13.350	400.500	4806.000	5607.000
1998	13.631	408.930	4907.160	5725.020
1999	13.877	416.320	4995.840	5828.480
2000	14.160	424.800	5097.600	5947.200
2001	14.448	433.450	5201.400	6068.300
2002	14.740	442.200	5306.400	6190.800
2003	15.033	451.000	5412.000	6314.000
2004	15.350	460.500	5526.000	6447.000
2005	15.660	469.800	5637.600	6577.200
2006	15.970	479.100	5749.200	6707.400
2007	16.640	499.200	5990.400	6988.800
2008	17.230	516.900	6202.800	7236.600
2009	17.570	527.240	6326.860	7381.330
2010	17.750	532.510	6390.130	7455.140
2011	17.750	532.510	6390.130	7455.140
2012	17.750	532.510	6390.130	7455.140
2013	17.750	532.510	6390.130	7455.140
2014	17.750	532.510	6390.130	7455.140
2015	17.750	532.510	6390.130	7455.140
2016	17.750	532.510	6390.130	7455.140

Notes: This table contains details about the evolution of the IPREM (*Indicador Publico de Renta de Efectos Multiples*) over time. Prior 2003, this table contains details for the SMI and not the IPREM.

3.D Additional Figures

Figure 3.D.1: Liquidity channel - Experienced Unemployed



Notes: This figure represents the evolution of unemployment length around the kink for two distinct groups among the unemployed population that already experienced an unemployment spell in the past: liquidity constrained and non liquidity constrained. The former group is made of individuals that are below the 85th percentile of IT paid computed among individuals that do pay IT.

3.E Additional statistics

Table 3.E.1: Summary Statistics

	FULL SAMPLE	1^{st} unemp spell	2^{nd} unemp spell	3^{rd} unemp spell	4^{th} unemp spell
	Unemployment characteristics				
Avg Unemployment length (days)	243.8	259.3	225.2	227.5	232.7
Median unemployment length (days)	180	183	154	160	164
Monthly UB amount	1,085	1,086	1,084	1,082	1,082
Daily UB amount	36.15	36.19	36.13	36.07	36.08
Monthly potential UB amount, no cap	1,146	1,164	1,132	1,122	1,109
Daily potential UB amount, no cap	38.21	38.79	37.74	37.40	36.95
Total amount of UB received	8,715	9,039	8,262	8,425	8,843
Median of total UB received	6,094	6,402	5,591	5,742	5,778
Entitlement period (days)	411.2	432.8	395	379.6	366.1
Median entitlement period (days)	420	420	395	360	360
Average time btwn unemp spells (days)	533.7	.	1,198	1,023	939.4
Median time btwn unemp spells (days)	.	.	731	561	477
	Demographic characteristics				
Fraction of woman	0.448	0.459	0.447	0.419	0.408
Age	29.08	27.85	29.70	31.39	33.04
Fraction with tertiary education	0.255	0.291	0.234	0.188	0.179
Average number of kids	0.108	0.0890	0.119	0.138	0.190
	Labor market characteristics				
Average reference wage	1,655	1,662	1,652	1,643	1,628
Median reference wage	1,446	1,444	1,446	1,454	1,454
Average tenure (days)	808.3	894.1	710.8	701	725.4
Median tenure (days)	457	541	374	366	386
Average employment experience (days)	2,138	1,810	2,317	2,750	3,117
Median employment experience (days)	1,825	1,434	2,004	2,470	2,838
Wage level at the kink	1,554	1,551	1,556	1,557	1,566
Fraction with wage>kink	0.419	0.419	0.416	0.426	0.422
Number of spells	37,182	19,578	11,197	4,992	1,415

Notes: This table contains summary statistics, for the whole sample and with a decomposition by unemployment spell. The amounts are in 2010 real euros. The *no cap* rows represent the amount of UB the individuals would have received had no maximum/minimum amount been fixed by the government.

Table 3.E.2 below provides summary statistics concerning the sample of repeaters.

Table 3.E.2: Summary statistics - Repeaters

	Repeaters 1st unemp spell	Repeaters 2nd unemp spell
	Unemployment characteristics	
Unemployment length (days)	174.7	184.5
Median unemployment length (days)	123	123
Monthly UB amount (2010 real euros)	1,088	1,078
Daily UB amount (2010 real euros)	36.27	35.92
Monthly potential UB amount, no caps (2010 real euros)	1,142	1,106
Daily potential UB amount, no cap (2010 real euros)	38.08	36.88
Total amount of UB received (2010 real euros)	6,479	7,089
Median of total UB received (2010 rel euros)	4,744	4,776
Entitlement period (days)	441.6	297.5
Median entitlement period (days)	420	253
Average time btwn unemp spells (days)	.	425.5
Median time btwn unemp spells (days)	.	288.5
	Demographic characteristics	
Fraction of woman	0.442	0.442
Age	27.23	28.86
Fraction with tertiary education	0.247	0.246
Average number of kids	0.0760	0.118
	Labor market characteristics	
Average reference wage	1,632	1,652
Median reference wage	1,430	1,444
Average tenure (days)	776.7	439.2
Median tenure (days)	455	271
Average employment experience (days)	1,763	2,121
Median employment experience (days)	1,438	1,826
Wage level at the kink (2010 real euros)	1,554	1,555
Fraction with wage>kink	0.395	0.416
Number of spells	5,351	5,352

Notes: This table contains summary statistics for the repeaters, with a decomposition between the first and second unemployment spell. The amounts are in 2010 real euros. The *no cap* rows represent the amount of UB the individuals would have received had no maximum/minimum amount been fixed by the government.

3.E.1 Further regressions

Decomposition by spell

Table 3.E.3 below estimates equation 3.6 by decomposing the sample by the number of unemployment spell already experienced in the past.

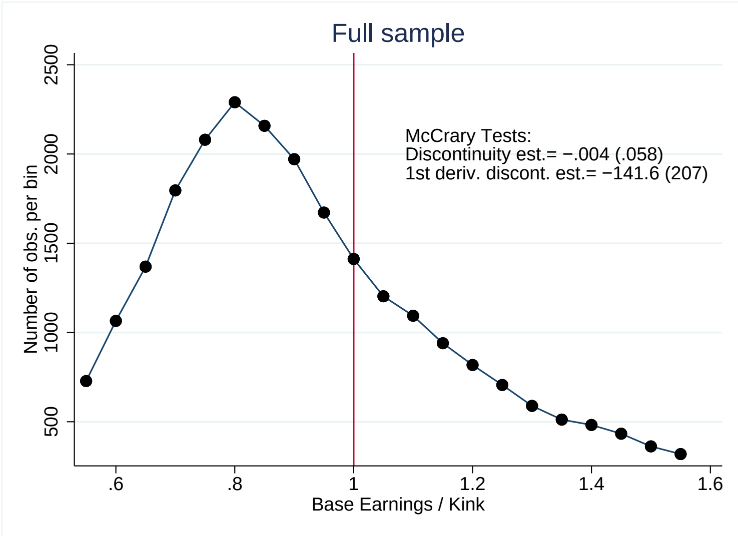
Table 3.E.3: Decomposition by unemployment spell

	Full sample No controls (1)	Full sample With controls (2)	Unemp 1 No controls (3)	Unemp 1 With controls (4)	Unemp 2 No controls (5)	Unemp 2 With controls (6)	Unemp 3 No controls (7)	Unemp 3 With controls (8)	Unemp 4 No controls (9)	Unemp 4 With controls (10)
β	0.0268	0.0224	0.0559**	0.0492**	0.0394	0.0433*	-0.0387	-0.0299	-0.112	0.00563
	(0.0168)	(0.0151)	(0.0238)	(0.0218)	(0.0293)	(0.0259)	(0.0436)	(0.0398)	(0.0878)	(0.0892)
$\varepsilon_{D,b}$	0.118	0.0989	0.231**	0.204**	0.189	0.208*	-0.184	-0.142	-0.522	0.0263
	(0.0742)	(0.0670)	(0.0985)	(0.0904)	(0.140)	(0.124)	(0.208)	(0.189)	(0.411)	(0.417)
Observations	31,032	29,763	16,210	15,841	9,436	8,926	4,195	3,928	1,191	1,095

Notes: This table contains estimated coefficients from equation 3.6 with a split by unemployment spell. For each specification and potential subgroup, the elasticity of unemployment duration with respect to UB is obtained as follows: $\varepsilon_{D,b} = \beta * \frac{\bar{b}}{\bar{D}}$, where $\bar{b}$ and $\bar{D}$ represent respectively the average UB level and duration of unemployment around the kink.
Robust standard errors in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

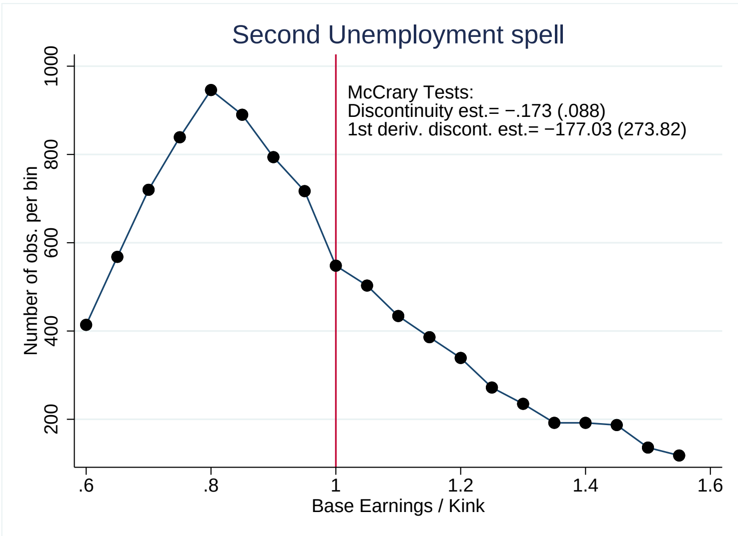
McCrary by spell

Figure 3.E.1: McCrary test - Full sample



Notes: This figure represents the evolution of the number of individuals in the whole sample as a function of the running variable. It displays two tests: a baseline McCrary (2008) test, and a test for the discontinuity of the first derivative of the pdf. See main text for further details.

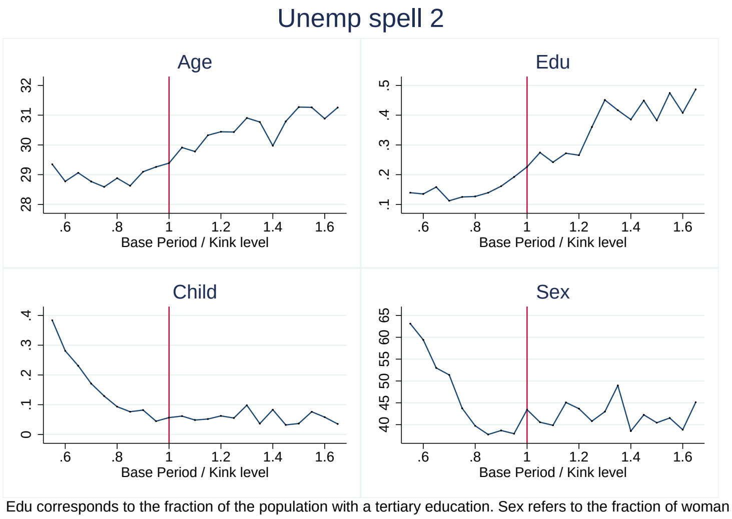
Figure 3.E.2: McCrary test - 2nd unemployment spell



Notes: This figure represents the evolution of the number of individuals in their second unemployment spell as a function of the running variable. It displays two tests: a baseline McCrary (2008) test, and a test for the discontinuity of the first derivative of the pdf. See main text for further details.

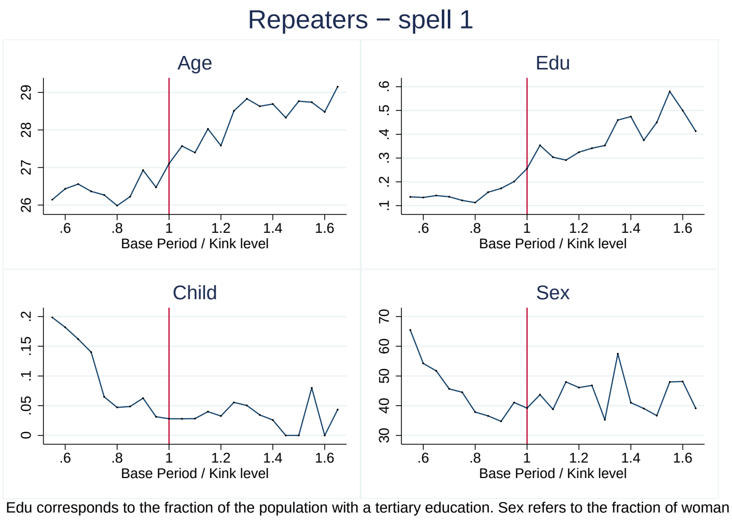
Covariates

Figure 3.E.3: Covariates - Second unemployment spell



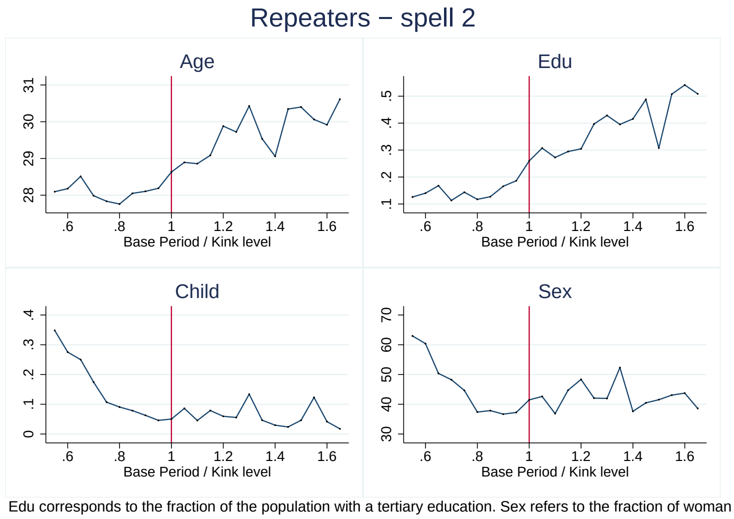
Notes: This figure represents, for the individuals in their second unemployment spell, the evolution of four covariates as a function of the running variable.

Figure 3.E.4: Covariates - Repeaters - First unemployment spell



Notes: This figure represents, for the population of repeaters in their first unemployment spell, the evolution of four covariates as a function of the running variable.

Figure 3.E.5: Covariates - Repeaters - Second unemployment spell



Notes: This figure represents, for the population of repeaters in their second unemployment spell, the evolution of four covariates as a function of the running variable.

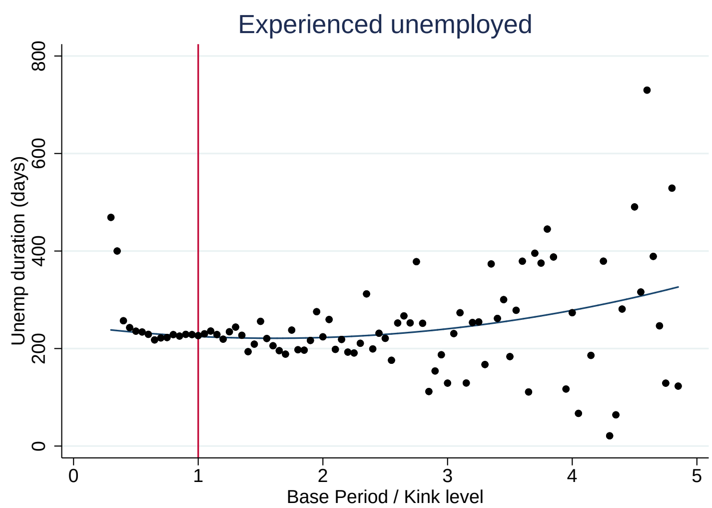
Permutation test

The following figures illustrate the relationship between the outcome and the assignment variable for a much larger window incorporating almost all the sample.

Figure 3.E.6: Permutation test - Raw data - Unemployment spells

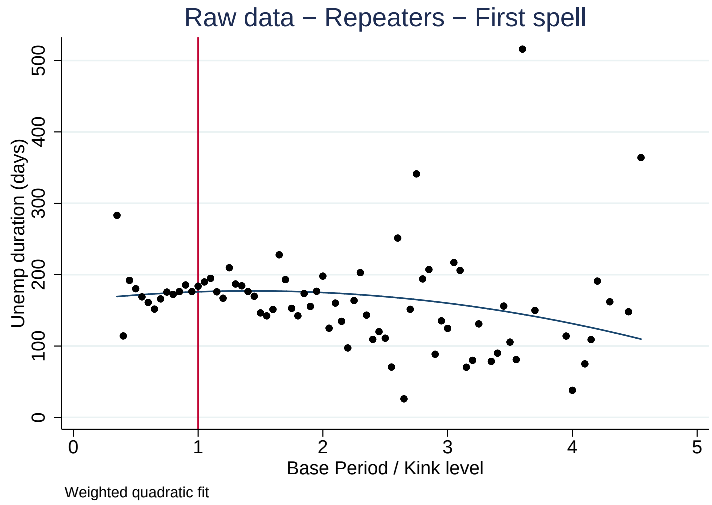
Notes: These figures illustrate the relationship between unemployment length and the running variable for each of the first four unemployment spells.

Figure 3.E.7: Permutation test - Raw data - Experienced



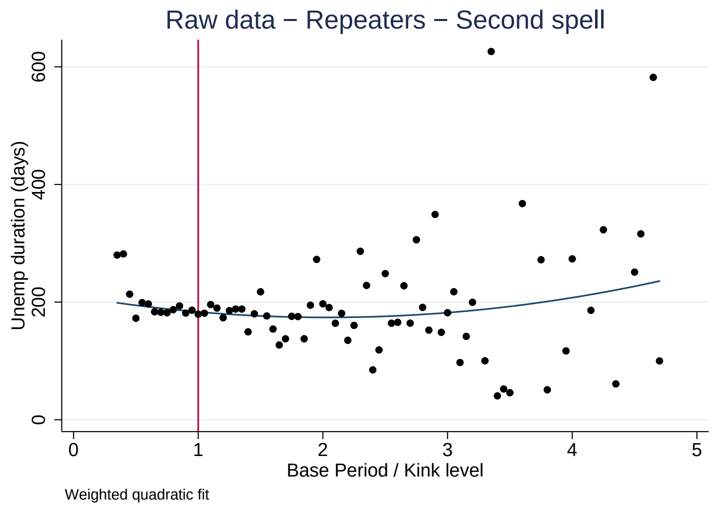
Notes: This figure illustrates the relationship between unemployment length and the running variable for the experienced unemployed altogether.

Figure 3.E.8: Permutation test - Raw data - Repeaters 1st spell



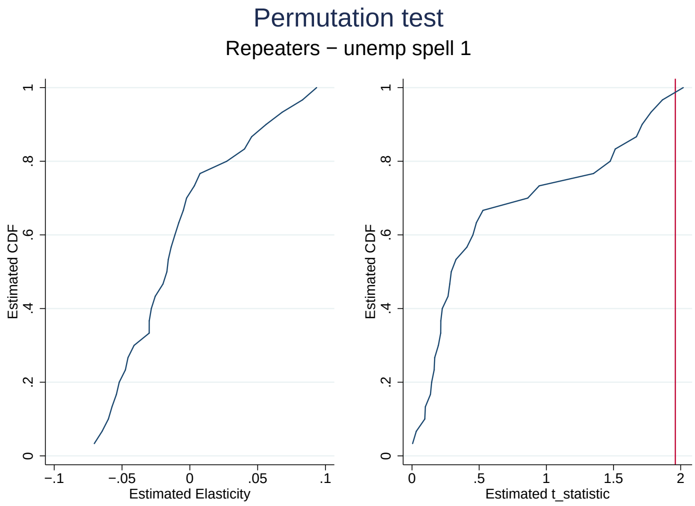
Notes: This figure illustrates the relationship between unemployment length and the running variable for the population of repeaters in their first unemployment spell.

Figure 3.E.9: Permutation test - Raw data - Repeaters 2nd spell



Notes: This figure illustrates the relationship between unemployment length and the running variable for the population of repeaters in their second unemployment spell.

Figure 3.E.10: Permutation test - Repeaters 1st spell



Notes: These figures contain the result of a permutation test *a la* Card et al. [2015b] for the population of repeaters in their first unemployment spell. First, I randomly select 10% of the sample and regress unemployment length on the four covariates contained in figures 3.9 and 3.10. I then predict the residuals from that regression on the remaining 90%, and use this later variable as a new outcome. I finally randomly create placebo kinks, and test for discontinuity in the slope of the new outcome variable around those kinks.

Robustness checks - Bandwidth selection

Table 3.E.4: Bandwidth selection test - Repeaters - First unemployment spell

	No Controls		**With Controls**	
Bandwidth	N	$\varepsilon_{D,b}$	N	$\varepsilon_{D,b}$
[0.9, 1.1]	1,218	–1.203	1,180	-1.863
[0.8, 1.2]	2,533	0.300	2.476	0.243
[0.7, 1.3]	3,572	0.113	2.489	0.156
[0.6, 1.4]	4,267	0.312	4,141	0.258
[0.5, 1.5]	4,649	0.0658	4,512	0.255
[0.5, 1.6]	4,759	0.427*	4,666	0.376
[0.6, 1.6]	**4,519**	**0.613*** **	**4,405**	**0.495*** **
[0.5, 1.7]	4,859	0.351***	4,711	0.265***
[0.5, 1.8]	4,942	0.341***	4,899	0.214*
[0.5, 1.9]	5,006	0.370**	4,905	0.217**
[0.5, 2]	5,052	0.369***	4,922	0.266**

Notes: This table contains results obtained after estimating equation 3.6 for different bandwidths and for the repeaters in their first unemployment spell. In bold, the main estimates used in this article.
*,**,*** denote significance at the 10%, 5% and 1% levels respectively.

Table 3.E.5: Bandwidth selection test - Repeaters - Second unemployment spell

	No Controls		**With Controls**	
Bandwidth	N	$\varepsilon_{D,b}$	N	$\varepsilon_{D,b}$
$[0.9, 1.1]$	1,118	-1.287	1,080	1.925
$[0.8, 1.2]$	2,500	-0.904	2,426	-0.229
$[0.7, 1.3]$	3,530	-0.242	2.459	0.0306
$[0.6, 1.4]$	4,203	-0.325	4,101	-0.0326
$[0.5, 1.5]$	4,612	-0.112	4,491	0.122
$[0.5, 1.6]$	4,742	-0.198	4,626	0.0416
[0.6, 1.6]	**4,508**	**-0.203**	**4,485**	**0.0639**
$[0.5, 1.7]$	4,807	-0.0242	4,701	0.170
$[0.5, 1.8]$	4,842	0.0161	4,729	0.215
$[0.5, 1.9]$	4,987	0.0423	4,897	-0.0847
$[0.5, 2]$	5,001	0.0903	4,912	0.0903

Notes: This table contains results obtained after estimating equation 3.6 for different bandwidths and for the repeaters in their second unemployment spell. In bold, the main estimates used in this article.
*,**,*** denote significance at the 10%, 5% and 1% levels respectively.

Table 3.E.6: Bandwidth selection test - Experienced Unemployed

	No Controls		**With Controls**	
Bandwidth	N	$\varepsilon_{D,b}$	N	$\varepsilon_{D,b}$
$[0.9, 1.1]$	4,061	-1.169	4,001	-0.245
$[0.8, 1.2]$	8,257	-0.256	8,202	0.217
$[0.7, 1.3]$	11,634	-0.0273	11,587	0.123
$[0.6, 1.4]$	13,831	-0.133	13,789	0.001
$[0.5, 1.5]$	15,172	-0.0019	15,100	0.0973
$[0.5, 1.6]$	15,584	-0.0680	15,498	0.0216
[0.6, 1.6]	**14,822**	**0.0141**	**14,761**	**0.0804**
$[0.5, 1.7]$	15,950	0.0145	15,890	0.090
$[0.5, 1.8]$	16,208	0.0255	16,128	0.144
$[0.5, 1.9]$	16,442	0.0613	16,382	0.128*
$[0.5, 2]$	16,611	0.0191	16,532	0.0842*

Notes: This table contains results obtained after estimating equation 3.6 for different bandwidths and for the experienced unemployed, *i.e.* for individuals that already experienced at least one unemployment spell in the past. In bold, the main estimates used in this article.
*,**,*** denote significance at the 10%, 5% and 1% levels respectively.

Table 3.E.7: Bandwidth selection test - Second unemployment spell

	No Controls		**With Controls**	
Bandwidth	N	$\varepsilon_{D,b}$	N	$\varepsilon_{D,b}$
$[0.9, 1.1]$	2,565	-0.802	2.545	0.743
$[0.8, 1.2]$	5,223	-0.0148	5,193	0.555
$[0.7, 1.3]$	7,394	0.164	7,304	0.373
$[0.6, 1.4]$	8,803	0.0790	7,948	0.172
$[0.5, 1.5]$	9,668	0.193	9,599	0.259*
$[0.5, 1.6]$	9,922	0.102	9,900	0.177
$[0.6, 1.6]$	**9,436**	**0.189**	**8,926**	**0.208**
$[0.5, 1.7]$	10,138	0.189	10,087	0.201
$[0.5, 1.8]$	10,291	0.203	10,171	0.246*
$[0.5, 1.9]$	10,441	0.232	10,371	0.253*
$[0.5, 2]$	10,551	0.187	10,491	0.207

Notes: This table contains results obtained after estimating equation 3.6 for different bandwidths and for individuals in their second unemployment spell. In bold, the main estimates used in this article.
*,**,*** denote significance at the 10%, 5% and 1% levels respectively.

Table 3.E.8: Bandwidth selection test - Third unemployment spell

	No Controls		**With Controls**	
Bandwidth	N	$\varepsilon_{D,b}$	N	$\varepsilon_{D,b}$
$[0.9, 1.1]$	1,166	-2.455	1,126	-2.833
$[0.8, 1.2]$	2,377	-0.627	2,347	-0.467
$[0.7, 1.3]$	3,316	-0.645	3,281	-0.502
$[0.6, 1.4]$	3,917	-0.611*	3,834	-0.443
$[0.5, 1.5]$	4,283	-0.292	4,201	-0.273
$[0.5, 1.6]$	4,409	-0.240	4,317	-0.167
$[0.6, 1.6]$	**4,195**	**-0.184**	**3,928**	**-0.142**
$[0.5, 1.7]$	4,523	-0.194	4,467	-0.130
$[0.5, 1.8]$	4,608	-0.206	4,528	-0.125
$[0.5, 1.9]$	4,675	-0.131	4,579	-0.0585
$[0.5, 2]$	4,721	-0.183	4,653	-0.110

Notes: This table contains results obtained after estimating equation 3.6 for different bandwidths and for individuals in their third unemployment spell. In bold, the main estimates used in this article.
*,**,*** denote significance at the 10%, 5% and 1% levels respectively.

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