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Abstract

The literature on unemployment insurance (UI) has primarily focused on its impact on unemployment outflows. Using administrative data and a discontinuity in UI benefits at a tenure threshold, I show that inflows also respond to the extent of UI. Based on bunching estimates, the elasticity of the employment duration to UI benefits is estimated to be 0.014, implying that 10% of layoffs in an 8-week window before the threshold are delayed. Examining the mechanisms, the evidence suggests that employers and employees bargain over contract termination. Taken together, employers and employees show sophisticated reactions to UI with effects on employment duration.

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1 Introduction

Unemployment insurance (UI) programs balance the insurance value of higher benefits against the adverse effects of a more generous UI. Beyond the pure monetary cost, the adverse effects of UI benefits arise from the behavioral response both from workers and employers, pre, post, and during unemployment. The literature has mainly focused on the UI disincentive effect on the job search effort of workers during unemployment (see Schmieder and Von Wachter (2016) for a review). However, little is known about the impact of UI on the behavior of employed workers and firms. The effect of UI on outcomes during employment is a relevant behavioral response usually not considered in the optimal UI framework. Ignoring it could lead to mismeasurement of the overall effect of higher UI benefits on employment duration.

In this paper, I investigate how a UI program in France affects the behavior of employers and employed workers. I focus on its effect on the timing of the redundancy decision, that is, when a firm facing economic difficulties decides to lay off part or the totality of its workforce. Redundancy decisions in France provide a natural experiment to study this behavioral response because the level of benefits offered to laid-off workers discontinuously jumps above a tenure threshold. This notch in the level of UI benefits allows me to measure whether the official contract termination is postponed when it benefits both employers and employees. Redundancies are also cases where the employers' decisions are constrained by the firm's economic difficulties. The measure of the strategic scheduling of contract termination in this context would likely provide a lower bound on the same behavioral response observed in other cases.

The main contribution of this paper is to highlight the existence of strategic behaviors from employers and employees in response to UI incentives, where UI is used to maximize the surplus from separation. Although the magnitude of the rescheduling is limited, proving the empirical existence of such behaviors and accurately measuring them have rarely been conducted in the literature. It is a particularly relevant behavioral response to identify, to the extent that similar discontinuities are found everywhere in tax and benefit schemes.[1] Spinnewijn (2020) has recently underlined the importance of the within-country variation in the level of UI benefits, and the need to account for it in the traditional trade-off between insurance and incentives. With respect to previous literature, I reveal that these strategic behaviors do not only affect the incidence of layoff, but another margin of optimization, that is the timing of the layoff in a context where the layoff decision is constrained.[2] Besides the average response that I measure, I explore its

[1] For example, in most European countries and in the US, UI benefits are received for a limited duration. After the exhaustion point, jobseekers are generally entitled to unemployment assistance benefits. In practical terms, it translates into a discontinuous decrease in the average replacement rate. It has been shown that flows out of unemployment react strongly at the discontinuity, both in terms of exit from registered unemployment, and re-entry intro employment (Card et al., 2007).

[2] As further detailed in the institutional section, economic layoffs are heavily regulated by the admin-

heterogeneity to shed light on the mechanisms that explain this response. This paper also contributes to insights into the employer–employee bargaining black box by analyzing the individual and firm-level determinants of collusion between the employees and the firm. I find that the bargaining is not necessarily supported by collective representation bodies as measured in my data, but is mostly individual, driven by more educated and skilled workers.

I focus on a French unemployment program targeted at laid-off workers called the *Contrat de sécurisation professionnelle* (CSP), which was implemented in 2005,[3] in addition to the standard UI benefits.[4] It introduced a nonlinearity in the compensation amount at a tenure threshold. That threshold went from two years to one year in 2011. Laid-off workers having completed one year (respectively two years before 2011) of tenure are entitled to a specific benefit equivalent to 80% of their previous gross earnings.[5] Those under this threshold only receive the standard benefit that represents 57.4% to 75% of previous gross earnings, depending on the past wage. This jump in the level of UI benefits incentivizes workers to reach the tenure threshold before being laid off.

This paper documents sizable bunching of worker layoffs right after the one-year tenure cutoff where UI benefits change abruptly. I argue that it is the incentives created by the UI that explain the observed bunching. This result is robust to several tests ruling out alternative hypotheses and to the use of a difference-in-bunching method (Brown, 2013).[6] To my knowledge, a setting with a two-sided negotiation—whereas the financial incentives lie mainly on the workers' side—has not yet been modeled in the bunching literature. I use a theoretical framework to clarify the cost and benefits of extending the employment spell on each side and to accommodate the key empirical findings. On the employee's side, the level of expected UI benefits affects whether the employee negotiates the termination date. In the presence of uncertainty on the demand,[7] the bargaining on contract duration may occur at the time of separation rather than at the time of hiring. On the firm's side, the employer may consider additional UI benefits to offset the psychological and social costs of the redundancy to the worker. A significant financial compensation is likely to deter workers from harming their employer's reputation or from

istration and need to be highly motivated by the economic difficulties of the firm.

[3]Law №2005-32 of January 18, 2005 - art. 74 JORF January 19, 2005.

[4]The UI scheme in France is characterized by a main insurance benefit, the *Allocation de retour à l'emploi* (ARE), designed for all workers having lost their jobs unintentionally, and fulfilling other nonrestrictive eligibility conditions. Laid-off workers account for a relatively small share of contract terminations (2.6% of all registrations to UI according to the French Unemployment Agency), but they represent about 7% of flows into paid unemployment. They stay unemployed longer than the average worker, and, therefore, represent 10% of the stock of unemployed people on benefits at any given time.

[5]It translates into virtually 100% of previous net earnings.

[6]The difference-in-bunching method uses the pre-CSP distribution of tenure at layoff (September, 2009–August, 2011) as a counterfactual to isolate the pure effect of the UI program, regardless of other labor regulations or social norms that could trigger some bunching unrelated to UI.

[7]Economic layoffs, by their nature, make this hypothesis more realistic.

claiming extralegal severance payment.[8] This reduction in the layoff cost can compensate for maintaining the wage for a few additional days, whereas the employer does not support the direct cost of the CSP.[9] French legislation defines minimum time periods between each step of the redundancy procedure. The scenario supported by this paper is that, through these minimum time periods, the employer has some flexibility to strategically extend the length of the procedure, and, thus, the length of the employment spell. Finally, I investigate the heterogeneity in bunching by a tentative decomposition of the factors explaining bunching into incentives, preferences, and ability to negotiate. To disentangle the role of financial incentives from other explanatory factors, I rely on previous results from the bunching literature to compute a reduced-form estimate of the employer's and employee's response to UI benefits in terms of contract length. By scaling the response to the magnitude of the jump in the replacement rate, the elasticity estimate serves as a metric neutralizing the variation in financial incentives and allowing other sources of heterogeneity to be explored. Although the level of financial incentives is the main explanatory variable, the remaining variation in bunching is primarily driven by the level of education and age when controlling for the firm's fixed effects.

This paper relates to several strands of the literature. First, it relates to the optimal unemployment insurance literature, centered around the optimality condition that equalizes the moral hazard cost of transferring one euro from the employed to the unemployed state, to the benefits of such a transfer, in terms of consumption smoothing (Baily, 1978; Chetty, 2006). Many papers have estimated the impact of UI benefits on unemployment duration (Van Ours and Vodopivec, 2006; Lalive et al., 2006; Lalive, 2007; Lalive et al., 2015; Landais, 2015; Kolsrud et al., 2018).[10] I show the existence of an effect of UI benefits on employers' and employees' behavior and estimate its magnitude. This result reveals that moral hazard while on the job is another key parameter to understand the impact of UI design on the labor market.

Although most works have tended to emphasize the effect of UI design on unemployment outflows, a few papers have empirically investigated the impact of UI on layoffs. Beyond the seminal papers of Feldstein (1976) and Baily (1977), some authors have studied the experience rating scheme in the US as a cost for adjusting the workforce of firms (Topel, 1983; Anderson, 1993; Anderson and Meyer, 2000). UI benefits can affect layoffs through two main channels: the increase in benefits changes the value of unemployment

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

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

[10] Although it is not the core of the paper, I show evidence of an unemployment duration response to the increase in the level of UI benefits. I then provide a comprehensive assessment of the UI benefits impact on both unemployment inflows and outflows.

for workers and possibly for employers; the way these benefits are financed can subsidize unemployment if experience rating is imperfect. Whereas these papers focus on the second mechanism, I explore the first. I reveal that even in a context where no experience rating is implemented, meaning that no variation in the UI-related cost of layoff in observed across firms and workers, the level of benefits still influences separation behaviors. I, therefore, exploit a salient, exogenous, and precisely measured change in UI incentives[11] to show how it affects unemployment inflows. Surprisingly, since the 1980's and 1990's, this question has been overlooked, until a recent wave of new papers: these papers generally focus either on the eligibility criteria to UI benefits (Christofides and McKenna, 1995; Green and Riddell, 1997; Jurajda, 2002; Rebollo-Sanz, 2012; Van Doornik et al., 2018; Albanese et al., 2020) or on changes in the potential benefit duration of older workers (Winter-Ebmer, 2003; Jäger et al., 2019; Tuit and van Ours, 2010; Baguelin, 2016; Baguelin and Remillon, 2014). They show that the separation rate is positively affected by these two parameters through strategic scheduling of contract termination according to UI entitlements. I contribute to this new evidence in three ways: (i) I widen the scope of previous studies by not focusing solely on older workers, whose labor supply decision is likely to be particular, and influenced by retirement considerations, or on workers on the eligibility margin;[12] (ii) the richness of the data allows me to thoroughly explore and decompose the driving factors of the collusion, which is crucial in terms of public policy implications. If the main optimization behavior is observed on the workers' side, the best policy response could be to reduce the generosity of UI benefits, whereas the strategic use of UI may be internalized by employers through experience rating; and (iii) most of existing papers measure an impact on the probability to exit employment, but do not disentangle what comes from the extensive or intensive margin. I show that the increase in UI benefits does not trigger layoffs that would not have occurred absent the notch, but a retiming of layoffs.[13] I am therefore able to highlight another margin of optimization from employers in a context where their incentives are even less straightforward, because the layoff decision has made it clear that the match was no longer profitable. Closest to my paper, Citino et al. (2019) exploit a setting where the PBD of laid-off workers depends on their age at layoff. If they identify manipulation, they are more interested in distinguishing the mechanical effect of a longer benefit duration from the behavioral effect of a decreased search effort on unemployment duration, despite manipulation. My analysis differs to the extent that it focuses on the analysis of the consequences manipulation has

[11] In contrast, the experience rating subsidy to unemployment can be difficult to compute at the firm level, and its cross-state variation potentially endogenous.

[12] In my sample, workers have at least one year of tenure in the firm, and often a longer and more stable work history than workers at the eligibility margin. They are therefore more representative of the average worker. Khoury et al. (2020) provide an analysis of the response of inflows to and outflows from unemployment to the UI eligibility criteria in France.

[13] I provide in the empirical part a complementary analysis on the extensive margin impact of the UI program indicating no effect on the number of layoffs.

on employment rather than unemployment duration, and the drivers of the collusion.[14] In this framework, UI is used as a way to soften the conditions of the redundancy, thereby leading to several types of inefficiencies.[15]

Tax and transfer policies often lead to the creation of nonlinearities in the budget set of individuals. These kinks or notches may create a bunch at this point of the distribution of earnings, that has been used to estimate labor supply responses (see Kleven (2016) for a review). Important optimization frictions have already been highlighted by several papers (Chetty et al., 2011; Kleven and Waseem, 2013), that attenuate the magnitude of bunching as predicted by the standard labor supply model. I characterize a new source of frictions, namely bargaining frictions, which make the adjustment of contract length not necessarily coincide with what would be optimal for the worker. Therefore, one contribution of this paper is to model bunching in a bargaining context mixing labor supply and demand responses.

Finally, this paper speaks to the literature on wage bargaining. The positive relationship between the presence of work councils and outcomes such as wages, job satisfaction, or employment relationships has been documented (Addison et al., 2004, 2010; Hübler and Jirjahn, 2003; Grund and Schmitt, 2011; Grund et al., 2016). However, less is known about the relationship between the presence of representatives and more individual outcomes, especially when the layoff decision has already been taken, and the worker is close to leaving the firm. Grund and Martin (2017) show that work councils have a positive impact on the incidence of severance payment for a plant closing, but a negative impact for the dismissal of an individual. I provide insights into the employer–employee bargaining black box, by identifying individual and firm-level determinants of bunching. I find evidence that the rescheduling is more a part of an individual than a collective negotiation process. My findings point to a nonsignificant impact of representation institutions on the incidence of bunching, although it is imperfectly measured. This may be explained by the fact that bunching is highly dependent on individual parameters and is not relevant for all laid-off workers.

The remainder of this paper is organized as follows. Section 2 gives an overview of the legislative framework and presents the data, Section 3 provides empirical evidence of bunching and estimates the elasticity of contract duration with respect to the level of UI benefits. Section 4 models the bargaining process and derives some predictions on the heterogeneity in bunching that are taken to the data. Section 5 provides some robustness checks and Section 6 concludes.

[14] I also provide, at the end of the paper, a brief analysis of the effect of higher benefits on unemployment duration for the population of manipulators.

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

2 Institutional Background and Data

The UI program under study is targeted at laid-off workers. In France, as inmost advanced economies, an employer who wants to separate from a permanent worker after the probation period has two main options: dismissal for personal reasons and layoff for economic reasons. The first motive is linked to the behavior of the person dismissed whereas the second motive is justified by economic difficulties faced by the firm. In both cases, the reason must be clearly stated and supported by objective and verifiable facts. The populations concerned by these different separation motives differ to some extent. Laid-off workers work mainly in shrinking industries and are, on average, older, more frequently male, less educated but with a higher wage and compensation duration than the other compensated workers (Unédic, 2015). This is the reason why a specific benefit package called the *Convention de reclassement personnalisée* (CRP) was introduced on April 5, 2005 for laid-off workers, in addition to the main UI benefits.[16] It was in effect during the first period studied in this paper (September 1, 2009–August 31, 2011). It was then transformed into the CSP, which came into effect during the second period of interest (October 1, 2011–September 30, 2014). In this paper, I focus on the CSP and I use the CRP only for comparative purposes.[17] In the following paragraph, the legislative rules regarding the CSP are presented, because the rules that apply to the CSP also apply to the CRP.[18]

2.1 Description of the CSP

In firms with less than 1,000 employees or in compulsory liquidation or receivership (whatever the workforce size), employers are required to offer the CSP to any employee they want to lay off, during the interview prior to layoff or after the last meeting of employees' representatives. To benefit from the CSP, the worker must also meet the standard UI eligibility criteria.[19]

Main features of the CSP – The CSP offers comprehensive and personalized support to help laid-off workers reintegrate into the labor market as soon as possible, and in good conditions. It gives access to career coaching, training, assistance in designing a

[16]This scheme is, to some extent, comparable to the Trade Adjustment Assistance in the US, although eligibility conditions are less restrictive in France.

[17]This choice is justified by the fact that the legislation has been more stable and with clearer incentives during the period the CSP was in place. Moreover, any changes taking place after September 30, 2014 are not considered, particularly the reform of April 1, 2015, which introduced many modifications to the scheme.

[18]The main change being to the tenure criteria to benefit from the higher compensation.

[19]These criteria are the following: (i) having worked at least 122 days or 610 hours within the last 28 months; (ii) not having reached the compulsory retirement age; (iii) living within the territory where the unemployment insurance is applicable; and (iv) being physically able to work. Those criteria also apply to receive the standard benefit (ARE).

professional project, and even psychological support, for a maximum period of 12 months. Besides these aspects, its main characteristic is to introduce a nonlinearity in the compensation amount at one year of tenure.[20] While all laid-off workers are entitled to the coaching and training components of the CSP, only those having completed one year of tenure are entitled to a specific benefit equivalent to 80% of previous gross earnings (which translates into virtually 100% of previous net earnings).[21] This specific benefit is called the *Allocation de sécurisation professionnelle* (ASP). Workers under the tenure threshold only receive the standard benefit, whose replacement rate lies between 57.4% to 75% of previous gross earnings, depending on the past wage. Therefore, the tenure condition does not determine the eligibility to the CSP, but to higher benefits (ASP). Eligible workers are offered the CSP but decide if they accept it or if they just get the standard compensation scheme. Under one year of tenure, if the worker takes up the CSP, he is entitled to the increased counseling and training, but will only receive the standard benefit. Next paragraph describes the whole layoff procedure to understand whether employers and employees have enough flexibility to strategically delay layoffs.

Steps of the layoff procedure – The procedure for redundancies implies several steps, whose number depends on the firm size and the number of workers laid off. It involves meeting and discussing with employees' representatives, when they are present in the firm, and respecting minimum periods of time between each step. The whole procedure is monitored by the Health and Safety Inspection (*Inspection du Travail*).

In the concerned firms, the employer, after having announced the economic layoff plan and discussed it with the employees' representatives, must offer the CSP, individually and in writing, to any eligible worker, either during the interview prior to layoff, or after the last meeting with the employees' representatives, or after the approval of the redundancy plan, if any.[22] The employee has a 21-day period to make his decision: if he refuses, he gets the standard benefit scheme; if he accepts, the work contract terminates at the end of the 21-day period, without any advance notice.[23] As a benchmark, there is a minimum of 22 days between the first layoff notification and the individual meeting where the employee is informed about the CSP (in firms of 11 to 49 employees laying off two to nine of them). If we add up this period to the 21 days the employee has to decide on the CSP, it means that all employees with an initial tenure of 322 days are eligible for a contract extension. This computation provides a lower bound, given that bigger firms may have

[20] This threshold went from two years (under the CRP) to one year (under the CSP) in 2011.

[21] This means that only the replacement rate changes discontinuously at the threshold, as the non-monetary aspects of the CSP are offered for any tenure value.

[22] It is compulsory for employers to inform each worker of the existence and terms of the CSP. Failure to do so results in the payment of a fine. It implies that there is little variation in the level of information among workers, and this should not play a role in the bunching phenomenon.

[23] Regardless of when the worker makes her decision, the contract cannot end before the end of the 21-day period. More generally, the worker has no control over the timing of the various stages.

longer periods between each step, and that these periods may be longer than the legal minimum. A detailed description of the legal layoff timeline is provided in Figure A1 of Appendix A.

While workers have strong incentives to go beyond one year of tenure, the layoff decision and its timing are in the hands of the employer. In theory, the layoff is only motivated by the economic difficulty of the firm, and should affect workers within the enterprise, if not randomly,[24] at least not according to some sharp eligibility thresholds. Two important consequences of the acceptance of the CSP are noted: first, as soon as the 21-day period ends, the worker starts to be compensated without any waiting period or notice;[25] second, for workers accepting the CSP, the breach of the work contract is no longer considered a layoff, but a mutually agreed termination that may imply fewer administrative constraints for the employer in the future.[26] The worker is still entitled to receive severance payments and to appeal to the Labor Court but is much less likely to do so. This reduction in trial risk can be considered a reduction in the layoff cost. The different possibilities and their consequences are summarized in Table A1.

Finally, redundancies can be either collective or individual. Collective redundancies are defined as the layoff of more than one employee within a 30-day period. The main steps of the legal procedure are essentially the same in the collective and the individual cases, although some requirements depend on the number of persons laid off. For instance, if the employer plans to lay off between two and nine employees, she must organize a meeting with the work council, whereas there is no such obligation for an individual layoff.

2.2 Data

I use administrative data (*Fichier national des allocataires*, FNA) collected by the organization in charge of unemployment insurance in France, the *Union nationale interprofessionnelle pour l'emploi dans l'industrie et le commerce* (UNÉDIC), for the years 2009 to 2014. I focus on two subperiods gathering respectively the workers under a permanent contract laid off between September 1, 2009 and August 31, 2011, and October 1, 2011 and September 30, 2014. I select only the layoffs opening entitlements to the CSP (in firms of less than 1,000 employees or in compulsory liquidation or receivership, whatever the workforce size). Although layoffs represent a relatively small share of total separations, it is equivalent to about 10% of entries into subsidized unemployment. The

[24] In setting up a collective redundancy plan, the employer has to follow some criteria to determine the workers who will be laid off as a priority. If the last-in-first-out rule applies, many other determinants are considered, among which are the family load, the tenure, social characteristics making the return to work difficult, and professional skills.

[25] On the contrary, laid-off workers refusing the CSP may be required to give notice. For them, the end of the contract occurs after the notice period. However, adjusting for the notice period when measuring tenure at layoff in the data does not change the results.

[26] The employer's obligations regarding future layoffs depend on the number of workers he laid off in the past.

sample comprises all workers eligible for the CSP, accounting for 482,497 observations in the first period, and 636,350 observations in the second period. The unit of observation is the layoff.

The maximum duration of the CSP is 12 months. If the worker has not found a job by the end of the 12 months, she can switch to the standard compensation scheme (ARE), if she was initially entitled to a compensation duration of greater than 12 months. Although I focus on the analysis of the employment duration, I also investigate effects of the CSP on unemployment duration. I do not directly observe the return to work. The information on the reason why the unemployed person left the UI register is largely missing and not entirely reliable. Therefore, I compute the unemployment spell by gathering days on benefits before any substantial interruption.[27] It also implies that I do not have information on the future employment spells after the layoff. I cannot analyze future job quality or the probability of being rehired by the same employer, for example. The analysis takes advantage of the high frequency of the main variable of interest: the information on tenure, given at the day level, comes from the certificate delivered by the employer either to the employee for him to receive unemployment benefits, or directly to the employment agency (*Pôle Emploi*). Because this certificate is mandatory for the employee to be compensated and determines the way he will be compensated, this information is closely monitored by the employment agency and is reliable. The contract termination occurs at the end of the reflection period granted to the employee eligible for the CSP.

In my sample, the CSP take-up rate is much lower below one year of tenure (26%) than above (56% between one and two years of tenure). Figure B1 shows that the CSP takeup rate follows closely the distribution of the replacement rate.[28] Workers accepting the CSP are on average, older, more frequently female, more educated, more skilled, and working in larger firms in the service or manufacturing sector (Table B1 of Appendix B.1). The multivariate regression (Table B2 of Appendix B.1) reveals that individual characteristics play a bigger role in explaining the decision to accept the CSP than do firm characteristics. Survey evidence also points out the intensive counseling and frequent meetings with the caseworker as a potential motive to refuse the CSP, despite sometimes much higher benefits (Unédic, 2017).

[27]An unemployed can leave the rolls because he found a job, or for other reasons, such as sickness, because a form is missing, etc. In order not to consider these short interruptions as true exits from unemployment, I define as the end of the spell any interruption of at least four months, which is the minimum work history to open a new UI entitlement. It also means that, for workers accepting the CSP, I gather together the CSP duration and the following benefit duration if they do not exit unemployment durably by the end of the CSP. The unemployment spell duration does not include these potential short interruptions into the computation.

[28]Other factors might influence the takeup rate, such as the cancellation of the waiting period before receiving UI benefits if the CSP is accepted. This presumably increases the desirability of the CSP but does not change at the tenure threshold.

3 Estimation of the elasticity of contract duration

I first provide graphical evidence of manipulation at the tenure threshold, that results in bunching. I then quantify the bunching and derive an elasticity parameter. I show that the estimate is robust to an alternative methodology, and discuss its economic interpretation.

3.1 Empirical Evidence of Bunching

Figure 1 plots the raw histogram of tenure at layoff around the one-year threshold: it shows striking evidence of manipulation, with a hole at the left side and a mass at the right side, two distinctive features of bunching. Figures B2a and B2b (Appendix B.2) display the corresponding McCrary (2008) test to quantify the jump in density. I measure a significant 36% increase in the density at the cutoff. This is first evidence of strategic scheduling of the separation date in response to UI incentives. However, I cannot yet rule out the possible role of other factors. First, I note that all the contract terminations for economic reasons examined in my sample are open-ended contracts, meaning that this pattern cannot be due to some regularity in the duration of fixed-term contracts.[29] Second, the observed spike cannot be accounted for by the existence of renewable trial periods, because the maximum duration that can be reached corresponds to eight months (for executive workers). However, the spike in the density that I observe at the cutoff could still be rationalized by psychological mechanisms (anchoring phenomenon on a reference point) or legislative features (the 365-day cutoff can serve as a threshold for other policies). To better support the scenario of manipulation in response to UI incentives, Figure 2 provides a placebo test on workers registered as unemployed after a permanent contract, excluding economic layoffs. We do not observe any significant discontinuity on these workers not eligible for the CSP.[30]

A last concern could be that the discontinuity observed in the population of eligible laid-off workers registered for UI would not be observed in the total population of eligible laid-off workers. It means that the bunching would be due to a discontinuity in the UI take-up rate whereas the separation rate would be smooth at the threshold. This is unlikely, because even workers right below the one-year threshold are still entitled to

[29] This legal feature implies that the sample is mostly made of workers directly hired under a permanent contract. Being first hired under a fixed-term contract that is then converted into a permanent one is a very common hiring channel in France. It means that this sample may include workers with a stronger attachment to the labor market and a stronger bargaining power than the average worker. At the same time, their relatively low tenure among workers under a permanent contract plays in the other direction. All in all, it is not clear whether workers in this sample are more able to bargain a contract extension than the average worker under a permanent contract, alleviating some external validity concerns (workers under a permanent contract account for 86.5% of all wage-earners in France in 2011 (Insee)).

[30] In the same spirit, Figure B5 of Appendix B.2 reports almost no discontinuity for workers whose employers did not propose the CSP. Arguably, these employers were not aware of the existence of this scheme, and, therefore, were not strategically scheduling layoffs.

57.4% to 75% of their past earnings for at least 12 months. Nevertheless, to rule out this hypothesis, I need to rely on another data source. I use the *Mouvements de main-d'oeuvre* (MMO) data, which gathers the quarterly forms any employer in a firm of 50 employees or more must return to the administration. It describes all the employment flows, with information on the starting and ending dates of the contract, on the type of contract, and on the separation motive. Because these data do not include information on registration for UI, I cannot directly plot the UI takeup rate at the threshold. Therefore, I plot the tenure distribution for workers eligible for the CSP. Figure B6a of Appendix B.2 shows a similar bunching at the one-year cutoff. Although the UI registration rate might differ from one side of the threshold to the other, Figure B6a confirms that the separation rate is indeed discontinuous at the threshold. This discontinuity is at least partly driven by the CSP, as we do not observe any similar bunching for non-eligible workers dismissed for personal reason (Figure B6b of Appendix B.2).

Drawing on the bunching literature (Saez, 2010; Chetty et al., 2011; Kleven and Waseem, 2013; Brown, 2013), I exploit the observed hole and spike in the tenure distribution to obtain an estimate of the elasticity of contract duration, using the relationship between observed bunching and elasticity brought to light by Saez (2010). I first estimate the elasticity using the standard methodology where the counterfactual density is computed. I then turn to an alternative counterfactual using the pre-CSP period to control for potential confounding factors.

3.2 Baseline methodology

Because the setting is an upward notch (*i.e.* disposable income increases at the one-year threshold), I cannot identify an area of strictly dominated choice. Therefore, I cannot estimate the optimization frictions pointed out by Chetty et al. (2011) and Kleven and Waseem (2013), although these frictions are likely to be important in this case, because the extension of the contract can only occur at some bargaining cost.

The usual methodology rests upon the standard labor supply model where the individual trades off the value of consumption (measured by the disposable income when employed or unemployed) against the cost of work effort (captured by the income before tax and benefits). However, in my setting, the optimization is at the level of the joint surplus, which complicates the derivation of a structural elasticity parameter. Moreover, the definition of the underlying structural parameter is not obvious *ex ante*, as the observed behavior is a mixture of the employer's response, the employee's response, and, potentially, some bargaining frictions. For these reasons, and because I am primarily interested in gaining insights into the differences in behaviors between subgroups, I implement a reduced-form strategy to uncover the elasticity of contract extension. On the one hand, this estimate sheds light on the response in the presence of important negotiation frictions, and then does not give a precise measure of the true workers' optimization behavior. On the other hand, it does not rely on any structural assumption.

Figure 3 illustrates the implications of introducing a notch in the budget set. It makes all the individuals located in the interval $[L^*, L^* - \Delta L^*)$ in the pre-notch distribution bunch at the notch point, with L^* being L_{365}. The value, $L^* - \Delta L^*$, corresponds to the pre-notch tenure of the marginal buncher, L_{min}, which is the lowest pre-notch tenure value of the bunchers. The marginal buncher is the one who is exactly indifferent between the notch point and his best interior solution after the introduction of the scheme. There is a hole in the post-notch density distribution because no individual is willing to locate between $L^* - \Delta L^*$ and L^*.

Excess bunching at the notch can be expressed as:

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

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

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

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

where $UI(L)$ gives the amount of UI benefits paid at any value of L. If we denote r_0 as the standard benefit replacement rate and Δr as the change in replacement rate at the notch, we have:

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

The elasticity parameter becomes:

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

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

The reduced-form elasticity depends on policy parameters, Δr, r_0, L^*, and the response in terms of extension days that needs to be estimated. The bunching methodology aims precisely at providing an estimate of the extension day response. It consists in estimating the excess mass of individuals laid off at a tenure value within the defined bunching area by computing a counterfactual tenure density and comparing it with the observed one. I start by fitting a polynomial to the empirical distribution, excluding an area around the notch point, which I refer to as the *excluded area*. The counterfactual distribution is then estimated using the same coefficients, from a regression of the following form:

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

where c_s is the number of individuals in bin s; L_s is the tenure value in bin s; J is the order of the polynomial; and $[L_l; L_u]$ is the excluded area around the notch point. The counterfactual distribution is computed as the predicted value from equation 1, omitting the contribution of the dummies around the notch point. It follows that the counterfactual density is given by:

$$\hat{c_s} = \sum_{j=0}^{J} \hat{\beta_j} \cdot (L_s)^j. \tag{2}$$

Empirically, excess bunching $\hat{B}$ is obtained by taking the excess number of individuals located at the notch of the observed distribution compared with the counterfactual one (Figure 4).

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

The excluded area upper bound can be determined visually without ambiguity, because the spike is typically sharp. Regarding the lower bound, the missing mass is harder to delimit because it is more diffuse: the standard methodology is to set the upper bound, and to determine the lower bound through an iterative process, by making it vary and reestimating the counterfactual density until the bunching mass ($\hat{B}$) and the missing mass ($\hat{M} = \sum_{L_l}^{L^*} \hat{c_s} - c_s$) equalize.[31]

[31]The missing mass must be equal to the bunching mass because all the bunchers come from the left

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

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

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

I derive elasticity estimates from the observed bunching using a reduced-form approach, relating the bunching mass to the extension day response entering the elasticity formula. Following Kleven and Waseem (2013), I allow for heterogeneity in the elasticity: it implies that the presence of some individuals in the missing mass area, between the notch point and the marginal buncher point, is attributed to low elasticity values.[32] In this context, bunching could be used to estimate an average extension day response $E[\Delta L_e^*]$. Denoting by $\bar{h_0}(L,e)$ the joint tenure-elasticity distribution in the pre-notch situation, we have:

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

Using the measure of $\hat{B}$ and an estimate $\hat{c_s}$ of $h_0(L^*)$ at the notch, I can retrieve the average extension day response. This method would give a lower bound to the reduced-form elasticity because it assumes that all the individuals in the missing mass area have not bunched because of a very small elasticity.[33] I also make the estimation window vary to test the robustness of the estimation.

Table 1 shows consistent reduced-form estimates throughout different estimation win-

side of the cutoff, creating a hole.

[32] Another approach, developed in Kleven and Waseem (2013), allows to recover ΔL^* using the point of convergence L_l, which makes the missing and excess masses equalize as an estimate of L_{min} ("convergence method"). Indeed, if the area delimiting the bunching mass can be determined visually, the lower bound corresponding to L_l, the marginal buncher tenure, is defined so that the missing and excess masses are equal. In my case, as the jump in benefits occurs above the threshold, the missing mass appears below the threshold. L_l corresponds to a "point of divergence", where the counterfactual and empirical distributions start to diverge. The distance between L_l and L^* provides a measure of the extension day response ΔL^*. This gives an upper bound of the true behavioral response, because it assumes that all the individuals located between L^* and L_l respond to the incentives by bunching at the notch point. This method provides consistent but higher estimates. Results are available upon request. The lower-bound estimates are preferred because they allow for heterogeneity in the elasticity parameter, and do not require to assume that the missing mass area is not entirely empty only because of optimization frictions.

[33] Because I have an upward notch in the level of disposable income, I am not able to identify a strictly dominated region to rescale $E[\Delta L_e^*]$ as in Kleven and Waseem (2013).

dows and excluded area boundaries.[34] Standard errors are computed using a bootstrap procedure generating many distributions and associated estimates of each variable. The standard errors correspond to the standard deviations of the distribution of each variable estimate. The elasticity appears quite small, but we must keep in mind that it does not account for the optimization frictions. Using the estimate computed on the one-year window, it means that a 10% increase in UI marginal replacement rate leads, on average, to a 0.52-day response, measured at one year. The economic interpretation of the estimate is further discussed in Section 3.4. Table B7 of Appendix B.8 reproduces the main elasticity estimation with different orders of the polynomial used to compute the counterfactual density. Results do not appear to be sensitive to this parameter.

3.3 The Difference-in-Bunching Strategy

I exploit the fact that a similar package existed before the introduction of the CSP in September 2011, the CRP. The main difference with the CSP is that, to qualify for the higher benefit, the requirement is *to have at least two years of tenure* instead of one. The density in the period preceding the introduction of the CSP (henceforth referred to as the pre-CSP period) can be used as an alternative counterfactual distribution in a difference-in-bunching strategy (following Brown (2013)), and allows to neutralize the effect of other factors unrelated to the behavior of interest. Figure B3 (Appendix B.2) exhibits discontinuities in the tenure density at the threshold corresponding respectively to the CSP and CRP legislation only for the relevant periods: I observe a discontinuity in the tenure density at two years in the period September 2009–August 2011, which disappears completely after the CRP was replaced by the CSP, in the 2011–2014 period. The density jumps by 32% at the cutoff, and the discontinuity is significantly different from zero. It means that, when there are no incentives created by the UI benefits schedule, no bunching is observed. Further, Figure B4 (Appendix B.2) shows that the shift in the discontinuity location from the CRP to the CSP threshold occurs rapidly. In the first semester after the introduction of the CSP, we already observe a sharp decrease in the two-year discontinuity and an important increase in the one-year discontinuity. However, Figure B3 also displays a small discontinuity at the one-year tenure threshold for the 2009–2011 period that is necessarily explained by some factors unrelated to the CRP–CSP, although much smaller in magnitude (a 16% jump instead of a 36% jump after the introduction of the CSP). The difference-in-bunching methodology is precisely accounting for any pattern in the tenure distribution at layoff that would not be a response to UI incentives. The identification relies on the assumption that, absent the notch, the shape

[34] I also compute the elasticity estimates accounting for the fact that there is regularity in the contract starting and ending dates. Appendix B.7 shows that adding round-number fixed effects leaves the results virtually unchanged.

of the tenure distribution should be the same in both periods.[35] If this assumption holds, taking the difference between the observed distributions before and after the introduction of the CSP isolates the bunching exclusively due to the incentives created by the CSP.

A comparison of the pre- and post-CSP densities on the whole distribution and on a tightened window around the notch point (Figure 5) is the first evidence that the period preceding the introduction of the CSP can be convincingly used as a counterfactual. Figure 5 clearly shows that both densities are at the same level and have the same shape, except around the notch point. The post-CSP density shows a hole before the threshold, and a spike after, compared with the pre-CSP density. For full transparency, Figure B8 of Appendix B.9 shows a yearly decomposition of the density of tenure at layoff. It indicates that the pre-CSP density around one year remains virtually unchanged throughout the years, until the introduction of the CSP where the bunching mass appears.

I reproduce the reduced-form methodology, adjusting the formula from Chetty et al. (2011) by measuring b and m as the areas between the before and after densities. Using the pre-CSP density as counterfactual yields estimates very close to those obtained with the computed density, suggesting that non-CSP factors do not play a major role in explaining the bunching. Most of the observed bunching can then be imputed to a behavioral response to incentives created by the CSP. If I focus on the one-year window, the difference-in-bunching estimate is equal to 0.011, which corresponds to 77% of the baseline estimate (Table 2). I make the time window vary, to ensure that I am not capturing the effect of the change in the economic context in the periods September 2009–August 2011 and October 2011–September 2014. Thus, I restrict the window to January 2011 and June 2012. I obtain estimates of the same order of magnitude, albeit slightly higher.[36] In any case, it makes us even more confident than the baseline elasticity estimates are not capturing a spurious phenomenon that would not be related to UI incentives.

3.4 Economic interpretation of the bunching estimate

The elasticity parameter is small, despite substantial bunching. However, the measured behavioral response is still economically significant, and can inform us on a broader phenomenon likely to occur in other contexts.

Lower-bound estimate – First, the rearrangement of the elasticity formula scales the behavioral response to the change in the marginal replacement rate rather than the average replacement rate, to make the notch comparable to an hypothetical kink, which

[35] The raw number of laid-off workers can vary across time, provided the distribution of tenure at layoff stays unchanged in both periods.

[36] This is potentially because the economic situation improved from 2009 to 2014, reducing the number of economic layoffs and the need to optimize unemployment compensation.

contributes to explain the small elasticity estimate. Second, the measure of the behavioral response relies on the hypothesis that people not responding in the bunching area do not do so because their elasticity is too small, therefore assuming no optimisation frictions.[37] However, optimisation frictions have been found to be significant in the literature (Chetty et al., 2011; Chetty, 2012; Kleven and Waseem, 2013; Gelber et al., 2020). In this setting, in particular, the bunching decision depends both on the employee and the employer, making the adjustment even more complicated. To that extent, the estimate elasticity likely provides a lower-bound to the true behavioral response.

Average and marginal buncher – The numerator of the elasticity estimate shows an average of a 5.6-day response at the cutoff, representing a 1.6% increase in contract duration. Depending on the specifications, the marginal buncher is located between 310 and 339 days, implying that the extension response of the marginal buncher ranges from 26 to 55 days, which is a 7.7% to 17.7% increase in contract duration. Based on the average response and the location of the marginal buncher, we can derive an estimate of the share of workers who manipulated their termination date in the missing area. In the spirit of Diamond and Persson (2016) and Citino et al. (2019), the missing (M) and bunching (B) masses together with the headcounts of workers under the counterfactual and observed distributions can be used to measure the number of manipulators and non-manipulators. From subsection 3.2, we know that:

$$\hat{M} = \sum_{L_l}^{L^*} \hat{c_s} - c_s$$

$\hat{Total_c} = \sum_{L_l}^{L^*} \hat{c_s}$ measures the total number of workers that would have been in the missing mass area absent the notch. Because $\hat{M}$ captures people missing below the cutoff as a response to the notch, the ratio $\frac{\hat{M}}{\hat{Total_c}}$ gives an estimation of the share of manipulators in the missing mass area. $\hat{m}$ is defined symmetrically to $\hat{b}$ as:

$$\hat{m} = \frac{\sum_{L_l}^{L^*} \hat{c_s} - c_s}{\sum_{L_l}^{L^*} \hat{c_s} / (L^* - L_l)}.$$

Therefore, the share of manipulators can be directly computed from Table 1 as the ratio $\frac{m}{(L^* - L_l)}$. Depending on the specifications, the share of workers manipulating their termination date is equal to 17.4% in the four weeks before one year (column 3), or 10% in eight weeks before one year (column 1).

Extrapolation to other settings – The case under study can inform us about a

[37] If we were to assume that some people are not responding in the bunching area exclusively because of optimisation frictions, the elasticity would range from 0.11 to 0.35.

more generalized behavior of manipulation of employment duration. Indeed, kinks and notches are found everywhere in tax and benefit schemes. Closest to my setting, it is common in UI schemes to have discontinuities in the level or duration of UI benefits depending on age or tenure, both characteristics subject to potential manipulation. In Italy, for example, the duration of UI benefits jumps discontinuously at an age threshold. Citino et al. (2019) show that this notch leads to manipulation around the age-at-layoff cutoff. The Italian scheme also includes a *mobility indemnity* for workers unemployed following a mass layoff or business reorganization and with a tenure greater than one year. In Finland, if the work history is higher than three years, potential benefit duration increases by 100 days. If the worker is older then 58 and also meets the work history requirement, benefit duration is further extended by 100 days. Similarly, Spain has a step-wise function linking previous work history with benefit duration, creating numerous jumps at various work history thresholds. In the UK, the level of benefits increases abruptly at age 25, and in Belgium, the pace of the decrease in UI benefits changes discontinuously depending on past work history. Spinnewijn (2020) provides additional evidence on how UI benefits vary across workers within a country. These examples highlight the fact that the behavioral response I measure is highly relevant for other contexts where workers and employers could manipulate the date of the contract termination to extract rent from UI. My estimate likely provides a lower-bound in the sense that the layoff decision is highly constrained by the legal framework in this context. It can be compared to what is found in papers looking at the effect of a change in the eligibility threshold to any benefits or to an extension of the PBD on unemployment inflows. I use the pre-CSP period to measure how the probability of lying in the bunching area, conditional on being laid-off, has evolved after the introduction of the CSP. Among all layoffs under 18 months of tenure, the probability to lie in the bunching area has significantly increased by 0.91 percentage points (10.7%). Lalive et al. (2011) find that extending the PBD from 30 to 52 weeks leads to a 10% increase in the inflow rate to unemployment. Albanese et al. (2020) find a similar 12% increase in the layoff probability when eligibility to UI benefits is attained.[38] Using another measure, Baguelin and Remillon (2014) find that following a decrease in PBD, contract terminations of older workers are delayed so that the unemployed are covered by UI until they retire. They estimate a 4-month increase in the mean age at separation for older workers affected by the reform, from a mean age around 57 years-old for this group. It translates into a 0.6% increase. Similarly, I measure a 7.8-day (2.4%) increase in the mean tenure at layoff for workers laid-off under 18 months

[38] The probability I measure is conditional on being laid off, and therefore differs from the probability to enter unemployment at the threshold, that I cannot compute because I only observe individuals registered for UI. However, I argue that the response I observe is entirely driven by retiming of layoffs that would have occurred even in the absence of a notch (see Appendix B.3 for more details on the absence of extensive margin response). The magnitude of the change that triggers such responses is also lower in my setting than in these other two, but the comparison gives a sense of how consistent my estimation is with other findings.

between the pre- and post-CSP periods. In contrast, I find no increase in average tenure after the reform in the area between 30 and 42 months that should not be affected by the reform (the point estimate is equal to 0.5 and is not significant). All in all, my estimates are consistent with the existing literature, and point to a new optimization mechanism of separation behaviors in response to UI incentives, where the response can be entirely attributed to a retiming phenomenon.

4 A Bargaining Process between Employers and Employees

The estimation of the elasticity of contract duration allows to measure the response of workers and employers to a change in the UI replacement rate. I then dig further into the mechanisms. My preferred scenario to explain the excess mass in layoffs after one year of tenure is that, conditional on the layoff having been decided,[39] employers and employees bargain over the date of contract termination because they have both incentives and the flexibility to do so. While workers have clear incentives to reach the one-year threshold, they have no discretion over how long the layoff procedure takes.[40] On the contrary, employers have discretion to stretch out the layoff procedure. As underlined in Section 2, their main motivations to do so would be (i) a reduction in the financial layoff cost, since the risk of strike or of paying damages is reduced; (ii) a reduction in administrative constraints, since a worker accepting the CSP is not considered laid-off anymore, and legal obligations as part of future layoffs depend on the number of workers previously laid-off; (iii) a reduction in the social cost, because allowing workers to get higher benefits positively affects the employer's reputation with the remaining employees. However, employers will not decide to extend the contract of all workers they plan to lay off, but only those for whom it is profitable to do so.[41]

4.1 Theoretical Framework of Negotiated Layoff

I develop an illustrative framework to guide the bargaining analysis, where employers and employees decide on contract duration by maximizing their joint surplus. The

[39] Appendix B.3 provides an empirical analysis of the absence of an extensive effect of the CSP, i.e. on the number of layoffs.

[40] It is also unlikely that the employee would be able to trigger his own layoff at a strategic time because the economic layoff is based on reasons unrelated to the employee's behavior.

[41] Alternative *scenarii* could be considered, such as pure altruism from the employer. While I cannot pin down the intrinsic motivation of the employer, I show that the type of worker who get their employment spell extended is not random. Their characteristics correspond to workers with a high bargaining power, who are more likely to be informed of their UI entitlement and to have enough human capital and threatening power to bargain with the employer, possibly convening the representative bodies present in the firm.

optimization is therefore at the firm-worker level. The layoff decision implies that, at a certain point in time, the work contract is no longer profitable. It could be the case that the employer faces a temporary increase in demand and needs to hire a worker for a certain duration. In Appendix B.4, I develop the benchmark case where I assume that there is no uncertainty. At the time of hiring, the employer and employee decide between setting the contract duration so that the separation occurs when the productivity starts falling below the wage, or setting the contract duration at one year, where unemployment benefits jump.[42] They decide based on the sum of their net utilities. Therefore, efficient bargaining through the wage at the time of hiring entails the maximization of the joint surplus, leading to three different possible outcomes: an internal optimum below the one-year cutoff; a corner solution at one year; an internal solution above one year. However, the introduction of frictions offers a more plausible framework, one that matches better the empirical observation of limited wage movements at the aggregate level. It has also been shown that wage rigidity allows to better account for the magnitude of the cyclical behavior of unemployment and vacancies (Shimer, 2005; Hall, 2005). I therefore develop a model of bargaining with frictions in the core of the paper.

The specificity of the setting under study is that, contrary to the traditional bunching scenario, the optimization is not at the worker level. The bunching methodology rests upon a standard labor supply framework. In my setting, I also consider the firm side, which enriches but also complicates the analysis. Therefore, although theory-driven, this analysis will not allow to uncover a structural parameter, as it would require the estimation of many unknown parameters. I therefore stick to a reduced-form formulation that allows to clarify each party's incentives and costs.[43]

On the firm's side, the profit can be written as:

$$\Pi(L) = (P(L) - w)L$$

where $P(L)$ is the worker's productivity; w is the wage; and L is the contract duration. P(L) is a decreasing and concave function of the contract duration. At some value L, the

[42] There is no formal mechanism to hold the employer accountable for the commitment made at the time of hiring. However, there is evidence, in Brazil for instance, that firms set a contract duration just long enough for workers to be eligible for UI benefits, and rehire them when UI benefits are exhausted (Van Doornik et al., 2018). In France, Khoury et al. (2020) also show that contract duration is scheduled to coincide with the minimum employment record to be eligible for UI benefits, and adjust when this minimum employment record changes. Strategic scheduling of contract duration in response to UI rules has thus already been observed in other contexts. Moreover, in France, 69% of hires in 2012 were made by recalling a former employee. If the employer wants to preserve his reputation among the remaining employees or the laid-off worker in anticipation of potential rehiring, it will be in his interest to stick to his commitment.

[43] A structural estimation would require to determine many unknown parameters on the demand side or related to the bargaining process, and is beyond the scope of this paper. I do not have, in my data, enough information to pin down the exact division rule of the surplus or to decompose what comes from the firm or the worker side.

productivity will turn lower than the wage ($P(L) < w$), which justifies the layoff.[44]

Similarly, on the worker's side, the utility can be written as:

$$U(L) = (w - \alpha)L + UI(L)$$

where α is the disutility from work; and $UI(L)$ the unemployment benefits. The introduction of the CSP implies:

$$UI(L) = r_0 wL + \Delta r wL \mathbb{1}_{L \geq L_{365}}$$

where r_0 is the standard replacement rate; and Δr stands for the jump in replacement rate at the threshold. The surplus is expressed as:

$$S(L) = (P(L) - \alpha)L + r_0 wL + \Delta r wL \mathbb{1}_{L \geq L_{365}}$$

This expression shows that the surplus is discontinuous at one year.

Condition for the contract extension

The employer-employee pair will decide on the contract duration based on the surplus maximization, and share the surplus from the contract. I derive the first-order condition to get the optimal interior solution:

$$\frac{\partial S}{\partial L} = P'(L)L + P(L) - \alpha + r'(L) = 0$$

$$L^* = \frac{P(L) + r'(L) - \alpha}{|P'(L)|} \tag{3}$$

Based on consideration of the surplus, this condition describes the optimal duration. As $P'(L)$ is negative, the optimal duration increases with the productivity, the replacement rate of UI benefits, and decreases with the disutility from work. After the introduc-

[44] We can imagine, for example, that a firm decides to hire a worker for a temporary increase in demand. In the absence of the CSP, the employer would have offered a fixed-term contract of, say, ten months. Because of the existence of the CSP, some firms would offer a permanent contract instead, if the cost of extending the contract from ten to twelve months, when the difference between the productivity and the wage has turned negative, is lower than the gain entailed by the jump in benefits from the CSP.

tion of the CSP, the maximum surplus S^* is defined as $S^* = max(S(L^*), S(L_{365}))$. The employer-worker pair will decide to bunch if the surplus derived at one-year exceeds the internal optimum below one year. L_0 is the lowest pre-CSP tenure value which verifies 3 and which leads to a surplus equivalent to the surplus at the threshold. In other words, it corresponds to the initial value of tenure of the marginal buncher.

There is bunching if and only if:

$$[P(L_{365}) - \alpha + (r_0 + \Delta r)w]365 - [P(L^*) - \alpha + r_0 w]L^* \geq 0 \tag{4}$$

For the marginal buncher, the surplus derived at one year equalizes the best interior solution. We have:

$$\begin{aligned} [P(L_0) - \alpha + r_0 w]L_0 &= [P(L_{365}) - \alpha + (r_0 + \Delta r)w]365 \\ \Leftrightarrow \frac{[P(L_0) + r_0 w - \alpha]^2}{|P'(L_0)|} &= 365[P(L_{365}) - \alpha + (r_0 + \Delta r)w)] \end{aligned}$$

All worker-employer pairs with an optimal pre-CSP contract duration $L \in [L_0; L_{365}]$ will bunch at the one-year threshold. This is illustrated on Figure 6.

So far, I have been silent on the underlying negotiation mechanism that makes this extension happen. To be implemented, negotiation needs to ensure that both parties are at least as well-off as if no extension takes place. In Appendix B.4, I develop the benchmark case of bargaining with no frictions, where employers and workers bargain on the wage.

Bargaining with frictions

In practice, several inefficiencies can arise in the bargaining process, such as imperfect information or inability to reach an agreement. The institutional framework can lead to different types of frictions, such as wage rigidity or constraints on the firing cost.

Minimum wage – We assumed, in the efficient bargaining case, that the wage was determined at the time of hiring so that the employer is compensated for maintaining the worker up to the one-year threshold. In practice, for workers paid at the minimum wage level, the wage cannot be adjusted downward. For these workers, the contract may not be maintained up to one-year although it would have be profitable, because they cannot compensate the employer through a lower wage. The distance to the minimum wage can then act as a bargaining friction.

Uncertainty and firing cost – Similarly, we assumed in the efficient bargaining case, that employers and workers agree on the wage that ensures the employer is compensated for designing a one-year contract. However, in practice, there is some uncertainty on the demand level that employers fail to anticipate. If the firm is hit by an unexpected productivity shock,[45] excess wages have already been paid to the worker and cannot be recovered. The remaining variable of adjustment can be found in the firing cost, broadly defined as encompassing the severance payment, but also the potential risk for the employer of paying damages to the worker, or having his reputation harmed. I choose to model this ex-post transfer as the contingent part of the severance payment, that is paid on top of the legal amount of severance payment.[46]

At the time of the productivity shock, the contract is no longer profitable, and the employer and the employee have to decide whether they extend it up to the tenure threshold. Conditional on the extension being efficient from a joint point of view, the employee and the employer bargain over the cost of the layoff.

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

$$(P(L) - w)L - t + t^0 \geq 0. \tag{5}$$

where t^0 denotes the payment made to the worker at the time of the layoff when there is no extension; and t is the payment made to the worker at the time of layoff when there is extension.

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

$$t \geq 0$$

It follows that, for the bunching to occur, we need:

$$t^0 \geq L(w - P(L)) \tag{6}$$

Provided that extending the contract to one-year is efficient, equation 6 describes the set of parameters that would lead to an efficient extension if there is uncertainty on the demand and constraints on the firing cost.[47]

[45] Legally, the economic layoff cannot be planned ahead, but must be the last resort solution to an unanticipated shock to the firm.

[46] The employer is legally bound to pay severance payments only for employees with more than one year of tenure. This, if anything, would provide even more incentives to lay off the worker before one year, and would lead to an underestimation of the effect I measure. However, I focus here on the discretionary part that may be paid on top of the legal minimum, no matter the tenure value.

[47] Regarding the determination of the solutions for t, I also consider a standard Nash bargaining

Different features of the institutional setting can lead to bargaining frictions that prevent employers and employees to set the contract duration at one-year, although it would have been efficient, as the worker cannot compensate enough the employer. I define L_1 as the smallest pre-CSP tenure value that verifies both efficiency and feasibility conditions. I observe empirically that those bunching are the ones with initial tenure greater than $L_{min} = max(L_0, L_1)$.

4.2 Testable predictions

Table 3 summarizes the correlations that I expect to see between the magnitude of the bunching and different parameters. Note that the CSP entails a change in other non-monetary aspects, such as the suppression of the notice period (Table A1 of Appendix A). Empirically, as the difference in benefits is large, it can be used as a proxy for the difference in utility from unemployment at different values of L.[48]

I am primarily interested in the conditions that make the bunching occur. However, I can still try to explore the division of the surplus, as I have information on wages. The role of wage is ambiguous. It is positively correlated to the probability to bunch (Eq 4) only because UI benefits are proportional to the wage. In the frictionless model, the wage is used as a transfer for the employer to set the contract at one-year. It means that, all else equal, we expect the wage of bunchers to be lower than the wage of non-bunchers. However, in the case of bargaining with frictions, the further away from the minimum wage, the more room the worker has to bargain with the employer. If we introduce uncertainty on the demand, severance payments are used as a transfer instead of the wage. In my data, I have no information available on the level of t, preventing me from discussing the empirical implications of the surplus division rule. However, the bargaining power may still influence the likelihood to bunch, because it is related to t^0 that enters the feasibility condition (Eq. 6). In this framework, t^0 has been taken as exogenous at the time of layoff, but we can imagine that the bargaining power that has determined the division of the surplus from extension is correlated with the bargaining power intervening in the determination of t^0. I derive the following predictions from the model:

Prediction 1: *The higher the jump in replacement rate at the threshold (Δr), the higher the likelihood to bunch.*

framework and find that the Nash solutions are a subset of the efficient solutions I describe in the core of the paper. I then stick to the more general formulation of the model. This alternative framework is available upon request.

[48]Further, the potential benefit duration is virtually equivalent to the tenure value. Therefore, if the contract is extended to one-year, the worker will also benefit from additional days of benefits. This mechanism will just add to the difference in UI(L) below and at the threshold.

Prediction 2: *In the presence of bargaining frictions, the likelihood to bunch is positively correlated with the distance to the minimum wage and with the bargaining power, in particular t^0.*

The following paragraphs precisely aim at analyzing the driving forces of the variation in bunching, guided by the predictions derived from the theoretical framework. The elasticity previously derived is used as a metric that neutralizes the variation in the level of financial incentives to investigate the role of the wage and severance payments. Finally, in a more exploratory part, I show that the profile of the buncher is consistent with the bargaining model.

Elasticity by financial incentives level – Since the standard benefit replacement rate varies according to previous earnings, the jump in expected benefits at the one-year threshold varies accordingly (from 5 to 22.6 percentage points, pp). I therefore split my sample into four categories of potential gain when crossing the cutoff (the distribution can be found in Table B8 of Appendix B.10). In line with prediction (1) from the model, the McCrary (2008) test run on the different gain categories shows that the bunching magnitude increases with financial incentives.[49] Because the population having a higher potential gain, and then higher earnings, is also significantly more educated, more skilled and more frequently male than the rest of the population (Table B9 of Appendix B.10), I use the elasticity as a metric that scales the observed response in extension days by the magnitude of the financial gain. In Table 4, the elasticity parameter shows that, keeping incentives fixed, the last gain category is more responsive to a change in UI benefits than the first three categories. Overall, the elasticity increases with the category, and is not even significant for the first one, although the pattern is less clear for the two middle categories. For a 10% increase in the replacement rate, workers in the highest gain category would increase the length of their contract by 0.67 days, on average, whereas workers in the third gain category would increase it by 0.39 days.[50] This positive relationship indicates that workers in the highest incentive category negotiate more over the contract termination date, not only because their gain from unemployment compensation is higher. I therefore test the role of other parameters included in the model and potentially influencing the magnitude of bunching.

Elasticity by wage level – Because the level of the potential gain and the wage are positively correlated, it is difficult to disentangle both channels. I then take advantage of the fact that above a certain earnings threshold, the replacement rate of the standard

[49]Results are available upon request.

[50]Figure B10 of Appendix B.10 illustrates that the positive relationship between the elasticity and the level of potential gain still holds when taking the pre-CSP density as a counterfactual.

benefit is held constant, at 57.4%. Therefore, while wage keeps on increasing, the gap in replacement rate between both types of benefits is fixed, providing an opportunity to study how bunching varies with wage, controlling for variation in financial incentives. I divide the distribution of wage above the threshold where the gap in replacement rate remains constant into two parts, above and below the median, and I compute the log difference in the tenure density at the one-year cutoff. Figure B11 of Appendix B.10 shows that bunching increases slightly with wage, which is in line with prediction (2) of the model. However, as standard errors are large, I cannot rule out the hypothesis that wage has no influence on the magnitude of bunching, at least in this area of the distribution. More particularly, the model predicts that workers at the minimum wage have a lower chance to bunch, as they are constrained in how they can compensate the employer for him to set the contract at one-year. Table B12 of Appendix B.10 shows that being paid at the minimum wage has a negative impact on the probability to bunch. The introduction of the potential gain variable makes the coefficient of the minimum wage decrease in magnitude, but it remains negative and significant.[51] Similarly, the addition of firm fixed-effects does not change the conclusion, although estimates are less precise.

To better disentangle the minimum wage and the potential gain effect, I estimate the elasticity for different groups of distance to the minimum wage. Table 5 indicates that, for a 1% increase in the replacement rate, workers closer to the minimum wage (in the lowest quartiles of distance), are less responsive. Their elasticity is less than half the elasticity of the fourth quartile.[52] It suggests that they have a lower chance to bunch not only because they have lower incentives to do so, but also because they are constrained in how they can compensate their employer.

Elasticity by level of severance payment – In the case of uncertainty on the demand level and the existence of a firing cost, employers and workers cannot adjust the wage at the time of hiring. They will, instead, use the payments made upon separation as a transfer. The model predicts that the magnitude of bunching is positively correlated with the level of the discretionary part of severance payments (t^0). Unfortunately, I have no direct measure of t^0 in my data for the sample of laid-off workers,[53] but I use the

[51] There is still variation in the potential gain among people paid at the minimum wage, although smaller than in the general population, for two reasons: there is across time variation in the minimum wage level and UI benefits formula; the minimum wage is computed according to the hourly wage whereas UI benefit are computed based on the daily wage, which could lead to a divergence for people with different working hours.

[52] We do not observe much difference in elasticity between the first two deciles. A potential explanation is that, because a large share of workers are paid at the minimum wage in the sample, even workers in the second decile of distance do not have much leeway to transfer part of their wage to the employer. Their average distance to the minimum wage is 2.5€ relative to an hourly minimum wage around 9.1€.

[53] Supralegal severance payments are recorded in the UI data because they define the legal delay between the day the job seeker registers as unemployed, and the first day he receives benefits. However, one of the consequences of accepting the CSP is that there is no such delay. The information on the supralegal severance payment is, therefore, not recorded for those accepting the CSP.

information available for dismissed workers after an open-ended contract.[54] I restrict the sample to those having a tenure of less than two years and I estimate a zero-inflated negative binomial model. The regression output and a discussion on the choice of the model are presented in Appendix C. I use the regression coefficients to predict the value of the supralegal severance payment that would have been paid to the laid-off workers of my sample. I can then test the model's prediction by computing the elasticity on a high supralegal severance payment versus a low supralegal severance payment subgroup. Table 6 shows that the high supralegal severance payment group exhibits a higher elasticity than those who would receive low supralegal severance payments when there is no extension. Although the value of t^0 is predicted and not directly observed, these results suggest that prediction (2) holds: a higher t^0 makes the extension of the contract more likely through the feasibility condition in the model. It can be argued that a higher t^0 is itself the result of higher bargaining power, which would imply a positive link between the bargaining power and the propensity to bunch.

What we can draw from this analysis is that the variation in financial incentives does not fully explain the variation in bunching, but other parameters of the model related to the bargaining power still play a role. The positive correlations between bunching and wage and between bunching and the baseline level of severance payment confirm the predictions of the bargaining model with frictions. In a more exploratory part, I now present additional evidence supporting the hypothesis of an individual bargaining process.

4.3 An individual bargaining process

The legal framework implies that employers have a lot of leeway to implement their waiting strategy for a significant amount of workers, as the minimum time period between the first layoff notification and the end of the contract in firms of 11 to 49 employees laying off two to nine of them is 43 days. It strengthens the bargaining hypothesis as opposed to an hypothesis where employers would select, among the workers they plan to layoff, those with a tenure right above one-year. Moreover, this selection strategy would not be compatible with the observed dip in the density of tenure right below the one-year cutoff. In addition, different pieces of evidence point to an individual bargaining scenario.

Layoff order – Layoffs can be either collective or individual. In my sample, among

[54] Workers dismissed for personal reasons may differ from laid-off workers in several respects, including their ability to get severance payment. Workers dismissed for serious misconduct are not eligible for any severance payment, whereas other dismissed workers are. Then, these dismissed workers do not correspond to extreme cases of misconduct, and can be deemed more comparable to laid-off workers, conditional on observables. If not perfect, this solution appears better than using the sample of laid-off workers refusing the CSP as they may have a very particular profile, itself influenced by the bunching phenomenon.

all firms laying off at least one worker during the period of observation, 41.2% of them had laid off at least two individuals. This corresponds to 76.8% of laid-off workers in the sample.[55] If the bargaining scenario were true, I would observe that workers laid off as part of the same collective layoff plan would have different end-of-contract dates.[56] Workers right above the cutoff might be laid off later than those far from the threshold (either above or below) and for which it is useless or too costly to manipulate the tenure. I gather all the layoffs observed from the same employer over a period of 30 days[57] into the same layoff spell to establish the rank, by the date of contract termination, of each layoff in the same spell. I find that having a tenure lying between 365 and 380 days is associated with a lower probability of being laid off first as part of a collective layoff plan (–12.8 pp), and with a higher probability of being laid off second (+10.4 pp). The remaining distribution is rather similar (Figure 8). Because the median of the rank variable is two, being in the second position means, in 50% of the cases, being the last person laid off in the layoff plan. Thus, it appears that workers close to the cutoff are indeed more frequently laid off later in the layoff plan.

In Appendix B.5, I provide two additional pieces of evidence: first, I look at the time between the first layoff date and one's own layoff date in collective layoffs to highlight strategic waiting behaviors. I show that this difference often corresponds to the required number of days to reach the threshold for workers with an initial tenure (at the time of the first layoff) right below the cutoff and a final tenure in the bunching area (Figure B7). Second, I show that bunching is more pronounced in firms shutting down, which is consistent with the fact that extending the employment spell of some workers is less costly for these firms because it won't jeopardize their future (Table B4).

Profile of the typical buncher – I derive the profile of the typical buncher following Diamond and Persson (2016) methodology. It adapts the bunching method to compute the counterfactual distribution of observable characteristics as a function of tenure, in the absence of the notch. By comparing the observed and counterfactual distributions, this approach allows to recover the characteristics of workers who were in a position to bunch[58] and who did or did not (see Appendix B.6 for further details on the methodology). Bunchers have characteristics associated with a high bargaining power (Table B5): they work more frequently full-time, have a higher level of education and skill, and are more frequently female and older. By contrast, workers who were in a position to bunch but did not have an opposite profile .

To control for the variation in financial incentives, I derive the elasticity for subpop-

[55] A more detailed distribution of the redundancy size is in Table B13 of Appendix B.11.

[56] Some steps of the layoff procedure are common to all workers, such as the meeting with employees' representatives, but others can be tailored to each individual tenure, like the individual meeting.

[57] The legal criterion to consider a layoff as collective is having several layoffs during a 30-day period.

[58] Meaning whose initial tenure was right below one year.

ulations defined in terms of firm size, sector, age, gender, and education.[59] Tables B10 and B11 of Appendix B.10 confirm that, even controlling for financial incentives, the highest estimates are typically found in small firms among highly educated individuals, in the retail, food and accommodation, and services sectors. Statistics on representation institutions indicate that the presence of representatives is an increasing function of firm size, and that the building sector is where the unionization rate of representatives is the highest (Breda, 2016). It suggests that the type of firms where the elasticity is the highest is typically not those where employees are better represented, arguing for an individual bargaining process.

To further examine this scenario, I use a fixed-effect logit model to determine if there is still variation in the magnitude of bunching within a firm.[60] This test is crucial because the positive correlation I find between the propensity to bunch and proxies for the individual bargaining power could be rationalized by differences in type and quality of the job. We could imagine that some firms in some sectors offer better jobs in terms of earnings, severance packages, or willingness to extend their contracts, and that these jobs are matched with higher-educated and higher-skilled individuals.[61] In this regression, bunching is defined as having a tenure at layoff falling between 365 and 397 days, because it corresponds to the bunching area for most bunching estimations. I use firm fixed-effects to neutralize any characteristic specific to the firm that I am not able to capture otherwise. I estimate a logit model on the subsample of firms for which I have several observations (i.e., several workers laid off), and with some variation in the bunching dummy variable. It leaves me with 20,106 observations distributed in 3,396 firms.

Table 7 shows that potential gain, age, and education level all have positive effects on the propensity to bunch, with potential gain and education explaining more of the variation in bunching. The last column indicates that, keeping firm characteristics and incentives constant, more educated and older workers are more able to take advantage

[59]In the same spirit, I summarize the relationship between the propensity to bunch and individual characteristics by computing the correlation between the predicted unemployment duration and the elasticity of employment duration with respect to the level of UI benefits. Elasticity is the highest in the middle of the predicted unemployment duration distribution. It confirms the insight from the model that those responding the most to the UI incentives have both a high financial gain—because they anticipate staying unemployment for a significant period of time—and bargaining resources to do so. Detailed results are available upon request.

[60]The only available data on representation institutions in the firm are aggregated at the firm-size level. Using this information, I find that collective representation institutions do not seem to contribute to bunching (results are available upon request). I therefore use a firm-fixed-effect model to control for the quality of representation in the firm.

[61]There could also be a concern that the difference in potential gain or bargaining power is correlated with the availability of the CSP. For example, workers with smaller potential gains and thus lower wages may work more frequently in small firms where the employer might not be aware of the existence of the CSP (although he has the legal obligation to offer it). However, I observe that the average potential gain of workers in firms not offering the CSP is equal to 18.9 pp as compared to 19.8 in other firms. If there is a difference, the magnitude is very small. Plus, the use of firm fixed-effects will take care of any difference in the CSP availability at the firm level.

of the representation structures that exist in the firm. This finding argues in favor of bunching being the result of an individual bargaining process rather than the consequence of better workers sorting in better firms. It is also compatible with the finding that the elasticity of contract duration is much larger for an individual layoff than for collective layoffs (Table B14 of Appendix B.11).

In sum, these results show that the predictions from the bargaining model with frictions are confirmed in the data. Further, although these findings are more exploratory, there is a strong pattern for age and education influencing positively the magnitude of bunching, even controlling for other individual characteristics and firm fixed-effects. These findings are in line with other cases of individual-level bargaining settings, where it has been shown that the level of qualification was a strong determinant of the bargaining outcome. For example, in France, we observe that executives can negotiate much higher severance payments in mutually agreed contract terminations, whereas low-skilled workers only receive the legal minimum payment in most cases (Bouvier, 2017). A potential explanation put forward in the study is the difference in the level of information and in the level of knowledge of the decision-making process in the firm.

5 Robustness Checks and Extensions

Bunching at two years – The existence of a similar setting at a different tenure value under a different time period is also exploited to compute elasticity estimates from the bunching response at the two-year cutoff during the pre-CSP period. This is a way to check whether the response is similar under both periods and cutoffs, and to confirm that the bunching measured is indeed a behavioral response to incentives created by the UI design, no matter the value of the threshold or the time period considered. Table B15 (Appendix B.12) exhibits elasticity estimates that are close to those from the main estimation, around 0.008.

The same threshold under the post-CSP period could have been used to measure some optimization frictions, the idea being that any bunching observed at two years after September 2011 is a sign of some workers not able to adjust to the new incentives. However, as depicted in Figure B3, no significant bunching occurs at this point under the post-CSP period. It follows that it is not possible to determine the missing mass lower bound that equalizes the missing and excess masses, and that delimits the area where the behavioral response is observed. In turn, this indicates that the main frictions attenuating the bunching of interest come from negotiation frictions that do not allow workers to adjust freely their layoff date, rather than from information frictions for example.

Welfare implications – To gain a more complete picture of the welfare impact of this UI program, I also examine the response to higher UI benefits in terms of unemployment duration. This response could come from moral hazard (*i.e.* the impact of receiving higher benefits on job search) or selection into manipulation (if bunchers have a higher initial unemployment risk). I measure a sizable 28-day increase in unemployment duration at the one-year threshold. This measure can be incorporated into a cost–benefit computation of the effect of this scheme (Appendix B.13), no matter the source of the increase. However, knowing whether this increase is entirely driven by moral hazard would also inform on the motivation of bunchers, and whether they bunch because they anticipate to stay unemployed longer than the average worker. Using the same methodology as in Appendix B.6 (similar to Diamond and Persson (2016); Citino et al. (2019)), I retrieve the unemployment duration of bunchers below and above the threshold, using the observed and counterfactual unemployment duration and the number of people under the observed and counterfactual densities. If I restrict to a 10-day window around the cutoff, receiving higher benefits makes the unemployment duration of bunchers increase by 12 days (3.5%), therefore indicating that the raw difference measured at the cutoff is partly due to moral hazard. Given that the treatment effect on the bunchers is lower that the observed difference at the cutoff, it suggests that compliers are negatively selected in terms of unemployment risk. Part of their motivation to manipulate the length of the contract could stem from the fact that they will stay unemployed for a long time. In that sense, bunching allows them to be better covered, although it entails an additional cost to the unemployment insurance. Impacts on both inflows and outflows to and from unemployment contribute a substantial cost for the UI.

6 Concluding Remarks

In this paper, I show that a UI program in France that makes the level of UI benefits discontinuously jump at one year of tenure has an impact on the timing of layoffs. I highlight a concentration of layoffs on the high-benefit side of the tenure distribution. Bunching evidence is used to quantify the sensitivity of the duration of the contract to UI financial incentives. The empirical evidence suggests that the bunching behavior is the consequence of the bargaining between employers and employees who agree to maximize the joint surplus by extending the contract when it is profitable to both parties.

These strategic behaviors have several public policy implications. First, they encourage the maintenance of a poor match that is no longer efficient. They also allow employers to reduce the risk and the cost associated with the layoff thanks to a transfer payment from the State. They can use the UI scheme as an instrument for social peace both for laid-off workers and for those staying in the firm, without bearing the cost of such a strategy, and without internalizing it in the conditions of a breach of the contract. The direct cost of this behavior is doubled up with an indirect cost, because receiving higher benefits itself increases the duration of the subsequent unemployment spell.

The analysis in terms of characteristics of the individuals and the firms reveals that, keeping incentives constant, the workers most likely to bunch and, then, to extract rent from the UI, are more educated, more skilled, and better integrated into the labor market. Representation structures do not seem to favor bunching. In this setting, I identify a mostly individual bargaining process, where more educated and skilled workers are better able to mobilize bargaining resources and obtain higher compensation. At the same time, bunchers are also older on average, and seem to be negatively selected in terms of unemployment risk. This ambiguous finding raises some questions about the efficiency of the UI in targeting the population most in need and further away from the labor market.

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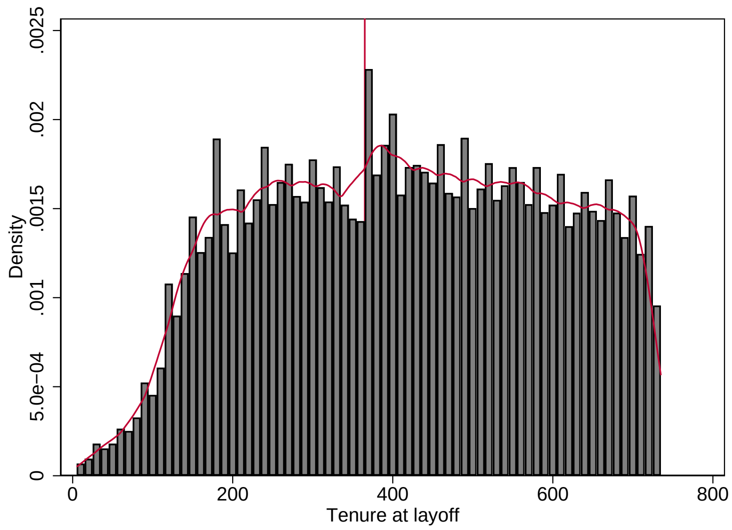
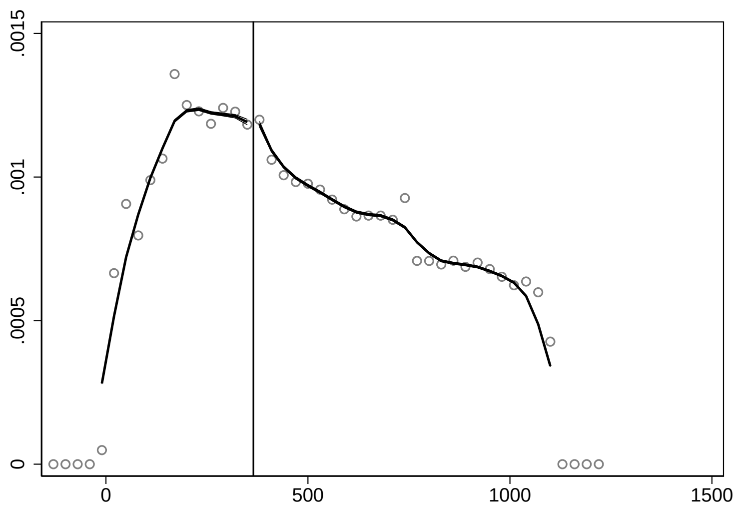
Tables and Figures

Figure 1: Tenure at layoff of workers eligible for the CSP



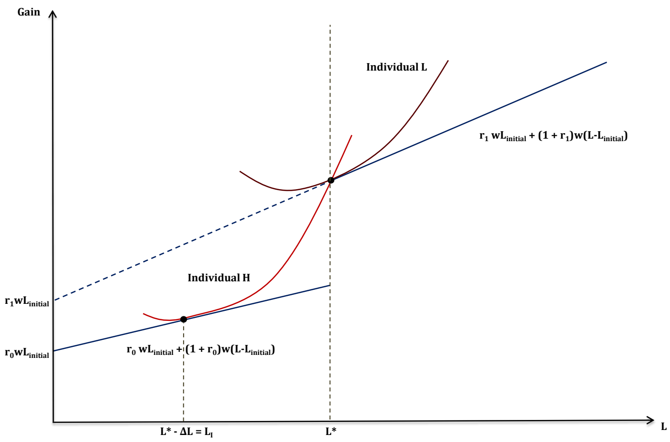
SOURCE: UI data (FNA).
NOTE: This graph plots the raw density of tenure at layoff for the whole population of workers eligible for the CSP entering unemployment between October 2011 and September 2014 (10-day bins), around the one-year cutoff. The red line represents the kernel density.

Figure 2: The McCrary test on all open-ended contracts (excluding workers eligible for the CSP)



SOURCE: UI data (FNA).
NOTE: This graph reproduces the (McCrary, 2008) test performed on the sample of unemployed workers after open-ended contracts over the period between October 2011 and September 2014, excluding workers eligible for the CSP. The size of the bin is 30 days, to reduce the regular spikes at each month interval (bandwidth: 100). Because there is no discontinuity at the one-year threshold, it provides further evidence that the jump observed in the distribution of laid-off workers at the same threshold is driven by the CSP and not by another motive that would concern all workers under a permanent contract.

Figure 3: Notch in the budget set



NOTE: This diagram illustrates how individuals bunch at the same point of the distribution of tenure when a notch is introduced in the level of UI benefits that depends on the value of tenure. The replacement rate jumps from r_0 to $r_0 + \Delta r = r_1$ at L^*, making all individuals located between $L^* - \Delta L^*$ and L^* on the pre-notch distribution bunch at the notch point. The marginal buncher is the one who is indifferent between the notch point L^* and the best interior solution L^I with $L^I < L^*$ after the introduction of the notch.

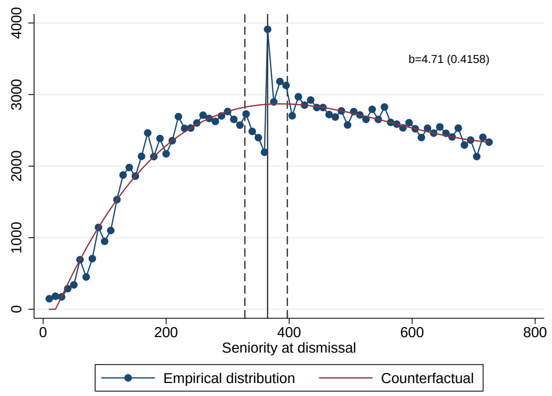
Table 1: Reduced-form elasticity estimates

Tenure window	[120;540]	[180;540]	[0;730[	[0;1100[
L_u	398	398	398	398
L_l	309***	323***	338***	339***
	(4.13)	(11)	(5.46)	(4.69)
b	5.55***	5.01***	4.72***	4.56***
	(0.3181)	(0.3478)	(0.3462)	(0.3295)
m	5.59***	5.05***	4.7***	4.53***
	(0.342)	(0.37)	(0.3564)	(0.3209)
% change in replacement rate	12	11.98	11.8	12
ϵ_{RF}	0.0173***	0.0154***	0.0143***	0.0137***
	(0.0012)	(0.0012)	(0.0012)	(0.0011)

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The elasticity is computed using the same formula as in Chetty et al. (2011) adjusted for the notch $\frac{(m/L^*)^2}{\Delta r/(1+r_0+L^*\Delta r/\Delta L^*)}$. More detailed explanations on the method are in Section 3.2. The different columns correspond to different tenure windows considered in the estimation.
Standard errors are calculated using a bootstrap procedure generating tenure distributions and associated estimates by random resampling. 600 replications.

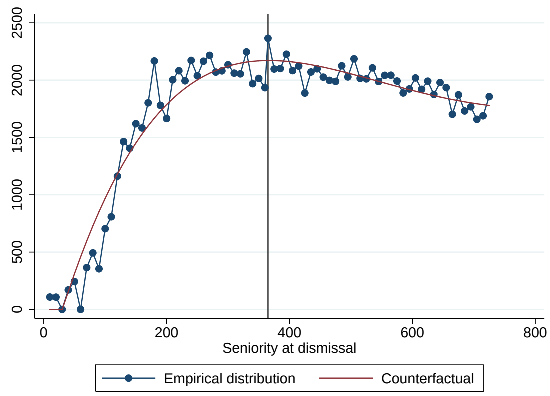
Figure 4: Empirical and counterfactual distributions of tenure at layoff at the one-year cutoff

October 2011–September 2014



SOURCE: UI data (FNA).
NOTE: This graph plots in blue the empirical distribution of tenure at layoff for workers eligible for the CSP, laid off between October 2011 and September 2014 (binsize: 10). The counterfactual distribution is computed by fitting a 4^{th}-order polynomial, excluding an area around the notch, and extrapolating the distribution in the excluded area. The excluded area upper bound is determined visually where the bunching stops. The lower bound is found through an iterative process such that the excess and missing masses equalize (see Section 3.2 for more details). The upper and lower bounds are represented by the dashed lines. The solid vertical line represents the one-year cutoff, where there are incentives to bunch during this period. The figure exhibits significant bunching at the notch, other than related to a round-number effect.

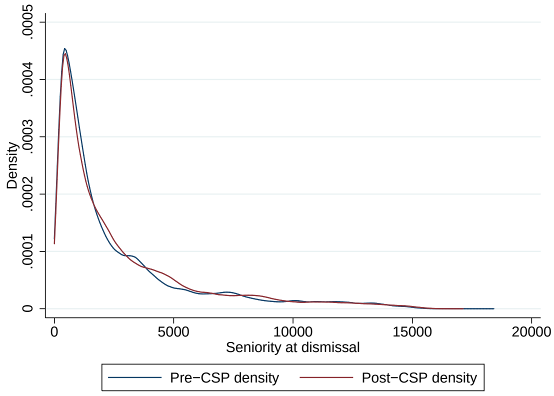
August 2009–August 2011



SOURCE: UI data (FNA).
NOTE: This graph plots in blue the empirical distribution of tenure at layoff for workers eligible for the CSP, laid off between August 2009 and August 2011 (binsize: 10). The counterfactual distribution is computed by fitting a 4^{th}-order polynomial, excluding an area around the notch, and extrapolating the distribution in the excluded area. The excluded area upper bound is determined visually where the bunching stops. The lower bound is found through an iterative process such that the excess and missing masses equalize (see Section 3.2 for more details). The solid vertical line represents the one-year cutoff, where there are no incentives to bunch during this period. The figure exhibits no significant bunching at the notch, other than related to a round-number effect.

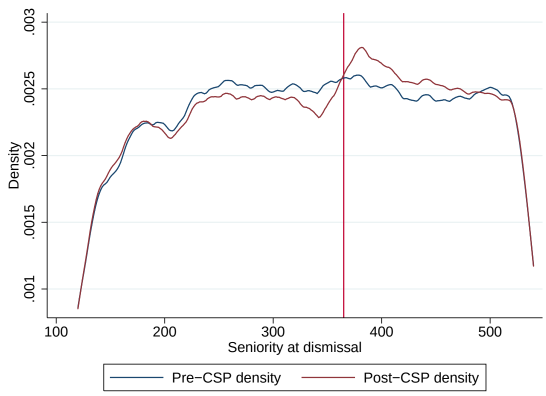
Figure 5: Pre- and post-CSP tenure density

On the whole distribution



SOURCE: UI data (FNA).
NOTE: This graph plots the density of tenure at layoff for workers eligible for the CSP for the pre-CSP period (August 2009–August 2011) and the post-CSP period (October 2011–September 2014), respectively, on the whole distribution. It shows that the density in the pre- and post-CSP periods looks similar, demonstrating that the pre-CSP density provides a good counterfactual distribution to measure the bunching exclusively related to the incentives created by the CSP.

Around the notch (120–540 days)



SOURCE: UI data (FNA).
NOTE: This graph plots the density of tenure at layoff for workers eligible for the CSP for the pre-CSP period (August 2009–August 2011) and the post-CSP period (October 2011–September 2014), respectively, between fmy and 18 months of tenure. The vertical line corresponds to the one-year cutoff. It shows that the density in the pre-CSP period is quite flat in the bunching area, whereas I observe a clear hole before and a mass after the one-year threshold in the post-CSP density. It demonstrates that the pre-CSP density provides a good counterfactual distribution to measure the bunching exclusively related to the incentives created by the CSP.

Table 2: Difference-in-bunching elasticity estimates

Time period	Sept 2009–Sept 2014			Jan 2011–June 2012	
L_u	395	395	395	395	395
L_l	325***	324***	325***	308***	308***
	(11.7)	(12.02)	(13.65)	(11.89)	(11.87)
Tenure window	[120;540]	[180;540]	[0;730[	[120;540]	[180;540]
b	3.24***	3.04***	3.75***	5.4***	5.04***
	(0.5525)	(0.5043)	(0.702)	(0.8718)	(0.9118)
m	3.18***	3.03***	3.7***	5.37***	4.93***
	(0.5141)	(0.4592)	(0.6763)	(0.8941)	(0.8429)
ϵ_{RF}	0.0094***	0.0089***	0.011***	0.0166***	0.0151***
	(0.0016)	(0.0014)	(0.0022)	(0.003)	(0.0028)

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The elasticity is computed using the same formula as in Chetty et al. (2011) adjusted for the notch $\frac{(m/L^*)^2}{\Delta r/(1+r_0+L^*\Delta r/\Delta L^*)}$. The counterfactual density is derived from the pre-CSP distribution of tenure. The first three columns correspond to the full time period (September 2009–September 2014), whereas the last two columns correspond to a restricted eight-month window on each side of the reform, to avoid capturing the effect of different economic conditions. Within each time period, the different columns correspond to different tenure windows considered for the estimation.
Standard errors are calculated using a bootstrap procedure generating tenure distributions and associated estimates by random resampling. 200 replications.

Figure 6: Bunching as the result of the surplus maximization

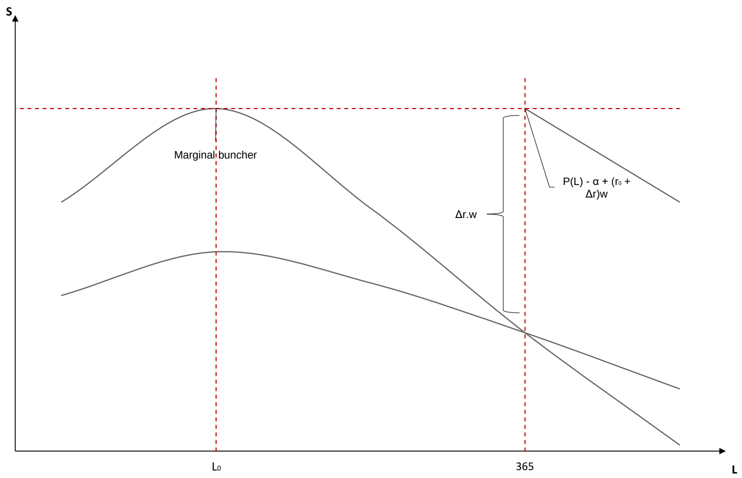


Table 3: Predicted correlations with the likelihood to bunch

Parameter	Correlation
After-shock productivity $P(L)$	(+)
Rate of decline of the productivity $\lvert P'(L)\rvert$	(+)
Jump in replacement rate Δr	(+)
Wage w	(+)
Disutility from work α	(-)
Baseline supralegal severance package t^0	(+)

NOTE: This table describes the predicted correlations derived from the model (Section 4.1). The likelihood to bunch is positively correlated with the productivity or the jump in replacement rate. On the contrary, if the wage or the initial level of severance payment is too low and cannot be adjusted downward, the worker cannot compensate enough the employer for him to accept the bear the cost of maintaining the contract.

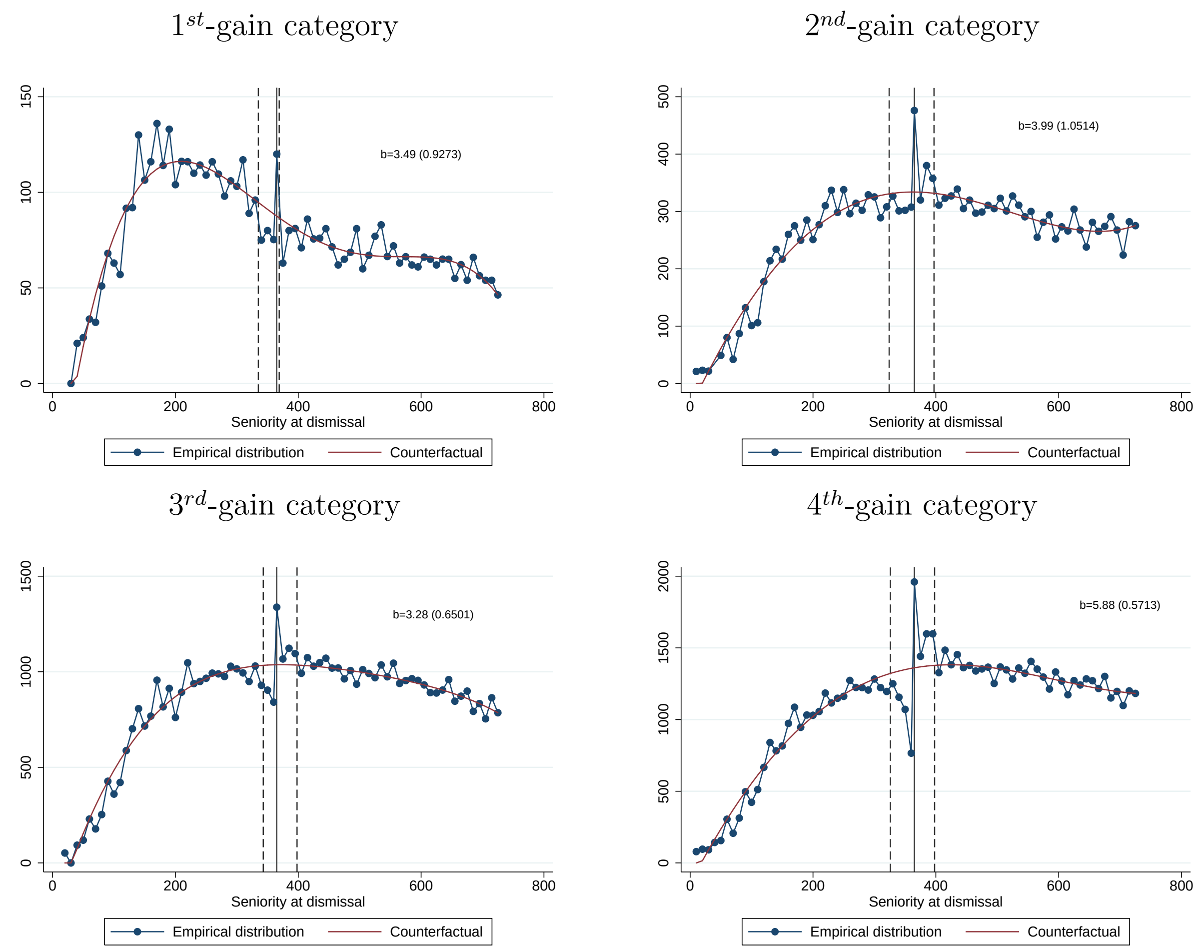
Table 4: Elasticity estimates by gain categories

Gain category	Average gain in replacement rate (in %)	L_u	L_l	b	m	ϵ_{RF}
$Gain < 10pp$	3.58	369	340***	1.8	1.69	0.00545
			(14.91)	(1.2776)	(1.1987)	(0.0044)
$10pp \leq Gain < 15pp$	8.66	398	339***	3.77***	3.76***	0.0113***
			(6.04)	(.8895)	(.8906)	(0.0026)
$15pp \leq Gain < 20pp$	10.95	398	342***	3.56***	3.53***	0.0106***
			(5.14)	(0.5203)	(0.5164)	(0.0017)
$Gain \geq 20pp$	14.04	398	334***	5.99***	6.02***	0.0184***
			(8.07)	(0.4764)	(0.4704)	(0.0016)

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

NOTE: The elasticity is computed separately for each gain category, using the same formula as in Chetty et al. (2011) adjusted for the notch $\frac{(m/L^*)^2}{\Delta r/(1+r_0+L^*\Delta r/\Delta L^*)}$. The counterfactual is estimated fitting a fourth order polynomial on the tenure area between zero and 730 days. The potential gain category is defined according to the difference in replacement rate between the standard benefit and the CSP benefit at the one-year threshold. The table indicates that, as the potential gain increases, the elasticity also increases, especially for the highest gain category. Standard errors are calculated using a bootstrap procedure generating tenure distributions and associated estimates by random resampling. 200 replications.

Figure 7: Empirical and counterfactual distributions of tenure at layoff by gain category (October 2011–September 2014)



SOURCE: UI data (FNA).
NOTE: This graph plots in blue the empirical distribution of tenure at layoff for workers eligible for the CSP, laid off between October 2011 and October 2014, and part, respectively, of the 1^{st}-, 2^{nd}-, 3^{rd}- and 4^{th}-gain category (binsize: 10). The counterfactual distribution is computed by fitting a 4^{th}-order polynomial, excluding an area around the notch, and extrapolating the distribution in the excluded area. The excluded area upper bound is determined visually where the bunching stops, and corresponds respectively to 369, 397, 398, and 398 days. The lower bound is found through an iterative process such that the excess and missing masses equalize (see Section 3.2 for more details), and corresponds respectively to 335, 324, 343, and 326 days. The two dashed vertical lines represent the excluded area bounds, and the solid vertical line represents the one-year cutoff. The figure exhibits significant bunching at the notch, other than related to a round-number effect, except for the 1^{st}-gain category. The gain is defined as the difference in replacement rate between the standard benefit and the ASP granted to those with a tenure of one year or more. This difference increases with previous earnings, because the replacement rate associated with the standard benefit decreases with earnings. The different gain categories correspond to a gain lower than 10 pp, higher or equal to 10 pp and lower than 15 pp, greater or equal to 15 pp and lower than 20 pp, and greater or equal to 20 pp.

Table 5: Elasticity estimates by distance to the minimum wage

Quartile of distance to the minimum wage	Average gain in replacement rate (in %)	L_u	L_l	b	m	ϵ_{RF}
1^{st} *quartile*	9.32***	398	322***	3.85***	3.73***	0.0114***
	(0.0044)		(13.69)	(0.5423)	(0.5214)	(0.0017)
2^{nd} *quartile*	12.34***	398	333***	3.8***	3.79***	0.0113***
	(0.0022)		(13.32)	(0.5853)	(0.5812)	(0.0019)
3^{rd} *quartile*	14.18***	398	315***	6.84***	7.06***	0.022***
	(0.0028)		(10.79)	(0.6767)	(0.7323)	(0.0026)
4^{th} *quartile*	14.36***	398	326***	7.4***	8.06***	0.0255***
	(0.001)		(8.44)	(0.7392)	(0.883)	(0.0032)

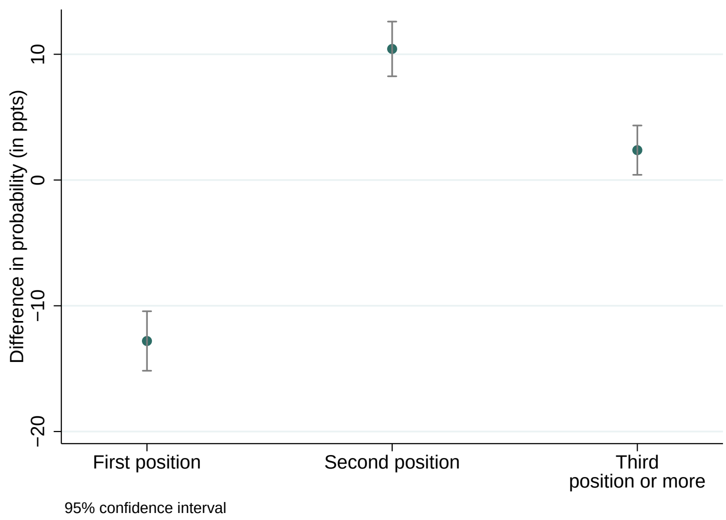
** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table displays elasticity estimates by quartile of distance to the minimum wage. The first quartile includes people who are the closest to the minimum wage, and the fourth one people who are the farthest from the minimum wage. Elasticity estimates are computed using the same formula as in Chetty et al. (2011) adjusted for the notch $\frac{(m/L^*)^2}{\Delta r/(1+r_0+L^*\Delta r/\Delta L^*)}$. The area used for estimating the counterfactual is included between 180 and 540 days. The polynomial fitting the tenure bin count is of order 4. Standard errors are calculated using a bootstrap procedure generating tenure distributions and associated estimates by random resampling. 500 replications. The distance to the minimum wage is computed based on the hourly wage. We observe that workers closer to the minimum wage, and then constrained in their transfer to the employer, have lower elasticities. It is in line with a prediction from the model, i.e. workers with more resources to compensate the employer have a higher chance to bunch.

Table 6: Elasticity estimates by baseline supralegal severance payment

Supralegal severance payment	Average gain in replacement rate (in %)	L_u	L_l	b	m	ϵ_{RF}
Low gain	11.27***	398	339***	4.43***	4.39***	0.0133***
	(0.0004)		(8.03)	(0.5665)	(0.5587)	(0.0018)
High gain	12.79***	398	329***	5.54***	5.58***	0.0171***
	(0.0004)		(9.76)	(0.5669)	(0.5832)	(0.002)

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table displays elasticity estimates by category of supralegal severance payment. Elasticity estimates are computed using the same formula as in Chetty et al. (2011) adjusted for the notch $\frac{(m/L^*)^2}{\Delta r/(1+r_0+L^*\Delta r/\Delta L^*)}$. The area used for estimating the counterfactual is included between zero and 1,100 days. The polynomial fitting the tenure bin count is of order 4. Standard errors are calculated using a bootstrap procedure generating tenure distributions and associated estimates by random resampling. 100 replications. Supralegal severance payments are predicted using a sample of similar dismissed workers with tenure lower than two years. The low (respectively high) severance payment category corresponds to a supralegal severance payment below (respectively above) the median. Although the difference is not very large, the elasticity estimate is higher for the high-payment category. It is in line with a prediction from the model, i.e. workers with more resources to compensate the employer have a higher chance to bunch.

Figure 8: Layoff order within the same layoff plan with respect to the distance from the cutoff (October 2011–September 2014)



SOURCE: UI data (FNA).
NOTE: The layoff plan gathers all layoffs from the same employer over a 30-day period. Being right above the cutoff means having a tenure lying between 365 and 380 days (included). Workers with a tenure right above the cutoff have a higher probability of being laid off in the second, third, or higher position within a layoff spell. It suggests that employers have waited for them to cross the cutoff before dismissing them.

Table 7: Fixed-effect logit of the propensity to bunch (odds ratio)

	Propensity to bunch	Propensity to bunch	Propensity to bunch	Propensity to bunch	Propensity to bunch (standardized coefficients)
Potential gain from CSP	2.471491***	5.540569***	2.171878**	4.701598***	1.131069***
	(0.850876)	(2.280127)	(0.698587)	(1.929778)	(0.036939)
Potential gain^2		1.195319***		1.171815***	1.195212***
		(0.062900)		(0.061894)	(0.071002)
Age			1.004217**	1.003589*	1.039698*
			(0.002019)	(0.002031)	(0.022870)
Level of education			1.031145***	1.029404**	1.060617**
			(0.012229)	(0.012221)	(0.025571)
Gender			0.932627	0.940633	0.970441
			(0.048924)	(0.049424)	(0.024998)
Being an executive			0.919180	0.911186	0.973689
			(0.080635)	(0.079979)	(0.024501)
Observations	20,106	20,106	20,061	20,061	20,061

** p<0.1, ** p<0.05, *** p<0.01. Standard errors in parentheses.*
NOTE: This table reports results from a firm fixed-effect model estimated using firms for which I observe several layoffs over the period. The propensity to bunch within a firm is regressed on individual characteristics and the level of the potential gain, measured by the gap in replacement rate at the one-year threshold. Bunching is defined as having a tenure at layoff between 365 and 398 days. I restrict the sample to tenure values between six and 18 months, to compare those bunching—having a tenure between 365 and 397 days—with those having a tenure close to but outside the bunching window. The last column reproduces the same specification as in column 4 using standardized variables to compare the magnitude of the coefficients. Neutralizing the effect of time-invariant firm characteristics, we still observe a positive impact of age and education, even after controlling for financial incentives. The effect on the log-likelihood of the successive inclusion of each variable and the standardized coefficients indicate that the rank of the variables in terms of explanatory power is the following: potential gain from CSP, level of education, age, gender, and being an executive.

Appendices

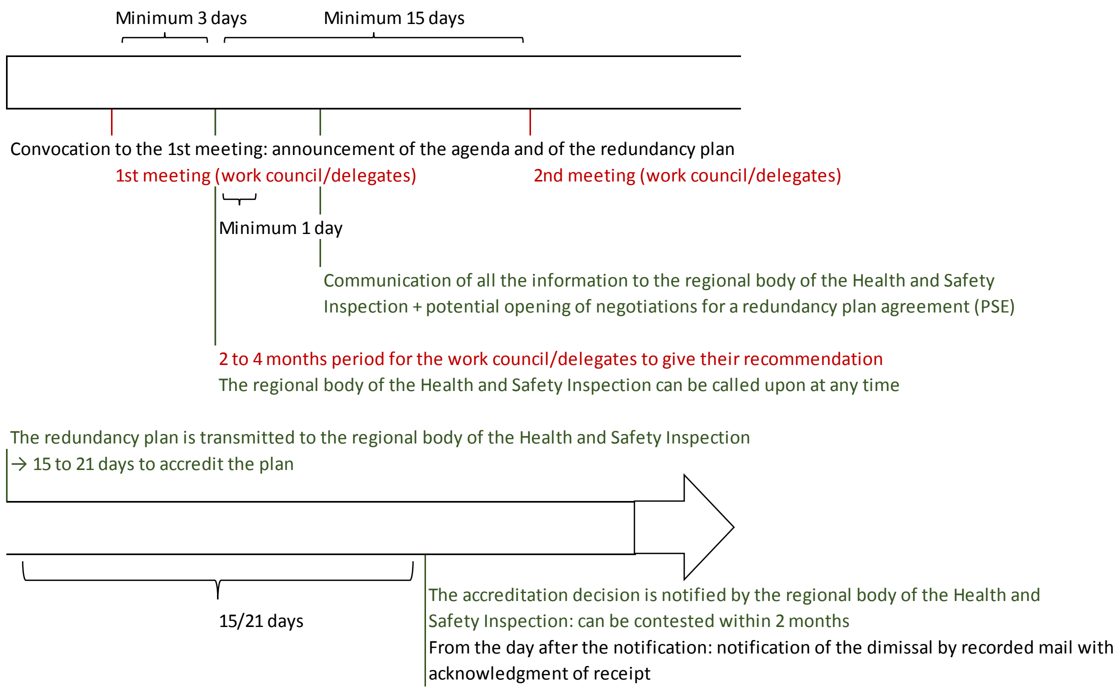
A Detailed institutional background

Table A1: Entitlements according to the worker's decision and tenure

Accepting the CSP		Refusing the CSP
Tenure < 365 days	Tenure ≥ 365 days	Whatever the tenure
Increased support + training	Increased support + training	Standard support + training
57.4% to 75% wage	80% wage	57.4% to 75% wage
compensation in lieu of notice	compensation in lieu of notice	
no waiting period	no waiting period	

This table details the consequences of accepting the CSP, depending on the tenure value. Workers accepting the CSP are all entitled to the increased support and training, but only those with a tenure value at least equal to one year are entitled to a benefit equal to 80% of their wage. Accepting the CSP also implies leaving immediately the firm (without further notice) and receiving benefits with no delay.

Figure A1: Economic layoff procedure



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

B Complementary analyses

B.1 Decision to accept the CSP

Table B1: Characteristics of laid-off workers accepting or refusing the CSP

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

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

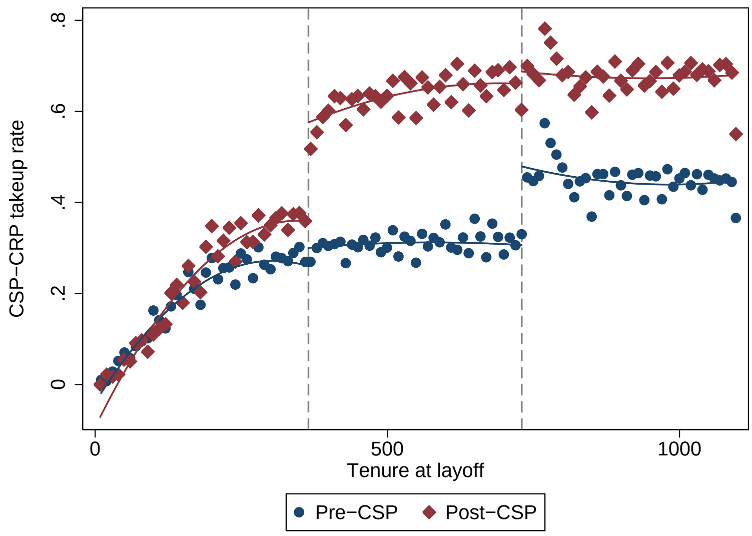
NOTE: This table reports the characteristics of workers laid off between October 2011 and September 2014 and eligible for the CSP (636,350 observations), according to their take-up status. It indicates that takers are, on average, older, more frequently female, more educated and skilled, and working in bigger firms in services or industry. Education ranges from 1 (no education) to 10 (5 years or more of higher education).

Table B2: Determinants of the probability to accept the CSP

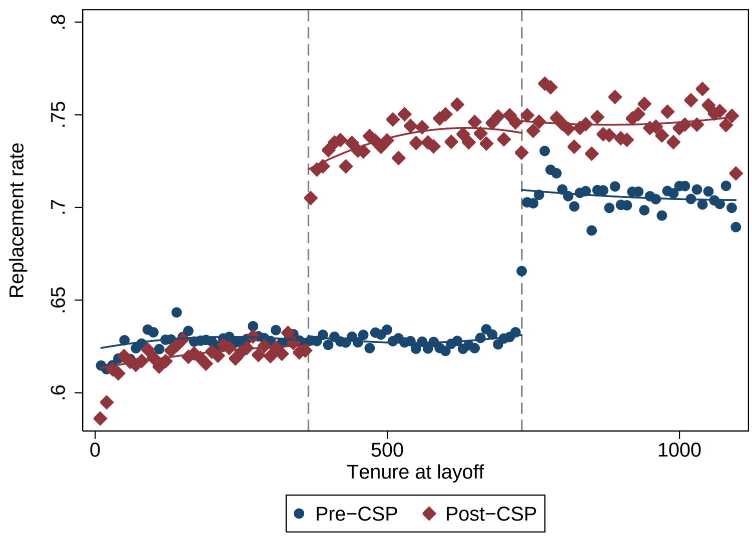
	Probability of accepting the CSP
Age	0.00003**
	(0.00001)
Female	0.0141***
	(0.00157)
Level of education	0.0363***
	(0.00039)
Unskilled employee	-0.0309***
	(0.00394)
Skilled worker	0.0152***
	(0.00323)
Skilled employee	0.0824***
	(0.00310)
Intermediate occupation	0.0969***
	(0.00477)
Executive	0.0516***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

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table reports a multivariate analysis of the determinants of take-up on workers laid off between October 2011 and September 2014 and eligible for the CSP. It indicates that the variables having the highest explanatory power are related to the skill and education level.
Education ranges from 1 (no education) to 10 (5 years or more of higher education). The reference category is unskilled worker in the manufacturing or agricultural sector.

Figure B1: Distribution of the CSP takeup rate and UI replacement rate



SOURCE: UI data (FNA).
NOTE: This graph plots the CSP-CRP takeup rate as a function of tenure at layoff, by period. In both periods, the takeup rate jumps dramatically at the relevant tenure threshold, where the replacement rate increases.

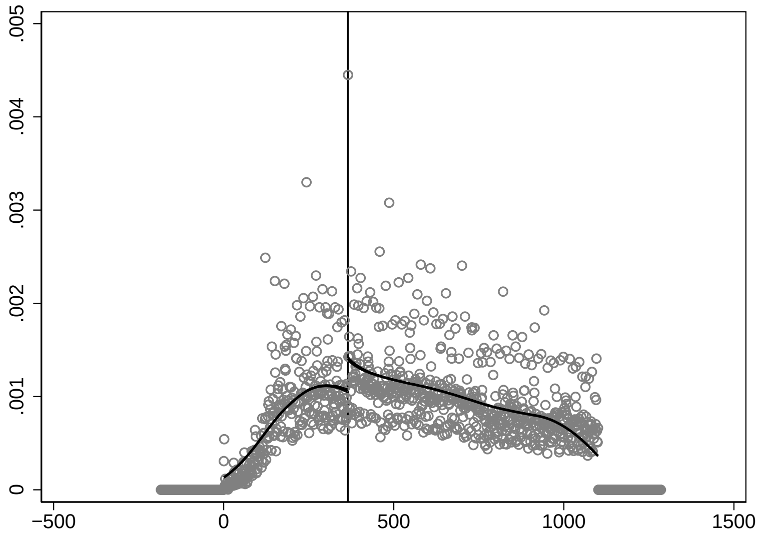


SOURCE: UI data (FNA).
NOTE: This graph plots the average actual UI replacement rate as a function of tenure at layoff, by period. In both periods, the replacement rate jumps dramatically at the relevant tenure threshold, which captures the legal increase in replacement rate induced by the CSP-CRP, attenuated by an incomplete takeup rate.

B.2 Bunching as a response to UI incentives

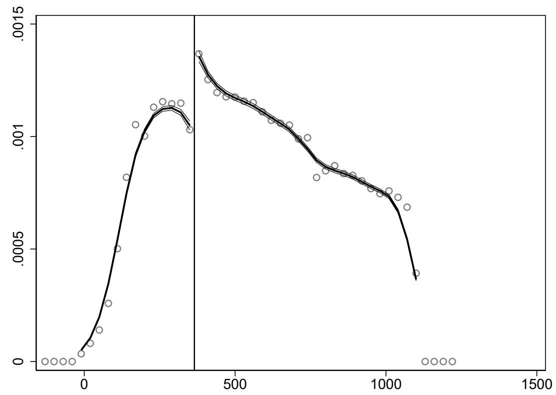
Figure B2: McCrary test on the tenure-at-layoff distribution at the 365-day cutoff (October 2011–September 2014)

(a) with optimal bandwidth



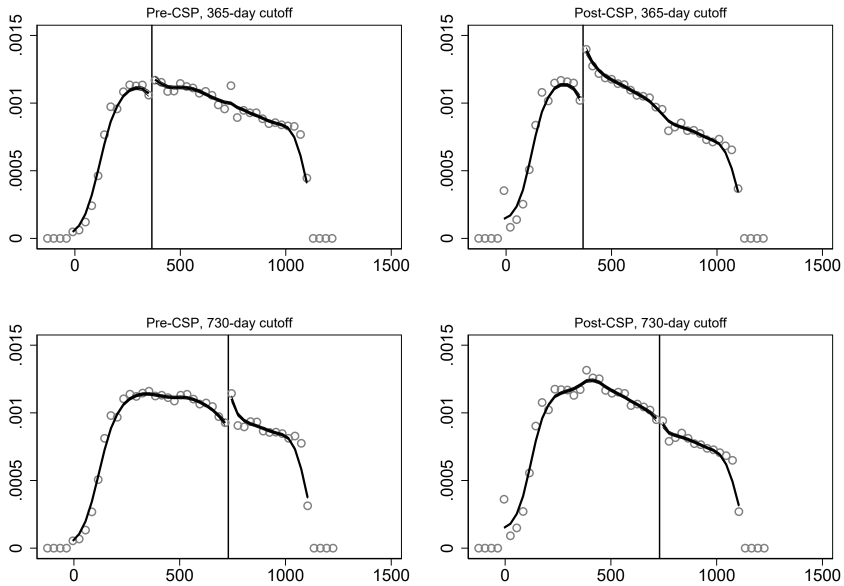
SOURCE: UI data (FNA).
NOTE: This graph illustrates the McCrary test (McCrary, 2008) on the density of tenure at layoff at the one-year threshold on the whole population of workers eligible for the CSP entering unemployment between October 2011 and September 2014 (optimal bandwidth computation).

(b) with 10-day binwidth



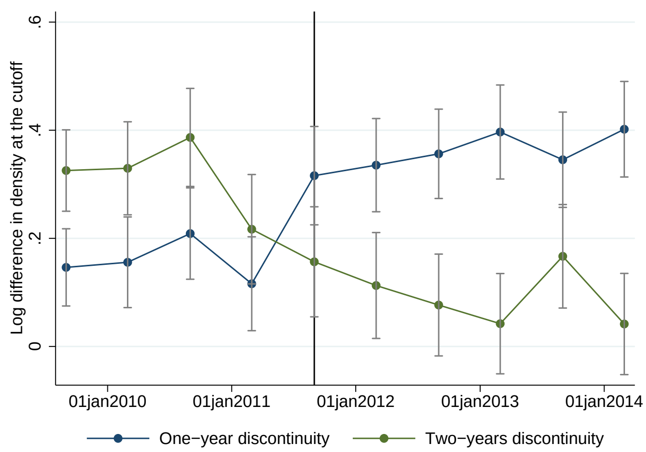
SOURCE: UI data (FNA).
NOTE: This graph illustrates the McCrary test (McCrary, 2008) on the density of tenure at layoff at the one-year threshold on the whole population of workers eligible for the CSP entering unemployment between October 2011 and September 2014. The size of the bin is 30 days, to reduce the regular spikes at each month interval (bandwidth: 100). The same graph with optimal bandwidth can be found in Figure B2a of Appendix B.2.

Figure B3: Test of discontinuity in the tenure density at the 365- and 730-day cutoffs for the two periods of interest



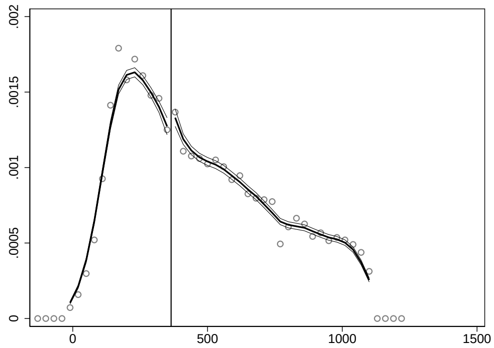
SOURCE: UI data (FNA).
NOTE: This graph reproduces the (McCrary, 2008) test on the density of tenure at layoff at the one- and two-year thresholds, respectively, on the whole population of workers eligible for the CSP entering unemployment in the periods September 2009–August 2011 and October 2011–September 2014 (1,118,847 observations). The size of the bin is 30 days, to reduce the regular spikes at each month interval (bandwidth: 100).

Figure B4: Evolution of the log difference in density at one and two years (2009–2014)



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

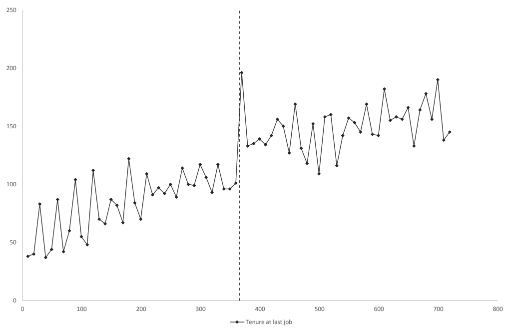
Figure B5: McCrary test on employers who did not offer the CSP (one-year cutoff, 2011–2014)



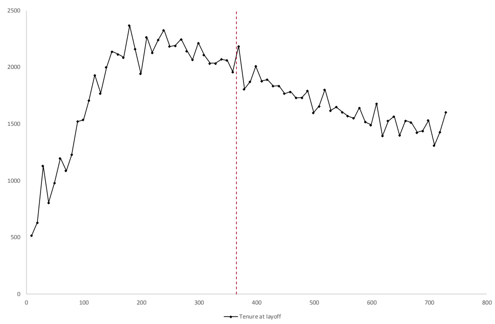
SOURCE: UI data (FNA).
NOTE: This figure plots the McCrary (2008) test on the subsample of workers eligible for the CSP, laid off between October 2011 and September 2014, who report not having been offered the CSP by their employer. The size of the bin is 30 days, to reduce the regular spikes at each month interval (bandwidth: 100). It is reasonable to think that employers who did not offer the CSP—although they have the legal obligation to do so—were not involved in bargaining over the extension of the contract for employees to obtain the higher CSP benefits. For them, I do not observe significant bunching other than related to a round-number effect (no hole before the cutoff or mass after). It provides additional evidence that the bunching is a response to CSP incentives.

Figure B6: Tenure distribution (France, 2011–2014)

(a) Eligible laid-off workers



(b) Non-eligible dismissed workers



SAMPLE: The whole population of workers separating between October 2011 and September 2014, who have been laid-off in firms of less than 1,000 employees (LHS) or dismissed for personal reason (RHS). MMO data. Binsize: 10.
NOTE: This figure plots the density of tenure at separation, respectively for laid-off workers eligible for the CSP (LHS) and workers dismissed for personal reason non eligible for the CSP (RHS). The data come from the mandatory form each employer in a firm of 50 employees or more has to return to the administration to inform on the employment flows. We observe a discontinuity at one year only for the sample of workers eligible for the CSP. It confirms that the discontinuity in the tenure distribution observed in the UI data does not come from a discontinuity in the UI registration rate, but from a discontinuity in the separation rate, that is driven by the CSP.

B.3 Extensive margin response

The bunching methodology allows us to measure the intensive margin response to an increase in the level of UI benefits, captured by the extension of the employment spell by a few days, conditional on the layoff. However, although legal constraints make it unlikely,[62] I cannot rule out the presence of an extensive margin response, that is, an increase in the total number of layoffs decided for tenure values in the bunching area after the implementation of the CSP. To measure such a response, I perform a difference-in-difference analysis to estimate the excess number of layoffs in the bunching window that would not have happened in the absence of the CSP. I take as a control group the workers with a tenure lying between eight and 10 months, because there is no reason that the CSP would affect the incentives to lay off in that tenure bracket. The first treatment group is defined as workers with a tenure between 12 and 14 months, because I expect a higher number of layoffs in that tenure bracket after the implementation of the CSP. Symmetrically, the second treatment group is defined as workers with a tenure between 10 and 12 months, where I expect fewer layoffs after the implementation of the CSP due to the postponement of the layoff date. In the absence of any extensive margin response, the excess and missing numbers of layoffs in the respective treatment groups should be equivalent. To avoid my estimation of the extensive margin effect to be confounded by changes in takeup behavior, I estimate the regression model using an administrative dataset gathering all employment flows in firms of more than 50 employees, not restricting to workers registering for UI. The dataset is built from mandatory declarations filled by employers each quarter (MMO, DARES).[63] Using these data instead of UI data alleviates the concern that the CSP may have affected the registration behavior differently in different tenure brackets, which would artificially affect the total number of layoffs in UI data. Table B3 displays the additional variation in the number of layoffs in each treated group after the implementation of the CSP relative to the control group. They show that the relative increase and decrease in layoffs are of similar magnitude, around 0.4 daily layoffs, on average.

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

[63] *La Direction de l'Animation de la Recherche, des Études et des Statistiques*, the Directorate for Research, Studies, and Statistics within the Ministry of Labor

Table B3: Difference-in-difference estimation of the number of excess layoffs

	Number of layoffs	
	Excess layoffs	Missing layoffs
Post-CSP	0.638***	0.615***
	(0.1824)	(0.1956)
12–14-month tenure *vs* 8 to 10-m	0.031	
	(0.0921)	
12–14-month tenure *vs* 8 to 10-m × Post-CSP	0.405***	
	(0.1264)	
10–12-month tenure *vs* 8 to 10-m		0.560***
		(0.0910)
10–12-month tenure *vs* 8 to 10-m × Post-CSP		-0.463***
		(0.1249)
Constant	1.149***	1.238***
	(0.1070)	(0.1122)
Observations	2094	2018

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
SOURCE: MMO data, DARES (top table) and UI data, FNA (bottom table). NOTE: This table reports the difference-in-difference estimates of the change in the number of layoffs in different tenure brackets before and after the implementation of the CSP. The first column compares the change in the number of layoffs in the 8–10-month tenure bracket (control group) and in the 12–14-month tenure bracket (treated group), where I expect excess layoffs. The second column compares the change in the number of layoffs in the 8–10-month tenure bracket (control group) and in the 10–12-month tenure bracket (treated group), where I expect missing layoffs. The numbers of layoffs are computed daily. Month and year fixed effects have been added. The idea of this regression is that, if the number of excess and missing layoffs coincides, the whole bunching response comes from a postponement behavior. I observe that there is approximately the same number of excess and missing layoffs after the implementation of the CSP. It indicates that there is no significant extensive margin response.

B.4 Theoretical framework with efficient bargaining

In the benchmark case, I assume that there is no uncertainty nor constraint on the transfer that ensures an efficient bargaining. The wage is therefore determined so that the employer is compensated for the cost of designing a one-year contract when the productivity no longer compensates the wage. The wage is assumed constant throughout the contract.

I consider the standard Nash bargaining framework where employers and employees maximize the product of their net utilities weighted by their respective bargaining power over w. As a result, the total surplus is split so that the firm gets a share β (capturing its bargaining power) and the worker a share $1-\beta$.

w^* is such that:

$$(1-\beta)\underbrace{[(P(L)-w)L]}_{Firm's\ profit}+\beta\underbrace{[(w-\alpha)L+UI(L)]}_{Worker's\ utility}=0$$
$$w^*=(1-\beta)P(L)-\beta[r_0w+\Delta rw\mathbb{1}_{L\geq L_{365}}-\alpha]$$

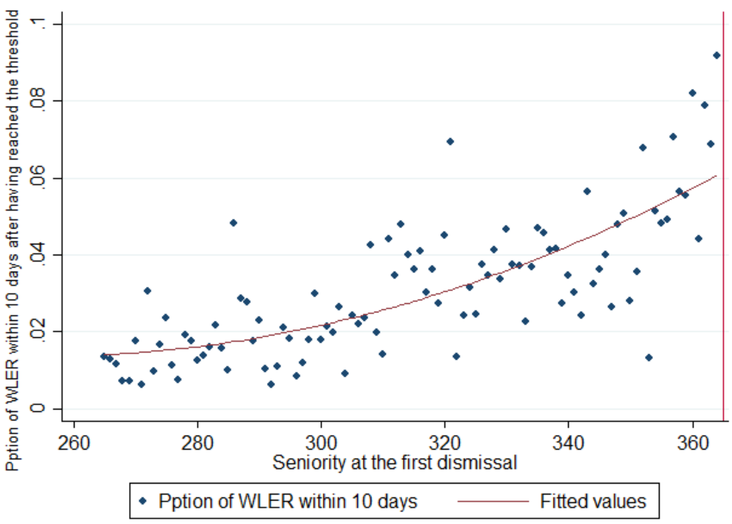
$$w^* = P(L) - \beta \underbrace{\left[P(L) + r_0 w + \Delta r w \mathbb{1}_{L \geq L_{365}} - \alpha\right]}_{\frac{Total\ surplus}{L}} \tag{7}$$

If the employer has full bargaining power ($\beta = 1$), the employee gets a wage equivalent to his outside option (the UI benefits plus the disutility from work) and the firm gets the whole surplus. On the contrary, if $\beta = 0$, the worker is paid at his full productivity and gets the whole surplus.

B.5 Additional evidence supporting the bargaining scenario

Evidence of a waiting strategy – I look at the relationship between the proportion of persons whose layoff seems strategically delayed and the value of tenure at the beginning of the layoff spell, in case of collective layoffs.

Figure B7: Proportion of workers laid off after a first layoff right after one year of tenure among workers laid off in the same redundancy plan (2011–2014)



SOURCE: UI data (FNA).
NOTE: This graph plots the proportion of workers: (i) laid off as part of a collective layoff plan; (ii) not laid off first (which suggests a waiting time); and (iii) laid off just (i.e., within 10 days) after reaching the one-year condition (which suggests the waiting time was related to the CSP), among workers fulfilling conditions (i) and (ii), as a function of the tenure value at the time of the first layoff of the redundancy plan. The redundancy plan gathers all layoffs from the same employer in a one-year period. This graph suggests that, in collective redundancy plans, employers wait to lay off workers initially right below the cutoff. They are not laid off first in the plan, which would allow them to reach the cutoff.

To construct Figure B7, I identify individuals: (i) laid off as part of a collective layoff plan; (ii) not laid off first (which suggests a waiting time); and (iii) laid off just (i.e., within 10 days) after reaching the one-year condition (which suggests the waiting time was related to the CSP). Figure B7 plots the proportion of people fulfilling these three conditions among people fulfilling conditions (i) and (ii) as a function of tenure at the time of the first layoff within the same collective redundancy. Indeed, this proportion rises as the gap between the tenure value at the time of the first layoff and the cutoff closes. This result suggests that the cost of waiting strategically increases with the initial distance to the cutoff.

This finding also confirms that the excess mass right above the one-year threshold comes from the area right below the threshold—consistent with the hole visible at the left-hand side of the cutoff (Figure B2)—which is in line with the hypothesis of strategic bargaining for people just below one year of tenure.[64]

Cost of extending the contract – The bargaining weighs the employee's benefit of moving to the high-benefit side of the 365-day cutoff against the cost for the employer to extend the contract. A likely hypothesis is that this cost varies across employers.[65] It might be higher for firms facing economic difficulties but continuing their activity than for firms definitively shutting down. For the latter, paying some workers additional weeks would not change the outcome, whereas it can put in jeopardy firms trying to overcome their difficulties. To have an idea of which firms are shutting down, I use two complementary definitions: a variable directly available in the data gives this information, but is not entirely reliable. For that reason, I compute another definition where firms shutting down lay off at least 80% of their workforce, or at least 200 individuals.[66] Table B4 indicates that being in a firm shutting down has always a positive and significant impact on the probability to bunch, no matter the definition of bunching or shutting down. The probability of bunching increases by 16% to 22% in firms shutting down, depending on the chosen definitions. These results show that employers seem more willing to grant contract extension when the firm is shutting down, and, therefore, when it represents a negligible cost for them.[67]

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

[65] The benefits of granting the extension may also vary among employers, as a function of the reputation risk for example. I exploit information on the local geographical area of the firm (French *départements*), available for a subsample of firms. I define the eight local areas with the highest number of firms (based on the SIRENE database that indexes all French firms) as corresponding to a high reputation cost, as we can expect that layoffs are more visible in dense areas (it has also been shown that plants closer to headquarters are more sensitive to social pressure in terms of hiring and firing decisions (Bassanini et al., 2017), which is more likely in dense areas). In this subsample, workers are 24% more likely to bunch in the dense areas, controlling for firm size, potential gain from the CSP, education and skill level of the worker.

[66] As firm size does not enter the computation of UI benefits, one may be worried about the quality of the information in the UI data. This variable is actually retrieved by the UI agency from another dataset that indexes all French firms (SIRENE database).

[67] One may be concerned that this result only captures a small-firm effect. Although the average firm size is more than twice as high in firms not shutting down than in firms shutting down when using the UI agency definition, if we look at the computed definition, the difference drops to 15 units (75 workers versus 90 workers on average), while there is still an effect on the probability to bunch.

Table B4: Effect of a firm shutting down on the bunching probability

	Probability of bunching (365-380-day tenure)		Probability of bunching (365-390-day tenure)	
Firms shutting down (computed definition)	0.0013*** (0.00031)		0.0020*** (0.00039)	
Firms shutting down (UI agency definition)		0.0012*** (0.00041)		0.0027*** (0.00052)
Constant	0.0075*** (0.00013)	0.0076*** (0.00012)	0.0120*** (0.00016)	0.0121*** (0.00015)
Observations	562154	562154	562154	562154

NOTE: This table reports the regression of a bunching dummy on an indicator for the firm shutting down. The definition of firms shutting down is provided by the UI agency (row 2), but as the variable is not entirely reliable, I compute another definition as a robustness check. Firms shutting down are defined as firms whose number of layoffs is at least equal to 80% of the workforce, or greater than 200. Bunching is defined as having a tenure at layoff lying between 365 and 380 days (columns 1 and 2) or between 365 and 390 days (columns 3 and 4). Being laid-off from a firm shutting down has a positive impact on the probability to bunch. It is compatible with the hypothesis that employers in firms shutting down no longer have anything to loose and are therefore more willing to grant the contract extension.

B.6 Characterization of the buncher

I use the methodology developed in Diamond and Persson (2016) to investigate the buncher profile by identifying the difference between the population eligible for bunching and the one bunching in terms of observable, predetermined characteristics. Summary statistics of the buncher can be recovered by applying the bunching methodology to individual characteristics instead of the tenure density. If I consider *passing the threshold* as a treatment, and my sample in a potential outcome framework, workers whose observed tenure falls into the bunching window and who are missing below the threshold can be considered compliers. Those under the threshold in the observed distribution can be thought of as never-takers because their tenure value makes them potentially eligible for manipulation whereas it did not occur. Finally, those whose observed and counterfactual tenure falls into the bunching window are always-takers.

For any observable characteristic X, I use workers outside of the manipulation region to estimate $E(X|s)$ at any tenure s inside the manipulation region:

$$X_{is} = \sum_{j=0}^{J} \beta_j \cdot (X_{is})^j + \nu_{is} \tag{8}$$

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

It provides an estimate of the expected value of any observable for each tenure bin, had there been no bunching. It can then be compared with the actual value of the variable in the bunching area above (respectively below) the threshold, denoted by $\bar{X}^{up\_actual}$ (respectively $\bar{X}^{down\_actual}$).

Workers located in the bunching area above the threshold include both "manipulators", who crossed the cutoff in response to the UI incentives, considered compliers, and workers who would have had the same tenure value absent UI incentives, who are always-takers:

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

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

Similarly, I define

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

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

I recover $\bar{X}^{down\_count}$ and $\bar{X}^{up\_count}$ by using extrapolation from equation 8 as well as the counterfactual density previously estimated with equation 2.

Equation 2 computed for the bunching regions below and above the cutoff allows the estimation of the number of always-takers and never-takers.

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

Finally, assigning equal weight to information obtained both from the excess and missing masses, it follows that:

$$\begin{aligned}\bar{X}^{compliers} = 0.5 \cdot &\left(\frac{N_{up}^{total}}{N_{up}^{total} - N_{up\_count}} \cdot \bar{X}^{up\_actual} - \frac{N_{up\_count}}{N_{up}^{total} - N_{up\_count}} \cdot \bar{X}^{up\_count}\right) + \\ 0.5 \cdot &\left(\frac{N_{down\_count}}{N_{down\_count} - N_{down}^{total}} \cdot \bar{X}^{down\_count} - \frac{N_{down}^{total}}{N_{down\_count} - N_{down}^{total}} \cdot \bar{X}^{down\_actual}\right).\end{aligned}$$

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

Compliers are, on average, older, more frequently female, more educated and more skilled, and work in smaller firms (Table B5).

Table B5: Characteristics of compliers, eligibles, and never-takers

	Compliers	Never-takers	Eligibles	Difference (2)–(1)	Difference (2)–(3)	Difference (1)–(3)
Age	36.34	35.57	35.58	-0.763***	-0.001	0.762***
	(1.801)	(0.112)	(0.042)	(0.128)	(0.008)	(0.127)
Gender	0.41	0.31	0.33	-0.095***	-0.017***	0.079***
	(0.027)	(0.004)	(0.002)	(0.002)	(0.000)	(0.002)
Education	7.13	6.22	6.31	-0.908***	-0.084***	0.824***
	(0.313)	(0.022)	(0.008)	(0.022)	(0.002)	(0.022)
Skills	2.89	3.52	3.47	0.631***	0.050***	-0.581***
	(0.182)	(0.013)	(0.005)	(0.013)	(0.001)	(0.013)
Working time	0.95	0.94	0.94	-0.007*	-0.002***	0.005
	(0.046)	(0.002)	(0.001)	(0.003)	(0.000)	(0.003)
Firm size	17.32	28.73	29.63	11.411***	-0.899***	-12.310***
	(4.645)	(0.938)	(0.472)	(0.335)	(0.074)	(0.330)

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The method described in Section B.6 is applied to workers eligible for the CSP, laid off between October 2011 and September 2014, to compare the characteristics of workers defined as compliers (bunching), always-takers (in the bunching area in the counterfactual and observed distributions of tenure at layoff), and never-takers (in the area below the cutoff in the counterfactual and observed distributions of tenure at layoff). Eligibles refer to workers in the area below the cutoff in the counterfactual distribution. The bunching boundaries are located at 324 days and 397 days. The area used to estimate the counterfactual is included between zero and 1,100 days. The polynomial fitting the tenure bin count is of order 4. Standard errors are calculated using a bootstrap procedure generating tenure distributions and associated estimates by random resampling. 200 replications.
Skills are given from a scale from 1 (high skills) to 6 (low skills). Education ranges from 1 (no education) to 10 (5 years or more of higher education).

Table B5 also shows the characteristics of the never-takers to gain insights into the bargaining frictions. Indeed, the never-takers are the ones for whom bargaining frictions are so high that they do not manage to negotiate the contract extension even though they are close to the tenure threshold.[68] They are significantly younger, less educated, and less skilled than eligible workers.

B.7 Round-Number Fixed Effects

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

A simple way of doing this is to add to the density regression round-number fixed effects, that is, use a dummy variable equal to one for each value of tenure around a multiple of 30. To account for the fact that a month lasts either 28, 29, 30, or 31 days, I choose the bandwidth such that the round-number dummy is equal to one for any number

[68]Never-takers are identified as those being in the missing mass area in the empirical distribution

i meeting the following condition: $k - 0.1 \leq \frac{i}{30} \leq k + 0.1, k \in \mathbb{N}$.

The regression model becomes:

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

where L_s is the tenure value in bin s; J is the order of the polynomial; $[L_l; L_u]$ is the excluded area around the notch point; $\mathbb{1}_{k-0.1 \leq \frac{L_s}{30} \leq k+0.1, k \in \mathbb{N}}$ is the round-number dummy; and $\mathbb{1}_{L_l \leq L_s < L^*}$ and $\mathbb{1}_{L^* \leq L_s \leq L_u}$ are set of dummies for being located in the missing and excess masses areas, respectively.

Adding these round-number fixed effects to the regression model does not substantially change the empirical results (Table B6). Not surprisingly, the estimated parameters are slightly smaller, because I remove part of the bunching only due to the regularity in hiring and firing dates.

Table B6: Reduced-form elasticity estimates with round-number fixed effects

L_u	398	398	398
L_l	312***	323***	342***
	(8.3)	(7.92)	(3.43)
Tenure window	[120;540]	[180;540]	[0;730]
b	5.19***	4.86***	4.1***
	(0.3307)	(0.3135)	(0.3314)
m	5.25***	4.92***	4.15***
	(0.3668)	(0.3494)	(0.3593)
Average change in replacement rate (in %)	11.98***	11.98***	11.98***
	(0.0003)	(0.0003)	(0.0002)
ϵ_{RF}	0.0161***	0.015***	0.0124***
	(0.0012)	(0.0012)	(0.0012)

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: Elasticity estimates are computed using the same formula as in Chetty et al. (2011) adjusted for the notch $\frac{(m/L^*)^2}{\Delta r/(1+r_0+L^*\Delta r/\Delta L^*)}$. The computation is adjusted with round-number fixed effects to account for the artificial bunching at each month interval, due to the regularity in contract starting and ending dates. The different columns represent different tenure windows considered in the estimation. Estimates are very close to those in the main estimation, ensuring that most of the bunching is due to the response to the CSP.

B.8 Sensitivity of elasticity estimates to the order of the polynomial

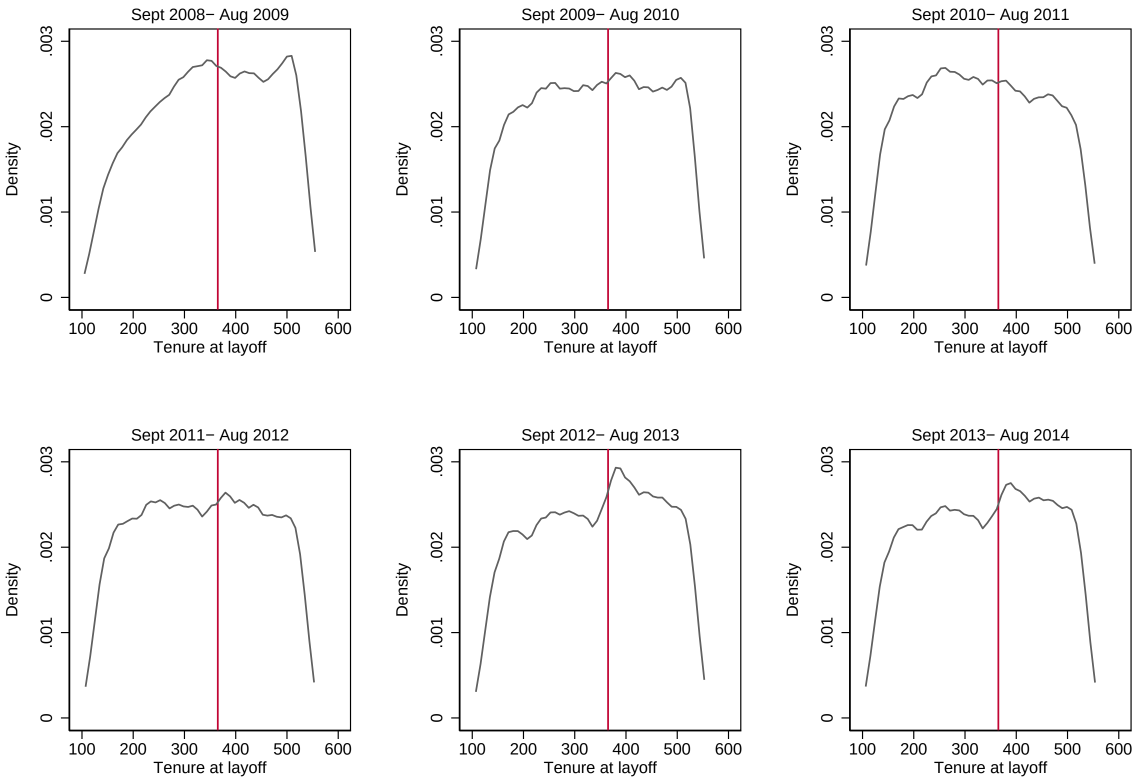
Table B7: Reduced-form elasticity estimates with alternative polynomial orders

Polynomial order	3	4 (baseline)	5	6	7
Tenure window	[180;540]	[180;540]	[180;540]	[180;540]	[180;540]
L_u	398	398	398	398	398
L_l	317***	323***	315***	308***	313***
	(9.21)	(11)	(6.45)	(4.69)	(11.38)
b	5.2***	5.01***	5.71***	6.35***	5.08***
	(0.3379)	(0.3478)	(0.3578)	(0.3585)	(0.5172)
m	5.22***	5.05***	5.65***	6.23***	5.21***
	(0.3616)	(0.37)	(0.3624)	(0.3562)	(0.4888)
% change in replacement rate	11.98	11.98	11.98	11.98	11.99
ϵ_{RF}	0.016***	0.0154***	0.0175***	0.0195***	0.016***
	(0.0012)	(0.0012)	(0.0012)	(0.0013)	(0.0017)

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: The elasticity is computed using the same formula as in Chetty et al. (2011) adjusted for the notch $\frac{(m/L^*)^2}{\Delta r/(1+r_0+L^*\Delta r/\Delta L^*)}$. More detailed explanations on the method are in Section 3.2. The different columns correspond to different polynomial orders used for the computation of the counterfactual. Standard errors are calculated using a bootstrap procedure generating tenure distributions and associated estimates by random resampling. 500 replications.
The elasticity estimates stay virtually unchanged when making the order of the polynomial vary.

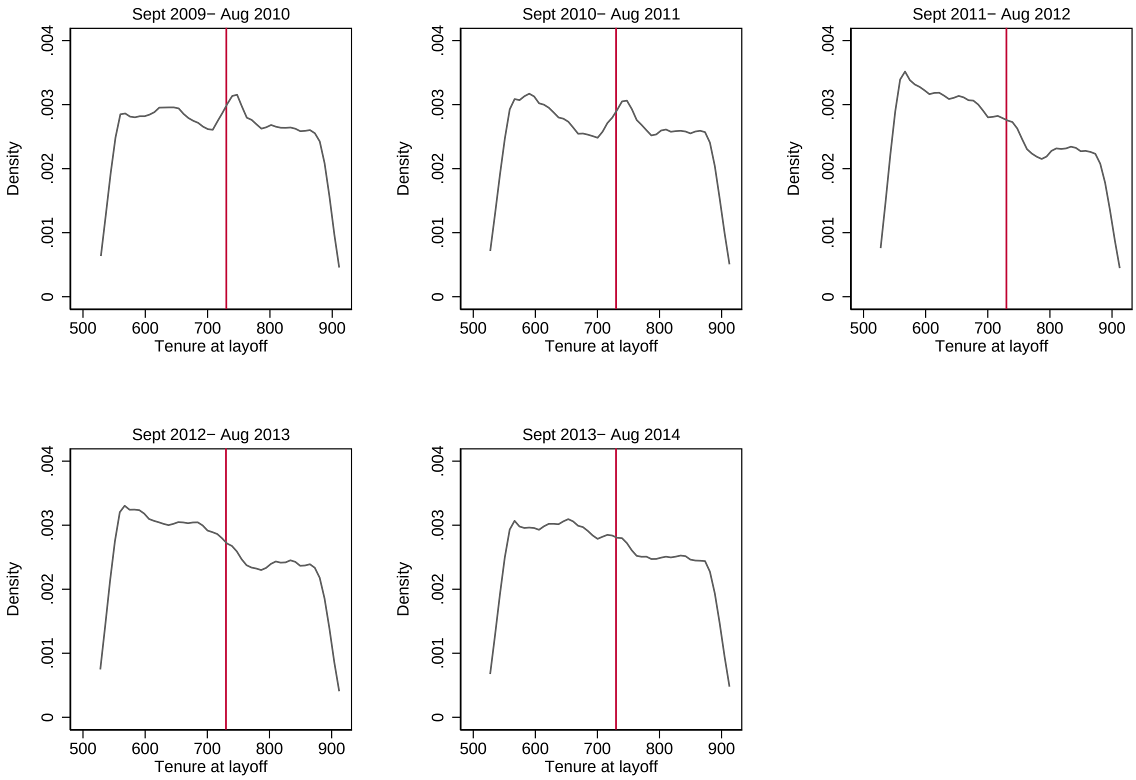
B.9 Difference-in-bunching

Figure B8: Yearly density of tenure at layoff around the one-year threshold



SOURCE: UI data (FNA)
NOTE: This graph displays the yearly density of tenure at layoff around the one-year cut-off. We observe that the density is pretty stable and comparable across years, until the introduction of the CSP (bottom row), where the bunching and missing masses appear. It supports the use of the difference-in-difference strategy and confirms that the difference-in-bunching elasticities cannot be explained by a change in macro conditions that would affect the distribution of tenure at layoff differently at different points in time.

Figure B9: Yearly density of tenure at layoff around the two-year threshold



SOURCE: UI data (FNA)
NOTE: This graph displays the yearly density of tenure at layoff around the two-year cutoff. We observe that the density is pretty stable and comparable across years, except around the two-year cutoff, where we observe a bunching and a missing mass, that disappear after the introduction of the CSP. 2008 has been excluded from the graph, as the CRP was not yet in place at that time. It supports the use of the difference-in-difference strategy and confirms that the difference-in-bunching elasticities cannot be explained by a change in macro conditions that would affect the distribution of tenure at layoff differently at different points in time.

B.10 Interaction between incentives and ability to bargain

Table B8: Gain distribution of the sample under two years of tenure

Gain category	Number of observations	Share of total sample
$Gain < 10pp$	5,916	3.47%
$10pp \leq Gain < 15pp$	20,311	11.9%
$15pp \leq Gain < 20pp$	62,288	36.5%
$Gain \geq 20pp$	82,189	48.2%

NOTE: This table presents the gain distribution of the sample of workers eligible for the CSP, laid off between October 2011 and September 2014, under two years of tenure. The gain is defined as the difference in replacement rate between the standard benefit and the CSP benefit granted to those with a tenure of one year or more. This difference increases with previous earnings, as the replacement rate associated with the standard benefit decreases with earnings. The majority of the sample has a jump in replacement rate higher than 20 pp.

Descriptive statistics by gain category – Belonging to a higher gain category is associated, on average, with a higher age, a higher level of education, a higher probability of being an executive, longer working hours, and higher earnings[69] (Table B9).

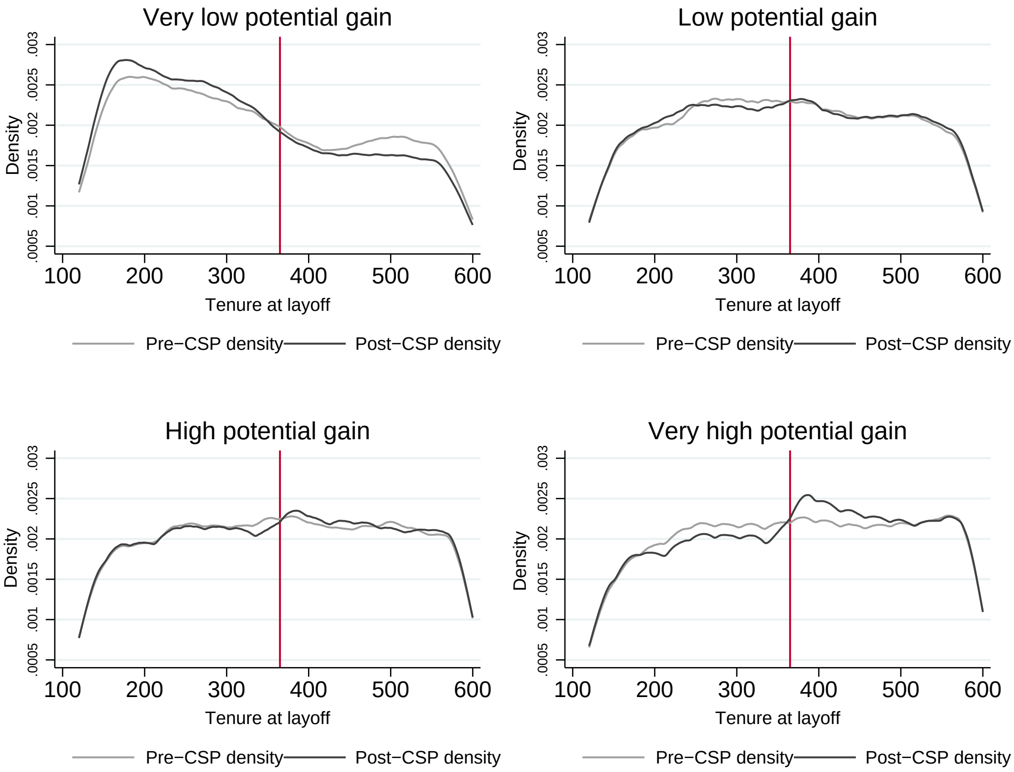
Table B9: Differences in observable characteristics by gain category

	Age	Sex	Education level	Proportion of executives	Proportion of intermediate professions	Proportion of unskilled employees	Proportion of skilled employees	Proportion of unskilled workers	Proportion of skilled workers
2^{nd} gain category	1.0625***	-.0573***	.0202	-.0013	.0017	-.0526***	.0224***	-.0032	.0329***
	(.1091)	(.0049)	(.0203)	(.0033)	(.0021)	(.0029)	(.0056)	(.0027)	(.0046)
3^{rd} gain category	2.0151***	-.144***	.279***	.0048	.0078***	-.1175***	.0246***	-.015***	.0954***
	(.1008)	(.0046)	(.0188)	(.0031)	(.0019)	(.0026)	(.0052)	(.0025)	(.0042)
4^{th} gain category	5.8161***	-.2507***	1.0104***	.173***	.0496***	-.1827***	-.038***	-.0577***	.0557***
	(.0996)	(.0045)	(.0186)	(.0031)	(.0019)	(.0026)	(.0051)	(.0025)	(.0042)
Constant	36.5246***	.5967***	5.6701***	.0055	.0041*	.2199***	.5228***	.1029***	.1448***
	(.0980)	(.0044)	(.01825)	(.003)	(.0019)	(.0026)	(.005)	(.0025)	(.0041)

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table presents descriptive statistics of the sample of workers eligible for the CSP, laid off between October 2011 and September 2014, divided by gain category. The reference category is the 1^{st} gain category. The gain is defined as the difference in replacement rate between the standard benefit and the ASP granted to those with a tenure of one year or more. This difference increases with previous earnings, as the replacement rate associated with the standard benefit decreases with earnings. The proportions of male, highly educated, and highly skilled individuals increase with gain category.

[69] The standard benefit replacement rate increases as earnings decrease, reducing the gap between the two types of benefit replacement rates.

Figure B10: Pre- and post-CSP tenure density around the notch by gain category



SOURCE: UI data (FNA). Tenure window: 120–600 days.
NOTE: This graph plots the density of tenure at layoff for workers eligible for the CSP, respectively, for the pre-CSP period (August 2009–August 2011) and the post-CSP period (October 2011–September 2014), decomposed by gain category. The vertical line corresponds to the one-year cutoff. Consistent with elasticity estimates computed by gain category, it shows no bunching for the 1^{st} gain category, and an increasingly pronounced bunching as the gain category increases. It means that the heterogeneity in bunching by gain category highlighted by the elasticity estimates reflects a true heterogeneity in response, and not a heterogeneity in the counterfactual distribution.

Elasticity estimates by cell

Table B10: Elasticity estimates by cell (small firms)

				Change in replacement rate (in %)	L_l	b	m	ϵ_{RF}
Industry, agriculture, building	Age < 45 years	Male	Low education	11.39***	324***	3.26***	3.26***	0.0097***
				0.08	21	0.98	0.89	0.0028
			High education	11.94***	345***	3.39***	3.63**	0.0109**
				0.12	9	1.26	1.42	0.0045
		Female	Low education	10.13***	315***	3.35*	3.43*	0.0105*
				0.22	15	1.84	1.87	0.0063
			High education	11.58***	331***	4.32**	4.80**	0.0150*
				0.19	19	2.03	2.45	0.0084
	Age ≥ 45 years	Male	Low education	11.97***	322***	3.50**	3.44**	0.0103**
				0.13	20	1.56	1.58	0.0051
			High education	12.86***	319***	2.48*	2.40*	0.0070*
				0.31	17	1.32	1.25	0.0038
		Female	Low education	10.51***	326***	3.22*	3.37*	0.0103*
				0.34	19	1.81	1.88	0.0062
			High education	11.82***	323***	1.67	1.66	0.0048
				0.37	16	1.32	1.24	0.0038
Retail, food and accommodation, services	Age < 45 years	Male	Low education	11.68***	322***	4.33***	4.54***	0.0139***
				0.10	17	1.34	1.40	0.0047
			High education	12.39***	310***	7.44***	6.89***	0.0218***
				0.08	13	1.20	1.24	0.0044
		Female	Low education	10.40***	321***	4.09***	4.39***	0.0136***
				0.11	20	1.40	1.44	0.0049
			High education	11.87***	315***	6.86***	8.12***	0.0265***
				0.08	10	1.06	1.24	0.0047
	Age ≥ 45 years	Male	Low education	12.29***	314***	5.53***	5.34***	0.0166***
				0.13	15	2.08	1.82	0.0062
			High education	13.24***	304***	13.14***	12.72***	0.0443***
				0.11	5	2.65	2.32	0.0099
		Female	Low education	10.89***	327***	5.71***	5.33***	0.0167***
				0.20	15	1.59	1.41	0.0049
			High education	12.47***	324***	7.79***	9.36***	0.0314***
				0.17	14	2.27	2.76	0.0107

** p<0.05, ** p<0.01, *** p<0.001. Standard errors are below the estimates.*

NOTE: This table displays elasticity estimates by cell, with their standard errors below. Elasticity estimates are computed using the same formula as in Chetty et al. (2011) adjusted for the notch $\frac{(m/L^*)^2}{\Delta r/(1+r_0+L^*\Delta r/\Delta L^*)}$. The area used for estimating the counterfactual is included between zero and 730 days. The polynomial fitting the tenure bin count is of order 4. Standard errors are calculated using a bootstrap procedure generating tenure distributions and associated estimates by random resampling. 100 replications. Small firms include those with less than 10 employees. Low education corresponds to having less than a high school diploma or at most a vocational high school degree.

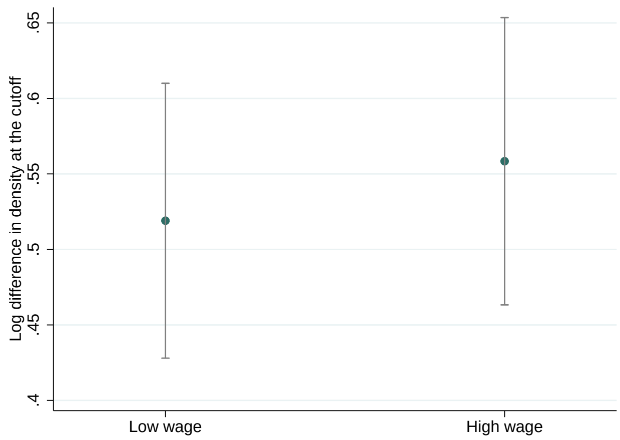
Table B11: Elasticity estimates by cell (large firms)

				Change in replacement rate (in %)	L_l	b	m	ϵ_{RF}
Industry, agriculture, building	Age < 45 years	Male	Low education	11.72***	311***	2.06**	1.98**	0.0057**
				0.09	11	0.91	0.92	0.0028
			High education	12.93***	307***	8.83***	8.71***	0.0285***
				0.11	11	2.22	2.03	0.0077
		Female	Low education	11.33***	323***	2.51	2.50	0.0074
				0.34	18	1.54	1.53	0.0048
			High education	12.40***	331***	3.18*	3.57*	0.0108
				0.16	22	1.85	2.03	0.0066
	Age ≥ 45 years	Male	Low education	13.05***	345***	2.71***	2.79**	0.0081**
				0.10	17	1.01	1.09	0.0034
			High education	13.93***	326***	2.37*	2.37*	0.0069
				0.17	21	1.42	1.43	0.0044
		Female	Low education	11.75***	322***	1.28	1.39	0.0040
				0.82	25	1.14	1.41	0.0043
			High education	13.26***	322***	1.37*	1.41	0.0040
				0.27	17	0.79	0.88	0.0026
Retail, food and accommodation, services	Age < 45 years	Male	Low education	12.18***	320***	4.10***	3.86***	0.0116***
				0.15	16	1.48	1.32	0.0043
			High education	12.92***	307***	6.29***	7.00***	0.0222***
				0.09	11	1.54	1.55	0.0056
		Female	Low education	11.04***	318***	3.79*	3.85**	0.0118*
				0.19	19	1.94	1.90	0.0064
			High education	12.32***	312***	7.11***	8.13***	0.0265***
				0.10	14	1.55	1.70	0.0063
	Age ≥ 45 years	Male	Low education	13.05***	323***	3.81*	4.01**	0.0121*
				0.16	19	2.00	2.00	0.0064
			High education	13.88***	319***	3.38*	3.48*	0.0104*
				0.13	19	1.81	1.85	0.0058
		Female	Low education	10.82***	316***	3.12	3.34	0.0102
				0.41	18	1.91	2.06	0.0068
			High education	13.47***	329***	2.70	2.97	0.0088
				0.22	20	1.65	1.86	0.0059

** p<0.05, ** p<0.01, *** p<0.001. Standard errors are below the estimates.*
NOTE: This table displays elasticity estimates by cell, with their standard errors below. Elasticity estimates are computed using the same formula as in Chetty et al. (2011) adjusted for the notch $\frac{(m/L^*)^2}{\Delta r/(1+r_0+L^*\Delta r/\Delta L^*)}$. The area used for estimating the counterfactual is included between zero and 730 days. The polynomial fitting the tenure bin count is of order 4. Standard errors are calculated using a bootstrap procedure generating tenure distributions and associated estimates by random resampling. 100 replications. Large firms include those with 10 employees or more. Low education corresponds to having less than a high school diploma or at most a vocational high school degree.

Impact of wage level on the probability to bunch

Figure B11: Magnitude of the bunching by wage half



SOURCE: UI data (FNA).
NOTE: This graph reports the coefficient from the McCrary (2008) test performed on workers eligible for the CSP, laid off between October 2011 and September 2014, divided into wage half, and restricted to workers with a wage above the threshold where the gap in replacement rate between the two types of benefits no longer varies. The log difference in the tenure density is computed for each half. It indicates that the bunching response increases as the wage increases with no variation in the level of incentives, although standard errors are large.

Table B12: Impact of the distance to the minimum wage on the probability to bunch

	Probability to bunch				
At the minimum wage	-0.02578***	-0.01706***	-0.01800***	-0.18683**	-0.11282
	(0.00284)	(0.00357)	(0.00358)	(0.07640)	(0.09063)
Potential gain from CSP		0.09365***	0.02382		0.66875
		(0.02582)	(0.02728)		(0.71415)
Age			0.00045***		0.00414**
			(0.00009)		(0.00205)
Level of education			0.00497***		0.03080***
			(0.00048)		(0.01191)
Gender			0.01187***		-0.06831
			(0.00200)		(0.05275)
Being an executive			0.02130***		-0.08268
			(0.00396)		(0.08797)
Firm fixed-effects	-	-	-	Yes	Yes
Constant	0.10338***	0.08517***	0.04605***		
	(0.00094)	(0.00518)	(0.00646)		
Observations	115438	114720	114539	20094	19944

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table displays estimates of the probability to bunch regressed on a dummy indicating if the worker was paid at the hourly minimum wage, and other control variables. The probability to bunch is defined as having a tenure lying between 365 and 398 days, as it corresponds to the bunching area defined in the bunching estimation (within an overall tenure window between 120 and 540 days). We observe that, even after controlling for the potential gain, being paid at the minimum wage has a negative impact on the probability to bunch. The inclusion of firm fixed-effects affects the precision of the results but does not change the conclusion. It is in line with a prediction from the model, i.e. workers with more resources to compensate the employer have a higher chance to bunch.

B.11 Bunching and collective representation institutions

Table B13: Distribution of the number of workers laid off by the same firm

Number of laid-off workers	Over the same 30-day spell		Over the full period	
	As a share of layoff spells	As a share of workers	As a share of firms	As a share of workers
1	68.49%	34.56%	58.79%	23.18%
2	16.41%	16.56%	18.68%	14.73%
3	6.19%	9.36%	8.23%	9.73%
4	2.95%	5.95%	4.26%	6.72%
5	1.71%	4.30%	2.52%	4.97%
6	1.06%	3.20%	1.68%	3.99%
7	0.69%	2.45%	1.14%	3.15%
8	0.49%	2.00%	0.84%	2.64%
9	0.35%	1.61%	0.60%	2.14%
10 and more	1.66%	20.01%	3.26%	28.76%

NOTE: This table reports the distribution of the number of workers laid off as part of the same layoff spell, with different definitions of the spell. The sample consists of laid-off workers eligible for the CSP. The full period of observation is October 2011–September 2014. A layoff spells gathers all layoffs occurring in the same 30-day period, because this is the legal period to assess whether the redundancy is collective or not. Most of the workers are laid off as part of individual or two-worker layoffs.

Using all the firms in my sample, I compute the elasticity estimate for individual and collective layoffs separately. Pointing to the individual bargaining scenario, Table B14 shows that the elasticity is much higher for individual layoffs. This result can be rationalized by the fact that one layoff date might be more easily scheduled according to UI incentives than several dates. It also strengthens the idea that the extension is negotiated in a one-to-one framework and that collective representation institutions are not necessarily involved in the process.

Table B14: Elasticity estimates by type of layoff

	Collective	Individual
ϵ_{RF}	.0071487***	.0235715***
	(.0011237)	(.00212)

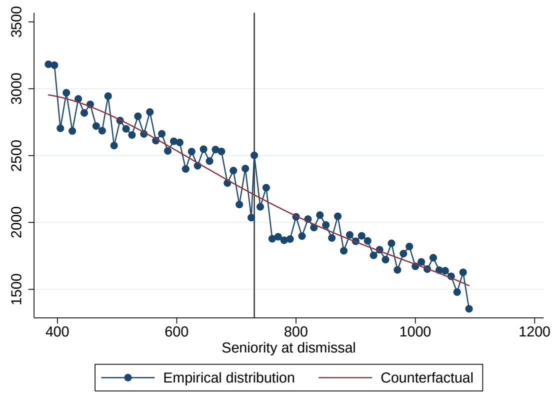
** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table displays elasticity estimates by type of layoff. Elasticity estimates are computed using the same formula as in Chetty et al. (2011) adjusted for the notch $\frac{(m/L^*)^2}{\Delta r/(1+r_0+L^*\Delta r/\Delta L^*)}$. The area used for estimating the counterfactual is included between zero and 730 days. The polynomial fitting the tenure bin count is of order 4. Standard errors are calculated using a bootstrap procedure generating tenure distributions and associated estimates by random resampling. 200 replications. Collective layoffs are defined as more than two workers laid off within the same 30-day-period within the same firm. The elasticity estimate is the highest for individual layoffs, presumably because the scheduling of a single layoff date is easier than the scheduling of multiple dates with multiple tenure values.

B.12 Bunching at the two-year threshold

As a robustness check, I use the existence of bunching at the two-year threshold (Figure B12) during the pre-CSP period to measure similar elasticities and to determine whether I obtain consistent estimates of the behavioral response over time.

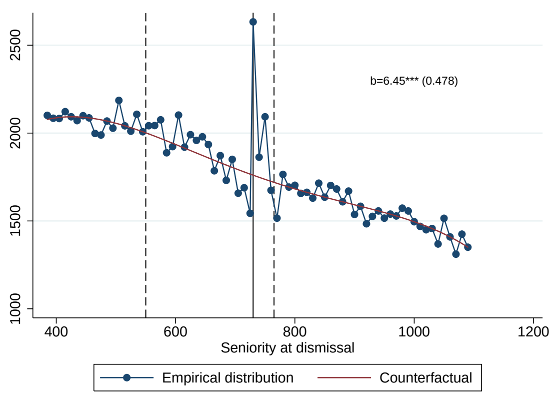
Figure B12: Empirical and counterfactual distributions of tenure at layoff at the two-year cutoff

October 2011–September 2014



SOURCE: UI data (FNA).
NOTE: This graph plots in blue the empirical distribution of tenure at layoff for workers eligible for the CSP, laid off between October 2011 and September 2014 (binsize: 10). The counterfactual distribution has been computed by fitting a 4^{th}-order polynomial, excluding an area around the notch, and extrapolating the distribution in the excluded area. The excluded area upper bound was determined visually where the bunching stops. The lower bound was found through an iterative process such that the excess and missing masses equalize (see Section 3.2 for more details). The solid vertical line represents the two-year cutoff, where there are no incentives to bunch during this period. The figure exhibits no significant bunching at the notch, other than related to a round-number effect.

August 2009–August 2011



SOURCE: UI data (FNA).
NOTE: This graph plots in blue the empirical distribution of tenure at layoff for workers eligible for the CSP, laid off between August 2009 and August 2011 (binsize: 10). The counterfactual distribution was computed by fitting a 4^{th}-order polynomial, excluding an area around the notch, and extrapolating the distribution in the excluded area. The excluded area upper bound was determined visually where the bunching stops. The lower bound was found through an iterative process such that the excess and missing masses equalize (see Section 3.2 for more details). The upper and lower bounds are represented by the dashed lines. The solid vertical line represents the two-year cutoff, where there are incentives to bunch during this period. The figure exhibits significant bunching at the notch, similar to that observed at the one-year cutoff between October 2011 and September 2014.

Table B15 shows that the elasticity estimates based on the average response are similar in both periods, although the bunching is fuzzier at the two-year threshold, which makes the missing mass area larger and the upper bound elasticity higher. These findings add strength to the idea that the bunching I measure during the post-CSP period is not related to some specificity at the one-year threshold or to something happening only from September 2011. The fact that it appears with the same magnitude at both thresholds and both periods suggests that it indeed captures a behavioral response to UI incentives.

Table B15: Reduced-form elasticity estimates at the two-year threshold during the pre-CSP period

L_u	765
L_l	586*** (9.4)
Tenure window	[0;1090]
b	6.38*** (0.4535)
m	5.61*** (0.4494)
Average change in replacement rate	11.99*** (0.0003)
ϵ_{RF}	0.0082*** (0.0007)

* *p<0.05, ** p<0.01, *** p<0.001*
NOTE: Elasticity estimates are computed using the same formula as in Chetty et al. (2011) adjusted for the notch ($\frac{(m/L^*)^2}{\Delta r/(1+r_0+L^*\Delta r/\Delta L^*}$). Standard errors of L_l are calculated using a bootstrap procedure generating tenure distributions and associated estimates by random resampling. 100 replications.

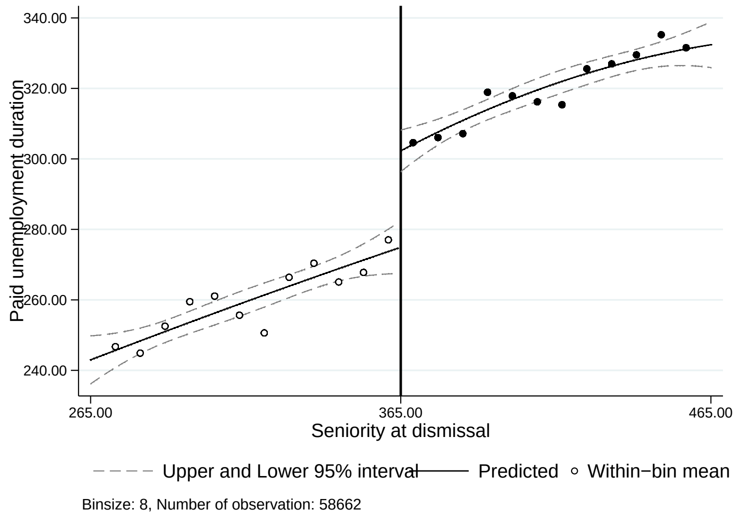
This setting with two different thresholds under two different periods could possibly be used to measure optimization frictions at the two-year threshold under the post-CSP period. Indeed, the presence of bunching, although very small, at tenure values that were relevant for optimization when incentives were different, is a sign of imperfect optimization, and can serve as a measure of optimization frictions.

However, Figure **??** shows that no significant bunching is present at the two-year cutoff after September 2011. This is confirmed by the fact that, when trying to estimate bunching at this cutoff, it is not possible to find any value for the missing mass lower bound that equalizes the missing and excess areas. It highlights the fact that most frictions in the bunching I observe come from negotiation frictions, so that tenure at layoff is the result of a bargaining process and is not entirely under the control of the worker.

B.13 Welfare analysis

The ultimate welfare impact of the extension of work contracts depends not only on the length of the employment spell but also on the length of the unemployment spell. It is a well-established fact that higher unemployment benefits are associated with longer unemployment duration (Schmieder and Von Wachter, 2016). I measure a 28-day (10%) discontinuous increase in subsidized unemployment duration at the one-year cutoff (Figure B13). This estimate mixes moral hazard and selection effect, precisely because there is bunching at the threshold. Using the methodology developed by Diamond and Persson (2016); Citino et al. (2019)), I retrieve an effect of higher benefits on the compliers, *i.e.* the bunchers, of 12 days. In any case, to have a complete picture of the welfare implications of this delay strategy, this effect on the unemployment spell must be taken into consideration.

Figure B13: Effect of CSP on paid unemployment spell duration



SOURCE: UI data (FNA).
NOTE: This figure shows the average unemployment spell duration with respect to tenure at layoff, with a sharp jump at the one-year threshold. It indicates that taking the CSP is associated with a longer unemployment spell duration, although the selection effect underlined by the existence of bunching cannot be distinguished from pure moral hazard. An unemployment spell has been defined as the addition of days on benefits with no interruption of more than four months, because four months is the minimum working time necessary to open a new UI entitlement.

I compute an estimate of the cost and benefits of the extension of the contracts from the point of view of the government. On the cost side, the direct loss due to the payment of higher benefits is doubled up with an indirect loss from the lengthening of the subsequent unemployment spell.[70] Using the same methodology as in section B.6, I compute the average daily wage (w), the unemployment spell duration (D), the daily standard benefit (SB), the daily CSP benefit (ASP), as well as the take-up rate ($t^{CSP}_{compliers}$) for the population of compliers and for the population of those who are eligible. It follows that:

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

[70]This indirect effect must be considered, although the CSP itself is limited to 12 months. After those 12 months, the job seeker can keep on receiving benefit but at the standard replacement rate.

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

B computed in section 3.2 gives the excess bunching, that is, the number of individuals in excess in the bunching area, who come from the area where a hole is observed.

On the other hand, a few more days of employment translate into more contributions paid to fund the UI scheme. Employers also contribute to the funding of the CSP by paying to the State part of the severance payment, S, they would have paid to the worker for any worker accepting the CSP. It means that the State receives more severance payment contributions only to the extent that there is a difference in take-up rate between compliers and those who are eligible.[71] With a total contribution rate of 6.4% over the whole period, we have the following.

$$\begin{aligned}\text{Individual difference in contributions paid} &= w_{compliers} * \Delta L * 0.064 + S * (t^{CSP}_{compliers} - t^{CSP}_{eligibles}) \\ \text{Aggregated difference in contributions paid} &= \text{Individual difference in contributions paid} * B\end{aligned}$$

ΔL gives the average response in terms of days of extension, and is derived from the measure of the bunching, as explained in section 3.2.

Using the values found in Table B16, the total additional benefits paid amount to 11,297,409€ over the whole three-year period. With total additional contributions paid equal to 1,861,068€, the net cost for the unemployment insurance is 9,436,341€.

[71] Other indirect benefits could arise if bunchers use their additional benefits to find a better quality job. Evidence on this topic is mixed (Card et al., 2007; Lalive, 2007; Van Ours and Vodopivec, 2008; Nekoei and Weber, 2017), and measuring such an impact is beyond the scope of this paper. From the buncher's point of view, additional benefit also allow consumption smoothing and potentially release liquidity constraints.

Table B16: Parameters used in the cost–benefit computation

w	76.69
	(0.002)
$D_{compliers}$	375.14
	(0.011)
$D^{above}_{compliers}$	354.26
	(0.015)
$D^{below}_{compliers}$	342.39
	(0.019)
$D_{eligibles}$	284.04
	(0.001)
$ASP_{compliers}$	65.24
	(0.002)
$SB_{compliers}$	45.28
	(0.001)
$t^{CSP}_{compliers}$	0.78
	(0.000)
$t^{CSP}_{eligibles}$	0.43
	(0.000)
ΔL	4.19
	(0.000)
B	1276.78
	(0.119)

Standard errors in parentheses.
NOTE: This table reports the parameters used for the cost–benefit computation of the bunching related to the CSP. Standard errors have been obtained by bootstrapping (1000 replications). The characteristics of the compliers and those who are eligible are computed using the methodology in Diamond and Persson (2016), as in section B.6. I compare the cost of bunching in terms of higher daily benefits and longer unemployment duration with the benefits in terms of additional contributions.

To put these figures into perspective, I compare them with the annual amount of CSP-related spending from the UI. UI accounts (Unédic, 2018) indicate annual net and gross CSP spending, but without considering either the standard benefit that would have been paid instead of the ASP or any behavioral response in terms of unemployment duration. Therefore, I also compute net spending without considering the counterfactual capital of benefits that would have been paid absent bunching (Table B17). Because workers with tenure value around one-year account only for a small share of workers taking the CSP, I also relate my figures to UI spending for workers taking the CSP with a tenure below two years.[72] Gross bunching-related cost accounts for about 4.5% of total gross spending for people taking the CSP under two years of tenure.

[72]For this comparison, I am only able to use gross spending because revenues cannot be disaggregated by tenure in the UI accounts.

Although this share is not huge, it exemplifies the distortions that can be created by UI design on the labor market.

Table B17: Relative annual cost of bunching without considering the counterfactual benefits

	Due to bunching	Overall	Ratio (in %)
Net spending	8,953,150	1,050,750,000	0.85%
Gross spending for CSP workers below two years of tenure	10,814,218	242,000,000	4.47%

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: These figures do not take into account what would have been paid to the workers absent the bunching. I have subtracted from the Unedic spending those figures related to the counseling and guidance part of the CSP, as well as those related to additional CSP-related benefits that I were not able to compute. I take the average values over the 2014–2017 period.

C Methodological discussion

The choice of a zero-inflated negative binomial model has been guided by the structure of the data. Indeed, supralegal severance payment is a necessarily positive amount, with a high frequency on the value zero,[73] and a variance much higher than the average. The Vuong (1989) test as well as the likelihood ratio test of $\alpha = 0$ both yield a significant output, indicating that the zero-inflated negative binomial model is better suited than the standard negative binomial model and the zero-inflated Poisson model.

Table C1: Zero-inflated negative binomial regression tests

Likelihood ratio test of $\alpha = 0$	7.1e+09 (0.000)
Vuong test	250.54 (0.000)

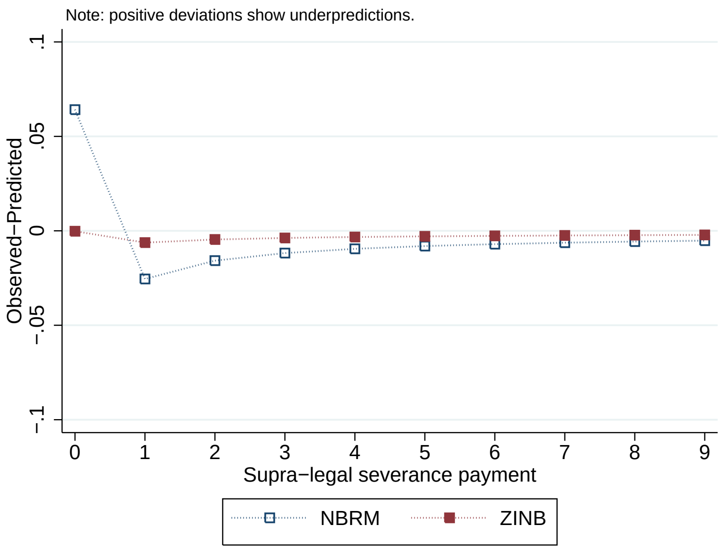
p-value in parenthesis
NOTE: This table displays results from the Vuong (1989) and the likelihood ratio test of $\alpha = 0$, showing that the zero-inflated negative binomial model is better suited than the standard negative binomial model and the zero-inflated Poisson model.

Figure C1 shows that, at least for the first 10 values, the zero-inflated negative binomial model does better at predicting the value of the supralegal severance payment than its standard equivalent.

Table C2 displays the regression output.

[73] I only consider here the *extra* amount paid to the worker, in addition to the legal minimum severance payment.

Figure C1: Comparison of the zero-inflated and standard negative binomial models



Source: UI data (FNA).
NOTE: The graph plots the difference between the observed and predicted values for the zero-inflated and the standard negative binomial models, for the values from zero to 10. It shows, especially for the value zero, that the zero-inflated model better fits the data.

Table C2: Zero-inflated negative binomial regression on supralegal severance payment

	Supralegal severance payment
Gender	0.1781
	(0.1578)
Age	0.4132*
	(0.1941)
Age^2	-0.0099
	(0.0053)
Age^3	0.0001
	(0.0000)
Level of education	0.3612
	(0.4766)
$Levelofeducation^2$	-0.1136
	(0.0940)
$Levelofeducation^3$	0.0080
	(0.0054)
Sector of activity	0.0061
	(0.0171)
Unskilled employee	0.6187*
	(0.2756)
Skilled worker	1.0540***
	(0.2502)
Skilled employee	0.5086**
	(0.1901)
Intermediate occupation	0.0783
	(0.2008)
Executive	0.7196*
	(0.3139)
Daily wage	-0.0002
	(0.0101)
$Dailywage^2$	0.0001
	(0.0001)
$Dailywage^3$	-0.0000
	(0.0000)
Firm size	-0.0001***
	(0.0000)
$Firmsize^2$	0.0000***
	(0.0000)
$Firmsize^3$	-0.0000***
	(0.0000)
Constant	2.0343
	(2.3626)
	Inflation equation
Level of education	0.0853*
	(0.0405)
$Levelofeducation^2$	-0.0293***
	(0.0082)
$Levelofeducation^3$	0.0015**
	(0.0005)
Unskilled employee	-1.1926***
	(0.0298)
Skilled worker	-0.2547***
	(0.0255)
Skilled employee	-1.2791***
	(0.0268)
Intermediate occupation	-1.3648***
	(0.0545)
Executive	-1.5476***
	(0.0400)
Age	0.1852***
	(0.0236)
Age^2	-0.0038***
	(0.0006)
Age^3	0.0000***
	(0.0000)
Constant	-3.2129***
	(0.2943)
$ln\alpha$	0.7568***
	(0.0601)
Observations	271,230

** p<0.05, ** p<0.01, *** p<0.001. Standard errors in parentheses.*
NOTE: This table displays results from the regression of supralegal severance payment on several covariates. The model used is a zero-inflated negative binomial regression to accommodate the specificity of the dependent variable, which takes only positive values with a large number of zeros. The regression is estimated on a sample of workers dismissed for noneconomic reasons after an open-ended contract, with tenure below two years. The reference category for qualification is unskilled worker.