The Transitional Labor Market Consequences of a Pension Reform*

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Abstract

Several countries have responded to budget pressures on public pension systems by increasing full retirement ages. Because of bunching in retirement at these ages, such reforms lead to increased retention of older workers at the employer firm. We study the effects of retaining senior workers on firms' short-run labor demand and public finances. We exploit an unanticipated Italian pension reform and leverage idiosyncratic variation in employees retention unrelated to firm's broad demographics. We document that workers on the cusp of retirement, younger co-workers and external hires are at least partially substitutes, as firms more affected by the reform increase firing and reduce hiring in its aftermath. Substitutability appears stronger for middle-aged workers in the same occupation group. We then show that raising full retirement ages affects labor earnings and the take-up of other social security programs by both retained senior employees and, due to labor demand adjustments, their younger co-workers. We conclude by estimating the fiscal externality of the reform, incorporating all behavioural responses into a model that allows for spillover on co-workers and program substitution. We estimate that from one-half to two-third of mechanical savings are lost in the short-run due to behavioural responses. Ignoring the effect of labor demand adjustments on co-workers would lead to a 0 fiscal externality, indicating the first-order importance of such effects in the short-run. In the long-run, these costs fade away whilst structural benefits in terms of sustainability of public finances remain.

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1 Introduction

In the past three decades most OECD countries increased the statutory retirement age in an effort to restore the long-term financial sustainability of social security threatened by population aging. The U.S. is following this trend by committing to gradually adjust the full retirement age from 66 to 67 by 2022 and the Congressional Budget Office has also 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). There exists a rich literature about the labor supply effects of pension reforms that raise the early or full retirement age (e.g. Mastrobuoni (2009), Behaghel and Blau (2012), Staubli and Zweimüller (2013), Vestad (2013), Cribb et al. (2016), Seibold (2016), Lalive et al. (2017)). These papers document higher observed retirement ages and bunching at statutory retirement ages, suggesting that these policies are effective in encouraging elderly labor force participation. There is far less evidence on demand-driven labor market effects of this type of reforms. Insofar as higher statutory retirement ages induce workers to delay retirement, they increase the retention of senior employees who were on the cusp of retirement under previous rules. As a consequence of increased retention, firms may adjust labor demand.

This paper studies how firms respond to an increase in the retention of older workers caused by higher statutory retirement ages and how labor demand adjustments impact public finances. We employ previously unused administrative data and exploit the features of an Italian pension reform. In the first part of the paper we ask whether and how employers change their demand for labor, revealing the degree of substitutability between workers of different ages. In the second part we show that labor demand adjustments affect the earning trajectories and the take-up of other social insurance programs of the co-workers of retained senior employees. We then incorporate these demand-driven behavioral responses into the estimation of the fiscal externality of the reform, i.e. the share of mechanical savings in pension outlays that is lost due to behavioral responses. This improves upon the existing literature, which focuses on the behavioral responses of older workers only.

Addressing these issues poses two main identification challenges. First, most pension reforms are anticipated, which makes it hard to isolate firm's responses due to confounding anticipation effects. Second, the extent to which a firm is affected by higher statutory retirement ages in the short run depends on its share of older workers. Firms with a high concentration of younger workers cannot serve as credible controls since a firm's workforce demographics is strongly correlated with labor demand trends. We address both identification issues by exploiting the unanticipated *Fornero* pension reform enacted by a newly appointed technical government in Italy in December 2011. The reform entered into effect in January 2012, which left little room for anticipatory effects, and gradually raised age and contribution requirements for old-age and seniority pensions. It induced heterogeneous changes in years until pension eligibility across otherwise similar senior workers, depending on their gender, age and years of qualifying retirement contributions. We leverage this feature of the reform. Drawing on previously unexploited full contribution histories for private sector employees of small-medium firms, we compute each worker's expected retirement date under pre- and post-reform rules. We restrict attention to workers who were on the cusp of retirement under the old rules on the date the reform was passed. Depending on idiosyncratic differences in the gender, age and contribution histories within this narrow set of employees, employers face a different change in the average expected retirement date of senior workers at the firm. We use this change as our treatment and show that its intensity is not related to firm broad demographics or to other observable firm characteristics in the pre-reform period and, most importantly, it does not predict differences in labor demand pre-trends. This measure has also a direct economic interpretation as the change in the retention rate of retirees (measured in years). To ease the interpretation of our estimates, we also use the treatment to instrument for the number of senior workers whose retirement date is postponed by one year or more, which on the other hand is endogenous. We are therefore able to estimate the causal effect of retaining an additional older worker on firm labor demand.

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 labor demand of differentially treated firms before and after the reform. Based on matched employer-employee records, we look at firm labor demand adjustments along two main margins: separations from incumbent workers and external hiring. Our results document that senior retained employees, younger co-workers and external hires are at least partially substitutable. The degree of substitutability seems larger with middle-aged individuals in the same occupation group. Indeed, we find that more intensely treated firms fire more permanent employees in the post-reform period: an additional retained worker at the firm causes 0.06 more layoffs, which corresponds to a 13% increase with respect to the average number of layoffs pre-reform. Layoffs do not involve only senior employees who were expected to retire soon, but also middle-aged (aged 35-55) and other older (aged above 55) workers. Hiring is only modestly reduced (by 3.2% of the average pre-reform figure) and its decline is largely explained by reductions in new hires of temporary middle-aged and young (aged below 35) workers. The effect on both dismissals and hiring is concentrated on incumbent workers or external hires who share the same occupation group (blue-collar, white-collar or manager) as senior retained employees. We further show that firms only respond in the short-run to the increase in the retention rate of senior workers on the cusp of retirement, whereas the change in the residual working life of younger employees does not matter. As a consequence, the post-reform dynamics of dismissals and hiring exhibit an inverse U-shape and effects fade away as older workers eventually leave the firm at the end of our sample period.

In the second part of the paper, we study how the reform affects senior workers on the cusp of retirement and younger co-workers by looking at labor earnings as well as the take-up of other social insurance programs. We start by co-workers, associating each of them to the firm where they were incumbent at the reform date and pooling together co-workers who shared incumbency at the same employer, aggregating their labor earnings and income from various social insurance programs. We first document that co-workers who were incumbent in more treated firms exhibit worse labor earnings dynamics in the post-reform period. We estimate that retaining an additional old worker leads to a 18000 euros drop in total labor earnings, including non-work subsidies, collectively received by co-workers, equivalent to 1.6% of the average in the pre-reform period. Second, we show that the relative decline in earnings is moderated when we take into account non-work subsidies. This suggests that part of the observed earning dynamics reflects the effect of the increased hazard of being laid-off. To gauge which share of the earning decline can be imputed 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 document that separations explain around half of the earnings drop, while the other half depends on within-firm earnings dynamics. The latter matter more for middle-aged workers, whose earnings loss four years after the reform is mostly explained (70%) by wage patterns within the firm, contrary to what happens to young workers (only 30%). This evidence is consistent with a model where the job ladder at the firm is based on seniority with middle-aged workers being the closest substitute to retained older workers. Finally, we look at the take-up of disability benefits, short-time work subsidies and benefits related to periods of sickness and leave, finding smaller and not always significant effects.

We then replicate the same analysis on workers on the cusp of retirement. We show that when their retirement date is postponed by an extra year they receive higher non-work subsidies and are more likely to receive disability pensions as well as benefits related to period of sickness and leave. However, savings on pension entitlements are far larger than the costs borne by other social insurance programs. On top of that, due to delayed retirement these workers have significantly higher labor earnings, generating a positive revenue externality from their labor income taxes.

We conclude the paper by estimating the fiscal externality of the reform. While the literature has mostly focused on behavioural responses of senior workers only, we show that behavioural responses of co-workers triggered by labor demand adjustments are important and should not be ignored. We develop an accounting model that allows for spillovers on co-workers and substitution between social insurance programs. We use this framework to estimate the reform's fiscal externality. We find that from one-half to two-third of revenues generated by the reform are lost in the short-run due to the behavioral responses of firms and workers. This result implies that savings on pension outlays due to the increase in the full retirement age are larger than costs borne by other social insurance programs - because of program substitution - and by the tax authority - due to lower labor income earned by co-workers. Moreover, we show that we would estimate a zero fiscal externality if we ignored the spillovers on the co-workers, because higher taxes from increased labor supply offset costs associated to senior workers' increased take-up of other social insurance programs. This is evidence of the fact that labor demand is pivotal to assess the short-term consequences of pension reforms. In the long-run, these costs attenuate whereas structural benefits in terms of the sustainability of the public pension system remain.

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 how substitutable workers are.[1] While these papers leverage a negative shock to the retention rate, we study a positive one. Moreover, unlike a worker's death, our treatment typically involves more than one incumbent worker, thus providing a larger shock to a firm's workforce. As the shock only affects older employees (in the short-run), it allows to study complementarities across workers of different ages. We relate our findings to labor demand theory and contribute to the understanding of workers' substitutability within firms as studied in models with heterogeneous labor and imperfect labor markets (Cahuc et al. (2008) and Pissarides (2000)). To these, we add evidence on changes in internal labor market dynamics (Baker et al. (1994)).[2]

Many studies investigate how the generosity of one social insurance program affects enrollment in other

[1] Other recent examples are Nguyen and Nielsen (2010), Bennedsen et al. (2010) and Adam (2015).

[2] Doeringer and Priore (1970) argued that market-based mechanisms cannot fully account for the patterns of workers' careers within firms; instead, workers' careers resemble an internal labor market. See Gibbons and Waldman (1999), Lazear and Oyer (2013), and Waldman (2013) for surveys of the theoretical literature on internal labor markets, and Waldman (2003), DeVaro and Morita (2013), DeVaro and Gurtler (2016), and Ke et al. (2018) for models of internal labor markets emphasizing slot-constrained promotion opportunities. Gibbs and Hendricks (2004) provides a review subsequent work documenting patterns of wage and promotion dynamics inside firms.

programs.[3] Most closely related to our work is Staubli and Zweimüller (2013), which studies the spillovers of a reform increasing the early retirement age on other government programs. We contribute to this literature by showing that changes to social security rules can generate spillovers not only because of the behavioral responses of older workers, but also because of demand-driven adjustments that affect younger co-workers. We also extend the focus to all government programs that transfer resources to retained senior employees and their co-workers.

Our paper also relates to the literature on workforce aging, which has been mostly concerned with the effects on firm performance. Macro-level studies deliver mixed evidence, with the degree of complementarity between older and younger workers, wage setting mechanisms and country-specific labor market institutions each playing a crucial role.[4] Lallemand and Rycx (2009), Gobel and Zwick (2010) and Guest and Stewart (2011) provide evidence with matched employer-employee data that a mixed aged workforce enhances productivity.[5] Several papers have studied the relationship between the employment rates of older and younger cohorts. A recent but 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 contribution is twofold: we rely on idiosyncratic variation in treatment intensity unrelated to broad firm demographics and we extend the scope of the analysis to multiple labor demand margins and social insurance programs. Firm-level studies are complemented by macro evidence on the relationship between employment in younger and older cohorts. While Gruber and Wise (2010) conclude, based on country case-studies, that the correlation is positive, a 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 minimum retirement age have a negative effect on youth employment. Exploiting variation in the age structure of the old population across U.S. commuting zones, Mohnen (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 discusses the main findings on firms' labor demand adjustments; Section 6 presents a battery of robustness checks; Section 7 documents the effect of the reform on co-workers' earnings; Section 8 builds a model to estimate the fiscal externality of the reform; Section 9 concludes.

2 Institutional Setting

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

[3]Some examples are studies 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)). More recent works along these lines are Inderbitzin et al. (2016) and Kline and Walters (2016).

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

[5]Along these lines, Shimer (2001) shows that an increase in the 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.

2.1 The Italian Pension System

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

2.2 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.[6] Benefits are computed using a combination of defined-benefits (DB) and notional defined-contributions (NDC) methods.[7] The less generous NDC scheme applies to contributions accrued from 1996 onward, with the exception of workers who accrued more than 18 years of contributions by December 1995. For these workers, following the *Fornero* reform, the NDC method applies to contributions accumulated from 2012 onward. The private-sector social security tax rate is 33%, of which around one-third is paid by the employee and two-thirds by the employer.

There exist two pathways to claim full benefits upon retirement: old-age pensions and seniority pensions. Both feature a requirement 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. As explained in sub-section 2.4, the *Fornero* reform significantly raised requisites to claim both types of pensions. A pathway for early retirement (i.e. to claim some benefits before satisfying the old-age or seniority pension requirements) called *opzione donna* is available for women only. Similarly to early retirement in the US, *opzione donna* allowed to claim benefits roughly 4 years before the statutory age under pre-2012 rules.[8] Retiring early, however, comes at the cost of receiving sizeably lower pension benefits.[9] 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 some other European countries, there is no reduction in layoffs protection when a worker become eligible to retire.

2.3 Statutory and Actual Retirement Age

The relationship between statutory retirement age and actual retirement date is a crucial aspect to determine the transitional effects of pension reforms. Indeed, it determines the extent to which workers delay retirement and firms see an increase in the retention rate of senior employees when statutory retirement ages are raised. We show in our data a spike of retirement around the statutory retirement date. This trend is common to other countries such as the United States where the share of workers retiring at full retirement age (FRA) has been increasing in the last decade and the share of workers claiming early retirement has starkly

[6] Since the public pension system was quite generous until the last decade, additional occupational pension plans are not widespread. Only around 7.3 million people - one-fourth of the workforce - had private pension plans in 2015, but the number has been growing significantly in the last years (COVIP (2015)). There exist both closed pension funds for companies or industries and open pension funds typically offered by banks and insurance companies. Almost all funds operate on a DC basis.

[7] 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 first capitalized based on a 5-year moving average of the nominal GDP growth rate and then transformed into yearly benefits using a transformation coefficient that depends on gender, age at retirement and life expectancy.

[8] Conditionally on having 35 years of contributions, early retirement for women was possible possible in 2011 upon turning upon turning 57 years old. The age requirement was later gradually adjusted upward to 57 years and 3 months, and then to 57 years and 6 months.

[9] The main reason is that pension benefits under *opzione donna* are computed applying the NDC regime also to contributions accrued before 1996.

dropped (Munnell and Chen (2015)). Estimates in Mastrobuoni (2009) for the United States document a strong elasticity of actual retirement date to FRA: an increase in the FRA by 2 months delays effective retirement by around 1 month.[10] A large and similar elasticity also emerges from our data, where a one year shift in the retirement date translates in a little more than 6 additional months of working life (see Section 4.4).

2.4 The Fornero Reform

On December 2011 the *Fornero* pension reform was passed as part of the "Save Italy" decree, an emergency package of measures in response to the increasing pressure of financial markets on Italian sovereign debt. Designed by a new government and approved three weeks after its appointment, it entered into force in January 2012. Although the need for a deficit reduction package was anticipated, its exact content was not known in advance. Moreover, the decision and implementation lags were both very short. As a result, anticipation effects were likely negligible. The reform raised age and contribution requirements to claim old-age and seniority pensions, effectively reducing the number of new retirees and increasing the average age at retirement.[11] 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, which we list in Appendix C, maintained the right to retire under pre-reform rules. For all other workers, Table 1 compares age and contribution requirements for old-age (Panel A) and seniority (Panel B) routes to retirement under pre-reform rules - had they remained in place - and under the rules designed by the *Fornero* reform, over the period 2012-2018.[12]

Old-age pensions: The reform progressively 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 smoothly risen to reach 61 years and 10 months for women and 65 years and 7 months for men. Following 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 therefore substantially larger for women than for men.[13]

Seniority pensions: The reform substantially re-designed the rules for claiming seniority pensions. Until 2011 a "quota" system was in place: 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, with the requirement of 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 have risen from 96 to 97.3 and later to 97.6 over the 2012-2108 period.[14] The

[10] Mastrobuoni (2009) lists as important factors for the timing of retirement social custom or liquidity constraints. Previous studies have documented that health (e.g., Dwyer and Mitchell (1999); McGarry (2004)) and previous job characteristics (e.g. Hurd and McGarry (1993)) are important determinants of the retirement decision, too.

[11] Figures A1 and A2 in Appendix G plot average age and retirement volumes by gender and pathway to retirement.

[12] Table 1 reports old and new requirements as they were written in the law and taking into account the anticipated upward adjustments due to increased life expectancy that took place in 2013 and 2016.

[13] 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.

[14] 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 be raised to 97.6. The contribution requirement would have been maintained at 35 years.

Fornero reform abolished the "quota" system and legislated that a seniority pension could only be claimed upon totalling not less than 41 years of contribution for women and 42 for men, with this thresholds increasing over time.[15] Workers planning to retire in the coming years under the "quota system" faced therefore a substantial increase in years until pension eligibility, up to 6-7 years.

The reform did not change the features of the pathway to early retirement available for women. Because of the cut in benefits, the take-up of the early retirement option was very low before the *Fornero* reform; after the reform, which raised requirements more heavily for women, the take-up significantly increased. Yet, even in the year when it peaked (2015), less than 20% of eligible women exercised the early retirement option. Moreover, only 80% of them made job-to-retirement transitions, the others being unemployed or out of the labor force when they retired (INPS (2016)). As a result, the take-up of such option remains limited in our sample contributing to a high elasticity of retirement choices to the full retirement age.

Because of the schedule of pre- and post-reform rules, the reform generated different changes in years until retirement eligibility among otherwise similar older workers. Firms with a similar senior workforce can therefore be affected by the reform to a different extent. As a first example, consider workers born in 1951 to 1953 who continuously contributed to social security since age 23, thus having 37 years of contributions upon turning 60. Those born in 1951 were not affected by the reform, while those born in 1952 faced a 4-year and 7 month increase in years until pension eligibility.[16] Workers born in 1953 were subject to a change ranging from 2 years and 7 months to 4 years and 7 months.[17] As another example, consider two women born in February 1952 who, due to several career interruptions, accrue 20 years of contributions at the end of 2014 and 2015, respectively. Under old rules, they would have become eligible for an old-age pension as soon as meeting the 20-year contribution requirement; under new rules they both become eligible in December 2015, upon turning 63 years and 9 months old. While the former face a 1-year increase in years until retirement eligibility, the second faces no change.

3 Data

We leverage high-quality and restricted-access administrative data available at the Italian Social Security Institute (INPS).

Matched employer-employee records: matched employer-employee records are available for the universe of non-agricultural firms with at least one employee, over the period 1983-2015. By filling the so-called UNIEMENS modules, firms have to report detailed information about employees covered by Social Security. The data covers 74% of private employers in Italy and 93% of private sector employees.[18] We use monthly

[15] 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 start working in 1996 or after have also 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.

[16] Workers born in 1951 could claim a seniority pension in 2011. Those born in 1952 could have claimed a seniority pension in 2012 under old rules, whereas they have to wait and turn 64 years and 7 months old under new rules.

[17] Assuming uninterrupted contributions from 2011 onward, male (female) workers born on January 1953 can claim a seniority pension in November 2017 (2016) under new rules, while they could have claimed a seniority (old-age) pension in April 2014 (2013) before. Men (women) born on December 1953 can claim a seniority pension in October 2019 (2018) under new rules, while they could have claim a seniority (old-age) pension in March 2015 (April 2014) under old rules.

[18] Self-employment covers most of the share of total private employment that we are missing, while the agricultural sector accounts for most of the missing share of private sector employees.

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; 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 can also link these records with 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 labor demand adjustments, ranging from hiring to firing of permanent workers and terminations of temporary contracts, both overall and across different categories of workers, as identified by their contract, occupation or demographic group.

Workers' Contribution Histories: Previously unexploited full contribution histories are available for all employees who have worked at some point 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. The recorded information includes: the number of qualifying weeks contributed, which is needed for computing the contribution requirement for both old-age and seniority pensions; the event triggering the payment of contributions (e.g. paid work, maternity leave, sickness leave, unemployment benefits) and the monetary value of the contribution.[21] Exploiting this information, we can construct comprehensive measures of earnings, which also include labor income from quasi-salaried employment, self-employment and public sector jobs. Furthermore, we can observe the take-up of social insurance programs, including unemployment and short-time work 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

We aim to build a treatment that captures the extent to which a firm is affected by the *Fornero* reform in the short-run. To this end, we compute the change in the expected retirement date for employees who were close to retire under pre-reform rules, who we henceforth define with the shorthand *CTR* workers (sub-section 4.1). Since the firm is our unit of investigation, we then aggregate individual changes to create a firm-level treatment that reflects the average variation in the expected retirement date of *CTR* workers employed at the firm when the reform is passed (sub-section 4.2). The treatment varies across firms due to idiosyncratic differences in the joint distribution of gender, exact age and years of contribution among the *CTR* workforce. We exploit such an identifying variation within a dynamic difference-in-difference estimation setting (sub-

[19] INPS collects matched employer-employee records with an annual frequency since 1983 and with a monthly frequency since 2005. Given that our analysis spans the period 2009-2015, we mostly use the latter dataset, while relying on the former to compute worker-level measures of experience and tenure.

[20] Specifically, we consider firms with 3 to 200 employees in the first quarter of 2009. The restriction stems from limitations on the maximum number of workers' contribution histories that could be made available by INPS for the sake of the project. We also choose to focus on small-medium firms because they are the most representative of the Italian productive landscape.

[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.

section 4.3).

4.1 Retirement Date Construction

For the purpose of computing predicted retirement dates a senior 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 of the triplet and on contribution histories to compute total years of contributions by the end of 2011, following the rules detailed in Appendix E. For every type θ we compute the reform-induced change in years until full retirement, thus excluding routes to early retirement. Insofar as early retirement choices are influenced by the reform, δ_θ represents an individual assignment to treatment as opposed to the actual change in the retirement date. Practically, we compute the predicted retirement dates according to old rules - had they remained in place - and new rules. We then take the difference between the latter and the former, which we denote with δ_θ:

$$\delta_\theta = \text{Years until retirement}_\theta^{new} - \text{Years until retirement}_\theta^{old} \tag{1}$$

To construct δ_θ, we take as given 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 pathway to retirement[22]

Assumption (i) requires that individuals work year-round and full-time in the post-reform period or that non-work periods are entirely covered by *figurative* contributions (see section 3). Data show 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 pension benefits. Within each regime, we compute the predicted retirement date associated to every available pathway and we select the earliest one. This also implies that the chosen pathway may change because of the reform, as there exist types θ for which the earliest exit date was associated to the old-age route under pre-reform rules, whereas it switches to the seniority route under post-reform rules, or viceversa. As discussed in section 1, an extensive literature has documented that retirement patterns display bunching at the acquisition of full pension rights.

4.2 Treatment Construction

The reform applies to all workers who did not qualify for either an old-age or a seniority pension under old rules by 2011. As we are interested in short-run labor demand adjustments, we primarily aim to capture the change in the retention rate of senior workers who were on the cusp of retirement under old rules. To

[22] 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. While overlooked by many commentators, the removal of the "waiting window" *de-facto* nullified the change in the age requirement for the male old-age pathway to retirement, whereas changes to other pathways remained substantial. We therefore 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.

this end, we classify as *CTR* workers those full-time employees who could have retired within 3 years, i.e. by 2014, under old rules. The three-year 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 their residual working lives because of the reform.[23] This is visible in Panel A of Figure A3 in Appendix G, which plots the distribution of the change in years until retirement among *CTR* workers: it displays a sizable variability, with the mean being 1.38 and the standard deviation being 1.42.[24] Furthermore, we show that the retention of senior workers farther away from retiring under old rules does not matter for short-run firm responses.

For *CTR* workers who actually retire between 2012 and 2015 we can compare the actual and predicted retirement date, to assess the validity of our assumptions. Panel A of Figure 1 plots the distribution of the "forecast error". The majority of forecast errors (63%) lies within a 1-year window, suggesting that our measure is quite accurate in predicting the actual retirement date. It also provides additional supportive evidence to assumption (ii), since 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 of forecast errors between -1 and -3 years. This likely arises because of two main reasons. First, as we show in our results, a portion of *CTR* workers is fired in the aftermath of the reform, triggering incentives to early retirement. Second, some workers maintain the right to retire under pre-reform rules (see Appendix C) and the pathway to early retirement is still available for women. Indeed, Panel B of Figure 1 shows that the forecast error is smaller for males than for females.

As discussed before, the reform induces a shift δ_θ of the expected retirement date, which is the same to every *CTR* worker belonging to the same type θ. To construct the firm-level treatment, we build a shift-share shock that weights the δ_θs by the share of every type θ in the *CTR* workforce employed at the firm. We have:

$$T_i = \sum_{\theta \in CTR} \pi_{\theta,i} \delta_\theta \tag{2}$$

workers of type θ in the *CTR* workforce employed at firm i in the last quarter of 2011. Notice that such shares depend neither on firm size nor on the share of *CTR* workers over the total firm's workforce. As a consequence, we show later that they are not correlated with the broad demographic composition of the firm. The treatment T_i has a straightforward economic interpretation. It measures the change in the retention rate (expressed in years) of *CTR* workers. It also captures the average shift in the retirement date of *CTR* workers. The distribution of T_i among firms that employ at least one *CTR* worker displays significant variability, with mean 1.40 and standard deviation 1.37 (Figure A3 in Appendix G).

[23] 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). Therefore, we exploit the variation in retirement dates coming from workers who would have retired in 2013 and 2014.

[24] For *CTR* workers predicted retirement dates 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, as explained in sub-section 4.1, due to the abolition of the waiting window few workers face a negative change, i.e. can retire sooner under new rules.

4.3 Empirical Specification and identifying assumptions

Empirical specification: We estimate a generalized difference-in-difference model featuring a continuous treatment and multiple pre- and post-reform periods. The baseline regression specification reads:

$$Y_{it} = \alpha + \lambda_i + \sum_{k=2009}^{2015} \beta_k \gamma_k + \sum_{k=2009}^{2015} \beta_k^T \gamma_k \times T_i + \varepsilon_{i,t} \tag{3}$$

where i indexes the firm and t indexes the year. Y_{it} is the outcome of interest. T_i indicates the firm-level treatment, which is interacted with a set of year fixed-effects $\{\gamma_k\}_{k=2009}^{k=2015}$. λ_i is a firm fixed-effect, which 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)).[25] The coefficients of interest are $\{\beta_k^T\}_{k=2009}^{k=2015}$, which show how the treatment affects more and less treated firms in year k relative to the reform year.[26]

In the baseline specification, we restrict our attention to the balanced sample of firms with 3-200 employees in the baseline quarter (q1-2009) which remain open throughout the period. Moreover, we focus on firms that employ at least one *CTR* worker in the quarter when the reform is passed (q4-2011). We restrict the analysis to these firms for internal validity. Firms with no *CTR* workers may not be an appropriate control group as they feature a different workforce 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.

Identification assumptions: We leverage variation in the characteristics of the *CTR* workforce for identification. Equation (2) shows that firms are affected to a different extent by the reform because of idiosyncratic variation in the shares of each type θ among their *CTR* workers. Formally, identification requires that the $\pi_{\theta,i}$s in (2) are not correlated with firm's unobservable time-varying characteristics (Goldsmith-Pinkham et al. (2017) and Borusyak et al. (2018)). This is equivalent to say that once a firm employs at least one *CTR* worker, the characteristics of the *CTR* workforce must not be correlated with firm's time-varying unobservable characteristics. Because $\pi_{\theta,i}$s depend neither on firm size nor on the number of *CTR* workers, we avoid treatment variation generated by the pre-reform overall demographic composition of the firm's workforce, which would be endogenous. We only exploit idiosyncratic differences in gender, age and years of contributions (i.e. in types θ) in the sub-sample of *CTR* workers. We can derive suggestive evidence of the exogeneity of our treatment by looking at pre-trends as captured by the pattern of coefficients $\{\beta_k^T\}_{k=2009}^{k=2011}$ in the pre-reform period. If trends are parallel in the pre-reform period, as required in a difference-in-difference setting, these coefficients should not be significantly different from 0. The absence of differential pre-trends is a necessary condition for the treatment to be plausibly exogenous. To further assess the validity of the identifying assumption, we also perform placebo tests and balancing tests.

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). Specifically, we artificially assign the date in which the reform first becomes effective to 2010 or 2011, rather than to 2012. We then estimate specification (3) on the pre-reform

[25] We also run specifications where we cluster at the province $\times$ two-digit sector level and results are virtually unchanged.
[26] For identification we impose $\beta_k^{2011} = 0$.

period only. We look at the effect of the placebo treatment on layoffs and new-hires, which are the main firm-level outcomes we study in Section 5. Table 2 shows that, as we should expect, the treatment has zero effect, indicating that there are no differential trends in labor demand for more and less treated firms.

Balancing tests: The distribution of age and years of contribution among the senior *CTR* employees is not necessarily informative about the distribution of these two variables in the overall workforce. The share of females among *CTR* workers, on the other hand, is more likely to reflect the overall gender composition of the firm. Since women are on average affected by the reform more than men (see sub-section 2.4), this could raise the concern that the treatment is systematically higher in firms who employ more female workers. In order to check this, we run a balancing test whereby we regress the share of male employees at baseline on the treatment. We replicate the same exercise on a rich set of firms' baseline characteristics. Table 3 reports the results. Column 1 displays results from a specification with no controls, whereas column 2 displays estimates that control for province fixed effects, sector fixed effects, as well as province $\times$ sector fixed effects; column 3 shows the mean of the outcome variable. The relationship between the treatment and the baseline characteristics of the firm appears to be very weak, although it is very precisely estimated. Notably, this holds true for the workforce' s age structure and gender composition. As the treatment increased by 1σ, the proportion of old workers (aged above 55) increases by 0.011, against an average of 0.123. The corresponding figures for the share of male workers are -0.034 and 0.658, respectively.

Treatment bite: The placebo and balancing tests presented above show that the treatment arguably captures a change in years until retirement of the CTR workforce that is not correlated with other firm characteristics. This may affect firms' labor demand if it actually translates into longer working lives for these senior workers. To check this, we run an individual-level version of specification (3) on the sample of *CTR* employees, where the treatment and the fixed effects are at the individual rather than at the firm level.[27] Figure 2 shows that postponing the retirement date by one year causes a decline in the number of months spent on retirement (1.5 in 2013). The effect increases over time as most *CTR* workers - who were eligible to retire by 2014 under old rules - would have continued working in the first post-reform years also under pre-reform rules. The observed decline in months spent on retirement is smaller than the one that would occur if all workers would retire at the date that we predict in sub-section 4.1. This can reflect both workers' decision to retire early or firms' decision to separate from their senior employees. Symmetrically, the number of months spent at work increases.

To quantify the elasticity of actual retirement to the shift in the full retirement date we also regress the difference between the actual retirement date and the predicted full retirement date under pre-reform rules on the individual-level change in years until retirement.[28] A 1-year raise in the full retirement date delays retirement by 6 months in the sample of *CTR* workers who retired by December 2016 (Table A3). The elasticity is lower for women (5.24 months against 6.57 for men), who have access to early retirement options.

[27] Furthermore, we augment the specification with the interaction of sex and age fixed-effects with time fixed effects. The residual variation therefore leverages idiosyncratic differences of contribution histories.

[28] The specification also include age, gender, province and sector fixed effects.

4.4 Sample

As discussed in sub-section 4.3, the master sample consists of a balanced panel of 64721 firms in the 3-200 size class in the baseline quarter (q1-2009), which remain open throughout the period and employ at least one CTR worker in the quarter when the reform is announced (q4-2011).

Table 4 compares the baseline characteristics of firms in the master sample to other firms in the same size class at baseline (3-200) and that also remain open throughout the period. Firms with at least one *CRT* worker 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, average gross daily wages are higher.

Table A1 in Appendix G compares *CTR* workers to other full-time workers employed in firms belonging to the master sample. As expected, *CTR* workers are older, more experienced and more tenured. They are slightly more likely to hold either blue-collar or managerial positions. A larger share has a permanent contract and the gross daily wage is higher. We also compare *CTR* workers to same-age senior employees who are not on the cusp of retirement, but who are otherwise similar along many dimensions.[29] We focus on absence patterns in 2011, the last pre-reform year. Table A2 in Appendix G shows that *CTR* workers 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 leave. This translates into higher monetary costs associated to these events. This evidence is suggestive that, as documented in the literature (Dostie (2011), Borsch-Supan and Weiss (2016) and Avolio et al. (1990)), employees who approach the end of their working lives reduce their effort. One possible reason could be the declining bite of career concerns.

5 Firm-level responses to the reform

In this Section we document how firm labor demand is affected by an increase of the retention rate of CTR workers, as captured by treatment T_i. We estimate specification (3) on the master sample of firms described in sub-section 4.4. For each outcome of interest, we plot the coefficients $\{\beta_k^T\}_{k=2009}^{2015}$, along with 95% confidence intervals. They show the effect on the various outcomes of interest of a 1σ increase in the intensity of the treatment, amounting to 1.38 extra-year until retirement per *CTR* workers, in a given year k relative to the reform year.[30]

We focus on layoffs and new hires as margins of labor demand adjustment. According to standard labor demand theory, evidence of a drop in labor demand caused by the retention of old workers can be reconciled with complementarity between younger and older cohorts only in case of a large increase in the wage of younger workers. As we document below, the labor demand of firms that are more affected by the reform drops. Moreover, a large increase in younger workers' wages is inconsistent with the evidence we provide in Section 7, which shows a drop in the earnings of younger cohorts. We conclude that the younger cohorts are substitutes for old workers retiring. Our evidence also excludes patterns of no substitutability between

[29] Specifically, 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.

[30] The coefficients $\{\beta_k^T\}_{k=2009}^{2015}$ are also reported in Tables A4 and A5 in Appendix G.

workers. Indeed, if this was the case, a drop in demand could not be explained by decreasing wages for younger cohorts. Because our results document a larger response of labor demand for middle-aged and old non-*CTR* workers relative to young (under 35) employees, we conclude that the former two cohorts are the closest substitutes to workers retiring. We develop a simple model of labor demand for different cohorts with some extensions to incorporate alternative wage bargaining models in Appendix A.

5.1 Layoffs

Panel A of Figure 3 shows that firms significantly increase layoffs of permanent workers in response to an unexpected change in the retention rate of *CTR* workers.[31] While no significant difference in the layoffs pattern of more and less treated firms is present before the reform (2009-2011), it emerges in its aftermath. The number of workers fired gradually rises over the period 2012-2014 up to 0.06 in 2014. This number is small in absolute terms, but it amounts to 13% of the pre-reform average of layoffs per year (0.43). The inverse u-shaped pattern of post-reform coefficients reflects the dynamics in the retention of *CTR* workers. Most *CTR* workers would have been working at the firm in 2012 under pre-reform rules when zero effect is detected. At the same time, when the number of unexpectedly retained workers rises in 2013 and 2014 we observe an increase in layoffs. As some *CTR* workers eventually retire in 2015, the number of workers fired because of the reform declines. Panel B of Figure 3 breaks down the effect by workers age. We define young the workers aged below 35, middle-aged the workers aged between 35 and 55, and old the workers aged above 55. Layoffs increase across all age groups, but to a greater extent among middle-aged and senior employees. In 2014, for instance, layoffs of middle-aged workers increase by 0.027 units and layoffs of old workers increase by 0.018 units, against an average of 0.23 and 0.06 firing per year in the pre-reform period. This implies a remarkable 11% and 30% spike, respectively. As expected, the strongest reaction to the shock is concentrated on old workers. However, some spillovers on other age groups arise and provide evidence of substitutability across age cohorts. A stronger response in the layoffs of middle-aged workers relative to young worker is consistent with the idea that such workers are the closest substitute to senior workers within the firm. Figure 3 also plots the pattern of old-age workers' layoffs when excluding *CTR* workers. Smaller coefficients show that part of the effect is borne by *CTR* workers directly. Since the coefficient halves and the share of *CTR* workers out of workers aged above 55 is around 65%, they are not however disproportionately affected relative to other senior employees.

5.2 New Hires

Panel A of Figure 4 displays the effect of the reform on new hires. Firms more affected by the reform appear to modify their hiring schedule, without appreciable differential pre-trends. A 1σ increase in treatment intensity is associated with a drop in hiring of up to 0.17 units per year over the period 2012-2014. Against an average of 5.23 new hires per year, it amounts to a modest 3.2% drop. Hiring recovers starting from 2015, when the coefficient is virtually zero. The u-shaped pattern of post-reform coefficients is consistent with firms

[31] We have also evaluated the effect of the reform on firings of fixed-term workers - that we do not report here for brevity - and we found no discernible effect. This is consistent with the fact that labor regulations force firms to pay a temporary worker until contract expiration if she is fired for economic reasons. Therefore, the cheapest way to part from a temporary worker is not to renew her contract at expiration. Both pre- and post-reform, firms have no incentive to fires employees under fixed-term contracts.

delaying their hiring schedule in response to tighter retirement rules: new hires drop in the aftermath of the reform and start bouncing back as *CTR* workers become eligible to retire under new rules. Panel B decomposes the effect by new hires' age. In unit terms, the drop is mostly and equally borne by young and middle-aged workers. Over the period 2012-2014, young and middle-aged new hires decline up to 0.085 and 0.077 units per year respectively, accounting for roughly 95% of the observed drop in hiring. In percentage terms, out of the average hiring per year before the reform, the drop is equal to 3.2% for young workers and 3.4% for middle-aged workers. As in the case of layoffs, middle-aged workers experience a larger relative adjustment. The drop in hiring is entirely borne by workers hired under fixed-term contracts (Figure A4 in Appendix G). Since firms typically hire junior workers under temporary contracts, the substantial heterogeneity observed between the two types of contract is consistent with the fact that the effect on new hires is concentrated among young and middle-aged workers. In 2015, the virtually null coefficient on total hiring in Figure 4 masks substantial heterogeneity: more affected firms still hire fewer workers on fixed-term contracts, but they hire more workers on permanent ones.[32]

5.3 Which *CTR* workers matter more?

As discussed in Section 2.4, all workers who do not qualify for retirement under old rules by the end of 2011 experience an increase in years left to pension eligibility. We argued that, in the short-run, the most proximate consequence for the firm is the unexpected increase of the retention rate of workers who were on the cusp of retirement under old rules, had they remained in place. We directly 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 do so by including in specification (3) two different treatment variables. The first is the average change in the retirement date computed on the sample of workers who were expected to retire by 2014 (i.e. the definition of *CTR* employees), the second is the average change on the sample of workers expected to retire in 2015 or 2016. Figure 5 shows that the first treatment generates the entire effect on layoffs and new hires, while the second treatment has virtually no impact. This result supports the view that firms' short-run responses depend only on the longer than expected retention of workers on the cusp of retirement and restricts the sample of workers who matter for capturing firm's responses. It also suggests that the effect of the reform is transitory on firms, as they increase layoffs and decrease new hires only to absorb a shock to the small set of workers who were expected to retire soon.

5.4 Response heterogeneity by occupation and turnover

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 set of workers in a specific occupation (blue-collar, white-collar or manager) and we only consider firms where there are at least two units that employ at least three workers each, although estimates that lift this restriction are virtually unchanged. We investigate whether labor demand responses are concentrated among co-workers or job applicants who share the same qualification as *CTR* employees. We estimate a version of (3) at the firm-unit level where we separately include a direct treatment defined on the *CTR* workers of the unit and an indirect treatment that measures the shock to all

[32] A generous package of incentives for fostering permanent contracts was in place in 2015. As more affected firms had been hiring fewer workers in the previous years, this pattern might reflect them exploiting more heavily such incentives as *CTR* workers start to retire.

other units within the same firm. Figure 6 plots the results of this exercise. Remarkably, only the within-unit treatment generates an effect on layoffs and new hires. Retaining a senior worker for longer than expected impacts the number of layoffs and new hires for same-occupation co-workers and job applicants, whereas it has only a very limited effect on these outcomes for other units within the firm. This result is line with the finding in Jäger (2016) that workers substitutability is larger within occupations. It also shows that the reform affects only a restricted sample of workers and its effects are not widespread across the entire firm's workforce.

Turnover: Firms with higher propensity to separate from workers should feature a more flexible management of the workforce and being more used to adjust along the employment margin to absorb shocks. We construct a measure of firm turnover defined as the share of separations over the total workforce in the period before the reform is enhanced. We label a separation as either a layoff, a non-renewed contract, or a voluntary quit. We then split firms into two groups based on whether they fall below or above the median of the distribution of the pre-reform turnover measure. Results are identical when we employ tertiles or quartiles of the distribution to create the groups. We estimate a triple difference specification that reads:

$$Y_{it} = \alpha + \lambda_i + \sum_{k=2009}^{2015} \beta_k \gamma_k + \sum_{k=2009}^{2015} \beta_k^T \gamma_k \times T_i + \sum_{k=2009}^{2015} \beta_k^{to} \gamma_k \times TO_i + \sum_{k=2009}^{2015} \beta_k^{T,to} \gamma_k \times T_i \times TO_i + \varepsilon_{i,t} \quad (4)$$

where TO_i is a dummy taking value 1 when the firm's turnover measure lies in the top half of the variable distribution. Most of the effect on layoffs is explained by high-turnover firms (Figure 7), while almost no effect can be detected on low-turnover firms. This is consistent with the fact that high-turnover firms are more flexible in their workforce decisions and easily manipulate the layoff margin. It also suggests that the workers facing a higher layoff probability as a consequence of the reform are those who already expect a higher probability of separation.

5.5 Reinterpreting the magnitudes

While the treatment T_i has the advantage to leverage as source of variation idiosyncratic differences among CTR workers which are not correlated with other firms' characteristics, it is also interesting to measure the effect of retaining one additional CTR worker. We propose an IV strategy whereby we instrument the number of retained CTR workers with T_i. This is motivated by the fact that the number of such workers is clearly strongly correlated with the size and the age structure of the firm, thus potentially capturing the effect of differences along these dimensions rather than the impact of the reform. Specifically, we define as retained a CTR worker whose retirement date is shifted forward by one year or more, so that the total number of workers retained in firm i is:

$$R_i = \sum_{j:j \in CTR_i} I(\delta_j \geq 1) \quad (5)$$

where δ_j is the change in the expected retirement date of individual j. Figure A5 in Appendix G plots pre-reform coefficients obtained by estimating specification (3) and using R_i as regressor. While differential trends are not visible on layoffs, they emerge on new hires: firms that retain more CTR workers hire less in the pre-reform period and as a consequence their workforce tends to be older.[33] This confirms the need to adopt an IV strategy. Figure A6 in Appendix (3) shows the results of the IV exercise: the coefficients have the same pattern as in the baseline specification, but the magnitude is of course different.

To give a more concise measure of the effect of retaining an additional *CTR* worker, we also estimate the IV specification by collapsing all periods pre- and post-reform so as to obtain a standard difference-in-difference model that reads:

$$y_{it} = \alpha + \lambda_i + \sum_{k=2009}^{2015} \beta_k \gamma_k + \beta^T Post_t \times R_i + \epsilon_{it} \tag{6}$$

where $Post$ is a dummy that takes value 1 after 2011 and R_i is instrumented with T_i.

Table 5 reports the results: following the retention of an additional *CTR* worker, layoffs increase by 0.11 units, amounting to 25% of the average number of layoffs in the pre-reform period. Layoffs significantly increase for all cohorts of workers. It is confirmed that middle-aged and senior workers are affected the most: the former in absolute terms, the coefficient being equal to 0.056 (24% of the pre-reform average), while the latter in relative terms, as 0.026 more dismissals amount to 43% of the pre-reform average. These results confirm the substitution patterns between *CTR* and over 35 workers that we observed in the previous analysis. We also observe adjustments on the number of new hires. Total new hires drop by 0.21 units (4% of the average pre-reform) in response to an additional *CTR* worker retained, although the effect is not statistically significant. The largest drop in absolute and relative terms occurs for middle-aged workers.

6 Sensitivity checks

We conduct several sensitivity checks to test the robustness of the results. Firm fixed effects in the baseline specification (3) do not control for time-varying pre-existing differences across firms. While Table 3 has shown that the correlation between the treatment and firm characteristics is very weak, we further address this concern by estimating an augmented specification that reads:

$$Y_{it} = \alpha + \lambda_i + \sum_{k=2009}^{2015} \beta_k \gamma_k + \sum_{k=2009}^{2015} \beta_k^T \gamma_k \times T_i + \sum_{k=2009}^{2015} \delta_k \gamma_k \times X_i + \varepsilon_{i,t} \tag{7}$$

where X_i is a vector of firms' characteristics at baseline, each of which is interacted with a set of year fixed effects. The augmented specification allows for differential trends in firms with different treatment intensity as long as these trends can be entirely predicted by baseline characteristics.

Figure 8 shows that results are robust to the inclusion of a rich set of controls. First, since new retirement rules on average affect women to a greater extent than men, we include dummies for the quintiles of the share of female employees, to reduce the concern that our estimates capture differential labor demand trends across firms with a different gender composition. 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 enriched with year fixed effects interacted with two-digit sector and province fixed effects, to

[33] A caveat about the exclusion restriction assumption arises. We must require that a change in retention has no effect on firms responses other than through the number of retained workers. This would not be the case if the heterogeneity among retained workers mattered for the effect on firms' choices, e.g. in a case where firms responded differently to the number of retained white collar workers and the number of retained blue collar workers.

check that our estimates are not capturing different trends in labor demand driven by heterogeneous economic cycles across sectors and provinces. Fourth, we add one year to the pre-reform period to confirm that labor demand trends were similar up to four years before the reform was implemented. For internal validity, our baseline specification leverages variation in treatment intensity among firms that employ at least one *CTR* worker in the last quarter of 2011. Finally, we check that estimates are robust to the inclusion of the universe of firms, by setting $T_i = 0$ if a firm employs no *CTR* workers. Despite they do not employ any *CTR* worker, these firms do not show differential trends in the pre-reform period. Moreover, the post-reform estimates are virtually identical to our baseline estimates suggesting that the baseline specification was close to capturing the effects on the outcomes in the entire economy.

We also check whether results on layoffs could be affected by a change in the dismissal discipline that was implemented in 2013 and that led to a reduction of the cost of layoffs for firms with more than 15 employees. Before 2013 if an employee was dismissed for economic reasons, the employer was forced to either reinstate the worker or pay 15 months of salary, depending on the employee's choice. Since 2013 the employer is only liable for the reparation and is not mandated to reinstate the worker. This change has partially reduced the uncertainty around the cost of dismissals. We check whether firms just below and above the threshold of 15 employees do not behave differently. Appendix Figure **??** shows that the percentage increase in layoffs is virtually identical for firms below 15 employees and firms between 15 and 30 employees. Therefore, changes in the cost of layoffs do not seem to have an impact on the firing behavior in the post-reform period.

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

In this section we study how lengthening working lives of *CTR* workers affect them as well as their co-workers. We focus the analysis on labor earnings and the take-up of other social insurance programs. The literature has documented that senior workers may substitute away from pension benefits into other social insurance programs following reforms that change retirement rules (e.g.Duggan et al. (2007)). Possible effects on younger co-workers' take-up of other social insurance programs, due to firm labor demand adjustments, have however received much less attention. Tax revenue externality can emerge as well insofar as pension reforms affect labor supply of both senior employees on the cusp of retirement and younger co-workers.

7.1 Co-workers

We define as co-workers all full-time non-*CTR* workers who are employed at a firm where there is at least one *CTR* individual in the quarter when the reform is passed. We match every such co-worker to the firm where she was incumbent at the time of the reform. We then estimate specification (3) using as dependent variable labor earnings - including income from other private employers, self-employment or public-sector employment - collectively earned by co-workers who share incumbency at the reform date in a given firm.[34] Panel A of Figure 9 shows that co-workers who were incumbent in more treated firms see in the aftermath of

[34]Because we focus on worker-level outcomes we do not require that firms remain open throughout the period. For this reason, the number of observations is larger than in the previous analysis. Results are unchanged if we use the balanced sample of firms that are always active between 2009 and 2015. Furthermore, the regression features weights based on firm size at the baseline quarter. While in section 5 we aim to capture the effect of the reform on the average firms, in this section we aim to obtain results that are representative for the entire economy, as they are then used to compute the fiscal externality of the reform in Section 8.

the reform a relative decline in their earnings, compared to non-CTR incumbents in less treated firms. The loss associated with a 1σ increase in the treatment grows over time, reaching 11395 euros in 2015 (1.8% of the pre-reform average).[35] Panel A of Table A6 in Appendix G estimates the IV specification (6). When one additional CTR worker is retained, total labor earnings by incumbent co-workers fall by around 18000 euros in the post-reform period. Breaking down results by age of the worker, middle-aged workers emerge as the most affected. Panel B of Figure 9 reports results when adding non-work subsidies to labor earnings. Notably, the decline becomes smaller. According to the IV specification (Panel B of Table A6) the retention of one additional CTR worker is associated to a decline in the sum of labor earnings and non-work subsidies that amounts to roughly 10000 euros.

The fact that the decline in earnings moderates after accounting for non-work subsidies, combined with the stronger effect found among middle-aged workers, suggest that this pattern is partially explained by the increase in layoffs documented in sub-section 5.1. Within-firm dynamics could also play a role, in case incumbent co-workers experience wage cuts or a slower earning growth as senior CTR employees remain at work. To gauge the relevance of separations relative to within-firm dynamics, we perform a decomposition exercise whereby we combine estimates on the total number involuntary separations caused by the reform with estimates about the earnings drop experienced by job losers.[36] The ideal experiment to estimate the causal effect of loosing a job would randomize such an event across workers and compare separated workers' to the control group of non-separated workers. In absence of such an experiment, we take every worker experiencing a separation in the post-reform years and perform a coarsened exact match to non-separated workers along several covariates.[37] We then assess the cost of separation by performing a difference-in-difference analysis where the treatment is a dummy equal to 1 if the worker experiences a separation and where we control for the matching covariates interacted with time fixed-effects. We weight controls based on the standard weights employed in coarsened exact matching procedures (see Iacus et al. (2011)).[38] Results are reported in Figure A8 in Appendix G. The estimated earnings drop is 5200 euros three years after the separation, equivalent to 20% of the average and in line with estimates in Couch and Placzek (2010).[39]

We combine our estimates of earnings losses with estimates of the number of workers involuntary separated from the employer firm due to the reform. The result of this exercise is presented in Figure 10. The blue-shaded area covers the portion of the total earnings loss for full-time workers that can be imputed to involuntary separations, which explains around 45.2% of the total effect of the reform on earnings. Separations, therefore, play a relevant role, but within-firm dynamics provide an important contribution as well. We also replicate the decomposition for young and middle-aged workers, respectively (Figure 11) Some important heterogeneity emerges. Separations explain a larger part of the earnings losses for young workers (63% of the total). In the first years after the reform is enhanced, we can impute to separations almost the entire total drop in their earnings. On the other hand, the picture is remarkably different for middle-aged workers, for whom within-firm dynamics account for most of the drop in earnings (around 70% of the total). The uncovered

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

[36] Among involuntary separations, we therefore also consider occurrences whereby a temporary contract is not renewed.

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

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

[39] Couch and Placzek (2010) revisit pioneering work by Jacobsen et al. (1993) and estimate 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.

heterogeneity is consistent with a model of seniority where earnings grow with age within the firm. As a consequence, middle-aged workers are the closest substitutes to *CTR* workers and experience the largest slowdown in earnings growth.

Beyond the effect on labor earnings and non-work subsides, we also study how the reform affects co-workers' take up of other social insurance programs: namely, short-time work benefits, disability benefits, benefits related to periods of leave and sickness, as well as pension entitlements. Table 6 reports in column (2) the sum of post-reform coefficients $\{\beta_k^T\}_{k=2012}^{k=2015}$ for all these outcomes. It is confirmed that co-workers incumbent in more treated firms make a larger use of non-work subsidies in the post-reform period. On the other hand, effects on the take-up of social insurance programs are small and in most cases non significant.

7.2 *CTR* workers

Similarly to the procedure used for studying the effect of the reform on co-workers, we match every *CTR* employee to the firm where she was incumbent at the reform period and we aggregate the outcomes of interest - labor earnings, pension entitlements and benefits related to other social insurance programs - across all *CTR* workers incumbent in the same firm. We then estimate specification (3).

Panel A of Figure 12 shows results relative to the take-up of social insurance programs other than public pension. *CTR* workers whose retirement date is shifted forward by more receive a larger amount of non-work subsidies in the post-reform period, the coefficient growing up to 850 euros in 2015. There is also a positive, although smaller, and significant effect on the take-up of disability benefits, short-time work benefits and benefits related to sickness and leaves. Total outlays related to these social insurance programs are then plotted in Panel B, alongside the effect on labor earnings and pension entitlements. Labor income significantly increases in the pre-reform period, the coefficient growing up to 9000 euros in 2015. At the same time, we observe a drop of similar magnitude of pension entitlements. Total outlays on other social insurance programs are therefore small compared to savings on pension benefits. The sum of post-reform coefficients $\{\beta_k^T\}_{k=2012}^{k=2015}$ for all outcomes is displayed in Table 6. Externalities associated to *CTR* workers' behavioral responses appear therefore to be modest. However, in sub-section 7.1 we show that also co-workers are affected by the reform, due to firms' labor demand adjustments. This has to be taken into consideration when estimating the fiscal externality of the reform, an exercise that we carry on in Section 8.

8 The fiscal externality of the reform

In section 7 we have documented that the reform affects' *CTR* employees and co-workers' labor earnings as well as the take up of other social insurance programs. While the literature has explored behavioural responses of senior workers on the cusp of retirement following changes in retirement rules, much less attention has been devoted to study behavioural responses of co-workers and firms. In this section we develop an accounting model that incorporates demand-driven behavioural responses so as to estimate the fiscal externality of the reform, i.e. the share of mechanical pension saving that is lost due to behavioural responses, taking also them into account.

We focus, as in the rest of the paper, on the short-term horizon. The model is highly stylized and ignores some of the general equilibrium effects of the policy, because it abstracts from the revenues lost on marginal workers who are not hired due to the reform. However, we provide conservative estimates of these revenue losses based on our estimates on the effect of the reform on new hires. Due to the lack of balance-sheet information, we also cannot take into account the effect of the reform on firms' performance.

8.1 An accounting model

Suppose the economy is populated by two types of agents: CTR workers (ctr) and their coworkers (c). Agents allocate their time across different labor-related activities, we call l_i^j the time that type i spends performing activity j. Alternatively, it can be interpreted as the share of individuals of type i performing activity j. The main activity is paid labor in a firm, but a non-negative share of workers of type i 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:

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

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 such that τ_i is a 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 retirement date, and P_i^E are pension benefits for workers who early retire.[40] 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. T is the horizon over which we evaluate the effects of the reform. The wage is denoted by w_i, we denote total labor earnings with $z_i = w_i l_i$ and non-work income with y_i. Following Hendren (2016) we consider a policy path $\Lambda(\theta)$ with θ in a neighborhood of 0:

$$\Lambda(\theta) = \left\{\tau_i(\theta), NW_i(\theta), ST_i(\theta), D_i(\theta), SL_i(\theta), P_i(\theta), T_i^P(\theta), P_i^E(\theta)\right\}$$

so that $\Lambda(0)$ represents policies in the status-quo. We investigate the effect of a reform intended to prolong the working life that we model as a change in $T_i^P(\theta)$. Suppose that after an increase in T_i^P a worker retires at the previously expected date: then she will receive a lower pension payment because $P_i^E < P_i$. Let $t_i(\theta)$ denote total net resources transferred to individual i under policy θ:

$$\begin{aligned} t_i(\theta) = {} & NW_i(\theta) l_i^{NW}(\theta) + ST_i(\theta) l_i^{ST}(\theta) + D_i(\theta) l_i^D(\theta) + SL_i(\theta) l_i^{SL}(\theta) \\ & + P_i(\theta)\left(T - T_i^P(\theta)\right) \cdot I\left(T > T_i^P(\theta)\right) + P_i^E(\theta) l_i^E(\theta) - \tau_i(\theta) z_i(\theta) \end{aligned}$$

We define the fiscal externality of the policy as the portion of mechanical revenues that is lost because of the behavioral responses:

$$FE(\theta) = -\frac{\sum_{i=ctr,c} \frac{n_i}{n_a}\left(NW_i \frac{dl_i^{NW}}{d\theta} + ST_i \frac{dl_i^{ST}}{d\theta} + D_i \frac{dl_i^D}{d\theta} + SL_i \frac{dl_i^{SL}}{d\theta} + P_i^E \frac{dl_i^E}{d\theta} - \tau_i \frac{dz_i}{d\theta}\right)}{\sum_{i=a,c} \frac{n_i}{n_a} \frac{dT_i^P}{d\theta} P_i \cdot I\left(T > T_i^P\right)} \tag{8}$$

[40] Notice that when workers early retire they do not receive the full pension payment P_i. Therefore, full pension outlays would 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 also exclude the possibility that workers retire after the statutory retirement date. As we discussed in section 4.1, this is a very low probability occurrence in the data.

where n_{ctr} and n_c denote the number of CTR workers and co-workers, respectively. The ratio n_c/n_{ctr} measures the average number of co-workers per CTR worker so that $\frac{n_c}{n_{ctr}}\frac{dt_c}{d\theta}$ captures the firm-level effect on net resources received by co-workers. Whenever the the fiscal externality is between -1 and 0 the reform generates an increase in government revenues. If the fiscal externality falls below -1 it means that the government looses the entire mechanical revenue generated by the reform through behavioral responses, which implies that there are local Laffer effects and the policy can be Pareto improved (Hendren (2017) and Werning (2007)).

8.2 Empirical implementation and results

The fiscal externality is a function of the estimates of the analysis carried out in Sections 5 and 7. The terms referring to NW, ST, D and LS in the numerator of equation (8) measure the total budget effect of the reform on policy instruments that were not directly affected by the reform dispositions. We compute them using the estimates of the causal effect of the reform on the amount of non-work subsidies, short-time work benefits, disability pensions and benefits related to sickness and leave received both by CTR employees and their co-workers. The last term in the numerator of (8) is the total effect on labor income tax revenues that is a function of the causal effect of the reform on CTR employees and co-workers' earnings. Finally, the term $P_i^E\frac{dl_i^E}{d\theta}$ measures the impact of changing full statutory ages on early retirement. In order to quantify it, we need estimates of $\frac{dl_i^E}{d\theta}$ that we obtain from estimating the effect of the reform on the months spent on 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 and we check alternative parametrizations of τ for robustness.[41] All coefficients, for both CTR employees and their co-workers, are reported in table 6. Finally, we obtain the mechanical effects in the denominator of (8) by subtracting the behavioral effect $P_i^E\frac{dl_i^E}{d\theta}$ from causal estimates of the effect of the reform on pension outlays.

The first three columns of Table 7 show the estimated values of the fiscal externality across alternative calibrations of pension entitlements and tax rates. We calibrate P_i using either the median or the mean value of pension entitements in the data (13,100 and 16,300 euros, respectively) and we use alternative average tax rates from 25% to 35%.[42] Furthermore, we also have a calibration whereby $P^e = 0.9 \times P$. Standard errors are computed via bootstrapping. Point estimates range from -57% to -61%. Taking into account the width of confidence intervals, this indicates that the savings on pension outlays overcome the cost of behavioral responses.

The baseline model outlined in sub-section 8.1, however, does not take into account 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, by assuming that every worker marginally non-hired would earn no labor income for as long as the median duration (13 months) of unemployment for individuals who eventually finds a job in 2012-2015. We then calibrate earnings losses using the median labor earnings of new hires in the first 13-month following the hiring event (5506 euros). We employ estimates of the effect of the reform on new hires from the first part of the analysis to calibrate the number of marginally non-hired workers. In this

[41] Notice that workers claiming *opzione donna* (early retirement) get roughly 65% of full pension benefits in the data (INPS (2016)). A small portion of workers, however, can retire before the statutory date thanks to some provisions introduced after the reform and obtain full pension entitlements as if they retired at the statutory date (see Appendix C). It follows that our calibration understates the true payment received when these workers retire before the statutory date.

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

conservative scenario (setting τ at 25%), point estimates for the fiscal externality are a bit larger, in the range from -65% to -67%, but still below 100%.

Finally, the last column of Table 7 shows how results would change if we ignored the demand-side responses to the reform by disregarding the effects on non-CTR employees. Remarkably, all estimates are close to zero because extra tax revenues on CTR workers who prolong their working lives offset costs related to spillovers on other social insurance programs. This result highlights the importance of incorporating demand-driven behavioural responses, so far mostly ignored in the literature, into the estimation of the fiscal externality of pension reforms that change full retirement ages.

9 Conclusions

This paper studies the short-run labor market effects of pension reforms that raise full retirement ages by exploiting a quasi-natural experiment occurred in Italy in 2011 and leveraging novel administrative data. In countries such as Italy or the US, where the elasticity of retirement age to the statutory full retirement age is high, the most proximate consequence for firms is the increase in the retention rate of senior workers who were on the cusp of retirement under previous rules. We therefore explore how firms respond to an unexpected increase in the retention of older workers caused by higher statutory retirement ages and how labor demand adjustments impact public finances.

In the first part of the paper we ask whether and how employers change their demand for labor, revealing the degree of substitutability between workers of different ages. We document that firms more affected by the reform increase layoffs and decrease hiring in the post-reform periods. The effect is concentrated on co-workers and job applicants who are the closest substitutes of workers on the cusp of retirement (which we label with the shortand CTR workers): middle-aged and other senior individuals who hold or seek a job in the same occupation group. We also provide evidence that firms only respond to shifts in the retirement date of workers who were expected to retire soon, with no reaction to changes in the residual working lives of employees farther away from retirement. For this reason, grandfathering clauses sometimes comprised in pension reforms that insulate workers on the cusp of retirement from new rules may moderate the short-term labour market costs of raising retirement ages. For instance, the raise of the FRA to 70 years old proposed in the U.S. by the CBO should have only mild effects on firms since it will change the retirement age of workers who are expected to retire many years from now.

In the second part of the paper we show that raising full retirement ages affects labor earnings as well as the take-up of other social insurance program of both CTR workers and, due to labor demand adjustments, their younger co-workers. While the literature has documented that higher statutory retirement ages may induce senior workers to substitute away from retirement into other welfare programs, much less attention has been devoted to spillovers driven by firms and co-workers' behavioural responses. We conclude the paper by showing that demand-driven behavioural responses should not be ignored, as they matter. We incorporate them into an accounting model to compute the fiscal externalities of the reform. Only considering CTR workers' behavioural responses would lead to a virtually 0 fiscal externality, because higher tax revenues from increased labour supply offset costs borne by other social insurance programs. When accounting for the effect of the reform on co-workers, a fiscal externality emerges and from 1/2 to 2/3 of mechanical savings are lost due to behavioural responses. Nonetheless, the reform still generates savings in the short-run. Moreover, labour market costs are transitional whereas benefits in term of the sustainability of the public pension system are structural and long-term.

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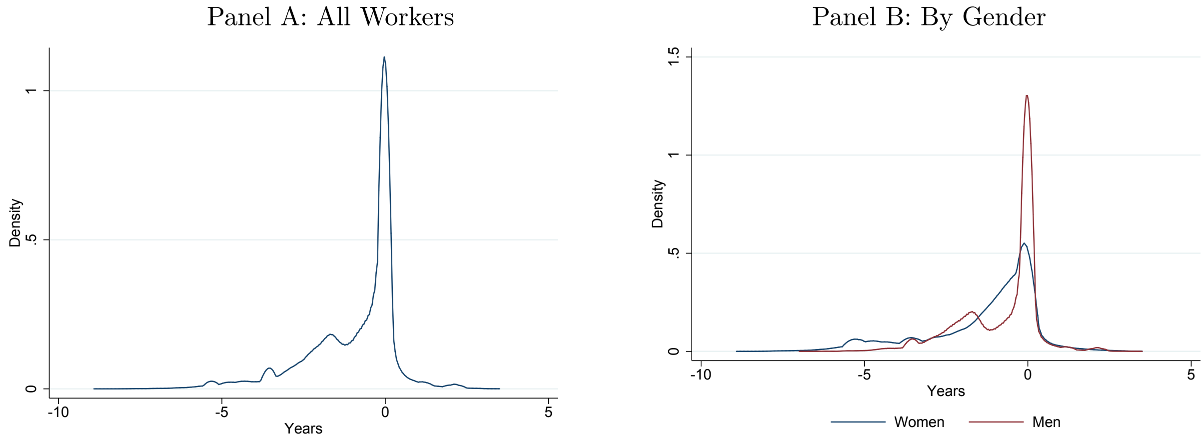
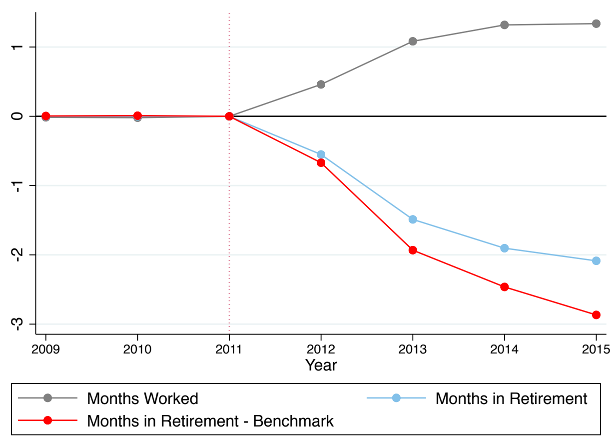
Figures

Figure 1: Post-reform retirement date - Forecast error



Notes: The Figure shows the density of the forecast error from comparing the predicted post-reform retirement date to the actual retirement date for *CTR* workers who retired in 2012-2015. N = 123,887.

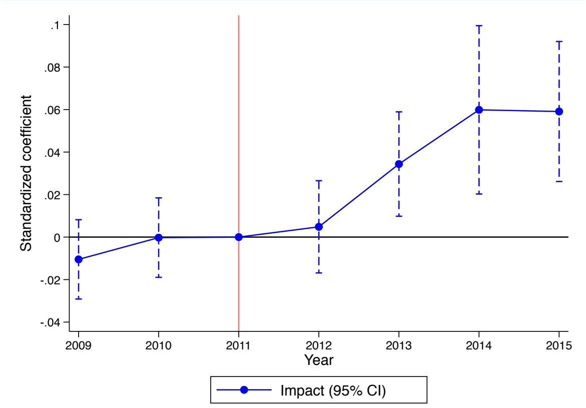
Figure 2: The effect of the reform on *CTR* workers' months spent working or in retirement



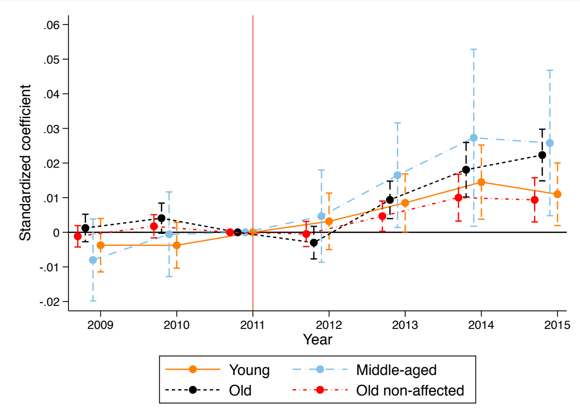
Notes: The Figure shows the effect of the reform on actual months in retirement, predicted months in retirement if all *CTR* workers retired at the predicted date and months spent working, alongside 95% confidence intervals. The regression is the individual-level version of specification (3) and it includes worker and year fixed-effects. Standard errors are clustered at the worker level.
N = 962,696. Pre-reform mean outcomes: months worked = 11.65.

Figure 3: Layoffs

Panel A: Layoffs



Panel B: Layoffs by age

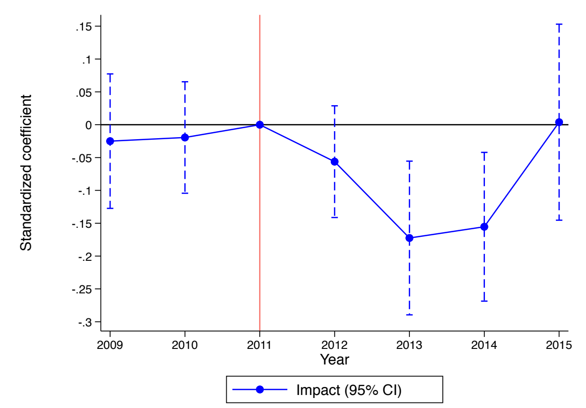


Notes: The Figure shows the response of total layoffs and layoffs by age to a 1σ change in the treatment, alongside 95% confidence intervals. Young workers are aged below 35, middle-aged workers are between 35 and 55 years old, old workers are aged above 55. The regression is based on specification (3) and includes firm and year fixed-effects. Standard errors are clustered at the firm level.

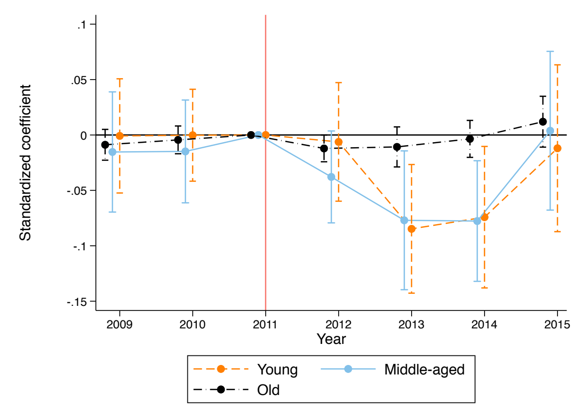
Number of observations = 453,047. 1σ of the treatment = 1.38 years. Mean outcome pre-reform: total = 0.43; young = 0.14; middle-aged = 0.23; old = 0.06.

Figure 4: New hires

Panel A: New hires



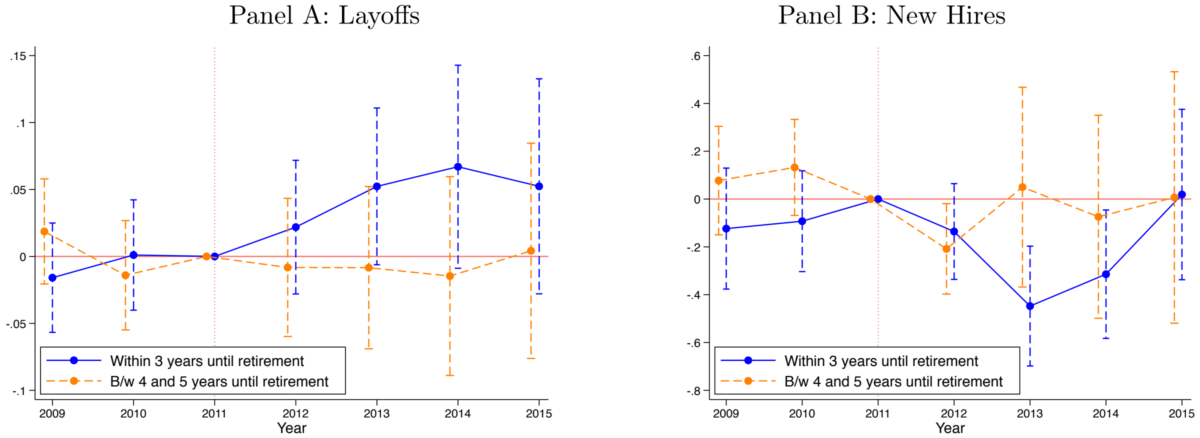
Panel B: New hires by age



Notes: The Figure shows the response of total layoffs and layoffs by age to a 1σ change in the treatment, alongside 95% confidence intervals. Young workers are aged below 35, middle-aged workers are between 35 and 55 years old, old workers are aged above 55. The regression is based on specification (3) and includes firm and year fixed-effects. Standard errors are clustered at the firm level.

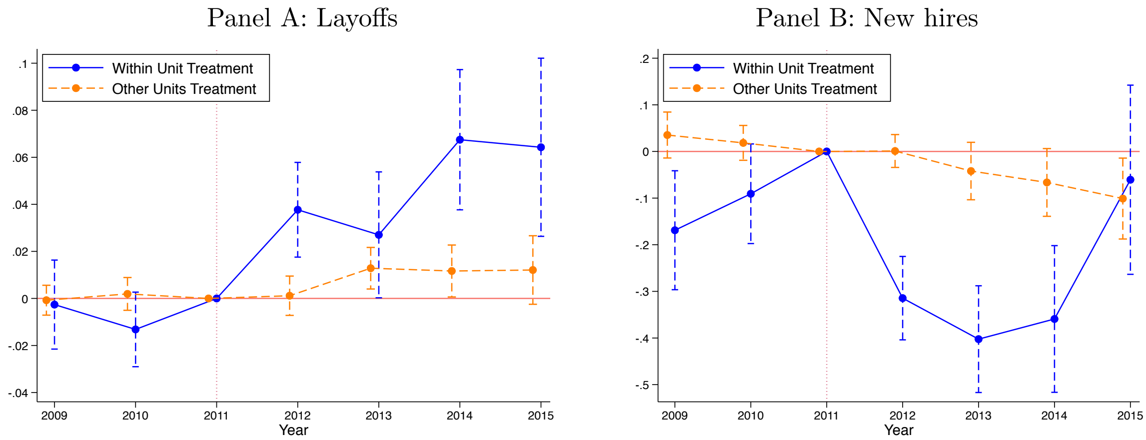
N.obs = 453,047. 1σ of the treatment = 1.38 years. Mean outcome (pre 2012): total = 5.23, young = 2.58, middle = 2.26, old = 0.38

Figure 5: Layoffs - different definitions of *CTR* workers



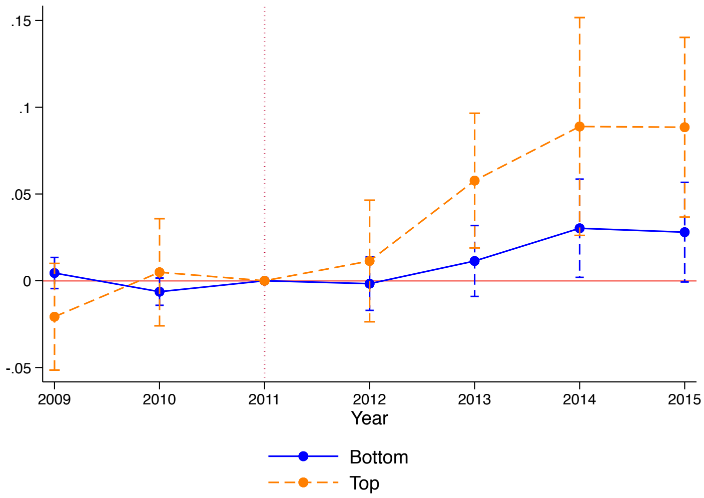
Notes: The Figure shows the differential effect on layoffs and new hires of a treatment defined on the sample of workers expected to retire by 2014 and on the sample of workers expected to retire between 2015 and 2016, alongside 95% confidence intervals. The regression is based on specification (3) and includes firm and year fixed-effects. Standard errors are clustered at the firm level.
N.obs = 453,047. Mean outcomes pre-reform: layoffs = 0.43; new hires = 5.23.

Figure 6: Layoffs and new hires within and across units



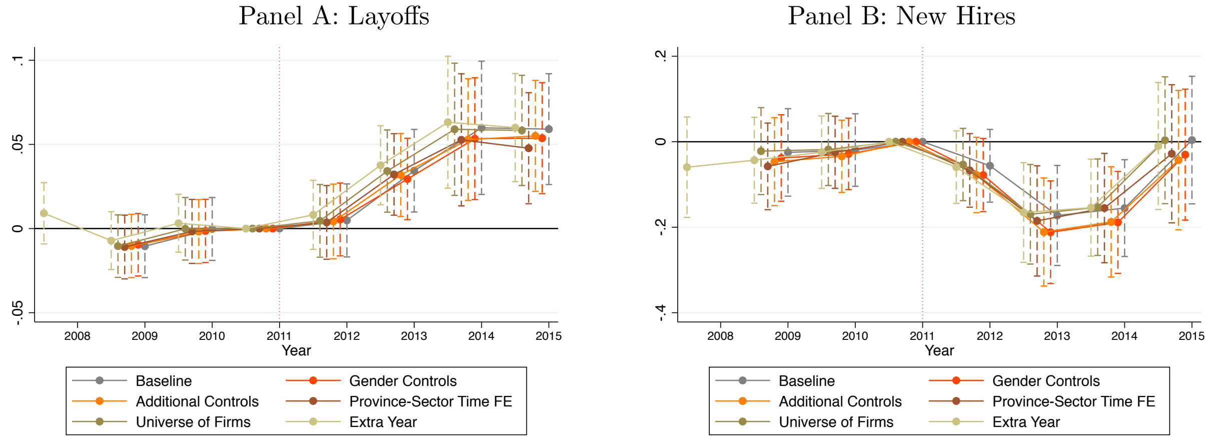
Notes: The Figure shows the heterogeneous effect of the treatment on layoffs and new hires within and across units, alongside 95% confidence intervals. The regression is a version of specification (3) where the unit of analysis is the firm-unit level and the sample is restricted to firms which have at least two units with at least three workers in the baseline period. It includes firm and year fixed-effects. Standard errors are clustered at the firm level.
N. obs = 622,986. Mean outcomes pre-reform: layoffs per unit = 0.16; new hires per unit = 2.38.

Figure 7: Layoffs in high and low-turnover firms



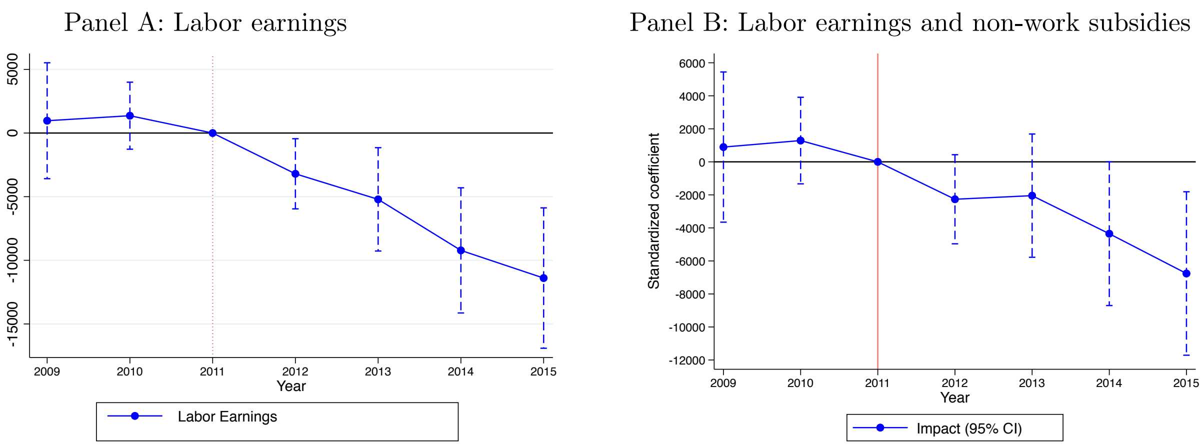
Notes: The Figure shows the heterogeneous response of layoffs in firms which are either above or below the median of turnover rates in the pre-reform period, alongside 95% confidence intervals. The regression is based on specification (4) and the coefficients plotted are the linear combination of β_k^Ts and $\beta_k^{T,to}$s. Standard errors are clustered at the firm level.
N.obs = 453,047. 1σ of the treatment = 1.38 years. Mean outcome pre-reform = 0.43.

Figure 8: Robustness checks



Notes: The Figure addresses the robustness of the main estimates. We confront the results of the baseline specification reported in Figures 3 and 4 with the results of sensitivity checks that employ the specification in (7). 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 CTR worker. This allows to check whether firms with no CTR workers 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.

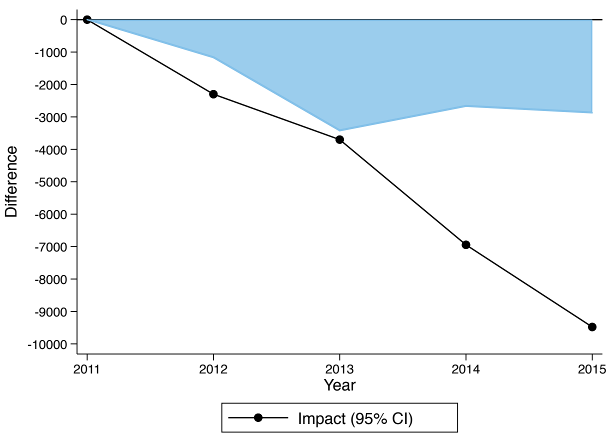
Figure 9: The effect of the reform on co-workers labor earnings and take-up of non-work subsidies



Notes: The Figure shows the effect of a 1σ increase in the treatment on total labor earnings of co-workers incumbent in the same firm at the reform year, either excluding non-work subsidies (Panel A) or including them (Panel B), alongside 95% confidence intervals. The firm-level regression is based on specification (3) and includes firm and year fixed effects. Observations are weighted according to firm size at baseline. Standard errors are clustered at the firm level.

N. Obs = 540,239. Mean outcomes pre-reform: labor earnings = 647061.12 ; labor earnings and non-work subsidies = 647170.31.

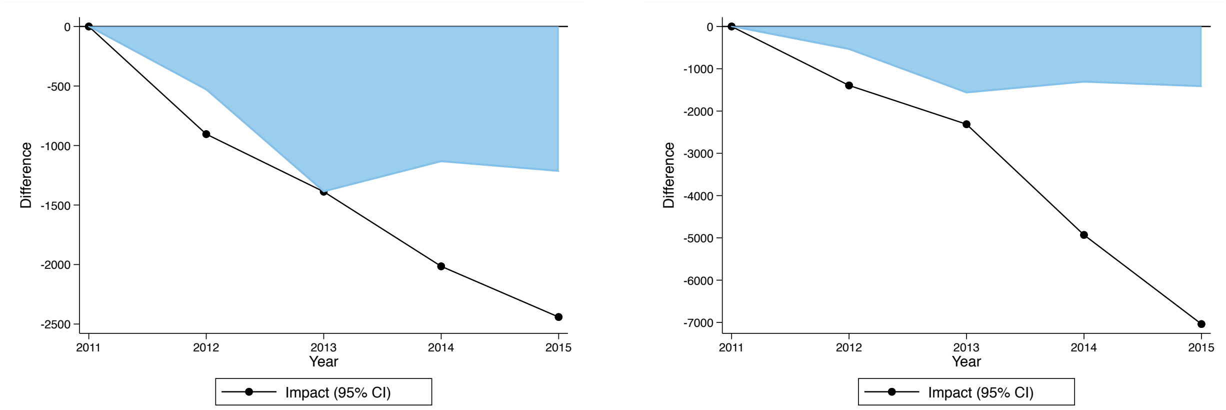
Figure 10: Decomposing coworkers' earnings Loss



Notes: The Figure shows the share of co-workers' earnings loss that can be imputed to involuntary separations. The black line plots the effect of a 1σ increase in the treatment on total labor earnings of co-workers who are incumbent at the same firm in the reform year. The blue-shaded area represents the share of earnings loss imputed to involuntary separations.

Figure 11: Decomposing cow-workers' earnings loss by age

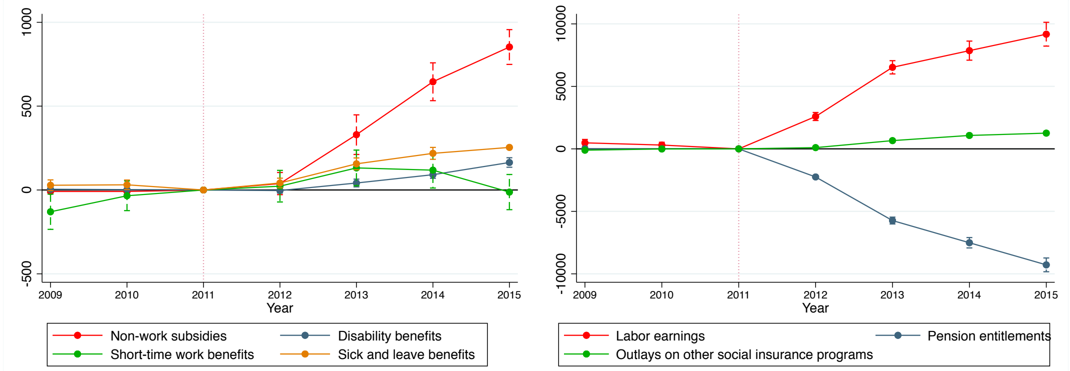
Panel A: Young Panel B: Middle-aged



Notes: The Figure shows the share of co-workers' earnings loss that can be imputed to involuntary separations, distinguishing young workers (aged below 35) from middle-aged ones (aged 35-55). The black line plots the effect of a 1σ increase in the treatment on total labor earnings of co-workers who are incumbent at the same firm in the reform year. The blue-shaded area represents the share of earnings loss imputed to involuntary separations.

Figure 12: The effect of the reform on *CTR* workers' labor earnings and take-up of social insurance programs

Panel A: Other insurance programs Panel B: Labor earnings and pension entitlements



Notes: The Figure shows the effect of a 1σ increase in the treatment on *CTR* workers' labor earnings, pension entitlements and the take-up of other social insurance programs, alongside 95% confidence intervals. The firm-level regression is based on specification (3) and includes firm and year fixed effects. Observations are weighted according to firm size at baseline. Standard errors are clustered at the firm level.
N = 539,196. Mean outcomes in the pre-reform period: labor earnings = 50837.06; pension entitlements = 44.13; disability benefits = 146.06; non-work subsidies = 15.92; short-time work benefits = 1097.329; benefits related to sickness and leave = 59.20.

Tables

Table 1: Old-age and seniority pension: requirements under pre- and post-reform rules

	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, by 2012, were at least 60 years old and had at least 20 years of contribution 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 2.4.

Table 2: Placebo Tests

	Layoffs	New Hires
Treatment X Post 2009	0.0084	0.020
	(0.0076)	(0.043)
Treatment 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. Treatment SD is 1.16 years.

Table 3: Balancing tests

	Cross-section	FE	Mean
	(1)	(2)	(3)
Share young workers (< 35)	0.000	0.001	0.298
	(0.001)	(0.001)	
Share middle-aged workers ($35 - 55$)	-0.011***	-0.010***	0.579
	(0.001)	(0.001)	
Share old workers (> 55)	0.011 ***	0.009***	0.123
	(0.001)	(0.001)	
Share male workers	-0.034 ***	-0.012 ***	0.658
	(0.001)	(0.001)	
Share white-collar workers	0.010 ***	0.001	0.328
	(0.001)	(0.001)	
Average gross daily real wage	0.631	1.275 *	92.05
	(0.538)	(0.635)	
Share full-time workers	-0.009 ***	-0.002*	0.885
	(0.001)	(0.001)	
Firm size	0.738 ***	0.453 ***	26563
	(0.108)	(0.453)	
Firm age	-0.139 **	-0.179**	19.856
	(0.049)	(0.051)	
Firm in manufacturing	-0.018 ***	-0.000	0.443
	(0.002)	(0.000)	
N. firms	64721	63483	

Notes: The table reports a set of balancing tests whereby firms' baseline characteristics are regressed on the firm-level treatment. Column (1) displays the estimates from the bi-variate regression, with robust standard errors in parentheses, whereas column (2) adds province fixed effects, sector fixed effects, as well as province $\times$ sector fixed effects, with standard errors clustered at the province $\times$ sector level in parenthesis. Column (3) displays mean values of the dependent variable.

Table 4: Comparison between firms with at least one CTR worker 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 with at least one CTR workers, as well as for other firms in the same size class (3-200) that also remain open throughout the period. Average workforce tenure and experience are truncated at 29 years, because matched employer-employee data are available since 1983.

Table 5: IV estimates of the effect of the reform on layoffs and hires

Panel A: Layoffs	All	Young	Middle-aged	Old	Old No Aff
	(1)	(2)	(3)	(4)	(5)
Post x R_i	0.110***	0.031***	0.056***	0.026***	0.015***
	(0.027)	(0.0093)	(0.016)	(0.0051)	(0.0043)
N	453,047	453,047	453,047	453,047	453,047
Mean pre-2012	0.43	0.14	0.23	0.06	0.04
KP F-statistics	14,364.877	14,364.877	14,364.877	14,364.877	14,364.877

Panel B: New Hires	All	Young	Middle-aged	Old	
	(1)	(2)	(3)	(4)	
Post x R_i	-0.21	-0.11	-0.096	0.0021	
	(0.14)	(0.075)	(0.061)	(0.019)	
N	453,047	453,047	453,047	453,047	
Mean pre-2012	5.23	2.58	2.26	0.38	
KP F-statistics	14,364.877	14,364.877	14,364.877	14,364.877	14,364.877

Notes: The table reports the results of the IV specification in (6) where we instrument R_i with T_i. The coefficients capture the effect of retaining an extra *CTR* worker. 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 workers net of affected workers. Standard errors are clustered at the firm-level. KP F-statistics =

Table 6: The effect of the reform on *CTR* workers and co-workers: labor earnings and take-up of social insurance programs

	CTR	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 (3). Column (1) reports the estimates for the sample of *CTR* workers, while column (2) displays the effect on their co-workers. All specifications include firm fixed effects and year fixed effects. Observations are weighted according to firm size at baseline. Standard errors in parentheses are clustered at the firm level. *** p <0.01; ** p <0.5; * p < 0.1.

Table 7: Fiscal Externality

	$\tau = 25$	$\tau = 30$	$\tau = 35$	W/ Loss on Non-Hired	CTR only
	(1)	(2)	(3)	(4)	(5)
Median Pension	-57.78	-58.30	-58.82	-65.08	8.97
	(13.9)	(14.38)	(14.93)	(13.52)	(15.45)
Mean Pension	-58.89	-59.39	-59.90	-66.00	6.78
	(15.37)	(15.78)	(16.26)	(14.67)	(22.62)
Early Retirement $= 0.9 \times$Pension	-60.42	-60.91	-61.40	-67.27	3.69
	(19.59)	(19.88)	(20.23)	(18.11)	(40.93)

Notes: The Table reports estimates of the fiscal externality based on the formula in (8). A negative externality between -100% 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 $P^e = 0.9 \times P$. The first three columns show calibrations with alternative levels of the average income tax rate (the average tax rate for the median income is 24%). The fourth column reports estimates from a model that augments the formula in (8) assuming that every marginally non-hired worker earns zero labor earnings for as long as the median duration of unemployment for workers who eventually find a job over the 2012-2015 period, i.e. 13 months. We calibrate the foregone earnings by using the median value of the first 13-months wage of newly hired workers in the period 2012-2015, i.e. 5560 euros and we employ estimates on the effect of the treatment of new hires to calibrate the number of marginally non-hired workers. Finally, the last column reports the estimates for the fiscal externality that ignore the spillover on non-CTR workers.

Appendix - For Online Publication

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. *CTR* workers) and young workers are their co-workers. Denote with n_0^y and n_1^y the number of young workers employed in period 0 and 1, respectively. Adjustments in the demand for young workers are referred to as x^y, so that $n_1^y = n_0^y + x^y$. A cost function $c\left(x^y\right)$ accounts for the cost of adjusting the young workforce, which is paid in period 0. We require $c\left(\cdot\right)$ to be twice continously differentiable and we assume that $c'\left(x^y\right) > 0$ for $x^y > 0$, $c'\left(x^y\right) \leq 0$ for $x^y < 0$, $c'\left(0\right) = 0$ and $c''\left(\cdot\right) \geq 0$. 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. This approximation comes without a significant loss since we study workers who are close to retire, enjoy high levels of EPL and rarely separate from the firm in the last years of work because, as we will discuss later, there are almost no options of early retirement.[43] We denote with n_0^o and $n_1^0 = sn_0^o$ the number of old workers in period 0 and 1, respectively. $s \leq 1$ 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 $F\left(n_t^o, n_t^y\right)$ in every period $t = 0, 1$, with $F_{11}, F_{22} \leq 0$ 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:

$$\pi = \pi_0 + \beta\left(F\left(sn_0^o, n_0^y + x^y\right) - w^o sn_0^o - w^y\left(n_0^y + x^y\right)\right) - c\left(x^y\right) \tag{9}$$

[43] As one could expect, there is no hire event for this type of workers. We show in the following analysis that firms marginally adjust their employment of workers close to retirement through layoffs, although to a lower extent relative to other over 55 years old workers.

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:

$$\beta \left(F_2 \left(s n_0^o, n_1^y \right) - w^y \right) = c' \left(x^y \right) \tag{10}$$

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:

$$\frac{\partial x^y}{\partial s} \propto \beta \left(\frac{\partial w^y}{\partial s} - F_{21} n_0^o \right) \tag{11}$$

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 $F_{21} < 0$.[44] Finally, in a competitive labor market where wages reflect the marginal productivity of young workers we would have no change in labor demand.

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 5 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 7, 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.

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

[44] 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.

by the worker. We allow all wages to be re-negotiated in period 1. The following holds in equilibrium:

$$\phi \frac{\partial \pi(s n_0^o, n_1^y)}{\partial n_1^y} = (1-\phi)(w^y(s n_0^o, n_1^y) - \underline{w}^y) \tag{12}$$

which implies the following expression for the equilibrium wage:

$$w^y = \eta F_2 - \frac{\eta}{\beta} c'(n_1^y - n_0^o) + \frac{(1-\phi)}{\phi\beta} \eta \underline{w}^y \tag{13}$$

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:

$$\frac{\partial w^y}{\partial s} = \eta[F_{21} n_0^o + F_{22} \frac{\partial n_1^y}{\partial s}] - \frac{\eta}{\beta} c''(n_1^y - n_0^o) \frac{\partial n_1^y}{\partial s} \tag{14}$$

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 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 assumption would be violated if the general equilibrium effects of the reform were large.

By using (14) in (11) we get the following expression for the adjustment in labor demand of young workers in period 1:

$$\frac{\partial x_1^y}{\partial s} = -\frac{\beta F_{21} n_0^o}{\beta F_{22} - c''(n_1^y - n_0^y)} \tag{15}$$

We conclude that 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:

$$\phi\pi = (1-\phi)(w^y - \underline{w}^y) \tag{16}$$

This implies the following:

$$\left(1-\phi+\beta\phi n_1^y\right)w^y=\beta\phi\left(\left(F-w^o sn_0^o\right)\beta-\frac{1}{\beta}c\left(n_1^y-n_0^y\right)\right)+\left(1-\phi\right)\underline{w}^y \tag{17}$$

Wages are determined by profits net of young workers' cost and by worker's outside option. We totally differentiate equation (17) to find an expression for the wage response to a change in s:

$$\frac{\partial w^y}{\partial s}=\tilde{\eta}n_0^o\left(F_1-w^o\right) \tag{18}$$

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.[45] By replacing (18) in (11) 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 problem would become:

$$\pi=\pi_0+\beta\left(F\left(sn_0^o,n_1^y\right)-w^o sn_0^o-n_1^{y\,\frac{1+e}{e}}\right)-c\left(n_1^y-n_0^y\right) \tag{19}$$

The firm's optimality condition is:

$$\beta\left(F_2-\frac{1+e}{e}n_1^{y\,\frac{1}{e}}\right)=c'\left(n_1^y-n_0^y\right) \tag{20}$$

We derive the following comparative statics:

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

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,

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

labor demand falls in response to a shock to the retention rate of old workers.

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.[46] 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.[47] 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. 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.[48] France and Germany experienced similar trends with a 10 percentage points and a 21 percentage points increase, respectively.

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.[49] 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.[50] 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.

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

[47] 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).

[48] Source: Eurostat, Employment statistics.

[49] Source: Eurostat, Employment statistics.

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

C Additional Details about the *Fornero* reform

Categories of workers the are not affected by the reform

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*).

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.[51] 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.[52] If more than one record survives criteria (a) to (d), we drop all but one randomly.

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

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 sickness 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.[53] 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.

[53]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.

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 dismissed worker. We call this combination a strata. After we match dismissed workers to workers who do not separate from the firm we estimate the following specification:

$$Y_{it} = \alpha + \lambda_i + \sum_{k=-3}^{3} \beta_k \gamma_k + \sum_{k=-3}^{3} \beta_k^l \gamma_k \times Layoff_i + \varepsilon_{i,t} \tag{22}$$

Since our sample ends in 2015 we estimate a model with only 3 periods after the layoff 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 (22) as the job loss four years after the layoff. 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:

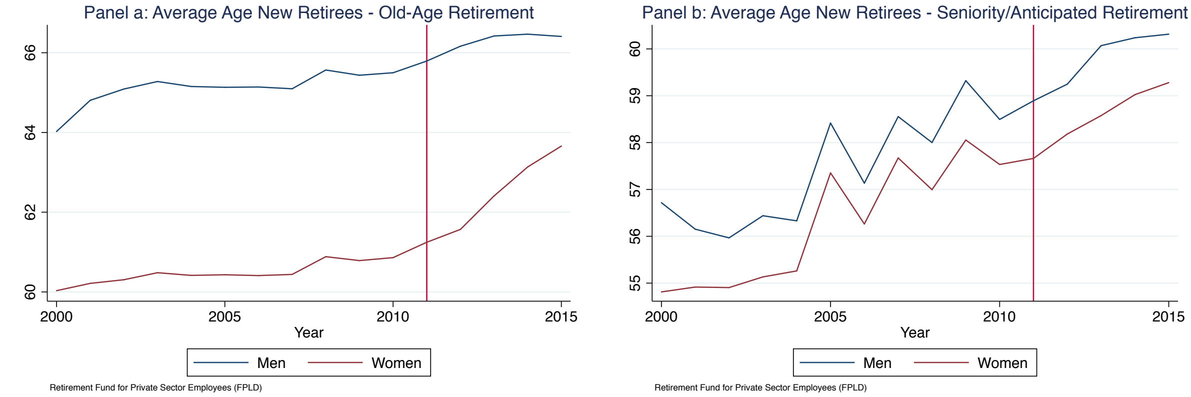
$$\sum_i w_i = \sum_{i \in C} w_i + \sum_{i \in T} w_i = \sum_{i \in C} w_i + N_T$$

$$= \frac{N_C}{N_T} \sum_s \sum_{i \in s} \frac{N_{Ts}}{N_{Cs}} + N_T$$

$$= N_C + N_T$$

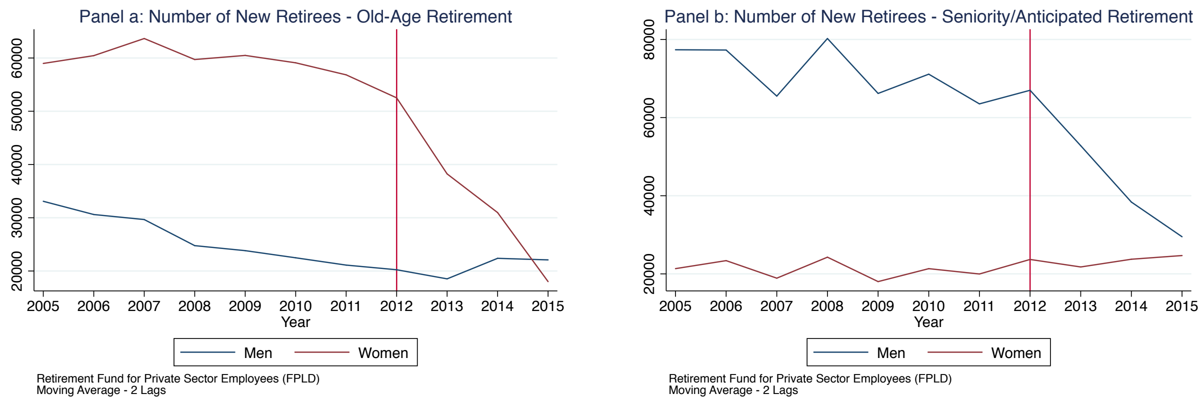
G Additional figures and tables - For Online Publication

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



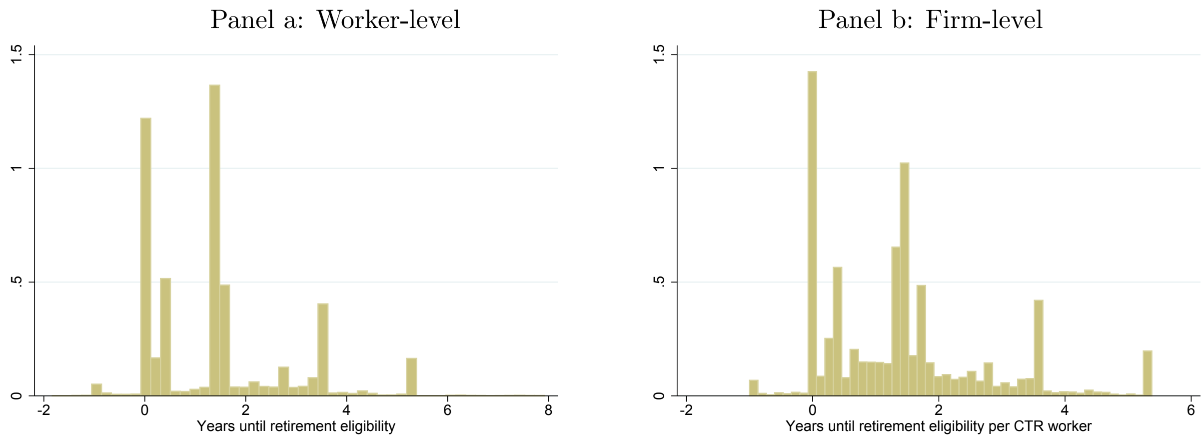
Notes: The figure shows the evolution of the average age at retirement, split by gender and type of pension.

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



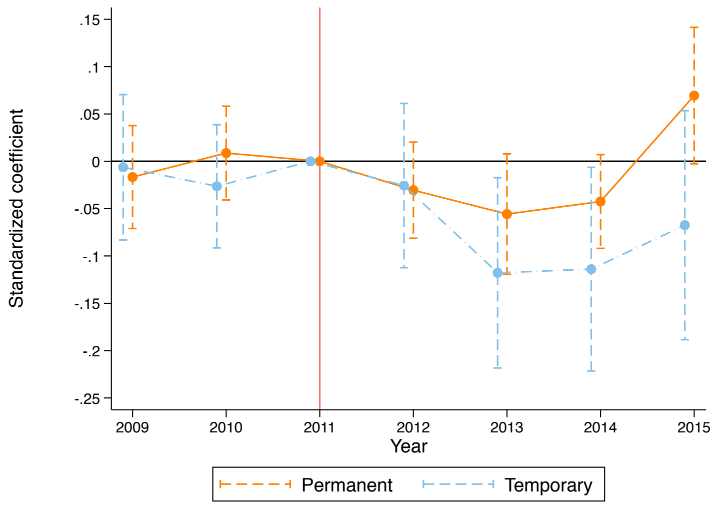
Notes: The figure shows the evolution of the number of new retirees, split by gender and type of pension.

Figure A3: Distribution of the worker-level treatment among CTR workers and of the firm-level treatment among firms with at least one CTR worker



Notes: Panel a shows the distribution of the worker-level treatment among CTR workers (i.e. full-time workers who were eligible to retire by the end of 2014 under pre-reform rules). 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 A4: New Hires by Type of Contract

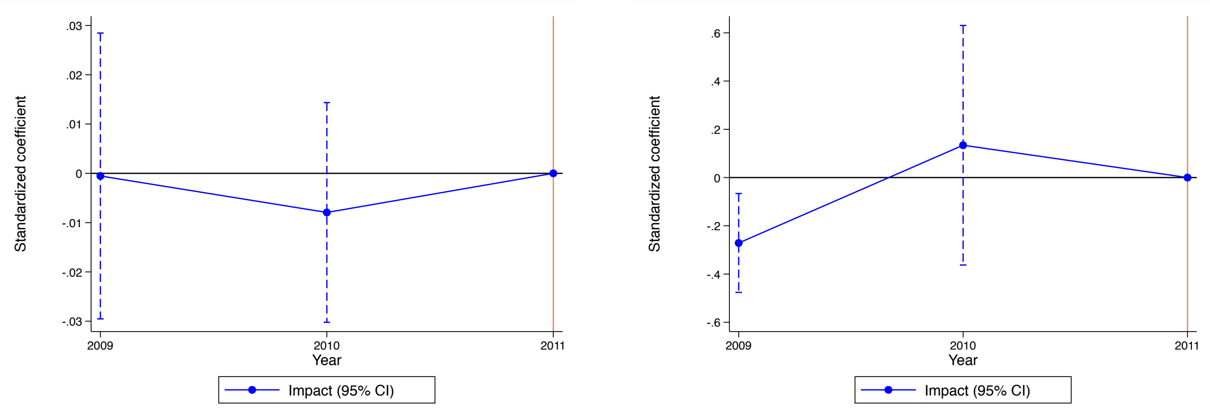


Notes: The Figure shows the response of new hires by type of contract to a 1σ change in the treatment, alongside 95% confidence intervals. The regression is based on specification (3) and includes firm and year fixed effects. Standard errors are clustered at the firm level.
N.obs = 453,047. 1σ of the treatment = 1.37 years. Pre-reform mean outcome: permanent = 1.59, temporary = 3.52

Figure A5: Retained workers as treatment - estimation pre-trends

Panel a: Layoffs

Panel b: New Hires

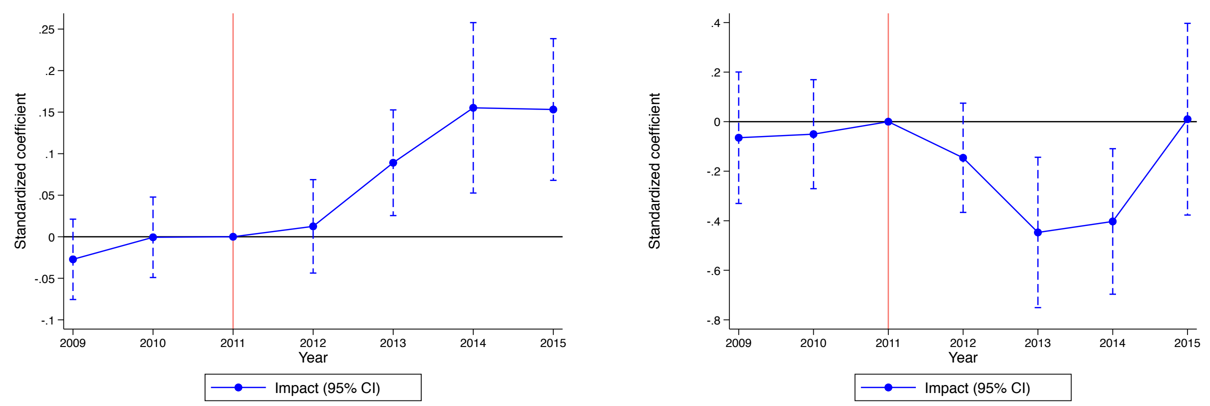


Notes: The Figure shows the effect of retaining an additional affected worker on layoffs and new hires in the pre-reform period, alongside 95% confidence intervals. Results are based on the OLS version of specification (6) where the treatment is the number of retained CTR workers. A worker is defined retained if her retirement date is shifted forward by more than one year. The total number of retained workers is computed according to the definition in (5). The regression specification includes firm and year fixed effects. Standard errors are clustered at the firm level.
Number of observations: 453,047. Pre-reform mean outcomes: layoffs = 0.43; new hires = 5.23

Figure A6: Rescaling the Magnitudes

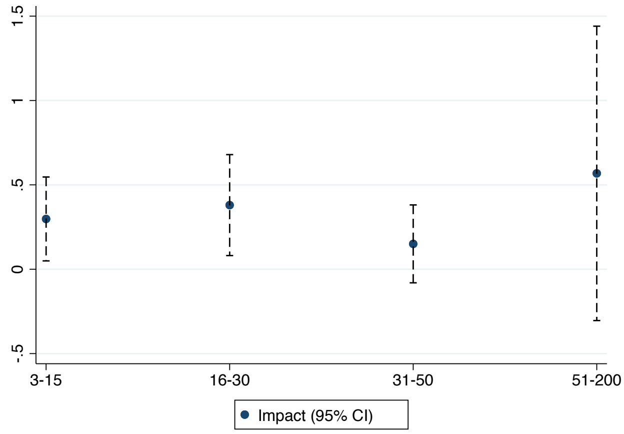
Panel a: Layoffs

Panel b: New Hires



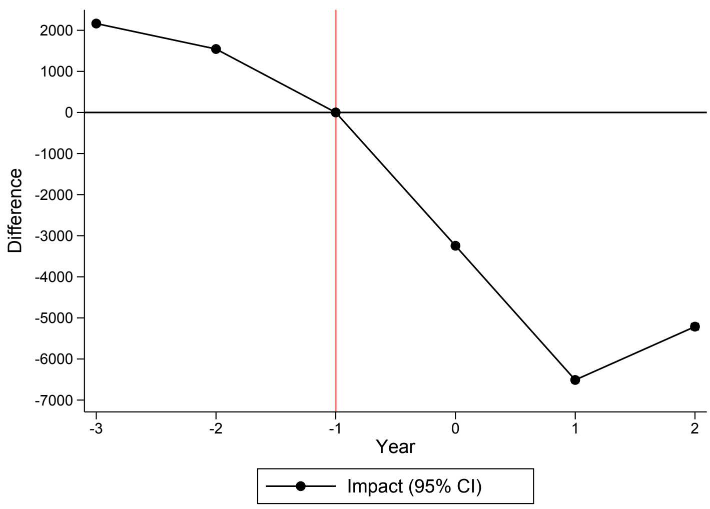
Notes: The Figure shows the effect of retaining an additional affected worker on layoffs and new hires, alongside 95% confidence intervals. Results are based on an IV strategy whereby the number of retained CTR workers is instrumented using T_i, the average shift in the retirement date of CTR workers in firm i. The regression is based on specification 3 and includes firm and year fixed effects.
Number of observations: 453,047. Pre-reform mean outcomes: layoffs = 0.43; new hires = 5.23. KP F-statistics = 2394.12

Figure A7: Layoffs by firm size



Notes: The figure shows the cumulative post-reform percentage change in firing by firm size, which is obtained by re-scaling the raw cumulative coefficients by the pre-reform mean outcome. The regression is based on specification 3 and it includes firm and year fixed effects. Standard errors are clustered at the firm level.
N.obs = 534,443; 1 SD of the treatment = 1.38 years Pre-reform mean outcome: 0-15 = 0.28; 15-30 = 0.42; 30-50 = 0.54; >50 = 0.90

Figure A8: Cost of Layoffs



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

Table A1: Comparison between CTR and other workers in the master sample

	CTR workers		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 CTR workers and co-workers employed in firms belonging to the master sample. Tenure and experience are truncated at 29 years, because matched employer-employee data are available since 1983 only.

Table A2: Absences from work for similar CTR and non CTR workers

	Not CTR		CTR		Difference	
	Mean	SD	Mean	SD		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 for 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 and its standard error.

Table A3: Elasticity of retirement to change in retirement age

	(1) All	(2) Male	(3) Female
ΔYLR	5.60***	6.57***	5.24***
	(0.03)	(0.06)	(0.04)

Notes: The table reports estimates from a cross-section regression where the outcome is the difference (in months) between the expected retirement date under pre-reform rules and the actual retirement date. The treatment is the individual-level change in years left to retirement caused by the reform. Column (1) shows the results for all CTR workers, column (2) and (3) show the results for male and female CTR workers, 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 A4: The effect of the reform on layoffs

	All	Young	Middle-aged	Old	Old Not affected
	(1)	(2)	(3)	(4)	(5)
$t-3$	-0.010	-0.0037	-0.0080	0.0012	-0.0012
	(0.0095)	(0.0039)	(0.0060)	(0.0020)	(0.0016)
$t-2$	-0.00025	-0.0038	-0.00056	0.0041*	0.0017
	(0.0095)	(0.0034)	(0.0062)	(0.0022)	(0.0017)
t	0.0048	0.0032	0.0046	-0.0030	-0.00049
	(0.011)	(0.0042)	(0.0068)	(0.0024)	(0.0019)
$t+1$	0.034***	0.0085**	0.016**	0.0094***	0.0047**
	(0.013)	(0.0043)	(0.0077)	(0.0028)	(0.0022)
$t+2$	0.060***	0.014***	0.027**	0.018***	0.010***
	(0.020)	(0.0055)	(0.013)	(0.0040)	(0.0035)
$t+3$	0.059***	0.011**	0.026**	0.022***	0.0094***
	(0.017)	(0.0046)	(0.011)	(0.0038)	(0.0033)
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

Notes: The table reports estimates from the specification (3). Column (1) shows the effect of the reform on total layoffs, column (2) to (5) the effect on layoffs of young, middle-aged, old and old net of *CTR* workers, respectively. The coefficients capture the effect of a 1 SD increase in the treatment, which is equivalent to 1.37 extra years left to retirement per *CTR* workers. 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. All specifications include firm fixed effects and year fixed effects. Standard errors in parentheses are clustered at the firm level. *** p <0.01; ** p <0.5; * p < 0.1.

Table A5: The effect of the reform on hiring

	All	Young	Middle-aged	Old
	(1)	(2)	(3)	(4)
$t-3$	-0.025	-0.00083	-0.015	-0.0088
	(0.052)	(0.026)	(0.028)	(0.0071)
$t-2$	-0.019	-0.00022	-0.015	-0.0044
	(0.043)	(0.021)	(0.024)	(0.0064)
t	-0.056	-0.0062	-0.038*	-0.012**
	(0.043)	(0.027)	(0.021)	(0.0061)
$t+1$	-0.17***	-0.085***	-0.077**	-0.011
	(0.060)	(0.030)	(0.032)	(0.0092)
$t+2$	-0.16***	-0.074**	-0.078***	-0.0035
	(0.058)	(0.033)	(0.028)	(0.0085)
$t+3$	0.0038	-0.012	0.0038	0.012
	(0.076)	(0.038)	(0.037)	(0.012)
N. obs.	453,047	453,047	453,047	453,047
Mean Outcome (pre 2012)	5.23	2.58	2.26	0.38

Notes: The table reports the results of the specification in (3). Column (1) shows the effect on total new hires, column (2) to (4) the effects on new hires of young, middle-aged and old workers, respectively. The coefficients capture the effect of a 1 SD increase in the treatment, which is equivalent to 1.37 extra years left to retirement per *CTR* workers. 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 (the reform year, i.e. 2011) is omitted, as it is set equal to 0 in the estimation. All specifications include firm fixed effects and year fixed effects. Standard errors in parentheses are clustered at the firm level. *** p <0.01; ** p <0.5; * p < 0.1.

Table A6: The effects of the reform on co-workers' labor earnings and take-up of non-work subsidies

Panel A: Labor Earnings				
	All	Young	Middle-aged	Old Non-affected
	(1)	(2)	(3)	(4)
Post x R_i	-18311.4***	-980.5	-14130.3***	-3200.5 ***
	(5235.5)	(2122.6)	(3570.5)	(1037.4)
N	540239	540239	540239	540239
Mean pre 2012				
KP F-Stat	1938.11	1938.11	1938.11	1938.11

Panel B: Total Earnings (labor earnings and non-work subsidies)				
	All	Young	Middle-aged	Old Non-affected
	(1)	(2)	(3)	(4)
Post x R_i	-10490**	691.1	-8894.8***	-2245.3
	(4664.3)	(2117.4)	(3045.5)	(985.1)
N	540239	540239	540239	540239
Mean pre 2012				
KP F-Stat	1938.11	1938.11	1938.11	1938.11

Notes: The table reports the results of specification (6). The treatment T_i instruments the number of retained workers R_i, so that the coefficients show the effect of retaining an extra-worker. 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 non-CTR senior workers (above 55 years old). Standard errors are clustered at the firm level.