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The labor substitutability and the impact of raising the retirement age

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Labor substitutability and the impact of raising the retirement age

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LA SOSTITUIBILITÀ TRA LAVORATORI E L'EFFETTO DI AUMENTARE L'ETÀ PENSIONABILE

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31 Ottobre 2018

SOMMARIO. Il livello di sostituibilità tra diverse tipologie di lavoratori può avere un effetto sull'incidenza delle politiche pubbliche, creando *spillover* nella domanda di lavoro all'interno delle imprese. In questo articolo studiamo la sostituibilità tra lavoratori appartenenti a diverse coorti di età, nel contesto di un'inaspettata riforma del sistema pensionistico pubblico che ha innalzato l'età pensionabile in Italia a partire dal 2012. Un aumento dell'età pensionabile crea incentivi per i lavoratori più anziani a posticipare il pensionamento e, quindi, ne aumenta temporaneamente il trattenimento presso le imprese che li impiegano. In questo lavoro esploriamo gli effetti del trattenimento di un lavoratore anziano sulla domanda di lavoro per gli altri lavoratori nelle imprese medio-piccole. Utilizzando le informazioni contenute in un ricco dataset longitudinale di tipo *matched employer-employee* per l'Italia, sfruttiamo la sostanziale variabilità dell'impatto dell'inattesa riforma pensionistica a livello di impresa, che permane anche dopo aver controllato per differenze nella composizione demografica della forza lavoro aziendale. Le nostre analisi suggeriscono che i lavoratori prossimi al pensionamento e i colleghi più giovani sono sostituti all'interno dell'impresa. Al trattenimento di un lavoratore anziano si associano, in media, 0,16 licenziamenti in più e 0,29 assunzioni in meno. I lavoratori più maturi inquadrati nella stessa occupazione sono i sostituti più prossimi dei lavoratori trattenuti che posticipano il pensionamento. Innalzare l'età pensionabile riduce anche i redditi da lavoro e aumenta il ricorso ad altri programmi di protezione sociale da parte dei colleghi dei lavoratori trattenuti. La sostituibilità tra diverse tipologie di lavoratori ha implicazioni importanti per l'incidenza delle politiche pubbliche. Ad esempio, nel breve periodo il costo fiscale della riforma, nonostante sia minore dei benefici, è più elevato di quanto si sarebbe potuto anticipare. Le nostre analisi suggeriscono che gli *spillover* nella domanda di lavoro spiegano quasi tutti i costi fiscali di breve periodo della riforma nel primo quadriennio successivo alla sua implementazione.

Parole chiave: riforme, sistema pensionistico, imprese, domanda di lavoro, esternalità fiscale

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LABOR SUBSTITUTABILITY AND THE IMPACT OF RAISING THE RETIREMENT AGE

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ABSTRACT. The degree of substitutability among different types of workers can affect the incidence of public policies by creating labor demand spillovers within firms. We study the substitutability between age cohorts in the context of an unexpected public pension reform increasing the full retirement age in Italy starting in 2012. An increase of the full retirement age creates incentives for older workers to delay retirement and temporarily increases their retention at the employer firm. We investigate how retaining an additional old worker affects employer labor demand for other workers in small and medium-sized firms. Using a large and rich matched employer-employee administrative data set for Italy, we exploit the substantial idiosyncratic firm-level variation in the impact of the unanticipated pension reform even after conditioning on the broad demographics of firms' work forces. We find that workers on the cusp of retirement and younger co-workers are substitutes. When a workers is retained firms fire on average 0.16 workers more and decrease hiring by 0.29 units. Older workers in the same occupation group are the closest substitute to the retained older workers who delay retirement. Extending the retirement age also decreases labor earnings and increases the take-up of social security programs of incumbent workers. Patterns of labor-labor substitutability have important implications for the incidence of public policies like this reform. For example, in the short run the fiscal cost of the reform, although smaller than the benefits, is higher than may have been anticipated due to the delayed retirement of incumbent workers. Our findings indicate that the labor demand spillovers explain almost all the short-run fiscal cost of the policy in the first four years after implementation.

Key words: reforms, pension system, firms, labour demand, fiscal externalities

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1. Introduction

Labor substitutability within firms matters for the consequences of public policies. When some workers are targeted by a policy, spillovers on their co-workers can arise creating "hidden" costs that have welfare consequences and are hard to detect when focusing on targeted individuals only. The costs and benefits of a policy are redistributed at the firm-level depending on whether firms find close substitutes or complements among incumbent employees. An example is substitutability between age cohorts that affects the incidence of social security policies.

We study the short-term impact of increasing the full retirement age on workers and firms to uncover the substitutability between workers of different ages.[1] When the retirement age is extended, old workers on the cusp of retirement face lower incentives to retire and work for longer than expected. When older workers do not retire, do firms fire young workers, or rather closer in age middle-aged workers? Do they hire less? Do they adjust the wages of incumbent employees? A rich literature studies the individual labor supply effects of increases in the retirement age (Mastrobuoni 2009; Staubli and Zweimüller 2013; Vestad 2013).[2] However, the evidence on the demand-driven labor market effects of these policies is limited. Adjustments in labor demand shed light on the subsitutability between workers of different age cohorts. The substitutability has important implications for the unintended consequences of these types of reforms, and potentially valuable complementary policies in the labor market.

This paper investigates how firms respond to an increase in the retention of older workers caused by a higher statutory retirement age and how the associated adjustments in labor demand spill over to other workers in the firm. We link Italian matched employer-employee records for small and medium firms to new and previously unused data sources (*estratti conto*), which contain information on the working histories of more than 6 millions of workers (about one third of total private employees in a given year). We then exploit the quasi-experimental variation of a unique pension reform implemented in 2011 that caused sudden and substantial changes in the full retirement age.

In the first part of the paper, we ask whether and how employers change their demand for

[1]Rapid population aging is threatening the long-term sustainability of the social security system in most OECD countries. Many governments have passed reforms to increase the retirement age to cope with it. 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 raising the statutory age to 70 to help reduce the budget deficit between 2017 and 2026 (CBO, 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 such policies. Other papers on the effects of these reforms on retirement behavior are Behaghel and Blau (2012), Cribb et al. (2016), Manoli and Weber (2016), Seibold (2016), Lalive et al. (2017).

labor. We link matched employer-employee records to a firm-level measure of the reform-induced shift in the retirement date that we compute using contribution histories. Our results reveal the degree of substitutability between workers of different ages. We then use detailed information on working histories to show that adjustments in labor demand affect the co-workers of retained older employees, impacting their earnings trajectories and take-up of social insurance programs. We conclude by discussing the implications of labor substitutability for the short-run impact on government finances of this particular policy, and also more generally for the impact of similar government policies. We incorporate the demand-driven behavioral responses into the estimation of the fiscal externality of the reform (the share of mechanical savings in pension outlays that is lost due to behavioral responses), improving upon the existing literature that only focuses on the behavioral responses of older workers.

Addressing our research questions poses two main identification challenges. First, most pension reforms are anticipated. Confounding anticipation effects make it hard to isolate firm's responses. Second, the extent to which a firm is affected in the short-run by a higher statutory retirement age depends on its share of old workers. Firms with a high concentration of younger workers cannot serve as credible controls since firm's demographics reflect pre-existing firm-level labor demand trends.

We address both identification issues by exploiting the unanticipated *Fornero* pension reform. It was enacted in Italy by a newly appointed technocratic government in December 2011 and it entered into effect in January 2012, leaving little room for anticipatory effects. The new law raised the age and contribution requirements for old-age and seniority pensions. To study workers substitutability, we want to measure the causal effect of retaining an extra worker close to retirement on a firm's labor demand margins. The number of older employees retained per effect of the policy depends on a firm's age distribution, which varies across firms because of labor demand trends and other unobservable characteristics. We solve this endogeneity by instrumenting the number of retained older employees with a firm-level idiosyncratic shift in the full retirement age. We construct this instrument by exploiting the reform-designed heterogeneous changes in years until pension eligibility across otherwise similar older workers. These changes depend on a worker's age, gender and years of retirement contributions. We compute each employee's full retirement date under pre- and post-reform rules, restricting attention to the workers directly affected by the policy in the short-run who are on the cusp of retirement when the reform was passed (i.e. who would have been eligible to retire by 2014 under old rules). Because of the design of the policy, employers face heterogeneous changes in the average expected retirement date of such older workers, depending on idiosyncratic differences in age, gender and contribution histories within this narrow set of employees. We use this change as our instrument. We show that its intensity

is not related to broad firm's demographics or to other observable firm characteristics in the data. Most importantly, the instrument does not predict differences in labor demand pre-trends. It has also a direct economic interpretation as the firm-level change in the full retirement date of older workers. For this reason, in the second part of the paper we use it to study the effect of raising the full retirement age on public finances in the short-run.

To investigate the short-run effects of the reform, we estimate a dynamic difference-in-difference model with a continuous treatment over the period 2009-2015, comparing the labor demand of differentially treated firms before and after the reform. We look at labor demand adjustments along two main margins: layoffs of incumbent workers and external hiring. Our results document that older retained employees and younger co-workers are substitutable. In particular, older workers are fired more than young employees, indicating a closer substitutability with old workers retiring. More strongly treated firms fire more permanent employees in the post-reform period. Specifically, the IV estimates suggest that retaining for at least one year an additional worker is associated to 0.16 more layoffs on average in the four years after the reform (2012-2015) - a 37% increase if compared with the average number of layoffs in the pre-reform period. As mentioned above, layoffs do not involve only older employees who were expected to retire soon. Young (under 35), middle-aged (aged 35-55) and other old (aged above 55) workers are also fired, causing spillovers within the firm. Hiring is reduced by 0.29 units (5.5% of the pre-reform average). Its decline is largely explained by drops in new hires of temporary 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 substitutability of older workers is larger with older employees in the same occupation group. We show that in the short-run firms mostly respond to the shift in the retirement date of older workers on the cusp of retirement. The change in the residual working life of younger employees is close to zero. As a consequence, the post-reform dynamics of dismissals and hiring exhibit an inverse U-shape. Effects fade away as older workers eventually leave the firm.

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 the take-up of social insurance programs. We associate each worker to the firm where she worked at the reform date and we aggregate the outcomes of interest for co-workers who shared incumbency at the same employer. We find that incumbent co-workers in more heavily treated firms exhibit worse labor earnings dynamics in the post-reform period. A 1.37 years shift of the full retirement date (one standard deviation) leads to a 11,395 euros drop in total labor earnings (i.e. including earnings from other private employers, as well as public employment or

self-employment). It is equivalent to 1.8% of the firm-level average total earnings in the pre-reform period. The decline in earnings is moderated when we take into account non-work subsidies. Hence, part of the observed dynamics reflects the effect of the increased layoff risk. To gauge which share of the earnings decline can be attributed to involuntary separations, we combine estimates of the cost of job losses obtained via a matching procedure with the estimates of the effect of the reform on separations. We find that separations explain around half of the earnings drop. The other half depends on within-firm earnings dynamics, which matter more for middle-aged workers. 70% of their earnings loss is explained by wage patterns within the firm. Only 40% (10% 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 workers are the closest substitute to retained older workers.

We show that total transfers to all incumbent workers increase after the reform. Non-work subsidies explain large part of this increase and are caused by layoffs. Older workers are more likely to receive disability pensions and sick leave benefits after the reform, while their co-workers experience mild increases in the take-up of these programs. Despite the increase in total outlays, savings on older workers' pension entitlements are far larger than the costs generated by their increased take-up of other social insurance programs.

Finally, 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. We estimate the fiscal externality of the policy to assess its cost. 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. Savings on pension outlays are larger than costs from extra outlays on social insurance programs and lower labor tax revenues. However, we find that one-half to 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 of older employees retiring. 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 conclude that labor substitutability is pivotal to assess the consequences of this reform and other policies that lower the incentives of older workers to leave their firm. It is important to stress, however, that due to data limitations we cannot incorporate in our computation of the fiscal externality neither the short-run effects on firms' performance nor the effects on workers on cusp of retirement who are not in employment at the time of the reform. Furthermore, our estimates only apply to senior employees and co-workers employed in small and medium-sized firms.

Our paper relates to the literature that explores firm responses to unforeseen shocks to their workforce. Jäger (2016) and Jaravel et al. (2017) exploit sudden workers' deaths to investigate workers substitutability.[3] While these papers leverage a negative shock to the retention rate, we study a positive one. Unlike a worker's death, our treatment may involve more than one incumbent worker, providing a larger shock to a firm's workforce. Since our shock affects older employees, it allows to study substitutabilities between workers of different ages. We relate our findings to labor demand theory and contribute to the understanding of labor substitutability within firms as studied in models with heterogeneous labor and imperfect labor markets (Cahuc et al., 2008 and Pissarides, 2000). We also add evidence on changes in internal labor market dynamics (Baker et al., 1994).[4]

Many studies investigate how the generosity of one social insurance program affects enrollment in other programs.[5] 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 including in our model the demand-driven spillovers on incumbent workers who are not affected by the policy in the short-term.

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.[6] 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.[7] Several papers have studied the relationship between the employment rates of older and younger cohorts. 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. Martins et al. (2009) study a Portuguese pension reform, while Boeri et al. (2017) evaluate the Italian *Fornero* pension reform. Both papers detect a negative effect of pension reforms on new hires. Their identification relies on the strong assumption that firms with different general demographics and gender compositions have parallel labor demand trends. Our new contribution is twofold. First, we rely on idiosyncratic variation in

[3] Other recent examples are Nguyen and Nielsen (2010), Bennedsen et al. (2010) and Adam (2015).
[4] Gibbons and Waldman (1999), Lazear and Oyer (2013), and Waldman (2013) survey the theoretical literature on internal labor markets.
[5] Some examples are works on the spillovers of changes in the disability insurance (Autor and Duggan (2003); Karlstrom et al. (2008); Borghans et al. (2010); Staubli (2011)) or unemployment insurance (Lammers et al. (2013)). Recent works along these lines are Inderbitzin et al. (2016) and Kline and Walters (2016). A similar work on early retirement provisions and the spillovers on other government programs is Vestad (2013).
[6] See Coile and Gruber (2007) for a review.
[7] 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.

treatment intensity unrelated to broad firm demographics. Second, we extend the scope of the analysis to multiple labor demand margins and social insurance programs, documenting the spillovers of the policy on all incumbent workers.

Firm-level studies are complemented by macro evidence on the relationship between young and old employment. Gruber and Wise (2010) conclude, based on country case-studies, that the correlation is positive. 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 raising the retirement age have negative effects on youth employment. Exploiting variation in the age structure of the old population across U.S. commuting zones, Mohen (2017) similarly finds that the retirement slowdown has increased youth unemployment.

The remainder of the paper is organized as follows: Section 2 illustrates the institutional setting; Section 3 describes the data; Section 4 outlines the identification strategy; Section 5 shows that old workers delay retirement in response to the policy; Section 6 discusses the main findings on firms' 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 fiscal externality 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.[8] A combination of defined-benefits (DB) and notional defined-contributions (NDC) methods determines pension benefits.[9] The NDC scheme applies to contributions accrued from 1996 onward, except for workers who accrued more than 18 years of contributions by December 1995. For these workers, the NDC method applies to contributions accumulated from 2012 onward. The private-sector social security tax rate is 33%. Around one-third is paid by the employee

[8]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). There exist closed pension funds for companies and open pension funds offered by banks and insurance companies. The almost entirety of funds operate on a DC basis.

[9]Under the DB regime, benefits are computed according to earning-based formula $b = \rho N \hat{w}_R$, where ρ is the accrual rate, N are years of contributions and $\hat{w}_R$ is the average salary earned during the last R years of a worker's career. Under the NDC scheme social security contributions accrue into a notional account, where they are capitalized based on a 5-year moving average of the nominal GDP growth rate. They are transformed into yearly benefits using a transformation coefficient that depends on age at retirement and life expectancy.

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 *Fornero* reform raised requisites to claim both types of pensions (see section 2.3). 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 before the statutory age.[10] Retiring early comes at the cost of receiving sizably lower pension benefits.[11] The average cut is estimated to be roughly 35% 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% 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 where 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

[10] Early retirement using *opzione donna* was possible in 2011 upon turning 57 years old, conditionally on having 35 years of contributions. The age requirement was later adjusted to 57 years and 3 months, and then to 57 years and 6 months.

[11] The main reason is that pension benefits for early retirement are computed applying the NDC regime also to contributions accrued before 1996.

[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.

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 2012-2018, showing age and contribution requirements for the old-age (Panel A) and seniority (Panel B) options.[14] Figure 1 shows the shifts in the retirement age for every combination of age, gender and years of contributions for the workers who were expected to retire by 2014 under the old pension rules.

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 women and 65 for men in 2011. Absent the reform, it would have risen to reach 61 years and 10 months for women and 65 years and 7 months for men. 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. The change in the age requirement was thus considerably larger for women than for men.[15] However, due to gradual adjustments to the target of 66 years and 7 months not all women faced the same 6 years change. Some faced smaller extensions depending on their age in 2011.

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 certain "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. Had rules not changed, the "quota" would

[13] Figures A1 and A2 plot average age and retirement volumes by gender and retirement option.

[14] Table 1 reports old and new requirements as they appear in the law. It takes into account the anticipated upward adjustments due to increased life expectancy that took place in 2013 and 2016.

[15] Women who were at least 60 years old in 2012 and had at least 20 years of contributions could exceptionally retire upon turning 64 years old in 2012, 64 year and 3 months old in 2013-2015 and 64 years and 7 months old from 2016 onward. Moreover, following the reform, individuals who start working in 1996 or later can also 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. stemming from work-related activities) contributions. This option was also available under pre-reform rules, but the age requirement was the same as the one for the standard old-age pensions.

have risen from 96 to 97.3 and later to 97.6 over the 2012-2108 period.[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.[17] 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% of eligible women claimed early retirement. Moreover, only 80% of them made job-to-retirement transitions. The remaining 20% were unemployed or out of the labor force when they retired (INPS, 2016). 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 generated different changes in years until retirement eligibility among otherwise similar older workers. Firms with a similar older workforce can therefore be affected to a different extent by the reform. Figure 1 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 who were between 58 and 59 years old and had less than 36 years of contributions in 2011. They were close to retire using the "quota 96" option. When the reform becomes effective their earliest available retirement option becomes either "anticipated" pension - that required 41 years of contributions - or old-age pension - that required at least 62 years of age. As a consequence, their retirement age shifts by three years or more. Smaller changes affect women with more than 37 years of contributions or closer to 60 years old. Among male workers, those who experienced the largest change in the retirement age were close to eligibility under "quota 96". After the reform, they must claim an old-age pension or an "anticipated" pension. Both options implied a significant shift in the retirement age. Milder changes affect male workers who were under 60 years old with 38 or more years of contributions. 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". Changes below one year affect male workers who were over 63 and aimed to retire under old-age pension.

[16]The age requirement associated to "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. The contribution requirement would have been maintained at 35 years.

[17]Workers who would have reached "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. Following the reform, individuals who started working in 1996 or after have the option to retire 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.

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% of private employment in Italy and 93% of private sector employees.[18] We use monthly data for the period 2009-2015.[19] 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. Given our focus on small and middle-sized firms, single-establishment firms are the vast majority.

For each worker-firm record, the following information is available with a monthly frequency: beginning and end date of the contract, if it is signed or terminated in that month, 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 in every month. 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. We define total new hires and layoffs of permanent workers. We also construct labor demand 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 a small-medium sized firm around the reform date (i.e. between 2009 and 2015).[20] We observe every contribution spell within any given year for more than 6 millions of workers. The recorded information includes: the number of qualifying weeks contributed, needed to compute the contribution requirement for old-age and seniority pensions; the event triggering the payment of contributions (e.g. paid work,

[18]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.

[19]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.

[20]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.

maternity leave, sick leave, unemployment benefits) and the monetary value of the contribution.[21] Exploiting this information, we construct comprehensive measures of earnings, including labor income from quasi-salaried employment, self-employment and public sector jobs. We also observe the take-up of social insurance programs, comprehensive of unemployment subsidies.

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 fund that disburses the pension.

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 on 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 the demographics of the firm. Variation in this variable likely reflects differences in the way of doing business or different labor demand shocks. 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 firm-level shift 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 henceforth 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 dynamic difference-in-difference estimation setting (sub-section 4.3).

4.1. Individual shift in the full retirement date.

To compute the predicted retirement date an old 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. 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. We compute

[21] In Appendix E, we detail the assumptions and the method used to compute total contributions as of December 2011 starting from the raw data.

the predicted retirement dates according to old and new rules. We denote the difference with δ_θ:

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

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.[22] Data shows that for workers aged 60 or above in 2012 the median annual contribution is 52 weeks and the average is 45 weeks, 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.[23] As discussed in section 1, 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 can compare actual and predicted retirement dates. Panel A of Figure 2 shows a "forecast quality" assessment. The majority of the differences (63%) 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 distribution is left-skewed due to a positive mass between -1 and -3 years. This likely arises because of two main reasons. First, pathways to early retirement are still available. Panel B of Figure 2 shows that the difference between actual and predicted retirement dates is smaller for males than for females, consistently with a more extensive use of *opzione donna*. Second, some workers maintain the right to retire under pre-reform rules (see Appendix C).

[22] Alternatively, we require that non-work periods are covered by *figurative* contributions (see Appendix E)
[23] The reform abolished the "waiting window", a rule whereby the first pension installment could be collected only 12 months after becoming eligible for either type of pension. Due to this, under old rules 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. For example, if a worker becomes first eligible for either pension in January 2013 under old rules, we assume that she would have retired on January 2014, when collecting the first installment.

4.2. **Firm-level shift in the full retirement date.** We are interested in short-run firm labor demand adjustments. Thus, we focus on the change in the retirement date of older workers who were 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). We show later that the retention of older workers farther away from retiring does not have significant effects for short-run firm responses. The three-years threshold 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.[24] Figure 3 Panel A plots the distribution of the change in years until retirement for *potential retirees*. It displays a sizable variability, with mean 1.38 and standard deviation 1.42.[25]

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 at the firm. We have:

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

where $\pi_{\theta,i}$ 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 later that they are not correlated with the broad demographics of the firm. The treatment 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 1.40 and standard deviation is 1.37 (Figure 3 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. 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.**

[24] Workers who would have retired in 2012 under old rules face no change in the retirement date because of the "waiting window" (see Section 4.1). Thus, we exploit the variation in retirement dates coming from workers who would have retired in 2013 and 2014.

[25] 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. Moreover, due to the abolition of the waiting window few workers face a negative change, i.e. can retire sooner under new rules.

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-difference model with a continuous treatment and multiple pre- and post-reform periods. The baseline regression specification reads:

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

where 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 different treatment levels. Standard errors are clustered at the firm level to address the potential concern of serial correlation across periods (Bertrand et al., 2004).[26] The coefficients of interest are $\{\beta_k^R\}_{k=2009}^{k=2015}$, which show how the treatment affects firms in year k relative to the reform year.[27] R_i measures the number of retained *potential retirees* in firm i, which we interact with year fixed-effects. We call a *potential retiree* retained if she retires one or more years after her pre-reform full retirement date. The total 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 retirement date and the pre-reform expected retirement date. R_i is strongly correlated with the size and the age structure of the firm. 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 T_i, exploiting the fact that *potential retirees* delay retirement per effect of the reform.

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

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

We use T_i since it measures the firm-level shift in the full retirement date. Hence, we exploit variation in the policy parameter that has been changed by the reform instead of using the increase in the number of retained employees.

To summarize the results, we run standard difference-in-differences regressions comparing

[26] We also run specifications where we cluster at the province × two-digit sector level and results are virtually unchanged.

[27] We impose $\beta_{2011}^R = 0$.

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 control for firm and year fixed-effects, but we interact the treatment with the dummy $Post_t$ that equals 1 in years 2012-2015. The coefficient β^T captures the effect of the treatment on the difference in the outcome between pre- and post-reform periods.

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. We focus on firms that employ at least one *potential retiree* in the quarter when the reform is passed (q4-2011) for internal validity. Firms with no *potential retirees* may not be an appropriate control group. Indeed, they feature a different demographic composition and are likely to differ along other time-varying characteristics that we do not observe and cannot control for. As a robustness test, we nonetheless show that results are confirmed when we employ the universe of firms in the 3-200 size class.

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 et al., 2017 and Borusyak et al., 2018). In other words, the firm-specific characteristics of the workforce of *potential retirees* must not correlate with firm's time-varying unobservable characteristics. 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 demographic composition. We only exploit idiosyncratic differences in gender, age and years of contributions (i.e. in types θ) in the sub-sample of *potential retirees*. To make an example, let's consider two firms - A and B - that both employ some *potential retiree*. These employees are similar in the sense that they are all on the cusp of retirement at the time of the reform. We make the assumption that the fact that firm A faces a larger shift of the expected retirement date of *potential retirees* than firm B, because it happens to have, among its set of *potential retirees*, a larger share of individuals who are subject to a larger change in their residual working lives, is idiosyncratic and does not reflect a systematic choice correlated with other unobserved traits of the firm. Evidence that the composition 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 employees because they hire less or because they fired more young workers in the pre-period. Pre-trends as captured by the coefficients $\{\beta_k^T\}_{k=2009}^{k=2011}$ can provide suggestive evidence of the exogeneity

of our treatment. If trends are parallel, these coefficients should not be significantly different from 0. We perform placebo tests and balancing tests to assess the validity of the identifying assumptions.

4.3.3. *Placebo tests.* We assess whether the treatment 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 treatment on layoffs and new-hires, which are the main firm-level outcomes we study in Section 6. Table 2 shows that the treatment has 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* correlates with hiring and firing decisions in the years preceding the reform.

4.3.4. *Balancing tests.* The distribution of age and years of contribution among *potential retirees* is not necessarily informative about the distribution of these variables in the firm's workforce. On the other hand, the share of females among *potential retirees* more likely reflects the gender composition of the firm. Women are on average more affected by the reform than men (see sub-section 2.3). For this reason, the treatment could be systematically higher in firms with more female workers. We run a balancing test whereby we regress a rich set of firm's baseline characteristics on the treatment. Table 3 reports the results. We run regressions without controls (Column 1) and controlling for province fixed effects, sector fixed effects, province × sector fixed effects (Column 2). 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, the share of male workers decreases by 0.01 against an average of 0.658. The shares of old, middle-aged and young workers display correlations of 0.009, -0.01 and -0.012 against averages of 0.123, 0.579 and 0.658. Importantly, T_i has also a close to zero correlation with the share of potential retirees. We show in Section 7 that results do not change when controlling for these covariates.

4.4. **Sample and Descriptive Statistics.** We study the effect of the reform on firm's labor demand on a balanced panel of 64,721 firms in the 3-200 size class in the baseline quarter (q1-2009). We require that firms employ at least one *potential retiree* at the reform date (q4-2011) and remain active throughout the period.

Table 4 compares the characteristics of firms in our main sample to other firms in the same

size class that remain active throughout the period. Firms with at least one *potential retiree* are on average three times as large as other firms and older. They are also more concentrated in the manufacturing sector, thus having a higher share of blue-collars. As expected, they employ a older, more experienced and more tenured workforce. Consistently, their average gross daily wages are higher.

Table 5 compares *potential retirees* to other full-time workers employed in firms belonging to the master sample. As expected, *potential retirees* are older, more experienced and more tenured. They are more likely to hold blue-collar and managerial positions, to have a permanent contract and their gross daily wage is higher. We compare *potential retirees* to older employees who are not on the cusp of retirement, but who are similar along many dimensions, including age and gender.[28] Our evidence indicates that employees who approach the end of their working lives reduce their effort (Dostie, 2011; Borsch-Supan and Weiss, 2016 and Avolio et al., 1990). Table A1 shows that before the reform *potential retirees* are 5% more likely to be absent from work because of sickness and 1-2% more likely to be absent due to work-related injuries or sick leave, generating higher monetary costs associated to these events.

5. Old workers delay retirement after the reform

The reform may affect firm's labor demand if it actually translates into longer working lives for older workers. We investigate the response of retirement choices to the full retirement age by running an individual-level version of specification (4.5) on the sample of *potential retirees*. The treatment measures the worker-level shift in the full retirement date as designed by the reform (i.e. δ_θ in equation 4.1). We augment the specification by including the interaction of sex and age fixed-effects with time fixed effects.[29] Figure 4 shows that postponing the retirement date by one year causes a decline in the number of months spent on retirement (up to 2 months in 2015). 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 observed decline in months spent on retirement is smaller than the one that would occur if all workers retired at the post-reform retirement date (benchmark). The difference between observed retirement and the benchmark reflects the early retirement response 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 the

[28]We perform a coarsened exact matching procedure. The 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.

[29]Thus, the residual variation leverages mostly differences in contribution histories or in months of age across workers who are born in the same year.

individual-level shift in the full retirement date. The specification also include 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 6). The response is slightly larger for women (6.81 months against 6.54 for men). These numbers are very close to estimates in Mastrobuoni (2009) for the US.

6. Firm-level 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 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% confidence intervals. They capture the effect of an extra retained *potential retiree* in a given year k relative to the reform year.[30] We instrument the number of *potential retirees* retained with the firm-level shift in the full retirement date T_i. The first stage is reported in Table 7.

We focus on layoffs and new hires as margins of adjustment in labor demand. According to standard labor demand theory, a drop in demand for younger employees caused by the retention of old workers can be reconciled with complementarity between cohorts only in case of an increase in younger workers wage. 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, showing a drop in the earnings of younger cohorts. We conclude that younger cohorts are substitutes for old workers retiring. Our results document a larger response of layoffs for incumbent middle-aged and old non-*potential retirees* relative to young (under 35) employees. We conclude that the former two cohorts are closest substitutes to workers retiring. In particular, old workers (over 55) appear to be very close substitute to *potential retirees*. To grasp the intuition, in Appendix A we develop a simple labor demand model with different cohorts and some extensions to incorporate alternative wage bargaining models.

6.1. Layoffs.

Firms increase layoffs of permanent employees in response to increased retention of *potential retirees* (Figure 5, Panel A).[31] No significant difference in the layoffs of more and less treated firms is present before the reform (2009-2011). The difference emerges in aftermath of the reform. When a *potential retiree* is retained, the number of workers fired gradually rises over the period 2012-2014 up to 0.22 in 2014. It amounts to about 50% of the pre-reform average of layoffs per year (0.43). An inverse u-shaped pattern of coefficients reflects the dynamics in the retention of *potential retirees*. Most *potential retirees* would have

[30] The coefficients $\{\beta_k^R\}_{k=2009}^{2015}$ are also reported in Tables A2 and A3.

[31] We found no effect of the reform on firings of fixed-term workers. 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 worker is not to renew her contract at end date. Thus, because both pre- and post-reform firms have no incentive to fire employees under fixed-term contracts, we observe very few of such cases.

been working at the firm in 2012 - where we detect a small effect - under pre-reform rules. 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 because of the reform slows down. Table 7 summarizes the results reporting the estimated coefficient from the specification in (4.6). On average, firms fire 0.16 more workers for every extra *potential retiree* retained, which amounts to 37% of the pre-reform average number of layoffs. To study the substitutability between age cohorts, Panel B of Figure 5 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 "old" over 55 years old workers. Interestingly, layoffs increase across all age groups and are not concentrated on *potential retirees* only. Despite the fact that firing young workers is easier since they are paid lower salaries, layoffs 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 old workers. In 2015, 0.08 extra old workers are fired relative to 0.04 young workers, despite the average share of young workers in the firm is twice as large as the share of old employees (29.8% against 12.3%). Figure 5 Panel C plots the response of the number of fired old employees when excluding *potential retirees*. The coefficients halve, indicating that half of the effect is concentrated on *potential retirees*. The firm median share of *potential retirees* out of old workers is 65%. Thus, *potential retirees* are not disproportionately affected relative to other old employees. Since *potential retirees* and other old workers earn similar wages, the cost of firing the two types of workers is the same. We find that firms trade them off in a similar fashion, indicating a strong substitutability. Layoffs increase more for middle-aged relative to young workers. In 2015, the estimated effect is 0.09 for middle-aged and 0.04 for young workers, with the share of middle-aged workers being the double of the share of young employees. Hence, we find some evidence that middle-aged employees are closer substitute to older workers within the firm relative to young employees. This finding is consistent with the results on labor earnings that we present in section 8.1.

6.2. **New Hires.** Panel A of Figure 6 displays the effect of the reform on new hires. Firms more affected by the reform change their hiring schedule, without differential pre-trends. A retained worker causes an average drop in hiring of up to 0.29 units per year (Table 7). Against an average of 5.23 new hires per year, it amounts to a 5.5% reduction. Hiring recovers starting from 2015, when the coefficient is virtually zero. The u-shaped pattern of coefficients indicates that firms delay the hiring schedule 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 6 decomposes the effect by new hires' age. The drop is equally borne by young and middle-aged workers. We find little effect on old workers, largely because there are very few hiring events of over 55 employees. Table 7 summarizes the results

showing that new hires of young and middle-aged decline by 0.16 and 0.14 units per year respectively, accounting for most of the observed drop in hiring. Relative to the pre-reform averages, the drop is equal to 6.2% for young workers and 6.1% for middle-aged workers. New hires drop more for fixed-term contracts (Figure 6, 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.[32]

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.[33] 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 7). Table A4 shows that the effects of the two treatments are statistically different at the 1% level for layoffs. The coefficients are not statistically different for new hires, although point estimates go in the expected direction. 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 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 that 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 firm size, occupation and turnover.**

6.4.1. *Firm size:* We check how the estimated effects on layoffs and hiring differ across firms of different sizes. To this end, we split firms in four groups, depending on the number of employees they had in the first quarter of 2009. The groups are: 3-15, 16-30, 31-50, and 51-200. Given that firms of different sizes feature different levels of layoffs and hiring in

[32]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.

[33] See Section 2.3.

the pre-reform period, we re-scale average post-reform coefficients by the average pre-reform number of layoffs or hiring in a given size class, to express estimates in percentage terms and thus make them comparable.[34] Table G displays the results of this exercise. As far as point estimates are concerned, those relative to firing are relatively constant across the different groups of firms, displaying a higher value in the class 51-200. With regards to hiring, coefficients are more heterogeneous, being largest in the central classes (16-30 and 31-50) and substantially smaller among smallest (3-15) and largest (51-200) firms. It is important to stress that confidence intervals are substantially larger for bigger firms. This likely reflects both a smaller sample size (73% of firms in our sample have no more than 30 employees) as well as the smaller variability of T_i among larger firms (see Section 4.2 for details). As a result, the coefficients relative to layoffs are not significant in the 31-50 and 51-200 classes, whereas the coefficient relative to new hires is not significant among the largest firms (51-200).

6.4.2. *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). Our analysis only considers firms with two or more units employing at least three workers each.[35] 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 8 plots the results of this exercise. The within-unit treatment generates a larger effect on layoffs and new hires. Retaining an old 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. Table A6 shows that the effects of the two treatments are statistically different at the 1% level for layoffs and at the 5% level for new hires. 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.

6.4.3. *Turnover:* Firms with a higher propensity to separate from workers should be more prone to adjust labor demand in response to shocks. 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

[34]We therefore compute $\gamma_j = \sum_{k=2012}^{k=2015} \beta^R_{kj} I(k=t) \frac{1}{4\bar{y}_j^{pre-2012}}$ where j indexes the size class and $\bar{y}_j^{pre-2012}$ is the mean value of the outcome in the pre-reform period.

[35]Estimates that lift this restriction are virtually unchanged.

distribution of the turnover measure. Results are identical when we use tertiles or quartiles of the distribution. We estimate a triple difference specification

$$Y_{it} = \lambda_i + \gamma_t + \sum_{k=2009}^{2015} \beta_k^R I(k=t) \times R_i + \sum_{k=2009}^{2015} \beta_k^{to} I(k=t) \times TO_i + \sum_{k=2009}^{2015} \beta_k^{R,to} I(k=t) \times R_i \times TO_i + \varepsilon_{i,t} \tag{6.1}$$

where TO_i is a dummy taking value 1 when the firm's turnover lies in the top half of the distribution. Most of the effect on layoffs is explained by high-turnover firms (Figure 9). Almost no effect is detected on low-turnover firms. Thus, high-turnover firms more easily manipulate the margin of layoffs. The evidence indicates 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

7.1. **Alternative specifications.** In Section 4.3 we argued that the number of retained *potential retirees* (R_i) reflects the age structure of the firm workforce and could, therefore, be correlated with other non-observed time-varying characteristics, motivating our choice of instrumenting it with the firm-level shift in the retirement date (T_i). Table A8 shows the non-IV specification that uses R_i as the treatment (columns 1 and 3), as well as the specifications that use the number of *potential retirees* (columns 2 and 4) and the share of retained *potential retirees* over firm employment in 2011 (columns 3 and 6).[36] When using the number of retained *potential retirees* or the number of *potential retirees* as the treatment, the post-reform coefficients display a similar dynamic pattern as those estimated using the preferred IV specification (4.3). However, pre-reform coefficients on hiring are less close to 0 (the one relative to year 2009 is statistically significant across both specifications), highlighting the virtue of the IV. When using the share of retained *potential retirees* as the treatment, pre-reform coefficients on both layoffs and new hires indicate the existence of substantial differential pre-trends, making post-reform coefficients not informative. This exercise therefore confirms that different shares of retained *potential retirees* are associated with different workforce's age structures and many other differences that prevent using these shares as the identifying source of variation.[37]

7.2. **Sensitivity checks.** In this section we discuss our robustness checks and some potential threats to the identification strategy. We start by looking at some observable channels that could confound our estimates. Firm fixed-effects do not control for time-varying differences across firms in our main specification. Table 3 has shown that the relation between the

[36]Remember that we define as retained *potential retirees* those *potential retirees* whose retirement date is shifted by at least one year due to changes brought about by the reform.

[37]When the share of retained *potential retirees* is instrumented by T_i, on the other hand, we recover very similar dynamics as those emerging from the estimation of (4.3).

treatment and firm characteristics is weak. We further address this concern by estimating an augmented specification

$$Y_{it} = \alpha + \lambda_i + \sum_{k=2009}^{2015} \beta_k \gamma_k + \sum_{k=2009}^{2015} \beta_k^T \gamma_k \times T_i + \sum_{k=2009}^{2015} \delta_k \gamma_k \times X_i + \varepsilon_{i,t} \tag{7.1}$$

where X_i is a vector of firm's characteristics at baseline that we interact with year-fixed effects. The specification allows for non-parallel trends as long as they can be explained by baseline characteristics. Figure 10 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 old (> 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 capturing labor demand trends driven 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. For internal validity, we estimated our baseline specification on firms with at least one *potential retiree* in the last quarter of 2011. We check that results are robust to the inclusion of the universe of firms. We set $T_i = 0$ if a firm employs no *potential retirees*. 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, indicating that the baseline specification was close to capturing the effects on the outcomes in the entire economy.

We conclude with a short discussion about general equilibrium and whether it could affect our identification. Our analysis relies on the absence of spillovers across firms. General equilibrium effects of the reform can threaten this assumption. 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 from other firms. However, our analysis focuses on a very short horizon (4 years), while it takes time for these processes to realize. In addition, reductions in the number of new hires are significant, but not sizable when compared to cycle fluctuations in hiring.[38] 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 general equilibrium in the market in four years after the reform.

[38]A retained *potential retiree* causes an average 0.29 drop in the number of new hires. There are about 104,000 *potential retirees* in our sample. If all of them were retained for more than 1 year, total new hires would drop by 30,000 units, which is 0.5% of the total number of new hires in 2011.

8. Workers' earnings and take-up of other social insurance programs

In this section we study how raising the retirement age affects *potential retirees* and their co-workers. We start by discussing the effects 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 insurance programs in response to reforms that change retirement rules (e.g. Duggan et al., 2007 and Staubli and Zweimüller, 2013). We find similar evidence. On the other hand, spillovers on younger co-workers - due to labor susbtitutability - 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 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.[39] 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 11, Panel A). A 1σ shift in the full retirement age of the firm (1.37 years) causes a drop of 11,395 euros in 2015 (1.8% of the pre-reform average).[40] Panel A of Table 8 estimates the IV version of (4.6). A retained *potential retiree* reduces total labor earnings by around 24,718 euros. Middle-aged and old workers emerge as the most affected, confirming their close substitutability with *potential retirees*. Their earnings drop by 18,426 and 4041 euros, 4% and 8% of the pre-reform averages, respectively. Earnings also drop for young workers, but the loss is not statistically significant and around 1%. Figure 11 reports results when adding non-work subsidies to labor earnings. The decline becomes smaller because non-work subsidies are triggered by an increase in the number of fires. A retained *potential retiree* causes a decline in the sum of labor earnings and non-work subsidies of 16,848 euros (Table 8, Panel B).

The decline in earnings moderates after accounting for non-work subsidies. Hence, 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

[39] We focus on worker-level outcomes and we consider firms that remain open throughout the pre-reform period. For this reason, 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 always active between 2009 and 2015. Furthermore, the regression are weighted based on firm size at the baseline quarter.

[40] We winsorize all the monetary outcomes at the 99% of their distribution.

earnings growth when *potential retirees* remain at work. To quantify the relative contribution of separations and within-firm dynamics 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.[41] 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.[42] To assess the cost of separation, we perform a difference-in-difference analysis 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. We also weight controls based on the standard CEM weights (see Iacus et al., 2011).[43] Figure A6 reports the results. The estimated earnings drop is 5,200 euros three years after the separation. It amounts to 20% of the average, 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 12 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 explains around 45.2% of the total effect of the reform on earnings. Although separations play a relevant role, within-firm dynamics provide an important contribution. We replicate the exercise for young and middle-aged workers (Figure 13). Some important heterogeneity emerges. Separations explain a larger part of the earnings losses for young workers (63% of the total). In particular, we can impute to separations almost the entire 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 70% of the total). The uncovered heterogeneity is consistent with a model of seniority where earnings grow with age within the firm. 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 insurance programs for *potential retirees* and their co-workers as a preliminary step to the quantify of 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

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

[42] The covariates are age, sex, wage, occupation, type of contract, experience, sector, province, firm size. We match 80.8% of the separated workers.

[43] We discuss the weighting and further details about the match in Appendix F.

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

match every *potential retiree* to the firm where she was incumbent at the reform date. We then add the outcomes of interest across all *potential retirees* incumbent in the same firm. Figure 14 reports the results for both categories of workers. *Potential retirees* and co-workers experience a spike in total transfers. In 2015, for example, a 1σ increase in the average retirement age among *potential retirees* at the firm is associated with an increase in transfers equal to 1,270 euros for *potential retirees* themselves and to 4,645 euros for their co-workers. Table A9 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 10,000 euros in 2015 following a 1σ increase in the full retirement age among *potential retirees* at the firm. Total firm-level savings on pension outlays in the first four years after the reform amount to 24,778 in the post-reform period - 8 times total extra transfers for *potential retirees*. There is zero effect on pension entitlements for co-workers, consistently with the fact that they were not expected to retire in the short-run before the reform is implemented.

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, generating unintended costs. In this section we develop an accounting model to estimate the implications of these costs for public finances in the short-run as a way to prove their relevance in the analysis of policy incidence. We focus on the fiscal externality of the reform that we define as the share of mechanical pension savings that 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 can be relevant for other public policies that reduce the incentives of old workers to leave the firm.

Our analysis focuses on the short-run as the rest of the paper. Labor substitutability can also influence the long-run dynamics in several potential ways. As we documented, some workers lose their job shortly after the reform and their earnings will still be affected by the job loss several years later. Moreover, we showed that some workers are not hired because of the policy shock, which will presumably lower their probability of employment. Finally,

labor substitutability between age cohorts will influence quantities and prices in the long-term depending on general equilibrium dynamics that are hard to disentangle given the lack of data and the absence of a model that outlines these mechanisms.[45]

9.1. **An accounting model.** To study the short-run incidence of the policy on the government budget, we construct a model of government accounting. We consider two types of agents and we define them as in our empirical analysis with the labels of *potential retirees* (pr) and their co-workers (c). Agents allocate their time across different labor-related activities. We call l_i^j the time that type i spends performing activity j.[46] The main activity is paid labor in a firm. A postive share of workers receives non-work subsidies, short-time work benefits, disability benefits, benefits related to sickness or leave, or pension entitlements. Each agent faces the following budget constraint:

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

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, 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.[47] 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:

$$FE = -\frac{\sum_{i=pr,c} n_i \left(NW_i \frac{dl_i^{NW}}{dT_i^P} + ST_i \frac{dl_i^{ST}}{dT_i^P} + D_i \frac{dl_i^D}{dT_i^P} + SL_i \frac{dl_i^{SL}}{dT_i^P} + P_i^E \frac{dl_i^E}{dT_i^P} - \tau_i \frac{dz_i}{dT_i^P} \right)}{\sum_{i=a,c} n_i \cdot dT_i^P \cdot P_i \cdot I\left(T > T_i^P\right)} \tag{9.1}$$

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. When FE is

[45] We further discuss the importance of short- and long-run for the normative analysis of the policy at the end of Section 9.2.

[46] Alternatively, l_i^j is the share of individuals of type i performing activity j.

[47] 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. We ignore for simplicity taxes paid on NW_i, ST_i, D_i, LS_i, P_i and P_i^E, which would reduce the cost of behavioral responses under the assumption that the total effect on transfers is smaller in absolute value than the mechanical savings on pension payments.

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. Moreover, insofar as 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. Finally, our analysis focuses 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 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.[48] 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 P^E as a conservative 70% of the average and median value of monthly pension payments in the data (13,100 and 16,300 euros, respectively).[49] We check alternative parametrizations of τ ranging from 25% to 35% for robustness.[50] 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. Standard

[48]Coefficients are reported in table A9.

[49] Workers claiming *opzione donna* (the main early retirement option) get roughly 65% 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 $P^e = 0.9 \times P$.

[50] The average income tax rate for the median income (roughly 22,000 euros) is 24%.

errors are bootstrapped via a wild bootstrap procedure with 1000 repetitions.[51]

The first column of Table 9 shows the estimates of the fiscal externality on the restricted sample of *potential retirees*. It shows what would happen if we ignored the spillovers on co-workers that are caused by labor substitutability. All estimates are close to zero. Since *potential retirees* work for longer per effect of the reform, they increase labor earnings. Tax revenues from the extra earnings offset the increase in government transfers from other social insurance programs.

We add the spillovers to co-workers to the model in Columns 2 to 4 and we show the estimated values of the fiscal externality across alternative calibrations of the average tax rate. Point estimates range from -0.57 to -0.61, indicating that, even when we add spillovers, the savings on pension outlays overcome the cost of behavioral responses. However, the spillovers generate - and entirely explain - a non-negligible loss of mechanical revenues. Revenues raised by the government are significantly lower than what the policy mechanically raises on the workers close to retirement. 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 an increased probability of being fired. The policy also reduces labor earnings for co-workers causing 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 14 suggests, savings come only from *potential retirees*, who represent a small share of the workers in our sample. In a longer horizon, a larger share of the workforce will contribute to generate mechanical savings increasing the benefits of the policy.

The baseline model outlined in Section 9.1 disregards tax revenue losses on marginally non-hired workers. We attempt to incorporate them into the computation of the fiscal externality. To provide an upper bound to the 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.[52] To calibrate the number of marginally non-hired workers, we use estimates from the first part of the analysis. In this conservative scenario, the fiscal externality is larger. It ranges from -0.65 to -0.67 indicating that up to 2/3 of mechanical savings are lost in the short-run.

[51] 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 under the assumption that the panel is short.

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

Our results show that within-firm spillovers significantly affect the cost of the policy changing our conclusions on the fiscal externality. Hence, labor substitutability is pivotal to study the incidence of this reform. Substitutability between age cohorts has 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, or lower monetary incentives for early retirement or for work after the full retirement date, or 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 will affect incumbent workers who are close susbstitutes to old workers, creating unintended costs similar to those documented in our analysis.

9.3. **Relating the fiscal externality to Welfare.** A normative analysis of the reform must evaluate the intertemporal benefits and costs of the policy. In particular, 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, which is determined by short- and long-run components. An important part of the willingness to pay depends on the extent to which future cohorts benefit from an extension of the retirement age and from complementary policies.[53]

We cannot estimate the long-run welfare effects of the policy because we do not have data on a long time horizon. Even if we had data, our conclusions would strongly rely on the general equilibrium effects of the policy that are hard to disentangle. Yet, we can highlight how the short-term components of welfare are affected by labor substitutability. First, the workers marginal willingness to pay for the reform is affected by the spillovers caused by adjustments in labor demand. Involuntary unemployment plays an important role since workers are fired in response to the policy. Hence, some behavioral responses have first-order utility effects depending 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. We quantify the other welfare component, measuring the short-run cost of the reform. The fiscal externality estimated above is a version of the cost of the policy where we express the behavioral impact of the policy on the government budget as a fraction of mechanical revenues. This measure allows us to study the consequences of substitutability for the government budget, which is important to derive implications for similar policies. Moreover, since the reform was implemented during a budget crisis, the fiscal externality is useful to quantify the effects of the policy on the short-run government balance.

[53]The willingness to pay can be high in case future polices improve redistribution across generations, or lower if the money is not transferred to younger cohorts.

10. Conclusions

This paper studies the importance of labor substitutability 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. In particular, middle-aged and old 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 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 plays and important role for studying the incidence of the policy.

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. These responses are an important component of the welfare effects of the reform. So is the overall cost of the policy. Our finding suggest very different conclusions on the fiscal costs once spillovers are incorporated in the model. In light of these findings, we argue that firms are an important vector for the transmission of public 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 can 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 have also some policy implications. We show leakages on the pension savings generated by the policy, but revenues are still raised in the short-run. However, 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. Hence, 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 at a later date as in the case where the extension of the full retirement age is grandfathered.

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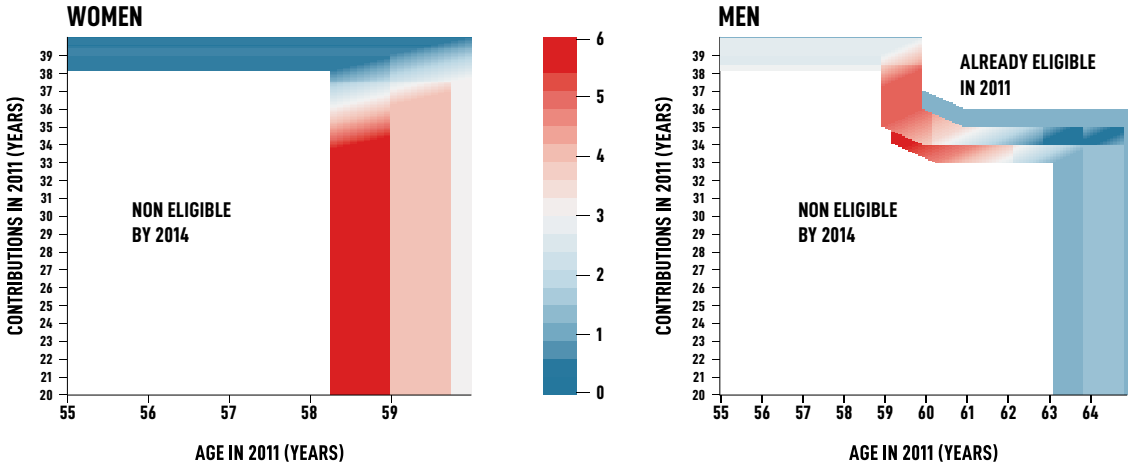
Figures

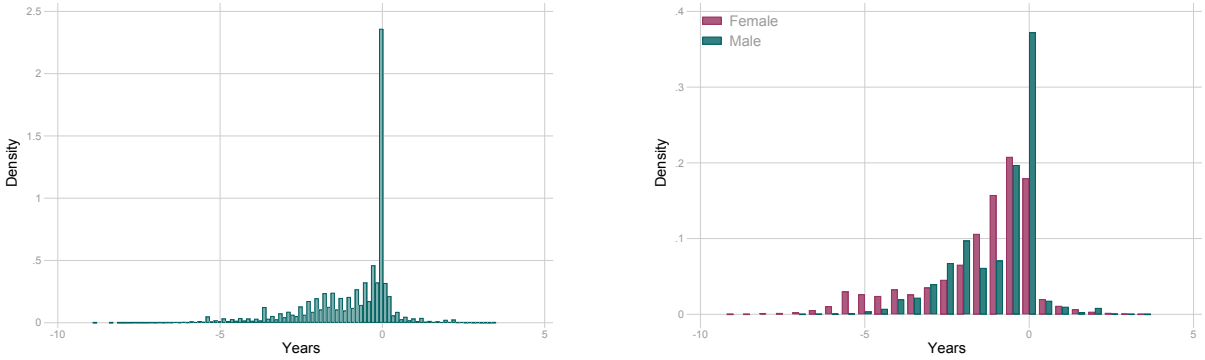
Figure 1. Reform-induced changes in the retirement age



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 age and years of contributions in December 2011 and gender. The shifts are constructed using the rules detailed in Table 1 under the assumptions listed in Section 4.1.

Figure 2. Post-reform retirement date - Forecast quality assessment

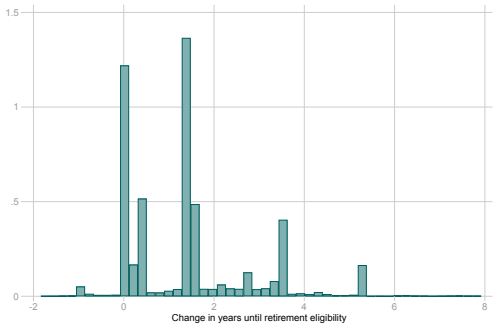
Panel A: All Workers Panel B: By Gender



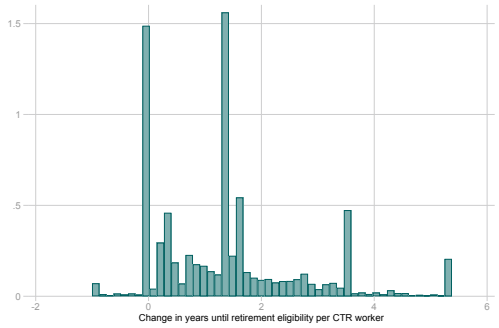
Notes: The Figure shows a forecast quality assessment of our individual treatment. The horizontal axis measures the difference between the post-reform predicted full retirement date and the actual retirement date. The sample includes workers who were expected to retire by 2014 under pre-reform rules 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 = 123,887.

FIGURE 3. Treatment distribution

Panel A: Worker-level



Panel B: Firm-level

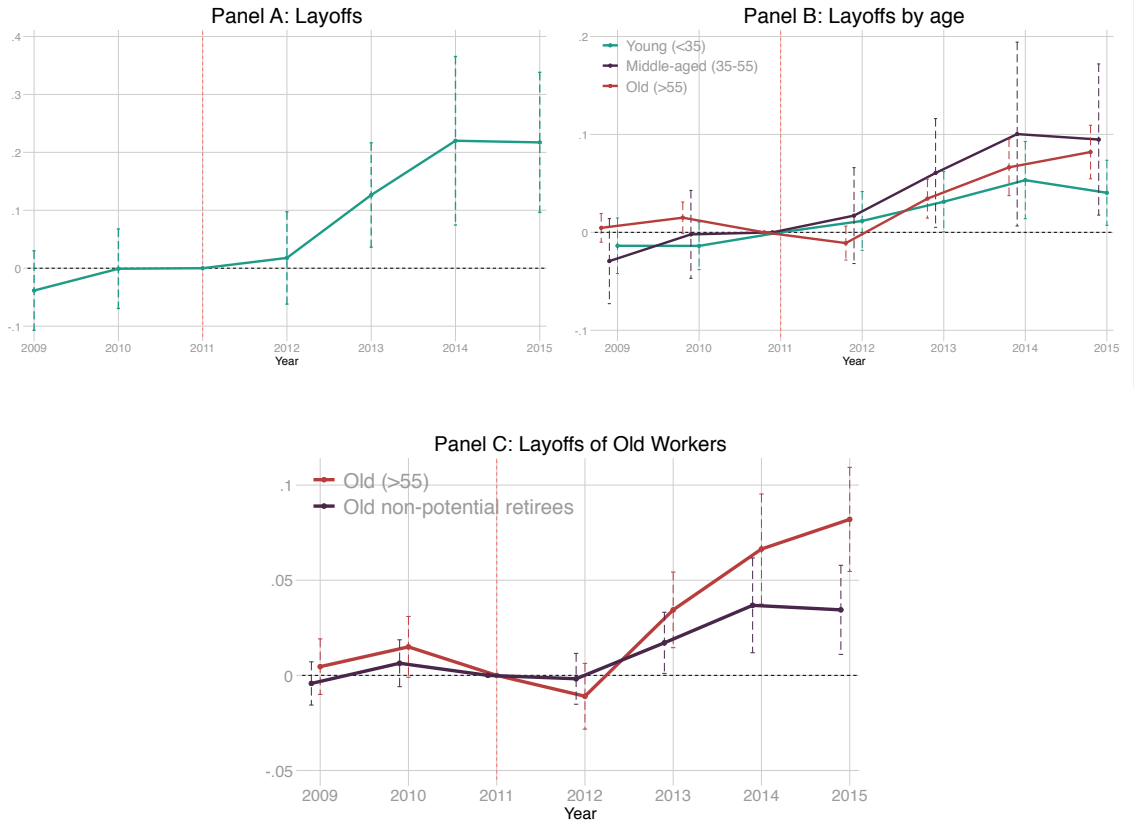


Notes: Panel A shows the distribution of the worker-level treatment among *potential retirees* (i.e. full-time workers who were eligible to retire by the end of 2014 under pre-reform rules) in our sample of firms. Panel B shows the distribution of the treatment at the firm level among firms that employ at least one CTR worker in the last quarter of 2011. Number of workers = 104,942. Worker-level treatment mean = 1.38 (sd = 1.42). Number of firms = 64,721. Firm-level treatment mean = 1.40 (sd = 1.37)

FIGURE 4. The reform prolongs the working life of *potential retirees*

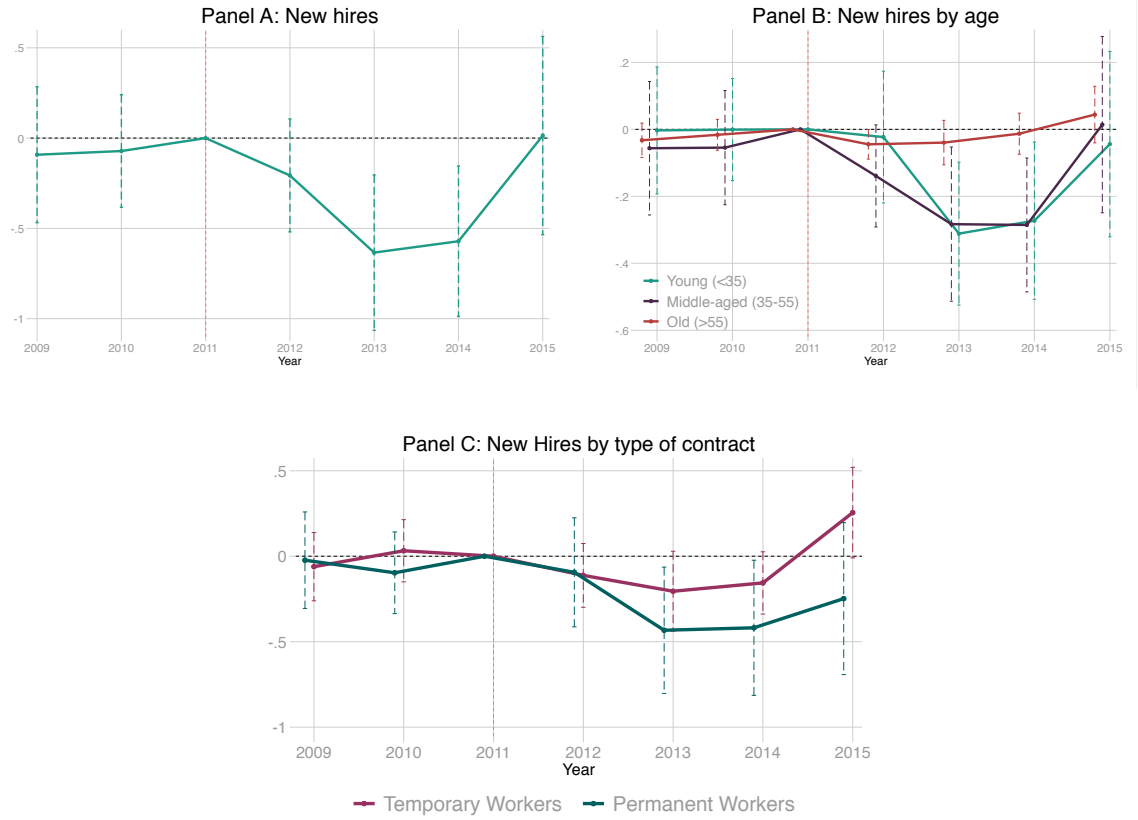
Notes: The Figure shows the effect of a one year shift of the retirement date based on the specification in (4.5) where the unit of analysis is the single *potential retiree*. We plot the coefficients of the regressions alongside 95% confidence intervals. Standard errors are clustered at the worker level. We define *potential retirees* those workers who were expected to retire within three years 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 the individual-level treatment. Number of observations = 962,696. Pre-reform mean outcomes: months worked = 11.65, months in retirement: 0.17, months in retirement (benchmark): 0.17.

FIGURE 5. Layoffs



Notes: The Figure shows the response of total layoffs (Panel A), layoffs by age group (Panel B) and layoffs of old workers (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 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 = 453,047. 1σ of $T_i = 1.38$ years. Mean outcome pre-reform: total = 0.43; young = 0.14; middle-aged = 0.23; old = 0.06, old non-*potential retirees* = 0.04. KP F-statistics = 1,214.09

FIGURE 6. New Hires

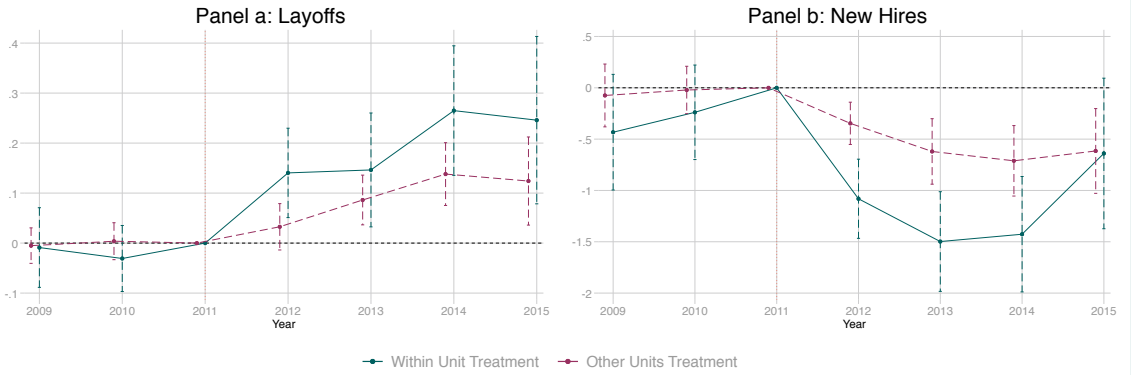


Notes: The Figure shows the response of total new hires (Panel A), new hires by age (Panel B) and new hires by type of contract (Panel C) to an extra retained *potential retiree*, alongside 95% confidence intervals. Standard errors are clustered at the firm level. We define *potential retirees* those 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 = 453,047. 1σ of the treatment = 1.38 years. Mean outcome (pre 2012): total = 5.23, young = 2.58, middle = 2.26, old = 0.38, permanent = 1.59, temporary = 3.52. KP F-statistics = 1,214.09

FIGURE 7. Firms respond only to shock on *potential retirees*

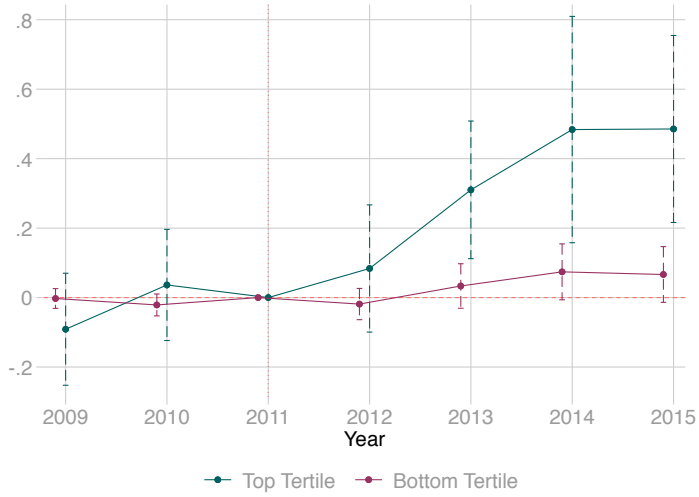
Notes: The Figure shows the effect on layoffs (Panel A) and new hires (Panel B) of two treatments, alongside 95% confidence intervals. Standard errors are clustered at the firm level. The regression is based on a variant of the specification (4.3) where two treatments are included. The first treatment 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 = 276,808. Mean outcomes pre-reform: layoffs = 0.43; new hires = 5.23. KP F-statistics = 129.12

FIGURE 8. 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 = 622,986. Mean outcomes pre-reform: layoffs per qualification = 0.16; new hires per qualification = 2.38. KP F-statistics = 239.87.

FIGURE 9. Layoffs in high and low-turnover firms

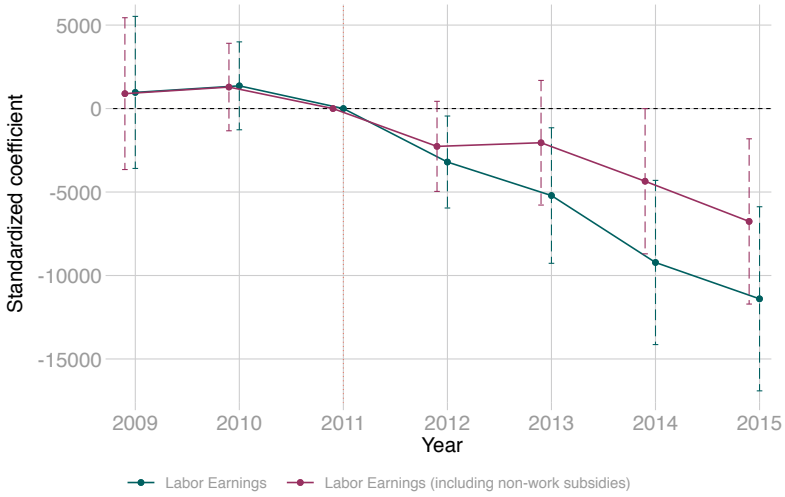


Notes: The Figure shows the heterogeneous response of layoffs in firms which are above and below the median of turnover rates in the pre-reform period, alongside 95% confidence intervals. Standard errors are clustered at the firm level. The regression is based on specification (6.1) and the plotted coefficients 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 = 453,047. 1σ of the treatment = 1.38 years. Mean outcome pre-reform = 0.43.

FIGURE 10. Robustness checks

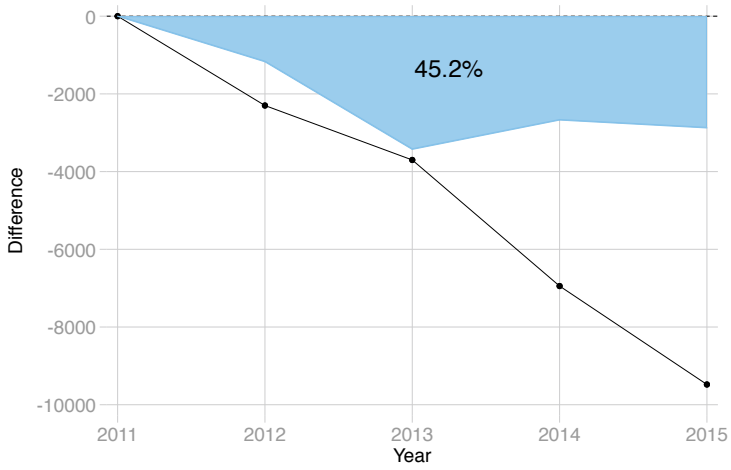
Notes: The Figure addresses the robustness of the main estimates on layoffs (Panel A) and new hires (Panel B). We confront the results of the baseline specification reported in Figures 5 and 6 with the results of sensitivity checks that employ the specification in (7.1). The plotted coefficients represent 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 different 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 $T_i = 0$ 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 11. Co-workers' labor earnings



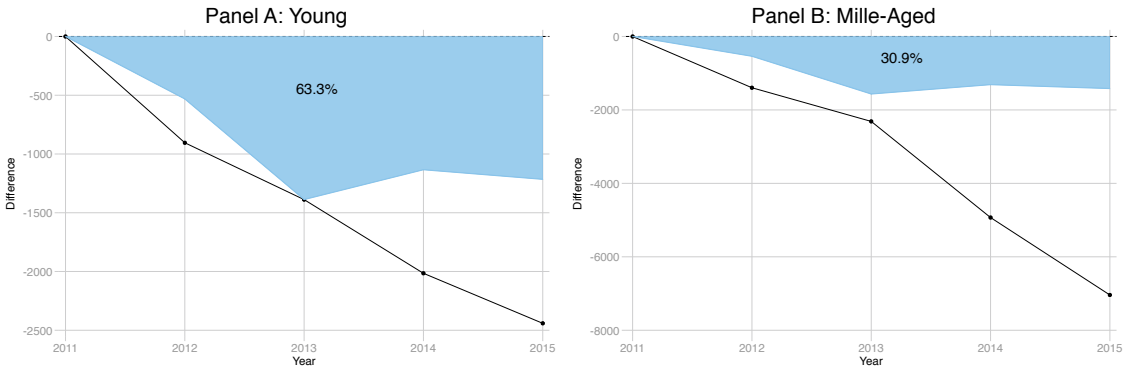
Notes: The Figure shows the effect of a 1σ increase in the treatment on total labor earnings of incumbent co-workers, with and without non-work subsidies, alongside 95% confidence intervals. Standard errors are clustered at the firm level. Incumbent co-workers are non-*potential retirees* who are employed at a firm employing at least one *potential retiree* in q4-2011. We define *potential retirees* those workers who were expected to retire within three years under the pre-reform rules when the reform is implemented. Labor earnings include earnings from private sector jobs, self-employment and public sector jobs. The firm-level regression is based on specification (4.5) and it includes firm and year fixed effects. The treatment is defined as the average change in the retirement date for *potential retirees* employed at the firm when the reform is implemented. The regression 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 = 540,239. Treatment SD = 1.38. Mean outcomes pre-reform: labor earnings = 647,061.12 ; labor earnings and non-work subsidies = 647,170.31.

FIGURE 12. Decomposing coworkers' earnings loss



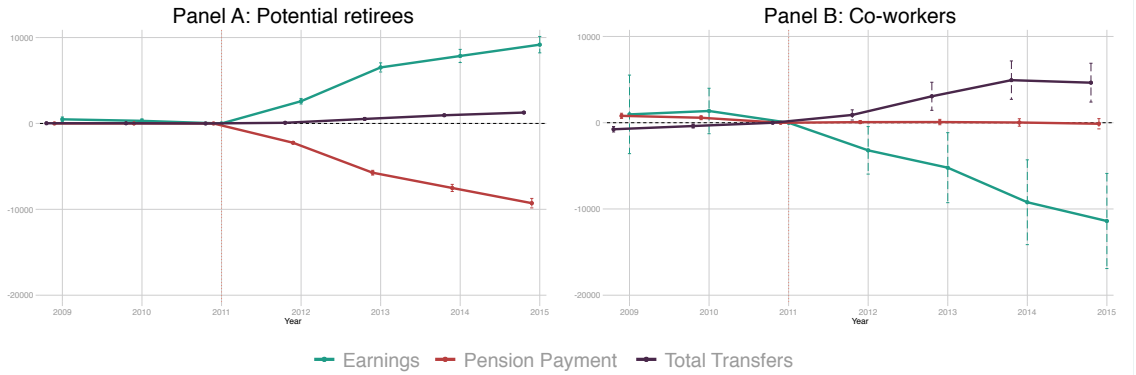
Notes: The Figure shows the share of incumbent co-workers' earnings loss that can be imputed to involuntary separations. Incumbent co-workers are non-*potential retirees* who are employed at a firm employing at least one *potential retiree* in q4-2011. We define *potential retirees* those workers who were expected to retire within three years under the pre-reform rules when the reform is implemented. The black line plots the effect of a 1σ increase in the treatment on total labor earnings of incumbent 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. The cost of job losses is obtained using a difference in differences strategy 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. This strategy provides the cost of the job loss for every year after the event occurs and its results are reported in Appendix Figure A6. Estimates of the effect of the reform on total separations are obtained running the specification in (4.5) on the total number of involuntary separations.

FIGURE 13. Decomposing cow-workers' earnings loss by age



Notes: The Figure shows the share of incumbent co-workers' earnings loss that can be imputed to involuntary separations, distinguishing young workers (aged below 35, Panel A) from middle-aged ones (aged 35-55, Panel B). Incumbent co-workers are non-*potential retirees* who are employed at a firm employing at least one *potential retiree* in q4-2011. 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 black line plots the effect of a 1σ increase in the treatment on total labor earnings of incumbent 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. The cost of job losses is obtained using a difference in differences strategy 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. This strategy provides the cost of the job loss for every year after the event occurs and its results are reported in Appendix Figure A6. Estimates of the effect of the reform on total separations are obtained running the specification in (4.5) on the total number of involuntary separations.

FIGURE 14. 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 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 = 1.38; Mean outcomes in the pre-reform period: *potential retirees'* labor earnings = 50,837.06; *potential retirees'* pension entitlements = 44.13; *potential retirees'* total transfers = 890.18; co-workers' labor earnings = 647,061.12; co-workers' pension entitlement = 3,083.81; co-workers' total transfers = 9,882.25.

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
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. 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. 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. Additional rules that apply to workers who accrue the first contribution in 1996 or later are detailed in sub-section 2.3.

TABLE 2. Placebo Tests

	Layoffs	New Hires
T_i X Post 2009	0.0084	0.020
	(0.0076)	(0.043)
T_i X Post 2010	0.0044	0.024
	(0.0086)	(0.043)
Observations	193,869	193,869
Mean Outcome (pre 2012)	.43	5.23
Treatment Mean	1.4	1.4
Treatment SD	1.37	1.37

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 treatment 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 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.

TABLE 3. Balancing tests

	(1)	Mean (2)
Share young workers (< 35)	0.001	0.298
	(0.001)	
Share middle-aged workers ($35 - 55$)	-0.010***	0.579
	(0.001)	
Share old workers (> 55)	0.009***	0.123
	(0.001)	
Share of *CTR* workers	-0.001***	0.116
	(0.000)	
Share male workers	-0.012 ***	0.658
	(0.001)	
Share white-collar workers	0.001	0.328
	(0.001)	
Average gross daily real wage	1.275 *	92.05
	(0.635)	
Share full-time workers	-0.002*	0.885
	(0.001)	
Firm size	0.453 ***	26563
	(0.453)	
Firm age	-0.179**	19.856
	(0.051)	
Firm in manufacturing	-0.000	0.443
	(0.000)	
N. firms	63483	

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 = 1.38 years.

TABLE 4. Firms with at least one *potential retiree* and other firms

	Master sample		Other firms	
	mean	sd	mean	sd
Firm size	26.56	32.70	8.23	10.22
Firm age	19.86	12.78	13.99	10.64
Share in manufacturing	0.43	0.50	0.25	0.44
Share in services	0.34	0.47	0.51	0.50
Share male workforce	0.66	0.29	0.55	0.35
Avg. workforce age	41.68	4.63	36.92	6.11
Share workforce aged < 35	0.30	0.19	0.46	0.28
Share workforce aged $35 - 55$	0.58	0.19	0.49	0.26
Share workforce aged > 55	0.12	0.12	0.05	0.10
Avg. workforce tenure	7.87	4.77	5.32	4.09
Avg. workforce experience	15.94	4.72	11.80	5.34
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	0.88	0.17	0.74	0.30
Share open-ended contracts	0.92	0.15	0.90	0.19
Avg. gross daily wage	92.05	142.93	78.92	187.34
Observations	64721		347747	

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 5. *Potential retirees* and other workers in the master sample

	Potential retirees		Other workers	
	mean	sd	mean	sd
Gender (1 = male)	0.71	0.45	0.72	0.45
Age	57.85	2.97	40.92	9.79
Tenure	14.90	9.34	9.01	7.45
Experience in private sector	23.67	8.75	15.07	9.86
Years since entered labor market	39.81	11.10	20.50	15.66
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	0.89	0.31
Gross daily wage	109.98	111.77	101.93	109.41
Observations	104904		1569764	

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 6. 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 is the retirement choice to the reform. 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 7. The effect of the reform on layoffs and hires

Panel A: Layoffs	All	Young	Middle-aged	Old	Old Non-potential retiree
	(1)	(2)	(3)	(4)	(5)
Post x R_i	0.16***	0.043***	0.079***	0.036***	0.021***
	(0.038)	(0.013)	(0.022)	(0.007)	(0.006)
N	453,047	453,047	453,047	453,047	453,047
Mean pre-2012	0.43	0.14	0.23	0.06	0.04
Coeff First Stage	0.27	0.27	0.27	0.27	0.27
SE First Stage	0.001	0.001	0.001	0.001	0.001
KP F-statistics	7,284.63	7,284.63	7,284.63	7,284.63	7,284.63
Panel B: New Hires	All	Young	Middle-aged	Old	
	(1)	(2)	(3)	(4)	
Post x R_i	-0.29	-0.16	-0.14	0.003	
	(0.19)	(0.11)	(0.087)	(0.027)	
N	453,047	453,047	453,047	453,047	
Mean pre-2012	5.23	2.58	2.26	0.38	
Coeff First Stage	0.27	0.27	0.27	0.27	
SE First Stage	0.001	0.001	0.001	0.001	
KP F-statistics	7,284.63	7,284.63	7,284.63	7,284.63	

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 retiree* 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.

TABLE 8. The effects of the reform on co-workers' labor earnings

Panel A: Labor Earnings

	All	Young	Middle-aged	Old Non-potential retiree
	(1)	(2)	(3)	(4)
Post x R_i	-24718.5***	-2251.1	-18426.2***	-4041.2***
	(7246.4)	(2839.0)	(4874.9)	(1467.3)
N	540,239	540,239	540,239	540,239
Mean pre 2012	647,061.12	151,053.31	446,461.16	49,546.64
KP F-Stat	1,204.13	1,204.13	1,204.13	1,204.13

Panel B: Total Earnings (labor earnings and non-work subsidies)

	All	Young	Middle-aged	Old Non-potential retiree
	(1)	(2)	(3)	(4)
Post x R_i	-16848.8***	-32.5	-13677.1***	-3139.1**
	(6514.8)	(2825.3)	(4243.1)	(1397.4)
N	540,239	540,239	540,239	540,239
Mean pre 2012	647,170.31	151,078.05	446,534.77	49,557.49
KP F-Stat	1,204.13	1,204.13	1,204.13	1,204.13

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. The dependent variable is labor earnings in Panel A, whereas it is the sum of labor earnings and non-work subsidies in Panel B. 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) and Column (4) on old non-*potential retirees* (above 55 years old). The regressions are 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.

TABLE 9. Fiscal Externality

	Potential Retirees only	All $\tau = 25$	All $\tau = 30$	All $\tau = 35$	W/ Loss on Non-Hired
Median Pension	0.090	-0.578	-0.583	-0.588	-0.651
	(0.056)	(0.124)	(0.139)	(0.134)	(0.124)
Mean Pension	0.068	-0.589	-0.594	-0.599	-0.660
	(0.081)	(0.124)	(0.128)	(0.133)	(0.123)
Early Retirement = 0.9×P	0.037	-0.604	-0.609	-0.614	-0.673
	(0.145)	(0.128)	(0.131)	(0.136)	(0.125)

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 non-*potential retirees*. 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%). 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.

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 old workers and their coworkers from younger cohorts. 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 for Italian firms by Card et al. (2014). Third, we study a monopsonistic labor market with constant labor supply elasticity. Consistently across settings, the change in labor demand is always inversely proportional to the degree of substitutability between old 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: old (o) and young (y). In our setting old 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 $c(x^y)$ accounts for the cost of adjusting the young workforce, which is paid in period 0. We require $c(\cdot)$ to be twice continuously differentiable and we assume that $c'(x^y) > 0$ for $x^y > 0$, $c'(x^y) \leqslant 0$ for $x^y < 0$, $c'(0) = 0$ and $c''(\cdot) \geqslant 0$. 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 old worker can be either hired or fired. We denote with n_0^o and $n_1^0 = sn_0^o$ the number of old workers in period 0 and 1, respectively. $s \leqslant 1$ captures the exogenous share of old workers who are left in period 1. We interpret s as a variable incorporating the exogenous separation rate of old workers as well as retirement rules. Output is produced according to technology $F(n_t^o, n_t^y)$ 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 old and young workers respectively. Optimality conditions require the following:

$$\beta\left(F_2\left(sn_0^o, n_1^y\right) - w^y\right) = c'\left(x^y\right) \tag{A.2}$$

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 old workers in period 1, *i.e.* an increase in s. The comparative statics for a change in s reads:

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

The sign of the comparative statics depends on two terms. 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 cut significantly the payroll to compensate the loss in marginal productivity of young workers that follows an exogenous increase in old 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 old 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 $F_{21} < 0$.[54] 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 old 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 old 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.

[54]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 old workers are overpaid can be used to provide incentives to young workers.

A.2. **Conceptual Framework - Alternative Wage 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. Assume 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. The following holds in equilibrium:

$$\phi \frac{\partial \pi(sn_0^o, n_1^y)}{\partial n_1^y} = (1-\phi)(w^y(sn_0^o, n_1^y) - \underline{w}^y) \tag{A.4}$$

which implies the following expression for the equilibrium wage:

$$w^y = \eta F_2 - \frac{\eta}{\beta} c'(n_1^y - n_0^o) + \frac{(1-\phi)}{\phi\beta} \eta \underline{w}^y \tag{A.5}$$

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 et al. (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:

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

The expression differs from the one in Jäger (2016) since we focus on a lower than expected separation rate and not on a drop in the labor force. For this reason, the change in wages is not only a function of the cross-marginal product between the two types of labor, but includes F_{22} that captures the change in the number of young workers caused by a change in s. The last term of our expression arise since we do not assume linear hiring costs. The wage change in response to a shock to the retention rate depends on the cross-marginal product between young and old labor, as well as on the slope of young workers' marginal product. 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:

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

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 old and young workers.

A.2.2. *Profit Sharing.* Card et al. (2014) present evidence of substantial profit sharing in Italian firms. We extend our model to account for profit sharing by allowing firms and workers to bargain over total profits such that:

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

This implies the following:

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

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 old workers' productivity and wages. Intuitively, the larger is the gap the more the marginal effect of the reform on profits will fall on young workers by decreasing their salary in order to preserve the wedge for old workers.[55] By 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 old 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 old workers.

A.2.3. *Monopsonistic Labor Market.* We consider the broadly used model of monopsonistic labor demand. 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

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

problem would become:

$$\pi = \pi_0 + \beta \left(F\left(sn_0^o, n_1^y\right) - w^o sn_0^o - n_1^{y\,\frac{1+e}{e}} \right) - c\left(n_1^y - n_0^y\right) \tag{A.11}$$

The firm's optimality condition is:

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

We derive the following comparative statics:

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

The expression above shows a one-to-one mapping between labor demand changes and the substitutability between old 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 lower room to adjust labor demand in response to the reform.

Result 4: A monopsonistic labor market delivers a one-to-one relationship between the labor demand response and the substitutability between young and old workers. If the two types of work are substitutable, labor demand falls in response to a shock to the retention rate of old 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.[56] 95% 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%, compared to 62.5% in Germany, 59.6% in France and 43.3% in the United States.[57] The share of employment in manufacturing is 18.2%, compared to 19.5% in Germany and 12.6% 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% in 2005 to 48.2% in 2015.[58] 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.[59] 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.

[56]Data from Eurostat, annual enterprise statistics. Financial and insurance sectors are included.
[57]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).
[58]Source: Eurostat, Employment statistics.
[59]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*).

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.[60] 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.[61] If more than one record survives criteria (a) to (d), we drop all but one randomly.

[60] If more than one records satisfies criterion (a), we then use criterion (b), and so on up to criterion (d).
[61] 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.[62] 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.

[62]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

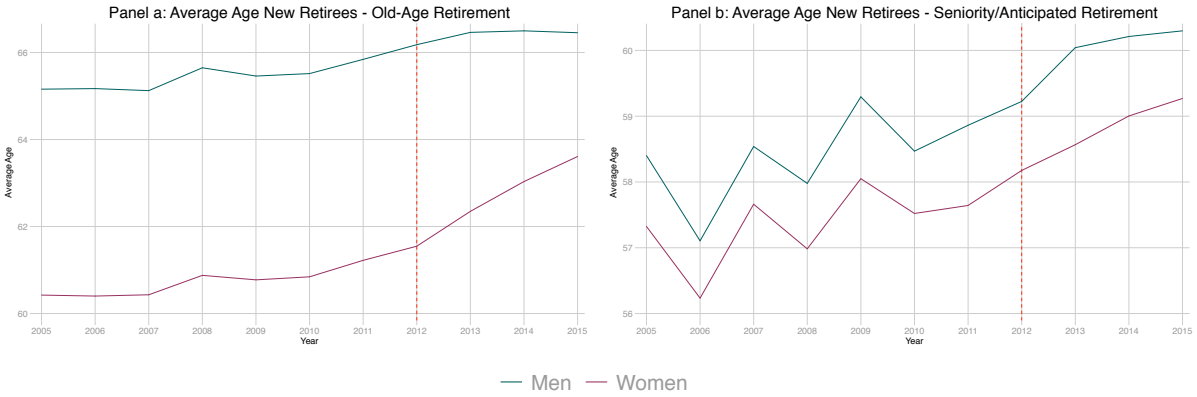
$$w_i = \frac{N_C}{N_T} \times \frac{N_{Ts}}{N_{Cs}}$$

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

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

Appendix G. Additional figures and tables - For Online Publication

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

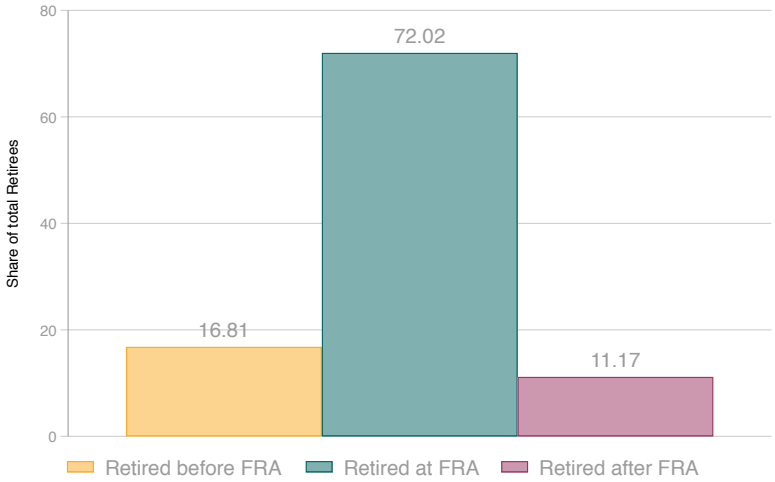


Notes: The figure shows the evolution of the average age at retirement, split by gender and type of pension. Panel A refers to old-age pensions, Panel B refers to seniority pensions. We classify as seniority pensions those claimed with "quota 40" and "quota 96" before the reform and replaced by "anticipated 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

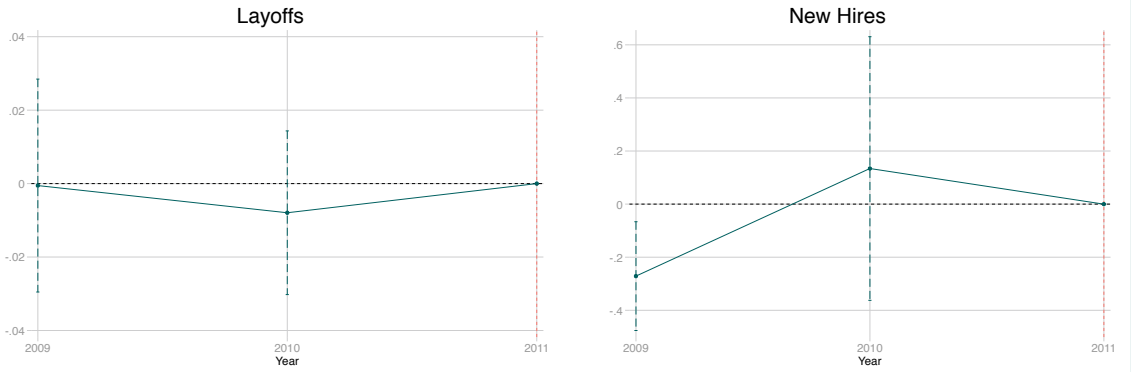
Notes: The figure shows the evolution of the number of new retirees, split by gender and type of pension. Panel A refers to old-age pensions, Panel B refers to seniority pensions. We classify as seniority pensions those claimed with "quota 40" and "quota 96" before the reform and replaced by "anticipated 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



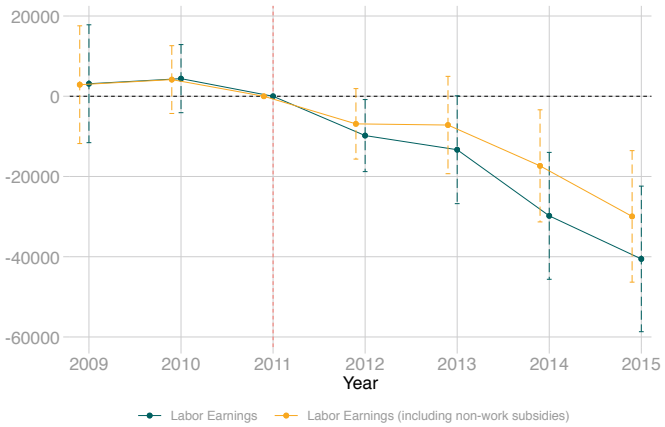
Notes: The figure shows the share of retirees retiring at full-retirement age; the share of workers early retiring (more than 1 year before the FRA); and the share of workers who retire more than 1 year after the full-retirement age. Shares are derived from authors calculations on the INPS register of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

FIGURE A4. Retained workers as treatment - estimation pre-trends



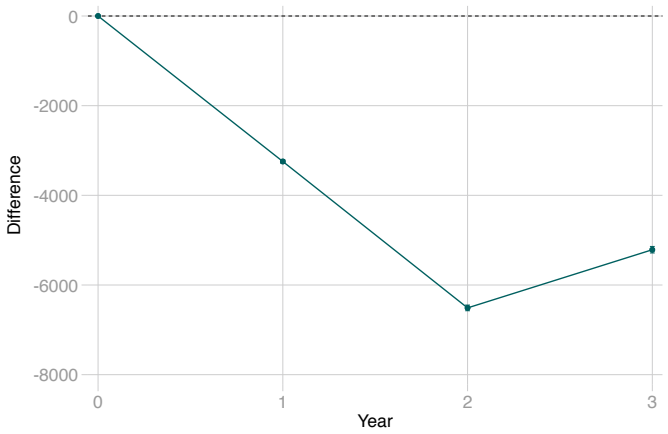
Notes: The Figure shows the effect of retaining an additional *potential retiree* on layoffs and new hires in the pre-reform period, alongside 95% confidence intervals. Standard errors are clustered at the firm level. Results are based on the OLS version of specification (4.3), which includes firm and year fixed effects. The treatment is the number of retained *potential retirees*. 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: 453,047. Pre-reform mean outcomes: layoffs = 0.43; new hires = 5.23

FIGURE A5. 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: 512,902; Pre-reform mean outcomes: Labor earnings = 647,061.12; Labor earnings including non-work subsidies = 647,170.31; KP F-Statistics: 171.03

FIGURE A6. Cost of Separations



Notes: The figure shows the effect of separating from a firm on subsequent labor earnings. 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: 10,114,492 Pre-reform mean outcome = 28,891.36

TABLE A1. Absences from work for similar *potential retirees* and non *potential retirees*

	Other workers		*Potential retirees*		Difference
	Mean	SD	Mean	SD	
Prob. sickness	0.29	0.45	0.34	0.47	0.050***
Prob. work-related injury	0.05	0.22	0.06	0.24	0.008***
Prob. leave	0.03	0.17	0.04	0.19	0.009***
Monetary cost of sickness	161.34	1431.96	198.55	1435.73	37.207***
Monetary cost of work-related injury	42.40	337.85	50.66	489.77	8.037***
Monetary cost of leave	11.69	145.33	19.96	1064.94	8.272***
Gross daily real wage	116.58	232.02	114.26	144.19	-2.318***
N. workers	841,101		161,401		

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 affected and non-affected workers 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 two columns report the difference in means.

TABLE A2. The effect of the reform on layoffs

	All	Young	Middle-aged	Old	Old Not affected
	(1)	(2)	(3)	(4)	(5)
$t-3$	-0.039	-0.014	-0.029	0.0046	-0.0043
	(0.035)	(0.014)	(0.022)	(0.0074)	(0.0058)
$t-2$	-0.00091	-0.014	-0.0021	0.015*	0.0064
	(0.035)	(0.012)	(0.023)	(0.0082)	(0.0063)
t	0.018	0.012	0.017	-0.011	-0.0018
	(0.041)	(0.015)	(0.025)	(0.0088)	(0.0068)
$t+1$	0.13***	0.031**	0.061**	0.034***	0.017**
	(0.046)	(0.016)	(0.028)	(0.010)	(0.0082)
$t+2$	0.22***	0.053***	0.10**	0.066***	0.037***
	(0.074)	(0.020)	(0.048)	(0.015)	(0.013)
$t+3$	0.22***	0.040**	0.095**	0.082***	0.034***
	(0.062)	(0.017)	(0.039)	(0.014)	(0.012)
N. obs.	453,047	453,047	453,047	453,047	453,047
Mean Outcome (pre 2012)	0.43	0.14	0.23	0.06	0.04
KP F-Statistics	1,214.09	1,214.09	1,214.09	1,214.09	1,214.09
Treatment Mean	.85	.85	.85	.85	.85
Treatment SD	1.02	1.02	1.02	1.02	1.02
Instrument Mean	1.4	1.4	1.4	1.4	1.4
Instrument SD	1.37	1.37	1.37	1.37	1.37

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 A3. The effect of the reform on hiring

	All	Young	Middle-aged	Old	Permanent Contract	Temporary Contract
	(1)	(2)	(3)	(4)	(5)	(6)
$t-3$	-0.092	-0.0030	-0.056	-0.033	-0.061	-0.023
	(0.19)	(0.097)	(0.10)	(0.026)	(0.10)	(0.14)
$t-2$	-0.072	-0.00080	-0.055	-0.016	0.032	-0.097
	(0.16)	(0.078)	(0.087)	(0.024)	(0.093)	(0.12)
t	-0.21	-0.023	-0.14*	-0.045**	-0.11	-0.094
	(0.16)	(0.10)	(0.078)	(0.023)	(0.095)	(0.16)
$t+1$	-0.63***	-0.31***	-0.28**	-0.040	-0.20*	-0.43**
	(0.22)	(0.11)	(0.12)	(0.034)	(0.12)	(0.19)
$t+2$	-0.57***	-0.27**	-0.29***	-0.013	-0.16*	-0.42**
	(0.21)	(0.12)	(0.10)	(0.031)	(0.093)	(0.20)
$t+3$	0.014	-0.044	0.014	0.044	0.26*	-0.25
	(0.28)	(0.14)	(0.13)	(0.043)	(0.14)	(0.23)
N. obs.	453,047	453,047	453,047	453,047	453,047	453,047
Mean Outcome (pre 2012)	5.23	2.58	2.26	0.38	1.61	3.62
KP F-Statistics	1,214.09	1,214.09	1,214.09	1,214.09	1,214.09	1,214.09
Treatment Mean	.85	.85	.85	.85	.85	.85
Treatment SD	1.02	1.02	1.02	1.02	1.02	1.02
Instrument Mean	1.4	1.4	1.4	1.4	1.4	1.4
Instrument SD	1.37	1.37	1.37	1.37	1.37	1.37

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 A4. Firms only respond to shock to *potential retirees*

	New Hires	Layoffs
Number of Retained Sample 3 X Post	-0.326	0.133***
	(0.222)	(0.041)
Number of Retained Sample 4-7 X Post	-0.123	-0.011
	(0.196)	(0.034)
Observations	276,808	276,808
Mean Outcome (pre 2012)	6.91	.5
KP F-Statistics	774.77	774.77
Mean Retained (Sample 3)	.85	.85
SD Retained (Sample 3)	1.02	1.02
Mean Retained (Sample 4-7)	2.42	2.42
SD Retained (Sample 4-7)	3.49	3.49
Instrument Mean (Sample 3)	1.4	1.4
Instrument SD (Sample 3)	1.37	1.37
Instrument Mean (Sample 4-7)	1.62	1.62
Instrument SD (Sample 4-7)	1.02	1.02
P-Value Difference Coefficients	0.265	0.001

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 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 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 A5. Heterogeneity by firm size

	Layoffs	New hires	Nobs
Size 3-15	29.923**	-2.907	247765
	(12.741)	(6.803)	
Size 16-30	35.461**	-15.726**	86639
	(14.236)	(7.130)	
Size 31-50	29.92863	-12.646*	54271
	(23.164)	(6.772)	
Size 51-200	48.811	-3.291	64372
	(38.156)	(8.716)	

Notes: The table reports the results of the specification in (4.3) on new hires and layoffs, splitting firms into 4 group (3-15, 16-30, 31-50, 51-200), based on the number of workers employed at the firm in the first quarter of 2009. To make estimates comparable, the table displays average post-reform coefficients re-scaled by the average value of the outcome in a given group in the pre-reform period. The specification includes year and firm fixed effects. Standard errors in parentheses are clustered at the firm level. 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 A6. Labor demand responses within and across qualifications

	New Hires	Layoffs
Number of Retained Same Qualification X Post	-0.94***	0.21***
	(0.26)	(0.043)
Number of Retained Other Qualifications X Post	-0.54***	0.096***
	(0.15)	(0.021)
Observations	622,986	622,986
Mean Outcome (pre 2012)	3.73	0.25
KP F-Statistics	1439.26	1439.26
Mean Retained (in Unit)	0.65	0.65
SD Retained (in Unit)	0.95	0.95
Mean Retained (other Units)	0.62	0.62
SD Retained (other Units)	1.07	1.07
Unit Instrument Mean	0.63	0.63
Unit Instrument SD	1.07	1.07
Other Unit Instrument Mean	0.70	0.70
Other Unit Instrument SD	1.05	1.05
P-Value Asymmetric	0.041	0.000

Notes: The table reports the results of the specification in (4.6) on new hires and layoffs when two treatments are included and the unit of analysis is the firm-qualification. Standard errors in parentheses are clustered at the firm level. Qualifications are: blue collar job, white collar job and manager. 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 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, (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.

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

TABLE A7. Labor demand responses in firms with high and low turnover

	Layoffs
Number Retained × High-Turnover × Post	0.31***
	(0.090)
Number Retained × Post	0.047**
	(0.019)
Observations	453,047
Mean Outcome (pre 2012)	.43
KP F-Statistics	2055.83
Treatment Mean	1.4
Treatment SD	1.37
Mean Retained	.85
SD Retained	1.02

Notes: The Table reports the results on new hires and layoffs of a variant of the specification in (6.1) where we compare pre- and post-reform years. Standard errors are clustered at the firm level. Turnover rate is the average total number of layoffs, quits and non-renewed contracts in the pre-reform period (2009-2011). We define high-turnover firms those laying in the upper median of the turnover measure distribution. The treatment is defined as the number of retained *potential retirees* employed at the firm when the reform is implemented. We define *potential retirees* those workers who were expected to retire within three years 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.

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

TABLE A8. Alternative specifications

	Layoffs			Hiring		
	R_i	Pot. Ret.	R_i/emp	R_i	Pot. ret	R_i
	(1)	(2)	(3)	(4)	(5)	(6)
$t-3$	0.013	0.002	0.205 ***	-0.250 *	-0.235 ***	4.422***
	(0.015)	(0.010)	(0.061)	(0.135)	(0.086)	(0.874)
$t-2$	0.006	-0.004	0.282 ***	0.334	0.181	1.330 ***
	(0.011)	(0.007)	(0.061)	(0.406)	(0.271)	(0.385)
t	0.014	0.016 *	-0.174 **	-0.377 ***	-0.178 ***	3.111 ***
	(0.013)	(0.010)	(0.071)	(0.078)	(0.052)	(0.262)
$t+1$	0.090 ***	0.046 ***	0.278 ***	-0.395 ***	-0.219 ***	4.327 ***
	(0.018)	(0.013)	(0.081)	(0.126)	(0.078)	(0.833)
$t+2$	0.164 ***	0.093 ***	0.220	-0.251	-0.112	3.466 ***
	(0.023)	(0.020)	(0.397)	(0.193)	(0.126)	(0.581)
$t+3$	0.196 ***	0.118 ***	-0.073	0.151	0.110	1.331 *
	(0.033)	(0.024)	(0.110)	(0.230)	(0.148)	(0.704)
N. obs.	453,047	453,047	453,047	453,047	453,047	453,047

Notes: The Table reports non-IV specifications alternative to IV specification (4.3), whereby the treatment is the number of retained *potential retirees* (columns (1) and (4)), the number of *potential retirees* (columns (2) and (5)) and the share of *potential retirees* over firm employment in 2011 (columns (3) and (6)). All specifications include firm and year fixed-effects. Standard errors in parentheses are clustered at the firm level. 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. The effect of the reform on *potential retirees* and co-workers

	Potential retirees	Co-workers
Labor earnings	26147.17***	-29028.35 ***
	(1252.833)	(8065.57)
Pension entitlements	-24778.16***	53.77
	(661.09)	(737.7164)
Disability benefits	293.54***	281.27
	(34.3412)	(190.82)
Short-time work subsidies	260.18	-4408.482*
	(189.92)	(2494.71)
Non-work Subsidies	1866.122***	13601.78 ***
	(164.31)	(2891.40)
Sick and leave benefits	670.19***	-330.77
	(59.20)	(488.54)
Early retirement (months)	3.03 ***	1.06 ***
	(0.18)	(0.34)

Notes: The table reports the sum of coefficients $\{\beta_k^T\}_{k=2012}^{k=2015}$ from the specification in (4.5). 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 and coefficients represent the effect of a one standard deviation increase in the treatment (1.37 years). 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.