Four essays on the labor market behavior of firms

Cyprien Batut

École des Hautes Études en Sciences Sociales

Paris-Jourdan Sciences Économiques - UMR8545

Doctorat

Discipline : Economie

Four essays on the labor market behavior of firms

Quatre essais sur le comportement des entreprises sur le marché du travail

Cyprien Batut

Thèse dirigée par Eric Maurin, Paris School of Economics EHESS

Date de soutenance : le 13 Novembre 2020

Rapporteurs :

1. Pauline Givord, OCDE, Paris, Directorate of Education and Skills.
2. Dominique Meurs, Université Paris Nanterre

Jury :

1. Maya Bacache, Telecom Paris-Tech
2. Antoine Bozio, Paris School of Economics

Remerciements

Je tiens à remercier toutes les personnes à mes côtés pendant les quatre ans qu'a duré ce doctorat. Si je devais faire un choix entre eux et toutes les pages qui vont suivre, je n'hésiterais pas une seconde à les choisir. Loin d'être un exercice solitaire, cette thèse est la somme des mes intéractions avec eux. Je pense en particulier à ma famille qui n'a jamais manqué de poser des questions sur l'avancement de ma thèse ("Et alors, ça va la thèse ?") et se trouvait alors condamné à écouter ma bredouillante réponse ("Euh oui ça peut aller, là je suis en train de travailler sur ..., mais ça n'avance pas trop"). Je pense également à mes amis, proches ou lointains, rencontrés durant ces quatres années ou bien avant. Je suis meilleur grâce à vous et ce n'est que la pudeur qui m'empêche d'écrire ici vos noms. J'espère que si vous lisez ces lignes vous vous reconnaitrez. Je pense ensuite à mes coauteurs, Andrea, Eve, Sarah et Eric, qui ont tous grandement particpé à cette thèse. Je suis fier d'avoir travaillé avec eux et je n'ose pas immaginer ce que j'aurais fait sans leur aide. Je pense bien sûr aux rapporteurs et aux membres du Jury. Il n' y a pas assez d'économistes en France et ils ont trop peu de temps pour que leurs efforts soient anodins. Enfin, je pense à mon directeur de thèse, Eric. Il a été pour moi durant ce doctorat une boussole qui a toujours indiqué le bon sens. Très souvent j'ai cru en mon fort intérieur avoir raison contre lui, très rarement les faits ont confirmé cette impression. Je ne regretterai jamais d'avoir poussé la porte de son bureau il y a plus de quatre ans pour parler d'un projet de thèse.

Résumé et mots-clés

Résumé Cette thèse s'appuie sur quatre études sur le marché du travail français. Le premier chapitre porte sur l'introduction en 2008 d'une prime à l'embauche visant les TPE. Pendant une période limitée, les TPE qui ont recruté des travailleurs proche du salaire minimum ont pu bénéficier du remboursement d'une partie de leurs cotisations patronales. Ce type de mesure a été reproduit à plusieurs reprises au cours des années suivantes en raison de son succès présumé. Le deuxième chapitre concerne l'introduction en 2008 d'une nouvelle façon de mettre fin aux CDI en France : la rupture conventionnelle. La rupture conventionnelle permet de mettre fin à moindre coût à la relation de travail, à condition que l'employeur et le salarié donnent tous deux leur accord. En 2018, dix ans après la réforme, les ruptures conventionnelles représentaient le deuxième type de rupture le plus fréquent en France. Le troisième chapitre examine le comportement des entreprises sur les marchés du travail locaux où la demande de travail est concentrée dans les mains de quelques entreprises. Sur les marchés du travail locaux où il y a peu d'employeurs potentiels, les options extérieures des salariés sont limitées, ce qui peut donner aux entreprises le pouvoir de fixer des salaires inférieurs au niveau qui serait observé sur un marché concurrentiel. Enfin, le dernier chapitre étudie la manière dont les personnes et les entreprises réagissent aux changements de leur environnement local, à savoir l'ouverture d'un centre de logement pour réfugiés dans la municipalité.

Pour donner un aperçu des résultats, le premier chapitre suggère que les primes à l'embauche temporaires ont un effet à long terme sur l'emploi. Ce résultat est conforme à l'hypothèse selon laquelle la création de postes de travail permanents est un investissement risqué qui entraîne des coûts d'ajustement importants. En aidant les entreprises à payer ces coûts, les primes à l'embauche peuvent avoir des effets à long terme car la rentabilité des postes crées n'est pas en cause. Le deuxième chapitre montre que l'introduction des ruptres conventionnelles a à la fois augmenté la fluidité du marché du travail et y a réduit les risques de conflits. Le chapitre 3 établit un lien entre le comportement des entreprises sur le marché du travail et la concentration de la demande de travail sur celui-ci. Il montre que les entreprises dont le marché du travail est concentré sont capables de réduire les salaires horaires des travailleurs en place, conformément à leur position monopsonistique. Enfin, le chapitre 4 montre que le nombre d'habitants des municipalités d'accueil des réfugiés diminue après l'ouverture des centres de réfugiés, principalement en raison de la réduction des flux de nouveaux arrivants dans ces villes. Il suggère que l'ouverture d'un centre de réfugiés affecte l'évaluation des municipalités d'accueil des nouveaux arrivants potentiels. Les entreprises suivent la marche et ont tendance à éviter également les municipalités qui ont des centres de réfugiés.

Mots-clés : Marché du travail, Entreprises, Politique publique, Prime à l'embauche, Rupture conventionnelle, Monopsone.

Abstract and Keywords

Abstract

This dissertation builds on four different studies on the French labor market. The first chapter focuses on the introduction in 2008 of a hiring credit for small firms. For a limited period, small firms who recruited low wage workers were eligible to the reimbursement of part of their employer contributions. This type of measure was replicated on several occasions in the following years on the account of its alleged success. The second chapter is about the introduction in 2008 of a new way to terminate indefinite term contracts in France : terminations by agreement (rupture conventionnelle). Terminations by agreement allow terminating employment relationships at lower cost, provided that employers and employees both give their consent. In 2018, ten years after the reform, terminations by agreement represent the second most frequent type of employment terminations in France. The third chapter looks at the behavior of firms in local labor markets where the labor demand is concentrated in the hand of a few firms. In local labor markets where there are few potential employers, the employees' outside options are limited and this may give firms the power to set wages below the level that would be observed in a competitive market. Eventually, the last chapter looks at how people and firms react to changes in their local environment, namely the openings of refugee housing center in the municipality.

To give a preview of the results, the first chapter suggests that temporary hiring credits have long term effect on employment. This result is consistent with the assumption that the creation of permanent job positions is an investment that entails significant adjustment costs. By helping firms pay these costs, temporary hiring credits can have longer term effects than mere wage subsidies. The second chapter shows that the introduction of terminations by agreement both increased labor market fluidity and reduced labor conflicts' risks. Chapter 3 links firm behavior on the labor market with their local market power. It shows that firms in concentrated labor market are able to reduce hourly wages of incumbent workers, consistent with their monopsonistic position. Eventually, Chapter 4 shows that the number of inhabitants in refugee-hosting municipalities decreases after the opening of refugee centers, mostly because inflows of newcomers to these cities are reduced. It suggests that the opening of a refugee center affects the valuation of refugee-hosting municipalities by prospective newcomers. Firms follow the flows and tend to avoid municipalities with refugee centers too.

Keywords : Labor market, Firms, Public policy, Hiring credits, Terminations by agreement, Monopsony.

Table des matières

General Introduction **14**

0.1 Contribution and outline of the dissertation 15

0.2 Chapter 1 : Subsidizing Adjustment Or Wages ? Evidence From A Hiring Credit In France 16

0.3 Chapter 2 : Termination of Employment Contracts by Mutual Consent and Labor Market Fluidity 17

0.4 Chapter 3 : Labor Market Concentration and Stayers' Wages : Evidence from France 18

0.5 Chapter 4 : Rival Guests or Defiant Hosts ? The Economic impact of Hosting Refugee 19

0.6 Conclusion 20

1 Subsidizing Adjustments Or Wages ? Evidence From a Hiring Credit In France **22**

1.1 Introduction 24

1.2 Context 26

1.3 Data 28

1.3.1 DADS 28

1.3.2 EMMO 30

1.3.3 Monitoring file of Pôle Emploi 31

1.4 Descriptive statistics 31

1.5 Econometric analysis 36

1.5.1 Methodology 36

1.5.2 Results 39

1.5.3 Discussion 41

1.6 Robustness tests 45

1.6.1 Selection in the sample 45

1.6.2 Placebo tests 48

1.6.3 Randomization inference 49

1.7 Conclusion 50

1.A Measure of eligibility criterion 51

1.B Use of the hiring credit 51

1.C Selection in the EMMO sample . . . 52
1.D Anticipations . . . 53

2 Termination of Employment Contracts by Mutual Consent and Labor Market Fluidity **56**
2.1 Introduction . . . 58
2.2 Institutional context . . . 60
2.2.1 Institutional context before 2008 . . . 61
2.2.2 The 2008 reform . . . 61
2.3 Potential effects of the reform : a conceptual framework . . . 62
2.4 Data . . . 64
2.4.1 Terminations by agreement and establishments' survival . . . 64
2.5 Terminations by agreement and establishments' exit flows : a graphical analysis . . 65
2.5.1 Terminations by agreement as a substitute for other forms of terminations . 66
2.5.2 Terminations by agreement and overall separation rates . . . 67
2.6 Regressions analysis . . . 68
2.6.1 Regression results . . . 69
2.6.2 An augmented specification . . . 71
2.6.3 Interaction between the timing of the reform and the one of the crisis . . . 72
2.7 Termination by agreement and worker mobility . . . 72
2.8 Conclusion . . . 74
Figures and Tables . . . 77
2.A Figures and Tables . . . 84
2.B Conceptual Framework . . . 89
2.B.1 Technology and adjustment costs . . . 89
2.B.2 First-order conditions and state variables . . . 90
2.B.3 Pre-reform optimal strategies . . . 90
2.B.4 After the reform . . . 92
2.C A "stacked" difference-in-difference approach . . . 95

3 Labor Market Concentration and Stayers' Wages : Evidence from France **97**
3.1 Introduction . . . 99
3.2 Literature review . . . 100
3.2.1 Modern Monopsony . . . 100
3.2.2 New classical Monopsony . . . 101
3.3 Empirical specification . . . 101
3.3.1 Labor market concentration . . . 101

3.3.2 Labor market concentration and wages 102
3.4 Data 105
3.5 Main results 106
3.6 Secondary results 108
3.6.1 Selection into the stayer status 108
3.6.2 Heterogeneity 108
3.7 Conclusion 109
Tables 113
3.A Not for publication *or* for online publication only 116

4 Rival Guests or Defiant Hosts ? The Local Economic Impact of Hosting Refugees 121

4.1 Introduction 123
4.2 Context and Data 126
4.2.1 Humanitarian migrants and housing centers in France 126
4.2.2 Data 128
4.3 Methodology 130
4.3.1 Econometric model 130
4.3.2 Identification Hypothesis 131
4.4 Results 133
4.4.1 The mobility response of natives 133
4.4.2 Economic consequences of native avoidance 137
4.5 Estimating the aggregate welfare cost of refugee center openings 138
4.6 Conclusion 142
4.A Alternative channels 149
Appendix 149
4.A.1 Local Labor market 149
4.A.2 Criminality 150
4.B Refugees and housing centers 151
4.B.1 Humanitarian migrants in France 151
4.B.2 Refugee centers openings 151
4.B.3 Refugee centers in the press 152
4.C Definition of population 152
4.D Matching 152
4.E Spillover effect 153
4.E.1 Identification 153
4.E.2 Estimation 153

4.F Main types of crime and misdemeanour 154
4.G Another amenity shock : day-care centers closures 154
4.H Other matching algorithms 155
4.I Local tax rates 155
4.J Identification problem 155
Tables 158
Figures 166

Table des figures

1.1 Amount of the subsidy in term of Minimum wage 27
1.2 Length of subsidy according to the month of the hire 28
1.3 Survival rate of firms between 6-14 FTE employees at the end of 2008 30
1.4 Credit take up rate in 2009 for firms between 5 and 20 FTE employees 32
1.5 Number of firms by size 33
1.6 Trends in employment level 34
1.7 Trends in employment growth 34
1.8 Number of hires and separations per quarter 35
1.9 Differential evolution of employment growth since January 2008 per month between eligible and ineligible firms 40
1.10 Results Model (2) : Number of hires and separations 41
1.11 Differential evolution of employment growth since January 2008 per month between eligible and ineligible firms in more or less impacted sectors/areas 43
1.12 Actual and counterfactual employment 44
1.13 Job creation and total number of additional months of work thanks to the credit . 44
1.14 Cost per additional months of work and temporal horizon 45
1.15 Differential incidence of exits between eligible and ineligible firms 47
1.16 Differential evolution of employment growth since January 2008 per month between eligible and ineligible firms in the unbalanced panel 47
1.17 Placebos : 10-15 vs. 15-20 16-20 vs. 20-24 FTE employees in 2008 48
1.18 Placebo : reform en 2008 49
1.19 Randomized inference : sampling distribution of the model according to eligibility and comparison with original results 49
1.20 Extract from the Cerfa form for obtaining the hiring credit 51
1.21 Evolution of the number of subsidized hires 52
1.22 Number of Google searches per week from 2008 to 2012 for two terms related to the 2009 hiring credit 53

2.1 Number of terminations by agreement between 2008 and 2017 77

2.2 Cumulative proportion of establishments that have already used terminations by agreement . . . 77
2.3 Rate of termination by agreement before and after the first use of the procedure . 78
2.4 Rate of dismissal for cause before and after the adoption of terminations by agreement. 78
2.5 Difference in rates of dismissal for cause between early adopters and late adopters 79
2.6 Rate of dismissal justified by economic reasons before and after the adoption of terminations by agreement . . . 79
2.7 Difference in rates of dismissal justified by economic reasons between early adopters and late adopters . . . 80
2.8 Overall rate of termination of permanent contracts before and after the adoption of terminations by agreement . . . 80
2.9 Difference in overall rates of termination of permanent contracts between early adopters and late adopters . . . 81
2.10 Quit rate before and after the adoption of terminations by agreement . . . 81
2.11 Difference in quit rates between early adopters and late adopters . . . 82
2.A.1 Adoption of terminations by agreement and establishments' survival in the balanced panel . . . 84
2.A.2 Adoption of terminations by agreement and differential rate of survival the balanced panel . . . 84
2.A.3 Retirement rate before and after the adoption of terminations by agreement . . . 85
2.A.4 Difference in retirement rates between early adopters and late adopters . . . 85
2.A.5 Number of employees before and after the adoption of terminations by agreement . 86
2.A.6 Difference in number of employees between early adopters and late adopters . . . 86

A1 Change in labor market concentration 2009-2015 : Hirings . . . 117
A2 Change in labor market concentration 2009-2015 : Employment . . . 117
A3 Distribution of labor market concentration in 2009 : Hirings . . . 118
A4 Distribution of labor market concentration in 2009 : Employment . . . 118
A5 Labor market concentration in French commuting zones in 2009 : Hirings . . . 119
A6 Labor market concentration in French commuting zones in 2009 : Employment . . 120

4.1 Housing centers in France . . . 166
4.2 Number of employed residents in hosting and non-hosting municipalities . . . 166
4.3 Number of employed residents in hosting and matched municipalities . . . 167
4.4 Dynamic effect of a refugee center opening on the resident employed population . . 167
4.5 Effect of a refugee center opening on the resident employed population by event . . 168

4.6 Longer-time dynamic effect of a refugee center opening on the resident employed population 168
4.7 Dynamic effect of a refugee center opening on the local labor market 169
4.8 Dynamic effect of a refugee center opening on the number of non-resident workers 169
4.9 Event analysis of the effect of a refugee center opening on crimes 170
4.10 Number of foreigners in the DADS 170
4.11 Effects on the population and migrant shares 171
4.12 Humanitarian migration between 2001 and 2017 in France 171
4.13 Refugee center openings between 1998 and 2018 in France 172
4.14 Refugees and housing centers occurrences in French newspapers 172
4.15 Different population definitions for a given municipality A 173
4.16 Common support of the matching specification 173
4.17 Spillover effects of a refugee center opening on the resident employed population . 174
4.18 Spillover effects of a refugee center opening on the number of workers 174
4.19 Effect of the distance of non-hosting municipalities to the refugee center 175
4.20 Identification problem – A Classical Problem – DAG 175
4.21 Identification problem - Two-way fixed effects estimator – DAG 175

Liste des tableaux

1.1 Selection of firms in our sample : 2008 29

1.3 Characteristics of employees of eligible and ineligible firms in 2008 33

1.4 Parallel trends - Employment level 37

1.5 Parallel trends - Employment growth 38

1.6 Difference-in-difference : probability of exiting the sample 46

1.7 Characteristics of employees of eligible and ineligible firms in 2008 52

2.1 The effect of adopting terminations by agreement on permanent contract terminations and number of persons employed 82

2.2 The effect of terminations by agreement on permanent contract terminations, by age, gender and occupational subgroups 83

2.3 Effect of potential exposure to terminations by agreement on workers' trajectories 83

2.A.1 Description of the balanced panel 87

2.A.2 The effect of adopting terminations by agreement on permanent contract terminations and number of persons employed : an analysis on the subsample where the two quarters prior to the first termination by agreement are dropped. 87

2.A.3 The effect of adopting terminations by agreement on permanent contract terminations and number of persons employed : an analysis on the unbalanced panel. . . . 88

2.A.4 Effect of potential exposure to terminations by agreement on workers' trajectories - placebo regressions 88

2.C.5 The effect of adopting terminations by agreement on permanent contract terminations and number of employees : Event-by-event analysis. 96

3.1 HHI based on Hirings - OLS 113

3.2 HHI based on Hirings - IV 113

3.3 HHI based on Employment - IV 114

3.4 HHI based on Employment and Hirings - Alternative IV 114

3.5 HHI based on Hirings - Alternative Dependant variable 115

3.6 HHI based on Hirings - Heterogeneity 115

A1 Descriptive statistics - Local labor markets 116
A2 Monopsony in Local Labor Markets in France 116
A3 Individual characteristics - Panel DADS, 2009-2015 116

4.1 Effect of a refugee center opening on the resident employed population 158
4.2 Effect of a refugee center opening on the fiscal population 158
4.3 Effect of a refugee center opening on inflows and outflows of employed residents . . 159
4.4 Effect of a refugee center opening on retirees and taxed households 159
4.5 Effect of a refugee center opening on firms outcomes 159
4.6 Effect of a refugee center opening on local taxes 160
4.7 Effect of a refugee center opening on local tax rates 160
4.8 Effect of a refugee center opening on hires of foreign workers 160
4.9 Effect of a refugee center opening on resident and non-resident workers 161
4.10 Effect of a refugee center opening on non-resident workers 161
4.11 Effect of a refugee center opening on crimes in the department 161
4.12 Effect of a refugee center opening on residents employed by skill and income 162
4.13 Hosting vs. matched municipalities 2 years before the openings 162
4.14 Hosting vs. other municipalities in 2002 163
4.15 Effect of a refugee center opening on the population employed in the municipality and in municipalities within a 10-km vicinity 163
4.16 Main type of crimes by category 164
4.17 Effect of day-care centers openings on the number of fiscal households 164
4.18 Effect of a refugee center opening on the resident employed population with other matching algorithms 165
4.19 Effect of a refugee center opening on the resident employed population (no weights) 165

General Introduction

The French labor market is characterized by high and persistent unemployment. Since the eighties, the unemployment rate has never fallen below 7%, even during periods of strong economic growth. The French labor market is also highly segmented between temporary labor contracts and indefinite term contracts (contrats à durée indéterminée, hereafter, CDI). The vast majority of French workers hold a CDI, but most of the adjustment in labor demand is obtained by varying the number of temporary jobs and by requiring temporary workers to change jobs through unemployment. From one year to another, transitions from employment to unemployment are on average about seven times more likely for temporary workers than for CDI workers. As it happens, indefinite term jobs are much more protected and harder to destroy than temporary ones.

The high segmentation of the French labor market is a source of inequalities between insiders and outsiders, it is also a source of anxiety and labor conflicts. Permanent workers are well aware that losing the protection associated to CDI may lead to a job insecurity trap. Most labor conflicts find their origin in CDI terminations that permanent employees consider unfair. More than a quarter of dismissals were contested in French Labor courts in 2015 according to Fraisse et al. (2015), which implies high procedural costs for both employers and employees. The lack of fluidity in the French labor market is also reflected in the persistence of very strong inequalities in unemployment rates across local labor markets.

In this context, successive French governments have implemented many reforms in an attempt to boost hiring, increase labor market fluidity, reduce segmentation and inequalities across local labor markets. In this dissertation, I provide an evaluation of two of the most emblematic of such reforms, namely the introduction of employment terminations by mutual consent and the introduction of hiring credits for small firms, both implemented in the late 2000s. Also, I try to shed light on some of the mechanisms that may explain inequalities across local labor markets. I will focus on two basic features of local labor markets, namely the number of firms in which employment is concentrated and the type of neighborhoods in which they are inserted.

Generally speaking, the different chapters of this thesis have in common that they focus on how firms adapt to the rules that frame the labor market and to their local environment. The working assumption is that a good understanding of firms' behavior is a key pre-requisite for designing new labor market institutions and improving labor market dynamism.

0.1 Contribution and outline of the dissertation

This dissertation builds on four different studies. The first chapter focuses on the introduction in 2008 of a hiring credit for small firms in France. For a limited period, small firms who recruited low wage workers were eligible to the reimbursement of part of their employer contributions. This type of measure was replicated on several occasions in the following years on the account of its alleged success. The second chapter is about the introduction in 2008 of a new way to terminate indefinite term contracts in France : terminations by agreement (rupture conventionnelle). Terminations by agreement allow terminating employment relationships at lower cost, provided that employers and employees both give their consent. In 2018, ten years after the reform, terminations by agreement represent the second most frequent type of employment terminations in France. The third chapter looks at the behavior of firms in local labor markets where the labor demand is concentrated in the hand of a few firms. In local labor markets where there are few potential employers, the employees' outside options are limited and this may give firms the power to set wages below the level that would be observed in a competitive market. Eventually, the last chapter looks at how people and firms react to changes in their local environment, namely the openings of refugee housing center in the municipality. Will people avoid or leave municipalities where refugees are ? Do firms avoid municipalities with refugee center or do they take the opportunity of cheap labor to thrive ?

To give a preview of the results, the first chapter suggests that temporary hiring credits have long term effect on employment. This result is consistent with the assumption that the creation of permanent job positions is an investment that entails significant adjustment costs. By helping firms pay these costs, temporary hiring credits can have longer term effects than mere wage subsidies. The second chapter shows that the introduction of terminations by agreement both increased labor market fluidity and reduced labor conflicts' risks. Chapter 3 links firm behavior on the labor market with their local market power. It shows that firms in concentrated labor market are able to reduce hourly wages of incumbent workers, consistent with their monopsonistic position. Interestingly enough, French local labor markets appear to be less concentrated than in the US or the UK. Moreover, French firms use their market power less than comparable firms in other developed countries. Eventually, Chapter 4 shows that the number of inhabitants in refugee-hosting municipalities decreases after the opening of refugee centers, mostly because inflows of newcomers to these cities are reduced. It suggests that the opening of a refugee center affects the valuation of refugee-hosting municipalities by prospective newcomers. Firms follow the flows and tend to avoid municipalities with refugee centers too. In the remainder of this introduction, I will present my different chapters in more details.

0.2 Chapter 1 : Subsidizing Adjustment Or Wages ? Evidence From A Hiring Credit In France

The first chapter of this dissertation provides an evaluation of the implementation of a hiring credit targeting small firms in France ("Aide au Très Petites Entreprises" or ATPE) from December 2008 to July 2010. It offered firms with less than 10 workers a temporary reduction in labor costs in the form of reimbursement of employer contributions for all hiring of workers paid below 1.6 times the minimum wage. I use exhaustive administrative datasets linking employees and employers to estimate the effect of the credit on employment growth and worker flows. Specifically, I compare firms just above and just below the 10-employee eligibility threshold before and after the introduction of the credit. Firms with between 10 to 14 workers in December 2008 could not benefit from the hiring credit and are used as a control group. If we assume that eligible firms with 6 to 10 workers would have evolve the same way as the 10 to 14 firms without the credit, then the differential evolution of employment growth between the two groups of firms can be attributed to the hiring credit. Using this strategy, I estimate that the credit had a short-run positive effect on employment growth while it lasted, which is consistent with Cahuc et al. (2019). It was mainly due to a surplus of hires. Looking at longer term effects (which is my central contribution), I find that employment growth in eligible firms goes back to its pre-subsidy trend after the end of the subsidy which means that jobs created thanks to the credit were not transitory. Otherwise employment growth in eligible firms would have fallen below its pre-subsidy trends, reflecting the destruction of the jobs initially created. Consistently, I observe no increase in separations after the end of the credit in eligible firms.

Generally speaking these results are suggestive that a temporary hiring credit implemented during a sharp recession can lead to stable job creation. The 2009 hiring credit was indeed implemented in the context of a global recession where many large firms stopped their recruitment. The credit led many very small firms to adopt a counter-cyclical behavior i.e to take more risk and invest in new permanent job positions. In the context of the 2009 crisis in France, this policy was highly cost-effective : the cost per job created for the public authorities can be estimated to be less than a quarter of the private cost for that job when taking into account the persistence of job creation. It should be noted, however, that the diagnosis would not necessarily be the same if the credit had been implemented at a different point in the economic cycle. Also, the fact that the subsidy was extended beyond 2009 likely also contributed to the sustainability of the jobs created before 2009. Further research is needed to explore this issue.

0.3 Chapter 2 : Termination of Employment Contracts by Mutual Consent and Labor Market Fluidity

The second chapter of this dissertation, co-written with Eric Maurin, studies the consequences of the introduction in France of a new way to terminate open-ended employment contracts : the rupture conventionnelle (which we translate in english by termination by agreement). In many countries (as well as in France before 2008), the termination of employment contracts has to be either on employer initiative or on employee initiative. Furthermore, the cost of the procedure is borne mainly by the contracting party who takes the initiative. There is little room for sharing termination costs and this lack of flexibility can prevent the termination of employment relationships that are considered unsatisfactory by both the employee and the employer. The implicit doctrine is that employment termination has to be the last resort, the ultima ratio. In 2008, the French government initiated a change in doctrine : it became possible to terminate employment contracts by mutual consent and at lower costs. The new procedure makes possible for employers to terminate employment contracts without any justification, provided that they get the consent of employees and accept to grant severance payments at least as high as the severance payments granted to dismissed workers. For employers, the new procedure has the advantage of reducing dramatically the risk of being sued for wrongful dismissal. With respect to employees, it becomes possible to leave one's employer without losing eligibility to receive severance payments and unemployment benefits (which would not be the case after a quit) and without enduring the stigmatization associated with dismissals.

To explore the effects of the reform, our paper builds on establishment-level administrative data with detailed quarterly information on workers' entries and exits across the 2004-2014 period. When we compare establishments who started to use terminations by agreement relatively early with those who started several quarters (or years) later, we find that the reform was followed by a decline in dismissals for non-economic reasons as well as by a significant rise in overall separation rates. By promoting separations by mutual consent, the reform reduced labor litigation risks (litigations are mostly related to dismissals for non-economic reasons) and boosted workers' flows. These results suggest that a reduction in separation costs does not necessarily come at the price of increased conflicts between employees and employers. Moreover, we do not detect any effect on firms' employment levels, which is suggestive that the increase in overall separation rates induced by the reform was offset by a symmetrical increase in hiring rates.

Looking at the individuals who were the most exposed to the introduction of Terminations by agreement from 2009 to 2011, we find that they were able to transition to better paid jobs without affecting their probability to be employed in 2012. The introduction of Terminations by agreement shows that, through mutual consent, it is possible reconcile economic efficiency and job protection.

0.4 Chapter 3 : Labor Market Concentration and Stayers' Wages : Evidence from France

The third chapter of this dissertation, co-written with Andrea Bassanini and Eve Caroli, investigates the impact of labor market concentration on stayers' wages. The local concentration of labor demand likely gives local employers a decisive power in wage determination as Boal and Ransom (1997) suggest. But do local firms use this market power ? Using administrative data for France, we first provide a measure of the concentration of labor demand in French local areas. Specifically, we compute the Herfindhal index (the sum of squared market shares) for employment and hires in French local labor market, where local labor markets are defined as the intersection between occupations and local commuting zones. The Herfindhal Index can be understood as the inverse of the number of potential employers when their market share is equal. It is traditionally used by antitrust authorities to assert the level of concentration of markets for goods and services. A burgeoning literature (see Azar et al. (2017) and Benmelech et al. (2018) among others for the US) uses this index to estimate the impact of employer market power on wages and other labor market outcomes. Aggregate effects on wages could in principle be driven by wage pressure on either new hires or incumbent workers. Marinescu et al. (2019) study the impact of labor market concentration on wages in France, but focus on new hires only. We choose instead to focus on incumbent workers, where incumbent workers are defined as individuals who remain employed in the same establishment for at least two years. Looking at these more senior workers is important since they represent a very large share of all employees in any given year in OECD countries and are the largest contributor to aggregate wage growth. So far, no paper in the literature tried to look at the specific effect of monopsony on these more senior workers. We show that the elasticity of stayers' wages to labor market concentration is about -0.014, after controlling for labor productivity, product market competition and match-specific heterogeneity. When the concentration of labor demand in a labor market doubles, stayers' wage decreases on average by 1.4%. This estimation is robust to the use of two instruments for the Herfindhal index : the inverse of the average number of competitors in similar markets in non-bordering commuting zones and the weighted (by the similarity of the productive structure) average Herfindhal index in non-bordering commuting zones. It means that firms in concentrated labor markets are able to advantageously renegotiate (and/or unilaterally adjust) wages of stayers who face fewer outside options. It is also fifteen times lower than the estimation of Marinescu et al. (2019) of the impact of labor market concentration on wages of new hires only : insiders in France are relatively well protected from Monopsony power coming from the concentration of labor demand. In a comparative perspective, our analysis of labor market concentration in France reveals that employment is much less concentrated than in the US or UK, especially in rural areas. It may be a legacy of the history of firm entrepreneurship in France. French firms are smaller and more evenly distributed in the territory. Second, it seems that that firms are less willing (or able) to use their monopsony power in France than in other countries : according to our estimates, the elasticity of wages to labor market concentration is indeed much lower in France than in the US (Azar et al. (2017)).

0.5 Chapter 4 : Rival Guests or Defiant Hosts ? The Economic impact of Hosting Refugee

Eventually, the fourth chapter of this dissertation, co-written with Sarah Schneider-Strawczynski, looks at the consequences of the opening of refugee centers on hosting municipalities. Specifically, we analyze the consequences of the opening of a refugee center in 98 French municipalities between 2004 and 2012. We compared the evolution of the local number of inhabitants and local economic outcomes in hosting and non-hosting municipalities two years before and after the opening of the centers.

Using French administrative data, we show that the opening of centers for refugees has a negative impact on the number of inhabitants as well as on the economic activity in hosting municipalities, even though the number of refugees in each center is typically very small. Before the opening of centers, the local number of inhabitants in hosting and non-hosting municipalities followed parallel trends. But two years after the opening of a center, the local population decreased on average by about 1.6%, while it increased by about 0.4% in non-hosting municipalities. Further investigations reveal that this local decline is mostly due to fewer people moving into municipalities hosting refugee centers (rather than to more people moving out). The decline in the local number of inhabitants also coincides with a decline in the local number of firms (about -3%) as well as in the value of their sales (-5%). Over time, lower inflows of people into hosting municipalities mean fewer customers which may be one explanation for the declining number of firms. We argue that these negative effects on local outcomes are likely driven by anti-refugee prejudices. The inflows of refugees are indeed too small to have any direct effect on economic activity. Overall, our paper provides evidence that prospective newcomers value negatively the presence of refugee centers and avoid them when deciding where to live. This has economics consequences and firms seem to adapt to this negative amenity shocks quite quickly. In the final part of the paper, we propose an estimation of the aggregate cost of hosting refugees : each opening “costs” about two millions euros to prospective newcomers in lower quality of life, lower wage and higher rent in their second choice municipality.

0.6 Conclusion

To conclude, Chapter 1 shows that well-targeted adjustment subsidies can be a valuable counter-cyclical policy while Chapter 2 suggests that it is possible to increase labor market fluidity without harming employers and employees. Chapter 3 highlights that monopsony is likely an overlooked issue, especially in more rural areas where the concentration of labor demand is higher but not that a pressing issue for workers in a stable job. Finally Chapter 4 shows that refugee centers create a negative amenity shock that repel prospective newcomers and firms

This PhD dissertation provides an empirical illustration of the variety of firm behaviors in the labor market. Even if their content is mostly empirical, the different chapters also develop simple theoretical models in order to better interpret firm behaviors. For example, the adjustment cost model developed in chapter 2 highlights how we can make sense of the evolution of the different transition rates before and after the 2008 reform. In Chapter 1, I try to highlight with a simple dynamic labor demand model the different scenarios where hiring credits can increase employment. The model suggests that a similar hiring credit policy could have had a very different impact, had it been implemented on another point of the business cycle. Pre-implementation experimentations and testing are necessary, but may not be sufficient if the underlying environment is not taken into account. Chapter 3 demonstrates that there is a significant degree of monopsony in French labor markets. It changes what to expect from policies affecting the minimum wage or working time regulations. A policy which proved its efficiency in a given economic context may not scale very well in others. This is also true for the timing of the testing. At the aggregate level as well as the local one, the behavior of firms is often pro-cyclical. Showing that hiring credits is a good policy in a time of recession does not mean that it would be cost-effective to implement a similar policy at another point of the business cycle. The effectiveness of hiring credits may be linked to the rise of the risk premium of entrepreneurs as uncertainty rise in time of crisis as it is showed in Chapter 2. In Chapter 4, we show that by fleeing refugee-hosting municipalities, firms may exacerbate the negative impact of refugee center openings. Firms are key players in the labor market and we have to take them into account even when we design and evaluate policies that do not target them directly. Further research is needed to better understand their labor market behaviors so as to better design our public policies.

Bibliographie

José Azar, Ioana Marinescu, and Marshall I Steinbaum. Labor market concentration. Technical report, National Bureau of Economic Research, 2017.

Efraim Benmelech, Nittai Bergman, and Hyunseob Kim. Strong employers and weak employees : How does employer concentration affect wages ? Technical report, National Bureau of Economic Research, 2018.

William M Boal and Michael R Ransom. Monopsony in the labor market. *Journal of economic literature*, 35(1) :86–112, 1997.

Pierre Cahuc, Stéphane Carcillo, and Thomas Le Barbanchon. The effectiveness of hiring credits. *The Review of Economic Studies*, 86(2) :593–626, 2019.

Henri Fraisse, Francis Kramarz, and Corinne Prost. Labor disputes and job flows. *ILR Review*, 68 (5) :1043–1077, 2015.

Robert E Hall. High discounts and high unemployment. *American Economic Review*, 107(2) : 305–30, 2017.

Ioana Elena Marinescu, Ivan Ouss, and Louis-Daniel Pape. Wages, hires, and labor market concentration. *Available at SSRN 3453855*, 2019.

CHAPITRE 1

Subsidizing Adjustments Or Wages ? Evidence From a Hiring Credit In France

Cyprien Batut [1]

1. Paris School of Economics/EHESS/Chaire Travail contact : cyprien.batut@gmail.com
I would like to thank Eve Caroli, Sébastien Grobon, Thomas Le Barbanchon, Eric Maurin and Fanny Mikol for their advice and comments on earlier versions of this work.

Abstract

This article studies the implementation of a hiring credit aimed at small firms in France during the Great Recession. Using French administrative data, I estimate its effect on employment growth and worker flows. The credit had a positive effect on employment growth due to more hires. Employment growth did not fall below its pre-treatment trends after the end of the credit, meaning created jobs are not transitory. Adjustment subsidies are more efficient than wage subsidies in recession, the public cost per job created is 16% of its private cost in 2012.

JEL : J21, J63, J68

Keywords : Hiring credit, Firm subsidies, counter-cyclical policy, adjustment

1.1 Introduction

Hiring involves significant costs for the employer : finding the right candidate for a position can be a long and arduous process. The existence of these fixed costs means that some hires that are profitable relatively to their variable costs do not take place because potential employers are not ready to pay these fixed costs in addition. Manning (2011) reports that hiring costs estimated in the literature are between 1.5% and 11% of the monthly labor cost. More recently, Algan *et al.* (2018) demonstrated, using a randomized experiment of recruitment assistance for firms, that these costs amounted to about 10% of the monthly labor cost. These average estimations may hide a strong heterogeneity depending on firm characteristics and the position in the economic cycle. Hiring costs are likely to be higher for a small firm that cannot benefit from a human resources department to vet candidates more efficiently. Moreover, when either the future activity or the quality of the worker itself are uncertain, these costs can be magnified. Risk-averse employers may not want to take the risk of a bad fit, especially when separation costs are significant. To finish with, the risk premium an employer add to these costs is likely to depend on the economic context and may be one of the major channel through which labor markets are impacted by economic crises (Hall (2017)).

This paper studies the introduction in France in 2008 of an hiring credit targeting small firms to shed new lights on how one can support employment in time of crisis. This credit was not announced in advance by the French Government and therefore could not be anticipated. For 16 months, all low wage hires (less than 1.6 times the minimum wage) in firms with less than 10 full-time equivalent workers in November 2008 were eligible to a refund of part of its employer contributions. One expects this hiring credit to increase employment in eligible firms, at least while it lasts. But it could do so for three different reasons : because it is integrated by firms as a wage subsidy (a reduction of labor cost), because it lead firms to change the timing of its hires to take the opportunity of the credit (a displacement effect, as in Crépon *et al.* (2013) for example) or because it is integrated as an adjustment subsidy and lead them to hire more. This will affect the sustainability of jobs created by the credit and consequently its efficiency. First, jobs created thanks to a wage subsidy are no longer profitable once the credit is over and should be eliminated. Second, jobs are not really created by a displacement effect as much as their timing is modified : in the long term, the total number of months worked in addition should be 0. To finish with, jobs created thanks to an adjustment subsidy are in theory sustainable beyond the duration of the credit because their variable costs are not in question. To sum up, in the short term, all of these scenarios lead to the same outcome : an increase of employment in eligible firms. But they will have different outcomes once the credit is over : stability of employment growth in the case of the hiring subsidy hypothesis and negative employment growth in the two others.

This paper follows two studies that have already sought to assess the effect of the 2009 hiring credit on employment and worker flows, Cahuc *et al.* (2019) and Ananian et Pons (2017). They used different databases and evaluation strategies and reached divergent conclusions. Cahuc *et al.* (2019) find a positive effect of the 2009 hiring credit on employment while Ananian et Pons (2017) find no effect on hiring and separation flows. Both however cannot identify why employment could increase in eligible firms because they only look at the static effect of the hiring credit in 2009 but not at its dynamics from 2009 to 2012. It is only by looking at what happens after 2009, and in particular whether jobs created in eligible firms are sustainable, that the channel of job creation can be determined. For this purpose, I use an exhaustive administrative database linking employer and employees (the Annual Social Data Declarations or DADS) to measure the evolution of full-time equivalent (FTE) employment in firms eligible and ineligible to the credit from 2007 to 2012. I can then estimate month by month the effect of eligibility on employment before and after the introduction of the hiring credit by taking the average difference between eligible and ineligible firms when controlling for their initial differences. That is I implement a difference-in-difference approach. I also use an administrative survey (the "Enquête sur les Mouvements de Main d'Oeuvre d'Oeuvre" or EMMO) to measure the effects of the credit on worker flows. My estimation approach is close to Cahuc *et al.* (2019) but compare to them, in addition to look at worker flows, eligibility is fixed over time which allow us to look at long term effects of the hiring credit not just its effect in 2009.

The hiring credit had a positive effect on employment in eligible firms. First, from January 2009 to July 2010, employment growth was consistently higher in eligible firms compared to similar ineligible firms controlling for initial differences. This is mainly explained by a surplus of hires. Second, the hiring credit had a more than transitory impact on employment in eligible firms : employment growth did not fall below similar ineligible firms following the end of the hiring credit, as one would have expected if jobs created by the credit were destroyed. This means that jobs created thanks to the credit are sustainable. Firms did not integrate the credit as a wage subsidy and there was no displacement effect. On the contrary, the credit led firms to create new positions even though their profitability was uncertain in the context of the 2009 recession. It may come from the fact that hiring costs in eligible and ineligible firms were magnified by an abnormally high risk premium at that time. In the end, I estimate that the public cost per job created of 90,000 € but also that, taking into account their persistence, the cost per month of work was low less than 16% of its private cost. This may also be due to the fact that the subsidy was extended beyond 2009. Further research will need to be conducted on this point, particularly in order to develop a full cost-benefit analysis of the jobs created.

I contribute to the literature that studies employer subsidies as a way to support employment by showing that hiring credits achieve this objective but for different reasons that traditional wage subsidies. In his review of the literature, Katz (1996) argues that it broadly support that wage subsidies are an efficient tool to raise the demand for labor, especially for disadvantaged groups. But he also models hiring subsidies akin to wage subsidies while there may be several important differences. More recent articles have focused specifically on hiring subsidies. Chirinko et Wilson (2016) find that state hiring credits in the US increase employment but also that they change the timing of hires, which may lead to overestimate their impact on employment. Neumark et Grijalva (2017), which are reviewing the effect of state hiring credits in the US too, find that hiring subsidies increase churning more than net employment but seem to be more efficient during recessions. I also contribute to the literature interested in finding the best tools to support employment in time of crisis. I argue the reason for the efficiency of hiring subsidies in time of crisis is risk premium for hires are cyclical as highlighted by Hall (2017). Subsidizing hires may help employer to overcome this risk premium and may be more efficient as a counter-cyclical policy.

This article is divided into several parts. In section 1.2, I present the 2009 French hiring credit and the context of its implementation. In section 1.3, I introduce the different sources of data I use. In Section 1.4, I highlight several important descriptive statistics. In Section 1.5, I describe the methodology to estimate the effects of the credit and the results obtained. Finally, in section 1.6, I show several robustness checks and in section 1.7, I conclude.

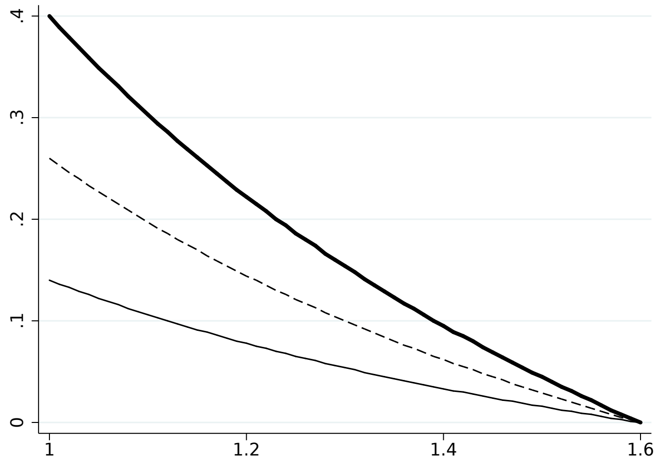
1.2 Context

The hiring credit, called ATPE ("Aide aux Très Petites Entreprises"), was suddenly announced by the Fillon Government on December 15 2008 and was implemented almost immediately by decree.[2] It originally targeted any new hire or renewal of short contracts for low-wage earners between 4 December 2008 and 31 December 2009 in firms with fewer than 10 FTE employees.

The amount paid in the form of a refund of employer contributions to the beneficiary firm decreases with the salary at the time of the hire : it becomes 0 at 160% of the minimum wage (Figure 1.1). At the minimum wage level, it is equivalent to a reduction in labor costs of 14% and 40% if it is added to the already existing Fillon relief, another reduction of employer contribution, i.e. a full refund of employer contributions. Over 12 months, an employer can then receive as much as 2400€ per hire thanks to the hiring subsidy in the form of reimbursement of its employer contributions, in addition to what he had already saved thanks to the Fillon relief.

2. The 19 December 2008 decree can be consulted here.

FIGURE 1.1 – Amount of the subsidy in term of Minimum wage



Note : The fine-line curve shows the evolution of the reduction in labor costs as a percentage of the minimum wage (x axis) allowed by the ATPE as a function of the salary at recruitment (y axis). The dashed curve does the same for the Fillon relief and the solid line curve corresponds to the sum of the two other curves. Cahuc *et al.* (2019) produce a similar figure.

Which firms are eligible for the credit ?

1. Private sector firms and associations
2. With less than 10 FTE employees on average over the first 11 months of 2008
3. Who were already eligible for Fillon relief
4. Who decide to hire for a wage lower than 1.6 times the minimum wage
5. For one or more positions of at least one month's duration
6. and for unoccupied vacancies in the last six months.
7. The new employee(s) must not have worked in the last 6 months in the same firm if their previous contract ended after December 2008.

These conditions prevent firms from laying off workers in order to take advantage of the opportunity of the credit, thus reducing the scope for manipulation. Firms meeting these conditions can apply for the hiring credit by filling in a form to be sent to Pôle Emploi, the French Public Unemployment Agency. Provided their application is accepted, they then receive a refund of part of the employer's contributions linked to the hire at the end of each quarter. Compared to traditional wage and hiring subsidies, the 2009 French hiring credit is then :

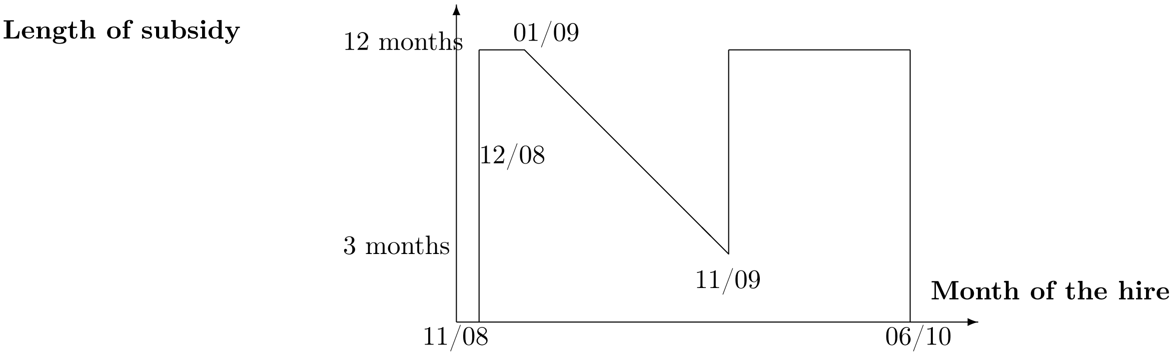
— decreasing in wages, which means it is unlikely to increase nominal net wages for workers.

— Paid as refund of employer's contribution at the end of each quarter of employment, which gives incentives to employers to keep subsidized positions for at least a year.

The ATPE was scheduled to end on December 31, 2009. However, it was extended retroactively for 12 months in November 2009 (which means that a December 2009 hire is supported until November 2010 and not for 2 months as originally planned) and extended until 30 June 2010.

The subsidy has therefore been distributed until June 2011 and this creates a discontinuity in the maximum amount that can be paid. The number of months of compensation changes according to the month of the hire (Figure 1.2). To ask for the credit in 2010, what mattered was eligibility in 2009 and not 2008, which means that firms that were ineligible in 2009 might become eligible in 2010, and vice-versa. This might blur the boundaries between the two groups as we study the long term effect of eligibility in 2008.

Figure 1.2 – Length of subsidy according to the month of the hire



Note : The figure above shows the change in the number of months of compensation for a new hire in a firm eligible for the ATPE according to the month of the hire. A recruitment made in January 2009 benefits from the credit for 12 months.

All in all, according to the monitoring file of the hiring credit by Pôle Emploi, 437,301 firms used the credit on the occasion of 1,023,327 hires. Almost 840 million euros were thus transferred to the participating firms by Pôle Emploi. The hiring credit attracted the attention of researchers, two studies have sought to estimate the effect of ATPE : Cahuc *et al.* (2019) and Ananian et Pons (2017).

1.3 Data

1.3.1 DADS

This study uses the DADS[3] (*Déclaration Annuelle des Données Sociales*), a database linking employees and employers. It covers approximately 85% of French employees in 2008. It is a database built by INSEE from forms sent to firms. They must declare for each position held the periods of employment, the number of hours worked and the corresponding salary. I use the data available from 2006 to 2012 to create a panel of firms as it allows us to follow the evolution of the level of FTE employment month by month. I limit the sample to firms in the market sector excluding associations and agriculture. I also set aside temporary workers and more generally temporary

3. The DADS are proprietary data of the INSEE that can be accessed by researchers provided they follow the procedures described in here.

employment agencies. The DADS allows us to measure the average number of full-time equivalent employees over the first eleven months of 2008 and therefore to determine the exact eligibility for the hiring credit. This is not possible in any other database of French firms. Section 1.A in the appendix describes more precisely how the full-time equivalent level of employment is measured by month.

I focus on a sub-sample of firms : those with 6-14 FTE employees in 2008. Among them, I distinguish two groups : the *eligible*, i.e. firms that had on average between 6 and 10 FTE employees in 2008 and can therefore apply for the hiring credit for a job starting in December 2008. I compare them to another group of firms : the *ineligible*, i.e. those who had on average between 10 and 14 FTE employees in the first eleven months of 2008 and therefore could not apply for the hiring credit at that time.

There are a total of 116 376 different firms fulfilling these conditions in 2008 in the sample : about 81 000 are eligible and 35 000 ineligible for the 2009 hiring credit. Table 1.1 describes the sample construction process in more detail.

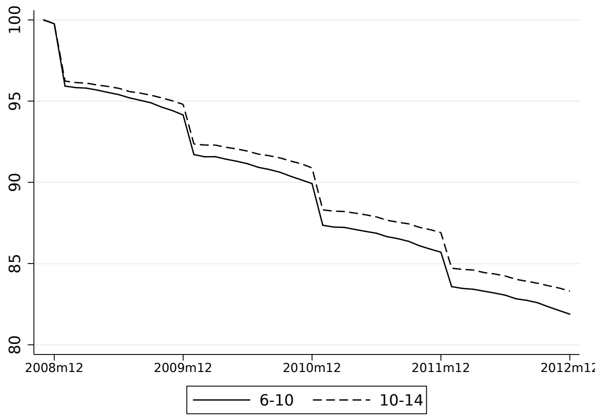
TABLE 1.1 – Selection of firms in our sample : 2008

Step	Total	<10	>10
All firms	1 521 811	1 334 302	187 509
- market sector and interim	807 983	662 774	145 209
- Agriculture	730 359	593 206	137 153
Between 6-14 FTE in 2008	116 376	80 929	35 447

Source : DADS 2008. Field : All firms. Note : Total number of firms by size after the different stages of sample construction.

I use the DADS of the following and previous years to build an unbalanced monthly panel of firms. Some firms disappear from the DADS because they close or change of ownership or of legal status. Figure 1.15 shows the percentage of firms present in November 2008 and still there in the following months, and thus the survival rate of the firms in our panel.

Figure 1.3 – Survival rate of firms between 6-14 FTE employees at the end of 2008



Source : DADS 2007-2012. Field : Firms with between 6 and 14 FTE employees in the first eleven months of 2008. Note : The solid line curve shows the evolution of the survival rate of firms with 6 to 10 FTE employees over the first eleven months of 2008 between January 2008 and December 2012. The dashed line curve compares the evolution of the survival rate of firms with 10 to 14 FTE employees at the same date.

A little less than 85% of the firms in the sample in 2008 are still here in December 2012. Attrition in the sample is homogeneous throughout the study period. Eligible firms, which are smaller, seem to fare less well even if their survival rate only differs by less than 2 points in December 2012. This takes into account the remark made by Picart (2008) about the DADS : some firms may disappear from the panel if for one reason or another the firm identifier, the SIREN, changes, even though there may not be any closure. To correct this, I create a new identifier which allows the continuity of a firm to be captured from 2008 to 2012 even if the SIREN changes. Following the advice of Picart (2008), I have identified each year the SIRENs that share at least 50% of their employees in N and $N-1$ and whose start and end of production coincide within 30 days and give them a common identifier. In practice, however, this does not fundamentally change the size of the initial sample in 2008 : 116376 vs. 116287. There is no obvious correlation between the evolution of the survival rate of eligible firms and the timing of the credit as it is shown more extensively in section 1.6.1.

1.3.2 EMMO

The EMMO [4] (*Enquête des Mouvement des Main d'Oeuvre*) is a database produced by DARES, the statistical office of the French labor ministry. Each year, approximately 50,000 establishments with fewer than 50 employees are randomly selected. For four quarters, they must fill in a survey giving details of each entry and exit from the establishment in the previous quarter : recruitment on permanent or fixed-term contracts, transfer to another establishment, quits, dismissal for economic

4. The EMMO are proprietary data of the DARES that can be accessed by researchers provided they follow the procedures described in here.

or other reasons, retirement, etc. This dataset has the advantage of differentiating between the entries and exits of establishments and their motivations.

The EMMO thus makes it possible to measure quarterly employment flows since 1988 for establishments with between 10 and 50 employees and was extended to very small establishments (from 1 to 9 employees) in 2007. Nevertheless, the EMMO is not exhaustive like the DADS and is therefore less precise : surveyed establishments represent less than 10% of the total population of firms of similar size.

1.3.3 Monitoring file of Pôle Emploi

To finish with, I was able to access the monitoring file of the 2009 hiring credit from Pôle Emploi. That is, a database cross-referencing all the payments made by Pôle Emploi on behalf of the credit and the firms that applied for it. This database gives information on the total cost of the hiring credit, the number of firms that requested it and the number of hires they declared when they applied.

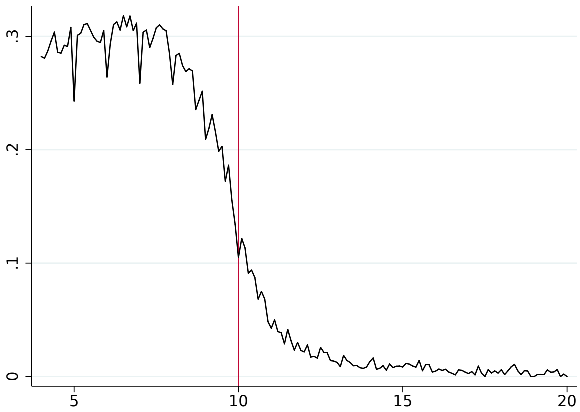
It is a key source for measuring the take-up rate for the credit, exactly when the hires were made and its impact on labor costs (the base also includes the amount of reimbursements thanks to the Fillon relief and the employer's contributions which are paid in normal times).

1.4 Descriptive statistics

The purpose of this section is to present a number of highlights about the 2009 hiring credit and its effect on firms. To begin with, the take-up rate for the hiring credit is presented Figure 1.4. It is calculated by crossing our sample with the Pôle Emploi monitoring file. Approximately 30% of eligible firms with between 5 and 10 FTE employees used the credit. The causes for not taking up the subsidy may be multiple, but the first is non-hiring : only about 50% of eligible firms recruited during this period.

The take-up rate is continuous around the eligibility threshold of 10 FTE employees as calculated in the DADS. This is explained by the fact that the level of employment determining the "real" eligibility for the credit is the one declared by the firms to Pôle Emploi and not the one calculated in the DADS. The measure of eligibility based on the DADS, although accurate in theory, is an approximation of the "true" measure. This means that some firms that are ineligible in theory are in practice ineligible and vice versa.

Figure 1.4 – Credit take up rate in 2009 for firms between 5 and 20 FTE employees

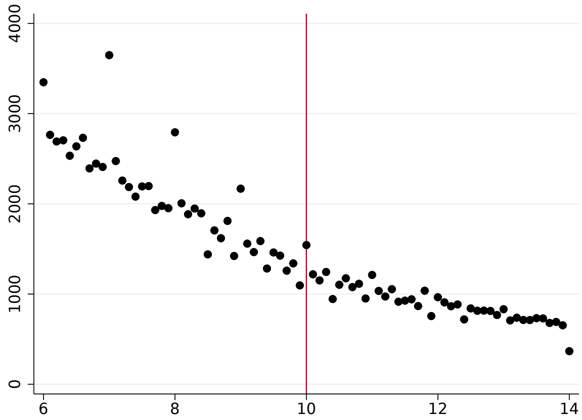


Source : Monitoring file APTE. DADS 2008. Note : Share of firms taking up the hiring credit at least once (y axis) according to their FTE level of employmet in 2008 (x-axis).

Are eligible firms fundamentally different from ineligible firms ? The threshold of 10 FTE employees is accompanied by some institutional changes. For example, employer's contributions to finance professional training double. The threshold of 11 employees is also non-negligible, especially on the employment protection side : compensation for dismissal without legitimate reason must be at least 6 months' salary (this is not the case below) and each firm must try to have an elected employee representative. However, if the threshold of 10 or 11 FTE employees represented a real change in regulation and jeopardised the comparability of our two groups of analysis then it is also likely that a greater number of firms would be observed just before this threshold [5] as it would impact the incentives of firms to actually have more employees than the said threshold. This is not the case : there is indeed an accumulation of firms near round numbers, but these discontinuities in the distribution are caused by the high proportion of full-time employees and they are exactly at the threshold and not before, as can be seen in Figure 1.5.

5. Ceci-Renaud et Chevalier (2010) also showed that employment growth did not slow down in anticipation of the 10 employee threshold and that there is no concentration of firms around this threshold (unlike the 50 employee threshold, which is accompanied by greater regulatory obligations).

FIGURE 1.5 – Number of firms by size



Source : DADS 2008 Note : Number of firms in the sample by their level of FTE employment (80 categories). Cahuc *et al.* (2019) produce a similar figure.

When comparing eligible and ineligible firms in 2008 from the DADS, there is little difference in terms of employee demographics : more employees earning less than 1.6 SMIC (+1.4 points) but less working full time (-1.4 points). However, the age distribution appears to be very similar. Most of these differences are statistically significant but that is not a surprise given the size of our sample. The two groups of firms are therefore not entirely similar, but still very close in the characteristics of their employees (most of these differences are lower than one percentage point).

TABLE 1.3 – Characteristics of employees of eligible and ineligible firms in 2008

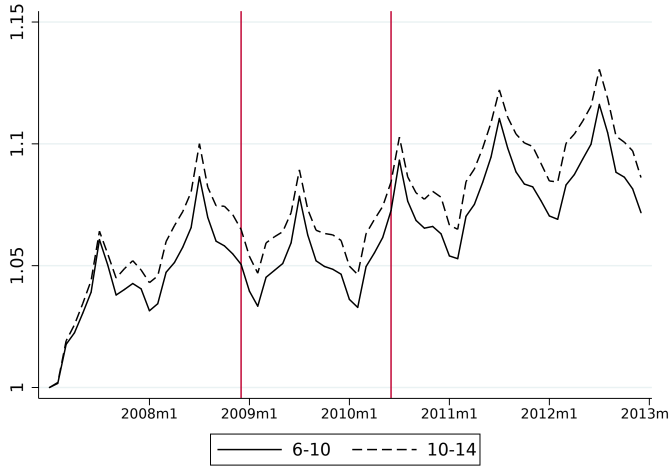
	(1) Non-eligibles	(2) Eligibles	(3) (1) vs. (2)
-26 years	0.188	0.189	-0.001
+40 years	0.404	0.409	-0.004**
< 1.6 Minimum wage	0.620	0.634	-0.014***
Full time	0.821	0.807	0.014***
N	36187	83345	119532

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Source : DADS 2008. Note : Characteristics of employees in non-eligible (1) and eligible (2) firms. P-value of the difference between the two (3).

What can be said naively about the effect of eligibility to the hiring credit when looking at the raw data ? Figure 1.6 compares eligible and ineligible firms from January 2007 to December 2012 in terms of employment when employment is normalized to be equal to 1 in January 2007. The two groups diverge in terms of employment fairly rapidly in 2007. This divergence increases gradually until 2008 but is almost zero by mid-2010. It then increases again.

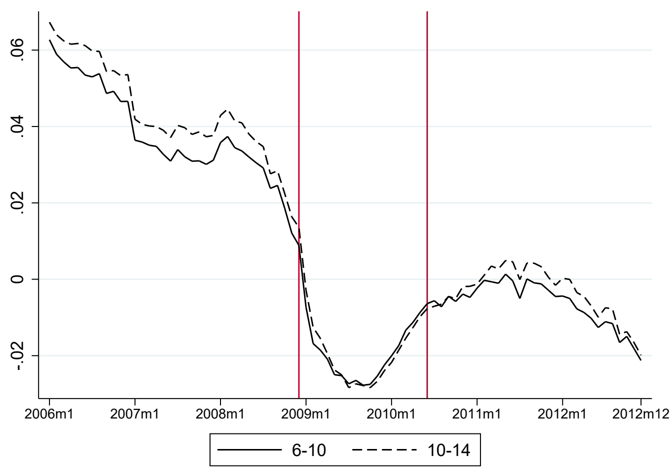
Figure 1.6 – Trends in employment level



Source : DADS 2007-2012. Field : Firms with between 6 and 14 FTE employees in the first eleven months of 2008 and present in December 2012 Note : Here I compare the evolution of total FTE employment from January 2007 to December 2012 between eligible and ineligible firms. Employment is normalised to be equal to 1 in January 2007. The solid line shows month by month the evolution of total employment for eligible firms and the dashed line for ineligible firms.

When comparing the two groups of firms in term of year-on-year growth (Figure 1.7), i.e. looking at employment in a given month compared with the same month the previous year (which has the advantage of cancelling out some of the seasonality observed in Figure 1.6), performances diverge only between 2009 and 2010. It seems that employment grows faster in general in ineligible firms, but that this relationship reverses as long as the credit lasts. Figures 1.6 and 1.7 complement each other : the reconvergence of the two groups of firms in term of employment can be explained by the fact that ineligible firms generally grow faster. The coincidence between the evolution of employment growth and the timing of the 2009 hiring credit is striking.

Figure 1.7 – Trends in employment growth

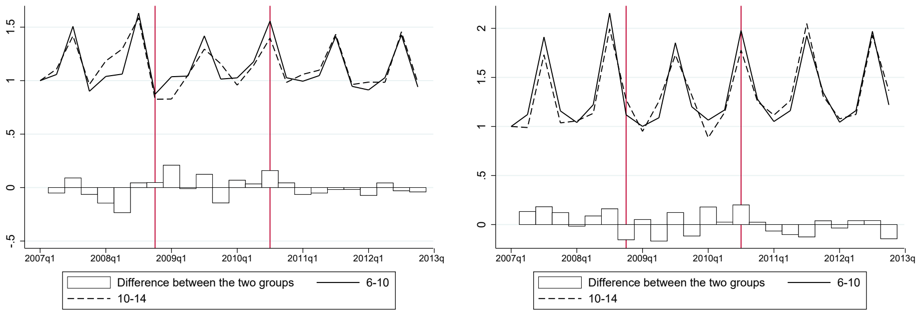


Source : DADS 2007-2012. Field : Establishments with between 6 and 14 FTE employees in the first eleven months of 2008 and present in the DADS in December 2012 Note :. Here I compare the year-on-year FTE employment growth from January 2006 to December 2012 between eligible and ineligible firms. The solid line shows month by month the evolution of eligible firms and the dashed line the evolution of ineligible firms.

On the basis of these raw data, eligible firms do indeed appear to have experienced stronger employment growth during the duration of the credit (Figure 1.7), reversing momentarily their otherwise constant disadvantage in term of employment growth.

To complement this, EMMO provides more information on the dynamics of hires and separations in eligible firms. Figure 1.8 shows the quarterly trend in the number of hires and separations in the EMMO sample.

FIGURE 1.8 – Number of hires and separations per quarter



Source : EMMO 2007-2013. DADS 2008 Field : firms with between 2 and 18 FTE employees in the first eleven months of 2008 and which can be linked to DADS Note : Average number of hirings (left) and separations (right) for firms in the sample from 2008 to 2012. The solid line represents the evolution of eligible firms (2-10 FTE employees) and the dashed line represents the evolution of ineligible firms (10-18 FTE employees).

Figure 1.8 tells us several things :

— In terms of the number of separations and hires, the two groups are very similar prior and after the credit. With a slight advantage for ineligible firms who seem to have an higher hiring rate as well as a lower separation rate (at least before the credit).
— There seems to be a surplus of hires during the credit in eligible establishments. While eligible firms were hiring less, they hired more while the credit lasted, in term of separation rate, the picture is more mixed.

It is difficult though to draw definitive conclusions from a naive comparison between two groups : the difference in dynamism between the two groups could be explained by a different positioning in the business life cycle (younger firms are generally more dynamic) and/or a differentiated exposure to the impact of the 2009 crisis. The objective of the following section is to try to isolate the causal effect of the 2009 hiring credit on employment growth and worker flows.

1.5 Econometric analysis

1.5.1 Methodology

Parallel trends or parallel growth

To study the effect of the 2009 hiring credit on employment in eligible firms, I use a balanced monthly panel of firms between 2007 and 2012. It includes all firms that had between 6 and 14 FTE employees over the first eleven months of 2008, month by month from January 2007 to December 2012. The idea is to compare the evolution of the dependent variables in eligible and ineligible firms relatively to the timing of the implementation of the hiring credit. This approach is similar to that of Cahuc *et al.* (2019) but the main differences are that the definition of the eligibility and ineligibility groups is fixed over time, which allows me to measure the effect of the hiring credit over a longer period of time, and that I try to have a precision at an infra-annual level (month by month). This is the only way to understand whether the effect is due to a reduction of labor cost, a displacement effect or an hiring subsidy. This has also the advantage to guard against the possible reclassification bias in Cahuc *et al.* (2019). Eligible firms in 2010, 2011 and 2012, control periods, are negatively selected compared to eligible firms in 2009, the treatment period. In a double-difference framework, it may lead to overestimate the impact of the hiring credit.

The identification hypothesis of my approach is that the evolution of employment in ineligible firms in 2008 is a good approximation of what would have happened in eligible firms in 2008 if the credit had not existed (a good counterfactual). This is known as the parallel trend hypothesis. In theory, this hypothesis is untestable, but it is often customary to check that the evolution of the dependent variable in the treatment and control groups is similar before treatment. Figure 1.6 shows that this is not the case for employment as both groups of firms seem to diverge before even the implementation of the credit.

Furthermore, as noted in Cahuc *et al.* (2019), there is no theoretical reason why employment patterns would remain the same in the absence of hiring credit for firms of different size. Suppose a production function $F(L)$ with labor cost w. The optimal level of employment is such that $F'(L) = w$. In this case, $dL = L.\epsilon$ where dL is the change in employment following a 1% decrease in labor cost and *epsilon* is the elasticity of labor demand. This means that even in the absence of a hiring credit, a common shock also affecting the labor costs of the two groups of firms will have a different effect on employment. For the same reason, looking at the infra-annual evolution of employment can be risky if seasonal cycles affect groups of firms of different sizes differently.

To get to the heart of the matter, I follow the suggestions of Bilinski et Hatfield (2018). That is, I compare the double difference estimator with that of similar models where the existence of different trends between the treatment and control groups is possible and where, for example, seasonal differences are allowed. I estimate three different models with increasing complexity where Y_{it} is the dependent variable for firm i in month t, knowing that $t = 0$ corresponds to November 2008, i.e. the month preceding the application of the 2009 hiring credit. T_i is a binary variable equal to 1 if firm i was eligible for the credit at the end of November 2008. γ_t and ρ_i are temporal and individual fixed effects.

$$Y_{i,t} = \alpha + \beta.[T_i \times \mathbb{1}\{t \in [1, 16]\}] + \gamma_t + \rho_i + e_{i,t} \tag{0.1}$$

$$Y_{i,t} = \alpha + \beta.[T_i \times \mathbb{1}\{t \in [1, 16]\}] + \delta.[t \times T_i] + \gamma_t + \rho_i + e_{i,t} \tag{0.2}$$

$$Y_{i,t} = \alpha + \beta.[T_i \times \mathbb{1}\{t \in [1, 16]\}] + \delta.[t \times T_i] + \mu_t \times T_i + \gamma_t + \rho_i + e_{i,t} \tag{0.3}$$

In all three models, β is the double difference estimator. In equations 0.2 and 0.3, δ captures the trend differential between the control and treatment groups and thus the validity of the parallel trend hypothesis. Finally, in equation 0.3, $\mu_t \times T_i$ is the interaction between a monthly fixed effect and the eligibility variable. The spirit of the non-inferiority test of Bilinski et Hatfield (2018) is that the estimation of the counterfactual is robust to a violation of the identification hypothesis if the estimate of β is similar in each equations. If I can rule out differences between coefficients above a given threshold, such as the minimum detectable model effect (MDE), then there is a good chance that the potential presence of differential trends will not affect the estimation of the counterfactual. Here I test this approach when the dependent variable is the logarithm of employment.

TABLE 1.4 – Parallel trends - Employment level

	(1) Employment	(2) Employment	(3) Employment
β	.00157	.00095	.00082
	(.00113)	(.00111)	(.00114)
δ		-.00015**	-.00015**
		(.00006)	(.00006)
$\mu_t \times T_i$	No	No	Yes
Difference with (1)		-.00062	-.00075
		(.00158)	(.00160)
N	6678936	6678936	6678936
R^2	0.607	0.607	0.607

Source : DADS 2005-2012. Columns (1), (2) and (3) report the estimation of gradually more complex double-difference models. Column (1) is the standard difference-in-differences model, column (2) adds differential linear trends $\delta.t \times T_i$, allowing for non parallel trends and column (3) adds differential seasonality ($\mu_t \times T_i$).

The credit does not appear to have any effect on the level of employment in eligible firms. However, in models 0.2 and 0.3, δ is significant and a quick calculation proves one cannot exclude that the difference between coefficients is higher than the MDE, thus failing the test of Bilinski et Hatfield (2018). An ex-post measure of the MDE is 0.0043 (at 80% statistical power and 95% confidence level, this is 2.8 times (0.84 + 1.96) times the standard error of the coefficient of interest).

For this reason, I chose to estimate the effect of the credit on year-over-year growth $Y_{it} = \frac{ETP_{it}-ETP_{it-12}}{ETP_{it}+ETP_{it-12}}$ where ETP_{it} is the full-time equivalent employment in the firm i in month t. Rather than parallel trends, the identification hypothesis becomes that of parallel growth (of which the parallel trend hypothesis is a special case) and therefore makes it easier to construct a counterfactual. Figure 1.7 seems to show that employment growth in both treatment and control groups is parallel.

Table 1.5 – Parallel trends - Employment growth

	(1) Growth	(2) Growth	(3) Growth
β	0.00368***	0.00381***	0.00391***
	(0.00099)	(0.00100)	(0.00103)
δ		0.00003	0.00003
		(0.00002)	(0.00002)
$\mu_t \times T_i$	No	No	Yes
Difference with (1)		.00013	.00023
		(.00140)	(.00142)
N	5995944	5995944	5995944
R^2	0.143	0.143	0.143

Source : DADS 2005-2012. Columns (1), (2) and (3) report the estimation of gradually more complex double-difference models. Column (1) is the standard difference-in-differences model, column (2) adds differential linear trends $\delta.t \times T_i$, allowing for non parallel trends and column (3) adds differential seasonality ($\mu_t \times T_i$).

The credit had a positive effect on employment growth. δ is never significantly different from zero, and a quick calculation verifies that the differences in coefficients are significantly lower than the minimum detectable effect. The minimum detectable effect can estimated ex-post to 0.004. Our estimate thus satisfy the criteria set by Bilinski et Hatfield (2018).

Dynamic model

The objective of this paper is to visualize precisely the dynamics of the effect of the hiring credit on employment. Our approach is similar to the dynamic double-difference estimation models [6]. This has been adopted notably by Angrist (1998) and Autor (1998), but also more recently by Bozio *et al.* (2018).

The estimated model is the following :

$$Y_{i,t} = \alpha + \sum_{k=-23}^{48} \delta_k.[T_i \times \mathbb{1}\{t = k\}] + \gamma_t + \rho_i + e_{it} \tag{1}$$

6. An alternative methodology could have been a regression by discontinuity, like Ananian et Pons (2017). The problem here is that the eligibility criterion does not correspond exactly to the size of the firms claiming the credit. The eligibility criterion in the DADS is therefore an approximation of the "true" eligibility criterion. The eligibility criterion is imprecise enough that the discontinuity in the use of the credit disappears, as can be seen from Figure 1.4. This can bias the regression estimator by discontinuity towards zero, or even make it indefinite as precised in Davezies et Le Barbanchon (2017)

The distribution of δ_k, the coefficients of the interaction terms between the eligibility variable T_i and the monthly fixed effects, gives the temporal dynamics of the effect of the credit on employment growth. Indeed, δ_k estimates the difference in FTE employment growth between eligible and ineligible firms in month k. Assuming that employment growth trends would have remained the same in the absence of the hiring credit when $k > 0$, then δ_k can be interpreted, when $k > 0$, as the measure of the impact of credit eligibility on year-on-year employment growth after k months.

Note that the estimates may be contaminated by a mean-reversion bias. Some eligible firms may be so because they have experienced exceptionally poor performance in 2008 and conversely for some ineligible firms. This can positively bias the estimate if firms in this case perform back to average the following year. Another obstacle is the one mentioned by Moscarini et Postel-Vinay (2012) : the largest firms grow more in line with the economic cycle than the smallest ones. This can give the illusion, because 2009 and 2010 are periods of recession, of a negative relationship between size and employment growth. I then check, thanks to several placebo experiments, that our results are not reproducible for other size categories or other periods. Moscarini et Postel-Vinay (2012) raise also another issue : a reclassification bias. A negative relationship between eligibility and employment growth could be observed if the largest firms become eligible by reducing their employment levels during the recession. This is not a problem in our case because the composition of the treatment and control groups remains fixed with respect to their eligibility in 2008 and therefore does not change from 2008 to 2012.

I also use the EMMO to study the impact of the hiring credit on the flow of hires and separations. The following model is estimated :

$$Y_{iq} = \alpha + \sum_{k=-7}^{16} \delta_k . [T_i \times \mathbb{1}\{t = k\}] + \gamma_q + \rho_i + e_{iq} \tag{2}$$

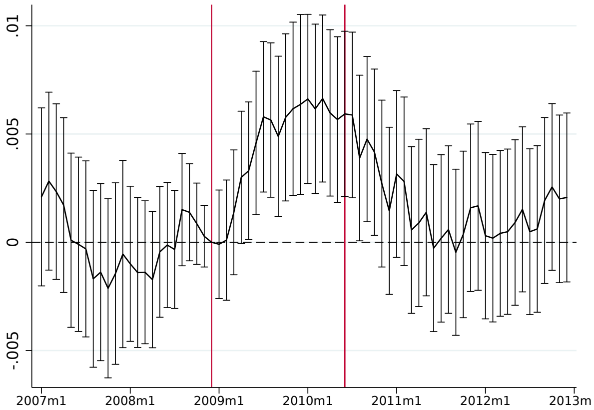
where Y_{iq} is the number of separations or hires in quarter q for firm i. T_i is a binary variable equal to 1 if firm i was eligible for the credit in November 2008. γ_t and ρ_i are individual quarterly fixed effects. This model is very similar to the previous one, however the stock of eligible and ineligible firms changes each year depending on the firms surveyed in the EMMO. Standard errors are robust and aggregated at the firm level.

1.5.2 Results

FTE employment growth

The results from the estimation of equation (1) are presented graphically in Figure 1.9, in order to show how the effect of the hiring credit on employment growth evolves from December 2008 to December 2012.

Figure 1.9 – Differential evolution of employment growth since January 2008 per month between eligible and ineligible firms



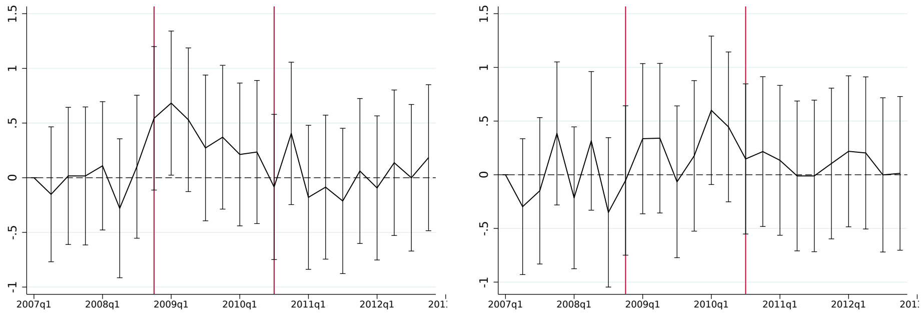
Source : DADS 2007-2012. Field : Firms with between 6 and 14 FTE employees in the first 11 months of 2008
Note : The solid line reproduces the monthly evolution of the δ_k coefficients estimated by the model. Each point is associated with its 95% confidence interval.

Figure 1.9 is a graphical representation of the results when the dependent variable is the year-on-year growth of FTE employment. The area delimited by the two red vertical lines corresponds to the period of application of the 2009 hiring credit (12/2008-06/2010). Hires outside this period are not subsidised differently between eligible and non-eligible firms. The hiring credit has therefore had a significant effect on employment growth. Between December 2008 and June 2010, employment growth in eligible and ineligible firms diverged : more than half a percentage point difference in June 2010. The two groups converge again afterwards, the estimated δ_k's are no longer significantly different from zero from 2011 onward. This means that the employment dynamics in eligible firms returned to normal with the end of the credit.

Transitions

I graphically present the results of the estimation of the equation (2) on worker flows using the EMMO database in Figure 1.10.

FIGURE 1.10 – Results Model (2) : Number of hires and separations



Source : EMMO 2007-2013. DADS 2008 : Establishments with between 2 and 18 FTE employees in the first eleven months of 2008 that can be linked to DADS Note : The solid line shows the evolution of the δ_k estimated by the model (2). To each point is associated its 95% confidence interval. The dependent variable is the number wth of hires on the left and of separations on the right.

Figure 1.10 shows that the credit seems to have a significant effect on worker flows in eligible firms during the quarters of application of the 2009 hiring credit. The number of hires increases significantly in the first quarter of application and remains high thereafter. Outside the period of application of the credit, differences between eligible and ineligible firms are minimal. The number of hires increases in line with the observed increase in the employment growth in Figure 1.9. These results show the positive effect on employment is caused by a surplus of hires.

1.5.3 Discussion

To summarize the results presented in the previous sections :

— Eligible firms experienced higher employment growth starting in December 2008 compared to ineligible firms but this growth differential decreases as soon as the credit ends and is close to zero by 2011 : Figure 1.9.

— Looking at labor flows in eligible and ineligible firms shows that eligible firms had a surplus of new hires when the credit was introduced : Figure 1.10.

These results seem to show that the credit had an effect on worker flows in eligible firms. Eligible firms hired more when they were able to profit from the credit. This may be due to the temporary nature of the 2009 hiring credit, comparable but permanent hiring credits do not have this kind of effect. For example, in the case of Saez *et al.* (2017) in Sweden a permanent hiring credit led to a reduction in the separation rate of eligible workers.

The hiring credit effectively functioned as a temporary adjustment subsidy, a windfall for the firms that benefited from it, leading to a surplus of hires. Job creations thanks to the credit were almost permanent. Some risk-averse employers are reluctant to hire when the marginal productivity of the future position is too uncertain. The credit may have caused the employer to take more risk as, in the context of the 2009 crisis in France, every other signals told them not to. I try to give some evidence of the counter-cyclical nature of the hiring credit in section 1.5.3. Two additional remarks. First, there was no comparable increase in separations in parallel to the increase in hires. This means that the additional hires are not of lower quality, but also that subsidizing hires does not mean subsidizing separations as well. Firms are likely to be present biased. Second, job creation was in the end higher than it would have been if the credit had been equivalent to a labor cost reduction because created jobs are not transitory. In section 1.5.3, I show the cost per month of work created is two times lower if we take into account the persistence of jobs after the end of the hiring credit.

A counter-cyclical policy ?

There are a lot of reasons why one would expect the hiring credit to be a counter-cyclical policy. The hiring credit allows firms to overcome their adjustment costs and, in line with Hall (2017), one might think part of adjustment costs are procyclical because the risk perception of employers is as well. Consequently, one expects that in sectors that are the most impacted by the crisis, the difference between eligible and ineligible firms will be starker thanks to the effect of the hiring credit. To test this hypothesis, we estimate the following model :

$$Y_{i,t} = \alpha + \sum_{k=-23}^{48} \delta_k.[T_i \times Impact_i \times \mathbb{1}\{t = k\}] + T_i \times \gamma_t + T_i \times Impact_i + Impact_i \times \gamma_t + \rho_i + e_{it} \quad (3)$$

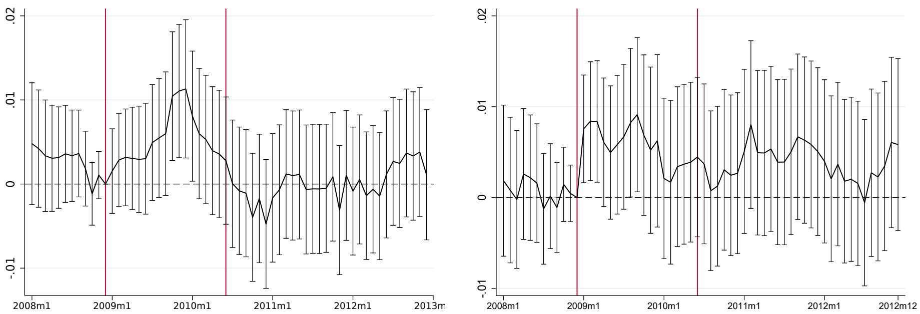
where $Impact_i$ is either i) a variable that measures whether a firm is in a sector where small firms were severely impacted by the crisis in 2009 ii) a variable that measure whether a firm is in a place that was severely impacted by the crisis in 2009. In the first case, it is a dummy variable equal to one for sectors [7] where the evolution of total value added between 2008 and 2009 of firms between 14 and 50 FTE employees (and then which are excluded from our analysis) is below the median. In the second case, it is a dummy variable equal to one for employment areas [8] where the evolution of total value added between 2008 and 2009 of firms between 14 and 50 FTE employees (and then which are excluded from our analysis) is below the median. We control for $T_i \times \gamma_t$, i.e. the interaction between our eligibility variable and the monthly fixed effects, to take into account time specific shocks in eligible and ineligibles firms ; we control for $Impact_i \times \gamma_t$, i.e. the interaction between our impact variable and the monthly fixed effects, to take into account time specific shocks in more and less impacted sectors and finally we control for ρ_i, firm fixed effects.

The distribution of δ_k's gives the temporal dynamics of the differential effect of the credit on employment growth between sectors or areas which were more and less impacted by the crisis in

7. NAF code with two digits, 88 different sectors.
8. There are 297 different employment areas in Metropolitan France.

2009. This approach is similar to a difference-in-difference-in-differences approach. If the hiring credit is indeed counter cyclical then we expect δ_k's to be significantly higher than 0 during the period of implementation of the hiring credit. Figure 1.11 shows that it is indeed the case, the hiring credit was more efficient at increasing employment growth in sectors and employment areas where production fall the most in 2009.

FIGURE 1.11 – Differential evolution of employment growth since January 2008 per month between eligible and ineligible firms in more or less impacted sectors/areas



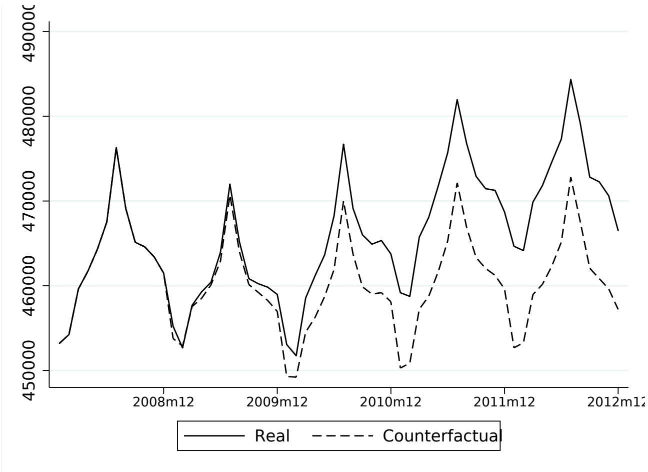
Source : DADS 2007-2012. Field : Firms with between 6 and 14 FTE employees in the first 11 months of 2008
Note : The solid line reproduces the monthly evolution of the δ_k coefficients estimated by the model with sectors on the left and employment areas on the left. Each point is associated with its 95% confidence interval.

Cost by job creation

An important question remains about whether the cost per job created by the hiring credit is sustainable. To estimate the cost per job, I do a fairly simple exercise. I neglect the fact that the subsidy was extended beyond 2009 and I use the estimated coefficients in Figure 1.9 up to December 2012, they allow us to compare the evolution of employment in the eligible firms with what would have happened without the credit : the counterfactual.

I estimate $E\hat{TP}_t^C$ where it is the employment predicted by our model in the eligible firms without the credit and compare it with ETP_t, the observed level of employment in eligible firms to measure the impact of the hiring credit on employment. I can estimate the counterfactual employment evolution by using the coefficients estimated with equation (1). Let δ_t be the coefficients estimated in our regression and g_t the observed year to year growth every month, I have $E\hat{TP}_t^C = E\hat{TP}_{t-12}^C + (g_t - \delta_t) \times E\hat{TP}_{t-12}^C$. Prior to the implementation of the credit, $E\hat{TP}_t^C$ and ETP_t are assumed to be similar, I thus estimate $E\hat{TP}_t^C$ from actual observations up to November 2008 and after thanks to our parameters. The following figures compare the evolution of the real and counterfactual employments.

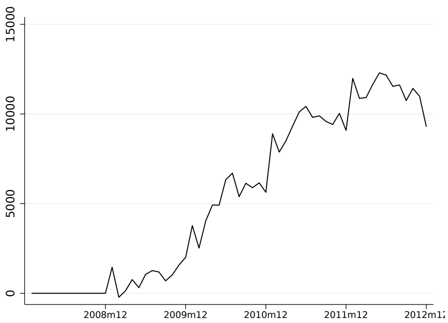
Figure 1.12 – Actual and counterfactual employment

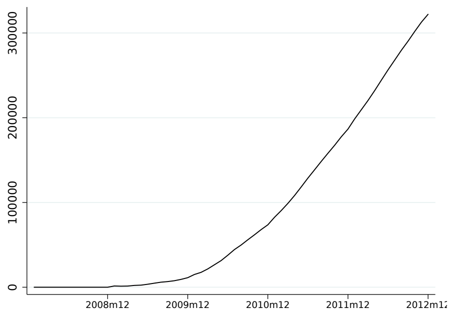


Source : DADS 2007-2012 Field : Establishments between 6 and 10 FTE employees in the first eleven months of 2008 Note : Evolution of FTE employment for eligible firms with and without the credit as predicted by the model.

As can be seen from Figure 1.12, the beneficial effect of the 2009 hiring credit does not end with the credit, as the people hired thanks to it continue to work. Additional months of work created by the credit increase after June 2010. Figure 1.13 helps to assess what this represents in terms of jobs created and the total number of working months that have been added thanks to the hiring credit in December 2012. It is estimated that there would have been 10,000 fewer people employed in eligible firms without the 2009 hiring credit. Cumulatively, at the end of 2012, this represents almost 300,000 months of full-time work thanks to the credit.

Figure 1.13 – Job creation and total number of additional months of work thanks to the credit

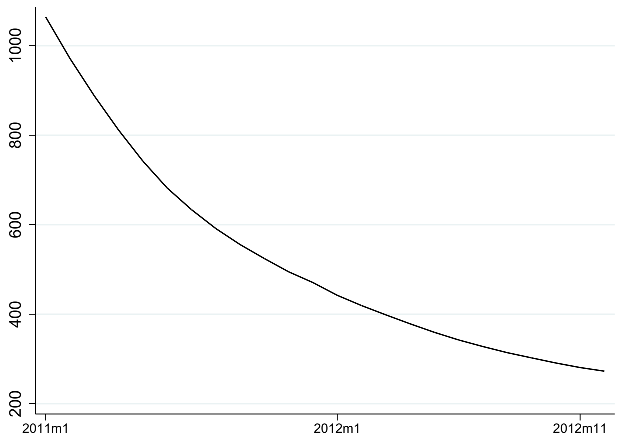




Source : DADS 2007-2012 Note : On the left is shown the difference between the predicted and counterfactual evolution of employment. This is the number of jobs created thanks to the credit. The right-hand side shows the development of the total number of months worked thanks to the credit.

Thanks to the Pôle Emploi monitoring file, I am also able to have the exact amount of aid transfered to the firms in the sample. By calculating the ratio between this figure and the total number of months worked thanks to the credit, I can therefore estimate the gross cost per additional month worked thanks to the credit. Given that 87,835,611 € were paid to eligible firms in our sample and that at the end of 2012 there were 322,073 months worked thanks to the credit, the cost per month of additional work is 273 €. The cost per stable job created is close to 90,000 €. This gross cost does not take into account savings in terms of unemployment insurance and other aids paid by the French State. This is lower than the Cahuc *et al.* (2019) estimate of 700 €per month of additional work, partly because my estimation is based only on the firms in my sample and takes into account the sustainability of the jobs created. Considering only the months worked thanks to the credit in July 2011 (138,579), i.e. the month when the very last payments due to the credit were made, leads to a similar estimate : 664 €. The cost of labor (super gross salary minus the Fillon relief) at the minimum wage level at the end of 2012 in France was 1610 € : the credit thus allows job creation for a little less than 16% of this cost. Figure 1.14 allows to visualize how the temporal horizon change our estimation of the cost per month of additional work : it plots the evolution of this estimation as the temporal horizon broaden. As we move the needle further away from the end of the hiring credit, the cost per month of additional work is decreasing : from more than 1000 € to our final estimation of 273 €.

FIGURE 1.14 – Cost per additional months of work and temporal horizon



Source : DADS 2007-2012 Field : Establishments between 6 and 10 FTE employees in the first eleven months of 2008 Note : Evolution of the estimation of the cost per additional months of work (y-axis) as the temporal horiozn increases (x-axis).

1.6 Robustness tests

1.6.1 Selection in the sample

As mentioned above, in our main approach we focused on a balanced panel of firms in order to capture the long term effect of the hiring credit on employment and so that the evolution of our coefficients would not be driven by composition effects because of firms exiting our groups of analysis. The drawbacks of such approach is that it may create a selection bias. Smaller firms in

2008 that survive the crisis may be on average more resilient and it may affect their employment growth trends during the crisis and the period of implementation of the hiring credit. Thus our coefficients may not necessarily isolate the effect of the hiring credit but may just reflect differential sample selection.

To check whether this might be a problem, I conduct the two following analysis on the unbalanced sample of firms between 6 and 14 FTE employees in the first 11 months of 2008. First of all, I estimate equations 0.1 and 0.2 from December 2008 to December 2012 where eligible firms are considered as treated while the hiring credit was effective i.e. from December 2008 to June 2010 when the dependant variable is a dummy variable equal to 1 when a firm exits the sample in the following month. The coefficient of interest, presented in Table 1.6, measures then the effect of the credit (or any group-specific shocks with the same timing) on the probability of exiting the sample of analysis. Second, in Figure 1.15, I reproduce the same approach as in Figure 1.9, but when dependant variable is a dummy variable equal to 1 when a firm exits the sample in the following month. Figure 1.15 shows the evolution the differential probability to exit the sample from December 2008 to December 2012.

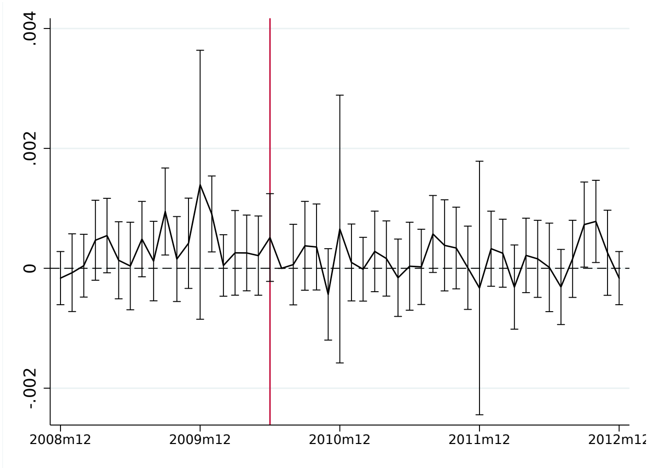
Table 1.6 – Difference-in-difference : probability of exiting the sample

	(1) Exit	(2) Exit
β	.00020	.00036
	(.00011)	(.00021)
δ		.00001
		(.00001)
Difference with (1)		.00016
		(.00016)
N	5847121	5847121
R^2	0.035	0.035

Source : DADS 2008-2012. Columns (1), (2) report the estimation of gradually more complex double-difference models. Column (1) is the standard difference-in-differences model, column (2) adds differential linear trends $\delta.t \times T_i$, allowing for non parallel trends. The dependant variable is the probability to exit the sample the next month.

Table 1.6 and Figure 1.15 show that group specific shocks that may happen while the hiring credit was implemented do not seem to affect significantly the probability of exiting the sample. Table 1.6 demonstrates that, even when accounting for differential trends, the probability of exiting the sample does not seem to increase or decrease during the period of implementation of the hiring credit. Figure 1.15 even shows that the differential probability of exiting the sample during the whole period of analysis is very small and almost never significantly different from 0.

Figure 1.15 – Differential incidence of exits between eligible and ineligible firms



Source : DADS 2008-2012. Field : Firms with between 6 and 14 FTE employees in the first 11 months of 2008
Note : The solid line reproduces the monthly evolution of the δ_k coefficients estimated by the model when the dependant variable is the probability of exit. Each point is associated with its 95% confidence interval.

But this absence of result could be explained because the statistical power of the estimation model is too weak to capture the punctually small but cumulatively important effect of hiring credit on firm survival. Over time, a small difference could accumulate and generate a significant selection bias. To remedy this, I estimate the distribution of δ_k in equation (1) the unbalanced panel of firms which were existing in January 2007 and November 2008 thus possibly capturing the cumulative effect of the hiring credit on attrition in addition to its effect on employment. As it can be seen in Figure 1.16, the distribution of δ_k is similar to one with the balanced panel in Figure 1.9. This is another piece of evidence that a selection bias because of differential attrition in treated and control groups do not explain our main results.

Figure 1.16 – Differential evolution of employment growth since January 2008 per month between eligible and ineligible firms in the unbalanced panel

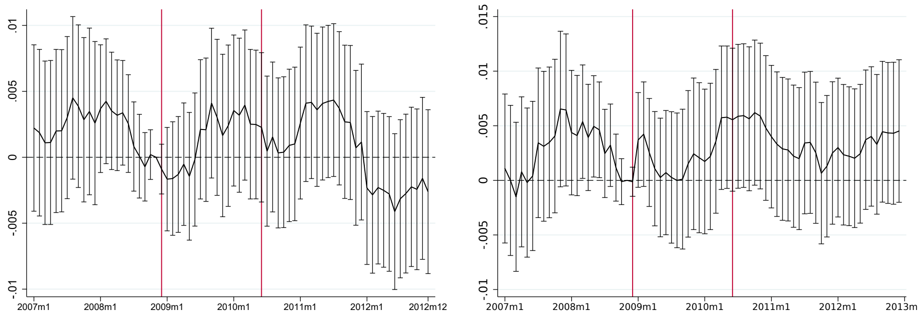
Source : DADS 2007-2012. Field : Firms with between 6 and 14 FTE employees in the first 11 months of 2008
Note : The solid line reproduces the monthly evolution of the δ_k coefficients estimated by the model. Each point is associated with its 95% confidence interval.

1.6.2 Placebo tests

The particular context of the hiring credit and the originality of the eligibility criterion may give rise to fears that its effect is not captured because of a bias of some kind. The credit take-up rate in eligible firms is only 30% (Figure 1.4).

To start with, the basic model was therefore reproduced using a different threshold : 20 FTE employees. I compare firms with 16 to 20 FTE employees (which are here the pseudo eligible group) and firms with 20 to 24 FTE employees (a pseudo control group). These two groups are not affected by the credit, so no difference between the two should be captured by applying an identical model to the one estimated so far. Coefficients (visible in Figure 1.17) are never significantly different from 0. Note that this does not provide definitive proof : the study sample is much smaller than for the previous analyses and the estimation is therefore more imprecise.

FIGURE 1.17 – Placebos : 10-15 vs. 15-20 16-20 vs. 20-24 FTE employees in 2008

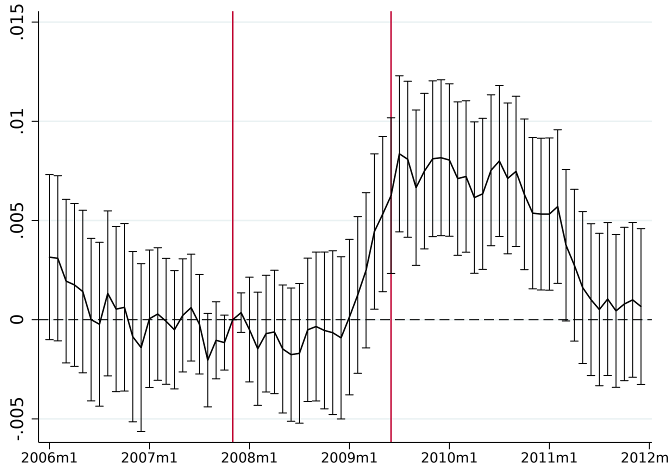


Source : DADS 2007-2012 Field : Establishments between 10 and 24 FTE employees in the first eleven months of 2008 Note : Reproduction of Figure 1.9 but comparing firms i) with 10-15 FTE employees with those with 15-20 employees and ii) with 16-20 FTE employees with those with 20-24 employees (not 6-10 vs. 10-14).

Going further, I estimate the effect of a pseudo hiring credit exactly similar to that of 2009 but if it had occurred in 2008. As for the 2009 hiring credit, I show the impact of this placebo credit on employment growth month by month from January 2006 to the end of 2011 in Figure 1.18.

Since eligibility in November 2007 is correlated to eligibility at the end of 2008, similar developments can be observed in 2009 and 2010 compared to Figure 1.9. No positive effect is observed in the first year after the pseudo-hiring-credit as in the case of eligibility for the credit at the end of 2008.

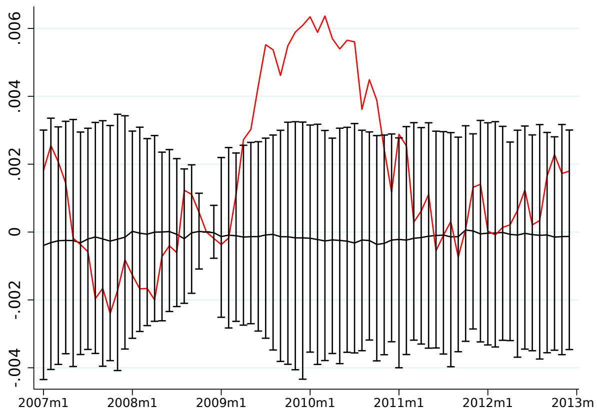
Figure 1.18 – Placebo : reform en 2008



Source : DADS 2006-2012 Field : Establishments between 6 and 14 FTE employees in the first eleven months of 2007. Reproduction of Figure 1.9 for a pseudo-reform beginning in 2007.

1.6.3 Randomization inference

Figure 1.19 – Randomized inference : sampling distribution of the model according to eligibility and comparison with original results



Source : DADS 2007-2012 Field : Establishments between 6 and 14 FTE employees in the first eleven months of 2007. On the one hand is represented the distribution (5th percentile, mean, 95th percentile) in black of the model estimates (1) when eligibility is randomized in the original sample (200 draws). The red line traces for comparison the coefficients in the Figure.

Estimations with panel datasets use many years of observations with serially correlated outcomes may lead to inconsistent standard error as shown in Bertrand *et al.* (2004). I adapt the randomized inference method suggested by Bertrand *et al.* (2004). Specifically, firms in the treatment and control groups were randomly assigned and 200 different monthly distributions of the effect of the credit between "pseudo-eligible" and "pseudo-ineligible" firms were estimated. The advantage of this permutation test is also that it allows us to determine the inference of the estimate in Figure 1.9 in a non-parametric way. It is also a good way to estimate the minimum detectable effect for each period.

Figure 1.19 compares the original distribution of the effect of the credit on growth in red with the black distributions of the estimates from 200 different random draws of the treatment status : the 95% confidence interval and the mean of the estimated coefficients. In black one has the field of possible estimates of our method when one randomly distributes the treatment status. When our estimates are included in this field, they cannot be considered as statistically significant. The original coefficients are above the 95th percentile of the distribution during the duration of the credit.

1.7 Conclusion

The 2009 hiring credit enabled French firms with fewer than 10 employees to reduce significantly their hiring costs from the end of 2008 to July 2010. By exploiting the DADS, I am able to accurately estimate the dynamics of the effect of the credit on employment growth since its introduction. I confirm it had a positive effect on employment : in July 2010 employment growth was more than half a point higher in firms eligible for the 2009 hiring credit compared to ineligible firms. Furthermore, by exploiting the EMMO, I show this employment effect is explained by a surplus of new hires in 2009.

I argue the 2009 hiring credit led to job creation because of the particular change of hiring costs during recessions. The rise of the risk premium of employer in recession is one of the main channel through which labor markets are negatively impacted. This leads to a more than optimal fall of hires that ultimately provoke a risk of the unemployment. Hiring credits could act precisely at this level and limit the negative impact of the rise of the employer risk premium. These effects are not quite what is expected in the literature on hiring subsidies, because they are typically equated to a particular form of wage subsidies (see Katz (1996) for example). In a recession, subsidizing adjustment may be more efficient in the long term than wages because its effect will survive the subsidy. On the contrary, outside of recession, when employers do not hesitate to take risks, subsidizing adjustment is likely to increase churning with a much lower net effect on employment. Precisely because of the permanence of its effect, the credit has been a relatively effective employment support mechanism for small businesses. I estimate it may have resulted in a cost per job created of about 90,000 € and, taking into account the permanence of these jobs, a cost per month of additional full-time work of 273 €, a little less than 16% of the cost of labor for a worker at the minimum wage level in France. Subsidizing hiring directly is a potentially effective weapon to support employment in crisis situations.

Annexe 1.A Measure of eligibility criterion

FIGURE 1.20 – Extract from the Cerfa form for obtaining the hiring credit

3 - Appréciation des effectifs

L'effectif de l'entreprise est apprécié au 30 novembre 2008, tous établissements confondus. Il est égal à la moyenne des effectifs déterminés pour chacun des onze premiers mois de l'année 2008.

Pour une entreprise créée entre le 1er janvier 2008 et le 30 novembre 2008, l'effectif est apprécié sur la moyenne des effectifs de chacun de ses mois d'existence.

Pour une entreprise créée entre le 1er décembre 2008 et le 31 décembre 2009, l'effectif est apprécié à la date de sa création.

Pour la détermination de la moyenne des effectifs, les mois au cours desquels aucun salarié n'est employé ne sont pas pris en compte.

Exemple : Une entreprise, créée en 2005, qui a embauché son premier salarié en février 2008, ne comptabilise son effectif que sur dix mois, en fonction de l'effectif en fin de mois, de février à novembre 2008.

Pour la détermination des effectifs du mois, il est tenu compte des salariés titulaires d'un contrat de travail le dernier jour de chaque mois, y compris les salariés absents, conformément aux dispositions des articles L. 1111-2, L. 1111-3 et L. 1251-54 du code du travail. Ainsi :

- **Les salariés à temps partiel**, quelle que soit la nature de leur contrat de travail (CDD, CDI, intérimaires, etc.), sont pris en compte à proportion de la durée du travail prévue dans leurs contrats de travail par rapport à la durée légale ou collective conventionnelle du travail.

Exemple : Un salarié à mi-temps n'est compté que pour 0,5. S'il fait 14 heures par semaine en moyenne dans une entreprise à la durée collective de 35 heures, il est compté pour (14/35)= 0,4. Un cadre en forfait jours à 109 jours par an représente (109/218)= 0,5.

- **Les salariés en CDI** sont pris en compte intégralement, même s'ils sont arrivés en cours de mois.

Exemple : Un salarié en CDI à temps complet embauché le 20 du mois est dans l'effectif en fin de mois et est compté pour 1 au titre de ce mois. A 80% d'un temps complet, il aurait été compté pour 0,8.

- **Les salariés en CDD, les intermittents et les salariés temporaires** sont pris en compte à due proportion de leur temps de présence au cours du mois. Ils sont cependant exclus de l'effectif s'ils remplacent un salarié absent ou dont le contrat de travail est suspendu (congé maternité, d'adoption, parental d'éducation, etc.).

Exemple : Un salarié en CDD embauché à temps complet en milieu de mois est compté pour 0,5. A mi-temps, il aurait compté pour 0,25, soit 0,5 (mois) x 0.5 (mi-temps).

S'il remplace un salarié absent, le salarié absent est compté, mais pas le salarié en CDD.

- **Ne sont pas retenus dans le calcul de l'effectif :** les apprentis, les salariés titulaires d'un contrat initiative emploi, d'un contrat d'avenir, d'un contrat d'accompagnement dans l'emploi, d'un contrat d'insertion revenu minimum d'activité, et d'un contrat de professionnalisation lorsqu'il est conclu à durée déterminée ou pendant toute la durée de l'action de professionnalisation lorsqu'il est conclu à durée indéterminée.

Exemple : Une entreprise qui, en fin de mois, emploie 13 salariés à temps complet, dont trois apprentis et un salarié en contrat initiative emploi, a un effectif de 9 salariés au titre de ce mois.

Figure 1.20 shows an extract of the form that allows a firm to apply for the credit and more specifically instructions for calculating the number of employees in order to determine eligibility. It is recommended that firms calculate an FTE headcount for each month and then take the mean of the first 11 months of the year. To measure eligibility for the credit, I follow these instructions using the start and end dates of employment periods reported by firms in the DADS.

For each position held, I multiply the number of days worked in a given month by its working time quota (the ratio of hours worked by the employee to hours worked by a full-time employee in the sector concerned). An employee who works full-time 30 days in the month has an FTE of 1, one who works part-time 0.5 and one who works full-time 15 days also 0.5. Summing up this measure for each month and for each position per firm makes it possible to trace the evolution of its FTE workforce with monthly precision.

Annexe 1.B Use of the hiring credit

Figure 1.21 reproduces the evolution of the number of hires benefiting from the ATPE each month. The red line marks the extension of the credit in December 2009. From this point on, each new hire benefits from the credit for twelve months, whereas a hire in November 2009 only benefited from the hiring credit for three months (until January 2010). Despite the increase in the money received thanks to the credit, if one compares the first 6 months of 2009 with those of 2010, the figures are almost identical. firms seem to use the credit mainly according to their need for work, not the number of months compensated. Only June 2010, the last month in which hires

Figure 1.21 – Evolution of the number of subsidized hires

Source : ATPE Monitoring File Note : The solid line shows the evolution of the number of hires subsidized through the ATPE from December 2008 to June 2010. The vertical red line marks the extension of the credit in November 2009.

were eligible for the credit, stands out. There were around 60,000 hires benefiting from the credit in June 2009 compared to more than 80,000 in June 2010.

Annexe 1.C Selection in the EMMO sample

Table 1.7 – Characteristics of employees of eligible and ineligible firms in 2008

	(1) Non-eligibles	(2) Eligibles	(3) (1) vs. (2)
Effectif moyen	15.067	9.726	5.341***
Taux séparation	0.051	0.052	-0.001
Taux embauches	0.070	0.080	-0.009***
-26 ans	0.175	0.185	-0.010***
+40 ans	0.417	0.413	0.004
-1.6 SMIC	0.621	0.647	-0.026***
Temps plein	0.754	0.688	0.065***
N	12914	16663	29577

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

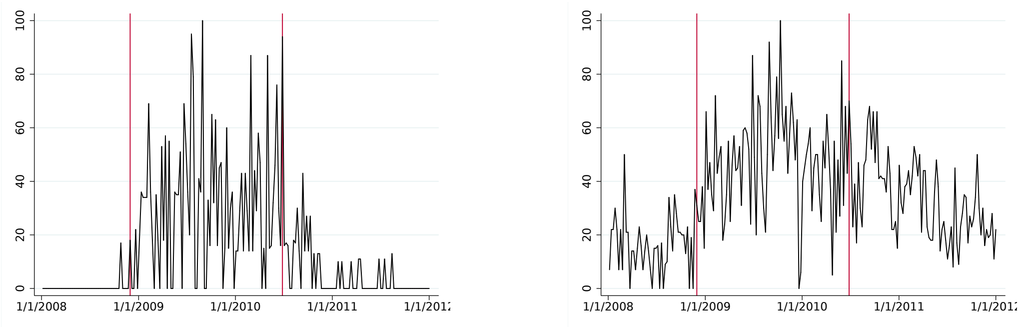
Source : EMMO 2008-2012 and DADS 2008. Note : Comparison (3) of the characteristics of the employees of non-eligible (1) and eligible (2) firms in the EMMO sample.

Table 1.7 compares eligible and ineligible firms at their first observation in the EMMO sample, i.e. the firms from the first sample that can be linked to the EMMO. Again, firms differ greatly. Eligible firms have more employees paid below 1.6 SMIC (+2.6 points) and fewer employees working full time (-6.5 points).

Annexe 1.D Anticipations

There is a risk that the credit has been anticipated by firms. This could lead some firms to manipulate their size or the timing of hires in order to take advantage of the credit. It is unlikely the credit was indeed anticipated. The measure was absent from the president to be Sarkozy's agenda in 2007 and from the debates around employment policies in 2008. This can be verified by retrieving data from Google searches [9]. For example, two terms, chosen for their syntactic proximity with the official name of the credit, "Zero charge" and especially "Aide Embauche", hardly appear in Google searches before the announcement of the credit in December 2008 (Figure 1.22).

FIGURE 1.22 – Number of Google searches per week from 2008 to 2012 for two terms related to the 2009 hiring credit



Source : Google Trends. Note : Number of Google searches per week between 2008 and 2011 for certain terms : "Zero charge" (left) and "Aide Embauche" (right). Cahuc *et al.* (2019) produce similar figures.

Moreover, in the case of an anticipation by firms, one can imagine their probable reaction : a manipulation of the size of employment in order to benefit from the credit and therefore an increase in the number of separations in eligible firms before the credit is introduced. Figure 1.8 does not show such a development.

9. The link to the search in question

Bibliographie

Yann ALGAN, Bruno CRÉPON et Dylan GLOVER : The value of a vacancy : Evidence from a randomized evaluation with local employment agencies in france. *Chaire securisation des parcours professionnels*, 2018.

Sévane ANANIAN et Sébastien PONS : L'impact du dispositif «zéro charges» de 2009 sur les embauches des très petites entreprises. 2017.

Joshua D ANGRIST : Estimating the labor market impact of voluntary military service using social security data on military applicants. *Econometrica*, pages 249–288, 1998.

David H. AUTOR : Outsourcing at will : The contribution of unjust dismissal doctrine to the growth of employment outsourcing. *Journal of Labor Economics*, 21 (1):1–42, 1998.

Marianne BERTRAND, Esther DUFLO et Sendhil MULLAINATHAN : How much should we trust differences-in-differences estimates ? *The Quarterly journal of economics*, 119(1):249–275, 2004.

Alyssa BILINSKI et Laura A HATFIELD : Seeking evidence of absence : reconsidering tests of model assumptions. *arXiv preprint arXiv :1805.03273*, 2018.

Antoine BOZIO, Thomas BREDA et Julien GRENET : Incidence of social security contributions : evidence from france. 2018.

Pierre CAHUC, Stéphane CARCILLO et Thomas LE BARBANCHON : The effectiveness of hiring credits. *The Review of Economic Studies*, 86(2):593–626, 2019.

Nila CECI-RENAUD et Paul-Antoine CHEVALIER : L'impact des seuils de 10, 20 et 50 salariés sur la taille des entreprises françaises. *Économie et statistique*, 437(1):29–45, 2010.

Robert CHIRINKO et Daniel WILSON : Job creation tax credits, fiscal foresight, and job growth : Evidence from us states. 2016.

Bruno CRÉPON, Esther DUFLO, Marc GURGAND, Roland RATHELOT et Philippe ZAMORA : Do labor market policies have displacement effects ? evidence from a clustered randomized experiment. *The quarterly journal of economics*, 128(2):531–580, 2013.

Laurent Davezies et Thomas Le Barbanchon : Regression discontinuity design with continuous measurement error in the running variable. *Journal of Econometrics*, 200(2):260–281, 2017.

Robert E Hall : High discounts and high unemployment. *American Economic Review*, 107 (2):305–30, 2017.

Lawrence F Katz : Wage subsidies for the disadvantaged. Rapport technique, National Bureau of Economic Research, 1996.

Alan Manning : *Imperfect competition in the labor market*, volume 4. Elsevier, 2011.

Giuseppe Moscarini et Fabien Postel-Vinay : The contribution of large and small employers to job creation in times of high and low unemployment. *American Economic Review*, 102(6):2509–39, 2012.

David Neumark et Diego Grijalva : The employment effects of state hiring credits. *ILR Review*, 70(5):1111–1145, 2017.

Claude Picart : Flux d'emploi et de main-d'oeuvre en france : un réexamen. *Economie et statistique*, 412(1):27–56, 2008.

Emmanuel Saez, Benjamin Schoefer et David Seim : Payroll taxes, firm behavior, and rent sharing : Evidence from a young workers' tax cut in sweden. 2017.

CHAPITRE 2

Termination of Employment Contracts by Mutual Consent and Labor Market Fluidity

Cyprien Batut, Eric Maurin [1]

1. We would like to thank Eve Caroli, Dominique Goux and Sara Signorelli for helpful comments on early versions of this paper as well as participants at the EEA conference in Manchester (2019) and at the IAAEU workshop on job security and employment protection (Trier, 2020). This paper was previously circulated under the title "From ultima ratio to mutual consent : the effect of changing employment protection doctrine".

Abstract :

In many countries, the termination of employment contracts has to be either on employer initiative or on employee initiative. In 2008, the French government introduced a change in doctrine : it became possible to terminate employment contracts by mutual consent at a lower cost. We show that the reform was followed by a very significant increase of about 20% in outflow of permanent workers as well as by the replacement of around 10% of dismissals for cause by terminations by mutual consent. By promoting terminations by mutual consent, the reform has improved labor market fluidity and reduced the risks of labor disputes.

JEL Classification : J23, J52, J63.

Keywords : employment termination, dismissal, quit, labor litigation, severance payment.

2.1 Introduction

In many countries, especially in Europe, the termination of employment contracts can be either on employer initiative or on employee initiative, there is little alternative [2]. Furthermore, the cost of the procedure is borne mainly by the contracting party who initiate the separation. For example, when an employee takes the initiative and decides to quit, he or she typically loses eligibility to receive unemployment benefits, whereas the employer bears no direct costs. By contrast, when an employer decides to dismiss an employee, the employer has typically to observe a notice period, pay a severance package and be able to prove that the circumstances of the dismissal correspond to circumstances under which it is legally possible to dismiss workers. Eventually, the employer bears the risks of being sued for unfair dismissal, especially when the dismissal cannot be justified by clear economic difficulties.

These legal constraints on job separation aim at protecting each one of the two contracting parties from the various problems involved by enduring an unexpected separation. One issue with these constraints, however, is that they may discourage worker reallocation and hamper productivity growth [3]. Also, because they make it difficult to share the costs and liability of separations, existing rules can be a source of conflicts between the two contracting parties. In particular, when employers take the initiative, they cannot avoid stigmatizing the employees that they dismiss, especially when these dismissals cannot be motivated by economic problems, but only by performance-related problems (Gibbons et Katz (1991), Okatenko (2010). The vast majority of labor litigations are actually about dismissals for cause and about their justifications (Guillonneau (2015)).

With the objective of reducing litigations and facilitating worker reallocation, the French government introduced in 2008 a new legal procedure for terminating indefinite-term employment contracts, called *rupture conventionnelle* (hereafter, termination by agreement). The new procedure makes it possible for employers to terminate employment contracts without any justification, provided that they get the consent of employees and accept to grant severance payments at least as high as the severance payments granted to dismissed workers. For employers, the new procedure has the advantage of reducing dramatically the risk of being sued in labor court. With respect to employees, it makes it possible to leave one's employer without losing eligibility to receive severance payments and unemployment benefits (which would not be the case after a quit) and without enduring the stigmatization associated with dismissals.

Once the 2008 reform was adopted, terminations by agreement only spread gradually in the economy. One year after the reform, only about 30% of French establishments had started to use

2. An overview of employment termination procedure in Europe can be found in European Commission (2006). For a broader discussion and description of the various employment regulations across the world (i.e., European-type doctrine vs US "employment at-will" doctrine) see ILO (2015) or OECD (2013).

3. On these issues see, e.g., Autor *et al.* (2007), Boeri et Jimeno (2005), Bassanini *et al.* (2009), OECD (2010) Haltiwanger *et al.* (2013), Martin et Scarpetta (2012).

terminations by agreement. Six years after the reform, the same proportion was about 80%.

From a theoretical viewpoint, the rise in terminations by agreement may simply be due to the fact that they represent an option which is less risky for employer and less stigmatizing for employees than dismissals, especially dismissals for cause. In this scenario, the rise in terminations by agreement would merely coincide with a decline in dismissals of a similar magnitude. But the rise in terminations by agreement may also reflect that, before 2008, some employees stayed with their employers only because the sole ways to become unemployed (and have time to look for another job) involved either losing eligibility to receive benefits or enduring the stigmatization of dismissals. After the reform, termination by agreement may represent the best option for both these would-be movers and their employers. In this second scenario, the rise in terminations by agreement would mainly coincide with an increase in outflow of permanent workers.

To test these assumptions, our paper builds on an establishment-level dataset with detailed information on workers' entries and exits across the 2004-2014 period. These data reveal that the introduction of terminations by agreement in an establishment mainly coincides with a very significant increase of about 20% in permanent workers' overall exit rates. This increase is even stronger for younger workers and women, who are also the categories for which we observe the largest increase in terminations by agreement. The data also reveal that the introduction of terminations by agreement is followed by the replacement of about 10% of dismissals for cause by terminations by agreement. However, the substitution of terminations by agreement for dismissals explains only a very small part of the total increase in terminations by agreement in the economy.

Generally speaking, our results are suggestive that terminations by agreement were used mostly in situations where no separation at all would have occurred pre-reform, consistent with the assumption that (pre-reform) a significant number of permanent workers (especially young ones and women) were staying with their employer only because it was impossible to become unemployed without either losing eligibility to unemployment benefit or enduring the stigmatization associated with dismissals. The reform induced a decline in termination costs for these would-be movers and this appears to have been the main driver of the diffusion of terminations by agreement in the economy.

When we further compare the number of employees of establishments before and after they start using termination by agreements, we find no evidence that the adoption of the new procedure was followed by an increase in employment levels, we even find some evidence of a marginally significant decrease in employment levels after the reform. The introduction of termination by agreement coincides with a large increase in outflow of permanent workers, but no real increase in employment levels.

Eventually, relying on an alternative employer-employee dataset, we provide evidence that permanent workers who have the opportunity to sign terminations by agreement with their employers end up back in jobs for which they are better paid. By helping would-be movers to leave their employers and move to other jobs, the reform appears to have contributed to improving the quality of matches between employees and employers.

Our paper contributes to the literature exploring the impact of employment termination regulations on firms' behaviors and flow of workers. Most existing literature focuses on reforms which entail reductions in dismissal costs either for groups of firms defined by their size or for groups of workers defined by their age or their seniority level [4]. Such reforms are in general strongly contested if only because they tend to increase unemployment risks for workers who are not willing to lose their job (and would likely have a hard time in getting re-employed). The change in doctrine analysed in this paper is an attempt to circumvent this problem by promoting separation by mutual consent and reducing termination costs for a specific group of workers only, namely workers who are willing to leave their employers but cannot afford losing eligibility to unemployment benefits. Consistent with the assumption that there exists a significant number of such would-be movers, our results reveal that this change in doctrine is able to improve long-term worker reallocation while at the same time reducing dismissals for cause and labor litigation risks.

The paper is organized as follows : section 2.2 describes the 2008 reform while section 2.3 develops our analytical framework. Section 2.4 describes our administrative dataset and our working sample. Sections 2.5 and 2.6 provide graphical and econometric evidence on why terminations by agreements were adopted by employers, using an event study methodology. Section 2.7 further explores the impact of the reform on worker mobility, using an alternative matched employer-employee dataset. Eventually, section 2.8 concludes.

2.2 Institutional context

This section first describes the institutional context that prevailed in France before the 2008 reform, when termination by mutual consent was not really an option and when the termination of an employment contract had to represent the last resort, the *ultima ratio*. Second, we describe how the reform contributed to promote a new doctrine, by providing employers and employees with the possibility to terminate employment contract by mutual consent, at potentially lower cost than dismissal or quit.

4. See e.g. Dias *et al.* (2013), Behaghel *et al.* (2008), Marinescu (2009), Garibaldi et Pacelli (2008). In these papers, the impact of change in separation costs is identified by comparing targeted and untargeted groups before and after the reform, the identifying assumption being that indirect effects on untargeted groups can be neglected. For an early analysis of separation costs using the same administrative data as those used in this paper, see Goux *et al.* (2001).

2.2.1 Institutional context before 2008

Before 2008, indefinite term labor contracts can be terminated in France either on employers' initiative (dismissals) or on employees' initiative (quits), there is no third option. Furthermore, the costs of the procedure are mainly borne by the contracting party who initiate the procedure.

Employees who choose to quit lose their eligibility to receive a severance package as well as their eligibility to receive unemployment benefits [5]. Employers who decide to dismiss employees have to justify their decisions and run the risk of being sued for unfair dismissal. This risk is often painted as one reason for the sclerosis of the French labor market.

Dismissals can be justified by economic reasons. In such a case, the employer has to prove the seriousness of its economic problems and has to pay severance payments. In case of collective dismissals for economic reasons, the employer has also to justify the choice of who is dismissed and who is not. French labor laws ask employers to dismiss lower seniority workers first, as well as workers with lower family responsibility (see article 1233-5 of French labor laws).

Dismissals can also be justified by non-economic reasons (dismissals for cause), most notably when employers consider that employees are guilty of misconduct. There are three levels of misconduct, namely simple, serious or very serious misconduct [6]. Employers have to pay severance payments, except in case of serious or very serious misconduct (article L.1234-9 of French Labor law). The vast majority of litigations follow dismissals for cause [7]. Between 1996 and 2003, about 25% of these non-economic terminations have been challenged in French courts (Fraisse *et al.* (2015)).

2.2.2 The 2008 reform

In June 2008, the French government introduced a third type of labor contract termination, called *rupture conventionnelle* (hereafter, termination by agreement). When an employer and an employee opt for such a termination, the liability is shared and the consent is mutual.

This was the result of years of negotiation between trade-union and employer representatives. Employer representatives were arguing for the introduction of mutual consent termination since the beginning of the 2000's and their key demand was that this type of termination would be litigation risk free. Trade-union representatives were responsive but their key demand was that

5. Specifically, employees who choose to quit can become eligible to receive unemployment benefits only after 4 months out of the labor force and only after obtaining a specific agreement from a regional committee of employer and employee representatives (called *Instance Paritaire Regionale*).

6. Serious misconducts include insubordination (refusal to perform tasks listed in the labor contract), abandonment of post, negligence (e.g. the night watchman sleeping during his shift), safety rule violation (drunk driving), violence in the workplace, harassment, theft. Very serious misconducts involve the wish to harm : deliberate deterioration, disloyalty (leaking intelligence to the competitor), embezzlement, etc.

7. According to the French Ministry of Justice, there are about 200,000 labor litigations each year in France. Close to 80% are about the justifications of dismissals for cause while close to 15% are about unpaid wages. Only a very small fraction (about 2%) are about dismissals justified by economic reasons (see Guillonneau (2015)).

this new termination would give workers eligibility to the unemployment insurance unlike quits. After a call from the government to social partners in 2007 to reflect on how to modernize the French labor market, employer and trade unions representatives agreed on a set of measures in January 2008, including the introduction of terminations by agreement, that led to the June 2008 law.

For employees, terminations by agreement bring several advantages compared to quits. After a termination by agreement, employees remain eligible to receive a severance package at least as important as the one they receive in case of an employer-initiated termination. They also remain eligible to receive unemployment benefits. To the best of our knowledge, France is the first country who introduced a procedure of termination by mutual agreement which does not entail, for employees, the loss of eligibility to receive unemployment benefits and severance packages.

For employers, the main advantage of terminations by agreement over dismissals is that terminations by agreement need not be justified. Termination by agreements do not exempt employers from giving layoff notices or paying severance package, but save them from having to explain why they wish to terminate the labor contract [8]. This alone reduces dramatically the risk of subsequent litigation [9] and, consequently, the termination costs expected by employers, especially in periods where terminations cannot be motivated by clear economic problems.

As shown by Figure 2.1, many employers and employees started to use the new procedure very soon after the reform and the number of termination by agreement has kept increasing since then. At the end of 2014, we observe about 30,000 terminations by agreement each month, namely twice as many terminations by agreement as dismissals for economic reasons. Building on administrative data, the Figure also confirms that the vast majority of these terminations by agreement are followed by a period of receipt of unemployment benefits. In the remainder of the paper, our basic research question is to understand the causes of this rise in terminations by agreement after 2008. Does it simply reflect the substitution of terminations by agreement for for other forms of terminations ? Or does it reflect an overall increase in separations and a more fundamental change in employment dynamics ?

2.3 Potential effects of the reform : a conceptual framework

Compared to dismissals, terminations by agreements represent an option which is less risky for employers and less stigmatizing for employees. Hence, we can hypothesize that the 2008 reform

8. The procedure involves a preliminary interview as well as the writing and signing of an agreement where the contract termination date and the amount of the severance pay are made explicit. After a period of 15 days (during which cancellation is possible), the agreement is sent for approval to local labor authorities. Local authorities have 15 days to either reject or approve the agreement. If not rejected after this period, the agreement is deemed valid. For more detail see Articles L. 1237-11 to L. 1237-16 of French Labor laws. See also : https://www.service-public.fr/particuliers/vosdroits/F19030.

9. According to Berta *et al.* (2012), only about 0.1% of termination by agreement lead to a litigation.

induced the substitution of terminations by agreements for some dismissals. In fact we can expect such substitutions to be even more likely for dismissals for cause, since they represent by far the greatest risk of litigation and the most stigmatizing terminations for employees (Gibbons et Katz (1991), Okatenko (2010)).

Compared to quits, terminations by agreements represent an option which is much less costly for employees, but not for employers. Hence, we can hypothesize that the 2008 reform had much weaker substitution effects on quits than on dismissals for cause [10].

Eventually, terminations by agreement may in some cases represent an improvement over no termination at all, for both employers and employees. Before 2008, no termination at all means that dismissal would be too costly for the employer while quitting would be too costly for the employee. But, it does not rule out that some workers would prefer to be on unemployment rather than with their current employer : they choose to stay with their current employer because the only possible ways to leave their employer involve either stigmatization costs (dismissal) or the loss of eligibility to receive unemployment benefits (quit). If the number of such would-be movers is significant and if terminations by agreement are perceived by employers as less risky and costly than dismissals, the 2008 reform may induce a rise in overall separation rate, i.e., a rise in terminations by agreement signed by people who would have stayed with their employer before the reform.

In Appendix 2.B, we develop a simple labor demand model that makes more precise how the introduction of terminations by agreement may affect firms' hiring and termination decisions. Assuming that terminations by agreement are actually less risky and costly than dismissals, the model shows that the introduction of terminations by agreement may or may not entail a rise in overall termination rates depending on the number of would-be movers and on how the magnitude of adverse labor demand shock (denoted Δ) compares to exogenous outflows of workers (denoted S).

In a nutshell, when Δ is larger than S, the difference $\Delta - S$ represents the downward adjustment that the firm would like to perform when it is hit by an adverse shock. In practice, the firm performs this downward adjustment only if labor adjustment costs are not too high. Hence, if the adjustment costs associated with terminations by agreement are sufficiently low compared to the adjustment costs associated with layoffs and if there exists a sufficiently large number of would-be movers, it may become possible for firms to make the $\Delta - S$ adjustment after the reform (using termination by agreements) whereas no adjustment would have been seen pre-reform (because of layoff costs).

In the remainder of this paper, we will build on an administrative establishment-level dataset with exhaustive quarterly information on flow of workers to test these different assumptions and to explore the consequences of the 2008 change in employment doctrine.

10. We cannot exclude, however, that some firms end up agreeing to sign terminations by agreement rather than keeping unmotivated potential quitters in their staff.

2.4 Data

We use administrative data from the "Declarations des Mouvements de Main d'Oeuvre" (DMMO) collected between the first quarter of 2004 and the last quarter of 2014 [11]. For each quarter and each establishment with 50 employees or more, the DMMO provide the number of entries and exits of workers for each type of hiring and termination. In particular, we have quarterly information on the number of dismissals for economic reasons, the number of dismissals for cause, the number of quits as well as on the number of retirements and (after 2008) the number of terminations by agreement. Our empirical analysis will mostly focus on the panel of 7085 establishments continuously observed throughout the 2004-2014 period. For each one of these establishments, we are able to precisely identify whether (and when) it starts using terminations by agreement. Table 2.A.1 in the appendix provides some descriptive statistics about the establishments in this working sample. They have on average 163 employees and 50% are in the service sector. About 18% have still not used terminations by agreement by the end of 2014. Pre-reform, dismissals for cause represent on average, each quarter, about 0.5% of total employment, whereas dismissals justified by economic reasons represent about 0.1% and quits about 1.1% of total employment.

2.4.1 Terminations by agreement and establishments' survival

As mentioned above, the basic advantage of focusing on a balanced panel of establishments is that we are able to precisely identify whether (and when) each one of them starts using terminations by agreement. It makes it possible to identify the effect of adopting terminations by agreement by comparing those who start using the new procedure early after the reform with those who start later, through an event analysis. One potential issue, however, is that selection into the balanced panel may be endogenous to the date on which establishments start using terminations by agreement. For example, it may be that establishments which start using terminations by agreement early after the reform tend to have a stronger probability to survive and, consequently, a stronger probability to be seen in our balanced panel. In such a case, the comparison of changes in behavior of early starters and late beginners may not necessarily isolate the effect of using termination by agreement ; it may also reflect differential sample selection.

To explore this issue, we have tested whether the probability to be selected in the balanced panel was dependent on whether (and when) establishments start using terminations by agreements. Specifically, for each possible date of adoption t_0 of terminations by agreement, we have compared the selection probability of establishments which survived until t_0 and started using terminations

11. Several papers have already used the DMMO to analyze flow of workers in France, see e.g. Abowd *et al.* (1999) or Goux *et al.* (2001).

by agreement on t_0 with the selection probability of establishments which survived until t_0, but did not start using terminations by agreement on t_0. Figures 2.A.1 and 2.A.2 in the online Appendix show that the survival rates and sample selection probabilities are on average very similar for these two groups of establishments. The rate of survival on $t_0 + k$ (with $k = 1, ..., 12$ quarters) is on average slightly stronger for establishment who starts using terminations by agreement on t_0, but the difference between the two groups is only about one percentage point and not significantly different from zero at standard level.

Overall, the date on which establishments start using terminations by agreement does not seem to have any significant influence on the probability to survive and be selected in the balanced panel, so that sample selection appears to be negligible. However, as a robustness check, we will replicate most of our regression analysis on a much larger unbalanced panel (N=17,965), which include all the establishments for which information on flow of workers are available for 80% (or more) of the quarters of the 2004-2014 period under consideration. As discussed below, we obtain very similar results with the unbalanced panel and with the balanced one.

Eventually, in the last section of the paper, we test for the impact of terminations by agreement on workers' mobility using an alternative data source which links social security data (called DADS) to unemployment insurance data (called FH). The corresponding matched dataset is referred to as the FH-DADS panel. The first dataset (DADS) comes from social security records that are filled by employers each year for each of their employees and that are used to compute social security contributions. It contains information on employees' level and duration of benefits for each unemployment spell. These two datasets have been matched for a subsample of the French population (1/12th) from 2002 to 2012, resulting in a matched employer-employee panel dataset which allows to track individual career path and transitions from employment to unemployment. We augmented this database with information (from DMMO) on the quarter in which employers began using terminations by agreement.

2.5 Terminations by agreement and establishments' exit flows : a graphical analysis

The 2008 reform introduced an entirely new and relatively complex termination procedure. Moreover, it can only be implemented in very specific situations, when neither the employer nor the employee have the capacity to take the initiative to break the labor contract on their own. In this context, the use of terminations by agreement can only have spread very gradually, as opportunities to sign terminations by agreement (and to pay the cost of adapting to the new procedure) gradually emerged.

To illustrate this fact, Figure 2.2 focuses on our balanced panel and shows the cumulative proportion of establishments which began to use terminations by agreement between 2008 and t, for each quarter t between 2008-Q1 and 2014-Q4. The Figure confirms that in 2009, one year after the reform, only about 30% of establishments had already signed a termination by agreement. In 2014, the same percentage was still not 100%, but about 80%.

To take one step further, Figure 2.3 focuses on establishments which began to use terminations by agreement at some point between 2008 and 2014 and shows the evolution of their number of terminations by agreement per employee over time, with the date of the first termination by agreement being taken as the origin of the time scale. The Figure shows that the number of terminations by agreement per employee jumps almost immediately after the first one. Afterward, it remains stable. This result is suggestive that, once the cost of adapting to the new procedure has been paid, the flow of terminations by agreement almost immediately reaches an equilibrium level.

In the remainder of this section, our first purpose is to explore graphically whether the date on which an establishment starts using terminations by agreement also coincides with a decline in the other forms of terminations. As discussed above, terminations by agreement represent an option which is likely to be less stigmatizing for employees and which involve much less litigation risks for employers than dismissals for cause. On the other hand, terminations by agreement entail adjustment costs that are stronger for employers than those entailed by quits or retirements, and as strong as those entailed by dismissal for economic reasons. Therefore, to the extent that the risks of labor disputes are effectively taken into account by employers, we expect the date of adoption of terminations by agreement to coincide with a decline in the number of dismissals for cause per employee, but not in the other forms of terminations.

2.5.1 Terminations by agreement as a substitute for other forms of terminations

To test these assumptions, Figures 2.4 and 2.5 compare the number of dismissals for cause per employee observed in establishments that started using terminations by agreement between 2008 and 2011 (early adopters) with those observed at the same dates, in the same industries, in establishments that had not yet started using terminations by agreement in 2014 (late adopters). Specifically, the solid line in Figure 2.4 shows the evolution of dismissals for cause in the first group of establishments, before and after the date on which they first use terminations by agreement (the date of first use is taken as the origin of the time scale). The dotted line in the same Figure shows the evolution of the same variable in the second group of establishments [12]. The Figure reveals that

12. To be very specific, for each date t and each establishment j in the first group, it is possible to define (a) the distance between t and the date $t_0(j)$ at which j starts using terminations by agreement and (b) Y_{jt} the number of

the date around which early adopters start using terminations by agreement (i.e., $t = 0$) coincides with a significant decrease in their use of dismissals for cause whereas no change is seen in the use of these terminations in late adopters. The solid line stays above the dotted line until early adopters start using terminations by agreement. After that date, the situation is reversed. To take one step further, Figure 2.5 shows the evolution of the difference between the solid and the dotted lines of Figure 2.4. The Figure confirms that this difference declines at about the same time that early adopters start using terminations by agreement. These results are clearly consistent with the assumption that terminations by agreement entail adjustment costs for employers that are lower than those entailed by dismissals for cause and represent a potential substitute for these dismissals.

It is possible to develop a similar analysis for dismissals justified by economic reasons (see Figures 2.6 and 2.7). This analysis shows no variation in the difference between the two groups of establishments after the date when early adopters start using terminations by agreement. There is no evidence that terminations by agreement were used as a substitute for dismissals justified by economic reasons, consistent with the fact that terminations by agreement are not necessarily easier to bargain and implement than dismissal for economic reasons during a downturn. They are not less costly either, since the severance packages associated to terminations by agreement have to be as generous as those associated to dismissals.

Eventually, Figures 2.10, 2.11, 2.A.3 and 2.A.4 compare the evolution of quits and retirements in early and late adopters. Again, they do not show any variation in the difference between the two groups of establishments after the date when early adopters start using termination by agreement. Put differently, there is no evidence that terminations by agreement were used as an early retirement device or as a substitute for quits. Terminations by agreement entail the payment of severance packages and, as such, are more costly for employers than quits or retirements. In this context, it is not surprising that we do not see any significant decline in quits or retirement after the adoption of terminations by agreement

2.5.2 Terminations by agreement and overall separation rates

The previous subsection provides suggestive evidence that terminations by agreement are used as a substitute for dismissal for cause. As discussed in the previous sections, it is also likely that terminations by agreement make it possible to terminate permanent contracts in circumstances when no terminations would have been possible before the reform. To explore this last assumption, we looked at whether the introduction of terminations by agreement was followed by an increase in the overall number of terminations of indefinite-term contracts, as measured, each quarter, by

dismissals per employee in j at t and (c) $\overline{Y_{jt}}$ the average number of dismissals per employee at t in establishments of the second group (i.e., same industry as j, but the date of the first termination by agreement is after 2014). The Figure shows the evolution of the average of Y_{jt} et $\overline{Y_{jt}}$ conditional on d, for d between -12 to +12. The two groups are defined so that each given observation contributes to either the solid line or the dotted line, never to both lines.

the sum of dismissals, quits, retirements and (after 2008) terminations by agreement. Figures 2.8 and 2.9 confirm that this is the case. When we compare the group of early adopters with the group of late adopters, we see that the overall number of terminations per employee increases in the first group (but not in the second one) just after it starts using terminations by agreement. When economic conditions are not particularly bad and do not justify downsizing, dismissals are difficult to justify and our results confirm that terminations by agreement represent an interesting alternative option for employers who are willing to reorganize their staff, i.e., destroy some old jobs and create new ones. The DMMO do not provide direct evidence on the number of new indefinite term contracts which are signed, each quarter, in each establishment [13]. Hence, it is not possible to rigorously assess whether the adoption of terminations by agreement is also followed by an increase in the number of new indefinite-term contracts signed each quarter. It remains possible, however, to test whether the rise in terminations coincide with a decline in the overall number of employees. Figures 2.A.5 and 2.A.6 in the online Appendix suggest that this is not the case. The date on which an establishment starts using terminations by agreement does not appear to coincide with any specific decline in its number of employees.

2.6 Regressions analysis

The previous section provides graphical evidence suggesting that the date on which an establishment starts using terminations by agreement coincides with a significant rise in the overall rate of terminations of indefinite-term contracts in this establishment. By contrast, the date on which an establishment starts using terminations by agreement does not seem to coincide with any significant change in its level of employment. In this section, we develop a regression analysis to test the robustness of these findings as well as whether they hold true in all industries and for all types of workers. We focus on the panel of establishments who starts using terminations by agreement between 2008 and 2014 and we assume the following two-way fixed effects model,

$$Y_{jt} = \gamma Post_{jt} + \alpha_j + \tau_t + \epsilon_{jt} \tag{2.1}$$

where Y_{jt} represents the outcome under consideration in establishment j during quarter t whereas $Post_{jt}$ is a dummy variable indicating whether quarter t is before or after the quarter $t_0(j)$ during which establishment j starts using terminations by agreement. Parameters α_j et τ_t represent a full set of quarter and establishment fixed effects. Eventually, ϵ_{jt} represents unobserved factors which affect j during t, but which variations over time are assumed uncorrelated with the dates at which establishments start using termination by agreement.

13. A significant fraction of indefinite-term contracts correspond to the transformations of temporary contracts into permanent ones (see, e.g., Goux *et al.* (2001)). The DMMO do not provide information on these transformations.

Following Abraham et Sun (2020), it is possible to cast model (2.1) in a potential outcomes setting where treatment effects are defined (for each establishment j, each potential date of treatment e and each date t) as the difference between outcomes that would be observed at t if establishment j started using the new procedure on e and outcomes that would be observed at t if establishment j never started. In this framework, Abraham et Sun (2020) show that a "parallel trend" and a "non-anticipation" assumption are sufficient for the two-way fixed effects estimator of parameter γ in model (2.1) to capture an average treatment effect. The "parallel trend" assumption states that - had terminations by agreement not been introduced - outcomes would have followed similar trends in establishments who start using terminations by agreement early after the reform and in establishments who start later. The "non-anticipation" assumption states that - had terminations by agreement not been introduced- we would have observed the same outcomes in the period before establishments start using terminations by agreement. Put differently, we assume that the reform did not induce establishments to adapt their behavior in anticipation, namely before they actually start using terminations by agreement. Under these two assumptions, the two-way fixed effects estimator of parameter γ recovers a weighted average of cohort-specific average treatment effects, where cohorts are defined by the date of introduction of termination by agreement. Assuming treatment homogeneity across cohorts, parameter γ can simply be interpreted as the difference between the average outcome observed after the introduction of terminations by agreement and the average outcome that would be observed in the same establishments, had terminations by agreement not been made available [14].

Generally speaking, the Figures presented in the previous section are consistent with our two identifying assumptions. As it turns out, when we compare establishments who start using terminations by agreement at a given date with establishments who will start only later, Figures do not show any significant divergence in their behavior in the period before the first group starts using termination by agreement [15].

2.6.1 Regression results

Consistent with our graphical analysis, the regression results in the panel A of Table 2.1 confirm that the introduction of terminations by agreement in an establishment mainly coincides with a

14. When treatment effects are heterogeneous across cohorts of adoption, the two-way fixed effects estimator of parameter γ can be more difficult to interpret since it recovers a linear combination of cohort-specific average treatment effects where weights are not necessarily positive, as shown by Abraham et Sun (2020). In Appendix 2.C, we build on the recent work by Cengiz *et al.* (2019) to show that our results are robust to the presence of heterogeneous treatment effects.

15. As discussed above, the only exception is seen for dismissals for cause : the decline in these dismissals starts a little before establishments actually starts using terminations by agreement. To further test the robustness of our results, however, we replicated our regression analysis on the sample obtained after dropping for each establishment the observations that correspond to the two quarters before the quarter at which it starts using termination by agreement (i.e., two observations that may be affected by anticipation effects). As discussed below, we obtain very similar results on this subsample and on the main sample.

significant increase in outflow of permanent workers. Specifically, the estimated effect ($\gamma \approx 0.35^{***}$ percentage points) corresponds to an increase of about 20% in overall separation rate and is about as strong as the estimated increase in the number of terminations by agreement per employee that follows the introduction of the new procedure ($\gamma \approx 0.41^{***}$). Most of the increase in termination by agreement appears to be a response to employees' desire to change employers, which was too costly to satisfy before the reform.

Regression results also confirm that the introduction of terminations by agreement is followed by a significant decline in the number of dismissals for cause per employee ($\gamma \approx -0.03^{***}$), but has little effect on quits, retirement or on dismissals justified by economic reasons. The estimated effect suggests that about 10% of dismissals for cause are replaced by terminations by agreement after the introduction of the procedure.

Panels B and C of Table 2.1 show our regression results when we look separately at establishments in the manufacturing industries and establishments in the service sector. They show that terminations by agreement induce a very significant rise in aggregate separation rates in both sectors. By contrast, the decline in dismissals for cause is mainly seen in the service sector, which is also the sector where this type of terminations is, by far, the most used [16]. Eventually, when we look separately at manufacturing and service industries, the small negative impact on employment levels appears to be significant at conventional levels in neither sector.

To take one step further, Table 2.A.2 in Appendix 2.A shows the results of replicating our econometric analysis after dropping - for each establishment - the observations which correspond to the two quarters before the establishment starts using terminations by agreement, so as to minimize anticipation effects. We find very similar regression results with this subsample as with the main sample, namely a positive effect on aggregate separation rates and a smaller negative effect on dismissals for cause. However, when we work with this subpanel, the negative effect of terminations by agreement on employment levels is not significant at standard level anymore. We find also no significant negative impact on employments levels in this specification and the magnitude of the coefficients are divided by two. It means that anticipationary behavior might explain the negative coefficients found in Table 2.1.

Eventually, to further test the robustness of our results, we consider establishments for which DMMO information is available for 80% of more of the quarters (i.e. 36 quarters or more, out of 44) and we replicated our econometric analysis on this much larger unbalanced panel (see Table 2.A.3 in the appendix). Generally speaking, we obtain similar results with this unbalanced panel as with the balanced one.

16. It likely reflects that the quality of employees' work is more likely to be subject to different interpretations in the service sector than in the manufacturing industry, maybe because service tasks tend to be more difficult to codify and evaluate.

2.6.2 An augmented specification

The results so far suggest that terminations by agreement are used partly as a substitute for dismissals for cause and partly as a means of terminating labor contracts that employers and employees find unsatisfactory, but which would be too costly to break.

If this interpretation is correct, the decline in dismissals for cause and the rise in overall separation rates that coincide with the adoption of terminations by agreement should be observed within firms for the same categories of workers as the rise in terminations by agreement. It is possible to test this prediction using the information available in our establishment-level data on the age, sex and occupation of employees exiting firms each quarter.

If, for example, Y_{kjt} represents the rate of termination by agreement signed in establishment j on year t by workers of type k (with $k = 0$ for women and $k = 1$ for men, for example), we can begin by estimating the following augmented version of model (2.1) :

$$Y_{kjt} = \theta Post_{jt} \times I_k + \mu_{jt} + \mu_{kt} + \mu_{kj} + e_{kjt} \tag{2.2}$$

where I_k is a dummy indicating that $k = 1$ and where parameter θ captures the difference in exposure to terminations by agreement between men and women within establishments. [17]

Table 2.2 shows the results of estimating model (2.2) for the different types of exit rates (terminations by agreement, quits, etc.) when we contrast male and female workers (panel A), executives and non-executives workers (panel B) or workers aged less than 40 and workers aged 40 years of more (panel C). With respect to terminations by agreement, the first column of the table shows that their diffusion within establishments was significantly stronger for female workers than for male workers as well as for executive workers than for non-executive ones. It was also stronger for younger workers than for older workers, consistent with the fact that younger workers are more likely to be in an employment situation that they feel could still be improved. Given this reality, the question becomes whether the rise in separations and the decline in dismissals are also more pronounced for younger workers than for older workers, for female workers than for male workers, or for executives than for non-executives. The columns (2) to (6) of Table 2.2 show that it is the case. Specifically, the stronger rise in terminations by agreement observed for executives within establishments coincides mostly with a stronger decline in their exposition to dismissals for cause while the stronger rise in terminations by agreement observed within establishments for female workers or for younger workers coincides mostly with a stronger rise in their overall separation rates. The introduction of terminations by agreement has enabled a number of younger workers and female workers to avoid having to stay with an employer that only the costs associated with

17. There is no information on job stocks by categories of workers in the DMMO database, there is only information on worker flows. In order to construct exit rates by categories of workers, the flows obtained from the DMMO were divided by the stocks obtained from the social security records (DADS).

resignations and dismissals prevented them from leaving. It has also enabled a number of executives who were in conflict with their employers to avoid the stigma of dismissal [18].

2.6.3 Interaction between the timing of the reform and the one of the crisis

The introduction of terminations by agreement coincided with the timing of the crisis as most establishments adopted terminations by agreement in 2009 at the heart of the crisis in France. Our specification, because we compare adoptions in the first quarter of 2009 to adoptions in last one of 2014, control for national trends thanks to the quarter fixed-effects θ_t and then for the impact of all shocks due to the crisis and the following recovery on worker flows that are common to all establishments.

However, the context of the crisis might have influenced the way the reform was perceived and the use of terminations by agreement may have changed over time, from a way to reallocate willing workers during the crisis to a substitution to quits in 2014 and 2015 as the economic engine was gaining steam. Because we only recover the average treatment effect on the treated, we may not capture very well how the use of termination by agreement changes. Studying the use of terminations by agreement more than five years after the reform will be difficult with an approach that focus on adoptions for identification, and further research that might want to follow this path will need to find another approach.

2.7 Termination by agreement and worker mobility

Using quarterly establishment-level data, the previous sections are suggestive that the adoption of terminations by agreement by an establishment facilitates worker mobility. In this last section, we provide an alternative test of this assumption using different data, namely matched employer-employee (annual) data which cover the 2002-2012 period and make it possible to look at whether workers' situation in 2012 (as well as workers' labor market transitions between 2008 and 2011) depend on whether their employer in 2008 adopted terminations by agreement relatively early or relatively late.

Specifically, we focus on the sample of workers who are employed in 2008 in an establishment that will come to adopt terminations by agreement between 2008 and 2011 and we consider five basic dependent variables : (a) a dummy variable indicating whether the worker signed a termination

18. A typical conflict between executives and their employers concerns transfers to other regions that employers may seek to impose. Where such transfers are permitted by the employment contract, the employee's refusal may justify dismissal for cause.

by agreement between 2008 and 2011, (b) a dummy variable indicating whether the worker went through a period of unemployment between 2008 and 2011, (c) a variable indicating the number of different jobs the worker held between 2008 and 2011, (d) a variable indicating whether the worker is employed in 2012, (e) a variable indicating workers' hourly wage in 2012 (conditional on employment in 2012). Table 2.3 shows the result of regressing these dependent variables on a variable Q indicating the number of quarters between the date on which the 2008 employer adopted terminations by agreement and 2011 [19]. The estimated impact of Q captures the effect of one additional quarter of potential exposure to terminations by agreement.

Comfortingly, the Table confirms that workers who were employed in 2008 by an establishment which adopted terminations by agreement earlier have a stronger probability of signing a termination by agreement between 2008 and 2011 as well as a significantly stronger probability of transiting on the labor market and changing job between 2008 and 2011. Their probability of being employed in 2012 is however not significantly different from that of workers who were employed in 2008 by a firm which adopted terminations by agreement later. These results are consistent with the main finding of our previous establishment-level analysis, namely the finding that the adoption of terminations by agreement is followed by a rise in overall separation rate, without any significant consequences on their employment level. The last column of Table 2.3 further focuses on workers who were employed in 2012, so as to look at whether their 2012 hourly wage depends on whether their employer in 2008 adopted terminations by agreement relatively early or relatively late. It reveals that workers who were employed in 2008 by an early adopter tend to earn significantly higher hourly wage [20].

Eventually, Table 2.A.4 in the online appendix shows the result of placebo regressions where we use 2008 hourly wages (or labor market transitions observed between 2004 and 2007) as dependant variables. Comfortingly, the Table does not reveal any significant correlation between these pre-reform outcomes and the date of adoption of terminations by agreement.

Taken together, the results in Table 2.3 (and Table 2.A.4) are suggestive that workers who sign terminations by agreement with their employers end up back in jobs for which they are better paid, and probably more productive. By helping would-be movers to actually leave their employers, the reform seems to have contributed to improving the quality of matches between employees and employers.

19. In these regressions, we also control for employees' age, sex and education as well as for the size, average wage, share of skilled workers of their employers in 2008.

20. In this analysis, we exclude the 5% observations with reported hourly wages below 8 euros/hour (i.e.,less than $0.85 \times$ the minimum wage).

2.8 Conclusion

In 2008, French labor laws introduced a new employment termination procedure, called *rupture conventionnelle*, and it became possible to terminate employment contracts by mutual consent at lower costs. By comparing employers who started to use the new procedure just after the reform with those who started a little later, we show that the adoption of termination by agreement coincides with a significant increase of about 20% in overall separation rates. This finding is suggestive that pre-reform many employment contracts were not broken only because termination costs could not be shared. We also provide evidence that workers who benefit from terminations by agreement are able to return to better-paid jobs, which suggests that terminations by agreement contribute to a better match between employees and employers.

In addition, we show that the adoption of the new procedure coincides with a small, but statistically significant decline of dismissals for cause, namely a decline in the form of termination that carries the greatest risk of labor disputes. This result confirms that the risks of labor disputes represent an important element of the costs of terminating employment contracts and that reducing these risks can contribute to speeding up worker reallocation.

Overall, our paper reveals that a reduction in separation costs does not necessarily come at the price of increased conflicts between employees and employers, even when it is followed by an actual increase in separation rates. As it happens, by changing employment doctrine and promoting separations by mutual consent, the 2008 reform induced an increase in separation rates, a reduction in litigation risks and an improvement in the quality of the matches between employees and employers. Eventually, we do not see any significant change in firms' employment levels after the reform, which suggests that the increase in overall separation rates induced by the reform was offset by a symmetrical increase in hiring rates, consistent with standard model of labor demand dynamics.

Bibliographie

John M Abowd, Patrick Corbel et Francis Kramarz : The entry and exit of workers and the growth of employment : an analysis of french establishments. *Review of Economics and Statistics*, 81(2):170–187, 1999.

Sarah Abraham et Liyang Sun : Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Unpublished Manuscript*, 2020.

David H Autor, William R Kerr et Adriana D Kugler : Does employment protection reduce productivity ? evidence from us states. *The Economic Journal*, 117(521):F189–F217, 2007.

Andrea Bassanini, Luca Nunziata et Danielle Venn : Job protection legislation and productivity growth in oecd countries. *Economic policy*, 24(58):349–402, 2009.

Luc Behaghel, Eve Caroli et Emmanuelle Walkowiak : Innovation and skill upgrading : The role of external vs internal labour markets. *Unpublished Manuscript*, 2008.

Nathalie Berta, Camille Signoretto et Julie Valentin : La rupture conventionnelle : objectifs officiels versus enjeux implicites. *Revue Francaise de Socio-economie*, 9(1):191–208, 2012.

Tito Boeri et Juan F Jimeno : The effects of employment protection : Learning from variable enforcement. *European Economic Review*, 49(8):2057–2077, 2005.

Doruk Cengiz, Arindrajit Dube, Attila Lindner et Ben Zipperer : The effect of minimum wages on low-wage jobs : Evidence from the united states using a bunching estimator. Rapport technique, National Bureau of Economic Research, 2019.

Clément De Chaisemartin et Xavier D'Haultfoeuille : Two-way fixed effects estimators with heterogeneous treatment effects. Rapport technique, National Bureau of Economic Research, 2019.

Daniel A Dias, Carlos Robalo Marques et Fernando Martins : Wage rigidity and employment adjustment at the firm level : Evidence from survey data. *Labour Economics*, 23:40–49, 2013.

European Commission : Termination of employment relationships legal situation in the member states of the european union. *Directorate General Employment, Social Affairs and Equal Opportunities*, 2006.

Henri Fraisse, Francis Kramarz et Corinne Prost : Labor disputes and job flows. *ILR Review*, 68(5):1043–1077, 2015.

Pietro Garibaldi et Lia Pacelli : Do larger severance payments increase individual job duration ? *Labour Economics*, 15(2):215–245, 2008.

Robert Gibbons et Lawrence F Katz : Layoffs and lemons. *Journal of labor Economics*, 9 (4):351–380, 1991.

Andrew Goodman-Bacon : Difference-in-differences with variation in treatment timing, 2018.

Dominique Goux, Eric Maurin et Marianne Pauchet : Fixed-term contracts and the dynamics of labour demand. *European Economic Review*, 45(3):533–552, 2001.

Serverin Serverin Guillonneau, Maud : Les litiges individuels du travail de 2004 a 2013 : des actions moins nombreuses, mais toujours plus contencieuses. *Infostat Justice*, 135, 2015.

John Haltiwanger, Ron S Jarmin et Javier Miranda : Who creates jobs ? small versus large versus young. *Review of Economics and Statistics*, 95(2):347–361, 2013.

ILO : World employment and social outlook 2015 : The changing nature of jobs. *Geneva : ILO*, 2015.

Ioana Marinescu : Job security legislation and job duration : Evidence from the united kingdom. *Journal of Labor Economics*, 27(3):465–486, 2009.

John P Martin et Stefano Scarpetta : Setting it right : Employment protection, labour reallocation and productivity. *De Economist*, 160(2):89–116, 2012.

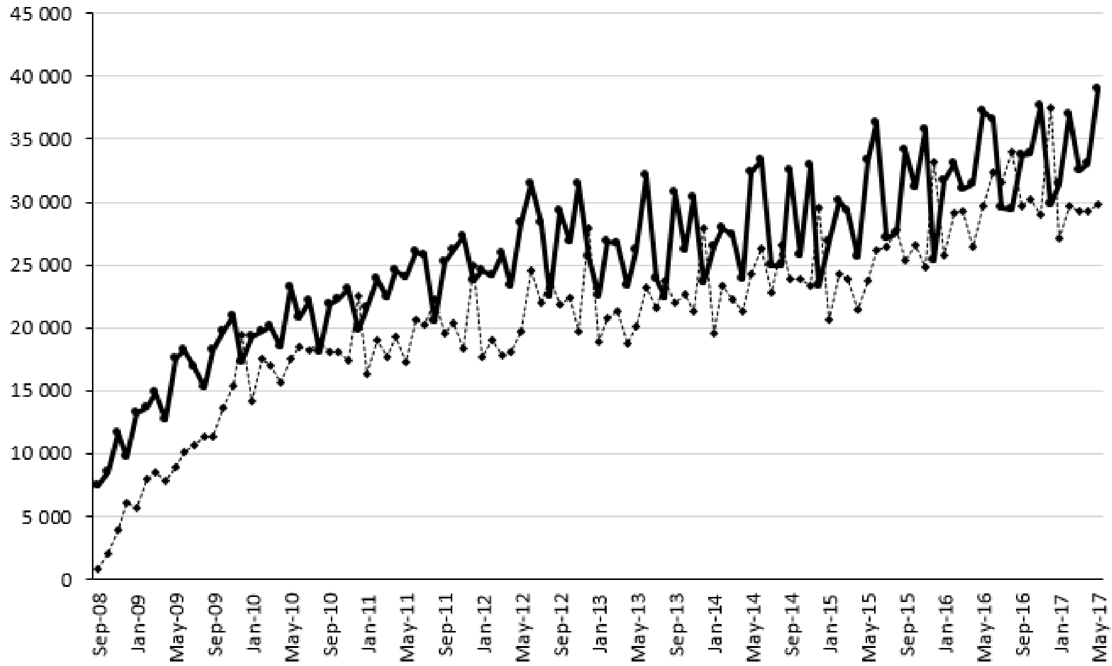
OECD : Moving beyond the jobs crisis. *OECD Employment Outlook*, 2010.

OECD : Protecting jobs, enhancing flexibility : A new look at employment protection legislation. *OECD Employment Outlook*, 2013.

Anna Okatenko : The impact of the reason for layoff on the subsequent unemployment duration. *Annals of Economics and Statistics/Annales d'Économie et de Statistique*, pages 285–315, 2010.

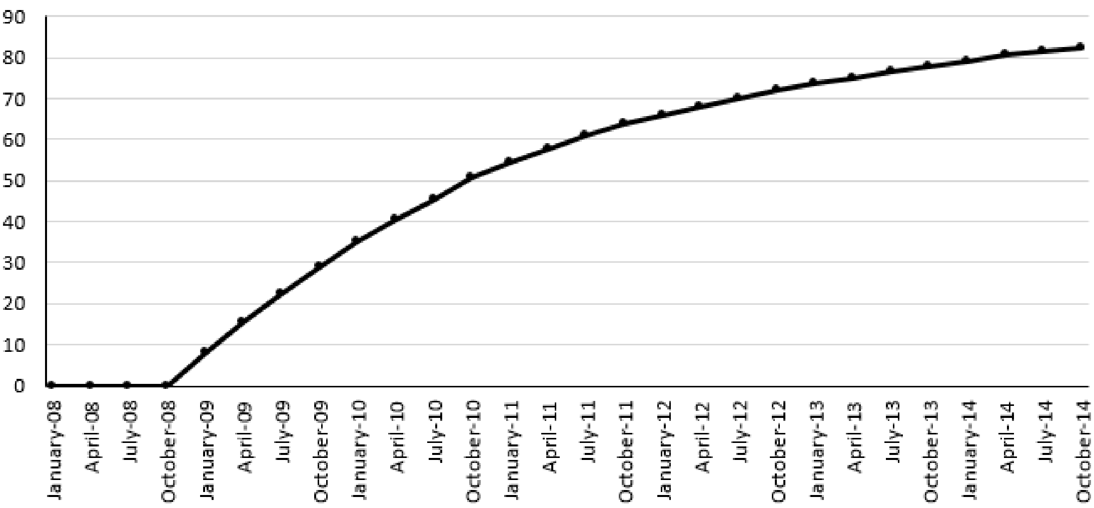
Figures and Tables

FIGURE 2.1 – Number of terminations by agreement between 2008 and 2017



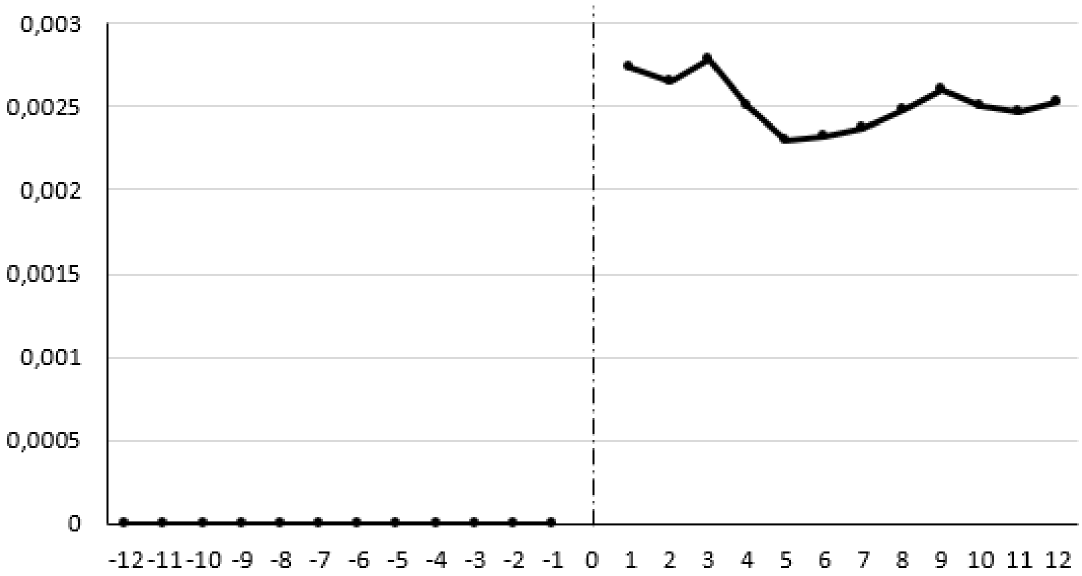
Note : The solid line shows the evolution of the number of terminations by agreement approved each month and the dotted line shows the evolution of the number of terminations by agreement which are followed by a registration into unemployment.

FIGURE 2.2 – Cumulative proportion of establishments that have already used terminations by agreement



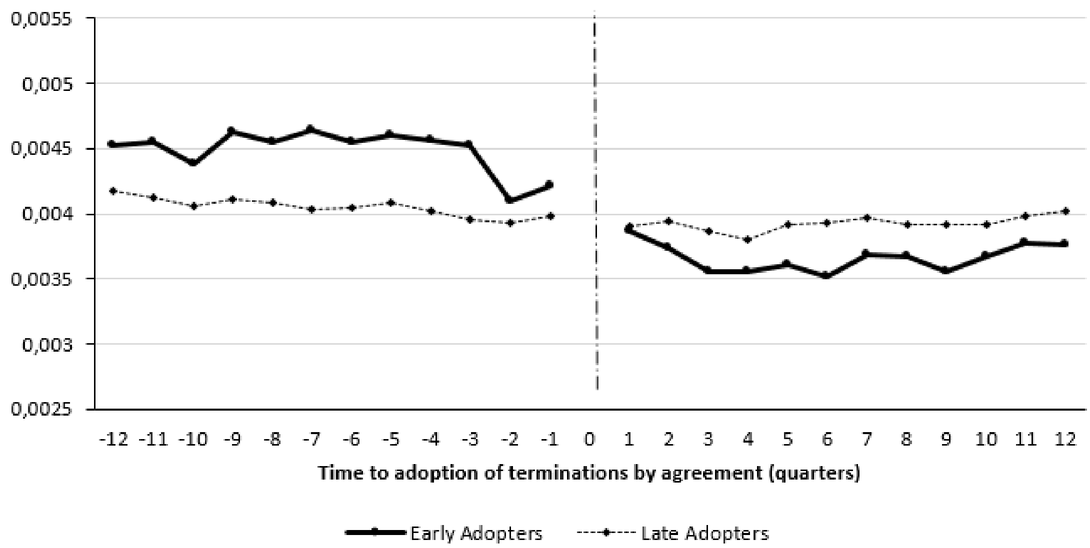
Note : The curve shows the evolution of the proportion of establishments in the balanced panel that have already used terminations by agreement. Reading : At the end of 2010, about 50% of establishments had already used terminations by agreement.

Figure 2.3 – Rate of termination by agreement before and after the first use of the procedure



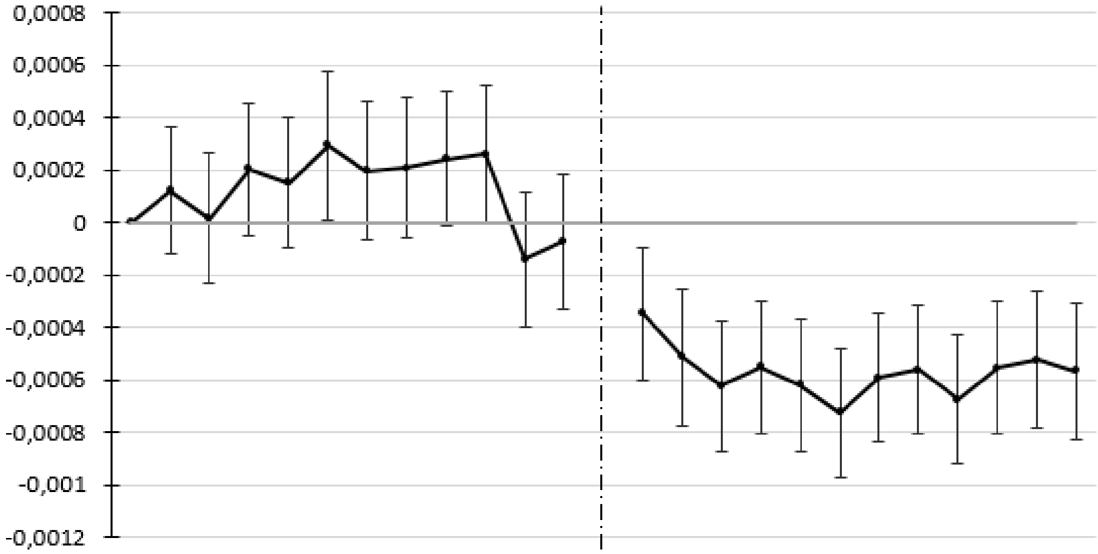
Note : The figure focuses on establishments which began to use terminations by agreement between 2008 and 2011. The curve shows the evolution of the number of terminations by agreement per employee and quarter, taking the quarter of the first termination by agreement as the origin of the time scale. Reading : Four quarters after the first termination by agreement, the number of terminations by agreement per employee is on average about .0025 each quarter.

Figure 2.4 – Rate of dismissal for cause before and after the adoption of terminations by agreement.



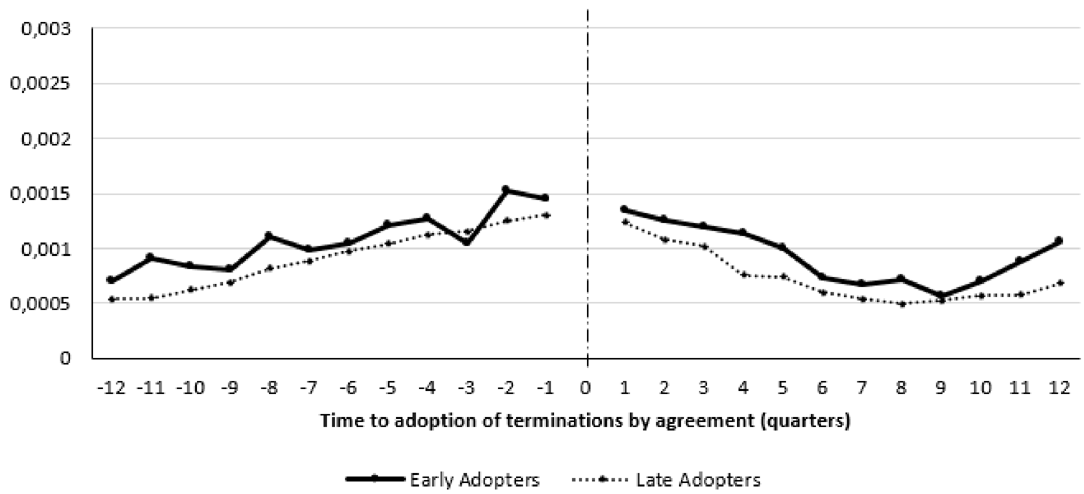
Note : The figure focuses on establishments which began to use terminations by agreement between 2008 and 2011. The solid line shows the evolution of the rate of dismissal for cause over a period of 6 years, taking the date of the first termination by agreement as the origin of the time scale. The dotted line shows the rate observed at the same dates in establishments that had still not began to use terminations by agreement by the end of 2014.

FIGURE 2.5 – Difference in rates of dismissal for cause between early adopters and late adopters



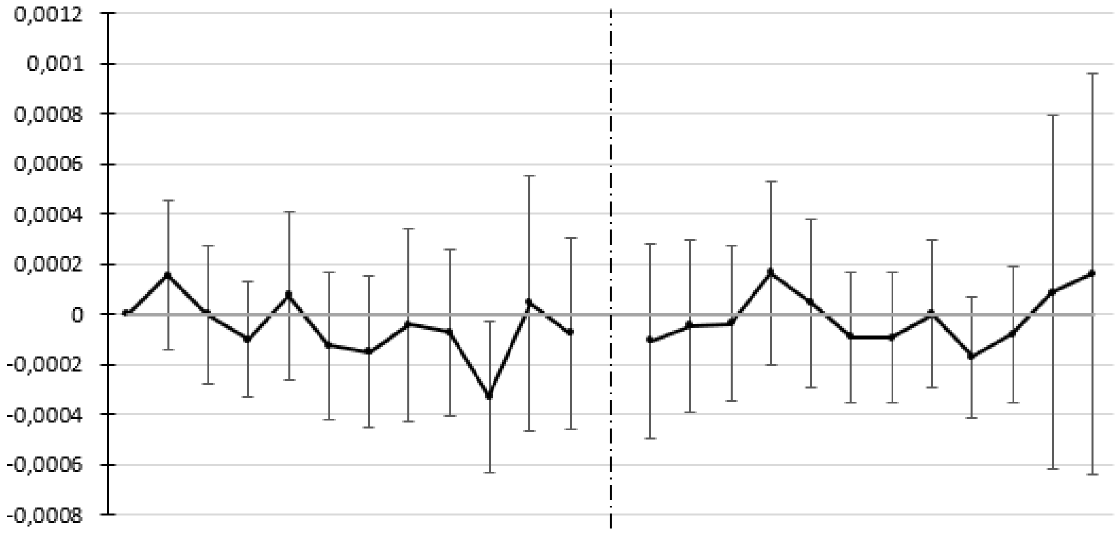
Note : The curve shows the evolution of the difference between the solid line and the dotted line shown in Figure 2.4. The vertical lines represent the confidence intervals. The source and field are the same as those used in Figure 2.4.

FIGURE 2.6 – Rate of dismissal justified by economic reasons before and after the adoption of terminations by agreement



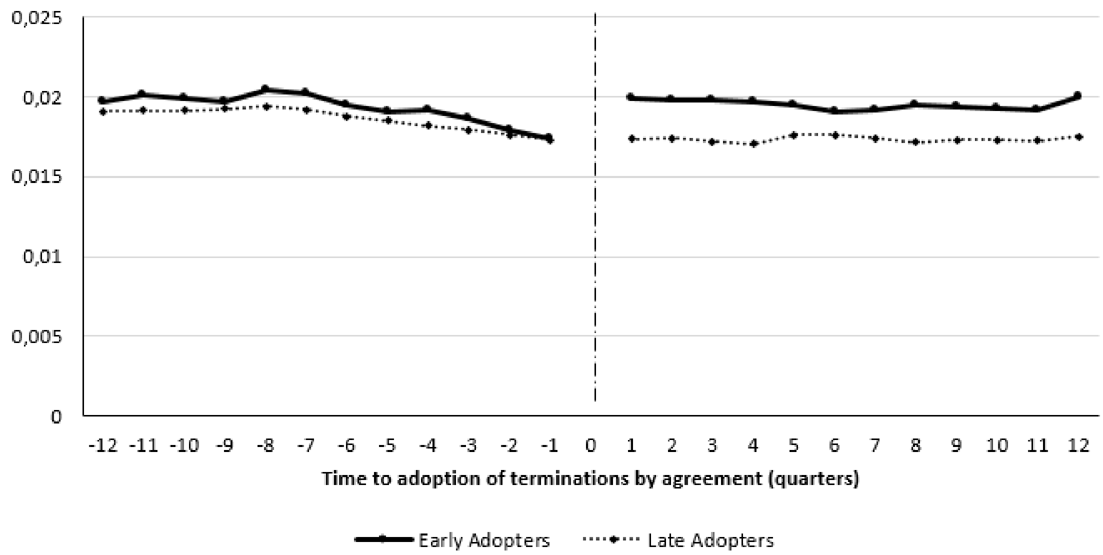
Note : The figure focuses on establishments which began to use terminations by agreement between 2008 and 2011. The solid line shows the evolution of the rate of dismissal for economic reasons over a period of 6 years, taking the date of the first termination by agreement as the origin of the time scale. The dotted line shows the rate observed at the same dates in establishments that had still not began to use terminations by agreement by the end of 2014.

Figure 2.7 – Difference in rates of dismissal justified by economic reasons between early adopters and late adopters



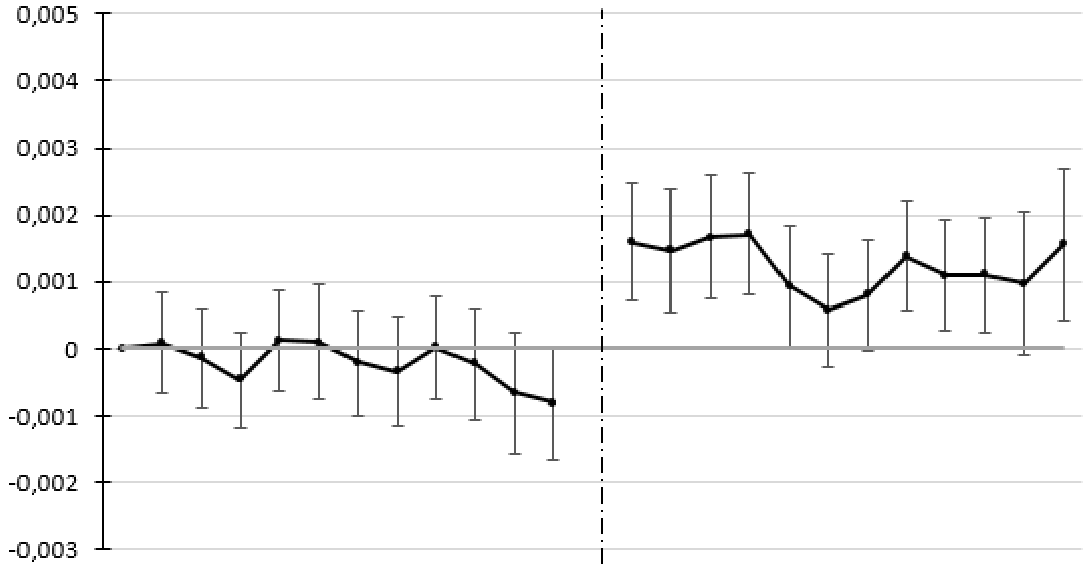
Note : The curve shows the evolution of the difference between the solid line and the dotted line shown in Figure 2.6. The vertical lines represent the confidence intervals. The source and field are the same as those used in Figure 2.6.

Figure 2.8 – Overall rate of termination of permanent contracts before and after the adoption of terminations by agreement



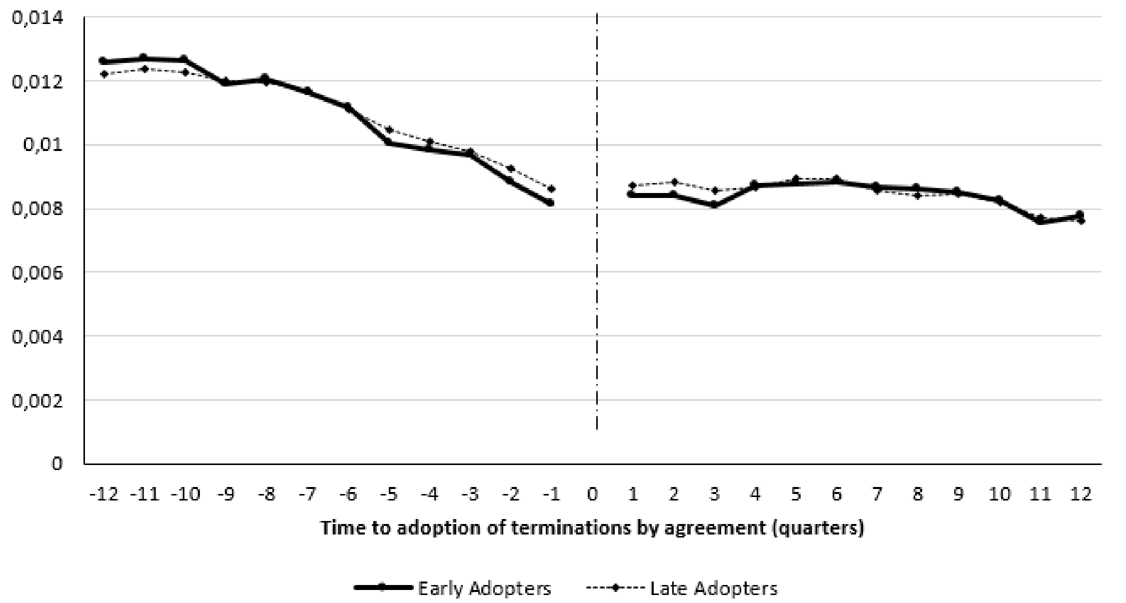
Note : The figure focuses on establishments which began to use terminations by agreement between 2008 and 2011. The solid line shows the evolution of the overall rate of termination of permanent contracts over a period of 6 years, taking the date of the first termination by agreement as the origin of the time scale. The dotted line shows the overall termination rate observed at the same dates in establishments that had still not began to use terminations by agreement by the end of 2014.

Figure 2.9 – Difference in overall rates of termination of permanent contracts between early adopters and late adopters



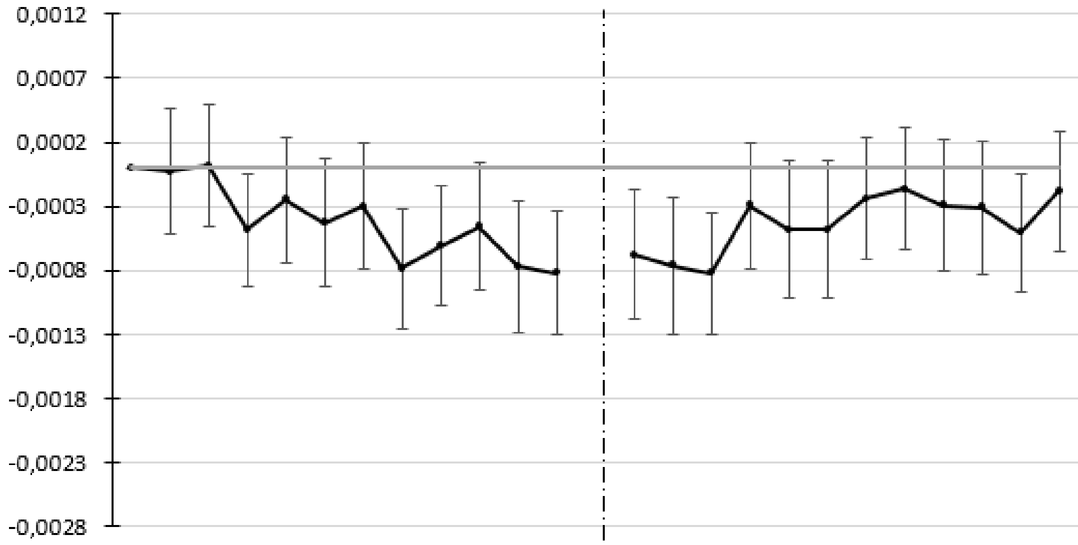
Note : The curve shows the evolution of the difference between the solid line and the dotted line shown in Figure 2.8. The vertical lines represent the confidence intervals. The source and field are the same as those used in Figure 2.8.

Figure 2.10 – Quit rate before and after the adoption of terminations by agreement



Note : The figure focuses on establishments which began to use terminations by agreement between 2008 and 2011. The solid line shows the evolution of the quit rate over a period of 6 years, taking the date of the first termination by agreement as the origin of the time scale. The dotted line shows the quit rate observed at the same dates in establishments that had still not began to use terminations by agreement by the end of 2014.

Figure 2.11 – Difference in quit rates between early adopters and late adopters



Note : The curve shows the evolution of the difference between the solid line and the dotted line shown in Figure 2.10. The vertical lines represent the confidence intervals. The source and field are the same as those used in Figure 2.10.

Table 2.1 – The effect of adopting terminations by agreement on permanent contract terminations and number of persons employed

	Termination by agreement	Dismissal For cause	Quit	Economic Dismissal	Retirement	Overall termination	Nb Employees (log)
				A- All industries			
$Post_{jt}$	.413***	-.031***	.001	.004	.001	.348***	-.0081***
	(.005)	(.007)	(.013)	(.011)	(.006)	(.022)	(.0027)
Obs.	256739	256739	256739	256739	256739	256739	256739
m	0.12	0.41	0.99	0.09	0.37	1.92	4.85
				B- Construction and manufacturing			
$Post_{jt}$	.408***	-.012	.003	.000	-.001	.373***	-.0066
	(.006)	(.009)	(.012)	(.019)	(.009)	(.028)	(.0086)
Obs.	135309	135309	135309	135309	135309	135309	135309
m	0.11	0.34	0.67	0.13	0.40	1.65	4.90
				C- Service			
$Post_{jt}$	.424***	-.052**	-.014	-.000	-.003	.328***	-.0051
	(.008)	(.010)	(.025)	(.009)	(.008)	(.035)	(.0039)
Obs.	120202	120202	120202	120202	120202	120202	120202
m	0.12	0.46	1.36	0.05	0.35	2.23	4.80

Note : Panel A refers to the balanced panel of establishments which adopted terminations by agreement before the end of 2014. Panel B covers the subpanel of establishments in manufacturing and construction sectors whereas panel C refers to the service sector. The table shows the result of establishment-level regressions where the dependent variable is the quarterly rate of (a) dismissals for cause (column 1), (b) quits (column 2), (c) dismissals for economic reasons (column 3), (d) retirements (column 4) as well as the overall rate of termination of permanent workers (column 5) and (e) the number of employees (in log) (column 6). The set of regressors includes a $Post_{jt}$ dummy indicating that the observation is after the beginning of the use of terminations by agreement, as well as a set of establishment fixed effects (5837 establishments) and quarter fixed effects (44 quarters). We only report estimated impact of $Post_{jt}$. *** p<0.01, ** p<0.05, * p<0.1. All coefficients are multiplied by 100 (and represent effects in ppt).

Table 2.2 – The effect of terminations by agreement on permanent contract terminations, by age, gender and occupational subgroups

	Termination by agreement	Non-econ. Dismissal	Quit	Economic Dismissal	Retirement	Overall termination
			A- Men vs. Women			
$Post_{jt} \times I_k$	-.106***	.016	-.034	.027	.007	-.104**
	(.010)	(.016)	(.027)	(.027)	(.017)	(.048)
Obs.	493812	493812	493812	493812	493812	493812
m	.11	.43	1.02	.12	.38	1.99
			B- Executives vs. non-executives			
$Post_{jt} \times I_k$	.252***	-.149***	-.050	-.015	.005	.033
	(.022)	(.045)	(.062)	(.020)	(.035)	(.102)
Obs.	413918	413918	413918	413918	413918	413918
m	.14	.49	1.04	.11	.45	2.25
			C- Older vs. Younger workers			
$Post_{jt} \times I_k$	-.090***	.009	-.025	.002	-.029*	-.137***
	(.009)	(.020)	(.031)	(.010)	(.014)	(.043)
Obs.	470162	470162	470162	470162	470162	470162
m	.11	.49	1.17	.10	.36	2.23

Note : The Table refers to the balanced panel of establishment which adopted terminations by agreement before the end of 2014. The Table shows the result of estimating model (2.2) when we contrast men and women (panel A), executive and non-executive workers (panel B), workers aged more than 40 and less than 40 (panel C). For each establishment j, subgroup k and quarter t, the dependent variable is the rate of terminations by agreement (column 1), the rate of dismissals for cause (column 2), the rate of quits (column 3), the rate of retirements (column 4) and the overall rate of termination of permanent workers (column 5). The Table reports the effect of ($Post_{jt} \times I_k$), namely the effect of the interaction between a dummy ($Post_{jt}$) indicating that the date of observation t is after the date of adoption of termination by agreement by establishment j and a subgroup dummy (I_k). The model also includes full sets of establishment × date, subgroup × date and establishment × subgroup fixed effects. *** p<0.01, ** p<0.05, * p<0.1. All coefficients are multiplied by 100 (and represent effects in ppt).

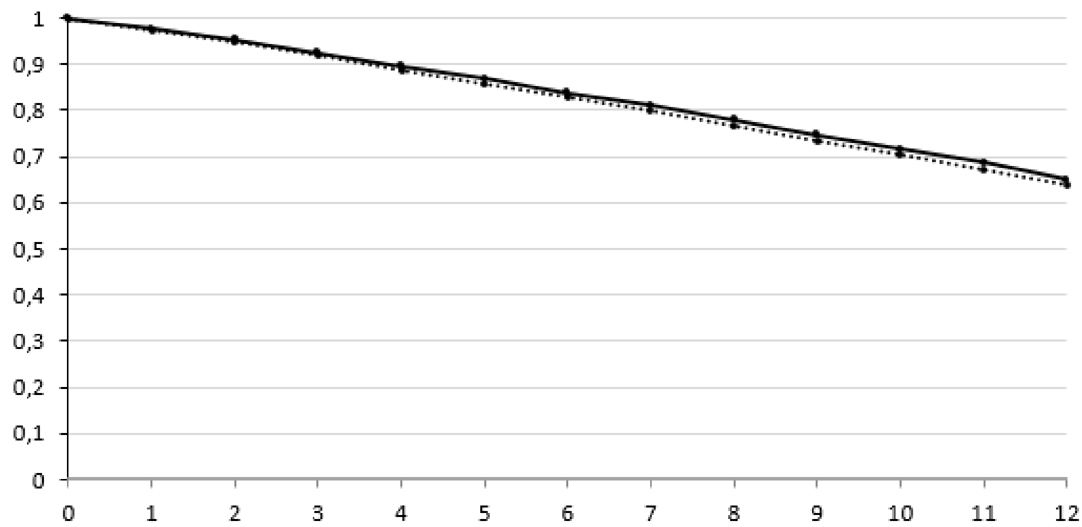
Table 2.3 – Effect of potential exposure to terminations by agreement on workers' trajectories

	Termination by Agreement	Unemployment spell	Number of Jobs	Employment in 2012	Hourly Wage in 2012
Q_i	.00137***	.00215***	.00416***	-.00036	.00127***
	(.00012)	(.00029)	(.00090)	(.00029)	(.00035)
Obs.	142791	142791	142791	142791	119131

Note : The table refers to the sample of workers who are employed in 2008 and whose 2008 employer adopt terminations by agreement between 2008 and 2011. The table shows the result of regressing workers' post-reform outcomes on their number of quarters of potential exposure to terminations by agreement (as predicted by the date on which their 2008 employed adopted the new procedure). The dependent variable is a dummy indicating that the worker signed a termination by agreement between 2008 and 2011 (column 1), a dummy indicating that the worker went through a period of unemployment between 2008 and 2011 (column 2), the (log) number of different jobs held between 2008 and 2011 (column 3), a dummy indicating unemployment in 2012 (column 4) and the 2012 hourly wage (column 5). Controls include individual age, gender, education as well as the size, average wage and share of skilled worker of the 2008 employer. *** p<0.01, ** p<0.05, * p<0.1.

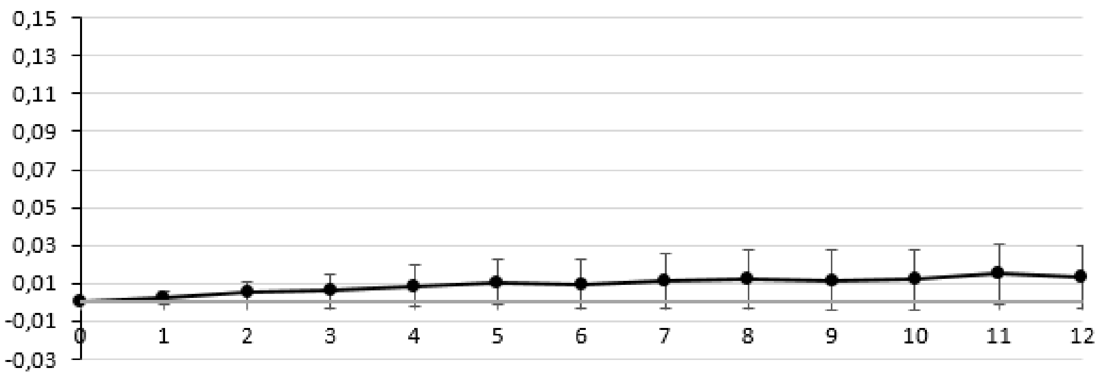
Annexe 2.A Figures and Tables

FIGURE 2.A.1 – Adoption of terminations by agreement and establishments' survival in the balanced panel



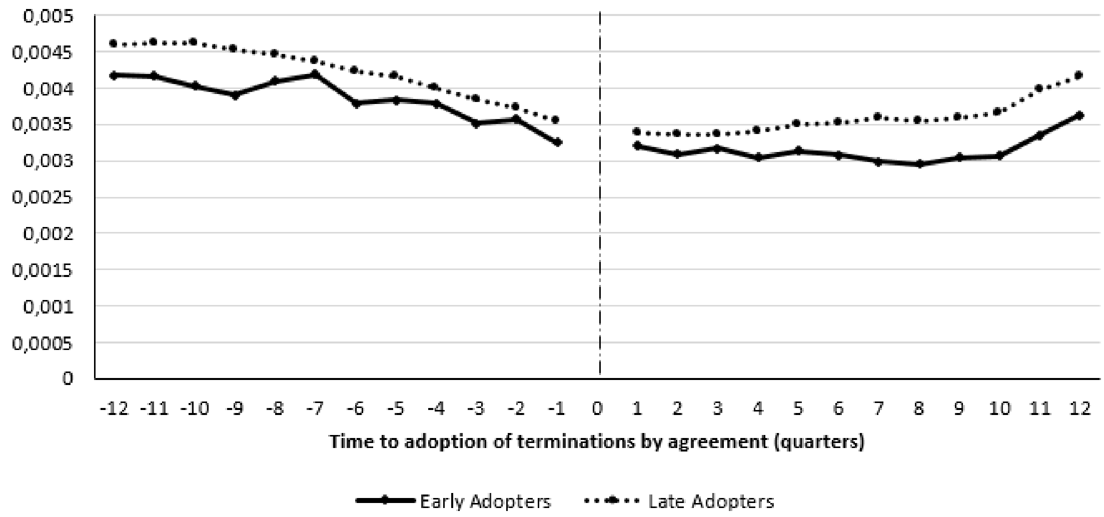
Note : For each potential date t_0 of adoption of terminations by agreement, it is possible to consider (i) establishments continuously present in the DMMO database from 2004-Q1 to t_0 and adopting terminations by agreement in t_0 and (ii) establishments continuously present in the database from 2004-Q1 to t_0, but not adopting terminations by agreement in t_0. For each of these two groups, it is then possible to compute the survival rate in the balanced panel k quarters after t_0. For $k = 1$ to 12, the solid line represents the average of the survival rates of the establishments in the first group across all possible t_0's while the dotted line represents the average of the survival rates of the establishments in the second group. Reading : 90% of the establishments that were present in the balanced panel at the time of their adoption of terminations by agreement are still in the balanced panel 4 quarters later. The survival rate is only slightly lower for institutions that were still in the balanced panel at the time the first adopted terminations by agreement, but had not yet adopted terminations by agreement at that time.

FIGURE 2.A.2 – Adoption of terminations by agreement and differential rate of survival the balanced panel



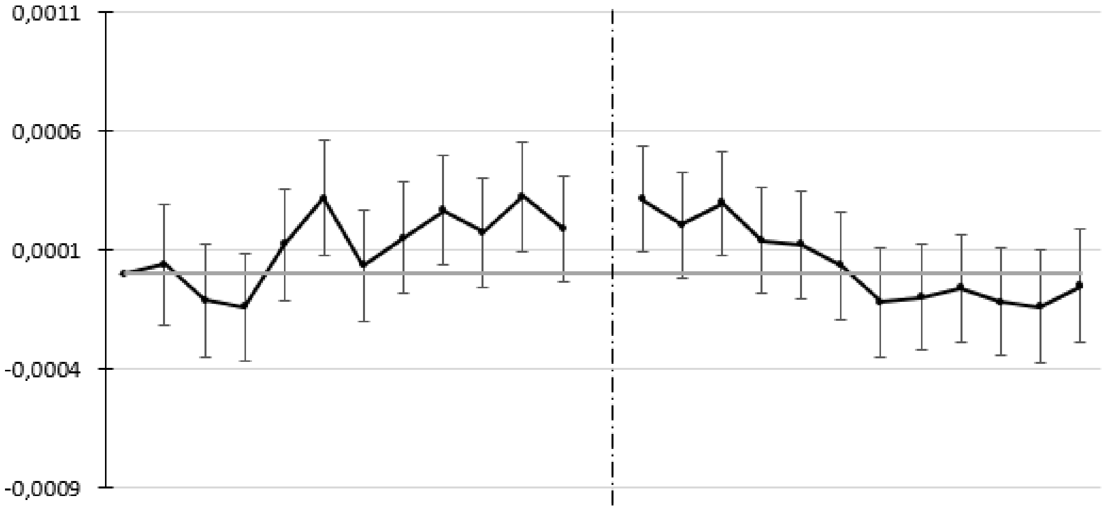
Note : The curve shows the evolution of the difference between the solid line and the dotted line shown in Figure 2.A.1. The vertical lines represent the confidence intervals. The source and field are the same as those used in Figure 2.A.1.

Figure 2.A.3 – Retirement rate before and after the adoption of terminations by agreement



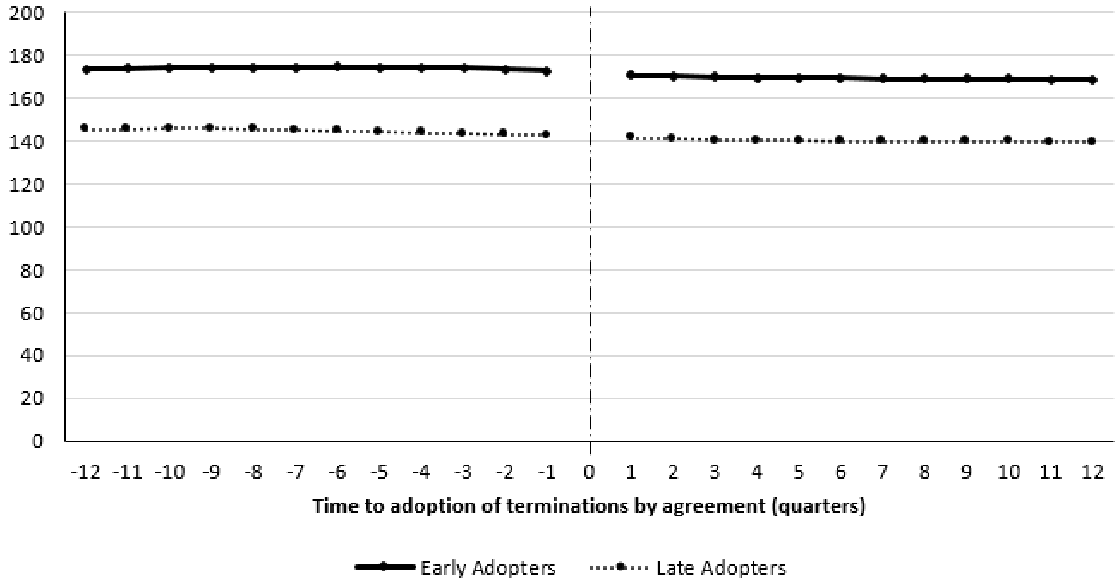
Note : The figure focuses on establishments which began to use terminations by agreement between 2008 and 2011.The solid line shows the evolution of the retirement rate over a period of 6 years, taking the date of first termination by agreement as the origin of the time scale. The dotted line shows the rate of retirement observed at the same dates in establishments that had still not began to use terminations by agreement by the end of 2014.

Figure 2.A.4 – Difference in retirement rates between early adopters and late adopters



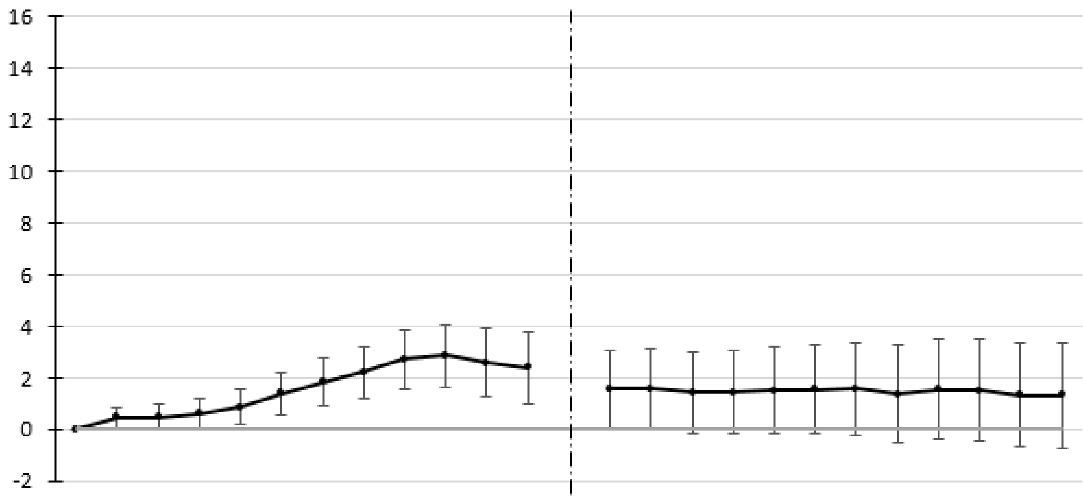
Note : The curve shows the evolution of the difference between the solid line and the dotted line shown in Figure 2.A.3. The vertical lines represent the confidence intervals. The source and field are the same as those used in Figure 2.A.3.

Figure 2.A.5 – Number of employees before and after the adoption of terminations by agreement



Note : The figure focuses on establishments which began to use terminations by agreement between 2008 and 2011. The solid line shows the evolution of their number of employees over a period of 6 years, taking the date of the first termination by agreement as the origin of the time scale. The dotted line shows the number of employees observed at the same dates in establishments that had still not began to use terminations by agreement by the end of 2014.

Figure 2.A.6 – Difference in number of employees between early adopters and late adopters



Note : The curve shows the evolution of the difference between the solid line and the dotted line shown in Figure 2.A.5. The vertical lines represent the confidence intervals. The source and field are the same as those used in Figure 2.A.5.

Table 2.A.1 – Description of the balanced panel

	All	Adoption before 2014	No adoption before 2014
Nb of employees 2004-Q1	163	167	143
Manufacturing and construction (%)	49	52	34
Service (%)	50	47	65
Parisian region (%)	5.2	5.3	4.9
dismissals for cause 2004-Q1 (%)	.48	.49	.45
Quits 2004-Q1 (%)	1.10	1.09	1.17
Economic dismissals 2004-Q1 (%)	.09	.10	.06
Terminations of permanent workers, 2004-Q1 (%)	1.84	1.84	1.80
N	7085	5837	1248

Note : The table shows the main characteristics (as measured in 2004-Q1) of the establishments of the balanced panel, i.e., the establishments present in the DMMO database from 2004-Q1 to 2014-Q4. The characteristics under consideration are the number of employees, the industries (manufacturing/service), the location (Paris region/other) and finally the different rates of permanent contract separation. The Table gives the average characteristics for all the establishments in the sample (first column) and then separately for those which adopted terminations by agreement before the end of 2014 (second column) and for those that had not yet used terminations by agreement by the end of 2014 (third column). Reading : the establishments in the balanced panel had an average of 163 employees at the beginning of 2004 and 49% of these establishments were in industry. During the first quarter of 2004, 1.10% of the workforce quitted the establishments.

Table 2.A.2 – The effect of adopting terminations by agreement on permanent contract terminations and number of persons employed : an analysis on the subsample where the two quarters prior to the first termination by agreement are dropped.

	Non-econ. Dismissal	Quit	Economic Dismissal	Retirement	Overall termination	Nb Employees (log)
			A- All industries			
$Post_{jt}$	-.038***	-.006	.002	-.005	.315***	-.0056
	(.008)	(.015)	(.012)	(.007)	(.025)	(.0030)
Obs.	245070	245070	245070	245070	245070	245070
m	0.41	1.00	.09	0.37	1.93	4.85
			B- Construction and manufacturing			
$Post_{jt}$	-.017	-.006	-.001	-.003	.377***	-.0029
	(.010)	(.014)	(.021)	(.010)	(0.032)	(.0031)
Obs.	129112	129112	129112	129112	129112	129112
m	.38	.68	.13	.40	1.65	4.90
			C- Service			
$Post_{jt}$	-.059***	-.019	.004	.001	.333***	-.0031
	(.012)	(.028)	(.011)	(.010)	(.040)	(.0041)
Obs.	114782	114782	114782	114782	114782	114782
m	0.46	1.36	0,05	0.33	2.23	4.80

Note : the Table replicates the regression analysis of Table 1 when we drop (for each establishment) the two observations before the adoption of terminations by agreement. *** p<0.01, ** p<0.05, * p<0.1. All coefficients are multiplied by 100 (and represent effects in ppt).

Table 2.A.3 – The effect of adopting terminations by agreement on permanent contract terminations and number of persons employed : an analysis on the unbalanced panel.

	Non-econ. Dismissal	Quit	Economic Dismissal	Retirement	Overall termination	Nb Employees (log)
			A- All industries			
$Post_{jt}$	-.020***	.009	.013	-.008*	.415***	-.0081***
	(.005)	(.010)	(.008)	(.004)	(.018)	(.0020)
Obs.	617855	617855	617855	617855	617855	617855
m	.49	1.16	.11	.35	2.17	4.92
			B- Construction and manufacturing			
$Post_{jt}$	-.017*	-.019	-.017	-.003	.436***	-.0092***
	(.007)	(.011)	(.018)	(.006)	(.026)	(.0031)
Obs.	268393	268393	268393	268393	268393	268393
m	.40	.70	.16	.39	1.76	4.95
			C- Service			
$Post_{jt}$	-.023**	-.008	.005	-.005	.409***	-.0032
	(.008)	(.016)	(.007)	(.005)	(.026)	(.0027)
Obs.	345739	345739	345739	345739	345739	345739
m	.55	1.51	.06	.31	2.50	4.89

Note : the Table replicates the regression analysis of Table 1 for the unbalanced panel of establishment which adopted terminations by agreement before the end of 2014 and for which we have DMMO observations for 80% or more of the quarters between 2004 and 2014. *** p<0.01, ** p<0.05, * p<0.1. All coefficients are multiplied by 100 (and represent effects in ppt).

Table 2.A.4 – Effect of potential exposure to terminations by agreement on workers' trajectories - placebo regressions

	Unemployment spell 2004-2007	Number of Jobs 2004-2007	Hourly Wage in 2008
Q_i	.-.00003	.00050	.00001
	(.00029)	(.00109)	(.00033)
Obs.	142791	142791	142791

Note : The table refers to the same sample as Table 2.3. It shows the result of regressing workers' pre-reform outcomes on their number of quarters of potential exposure to terminations by agreement (as predicted by the date on which their 2008 employed adopted the new procedure). The dependent variables are a dummy indicating that the worker went through a period of unemployment between 2004 and 2007 (column 1), the (log) number of different jobs held between 2004 and 2007 (column 2), a dummy indicating unemployment in 2012 (column 4) and the 2008 hourly wage (column 3). Controls include individual age, gender, education as well as the size, average wage and share of skilled worker of the 2008 employer. *** p<0.01, ** p<0.05, * p<0.1.

Annexe 2.B Conceptual Framework

In this appendix, we develop a simple conceptual framework to make precise how exactly the introduction of terminations by agreements may affect firms' behavior. We first develop a model for firms' behavior before the introduction of terminations by agreements. In a second step, we look at how (and when) the introduction of terminations by agreement entails a change in these behaviors.

2.B.1 Technology and adjustment costs

As regards technology, we assume that the production function (denoted F) depends on labor input only. Specifically, we assume that $y_{jt} = F(x_{jt}, \pi_{jt})$ where, for each time interval $[t, t+1]$, y_{jt} represents the output of firm j, x_{jt} the number of workers and π_{jt} a productivity parameter.

Entries and exits of workers are assumed to take place at the beginning of each time interval. We denote h_{jt} the number of hiring and l_{jt} the number of workers who are dismissed for economic reasons at the beginning of $[t, t+1]$. Also, we denote q_{jt} the number of workers who quit, f_{jt} the number of workers who are dismissed for cause and r_{jt} the number of workers who retire at the beginning of $[t, t+1]$.

Hiring and dismissals justified by economic reasons are assumed to be under the control of the firm whereas the flows of quits, dismissals for cause and retirements are assumed to be taken as given by the firm [21]. We denote $s_{it} = q_{it} + f_{it} + r_{it}$ the aggregate number of exogenous exits at the beginning of $[t, t+1]$. In this framework, the objective of the firm is to choose h_{jt} and l_{jt} as a function of π_{jt} and s_{jt} so as to maximize an objective function which can be written as,

$$V_{jt} = E_t\{\sum_{k\geq t} \delta^{k-t}(F(x_{jk}, \pi_{jk}) - w_{jk}x_{jk} - c_H h_{jk} - c_L l_{jk})\} \tag{2.3}$$

subject to conditions (a) $x_{jk} = x_{jk-1} + h_{jk} - l_{jk} - s_{jk}$, (b) $h_{jk} \geq 0$ and (c) $l_{jk} \geq 0$,

where w_{jt} represents the wage rate and where adjustment costs are assumed linear, with c_H representing the per unit hiring cost and c_L the per unit lay off costs. The discount rate δ is assumed to be less than one (i.e., $\delta \leq 1$).

21. The model assumes implicitly that dismissals for cause can occur only in very specific cases (serious misconduct, individual performance-related problems, etc.) and that, in these instances, firms cannot avoid terminating employment contracts (using either dismissals for cause or terminations by agreement). The fact that dismissals for cause do not really increase during economic downturn is consistent with their being difficult to manipulate.

2.B.2 First-order conditions and state variables

After dropping subscript j, the (three) first-order conditions can be written,

$$F'(x_t, \pi_t) - w_t - \lambda_t + E_t\{\lambda_{t+1}\} = 0, c_H + \lambda_t + \gamma_{Ht} = 0 \text{ and} - c_L - \lambda_t + \gamma_{Lt} = 0 \quad (2.4)$$

where λ_t, γ_{Ht} and γ_{Lt} represent the Lagrange multipliers associated to constraints (a), (b) and (c). These Lagrange multipliers satisfy $\gamma_{Ht}h_t = \gamma_{Lt}l_t = 0$ so that, taken together, the two last first-order conditions imply that

$$(c_H + c_L)h_t l_t = 0 \quad (2.5)$$

It entails that hiring and layoffs for economic reasons cannot be strictly positive at the same time and that there are only three possible optimal responses at the beginning of each period. The first response involves some dismissals for economic reasons ($l_t > 0$), but no hiring ($h_t = 0$). It corresponds to periods of employment downsizing through both exogenous exits and layoffs for economic reasons. The second response involves neither hiring nor dismissals for economic reasons ($l_t = h_t = 0$). It corresponds to periods of employment downsizing through exogenous exits only. The last response involves some hiring ($h_t > 0$), but no dismissals ($l_t = 0$). It corresponds to periods of expansion (when the flows of hiring exceeds the flows of exogenous exits) or to periods of downsizing through partial replacement of quitters and retirees (when the flows of hiring are not as large as the flows of exogenous exits).

Eventually, given that $h_t l_t = 0$, both h_t and l_t depends only on $(x_t - x_{t-1})$, namely $h_t = (x_t - x_{t-1} + s_t)$ and $l_t = 0$ when $x_t - x_{t-1} + s_t \geq 0$ while $h_t = 0$ and $l_t = -(x_t - x_{t-1} + s_t)$ when $x_t - x_{t-1} + s_t < 0$. Hence the only endogenous state variable is x_t and the only question at the beginning of each period is to define the value of x_t which maximize the objective function as a function of present and past productivity shocks.

2.B.3 Pre-reform optimal strategies

To further analyze how firms choose between the different possible strategies, we are going to focus on the case where F can be proxied by a linear-quadratic function (i.e., $F(x, \pi) = \pi x - \frac{bx^2}{2}$) and where the shocks $\epsilon_t = \pi_t - w_t$ to the marginal profit per worker follow a two-state markovian chain. We denote ϵ^+ and ϵ^- the two values that $\epsilon_t = \pi_t - w_t$ can take over time and $p(q)$ will represent the probability of moving from ϵ^+ to ϵ^- (ϵ^- to ϵ^+) from one period to the next.

Parameter $\Delta = \frac{\epsilon^+ - \epsilon^-}{b}$ represents the magnitude of the downward shift in labor demand that would be observed after a bad shock if adjustment costs were negligible (i.e., if c_H and c_L were negligible). Eventually, we assume that exogenous exits are constant over time and we denote S their aggregate level. In this set up, it is possible to show that the optimal adjustment strategy of the firm depends not only on adjustment costs (as measured by c_H and c_L), but also on the $\Delta - S$ parameter, namely the magnitude of the downward adjustment that firm would find optimal to perform if adjustment costs were negligible.

Proposition 1 (pre-reform behavior) :

Denoting $\Delta = \frac{\epsilon^+ - \epsilon^-}{b}$ the magnitude of labor demand shocks, $C_{pre} = \frac{c_H + c_L}{b}$ the magnitude of adjustment costs and S the aggregate flows of exogenous exits, the pre-reform behavior of firms depends on $\Delta - S$ and C_{pre}.

— If $\Delta - S < 0$ firms' employment level follows a two-state markovian chain and firms adjust to changes in economic context through changes in hiring rates only. Hiring is below the replacement level during economic slowdown, above the replacement level during economic recovery and at the replacement level the rest of the time.

— If $0 < \Delta - S < (1 + (1 - \delta)p)C_{pre}$, firms' employment level follows a three-state markovian chain and firms adjust to labor demand shocks either through changes in hiring rates or by staying put. Specifically, they stay put during economic slowdown and hires workers the rest of the time, with hiring being either below, above or at the replacement level depending on the economic context.

— If $\Delta - S > (1 + (1 - \delta)p)C_{pre}$, firms' employment level follows a three-state markovian chain and firms adjust to labor demand shocks either through changes in hiring rates or by dismissing workers. Specifically, they dismiss workers during economic slowdown and hires workers the rest of the time, with hiring being either below, above or at the replacement level depending on the economic context.

[Proof :

— If $\Delta - S < 0$, one checks that the two state markovian chain defined by $x(\epsilon_t) = \frac{\epsilon_t - c_H(1-\delta)}{b}$ satisfies the first-order conditions. Given that the return function is concave, first-order conditions are also sufficient, so that this plan represents the optimum. The firm adapt to shocks by setting $h_t = S + \frac{\epsilon_t - \epsilon_{t-1}}{b}$, namely by setting ht either above, below or at the replacement level S (depending on $\epsilon_t - \epsilon_{t-1}$).

— If $0 < \Delta - S < (1 + (1 - \delta)p)C_{pre}$, we can use a similar reasoning to show that the solution is now given by the three state markovian chain defined by $x_t = x(\epsilon_{t-1}, \epsilon_t)$ with : $x(\epsilon^+, \epsilon^+) = x(\epsilon^-, \epsilon^+) = \frac{\epsilon^+ - (1-\delta p)c_H + \delta(1-p)\lambda^{+/-}}{b}$; $x(\epsilon^+, \epsilon^-) = \frac{\epsilon^- + \delta c_H - \lambda^{\pm}}{b} = x(\epsilon^+, \epsilon^+) - S$ and $x(\epsilon^-, \epsilon^-) = \frac{\epsilon^- - (1-\delta)c_H}{b}$, where $\lambda^{+/-} = \frac{b(S-\Delta) + (1+\delta(1-p)c_H)}{1+\delta(1-p)}$ is the Lagrange multiplier when $\epsilon_t = \epsilon^-$ and $\epsilon_{t-1} = \epsilon^+$. It is easy to check that $-c_L < \lambda^{\pm} < c_H$ which is the condition for both hiring and lay off to be zero when $\epsilon_t = \epsilon^-$ and $\epsilon_{t-1} = \epsilon^+$.

— Eventually, if $\Delta - S > (1 + (1 - \delta)p)C_{pre}$, the solution is given that the three-state markovian chain defined by $x_t = x(\epsilon_{t-1}, \epsilon_t)$ with : $x(\epsilon^+, \epsilon^+) = x(\epsilon^-, \epsilon^+) = \frac{\epsilon^+ - (1-\delta p)c_H - (1-p)\delta c_L}{b}$; $x(\epsilon^+, \epsilon^-) = \frac{\epsilon^- + \delta c_H + c_L}{b}$ and $x(\epsilon^-, \epsilon^-) = \frac{\epsilon^- - (1-\delta)c_H}{b}$.]

2.B.4 After the reform

After the reform, employers may first find of interest to sign terminations by agreement with workers that would otherwise be dismissed for cause. Among the f_t workers who are about to be dismissed for cause during $[t, t+1]$, we denote f_{rt} (with $f_{rt} \leq f_t$) the number of those with whom it is possible to sign a termination by agreement at a cost which is not as large as the expected cost of dismissing these workers for cause. For the sake of simplicity, we assume that f_{rt} is taken as given by the firm.

Some other workers are not about to be dismissed for cause, nor about to quit their firms, but are nonetheless ready to sign a termination by agreement. As discussed above, these workers are typically those who would like to leave their employer, but have no clear outside option yet. For them, signing a termination by agreement represents a better option than quitting, because it does not involve loosing eligibility to severance payments and unemployment benefits. Denoting c_R the cost for the employer of signing a termination by agreement with these workers and assuming that c_R is weaker than the cost of dismissing these workers for economic reason (denoted c_L), employers may find of interest to sign terminations by agreement with these workers . In the remainder, we denote rc_{mt} the number of such workers, which also represent the maximum number of terminations by agreement that the employer can sign with employees who are neither about to be dismissed for cause nor about to quit. We assume that rc_{mt} is taken as given by the firm, exactly as quits. For each time interval and each firm, we will keep on denoting h_t the number of hiring, l_t the number of layoffs and we will denote rc_{jt} the number of termination by agreement that are actually signed (with $rc_t \leq rc_{mt}$). With these notations, the post-reform objective of the firm becomes to choose h_{jt}, l_{jt} and rc_{jt} as a function of π_t and s_t so as to maximize an objective function which can be written as,

$$V_{jt} = E_t\{\sum_{k \geq t} \delta^{k-t}(F(x_k, \pi_k) - w_k x_k - c_H h_k - c_L l_k - c_R rc_k)\}, \tag{2.6}$$

subject to : $h_k \geq 0, l_k \geq 0, rc_{mk} \geq rc_k \geq 0$ and $x_k = x_{k-1} + h_k - l_k - rc_k - s_k$,

where δ, w_t, c_H and c_L represent the same economic variables and parameters as in the previous subsection and where c_R captures per unit cost of termination by agreements. We keep on assuming that exogenous outflows are constant over time (still denoted S) and, for the sake of simplicity, we further assume that rc_{mt} is constant over time (denoted R). Also, we still denote $\Delta = \frac{\epsilon^+ - \epsilon^-}{b}$ the magnitude of the downward shift in labor demand that would be observed after a bad shock if adjustment costs were negligible (i.e., if c_H, c_L and c_R were negligible), so that $\Delta - S$ still represents the magnitude of the downward adjustment that firms would find optimal to perform if adjustment costs were negligible. In this set-up, the optimal strategy of the firm still depends on $\Delta - S$, but also on R.

Proposition 2 (firms' behavior after the reform)

Denoting $C_{post} = \frac{c_H + c_R}{b}$ a measure of post-reform adjustment costs and R the number of workers who are not about to quit or to be dismissed, but who are nonetheless ready to sign a termination by agreement, the behavior of firms after the reform is the same as before the reform only when R is negligible or when $\Delta - S$ is not too large. Specifically, we have,

— If $\Delta - S < 0$, the adjustment regime is the same after the reform as before the reform. Firms keep on adjusting labor input by setting the number of hiring either above, below or at the replacement level.

— If $0 < \Delta - S < (1 + \delta(1 - p))C_{post}$, the adjustment regime is again the same after the reform as before the reform. The firms stay put during economic downturn and adjust the number of hiring the rest of the time.

— If $(1 + \delta(1 - p))C_{post} < \Delta - S < (1 + \delta(1 - p))C_{post} + R$, the optimal adjustment regime is not the same after and before the reform. For these values of $\Delta - S$, firms start using terminations by agreement during economic downturn whereas they would have stayed put pre-reform. For these values of $\Delta - S$, the reform induces a rise in separation rates, but no substitution of terminations by agreement for dismissals justified by economic reasons.

— If $R + (1 + \delta(1 - p))C_{post} < \Delta - S < (1 + \delta(1 - p))C_{pre} + R$, the optimal adjustment regime is not the same after and before the reform. For these values $\Delta - S$, firms use the maximum number of terminations (i.e., R) by agreement during economic downturn whereas they would have stayed put pre-reform. For these values of $\Delta - S$, the reform induces again a rise in separation rates, but no substitution of terminations by agreement for dismissals justified by economic reasons.

— For even larger value of $\Delta - S$, firms use terminations by agreement in contexts where, pre-reform, they would have used dismissals for economic reason only. For these larger values $\Delta - S$, the reform induced a rise in overall separation rates as well as substitution of terminations by agreement for dismissals justified by economic reasons.

[Proof : The proof follows the same line as the proof of proposition 1.

— When $\Delta - S < 0$ or when $0 < \Delta - S < (1 + \delta(1 - p))C_{post}$, it is not difficult to check that the two-state and three-state markovian chains described at the beginning of the proof of Proposition 1 still remain optimal plans.

— By contrast, when $(1+\delta(1-p))C_{post} < \Delta - S < (1+\delta(1-p))C_{post} + R$, the optimal solution is given that the three-state markovian chain defined by $x_t = x(\epsilon_{t-1}, \epsilon_t)$ with : $x(\epsilon^+, \epsilon^+) =$

$x(\epsilon^-, \epsilon^+) = \frac{\epsilon^+ - (1-\delta p)c_H - (1-p)\delta c_R}{b}$; $x(\epsilon^+, \epsilon^-) = \frac{\epsilon^- + \delta c_H + c_R}{b}$ and $x(\epsilon^-, \epsilon^-) = \frac{\epsilon^- - (1-\delta)c_H}{b}$.

— When $(1 + \delta(1 - p))C_{post} + R < \Delta - S < (1 + \delta(1 - p))C_{pre} + R$, the optimal solution is given by the three-state markovian chain defined by $x_t = x(\epsilon_{t-1}, \epsilon_t)$ with : $x(\epsilon^+, \epsilon^+) = x(\epsilon^-, \epsilon^+) = \frac{\epsilon^+ - (1-\delta p)c_H + \delta(1-p)\lambda^{+/-}}{b}$; $x(\epsilon^+, \epsilon^-) = \frac{\epsilon^- + \delta c_H - \lambda^{+/-}}{b} = x(\epsilon^+, \epsilon^+) - S - R$ and $x(\epsilon^-, \epsilon^-) = \frac{\epsilon^- - (1-\delta)c_H}{b}$, where $\lambda^{+/-} = \frac{b(S+R-\Delta)+(1+\delta(1-p)c_H)}{1+\delta(1-p)}$ is the Lagrange multiplier when $\epsilon_t = \epsilon^-$ and $\epsilon_{t-1} = \epsilon^+$.

— Eventually, when $(1 + \delta(1 - p))C_{pre} + R < \Delta - S$, the optimal solution is given that the three-state markovian chain defined by $x_t = x(\epsilon_{t-1}, \epsilon_t)$ with : $x(\epsilon^+, \epsilon^+) = x(\epsilon^-, \epsilon^+) = \frac{\epsilon^+ - (1-\delta p)c_H - (1-p)\delta c_L}{b}$; $x(\epsilon^+, \epsilon^-) = \frac{\epsilon^- + \delta c_H + c_L}{b}$ and $x(\epsilon^-, \epsilon^-) = \frac{\epsilon^- - (1-\delta)c_H}{b}$.]

In our set up, the difference $\Delta - S$ represents the magnitude of the downward adjustment that firms would like to perform when they are hit by adverse shocks. In practice, firms will perform these adjustments only if adjustment costs are not too large. Assuming that $c_R < c_L$ and that the number R of would-be movers is positive, it may become possible for firms to perform some downward adjustments after the reform (through terminations by agreements) in cases where no adjustments would have been seen pre-reform (because of layoff costs). In this scenario, the introduction of terminations by agreement coincides not only with a decline in dismissals for cause, but also with a rise in the overall number of separations. It is an empirical question, however, whether firms meet these conditions.

Annexe 2.C A "stacked" difference-in-difference approach

In an event analysis with a staggered design (where all units are progressively treated, cohort by cohort), the two way fixed effect estimator of parameter γ in our main model may be difficult to interpret (see Abraham et Sun (2020), Goodman-Bacon (2018), De Chaisemartin et D'Haultfoeuille (2019)). Specifically, when treatment effects are heterogeneous across cohorts ; this estimator recovers a linear combination of cohort specific average treatment effects where some weights can be negative, mostly because early and late cohorts are not observed on intervals of time of same length.

To test the robustness of our results to heterogeneous effects, we developed an event-by-event analysis in the spirit of Cengiz *et al.* (2019)). The first step of the procedure consists in estimating the impact of the treatment separately for each cohort, using cohort-specific sample covering time intervals of same length (so that effects for early and late cohorts are estimated on time intervals of same length). The second step consists in taking the average across these cohort-specific effects.

To be more specific, for each one of the twelve quarters e between $e =$2009-Q1 and $e =$2011-Q4, we first consider A_e the subset of establishments which introduced termination by agreements either in e or after $e + 12$ (i.e., three or more years later). Secondly, for each establishment j in A_e, we consider S_{je} the sample of observations of establishment j made between $t = e - 12$ and $t = e + 12$, namely between three years before and three years after t_e. Eventually, for each t_e between 2009-Q1 and 2011-Q4, we define S_e, the union of the different S_{je} for j in A_e. For each e, sample S_e makes it possible to compare over the period $[e - 12, e + 12]$ the establishments that are treated in e with the establishments that will be treated three or more years later. Specifically, we re-estimated our main model (2.1) on each one of the twelve cohort-specific samples S_e so as to obtain twelve estimated parameters γ_e. Table 2.C.5 shows the weighted average of these estimated γ_e for the different outcomes of interest, where weights are proportional to the size of the different S_e. Generally speaking, we obtain average effects that are very similar to those shown in Table 2.1.

TABLE 2.C.5 – The effect of adopting terminations by agreement on permanent contract terminations and number of employees : Event-by-event analysis.

	Non-econ. Dismissal	Quit	Economic Dismissal	Retirement	Overall termination
			A- All industries		
$Post_{jt}$	-.031***	.010	.004	-0.019*	0.272***
	(.008)	(.015)	(.011)	(.008)	(.024)
Obs.	1262349	1262349	1262349	1262349	1262349
m	.49	1.16	.11	.35	2.17

Note : The table shows the result the event-by-event analysis described above where the dependent variable is the quarterly rate of (a) dismissals for cause (column 1), (b) quits (column 2), (c) dismissals for economic reasons (column 3), (d) retirements (column 4) as well as the overall rate of termination of permanent workers (column 5). *** p<0.01, ** p<0.05, * p<0.1. All coefficients are multiplied by 100 (and represent effects in ppt).

CHAPITRE 3

Labor Market Concentration and Stayers' Wages : Evidence from France

Cyprien Batut [1]

1. version étendue d'un article joint avec Andrea Bassanini et Eve Caroli. The views expressed here do not represent those of the OECD or its member countries. Cyprien Batut acknowledges support from the PSE Labor Chair. This research has also been supported by a public grant overseen by the French National Research Agency (ANR) as part of the 'Investissements d'avenir' program (reference : ANR-10-EQPX-17, Centre d'accès sécurisé aux données, CASD). Usual disclaimers apply.

Abstract :

We investigate the impact of labor market concentration on stayers' wages, where stayers are defined as individuals who remain employed in the same firm for at least two consecutive years. Using administrative data for France, we show that the elasticity of stayers' wages to labor market concentration is negative but small (about -0.014) once controlling for firm productivity, product market competition and match-specific heterogeneity. Given the strong wage rigidities characterizing the French labor market, this estimate can be seen as a lower bound of the effect of labor market concentration on stayers' wages in an international perspective.

JEL Codes : J31, J42, L41

Keywords : labor market concentration, monopsony, wages, stayers, match-specific heterogeneity

3.1 Introduction

How labor market concentration affects wages has been the subject of a burgeoning literature in recent years. Many papers have shown that a substantial proportion of individuals are employed in labor markets that are at least moderately concentrated according to the thresholds defined by the US Horizontal Merger Guidelines and that this has a depressing effect on average wages, consistent with a monopsony model (Azar *et al.*, 2018; Martins, 2018; Abel *et al.*, 2018; Rinz, 2018; Benmelech *et al.*, 2018). Beyond average wages, there is evidence that concentration affects posted wages for new vacancies (Azar *et al.*, 2020) and actual wages of new hires (Marinescu *et al.*, 2019).

In this paper we investigate the impact of labor market concentration on stayers' wages, where stayers are defined as individuals who remain employed in the same firm for at least two consecutive years. Looking at stayers is important since they represent a large share of all employees in any given year in all OECD countries (OECD, 2010). Stayers' wages have also been shown to be the largest contributor to aggregate wage growth, at least in recent years (Hahn *et al.*, 2017, 2018). Understanding the impact of monopsony on this group of workers is therefore important to understand how monopsony may explain wage trends. However, so far, no systematic evidence has been provided on how labor market concentration affects stayers' wages. Aggregate effects found in the literature could in principle be driven by new hires only or by both new hires and stayers. In the former case though, this would imply that labor market concentration only affects wages at the margin, i.e. for individuals who change jobs. In this paper, we use French data and show that the elasticity of stayers' wages to labor market concentration is negative but small : about -0.014 in our preferred specification. France is interesting in this respect since, due to automatic extension of collective agreements, almost all employees are covered by them (Babecký *et al.*, 2010) so that wage rigidities are strong. Our results suggest that, even in this context, labor market concentration has a small depressing effect on the earnings of the vast majority of workers, i.e. those who do not change employer from one year to the other.

Estimating the elasticity of wages to labor market concentration on stayers only also has the advantage of cancelling out any potential confounding effects of changes in the composition of the workforce and/or assortative matching between workers and firms. Qiu et Sojourner (2019) find that labor market concentration tends to reduce the average level of education of employees, so that part of the decrease in wages associated with higher concentration could be due to a reduction in the quality of the workforce. Moreover, Macaluso *et al.* (2019) show that the greater the labor market concentration, the higher the skill requirements imposed by firms, conditional on workers' education. This suggests that firms in concentrated labor markets are more selective in choosing workers who best fit their specific needs. The effects we estimate in the current paper are net of any composition and/or sorting effect since not only do we account for time-invariant workers' characteristics but also for match-specific heterogeneity.

To our knowledge, the only other paper estimating the impact of labor market concentration on stayers' wages is Arnold (2019). It shows that mergers and acquisitions that lead to higher concentration have a negative effect on the wages of employees who stay in the firms that have merged. We identify the effect of labor market concentration on non-merged companies and show that when labor market concentration changes independently of changes in the firms boundaries due to mergers, stayers' wages are moderately affected. This suggests that employers may be able to take advantage of the reduction in outside options induced by greater concentration to reduce stayers' wages. However, this effect is limited in magnitude presumably because of generalized coverage by collective agreements in France.

The remainder of the paper is structured as follows. Section 3.2 reviews the existing literature. Section 3.3 lays out our empirical strategy. Section 3.4 describes the data that we use and presents summary statistics. Section 3.5 presents the main results, Section 3.6 the secondary ones and Section 3.7 concludes.

3.2 Literature review

3.2.1 Modern Monopsony

The following summary of the monopsony literature draw heavily on the one of Manning (2020). The idea that employer have market power to set wages at another level than marginal product is not a new one. In her seminal book, Robinson (1933), Joan Robinson introduced monopsony as one way to model this. But this is only at the end of the 1990's and the start of the 2000's that the idea really took off, Monopsony came then as a way to explain high levels of inequality and falling labor shares in national income : it was the result of an imbalance in economic power between employers and workers in the labor market. The Boal et Ransom (1997) article in the Journal of Economic Literature led to a rediscovery of the work of Joan Robinson and the book *Monopsony in motions* by Alan Manning (Manning (2003)) modernized the monopsony theory and link it with the most used models in labor economics. The key idea of this first wave of papers can be described very simply : the labor supply curve to an individual employers is not infinitely elastic so that employers can cut wages without losing all of their employees to competitors.

Manning refer to the main strand of the literature as the "modern or dynamic monopsony" strand, in which the main theoretical cause of monopsony power are search frictions in the labor market : it takes time for workers to find a new position if they lose their actual job. In the reference search model of Burdett et Mortensen (1998), the elasticity of the labor supply curve facing the firm can be written as the elasticity of the recruitment function minus the elasticity of the quit function. The elasticity of the quit function measures how much quits react to a wage cut and is the key parameter there. Papers that try to estimate the elasticity of the labor supply curve, like Staiger *et al.* (2010) or Dube *et al.* (2020), find it is in general lower than 1. Regarding quit elasticities, Bassier *et al.* (2020) find separation elasticities between -1 and -3. All of these estimations imply a high level of monopsony in the labor market.

3.2.2 New classical Monopsony

Another strand of the literature, that Manning calls the "New Classical Monopsony" literature focus more on idiosyncratic tastes as the reason why labor supply curve facing individual firms is not perfectly elastic. Amenities (such as working conditions or the length of commute) offered by different firms are not fully priced into wages. Monopsony power comes then from the concentration of labor demand within a local labor market rather than search frictions. The key idea is that there are only a small number of firms offering a particular package of wages and amenities. Card *et al.* (2018) and Jarosch *et al.* (2019) provide two different ways to theoretically show how the market share of a firm is directly linked to the elasticity of the labor supply curve it is facing.

The present paper is part of this strand of the literature, which since Azar *et al.* (2020) has tried to use the Herfindahl-Hirschman Index (HHI), a traditional measure of market concentration commonly used in anti-trust, to empirically link local monopsony measures to individual outcomes (mainly wages). We give more details about this strand of the literature in the following section. Its biggest pitfall is that it must divide labor markets into discrete segments, with the implicit assumption that workers cannot move across them without cost (i.e search frictions).

3.3 Empirical specification

3.3.1 Labor market concentration

As is standard in the literature (Azar *et al.*, 2020; Martins, 2018; Marinescu *et al.*, 2019), we measure employer concentration using the Herfindhal-Hirschman Index (HHI) computed on hirings :

$$HHI_{o,z,t} = \sum_{f=1}^{N_{o,z,t}} s^2_{f,o,z,t} \tag{3.1}$$

where $HHI_{o,z,t}$ is the HHI for occupation o in commuting zone z (which define the local labor market $l = (o, z)$) at year t. $N_{o,z,t}$ is the number of firms that have positive hirings in local labor market l at time t and $s_{f,o,z,t}$ is the share of firm f in hirings in local labor market l at time t. With this definition, $HHI_{o,z,t}$ ranges from 0 (no concentration) to 1 (one firm hiring in the market).

We use an HHI based on hirings to measure labor market concentration since, as emphasized by Marinescu *et al.* (2019), this is the best way to capture job opportunities available to workers searching for a job. To substantiate this claim, consider a search and matching model with granular search where concentration affects wages by affecting workers' outside options - see Jarosch *et al.* (2019). In this model, the index of labor market concentration that is relevant for wage determination may be measured indifferently with an HHI based on hirings or on the employment stock, as long as the environment is stationary. However, when the environment in non-stationary, an HHI based on hirings is much more relevant since downsizing firms may have a positive share of the stock of employment in a local labor market, while their hirings are zero so that they do not contribute to creating outside options for workers in that labor market. Nevertheless, since prior literature has used employment stocks to measure labor market concentration (Abel *et al.*, 2018; Rinz, 2018; Benmelech *et al.*, 2018), and since in a standard Cournot model of oligopsony, wages are inversely related to the HHI measured in terms of employment (Boal et Ransom, 1997), we also use the latter as a robustness check.

3.3.2 Labor market concentration and wages

We estimate the impact of labor market concentration on individual wages. Our baseline specification is as follows :

$$log(w_{i,f,o,z,s,t}) = \beta log(HHI_{o,z,t}) + X_{i,f,o,z,s,t}\gamma + \mu_i + \mu_{f,t} + \mu_{o,z} + \epsilon_{i,f,o,z,t} \tag{3.2}$$

where i indexes the individual, f the firm and s the sector in which the firm is active. w denotes the individual wage, X is a vector of age dummies - one for each year of age - and μ are fixed effects. In this baseline specification, we control for individual and local-labor-market - $l = (o, z)$ - fixed effects. We also control for firm-by-time fixed effects to capture firm productivity. Standard errors are clustered at the commuting zone level.

We estimate equation (3.2) on stayers only, i.e. on individuals who remain employed in the same firm for at least two consecutive years. Focusing on stayers as opposed to movers is interesting *per se* since they represent the vast majority of the labor force (more than 80% in France - see Section 3.4). Moreover, as emphasized by Qiu et Sojourner (2019), labor market concentration and wages may be affected by workforce composition. In turn, this may generate assortative matching between workers and firms. For example, firms with buyer power on the labor market may become more selective and retain only those workers that better match their idiosyncratic needs. Focusing on stayers' wages has another advantage in this respect : it allows controlling for match-specific heterogeneity by augmenting equation (3.2) with a spell fixed effect ($\mu_{i,S,f}$), where S denotes the spell of individual i in firm f - see equation (3.5) below. When doing so, $\hat{\beta}$ is identified only by variations within the same match, defined as a consecutive spell of employment of a worker within a firm.

Arguably, when estimating the impact of labor market concentration on wages, product market competition is a potential confounder. In our baseline specification, we control for firm-by-time fixed effects. If local firms produce for the national or international market - and not only for the local one -, product market competition is firm specific and firm-by-year fixed effects would control for it. However, if firms in a given geographical area produce for the local market, a better way to control for product market competition is to include sector-by-commuting-zone-by-year fixed effects. This is what we do in our extended specifications - see equation (3.5) below.

A key threat to identification in this set-up is that an omitted time-varying variable could be correlated with the HHI and determine wages. This is the case, for example, if a negative shock on the supply of labor takes place in a local labor market $l = (o, z)$. This shock is likely to raise wages. If productivity stays unchanged, unit labor costs go up, thereby likely reducing the number of local firms which find it profitable to employ this type of labor. As a consequence, labor market concentration would increase thus giving rise to a positive correlation between HHI and wages that would, in fact, be due to reverse causality. To deal with this endogeneity problem, the literature suggests instrumenting $log(HHI_{o,z,t})$ with the average of $log(1/N_{o,z',t})$ - where $N_{o,z',t}$ is the number of firms with positive hirings in all other commuting zones z' for the same occupation and time period (Azar *et al.*, 2020; Martins, 2018; Qiu et Sojourner, 2019). $1/N_{o,z',t}$ corresponds to the value of the HHI in local labor market $l' = (o, z')$ when all firms have the same hiring share in that market. This instrument provides a source of variation in labor market concentration relying on national rather than local changes in the occupation we consider. [2]

However, since individuals living close to the border of a commuting zone may be working either in this zone or in the bordering one, any shock on the local labor supply in a given occupation taking place in the periphery of a commuting zone is likely to affect the bordering commuting zones too. To deal with this issue, when building our instrument, we improve on the existing literature by not only removing the commuting zone we consider but also all the zones that have a common border with it. Thus doing, we considerably reduce the risk that spillovers across local labor markets may threaten the orthogonality of our instrument.

Another problem raised by this instrument is that since its variation essentially relies on national changes in occupations (i.e. variations in the o and t dimensions only), it does not allow including

2. Instrumenting a variable in one zone using the average of this variable in other zones (Hausman instruments) is standard in international economics and industrial organization - see e.g. Hausman *et al.* (1994), Autor *et al.* (2013), Bai *et al.* (2017) and Azar *et al.* (2019a)

occupation-by-time fixed effects in the model. Now, if there is a national demand or supply shock in occupation o at time t, the instrument may capture this shock, which will affect both labor market concentration and wages in $l' = (o, z')$ and in $l = (o, z)$. In this case, the instrument is no longer exogenous. To allow controlling for aggregate occupational shocks (by including occupation-by-time fixed effects in our regressions), we develop a related instrument exploiting the industry composition within occupations. Namely, we instrument $log(HHI_{o,z,t})$ with the weighted sum of $log(1/N_{o,z',t})$, where the weights are indexed on the proximity in the industry composition of each occupation across commuting zones z and z'.

More specifically, for a given occupation, we take each commuting zone in which this occupation exists and we consider the employment share of each industry s within that local labor market ($e_{s,o,z,t}$). We then define the proximity of labor markets $l = (o, z)$ and $l' = (o, z')$ as regards industry s as :

$$p_{s,o,z,z',t} = \sqrt{1 - (e_{s,o,z,t} - e_{s,o,z',t})^2} \tag{3.3}$$

The intuition behind this measure of proximity is that whenever two local labor markets have the same industry composition, $e_{s,o,z,t} = e_{s,o,z',t}$, so that proximity p is equal to 1.

In order to compute the proximity in industry composition across labor markets $l = (o, z)$ and $l' = (o, z')$, one could be tempted to average $p_{s,o,z,z',t}$ across all industries. However, thus doing, proximity would be equal to 1 whenever an industry is absent from both labor markets. This would give too large a weight to industries in which both labor markets are not specialized. More generally this would be a problem for all industries whose employment share is small in both labor markets.

To overcome this problem we define an adjusted measure of proximity in industry composition across labor markets $l = (o, z)$ and $l' = (o, z')$:

$$adj\_p_{o,z,z',t} = \frac{1}{n_s}\sqrt{\sum_{s=1}^{n_s}(e_{s,o,z,t} \times p_{s,o,z,z',t})(e_{s,o,z',t} \times p_{s,o,z,z',t})} \tag{3.4}$$

where n_s is the number of industries.[3] By multiplying p by the employment share of industry s in each local labor market, we ensure that when these employment share are small, adjusted proximity is small as well.

Our instrument is then computed as the weighted sum of $log(1/N_{o,z',t})$, where the weights are given by the vector of $adj\_p_{o,z,z',t}$ normalized so that $\sum_{z'} adj\_p_{o,z,z',t} = 1$. It provides a source of variation in labor market concentration based on national industry shocks that affect the number of firms active in specific local labor markets depending on their exposure to that particular industry. To the extent that it varies with the industry composition of local labor markets, this instrument

3. We compute this proximity as of 2009 using 17 aggregate sectors from NAF Rev.2, which is approximately equivalent to the 1-digit letter of the NACE Rev.2 classification.

has a significant variation in the o, z and t dimensions. This allows controlling for both occupation-by-time and occupation-by-sector fixed effects. To give some more details on the type of shocks our instrument is exploiting, we might think of a international competition shock, similar for example to "China shock" in the US. Such a shock will affect differently the concentration of employment in local labor markets depending on their sectoral exposure to this additional competition (Autor *et al.* (2013) are using similar variations). Another example might be a trade war between two countries, some sectors may be more affected than others.

Our most complete specification therefore writes :

$$log(w_{i,f,o,z,s,t}) = \beta log(HHI_{o,z,t}) + X_{i,f,o,z,s,t}\gamma + \mu_{iSf} + \mu_{f,t} + \mu_{o,z} + \mu_{s,z,t} + \mu_{o,t} + \mu_{o,s} + \epsilon_{i,f,o,z,t} \quad (3.5)$$

where μ_{iSf} is the spell fixed effect.

3.4 Data

We use two datasets extracted from the French Social Security records (DADS). The first dataset (DADS-Postes) covers the universe of workers and establishments in all industries except agriculture, part of the food-processing industry, rural financial institutions (e.g. Crédit Agricole) and public administrations. This contains information on establishment location (municipality) and the firm to which the establishment belongs. Moreover it provides information on hours worked, gross wages (constructed as gross annual wages divided by the number of hours worked), workers' age, gender and 4-digit occupation for all employees with non-zero hours worked in a given year. Establishments and firms have a unique identifier which is invariant over time, except when sold out to another company, in which case they are assigned a new identifier. By contrast, for the sake of anonymity, workers' identifiers are changed every year. However, for any given year, we know in which establishments employees were working the year before. We use data starting in 2009 since information on occupations was not systematically reported before that date. We match each municipality contained in the DADS-postes with the 2010 commuting zones using a mapping provided by the French Statistical Institute (INSEE).

For the subset of workers in the DADS-Postes who are born in October of each year, there exists a panel which maintains the same identifier over time for each worker and hence allows following workers across various employers and years. This panel (DADS-Panel) is currently available until 2015. For this reason, we limit our analysis to 2009-2015.

We use the whole DADS-Postes to construct HHIs based on hirings (and, as a robustness check, on employment). We only consider business companies, and exclude workers on training contracts or on occasional jobs.[4] Employment is defined in full-time equivalent terms. A new hire in a given year is defined as a worker who did not work for any establishment of the firm the year before. We only keep local labor markets with at least 10 employees in each year of our time window.

Descriptive statistics of concentration in French local labor markets are reported in Appendix Table A1 and Figures A1 and A2. When measured with reference to hirings, mean concentration weighted by employment is relatively stable over time around 0.11, which is below the threshold for moderate concentration (0.15) defined by the US antitrust authorities. Unsurprisingly, it is even lower when measured on the basis of employment : about 0.09. However, mean values of HHIs turn out to be much larger than median values, suggesting that a number of local labor markets are highly concentrated. As a matter of fact, although 79.2% of workers are employed in a market where the HHI based on hirings is lower than 0.15 (resp. 83.8% for the HHI based on employment), 11.4% (resp. 9.1%) are employed in local labor markets where the HHI based on hirings (resp. employment) is higher than 0.25, which corresponds to high concentration - see Appendix Table A2. Moreover, the (unweighted) proportion of local labor markets with an HHI above 0.25 is non negligible - see Figures A3 and A4 -, at least when labor market concentration is defined on the basis of hirings. In this case, almost 6% of labor markets even have $HHI = 1$ in 2009. This is consistent with the rather low share of individuals facing highly concentrated labor markets since large markets tend to be less concentrated than smaller ones.

As in most countries - see Abel *et al.* (2018), Rinz (2018) and Azar *et al.* (2019b) - local labor markets are more concentrated in mostly rural than in mostly urban commuting zones in France - see Figures A5 and A6. This is accounted for in our regressions by including occupation-by-commuting zone fixed effects.

We estimate our wage regressions on the subset of stayers employed in business companies and for whom we dispose of a panel. We keep workers aged 15 to 74. As is standard when using the DADS and to eliminate implausible values of hourly wages due to misreporting of either annual wages or hours worked, we drop the lowest and highest percentiles of the hourly wage distribution each year. Descriptive statistics for this panel are presented in Appendix Table A3. Our observations are individual-by-firm-by-year triples. Stayers represent 83.5% of this sample. Their average age is 40, and men represent about 51.9% of this group, i.e. about the same proportion as in the whole sample.

3.5 Main results

We first estimate the impact of labor market concentration on individual wages using a measure of the HHI based on hirings. As evidenced in Table 3.1, OLS estimates are negative although

4. The so-called *emplois annexes*.

insignificant at conventional levels, no matter which set of fixed effects we include. These results could suggest that labor market concentration has no impact on stayers' wages, as one could expect in a country with high wage rigidity. However, this could also be due to endogneity if local labor supply shocks simultaneously drive wages and the number of firms in the local labor market - see Section 3.3.2.

In order to disentangle between these explanations, we run IV estimates in which $Log(HHI_{o,z,t})$ is instrumented by the weighted sum of $log(1/N_{o,z',t})$, where $N_{o,z',t}$ is the number of firms with positive hirings in commuting zones z' excluding z and all commuting zones that have a border with z. The weights are proportional to the adjusted proximity in the industry composition within occupation o across commuting zones - see equation (3.4). This instrument is strongly correlated with labor market concentration, as evidenced by the first-stage F-statistics reported at the bottom of Table 3.2. When estimated in this way, the impact of the HHI on individual wages turns out to be negative and significant, whatever the specification we consider - except in col (3). Controlling for individual and firm-by-time fixed effects or for spell fixed effects yield very similar results - see cols (1) and (2). When controlling for product market competition and aggregate occupational shocks, by adding sector-by commuting zone-by-year fixed effects and occupation-by-time fixed effects, our estimates become larger - see col. (4). A similar point estimate is found when adding sector-by-occupation fixed effects - see col (5).

As a robustness check, we estimate the impact of labor market concentration on individual wages using a measure of the HHI based on employment. Point estimates are close in magnitude to those obtained when using an HHI based on hirings - see Table 3.3. Whatever measure of HHI we use, our results suggest that, on average, a 10% increase in labor market concentration decreases stayers' wages by 0.14 to 0.15% - see col (5) of Tables 3.2 and 3.3 -, corresponding to an elasticity of -0.014 (-0.015 respectively). This suggests that labor market concentration has a limited depressing effect on stayers' wages in France.

We also re-estimate our IV model with the standard instrument used in the literature. Namely, we instrument $Log(HHI_{o,z,t})$ with the average of $log(1/N_{o,z',t})$, computed over all commuting zones z' that have no border with z. As mentioned in Section 3.3.2, when doing so, we cannot include occupation-by-time fixed effects since this is essentially the dimension of the instrument. The results are presented in Table 3.4. The point estimates are of the same order of magnitude as those obtained with our instrument, although slightly larger than in the corresponding specifications of Tables 3.2 and 3.3 - cols (1) to (3). Results in column (3) of Table 3.4 can be roughly compared to what Marinescu *et al.* (2019) obtain for new hires in France insofar as they control for firm-level labor productivity and concentration in the product market, along with individual fixed-effects. Their preferred estimate corresponds to an elasticity of -0.09, which suggests that the impact of labor market concentration on stayers' wages is 15 times lower than that estimated on new hires.

This is not surprising given the importance of collective wage setting regulations on the French labor market. However, this suggests that for the vast majority of workers, who do not change employer from one year to the other, labor market concentration is unlikely to have a massive depressing effect on wages.

3.6 Secondary results

3.6.1 Selection into the stayer status

In this subsection, we investigate whether monopsony may reduce labor mobility and induce people to stay in their job. Given that we focus in this paper on stayers, selection into the stayer status may bias our main results. Table 3.5 shows the result of our main specification (equation 3.5) when we replace spell fixed-effects by individual fixed-effects and the dependant variable is now the dummy variable equal to one when a people stay in the same job. The estimation sample are all individuals in the DADS-Panel between 15 to 74 years old. In this case, our estimation measures how a 1% increase in HHI changes the probability to stay in the same firm. When using our main instrument, we find that a 1% increase in HHI decreases the probability to be a stayer by 0.012% though this coefficient is not statistically significant. It seems monopsony only marginally changes the probability to be a stayer and won't then lead to a significant selection in our main estimation sample.

3.6.2 Heterogeneity

Monopsony power may interact with individual characteristics and is not likely to be homogeneous. Workers that face higher search frictions will be more impacted by a change in the concentration of labor demand in a particular labor market because their ability to "escape it" is lower. Given that spatial mobility costs are nominally similar across occupations, they will be higher relatively to income in occupations that typically offer a lower wage, for example blue-collar occupations. Schmutz *et al.* (2020) shows that depending on employment status, spatial frictions are between 1.5 and 3.5 times higher for blue-collar workers. Similarly, women tend to face higher search frictions on the labor market and monopsony may also "bite" them more. Webber (2016) finds that on average gender-specific search frictions are associated with 3.3 percent lower earnings for women relative to men. To investigate these two dimensions, we show in Table 3.6 the result of our main specifications separately for executives and non-executives and for women and men. We find that coefficients are negative and of the same magnitude as in our main samples for women and for non-executives, while close to 0 for executives and men. This might be because non-executives and women face higher search frictions.

3.7 Conclusion

Using French administrative data, we have investigated the impact of labor market concentration on stayers' wages. By focusing on stayers and using a new instrument that exploits industry composition within occupations, we are able to control, not only for labor productivity and product market competition, as standard in the literature, but also for match-specific heterogeneity as well as any occupational shock at the aggregate level. Our findings show that when labor market concentration increases by 10%, stayers' wages decrease by 0.14%. Due to the automatic extension of collective agreements, wage rigidities are strong in France. This probably explains why the effect we find on stayers' wages is small, although negative. As such, it can be considered as a lower bound in an international perspective.

Our results complement Marinescu *et al.* (2019) who find that labor market concentration reduces the wages of new hires in France with an elasticity of -0.09. The effect that we find on stayers is about 15 times smaller than on new hires. Our findings also complement those of Arnold (2019) who finds that mergers that increase labor market concentration reduce stayers' wages in merged companies. We find that concentration affects stayers' wages in non-merged companies too, although to a limited extent.

To the extent that stayers represent the vast majority of the labor force - more than 80% in France -, our results suggest that in countries where coverage by collective agreements is the norm, labor market concentration is unlikely to put strong downward pressure on wages. This may be one of the reasons why the labor share remained quite stable in France since the end of the 1980s, while it substantially decreased, e.g. in the USA (Cette *et al.*, 2019). Replicating this analysis on US data - or data from other countries - would allow investigating the role of the concentration-wage channel as a determinant of changes in the labor share. This is a promising avenue for further research.

Bibliographie

Will Abel, Silvana Tenreyro et Gregory Thwaites : Monopsony in the UK. *CEPR Discussion Paper*, No. DP13265, 2018.

David Arnold : Merger and acquisitions, labor market concentration and worker outcomes. *Mimeo*, Princeton University:https ://scholar.princeton.edu/sites/default/files/dharnold/files/jmp.pdf, 2019.

David H Autor, David Dorn et Gordon H Hanson : The China syndrome : Local labor market effects of import competition in the United States. *American Economic Review*, 103(6):2121–68, 2013.

José Azar, Steven Berry et Ioana Elena Marinescu : Estimating labor market power. *Available at SSRN 3456277*, 2019a.

José Azar, Ioana Marinescu et Marshall Steinbaum : Measuring labor market power two ways. *AEA Papers and Proceedings*, 109(5):317–21, 2019b.

José Azar, Ioana Marinescu et Marshall Steinbaum : Labor market concentration. *Journal of Human Resources*, pages 1218–9914R1, 2020.

José A Azar, Ioana Marinescu, Marshall I Steinbaum et Bledi Taska : Concentration in US labor markets : Evidence from online vacancy data. *NBER Working Paper*, No. 24395, 2018.

Jan Babeckỳ, Philip Du Caju, Theodora Kosma, Martina Lawless, Julián Messina et Tairi Rõõm : Downward nominal and real wage rigidity : Survey evidence from European firms. *The Scandinavian Journal of Economics*, 4(112):884–910, 2010.

Jie Bai, Seema Jayachandran, Edmund J Malesky et Benjamin A Olken : Firm growth and corruption : Empirical evidence from Vietnam. *The Economic Journal*, 129(618):651–677, 2017.

Ihsaan Bassier, Arindrajit Dube et Suresh Naidu : Monopsony in movers : The elasticity of labor supply to firm wage policies. Rapport technique, National Bureau of Economic Research, 2020.

Efraim Benmelech, Nittai Bergman et Hyunseob Kim : Strong employers and weak employees : How does employer concentration affect wages ? *NBER Working Paper*, No. 24307, 2018.

William M Boal et Michael R Ransom : Monopsony in the labor market. *Journal of economic literature*, 35(1):86–112, 1997.

Kenneth Burdett et Dale T Mortensen : Wage differentials, employer size, and unemployment. *International Economic Review*, pages 257–273, 1998.

David Card, Ana Rute Cardoso, Joerg Heining et Patrick Kline : Firms and labor market inequality : Evidence and some theory. *Journal of Labor Economics*, 36(S1):S13–S70, 2018.

Gilbert Cette, Lorraine Koehl et Thomas Philippon : Labor shares in some advanced economies. Rapport technique, National Bureau of Economic Research, 2019.

Arindrajit Dube, Jeff Jacobs, Suresh Naidu et Siddharth Suri : Monopsony in online labor markets. *American Economic Review : Insights*, 2(1):33–46, 2020.

Joyce K Hahn, Henry R Hyatt et Hubert P Janicki : Job ladders and growth in earnings, hours, and wages. *Society for Economic Dynamics Meeting Papers*, No. 908, 2018.

Joyce K Hahn, Henry R Hyatt, Hubert P Janicki et Stephen R Tibbets : Job-to-job flows and earnings growth. *AEA Papers and Proceedings*, 107(5):358–63, 2017.

Jerry Hausman, Gregory Leonard et J Douglas Zona : Competitive analysis with differenciated products. *Annales d'Economie et de Statistique*, pages 159–180, 1994.

Gregor Jarosch, Jan Sebastian Nimczik et Isaac Sorkin : Granular search, market structure, and wages. *NBER Working Paper*, No. 26239, 2019.

Claudia Macaluso, Brad Hershbein et Chen Yeh : Concentration in US local labor markets : Evidence from vacancy and employment data. *Society for Economic Dynamics Meeting Papers*, No. 1336, 2019.

Alan Manning : *Monopsony in motion : Imperfect competition in labor markets*. Princeton University Press, 2003.

Alan Manning : Monopsony in labor markets : a review. *ILR Review*, page 0019793920922499, 2020.

Ioana Elena Marinescu, Ivan Ouss et Louis-Daniel Pape : Wages, hires, and labor market concentration. *SSRN*, No. 3453855, 2019.

PMRDS Martins : Making their own weather ? Estimating employer labour-market power and its wage effects. *QMUL Working Papers*, No. 95, 2018.

OECD : *Employment Outlook*. Organisation for Economic Co-operation and Development, 2010.

Yue QIU et Aaron SOJOURNER : Labor-market concentration and labor compensation. *SSRN*, No. 3312197, 2019.

Kevin RINZ : Labor market concentration, earnings inequality, and earnings mobility. *Center for Administrative Records Research and Applications Working Paper*, 10, 2018.

Joan ROBINSON : *The economics of imperfect competition.* Springer, 1933.

Benoıt SCHMUTZ, Modibo SIDIBE et Elie VIDAL-NAQUET : Why are low-skilled workers less mobile ? the role of mobility costs and spatial frictions. *Série des Documents de Travail CREST*, No 2020-15, 2020.

Douglas O STAIGER, Joanne SPETZ et Ciaran S PHIBBS : Is there monopsony in the labor market ? evidence from a natural experiment. *Journal of Labor Economics*, 28(2):211–236, 2010.

Douglas A WEBBER : Firm-level monopsony and the gender pay gap. *Industrial Relations : A Journal of Economy and Society*, 55(2):323–345, 2016.

Tables

TABLE 3.1 – HHI based on Hirings - OLS

	(1) Log(Wage)	(2) Log(Wage)	(3) Log(Wage)	(4) Log(Wage)	(5) Log(Wage)
$Log(HHI_{o,z,t})$	-.00004	-.00004	-.00000	-.00012	-.00008
	(.00025)	(.00024)	(.00023)	(.00019)	(.00019)
Individual FE	Yes	No	No	No	No
Spell FE	No	Yes	Yes	Yes	Yes
Firm×Year FE	Yes	Yes	Yes	Yes	Yes
Commuting zone×Occupation FE	Yes	Yes	Yes	Yes	Yes
Sector×Commuting zone×Year FE	No	No	Yes	Yes	Yes
Occupation×Year FE	No	No	No	Yes	Yes
Occupation×Sector FE	No	No	No	No	Yes
Age Dummies	Yes	Yes	Yes	Yes	Yes
Observations	10,965,520	10,965,520	10,965,520	10,965,520	10,965,520
R^2	0.928	0.941	0.942	0.943	0.943

Note : *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the commuting zone level. Age dummies include one dummy variable for each year of age of the individual.

TABLE 3.2 – HHI based on Hirings - IV

	(1) Log(Wage)	(2) Log(Wage)	(3) Log(Wage)	(4) Log(Wage)	(5) Log(Wage)
$Log(HHI_{o,z,t})$	-.00490**	-.00509*	-.00299	-.01421**	-.01369**
	(.00246)	(.00256)	(.00236)	(.00683)	(.00696)
Individual FE	Yes	No	No	No	No
Spell FE	No	Yes	Yes	Yes	Yes
Firm×Year FE	Yes	Yes	Yes	Yes	Yes
Commuting zone×Occupation FE	Yes	Yes	Yes	Yes	Yes
Sector×Commuting zone×Year FE	No	No	Yes	Yes	Yes
Occupation×Year FE	No	No	No	Yes	Yes
Occupation×Sector FE	No	No	No	No	Yes
Age Dummies	Yes	Yes	Yes	Yes	Yes
F-test on instrument	144.1	135.38	141.27	130.80	131.38
Observations	10,965,520	10,965,520	10,965,520	10,965,520	10,965,520

Note : *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the commuting zone level. Age dummies include one dummy variable for each year of age of the individual.

Table 3.3 – HHI based on Employment - IV

	(1) Log(Wage)	(2) Log(Wage)	(3) Log(Wage)	(4) Log(Wage)	(5) Log(Wage)
$Log(HHI_{o,z,t})$	-.00475**	-.00497*	-.00282	-.01519**	-.01479*
	(.00242)	(.00252)	(.00230)	(.00759)	(.00775)
Individual FE	Yes	No	No	No	No
Spell FE	No	Yes	Yes	Yes	Yes
Firm×Year FE	Yes	Yes	Yes	Yes	Yes
Commuting zone×Occupation FE	Yes	Yes	Yes	Yes	Yes
Sector×Commuting zone×Year FE	No	No	Yes	Yes	Yes
Occupation×Year FE	No	No	No	Yes	Yes
Occupation×Sector FE	No	No	No	No	Yes
Age Dummies	Yes	Yes	Yes	Yes	Yes
F-test on instrument	281.6	265.5	298.5	56.19	55.27
Observations	10,965,520	10,965,520	10,965,520	10,965,520	10,965,520

Note : *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the commuting zone level. Age dummies include one dummy variable for each year of age of the individual.

Table 3.4 – HHI based on Employment and Hirings - Alternative IV

	(1) Log(Wage)	(2) Log(Wage)	(3) Log(Wage)	(1) Log(Wage)	(2) Log(Wage)	(3) Log(Wage)
HHI based on	Hirings	Hirings	Hirings	Employment	Employment	Employment
$Log(HHI_{o,z,t})$	-.00631***	-.00663***	-.00474**	-.00655**	-.00691**	-.00483*
	(.00245)	(.00257)	(.00240)	(.00257)	(.00270)	(.00247)
Individual FE	Yes	No	No	Yes	No	No
Spell FE	No	Yes	Yes	No	Yes	Yes
Firm×Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Commuting zone×Occupation FE	Yes	Yes	Yes	Yes	Yes	Yes
Sector×Commuting zone×Year FE	No	No	Yes	No	No	Yes
Age Dummies	Yes	Yes	Yes	Yes	Yes	Yes
F-test on instrument	168.1	157.7	163.5	283.4	269.9	299.0
Observations	10,965,520	10,965,520	10,965,520	10,965,520	10,965,520	10,965,520

Note : *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the commuting zone level. Age dummies include one dummy variable for each year of age of the individual.

Table 3.5 – HHI based on Hirings - Alternative Dependant variable

	(1) Stayed	(2) Stayed
	OLS	IV
$Log(HHI_{o,z,t})$	-.00759***	-.01192
	(.00052)	(.01790)
Individual FE	Yes	Yes
Firm×Year FE	Yes	Yes
Commuting zone×Occupation FE	Yes	Yes
Sector×Commuting zone×Year FE	Yes	Yes
Age Dummies	Yes	Yes
F-test on instrument		134.1
Observations	13,123,026	13,123,026

Note : *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the commuting zone level. Age dummies include one dummy variable for each year of age of the individual.

Table 3.6 – HHI based on Hirings - Heterogeneity

	(1) Log(Wage)	(2) Log(Wage)	(3) Log(Wage)	(4) Log(Wage)
Category	Executives	Non-executives	Men	Women
$Log(HHI_{o,z,t})$	-.00083	-.01115	0.00318	-.01252
	(.01225)	(.00753)	(.00956)	(.00875)
Spell FE	Yes	Yes	Yes	Yes
Firm×Year FE	Yes	Yes	Yes	Yes
Commuting zone×Occupation FE	Yes	Yes	Yes	Yes
Sector×Commuting zone×Year FE	Yes	Yes	Yes	Yes
Age Dummies	Yes	Yes	Yes	Yes
F-test on instrument	45.6	127.7	38.2	76.5
Observations	1,184,869	9,780,651	5,166,154	5,596,614

Note : *** p<0.01, ** p<0.05, * p<0.1. Standard errors are clustered at the commuting zone level. Age dummies include one dummy variable for each year of age of the individual.

Annexe 3.A Not for publication *or* for online publication only

Table A1 – Descriptive statistics - Local labor markets

(1) Year	(2) Mean	(3) SD	(4) Median	(5) Obs.	(6) % HHI=1
HHI based on Hirings					
2009	0.1091	0.1635	0.0474	55,818	5.7
2010	0.1082	0.1611	0.048	55,818	4.5
2011	0.1049	0.1541	0.0480	55,818	4.2
2012	0.1093	0.1594	0.0493	55,818	4.8
2013	0.1071	0.1578	0.0487	55,818	4.9
2014	0.1073	0.1567	0.0487	55,818	4.8
2015	0.1064	0.1559	0.0488	55,818	4.9
HHI based on Employment					
2009	0.0844	0.1415	0.0317	55,818	0.4
2010	0.0898	0.1486	0.0334	55,818	0.5
2011	0.0856	0.1407	0.0333	55,818	0.4
2012	0.0888	0.1456	0.0343	55,818	0.5
2013	0.0890	0.1466	0.0350	55,818	0.5
2014	0.0889	0.1453	0.0350	55,818	0.5
2015	0.0887	0.1452	0.0347	55,818	0.5

Note : The mean, standard deviation and median value of HHIs reported in cols (2) to (4) are weighted by employment in each local labor market.

Table A2 – Monopsony in Local Labor Markets in France

	Unconcentrated (HHI<0.15)	Mildly-concentrated (0.15<HHI<0.25)	Highly-concentrated (HHI>0.25)
HHI based on Hirings	79.2	9.4	11.4
HHI based on Employment	83.8	7.1	9.1

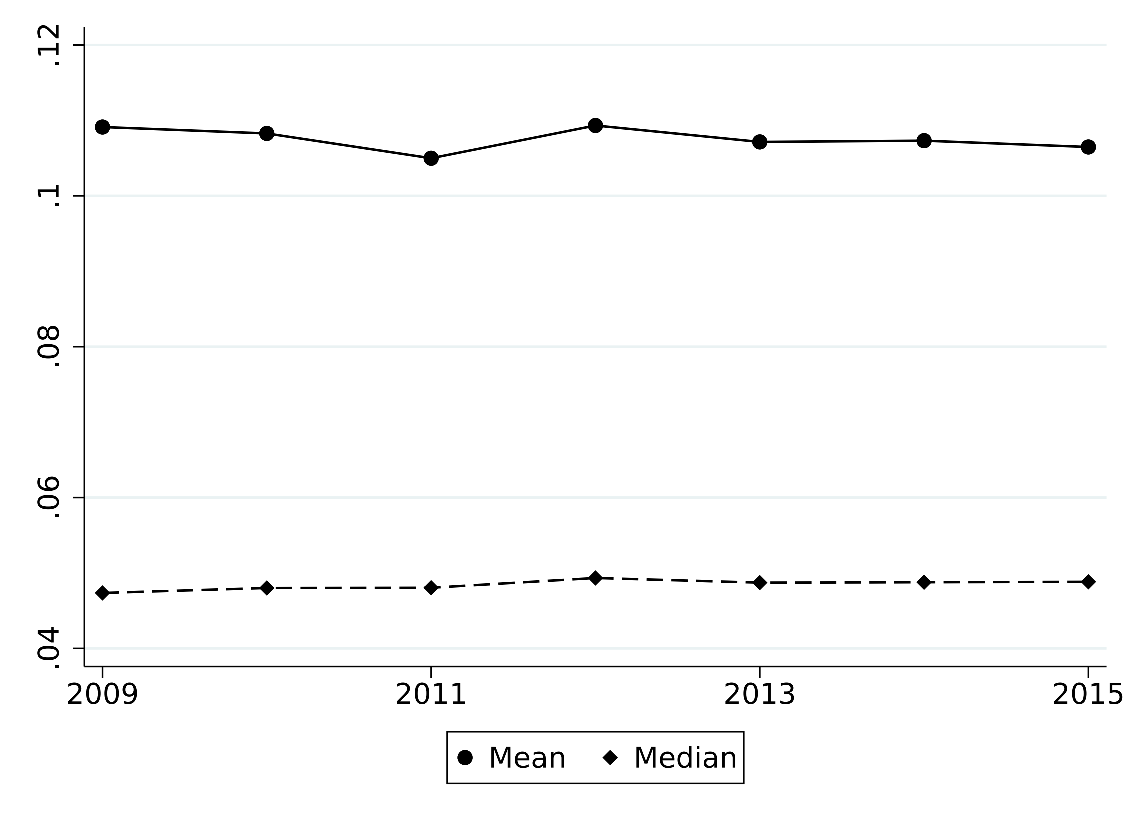
Note : Proportion of local labor markets (weighted by their employment) in the DADS-Postes 2009-2015 according to their level of concentration. Our categorization is similar to the one used by the US Department of Justice.

Table A3 – Individual characteristics - Panel DADS, 2009-2015

Mean	Wage	Men	Age	% Stayers	Observations
All	12.13	51.7	39	83.5	13,123,026
Stayers	12.51	51.9	40	-	10,965,520

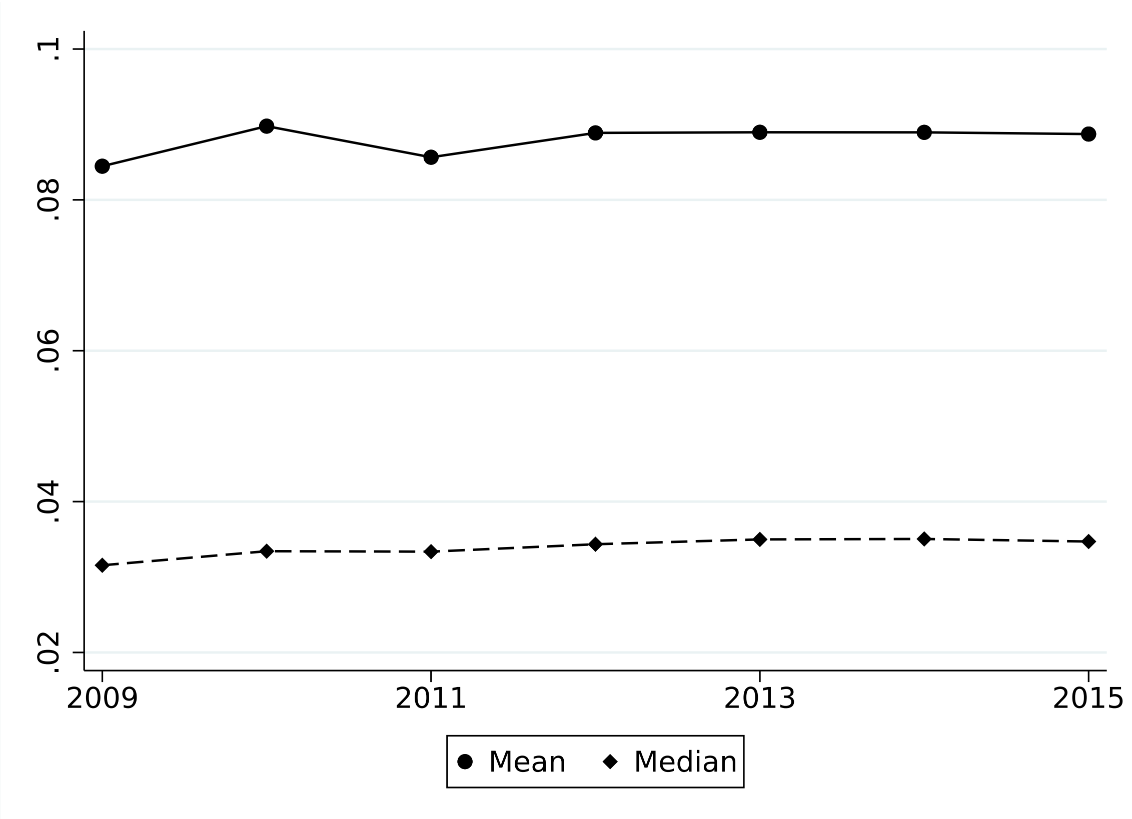
Note : Each observation is an individual-by-firm-by-year triple. A stayer is a triple observed for at least two consecutive years in a given firm.

Figure A1 – Change in labor market concentration 2009-2015 : Hirings



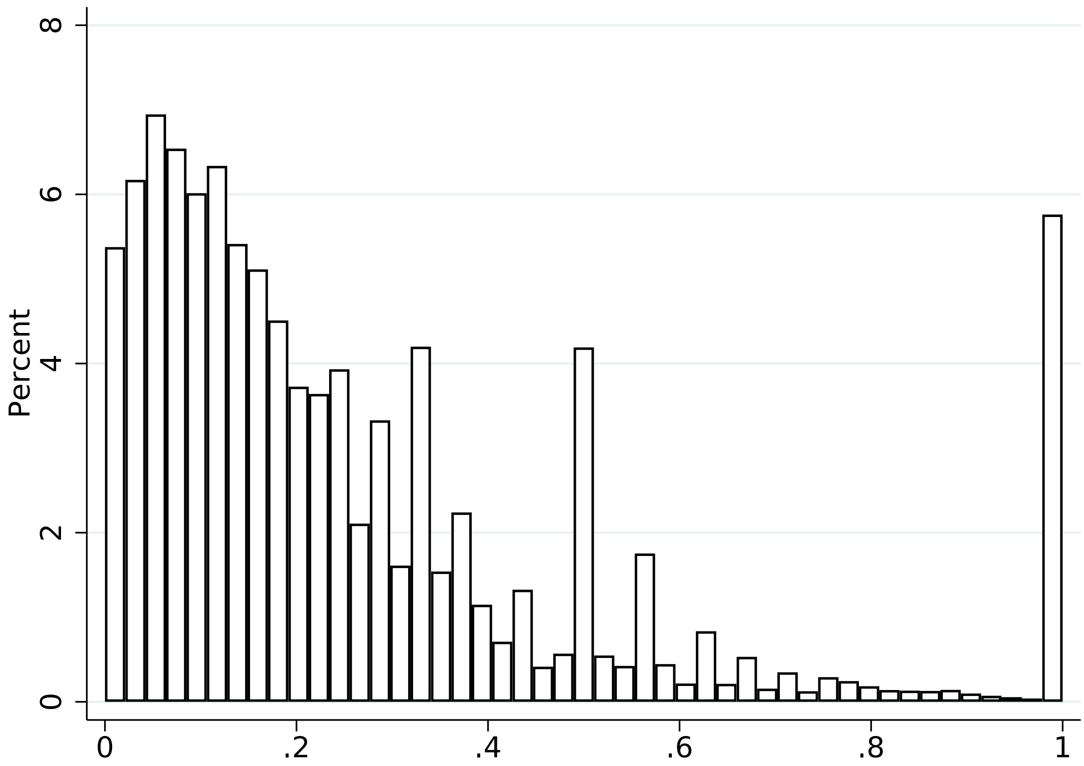
Note : Average HHI (weighted by employment) by year.

Figure A2 – Change in labor market concentration 2009-2015 : Employment



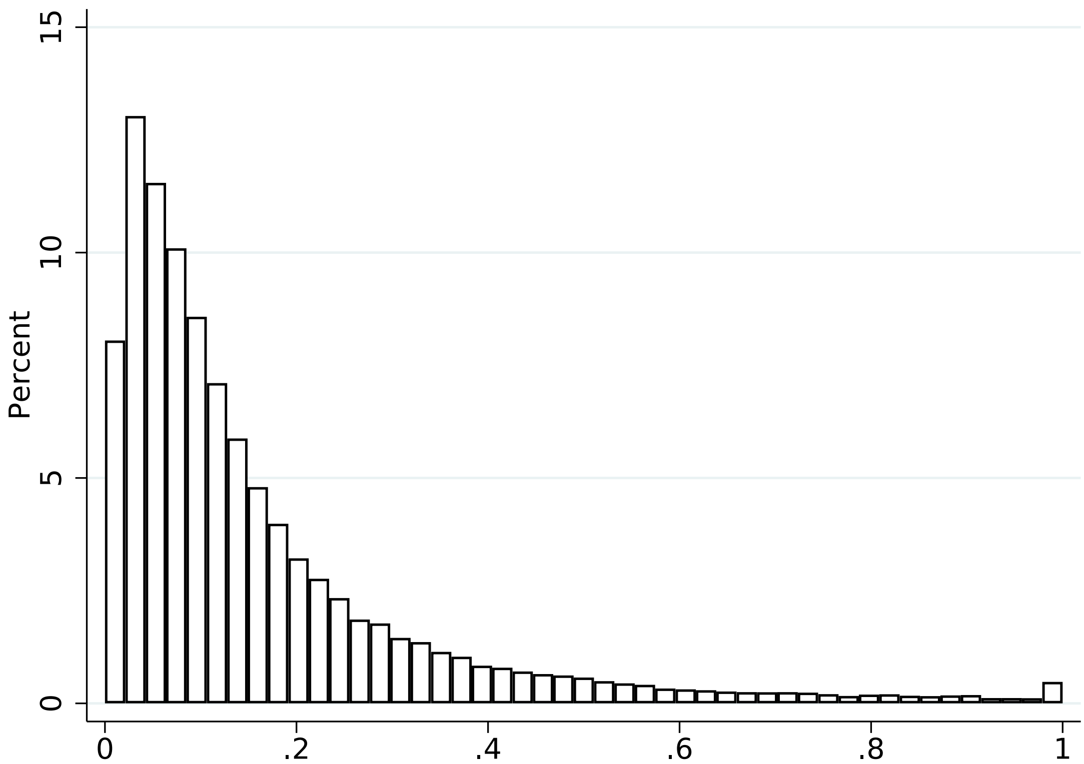
Note : Average HHI (weighted by employment) by year.

Figure A3 – Distribution of labor market concentration in 2009 : Hirings



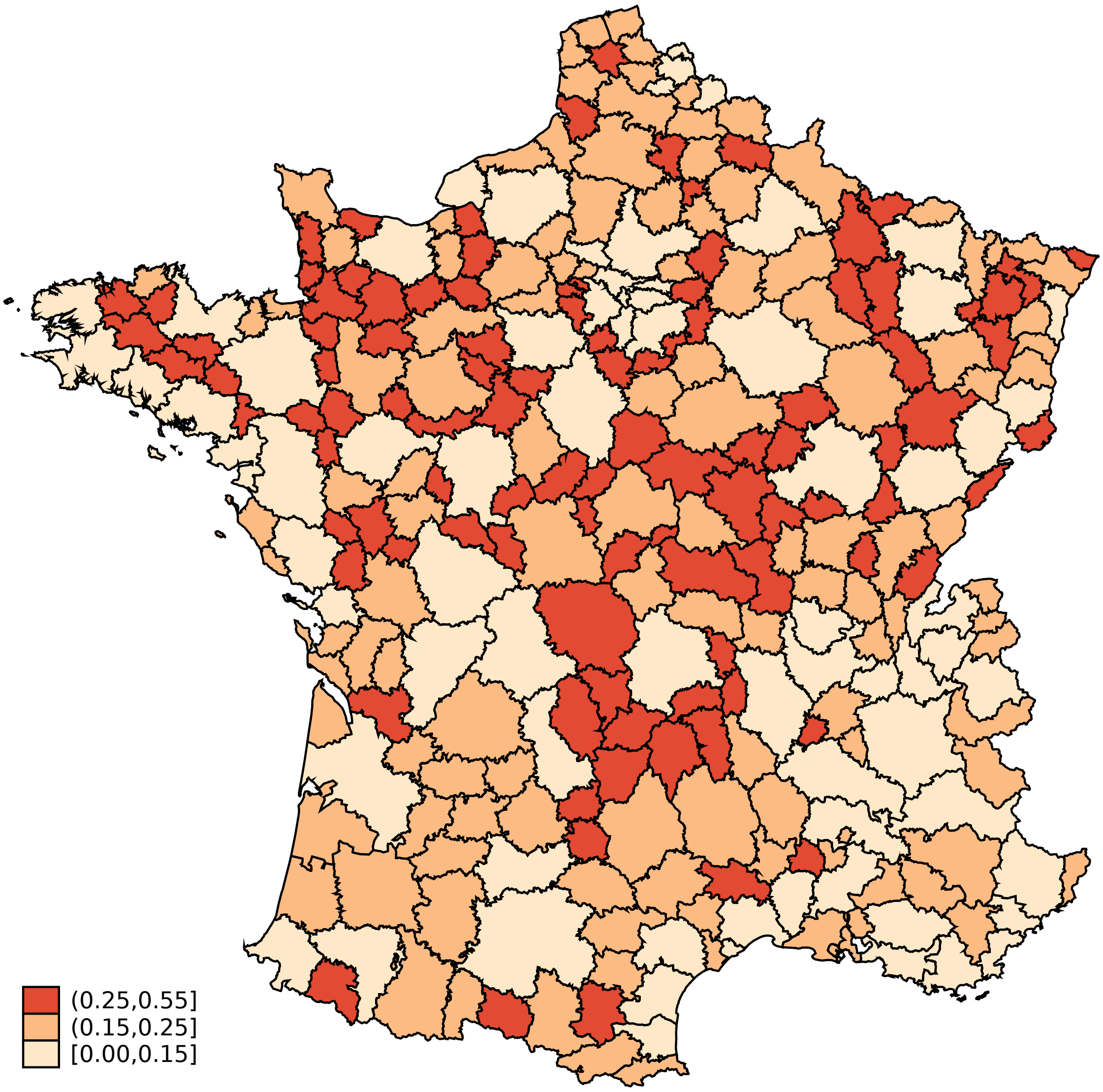
Note : Each observation is a local labor market in 2009. The figure plots the unweighted distribution of hiring-based HHI across these labor markets.

Figure A4 – Distribution of labor market concentration in 2009 : Employment



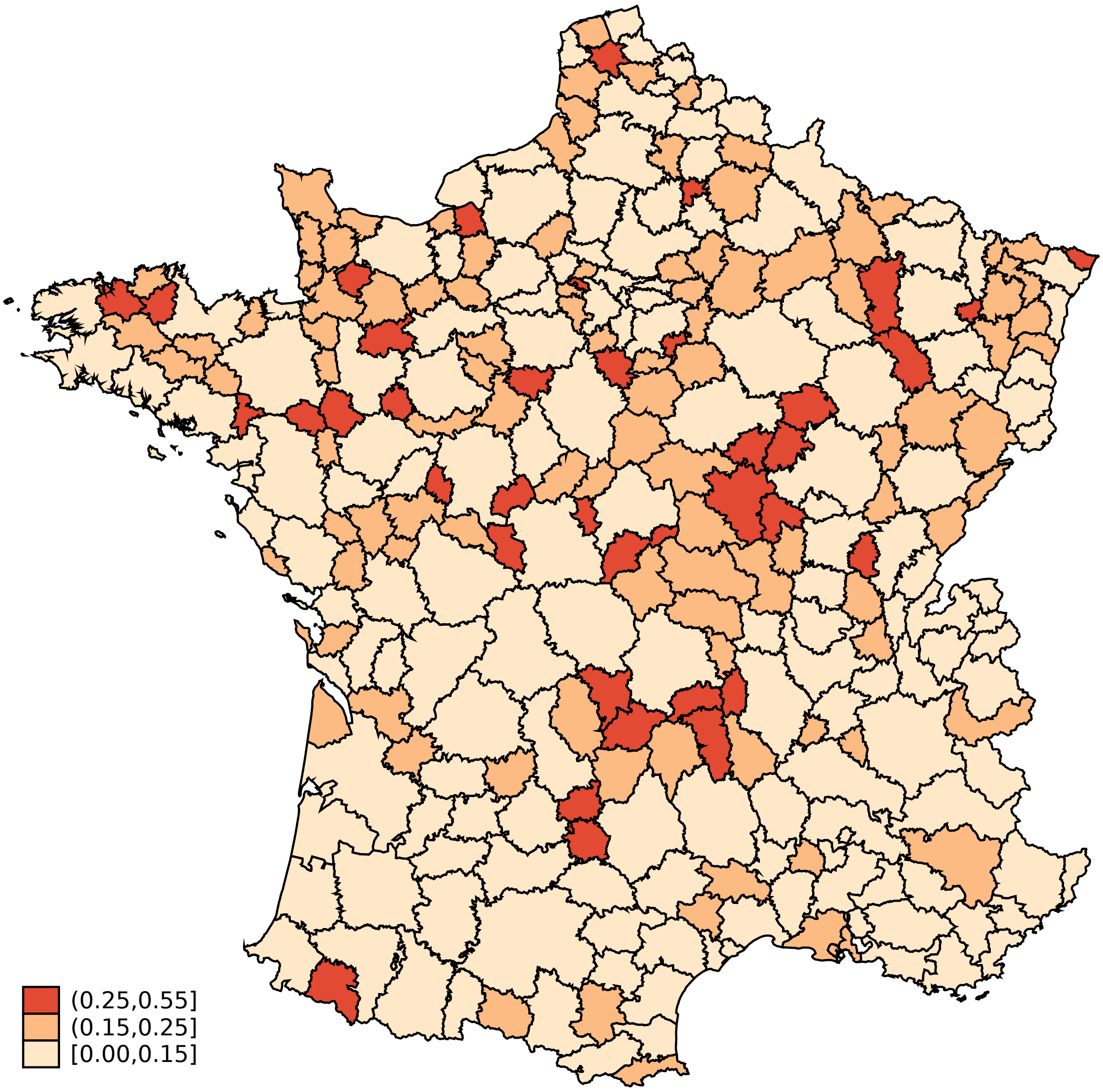
Note : Each observation is a local labor market in 2009. The figure plots the unweighted distribution of employment-based HHI across these labor markets.

Figure A5 – Labor market concentration in French commuting zones in 2009 : Hirings



Note : Average HHI (weighted by employment) by commuting zone.

Figure A6 – Labor market concentration in French commuting zones in 2009 : Employment



Note : Average HHI (weighted by employment) by commuting zone.

CHAPITRE 4

Rival Guests or Defiant Hosts ? The Local Economic Impact of Hosting Refugees

Cyprien Batut, Sarah Schneider-Strawczynki [1]

1. Contact : cyprien.batut[at]gmail.com.
We would like to thank Eve Caroli, Julien Grenet, Eric Maurin, Thomas Piketty, Hillel Rapoport, Alesandro Tondini, Liam Wren-Lewis, and seminars participants for their advice and comments on earlier versions of this work.

Abstract :

This paper investigates the local economic cost of hosting refugees. Using administrative data in France, we show that the opening of small housing centers for refugees decreases economic activity in hosting municipalities. We demonstrate that this downturn is related to a decline in the population by around two percent due to fewer people moving into hosting municipalities. This avoidance behavior of natives results from prejudices since refugee inflows are not sufficient to generate labor market shocks. We also estimate the aggregate cost of hosting refugees.

JEL : F22, R23, J61

Keywords : Migration, Segregation, Refugees, Amenity Shock

4.1 Introduction

More than 1,300,000 asylum applications were submitted in Europe in 2015, while less than 200,000 applications were received 30 years earlier. Since the collapse of the Communist bloc in the late 1980s, the rise of refugee inflows to Europe has challenged the existing policies and accommodation schemes. In this context, two phenomena could trigger popular resentment. First, natives may fear the potential economic weight and the cultural threat of a growing number of humanitarian migrants. Second, the spatial concentration of refugees could raise fairness issues for communities who are more exposed. To avoid public discontent, most European governments have tried to "spread" the burden of refugee hospitality (Robinson *et al.*, 2003). One rationale behind this approach is that anti-refugee feelings, native defiance, stem from the competition between refugees and natives over the sharing of scarce resources. We call this competition refugee rivalry. By making the presence of refugees negligible at the local level, refugee dispersal policies reduce the threat they could pose to natives and render hospitality more acceptable. But such policies can also come at a cost. First, they can isolate refugees from more dynamic labor markets and their ethnic networks, potentially adversely affecting their chances of integration (Edin *et al.*, 2004; Clemens *et al.*, 2018; Fasani *et al.*, 2018; Martén *et al.*, 2019). Second, if anti-refugee sentiments are driven not only by fears of competition but also by cultural insecurity or xenophobia, they might not dispel local discontent.

In this paper, we answer the following question : what is the local economic impact of refugee accommodation policies ? We focus on how refugee dispersal might dilute the impact of humanitarian migrants – asylum seekers, refugees, and beneficiaries of the subsidiary protection – inflows. On the one hand, dispersal may be enough if the impact is driven by refugee rivalry. For instance, if refugees compete with natives over scarce resources, such as jobs, housings, public goods, or even public order. On the other hand, it may not be sufficient if the economic impact is due to native defiance because of cultural insecurity or xenophobia. In particular, we could expect self-segregation as a reaction to refugee inflows, either because of native flight, with natives leaving the area when refugees arrive, or because of native avoidance, with natives avoiding to move to places with refugees. Compared to studies of large refugee shocks (Card, 1990; Hunt, 1992; Balkan *et al.*, 2018), considering refugee dispersal policies is interesting because they are more representative of typical patterns of refugee-hosting (Peri, 2016). Moreover, it allows us to discriminate between native defiance and refugee rivalry.

We exploit the opening of refugee centers in ninety-eight municipalities in France between 2004 and 2012 to identify the effect of small humanitarian migrant inflows on local communities. We compare the evolution of the population, and other outcomes, in hosting and non-hosting neighboring municipalities up to two years before and after the opening of a refugee center. We focus on the evolution of the local population because it is a relevant indicator of local shocks and shifts in preferences (Tiebout, 1956; Hsieh et Moretti, 2019). In France, the refugee center opening process is centralized, leaving municipalities with very little discretion over the opening. Asylum seekers, subsidiary protection recipients, and refugees do not choose centers they are assigned to. The opening of a refugee center is thus a good experiment to study what happens when a locality is exposed to refugees. To circumvent the lack of precise and regular population census data, we use two population proxies, which are the number of employed residents and the number of fiscal households. The first measure enables us to provide a monthly estimate of population change and the second one allows us to confirm the validity of our results for the non-employed.

We find that, on average, the number of employed residents and fiscal households in hosting municipalities decreases by up to two percent two years after the opening of the refugee center. This effect persists when we account for potential spillovers. This finding also holds when looking at the number of retiree households. We also show that wealthier households react more to the openings. Since the median size of a refugee center relative to the local workforce is 0.3%, this implies that there are three to six fewer people in the municipality for one refugee place in the center. This decrease in population is explained by a reduction in native inflows to hosting municipalities. Finally, for a subsample of openings, we find that the population stops decreasing after three years.

Negative valuation by prospective newcomers of the presence of a refugee center, that is native avoidance, drives the local costs of hosting refugees. Over time, lower inflows of natives to hosting municipalities mean fewer customers and taxpayers. We find that tax collected by hosting municipalities falls by around three percent after opening a refugee center. The economic activity of firms located in the hosting municipalities is also decreasing : their number is declining by 3 percent and the value of their sales by more than 5 percent.

We use a spatial general equilibrium model to estimate the aggregate cost of the population redistribution generated by self-segregation. The intuition, common with Hsieh et Moretti (2019), is that the population change at the local level is a sufficient statistic for the welfare effects of local shocks at the aggregate level. We find that opening a refugee center reduces aggregate welfare growth by 10^{-4} percent on average.

This paper first contributes to the literature on the impact of refugee inflows on residential preferences. Card *et al.* (2008) and Aldén *et al.* (2015) show that natives either leave or settle less in neighborhoods where the proportion of non-European immigrants hits a tipping point. But we show that this segregation occurs with native avoidance even for smaller refugee inflows. In terms of magnitude, our results are close to the one of Boustan (2010). She finds that, after World War 2, every black arrival in northern US cities led to 2.7 white departures. To our knowledge, we are the first to demonstrate a comparable displacement in response to refugees in the current period. According to Balkan *et al.* (2018), refugee inflows in Turkey have increased demand for housing in native-dominant neighborhoods. In the same vein, Van Vuuren *et al.* (2019) and Hennig (2018) show a decrease in housing prices following the opening of temporary accommodation for refugees in the vicinity. Our results are consistent with their findings but we can differentiate between native flight and native avoidance, and we study a less urban context in which housing prices may not react quickly enough to compensate defiant potential newcomers.

Our paper also contributes to the literature on the impact of immigration on the local population's preferences. Results are mixed when it comes to the effect of migration on political outcomes as the vote for the extreme right seems to increase with a rise in the migrant population (Halla *et al.*, 2017; Otto et Steinhardt, 2014; Dustmann *et al.*, 2018; Edo *et al.*, 2019), but decreases as a result of a refugees-inflows shock (Vertier et Viskanic, 2019; Steinmayr, 2020) because of the contact theory (Allport *et al.*, 1954). We reconcile these two strands of the literature by showing that natives who are in contact with humanitarian migrants do not leave refugee-hosting municipalities, while it does deter those who are less in contact from moving in the municipality. Furthermore, while natives' preferences towards immigrants have been studied, showing that they may relate to fears about the economic (Mayda 2006; Blanchflower et Shadforth 2009), social (Bansak *et al.* 2016b; Card *et al.* 2012a), and cultural (Facchini *et al.* 2013; Hainmueller et Hopkins 2014) impact of immigration, we add to this literature by studying the local economic consequences of these preferences per se.

Our paper also indirectly contributes to the literature studying the competition between refugees and natives, what we call refugee rivalry, by demonstrating that part of the economic impact of the refugee arrival is driven by natives themselves. In the literature, evidences of an adverse effect of migration inflows on the labor market (Borjas, 2017; Borjas et Monras, 2017; Card, 1990; Peri et Yasenov, 2018; Mitaritonna *et al.*, 2017; Edo, 2017; Hunt, 1992) and on local amenities (Geay *et al.*, 2013; Ballatore *et al.*, 2018) are mixed. Dustmann *et al.* (2016) discuss several reasons why this might be the case. One of them is that many studies assume that the labor supply of natives is fixed in the short term. In our paper, we show that this may not be the case. Indeed, as the median capacity of these centers relative to the local population is 0.3%, the refugee rivalry channel is unlikely. Because we cannot differentiate between refugees, subsidiary protected and asylum seekers in the centers, and as they could stay in the same municipality once they exit the center, we still check in the Appendix 4.A.1 that the effect we identify is not due to a labor market shock. We found no consistent links between the size of the potential labor supply shock and the evolution of the workforce.

This article is generally relevant to the literature interested in the dynamics of population at the local level and the reasons for the uneven development of places. We show that amenity shocks are a potentially significant source of divergence between municipalities. Our contribution is that we develop a methodology to study the welfare-cost of local amenity shocks, such as the opening of a refugee center, and the redistribution of population it generates. The general spatial equilibrium model introduced by Rosen (1979) and Roback (1982) relates changes in population in municipalities to changes in resident utility. Hsieh et Moretti (2019) use this framework to show that the evolution of the local population over a defined period is a sufficient statistic for the effect of all local shocks on aggregate utility. In our proposed methodology, we apply this framework, using the identified change of population due to a refugee center opening, to measure the magnitude of its aggregate welfare cost.

To finish with, our results illustrate the need for a careful analysis of the cost of refugee dispersal policies to local economies. Refugee dispersal policies aim to address costs related to refugee rivalry and native defiance by spreading the burden of refugee hospitality across the territory in small centers. Small inflows of refugees at the local level are unlikely to be a credible threat to natives. Still, natives avoid municipalities where a refugee center has opened. Smaller refugee inflow are not sufficient to prevent an adverse impact of hosting refugees because of native defiance. Part of this cost may be due to centralized nature of the opening process in France before 2015, Including mayors in the opening process of refugee housing centers may lead to better place them. Placing refugee centers where housing (and labor) markets are more dynamic may reduce native avoidance thanks to lower housing prices (and improve refugee's integration chances). Also, it shows that addressing the negative perception of refugees, as emphasized in Card *et al.* (2012b) and in Bansak *et al.* (2016a), could be economically profitable and a key objective for policy-makers.

The paper is structured as follows. Section 4.2 presents our context and data. Section 4.3 details our identification strategy. Section 4.4 investigates the effect of refugee center openings on local economies. Section 4.5 aims to estimate the aggregate welfare cost of refugee hosting. We conclude with Section 4.6.

4.2 Context and Data

4.2.1 Humanitarian migrants and housing centers in France

This paper considers housing centers for humanitarian migrants, namely refugees, subsidiary protection recipients, and asylum seekers. Asylum seekers are people who have applied for refugee status and are awaiting the *Office Francais de Protection des Refugiés et Apatrides*'s (OFPRA, French Office for the Protection of Refugees and Stateless Persons) decision to determine their right to stay or not. Importantly, asylum seekers have no right to work since 1991. Temporary work permits can be issued after nine months, but this rarely happens.

Asylum seekers who acquire the refugee status obtain a ten-year renewable residence permit and, except for voting, have the same rights as natives. Asylum seekers who are not eligible for refugee status but who cannot return to their origin country receive a subsidiary protection status. Subsidiary protection recipients obtain a one-year renewable residence permit and the right to work. In 2019, 17% of asylum seekers were eventually granted the refugee status, while 10% received the subsidiary protection.

The *Office Francais pour l'Integration des Immigrés* (OFII, French Office for the Integration of Immigrants) is the institution that assigns a place in a housing center to asylum seekers. If they accept the housing solution, they will receive a living allowance, the *Allocation pour Demandeurs d'Asile* (ADA, Asylum Seekers Allowance). Asylum seekers who then obtain a protection status can stay in their center for several months, go to centers dedicated to refugees, or exit the national hosting scheme. Once humanitarian migrants exit the centers, they could either stay in the original municipality or leave for other areas.

Due to the increase in asylum applications, it is frequently necessary to open new centers. Between 2004 and 2012, as can be seen in Figure 4.13 in Appendix 4.B.2, 98 housing centers were opened for the first time in a municipality. As shown in Figure 4.1, there are about 1,000 refugee centers homogeneously spread across the French metropolitan area. The average capacity of a housing center is of 66 humanitarian migrants.

The opening of housing centers is consistent with the government's willingness to "spread the burden" of refugee-hosting across the territory. The opening process is as follows : frequent regional project calls are launched at the initiative of the French Ministry of the Interior. NGOs and national housing landlords [2] apply to open and run housing centers. We know from discussions with French asylum actors that the selection of a project mainly depends on the architectural plan, the quality of the project and of the operator, and the financing modalities. Centers are opening in varieties of buildings – former hotels, housing centers for other types of population, police or fireman stations, blocks of private apartments – that can be either found in the existing lot of buildings owned by the center operator or rented for at least 15 years. It is only since 2015 that municipalities have to be consulted before the opening of a housing center for refugees. Focusing on centers that opened between 2004 and 2012, we avoid endogeneity concerns related to the involvement of local political forces. In Section 4.B.3 of the Appendix, we use press data to show that there was little public interest over refugee centers in our period of analysis.

There is a high turnover of refugees and asylum seekers in housing centers. In centers for asylum seekers, those who obtained the refugee status will have to leave in the next three months and a new asylum seeker will take his place. Because asylum seekers do not choose where they are allocated, the composition of refugees at the opening of a refugee housing center is determined primarily by the national refugee inflow. Controlling for national trends will then allow us to control for the

2. The so-called "Bailleurs sociaux" in France. They are semi-public bodies that rent social housing for low, means-tested household rent. They are also responsible for the social housing of certain vulnerable populations.

change in the refugee composition during the period of analysis.

4.2.2 Data

Housing centers

[Figure 4.1 here]

Our database of centers for humanitarian migrants was obtained from the French Ministry of the Interior. It includes all centers for asylum seekers (HUDA, CADA, AT-SA [3]) and all centers for refugees and beneficiaries of the subsidiary protection (CPH [4]) as of April 2018. This distribution of humanitarian migrants by legal status is not airtight as centers for asylum seekers can keep hosting refugees or beneficiaries of the subsidiary protection several months after they obtain the protection status. These centers are intended to be there for the long run, usually for at least 15 years. We double-checked the quality of our data with the FINESS [5] database for already closed centers. Our housing center database provides information on the exact location of the housing center, the name of the NGO or social housing landlord running the center, its opening date, and its capacity.

In our analysis, we focus on municipalities that have opened a housing center for humanitarian migrants for the first time. Second-time openings may not have the same impact on the labor market or on the hearts and minds of natives. Figure 4.1 displays the commuting zones where, between 2004 and 2012, a municipality, or several, opened for the first time a housing center. Figure 4.13 shows the timing of the ninety-eight openings we study in our paper. Table 4.14 in Appendix 4.D compares hosting municipalities in our sample with the average municipality in 2002. We see that hosting municipalities were on average more populated than the typical municipality, less rural, with a higher unemployment rate and fewer unskilled workers.

Municipality data

This study uses the DADS ("Declaration Annuelle des Donnees Sociales"), a database linking employees and employers. It covers approximately 85% of French employees in 2008 [6]. The DADS uses forms sent by private companies for the payment of employer contributions. Firms report periods of employment, the corresponding wage, and most importantly the municipality of residence (where the employee lives) and the municipality of employment (where the employee works) for each position held. We are also able to monthlyize the annual data given that we know when the

3. (HUDA : Emergency Accommodation for Asylum Seekers. CADA : Reception Centres for Asylum Seekers. AT-SA : Temporary Reception Asylum Service.
4. CPH :Provisional Accommodation Centre.
5. *Fichier National des Etablissements Sanitaires et Sociaux* database. We did not use this database for our analysis because it does not provide the capacity of housing centers and because the information on the date of the opening is sometimes inaccurate.
6. Public sector, and self-employed workers are not included.

job starts and ends during the year. We use the data available from 2002 to 2014 to create a panel of all French municipalities.

As a result, we have a monthly panel of more than 34,000 municipalities between 2002 and 2014, with the number and average wage of people *working* in the town and the number and average wage of people *living* in the town. According to Coudene et Levy (2016), this distinction is important since only 36% of active residents lived and worked in the same municipalities in 2013 in France. With the DADS data, we can use both the workplace and the residence to analyze the location decisions of natives. Though we cannot consistently differentiate foreigners and natives in the DADS, we explain in Appendix **??** why the population response is likely to be driven by natives.

The number of workers in the municipality informs us about whether people change their valuation of their *living* environment and local amenities. Natives may expect refugees to have a detrimental impact on the quality of local amenities, which could lead them to avoid or leave the area. We use the number of working residents as a measure of population as the annual municipal population statistics in the census are only collected every five years in municipalities of less than nine thousand inhabitants. Because the resident working population does not encompass some categories of the population such as retirees or people outside employment, we also use the IRCOM database from the *Direction Generale des Impots*, the French government agency in charge of taxable revenues. It provides information on the total number of fiscal households, of retiree households, of households that pay the income tax, on the total taxable revenues, and other variables at the municipal level. A fiscal household is the family unit composed of either a single person, or two partners and their children or other dependents who pay the French income tax as a group. Natives may also expect refugees to compete with them in local labor markets and to affect their wages and the number of available jobs. We also look at whether people change their valuation of their working environment by looking at workers in the municipality, instead of the number of employed residents.

To finish with, we use the so-called FICUS (until 2007) and FARE (since 2008) datasets that contains the financial and fiscal accounts sent by all French firms to the fiscal authority. We aggregate them to retrace at a yearly frequency the evolution of the number of firms, the total value-added, and the value of sales for the municipalities in our panel.

4.3 Methodology

4.3.1 Econometric model

Our methodology is as follows : we compare hosting municipalities with other municipalities within their commuting zone two years before and after the opening of a refugee center. We call an "event group" the group made up of one hosting municipality and its control municipalities. In our study, we observe 98 event groups for 49 months. We evaluate the average effect of the opening of refugee centers by estimating the following OLS model :

$$Log(Y_{igt}) = \beta.Opening_{it} + \mu_{gt} + \omega_i + \delta_t + \epsilon_{igt} \tag{4.1}$$

where $Opening_{it}$ is a dummy variable equal to one if a refugee center has opened in the municipality i at time t in the event group g. μ_{gt} capture time-varying shocks in the event group g. We only include observations that are less than 24 months away from the opening in a given event group g. δ_t, ω_i are calendar time and municipality fixed effects. Under conventional identification hypothesis, the OLS estimated coefficient of β measures the average deviation of hosting municipalities relative to their commuting zone trends (μ_{gt}) after the opening.

The separation by event group g makes our approach similar to a "stacked" difference-in-differences approach where we estimate a separate difference-in-differences model in every event group g. There is only one hosting municipality per event group g, but if there are several openings within a commuting zone, the commuting zone can be present several times. Hosting municipalities are never included in any control group. It is then equivalent to estimate separately the following model [7] in each event group two years before and after the opening :

$$Log(Y_{it}) = \beta_g.Opening_{it} + \omega_i + \delta_t + \epsilon_{it} \tag{4.2}$$

We can then aggregate all the coefficients β_g to get the average treatment on the treated β. Furthermore, we estimate the following model to look at the dynamic effect of openings :

$$Log(Y_{igt}) = \sum_{k=-24}^{24} \beta_l.\mathbb{1}\{l = k\} \times T_i + \mu_{gt} + \omega_i + \delta_t + \epsilon_{igt} \tag{4.3}$$

Where T_i is a dummy variable equal to 1 for refugee-hosting municipalities and l the time relative to the opening (refugee centers open when $l > 0$ and $l \in [-24, 24]$). Under conventional identification hypothesis, the OLS estimated coefficients β_l measures the causal impact of a housing

7. This approach is not equivalent to the two way fixed effect model with a dummy treatment variable that is traditionally used for difference-in-differences. Recent literature, De Chaisemartin et D'Haultfoeuille (2019), Goodman-Bacon (2018) or even Abraham et Sun (2018), has shown that when treatment effects are heterogeneous across cohorts ; the two-way fixed effect estimator recovers a linear combination of cohort-specific average treatment effects on the treated where some weights can be negative, mostly because early and late cohorts are not observed on an interval of time of the same length. In this paper we adopt the elegant solution proposed in Appendix D of Cengiz *et al.* (2019) : a "stacked" difference in difference approach which solves the issue by aligning event groups by event time and not calendar time : all cohorts are observed for the same number of time before and after the opening.

center opening l months away from the center opening on Y_{igt}. Standard errors are clustered at the municipality level.

4.3.2 Identification Hypothesis

Our approach is similar to a difference-in-differences as shown in equation (4.2) and Section 4.J of the Appendix. There are two hypothesis under which we estimate a causal effect : the parallel trend and the stable unit treatment value assumptions.

Parallel trend hypothesis : do refugee centers open in places with worse demographic prospects ?

[Figure 4.2 here]

The first hypothesis is that outcome trends in control municipalities are an appropriate estimate of what would have occurred in hosting municipalities without a refugee center opening. We test whether hosting and non-hosting municipalities within the same commuting zone follow a similar trend before a refugee center opens. If they have the same pre-trends, it is more likely that both groups would have evolved in the same way in the absence of a housing center opening in hosting municipalities.

Figure 4.2 shows that, on average, the opening date coincides with a significant and gradual decrease in the number of employed residents in hosting municipalities, whereas we see no change in other municipalities of the commuting zone. The divergence between the two groups could be overstated— e.g. all outflows from hosting municipalities could go to the control group after opening. Overall, the solid (hosting municipalities) and dashed (control municipalities) lines follow a similar trend before the opening but start to diverge at $l = 0$. This tends to support the parallel trend assumption as well as to indicate a negative impact of openings of refugee centers on population. Because hosting and non-hosting municipalities do not seem to diverge before the opening, this supports the hypothesis that refugee centers do not open in areas with worse demographic and economic prospects. The only drawback with Figure 4.2 is that it does not take into account potential spillover effects.

Stable unit treatment value assumption

The second hypothesis is that the opening of a refugee center in one municipality does not impact its control municipalities' potential outcome (stable unit treatment value assumption or SUTVA). In our case, we could face a breach of the SUTVA hypothesis as the impact of a refugee center opening may spill over neighboring towns. In particular, natives may move to neighboring towns in response to the opening of the refugee center. Another possibility is that the presence of a refugee housing center in the neighboring towns may also influence the decision to move to the municipality.

We use two different strategies to control for potential spillover effects. First, we replicate our analysis using an alternative control group. We match hosting municipalities with credibly unexposed municipalities outside the hosting commuting zone thanks to a propensity score. We match on the characteristics of localities two years before the opening of the center. The municipality characteristics come from the French Censuses : the population, whether the municipality is rural or not, the share of men, the share of unskilled workers, the number of migrants, the number of active workers, and the number of empty buildings. We bootstrap standard errors using the wild-bootstrap procedure and 500 repetitions (see Cameron *et al.* (2008)). For cities outside the commuting zone, the assumption that there is no spillover effect is more palatable.

[Figure 4.3 here]

We assess the quality of the matching procedure in Appendix 4.D. More importantly, we also check whether hosting and matched control municipalities share similar economic prospects before the opening of the refugee shelter. We compare the evolution of the number of working residents in hosting municipalities in a four-year window around the opening of the refugee center with the corresponding trend in the matched control group. In Figure 4.3, we see that hosting and matching control municipalities follow a similar trend before the opening of the refugee center. After the opening of the refugee center, the number of employed residents in the hosting municipalities decreased relative to control municipalities.

In our second and preferred approach, we keep municipalities within the same commuting zone in the control group. However, we control for the distance to the hosting municipality and its interaction with the time relative to the refugee center's opening. The inspiration comes from Clarke (2017) who derives a set of conditions with which difference-in-differences estimates can be unbiased even with a spillover effect on control units. To account for spillovers, we thus add $\sum_{k=-24}^{24} \nu_{ilg}.\mathbb{1}\{l = k\} \times d_{ig}$ to our equations of interest. d_{ig} measures the geographical distance to the hosting municipality in the control group and ν_{ilg} captures time-varying shocks related to the distance of municipality i to the treated municipality in the event group g. If spillover effects do not depend on municipality-specific time variant components, then ν_{ilg} captures local spillovers, that is the effect of being close to the hosting municipality when a refugee center opens. The assumption here is that the spillover effect is a linear function of the distance to the opened center. ν_{ilg} is also likely to control much more precisely for local shocks, even at the sub-commuting zone level. In Appendix 4.E, we further investigate potential spillover effects and propose another estimation strategy to take them into account.

4.4 Results

4.4.1 The mobility response of natives

Resident employed population

We estimate Equation (4.1) on a panel of French municipalities between 2002 and 2014 to look at the impact of refugee center openings on natives' mobility. Table 4.1 shows the estimated β for the number of working residents in five different specifications. Columns (1) and (2) present the estimated ATT (Average Treatment on the Treated) when a simple Difference-in-Difference approach is estimated using a 2-way fixed effects model. In column (2), we add event group fixed effects. Column (3) compares hosting municipalities with a matched sample of municipalities outside the hosting commuting zone. Columns (4) and (5) compare hosting municipalities with municipalities within their hosting commuting zone, but in column (5) we also take into account potential spillover effects. Column (5) is our preferred specification. Results are qualitatively similar across all specifications but vary in magnitude. On average, the number of residents employed decrease by about one to two percent relative to the control group following the refugee center opening.

[Table 4.1 here]

[Figure 4.4 here]

Figure 4.4 shows the estimated β_l coefficients of Equation (4.3) for the number of employed residents and their confidence interval using the same specification as in Column (5). β_l measures the impact on the population of the refugee center opening l months away in refugee-hosting municipalities compared to other municipalities in the same commuting zone. We see that hosting municipalities deviate from their control municipalities, starting from the time of the opening and not before. Twenty-four months after the opening, the population of employed residents in the hosting municipalities decreased by about 1.8% compared to the control group. Since the median size of a refugee center relative to the local workforce is 0.3%, this implies that there are three to six fewer people in the municipality for one refugee place in the center. Because humanitarian migrants could stay in the municipality once they exit the center, this gives an upper bound of the relationship between refugee inflow and the native population.

[Figure 4.5 here]

As mentioned earlier, estimating equation (4.1) is equivalent to estimating equation (4.2) separately for each event group g. In Figure 4.5, we plot β_g municipality by municipality, sorting by the size of the municipality treatment effect.

The advantage of Figure 4.5 is that it does not hide the heterogeneity of the effect, unlike previous tables and figures. In some municipalities, the opening of a refugee center has a positive effect on the number of residents. They may be false positives or indicate that the situation is more complex depending on the municipality, for example, openings may coincide with other events in some specific cases. Still, the majority of β_g is significantly lower than or not significantly different from 0. The red line is the estimated average treatment on the treated effect that we observe in column (5) of Table 4.1. Overall, Figure 4.5 confirms the results obtained so far and shows that they are not driven by outlier municipalities.

Fiscal households

So far, we have used changes in the number of employed residents as a proxy for changes in the population. Municipal population statistics from the census are only collected every five years in most municipalities. Using the number of employed residents was thus the only way to obtain a monthly measure of population for all municipalities. However, the number of employed residents is an imperfect measure of the population, since it does not include inactive people and people working in the public sector. The average correlation in cross-sections between the number of residents employed and the census population is 0.83. Within municipalities, the correlation between the evolution of population and the number of working residents is 0.25 [8]. To be sure that we are capturing the evolution of the population, we reproduce our approach in a yearly panel on the number of fiscal households that we obtained from the IRCOM dataset. Columns (1), (2) and (3) of Table 4.2 reproduce in the same order the models of columns (3), (4) and (5) from Table 4.1. They confirm our previous results both in sign and in magnitude.

[Table 4.2 here]

Native avoidance or native flight ?

If we identify a significant decrease in population due to the opening of the refugee center, this could mean that more people leave the municipality (an increase in outflows) or that fewer people arrive (a decrease in inflow) or a combination of both. In the literature, these two phenomenons are referred to as "native flight" and "native avoidance".

For each municipality, we measure the outflows as the number of resident workers who were in the locality the year before but are no longer, and inflows as the number of resident workers who were not in the locality last year but who are now. We are able to do this because we know for every position occupied by a worker in the year T the location of his job and his residence and whether he worked in the year T-1. Our definitions of outflows and inflows are similar to the one used by Dustmann *et al.* (2017).

8. It is the within R-squared of the OLS relating census population to the number of working residents when controlling for municipality fixed-effects.

In Table 4.3, we reproduce the approach of column (5) of Table 4.1 in a yearly panel with the log number of inflows in the municipality in column (1) and the log number of outflows in column (2). Due to the decline in population, both inflows and outflows decreased following the refugee center opening. The decrease in the number of inflows is, however, much higher than the decrease in outflows, and only the decrease in inflows is statistically significant. These results indicate that the decrease in population is due to fewer people moving in hosting municipalities following the opening of the refugee center. Native avoidance, rather than native flight, could, therefore, be the mechanism explaining the decrease in population following the opening of a refugee center.

[Table 4.3 here]

Heterogeneity

With the IRCOM dataset, we can also look more closely at which households react more strongly to the opening of a refugee center. In Table 4.4, we look at the effect of the opening on the number of retiree households (i.e. households with at least one pensioner) and the number of households taxed (i.e. households that pay the French income tax). In France, households who pay the income tax represent about half of the total population in 2014. This is because the income per unit to qualify for the income tax was 9,700€ in 2017 and is combined with other possible exemptions.

Retiree households are affected in the same way as the general population, which confirms that the origin of population decline cannot wholly be attributable to an effect on the labor market as we discuss in Appendix 4.A.1. Wealthier households that pay income tax react more strongly to the opening, which makes sense given that they are likely to have more alternative locations.

[Table 4.4 here]

We also investigate the effect on different sub-populations according to income skills and we do not find evidence of heterogeneous effects. We built different sub-populations of employed residents at the municipal level from the DADS. First, we divided the population by job type. On the one hand, there are residents working in low-skilled jobs (manual workers and employees PCS 5 and 6 in the French professional categories) and, on the other hand, residents employed in high-skilled jobs (professionals, executives and middle managers, PCS 3 and 4 in the French professional categories). Second, we divided the population in terms of their income by looking at the number of employed residents working above and below the national median wage.

[Table 4.12 here]

In Table 4.12, we do not observe any obvious heterogeneity of the effect. We might not see heterogeneous effects because we can only look at broad categories. Otherwise, we could end up dropping many small municipalities where the sub-population is 0. Also, the working wage may not be an important determinant for location decisions but rather wealth and capital income that we cannot measure at the local level. Finally, avoiding a hosting municipality is less costly than moving out of it. Avoidance is thus less related to income (and skill-related income) than flight. Overall, these results reinforce the unlikeliness of a labor market shock as it would likely be heterogeneous along skills and wages.

Because previous migrants can be more exposed to the economic impact of subsequent migrant inflows (Ottaviano et Peri, 2012), it would have been interesting to distinguish national and foreign workers. Already established migrants could bear the brunt of the local economic impact of the refugee center opening. Unfortunately, the DADS does not provide a coherent way to differentiate between national and foreign workers. In Figure 4.10, we show inconsistently large variations in the number of foreign workers every year that are likely explained by different information collection methods over the years. Nevertheless, we used the DMMO database to look at whether openings of refugee centers impacted the number of hires of foreigners in hosting municipalities. In Table 4.8 of the Appendix 4.A.1, we find no effect of the openings on the number of hires of foreign workers. In addition, several elements convince us that avoidance is not a reaction of the migrant population. First, migrants are underrepresented in the retiree population and retirees react as much (Table 4.4) than the general population (Table 4.2), which can also confirm that the avoidance behavior seems not to be related to a labor market threat of refugees. Second, municipalities with a higher migrant share do not experience higher native avoidance as shown in Figure 4.11.

[Figure 4.10 here]

[Figure 4.11 here]

Longer term effect

To investigate the longer term effect, we looked at the 76 openings between 2004 and 2010 in Figure 4.6 to extend the post-opening period of analysis to 48 months. As in our main analysis, we study the evolution of the number of employed residents. We also examine the effect of the openings on the year-to-year growth of the number of residents to identify exactly when the decrease in the population is the strongest. Taking year-to-year growth as a dependent variable is robust to violations of the parallel trend assumption in the number of residents. This identification now requires parallel growth rather than parallel trends, thus allowing for differential trends in the number of residents.

[Figure 4.6 here]

Figure 4.6 cannot be fully compared to Figure 4.4 as the samples of openings differ but results are similar. After the opening the number of employed residents and the growth in the number of employed residents decline. First, the number of employed residents increasingly decreases for more than two years. Then the decrease decelerates to become more or less stable after three years. This implies that after three years, either native avoidance becomes weaker, maybe because the signal effect of refugee centers fades, or native avoidance is compensated by something else. A fall of housing prices, reflecting the negative amenity shock of the center opening, could explain this. Unfortunately, we couldn't access to housing prices data at the municipality level going back to 2004 to make sure of that.

4.4.2 Economic consequences of native avoidance

Firm reaction

For refugee-hosting municipalities, native avoidance represents a decrease in the local pool of consumers. In addition, in subsection 4.4.1, we bring evidence that households not coming to the municipalities were richer than the average. We could expect the decrease in the number of workers shown in Appendix 4.A.1 to be driven by the decrease of the local demand for labor and goods in general.

We use data from the FICUS-FARE datasets, which gather individual data and statistics on company profit declarations to the French Tax General Directorate. We aggregate at the municipality level data on the total value-added, the value of sales, and the number of firms. We reproduce in Table 4.5 the models of columns (3) and (5) from Table 4.1 to see how the logarithm of these three outcomes evolve after the opening of a refugee center.

[Table 4.5 here]

Both approaches yield similar results in magnitude but not in significance. The total value-added and the value of sold production decrease from two to five percent after the opening. Using matched municipalities outside the commuting zone exhibit less statistical power than using municipalities within the commuting zone. We see that the number of firms, the value-added, and the number of sales decrease when we compare refugee-hosting municipalities to municipalities within the same commuting zone.

The total value of sales is decreasing, which could also indicate that the number of consumers falls. This reduction of demand could lead firms to relocate or to go out of business. But this could also be the result of firm avoidance : local entrepreneurs avoid locating their future business in refugee hosting municipality. For concerned entrepreneurs, the presence of a refugee housing center may be a bad signal and lead them to settle their business elsewhere. As much as native avoidance, this may decrease activity in refugee hosting municipality. We find no evidence that the opening rate of firms decreases in refugee hosting municipalities but the closing rate seems to increase in one specification, as shown in 4.5. Natives avoid refugee hosting municipalities and firms flee them in reaction.

In one of our specifications, the number of firms decreases by about three percent. In both cases, labor demand is likely to decrease. As the activity of local firms is decreasing after the opening of the refugee centers, the demand for native workers diminishes. Hosting refugees seem to depress economic activity because of native avoidance.

Impact on local taxes

At the municipal level, the reduction of the population can have a cost for local authorities in terms of tax base. We estimate that the opening of a refugee center is associated with a reduction of approximately five points of the tax base, as we can see in Table 4.6. The tax-base reduction can have important consequences for the finance of hosting municipalities. In order to learn more about the state of local finances, we exploit a dataset from the DGFIP that records the different sources of income of French municipalities since 2000. French municipalities have two main sources of financing : local taxes collected by the municipality and direct transfers from the State.

[Table 4.6 here]

There are different types of local taxes. One of the main ones is the housing tax, *taxe d'habitation*, which is a rental value tax paid by inhabitants. The second is a property tax on constructed lands, *taxe foncière sur les propriétés bâties*, which is paid by landowners. We expect the fiscal base of these taxes to decline with the openings such that their total income decrease as well. In Table 4.6 we check whether the revenues of the local taxes are negatively affected. The proceed of local taxes decrease by more than 3 percent when we compare refugee-hosting municipalities with municipalities within the same commuting zone, and by one percent with matched municipalities outside the commuting zone. This is because the proceeds of the housing tax and the property tax are both declining, by almost six and three percent in our less conservative estimates. Indeed, native avoidance can result in some non-rented housing units, and as owners can be exempted from the housing and property tax if the housing is vacant, the total municipal revenues from these taxes would decrease. In Appendix 4.I we verify that this evolution is not explained by municipalities reducing local tax rates for housing and property taxes.

4.5 Estimating the aggregate welfare cost of refugee center openings

In this section, we model the effect of a refugee center opening as a negative amenity shock. Native avoidance means that the opening of refugee housing centers decreases the perceived quality of amenities by prospective residents. Following our previous findings, the opening of a refugee center should be seen above all as an amenity shock at the local level that may or may not have an indirect impact on the labor market. Focusing on the quality of amenities is also consistent with what people feel about immigration. Card *et al.* (2012b) find that compositional concerns are 2 to 5 times more important in explaining variations in individual attitudes towards immigration policy than concerns about wages and taxes.

We base our model on the spatial general equilibrium models described by Roback (1982) and Rosen (1979), which have been used to analyze the location decisions of individuals between cities. The specificity of their model is that, in addition to housing costs, both income and amenities can vary. In particular, housing costs and wages may fluctuate in order to compensate consumers for differences between cities in terms of actual or perceived quality of life.

Residents choose to locate in a city that maximizes their utility, i.e. where their wage net of housing and amenity costs is the highest. However, there is no perfect mobility of labor, given that workers have different preferences over locations. Migration is costly, depending on the preference of workers. We can write the direct utility of the resident j in the city i as :

$$V_{ji} = \epsilon_{ji}.\frac{w_i.z_i}{P_i^{\beta}} \tag{4.4}$$

where ϵ_{ji} is a random variable measuring the preference for a city i by an individual j, w_i is the local nominal wage, z_i measures the value of local amenities between 0 and 1, P_i the local housing prices, and β is the share of housing costs in the resident's consumption. $\frac{w_i.z_i}{P_i^{\beta}}$ is the wage net of the cost for housing and amenities.

Each city i produces a homogeneous good and has the following Cobb-Douglas constant return to scale production function :

$$Y_i = A_i.L_i^{\alpha}.T_i^{1-\alpha} \tag{4.5}$$

where A_i is the total factor productivity in city i, T_i is the land available for business use, L_i is the number of working residents and α is the elasticity of production with respect to labor, also known as the labor share of income. For simplicity, we choose a production function without capital, but this would not alter our final predictions if we were to include it [9].

The local housing price is given by :

$$P_i = \bar{P}_i.L_i^{\gamma} \tag{4.6}$$

with γ being the inverse elasticity of housing supply with respect to the number of residents in the city, and $\bar{P}_i$ the part of the local housing price that does not vary with the number of residents. We assume γ to be the same across cities [10].

9. Hsieh et Moretti (2019) arrive at the same conclusion with a production function where capital is a production factor.
10. This assumption does not change the main predictions of the model.

In this simple model, we assume, like Kline et Moretti (2014), that the joint distribution of ϵ_{ji} is given by $F_g(\epsilon_1, ..., \epsilon_N) = e^{-\sum_i^N \epsilon_i^{-\theta}}$ where $1/\theta$ measures the degree of labor mobility. This assumption means that the labor supply curve is upward slopping and that its slope depends on the heterogeneity in labor mobility. The inverse local labor supply of a given city is :

$$W_i = V.\frac{\bar{P}_i^{\beta}.L_i^{1/\theta}}{Z_i} \tag{4.7}$$

where V is the average worker utility in all cities. This expression means that when a city experiences a change in wages, amenities, or housing prices, the number of people willing to move in or out depends on $1/\theta$. The case of perfect mobility is $\theta = \infty$.

At equilibrium, the marginal product of labor is equal to the local nominal wage and the inverse local labor demand that determines the number of residents is then :

$$L_i = (\frac{\alpha}{V}.A_i.T_i^{1-\alpha}.\frac{z_i}{\bar{P}_i^{\beta}})^{\frac{1}{1-\alpha+\beta(\gamma+1/\theta)}} \tag{4.8}$$

The partial local equilibrium of the model is given by equations (4.6) for housing prices, (4.7) for local wage, and (4.8) for the number of residents. Differences in population across cities are driven by differences in amenities, real wage, and housing prices.

In previous sections, we showed that the opening of a refugee center constituted a negative amenity shock for hosting municipalities. We now look at the effect of this redistribution of residents at an aggregate level. As hosting municipalities become less attractive, natives move out or avoid hosting municipalities such that demand for housing increases in non-hosting municipalities. To measure the aggregate welfare cost of such a refugee shock, we need to look at its impact on the aggregate utility V. In the following, we explicitly state the general equilibrium conditions and use the model to estimate the aggregate welfare costs of opening a refugee center.

The related intuition of this study is that people "vote with their feet" as in Tiebout (1956) and that the evolution of the population is an important outcome to consider when studying the opening of refugee centers. Following Hsieh et Moretti (2019), we use the assumption that the change in the number of people employed in a city relative to the national average is a sufficient statistics for the aggregate welfare effect of all the local shocks in the town in a Rosen-Roback model.

At complete equilibrium, residents need to be indifferent across cities, which means that the local price in a city Q_i $(= \frac{P_i^{\beta}}{z_i})$ relative to the national (resident weighted) average corrected for imperfect labor mobility $\bar{Q}(= \sum_i L_i^{1+1/\theta}.Q_i)$ is equal to the local wage (W_i) relative to the national average ($\bar{W}$) :

$$\frac{Q_i}{\bar{Q}} = \frac{W_i}{\bar{W}} \tag{4.9}$$

If we now impose that aggregate labor demand is equal to aggregate labor supply (normalized to one), we get the following expression for aggregate output Y :

$$Y = \sum_i (A_i . \frac{Q_i}{\bar{Q}} . T_i^{1-\alpha}) \tag{4.10}$$

Also, since the labor share of income is α, aggregate utility is given by :

$$V = \frac{\alpha . Y}{\bar{Q}} \tag{4.11}$$

The ratio of aggregate labor income to average local price across all cities is $\bar{Q}$. The general equilibrium of this model is defined by the equations for local population (4.8), housing prices (4.6), nominal wages (4.7), aggregate output (4.10) and aggregate utility (4.11).

As in Hsieh et Moretti (2019), population change is a sufficient statistic for the aggregate effect of all the local forces involved in the model : total factor productivity, amenities, and housing prices. To understand this, consider a change in aggregate utility after an amenity shock :

$$\Delta V \propto \Delta (\frac{z_i}{P_i^{\beta}})^{\frac{1}{1-\alpha+1/\theta}} \tag{4.12}$$

We can decompose this expression in two parts. The first part of the equation, $z_i^{\frac{1}{1-\alpha+1/\theta}}$, is the direct effect of a change in amenities on aggregate utility (the effect of changing weighted average local amenities). The second part $P_i^{-\beta . \frac{1}{1-\alpha+1/\theta}}$, represents the missallocation effect, which results from the change in the local marginal product of labor relative to the rest of the country, and the price effect, which results from how the change in local prices Q_i modifies the local prices average in the country $\bar{Q}$.

In addition, if we substitute equation (4.7) into equation (4.8), we find that the change in population is proportional to :

$$\Delta L_i \propto \Delta (A_i . \frac{z_i}{P_i^{\beta}})^{\frac{1}{1-\alpha+1/\theta}} \tag{4.13}$$

Comparing equations (4.12) and (4.13), we realize that the evolution of the local population in the event of an amenity shock is proportional to the change in aggregate utility if productivity (A_i) is fixed. In other words, the evolution of the local population is a sufficient statistic for the effect of local shocks on aggregate utility or welfare if we control for A_i.

From Table 4.1 and Figure 4.4, we know that the average change in the number of residents in hosting municipalities relative to other municipalities within the same commuting zone is between 1 and 2%. Following the model, we can measure the contribution of the refugee center openings to welfare growth by calculating its contribution to overall population growth.

We are not assuming a measure of welfare here, but we are just measuring how much average welfare would have grown without one additional opening of a refugee center. The average hosting municipality population accounts for 0.05% of the total French population. This means that one opening reduces on average aggregate welfare growth by 10^{-4} percentage points (0.05×0.02) over two years.

We can go even further. According to equation (4.11), if we assume that local shocks do not affect national average prices and that the labor share of income remains stable, the evolution of the number of residents is a sufficient statistic for the effect of local shocks on aggregate output. As it turns out, 10^{-4} points of output amounted to around 2.1 million euros in France in 2014. Under these broad assumptions, this is then a measure of the indirect costs of hosting refugees. This figure should be used only for rough comparisons but gives an approximation of the costs – in terms of lost wages, higher housing prices, and lower non-refugee amenities – that on aggregate potential newcomers are ready to pay in order not to be living near refugee centers. This is also the compensating variation of the refugee center opening i.e. the amount of money you would need to collectively give to prospective newcomers to reverse native avoidance.

4.6 Conclusion

In this paper, we tried to understand the local economic impact of refugee accommodation policies. We show that municipalities hosting refugees experience a decrease in their population and in their economic performances. We demonstrate that this is not because refugee centers open in municipalities with far worse economic prospects nor the result of the actions of refugees. We are the first paper to explain this adverse economic impact of hosting refugees by inefficiencies that natives themselves have produced by avoiding refugee-hosting localities. We exploit the openings of housing centers for refugees and asylum seekers in about one hundred municipalities between 2004 and 2012 in France. We compare these municipalities with a control group that did not experience the opening of a refugee center. After a center opens, the population decreases by about two percent. We show that this decline is because fewer people move in refugee-hosting municipalities.

This change in population reflects a shift in the preference of natives for hosting municipalities following the opening of refugee housing centers. Households with higher incomes, and then probably more location choices, react more to the opening. On the one hand, natives could see the opening of the refugee center as a signal for higher competition for local resources. On the other hand, natives may avoid hosting municipalities because of cultural insecurity or xenophobia. In our context, the competition channel is not credible as refugee inflows are negligible even at the local level and our results point toward the latter channel. We demonstrate that people living near refugees do not leave at a higher rate, which would indicate that the deterrent effect of refugees is due to prejudices of those who are not in contact with them.

We find a local negative economic impact of hosting refugees, although it is not due to direct harm caused by refugees but to native avoidance and firm avoidance. Fewer natives entering host municipalities means fewer taxpayers and fewer consumers. We find that hosting municipalities, because of lower tax collection, and firms, because of lower sales, suffer from it. Unfortunately, we weren't able to combine our rich source of data with housing prices data to understand the connection between the effects we observe and the housing market. Housing price reactions could compensate for the negative amenity shock of the refugee center opening and explain why native avoidance seem to fade after 3 years.

We use the evolution of the population in hosting municipalities as a sufficient statistics to assess the aggregate welfare cost due to the redistribution of the population because of the openings. We find that opening a refugee center reduces aggregate welfare growth by 10^{-4} percentage points on average. For ballpark comparisons and under additional assumptions to equalize changes in welfare to changes in output, we estimate that the cost of opening a refugee center amounts on average to 2.1 million euros over two years. This is the welfare loss (due to either lower wage, higher housing prices or lower amenities) incurred by people who are moving in their second-best location instead of the refugee hosting municipality because of the presence of refugee hosting center.

Our results show that refugee inflows can provoke segregation at the municipality level. The policy implications of our findings are twofold. First, refugee dispersal policies could cost more at the local level than it would appear because of native avoidance. Segregation after the opening of the refugee housing center has a significant impact on the local tax base and may depress the local economy. This put into question the pertinence of refugee dispersal policies : opening less but bigger refugee centers near more dynamic labor and housing markets may reduce native avoidance and improve refugee integration chances. Second, we may also reduce the cost of housing refugees by addressing the root of self-segregation – native prejudices against refugees – rather than focusing on potential refugee rivalry. Active campaigns to change people's view of refugees may be needed and be economically profitable. This conclusion extends to any prejudices that may lead people to change their location decision, this paper is also about the cost of self-segregation on society.

Bibliographie

Sarah Abraham et Liyang Sun : Estimating dynamic treatment effects in event studies with heterogeneous treatment effects. *Unpublished Manuscript*, 2018.

Lina Aldén, Mats Hammarstedt et Emma Neuman : Ethnic Segregation, Tipping Behavior, and Native Residential Mobility. *International Migration Review*, 49(1):36–69, mars 2015. ISSN 0197-9183.

Gordon Willard Allport, Kenneth Clark et Thomas Pettigrew : The nature of prejudice. 1954.

Binnur Balkan, Elif Tok, Huzeyfe Torun et Semih Tumen : Immigration, Housing Rents, and Residential Segregation : Evidence from Syrian Refugees in Turkey. SSRN Scholarly Paper ID 3205899, Social Science Research Network, Rochester, NY, juillet 2018.

Rosario Maria Ballatore, Margherita Fort et Andrea Ichino : Tower of Babel in the Classroom : Immigrants and Natives in Italian Schools. *Journal of Labor Economics*, 36(4):885–921, 2018.

K. Bansak, J. Hainmueller et D. Hangartner : How economic, humanitarian, and religious concerns shape European attitudes toward asylum seekers. *Science*, 354(6309):217–222, octobre 2016a. ISSN 0036-8075, 1095-9203.

Kirk Bansak, Jens Hainmueller et Dominik Hangartner : How economic, humanitarian, and religious concerns shape european attitudes toward asylum seekers. *Science*, 354(6309):217–222, 2016b.

David G. Blanchflower et Chris Shadforth : Fear, unemployment and migration. *The Economic Journal*, 119(535):F136–F182, 2009.

George J. Borjas : The wage impact of the Marielitos : A reappraisal. *ILR Review*, 70(5):1077–1110, 2017.

George J. Borjas et Joan Monras : The labour market consequences of refugee supply shocks. *Economic Policy*, 32(91):361–413, 2017.

Leah Platt Boustan : Was postwar suburbanization “white flight” ? evidence from the black migration. *The Quarterly Journal of Economics*, 125(1):417–443, 2010.

A Colin Cameron, Jonah B Gelbach et Douglas L Miller : Bootstrap-based improvements for inference with clustered errors. *The Review of Economics and Statistics*, 90(3):414–427, 2008.

David Card : The impact of the Mariel boatlift on the Miami labor market. *ILR Review*, 43 (2):245–257, 1990.

David Card, Christian Dustmann et Ian Preston : Immigration, Wages, and Compositional Amenities. *Journal of the European Economic Association*, 10(1):78–119, février 2012a. ISSN 1542-4766.

David Card, Christian Dustmann et Ian Preston : Immigration, Wages, and Compositional Amenities. *Journal of the European Economic Association*, 10(1):78–119, février 2012b. ISSN 1542-4766.

David Card, Alexandre Mas et Jesse Rothstein : Tipping and the dynamics of segregation. *The Quarterly Journal of Economics*, 123(1):177–218, 2008.

Doruk Cengiz, Arindrajit Dube, Attila Lindner et Ben Zipperer : The effect of minimum wages on low-wage jobs : Evidence from the united states using a bunching estimator. Rapport technique, National Bureau of Economic Research, 2019.

Damian Clarke : Estimating difference-in-differences in the presence of spillovers. 2017.

Michael Clemens, Cindy Huang, Jimmy Graham *et al.* : The economic and fiscal effects of granting refugees formal labor market access. *Center for Global Development and TENT. https ://www. cgdev. org/publication/economic-and-fiscal-effects-granting-refugees-formal-labor-market-access-brief*, 2018.

Maud Coudene et David Levy : De plus en plus de personnes travaillent en dehors de leur commune de residence. *INSEE PREMIERE*, N.1605, 2016.

Julie Berry Cullen et Steven D Levitt : Crime, urban flight, and the consequences for cities, 1999.

Ernesto Dal Bó, Frederico Finan et Martín A Rossi : Strengthening state capabilities : The role of financial incentives in the call to public service. *The Quarterly Journal of Economics*, 128 (3):1169–1218, 2013.

Clément De Chaisemartin et Xavier D'Haultfoeuille : Two-way fixed effects estimators with heterogeneous treatment effects. Rapport technique, National Bureau of Economic Research, 2019.

Christian Dustmann, Uta Schönberg et Jan Stuhler : The Impact of Immigration : Why Do Studies Reach Such Different Results ? *Journal of Economic Perspectives*, 30(4):31–56, novembre 2016. ISSN 0895-3309.

Christian DUSTMANN, Uta SCHÖNBERG et Jan STUHLER : Labor Supply Shocks, Native Wages, and the Adjustment of Local Employment. *The Quarterly Journal of Economics*, 132(1):435–483, février 2017. ISSN 0033-5533.

Christian DUSTMANN, Kristine VASILJEVA et Anna PIIL DAMM : Refugee migration and electoral outcomes. *The Review of Economic Studies*, 86(5):2035–2091, 2018.

Per-Anders EDIN, Peter FREDRIKSSON et Olof *Aslund* : $Settlement policies and the economic success of immigrants. Journal of Population Economics, 17$ $(1) : 133 - -155, 2004.$

ANTHONY EDO : THE IMPACT OF IMMIGRATION ON WAGE DYNAMICS : EVIDENCE FROM THE ALGERIAN INDEPENDENCE WAR. 2017.

ANTHONY EDO, YVONNE GIESING, JONATHAN ÖZTUNC ET PANU POUTVAARA : IMMIGRATION AND ELECTORAL SUPPORT FOR THE FAR-LEFT AND THE FAR-RIGHT. *European Economic Review*, 115:99–143, 2019.

GIOVANNI FACCHINI, ANNA MARIA MAYDA ET MARIAPIA MENDOLA : WHAT DRIVES INDIVIDUAL ATTITUDES TOWARDS IMMIGRATION IN S OUTH A FRICA ? *Review of International Economics*, 21(2):326–341, 2013.

TORBERG FALCH : THE ELASTICITY OF LABOR SUPPLY AT THE ESTABLISHMENT LEVEL. *Journal of Labor Economics*, 28(2):237–266, 2010.

FRANCESCO FASANI, TOMMASO FRATTINI ET LUIGI MINALE : (THE STRUGGLE FOR) REFUGEE INTEGRATION INTO THE LABOUR MARKET : EVIDENCE FROM EUROPE. 2018.

CHARLOTTE GEAY, SANDRA MCNALLY ET SHQIPONJA TELHAJ : NON-NATIVE SPEAKERS OF ENGLISH IN THE CLASSROOM : WHAT ARE THE EFFECTS ON PUPIL PERFORMANCE ? *The Economic Journal*, 123(570):F281–F307, 2013.

MARKUS GEHRSITZ ET MARTIN UNGERER : JOBS, CRIME, AND VOTES : A SHORT-RUN EVALUATION OF THE REFUGEE CRISIS IN GERMANY. 2017.

ANDREW GOODMAN-BACON : DIFFERENCE-IN-DIFFERENCES WITH VARIATION IN TREATMENT TIMING, 2018.

JENS HAINMUELLER ET DANIEL J HOPKINS : PUBLIC ATTITUDES TOWARD IMMIGRATION. *Annual Review of Political Science*, 17:225–249, 2014.

MARTIN HALLA, ALEXANDER F WAGNER ET JOSEF ZWEIMÜLLER : IMMIGRATION AND VOTING FOR THE FAR RIGHT. *Journal of the European Economic Association*, 15(6):1341–1385, 2017.

Jakob Hennig : Refugee shelters, Neighbourhood Quality and Electoral Outcomes in Germany. page 45, 2018.

Chang-Tai Hsieh et Enrico Moretti : Housing constraints and spatial misallocation. *American Economic Journal : Macroeconomics*, 11(2):1–39, 2019.

Jennifer Hunt : The impact of the 1962 repatriates from Algeria on the French labor market. *ILR Review*, 45(3):556–572, 1992.

Guido W. Imbens : Potential outcome and directed acyclic graph approaches to causality : Relevance for empirical practice in economics. *WP*, 2019.

Steiner P. Kim, Y. : Causal graphical views of fixed effects and random effects models. *WP*, 2019.

Patrick Kline et Enrico Moretti : People, places, and public policy : Some simple welfare economics of local economic development programs. 2014.

Linna Martén, Jens Hainmueller et Dominik Hangartner : Ethnic networks can foster the economic integration of refugees. *Proceedings of the National Academy of Sciences*, 116(33):16280–16285, 2019.

Anna Maria Mayda : Who Is Against Immigration ? A Cross-Country Investigation of Individual Attitudes toward Immigrants. *The Review of Economics and Statistics*, 88(3):510–530, août 2006. ISSN 0034-6535.

Cristina Mitaritonna, Gianluca Orefice et Giovanni Peri : Immigrants and firms' outcomes : Evidence from France. *European Economic Review*, 96:62–82, juillet 2017. ISSN 0014-2921.

Gianmarco I. P. Ottaviano et Giovanni Peri : Rethinking the Effect of Immigration on Wages. *Journal of the European Economic Association*, 10(1):152–197, février 2012. ISSN 15424766.

Alkis Henri Otto et Max Friedrich Steinhardt : Immigration and election outcomes—evidence from city districts in hamburg. *Regional Science and Urban Economics*, 45:67–79, 2014.

Judea Pearl : *Causality*. Cambridge university press, 2009.

Giovanni Peri : Immigrants, Productivity, and Labor Markets. *Journal of Economic Perspectives*, 30(4):3–30, novembre 2016. ISSN 0895-3309.

Giovanni Peri et Vasil Yasenov : The labor market effects of a refugee wave : Synthetic control method meets the Mariel boatlift. *Journal of Human Resources*, page 0217_8561R1, 2018.

Jennifer Roback : Wages, rents, and the quality of life. *Journal of political Economy*, 90(6):1257–1278, 1982.

Vaughan Robinson, Roger Andersson et Sako Musterd : *Spreading the'burden' ? : A Review of Policies to Disperse Asylum Seekers and Refugees.* Policy Press, 2003.

Simon Rosen : Wage-based indexes of urban quality of life. *Current Issues in Urban Economics*, pages 74–104, 1979.

Andreas Steinmayr : Contact versus exposure : Refugee presence and voting for the far-right. *Review of Economics and Statistics*, pages 1–47, 2020.

Charles M Tiebout : A pure theory of local expenditures. *Journal of political economy*, 64(5):416–424, 1956.

Aico Van Vuuren, Josef Kjellander et Viktor Nilsson : Refugees and apartment prices : A case study to investigate the attitudes of home buyers. *Regional Science and Urban Economics*, 77:20–37, 2019. ISSN 18792308.

Paul Vertier et Max Viskanic : Dismantling the'jungle' : Migrant relocation and extreme voting in france. *Available at SSRN 2963641*, 2019.

Annexe 4.A Alternative channels

4.A.1 Local Labor market

[Table 4.8 here]

We examine whether the competition on the labor market between refugees and natives could explain the decrease in local population. For instance, natives could flee or restrain from entering local labor market because of increased competition. To do so, we start by investigating potential evidence of labor supply shock from refugees. We study the effect of openings on the logarithm of the number of hires of foreigners in municipalities, which we obtained from the DMMO database, in Table 4.8. We find no indication that the number of hires for foreigners increases, which is what we expected given the small size of humanitarian inflows and that asylum seekers do not have the right to work. However, the opening of a refugee center could lead to an increase of unobserved informal employment. We then look at the effect of openings on workers in the municipality, which include both resident and non-resident workers. Table 4.9, Figure 4.7 and Figure 4.7 can be interpreted in the same way as Table 4.1 and Figure 4.4. In Table 4.9, we use the approach of column (3) and (5) of Table 4.1 to look at two different outcomes that are the number of workers employed and their average hourly wage. Looking at the 95% confidence intervals, we see that we can exclude a decrease of more than 1.5% of the hourly wage. The number of workers may slightly decrease, but this effect is far more imprecise than the one on residents.

[Table 4.9 here]

[Figure 4.7 here]

To investigate whether the evolution of employment may be caused by the change in the resident population, we look at the evolution of the number of non-resident workers in the municipality following the opening of the center. Table 4.10 shows no significant effects of the openings on the number of non-resident workers. Compared to Figure 4.7 where we consider all workers, Figure 4.8 does not provide any evidence of a decrease in the number of non-resident workers, which indicates that the decrease in the number of workers is likely to be driven by workers who are also residents.

[Table 4.10 here]

However, we cannot totally exclude the possibility that we don't capture a significant effect on the number of workers in the hosting municipality because this is a more noisy variable given that people change more of jobs than of residence. Some coefficients in Tables 4.1 and 4.10 are negative and of the same magnitude as in Table 4.9, although their standard error is much higher.

Nevertheless, given the small size of refugee inflow compared to the local labor supply, we believe that this might be due to a decrease in labor demand. A simple labor and supply model would show that in order to achieve a reduction in the number of workers without a decrease in wages, there must be either a decrease in labor demand or an exceptionally high labor supply elasticity. A ballpark calculation using β_{+24} for the number of workers and hourly wage from Figure 4.7 gives an estimated labour supply elasticity of about 6.5 if labor demand does not adjust. This much higher than the range of elasticities of 1 to 1.9 reported by Falch (2010) for Norwegian teachers and the estimate of 2 reported by Dal Bó *et al.* (2013).

[Figure 4.8 here]

Finally, our results are in accordance with the ones of Vertier et Viskanic (2019) where they look at the effect of opening CAO housing centers in France from 2012 to 2017. They find no significant difference in net job creation per inhabitant between municipalities which eventually received a CAO center and those that did not. We complement this result by showing that employment and wages in hosting municipalities are also not affected.

4.A.2 Criminality

Can the population decline be explained by an increase in crime due to the arrival of refugees ? This is a relevant question if we assume that safety concerns could affect the relocation choice of natives. Moreover, even a small inflow of criminals could have a large impact on criminality.

Gehrsitz et Ungerer (2017) studied the 2015 refugee wave and its influence on the number of crimes committed in Germany. They find that there is a substantial and positive correlation between the number of refugees distributed to a county and an increase in crime, even after they removed immigration violations from the crime statistics. In particular, they put forth a 1.5% increase in crime in regions that welcomed a higher number of refugees compared to regions that welcomed a lower amount. This section aims to replicate these results in the French case, to investigate whether this could be a explanation of why natives change their location decision after the opening of a refugee center.

We look at departmental crime databases from 2002 to 2014, where the police report the monthly number of crimes and misdemeanours. We compare departments where refugee centers have opened and departments where they have not. We divide crime and misdemeanour into several categories, such as murders or murder attempts, thefts, sexual assaults of any kind, drug-related crimes or misdemeanours. In Table 4.16 of the Appendix, we describe how we classify the original 108 categories of crimes and misdemeanours in these four broader categories.

[Table 4.11 here]

Table 4.11 shows the difference-in-difference coefficients when the outcome is the logarithm of the monthly count of crimes at the departmental level. The minimum detectable effect for the total number of crimes is 1.22 [11], which means that if the number of crimes increased by more than 1.2%, we would have detected it. This is a level below what Gehrsitz et Ungerer (2017) found. Figure 4.9 plots the evolution of the number of crimes or misdemeanours committed in departments experiencing the opening of a refugee center compared its average evolution in other departments. Events are weighted by the capacity of the refugee center to give greater importance to departments with a higher influx of refugees and asylum seekers.

In this section, we do not claim to draw a definite conclusion as to the relationship between the opening of refugee centers and crimes. The report of crimes is endogenous, as an increase in the number of crimes or misdemeanours reported may indicate a real increase or an increase in reporting. Moreover, unlike Gehrsitz et Ungerer (2017), we do not identify the victim or the perpetrator. Finally, we only observe crime statistics at the departmental level and not at the municipal level as in the rest of our study, and we are unable to identify anti-refugee crimes with the existing French Police classification.

[Figure 4.9 here]

Overall, there is no real difference between the criminality time series in refugee-hosting departments and in other departments. We cannot link the opening of a refugee center to an outbreak of crime. Looking at American cities in the 1990s, Cullen et Levitt (1999) finds that a 10% increase in crime has led to a 1% decline in population. Criminality is therefore unlikely to explain the full extent of what we observe in refugee-hosting municipality.

Annexe 4.B Refugees and housing centers

4.B.1 Humanitarian migrants in France

[Figure 4.12 here]

4.B.2 Refugee centers openings

[Figure 4.13 here]

11. This is an ex-post measurement, based on the fact that the minimum detectable effect is 2.8 times the standard error.

4.B.3 Refugee centers in the press

Our period of analysis is relatively calm regarding the public interest in refugee issues. Figure 4.14 shows the evolution of the proportion of articles in the French regional and national press relating to the terms "refugees" and "housing centers" between 2000 and 2018. Between 2004 and 2012 there were 4 times fewer articles including those two terms than after 2014, or than during the refugee crisis of the Yougoslav Wars from 2001. We believe this minimizes the risk that residents anticipated the openings.

[Figure 4.14 here]

Annexe 4.C Definition of population

[Figure 4.15 here]

Annexe 4.D Matching

[Table 4.13 here]

To assess the quality of the matching procedure, we start by checking whether the propensity score properly balances the characteristics between hosting municipalities and comparison group units. Table 4.13 shows the mean for all the Census variables used to estimate the propensity score matching for the five nearest neighbors in the control sample (our preferred matching algorithm [12]). Matching was efficient as the characteristics of both samples are reasonably balanced, with no statistically significant differences.

We also investigate whether municipalities that have similar characteristics have the same likelihood of being both in the control and treatment group. Testing for the common support assumption is a way to test the performance of the propensity score-matching algorithm. In Figure 4.16, we thus look at the distribution of propensity scores for hosting and control municipalities. Both distributions are very close, indicating that the propensity score matching algorithm performs well.

[Figure 4.16 here]

12. The result of our approach with one and ten nearest neighbors is available in Appendix 4.H

Annexe 4.E Spillover effect

4.E.1 Identification

To ensure the proper identification of spillover effect in our case, there are two identification hypothesis :

1. Parallel trends between the treatment group and the municipalities further away from the refugee center but still within the commuting zone (which act as a kind of "super control" group).
2. Parallel trends between municipalities in the commuting zone close and further away from the refugee center (as municipalities closer to the center serve as a second treatment group and are compared to the "super control" group).

We take into account potential spillover effects assuming it evolves homogeneously and linearly with distance.

4.E.2 Estimation

In this subsection, we look at spillover effects from the opening of a refugee center within a commuting zone. To do so, we plot the evolution of the coefficients of the interaction between categories of distance to the hosting municipality and event time. Effectively, it measures how the opening of a housing center ripple through the neighboring area. Native avoidance may also affect neighboring municipalities of refugee-hosting municipality.

The following figures show how the interaction terms between distance and event time for four categories (ν_{ilg} in our model) evolve over time relatively to the opening for residents and workers. Overall, like in previous instances, we capture no effect significantly different from zero when looking at workers (Figure 4.18). However, the number of residents in municipalities more than ten kilometers away from the refugee center increased by about one percent compared to closer municipalities while they follow similar trend before the opening (Figure 4.17). We interpret this as an evidence that after the opening, residents move from the hosting town to go the municipalities further away from the housing center. It is a confirmation of the need to control for potential spillover effects.

[Figure 4.17 here]

[Figure 4.18 here]

To go further in this direction, we present in Table 4.15 an alternative estimation methodology to take into account potential spillover effect. We estimate as before equation (4.1) but $Opening_{it}$ is now equal to one for the hosting municipalities after the refugee-center opening but also for all municipalities less than 10 kilometers away. When we do so, we obtain similar coefficients than in Tables 4.1 and 4.9 even though we probably underestimate of the effect of refugee center openings (because we consider as treated municipalities that are less likely to be impacted by the opening).

[Table 4.15 here]

The choice of a 10 kilometer cutoff may seem arbitrary but this methodology has the advantage of not assuming any particular parametric form to the spillover effect on the contrary of our preferred estimation method. Moreover, in Figure 4.19, we plot the coefficients of the interaction terms between $Opening_{it}$ and d_{ig} when taking $d_{ig} = 10km$ as a base level. We notice that there is a real divergence between municipalities less and more than 10 kilometers away from the center. This is what motivated us to choose this particular distance cutoff.

[Figure 4.19 here]

Annexe 4.F Main types of crime and misdemeanour

[Table 4.16 here]

Annexe 4.G Another amenity shock : day-care centers closures

Our methodology has its limitations. It assumes that after only two years an equilibrium has been reached, that an amenity shock has no impact on productivity, or that housing prices alone would be able to offset the welfare loss due to the opening of housing centers. While evaluating these figures, these vulnerabilities must be taken into account. For the sake of comparison, we replicate this method to compute the welfare cost of closing day-care centers in rural France.

As a sanity check for our methodology, we try to use it to study the welfare effect of the closings of day-car centers. The approach is similar to the one we used throughout the paper : we conduct an event analysis at the municipality level to compare how population (the number of fiscal households according to the IRCOM dataset here) evolve at the time of the day-care center closing compared to a control group of municipalities. There were about 245 day-car centers closing between 2004 and 2012, so we have 245 different event groups (more than two times the number of event groups for our study of refugee center openings).

[Table 4.17 here]

Table 4.17 can be read the same way as Table 4.2, its coefficients represents the average population decrease after two years in municipalities that experienced a day-care center closing relatively to matched municipalities outside the commuting zone (column 1) and municipalities within the commuting zone accounting for spillover effect (column 3). Population decreased by almost 1.5 percent. The magnitude of the population effect is similar to the one a migrant housing center opening. The average hosting municipality accounts for 0.05% of the French population. It means that one closing reduces on average aggregate welfare growth by 7.5×10^{-5} points. This is 75% of the effect of a refugee center opening. Please remember that the validity of this estimate is subject to the identification assumptions exposed before. If the no spillover assumption is not met then we might underestimate or overestimate the welfare cost of the closing and if the parallel trend assumption is not met, we are not sure which local shock we are studying the effect of.

Annexe 4.H Other matching algorithms

[Table 4.18 here]

Annexe 4.I Local tax rates

[Table 4.7 here]

In Table 4.7, we see that the housing and property tax rates do not decrease following the opening of a housing center. This confirms that the reduction in the revenues from the housing and property taxes must come from a decrease in the tax-base.

Annexe 4.J Identification problem

[Figure 4.20 here]

In Figure 4.20, we give a representation of the classical identification problem with a directed acyclic graph [13] (DAG). Each node represents a variable or a group of variables and each arrow a causal relationship between them.

In this paper, we want to study the direct effect of the opening of a refugee center (the treatment) on several local outcomes. To do so, we cannot directly compare municipalities with and

13. See Pearl (2009) and Imbens (2019) for a more thorough discussion on the relevance of such approach in social sciences.

without openings. Unobserved or not, confounders are municipality characteristics that may affect both the opening of a refugee housing center and local outcomes. It may generate spurious correlations between the two groups of variables (Treatment ← Unobs. confounders → Outcome). Our identification strategy has then to condition for potential confounders, e.g. past economic performance, local economic shocks, etc...

Yet, it should not control for potential mediator variables. The opening of a refugee center may affect the local economic environment and even other variables : local politics, demography, the provision of public goods, etc... They may in turn affect local outcomes. Conditioning on these variables would partial out the measured effect of the opening from these indirect paths.

To finish with, the identification strategy should also not control for potential collider variables. That is to say, other municipality characteristics affected by the opening but also by the evolution of local outcomes (population, local wage, ...), like the workforce composition or the municipalities tax base. To control for them may induce a spurious bias between the opening and local outcomes. For example, if size had no significant correlation with marking ability in the NBA, it would not not mean that size does not help to score basketball points but that smaller NBA basketball players can compensate with other skills. To sum up, controlling for collider and mediator variables is a threat to identification and any post-opening local variable could be suspect of being part of these two groups.

To solve these issues, we exploit our panel of hosting and non-hosting municipalities from 2002 to 2014. We control for confounders by comparing hosting and non-hosting municipalities before and after the opening. Not all non-hosting municipality can be part of the control group : we compare each hosting municipalities to other municipalities within their commuting zone and only look at how outcome trends evolve before and after the opening of the center. We do so because our identifications assumptions are then more credible.

[Figure 4.21 here]

The identification problem can be further refined as shown in Figure 4.21 in a panel with two periods (pre-treatment in $t-1$ and post-treatment in t) and two types of unit (hosting municipalities (1) and non-hosting ones (2)). This is the approach of most event analysis and difference-in-difference strategy. Figure 4.21 was inspired by the one-way fixed effect model representation of Kim (2019) and is just an extension to two-way fixed effects. The identification of β (the treatment effect) requires that all non-causal paths are blocked by conditioning on middle-variables on the non-causal paths. By definition, unobserved confounders cannot be controlled for, which means the causal path |Center opening in t ← Unobserved cofounders → $Y_{t,1}$ | cannot be blocked.

The linear structural models which decribes how the different variables are linked together in Figure 4.21 can be written as follow :

— $Y_{t,1} = \beta.T_{t,1} + \gamma_1.U_i + \delta.t$

— $Y_{t-1,1} = \gamma_1.U_i + \delta.(t-1)$

— $Y_{t,2} = \gamma_2.U_i + \delta.t$

— $Y_{t-1,2} = \gamma_2.U_i + \delta.(t-1)$

To control for unobserved cofounders, one solution is to use a first difference estimator ($Y_{t,1} - Y_{t-1,1}$) to recover β. It is possible if the repeated outcomes are affected by the same unobserved cofounders to the same extent (γ_1). However, doing so is problematic because the first difference will also capture the effect of time on outcome Y (as $Y_{t,1} - Y_{t-1,1} = \delta + \beta$). To control for it, we need to do another difference with the first difference for a group of unit which is not experiencing a center opening ($Y_{t,2} - Y_{t-1,2} = \delta$). This double differences will then be an estimator of β (as $(Y_{t,1} - Y_{t-1,1}) - (Y_{t,2} - Y_{t-1,2}) = \beta$).

To sum up, there are four identification hypothesis :

1. Repeated outcomes are affected by the same unobserved cofounders to the same extent ($Y_{t,i} - Y_{t-1,i} = \gamma_i + \delta$ i.e. common trend assumption for unit fixed effects).
2. Control and treatment units are following the same time trends ($Y_{t,2} - Y_{t-1,2} = \delta$ i.e. common trend assumption for time fixed-effects).
3. There is no causal paths between Center opening in t and $Y_{t,2}$ (no spillover of the opening on non-hosting municipalities).
4. There is no causal paths between Center opening in t and $Y_{t-1,1}$ (no anticipation of the opening in hosting municipalities) [14].

14. During our period of analysis, refugee hosting was not at heart of news as shown in Figure 4.14. It makes this assumption likely.

Tables

TABLE 4.1 – Effect of a refugee center opening on the resident employed population

	Log number of employed residents in the municipality				
	(1)	(2)	(3)	(4)	(5)
$Opening_{it}$	-.09618***	-.08568***	-.01721**	-.01074*	-.02113***
	(.00828)	(.00818)	(.00734)	(.00642)	(.00482)
Method	DD	DD	Matching	EA	EA
Observations	5567538	5565114	18461	840633	840633
R^2	0.992	0.993	0.999	0.999	0.999

Source : DADS 2002-2014. Note : *** p<0.01, ** p<0.05, * p<0.1. "DD" stands for Difference-in-Difference and "EA" stands for Event Analysis. Weighted by the population two years before the opening. Standard errors are clustered at the municipality level. We control for municipality, relative and calendar time, and event group fixed effects in columns (2) to (5). An event group is composed of a hosting municipality and other municipalities within the same commuting zone for EA and DD, and of matched municipalities outside the commuting zone for the matching. We control for spillovers as in Clarke (2017) in column (5). Unweighted estimates in Table 4.19. Reading : According to column (5), after the opening of a refugee center, the number of employed residents decrease by 2,1 % in refugee-hosting municipalities compared to other municipalities within the same commuting zone.

Back

TABLE 4.2 – Effect of a refugee center opening on the fiscal population

	Log number of fiscal households in the municipality		
	(1)	(2)	(3)
$Opening_{it}$	-.0121***	-.0258***	-.0261***
	(.0032)	(.0027)	(.0027)
Method	Matching	Event Analysis	Event Analysis
Observations	3064	98370	98370
R^2	0.999	0.999	0.999

Source : IRCOM 2002-2014. Note : *** p<0.01, ** p<0.05, * p<0.1. Weighted by population two years before the opening. Standard errors are clustered at the municipality level. We control for municipality, relative and calendar time, and event group fixed effects. An event group is composed of a hosting municipality and other municipalities within the same commuting zone for the event analysis, or of matched municipalities outside the commuting zone for the matching. We control for spillovers as in Clarke (2017) in column (3).

Back

Table 4.3 – Effect of a refugee center opening on inflows and outflows of employed residents

	Inflows	**Outflows**
	(1)	(2)
$Opening_{it}$	-.0282***	-.0149
	(.0098)	(.0104)
Method	Event Analysis	Event Analysis
Observations	86695	86695
R^2	0.997	0.997

Source : DADS 2002-2014. Note : *** p<0.01, ** p<0.05, * p<0.1. Inflows corresponds to the *log* number of employed residents that move in the municipality. Outflows corresponds to the *log* number of employed residents that move out of the municipality. Standard errors are clustered at the municipality level. Weighted by population two years before the opening. We control for municipality, relative and calendar time, and event group fixed effects. An event group is composed of a hosting municipality and other municipalities within the same commuting zone. We control for spillovers as in Clarke (2017).

Back

Table 4.4 – Effect of a refugee center opening on retirees and taxed households

	Number of retiree households			**Number of taxed households**		
	(1)	(2)	(3)	(4)	(5)	(6)
$Opening_{it}$	-.0173***	-.0313***	-.0210***	-.0281***	-.0491***	-.0541***
	(.0040)	(.0038)	(.0038)	(.0046)	(.0041)	(.0041)
Method	Matching	EA	EA	Matching	EA	EA
Observations	2935	96802	96802	2919	97048	97048
R^2	0.999	0.999	0.999	0.999	0.999	0.999

Source : IRCOM 2002-2014. Note : *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the *log* number of fiscal households receiving pensions, retirements, or annuities in the municipality in columns (1) to (3), and the *log* number of fiscal households that pay the income tax in the municipality in columns (4) to (6). "EA" stands for Event Analysis. Weighted by population two years before the opening. Standard errors are clustered at the municipality level. We control for municipality, relative and calendar time, and event group fixed effects. An event group is composed of a hosting municipality and other municipalities within the same commuting zone for EA, and of matched municipalities outside the commuting zone for the matching. We control for spillovers as in Clarke (2017) in columns (3) and (6).

Back

Table 4.5 – Effect of a refugee center opening on firms outcomes

	Value-added		**Sales**		**Number of firms**		**Closing rate**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Opening_{it}$	-.0290	-.0285*	-.0391	-.0572***	-.0036	-.0292***	.0624**	.0132
	(.0183)	(.0160)	(.0267)	(.0215)	(.0132)	(.0100)	(.0270)	(.0208)
Method	Matching	EA	Matching	EA	Matching	EA	Matching	EA
Observations	2405	77150	2400	76733	2407	77498	1998	44751
R^2	0.996	0.975	0.994	0.967	0.998	0.982	0.980	0.675

Source : FICUS-FARE 2002-2014. Note : *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the *log* total value added of firms in the municipality in columns (1) and (2), *log* total value of firms' sales in the municipality in columns (3) and (4), the *log* number of firms in the municipality in columns (5) and (6) and the closing rate of firms in columns (7) and (8). Weighted by population two years before the opening. Standard errors are clustered at the municipality level. We control for municipality, relative and calendar time, and event group fixed effects. An event group is composed of a hosting municipality and other municipalities within the same commuting zone for EA, and of matched municipalities outside the commuting zone for the matching. In the Event Analysis ("EA"), we control for spillovers as in Clarke (2017).

Back

TABLE 4.6 – Effect of a refugee center opening on local taxes

	Tax base		Local taxes		Housing tax		Property tax	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Opening$_{it}$	-.0143	-.0556***	-.00868	-.0334***	-.0307**	-.0592***	-.0145*	-.0360***
	(-.0136)	(-.0045)	(.0118)	(.00841)	(.0108)	(.00673)	(.00721)	(.00585)
Method	Matching	EA	Matching	EA	Matching	EA	Matching	EA
Obs.	3064	98364	2830	117095	2830	117095	2830	117095
R^2	0.996	0.999	0.998	0.996	0.998	0.998	0.999	0.998

Source : IRCOM 2002-2014 in (1) and (2) and DGFIP 2002-2014 in (3) to (8). Note : *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the *log* average tax reference income in the municipality in columns (1) and (2), the *log* amount of local taxes that are levied by local authorities in the municipality in columns (3) and (4), the *log* amount of housing taxes that is levied by the municipality in columns (5) and (6), the *log* amount of property taxes that is levied by the municipality in columns (7) and (8). Weighted by population two years before the opening. Standard errors are clustered at the municipality level. We control for municipality, relative and calendar time, and event group fixed effects. An event group is composed of a hosting municipality and other municipalities within the same commuting zone for EA, and of matched municipalities outside the commuting zone for the matching. In the Event Analysis ("EA"), we control for spillovers as in Clarke (2017).

Back

TABLE 4.7 – Effect of a refugee center opening on local tax rates

	Housing tax rate		Property tax rate	
	(1)	(2)	(3)	(4)
Opening$_{it}$	-.0179	-.0144	.00135	.00192
	(.00965)	(.00811)	(.00569)	(.00669)
Method	Matching	EA	Matching	EA
Observations	2830	117170	2830	117170
R^2	0.971	0.949	0.991	0.968

Source : DGFIP 2002-2014. Note : *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the housing tax rate that is applied in the municipality in columns (1) and (2) and the property tax rate that is applied in the municipality in columns (3) and (4). Weighted by population two years before the opening. Standard errors are clustered at the municipality level. We control for municipality, relative and calendar time, and event group fixed effects. An event group is composed of a hosting municipality and other municipalities within the same commuting zone for EA, and of matched municipalities outside the commuting zone for the matching. In the Event Analysis ("EA"), we control for spillovers as in Clarke (2017).

Back

TABLE 4.8 – Effect of a refugee center opening on hires of foreign workers

	Log number of foreign workers hired		
	(1)	(2)	(3)
Opening$_{it}$	.0108	.0013	-.0062
	(.0142)	(.0054)	(.0111)
Method	Matching	EA	EA
Observations	3366	31246	31246
R^2	0.579	0.461	0.462

Source : DMMO 2002-2014. Note : *** p<0.01, ** p<0.05, * p<0.1. “EA” stands for Event Analysis. Weighted by population two years before the opening. Standard errors are clustered at the municipality level. We control for municipality, relative and calendar time, and event group fixed effects. An event group is composed of a hosting municipality and other municipalities within the same commuting zone for EA, and of matched municipalities outside the commuting zone for the matching. We control for spillovers as in Clarke (2017) in column (3).

Back

Table 4.9 – Effect of a refugee center opening on resident and non-resident workers

	Hourly wage		**Number of workers**	
	(1)	(2)	(3)	(4)
$Opening_{it}$	.00534	.00039	-.02036	-.00365
	(.00662)	(.00578)	(.01619)	(.01104)
Method	Matching	EA	Matching	EA
Observations	16599	707348	16602	707645
R^2	0.838	0.987	0.996	0.999

Source : DADS 2002-2014. Note : *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the *log* average hourly wage of residents and non-resident workers in the municipality in columns (1) and (2), and the *log* number of residents and non-resident workers in columns (3) and (4). Weighted by population two years before the opening. Standard errors are clustered at the municipality level. We control for municipality, relative and calendar time, and event group fixed effects. An event group is composed of a hosting municipality and other municipalities within the same commuting zone for EA, and of matched municipalities outside the commuting zone for the matching. In the Event Analysis ("EA"), we control for spillovers as in Clarke (2017).

Back

Table 4.10 – Effect of a refugee center opening on non-resident workers

	Log number of non-resident workers		
	(1)	(2)	(3)
$Opening_{it}$	-.02562	-.00319	.00395
	(.01885)	(.00829)	(0.01272)
Method	Matching	EA	EA
Observations	17072	693657	683510
R^2	0.995	0.998	0.998

Source : DADS 2002-2014. Note : *** p<0.01, ** p<0.05, * p<0.1. “EA” stands for Event Analysis. Weighted by population two years before the opening. We control for event group fixed effects and standard errors are clustered at the municipality level. An event group is composed of a hosting municipality and other municipalities within the same commuting zone for EA, and of matched municipalities outside the commuting zone for the matching. We control for spillovers as in Clarke (2017) in column (3).

Back

Table 4.11 – Effect of a refugee center opening on crimes in the department

	Total	**Murders**	**Thefts**	**Sexual Assaults**	**Drugs**
	(1)	(2)	(3)	(4)	(5)
$Opening_{dt}$	.0002	-.0047	.0021	.0154	-.0340**
	(.0044)	(.0395)	(.0045)	(.0136)	(.0152)
Observations	14361	14361	14361	14361	14361
R^2	0.984	0.436	0.984	0.832	0.830

Source : French Ministry of the Interior. Note : *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the *log* number of crimes committed in the department in column (1), the *log* number of murders in (2), the *log* number of thefts in (3), the *log* number of sexual assaults in (4), the *log* number of drug-related crimes in (5). Standard errors clustered at the departmental level. Reading : According to column (1), after the the opening of a refugee center, the total number of crimes increased on average by 0.02 % in refugee-hosting departments compared to other departments after the openings.

Back

TABLE 4.12 – Effect of a refugee center opening on residents employed by skill and income

	Highly skilled		**Low-skilled**		**Low income**		**High income**	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Opening_{it}$	-.0196**	-.0247***	-.0109	-.0226***	-.0171*	-.0163**	-.0146*	-.0214***
	(.0082)	(.0064)	(.0108)	(.0064)	(.0097)	(.0064)	(.0075)	(.0067)
Method	Matching	EA	Matching	EA	Matching	EA	Matching	EA
Obs.	18551	844922	18205	856735	18542	855284	18444	853153
R^2	0.998	0.999	0.998	0.999	0.998	0.999	0.998	0.999

Source : DADS 2002-2014. Note : *** p<0.01, ** p<0.05, * p<0.1. The dependent variable is the *log* number of resident employed in high-skilled jobs (professionals, executives and middle managers, PCS 3 and 4 in the French professional categories) in columns (1) and (2), the *log* number of residents employed in low-skilled jobs (manual workers and employees PCS 5 and 6 in the French professional categories) in columns (3) and (4), the *log* number of employed residents with an income below the national median wage in columns (5) and (6), the *log* number of employed residents with an income above the national median wage in columns (7) and (8). Weighted by population two years before the opening. Standard errors are clustered at the municipality level. We control for municipality, relative and calendar time, and event group fixed effects. An event group is composed of a hosting municipality and other municipalities within the same commuting zone for EA, and of matched municipalities outside the commuting zone for the matching. In the Event Analysis ("EA"), we control for spillovers as in Clarke (2017).

Back

TABLE 4.13 – Hosting vs. matched municipalities 2 years before the openings

	Matched municipalities	**Hosting municipalities**	**P-value**
Unskilled (%)	41.3	42.0	0.845
Rural (%)	8.7	7.2	0.641
Unemployed (nb)	1663	2088	0.272
Active (nb)	9094	11324	0.213
Men (%)	50.9	50.5	0.694
Population	33430	38934	0.389
Migrants	3997	4928	0.254
Vacant buildings (nb)	1454	1563	0.729
Observations	485	98	

Source : Census 2002-2010. The table compare municipalities which experienced a refugee housing center opening between 2004 and 2012 with their matched municipalities two years before the opening.

Back

TABLE 4.14 – Hosting vs. other municipalities in 2002

	Other municipalities	**Hosting municipalities**	**P-value**
Unskilled (%)	47.5	44.2	0.003
Rural (%)	50.1	7.2	0.000
Unemployed (nb)	63	2134	0.000
Active (nb)	533	11951	0.000
Men (%)	50.3	47.9	0.000
Population	1611	39377	0.000
Migrants	93	3158	0.000
Vacant buildings (nb)	55	1515	0.000
Observations	34536	98	

Source : Census 2002. The table compares municipalities that experienced a refugee housing center opening between 2004 and 2012 with all other municipalities in 2002.

Back

TABLE 4.15 – Effect of a refugee center opening on the population employed in the municipality and in municipalities within a 10-km vicinity

	Residents	**Residents' wage**	**Workers**	**Workers' wage**
	(1)	(2)	(3)	(4)
$Opening_{it}$	-.0090***	.0008	-.0072*	-.0003
	(.0015)	(.0008)	(.0037)	(.0018)
Observations	871177	871177	718231	717922
R^2	0.999	0.996	0.999	0.987

Source : DADS 2002-2014 Note : *** p<0.01, ** p<0.05, * p<0.1. We estimate as equation (4.1) with $Opening_{it}$ is now equal to one after the refugee-center opening for hosting municipalities and municipalities less than 10 kilometers away. The dependent variable is the *log* number of employed resident in the municipality in column (1), the *log* average hourly wage of employed residents in (2), the *log* number of employed resident and non-residents in the municipality in (3), the *log* average hourly wage of employed residents and non-residents in (4). Weighted by population two years before the opening. Standard errors are clustered at the municipality level. We control for municipality, relative and calendar time, and event group fixed effects. An event group is composed of a hosting municipality and other municipalities within the same commuting zone. In this Event Analysis, we control for spillovers as in Clarke (2017).

Back

Table 4.16 – Main type of crimes by category

Category	Type of crime or misdemeanours
Murder or murder attempts	Murder for robbery and theft Murder for other reasons Attempted homicides to steal and during robberies Attempted homicides to steal and during robberies Assault and battery followed by death
Theft	Violations of residence Armed robberies with firearms against financial institutions Armed robberies with firearms against industrial or commercial establishments Armed robberies with firearms against CIT companies Armed robberies with firearms against individuals in their homes Other armed robberies with firearms Edged-arms robberies against financial, commercial or industrial institutions Flights with edged weapons or by destination against private individuals in their homes Other thefts with knives or by destination Theft with unarmed violence against financial, commercial or industrial establishments Theft with unarmed violence against individuals in their homes Theft with unarmed violence against women on public streets or other public places Theft with unarmed violence against other victims Burglary of main living quarters Burglaries of second homes Burglary of industrial, commercial or financial premises Burglaries from other places Flights with trickery into any place Pickpocketing Shoplifting Cargo transport vehicle theft Auto theft Two-wheeled motor vehicle theft Caravan flights Theft of accessories on registered motor vehicles Simple flights on site Simple theft on farms Other simple thefts against public or private establishments Other simple thefts against private individuals in private premises Other simple thefts against individuals in public premises or places
Sexual assaults of any sort	Rape of adults Rape of minors Sexual harassment and other sexual assaults against adults Sexual harassment and other sexual assaults against minors Sexual offences
Drugs related	Trafficking and resale without the use of drugs Use and resale of drugs Use of drugs Other drug law offences
Forgeries	Forgery of identity documents forgery of documents concerning vehicle traffic Other forgery of administrative documents Forgery of public and authentic writing Other forgeries in writing
Clandestine employment	Clandestine employment Employment of foreigners without a work permit

Back

Table 4.17 – Effect of day-care centers openings on the number of fiscal households

	Log number of fiscal households		
	(1)	(2)	(3)
$Opening_{it}$	-.0134***	-.0260***	-.0149*
	(.0021)	(.0039)	(.0083)
Method	Matching	EA	EA
Observations	6827	133056	133056
R^2	1.000	0.999	0.999

Source : IRCOM 2002-2014. Note : *** p<0.01, ** p<0.05, * p<0.1. "EA" stands for Event Analysis. Weighted by population two years before the opening. Standard errors are clustered at the municipality level. We control for municipality, relative and calendar time, and event group fixed effects. An event group is composed of a hosting municipality and other municipalities within the same commuting zone for EA, and of matched municipalities outside the commuting zone for the matching. We control for spillovers as in Clarke (2017) in column (3).

Back

Table 4.18 – Effect of a refugee center opening on the resident employed population with other matching algorithms

	Log number of employed residents	
	(1)	(2)
$Opening_{it}$	-.00151	-.01301*
	(.00684)	(.00682)
Method	No replacements	10 nearest neighbors
Observations	8416	26698
R^2	1.000	0.999

Source : DADS 2002-2014. Note : *** p<0.01, ** p<0.05, * p<0.1. The table reproduce the approach of the column (3) in Table 4.1 with two different matching algorithms : in (1), a propensity score matching to the nearest neighbor with no replacements and in (2), a matching to the 10 nearest neighbors with replacements. Weighted by population two years before the opening. Standard errors are clustered at the municipality level. We control for municipality, relative and calendar time, and event group fixed effects. An event group is composed of a hosting municipality and matched municipalities.

Back

Table 4.19 – Effect of a refugee center opening on the resident employed population (no weights)

	Log number of employed residents in the municipality		
	(3)	(4)	(5)
$Opening_{it}$	-.01670**	-.03161***	-.05464***
	(.00734)	(.00429)	(.00560)
Method	Matching	EA	EA
Observations	18559	871704	857853
R^2	0.999	0.997	0.997

Source : DADS 2002-2014. Note : *** p<0.01, ** p<0.05, * p<0.1. "DD" stands for Difference-in-Difference and "EA" stands for Event Analysis. Standard errors are clustered at the municipality level. We control for municipality, relative and calendar time, and event group fixed effects in columns (2) to (5). An event group is composed of a hosting municipality and other municipalities within the same commuting zone for EA and DD, and of matched municipalities outside the commuting zone for the matching. We control for spillovers as in Clarke (2017) in column (5). Reading : According to column (5), after the opening of a refugee center, the number of employed residents decrease by 2,1 % in refugee-hosting municipalities compared to other municipalities within the same commuting zone.

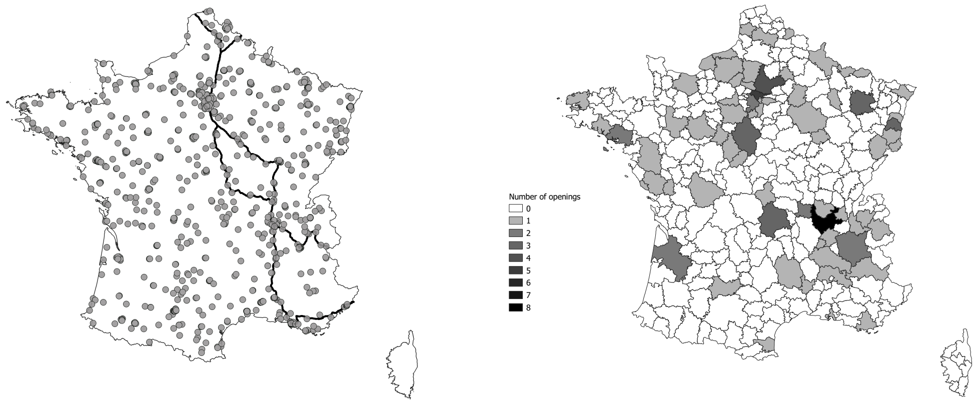
Back

Figures

Figure 4.1 – Housing centers in France

Housing centers localisation and migratory routes

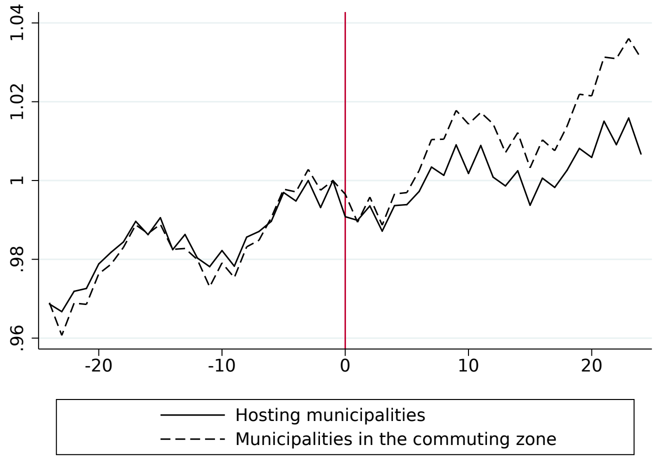
Refugee center openings by commuting zones between 2004 and 2012



Note : On the left is the location of all housing center for refugee in France in April 2018 and the migratory routes (from IOM - monitoring flows). On the right is a map of the French commuting zones. White areas are commuting zones that did not have any opening between 2004 and 2012. Colored areas are commuting zones that had at least one opening during the period.

Back

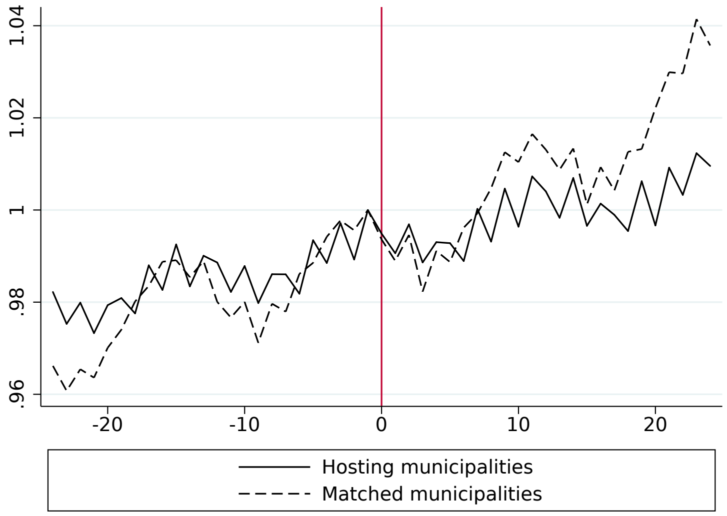
Figure 4.2 – Number of employed residents in hosting and non-hosting municipalities



Source : DADS 2002-2014. Note : The solid line represents the evolution of the number of employed residents in hosting municipalities and the dashed line the evolution within other municipalities from the same commuting zone. They are standardized to one the month before the refugee housing center is opened. Time series are deseasonalized for better readability.

Back

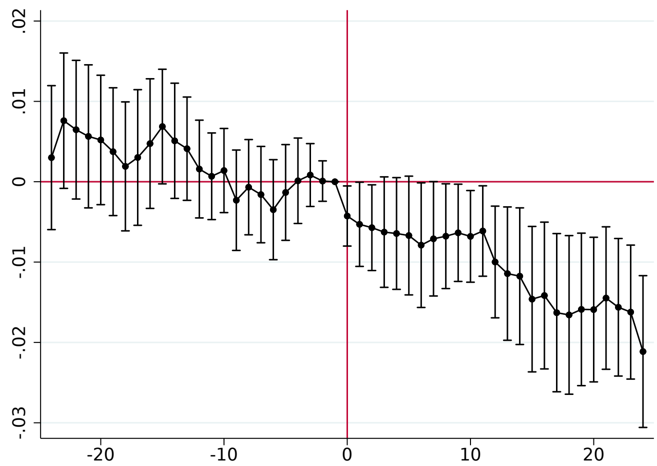
Figure 4.3 – Number of employed residents in hosting and matched municipalities



Source : DADS 2002-2014. Note : The solid line represents the evolution of the number of employed residents in hosting municipalities. The dashed line represents the evolution of the number of employed residents in matched municipalities. They are normalized to one month before the opening of the refugee housing center. Time series are deseasonalized for better readability.

Back

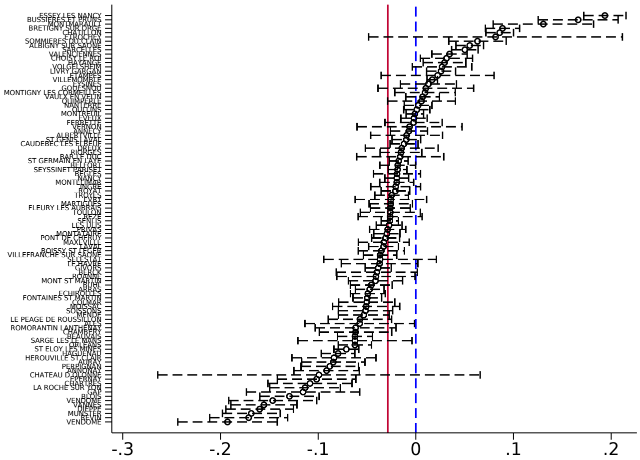
Figure 4.4 – Dynamic effect of a refugee center opening on the resident employed population



Source : DADS 2002-2014. Note : Estimated β_l from equation (4.3) where l (x-axis) is the number of months relative to the opening of the refugee housing center and where the outcome is the number of residents employed. The incertitude of each point is asserted with a 95% confidence interval. Reading : Twenty-four months after the opening, the number of working residents has decreased by about 2 percent in refugee-hosting municipality compared to the other municipalities within their commuting zone.

Back

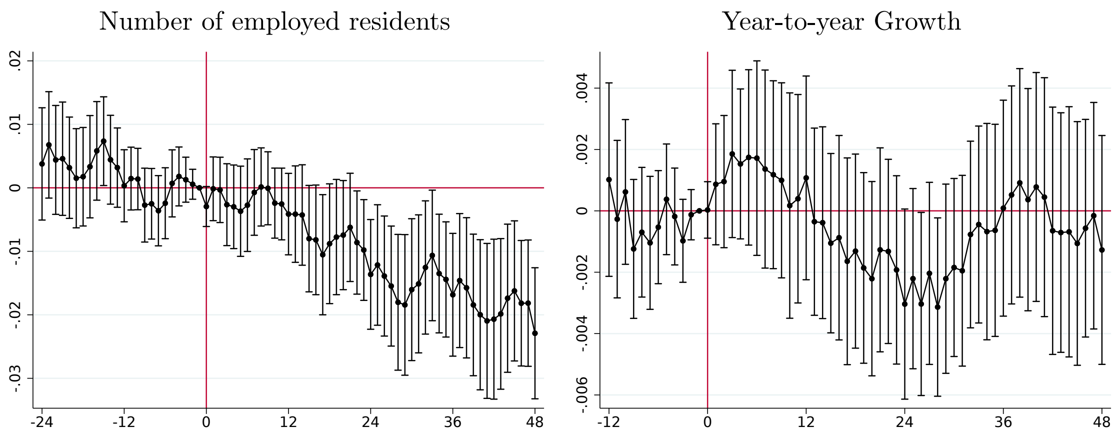
FIGURE 4.5 – Effect of a refugee center opening on the resident employed population by event



Source : DADS 2002-2014. Note : Distribution of the estimated β_g from equation (4.2) in each event group g. An event group is composed of a hosting municipalities and of other municipalities in the same commuting zone. It is referenced here by the name of the hosting municipality. The red line marks the average treatment effect on the treated.

Back

FIGURE 4.6 – Longer-time dynamic effect of a refugee center opening on the resident employed population



Source : DADS 2002-2014. Note : Estimated β_l from equation (4.3) where l (x-axis) is the number of months relative to the opening of the refugee housing center and where the dependent variable is either the number of residents employed or its year-to-year growth. Compared to Figure 4.4, the period of analysis has been extended to 4 years after the opening of a refugee center. The incertitude of each point is asserted with a 90% confidence interval.

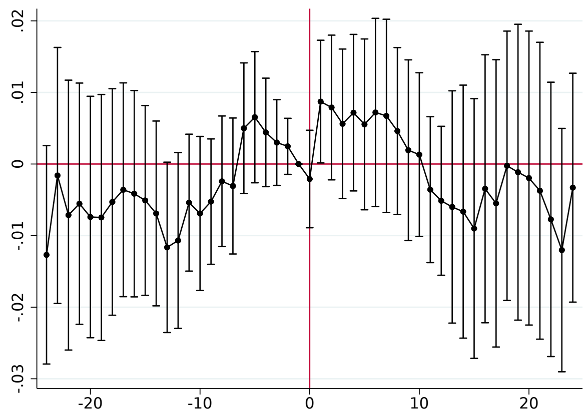
Back

FIGURE 4.7 – Dynamic effect of a refugee center opening on the local labor market

Source : DADS 2002-2014. Note : Estimated β_l from equation (4.3) where l (x-axis) is the number of months relatively to the opening of the refugee housing center for the *log* number of residents and non-resident workers and their *log* hourly wage. The incertitude of each point is asserted thanks to a 95% confidence interval.

Back

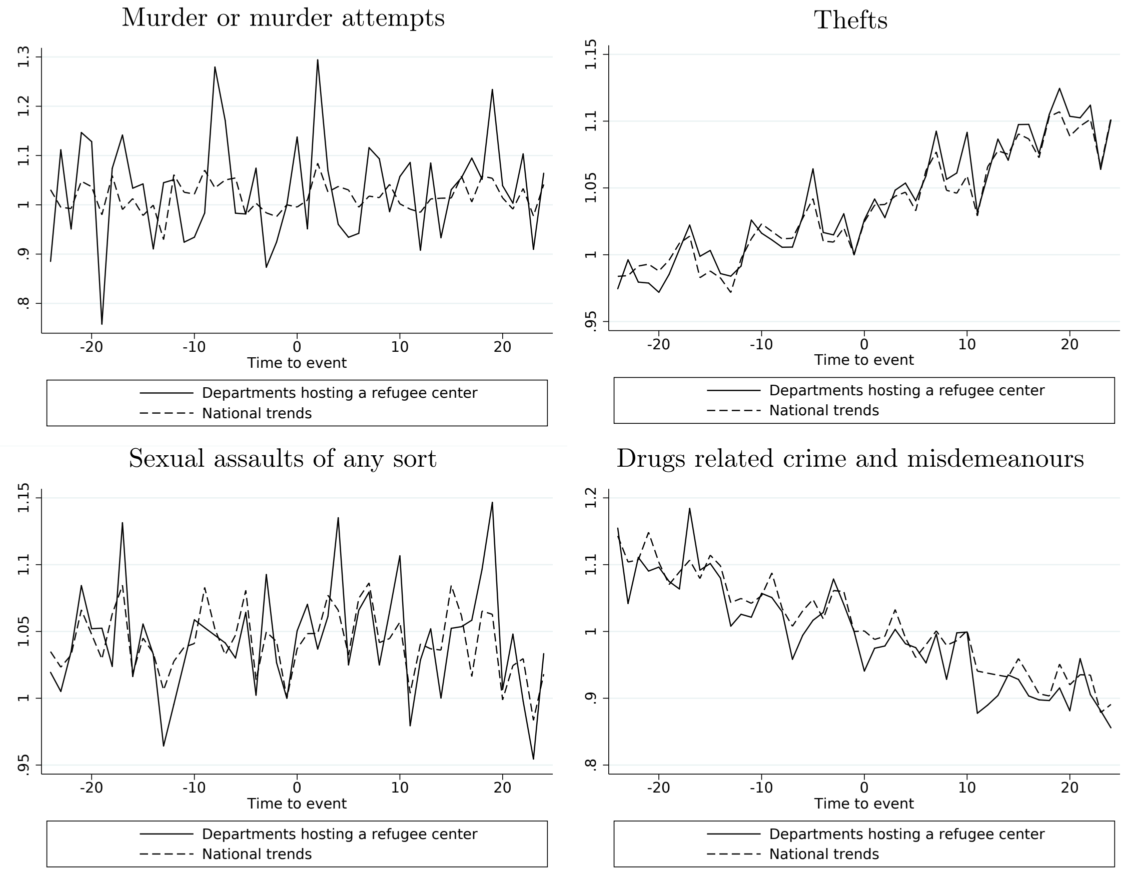
FIGURE 4.8 – Dynamic effect of a refugee center opening on the number of non-resident workers



Source : DADS 2002-2014. Note : Estimated β_l from equation (2) where l (x-axis) is the number of months relatively to the opening of the refugee housing center for the log number of non-resident workers. The incertitude of each point is asserted thanks to a 95% confidence interval.

Back

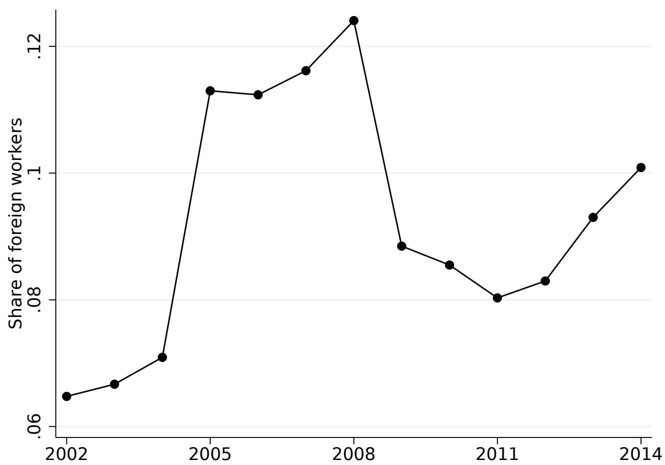
FIGURE 4.9 – Event analysis of the effect of a refugee center opening on crimes



Source : Ministry of the Interior 2002-2014. Note : Comparison of the evolution of different types of crimes or misdemeanours in departments that hosted a refugee center (solid line) and in other departments (dashed line). The number of crimes is normalized to one the month before the opening of the refugee center.

Back

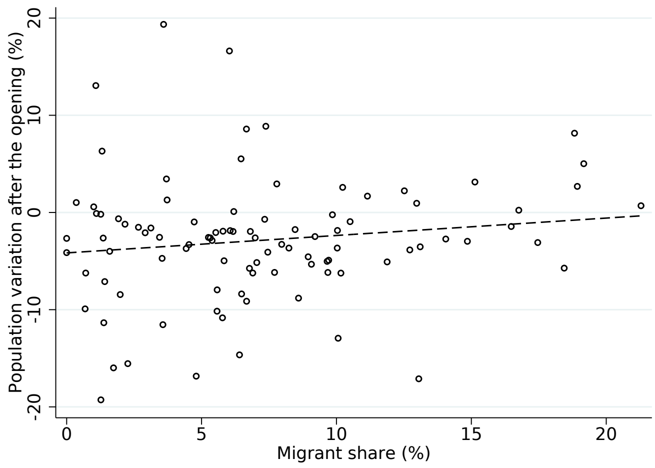
FIGURE 4.10 – Number of foreigners in the DADS



Source : DADS 2002-2014. Note : Share of workers identified as foreigners in the DADS from 2002 to 2014 in all municipalities.

Back

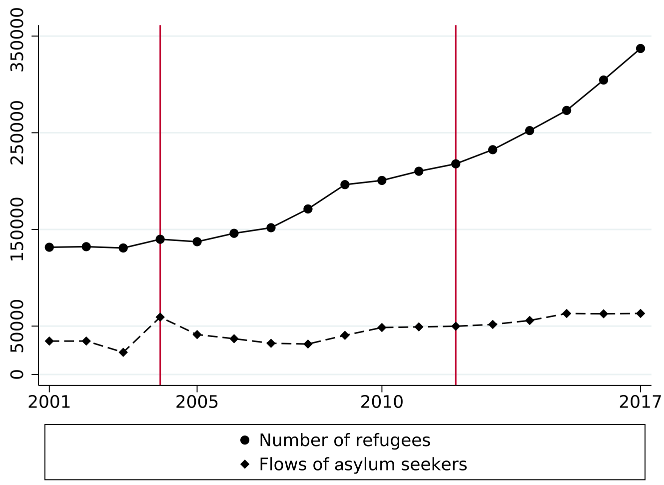
Figure 4.11 – Effects on the population and migrant shares



Source : DADS & Census 2002-2010. Note : The figure relates the migrant share two years before the opening with the municipality treatment effect. Reading : When the migrant share amounts to 5%, the population in the municipality decreases on average by 2%.

Back

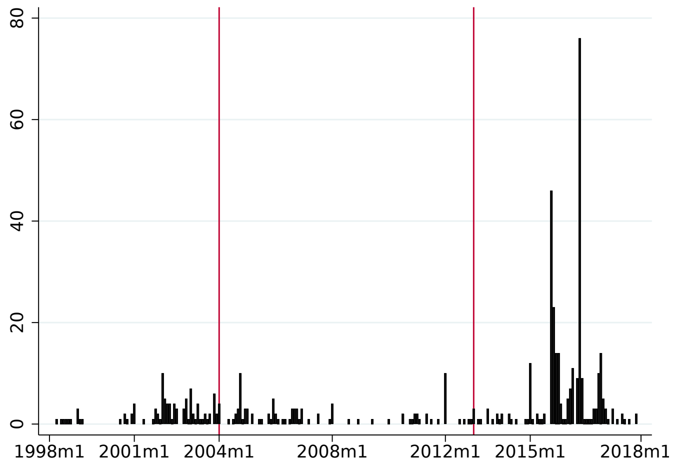
Figure 4.12 – Humanitarian migration between 2001 and 2017 in France



Source : OFPRA. Note : The figure presents the number of refugees (solid line) and asylum seekers (dashed line) on a yearly basis between 2001 and 2017.

Back

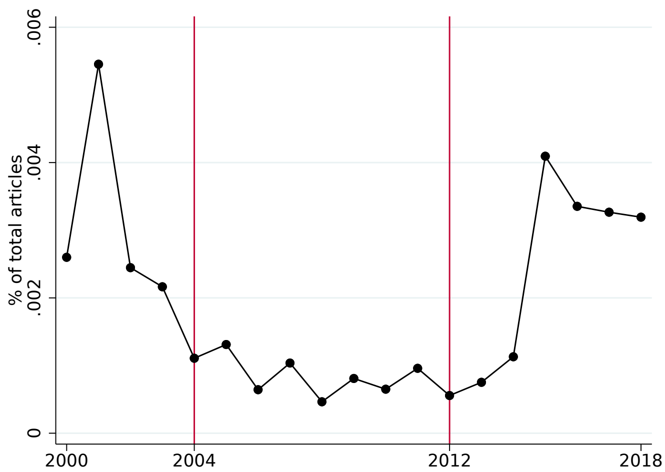
Figure 4.13 – Refugee center openings between 1998 and 2018 in France



Source : Authors' computations. Note : The figure represents the number of refugee center openings on a monthly basis between January 1998 and December 2018.

Back

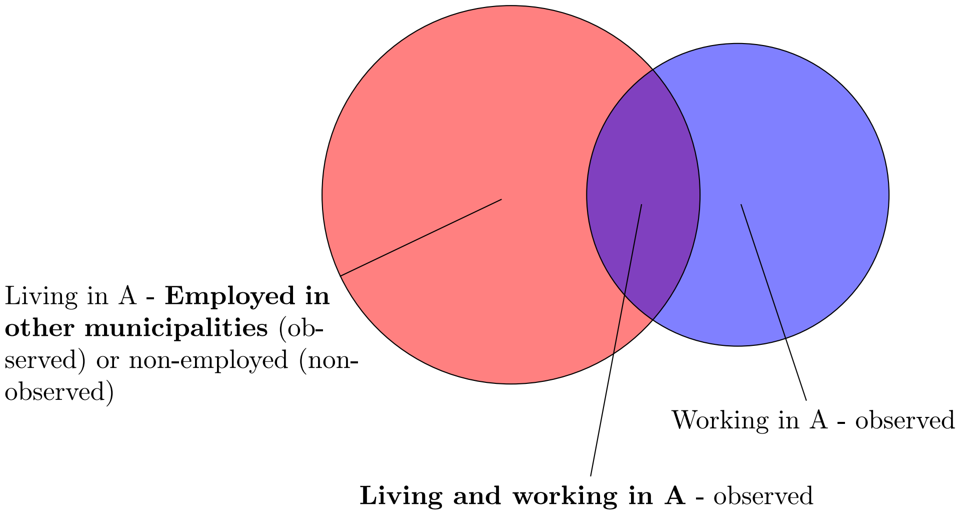
Figure 4.14 – Refugees and housing centers occurrences in French newspapers



Source : Europresse. Note : The figure represents the proportion of articles in the national and regional press that include the keywords "Centre d'hébergement" and "réfugié".

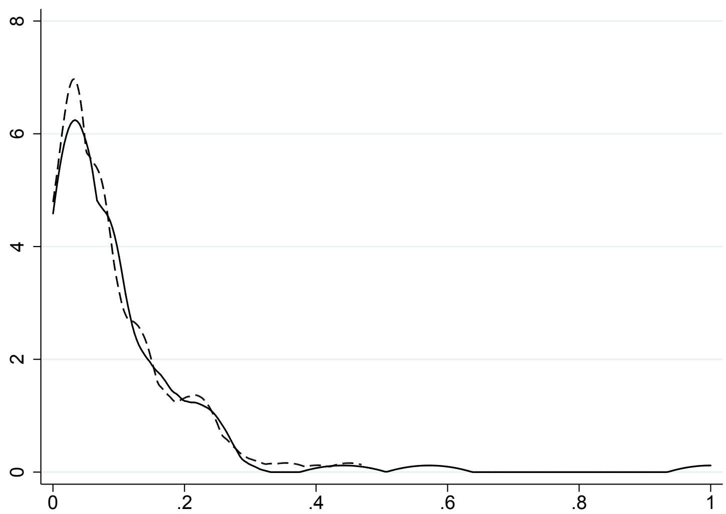
Back

FIGURE 4.15 – Different population definitions for a given municipality A



Back

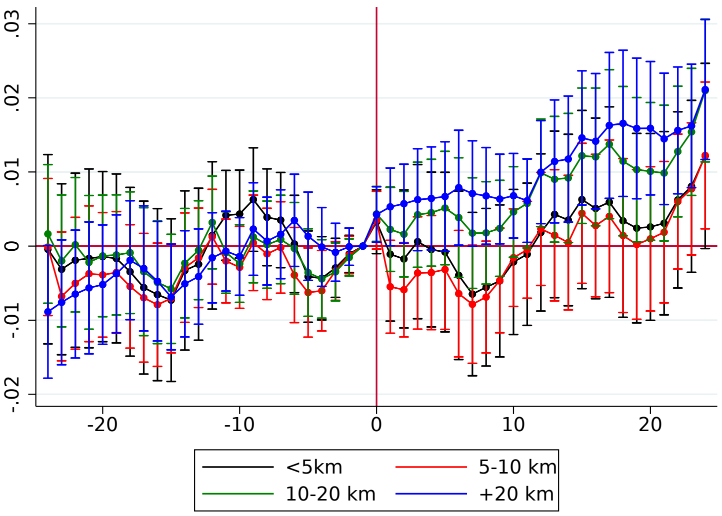
FIGURE 4.16 – Common support of the matching specification



Source : Census 2002. Note : The solid line represents the distribution of the propensity score when a matching algorithm with five nearest neighbors is used with the 2002 census variables. The dashed line shows the distribution of the propensity score for municipalities selected in the control group.

Back

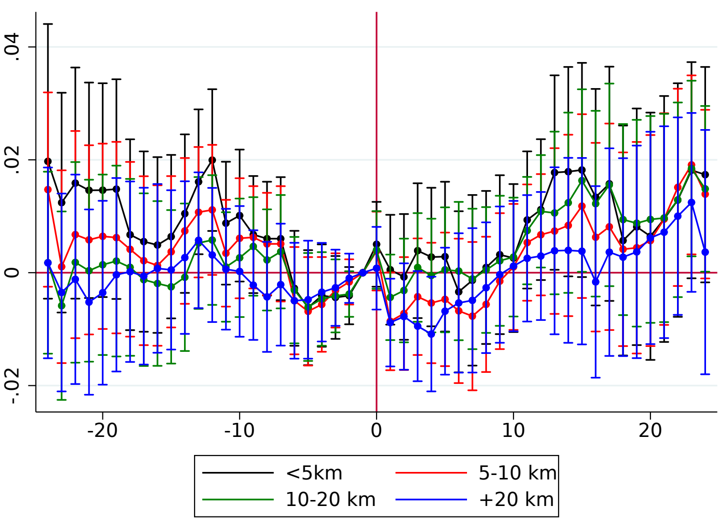
Figure 4.17 – Spillover effects of a refugee center opening on the resident employed population



Source : DADS 2002-2014. Note : Estimated ν_{ilg} from equation (2) where l (x-axis) is the number of months relatively to the opening of the refugee housing center for the *log* number of employed residents. The incertitude of each point is asserted thanks to 95% confidence interval.

Back

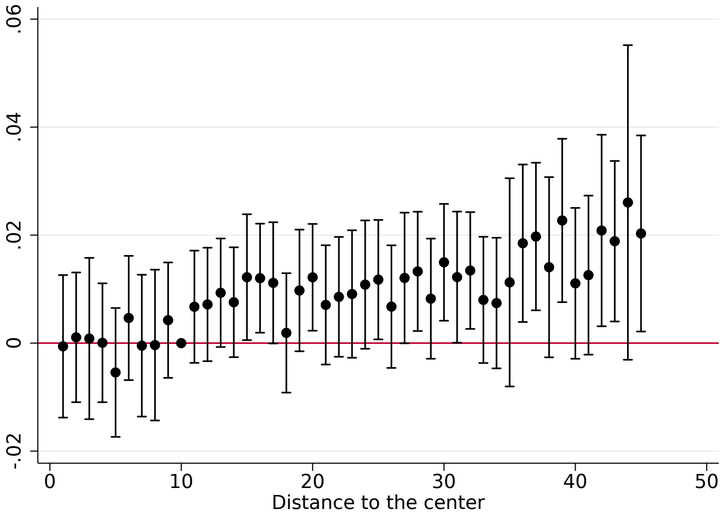
Figure 4.18 – Spillover effects of a refugee center opening on the number of workers



Source : DADS 2002-2014. Note : Estimated ν_{ilg} from equation (2) where l (x-axis) is the number of months relatively to the opening of the refugee housing center for the *log* number of workers. The incertitude of each point is asserted thanks to 95% confidence interval.

Back

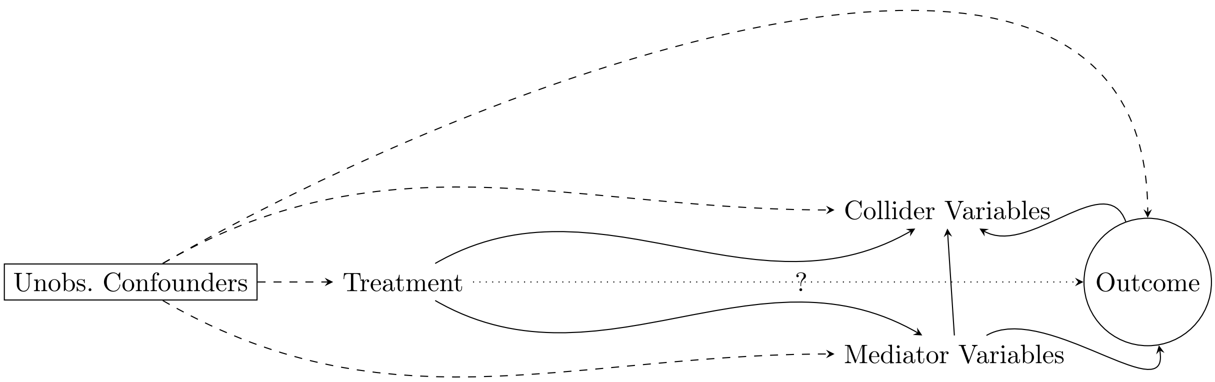
FIGURE 4.19 – Effect of the distance of non-hosting municipalities to the refugee center



Source : DADS 2002-2014. Note : Estimated $d_{ig} \times Opening_{it}$ from Equation (1) when taking $d_{ig} = 10$ as a base level. Reading : The number of employed residents has increased by 2 percent more in municipalities 45 kilometers away from the refugee center compared to municipalities 10 kilometers away.

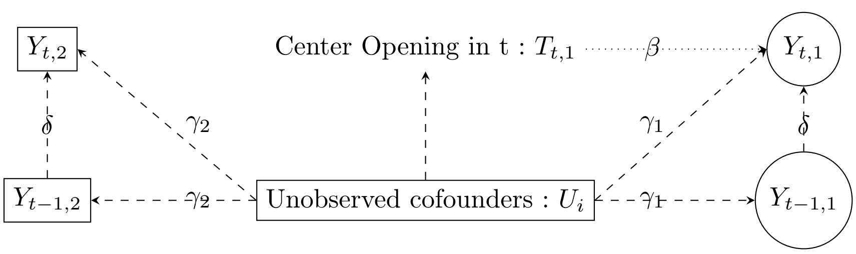
Back

FIGURE 4.20 – Identification problem – A Classical Problem – DAG



Back

FIGURE 4.21 – Identification problem - Two-way fixed effects estimator – DAG



Back