Choosing Unemployment Benefits:the Role of Adverse Selection and Moral Hazard

Laura Khoury

PARIS SCHOOL OF ECONOMICS
ECOLE D'ECONOMIE DE PARIS

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Abstract

Most unemployment insurance (UI) schemes mandate a single benefit schedule, while little empirical findings support this mandate. In this paper, I exploit a French program where workers are given a choice between two different UI schedules, providing an ideal setup to evaluate both moral hazard and selection into UI. Using high-quality administrative data, I measure significant adverse selection by relating the entitlement choice with the characteristics of the insured. Moral hazard is even larger, as shown by a fuzzy regression discontinuity design using an eligibility criterion: choosing a short schedule with higher average benefits increases unemployment duration by six months.

Keywords: Unemployment, moral hazard, adverse selection, insurance design

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†Norwegian School of Economics, Helleveien 30, 5045 Bergen, Norway. Email: laura.khoury@nhh.no

1 Introduction

The very principle of offering mandated public unemployment insurance (UI) is rarely questioned. This is because theory has well-established that a mandate can solve the underprovision of insurance in a competitive private equilibrium that is due to adverse selection (Rothschild and Stiglitz, 1976; Akerlof, 1978).[1] In line with this theoretical argument, we observe that most existing UI schemes offer a single mandated schedule at the national level. However, empirical evidence supporting this argument in the specific context of UI is lacking, the main challenge being that mandated schemes do not allow researchers to observe insurance choices.[2]

In this paper, I take advantage of an uncommon UI program that lets jobseekers choose between two benefit schedules. Both schedules differ horizontally in benefit level, duration, and time profile, and vertically, since the total sum of potential benefits collected is higher in one option. This allows me to measure both the adverse selection and moral hazard costs of UI benefits in a unique setting. Under this program, jobseekers can choose between either a long length of time with on average lower benefits, or a shorter length of time with on average higher benefits. Leveraging high-quality administrative data, I first assess the extent of moral hazard after a simultaneous change in the level, duration, and time profile of benefits (while those parameters are generally analyzed separately). To do so, I measure the causal impact of choosing a shorter unemployment compensation with on average higher benefits using a regression discontinuity design (RDD) on an eligibility threshold.[3] Second, I identify adverse selection into UI by relating the choice of the insured to her initial level of risk.[4] While previous empirical literature on UI has mainly focused on moral hazard, evidence on the empirical existence of selection into UI remains scarce. I measure adverse selection and separate it from moral hazard using the estimate from the RDD.

The program under study, called the *option right* (OR), was introduced in France in 2015, to allow unemployed workers alternating short employment and unemployment spells to better smooth their income. More precisely, it targets people who received

[1] Adverse selection may arise when there is heterogeneity in the level of risk, and individuals have private information on their risk type (Rothschild and Stiglitz, 1976; Akerlof, 1978). The intuition is that the insurer prices according to the average risk level, as he has no information on individual risk type. The presence of high-risk types makes this average premium too costly for low-risk types, who exit the market. As low-risk types leave, the average premium goes up, driving ever more low-risk types out of the market, and ending up in its collapse.

[2] Sweden, Denmark, Finland and Iceland are the only countries where choice is widely available in the UI system.

[3] Noneligibles cannot choose and are offered the default option, which is the long schedule with on average lower benefits. Moral hazard in the paper refers to the behavioral change in response to this increase in the average level of benefits entailed by the high-short-benefit schedule, although it implies a higher benefit at the start of the spell and a lower benefit later in the spell.

[4] In this setting, adverse selection is assessed by linking the initial unemployment risk to the choice of the low-long option which also offers a higher total amount of benefits.

low UI benefits in the past, which they have not exhausted, and who are returning to unemployment. These workers are given a choice between either (i) starting with the remainder of the former compensation followed by their new one or (ii) directly using their new right (computed on the basis of only their last employment spell). Because the remainder of the former right is associated with a low level of benefits, and the new right with a higher one,[5] it implies that the first option offers the worker a low level of initial benefits, followed by a higher compensation, for a long total potential benefit duration (PBD). The second option provides the worker with higher benefits immediately for a shorter total PBD (see Figure **??** for an example). For the sake of simplicity, in the remainder of the paper, the first option will be referred to as the low-long-benefit schedule, and the second one as the high-short-benefit schedule.[6] The RDD is then comparing workers offered a choice between both schedules (below the cutoff) with workers offered only the long and low-benefit schedule, which is the default option.[7] Moral hazard, in the paper, therefore refers to the behavioral change in response to the increase in the average level of benefits entailed by the high-short-benefit schedule, although it implies a higher benefit at the start of the spell and a lower benefit later in the spell. From an insurance perspective, what makes the worker better off is *a priori* unclear. The two schedules are both horizontally- and vertically-differentiated: opting for the high-short-benefit schedule is equivalent to trading tomorrow's benefits off for today's benefits. At the same time, retaining the remainder of the former right reduces the risk of exhausting benefits and potentially provides a larger total amount of benefits. If the premium paid does not directly differ between both options,[8] choosing the low-long benefit implies an extension of the coverage at the expense of lower benefits in the short run. Having an option that unambiguously entails a higher total amount of benefits – but with a lower average one – makes such a setting particularly appropriate for measuring both adverse selection and moral hazard.

To understand better the determinants of the OR take-up, I first provide descriptive evidence on the characteristics of the unemployed choosing the high-short-benefit option. The likely existence of private information about employment prospects encourages to test for the presence of adverse selection and try to quantify its magnitude. Leveraging rich administrative data and machine learning methods, I predict the unemployment duration of each individual in my sample, capturing most of the information available to the worker when he makes his insurance choice. Performing the correlation test between

[5] This is by construction of the eligibility conditions.

[6] This is a simplification because the first option allows the jobseeker to receive higher benefits at the end of the UI spell, and because the second option entails a shorter benefit duration, but which can still be long in absolute terms.

[7] People above the eligibility threshold could also be eligible if they meet other conditions, that I am not able to observe. For that reason, the design is fuzzy. For simplicity, I will refer to workers below the threshold as having a choice and workers above the threshold as not having a choice.

[8] The contributions paid during employment spells are the same in both options.

insurance coverage and the predicted unemployment duration, as opposed to the realized one, presumably provides a better test of adverse selection, one that is not confounded with moral hazard. To quantify moral hazard, I compare people on either side of a benefit cutoff who differ in their ability to choose their schedule. This fuzzy RDD allows to study the response of unemployed people to a change in the level and potential duration of benefits. In theory, the effect of both parameters is expected to go in opposite directions, as most of the literature has found a positive relationship between the level of benefits and unemployment duration on the one hand, and PBD and unemployment duration on the other (see Schmieder and Von Wachter (2016) for a review). Therefore, the net joint impact is ambiguous. I measure this joint impact, both in financial and employment terms. Because I have no information on the job found after leaving unemployment, my main outcome variable is the duration of the unemployment spell. Finally, I use the estimate of moral hazard that I obtain from the RDD to decompose the observed difference in risk between adverse selection and moral hazard.

The analysis points to five main results. (i) I measure significant adverse selection, since individuals with the highest initial unemployment risk tend to select the longest coverage. (ii) Workers able to choose the high-benefit option have a much longer unemployment spell duration, indicating sizable moral hazard. (iii) However, the effect seems to fade out over time. Workers offered a choice do not differ in terms of total number of days on benefits on a longer time horizon. (iv) Findings suggest that additional number of days unemployed in the short run are not used to find better-quality jobs. Workers offered a choice work more frequently while being unemployed,[9] but this type of partial employment is often associated with part-time and temporary contracts.[10] (v) Finally, when individuals with the highest initial unemployment risk are offered a choice, they also suffer a larger negative impact. This suggests, together with a heterogeneity analysis, that the unemployed already experiencing difficulties in the labor market – the less skilled, less educated, and younger ones – are the most harmed by the OR. This raises some questions on the welfare cost of allowing the unemployed to choose their insurance coverage. The increase in unemployment duration for workers choosing the high-short-benefits is strikingly large (*i.e.* a 157-day response), but is in line with the literature showing that workers are generally more sensitive to the level than the duration of benefits (see Schmieder et al. (2016) for a review of elasticities) and that the moral hazard cost of UI is higher early in the spell (Kolsrud et al., 2018). Moreover, workers on which the response is estimated may exhibit a particularly large response since (i) they self-select into high-short benefits; (ii) but are still entitled to a long benefit duration; (iii) and are

[9]The French UI scheme allows jobseekers to keep receiving part of their UI benefits when they go back to work if their earnings and working time are below a certain threshold.

[10]However, a definitive answer to this question cannot be established. I do not observe the quality of the job found at the end of the unemployment spell, but only that of the jobs taken during the unemployment spell.

likely to be liquidity-constrained.

This paper relates to several strands of the literature. Although theoretical work (Akerlof, 1978; Rothschild and Stiglitz, 1976) has preceded empirical evidence, there is now a large body of papers testing for the presence of selection in insurance markets. Recent papers go beyond the correlation test (Chiappori and Salanie, 2000) that mixes adverse selection with moral hazard, as both predict that individuals with the highest coverage have higher claims. They use quasi-experimental variations in prices to reconstruct the demand and cost curves in various insurance markets, such as health insurance (Einav et al., 2010; Finkelstein et al., 2019). Finkelstein and Poterba (2002, 2004, 2014) have used the fact that moral hazard is unlikely in annuity markets, as it would entail a behavioral response on life duration to income in the form of an annuity. However, because such an assumption does not hold in the UI context, and because private schemes are virtually nonexistent, precluding the observation of insurance choices, empirical evidence on the presence of selection into UI is lacking.[11] There are two notable exceptions.[12] Hendren (2017) develops a methodology that does not rely on observing insurance choices. He uses elicited beliefs about job loss to demonstrate that no private UI scheme can be sustained, as a complement of the existing public UI scheme in the US, because workers' private information on their level of risk would entail too much selection. Landais et al. (2021) use the coexistence of minimum mandated coverage with private additional insurance that can be purchased on a voluntary basis in Sweden to confirm the presence of adverse selection, controlling for moral hazard, and explore the welfare consequences of a mandate. My setting differs from Hendren (2017) because I directly observe insurance choices, that I can relate to the initial unemployment risk of the workers. My setting also differs from Landais et al. (2021) because the two insurance contracts vary not only in the extent of coverage, but also in how it is allocated over time.[13] This horizontal differentiation partly affects the interpretation of adverse selection and moral hazard: I show that there is still adverse selection in a context where both options do not differ in terms of premium,[14] but where benefits of the option with the highest coverage are lower

[11] Reviewing the empirical literature on adverse selection in social insurances, Chetty and Finkelstein (2013) (p. 134) explain "In contrast to the study of selection in annuity and health insurance markets there is, to our knowledge, a dearth of work on adverse selection in several settings where there are important social insurance programs including disability insurance, unemployment insurance, and worker's compensation".

[12] Parsons et al. (2003) also investigate the extent of selection in the Danish context of voluntary UI. Using a multinomial logit model, they show that workers purchasing UI have a higher unemployment risk. However, their estimation is complicated by the fact that UI purchase depends on union membership and the eligibility to high social assistance benefits, that they try to take into account using the sample of switchers into and out of the UI scheme. They also control for moral hazard by measuring unemployment risk the first year of membership, during which workers are not eligible for benefits yet. However, the prospect of being able to receive UI benefits in the near future could still influence workers' behavior. The use of quasi experimental methods in my paper presumably provides a more robust measure of moral hazard.

[13] As there is a simultaneous change in replacement rate, benefit duration and profile.

[14] It means that the willingness to pay does not influence the insurance decision.

in the first period. It suggests that riskier individuals prioritize the length of the coverage or minimize the risk of having no benefit at all. The moral hazard response is complex and measured on multiple outcomes, both in the short and long run. Because the net effect of having the high-short benefit option is an increase in unemployment duration, it suggests that less risky individuals are more sensitive to the level than the duration of benefits in their search behavior.

This paper also contributes to the optimal unemployment insurance literature (Baily, 1978; Chetty, 2006), as it explores the behavioral response of workers to different parameters of UI, to measure its distortion cost on labor supply. A large literature has quantified the distorting effect of UI generosity on unemployment duration or reservation wage (Meyer, 1988; Feldstein and Poterba, 1984; Van Ours and Vodopivec, 2006; Lalive et al., 2006; Lalive, 2007; Lalive et al., 2015; Le Barbanchon et al., 2017). Some authors (Landais, 2015; Kolsrud et al., 2018) have built on the initial framework to study the optimal time profile of UI, drawing attention also to the duration of UI entitlements. Elasticities with respect to both the level and the duration of benefits are crucial to evaluating the cost and welfare effect of UI and to improve its design. This paper contributes to insights into this issue. Often examined separately, the analysis focuses here on the combined effect of both parameters on labor market outcomes. Establishing which of the two parameters prevails is important, especially for policymakers concerned with reforming UI by limiting behavioral responses. Moreover, this recent literature has also highlighted the importance of how UI benefits were allocated over time, as the moral hazard cost and insurance values are not likely to be constant throughout the unemployment spell. In the setting I study, the high-benefit option amounts to an increase in benefit early in the spell and a decrease to zero later in the spell.[15] Given the large moral hazard cost triggered by the high-benefit option, this paper suggests that a declining profile is not optimal. This finding is consistent with Kolsrud et al. (2018) who find that the moral hazard cost of UI benefits is larger early in the spell, whereas the insurance value is larger later in the spell, advocating for an inclining benefit profile. However, Lindner and Reizer (2016) take advantage of an experiment in Hungary frontloading UI benefits, keeping constant the total UI benefit amount, and find opposite effects. The discrepancy between these findings can be explained by the Hungarian reform being more salient, and people being more aware of future benefit cuts. My paper adds to this ongoing debate by demonstrating the positive impact on unemployment duration of receiving high benefits immediately as opposed to an inclining benefit path.[16]

Choosing the short and high-benefit option can be related, to a certain extent, to risk

[15] Unemployed workers choosing this option may qualify for social minima after the exhaustion point, but will not necessarily because they are means-tested based on household income. Their level is also generally lower than UI benefits.

[16] In the French context, the choice between both options was presented in rather complex terms, as a mix between the former and new entitlements, rather than as a choice between two benefit profiles.

aversion, present-biased preferences, or optimism. While I do not measure these parameters, several behavioral mechanisms can rationalize my results. For example, hyperbolic time preferences, with a low short-term discount parameter, could explain both the decision to opt for the short and high-benefit schedule and poor labor market outcomes. Indeed, it has been shown that unemployed people, especially at low levels of wages, exhibit hyperbolic time preferences (Paserman, 2008) and that impatience associated with hyperbolic time preferences has a negative impact on job search (DellaVigna and Paserman, 2005). Exploring another channel, Mueller et al. (2018) find that unemployed workers in general, and the long-term unemployed in particular, are overoptimistic about their employment prospects. Overoptimism can lead workers to choose the high-benefit schedule and to be overly selective and stay longer in unemployment. A further exploration of how OR choices could be used to estimate time discounting or risk aversion is left to future work.

The remainder of the paper proceeds as follows. Section 2 describes the institutional background as well as the data. Section 3 analyzes the determinants of the take-up and the extent of selection. Section 4 estimates the impact of the OR on future labor market outcomes and investigates the magnitude of moral hazard and adverse selection. Section 5 concludes.

2 Institutional setting and data

Institutional background – The option right was introduced as a corrective to the 2014 UI Agreement[17] because some unemployed people were adversely affected by it. Indeed, the 2014 Agreement introduced two principles: (i) the automatic resumption of the former right, meaning that a person taking a new job before exhausting his UI right automatically benefited from the remainder of his former right when again becoming unemployed; and (ii) the recharging of the right, meaning that, at the exhaustion point of his former right, the worker was allowed to extend the entitlements based on his last employment spells.[18] This mechanism was a way to use any employment spell, even short ones, to extend UI entitlements and maximize the coverage duration without any interruption to payments. However, this mechanism, meant to be more favorable, turned out to have unintended consequences for workers whose last employment spell was highly paid whereas the remainder of their former right was associated with a low daily benefit (DB). Making the resumption of the former right automatic at the end of the employment spell would cause a large drop in income. This regulation has then been rectified by an amendment to the 2014 Agreement, which gave the possibility to these types of workers

[17]Amendment №1 of March 25, 2015 modifying the general regulation appended to the Convention of May 14, 2014 on unemployment insurance.

[18]This holds once at least 150 hours of new employment spells that have never been used to compute any UI entitlements have accumulated.

to choose between benefiting from the remainder of their former right and then recharging with the new one or to jump directly to the new right associated with a higher average daily benefit. The first option implies longer benefits, starting with a low level and followed by higher ones. The second option leads to shorter benefits but starting directly with the higher ones.[19] This possibility to choose between both schedules, referred to as the option right, was introduced in April 2015. Because the long and low-benefit schedule is the default option, workers choosing the shorter unemployment compensation with on average higher benefits are referred to as takers.

The eligibility criteria to be granted this choice are the following: (i) having a residual from the former right; (ii) having worked at least 122 days or 610 hours since becoming eligible for the former right (corresponding to the minimum work history to open a UI right); (iii) having a DB associated with the former right no greater than 20€ *or* having a DB associated with the new right at least 30% greater than the former one. The last condition is the most crucial one and will allow the use of the thresholds as part of the identification strategy. If eligible workers choose to exercise their OR, they will directly benefit from their new right, and abandon the residual of their former right. An illustrative example can be found in Figure A1.

Data – I use administrative data from *Unédic*,[20] the organization in charge of UI in France. It gathers all the information needed to compute UI entitlements, on the characteristics of the unemployment spell, as well as sociodemographic variables. It allows me to follow the universe of registered unemployed workers with exhaustive information on their successive unemployment spells for the period of interest (October 2014–May 2017).[21] However, two important data limitations should be noted.

First, although numerous details are available on the characteristics of the unemployment spell, much less is known about what happens to unemployed people when they leave the unemployment roll. If they interrupt the unemployment spell for any reason – sickness, maternity leave, a new job – while remaining registered, we can still follow them in the database even if we do not necessarily know the reason for the interruption. The database shows whether the person uses his right and/or receives benefits. If a person fails to remain registered, that person just disappears from the database, without necessarily providing the motive for the exit. With this information at hand and knowing that we are interested in employment spells of at least 122 days between two unemployment spells, I define an unemployment spell as any period during which the person is registered

[19] For simplicity, in the remainder of the paper, the first option is referred to as the long and low-benefit schedule, and the second option is referred to as the short and high-benefit schedule.

[20] *Union nationale interprofessionnelle pour l'emploi dans l'industrie et le commerce*, the French national interprofessional union for employment in industry and trade.

[21] October 2014–May 2017 is the main period of interest used throughout the paper. However, to compute the unemployment probability of workers having started their UI spell during this period, I extend the observation window to February 2018.

as unemployed with interruptions of less than 4 months. Any interruption of at least 4 months, even if the person is still registered but does not use the right and is not paid, means the end of the unemployment spell, except if we know explicitly that it is for a reason other than a return to the labor market. Then, I define as the *paid unemployment spell duration* the addition of all the periods consumed and paid, and as the *full unemployment spell duration* the addition of all registered periods, paid and unpaid.[22]

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

I build a final sample of people who began an unemployment spell from October 1, 2014[23] who meet at least the first two eligibility criteria, that is, not having exhausted a former right and having worked at least 122 days between two unemployment spells. Among those, I can only observe whether the person is eligible but does not exercise his OR under the 20€ condition, and whether the OR is exercised under both conditions. Table B1 details the sample composition, with some proxies for the take-up rate, as the true rate could only be determined if we had the exact number of eligible people. I end up with a sample of 2,209,471 individuals, of whom more than 200,000 are eligible under the 20€ condition. Restricting ourselves to the 20€ condition, the take-up rate is equal to 34%, although it may not perfectly reflect the overall take-up rate in the population. This rate may seem small, given the very low daily benefits associated with the former right for this population (under 20€); however, there are many reasons for not exercising the OR that can stem from both the unemployed person and their caseworker. Indeed, survey data from Unédic reveals that caseworkers were sometimes reluctant to advertise this choice and to argue in favor of opting for the new right, because they felt that it was risky for a population of workers generally experiencing difficulties in the labor market.

[22] By definition of unemployment spells, the unpaid periods within the spell are necessarily less than 4 months. These periods are accounted for in the full unemployment spell duration computation only if the person maintains registration as unemployed and thus remains in the database.

[23] The amendment was passed on March 29, 2015 and applied from April 1, 2015 retrospectively on unemployment spells starting from October 1, 2014 onward. It means that a person who automatically resumed their former right between October 1, 2014 and April 1, 2015 could decide from the latter date to exercise the OR and to switch directly to that new right.

In addition, unemployed people are not necessarily aware of the existence of such a possibility, and the default option is not to exercise the OR. By law, applying for the OR is at the initiative of the jobseeker. That is why we observe an ascending trend in the number of takers over the months, with a seasonality component, as displayed in Figure E1.

Description of the sample – Because of data limitations, this study focuses on a specific unemployed population with very low daily benefit (lower than or equal to 20€). Yet, if specific, this population is nonnegligible, as it accounts for 12% of the flows to compensated unemployment as part of the main UI benefit.[24] This population is also particularly hard hit in the labor market because their benefits and thus their previous earnings were very low, which is often associated with low qualifications and a low probability of reemployment. Both their weight for all the people receiving UI benefits as well as their situation in the labor market warrant our attention to their labor market outcomes. Moreover, while the results obtained cannot necessarily be extrapolated to the whole unemployed population, they are of particular policy relevance if we believe the State should provide specific support to populations that are more distant from the labor market.

Table 1 describes the characteristics of the eligible population under the 20€ cutoff compared to the population of noneligible nontakers.[25] Consistent with their low benefit level, eligible workers are, on average, younger, more frequently female, less skilled and educated. They also have a lower tenure and working hours, signaling their lower attachment to the labor market. Their initial potential benefit duration is also lower, indicating less secure entitlements, in line with their lower tenure. Overall, eligible people under the 20€ condition are a more fragile population, namely more in difficulty in the labor market compared with similar noneligible workers.

3 Determinants of the take-up

3.1 Descriptive evidence

The profile of takers – An exploration of the observable characteristics of eligible workers choosing the short and high-benefit option provides useful insights on the profile of the takers. Although not allowing us to fully understand their preferences, it can proxy for their prospects in the labor market, the way they anticipate them, and their impatience and risk aversion. In the following paragraphs, I describe: (i) the population of takers under the 20€ cutoff; (ii) the population of takers fulfilling the 20€ but not

[24] As measured between January 2014 and March 2015.

[25] This noneligible nontaker population refers to the population of people similar to eligible workers except for the third eligibility condition, that is, having a former daily benefit lower than 20€. However, it may include workers eligible as part of the ratio criterion that I am not able to identify.

the 30% condition, on which I will measure the moral hazard response (later referred as the "complier" population); (iii) how they compare to eligible nontakers under the 20€ cutoff, to better understand the determinants of the take-up.

The profile of the taker is consistent across observable variables (Table 1, column (2)). When compared with eligible nontakers, takers are, on average, younger and more frequently male, characteristics generally associated with riskier behaviors or lower loss aversion (Falk et al., 2015; Albert and Duffy, 2012; Gächter et al., 2007; Holt and Laury, 2002; Jianakoplos and Bernasek, 2006). Takers are also more skilled and more educated, which could explain why they may anticipate a quick return to the labor market and may be less reluctant to give up additional days of entitlement. They work more hours, indicating a stronger attachment to the labor market and more stable jobs, which is also in line with the higher proportion of men. The lower tenure can be explained by the fact that takers are younger and so have less experience in the labor market. Concerning the characteristics of the UI right, the analysis is less straightforward. The initial PBD associated with the former right is lower for takers, which can explain why they are less reluctant to give up their former right. However, the remaining benefit duration at the moment they have to decide whether to exercise the OR or not is higher for takers, which may seem surprising. Both indicators taken together mean that they have consumed less of their former right and then that they have spent less time unemployed as part of their former right, which may explain why they anticipate a quick return to the labor market and choose to exercise their OR despite a high remaining PBD. This is in line with the fact that they have experienced fewer unemployment spells over the whole period (October 2014–May 2017).[26] Column (1) of Table 1 describes the compliers' characteristics, that is, the population of takers meeting the 20€ but not the 30% condition. Compared with eligible nontakers, compliers are, on average, older, more frequently female and more skilled. They also have a lower tenure, but a higher PBD from the former right, which can explain their takeup decision. Overall, takers under 20€ are in a better situation in the labor market, in terms of education and working hours, compared with compliers and even eligible nontakers. Compliers are nearly all women, older, and skilled employees. The fact that their new right benefit is associated with a gain lower than 30% means that they keep with low benefits and low earnings. This can be associated with a profile of women locked into low-paid part-time jobs,[27] whereas other takers usually correspond with situations of workers at the beginning of their careers, and are thus more likely to have low-paid jobs, or to be on an increasing wage profile (for those meeting the 30%

[26] The profile of takers may be influenced by their time or risk preferences, but also by the parameters of their insurance options. Because I do not observe the full menu of insurance contracts for all eligibles, I cannot exclude that differences in takeup are partly explained by differences in treatment intensity.

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

criterion).

If eligible people under the 20€ condition are a more fragile population, among them, unemployed people who decide to exercise their OR appear to be less precarious, and to perform better in the labor market. Their higher education and qualifications can explain why they may be more confident in their labor market prospects and why they may favor the UI's generosity over the duration of the coverage.

The takeup decision – Table F1 runs a multivariate analysis of the probability of being a complier or taker on the sample of eligibles, to examine the marginal effect of each variable, as they are potentially correlated. Only looking at predetermined characteristics, what seems the most important influence for probability of taking up the OR for eligibles under the 20€ criterion is age, being female – both negatively correlated – and the level of qualifications, all else being equal.[28] Interestingly, the level of education has a negative impact, whereas descriptive evidence (Table 1) shows that takers are, on average, more educated than eligible nontakers. The effect of education reverses when right's characteristics are added, while the coefficients on other predetermined characteristics slightly decrease but stay of the same sign. The effect of age, gender and skills seems to be partly captured by the positive and significant impact of working hours. The multivariate analysis confirms previous descriptive evidence, where characteristics associated with a higher taste for risk and better employment prospects influence positively the decision of taking up the OR.

The picture looks different for compliers. Consistent with the higher proportion of older and female workers in this population, age and being a woman have both a positive marginal impact, although the gender effect disappears when right's characteristics are added. The level of skills play a positive role in both regressions, and the effect of right's characteristics goes in the same direction as in the case of takers, although the magnitude of the coefficients is much smaller when looking at the probability of being a complier. The decision to choose the short and high-benefit schedule among compliers does not seem to be necessarily related to better employment prospects, but is rather consistent with a profile of workers durably in part-time jobs with a medium number of hours worked and fewer variations in their employment spell characteristics, and with very low levels of benefits that make them presumably more sensitive to this parameter.

Characteristics of insurance options of takers – To gain a more complete picture of the determinants of their choice, we can also look at the exact terms of the trade-off faced by takers (Table B2). Takers are characterized by a new DB that is, on average, more than twice the former DB. This is not surprising because their choice to exercise

[28]The level of qualifications has a reversed scale, meaning that it has a positive impact on the probability of being a taker.

the OR must be motivated by a high financial gain to compensate for the loss in terms of PBD. This ratio is even higher at close to 3 for takers having a former DB lower than 20€, which is in line with the fact that, as their former DB is very low, the new one is likely to be much higher. However, compliers, who, by definition, have a ratio between new and former benefit lower than 1.3, gain only 18% in terms of level of benefits, on average.[29] Row 3 of Table B2 indicates that the new PBD the taker is entitled to is 1.35 longer than the PBD he gives up by exercising the OR. This ratio is lower in the case of those taking under the 20€ criterion. It is reasonable to believe that, at these very low levels of DB, unemployed people are willing to give up a remaining PBD that, in proportion, represents more of their new PBD if it allows them to earn higher benefits. In other words, in the amount–duration trade-off, they are likely to put more weight on the amount of their DB. For both types of takers, the total initial PBD associated with both rights is almost the same, which can also motivate their choice. Indeed, they are offered, as part of their new right, a PBD that is equal to what they were entitled to at the beginning of their former right. By definition, if they are eligible for the OR, they are in a situation where they did not exhaust their former right. Then, based on their very last experience, it makes sense for them to anticipate that they will not entirely consume their new right if they take it, and then, that they do not need a longer coverage, and that they should exercise their OR.

However, the last row indicates that by doing so, takers choose to receive benefits for a period of time that is slightly more than half what they could have if they had not exercised the OR. In the case of compliers, they lose less in terms of duration, which is partly explained by a long new right, longer than their former one, and much longer than the remaining PBD (row 3). This is also consistent with their choice. Because taking up the OR for them is only associated with a small increase in the level of benefits, they might be more willing to take it only if they are assured of a long coverage despite the withdrawal of the residual of their former right.

To put the numbers into perspective, Figures F1, F2, and F3 show the average daily benefit and PBD for takers and nontakers, as a function of the previous level of daily benefit. UI benefits are higher for takers, as a direct consequence of the OR. The slope increases slightly after the 20€ threshold, as takers above this threshold necessarily need to fulfil the 30% criterion. For takers, the PBD would be almost twice as high as their actual PBD, had they not exercised the OR, with a linear increasing pattern along the daily benefit distribution. Comparing the PBD of takers and nontakers is not straightforward, as we do not have information on the potential new right for eligible nontakers. Figure F3 only compares the actual PBD of takers with the remainder of the former right for nontakers, which accounts for only part of the coverage duration to which they are entitled. The PBD of takers increases continuously along the previous daily benefit distribution,

[29]It should be noted that this ratio is necessarily higher than one.

which indicates that there is a positive relationship between the level and the duration of benefits. This is not surprising because workers with higher benefits have also higher wages and therefore a stronger attachment to the labor market, which is associated with more tenure and thereby a higher PBD. It could also be the case that as the previous daily benefit increases, liquidity constraints pushing in favor of the OR are alleviated. Therefore, a higher PBD duration is needed to justify the choice to exercise the OR. Conversely, the residual of the former right decreases slightly in the level of the previous benefit. It can be related to the positive relationship between unemployment duration and the level of UI benefits found in most studies. The higher the level of benefits, the longer the unemployment spell, the lower the residual of the right when the person finds a job.

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

3.2 Selection into UI

The previous subsection has demonstrated that takers exhibit specific characteristics. I now exploit the information on a rich set of covariates and the choice feature of the OR to try to measure the extent of adverse selection into UI. Indeed, the main rationale for the implementation of a UI mandated at the national level comes from the Rothschild–Stiglitz demonstration (Rothschild and Stiglitz, 1976), namely that, because of heterogeneity in risk types and asymmetry of information, there is no equilibrium supporting the provision of insurance. Empirically, most papers have taken this result at face value without questioning the actual presence of adverse selection, and instead focus on moral hazard. In this subsection, I use the standard positive correlation test (Chiappori and Salanie, 2000) to check for the presence of selection. Because of the likely existence of moral hazard, I cannot use observed unemployment duration as a measure of the expected costs to the insured. Unemployment duration is therefore predicted using a sample of similar jobseekers during the two years preceding the implementation of the OR. I use a large set of covariates associated with the worker and his last employer to capture as accurately as possible all the information that is available to the worker when he has to decide on his benefit schedule. Different specifications are tested, such as a simple OLS, a Poisson model to account for the fact that the dependent variable is positive, or models for zero-inflated count data. Finally, I implement a machine learning algorithm to avoid making any assumption on the functional form of the relationship between unemployment duration and individual and job characteristics. The choice of the model is based on several goodness-of-fit indicators, such as the root mean squared error. A detailed discussion of the model is provided in Appendix G.

Table 2 reports the predicted unemployment duration on the sample of interest, namely takers and eligible nontakers under the 20€ cutoff. Controls are workers on which the prediction model has been trained. The table shows that the predicted unemployment duration is higher for eligible nontakers than for takers below the 20€ threshold. The 33-day difference, representing a 13% increase relative to the predicted unemployment duration of takers, is indicative of significant adverse selection. Jobseekers with higher predicted unemployment duration are more likely to choose the longest UI coverage. The same prediction exercise using a flexible OLS yields lower predicted duration in absolute terms, but a similar 10% difference between takers and nontakers (Table G2).

While there is a clear and robust difference in predicted unemployment duration between takers and nontakers, pointing to significant adverse selection, potential alternative interpretations should be mentioned. First, given that the sample under study is made of workers with low labor market attachment, it is not unlikely that they are not all informed about the existence of the OR or fully understand its consequences. This may be particularly true for low-educated workers for example, who are also likely to exhibit a higher predicted unemployment risk. If I cannot fully rule out the information channel, I can check whether the difference in predicted unemployment duration still holds after the OR had been in place for some time. I measure that the difference between takers and nontakers goes in the same direction and is even larger if we focus on workers starting their spell at least one year after the implementation of the OR (Table G3 of Appendix G). It is reasonable to think that after one year, workers had time to learn about the OR scheme, especially the ones with the highest unemployment risk who experience frequent unemployment spells and are therefore more familiar with the UI legislation. Second, as previously mentioned, there exists anecdotal evidence that caseworkers may influence the decision of jobseekers, which would therefore change the interpretation of the difference in predicted unemployment risk between takers and nontakers. While there is no register data or survey evidence that would allow to quantify and potentially rule out this possibility, I can, however, provide some details on the institutional framework. First, there is no particular incentive for the caseworker to make the jobseeker choose one option or the other. The administrative cost does not differ between both options. The caseworker may advice some jobseekers that they perceive as riskier to opt for the longest option as it is considered safer. In that sense, the caseworker would use his own private information to make an insurance choice for the jobseeker. The adverse selection from jobseekers will therefore be reinforced by the adverse selection coming from caseworkers. Although caseworkers may be better informed about the state of the labor market in a particular region or occupation, this information can be assumed public and available to the jobseeker (possibly at some cost). However, it is reasonable to think that the jobseeker has private information on his own level of risk that he will not necessarily reveal to the caseworker.[30] Second, according to the law, the initiative of taking up the option right must come from the jobseeker himself. These two facts suggest that most of the adverse selection comes from the jobseeker.

The next step of the analysis focuses on the measure of moral hazard using a RDD.

4 The moral hazard cost of UI benefits

4.1 Empirical methodology

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

Empirical methodology – The estimated equation is the following:

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

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

[30] Indeed, caseworkers do not have access to the full employment history of the worker for example, and a jobseeker may be assigned several caseworkers during his unemployment spell.

of exercising the OR does not jump from 1 to 0 when crossing the 20€ threshold, for two reasons: (i) all eligible people below 20€ will not exercise it; and (ii) some people above 20€ are eligible under the 30% ratio condition and will choose to exercise the OR. Both imperfect take-up and the existence of other eligibility criteria take us away from the standard "sharp" RD design. Yet, the identification remains possible as long as we have a jump in the probability of treatment at the cutoff, although lower than one:

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

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

The "fuzzy" RDD exploits the discontinuity in the probability of treatment at the threshold. The treatment effect can then be recovered by dividing the jump in the relationship between the outcome and the OR treatment by the jump in the relationship between the OR treatment and the running variable – previous daily benefit – at the cutoff. The estimand can be interpreted as a weighted local average treatment effect, as it is computed on the population of compliers, where the weight represents the ex ante probability of being around the threshold.

The identification rests upon two assumptions: (i) monotonicity, that is, the fact that crossing the 20€ cutoff does not cause, at the same time, some units to be treated and others to be excluded from treatment; and (ii) excludability, that is, the fact that crossing the 20€ cutoff does not have an impact on Y other than through the OR. If the first assumption is verified by definition of the design of the OR eligibility rules,[31] the second assumption cannot be ultimately tested; however, some elements make it more credible and these will be developed further in the following paragraphs. Theoretically, if the window considered is not too large, there is no reason for it being located right below or right above the 20€ cutoff to affect labor market outcomes other than through the eligibility for the OR. If the previous daily benefit level is linked to past employment history and then relates to future labor market performance, this effect has no reason not to be continuous at the 20€ threshold. To make this excludability assumption more plausible, three types of tests are performed: (i) a check on the continuity of the running variable density at the cutoff to eliminate any manipulation suspicion; (ii) a check on the continuity of observed baseline covariates at the cutoff to confirm the nonselection and comparability of populations at each side of the cutoff; and (iii) a check of the existence of a jump in the probability of being treated at the cutoff, a necessary first stage to detect any effect.

[31]Crossing the 20€ cutoff leads to some units no longer being eligible, and to others staying eligible if they also meet the 30% criterion.

Validity conditions of the RDD – One key assumption to check for the RDD to be valid is that there is no manipulation at the threshold, or strategic sorting of workers at either side of the threshold. Theoretically, there are several reasons why unemployed people would not have an interest in reporting lower earnings to have a DB just below the 20€ cutoff: (i) they would receive very low benefits, lower than they were entitled to if they had reported their true earnings; (ii) the earnings value used to compute DB is reported on a certificate delivered by the employer to open UI rights, making falsification very unlikely; and (iii) manipulating their earnings value in anticipation of a future OR would require very accurate foresight, as well as a precise knowledge of UI legislation.[32]

Although the manipulation scenario seems implausible, I still perform a McCrary test (McCrary, 2008) to check that the density of the former DB distribution is smooth at the 20€ cutoff (Figure C1). Some regularities in the level of earnings or in the UI parameters tend to create small spikes at different points of the distributions, without threatening the validity of the RDD, as these spikes are not in the neighborhood of the cutoff. For example, we observe a big jump in density around 32€, as this corresponds to the level of DB for a person who has worked full-time at the minimum wage. As there is no precise sorting at the threshold, RDD is considered "as good as randomization" in the neighborhood of the threshold.

If I chose to focus on one eligibility criterion for data limitation issues, it would still be possible to observe which eligibility criterion was binding for eligible workers who chose to exercise the OR. The distribution across eligibility conditions for takers shows that the most decisive criterion is having a new daily benefit greater than the former one by at least 30%. Indeed, 97.5% of takers having a previous DB lower than 20€ also fulfill the ratio criterion, and 92.2% of all takers fulfill the ratio criterion (Table E1). This distribution emphasizes the fact that having information on both criteria would have helped to capture the OR impact in a more exhaustive way and that the population of compliers is very specific, being made up of people eligible under the 20€ criterion but not under the 30% criterion. Indeed, the share of people eligible based on the 30% criterion have no reason not to be continuous at the 20€ threshold,[33] meaning that the compliers have a financial gain when exercising the OR necessarily lower than 30%, translating into at most 6€ daily. This implies that compliers are willing to give up on a significant additional coverage duration (336 days on average) for a limited increase in income, demonstrating either particular preferences, very tight financial constraints, or

[32] It would require unemployed people to be aware of the existence of the OR, to anticipate that they will find a job and lose it again, and then that they might be eligible, and to know very precisely the rules to compute DB from earnings, with some parameters being updated every semester.

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

very optimistic anticipation about their return to the labor market.

To conclude fully that the difference in outcomes we observe between populations at each side of the threshold can be imputed to differences in OR take-up, we need to rule out the influence of other variables at the threshold. Appendix H displays different tests of the continuity of covariates at the threshold. Figures H1–H7 do not depict any clear jump in the distribution of covariates at the threshold, and the whole distribution pattern shows numerous bumps and lumps at other values of the covariates. In addition, Figure C2 (Appendix C) provides the corresponding RD estimates, where each covariate is used as the dependent variable in the RD regression, and previous daily benefit as the running variable. None of the coefficient is significantly different from zero, except for the tenure at past job. Although strategic sorting of people on either side of the threshold is very unlikely,[34] I also test the continuity of the predicted unemployment duration, which can be considered an index of various characteristics. Unemployment duration is predicted using a sample of similar workers unemployed during the two years preceding the introduction of the option right, and performing an out-of-sample prediction on the sample under study. Included variables are age, gender, skill level (5 categories), education level (10 categories), sought occupation (14 categories), part-time coefficient, region of residence (31 categories), sector of activity (11 categories), month of contract termination, number of children, family situation (5 categories), previous occupation (81 categories), firm and plant size. A flexible linear model is used, yielding a R^2 equal to 7% and a RMSE equal to 281. The rather low R^2 can be explained by the fact that I do not include any variable related to the UI entitlements, because these variables may interfere with the effect of the option right itself. Figure H8 (Appendix H) shows no discontinuity in the distribution of predicted unemployment duration at the eligibility threshold. Further, Table C1 reports the RD estimates without controls, controlling for predetermined variables (age, gender, level of education) and controlling for the value of predicted unemployment. Although coefficients move a bit, they are qualitatively similar and do not alter the conclusion that the option right significantly increases paid unemployment duration.

First-stage estimation – Empirically, I estimate Equation 1 nonparametrically using a restricted window around the threshold. To demonstrate the robustness of the effect, results will be shown for a range of different polynomial orders and bandwidth sizes.[35] Equation 1 shows the reduced form of two equations capturing the first-stage

[34] Sorting would imply that workers anticipate, when opening their UI right, that they might exercise the OR in the future if they work in a better paid job (in some cases even before the OR has been implemented), and, in order to be eligible, they should be willing to falsify their work certificate to receive lower benefits immediately

[35] Table I1 and Figure D1 of Appendices D and I reproduce the main results making the size of the bandwidth vary (choosing, in particular, between 0.5 and 3 times the optimal bandwidth value).

relationship between the previous daily benefit level and the OR take-up (Eq. 2) and the second-stage relationship between the OR take-up and labor market outcomes (Eq. 3).

$$OR = \alpha_f + \tau_f \mathbb{1}_{DB_p \leq 20} + \beta_{f_f} f_f(DB_p - c) + \beta_{g_f} g_f((DB_p - c)\mathbb{1}_{DB_p \leq 20}) + \mu_f \tag{2}$$

$$Y = \alpha_s + \tau_s OR + \beta_{f_s} f_s(DB_p - c) + \beta_{g_s} g_s((DB_p - c)OR) + \mu_s \tag{3}$$

The estimate τ_s corresponds to a local average treatment effect. Table 3 shows that being located at the right-hand side of the cutoff makes the probability of taking up the OR decrease significantly, by 1–6 percentage points, depending on the specification. Although the effect is not very strong, the estimate is highly significant, and the jump in the probability is clear, as depicted in Figure 1. The weak first-stage regression could raise some precision issues. However, as the sample size is large, we can be confident in having precisely estimated treatment effects.[36] Graphically (Figure 1), we observe a drop of about 4 percentage points, from an initial probability of around 19%. It means that the drop translates into a 21% increase in the probability of taking up the OR when crossing the 20€ cutoff.[37]

4.2 Results

4.2.1 Labor market impact of the option right

Option right impact on unemployment spell duration – The literature shows that the elasticities of unemployment duration with respect to the level of unemployment benefits as well as the PBD were positive (see Schmieder and Von Wachter (2016) for a review). Then, in this setting, we expect the effect of the OR on the unemployment spell duration to go in two opposite directions; thus, the effect of receiving higher benefits for a shorter potential duration is *a priori* unclear.

With the data at hand, I define two measures of the unemployment spell duration. The first measure – the paid unemployment spell duration – corresponds to the addition of all subperiods during which benefits were paid, within the same spell.[38] The full

[36] A parametric fist-stage regression always leads to F-statistics above 57, and passes the Anderson et al. (1949) test.

[37] The decrease around 18€ may be explained by the presence of numerous subsidized jobs paying the minimum wage for a 20-hour weekly working time, which is the minimum working time for these types of contracts, translating into a daily benefit of around 18€. These types of jobs are generally offered to long-term unemployed people who have been away from the labor market for a long time and who have experienced great difficulties in finding a job. It means that, if they had been eligible for the OR, caseworkers would be very unlikely to advise them to take it and they themselves may be reluctant to give up additional compensation days, given their poor labor market prospects.

[38] An unemployment spell was defined in Section 2 as any period of registered unemployment with

unemployment spell duration corresponds to all registered subperiods within the same spell, including the unpaid ones, which, by definition of the spell, last less than 4 months. It allows to include days when jobseekers have potentially exhausted their benefits but stay registered. However, restricting the analysis to the unemployment spell duration may sometimes not be relevant; if the person keeps going back and forth, in and out of the labor market, the unemployment spell may be short without necessarily corresponding to a stable exit from the labor market.[39] That is why this measure will be presented with other complementary outcome variables intended to capture a medium- to long-term effect. Tables 4 and 5 show that taking up the OR has a strong and significant effect on unemployment spell duration – both paid and unpaid. If we focus on the quadratic specification without any controls of Table 4, the OR leads to an increase in paid unemployment duration of about 157 days. The effect is markedly large, and the OR seems to have a very detrimental impact on the employment outcomes of a population already in a precarious situation. In particular, if we consider that the average duration of a spell at the cutoff is around 92 days, the effect is equivalent to multiplying the spell duration by 2.7.[40] At first sight, evidence would lead to the conclusion that letting the unemployed choose the terms and conditions of their compensation is a very inefficient way of ensuring satisfactory coverage and a quick return to the labor market. The strong and positive effect on unemployment duration is confirmed by Figures 2 and 3. The addition of covariates does not change the order of magnitude of the results for any specification, which is reassuring on the validity of the RDD. The results are also very consistent across the local linear and quadratic polynomials. Tables D1 and D2 of Appendix D report the reduced-form coefficients, which range from 7 to 8 days for the paid unemployment duration, and 11 to 16 days for the full unemployment duration. For both outcome variables, it represents about a 10% increase relative to the control outcome mean, and coefficients are remarkably stable across specifications.

These different findings indicate that benefiting from a shorter potential duration with a higher average level of benefits and a declining profile makes the duration of the unemployment spell increase. In this specific context, the elasticity of unemployment duration to the level of unemployment benefits outweighs the elasticity of unemployment duration with respect to the PBD. This result is in line with the literature, as elasticities of nonemployment duration or benefit duration with respect to the benefit level are usually higher than the same elasticities measured with respect to PBD (see Schmieder

interruptions shorter than 4 months. Then, the paid unemployment spell duration refers to the addition of registered and paid subperiods without counting the time elapsed during the interruptions.

[39]Nevertheless, as unemployment spells are defined so that they are separated by at least 4 months of interruption, we can be fairly confident that the end of an unemployment spell corresponds to a job of at least several months.

[40]It should be noted that the control outcome mean refers to the mean paid unemployment spell duration for values of the DB right above the cutoff, between 20€ and 30€, but the counterfactual average duration of the spell for compliers in the absence of the OR may differ.

and Von Wachter (2016) for a recent review). The average gain in replacement rate[41] for compliers is equal to 10.2 percentage points, which is equivalent to an 18.5% increase. As indicated by row 4 of Table B2, their average loss in PBD amounts to 35.4%. If we take, based on average values of paid unemployment duration elasticities from a panel of recent studies in Europe (Schmieder and Von Wachter, 2016), an elasticity of unemployment duration with respect to the replacement rate of 1 (noted $\epsilon_{B,RR}$) and an elasticity of unemployment duration with respect to PBD of 0.4 (noted $\epsilon_{B,PBD}$), we can carry out a simple computation exercise:

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

with ΔB being the unemployment duration response, $\overline{Duration}$ is the average spell duration of unemployed people having a previous daily benefit between 20€ and 22€ , $\frac{\Delta RR}{RR}$ is the change in replacement rate, and $\frac{\Delta PBD}{PBD}$ is the change in PBD. The net effect on paid unemployment duration is positive, as confirmed by my results. In terms of magnitude, the predicted increase is of 4.34%, which translates into 4.15 days on average at the threshold. This figure is much lower than my local average treatment effect (LATE) estimate of 157 days for specification (2), emphasizing the fact that the population of compliers is likely to be specific in terms of elasticity and time preferences. It can also be related to the declining benefit profile faced by takers in my setting. Kolsrud et al. (2018) have shown that the moral hazard cost of more generous benefits was higher earlier in the spell, which could contribute to the very large response I find.

One should keep in mind that the RD estimate is similar to a LATE, weighted by the preassignment probability of being located just below the threshold. Then, it is valid for this specific threshold, and informs about the behavior of this peculiar population of compliers around the 20€ threshold. The fact that these workers are more sensitive to the level of benefits rather than to the PBD can be explained by their profile: (i) they have such a low benefit that they may face sizable liquidity constraints, so any increase in their income may have a substantial effect; (ii) even when choosing the shortest option, they are still entitled to a long coverage in absolute terms (560 days on average for compliers and 456 for takers below the 20€ threshold); and (iii) they are used to entering and leaving the labor market, alternating very short employment and unemployment spells. Then, they are used to not exhausting their right and using their frequent employment spells to extend it, which can explain why they put less weight on the PBD when opti-

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

mizing their search behavior. (iv) Finally, the population on which the effect is measured has self-selected into the treatment, implying that they are likely to react more than in other settings found in the literature where no choice is involved.

Longer-term impact on the professional path – Looking only at this first evidence on unemployment duration would lead one to conclude that the OR has a negative impact on employment. However, the ultimate impact on a worker's welfare depends on whether this increase in the duration of the subsequent unemployment is driven by the fact that the person can afford to take more time to find a job, and that this job will be more stable and of better quality. Even though job quality cannot be measured directly with the available data, because I have no information on the job found when the unemployed person leaves the rolls, we can still try to capture a longer-run effect, by measuring the total number of days spent unemployed after the exercise of the OR. If the OR was associated with an increase in job quality, we would observe that, despite a longer immediate unemployment spell, people exercising the OR would be less unemployed over the whole subsequent period. I now define two new outcome variables: the total number of days unemployed over the subsequent period, and the total number of days on UI benefits over the subsequent period. Figures I1 and I2 (Appendix I) exhibit a drop in the total number of days registered as unemployed over the subsequent period, but not in the total number of days on UI benefits over the subsequent period. Consequently, Tables 6 and 7 show that the effect on the total number of days registered as unemployed is significant for all specifications, whereas it is never significant for the total number of days on benefits. The total number of days unemployed is measured on time periods of different lengths depending on the starting date of the spell. This should not be an issue to the extent that the starting date of the spell is uniformly distributed across treated and controls. Figures E2 and E3 are reassuring on this issue.[42] However, I provide the results on the total number of days unemployed over a two and three-year period (Table I6 of Appendix I). Results are less precise due to the small sample size: they point to a positive to nonsignificant effect of the OR on the longer-run number of days unemployed. This is confirmed by the graphical evidence displayed on Figures I4 and I3. If anything, the effect would be positive on the total number of days registered as unemployed, consistent with Tables 6 and 7.

Several interpretations of this difference between the effect on the total number of days on benefits and the total number of days registered as unemployed can be put forward. Being registered as unemployed without receiving benefits generally corresponds to either

[42] Figure E2 shows that the distribution of the starting date of the spell is rather uniform across groups of workers with the former benefit above and below the 20€ cutoff. Figure E3 further shows that the probability to fall under the 20€threshold is rather stable over time. The difference in entry date, if any, would rather lead to underestimating the results in terms of unemployment duration as those earning more than 20€ enter more frequently at the beginning of the period.

(i) periods during which the unemployed receives assistance benefits, or (ii) periods during which the person works while registered as unemployed, because he is still looking for another job or because his contract is short. I examine the plausibility of each motive in the following paragraphs. Distinguishing between both explanations is a key issue. The first scenario would mean that the higher number of days registered as unemployed but not paid corresponds to unemployed people at the exhaustion point of their right, staying registered to keep benefiting from the support of the caseworker. In particular, to receive assistance benefits or the minimum income, it is required to be registered as unemployed. This would be compatible with the fact that takers have mechanically shorter PBD than nontakers. It would mean that evidence in Tables 6 and 7 and Figure I1 supports the hypothesis that the OR slows down the return to work, even in the long run, and forces takers to switch to assistance benefits as they are no longer entitled to UI benefits. If the second scenario prevails, it implies that, in the long run, takers work slightly more, although it can be under temporary and part-time contracts. My data do not contain, at the moment, information on assistance benefits,[43] but the data can still include jobseekers staying registered without receiving any income from UI, after they exhausted their benefits for instance. Table 8 indicates that compliers are more likely to reach the exhaustion point of their benefits by 13–17 percentage points depending on the specification, from a baseline of around 4% (Figure 4). Then, the difference we observe in terms of number of days registered could be partly explained by the fact that takers run out of benefits more frequently.[44]

The second motive may more plausibly explain the main difference in the number of days registered but not paid. Anecdotal evidence has revealed that caseworkers were advising unemployed people who found a job under a fixed-term contract to maintain registration at the job center to avoid starting the whole procedure again at the end of their contract. It is also particularly recommended when the job is temporary, part-time, or corresponds to qualifications that do not perfectly match those of the worker, so that the person can keep looking for a better job and benefiting from support and guidance from the caseworkers. Therefore, if we believe that the higher number of days registered but not paid corresponds to trial periods at the beginning of an open-ended contract, when the worker is not sure yet of being permanently hired, we may consider that the OR acts as a stepping-stone to a more stable job in the long run. Another reason people would stay registered as unemployed while working is that they earn a sufficiently low wage to be entitled to receive complementary benefits from UI. The benefits received are lower than a full month of complete compensation, and the person would then appear as being on benefits for some days in the month and registered but not receiving benefits

[43] Assistance benefits such as the minimum income are managed by another administration.

[44] Indeed, if I do not observe jobseekers receiving the minimum income for example,

for the rest of the month.[45] In other words, these periods during which the person is registered without receiving benefits generally correspond to employment spells under unstable, temporary, and/or part-time contracts. For example, if, in a given month, the person is employed under a part-time contract and is entitled to receive one-third of the monthly benefits he would receive with no job at all, he will appear as registered on benefits for 10 days in the month, and registered without benefits for the other 20 days. However, if the person has no job at all, he will appear as registered on benefits for all 30 days. This scenario is then compatible with takers having a similar total number of days on UI benefits with a higher total number of days registered without benefits at the same time. Overall, the evidence suggests that in the medium to long run, the OR does not impact negatively on the professional path in terms of unemployment probability, although it may encourage temporary, unstable, and part-time contracts.

The next subsection investigates in greater detail whether the difference in terms of days registered as unemployed can be explained by takers having more frequent small employment spells while maintaining registration.

Impact on partial employment – If an unemployed person works while registered as unemployed, I am able to track the employment spells and to have information on the number of hours worked and total earnings, no matter whether still earning benefits during this period. These types of employment spells typically include short-term and part-time contracts rather than a stable job.[46] In the remainder of the paper, I will refer to these periods of employment while registered as unemployed as *partial employment*. I observe that people exercising the OR have a probability of experiencing partial employment during the unemployment spell that is much higher than the control group (Table 9).[47] This finding is consistent with the fact that the impact I measure on unemployment duration is higher in terms of number of days registered than in the number of days receiving benefits, both in the short and long term. The fact that compliers earn more income from labor during the spell (Table I3 of Appendix I), conditional on working at least 1 hour during the spell, is presumably explained by more hours worked (Table I4 of Appendix I). I also measure the unconditional impact of the OR on hourly wage earned while registered as unemployed. This impact is unbiased as it is not conditional on working during the UI spell, which is endogenous, but does not allow us to separate the impact on the probability of work from the intensive margin impact. Table D4 of Appendix D show nonsignificant results, suggesting that, given the positive impact on the probability

[45] Jobseekers are entitled to receive benefits as long as the sum of their labor income and their UI benefits does not exceed their previous earnings.

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

[47] The RD estimation yields estimates larger than one because it does not account for the binary nature of the variable. A parametric estimation using bivariate probit indicates a 55 pp increase in the probability to work during the UI spell caused by the option right (Table D3 of Appendix D).

to work during the UI spell, there is no positive impact of the OR on hourly wage.[48] Although compliers resort more often to partial employment during the UI spell, the type of job they find is not of better quality, if we assume that the wage rate is a good proxy for job quality.

Taken together, these findings suggest that people exercising the OR are not only on benefits for a longer time in the short run, but also experience a more unstable path, alternating frequently between short periods of employment and unemployment. It could also be the case that people accustomed to partial employment are less worried about having a shorter unemployment right and focus more on the generosity of UI benefits, as they know they will find short-term employment contracts to extend the length of their UI entitlements.[49]

4.2.2 The optimization ability of the unemployed

Because the impact of the OR on unemployment duration is markedly negative in the short run, and less clear in the long run, the ultimate welfare impact is not straightforward. The presence of adverse selection discussed in section 3.2 suggests that jobseekers have a significant amount of private information about their unemployment risk. If that is the case, they should be able to choose the option that optimizes their compensation based on this information. Answering the question of whether the unemployed made the right decision is complex, and made difficult by the lack of data on the full set of insurance contracts for eligible nontakers. However, I explore several outcomes that shed light at the link between the OR and the welfare and optimization ability of jobseekers. A first dimension relates to the comparison of total income earned by compliers and controls, and a second one relates to the risk of ending up with no income at all.

I start investigating the first dimension by measuring the effect of OR on daily earnings, defined as the sum of UI benefits and earnings from work divided by the duration of the unemployment spell.[50] It allows to account both for the fact that earning more benefits or wage presumably increases welfare, whereas having a longer UI spell may decrease welfare. Table I5 (Appendix I) indicates a negative impact on daily earnings, suggesting that the effect of OR on unemployment duration outweighs the effect of OR on the total amount of benefits and the probability to work while unemployed.[51]

[48] RD estimates on the conditional hourly wage are negative and nonsignificant. Tables are available upon request.

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

[50] I do not use any time discount factor in this analysis.

[51] It should be again emphasized that earnings from work are only observed if the person stays registered as unemployed. Although it is a limitation, since daily earnings are measured during the unemployment spell, taking a job implies in most cases staying registered as unemployed, and working only for short spells. Given the results on the probability of working while unemployed, and the fact that estimates on daily earnings are negative, it suggests that, if anything, the effect is underestimated.

A second important outcome to take into account is whether takers reach the exhaustion point of their entitlements. Indeed, if by exercising the OR they lose benefit duration, they could still be entitled to a longer coverage. If they are not at risk of running out of benefits, taking up the OR would simply mean having higher benefits, and would be a risk-free way to maximize benefits collected. Table I2 of Appendix I shows that indeed only a small percentage of takers, 17%, exhaust their UI entitlement. However, the strong effect of the OR on the probability of exhausting benefits (+15 percentage points, Table 4) suggests that at least a portion of the unemployed choosing the OR because they anticipate a quick return to the labor market fail in their prediction.

To have a sense of whether some groups of unemployed are better optimizers relative to others, I perform a heterogeneity analysis, intersecting the propensity to take the OR with the outcome in terms of unemployment spell duration. The idea is to analyze which subpopulations are more likely to take the OR, and whether they experience a negative impact on their employment prospects by doing so. Using age, gender, and education categories and comparing the first and second stage for each category, I observe that younger unemployed people have a higher jump in the probability of taking up the OR at the 20€ threshold (Table D5 of Appendix D), which is compatible with the less stable professional status that is often experienced in the early years of the career. Younger workers (under 35 years old) do not optimize perfectly, because both their take-up and the negative impact on unemployment duration are of high magnitude. Older workers have insignificant first- and second-stage estimates. Some exercise the OR but they do not seem to resort to it under the 20€ criterion. Similarly, Table D5 points to a high take-up jump for people with a higher level of education, with a limited negative impact for the very top of the distribution. In particular, we observe that people with higher education display a significant and substantial jump in take-up while the impact on unemployment duration is lower than the average, whereas it is negative and of higher magnitude for people who only completed high school, despite a higher first stage. In line with this result on education, we observe that unskilled employees and workers often exercise the OR whereas the impact is markedly negative for them. On the contrary, the skilled worker and executive categories are where the jump in take-up is highest with a limited or insignificant effect on unemployment duration. This means that their qualifications play a role in protecting them from the adverse impact of the OR on labor market performance. The OR is also more detrimental to men, because they exhibit a higher discontinuity in take-up, coupled with a similar negative impact on labor market outcomes than women.

Overall, it seems that workers in the middle and the top of the age and skills distribution, as well as the highly educated, are those gaining the most advantage from the OR, with a limited impact on their subsequent unemployment spell duration. One interpretation is that they are better at predicting their reemployment probability, and that they have at the same time better objective labor market prospects. Better educated workers are more likely to take the risk of exercising the OR without increasing their unemployment spell duration too much, because they are better equipped to find a job rapidly and they may have a more stable professional status. However, it is also possible that different subgroups face different treatment intensity: for example, young people are likely to experience an erratic earnings trajectory in the beginning of their career, with substantial jumps in income from one job to the other. Therefore, the difference in benefit between both options may be larger than for other categories. Their higher propensity to take the OR could then be rationalized by a different treatment intensity, keeping preferences and employment prospects equal. I cannot observe the full set of insurance contracts offered to different subgroups, but only those offered to takers.[52] Based on this information, I regress the ratio of benefits and the ratio of PBD in case of takeup relative to nontakeup on a number of observable characteristics.[53] Younger and more skilled takers have, on average, a higher PBD ratio but a lower benefit one, making the net effect on treatment intensity ambiguous. Level of education, is, however, negatively correlated with both ratios. It means that highly-educated workers have a higher propensity to take the OR despite the fact that we observe that highly-educated workers gain less by taking the OR than the average of other takers. It suggests that, assuming they have the same preferences than the average worker, they anticipate a quick return to the labor market. The fact that they experience only a limited increase in their unemployment duration, however, can be explained both by less intensive treatment or better employment prospects.

4.3 Adverse selection and moral hazard

The correlation between observed unemployment duration and the OR take-up mixes moral hazard and adverse selection. The challenge of the analysis is to disentangle the two, and to compare their magnitude. Table 2 points to the existence of significant adverse selection in this setting. This is confirmed by Tables 10 and 11 where the population is divided into quintiles of predicted unemployment duration. Table 10 focuses on the subsample of workers with previous daily benefit lower than 20€, where we can precisely identify the population of eligibles. On this sample, we observe that the take-up rate is a decreasing function of the predicted unemployment duration, except for the fourth quintile, which means that the higher the unemployment risk, the more likely it is that the worker will choose the longest coverage. Consistently, Table 11 shows, on the whole sample, that the jump in take-up at the threshold is the highest in the lowest quintiles.

[52] The information on the options faced by takers does not allow to fully conclude on the potential heterogenous treatment intensity, because it is conditional to the endogenous takeup decision. However, it is still informative on the expectations of different subgroups of takers.

[53] Tables are available upon request.

For both tests, the pattern in the middle of the distribution is less clear. However, they indicate that the bottom of the predicted unemployment duration is systematically associated with a higher takeup, whereas the top of the distribution is systematically associated with a lower takeup. This correlation test with the *predicted* unemployment duration, although it may not capture the role of unobservables, is indicative of adverse selection.

We know from the previous section that the moral hazard response to the OR is substantial, as measured by the increase in the unemployment spell duration at the eligibility threshold. I try to go further by analyzing the response on the different predicted unemployment duration quintiles (Table 12). It shows that, as the predicted unemployment duration increases, the negative impact of the OR also increases, meaning that those who are initially predicted to stay unemployed longer will suffer even more from choosing a shorter coverage with higher benefits. Although, here again, the relationship is less clear in the middle of the distribution, we observe that workers in the top quintile of predicted unemployment duration suffer from a negative impact of the option right on their employment prospects three times larger than workers in the bottom quintile. These results suggest that the policy designed to give a certain degree of flexibility in the UI choice is widening inequalities in terms of employment probability.

Following **?**, I use the following decomposition to separate moral hazard (MH) from adverse selection (AS).

$$
\begin{aligned}
E_S[\pi|Short] - E_L[\pi|Long] &= \underbrace{E_S[\pi|Short] - E_S[\pi|Long]}_{MH} + \underbrace{E_S[\pi|Long] - E_L[\pi|Long]}_{AS} \\
AS &= \text{Observed difference} - \text{RDD estimate} \\
&= 79 - 157 \\
&= -78 \\
|AS| &\approx 50\% \;\; \text{MH}
\end{aligned}
$$

The change in unemployment duration between people randomly offered the choice of the short option versus a situation in which everyone is assigned to the long option is measured by the RDD coefficient, and captures the moral hazard cost.[54] The observed difference is measured as the difference between takers and nontakers under the 20€ cutoff. The choice to restrict to this subsample is justified by the fact that the adverse selection measured in Table 2 is computed on the same sample, as it is the only population where

[54]The moral hazard estimate goes in the opposite direction to what is usually found. This counter-intuitive result is actually explained by the fact that it measures a combination of a change in the overall amount of UI benefits as well as a change in the benefit profile. The estimate captures the effect of receiving higher benefits in the short-run and lower benefit in the long-run compared to the opposite profile. It therefore suggests that the moral hazard cost is higher in the beginning of the spell, ending up in a positive net effect of the OR on unemployment spell duration.

I can assess the eligibility criteria even for nontakers. Because I want to measure the relationship between unemployment risk and insurance contract decision, I want to ensure that nontakeup is not actually explained by noneligibility. In addition, the moral hazard cost is estimated on the population of compliers, who are workers meeting the 20€ but not the 30% condition. I cannot measure the observed difference on the same population, by comparing the observed unemployment duration of compliers and nontakers meeting the 20€ but not the 30% condition, because I do not observe this last condition on nontakers. However, restricting to the sample below 20€ allows to get closer to the sample where I measure moral hazard. Reassuringly, the observed difference between takers and nontakers above the 20€ cutoff is also equal to 79 days. Adverse selection is retrieved from the subtraction of moral hazard from the observed difference in the unemployment spell duration between people choosing the long and the short options. The figures used are based on the duration of the paid unemployment spell immediately following choosing whether to exercise the OR. This computation suggests that, although adverse selection is sizable, moral hazard is far more substantial in this setting.

4.4 Robustness tests

Definition of the unemployment spell – The preferred definition of the unemployment spell used throughout the paper is the gathering of days registered as unemployed without any interruption of at least 4 months. The 4-month criterion has been chosen to ensure that the interruption reflects a stable return to work, and because it is the minimum working requirement to be able to open a new UI right. The definition chosen should not affect the result to the extent that we are comparing people at the direct neighborhood of the 20€ cutoff. Nonetheless, it could be argued that if people exerting the OR are more prone to experience very short employment spells while still being registered as unemployed, it could inflate the duration of the unemployment spell as defined earlier, even though these small employment periods are not counted in the unemployment spell duration. To alleviate this concern, I perform the same analysis on the duration of the spell before an interruption in payment of different durations (from 1 day to 2 months). Table D6 of Appendix D exhibits very similar reduced-form estimates when expressed relative to the average duration of the spell (the last two columns reproduce the main specification). Then, the measured impact on unemployment spell duration does not depend on the definition of the spell I choose to adopt. I also reproduce the main results looking at the probability of being unemployed at different time horizons (Tables I7, I8 of Appendix I). These findings confirm that the main effect on employment probability occurs in the short term, more precisely within a 9-month horizon.

Permutation test – I perform a nonparametric permutation test in the same spirit of Chetty et al. (2009) where RD estimates are computed for 1,000 randomly chosen values of the cutoff. Defining $F(\bar{\beta}_p)$ to be the empirical cumulative distribution function of these placebo effects, the statistic $F(\beta)$ gives a p-value for the hypothesis that $\beta = 0$. Intuitively, if the option right had a significant effect on unemployment duration, we would expect the estimated reduced-form coefficient to be in the lower tail of estimated placebo effects. Figure I5 (Appendix I) reports the cumulative distribution function of the RD estimate for paid unemployment spell duration measured using these placebo values. The solid vertical line denotes the treatment effect on the real cutoff, which lies outside the 95% confidence interval (dashed lines). I obtain $F(\beta) = 0.04$. The p-value is larger than the one obtained using t-test, but it confirms that the option right has a true positive effect on unemployment duration.[55]

5 Concluding remarks

This paper takes advantage of an uncommon setting in which unemployed people are offered a choice between two unemployment benefits schedules. It is *a priori* unclear what makes the worker better-off between receiving on average higher benefits for a shorter duration or having a longer PBD with lower average benefits. This paper looks precisely at the combined effect of a variation in the level, duration and profile of benefits on labor market outcomes. Opting for the short and high-benefit schedule increases dramatically the length of the subsequent unemployment spell. This finding suggests that the moral hazard cost of UI benefits is larger early in the spell. This effect is particularly worrying if we consider that the targeted population is already at risk in the labor market, alternating unemployment and employment spells. However, the effect is no longer significant when considering a longer time horizon: the unemployed choosing the short and high-benefit schedule do not experience more days on UI benefits over the whole of the following period, but they resort more to short-term and part-time work contracts. The main mechanism explaining both the divergence between the short- and long-run effects and between the impact on paid and registered unemployment is the more regular use of partial employment. Determining whether experiencing numerous short employment spells has a positive impact on the long-run professional path is an open question. However, findings on the characteristics of this partial employment suggest that the additional days of unemployment for compliers are not necessarily used to improve the quality of the job. Finally, observing a choice on the part of the insured in the UI context is a unique opportunity to test the presence of adverse selection. I add to the scarce empirical evidence by showing that adverse selection is substantial in this setting, although the main cost of this UI scheme is the moral hazard cost. Letting the unemployed

[55] The reduced-form effect is negative because it measures the effect of having a daily benefit higher than 20€.

choose the parameters of their coverage seems to have a detrimental impact, in particular on those already experiencing difficulties in the labor market, namely the less skilled and educated, the youngest, and those with the highest predicted unemployment risk.

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Tables and Figures

Table 1: Individual characteristics of compliers, takers and eligibles

	Compliers	Takers	Eligible Nontakers	All eligibles	Noneligible Nontakers	(3) - (1)	(3) - (2)	(5) - (4)
Demographics								
Age	39.496	30.317	35.169	33.862	37.089	-4.327***	4.852***	3.228***
						(0.324)	(0.057)	(0.024)
Percentage of female	0.851	0.570	0.690	0.658	0.455	-0.161***	0.120***	-0.203***
						(0.012)	(0.002)	(0.001)
Qualification								
Executive	0.004	0.016	0.004	0.008	0.071	-0.000	-0.012***	0.063***
						(0.002)	(0.001)	(0.001)
Intermediate occupation	0.016	0.024	0.005	0.013	0.030	-0.010***	-0.019***	0.017***
						(0.002)	(0.001)	(0.001)
Skilled employee	0.620	0.580	0.513	0.539	0.515	-0.107***	-0.067***	-0.023***
						(0.015)	(0.003)	(0.002)
Skilled blue collar worker	0.210	0.265	0.222	0.239	0.245	0.012	-0.043***	0.006***
						(0.013)	(0.003)	(0.001)
Unskilled employee	0.139	0.094	0.224	0.173	0.101	0.084***	0.130***	-0.073***
						(0.013)	(0.002)	(0.001)
Unskilled blue collar worker	0.011	0.021	0.032	0.028	0.038	0.021***	0.011***	0.010***
						(0.005)	(0.001)	(0.001)
Level of education								
No education	0.039	0.023	0.041	0.036	0.030	0.002	0.017***	-0.006***
						(0.005)	(0.001)	(0.000)
Elementary school completed	0.022	0.011	0.020	0.018	0.011	-0.002	0.009***	-0.007***
						(0.004)	(0.001)	(0.000)
6^{th} to 8^{th} grade	0.024	0.013	0.023	0.020	0.015	-0.001	0.010***	-0.005***
						(0.004)	(0.001)	(0.000)
Middle school completed	0.048	0.042	0.060	0.055	0.040	0.012*	0.019***	-0.016***
						(0.006)	(0.001)	(0.000)
10^{th} to 11^{th} grade	0.015	0.011	0.016	0.014	0.012	0.001	0.005***	-0.002***
						(0.003)	(0.001)	(0.000)
Vocational diploma	0.424	0.469	0.405	0.422	0.367	-0.019	-0.064***	-0.055***
						(0.013)	(0.002)	(0.001)
High school diploma - Baccalaureate	0.247	0.228	0.255	0.248	0.253	0.008	0.028***	0.005***
						(0.011)	(0.002)	(0.001)
Two-year Higher education degree	0.101	0.111	0.094	0.099	0.140	-0.007	-0.018***	0.042***
						(0.008)	(0.001)	(0.001)
Three to four-year Higher education degree	0.056	0.056	0.058	0.057	0.073	0.002	0.001	0.016***
						(0.006)	(0.001)	(0.001)
Five-year and more Higher education degree	0.025	0.036	0.029	0.031	0.059	0.004	-0.007***	0.028***
						(0.004)	(0.001)	(0.001)
Rights' characteristics								
Tenure	395.287	393.067	681.725	632.185	917.700	286.438***	288.658***	285.515***
						(66.979)	(9.367)	(5.436)
Part-time coefficient	0.694	0.689	0.645	0.650	0.958	-0.049***	-0.044***	0.308***
						(0.013)	(0.002)	(0.000)
Number of unemployment spells over the period	1.336	1.370	2.811	2.423	2.471	1.475***	1.441***	0.048***
						(0.073)	(0.012)	(0.005)
PBD of the former right	446.454	468.780	550.358	540.413	564.500	103.904***	81.578***	24.087***
						(11.563)	(1.725)	(0.607)
Remaining PBD from former right	341.257	355.737	324.740	333.061	330.519	-16.517**	-30.997***	-2.541***
						(5.921)	(1.063)	(0.515)
Observations	1508	62397	169234	231631	1664287	170742	231631	1895918

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

NOTE: This table compares the characteristics of compliers, takers, eligible nontakers and all eligibles, restricting to the sample under 20€. Compliers are those meeting the 20€ but not the 30% condition, as they would not be treated absent the 20€ criterion. They are older and more frequently female than all other categories. They are also overrepresented among skilled employees. They are typically found in preschool assistant occupations, where income fluctuations are common, as wage depends on the number of children cared for. Eligibles show characteristics associated with a lower attachment to the labor market compared with noneligible nontakers, whereas, among them, takers are younger, more skilled and educated.

Table 2: Predicted unemployment duration by take-up

	Controls	Takers	Non takers	Difference (3)-(2)
Predicted paid unemployment spell duration	263.95	253.67	286.43	32.76***
				(0.967)
Observations	115900	62397	166269	228666

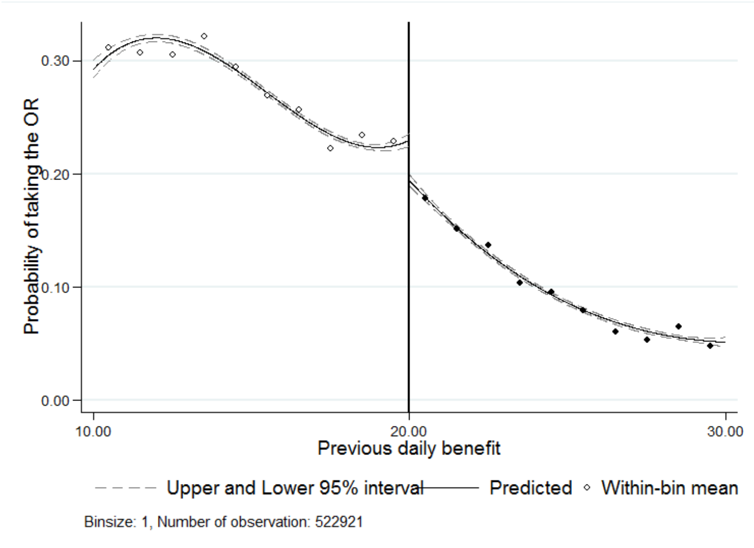
** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The table presents the average predicted unemployment duration for the group of controls, takers and nontakers below the 20€ cutoff. Unemployment duration has been predicted using information on age, gender, skill level (6 items), education level (10 items), tenure in past job, wage, sought occupation (81 items), working hours, separation motive (8 items), region (31 items), sector of activity (21 items), number of children, marital status, month and year of separation, occupation in past job (81 items), firm size, plant size, number of previous unemployment rights, daily benefit, potential benefit duration, average benefit level, and duration based on previous unemployment rights. The model used is a random forest with 2,000 iterations and a maximum depth of 50, trained on a sample of jobseekers similar to those under study who were unemployed during the 2 years preceding the implementation of the OR. All variables related to the UI right or to past job or firm are those corresponding to the previous UI right that was not exhausted, as the information is available for all eligible jobseekers, whereas information on the potential new right is available only for takers, and therefore cannot be used for the prediction.

Table 3: Impact of having a DB lower or equal to 20€ on OR take-up

	Taking up the option right					
RD_Estimate	-0.011*	-0.062***	-0.046***	-0.016***	-0.039***	-0.044***
	(0.0063)	(0.0057)	(0.0058)	(0.0058)	(0.0049)	(0.0063)
Robust 95% CI	[-.027 ; -.001]	[-.076 ; -.052]	[-.06 ; -.037]	[-.031 ; -.006]	[-.051 ; -.031]	[-.058 ; -.031]
Observations	1914144	1914144	1914144	1882340	1882340	1882340
Order Loc. Poly. (p)	1	2	3	1	2	3
Control outcome mean	.09	.09	.09	.09	.09	.09
Covariates	No	No	No	Yes	Yes	Yes

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the first-stage estimation results of the RD. It shows that having a daily benefit lower than 20€ is associated with a jump in the OR take-up rate of about 4 percentage points. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector, and local linear, quadratic and cubic polynomials have been used, with and without controls. The control outcome mean measures the mean of the outcome variable right above the cutoff, for workers with a previous daily benefit between 20€ and 30€.

Figure 1: Probability of taking up the option right



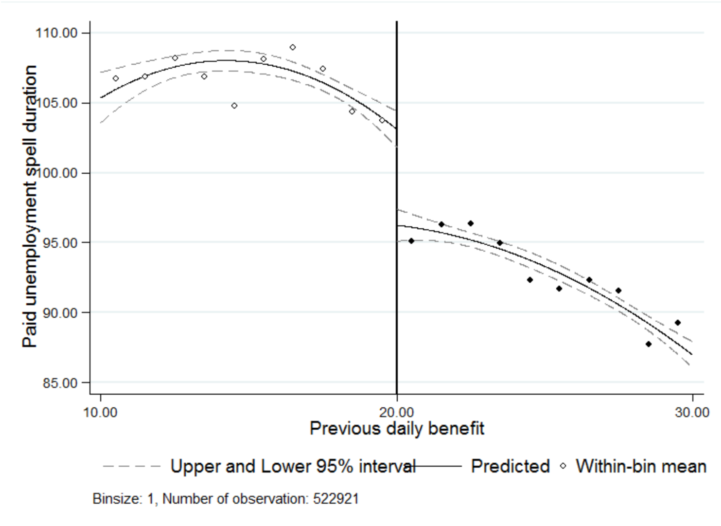
SOURCE: UI data (FNA).
NOTE: This graph reports the first-stage relationship between the running variable, which is the former daily benefit, and the probability of taking up the OR. It shows that having a daily benefit lower than 20€ is associated with a jump in the OR take-up rate of about 4 percentage points.

Table 4: Impact of the option right on paid unemployment duration

	Paid unemployment spell duration			
RD_Estimate	253.4***	157.3***	185.9***	201.6***
	(77.72)	(33.44)	(39.50)	(39.12)
Robust 95% CI	[59.218 ; 386.082]	[83.864 ; 231.446]	[64.39 ; 249.048]	[104.734 ; 266.887]
Observations	1888093	1888093	1882340	1882340
Order Loc. Poly. (p)	1	2	1	2
Control outcome mean	92.24	92.24	92.24	92.24
Covariates	No	No	Yes	Yes

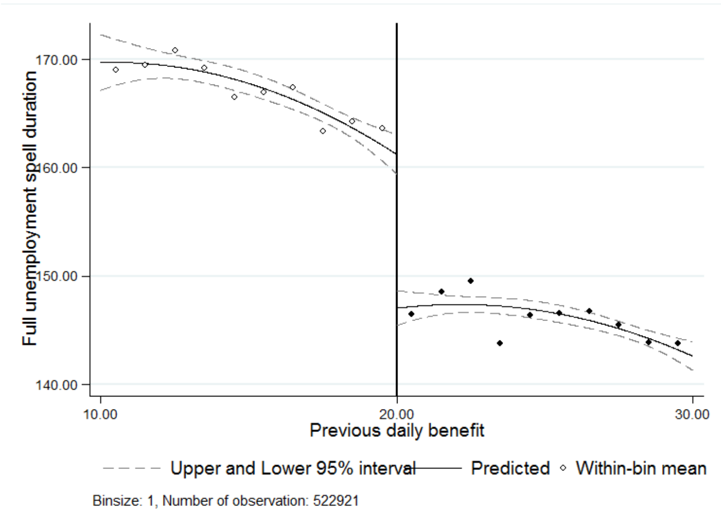
** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the RD estimates on the duration of the subsequent unemployment spell. The dependent variable is the paid unemployment duration, which only includes days the jobseeker receives benefits. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector, and local linear and quadratic polynomials have been used, with and without controls. The control outcome mean measures the mean of the outcome variable right above the cutoff, for workers with a previous daily benefit between 20€ and 30€. The OR increases the subsequent paid unemployment spell duration by 5 to 8 months.

Figure 2: Paid unemployment spell duration



SOURCE: UI data (FNA).
NOTE: This graph reports the second-stage relationship between the running variable, which is the former daily benefit, and the duration of the subsequent unemployment spell (second-order polynomial). The unemployment spell is defined as the paid unemployment spell, that is, the addition of the days the jobseeker receives benefits. It shows that having a daily benefit lower than 20€ is associated with a jump in the duration of the paid unemployment spell from about 97 to 104 days.

Figure 3: Full unemployment spell duration



SOURCE: UI data (FNA).
NOTE: This graph reports the second-stage relationship between the running variable, which is the former daily benefit, and the duration of the subsequent unemployment spell (second-order polynomial). The unemployment spell is defined as the full unemployment spell, that is, the addition of all the days the jobseeker is registered as unemployed, regardless whether receiving benefits or not. It shows that having a daily benefit lower than 20€ is associated with a jump in the duration of the full unemployment spell from about 148 to 161 days.

Table 5: Impact of the option right on full unemployment duration

	Full unemployment spell duration			
RD_Estimate	361.7***	335.0***	384.3***	472.4***
	(30.86)	(49.38)	(46.81)	(63.20)
Robust 95% CI	[275.388 ; 408.345]	[225.373 ; 423.782]	[299.185 ; 493.74]	[304.341 ; 586.861]
Observations	1888093	1888093	1882340	1882340
Order Loc. Poly. (p)	1	2	1	2
Control outcome mean	145.84	145.84	145.84	145.84
Covariates	No	No	Yes	Yes

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the RD estimates on the duration of the subsequent unemployment spell. The dependent variable is the total unemployment duration, including days the jobseeker is registered as unemployed, regardless whether receiving benefits or not. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector, and local linear and quadratic polynomials have been used, with and without controls. The control outcome mean measures the mean of the outcome variable right above the cutoff, for workers with a previous daily benefit between 20€ and 30€. The OR increases the subsequent full unemployment spell duration by about 12 months.

Table 6: Impact of the option right on the total number of days on UI benefits

	Total number of days receiving UI benefits after OR			
RD_Estimate	182.3	1.6	8.9	43.2
	(182.28)	(36.91)	(31.22)	(41.07)
Robust 95% CI	[-265.242 ; 486.825]	[-84.771 ; 72.162]	[-79.563 ; 67.175]	[-58.647 ; 115.317]
Observations	1888093	1888093	1882340	1882340
Order Loc. Poly. (p)	1	2	1	2
Control outcome mean	136.07	136.07	136.07	136.07
Covariates	No	No	Yes	Yes

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the RD estimates on the total number of days on UI benefits over the whole observed period (October 2014–May 2017). Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector, and local linear and quadratic polynomials have been used, with and without controls. The control outcome mean measures the mean of the outcome variable right above the cutoff, for workers with a previous daily benefit between 20€ and 30€. The OR does not have a significant impact on the total number of days on UI benefits.

Table 7: Impact of the option right on the total number of days registered as unemployed

	Total number of days unemployed after OR			
RD_Estimate	180.7	134.8**	176.5***	226.0***
	(190.25)	(52.73)	(43.28)	(53.01)
Robust 95% CI	[-255.745 ; 534.962]	[20.595 ; 233.659]	[58.228 ; 261.538]	[124.935 ; 348.938]
Observations	1888093	1888093	1882340	1882340
Order Loc. Poly. (p)	1	2	1	2
Control outcome mean	206.02	206.02	206.02	206.02
Covariates	No	No	Yes	Yes

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

NOTE: This table reports the RD estimates on the total number of days registered as unemployed over the whole observed period (October 2014–May 2017). Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector, and local linear and quadratic polynomials have been used, with and without controls. The control outcome mean measures the mean of the outcome variable right above the cutoff, for workers with a previous daily benefit between 20€ and 30€. The OR increases the total number of days registered as unemployed by about 180 days in the linear specification.

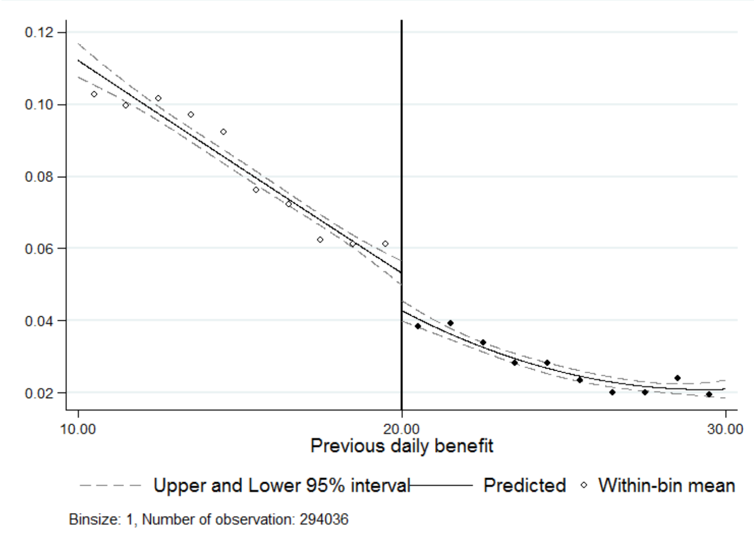
Table 8: Impact of OR on the probability to exhaust benefits

	Probability of exhausting UI benefits			
RD_Estimate	0.166***	0.134***	0.151***	0.153***
	(0.0530)	(0.0488)	(0.0493)	(0.0519)
Robust 95% CI	[.031 ; .275]	[.016 ; .222]	[.026 ; .253]	[.029 ; .248]
Observations	1914144	1914144	1882340	1882340
Order Loc. Poly. (p)	1	2	1	2
Control outcome mean	.01	.01	.01	.01
Covariates	No	No	Yes	Yes

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

NOTE: This table reports the RD estimates on the probability of exhausting UI benefits. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector, and local linear and quadratic polynomials have been used, with and without controls. The control outcome mean measures the mean of the outcome variable right above the cutoff, for workers with a previous daily benefit between 20€ and 30€. The right here refers to the new right for those exercising the OR, and to the residual of the former right for those not exercising. If an eligible nontaker has exhausted the residual and I do not observe that he recharges his right based on his last employment spells, I assume that he is not able to recharge, and has exhausted all his entitlements. If a recharging is observed for nontakers, I take into account the addition of the residual and the new right. The OR increases the probability of exhausting UI benefits by about 15 percentage points.

Figure 4: Impact on the probability of exhausting the UI right



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

Table 9: Impact of the option right on the probability of working within the spell

	Has worked during unemployment spell			
RD_Estimate	1.422***	1.143***	1.175***	1.221***
	(0.3437)	(0.1740)	(0.1626)	(0.2022)
Robust 95% CI	[.642 ; 2.167]	[.763 ; 1.498]	[.905 ; 1.647]	[.802 ; 1.692]
Observations	1914144	1914144	1882340	1882340
Order Loc. Poly. (p)	1	2	1	2
Control outcome mean	.39	.39	.39	.39
Covariates	No	No	Yes	Yes

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the RD estimates on the probability of working within the subsequent unemployment spell, while staying registered as unemployed. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector, and local linear and quadratic polynomials have been used, with and without controls. The control outcome mean measures the mean of the outcome variable right above the cutoff, for workers with a previous daily benefit between 20€ and 30€. The OR increases the probability of working during the spell by more than 100 percentage points, because the RD estimation does not account for the binary nature of the dependent variable.

Table 10: Summary statistics by predicted unemployment duration

	1^{st} quintile	2^{nd} quintile	3^{rd} quintile	4^{th} quintile	5^{th} quintile
Taking the option right with previous DB ≤ 20€	0.297	0.249	0.264	0.327	0.227
Actual paid unemployment spell duration	90.816	106.100	110.265	114.149	112.012
Observations	46491	46244	46264	46329	46303

Standard errors in parentheses.
NOTE: The table reports the takeup rate and actual paid unemployment duration by quintiles of predicted paid unemployment spell duration on the sample of workers under the 20€ cutoff, because this is the only subsample where we can identify eligible nontakers. Included covariates are on age, gender, skill level (6 items), education level (10 items), tenure in past job, wage, sought occupation (81 items), working hours, separation motive (8 items), region (31 items), sector of activity (21 items), number of children, marital status, month of separation, occupation in past job (81 items), firm size, plant size, number of previous unemployment rights, daily benefit, potential benefit duration, average benefit level, and duration based on previous unemployment rights. The model used is a random forest with 2,000 iterations and a maximum depth of 50, trained on a sample of jobseekers similar to those under study who were unemployed during the 2 years preceding the implementation of the OR. All variables related to the UI right or to past job or firm are those corresponding to the previous UI right that was not exhausted, as the information is available for all eligible jobseekers, whereas information on the potential new right is available only for takers, and therefore cannot be used for the prediction.

Table 11: First-stage regression by predicted unemployment duration quintile

	Taking up the option right				
	1^{st} quintile	2^{nd} quintile	3^{rd} quintile	4^{th} quintile	5^{th} quintile
RD_estimate	-.0627***	-.0347**	-.0327***	-.0531***	-.0198***
	.00978	.01457	.01068	.00891	.00741
Observations	382395	376179	377199	381807	370513

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The table reports the jump in takeup rate at the 20€ threshold using a RD specification separately on the different predicted paid unemployment spell duration quintiles. Included covariates are on age, gender, skill level (6 items), education level (10 items), tenure in past job, wage, sought occupation (81 items), working hours, separation motive (8 items), region (31 items), sector of activity (21 items), number of children, marital status, month of separation, occupation in past job (81 items), firm size, plant size, number of previous unemployment rights, daily benefit, potential benefit duration, average benefit level, and duration based on previous unemployment rights. The model used is a random forest with 2,000 iterations and a maximum depth of 50, trained on a sample of jobseekers similar to those under study who were unemployed during the 2 years preceding the implementation of the OR. All variables related to the UI right or to past job or firm are those corresponding to the previous UI right that was not exhausted, as the information is available for all eligible jobseekers, whereas information on the potential new right is available only for takers, and therefore cannot be used for the prediction.

Table 12: Second-stage regression by predicted unemployment duration quintile

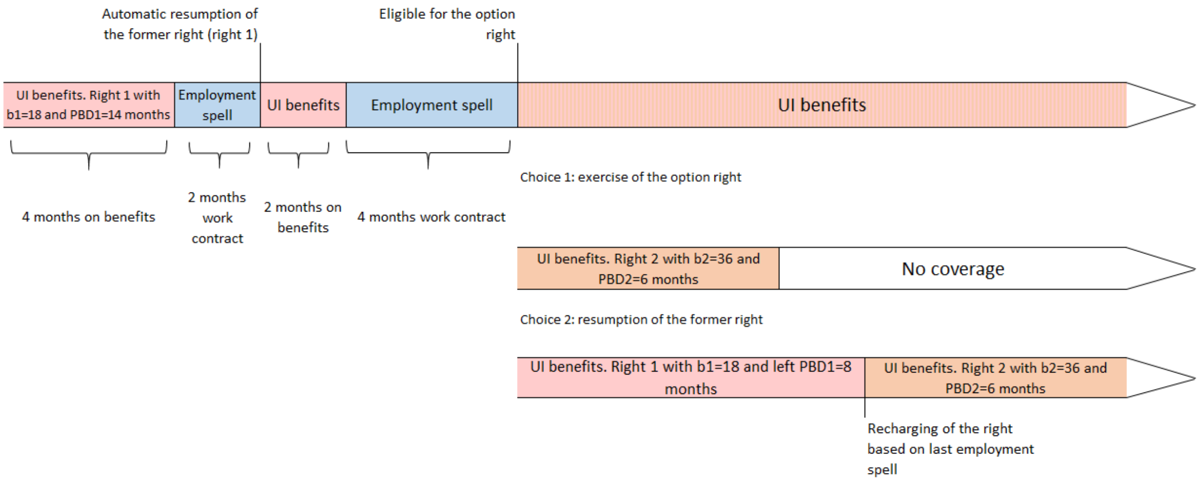
	Actual paid unemployment spell duration				
	1^{st} quintile	2^{nd} quintile	3^{rd} quintile	4^{th} quintile	5^{th} quintile
RD_Estimate	104.82***	173.66	320.24***	92.55**	292.78**
	(30.102)	(107.658)	(120.119)	(46.429)	(134.949)
Observations	382395	376179	377199	381807	370513

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The table reports the RD estimates of the effect of the OR on paid unemployment spell duration, separately for the different predicted paid unemployment spell duration quintiles. Unemployment duration has been predicted using information on age, gender, skill level (6 items), education level (10 items), tenure in past job, wage, occupation sought (81 items), working hours, separation motive (8 items), region (31 items), sector of activity (21 items), number of children, marital status, month of separation, occupation in past job (81 items), firm size, plant size, number of previous unemployment rights, daily benefit, potential benefit duration, average benefit level and duration based on previous unemployment rights. The model used is a random forest with 2,000 iterations and a maximum depth of 50, trained on a sample of jobseekers similar to those under study who were unemployed during the 2 years preceding the implementation of the OR. All variables related to the UI right or to past job or firm are those corresponding to the previous UI right that was not exhausted, as the information is available for all eligible jobseekers, whereas information on the potential new right is available only for takers, and therefore cannot be used for the prediction.

Appendices

A Institutional background

Figure A1: Option right trade-off



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

B Additional descriptive statistics

Table B1: Sample composition

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

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

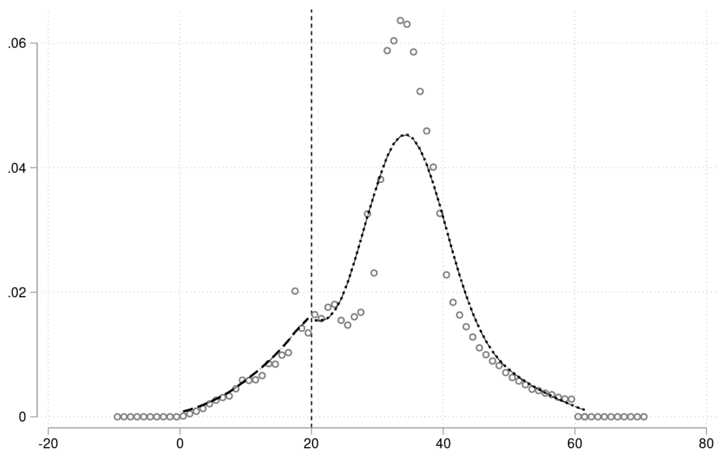
Table B2: New and former right characteristics of takers

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

NOTE: This table shows the characteristics associated with the former and the new rights for takers, decomposed by eligibility criterion. Compliers are those taking up the OR under the 20€ condition, but not fulfilling the 30% criterion. By construction, the ratio of the new and the former daily benefits is lower for them, as it is constrained to be under 1.3. However, they lose less in terms of duration, as shown by the last row.

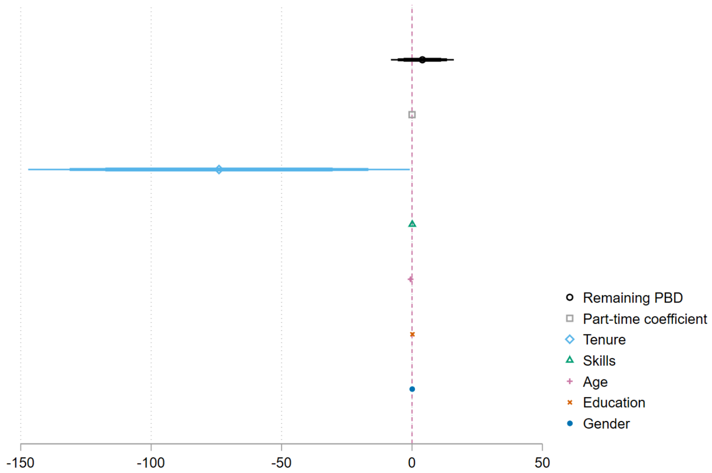
C Assumptions of the RDD

Figure C1: McCrary test on previous DB distribution



SOURCE: UI data (FNA).
NOTE: This graph shows the McCrary (2008) test (binsize = 1, bandwidth = 10) performed on the previous daily benefit distribution to test the hypothesis of continuity of the running variable distribution at the threshold. The density exhibits no discontinuity at the cutoff.

Figure C2: RD estimates on covariates



SOURCE: UI data (FNA).
NOTE: This graphs tests the assumption of continuity in the distribution of covariates at the eligibility threshold by using each covariate as a dependent variable in the RDD regression. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector, and a local quadratic polynomial is used. The reported RD coefficients do no appear significant, except for the tenure variable.

Table C1: Impact on paid unemployment spell duration controlling for predicted unemployment duration

	Paid unemployment spell duration					
RD_Estimate	253.4***	157.3***	185.9***	201.6***	114.6***	151.6***
	(77.72)	(33.44)	(39.50)	(39.12)	(30.11)	(34.93)
Robust 95% CI	[59.218 ; 386.082]	[83.864 ; 231.446]	[64.39 ; 249.048]	[104.734 ; 266.887]	[28.926 ; 165.39]	[75.036 ; 216.544]
Observations	1888093	1888093	1882340	1882340	532111	532111
Order Loc. Poly. (p)	1	2	1	2	1	2
Control outcome mean	92.24	92.24	92.24	92.24	92.24	92.24
Covariates	No	No	Yes	Yes	Predicted unemployment duration	Predicted unemployment duration

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reproduces the main result on the impact of the OR on the duration of the subsequent paid unemployment spell, without controls (columns 1-2), controlling for age, gender and education (columns 3-4), and controlling for predicted unemployment duration. The RD is estimated with the optimal bandwidth (MSE criterion), and local linear and quadratic polynomials are used. Unemployment duration is predicted using a sample of similar workers unemployed during the two years preceding the introduction of the option right, and performing an out-of-sample prediction on the sample under study. Included variables are age, gender, skill level (5 categories), education level (10 categories), sought occupation (14 categories), part-time coefficient, region of residence (31 categories), sector of activity (11 categories), month of contract termination, number of children, family situation (5 categories), previous occupation (81 categories), firm and plant size. A flexible linear model is used, yielding a R^2 equal to 7% and a RMSE equal to 281. The rather low R^2 can be explained by the fact that I do not include any variable related to the UI entitlements.

D Additional tables and figures

Table D1: Impact of the option right on paid unemployment duration – Reduced-Form estimates

	Paid unemployment spell duration			
RD_Estimate	-7.6***	-8.5***	-7.1***	-8.3***
	(1.15)	(1.52)	(1.14)	(1.51)
Robust 95% CI	[-9.609 ; -4.562]	[-12.182 ; -5.715]	[-9.252 ; -4.214]	[-12.044 ; -5.656]
Observations	1888093	1888093	1882340	1882340
Order Loc. Poly. (p)	1	2	1	2
Control outcome mean	92.24	92.24	92.24	92.24
Covariates	No	No	Yes	Yes

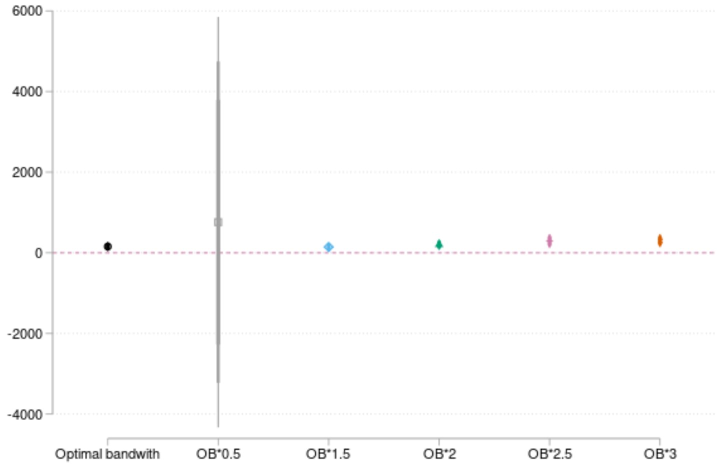
** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the reduced-form estimates from the RD regressions on the duration of the subsequent unemployment spell. The dependent variable is the paid unemployment duration, which only includes days the jobseeker receives benefits. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector, and local linear and quadratic polynomials have been used, with and without controls. The control outcome mean measures the mean of the outcome variable right above the cutoff, for workers with a previous daily benefit between 20€ and 30€. Having a daily benefit above 20€ decreases the subsequent paid unemployment spell duration by 7 to 8 days.

Table D2: Impact of the option right on full unemployment duration – Reduced-Form estimates

	Full unemployment spell duration			
RD_Estimate	-16.0***	-11.8***	-11.2***	-12.1***
	(1.56)	(1.61)	(1.45)	(1.60)
Robust 95% CI	[-18.755 ; -11.975]	[-14.934 ; -7.929]	[-14.114 ; -7.55]	[-15.992 ; -9.187]
Observations	1888093	1888093	1882340	1882340
Order Loc. Poly. (p)	1	2	1	2
Control outcome mean	145.84	145.84	145.84	145.84
Covariates	No	No	Yes	Yes

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the reduced-form estimates from the RD regressions on the duration of the subsequent unemployment spell. The dependent variable is the total unemployment duration, including days the jobseeker is registered as unemployed, regardless whether receiving benefits or not. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector, and local linear and quadratic polynomials are used, with and without controls. The control outcome mean measures the mean of the outcome variable right above the cutoff, for workers with a previous daily benefit between 20€ and 30€. Having a daily benefit above 20€ decreases the subsequent full unemployment spell duration by 11 to 16 days.

Figure D1: Impact of paid unemployment spell duration with different bandwidths



SOURCE: UI data (FNA).
NOTE: This graph reproduces the main result regarding the impact of the OR on the duration of the subsequent paid unemployment spell, using different bandwidths and a local quadratic polynomial. Optimal bandwidth has been multiplied respectively by 0.5, 1.5, 2, 2.5, and 3. The results are overall similar, but standard errors become very large when choosing a bandwidth equivalent to half of the optimal one.

Table D3: Impact of the option right on the probability of working within the spell – Bivariate probit

	Has worked during the unemployment spell					
Taking up the OR	0.586***	0.544***	0.551***	0.560***	0.558***	0.530***
	(0.019)	(0.019)	(0.013)	(0.018)	(0.012)	(0.009)
Observations	177,324	249,863	312,648	112,307	242,463	378,615
Order polynomial	1	2	3	1	2	3
Covariates	No	No	No	Yes	Yes	Yes
Chi^2	60.3269	78.7632	160.224	78.1139	180.387	310.681

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the coefficient from a bivariate probit regression of the impact of taking up the OR on the probability of working within the subsequent unemployment spell, while staying registered as unemployed. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector. Linear, quadratic, and cubic specifications are used, with and without controls. The OR increases the probability of working during the spell by about 55 percentage points.

Table D4: Impact of the option right on hourly wage from work during the UI spell

	Mean hourly wage over the UI spell			
RD_Estimate	2.6	-8.2	-15.1	22.9
	(43.80)	(17.81)	(26.03)	(21.60)
Robust 95% CI	[-99.485 ; 86.428]	[-46.504 ; 32.456]	[-76.66 ; 33.963]	[-26.808 ; 69.801]
Observations	1914144	1914144	1882340	1882340
Order Loc. Poly. (p)	1	2	1	2
Control outcome mean	6.55	6.55	6.55	6.55
Covariates	No	No	Yes	Yes

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the RD estimates on the hourly wage earned during the subsequent unemployment spell, while staying registered as unemployed. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector, and local linear and quadratic polynomials are used, with and without controls. The control outcome mean measures the mean of the outcome variable right above the cutoff, for workers with a previous daily benefit between 20€ and 30€. The hourly wage is computed as the sum of wages earned during the spell divided by the number of hours worked, unconditional to having worked. It means that I impute a zero value to jobseekers who have not worked during the UI spell, to avoid conditioning on an endogenous outcome. Results are not significant. Given that the OR has a positive significant impact on the probability to work during the UI spell, the absence of a significant positive impact on hourly wage suggests that, if anything, jobseekers choosing the OR work more but for lower hourly wages.

Table D5: RDD coefficients on subpopulations

	Taker	Paid unemployment spell duration
Gender		
Male	-0.0610***	206.4816***
	(0.00825)	(53.18824)
Female	-0.0196**	206.0439***
	(0.00808)	(42.15687)
Age		
Less than 25yo	-0.0400***	126.0005***
	(0.01103)	(25.34686)
25 to 34yo	-0.0323***	119.4231**
	(0.00752)	(48.47335)
35 to 44yo	0.0026	1227.3205
	(0.00766)	(2040.95902)
45 to 54yo	-0.0015	955.9014
	(0.00776)	(8831.66319)
55yo and over	-0.0085	983.7691
	(0.01163)	(1022.45154)
Level of Education		
Less than High school completed	-0.0126	784.2686
	(0.00895)	(748.27010)
Vocational High school degree	-0.0002	348.6353*
	(0.00933)	(190.16862)
General High school degree	-0.0457***	142.4035***
	(0.01171)	(37.81848)
Higher Education	-0.0338**	100.1442***
	(0.01313)	(32.73102)
Skill level		
Executives and Intermediate occupations	-0.0687*	-275.1663
	(0.03715)	(336.64902)
Skilled employees	-0.0734***	106.7712***
	(0.01172)	(36.77632)
Skilled blue-collar workers	0.0061	87.9350
	(0.02458)	(117.07776)
Unskilled employees and blue-collar workers	-0.0257**	304.6875**
	(0.01068)	(147.96209)

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

NOTE: This table reports RD estimates by sub-groups. The first column displays the first-stage estimates of the probability of taking up the OR on a binary variable indicating whether the former daily benefit was lower than 20€. The second column reports the second-stage estimates of the paid unemployment spell duration. The regressions have been run separately on different gender, age, education, and skills groups. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector and local linear polynomial has been used. The same regressions have been run using a local quadratic polynomial and results, available upon request, are similar. This table indicates for which subgroups the OR is particularly detrimental.

Table D6: Reduced-form estimates on paid UI spell duration using alternative definitions of the spell

	Paid unemployment spell duration									
	Before any interruption		15-day definition		31-day definition		61-day definition		122-day definition	
RD_Estimate	-3.0*** (1.07)	-2.8** (1.25)	-3.1*** (1.07)	-2.7** (1.26)	-3.0*** (1.09)	-2.8** (1.26)	-3.4*** (1.08)	-3.3*** (1.27)	-3.7*** (1.10)	-3.5*** (1.30)
Robust 95% CI	[-5.26 ; -.287]	[-5.38 ; .135]	[-5.391 ; -.395]	[-5.373 ; .197]	[-5.325 ; -.243]	[-5.558 ; .028]	[-5.588 ; -.632]	[-6.037 ; -.427]	[-5.912 ; -.887]	[-6.305 ; -.567]
Relative effect	-5.3%	-4.8%	-5.2%	-4.6%	-5.1%	-4.8%	-5.6%	-5.5%	-6%	-5.7%
Observations	1131038	1131038	1131038	1131038	1131038	1131038	1131038	1131038	1131038	1131038
Order Loc. Poly. (p)	1	2	1	2	1	2	1	2	1	2
Control outcome mean	57.46	57.46	58.97	58.97	59.11	59.11	59.85	59.85	62.12	62.12
Covariates	No	No	No	No	No	No	No	No	No	No

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

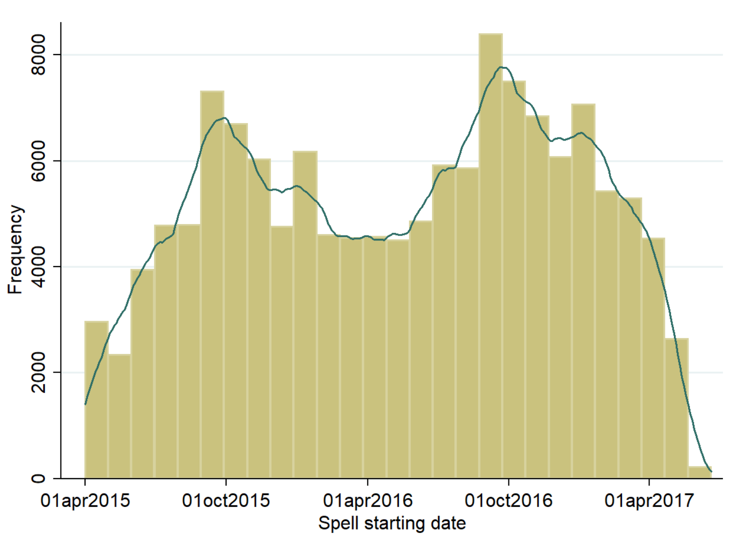
NOTE: This table reproduces the reduced-form RD estimates from the main regression on the paid unemployment duration by taking alternative definitions of the unemployment spell. The preferred definition used throughout the paper is the gathering of days registered as unemployed without any interruption of at least 4 months. The 4-month criterion has been chosen to ensure that the interruption reflects a stable return to work, and because it is the minimum working requirement to be able to open a new UI right. The definition chosen should not affect the result to the extent that we are comparing people in the direct neighborhood of the 20€ cutoff, and that there is, *a priori*, no reason for the 4-month interruptions to be more or less frequent for a person earning 19€ or 21€ daily, apart from the effect of the OR. Nonetheless, it could be argued that if people exercising the OR are more prone to experience very short employment spells while still being registered as unemployed, it could inflate the duration of the unemployment spell as defined earlier, even though these small employment periods do not enter the counting of the unemployment spell duration. To alleviate this concern, I perform the same analysis on the duration of the spell before interruption in payment (that is, going from subsidized unemployment to nonsubsidized unemployment is considered an interruption here) of different durations. Note that the sample size is slightly lower because the raw data necessary to redefine the unemployment spell and available to the author do not include the whole original sample. However, this subsample does not differ from the original one. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector, and local linear and quadratic polynomials are used, with and without controls. The control outcome mean measures the mean of the outcome variable right above the cutoff, for workers with a previous daily benefit between 20€ and 30€.

Reduced-form estimates have been preferred to be able to compare their magnitude across definitions of the spell without being influenced by the size of the jump in takeup. Estimates are smaller in absolute terms but very similar relative to the average duration of the different spells. Then, the measured impact on unemployment spell duration does not depend on the definition of the spell I choose to adopt.

Appendices - For online publication only

E Data appendix

Figure E1: Number of people exercising the option right over time



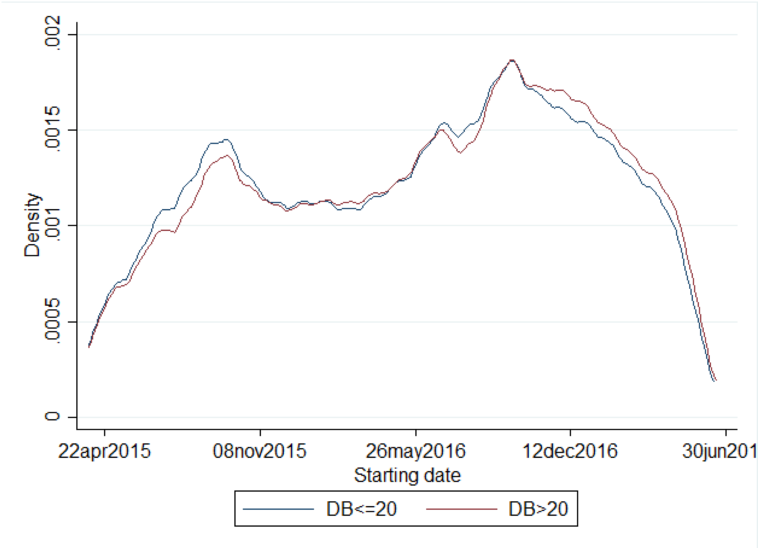
SOURCE: UI data (FNA).
NOTE: This graph displays the distribution of the number of people taking up the OR over time since its implementation in April 2015. We see an overall increase over time, with a seasonal pattern.

Table E1: Eligibility criteria distribution for takers

		Takers based on the 20€ criterion	
		0	1
Takers based on the 30% ratio criterion	0	9,022	1,791
	1	58,707	69,734

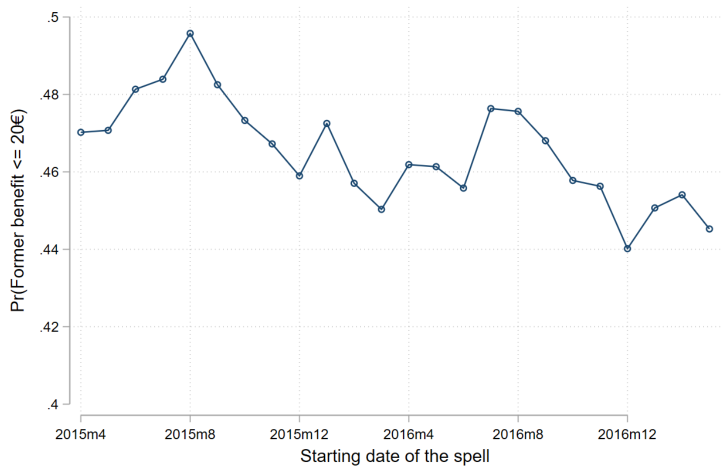
NOTE: This table reports the distribution of takers according to both eligibility criteria. Note that most of the takers under the 20€ condition also fulfill the 30% criterion, as it is uncommon to earn these very low levels of benefits for several consecutive unemployment spells.

Figure E2: Starting date distribution



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

Figure E3: Probability of having a benefit lower than 20€ over time



SOURCE: UI data (FNA).
NOTE: This graph plots the share of workers with a daily benefit equal to or lower than 20€ over time. It shows the probability fo being eligible as part of the 20€ criteria is rather stable over the period under study.

F Additional descriptive statistics

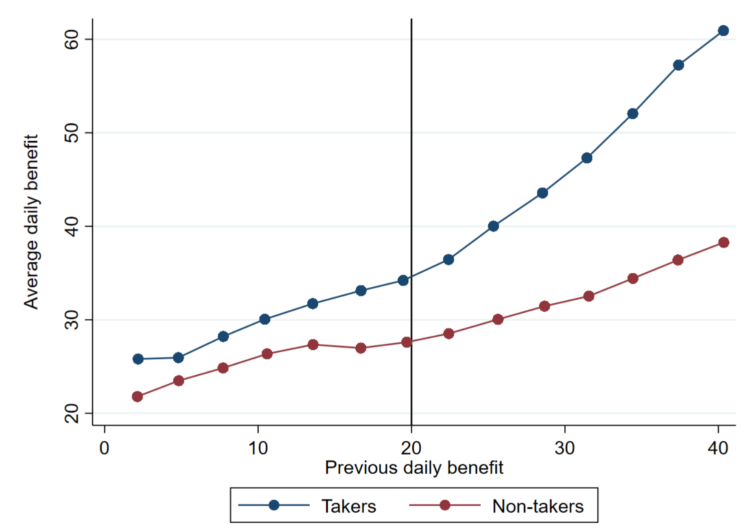
Table F1: Characteristics associated with option right take-up – takers

	Probability of being complier among eligibles		Probability of being taker among eligibles	
Level of education	-0.000086	0.000093	-0.002359***	0.001874***
	(0.000274)	(0.000132)	(0.000824)	(0.000674)
Level of skills	-0.005252***	-0.001014***	-0.082374***	-0.039415***
	(0.000615)	(0.000296)	(0.001595)	(0.001404)
Age	0.000130***	0.000068***	-0.010898***	-0.003128***
	(0.000044)	(0.000022)	(0.000114)	(0.000109)
Percentage of female	0.008597***	0.000516	-0.105833***	-0.047973***
	(0.001376)	(0.000605)	(0.003099)	(0.002674)
Tenure		-0.000001**		-0.000029***
		(0.000000)		(0.000002)
Part-time coefficient		0.007112***		0.094219***
		(0.000996)		(0.004321)
Number of unemployment spells over the period		-0.001085***		-0.051008***
		(0.000242)		(0.001238)
PBD of the former right		-0.000007***		-0.000256***
		(0.000002)		(0.000010)
Remaining PBD from former right		0.000003*		0.000311***
		(0.000002)		(0.000010)
Observations	64268	63425	103102	73651

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

NOTE: This table regresses the probability of being a complier and of taking up the OR on the sample of eligible workers under 20€, on a set of observable individual and right characteristics. A probit model has been used, and marginal effects are reported. The skill variable has a reversed scale, meaning that a negative coefficient implies that an increase in the level of skills has a positive impact on the dependent variable.

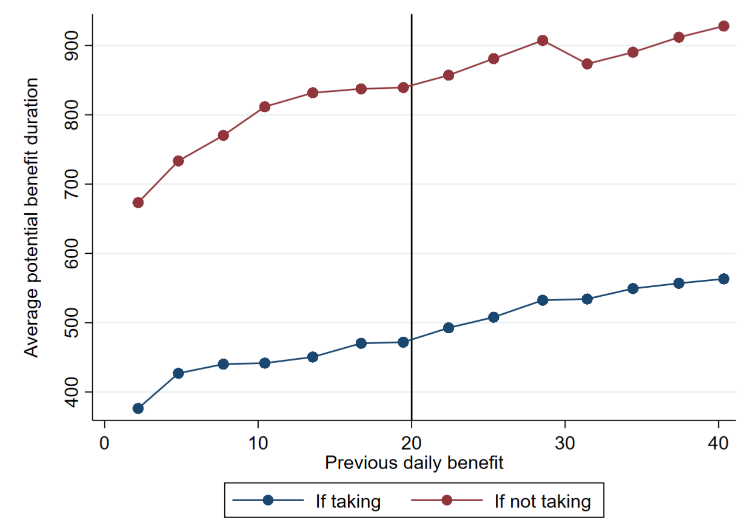
Figure F1: Average daily benefit for takers



SOURCE: UI data (FNA)

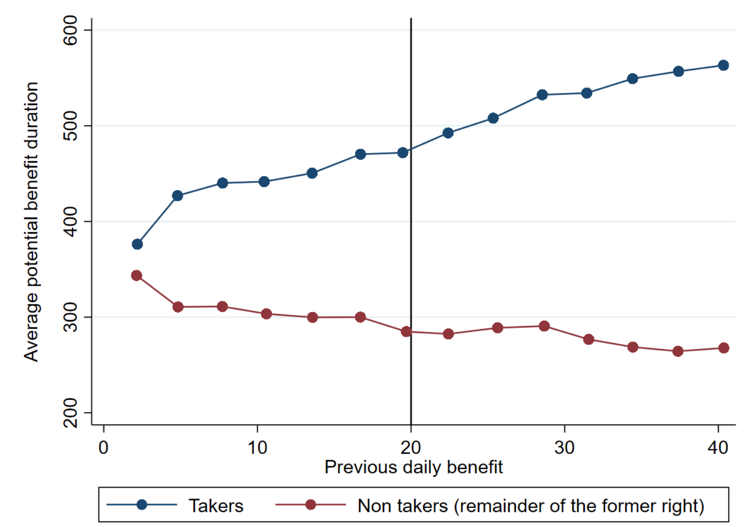
NOTE: This figure plots the average daily benefit as a function of the daily benefit of the former right for takers and nontakers. Overall, it shows a positive relationship, as there is a correlation between past and current earnings. Even for nontakers, the current daily benefit is, on average, higher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

Figure F2: Average PBD for takers if taking up or not taking up



SOURCE: UI data (FNA)
NOTE: This figure plots the average potential benefit duration as a function of the daily benefit of the former right for takers. I compute the potential PBD they would have had if they had not taken up the OR. Both lines follow the same pattern. Although the average PBD duration is much higher in the "if not taking" scenario, takers are still entitled to a long duration in absolute terms.

Figure F3: Average PBD without taking into account recharging



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

G Prediction of unemployment duration

The prediction of unemployment duration draws on a large number of covariates and observations. Different models have been tested: first, parametric models have been used, such as OLS, Poisson model to account for the fact that the dependent variable is positive, zero-inflated Poisson model to account for the fact that the dependent variable is skewed toward zero, and negative binomial regression to account for the fact that the dependent variable is positive and overdispersed. Second, I implemented a random forest algorithm, making the different parameters vary. The choice of the final model, a random forest with 2,000 iterations and a maximum depth of 50,[56] has been guided by the comparison of the root mean squared error in the training and test samples (accounting respectively for 80% and 20% of total sample). Because the ultimate goal is the an out-of-sample prediction, the goodness-of-fit in the test sample has been prioritized. Table G1 shows root mean squared error estimates for all models, both computed in the training and test samples. We observe that the in-sample fit greatly improves using machine learning methods, but not at the expense of out-of-sample fit. The final two models perform almost exactly the same, and are the preferred ones both in terms of in-sample and out-of-sample goodness-of-fit. The last one has been preferred for time-of-computation reasons.

[56] I observe that a higher number of iterations was not improving the in-sample and out-of-sample fit.

Table G1: Root mean squared error of models for the prediction of unemployment duration

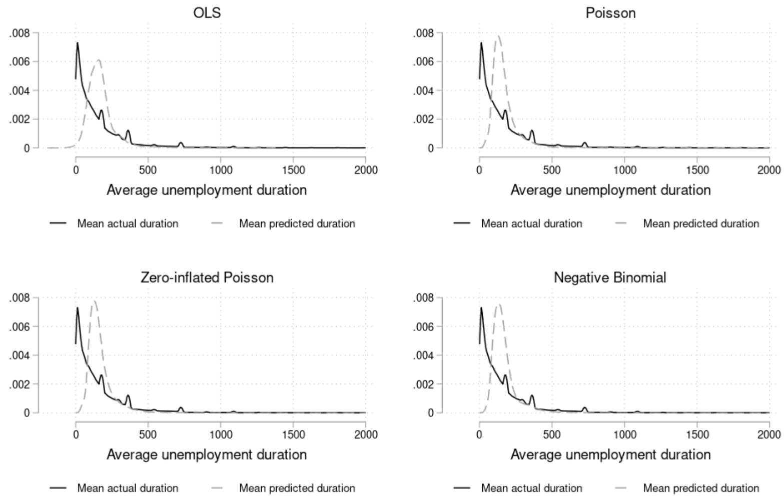
	Training sample	Test sample
OLS	228.707	266.642
Poisson	226.906	229.289
Zero-Inflated Poisson	226.931	229.295
Negative Binomial	230.362	231.726
Random Forest (Iter=10, Max Depth=50)	102.389	236.684
Random Forest (Iter=200, Max Depth=50)	86.750	226.161
Random Forest (Iter=2000, Max Depth=1000)	86.154	225.612
Random Forest (Iter=2000, Max Depth=50)	86.193	225.668

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the root mean squared error computed in the training sample (80% of the sample) and in the test sample (20% of the sample) for each model. For all models, included covariates are age, gender, skill level (6 items), education level (10 items), tenure in past job, wage, sought occupation (81 items), working hours, separation motive (8 items), region (31 items), sector of activity (21 items), number of children, marital status, month and year of separation, occupation in past job (81 items), firm size, plant size, number of previous unemployment rights, daily benefit, potential benefit duration, average benefit level, and duration based on previous unemployment rights. The regression has been made on jobseekers similar to those under study who were unemployed during the 2 years preceding the implementation of the OR (April 2013–March 2015). All variables related to the UI right or to past job or firm are those corresponding to the previous UI right that was not exhausted, as the information is available for all eligible jobseekers, whereas information on the potential new right is available only for takers, and therefore cannot be used for the prediction.

Figures G1 and G2 show the distribution of the actual and predicted unemployment duration for each model. Among the parametric ones, the negative binomial prediction fits better low values and has a larger variance. This is even more the case when we examine non-parametric models, in particular when the number of iterations increases. Both the visual inspection of the distributions and the RMSE point to random forest performing better at predicting unemployment duration.

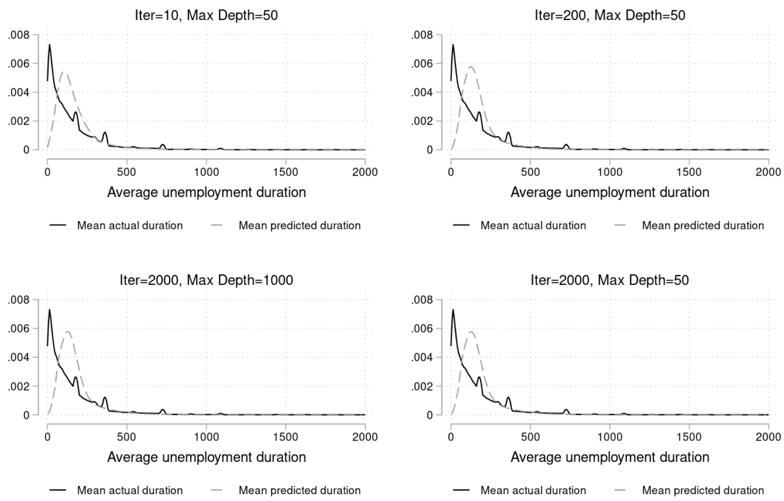
As a robustness check, I also report the prediction for takers and nontakers under 20€ using a flexible OLS (Table G2). If the level of unemployment duration is lower in absolute terms than in my main model, the difference between takers and nontakers goes in the same direction and is of the same order of magnitude (10% of the predicted duration of takers).

Figure G1: Actual and predicted unemployment duration density - Parametric models



SOURCE: UI data (FNA).
NOTE: This graph compares the actual and predicted unemployment duration densities for different prediction models. The Poisson model improves on the OLS because it does not predict negative values. The zero-inflated Poisson does slightly better at predicting very small values, while the Negative Binomial regression predicts duration with a larger variance.

Figure G2: Actual and predicted unemployment duration density - Non-Parametric models



SOURCE: UI data (FNA).
NOTE: This graph compares the actual and predicted unemployment duration densities for different prediction models. The distribution is very similar throughout the different models.

Table G2: Predicted unemployment duration by take-up

	Controls	Takers	Non takers	Difference (3) - (2)
Predicted paid unemployment spell duration	169.53	152.29	167.86	15.57***
				(0.397)
Observations	224,169	24,136	96,779	120,915

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The table presents the average predicted unemployment duration for the group of controls, takers and nontakers below the 20€ cutoff. Unemployment duration has been predicted using information on age, gender, skill level (6 items), education level (10 items), tenure in past job, wage, sought occupation (81 items), working hours, separation motive (8 items), region (31 items), sector of activity (21 items), number of children, marital status, month and year of separation, occupation in past job (81 items), firm size, plant size, number of previous unemployment rights, daily benefit, potential benefit duration, average benefit level, and duration based on previous unemployment rights. The regression has been made on jobseekers similar to those under study who were unemployed during the 2 years preceding the implementation of the OR (April 2013–March 2015). The coefficients are reused to compute the predicted unemployment duration for eligible jobseekers. All variables related to the UI right or to past job or firm are those corresponding to the previous UI right that was not exhausted, as the information is available for all eligible jobseekers, whereas information on the potential new right is available only for takers, and therefore cannot be used for the prediction.

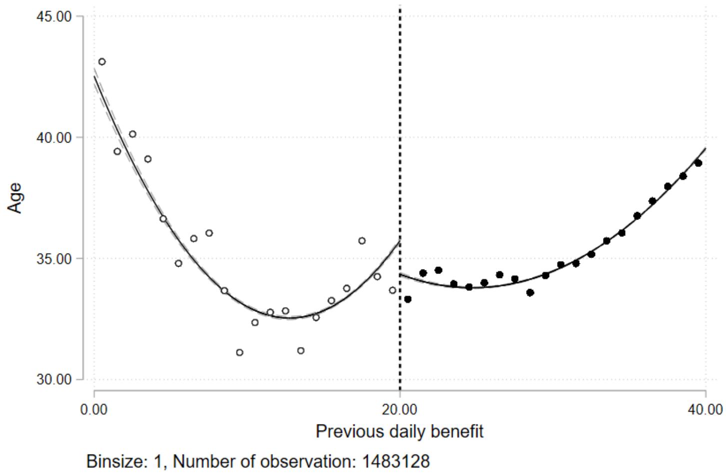
Table G3: Predicted unemployment duration by take-up for workers starting their spell after April 2016

	Controls	Takers	Non takers	Difference (3) - (2)
Predicted paid unemployment spell duration	176.03	253.738	291.760	38.022***
				(1.334)
Observations	468382	35104	101715	136819

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The table presents the average predicted unemployment duration for the group of controls, takers and nontakers below the 20€ cutoff, who have started their spell after April 2016 when it is more likely that jobseekers are informed about the OR scheme. Unemployment duration has been predicted using information on age, gender, skill level (6 items), education level (10 items), tenure in past job, wage, sought occupation (81 items), working hours, separation motive (8 items), region (31 items), sector of activity (21 items), number of children, marital status, month and year of separation, occupation in past job (81 items), firm size, plant size, number of previous unemployment rights, daily benefit, potential benefit duration, average benefit level, and duration based on previous unemployment rights. The regression has been made on jobseekers similar to those under study who were unemployed during the 2 years preceding the implementation of the OR (April 2013–March 2015). The coefficients are reused to compute the predicted unemployment duration for eligible jobseekers. All variables related to the UI right or to past job or firm are those corresponding to the previous UI right that was not exhausted, as the information is available for all eligible jobseekers, whereas information on the potential new right is available only for takers, and therefore cannot be used for the prediction.

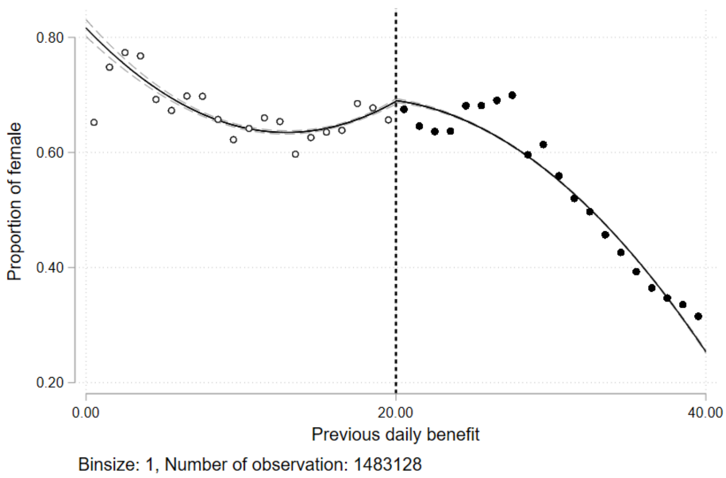
H Assumptions of the RDD

Figure H1: Age distribution



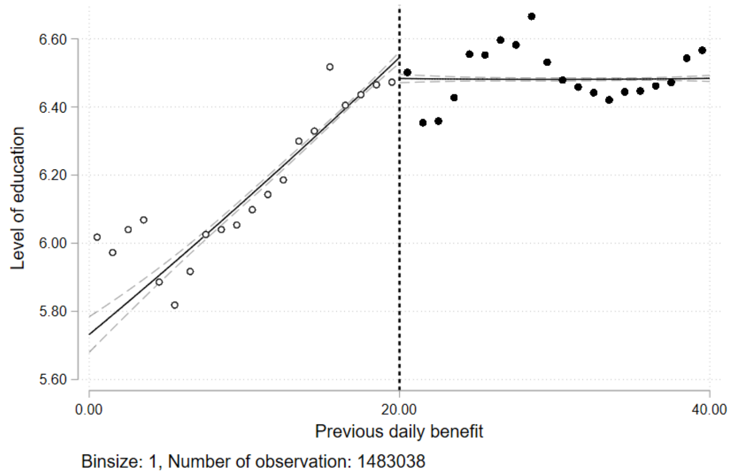
SOURCE: UI data (FNA).
NOTE: This graph plots average age as a function of the level of the former daily benefit (second-order polynomial). It tests the assumption of continuity in the distribution of covariates at the threshold. Reassuringly, we do not see any discontinuity at the 20€ threshold.

Figure H2: Distribution of proportion of women



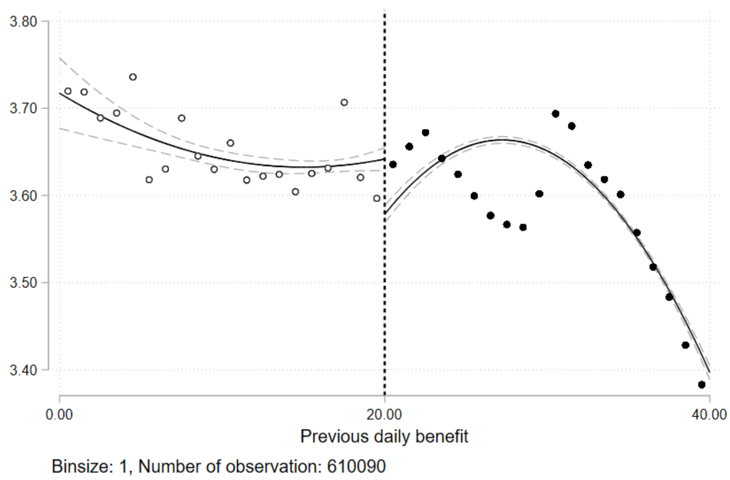
SOURCE: UI data (FNA).
NOTE: This graph plots the proportion of women as a function of the level of the former daily benefit (second-order polynomial). It tests the assumption of continuity in the distribution of covariates at the threshold. Reassuringly, we do not see any discontinuity at the 20€ threshold.

Figure H3: Level of education distribution



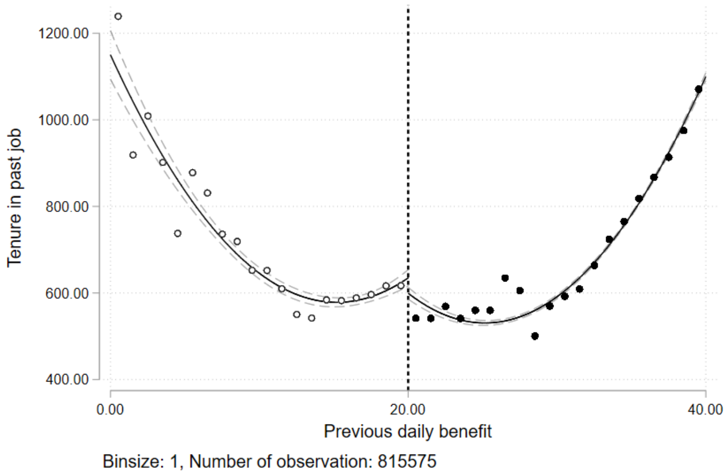
SOURCE: UI data (FNA).
NOTE: This graph plots the average level of education as a function of the level of the former daily benefit (second-order polynomial). It tests the assumption of continuity in the distribution of covariates at the threshold. Reassuringly, we do not see any discontinuity at the 20€ threshold.

Figure H4: Skill distribution



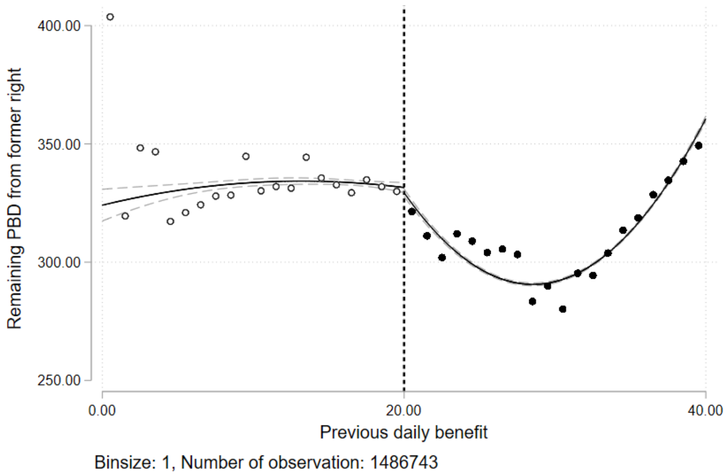
SOURCE: UI data (FNA).
NOTE: This graph plots average skill level as a function of the level of the former daily benefit (second-order polynomial). It tests the assumption of continuity in the distribution of covariates at the threshold. Reassuringly, we do not see any discontinuity at the 20€ threshold.

Figure H5: Tenure at past job distribution



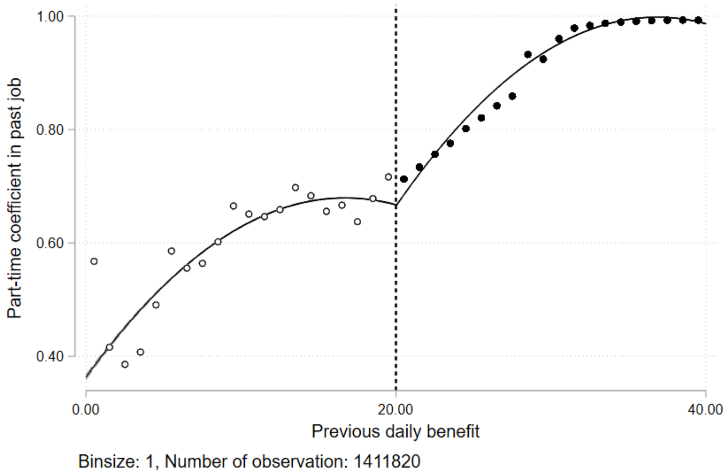
SOURCE: UI data (FNA).
NOTE: This graph plots average tenure at past job as a function of the level of the former daily benefit (second-order polynomial). It tests the assumption of continuity in the distribution of covariates at the threshold. Reassuringly, we do not see any discontinuity at the 20€ threshold.

Figure H6: Remaining PBD distribution



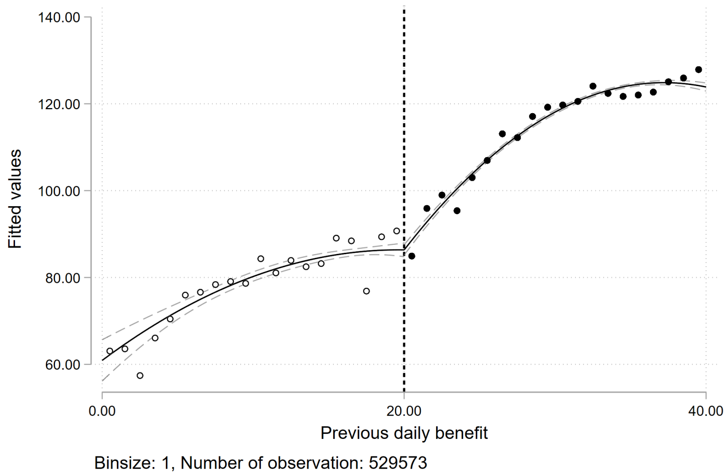
SOURCE: UI data (FNA).
NOTE: This graph plots average remaining PBD as a function of the level of the former daily benefit (second-order polynomial). It tests the assumption of continuity in the distribution of covariates at the threshold. Reassuringly, we do not see any discontinuity at the 20€ threshold.

Figure H7: Part-time coefficient distribution



SOURCE: UI data (FNA).
NOTE: This graph plots average working-hour coefficient as a function of the level of the former daily benefit (second-order polynomial). The working-hour coefficient ranges from 0 to 1 and is equal to one in case of full-time employment. It tests the assumption of continuity in the distribution of covariates at the threshold. Reassuringly, we do not see any discontinuity at the 20€ threshold.

Figure H8: Predicted UI duration distribution



SOURCE: UI data (FNA).
NOTE: This graph plots average predicted unemployment duration as a function of the level of the former daily benefit (second-order polynomial). It tests the assumption of continuity in the distribution of covariates at the threshold. Predicted unemployment duration is computed using only pre-determined variables, and excluding any UI entitlement-related variables that could affect unemployment duration. Reassuringly, we do not see any discontinuity at the 20€ threshold.

I Additional tables and figures

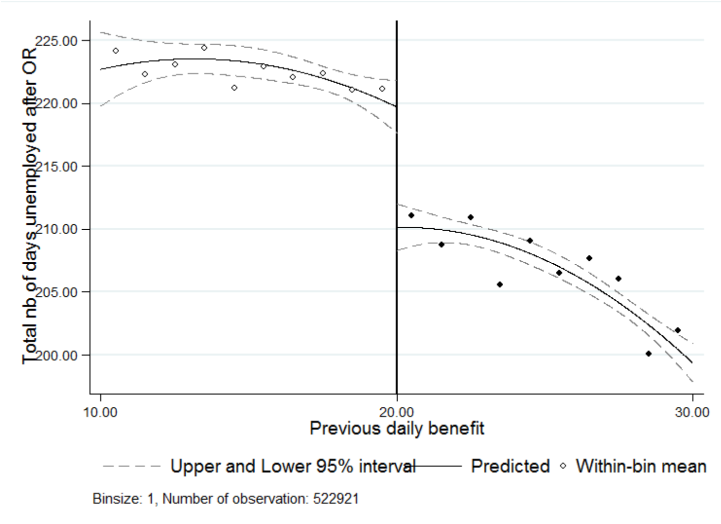
Table I1: Impact of paid unemployment spell duration with different bandwidths

	Paid unemployment spell duration							
	Local linear polynomial				Local quadratic polynomial			
RD_Estimate	208.5***	-939.3	178.1***	163.3***	155.3***	759.9	145.1***	202.9***
	(31.71)	(1140.96)	(35.98)	(27.88)	(34.90)	(1546.81)	(31.14)	(39.13)
Robust 95% CI	[111.838 ; 260.196]	[-1665.932 ; 4907.587]	[44.776 ; 262.732]	[94.806 ; 262.23]	[75.238 ; 216.045]	[608.051 ; 8725.98]	[81.841 ; 243.847]	[-39.16 ; 162.104]
Observations	1888093	1888093	1888093	1888093	1888093	1888093	1888093	1888093
Order Loc. Poly. (p)	1	1	1	1	2	2	2	2
Control outcome mean	92.24	92.24	92.24	92.24	92.24	92.24	92.24	92.24
Bandwidth	Optimal bandwith (OB)	OB × .5	OB × 1.5	OB × 2	Optimal bandwith (OB)	OB × .5	OB × 1.5	OB × 2

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

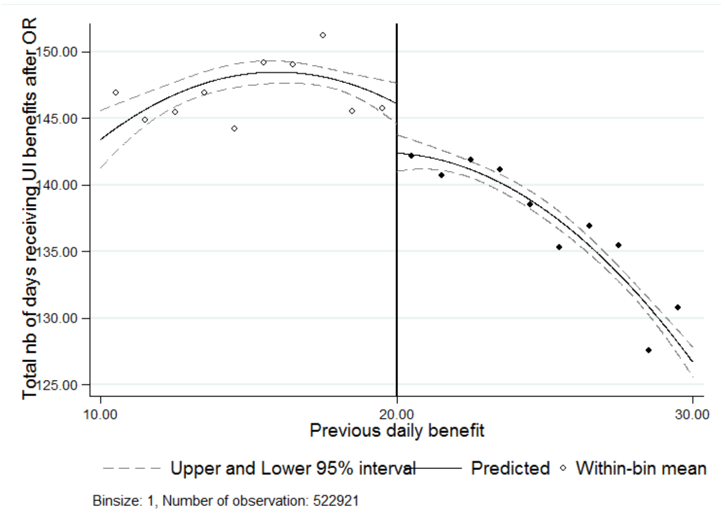
NOTE: This table reproduces the main result on the impact of the OR on the duration of the subsequent paid unemployment spell, using different bandwidths. The RD is estimated with the optimal bandwidth (MSE criterion), and the optimal bandwidth (OB) multiplied by 0.5, 1.5 and 2. Local linear and quadratic polynomials are used. The results are overall similar, but not significant when choosing a bandwidth equivalent to half of the optimal one.

Figure I1: Total number of days unemployed after the exercise of the option right



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

Figure I2: Total number of days on UI benefits after the exercise of the option right



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

Table I2: Probability of exhausting the UI right

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

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

Table I3: Impact of the option right on labor income earned over the spell

	Labour income earned over the UI spell			
RD_Estimate	8918.2*** (2100.18)	7713.2*** (2301.79)	10771.2*** (2843.83)	9229.8*** (2476.81)
Robust 95% CI	[4885.951 ; 14272.303]	[2724.9 ; 12667.919]	[5592.068 ; 17788.017]	[4825.408 ; 15335.848]
Observations	766113	766113	766065	766065
Order Loc. Poly. (p)	1	2	1	2
Control outcome mean	1465.84	1465.84	1465.84	1465.84
Covariates	No	No	Yes	Yes

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the RD estimates on the labor income earned during the subsequent unemployment spell, while remaining registered as unemployed. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector, and local linear and quadratic polynomials are used, with and without controls. The control outcome mean measures the mean of the outcome variable right above the cutoff, for workers with a previous daily benefit between 20€ and 30€. The OR increases the labor income earned during the spell by about 8,000€, depending on the specification. This result has been computed on the labor income conditional on having worked during the spell, which is itself endogenous to the OR. Therefore, we cannot exclude that there is a composition effect mixing with the pure effect on labor income.

Table I4: Impact of the option right on the number of hours worked during a spell

	Number of hours worked over the UI spell			
RD_Estimate	700.2***	838.5***	746.3***	1013.0***
	(150.16)	(257.00)	(225.43)	(346.77)
Robust 95% CI	[417.482 ; 1105.52]	[232.625 ; 1326.769]	[397.005 ; 1340.172]	[247.225 ; 1768.913]
Observations	766113	766113	766065	766065
Order Loc. Poly. (p)	1	2	1	2
Control outcome mean	131.46	131.46	131.46	131.46
Covariates	No	No	Yes	Yes

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the RD estimates on the number of hours worked during the subsequent unemployment spell, while staying registered as unemployed. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector, and local linear and quadratic polynomials are used, with and without controls. The control outcome mean measures the mean of the outcome variable right above the cutoff, for workers with a previous daily benefit between 20€ and 30€. The OR increases the number of hours worked during the spell by about 800 hours, depending on the specification. This result has been computed on the number of hours conditional on having worked during the spell, which is itself endogenous to the OR. Therefore, we cannot exclude that there is a composition effect mixing with the pure effect on the number of hours worked.

Table I5: Impact of the option right on the daily earnings over the subsequent spell

	Daily earnings			
RD_Estimate	211.1***	214.1***	307.3***	177.8***
	(67.28)	(61.98)	(98.38)	(66.53)
Robust 95% CI	[75.799 ; 371.121]	[94.175 ; 360.654]	[111.295 ; 527.484]	[38.649 ; 314.652]
Observations	1859860	1859860	1854187	1854187
Order Loc. Poly. (p)	1	2	1	2
Control outcome mean	31.12	31.12	31.12	31.12
Covariates	No	No	Yes	Yes

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports the RD estimates on the average level of daily earnings over the subsequent unemployment spell. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector, and local linear and quadratic polynomials are used, with and without controls. The control outcome mean measures the mean of the outcome variable right above the cutoff, for workers with a previous daily benefit between 20€ and 30€. Daily earnings are computed by dividing the sum of UI benefits and earnings from work by the duration of the UI spell. Earnings from work are measured only if the person stays registered as unemployed, which is likely for the type of short unemployment spells that are not long enough to define the end of the UI spell. The duration of the UI spell is measured as the time elapsed between the starting and the ending dates of the spell, including potential days of interruptions.

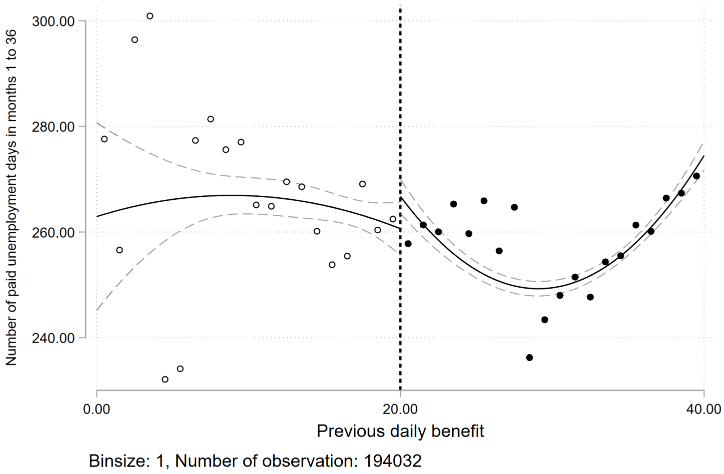
Table I6: Impact of OR on the total number of days unemployed over two to three years

	Number of paid unemployment days				Number of registered unemployment days			
	In months 1 to 24		In months 1 to 36		In months 1 to 24		In months 1 to 36	
RD_Estimate	8.0 (74.29)	-112.0 (91.97)	122.9 (79.06)	70.7 (79.49)	-16.7 (98.46)	-120.4 (124.43)	270.9*** (100.64)	205.8* (115.45)
Robust 95% CI	[-176.491 ; 158.241]	[-315.398 ; 71.541]	[-79.335 ; 278.841]	[-113.843 ; 230.48]	[-290.646 ; 140.617]	[-430.981 ; 122.45]	[-24.159 ; 426.697]	[-82.49 ; 418.65]
Observations	650407	650407	251401	251401	650407	650407	251401	251401
Order Loc. Poly. (p)	1	2	1	2	1	2	1	2
Control outcome mean	217.1	217.1	255.1	255.1	350.3	350.3	380.9	380.9
Covariates	No	No	No	No	No	No	No	No

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

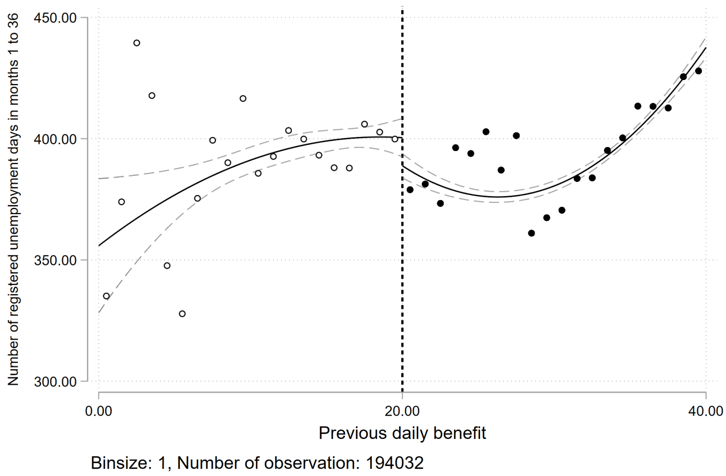
NOTE: This table reports the RD regressions on the total number of days on benefits or registered as unemployed over the 24 or 36 months following the start of the initial spell where jobseekers are offered the option right. The regression is estimated over the period between October 2014 and February 2018, on the sample of jobseekers who started their spell sufficiently early in the period under study to be observed after 24 or 36 months, making the sample size drop mechanically. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector, and local linear and quadratic polynomials are used, with and without controls. The control outcome mean measures the mean of the outcome variable right above the cutoff, for workers with a previous daily benefit between 20€ and 30€.

Figure I3: Impact on the total number of days on UI benefits between months 1 and 36



SOURCE: UI data (FNA).
NOTE: This graph reports the second-stage relationship between the running variable, which is the former daily benefit, and the total number of days on UI benefits over the three years following the choice of exercising or not the OR (second-order polynomial). The sample has been restricted to jobseekers starting their spell sufficiently early to be observed over three years.

Figure I4: Impact on the total number of days registered as unemployed between months 1 and 36



SOURCE: UI data (FNA).
NOTE: This graph reports the second-stage relationship between the running variable, which is the former daily benefit, and the total number of days registered as unemployed over the three years following the choice of exercising or not the OR (second-order polynomial). The sample has been restricted to jobseekers starting their spell sufficiently early to be observed over three years.

Table I7: Impact on the probability of being in paid unemployment

	Probability of being in paid unemployment											
	In month 3			In month 6			In month 12			In month 24		
Taker=1	0.182***	0.106*	0.123***	0.140***	0.068	0.070*	0.024	-0.029	-0.015	-0.006	0.161	-0.000
	(0.0514)	(0.0561)	(0.0362)	(0.0415)	(0.0596)	(0.0395)	(0.0564)	(0.0546)	(0.0518)	(0.0184)	(0.5355)	(0.0106)
Observations	213773	209485	299629	277736	217824	300174	180755	181714	195848	11481	14412	19459
Order Loc. Poly. (p)	1	2	3	1	2	3	1	2	3	1	2	3
Control outcome mean	.549	.549	.549	.384	.384	.384	.228	.228	.228	.002	.002	.002
Covariates	No	No	No	No	No	No	No	No	No	No	No	No

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

NOTE: This table reports the impact of the OR on the probability of being in paid unemployment at different times following the decision whether to exercise the OR, using a bivariate probit regression. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector, and linear, quadratic and cubic specifications are used. The control outcome mean measures the mean of the outcome variable right above the cutoff, for workers with a previous daily benefit between 20€ and 30€. It shows that the main effect occurs in the short term.

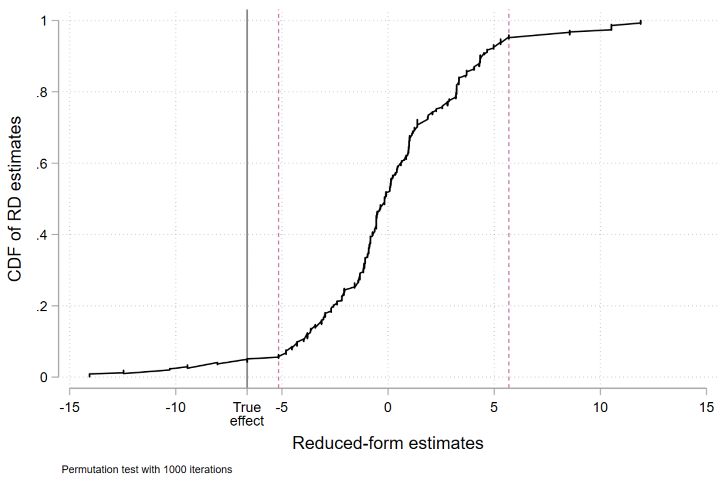
Table I8: Impact on the probability of being registered as unemployed

	Probability of being registered as unemployed											
	In month 3			In month 6			In month 12			In month 24		
Taker=1	0.173***	0.129***	0.184***	0.162***	0.082	0.167***	0.025	-0.115**	-0.103**	-0.010	0.002	-0.004
	(0.0346)	(0.0317)	(0.0176)	(0.0558)	(0.0507)	(0.0384)	(0.0688)	(0.0559)	(0.0523)	(0.0207)	(0.0363)	(0.0090)
Observations	205119	227010	330930	191054	216171	261641	174038	172639	189891	11302	14303	19342
Order Loc. Poly. (p)	1	2	3	1	2	3	1	2	3	1	2	3
Control outcome mean	.783	.783	.783	.627	.627	.627	.429	.429	.429	.004	.004	.004
Covariates	No	No	No	No	No	No	No	No	No	No	No	No

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

NOTE: This table reports the impact of the OR on the probability of being registered as unemployed at different times following the decision whether to exercise the OR, using a bivariate probit regression. Bandwidth has been computed using the mean squared error (MSE) optimal bandwidth selector, and linear, quadratic and cubic specifications are used. The control outcome mean measures the mean of the outcome variable right above the cutoff, for workers with a previous daily benefit between 20€ and 30€. It shows that the main effect occurs in the short term.

Figure I5: Permutation test for the impact on the duration of the paid unemployment spell



SOURCE: UI data (FNA).
NOTE: This permutation test randomly selects a cutoff value between 0 and 100 and computes the corresponding RD reduced-form estimate (1,000 replications). The two dashed lines indicates values 0.05 and 0.95 of the CDF. We observe that the true effect is outside this confidence interval, confirming that it the OR has a significant effect on unemployment duration.