Can I Stay or Should I Go?
Mandatory Retirement and Labor Force Participation of Senior Workers

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Abstract

The growing pressure for reforming financially unstable pension systems makes it crucial to understand the determinants of retirement decision. This article focuses on a paramount though often disregarded channel, namely demand-side induced retirement through mandatory retirement. Labor supply and demand determinants of retirement are often difficult to disentangle. In this paper, I take advantage of a unique natural experiment, the progressive ban of mandatory retirement in France in the 2000s. Drawing on an extensive administrative dataset, I use inter-industry reform-induced variations in mandatory retirement legislation, to insulate this factor from other determinants of retirement, such as financial incentives. I find that demand-side determinants do play a role in retirement behavior, as the repeal of mandatory retirement increased employment of older workers. This channel, however, does not account for the major part of the large increase in labor force participation of older workers observed in the last two decades. Secondly, as in the French pension system the mandatory retirement age coincides with the full rate age, I exhibit a previously uncovered determinant of the large spike in retirement distribution at this age. Mandatory retirement is estimated to explain around 7% of the observed bunching at the full rate.

Keywords: Retirement · Employment · Labor demand · Labour market discrimination
JEL: J26 · J21 · J23 · J7

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1 Introduction

Increasing retirement age is the most common lever chosen by policy makers to relieve financial pressure over public pension systems. As forecasting the effect of undergoing and future reforms is needed to assess the financial sustainability of the system, it is essential to understand the mechanisms underlying retirement behavior.

After several decades of steep decline, French employment rates at higher ages have started to increase sharply since the early 2000s, as illustrated in figure 1. The underlying causes of this trend reversal are not well understood thus far. The three main explanations usually brought forward are the shutdown of most early retirement public schemes, the strengthening of financial incentives to pursued activity (actuarial adjustment beyond the full rate, relaxed earnings restrictions for pensioners), and the decrease in replacement rates (decreasing annuity rate, less generous indexation rules). The relative role of each of these potential causes is difficult to identify, as many reforms were implemented within a short period of time.

This article focuses on another potentially paramount though often disregarded channel, namely demand-side induced retirement through mandatory retirement. Mandatory retirement refers to the possibility given to firms to force older workers to retire. It corresponds to a lay-off but with much less restrictions than in the general case.

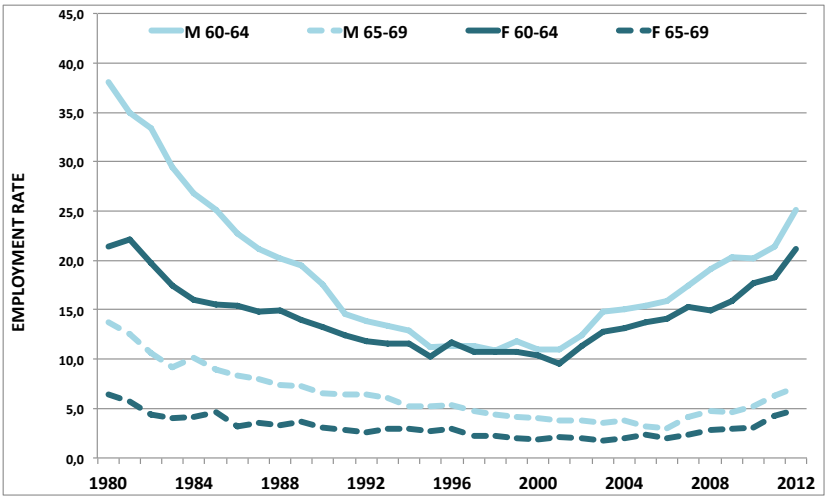
Demand-side effects are usually pinpointed as an important driver of senior workers labor force participation (see for example Lumsdaine and Mitchell, 1999; or Duval, 2003). The theoretical mechanism is straightforward: with (perceived) declining productivity with age and some degree of wage rigidity, firms have incentives to lay-off older workers and hire young ones. Yet in most existing models, retirement is described as the result of an individual trade-off between work and leisure.[1] The common implicit assumption is that retirement is the individual worker's choice, which may not always be the case.

In this paper, I take advantage of a unique quasi-natural experiment, the progressive ban of mandatory retirement in the 2000s in France, to properly identify an employers' effect over retirement behavior. From 2003 to 2010, the mandatory retirement age was first increased from the full rate age (which can be reached from age 60) to 65, then to 65 to 70. Reforms were implemented progressively over time and at different speeds according to the type of employer, due to industry-based specific labor market legislation in France. I use variations in mandatory retirement legislation over time, between industries and different types of workers, to identify its effect on withdrawal from the labor force. This makes it possible to insulate this factor from other determinants of retirement, in particular financial incentives.

To explore these questions, this work relies on an extensive administrative database, provided by the *Caisse nationale d'assurance vieillesse* (*CNAV*), the public pension scheme for

[1] There are two main strands in the literature on retirement behavior: structural models (e.g Rust and Phelan, 1997; or French, 2005), or reduced-form estimations eliciting the main determinants of retirement decision (Coile and Gruber, 2007). In both of them, retirement is modeled as a purely individual (or household) decision.

Figure 1: Employment rates by gender and age group in France



SOURCE: Minni (2015)

wage earners of the private sector. It is the biggest pension scheme in France, covering two thirds of the working population.

The approach followed in this paper directly relates to some papers studying the effect of banning mandatory retirement in the US (Neumark and Stock, 1999; Ashenfelter and Card, 2002; Adams, 2004) or in Canada (Shannon and Grierson, 2004), using state-specific legislation. As reviewed in Neumark (2003), the literature demonstrates that increasing employment protection for older workers had a positive but overall modest effect over their labor force participation. Most of the existing literature on the subject focuses on the specific context of the North-American labor market, with reforms occurring in the 1960-1980s. Paradoxically enough, to my knowledge no paper has studied the effect of this type of scheme in Europe, where labor market legislation is suspected to have a strong impact on employment rates of older workers. As mandatory retirement is still possible in some European countries (Italy, Germany in some specific contracts) and was recently removed in others (Spain, UK), the scope of this study clearly goes beyond the French national framework.

This paper also relates to the literature studying the massive spikes in retirement rates at some key ages of the social security system, namely the minimum retirement age and the full retirement age. Those spikes have been largely documented in the literature, for many different countries (Gruber and Wise, 2004). Bunching in retirement age distribution observed at the full retirement age has received many complementary explanations: social security incentives with lower than actuarially-fair adjustment incentives of firms private plans, or interaction with medicare. All those explanations taken together, however, may not be enough to explain the magnitude of the spikes, as documented by Lumsdaine *et al.* (1996). The residual part of the spikes that cannot be explained is usually attributed to norms or framing (Mastrobuoni, 2009; Blau and Behaghel, 2012). In this paper, we exhibit an original

demand-side determinant of the usual "puzzle" of bunching in retirement behavior: in France, the mandatory retirement age has for a long time coincided with the full rate age, so that the concentration of retirement at the latter can be partly explained by the former.

The contribution of this paper is twofold. First, I identify a direct effect of employer's choices over labor force participation of the 60-64 age group. The effect is sizable: exit rates from employment increase by 2pp for workers that can be forced to retire through mandatory retirement. But the repeal of mandatory retirement accounts for only a minor share of the large increase in labor force participation of older workers observed in the last decade. Secondly, I show that mandatory retirement has been an important and previously uncovered determinant of the large spike at the full rate age in the observed retirement distribution.

Note that this paper focuses on short term effects of banning mandatory retirements. Even if it has not been the case in the US in the past, in theory increasing labor market protection of older workers can have adverse effects on their employment rates (as it increases the cost of job termination) on the long run. These potential effects are set-aside in the present work and offer a fruitful avenue for future research.

The rest of the paper proceeds as follows. In the next section, we briefly describe the institutional context regarding pension and mandatory retirement rules. Section 3 describes the French data used in this paper. Section 4 presents the identification strategies and models that are estimated in section 5. Section 6 concludes.

2 Background

2.1 Overview of the French pension system

The public pension system in France is very large and fragmented, with more than 130 different pension schemes providing benefits amounting to roughly 14% of GDP (COR, 2015). In this paper, we focus on the *Régime général* (*RG*), the main scheme for wage earners of the private sector.[2] It is the most important public pension scheme in France, covering more than two thirds of the working population. Together with its complementary point-based public second pillar, it provides the main part of income during retirement. The benefits can be claimed from the minimum age of eligibility (MA), equals to 60 for the period of interest.

The general formula for computing benefits is the following: $B = \tau \times CP \times W_{\text{ref}}$. The reference wage W_{ref} is the average of the 25 best yearly earnings under the Social Security ceiling. The conversion rate τ corresponds to a reference rate τ_{ref} that can be increased by a bonus or decreased by a penalty. The main peculiarity of the French pension system is that the full rate (defined as $\tau \geq \tau_{\text{ref}}$) depends on both the current age and the past work duration, and not only on age as in many countries. Since the 1980s indeed, the full rate

[2]See Mahieu and Blanchet (2004) for a more comprehensive description of the French public pension system in English language.

age (FRA) can be reached under two conditions: either an age condition, when the normal retirement age (NRA) is reached; or a work duration condition, if the insurance duration D equals the full rate work duration (D_{FR}). For the cohorts under study, NRA is equal to 65, and D_{FR} depends on the year of birth (equals to 160 trimesters for generation 1943). The most favorable condition for the workers (the one which is reached first) is retained. It implies that one can reach the full rate as soon as she reaches the minimum age of eligibility, if she has contributed the required amount of trimesters. The total insurance duration (D) is the sum of the trimesters validated in the general scheme and the years validated in other schemes. Within each scheme, insurance duration corresponds to the sum of periods worked with paid contribution and non-contributory periods (*périodes assimilées*). The latter are trimesters of insurance that can be validated without work, for periods of unemployment, sickness, or child bearing.

2.2 Mandatory retirement: historic and recent reforms

In France, retirement decisions may interact with many labor market mechanisms that can influence workers and firms' behavior. In the 1980s-1990s, successive governments have implemented reforms providing incentives for early withdrawal of older workers from the labor force. The driving idea was to make room for the unemployed and younger workers entering the labor force: extension of early retirements (before the minimum age of 60), extension of unemployment benefits for older people (longer duration, less counterparts in terms of job search), and the one we focus on in this paper, simplified procedures to lay-off older workers (*mise à la retraite d'office*, *i.e* mandatory retirement).

In France, typical long-term contracts can only be terminated under specific circumstances (economic redundancies or dismissal for professional faults), otherwise the employer must pay a high dismissal compensation. Such compensations were not required when separations occur at the mandatory retirement age. Senior workers indeed used to receive a special treatment: a reform in 1987[3] allowed firms to lay-off senior workers without any justification after 60, as soon as they reached the full rate age, either under the age or the duration condition (see previous subsection).

From the early 2000s, unbalances in the pension system made it necessary to maintain old-aged workers in the labor force. Most schemes providing incentives to retire as early as possible were progressively removed. In that vein, mandatory retirement was soon restricted with the 2003 reform.[4] It set the minimum age for compulsory retirement at 65 instead of the previous double condition of age (60) and full rate. However some industry-wide derogatory agreements could be implemented, if signed before 2008, January 1st.[5] Some

[3]Law 87-588, July 30th,1987

[4]Law 2003-775, August 21st, 2003, article 16th

[5]Note that those derogatory agreements were not mentioned in the first draft of the law, suggesting a resistance to this change, possibly from industrial lobbies.

industrial branches were allowed to keep on the previous scheme, if they signed a collective agreement including some rather formal employment-related compensations (e.g hiring one new worker for two compulsory retirements). Facing a wave of derogatory agreements, the legislator forbade any new signature in 2006[6], and derogatory schemes were scheduled to be closed by January 2010. A last reform in 2010 increased further the minimum age for compulsory retirement from 65 to 70. Table 1 summarizes the evolution of the legislation of MR, by date, age group and work duration. These are the reform-induced variations that will be used as source of identification of the effect of MR over labor force participation in this paper.

Table 1: Evolution of MR legislation

	Age < 60	$60 \leq$ Age < 65		Age ≥ 65
		$D < D_{\text{FR}}$	$D \geq D_{\text{FR}}$	
Before 2003	✗	✗	✓	✓
From 2003 to 2010				
with DA	✗	✗	✓	✓
without DA	✗	✗	✗	✓
From January 2010	✗	✗	✗	✗

Note: ✓= MR possible, ✗= MR impossible
DA=derogatory agreement, D=work duration, D_{ref}= full rate work duration

Note that the evolution of the legislation on mandatory retirement is contemporaneous with other important reforms of the pension system. In particular two reforms directly interact with the 2003 reform of MR. The first one in the implementation of a bonus for working beyond NRA: before the 2003 reform, there was no increase in the pension conversion rate once the individual had reached the required duration, D_{FR}. The reform introduced a close to actuarially-fair adjustment, from January 1st 2004. This change goes in the same direction as the repeal of MR at full rate before 65: it should increase the probability to work beyond that point, as shown in Benallah (2011). In the absence of derogatory agreements it would have been impossible to disentangle between the labor-demand (end of compulsory retirement) and labor supply (financial incentives) effects. Derogatory agreements make it possible to disentangle between the two reforms. But this source of identification comes at the cost of a potential bias since derogatory agreements are likely to be endogenous (see discussion in section 4). Another important element of the 2003 reform is the implementation of early retirements before the minimum retirement age (60 at the time). Those early retirements were only available for workers who have worked for a very long time and have started to work very early. This reform interacts with the one we focus on for different reasons. Some firms (illegally) used these early retirements to extend compulsory retirement to individuals under 60 satisfying the conditions, with potential effects on exit rate before 60. It can also

[6]Law 2006-1640, December 21st, 2006

induce a selection bias on the population of interest (aged 60 to 64) since individuals still employed at 60 even though they could have retired earlier may be specific. This potential bias will be dealt with in section 6, where we estimate our models on a sub-population that is not affected by the early retirement reform.

3 Data and descriptives

3.1 The French Social Security administrative databases

To examine the impact of mandatory retirement on labor force participation, we use administrative data from the general scheme of wage earners of the private sector (*Caisse nationale d'assurance vieillesse, Cnav*).

We use two distinct administrative databases. The first one provides detailed information on labor market trajectories for both workers and retirees. The second one focuses on pensioners, is much bigger and includes more recent flows of retirees. The former will be used to estimate the effect of the 2003 reform of mandatory retirement, the latter will be used to study more precisely the contribution of MR to the amount of bunching at full rate (see section 4).

The *Cnav* 1/20$^{\text{th}}$ sample (thereafter sample 1): The sample is a random draw of $1/20^{th}$ of the population of the general scheme (both workers and retires), based on individual Social Security number. The sample contains information on work history (from 1946 to 2012), and pension rights when the individual is retired. In the initial sample there are about 2 billion observations (on average 50,000 by generation), among which 75% of workers and 25% of retirees. As it is an administrative dataset, we have only few demographic variables: date of birth, birth location (France or foreign), and gender.

On the other hand, labor market outcomes are quite detailed, with information coming from different sources: DADS[7] and DNT[8] for employment and corresponding wage, Caf[9] for child-bearing periods, *Pôle Emploi* for unemployment spells, and Cnam[10] for sick leaves. The data contains, for each individual and for each year, the number of trimesters validated for pension computation, for each type of validation (work, unemployment, child-bearing, sick leave). A wage is recorded for relevant occupation. The data also contain several firm-specific variables: geographical location, industry affiliation (*naf* code) and firm identifiers. Unfortunately, those individuals-firms linked information are only available from 2000 onward.

This rich data, however, suffers some important limitations. Firstly, the yearly step used

[7] *Déclaration annuelle des données sociales*, yearly employer-provided administrative declaration about number of employees and wages

[8] *Déclarations Nominatives Trimestrielles*

[9] *Caisse des allocations familiales*

[10] *Caisse nationale de l'assurance maladie*

in the data is not precise enough to have a true calendar of the employment trajectory. Secondly, as the main objective of the data is to compute pension benefits of the affiliates of the general scheme, information that are not directly relevant for this regard may not be available. For example, the amount of pension rights accumulated in other schemes is not available. Last but not least, the career history out of the general scheme is not well reported before retirement (the moment where the general scheme needs to have complete information in order to compute the benefits). The number of children and some periods worked in other pension schemes, that both contributed to insurance duration in the private sector, are not always reported before claiming. This implies that, before retirement, we are not able to compute precisely the insurance duration validated at each point of the time. This constrains the empirical strategy since the rules for mandatory retirement depend on this insurance duration: as explained in the previous section, before 2003 in the general case and until 2010 with derogatory agreements, MR before 65 is possible as soon as the reference duration D_{FR} is reached (see section 4).

The exhaustive flows of retirees (thereafter sample 2): This alternative sample comes from the records on pensioners[11], it registers all retirements that have taken place from 2004. The main advantage of this alternative database is its size: the exhaustive flows of retirements from 2004 to 2014. For those individuals we have the same information as in the main sample. We use this sample to focus on a specific cohort for which we can reconstitute the whole universe of retirement (individuals born in 1946). This dataset is used to have a more thorough analysis on the effect of MR on retirement at full rate.

Samples selection: Since we do not have detailed information about work history outside the general scheme, we focus on individuals who have at last one period reported as worked in this scheme. As we only observe information on employers from year 2000 on, we will focus on recent generations, the first one being generation 1940 (reaching age 60 in year 2000). Finally, since we focus on employment rates from age 60 and above, the initial sample is further restricted to individuals who are still employed in the *régime général* the year they reach 60. It is a strong restriction, especially in France where a large part of the population has already withdrawn for the labor force when reaching this age.[12] Table 2 presents the proportion of the sample we remove at each step of our sample selection.

3.2 Imputation of the treatment variable:

As developed in the next section, our main identifying variable is the extension of mandatory retirement after 2003, through the signature of collective industry-based derogatory

[11] SNSP, *Système National Statistiques Prestataires*

[12] According to Brossard 2008, around 35% of the affiliates of the *régime général* is still employed at 60 (all occupations considered).

Table 2: Samples selections

	Sample 1: 1/20th sample		
Filters	Nb of individuals	% of initial sample	% of previous step
Initial sample	5,239,448	100.0	100.0
Cohort (dob$\geq$ 1940)	782,439	1.5	1.5
Scheme (at least one report)	699,313	1.3	89.4
Activity (age withdraw LF$\geq$60)	167,876	0.3	24
	Sample 2: Exhaustive flow of retirees		
Filters	Nb of individuals	% of initial sample	% of previous step
Initial sample	7,289,554	100.0	100.0
Cohort (dob=1946)	720,894	9.9	9.9
Activity (age withdraw LF$\geq$60)	202,992	2.8	28.1
Type of benefits (no disability)	185,008	2.5	91.1

Source: $1/20^{th}$ sample (sample 1) and exhaustive flows of retirees (sample 2).

agreements. We define as treated individuals working in a firm in which a derogatory agreement applies.

The identification of the effect of mandatory retirement then requires to know if individual i working in firm j at time t can be forced to retire through mandatory retirement ($MR_{i,j,t}$=1). As previously mentioned, this is always the case before 2003 and not possible anymore from 2010. In-between, we need to know whereas the worker is concerned by a derogatory agreement. This requires two things: (i) gathering the list of derogatory agreements signed between 2003 and 2006 and their date of implementation and (ii) identifying which collective agreement is relevant for a worker i in t.

Regarding the first point, so far we have been able to recover 61 derogatory agreements, on a total of 91 according to administrative sources (Bur, 2007). The list of agreements, with the associated industry, the date of signature and the date of implementation is reported in Appendix A.

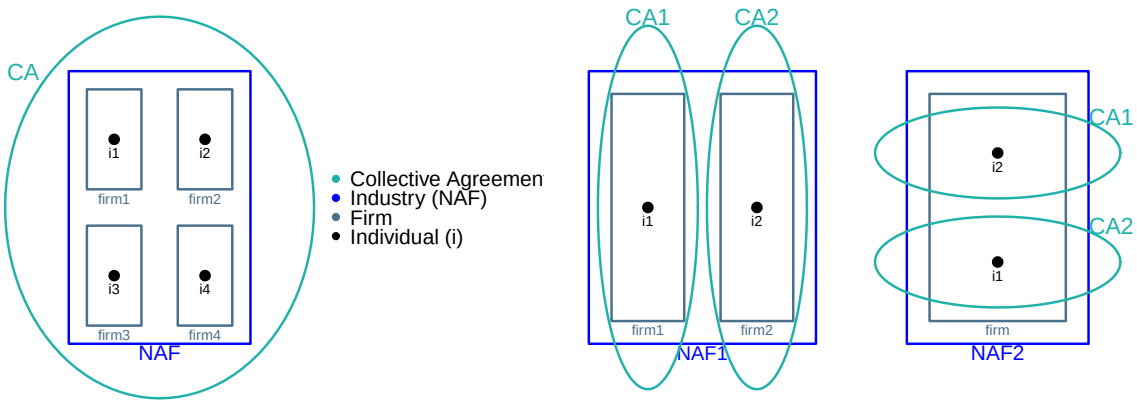
Regarding the correspondence between a collective agreement and an individual, we unfortunately do not observe directly the relevant collective agreement (CA)tat a given worker belongs to, and we have to impute it. The main employer related variables available are the employer administrative number and the industry code at the most disaggregated level (*naf* code, *Nomenclature d'activité française*). The relevant CA (and the attached derogatory agreements) are imputed on the basis of a table of correspondence between the industry code and the collective agreement. It gives, for a given year,[13] the percentage of workers attached to the different collective agreements for all existing industry code[14]. As presented at figure

[13]Years 2009 and 2012 are available. Tables are constructed and provided by the French Ministry of Labor (http://travail-emploi.gouv.fr/conventions-collectives,675/table-de-passage-entre-secteur-d,14612.html

[14]Information is missing for a non-negligible number of industries (around 20%). This is the case when an industry is not covered by collective agreement or when it is subject to statistical confidentiality

2, industry and collective agreements do not exactly match, as the latter is defined according to the type of job, hence different workers within a same firm can be covered by different agreements.

Figure 2: Matching industry and collective agreement



(a) The good case

(b) The bad cases

The following imputation rule is applied: we sum the proportion of individual from each collective agreement and we consider that an industry is in the treatment group if at least x% (e.g 50) of its workers is covered by a collective agreement in which a derogatory agreement has been signed. Conversely, we consider as non-treated only industries in which at least x% of the workers belongs to a collective agreement in which no derogatory agreement was signed. For some industries (around one fifth), we do not manage to impute the treatment variable (if the correspondence table does not provide information for this given industry, or if the imputation thresholds are not reached).

As a result, our main explanatory variable is imprecisely measured: some individuals will be considered as treated at a given date when they are not, and *vice versa*. The quality of the imputation can be assessed using the latest version of the *Echantillon inter-régime des retraités*[15] (EIR 2012)that includes direct information on workers' collective agreement from year 2005. We apply our matching methods to the EIR sample and we can compare the true collective agreement to the imputed one, and then compute to proportion of errors of type I (MR wrongly set to 0) and type II (MR wrongly set to 1). Doing so, we find that we wrongly classify only 10% of the individuals (see table 9 of Appendix A).

This measurement errors at the level of the main explanatory variable is a concern only if it induces a bias in our estimation. As a robustness check (see table 10 of Appendix A), we

[15] A panel of retirees with administrative career record in most existing pension schemes. See Mahieu and Blanchet (2004) for a detailed presentation of the data.

test the sensitivity of our estimations to different thresholds used for the attribution of the collective agreement, and to alternative grouping methods.

A related source of concern lies in the selection our matching can induce. The imputation process indeed creates three distinct groups of employees. Some work in a firm that is assigned as treated (in which a derogatory agreement has been signed), some other in a firm assigned as non-treated (no derogatory agreement imputed for this industry) and some for which we cannot impute a main collective agreement, that are dropped.[16] Tables 3 and 4 present some descriptive statistics for the three groups, for the two samples used in the empirical analysis. For the population of workers and retirees (sample 1, table 3), the treated and control groups are of equal size, and amount to around two thirds of the initial sample. They differ significantly, however, in their composition: treated individuals are more often men (30% vs. 46%), have started to work younger, and have validated more trimesters for retirement when they reach 60 (around 2 years and a half). The mean wage at 60 is also much higher. Those differences stem from the fact that derogatory agreements were not signed randomly among industries. Two main industries, *Manufacturing* and *Construction*, represent half of the treated group and only 10% of the control group. Conversely the control group is over represented among trade, transportation or food industries. Finally, the main industries that we are not able to match are semi-public occupations (*Public administration* and *Health, and social work*), due to the fact that information on collective agreement for the corresponding employers is not publicly provided.

The fact that the treated and control groups are different was expected, and this is not crucial for our main identification strategies which relies on a differences-in-differences approach. We discuss the potential violations of the common trend assumption in the next section. The specificity of the unmatched sample is a concern only for external validity. Descriptive statistics for the sample of retirees (sample 2, table 4) are similar,[17] and can be enriched with the comparison on retirement outcomes. Individuals working in firms for which we imputed a derogatory agreement (treated group) retire on average younger and with a higher pension. This stems from the fact that they have higher wage and insurance duration, both variables that are positively related with the level of benefits (see section 2). They are also less likely to retire before the full rate (with a penalty), and more likely to retire with a *surcote*. This last point seems at odds with what was expected: it should be more difficult to work beyond the full rate if firms where mandatory retirement is possible. On the other hand, the sooner you reach the full rate, the easier it is to exceed it. It is all the more true since, in most cases, derogatory agreements provide a notice period of three to six months for mandatory retirement, so that there can be a gap between the decision and the effective layoff during which the workers can validate trimesters of *surcote*. This mechanically increases

[16] An alternative solution would be to include them in the control group. See discussion and tests in Appendix A.

[17] Statistics on industry are similar for sample 1 and sample 2 and are not presented for the latter.

the proportion of *surcote* among workers of the treated group. This reading of the data is confirmed by the fact that the average number of trimesters of surcote is lower for the treated group, as is the proportion of workers with at least one year of work duration beyond the full rate.

Table 3: Descriptive statistics for the sample of affiliates

	Treated Group	**Control Group**	**Unmatched Group**
Sample size			
Nb obs	88,320	71,956	145,922
% of initial nb obs	0.29	0.23	0.48
Nb indiv	47,013	34,749	67,352
% of initial nb indiv	0.33	0.24	0.47
Demographics			
Proportion of women	0.30	0.46	0.57
Proportion of natives	0.77	0.76	0.81
Career			
Mean age of	18.9	19.6	19.7
Mean insurance duration at 60 (men)	154	143	141
Mean insurance duration at 60 (women)	156	140	146
Mean wage at 60 (men)	19644	16813	18931
Mean wage at 60 (women)	17232	13601	15579
Industry			
Agriculture	0.00	0.00	0.00
Mining	0.00	0.00	0.00
Manufacturing	0.32	0.12	0.02
Energy	0.00	0.00	0.00
Water supply	0.01	0.01	0.00
Construction	0.23	0.00	0.00
Trade	0.11	0.20	0.09
Transportation and storage	0.01	0.15	0.01
Accommodation and food	0.00	0.15	0.01
Information and communication	0.03	0.01	0.02
Financial and insurance activities	0.09	0.01	0.03
Real estate activities	0.03	0.00	0.03
Technical activities	0.10	0.06	0.06
Administrative service activities	0.06	0.14	0.08
Public administration	0.00	0.01	0.22
Education	0.01	0.04	0.05
Human health and social work	0.00	0.04	0.27
Arts and entertainment	0.00	0.02	0.02
Other service activities	0.00	0.03	0.07
Extraterritorial activities	0.00	0.00	0.00

Source: *Cnav* $1/20^{th}$ sample.

Table 4: Descriptive statistics for sample of retires

	Treated Group	**Control Group**	**Unmatched Group**
Sample size			
Nb obs	95,686	78,322	161,941
% of initial nb obs	0.28	0.23	0.48
Nb indiv	48,447	35,279	69,096
% of initial nb indiv	0.33	0.24	0.47
Demographics			
Proportion of women	0.29	0.46	0.57
Proportion of natives	0.79	0.80	0.84
Career			
Mean age of entry in the labor force	18.5	18.7	19.0
Mean insurance duration at 60 (men)	161	158	155
Mean insurance duration at 60 (women)	162	152	155
Mean annual earnings at 60 (men)	18,464	16,073	17,920
Mean annual earnings at 60 (women)	15,378	12,599	14,248
Retirement			
Mean claiming age	61.5	62.0	62.1
Mean retirement age	61.2	61.6	61.7
Mean yearly benefits	11,703	9,077	9,632
% with *decote*	3.1	4.6	4.9
% with *surcote*	28.6	27.1	30.4
Mean $\text{trim}_{\text{surcote}}$	1.9	2.0	2.3
% with $0 \leq \text{trim}_{\text{surcote}} \leq 4$	14.0	12.0	13.4
% with $\text{trim}_{\text{surcote}} > 4$	14.6	15.1	17.1

Source: Exhaustive flows of retirees of the *Cnav*.

4 Identification strategies

As we want to estimate the effect of mandatory retirement (MR), the treatment will be defined as the possibility for the employer to dismiss workers before 65, which can only be done if the pre-2003 rules of MR have been extended with a derogatory agreement.

As summarized in table 1 of section 2.2, the treatment variable MR varies along four dimensions: time, age, industry, and work duration. Identification of the effect of MR will then rely on the comparison of the outcome variable for different groups.

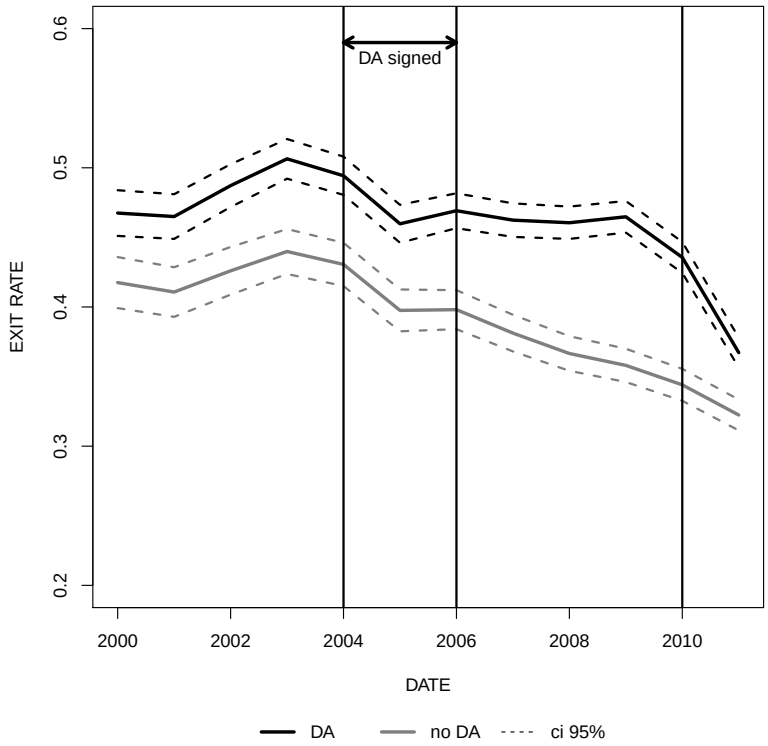
The most straightforward dimension one could think about is age: for example the 2003 reform only impacted workers aged 60 to 64, so that we could use as control group younger (say 55-59) or older (65-69) workers. In this case, however age may not be a proper grouping variables, for at least two reasons. The first one is that, as shown in figure 1, employment rates for different groups were quite different over the period, so that the common trend assumption is not likely to hold. Secondly, the 60 and 65 age thresholds are keys ages of the French pension system (early and normal retirement ages), so that comparing individuals from both sides of the 60-64 window may be misleading. Overall the age dimension may not be a relevant one in this framework, as underlined in Shannon and Grierson (2004). The other dimensions (time, industry, work duration) will be used in two different settings.

4.1 The effect of MR on labor force participation: a diff-in-diff analysis

The main reform of interest is the 2003 pension reform that banned mandatory retirement before the age of 65 in the general case. The expected effect of the reform is an increase in the labor force participation for individuals aged 60-64, as some workers that would have been forced to retire under MR can keep on working beyond the full rate work duration. A simple before/after analysis is not possible in this case, since the 2003 also implemented a pension bonus for continued work beyond the full rate (the *surcote*), that did not exist before and has an effect of same direction as banning MR. Identification then takes advantage of the derogatory agreements that extended the old scheme in some industries. This suggests a classic difference-in-difference framework, the treated group being the industries that signed a derogatory agreement, and the control group those who did not. Figure 3 presents graphical evidence of the variations the identification relies on. Figure 3a shows the evolution of the exit rate from employment (the probability to leave your current employment from one year to the other) for the 2000-2011 period. It appears that, starting from a relatively parallel trajectory, the two curves clearly diverge from the mid 2000s, when derogatory agreements are signed. Exit rates decrease by around 10pp when there is no derogatory agreement any more, and remain stable when mandatory retirement is maintained. It suggests that mandatory retirement has prevented the increase in senior worker labor force participation (stemming for example from the implementation of the *surcote*).

Figure 3: Exit rate from employment by years (between 60 and 64)

(a) Treatment (derogatory agreement) vs. Control (no derogatory agreement)



(b) Treatment group by year of agreement

In 2010, when MR before 65 is not possible anymore, the exit rate rapidly declined in the previously treated group, and converge towards the control group level. This rapid decline after MR was banned makes a strong case for a causal impact of MR over labor market participation of older workers. Figure 3b provides additional support for this causal link with the breakdown of the treated group by year of the implementation of the derogatory agreement. Exit rates increase (or stop decreasing for the 2005 group) from the year the agreement is signed. It suggests that the observed global trends are not driven by other contemporaneous changes, for example in the macroeconomic context.

Arguably, our "treatment" does not match the usual standard of a natural experiment, since it is possible that the ratification of a derogatory agreement is correlated with other industry-level determinants of labor force participation of older workers. For example we could think that agreements would be signed in more constrained industries, that would have a stronger turnover even in the absence of MR. We could also imagine a positive correlation between MR and some determinants of labor supply, for example preference for leisure[18]. This could imply that the decline of exit rate we observe in the non-treated group, which is partly driven by the contemporaneous implementation of the *surcote*, would not have occurred to the same extent in the treated group. The patterns exposed in figure 3, and particularly the sharp decline observed in the treated group when MR in removed, however suggests that MR legislation is the main driver of the observed divergence between 2005 and 2009. We will thus consider that potential biases arising from the endogenetiy of the treatment are of second order of importance.

We adopt a classic difference-in-difference specification, with time and group fixed effects and a dummy for the treatment.

$$Y_{i,j,t} = \alpha + \lambda_t + \mu_j + \delta \mathrm{MR}_{j,t} + \beta_X X_{i,j,t} + \epsilon_{i,j,t} \tag{1}$$

with :

$$\begin{cases} Y_{i,j,t} : \text{Labor market outcome} \\ \lambda_t : \text{Time dummy} \\ \mu_j : \text{Industry dummy} \\ \mathrm{MR}_{j,t} : \text{MR dummy} \\ X_{i,j,t} : \text{Controls} \end{cases}$$

Three different dependent variables $Y_{i,j,t}$ could be considered. The most usual ones in the retirement literature are retirement (definitive withdrawal from the labor force) or benefits claiming. They may not be the most relevant for this specification for both theoretical and practical reasons. Formally, mandatory retirement does not correspond to retirement but to a layoff. It implies that a worker can theoretically find another job after he was forced to retire. So MR can occurs before benefits claiming and even withdrawal from labor force.[19] It is all

[18] The fact that, in many cases, important workers' unions also signed the agreement goes in that direction.

[19] In France, the average duration between exit from the labor force and benefit claiming is 2 years and 9 month for the 1944 generation, according to Mette (2013).

the more problematic since we observe recent generations that are not entirely retired in our dataset, so that if we observe a worker loosing her job in the most recent years of observation we cannot tell if she has withdrawn from the labor force on not. We thus fall back on a more general labor market outcome: job exit. $Y_{i,j,t}$ is equal to one if the individual i works in firm j at time t but not anymore at date $t+1$ (with either a transition to another firm or not).

4.2 Full rate analysis

Mandatory retirement before 65 is only possible when the worker has reached the full rate, that is when she has validated at least the required number of trimesters, D_{FR}. This provides another potential source of identification of the effect of MR: only individuals with the required duration D_{FR} can be impacted by the 2003 reform. Ideally, this could be introduced in the previous specification, adding a dummy variable interacted with the treatment variables in a triple differences framework. Unfortunately, it is impossible to locate precisely the full rate in our main sample. As explained in section 3, we do not have an exhaustive career report before pension claiming, so that we are unable to know exactly how many trimesters a given worker has validated at a given age. Hence before retirement, we cannot know for sure if an individual is working beyond her full rate or not. One possibility could be to restrict our sample to a sub-population for which the full rate age can be precisely identified, for example men (who do not get insurance duration expansion for children) who have spent their whole career in the *régime général*. This will be done as a sensitivity analysis of the previous specification. The other solution, that is adopted here, is to focus on a population of retirees, for which we have a complete career record.

We focus of individuals of cohort 1946, who reach 60 in 2006, when most derogatory agreements have been implemented, and are already 64 in 2010 when MR before age 65 is banned for everyone. Using only one cohort, we loose the identifying time variations introduced by the reforms. This approach, however, offers some compensatory features. First, as we work on retirees, we can take advantage of the sample size of the exhaustive dataset of retirees (see section 3) and estimate demanding specifications, for example with firms' fixed effects. Secondly, as we have information on the whole career, we can include many controls on careers and financial incentives.

With $Y_{i,t}$ a labor force participation indicator and $MR_{i,t}$ equals to one if the individuals works in a firm allowed to lay-off before 65 (i.e equals for firms with derogatory agreement), we want to estimate the following model :

$$Y_{i,t} = \beta_1 MR_{i,t} + \beta_2 FR_{i,t} + \beta_3 (MR_{i,t} * FR_{i,t}) + \text{controls} + u_{i,t} \tag{2}$$

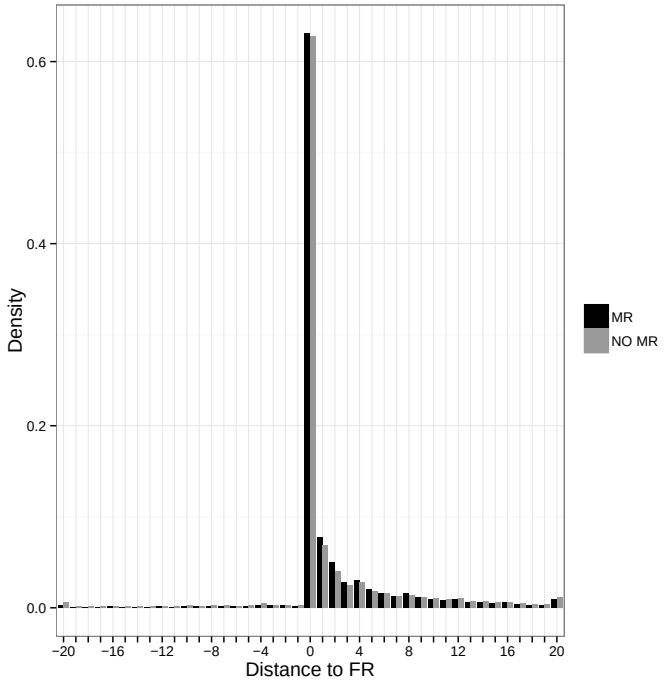
The expected effect of the agreement is to increase the probability of retirement at the full rate, so that we expect a positive sign for β_3.

The estimation relies on the following identifying assumption: once controlled for covariates, the increase in the probability of leaving employment when the full rate is reached is only due to the possibility given to the employer to induce retirement. This is only convincing if we can add a rich set of controls, which is arguably the case with our data.

Note that there a two possible definitions for the full rate variable: it can correspond to the exact date at which full rate is reached ($FR_{i,t} = 1$ at this date, and is null before and after), or to the state of having reached the full rate ($FR_{i,t} = 1$ for all years equal or subsequent to the exact date). Theoretically, the first definition is the most interesting as MR should has an effect at this exact point. Firms' behaviors, on the other hand, may be based on the second definition since they can react to the approaching repeal of MR (in 2003 and 2009) or its re-implementation (after a derogatory agreement) by dismissing people at the full rate, including those beyond the full rate. We present results only for the second definition as they are similar to results using the first one. As we observe work trajectories until the advanced age of 68, we can use withdrawal from the labor force as alternative dependent variable in addition to job exit.

Figure 4 presents the distributions of the distance to full rate for retirees of cohort 1946, with and without MR, the year they withdrew from labor force. Both distributions present a large spike at the exact location of the full rate (at the point where the distance to full rate is equal to zero). This bunching at the full rate was documented in previous work on French data (for example Blanchet and Pele, 1999). Apparently, bunching at full rate is not more pronounced when MR is possible, with around 60% of retirement exactly at the full rate in both groups. But as commented for table 4, the two groups differ with respect to the main variables determining the access to the full rate (age structure and work duration), so that raw statistics are of little relevance here. The main goal of our econometric specification is to measure the part of the spike that can be attributed to employers' decisions through mandatory retirement.

Figure 4: Distribution of distance to full rate



5 Empirical results

5.1 Effect of the 2003 reform

5.1.1 Main results

The first two columns of table 5 presents the results of the estimation of equation 1 by OLS. As standard in the recent literature, since suggested by Bertrand *et al.* (2004), we use clustered standard-errors at the level of the grouping variable, *i.e* the industry code. The last two columns provide insights on the dynamics of the effect, by enriching the initial model with leads and lags dummies around the treatment date, as in Autor (2003). It is a way to check that there is no reverse causality between the outcome variable and the treatment (if coefficients for the lags are not significant). It also gives a sense of the timing of the effect. Columns (2) and (4) add to the initial models a set socio-demographic control variables (age dummies, gender, country of birth, age when entering the labor force). Reassuringly, including controls for covariates that differ systematically between the treatment and the control group does not change much the point estimates.

In the main specification (column 2 of table 5), mandatory retirement is estimated to increase the yearly exit rate from employment of workers aged 60-64 by 2.2 percentage points. Compared to a baseline of 37% yearly job exit rate in the control group before the reform, it implies that workers of a firm allowing mandatory retirement are 6% more likely to exit employment compared to workers who cannot be forced to retire before 65. The magnitude of the effect seems low but it may not be that negligible. If we consider that (i) every exit is permanent, (ii) that there is not adverse effect of banning MR on hiring, (iii) that there are around 750,000 workers in the private sector aged 60-64,[20], and (iv) that MR affects of 25% them (as estimated in our sample), then a 6% jump in the exit rate with MR corresponds to around 12,000 employment terminations every year between 2005 and 2010. The magnitude of the effect is close to the one found in Adams (2004).[21]

It is not possible to quantify, however, how much MR contributed to the decrease in labor force participation observed in the 1980-1990s. We indeed measure the effect of mandatory retirement when workers have some incentives to work beyond the full rate, as it is the case from 2004 on. The positive effect we find suggests that at least some workers would have liked to work and benefit from the *surcote*. This does not mean that before the 2003 reform, MR had an impact. One could argue that without any incentive to work beyond the full rate the effect of mandatory retirement was only marginal. In any case, it is not possible to identify a specific effect of MR before 2003 since workers and firms incentives have been aligned for a long time.

[20] From a total of 915,000 workers in the 60-64 age group (Source: Insee).

[21] An estimated increase of 2.75pp in employment for the concerned population (age 50 and above), corresponding to a 4.45% increase compared to the baseline rate.

This analysis is confirmed by the timing of the effect (columns 3 and 4 of table 5) : the effect is much stronger from the third year after the signature of the derogatory agreement. The removal of the labor demand constraint does not have a big instantaneous effect (insignificant for the year of the signature), suggesting that it was not strongly binding on workers' choices. Time passing, labor supply adapts to the changes in financial incentives: workers are more and more willing to work beyond their full rate. Firms and workers preferences are then more likely to be antagonistic, and mandatory retirement starts to play an important role.

Table 5: Effect of extented mandatory retirement: main results

	Y = exit from employment			
	(1)	(2)	(3)	(4)
Outcomes				
After DA	0.022*	0.022**		
	(0.012)	(0.010)		
DA_{t-2}			0.003	−0.004
			(0.012)	(0.011)
DA_{t-1}			−0.002	−0.005
			(0.014)	(0.013)
DA_t			0.007	0.005
			(0.012)	(0.011)
DA_{t+1}			0.023*	0.020*
			(0.014)	(0.012)
DA_{t+2}			0.021	0.016
			(0.015)	(0.014)
$DA_{\geq t+3}$			0.036**	0.033***
			(0.015)	(0.013)
R^2	0.046	0.116	0.047	0.116
Nb. obs.	88506	88506	88506	88506
Nb. ind.	49993	49993	49993	49993
Controls	No	Yes	No	Yes
Industry dummy	Yes	Yes	Yes	Yes
Year dummy	Yes	Yes	Yes	Yes

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Note: Controls include age dummies, gender dummy, age of entrance in the labor market, and a dummy for being born in France. Standard errors are clustered at the industry level.

Source: *Cnav* $1/20^{th}$ sample.

5.1.2 Sensitivity analysis

In this subsection, the main model is estimated on different sub-populations, to test the robustness of the results and to test for potential heterogeneity in the effect of mandatory retirement. The focus is put on three different types of sub-groups.

Even if we are not able to precisely identify individuals who have reached the full rate with this sample (see section 3 for details and next section for the focus on the full rate), it is still possible to use the information we have to look for heterogeneous effects. Recall that since our problem is that some periods are not identified in career records (some periods worked in other schemes and insurance bonuses for child-bearing), the insurance duration we measure is a lower bound. On the other hand, it implies that we are able to identify some workers who reach the full rate before 65 for sure. Conversely, individuals with very low insurance duration may not be able to reach the full rate before 65. We then split the sample into the following groups: men (to avoid the children related insurance bonus) who already have the targeted duration when they reach the minimum retirement age ($D_{60} \geq D_{FR}$) and those who are much further from D_{FR} ($D_{60} \leq D_{FR} - 20$ trimesters). Since our treatment (mandatory retirement at full rate before 65) is more likely to hinge on the first group than on the second one, the effect is expected to be higher for the former.

We secondly differentiate by earnings, splitting the sample between above and below the median wage at 60 and estimating the model separately for the two populations. The rational is the following. On one hand, the higher the earnings, the stronger is the incentive to keep on working, through both the forgone earnings in case of retirement and the bonus (*surcote*), which is to some extent proportional to the level of earnings. Overall high earnings are likely to be positively correlated with a strong willingness to work on the employees' side. On the other hand, firms may want to get rid of high wage workers in priority, as they put more strain on their wage bill.[22] The effect of mandatory retirement is then likely to be much stronger for high earnings workers, since they are more willing to work beyond the full rate when they can and firms are more willing to lay them off when it is possible.

Finally, as mentioned in section 2, during the period we cover another important reform of the pension system was implemented, which allowed early retirement before the minimum claiming age (under condition of work duration and early entrance in the labor market). This does not affect directly our population of interest since it impacts employment before age 60. It could however bias our estimation through the selection of workers still in employment at 60, if the treated and the control groups are differentially impacted by the reform. This is likely to be the case since the two groups differ regarding the main variables determining access to early retirement as shown in table 3. The expected direction of the bias is not straightforward though. Workers of the treated group are more likely to be eligible to early

[22] One could think that high earnings workers are also the most productive ones, that firms may want to keep. But it may not be the case with Lazear-type contracts in which wage increases with wage without a direct link with productivity (Lazear, 1979)

retirement (lower average age of first report, higher average insurance duration are 60), so that we can think that more workers of the treated group would exit employment through early retirement. If any, we could then expect a negative bias on our estimation, since early retirement relatively reduced the need for firms of the treated group to dismiss older workers. There is no direct way of controlling for this bias. As a robustness test, we simply run our estimation on a sub-sample of individuals that were not eligible to early retirement (both after and before its implementation). To do so, we keep only those who entered the labor force after 17, who do not qualify for the age of first report criterion of eligibility.

Results for those alternative estimations are reported in table 6. The first column reproduces the second column of table 5, and the other ones present results for the estimation of the same model on different sub-samples of the initial sample. Columns (2) and (3) respectively compare the results for male workers with high and low insurance duration at 60. As expected, the estimated effect is stronger for the first group, but due to limited sample size none of them is significant, and neither is the difference between the two coefficients. In columns (4) and (5), the sample is broken down in two groups of earnings, above and below the median (computed separately in the treated and control groups). Interestingly, it appears that the effect obtained in our main estimation is mostly driven by the upper part of the wage distribution. The estimated coefficient is almost three times bigger for high earnings group compared to low earnings one, and is not significant for the latter. This confirms that mandatory retirement was likely to be particularly used by firms to lay off high wage workers, who were also those with the strongest incentive to delay retirement. Finally, results are robust when we restrict our sample to workers that are not eligible to early retirement scheme (column 6). The point estimate does not change, suggesting that the contemporaneous reform of early retirement does not severally biases our main estimation.

5.2 Results on retirement at full rate

Table 7 presents the results of the estimation of equation 2 by OLS, with two distinct definitions of labor force participation as outcome variable: exit from employment (columns 1 to 3) and retirement (columns 4 to 6). Our main variables of interest are the dummies indicating if the individual has reached the full rate (FR) and if she works in a firm where mandatory retirement is possible (MR), and the interaction between the two. Controls include age dummies, a gender dummy, the age of entrance in the labor market, a dummy for being born in France, and also current yearly wage and yearly benefits at retirement. We also introduce fixed effects at the level of the industry (columns 2 and 5) and of the firm (columns 3 and 6). When we include industry or firm fixed effects, the coefficient on MR is not identified since the MR variable does not vary within a given industry code, all the more so within a firm. When identified, MR increase the baseline exit (or retirement) rate by 5 pp. This confirms what was already shown in figure 3: baseline turnover is more important in firms

Table 6: Effect of extented mandatory retirement: heterogeneity

	Y = exit from employment					
	(1) Ref	(2) High D_{60}	(3) Low D_{60}	(4) High earnings	(5) Low earnings	(6) No early ret.
After DA	0.022**	0.020	0.016	0.039***	0.014	0.023**
	(0.010)	(0.016)	(0.016)	(0.013)	(0.011)	(0.010)
R^2	0.116	0.158	0.086	0.058	0.285	0.100
Nb. obs.	88506	23554	15874	48551	32599	65677
Nb. ind.	49993	17032	7435	22222	22222	35244
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry dummy	Yes	Yes	Yes	Yes	Yes	Yes
Year dummy	Yes	Yes	Yes	Yes	Yes	Yes

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Note: Controls include age dummies, gender dummy, age of entrance in the labor market, and a dummy for being born in France.
Standard errors are clustered at the industry level.

Source: *Cnav* $1/20^{th}$ sample.

where mandatory retirement as extended beyond 2003. The coefficient on the FR variable confirms the visual impression of figure 4: the age of full rate is a strong determinant of labor force exit, with a 33pp increase in the probability of exit rate at this age. Finally, the term of interaction between mandatory retirement and the full rate age is significant and of the expected sign: the effect of reaching the full rate is 2.2pp bigger in firms where mandatory retirement is possible. Which can be reformulated as follows: mandatory retirement explains $2.2/33 \approx 7\%$ of the observed bunching at full rate.

Results are robust to the inclusion of industry or firms fixed effect. The effect is even higher with firm fixed effects, with a 20% increase in the probability to retire at full rate when MR is possible. But this is mainly due to sample selection, since we focus on larger firms (represented by at least 10 individuals of the sample). The firm fixed effect specification comes as a robustness checks, showing that the effect is not driven by the peculiarity of firms selected in the MR group. Within a given firm, exit and retirement rates increase significantly when the worker reaches the full rate age, and this effect is stronger when mandatory retirement is possible in the firm.

We also run the same kind of heterogeneity analysis as in the previous subsection. Results are reported in table 8. The first column reproduced the first column of table 7. The second and third columns estimate the model separately for individuals who already reached the full rate at 60 (column 2) and those who reached it between 60 and 64. Since everybody has the full rate, there is no estimated effect of the FR variable or its interaction with MR for the

former group. The effect is stronger than in the baseline case when the full rate is reached after the minimum claiming age, with a 19% increase in the probability to exit employment at full rate with MR. In columns (4) and (5) the sample is broken down above and below the median wage. As previously observed, the overall effect is mostly driven by higher wages, for which mandatory retirement is more likely to matter. Finally, results are robust to the restriction of the sample to workers that are not eligible to early retirement schemes for long careers (column 6).

Table 7: Full rate analysis: main results

	Y = exit from employment			Y = retirement		
	(1)	(2)	(3)	(4)	(5)	(6)
Constant	0.527***	0.551***	0.620***	0.429***	0.516***	0.632***
	(0.015)	(0.014)	(0.054)	(0.015)	(0.013)	(0.051)
MR	0.049***			0.044***		
	(0.012)			(0.012)		
FR	0.324***	0.324***	0.344***	0.357***	0.356***	0.362***
	(0.009)	(0.007)	(0.014)	(0.008)	(0.006)	(0.017)
MR.FR	0.022**	0.027***	0.070***	0.014	0.017**	0.061***
	(0.011)	(0.010)	(0.020)	(0.010)	(0.008)	(0.020)
R^2	0.350	0.361	0.524	0.413	0.427	0.568
Nb. obs.	164589	164589	27562	164589	164589	27562
Nb. ind.	81819	81819	13677	81819	81819	13677
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	None	Industry	Firms	None	Industry	Firms

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Note: Controls include age dummies, gender dummy, age of entrance in the labor market, and a dummy for being born in France, yearly benefits.
Standard errors are clustered at the industry level.

Source: Exhaustive flow of retirees of the *Cnav*.

Table 8: Full rate analysis: heterogeneity

Y = exit from employment						
	(1) Ref	(2) High D_{60}	(3) Low D_{60}	(4) High earnings	(5) Low earnings	(6) No early ret.
Constant	0.527***	0.846***	0.541***	0.541***	0.498***	0.551***
	(0.015)	(0.023)	(0.018)	(0.019)	(0.019)	(0.017)
MR	0.049***	0.063***	0.023**	0.045***	0.043**	0.047***
	(0.012)	(0.009)	(0.011)	(0.012)	(0.017)	(0.011)
FR	0.324***		0.322***	0.305***	0.335***	0.337***
	(0.009)		(0.008)	(0.009)	(0.013)	(0.009)
MR.FR	0.022**		0.061***	0.033***	0.007	0.022*
	(0.011)		(0.013)	(0.012)	(0.015)	(0.011)
R^2	0.350	0.330	0.304	0.221	0.466	0.352
Nb. obs.	164589	77167	51489	100999	61839	122108
Nb. ind.	81819	50588	19922	38630	38630	58225
Controls	Yes	Yes	Yes	Yes	Yes	Yes
Fixed effects	None	None	None	None	None	None

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Note: Controls include age dummies, gender dummy, age of entrance in the labor market, a dummy for being born in France, and yearly benefits.
Standard errors are clustered at the industry level.

Source: Exhaustive flow of retirees of the *Cnav*.

6 Conclusion

The 2003 reform of the French pension system fostered labor force participation of senior through both labor demand and labor supply channels: it increased financial incentives to work beyond the full rate age, and forbade mandatory retirement at full rate before 65. Absent the possibility given to firms to keep on the old mandatory retirement scheme through derogatory agreements, we would not be able to distinguish between the two effects. This paper relies on the industry-level variations induced by the derogatory agreements to isolate the effect of mandatory retirement: employment for the 60-64 age group increased less in firms where mandatory retirement remained possible. This can be interpreted as first evidence that at least some of the increase in the labor force participation of senior workers is driven by the demand-side of labor market. Interestingly enough, the effect is mainly driven by high earnings workers, who are often more willing to work longer but also the main potential targets when firms aim at reducing their wage bill.

Overall, this paper shows that demand and supply sides of labor force participation go hand in hand and benefit from being studied altogether. Increasing financial incentive to pursuing work can be a relevant tool for raising the employment rate of senior workers, if and only if constraints from the demand side are alleviated at the same time.

The overall estimated effect of mandatory retirement is moderate (a 6% increase in job exit in the presence of mandatory retirement), and admittedly explains only a small part of the fast increase in employment rate of senior workers observed in the last few years. This does not necessarily mean, however, that labor demand is only a minor determinant of labor force participation of senior worker, since mandatory retirement is only one part of the story. Firms may influence labor force participation through other channels, for example hiring or discouragement of older workers. In this regard, the potential adverse effect of increasing employment protection on hiring of workers (in particular senior) is an important question. This dimensions was set aside in this paper and offers interesting prospects for future research.

The second main contribution to this paper is to exhibit a previously unseen determinant of bunching in retirement behavior at the full rate age, a common feature observed in many countries. Mandatory retirement coincided with the full rate age for a long time, and is estimated to explain 7% of the observed bunching at this point. The repeal of mandatory retirement might thus be accompanied by a reduction of the size of bunching for most recent generations. We will be able to assess it in a few years when those generations will be entirely retired. The extent to which this channel contributes to shaping retirement behavior in other countries, where mandatory retirement is still effective or was recently removed, remains to be assessed.

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Appendices

A. Alternative imputation methods for the MR variable

As described in subsection 3.2, the extension after 2003 of mandatory retirement before 65, which is our main treatment variable, is not directly observed and must be imputed from the data. In this appendix, alternative methods of imputation are implemented. For each method, we provide results on the trade-off between measurement error and selection. We use the *EIR* dataset to compute the type I (MR wrongly set to 0) and type II (MR wrongly set to 1) imputation errors, that can be balanced against the implied sample selection. The preferred specification (column (2) of table 5.1) is then estimated for each method to assess the sensitivity of our estimation to the imputation process.

Alternative methods: The goal is to impute for each every industry code (*code naf*) a corresponding collective agreement (CA), using a correspondence table between industry and CA. The table gives, for each industry, the percentage of workers attached to the different collective agreements for most existing industry codes. Information is however missing for about 20% of industry codes, due to statistical confidentiality (for public related industries or sensitive ones such as oil extraction) or when only a only a negligible fraction of workers is linked to a CA (in agricultural industry for example).

Two main approaches are considered for imputing the treatment variable MR, using the corresponding able between industries and collective agreements:

M1: For every industry, the CA representing the most workers is imputed as the reference CA, when the CA represents more than x% of the industry's workforce. Different values of the threshold x% are used: 0% (the CA attached is just the maximal pct), 50% and 75%, respectively methods M1a, M1b and M1c). The treatment variable is then directly imputed: the MR dummy is set to 1 from the date of the derogatory agreement, if one has been signed for the imputed CA.

M2: We sum the percentage of each CA within each industry (with and without derogatory agreement) and we consider as treated the ones for which more than x% of the workers belong to a CA with a derogatory agreement. When an industry is treated, we impute as year of implementation of the derogatory agreement the one of the most represented CA. M2a and M2b correspond to the variants with threshold x equals to 50 and 75.

Two possible designs of the control group are considered. A first solution is to adopt the same method M1 and M2 for defining the control group. The second one consists in putting every firm that has not been imputed in the treated group as control. This corresponds to M1bis and M2bis models. The method used in the core of the paper is the model M1b: we keep as treated (resp. control) every industry for which at least 50% of the workforce belong to a CA that signed a DA (resp. that did not sign a DA).

Matching quality: Table 9 presents, for each matching method, the proportion of each group (control and treated), and the share of the whole sample it represents. We also compute, on the basis of the observed CA in the EIR dataset, the percentage of misclassification of the treatment variable. There is a trade-off between the sample size and selection on one hand (we want to keep as many workers from the initial sample), and classification errors on the other hand. The lighter the rules to attribute control and treatment status, the bigger the sample and the higher the chance of misclassification. In the "bis" methods, when everything that is not classified as treated is put in the control group, we keep the whole initial sample but we also make more errors of type I, that is we put in the control group a bigger share of treated workers. When we increase the imputation thresholds (for example from M1.a to M1.c), we decrease sample size (from 83% to 34% of the sample) but also have more balanced groups in terms of size, and less classification errors. The M1.b method, which is chosen in the core of the paper, keeps a fair share of the initial sample (42%) and induces rather small misclassification rates.

Table 9: Testing alternative imputation methods: matching quality

	Ref	M1.a	M1.b	M1.c	M2.a	M2.b	M1bis.a	M1bis.b	M1bis.c	M2bis.a	M2bis.b
Number of obs	223,105	185,933	93,961	75,612	110,058	96,895	223,105	223,105	223,105	223,105	223,105
% initial sample	1.00	0.83	0.42	0.34	0.49	0.43	1.00	1.00	1.00	1.00	1.00
Number of T	53,454	50,045	46,381	37,574	49,231	44,206	50,045	46,381	37,574	49,231	44,206
Share of T	0.24	0.27	0.49	0.50	0.45	0.46	0.22	0.21	0.17	0.22	0.20
Number of NT	169,651	135,888	47,580	38,038	60,827	52,689	173,060	176,724	185,531	173,874	178,899
Share of NT	0.76	0.73	0.51	0.50	0.55	0.54	0.78	0.79	0.83	0.78	0.80
% wrong match	0.00	0.12	0.10	0.09	0.11	0.09	0.10	0.10	0.12	0.09	0.10
% good match	1.00	0.88	0.90	0.91	0.89	0.91	0.90	0.90	0.88	0.91	0.90
% type I error	0.00	0.09	0.04	0.02	0.07	0.04	0.07	0.08	0.12	0.07	0.09
% type II error	0.00	0.18	0.16	0.16	0.17	0.16	0.18	0.16	0.16	0.17	0.16

Note: T: treated, NT: non treated Type I error: T imputed as NT Type II error: NT imputed as T

Sensitivity of the results to the imputation method: In table 10, our main specification is estimated with the alternative matching method presented. We only presents the main coefficient of interest, the effect of the derogatory agreement on exit rates. Reassuringly, the estimated effect does not seem to depend too strongly on the imputation method, at least for methods M1 and M2. The effect are however smaller and sometimes not significant at conventional level (though not far from it) for the "bis" models. We interpret this as follows. Applying the same method for selecting the treated and the control group is a way to have more comparable groups, both in terms of size and composition. Presumably, industries for which we are not able to impute a collective agreement CA are specific. As mentioned above, the industry we are not able to match with a CA are either those for which information is not available (due to statistical confidentiality) or those for which a majority of workers does

not depend on a collective agreement. Applying the same rule for selecting the treated and control groups is then a way to have more comparable groups, with workers of the classic private sector attached to a derogatory agreement. It provides a cleaner identification setting, at the expense of external validity.

Table 10: Testing alternative imputation methods: estimation sensitivity

	M1.a	M1.b	M1.c	M2.a	M2.b	M1bis.a	M1bis.b	M1bis.c	M2bis.a	M2bis.b
After DA	0.016^{*}	0.022^{**}	0.024^{**}	0.017^{***}	0.017^{***}	0.016^{*}	0.015	0.010	0.015^{*}	0.014
	(0.009)	(0.010)	(0.011)	(0.010)	(0.010)	(0.009)	(0.010)	(0.010)	(0.009)	(0.009)
R^2	0.106	0.116	0.110	0.110	0.110	0.106	0.106	0.106	0.106	0.106
Nb. obs.	114487	88506	73246	104509	92982	114487	114487	114487	114487	114487
Nb. ind.	62920	49993	40901	57962	51548	62920	62920	62920	62920	62920

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

B. List of derogatory agreements

Count	CA number	CA name	date of DA signature	date of implementation	date *Journal officiel*
1	18	textile industrie	04/11/03	2003	15/06/04
2	43	importation exportation commission courtage	26/03/2004	2004	01/01/05
3	44	chimie industrie	02/02/04	2004	17/06/04
4	45	caoutchouc industrie	23/02/2004	2004	31/07/04
5	87	carrieres materiaux industrie ouvriers	15/11/04	26/03/05	24/02/05
6	135	carrieres materiaux industrie etam	15/11/04	26/03/05	24/02/05
7	211	carrieres materiaux industrie cadres	15/11/04	26/03/05	24/02/05
8	112	laitiere industrie	15/07/05	03/09/05	08/03/06
9	176	pharmaceutique industrie	19/01/04	2004	17/06/04
10	200	exploitations frigorifiques	10/05/04	2004	16/04/05
11	275	transport a rien personnel au sol	13/04/2005	02/07/05	25/01/06
12	247	habillement industries	02/11/05	07/01/06	05/04/06
13	650	metallurgie ingenieurs et cadres	19/12/03	2004	12/05/04
14	925	papiers cartons distribution commerce gros cadres	20/01/05	07/05/05	17/01/06
15	802	papiers cartons distribution commerce gros oetdam	20/01/05	07/05/05	17/01/06
16	716	cinema distribution employes et ouvriers	28/04/2005	16/07/05	08/03/06
17	892	cinema distribution cadres et agents de ma?trise	28/04/2005	16/07/05	08/03/06
18	731	quincaillerie commerces cadres	28/06/04	2004	28/04/05
19	1383	quincaillerie commerces employes	28/06/04	2004	28/04/05
20	787	experts-comptables et commissaires aux comptes	12/05/2004	2004	06/11/04
21	1000	avocats cabinets personnel salarie	09/07/2004	2004	31/12/04
22	1043	gardiens concierges et employes d'immeubles	24/03/05	18/06/05	29/10/06
23	1044	horlogerie	01/09/04	21/05/05	16/04/05
24	1077	produits du sol engrais negoce et industrie	30/03/04	2004	28/06/05

25	1090	automobile services	18/02/04	2004	19/08/04
26	1170	tuiles et briques industrie	20/12/04	2005	08/03/06
27	1388	petrole industrie	29/03/04	2004	21/07/04
28	1408	combustibles solides liquides gazeux negoce	07/05/2004	2004	11/12/04
29	1412	aeraulique installation entretien reparation	14/06/2004	2004	26/12/04
30	1483	habillement articles textiles commerce de detail	23/11/04	29/10/05	06/10/05
31	1486	bureaux d'etudes techniques	11/09/2003	2004	28/07/04
32	1512	promotion immobiliere	20/09/2004	04/06/05	27/04/05
33	1513	eaux boissons sans alcool production	16/09/05	10/12/05	01/01/06
34	1518	animation	13/01/2004	2004	28/07/04
35	1527	immobilier	25/09/05	29/04/06	05/04/06
36	1555	pharmaceutique produits fabrication commerce	02/05/2005	30/07/05	05/04/06
37	1586	charcutieres industries	14/04/2005	27/08/05	16/03/06
38	1589	mareyeurs-expediteurs	18/03/2005	11/06/05	10/02/06
39	1621	pharmaceutique repartition	03/11/04	27/08/05	19/07/05
40	1672	assurances societes	14/10/2004	13/08/05	13/08/05
41	1679	assurance inspection societes	14/10/2004	13/08/05	13/08/05
42	1850	avocats salaries	05/11/2004	10/09/05	26/07/05
43	1942	textiles artificiels et synthetiques industries	19/12/2003	2004	28/07/04
44	1947	bois d'oeuvre et produits derives negoce	02/11/2005	18/02/06	05/04/06
45	2120	banque	29/03/2005	21/05/05	26/07/05
46	2128	mutualite	29/03/2005	11/06/05	13/10/05
47	2149	dechet activites	09/02/2004	2004	26/11/04
48	2174	navigation interieure marchandises pers sedentaire	10/01/2005	19/03/05	19/10/05
49	2216	commerce detail et gros à predominance alimentaire	09/06/04	2004	25/11/04
50	2264	hospitalisation privee	17/03/2004	2004	02/09/05
51	2272	assainissement et maintenance industrielle	04/10/2004	2005	16/04/05

52	2335	assurances agences generales personnel	28/04/2004	2004	28/07/04
53	2336	foyers services jeunes travailleurs organismes	15/09/2004	2005	08/07/05
54	2420	bâtiment cadres	01/06/2004	2004	31/12/04
55	2409	travaux publics cadres	01/06/2004	2004	31/12/04
56	2567	glaces sorbets cre_mes glace_es industrie	13/09/04	2004	13/04/05
57	2728	sucreries, sucreries-distilleries	14/06/05	13/08/05	13/08/05
58	1436	sucreries, sucreries-distilleries	14/06/05	13/08/05	13/08/05
59	3109	regroupement industrie alimentaire	21/07/04	2004	24/04/05
60	504	regroupement industrie alimentaire	21/07/04	2004	24/04/05
61	503	regroupement industrie alimentaire	21/07/04	2004	24/04/05
62	454	remontees mecaniques et domaines skiables	26/05/2004	2004	12/08/05
63	832	ciments industrie fabrication ouvriers	02/04/2004	2004	28/07/04
64	833	ciments industrie fabrication etdam	02/04/2004	2004	28/07/04
65	493	vins, cidres, spiritueux	10/02/05	07/05/05	12/08/05
66	2075	oeufufs conditionnement transformation	07/04/05	29/19/2005	05/04/06
67	2410	biscottteries chocolateries	21/07/04	2004	14/02/05
68	1930	meunerie	27/10/04	2004	18/08/05
69	2344	siderurgie	18/05/2004	2004	12/05/04