



# Essays on Firms and Public Policies

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# **Essays on Firms and Public Policies**

A dissertation presented

by

**Matteo Paradisi**

to

The Department of Economics

in partial fulfillment of the requirements

for the degree of

Doctor of Philosophy

in the subject of

Economics

Harvard University

Cambridge, Massachusetts

May 2019

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## **Essays on Firms and Public Policies**

### **Abstract**

This dissertation is composed of three essays on firms and public policies. The first essay investigates the role substitutability among age cohorts within the firm for the consequences of an increase in the full retirement age, and for other policies that lower the turnover of older workers. The second essay examines the redistribution of labor costs within the firm. The third essay investigates the role of firms for the passthrough of the effects of public policies.

In the first essay, which is joint work with Giulia Bovini, we study the effects of an increase in the full retirement age on labor demand, incumbent workers, and on the revenues raised on incumbent employees through this policy. Using administrative data, we first document that older workers delaying retirement and younger co-workers are substitutes. We then show that labor demand spillovers cause large leakages in the revenues raised on a firm's employees through the pension reform. We conclude that firm's behavior and labor substitutability have important implications for the impact of policies that lower the turnover of older workers.

In the second essay, I ask how labor costs are redistributed within firms. I use administrative data for the universe of French private sector companies to study a payroll tax reform implemented in France in 2003. The policy changed payroll tax subsidies for low-paid workers leaving tax rates unchanged for other workers. I show that tax cuts affect both low-paid and high-paid employees by increasing their net wages. This redistribution reduces the effectiveness of the policy in transferring resources to low-paid workers.

In the third essay, I study the role of firms for the consequences of public policies. I develop a framework to incorporate firms' responses to public policies in welfare analysis. The model rationalizes the use of firm-level causal estimates of the effects of a policy to study its welfare impact. I apply the model to existing estimates in the literature on the effects of policies. The new approach leads to significantly different conclusions relative to standard models of welfare.

# Contents

|                                                                                     |          |
|-------------------------------------------------------------------------------------|----------|
| Abstract . . . . .                                                                  | iii      |
| Acknowledgments . . . . .                                                           | x        |
| <b>Introduction</b>                                                                 | <b>1</b> |
| <b>1 Labor Substitutability and the Impact of Raising the Retirement Age</b>        | <b>4</b> |
| 1.1 Introduction . . . . .                                                          | 4        |
| 1.2 Institutional Setting . . . . .                                                 | 12       |
| 1.2.1 The Italian pension system . . . . .                                          | 12       |
| 1.2.2 Statutory and actual retirement age . . . . .                                 | 13       |
| 1.2.3 The Fornero reform . . . . .                                                  | 13       |
| 1.3 Data . . . . .                                                                  | 16       |
| 1.4 Empirical Strategy . . . . .                                                    | 18       |
| 1.4.1 Individual shift in the full retirement date . . . . .                        | 19       |
| 1.4.2 Firm-level shift in the full retirement date . . . . .                        | 20       |
| 1.4.3 Empirical Specification and identifying assumptions . . . . .                 | 22       |
| 1.4.4 Sample and Descriptive Statistics . . . . .                                   | 25       |
| 1.5 Older workers delay retirement after the reform . . . . .                       | 26       |
| 1.6 Labor demand responses to the reform . . . . .                                  | 28       |
| 1.6.1 Layoffs . . . . .                                                             | 29       |
| 1.6.2 New Hires . . . . .                                                           | 30       |
| 1.6.3 Which <i>potential retirees</i> matter more? . . . . .                        | 33       |
| 1.6.4 Response heterogeneity by occupation and turnover . . . . .                   | 35       |
| 1.7 Sensitivity checks and Discussion . . . . .                                     | 36       |
| 1.8 Workers' earnings and take-up of other Social Security programs . . . . .       | 38       |
| 1.8.1 Earnings of Incumbent Employees . . . . .                                     | 39       |
| 1.8.2 Spillovers to Government Programs . . . . .                                   | 42       |
| 1.9 Implications of substitutability for the revenues raised on employees . . . . . | 42       |
| 1.9.1 An accounting model . . . . .                                                 | 44       |
| 1.9.2 Empirical implementation and results . . . . .                                | 45       |
| 1.9.3 Substitutability and welfare in the short-run . . . . .                       | 48       |

|          |                                                                                |            |
|----------|--------------------------------------------------------------------------------|------------|
| 1.10     | Conclusions . . . . .                                                          | 49         |
| <b>2</b> | <b>Firm-level Tax Incidence: Redistribution of Labor Costs Within the Firm</b> | <b>51</b>  |
| 2.1      | Introduction . . . . .                                                         | 51         |
| 2.2      | Conceptual Framework . . . . .                                                 | 56         |
| 2.3      | Quasi-natural experiment: the Fillon's social security reform . . . . .        | 59         |
| 2.4      | Data . . . . .                                                                 | 61         |
| 2.4.1    | The DADS matched employer-employee and panel data . . . . .                    | 61         |
| 2.4.2    | FICUS/FARE balance sheet data . . . . .                                        | 62         |
| 2.4.3    | ACOSS social security data . . . . .                                           | 63         |
| 2.5      | Empirical Strategy . . . . .                                                   | 63         |
| 2.5.1    | Empirical framework . . . . .                                                  | 63         |
| 2.5.2    | Reform-induced changes to labor costs . . . . .                                | 67         |
| 2.5.3    | Instrumenting the total change in labor costs . . . . .                        | 68         |
| 2.5.4    | Empirical specification and sample definition . . . . .                        | 68         |
| 2.6      | Wage tax incidence . . . . .                                                   | 70         |
| 2.6.1    | The reduction in labor costs caused by the Fillon's reform . . . . .           | 70         |
| 2.6.2    | Incumbent workers' wages respond to changes in taxes . . . . .                 | 72         |
| 2.7      | Discussion and conclusions . . . . .                                           | 76         |
| <b>3</b> | <b>Firms and Policy Incidence</b>                                              | <b>78</b>  |
| 3.1      | Introduction . . . . .                                                         | 78         |
| 3.2      | The Model . . . . .                                                            | 80         |
| 3.2.1    | Policy, welfare and the marginal willingness to pay for a policy change        | 83         |
| 3.2.2    | The value of employment . . . . .                                              | 85         |
| 3.2.3    | Firm-level policy responses . . . . .                                          | 87         |
| 3.2.4    | Revisiting the Marginal Value of Public Funds . . . . .                        | 88         |
| 3.3      | Discussion and Extensions . . . . .                                            | 90         |
| 3.4      | Applications . . . . .                                                         | 91         |
| 3.4.1    | Payroll tax subsidy . . . . .                                                  | 91         |
| 3.4.2    | Raise in full retirement age . . . . .                                         | 94         |
| 3.4.3    | Cut in top income tax rates . . . . .                                          | 97         |
| 3.4.4    | Changes in corporate taxes . . . . .                                           | 99         |
| 3.5      | Conclusions . . . . .                                                          | 100        |
|          | <b>References</b>                                                              | <b>102</b> |

|                   |                                                             |            |
|-------------------|-------------------------------------------------------------|------------|
| <b>Appendix A</b> | <b>Appendix to Chapter 1</b>                                | <b>109</b> |
| A.1               | The Italian labor Market . . . . .                          | 109        |
| A.2               | Additional Details about the Fornero reform . . . . .       | 111        |
| A.3               | Procedure to clean matched employer-employee data . . . . . | 113        |
| A.4               | Computation of years of qualifying contribution . . . . .   | 114        |
| A.5               | Conceptual Framework . . . . .                              | 115        |
| A.5.1             | Labor Demand Model . . . . .                                | 115        |
| A.5.2             | Alternative Wage Formation Models . . . . .                 | 118        |
| A.6               | Matching procedure and the cost of separations . . . . .    | 122        |
| A.7               | The Fiscal Externality . . . . .                            | 124        |
| A.7.1             | Derivation of The Fiscal Externality . . . . .              | 124        |
| A.7.2             | Empirical implementation and results . . . . .              | 125        |
| A.8               | Supplementary Figures and Tables . . . . .                  | 126        |
| <b>Appendix B</b> | <b>Appendix to Chapter 2</b>                                | <b>148</b> |
| B.1               | Model - Frictionless case . . . . .                         | 148        |
| B.2               | Supplementary Figures . . . . .                             | 149        |
| <b>Appendix C</b> | <b>Appendix to Chapter 3</b>                                | <b>151</b> |
| C.1               | Derivation of the marginal willingness to pay . . . . .     | 151        |
| C.2               | Supplementary Tables . . . . .                              | 153        |

## List of Tables

|      |                                                                                                      |     |
|------|------------------------------------------------------------------------------------------------------|-----|
| 1.1  | Pre- and post-reform pension requirements . . . . .                                                  | 14  |
| 1.2  | Response of retirement choices to the change in retirement age . . . . .                             | 28  |
| 1.3  | The effect of the reform on layoffs and new hires . . . . .                                          | 34  |
| 1.4  | Fiscal Externality . . . . .                                                                         | 47  |
| 2.1  | Fillon's Reform Rules . . . . .                                                                      | 60  |
| 2.2  | Descriptive Statistics . . . . .                                                                     | 66  |
| 2.3  | The Effect of the Fillon's Reform on Wages . . . . .                                                 | 76  |
| 3.1  | Marginal Value of Public Funds for Different Policies . . . . .                                      | 97  |
| A.1  | Pre and post-reform pension requirements - Additional Details . . . . .                              | 137 |
| A.2  | Placebo Tests . . . . .                                                                              | 138 |
| A.3  | Relation between the firm-level shift in the full retirement date and firm's<br>covariates . . . . . | 139 |
| A.4  | Firms with at least one <i>potential retiree</i> and other firms . . . . .                           | 140 |
| A.5  | <i>Potential retirees</i> and other workers . . . . .                                                | 141 |
| A.6  | Absences from work for employees closer and further away from retirement                             | 142 |
| A.7  | The effect of the reform on layoffs . . . . .                                                        | 143 |
| A.8  | The effect of the reform on hiring . . . . .                                                         | 144 |
| A.9  | Firms only respond to shock to <i>potential retirees</i> . . . . .                                   | 145 |
| A.10 | The effect of the reform on incumbents' labor earnings . . . . .                                     | 146 |
| A.11 | The effect of the reform on <i>potential retirees</i> and co-workers . . . . .                       | 147 |
| C.1  | Payroll tax subsidy (Saez <i>et al.</i> , 2019) - Calibrations . . . . .                             | 153 |
| C.2  | Raise in Full Retirement Age (Bovini and Paradisi, 2019) - Calibrations . . . .                      | 154 |
| C.3  | Decrease in top income tax rate (Labanca and Pozzoli, 2018) - Calibrations .                         | 155 |
| C.4  | Change in Corporate Tax Rate (Fuest <i>et al.</i> , 2018) - Calibrations . . . . .                   | 155 |

## List of Figures

|      |                                                                                |     |
|------|--------------------------------------------------------------------------------|-----|
| 1.1  | Shift in full retirement date: distribution . . . . .                          | 21  |
| 1.2  | The reform prolongs the working life of <i>potential retirees</i> . . . . .    | 27  |
| 1.3  | Layoffs . . . . .                                                              | 31  |
| 1.4  | New Hires . . . . .                                                            | 32  |
| 1.5  | Incumbents' labor earnings . . . . .                                           | 40  |
| 1.6  | The effect of the reform on <i>potential retirees</i> and co-workers . . . . . | 43  |
|      |                                                                                |     |
| 2.1  | Reduced Social Security Contributions - Pre and Post Fillon's Reform . . . . . | 61  |
| 2.2  | Average Social Security Contributions - Treatment vs Control . . . . .         | 71  |
| 2.3  | Change in Total Social Security Contributions . . . . .                        | 72  |
| 2.4  | Average Net Wages - Treatment vs Control . . . . .                             | 73  |
| 2.5  | The Effects of Payroll Taxes on Net Wages . . . . .                            | 74  |
| 2.6  | The Effects of Payroll Taxes on Net Wages . . . . .                            | 75  |
|      |                                                                                |     |
| A.1  | Share of workers retiring at FRA . . . . .                                     | 126 |
| A.2  | Age of new retirees by gender and type of pension . . . . .                    | 127 |
| A.3  | Number of new retirees by gender and type of pension . . . . .                 | 128 |
| A.4  | Reform-induced changes in the retirement age . . . . .                         | 129 |
| A.5  | Post-reform retirement date - Forecast quality assessment . . . . .            | 130 |
| A.6  | Share of retained employees and trends in pre-reform labor demand . . . . .    | 131 |
| A.7  | Firms mostly respond to shock on <i>potential retirees</i> . . . . .           | 132 |
| A.8  | Labor demand within and across occupations . . . . .                           | 133 |
| A.9  | Layoffs in high and low-turnover firms . . . . .                               | 134 |
| A.10 | Robustness to alternative specifications and samples . . . . .                 | 135 |
| A.11 | Cost of Separations . . . . .                                                  | 136 |
|      |                                                                                |     |
| B.1  | Working Hours Distribution . . . . .                                           | 149 |
| B.2  | Gross Wages - Distribution . . . . .                                           | 149 |
| B.3  | Gross Wages - Pre-reform Trends . . . . .                                      | 150 |

## Acknowledgments

I am indebted to my committee members, colleagues, friends, and family.

My amazing group of advisors – Stefanie Stantcheva, Nathan Hendren, and Larry Katz – provided me with constant support, ideas, and invaluable suggestions. I have been really fortunate to have them on my side throughout the past five years.

My research also benefited greatly from the constant interactions with faculty members in the Economics department and in other Universities, including Philippe Aghion, Ed Glaeser, Luigi Guiso, David Laibson, John Friedman, Amanda Pallais. Special thanks to my coauthors Alberto Alesina, Omar Barbiero, Nicola Bianchi, Giulia Bovini, Enrico Di Gregorio, Francesco Giavazzi, Carlo Favero, Jin Li, and Mike Powell.

My research, and more in general my life in Cambridge, have benefited from the presence of many friends: special thanks to Edoardo Acabbi, Enrico Di Gregorio, Luca Maini, Armando Miano, Andrea Passalacqua, Edoardo Teso, and Hannah Shaffer.

My biggest thanks go to my family, and in particular to my parents, Vanna Rovi and Gabriele Paradisi, my brother, Tommaso Paradisi, and my grandmother, Adriana Bertini, for giving me their continuous and unconditional support, without which none of this would have been possible.

Finally, Federica Esu has shared the happy and stressful moments with me during the past five years, and her love contributed in a unique way to make this a successful journey.

Ai miei genitori

# Introduction

This dissertation is composed of three essays on firms and public policies. The first essay studies the role of labor substitutability among ages for policies that lower the turnover of older employees. It investigates the effects of an increase in the full retirement age on labor demand, incumbent employees, and on the revenues collected on a firm's employees. The second essay examines the redistribution of labor costs within the firm, with a focus on policies that transfer resources to low-paid workers. The third essay investigates the role of firms for the passthrough of the effects of public policies and for their welfare impact.

In the first essay, which is joint work with Giulia Bovini, we study how the substitutability among different types of workers may affect the impact of public policies by creating labor demand spillovers within firms. We study the substitutability between age cohorts in the context of an unexpected public pension reform increasing the full retirement age in Italy starting in 2012. An increase of the full retirement age creates incentives for older workers to delay retirement and temporarily increases their retention at the employer firm. We investigate how retaining an additional older worker affects employer labor demand for other workers. Using a large and rich matched employer-employee administrative data set for Italy, we exploit the substantial idiosyncratic firm-level variation in the impact of the unanticipated pension reform persisting even after conditioning on the demographics at the firm. We find that workers on the cusp of retirement and younger co-workers are substitutes. Older workers in the same occupation group are the closest substitute to the retained older workers who delay retirement. Extending the retirement age also decreases labor earnings and increases the take-up of social security programs of incumbent workers. Patterns of

labor-labor substitutability have important implications for the incidence of public policies that increase retention of older workers like this reform. We provide supporting evidence using a government accounting model that allows for spillovers across government programs and workers. Our results show that labor demand spillovers cause large leakages in the revenues raised on a firm's employees through the pension reform.

In the second essay, I study how the cost of payroll taxes is redistributed within the firm. Standard tax incidence theory is based on the assumption that labor markets are perfectly competitive and firm-level shocks to the cost of labor have no effects on the wages of incumbent workers. Identification techniques exploit individual-level variation in taxes to balance unobservable individual characteristics and by construction rule out firm idiosyncratic characteristics. As a result, the estimated elasticity misses an important channel of the pass-through of taxes to wages. I attempt to identify this component by studying how wages respond to firm-level changes in the cost of labor. I use administrative data for the universe of French private sector firms that I match to information on paid social security contributions to study a payroll tax reform implemented in France in 2003. The policy changed payroll tax subsidies for low-paid workers leaving tax rates unchanged for other workers. I exploit variation in the wage distribution of the subset of low-paid workers to create firm-level shifts in the average payroll tax rate of low-paid employees and I use them as an instrument for the total change in the cost of labor. I show that tax cuts affect both low-paid and high-paid employees by increasing their net wages. This redistribution reduces the effectiveness of the policy in transferring resources to low-paid workers.

In the third essay, I study the role of firms for the consequences of public policies. I develop a framework to incorporate firms' responses to public policies in welfare analysis. Building on the existing sufficient statistics frameworks, I allow for involuntary unemployment caused by a firm's employment decisions, and heterogeneous prices and quantities across firms. The model rationalizes the use of firm-level causal estimates of the effects of a policy to study its welfare impact. I apply the model to existing estimates in the literature on policies that change (i) payroll taxes, (ii) full retirement age, and (iii) personal income

taxes. The new approach leads to significantly different conclusions relative to standard models of welfare.

# Chapter 1

## Labor Substitutability and the Impact of Raising the Retirement Age<sup>1</sup>

### 1.1 Introduction

Substitutability among workers within the firm is crucial for the study of public policies. When a fraction of workers are targeted by a policy that alters incentives to work, spillovers on their co-workers may arise depending on whether the employer finds close substitutes or complements among incumbent employees. These “hidden” micro-level effects may have welfare implications that are hard to detect when looking at targeted individuals. Labor substitutability may exist along several worker’s characteristics: an example is substitutability between age cohorts that can affect the incidence of Social Security policies targeting older employees. In this paper, we propose a novel approach that regards firms as active agents in the analysis of delayed retirement policies and extends to other policies that lower the turnover of older workers.

Raising the retirement age provides older employees with incentives to postpone retire-

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<sup>1</sup>Co-authored with Giulia Bovini. The views expressed here belong solely to the authors and do not necessarily reflect those of the Bank of Italy or INPS.

ment.<sup>2</sup> A rich literature documents the positive effects of these reforms on old-age labor supply (e.g. Mastrobuoni 2009; Staubli and Zweimüller 2013; Vestad 2013).<sup>3</sup> There is, on the other hand, limited evidence on how firms adjust labor demand when older employees work for longer than expected. Do firms change their hiring and firing policies? Do they modify the wages of incumbent employees? Do labor demand adjustments uniformly affect all cohorts of workers or disproportionately some of them? Answering these questions will shed light on the degree of substitutability among workers of different ages within the firm. The extent to which younger and older employees are substitutes has important implications for the unintended consequences of delayed retirement policies as well as of other policies that similarly reduce the turnover of older workers. Therefore, it is primarily important to assess firm responses to changes in the design of Social Security. In addition, it is crucial to establish whether and how labor substitutability within the firm may affect the revenues generated by an increase in the statutory retirement age, a key component of welfare.

To address these questions, we exploit the quasi-experimental variation of a unique pension reform implemented in Italy in 2012 that caused sudden, substantial and heterogeneous changes in the full retirement age, *i.e.* the age at which workers can claim full pension benefits.<sup>4</sup> We link Italian matched employer-employee records for small and medium firms to novel records (*estratti conto contributivi*) that track all contributions to Social Security made by their workers (more than 6 million of individuals, over one third of private employees in 2009). Leveraging this novel match of different data sources, we can build firm-level measures of the reform-induced shock to the retirement date of older employees. Furthermore, we can observe the entire working career of all workers and their participation

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<sup>2</sup>Many governments have passed reforms that increase the retirement age to cope with population aging and its threat to the sustainability of social security systems. The U.S. is following such trend by committing to gradually adjust the full retirement age from 66 to 67 by 2022. The Congressional Budget Office has suggested further raising the statutory age to 70 to help reducing the budget deficit between 2017 and 2026 (CBO, 2016). Most European countries have implemented similar measures since 2000 (Carone *et al.*, 2016).

<sup>3</sup>This literature documents bunching in retirement at statutory retirement ages and large responses of retirement choices to retirement ages. Other papers on the effects of these reforms on retirement behavior are Behaghel and Blau (2012), Cribb *et al.* (2016), Manoli and Weber (2016), Seibold (2017), Lalive *et al.* (2017).

<sup>4</sup>Full retirement age is intended throughout the paper as the age at which workers can claim full pension benefits as opposed to the early retirement date where a different - typically less generous - benefit rule applies.

to various Social Security programs.

We start by investigating whether and how employers change their demand for labor in response to the reform. Our results reveal the degree of substitutability between workers of different ages. We then document that adjustments in labor demand affect the co-workers of retained older employees, impacting their earnings trajectories and their take-up of Social Security programs. We conclude by discussing the implications of micro-level substitutability for the revenues collected due to the reform from a firm's employees in the short-run. To this end, we incorporate the demand-driven behavioral responses into the estimation of the fiscal externality of the policy (the leakages on the revenues that the government hopes to raise on a firm's employees), improving upon the existing literature that focuses only on the behavioral responses of older workers. This exercise represents a first step to evaluate the importance of labor substitutability within the analysis of Social Security policies.

Estimating labor demand responses to reforms that increase the full retirement age poses two main identification challenges. First, most pension reforms are anticipated. Confounding anticipation effects make it hard to isolate firms responses. Second, to shed light on workers substitutability within the firm, we aim to measure the causal effect of retaining an additional older worker on the demand for co-workers and new hires. However, the number of older employees retained at any given employer per effect of the policy depends on the workforce's age distribution, which varies across firms possibly due to differences in labor demand trends and other unobservable, time-varying characteristics. Hence, firms with a high concentration of younger workers, which are not affected much by the reform, may not serve as credible controls.

The features of the 2011 Italian pension reform (the *Fornero* reform) allow us to address both identification issues. The reform was enacted by a newly appointed technocratic government in December 2011 and it entered into effect in January 2012, leaving limited room for anticipatory effects. The new law raised the age and contribution requirements for old-age and seniority pensions. The design of the policy generated heterogeneous changes in years until retirement eligibility across otherwise similar older workers. Specifically, these

changes depend on small differences in the ingredients that determine the full retirement age, *i.e.* age, gender, and years of retirement contributions. As a result, firms with a similar workforce composition underwent differential shifts of the retirement date of their senior employees.

We leverage this feature of the reform to solve the endogeneity problem. First, we restrict our attention to the subset of workers on the cusp of retirement at the time of the reform and we define as retained those who delay their retirement by at least one year. Then, we instrument the number of retained workers in a firm with the average firm-level shift in their retirement date. We show that the instrument is weakly related to the demographics of the firm' workforce: this is because it leverages small idiosyncratic differences across firms in the age, retirement contributions and gender within the narrow subset of employees on the cusp of retirement. Importantly, the instrument does not predict differences in labor demand trends in the pre-reform period. Furthermore, it has a direct economic interpretation as a firm-level shift in the policy parameter, *i.e.* the retirement date of older workers. For this reason, in the second part of the paper we use it as the treatment variable to study the short-run effect of raising the full retirement age on the revenues collected from a firm's employees.

To conduct our analysis, we estimate a difference-in-differences model with a continuous treatment over the period 2009-2015. In the first part of the paper, we compare the labor demand of differentially treated firms before and after the reform. We look at two main margins: layoffs of incumbent workers and external hiring. Our results document that older employees who delay retirement and their co-workers are substitutes. More strongly treated firms in fact fire more permanent employees in the post-reform period. One additional retained worker causes 0.17 more layoffs, 44 percent of the average number of layoffs pre-reform. Layoffs do not only involve older employees who were expected to retire soon. Young (under age 35), middle-aged (aged 35-55) and other older (aged above 55) workers are also fired, causing spillovers within the firm. In particular, older incumbent workers are fired more than young employees, indicating a closer substitutability with older workers who

were expected to retire soon. Hiring is reduced by 0.35 units (7 percent of the pre-reform average). Its decline is largely explained by drops in new hires of middle-aged and young workers. The effect on dismissals and hiring is concentrated on incumbent workers or external hires who share the same qualification (blue-collar, white-collar or manager) as older retained employees. We conclude that the closest substitutes to senior workers who defer their retirement date are older incumbent workers in the same occupation group.

In the second part of the paper, we study how firm's adjustments affect workers on the cusp of retirement and younger co-workers. We look at labor earnings and take-up of social insurance programs. We associate each worker to the firm where she worked at the reform date. Then, we aggregate the outcomes of interest across co-workers who shared incumbency at the same employer and we replicate the same aggregation procedure across fellow incumbent employees close to retirement. We find that incumbent co-workers in more heavily treated firms exhibit lower earnings in the post-reform period. A 1.33 years shift of the full retirement date (one standard deviation) leads to a 13,379 euros drop in total labor earnings, equivalent to 2.2 percent of the average total earnings in the pre-reform period. The decline in earnings moderates when we take into account non-work subsidies. Hence, part of the observed dynamics reflect the effect of the increased layoff risk. To quantify which share of the earnings decline can be attributed to involuntary separations, we combine estimates of the cost of job losses, obtained via a procedure that matches similar individuals who did and did not lose their job, with the estimates of the effect of the reform on separations. We find that separations explain around one-fifth of the earnings drop. The remainder depends on within-firm earnings dynamics, which matter more for middle-aged workers for whom 80 percent of the earnings loss is explained by wage patterns within the firm. About 55 percent (70 percent in the first two years) of the young workers' earnings loss depends on within firm dynamics. This evidence is consistent with a model where the firm job ladder is based on seniority and middle-aged employees are more substitutable to retained older employees than younger co-workers.

We show that total Social Security transfers to all incumbent workers increase after the

reform. Non-work subsidies explain large part of this increase and are caused by layoffs. Moreover, older workers who were eligible to retire soon under pre-reform rules are more likely to receive disability pensions and sick leave benefits after the reform, while their co-workers experience milder increases in the take-up of these programs. Focusing only on older workers on the cusp of retirement, savings on pension entitlements are far larger than the costs generated by their increased take-up of other social insurance programs. Yet, a more comprehensive evaluation requires to take into considerations the consequences on younger co-workers.

For this purpose, we evaluate the implications of labor substitutability for the reform incidence by estimating the revenues collected from a firm's employees in the short-run. Previous literature has exclusively focused on the behavioral responses of older workers (Staubli and Zweimüller 2013; Vestad 2013). We show that spillovers on their co-workers - caused by labor substitutability - are important. We develop an accounting model that allows for spillovers on co-workers and substitution between government programs. We then estimate the fiscal externality of the policy, *i.e.* the share of mechanical savings on the pension payments to a firm's employees that the government loses because of the behavioral responses of the employees and of the firm. According to our estimates, savings on pension disbursements are larger than costs from extra outlays on social insurance programs and lower labor tax revenues. However, we find that around two-thirds of the savings are lost in the short-run. The cost is entirely explained by spillovers on the co-workers. By ignoring these spillovers, we would estimate a close to zero fiscal externality, indicating that no savings on pension outlays would be lost. We therefore conclude that labor substitutability is pivotal to assess the consequences of this reform and potentially similar policies that lower the turnover of older workers.

**Relation to previous literature:** Our paper relates to the literature that explores the substitutability between workers within the firm using firm responses to unforeseen shocks to their workforce. Jäger and Heining (2019) and Jaravel *et al.* (2017) exploit sudden workers'

deaths.<sup>5</sup> While these papers leverage a negative shock to the retention rate, we study a positive one. Unlike a worker's death, our treatment may involve more than one incumbent worker, providing a larger shock to a firm's workforce. Since our shock affects older employees, it allows to study substitutabilities between workers of different ages. We relate our findings to labor demand theory and contribute to the understanding of labor substitutability within firms as studied in models with heterogeneous labor and imperfect labor markets (Cahuc *et al.*, 2008 and Pissarides, 2000). We also add evidence on changes in internal labor market dynamics (Baker *et al.*, 1994).<sup>6</sup>

We study the implications of substitutability for the revenues generated by the reform relating to many studies that investigate how the generosity of one social insurance program affects enrollment in other programs.<sup>7</sup> Closely related to our work is Staubli and Zweimüller (2013) that examines a reform increasing the early retirement age. Like them, we show that changes to Social Security rules can generate spillovers on other government programs. However, we bring a new perspective to the analysis of policy incidence by treating the firm as an active agent for the transmission of the effects of the pension reform. To do so, we include in our model the demand-driven spillovers on incumbent workers who are not affected by the policy in the short-term.

We provide firm-level evidence of substitutability between age cohorts. Several papers have studied the relationship between young and older employment within macro-areas.<sup>8</sup> Gruber and Wise (2010) conclude that the correlation is positive looking at country case-studies. On the other hand, more recent work by Bertoni and Brunello (2017) that exploits variation in the age structure of Italian local labor markets argues that pension reforms

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<sup>5</sup>Other recent examples are Nguyen and Nielsen (2010), Bennedsen *et al.* (2010) and Adam (2015).

<sup>6</sup>Gibbons and Waldman (1999), Lazear and Oyer (2013), and Waldman (2013) survey the theoretical literature on internal labor markets.

<sup>7</sup>Some examples are works on the spillovers of changes in the disability insurance (Autor and Duggan (2003); Karlstrom *et al.* (2008); Borghans *et al.* (2010); Staubli (2011)) or unemployment insurance (Lammers *et al.* (2013)). Recent works along these lines are Inderbitzin *et al.* (2016) and Kline and Walters (2016). A similar work on early retirement provisions and the spillovers on other government programs is Vestad (2013).

<sup>8</sup>In a recent strand of literature, Acemoglu and Restrepo (2018) study the interaction of demography and automation showing that robots substitute for middle-aged workers.

causing fewer older workers to retire have negative effects on youth employment. Exploiting variation in the age structure of the older population across U.S. commuting zones, Mohnen (2019) similarly finds that the retirement slowdown has decreased youth employment. As we discuss later, our results can be regarded as an investigation of the micro-level mechanisms that deliver substitutability at a macro-level. A recent and limited literature has used micro-data to investigate how pension reforms that raise elderly labor force participation affect demand for new hires at the firm-level. Martins *et al.* (2009) study a Portuguese pension reform, while Boeri *et al.* (2017) evaluate the Italian *Fornero* pension reform. They detect a negative effect of pension reforms on new hires. Their identification relies on the strong assumption that firms with different demographics and gender compositions have parallel labor demand trends. Our new contribution is twofold. First, we rely on idiosyncratic variation in treatment intensity that is unrelated to broad firm demographics. Second, by extending the scope of the analysis to multiple firm's margins, we are able to carefully document age substitutability. We also study how the reform affects the take-up of social insurance programs, uncovering the spillovers of the policy on all incumbent workers and showing novel evidence on the importance of firms for the incidence of this type of policies.

Finally, our paper connects to the literature on workforce aging. Macro-level studies deliver mixed evidence on the effects of aging on firm performance. The complementarity between older and younger workers, wage setting mechanisms and country-specific labor market institutions play a crucial role.<sup>9</sup> Lallemand and Rycx (2009), Gobel and Zwick (2010) and Guest and Stewart (2011) provide evidence based on matched employer-employee data that a mixed aged workforce enhances productivity.<sup>10</sup>

The remainder of the paper proceeds as follows: Section 1.2 illustrates the institutional setting; Section 1.3 describes the data; Section 1.4 outlines the identification strategy; Section 1.5 shows that older workers delay retirement in response to the policy; Section 1.6 discusses

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<sup>9</sup>See Coile and Gruber (2007) for a review.

<sup>10</sup>Along these lines, Shimer (2001) shows that a larger share of youth in the working age population causes a reduction in the unemployment rate and a modest increase in the labor force participation rate.

the main findings on firm's labor demand adjustments; Section 1.7 presents a battery of robustness checks; Section 1.8 documents the effect of the reform on co-workers' earnings; Section 1.9 builds a model to estimate the revenues effects of the reform; Section 1.10 concludes.

## 1.2 Institutional Setting

We focus this section on the Italian pension system. We provide statistics and institutional details on the Italian labor market in Appendix A.1.

### 1.2.1 The Italian pension system

As for many OECD countries, including the U.S., the main pillar of the Italian pension system is a compulsory pay-as-you-go plan.<sup>11</sup> A combination of defined-benefit (DB) and notional defined-contribution (NDC) methods determines pension benefits. The Social Security tax rate for private payroll employment is 33 percent. Around one-third is levied on the employee, while the remaining two-thirds are paid by the employer.

There are two options to claim full retirement benefits: old-age pensions and seniority pensions. They both feature requirements on age and on years of contributions to Social Security. While the age requirement is higher for old-age pensions, the contribution requirement is heavier for seniority pensions. The main early retirement option, called *opzione donna*, is available for women only; it allows to claim benefits before meeting the old-age or seniority pension requirements. Similarly to early retirement in the U.S., before 2012 *opzione donna* allowed to claim benefits about 3 years before the statutory age.<sup>12</sup> Retiring early comes at the cost of receiving sizably lower pension benefits. The average cut is

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<sup>11</sup>Extra occupational pension plans are not widespread, since the public pension system was quite generous until the last decade. Only 7.3 million people (one-fourth of the workforce) had private pension plans in 2015. The number has been growing in the last years (COVIP, 2015).

<sup>12</sup>Early retirement using *opzione donna* was possible in 2011 upon turning 57 years old (with 35 years of contributions). In the same year, female employees could claim an old-age pension upon turning 60 years old (with 20 years of contributions).

estimated to be roughly 35 percent of full benefits (INPS, 2016, p. 111). Retirement is not mandatory and working past retirement is allowed. Unlike other European countries, there is no reduction in layoffs protection when a worker becomes eligible to retire.

### **1.2.2 Statutory and actual retirement age**

The relationship between the statutory retirement age and retirement choices determines the effects of pension reforms. Indeed, it regulates the extent to which workers delay retirement and firms experience an increase in the retention rate of older employees when the retirement age increases. Retirement spikes around the statutory retirement date in our data: more than 70 percent of individuals retire within a 1-year window around the full retirement date in 2012 (Figure A.1). This trend is common to other countries. In the U.S. the share of workers retiring at full retirement age has been increasing in the last decade; the share of early retirees has also starkly dropped (Munnell and Chen, 2015). Estimates in Mastrobuoni (2009) for the U.S. document a strong response of retirement choices to the full retirement date: an increase in the full retirement age by 2 months delays observed retirement by around 1 month.<sup>13</sup> A similar response emerges from our data: a one year shift in the full retirement date translates in an almost 7-month delay in retirement (see Section 1.5).

### **1.2.3 The Fornero reform**

The *Fornero* pension reform was passed in December 2011. It was part of the “Save Italy” decree, an emergency package of measures in response to the mounting pressure of financial markets on the Italian sovereign debt. Designed by a new technocratic government and approved three weeks after its appointment, it entered into force in January 2012. Although the need for a deficit reduction package was anticipated, its exact content was not known in advance. Moreover, the decision and implementation lags were both very short. As a result, anticipatory effects were likely negligible. The reform raised the requirements to

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<sup>13</sup>Mastrobuoni (2009) lists liquidity constraints as a determinant of retirement at the full retirement age.

claim old-age and seniority pensions, reducing the number of new retirees and increasing the average age at retirement.<sup>14</sup> The new rules applied to all workers who did not accrue the right to claim retirement under pre-reform rules by the end of 2011. Only few other categories of workers - listed in Appendix A.2 - were grandfathered. For all other private sector employees, Table 1.1 compares the main features of pre- and post-reform retirement rules over the period 2012-2015, for old-age (Panel A) and seniority (Panel B) pensions.<sup>15</sup>

**Table 1.1: Pre- and post-reform pension requirements**

| <b>Panel A: Old-age pension</b> |            |              |            |              |
|---------------------------------|------------|--------------|------------|--------------|
|                                 | Men        |              | Women      |              |
|                                 | Pre-reform | Post-reform  | Pre-reform | Post-reform  |
| 2011                            | 65YA       | Not in place | 60YA       | Not in place |
| 2012                            | 65YA       | 66YA         | 60YA       | 62YA         |
| 2013                            | 65YA+3MA   | 66YA+3MA     | 60YA+3MA   | 62YA+3MA     |
| 2014                            | 65YA+3MA   | 66YA+3MA     | 60YA+4MA   | 63YA+9MA     |
| 2015                            | 65YA+3MA   | 66YA+3MA     | 60YA+6MA   | 63YA+9MA     |

| <b>Panel B: Seniority pension</b> |              |                      |          |                       |
|-----------------------------------|--------------|----------------------|----------|-----------------------|
|                                   | Pre-reform   |                      |          | Post-reform           |
|                                   | Both genders |                      |          | Men      Women        |
| 2011                              | Quota 96     | (60YA and 35 YC)     | or 40 YC | Not in place          |
| 2012                              | Quota 96     | (60YA and 35 YC)     | or 40 YC | 42YC+1MC    41YC+1 MC |
| 2013                              | Quota 97.3   | (61YA+3MA and 35 YC) | or 40 YC | 42YC+5MC    41YC+5MC  |
| 2014                              | Quota 97.3   | (61YA+3MA and 35 YC) | or 40 YC | 42YC+6MC    41YC+6MC  |
| 2015                              | Quota 97.3   | (61YA+3MA and 35 YC) | or 40 YC | 42YC+6MC    41YC+6MC  |

*Note:* The table shows the pre- and post-reform requirements for old-age and seniority pensions, over the period 2012-2015. YA and MA flag the age requirement in terms of years and months, respectively. YC and MC flag the contribution requirement in terms of years and months, respectively. Additional details can be found in Table A.1.

**Old-age pensions:** The reform raised the age requirement for old-age pensions, whilst leaving the contribution requirement (20 years) unchanged. The statutory retirement age

<sup>14</sup>Figures A.2 and A.3 plot average age and retirement volumes by gender and retirement option. The participation rate in the 55-64 age group increased from 39.4 percent in 2011 to 57.0 percent in 2018, halving the gap with respect to the average in euro-area countries.

<sup>15</sup>Additional details and further features of the reform are provided in Table A.1 and Appendix A.2.

was 60 for women and 65 for men in 2011.<sup>16</sup> Per effect of the reform, the old-age statutory retirement age has gradually increased to reach 66 years and 7 months for both genders in 2018. Hence, the change in the age requirement was considerably larger for women than for men. However, due to gradual adjustments to the target of 66 years and 7 months, not all women faced the same 6 years change. Many female employees faced smaller extensions depending on their age in 2011 (Table A.1).

**Seniority pensions:** The reform re-designed the rules for claiming seniority pensions. A “quota” system was in place until 2011. Workers could retire as soon as their age and years of contributions summed to a “quota”, conditional on both surpassing a certain threshold. In 2011 the quota was set to 96, conditional on being at least 60 years old and having at least 35 years of contributions. Alternatively, workers could retire upon totalling 40 years of contributions, regardless of their age.<sup>17</sup> The *Fornero* reform abolished the “quota” system. It legislated that a seniority pension could be claimed upon totalling at least 41 years of contribution for women and 42 for men. Thus, workers planning to retire under the “quota system” faced a large increase in years until pension eligibility, up to 6-7 years.

The reform did not change the early retirement rules. The take-up of early retirement was very low before the reform because of the cut in benefits. After the reform, which heavily raised requirements for women, the take-up of *opzione donna* increased. Yet, even in the year when it peaked (2015), less than 20 percent of eligible women claimed early retirement. Moreover, only 80 percent of them made job-to-retirement transitions (INPS, 2016).<sup>18</sup> As a result, the take-up of *opzione donna* remains limited in our sample contributing to a high response of retirement behavior to the full retirement age.

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<sup>16</sup>Absent the reform, it would have risen to reach 61 years and 10 months for women and 65 years and 7 months for men.

<sup>17</sup>Had rules not changed, the “quota” would have risen from 96 to 97.3 and later to 97.6 over the 2012-2108 period. The age requirement for “quota” 97.3 would have been 61 years and 3 months, later increased to 61 years and 7 months when the “quota” was scheduled to raise to 97.6 with the contribution requirement unchanged.

<sup>18</sup>The remaining 20 percent were unemployed or out of the labor force when they retired.

The reform caused heterogeneous changes in years until retirement eligibility among otherwise similar older workers. As a result, firms with a similar older workforce are affected to a different extent by the reform. Figure A.4 shows the relationship between age and years of contributions in 2011 and the shift in the retirement age by gender. Among female workers, the most affected are those between 58 and 59 years old with less than 36 years of contributions in 2011. Their retirement age shifts by three years or more.<sup>19</sup> Smaller changes affect women with more than 37 years of contributions or closer to 60 years old. Among male workers, the ones close to eligibility under “quota 96” experience the largest change in the retirement age.<sup>20</sup> Milder changes affect male workers who were under 60 years old with 38 or more years of contributions.<sup>21</sup>

### 1.3 Data

We leverage high-quality and confidential administrative data available at the Italian Social Security Institute (INPS).<sup>22</sup> We describe below the three main sources that we combine to build the dataset used for the analysis.

**Workers’ contribution histories:** we have access to previously unexploited contribution histories for all employees (more than 6 millions) who worked in small-medium sized firms around the reform (*i.e.* between 2009 and 2015).<sup>23</sup> For every contribution spell in any given

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<sup>19</sup>They were close to retire claiming “quota 96”. After the reform, their earliest available retirement option becomes either “anticipated” pension (41 years of contributions) or old-age pension (62 or more years of age).

<sup>20</sup>After the reform, they must claim an old-age pension or an “anticipated” pension. Both options imply a significant shift in the retirement age.

<sup>21</sup>For these workers, the seniority pension is delayed by a couple of years when “quota 40” is replaced by the 42 years of contributions requirement of the “anticipated pension”.

<sup>22</sup>This study uses the anonymous data from the Italian Institute of Social Security (INPS). Data access was provided as part of the VISITINPS Initiative. We are very grateful to Massimo Antichi, Mariella Cozzolino, Edoardo Di Porto, Paolo Naticchioni, Vito Lamonica, Marcella Nunzi and Maria Domenica Carnevale for their invaluable help in making this project possible and to Elio Bellucci, Luca Cammarata and Massimo Ascione for their support with the data. Matteo Paradisi also thanks Robert Hannuna for his guidance.

<sup>23</sup>We consider firms with 3 to 200 employees in the first quarter of 2009. The restriction stems from limitations to the maximum number of workers’ contribution histories that could be made available by INPS for the sake of

year we observe the following information: the number of qualifying weeks contributed that determines whether workers meet the contribution requirement for old-age and seniority pensions; the event triggering the payment of contributions (e.g. paid work, maternity leave, sick leave, unemployment benefits) and their monetary value. We use this information to construct comprehensive measures of earnings, including labor income from quasi-salaried employment, self-employment and public sector jobs. We also observe the take-up of social insurance programs that we use to study the revenues consequences of the policy.

**Matched employer-employee records:** we also exploit linked employer-employee records available over the period 1983-2015 for the universe of non-agricultural firms with at least one employee. Firms report detailed information about employees covered by Social Security filling the so-called *Uniemens* modules. The data covers 74 percent of private employment in Italy and 93 percent of private sector employees.<sup>24</sup> We use monthly data for the period 2009-2015.<sup>25</sup> For its purposes, INPS classifies as a firm a unit provided with a unique Tax Identification Number (TIN). In case of a multi-establishment firm, all establishments feature the same TIN.

For each worker-firm record, the following information is available: beginning and end date of the contract, alongside the underlying motivation (e.g. layoff, quit); type of contract (permanent vs fixed-term, full-time vs part-time); broad occupation group (blue-collar, white-collar or manager); wage; number of days worked.<sup>26</sup> We link these records to workers' and firms' registers containing baseline information, such as gender and age of employees as well as opening date, sector and location of businesses. Drawing on this, we build yearly

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the project.

<sup>24</sup>Self-employment covers most of the share of total private employment that we are missing. The agricultural sector accounts for most of the missing share of private sector employees.

<sup>25</sup>INPS collects matched employer-employee records with an annual frequency since 1983 and with a monthly frequency since 2005. Since our analysis spans the period 2009-2015, we mostly use the latter dataset, relying on the former to compute worker-level measures of experience and tenure.

<sup>26</sup>We deal with multiple records for individual-firm-month combinations using the procedure in Appendix A.3.

firm-level measures of adjustments in labor demand measuring total new hires and layoffs of permanent workers. We also construct these measures for different categories of workers, as identified by their contract, occupation or demographic group.

**Register of retirees:** the register of retirees provides information about the type of pension paid to each retiree, including disability benefits, as well as the date when the first pension payment was collected and the amount received.

## 1.4 Empirical Strategy

The purpose of our empirical analysis is twofold. On the one hand, we aim to study the substitutability between workers by measuring the effect of retaining an extra older employee on labor demand. On the other hand, we want to evaluate the implications of substitutability for the short-run government revenues generated by an increase in the full retirement age. The number of workers retained because of the reform is strongly related to a firm's demographics. Variation along this dimension likely reflects differences in unobservable labor demand dynamics. In the first part of the analysis, we solve this endogeneity by instrumenting the number of retained older workers with the firm-level shift in the full retirement date of employees on the cusp of retirement. In the second part of the paper, we use the latter to study the effect of increasing the retirement age on the government budget. To construct the shift, we follow a two-step procedure. First, we compute the change in the expected retirement date for employees close to retirement before the reform, who we define *potential retirees* (sub-section 1.4.1). Second, we construct the average variation in the full retirement date of *potential retirees* employed at the firm when the reform is passed (sub-section 1.4.2). This variable changes across firms due to idiosyncratic differences in the distribution of gender, age and years of contribution among the narrow set of *potential retirees*. We exploit such an identifying variation within a difference-in-differences regression framework (sub-section 1.4.3).

### 1.4.1 Individual shift in the full retirement date

For the purpose of computing the retirement date, an older worker can be summarized by her type  $\theta(g, a, c)$ , where  $g$  is gender, while  $a$  and  $c$  are age and years of contributions as of December 2011, respectively. We draw on workers' demographics to build the first two variables and on contribution histories to compute total years of contributions, following the rules detailed in Appendix A.4. For every type  $\theta$  we compute the reform-induced change in years until full retirement, thus excluding early retirement options. To this end, we construct the predicted retirement dates according to pre- and post-reform rules and denote the difference with  $\delta_\theta$ .<sup>27</sup>

$$\delta_\theta = \text{Years until full retirement date}_\theta^{\text{post-reform}} - \text{Years until full retirement date}_\theta^{\text{pre-reform}} \quad (1.1)$$

If early retirement choices are influenced by the reform,  $\delta_\theta$  is an individual assignment to treatment as opposed to the actual change in the retirement date. To construct  $\delta_\theta$ , we take as given the contribution history observed in the data up to 2011 and we make the following assumptions on the post-2011 contribution history:

- i) workers accrue full contributions on their accounts (52 weeks per year) until retirement
- ii) the predicted retirement date is the earliest date at which the worker becomes eligible to claim either the old-age or the seniority pension

Assumption (i) requires that individuals work year-round and full-time in the post-reform period.<sup>28</sup> Data shows that the median annual contribution is 52 weeks for workers aged 60 or above in 2012, suggesting that assumption (i) has solid ground. Assumption (ii) provides a criterion to select among the different options to claim full benefits: after predicting the retirement date associated to every available option, we select the earliest one. As discussed

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<sup>27</sup>Before the *Fornero* reform abolished them, the so-called 'waiting windows' (*finestre mobili*) were in place: workers could start collecting pension benefits only 12 months after becoming eligible. Most workers therefore postponed retirement until that moment. We incorporate this feature of pre-reform rules by assuming that, had the reform not passed, employees would have retired one year after meeting the requirements for eligibility.

<sup>28</sup>Alternatively, we require that non-work periods are covered by *figurative* contributions (see Appendix A.4).

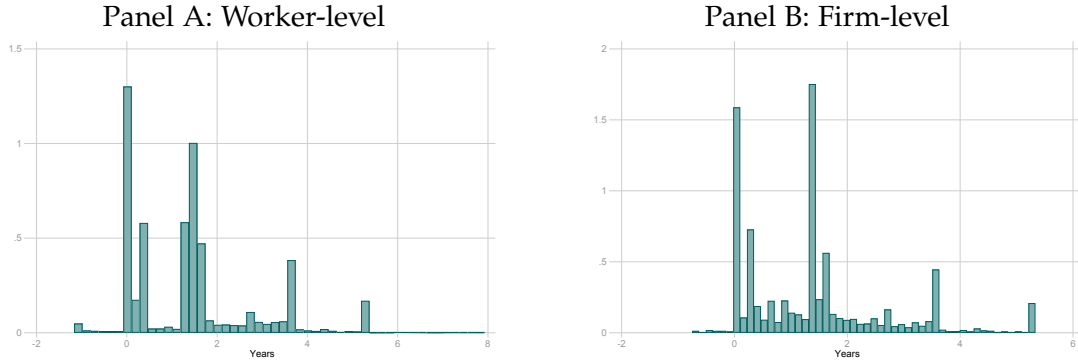
earlier, an extensive literature has documented that retirement behavior displays bunching at the acquisition of full pension rights. Figure A.1 shows consistent evidence for Italy. Under our assumption, the option to claim full benefits may change because of the reform: some types  $\theta$  could reach eligibility to an old-age pension first under pre-reform rules, while the seniority pension may become the earliest option available under post-reform rules, and viceversa.

For all workers who were expected to retire by 2014 under pre-reform rules and that actually retired by 2017, we compare actual and predicted retirement dates. Figure A.5 shows a “forecast quality” assessment. The majority of the differences between the two dates (69 percent) lies within a 1-year window, indicating that our measure is quite accurate in predicting the actual retirement date (Panel A). This also provides supportive evidence to assumption (ii), because a thin right tail of the distribution implies that workers rarely retire later than we predict. The left-skewness arises because of two main reasons. First, women can use the early retirement option (*opzione donna*), which causes a larger difference between actual and predicted retirement dates for female workers (Panel B). Second, some workers maintain the right to retire under pre-reform rules (see Appendix A.2).

### 1.4.2 Firm-level shift in the full retirement date

We focus on the change in the retirement date of older workers on the cusp of retirement before the reform. We classify as *potential retirees* the full-time employees who could have retired within 3 years under old rules (*i.e.* by 2014) and who are directly affected by the reform in the short-run. We show later that the retention of older workers further away from retirement at the reform date has minor effects on short-run firm responses. The 3-year threshold also allows us to focus on a subset of workers with similar age and contribution histories, who at the same time face a diverse enough variation in the residual working life because of the reform. Figure 1.1 plots the distribution of the change in years until the full retirement date for *potential retirees* (Panel A). As already emerged from heat-maps in Figure A.4, it displays a substantial variability, with mean 1.36 and standard deviation 1.4.

**Figure 1.1:** *Shift in full retirement date: distribution*



*Notes:* The figure shows the distribution of the reform shock. Panel A shows the distribution of the worker-level shift in the full retirement age among *potential retirees* in our sample of firms (as defined in section 1.4.4). Panel B shows the distribution of  $T_i$  (equation 1.2) among firms in our sample, excluding first and last percentiles. The predicted retirement dates of *potential retirees* under post-reform rules are capped at December 2020, as dispositions available in 2012 did not span a longer horizon. The capping, nonetheless, only applies to very few individuals. Due to the abolition of the waiting window few workers face a negative change, i.e. can retire sooner under new rules. Number of workers = 98,358. Worker-level shift mean = 1.36 (std. dev. = 1.4). Number of firms = 61,434. Firm-level treatment mean = 1.37 (std. dev. = 1.33)

Every *potential retiree* of type  $\theta$  experiences the same shift  $\delta_\theta$  of the expected full retirement date. To construct the firm-level change in the full retirement date, we then build a shift-share shock. We weight the  $\delta_\theta$ s by the share of every  $\theta$  in the subset of *potential retirees* employed in the firm. We therefore have:

$$T_i = \sum_{\theta \in \text{Potential retirees}} \pi_{\theta,i} \delta_\theta \quad (1.2)$$

$\pi_{\theta,i}$  is the share of type- $\theta$  workers among *potential retirees* employed at firm  $i$  in the last quarter of 2011. The  $\pi_{\theta,i}$ s depend neither on firm size nor on the share of *potential retirees* out of the total firm's workforce. As a consequence, we show that they do not reflect meaningful differences in the broad demographics of the firm.  $T_i$  therefore captures the idiosyncratic firm-level shift in the full retirement date of *potential retirees*. It also has a straightforward interpretation as the change in the policy parameter shifted by the reform. Because of these properties, in the first part of the analysis we leverage  $T_i$  as an instrument for the number of retained *potential retirees*, while in the second part we directly use it to quantify how workers, as well as government revenues, are affected by a policy that increases the full retirement

age.

The distribution of  $T_i$  for firms that employ at least one *potential retiree* displays significant variability. The mean is 1.37 and the standard deviation is 1.33 (Figure 1.1, Panel B). By construction,  $T_i$  converges to the average shift of the full retirement date in the population of *potential retirees* the larger is the firm.<sup>29</sup> This is one of the reasons why we focus on small and medium firms, where the variability of  $T_i$  - although still declining in firm size - is greater.

### 1.4.3 Empirical Specification and identifying assumptions

#### Empirical specification

To study workers substitutability, in the first part of the analysis we measure the effect of retaining an extra *potential retiree* on labor demand margins. We estimate a difference-in-differences model with a continuous treatment and multiple pre- and post-reform periods:

$$Y_{it} = \lambda_i + \gamma_t + \sum_{k=2009}^{2015} \beta_k^R I(k=t) \times R_i + \varepsilon_{it} \quad (1.3)$$

$i$  indexes the firm and  $t$  indexes the year.  $Y_{it}$  is the outcome of interest.  $\lambda_i$  is a firm fixed-effect that captures time-invariant heterogeneity across firms, including differences in average outcomes across treatment levels;  $\gamma_t$  are year fixed effects that control for year-specific common shocks to all firms. Standard errors are clustered at the firm level to address the potential concern of serial correlation across periods (Bertrand *et al.*, 2004). The coefficients of interest are  $\{\beta_k^R\}_{k=2009}^{k=2015}$  and show how the treatment  $R_i$  affects firms in year  $k$  relative to the reform year.<sup>30</sup>  $R_i$  measures the number of retained *potential retirees* in firm  $i$ . We define a *potential retiree* retained if she retires one or more years after the predicted full retirement date under pre-reform rules. We do not require that the employee continues working for the

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<sup>29</sup>This is because larger firms employ more *potential retirees*, so that the distribution of age, gender and contributions among them is more likely to mirror the one prevailing in the universe of employees on the cusp of retirement. For the same reason,  $T_i$  does not exhibit substantial variation across local labor markets.

<sup>30</sup>We set  $\beta_{2011}^R$  equal to 0.

same employer until retirement, because labor demand adjustments for this type of workers are among the outcomes we aim to study. Our treatment therefore measures the number of *potential retirees* that, absent any labor demand adjustment, would remain employed in the firm for longer than expected. The number of workers retained in firm  $i$  therefore is:

$$R_i = \sum_{j: j \in \text{Potential Retirees}_i} I(\tilde{\delta}_j \geq 1) \quad (1.4)$$

where  $\tilde{\delta}_j$  is the observed change in the full retirement date of individual  $j$ , *i.e.* the difference between the observed retirement date and the predicted full retirement date under pre-reform rules.  $R_i$  is however strongly correlated with the size and the age structure of the firm.<sup>31</sup> Hence, it may capture the effect of differences along these or other unobserved dimensions rather than the impact of the reform. To cope with this endogeneity problem, we therefore propose an IV strategy whereby we instrument  $R_i$  with the firm-level shift in the retirement date  $T_i$ , exploiting the fact that *potential retirees* delay retirement to a different extent per effect of the reform.

To summarize the results, we also estimate standard difference-in-differences regressions comparing pre-reform years (2009-2011) to post reform years (2012-2015):

$$Y_{it} = \alpha + \lambda_i + \sum_{k=2009}^{2015} \beta_k \gamma_k + \beta^R \text{Post}_t \times R_i + \varepsilon_{it} \quad (1.5)$$

We interact the treatment with the dummy  $\text{Post}_t$  that equals 1 in years 2012-2015.  $\beta^R$  captures the treatment effect on the difference in the outcomes between pre- and post-reform periods.<sup>32</sup>

In the second part of the paper, we study the effect of raising the full retirement date on public finances, estimating:

$$Y_{it} = \lambda_i + \gamma_t + \sum_{k=2009}^{2015} \beta_k^T I(k = t) \times T_i + \varepsilon_{it} \quad (1.6)$$

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<sup>31</sup>Firms with different shares of retained *potential retirees* have non-parallel pre-reform labor demand paths (Figure A.6).

<sup>32</sup>We instrument  $R_i$  with  $T_i$  as in specification 1.3.

Thus, we exploit variation in the policy parameter ( $T_i$ ) that has been changed by the reform instead of the increase in the number of retained *potential retirees*.

### Identification assumptions

We leverage variation in the characteristics of *potential retirees* for identification. The extent to which firms are affected by the reform depends on the distribution of the shares of types  $\theta$  among their *potential retirees* (equation 1.2). Identification requires that  $\pi_{\theta,i}$ s do not correlate with firm's unobservable time-varying characteristics (Goldsmith-Pinkham *et al.*, 2017 and Borusyak *et al.*, 2018). In other words, the characteristics of *potential retirees* employed at a given firm should not correlate with the firm's time-varying unobservables. The shares  $\pi_{\theta,i}$ s depend neither on firm size nor on the number of *potential retirees*. Thus, we leverage a source of variation that does not depend explicitly on the firm's demographics; we only exploit the variability that stems from idiosyncratic differences across firms in the gender, age and years of contributions (i.e. in the distribution of types  $\theta$ ) of the narrow set of *potential retirees*. Evidence that the composition of *potential retirees* in a firm relates to trends in labor demand would provide a sign that identification is failing. Pre-trends as captured by the coefficients  $\{\beta_k^T\}_{k=2009}^{k=2011}$  provide suggestive evidence of the exogeneity of  $T_i$ . If trends are parallel, these coefficients should not be different from zero. We perform placebo and balancing tests to assess the validity of the identifying assumptions.

### Placebo tests

We assess whether  $T_i$  predicts labor demand trends by running a series of placebo tests on the pre-reform period (2009-2011). To this end, we artificially assign the date in which the reform becomes effective to 2010 or 2011, rather than to 2012. We then estimate on the pre-reform period a version of specification (1.5) where we replace  $R_i$  with  $T_i$ .<sup>33</sup> We test the effect of  $T_i$  on layoffs and new-hires, which are the main firm-level outcomes we study in Section 1.6. Table A.2 shows that  $T_i$  has virtually zero effect, indicating that firms

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<sup>33</sup>This is the reduced form of the IV estimation of equation (1.5).

that will face heterogeneous shifts in the full retirement age of their *potential retirees* in 2011 do not exhibit differential demand trends in the pre-period. Hence, we exclude that the composition of *potential retirees* in a given firm in 2011 predicts hiring and firing decisions in the years preceding the reform. We nonetheless show that pre-reform coefficients from (1.3) and (1.6) are in general not significant.

### **Balancing tests**

We run balancing tests whereby we regress a rich set of firm's baseline characteristics on  $T_i$  (Table A.3). The correlation between  $T_i$  and baseline firm characteristics is very weak, although precisely estimated. Importantly, this holds true for the gender composition and the workforce age structure. As  $T_i$  increases by 1 year, the share of male workers in the firm decreases by only 0.025 against an average of 0.656, despite that fact that women are on average more affected by the reform than men. The shares of older, middle-aged and young workers have coefficients 0.009, -0.009 and 0 against averages of 0.123, 0.58 and 0.297. When controlling for province, sector, and province $\times$ sector fixed-effects the magnitudes of these correlations drop and some become not significant. Furthermore, we show that results on labor demand responses do not change when controlling for these covariates (Section 1.7).

#### **1.4.4 Sample and Descriptive Statistics**

In the baseline specification, we restrict our attention to the sample of firms with 3-200 employees in the first quarter of 2009 that remain active throughout 2009-2015. For internal validity we focus on firms that employ at least one *potential retiree* in the quarter when the reform is passed (q4-2011). Firms with no *potential retirees* may not be an appropriate control group. Indeed, they have a different demographic composition and are likely to differ along other time-varying characteristics that we do not observe and cannot control for. We nonetheless show that results are confirmed on the universe of firms in the 3-200 size class. Finally, we keep firms with a single Social Security code to exclude multi-establishment

companies.<sup>34</sup> These restrictions leave us with a panel of 61,434 firms.

Table A.4 compares the characteristics of firms in our main sample to other single-establishment same-sized firms active in 2009-2015. Firms with at least one *potential retiree* are on average three times as large as other firms and older. They are more concentrated in the manufacturing sector and have a higher share of blue-collars. Their workforce is older, more paid, more experienced, and has higher tenure. Table A.5 shows that *potential retirees* are older, more experienced, and have higher tenure than other full-time employees in our sample. They have higher gross daily wages, and are more likely to have a permanent contract. Table A.6 compares workers within 3 years from retirement in 2011 to other older employees similar along many dimensions.<sup>35</sup> Before the reform employees closer to retirement are 5 percentage points more likely to be absent from work because of sickness and 1 percentage point more likely to be absent due to work-related injuries or sick leave. These results are in line with Dostie, 2011 and Borsch-Supan and Weiss, 2016 who find that employees approaching the end of their working lives reduce effort.

## 1.5 Older workers delay retirement after the reform

The reform may affect a firm's labor demand if it prolongs the working lives of older workers. We investigate the response of retirement choices to the full retirement age by estimating an individual-level version of (1.6) on the sample of *potential retirees*. We use the worker-level shift in the full retirement date as treatment ( $\delta_\theta$  in equation 1.1). Extending the retirement date by one year causes a decline in the number of months spent on retirement up to 2 months in 2015 (Figure 1.2). The effect increases over time because most *potential retirees* - eligible to retire by 2014 under old rules - would have worked in the first post-reform years even under pre-reform rules. The decline is smaller than what would have occurred if all workers retired at the post-reform retirement date (benchmark in Figure 1.2). The difference

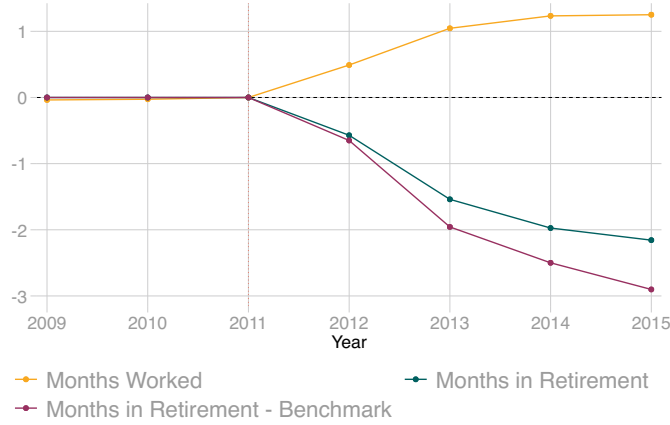
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<sup>34</sup>Firms typically have one Social Security code per establishment.

<sup>35</sup>We perform a coarsened exact matching procedure. Matching covariates are: age, gender, type of contract (full-time vs part-time, open-ended vs fixed-term), occupation, as well as firm's province, sector and size.

between the two lines reflects early retirement responses to the shift in the full retirement date.

**Figure 1.2:** *The reform prolongs the working life of potential retirees*



*Notes:* The figure shows the effect of a 1-year shift of the full retirement date. It is based on the specification in (1.6) where the unit of analysis is the single *potential retiree*. We plot the coefficients of the regressions alongside 95 percent confidence intervals. Standard errors are clustered at the worker level. We define *potential retirees* those workers who were expected to retire within three years (by 2014) under pre-reform rules when the reform is implemented. The Figure shows results on actual months in retirement, predicted (benchmark) months in retirement if the workers retired at the post-reform predicted date and months at work. The difference between months spent in retirement and predicted (benchmark) months spent in retirement captures the extent to which workers change early retirement choices in response to an extension of the full retirement age. Number of observations = 853,839. Pre-reform mean outcomes: months worked = 11.09, months in retirement = 0.00, months in retirement (benchmark) = 0.00.

To better quantify the response of observed retirement to the policy, we also regress the difference between the observed retirement date and the pre-reform full retirement date on  $\delta_\theta$ . The specification includes age, gender, province and sector fixed-effects. A 1-year raise in the full retirement date delays retirement by 6.73 months in the sample of *potential retirees* who retired by December 2017 (Table 1.2). The response is slightly larger for women (6.81 months against 6.54 for men). These figures are close to estimates in Mastrobuoni (2009) for the U.S., where a 1-year increase in the normal retirement age causes a 6-month delay in retirement.

**Table 1.2:** *Response of retirement choices to the change in retirement age*

|                                    | (1)<br>All         | (2)<br>Male        | (3)<br>Female      |
|------------------------------------|--------------------|--------------------|--------------------|
| Change in Retirement Date (months) | 6.73***<br>(0.034) | 6.54***<br>(0.073) | 6.81***<br>(0.045) |
| Observations                       | 134,832            | 87,072             | 47,751             |
| Treatment Mean                     | 1.43               | 1.21               | 1.82               |
| Treatment Std. Dev.                | 1.57               | 1.08               | 2.14               |

*Notes:* The table reports estimates from a cross-section regression where the outcome is the difference (in months) between the observed retirement date and the expected retirement date under pre-reform rules. The treatment is the individual-level change in years left to retirement caused by the reform. Column (1) shows the results for all *potential retirees*, column (2) and (3) show the results for male and female *potential retirees*, respectively. The coefficients capture how responsive to the reform is the retirement choice. The regression controls for age, gender, province and sector fixed-effects. Standard errors in parentheses are clustered at the province  $\times$  sector level. \*\*\*  $p < 0.01$ ; \*\*  $p < 0.05$ ; \*  $p < 0.1$ .

## 1.6 Labor demand responses to the reform

In this Section we document how firm's labor demand for (typically younger) co-workers responds when an extra *potential retiree* is retained in a firm. We focus on layoffs and new hires as margins of adjustment in labor demand and we estimate (1.3) on the sample described in sub-section 1.4.4. For each outcome of interest, we plot the coefficients  $\{\beta_k^R\}_{k=2009}^{2015}$ , along with 95 percent confidence intervals. The number of retained *potential retirees* is instrumented with the firm-level shift in the full retirement date  $T_i$ . The first stage is reported in Table 1.3.

According to labor demand theory, a drop in demand for co-workers caused by the retention of older employees can be reconciled with complementarity between the two types of labor only in case of an increase in co-workers' wages. As we document below, the labor demand for co-workers in firms that are more affected by the reform drops. A large wage increase is inconsistent with the evidence in Section 1.8 that shows a decline in the earnings of co-workers. We conclude that co-workers are substitutes for older workers retiring. We also show a drop in the demand for younger co-workers, which indicates substitutability across ages. To grasp the intuition, in Appendix A.5 we develop a labor demand model with heterogenous labor. We incorporate different wage formation models to show that

under alternative assumptions our results are only consistent with age substitutability.

### 1.6.1 Layoffs

Firms increase layoffs of permanent employees in response to increased retention of *potential retirees* (Figure 1.3, Panel A and Table A.7).<sup>36</sup> No significant difference in the layoffs of more and less treated firms is present before the reform. The difference emerges in its aftermath. When an extra *potential retiree* is retained, the number of fired workers rises over the period 2012-2015, up to 0.26 in 2015. This amounts to about 66.6 percent of the pre-reform average of layoffs per year (0.39). A concave pattern of coefficients reflects the dynamics in the retention of *potential retirees*. Most retained *potential retirees* would have worked in 2012, the year when we detect the smallest effect, also under the pre-reform rules. The effect on layoffs then grows significantly in 2013 and 2014, when most retained *potential retirees* would have retired in absence of the reform. Finally, in 2015 some of the *potential retirees* that were retained eventually retire, so that the growth in the number of layoffs flattens out. Table 1.3 summarizes the results reporting the estimated coefficient from the more compact specification (1.5). Firms fire 0.17 more workers per year in the four years after the reform for every extra *potential retiree* retained, which amounts to 43.6 percent of the pre-reform average number of layoffs. To study the substitutability between cohorts, Panel B of Figure 1.3 breaks down the effect by workers age. We classify workers under age 35 as “young”, employees aged between 35 and 55 as “middle-aged”, and individuals aged 55 or older as “old”. Layoffs increase across all age groups. Comparing the coefficients in Table 1.3 to the cohort-specific average number of layoffs before the reform, the percentage increase is stronger among middle-aged and older workers. As expected, the strongest reaction to the shock is concentrated on older workers. Figure 1.3 Panel C shows the estimates when the dependent variable is the ratio between the number of layoffs in every cohort in a given year and the respective number of incumbent employees at the beginning of the

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<sup>36</sup>The reform has no effect on firings of fixed-term workers. Since labor regulations force firms to pay a temporary worker until the contract end date if she is fired for economic reasons, the cheapest way to part from a temporary employee is not to renew her contract. Thus, we observe very few of such cases.

period. The coefficients therefore capture the effect of retaining a *potential retiree* on the probability of being laid-off. Older workers face a significantly larger probability of being fired than younger co-workers, indicating a closer substitutability with *potential retirees*. The increase in layoffs probability for middle-aged and young workers is identical, despite the fact that middle-aged are fired more in number and relative to the layoffs mean before the reform. Since we detect a larger increase in the firing probability of older workers, we investigate whether the adjustment is concentrated on *potential retirees* (Figure 1.3, Panel D). When excluding *potential retirees* from the group of older workers the coefficients halve, suggesting that half of the effect is concentrated on this category of workers. The firm median share of *potential retirees* out of older workers is 65 percent. Thus, *potential retirees* are not disproportionately affected relative to other older employees. Since *potential retirees* and other older workers earn similar wages, the cost of firing the two types of workers is the same.<sup>37</sup> We find that firms trade them off in a similar fashion, suggesting a strong substitutability.

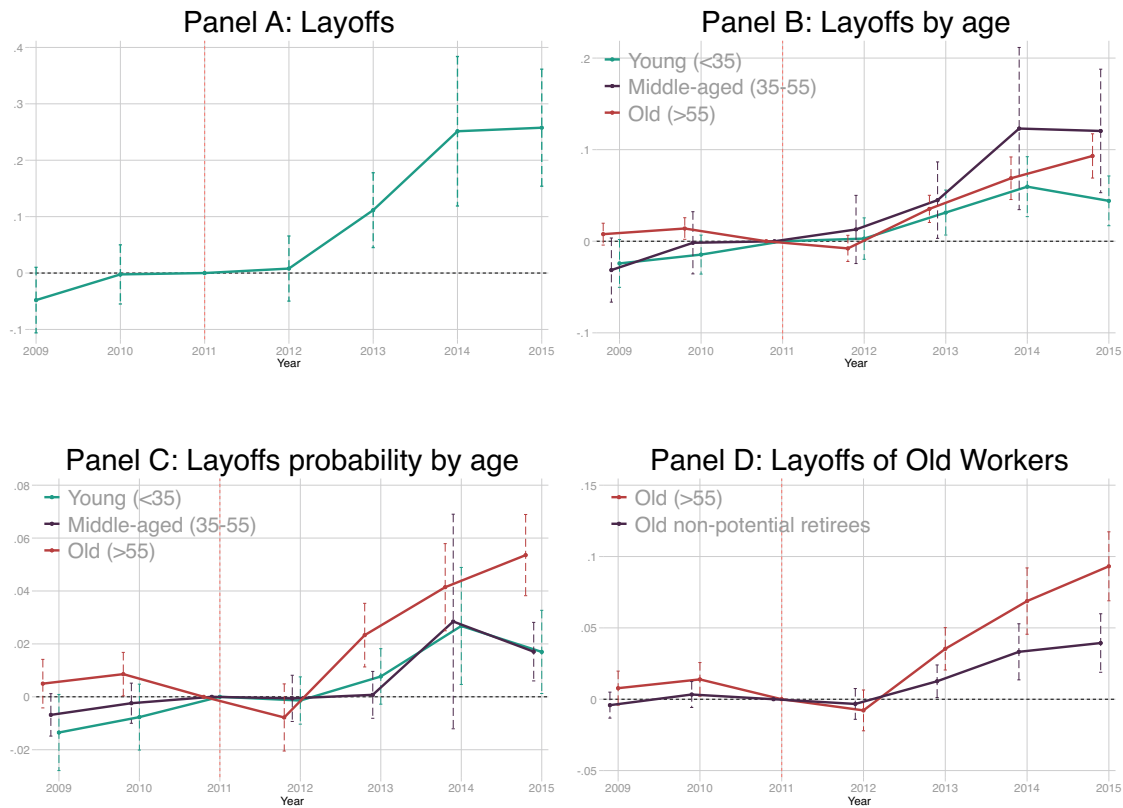
### 1.6.2 New Hires

Firms more affected by the reform change their hiring schedule in the post-reform period, while there is no evidence that they were on a different trend in the pre-reform period (Figure 1.4, Panel A and Table A.8). A retained *potential retirees* causes an average drop in hiring of up to 0.49 units per year. Against an average of 4.79 new hires per year, it amounts to a 10.2 percent reduction. The effect vanishes in 2015, when the coefficient is close to zero. The u-shaped pattern of coefficients suggests that firms delay hiring in response to the reform. New hires drop in the reform aftermath and bounce back as *potential retirees* become eligible to retire under new rules. Panel B of Figure 1.4 decomposes the effect by new hires' age. The drop is equally borne by young and middle-aged workers. We find little effect on older workers, largely because there are very few hires of workers aged over 55. Table 1.3 summarizes the results showing that new hires of young and middle-aged decline

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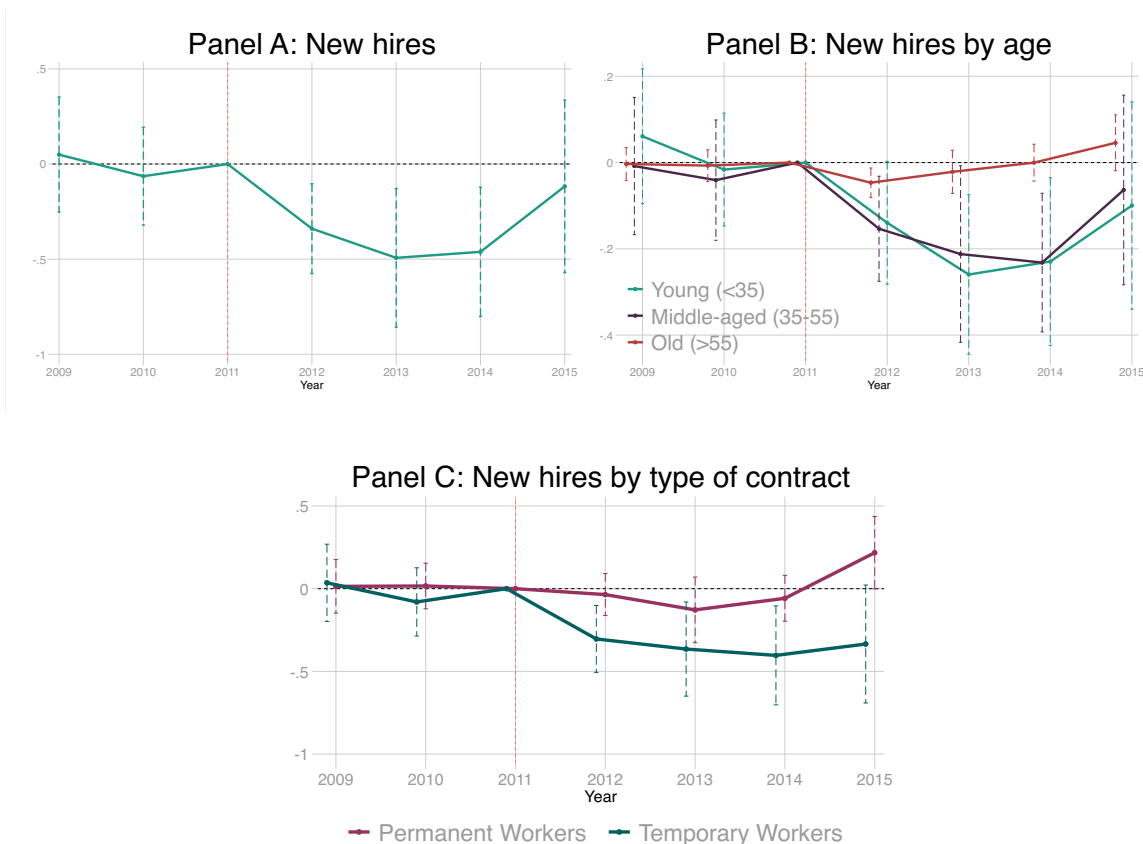
<sup>37</sup>The cost of layoffs - when ruled unfair by a labor court - is a function of the fired worker's wage.

Figure 1.3: Layoffs



Notes: The figure shows the response of total layoffs (Panel A), layoffs by age group (Panel B), layoffs probability by age group (Panel C), and layoffs of older workers (Panel D) to an extra retained *potential retiree*, alongside 95 percent confidence intervals. Standard errors are clustered at the firm level. The layoffs probability in Panel C is defined as the ratio between the number of layoffs in every cohort in a given year and the respective number of incumbent employees at the beginning of the period. We define *potential retirees* those workers who were expected to retire within three years (by 2014) under pre-reform rules when the reform is implemented. Young workers are aged below 35, middle-aged workers are between 35 and 55 years old, old workers are over 55 years old and old non-potential retirees are old workers who in 2011 were not expected to retire within three years under pre-reform rules. The regression is based on specification (1.3). The treatment is defined as the number of retained *potential retirees* employed at the firm when the reform is implemented. A *potential retiree* is retained if she retires at least one year after her pre-reform full retirement date. The number of retained *potential retirees* is instrumented with the firm-level average change in their full retirement date defined in equation (1.2). The regressions are estimated on the universe of single-establishment private sector firms that (i) were active every year in the period 2009-2015, (ii) employed between 3 and 200 employees in the first quarter of 2009, and (iii) employed at least one *potential retiree* in the last quarter of 2011. In Panel C the sample of firms is further restricted to include only firms with at least one worker in each cohort at the beginning of the period. Number of observations = 430,038 (305,319 in Panel C). Mean outcome pre-reform: total = 0.39; young = 0.13; middle-aged = 0.2; old = 0.06, old non-potential retirees = 0.04.

**Figure 1.4: New Hires**



*Notes:* The figure shows the response of total new hires (Panel A), new hires by age (Panel B) and new hires by type of contract (Panel C) to an extra retained *potential retiree*, alongside 95 percent confidence intervals. Standard errors are clustered at the firm level. We define *potential retirees* those workers who were expected to retire within three years (by 2014) under pre-reform rules when the reform is implemented. Young workers are aged below 35, middle-aged workers are between 35 and 55 years old, old workers are over 55 years old. The regression is based on specification (1.3). The treatment is defined as the number of retained *potential retirees* employed at the firm when the reform is implemented. A *potential retiree* is retained if she retires at least one year after her pre-reform full retirement date. The number of retained *potential retirees* is instrumented with the firm-level average change in their full retirement date defined in equation (1.2). The regressions are estimated on the universe of single-establishment private sector firms that (i) were active every year in the period 2009-2015, (ii) employed between 3 and 200 employees in the first quarter of 2009, and (iii) employed at least one *potential retiree* in the last quarter of 2011. Number of observations = 430,038. Mean outcome (pre 2012): total = 4.79, young = 2.38, middle = 2.06, old = 0.35, permanent = 1.46, temporary = 3.32.

by 0.20 and 0.15 units per year respectively, accounting for virtually all of the observed drop in hiring. Relative to the pre-reform average number of hiring, the drop is equal to 8.4 percent for young workers and 7.3 percent for middle-aged workers. The decline predominantly concerns fixed-term employment relationship (Figure 1.4, Panel C). Since firms typically hire junior workers under temporary contracts, this is consistent with the effect being concentrated among young and middle-aged workers. It also emerges that the null coefficient on total hiring in 2015 (Panel A) masks substantial heterogeneity. More affected firms still hire fewer workers on fixed-term contracts, but they hire more workers on permanent ones.<sup>38</sup>

### 1.6.3 Which *potential retirees* matter more?

All workers experience an increase in years left to full retirement, except those who become eligible by the end of 2011.<sup>39</sup> We argued that, in the short-run, the most proximate consequence for the firm is the increase of the retention rate of workers on the cusp of retirement. We test the validity of our argument by checking whether firms respond to changes in the retention rate of workers who were less close to retire. To this end, we augment specification (1.3) to include two treatment variables: the first is the number of retained *potential retirees*  $R_i$ , the second is the number of retained individuals who were expected to retire further away in time, in the 2015-2018 period.<sup>40</sup> Only the first treatment has significant effects on layoffs and new hires (Figure A.7). Table A.9 shows that the effects of the two treatments are statistically different at the 1 percent level for layoffs and at 10 percent for new hires. Hence, the change in the full retirement date of *potential retirees* is sufficient to explain most of the adjustments in labor demand in the short-run. The change in the full retirement date

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<sup>38</sup>Part of the increase in 2015 could be caused by a generous package of incentives for fostering permanent contracts that was put in place in 2015. More affected firms had been hiring fewer workers in the previous years. Thus, the increase in new hires of permanent workers could be the consequence of firms exploiting such incentives more heavily as *potential retirees* start to retire.

<sup>39</sup>See Section 1.2.3.

<sup>40</sup>The first treatment is then instrumented with  $T_i$ , while the second treatment is instrumented by the firm-level shift in the full retirement date of individuals who were expected to retire in the 2015-2018 period.

**Table 1.3:** *The effect of the reform on layoffs and new hires*

| <b>Panel A: Layoffs</b>  | All                 | Young               | Middle-aged         | Old                 | Old<br>Non-potential<br>retiree |
|--------------------------|---------------------|---------------------|---------------------|---------------------|---------------------------------|
|                          | (1)                 | (2)                 | (3)                 | (4)                 | (5)                             |
| $R_i \times \text{Post}$ | 0.174***<br>(0.030) | 0.047***<br>(0.009) | 0.086***<br>(0.019) | 0.040***<br>(0.006) | 0.021***<br>(0.005)             |
| N                        | 430,038             | 430,038             | 430,038             | 430,038             | 430,038                         |
| Mean outcome pre-2012    | 0.39                | 0.13                | 0.2                 | 0.06                | 0.04                            |
| Mean $R_i$               | 0.83                | 0.83                | 0.83                | 0.83                | 0.83                            |
| Std. Dev. $R_i$          | 0.99                | 0.99                | 0.99                | 0.99                | 0.99                            |
| Mean $T_i$               | 1.37                | 1.37                | 1.37                | 1.37                | 1.37                            |
| Std. Dev. $T_i$          | 1.33                | 1.33                | 1.33                | 1.33                | 1.33                            |
| Coeff first stage        | 0.21                | 0.21                | 0.21                | 0.21                | 0.21                            |
| SE first stage           | 0.00                | 0.00                | 0.00                | 0.00                | 0.00                            |
| KP F-statistics          | 8,288.71            | 8,288.71            | 8,288.71            | 8,288.71            | 8,288.71                        |

| <b>Panel B: New Hires</b> | All                 | Young               | Middle-aged         | Old               | Permanent         | Temporary          |
|---------------------------|---------------------|---------------------|---------------------|-------------------|-------------------|--------------------|
|                           | (1)                 | (2)                 | (3)                 | (4)               | (5)               | (6)                |
| $R_i \times \text{Post}$  | -0.348**<br>(0.159) | -0.197**<br>(0.090) | -0.149**<br>(0.072) | -0.002<br>(0.018) | -0.011<br>(0.067) | -0.34**<br>(0.131) |
| N                         | 430,038             | 430,038             | 430,038             | 430,038           | 430,038           | 430,038            |
| Mean outcome pre-2012     | 4.79                | 2.38                | 2.06                | 0.35              | 1.46              | 3.32               |
| Mean $R_i$                | 0.83                | 0.83                | 0.83                | 0.83              | 0.83              | 0.83               |
| Std. Dev. $R_i$           | 0.99                | 0.99                | 0.99                | 0.99              | 0.99              | 0.99               |
| Mean $T_i$                | 1.37                | 1.37                | 1.37                | 1.37              | 1.37              | 1.37               |
| Std. Dev. $T_i$           | 1.33                | 1.33                | 1.33                | 1.33              | 1.33              | 1.33               |
| Coeff first stage         | 0.21                | 0.21                | 0.21                | 0.21              | 0.21              | 0.21               |
| SE first stage            | 0.00                | 0.00                | 0.00                | 0.00              | 0.00              | 0.00               |
| KP F-statistics           | 8,288.71            | 8,288.71            | 8,288.71            | 8,288.71          | 8,288.71          | 8,288.71           |

*Notes:* The table is based on specification (1.5). Standard errors in parentheses are clustered at the firm level. Panel A shows the effect on layoffs and Panel B on new hires. Column (1) shows the effect on all workers, Column (2) on young workers (below 35 years old), Column (3) on middle-aged workers (35-55 years old), Column (4) on old workers (above 55 years old). Column (5) Panel A shows the effect on old non-potential retirees. Columns (5) and (6) Panel B show the effects on new hires of permanent and temporary contract workers, respectively. The treatment is defined as the number of retained *potential retirees* employed at the firm when the reform is implemented. A *potential retiree* is retained if she retires at least one year after her pre-reform full retirement date. The number of retained *potential retirees* is instrumented with the firm-level average change in their full retirement date defined in equation (1.2). The regressions are estimated on the universe of single-establishment private sector firms that (i) were active every year in the period 2009-2015, (ii) employed between 3 and 200 employees in the first quarter of 2009, and (iii) employed at least one *potential retiree* in the last quarter of 2011. \*\*\*  $p < 0.01$ ; \*\*  $p < 0.05$ ; \*  $p < 0.1$ .

of employees between 4 to 7 years from retirement when the reform becomes effective has small effects in the first four years after the reform. We cannot exclude that firms react more strongly to this shock in the following years - for which we do not have data - when the workers between 4 to 7 years from retirement were expected to exit the labor market.

#### 1.6.4 Response heterogeneity by occupation and turnover

##### **Within and across occupations:**

We further explore how the shock to the retirement date of *potential retirees* is absorbed within the firm by looking at the decisions of its units. We call unit the group of employees in a specific qualification (blue-collar, white-collar or manager). We estimate a version of (1.3) at the firm-unit level. Our specification includes separately the number of retained *potential retirees* in the unit as well as the number of retained *potential retirees* in other units of the same firm.<sup>41</sup> Figure A.8 plots the results of this exercise. The within-unit treatment generates a larger effect on layoffs and new hires. Retaining a *potential retiree* for longer than expected impacts the number of layoffs and new hires in the same occupation. On the other hand, there is limited spillover across units in the same firm. Our evidence is consistent with higher substitutability between workers who perform similar tasks. Similarly, Jäger and Heining (2019) finds that workers substitutability is larger within occupations.

##### **Turnover:**

Firms with a higher propensity to separate from workers should be more prone to adjust labor demand in response to shocks. To test this, we construct a measure of firm's turnover by using the share of separations over the total workforce in the pre-reform period. We label a separation as either a layoff, termination of a temporary contract, or a voluntary quit. We then split firms into two groups based on whether they fall below or above the median of the distribution of the turnover measure. We estimate a triple difference specification using

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<sup>41</sup>The former treatment is instrumented with the shift in the full retirement date of *potential retirees* in the unit, the latter with the shift in the full retirement date of *potential retirees* in other units.

the two groups of firms. Most of the effect on layoffs is explained by high-turnover firms (Figure A.9). Almost no effect is detected on low-turnover firms. Thus, high-turnover firms more easily manipulate the margin of layoffs. We conclude that the workers facing a higher layoff probability as a consequence of the reform are those who already expect a higher probability of separation.

## 1.7 Sensitivity checks and Discussion

In this section we discuss our robustness checks and some potential threats to identification. We then discuss general equilibrium effects and the relationship between our results and the previous literature.

**Potential confounding factors:** We start by looking at some observables that could confound our estimates. Firm fixed-effects do not control for time-varying differences across firms in our main specification. Table A.3 has shown that the relation between  $T_i$  and firm characteristics is weak. We further address this concern by estimating an augmented specification that adds to (1.3) the interaction between a vector of covariates and year dummies. Figure A.10 shows that results are robust to the inclusion of a rich set of controls. First, we include in the vector of covariates a set of dummies for the quintiles of the share of female employees, because new retirement rules affect women to a greater extent than men. Adding these controls reduces the concern that non-parallel labor demand trends across firms with a different gender composition confound our estimates. Second, we add to the vector of covariates dummies for quintiles of several other variables: firm size, firm age, the share of young ( $< 35$ ), middle-aged ( $35 - 55$ ) and older ( $> 55$ ) workers. Third, we estimate a specification with year fixed-effects interacted with two-digit sector and province fixed-effects to check that our estimates are not confounded by heterogeneous economic cycles across sectors and provinces. Fourth, we add one year (2008) to the pre-reform period to prove that labor demand trends were similar up to four years before the reform in a longer balanced panel. Finally, we check whether results are robust in the universe of

single-establishment firms employing between 3 to 200 employees in the first quarter of 2009.<sup>42</sup> For this purpose, we set  $T_i = 0$  and  $R_i = 0$  if a firm employs no *potential retirees*. While heterogeneously treated firms appear to be on parallel trends in terms of firings before the reform, this is less the case for hiring, consistently with our conjecture that younger firms are not a good counterfactual for firms employing *potential retirees*. Nonetheless, the post-reform coefficients are similar to our baseline estimates for both outcomes.

**General equilibrium effects:** We conducted a partial equilibrium analysis of the short-run responses to the reform. However, general equilibrium dynamics could affect our identification threatening the implicit assumption on the absence of spillovers across firms. The responses of labor demand and supply to a higher retirement age may affect market tightness and the outside option of different cohorts of workers. Hence, firms that are not directly affected by the reform can change their behavior because of spillovers caused by other firms. However, it takes time for these dynamics to realize, so that general equilibrium effects are not likely to play a significant role over the horizon of our analysis (i.e. the first 4 years after the implementation of the reform). All the outcomes respond in the couple of years after implementation when general equilibrium is not likely to play a role. In addition, reductions in the number of new hires are significant, but tiny when compared to total new hires and cyclical fluctuations in hiring.<sup>43</sup> Finally, we showed in Section 1.6.3 that firms mostly react to the shock to the restricted set of *potential retirees*. This reduces the extent to which their responses can affect the labor market general equilibrium in the 4 years after the reform.

**Relation to the literature on age substitutability:** The *Fornero* reform offers a neat experiment to document how employers trade-off workers of different ages within the firm. We

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<sup>42</sup>Also in this case, we restrict the attention to firms that remain active between 2009 and 2015.

<sup>43</sup>A retained *potential retiree* causes a 0.35 drop in the number of new hires. There are 61,434 firms in our sample and the average number of retained *potential retiree* per firm is 0.83. A back of the envelope calculation predicts a drop in total new hires of 17,850 units, 0.2 percent of total new hires in 2011 and less than 5 percent of the average yearly fluctuation in hires.

exploit a firm-idiosyncratic shock to uncover firm’s production complementarities, and our results show clear evidence of substitutability among age cohorts within firms. This firm-level substitutability contributes to equilibrium allocations in local labor markets, but it is not sufficient to draw definitive conclusions about the relationship between the employment dynamics of different cohorts as studied by Gruber and Wise (2010). Documenting age complementarities in labor markets requires economy-wide demographic shocks instead of firm-level ones, and a long-run perspective that allows for slow adjustments in workers’ outside options and employment flows. For instance, a recent work by Mohnen (2019) exploits a slowdown in retirement of older workers to uncover substitutability among age cohorts in U.S. counties looking at decade-long horizons. His evidence confirms our substitutability results at a larger scale. Similar results have been found by Bertoni and Brunello (2017) for Italian local labor markets. In light of these recent advancements, our results can be regarded as a first step in uncovering the micro-level mechanisms that explain patterns of macro-level substitutability among age cohorts.

## **1.8 Workers’ earnings and take-up of other Social Security programs**

In this section we study how labor demand adjustments in response to an increase of the full retirement age affect *potential retirees* and their co-workers. The information contained in the contribution histories allows to track workers across jobs and to observe their take-up of various Social Security programs. We can therefore study the consequences of the policy for all employees who are incumbent in our sample of firms at the reform date, including workers who leave the firm in the post-reform period. We start by discussing the effects of the policy on labor earnings. We document that labor earnings increase for *potential retirees*, whereas they decline for their co-workers. We then focus on the take-up of social insurance programs. The literature has documented that older workers substitute away from pension benefits into other Social Security programs in response to reforms that

change retirement rules (e.g. Duggan *et al.*, 2007 and Staubli and Zweimüller, 2013). We find similar evidence. On the other hand, spillovers on younger co-workers caused by labor substitutability have received less attention in the literature. This section documents these spillovers as a preliminary step to the quantification of the revenues collected in the short-run thanks to the reform from a firm's employees.

### 1.8.1 Earnings of Incumbent Employees

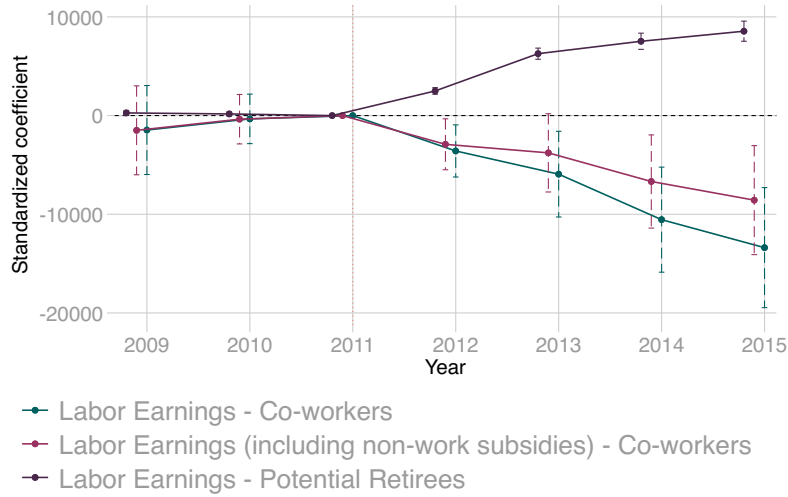
The first part of our analysis focuses on co-workers earnings. We define as co-workers all individuals who were not expected to retire within 3 years when the reform is passed and who worked in a firm with at least one *potential retiree*. We match every co-worker to the firm where she was incumbent at the time of the reform. We estimate (1.6) using total coworkers' labor earnings as dependent variable: specifically,  $Y_{it}$  is total labor earnings in year  $t$  of all co-workers who shared incumbency at firm  $i$  in the last quarter of 2011.<sup>44</sup> Labor earnings include income from other private employers, self-employment, and public-sector employment. The co-workers who were incumbent in more treated firms experience a decline in earnings after the reform, and the loss grows over time (Figure 1.5 and Table A.10, column 2). A  $1\sigma$  shift in the full retirement age of *potential retirees* at the firm (1.33 years) causes a drop of 13,379 euros in 2015, which is equal to 2.2 percent of average firm total labor earnings pre-reform. When adding non-work subsidies to labor earnings, the decline becomes smaller (1.4 percent).

Permanent contract middle-aged and older workers emerge as the most affected. Their earnings drop by 7,941 and 2,912 euros in 2015, 1.9 percent and 6.5 percent of the pre-reform averages, respectively. Earnings also drop for young workers, with a loss around 1.5 percent. In line with the adjustments on layoffs documented in the first part of the analysis, firms adjust the margin on older employees significantly more than the one on younger workers.

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<sup>44</sup>We collapse observations from the worker-year level to the firm-year level, by aggregating workers who shared incumbency at the same firm in the reform year. Since in this part of the analysis the focus is on workers' variables, we impose a milder restriction on firms: they have to remain open throughout the pre-reform period (2009-2011) rather than over the entire sample period. Hence, the number of observations is larger than in the previous analysis. Regressions are weighted based on firm size at the baseline quarter.

**Figure 1.5: Incumbents' labor earnings**



*Notes:* The figure shows the effect of a  $1\sigma$  increase in the full retirement date among *potential retirees* ( $T_i$ ) on total labor earnings of *potential retirees* and of their co-workers, alongside 95 percent confidence intervals. Standard errors are clustered at the firm level. Incumbent co-workers are individuals who were not expected to retire within 3 years when the reform is passed and who worked in a firm with at least one *potential retiree*. Labor earnings include earnings from private sector jobs, self-employment and public sector jobs. We winsorize the outcomes at the 99th percentile. The regression is based on specification (1.6) and is estimated on the universe of single-establishment private sector firms that (i) were active every year in the period 2009-2011, (ii) employed between 3 and 200 employees in the first quarter of 2009, and (iii) employed at least one *potential retiree* in the last quarter of 2011. Observations are weighted according to firm size at baseline. Number of observations = 511,847. Treatment std. dev. = 1.33. Mean outcomes pre-reform: labor earnings - co-workers = 621,032.16; labor earnings including non-work subsidies - co-workers = 621,137.28; labor earnings - *potential retirees* = 49,985.63.

However, since the decline in earnings moderates after accounting for non-work subsidies, the increase in layoffs documented in Section 1.6.1 can partly explain the earnings losses. To establish whether older workers actually experience a lower within firm earnings growth relative to young co-workers, we compute estimates of the cost of a job loss by age group. The ideal experiment to estimate the effect of a job loss would randomize such event across workers. In absence of such experiment, we match every worker separating after the reform to non-separated workers. We perform a coarsened exact match (CEM) along several covariates. To assess the cost of separation, we then perform a difference-in-differences analysis on the matched sample with a dummy treatment equal to 1 if the worker separates from the employer.<sup>45</sup> The estimated earnings drop is 5,057 euros three years after the separation for the average worker (Figure A.11). It amounts to a 22.7 percent decline, in line with estimates in Couch and Placzek (2010).<sup>46</sup> Older workers experience an average yearly 20 percent drop, whereas younger workers' drop is 16 percent. Since the increase in layoffs for the two cohorts is comparable (Figure 1.3, Panel B), the difference in the cost of job losses is not enough to explain a 5 percentage point difference in the earnings drop. We conclude that firms cut the wages of incumbent older workers significantly more, providing further evidence of a closer substitutability with *potential retirees*.

We also study earnings dynamics for *potential retirees*. As we did for co-workers, we match every *potential retiree* to the firm where she was incumbent at the reform date and we add labor earnings across all *potential retirees* incumbent in the same firm. Figure 1.5 and Table A.10 column 1 show an increase in their labor earnings, which is the natural consequence of a prolonged working life.

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<sup>45</sup>We add to the specification the matching covariates interacted with time fixed-effects. The covariates are age, sex, wage, occupation, type of contract, experience, sector, province, firm size. We also weight controls based on the standard CEM weights (see Iacus *et al.*, 2011). We discuss the weighting and further details about the match in Appendix A.6.

<sup>46</sup>Couch and Placzek (2010) revisit pioneering work by Jacobsen *et al.* (1993). They find that the earnings loss for displaced workers is around 30 percent after one year and 9 percent six years after the dismissal. See also Davis *et al.* (2011) and Farber (2017) for more recent estimates of the cost of job loss.

### 1.8.2 Spillovers to Government Programs

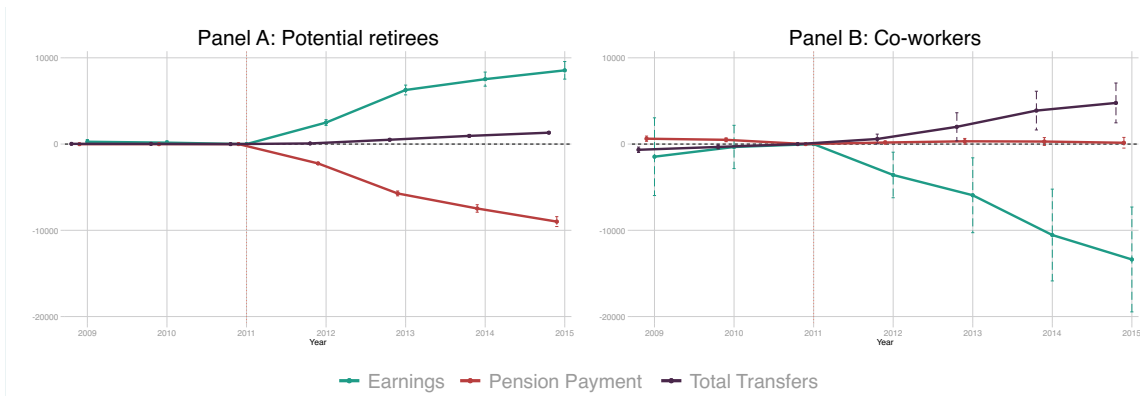
We study how the reform affects the take up of Social Security programs for *potential retirees* and their co-workers as a preliminary step to quantify the cost of the reform in the short-run. We focus on non-work subsidies, disability benefits, sick leave benefits and pension entitlements. Figure 1.6 reports the results for both categories of workers. *Potential retirees* and co-workers experience a spike in total Social Security transfers. The transfers increase by 1,321 euros for *potential retirees* and by 4,770 euros for co-workers in 2015. Table A.11 breaks down the cumulative effects of the various components of total transfers. A larger use of non-work subsidies - triggered by layoffs - drives most of the increase. For co-workers, the effects on the take-up of all the other programs are small and in most cases non-significant. *Potential retirees* increase the take-up of sick leave benefits. Importantly, they enroll more into disability insurance in response to the reform, showing a propensity to substitute away from full pension benefits and rely on other types of pensions. Staubli and Zweimüller (2013) document similar evidence on Austrian data.

Because the reform increases the full retirement age, we observe a drop of pension entitlements for *potential retirees*. Retirement benefits drop by almost 9,000 euros in 2015. Total firm-level savings on pension outlays in the first four years after the reform amount to 24,421 in the post-reform period - more than 8 times total extra transfers for *potential retirees*. There is zero effect on pension entitlements for co-workers, consistently with the fact that they were not expected to retire in the short-run before the reform is implemented.

## 1.9 Implications of substitutability for the revenues raised on employees

Our results document that - due to labor substitutability - the reform caused large spillovers on all incumbent workers, creating unintended costs. As a way to prove their relevance in the analysis of policy incidence, we develop a model to estimate the implications of these costs for the revenues collected on incumbent employees in the short-run. We estimate

**Figure 1.6:** *The effect of the reform on potential retirees and co-workers*



*Notes:* The figure shows the effect of a  $1\sigma$  increase in the treatment on *potential retirees'* and their co-workers' labor earnings, pension entitlements and total Social Security transfers, alongside 95 percent confidence intervals. Standard errors are clustered at the firm level. The firm-level regression is based on specification (1.6) and it includes firm and year fixed effects. Total transfers include the take-up of social insurance programs such as non-work subsidies, disability pensions, sick leave. We define *potential retirees* those workers who were expected to retire within three years under the pre-reform rules when the reform is implemented. Incumbent co-workers are non-*potential retirees* who are employed at a firm employing at least one *potential retiree* in the last quarter of 2011. Observations are weighted according to firm size at baseline. Treatment std. dev. = 1.33; Mean outcomes in the pre-reform period: *potential retirees'* labor earnings = 49,985.63 ; *potential retirees'* pension entitlements = 7.25; *potential retirees'* total transfers = 895.37; co-workers' labor earnings = 621,032.16; co-workers' pension entitlement = 2,880.04; co-workers' total transfers = 9,670.91.

the share of mechanically raised revenues due to public pension savings on incumbents, which is lost due to the behavioral responses of a firm and of its workers. This is the fiscal externality that stems from the channels that we observe. We incorporate the costs caused by labor substitutability into our model, which have so far received little attention in the literature. We then discuss the contribution of our results to the analysis of the welfare effect of the reform and their relevance for other public policies that reduce the incentives of older workers to leave the firm.

### 1.9.1 An accounting model

We construct a model of government accounting that considers two types of agents defined as in our empirical analysis with the labels of *potential retirees* ( $p$ ) and *co-workers* ( $c$ ). Agents perform different labor-related activities. The main activity is paid labor in a firm. A positive share of workers receives transfers such as non-work subsidies, short-time work benefits, disability benefits, benefits related to sickness or leave, or pension entitlements. The budget constraint for individual  $i$  is:

$$x_i \leq (1 - \tau_i) w_i l_i^w + \hat{T}_i \quad (1.7)$$

$x_i$  is consumption. Labor  $l_i$  in a firm is paid a wage  $w_i$  and taxed at rate  $\tau_i$ . The worker receives total transfers  $\hat{T}_i$  that depend on time spent in different labor and non-labor activities, including retirement. We describe the details of all the components of  $\hat{T}_i$  in Appendix A.7.

The fiscal externality is the share of mechanical revenues raised through the reform from a firm's employees that is lost because of the behavioral responses:

$$FE = - \frac{\text{Cost of Behavioral Responses}}{\text{Mechanical Public Pension Savings}} \quad (1.8)$$

The formula is derived in Appendix A.7. The numerator represents the costs incurred by the government because of behavioral responses. These costs occur because extra non-work subsidies are paid to fired employees, more workers enroll into disability insurance or other government programs, and lower tax revenues are raised from labor income when

incumbent workers face a drop in earnings. Mechanical revenues in the denominator are the resources that the government would save from a firm's employees because of the policy, absent any change in the behavior of the workers and the firm. Hence, they measure the savings that would arise if every *potential retiree* retired at the post-reform full retirement date. When  $FE$  is between -1 and 0 the reform generates an increase in the revenues collected. If the fiscal externality falls below -1, the government loses the entire mechanical revenues on incumbents because of behavioral responses. This is the case of local Laffer effects (Hendren, 2017 and Werning, 2007).

Our framework is highly stylized and ignores some of the general equilibrium effects of the policy. The model abstracts from the revenues lost on marginal workers who are not hired due to the reform. However, we provide estimates of these losses based on conservative assumptions in our calibrations. Due to the lack of balance-sheet information, we cannot incorporate the effect of the reform on firm's performance. To the extent to which the reform affects revenues and profits, our model misses their externalities on the government budget. We also lack information on *potential retirees* and other workers who are not employed in a firm at the time of the reform, but on whom the reform generates mechanical savings in pension outlays.<sup>47</sup> The reform could affect workers' health creating externalities for the public health system, for which we do not have data. Finally, our analysis focuses only on small and medium firms as the rest of the paper.

## 1.9.2 Empirical implementation and results

The fiscal externality is a function of the responses of labor earnings and Social Security transfers to the policy that we derived in Section 1.8. We provide a detailed description of the empirical implementation of (1.8) in Appendix A.7, with a discussion on the alternative calibrations that we implement.

Table 1.4 presents the main results where the standard errors of the estimates are

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<sup>47</sup>If a delayed full retirement age increases labor force participation and some of these workers find a job, extra revenues could be raised on their labor earnings.

bootstrapped via a wild bootstrap procedure with 1000 repetitions.<sup>48</sup> We start by computing the fiscal externality following the standard approach in the literature. This exercise illustrates what would happen if we ignored the spillovers on co-workers that are caused by labor substitutability and we focused only on the restricted sample of *potential retirees* who are directly affected by the reform. The first column of Table 1.4 shows that all estimates are close to zero, indicating that the government raises all the money that is mechanically saved through the policy on a firm's incumbent workers. Since the *potential retirees* work for longer per effect of the reform, they increase labor earnings (Figure 1.6). Tax revenues from the extra earnings are sufficient to offset the increase in Social Security transfers.

We add the spillovers on co-workers to the model in columns 2 to 4 and we show the fiscal externality estimates across alternative calibrations of the average tax rate. Point estimates range from -0.60 to -0.69, indicating that, even when we add spillovers, the savings on pension outlays overcome the cost of behavioral responses. However, the spillovers generate - and entirely explain - a non negligible loss of mechanical revenues. Revenues raised by the government are significantly lower than what the policy mechanically raises on the incumbent *potential retirees*. The reason is twofold. First, large spillovers arise because of labor substitutability, suggesting its importance for the cost of this reform. Co-workers and *potential retirees* experience an increase in non-work subsidies as a consequence of layoffs. Moreover, a reduction in the labor earnings of co-workers causes losses in labor tax revenues. Second, mechanical savings in pension outlays are raised only on the workers who were expected to retire within our horizon of analysis. As Figure 1.6 suggests, savings only come from *potential retirees*, who represent a small share of the workers in the sample. Over a longer time horizon, a larger share of the workforce will contribute to generate mechanical savings increasing the revenues from the policy.

Finally, we extend the model to provide a more conservative estimate of the fiscal externality. We have so far disregarded the tax revenue losses on marginally non-hired

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<sup>48</sup>We perform a block bootstrap that corrects residuals using the wild bootstrap procedure introduced by Wu (1986), Liu (1988) and Mammen (1993). This procedure allows to obtain asymptotic refinement for standard errors when residuals are correlated within firm and *iid* across firms.

**Table 1.4: Fiscal Externality**

|                                | <i>Potential<br/>retirees</i> | All<br>$\tau = 20$ | All<br>$\tau = 25$ | All<br>$\tau = 30$ | W/ Loss on<br>Non-Hired |
|--------------------------------|-------------------------------|--------------------|--------------------|--------------------|-------------------------|
| Median Pension                 | 0.010<br>(0.012)              | -0.606<br>(0.121)  | -0.626<br>(0.129)  | -0.648<br>(0.140)  | -0.680<br>(0.135)       |
| Mean Pension                   | -0.005<br>(0.013)             | -0.635<br>(0.122)  | -0.656<br>(0.131)  | -0.680<br>(0.142)  | -0.710<br>(0.136)       |
| Early Retirement = 0.9*Pension | -0.032<br>(0.013)             | -0.649<br>(0.117)  | -0.669<br>(0.126)  | -0.692<br>(0.137)  | -0.721<br>(0.131)       |

*Notes:* The table reports estimates of the fiscal externality based on the formula in (1.8). A negative externality between -1 and 0 implies that savings on pension outlays are larger than the revenue cost of behavioral responses. A positive fiscal externality implies that behavioral responses generate additional resources for the government on top of mechanical savings on pension spending. The first row calibrates  $P$  using the median pension (13,127 euros); the second uses the mean pension (16,279 euros); the third uses the median pension and calibrates  $P^e = 0.9 \times P$ . Column (1) reports the estimates for the fiscal externality that ignore the spillover on co-workers and sets  $\tau = 25$  percent. Columns (2) to (4) show calibrations with alternative levels of the average income tax rate (the average tax rate for the median income is 24 percent excluding tax credits). Column (5) reports estimates from a model that augments the formula in (1.8) assuming that every marginally non-hired worker earns zero labor earnings for as long as the median duration of unemployment for workers who eventually find a job over the 2012-2015 period, i.e. 13 months. We calibrate the foregone earnings by using the median value of the first 13 months wage of newly hired workers in the period 2012-2015 (i.e. 5560 euros) and we employ estimates on the effect of the treatment of new hires to calibrate the number of marginally non-hired workers. The tax rate is calibrated as  $\tau = 25$  percent. More details on calibrations and estimation are reported in Appendix A.7.

workers. To provide an upper bound to these losses, we assume that every marginally non-hired worker would earn no labor income for as long as the median duration (13 months) of unemployment for individuals who find a job in 2012-2015. We calibrate earnings losses using the median labor earnings of new hires in the 13-month period following the hiring event and we calibrate the number of marginally non-hired workers using estimates from the first part of the analysis.<sup>49</sup> In this conservative scenario, the fiscal externality is larger (column 5). It ranges from -0.68 to -0.72, indicating that more than 2/3 of mechanical savings are lost in the short-run.

Our results show that spillovers within the firm significantly affect the revenues raised on incumbent employees, changing our conclusions on the fiscal externality. Hence, the behavior of firms and labor substitutability are important to study the incidence of this reform. Substitutability between age cohorts has potentially important implications for

<sup>49</sup>We consider all workers, including those on short temporary contracts. The median value of earnings is 5,506 euros. We also set  $\tau$  at 25 percent in the calibration.

other policies that affect the incentives of older workers in a similar way. Examples are an increase in the early retirement age, lower monetary incentives for early retirement, lower monetary incentives for work after the full retirement date, changes in the criteria for disability insurance. All these policies extend the time that older employees spend at work, increasing their retention at the employer firm. The response of firms to an increased retention will affect incumbent workers who are substitutes to older workers, creating unintended consequences similar to those documented in our analysis.

### **1.9.3 Substitutability and welfare in the short-run**

We conclude by discussing how firms' behavior and labor substitutability affect the welfare effects of the policy. The reform welfare impact has two components that run into the future. The first is the fiscal cost of the policy. Because of its design, the reform will generate large savings for the government, especially in the long-term. The second component is the workers marginal willingness to pay for the policy. An important part of the willingness to pay depends on the extent to which future cohorts benefit from an extension of the retirement age.<sup>50</sup>

We cannot estimate the long-run welfare effects of the policy because we lack data on an extended time horizon. Even if the data was available, conclusions would strongly depend on general equilibrium effects that are hard to disentangle. Yet, we can highlight how the short-term components of welfare are affected by labor substitutability and firms' decisions. First, the workers marginal willingness to pay for the reform is affected by the spillovers caused by adjustments in labor demand. Involuntary unemployment plays an important role since workers are fired in response to the policy. Hence, a firm's responses have first-order utility effects, which depend on how much workers value employment. Estimates of the latter are hard to obtain in our context, but we expect the costs to be sizable given the large increase in layoffs of co-workers. We quantify part of the other welfare

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<sup>50</sup>The willingness to pay can be high since the policy improves the sustainability of the Social Security system.

component, measuring the short-run revenues collected due to the reform on incumbent employees. Since the reform - as many similar policies - was implemented during a budget crisis, this measure is a useful step to quantify the effects of the policy on the short-run government balance. By estimating the fiscal externality, we show the sizable effects of spillovers - caused by substitutability - on the government budget, which are important to derive implications for similar policies.

## **1.10 Conclusions**

This paper studies the importance of labor substitutability and firm's decisions for the incidence of an increase in the full retirement date. In contexts where the response of retirement choices to the full retirement age is high, the most proximate consequence of this policy for a firm is the increase in the retention of workers on the cusp of retirement. We develop a novel empirical strategy particularly effective for small and medium firms and we show that labor demand responses generate large spillovers for all incumbent workers. Our findings are consistent with firm-level substitutability among ages. In particular, older workers are the closest substitutes to the workers on the cusp of retirement. Incumbent middle-aged and older employees bear large part of the cost, running counter the idea that very young workers are the most affected cohort in the short-run. Spillovers within the firm also have significant implications for the revenues raised on incumbent employees through the reform. They cause all of the revenue losses in the first four years after implementation, indicating that labor substitutability and firm's decisions play an important role for studying the incidence of this and similar policies.

Our results show that the cost of the policy is redistributed at the firm-level. Disregarding within-firm spillovers would miss sizable consequences for workers who are not affected in the short-run, which are an important component of the welfare effects of the reform. So is the overall cost of the policy. Our findings suggest very different conclusions on the fiscal costs once spillovers are incorporated into the model. Despite firm's responses might have been amplified by adverse economic conditions, the relative contribution of spillovers to the

government revenues is unlikely affected by the economic cycle. In light of these findings, we argue that firms are an important vector for the passthrough of Social Security policies and thus should be included in welfare calculations. Clearly, our estimates cannot be directly extrapolated to other contexts. Yet, our results on substitutability extend to other policies that lower the incentives of older workers to leave the firm. Examples are increases in the early retirement age (Staubli and Zweimüller, 2013) or changes in the eligibility criteria of disability insurance (Staubli, 2011).

## Chapter 2

# Firm-level Tax Incidence: Redistribution of Labor Costs Within the Firm

### 2.1 Introduction

What is the effect of payroll taxes on wages? Standard approaches to this question relate the wage elasticity to payroll taxes to the elasticity of aggregate labor demand and supply in the economy. Under the implicit assumption of perfectly competitive labor markets, no role has been attributed to a single firm's labor costs. However, many empirical studies document the importance of employers in the wage setting process. There is substantial evidence of the propagation of shocks of different nature within the firm. Some examples are increases in profits, demand shocks, credit shocks, and workforce shocks.<sup>1</sup> Yet, little is known about how tax changes are absorbed within the firm. In this paper I revisit the standard wage tax incidence framework to document the role of firms in the redistribution of the burden of payroll tax increases.

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<sup>1</sup>See Card *et al.* (2014) for the redistribution of profits, Guiso *et al.* (2005) for the redistribution of economic risk within the firm, Garin and Silverio (2018) for the effects of demand shocks on wages, and Jäger and Heining (2019) for the effects of workforce shocks on wages.

How does a change in payroll taxes for some employees propagate within the firm? For instance, what happens to the wages of high-paid workers when payroll taxes are lowered for their low-paid co-workers? How does this firm-level redistribution affect the effectiveness of the policy? The answer to these questions provides an important step forward in understanding wage tax incidence and in evaluating the welfare effects of tax reforms.

To investigate these questions I exploit a quasi-experimental variation in payroll taxes induced by a reform passed in France by the Fillon's government in 2003. The Fillon's reform harmonized reduced social contributions in order to unify the different schemes that were previously in place for firms under the 39- and 35-hours per week regimes. The harmonization generated changes in the cost of labor up to more than 10% for low-paid workers, whose wages fell below two times the minimum wage. I evaluate the policy using administrative data for the universe of private sector firms provided by the French National Institute of Statistics and Economic Studies (INSEE). I link matched employer-employee data (DADS) to balance-sheet information (FICUS/FARE) in order to study wage dynamics within the firm.

I provide two main contributions. First, I propose a theoretical and empirical framework to understand the components of the elasticity of wages to tax changes. The model extends the standard tax incidence framework to account for labor market frictions and firm-level wage bargaining. I discuss - in relation to the model - the elasticities identified by standard empirical approaches from the literature on payroll tax incidence, showing that they do not identify the total elasticity of wages to taxes. Second, I develop a new estimator to identify the component of the wage elasticity that depends on the firm-level passthrough of labor costs. Using the Fillon's reform as a natural experiment I test for the existence of spillovers within the firm and uncover the total wage elasticity.

The standard tax incidence model implicitly relies on the assumption that labor markets are perfectly competitive. In this framework, the elasticity of wages to the payroll tax rate depends on aggregate labor demand and supply elasticities. When the supply is more

elastic, the burden of taxation falls more heavily on the employer. Firm-idiosyncratic shocks to the cost of labor do not affect the wages of incumbent workers since every employee gets paid the outside option wage, which does not change as a result of the idiosyncratic shock. Consistently with the model's assumptions, standard identification techniques rely on worker-level variation in taxes and strive to balance unobservable individual characteristics. One example is the use of regression discontinuity or regression at kink designs. However, if labor markets are imperfectly competitive - e.g. in the case of recruiting and firing costs - the firm must pay a premium over the outside option that potentially depends on total labor costs. The elasticity of this premium contributes to the total elasticity of wages to taxes. By balancing individual unobservables, the standard empirical approach cannot identify the latter component, but only the change in a worker's outside option. In order to identify the elasticity of the wage premium, one needs to exploit firm-idiosyncratic shifts in the cost of labor. Studying the passthrough of labor costs allows to determine the total wage elasticity and uncover spillovers of payroll taxes to workers who are not directly targeted by the tax change. These effects have important implications for the effectiveness of tax policies and for their welfare consequences. I document the existence of spillovers studying the Fillon's social security tax reform.

Studying firm responses to the Fillon's tax reform poses an identification challenge. Since the policy changed tax rates for low-paid workers, the extent to which a firm is affected depends on the concentration of low-paid employees in its workforce. However, firms with different concentrations of low-paid workers cannot be used as counterfactuals since a firm's wage distribution may reflect its wage dynamics absent the policy change. For this reason, I instrument the firm-level change in the cost of labor with the average change in the tax rate applied to the group of low-paid incumbent workers. I exploit the fact that heterogeneous changes in payroll tax rates apply to different wage levels for low-paid employees. The instrument has a direct economic interpretation as the change in the policy parameter shifted by the reform, i.e. the average payroll tax rate of low-paid employees. Thanks to this approach, the instrument variation is unrelated to the share of low-paid

workers in the firm, but only depends on the wage distribution of the subset of low-paid employees, which I show does not predict wage dynamics in the pre-reform period.

As a first stage, I document that the instrument causes changes in total social security contributions paid by firms in the post-reform period. I then focus on the effects of the tax decrease on incumbent workers. I start with a descriptive approach comparing average wage growth in groups of firms that are treated by the instrument with different intensities and that employ different shares of low-paid workers. I then include the instrument into a difference-in-differences model with multiple years before and after the reform. My estimates show that wage dynamics are balanced in the three years before the reform among firms that are treated with different intensities. After the Fillon's reform enters into effect I uncover a significant and negative effect of firm-idiosyncratic tax increases on incumbent workers' wages. A 1 percent increase in the average tax rate faced by low-paid workers causes a 1.5 percent drop in the wages relative to 2002 for the workers directly affected by the tax change, and a 1 percent decrease among the high-paid incumbent workers. Results suggest a significant redistribution of the cost/benefit of payroll taxes at the firm-level. The government incurs into unintended costs when transferring resources to low-paid workers since part of every dollar given to low-paid employees through lower taxes is redistributed to high-paid co-workers.

This paper relates to the literature on payroll tax incidence. A classic literature developed the theoretical tools to analyze the impact of social security contributions on wages (Musgrave, 1959; Atkinson and Stiglitz, 1980; Kotlikoff and Summers, 1987; Fullerton and Metcalf, 2002). Early empirical studies using within country variation deliver mixed evidence (Brittain, 1972; Feldstein, 1972; Hamermesh, 1979; Holmlund, 1983).

In more recent years, a first wave of papers using micro-data has showed full shifting of payroll taxes to employees (Gruber and Krueger, 1991 on the US compensation insurance; Gruber, 1994 on US maternity mandated benefits; Gruber, 1997 on Chile; Anderson and Meyer, 1997 and Anderson and Meyer, 2000 for the US unemployment insurance payroll tax; Cruces *et al.*, 2010 on Argentina). These results have been challenged by a more

recent number of papers that show only partial shifting to the employee. Kugler and Kugler (2009) find that only 14 to 23 percent of a large payroll tax increase in Colombia is passed to employees. Saez *et al.* (2012) show evidence in favor of the relevance of statutory incidence, in contrast with the standard incidence theory view. Exploiting a cohort-based discontinuity in social security contributions tax rates, they show full passthrough of employers' contributions to employers and of employees' contributions to employees. Bozio *et al.* (2017) also find evidence against full tax shifting to workers after reforms that increased social security contributions with little or no tax-benefit linkage. On the other hand, they document full shifting when there is a strong and salient tax-benefit linkage.

This paper takes a firm-level perspective to the analysis of payroll taxes on low-paid workers. Hence, I relate to the literature studying the impact of payroll tax policies designed to foster employment of some categories of workers.<sup>2</sup> Katz (1998) focuses on US wage subsidy policies for the disadvantaged. Closely related to my paper, Kramarz and Philippon (2001) show a positive effect of payroll tax cuts for minimum wage workers on their employment probability. More recently, Saez *et al.* (2019) document the effects of a payroll tax cut targeting young workers in Sweden.<sup>3</sup> They show no effect on the net wages of young workers treated by the policy. Similar in spirit to my work, they find that firms with higher concentration of young workers exploit the tax windfall to increase employment, capital, sales, and profits. Importantly, firms redistribute the benefits of lower payroll taxes among all incumbent workers. Along these lines, Benzarti *et al.* (2019) exploit a discontinuity in Finland that allows entrepreneurs not to pay social insurance contributions to show that relaxing social insurance mandates allows entrepreneurs to increase cash in their firms and affects the business activity.

The rest of the paper is organized as follows. Section 2.2 presents the conceptual

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<sup>2</sup>Neumark (2013) surveys the literature on hiring credits with a specific focus on the design of such policies during severe recessions. In a recent paper, Cahuc *et al.* (2019) study the effects of tax credits in France during the Great Recession.

<sup>3</sup>Earlier papers on the same payroll tax reform are: Bennismarker *et al.* (2014), Skedinger (2014), Malm *et al.* (2016), and Kaunitz and Egebark (2017), Egebark and Kaunitz (2013, 2018).

framework; section 2.3 describes the reform I use as a quasi-experiment. I present the data in section 2.4 and present the empirical framework and identification assumptions in section 2.5.

## 2.2 Conceptual Framework

I develop a theoretical framework to analyze the role of firms for the incidence of payroll tax changes. I nest the standard theoretical framework of tax incidence into a model with imperfectly competitive labor markets and I discuss the results of the extended framework drawing a comparison between the two.

I consider a firm employing different types of labor  $\mathbf{L} = (L_1, L_2, \dots, L_N)$ . There is a perfectly competitive external labor market for each type. When a worker of type  $i$  exits the firm and enters the external labor market she receives a competitive wage  $\underline{w}_i$ . The competitive wage depends on aggregate labor demand and supply in the economy. Every type of labor is taxed at rate  $\tau_i$ , which in this example is a payroll tax on the employer.<sup>4</sup> The vector of taxes  $\boldsymbol{\tau} = (\tau_1, \tau_2, \dots, \tau_N)$  affects labor demand and supply that determine the outside option  $\underline{w}_i(\boldsymbol{\tau})$  of type  $i$ . Competitive wages are such that labor demand equals labor supply on the external labor market. Hence,  $D_i^{hire}((1 + \tau_i) \underline{w}_i) = S_i(\underline{w}_i)$  so that the demand for  $i$  depends on  $\tau_i$  since I have assumed that the statutory incidence falls on the employer.<sup>5</sup> The elasticity of the competitive wage  $\underline{w}_i$  to payroll tax  $\tau_i$  is

$$\varepsilon_{\underline{w}_i, 1+\tau_i} = \frac{\varepsilon_{D_i, (1+\tau_i)\underline{w}_i}}{\varepsilon_{S_i, \underline{w}_i} - \varepsilon_{D_i, (1+\tau_i)\underline{w}_i}} \quad (2.1)$$

Equation (2.1) represents the standard tax incidence formula in a model with competitive labor markets. When labor demand is very elastic, the elasticity of competitive wages to the tax rate is larger and the cost of a tax increase falls mostly on the workers. On the contrary,

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<sup>4</sup>The model can be easily extended to incorporate payroll taxes on the employee.

<sup>5</sup>The model can be extended to relax this assumption and let demand depend on the vector of after-tax competitive wages  $(\mathbf{1} + \boldsymbol{\tau}) \cdot \underline{\mathbf{w}}$ . This would be the case if there were complementarities between different types of labor, which would determine a change in the demand for type  $j$  in response to an increase in  $\tau_i$ .

when labor supply is very elastic, the firm bears a larger part of the cost and  $\underline{w}_i$  is less responsive. Full passthrough on workers occurs when labor supply is perfectly inelastic.

In an economy without labor market frictions, (2.1) captures the incidence of taxes on wages. However, tax incidence changes in the presence of frictions or imperfectly competitive labor markets. I model the latter assuming imperfect substitutability between incumbent workers and new hires. Imperfect substitutability can arise for several reasons including fire costs (e.g. severance pay); hire costs (e.g. cost of screening, cost of posting vacancies); or firm-specific human capital (e.g. training for firm-specific technologies). Production in firm  $j$  occurs according to  $f(\mathbf{L}_j^{\text{inc}}, \mathbf{L}_j^{\text{hire}})$  where  $(\mathbf{L}_j^{\text{inc}}, \mathbf{L}_j^{\text{hire}})$  are the vectors of incumbent workers and new hires. Total firm's profits are

$$\Pi_j(\mathbf{L}_j^{\text{inc}}, \mathbf{L}_j^{\text{hire}}) = f(\mathbf{L}_j^{\text{inc}}, \mathbf{L}_j^{\text{hire}}) - \sum_{i=1}^T (1 + \tau_i) \underline{w}_i L_{ij}^{\text{hire}} - \sum_{i=1}^T (1 + \tau_i) w_{ij} L_{ij}^{\text{inc}} \quad (2.2)$$

labor costs are influenced by taxes on the employer. I assume that each type of incumbent worker bargains separately with the firm over the wage, while hours of work are taken as given. Every incumbent worker can leave the firm and earn wage  $\underline{w}_i$ . Hence, the firm must pay a wage greater or equal than  $\underline{w}_i$  to retain the worker. If incumbent workers of type  $i$  leave the firm, profits would be  $\Pi(\mathbf{L}_{-ij}^{\text{inc}}, \mathbf{L}_j^{\text{hire}})$  where  $\mathbf{L}_{-ij}^{\text{inc}} = (L_{1j}^{\text{inc}}, L_{2j}^{\text{inc}}, \dots, L_{ij}^{\text{inc}} = 0, \dots, L_{Tj}^{\text{inc}})$ . The firm and incumbent workers bargain over the wage according to Nash bargaining where the worker has bargaining power  $\beta \in (0, 1)$ . As a result of bargaining, the worker will receive a gross wage equal to her outside option plus a share of the total per capita surplus generated by the match

$$\begin{aligned} w_{ij} &= \frac{\beta}{L_i^{\text{inc}} (1 + \tau_i)} \left( \tilde{\Pi}_j^* (\mathbf{L}_j^{\text{inc}}, \mathbf{L}_j^{\text{hire}}) - \tilde{\Pi}_j^* (\mathbf{L}_{-i}^{\text{inc}}, \mathbf{L}_j^{\text{hire}}) - (1 + \tau_i) \underline{w}_i L_i^{\text{inc}} \right) + \underline{w}_i \\ &= \underbrace{\Omega_{ij}(C_j)}_{\text{Quasi-rent}} + \underbrace{\underline{w}_i(\tau)}_{\text{Outside option}} \end{aligned} \quad (2.3)$$

where  $\tilde{\Pi}_j^* (\mathbf{L}_j^{\text{inc}}, \mathbf{L}_j^{\text{hire}}) = \max_{\mathbf{L}_j^{\text{hire}}} f(\mathbf{L}_j^{\text{inc}}, \mathbf{L}_j^{\text{hire}}) - \sum_{i=1}^T (1 + \tau_i) \underline{w}_i L_{ij}^{\text{hire}}$  is the value of profits net of incumbent labor costs earned by a firm that optimally chooses  $\mathbf{L}_j^{\text{hire}}$  given

an initial vector of incumbent workers  $L_j^{\text{inc}}$ . The wage is given by the sum of two terms: the outside option in the competitive labor market and a quasi-rent that depends on the existence of imperfect substitutability between incumbents and new hires. The quasi-rent generates from a hold-up problem and depends on firm  $j$ 's labor costs  $C_j$ . Although it is influenced by complementarities between worker types, the quasi-rent only arises because of imperfect substitution between incumbent and new hires. Appendix (B.1) shows that under perfect substitutability the model would become a perfectly competitive labor market model.

When  $\tau_i$  increases the effect on net wages at firm  $j$  is the sum of two terms

$$\frac{\partial w_{ij}}{\partial \tau_i} = \underbrace{\frac{\partial \Omega_{ij}}{\partial C_j} \frac{\partial C_j}{\partial \tau_i}}_{\text{Passthrough of labor costs}} + \underbrace{\frac{\partial w_i}{\partial \tau_i}}_{\text{Change in the outside option}} \quad (2.4)$$

The first term is determined by the presence of frictions in the labor market and it captures the passthrough of firm-specific labor costs. To identify this term in the data I will exploit quasi-random variation in labor costs across firms. The second term is the change in the outside option. Section 2.5.1 will discuss how standard identification strategies identify only the second of the two terms. Importantly, the passthrough term generates spillovers of tax increases to workers who are not directly targeted by the tax reform. The effect of a change in  $\tau_i$  on worker's  $k$  wage is

$$\frac{\partial w_{kj}}{\partial \tau_i} = \underbrace{\frac{\partial \Omega_{kj}}{\partial C_j} \frac{\partial C_j}{\partial \tau_i}}_{\text{Passthrough of labor costs}} + \underbrace{\frac{\partial w_k}{\partial \tau_i}}_{\text{Change in the outside option}} \quad (2.5)$$

Under the assumption that there is no change in the outside option of worker  $k$  when  $\tau_i$  changes, the passthrough term fully captures the incidence of the tax on  $w_k$ .

**Result 1:** *Wages respond to changes in a firm-specific labor costs only if the labor market is not perfectly competitive.*

For this reason, the presence of a positive passthrough elasticity provides an indirect test of

the existence of a perfectly competitive labor market (Garin and Silverio 2018).

An interesting case arises when the elasticity of labor supply is zero and there is full pass-through to the outside option. When this happens, the outside option  $\underline{w}_i$  absorbs the entire cost of an increase in the tax and gross wages of incumbent workers do not change in response to the tax. Workers whose tax increases bear the entire cost and there is no spillover on incumbent workers who are not targeted by the policy.

**Result 2:** *In case of full passthrough of a tax increase to the targeted workers, there is zero spillover on workers who are not affected by the tax change.*

The proof of the result is straightforward from the fact that quasi-rents are unaffected when gross wages do not change.

## 2.3 Quasi-natural experiment: the Fillon's social security reform

I exploit variation in payroll tax rates caused by the Fillon's social security reform implemented in France in 2003. The reform was implemented with the goal of harmonizing reductions in social security contributions applied to low-wage employees and paid by the employer. Following the Aubry laws of 1998 and 2000, two working hour regimes coexisted in France. The first was the old 39-hours regime, the second was the 35-hours regime that had been introduced with the Aubry laws. Different reductions to social security contributions applied to the two regimes. Firms under the 39-hour week benefited from the low-wage tax credits, while firms adopting the 35-hour week enjoyed the more generous working-time reduction tax cut.

**Table 2.1:** *Fillon's Reform Rules*

| Year | 35h                                             | 39h                         |
|------|-------------------------------------------------|-----------------------------|
| 2002 | Max: 0.26; Limit: 1.73*GMR2, stable at 3% above | Max: 0.186; Limit: 1.3*SMIC |
| 2003 | Max: 0.26; Limit: 1.7*GMR2                      | Max: 0.208; Limit: 1.5*SMIC |
| 2004 | Max: 0.26; Limit: 1.6*GMR2                      | Max: 0.234; Limit: 1.6*SMIC |
| 2005 | Max: 0.26; Limit: 1.6*SMIC                      |                             |

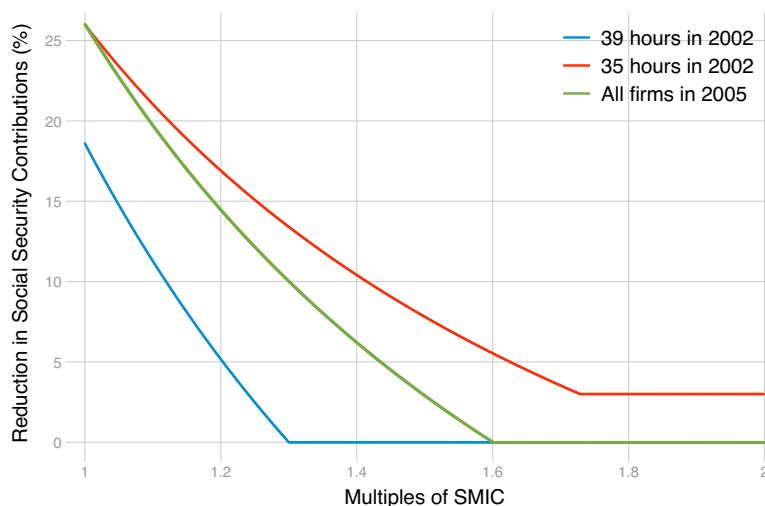
*Notes:* the Table reports the rules of the Fillon's reform for 39 and 35-hours firms. The reform was introduced in 2002 and the harmonization was gradually introduced in the period 2003-2005.

The Fillon reform unified reduced social security contributions across the two regimes generating an increase in payroll taxes for 35-hour firms and a decrease for 39-hours firms. The shift of payroll taxes depended on the worker's wage. Reductions to social security contributions applied to 39-hour firms starting from 18.6% at the minimum wage (SMIC) and decreasing linearly to 0% at 1.3 times the minimum wage (see Figure 2.1 and Table 2.1).<sup>6</sup> For 35-hours firms they were 26% at the GMR (a minimum wage for 35-hour firms) linearly decreasing until 1.73 times the GMR where the rate was 3%.<sup>7</sup> The post reform tax rate (effective starting in 2005) was 26% at the SMIC and decreased linearly to 0% at 1.6 times the SMIC. As a result of the reform rules, heterogeneous changes involved low-paid workers depending on their wage. In section 2.5.2 I exploit this variation to compute firm-level virtual changes in labor costs.

<sup>6</sup>SMIC stands for *Salaire Minimum National Interprofessionnel Garanti* (minimum guaranteed wage across all professions).

<sup>7</sup>The GMR (*garanties mensuelles de rémunération*) is a hourly minimum wage for 35-hour firms. When the 35-hour regime was introduced a new minimum wage was created for firms converting to the new regime. GMR was built multiplying the SMIC (applied to 39-hour firms) by 169 hours to convert it on a monthly basis.

**Figure 2.1:** *Reduced Social Security Contributions - Pre and Post Fillon's Reform*



*Notes:* the Figure plots the reduction in social security contributions determined by the Fillon's reform for 39 and 35-hours firms. The reductions are a function of the gross wage.

## 2.4 Data

I leverage three main data sources. First, I use matched employer-employee data from the DADS (*Déclaration Annuelle de Données Sociales*) to define the treatment variables and to study wage dynamics at the firm. Second, I use FICUS/FARE data to gather information about the size of the firm and balance sheet data. Third, I use ACOSS (*Agence centrale des organismes de Sécurité Sociale*) databases to identify establishments receiving reductions in social security contributions.

### 2.4.1 The DADS matched employer-employee and panel data

DADS data comes from the compulsory declarations made annually by all employers for each employee with the purpose of collecting useful information for the payment of social security programs, including pensions. The data is constructed by the French National Institute of Statistics (INSEE) and it covers the universe of French private sector firms. While

it contains firm's and establishment's identifiers, it does not report individual identifiers. The database consists of a repeated cross-section of all the active contracts with a one-year lag that allows to track every contract's information for two consecutive years. The absence of individual identifiers makes it impossible to track job transitions across firms. To deal with the lack of individual identifiers, INSEE has built a panel version of the DADS for a 1/25 sample of private sector employees born in October of even-numbered years (about 1.1 million workers). The panel has been extended to include 1/12 of all private sector workers after 2002 (about 2.2 million workers).

DADS contains the firm and establishment identifiers; establishment location; contract type (full-time vs part-time, temporary vs permanent); hours worked; workers' demographics (age and gender). The database reports measures of gross and net wages for every contract. Gross wages include all the mandatory social security contributions, but exclude social security contributions paid by the employer.<sup>8</sup> Net wages measure the wage after the deduction of social security contributions, pension and supplementary pension plans contributions, but include personal income taxes.

#### **2.4.2 FICUS/FARE balance sheet data**

FICUS/FARE data is collected and provided by DGFIP (Direction Générale des Finances Publiques) and INSEE. The information comes from the compulsory firm's declarations and from the income statements submitted to the fiscal authorities. It covers the universe of private sector firms for the entire sample period. I gather from this data information about firm's balance sheet including profits, investments, revenues, value added and total workforce. Since the information comes from financial statements, contrarily to DADS data there is no information on the establishment.

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<sup>8</sup>Gross wage is measured starting from the tax base of the generalized social security contribution (Contribution sociale généralisée, CSG), which also includes remunerations in the form of stock options, or profit-sharing plans.

### 2.4.3 ACOSS social security data

The Central Agency of Social Security Organizations (ACOSS) collects establishment-level information on social security contributions, workforce and wage bills. I access four main sources of data: BRC, SEQUOIA, AROME and ORME. They contain detailed data on payroll taxes paid by the employer, reductions to social security contributions (by category), the number of workers receiving these reduction, workforce numbers and total labor costs. I use this data to measure reductions in social security contributions received by the employer and classify firms as 35 or 39-hours depending on the type of subsidy received in the sample period.

## 2.5 Empirical Strategy

I take advantage of a social security reform implemented in 2003 by the Fillon's government that changed payroll tax rates for low-paid employees. I first outline the empirical framework relating the model in section 2.2 to the empirical implementation (section 2.5.1); then I present the quasi-experimental variation exploited by my strategy and I discuss the potential threats to identification (section 2.5.2). I then explain the instrumental variable strategy I adopt to overcome the endogeneity issues (section 2.5.3). Finally, I discuss the empirical specifications that I use throughout the analysis and I describe the analysis sample (section 2.5.4).

### 2.5.1 Empirical framework

Standard approaches to the study of tax incidence exploit individual-level variation in tax rates to identify the impact of payroll taxes on wages. Suppose the wage of a worker  $i$  employed in firm  $j$  taxed with rate  $\tau$  is

$$w_{ij} = \underline{w}_i(\tau) + \eta_{ij} \quad (2.6)$$

The error term can be written as  $\eta_{ij} = \rho_i(C_j) + \varepsilon_{ij}$ , where  $C_j$  is the total labor cost in firm  $j$  and  $\varepsilon_{ij}$  captures other unobservables. Suppose a tax change applies to a subset of the population such that some workers face tax  $\tau'$  and some others face an increase in taxes  $\delta\tau'$ . Standard estimators have the following structure:

$$\begin{aligned} E[w_{ij} | \tau = \tau' + \delta\tau'] - E[w_{ij} | \tau = \tau'] &= \underline{w}_i(\tau' + \delta\tau') - \underline{w}_i(\tau') \\ &+ E[\rho_i(C_j) | \tau = \tau' + \delta\tau'] - E[\rho_i(C_j) | \tau = \tau'] \\ &+ E[\varepsilon_{ij} | \tau = \tau' + \delta\tau'] - E[\varepsilon_{ij} | \tau = \tau'] \end{aligned} \quad (2.7)$$

Identification techniques applied to this standard case make sure that the tax rate increase is as good as randomly assigned. Hence, firm-level labor cost and other unobservables are balanced between treatment and control groups so that  $E[\rho_i(C_j) | \tau = \tau' + \delta\tau'] - E[\rho_i(C_j) | \tau = \tau'] = 0$  and  $E[\eta_{ij} | \tau = \tau' + \delta\tau'] - E[\eta_{ij} | \tau = \tau'] = 0$ . For instance, this is achieved in a regression discontinuity design (e.g. Matsaganis *et al.* 2012). This approach identifies the effect of a tax change on two workers whose unobservables are balanced. In other words, the identification approximates an experiment that randomizes individual tax changes across workers employed in identical firms. In an economy where wages are the sum of a worker's outside option and a worker-firm component (as in 2.3), the identified effect is the change in the outside option of the worker.

To estimate the total effect of the tax on the wage of incumbent workers, one needs to study how changes in the labor cost pass through to wages. This is achieved by comparing workers facing heterogeneous changes in labor costs:

$$\begin{aligned} E[w_{ij} | \tau, C_j = C'_j + \delta C'_j] - E[w_{ij} | \tau, C_j = C'_j] &= \rho_{ij}(C'_j + \delta C'_j) - \rho_{ij}(C'_j) \\ &+ E[\varepsilon_{ij} | C'_j + \delta C'_j] - E[\varepsilon_{ij} | C'_j] \end{aligned} \quad (2.8)$$

The identification of this effect relies on the assumption that changes in the cost of labor are as good as randomly assigned, i.e.  $E[\varepsilon_{ij} | C'_j + \delta C'_j] - E[\varepsilon_{ij} | C'_j] = 0$ . Importantly, equation 2.8 holds for any level of the tax rate  $\tau$ , which implies that the variation in labor costs can be used to identify the causal effect of a tax reform on workers who are not directly

affected by changes in the payroll tax rate.

To identify shifts in the labor cost, I will employ a two-stages estimator of the following form

$$\frac{E \left[ w_{ij} \mid \tau, t_j = t'_j + \delta t'_j \right] - E \left[ w_{ij} \mid \tau, t_j = t'_j \right]}{E \left[ C_j \mid \tau, t_j = t'_j + \delta t'_j \right] - E \left[ C_j \mid \tau, t_j = t'_j \right]} = \rho'_i \left( C'_j \right) \delta C'_j$$

where  $t_j$  represents a generic payroll tax rate that could apply to worker  $i$  or to other workers in the same firm. Since the ultimate goal of the analysis is to estimate the total wage elasticity to a tax rate change, the relevant elasticity is estimated by the reduced-form. Yet, studying the pass-through of labor costs is useful to shed light on the role of firms for the transmission of the effects of payroll tax changes. Next section outlines the strategy I use to identify the causal effect of changes in the firm's labor cost.

**Table 2.2: Descriptive Statistics**

|                                               | Mean    | Median | p 10   | p 90   |
|-----------------------------------------------|---------|--------|--------|--------|
| <b>Panel A: Analysis Sample</b>               |         |        |        |        |
| Social Security Contributions (per capita)    | 21.7    | 11.33  | 6.60   | 26     |
| Hourly Gross wage (median)                    | 10.7    | 9.89   | 7.94   | 14.08  |
| Hourly Net wage (median)                      | 8.4     | 7.75   | 6.24   | 11.06  |
| Employment                                    | 10.51   | 7      | 3      | 18     |
| Hourly Net wage (directly affected employees) | 6.9     | 6.86   | 6.07   | 7.75   |
| Hourly Net wage (co-workers)                  | 16.98   | 15.22  | 11.48  | 24.14  |
| Total revenues                                | 2853.01 | 928    | 316    | 5957   |
| Profits                                       | 833.33  | 374    | 145    | 1629   |
| Investments                                   | 42.51   | 6      | -16.29 | 132.83 |
| $\Delta \bar{\tau}_j$                         | -0.05   | -0.05  | -0.09  | -0.02  |
| $\Delta C_j$                                  | -0.03   | -0.03  | -0.06  | 0      |
| Share of co-workers                           | 3.18    | 3      | 2      | 4      |
| Share of directly affected workers            | 2.96    | 3      | 2      | 4      |
| Employment (in 2000)                          | 10.41   | 6      | 3      | 18     |
| <b>Panel B: Universe of 39-Hours Firms</b>    |         |        |        |        |
| Social Security Contributions (per capita)    | 27.48   | 11.5   | 6.29   | 33.67  |
| Hourly Gross wage (median)                    | 11.35   | 9.78   | 7.74   | 15.88  |
| Hourly Net wage (median)                      | 8.91    | 7.67   | 6.08   | 12.46  |
| Employment                                    | 8.98    | 6      | 3      | 15     |
| Hourly Net wage (directly affected employees) | 6.86    | 6.8    | 6      | 7.76   |
| Hourly Net wage (co-workers)                  | 17.57   | 15.49  | 11.56  | 25.32  |
| Total revenues                                | 2833.07 | 807    | 272    | 6003   |
| Profits                                       | 781.13  | 331    | 126    | 1659   |
| Investments                                   | 42      | 5      | -15    | 124    |
| $\Delta \bar{\tau}_j$                         | -0.05   | -0.05  | -0.09  | 0      |
| $\Delta C_j$                                  | -0.03   | -0.03  | -0.08  | 0      |
| Share of co-workers                           | 2.92    | 3      | 1      | 4      |
| Share of directly affected workers            | 2.89    | 3      | 1      | 4      |
| Employment (in 2000)                          | 9.01    | 5      | 3      | 15     |

Notes: the Table reports descriptive statistics. The statistics refer to the period 2000-2002 for firms included in the analysis sample (Panel A), and all firms that adopted the 39-hours regime in 2002 (Panel B).

### 2.5.2 Reform-induced changes to labor costs

The Fillon's reform harmonized reduced social security contributions for low-wage employees across 35 and 39-hours firms. The harmonization caused firm-level shifts in labor costs that depend on the wage distribution of low-wage incumbent employees in 2002. Denote  $\delta C(w_{2002})$  the change in labor cost for a low-wage worker with wage  $w_{2002}$  in 2002 (before the reform becomes effective). Changes in labor costs occur for all the workers in the interval  $[w_{min}, \bar{w}]$ , where  $w_{min}$  is the minimum wage (SMIC) and  $\bar{w}$  is the highest wage to which the harmonization applies. The total percentage change in labor costs for firm  $j$  is

$$\Delta C_j = \sum_{w_{2002} \in [w_{min}, \bar{w}]} \frac{N_j(w_{2002})}{N_j} \frac{\delta C(w_{2002})}{\bar{C}_{j,2002}} \quad (2.9)$$

where  $N_j(w_{2002})$  is the number of workers with wage  $w_{2002}$  employed in firm  $j$  and  $\bar{C}_{j,2002}$  is the per-worker labor cost in 2002 in firm  $j$ .<sup>9</sup> The total change in labor costs depends on the share of employees with  $w_{2002} \in [w_{min}, \bar{w}]$  and on their wage distribution within this range. Hence, the concentration of low-wage employees in firm  $j$  determines the reform-induced shift in total labor costs. Firms with different concentrations of low-paid workers may not be good counterfactuals to each other. For instance, the firm-level wage distribution is correlated with the type of activity or with the firm's location. High-tech industries have lower concentrations of low-paid employees relative to manufacturing firms and wages tend to be higher in urban relative to rural areas. All these factors could be confounders for an analysis motivated by the goal of investigating the effects of increases in labor costs on the wages of incumbent employees. To solve this issue, I find an instrument that exploits the reform quasi-experimental variation, but avoids variation in the broad wage distribution of the firm. Hence, the instrument allows me to compare firms with similar wage structure that face heterogeneous changes in the labor costs of low-wage employees.

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<sup>9</sup>Equation 2.9 relies on the assumption that the wage interval  $[w_{min}, \bar{w}]$  can be discretized.

### 2.5.3 Instrumenting the total change in labor costs

I instrument the total change in firm  $j$ 's labor costs with the average tax rate change for the low-paid incumbent employees in  $j$  in 2002. This variable measures a firm-level change in the policy parameter (the tax rate of low-wage workers) that has been shifted by the reform. Denote  $\delta\tau(w_{2002})$  the payroll tax rate change for workers with wage  $w_{2002}$  in 2002. The instrument is

$$\Delta\bar{\tau}_j = \sum_{w'_{2002} \in [w_{min}, \bar{w}]} \frac{N_j(w'_{2002})}{N_j(w_{2002} \in [w_{min}, \bar{w}])} \delta\tau(w'_{2002}) \quad (2.10)$$

where  $N_j(w_{2002} \in [w_{min}, \bar{w}])$  is the number of incumbent employees with wage in the range  $[w_{min}, \bar{w}]$  in 2002. Since the instrument captures an average change in the tax rate, it does not depend on the concentration of workers in the left tail of the wage distribution. On the contrary, it reflects variation in the distribution of wages within this lower tail. For this reason, variation in  $\Delta\bar{\tau}$  persists even after controlling for the firm-level wage distribution. Equation (2.10) shows that the instrument is of the shift-share form. Shares are measured by the term  $N_j(w'_{2002}) / N_j(w_{2002} \in [w_{min}, \bar{w}])$ , while the shifts  $\delta\tau$ s - unlike in most applications of the Bartik instrument - are measured in the content of the reform. Instrument variation across firms arises from differences in the combination of shares, which determines the empirical distribution of wages within the reform range  $[w_{min}, \bar{w}]$ .

### 2.5.4 Empirical specification and sample definition

I study the effects of changes in labor costs on the wages of incumbent workers by estimating a multiple period difference-in-differences model with a continuous treatment. The specification reads

$$Y_{jt} = \lambda_j + \gamma_t + \sum_{k=2000}^{2008} \beta_k I(k=t) \Delta C_j + \sum_{k=2000}^{2008} \delta_k I(k=t) X_j + \varepsilon_{jt} \quad (2.11)$$

where  $Y_{jt}$  is a firm-level outcome of interest such as the average wage of high or low-paid incumbent employees. I introduce firm and year fixed-effects denoted respectively by  $\lambda_j$  and

$\gamma_t$ . Firm fixed-effects capture time invariant heterogeneity across firms including differences in the average level of  $Y_j$ . Identification relies on the assumption that more and less treated firms would have parallel trends in  $Y$  absent the reform. The coefficients  $\{\beta_k\}_{k=2000}^{2002}$  serve as placebo tests for the existence of parallel trends in the pre-reform period. Testing for their significance allows to establish if firms that are differentially exposed to the reform have different trends in wage dynamics. I control for a set of firm covariates  $X_j$  measured at the baseline year (i.e. 2000) and interacted with year fixed-effects. Hence, I allow for non-parallel trends in  $Y$  as long as they can be entirely explained by differences in the vector of covariates  $X$ . I cluster standard errors at the firm level to address the potential concern of serial correlation across periods (Bertrand *et al.*, 2004).

The coefficients  $\{\beta_k\}_{k=2003}^{2008}$  describe the effect of shifts in the cost of labor on the outcome  $Y$  in the post-reform period. I instrument  $\Delta C_j$  with the average shift in the tax rate  $\Delta \tilde{\tau}_j$ . Exclusion restriction relies on the assumption that  $\Delta \tilde{\tau}_j$  does not correlate with time-varying unobservable characteristics summarized by the error term  $\varepsilon_{jt}$ . This assumption would fail if the distribution of low-paid workers' wages was correlated to unobservable factors that are predictive of the firm's wage dynamics. A sign of the failure of this assumption is the presence of differential trends in wage dynamics in the pre-reform period. For instance, differential trends could arise if the reason for a high  $\Delta \tilde{\tau}_j$  was that firm  $j$ 's low-paid workers' wages were close to the minimum wage and if this distribution depended on very slow wage growth for this group of workers in the pre-reform period. I test for the existence of differential trends in wage dynamics throughout the analysis.

I estimate the specification on the universe of private sector firms that are active every year in the period 2000 to 2008. I restrict the sample to firms that employ between 3 to 300 employees and at least one employee whose wage in 2002 is subject to changes in payroll taxes because of the reform. To better study spillovers within the firm, I focus on 39-hours firms where payroll taxes are unchanged for high-paid workers, which allows me to isolate a wage effect on workers who are not directly affected by the policy. These restrictions leave me with a sample of almost 120,000 firms. Table 2.2 compares firms in this sample to the

universe of 39-hours firms. Firms in my master sample pay slightly lower median hourly wages. The difference is entirely due to the salaries of high-paid workers, which are higher in the universe of 39-hours firms. The firms in my sample are more profitable and larger in size than the average firm in the universe, but their total investments are very similar.

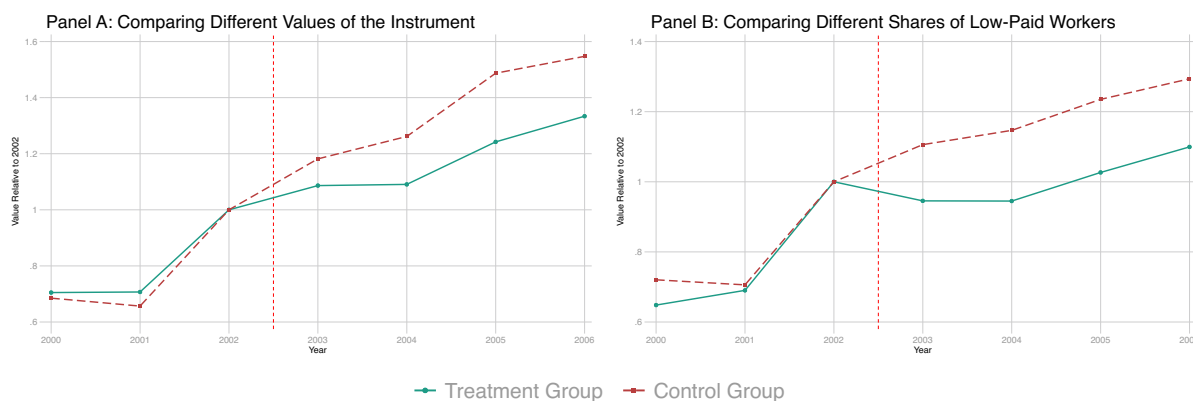
## **2.6 Wage tax incidence**

I exploit the reform shock to study the effect of payroll taxes on wages following the empirical framework described in section 2.5. First, I document that the policy generates significant changes in labor costs. Second, I study how shifts in labor costs are redistributed within the firm.

### **2.6.1 The reduction in labor costs caused by the Fillon's reform**

The Fillon's reform changed payroll taxes for low-paid workers and caused shifts in labor costs. Firms faced heterogeneous changes in the cost of labor depending on the wage distribution of incumbent workers and on their hiring decisions in the post-reform period. I study how the instrument (i.e. a change in the projected average tax rate of low-paid workers) affects the total social security contributions paid by the employer. I start with some descriptive evidence that compares average total social security contributions paid by firms in different quintiles of the instrument distribution. I split firms in two groups and I call "treated" the firms in the bottom two quintiles and "control" the firms in the top 3 quintiles. Figure 2.2 Panel A shows that the two groups have similar dynamics in labor costs in the pre-reform period, but divergent patterns after the reform becomes effective. Treated firms have lower growth in social security contributions relative to controls. The difference grows over time and is equal to 20 percent 4 years after the reform. I also compare the dynamics of social security contributions in firms with different shares of low-paid workers (Panel B). Firms with a large share of low-paid workers are remarkably similar to firms with a median share of low-paid workers before 2003, but experience a large decrease after the reform.

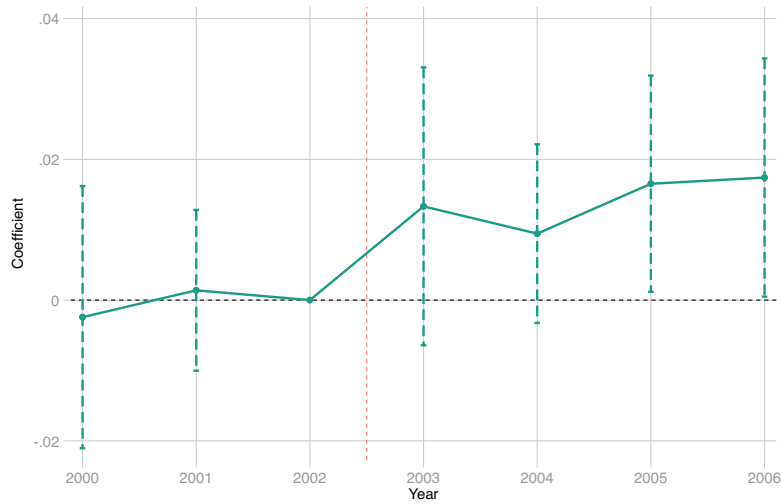
**Figure 2.2: Average Social Security Contributions - Treatment vs Control**



*Notes:* the Figure plots the average social security charges for firms in treatment and control groups relative to year 2002. In Panel A the treatment group includes firms that fall in the bottom two quintiles of the distribution of the instrument defined in (2.10), and the control groups include all other firms. In Panel B the treatment group includes firms in the top quintile of the distribution of the share of low-paid workers, and the control group includes firms in quintiles 2, 3 and 4. The sample includes a balanced panel of firms that were always active in the period 2000 to 2008 and that adopted a 39-hours regime in 2002.

I run a linear regression version of the analysis presented above by estimating (2.11) including as a treatment the instrument  $\Delta \bar{\tau}_j$ . The coefficients for 2000 and 2001 in Figure 2.3 show that the instrument predicts no differences in the amount of social security contributions in the pre-reform years. The estimates for post-reform years are positive and significant, indicating that an increase in average payroll taxes for low-paid workers causes higher total social security contributions for the employer. In particular, a 1 percent raise in the instrument increases social security contributions by close to 2 percent five years after the reform.

**Figure 2.3:** *Change in Total Social Security Contributions*



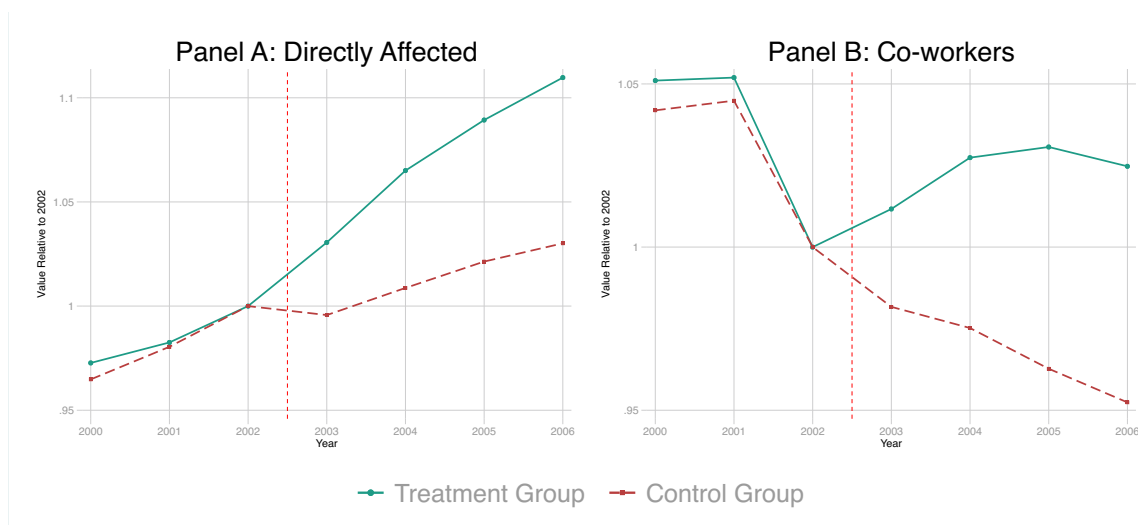
*Notes:* the Figure plots the results of specification (2.11). The dependent variable is the amount of social security contributions per capita paid by the firm (normalized by the level of this variable in 2002). The independent variable is the instrument as defined in (2.10). The sample includes a balanced panel of firms that were always active in the period 2000 to 2008 and that adopted a 39-hours regime in 2002.

### 2.6.2 Incumbent workers' wages respond to changes in taxes

The reform changed payroll tax rates for low-paid workers only. I study how this shock affects all incumbent workers to determine how the cost of payroll taxes is redistributed at the firm-level. I show that the benefits of lower payroll taxes are shared between all incumbents, including high-paid employees. First, I provide some descriptive evidence comparing the wage dynamics of incumbent workers employed in firms in different quintiles of the instrument distribution (Figure 2.4). Following a similar approach to the one in section 2.6.1, I split firms into a treatment and control group. Treated firms lie in the bottom two quintiles, while controls belong to the top three quintiles. The two groups have similar pre-reform wage dynamics for both low-paid and high-paid incumbent workers. After the reform enters into effect, the wages of treated firms experience a faster increase for low-paid workers and their co-workers, indicating that the benefit of lower payroll taxes

is redistributed to all incumbents and not only to the workers who benefit from reduced social security contributions.

**Figure 2.4:** *Average Net Wages - Treatment vs Control*

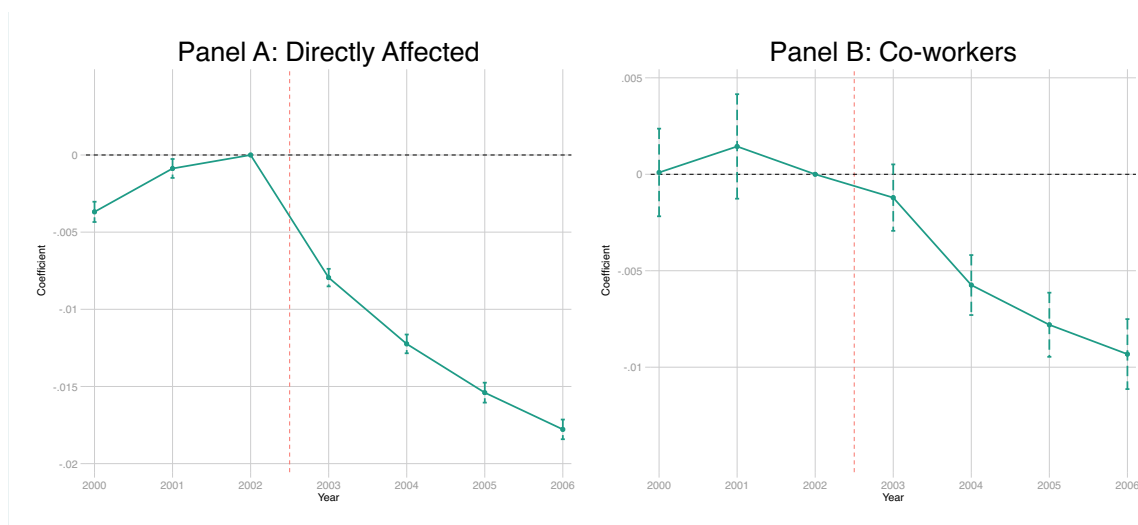


*Notes:* the Figure plots the average net hourly wages for workers directly affected by the Fillon's reform (Panel A) and their co-workers (Panel B) for firms in treatment and control group relative to year 2002. The treatment group includes firms that fall in the bottom two quintiles of the distribution of the instrument defined in (2.10). The sample includes a balanced panel of firms that were always active in the period 2000 to 2008 and that adopted a 39-hours regime in 2002.

Results are confirmed into a regression framework where I estimate (2.11) using the instrument as a treatment. I summarize the results in Table 2.3. Figure 2.5 shows a significant reduction in the net wages of incumbents who are directly affected by the reform. A 1 percent increase in the firm-level average payroll tax on low-paid workers causes a drop in net wages up to 1.5 percent of the 2002 wage. The co-workers face a drop equal to a little less than 1 percent in 2006, indicating that they appropriate part of the surplus generated by the policy. I rescale these estimates to obtain the effect of changes in the total labor cost on net wages by implementing the instrumental variable strategy presented in section 2.5. I instrument the firm-level change in labor costs  $\Delta C_j$  with the firm-level change in payroll taxes for low-paid workers that I have so far used in reduced-form. Figure 2.6 shows that a

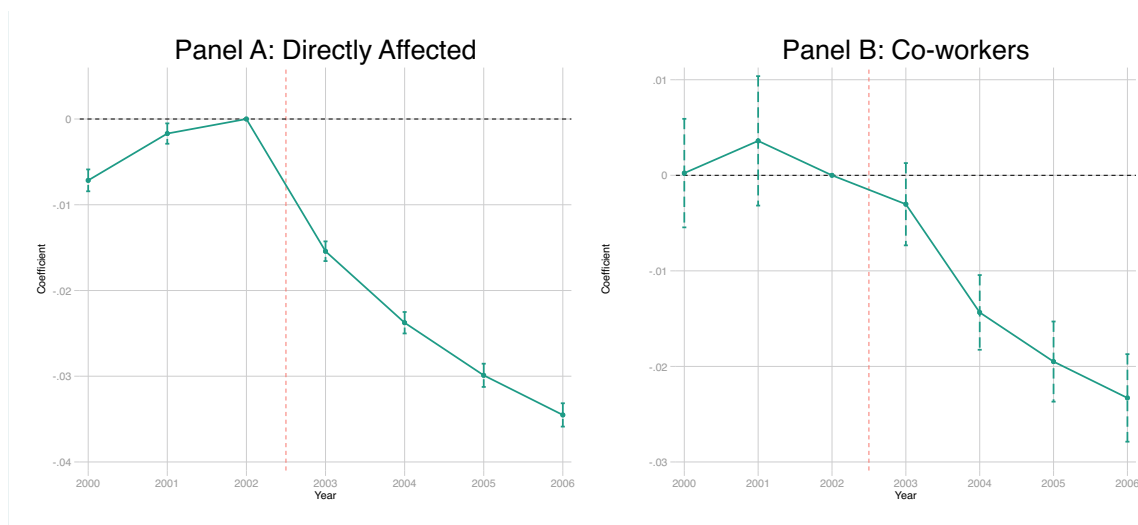
1 percent increase in labor costs caused a 3 percent drop in the wages of low-paid workers and a 2 percent drop in the net wages of high-paid employees. I conclude that firms conduct a redistribution of the costs of the policy generating spillovers on all incumbent workers. Because the data does not contain a worker identifier, all wage outcomes are affected by new hires' salaries. Heterogenous behavior in wage posting across more or less treated firms could create a wedge between my estimates and the effects of the reform on always incumbent workers. However, since I detect significant (and similar in magnitude) effects in the first year after the reform when the hiring response is unlikely to change a firm's wage distribution, the estimated effects are likely close to the ones for always incumbent workers.

**Figure 2.5:** *The Effects of Payroll Taxes on Net Wages*



*Notes:* the Figure plots the results of specification (2.11). The dependent variables are the average net wage of low-paid employees (Panel A) and the net wage for their co-workers (Panel B). Both outcomes are normalized by their value in 2002. The independent variable is the instrument as defined in (2.10). The sample includes a balanced panel of firms that were always active in the period 2000 to 2008 and that adopted a 39-hours regime in 2002.

**Figure 2.6:** *The Effects of Payroll Taxes on Net Wages*



*Notes:* the Figure plots the results of specification (2.11). The dependent variables are the average net wage of low-paid employees (Panel A) and the net wage for their co-workers (Panel B). Both outcomes are normalized by their value in 2002. The independent variable is the change in labor costs  $\Delta C$  that I define in (2.9). I instrument  $\Delta C$  with the instrument as defined in (2.10). The sample includes a balanced panel of firms that were always active in the period 2000 to 2008 and that adopted a 39-hours regime in 2002.

**Table 2.3:** *The Effect of the Fillon's Reform on Wages*

|                                   | Directly Affected Workers Net Wages |                        |                         | Co-Workers Net Wages   |                         |                        |
|-----------------------------------|-------------------------------------|------------------------|-------------------------|------------------------|-------------------------|------------------------|
|                                   | (1)                                 | (2)                    | (3)                     | (4)                    | (5)                     | (6)                    |
| $\Delta \bar{\tau}_j \times Post$ | -0.016***<br>(0.00041)              | -0.012***<br>(0.00031) | -0.0095***<br>(0.00035) | -0.0079***<br>(0.0012) | -0.0084***<br>(0.00084) | -0.0071***<br>(0.0010) |
| Observations                      | 185,076                             | 173,295                | 173,295                 | 246,204                | 225,936                 | 225,936                |
| Mean Outcome (pre 2003)           | .99                                 | .99                    | .99                     | 1.02                   | 1.02                    | 1.02                   |
| Treatment Mean                    | -.06                                | -.06                   | -.06                    | -.05                   | -.05                    | -.05                   |
| Treatment SD                      | .02                                 | .02                    | .02                     | .03                    | .03                     | .03                    |
| Wage Controls                     | No                                  | Yes                    | Yes                     | No                     | Yes                     | Yes                    |
| Baseline Controls                 | No                                  | No                     | Yes                     | No                     | No                      | Yes                    |

*Notes:* the Table reports the results of a variant of specification (2.11) where I collapse periods in pre and post-2002. The dependent variables are the average net wage of low-paid employees (Columns 1 to 3) and the net wage for their co-workers (Columns 4 to 6). Both outcomes are normalized by their value in 2002. The independent variable is the instrument as defined in (2.10). *Post* is a dummy taking value 1 in the post-2002 period. The sample includes a balanced panel of firms that were always active in the period 2000 to 2008 and that adopted a 39-hours regime in 2002.

## 2.7 Discussion and conclusions

This paper studies the redistribution of the cost of payroll taxes within the firm. I extend the standard tax incidence framework to incorporate the role of firms into the analysis of the effect of taxes on wages. I develop a theoretical model to show that the total elasticity of wages to payroll tax rates depends on two elements. First, the elasticity of the worker's outside option, which is typically estimated in the literature. Second, the passthrough of firm's labor costs, which relates to the change in the firm's surplus caused by the shift in taxes. I provide evidence of the second element exploiting a payroll tax reform passed in

France in 2003. In the context of the policy I show that when payroll taxes are lowered for low-paid workers part of the benefit is captured by high-paid employees in the same firm.

My approach represents a step forward in understanding of the effects of taxes on wages. The redistribution of the burden of taxes within the firm shows the importance of considering the employer as an active agent in the analysis of public policies. In particular, firms play a pivotal role in the wage formation process and should not be disregarded when looking at wage elasticities. They provide a natural dimension to capture the existence of spillovers that have important implications for the effectiveness of government's tax policies and - more in general - for welfare. This paper shows that tax policies designed to transfer resources to low-paid employees may have unintended costs caused by a redistribution of the firm's surplus to high-paid employees. These unintended costs make payroll taxes a less effective instrument to transfer resources to the bottom of the wage distribution.

Further work is needed to improve the understanding of the mechanisms behind the transmission of taxes to wages. The theoretical model I present in this paper shows a natural test for rent-sharing theories. I show that in case of full passthrough to the employees targeted by a tax change, there is zero spillover on the workers who are not targeted by the policy. This proposition could be tested in the context of the Fillon tax reform. Firms under the 35-hours regime at the time of the reform face an increase in payroll taxes, which in many contexts associated to full passthrough. My identification strategy can be applied to this sample of firms to test for the existence of spillovers and provide evidence of rent-sharing.

## Chapter 3

# Firms and Policy Incidence

### 3.1 Introduction

A recent and growing literature has studied how to conduct welfare analysis using causal estimates of the effects of policy changes. The approach relies on sufficient statistics derived by theoretical models and estimated on the data. These models mainly focus on individual-level responses to policy changes and do not regard firms as active agents in the passthrough of the effects of public policies. This paper is an attempt to extend the standard frameworks to clarify the statistics that one needs to estimate when firms respond to the incentives created by policies targeting individuals.

Firms have a central role in employment and wage decisions. Their response to policy shocks has implications for employees as well as prospect new hires. A growing literature shows the response of firms to policies that affect their employees. Examples are policies that affect the cost of inputs such as payroll or personal income taxes (Labanca and Pozzoli, 2018; Saez *et al.*, 2019), or policies changing workers' labor supply incentives such as retirement rules (Bovini and Paradisi, 2019). A firm's response to a policy generates a redistribution of its costs and benefits at the firm-level, creating important implications for welfare.

First, I include in a welfare model the notion of involuntary unemployment, which may respond to a firm's decisions. This allows me to trace the welfare effects of changes

in employment caused by a policy shift. Second, I show that firm-level causal responses to a policy are sufficient for welfare. This shift in focus is crucial whenever firms play an active role in the passthrough of policies. The new approach clarifies the relevant elasticities of prices and quantities that one needs for welfare and that are rarely estimated in the literature. Also, this focus allows to more easily capture the spillovers of a policy on workers who are not directly affected, but face the consequences of firm's decisions.

I propose a model with heterogeneous individuals who can be either employed in firms or unemployed. Firms are heterogeneous and can differ in the wages that they pay and in the number of workers that they employ. Workers perform a series of labor-related activities that can be influenced by their employer. The government designs policies targeting each labor-related activity, and sets lump-sum transfers to different types of workers. The welfare impact of a policy requires two elements. First, the causal impact of the policy on firm-level quantities and prices and its consequences for the government budget (Hendren, 2016). Second, the cost (benefit) of losing (finding) a job and firm-level estimates of the causal effect of the policy on employment. These causal estimates require variation in the policy across firms, rather than individual-level variation that is often exploited in empirical contexts. I derive a version of the marginal value of public funds that extends the one derived by Hendren (2016). My formula includes two new terms. The first captures the firm-level adjustment in the prices of labor activities (e.g. wages). It arises for instance when firms change their wage decisions in response to a policy shock and it is the result of “general equilibrium” dynamics within the firm. The second term measures the utility cost associated to changes in employment. When policies have employment consequences, this term may largely affect the welfare conclusions.

I show that the model is easy to implement using estimates from the literature. I apply the framework to policies that change payroll taxes (Saez *et al.*, 2019); raise the full retirement age (Bovini and Paradisi, 2019); change top personal income tax rates (Labanca and Pozzoli, 2018); change local corporate income taxes (Fuest *et al.*, 2018). I find that including firm-level responses into the analysis changes the welfare conclusions in most cases by a sizable

amount. I conclude that firms are an important vector for the passthrough of the effects of public policies.

My model builds on the idea of deriving sufficient statistics for welfare analysis. This idea has been recently debated by (Chetty, 2009a,b) and was previously adopted by Feldstein (1999). Hendren (2016) provides a framework to show that causal estimates of the effects of a policy (i.e. policy elasticities) are sufficient to capture its welfare impact, and a decomposition in compensated and uncompensated elasticities is not necessary. Hendren (2016) also builds on the concept of the marginal value of public funds that was previously introduced by Mayshar (1990), Slemrod and Yitzhaki (1996) and Kleven and Kreiner (2006). He shows that it can be estimated using the effect of a policy on the government budget. Finkelstein (2019) provides a useful interpretation of this work in relation to the “traditional” public finance literature. I provide a characterization of the marginal value of public funds in the context of my model discussing the differences with the standard model, and I provide estimates of this quantity using papers from the literature that study the response of firms to public policies.

The rest of the paper proceeds as follows: Section 3.2 sets up the model, derives expressions for willingness to pay, and the marginal value of public funds; section 3.3 discusses some extensions; section 3.4 applies the model to recent papers in the literature on firm’s responses to public policies; section 3.5 concludes.

## 3.2 The Model

The economy is populated by individuals of different types, indexed by  $i$ . There are  $N_i$  individuals for each  $i$  of whom  $n_i^e$  are employed ( $e$ ), while  $n_i^u$  are unemployed ( $u$ ). Employed individuals work in firms that I index with  $j \in J$ , and there is a total of  $|J|$  firms. Every individual has preferences over consumption ( $x$ ) and labor-related activities ( $\mathbf{L}$ ). The vector  $\mathbf{L} = (\mathbf{l}, \bar{\mathbf{l}})$  includes activities that the individual is free to choose ( $\mathbf{l}$ ) and activities that are taken as given ( $\bar{\mathbf{l}}$ ). The vector  $\mathbf{l}$  may include for instance labor in a firm and social security transfers;  $\bar{\mathbf{l}}$  may contain mandatory programs (required by a firm or the government), firm or

location-specific amenities or firm's work hours if they are set by the employer.<sup>1</sup> Preferences can be represented by a utility function

$$u_i(x_i, \mathbf{L}_i)$$

The government designs policies that affect three elements of the model: it can influence the prices of all labor activities changing policies  $(\rho_i, \bar{\rho}_i)$  and can set a transfer  $T_i$ .<sup>2</sup> Given government policies and prices, each agent has a budget set  $B$  that depends on her employment condition

$$\begin{cases} B^e : x_{ij}^e \leq (\mathbf{1} - \rho_i) \mathbf{w}_{ij} \mathbf{l}_{ij}^e + (\mathbf{1} - \bar{\rho}_i) \bar{\mathbf{w}}_{ij} \bar{\mathbf{l}}_{ij}^e + T_i + y_i & \text{if employed} \\ B^u : x_i^u \leq (\mathbf{1} - \rho_i) \mathbf{w}_i \mathbf{l}_i^u + (\mathbf{1} - \bar{\rho}_i) \bar{\mathbf{w}}_i \bar{\mathbf{l}}_i^u + T_i + y_i, \text{ with } \ell_i^u, \bar{\ell}_i^u = 0 & \text{if unemployed} \end{cases}$$

For employed individuals  $j$  indexes the firm and  $(\mathbf{w}_{ij}, \bar{\mathbf{w}}_{ij})$  are the prices of labor activities earned by type  $i$  in firm  $j$ . I assume that consumption goods are produced under perfect competition.<sup>3</sup> Importantly, the model allows for variation in the wages of workers of type  $i$  across firms to incorporate a firm-specific wage component.  $y_i$  is income from labor unrelated activities and can incorporate profit shares as I discuss in section (3.3). Unemployed individuals face a similar budget constraint, but some of their labor activities (e.g. labor in a firm) are equal to zero.

Each individual takes prices and government policies as given to maximize her utility subject to the budget constraint. This maximization delivers an indirect utility  $V_i^k$  for individuals with employment status  $k = e, u$

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<sup>1</sup>Notably,  $\bar{\mathbf{l}}$  can also include government spending in case it delivers utility to the workers.

<sup>2</sup>The model can easily incorporate consumption taxes, which I have omitted to ease the notation and because they are not a primary focus of the paper.

<sup>3</sup>In section (3.3) I discuss how the model can be extended to include producer's surplus.

$$V_i^k(\mathbf{w}_i, \bar{\mathbf{w}}_i, \boldsymbol{\rho}_i, \bar{\boldsymbol{\rho}}_i, \bar{\mathbf{l}}_i, T_i, y_i) = \max_{x_i^k, \mathbf{L}_i^k \in B^k} u_i(x_i^k, \mathbf{L}_i^k)$$

The optimal choices of consumption and labor activities can be represented by  $x_i^{k*}(\mathbf{w}_i, \bar{\mathbf{w}}_i, \boldsymbol{\rho}_i, \bar{\boldsymbol{\rho}}_i, T_i, y_i)$  and  $\mathbf{L}_{ij}^{k*}(\mathbf{w}_i, \bar{\mathbf{w}}_i, \boldsymbol{\rho}_i, \bar{\boldsymbol{\rho}}_i, T_i, y_i)$ . Given the heterogeneity across types and across employment status within the same type, I will normalize utility by the individual's marginal utility of income

$$\frac{\partial V_{ij}^e}{\partial y_{ij}} = \lambda_{ij}, \quad \frac{\partial V_i^u}{\partial y_i} = \lambda_i$$

where the  $\lambda$ s are the Lagrangian multipliers from the individual's maximization problem. I define the average utility of individuals of type  $i$  as a weighted average of the indirect utilities of employed and unemployed individuals

$$\bar{V}_i(\mathbf{n}_i^e, \mathbf{w}_i, \bar{\mathbf{w}}_i, \boldsymbol{\rho}_i, \bar{\boldsymbol{\rho}}_i, T_i, y_i) = \sum_j \frac{n_{ij}^e}{N_i} V_{ij}^e(\mathbf{w}_{ij}, \bar{\mathbf{w}}_{ij}, \boldsymbol{\rho}_i, \bar{\boldsymbol{\rho}}_i, \bar{\mathbf{l}}_i, T_i, y_i) + \frac{(N_i - \sum_j n_{ij}^e)}{N_i} V_i^u(\mathbf{w}_i, \bar{\mathbf{w}}_i, \boldsymbol{\rho}_i, \bar{\boldsymbol{\rho}}_i, \bar{\mathbf{l}}_i, T_i, y_i)$$

where  $\mathbf{n}_i^e$  and  $(\mathbf{w}_i, \bar{\mathbf{w}}_i)$  are the vectors of employment and wages of types  $i$  across employment status and occupation. Alternatively, one can think of  $\bar{V}_i$  as the expected utility of a workers who was born with type  $i$ . In order to define a measure of total welfare in the economy, I use a vector of Pareto weights  $\{\psi_i\}_{i \in I}$  to weight the average utilities of all types in the economy.<sup>4</sup> The aggregate measure of welfare is

$$W(\{\mathbf{n}_i^e, \mathbf{w}_i, \bar{\mathbf{w}}_i, \boldsymbol{\rho}_i, \bar{\boldsymbol{\rho}}_i, T_i, y_i\}_i) = \int_{i \in I} \psi_i N_i \bar{V}_i(\mathbf{n}_i^e, \mathbf{w}_i, \bar{\mathbf{w}}_i, \boldsymbol{\rho}_i, \bar{\boldsymbol{\rho}}_i, \bar{\mathbf{l}}_i, T_i, y_i) di$$

This expression relates indirect utilities for all types in the economy to total welfare and it shows the importance of characterizing the impact of policies on  $\bar{V}_i$  as a way to measure the welfare implications of a government intervention.

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<sup>4</sup>The model also allows the Pareto weight to depend on the worker's employment status so that  $\psi_i^k$  is the Pareto weight for types  $i$  with employment status  $k$ .

### 3.2.1 Policy, welfare and the marginal willingness to pay for a policy change

I define a policy path in the context of the model by following the approach in Hendren (2016). The latter relates to the definition of causal effects as differences in potential outcomes (Angrist and Pischke, 2008) and builds on the idea of evaluating welfare using marginal policy changes discussed by (Mayshar, 1990). A policy path  $P(\theta)$  is a vector of policies for any  $\theta \in (-\varepsilon, \varepsilon)$ , with  $\varepsilon$  small enough

$$P(\theta) = \{\rho_i(\theta), \bar{\rho}_i(\theta), T_i(\theta)\}_{i \in I}$$

I call the status-quo  $\theta = 0$ . A policy is a perturbation of the status quo in a certain direction for every policy instrument. For instance,  $d\rho_{is}(\theta)/d\theta > 0$  represents an increase policy  $\rho_{is}$ . Individual choices depend on  $\theta$  so that  $x(\theta)$  and  $\mathbf{l}(\theta)$  are optimal consumption and labor decisions with policy  $P(\theta)$ , or alternatively the potential outcomes under this policy (Angrist and Pischke, 2008). The effect of a policy on these choices can then be represented as  $dx(\theta)/d\theta$  and  $d\mathbf{l}(\theta)/d\theta$ , which measure differences in the potential outcomes under the policy paths  $P(\theta)$  and  $P(0)$ .

Given a policy  $P(\theta)$ , the resources transferred by the government to of type  $i$  with employment status  $k$  are:

$$t_i^k(\theta) = -\rho_i(\theta) \mathbf{w}_i^k(\theta) \mathbf{l}_i^k(\theta) - \bar{\rho}_i(\theta) \bar{\mathbf{w}}_i^k(\theta) \bar{\mathbf{l}}_i^k(\theta) + T_i(\theta)$$

Aggregating the transfers to individuals  $i$  employed in the same firm  $j$ , one can define the total government transfers to individuals  $i$  in firm  $j$  as  $\tilde{t}_{ij}^e(\theta) = n_{ij}^e(\theta) t_{ij}^e(\theta)$ . Using the definitions above, the average indirect utility of each type  $i$  can be written as  $\bar{V}_i(\theta)$  and its change in response to a policy change is  $\frac{d\bar{V}_i}{d\theta}$

$$\begin{aligned}
\frac{d\bar{V}_i(\theta)}{d\theta} = & \underbrace{\sum_j \frac{(V_{ij}^e(\theta) - V_i^u(\theta))}{N_i} \frac{dn_{ij}^e(\theta)}{d\theta}}_{\text{Utility cost of unemployment}} \\
& + \underbrace{\sum_j \frac{n_{ij}^e(\theta)}{N_i} \frac{dV_{ij}^e(\theta)}{d\theta}}_{\text{Utility change for employed}} + \underbrace{\frac{n_i^u(\theta)}{N_i} \frac{dV_i^u(\theta)}{d\theta}}_{\text{Utility change for unemployed}}
\end{aligned} \tag{3.1}$$

The change in average indirect utility is composed by three terms. First, the utility cost of unemployment, which is proportional to the change in employment and to the drop in utility associated to a change in employment status. Second, the change in utility for employed incumbent workers across all firms in the economy (keeping their number constant). Third, the change in utility for unemployed workers. I start by discussing the last two terms.

Normalizing the change in utility by the marginal utility of income I can recover the marginal willingness to pay for the policy change:  $\frac{dV_i(\theta)}{\lambda_i}$ . For incumbent workers, the marginal willingness to pay is

$$\begin{aligned}
\frac{\frac{dV_{ij}^e(\theta)}{d\theta}}{\lambda_{ij}} = & \underbrace{\frac{dt_{ij}^e}{d\theta}}_{\text{Effect on transfers}} + \underbrace{\sum_s l_{ijs}^e \frac{dw_{ijs}^e}{d\theta} + \sum_s \bar{l}_{ijs}^e \frac{d\bar{w}_{ijs}^e}{d\theta}}_{\text{Effect of price changes}} \\
& + \underbrace{\sum_s \rho_{is} w_{ijs}^e \frac{dl_{ijs}^e}{d\theta}}_{\text{Cost of behavioral responses}} + \underbrace{\sum_s \frac{\partial u_i}{\partial l_{ijs}^e} \frac{d\bar{l}_{ijs}^e}{\lambda_{ij}^e} \frac{d\bar{l}_{ijs}^e}{d\theta}}_{\text{Direct utility effect}}
\end{aligned} \tag{3.2}$$

I report the details about the derivation of (3.2) in Appendix (C.1). The willingness to pay of unemployed workers takes a similar form. As it is well known in the sufficient statistics literature, the marginal willingness to pay is a combination of the change in government resources provided to the workers  $\frac{dt_{ij}^e}{d\theta}$  and the behavioral responses of the workers to the policy  $\left\{ \frac{dl_{ijs}^e}{d\theta} \right\}_s$ . Recognizing an active role to firms, my model emphasizes the importance of

general equilibrium effects on wages ( $\left\{\frac{dw_{ijs}^e}{d\theta}\right\}_s$  and  $\left\{\frac{d\bar{w}_{ijs}^e}{d\theta}\right\}_s$ ) and the direct utility effect  $\frac{\partial u_i}{\partial \bar{w}_{ijs}^e} \frac{d\bar{w}_{ijs}^e}{d\theta}$  of changes in  $\bar{l}$ .<sup>5</sup>

The welfare effect of the policy requires an aggregation across all types. The total welfare effect of the policy change for incumbent employees is

$$\begin{aligned} \int_{i \in I} \psi_i \sum_j \lambda_{ij} n_{ij}^e \frac{dV_{ij}^e(\theta)}{d\theta} \Big|_{\theta=0} di &= \int_{i \in I} \tilde{\eta}_i^e \underbrace{\sum_j n_{ij}^e \frac{dV_{ij}^e(\theta)}{d\theta} \Big|_{\theta=0}}_{\text{Total WTP of } i} \frac{1}{\lambda_{ij}} di \\ &= \int_{i \in I} \tilde{\eta}_i^e \cdot WTP_i di \end{aligned}$$

The total welfare effect for incumbents can be computed aggregating the product of the total willingness to pay by type and the social marginal welfare weight  $\tilde{\eta}_i^e$ . The latter measures how much society values the marginal consumption of employees of type  $i$  (Saez and Stantcheva, 2016).  $\tilde{\eta}_i^e = \psi_i \tilde{\lambda}_i^e$  is a combination of  $i$ 's Pareto weight and the marginal utility of income for employees  $i$ .<sup>6</sup> With the same logic, the social marginal welfare weight of unemployed  $i$  is  $\tilde{\eta}_i^u = \psi_i \lambda_i^u$ .

### 3.2.2 The value of employment

The utility cost of unemployment is a crucial component of the welfare impact of a policy. It measures how much individuals value their employment condition relative to unem-

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<sup>5</sup>The marginal willingness to pay of unemployed workers is

$$\frac{dV_i^u(\theta)}{d\theta} \Big|_{\theta=0} = \frac{dt_i^u}{d\theta} + \sum_s l_{is}^u \frac{dw_{is}^u}{d\theta} + \sum_s \bar{l}_{is}^u \frac{d\bar{w}_{is}^u}{d\theta} + \sum_s \rho_{is} w_{is}^u \frac{dl_{is}^u}{d\theta} + \sum_s \frac{\partial u_i}{\partial \bar{l}_{is}^u} \frac{d\bar{l}_{is}^u}{d\theta}$$

<sup>6</sup>The marginal utility of income  $\tilde{\lambda}_i^e$  is a weighted average of the marginal utilities of income for types  $i$  across firms

$$\tilde{\lambda}_i^e = \sum_j \lambda_{ij} \left( \frac{n_{ij}^e \frac{dV_{ij}^e(\theta)}{d\theta} \Big|_{\theta=0}}{\sum_j n_{ij}^e \frac{dV_{ij}^e(\theta)}{d\theta} \Big|_{\theta=0}} \right)$$

It represents an “average” marginal utility of income that assigns larger weights to employees  $i$  who are more willing to pay for the policy change. Hence, relative to the standard model, the marginal social welfare weight is endogenous to the policy.

ployment. The term is proportional to  $V_{ij}^e(\theta) - V_i^u(\theta)$ , the difference in indirect utilities across the two states. I propose a simple way to implement the first term in (3.1). First, I approximate  $V_{ij}^e(\theta) - V_i^u(\theta)$  using a first-order Taylor approximation

$$\begin{aligned} V_{ij}^e(\theta) - V_i^u(\theta) &= u_i(x_{ij}^e, \mathbf{l}_{ij}^e) - u_i(x_i^u, \mathbf{l}_i^u) \\ &\cong u_{i,x}(x_i^u, \mathbf{l}_i^u) \Delta c_{ij} + \sum_s u_{i,l_s}(x_i^u, \mathbf{l}_{is}^u) \Delta l_{ijs} \end{aligned} \quad (3.3)$$

Among the various labor activities,  $l^w$  is the number of hours of paid labor in a firm so that  $l^w = 0$  in the unemployment state. Using (3.3), the total utility cost of unemployment in units of consumption of the unemployed individual  $i$  is approximated by

$$\begin{aligned} \sum_j \frac{(V_{ij}^e(\theta) - V_i^u(\theta))}{\lambda_i^u} \frac{dn_{ij}^e(\theta)}{d\theta} &\cong \frac{1}{\lambda_i^u} \left[ u_{i,x}^u \overline{\Delta c_i} + \sum_s u_{i,l_s}^u \overline{\Delta l_{is}} \right] \frac{dn_i^e(\theta)}{d\theta} |J| \\ &= \left[ \overline{\Delta c_i} + \sum_s \frac{u_{i,l_s}^u}{\lambda_i^u} \Big|_{l^w=0} \overline{\Delta l_{is}} \right] \frac{dn_i^e(\theta)}{d\theta} |J| \end{aligned} \quad (3.4)$$

$\overline{\Delta c_i}$  and  $\overline{\Delta l_{is}}$  represent the average drop in consumption and labor activities for employees changing employment status. If a change in status activates some government transfers,  $\overline{\Delta c_i}$  is the drop in earnings (including government transfers) caused by the job loss. This statistics is easily estimated in the data by looking at the earnings consequences of job losses.  $\frac{dn_i^e(\theta)}{d\theta}$  is the firm-level change in employment. The first approximation in (3.4) exploits the fact that  $u_{i,x}$  and  $u_{i,l^w}$  are constant across  $js$ .<sup>7</sup> The second line uses the identity  $\lambda_i^u = u_{i,x}^u$  from the optimization problem of unemployed agent  $i$ . Only the marginal utilities  $\left\{ \frac{u_{i,l_s}^u}{\lambda_i^u} \right\}_s$  are not easy to estimate in the data. In the standard case where agents derive utility from consumption and labor in a firm ( $l^w$ ),  $\frac{u_{i,l^w}^u}{\lambda_i^u}$  represents the marginal disutility of labor in consumption units of unemployed individual  $i$ , i.e. how much an unemployed worker is willing to pay to avoid an extra hour of work. I will assume in section 3.2.4 that  $\lim_{l_i^w \rightarrow 0} \frac{u_{i,l^w}^u}{\lambda_i^u} = 0$ . In other words, I rely on the assumption that unemployed individuals - who are constrained at  $l^w = 0$  - have

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<sup>7</sup>The reason is that they represent the marginal utilities of unemployed workers

a close to zero marginal disutility from working.

The value of unemployment requires an estimate of the effects of a policy on employment flows. Firms can often adjust along two main margins: layoffs and hires. The two margins have different implications for the definition of  $\overline{\Delta c}$  in the implementation of (3.4) (section 3.4.2). In case of layoffs,  $\overline{\Delta c}$  represents the cost of losing the job, which is proportional to the wage loss following the separation event (Couch and Placzek, 2010). On the other hand, for new hires  $\overline{\Delta c}$  is the gain from finding a job, which depends on the new hires average wage.

### 3.2.3 Firm-level policy responses

This section discusses the estimates needed to implement the formulas derived above. I define these estimates as firm-level policy responses. A firm-level policy response of quantity  $q$  is

$$\frac{d\bar{q}_i^e}{d\theta} = \sum_j \frac{n_{ij}^e}{|J|} \frac{dq_{ij}^e}{d\theta} \Big|_{\theta=0} \quad (3.5)$$

It is the policy-induced change in the total firm amount  $Q_{ij} = n_{ij}q_{ij}$  when keeping  $n_{ij}$  constant. In the data, it is a measure of the consequences of a policy for the incumbent workers at the policy implementation date. Importantly, the quantity  $Q_{ij}$  can be heterogenous across firms, but an estimate of the response in the average firm is sufficient for welfare. Such interpretation suggests an estimation strategy that focuses on firm-level responses rather than individual-level ones. This is because firms can affect  $Q_{ij}$ , implying that the welfare-relevant statistics is  $\frac{\partial Q_{ij}(\theta)}{\partial q_{ij}} \frac{dq_{ij}}{d\theta} \Big|_{\theta=0}$  that compares  $Q_{ij}$  across firms that are heterogeneously affected by the policy. If firms play no role in the response of  $Q_{ij}$ , a firm-level approach delivers the same result as the individual-level one. However, in many contexts individual responses might lose important welfare components. For instance, in the case of spillovers within the firm that are caused by firm's decisions. Firm-level estimates are also useful because they automatically account for the relative numerosity of different types  $i$  when multiple categories of workers are affected by the consequences of a policy. Having established the importance of estimating firm-level responses, one needs to exploit the right

policy variation. This amounts to exploit policy variation across firms rather than across individuals even when the policy is targeted to individuals, which requires a novel approach in the analysis of many policies.

Although the definition in (3.5) refers to quantities, firm-level changes in prices are also crucial for the welfare impact of a policy. As I discuss in section 3.2.4, firm-level changes in the prices of labor activities are one component of the marginal value of public funds. For instance, Saez *et al.* (2019) show that using individual-level variation in payroll taxes to estimate the wage tax incidence misses the effects of a tax cut on incumbent workers. Variation in firm-level tax windfalls instead identifies significant rent sharing affecting all employees, which has relevant welfare consequences as I show in section 3.4.1.

### 3.2.4 Revisiting the Marginal Value of Public Funds

When policies are not budget neutral, their effect on government revenues must be taken into account to evaluate the welfare impact. To this end, I define the marginal value of public funds in the context of my model as a ratio of benefits to costs. The concept was introduced by Mayshar (1990), Slemrod and Yitzhaki (1996) and Kleven and Kreiner (2006). Hendren (2016) clarified its definition, showing that it can be expressed as a function of the causal effects of a policy on the government budget.

The marginal value of public funds measures the total individual welfare gains/losses for every dollar spent by the government. In the context of my model, the marginal value of public funds in units of consumption of individual  $\hat{i}$  is

$$MVPF_{\theta}^{\hat{i}} = \frac{\int_{i \in I} \frac{1}{\eta_i} \left( \tilde{\eta}_i^e \sum_j n_{ij}^e \frac{\frac{dV_{ij}^e(\theta)}{d\theta}|_{\theta=0}}{\lambda_{ij}} + \eta_i^u n_i^u \frac{\frac{dV_i^u(\theta)}{d\theta}|_{\theta=0}}{\lambda_i^u} + \eta_i^u \sum_j \frac{(V_{ij}^e(\theta) - V_i^u(\theta))}{\lambda_i^u} \frac{dn_{ij}^e(\theta)}{d\theta} \right) di}{\int_{i \in I} \left( \overline{\frac{dt_i^e(\theta)}{d\theta}} |J| + n_i^u \overline{\frac{dt_i^u(\theta)}{d\theta}} + \overline{\Delta t_i} \overline{\frac{dn_i^e(\theta)}{d\theta}} |J| \right)}$$

The numerator is the total marginal willingness to pay for the policy of all individuals in the economy. The normalization by  $\eta_i$  converts the marginal willingness to pay in units of consumption of  $\hat{i}$ . It can be decomposed into three terms: the marginal willingness

to pay of incumbent employees, of unemployed individuals, and of individuals who lose (find) a job per effect of the policy. Using (3.2) and (3.4) it can be written as a function of the firm-level responses described in section 3.2.3. The denominator instead quantifies the cost of the policy for the government. It is the change in total government transfers to the three groups of individuals.  $\frac{dt_i^e(\theta)}{d\theta}$  is the firm-level response of government transfers to incumbent employees, i.e. the average effect of the policy on firm-level government transfers  $\sum_j \frac{1}{|J|} \frac{d\tilde{t}_{ij}^e(\theta)}{d\theta}$ . To quantify this term, it is sufficient to look at the change in total transfers to the incumbent workers at the policy implementation date.  $\frac{dt_i^u(\theta)}{d\theta}$  is the average response of government transfers to unemployed workers. Finally,  $\frac{dn_i^e(\theta)}{d\theta}$  represents the average firm change in employment.

I study a simplified case where the policy affects only one type of workers and does not impact unemployed individuals, assuming that the social marginal welfare weight is the same for employed and unemployed. For changes in  $(\mathbf{a}_i, T_i)$ , the MVPF simplifies to

$$\begin{aligned}
MVPF_{\theta}^i &= \frac{\sum_j \frac{n_{ij}^e}{|J|} \frac{dV_{ij}^e(\theta)}{d\theta} \big|_{\theta=0} + \sum_j \frac{1}{|J|} \frac{(V_{ij}^e(\theta) - V_i^u(\theta))}{\lambda_i^u} \frac{dn_{ij}^e(\theta)}{d\theta} di}{\frac{dt_i^e(\theta)}{d\theta} + \Delta T_i \frac{dn_i^e(\theta)}{d\theta}} \\
&= \underbrace{\frac{1}{1 + FE(\theta)}}_{\text{Standard term}} + \underbrace{\frac{\sum_s l_{is}^e \frac{dw_{is}^e}{d\theta}}{\frac{dt_i(\theta)}{d\theta}}}_{\text{GE effect of price change}} + \underbrace{\frac{\Delta C_i \frac{dn_i^e(\theta)}{d\theta}}{\frac{dt_i(\theta)}{d\theta}}}_{\text{Cost of unemployment}} \quad (3.6)
\end{aligned}$$

The second line shows that the marginal value of public funds can be decomposed into three components. The first term is familiar to the literature on sufficient statistics.  $FE(\theta)$  is the so called fiscal externality. In the standard model it represents the ratio between the behavioral impact on government budget and the mechanical cost of the policy for the government. In this new formula the fiscal externality only captures the behavioral impact on government budget for incumbent employees. The second term measures the utility cost of price changes at the firm-level. Thanks to an envelope argument, it is proportional to the average change in prices  $\mathbf{w}_i$  across firms. The term captures price adjustments caused by general equilibrium dynamics within (or outside) the firm. For instance, firms could cut wages in response to

higher payroll taxes or in response to an increased labor supply of incumbent employees, causing first-order effects on the willingness to pay for the policy.<sup>8</sup> The last term captures the utility cost of unemployment. It is proportional to the employment change and depends on the drop in consumption caused by a change in employment status and the disutility from labor.

### 3.3 Discussion and Extensions

**Incorporating the effects on profits:** The baseline version of the model assumes perfect competition on the consumption good market. However, one can easily include producer's surplus by redefining total welfare as  $\mathcal{W}(\theta) = W(\theta) + \psi^p PS(\theta) + R(\theta)$ , where  $R(\theta)$  is government revenues,  $PS(\theta)$  is total producer surplus, and  $\psi^p$  is the Pareto weight that the society attaches to producers. To study the welfare impact, one needs to model  $PS(\theta)$  to determine which causal estimates are sufficient for welfare. Firm-level estimates are a natural benchmark to study the impact of the policy on  $PS$ . Suppose that  $PS$  equals total profits, it is sufficient to estimate the effect of the policy on a firm's profits to determine  $\frac{dPS(\theta)}{d\theta}$  and the externality on the government budget. Profits could also be partly rebated to workers. In this case, they would enter  $y_i$  so that  $y_i = f(PS)$  and one needs to model  $f(\cdot)$  to determine the welfare effect of the policy.

**Firm-targeted policies:** I have focused so far on policies that target workers. My framework can easily apply to policies that directly target firms such as changes in corporate taxes, VAT taxes or import/export tariffs. There are two elements that should be taken into account for this purpose. First, one must study the effects of these policies on workers. A growing literature focuses on the passthrough of firm-level taxes to wages and employment (Serrato and Zidar, 2016; Fuest *et al.*, 2018). Second, one must estimate the effects of these policies on producer's surplus with a focus on profits and other firm's margin that might

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<sup>8</sup>The standard sufficient statistics models could incorporate general equilibrium effects (see Hendren, 2016, Appendix D). My model stresses the importance of these dynamics and clarifies the firm-level statistics that is sufficient for studying the welfare impact.

have externalities on the government budget. As I discussed above, profits can affect the worker's willingness to pay depending on their redistribution. Moreover, to the extent to which profits, revenues or investments are taxed/subsidized, firm's responses will create fiscal externalities. I provide an application of the model to corporate taxes in section 3.4.

**Incorporating local labor markets:** The model focuses on firms as a vector of policy incidence and a dimension within which policy shocks propagate. By changing the interpretation of the  $j$ 's dimension, one can study the effects of local labor market level shocks allowing for heterogeneity across local labor markets. This dimension may be relevant when studying local taxes or the welfare impact of economic shocks.<sup>9</sup>

## 3.4 Applications

### 3.4.1 Payroll tax subsidy

I analyze the welfare consequences of a change in payroll taxes. Recent work by Saez *et al.* (2019) has investigated the role of firms for the passthrough of taxes to wages using a payroll tax reform implemented in Sweden in 2007 that lowered payroll taxes for young workers. They document a redistribution of the benefits of payroll tax subsidies that causes spillovers within the firm. Furthermore, they show positive effects of tax subsidies on employment. I apply my model to the context of this reform. I distinguish two types of workers: young workers ( $y$ ) and their co-workers ( $c$ ). The budget constraint for employees of any of the two groups is

$$x_{ij} \leq (1 - \tau_i^w) w_{ij} l_{ij}^w + \sum_s p_{is}^d l_{isj}^d + T_i + y_i \quad (3.7)$$

$l^w$  is paid labor in a firm, and  $\tau^w$  is the payroll tax. The marginal willingness to pay for the reform of a group of employees  $i$  is

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<sup>9</sup>When investigating the effect of economic shocks, policies will be set to the status quo. The model only requires the elasticities of quantities and prices to the economic shock analyzed.

$$\sum_j \frac{dV_{ij}}{\lambda_{ij}} = \sum_j n_{ij}^e \left[ -w_{ij} l_{ij}^w \frac{d\tau_i^w}{d\theta} + (1 - \tau_i^w) l_{ij}^w \frac{dw_{ij}}{d\theta} \right]$$

It depends on two terms. First, the mechanical change in the net wage caused by the shift in the tax. Second, the change in the net wage caused by an adjustment of the gross wage, which depends on firm's wage policies in response to the reform. In the first part of the paper the authors show that the net wages of younger workers (those targeted by the policy) are virtually unchanged after the reform, indicating that the employers capture all the benefits of the tax cut. This is because gross wages adjust so that  $\frac{dw_{ij}}{d\theta}$  compensates the change in payroll taxes. However, the second part of the analysis shows that the benefits of lower payroll taxes are redistributed among incumbent workers. Hence, a firm-level perspective allows to capture wage dynamics that individual-level estimates miss. The policy also causes an increase in employment for both groups of workers that determines a utility benefit. I assume that there are no significant behavioral responses on the intensive margin of labor supply. Under these assumptions, and when the social marginal welfare weights of young and older workers are equal, the marginal value of public funds of the reform is

$$MVPF_{P\tau^w}^y = \frac{\overbrace{\sum_{i=y,o} \left[ -\overline{w_i} l_i^w \frac{d\tau_i^w}{d\theta} + (1 - \tau_i^w) \overline{l_i^w} \frac{dw_i}{d\theta} \right]}^{\text{Incumbent's marginal WTP}} + \overbrace{\sum_{i=y,o} (1 - \tau_i^w) \Delta \bar{w}_i \frac{dn_i^e}{d\theta}}^{\text{Benefit of employment increase}} - \overbrace{\frac{\psi^p}{\eta_y} \frac{dP}{d\theta}}^{\text{Producer's Surplus}}}{\underbrace{\sum_{i=y,o} \left[ -\overline{w_i} l_i^w \frac{d\tau_i^w}{d\theta} - \tau_i^w \overline{l_i^w} \frac{dw_i}{d\theta} \right] - \sum_{i=y,o} \tau_i^w \Delta \bar{w}_i \frac{dn_i^e}{d\theta} - \tau^\pi \frac{d\pi}{d\theta}}_{\text{Cost of the policy}}} \quad (3.8)$$

The first term in the numerator is proportional to the firm-level average effect of the policy on wages. I set  $\frac{d\tau_o}{d\theta} = 0$  since the reform leaves payroll taxes unchanged for older workers. I take estimates of  $\frac{dw_i}{d\theta}$  for the two groups of workers from the firm-level analysis in the second part of the paper. The latter also provides estimates of the effect of the policy on firm

employment, which I use to quantify  $\overline{\frac{dn_i^e}{d\theta}}$ . Firms with a high share of young workers increase average gross wages by 0.33 percent and increase employment by 4.6 percent relative to medium share firms. Divided by 2.4 (the change in labor costs between high and low share firms) it amounts to an increase of 0.14 and 1.92 percent respectively in response to a 1 percent decrease in labor costs. In order to implement the formula in (3.8), I multiply these numbers by the change in labor costs that is caused by a shift in  $\tau^w$ . The policy has also an effect on the producer's surplus.  $\psi^p$  is the Pareto weight on producers, which I assume equal to the workers social marginal welfare weight. I also assume that the producer surplus is measured by profits so that  $\frac{dP}{d\theta} = (1 - \tau^\pi) \bar{n}_y^e \left( \bar{w}_y^{net} \frac{d\tau_y^w}{d\theta} + (1 + \tau^w) \frac{d\bar{w}_y^{net}}{d\theta} \right)$ . The producer's willingness to pay depends on the mechanical impact of the reform on profits, and on the mechanical effect of wage changes. The former is proportional to the reduction in labor costs caused by the reform, the latter depends on the change in net wages. Finally, I account for the externalities of changes in profits on the government budget by calibrating a corporate tax rate  $\tau^\pi$ . Details about the calibrations are reported in Table C.1.

Under the standard approach that focuses on individual-level responses to the policy, the marginal value of public funds would be

$$\widetilde{MVPF}_{P\tau^w}^y = \frac{\overbrace{-w_y l_y^w \frac{d\tau_y^w}{d\theta} + (1 - \tau_y^w) l_y^w \frac{dw_y}{d\theta}}^{\text{Young incumbents' marginal WTP}} + \overbrace{\Delta \bar{w}^h \frac{dh_y}{d\theta} - \Delta \bar{w}^f \frac{df_y}{d\theta}}^{\text{Benefit of employment increase}}}{\underbrace{-w_y l_y^w \frac{d\tau_y^w}{d\theta} - \tau_y^w l_y^w \frac{dw_y}{d\theta} - \tau_y \Delta w^h \frac{dh_y}{d\theta} + \tau_y \Delta w^f \frac{df_y}{d\theta}}_{\text{Cost of the policy}}}$$

This approach would only consider the individuals targeted by the policy, and the welfare impact would depend on the effect of the reform on workers  $y$  net wages (first two terms in the numerator) and employment probability. The cost of the reform for the government would depend on the money spent on the subsidy and on the extra taxes raised on the workers who find an employment due to the reform.

I implement the formulas above and report the results in Table 3.1. The marginal value of public funds under the standard approach is 0.04, indicating that young workers are

only willing to pay 4 cents per dollar spent by the government. Firms are capturing all the surplus from the subsidy, leaving net wages almost unchanged. The willingness to pay derives only from an increase in the employment probability and is proportional to the wage earned when employed. On the other hand, when I implement the formula in (3.8) the marginal value of public funds is 1.51 dollars. Firm-level redistribution of the benefits of the tax cut generates a willingness to pay for all incumbent workers. Moreover, the reform increases employment significantly and boosts producer's surplus. A comparison between the two approaches suggests that a firm's active role in response to policies can significantly influence the welfare impact.

### 3.4.2 Raise in full retirement age

Public policies that affect the labor supply of some workers are likely to trigger firm's responses, which in turn create spillovers on all incumbent workers. Examples are policies that lower turnover of older workers such as an increase in the full retirement age. Bovini and Paradisi (2019) study the short-run effects of increasing the retirement age on workers and firms, uncovering patterns of substitutability across age cohorts and the importance of firms for the consequences of the policy. To analyze the welfare effects of this policy, I classify workers into two categories: older workers close to retirement ( $o$ ) and their co-workers ( $c$ ). The former group is directly affected by the policy in the short-run (i.e. the horizon of analysis), the latter group does not face any direct effect of the delayed retirement policy in the years immediately following the reform. The budget constraint of workers of type  $o$  is

$$x_{oj} \leq (1 - \tau_o^w) w_{oj} l_{oj}^w + p_o^{full} (T - R_o^{full}) I(R_o = R_o^{full}) \\ + p_o^{early} (T - R_o) I(R_o < R_o^{full}) + \sum_s p_{os} l_{osj} + T_o + y_o$$

Older workers can choose their retirement date  $R_o$ . If they early retire, they earn a benefit  $p_o^{early} < p_o^{full}$ . They work  $l^w$  hours in firms, earning wage  $w$ . A number of transfers are available and provide payments  $p_{o,s}$  for the time the workers receive them. Disability

insurance is available among these transfers together with non-work subsidies, sick leave benefits and short-time work. Similarly, their co-workers have a budget constraint of the following form

$$x_{cj} \leq (1 - \tau_c^w) w_{cj} l_{cj}^w + \sum_s p_{cs}^d l_{csj}^d + p_c^{early} (T - R_c) I(R_c < R^{full}) + T_c + y_c$$

Being not close enough to full retirement within the short-run, they can only claim early retirement benefits. For all workers the preferences over consumption and labor are represented by the following utility function<sup>10</sup>

$$u_i(x_i, l_i^w)$$

It follows that the marginal willingness to pay for the policy of incumbent older workers is

$$\sum_j \frac{\frac{dV_{oj}}{d\theta}}{\lambda_{oj}} = \sum_j n_{oj}^e \left[ (1 - \tau_o^w) l_{oj}^w \frac{dw_{oj}}{d\theta} - p_i^{full} \frac{dR^{full}}{d\theta} I(R_i = R^{full})|_{\theta=0} \right]$$

The first term measures the change in wages caused by firm-level adjustments, the second term is the mechanical effect of the policy on public pension payments. The marginal willingness to pay of the co-workers only includes the first term since these workers are not eligible for full retirement in the horizon of analysis. Bovini and Paradisi (2019) also document a significant effect of the policy on employment, which generates an associated utility cost of unemployment that affects the marginal willingness to pay. Assuming that the social marginal welfare weight is the same across the two groups of workers, the marginal value of public funds in units of consumption of older workers is

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<sup>10</sup>Utility can also depend on the time spent in retirement so that utility is  $u_i(x_i, l_i^w, T - R_i)$ . In this case one needs an estimate of  $\frac{\partial u_i}{\partial (T - R_i)}$  to approximate the cost of changing employment status. This is because if the change in status influences the retirement decision one must account for the change in utility caused by this choice.

$$MVPF_{p^{full}}^o = \frac{\overbrace{(1 - \tau_c^w) l_c^w \frac{dw_c}{d\theta} - p_i^{full} \frac{dR^{full}}{d\theta} \bar{n}_o^{full}}^{\text{Incumbents' marginal WTP}} - \overbrace{\sum_{i=o,c} \overline{\Delta c_i^f} \frac{df_i(\theta)}{d\theta} + \sum_{i=o,c} \overline{\Delta c_i^h} \frac{dh_i(\theta)}{d\theta}}^{\text{Utility cost of unemployment}}}{\underbrace{\frac{dt_c(\theta)}{d\theta} + \frac{dt_o(\theta)}{d\theta}}_{\text{Cost of the policy}}} \quad (3.9)$$

First, I calibrate the mechanical effect of the policy on full pension benefits  $p_i^{full} \frac{dR^{full}}{d\theta} \bar{n}_o^{full}$ . I use the median pension payment  $p_i^{full} = 16,279$ , and I set the share of older workers retiring at the full retirement age equal to 72.2 percent (Figure A3). Then I assume that the workers of type  $o$  do not face any significant change in their wage after the policy is implemented. The willingness to pay of workers  $c$  depends on the change in their wages within the firm, which the authors document in section 8.1. Incumbent co-workers face a cumulative drop in wages for an amount equal to  $-16,930$  euros.<sup>11</sup> I calibrate the tax rate following the calibrations provided by the authors.<sup>12</sup> I derive estimates of the cost of unemployment by using the effects of the reform on new hires and layoffs ( $\frac{df_i(\theta)}{d\theta}$  and  $\frac{dh_i(\theta)}{d\theta}$ ) and the cost of job losses provided in section 8.1.<sup>13</sup> For what concerns new hires, I calibrate the  $\Delta c_i^h$  using the procedure used to produce the estimates presented in the last column of Table 5. Finally, I compute the total effect of the reform on the government budget following the accounting model presented in section 9 and using the estimates reported in the Appendix of the paper. The marginal value of public funds under the individual-level approach would take the same form as (3.9), but would only include the components that refer to older workers. Additional details about the calibrations are reported in Table C.2.

As in the case of payroll tax cuts, I show that switching the focus to firms delivers different conclusions about the welfare impact of the policy (Table 3.1). When I only include

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<sup>11</sup>This change refers to the portion of the earnings drop that depends on within firm wage dynamics.

<sup>12</sup>I set  $\tau^w = 0.25$ .

<sup>13</sup>Combining the estimate on the increase in separations and the estimates of the cost of losing a job I calibrate a total cost of 6,314 euros for workers fired because of the reform.

older workers into the welfare calculation, the marginal value of public funds is 1.02. Older workers are willing to pay to avoid the increase in the full retirement age and retire earlier. However, the government saves a significant amount of money by delaying retirement so that benefits and costs are equal. When the spillovers on their co-workers are included, the figure completely changes: I estimate a marginal value of public funds of 4.97. The co-workers face a drop in earnings and a larger probability of losing/not finding a job, which causes a large willingness to pay. At the same time, as documented by Bovini and Paradisi (2019), the government saves a small share of mechanical savings due to behavioral responses, which reduces the gains of the policy in the short-run. Bovini and Paradisi (2019) discuss the need to quantify the long-run savings from the policy to draw correct conclusions about welfare. Regardless of the total welfare impact, these computations show the importance of shifting the focus of the welfare analysis to firms.

**Table 3.1:** *Marginal Value of Public Funds for Different Policies*

| Policy                                                      | MVPF (individual focus) | MVPF (firm focus)                                                     |
|-------------------------------------------------------------|-------------------------|-----------------------------------------------------------------------|
| Payroll Tax Subsidy (Saez <i>et al.</i> , 2019)             | 0.04                    | 1.51                                                                  |
| Raise in FRA (Bovini and Paradisi, 2019)                    | 1.03                    | 4.97                                                                  |
| Decrease in top income tax rate (Labanca and Pozzoli, 2018) | 0.69                    | $0.33 + 0.0001 \frac{\partial u_i}{\partial l_{ij}^w} / \lambda_{ij}$ |
| Increase in corporate tax rate (Fuest <i>et al.</i> , 2018) | N.A.                    | 0.16                                                                  |

### 3.4.3 Cut in top income tax rates

Firm production requires coordination between workers that might lead firms to pay a premium to employees in order to reduce their discretion in choosing working hours. Labanca and Pozzoli (2018) document this phenomenon in the context of Danish firms and show its implications for the effect of a decrease in top income tax rates. I apply my model to this policy to investigate the role of firms for the passthrough of changes in personal income taxes. I classify workers into two categories: top earners ( $t$ ) and low earners ( $l$ ). They are

employed in two types of firms: high coordination ( $hc$ ) and low-coordination ( $lc$ ). Workers in low-coordination firms optimize over the number of working hours; while employees in high-coordination firms take the number of hours as given. The budget constraint for a worker of type  $i$  is

$$x_{ij} \leq (1 - \tau_i) w_{ij} l_{ij}^w I(j = lc) + (1 - \tau_i) w_{ij} \bar{l}_{ij}^w I(j = hc) + T_i + y_i$$

The indicators  $I(j = lc)$  and  $I(j = hc)$  take value one if the worker is employed in a low-coordination or high-coordination firm, respectively. Since the authors show non significant effects of the policy on wages, I assume that wages stay constant.  $\tau_i$  is the personal income tax rate. The marginal willingness to pay for the tax change of individual  $i$  in firm  $j$  is

$$\sum_j \frac{dV_{ij}}{\lambda_{ij}} = \sum_j n_{ij}^e \left[ -l_{ij}^w w_{ij} \frac{d\tau_i}{d\theta} I(j = lc) - \bar{l}_{ij}^w w_{ij} \frac{d\tau_i}{d\theta} I(j = hc) + \frac{\partial u_i}{\partial \bar{l}_{ij}^w} \frac{d\bar{l}_{ij}^w}{d\theta} I(j = hc) \right]$$

The first two terms represent the mechanical effect of the tax change. The third term captures the first-order utility effect of the change in number of hours worked and it is proportional to the marginal disutility of hours worked. The marginal value of public funds is

$$MVPF_{P\tau}^t = \frac{\overbrace{\sum_{i=t,l} \frac{\eta_i}{\eta_t} \left( -\bar{l}_i^w w_i \frac{d\tau_i}{d\theta} \bar{n}_i^{lc} - \bar{l}_i^w w_i \frac{d\tau_i}{d\theta} \bar{n}_i^{hc} + \frac{\partial u_i}{\partial \bar{l}_{ij}^w} \frac{d\bar{l}_{ij}^w}{d\theta} \bar{n}_i^{hc} \right)}^{\text{Incumbents' marginal WTP}}}{\underbrace{\sum_{i=t,l} \left( -\bar{l}_i^w w_i \frac{d\tau_i}{d\theta} \bar{n}_i^{lc} - \bar{l}_i^w w_i \frac{d\tau_i}{d\theta} \bar{n}_i^{hc} - \tau_i w_i \frac{d\bar{l}_i^w}{d\theta} \bar{n}_i^{lc} - \tau_i w_i \frac{d\bar{l}_{ij}^w}{d\theta} \bar{n}_i^{hc} \right)}_{\text{Cost of the policy}}}$$

The decrease in tax rates generates a positive willingness to pay for incumbent employees. I derive estimates of the elasticity of hours worked from Tables 5 and 7 in the paper. I follow the procedure described in Appendix A.6.1 to determine the total behavioral responses. Labanca and Pozzoli (2018) show that income effects prevail in response to the policy so that labor hours decrease for top earners in high and low-coordination firms

following a cut in top income tax rates. These responses create a negative externality on the government budget that increases the fiscal cost. Spillovers on low-earners arise due to hours coordination and influence both the willingness to pay and the cost of the policy for the government. I estimate the marginal value of public funds of the policy leaving  $\frac{\frac{\partial u_i}{\partial l_{ij}^w}}{\lambda_{ij}}$  as an unknown. Because the change in working hours caused by coordination is very small, this term has second order effects on the welfare impact of the policy. However, the externality on the government budget caused by  $\frac{d\bar{l}_{ij}^w}{d\theta}$  changes the fiscal cost of the policy significantly. Ignoring the term in  $\frac{\frac{\partial u_i}{\partial l_{ij}^w}}{\lambda_{ij}}$  the marginal value of public funds changes from 0.69 to 0.33 when I include the spillovers from hours coordination.<sup>14</sup> Once again, firm-level behavioral responses significantly change the conclusions on the welfare impact of the policy.

### 3.4.4 Changes in corporate taxes

My model directly applies to the analysis of the welfare effects of firm-targeted policies. Fuest *et al.* (2018) study the effect of local corporate taxes on wages, testing alternative models of wage formation. I calibrate my model using estimates from their analysis. Suppose there is only one type of workers whose budget constraint has the same form as the one in (3.7). The marginal value of public funds is

$$MVPF_{pr\pi}^i = \frac{\overbrace{(1 - \tau_i^w) l_i^w \frac{dw_i}{d\theta}}^{\text{Incumbents' marginal WTP}} + \overbrace{\sum_{i=y,o} (1 - \tau_i^w) \Delta \bar{w}_i \frac{dn_i^e}{d\theta}}^{\text{Utility cost of unemployment}} - \overbrace{\frac{\psi^p}{\eta_i} \left( \bar{\pi} \frac{d\tau^\pi}{d\theta} + (1 - \tau^\pi) (1 + \tau^w) l_i^w \frac{dw_i}{d\theta} \right)}^{\text{Producer's surplus}}}{\underbrace{-\tau_i^w l_i^w \frac{dw_i}{d\theta} - \sum_{i=y,o} \tau_i^w \Delta \bar{w}_i \frac{dn_i^e}{d\theta} - \bar{\pi} \frac{d\tau^\pi}{d\theta} - \tau^\pi \frac{d\bar{\pi}}{d\theta}}_{\text{Cost of the policy}}}$$

The first term represents the marginal willingness to pay of incumbent workers, which is proportional to the effect of corporate taxes on wages. I calibrate it using the baseline

<sup>14</sup>Details about the calibrations are reported in Table C.3.

estimate in the paper, i.e. an elasticity of the wage to the local business tax rate of 0.39. I omit the cost of unemployment in my calibrations since the authors estimate it to be null. Finally, assuming that the municipal's GDP is approximated by the sum of value added across firms, I apply the elasticity of GDP (0.4) to profits.<sup>15</sup> I also assume that  $\psi^p = \eta_i$ . Additional details about the calibrations are reported in Table C.4. The estimate in Table (3.1) shows that the willingness to pay to avoid an increase in corporate taxes is significantly lower (in absolute value) than the revenues collected by the government. The reason is that tax changes have limited effects on wages and a null effect on unemployment, but they generate large increases in government revenues.

### 3.5 Conclusions

This paper develops a new framework to incorporate firm's responses to public policies into welfare analysis. The model builds on and extends the existing literature on sufficient statistics for welfare evaluations. The framework highlights the importance of two welfare components that are often disregarded in the standard approach. First, the utility effect of changes in employment that are caused by adjustments in firm's employment decisions. I propose a simple way to estimate this component based on an approximation that only requires to quantify the wage consequences of losing a job. Second, the model underlines the importance of firm-level price changes. This component introduces general equilibrium effects into the standard framework, and becomes crucial when firms respond to policies by adjusting the wages of incumbent workers and redistributing the costs/benefits of a policy among incumbent workers.

I implement the welfare formulas using estimates from the literature to prove that the model is easy to apply to the existing evidence. My calibrations show that incorporating firm's responses to the model sizably changes the estimates of the welfare impact. Although the estimates from a single paper are hard to take at face value and extrapolate to other

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<sup>15</sup>I take values for mean value added and firm employment from the working paper version since they are not reported in the published manuscript (ZEW Discussion Paper No. 16-003).

contexts, the relative contribution of firms to the welfare impact is likely to be relevant across various settings. I conclude that firms are an important channel for the passthrough of the effects of public policies.

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# Appendix A

## Appendix to Chapter 1

### A.1 The Italian labor Market

Italy is the European country that features the highest number of enterprises, totalling around 3.9 millions in the period 2008-2014.<sup>1</sup> 95 percent of Italian firms are considered micro-enterprises and have less than 9 employees. The share of workers employed in firms with less than 250 employees is around 66.8 percent, compared to 62.5 percent in Germany, 59.6 percent in France and 43.3 percent in the United States.<sup>2</sup> The share of employment in manufacturing is 18.2 percent, compared to 19.5 percent in Germany and 12.6 percent in France. As we conduct our analysis on firms having between 3 and 200 employees, we are considering a sample that is highly representative of the Italian productive landscape.

**Workforce demography:** The age structure of the Italian workforce underwent profound changes during the last decade. The share of workers aged between 55 and 64 has increased

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<sup>1</sup>Data from Eurostat, annual enterprise statistics. Financial and insurance sectors are included.

<sup>2</sup>Figures are the result of authors' computations that used the total number of workers employed in small and medium enterprises (data for 2012) and an average total employed population of 22 million people for Italy and 26 million for France (source: Eurostat, Statistics on small and medium-sized enterprises). Data for Germany are already provided as a percentage of total employment in Eurostat, Statistics on small and medium-sized enterprises. Data for the United States are based on computations in Jäger (2016).

from 31.4 percent in 2005 to 48.2 percent in 2015.<sup>3</sup> France and Germany experienced similar trends with a 10 percentage points and a 21 percentage points increase, respectively. Understanding the consequences of retaining older workers at firms is therefore of great relevance.

**Dismissals protection:** Italy is one of the countries with the highest degree of employment protection in Europe, together with Germany and France.<sup>4</sup> Fair dismissals carry no severance payments. Additional regulation, involving bargaining with unions, is imposed on collective dismissals (more than 5 workers) in firms with more than 15 employees. The 2015 *Jobs Act* revised the discipline of unfair individual dismissals for firms with more than 15 employees, to narrow the circumstances under which they lead to reinstate the worker. Specifically, for workers hired after March 2015, unfair dismissals that are not discriminatory only entail a severance payment that is a smooth function of tenure, capped at 24 months. This applies also to workers hired prior to that date as long as a firm crosses the 15-employee threshold because of new hires made after that date.

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<sup>3</sup>Source: Eurostat, Employment statistics.

<sup>4</sup>See OECD (2015) data on employment protection legislation.

## A.2 Additional Details about the Fornero reform

**Grandfathering clauses** The new rules introduced by the *Fornero* do not apply to individuals who satisfied the requirements to claim either an old-age or a seniority pension by the end of 2011. It was furthermore legislated that some specific categories of employees were grandfathered as well. These are mainly workers who, at the passage of the reform, were collocated on redundancy schemes or on short-time work programs. According to the law, the categories of private-sector employees who could still retire under pre-reform rules are therefore the following:

- i) Workers who satisfy requisite for claiming either an old-age or a seniority pension by 12/31/2011
- ii) Workers *collocati in mobilità* according to law 223/91 and based on collective agreements signed before 12/4/2011. Workers *collocati in mobilità* were laid-off workers who received a specific monetary support and were engaged in redeployment programs;
- iii) Workers who, as of 12/4/2011, were beneficiaries of *prestazioni straordinarie a carico dei fondi di solidarietà di settore*. These are workers on short-time work who received monetary support from *ad-hoc* sectoral solidarity funds
- iv) Workers who, as of 12/4/2011, had ceased to work but had been authorized to continue to pay contributions

In the following years, further specific categories of workers were grandfathered (the so-called *salvaguardati*). In our analysis, when predicting full retirement dates under pre- and post-reform rules, we only apply grandfathering clauses to workers listed in (i), because we do not have the necessary information to identify employees belonging to categories (ii) to (iv). Furthermore, as far as *salvaguardati* are concerned, we do not take into account changes to the law that occurred in later periods, because we aim to build a measure of the shock to the full retirement date at the time when the reform came into force.

**Other provisions** Beyond changes shown in Table 1.1 and Table A.1, the *Fornero* pension reform also introduced the following provisions, which we take into account when computing the predicted full retirement dates as described in sub-section 1.4.1:

- i) Women who were at least 60 years old and had at least 20 years of contribution by 2012 can exceptionally retire upon turning 64 years old in 2012, 64 and 3 months old in 2013-2015 and 64 years and 7 months old from 2016 onward. This possibility is granted to all workers who would have reached quota 96 in 2012.
- ii) Before 2012, individuals who started working in 1996 or later could also claim an old-age pension upon satisfying the same age requirement as other individuals, but conditional on a lower contribution requirement: 5 years of *effective* contributions, rather than 20. The *Fornero* reform maintains the same milder contribution requirement, but substantially raises the age requisite: 70 years old in 2012, 70 years and 3 months old in 2013-2015 and 70 years and 7 months old from 2016 onward.
- iii) Workers who started working after 1995 can also claim a seniority pension upon turning 63 years old in 2012, 63 years and 3 months old in 2013-2015 and 63 years and 7 months old from 2016 onward, conditional on having at least 20 years of qualifying contributions.

### A.3 Procedure to clean matched employer-employee data

Firm covariates and outcomes come from matched employer-employee data over the period 2009-2015. The unit of observation is the worker-firm relationship in a given month. More than one relationship between a worker and firm in a given month may exist. This is because firms are required to compile two *Uniemens* modules for a given employee if a characteristics of her contract changes during the month. In such a case, we isolate and retain only the prevailing relationship, according to the following multi-step procedure:

- i) We drop records where the wage is set equal to 0. If all records have this feature, we keep one randomly.
- ii) If there are records that feature the same contract characteristics (occupation, duration, full-time or part-time status, type of collective contract) and the same wage, we drop all but one randomly.
- iii) We drop records that feature lower numbers of paid days.
- iv) When multiple records arise in a given month, but not in the months immediately before or after that, we look at the characteristics of the worker-firm relationship in the preceding and in the following month. We then keep the single record that satisfies the following (ranked) criteria: a) modal occupation; b) wage closest to the average one in the neighbouring months; c) highest number of paid days; d) highest wage.<sup>5</sup> If more than one record survives criteria (a) to (d), we drop all but one randomly.
- v) When multiple records arise in each of a set of consecutive months, within each month we keep the single records that satisfies the following (ranked) criteria: a) highest number of paid days; b) highest wages.<sup>6</sup> If more than one record survives criteria (a) to (b), we drop all but one randomly.

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<sup>5</sup>If more than one records satisfies criterion (a), we then use criterion (b), and so on up to criterion (d).

<sup>6</sup>If more than one records satisfies criterion (a), we then use criterion (b).

## A.4 Computation of years of qualifying contribution

Contributions are of two types: *effective* contributions, which arise as a result of periods of paid work, and *figurative* contributions, which arise as a result of events that include sick leave, maternity leave, short-time work, unemployment and disability. *Figurative* contributions are not paid out by the workers, but they nevertheless accrue on their accounts. Depending on the type of pension, *figurative* contributions may not count toward the accrual of the right to retire (while still counting toward the determination of the amount of the pension benefit).

Workers' contribution histories record the event giving raise to each contribution spell, allowing to distinguish *effective* contributions from *figurative* ones. For every type of pension, we therefore only take into account relevant contributions, improving the accuracy of predicted retirement dates. We first sum contribution spells (expressed in weeks) in any given year, capping them at 52 weeks, which is the maximum number of weeks of contributions acquirable every year. Following rules for totalling contributions used at INPS, in case of (partially or totally) overlapping spells we avoid double counting. We then sum contributions across years, up to December 2011. The underlying assumption is that, in case of workers who accrue contributions across different funds, they choose to (onerously) exercise the so-called *ricongiunzione* option, which allows them to bring all contributions together into a unique fund, so that they can be summed toward the accrual of pension rights.

## A.5 Conceptual Framework

To guide our empirical analysis of firms' responses to pension reforms, we outline a labor demand model that features a shock to the retention rate of older workers. We focus on firm-driven changes in the employment of every type of worker. We then investigate how this response relates to the degree of substitutability between *potential retirees* and their co-workers. We start by analyzing a standard model where we remain agnostic about the wage formation process. We then study the behavior of labor demand in different wage bargaining settings. First, we analyze the standard Nash-bargaining model. Second, we introduce bargaining over profits to capture the profit-sharing behavior that has been documented by Card *et al.* (2014) in the Italian context. Third, we study a monopsonistic labor market with constant labor supply elasticity. Consistently across settings, the change in labor demand is inversely proportional to the degree of substitutability between *potential retirees* and co-workers.

### A.5.1 Labor Demand Model

Consider a two-period model where the firm chooses the optimal employment in period 1 given the employment in period 0. We assume that there are two types of workers: *potential retirees* ( $p$ ), and co-workers ( $c$ ). In our empirical setting *potential retirees* are older workers close to retirement, and co-workers are other older workers further away from retirement or younger employees in the same firm. Denote with  $n_0^c$  and  $n_1^c$  the number of co-workers employed in period 0 and 1, respectively. Adjustments in the demand for co-workers are referred to as  $x^c$ , so that  $n_1^c = n_0^c + x^c$ . A cost function  $c(x^c)$  accounts for the cost of adjusting the co-workers workforce, which is paid in period 0. We require  $c(\cdot)$  to be twice continuously differentiable and we assume that  $c'(x^c) > 0$  for  $x^c > 0$ ,  $c'(x^c) \leq 0$  for  $x^c < 0$ ,  $c'(0) = 0$  and  $c''(\cdot) \geq 0$ . This cost is flexible enough to incorporate any asymmetry in adjusting downwards or upwards the co-workers' labor demand. For the sake of simplicity, we assume that no *potential retiree* can be either hired or fired. We denote with  $n_0^p$  and  $n_1^p = sn_0^p$  the number of *potential retirees* in period 0 and 1, respectively.  $s \leq 1$

captures the exogenous share of *potential retirees* who are left in period 1. We interpret  $s$  as a variable incorporating the exogenous separation rate of *potential retirees* as well as retirement rules. Output is produced according to technology  $F(n_t^p, n_t^c)$  in every period  $t = 0, 1$ , with  $F_{pp}, F_{cc} \leq 0$ ,  $F_p, F_c \geq 0$ , and we impose no restriction on cross derivatives. The firm is wage and price taker, and the price of output is normalized to 1. The demand of co-workers in period 1 is chosen so as to maximize profits, which are given by

$$\pi = \pi_0 + \beta (F(sn_0^p, n_0^c + x^c) - w^p sn_0^p - w^c (n_0^c + x^c)) - c(x^c) \quad (\text{A.1})$$

where  $\pi_0$  are profits in period 0,  $\beta$  is a discount factor, and  $w^p$  and  $w^c$  are the wages in period 1 of *potential retirees* and co-workers respectively. Optimality conditions require the following

$$\beta (F_c(sn_0^p, n_1^c) - w^c) = c'(x^c) \quad (\text{A.2})$$

The firm equates the marginal increase in revenues net of wage expenditures to the marginal cost of adjusting co-workers' labor demand. A change in retirement rules that increases the retirement age can be approximated by a smaller than expected drop in the number of *potential retirees* in period 1, *i.e.* an increase in  $s$ . The comparative statics for a change in  $s$  is

$$\frac{\partial x^c}{\partial s} \propto \beta \left( F_{pc} n_0^p - \frac{\partial w^c}{\partial s} \right) \quad (\text{A.3})$$

The sign of the comparative statics depends on two terms. First, the degree of substitutability between the two types of labor captured by  $F_{pc}$ . Second, the extent to which wages adjust after the policy shock. If the two types of workers are substitutes, only a strong decrease in  $w^c$  can lead to an increase in the demand for co-workers. Indeed, in order to hire co-workers, the firm must significantly cut the payroll to compensate the loss in marginal productivity of co-workers that follows an exogenous increase in the number of *potential retirees*. However, wages are usually expected to be sticky, with the implication that when the two types of workers are substitutes we likely observe a drop in the demand of co-workers. We present here a few interesting cases. First, if wages are sticky (*i.e.*  $\partial w^c / \partial s = 0$ ), the response of co-workers' labor demand depends on the substitutability between co-workers and *potential*

*retirees*. If the two are substitutes - that is  $F_{pc} < 0$  - the firm decreases demand for co-workers. Second, if wages are flexible and partially follow the change in the marginal productivity of co-workers (i.e.  $\partial w^c / \partial s = \alpha n_0^p F_{pc}$  with  $\alpha < 1$ ), labor demand decreases as long as the two types of work are substitutes and  $F_{pc} < 0$ .<sup>7</sup> Finally, in a competitive labor market where wages reflect the marginal productivity of co-workers we would have no change in labor demand since prices fully adjust to absorb the shock.

**Result 1:** *Evidence of a drop in labor demand for co-workers can be reconciled with complementarity between co-workers and potential retirees only in case of a large increase in the wage of co-workers.*

We document in Section 1.6 a drop in the labor demand for co-workers in firms that are more affected by the reform. Moreover, a large increase in co-workers' wages is inconsistent with the evidence we provide in Section 1.8, which shows a drop in earnings for co-workers. We conclude that co-workers are substitutes for *potential retirees*. Our evidence also excludes patterns of no substitutability between workers (i.e.  $F_{pc} = 0$ ). Indeed, if this was the case, a drop in demand could not be reconciled with decreasing wages for co-workers.

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<sup>7</sup>There are different explanations for having co-workers' wages non perfectly reflecting their marginal productivity. Lazear (1979) shows in a dynamic model that an increasing wage path where older workers are overpaid can be used to provide incentives to young workers.

## A.5.2 Alternative Wage Formation Models

### Intrafirm Bargaining

So far we have been agnostic about the wage formation process. We now consider the case where wages are set according to Nash bargaining between the firm and individual workers as it is standard in the labor search literature. Suppose that co-workers have bargaining power  $\phi$  and outside option  $\underline{w}^c$ . Firms and workers bargain over the surplus generated by a match, which we write as a function of the marginal profit generated by the worker. We allow all wages to be re-negotiated in period 1. In equilibrium

$$\phi \frac{\partial \pi(s n_0^p, n_1^c)}{\partial n_1^c} = (1 - \phi)(w^c - \underline{w}^c) \quad (\text{A.4})$$

which implies the following expression for the equilibrium wage

$$w^c = \underbrace{\eta F_c - \frac{\eta}{\beta} c'(n_1^c - n_0^c)}_{\text{Marginal output net of adjustment costs}} + \underbrace{\frac{(1 - \phi)}{\phi \beta} \eta \underline{w}^c}_{\text{Reservation wage}} \quad (\text{A.5})$$

where  $\eta = \phi \beta / (\phi \beta + 1 - \phi)$ . When co-workers have no power in the bargaining the wage is set exactly equal to the outside option. The expression is analogous to the one derived by Cahuc *et al.* (2008). Wages in equilibrium are a function of co-workers' marginal output net of marginal cost and of co-worker's reservation wage. We are interested in the effect of a change in the separation rate on wages that reads:

$$\frac{\partial w^c}{\partial s} = \eta [F_{pc} n_0^p + F_{cc} \frac{\partial n_1^c}{\partial s}] - \frac{\eta}{\beta} c''(n_1^c - n_0^c) \frac{\partial n_1^c}{\partial s} \quad (\text{A.6})$$

The wage change in response to a shock to the retention rate depends on the cross-marginal product between co-workers and *potential retirees*, as well as on the slope of co-workers' marginal product. The last term in (A.6) arises since we do not assume linear hiring costs. Notice that we implicitly relied on the assumption that the worker's outside option does not change per effect of the reform. This is because we consider a firm-specific shock to the retirement age. The assumption would be violated if the general equilibrium effects of the reform were large.

By using (A.6) in (A.3) we get the following expression for the adjustment in labor demand of co-workers in period 1:

$$\frac{\partial x_1^c}{\partial s} = -\frac{\beta F_{pc} n_0^p}{\beta F_{pp} - c''(n_1^c - n_0^c)} \quad (\text{A.7})$$

When the reservation wage does not change, the shift in  $s$  does not have any first order effect on the wage. Hence, there is a one to one mapping between workers' complementarity and the change in labor demand.

**Result 2:** *In a model of intra-firm bargaining where workers and firms bargain over marginal profits and worker's surplus, there is a one-to-one relationship between changes in the labor demand of co-workers and the complementarity between the two types of labor. It follows that a drop in co-workers' labor demand caused by a change in  $s$  is only consistent with substitutability between potential retirees and co-workers.*

### Profit Sharing

Card *et al.* (2014) present evidence of substantial profit sharing in Italian firms. We extend our model to account for profit sharing by allowing firms and workers to bargain over total profits. In equilibrium

$$\phi \pi = (1 - \phi) (w^c - \underline{w}^c) \quad (\text{A.8})$$

This implies the following:

$$(1 - \phi + \beta \phi n_1^c) w^c = \beta \phi \left( (F - w^p s n_0^p) \beta - \frac{1}{\beta} c(n_1^c - n_0^c) \right) + (1 - \phi) \underline{w}^c \quad (\text{A.9})$$

Wages are determined by profits net of co-workers' cost and by worker's outside option. We totally differentiate equation (A.9) to find an expression for the wage response to a change in  $s$ :

$$\frac{\partial w^c}{\partial s} = \tilde{\eta} n_0^p (F_p - w^p) \quad (\text{A.10})$$

Because of an envelope argument, the effect of the reform on co-workers' wages is proportional to the wedge between *potential retirees'* productivity and wage. Intuitively, a larger gap increases the passthrough of the reform to co-workers' wages. In response to the change in profits caused by the reform, firms decrease the salary of co-workers to preserve the wedge for *potential retirees*.<sup>8</sup> After replacing (A.10) in (A.3) it follows that if wages for co-workers decline, labor demand can drop only in case  $F_{pc} < 0$ .

**Result 3:** *In a case where potential retirees get paid more than their productivity, the reform causes a drop in co-workers' salaries. Therefore, evidence of a fall in co-workers' labor demand can only be reconciled with substitutability between co-workers and potential retirees.*

### Monopsonistic Labor Market

We consider the broadly used model of monopsonistic labor demand and we solve a simple version with constant labor supply. Suppose the firm was not a price taker and chose employment anticipating the labor supply elasticity and the consequences of labor demand on the wage. We further assume that labor supply is such that  $n_1^c = w^e$ , where  $e$  is the elasticity of labor supply to the wage and  $e > 0$ . The firm's problem would become

$$\pi = \pi_0 + \beta \left( F(s n_0^p, n_1^c) - w^p s n_0^p - n_1^c \frac{1+e}{e} \right) - c(n_1^c - n_0^c) \quad (\text{A.11})$$

The firm's optimality condition in this model is

$$\beta \left( F_c - \frac{1+e}{e} n_1^c \frac{1}{e} \right) = c'(n_1^c - n_0^c) \quad (\text{A.12})$$

From A.12 we derive a new comparative statics

$$\frac{\partial x_1^c}{\partial s} = - \frac{F_{pc} n_0^p}{F_{cc} + \frac{1+e}{e^2} n_1^c \frac{1-e}{e} - \frac{1}{\beta} c''(n_1^c - n_0^c)} \quad (\text{A.13})$$

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<sup>8</sup>If firms were able to adjust *potential retirees'* wages the total pass-through on co-workers would be smaller.

The expression above shows a one-to-one mapping between labor demand changes and the substitutability between *potential retirees* and co-workers. The extent to which labor demand drops decreases with the elasticity of labor supply. When labor supply is more elastic, the firm has limited room to adjust co-workers' labor demand in response to the reform.

**Result 4:** *A monopsonistic labor market delivers a one-to-one relationship between co-workers' labor demand responses and the substitutability between co-workers and potential retirees. If the two types of work are substitutable, co-workers' labor demand falls in response to a shock to the retention rate of potential retirees.*

## A.6 Matching procedure and the cost of separations

**Matching procedure** Matching covariates are: age, sex, wage, occupation, dummy for permanent contract, experience, sector, province and firm size. We partition each variable in several bins and match only control workers who fall in the same combination of bins as at least one separated worker. We call this combination a strata. After we match separated workers to workers who do not separate from the firm we estimate the following specification:

$$Y_{it} = \alpha + \lambda_i + \sum_{k=-3}^3 \beta_k \gamma_k + \sum_{k=-3}^3 \beta_k^l \gamma_k \times \text{Separation}_i + \sum_{k=-3}^3 \beta_k^x \gamma_k \times X_i + \varepsilon_{i,t} \quad (\text{A.14})$$

$X_i$  is the vector of matching covariates. Since our sample ends in 2015 we estimate a model with only 3 periods after the separation to make sure all coefficients are identified by the same number of observations. For this reason, we focus on layoffs occurring in years 2012 and 2013. We then impute the estimate of  $\beta_3^l$  in (A.14) as the job loss four years after the separation event. Given the decreasing trend of the estimates, this assumption is likely conservative.

**Coarsened Exact Matching (CEM) weights** Let  $N_C$  and  $N_T$  be the number of control and treatment units in the matched sample. Suppose we have  $S$  strata where  $s = 1, \dots, S$  and each of them contains  $N_{T,s}$  treated unit and  $N_{C,s}$  control units. The CEM weight for a control unit is the following

$$w_i = \frac{N_C}{N_T} \times \frac{N_{T,s}}{N_{C,s}}$$

while each treated unit receives weight equal to 1 (see Iacus *et al.* (2011)). This guarantees that weights sum to total matched observations:

$$\begin{aligned} \sum_i w_i &= \sum_{i \in C} w_i + \sum_{i \in T} w_i = \sum_{i \in C} w_i + N_T \\ &= \frac{N_C}{N_T} \sum_s \sum_{i \in s} \frac{N_{T,s}}{N_{C,s}} + N_T \end{aligned}$$

$$= N_C + N_T$$

## A.7 The Fiscal Externality

### A.7.1 Derivation of The Fiscal Externality

We consider two types of agents and we define them as in our empirical analysis with the labels of workers *potential retirees* ( $p$ ) and co-workers ( $c$ ). Agents perform different labor-related activities. We call  $l_i^j$  the share of individuals of type  $i$  performing activity  $j$ . Each agent faces the following budget constraint:

$$x_i \leq (1 - \tau_i) w_i l_i^w + (1 - \tau_i) \left( NW_i l_i^{NW} + ST_i l_i^{ST} + D_i l_i^D + SL_i l_i^{SL} \right) + (1 - \tau_i^P) \left( P_i (T - T_i^P) \cdot I(T > T_i^P) + P_i^E l_i^E \right) + y_i \quad (\text{A.15})$$

where  $\{\tau_i, \tau_i^P, NW_i, ST_i, D_i, SL_i, P_i, T_i^P, P_i^E\}$  is a vector of policies targeted to agent  $i$ .  $\tau_i$  is an average labor earnings tax,  $\tau_i^P$  is a tax on pension payments,  $NW_i$  are non-work subsidies,  $ST_i$  are short-time work benefits,  $D_i$  are disability benefits,  $SL_i$  are benefits associated to sickness and leave,  $P_i$  are regular pension entitlements,  $T_i^P$  is the full retirement date, and  $P_i^E$  are pension benefits for workers who early retire.<sup>9</sup>  $T$  is our evaluation horizon.  $w_i$  denotes the wage, we denote total labor earnings with  $z_i = w_i l_i$  and non-work income with  $y_i$ . We model the reform as a change in the full retirement date  $T_i^P$ . If after an increase in  $T_i^P$  a worker retires at the previously expected date, she will receive a lower pension payment because  $P_i^E < P_i$ .

The fiscal externality of the policy is the share of mechanical revenues that is lost because of the behavioral responses:

$$FE = - \frac{\sum_{i=p,c} n_i \left[ (1 - \tau_i) \left( NW_i \frac{dl_i^{NW}}{dT_i^P} + ST_i \frac{dl_i^{ST}}{dT_i^P} + D_i \frac{dl_i^D}{dT_i^P} + SL_i \frac{dl_i^{SL}}{dT_i^P} \right) + (1 - \tau_i^P) P_i^E \frac{dl_i^E}{dT_i^P} - \tau_i \frac{dz_i}{dT_i^P} \right]}{\sum_{i=p,c} n_i dT_i^P (1 - \tau_i^P) P_i I(T > T_i^P)} \quad (\text{A.16})$$

where  $n_p$  and  $n_c$  denote the number of *potential retirees* and co-workers, respectively. The numerator represents the costs incurred by the government because of behavioral responses.

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<sup>9</sup>Notice that when workers early retire they do not receive the full pension payment  $P_i$ . Full pension outlays should be  $P_i (T - T_i^P) \cdot I(T > T_i^P) \cdot I(l_i^E = 0)$ , but we omit the  $I(l_i^E = 0)$  term to ease the notation. However, we take this aspect into consideration in our empirical implementation.

Mechanical revenues in the denominator instead measure the resources that the government would save through the policy from incumbent employees, absent any change in the behavior of workers and firms.

### A.7.2 Empirical implementation and results

The fiscal externality is a function of the estimates in Sections 1.6 and 1.8. The terms referring to  $NW$ ,  $ST$ ,  $D$  and  $LS$  in the numerator of (A.16) measure the budget consequences of the reform on policy instruments that are not affected by its dispositions. We quantify them using causal estimates of the effect of the reform on the different outcomes.<sup>10</sup> The last term in the numerator of (A.16) is the total effect on labor income tax revenues. It is a function of the causal effect of the reform on *potential retirees* and co-workers' earnings. Finally, the term  $P_i^E \frac{dl_i^E}{dT_i^P}$  measures the impact of changing the full retirement age on early retirement. To quantify it, we need estimates of  $\frac{dl_i^E}{dT_i^P}$  that we get by estimating the effect of the reform on months spent in retirement before the statutory retirement date. We calibrate  $P^E$  as a conservative 70 percent of the average and median value of monthly pension payments in the data (13,100 and 16,300 euros, respectively).<sup>11</sup> We check alternative parametrizations of  $\tau$  ranging from 20 percent to 30 percent for robustness. Notice that the average income tax rate for the median income (roughly 22,000 euros) is 24 percent without considering tax credits. We calibrate  $\tau_i^P$  starting from  $\tau_i$  and including the tax credit available for the median or average value of the pension payment, depending on the one we use in the calibration.<sup>12</sup> Finally, we obtain the mechanical effects in the denominator of (1.8) by subtracting the behavioral effect  $P_i^E \frac{dl_i^E}{dT_i^P}$  from causal estimates of the effect of the policy on pension outlays.

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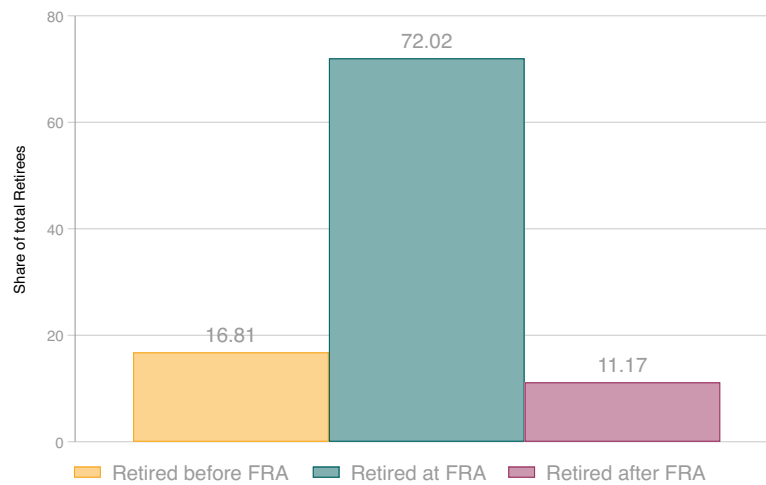
<sup>10</sup>Coefficients are reported in table A.11.

<sup>11</sup>Workers claiming *opzione donna* (the main early retirement option) get roughly 65 percent of full pension benefits in the data (INPS, 2016). Also, a small number of workers can retire before the statutory date obtaining full pension entitlements thanks to some provisions introduced after the reform (see Appendix A.2). Hence, our calibration understates the benefit received when they retire before the statutory date. We also show a calibration whereby  $P^e = 0.9 \times P$ .

<sup>12</sup>For pensions below 15,000 euros the tax credit is equal to  $1297 + (583 \times (15000 - \text{Pension})/7000)$ . For pensions between 15,000 and 55,000 euros the formula is  $1297 \times (55000 - \text{Pension})/40000$ .

## A.8 Supplementary Figures and Tables

**Figure A.1:** *Share of workers retiring at FRA*



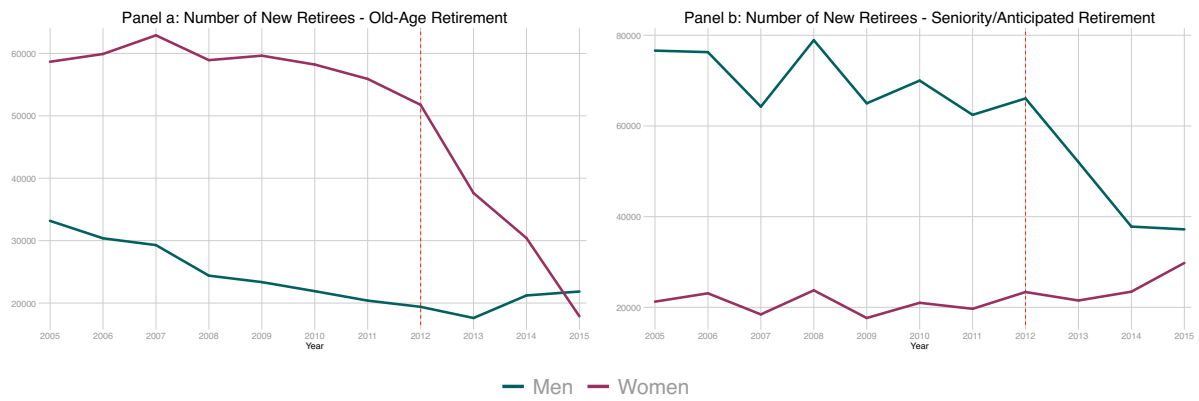
*Notes:* The figure shows the share of individuals who: i) retire in a 1-year window around the full retirement age ("Retired at FRA"); ii) retire more than 1 year before reaching the full retirement age ("Retired before FRA"); iii) retire more than 1 year after reaching the full retirement age ("Retired after FRA"). Shares are derived from authors calculations on the INPS register of retirees.

**Figure A.2:** Age of new retirees by gender and type of pension



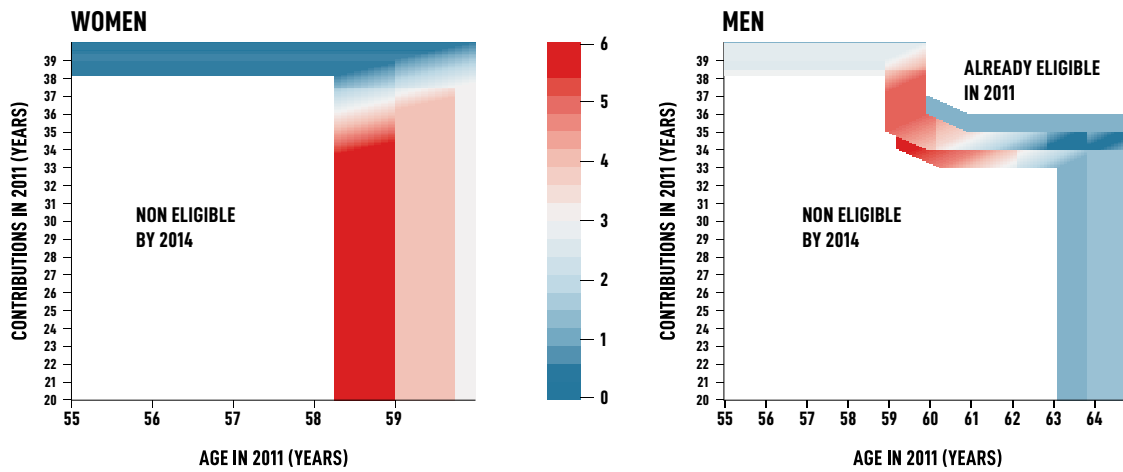
*Notes:* The figure shows the evolution of the average age at retirement, split by gender and type of pension. Panel A refers to old-age pensions, Panel B refers to seniority pensions. We classify as seniority pensions those labelled as “pensioni di anzianità” before the reform and as “pensioni anticipate” from 2012 onwards. The vertical dotted line represents the year when the *Fornero* pension reform becomes effective (2012).

**Figure A.3:** *Number of new retirees by gender and type of pension*



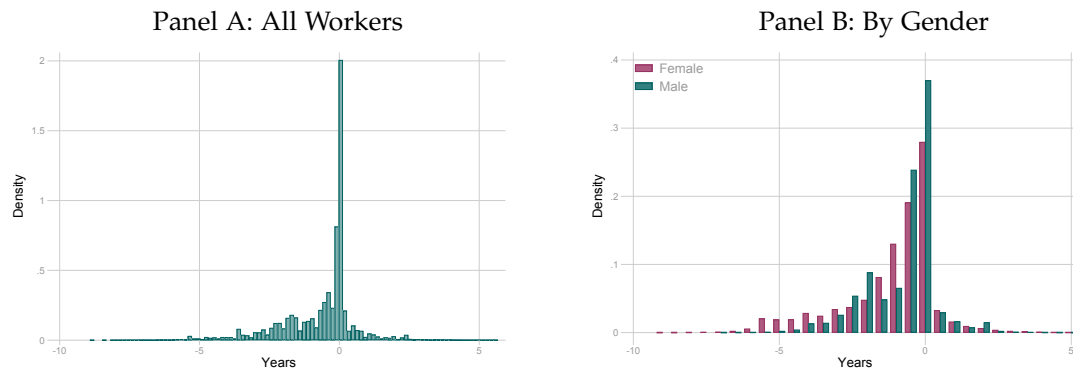
*Notes:* The figure shows the evolution of the number of new retirees, split by gender and type of pension. Panel A refers to old-age pensions, Panel B refers to seniority pensions. We classify as seniority pensions those labelled as “pensioni di anzianità” before the reform and as “pensioni anticipate” from 2012 onwards. The continuous lines plot two-years moving averages using lags only. The vertical dotted line represents the year when the *Fornero* pension reform becomes effective (2012).

**Figure A.4:** *Reform-induced changes in the retirement age*



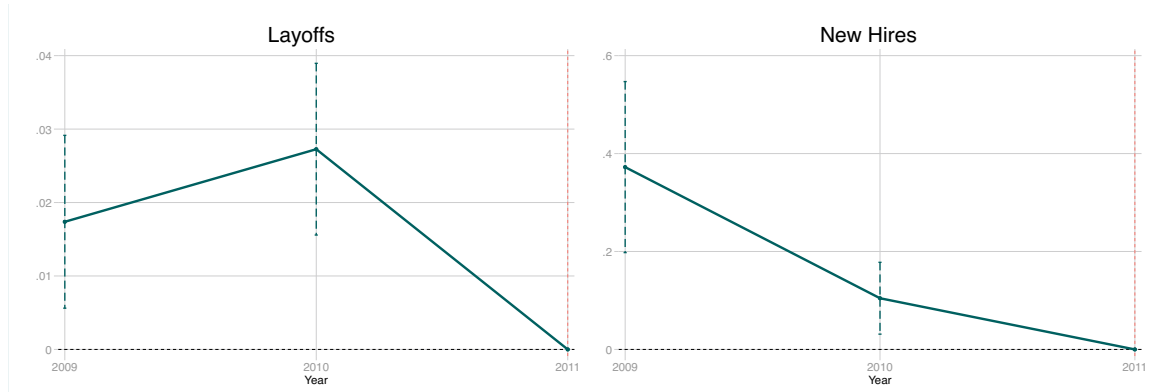
*Notes:* The figure plots heatmaps showing the relationship between the reform-induced shift in the retirement age and the characteristics of the worker in 2011. The characteristics are worker's gender, age and years of contributions in December 2011. The shifts are constructed using the rules detailed in Table A.1 under the assumptions listed in Section 1.4.1.

**Figure A.5:** *Post-reform retirement date - Forecast quality assessment*



*Notes:* The figure shows a forecast quality assessment of our worker-level treatment (as defined in equation 1.1). The horizontal axis measures the difference between the observed retirement date and the predicted full retirement date under post-reform rules. The sample includes all workers who were expected to retire by 2014 under pre-reform rules and retired in the period 2012-2017. A positive difference implies that a worker retires after her predicted full retirement date, a negative difference means that the individual retires earlier. Panel A shows the distribution for the entire sample of workers, Panel B shows the breakdown by gender. Number of workers = 160,527.

**Figure A.6:** Share of retained employees and trends in pre-reform labor demand



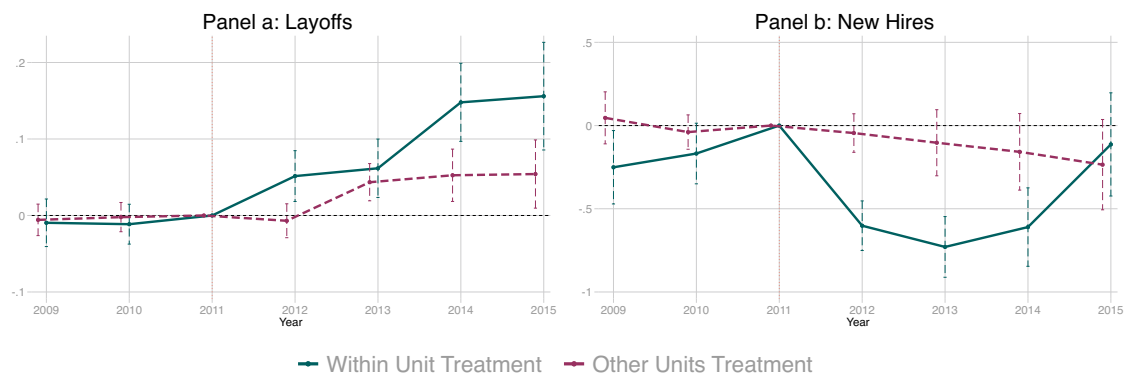
*Notes:* The figure shows the effect of retaining 10 percent of the total firm employees on layoffs and new hires in the pre-reform period, alongside 95 percent confidence intervals. Standard errors are clustered at the firm level. The plotted coefficients are  $\beta_{2009}^R$ ,  $\beta_{2010}^R$ , and  $\beta_{2011}^R$  from a version of specification (1.3) where the treatment is the number of retained *potential retirees* over the number of employees in 2011. We define *potential retirees* those full-time workers who were expected to retire within three years under the pre-reform rules when the reform is implemented. A *potential retiree* is retained if she retires more than one year after the pre-reform predicted retirement age. The regression is run on the universe of single-establishment private sector firms that (i) were active every year in the period 2009-2015, (ii) employed between 3 and 200 employees in the first period of 2009, and (iii) employed at least one *potential retiree* in the last quarter of 2011. Number of observations: 430,038. Pre-reform mean outcomes: layoffs = 0.39; new hires = 4.79.

**Figure A.7:** *Firms mostly respond to shock on potential retirees*



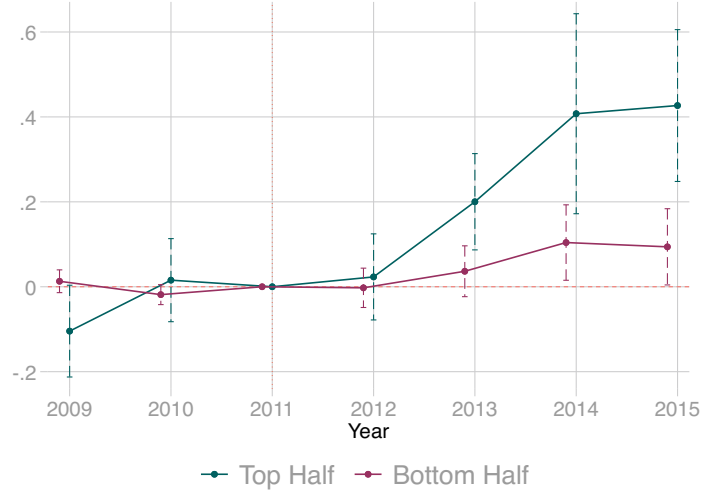
*Notes:* The figure shows the effect on layoffs (Panel A) and new hires (Panel B) of two treatments, alongside 95 percent confidence intervals. Standard errors are clustered at the firm level. The regression is based on a variant of specification (1.3) that features two treatments. The first treatment is defined as the number of retained workers who - under pre-reform rules - were expected to retire within 3 years when the reform is passed (i.e. those that we label as *potential retirees*). The second treatment is defined as the number of retained workers who - under pre-reform rules - were expected to retire in 4 to 7 years when the reform is passed. A worker is retained if she retires more than one year after the pre-reform predicted full retirement age. The two treatments are instrumented with the average shift in the full retirement date in the respective sample of workers. The regressions are estimated on the universe of single-establishment private sector firms that (i) were active every year in the period 2009-2015, (ii) employed between 3 and 200 employees in the first quarter of 2009, and (iii) employed at least one *potential retiree* in the last quarter of 2011. Furthermore, the sample is restricted to firms that in the last quarter of 2011 also had at least one worker who was expected to retire between 4 to 7 years later. Number of observations = 189,861. Mean outcomes pre-reform: layoffs = .47; new hires = 7.07.

**Figure A.8:** Labor demand within and across occupations



*Notes:* The figure shows the heterogeneous effect of the treatment on layoffs and new hires within and across occupations, alongside 95 percent confidence intervals. Standard errors are clustered at the firm level. Occupations define units in a firm and are: blue-collar, white-collar, and manager. The regression is a version of specification (1.3) where the unit of analysis is the firm-unit and two treatments are included. The first treatment measures the number of retained *potential retirees* employed at the firm-unit level when the reform is implemented; the second treatment is the number of retained *potential retirees* employed at the same firm but in other units when the reform is implemented. A worker is retained if she retires more than one year after the pre-reform predicted full retirement age. We define *potential retirees* those workers who were expected - under the pre-reform rules - to retire within 3 years when the reform is implemented (by 2014). The two treatments are instrumented with the average shift in the full retirement date in the respective sample of workers. The regressions are estimated on the universe of single-establishment private sector firms that (i) were active every year in the period 2009-2015, (ii) employed between 3 and 200 employees in the first quarter of 2009, (iii) had at least two units with at least three workers at the beginning of 2009, and (iv) employed at least one *potential retiree* in the last quarter of 2011. Number of observations = 580,734. Mean outcomes pre-reform at the unit level: layoffs = 0.14; new hires = 2.18.

**Figure A.9:** Layoffs in high and low-turnover firms

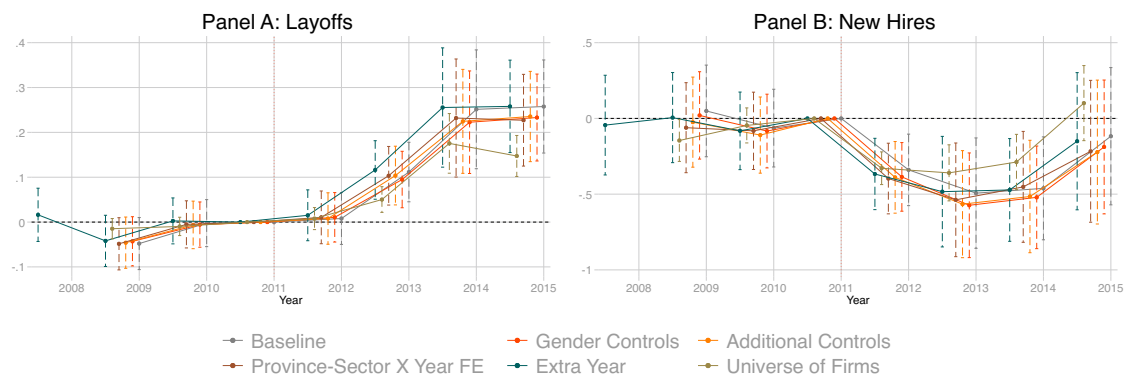


*Notes:* The figure shows the heterogeneous response of layoffs in firms which are above and below the median of turnover rates in the pre-reform period, alongside 95 percent confidence intervals. Standard errors are clustered at the firm level. The regression is based on a triple difference-in-differences specification:

$$Y_{it} = \lambda_i + \gamma_t + \sum_{k=2009}^{2015} \beta_k^R I(k=t) \times R_i + \sum_{k=2009}^{2015} \beta_k^{to} I(k=t) \times TO_i + \sum_{k=2009}^{2015} \beta_k^{R,to} I(k=t) \times R_i \times TO_i + \varepsilon_{it}$$

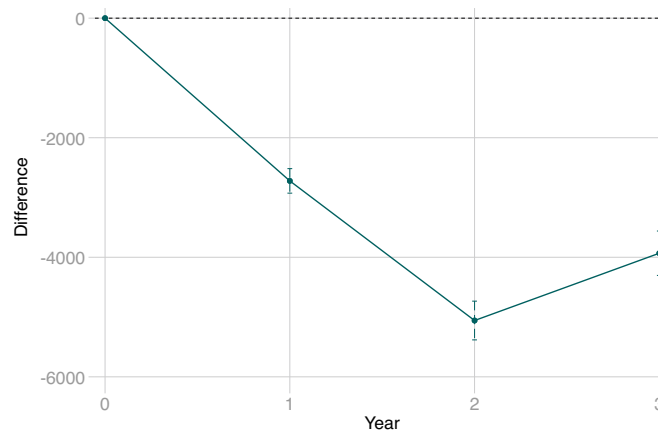
The plotted coefficients are the linear combination of  $\beta_k^R$ s and  $\beta_k^{R,to}$ s. Turnover rate is the share of separations over total employment in the pre-reform period (2009-2011). We label a separation as either a layoff, a quit or a termination of temporary contracts.  $TO_i$  takes value one if the firm lies in the top half of the distribution of turnover rates. The treatment  $R_i$  is defined as the number of retained *potential retirees* employed at the firm when the reform is implemented. We define *potential retirees* those workers who were expected to retire within three years (by 2014) under pre-reform rules when the reform is implemented. A *potential retiree* is retained if she retires at least one year after her pre-reform full retirement date. The number of retained *potential retirees* is instrumented with the firm-level average change in their full retirement date defined in equation (1.2). The regression is estimated on the universe of single-establishment private sector firms that (i) were active every year in the period 2009-2015, (ii) employed between 3 and 200 employees in the first quarter of 2009, and (iii) employed at least one *potential retiree* in the last quarter of 2011. Number of observations = 430,038. Mean outcome pre-reform = 0.39.

**Figure A.10: Robustness to alternative specifications and samples**



*Notes:* The figure addresses the robustness of the main estimates on layoffs (Panel A) and new hires (Panel B). As a first step, we confront the results of the baseline specification reported in Figures 1.3 and 1.4 (“Baseline”) with the results of sensitivity checks that employ the specification in (1.3) augmented to include control variables interacted with year fixed-effects. The Figure also shows 95 percent confidence intervals. Standard errors are clustered at the firm level. First, we control for quintiles of the share of female workers at the firm to take into account possibly differential trends across firms with a different gender composition (“Gender controls”). Second, we add to the vector of covariates dummies for quintiles of several other variables: firm size, firm age, the share of young (< 35), middle-aged (35 – 55) and older (> 55) workers (“Additional controls”). Third, we interact year fixed-effects with two-digit sector and province fixed effects, to capture heterogeneous business cycles across provinces and sectors (“Province-Sector  $\times$  Year FE”). As a second check, we include an extra year (2008) in the pre-reform period, to check pre-reform labour demands trends on a longer balanced panel of firms (“Extra Year”). Finally, we check whether results are robust to the inclusion in the estimation sample of firms that did not employ any *potential retiree* at the time of the reform (“Universe of Firms”).

**Figure A.11: Cost of Separations**



*Notes:* The figure shows the effect of separating from a firm on subsequent labor earnings. We code as separations layoffs and events of non renewal of temporary contracts. Estimates are obtained using a difference in differences strategy run on a sample constructed through a coarsened exact match of workers experiencing separations to similar workers across several covariates. We match on age, sex, wage, occupation, dummy for permanent contract, experience, sector, province and firm size. Number of observations: 1,240,824 Pre-reform mean outcome = 22,301.77

**Table A.1:** Pre and post-reform pension requirements - Additional Details

| Panel A: Old-age pension        |            |                      |             |          |              |              |
|---------------------------------|------------|----------------------|-------------|----------|--------------|--------------|
|                                 |            | Men                  |             |          | Women        |              |
|                                 |            | Pre-reform           | Post-reform |          | Pre-reform   | Post-reform  |
| <i>Age requirement</i>          |            |                      |             |          |              |              |
| 2011                            | 65YA       | Not in place         |             |          | 60YA         | Not in place |
| 2012                            | 65YA       | 66YA                 |             |          | 60YA         | 62YA         |
| 2013                            | 65YA+3MA   | 66YA+3MA             |             |          | 60YA+3MA     | 62YA+3MA     |
| 2014                            | 65YA+3MA   | 66YA+3MA             |             |          | 60YA+4MA     | 63YA+9MA     |
| 2015                            | 65YA+3MA   | 66YA+3MA             |             |          | 60YA+6MA     | 63YA+9MA     |
| 2016                            | 65YA+7MA   | 66YA+7MA             |             |          | 61YA+1MA     | 65YA+7MA     |
| 2017                            | 65YA+7MA   | 66YA+7MA             |             |          | 61YA+5MA     | 65YA+7MA     |
| 2018                            | 65YA+7MA   | 66YA+7MA             |             |          | 61YA+10MA    | 66YA+7MA     |
| <i>Contribution requirement</i> |            |                      |             |          |              |              |
|                                 |            | 20YC                 | 20YC        |          | 20YC         | 20YC         |
| <i>Waiting window</i>           |            |                      |             |          |              |              |
|                                 |            | 12 months            | No          |          | 12 months    | No           |
|                                 |            |                      |             |          |              |              |
| Panel B: Seniority pension      |            |                      |             |          |              |              |
|                                 |            | Pre-reform           |             |          | Post-reform  |              |
|                                 |            | Both genders         |             |          | Men          | Women        |
| 2011                            | Quota 96   | (60YA                | and 35 YC)  | or 40 YC | Not in place |              |
| 2012                            | Quota 96   | (60YA                | and 35 YC)  | or 40 YC | 42YC+1MC     | 41YC+1 MC    |
| 2013                            | Quota 97.3 | (61YA+3MA and 35 YC) |             | or 40 YC | 42YC+5MC     | 41YC+5MC     |
| 2014                            | Quota 97.3 | (61YA+3MA and 35 YC) |             | or 40 YC | 42YC+6MC     | 41YC+6MC     |
| 2015                            | Quota 97.3 | (61YA+3MA and 35 YC) |             | or 40 YC | 42YC+6MC     | 41YC+6MC     |
| 2016                            | Quota 97.6 | (61YA+7MA and 35 YC) |             | or 40 YC | 42YC+10MC    | 41YC+10MC    |
| 2017                            | Quota 97.6 | (61YA+7MA and 35 YC) |             | or 40 YC | 42YC+10MC    | 41YC+10MC    |
| 2018                            | Quota 97.6 | (61YA+7MA and 35 YC) |             | or 40 YC | 42YC+10MC    | 41YC+10MC    |
| <i>Waiting window</i>           |            |                      |             |          |              |              |
|                                 |            | 12 months            |             |          | No           |              |

*Note:* The table reports requirements to claim old-age (Panel A) and seniority (Panel B) pensions under pre-reform rules - had they remained in place - and under post-reform rules, for private sector employees over the period 2012-2018. It takes into account the anticipated upward adjustments due to increased life expectancy that took place in 2013 and 2016. YA and MA flag the age requirement in terms of years and months, respectively. YC and MC flag the contribution requirement in terms of years and months, respectively. The "waiting window" is the period that elapses between the date when an individual becomes eligible to claim a pension and the date when she can collect the first pension benefits. The "waiting window", that was set to 12 months in 2011, was then abolished by the 2012 *Fornero* pension reform.

**Table A.2: Placebo Tests**

|                         | Layoffs             | New Hires         |
|-------------------------|---------------------|-------------------|
| $T_i$ X Post 2009       | 0.0100*<br>(0.0053) | -0.017<br>(0.027) |
| $T_i$ X Post 2010       | 0.0054<br>(0.0053)  | 0.0015<br>(0.028) |
| Observations            | 184,302             | 184,302           |
| Mean Outcome (pre 2012) | 0.39                | 4.79              |
| Mean $T_i$              | 1.37                | 1.37              |
| Std. Dev. $T_i$         | 1.33                | 1.33              |

*Notes:* The table reports the coefficients from a set of placebo tests where we re-allocate the reform effective date either in year 2010 or in year 2011 and estimate the effect of a 1-year change of  $T_i$ , the firm-level shift in the full retirement date of *potential retirees*, on the main outcomes. We define *potential retirees* those workers who were expected to retire within three years under the pre-reform rules when the reform is implemented. The sample is restricted to the pre-reform period 2009-2011. The regression is based on a version of specification (1.5) where we replace  $R_i$  with  $T_i$ . The regressions are estimated on the universe of single-establishment private sector firms that (i) were active every year in the period 2009-2015, (ii) employed between 3 and 200 employees in the first quarter of 2009, and (iii) employed at least one *potential retiree* in the last quarter of 2011.

**Table A.3:** *Relation between the firm-level shift in the full retirement date and firm's covariates*

|                                     | Coefficient<br>(1)   | Dep. Var. Mean<br>(2) |
|-------------------------------------|----------------------|-----------------------|
| Share young workers (< 35)          | 0.000<br>(0.001)     | 0.297                 |
| Share middle-aged workers (35 – 55) | -0.009***<br>(0.001) | 0.58                  |
| Share old workers (> 55)            | 0.009***<br>(0.000)  | 0.123                 |
| Share male workers                  | -0.025***<br>(0.001) | 0.656                 |
| Share white-collar workers          | 0.009***<br>(0.001)  | 0.327                 |
| Average gross daily real wage       | 0.112<br>(0.450)     | 92.042                |
| Share full-time workers             | -0.007***<br>(0.001) | 0.885                 |
| Firm size                           | 0.604***<br>(0.082)  | 25.848                |
| Firm age                            | -0.065<br>(0.038)    | 19.935                |
| Firm in manufacturing               | -0.014***<br>(0.000) | 0.449                 |
| Firm in primary sector              | 0.000<br>(0.000)     | 0.013                 |
| Firm in services                    | 0.014***<br>(0.001)  | 0.537                 |
| N. firms                            | 61,434               |                       |

*Notes:* The table reports a set of balancing tests whereby firms' characteristics at the beginning of the period (first quarter of 2009) are regressed on  $T_i$ , the firm-level shift in the full retirement date of *potential retirees*. We define *potential retirees* those workers who were expected to retire within three years under the pre-reform rules when the reform is implemented. Column (1) reports coefficients and robust standard errors in parenthesis. We also have an alternative specification - available upon request - where we add to the regressions province fixed effects, sector fixed effects, as well as province  $\times$  sector fixed effects and we cluster standard errors at the province  $\times$  sector level. Column (2) displays mean values of the dependent variable. The regressions are estimated on the universe of single-establishment private sector firms that (i) were active every year in the period 2009-2015, (ii) employed between 3 and 200 employees in the first quarter of 2009, and (iii) employed at least one *potential retiree* in the last quarter of 2011.

**Table A.4:** *Firms with at least one potential retiree and other firms*

|                                | Sample |        | Other Firms |        |
|--------------------------------|--------|--------|-------------|--------|
|                                | mean   | sd     | mean        | sd     |
| Firm size                      | 25.85  | 32.05  | 8.06        | 9.83   |
| Firm age                       | 19.93  | 12.78  | 14.07       | 10.68  |
| Share in manufacturing         | 0.44   | 0.50   | 0.26        | 0.44   |
| Share in services              | 0.34   | 0.47   | 0.51        | 0.50   |
| Share male workforce           | 0.66   | 0.30   | 0.55        | 0.35   |
| Share workforce aged $\leq 35$ | 0.30   | 0.19   | 0.46        | 0.28   |
| Share workforce aged (35-55]   | 0.58   | 0.19   | 0.49        | 0.27   |
| Share workforce aged $> 55$    | 0.12   | 0.12   | 0.05        | 0.11   |
| Avg. workforce tenure          | 8.21   | 4.82   | 5.67        | 4.13   |
| Avg. workforce experience      | 16.46  | 4.21   | 12.41       | 4.94   |
| Share blue collars             | 0.61   | 0.32   | 0.56        | 0.37   |
| Share white collars            | 0.33   | 0.30   | 0.34        | 0.36   |
| Share managers                 | 0.02   | 0.07   | 0.01        | 0.06   |
| Share full-time contracts      | 0.89   | 0.17   | 0.74        | 0.30   |
| Share open-ended contracts     | 0.92   | 0.15   | 0.90        | 0.19   |
| Avg. real daily wage           | 92.04  | 146.07 | 78.89       | 188.87 |
| Observations                   | 61,434 |        | 333,800     |        |

*Notes:* The table reports descriptive statistics for firms in our sample, as well as for other single-establishment firms in the same size class (3-200) that remain active throughout the period 2009-2015. Firm's characteristics are measured at the beginning of the period (first quarter of 2009). Average workforce tenure and experience are truncated at 27 years, because matched employer-employee data are available since 1983. Firms in our sample are (i) single-establishment, (ii) were active every year in the period 2009-2015, (iii) employed between 3 and 200 employees in the first quarter of 2009, and (iv) employed at least one *potential retiree* in the last quarter of 2011.

**Table A.5:** Potential retirees *and other workers*

|                                  | <i>Potential retirees</i> |        | Other workers |        |
|----------------------------------|---------------------------|--------|---------------|--------|
|                                  | mean                      | sd     | mean          | sd     |
| Male                             | 0.71                      | 0.46   | 0.71          | 0.45   |
| Age                              | 57.77                     | 2.89   | 40.91         | 9.77   |
| Tenure                           | 14.24                     | 9.37   | 8.81          | 7.44   |
| Experience in private sector     | 23.86                     | 8.60   | 15.30         | 9.81   |
| Years since entered labor market | 39.80                     | 7.58   | 20.53         | 15.77  |
| Blue collar                      | 0.66                      | 0.47   | 0.60          | 0.49   |
| White collar                     | 0.29                      | 0.45   | 0.34          | 0.47   |
| Manager                          | 0.05                      | 0.21   | 0.04          | 0.19   |
| Open-ended contract              | 0.96                      | 0.20   | 0.90          | 0.30   |
| Daily gross real wage            | 109.74                    | 114.52 | 102.17        | 111.78 |
| Work in manufacturing            | 0.50                      | 0.50   | 0.52          | 0.50   |
| Work in services                 | 0.30                      | 0.46   | 0.28          | 0.45   |
| Firm size                        | 45.99                     | 60.72  | 75.14         | 81.42  |
| Observations                     | 98,358                    |        | 1,434,381     |        |

*Notes:* The table reports the characteristics at the beginning of the period (first quarter of 2009) of *potential retirees* and full-time co-workers employed in firms belonging to our sample. Tenure and experience are truncated at 27 years, because matched employer-employee data are available since 1983 only. Firms in our sample are (i) single-establishment, (ii) were active every year in the period 2009-2015, (iii) employed between 3 and 200 employees in the first quarter of 2009, and (iv) employed at least one *potential retiree* in the last quarter of 2011.

**Table A.6:** *Absences from work for employees closer and further away from retirement*

|                                      | Away from retirement |           | Close to retirement |           | Difference |
|--------------------------------------|----------------------|-----------|---------------------|-----------|------------|
|                                      | Mean                 | Std. Dev. | Mean                | Std. Dev. |            |
| Prob. sickness                       | 0.29                 | 0.45      | 0.34                | 0.47      | 0.049***   |
| Prob. work-related injury            | 0.05                 | 0.22      | 0.06                | 0.24      | 0.008***   |
| Prob. leave                          | 0.03                 | 0.17      | 0.04                | 0.20      | 0.009***   |
| Monetary cost of sickness            | 159.53               | 1625.58   | 195.81              | 1604.52   | 36.280***  |
| Monetary cost of work-related injury | 41.99                | 364.48    | 50.58               | 537.53    | 8.585***   |
| Monetary cost of leave               | 12.14                | 229.75    | 21.39               | 1197.77   | 9.249***   |
| N. workers                           | 609,079              |           | 127,158             |           |            |

*Notes:* The table reports the probability of being absent from work due to sickness, work-related injury or leave during 2011, as well as the associated monetary cost, for employees within 3 years from retirement under pre-reform rules and similar employees further away from retirement who are matched - via an exact matching procedure - along several dimensions. Matching covariates are: age, experience, gender, full-time and open-ended status, qualification, as well as firm's province, sector and size. The last column reports the difference in means between the third and the first column.

**Table A.7:** *The effect of the reform on layoffs*

|                          | All                | Young               | Middle-aged        | Old                  | Old<br>Non-potential<br>retirees |
|--------------------------|--------------------|---------------------|--------------------|----------------------|----------------------------------|
|                          | (1)                | (2)                 | (3)                | (4)                  | (5)                              |
| $R_i \times I(t = 2009)$ | -0.048<br>(0.030)  | -0.024*<br>(0.013)  | -0.031*<br>(0.018) | 0.0078<br>(0.0061)   | -0.0041<br>(0.0046)              |
| $R_i \times I(t = 2010)$ | -0.0024<br>(0.027) | -0.015<br>(0.011)   | -0.0017<br>(0.017) | 0.014**<br>(0.0060)  | 0.0033<br>(0.0046)               |
| $R_i \times I(t = 2012)$ | 0.0080<br>(0.029)  | 0.0029<br>(0.012)   | 0.013<br>(0.019)   | -0.0078<br>(0.0073)  | -0.0033<br>(0.0055)              |
| $R_i \times I(t = 2013)$ | 0.11***<br>(0.034) | 0.031**<br>(0.013)  | 0.045**<br>(0.021) | 0.035***<br>(0.0076) | 0.013**<br>(0.0058)              |
| $R_i \times I(t = 2014)$ | 0.25***<br>(0.068) | 0.060***<br>(0.017) | 0.12***<br>(0.045) | 0.069***<br>(0.012)  | 0.033***<br>(0.010)              |
| $R_i \times I(t = 2015)$ | 0.26***<br>(0.053) | 0.044***<br>(0.014) | 0.12***<br>(0.034) | 0.093***<br>(0.012)  | 0.039***<br>(0.010)              |
| N. obs.                  | 430,038            | 430,038             | 430,038            | 430,038              | 430,038                          |
| Mean outcome pre-2012    | 0.39               | 0.13                | 0.2                | 0.06                 | 0.04                             |
| Mean $R_i$               | 0.83               | 0.83                | 0.83               | 0.83                 | 0.83                             |
| Std. Dev. $R_i$          | 0.99               | 0.99                | 0.99               | 0.99                 | 0.99                             |
| Mean $T_i$               | 1.37               | 1.37                | 1.37               | 1.37                 | 1.37                             |
| Std. Dev. $T_i$          | 1.33               | 1.33                | 1.33               | 1.33                 | 1.33                             |

*Notes:* The table is based on specification (1.3). Standard errors in parentheses are clustered at the firm level. Column (1) shows the effect of the reform on total layoffs, columns (2) to (5) show the effect on layoffs of young (below 35), middle-aged (35-55), old (over 55) and old non-potential retirees, respectively. The treatment is defined as the number of retained *potential retirees* employed at the firm when the reform is implemented. A *potential retiree* is retained if she retires at least one year after her pre-reform full retirement date. The number of retained *potential retirees* is instrumented with the firm-level average change in their full retirement date defined in equation (1.2). The coefficient  $R_i \times I(t = k)$  shows how the difference in layoffs between firms with different levels of  $R_i$  in year  $k$  compares to year 2011, when the difference is normalized to zero. The regressions are estimated on the universe of single-establishment private sector firms that (i) were active every year in the period 2009-2015, (ii) employed between 3 and 200 employees in the first quarter of 2009, and (iii) employed at least one *potential retiree* in the last quarter of 2011.

\*\*\*  $p < 0.01$ ; \*\*  $p < 0.05$ ; \*  $p < 0.1$ .

**Table A.8:** *The effect of the reform on hiring*

|                          | All                | Young               | Middle-aged         | Old                  | Permanent Contract | Temporary Contract |
|--------------------------|--------------------|---------------------|---------------------|----------------------|--------------------|--------------------|
|                          | (1)                | (2)                 | (3)                 | (4)                  | (5)                | (6)                |
| $R_i \times I(t = 2009)$ | 0.049<br>(0.15)    | 0.061<br>(0.080)    | -0.0081<br>(0.081)  | -0.0033<br>(0.019)   | 0.014<br>(0.083)   | 0.035<br>(0.12)    |
| $R_i \times I(t = 2010)$ | -0.064<br>(0.13)   | -0.016<br>(0.067)   | -0.041<br>(0.071)   | -0.0070<br>(0.019)   | 0.016<br>(0.070)   | -0.080<br>(0.11)   |
| $R_i \times I(t = 2012)$ | -0.34***<br>(0.12) | -0.14*<br>(0.073)   | -0.15**<br>(0.062)  | -0.047***<br>(0.017) | -0.036<br>(0.065)  | -0.30***<br>(0.10) |
| $R_i \times I(t = 2013)$ | -0.49***<br>(0.19) | -0.26***<br>(0.094) | -0.21**<br>(0.10)   | -0.022<br>(0.026)    | -0.13<br>(0.10)    | -0.37**<br>(0.15)  |
| $R_i \times I(t = 2014)$ | -0.46***<br>(0.17) | -0.23**<br>(0.099)  | -0.23***<br>(0.082) | -0.00016<br>(0.022)  | -0.058<br>(0.071)  | -0.40***<br>(0.15) |
| $R_i \times I(t = 2015)$ | -0.12<br>(0.23)    | -0.100<br>(0.12)    | -0.064<br>(0.11)    | 0.046<br>(0.033)     | 0.22*<br>(0.11)    | -0.33*<br>(0.18)   |
| N. obs.                  | 430,038            | 430,038             | 430,038             | 430,038              | 430,038            | 430,038            |
| Mean outcome pre-2012    | 4.79               | 2.38                | 2.06                | .35                  | 1.46               | 3.32               |
| Mean $R_i$               | 0.83               | 0.83                | 0.83                | 0.83                 | 0.83               | 0.83               |
| Std. Dev. $R_i$          | 0.99               | 0.99                | 0.99                | 0.99                 | 0.99               | 0.99               |
| Mean $T_i$               | 1.37               | 1.37                | 1.37                | 1.37                 | 1.37               | 1.37               |
| Std. Dev. $T_i$          | 1.33               | 1.33                | 1.33                | 1.33                 | 1.33               | 1.33               |

*Notes:* The table is based on specification (1.3). Standard errors in parentheses are clustered at the firm level. Column (1) shows the effect on total new hires, columns (2) to (4) show the effects on new hires of young (below 35), middle-aged (35-55) and old (over 55) workers, respectively. The treatment is defined as the number of retained *potential retirees* employed at the firm when the reform is implemented. A *potential retiree* is retained if she retires at least one year after her pre-reform full retirement date. The number of retained *potential retirees* is instrumented with the firm-level average change in their full retirement date defined in equation (1.2). The coefficient  $R_i \times I(t = k)$  shows how the difference in hiring between firms with different levels of  $R_i$  in year  $k$  compares to year 2011, when the difference is normalized to zero. The regressions are estimated on the universe of single-establishment private sector firms that (i) were active every year in the period 2009-2015, (ii) employed between 3 and 200 employees in the first quarter of 2009, and (iii) employed at least one *potential retiree* in the last quarter of 2011.

\*\*\*  $p < 0.01$ ; \*\*  $p < 0.05$ ; \*  $p < 0.1$ .

**Table A.9:** *Firms only respond to shock to potential retirees*

|                                 | Layoffs             | New Hires           |
|---------------------------------|---------------------|---------------------|
| $R_i^3 \times \text{Post}$      | 0.144***<br>(0.041) | -0.493**<br>(0.249) |
| $R_i^{4-7} \times \text{Post}$  | -0.001<br>(0.017)   | -0.053<br>(0.112)   |
| Observations                    | 189,861             | 189,861             |
| Mean outcome pre-2012           | 0.47                | 7.07                |
| KP F-Statistics                 | 1304.05             | 1304.05             |
| Mean $R_i^3$                    | 1.27                | 1.27                |
| Std. Dev. $R_i^3$               | 1.44                | 1.44                |
| Mean $R_i^{4-7}$                | 2.88                | 2.88                |
| Std. Dev. $R_i^{4-7}$           | 3.53                | 3.53                |
| Mean $T_i^3$                    | 1.37                | 1.37                |
| Std. Dev. $T_i^3$               | 1.21                | 1.21                |
| Mean $T_i^{4-7}$                | 2.94                | 2.94                |
| Std. Dev. $T_i^{4-7}$           | 2.13                | 2.13                |
| P-Value Difference Coefficients | 0.003               | 0.084               |

*Notes:* The table reports the results of specification (1.5) on new hires and layoffs when two treatments are included. Standard errors in parentheses are clustered at the firm level. The first treatment is defined as the number of retained *potential retirees* who - under pre-reform rules - were expected to retire within 3 years when the reform is passed (i.e. those that we label as *potential retirees*,  $R_i^3$ ). The second treatment is defined as the number of retained workers who - under pre-reform rules - were expected to retire in 4 to 7 years when the reform is passed ( $R_i^{4-7}$ ). A worker is retained if she retires more than one year after the pre-reform predicted full retirement age. The two treatments are instrumented with the average shift in the full retirement date in the respective sample of workers. The Table also reports the p-value of a test for the difference between the two reported coefficients. The regressions are estimated on the universe of single-establishment private sector firms that (i) were active every year in the period 2009-2015, (ii) employed between 3 and 200 employees in the first quarter of 2009, and (iii) employed at least one *potential retiree* in the last quarter of 2011. Furthermore, the sample is restricted to firms that in the last quarter of 2011 also have at least one worker who was expected to retire between 4 to 7 years later.

\*\*\*  $p < 0.01$ ; \*\*  $p < 0.05$ ; \*  $p < 0.1$ .

**Table A.10:** *The effect of the reform on incumbents' labor earnings*

|                          | <i>Potential retirees</i> |                         | Co-workers                        |                       |                        |                       |
|--------------------------|---------------------------|-------------------------|-----------------------------------|-----------------------|------------------------|-----------------------|
|                          | All                       | All                     | All<br>(w/ non-work<br>subsidies) | Young                 | Middle-aged            | Older                 |
|                          | (1)                       | (2)                     | (3)                               | (4)                   | (5)                    | (6)                   |
| $T_i \times I(t = 2009)$ | 269.8**<br>(130.3)        | -1463.3<br>(2299.5)     | -1495.0<br>(2297.9)               | -473.8<br>(781.7)     | 1901.8<br>(1442.8)     | 60.1<br>(283.5)       |
| $T_i \times I(t = 2010)$ | 167.2<br>(104.8)          | -334.4<br>(1279.9)      | -366.0<br>(1277.8)                | 86.6<br>(415.0)       | 1357.0*<br>(753.9)     | -85.7<br>(224.4)      |
| $T_i \times I(t = 2012)$ | 2496.1***<br>(170.1)      | -3582.4***<br>(1347.6)  | -2910.3**<br>(1316.8)             | -627.6*<br>(343.0)    | -1564.0*<br>(877.3)    | -856.5***<br>(269.1)  |
| $T_i \times I(t = 2013)$ | 6272.1***<br>(287.9)      | -5934.4***<br>(2215.3)  | -3775.4*<br>(2022.9)              | -1002.2*<br>(533.5)   | -2763.1*<br>(1459.6)   | -1574.5***<br>(410.6) |
| $T_i \times I(t = 2014)$ | 7530.1***<br>(417.9)      | -10546.6***<br>(2715.7) | -6678.4***<br>(2408.4)            | -1682.5**<br>(665.9)  | -5597.6***<br>(1820.0) | -2347.4***<br>(519.1) |
| $T_i \times I(t = 2015)$ | 8549.9***<br>(520.2)      | -13379.4***<br>(3102.3) | -8566.2***<br>(2817.0)            | -2066.5***<br>(775.3) | -7941.9***<br>(2096.4) | -2912.7***<br>(632.1) |
| N. obs.                  | 511,980                   | 511,847                 | 511,847                           | 511,847               | 511,847                | 511,847               |
| Mean outcome pre-2012    | 49985.63                  | 621,032.16              | 621,137.28                        | 130,288.84            | 412,155.77             | 44,559.9              |
| Mean $T_i$               | 1.36                      | 1.35                    | 1.35                              | 1.35                  | 1.35                   | 1.35                  |
| Std. Dev. $T_i$          | 1.34                      | 1.33                    | 1.33                              | 1.33                  | 1.33                   | 1.33                  |

*Notes:* The table is based on specification (1.6) and shows the effect of the reform on total labor earnings of the workers close to retirement and their co-workers who share incumbency in a firm at the reform date. Standard errors in parentheses are clustered at the firm level. Column (1) and (2) do not include non-work subsidies, while column (2) does. Column (3) to (5) display the effect on full-time young (below 35), middle-aged (35-55), older (over 55) co-workers, respectively. The treatment is defined as the firm-level average change in the full retirement date of *potential retirees* defined in equation (1.2). The coefficient  $T_i \times I(t = k)$  shows the effect of a  $1\sigma$  increase in  $T_i$  on labor earnings in year  $k$ , relative to year 2011 when the effect is normalized to zero. The regressions are estimated on the universe of single-establishment private sector firms that (i) were active every year in the period 2009-2011, (ii) employed between 3 and 200 employees in the first quarter of 2009, and (iii) employed at least one *potential retiree* in the last quarter of 2011. Observations are weighted according to firm size at baseline.

\*\*\*  $p < 0.01$ ; \*\*  $p < 0.05$ ; \*  $p < 0.1$ .

**Table A.11:** *The effect of the reform on potential retirees and co-workers*

|                           | <i>Potential retirees</i> | <i>Co-workers</i>           |
|---------------------------|---------------------------|-----------------------------|
| Labor earnings            | 18512.150***<br>(996.510) | -24967.670***<br>(6440.490) |
| Pension entitlements      | -18194.5***<br>(512.820)  | 713.090<br>(578.690)        |
| Disability benefits       | 212.900***<br>(24.920)    | 122.330<br>(150.470)        |
| Short-time work subsidies | 509.909***<br>(114.189)   | -4408.482*<br>(2494.71)     |
| Non-work Subsidies        | 1393.060***<br>(122.800)  | 8594.920***<br>(2171.800)   |
| Sick and leave benefits   | 513.440***<br>(45.550)    | -320.440<br>(379.010)       |
| Early retirement (months) | 2.030***<br>(0.140)       | 0.930***<br>(0.270)         |

*Notes:* The table reports the sum of coefficients  $\{\beta_k^T\}_{k=2012}^{k=2015}$  from the specification in (1.6) where we use as treatment the firm-level average change in the full retirement date of *potential retirees* defined in equation (1.2). Standard errors in parentheses are clustered at the firm level. Column (1) reports the estimates for the sample of *potential retirees*, while column (2) displays the effect on their co-workers. The regressions are estimated on the universe of single-establishment private sector firms that (i) were active every year in the period 2009-2011, (ii) employed between 3 and 200 employees in the first quarter of 2009, and (iii) employed at least one *potential retiree* in last quarter of 2011.

\*\*\*  $p < 0.01$ ; \*\*  $p < 0.05$ ; \*  $p < 0.1$ .

## Appendix B

# Appendix to Chapter 2

### B.1 Model - Frictionless case

Suppose there were no frictions such that incumbent workers and new hires were perfect substitutes. If this was the case  $f(\mathbf{L}^{\text{inc}}, \mathbf{L}^{\text{hire}}) = f(\mathbf{L}^{\text{inc}} + \mathbf{L}^{\text{hire}})$ . Under the assumption that the firm hires a positive number of workers

$$\tilde{\Pi}_j^*(\mathbf{L}^{\text{inc}}, \mathbf{L}^{\text{hire}}) - \tilde{\Pi}_j^*(\mathbf{L}^{\text{inc}}_{-i}, \mathbf{L}^{\text{hire}}) = (1 + \tau_i) \underline{w}_i L_i^{\text{inc}} \quad (\text{B.1})$$

Using (B.1) and (2.3), it follows that  $\Omega_j = 0$ . When  $\Omega_j = 0$ , workers are paid the outside option determined in a perfectly competitive labor market.

## B.2 Supplementary Figures

**Figure B.1:** *Working Hours Distribution*

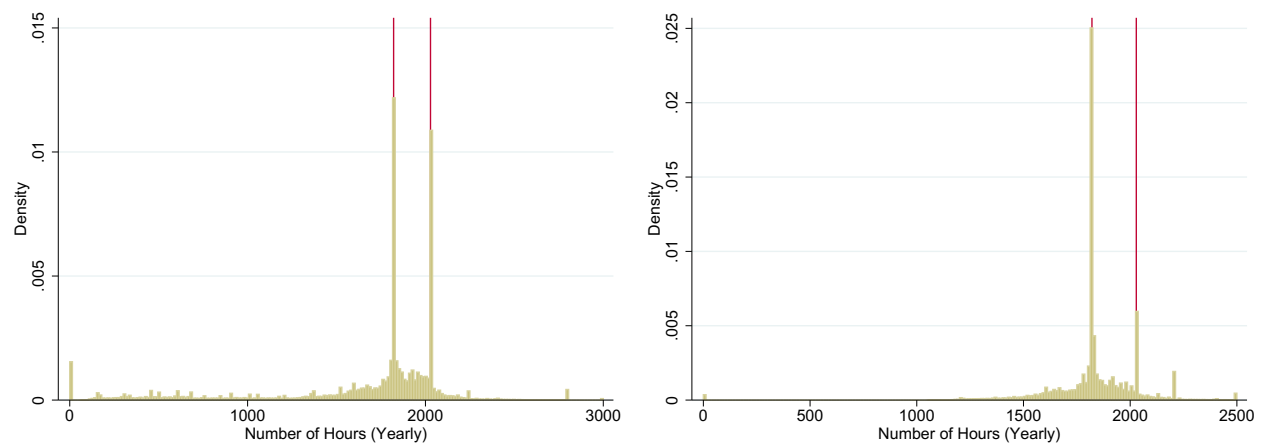
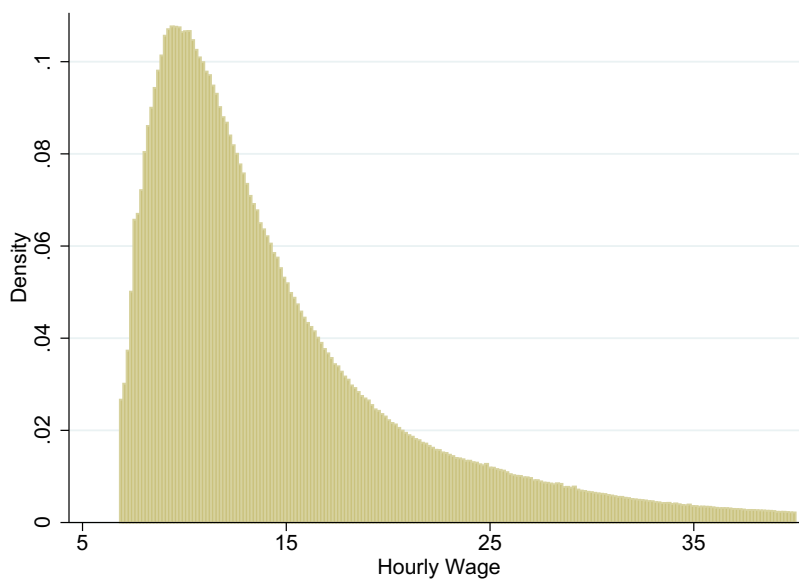
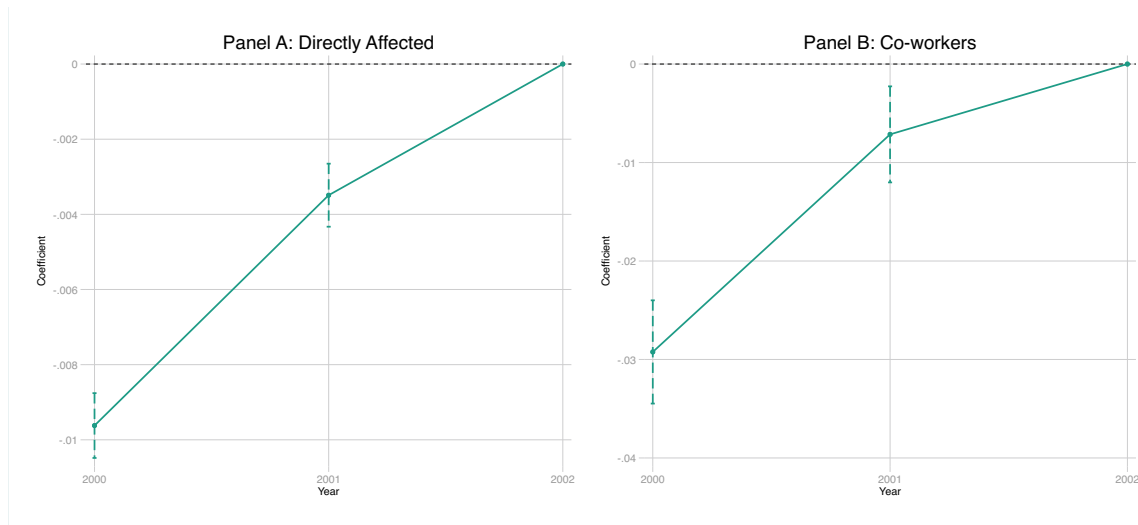


**Figure B.2:** *Gross Wages - Distribution*



*Notes:* the Figure plots the distribution of hourly wages in 2002 for full-time workers. The distribution is trimmed at the SMIC (*i.e.* 6.83).

**Figure B.3: Gross Wages - Pre-reform Trends**



*Notes:* the Figure plots the estimated pre-trends from a regression with specification as in (2.11), where the treatment variable is the change in labor costs as defined in (2.9). The sample includes a balanced panel of firms that were always active in the period 2000 to 2008 and that adopted a 39-hours regime in 2002.

## Appendix C

# Appendix to Chapter 3

### C.1 Derivation of the marginal willingness to pay

The effect of a policy on the indirect utility of incumbent employees is

$$\begin{aligned} \frac{\frac{dV_{ij}^e(\theta)}{d\theta}}{\lambda_{ij}} &= \sum_s \frac{dV_{ij}^e(\theta)}{dw_{ijs}} \frac{dw_{ijs}}{d\theta} + \sum_s \frac{dV_{ij}^e(\theta)}{d\bar{w}_{ijs}} \frac{d\bar{w}_{ijs}}{d\theta} + \sum_s \frac{dV_{ij}^e(\theta)}{d\rho_{is}} \frac{d\rho_{is}}{d\theta} + \sum_s \frac{dV_{ij}^e(\theta)}{d\bar{\rho}_{is}} \frac{d\bar{\rho}_{is}}{d\theta} \\ &\quad + \sum_s \frac{dV_{ij}^e(\theta)}{d\bar{l}_{ijs}^p} \frac{d\bar{l}_{ijs}^p}{d\theta} + \frac{dV_{ij}(\theta)}{dT_i} \frac{dT_i}{d\theta} \end{aligned} \quad (C.1)$$

Using the envelope theorem

$$\frac{dV_{ij}^e(\theta)}{dw_{ij}} = \lambda_{ij} (1 - \rho_i) l_{ij}^w$$

$$\frac{dV_{ij}^e(\theta)}{d\bar{w}_{ij}} = \lambda_{ij} (1 - \rho_i) \bar{l}_{ij}^w$$

$$\frac{dV_{ij}(\theta)}{d\rho_{is}} = \lambda_{ij} l_{ijs}^p$$

$$\frac{dV_{ij}(\theta)}{d\bar{\rho}_{is}} = \lambda_{ij} \bar{l}_{ijs}^p$$

$$\frac{dV_{ij}(\theta)}{d\bar{l}_{ijs}^p} = \frac{\partial u_i}{\partial \bar{l}_{ijs}^p} + \lambda_{ij}\bar{\rho}_{ijs}$$

$$\frac{dV_{ij}(\theta)}{dT_i} = \lambda_{ij}$$

By envelope theorem, behavioral responses do not have any first-order utility effect. The only behavioral response that affects the marginal willingness to pay is  $\frac{d\bar{l}_{ijs}^p}{d\theta}$ , which changes exogenously for incumbent workers. Substituting the terms derived above into (C.1)

$$\begin{aligned} \frac{dV_{ij}^e(\theta)}{\lambda_{ij}} &= \sum_s (1 - \rho_{is}) l_{ijs} \frac{dw_{ijs}}{d\theta} + \sum_s (1 - \bar{\rho}_{is}) \bar{l}_{ijs} \frac{d\bar{w}_{ijs}}{d\theta} - \sum_s w_{ijs} l_{ijs} \frac{d\rho_{is}}{d\theta} - \sum_s \bar{w}_{ijs} \bar{l}_{ijs} \frac{d\bar{\rho}_{is}}{d\theta} \\ &\quad + \sum_s \left[ \frac{\frac{\partial u_i}{\partial \bar{l}_{ijs}^p}}{\lambda_{ij}} - \bar{\rho}_{is} \bar{w}_{ijs} \right] \frac{d\bar{l}_{ijs}^p}{d\theta} + \frac{dT_i}{d\theta} \\ &= \frac{dt_{ij}^e}{d\theta} + \sum_s l_{ijs} \frac{dw_{ijs}}{d\theta} + \sum_s \bar{l}_{ijs} \frac{d\bar{w}_{ijs}}{d\theta} + \sum_s \rho_{is} w_{ijs} \frac{dl_{ijs}^e}{d\theta} + \sum_s \frac{\frac{\partial u_i}{\partial \bar{l}_{ijs}^e}}{\lambda_{ij}} \frac{d\bar{l}_{ijs}^e}{d\theta} \end{aligned}$$

The same procedure applied to unemployed individuals delivers the following formula

$$\frac{dV_i^u(\theta)}{\lambda_i^u} = \frac{dt_i^u}{d\theta} + \sum_s l_{is}^u \frac{dw_{is}^u}{d\theta} + \sum_s \bar{l}_{is}^u \frac{d\bar{w}_{is}^u}{d\theta} + \sum_s \rho_{is} w_{is}^u \frac{dl_{is}^u}{d\theta} + \sum_s \frac{\frac{\partial u_i}{\partial \bar{l}_{is}^u}}{\lambda_i^u} \frac{d\bar{l}_{is}^u}{d\theta}$$

## C.2 Supplementary Tables

**Table C.1:** Payroll tax subsidy (Saez et al., 2019) - Calibrations

| Quantity                                                                                         | Calibration                      | Source                                 |
|--------------------------------------------------------------------------------------------------|----------------------------------|----------------------------------------|
| <b>Firm-level Calibration</b>                                                                    |                                  |                                        |
| $\frac{d\tau_y^w}{d\theta}$                                                                      | 0.1693                           | Page 6                                 |
| $\frac{1}{\bar{n}^e} \frac{d\bar{n}_i^e}{d\theta}$                                               | 0.046                            | Table 4                                |
| $\frac{1}{\bar{w}_y} \frac{d\bar{w}_y}{d\theta}, \frac{1}{\bar{w}_o} \frac{d\bar{w}_o}{d\theta}$ | 0.0033                           | Table 4                                |
| $\frac{1}{\bar{\pi}} \frac{d\bar{\pi}}{d\theta}$                                                 | 0.081                            | Table 4                                |
| $\tau_y^w, \tau_o^w$                                                                             | 0.3242                           | Page 6                                 |
| $\tau^\pi$                                                                                       | 0.263                            |                                        |
| $\bar{n}^e$                                                                                      | 9.46                             | Table 3                                |
| $\bar{w}$                                                                                        | 35,230                           | Table 3                                |
| $\bar{\pi}$                                                                                      | 68,730                           | Table 3                                |
| $\Delta\bar{w}$                                                                                  | 0.3 $\bar{w}$                    | Couch and Placzek (2010)               |
| $\frac{\bar{n}_y^e \bar{w}_y}{\sum_{i=y,o} \bar{n}_i^e \bar{w}_i}$                               | 0.125                            | (Figure 5, mean share in middle group) |
| $w_y^{net}$                                                                                      | $\bar{w} \bar{n}^e (1 - \tau^w)$ |                                        |
| <b>Individual-level Calibration</b>                                                              |                                  |                                        |
| $\frac{dw_y^{net}}{d\theta}$                                                                     | -1.551 (monthly)                 | Table 1                                |
| $\frac{dw_y^{gross}}{d\theta}$                                                                   | -209.154 (monthly)               | Table 1                                |
| $\frac{1}{f_y} \frac{df_y}{d\theta}$                                                             | -0.012                           | Table 2                                |
| $\frac{1}{h_y} \frac{dh_y}{d\theta}$                                                             | 0.01                             | Table 2                                |
| $\Delta\bar{w}$                                                                                  | 0.3 $\bar{w}$                    | Couch and Placzek (2010)               |

**Table C.2:** *Raise in Full Retirement Age (Bovini and Paradisi, 2019) - Calibrations*

| Quantity                                     | Calibration      | Source     |
|----------------------------------------------|------------------|------------|
| $\overline{l_c^w \frac{dw_c}{d\theta}}$      | -16,930          | Figure 6   |
| $\frac{dt_o}{d\theta}, \frac{dt_c}{d\theta}$ |                  | Table A12  |
| $p_i^{full}$                                 | 16279.58         | Appendix G |
| $p_i^{early}$                                | $0.7 p_i^{full}$ | Appendix G |
| $\bar{n}$                                    | 25.85            | Table A4   |
| $\frac{\bar{n}_o}{n}$                        | 0.12             | Table A3   |
| $\tau^w$                                     | 0.2              |            |
| $\Delta \bar{c}_c^h, \Delta \bar{c}_o^h$     | -6,664.427       | Appendix G |
| $\Delta \bar{c}_c^h, \Delta \bar{c}_o^h$     | -6,314.86        | Figure 6   |

**Table C.3:** *Decrease in top income tax rate (Labanca and Pozzoli, 2018) - Calibrations*

| Quantity                                                                                                       | Calibration   | Source                                                    |
|----------------------------------------------------------------------------------------------------------------|---------------|-----------------------------------------------------------|
| $\frac{d\Delta \log l_t}{d\Delta \log(1-\tau_t)}$                                                              | -0.097        | Table 5 (Column 5)                                        |
| $\frac{d\Delta \log \bar{l}_t}{d\Delta \log(1-\tau_t)}$                                                        | -0.017        | Table 5 (Column 4)                                        |
| $\frac{d\Delta \log \bar{l}_l}{d\Delta \log \bar{l}_t}, \frac{d\Delta \log \bar{l}_l}{d\Delta \log \bar{l}_t}$ | 0.878         | Table 7 (Column 3)                                        |
| $\frac{d\Delta \log \bar{l}_t}{d\Delta \log(1-\tau_t)}$                                                        | -0.432        | Table D.34 (Column 3)                                     |
| $w_t$                                                                                                          | 183.65        | Table 1                                                   |
| $w_l$ (yearly)                                                                                                 | 200,000       | Not provided, set arbitrarily below $\underline{w}^{top}$ |
| $\bar{n}_j$                                                                                                    | 43.37         | Table 1                                                   |
| $\frac{\bar{n}_t}{n_j}$                                                                                        | 0.54          | p. 26                                                     |
| $\frac{\bar{n}_l}{n_j}$                                                                                        | 0.34          | p. 26                                                     |
| $l_t, l_l, \bar{l}_t, \bar{l}_l$                                                                               | 1896.19+27.62 | Table 1                                                   |
| $\tau_t$                                                                                                       | 0.6228        | Appendix B.5                                              |
| $\tau_l$                                                                                                       | 0.3954        | Appendix B.5                                              |
| $\underline{w}^{top}$                                                                                          | 279,800       | Appendix B.5 (lower bound top bracket, yearly)            |

**Table C.4:** *Change in Corporate Tax Rate (Fuest et al., 2018) - Calibrations*

| Quantity                             | Calibration | Source                                     |
|--------------------------------------|-------------|--------------------------------------------|
| $\frac{d \log w}{d \log \tau^\pi}$   | -0.39       | Table 1 (Column 1)                         |
| $\frac{d \log \pi}{d \log \tau^\pi}$ | -0.4        | Figure 4, Panel A                          |
| $\bar{n}$                            | 52          | Table C4 (ZEW Discussion Paper No. 16-003) |
| $\bar{\pi}$                          | 129,606     | Table C4 (ZEW Discussion Paper No. 16-003) |
| $\bar{w}$                            | 2717        | Table C5                                   |
| $\tau^w$                             | 0.4         |                                            |
| $\tau^\pi$                           | 0.1865      | Table C5                                   |