I Z A Institute
of Labor Economics
Initiated by Deutsche Post Foundation

DISCUSSION PAPER SERIES

IZA DP No. 18014

Unemployment Insurance Eligibility and Employment Duration

Clément Brébion
Simon Briole
Laura Khoury

JULY 2025

DISCUSSION PAPER SERIES

IZA DP No. 18014

Unemployment Insurance Eligibility and Employment Duration

Clément Brébion
Rockwool Foundation Research Unit, Copenhagen Business School, Centre d'Etudes de l'Emploi et du Travail and IZA

Simon Briole
Paris School of Economics and University of Montpellier

Laura Khoury
IZA and University Paris-Dauphine–PSL

JULY 2025

ISSN: 2365-9793

IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Unemployment Insurance Eligibility and Employment Duration

While extensive research on unemployment insurance (UI) has examined how benefits affect workers' job search, little is known about how eligibility conditions shape firms' hiring decisions. These conditions, often requiring a minimum work history, affect the value workers place on contracts meeting the eligibility threshold. Exploiting a French reform that modified these requirements after 2009, we show that firms internalize workers' preferences and adjust contract durations to align with the new threshold. This reveals an overlooked ex-ante mechanism, where firms respond to UI incentives when posting vacancies—before meeting workers—rather than only through ex post adjustments. This response shifts contract duration distributions, also affecting workers already eligible for UI. Our findings have two implications: first, UI shapes firms' behavior at the vacancy stage, influencing job creation decisions ex ante, not just separation decisions ex post; second, UI eligibility conditions generate significant spillover effects.

JEL Classification: J08, J64, J65, H32

Keywords: unemployment insurance, employment duration, firm behavior, temporary employment

Corresponding author:
Laura Khoury
PSL Université Paris Dauphine
Pl. du Maréchal de Lattre de Tassigny
75016 Paris
France

E-mail: laura.khoury@dauphine.psl.eu

Introduction

The design of unemployment insurance (UI) systems has far-reaching impacts on labor market outcomes. The economic literature has widely documented the effect of the level and duration of benefits on non-employment duration (Schmieder et al., 2016; Le Barbanchon et al., 2024). By contrast, the impact of UI on employment duration and the role of eligibility conditions to qualify for UI benefits have received less attention. Eligibility conditions determine how much individuals must contribute to the funding of UI schemes to gain entitlement to UI benefits. They generally consist in a minimum work history condition.[1] These conditions introduce a sharp discontinuity in the value of unemployment for workers at the work history threshold, that may be internalized by employers. In addition to distorting job search, UI eligibility may therefore also affect hiring and separation decisions. In that sense, both labor supply and demand may respond to eligibility conditions, which would affect the equilibrium duration of employment.

The main contribution of this paper is to show that UI eligibility conditions affect not only job separations but also firms' job creation decisions. While most of the existing literature investigating the effects of UI on employment duration emphasizes the role of layoffs, we uncover a new mechanism through which firms adjust hiring strategies. We exploit a natural experiment in France, where a 2009 reform introduced a new UI eligibility threshold after four months of work history, increasing the value of contracts just above the threshold (4-month) relative to those just below (3-month). We show that firms internalize the impact of UI eligibility conditions for workers by designing more contracts whose duration lines up with the new work history threshold.

Building on an administrative dataset covering the full universe of employment relationships in France from 2005 to 2014, we compare the within-plant variation in the number of 4-month fixed-term contracts (FTCs) relative to 3-month FTCs, before and after the reform. Our analysis shows that the introduction of the new threshold led to an 11% increase in the number of 4-month FTCs relative to 3-month FTCs. This effect is stronger among firms with high separation rates or with a large share of hires from the unemployment rolls—firms that are arguably more aware of UI eligibility rules. It is also more pronounced in sectors facing labor shortages, where firms have greater incentives to adapt contract duration to attract workers. Examining the threshold that is removed after the reform (6-month), we find limited evidence of a symmetrical effect. The absence of a clear decrease in the number of 6-month FTCs relative to 5-month FTCs suggests that the removal of a threshold is less salient than its introduction.

We interpret the increase in the relative prevalence of 4-month FTCs compared to 3-month FTCs as the result of an *ex-ante* mechanism, whereby firms take into account the value of UI

[1] Eligibility conditions vary considerably across countries (Boeri and Van Ours, 2013; OECD, 2014) and within countries over time. For instance, in 2014, the minimum work history condition to qualify for UI benefits varied from 0 to 24 months across OECD countries (OECD, 2014). Within each country, policy makers also regularly update the value of this parameter, as it was the case in the 2020 UI reform in France.

eligibility for workers when posting a job vacancy, prior to meeting workers. To reinforce this interpretation, we leverage vacancy data and provide more direct evidence that the observed pattern is indeed driven by a job posting mechanism. Specifically, we find that, after the reform, firms post more vacancies for 4-month FTCs relative to 3-month FTCs. This ex-ante mechanism is compatible with a framework where firms weigh the probability of filling a vacancy against contract duration. A longer contract increases the risk of being affected by a negative productivity shock, while raising the vacancy filling rate. The introduction of a UI eligibility threshold induces a discrete upward shift in the value that workers assign to contracts that meet or exceed the threshold, relative to those that just fall short. This creates a discontinuity in labor market tightness at the eligibility threshold, incentivizing some firms to strategically cluster just above it when choosing contract durations.

Since firms do not observe the work history of their (future) hires when posting a vacancy, we test the hypothesis that this shift in contract durations also affect workers not seeking for UI eligibility. Consistently, we find a similar change in the distribution of 4- and 3-month FTCs even when focusing on individuals who were already eligible for UI benefits at the time of hiring. This result highlights a new form of externality, whereby UI eligibility rules affect not only workers seeking eligibility, but also those who are already eligible.

By contrast, we find no evidence of (*ex post*) bargaining between firms and workers regarding the timing of job separations. To investigate this issue, we analyze the evolution of layoffs at four months versus three months of tenure around the time of the reform and find no significant changes in separation patterns. One possible explanation is that, in France, hiring costs for short fixed-term contracts are relatively low—both in absolute terms and compared to firing costs (Kramarz and Michaud, 2010). Consequently, firms have little incentive to renegotiate contract durations or termination timing *ex post* and instead adjust their behavior primarily through *ex-ante* contract design.

To complete our analysis, we investigate whether the reform also had an impact on wages. While wages for short and low-skilled jobs tend to be rigid, they could also be a choice variable for firms who would trade an eligibility-granting contract against a lower wage. We test this hypothesis using administrative payroll data and find no significant effect of the reform on the relative wage of 4- and 3-months FTCs. Our estimates allow us to rule out effects larger than 0.9% in absolute value. Moreover, we find no evidence of compositional changes in observable worker characteristics that could mask a wage effect. These results provide little support for the idea that firms and workers share the rent associated with UI eligibility through a change in wage.

Our paper contributes to the literature on the impact of UI systems on employment. UI can influence employment through three main channels: (i) the job search channel; (ii) the separation channel; (iii) the job creation channel. Building on the seminal paper of Baily (1978), later extended by Chetty (2006), a predominantly microeconomic strand of the literature has extensively documented the first channel. Treating unemployment as a random and irreversible shock that severs the firm-worker relationship, these studies have examined the impact of UI on workers' incentives to search and accept jobs.[2] We revisit this question by examining the job creation and separation channels, on top of the traditional job search channel. We therefore shift the focus from (i) the effect on *non-employment* duration to the effect on *employment* duration; (ii) the *workers*' behavioral responses to the *firms*' response. Finally, we also look at the impact of UI eligibility conditions—a relatively understudied dimension compared to benefit levels or durations.

A second strand of the literature—rooted in the perspective of Feldstein (1976)—has emphasized how UI affects separations (i.e., the second channel). In this framework, firms strategically use temporary layoffs as a cost-minimizing response to demand shocks.[3] The availability of UI benefits encourages firms to rely more on layoffs, reducing the impact of demand fluctuations on hours worked while amplifying their effect on overall employment reductions. Although this view has received less attention in recent years—especially in the US, where short-term employment has declined—it remains highly relevant in dual labor markets such as in Europe, where short FTCs account for a significant share of employment flows.[4] Empirical evidence supporting this separation channel has been highlighted in a recent body of research showing that both potential benefit duration (Winter-Ebmer, 2003; Baguelin and Remillon, 2014; Jäger et al., 2023; Jessen et al., 2025) and UI eligibility conditions (Christofides and McKenna, 1996; Green and Riddell, 1997; Baker and Rea Jr, 1998; Jurajda, 2002; Rebollo-Sanz, 2012; Albanese et al., 2020; Martins, 2021; Van Doornik et al., 2023) affect job separations. Our contribution is to show that firms internalize UI incentives already at the contract posting stage, rather than only adjusting behavior after hires. In other words, we provide strong evidence that UI enters firms' optimization decisions *ex ante*, thereby shaping job offers and contract structures, rather than merely inducing *ex post* behavioral responses, such as layoffs.

Our paper also speaks to the literature on the macro vs. micro elasticity of employment with respect to UI. The micro elasticity captures the effect of UI on workers' search effort, failing to account for general equilibrium effects that could arise through changes in labor market tightness. UI extensions can generate important crowding-out effects, consistent with

[2] Many papers have highlighted the impact of UI generosity and potential benefit duration on unemployment duration (Lalive et al., 2006; Lalive, 2007; Landais, 2015) or reservation wages (Feldstein and Poterba, 1984; Krueger and Mueller, 2016; Le Barbanchon et al., 2017) although the last two papers cannot reject the null hypothesis of a zero effect. Reviews by Schmieder and Von Wachter (2016), Le Barbanchon et al. (2024) and Cohen and Ganong (2025) summarize recent works on this issue. Other theoretical papers have highlighted the impact of UI eligibility through a "re-entitlement effect", i.e., the fact that the prospect of UI eligibility makes current employment more attractive, and thus counteracts the standard disincentive effect of UI on job search (Mortensen, 1977; Ortega and Rioux, 2010). This entitlement effect affects the design of optimal UI (Hopenhayn and Nicolini, 2009; Zhang and Faig, 2012; Andersen et al., 2018).

[3] See also Baily (1977), which extends this perspective by providing a more general theoretical framework encompassing both temporary and permanent layoffs. This effect of UI on layoffs is also explained by imperfect experience rating, as shown by Topel (1983); Anderson (1993); Anderson and Meyer (2000).

[4] FTCs can be seen as the European counterpart to temporary layoffs, and are at the core of the adjustment margin we study. In France, between 2012 and 2019, FTCs accounted for 97% of recalls, with a recall rate of 52% (Charlot et al., 2024).

a job rationing model (Michaillat, 2012). In this model, UI impacts job finding rate through a "rat-race" mechanism: a lower search effort by some jobseekers increases the probability of matching for others if the labor demand response is limited (Landais et al., 2018). By contrast, our findings point to a different mechanism: firms adjust their vacancy postings in response to changes in UI eligibility rules. In that sense, our results are more in line with models that explicitly incorporate the job creation channel (Hagedorn et al., 2013, 2015; Mitman and Rabinovich, 2015; Jung and Kuester, 2015; Hartung et al., 2022).[5] These models allow firms to endogenously adjust their hiring behavior in response to UI incentives. Our evidence of firms shifting from 3- to 4-month FTCs after the reform directly supports this view. These general equilibrium effects operating through the firm channel are reinforced by the effects we find on indirectly treated workers—those who are already eligible for UI. In the spirit of Doniger and Toohey (2022), we document that spillover effects also exist in response to the eligibility criteria and are quantitatively important.[6] Not only does the work history criteria affect labor demand, but it does so for a whole segment of the labor market. Focusing on the group of directly treated workers, i.e. non-eligible workers, would ignore this indirect response.

Finally, we also contribute to the literature investigating the determinants of employment duration, in particular the role of labor market institutions. This literature shows that employment protection legislation plays a major role in the spread of temporary employment (Boeri, 2011; Bentolila et al., 2012; Cahuc et al., 2016; Bentolila et al., 2020).[7] By contrast, little attention has been paid to the impact of UI parameters on the rise in temporary work and job insecurity. Our main contribution to this literature is to shed light on a new mechanism through which labor market institutions may affect the segmentation of the labor market: to our knowledge, this paper is the first to document an impact of UI eligibility rules on the duration of FTCs.

The remainder of the paper proceeds as follows: Section 1 provides some institutional background on UI and temporary jobs. Section 2 presents our data. The following sections provide evidence on the impact of UI eligibility rules on employment duration (Section 3) and on wages (Section 4). Section 5 concludes.

[5] Whether the job rationing or the job creation channel prevails depends on the impact of UI on labor market tightness (Landais et al., 2018). If it is positive, as in Lalive et al. (2015); Marinescu (2017), the macro elasticity is smaller than the micro elasticity. If it is negative, the macro elasticity is larger than the micro elasticity. Note also that Johnston and Mas (2018) and Jessen et al. (2023) find a micro effect of changes in UI potential benefit duration on unemployment similar to the macro effect.

[6] Doniger and Toohey (2022) use changes in either UI benefit replacement rate or cap to show that non-directly affected workers respond to these changes, in line with a model including information frictions and *ex ante* wage commitments.

[7] See also Bentolila et al. (2020) for a recent review on this question. Note that an earlier strand of this literature emphasizes the influence of macroeconomic shocks on temporary employment duration in the US and Canada (Gray, 1978; Danziger, 1988; Wallace, 2001).

1 Institutional background

1.1 Unemployment Insurance

Like in most advanced economies, the French UI system relies on two components: an insurance part and an assistance part. The insurance part is characterized by a strong contributory link: what is paid to claimants is tightly linked to their contribution to the scheme. This general principle translates into three different rules: (i) eligibility primarily depends on a minimum work history condition;[8] (ii) the potential benefit duration (PBD) is proportional to work history; (iii) the amount of benefits and social security contributions is proportional to past earnings and is constant during the unemployment spell.[9] As for the assistance component, unemployed workers who exhaust their rights to UI benefits or are not eligible for them in the first place may receive assistance benefits. Upon reaching the work history threshold, workers experience a significant increase in the value of their outside option in the event of involuntary unemployment, shifting from assistance-level benefits to the more generous UI benefits. This increase is particularly pronounced for younger workers: individuals under 25 are not eligible for the main assistance benefits in France. For older individuals, the level of assistance benefits varies based on factors such as the number of dependent children. For instance, a single individual without children, living alone, and earning the minimum wage could expect approximately €800 per month in net UI benefits if eligible around the time of the reform. If not eligible, she would instead receive €460 from the main assistance benefits, roughly half the level of UI benefits. This gap widens for workers with higher previous earnings and, as noted, is largest for younger people.

The 2009 reform

Our empirical analysis leverages an exogenous change in the minimum work history requirement for UI eligibility to examine its impact on employment duration. Specifically, we exploit a UI reform implemented on April 1, 2009,[10] which introduced two major changes for individuals terminating a contract from that date onwards: (i) a reduction of the minimum work history condition, from 6 months over the last 22 months to 4 months over the last 28 months; (ii) the introduction of a one-to-one relationship between the number of days worked over the

[8]Besides the minimum work history condition, unemployed workers also have to fulfill the following requirements in order to receive benefits: (i) be younger than the compulsory retirement age; (ii) live on the territory where unemployment insurance is applicable; (iii) be physically able to work; (iv) the job loss must be involuntary, even though some cases of resignation open entitlements to UI; (v) be actively looking for a job and be available to work.

[9]The gross replacement rate is a decreasing function of the level of the previous gross wage within the range of 57.4% to 75%.

[10]The decree of March, 30^{th}, 2009 (*Arrêté du 30 mars 2009 portant agrément de la convention du 19 février 2009 relative à l'indemnisation du chômage et de son règlement général annexé*) is available here.

last 28 months and the PBD.[11] Table 1 summarizes these changes. The 2009 reform did not affect the other main features of the UI system described in the previous paragraph.

After the reform, the minimum work history required for UI eligibility was reduced to 4 months. We use this natural experiment to identify the effect of eligibility conditions on employment duration (Section 3). While the reform also affected the PBD, the change was marginal at the 6-month eligibility threshold. We cautiously check that our results are not affected by this small change.

Importantly for identification, workers and firms could not anticipate the change in the eligibility parameters implemented in April 2009. The reform aimed at: (i) providing a replacement income to non-employed workers with a short work history, especially those under the age of 25;[12] and at (ii) simplifying the complex rules which determined the potential benefit duration at that time (see Table 1). The purpose of the law was stated as early as January 2008 in a collective agreement at the national level between trade unions and employers' associations who manage the UI system in France. However, the actual parameter change in the eligibility criteria—from 6 months to 4 months—was not decided before late December 2008, as a result of a long negotiation process between employers' and employees' representatives.[13] The bargaining process over the parameters of the UI system is known for being highly non-transparent and not well covered in the media, and the risk of anticipation behavior is therefore limited.

Table 1: Main changes in the regulations governing unemployment insurance rules over the 2005-2015 period

Contract termination	Minimum work history	Potential benefit duration according to the work history (WH)
Jan 1, 2002 to Jan 17, 2006	6 months over the last 22 months	7 months if WH $\geq$ 6 months over the last 22 months 23 months if WH $\geq$ 12 months over the last 24 months
Jan 17, 2006 to April 1,2009	6 months over the last 22 months	7 months if WH $\geq$ 6 months over the last 22 months 12 months if WH $\geq$ 12 months over the last 20 months 23 months if WH $\geq$ 16 months over the last 26 months
April 1, 2009 to Oct 1, 2017	4 months over the last 28 months	1 to 1 relationship up to 2 years

NOTE: This table offers an overview of the main changes affecting unemployment insurance rules over our period of analysis (2005-2015). The 2006 reform does not affect contracts shorter than 12 months and is therefore expected to be neutral for employers' and employees' behavior with regards to 4- and 6-month contracts. After the 2009 reform, the PBD cannot exceed 2 years, except for workers older than 50 years, for whom the maximum PBD is 3 years.

[11] Before the reform, the PBD was a step function of workers' work history.

[12] As mentioned, in France, individuals under the age of 25 are not eligible for the assistance benefits (*Revenu de Solidarité Active*)

[13] The text of the January 2008 agreement is available here, and the text of the December 2008 agreement here.

1.2 Temporary employment

Non-standard forms of employment have rapidly expanded since the 80's in most developed economies, including France. As a result, labor market segmentation and inequalities among workers have been increasing (Kalleberg et al., 2000; Boeri, 2011; OECD, 2014). These jobs are predominantly held by vulnerable workers (Kahn, 2010). They create uncertainty and are most often not stepping stones to stable employment.[14]

The share of temporary jobs in total employment has steadily increased over the past three decades in OECD countries (Figure B1). This trend is particularly pronounced in European countries, where it has risen from 9% in 1980 to 15% in 2019, in part because these countries have implemented several policies liberalizing the use of fixed-term work contracts (Berton and Garibaldi, 2012; Boeri and Van Ours, 2013). In contrast, the regulation of permanent contracts has been left essentially unaltered, resulting in dual labor markets (Jahn et al., 2012).

Temporary employment in France

Fixed-term contracts (FTCs) were introduced in the late 70's in France in order to reduce the cost related to job separations (Blanchard and Landier, 2002).[15] They are used to hire workers on a specific and temporary task and, except in cases of serious misconduct on the part of the worker, they cannot be terminated before their end.[16] Employment protection is much higher for permanent contracts than for FTCs, resulting in a dual labor market in which transition rates from fixed-term to permanent employment are relatively low (Bentolila et al., 2012). This duality is reflected in average monthly job destruction rates: over the pre-reform period (2005–2008), 29.5% of FTCs were terminated each month, as compared to 3.2% for open-ended contracts.[17] Figure B2, which plots the evolution of the monthly destruction rate by contract type, also shows the stronger cyclical sensitivity of FTCs as compared to permanent contracts.

Like in other OECD countries, the share of FTCs in total employment has been growing

[14] See for example Güell and Petrongolo (2007); David and Houseman (2010); Givord and Wilner (2015) or García-Pérez et al. (2019) on the absence of stepping stone effect. Note also that workers under this type of contract also suffer a higher risk of adverse health effects (Virtanen et al., 2005), exhibit higher levels of job dissatisfaction and insecurity (Clark and Postel-Vinay, 2009) and have more unstable family situations (Landaud, 2021).

[15] Permanent contracts are subject to a high level of employment protection and firms can lay off workers under this type of contract only for two reasons: (i) personal, in which case firms have to prove that the worker is unable to do the job for which she was hired; (ii) economic, in which case, firms have to prove that they need to downsize. Except in cases of serious misconduct on the part of the worker, firms are required to grant a notice period and a severance pay to the worker. Note also that systematically hiring workers under permanent contracts and dismissing them during the probationary period instead of using temporary contracts is illegal (Cahuc et al., 2020).

[16] Examples of reasons to hire under a FTC are: (i) the replacement of an employee on leave; (ii) a temporary increase in activity; (iii) a seasonal activity; or (iv) to facilitate employment of targeted groups (youth, long-term unemployed, etc.). See e.g., Cahuc et al. (2016) for more details on the legal rules for the termination of temporary contracts in France.

[17] These numbers were computed by the authors, based on the MMO dataset which is described in Section 2.

in France. It increased more than threefold between the mid 80's and 2019 (from 5% to 16%, Figure B1). The importance of FTCs is even more striking in terms of hiring flows. In our data, we measure that FTCs accounted for 62% of all hiring flows during the pre-reform period (2005-2008) (Table B2; see also Givord and Wilner (2015)). Beyond their prevalence, FTCs are strikingly short: 57% of the FTCs finishing in the pre-reform period lasted less than one month, and 93.5% lasted less than a year. This indicates that a large proportion of labor market flows involve FTCs shorter than one year. These trends have strengthened in more recent years (Milin, 2018). As expected, FTCs also strongly contribute to the unemployment inflows. Over the pre-reform period, 75% of individuals registering as unemployed were previously employed under FTCs, as opposed to 25% for permanent contracts (Table B1). Our main empirical analysis focuses on the new 4-month cutoff introduced by the UI reform. FTCs of 3 and 4 months combined represent 4.9% of all hiring flows, 7.9% of all FTCs, 18.9% of the FTCs longer than a month (Table B2) and 5.6% of total unemployment inflows during this period (Table B1).

2 Data and working samples

To analyze the impact of UI eligibility on employment duration, we mainly draw on an administrative database which tracks firms over the 2005-2014 period and covers the universe of work contracts in France, namely the *Mouvements de Main d'Oeuvre* (hereafter, *MMO*). All other datasets exploited in this paper are described in Appendix A.

2.1 The MMO data

The MMO dataset is provided by the French Ministry of labor and comes from monthly records that employers from private and public plants with at least 50 workers as well as self-employed workers must fill.[18] It includes all employment inflows and outflows in those plants and provides information on the date and type of flows (separation motive, nature of the contract), on workers' characteristics (gender, nationality, occupation, month and year of birth, municipality of residence) and on plants' characteristics (size, economic sector). Importantly, it also includes a unique identifier per plant, allowing us to track plants over time.[19]

The MMO data have the notable advantage of providing accurate information on the start and end dates of employment contracts as well as the reason for separation. The unit of observation is therefore the work contract. As French law imposes a waiting period between two successive FTCs, the vast majority of successive FTCs are observed as two separate observations in the MMO data. However, job-to-job transitions that involve the same employer and

[18]Plants of less than 50 employees are surveyed to complement the dataset.

[19]Note, however, that the MMO dataset does not include a worker identifier for our period of analysis, such that it is impossible to follow workers over time.

do not require a waiting period—such as FTCs converted directly into permanent positions—are treated as a single employment spell. In the analyses presented in Section 3.1, we therefore need to assume that conversion behavior—and immediate renewal behavior in the few cases where the waiting-period rule is not binding—did not change *differentially* by contract duration after the reform. Although we cannot formally test this assumption, we provide evidence supporting it in Appendix C1, based on the French *Labor Force Survey*. In addition, we show in Section 3.2.1 that our main results are robust to using vacancy data, which do not suffer from this limitation.

Our data gathers all contracts ending between 2005 and 2014, disaggregated by separation type. A vast majority of the observations corresponds to ends of FTCs (21,471,092). Other separation motives mainly include quits (3,113,375), personal dismissals (1,359,754), end of trial periods (895,680) and retirement (781,744). Economic layoff (330,480) and pre-retirement (15,825) are the least observed motives.

Working sample

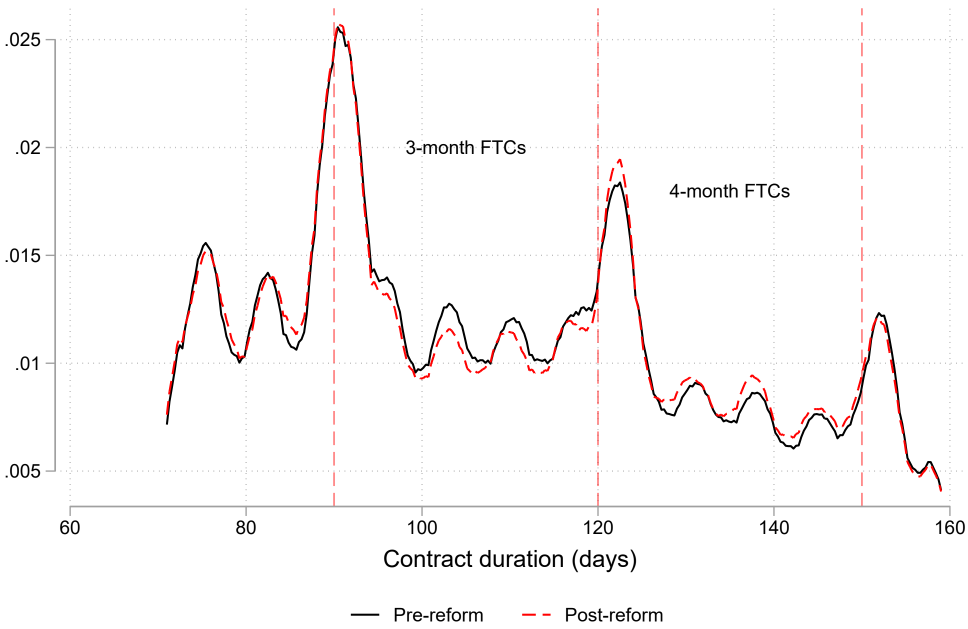
Our main empirical analysis focuses on the new cutoff introduced by the UI reform at 4 months. Our main analysis focuses on the sample of plants who had at least one FTC of 3 or 4 months—that is, +/- 30 days around the cutoff—over the pre-reform period (2005-2008).[20] Our main working sample includes 45,295 plants and covers a large share of the FTC labor market: plants in our sample represent 27% of the plants hiring workers under FTCs in the private sector before the reform (2005-2008), and account for two-thirds of the FTCs signed in the economy over this period. They signed about 400,000 3- and 4-month FTCs over this period, which represents 7.9% of all FTCs signed in the private sector before the reform or 10.7% if we consider employment duration.[21] In our empirical analyses, we also look at the potential effect of the reform on other cutoffs. When we do so, we construct the working sample along the same lines: for example, when working on the 6-month cutoff, we use the sample of firms who had at least one FTC of 5 or 6 months over the pre-reform period. Figures 1a and 1b depicts the distribution of contract duration observed in the full sample of FTCs, focusing on the 2007-2010 period (i.e., a short symmetrical window around the reform). Both figures show a clear regularity in contract duration at the month and, to some extent, week level. More importantly, Figure 1a reveals a clear shift from 3-month contracts to 4-month contracts after the reform: the post density lies under the pre density for every duration between 3 and 4 months, while it lies over the pre density for every duration between 4 and 5 months. Such "reallocation" is less

[20]We check that our main result is robust to using the whole sample of plants, without this restriction (see Appendix B). In the empirical analysis, the daily contract duration is rounded down to the bottom monthly value. We therefore call "4-month contracts", contracts of duration between 122 and 151 days and "3-month contracts", contracts of duration between 91 and 121 days.

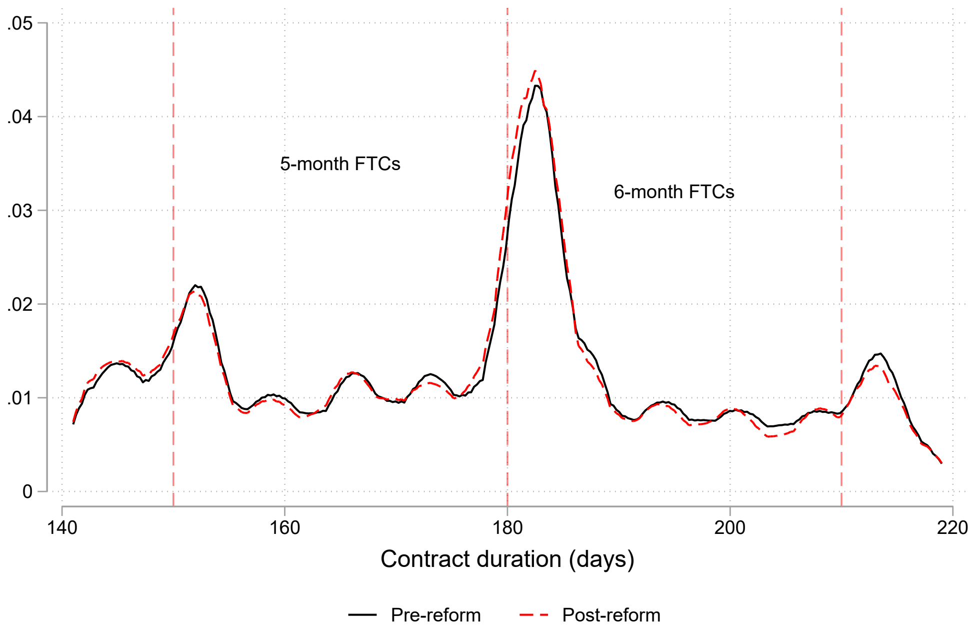
[21]As shown in Table B2, short FTCs are disproportionately found in the French economy. Therefore, the share of FTCs signed by firms in our sample among all FTCs of at least a month increase to 18.9% and 11.4% in terms of number of contracts and employment duration respectively.

visible to the eye in the vicinity of the 6-month cutoff (Figure 1b). We thoroughly investigate the effect of the reform on these two cutoffs in Section 3.

Figure 1: Distribution of the duration of fixed-term contracts before and after the reform



(a) 4-month threshold



(b) 6-month threshold

Note: This figure shows the distribution of fixed-term contract duration in the *MMO* dataset, over the 2007-2010 period. The black solid curve corresponds to the pre-reform period (2007-2008) and the red dashed curve to the post-reform period (2009-2010). The vertical lines correspond to the 3, 4 and 5-month thresholds in Figure 1a, and to the 5, 6 and 7-month thresholds in Figure 1b.

2.2 Other datasets

Complementary analyses rely on five additional datasets: (i) the FNA dataset, i.e. data from the unemployment insurance agency; (ii) the FH-DADS dataset, which matches payroll data with UI data; (iii) the French Labor Force Survey; (iv) Vacancy data from the French Public Employment Services; (v) the *Réponse* survey asking employers and employees about the state of employment relationships. All these datasets are described in details in Appendix A.

3 The impact of UI eligibility on employment duration: empirical analysis

Before presenting our main analyses regarding the effect of the reform on the design of FTCs, we first analyze whether workers discontinuously exit employment when they reach the UI eligibility threshold—a result already demonstrated in the literature (see, e.g., Rebollo-Sanz (2012), Albanese et al. (2020), and Martins (2021)). We proceed in two steps. Using the *FH-DADS* data over the period 2005–2012, we check that the probability of transitioning from employment to registered unemployment discontinuously increases at the work history thresholds, both before and after the 2009 UI reform. Then, we demonstrate that the overall probability of exiting employment—including transitions to both registered and non-registered unemployment—discontinuously rises at the same thresholds.[22] These analyses are detailed in Appendix C3.

In this section, we push the analysis one step further by examining the extent to which the design of FTCs is affected by the UI eligibility threshold. Firms deciding on contract duration balance the attractiveness of a contract and the risk of retaining a non-productive worker in a long-term employment relationship. The 2009 UI reform increased the value of 4-month FTCs for workers seeking UI eligibility. We expect firms to internalize this change, leading to bunching in contract duration just above the 4-month threshold. In this section, we test this idea by empirically assessing whether firms reacted to the introduction of a new work history threshold for UI eligibility by changing the duration of the FTCs they offer (Section 3.1). Our results show that, in addition to the *ex-post* mechanism highlighted by the literature and confirmed in Appendix C3, UI eligibility also affects the *ex ante* design of contracts. Next, we present evidence on the job posting mechanism driving this response (Section 3.2) and examine spillover effects on workers beyond those seeking UI eligibility (Section 3.3). Finally, we investigate the effect of removing the 6-month threshold on the duration of FTCs on the 5-6 month FTC segment (Section 3.4).

[22] Registered unemployment refers to individuals who formally register with the unemployment insurance agency to claim benefits or access job placement and counseling services. Non-registered unemployment refers to individuals who are unemployed but do not register with such agencies, either because they are not eligible, choose not to register, or are seeking work informally. Registered and non-registered unemployment add up to total non-employment.

3.1 The impact of introducing a new work history threshold for UI eligibility

3.1.1 Empirical methodology

In this section, we investigate whether the introduction of a new work history threshold for UI eligibility affects the duration of FTCs scheduled by employers. Firms may strategically set contract lengths just above the threshold to increase the probability of filling their vacancy. To empirically test this idea, we exploit the *MMO* dataset and compare the relative change in the number of 3- and 4-month FTCs around the time of the reform in our main working sample, which is described in Section 2. We aggregate observations at the plant × year × contract-duration level[23] and we compare trends in the within-plant number of 3-month and 4-month FTCs finishing each year, before and after the reform. In practice, we estimate the following equation:

$$Y_{it}^{d} = \alpha + \beta_1.post_t + \beta_2.\mathbb{1}_{d=4} + \beta_3.post_t * \mathbb{1}_{d=4} + \mu_i + \epsilon_{it}^{d} \tag{1}$$

where Y_{it}^{d} is equal to the number of contracts of duration $d \in \{3;4\}$ ending in plant i in year t, $post_t$ is a dummy variable that equals 0 before the reform was implemented in April 2009 and 1 for all subsequent periods, and $\mathbb{1}_{d=4}$ is a dummy variable indicating 4-month contracts.[24] We add plant fixed effects (μ_i) to account for plants' time-invariant determinants of recruitment policies. Standard errors are clustered at the plant level. When no FTC of duration d is terminated in firm i during year t, Y_{it}^{d} takes the value 0. The parameter of interest, β_3, captures the change in the average number of 4-month contracts ending each year in each plant before and after the reform, relative to the number of 3-month contracts.

3.1.2 Main results

Results displayed in the first column of Table 2 indicate that the introduction of a new UI eligibility threshold at 4 months caused by the 2009 reform induced a significant increase in the number of 4-month FTCs relative to the number of 3-month FTCs. The estimated coefficient implies a relative increase of 0.11 4-month FTCs ending each year in each plant. This is a large effect: it represents a 11% increase with regards to the pre-reform outcome mean. Equivalently, our results mean that 15,000 contracts were affected in our sample of firms after the reform (45 295 (plants) x 0.11 x 6 (years)), which represents 0.6% of the FTCs of at least a month signed in these plants over the post-reform period.[25] The main effect estimated in Table 2 is identified

[23] The daily contract duration is rounded down to the bottom monthly value. We therefore include contracts of duration between 122 and 151 days (hereafter "4-month contracts") and contracts of duration between 91 and 121 days (hereafter "3-month contracts").

[24] Note that each observation for year t covers the period from April, 1 of year t to March,31 of year $t+1$.

[25] Our 0.11 effect accounts for 30,000 contracts. However, since these 30,000 corresponds both to the decrease in 3-month contracts and the increase in 4-month contracts as a result of the shift in contract duration, we can consider that only half of the 30,000 are affected.

over all of our period of observation before and after the reform (2005-2014). In column (2), we apply the same approach to observations from years closer to the reform (2007–2010) to reduce the risk of other factors, such as macroeconomic shocks or other labor policies, confounding our results. With this adjustment, the estimated effect of the reform increases to 0.17, i.e. 17% of the pre-reform outcome mean (see column 2). This is equivalent to 1% of the FTCs of at least a month signed in these plants over the period 2007-2010.

Table 2: Impact of the reform on the number of 4-month FTCs relative to 3-month FTCs

	Number of contracts	
	(1)	(2)
4-month FTC	-0.3804***	-0.3937***
	(0.01779)	(0.01922)
Post-reform	-0.2645***	-0.3108***
	(0.01622)	(0.01720)
4-month FTC × Post-reform	0.1096***	0.1728***
	(0.01570)	(0.01905)
Constant	1.2925***	1.3339***
	(0.01252)	(0.01246)
Firm fixed-effect	✓	✓
Time period	2005-2014	2007-2010
Observations	905,900	362,360
Outcome mean (pre reform)	0.976	1.025

Note: The table shows the effect of the UI reform on the number of 4-month fixed-term contracts relative to 3-month fixed-term contracts, based on MMO data, using Equation (1). Each observation corresponds to the number of fixed-term contracts of a certain duration (3 or 4 months) terminated in a given plant in a given year. Column (1) refers to our main working sample and column (2) restricts to observations close to the reform year (i.e., years 2007-2010). Standard errors (in parentheses) are clustered at the plant level.* p<0.10, ** p<0.05, *** p<0.01.

Model (1) identifies the causal impact of the change in the eligibility criterion under the assumption that, absent the reform, the difference in the number of 3- and 4-month FTCs ending each year in each plant would have remained stable on average. While this assumption cannot be formally tested, we provide evidence that it is a reasonable assumption. To estimate the dynamic effect of the reform and check for the absence of different pre-trends, we estimate Equation (2). In this model, we interact the dummy variable indicating 4-month FTCs with yearly dummies $Year_{k,t}$ instead of a post variable. Each year dummy covers the period from April k to March $k+1$ and takes the value 1 when t equals k:

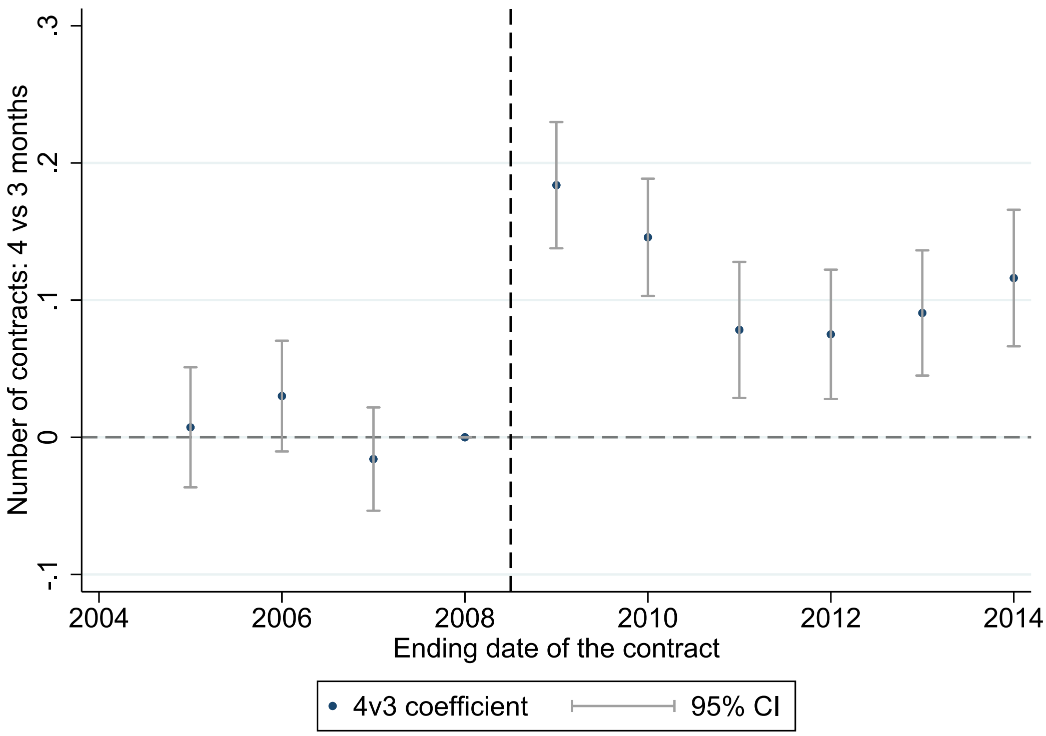
$$Y_{it}^{d} = \alpha + \beta.\mathbb{1}_{d=4} + \sum_{k\neq 2008} \gamma_k.Year_{k,t} * \mathbb{1}_{d=4} + \sum_{k\neq 2008} \delta_k.Year_{k,t} + \mu_i + \epsilon_{it}^{d} \tag{2}$$

The resulting estimates of the parameters γ_k are displayed in Figure 2. It depicts a clear jump after the reform, with the number of 4-month contracts (relative to 3-month contracts) remaining at higher levels in all subsequent years, whereas no significant difference is observed before the reform. This provides support to our identification assumption: one can clearly reject the presence of different pre-trends in the number of each type of contracts before the reform and the effect kicks in right after the reform.

Overall, the results outlined in Table 2 and Figure 2 highlight a long-lasting change in the duration of FTCs following the introduction of the new UI eligibility threshold. The results are in line with an *ex-ante* mechanism: firms internalize the incentive for workers to be eligible for UI benefits. As a result, they are more likely to offer FTCs that grant UI eligibility to attract more workers and increase their chances to fill their vacancy. In Section 3.2, we provide further evidence based on vacancy data to rule out that our effect is driven by *ex-post* bargaining.

The effect of the reform should be particularly pronounced for firms: (i) that are well aware of UI rules; (ii) that experience difficulties in recruiting workers. We empirically test (and confirm) these intuitions in Section 3.1.4, after checking the robustness of our main result in Section 3.1.3.

Figure 2: Impact of the reform on the number of 4-month FTCs relative to 3-month FTCs: Yearly estimates



Note: The figure plots the γ_k coefficient obtained from the estimation of Equation (2). They measure the yearly estimates of the effect of the UI reform on the number of 4-month fixed-term contracts relative to 3-month fixed-term contracts, based on MMO data. The reference year is 2008, the last pre-reform year. Standard errors are clustered at the plant level. The vertical dotted line shows the timing of the reform.

3.1.3 Robustness checks

In this section, we present three types of robustness checks: (i) we first test the robustness of our main result to an alternative sample selection; (ii) we replace the outcome variable in levels by it equivalent in logs; (iii) we confirm that our result is specific to the 3-to-4-month threshold. Appendix C2 provides a detailed explanation of these tests and their exact findings. Overall, the results confirm the robustness of our analysis when using the full sample of plants, including those that did not hire any workers on either 3-month or 4-month contracts before the reform. Similarly, specifying the dependent variable in logs rather than levels does not alter the conclusions. Lastly, when analyzing alternative thresholds that should be minimally, if at all, affected by the reform, we find that the pattern observed at four months remains unique.

3.1.4 Heterogeneity analysis

In this section, we investigate the mechanisms driving our main result. Firms with greater knowledge of UI regulations are likely to respond more strongly to the introduction of the new UI eligibility threshold. Likewise, firms operating in certain economic sectors—particularly those struggling with labor shortage—are expected to have stronger incentives to adjust the duration of FTCs to attract more workers. To test these hypotheses, we examine the heterogeneity of the main effect along these dimensions.

Firm knowledge of UI rules. We use two proxies for firms' knowledge of UI rules: (i) the separation rate; (ii) the share of workers hired from the unemployment rolls during the pre-reform period (2005–2008). Firms that frequently hire workers on short-term contracts are likely to have a better understanding of UI rules, particularly those affecting workers at the eligibility margin. This should also be the case for firms frequently recruiting workers from the unemployment pool, as employers may learn about UI rules from their employees. We therefore expect firms with a high separation rates and firms with a high share of hires from the unemployment rolls to be more responsive to the introduction of a new UI eligibility threshold.

To measure plant separation rate, we use the MMO data and compute the annual average number of terminated FTCs divided by the total number of employees in 2005-2008. To measure the share of employees recruited from the unemployment rolls, we leverage the *FH-DADS* dataset (see Appendix A).[26] We then estimate Equation (1) separately for plants above vs. below the median of each of these measures. To reduce noise in both measures, we restrict the analysis to plants with at least 20 employees over the 2005-2008 period (i.e., 85%

[26]The index is computed as the share of employees hired between 2005 and 2008 who were registered as unemployed the day before being hired. Over this period, slightly more than half of plants recruited at least one worker from the unemployment rolls in our data. As the *FH-DADS* is a representative panel covering 1/12 of the workforce, some plants do not appear during the observed period, in which case they are dropped from this analysis.

of plants).[27] Figure 3a shows that the effect of the reform is overwhelmingly driven by plants above the median of the pre-reform separation rate distribution, with a coefficient implying a relative increase of 0.23 4-month FTCs ending each year in each plant. Conversely, the effect is null and insignificant for plants below the median. Similarly, the effect of the UI reform is more pronounced for plants with a pre-reform share of hires from unemployment rolls above the median. The estimated coefficient is more than twice as large as for above-median plants compared to below-median plants (Figure 3b). The same pattern is observed when using the log of the number of contracts as the dependent variable (Figures B3a and B3b).

To confirm these results, we estimate a triple interaction model. For each of our two variables measuring firm knowledge of UI rules, we define a dummy variable Med_i, which equals 1 if the firm falls within the top 50% of the pre-reform distribution of this variable. We then estimate an extended version of Equation (1) as follows:

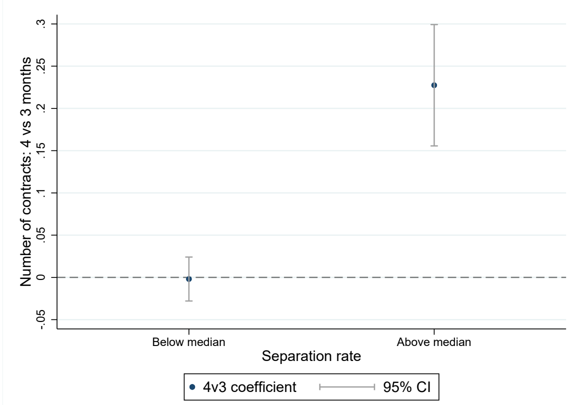
$$\begin{aligned} Y_{it}^d = \alpha + \beta_1.post_t + \beta_2.\mathbb{1}_{d=4} + \beta_3.post_t * \mathbb{1}_{d=4} + \beta_4.\mathbb{1}_{d=4} * Med_i \\ + \beta_5.post_t * Med_i + \beta_6.post_t * \mathbb{1}_{d=4} * Med_i + \mu_i + \epsilon_{it}^d \end{aligned} \tag{3}$$

Results presented in Table 3 confirm the significantly stronger effect of the UI reform for plants with a pre-reform separation rate above the median (column 1) and for plants with a pre-reform share of hires from unemployment above the median (column 2). These results are robust to the use of a log specification (columns 1 and (2) of Table B3) or to the inclusion of plants with less than 20 employees (columns 5 and 6 of Table B3). They are also robust to an alternative specification which combines both variables into a quadruple interaction model.[28] Results presented in Table B3 (column 4) show that the reform's effect is more pronounced in plants with a high separation rate *and* that frequently recruit from the unemployment rolls: the coefficient of the quadruple interaction is positive and highly significant.

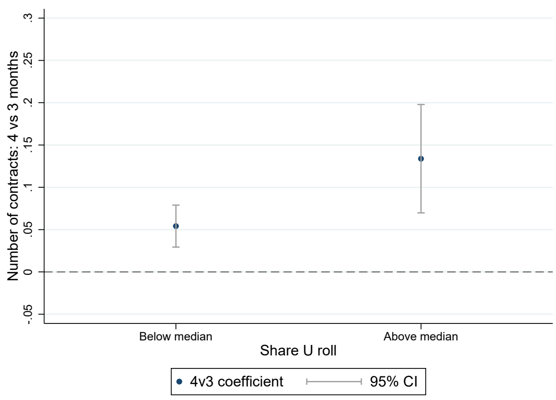
[27] This restriction allows us to focus on plants with a sufficient number of hires, ensuring that our measures of separation and share of hires from unemployment are meaningful and not overly noisy. For instance, in our data, 43% of plants with fewer than 20 employees made no hires at all during the 2005–2008 period, compared to 13% for larger firms. We nevertheless show that our results are robust to the inclusion of these plants.

[28] We estimate an extended version of Equation (1), which further interacts $post_t * \mathbb{1}_{d=4}$ with a dummy indicating that the plant falls within the top 50% of the pre-reform distribution of the separation rate variable and with a dummy indicating that the plant falls within the top 50% of the pre-reform distribution of hires from the unemployment rolls (along with all relevant double and triple interactions not accounted for in fixed effects).

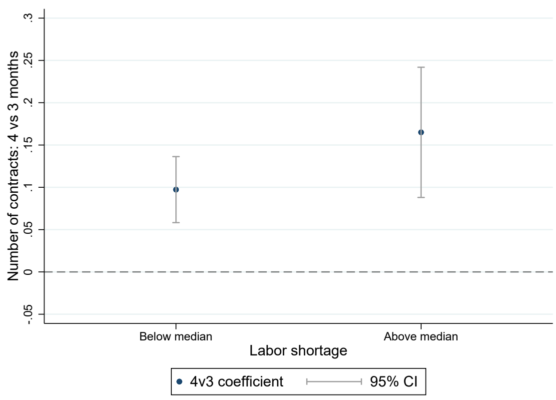
Figure 3: Impact of the reform on the number of 4-month FTCs relative to 3-month FTCs: Heterogeneity analysis



(a) Separation rate



(b) Share of hires from unemployment rolls



(c) Labor shortage

Note: This figure plots coefficient β_3 obtained from the estimation of Equation (1). Each coefficient is estimated separately on plants above and below the median of the heterogeneity variables. Figure 3a compares plants above and below the median of the plant-level separation rate (i.e., the plant's annual average number of terminated FTCs divided by total employment). Figure 3b compares plants above and below the median share of hires from the unemployment register. Figure 3c compares plants in sectors with labor shortages above and below the median value of the labor shortage indicator. All heterogeneity variables are defined over the pre-reform period (2005-2008). Standard errors are clustered at the plant level.

Table 3: Impact of the reform on the number of 4-month FTCs relative to 3-month FTCs: Heterogeneity by plant separation rate and recruitment from unemployment roll

	Number of contracts	
	(1)	(2)
4-month FTC	-0.1472***	-0.1533***
	(0.00552)	(0.01742)
Post-reform	0.0098	-0.1771***
	(0.01462)	(0.01262)
4-month FTC × Post-reform	-0.0019	0.0542***
	(0.01327)	(0.01266)
4-month FTC × Separation	-0.5888***	
	(0.04451)	
Post-reform × Separation	-0.5697***	
	(0.03994)	
4-month FTC × Post-reform × Separation	0.2293***	
	(0.03896)	
4-month FTC × U rolls		-0.5044***
		(0.04084)
Post-reform × U rolls		-0.1284***
		(0.03630)
4-month FTC × Post-reform × U rolls		0.0797**
		(0.03504)
Constant	1.4716***	1.5741***
	(0.01489)	(0.01611)
Firm fixed-effect	✓	✓
Observations	749,560	691,080

Note: The table shows the heterogeneity of the UI reform's effect on the number of 4-month fixed-term contracts relative to 3-month fixed-term contracts, according to plant separation rate and plant share of hires from the unemployment rolls. A triple interaction model described in Equation (3) is estimated using our main MMO working sample, restricted to plants with at least 20 workers over the pre-reform period (2005-2008). The *Separation* variable is a dummy indicating that the plant belongs to the top 50% of the separation rate variable. The *U rolls* variable is a dummy indicating that the plant belongs to the top 50% of the share-of-hires-from-unemployment variable. Standard errors (in parentheses) are clustered at the plant level. * p<0.10, ** p<0.05, *** p<0.01.

Labor shortage. Firms in economic sectors experiencing labor shortages are also expected to be more affected by the reform, as they have stronger incentives to align FTC durations with the eligibility cutoff to attract more workers. To examine this, we start by analyzing the heterogeneity of the reform's effect across economic sectors. We repeat the estimation of Equation (1) separately in each of the sixteen 1-digit economic sectors and we display the distribution of sector-specific estimates in Figure B4a. The figure reveals significant heterogeneity, with some sectors being much more affected than others.[29] To take one step further, we then build a measure of labor shortage at the sector level, based on data from the 2004-2005 *Réponse* survey (see Appendix A). We use the proportion of firms reporting hiring difficulties in the

[29] A similar pattern is observed when estimating a model in which the dependent variable is expressed in log (Figure B4b).

sector.[30] We then estimate Equation (1) separately for plants in sectors above vs below the median of the labor shortage index. We find that the effect of the UI reform is larger for plants in sectors above the median of the pre-reform labor shortage distribution, with an estimated coefficient 1.6 times as large as for plants below the median (Figure 3c). The contrast between both subsamples is even more marked when estimating a model in which the dependent variable is expressed in log (Figure B3c). We also estimate a triple interaction model (Equation (3)) where Med_i is a dummy variable indicating that the plant operates in a sector above the median of the pre-reform labor shortage variable. The results presented in Table 4 tend to confirm that the reform's effect is larger in economic sectors where firms face hiring difficulties. The estimated coefficient for the triple interaction is positive and marginally not significant (p-value = 0.14). Reassuringly, the coefficient is positive and strongly significant when using a log specification (Column (3) of Table B3).

Other heterogeneity dimensions. To complete our heterogeneity analysis, we also examine how the reform's effect varies with plant size and workers' characteristics. Splitting firms into size quartiles, we observe that firms in the top 25% of the firm size distribution appear to drive the effect in absolute terms (Figure B5a). However, this is not true for the relative effect (Figure B5b). Finally, we also examine whether the effect of the reform varies according to workers' characteristics. To do so, we estimate Equation (1) on different subsets of our main working sample, defined according to workers' age, gender, socioeconomic status and nationality. This analysis reveals very little heterogeneity along workers' characteristics (Figure B6).

[30] Specifically, we use the following question: "In 2004, did your firm experience hiring difficulties for some categories of workers?".

Table 4: Impact of the reform on the number of 4-month FTCs relative to 3-month FTCs: Heterogeneity by labor shortage in the economic sector

	Number of contracts (1)
4-month FTC	-0.0875***
	(0.00198)
Post-reform	-0.1250***
	(0.00295)
4-month FTC × Post-reform	0.0305***
	(0.00218)
4-month FTC × Shortage	-0.0389***
	(0.00340)
Post-reform × Shortage	-0.0017
	(0.00475)
4-month FTC × Post-reform × Shortage	0.0123***
	(0.00360)
Constant	0.5100***
	(0.00150)
Firm fixed-effect	✓
Observations	711,300

Note: The table shows the heterogeneity of the UI reform's effect on the number of 4-month fixed-term contracts relative to 3-month fixed-term contracts, according to labor shortage in the economic sector. A triple interaction model described in Equation (3) is estimated using our main MMO working sample. The *Shortage* variable is a dummy indicating sectors in the top 50% of the labor shortage variable (defined as the share of plants' declaring hiring difficulties in this sector in the 2004-2005 *Reponse* survey). Standard errors (in parentheses) are clustered at the plant level. * p<0.10, ** p<0.05, *** p<0.01.

3.2 Evidence on job posting

In the previous section, we showed that the relative prevalence of 4-month FTCs increased compared to 3-month FTCs following the introduction of the 4-month threshold. We interpret this shift as an *ex-ante* mechanism, where firms factor in UI eligibility value for workers when posting job vacancies, before meeting them. However, as noted in Section 2, a key limitation of the MMO dataset is its inability to precisely track immediate FTC renewals or conversions to permanent contracts, making it difficult to fully rule out the influence of ex-post bargaining. While additional analyses in Appendix C1 provide reassurance against this possibility, we conduct a more rigorous investigation in this section. To do so, we rely on an additional job vacancy dataset as well as on the analysis of layoffs in the MMO data. Our findings confirm that the rise in 4-month FTCs stems from an *ex-ante* strategic decision by firms, rather than an *ex-post* adjustment through bargaining between firms and workers.

3.2.1 Analysis on vacancy data

We first exploit vacancy data from *France Travail*, the French Public Employment Services (PES), which represents the largest source for job ads on the French job market.[31] The dataset

[31]For instance, in 2010, vacancies posted at PES represented 60% of all hires in France (Le Barbanchon et al., 2021).

gathers all job ads published on the PES website over the 2005-2014 period. For each observation, the data provides information on the type of contract and the expected duration of the job. Firms are not allowed to tie job ads to individual candidates, and they cannot offer less generous contracts than those stated in the ad. Job ads thus represent a very pure measure of the *ex-ante* contract duration.

To investigate whether the introduction of the new UI eligibility threshold has affected the duration of FTCs as measured in job vacancies, we follow the same approach as in our main analysis. We focus on observations corresponding to 3- and 4-month FTCs and evaluate the evolution of the relative prevalence of vacancies for these types of contracts. However, one limitation of this dataset is that it does not include a firm identifier. As a result, we aggregate the data at the monthly date × 2-digit occupation × region (*département*) level.[32] It allows to include interacted month, region and occupation fixed effects, to control for occupation- and region-specific monthly trends that could affect our results. Specifically, we estimate the following model:

$$Y^d_{mtor} = \alpha + \beta_1.post_{mt} + \beta_2.\mathbb{1}_{d=4} + \beta_3.post_{mt} * \mathbb{1}_{d=4} + \kappa_m \times \gamma_o \times \lambda_r + \epsilon^d_{mtor} \tag{4}$$

where Y^d_{mt} is the total number of vacancies for FTCs of duration d posted in month m of year t. Standard errors are clustered at the month level.

Table 5 reports the results of the estimation of Equation 4. Columns (1) and (3) report the estimation for the entire period of analysis (2005-2014) while columns (2) and (4) focus on observations close to the reform (2007-2010). Columns (1) and (2) report results where the number of vacancies is computed at the national level for each month, and no fixed effect is included, while columns (3) and (4) report results with month × occupation × region fixed effects. Both specifications and both time periods yield positive and significant estimates. The prevalence of vacancies corresponding to 4-month FTCs has significantly increased after the reform compared to the prevalence of vacancies corresponding to 3-month FTCs. The effect does not depend on the set of fixed effects included, with a magnitude lying between 13% and 33% relative to the pre-reform outcome mean. To investigate the dynamic of the effect, we also estimate the following model:

$$Y^d_{mtor} = \alpha + \beta.\mathbb{1}_{d=4} + \sum_{k\neq 2008} \delta_k.Year_{k,t} + \sum_{k\neq 2008} \gamma_k.Year_{k,t} * \mathbb{1}_{d=4} + \kappa_m \times \gamma_o \times \lambda_r + \epsilon^d_{mtor} \tag{5}$$

Figure 4 reports the γ_k coefficients from the estimation of Equation (5). Coefficients follow a very similar pattern to those from our main analysis with MMO data: the coefficients are

[32] Firm identifiers are only available for the most recent years, which means that in this analysis, we cannot work at the firm level. We aggregate the data at the monthly level, rather than at the yearly level, in order to avoid overly aggregated data and a too-small sample size. Table 5 also shows results where we aggregate the data at the monthly level for the entire economy. The absence of a firm identifier also explains why we do not use the vacancy data in our main analysis.

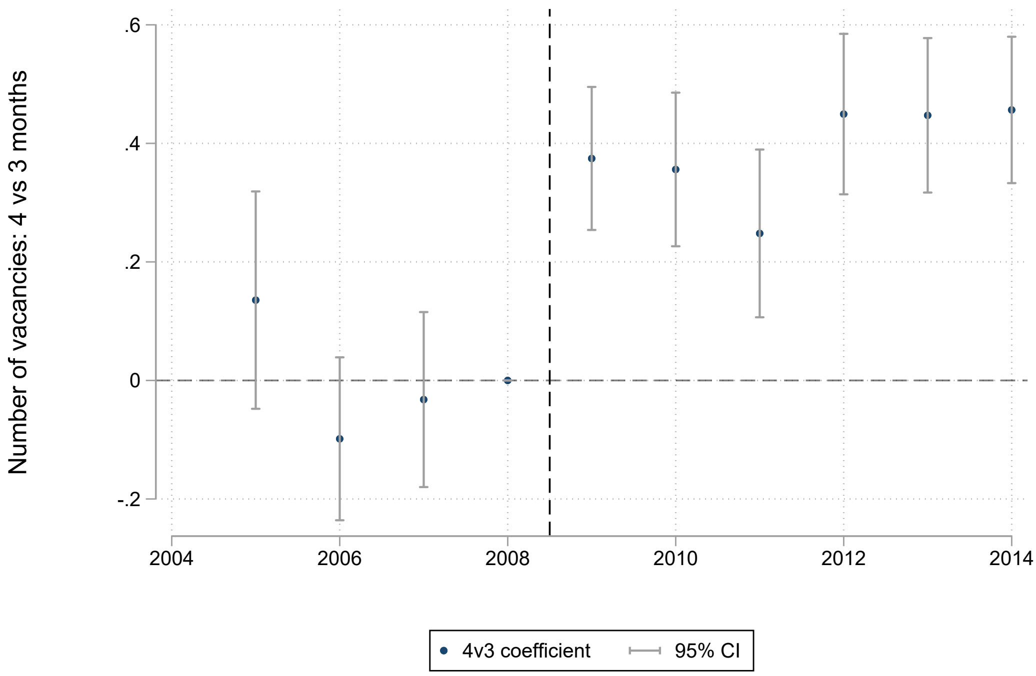
null before the reform and become strongly positive after the reform. The results presented in Table 5 and Figure 4 are also robust to the use of a log specification (Table B4 and Figure B7). Figures B8a to B8e further replicate the falsification tests performed in Section 3.1.3 on thresholds that should be minimally (if at all) affected by the reform, i.e., between 7 and 12 months. Again, these figures show that none of the other thresholds exhibit the same pattern observed at the 4-month threshold, supporting the idea that the observed effect on this threshold is due to the reform. Overall, analyses based on vacancy data confirm the existence of an *ex-ante* mechanism through which firms post 4-month job vacancies more frequently than 3-month vacancies in response to the introduction of the new UI eligibility threshold.

Table 5: Impact of the reform on the number of 4-month vacancies relative to 3-month vacancies

	Number of vacancies			
	(1)	(2)	(3)	(4)
4-month FTC	-17866.7500***	-18161.8333***	-1.4935***	-1.8711***
	(1044.23002)	(1118.03445)	(0.04045)	(0.06188)
Post-reform	-5758.3569***	-5073.8333***	-0.7533***	-0.6392***
	(611.94670)	(914.37158)	(0.02120)	(0.03270)
4-month FTC × Post-reform	5358.3728***	4811.8953***	0.3809***	0.4895***
	(550.45090)	(754.08979)	(0.04119)	(0.06549)
Constant	26738.6064***	27049.3401***	3.7558***	4.0336***
	(691.69471)	(744.93976)	(0.01569)	(0.02285)
Month fixed-effect	✓	✓	-	-
Month-by-occupation -by-region fixed-effect	-	-	✓	✓
Time period	2005-2014	2007-2010	2005-2014	2007-2010
Observations	230	94	1085712	375791
Outcome mean (pre reform)	15956.896	16804.883	2.937	3.222

Note: The table shows the effect of the UI reform on the number of 4-month vacancies relative to 3-month vacancies, based on the French Public Employment Service data, using Equation (4). Each observation corresponds to the number of vacancies of a certain duration (3 or 4 months) offered in a given month. Columns (1) and (3) refer to the 2005-2014 period and columns (2) and (4) restrict to observations close to the reform year (i.e., years 2007-2010). Columns (1) and (2) use the national level of vacancies each month while columns (3) and (4) use the number of vacancies in a given month, region and 2-digit occupation. Columns (3) and (4) include month × occupation × region fixed effects. Standard errors (in parentheses) are clustered at the month level in columns (1) and (2), and month × occupation × region level in columns (3) and (4).* p<0.10, ** p<0.05, *** p<0.01.

Figure 4: Impact of the reform on the number of 4-month vacancies relative to 3-month vacancies: Yearly estimates



Note: The figure plots the γ_k coefficient obtained from the estimation of Equation (4). They measure the yearly estimates of the effect of the reform on the number of 4-month vacancies relative to 3-month vacancies over the 2005-2014 period, based on the French Public Employment Service data. The reference year is 2008, the last pre-reform year. Standard errors are clustered at the month × occupation × region level. The vertical dotted line shows the timing of the reform.

3.2.2 The timing of layoffs

In this section, we examine whether the introduction of a new UI eligibility threshold also induced behavioral responses implying bargaining between firms and workers. In particular, we investigate whether firms tend to lay off more often workers at 4 months of tenure. To do so, we estimate again Equations (1) and (2) on employment spells terminated as economic layoffs or dismissals on personal grounds at 3 or 4 months of tenure, using the MMO dataset. As shown in Table B5 and Figure B9, there is no differential evolution in the number of layoffs at 3 and 4 months of tenure, neither before nor after the reform. We thus find no evidence of any *ex-post* negotiation between worker and employer in response to the introduction of the UI threshold. A potential explanation of this result lies in the fact that in France, firing costs are high, both in absolute terms and relative to hiring costs on short fixed-term contracts (Kramarz and Michaud, 2010).

3.3 Spillover effects of UI eligibility threshold

The empirical analyses provided so far support the idea that the reform-induced change in the value of 4-month contracts for workers with no work history, who are looking for UI eligibility, leads firms to hire temporary workers for this duration relatively more often after the reform. However, in practice, firms that post a vacancy do not observe the profile of the future applicants. Even during the hiring process, they only imperfectly observe their work history. Firms respond to the new cutoff by shifting the distribution of contracts. The excess mass of contracts at four months may end up being taken by workers who do not need a 4-month employment spell to qualify for UI benefits.

To test this hypothesis, we use the *FH-DADS* dataset, which was also used in the analyses presented in Appendix C3. This administrative dataset follows individual workers over the period 2004–2012 by linking payroll data ("Déclarations Annuelles des Données Sociales", hereafter *DADS*), with unemployment data (hereafter *FH*). The *DADS* and *FH* datasets were matched for a random subsample of 1/12th of the French population between 2004 and 2012.[33] The result is an individual panel that enables us to track workers' career paths and transitions between employment and unemployment. A unique feature of this dataset is that it allows us to compute, for each worker who completes an employment spell of a given duration, their work history prior to starting that employment spell. Note, however, that the data only includes one observation per individual per plant per year. As a result, multiple consecutive employment spells for the same individual—potentially interrupted by periods of non-employment—at the same plant in a given year would be recorded as a single observation. The observation will record the start date of the first contract and the end date of the last spell. This could introduce measurement error in both our measures of employment spell duration and work history. To address this issue, we focus on observations where the total number of hours worked during the entire period corresponds to the number of hours worked by a full-time employee over the same period. This restriction ensures that each observation corresponds to a single contract.

To explore the extent to which the relative increase in the number of 4-month FTCs with respect to the number of 3-month FTCs after the reform is observed for workers already eligible for UI, we adopt the same approach as for our main analyses based on MMO data (Section 3.1.2). We aggregate the data at the level of plant × year × FTC duration and use the same working sample (i.e., firms that have at least one FTC of 3 or 4 months before the reform). We focus exclusively on workers who begin the FTC with at least 6 months of work history. These workers are eligible for UI both before and after the reform when starting their FTC and at the end of their FTC no matter its length. We then estimate Equations (1) and (2) on this FH-DADS sample.

The results displayed in Table 6 show that the introduction of the new UI eligibility threshold had an impact on the duration of FTCs for workers already eligible for UI. Specifically, the coefficient presented in column (1) suggests a relative increase of 0.034 4-month FTCs ending each year in each plant with respect to the number of 3-month FTCs. This represents a 43% increase compared to the pre-reform period. When focusing on the years closest to the reform, this figure rises to 48%. As shown in Figure 5, the dynamics of the effect closely mirror those

[33]More specifically, this subsample consists of individuals born in October.

observed in our main analysis with MMO data: with the exception of 2005, the coefficients are null and not significant before the reform and become strongly positive and significant after the reform. These results remain robust when considering a log specification (columns 1 and 2 of Table B6 and Figure B10a). They also hold when we limit the sample to workers with more than 12 months of work history, i.e., workers even less sensitive to UI eligibility incentives (columns 3 and 4 of Table B6 and Figure B10b).

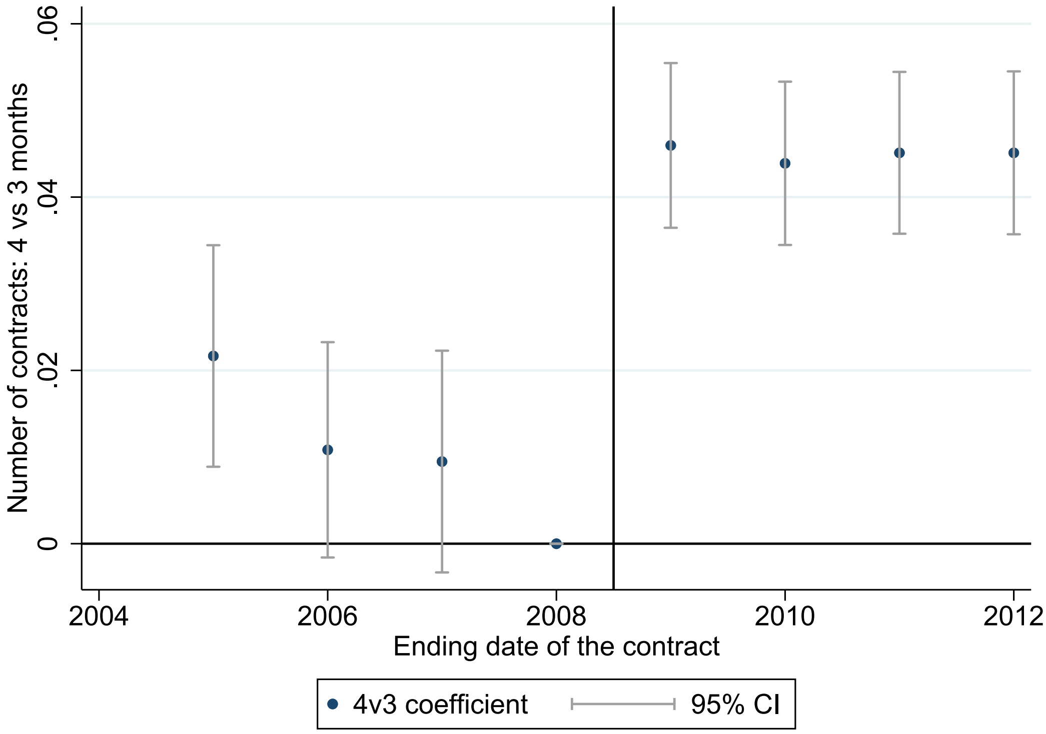
The findings presented in this section indicate that the reform also impacted the contract duration of workers who were not actively seeking UI eligibility. This result aligns with the idea that UI rules influence the duration of temporary workers' employment spells beyond those actively seeking UI eligibility, highlighting a new form of labor market externalities. Evidence of similar spillover effects of UI benefit level have been documented in the US (Doniger and Toohey, 2022). Using changes in either UI benefit replacement rate or cap, they show that non-directly affected workers respond to these changes, in line with a model including information frictions and *ex ante* wage commitments.

Table 6: Impact of the reform on the number of 4-month FTCs relative to 3-month FTCs for workers already eligible for UI

	Number of contracts	
	(1)	(2)
4-month FTC	-0.0369***	-0.0426***
	(0.00227)	(0.00328)
Post-reform	-0.1417***	-0.1506***
	(0.00134)	(0.00225)
4-month FTC × Post-reform	0.0345***	0.0402***
	(0.00235)	(0.00345)
Constant	0.1587***	0.1695***
	(0.00118)	(0.00181)
Firm fixed-effect	✓	✓
Time period	2005-2014	2007-2010
Observations	224,528	112,264
Outcome mean (pre-reform)	0.078	0.083

Note: The table shows the effect of the UI reform on the number of 4-month FTCs relative to 3-month FTCs, based on FH-DADS data, using Equation (1). Each observation corresponds to the number of fixed-term contracts of a certain duration (3 or 4 months) terminated in a given plant in a given year, considering only workers with at least 6 months of work history when starting the FTC. Column (1) refers to our main working sample and column (2) restricts to observations close to the reform (i.e., years 2007-2010). Standard errors (in parentheses) are clustered at the plant level.* p<0.10, ** p<0.05, *** p<0.01.

Figure 5: Impact of the reform on the number of 4-month FTCs relative to 3-month FTCs for workers already eligible for UI: Yearly estimates



Note: The figure plots the γ_k coefficient obtained from the estimation of Equation (2). They measure the yearly estimates of the effect of the UI reform on the number of 4-month fixed-term contracts relative to 3-month fixed-term contracts for workers with at least 6 months of work history when starting the FTC, based on FH-DADS data. The reference year is 2008, the last pre-reform year. Standard errors are clustered at the plant level. The vertical dotted line shows the timing of the reform.

3.4 The impact of removing an existing work history threshold for UI eligibility

In this section, we assess whether the removal of the work history threshold for UI eligibility, which was set at 6 months before the reform, also impacted the duration of FTCs. Similar to the main analysis, we compare the within-plant evolution of the number of 5-month and 6-month FTCs ending each year, before and after the reform. Specifically, we estimate Equations (1) and (2) using the sample of firms who had at least one FTC of 5 or 6 months during the pre-reform period.

The results presented in the first column of Table 7 indicate that the removal of the UI eligibility threshold at six months had little impact on the number of six-month FTCs relative to five-month FTCs. While the coefficient is, as expected, negative, it is not statistically significant at conventional levels. However, as shown in the second column of the table, the coefficient becomes larger and highly significant when we focus on years closer to the reform, suggesting a 6.6% decrease in 6-month FTCs relative to 5-month FTCs. However, the dynamic effects displayed in Figure 6 call for caution in interpreting this negative coefficient. The figure reveals the presence of substantial pre-trends and also shows unstable effects after the reform. The analysis on the 6-month threshold using the outcome variable in logs does not alter the conclusion (Figure B11). Table B7 and Figure B12 also show that there is no effect on economic layoffs after 6 months vs 5 months of tenure.

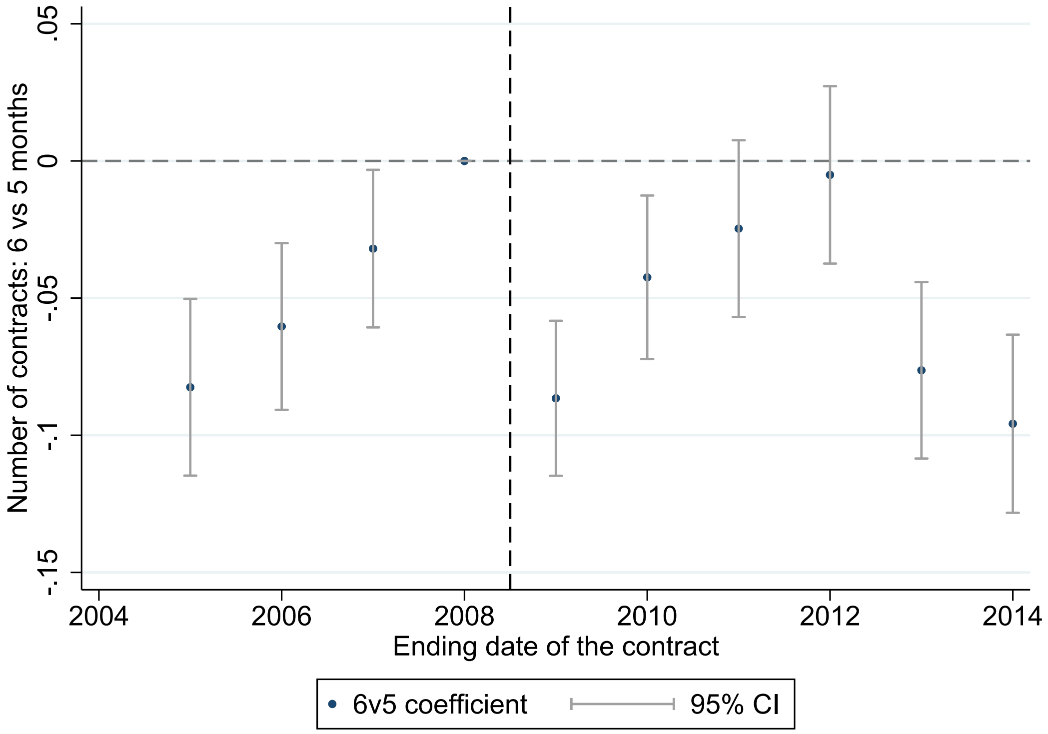
Overall, while the results for the 6-month threshold are broadly negative, they do not allow us to draw a clear conclusion. The removal of the incentive linked to the previous threshold does not appear to have had a perfectly symmetrical effect compared to the introduction of a new threshold. The presence of asymmetrical effects in bunching contexts is well-documented in the economic literature (Kleven, 2016). Bunching in the absence of economic incentives can be explained by thresholds representing round numbers (Kleven and Waseem, 2013; Best and Kleven, 2018), or focal points (Seibold, 2021). In our context, the asymmetry in the reform's effects on the two main thresholds could be explained by differences in salience—with the creation of a new threshold being more noticeable than the removal of an existing one—and by some degree of reference dependence. In the context of retirement decisions, Deshpande et al. (2024) show that economic agents can exhibit sticky behavior, continuing to bunch at an old threshold long after it has been removed. Similarly, Bergstrom et al. (2025) find that a significant share of households persist in bunching at outdated income thresholds in the context of a means-tested anti-poverty program in Brazil. While we cannot directly test this mechanism, it appears as a plausible explanation for the asymmetric effects at both thresholds.

Table 7: Impact of the reform on the number of 6-month FTCs relative to 5-month FTCs

	Number of contracts	
	(1)	(2)
6-month FTC	0.1378***	0.1655***
	(0.00996)	(0.01202)
Post-reform	-0.1424***	-0.1589***
	(0.00908)	(0.00997)
6-month FTC × Post-reform	-0.0114	-0.0485***
	(0.00932)	(0.01142)
Constant	0.7028***	0.7393***
	(0.00679)	(0.00718)
Firm fixed-effect	✓	✓
Time period	2005-2014	2007-2010
Observations	834,980	333,992
Outcome mean (pre reform)	0.683	0.730

Note: The table shows the effect of the UI reform on the number of 6-month fixed-term contracts relative to 5-month fixed-term contracts, based on MMO data, using Equation (1). Each observation corresponds to the number of fixed-term contracts of a certain duration (5 or 6 months) terminated in a given plant in a given year. Column (1) refers to the main working sample of plants (i.e., plants where at least one FTC of 5- or 6 months was terminated in the pre-reform period) and column (2) restricts to observations close to the reform (i.e., years 2007-2010). Standard errors (in parentheses) are clustered at the plant level.* p<0.10, ** p<0.05, *** p<0.01.

Figure 6: Impact of the reform on the number of 6-month FTCs relative to 5-month FTCs: Yearly estimates



Note: The figure plots the γ_k coefficient obtained from the estimation of Equation (2). They measure the yearly estimates of the effect of the UI reform on the number of 6-month fixed-term contracts relative to 5-month fixed-term contracts, based on MMO data. The reference year is 2008, the last pre-reform year. Standard errors are clustered at the plant level. The vertical dotted line shows the timing of the reform.

4 The impact of UI eligibility on wages

In the previous section, we demonstrated that the introduction of a new UI eligibility threshold impacts the number of FTCs offered by firms near the threshold. In this section, we explore whether the eligibility threshold also affects the wages of contracts close to the threshold. In particular, the rent created by UI eligibility could push down the wages of 4-month contracts relative to 3-month contracts after the reform.

4.1 Main results

We use payroll data (the DADS part of the *FH-DADS* data). We examine the evolution of wages of 4-month FTCs relative to 3-month FTCs.[34] Using the same approach as in our main analysis, we estimate the following equation at the contract level:

$$log(w)^d_{ijt} = \alpha + \beta_1.post_t + \beta_2.\mathbb{1}_{d=4} + \beta_3.post_t * \mathbb{1}_{d=4} + \beta_4 X_j + \mu_i + \epsilon^d_{ijt} \quad (6)$$

[34] Note that the vacancy data used in Section 3.2.1 do not include information on posted wages for our period of interest.

where $log(w)^d_{ijt}$ represents the log of the wage of a FTC of duration $d \in \{3; 4\}$ held by worker j ending in plant i in year t. X_j represents a vector of worker characteristics, including gender, age, age square and 2-digit occupation. The other terms of the equation are similar to those in Equation (1). Standard errors are clustered at the plant level. The parameter of interest, β_3, captures the change in the average wage of 4-month contracts before and after the reform, relative to the evolution of the average wage of 3-month contracts.

Results presented in the first column of Table 8 suggest no effect of the UI reform on the wages of 4-month FTCs relative to 3-month FTCs. The estimated coefficient is not statistically significant at conventional levels and allows us to rule out an effect larger than 0.9% in absolute value. The table also shows that this null effect holds when focusing on the types of firms identified in Section 3.1.4 as being most affected by the reform. Columns 2 to 4 successively restrict the sample to plants above the median in terms of separation rate, share of hires from unemployment rolls, and labor shortage. None of the estimated coefficients is statistically significant at conventional levels.

Table 8: Impact of the reform on wages in 4-month FTCs relative to 3-month FTCs

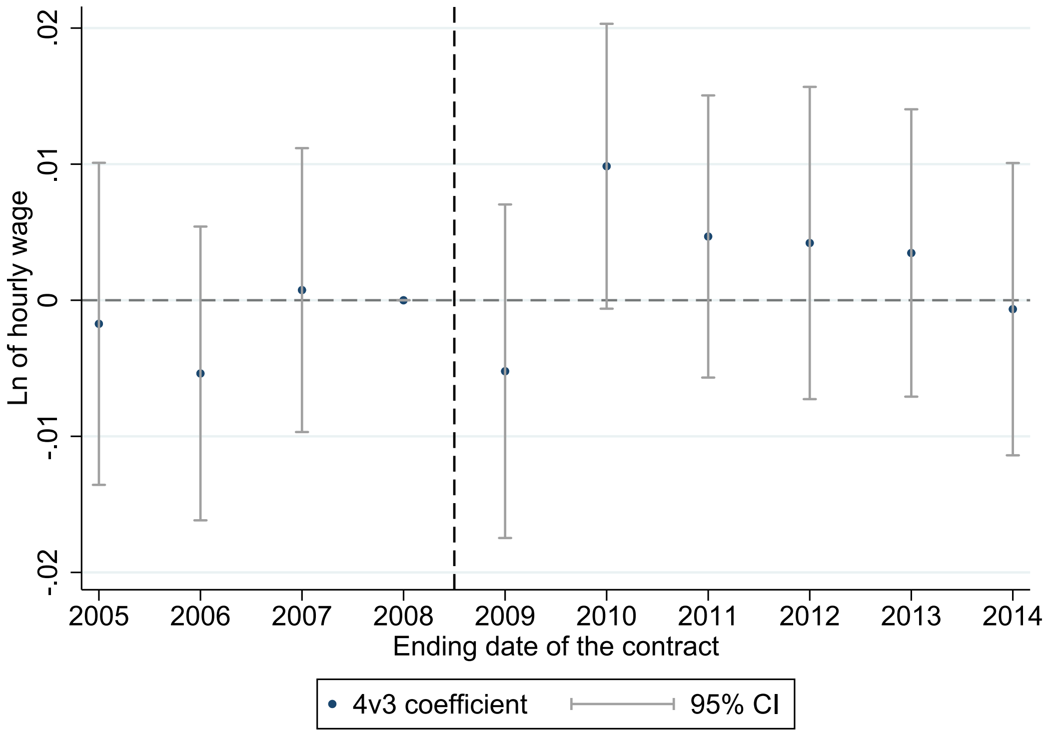
	(1) All FTCs	(2) High separation rate	(3) High share U rolls	(4) High labor shortage
4-month FTC	0.0014	0.0017	0.0052**	0.0018
	(0.00207)	(0.00262)	(0.00235)	(0.00358)
Post-reform	0.0971***	0.0903***	0.0854***	0.1097***
	(0.00256)	(0.00318)	(0.00282)	(0.00387)
4-month FTC ×Post-reform	0.0041	0.0007	-0.0011	0.0031
	(0.00274)	(0.00361)	(0.00341)	(0.00459)
Constant	2.2532***	2.2521***	2.2416***	2.2402***
	(0.00914)	(0.01391)	(0.01259)	(0.01441)
Worker controls	✓	✓	✓	✓
Firm fixed effects	✓	✓	✓	✓
Observations	276,440	128,212	151,605	39,836
Outcome mean (pre reform)	2.491	2.496	2.486	2.402

Note: The table reports the estimated effect of the UI reform on the average hourly wage of 4-month fixed-term contracts relative to 3-month fixed-term contracts, using *FH-DADS* data and Equation (6). Each observation corresponds to an individual contract of either 3 or 4 months. Column (1) presents estimates based on the full sample. Columns (2) to (4) restrict the sample to plants above the median in terms of separation rate, share of hires from unemployment, and sectoral labor shortage, respectively. Standard errors (in parentheses) are clustered at the plant level. * p<0.10, ** p<0.05, *** p<0.01.

We also estimate a dynamic model, as in Equation (2). As shown in Figure 7, the difference in wages between 4-month and 3-month FTCs remains stable before the reform, with no statistically significant change in this trend following the reform's implementation. This pattern also holds when focusing on the types of firms identified in Section 3.1.4 as being most affected by the reform (Figures B13a to B13c).

To ensure that the null result is not driven by a composition effect (i.e., sorting of workers around the 4-month cutoff), we re-estimate Equation (6) using observable worker characteristics available in our dataset as dependent variables, and removing the covariates X_j from the RHS of the equation. Specifically, we consider worker's gender, age, 1-digit occupation, and rank in the wage distribution (within their sector and 2-digit occupation) prior to the reform.

Figure 7: Impact of the reform on wages in 4-month FTCs relative to 3-month FTCs: Yearly estimates



Note: The figure plots the yearly estimates of the effect of the UI reform on wages in 4-month fixed-term contracts relative to 3-month fixed-term contracts, based on FH-DADS data, using Equation (6). The reference year is 2008, the last pre-reform year. Standard errors are clustered at the plant level. The vertical dotted line shows the timing of the reform.

The results, presented in Figure B14, provide no evidence of sorting at the threshold based on the available worker characteristics.

4.2 Discussion

Overall, our analysis provide little support for the hypothesis that firms and workers adjust wages to share the rent associated with UI eligibility for 4-month contracts. The absence of a wage response to UI eligibility may reflect several mechanisms. First, downward nominal wage rigidity is a well-documented feature of labor markets (Dickens et al., 2007). Such rigidity may also affect new hires, as wage offers often reflect existing internal pay structures. As noted by Elsby and Solon (2019), firms may avoid offering lower wages to new hires in order to maintain internal pay coherence. Second, institutional features of the French labor market may limit the scope for individual bargaining at the hiring stage. Almost all workers are covered by collective agreements, and wage negotiations take place at national, industry, and firm levels (Avouyi-Dovi et al., 2013). Moreover, empirical evidence suggests that entry wages are closely linked to sectoral wage floors and the minimum wage, which affect a large share of workers. They exhibit limited flexibility, especially downward (Fougère et al., 2018; Gautier et al., 2022). Finally, in our context, firms that respond to the reform by offering longer contracts may be those facing hiring difficulties. In such cases, employers may have limited bargaining power at the hiring stage, reducing their ability to adjust wages downward.

The absence of a wage response in our analysis is consistent with recent empirical evidence suggesting that UI has limited effects on wages. Jäger et al. (2020), for instance, show that wages are largely insensitive to the value of non-employment, even among already employed workers. In a different perspective, Le Barbanchon et al. (2024) provide a recent review of the literature on reemployment wages, highlighting that although some effects can be detected, they are typically of second order. While our outcome differs (entry wages for newly hired workers), our null result reinforces the idea that UI affects wages only marginally, if at all.

5 Conclusion

This paper provides new evidence on how unemployment insurance (UI) eligibility conditions affect employment duration, focusing on how the minimum work history requirement shapes firms' hiring decisions. We exploit a policy that changed the work history threshold from six months to four months in 2009 in France. Building on an administrative dataset that allows us to track plants over time and includes all employment flows in France over the 2005-2014 period, we compare the within-plant variation over time in the number of 4-month FTCs relative to the number of 3-month FTCs. Our analysis shows that firms strategically adjust contract durations to align with the new work history threshold: the reform was followed by an 11% increase in the number of 4-month FTCs relative to 3-month FTCs. This effect is more pronounced for firms arguably more aware of UI eligibility rules and for those with stronger incentives to adjust the duration of FTCs to attract workers. We interpret the increase in the relative prevalence of 4-month FTCs compared to 3-month FTCs as the result of an *ex-ante* mechanism, whereby firms take into account the value of UI eligibility for workers when posting a job vacancy, prior to meeting workers. This *ex ante* interpretation is further supported by the analysis of vacancy data, which provide a measure of expected contract duration and reveal a strikingly similar pattern. Additionally, we find that firms' response also affect workers that are already eligible for UI, uncovering a new form of labor market externalities.

Taken together, our results highlight that firms account for the change in the value of contracts just above the UI eligibility threshold when making hiring decisions. These insights highlight the need, when assessing the effects of UI on employment, to consider not just worker-side responses to UI, but also firms' strategic adjustments, which can have broader implications for employment stability and job duration.

Acknowledgments

We are grateful to Torben M. Andersen, Philippe Askenazy, Stéphane Auray, Cyprien Batut, Luc Behaghel, Antoine Bertheau, Antoine Bozio, Christine Ehrel, François Fontaine, Catalina Franco, Camille Landais, Thomas Le Barbanchon, Brice Magdalou, Éric Maurin, Barbara Petrongolo, Arthur Poirier, Mariona Segu, Daphne Skandalis, and Andrea Weber for their help and comments, as well as to the participants of the following seminars and conferences for their valuable comments: ASSA 2021, EEA 2020, SOLE 2021, IZA 2020, Econ brown bag seminar at CBS 2021, 10th Search and Matching conference CBS 2021, Junior seminar CBS 2021, AFSE 2021, IIPF 2021, ESPE 2021, JMA 2021, LAGV 2021, IAB 2020. We thank the *Unédic* and the CASD for providing us access to the confidential data used in this work. We also thank the support of the EUR grant ANR-17-EURE-0001 as well as the support of the Chair *Economics of Climate Change and Human Health* at the University of Montpellier and the Chair *Public Policy Evaluation (Effectiveness, Equity and Acceptability)* at the University Paris-Dauphine—PSL. We benefited from the Labor Chair at Paris School of Economics, the LABEX OSE and Cepremap funding for data access.

References

Albanese, Andrea, Matteo Picchio, and Corinna Ghirelli, "Timed to say goodbye: does unemployment benefit eligibility affect worker layoffs?," *Labour Economics*, 2020, *65*, 101846.

Andersen, Torben M, Mark Strøm Kristoffersen, and Michael Svarer, "Benefit reentitlement conditions in unemployment insurance schemes," *Labour Economics*, 2018, *52*, 27–39.

Anderson, Patricia M, "Linear adjustment costs and seasonal labor demand: evidence from retail trade firms," *The Quarterly Journal of Economics*, 1993, *108* (4), 1015–1042.

_ **and Bruce D Meyer**, "The effects of the unemployment insurance payroll tax on wages, employment, claims and denials," *Journal of public Economics*, 2000, *78* (1-2), 81–106.

Avouyi-Dovi, Sanvi, Denis Fougère, and Erwan Gautier, "Wage rigidity, collective bargaining, and the minimum wage: evidence from French agreement data," *Review of economics and statistics*, 2013, *95* (4), 1337–1351.

Baguelin, Olivier and Delphine Remillon, "Unemployment insurance and management of the older workforce in a dual labor market: Evidence from France," *Labour Economics*, 2014, *30*, 245–264.

Baily, Martin Neil, "On the theory of layoffs and unemployment," *Econometrica*, 1977, pp. 1043–1063.

_ , "Some aspects of optimal unemployment insurance," *Journal of Public Economics*, 1978, *10* (3), 379–402.

Baker, Michael and Samuel A Rea Jr, "Employment spells and unemployment insurance eligibility requirements," *Review of Economics and Statistics*, 1998, *80* (1), 80–94.

Barbanchon, Thomas Le, Johannes Schmieder, and Andrea Weber, "Job search, unemployment insurance, and active labor market policies," in "Handbook of Labor Economics," Vol. 5, Elsevier, 2024, pp. 435–580.

_ **, Roland Rathelot, and Alexandra Roulet**, "Gender differences in job search: Trading off commute against wage," *The Quarterly Journal of Economics*, 2021, *136* (1), 381–426.

Bentolila, Samuel, Juan J Dolado, and Juan F Jimeno, "Dual Labor Markets Revisited," in "Oxford Research Encyclopedia of Economics and Finance" 2020.

_ **, Pierre Cahuc, Juan J Dolado, and Thomas Le Barbanchon**, "Two-tier labour markets in the great recession: France Versus Spain," *The economic journal*, 2012, *122* (562), F155–F187.

Bergstrom, Katy, William Dodds, and Juan Rios, "Welfare Analysis of Changing Notches: Evidence from Bolsa Família," *American Economic Journal: Economic Policy*, 2025.

Berton, Fabio and Pietro Garibaldi, "Workers and firms sorting into temporary jobs," *The Economic Journal*, 2012, *122* (562), F125–F154.

Best, Michael Carlos and Henrik Jacobsen Kleven, "Housing market responses to transaction taxes: Evidence from notches and stimulus in the UK," *The Review of Economic Studies*, 2018, *85* (1), 157–193.

Blanchard, Olivier and Augustin Landier, "The perverse effects of partial labour market reform: fixed-term contracts in France," *The Economic Journal*, 2002, *112* (480), F214–F244.

Blank, Rebecca M and David E Card, "Recent trends in insured and uninsured unemployment: is there an explanation?," *The Quarterly Journal of Economics*, 1991, *106* (4), 1157–1189.

Blasco, Sylvie and François Fontaine, "Unemployment duration and the take-up of unemployment insurance," Technical Report, IZA Discussion Paper 2021.

Boeri, Tito, "Institutional reforms in European labour markets. In O. Ashenfelter & D. Card (Eds.)," in "Handbook of Labor Economics," Vol. 4 Amsterdam: North Holland. 2011, pp. 1173–1236.

_ and Jan Van Ours, *The economics of imperfect labor markets*, Princeton University Press, 2013.

Cahuc, Pierre, Olivier Charlot, and Franck Malherbet, "Explaining the spread of temporary jobs and its impact on labor turnover," *International Economic Review*, 2016, *57* (2), 533–572.

_ , _ , _ , Helène Benghalem, and Emeline Limon, "Taxation of temporary jobs: good intentions with bad outcomes?," *The Economic Journal*, 2020, *130* (626), 422–445.

Calonico, Sebastian, Matias D Cattaneo, and Max H Farrell, "Optimal bandwidth choice for robust bias-corrected inference in regression discontinuity designs," *The Econometrics Journal*, 2020, *23* (2), 192–210.

Charlot, Olivier, Franck Malherbet, and Eloise Menestrier, "Fragmented Stability: Recalls and Fixed-Term Contracts in the French Labour Market," *IZA Discussion Papers*, 2024.

Chetty, Raj, "A general formula for the optimal level of social insurance," *Journal of Public Economics*, 2006, *90* (10), 1879–1901.

Christofides, Louis N and Chris J McKenna, "Unemployment insurance and job duration in Canada," *Journal of Labor Economics*, 1996, *14* (2), 286–312.

Clark, Andrew and Fabien Postel-Vinay, "Job security and job protection," *Oxford Economic Papers*, 2009, *61* (2), 207–239.

Cohen, Jonathan P and Peter Ganong, "Disemployment Effects of Unemployment Insurance: A Meta-Analysis," 2025.

Danziger, Leif, "Real shocks, efficient risk sharing, and the duration of labor contracts," *The Quarterly Journal of Economics*, 1988, *103* (2), 435–440.

David, H and Susan N Houseman, "Do temporary-help jobs improve labor market outcomes for low-skilled workers? Evidence from" Work First"," *American economic journal: applied economics*, 2010, *2* (3), 96–128.

Deshpande, Manasi, Itzik Fadlon, and Colin Gray, "How Sticky Is Retirement Behavior in the United States?," *Review of Economics and Statistics*, 2024, *106* (2), 370–383.

Dickens, William T, Lorenz Goette, Erica L Groshen, Steinar Holden, Julian Messina, Mark E Schweitzer, Jarkko Turunen, and Melanie E Ward, "How wages change: micro evidence from the International Wage Flexibility Project," *Journal of Economic Perspectives*, 2007, *21* (2), 195–214.

Doniger, Cynthia L. and Desmond Toohey, "These Caps Spilleth Over: Equilibrium Effects of Unemployment Insurance," Finance and Economics Discussion Series 2022-074, Board of Governors of the Federal Reserve System 2022.

Doornik, Bernardus Van, David Schoenherr, and Janis Skrastins, "Strategic formal layoffs: Unemployment insurance and informal labor markets," *American Economic Journal: Applied Economics*, 2023, *15* (1), 292–318.

Elsby, Michael WL and Gary Solon, "How prevalent is downward rigidity in nominal wages? International evidence from payroll records and pay slips," *Journal of Economic Perspectives*, 2019, *33* (3), 185–201.

Feldstein, Martin, "Temporary layoffs in the theory of unemployment," *Journal of political economy*, 1976, *84* (5), 937–957.

_ **and James Poterba**, "Unemployment insurance and reservation wages," *Journal of Public Economics*, 1984, *23* (1), 141–167.

Fougère, Denis, Erwan Gautier, and Sébastien Roux, "Wage floor rigidity in industry-level agreements: Evidence from France," *Labour Economics*, 2018, *55*, 72–97.

García-Pérez, J Ignacio, Ioana Marinescu, and Judit Vall Castello, "Can fixed-term contracts put low skilled youth on a better career path? Evidence from Spain," *The Economic Journal*, 2019, *129* (620), 1693–1730.

Gautier, Erwan, Sébastien Roux, and Milena Suarez Castillo, "How do wage setting institutions affect wage rigidity? Evidence from French micro data," *Labour Economics*, 2022, *78*, 102232.

Givord, Pauline and Lionel Wilner, "When does the stepping-stone work? Fixed-term contracts versus temporary agency work in changing economic conditions," *Journal of Applied Econometrics*, 2015, *30* (5), 787–805.

Gray, Jo Anna, "On indexation and contract length," *Journal of political Economy*, 1978, *86* (1), 1–18.

Green, David A and W Craig Riddell, "Qualifying for unemployment insurance: An empirical analysis," *The Economic Journal*, 1997, pp. 67–84.

Güell, Maia and Barbara Petrongolo, "How binding are legal limits? Transitions from temporary to permanent work in Spain," *Labour Economics*, 2007, *14* (2), 153–183.

Hagedorn, Marcus, Fatih Karahan, Iourii Manovskii, and Kurt Mitman, "Unemployment Benefits and Unemployment in the Great Recession: The Role of Macro Effects," Working Paper 19499, National Bureau of Economic Research 2013.

_ **, Iourii Manovskii, and Kurt Mitman**, "The Impact of Unemployment Benefit Extensions on Employment: The 2014 Employment Miracle?," *NBER Working Paper No. 20884*, 2015.

Hartung, Benjamin, Philip Jung, and Moritz Kuhn, "Unemployment Insurance Reforms and Labor Market Dynamics," *American Economic Journal: Macroeconomics*, 2022, *14* (3), 1–37.

Hopenhayn, Hugo A and Juan Pablo Nicolini, "Optimal unemployment insurance and employment history," *The Review of Economic Studies*, 2009, *76* (3), 1049–1070.

Jäger, Simon, Benjamin Schoefer, and Josef Zweimüller, "Marginal jobs and job surplus: a test of the efficiency of separations," *The Review of Economic Studies*, 2023, *90* (3), 1265–1303.

_ , _ , Samuel Young, and Josef Zweimüller, "Wages and the Value of Nonemployment," *The Quarterly Journal of Economics*, 2020, *135* (4), 1905–1963.

Jahn, Elke J, Regina T Riphahn, and Claus Schnabel, "Feature: flexible forms of employment: boon and bane," *The Economic Journal*, 2012, *122* (562), F115–F124.

Jessen, Jonas, Robin Jessen, Andrew C. Johnston, and Ewa Gałecka-Burdziak, "Moral Hazard among the Employed: Evidence from Regression Discontinuity," *NBER Working Papers #33450*, 2025.

_ , _ , Ewa Gałecka-Burdziak, Marek Góra, and Jochen Kluve, "The Micro and Macro Effects of Changes in the Potential Benefit Duration," *IZA DP No. 15978*, 2023.

Johnston, Andrew C and Alexandre Mas, "Potential unemployment insurance duration and labor supply: The individual and market-level response to a benefit cut," *Journal of Political Economy*, 2018, *126* (6), 2480–2522.

Jung, Philip and Keith Kuester, "Optimal labor-market policy in recessions," *American Economic Journal: Macroeconomics*, 2015, *7* (2), 124–156.

Jurajda, Štěpán, "Estimating the effect of unemployment insurance compensation on the labor market histories of displaced workers," *Journal of Econometrics*, 2002, *108* (2), 227–252.

Kahn, Lawrence M, "Employment protection reforms, employment and the incidence of temporary jobs in Europe: 1996–2001," *Labour Economics*, 2010, *17* (1), 1–15.

Kalleberg, Arne L, Barbara F Reskin, and Ken Hudson, "Bad jobs in America: Standard and nonstandard employment relations and job quality in the United States," *American sociological review*, 2000, pp. 256–278.

Kleven, Henrik J and Mazhar Waseem, "Using notches to uncover optimization frictions and structural elasticities: Theory and evidence from Pakistan," *The Quarterly Journal of Economics*, 2013, p. qjt004.

Kleven, Henrik Jacobsen, "Bunching," *Annual Review of Economics*, 2016, *8* (1), 435–464.

Kramarz, Francis and Marie-Laure Michaud, "The shape of hiring and separation costs in France," *Labour Economics*, 2010, *17* (1), 27–37.

Krueger, Alan B and Andreas I Mueller, "A contribution to the empirics of reservation wages," *American Economic Journal: Economic Policy*, 2016, *8* (1), 142–79.

Lalive, Rafael, "Unemployment benefits, unemployment duration, and post-unemployment jobs: A regression discontinuity approach," *American Economic Review*, 2007, *97* (2), 108–112.

— **, Camille Landais, and Josef Zweimüller**, "Market externalities of large unemployment insurance extension programs," *The American Economic Review*, 2015, pp. 3564–3596.

— **, Jan Van Ours, and Josef Zweimüller**, "How changes in financial incentives affect the duration of unemployment," *The Review of Economic Studies*, 2006, *73* (4), 1009–1038.

Landais, Camille, "Assessing the welfare effects of unemployment benefits using the regression kink design," *American Economic Journal: Economic Policy*, 2015, *7* (4), 243–278.

— **, Pascal Michaillat, and Emmanuel Saez**, "A Macroeconomic Approach to Optimal Unemployment Insurance: Theory," *American Economic Journal: Economic Policy*, 2018, *10* (2), 152–181.

Landaud, Fanny, "From employment to engagement? Stable jobs, temporary jobs, and cohabiting relationships," *Labour Economics*, 2021, *73*, 102077.

Le Barbanchon, Thomas, Roland Rathelot, and Alexandra Roulet, "Unemployment insurance and reservation wages: Evidence from administrative data," *Journal of Public Economics*, 2017.

Marinescu, Ioana, "The general equilibrium impacts of unemployment insurance: Evidence from a large online job board," *Journal of Public Economics*, 2017, *150*, 14–29.

Martins, Pedro S., "Working to get fired? Unemployment benefits and employment duration," *Journal of Policy Modeling*, 2021, *43* (5), 1016–1030.

Michaillat, Pascal, "Do matching frictions explain unemployment? Not in bad times," *American Economic Review*, 2012, *102* (4), 1721–50.

Milin, Kévin, "CDD, CDI: comment évoluent les embauches et les ruptures depuis 25 ans?," *Dares analyses*, 2018, *26*.

Mitman, Kurt and Stanislav Rabinovich, "Optimal Unemployment Insurance in an Equilibrium Business-Cycle Model," *Journal of Monetary Economics*, 2015, *71*, 99–118.

Mortensen, Dale T, "Unemployment insurance and job search decisions," *ILR Review*, 1977, *30* (4), 505–517.

OECD, *OECD Employment Outlook 2014* 2014.

Ortega, Javier and Laurence Rioux, "On the extent of re-entitlement effects in unemployment compensation," *Labour Economics*, 2010, *17* (2), 368–382.

Rebollo-Sanz, Yolanda, "Unemployment insurance and job turnover in Spain," *Labour Economics*, 2012, *19* (3), 403–426.

Schmieder, Johannes F and Till Von Wachter, "The effects of unemployment insurance benefits: New evidence and interpretation," *Annual Review of Economics*, 2016, *8*, 547–581.

__ , **Till von Wachter, and Stefan Bender**, "The effect of unemployment benefits and nonemployment durations on wages," *American Economic Review*, 2016, *106* (3), 739–77.

Seibold, Arthur, "Reference points for retirement behavior: Evidence from german pension discontinuities," *American Economic Review*, 2021, *111* (4), 1126–1165.

Topel, Robert H, "On layoffs and unemployment insurance," *The American Economic Review*, 1983, *73* (4), 541–559.

Virtanen, Marianna, Mika Kivimäki, Matti Joensuu, Pekka Virtanen, Marko Elovainio, and Jussi Vahtera, "Temporary employment and health: a review," *International journal of epidemiology*, 2005, *34* (3), 610–622.

Wallace, Frederick H, "The effects of shock size and type on labor-contract duration," *Journal of Labor Economics*, 2001, *19* (3), 658–681.

Winter-Ebmer, Rudolf, "Benefit duration and unemployment entry: A quasi-experiment in Austria," *European Economic Review*, 2003, *47* (2), 259–273.

Zhang, Min and Miquel Faig, "Labor market cycles, unemployment insurance eligibility, and moral hazard," *Review of Economic Dynamics*, 2012, *15* (1), 41–56.

Appendix A Data Sources

by alphabetical order

FNA. The FNA dataset is the centralized database of the French public employment agency (*Pôle Emploi*). It records all unemployment spells at the individual level for the whole French population as long as they register at the unemployment agency. For each of these spells, it provides information on the benefit level and duration as well as on jobseekers' previous work history. It also gives information on the characteristics of the last work contract (firm size, industry, type of contract, separation motive, tenure, etc.). We use this data over the 2005-2014 period.

FH-DADS. This dataset tracks individual workers over the 2004-2012 period by linking payroll data called "Déclarations Annuelles des Données Sociales" (hereafter *DADS*) to unemployment data (hereafter *FH*). The *DADS* dataset comes from mandatory employer records that are filled each year by every firm for each of their employees. These records are used to compute social security contributions. They include detailed information on workers' earnings, number of days worked, type of job, establishment size, industry and occupation, as well as workers' unique identifiers. The unit of observation is therefore the contract. However, if an individual is employed under several non-successive contracts in the same year in the same plant, all contracts will be aggregated into a single observation. The observation records the start date of the first contract and the end date of the last contract. This feature of the data makes it impossible to estimate, for instance, the share of workers recalled by the same plant within a year. The *FH* dataset is similar to the FNA dataset. The *DADS* and *FH* datasets have been matched together for a random subsample of a $1/12^{th}$ of the French population from 2004 to 2012,[35] resulting in an individual panel which allows us to track individuals' career path and transitions from employment to unemployment. There is one single observation per individual × plant × year in the *FH-DADS*, that potentially gathers non-successive contracts in the same year at the same plant. For this reason, when we need a precise estimate of work history, we restrict our sample to individuals whose working time during the employment spell matches that of a full-time worker over the same period, as measured by the difference between the start and end dates. This ensures that employment spells do not include gaps of non-employment between non-successive contracts within the same year at the same plant. This sample selection applies to Figures C4c, C4d, 5, B10b and Table C5, 6, B6, but it does not apply to the wage analysis. DOI

Labor Force Survey. The French Labor Force Survey ("*Enquête Emploi en Continu*", hereafter LFS) aims at describing the state of the labor market and its evolution: the profile of both employed and unemployed people, in terms of age, gender or qualifications for example, the characteristics of the jobs held (occupation, working hours, type of contract, etc.) and past work experience. The LFS is a rotating panel where the same household is surveyed 6 quarters in a row. Each quarter, 1/6 of the sample is therefore renewed. Analyses presented in Appendix Section C1 use this data over the 2008-2012 period. DOI

MMO. Our main analyses exploit the *Mouvements de Main d'Oeuvre* data (hereafter, *MMO*), an administrative database which tracks firms and covers the universe of work contracts in

[35] More specifically, this subsample is made up of individuals born in October.

France, over the 2005-2014 period. This dataset is provided by the French Ministry of Labour and comes from monthly records that employers from private and public plants with at least 50 workers as well as self-employed workers must fill. Plants of less than 50 employees are surveyed to complement the dataset. It includes all employment inflows and outflows in those plants and provides information on the date and type of flows (separation motive, nature of the contract), on workers' characteristics (gender, nationality, occupation, month and year of birth, municipality of residence) and on plants' characteristics (size, economic sector). The unit of observation is a flow (in or out of employment). Importantly, it also includes a unique identifier per plant, allowing us to track plants over time. However, it does not include a worker identifier. The MMO data have the notable advantage of providing accurate information on the start and end dates of employment contracts as well as the reason for separation. DOI

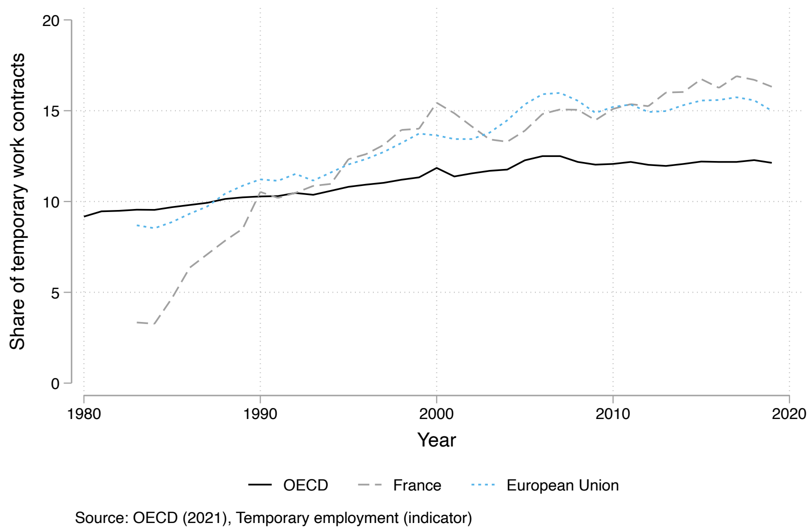
Reponse survey. The Reponse survey ("*Relations professionnelles et négociations d'entreprise*") is a firm survey carried out every 6 years. Its aim is to understand the state of employment relationships between management, staff representative bodies and employees. We use the last survey wave prior to the reform (2004-2005). The survey covers establishments in the commercial and associative sectors (excluding agriculture) in metropolitan France excluding Corsica. Establishments may be fully-fledged companies or only one of the establishments of a company. DOI

Vacancy Data. Analyses presented in Section 3.2.1 use the "STMT" dataset which provides monthly statistics on job offers. This dataset gathers all job adds published on the French Public Employment Services (PES) website and represents the largest source for job adds on the French job market. It includes information on the type of job offered (temporary or permanent), on the duration of the job and on the economic sector. We use these data over the 2005-2014 period. It does not include a firm identifier nor the offered wage for our period of study. DOI

Appendix B Additional Tables and Figures

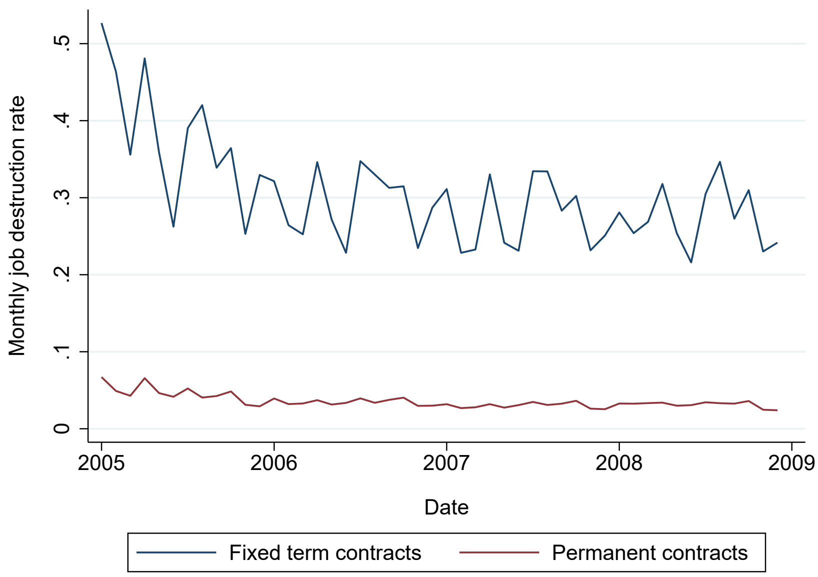
Descriptive statistics

Figure B1: Evolution of the share of temporary employment in OECD countries



Note: This figure shows the evolution of the share of temporary employment in total employment in OECD countries, in European countries (EU 27) and in France, over the 1980-2019 period.

Figure B2: Evolution of monthly job destruction by type of contract



Note: This figure shows the evolution of the monthly job destruction rate in the French economy, separately for fixed-term contracts and open-ended contracts, computed on MMO data over the period 2005-2008.

Table B1: Summary statistics - Decompositon of UI inflows by type of contract, 2005-2008

Variable	Share (%)
Share among all contracts:	
Fixed-term contracts (FTC)	75%
Open-ended contracts	25%
FTC of less than 1 year	71%
3-month FTC	3.2%
4-month FTC	2.4%
Share among FTCs of 1 month and more:	
3-month FTC	11.2%
4-month FTC	8.5%

Note: Authors' calculations, using FNA data. This table shows the proportion of contracts of each type held by workers before starting a new UI spell, over the 2005-2008 period. The calculation is based on the last contract before starting the UI spell. The first five rows show the share of a contract type among all new UI spells. The last 2 lines show these statistics on a sub-sample excluding FTCs of less than one month.

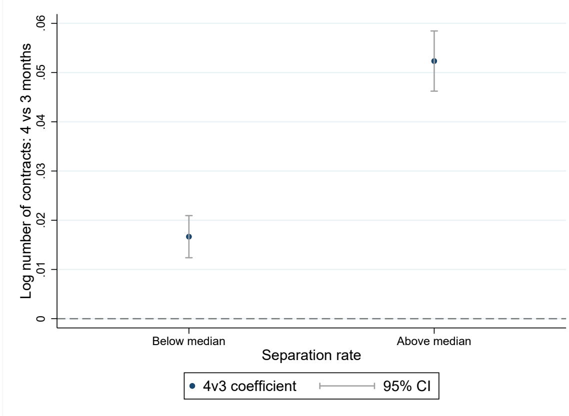
Table B2: Summary statistics - Decomposition of employment inflows by type of contract, 2005-2008

Variable	Share
Share among all contracts:	
Fixed-term contracts (FTC)	62%
3-month and 4-month contracts	4.9%
Share among FTCs:	
FTC of less than 1 month	57%
Contracts of less than 1 year	93.5%
3-month contracts	4.6%
4-month contracts	3.3%
Share among FTCs of 1 month and more:	
3-month contracts	11.0%
4-month contracts	7.9%

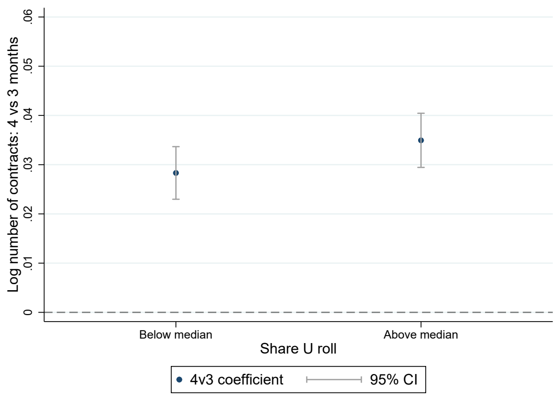
Note: Authors' calculations, using MMO data. This table shows the proportion of each type of contracts among all hiring flows between 2005 and 2008. The first two rows show the share of a contract type among all contracts. Rows 3 to 6 show these statistics on the sub-sample of FTCs. The last two rows show these statistics on the sub-sample of FTCs of one month and more.

Heterogeneity analysis

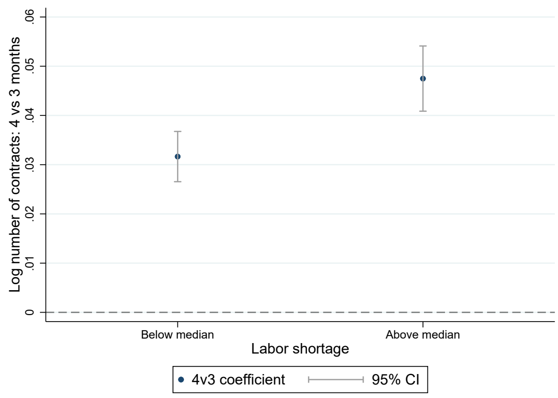
Figure B3: Impact of the reform on the number of 4-month FTCs relative to 3-month FTCs: Heterogeneity analysis with a log specification



(a) Separation rate



(b) Share of hires from unemployment roll



(c) Labor shortage

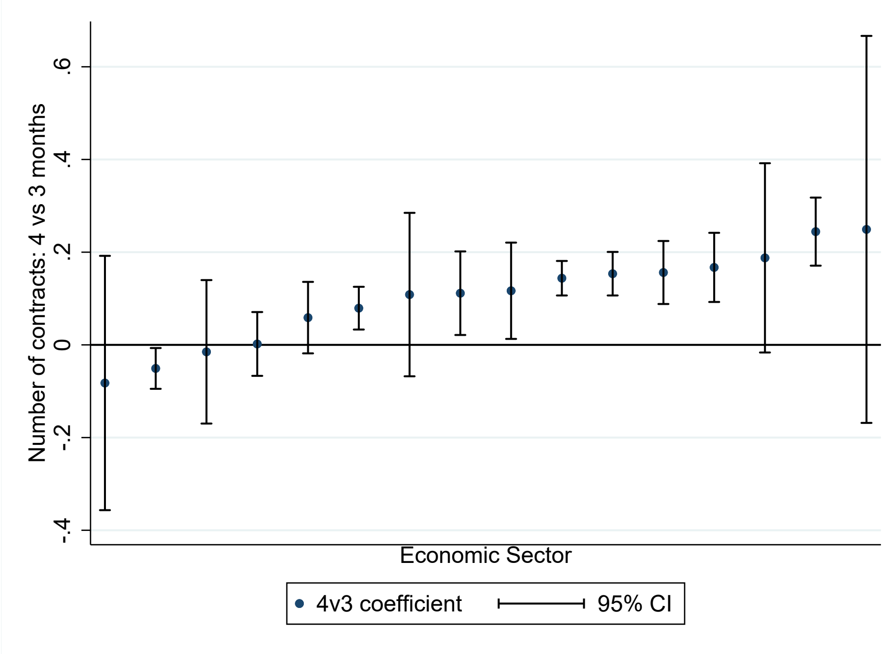
Note: This figure illustrates the effect of the UI reform on the log number of 4-month fixed-term contracts relative to 3-month fixed-term contracts, using MMO data. Each coefficient is estimated separately using Equation (1) where the dependent variable is expressed in log on plants above and below the median of the heterogeneity variables. Figure B3a compares plants above and below the median of the plant-level separation rate (i.e., the plant's annual average number of terminated FTCs divided by total employment). Figure B3b compares plants above and below the median share of hires from the unemployment register. Figure B3c compares plants in sectors with labor shortages above and below the median value of the labor shortage indicator. All heterogeneity variables are defined over the pre-reform period (2005-2008). Standard errors are clustered at the plant level.

Table B3: Impact of the reform on the number of 4-month FTCs relative to 3-month FTCs: Heterogeneity by plant separation rate, recruitment from unemployment rolls, labor shortage - Robustness tests

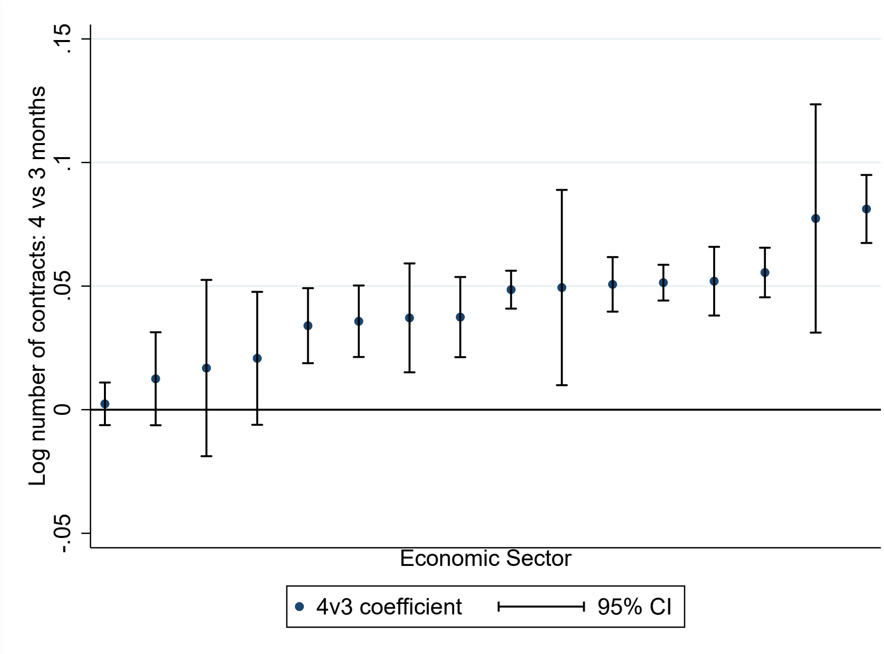
	Log Number of Contracts			Number of Contracts		
	Firms size ≥ 20		**Full sample of firms**	**Firms size ≥ 20**	**Full sample of firms**	
	(1)	(2)	(3)	(4)	(5)	(6)
4-month FTC	-0.0622***	-0.0764***	-0.0977***	-0.0967***	-0.1384***	-0.1457***
	(0.00183)	(0.00243)	(0.00205)	(0.00566)	(0.00490)	(0.01454)
Post-reform	-0.0488***	-0.0963***	-0.1323***	-0.0552***	-0.0103	-0.1925***
	(0.00270)	(0.00316)	(0.00321)	(0.00937)	(0.01290)	(0.01059)
4-month FTC × Post-reform	0.0167***	0.0283***	0.0359***	0.0089	0.0067	0.0620***
	(0.00218)	(0.00273)	(0.00229)	(0.00846)	(0.01172)	(0.01066)
4-month FTC × Separation	-0.1230***			-0.1572***	-0.4771***	
	(0.00338)			(0.04898)	(0.03574)	
Post-reform × Separation	-0.1335***			-0.3371***	-0.5036***	
	(0.00535)			(0.03245)	(0.03249)	
4-month FTC × Post-reform × Separation	0.0357***			0.1240***	0.2018***	
	(0.00381)			(0.03362)	(0.03165)	
4-month FTC × U rolls		-0.0827***		-0.1120***		-0.4788***
		(0.00349)		(0.01216)		(0.03738)
Post-reform × U rolls		-0.0209***		0.1897***		-0.1180***
		(0.00528)		(0.03245)		(0.03345)
4-month FTC × Post-reform × U rolls		0.0066*		-0.0548**		0.0754**
		(0.00391)		(0.02743)		(0.03229)
4-month FTC × Post-reform × Separation × U rolls				0.1776***		
				(0.06802)		
4-month FTC × Shortage			-0.0421***			
			(0.00352)			
Post-reform × Shortage			-0.0020			
			(0.00500)			
4-month FTC × Post-reform × Shortage			0.0135***			
			(0.00378)			
Constant	0.4731***	0.4978***	0.4400***	1.5609***	1.2839***	1.4333***
	(0.00162)	(0.00177)	(0.00157)	(0.01614)	(0.01258)	(0.01433)
Firm fixed-effect	✓	✓	✓	✓	✓	✓
Observations	749,560	691,080	711,300	682,480	893,080	780,920

Note: The table presents the heterogeneity of the UI reform's effect on the number and log number of 4-month fixed-term contracts (FTCs) relative to 3-month FTCs, considering plant separation rate, share of hires from the unemployment roll and sectoral labor shortage. A triple interaction model described in Equation (3) is estimated using our main MMO working sample. The *Separation* variable is a dummy indicating plants in the top 50% of the separation rate variable. The *U rolls* variable is a dummy indicating plants in the top 50% of hires from unemployment. The *Shortage* variable is a dummy indicating sectors in the top 50% of the labor shortage variable (defined as the share of plants' declaring hiring difficulties in this sector in the 2004-2005 *Reponse* survey). Columns (1) to (3) use the log of the number of contracts as the dependent variable. Columns (4) to (6) use the number of contracts as the dependent variable. Columns (3), (5), (6) use the full sample of firms, including those with less than 20 employees. Columns (1), (2), (4) restrict to firms with 20 employees or more. Standard errors (in parentheses) are clustered at the plant level. * p<0.10, ** p<0.05, *** p<0.01.

Figure B4: Impact of the reform on the number of 4-month FTCs relative to 3-month FTCs: Heterogeneity by economic sector



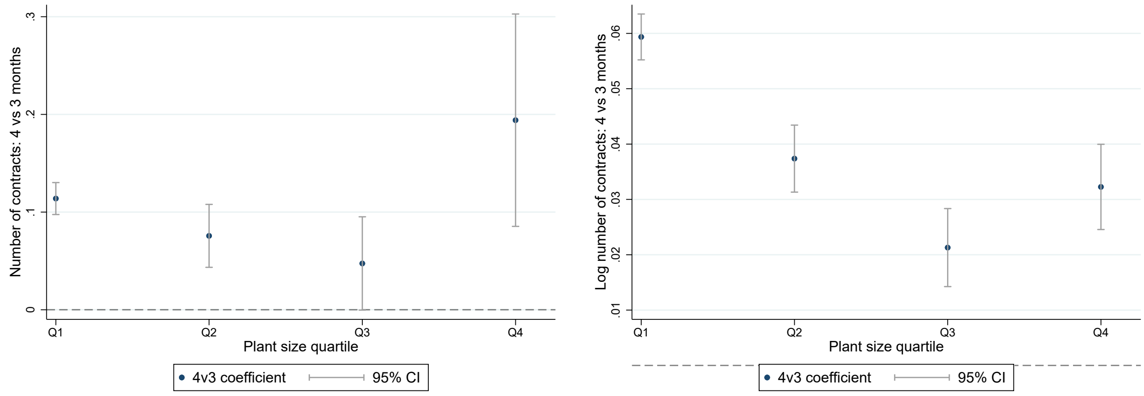
(a) Number of contracts



(b) Log number of contracts

Note: This figure represents the effect of the UI reform on the number of 4-month fixed-term contracts relative to 3-month fixed-term contracts, based on MMO data, using Equation (1). Each coefficient corresponds to a regression on a specific subsample, defined by economic sector. Figure B4a plots the coefficients using the dependent variable in levels while Figure B4b plots the coefficients using the dependent variable in logs. Standard errors are clustered at the plant level.

Figure B5: Heterogeneity analysis by plant size

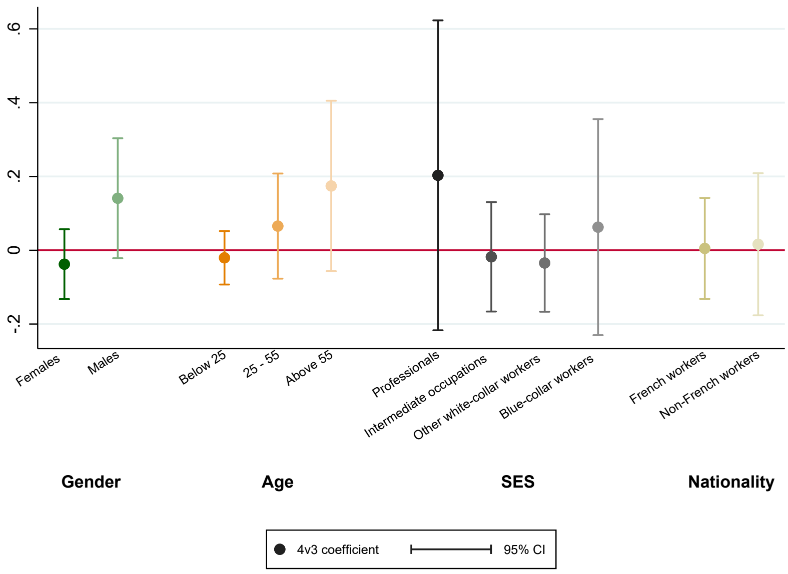


(a) Number of contracts

(b) Log number of contracts

Note: These figures represent the effect of the UI reform on the number of 4-month fixed-term contracts relative to 3-month fixed-term contracts, based on MMO data, using Equation (1). Each coefficient corresponds to a regression on a specific subsample, defined by quartiles of the plant's annual average of the total number of employees over the 2005-2008. In Figure B5a, the dependent variable is expressed in level, while in in Figure B5b, the dependent variable is expressed in log.

Figure B6: Heterogeneity analysis by worker characteristics



Note: This figure represents the effect of the UI reform on the number of 4-month fixed-term contracts relative to 3-month fixed-term contracts, based on MMO data, using Equation (1). Each coefficient corresponds to a regression on a specific subsample, defined by workers' gender, age, SES or nationality.

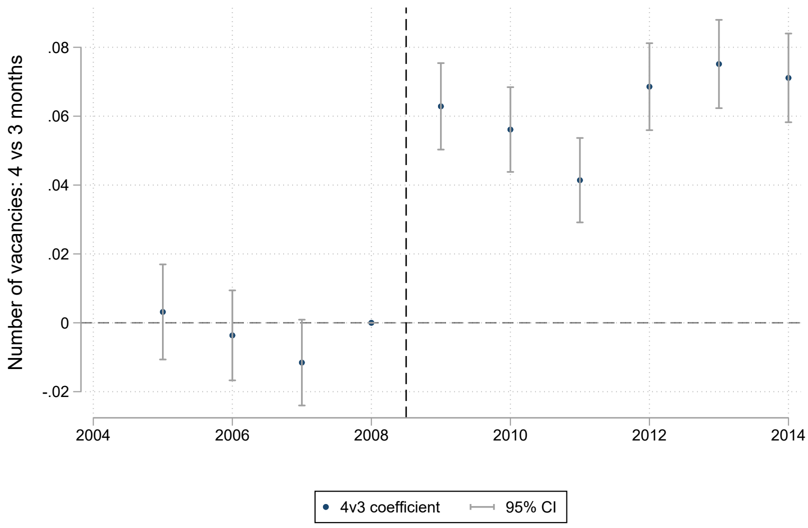
Analysis on vacancy data: Robustness checks

Table B4: Impact of the reform on the number of 4-month vacancies relative to 3-month vacancies: Log specification

	Log number of vacancies			
	(1)	(2)	(3)	(4)
4-month FTC	-1.1127***	-1.1147***	-0.3272***	-0.3881***
	(0.02747)	(0.02538)	(0.00316)	(0.00441)
Post-reform	-0.2436***	-0.1958***	-0.1178***	-0.1060***
	(0.01672)	(0.03265)	(0.00193)	(0.00298)
4-month FTC × Post-reform	0.1998***	0.1715***	0.0642***	0.0820***
	(0.00862)	(0.02032)	(0.00323)	(0.00523)
Constant	10.1720***	10.1775***	0.7131***	0.7869***
	(0.01427)	(0.01737)	(0.00137)	(0.00184)
Month fixed-effect	✓	✓	-	-
Month-by-occupation -by-region fixed-effect	-	-	✓	✓
Time period	2005-2014	2007-2010	2005-2014	2007-2010
Observations	230	94	1085712	375791

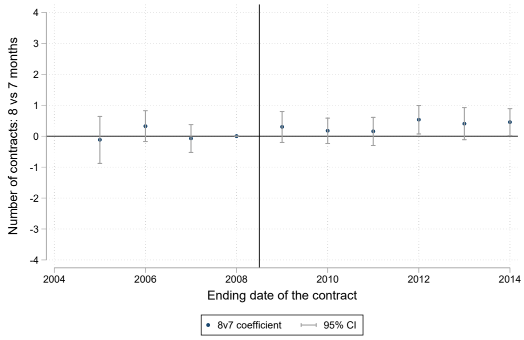
Note: The table shows the effect of the UI reform on the log number of 4-month vacancies relative to 3-month vacancies, based on the French Public Employment Service data, using a modified version of Equation (4) in which the variable is expressed in log. Each observation corresponds to the (log) number of vacancies of a certain duration (3 or 4 months) offered in a given month. Columns (1) and (3) refer to the 2005-2014 period and columns (2) and (4) restrict to observations close to the reform year (i.e., years 2007-2010). Columns (1) and (2) use the national level of vacancies each month while columns (3) and (4) use the log number of vacancies in a given month, region and 2-digit occupation. Columns (3) and (4) include month × occupation × region fixed effects. Standard errors (in parentheses) are clustered at the month level in columns (1) and (2), and month × occupation × region level in columns (3) and (4). Standard errors (in parentheses) are clustered at the month level.* p<0.10, ** p<0.05, *** p<0.01.

Figure B7: Impact of the reform on the number of 4-month vacancies relative to 3-month vacancies: Yearly estimates with a log specification

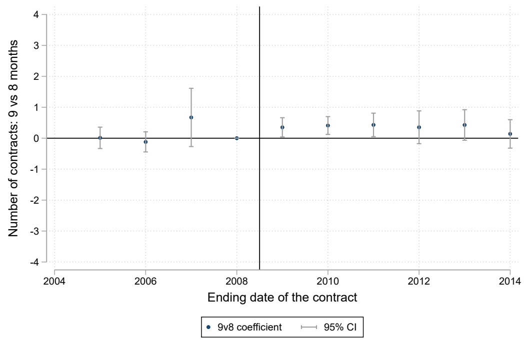


Note: The figure plots the yearly estimates of the effect of the UI reform on the log number of 4-month vacancies relative to 3-month vacancies over the 2005-2014 period, based on the French Public Employment Service data, using a modified version of Equation (5) in which the variable is expressed in log. The reference year is 2008, the last pre-reform year. Standard errors are clustered at the month × occupation × region level . The vertical dotted line shows the timing of the reform.

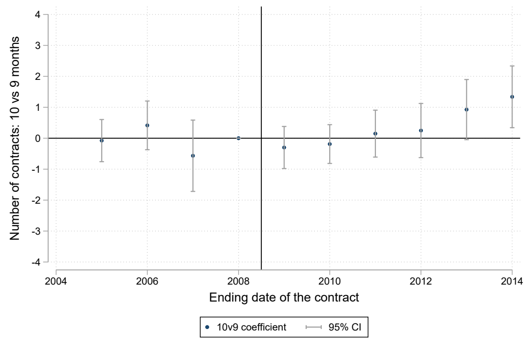
Figure B8: Placebo tests: Effect of the reform on thresholds between 7 and 12 months with vacancy data



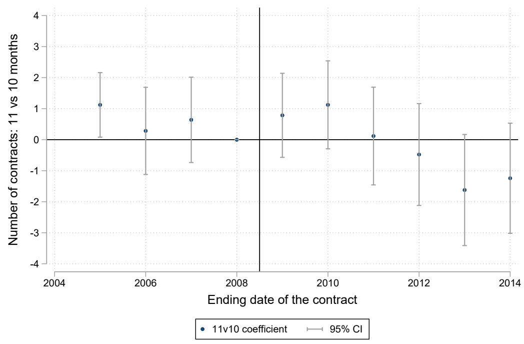
(a) 8 v 7



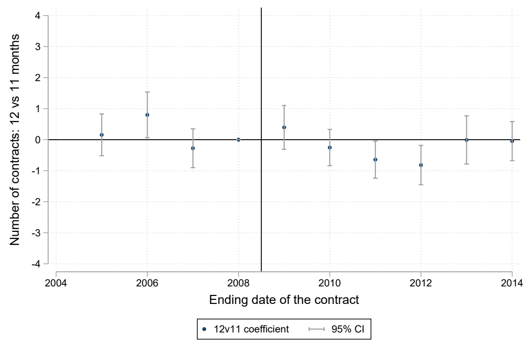
(b) 9 v 8



(c) 10 v 9



(d) 11 v 10



(e) 12 v 11

Note: The figure plots yearly estimates of the effect of the UI reform for each thresholds between 7 and 12 months, based on Equation (5), using data from the French Public Employment Service. Figure B8a represents yearly estimates of the effect of the reform on the number of 8-month vacancies relative to 7-month vacancies. Figures B8b to B8e respectively shows the same estimates for the 9-month, 10-month, 11-month and 12-month thresholds. The reference year is 2008, the last pre-reform year. Standard errors are clustered at the month × occupation × region level. The vertical dotted line shows the timing of the reform.

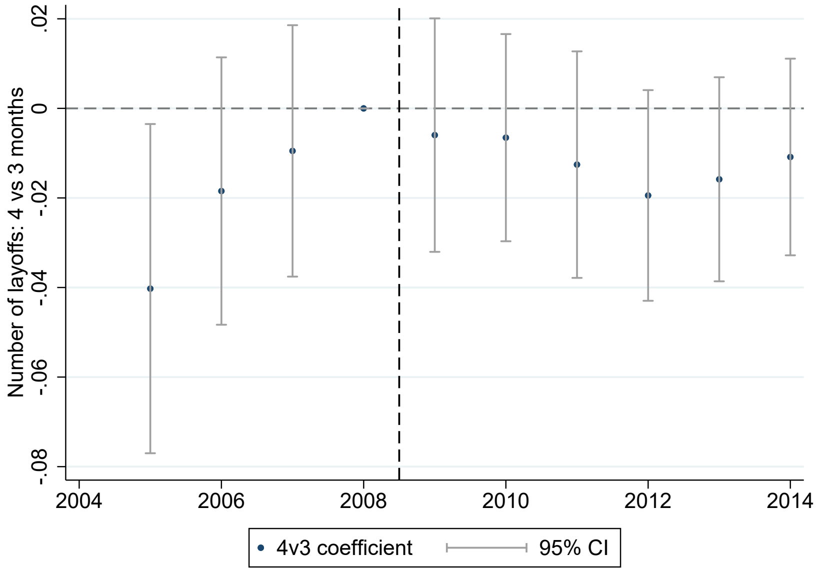
The timing of layoffs

Table B5: Impact of the reform on the number of layoffs after 4 vs 3 months of tenure

	Number of layoffs	
	(1)	(2)
4-month FTC	-0.0023	0.0100
	(0.00608)	(0.00765)
Post-reform	-0.2495***	-0.2208***
	(0.00703)	(0.00842)
4-month FTC × Post-reform	0.0052	-0.0015
	(0.00648)	(0.00867)
Constant	0.3619***	0.3652***
	(0.00498)	(0.00529)
Firm fixed-effect	✓	✓
Time period	2005-2014	2007-2010
Observations	227,080	90,832
Outcome mean (pre-reform)	0.213	0.259

Note: The table shows the effect of the UI reform on the number of layoffs after 4 vs 3 months of tenure, based on MMO data, using Equation (1). Each observation corresponds to the number of permanent contracts terminated in a given plant in a given year after 4 or 3 month of tenure. Column (1) refers to the 2005-2014 period and column (2) restricts to observations close to the reform (i.e., years 2007-2010). Standard errors (in parentheses) are clustered at the plant level.* p<0.10, ** p<0.05, *** p<0.01.

Figure B9: Impact of the reform on the number of layoffs after 4 vs 3 months of tenure: Yearly estimates



Note: The figure plots yearly estimates of the effect of the UI reform on the number of layoffs at 4 months vs. 3 months of tenure, based on MMO data, using Equation (2). The reference year is 2008, the last pre-reform year. Standard errors are clustered at the plant level. The vertical dotted line shows the timing of the reform.

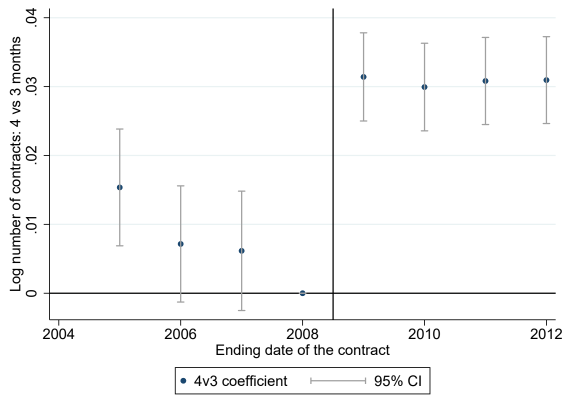
Spillover effects of UI eligibility threshold: Robustness checks

Table B6: Effect of the reform on the number of 4-month FTCs relative to 3-month FTCs

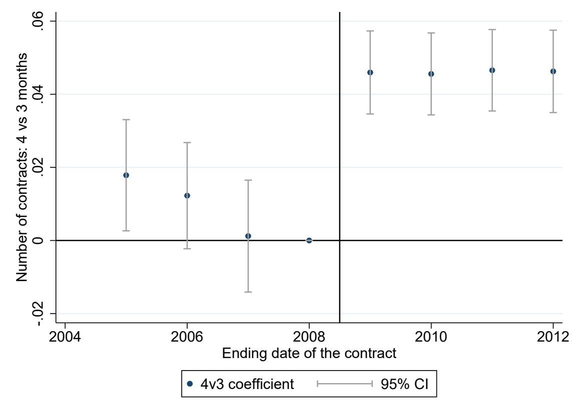
	Log number of contracts		Number of contracts	
	Work history ≥ 6 months		Work history ≥ 12 months	
	(1)	(2)	(3)	(4)
4-month FTC	-0.0253***	-0.0294***	-0.0401***	-0.0474***
	(0.00153)	(0.00222)	(0.00263)	(0.00390)
Post-reform	-0.0974***	-0.1038***	-0.1430***	-0.1583***
	(0.00088)	(0.00150)	(0.00153)	(0.00262)
4-month FTC × Post-reform	0.0236***	0.0276***	0.0383***	0.0452***
	(0.00158)	(0.00231)	(0.00270)	(0.00408)
Constant	0.1085***	0.1161***	0.1575***	0.1746***
	(0.00079)	(0.00122)	(0.00137)	(0.00215)
Firm fixed-effect	✓	✓	✓	✓
Time period	2005–2014	2007–2010	2005–2014	2007–2010
Observations	224,528	112,264	160,480	80,240
Outcome mean (pre-reform)	–	–	0.075	0.083

NOTE: The table shows the effect of the UI reform on the number of 4-month fixed-term contracts (FTCs) relative to 3-month FTCs, using FH-DADS data and Equation (1). Columns (1) and (2) report the effect on the log number of contracts on workers with at least 6 months of work history. Columns (3) and (4) report the effect on the number of contracts in levels on workers with at least 12 months of work history. Columns (1) and (3) use the full time period (2005-2014); Columns (2) and (4) restrict to years close to the reform (2007–2010). Standard errors clustered at the plant level. * *p<0.10, ** p<0.05, *** p<0.01*.

Figure B10: Impact of the reform on the number of 4-month FTCs relative to 3-month FTCs for workers already eligible for UI



(a) Log specification – work history ≥ 6 months

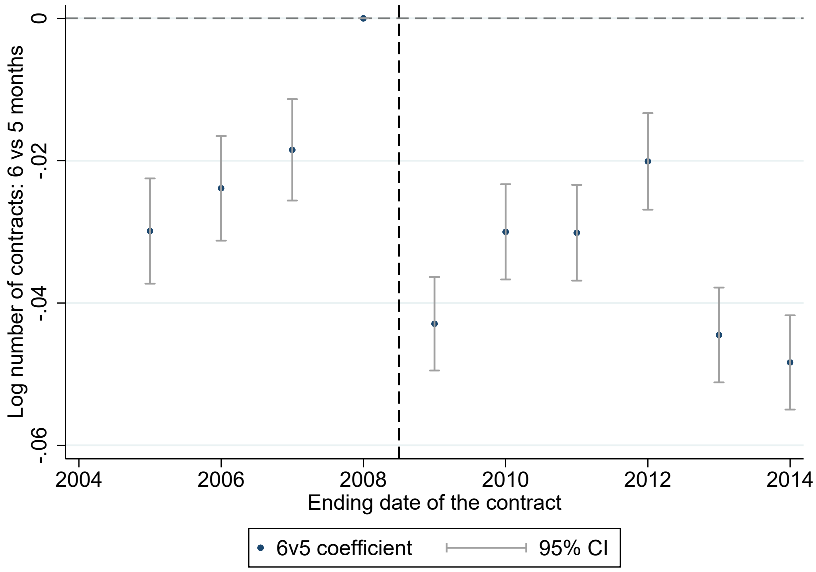


(b) Linear specification – work history ≥ 12 months

NOTE: These graphs plot yearly estimates of the effect of the UI reform on the number of 4-month fixed-term contracts (FTCs) relative to 3-month FTCs for workers already eligible for unemployment insurance. Figure B10a uses a log specification of the dependent variable and focuses on workers with at least 6 months of work history at FTC start. Figure B10b presents estimates for workers with at least 12 months of work history using a linear specification. Both are based on FH-DADS data and estimated using Equation (2). The reference year is 2008. Standard errors are clustered at the plant level. The vertical dotted line indicates the timing of the reform.

The impact of removing an existing work history threshold for UI eligibility: Robustness checks

Figure B11: Impact of the reform on the number of 6-month FTCs relative to 5-month FTCs: Yearly estimates with a log specification



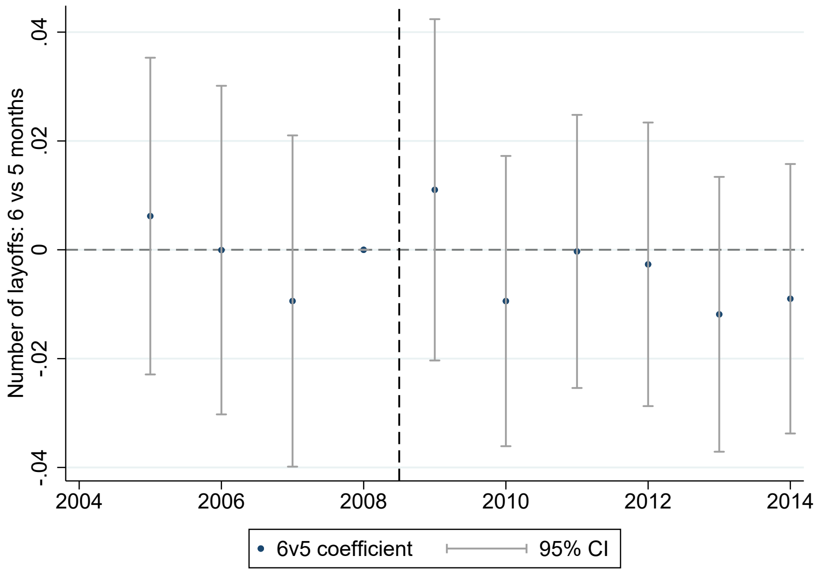
Note: The figure plots the yearly estimates of the effect of the UI reform on the log number of 6-month fixed-term contracts relative to 5-month fixed-term contracts, based on MMO data, using Equation (2) in which the dependent variable is expressed in log. The reference year is 2008, the last pre-reform year. Standard errors are clustered at the plant level. The vertical dotted line shows the timing of the reform.

Table B7: Impact of the reform on the number of layoffs after 6 vs 5 months of tenure

	Number of layoffs	
	(1)	(2)
6-month FTC	-0.0018	-0.0057
	(0.00504)	(0.00791)
Post-reform	-0.2267***	-0.2087***
	(0.00598)	(0.00786)
6-month FTC × Post-reform	-0.0029	0.0055
	(0.00604)	(0.00979)
Constant	0.3477***	0.3647***
	(0.00400)	(0.00533)
Firm fixed-effect	✓	✓
Time period	2005-2014	2007-2010
Observations	234,140	93,656
Outcome mean (pre reform)	0.210	0.259

Note: The table shows the effect of the UI reform on the number of layoffs after 6 vs 5 months of tenure, based on MMO data, using Equation (1). Each observation corresponds to the number of permanent contracts terminated in a given plant in a given year after 6 or 5 month of tenure. Column (1) refers to the 2005-2014 period and column (2) restricts to observations close to the reform (i.e., years 2007-2010). Standard errors (in parentheses) are clustered at the plant level.* p<0.10, ** p<0.05, *** p<0.01.

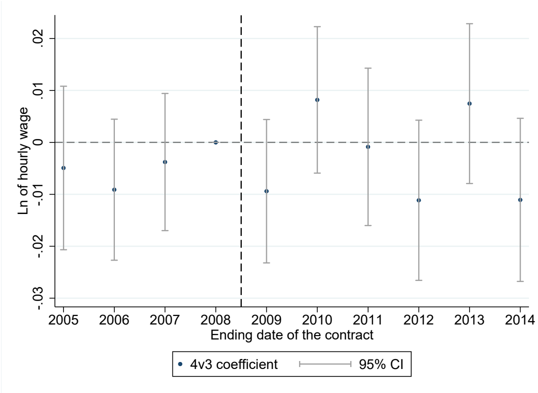
Figure B12: Impact of the reform on the number of layoffs after 6 vs 5 months of tenure: Yearly estimates



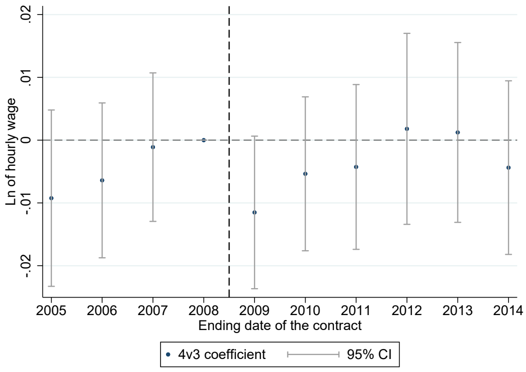
Note: The figure plots yearly estimates of the effect of the UI reform on the number of layoffs at 6 months v.s. 5 months of tenure, based on MMO data, using Equation (2). The reference year is 2008, the last pre-reform year. Standard errors are clustered at the plant level. The vertical dotted line shows the timing of the reform.

The impact of introducing a new work history threshold for UI eligibility: wage effect

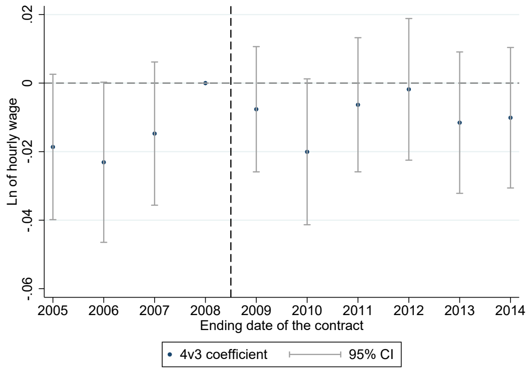
Figure B13: Impact of the reform on wages in 4-month FTCs relative to 3-month FTCs: Yearly estimates



(a) High Separation Rate



(b) High Share from U Rolls

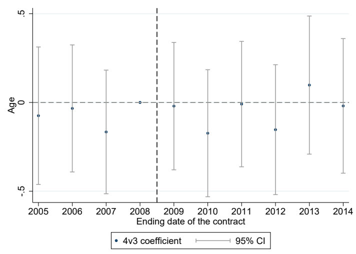


(c) High Shortage

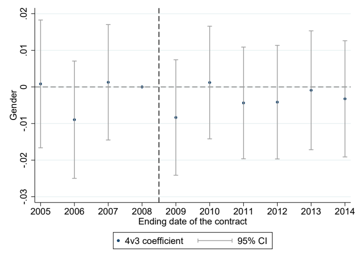
Note: The figure plots the yearly estimates of the effect of the UI reform on wages in 4-month fixed-term contracts relative to 3-month fixed-term contracts, based on FH-DADS data, using Equation (6). The sample is restricted to firms above the median of the pre-reform separation rate distribution (Figure B13a); of the pre-reform share of hires from unemployment rolls distribution (Figure B13b); of the pre-reform labor shortage distribution (Figure B13c). The reference year is 2008, the last pre-reform year. Standard errors are clustered at the plant level. The vertical dotted line shows the timing of the reform.

The impact of introducing a new work history threshold for UI eligibility: sorting

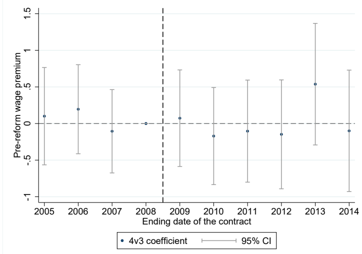
Figure B14: Effect of the reform on the characteristics of workers on 3- and 4-month FTCs



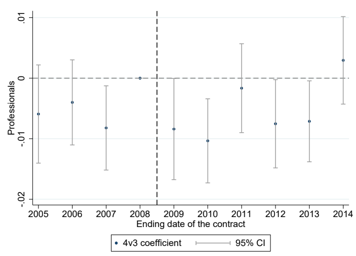
(a) Age



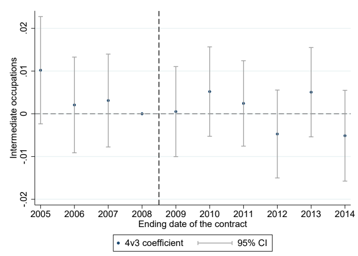
(b) Gender



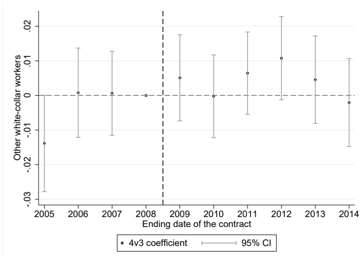
(c) Rank in the pay scale



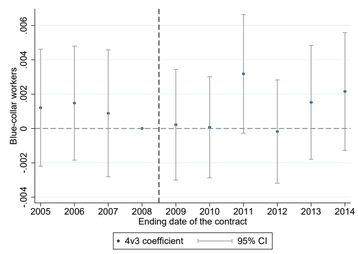
(d) Professionals



(e) Intermediate occupations



(f) Other white-collar workers



(g) Blue-collar workers

NOTE: These graphs present yearly estimates of the effect of the UI reform on the characteristics of workers in 4-month fixed-term contracts (FTCs) relative to those in 3-month FTCs, based on FH-DADS data and using an equation similar to Equation (6). Figures B14a–B14c correspond respectively to age, gender, and rank in the wage distribution (pre-reform average within sector and occupation). Figures B14d–B14g refer to occupational categories: being a professional, belonging to intermediate occupations, other white-collar workers (*employés*), or blue-collar workers (*ouvriers*). The reference year is 2008, the last pre-reform year. Standard errors are clustered at the plant level. The vertical dotted line shows the timing of the reform.

Appendix C Additional Analyses

C1 Evolution of fixed-term contracts conversion and immediate renewal around the reform

In the MMO data we use for the main analysis, job-to-job transitions that involve the same employer and do not require a waiting period—such as fixed-term contracts (FTCs) directly converted into permanent positions—are treated as a single employment spell. Therefore, in our empirical analysis using this dataset, we must assume that conversion (and immediate renewal) behaviors did not change *differentially* by contract duration after the reform.

This assumption has two key implications: (1) Conversion and immediate renewal rates should not evolve differentially for 3 and 4-month FTCs around the reform; (2) Some observed 3- and 4-month employment spells may actually result from a sequence of shorter contracts (e.g., three consecutive 1-month contracts, with no break between them, appearing as a single 3-month employment spell). The frequency of this type of renewal must not have evolved *differentially* for 3- and 4-month employment spells in our data. Regarding this second point, note that this effect should be marginal since French labor law requires a waiting period between successive FTCs. As a result, outside of the few exceptions that apply to this non-waiting rule, our MMO dataset does capture cases of successive FTCs.

In this appendix, we leverage the French Labor Force Survey ("*Enquête Emploi en Continu*", hereafter LFS) to provide supporting evidence for our assumption. We use all observations where information on contract type and expected duration is available (i.e., when an individual is surveyed for the first time or starts a new type of contract), over the period 2008–2012. The panel structure of the data allows us to measure the probability that a FTC of a given duration is either renewed or converted into a permanent contract.

To examine the first point, we first identify 3- and 4-month FTCs. For each individual, we then flag the first survey wave after the expected end of the FTC and create a dummy variable equal to 1 if the individual is still working in the same plant during that wave and 0 otherwise. This indicator captures both renewal and conversion to a permanent contract.[36] We then estimate a model as similar as possible to our main approach (Equation (1)) to assess whether the renewal and conversion rates of 3- and 4-month FTCs evolved differentially around the reform.[37] The estimated equation, using the sample of 3- and 4-month FTCs, is:

$$Y_{imt}^{d} = \alpha + \beta_1.post_t + \beta_2.\mathbb{1}_{d=4} + \beta_3.post_t * \mathbb{1}_{d=4} + \kappa_m + \epsilon_{imt} \tag{7}$$

where Y_{imt}^{d} is a dummy indicating that individual i is still working in the same firm during the first survey wave t following the expected end of the FTC of duration $d = \{3,4\}$. We also include month-of-the-year fixed effects to account for seasonality in employment flows. Results are presented in Table C1. These results are in line with our assumption. They show no differential change in the proportion of 3- and 4-month FTCs that were renewed or converted into permanent contracts around the reform.

Next, we investigate the second point, i.e. whether the probability that 3- and 4-month employment spells result from successive short-term contracts evolved differentially around the reform. Since the LFS data does not allow us to precisely measure the duration of successive contracts, we instead investigate whether the renewal probability of FTCs shorter than 4 months changed with the reform. To do so, we estimate, for each FTC duration of 4 months or less,

[36] The LFS data does not allow us to distinguish between both, as we are only able to observe the type of contract for the first contract.

[37] We cannot include firm fixed effects due to the resulting small sample size (69 observations).

the pre-post reform difference in the probability that the FTC is renewed or converted into a permanent contract. Specifically, we estimate the following equation for each subsample of FTCs defined by duration:

$$Y^d_{imt} = \alpha + \beta_1.post_t + \kappa_m + \epsilon_{imt} \tag{8}$$

Columns (1) to (5) in Table C2 report the results of this estimation for FTCs of 4, 3, 2, and 1 month(s), as well as FTCs shorter than a month. Column (6) further presents results for all FTCs of 4 months or less taken altogether. Reassuringly, none of the post-reform coefficients is statistically significant, confirming the absence of systematic changes in renewal/conversion behavior for short FTCs around the reform. Overall, based on LFS data, we find no evidence that renewal/conversion behaviors for short FTCs were affected by the reform.

Table C1: Differential immediate renewal/conversion probabilities before and after the reform: 4-month vs 3-month FTCs

	Renewal/conversion (1)
4-month FTC	-0.045 (0.035)
Post-reform	-0.031 (0.020)
4-month FTC × Post-reform	0.043 (0.039)
Constant	0.321*** (0.018)
Observations	4,027

Note: The table shows the effect of the UI reform on the difference in immediate renewal or conversion probabilities for 4-month and 3-month FTCs, using data from the French Labor Force Survey over the 2008-2012 period. Estimations are based on Equation (7). Standard errors are in parentheses.* p<0.10, ** p<0.05, *** p<0.01.

Table C2: Immediate renewal/conversion probabilities before and after the reform by FTC duration

	4-month FTCs (1)	3-month FTCs (2)	2-month FTCs (3)	1-month FTCs (4)	FTCs < 1 months (5)	FTCs ≤ 4 months (6)
Post-reform	0.007 (0.034)	-0.032 (0.020)	0.020 (0.020)	-0.000 (0.016)	0.013 (0.012)	0.000 (0.009)
Constant	0.280*** (0.030)	0.321*** (0.018)	0.165*** (0.018)	0.155*** (0.015)	0.061*** (0.010)	0.187*** (0.008)
Observations	1,120	2,914	2,149	2,930	2,960	11,929

Note: The table shows the effect of the UI reform on the immediate renewal or conversion probabilities for FTCs shorter or equal to 4 months, using data from the French Labor Force Survey over the 2008-2012 period. Columns 1 to 4 respectively show this results for 4-month, 3-month, 2-month and 1-month FTC. Column 5 shows this results for FTCs shorter than a month. Column 6 shows this results for all FTCs equal or shorter to 4 months taken altogether. Estimations are based on Equation (8). Standard errors are in parentheses.* p<0.10, ** p<0.05, *** p<0.01.

C2 The impact of introducing a new work history threshold for UI eligibility: Robustness checks

In this appendix section, we first check the robustness of our main result to the inclusion of plants who never recruited a worker on a 3- or 4-month FTC before the reform.[38] To do so, we estimate Equations (1) and (2) on the full sample of plants observed in the MMO data. Results are in line with those obtained on our main working sample: the estimated coefficient displayed in Table C3 implies a relative increase of 0.03 4-month FTCs ending each year in each plant, representing a 6% increase with regards to the pre-reform outcome mean. The relatively smaller magnitude of the effect on the full sample is consistent with the idea that only firms whose optimal FTC duration is close to the threshold may react to the introduction of an eligibility threshold. Figure C1a also reveals a very similar pattern for the dynamic effect as the one we get on our main sample, though estimated coefficients are slightly smaller in magnitude and not always significant for years far away from the reform.

Table C3: Impact of the reform on the number of 4-month FTCs relative to 3-month FTCs: Full MMO sample

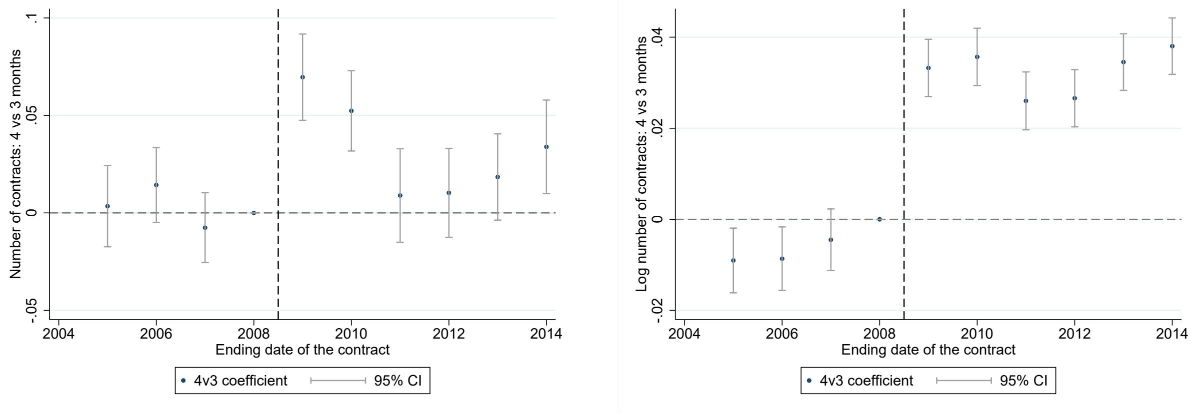
	Number of contracts	
	(1)	(2)
4-month FTC	-0.1811***	-0.1874***
	(0.00849)	(0.00917)
Post-reform	-0.0622***	-0.0967***
	(0.00791)	(0.00835)
4-month FTC × Post-reform	0.0297***	0.0648***
	(0.00756)	(0.00917)
Constant	0.6153***	0.6351***
	(0.00604)	(0.00598)
Firm fixed-effect	✓	✓
Time period	2005-2014	2007-2010
Observations	1,902,800	761,120
Outcome mean (pre reform)	0.496	0.509

Note: The table shows the effect of the UI reform on the number of 4-month fixed-term contracts relative to 3-month fixed-term contracts, based on Equation (1), using the full MMO sample of plants. Each observation corresponds to the number of fixed-term contracts of a certain duration (3 or 4 months) terminated in a given plant in a given year. Column (1) refers to the full MMO sample and column (2) restricts to observations close to the reform (i.e., years 2007-2010). Standard errors (in parentheses) are clustered at the plant level.* p<0.10, ** p<0.05, *** p<0.01.

To account for the fact that plants may have fairly different levels of employment in general and different levels of FTCs in particular, we also check that our main result is robust to using a log specification. To do so, we estimate a modified version of Equation (2), in which we replace the dependent variable Y_{it}^d by $log(Y_{it}^d + 1)$, in order to work on the same sample as in our main specification, which includes observations with zero FTC. Again, we get very similar results with this alternative specification to the ones we get using our main specification. As can be seen in Figure C1b, there is a sharp and stable increase in the prevalence of 4-month FTCs relative to 3-month FTCs after the reform.

[38] Recall that according to our model, optimal contract durations for these plants were further away from the new eligibility cutoff before the reform, which decreases the probability that they reacted to the reform on this margin.

Figure C1: Impact of the reform on the number of 4-month FTCs relative to 3-month FTCs



(a) Yearly estimates – full sample

(b) Yearly estimates – log specification

NOTE: These graphs plot yearly estimates of the effect of the UI reform on the number of 4-month fixed-term contracts (FTCs) relative to 3-month FTCs. Figure C1a is based on a linear specification using the full MMO plant sample. Figure C1b uses a log specification of the dependent variable. Both are estimated using Equation (2), with 2008 as the reference year. Standard errors are clustered at the plant level. The vertical dotted line marks the timing of the reform.

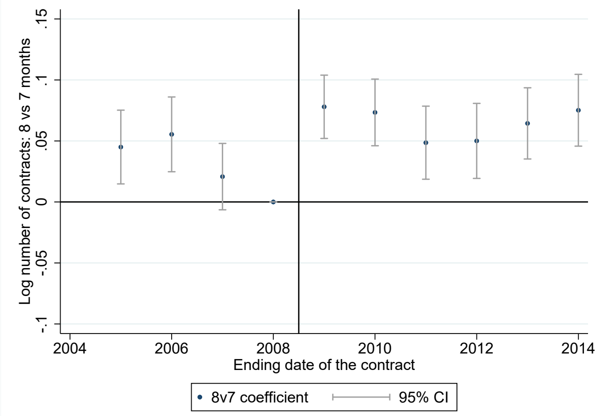
Finally, we examine other thresholds that should be minimally (if at all) affected by the reform, to verify that our main finding regarding the increase in 4-month FTCs relative to 3-month FTCs is not driven by a broader trend in FTC durations. Specifically, we analyze all possible thresholds between 7 and 12 months. These analyses are not pure falsification tests: as shown in Table 1, FTCs of these durations may have been influenced by minor changes in potential benefit duration (PBD).[39] We therefore interpret these tests as both an exploration of the potential impact of PBD on the distribution of FTC durations and a validation that the pattern observed at the 4-month threshold stands out as a distinct effect attributable to the introduction of the new eligibility criterion.

As shown in Figures C2a to C2e, none of these thresholds display the pattern observed for the 4-month versus 3-month comparison (including the 12-month versus 11-month threshold, where the relative change in PBD is the largest). Instead, they either show a clear null effect or exhibit substantial pre-reform trends and unstable post-reform coefficients. The only potential exception is the 9-month versus 10-month threshold, where 10-month FTCs tend to increase relative to 9-month FTCs after the reform, consistent with the (small) relative increase in PBD.[40] Overall, we find little evidence that PBD significantly influences the distribution of FTC durations. These results confirm that the pattern observed at the 4-month threshold is unique and consistent with the hypothesis that it reflects the effect of the new UI eligibility criterion introduced by the reform. Analyses presented in Section 3.2.1, based on vacancy data, further confirm this result.

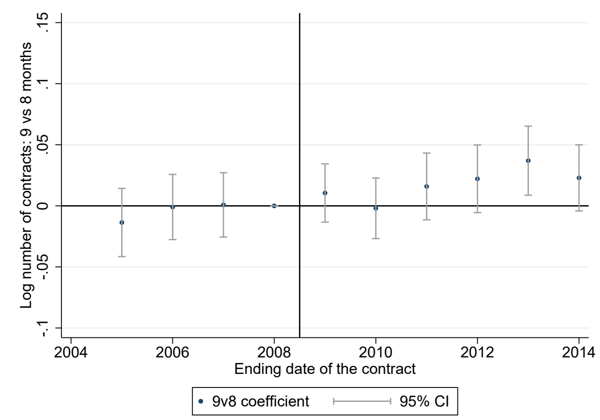
[39]For instance, before the reform, 11-month and 12-month FTCs respectively provided access to 7 and 12 months of PBD. After the reform, 12-month FTCs still provide access to 12 months of PBD, while 11-month FTCs now grant access to 11 months of PBD.

[40]Note however that the analysis using vacancy data do not reveal any differential evolution of vacancies of 10 vs 9 months (Figure B8c). We present these analyses in detail in Section 3.2.1.

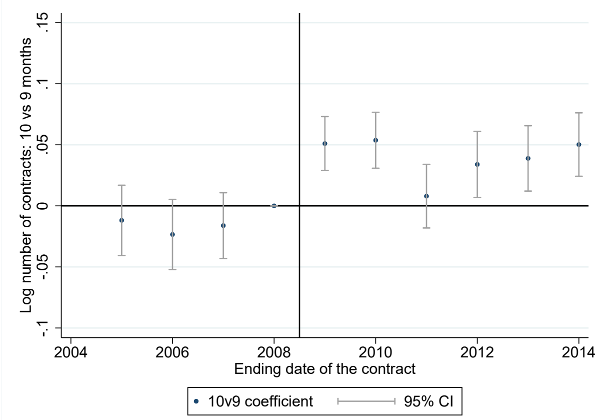
Figure C2: Placebo tests: Effect of the reform on thresholds between 7 and 12 months



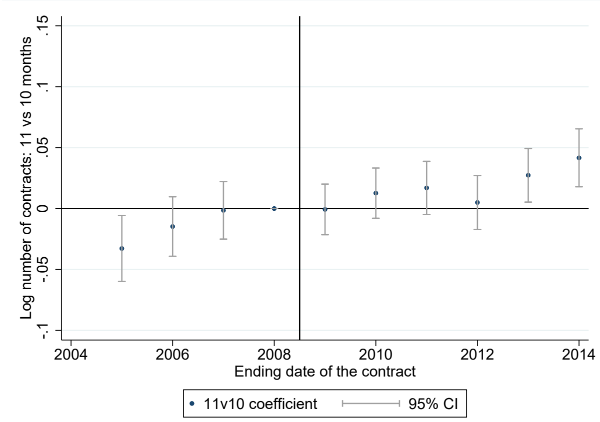
(a) 8 vs. 7



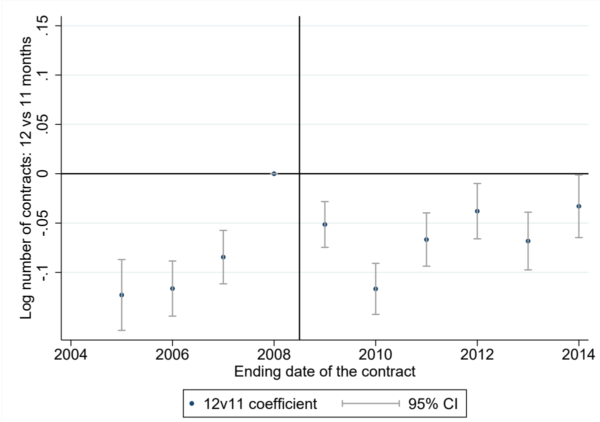
(b) 9 vs. 8



(c) 10 vs. 9



(d) 11 vs. 10



(e) 12 vs. 11

Note: The figure plots yearly estimates of the effect of the UI reform for each thresholds between 7 and 12 months, based on Equation (2), using MMO data. Figure C2a represents the effect of the reform on the number of 8-month FTC relative to 7-month FTC. Figures C2b to C2e respectively shows the same estimates for the 9-month, 10-month, 11-month and 12-month thresholds. The reference year is 2008, the last pre-reform year. Standard errors are clustered at the plant level. The vertical line shows the timing of the reform.

C3 The impact of UI eligibility on transitions out of employment

In this appendix, we empirically test whether the discontinuity in the value of non-employment for workers reaching the eligibility threshold induces an increased probability of exiting employment. To do so, we exploit the *FH-DADS* dataset (described in Appendix A). Our sample selection proceeds in several steps. We remove from the dataset workers who experience particular forms of employment and are subject to different rules in terms of UI, such as home employees for private employers or public sector workers. We end up with a working sample of 983,256 individual workers. We compute work history for each worker in our sample by summing up the number of days employed over a rolling window of 28 months. If an individual experiences an unemployment spell, the work history is reset to zero. The rolling window over which we compute work history then starts at the beginning of the first employment spell following the unemployment spell. We then focus on workers with a work history between 0 and 200 days over the period 2005-2012. Workers in our working sample are 7 years younger, have 6 fewer years of work experience, have lower levels of education and are more often in low-SES occupations than workers with longer work history (more than 200 days). They also tend to work under less favorable employment conditions overall: they earn 17% less, are much less likely to be on permanent (20% vs. 52%) or full-time (60% vs. 72%) contracts, work fewer hours per day, and work in smaller firms. Workers in our working sample are over-represented in sectors that frequently use very short-term contracts like agriculture, food and accommodation, administrative services or arts and entertainment, in line with previous studies (DARES, 2019),[41] as compared to workers with longer work history.[42]

Based on this sample, we first check whether the probability of transitioning from employment to *registered unemployment*, conditional on contract termination, discontinuously increases at the work history thresholds, both before and after the 2009 UI reform (Section C3.1). The observation is therefore a contract termination. We then show that the unconditional probability of transitioning from employment to *non-employment*—including both registered and non-registered unemployment—also increases discontinuously at the threshold (Section C3.2). For this analysis, we choose as a starting date the first job in the pre or the post-reform period. We then follow workers and flag the type of transition they make every 15 days. We also compute their work history at a biweekly frequency.

C3.1 Transitions from employment to registered unemployment

As a sanity check, we test that eligibility to UI *actually* induces more workers to register for UI after an employment spell. To do so, we plot the probability of registering for UI within the next 15 days against work history, conditional on job separation, before (Figure C3a) and after (Figure C3b) the 2009 reform. As the figure shows, conditional on ending an employment spell, the probability of registering for UI jumps at the relevant eligibility threshold in both periods. By contrast, the figure reveals no discontinuity at the pre-reform eligibility threshold in the post-reform period and vice-versa.[43] We check that this absence of discontinuity is not due to the change in the time window considered to compute work history (28 months vs. 22 months), and is still observed when using the post-reform reference base period for the calculation of work history for employment spells which ended pre-reform and vice-versa (Figures C3c and

[41]DARES, “Comment les employeurs mobilisent-ils les contrats très courts ?,” Technical Report, Ministry of Labour 2019.

[42]Tables showing these descriptive statistics are available upon request.

[43]Note, however, that we observe a spike at 6 months in the post-reform period due to the regularity in FTC duration.

C3d).

To take one step further, we then investigate whether eligibility to UI *unconditionally* affects the probability to transit from employment to registered unemployment. To do so, we compute workers' transitions from employment to registered unemployment at the biweekly level and we plot the transition rate along the work history distribution for our main sample of workers observed after the reform (Figure C4a). The figure shows a clear jump in the probability of transiting from employment to registered unemployment at the 4-month eligibility threshold. Regression analyses confirm that having a work history right above the eligibility threshold is associated with a 0.8 percentage point increase in the probability of moving from employment to registered unemployment, from an almost 0 probability at the left-hand side of the cutoff (Table C4). This low transition rate in absolute terms is mainly due to the fact that the vast majority of workers reaching the eligibility threshold do not separate from their jobs. For individuals separating from their job, it can also be due to an important non take-up of unemployment benefits, which typically ranges from 30 to 70% in high-income countries (see for example Blank and Card (1991) or Blasco and Fontaine (2021) for the US and France).

C3.2 Transitions from employment to non-employment

Our previous analyses provided clear evidence of a discontinuous increase in transitions from employment to registered unemployment at the eligibility threshold. However, it is not clear whether this result is due to an increase in job separations and/or a decrease in job search effort, or simply reflects a substitution from unregistered non-employment to registered unemployment.

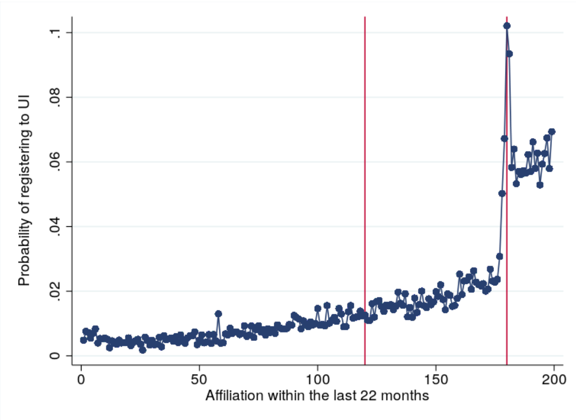
To investigate this issue, we compute workers' transitions from employment to non-employment at the biweekly level and we plot the transition rate along the work history distribution. We first look at transitions after the reform for our main sample of workers (Figure C4b).[44] In spite of a discontinuity observed at the 4-month cutoff, the figure is noisy and trends at the left and right of the cut-off are poorly estimated, which makes it difficult to be conclusive on this point. This could be partly due to measurement error in the computation of work history related to the structure of the *FH-DADS* dataset. In particular, this dataset includes one single observation per individual × plant × year, ignoring potential interruptions between two successive contracts in the same plant. To minimize measurement error, we focus on observations for which the number of hours worked over the whole period covered by the observation corresponds to the number of hours worked for a person employed full-time over the same period. This restriction ensures that each observation corresponds to one single contract.

Figures C4c and C4d plot the probability to move from employment to non-employment along the work history distribution for this sample of workers, respectively before and after the reform. They show a clear discontinuity at the post-reform eligibility threshold after the reform has been implemented, but no discontinuity at the same threshold before the reform. It also shows a discontinuity in transitions from employment to non-employment at the pre-reform eligibility threshold before the reform was implemented, but no discontinuity at the same threshold after the reform. To test more formally the presence of discontinuities in transitions out of employment, we further implement a regression discontinuity strategy at the 4-month and 6-month cut-offs on this restricted sample, before and after the reform. The re-

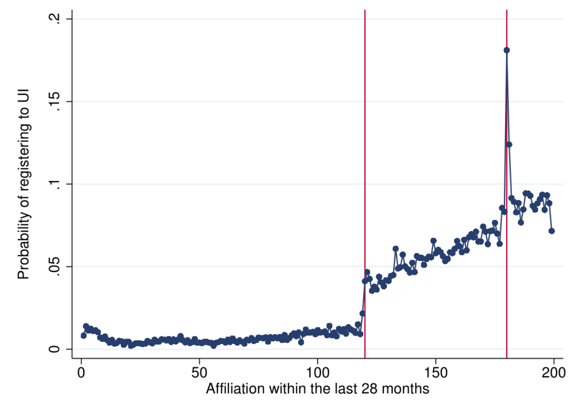
[44] To account for the spikes in the probability of exiting employment at 4 and 6 months for reasons unrelated to UI eligibility (e.g. many FTCs typically last 1, 2, 3, 4 or 6 months exactly, as outlined in Section 2), we drop from these analyses observations corresponding to a work history equal to the eligibility threshold +/- 2 days of work history.

sults outlined in Table C5 confirm that discontinuities in the transition rate from employment to non-employment are only observed at the pre-reform cut-off for pre-reform observations and at the post-reform cut-off for post-reform observations. The table also indicates that the jump is significant and equivalent to a 1.8-2 percentage points increase in both cases, which represents a 20% to 50% increase relative to the rate right below the cutoff.

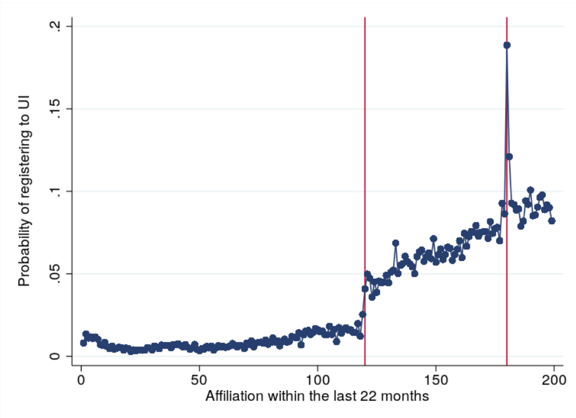
Figure C3: Probability of UI registration conditional on job separation



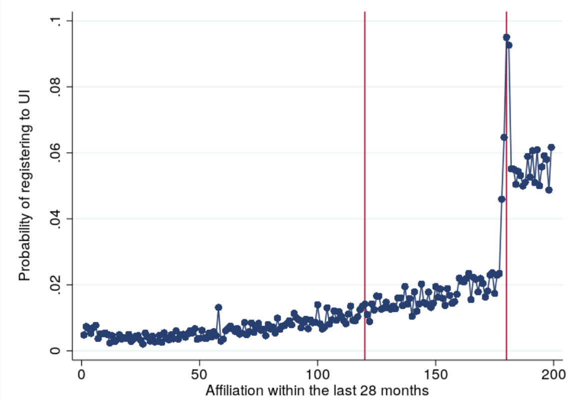
(a) Pre-reform, 22-month base reference period



(b) Post-reform, 28-month base reference period



(c) Post-reform obs., 22-month base ref. period



(d) Pre-reform obs., 28-month base ref. period

SOURCE: FH-DADS.
NOTE: This figure plots the probability to transit from employment to registered unemployment conditional on ending an employment spell, with respect to work history computed over the relevant base reference period used to determine UI eligibility. Figures C3a and C3b refer to pre- and post-reform observations with corresponding base periods (22 and 28 months). Figures C3c and C3d show placebo tests using mismatched reference periods. The two vertical lines represent 4 months and 6 months of work history.

Table C4: Discontinuity in the transition rate from employment to registered unemployment

	Probability of transiting from employment to registered unemployment		
	Linear (1)	Quadratic (2)	Cubic (3)
RD_Estimate	0.008*** (0.001)	0.007*** (0.001)	0.010*** (0.001)
Observations	1,270,880	1,270,880	1,270,880

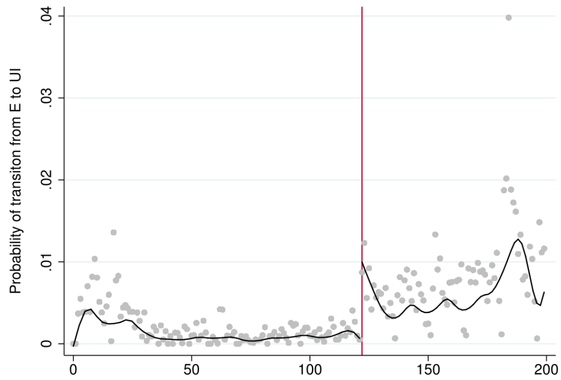
** p<0.10, ** p<0.05, *** p<0.01. Standard errors in parentheses.*
NOTE: The regression shows in a regression discontinuity design spirit the discontinuity in the biweekly transition rate from employment to UI. The running variable is the work history over the last 28 months and the cutoff value is 4 months. We use the optimal data-driven bandwidth selection procedures proposed by Calonico et al. (2020). Linear, quadratic and cubic specifications.

Table C5: Discontinuities in the transition rate from employment to non-employment before and after the reform

	(1) Linear	(2) Quadratic	(3) Cubic
Panel A: Post-reform observations, discontinuity at 122 days			
RD_Estimate	0.018** (0.009)	0.020* (0.012)	0.026 (0.020)
Observations	158,169	158,169	158,169
Panel B: Pre-reform observations, discontinuity at 122 days (Falsification test)			
RD_Estimate	0.005 (0.013)	0.009 (0.015)	-0.034 (0.037)
Observations	55,180	55,180	55,180
Panel C: Pre-reform observations, discontinuity at 182 days			
RD_Estimate	0.020** (0.010)	0.020 (0.013)	0.021 (0.015)
Observations	80,618	80,618	80,618
Panel D: Post-reform observations, discontinuity at 182 days (Falsification test)			
RD_Estimate	-0.002 (0.005)	-0.008 (0.009)	-0.006 (0.011)
Observations	238,317	238,317	238,317

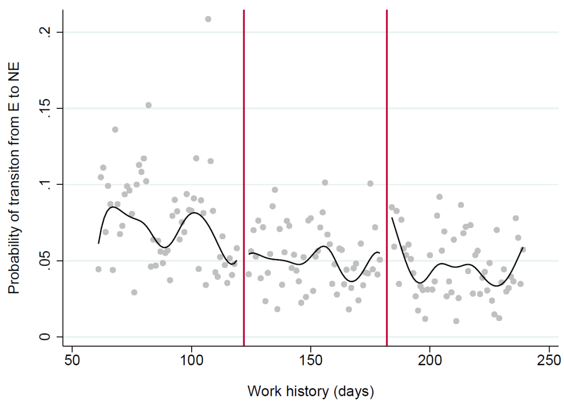
** p<0.10, ** p<0.05, *** p<0.01. Standard errors in parentheses.*
SOURCE: FH-DADS.
NOTE: The table shows in a regression discontinuity design spirit the discontinuity in the biweekly transition rate from employment to non-employment, using linear (col. (1)), quadratic (col. (2)) and cubic specifications (col. (3)). For Panels A and B, the running variable is the work history over the last 28 months and the cutoff value is 122 days. For Panels C and D, the running variable is the work history over the last 22 months and the cutoff value is 182 days. Panels A and D refer to post-reform observations while panels B and C refer to pre-reform observations. Each panel corresponds to a separate regression for which a different bandwidth is selected through the MSE optimal bandwidth selector (Calonico et al., 2020).

Figure C4: Transition rates from employment along the work history distribution



(a) To registered unemployment (post-reform)

(b) To non-employment (post-reform, full sample)



(c) To non-employment (pre-reform)

(d) To non-employment (post-reform)

SOURCE: FH-DADS.
NOTE: These graphs plot the biweekly transition rates from employment to either registered unemployment or non-employment, with respect to work history computed over the relevant base reference period (22 or 28 months). Figures C4a and C4b refer to the post-reform period (contracts starting after April 1st, 2009). Figures C4c and C4d display pre- and post-reform transition rates to non-employment. Vertical lines correspond to 4 months (122 days) and 6 months (182 days) of work history.