LABOR SUBSTITUTABILITY AND THE IMPACT OF RAISING THE RETIREMENT AGE

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ABSTRACT. Substitutability among different types of workers may affect the impact of public policies by creating labor demand spillovers within firms. Using administrative data, we study the substitutability between age cohorts in the context of a public pension reform that increased the full retirement age in Italy in 2012. We find that older workers delaying retirement and younger co-workers are substitutes. Labor demand spillovers cause almost the entire short-run fiscal cost of 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.

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1. Introduction

Substitutability among workers within the firm is crucial for the evaluation 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 often "hidden" effects may have welfare implications that are hard to detect when looking at targeted individuals. Moreover, they may lead to a redistribution of the costs and benefits of a policy within the firm. 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 retirement.[1] 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).[2] 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 how labor substitutability within the firm affects 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.[3] We

[1] 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 (Congressional Budget Office, 2016). Most European countries have implemented similar measures since 2000 (Carone et al., 2016).

[2] 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, Emmerson, and Tellow (2016), Manoli and Weber (2016), Seibold (2017), Lalive, Magesan, and Staubli (2017).

[3] 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.

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 to various Social Security programs.

In the first part of the paper, we ask 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 take-up of Social Security programs. We conclude by discussing the implications of labor substitutability for the short-run impact on government finances of this particular policy and, more generally, of policies that lower the incentive of older employees to leave the firm. To this end, we incorporate the demand-driven behavioral responses into the estimation of the fiscal externality of the reform (the loss on the revenues that the government hopes to raise), improving upon the existing literature that focuses only on the behavioral responses of older workers.

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 workforces 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 idyosinctraic 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-tun effect of raising the full retirement age on public finances.

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 by more than one year 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. We also show that in the short-run firms mostly respond to the shift in the retirement date of the workers on the cusp of retirement. The effect of a change in the residual working life of younger employees is close to zero.

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 young workers' earnings loss depends on within firm dynamics. The 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 full account of the effect of the reform requires to also take into considerations the consequences on younger co-workers.

To this end, we evaluate the implications of the observed patterns of labor substitutability for the incidence of the policy by estimating the fiscal cost of the public pension reform in the short-run. Previous literature has focused on the behavioral responses of older workers only (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 to assess its cost. The fiscal externality measures the share of mechanical savings on pension outlays that the government loses because of behavioral responses of workers and firms. According to our estimates, savings on pension outlays 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 revenues generated by the policy are lost in the short-run. The cost is entirely explained by spillovers on the co-workers. If we ignored these spillovers we would estimate a close to zero fiscal externality, indicating that no savings on pension payments would be lost. We therefore conclude that labor substitutability is pivotal to assess the consequences of this reform and potentially of 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 (2016) and Jaravel, Petkova, and Bell (2017) exploit sudden workers' deaths.[4]

[4] Other recent examples are Nguyen and Nielsen (2010), Bennedsen, Perez-Gonzales, and Wolfenzon (2010) and Adam (2015).

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, Marque, and Wasmer, 2008 and Pissarides, 2000). We also add evidence on changes in internal labor market dynamics (Baker, Gibbs, and Holmstrom, 1994).[5]

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.[6] 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.[7] 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 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, Mohen (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, Novo, and Portugal (2009) study a Portuguese pension reform, while Boeri, Garibaldi, and Moen (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

[5]Gibbons and M. Waldman (1999), Lazear and Oyer (2013), and M. Waldman (2013) survey the theoretical literature on internal labor markets.

[6] Some examples are works on the spillovers of changes in the disability insurance (Autor and Duggan (2003); Karlstrom, Palme, and Svensson (2008); Borghans, Gielen, and Luttmer (2010); Staubli (2011)) or unemployment insurance (Lammers, Bloemen, and Hochguertel (2013)). Recent works along these lines are Inderbitzin, Staubli, and Zweimüller (2016) and Kline and Walters (2016). A similar work on early retirement provisions and the spillovers on other government programs is Vestad (2013).

[7]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.

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.[8] 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.[9]

The remainder of the paper proceeds as follows: Section 2 illustrates the institutional setting; Section 3 describes the data; Section 4 outlines the identification strategy; Section 5 shows that older workers delay retirement in response to the policy; Section 6 discusses the main findings on firm's labor demand adjustments; Section 7 presents a battery of robustness checks; Section 8 documents the effect of the reform on co-workers' earnings; Section 9 builds a model to estimate the revenues effects of the reform; Section 10 concludes.

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 B.

2.1. **The Italian Pension System.**

As for many OECD countries, including the US, the main pillar of the Italian pension system is a compulsory pay-as-you-go plan.[10] A combination of defined-benefits (DB) and notional defined-contributions (NDC) methods determines pension benefits. The private-sector Social Security tax rate is 33 percent. Around one-third is paid by the employee and two-thirds 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. 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. It allows to claim benefits before meeting the old-age or seniority pension requirements. Similarly to early retirement in the US, before 2012 *opzione donna* allowed to claim benefits about 4 years

[8] See Coile and Gruber (2007) for a review.

[9] 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.

[10] 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).

before the statutory age.[11] Retiring early comes at the cost of receiving sizably lower pension benefits. The average cut is estimated to be roughly 35 percent of full benefits (INPS, 2016). 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.

2.2. **Statutory and Actual Retirement Age.** The relationship between the statutory retirement age and retirement choices determines the transitional effects of pension reforms. Indeed, it regulates the extent to which workers delay retirement and firms see 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 retirees retire at the full-retirement date in 2012 (Figure A3). This trend is common to other countries. In the US 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 United States 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.[12] A large and similar response emerges from our data: a one year shift in the retirement date translates in almost 7 extra months of work (see Section 4.4).

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 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 age and contribution requirements to claim old-age and seniority pensions, reducing the number of new retirees and increasing the average age at retirement.[13] The new rules applied to all workers who did not accrue the right to claim either pension by the end of 2011. Few categories of workers - listed in Appendix C - maintained the right to retire under pre-reform rules. For all other workers, Table 1 compares pre and post-reform rules over the period 2012-2015, showing age and contribution requirements for the old-age (Panel A) and seniority (Panel B) options.[14]

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 was 60 for

[11] Early retirement using *opzione donna* was possible in 2011 upon turning 57 years old, conditionally on having 35 years of contributions.
[12] Mastrobuoni (2009) lists social custom or liquidity constraints as important factors for retirement choices. Previous studies documented that also health (e.g., Dwyer and Mitchell, 1999; McGarry, 2004) and job characteristics (e.g. Hurd and McGarry, 1993) are important determinants of the retirement decision.
[13] Figures A1 and A2 plot average age and retirement volumes by gender and retirement option.
[14] We report the few exceptions to these rules in Appendix C and additional details in Table A1.

women and 65 for men in 2011.[15] 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 A1).

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.[16] 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).[17] 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.

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 A4 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.[18] 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

[15]Absent the reform, it would have risen to reach 61 years and 10 months for women and 65 years and 7 months for men.

[16]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 witht he contribution requirement unchanged.

[17]The remaining 20 percent were unemployed or out of the labor force when they retired

[18]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).

under "quota 96" experience the largest change in the retirement age.[19] Milder changes affect male workers who were under 60 years old with 38 or more years of contributions.[20]

3. Data

We leverage high-quality and restricted-access administrative data available at the Italian Social Security Institute (INPS). In particular, we use contribution histories for 6 million workers for the first time. As we explain later, this is crucial information to conduct our analysis.

Matched employer-employee records: matched employer-employee records are 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.[21] We use monthly data for the period 2009-2015.[22] 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 with a monthly frequency: 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 (blu-collar, white-collar or manager); monthly wage; number of days worked. 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 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.

Workers' Contribution Histories: Previously unexploited contribution histories are available for all employees who worked in small-medium sized firms around the reform (*i.e.* between 2009 and 2015).[23] We observe contribution spells within any given year for more than 6 millions of workers. The recorded information includes: 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

[19] After the reform, they must claim an old-age pension or an "anticipated" pension. Both options imply a significant shift in the retirement age.

[20] 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".

[21] 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.

[22] 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.

[23] 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 the project.

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.

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 installment was collected and the amount disbursed.

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 the demand for younger cohorts. On the other hand, we want to evaluate the implications of substitutability for the short-run 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 in this variable likely reflects different labor demand shocks or differences in other unobservables. 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. We then 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 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 4.2). This variable changes across firms due to idiosyncratic differences in the distribution of gender, age and years of contribution among *potential retirees*. We exploit such an identifying variation within a difference-in-differences estimation (sub-section 4.3).

4.1. Individual shift in the full retirement date.

To compute 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 E. For every type θ we compute the reform-induced change in years until full retirement, excluding routes to early retirement. We compute the predicted retirement dates according to old and new rules. We denote the difference with δ_θ:

$$\delta_\theta = \text{Years until retirement}_\theta^{\text{new}} - \text{Years until retirement}_\theta^{\text{old}} \tag{4.1}$$

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

i) workers accrue full contributions on their accounts (*i.e.* 52 weeks per year) until retirement

ii) the predicted retirement date is the earliest date at which the worker can collect the first pension installment by exploiting either the old-age or the seniority option to retirement

Assumption (i) requires that individuals work year-round and full-time in the post-reform period.[24] 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 pathways to claim full benefits. We compute the predicted retirement date associated to every available option and we select the earliest one.[25] As discussed earlier, an extensive literature has documented that retirement behavior displays bunching at the acquisition of full pension rights. Figure A3 shows consistent evidence for Italy. Under our assumption, the chosen pathway to retirement may change because of the reform. For some types θ the earliest exit date was associated to the old-age pension under pre-reform rules, whereas it switches to the seniority pension under post-reform rules, or viceversa.

For workers who retire before 2017 we compare actual and predicted retirement dates. Figure A5 shows a "forecast quality" assessment (Panel A). The majority of the differences (69 percent) lies within a 1-year window, indicating that our measure is quite accurate in predicting the actual retirement date. It also provides supportive evidence to assumption (ii). A very 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 claim the early retirement option (*opzione donna*) after the reform causing a larger difference between actual and predicted retirement dates for female workers (Figure A5, Panel B). Second, some workers maintain the right to retire under pre-reform rules (see Appendix C).

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 away from retirement has minor effects on short-run firm responses. The three-years 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.[26] Figure 1 Panel A plots the distribution of the change in years until

[24] Alternatively, we require that non-work periods are covered by *figurative* contributions (see Appendix E)

[25] The reform abolished the "waiting window", a rule whereby the first pension installment could be collected only 12 months after becoming eligible for full retirement. Most workers were postponing retirement until the date when benefits would be eventually paid in. We take into account the existence of the "waiting window" when computing the predicted retirement date under pre-reform rules.

[26] Workers who would have retired in 2012 under old rules face no change in the retirement date. Thus, we mainly exploit the variation in retirement dates coming from workers who would have retired in 2013 and 2014.

retirement for *potential retirees*. It displays a sizable variability, with mean 1.36 and standard deviation 1.4.

Every *potential retiree* of type θ experiences the same shift δ_θ of the expected retirement date. To construct the firm-level change in the full retirement date, we build a shift-share shock. We weight the δ_θs by the share of every θ in the workforce of *potential retirees* employed in the firm. We have

$$T_i = \sum_{\theta \in \text{Potential retirees}} \pi_{\theta,i}\, \delta_\theta \tag{4.2}$$

$\pi_{\theta,i}$ is the share of workers θ in the workforce of *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. The instrument T_i captures the idiosyncratic firm-level shift in the retirement date of *potential retirees*. It has a straightforward interpretation as the change in the policy parameter shifted by the reform. The distribution of T_i for firms that employ at least one *potential retiree* displays significant variability. The mean is 1.37 and standard deviation is 1.33 (Figure 1 Panel B). By construction, T_i converges to the average shift in the population the larger is the firm. This is because larger firms employ more *potential retirees* and more likely reflect the distribution of age, gender and contributions in the universe of these workers.[27] This is one of the reasons why we focus on small and medium firms, where the variability of our treatment - although still declines in firm size - is greater and our instrument more effective.

4.3. **Empirical Specification and identifying assumptions.**

4.3.1. *Empirical specification.* To study workers substitutability, 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

$$Y_{it} = \lambda_i + \gamma_t + \sum_{k=2009}^{2015} \beta_k^R I(k = t) \times R_i + \varepsilon_{i,t} \tag{4.3}$$

i indexes the firm and t indexes the year. Y_{it} is the outcome of interest. λ_i is a firm fixed-effect that captures time-invariant heterogeneity across firms, including differences in average outcomes across treatment levels. Standard errors are clustered at the firm level to address the potential concern of serial correlation across periods (Bertrand, Duflo, and Mullainathan, 2004).[28] The coefficients of interest are $\{\beta_k^R\}_{k=2009}^{k=2015}$ and show how the treatment affects firms in year k relative to the reform year. R_i measures the number of retained *potential retirees* in firm i. We call a *potential retiree* retained if she retires one or more years after her pre-reform full

[27] For the same reason, our instrument is uniformly distributed across local labor markets.

[28] Results are virtually unchanged when we cluster at the province × two-digit sector level.

retirement date. The number of workers retained in firm i is

$$R_i = \sum_{j:j\in\text{Potential Retirees}_i} I(\tilde{\delta}_j \geqslant 1) \tag{4.4}$$

where $\tilde{\delta}_j$ is the observed change in the retirement date of individual j, *i.e.* the difference between the observed and the pre-reform expected retirement date. R_i is strongly correlated with the size and the age structure of the firm.[29] Hence, it may capture the effect of differences along these dimensions rather than the impact of the reform. We 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 per effect of the reform.

We then study the effect of raising the full retirement date on public finances, estimating

$$Y_{it} = \lambda_i + \gamma_t + \sum_{k=2009}^{2015} \beta_k^T I(k=t) \times T_i + \varepsilon_{i,t} \tag{4.5}$$

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 employees.

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

$$Y_{it} = \alpha + \lambda_i + \sum_{k=2009}^{2015} \beta_k \gamma_k + \beta^T Post_t \times T_i + \varepsilon_{i,t} \tag{4.6}$$

We interact the treatment with the dummy $Post_t$ that equals 1 in years 2012-2015. β^T captures the treatment effect on the difference in the outcomes between pre- and post-reform periods.

4.3.2. *Identification assumptions.* We leverage variation in the characteristics of the workforce of *potential retirees* for identification. The extent to which firms are affected by the reform depends on variation in the shares of types θ among their *potential retirees* (equation 4.2). Identification requires that $\pi_{\theta,i}$s do not correlate with firm's unobservable time-varying characteristics (Goldsmith-Pinkham, Sorkin, and Swift, 2017 and Borusyak, Hull, and Jaravel, 2018). In other words, the firm-specific characteristics of the workforce of *potential retirees* must not correlate with 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 avoid treatment variation depending explicitly on the broad firm's demographics. We only exploit idiosyncratic differences in gender, age and years of contributions (*i.e.* in types θ) in the sub-sample of *potential retirees*. Evidence that the composition of the workforce of *potential retirees* relates to trends in labor demand would provide a sign that identification is failing. For instance, more affected firms could have older *potential retirees* because they hired less (or fired more) young workers in the pre-period. Pre-trends as captured by the coefficients $\{\beta_k^T\}_{k=2009}^{k=2011}$ provide suggestive evidence of the exogeneity

[29] Firms with larger shares of employees retained have different pre-reform labor demand paths (Figure A6).

of our treatment. 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.

4.3.3. *Placebo tests.* We assess whether our instrument predicts labor demand trends by running a series of placebo tests on the pre-reform period (2009-2011). We artificially assign the date in which the reform becomes effective to 2010 or 2011, rather than to 2012. We then estimate specification (4.5) on the pre-reform period only. We test the effect of the placebo instrument on layoffs and new-hires, which are the main firm-level outcomes we study in Section 6. Table A2 shows that the instrument has virtually zero effect, indicating that there are no differential trends in labor demand for more and less treated firms. Hence, we exclude that the composition of the workforce of *potential retirees* predicts hiring and firing decisions in the years preceding the reform. We nonetheless show pre-trends in all the outcomes we analyze to provide evidence that they are balanced before the policy is implemented.

4.3.4. *Balancing tests.* We run a balancing test whereby we regress a rich set of firm's baseline characteristics on the instrument (Table A3). The correlation between the T_i and baseline firm characteristics is very weak, although precisely estimated. This holds true for the gender composition and the workforce age structure. As the shift in the full retirement age increases by 1σ (1.33 years), the share of male workers decreases by only 0.008 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 0.008, -0.008 and 0 against averages of 0.123, 0.58 and 0.297. T_i has also a close to zero relation with the share of *potential retirees*. Importantly, results do not change when controlling for these covariates (Section 7).

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 baseline quarter (q1-2009) 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.[30] These restrictions leave us with a panel of 61,434 firms.

Table A4 compares the characteristics of firms in our main sample to other 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. As expected, they employ a older, more paid, more experienced and tenured workforce. *Potential retirees* are older, more experienced and more tenured than other full-time employees in firms belonging to the master sample (Table A5). They have higher gross

[30]Firms typically have one social-security code per establishment.

daily wages, and are more likely to have a permanent contract. We compare *potential retirees* to older employees not on the cusp of retirement, but similar along many dimensions (Table A6).[31] Employees approaching the end of their working lives reduce their effort (Dostie, 2011; Borsch-Supan and Weiss, 2016 and Avolio, D. A. Waldman, and McDaniel, 1990). Before the reform *potential retirees* are 5 percent more likely to be absent from work because of sickness and 1 percent more likely to be absent due to work-related injuries or sick leave.

5. Older workers delay retirement after the reform

The reform may affect firm's labor demand if it translates into longer working lives for older workers. We investigate the response of retirement choices to the full retirement age by estimating an individual-level version of (4.5) on the sample of *potential retirees*. We use the worker-level shift in the full retirement date as treatment (*i.e.* δ_θ in equation 4.1). We augment the specification including the interaction of sex and age fixed-effects with time dummies. 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 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 2). The difference between the two lines reflects early retirement responses to the shift in the retirement date.

To better quantify the response of observed retirement to the policy, we regress the difference between the observed retirement date and the pre-reform full retirement date on δ_θ. 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 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 US that would predict a 6 months increase.

6. Labor demand responses to the reform

In this Section we document how firm's labor demand 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 (4.3) on the sample described in sub-section 4.4. For each outcome of interest, we plot the coefficients $\{\beta_k^R\}_{k=2009}^{2015}$, along with 95 percent confidence intervals.[32] 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 3.

[31] 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.

[32] The coefficients $\{\beta_k^R\}_{k=2009}^{2015}$ are also reported in Tables A7 and A8.

According to labor demand theory, a drop in demand for younger employees caused by the retention of older workers can be reconciled with complementarity between cohorts only in case of an increase in younger workers' wages. As we document below, the labor demand of firms that are more affected by the reform drops. A large wage increase is inconsistent with the evidence in Section 8 that shows a decline in the earnings of younger cohorts. We conclude that younger cohorts are substitutes for older workers retiring. Our results document a larger response of layoffs for incumbent older workers (over 55) relative to younger employees, indicating that the former cohort is the closest substitute to the workers retiring. To grasp the intuition, in Appendix A 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.

6.1. **Layoffs.** Firms increase layoffs of permanent employees in response to increased retention of *potential retirees* (Figure 3, Panel A).[33] No significant difference in the layoffs of more and less treated firms is present before the reform. The difference emerges in its aftermath. When a *potential retiree* is retained, the number of fired workers rises over the period 2012-2014 up to 0.26 in 2015. It 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 *potential retirees* under pre-reform rules would have been working at the firm in 2012 where we detect a small effect. At the same time, when the number of retained workers rises in 2013 and 2014 we observe an increase in layoffs. As some *potential retirees* retire in 2015, the increase in the number of workers fired slows down. Table 3 summarizes the results reporting the estimated coefficient from the specification in (4.6). Firms fire 0.17 more workers per year 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 age cohorts, Panel B of Figure 3 breaks down the effect by workers age. We define as "young" under 35 years old workers; "middle-aged" the workers aged between 35 and 55; and "older" over 55 years old workers. Layoffs increase across all age groups and are not concentrated on *potential retirees* only. They increase to a greater extent among middle-aged and older employees, who bear a larger cost in the short-term. As expected, the strongest reaction to the shock is concentrated on older workers. In 2015, 0.093 extra older workers are fired relative to 0.044 young workers, despite the average share of young workers in the firm is twice as large as the share of older employees (29.7 percent against 12.3 percent). Figure 3 Panel C shows the estimated effects when we divide the number of layoffs in every age cohort by the respective number of incumbent employees. The coefficients 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

[33]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.

substitutability with *potential retirees*. Figure 3 Panel D compares the effect of the reform on fired older employees when excluding *potential retirees* against the response of the total number of older workers. When excluding *potential retirees* the coefficients halve, indicating 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.[34] We find that firms trade them off in a similar fashion, suggesting a strong substitutability. Layoffs also increase more in number for middle-aged relative to young workers. In 2015, the estimated effect is 0.12 for middle-aged and 0.044 for young workers. Since the share of middle-aged workers is almost the double of the share of young employees, the responses of the layoffs probabilities in these two cohorts are identical (Figure 3, Panel C). Hence, by only looking at layoffs we do not find strong evidence that middle-aged employees are closer substitute to older workers relative to young employees.

6.2. **New Hires.** Firms more affected by the reform change their hiring schedule, without differential pre-trends (Figure 4, Panel A). A retained worker causes an average drop in hiring of up to 0.49 units per year (Table 3). Against an average of 4.79 new hires per year, it amounts to a 10.2 percent reduction. Hiring recovers starting from 2015, when the coefficient is close to zero. The u-shaped pattern of coefficients indicates 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 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 over 55 employees. Table 3 summarizes the results showing that new hires of young and middle-aged decline by 0.20 and 0.15 units per year respectively, accounting for most of the observed drop in hiring. Relative to the pre-reform averages, the drop is equal to 8.4 percent for young workers and 7.3 percent for middle-aged workers. New hires drop more for fixed-term contracts (Figure 4, Panel C). Since firms typically hire junior workers under temporary contracts, the observed heterogeneity is consistent with the drop being concentrated among young and middle-aged workers. 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. Hence, when *potential retirees* retire firms start hiring more permanent workers to replace them.[35]

[34]The cost of layoffs is a function of the fired worker's wage.

[35]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 as *potential retirees* start to retire.

6.3. **Which *potential retirees* matter more?** All workers experience an increase in years left to pension eligibility, except those who qualify for retirement by the end of 2011.[36] 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. We include in (4.3) two treatment variables. The first is T_i, computed on the sample of *potential retirees*. The second is the average change in the full retirement date for workers expected to retire in 2015 or 2016. Only the first treatment has significant effects on layoffs and new hires (Figure A7). Table A9 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 retirement date of *potential retirees* is sufficient to explain most of the adjustments in labor demand in the short-run. The change in the retirement date of employees between 4 to 5 years from retirement when the reform becomes effective has very small effects in the first four years after the reform. Unfortunately, we cannot investigate the response in the following years because we have no access to the data after 2015. For this reason, we cannot exclude that firms react more strongly to the longer-term shock in the following years when the workers between 4 to 5 years from retirement were expected to leave the firm.

6.4. **Response heterogeneity by occupation and turnover.**

6.4.1. *Within and across occupations:* We further explore how the shock 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 (4.3) at the firm-unit level. Our specification includes a treatment for the *potential retirees* of the unit and one measuring the shock to other units within the same firm. Figure A8 plots the results of this exercise. The within-unit treatment generates a larger effect on layoffs and new hires. Retaining an older worker for longer than expected impacts the number of layoffs and new hires in the same occupation. There is limited spillover across units in the same firm. Jäger (2016) finds that workers substitutability is larger within occupations. We only have a broad classification of the qualification, so we cannot run a similar analysis. However, our evidence is consistent with higher substitutability between workers who perform similar tasks.

6.4.2. *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, a non-renewed 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.[37] We estimate a triple difference specification using the two groups

[36] See Section 2.3.

[37] Results are identical when we use tertiles or quartiles of the distribution.

of firms. Most of the effect on layoffs is explained by high-turnover firms (Figure A9). 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.

7. Discussion and Sensitivity checks

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 A3 has shown that the relation between the instrument and firm characteristics is weak. We further address this concern by estimating an augmented specification that adds to (4.3) the interaction between a vector of covariates and time dummies. Figure A10 shows that results are robust to the inclusion of a rich set of controls. First, we include dummies for the quintiles of the share of female employees. New retirement rules affect women to a greater extent than men. Gender controls reduce the concern that non-parallel labor demand trends across firms with different gender compositions confound our estimates. Second, we add dummies for quintiles of firm size, firm age, the share of young (< 35), middle-aged ($35 - 55$) and older (> 55) workers, and average firm wage. 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 to the pre-reform period to prove that labor demand trends were similar up to four years before the reform in the new balanced panel. Finally, we check that results are robust to the inclusion of the universe of firms. We set $T_i = 0$ and $R_i = 0$ if a firm employs no *potential retirees* and was not included in the master sample. Despite they do not employ any *potential retiree*, these firms do not show differential trends in the pre-reform period. Moreover, the post-reform coefficients are virtually identical to our baseline estimates.

We also test whether results on layoffs could be confounded by complementary policies. A change in the dismissal discipline was implemented in 2013. The new law reduced the cost of layoffs for firms with more than 15 employees.[38] We check whether firms below and above this threshold behave differently. Figure A11 shows that the percentage increase in layoffs is virtually identical for firms below 15 employees and firms between 15 and 30 employees. We conclude that changes in the layoff cost do not seem to have a first order impact on the firing behavior in the post-reform period.

[38]Before 2013, if an employee was dismissed for economic reasons, the employer was mandated to either reinstate the worker or pay 15 months of salary, depending on the employee's choice. Since 2013 the employer is liable for the reparation, but is not mandated to reinstate the worker, reducing the uncertainty on the dismissal cost.

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 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, our analysis focuses on a very short horizon (4 years), while it takes time for these dynamics to realize.[39] In addition, reductions in the number of new hires are significant, but not sizable when compared to cycle fluctuations in hiring.[40] Finally, we showed in Section 6.3 that firms respond to the shock to the restricted set of *potential retirees* only. This reduces the extent to which their responses can affect the labor market general equilibrium in the four 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 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 the equilibrium allocations in local labor markets, but it is not sufficient to draw definitive conclusions about employment dynamics across 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, recent work by Mohen (2019) exploits a slowdown in retirement of older workers to uncover substitutability among age cohorts in US 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.

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 retirement age affect *potential retirees* and their co-workers. Our data crucially tracks the workers across all jobs, allowing to study the consequences of the policy for all incumbent employees a the reform date, including the workers who leave the firm in the post-reform period. We start by discussing the effects of the policy on labor earnings. 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

[39] All the outcomes respond shortly after implementation when general equilibrium is not likely to play a role.

[40] A retained *potential retiree* causes a 0.35 drop in the number of new hires. There are about 98,000 *potential retirees* in our sample. If they were all retained for more than 1 year, total new hires would drop by 34,000 units, 0.6 percent of total new hires in 2011.

retirement rules (e.g. Duggan, Singleton, and Song, 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 short-run cost of the reform.

8.1. **Co-workers Earnings.** The first part of our analysis focuses on co-workers earnings. We define as co-worker a full-time non-*potential retiree* who works in a firm with at least one *potential retiree* in the quarter when the reform is passed. We match every co-worker to the firm where she was incumbent at the time of the reform. We estimate (4.5) using total labor earnings as dependent variable. Labor earnings include income from other private employers, self-employment, and public-sector employment. We use total earnings for the co-workers who share incumbency at the reform date in a given firm.[41] The co-workers who were incumbent in more treated firms experience a decline in earnings after the reform, with the loss growing over time (Figure 5). A 1σ shift in the full retirement age of the firm (1.33 years) causes a drop of 13,379 euros in 2015 equal to 2.2 percent of average firm total labor earnings pre-reform. Middle-aged and older workers emerge as the most affected, confirming their close substitutability with *potential retirees*. 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 (Table A10). Earnings also drop for young workers, with a loss around 1.5 percent. When adding non-work subsidies to labor earnings the decline becomes smaller (1.3 percent), because non-work subsidies are triggered by an increase in the number of fires.

Since the decline in earnings moderates after accounting for non-work subsidies, the increase in layoffs documented in Section 6.1 can partly explain the earnings losses. Within-firm dynamics could also play a role if incumbent co-workers experience wage cuts or a slower earnings growth when *potential retirees* remain at work. To quantify the relative contribution of these two channels to total earnings losses, we perform a decomposition exercise. We combine causal estimates on the number of involuntary separations with estimates of the cost of a job loss.[42] 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. We add to the specification the matching covariates interacted with time fixed-effects.[43] The

[41] We consider employees firms that remain open throughout the pre-reform period. Hence, the number of observations is larger than in the previous analysis. Results do not change if we use the balanced sample of firms that are active between 2009 and 2015. Regressions are weighted based on firm size at the baseline quarter.

[42] We classify as involuntary separations layoffs and non-renewed temporary contracts.

[43] 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, King, and Porro, 2011). We discuss the weighting and further details about the match in Appendix F.

estimated earnings drop is 5,057 euros three years after the separation (Figure A13). It amounts to a 22.7 percent decline, in line with estimates in Couch and Placzek (2010).[44]

We combine our estimates of earnings losses after separation with causal estimates on the number of involuntary separated workers. Figure 6 presents the result of this exercise. The blue-shaded area is the share of the total earnings loss of full-time workers that we can impute to involuntary separations. It covers around 27.17 percent of the total effect of the reform on earnings. Although separations play a relevant role, within-firm dynamics provide a major contribution. We replicate the exercise for young and middle-aged workers (Figure 7). Some important heterogeneity emerges. Separations explain a larger part of the earnings losses for young workers (43.32 percent of the total). In particular, we can impute to separations most of the total drop in their earnings in the first years after the reform. The picture is remarkably different for middle-aged workers. Within-firm dynamics account for most of the drop in their earnings (around 81 percent of the total). The uncovered heterogeneity is consistent with a model of seniority where earnings grow with age within the firm (Bianchi et al., 2018). As a consequence, middle-aged workers are closest substitutes to *potential retirees* and face the largest slowdown in earnings growth.

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. 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 the outcomes of interest across all *potential retirees* incumbent in the same firm. Figure 8 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 A12 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

[44] Couch and Placzek (2010) revisit pioneering work by Jacobsen, LaLonde, and Sullivan (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.

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.

9. Implications of substitutability for the fiscal cost of the reform

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 public finances in the short-run. We focus on the fiscal externality of the reform that we define as the share of mechanically raised public pension savings, which is lost due to behavioral responses. Despite the literature has so far devoted little attention to the behavioral responses of firms and their implications for co-workers, we incorporate the costs caused by labor substitutability into our model. We then discuss how our results relate to welfare and how they can be relevant for other public policies that reduce the incentives of older workers to leave the firm.

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* (pr) 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

$$x_i \leqslant (1 - \tau_i)\, w_i\, l_i^w + T_i$$

x_i is consumption. Labor l_i in a firm is paid a wage w_i and taxed at rate τ_i. The worker receives total transfers 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 T_i in Appendix G.

Using the budget constraint, the fiscal externality of the policy is the share of mechanical revenues that is lost because of the behavioral responses

$$FE = -\frac{\text{Cost of Behavioral Responses}}{\text{Mechanical Public Pension Savings}} \tag{9.1}$$

The formula is derived in Appendix G. 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 workers, 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 represent the resources that the government would save through the policy absent any change in the behavior of workers and firms. In other words, it measures 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 government revenues. If the fiscal externality falls below -1, the government loses

the entire mechanical revenue 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.[45] Finally, our analysis focuses only on small and medium firms as the rest of the paper.

9.2. **Empirical implementation and results.** The fiscal externality is a function of the responses to the policy of labor earnings and Social Security transfers that we derived in Sections 6 and 8. We provide a detailed description of the empirical implementation of (9.1) in Appendix G, with a discussion on the alternative calibrations that we present in our results.

We start by computing the fiscal externality following the standard approach in the literature. The first column of Table 4 shows the estimates of (9.1) on the restricted sample of *potential retirees*. Standard errors to the estimates are bootstrapped via a wild bootstrap procedure with 1000 repetitions.[46] Results show what would happen if we ignored the spillovers on co-workers that are caused by labor substitutability. All estimates are close to zero, indicating that the government raises all the money that is mechanically saved by the policy. Since *potential retirees* work for longer per effect of the reform, they increase labor earnings (Figure 8). Tax revenues from the extra earnings offset the increase in Social Security transfers.

We add the spillovers to 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 *potential retirees*. The reason is twofold. First, large spillovers arise because of labor substitutability, indicating 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 in our horizon of analysis. As Figure 8 suggests,

[45] 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.

[46] 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.

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 benefits of 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 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 first 13-month following the hiring event and we calibrate the number of marginally non-hired workers using estimates from the first part of the analysis.[47] In this conservative scenario, the fiscal externality is larger. 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 cost of the policy, changing our conclusions on the fiscal externality. Hence, the behavior of firms and labor substitutability are pivotal to study the incidence of this reform. Substitutability between age cohorts has potentially important implications for other policies that affect the incentives of older workers in a similar way. For instance, 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 increase 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 costs similar to those documented in our analysis.

9.3. **Substitutability and welfare in the short-run.** We conclude by discussing how firm's behavior and labor substitutability affect the welfare effects of the policy. The welfare change 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.[48]

We cannot estimate the long-run welfare effects of the policy because we lack data on a long time horizon. Even if the data was available, our conclusions would strongly rely 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 firm's 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

[47]We include all lengths of contract spells for new hires including temporary and permanent contract workers. The median value of the earnings loss is 5,506 euros. We also set τ at 25 percent in the calibration.

[48]The willingness to pay can be high since the policy improves the sustainability of the Social Security system.

response to the policy. Hence, 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 younger cohorts. We quantify the other welfare component, measuring the short-run cost of the reform. The fiscal externality is a version of this cost that expresses the behavioral impact of the reform on the government budget as a fraction of mechanical revenues. Since the reform - as many similar policies - was implemented during a budget crisis, this measure is useful 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.

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. 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 cost of 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 and 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 total reform cost is unlikely affected by the economic cycle. In light of these findings, we argue that firms are an important vector for the transmission of Social Security policies and thus they 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).

Our findings also have some direct policy implications. We show leakages on the pension savings generated by the policy, but revenues are still raised in the short-run. A the same time,

some workers pay for the reform twice: in the short-run because of a higher probability of being fired and lower earnings; in the long-run because their retirement date is postponed. The money saved in the short-run could be used to mitigate the costs for younger cohorts. For instance, by extending subsidies to workers fired in firms employing older workers or by lowering the cost of firing employees on the cusp of retirement to reduce spillovers. Moreover, firms could be able to smooth their adjustments in labor demand if the shock occurred on a later date as in the case where the extension of the full retirement age was grandfathered.

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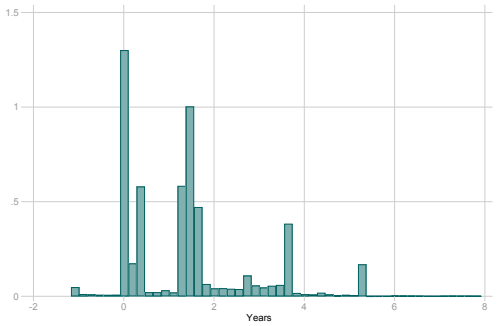
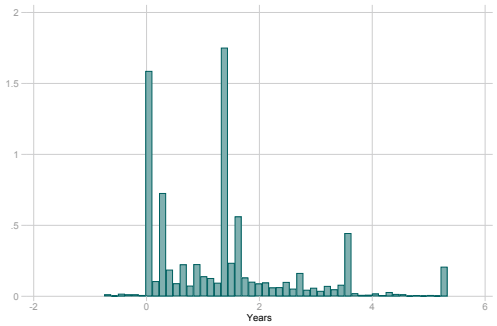
Figures

Figure 1. Shift in full retirement date: distribution

Panel A: Worker-level

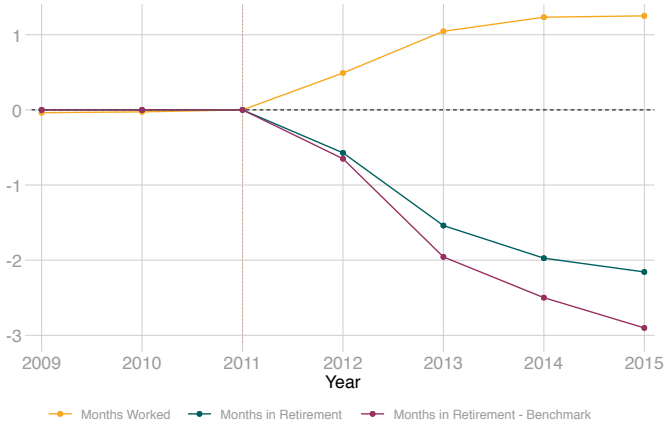


Panel B: Firm-level



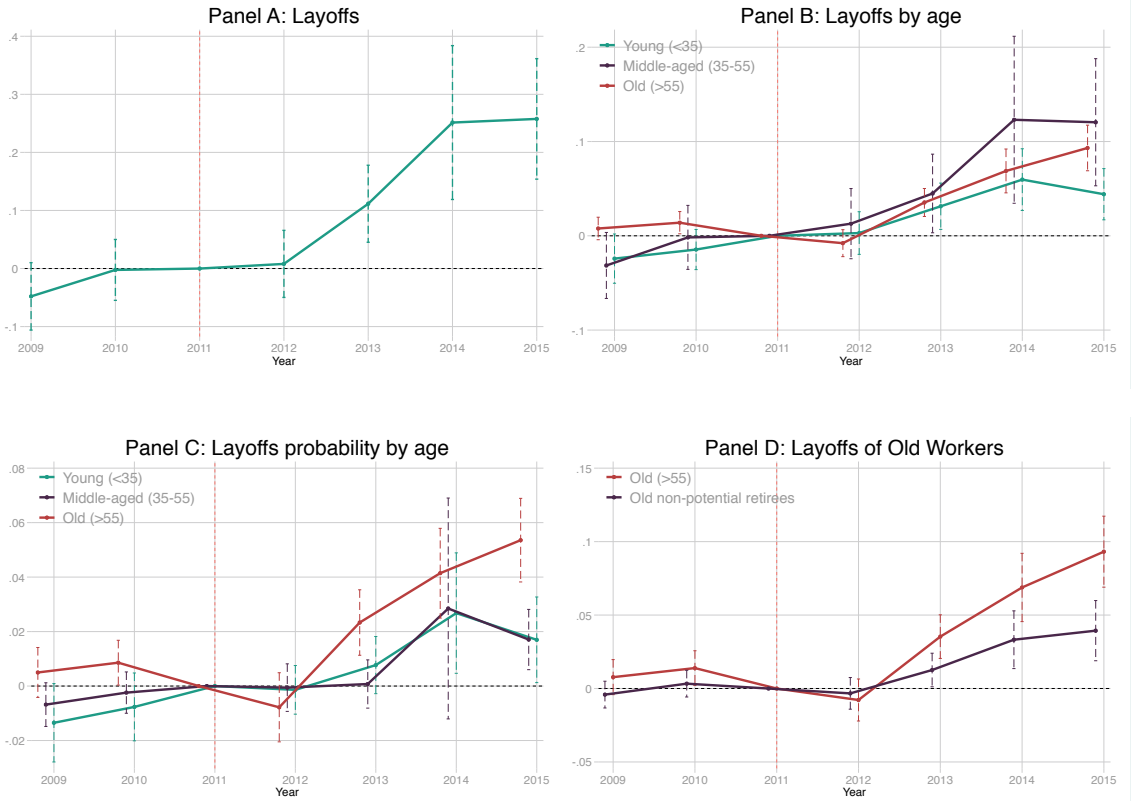
Notes: Panel A shows the distribution of the worker-level shift in the full retirement age among *potential retirees* in our sample of firms. Panel B shows the distribution of the instrument among firms that employ at least one *potential retiree* in the last quarter of 2011. The predicted retirement 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 (sd = 1.4). Number of firms = 61,434. Firm-level treatment mean = 1.37 (sd = 1.33)

Figure 2. The reform prolongs the working life of *potential retirees*



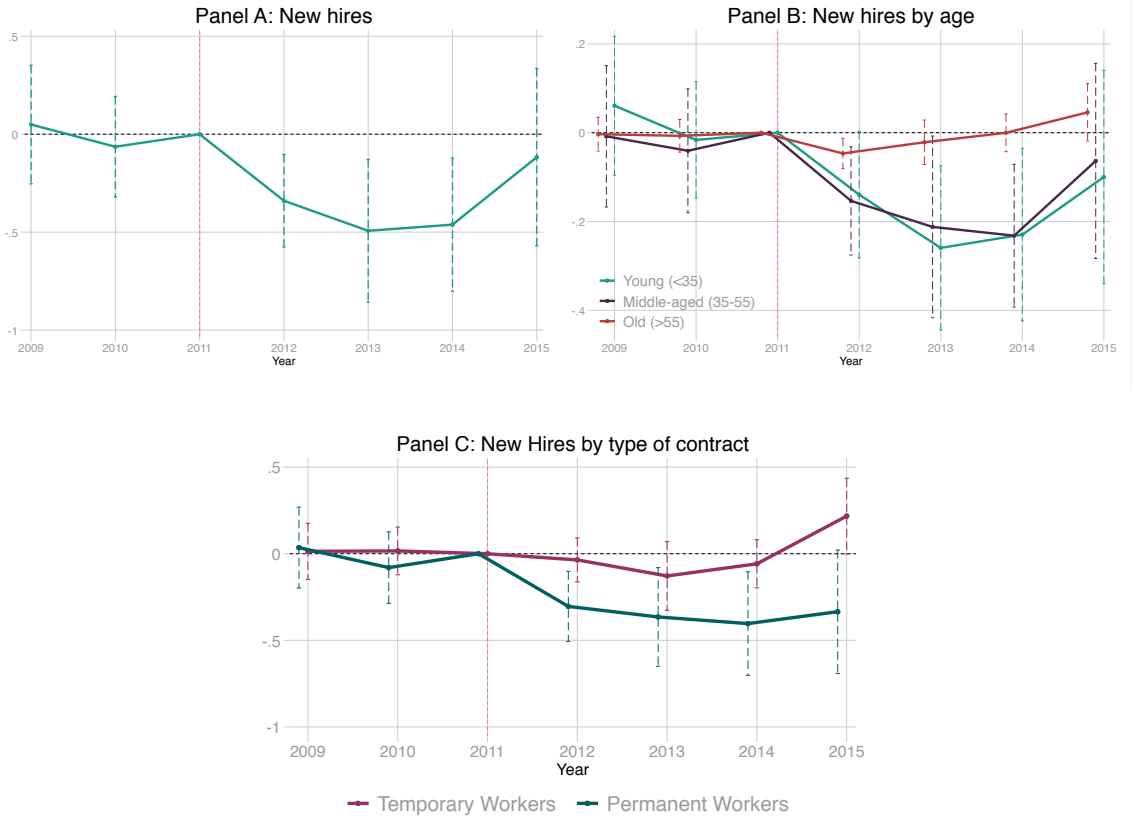
Notes: The Figure shows the effect of a one year shift of the full retirement date. It is based on the specification in (4.5) where the unit of analysis is the single *potential retiree*. We plot the coefficients of the regressions alongside 95% confidence intervals. Standard errors are clustered at the worker level. We define *potential retirees* those workers who were expected to retire within three years (by 2014) under the pre-reform rules when the reform is implemented. We control for individual fixed-effects, age and gender fixed-effects interacted with time dummies. 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.

FIGURE 3. Layoffs



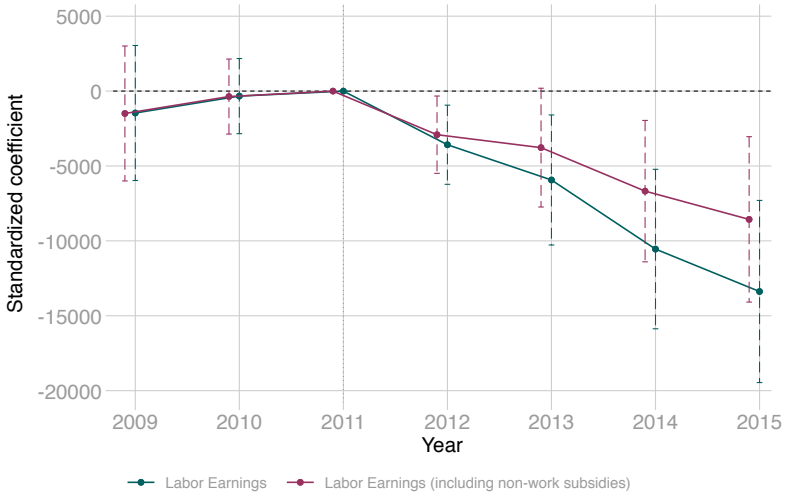
Notes: The Figure shows the response of total layoffs (Panel A), layoffs by age group (Panel B), layoffs probability by age group (Panel C), and layoffs of older workers (Panel D) to an extra retained *potential retiree*, alongside 95% confidence intervals. Standard errors are clustered at the firm level. We define *potential retirees* those workers who were expected to retire within three years under the 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 over 55 years old workers who were not expected to retire within three years under pre-reform rules. The regression is based on specification (4.3) and it includes firm and year fixed-effects. The treatment is defined as the number of *potential retirees* employed at the firm when the reform is implemented. A *potential retiree* is retained if her observed retirement date falls one or more year after her pre-reform full retirement date. The number of *potential retirees* is instrumented with the firm-level average change in their full retirement date defined in equation (4.2). The regressions are run on the universe of private sector firms that (i) were active every year in the period 2009-2015, (ii) employed between 3 and 200 employees in q1-2009, (iii) employed at least one *potential retiree* in q4-2011. Number of observations = 430,038. 1σ of T_i = 1.33 years. Mean outcome pre-reform: total = 0.39; young = 0.13; middle-aged = 0.2; old = 0.06, old non-*potential retirees* = 0.04. KP F-statistics = 8,288.70

FIGURE 4. New Hires



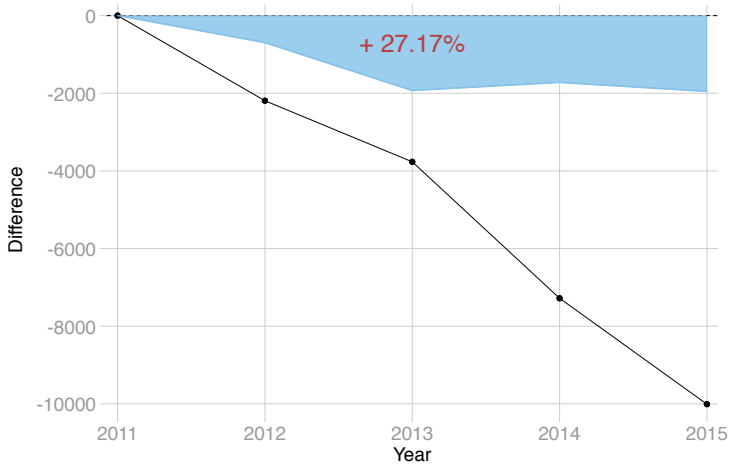
Notes: The Figure shows the response of total new hires (Panel A), new hires by age (Panel B) and new hires by type of contract (Panel C) to an extra retained *potential retiree*, alongside 95% confidence intervals. Standard errors are clustered at the firm level. We define *potential retirees* those workers who were expected to retire within three years under the 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 (4.3) and it includes firm and year fixed-effects. The treatment is defined as the number of *potential retirees* employed at the firm when the reform is implemented. A *potential retiree* is retained if her observed retirement date falls one or more year after her pre-reform full retirement date. The number of *potential retirees* is instrumented with the firm-level average change in their full retirement date defined in equation (4.2). The regressions are run on the universe of private sector firms that (i) were active every year in the period 2009-2015, (ii) employed between 3 and 200 employees in q1-2009, (iii) employed at least one *potential retiree* in q4-2011. Number of observations = 430,038. 1σ of the treatment = 1.33 years. Mean outcome (pre 2012): total = 4.79, young = 2.38, middle = 2.06, old = 0.35, permanent = 1.46, temporary = 3.32. KP F-statistics = 8,288.70

FIGURE 5. Co-workers' labor earnings



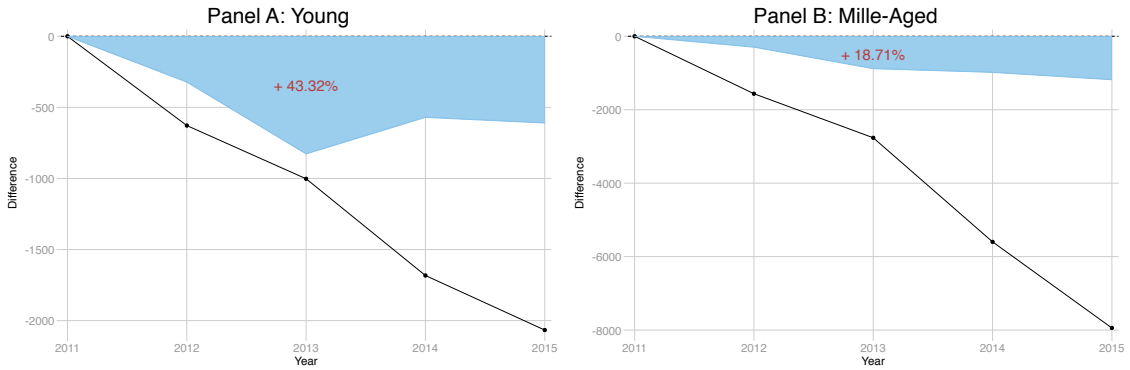
Notes: The Figure shows the effect of a 1σ increase in the treatment on total labor earnings of incumbent co-workers alongside 95% confidence intervals. Standard errors are clustered at the firm level. Incumbent co-workers are non-*potential retirees* who are employed in a firm employing at least one *potential retiree* in q4-2011. 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 (4.5) and is run on the universe of workers employed in private sector firms that (i) were active every year in the period 2009-2011, (ii) employed between 3 and 200 employees in q1-2009, (iii) employed at least one *potential retiree* in q4-2011. Observations are weighted according to firm size at baseline. Number of observations = 511,847. Treatment SD = 1.33. Mean outcomes pre-reform: labor earnings = 621,032.16 ; labor earnings and non-work subsidies = 621,137.28.

FIGURE 6. Decomposing co-workers' earnings loss



Notes: The Figure shows the share of incumbent co-workers' earnings loss that can be imputed to involuntary separations. Incumbent co-workers are non-*potential retirees* who are employed at a firm employing at least one *potential retiree* in q4-2011. The black line plots the effect of a 1σ increase in the treatment on total labor earnings of incumbent full-time co-workers. Labor earnings include earnings from private sector jobs, self-employment and public sector jobs. The blue-shaded area represents the share of earnings loss imputed to involuntary separations. This area is the result of computations employing estimates of the cost of job losses and estimates of the effect of the reform on total separations (Section 8.1).

FIGURE 7. Decomposing co-workers' earnings loss by age



Notes: The Figure shows the share of incumbent co-workers' earnings loss that can be imputed to involuntary separations for young workers (aged below 35, Panel A) and middle-aged workers (aged 35-55, Panel B). See notes to Figure 6 for more details.

FIGURE 8. The effect of the reform on *potential retirees* and co-workers

Notes: The Figure shows the effect of a 1σ increase in the treatment on *potential retirees'* and their co-workers' labor earnings, pension entitlements and total Social Security transfers, alongside 95% confidence intervals. Standard errors are clustered at the firm level. The firm-level regression is based on specification (4.5) 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, sickness 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 q4-2011. Observations are weighted according to firm size at baseline. Treatment SD = 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.

Tables

Table 1. Pre and post-reform pension requirements

	Panel A: Old-age pension			
	Men		Women	
	Pre-reform	Post-reform	Pre-reform	Post-reform
	Age requirement			
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

	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

Note: 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 A1.

Table 2. Response of retirement choices to the change in retirement age

	(1) All	(2) Male	(3) Female
Change in Retirement Date (months)	6.73***	6.54***	6.81***
	(0.034)	(0.073)	(0.045)
Observations	134,832	87,072	47,751
Treatment Mean	1.43	1.21	1.82
Treatment SD	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×sector level. *** $p < 0.01$; ** $p < 0.5$; * $p < 0.1$.

TABLE 3. The effect of the reform on layoffs and new hires

Panel A: Layo s	All	Young	Middle-aged	Old	Old Non-potential retiree
	(1)	(2)	(3)	(4)	(5)
Post x R_i	0.17***	0.047***	0.086***	0.040***	0.021***
	(0.030)	(0.0093)	(0.019)	(0.006)	(0.005)
N	430,038	430,038	430,038	430,038	430,038
Mean pre-2012	0.39	0.13	0.2	0.06	0.04
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

Panel B: New Hires	All	Young	Middle-aged	Old
	(1)	(2)	(3)	(4)
Post x R_i	-0.35**	-0.20**	-0.15**	-0.0022
	(0.159)	(0.090)	(0.072)	(0.018)
N	430,038	430,038	430,038	430,038
Mean pre-2012	4.79	2.38	2.06	0.35
Coeff First Stage	0.21	0.21	0.21	0.21
SE First Stage	0.00	0.00	0.00	0.00
KP F-statistics	8,288.71	8,288.71	8,288.71	8,288.71

Notes: The table reports the results of the IV specification in (4.6). Standard errors are clustered at the firm-level. The treatment T_i instruments the number of retained workers R_i. The coefficients on R_i capture the effect of retaining an extra *potential retiree*. We define *potential retirees* those 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 instrument T_i is defined as the average change in the retirement date of *potential retirees* employed at the firm when the reform is implemented. 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) and Column (5) on old non-*potential retirees*. The regressions are run on the universe of private sector firms that (i) were active every year in the period 2009-2015, (ii) employed between 3 and 200 employees in q1-2009, (iii) employed at least one *potential retiree* in q4-2011. *** $p < 0.01$; ** $p < 0.5$; * $p < 0.1$.

TABLE 4. Fiscal Externality

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

Notes: The Table reports estimates of the fiscal externality based on the formula in (9.1). A negative externality between -1 and 0 implies that savings on pension outlays are larger that 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 $\tau = 25\%$. 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% excluding tax credits). Column (5) reports estimates from a model that augments the formula in (9.1) 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 $\tau = 25\%$. More details on calibrations and estimation are reported in Appendix G.

APPENDIX - For Online Publication

Appendix A. 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 older workers and their younger 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, Devicienti, and Maida (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 older and younger cohorts.

A.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: older (o) and young (y). In our empirical setting older workers are those close to retirement (*i.e. potential retirees*) and young workers are their co-workers. Denote with n_0^y and n_1^y the number of young workers employed in period 0 and 1, respectively. Adjustments in the demand for young workers are referred to as x^y, so that $n_1^y = n_0^y + x^y$. A cost function $c\left(x^y\right)$ accounts for the cost of adjusting the young workforce, which is paid in period 0. We require $c\left(\cdot\right)$ to be twice continuously differentiable and we assume that $c'\left(x^y\right) > 0$ for $x^y > 0$, $c'\left(x^y\right) \leqslant 0$ for $x^y < 0$, $c'\left(0\right) = 0$ and $c''\left(\cdot\right) \geqslant 0$. This cost is flexible enough to incorporate any asymmetry in adjusting downwards or upwards the young labor demand. For the sake of simplicity, we assume that no older worker can be either hired or fired. We denote with n_0^o and $n_1^0 = sn_0^o$ the number of older workers in period 0 and 1, respectively. $s \leqslant 1$ captures the exogenous share of older workers who are left in period 1. We interpret s as a variable incorporating the exogenous separation rate of older workers as well as retirement rules. Output is produced according to technology $F\left(n_t^o, n_t^y\right)$ in every period $t = 0, 1$, with $F_{11}, F_{22} \leqslant 0$ 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 young workers in period 1 is chosen so as to maximize profits, which are given by

$$\pi = \pi_0 + \beta\left(F\left(sn_0^o, n_0^y + x^y\right) - w^o sn_0^o - w^y\left(n_0^y + x^y\right)\right) - c\left(x^y\right) \tag{A.1}$$

where π_0 are profits in period 0, β is a discount factor, and w^o and w^y are the wages in period 1 of older and young workers respectively. Optimality conditions require the following

$$\beta \left(F_2 \left(s n_0^o, n_1^y \right) - w^y \right) = c' \left(x^y \right) \tag{A.2}$$

The firm equates the marginal increase in revenues net of wage expenditures to the marginal cost of adjusting young 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 older workers in period 1, *i.e.* an increase in s. The comparative statics for a change in s is

$$\frac{\partial x^y}{\partial s} \propto \beta \left(F_{21} n_0^o - \frac{\partial w^y}{\partial s} \right) \tag{A.3}$$

The sign of the comparative statics depends on two terms. First, the degree of substitutability between the two types of labor captured by F_{12}. 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^y can lead to an increase in the demand for young workers. Indeed, in order to hire young workers, the firm must significantly cut the payroll to compensate the loss in marginal productivity of young workers that follows an exogenous increase in the number of older workers. 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 young workers. We present here a few interesting cases. First, if wages are sticky (*i.e.* $\partial w^y / \partial s = 0$), the response of young labor demand depends on the substitutability between young and older workers. If the two are substitutes - that is $F_{21} < 0$ - the firm decreases demand for young workers. Second, if wages are flexible and partially follow the change in the marginal productivity of young workers (i.e. $\partial w^y / \partial s = \alpha n_0^o F_{21}$ with $\alpha < 1$), labor demand decreases as long as the two types of work are substitutes and $F_{21} < 0$.[49] Finally, in a competitive labor market where wages reflect the marginal productivity of young 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 can be reconciled with complementarity between young and older workers only in case of a large increase in the wage of young worker.*

We document in Section 6 a drop in the labor demand of firms that are more affected by the reform. Moreover, a large increase in younger workers' wages is inconsistent with the evidence we provide in Section 8, which shows a drop in earnings for younger cohorts. We conclude that younger cohorts are substitutes for older workers retiring. Our evidence also excludes patterns of no substitutability between workers (i.e. $F_{12} = 0$). Indeed, if this was the case, a drop in demand could not be explained by decreasing wages for younger cohorts.

[49] There are different explanations for having young 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.2. **Alternative Wage Formation Models.**

A.2.1. *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 young workers have bargaining power ϕ and outside option $\underline{w}^y$. 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

$$\phi \frac{\partial \pi(s n_0^o, n_1^y)}{\partial n_1^y} = (1-\phi)(w^y - \underline{w}^y) \tag{A.4}$$

which implies the following expression for the equilibrium wage

$$w^y = \underbrace{\eta F_2 - \frac{\eta}{\beta} c'(n_1^y - n_0^y)}_{\text{Marginal output net of adjustment costs}} + \underbrace{\frac{(1-\phi)}{\phi\beta} \eta \, \underline{w}^y}_{\text{Reservation wage}} \tag{A.5}$$

where $\eta = \phi\beta/(\phi\beta + 1 - \phi)$. When young 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, Marque, and Wasmer (2008). Wages in equilibrium are a function of young workers' marginal output net of marginal cost and of worker's reservation wage. We are interested in the effect of a change in the separation rate on wages that reads:

$$\frac{\partial w^y}{\partial s} = \eta[F_{21} n_0^o + F_{22} \frac{\partial n_1^y}{\partial s}] - \frac{\eta}{\beta} c''(n_1^y - n_0^y) \frac{\partial n_1^y}{\partial s} \tag{A.6}$$

The wage change in response to a shock to the retention rate depends on the cross-marginal product between young and older labor, as well as on the slope of young 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 young workers in period 1:

$$\frac{\partial x_1^y}{\partial s} = -\frac{\beta F_{21} n_0^o}{\beta F_{22} - c''(n_1^y - n_0^y)} \tag{A.7}$$

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 young workers and the complementarity between the two types of labor. It follows*

that a drop in young labor demand caused by a change in s is only consistent with substitutability between older and young workers.

A.2.2. *Profit Sharing.* Card, Devicienti, and Maida (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

$$\phi\pi = (1-\phi)\left(w^y - \underline{w}^y\right) \tag{A.8}$$

This implies the following:

$$(1-\phi+\beta\phi n_1^y)\, w^y = \beta\phi\left(\left(F - w^o s n_0^o\right)\beta - \frac{1}{\beta}c\left(n_1^y - n_0^y\right)\right) + (1-\phi)\,\underline{w}^y \tag{A.9}$$

Wages are determined by profits net of young 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:

$$\frac{\partial w^y}{\partial s} = \tilde{\eta} n_0^o \left(F_1 - w^o\right) \tag{A.10}$$

Because of an envelope argument, the effect of the reform on young workers' wages is proportional to the wedge between older workers' productivity and wage. Intuitively, a larger gap increases the passthrough of the reform to young workers' wages. In response to the change in profits caused by the reform, firms decrease the salary of younger workers to preserve the wedge for older workers.[50] After replacing (A.10) in (A.3) it follows that if wages for young workers decline, labor demand can drop only in case $F_{12} < 0$.

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

A.2.3. *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 $n_1^y = w^e$, where e is the elasticity of labor supply to the wage and $e > 0$. The firm's problem would become

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

[50] If firms where able to adjust older workers wages the total pass-through on young workers would be smaller.

The firm's optimality condition in this model is

$$\beta\left(F_2 - \frac{1+e}{e} n_1^{y\frac{1}{e}}\right) = c'\left(n_1^y - n_0^y\right) \tag{A.12}$$

From A.12 we derive a new comparative statics

$$\frac{\partial x_1^y}{\partial s} = -\frac{F_{21} n_0^o}{F_{22} + \frac{1+e}{e^2} n_1^{y\frac{1-e}{e}} - \frac{1}{\beta} c''(n_1^y - n_0^y)} \tag{A.13}$$

The expression above shows a one-to-one mapping between labor demand changes and the substitutability between older and younger 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 labor demand in response to the reform.

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

Appendix B. 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.[51] 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.[52] 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 from 31.4 percent in 2005 to 48.2 percent in 2015.[53] 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.[54] 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.

[51] Data from Eurostat, annual enterprise statistics. Financial and insurance sectors are included.

[52] 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).

[53] Source: Eurostat, Employment statistics.

[54] See OECD (2015) data on employment protection legislation.

Appendix C. Additional Details about the Fornero reform

Grandfathering clauses. The new rules brought about by the *Fornero* reform apply to all workers who did not qualify for either old-age or seniority pensions under previous rules by the end 2011. The law moreover allows for some specific categories of workers to exceptionally continue retiring under old rules. 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 workers who could still retire under old rules are the following:

i) Workers who accrue their old-age or seniority pension rights by 31/10/2011;
ii) Workers *collocati in mobilitá* according to law 223/91 and based on collective agreements signed before 31/10/2011. Workers *collocati in mobilitá* were laid-off workers who received a specific monetary support and were engaged in redeployment programs;
iii) Workers who, as of 31/10/2011, were beneficiaries of *prestazioni straordinarie a carico dei fondi di solidarietá di settore*. These are workers on short-time work who received monetary support from *ad-hoc* sectoral solidarity funds;
iv) Workers who, as of 31/10/2011, had ceased to work but had been authorized to continue to pay contributions.

In the following years, specific categories of workers where granted the right to still retire under old rules (so-called *salvaguardie*).

Exceptions to the main requirements. Some exceptions applied to the requirements reported in Table A1. Women who were at least 60 years old and had at least 20 years of contribution by 2012 can also 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. The same exception is granted to all workers who would have reached quota 96 in 2012. Following the reform, individuals who start working in 1996 or later can claim an old-age pension upon turning 70 years old in 2012, 70 years and 3 months old in 2013-2015 and 70 years and 7 months old from 2016 onward, conditional on having 5 years of effective (i.e. from work-related activities) contributions. Alternatively, 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. Workers eligible for "quota" 96 by 2012 under old rules could exceptionally retire upon turning 64 years old in 2012, 64 years and 3 months old in 2013-2015 and 64 years and 7 months old from 2016 onward.

Appendix D. 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 that feature 0 wage. If all records feature 0 wage, we keep one randomly.

ii) If there are records that feature the same contract characteristics (occupation, duration, full-time or part-time status, typology 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 only in a single month, 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.[55] 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 payd days b) highest wages.[56] If more than one record survives criteria (a) to (d), we drop all but one randomly.

[55] If more than one records satisfies criterion (a), we then use criterion (b), and so on up to criterion (d).
[56] If more than one records satisfies criterion (a), we then use criterion (b).

Appendix E. 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). Specifically:

i) Old-age pensions: both under new and old rules, all contributions count toward totalling the requested 20 years of qualifying contributions. Workers who accrue the first contribution after January 1, 1996 can retire when meeting the same age requirement as others (old rules) or when turning 70 years old (new rules), conditional on having 5 years of effective qualifying contributions.

ii) Seniority pensions: under old rules workers can retire when the sum of their age and years of qualifying contributions reaches a certain "quota". All contributions except those associated to unemployment and maternity leave count toward meeting the "quota". Alternatively, they can retire when they accrue 40 years of qualifying contributions, regardless of their age. All contributions count, but conditional on having accrued at least 35 years of effective qualifying contributions. Under new rules, all contributions count toward accruing the pension rights.

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 sum 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.[57] Following rules for totalling contributions used at INPS, in case of (partially or totally) overlapping spells we count the overlap only once. 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.

[57] Workers in entertainment and sport industries can accrue more than 52 weeks per contributions per year. We take this exception into account, by not capping contributions for these categories of workers.

Appendix F. 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:

$$Y_{it} = \alpha + \lambda_i + \sum_{k=-3}^{3} \beta_k \gamma_k + \sum_{k=-3}^{3} \beta_k^l \gamma_k \times \text{Separation}_i + \varepsilon_{i,t} \tag{F.1}$$

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 β_3^l in (F.1) 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 $s = 1, ..., S$ 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

$$w_i = \frac{N_C}{N_T} \times \frac{N_{Ts}}{N_{Cs}}$$

while each treated unit receives weight equal to 1 (see Iacus, King, and Porro (2011)). This guarantees that weights sum to total matched observations:

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

Appendix G. The Fiscal Externality

G.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 *potential retirees* (pr) 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:

$$\begin{aligned} x_i \leqslant\ & (1-\tau_i)\, w_i l_i^w + (1-\tau_i)\left(NW_i l_i^{NW} + ST_i l_i^{ST} + D_i l_i^D + SL_i l_i^{SL}\right) \\ & + (1-\tau_i^p)\left(P_i\left(T - T_i^P\right)\cdot I\left(T > T_i^P\right) + P_i^E l_i^E\right) + y_i \end{aligned} \tag{G.1}$$

where $\left\{\tau_i, NW_i, ST_i, D_i, SL_i, P_i, T_i^P, P_i^E\right\}$ is a vector of policies targeted to agent i. τ_i is an average labor earnings tax, τ_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, LS_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.[58] 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:

$$FE = -\frac{\sum_{i=pr,c} n_i \left[(1-\tau_i)\left(NW_i \frac{dl_i^{NW}}{dT_i^P} + ST_i \frac{dl_i^{ST}}{dT_i^P} + D_i \frac{dl_i^D}{dT_i^P} + SL_i \frac{dl_i^{SL}}{dT_i^P}\right) + (1-\tau_i^p) P_i^E \frac{dl_i^E}{dT_i^P} - \tau_i \frac{dz_i}{dT_i^P}\right]}{\sum_{i=a,c} n_i dT_i^P (1-\tau_i^p) P_i I\left(T > T_i^P\right)} \tag{G.2}$$

where n_{pr} and n_c denote the number of *potential retirees* and co-workers. The numerator represents the costs incurred by the government because of behavioral responses. Mechanical revenues in the denominator instead measure the resources that the government would save through the policy absent any change in the behavior of workers and firms.

G.2. **Empirical implementation and results.** The fiscal externality is a function of the estimates in Sections 6 and 8. The terms referring to NW, ST, D and LS in the numerator of (9.1) 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.[59] The last term in the numerator of (9.1) 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

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

[59] Coefficients are reported in table A12.

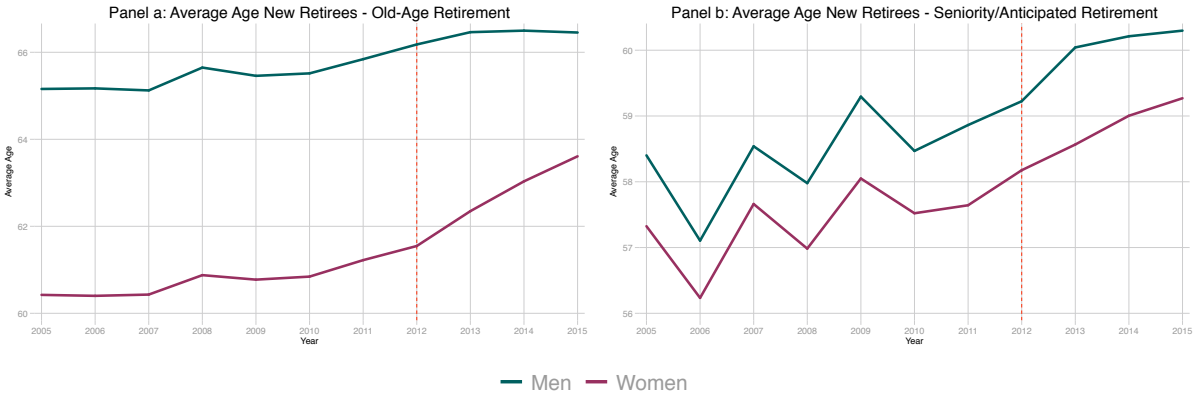
P^E as a conservative 70 percent of the average and median value of monthly pension payments in the data (13,100 and 16,300 euros, respectively).[60] We check alternative parametrizations of τ ranging from 20 percent to 30 percent for robustness. Notice that the average income tax rate for the median income (roughly 22,000 euros) is 24 percent without considering tax credits. We calibrate τ_i^p starting from τ_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.[61] Finally, we obtain the mechanical effects in the denominator of (9.1) 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.

[60] 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 C). Hence, our calibration understates the benefit received when they retire before the statutory date. We also show a calibration whereby $P^e = 0.9 \times P$.

[61] 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 $1297 \times (55000 - \text{Pension})/40000$.

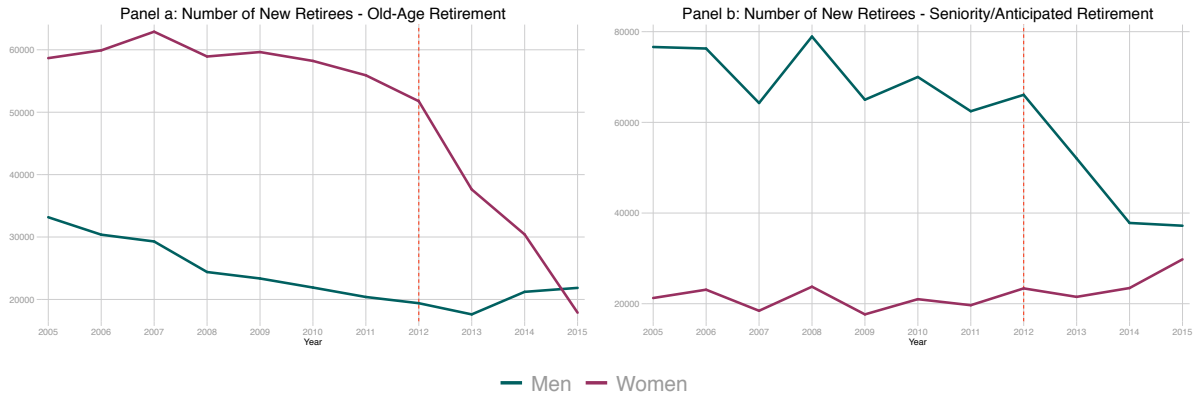
Appendix H. Additional figures and tables - For Online Publication

Figure A1. Average age of new retirees by gender and type of pension



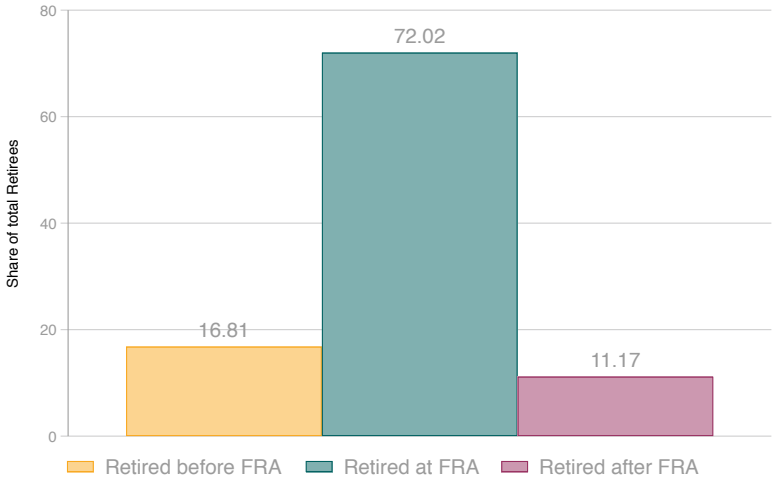
Notes: The figure shows the evolution of the average age at retirement, split by gender and type of pension. Panel A refers to old-age pensions, Panel B refers to seniority pensions. We classify as seniority pensions those claimed with "quota 40" and "quota 96" before the reform and replaced by "anticipated pension" in 2012. The vertical line represents the year when the reform becomes effective (2012).

Figure A2. Number of new retirees by gender and type of pension



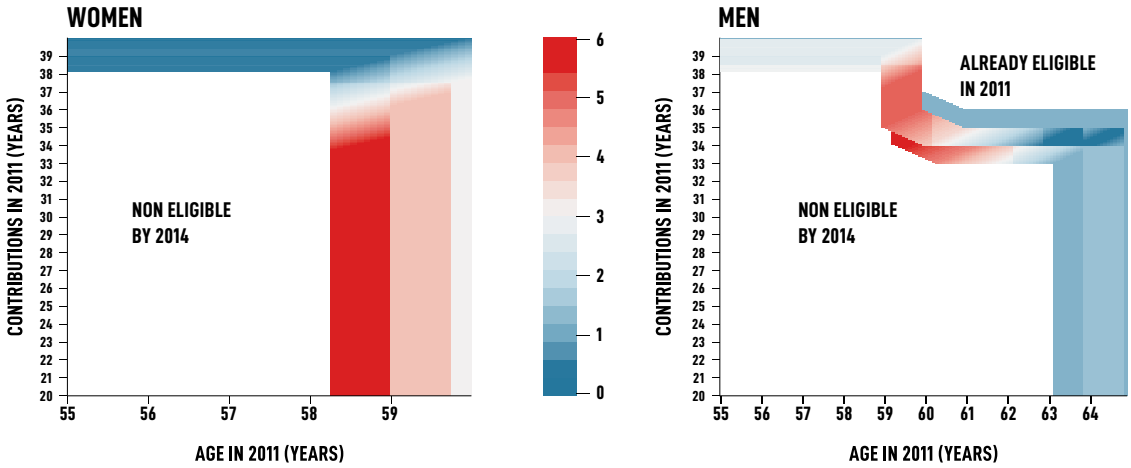
Notes: The figure shows the evolution of the number of new retirees, split by gender and type of pension. Panel A refers to old-age pensions, Panel B refers to seniority pensions. We classify as seniority pensions those claimed with "quota 40" and "quota 96" before the reform and replaced by "anticipated pension" in 2012. The vertical line represents the year when the reform becomes effective (2012). The lines plot two-years moving averages using lags only.

FIGURE A3. Share of workers retiring at FRA



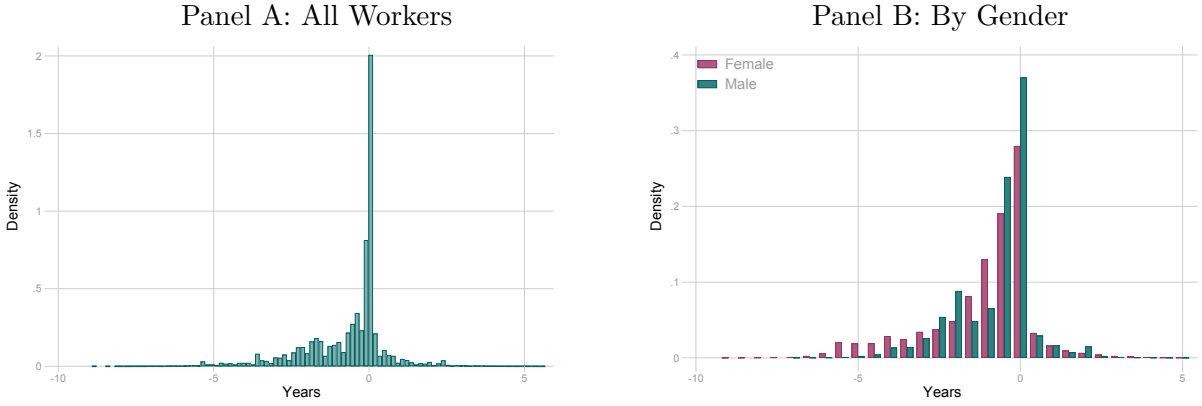
Notes: The figure shows the share of retirees retiring at full-retirement age; the share of workers early retiring (more than 1 year before the FRA); and the share of workers who retire more than 1 year after the full-retirement age. Shares are derived from authors calculations on the INPS register of retirees.

FIGURE A4. Reform-induced changes in the retirement age



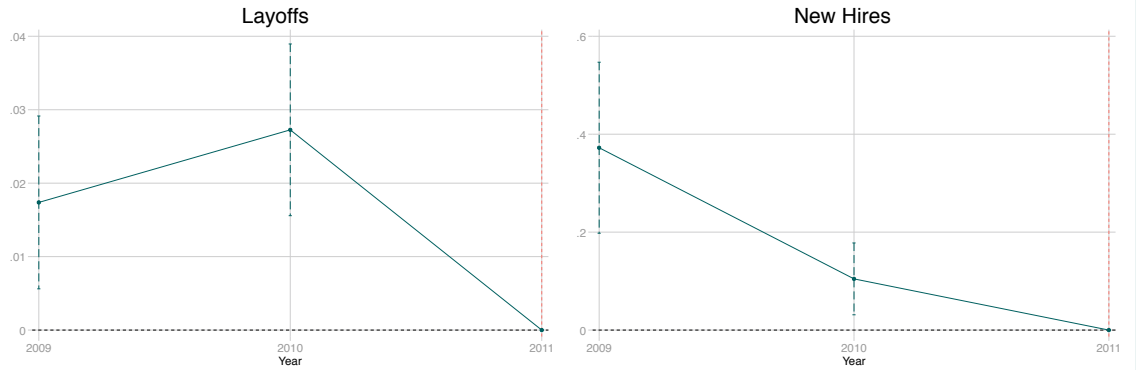
Notes: The Figure plots heatmaps showing the relationship between the reform-induced shift in the retirement age and the characteristics of the worker in 2011. The characteristics are worker's gender, age and years of contributions in December 2011. The shifts are constructed using the rules detailed in Table A1 under the assumptions listed in Section 4.1.

FIGURE A5. Post-reform retirement date - Forecast quality assessment



Notes: The Figure shows a forecast quality assessment of our individual treatment. The horizontal axis measures the difference between the post-reform predicted full retirement date and the observed retirement date. The sample includes 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 the worker early retires. Panel A shows the distribution for the entire sample of workers, Panel B shows the breakdown by gender. Number of workers = 160,527.

FIGURE A6. Share of employees retained as treatment - estimation pre-trends

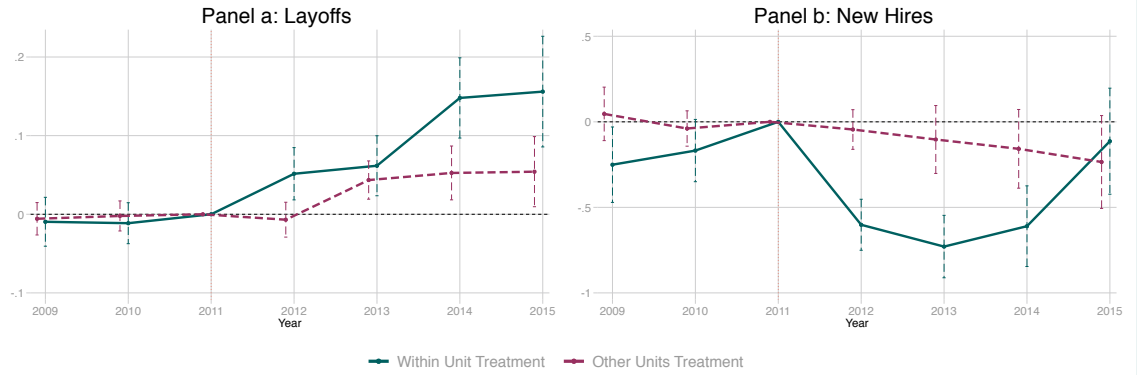


Notes: The Figure shows the effect of retaining 10% of the total firm employees on layoffs and new hires in the pre-reform period, alongside 95% confidence intervals. Standard errors are clustered at the firm level. Results are based on the OLS version of specification (4.3), which includes firm and year fixed effects. The treatment is the number of retained *potential retirees* over the number of employees in 2011. We define *potential retirees* those 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 private sector firms that (i) were active every year in the period 2009-2015, (ii) employed between 3 and 200 employees in q1-2009, (iii) employed at least one *potential retiree* in q4-2011. Number of observations: 430,038. Pre-reform mean outcomes: layoffs = 0.39; new hires = 4.79

FIGURE A7. Firms mostly respond to shock on *potential retirees*

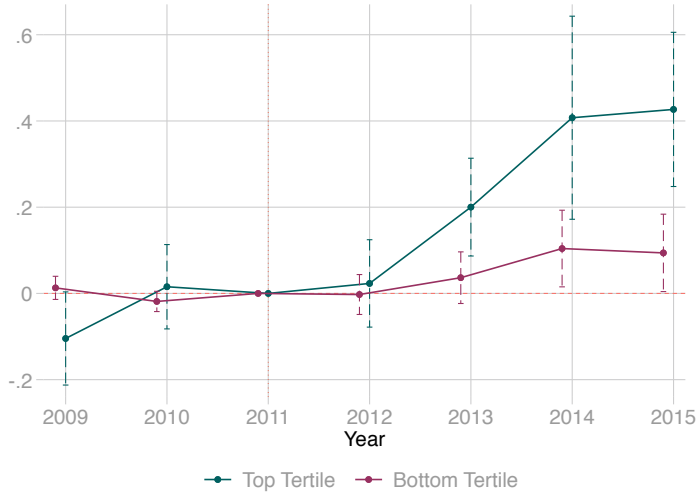
Notes: The Figure shows the effect on layoffs (Panel A) and new hires (Panel B) of two treatments, alongside 95% confidence intervals. Standard errors are clustered at the firm level. The regression is based on a variant of the specification (4.3) where two treatments are included. The first treatment is defined as the number of retained workers who - under pre-reform rules - were expected to retire within three years when the reform is passed. The second treatment is defined as the number of retained workers who - under pre-reform rules - were expected to retire in four to five years when the reform is passed. A worker is retained if she retires more than one year after the pre-reform predicted retirement age. The two treatments are instrumented with the average shift in the retirement date in the respective sample of workers. The regressions are run on the universe of private sector firms that (i) were active every year in the period 2009-2015, (ii) employed between 3 and 200 employees in q1-2009 and (iii) employed at least one worker expected to retire by 2014 under pre-reform rules and one workers expected to retire in 2015 or 2016 in q4-2011. Number of observations = 189,861. Mean outcomes pre-reform: layoffs = .47; new hires = 7.07. KP F-statistics = 32.52

FIGURE A8. Labor demand within and across qualifications



Notes: The Figure shows the heterogeneous effect of the treatment on layoffs and new hires within and across qualifications, alongside 95% confidence intervals. Standard errors are clustered at the firm level. Qualifications are: blue collar job, white collar job and manager. The regression is a version of specification (4.3) where the unit of analysis is the firm-qualification and two treatments are included. The first treatment measures the number of retained *potential retirees* employed at the firm-qualification level when the reform is implemented; the second treatment is the number of retained *potential retirees* employed at the same firm but with other qualifications when the reform is implemented. A worker is retained if she retires more than one year after the pre-reform predicted retirement age. We define *potential retirees* those workers who were expected - under the pre-reform rules - to retire within three years when the reform is implemented. The two treatments are instrumented with the average shift in the retirement date in the respective sample of workers. The regressions are run on the universe of private sector firms that (i) were active every year in the period 2009-2015, (ii) employed between 3 and 200 employees in q1-2009, (iii) had at least two qualifications with at least three workers in the baseline period, (iv) employed at least one *potential retiree* in q4-2011. Number of observations = 580,734. Mean outcomes pre-reform: layoffs per qualification = 0.14; new hires per qualification = 2.18. KP F-statistics = 794.65.

FIGURE A9. Layoffs in high and low-turnover firms

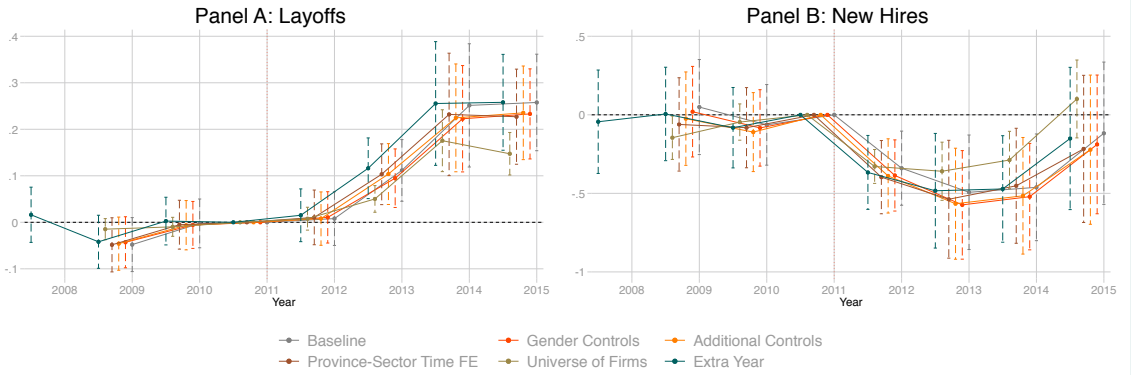


Notes: The Figure shows the heterogeneous response of layoffs in firms which are above and below the median of turnover rates in the pre-reform period, alongside 95% confidence intervals. Standard errors are clustered at the firm level. The regression is based on a triple difference specification

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

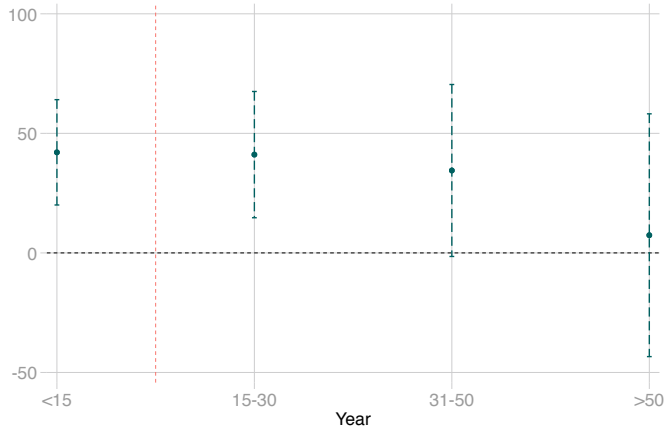
The plotted coefficients are the linear combination of β_k^Rs and $\beta_k^{R,to}$s. Turnover rate is the average total number of layoffs, quits and non-renewed contracts in the pre-reform period (2009-2011). The treatment is defined as the the number of retained *potential retirees* employed at the firm when the reform is implemented. We define *potential retirees* those workers who were expected to retire within three years under the pre-reform rules when the reform is implemented. We instrument the treatment with the average shift in the retirement date of *potential retirees* employed at the firm when the reform is implemented. The regression is run on the universe of private sector firms that (i) were active every year in the period 2009-2015, (ii) employed between 3 and 200 employees in q1-2009, (iii) employed at least one *potential retiree* in q4-2011. Number of observations = 430,038. 1σ of the treatment = 1.33 years. Mean outcome pre-reform = 0.39.

FIGURE A10. Robustness to alternative specifications and samples



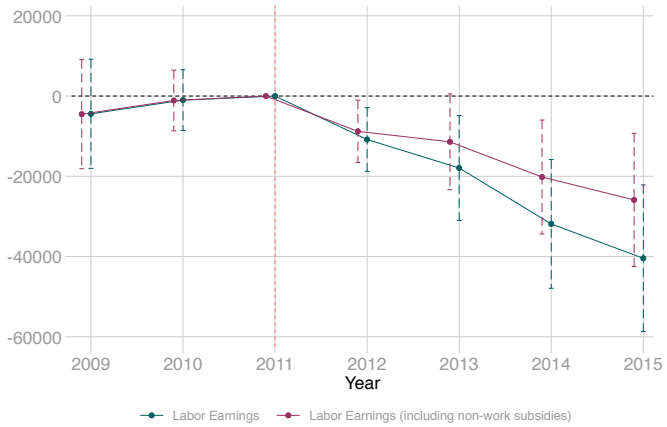
Notes: The Figure addresses the robustness of the main estimates on layoffs (Panel A) and new hires (Panel B). We confront the results of the baseline specification reported in Figures 3 and 4 with the results of sensitivity checks that employ the specification in (??). The plotted coefficients represent a 1σ change in the treatment. First, we control for quintiles of the share of male workers at the firm to eliminate differential trends explained by gender compositions. Second, we add as controls quintiles of the share of young (< 35), middle-aged ($35 - 55$) and old (> 55) workers, firm size, firm age and firm's average wage. Third, we allow for differential time trends in provinces and two-digits sectors to capture different states of the business cycle. Fourth, we run our analysis on the universe of firms by setting $T_i = 0$ and $R_i = 0$ for the firms that do not employ any *potential retiree*. This allows to check whether firms with no *potential retirees* in the workforce have different trends prior to the reform. Finally, we include an extra year in the pre-period to check that trends are balanced over a longer period of time. The Figure reports 95% confidence intervals. Standard errors are clustered at the firm level.

FIGURE A11. Layoffs by firm size



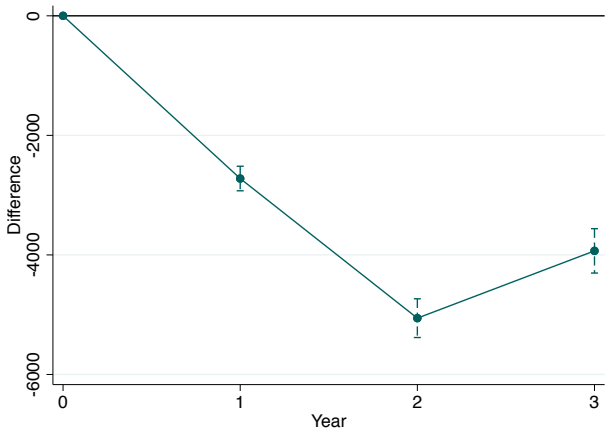
Notes: The figure shows the cumulative post-reform percentage change in layoffs by firm size. The plotted coefficients are obtained by re-scaling the post-reform cumulative coefficients from specification (4.5) by the pre-reform mean outcome. The vertical red line separates firms where a more stringent dismissal discipline applies (> 15 employees) from other firms. The regression includes firm and year fixed effects and standard errors are clustered at the firm level. The treatment is defined as the average change in the retirement date for *potential retirees* employed at the firm when the reform is implemented. We define *potential retirees* as those workers who were expected to retire within three years under the pre-reform rules when the reform is implemented. The regressions are run on the universe of private sector firms that (i) were active every year in the period 2009-2015, (ii) employed between 3 and 200 employees in q1-2009, (iii) employed at least one *potential retirees* in q4-2011. Number of observations = 430,038; 1 SD of the treatment = 1.33 years Pre-reform mean outcome: 0-15 = 0.28; 15-30 = 0.42; 30-50 = 0.54; > 50 = 0.90

FIGURE A12. Effect on earnings: IV Estimates



Notes: The Figure shows the effect of retaining an additional *potential retiree* on co-workers earnings, alongside 95% confidence intervals. Standard errors are clustered at the firm level. The regression is based on specification 4.3 and includes firm and year fixed effects. Results are based on an IV strategy whereby the number of retained *potential retirees* is instrumented using T_i, the average shift in the retirement date of *potential retirees* in firm i. We define *potential retirees* as those 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. Incumbent co-workers are non-*potential retirees* who are employed at a firm employing at least one *potential retiree* in q4-2011. The regression is run on the universe of private sector firms that (i) were active every year in the period 2009-2015, (ii) employed between 3 and 200 employees in q1-2009, (iii) employed at least one *potential retiree* in q4-2011. Number of observations: 511,847; Pre-reform mean outcomes: Labor earnings = 621,032.16; Labor earnings including non-work subsidies = 621,137.28; KP F-Statistics: 243.54

FIGURE A13. 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 A1. Pre and post-reform pension requirements - Additional Details

Panel A: Old-age pension

	Men		Women	
	Pre-reform	Post-reform	Pre-reform	Post-reform
		Age requirement		
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
		Contribution requirement		
	20YC	20YC	20YC	20YC
		Waiting window		
	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
		Waiting window			
	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, 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.

TABLE A2. Placebo Tests

	Layoffs	New Hires
Treatment X Post 2009	0.0100*	-0.017
	(0.0053)	(0.027)
Treatment X Post 2010	0.0054	0.0015
	(0.0053)	(0.028)
Observations	184,302	184,302
Mean Outcome (pre 2012)	.39	4.79
Treatment Mean	1.37	1.37
Treatment SD	1.33	1.33

Notes: The Table reports the coefficients from a set of placebo tests where we re-allocate the reform effective date in the years 2010 and 2011 and test the effect of a 1 SD of the instrument on the main outcomes. The sample is restricted to the period 2009-2011. The regression is based on specification (4.6) and it includes firm and year fixed-effects. The treatment is defined as the firm-level average change in the full retirement date of *potential retirees* defined in equation (4.2). The regressions are run on the universe of private sector firms that (i) were active every year in the period 2009-2015, (ii) employed between 3 and 200 employees in q1-2009, (iii) employed at least one *potential retiree* in q4-2011.

TABLE A3. Relation between the instrument and firm's covariates

	(1)	Mean (2)
Share young workers (< 35)	0.000	0.297
	(0.001)	
Share middle-aged workers ($35 - 55$)	-0.008***	0.58
	(0.001)	
Share old workers (> 55)	0.008***	0.123
	(0.000)	
Share of *potential retirees*	-0.000	0.118
	(0.000)	
Share male workers	-0.008***	0.656
	(0.001)	
Share white-collar workers	0.001	0.327
	(0.001)	
Average gross daily real wage	0.500	92.042
	(0.462)	
Share full-time workers	-0.001*	0.885
	(0.001)	
Firm size	0.370***	25.848
	(0.086)	
Firm age	-0.114**	19.935
	(0.038)	
Firm in manufacturing	-0.000***	0.449
	(0.000)	
N. firms	61,434	

Notes: The table reports a set of balancing tests whereby firms' baseline characteristics are regressed on the firm-level treatment. We add to the regressions province fixed effects, sector fixed effects, as well as province × sector fixed effects. Column (1) reports coefficients and standard errors clustered at the province × sector level in parenthesis. Column (2) displays mean values of the dependent variable. The treatment is defined as the firm-level average change in full retirement date of *potential retirees* as defined in equation (4.2). 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 regressions are run on the universe of private sector firms that (i) were active every year in the period 2009-2015, (ii) employed between 3 and 200 employees in q1-2009, (iii) employed at least one *potential retiree* in q4-2011. Treatment SD = 1.33 years.

TABLE A4. Firms with at least one *potential retiree* and other firms

	Master 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 $\leqslant 35$	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	61434		333800	

Notes: The table reports descriptive statistics for the master sample of firms, as well as for other firms in the same size class (3-200) that remain active throughout the period 2009-2015. Average workforce tenure and experience are truncated at 29 years, because matched employer-employee data are available since 1983. Firms in the Master sample (i) were active every year in the period 2009-2015, (ii) employed between 3 and 200 employees in q1-2009, (iii) employed at least one *potential retiree* in q4-2011.

TABLE A5. *Potential retirees* and other workers in the master sample

	Potential retirees		Other workers	
	mean	sd	mean	sd
Treatment	1.36	1.40	1.19	1.24
Gender (1 = male)	0.71	0.46	0.71	0.45
Age	57.77	2.89	40.91	9.77
Tenure (mean)	14.24	9.37	8.81	7.44
Tenure (median)	12.25	0.00	6.75	0.00
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
Full-time contract	1.00	0.00	1.00	0.00
Daily gross real wage	110.61	149.79	102.72	136.70
Daily gross real wage (windsorized, mean)	109.74	114.52	102.17	111.78
Daily gross real wage (windsorized, median)	86.46	0.00	79.95	0.00
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	98358		1434381	

Notes: The table reports the baseline characteristics of *potential retirees* and co-workers employed in firms belonging to the master sample in 2009. Tenure and experience are truncated at 29 years, because matched employer-employee data are available since 1983 only. Firms in the Master sample (i) were active every year in the period 2009-2015, (ii) employed between 3 and 200 employees in q1-2009, (iii) employed at least one *potential retiree* in q4-2011.

TABLE A6. Absences from work for similar *potential* and non *potential retirees*

	Non Potential retirees		Potential retirees		Difference
	Mean	SD	Mean	SD	
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***
Gross daily real wage	109.05	147.34	107.69	143.81	-1.361***
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 *potential retirees* and non-*potential retirees* 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.

TABLE A7. The effect of the reform on layoffs

	All	Young	Middle-aged	Old	Old Not affected
	(1)	(2)	(3)	(4)	(5)
$t-3$	-0.048	-0.024*	-0.031*	0.0078	-0.0041
	(0.030)	(0.013)	(0.018)	(0.0061)	(0.0046)
$t-2$	-0.0024	-0.015	-0.0017	0.014**	0.0033
	(0.027)	(0.011)	(0.017)	(0.0060)	(0.0046)
t	0.0080	0.0029	0.013	-0.0078	-0.0033
	(0.029)	(0.012)	(0.019)	(0.0073)	(0.0055)
$t+1$	0.11***	0.031**	0.045**	0.035***	0.013**
	(0.034)	(0.013)	(0.021)	(0.0076)	(0.0058)
$t+2$	0.25***	0.060***	0.12***	0.069***	0.033***
	(0.068)	(0.017)	(0.045)	(0.012)	(0.010)
$t+3$	0.26***	0.044***	0.12***	0.093***	0.039***
	(0.053)	(0.014)	(0.034)	(0.012)	(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
KP F-Statistics	1,214.09	1,214.09	1,214.09	1,214.09	1,214.09
Treatment Mean	0.83	0.83	0.83	0.83	0.83
Treatment SD	0.99	0.99	0.99	0.99	0.99
Instrument Mean	1.37	1.37	1.37	1.37	1.37
Instrument SD	1.33	1.33	1.33	1.33	1.33

Notes: The Table is based on specification (4.3) and it includes firm and year fixed-effects. Standard errors in parentheses are clustered at the firm level. Column (1) shows the effect of the reform on total layoffs, column (2) to (5) 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 *potential retirees* employed at the firm when the reform is implemented. A *potential retiree* is retained if her observed retirement date falls one or more year after her pre-reform full retirement date. The number of *potential retirees* is instrumented with the firm-level average change in the full retirement date defined in equation (4.2). The coefficients refer to years from $t-3$ to $t+3$, where the first is calendar year 2009 and the latter 2015. The effect on year $t-1$ (the reform year, *i.e.* 2011) is omitted, as it is set equal to 0 in the estimation. The regressions are run on the universe of private sector firms that (i) were active every year in the period 2009-2015, (ii) employed between 3 and 200 employees in q1-2009, (iii) employed at least one *potential retiree* in q4-2011.

*** p < 0.01; ** p < 0.5; * p < 0.1.

TABLE A8. The effect of the reform on hiring

	All	Young	Middle-aged	Old	Permanent Contract	Temporary Contract
	(1)	(2)	(3)	(4)	(5)	(6)
$t-3$	0.049	0.061	-0.0081	-0.0033	0.014	0.035
	(0.15)	(0.080)	(0.081)	(0.019)	(0.083)	(0.12)
$t-2$	-0.064	-0.016	-0.041	-0.0070	0.016	-0.080
	(0.13)	(0.067)	(0.071)	(0.019)	(0.070)	(0.11)
t	-0.34***	-0.14*	-0.15**	-0.047***	-0.036	-0.30***
	(0.12)	(0.073)	(0.062)	(0.017)	(0.065)	(0.10)
$t+1$	-0.49***	-0.26***	-0.21**	-0.022	-0.13	-0.37**
	(0.19)	(0.094)	(0.10)	(0.026)	(0.10)	(0.15)
$t+2$	-0.46***	-0.23**	-0.23***	-0.00016	-0.058	-0.40***
	(0.17)	(0.099)	(0.082)	(0.022)	(0.071)	(0.15)
$t+3$	-0.12	-0.100	-0.064	0.046	0.22*	-0.33*
	(0.23)	(0.12)	(0.11)	(0.033)	(0.11)	(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
KP F-Statistics	1,214.09	1,214.09	1,214.09	1,214.09	1,214.09	1,214.09
Treatment Mean	0.83	0.83	0.83	0.83	0.83	0.83
Treatment SD	0.99	0.99	0.99	0.99	0.99	0.99
Instrument Mean	1.37	1.37	1.37	1.37	1.37	1.37
Instrument SD	1.33	1.33	1.33	1.33	1.33	1.33

Notes: The Table is based on specification (4.3) and it includes firm and year fixed-effects. Standard errors in parentheses are clustered at the firm level. Column (1) shows the effect on total new hires, column (2) to (4) 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 *potential retirees* employed at the firm when the reform is implemented. A *potential retiree* is retained if her observed retirement date falls one or more year after her pre-reform full retirement date. The number of *potential retirees* is instrumented with the firm-level average change in the full retirement date defined in equation (4.2). The coefficients refer to years from $t-3$ to $t+3$, where the first is calendar year 2009 and the latter 2015. The effect on year $t-1$ (the reform year, *i.e.* 2011) is omitted, as it is set equal to 0 in the estimation. The regressions are run on the universe of private sector firms that (i) were active every year in the period 2009-2015, (ii) employed between 3 and 200 employees in q1-2009, (iii) employed at least one *potential retiree* in q4-2011.

*** p < 0.01; ** p < 0.5; * p < 0.1.

TABLE A9. Firms only respond to shock to *potential retirees*

	New Hires	Layoffs
Retained Sample 3 X Post	-0.493**	0.144***
	(0.249)	(0.041)
Retained Sample 4-5 X Post	-0.053	-0.001
	(0.112)	(0.017)
Observations	189,861	189,861
Mean Outcome (pre 2012)	7.07	0.47
KP F-Statistics	1304.05	1304.05
Mean # Retained (Sample 3)	1.27	1.27
SD # Retained (Sample 3)	1.44	1.44
Mean # Retained (Sample 4-5)	2.88	2.88
SD # Retained (Sample 4-5)	3.53	3.53
Treatment Mean (Sample 3)	1.37	1.37
Treatment SD (Sample 3)	1.21	1.21
Treatment Mean (Sample 4-5)	2.94	2.94
Treatment SD (Sample 4-5)	2.13	2.13
P-Value Difference Coefficients	0.084	0.003

Notes: The table reports the results of the specification in (4.6) on new hires and layoffs when two treatments are included. Standard errors in parentheses are clustered at the firm level. The regression includes two treatments. The first treatment is defined as the number of retained *potential retirees* who - under pre-reform rules - were expected to retire within three years when the reform is passed. The second treatment is defined as the number of retained workers who - under pre-reform rules - were expected to retire in four to five years when the reform is passed. A worker is retained if she retires more than one year after the pre-reform predicted retirement age. The two treatments are instrumented with the average shift in the retirement date in the respective sample of workers. The Table also report the p-value of a test for the difference between the two reported coefficients. The regressions are run on the universe of private sector firms that (i) were active every year in the period 2009-2015, (ii) employed between 3 and 200 employees in q1-2009 and (iii) in q4-2011 employed at least one worker expected to retire by 2014 and at least one workers expected to retire in 2015 or 2016 under pre-reform rules.

*** $p < 0.01$; ** $p < 0.5$; * $p < 0.1$.

TABLE A10. The effect of the reform on total labor earnings

	All	Young	Middle-aged	Older
	(1)	(2)	(3)	(4)
$t-3$	-1463.3	-473.8	1901.8	60.1
	(2299.5)	(781.7)	(1442.8)	(283.5)
$t-2$	-334.4	86.6	1357.0*	-85.7
	(1279.9)	(415.0)	(753.9)	(224.4)
t	-3582.4***	-627.6*	-1564.0*	-856.5***
	(1347.6)	(343.0)	(877.3)	(269.1)
$t+1$	-5934.4***	-1002.2*	-2763.1*	-1574.5***
	(2215.3)	(533.5)	(1459.6)	(410.6)
$t+2$	-10546.6***	-1682.5**	-5597.6***	-2347.4***
	(2715.7)	(665.9)	(1820.0)	(519.1)
$t+3$	-13379.4***	-2066.5***	-7941.9***	-2912.7***
	(3102.3)	(775.3)	(2096.4)	(632.1)
N. obs.	511,847	511,847	511,847	511,847
Mean Outcome (pre 2012)	621,032.16	130,288.84	412,155.77	44,559.9
Treatment Mean	1.35	1.35	1.35	1.35
Treatment SD	1.33	1.33	1.33	1.33

Notes: The Table is based on specification (4.5) and it includes firm and year fixed-effects. Standard errors in parentheses are clustered at the firm level. Column (1) shows the effect of the reform on total labor earnings, column (2) to (4) the effect on labor earnings of young (below 35), middle-aged (35-55), older (over 55) non-*potential retirees*, respectively. The treatment is defined as the firm-level average change in the full retirement date of *potential retirees* defined in equation (4.2). The coefficients refer to years from $t-3$ to $t+3$, where the first is calendar year 2009 and the latter 2015. The effect on year $t-1$ (the reform year, *i.e.* 2011) is omitted, as it is set equal to 0 in the estimation. The regressions are run on the universe of private sector firms that (i) were active every year in the period 2009-2015, (ii) employed between 3 and 200 employees in q1-2009, (iii) employed at least one *potential retiree* in q4-2011.

*** $p < 0.01$; ** $p < 0.5$; * $p < 0.1$.

TABLE A11. The effect of the reform on total labor earnings (w/ non-work subsidies)

	All	Young	Middle-aged	Older
	(1)	(2)	(3)	(4)
$t-3$	-1495.0	-2446.6**	1012.9	-61.3
	(2297.9)	(1002.9)	(1481.2)	(290.5)
$t-2$	-366.0	-1131.4**	875.5	-110.2
	(1277.8)	(564.5)	(818.2)	(231.6)
t	-2910.3**	-273.1	-1783.6**	-853.6***
	(1316.8)	(441.7)	(895.8)	(277.2)
$t+1$	-3775.4*	-302.8	-2130.2	-1342.4***
	(2022.9)	(687.3)	(1372.8)	(415.2)
$t+2$	-6678.4***	-706.4	-4101.5***	-1870.5***
	(2408.4)	(902.2)	(1582.8)	(509.4)
$t+3$	-8566.2***	-653.7	-5686.2***	-2226.3***
	(2817.0)	(1091.2)	(1852.4)	(623.0)
N. obs.	511,847	511,847	511,847	511,847
Mean Outcome (pre 2012)	621,137.28	144,154.91	429,643.97	47,338.4
Treatment Mean	1.35	1.35	1.35	1.35
Treatment SD	1.33	1.33	1.33	1.33

Notes: The Table is based on specification (4.5) and it includes firm and year fixed-effects. Standard errors in parentheses are clustered at the firm level. Column (1) shows the effect of the reform on total labor earnings including non-work subsidies, column (2) to (4) the effect on the same outcome for young (below 35), middle-aged (35-55), older (over 55) non-*potential retirees*, respectively. The treatment is defined as the firm-level average change in the full retirement date of *potential retirees* defined in equation (4.2). The coefficients refer to years from $t-3$ to $t+3$, where the first is calendar year 2009 and the latter 2015. The effect on year $t-1$ (the reform year, *i.e.* 2011) is omitted, as it is set equal to 0 in the estimation. The regressions are run on the universe of private sector firms that (i) were active every year in the period 2009-2015, (ii) employed between 3 and 200 employees in q1-2009, (iii) employed at least one *potential retiree* in q4-2011.
*** p < 0.01; ** p < 0.5; * p < 0.1.

TABLE A12. The effect of the reform on *potential retirees* and co-workers

	Potential retirees	Co-workers
Labor earnings	18512.150***	-24967.670***
	(996.510)	(6440.490)
Pension entitlements	-18194.5***	713.090
	(512.820)	(578.690)
Disability benefits	212.900***	122.330
	(24.920)	(150.470)
Short-time work subsidies	260.18	-4408.482*
	(189.92)	(2494.71)
Non-work Subsidies	1393.060***	8594.920***
	(122.800)	(2171.800)
Sick and leave benefits	513.440***	-320.440
	(45.550)	(379.010)
Early retirement (months)	2.030***	0.930***
	(0.140)	(0.270)

Notes: The table reports the sum of coefficients $\{\beta_k^T\}_{k=2012}^{k=2015}$ from the specification in (4.5) where we use the standardized instrument in reduced-form to capture the effect of a 1σ (1.33 years). 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. All specifications include firm fixed effects and year fixed effects. The treatment is the average shift in the retirement date of *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 under the pre-reform rules when the reform is implemented. Observations are weighted according to firm size at baseline. The regression is run on the universe of private sector firms that (i) were active every year in the period 2009-2011, (ii) employed between 3 and 200 employees in q1-2009, (iii) employed at least one *potential retiree* in q4-2011.

*** $p < 0.01$; ** $p < 0.5$; * $p < 0.1$.