Employment of young and older workers: three policy evaluations

Andrea Albanese

2015

Supervisors:

Prof. Dr. Lorenzo Cappellari

Prof. Dr. Bart Cockx

Submitted to the Faculty of Economics and Business Administration of Ghent University and to DEFAP Graduate School in Public Economics of University of Milan-Bicocca and Catholic University of the Sacred Heart, in fulfilment of the requirements for the joint degree of Doctor in Economics

Employment of young and older workers: three policy evaluations

Andrea Albanese

Supervisors: Prof. Dr. Lorenzo Cappellari (Catholic University of the Sacred Heart)
Prof. Dr. Bart Cockx (Ghent University)

Submitted to the Faculty of Economics and Business Administration of Ghent University and to DEFAP Graduate School in Public Economics of University of Milan-Bicocca and Catholic University of the Sacred Heart, in fulfilment of the requirements for the joint degree of Doctor in Economics

UNIVERSITEIT GENT

FACULTY OF ECONOMICS AND BUSINESS ADMINISTRATION

UNIVERSITÀ CATTOLICA del Sacro Cuore

Defap Graduate School

PhD in Public Economics

Joint project by Università Cattolica (Milano) & Università degli Studi Milano-Bicocca

UNIVERSITÀ DEGLI STUDI DI MILANO BICOCCA

Doctoral committee:

Prof. Dr. Lorenzo Cappellari (Catholic University of the Sacred Heart);

Prof. Dr. Bart Cockx (Ghent University);

Prof. Dr. Michael Lechner (University of St. Gallen);

Prof. Dr. Marco Leonardi (University of Milan).

Submitted to the Faculty of Economics and Business Administration of Ghent University and to DEFAP Graduate School in Public Economics of University of Milan-Bicocca and Catholic University of the Sacred Heart, in fulfilment of the requirements for the joint degree of Doctor in Economics

Acknowledgments

This dissertation is not just the result of my intense studies and hard work, but it is the combined product of the support of many people around me who helped me during these last four years. Hereafter I want to show my sincere gratitude to a few people that made this thesis possible.

I would like to express my deep thanks to my advisor Professor Dr. Bart Cockx. I am really grateful for all the opportunities he gave me and for the time, the resources and the effort he invested in me. I have immensely learned from his rigorous and thoughtful scientific approach. His attentive guidance highly benefitted the quality of this thesis and with no doubts my future professional career.

I am very grateful to my advisor Professor Dr. Lorenzo Cappellari for his invaluable tutoring and advice. I am also very thankful for the trust he has been showing in my work and my abilities since the beginning of my PhD. He was the first person to support me during my doctoral studies and without his constant endorsement this joint Ph.D. would not have been possible.

I thank Professor Dr. Michael Lechner and Professor Dr. Marco Leonardi for the privilege of having them as part of my examination committee. I am indebted for the time invested in reading my dissertation and their precious comments that have highly enhanced the quality of this manuscript.

A deep thank also goes to the other members of the WOLDEMP project at Université Catholique de Louvain: Prof. Dr. Bruno Van der Linden and Prof. Dr. Muriel Dejemeppe. They have been important mentors through all the support they gave me, which went well beyond the activities of the project.

I acknowledge the Italian Ministry of Research and the Special Research Fund of Ghent University for financing my doctoral studies (BOF - code 01SF3612) as well as the Belgian Science Policy within the programme “Society and Future” (contract n° TA/00/044). I thank the Belgian Crossroads Bank for Social Security (report no. 12/080), the Italian National Institute of Social Security and the Italian National Institute of Statistics (ISTAT) for the delivery of the data.

I am grateful to my colleagues at Ghent University for the feedback and the enjoyable breaks during these years of intense work. A special thank goes to Jose Luis, Corinna, Yannick, Eva, Marco and Celine.

I am truly indebted to my family who supported me in all my choices and was close to me also during my doctoral studies abroad. Finally, my biggest thanks to my fiancée Laura – all of this would not have been possible without you. Mulțumesc.

Milan, December 2015
Andrea Albanese

Table of Contents

List of Tables

List of Figures

Overall Introduction

The context

Involving most of the population in the labour market is a cardinal goal of modern societies. The ageing population is changing the demographic structure of Western countries by decreasing the share of the active population. In the European Union, the old-age dependency ratio is projected to increase from 28.2% in 2014 to 50.2% in 2060 (EU28 average - Eurostat 2015),[1] which means that for each person aged between 15 and 64 there will be one person above 65 years of age. This creates high pressure on the Welfare States of European countries and the provision of social security such as pensions and social assistance is at threat. To tackle this challenge, in June 2010 the European Council agreed on the "Europe 2020" strategy (17/6/2010 n. EUCO 13/10), through which Member States committed to raise the employment rate of the population to 75% by 2020. Involving the highest number of people in employment may alleviate the increasing burden on the current Welfare State. As prime age men tend to have a higher employment rate, specific attention was dedicated to marginalised categories such as women, young and older workers, who have a high potential of improvement.

Though the economic crisis affected the whole European population, in 2014 the employment rate of prime-age men (aged 25-54) was still relatively high: 83.3% (declining from 86.9% in 2008 - OECD 2015).[2] The picture is different if we look at other groups such as young and older workers. Older individuals, have a significantly lower employment rate than the prime-age population. While in 2014 the employment rate of men aged 55-64 was 59.0%, women in the same age-class had an employment rate of 45.4% (compared to 71.7% of women aged 25-54). This is due to their low participation in the labour market as only few older workers are unemployed (7.3% in 2014 in EU28, Ibid). Low participation can be explained by the high share of people in early retirement or disability schemes. On the other side of the age spectrum, young workers also show a low level of employment. As many of them are still in the education system, one should only focus on the active population. In particular, the unemployment rate of youth aged 15-24 was about 21.9% in 2014, showing a very steep trend since the economic crisis (in 2008 this was 15.5%, Ibid). Even if the unemployment rate of prime-age workers also raised after the recession, it is still less than half: 9.4% in 2014 (compared to 6.1% in 2008, Ibid). Furthermore, while 7.5% of the European population aged 15-29 is looking for a job, 7.8% is discouraged and is neither in the labour force nor in education (NLFET - Eurostat, 2015b).[3]

---

[1] Eurostat, 2015a. Old-age-dependency ratio: tsdde510.
[2] OECD, 2015. Dataset: LFS - Sex and Age Indicators (Dataset Level Metadata).
[3] Eurostat, 2015b. Young people neither in employment nor in education and training by sex and age: edat_lfse_20.

The economic theory mainly explains the structurally higher youth unemployment rate as a consequence of their low working experience. Entering from the educational systems, younger workers might lack the appropriate general and firm-specific human capital to be stably integrated in the labour market. Furthermore, the human capital received in the education system might not meet the needs of the employers, generating a “skill-mismatch” (ILO, 2014).[4] Youth might also be less efficient in finding a job due to the lack of job-search abilities or budget constraints, which might decrease their mobility (ILO, 2006).[5] Other reasons for their high unemployment regard institutional barriers such as stringent employment protection legislation, which might discourage firms to hire “outsiders”. To facilitate their integration and preserve the employment protection in the main open-end contract, temporary contracts have been thought as a possible solution. However, temporary jobs are intrinsically less stable and, together with possible seniority-based job security ("last in, first out"), are likely to have increased the already high sensitivity of youth employment to the business cycle.

Concerning the *older workers*, their low level of employment can be explained by other characteristics of the demand and supply (*pull* and *push* factors), shifting the equilibrium in the labour market to a low level of employment. Concerning the older workers’ labour supply, *entitlement to early retirement* schemes introduces an implicit tax on work due to the higher non-labour income. The more attractive outside option can also take the form of more generous *unemployment benefits* or relaxed eligibility to *disability schemes* for older unemployed. Furthermore, older workers can also afford more leisure time thanks to past savings (e.g. Mitchell and Fields, 1984).[6] All these factors increase the older workers’ reservation wage and shift their labour supply to the left. On the other side, several push factors can decrease the firms’ demand for older workers. First, due to deferred compensation rules (Lazear, 1979)[7] and possibly declining productivity related to age and induced by the rapid technological change, over the years the pay might rise more steeply than productivity (for an example of empirical study, see Vandenberghe et al., 2013).[8] Once the pay excessively exceeds the worker’s productivity, employers may encourage the employee to leave the firm by exploiting early retirement schemes and may also stop hiring older unemployed. Second, the expected short employment horizon can induce employers to disregard older job-seekers due to firm-specific training cost (e.g. Daniel and Heywood, 2007);[9] Third, higher employment protection legislation for older workers might also

[4] ILO, 2014. Skills mismatch in Europe. International Labour Organization, Department of Statistics. Geneva.
[5] ILO, 2006. Global Employment Trends for Youth. International Labour Organization, Geneva.
[6] Mitchell, O.S., Fields, G.S., 1984. The Economics of Retirement Behavior. Journal of Labor Economics 2, 84–105.
[7] Lazear, E., 1979. Why Is There Mandatory Retirement? Journal of Political Economy 87, 1261–84.
[8] Vandenberghe, V., Waltenberg, F., Rigo, M., 2013. Ageing and Employability. Evidence from Belgian Firm-Level Data. Journal of Productivity Analysis 40, 111–136.
[9] Daniel, K., Heywood, J., 2007. The determinants of hiring older workers: UK evidence. Labour Economics 14, 35–51.

discourage employers from hiring new older workers (e.g. Behaghel et al., 2008).[10] Finally, employers may discriminate older workers due to negative perception on several characteristics such as the lack of flexibility, absenteeism and health (e.g. Remery et al., 2003).[11]

Many labour market policies have been implemented to encourage the labour market participation such as “stick” or “carrot” measures. While the first one makes the non-labour option less attractive (e.g. tightened requirements for the unemployment benefits or increasing the firing costs), the second category enhances the surplus produced by the match worker-firm (e.g. training or employment subsidies). In this dissertation we focus on the second category. The three measures analysed in this dissertation are of different type and include training contracts and part-time schemes. However, all of them are characterised by the provision of financial incentives to the beneficiary, be it the employer or the employee. Besides operating as stand-alone measures, employment subsidies can also be integrated with several other active labour market policies, such as training contracts or part-time schemes, to make them more attractive. Employment subsidies are meant to increase the welfare of disadvantaged categories mainly by fostering their labour market participation and secondly by sustaining their labour income. While the first objective can be achieved if the firms’ labour costs decrease, the second goal requires an increase of the employees’ net wage.

In a broad definition, employment subsidies include *wage cost subsidies* (i.e. subsidies for the employers) and *in-work benefits* (i.e. subsidies for the employees). Though perfectly competitive labour market theory affirms that the statutory beneficiary does not matter for determining the employment effect of a wage subsidy, several frictions on the labour market such as minimum wages can actually generate different effects (for an empirical study see Woodbury and Spiegelman, 1987).[12] If the subsidy is targeting only the newly created jobs (or to the unemployed), then it is defined as a *“hiring subsidy”*. Subsidies are also defined according to the time dimension: whether they are only temporary (either one-off-payment or fixed-duration) or of unlimited duration (maybe until certain requirements are respected e.g. age or wage ceiling). Finally, employment subsidies can be paid or can grant a tax credit on the labour taxes (e.g. Social Security Contributions exemptions).

[10] Behaghel, L., Crépon, B., Sédillot, B., 2008. The perverse effects of partial employment protection reform: The case of French older workers. Journal of Public Economics 92, 696–721.
[11] Remery, C., Henkens, K., Schippers, J., Ekamper, P., 2003. Managing an Aging Workforce and a Tight Labor Market: Views Held by Dutch Employers. Population Research and Policy Review 22, 21–40.
[12] Woodbury, S.A., Spiegelman, R.G., 1987. Bonuses to Workers and Employers to Reduce Unemployment: Randomized Trials in Illinois. The American Economic Review 77(4), 513–530.

Motivation

The 2008 financial crisis and the later 2011 European Sovereign Crisis had dire effects on the labour market of the European countries. To adjust their labour force to the business cycle, firms reduced the hiring and the renewals of fixed-term contracts, besides intensifying the usage of short-time compensation and early retirement schemes. As an effect of the economic crisis, the general unemployment rate steadily increased, but above all the youth unemployment skyrocketed to an emergency level. Furthermore, dismissed older workers are also very difficult to re-absorb in the labour market, paving the way to their early retirement. Because of the weak economic growth in several European countries it will require time to bring the situation back to the pre-crisis levels and there is the risk of a lost generation among the youth, with severe consequences for their lifetime job prospects.

This dissertation has the goal of providing a better understanding on the effectiveness of specific active labour market policies that aim at integrating young and older workers. Over the last years, many European governments put in place new policies to counteract the negative effects of the economic crisis and to minimise the negative effects on these disadvantaged categories. Policymakers need to be provided with recommendations on how to enhance current programmes and distinguish between effective and ineffective measures by rigorous cost-benefit analyses. This represents a crucial step to sustain the recovery of the economy currently characterised by both job losses and tight public budget. These evaluations should also aim to help spread a scientific culture among the European policymakers. It is the duty of social scientists to contribute to a constant improvement of the welfare of our society and help correct market and government failures. As the three policies analysed are still operative, this dissertation moves in this direction.

Structure of the dissertation

The first chapter is entitled “*Reforming the Apprenticeship Contract in Italy: A natural experiment*“. In this paper, the effectiveness of the 2003 reform of the Italian apprenticeship regime is assessed. This reform raised the age eligibility and revised the training component. The policy is particularly relevant for the policymaker as, despite its limited coverage (only 3.8% of all employees in the private sector in 2013), it absorbed 32.4% of the whole budget for active labour market policies in Italy. This is mainly due to the large hiring subsidy incorporated into the contract. To estimate the Average Treatment effect on the Treated (ATT) and the Intention-to-Treat (ITT) of the reform, we exploit the different timing of the implementation of the reform in the Italian regions and sectors. Analyses on self-selection into the treatment are performed, looking at the impact of the reform on migration and commuting flows to the early implementing regions, as well as at the differences in regional labour market characteristics (Labour Force Survey data - LFS). To estimate the ATT of the reformed regime (compared to the old regime) on the apprentices' transition to other contracts, the Covariate Balancing Propensity Score estimator is implemented (CBPS - Imai and Ratkovic, 2014)[13] by employing a large set of covariates. The inflow sample of about 18,000 apprentices hired in 2007 is drawn from administrative data of the Social Security Institutions (INPS). Four years after hiring, the reform induced an increase in the transition rate to permanent jobs in the same firm and boosted the average wage of the apprentices. The results are robust to different specifications. Finally, by a DiD estimator on the LFS we estimate the ITT of the policy-reform. First, it is found that non-employed youth becoming eligible to the apprenticeship have a higher employment transition of about +14%. Second, the higher diffusion of the apprenticeship among the youth becoming eligible is offset by a reduction of other temporary jobs.

The aim of the second and third Chapters is to assess the effectiveness of Belgian federal policies to boost the employment rate of the older population. To evaluate them, we rely on an endogenous stratified sample of administrative data containing about 244,000 individuals (aged between 52 and 61 in 2002) with their employment history since 1957. Because we could not have access to population data for reasons of confidentiality and since many individuals are inactive at those ages, we over-represent those employed in the period of analysis, especially women. Moreover, we draw the full population of individuals transitioning into and from employment within the period analysed. To avoid bias induced by this endogenous sampling and infer our estimates to the Belgian population, we weight the sample appropriately.

The second Chapter is co-authored with one of my supervisors, Prof. Dr. Bart Cockx, and is entitled "*Permanent Wage Cost Subsidies for Older Workers. An Effective Tool for Increasing Working Time and Postponing Early Retirement?*". In this paper we evaluate the impact of a reduction in the employers' Social Security Contributions for workers older than 58 in Belgium. The analysis is performed on multiple repeated cross-sections even if panel data are available, to account for age-varying confounding factors. We use a CDiD estimator (Heckman et al., 1997)[14] and, when needed, a trend-adjusted version of it (Wolfers, 2006).[15] To facilitate the integration of endogenous sampling weights in this estimator, we implement it as an Inverse Probability Weighting (IPW) estimator, which we extend to allow for multiple cross-sections in the before and after periods. We construct a simple theoretical model that can predict the existing findings in the literature as well as ours. Key in this

---

[13] Imai, K., Ratkovic, M., 2014. Covariate Balancing Propensity Score. Journal of the Royal Statistical Society: Series B (Statistical Methodology) 76, 243–263.

[14] Heckman, J., Ichimura, H., Todd, P., 1997. Matching as an Econometric Evaluation Estimator: Evidence from Evaluating a Job Training Programme. Review of Economic Studies 64, 605–54.

[15] Wolfers, J., 2006. Did Unilateral Divorce Laws Raise Divorce Rates? A Reconciliation and New Results. American Economic Review 96, 1802–1820.

model is that it integrates the presence of early retirement schemes and a pay-productivity gap (due to declining productivity and/or deferred compensations) in the analysis of the impact of a permanent wage subsidy. We find small positive short-run impacts on working time and larger ones on the employment rate, but only for employees at high risk of leaving to early retirement. The wage is not affected. In a cost-benefit analysis, we estimate that during the 1.25 years after its introduction the subsidy imposed a net monthly cost of 330€ per saved job to society. Had the subsidy been targeted to sectors where early retirement schemes are widely used, society would instead have gained 2,080€ per saved job. As the subsidy has a positive employment effect only by counteracting the (opposite) effects of another policy-measure (early retirement schemes), we conclude that it might be more cost effective to tighten the eligibility requirements of the latter.

The third Chapter is entitled "*Working Time Reductions at the End of the Career. Do they Prolong the Time Spent in Employment?”* which assesses the impact of the Belgian part-time Time-Credit scheme for older workers. This work is a joint paper with Yannick Thuy and Prof. Dr. Bart Cockx of Ghent University. The policy measure allows older workers to reduce their working time by 20% (or 50%) with the goal of postponing their retirement decision and possibly improving their work-life balance. Workers receive a lump-sum in-work benefit of about € 215 (€ 385), granting an average income replacement of 90% (66%) of the full-time wage. We assess the ATT on the survival in employment and we control for selection on observables (employing the IPW estimator) based on information of the employment history of eligible workers since 1957, amongst other. As control units can enter the treatment in later periods, we explicitly take into account this dynamic, possibly selective, assignment to treatment. In addition, we take selective (on observables) right censoring into account, which is particularly important for the competing risks analyses that we perform (Vikström, 2014).[16] Our estimates indicate a positive employment effect in the short-run followed by a highly negative impact after a few years. The negative effect is essentially explained by the fact that the time-credit beneficiaries who enter statutory early retirement remain entitled to the level of benefits of a full-time worker, which makes early retirement relatively more attractive for those workers. More positive effects are found for women, but in general the policy does not pass the cost-benefit test.

[16] Vikström, J., 2014. IPW estimation and related estimators for evaluation of active labor market policies in a dynamic setting. Working Paper Series from IFAU - Institute for Evaluation of Labour Market and Education Policy, No 2014:1, Uppsala.

1

Reforming the Apprenticeship Contract in Italy
A Natural Experiment

1.1. Introduction

In countries with high Employment Protection Legislation (EPL), temporary jobs have been considered a possible tool to facilitate the inclusion of youth in the labour market. During the last 20 years, Italy has been one of the European countries relying the most on temporary jobs. The share of young workers with temporary contracts steadily increased reaching 56% in 2014 from 34% in 2004. The trend is steeper compared to other European countries as in the same period it passed from 37.6% to 43.3% in EU28 (Eurostat, 2015a). Following the European economic recession, firms reduced the hiring as well as the renewal of fixed-term contracts to adjust their labour force to the Business Cycle (Labour Force Survey - LFS). Consequently, in 2014 the youth unemployment rate touched emergency levels, reaching 42.7% in Italy and 22.2% in EU28 (Eurostat, 2015b). Policymakers have had to implement rapid measures to tackle the emergency and are currently rethinking the characteristics that temporary contracts should have to be an effective port-of-entry in stable jobs.

Several studies have addressed the question of whether temporary jobs can represent a port-of-entry to stable employment. However, only recently the literature has started to distinguish the effects of the different types of fixed-term contracts. Among the many temporary contracts present in its economy, the Italian policymaker considers the apprenticeship the main tool to facilitate the passage from the educational system into typical employment. Apprentices receive vocational training to enhance their professional skills and competences while lower wage cost compensates employers through payroll tax rebates and lower remuneration. Enhancing the features of the apprenticeship is important from a public budget perspective as, despite its narrow coverage, it is a very expensive policy due to the extensive tax rebate. In 2013, apprentices amounted to 3.8% of the employees in the private sector or 20.6% of the young employees below 29 years old. However, in the same year it absorbed 0.12% of the GDP or 32.4% of the budget allocated to active labour market policies (categories 2-7 i.e. “LMP measures” - Ministero del Lavoro e delle Politiche Sociali, 2015).

The empirical literature on the transition of the apprentices to open-end contracts in Italy is scarce and shows ambiguous effects. Berton et al. (2011) find that other temporary contracts outperform the apprenticeship regime during the period 1998-2004. Conversely, Picchio and Staffolani (2013) show opposite results on the people aged 30 during the period 2009-2012. In this research, we aim to understand whether the different features of the apprenticeship regime also drive these contrasting results in the two periods, besides other differences in business cycle and population analysed.[1] In particular, we focus on the changes introduced by the Law no. 30/2003, which raised the age eligibility from 24 to 29 years old, set a minimum wage to the apprentices’ remuneration and allowed

[1] The fixed-term contract has also been reformed between the two periods.

firms to provide part of the training on-the-job. As further reforms have given progressively more importance to the internal training, becoming the main training approach, it is essential to understand the effects of such a provision. To estimate the effects of the 2003 reform, we exploit the dual apprenticeship system existing between 2005 and 2010 due to the heterogeneous time of the implementation between regions and sectors. Tests on the exogeneity of treatment eligibility are performed. First, we look at differences in regional labour market characteristics before the implementation of the reforms. Second, we test whether variation of migration or commuting flows affected the composition of the eligible youth in the treated regions.

We assess using administrative data of Social Security Institutions (INPS) the Average Treatment effect on the Treated apprentices. By comparing the employment path of the apprentices in the new regime to the ones in the old regime, we estimate the ATT during the five years following the hiring. Even if firms cannot decide which regime to use, we control for selection on a large set of apprentices and firms' observable characteristics to take into account possible endogenous selection in the apprenticeship regime. We implement the Covariate Balancing Propensity Score estimator (Imai and Ratkovic, 2014), which has been shown in the simulation of Frölich et al. (2015) to be overall the best performing semi-parametric estimator. In a second analysis, we estimate the eligibility effect (Intention-To-Treat, ITT) by Difference-in-Differences on the employment, the transition from non-employment and the contract diffusion. The later analysis is on Labour Force Survey (LFS) data as our administrative data do not contain information on non-employed individuals who are not in insured unemployment schemes.

Our estimates indicate the reform managed to decrease the early dropout of the apprentices by 11.5% in the first two years. Furthermore, it increased the transformation to open-end jobs after four years from the hiring (+28.6%). Analyses on heterogeneous effects indicate that the positive impact mostly occurred in firms with more than ten employees. The ATT on the dropout in the first two years of apprentices in large firms is -17.0%, while it is +36.1% on transformation to open-end jobs in the same firm after four years. Small firms, which absorb two-thirds of the apprentices hired in 2007, show a smaller response. In general, there are signs of positive wage effects over the five years as the ATT on the total cumulated remuneration is 9 percentage points higher than the ATT on the total full-time working days. Finally, we provide more robust evidence on the relative performance of the apprenticeship regime compared to other temporary contracts. Sensitivity checks are performed by varying several tuning parameters of the weighting estimator.

Concerning the ITT on the eligible youth, we find that granting the eligibility to non-employed youth aged 25-29 boosted their transition to employment by about 14%. We also estimate that raising the age eligibility increased the share of apprentices in the economy and decreased the usage of other temporary contracts. The conclusion is partly in line with Cappellari et al. (2012) on firm-level data on the same reform, underlying the high level of substitutability among temporary contracts. The positive impact estimated on the apprentices' transition on administrative data does not translate into a positive effect on the employment of the eligible youth, which might be explained by several factors. First, it is possible that positive effects have come to the detriment of other youth in the economy via substitution effects. Second, the positive impact on the apprentices, who represent only a part of the young population, could be not large enough to be observed on the overall young population. Lastly, other reasons are related to the limits of the LFS, which may downwardly bias the estimated ITT.

The paper is structured as follows. Section 1.2 describes the apprenticeship contract in the period of the reform and the changes introduced by the Law 30/2003. Section 1.3 summarizes the findings of the literature on the apprenticeship contract and provides intuitions on the expected effects of the 2003 reform. In Section 1.4 we describe the datasets. Section 1.5 presents the identification strategies for estimating the ATT and the ITT of the reform. Results and robustness tests are shown in Section 1.6, while the last section concludes.

1.2. The Italian apprenticeship and the 2003 reform

The apprenticeship is a special temporary contract meant to transmit to the youth professional competences by practical on-the-job training and part-time formal education. A firm hiring an apprentice has to provide him/her with vocational training while the apprentice is paid a salary below the compensation of a qualified worker. Differently from stand-alone training programmes, the apprenticeship regime is in general heavily regulated by governments and social partners. The actual implementation of the apprenticeship regime in the European countries has followed very different routes. In some countries the apprenticeship is integrated in the educational system and high importance is reserved to the theoretically-based training in schools and certificated institutions. This is the case of the French apprenticeship system, where the apprentices can earn a certificate equivalent to one obtained in the educational system. Other countries such as Austria, Denmark, Germany and Switzerland integrate the apprenticeship in the educational system but give importance to both the theoretically-based learning and the on-the-job training ("dual system").

Similarly to the English system, the Italian apprenticeship is instead separate and contiguous to the formal education. The learning is mostly firm-based and focuses on the practical activities on-the-job. The Italian policymaker considers it the main active labour market policy to facilitate youth's transition from education to the labour market. The contract was introduced in Italy during the Fifties and the regime was substantially reshaped by a series of reforms in the last two decades. In this paper, we focus on the changes introduced by Law no. 30/2003 as it made the apprenticeship more similar to the current regime. Its heterogeneous time of the implementation between regions and sector resulted in a dual apprenticeship regime during the period 2005-2011. This allows us to assess the different performance of the two regimes in the same period and grants us with a sufficiently long time horizon. In this section, we describe the common features of the contract in the analysed period, some of which may be applicable today, and the changes introduced by the reform.

1.2.1. *Common features*

Since its introduction, the apprenticeship contract has been meant to provide the youth with training for technical and professional qualifications. The only requirement for eligibility was age while the lack of previous work experience was not a pre-condition.[2] Firms had to appoint an internal advisor to mentor the apprentice. The latter attended external preparatory training and could follow at most five apprentices. Only firms in the private sector could use the contract, and the maximum number of apprentices in a firm had to be below the number of employees. Yet, enterprises with less than three employees could hire up to three apprentices. The training counted about 120 hours per year and was divided into basic skills, covering about 35% of the hours, and technical competences for the profession. The first category included training on labour laws, work organization, safety and communication while the second regarded products and services, production processes, tools and materials of productions. Firms could decide the training from a regional sector catalogue, whereas lower remuneration (defined by the sector Collective Bargaining Agreements - CBAs) and high tax rebates compensated the employers for the training requirement. While the employers' Social Security Contributions (SSC) for open-end and fixed-term contracts counted 30% of the gross earnings, these were only 10% of the apprentices' remuneration.

Further incentives for firms with less than ten employees existed, granting almost full tax exemptions for the first two years of apprenticeship. For example, between 2007 and 2011 the SSC counted 1.5% (3%) of the gross remuneration for the first (second) year while from 2012 employers received full exemption for the first three years. Until 2012, the Law on the apprenticeship regime did not oblige employers to hire the apprentice at the end of the contract. However, additional incentives were present as the payroll tax rebates were extended for another year in case of conversion to an open-end contract (Law no. 56/1987). The probationary period was at most two months, and apprentices could only be laid off for just cause (with the standard dismissal distinctions by firm dimension). Since 2009 apprentices dismissed for economic reasons with three months of seniority were entitled to unemployment insurance for 90 days (Law 2/2009).

[2] Former apprentices were also eligible, but in case they worked in the same sector, the total time in apprenticeship must be shorter than the standard duration (if interruptions between jobs were shorter than a year - Law no. 25/1955).

1.2.2. Differences introduced by the 2003 reform

The regime before the 2003 reform (Law no. 196/1997) was partially operative until 2011 with different features. First, age eligibility at hiring was 25 years old (26 in "objective 1" regions apart from Abruzzi and 29 in the small firms in the craft sector). Second, the duration of the contracts was between eighteen months and four years (five in the craft sector). Third, the training could only be provided by external authorities such as local and accredited institutes sponsored by the regions. The Law no. 30/2003 introduced several changes to the regime, which was renamed "*Vocational Apprenticeship*".[3] First, to facilitate the use of the apprenticeship contract, it raised the age ceiling to below 30 years old. Second, it liberalized the training component and allowed part of it to be performed on-the-job. Third, it set the contract duration between two years and six years, though the CBAs could specify a narrower range within this limit. In general, this remained in the lower part of the range, sometimes even setting a minimum length under the two years (e.g. specific CBAs in the retail trade and banking sectors).[4] The average duration range marginally increased as at the end of 2008, the average maximum and minimum length in the CBAs was 33 and 56 months (ISFOL, 2010). Finally, Law no. 30/2003 introduced a minimum level of remuneration paid to apprentices: at least two levels below the remuneration of a similarly qualified worker (i.e. position reached by the end of the contract).

Table 1.1: Changes in the apprenticeship regime introduced by the 2003 reform

	Before reform	**After reform**
Age at hiring	< 25 (30 in some exceptions)	< 30
Training component	By external authorities	External and Internal (if firm declares training capabilities)
Length of the contract (range)	1.5 – 4 / 5 years	Usually 2.75 - 4.5 years (2008 CBAs)
Lower remuneration	Set by CBAs	Minimum wage to the remuneration set by CBAs

On-the-job training was allowed to firms satisfying specific requirements on training competences, tutoring and place of training. Some regions and CBAs allowed under stricter rules that also the basic training could be performed on-the-job, but in fact it mostly covered the technical-professional competences (ISFOL, 2010). Though the policymaker created a system of administrative sanctions,[5] it became more difficult for the authorities to verify firms' compliance with the training requirements. However, non-compliance also occurred for the external training as, in fact, many apprentices were not involved in public training due to the lack of funding. For example, in 2004 only 24.8% of the apprentices participated in training and just 17% completed the compulsory 120 hours (ISFOL, 2006).

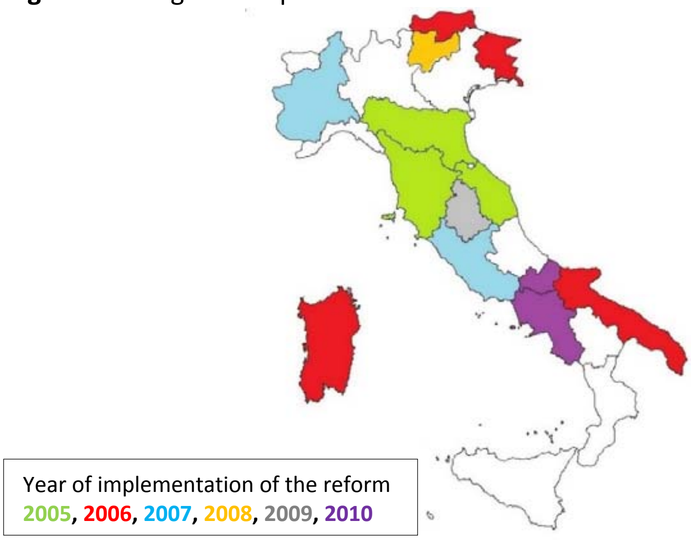
The reform was not immediately operative. Law no. 30/2003 was passed by the Legislative Decree no. 276/2003 of September 10 2003. However, since the Italian regions have exclusive competence in the field of vocational training, the regional governments had a high degree of autonomy in its actual

---

[3] Other two forms of apprenticeship were introduced, covering a minority of the contracts (1.1% in 2007 - ISFOL, 2010).
[4] The minimum length requirement was later abolished in 2008 (Law no. 112/2008).
[5] Employers not complying with the training requirements had to pay back twice the tax exemption received and possibly convert the apprenticeship to an open-end contract (INPS circular No. 40/2004)

implementation. The Italian regions started to pass Regional Laws only from 2005 onwards and the actual implementation was heterogeneous in time as it required agreements among many actors (regional governments, trade and firms' unions). Furthermore, most of the regions were not ready to revise immediately the training offer. Several regions preferred implementing pilot tests in specific sectors before fully implementing the reform. This is the case of sectors such as the Retail Trade (in Lombardy, Umbria, Marche, Lazio, Sicily), Banking (in Piedmont, Lombardy, Marche, Sicily), Metal Manufacturing, Construction and Textile (in Marche), and Tourism (in Lazio). The regions fully implementing the reform by Regional Laws were Tuscany, Emilia and Marche in 2005, Friuli Venetia Giulia, Puglia, Bolzano and Sardinia in 2006, Lazio and Piedmont in 2007, Trento in 2008, Umbria in 2009, Molise and Campania in 2010 (Figure 1.1).

Figure 1.1: Regional implementation of the reform until the 1st quarter of 2011



Year of implementation of the Regional Laws.

To speed up the implementation process the government allowed that, in the absence of a Regional Law, the sector-specific CBAs could revise the training content and start the new regime (Law 80/2005). Since 2005 firms covered by such CBAs could start using the new contract. The most important sectors implementing the reform were Metal Manufacturing, Metallurgy, Transport, Banking, Insurance, Energy, Rubber, Chemicals, Retail and Wholesale trade, Tourism, Gas, Construction, Agriculture, Food Products, Textile, Footwear, Tobacco and Wood Products sector.

Because of the heterogeneous time of the implementation, a dual system of apprenticeship regimes existed until its reorganization (Legislative Decree N.167 of September 2011). Notably, the two regimes implied a different level of remuneration for the apprentices and type of training combination (only external or both internal and external). In general, regional authorities organised the same external training for both regimes (ISFOL, 2010). Importantly, firms could not decide which regime to use as this depended on the sectors and the region of activity at the moment of hiring. Transformations of old contracts into new ones were not allowed to keep the two regimes separated and limitations were also set to avoid firms dismissing an old regime apprentice and re-hiring her with the new one. In practice, only a minority of apprentices were re-hired as in our inflow sample of 2007 (see Section 1.4.1) we observe that the highest share of youth in another apprenticeship contract in the same firm is 2.3% for the old regime apprentices (reached three months from the first hiring).

1.3. Literature review

1.3.1. Theory

According to the human capital theory formalised by Becker (1962), human capital can be divided in firm-specific and general skills. While firm-specific human capital improves the individual productivity only in one firm, general (or "transferable") skills enhance the worker productivity in many firms. Though training may provide a mix of the two types of skills (or occupation-specific skills e.g. Stevens, 1994; Bishop, 1996), one can argue that firm-specific skills are more likely obtained by on-the-job training, whereas general human capital is more easily obtained in the context of formal education.

A part of the theoretical literature describes why firms hire apprentices and train the inexperienced youth (for an extensive review see Wolter and Ryan, 2011). By following the definition of Becker (1962), in a perfect labour market firms should have small incentives in providing general training, and its costs should be borne mostly by the individuals. Imperfections in the labour market can, however, explain the use of apprenticeship dedicating large importance to the general learning. Information asymmetries, search costs and matching frictions, moral hazard and collective bargaining (e.g. Acemoglu and Pischke, 1998, 1999) can indeed compress the wage structure and create a surplus in the provision of general training, which the firms can eventually extract.

The theoretical literature on the effects on the apprentices' transition to stable employment is not univocal. On the one hand, firm-specific human-capital provided by on-the-job training should encourage transformation to permanent positions within the same firms. Even if the contract is not converted to an open-end job after its expiration, the apprentice can enhance her employability thanks to the expanded social network and the enhanced general skills especially if proven by a certification.

On the other hand, apprenticeship contracts allow paying lower wages and, in some Countries, even payroll taxes (e.g. Italy, Spain, Austria). While this is meant to compensate the firms for the training costs, these financial incentives may undermine the positive effects of the apprenticeship by encouraging some churning behaviour from the firms. Indeed, employers may maximise the profits by offering little training and hiring apprentices as cheap labour for production-related reasons. In a deregulated labour market, only the firms' reputational costs damaging the attraction of new apprentices and the concerns about the future productivity of their own workers can refrain the employers from this behaviour (e.g. Katz and Ziderman, 1990; Chang and Wang, 1996; Malcomson et al., 2003). As firms can also use temporary contracts to screen the youth's ability in the presence of incomplete information and high EPL (Autor, 2001; Faccini, 2014), these incentives are potentially disruptive. Indeed, if the youth undergoes several temporary contracts, a "scarring effect" can undermine her employability (e.g. Magnac, 2000; D'Addio and Rosholm, 2005).

The expected effect of the 2003 reform

To provide some intuitions on the potential effects of the 2003 reform we first describe the basic mechanisms behind the apprenticeship contract. In general, employers hire an apprentice if the gains on the wage cost are larger than the training cost, considering the apprentice's higher productivity by the end of the training. Firms do not use apprenticeship contracts if they bear too high training cost and have low expected gains, possibly because of small productivity gains from the training or high job destruction. When the apprenticeship contract expires, the firms decide whether to transform it to an open-end job. The conversion occurs if the productivity gain of the apprentice has become larger than the increase of labour costs and EPL. Otherwise, the financial incentive generates a *churning effect* and the firm hires a new apprentice to enjoy from the lower wage cost and EPL.

A part of the firms might use the contract to train young workers and later offer them permanent positions, while another may hire apprentices just as cheap labour. Indeed, the lower wage cost can also encourage a churning effect especially in firms where: 1) human capital is a secondary factor, hence, the productivity gain at the end of the training is low; 2) the high probability of job destruction decreases the expected return of a trained worker and increases the expected cost of EPL; 3) the financial incentives for using the apprenticeship are higher (e.g. small firms – see Section 1.2.1).

By knowing these mechanisms, we can predict the main effects of the 2003 reform. First, by setting a minimum wage the wage cost of the apprentices rises, which reduces the hiring of apprentices but also fosters transformations as there is a smaller gain in churning. The two effects go in the same direction in decreasing the stock of apprentices in the economy. Second, allowing on-the-job training could ease the cost of production as apprentices might participate in the production processes during the training. However, if the training is performed by the firm, the training cost might, in fact, increase as employers have to organise it. Hence, the effect on the training costs is a priori-unknown. Third, on-the-job training might positively affect the productivity gain of the youth at the end of the apprenticeship thanks to more firm-specific human capital accumulation. Thus, more apprentices are hired with the goal of converting these contracts at the end of the period.

At the same time, substituting external training with on-the-job training may reduce the employability in other firms due to the smaller general human capital accumulation. However, we cannot say whether compliance to the training requirement would be higher for the external or on-the-job training (i.e. public authorities versus employers' compliance – see Section 1.2.2). In conclusion, while the reform has a priori ambiguous effects on the hiring, the reform on the apprenticeship should discourage the churning behaviour of firms and boost the transformation rate (Table 1.2).

Table 1.2: Expected effect of the reform on hiring, transformation and stock of apprentices

Reform	Effects	Hiring	Transformation	Stock of apprentices
Minimum wage	Smaller labour cost gains	-	+	-
Internal training	Unknown effect on training costs	?	?	?
	More firm-specific human capital	+	+	?

1.3.2. *Empirical studies*

The empirical literature on the effects of the apprenticeship regime is relatively optimistic. Booth and Satchell (1994) estimate in the UK that apprentices completing the training have a higher probability of continuing to work in the same firm. In Germany, the apprenticeship enhances the general human capital and increases the wage of the youth in a similar way as the educational system (Clark and Fahr, 2002). However, Acemoglu and Pischke (1998) show that youths quitting the apprenticeship because of compulsory military service are later paid more than former-apprentices remaining in the same firm. This might suggest that firms extract a rent from stayers to recover the training cost. In France apprentices are less likely to undergo long-term unemployment compared to youth opting for full-time vocational schooling (Bonnal et al., 2002). In a study on Austria, Fersterer et al. (2008) estimate in an IV framework that a year of apprenticeship training increases the pay of about 5%.

As the apprenticeships in Europe differ in many aspects, we focus our attention on the literature on the Italian regime. Part of the literature studies the relative performance of the apprenticeship contract in encouraging transitions to open-end position compared to other temporary contracts. The estimation on the relative performance of specific contracts is, however, a complex task. As individuals and firms can self-select in the type of contracts, it is likely that jobs with specific contracts are intrinsically different from the others. While firms and individuals' self-selection can be dealt with estimators controlling for time-invariant observables or unobservable differences (e.g. matching or fixed effects), it is difficult to control for self-selection within the same firms. For example, employers may fill vacancies for longer-term projects with specific contracts and the different performance of workers in these jobs would be a mix of the treatment effect and the effect of the specific vacancy.

Berton et al. (2011) estimate on the period 1998-2004 a port-of-entry effect for the apprenticeship contract. This is estimated by implementing a dynamic multinomial logit model that controls for individual fixed effects. The apprenticeship is outperformed by other shorter-term training contracts and the fixed-term contract. While the individual time-invariant unobservable characteristics are considered, the authors are not able to control for demand side characteristics such as sector and firm fixed effect, besides the more complex issues of "self-selection" within the same firm. Furthermore, we notice that the estimates refer to transition at most at two-year intervals, which may just capture the initial lock-in effect (van Ours, 2004) of a long-term contract such as the apprenticeship.

By exploiting the discontinuity in the eligibility at 30 years old, Picchio and Staffolani (2013) estimate by Regression Discontinuity the relative performance of the apprenticeship compared to other temporary jobs over the three years from the hiring. In particular, the authors observe that apprentices hired in 2009 have higher chances of transiting to open-end contracts within the same firm. While the identification strategy is credible, the Local Average Treatment Effect is estimated on few individuals entering the apprenticeship contracts at 29 years old. Apart from differences in the identification strategy and population, the contrasting results with Berton et al. (2011) may be driven by the different apprenticeship regime analysed. This paper aims at getting further insights on the issue.

Finally, Cappellari et al. (2012) study on firm-level data the effect of the 2003 reform. The authors exploit the different timing in regional and sector implementation of the reform and use a trend-adjusted Difference-in-Differences estimator. Estimates show that the reform induced firms to substitute other temporary contracts (i.e. collaboration contracts) with apprentices, which might have generated a positive effect on the firms' productivity.

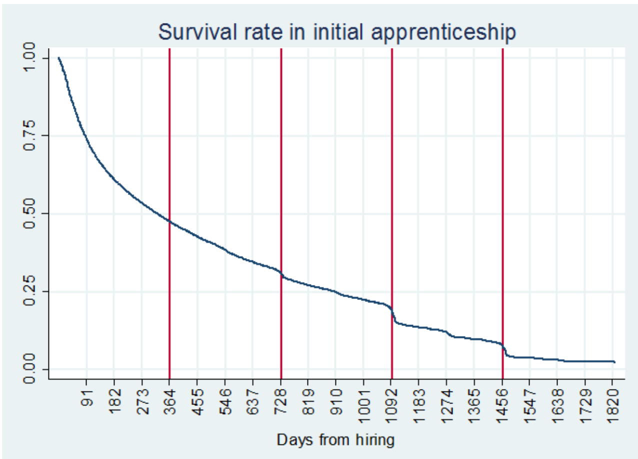
In a recent paper, D'Agostino et al. (2015) assess the effect of the 2003 on the individual job transitions by using AD-SILC data. They exploit the sector-region selection and implement the Inverse Probability Weighting (IPW) estimator in a Markov-chain of order one. The authors find that individuals in treated regions have a higher transition from non-employment and longer retention in apprenticeship, which translate in a lower transition to open-end jobs. As the identification strategy focuses on the short-run, their findings mostly cover the initial lock-in. The main drawbacks of the study are the low number of *individuals,* the stock sample and the few observable characteristics used in the IPW estimator.

1.4. Data

1.4.1. *Administrative data*

In this section, we describe the data and provide descriptive statistics on the apprentices' transition. In the first part of the paper (Sections 1.6.1-1.6.4), we use a panel from administrative data of Social Security Institutions (INPS), containing 1/15 of the Italian population working in the salaried private sector (Longitudinal Sample INPS – LoSai).[6] The dataset contains the individual yearly employment history in the salaried private sector from 1985 to 2012 with the date of hiring and dismissal. Furthermore, it covers history on semi-subordinate employment (e.g. collaborators) from 2000, firms' characteristics from 1990 and unemployment benefits from 1996. As the dataset contains information about the apprenticeship regime from 2007 onwards, we select an inflow sample of new apprentices between 19-24 years old (17,950 apprentices) in that year.[7] As the database ends at the end of 2012, we follow the apprentices for the following five years with a monthly frequency. In another analysis (Section 1.6.2-1.6.3) we also use an inflow sample of 24,793 youth hired with temporary contracts (excluding interim agency workers and collaborators). The time span is sufficiently long to allow us to study the employment effect at the end of the apprenticeship as this contract has a rather long duration, if not terminated earlier. However, early termination should not be ignored. In our sample, we observe 19% of the apprentices exiting the contract within the two months of the probationary period, and as many as 40% terminating the contract within the first six months after hiring. Few apprentices receive a stable training since only 29% have a duration in the contract longer than two years (Figure 1.2 and Table 1.12 in Appendix 1.8.2).

Figure 1.2: Survivor function - exit from the initial apprenticeship – whole sample



Survival rate for the initial apprenticeship contract. Inflow sample of 17,950 apprentices hired in 2007 with 19-24 years old.

If we look at the official reason of the contract dissolution, we see that in the first year 61% of the terminations were due to resignations. In only 12% of the cases this was the dismissal, while 8% was due to transformation to another contract, 6% to expiration of the contract,[8] and 13% to unknown reasons (Figure 1.10 in Appendix 1.8.2). These figures should be carefully interpreted. Empirical studies in other countries suggest that firms may hire "cheap" apprentices to adjust their workforce to the

[6] Private-sector employees born on the 1st and the 9th day of each month.

[7] We remove the apprentices above 24 years old as they could be hired in the old regime only under specific exceptions. Similarly, we do not consider apprentices born before 1989 as underage apprentices could only be hired under the old regime (the implementation of the new regime for youth under 18 years old was postponed until 2011).

[8] If we exclude former apprentices (which can have shorter contract duration), seasonal apprentices and the probationary period (first three months) the expiration rate becomes 2.7%, which could nevertheless be due also to administrative error.

business cycle (e.g. Merrilees, 1983; Askilden and Nilsen, 2005). Besides, employers might encourage workers' resignation to get around the high Employment Protection Legislation. If we look at the exit destination of the early leaver in the first year, we see that 72% of them exit our database the month after the end of the contract. These youths are not employees in the salaried private sector, collaborators, or insured unemployed. If we extend the horizon to the second or the third month after the end of the contract, the figures are still as high as 63% and 60% (Figure 1.12).

After five years, almost all contracts are terminated with two spikes of the hazard rate at the beginning of the third and the fourth year. This reflects in a drop of 6 pp and 4 pp of the survival rate in the initial apprenticeship contract (Figure 1.2). Most youth exiting from an apprenticeship at the beginning of the fourth and fifth year transform their contract into an open-end job within the same firms. Differently, the share of open-end jobs in other firms constantly increases over time (Figure 1.3). The employment rate in the salaried private sector decreases until the third year (these are people exiting early from the contract and not starting another job) and after it stabilises just below 65%. At the end of the fifth year, 64% of the youth are still employed and 37% have an open-end contract (16% within the same firm, 21% in another firm - Table 1.3). Concerning youth in other temporary jobs, 11% still have an apprentice contract (of which 3% in the initial firm), 12% have another temporary contract, 2% are an external collaborator. Finally, 2% of the youth are in unemployment schemes and 36% are not in our database anymore. As the apprenticeship regime grants unemployment benefits only in special cases, most of the latter are likely uninsured unemployed (we refer to this category as "out-of-database").[9]

Table 1.3: Evolution of the share of youth in several contracts (%)

	Year (end)	Appr. initial firm (1)	Other appr. (2)	Open-End same firm (3)	Open-End other firms (4)	Temporary (5)	Collaborator (6)	Insured unemployed (7)	Out-of-database (8)
N = 17,950	1	48.8%	9.9%	1.6%	4.0%	8.4%	0.9%	0.1%	26.4%
	2	32.3%	13.3%	3.8%	7.5%	9.0%	1.1%	0.6%	32.5%
	3	20.3%	13.2%	8.0%	11.3%	10.3%	1.1%	1.2%	34.6%
	4	8.4%	11.1%	14.0%	16.4%	11.6%	1.3%	1.8%	35.4%
	5	3.0%	8.6%	15.7%	20.9%	11.8%	1.8%	2.4%	35.9%

Inflow sample of 17,950 apprentices hired in 2007 aged 19-24. Status at the end of the year after hiring: (1) apprenticeship in the first firm, (2) other apprenticeship, (3) open-end contract in the same firm, (4) open-end contract in another firm, (5) other temporary contract, (6) collaborator, (7) insured unemployed, (8) not in salaried employment in the private sector. Individuals with more jobs are considered only in one position following this order: initial apprenticeship, open-end contract (same, other firms), other apprenticeship, other temporary contract, collaborator, insured unemployment and not in salaried private sector employment.

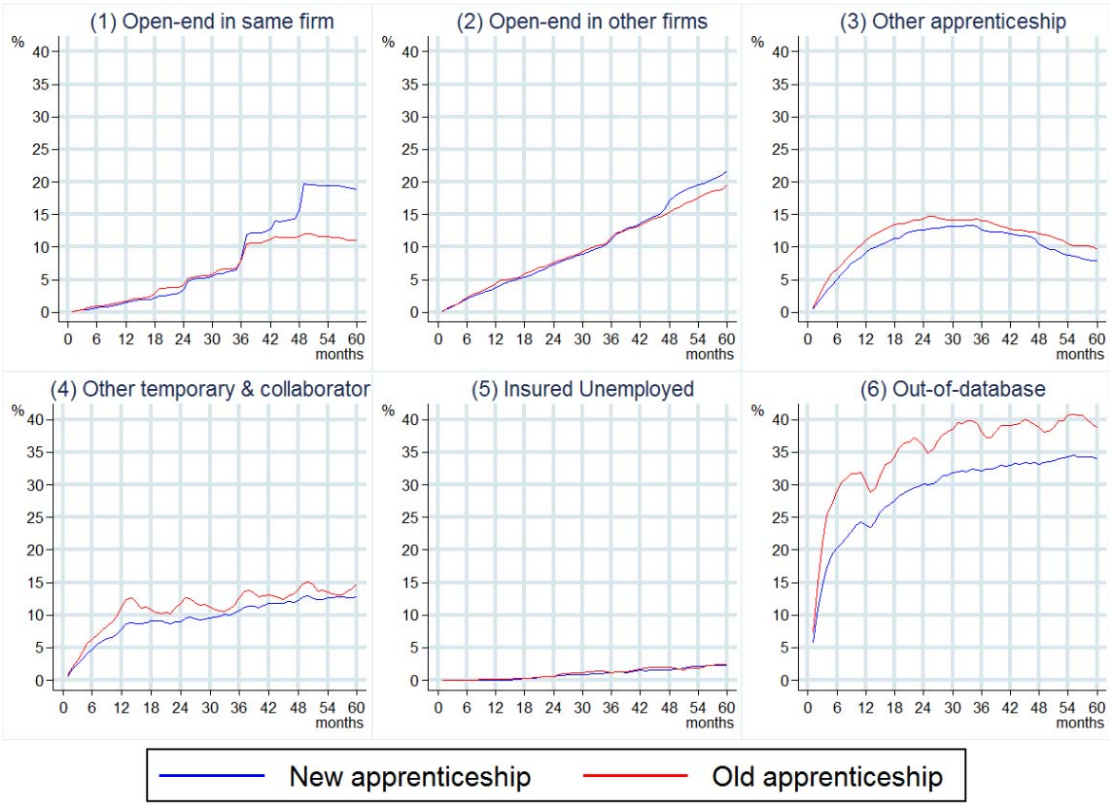
Noticeable differences can be observed when we look at the firm size where the apprentice is hired. First, the early dropout is much more pronounced in small firms with less than ten employees (Table 1.14 in Appendix 1.8.2). One of the possible explanations could be the looser dismissal rules for firms below fifteen employees in Italy and the larger incentive to churn due to higher tax rebates. The most important difference is observed when we look at the conversion to open-end contracts. After five

[9] The database does not contain self-employed, public employees, inactive and uninsured unemployed.

years, apprentices in large firms are much more likely to have a permanent job in the same firm (22% versus 12% in small firms) and just slightly more in other firms (22% versus 20%). This is reflected by a smaller number of people who are not in salaried private sector employment (30% versus 39%). Large firms seem to be able to offer better career opportunities to the apprentices though two-thirds of the apprentices are hired in firms with less than ten employees. An explanation for the high popularity among small firms is the larger tax rebate that these firms enjoy.

If we look at differences by gender, fewer men work in other temporary jobs at the end of the fifth year (-3 pp). Conversely, their share of non-employed in the salaried private sector is higher (+4 pp). Part of this can be explained by the higher number of men working as independent workers after an apprenticeship (ISFOL, 2013). Other differences are observed when we compare the apprentices with previous work experience to the apprentices in their first job. As expected, the first are more likely to move to an open-end contract at the end of the fifth year (38% versus 34%). Furthermore, they show smaller chances to end up exiting the salaried private sector employment (34% versus 40%).

Figure 1.3: Evolution of the employment rate by type of apprenticeship – inflow sample in 2007



Inflow sample of 17,950 apprentices hired in 2007 with 19-24 years old. Status at the end of the month: (1) Open-end contract in the same firm, (2) open-end contract in other firms, (3) other apprenticeship, (4) other temporary contract & collaborator, (5) insured unemployment, (6) not in salaried private sector employment. Individuals with more jobs are considered only in one position following the above order. Blue Lines: apprentices hired in the new apprenticeship. Red lines: apprentices hired in the old apprenticeship.

As explained in Section 1.2, two apprenticeship regimes coexisted depending on the geographic region and the sector of activities of the firm at the moment of hiring. In our inflow sample 10,758 and 7,192 apprentices were hired with the new and the old regime. If we split the sample by apprenticeship regime we observe noteworthy differences. As showed in Figure 1.3, apprentices in the new regime tend to transit more to open-end jobs from the fourth year onwards, especially within the same firm.

Apprentices in the old regime move more to temporary contracts and other apprenticeship. Besides, an important share of apprentices in the old regime moves out of our database already in the first months. For these youths we observe a cyclical pattern for both the share of youth out-of-database and the share of temporary contracts, indicating some sort of seasonal work. This is likely caused by the implementation of the reform by CBAs, which saw some sectors such as the tourism postponing the reform. Furthermore, the early implementing regions might differ in labour market characteristics affecting the future employment rate of the apprentices. Moreover, youth in regions with the old regime may self-select in the reform by migrating or commuting to early implementing regions.

As shown in Table 1.19 in Appendix 1.8.2, the type of apprenticeship regime is indeed correlated with several characteristics.[10] Because of the implementation via CBAs, the most noticeable difference is the concentration in sectors such as Wholesale, Retail Trade, Banking and Construction for the new regime and Tourism and Personal Services for the old regime. Seasonal work and smaller-size and single company firms are correlated with the old regime. New regime apprentices are characterised by a better employment history such as higher past remuneration and higher chances of having already experienced an open-end contract in the past. It is clear that the apprentices in the two regimes are different, which prevents us from interpreting the observed differences as the effect of the reform.

1.4.2. *Labour Force Survey (LFS) data*

In Section 1.6.5, we assess the impact of the reform on the employment rate, the transition from non-employment and the contract diffusion (Intention-To-Treat, ITT). While administrative data are suitable to compare the relative performance of the two regimes, the main drawback is that they cannot be used to assess the effects on the youth labour market. First, administrative data do not contain information on non-employed individuals who are not in insured unemployment schemes. Second, the region where the individual live, essential to determine when the new regime becomes operative, is available only for the year 2012. Hence, for these evaluations we use a stock sample of the youth from the Italian Labour Force Survey (LFS) which counts 0.3% of the Italian population.

The Italian LFS consists in a stratified dataset from a two-stage sample selection where the primary and secondary sampling units are municipalities and households. These are interviewed during two consecutive quarters (first and second waves) and again at one-year distance (third and fourth waves - 2:2:2 rotation rule). Households moving to other municipalities are removed. Due to the compulsory

[10] From now on, we remove 36 units because of missing information on the covariates, ending up with 17,914 apprentices.

participation enforcement, the response rate is relatively high (e.g. in 2008 it averaged 88.2%, compared to 80% in the other EU and EFTA countries - European Commission, 2010).[11] We use the quarterly LFS (cross-sectional) rather than the longitudinal LFS as it tends to be more precise thanks to the higher number of observations (the longitudinal one has only one cross-section per year). Furthermore, it allows us to identify better the moment when the reform starts (Table 1.13 in Appendix 1.8.2). The sample size by regional treatment groups is showed in Table 1.15, while Table 1.16 regards only the employed individual and include also the treatment by sector.[12] As the CBAs implementing the reform are highly specific, to define the latter we look at the sectors where the most important compartments had an agreement, and we approximate the treatment by a set of NACE dummies (Section 1.2.2). The sources we refer to are ISFOL (2007, 2010) and our administrative data.

We stop the analysis in the first quarter 2011 to isolate anticipation effects of the new reform of September 2011. As individuals become treated in a different moment of time, some regions and sectors implement the reform in very different business cycles. Descriptive evidence shows that the financial crisis severely affected the usage of apprenticeship contracts (-22% on the number of apprentices from 2008 to 2011 - ISFOL, 2012). To have an estimate on a specific business cycle, we also end the analysis on the 3rd quarter of 2008 and estimate the impact in the shorter-run.

Compared to our sample from INPS, LFS data are more suitable to study the effects of the reform on the youth employment rate. However, when we look at descriptive statistics on the share of apprentices in the economy, the LFS shows a discrepancy with respect to administrative data. If we consider the share of employees with an apprenticeship contract between 20 and 29 years old, the LFS reports it to be 9.2%. According to administrative data from INPS, the share is much higher: 19.5% (2010 data). The reasons for such a discrepancy may be multiple. First, youth might be unaware of their contract. Second, there may be a higher non-response from the apprentices (and the survey weights are not able to correct for it). Third, among the public employee the LFS allow us to identify only the ones in the public administration (from the NACE sector information). Thus, descriptive statistics on the LFS include also part of the youth working in the public sector - where the apprenticeship cannot be used. Finally, differently from administrative data, in principle the LFS also contains workers in the informal sector – which raises the denominator of the share.[13] In case the LFS really underestimates the proportion of apprentices in the economy, then the ITT estimated on these data are also a downward biased estimate of the true effect of the reform.

[11] For our analyses, we do not rely on the survey weights as, in the absence of endogenous stratification, it is not guaranteed that such weights reduce bias, while they increase the variance of the estimates (Cameron and Trivedi, 2005).
[12] Since the Regional Law of Puglia was annulled by the Constitutional Cohort, we remove the region as it is not clear when the actual treatment starts. We also remove the province of Trento and Bolzano as they cannot be separated in the LFS.
[13] ISTAT (2010) estimate that in 2009 the jobs in the informal economy counted 12.2% of total employment.

1.5. Identification Strategy

In this section, we describe the identification strategy to estimate the effects of the reform. In Section 1.5.1, we describe the Covariate Balancing Propensity Score estimators used to assess the Average Treatment Effect on the Treated apprentices (ATT) and their transition to other contracts (administrative data – Sections 1.6.1-1.6.4). In Section 1.5.2, we present the DiD estimator to assess the Intention-to-Treat effect (ITT) on the employment rate, the transition from non-employment and the contract diffusion (LFS data – Section 1.6.5).

1.5.1. *Treatment Effect on the transition from the apprenticeship*

In this section, we describe the identification strategy to estimate the effect of the reform on the apprentices. In the analysis we use an inflow sample of apprentices aged 19-24 hired in 2007 from administrative data of Social Security Institution (see Section 1.4.1). The outcomes of analysis are the share of youth in specific contracts over the following five years with monthly observation.

We focus on the Average Treatment effect on the Treated (ATT) which represents the impact of the reform on the individuals in the new regime (i.e. treated). Other causal parameters will be also considered such as the Average Treatment Effect (ATE) and the Average Treatment effect on the Untreated (ATU). The latter represents the effect of the reform on the apprentices in the old regime (i.e. control) while the first is a weighted average of the ATU and the ATT on the relative sample size of the treated and control groups. In general, the ATU and the ATT differ if the treatment response is heterogenous and depends on the type of participants. As apprentices in the old regime have characteristics that may drive a different response such as the smaller firms-size, in a sensitivity analysis we estimate the ATU and the ATE. Retrieving the ATU is a symmetrical problem to the estimation of the ATT and in the following formulas can be obtained by replacing D with (1-D).

The ATT is defined as the difference between the (observed) outcomes of the treated under treatment and the outcome in case the treatment did not take place. In formula, this can be written as follows, with D equal to one when we refer to the treated group, Y^1_i the individual outcome in the case of treatment, and Y^0_i in the absence of treatment.

$$ATT = E(Y^1 \mid D=1) - E(Y^0 \mid D=1)$$

As it is not possible to observe the counterfactual, we have to rely on a control group not receiving the treatment that is identical to the treated units in all the relevant characteristics affecting the outcome Y. In our analysis, we use the apprentices hired with the old apprenticeship contract to form such a control group. The main assumptions needed to estimate the ATT are the following:

- *Conditional Independence Assumption (CIA)*: conditional on Xs characteristics, the potential outcome in the absence of treatment Y^0 is independent on the treatment status D.

$$Y^0 \perp D \mid X$$

- *Stable-Unit-Treatment-Value Assumption (SUTVA):* the potential outcome in the absence of treatment Y_i^0 of the unit *i* is not affected by the treatment of other individuals (Rubin, 1980).
- *Common Support*: given the units' characteristics, the treatment should not be certain.

$$\Pr(D{=}1 \mid X) < 1$$

First, the CIA assumption requires conditioning on Xs characteristics not influenced by the treatment. Including such variables might eliminate part of the treatment effect if the policy affects their value (Pearl, 2000). In our analysis the Xs variables concerning the history and information on the last job are measured 30 days before the start of the apprenticeship to isolate potential anticipation effects. A few time-varying characteristics are measured the day the treatment starts (i.e. part-time work and reason of hiring – see Section 1.5.1) to control for differences in the actual type of apprenticeship between the two regimes.[14] Second, the SUTVA can be a strong assumption in presence of important policy-measures since spillovers such as displacement effects or changes on the general level of wage might change the behaviour of non-participants. Since the apprentices in the two regimes do not directly compete in the same labour market, as they work in different regions or economic sectors, the magnitude of the bias coming from the failure of the SUTVA is likely not large.

As it is difficult to find individuals similar in all the covariates (i.e. *the curse of dimensionality*), we rely on the Propensity Score (PS). This represents the likelihood of the individual to be treated given her characteristics. If the PS is correctly estimated, units with similar PS values should also have similar observable characteristics, which means similar potential outcomes in the absence of treatment.

$$Y^0 \perp D \mid P(X)$$

Researchers do not usually know the true PS but they estimate it. If the model estimating the treatment probability is correctly specified, after reweighting or matching the sample the two groups should have a similar distribution of the observable characteristics. For example, the Inverse Probability Weighting estimator (IPW – see e.g. Horvitz and Thompson, 1952; Hirano et al., 2003) balances the covariates of the control group to match the treated group distribution by reweighting the individual outcomes of the control by $\frac{\hat{P}_i(X)}{1-\hat{P}_i(X)}$. As the IPW estimator performs better if the weights are normalized to one (Busso et al. , 2014), the ATT is estimated as follows:

[14] In a sensitivity analysis we exclude those two variables and results do not significantly change (Figure A.7 in Appendix A).

$$\text{ATT} = \frac{1}{N_1}\sum_i^N D_i Y_i - \frac{1}{N_0}\sum_i^N \left(\frac{\frac{\hat{P}_i(X)}{1-\hat{P}_i(X)}}{\frac{1}{N_0}\sum_i \frac{\hat{P}_i(X) * (1-D_i)}{1-\hat{P}_i(X)}} (1-D_i)Y_i \right) = \frac{1}{N_1}\sum_i^N D_i Y_i - \frac{1}{N_0}\sum_i^N (W_i(1-D_i)Y_i)$$

With D_i equal to one for the treated units and zero for the controls, N the sample size, N_0 the number of control units, X the vector of K covariates and $\hat{P}(X_i)$ the estimated Propensity Score given X. The ATT can be estimated by regressing the outcome variable Y_i on the treatment group variable using a weight of one for the treated and W_i for the control units.

Misspecification of the model estimating the PS can lead to unbalanced covariates, which may bias the estimated ATT (e.g. Smith and Todd, 2005; Kang and Schafer, 2007). To attenuate the bias, Imai and Ratkovic (2014) propose the estimate $\widehat{\mathrm{P}}_{\mathrm{i}}(\mathrm{X})$ by the Covariate Balancing Propensity Score estimator (CBPS). Imai and Ratkovic (2014) argue that the CBPS estimator drastically improves the performance of the IPW estimator. Frölich et al. (2015) confirm this as it appears to be the overall best performing semi-parametric estimator, outperformed only by some non-parametric estimators.[15]

Within the framework of the empirical likelihood approach, the CBPS extends the Inverse Probability Weighting estimator by the means of a PS also maximizing the balancing of the covariates. Differently from the other estimators recently proposed, which just maximize the balancing (e.g. Entropy Balancing - Hainmueller, 2012), the CBPS still models the probability of being treated given the X characteristics of the individual, similarly to the standard Maximum Likelihood (ML) approach.

In other words, the CBPS estimator maximizes the mean independence between the treatment and the vector of X_i covariates of dimension K ("balancing moment condition" - equation 1) and the first-order conditions like a logit model (i.e. expected score equal to zero – equation 2). Equation 3 shows the J-statistic which is minimised in the estimation of the coefficients in a similar way to an Instrument Variable estimator with many instruments (over-identification). In particular, J can be minimised by using the two-step or the continuous updating GMM estimator (Hansen et al., 1996).[16]

$$BAL(\beta) = \frac{1}{N_1}\sum_i^N D_i X_i - \frac{1}{N_0}\sum_i^N \left(\frac{\hat{P}(X_i,\beta)}{1-\hat{P}(X_i,\beta)} (1-D_i) X_i \right) \quad (1)$$

$$s(D_i, X_i, \beta) = D_i \frac{\frac{d\,\hat{P}(X_i,\beta)}{d\,\beta}}{\hat{P}(X_i,\beta)} - (1-D_i) \frac{\frac{d\,\hat{P}(X_i,\beta)}{d\,\beta}}{1-\hat{P}(X_i,\beta)} \quad (2)$$

$$\hat{\beta} = \arg min_\beta \ \mathrm{J} = \arg min_\beta \left[\bar{g}(D,X,\beta)' \left(\vartheta(D,X,\beta)\right)^{-1} \bar{g}(D,X,\beta) \right] \quad (3)$$

[15] Frölich et al. (2015) complement the work of Huber et al. (2013) by adding non-parametric and recent semi-parametric estimators. The best performing estimator of Huber et al. (2013), a radius matching estimator combined with regression proposed in Lechner et al. (2011), is outperformed by the CBPS estimator.

[16] As recommended by the authors, we use the continuous updating GMM as it has better finite sample properties.

Equation 1 regards the balancing moment conditions, while Equation 2 is about the first-order condition and $s(D_i, X_i, \beta)$ is the score function of the ML estimator of β. Equation 3 regards the maximization of the balancing and the log-likelihood (i.e. minimization of the deviations of equation 1 and 2 from zero). In particular, $\bar{g}(D, X, \beta) = \frac{1}{N}\sum_{i=1}^{N} g(D_i, X_i, \beta) = \frac{1}{N}\sum_{i=1}^{N} \binom{s(D_i, X_i, \beta)}{BAL(\beta)}$, which combines the balancing and the score conditions. Finally, $\vartheta(D, X, v)$ is the covariance matrix:

$$\vartheta(D, X, \hat{\beta}) = \frac{1}{N}\sum_{i}^{N} \begin{pmatrix} \hat{P}(X_i, \beta)(1 - \hat{P}(X_i, \beta))X_i X_i' & N\,\hat{P}(X_i, \beta)X_i X_i'/N_1 \\ N\,\hat{P}(X_i, \beta)X_i X_i'/N_1 & N^2 \hat{P}(X_i, \beta)/[N_1^2(1 - \hat{P}(X_i, \beta)]\,X_i X_i' \end{pmatrix}$$

It is possible to test for misspecifications of the model by using Hansen's J-statistics (eq. 3) as in a GMM framework with many instruments. The J-statistics follows a Chi-squared distribution with K degrees of freedom under the null hypothesis of correct specification.[17] Finally, the Covariate Balancing Propensity Score $\hat{P}(X_i, \beta)$ is used to estimate the ATT similarly to the IPW estimator.[18] While efficient Standard Errors (SEs) can be obtained by bootstrapping, we rely on conservative robust SEs due to the computing intensive estimation of the continuously updating approach with many covariates. As we follow individuals over time, SEs are clustered by individual to take into account serial correlation.

Covariates

The literature offers little guidance about the choice of covariates and mostly argues that the researcher should include those variables affecting the treatment and the outcome. For our case there are two sources of selection in the reform of the apprenticeship: the Regional Laws and the Collective Bargaining Agreements (CBAs). In Appendix 1.8.1, we perform some tests on the selection into treatment by Regional Laws. First, we test whether there are signs of different labour market characteristics between early and late implementing regions, which may affect the future employment of the treated apprentices. Second, we verify that the reform did not increase the migratory or commuting flows to early implementing regions. No evidence of endogenous selection into regional treatment is found and early implementing regions do not statistically differ in the main labour market characteristics.

The second source of selection is related to firm-specific characteristics such as sector and dimension. This is likely very important as sectors and firm-size are also correlated with different transformation and churning incentives. While we can control for this source of selection in a very detailed way (26 sectoral and 12 firm-size dummies), it is not possible to fully isolate it as the CBAs are highly specific. Thus, we use other available information to capture the remaining unobservable differences (possibly

[17] However, the authors warn that the test might lack statistical power.
[18] We eventually normalise the weights as this should improve the performance of reweighting estimators.

also in the regional selection) which might affect the outcome. These covariates may also help us to control for changes in the hiring behaviour of the firms induced by expectations on the future implementation of the reform. Furthermore, we include the quarter of hiring as the available apprenticeship regime evolves over time (e.g. the tourism sector passes the CBA during 2007).

In the analysis we use observable characteristics on the individual employment history and firms' characteristics which are likely relevant in affecting the outcomes. Thanks to a very rich database, we end up with 117 variables. The covariates can be regrouped in characteristics measured at the moment of hiring driving the selection (e.g. sectors, dimension and position of the firm, quarter of hiring and seasonal apprenticeship), apprentices' characteristics (age and gender), information about the last job[19] in the salaried private sector (e.g. type of contract, gross remuneration, reason of ending the contract, part-time, length of contract, same firm or sector) and previous employment history in the salaried private sector and as collaborators (e.g. total experience, age in the first job, average full-time remuneration, number of jobs). The full list of variables is shown in Table 1.19 in Appendix 1.8.2.

Information about seasonal apprenticeship, firm position, the reason of hiring, previous experience in an apprenticeship in the same firm or sector (and its duration – see footnote 2) are included as they likely affect the duration of the new apprenticeship contract. Similarly, average job tenure, tenure in the last job, the number of previous jobs and job-to-job transition might be correlated with future contract length. Previous experience is important in determining the chances of moving to open-end contracts, while previous part-time work experience helps us give the right importance to the accumulated experience. Experience together with age at the first job and current age can give some insights on the education level of the apprentice. Information on the last contract (or experience by type of contracts) might be correlated with the likelihood of experiencing the same contract again in the future (e.g. scarring effect). Previous salary, the duration of the last unemployment spell and the reason for ending the last contract can be seen as a proxy for skills. A youth that has been in insured unemployment has already obtained eligibility for it, and in the future she will less likely transit to uninsured non-employment. Part-time apprentices might have fewer chances to improve their skills compared to full-time apprentices. Finally, experience by collar type can help us to understand if the apprentice is a blue or a white collar and the type of training received.

When we estimate the CBPS, we notice that among the workers' employment history the only statistically significant variables in determining the treatment probability is not having any previous work experience (significant at 1%), and the full-time weekly remuneration in the last job (significant at 10%, both with positive correlation - Table 1.18 in Appendix 1.8.2). As expected, variables on the

[19] This is the most recent job at least 30 days before the hiring (period to attenuate potential anticipatory effects).

demand side such as the sector, the firm size and position, and the quarter of hiring drive most of the selection. The lack of significance for most of the individual characteristics confirms our claim of no individual self-selection into the treatment. Yet, including them can decrease the variance if they affect the outcome (Brookhart et al., 2006). As reported in Table 1.19 in Appendix 1.8.2, the modelled selection into the treatment is moderate as the Pseudo R-squared of a standard logit model is 0.179.

After estimating the PS, we trim treated units with too high PS and not having a correspondent control unit in a neighbourhood. Indeed the lack of overlap of the PS can bias the estimator and increase the variance (Lechner and Strittmatter, 2014). We remove the treated units with a PS above the 99.9 percentile of the control units. As shown in Figure 1.11 in Appendix 1.8.2, the 99.9 percentile removes the thinnest part of the PS distribution. Huber et al. (2013) also propose to remove the control units with a weight higher than 4% of the total. However, because the sample of control units is large, this additional trimming is not required (the highest relative weight is only 0.4% of the total sample). After trimming we remain with about 94.1% of the treated units and a total of 17,277 individuals.

Despite the many covariates, the CBPS performs remarkably well in balancing their distribution: the median Standardized Bias (SB) is as low as 0.6%, the highest SB is 5.4%, the Pseudo R-squared of the reweighted sample is 0.003 and the Wald test[20] for the joint significance of the variables after the reweighting produces a p-value of 1.[21] The balancing tests are better than the ones obtained by the standard logit model (IPW). Indeed, IPW weights generate a median SB of 1.9%, the highest SB is 17.3%, the pseudo R2 of the reweighted sample is 0.013. The over-identification test on misspecification of the model does not reject the null hypothesis of good specification (p-value of 0.964). The better balancing comes to the detriment of slightly lower log-likelihood: -9926.05 versus -9905.23. We report the full list of balancing tests by CBPS-weights in Table 1.19 in Appendix 1.8.2. In conclusion, we argue that the (reweighted) differences between the apprentices with the new or the old regime can be interpreted as the causal effect of the reform on the basis of the following arguments. First, firms cannot decide the type of apprenticeship regime, since it depends on the sector and region at the moment of hiring. Second, we do not find evidence on individual self-selection to treated regions regarding migratory or commuting flows (Appendix 1.8.1). Third, no statistical differences in labour market characteristics are observed between treated and control regions before the reform. Fourth, we control for the sector and the firm-size dimension in a very detailed way, while remaining unobservable differences are captured by conditioning on the whole apprentices' employment history.

[20] The Wald test should be used to assess the balancing as the log-likelihood ratio test proposed by Sianesi (2004) tends to over-reject the null of good balancing of a reweighted sample due to the heteroskedasticity induced by the weights.

[21] If we trim by the max-rule the balancing tests are not so good (median SB of 0.8%, the highest SB is 10.5%, the Pseudo R-squared of the reweighted sample is 0.004), while without trimming the balancing tests are noticeable worse (median SB of 0.9%, the highest SB is 20.3%, the Pseudo R-squared of the reweighted sample is 0.009).

1.5.2. *Intention-To-Treat (ITT)*

In this section, we describe the identification strategy to estimate the Intention-To-Treat effect (ITT) on the youth employment rate in the salaried private sector, the transition from non-employment and the contract diffusion. For this analysis, we use data from the cross-sectional LFS (Section 1.4.2).

As the Italian regions are heterogeneous in terms of economic characteristics, we cannot directly compare the outcome between regions that had previously enacted the regional Law and the ones that have not (yet) implemented. Indeed, this would produce biased estimates as it captures also pre-existing differences. It is possible to remove time-invariant unobserved heterogeneity by implementing a Difference-in-Differences (DiD) estimator. This estimator subtracts the differences observed in the pre-treatment period from the differences observed in the treatment period. Any remaining variation is interpreted as the effect of the policy. The key assumption of the DiD estimator is that in the absence of the policy the different regions follow a similar trend for the outcome considered ("parallel path"). While we test whether this held during the period 2000-2003 (Appendix 1.8.1), as "placebo tests" we also test if there is a deviation from the parallel path 12-7 months before the implementation of the policy. Alternatively, we rely on the "*parallel growths*" assumption by controlling for pre-existing differences in trends among regions and assuming that these would have been observed also in the treatment period in the absence of the policy-change.

The common trend assumption is more realistic if the two treatment groups do not show a compositional change and a common response to the business cycle. The first condition is likely violated in our analysis as the composition of the treated and the control groups partially changes in each quarter (due to the rotating panel sampling of the LFS - Section 1.4.2). One can control for the compositional change by adding in the DiD exogenous control variables determining different evolution of the outcome. As most of the information in the LFS is related to labour market outcomes or other potentially endogenous variables,[22] we do not have many available covariates. However, we can importantly control for education in a very detailed way ($\mathrm{X_{it}}$), which is one of the most important predictor of youth employment. In addition, we include information on the gender of the individuals. Controlling for such variables might strengthen the common trend assumption by attenuating the compositional change. The distribution of the variables on the education level of the sample can be found in Table 1.17 in Appendix 1.8.2.

We implement the DiD estimator by using many treated groups and time periods (e.g. Bertrand et al., 2004). In particular, we control for regional fixed-effect (dummies $\mathrm{dREGION_r}$), common shocks (calendar time dummies $\mathrm{dT_t}$), and the above-mentioned set of exogenous control variables $\mathrm{X_{it}}$. We use

[22] For example, household composition is endogenous as youth might move out of the family house if they find a job.

a specification with a unique treatment dummy for all the quarters after the implementation of the reform by Regional Law (A=1). However, this leads to biased estimates if the effects are not homogenous over time (for a survey on DiD see Lechner, 2011). Thus, our preferred specification has treatment dummies ($TREATMENT_a$) per each *a^th^ year* after the implementation of the reform in the region, where we gather all the remaining quarters after the sixth year (A=6).[23] As the enactment of a Regional Law is a long and possibly anticipated process, we control for anticipation effects by adding a treatment dummy for the six months before the implementation of the reform (time a=0). As placebo tests, we rerun the analysis adding a placebo dummy for the two quarters before the anticipatory dummy (a = -1)[24] Hence, the DiD regression takes the following form:

$$Y_{it} = \sum_{t=1}^{T-1}(\vartheta_k dT_{it}) + \sum_{r=1}^{R}(\eta_r * dREGION_{ir}) + \sum_{a=0}^{A}(\delta_a * TREATMENT_{ia}) + \sum_{k=1}^{K}(\beta_k X_{it}) + \epsilon_{it}$$

Our sample changes depending on the outcome considered. First, we use the whole population (excluding students) for evaluating the impact on the employment rate in the salaried private sector. Second, we assess the effect on the transition from non-employment by considering people self-reporting not to be employed (or student) one year before t. For robustness, we also control for ex-ante difference in regional trends by including a linear trend per each region ($t * dREGION_r$ i.e. *"parallel growths or trend-adjusted DiD"* - Wolfers, 2006). In this specification the treatment interaction ($TREATMENT_a$) is referred to each *q^th^ quarter* after the implementation of the Regional Law to avoid the trend capturing part of the treatment effect (A = Q). Since we saturate the model in the treatment period, the estimated trend is only in the pre-treatment period. Note that identification of the trends requires normalizing an additional time dummy to zero.

$$Y_{it} = \sum_{t=1}^{T-2}(\vartheta_k dT_{it}) + \sum_{r=1}^{R} dREGION_{ir}\,(\eta_r + \gamma_r t) + \sum_{a=0}^{\mathbf{Q}}(\delta_a * TREATMENT_{ia}) + \sum_{k=1}^{K}(\beta_k X_{it}) + \epsilon_{it}$$

As sector CBAs could also implement the treatment, this might affect the employment rate in not yet treated regions. Thus, for robustness we perform the DiD with multilevel treatment (Abadie, 2005) in which the intensity of treatment ($TREATMENT_i$) reaches the maximum of one for people living in regions passing the Regional Laws. For people living in regions not yet treated, the intensity of treatment in t is equal to the share of employees working in 2004 in a sector who passed the CBAs by t (the maximum intensity is one – the share is referred either to the full or the young population). As

---

[23] We gather the quarters by year to produce more precise estimates. For the analysis only on the short run (ending on the 3rd quarter 2008) the treatment dummies end on the 4th year after the treatment (A = 4).

[24] The placebo dummy (a = -1) cannot be longer than 2 quarters or the regions implementing the policy in 2005q2 do not have any quarters left before the placebo as the dataset starts on the 1st quarter of 2004.

the treatment intensity increases over time for the same regions, in this specification we cannot distinguish between shorter and longer run effects of the treatment dosage.

$$Y_{it} = \sum_{t=1}^{T-1} (\vartheta_k dT_{it}) + \sum_{r=1}^{R} (\eta_r * dREGION_{ir}) \; + \; \delta * TREATMENT_i \; + \; \sum_{k=1}^{K} (\beta_k X_{it}) + \epsilon_{it}$$

Finally, we analyse the impact on the diffusion of the apprenticeship contract by considering only the salaried employees, excluding the public administration. Besides estimating the effects on the share of apprenticeship contracts, we test whether a substitution from the other contracts occurs (i.e. ITT on other temporary contracts, open-end contracts, collaborator contracts, and self-employed[25]). As the treatment could also start after the signing of a sector Collective Bargaining Agreement or regional-sectoral pilot tests, we control for sector fixed-effects ($dSECT_S$) and the treatment dummies take the value of one also when the worker's sector had implemented the CBA (or a regional pilot test). Once the sector (region) is treated, we ignore successive regional (sector) implementation.

$$Y_{it} = \sum_{t=1}^{T-1} (\vartheta_k dT_{it}) + \sum_{r=1}^{R} (\eta_r * dREGION_{ir}) \; + \sum_{s=1}^{S-1} (\rho_S \; dSECT_{iS}) + \sum_{a=0}^{A} (\delta_a * TREATMENT_{ia}) \; + \sum_{k=1}^{K} (\beta_k X_{it}) + \epsilon_{it}$$

Another issue regards the inference. As individual outcomes in the same regions are likely correlated, we cluster the Standard Errors (SEs) by region. These large clusters allow us also to take into account for individual serial correlation between cross-sections, as individuals changing municipality are not re-interviewed in the LFS. Since the number of clusters is relatively small (17 after removing Puglia and Trentino Alto Adige), we use the wild cluster bootstrap-t procedure proposed by Cameron et al. (2008).

The wild bootstrap estimates the p-value of a coefficient as follows. First, it estimates the u_r residuals from the constrained OLS estimation imposing the null hypothesis (H_0: $\delta = 0$) to improve efficiency (Davidson and MacKinnon, 1999).[26] Afterwards, the residuals of all the units in each cluster are transformed with a fixed probability, which in empirical work is usually the Rademacher transformation (i.e. the residuals change sign with 50% probability).[27] From the newly transformed residuals $\hat{u}^*_r$ we obtain a new outcome Y^*_i under the null hypothesis (predicted $\hat{Y}_i + \hat{u}^*_r$), which we regress on the unconstrained model and from which we obtain a t-statistics for the coefficient of interest (with Cluster-Robust SEs). The procedure is repeated B times and the bootstrapped p-value is obtained by looking at the position of the initial cluster robust t-statistics on the distribution of the bootstrapped t-statistics. In particular, the (equal-tail) bootstrap p-value is equal to the following, with

[25] For self-employment, we widen the sample to all the workers, only excluding the public administration.
[26] This does not allow constructing Confidence Intervals which would need to be bootstrapped too (MacKinnon, 2014).
[27] As we have a moderate number of clusters, the gain in following the adjustment of Webb (2014) is small.

t_j^* the bootstrapped t-statistics, *t* the t-statistics from the unconstrained equation with CRSE and B the total number of repetitions. We set B to 2000 to have precision of 0.001 (= 2* 1/2000).

$$p = 2 * \min\left(\frac{1}{B}\sum_{j=1}^{B} I(t_j^* \leq t), \frac{1}{B}\sum_{j=1}^{B} I(t_j^* > t)\right)$$

Finally, for robustness we rerun the Difference-in-Differences estimator in a nonlinear framework as the outcomes are binary variables. However, nonlinearities make the time effects on the outcome differ between the treatment and the control groups, which invalidate the common trend assumption on the outcome. One can still estimate the treatment effect by using the DiD estimator if the common trend assumption is transferred to the index function (latent dependent variable - Blundell and Costa Dias, 2009, p. 583; Lechner, 2011, p. 33). As shown by Puhani (2012), the treatment effect on the treated (ITT in our case) can be estimated by averaging the marginal probability effect of the treatment over the individuals belonging to the treated group in the treatment period.

$$\text{ITT} = \frac{1}{N_1}\sum_{i}^{N_1}\left(\Phi_i(\eta + \vartheta + \delta + \beta\,\mathrm{X_i}) - \Phi_i(\eta + \vartheta + \beta\,\mathrm{X_i})\right)$$

Where N_1 is the size of the treated group in the treatment period and $\Phi_i(\cdot)$ is the predicted probability from the nonlinear model for the generic treated unit *i* in the treatment period. The other scalars are the coefficients of the nonlinear model where η regards the treated effect, ϑ the time effect, δ is the coefficient (or marginal index effect) of the interaction of the treated group with the "after treatment" period and β is the marginal index effect of the other covariates. As the model is nonlinear, the treatment effect varies with the value of the covariates, which justifies the inclusion of $\mathrm{X_i}$. In practices, we run the nonlinear DiD estimator and predict the outcome for the treated group in each treatment period "a". The ITT is then obtained by subtracting from the average prediction (of each "a") the average counterfactual predictions of the same units, but imposing δ equal to zero.

The wild cluster bootstrap-t procedure is not applicable to a nonlinear model as the errors are not additively separable (Cameron and Miller, 2015, p. 33). As the treatment effect is zero if and only if the marginal index effect δ is zero (Puhani, 2012), we apply the Score bootstrap of Kline and Andres (2012) to obtain a pvalue on the marginal index effect of the treatment. The Score bootstrap works as follow. After estimating the nonlinear model, we generate the scores for all the observations which are, in the same spirit of the wild cluster bootstrap-t, transformed in each replication by using the Rademacher weights (note that this does not require re-estimating the Maximum Likelihood model).[28] Each bootstrap replication is then used to obtain a test statistic and eventually a bootstrapped p-value.

---

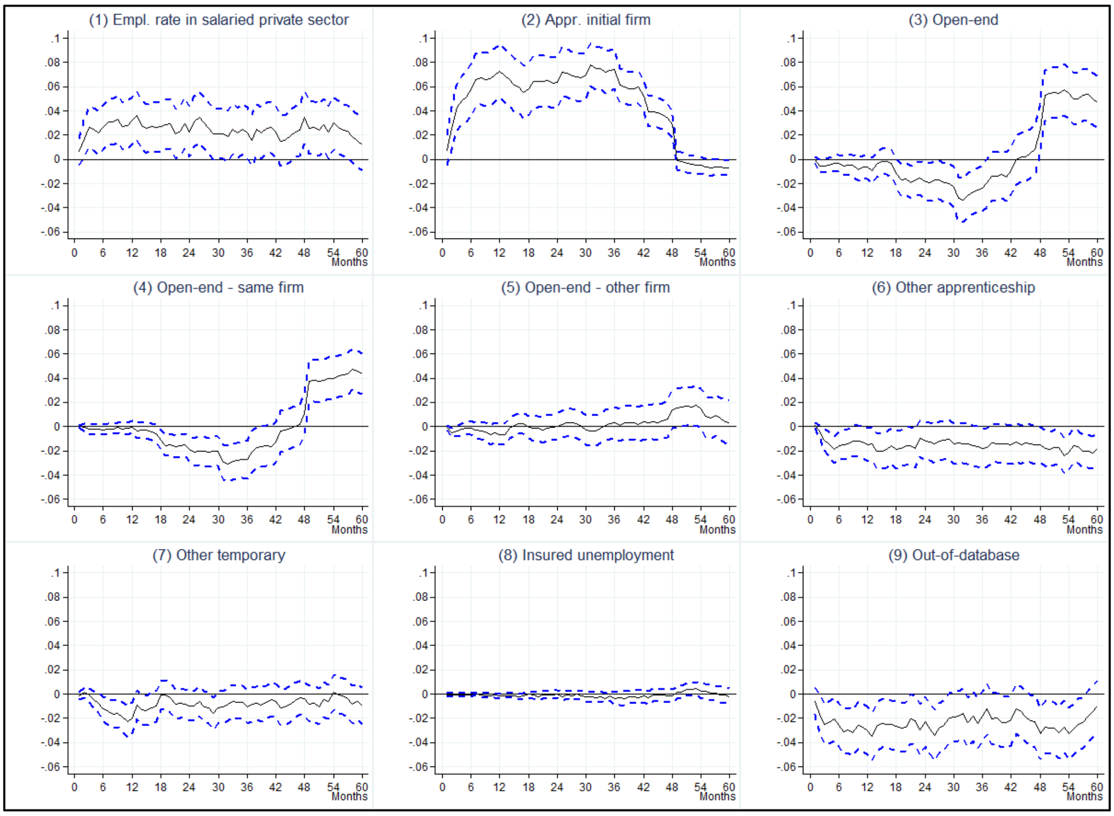
[28] The score is restricted to the null hypothesis (similar to Davidson and MacKinnon 1999), but the variance estimate is unrestricted ("Score2 Wald" - Kline and Andres, 2012).

1.6. Results

1.6.1. Transition from the apprenticeship: new regime versus old regime

In this section, we estimate the impact of the reform on the apprentices' employment rate in several contracts over the five years after hiring. The data and the identification strategy are described in Section 1.4.1 and 1.5.1. The counterfactual status is hired with the old apprenticeship regime, which only allowed training by external authorities and did not set minimum remunerations. In particular, we estimate the ATT on the share of youth in: (1) employment in the salaried private sector, (2) apprenticeship in the initial firm,[29] (3) open-end contracts (divided in (4) the same firm or (5) other firms), (6) other apprenticeship, (7) temporary contracts (fixed-term and collaborators), (8) insured unemployment and (9) out-of-database (not employed in the salaried private sector). The latter category likely includes most of the unemployed as the apprenticeship grant unemployment benefits only in special cases. As people can have multiple jobs at the end of each month, we assign to the individuals only a status in the order mentioned above.

Figure 1.4: ATT on the apprentices in the next five years



ATT by CBPS of the reformed apprenticeship versus the old apprenticeship on a sample of 17,914 apprentices hired in 2007 aged 19-24. Status at the end of each month after hiring (from left to right and top to bottom): (1) employment rate in the salaried private sector, (2) apprenticeship in the initial firm, (3) open-end contract (subdivided into the same firm (4) or another firm (5)), (6) other apprenticeship, (7) other temporary or collaborator contracts, (8) insured unemployed, (9) neither in salaried private sector employment nor in insured unemployment. Individuals with more jobs are considered only in one position using the order mentioned above. Standard Errors are clustered by individual to take into account serial correlation.

[29] The ATT on *another* apprenticeship in the same firm is negative but significant only in the first nine months (Figure 1.13).

By looking at Figure 1.4 and Table 1.20 in Appendix 1.8.2, it immediately appears that the early dropout is reduced already from the first months (-11.5% in the first two years). In particular, the share of youth in the apprenticeship in the initial firm increases by +5.8 pp in the first two years (+11.7%), +6.0 pp in the third and fourth year (+38.8%), and becomes zero from the fifth year. Since the impact is already observed in the first months and the minimum length of the pre-reform apprenticeship contract was 18 months, the estimated effect may not come from the longer duration of the reformed apprenticeship. The reduction of early dropout is more likely the result of the combined effect of the minimum remuneration and the higher (expected) productivity of the apprentices due to the enhanced firm-specific human capital. In Section 1.3.1, indeed, it was argued that these policy changes should discourage firms from churning and boost the conversion to open-end contracts.

This is confirmed from the estimates on the share of apprentices transiting to open-end contracts in the same firm. Indeed, we observe a boosted transformation rate from the fifth year of about +4.1 pp (+28.6%), which is significant at 1% level. The treated youth might have received a longer training thanks to the extended lock-in effect. This could have reinforced the effect of the internal training and the minimum remuneration on encouraging transformations at the termination of the contract.

The conversion to open-end contracts is reflected by a lower number of individuals not employed in the salaried private sector in the fifth year (-2.5 pp / -6.6% significant at 5%) and in another apprenticeship contract (-1.9 pp / -17.8% significant at 1% level). This suggests a diminished churning behaviour of the firms. Interestingly, four years after the hiring treated individuals do not show a lower share in open-end contracts in other firms, despite receiving less intensively the more general human capital via external training. A possible explanation is that, as shown in Section 1.2.2, only a part of the pre-reform apprentices benefited from the external training due to lack of public funding. Furthermore, as control units tend to exit more from salaried private employment, it is not granted that they actually received more general training than the treated. Overall, the combined lock-in and transformation effects had a long-lasting employment impact on the treated youth: +2.5 pp or +3.7%.

We estimate heterogeneous effects by looking at the different responses by firm size, gender and apprentices' experienced (Table 1.20 in Appendix 1.8.2). Noticeable differences are found when we look at the first dimension. We divide the sample between firms with less and more than ten employees at the moment of hiring, which also corresponds to the different eligibility to the higher tax rebate (see Section 1.2.1). Once we focus on the large firm-size group, the CBPS is not able to balance the dummy "hired in a firms employing more than 500 employees". Indeed, we do not have a sufficient number of apprentices in the old regime to balance the treated group: 57 versus 1,040 treated units. We remove the individuals in firms larger than 500 employees to have more robust estimates on the apprentices hired in firms employing between 11 and 499 employees (i.e. trimming on covariates).

Thus, the overall ATT is not necessarily between the ATT for the apprentices in the large and small firms.

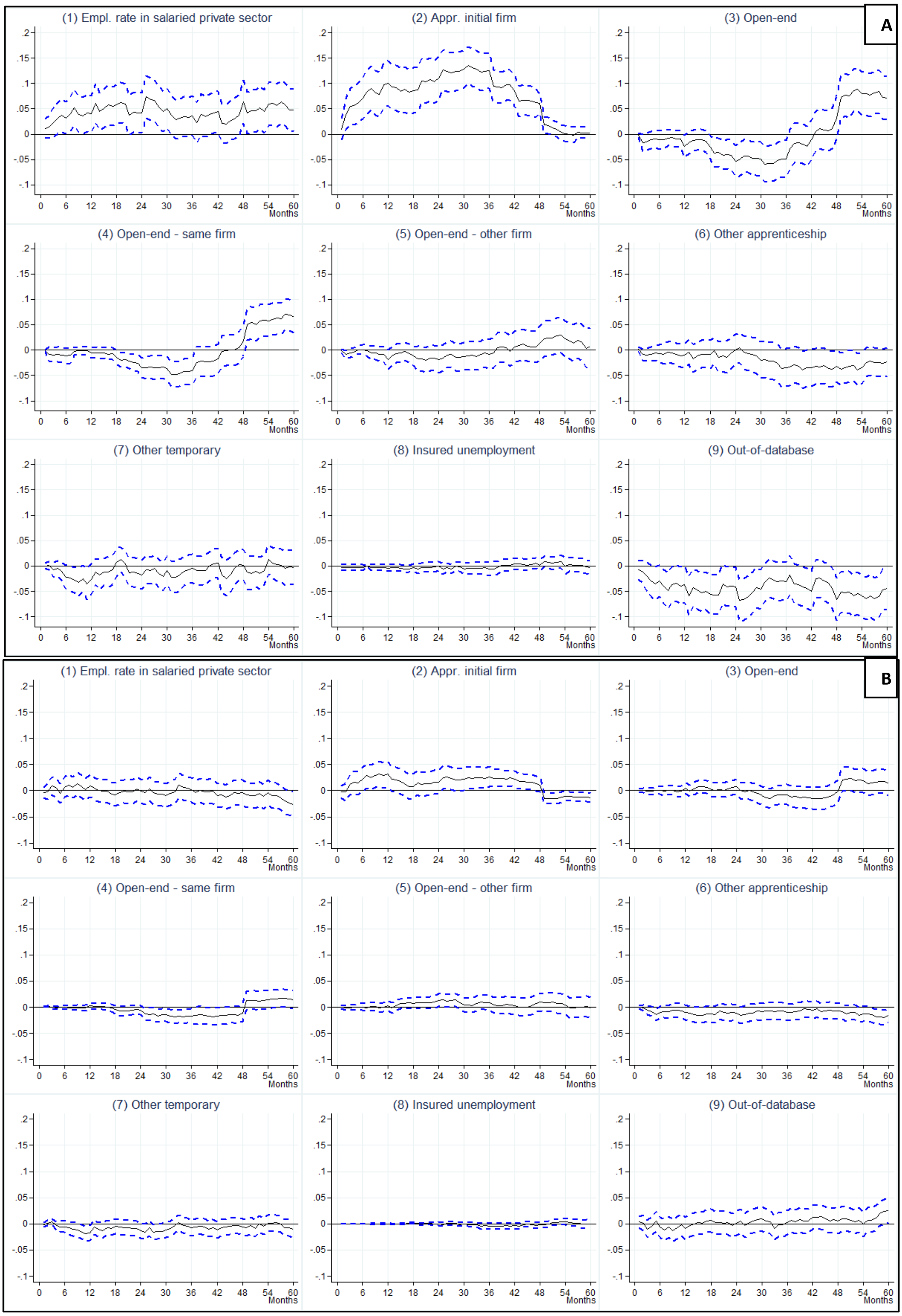
As shown in Figure 1.5, the positive effects that we have estimated on the overall treated sample seem to come mostly from the apprentices hired in large firms. The positive lock-in effect is much smaller in firms with less than ten employees. [30] In the fifth year, the different lock-in effect translates into very different chances of converting the apprenticeship to an open-end contract. In particular, though the effect is still positive for apprentices in small firms, it is small and significant only at 10% (+1.5 pp / +12.8%). In large firms the impact on open-end contract in the same firm is +6.0 pp / 36.3% in the fifth year, after the initial negative lock-in effect for the first four years. Large firms also show a reduction of the youth exiting from employment in the salaried private sector ("out-of-database"), while no significant impact is found for apprentices hired in small firms. Possible explanations for the low impact of the reform on small firms are the higher incentive to churn and the lack of firms' capabilities to perform on-the-job training, which prevented them from enjoying this part of the reform.

To obtain more insights we also estimate the ATT of performing the apprenticeship in a large firm rather than in a small one. We fix the reference characteristics to the ones of the apprentices in larger firms and we control for the type of apprenticeship regime (we remove size and position of the firm in the CBPS). It is clear from Figure 1.6 that apprentices hired in large firms have higher chances of transforming their contracts to open-end positions within the same firms and lower to end-up out of the database. Results are symmetric, but with some lower magnitude, if we fix the reference covariates distribution to the one of the apprentices in smaller firms (Figure A.8 in the Supplementary Appendix A). While part of these differences may be due to the different response to the treatment and share of treated, it is clear that larger firms have a higher transformation rate.[31] We argue that the different performance is driven by the stronger incentive to churn for smaller firms. Indeed, these firms are almost totally exempted from the payment of Social Security Contributions for the apprentices. This creates a strong incentive to keep on hiring new apprentices to enjoy cheap labour.

[30] In the first two years we estimate a lock-in effect of +1.7 pp / +3.5% (-3.4% on the dropout) versus +8.2 pp / +15.8% in the large firms (-17.0% on the dropout). In the third and fourth year we estimate +2.2 pp / +13.3% versus +10.3 pp / +66.5%.

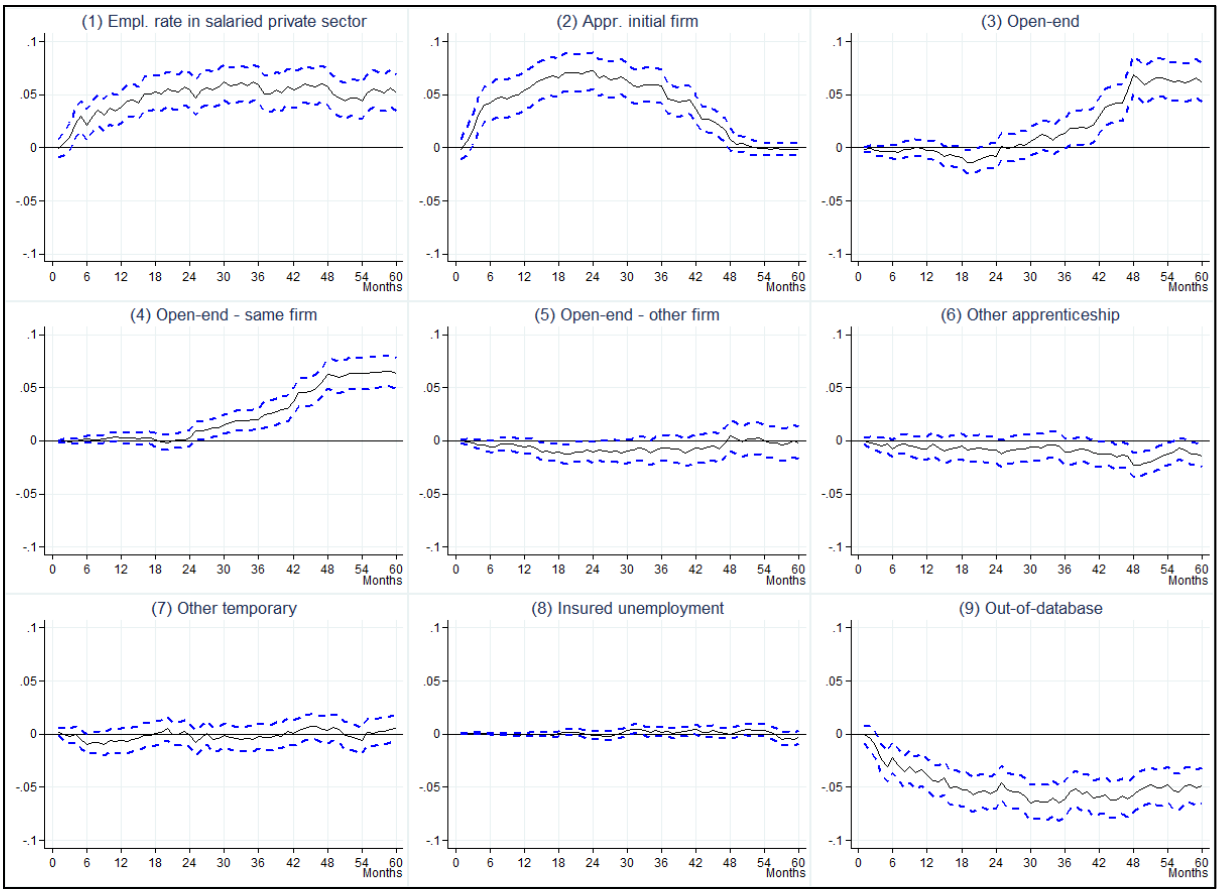
[31] In principle it is possible to compare apprentices within the same firm size and apprenticeship regime. However, this decreases even more the sample and it becomes relatively difficult to control for all the covariates.

Figure 1.5: ATT on the apprentices in the next five years by firm size (A large, B small)



ATT by CBPS of the reformed apprenticeship versus the old apprenticeship on an inflow sample of 17,914 apprentices. Panel A: Firms with at least 11 employees at the moment of hiring, Panel B: firms with fewer than ten employees. Status at the end of each month after hiring (from left to right and top to bottom): (1) in salaried private sector, (2) apprenticeship in initial firm, (3) open-end contract (in 4: in the same firm; 5: in another firm), (6) other apprenticeship, (7) other temporary or collaborator contracts, (8) insured unemployed, (9) neither in salaried private sector employment nor in insured unemployment. Individuals with more jobs are considered only in one position by the order mentioned above. Cluster Robust Standard Errors by individual to take into account serial correlation.

Figure 1.6: ATT - apprentice hired in large vs. small firm - CBPS



ATT by CBPS of apprenticeship in a large versus small firm (above or below 10 employees). Inflow sample of 17,914 apprentices. CBPS controlling for the type of apprenticeship regime and the above-mentioned covariates (without firm size and position dummies). Status at the end of each month after hiring (from left to right and top to bottom): (1) in salaried private sector, (2) apprenticeship in initial firm, (3) open-end contract (in 4: in the same firm; 5: in another firm), (6) other apprenticeship, (7) other temporary or collaborator contracts, (8) insured unemployed, (9) neither in salaried private sector employment nor in insured unemployment. Individuals with more jobs are considered only in one position by the order mentioned above. Cluster Robust Standard Errors by individual to take into account serial correlation.

In addition, we estimate the Average Treatment Effect on the Untreated apprentices (ATU) and compare it to the estimates on the treated apprentices (ATT). As control apprentices are concentrated in smaller firms we expect the reform to have lower potential in boosting their transformations to permanent positions. This is confirmed by our estimates showing a smaller lock-in effect, which is reflected in a lower impact on the conversions. Moreover, the reduction of transitions to other apprenticeship contracts is smaller and not significantly different from zero. This evidence might suggest that early implementing regions and sectors had a stronger incentive in accelerating the implementation of the reform as they were the ones enjoying the most from the liberalization of the training component. Contrarily, the sectors and regions benefitting the least from the reform postponed its implementation until later date. The effect of the reform on the overall apprentice population (ATE) and on the untreated (ATU) are reported in Figure 1.14 in Appendix 1.8.2.

Concerning other heterogeneous effects of the reform we estimate a stronger treatment response of the apprentices with previous work experience. The ATT without previous work experience is smaller for both the initial lock-in effect and the conversion to permanent jobs in the same firm (Figure 1.15 in Appendix 1.8.2). This might indicate that the most productive workers were the ones benefitting the most from the internal training. Finally, women and men show similar effects on the transformation within the same firm, but only women have a statistically significant reduction on people not employed in the salaried private sector (Figure 1.16 in Appendix 1.8.2).

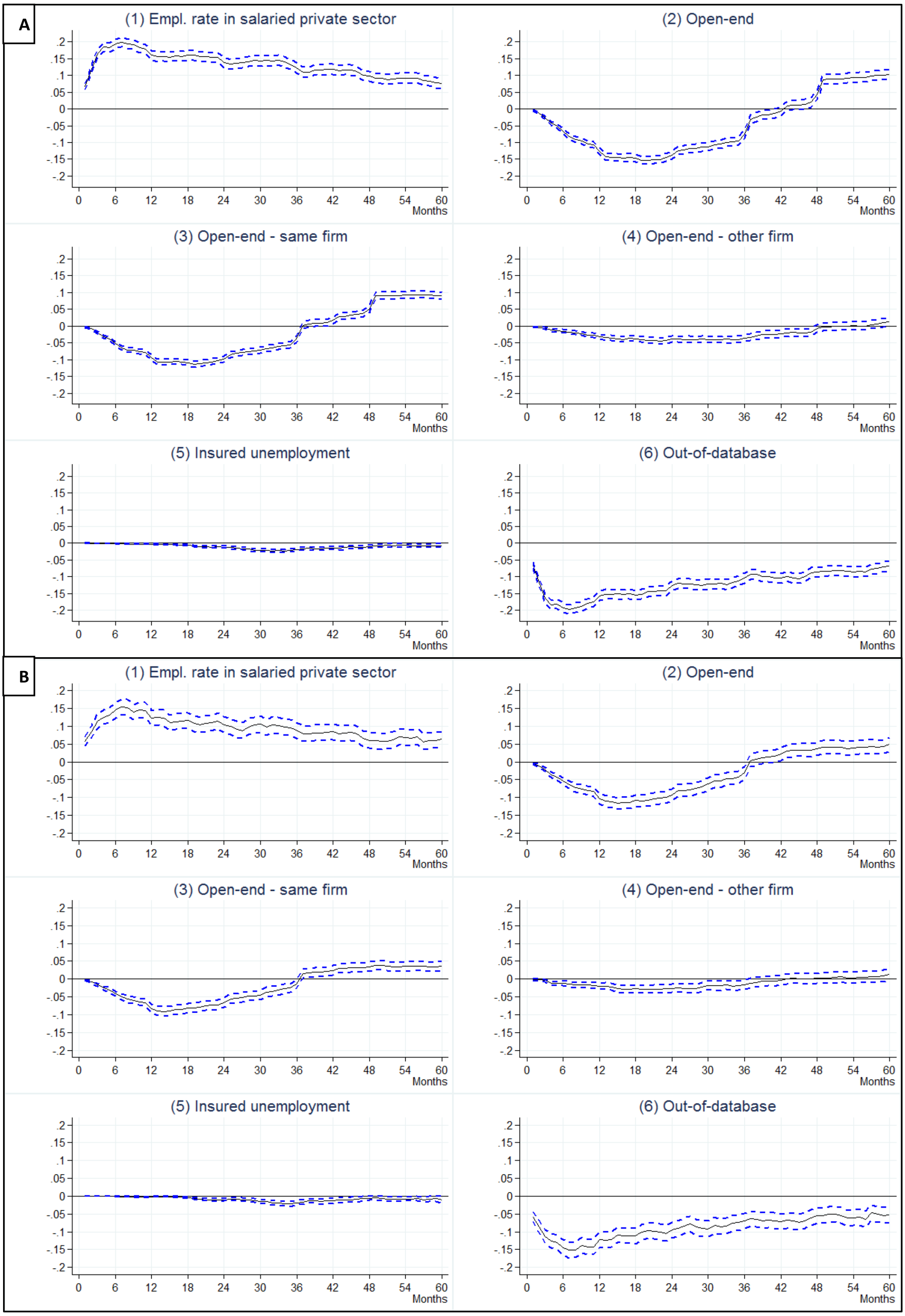
1.6.2. Apprenticeship versus other temporary contracts

In the section, we obtain some insights on the employment path of the apprentices compared to youth hired with other temporary contracts. We control for differences in 119 characteristics by CBPS (we add more detailed sector and firm size variables) and use a control group composed of 24,793 individuals hired with other temporary contracts in 2007. We exclude from the control group as we do not have information on the identifier of the final firm which implies a zero transformation rate. For similar reasons, we exclude also collaborators. As showed in Figure 1.8, apprentices have a much higher employment rate in temporary jobs, which includes the initial job. This is explained by the longer length of the initial contract for apprentices. Differences are even larger for the new regime apprentices, who also show slightly smaller chances to be in a temporary contract in the last months of the analysis.

Interestingly, apprentices have higher probabilities of moving to open-end jobs after three years, especially in the new regime (Figure 1.7). The higher transition to open-end contracts is due to conversions in the same firms and is observed after some initially negative effects in the first three years. The later estimates on the lock-in are in line with the findings of Berton et al. (2011), who estimated that the other temporary jobs grant a higher port-of-entry effect in the first two years. Our estimates show that the lock-in is even stronger for the new regime apprenticeship.

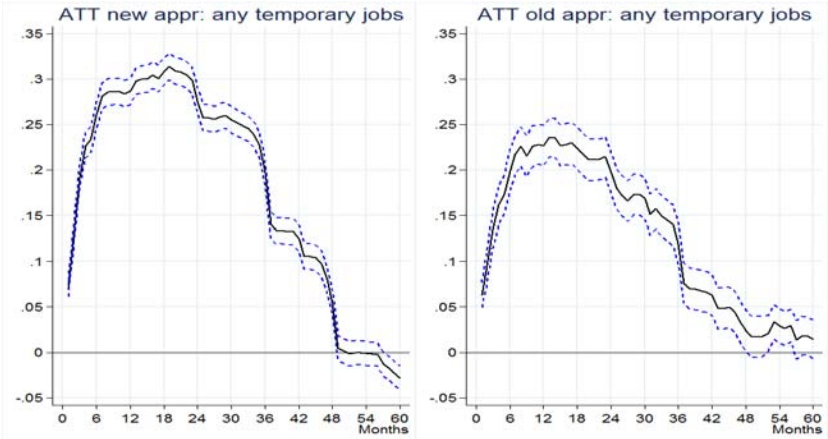
As explained in Section 1.3.2, we have to be cautious about giving a causal interpretation to these estimates as the Conditional Independence Assumption may fail. As individuals and firms can self-select in the type of contracts, it is likely that jobs with specific contracts are intrinsically different from the others. Furthermore, it is difficult to control for self-selection within the same firms. For example, firms can decide about the contract to use when hiring a youth, and they might fill vacancies for longer-term projects with an apprentice rather than with another temporary contract. Thus, rather than a causal impact, our ATTs can be interpreted as more robust descriptive evidence on the employment path of the apprentices compared to the youth with similar employment history and employer characteristics but hired with other temporary contracts.

Figure 1.7: ATT by CBPS on the new (A) and old (B) apprenticeship vs. other temporary contracts



ATT by CBPS of the reformed apprenticeship (A) and the old apprenticeship (B) versus other temporary jobs. Inflow sample of 9,944 treated units (A) or 6,808 treated units (B) and 24,793 control units. Treated group: apprentices hired with the new (A) or the old (B) apprenticeship regime in 2007; Control group: youth with a temporary contract in 2007; Status at the end of the month (from left to right and top to bottom): (1) in salaried private sector, (2) Open-end contract, (3) open-end contract in the same firm; (4) open-end contract in another firm, (5) insured unemployed, (6) neither in salaried private sector employment nor in insured unemployment. Cluster Robust Standard Errors by individual.

Figure 1.8: ATT by CBPS on the new and old apprenticeship versus other temporary contracts



ATT by CBPS of the reformed apprenticeship (A) and the old apprenticeship (B) versus other temporary jobs. Inflow sample of 9,944 treated units (A) or 6,808 treated units (B) and 24,793 control units. Treated group: apprentices hired with the new (A) or the old (B) apprenticeship regime in 2007; Control group: youth hired with a temporary contract in 2007; Cluster Robust Standard Errors by individual.

1.6.3. *Wage effect*

After looking at the employment rate, we focus our attention on the apprentice wages. We estimate the effect on the *full-time daily wage* (at 2007 prices) for the *initial job* (hiring) and the *last month of the analysis* (60 months after hiring).[32] To estimate wage effects, we re-estimate the PS for those people that are employed in a given month (everybody for the estimates on the initial wage). The estimates on the daily wage for any months after hiring are possibly biased as the employment effects might change the composition of the treated group, which is difficulty controlled by our covariates.

Thus, we also estimate the ATT on the *total cumulated remuneration* collected by the apprentices over the five years after hiring, which is unbiased but less informative since we cannot distinguish between wage and employment effect. To get more insights on its interpretation, we look at the ATT on the *total cumulated number of full-time working day*s over the five years after hiring.[33] Depending on which effect is bigger, it is possible to understand whether the full-time wage increased. As the outcomes is in logs, for this latter analysis we remove the individuals with zero value for any of the outcome (the *total cumulated number of full-time working day*s, *total cumulated remuneration*, *entry salary*), which regards only 0.3% of the treated, likely caused by measurement errors.

As shown in Table 1.4, we estimate a positive effect of the *apprenticeship reform* on the entry wage, remunerations and full-time daily wages. First, the reform on the minimum wage for the apprentices seems binding as the daily entry remuneration increased by 18%, a bit smaller than the raw differences on the unweighted and the untrimmed sample (+25%). The estimated effect could, however,

[32] As we do not have monthly remuneration, the outcome is approximated by summing the yearly remuneration for all the active jobs in a given month and dividing it by the total number of days worked (in the year) in those jobs.

[33] As we right censored the units five years after the hiring, we subtract from the total yearly remuneration/working days of the still active jobs at the moment of censoring, an amount proportional to the job-spell not yet finalized in that calendar year. As we control for the quarter of hiring, ignoring such adjustment does not significantly change the results.

incorporate anticipatory effects of the reform as thanks to the revised training firms might anticipate that the apprentices will become more productive. Second, five years after hiring we observe that among the employed, youth previously hired with the new apprenticeship regime earn on average 4.6% more. Over the five years the treated apprentices earned 16.4% more (cumulated remuneration). The effect is +8.0% for the ones hired in small firms and +18.1% in large firms. The estimates on the total full-time working days is +7.4% (+10.4% in large firms and zero in small firms). Thus, in five years the wage increased on average by 9% (no significant differences are observed by firm size).

Table 1.4: Impact on the log of wage, cumulated remuneration and full-time working days.

			Daily wage if employed (1)		Total remuneration(2)	Total FT working days (3)
			At hiring	**5 years later**	**After the hiring**	**After the hiring**
New versus old apprenticeship (A)	**Overall**	**ATT**	0.1855***	0.0459***	0.1642***	0.0737***
		CI	[0.171; 0.200]	[0.033; 0.058]	[0.114; 0.214]	[0.027; 0.121]
		p-value: ATT=0	0.000	0.000	0.000	0.002
		N	17,196	10,129	17,196	17,196
	Small	**ATT**	0.1731***	0.0310***	0.0800***	0.0009
		CI	[0.159; 0.187]	[0.015; 0.046]	[0.027; 0.131]	[-0.047; 0.049]
		p-value: ATT=0	0.000	0.000	0.003	0.971
		N	11,601	6,529	11,601	11,601
	Large	**ATT**	0.1760***	0.0230**	0.1811***	0.1038***
		CI	[0.150; 0.201]	[0.001; 0.045]	[0.102; 0.260]	[0.032; 0.175]
		p-value: ATT=0	0.000	0.038	0.000	0.005
		N	5,100	3,186	5,100	5,100
New apprenticeship versus other temporary (B)		**ATT**	-0.1496***	0.0119**	0.4180***	0.4834***
		CI	[-0.160; -0.139]	[0.002; 0.022]	[0.381; 0.454]	[0.448; 0.519]
		p-value: ATT=0	0.000	0.017	0.000	0.000
		N	34,207	18,955	34,207	34,207
Old apprenticeship versus other temporary (B)		**ATT**	-0.3172***	-0.0151**	0.2858***	0.4255***
		CI	[-0.333; -0.302]	[-0.027; -0.002]	[0.232; 0.339]	[0.373; 0.478]
		p-value: ATT=0	0.000	0.018	0.000	0.000
		N	30,767	16,541	30,767	30,767

ATTs by CBPS of the reformed apprenticeship versus the old apprenticeship (A) and apprenticeship versus other temporary contracts (B). Inflow sample of 17,822 apprentices (before trimming with 99.9 percentile rule) and 24,504 youth hired in other temporary jobs. Cluster Robust Standard Errors by individual to take into account serial correlation. Dependent variables (log transformed): Column 1 -the daily wage conditional on employment at hiring (everybody) or 60 months after the hiring (CBPS only on employed); Column 2 – cumulated remuneration in the five years after the hiring. Column 3 – cumulated full-time working days in the five years after hiring. Prices in 2007 €.

Afterward, we look at the difference in the daily wage, the total remuneration and full-time working days between *apprentices* and youth hired with *other temporary contracts* in 2007. We estimate that compared to other temporary staff, apprentices are paid less at hiring: -15.0% and -31.7% for the new and old regime, respectively. After five years, apprentices in the new regime earn 1.2% more than other temporary staff, while the negative gap shrinks to -1.5% for the old regime. Apprentices also accumulate a higher total remuneration over the five years: +41.8% and +28.6% for the new and the old regime, respectively. Compared to other temporary contracts apprentices show an even higher number of full-time working days (+48.3% and +42.5% for the new and old regime). Thus, the daily wage over the five-year period was on average 6.5% and 14.0% lower for the apprentices in the two regimes compared to staff in other temporary contracts. These figures are within the entry and the final daily wage we have previously estimated. Again, due to high self-selection in the contracts, we should be cautious in arguing that these estimates represent an unbiased causal impact.

1.6.4. Robustness checks and summary of results

We perform some robustness tests on the estimates on the whole population. *First*, we estimate the ATT by the standard Inverse Probability Weighting. *Second*, we add as an additional covariate the regional unemployment rate in 2004. As this variable has a high measurement error, we do this analysis only for robustness.[34] *Third*, we estimate the ATT with the max trimming rule and on the untrimmed sample. *Fourth*, we use the shrinkage method of Pohlmeier et al. (2014) on the CBPS and the IPW by cross-validation method. This method shrinks the estimated Propensity Score towards the estimated unconditional mean (i.e. the share of treated) to avoid giving to some units excessive weights. The authors show in their simulations that this decreases the mean squared errors of the ATT. As suggested by the authors, we rely on the 0.9 trimming rule of Crump et al. (2009). We apply this method to both the CBPS and the IPW estimator. *Fifth*, we only select the sub-sample of the variables most statistically significant in affecting the outcome of the control group.[35] To choose the variables, we regress the outcome "transiting to open-end contracts in the same-firm" on the pre-selected covariates and we test one-by-one all the other remaining variables. The pre-selected covariates are just time dummies in the first repetition (the analysis is on 60 months). The variable with the lowest p-value is then added to the set of pre-selected covariates. The loop is repeated until no variable is left in the set of "testable" variables with a significance correlation of at least 10%. As reported in Table 1.5 and Figures A.3-A.7 in the Supplementary Appendix A, results are not significantly different from the benchmark estimates.[36]

To sum up, our estimates indicate a positive effect of the reform on the employment and remuneration of the apprentices, but mostly in firms with more than ten employees. As the effects occur already from the first months, we argue that these are more likely driven from the revision of the training and the minimum wage rather than from the longer contract length. Possible explanations for the lower effect in small firms are the lack of firms' capabilities for internal training, which prevented them from enjoying the liberalization of the training, and their higher incentive to churn due to very high tax rebates (e.g. their employers' SSC for apprentices are almost zero – which is still applicable today).

The apprenticeship contract is a very expensive policy for the public budget and in 2013 it absorbed 32.4% of the budget for active labour market policies. No effect on the apprentices hired in small firms is worrisome as the transformation rate to open-end contracts in these firms was lower also in the absence of the policy change. Our estimates show that the counterfactual in the absence of treatment

[34] Information on the region where the individual lives is available only for 2012. As we do not find significant effects on individual migration (Appendix 1.8.1), incorrect information should mostly affect the measurement error.
[35] We exclude treated units as some variables might be significant just because they are correlated with the treatment (which may affect the outcome). The inclusion of variables similar to instruments can make thinner the support, without bringing new information. This increases the variance of the ATT without decreasing the bias (Brookhart et al., 2006).
[36] Not trimming produces larger estimates as the CBPS fails in balancing a firm size dummy (>500 employees).

is 12.8% for apprentices hired in smaller firms and 16.6% in larger firms. Due to the high tax rebate small firms have a strong preference for the apprenticeship and in 2007 they absorbed about two-thirds of the hirings. In the same year, only 34% of the other temporary jobs were created in these firms.[37] If the goal of the apprenticeship is to improve the employability of the youth, rather than just providing cheap labour to small firms, we believe there is an inefficient use of public resources. The policymaker either needs to find other solutions to encourage transformations in these firms or to reconsider the higher subsidy that they are granted. Realigning the tax rebate of the smaller firms to the ones of the larger firms might reduce the churning behaviour of the first.

Table 1.5: Robustness - ATT on the cumulated remuneration & full-time working days (logs)

		Entry daily wage (1)	Total remuneration (2)	Total Full-Time working days (3)
Benchmark: CBPS - 99.9 trimming (A)	ATT	0.1855***	0.1642***	0.0737***
	CI	[0.171; 0.200]	[0.114; 0.214]	[0.027; 0.121]
	p-value: ATT=0	0.000	0.000	0.002
	N	17,196	17,196	17,196
CBPS with unempl. rate (B)	ATT	0.1859***	0.1648***	0.0749***
	CI	[0.171; 0.200]	[0.111; 0.215]	[0.027; 0.122]
	p-value: ATT=0	0.000	0.000	0.002
	N	17,100	17,100	17,100
CBPS – max trimming (C)	ATT	0.1922***	0.1803***	0.0850***
	CI	[0.178; 0.206]	[0.131; 0.230]	[0.038; 0.132]
	p-value: ATT=0	0.000	0.000	0.000
	N	17,572	17,572	17,572
CBPS – no trimming (D)	ATT	0.1963***	0.1885***	0.0903***
	CI	[0.182; 0.210]	[0.139; 0.238]	[0.043; 0.137]
	p-value: ATT=0	0.000	0.000	0.000
	N	17,821	17,821	17,821
IPW – 99.9 trimming (E)	ATT	0.1694***	0.1514***	0.0721**
	CI	[0.151; 0.187]	[0.090; 0.213]	[0.013; 0.131]
	p-value: ATT=0	0.000	0.000	0.017
	N	17,158	17,158	17,158
IPW - shrinkage & Crump trimming (F)	ATT	0.1915***	0.1614***	0.0694***
	CI	[0.179; 0.203]	[0.118; 0.205]	[0.029; 0.109]
	p-value: ATT=0	0.000	0.000	0.001
	N	16,220	16,220	16,220
CBPS - shrinkage & Crump trimming (G)	ATT	0.1993***	0.1652***	0.0681***
	CI	[0.187; 0.211]	[0.123; 0.206]	[0.029; 0.107]
	p-value: ATT=0	0.000	0.000	0.001
	N	16,544	16,544	16,544

Robustness tests on the treatment effect on the logarithm transformation of: Entry daily wage (1), Cumulated remuneration in the five years after hiring (2), Cumulated full-time working days in the five years after hiring (3). Inflow sample of 17,822 apprentices (before trimming). Different specifications by row: (A) Benchmark (CBPS with 99.9 trimming rule), (B) CBPS with 99.9 trimming rule and regional unemployment rate, (C) CBPS with max-trimming rule, (D) CBPS on the untrimmed sample, (E) IPW with 99.9 trimming rule, (F) IPW and (G) CBPS with shrinkage and Crump trimming rule. Cluster Robust Standard Errors by individual to take into account serial correlation. Prices in 2007 €.

1.6.5. *ITT - employment rate, transition to employment & contract diffusion*

After estimating the ATT on the apprentices' transition, we assess the impact of the reform on the labour market of the eligible population (Intention-To-Treat). The identification strategy is described in Section 1.5.2, which is based on a DiD estimator on the youth becoming eligible to the reform. The treatment starts when the region, where the youth lives, enacts the Regional Law (for the analyses on the employment rate and employment transition) or also when the sector, where the youth works,

[37] Inflow sample of youth aged 19-24 hired with in temporary job (excluding temporary agency workers and collaborators).

passes a CBA (for the analysis on the contract diffusion). To increase the precision on the contract diffusion, we limit the sample to the employees in salaried private sector. The sample is enlarged to all workers (not in the public administration) if we look at the substitution effects on self-employment.

As described in Section 1.4.2, we use LFS data since our administrative data do not contain information on non-employed individuals not in insured unemployment schemes and on the region where the individual lives (apart from the year 2012). To have an estimate on a specific business cycle, we also end the analysis on the 3rd quarter of 2008 and estimate the impact in the shorter-run.

We divide the sample by age class to capture the different effects of the reform.[38] In particular, for the age class 20-24 we estimate the effects of the reformed regime. The age class 25-29 mostly covers youth becoming eligible for the apprenticeship (the reform raised the age-eligibility to 30 - see Section 1.2.2). The age class 30-34 includes the people that might undergo to spill-over effects due to the high substitutability with the youth aged 25-29. As we do the analysis on stock data, it is not possible to disentangle fully the eligibility and the spillover effects since the age eligibility is measured at hiring.

In the first analysis, we look at the impact on the youth *employment rate* in the salaried private sector after the implementation of the Regional Laws. The treatment dummies are set to one after this implementation with two alternative specifications: a unique treatment dummy or a treatment dummy for each year after the reform. As shown in Table 1.6, we do not find a significant impact on the youth employment rate. Our placebo tests do not show evidence against the assumption of parallel path, which is in line with the period 2000-2003 (see Appendix 1.8.1) and the results remain insignificant when we control for the regional trends (Table A.2 in the Supplementary Appendix A).

Afterwards, we look at the impact on the *transition from non-employment*, which is the employment rate of individuals not employed (and not students) one year before. We find evidence of a positive effect on the population becoming eligible (age 25-29) after the third year with a magnitude that moderately changes according to the specification (Table 1.7). The specification until 2011 with yearly treatment dummies shows an effect of +3.2 pp / +14.2% after the third year.[39] This effect is in line with the findings of D'Agostino et al. (2015) and suggests that granting eligibility to the apprenticeship helped the unemployed to find a job. This is an expected effect as these youths become also eligible to the large hiring subsidy linked to the apprenticeship. Finally, the estimates on the employment rate and transition remain stable when we perform the analyses in continuous treatment (Table 1.23 in Appendix 1.8.2) or by nonlinear DiD (see Tables A.4-A.5 in the Supplementary Appendix A).

[38] We do not estimate effects by gender since splitting the sample reduce precision and several placebo tests fail.

[39] If we widen the definition to self-employment we do not find a significant impact (Table 1.21 in Appendix 1.8.2). This is likely due to a decrease in precision as the effects on transition to self-employment are insignificant (Table 1.22).

Table 1.6: ITT on employment rate in salaried private sector - parallel path (OLS)

	2004q1- 2011q1 (1, A)			2004q1-2008q3 (2, A)			2004q1- 2011q1 (1, B)			2004q1-2008q3 (2, B)		
Age class	**20-24**	**25-29**	**30-34**	**20-24**	**25-29**	**30-34**	**20-24**	**25-29**	**30-34**	**20-24**	**25-29**	**30-34**
ITT pp – year 1	-1.2	-0.1	0.2	-0.5	-0.6	-0.1	-0.4	-0.7	-0.4	-0.4	-0.6	-0.5
p-value	0.462	0.913	0.704	0.733	0.566	0.891	0.751	0.380	0.556	0.745	0.570	0.477
ITT pp – year 2	-	-	-	-	-	-	-1.9	-0.2	0.0	-1.1	-0.8	0.4
p-value	-	-	-	-	-	-	0.320	0.704	1.000	0.497	0.438	0.863
ITT pp – year 3	-	-	-	-	-	-	-1.3	1.1	1.1	0.4	0.3	0.9
p-value	-	-	-	-	-	-	0.679	0.267	0.114	0.818	0.924	0.491
ITT pp – year 4	-	-	-	-	-	-	-2.6	1.0	1.7*	1.6	-1.0	2.8
p-value	-	-	-	-	-	-	0.321	0.207	0.091	0.473	0.454	0.118
ITT pp – year 5	-	-	-	-	-	-	-1.6	-0.6	-0.3	-	-	-
p-value	-	-	-	-	-	-	0.584	0.381	0.851	-	-	-
ITT pp – year 6+	-	-	-	-	-	-	-3.2	1.4	0.1	-	-	-
p-value	-	-	-	-	-	-	0.350	0.405	0.887	-	-	-
PLACEBO in *pp*	-	-	-	-	-	-	0.5	0.3	-0.1	0.4	0.6	0.4
p-value	-	-	-	-	-	-	0.654	0.765	0.918	0.713	0.494	0.559
N	128,821	193,073	258,357	89,375	134,849	178,113	128,821	193,073	258,357	89,375	134,849	178,113

*ITT on the employment rate in the salaried private sector in pp by age class. DiD by OLS regression controlling for individual characteristics, time dummies, regional fixed-effect and anticipation dummies. Specification either with one treatment dummy for all the quarters after the treatment (A) or divided per each year after the Regional Law implementation (B). Sample: all youth excluding students. P-value obtained by wild cluster bootstrap-t (2000 repetitions) with 17 regions as clusters. ***: significant at 1%, **: significant at 5%, *: significant at 10%.*

Table 1.7: ITT on transition from non-employment - parallel path (OLS)

	2004q1- 2011q1 (1, A)			2004q1-2008q3 (2, A)			2004q1- 2011q1 (1, B)			2004q1-2008q3 (2, B)		
Age class	**20-24**	**25-29**	**30-34**	**20-24**	**25-29**	**30-34**	**20-24**	**25-29**	**30-34**	**20-24**	**25-29**	**30-34**
ITT pp – year 1	0.2	1.7*	0.5	1.0	3.0**	0.7	1.1	1.1	0.4	1.5	3.1	0.4
p-value	0.915	0.053	0.295	0.625	0.039	0.471	0.352	0.615	0.587	0.340	0.132	0.678
ITT pp – year 2	-	-	-	-	-	-	-1.3	1.0	0.7	-0.4	1.9	1.3
p-value	-	-	-	-	-	-	0.617	0.487	0.308	0.880	0.445	0.265
ITT pp – year 3	-	-	-	-	-	-	-0.0	3.6**	0.8	0.5	4.8**	0.9
p-value	-	-	-	-	-	-	0.955	0.018	0.396	0.808	0.044	0.581
ITT pp – year 4	-	-	-	-	-	-	-0.6	3.0**	0.8	4.4	4.3	-0.3
p-value	-	-	-	-	-	-	0.837	0.031	0.403	0.323	0.406	0.909
ITT pp – year 5	-	-	-	-	-	-	1.2	0.4	-1.1	-	-	-
p-value	-	-	-	-	-	-	0.720	0.793	0.257	-	-	-
ITT pp – year 6+	-	-	-	-	-	-	-2.1	5.7**	1.1	-	-	-
p-value	-	-	-	-	-	-	0.476	0.032	0.315	-	-	-
PLACEBO in *pp*	-	-	-	-	-	-	1.6	0.2	0.1	1.5	2.2	0.6
p-value	-	-	-	-	-	-	0.352	0.838	0.880	0.651	0.270	0.550
N	52,202	60,841	71,520	35,518	41,667	48,485	52,202	60,841	71,520	35,518	41,667	48,485

*ITT on the transition to the salaried private sector from non-employment in pp by age class. DiD on parallel path by OLS regression controlling for individual characteristics (education and gender), time dummies, regional fixed-effect and anticipation dummies (for the two quarters before the treatment). Specification either with one treatment dummy for all the quarters after the treatment (A) or divided per each year after the Regional Law implementation (B). As placebo tests, we rerun the analysis adding a placebo dummy for the 4th and the 3rd quarter before the treatment. Sample: population non-employed one year before t (excluding students in t-1 year and t). P-value by wild cluster bootstrap-t (2000 repetitions) with 17 regions as clusters. ***: significant at 1%, **: significant at 5%, *: significant at 10%.*

In addition, we look at the effects of the reform on the *contract diffusion*. In this analysis, we include sector dummies and the treatment dummies take the value of one also when a CBA (or a regional-sector pilot test) implements the reform place. From the third year we estimate an effect on the share of apprentices aged between 25-29, which is the same moment when we estimate an increase in the transition from non-employment for the same age group. This may indicate that even if the Regional Law should have implemented the policy reform, it took longer time for its actual full implementation.

In particular, we estimate an ITT of +0.7 pp (third year; +19%), +1.3 pp (fourth year; +34%), +1.5 pp (fifth year; +42%), and +3.2 pp (after six years; +95% – Table 1.8). The effects are larger if we limit the analysis on the positive Business Cycle (ending in the 3rd quarter 2008). Youth aged 20-24 do not have a statistically significant increase. As explained in Section 1.3.1, the changes introduced by the reform go in different directions regarding the diffusion of the apprenticeship contract. For example, the introduction of the minimum wage should discourage the usage of the apprenticeship contract. At the same time, on-the-job training might increase the hiring of apprentices but also decrease the churning. The lack of significant effect confirms the unclear incentives introduced by the reform.

Finally, despite the increase in the number of apprentices aged between 25 and 29, we do not find an impact on their employment rate. Our estimates indicate that firms have substituted temporary jobs with apprentices. The share of other temporary contracts decreased by about 2.1 pp (fourth year; -19%), 2.5 pp (fifth year; -13%) and 2.9 pp (after six years; -14%). These results are, however, not very robust since if we limit the horizon to the 3rd quarter 2008 (before the crisis) then they lose significance at 5%. Moreover, as fewer individuals are employed at 20-24 years old, estimates for these age-class are much more volatile. Placebo tests are not rejected at 5% level, which makes us more trustful about the parallel path assumption. For robustness we also rerun the analysis excluding the sector dimension. As expected, ignoring this treatment decreases the impact on the diffusion of the apprenticeship contract (Table A.3 in the Supplementary Appendix A). Overall, these results are partly in line with Cappellari et al. (2012) on firm-level data on the same reform, underlying the high level of substitutability among temporary contracts.

To conclude, the ATT estimated on administrative data is not reflected in the ITT on the eligible youth in the LFS, which might be explained by several factors. First, estimators on the ITT might lack of statistical power. In other words, the positive effect on the apprentices might not be large enough to be significantly observed on the full youth population. Second, the ATT on the apprentices ignores the other effects introduced by the reform on the rest of the economy. For example, the positive effects on the apprentices might come to the detriment of other youth via substitution effects.

Lastly, other explanations regard the limits of the LFS and possibly biased estimates by using the DiD estimator. First, on the employment rate and transition from non-employment we cannot obtain clean estimates as control regions are also affected. Indeed, control regions are partially treated by sector CBAs, which might underestimate the ITT. The DiD with treatment intensity tries to correct for this but assume instantaneous effects of the “dosage”, which is unlikely to hold as the effects we observed on the other specification occurred a few years later. Second, the estimates on contract diffusion suffer from high measurement error due to the highly specific CBAs and the difficult matching with the NACE 2 digit dummies. Second, survey data might be less precise than administrative data. This, together with the impossibility to separate clearly the private from the public sector and the lower number of apprentices with respect to statistics from INPS (e.g. 9.2% versus 19.5% in 2010), may downwardly bias the ITT. Thus, no clear-cut conclusion on the effects on the youth labour market can be drawn.

Table 1.8: ITT on the contract diffusion (in pp - OLS)

Specification (A)		Age class: 20-24					Age class: 25-29					Age class: 30-34				
2004q1-2011q1		Appr. (1)	Open-end(2)	Collab.(3)	Temp.(4)	Self-empl.(5)	Appr.(1)	Open-end(2)	Collab.(3)	Temp.(4)	Self-empl.(5)	Appr.(1)	Open-end(2)	Collab.(3)	Temp.(4)	Self-empl.(5)
α	ITT – 1 dummy	-1.0	2.0**	0.2	-1.3	0.0	0.3	0.9	-0.5	-0.6	-0.2	0.0	0.4	-0.1	-0.4	-0.3
α	p-value	0.206	0.026	0.425	0.117	0.978	0.280	0.228	0.283	0.410	0.728	0.936	0.588	0.837	0.447	0.572
β	ITT – year 1	-1.2	1.3	0.4	-0.6	0.4	0.1	0.9	-0.6**	-0.3	0.2	-0.0	0.2	-0.2	-0.0	0.1
β	p-value	0.118	0.253	0.222	0.477	0.483	0.754	0.233	0.042	0.666	0.735	0.615	0.732	0.670	0.996	0.741
β	ITT– year 2	-1.2*	3.8***	-0.1	-2.5**	-0.1	0.1	1.1	-0.5	-0.7	0.0	-0.0	0.9	-0.1	-0.8	-0.4
β	p-value	0.095	0.003	0.691	0.015	0.874	0.819	0.221	0.385	0.391	0.972	0.878	0.252	0.760	0.108	0.568
β	ITT– year 3	-0.1	1.3	0.0	-1.3	-0.7	0.7**	0.3	-0.3	-0.6	-1.2	0.1	-0.0	0.1	-0.2	-1.2
β	p-value	0.949	0.324	0.929	0.290	0.262	0.019	0.682	0.629	0.196	0.331	0.349	0.977	0.865	0.721	0.268
β	ITT– year 4	0.2	0.2	0.3	-0.8	-0.9	1.3***	0.8	-0.0	-2.1**	-1.7	0.1	1.0	0.1	-1.2	-1.1
β	p-value	0.844	0.869	0.412	0.405	0.156	0.008	0.376	0.961	0.015	0.170	0.432	0.439	0.920	0.209	0.230
β	ITT– year 5	0.3	-2.2	0.3	1.6*	0.6	1.5**	0.9	0.1	-2.5**	-2.0	0.3*	-0.7	0.1	0.3	-1.4
β	p-value	0.811	0.214	0.677	0.067	0.345	0.038	0.610	0.942	0.042	0.147	0.093	0.526	0.926	0.679	0.304
β	ITT– year 6+	1.5	-2.6	0.1	1.0	-0.2	3.2**	-0.3	0.0	-2.9**	-1.6	0.3*	-0.3	-0.1	0.1	-1.8
β	p-value	0.475	0.200	0.922	0.445	0.809	0.013	0.863	1.000	0.032	0.350	0.064	0.869	0.890	0.957	0.281
π	PLACEBO	-0.4	0.8	0.7*	-1.1	-0.2	-0.5*	0.6	-0.3	0.2	0.6	-0.0	0.1	0.2	-0.2	0.2
π	p-value	0.610	0.449	0.092	0.203	0.628	0.065	0.514	0.505	0.774	0.339	0.517	0.862	0.612	0.608	0.802
	N	68,153	68,153	68,153	68,153	76,831	107,792	107,792	107,792	107,792	130,934	140,716	140,716	140,716	140,716	182,231

Specification (B)		Age class: 20-24					Age class: 25-29					Age class: 30-34				
2004q1-2008q3		Appr. (1)	Open-end(2)	Collab.(3)	Temp.(4)	Self-empl.(5)	Appr.(1)	Open-end(2)	Collab.(3)	Temp.(4)	Self-empl.(5)	Appr.(1)	Open-end(2)	Collab.(3)	Temp.(4)	Self-empl.(5)
α	ITT – 1 dummy	-0.6	2.4**	-0.2	-1.6	0.5	0.7	1.5	-0.8	-1.4	0.3	-0.0	0.9	-0.4	-0.5	0.2
α	p-value	0.159	0.023	0.269	0.131	0.862	0.462	0.200	0.260	0.442	0.591	0.942	0.712	0.930	0.577	0.486
β	ITT– year 1	-0.7*	1.7	0.0	-1.0	0.6	0.6	1.3	-0.8*	-1.1	0.3	-0.1	0.7	-0.3	-0.3	0.4
β	p-value	0.076	0.172	0.158	0.439	0.595	0.814	0.136	0.074	0.691	0.857	0.492	0.757	0.762	0.892	0.776
β	ITT– year 2	-0.1*	4.0***	-0.9	-3.1**	0.3	0.7	2.1	-0.8	-2.0	0.4	-0.0	1.6	-0.5	-1.1	-0.2
β	p-value	0.066	0.004	0.987	0.019	0.856	0.746	0.193	0.319	0.473	0.966	0.567	0.295	0.780	0.158	0.609
β	ITT– year 3	0.8	2.6	-1.3	-2.0	-0.2	1.9**	1.2	-1.1	-2.1	-0.8	0.1	1.1	-0.7	-0.5	0.0
β	p-value	0.731	0.199	0.664	0.313	0.176	0.040	0.503	0.491	0.337	0.306	0.557	0.875	0.831	0.803	0.261
β	ITT– year 4+	0.8	2.3	-1.2	-1.9	0.2	2.9**	1.6	-0.8	-3.7*	-2.0	-0.0	3.6	-1.2	-2.4	0.8
β	p-value	0.904	0.615	0.385	0.855	0.254	0.027	0.661	0.805	0.065	0.188	0.387	0.984	0.874	0.797	0.138
π	PLACEBO	0.1	0.8	0.5	-1.4	-0.4	-0.1	0.6	-0.4	-0.1	0.6	-0.0	0.4	-0.0	-0.4	0.2
π	p-value	0.904	0.450	0.220	0.122	0.549	0.658	0.597	0.487	0.894	0.362	0.464	0.482	1.000	0.402	0.795
	N	48,558	48,558	48,558	48,558	54,837	76,017	76,017	76,017	76,017	92,889	96,799	96,799	96,799	96,799	126,559

*ITT on the contract diffusion in pp by age class. DiD on parallel path by OLS regression controlling for individual characteristics (education and gender), time dummies, regional and sector fixed-effect and anticipation dummies (for the two quarters before the treatment). Dependent variable by column: 1) Apprenticeship, 2) Open-End, 3) Collaborators, 4) Temporary contract, 5) Self-employed. Sample: youth employed excluding employees in the public administration (column 5) and self-employed (columns 1-4). Specifications: α-rows) a unique treatment dummy for all the years after the implementation of the Regional Law/Sector Collective Bargaining Agreement; β-rows) treatment dummies per each year after the treatment; π-rows) Placebo tests adding to specification β a treatment dummy for the 4th and the 3rd quarter before the treatment. Panel A) period 2004q1-2011q1, Panel B) Period 2004q1-2008q3. P-value obtained by wild cluster bootstrap-t (2000 repetitions) with 17 regions as clusters. ***: significant at 1%, **: significant at 5%, *: significant at 10%.*

1.7. Conclusion

In countries with high Employment Protection Legislation, temporary contracts were considered a possible tool to provide flexibility to firms and help the integration of youth in the labour markets. Since 2000, Italy has been one of the European countries relying the most on temporary jobs, and the apprenticeship contract was thought as the main active labour market policy for youth. Apprentices receive vocational training to enhance their professional skills while employers are compensated by paying lower remuneration and payroll taxes. A huge amount of public resources is committed to this contract, counting one-third of the whole budget for active labour market policy in Italy despite its narrow coverage (3.8% of employees in the private sector in 2013). To encourage its diffusion, in 2003 the policymakers decided to raise the age eligibility from 24 to 29 years old and allowed firms to perform on-the-job training. Despite the policymaker's intentions, the few evaluations that exist on the Italian apprenticeship contract show different results on the apprentices' transition to open-end jobs (Berton et al., 2011; Picchio and Staffolani, 2013). This paper aims to understand whether the contrasting results of the literature are also driven by the different apprenticeship regimes analysed.

We estimate the effects of the 2003 reform by exploiting the dual apprenticeship system existing between 2005 and 2010 due to the heterogeneous time of the implementation between regions and sectors. Some regions implemented the reform earlier seemingly because of the higher importance of the apprenticeship contract in their economy. No other significant differences in labour market characteristics are found and the policy did not significantly affect migration or commuting flows.

Our estimates on the Italian Labour Force Survey indicate that granting the eligibility to the non-employed youth aged 25-29 boosted their employment transition by about 14%. At the same time, we observe a substitution effect from other temporary jobs to apprenticeship contracts for the newly eligible youth, which may be a reason for the lack of effect on the overall youth employment rate.

Second, we compare the relative performance of the two apprenticeship regimes. In particular, we estimate the Average Treatment Effect on the employment path of the Treated (ATT) apprentices five years after the hiring. For this analysis, we use administrative data of the Italian Social Security Institutions (INPS) with a flow sample of 17,950 apprentices and compare the apprentices in the new regime to the ones in the old regime. We implement the Covariate Balancing Propensity Score estimator (Imai and Ratkovic, 2014) to control for a large set of individual and firm characteristics.

Our estimates show the reform of the apprenticeship regime managed to decrease the early dropout of the apprentices. Results are robust to different specifications of the weighting estimator. Four years from hiring, the reform boosted the conversion to open-end jobs in the same firms (+4.1 pp, +28.6%).

These results suggest that the revised training has been successful in providing firm-specific human capital to the apprentices. At the same time, the minimum wage for the apprentices has discouraged the firms' churning behaviour. The reform also seems to have positively affected the youth wage.

Analyses on heterogeneous response indicate that the positive impact mostly occurred in firms with more than ten employees, while small firms showed insignificant effects on the transformation to open-end contracts. This can be explained by the higher incentive to churn due to the higher subsidy for small firms. Furthermore, the lack of internal training capabilities might have also prevented small firms from enjoying the new on-the-job training. As we estimate a larger ATT compared to the ATU, we argue that the regions and sectors early implementing the reform were the ones most enjoying from the changes. Since in 2007 small firms absorbed two-thirds of the hired apprentices (but only one-third of other temporary jobs) and the transformation rate to open-end jobs was already low, the policymaker needs to find other solutions to boost the effectiveness of the apprenticeship contract. There is a significant need to reassess the merit of the higher financial incentive to small firms given their low performance in enhancing the employment possibilities of the apprentices. Realigning the tax rebate of the smaller firms to the ones of the larger firms might reduce their churning behaviour. Other forms of financial incentives to sustain small firms in the Italian economy may instead be found.

Bibliography

Abadie, A., 2005. Semiparametric Difference-in-Differences Estimators. Review of Economic Studies 72(1), 1–19.

Acemoglu, D., Pischke, J.-S., 1998. Why Do Firms Train? Theory and Evidence. The Quarterly Journal of Economics 113(1), 78–118.

Acemoglu, D., Pischke, J.-S., 1999. Beyond Becker: Training in Imperfect Labour Markets. Economic Journal 109(453), F112–42.

Askilden, J.E., Nilsen, Ø., 2005. Apprentices and Young Workers: A Study of the Norwegian Youth Labour Market. Scottish Journal of Political Economy 52(1), 1–17.

Autor, D., 2001. Why Do Temporary Help Firms Provide Free General Skills Training? The Quarterly Journal of Economics 116(4), 1409–1448.

Becker, G., 1962. Investment in Human Capital: A Theoretical Analysis. Journal of Political Economy 70.

Berton, F., Devicienti, F., Pacelli, L., 2011. Are Temporary Jobs a Port of Entry into Permanent Employment?: Evidence from Matched Employer-Employee. International Journal of Manpower 32(8), 879–899.

Bertrand, M., Duflo, E., Mullainathan, S., 2004. How Much Should We Trust Differences-in-Differences Estimates? The Quarterly Journal of Economics 119(1), 249–275.

Bishop, J.H., 1996. What we know about employer-provided training: A review of literature. Cornell University, Center for Advanced Human Resources Studies, Working Paper 96–09.

Blundell, R., Costa Dias, M., 2009. Alternative Approaches to Evaluation in Empirical Microeconomics. Journal of Human Resources 44(3), 565–640.

Bonnal, L., Mendes, S., Sofer, C., 2002. School-to-work transition: apprenticeship versus vocational school in France. International Journal of Manpower 23(5), 426–442.

Booth, A., Satchell, S.E., 1994. Apprenticeships and Job Tenure. Oxford Economic Papers 46(4), 676–95.

Brookhart, M.A., Schneeweiss, S., Rothman, K.J., Glynn, R.J., Avorn, J., Stürmer, T., 2006. Variable selection for propensity score models. American Journal of Epidemiology 163(12), 1149–1156.

Busso, M., DiNardo, J., McCrary, J., 2014. New Evidence on the Finite Sample Properties of Propensity Score Reweighting and Matching Estimators. The Review of Economics and Statistics 96(5), 885–897.

Cameron, A.C., Trivedi, P.K., 2005. Microeconometrics: methods and applications. Cambridge University Press.

Cameron, A., Miller, D., 2015. A Practitioner's Guide to Cluster-Robust Inference. Journal of Human Resources 50(2), 317–372.

Cappellari, L., Dell'Aringa, C., Leonardi, M., 2012. Temporary Employment, Job Flows and Productivity: A Tale of Two Reforms. Economic Journal 122(562), F188–F215.

Cappelli, P., 2004. Why do employers pay for college? Journal of Econometrics 121(1-2), 213–241.

Chang, C., Wang, Y., 1996. Human Capital Investment under Asymmetric Information: The Pigovian Conjecture Revisited. Journal of Labor Economics 14(3), 505–19.

Clark, D., Fahr, R., 2002. The Promise of Workplace Training for Non-College Bound Youth: Theory and Evidence from German Apprenticeship . CEP Discussion Paper No. dp0518, LSE, London.

Crump, R., Hotz, V.J., Imbens, G., Mitnik, O., 2009. Dealing with Limited Overlap in Estimation of Average Treatment Effects. Biometrika 96(1), 187–199.

D'Addio, A., Rosholm, M., 2005. Exits from Temporary Jobs in Europe: A Competing Risks Analysis. Labour Economics 12(4), 449–468.

D'Agostino, G., Pieroni, L., Scarlato, M., 2015. Revisiting the Effects of Enhanced Flexibility on the Italian Labour Market . MPRA Paper.

Davidson, R., MacKinnon, J., 1999. The Size Distortion of Bootstrap Tests. Econometric Theory 15(03), 361–376.

European Commission, 2010. Labour force survey in the EU, candidate and EFTA countries - Main characteristics of the national surveys. Publications Office of the European Union, Luxembourg.

Eurostat, 2015a. Temporary employees as percentage of the total number of employees, by sex and age (%) - lfsa_etpga.

Eurostat, 2015b. Unemployment rate by sex and age groups - annual average (%) - une_rt_a.

Euwals, R., Winkelmann, R., 2004. Training intensity and first labor market outcomes of apprenticeship graduates. International Journal of Manpower 25(5), 447–462.

Faccini, R., 2014. Reassessing Labour Market Reforms: Temporary Contracts as a Screening Device. Economic Journal 124(575), 167–200.

Fersterer, J., Pischke, J.-S., Winter-Ebmer, R., 2008. Returns to Apprenticeship Training in Austria: Evidence from Failed Firms. Scandinavian Journal of Economics 110(4), 733–753.

Frölich, M., Huber, M., Wiesenfarth, M., 2015. The Finite Sample Performance of Semi- and Nonparametric Estimators for Treatment Effects and Policy Evaluation . IZA Discussion Papers No. 8756, IZA, Bonn.

Hainmueller, J., 2012. Entropy Balancing for Causal Effects: A Multivariate Reweighting Method to Produce Balanced Samples in Observational Studies. Political Analysis 20, 25–46.

Hansen, L., Heaton, J., Yaron, A., 1996. Finite-Sample Properties of Some Alternative GMM Estimators. Journal of Business & Economic Statistics 14(3), 262–80.

Hirano, K., Imbens, G.W., Ridder, G., 2003. Efficient Estimation of Average Treatment Effects Using the Estimated Propensity Score. Econometrica 71(4), 1161–1189.

Horvitz, D.G., Thompson, D.J., 1952. A Generalization of Sampling Without Replacement from a Finite Universe. Journal of the American Statistical Association 47(260), 663–685.

Huber, M., Lechner, M., Wunsch, C., 2013. The Performance of Estimators Based on the Propensity Score. Journal of Econometrics 175(1), 1–21.

Huber, P., 2014. Are Commuters in the EU Better Educated than Non-commuters but Worse than Migrants? Urban Studies 51(3), 509–525.

Imai, K., Ratkovic, M., 2014. Covariate Balancing Propensity Score. Journal of the Royal Statistical Society: Series B (Statistical Methodology) 76(1), 243–263.

ISFOL, 2006. La Transizione dall'Apprendistato agli Apprendistati: Monitoraggio 2004-2005 . I libri del Fondo sociale europeo No. 79 , 268, Rome.

ISFOL, 2007. L'apprendistato fra Regolamentazioni Regionali e Discipline Contrattuali: Monitoraggio sul 2005-2006 . I libri del Fondo sociale europeo No. 96 , 325, Rome.

ISFOL, 2010. Apprendistato: Un Sistema Plurale. X Rapporto Di Monitoraggio . I libri del Fondo sociale europeo No. 141 , 350, Rome.

ISFOL, 2012. Monitoraggio sull'apprendistato: XIII rapporto . Ministero del Lavoro e delle Politiche Sociali, ISFOL, INPS, Rome.

ISFOL, 2013. Monitoraggio sull'apprendistato: XIV rapporto . Ministero del Lavoro e delle Politiche Sociali, ISFOL, INPS, Rome.

ISTAT, 2010. Dossier L'economia sommersa: stime nazionali e regionali . National Institute for Statistics (ISTAT), Rome.

Kang, J.D.Y., Schafer, J.L., 2007. Demystifying Double Robustness: A Comparison of Alternative Strategies for Estimating a Population Mean from Incomplete Data. Statistical Science 22(4), 523–539.

Katz, E., Ziderman, A., 1990. Investment in General Training: The Role of Information and Labour Mobility. Economic Journal 100(403), 1147–58.

Kline, P., Andres, S., 2012. A Score Based Approach to Wild Bootstrap Inference. Journal of Econometric Methods 1(1), 23–41.

Lechner, M., 2011. The Estimation of Causal Effects by Difference-in-Difference Methods. Foundations and Trends(R) in Econometrics 4(3), 165–224.

Lechner, M., Miquel, R., Wunsch, C., 2011. Long-Run Effects of Public Sector Sponsored Training in West Germany. Journal of the European Economic Association 9(4), 742–784.

Lechner, M., Strittmatter, A., 2014. Practical Procedures to Deal with Common Support Problems in Matching Estimation . Economics Working Paper Series No. 1410, University of St. Gallen, St. Gallen.

MacKinnon, J., 2014. Wild Cluster Bootstrap Confidence Intervals . Working Paper No. 1329.

Magnac, T., 2000. Subsidised Training and Youth Employment: Distinguishing Unobserved Heterogeneity from State Dependence in Labour Market Histories. Economic Journal 110(466), 805–37.

Malcomson, J., Maw, J.W., McCormick, B., 2003. General training by firms, apprentice contracts, and public policy. European Economic Review 47(2), 197–227.

Martí, M., Ródenas, C., 2007. Migration Estimation Based on the Labour Force Survey: An EU-15 Perspective. International Migration Review 41(1), 101–126.

Merrilees, W.J., 1983. Alternative models of apprentice recruitment: with special reference to the British engineering industry. Applied Economics 15(1), 1–21.

Ministero del Lavoro e delle Politiche Sociali, 2015. Spesa per le politiche occupazionali e del lavoro – anno 2013 . Quaderni di studi e statistiche sul mercato del lavoro No. 7.

Pearl, J., 2000. Causality: Models, Reasoning, and Inference.

Picchio, M., Staffolani, S., 2013. Does Apprenticeship Improve Job Opportunities? A Regression Discontinuity Approach . IZA Discussion Paper No. 7719, IZA, Bonn.

Pohlmeier, W., Seiberlich, R., Uysal, S.D., 2014. A simple and successful shrinkage method for weighting estimators of treatment effects. Computational Statistics & Data Analysis.

Puhani, P.A., 2012. The treatment effect, the cross difference, and the interaction term in nonlinear "difference-in-differences" models. Economics Letters 115(1), 85–87.

Rubin, D.B., 1980. Randomization Analysis of Experimental Data: The Fisher Randomization Test Comment. Journal of the American Statistical Association 75(371), 591–593.

Rubin, D.B., 2001. Using Propensity Scores to Help Design Observational Studies: Application to the Tobacco Litigation. Health Services & Outcomes Research Methodology 2(3-4), 169–188.
Sianesi, B., 2004. An Evaluation of the Swedish System of Active Labor Market Programs in the 1990s. The Review of Economics and Statistics 86(1), 133–155.
Smith, J., Todd, P., 2005. Does matching overcome LaLonde's critique of nonexperimental estimators? Journal of Econometrics 125(1-2), 305–353.
Stevens, M., 1994. A Theoretical Model of On-the-Job Training with Imperfect Competition. Oxford Economic Papers 46(4), 537–62.
van Ours, J., 2004. The locking-in effect of subsidized jobs. Journal of Comparative Economics 32(1), 37–55.
Webb, M., 2014. Reworking Wild Bootstrap Based Inference for Clustered Errors . Queen's Economics Department Working Paper No. 1315, Queen's University, Ontario.
Wolfers, J., 2006. Did Unilateral Divorce Laws Raise Divorce Rates? A Reconciliation and New Results. American Economic Review 96(5), 1802–1820.
Wolter, S., Ryan, P., 2011. Apprenticeship . in E.A. Hanushek, S. Machin, and L. Wössmann (eds.): Handbook of the Economics of Education, Vol. 3, Amsterdam: Elsevier, ch. 11, 521-576.

1.8. Appendix

1.8.1. Regional differences and compositional change

In this appendix, we perform further robustness tests to verify whether the early implementing regions significantly differ from the late implementers and whether individuals self-selected in the policy by migrating or commuting from and to treated regions. Regional differences in the level and evolution of the labour market characteristics might bias the estimates. In particular, we are worried by differences in levels (for the ATT) and differences in evolution (for the ATT and ITT). We use data from the Italian LFS on the period 1st quarter 2000-4th quarter 2003, before any regional implementation.

The treated group includes the regions implementing the Law before 2007 (when we draw our inflow sample of apprentices). The outcomes we consider are the regional employment and unemployment rates, the diffusion of "temporary contracts" and "apprenticeship and training contracts" in the salaried employment (the latter are also included in the first category), and the job tenure - both for the youth (age 18-30) and the overall population. In Figure 1.9 and Table 1.9, we show the difference in level and evolution. Such differences are estimated by OLS regression of the regional outcome $\mathrm{Y_{it}}$ on the treated group dummy ($\mathrm{Treated}_i$), quarterly time dummies (dT_{it}), a common and a treated linear trend (t and $\mathrm{t} * \mathrm{Treated}_i$) and possibly quadratic trends (divided in common and treated trends). As we have few clusters (18 regions as in this analysis we can differentiate the region of Piedmont and Aosta), p-values are obtained by wild cluster bootstrap-t.

$$\mathrm{Y_{it}} = \sum_{t=1}^{T-1} (\vartheta_k \mathrm{dT}_{it}) + \eta * \mathrm{Treated}_i + \delta * \mathrm{t} + \gamma * \mathrm{t} * \mathrm{Treated}_i + \epsilon_{\mathrm{it}}$$

In general, differences in evolution and level are not statistically significant. Furthermore, using the survey weights to recalibrate the regional employment rate does not change the results. The only statistically significant difference at 5% is the relative diffusion of apprenticeship and training contracts among the youth, as the early implementing regions were the ones more relying on these contracts. This indicates that early implementers were more incentivised to revise the apprenticeship regime due to the relative importance of the contract in their economy, which is in line with the findings on the ATT and the ATU. Furthermore, we test the significance of some aggregate regional variables for the period 2000-2004: expenditure on apprenticeship per capita, the logarithm of GDP per capita and GDP growth. As shown in Table A.1 in the Supplementary Appendix A, no variable is statistically different at 5%, likely due to the very small sample size (i.e. number of regions for all the years). In line with the findings on the apprenticeship diffusion, early implementers are the regions allocating more funds to the regime. As the government did not provide the regions with sufficient funds, those first revising the apprenticeship had to rely both on their own and European resources (ISFOL, 2006, p. 65).

Table 1.9: Difference in evolution of regional labour market characteristics (2000q1-2003q4)

(A) ALL POPULATION	Employment rate in pp (1)		% temporary in pp (2)		% apprenticeship in pp (3)		Unemployment rate in pp (4)		Job Tenure – months (5)	
η – Treated	4.3	4.5	-0.5	-0.4	0.5*	0.5**	-0.6	-0.6	2.3	2.2
P-value	0.346	0.317	0.712	0.767	0.073	0.043	0.690	0.648	0.730	0.761
γ - Treated trend	0.06*	-0.04	0.04	0.00	0.01	-0.00	-0.01	0.01	-0.22*	-0.18
P-value	0.098	0.656	0.339	0.962	0.644	0.940	0.735	0.878	0.053	0.642
$\gamma 2$- Treated quadratic trend	-	0.01	-	0.00	-	0.00	-	-0.00	-	-0.00
P-value	-	0.252	-	0.749	-	0.800	-	0.647	-	0.880
N clusters	18	18	18	18	18	18	18	18	18	18
N obs.	1,821,042	1,821,042	725,190	725,190	725,190	725,190	1,089,302	1,089,302	965,913	965,913

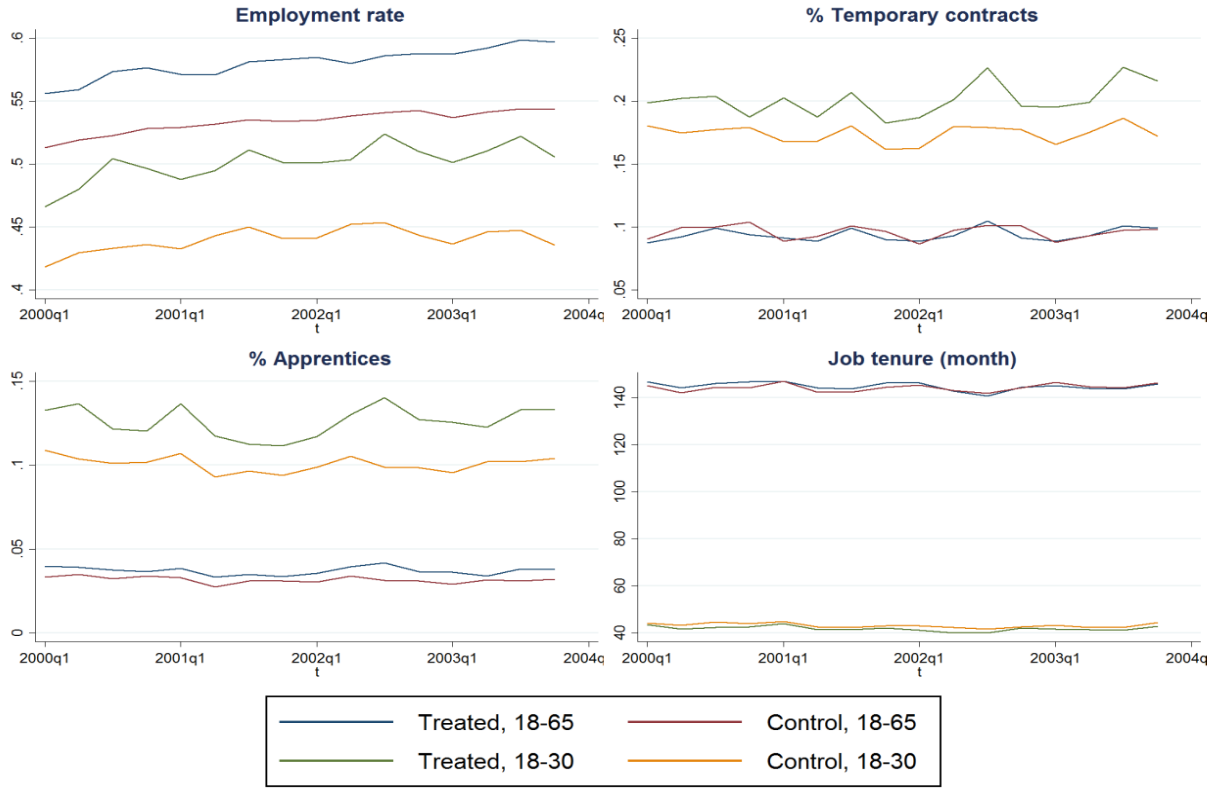
(B) YOUTH AGED 18-30	Employment rate in pp (1)		% temporary in pp (2)		% apprenticeship in pp (3)		Unemployment rate in pp (4)		Job Tenure– months (5)	
η – Treated	5.3	5.4	1.9	2.3	2.2**	2.5**	0.8	1.0	-1.4	-0.4
P-value	0.596	0.607	0.502	0.412	0.025	0.027	0.652	0.592	0.373	0.335
γ - Treated trend	0.11	-0.03	0.11	-0.07	0.04	-0.09	-0.03	-0.11	0.01	0.01
P-value	0.353	0.846	0.296	0.813	0.633	0.671	0.373	0.347	0.945	0.972
$\gamma 2$- Treated quadratic trend	-	0.00	-	0.01	-	0.01	-	0.01	-	-0.00
P-value	-	0.874	-	0.388	-	0.531	-	0.556	-	0.962
N clusters	18	18	18	18	18	18	18	18	18	18
N obs.	457,674	457,674	168,722	168,722	168,722	168,722	258,838	258,838	200,286	200,286

*Period 2000q1-2003q4 on quarterly LFS data. OLS with dependent variable: (1) employment rate in pp, (2) share of all temporary contracts (including apprenticeship) in pp, (3) share of apprenticeship and training contract in pp, (4) unemployment rate in pp, (5) job tenure in months (for employed). Sample: overall population (Panel A), youth aged 18-30 (Panel B). Covariates: quarterly time dummies, treated group dummy, common and treated linear trend (and possibly a quadratic trend). Treated regions passed Regional Laws before 2007. P-value obtained by wild cluster bootstrap-t with 2000 repetitions (18 regions as clusters).***: significant at 1%, **: significant at 5%, *: significant at 10%.*

One might argue that even if the labour markets of the two regional groups differ statistically, individuals might still assign themselves to the treatment by moving from and to the regions differing in treatment status. Thus, we test on the LFS whether inter-regional migration inflows and the commuting flows changed after the implementation of the reform. We follow the same approach of the DID-identification strategy used to estimate the impact on the employment rate (Section 1.5.2).

We either include a unique treatment dummy for the periods after the regional treatment (assuming homogenous effects - $A = 1$) or a set of dummies per each year after the treatment (A = 6 or 4).

Figure 1.9: Regional labour market characteristics in 2000-2003 (2007 treatment status)



Period 2000-2003 on quarterly LFS data. Evolution of employment rate, share of temporary contracts (including apprenticeship), share of apprenticeship and training contracts, job tenure in months (age 18-65 & 18-30). Treated regions implemented the treatment before 2007.

Concerning the estimation of *migration inflows*, we use the quarterly version of the LFS and the variable on the self-reported official place of residence one year earlier (for an analysis on immigration and commuting in 14 European LFS, see Huber 2014). Since the LFS follows households remaining in the same municipality over time, we only take the first waves in each cross-section to minimise the underestimation of the migratory flow (Martí and Ródenas, 2007). Consequently, we start the analysis from the 1st quarter 2005 as the information on the waves is not available before. The outcome Y_{it} is a binary variable taking the value of one if the individual self-reported to live in another region one year earlier. Regional dummies capture the fixed-effect in inter-regional migration inflows.

Concerning the estimation on *commuting*, we rely on the *longitudinal* LFS as it has information on the region where the person works. Note that as the longitudinal LFS contains provincial information, we include the provinces of Bolzano and Trento and Aosta region. The outcome Y_{it} takes value one if the person works in another region with respect to the region of residence. We analyse both the outflow, where the regional and treatment dummies regard the region where the person lives, and the inflow, where the regional and treatment dummies are about the region where the person works.

In the two analyses, we control for exogenous characteristics such as age, gender, education and citizenship. In addition, in the analysis on commuting we can control for the distance to the closest province in other regions (km by car). Finally, since the number of treated regions and implementing sectors progressively increased, we expect the effects on the early implementers to be stronger. Thus, for robustness we limit the horizon to the 3rd quarter of 2008. As people aged 25-29 have a larger incentive to move (they become eligible to the apprenticeship), we look for heterogeneous effects. Inference is performed by wild cluster bootstrap-t with regional clustering.

Descriptive evidence shows that in Italy fewer people report having changed the region of residence in one year distance compared to other European countries (e.g. in 2006 it was 0.24%; while in 14 European countries it was 0.6% – Huber, 2014). Commuting is more common though still under the average: the rate of inter-regional commuters in 2006 was 2.1%, while in EU-14 it was about 4.2%. The figures are higher if we only look at the Italian youth between 20 and 29 years old: 0.73% for immigration and 3.6% for commuting (before the treatment i.e. 1st quarter 2005). In this low mobility context, we estimate a non-statistically significant effect of the reform on the youth's migration and commuting behaviour (Table 1.10 & Table 1.11). The effect is insignificant even if we divide the population by age or we consider only the shorter run effects and stop the analysis by the 3rd quarter 2008. Finally, as the outcomes are binary variables we also implement the nonlinear DiD of Puhani (2012) and the Score Bootstrap of Kline and Andres (2012). Results are similar and are shown in Table A.6-A.7 of the Supplementary Appendix A.

Table 1.10: Compositional changes - effect on immigration inflows in treated regions

	2005q1-2011q1 (1, A)			**2005q1-2008q3 (2, A)**			**2005q1- 2011q1 (1, B)**			**2005q1-2008q3 (2, B)**		
Age class	**20-24**	**25-29**	**20-29**	**20-24**	**25-29**	**20-29**	**20-24**	**25-29**	**20-29**	**20-24**	**25-29**	**20-29**
Effect in pp – year 1	0.0	-0.1	-0.1	0.0	-0.3	-0.2	0.1	-0.1	-0.0	0.1	-0.4	-0.2
p-value	0.931	0.568	0.573	0.907	0.342	0.386	0.836	0.672	0.671	0.785	0.237	0.299
Effect in pp – year 2	-	-	-	-	-	-	-0.1	-0.0	-0.1	-0.1	-0.1	-0.1
p-value	-	-	-	-	-	-	0.637	0.795	0.677	0.753	0.838	0.641
Effect in pp – year 3	-	-	-	-	-	-	0.0	-0.2	-0.1	-0.1	-0.3	-0.2
p-value	-	-	-	-	-	-	0.921	0.303	0.362	0.854	0.216	0.422
Effect in pp – year 4	-	-	-	-	-	-	0.3	-0.4	-0.1	0.7	-1.1	-0.4
p-value	-	-	-	-	-	-	0.380	0.369	0.481	0.373	0.148	0.144
Effect in pp – year 5	-	-	-	-	-	-	-0.4	-0.5*	-0.5*	-	-	-
p-value	-	-	-	-	-	-	0.415	0.061	0.096	-	-	-
Effect in pp – year 6+	-	-	-	-	-	-	0.3	-0.7	-0.3*	-	-	-
p-value	-	-	-	-	-	-	0.335	0.101	0.076	-	-	-
N	27,998	42,178	70,176	17,779	26,894	44,673	27,998	42,178	70,176	17,779	26,894	44,673

*DiD on inter-regional migration inflows - controlling for: individual characteristics (education, gender and citizenship), time dummies and regional fixed-effect. The dependent variable is equal to one if the person lived in another region one year before. Specification either with a unique treatment dummy for all the period after the treatment (columns A) or a treatment dummy per each year after the treatment (columns B). Columns 1 end the analysis in the 1st quarter 2011, while columns 2 in the 3rd quarter 2008. The sample only contains the first waves from the quarterly LFS. P-value by wild cluster bootstrap-t (2000 repetitions) with 17 regions as clusters. ***: significant at 1%, **: significant at 5%, *: significant at 10%*

Table 1.11: Compositional changes - effect on commuting inflows and outflows

α - INFLOW	2004q1-2011q1 (1, A)			2004q1-2008q3 (2, A)			2004q1- 2011q1 (1, B)			2004q1-2008q3 (2, B)		
Age class	**20-24**	**25-29**	**20-29**	**20-24**	**25-29**	**20-29**	**20-24**	**25-29**	**20-29**	**20-24**	**25-29**	**20-29**
Effect in pp – year 1	-1.0	-0.6	-0.7	-1.2	-0.4	-0.7	-1.3*	-0.7	-0.9	-1.1	-0.5	-0.7
p-value	0.261	0.443	0.148	0.176	0.586	0.153	0.098	0.347	0.081	0.249	0.609	0.211
Effect in pp – year 2	-	-	-	-	-	-	-1.3	-0.2	-0.7	-1.8	-0.3	-0.9
p-value	-	-	-	-	-	-	0.137	0.852	0.366	0.192	0.788	0.246
Effect in pp – year 3	-	-	-	-	-	-	0.1	-0.9	-0.6	0.4	-0.4	-0.1
p-value	-	-	-	-	-	-	0.137	0.852	0.366	0.718	0.858	0.924
Effect in pp – year 4	-	-	-	-	-	-	-1.2	-0.6	-0.8	-	-	-
p-value	-	-	-	-	-	-	0.349	0.415	0.241	-	-	-
Effect in pp – year 5	-	-	-	-	-	-	-1.5	-0.6	-0.9	-	-	-
p-value	-	-	-	-	-	-	0.171	0.531	0.157	-	-	-
Effect in pp – year 6+	-	-	-	-	-	-	0.8	0.2	0.4	-	-	-
p-value	-	-	-	-	-	-	0.897	0.834	0.791	-	-	-
N	11,215	16,844	28,059	7,944	11,569	19,513	11,215	16,844	28,059	7,944	11,569	19,513

β - OUTFLOW	2004q1-2011q1 (1, A)			2004q1-2008q3 (2, A)			2004q1- 2011q1 (1, B)			2004q1-2008q3 (2, B)		
Age class	**20-24**	**25-29**	**20-29**	**20-24**	**25-29**	**20-29**	**20-24**	**25-29**	**20-29**	**20-24**	**25-29**	**20-29**
Effect in pp – year 1	-0.4	-0.2	-0.2	-1.0	0.2	-0.3	-0.6	-0.6	-0.6	-0.8	0.4	-0.1
p-value	0.540	0.689	0.635	0.349	0.799	0.784	0.546	0.499	0.434	0.502	0.664	0.938
Effect in pp – year 2	-	-	-	-	-	-	-1.0	-0.1	-0.5	-1.5	0.0	-0.6
p-value	-	-	-	-	-	-	0.169	0.914	0.564	0.160	0.997	0.513
Effect in pp – year 3	-	-	-	-	-	-	0.7	-1.2	-0.4	0.0	0.1	0.1
p-value	-	-	-	-	-	-	0.169	0.914	0.564	1.000	0.933	0.939
Effect in pp – year 4	-	-	-	-	-	-	0.7	1.4*	1.1	-	-	-
p-value	-	-	-	-	-	-	0.306	0.070	0.027	-	-	-
Effect in pp – year 5	-	-	-	-	-	-	-1.0	0.0	-0.4	-	-	-
p-value	-	-	-	-	-	-	0.497	0.977	0.628	-	-	-
Effect in pp – year 6+	-	-	-	-	-	-	-1.4*	0.5	-0.2	-	-	-
p-value	-	-	-	-	-	-	0.071	0.492	0.757	-	-	-
N	11,199	16,825	28,024	7,931	11,553	19,484	11,199	16,825	28,024	7,931	11,553	19,484

*DiD on the inter-regional commuting flows by age class (only employed people). We control for individual characteristics (education, gender, citizenship and distance to the closest province in another region), time dummies and regional fixed-effect. The dependent is equal to one if the person lives in another region with respect to the region of work. Commuting outflow (panel α above): regional dummies regard the region where the person lives. Commuting inflow (panel β below): regional dummies refer to the region where the person works. Specification either with a unique treatment dummy for all the period after the treatment (columns A) or a treatment dummy per each year after the treatment (columns B). Columns 1 end the analysis in the 1st quarter 2011, while columns 2 in the 3rd quarter 2008. P-value by wild cluster bootstrap-t (2000 repetitions) with 19 regions as clusters. ***: significant at 1%, **: significant at 5%, *: significant at 10%.*

1.8.2. *Tables and Figures*

Table 1.12: Life table on the full sample

Interval		Total	Deaths	Lost	Survival	SEs	[95% Conf. Int.]	
0	1	17950	1703	0	0.9051	0.0022	0.9007	0.9093
1	2	16247	1644	0	0.8135	0.0029	0.8078	0.8192
2	3	14603	1336	0	0.7391	0.0033	0.7326	0.7455
3	4	13267	988	0	0.6841	0.0035	0.6772	0.6908
4	5	12279	731	0	0.6433	0.0036	0.6363	0.6503
5	6	11548	619	0	0.6089	0.0036	0.6017	0.6160
6	7	10929	491	0	0.5815	0.0037	0.5742	0.5887
7	8	10438	440	0	0.5570	0.0037	0.5497	0.5642
8	9	9998	416	0	0.5338	0.0037	0.5265	0.5411
9	10	9582	367	0	0.5134	0.0037	0.5060	0.5207
10	11	9215	358	0	0.4934	0.0037	0.4861	0.5007
11	12	8857	343	0	0.4743	0.0037	0.4670	0.4816
12	13	8514	314	0	0.4568	0.0037	0.4495	0.4641
13	14	8200	263	0	0.4422	0.0037	0.4349	0.4494
14	15	7937	296	0	0.4257	0.0037	0.4184	0.4329
15	16	7641	242	0	0.4122	0.0037	0.4050	0.4194
16	17	7399	225	0	0.3997	0.0037	0.3925	0.4068
17	18	7174	289	0	0.3836	0.0036	0.3764	0.3907
18	19	6885	295	0	0.3671	0.0036	0.3601	0.3742
19	20	6590	212	0	0.3553	0.0036	0.3483	0.3623
20	21	6378	201	0	0.3441	0.0035	0.3372	0.3511
21	22	6177	171	0	0.3346	0.0035	0.3277	0.3415
22	23	6006	161	0	0.3256	0.0035	0.3188	0.3325
23	24	5845	323	0	0.3076	0.0034	0.3009	0.3144
24	25	5522	362	0	0.2875	0.0034	0.2809	0.2941
25	26	5160	168	0	0.2781	0.0033	0.2716	0.2847
26	27	4992	145	0	0.2700	0.0033	0.2636	0.2765
27	28	4847	126	0	0.2630	0.0033	0.2566	0.2695
28	29	4721	123	0	0.2562	0.0033	0.2498	0.2626
29	30	4598	160	0	0.2472	000320	0.2410	0.2536
30	31	4438	208	0	0.2357	0.0032	0.2295	0.2419
31	32	4230	99	0	0.2301	0.0031	0.2240	0.2363
32	33	4131	125	0	0.2232	0.0031	0.2171	0.2293
33	34	4006	119	0	0.2165	0.0031	0.2105	0.2226
34	35	3887	112	0	0.2103	0.0030	0.2044	0.2163
35	36	3775	483	0	0.1834	0.0029	0.1778	0.1891
36	37	3292	693	0	0.1448	0.0026	0.1397	0.1500
37	38	2599	97	0	0.1394	0.0026	0.1344	0.1445
38	39	2502	84	0	0.1347	0.0025	0.1298	0.1397
39	40	2418	71	0	0.1308	0.0025	0.1259	0.1357
40	41	2347	86	0	0.1260	0.0025	0.1212	0.1309
41	42	2261	188	0	0.1155	0.0024	0.1109	0.1202
42	43	2073	222	0	0.1031	0.0023	0.0987	0.1076
43	44	1851	55	0	0.1001	0.0022	0.0957	0.1045
44	45	1796	55	0	0.0970	0.0022	0.0927	0.1014
45	46	1741	70	0	0.0931	0.0022	0.0889	0.0974
46	47	1671	108	0	0.0871	0.0021	0.0830	0.0913
47	48	1563	316	0	0.0695	0.0019	0.0658	0.0733
48	49	1247	524	0	0.0403	0.0015	0.0375	0.0432
49	50	723	46	0	0.0377	0.0014	0.0350	0.0406
50	51	677	22	0	0.0365	0.0014	0.0338	0.0393
51	52	655	37	0	0.0344	0.0014	0.0318	0.0372
52	53	618	39	0	0.0323	0.0013	0.0297	0.0349
53	54	579	53	0	0.0293	0.0013	0.0269	0.0318
54	55	526	49	0	0.0266	0.0012	0.0243	0.0290
55	56	477	11	0	0.0260	0.0012	0.0237	0.0284
56	57	466	7	0	0.0256	0.0012	0.0233	0.0280
57	58	459	9	0	0.0251	0.0012	0.0229	0.0274
58	59	450	4	0	0.0248	0.0012	0.0226	0.0272
59	60	446	41	405	0.0207	0.0011	0.0185	0.0230

Lifetable on the Survival Rate in the initial apprenticeship contract. Inflow sample of 17,950 apprentices hired in 2007 aged 19-24.

Table 1.13: Regional implementation of the reform until the 1st quarter of 2011

Year	Quarter	Regions	Description
2005	2	Tuscany	Regional Law no. 20/2005. Execution in April (Resolution no. 427).
	3	Marche	Regional Law no. 2/2005. Execution in August (Resolution no. 976).
	3	Emilia	Regional Law no. 17/2005. Execution in September (Resolution no. 1256).
2006	1	Friuli-Venezia Giulia	Regional Law no. 18/2005. Execution in 15 December 2005 (Resolution n.2938).
	1	Sardinia	Regional Law no. 20/2005. Execution in February (Resolution no. 8/15).
	2	Bolzano	Regional Law no. 2/2006. Several executions in later time (e.g. July and November 2006).
	3	Puglia	Regional Law no. 13/2005. Execution in July (Resolution no. 1125). Law annulled by the Constitutional Court (n. 24/2007) and modified by the Regional Law no.40/2006.
2007	3	Lazio	Regional Law no. 9/2006. Execution in July (Resolution no. 7/2007).
	4	Piedmont	Regional Law no. 2/2007. Execution in November (Resolution no. 73).
2008	3	Trento	Regional Law no. 6/2006. Execution in September.
2009	1	Umbria	Regional Law no. 18/2007. Execution in January 2008 (Resolution no. 5).
2010	2	Molise	Regional Law no. 3/2008. Execution in March 2010 (Resolution no. 208).
	4	Campania	Regional Law no. 14/2009. Execution in October 2010 (Resolution no. 690).

Date on the quarter of implementation of the Regional Laws.

Table 1.14: Evolution of the share of youth in several contracts (%) – heterogenous sample

	Year	Appr. initial firm (1)	Other appr. (2)	Open-End same firm (3)	Open-End other firms (4)	Temporary (5)	Collaborator (6)	Insured unemployed (7)	Out-of-database (8)
Men (N=10,392) (A)	1	48.8%	9.5%	1.7%	4.4%	8.2%	0.7%	0.1%	26.6%
	2	32.1%	12.4%	4.0%	7.7%	8.3%	0.9%	0.7%	33.7%
	3	20.6%	12.4%	8.2%	11.2%	9.6%	0.9%	1.1%	36.0%
	4	8.2%	10.3%	14.7%	16.3%	10.7%	1.3%	1.7%	36.8%
	5	3.1%	8.3%	16.2%	20.2%	10.4%	1.7%	2.3%	37.7%
Women (N = 7,558) (B)	1	48.8%	10.4%	1.4%	3.5%	8.6%	1.2%	0.1%	26.1%
	2	32.5%	14.4%	3.5%	7.1%	9.9%	1.4%	0.4%	30.8%
	3	20.0%	14.4%	7.6%	11.4%	11.3%	1.5%	1.3%	32.6%
	4	8.5%	12.1%	13.1%	16.6%	12.8%	1.4%	1.9%	33.5%
	5	2.8%	8.9%	15.1%	21.7%	13.6%	1.9%	2.4%	33.5%
Small firms (N =11,760) (C)	1	45.6%	10.4%	1.4%	4.3%	8.3%	0.9%	0.1%	29.1%
	2	28.7%	13.8%	3.4%	7.8%	8.9%	1.1%	0.5%	35.6%
	3	17.5%	13.7%	6.8%	11.3%	10.2%	1.2%	1.0%	38.2%
	4	8.0%	11.9%	10.9%	15.7%	11.4%	1.4%	1.8%	38.9%
	5	3.3%	9.2%	12.3%	20.3%	11.6%	1.8%	2.4%	39.1%
Large firms (N=6,190) (D)	1	54.9%	9.0%	1.8%	3.6%	8.4%	0.8%	0.1%	21.3%
	2	39.1%	12.2%	4.5%	6.8%	9.0%	1.1%	0.7%	26.6%
	3	25.7%	12.4%	10.1%	11.3%	10.5%	0.9%	1.4%	27.7%
	4	9.1%	9.5%	20.0%	17.8%	11.9%	1.3%	1.7%	28.7%
	5	2.4%	7.4%	22.3%	22.0%	12.0%	1.8%	2.3%	29.7%
Without work experience (N = 6,367) (E)	1	51.2%	8.4%	1.2%	3.2%	5.9%	0.8%	0.0%	29.3%
	2	35.1%	12.0%	2.6%	6.5%	7.0%	1.2%	0.3%	35.5%
	3	22.2%	12.6%	7.1%	10.0%	8.0%	1.0%	0.8%	38.2%
	4	9.6%	10.7%	13.5%	14.9%	9.5%	1.4%	1.1%	39.2%
	5	4.1%	8.2%	15.3%	19.2%	10.1%	1.8%	1.8%	39.7%
With work experience (N =11,583) (F)	1	47.5%	10.7%	1.7%	4.5%	9.7%	0.9%	0.1%	24.8%
	2	30.8%	14.0%	4.5%	8.0%	10.1%	1.1%	0.8%	30.9%
	3	19.3%	13.6%	8.4%	12.0%	11.6%	1.2%	1.4%	32.6%
	4	7.6%	11.3%	14.3%	17.3%	12.7%	1.3%	2.1%	33.3%
	5	2.4%	8.8%	16.0%	21.8%	12.7%	1.8%	2.7%	33.8%

Inflow sample of 17,950 apprentices hired in 2007 aged 19-24. Different sub-sample based on characteristics at hiring: (A) men, (B) women, (C) firms with at most 10 employees, (D) firms with at least 11 employees, (E) apprentices without previous work experience, (F) apprentices with previous work experience. Status at the end of the year after hiring: (1) apprenticeship in the first firm, (2) other apprenticeship, (3) open-end contract in the same firm, (4) open-end contract in another firm, (5) other temporary contract, (6) collaborator, (7) insured unemployed, (8) not in salaried employment in the private sector. Individuals with more jobs are considered only in one position following this order: initial apprenticeship, open-end contract (same, other firms), other apprenticeship, other temporary contract, collaborator, insured unemployment and not in salaried private sector employment.

Figure 1.10: Descriptive Statistics - Reason for ending contract in each month

Reasons for the termination of the initial apprenticeship for the apprentices terminating the contract in each month (the graph on unknown reason is not reported). Inflow sample of 17,950 apprentices hired in 2007 aged 19-24. 1) Expiration of the contract, 2) Apprentice resignation, 3) Transformation to another contract, 4) Dismissal.

Table 1.15: Descriptive Statistics - sample size by treatment status

	20-24 years old (1)				25-29 years old (2)				30-34 years old (3)			
Time	Control	Treated	% Treat.	Total	Control	Treated	% Treat.	Total	Control	Treated	% Treat.	Total
1/04	5,201	0	0%	5,201	7,895	0	0%	7,895	10,024	0	0%	10,024
2/04	5,065	0	0%	5,065	7,920	0	0%	7,920	9,894	0	0%	9,894
3/04	5,422	0	0%	5,422	8,044	0	0%	8,044	10,336	0	0%	10,336
4/04	5,285	0	0%	5,285	8,060	0	0%	8,060	10,117	0	0%	10,117
1/05	5,322	0	0%	5,322	8,165	0	0%	8,165	10,284	0	0%	10,284
2/05	4,919	231	4%	5,150	7,320	435	6%	7,755	9,351	534	5%	9,885
3/05	4,149	636	13%	4,785	6,256	1,079	15%	7,335	7,864	1,451	16%	9,315
4/05	4,104	641	14%	4,745	6,196	1,147	16%	7,343	8,122	1,509	16%	9,631
1/06	3,751	918	20%	4,669	5,534	1,569	22%	7,103	7,511	2,106	22%	9,617
2/06	3,754	908	19%	4,662	5,476	1,467	21%	6,943	7,406	2,021	21%	9,427
3/06	3,614	871	19%	4,485	5,370	1,422	21%	6,792	7,037	1,950	22%	8,987
4/06	3,474	897	21%	4,371	5,217	1,492	22%	6,709	7,089	1,994	22%	9,083
1/07	3,697	897	20%	4,594	5,347	1,448	21%	6,795	7,299	2,087	22%	9,386
2/07	3,473	844	20%	4,317	5,111	1,368	21%	6,479	6,861	2,020	23%	8,881
3/07	3,100	1,117	26%	4,216	4,583	1,765	28%	6,348	6,135	2,453	29%	8,588
4/07	2,716	1,544	36%	4,260	3,939	2,451	38%	6,390	5,274	3,554	40%	8,828
1/08	2,890	1,592	36%	4,482	4,074	2,478	38%	6,552	5,532	3,589	39%	9,121
2/08	2,711	1,534	36%	4,245	3,840	2,303	37%	6,143	5,107	3,394	40%	8,501
3/08	2,588	1,511	37%	4,099	3,731	2,347	39%	6,078	4,811	3,398	41%	8,209
4/08	2,566	1,470	36%	4,036	3,900	2,387	38%	6,287	4,923	3,576	42%	8,499
1/09	2,604	1,607	38%	4,211	3,680	2,472	40%	6,152	4,947	3,667	43%	8,614
2/09	2,406	1,519	39%	3,925	3,341	2,252	40%	5,593	4,578	3,447	43%	8,025
3/09	2,280	1,497	40%	3,777	3,159	2,260	42%	5,419	4,241	3,386	44%	7,627
4/09	2,479	1,589	39%	4,068	3,570	2,490	41%	6,060	4,790	3,684	43%	8,474
1/10	2,433	1,590	40%	4,023	3,497	2,460	41%	5,957	4,723	3,535	43%	8,258
2/10	2,235	1,603	42%	3,838	3,099	2,501	45%	5,600	4,290	3,468	45%	7,758
3/10	2,210	1,547	41%	3,757	3,055	2,455	45%	5,510	4,130	3,508	46%	7,638
4/10	1,933	1,943	50%	3,876	2,799	2,997	52%	5,796	3,684	4,100	53%	7,784
1/11	1,928	2,007	51%	3,935	2,752	3,098	53%	5,850	3,609	3,958	52%	7,567
Total	98,309	30,512	24%	128,821	144,930	48,143	25%	193,073	189,968	68,389	26%	258,357

Distribution by treatment status and age class (1: 20-24, 2: 25-29, 3: 30-34). Quarterly LFS data excluding Puglia and Trentino Alto Adige and students during the current time t. Treatment status defined as living in a region that has implemented the Regional Law by time t.

Table 1.16: Descriptive Statistics - LFS sample (regional and sector treatment)

	20-24 years old (1)				25-29 years old (2)				30-34 years old (3)			
Time	Control	Treated	% Treat.	Total	Control	Treated	% Treat.	Total	Control	Treated	% Treat.	Total
1/04	3,174	0	0.0%	3,174	5,417	0	0.0%	5,417	7,074	0	0.0%	7,074
2/04	3,096	0	0.0%	3,096	5,471	0	0.0%	5,471	7,048	0	0.0%	7,048
3/04	3,340	0	0.0%	3,340	5,561	0	0.0%	5,561	7,288	0	0.0%	7,288
4/04	3,174	15	0.5%	3,189	5,580	23	0.4%	5,603	7,161	28	0.4%	7,189
1/05	3,114	12	0.4%	3,126	5,488	23	0.4%	5,511	7,193	35	0.5%	7,228
2/05	2,950	237	7.4%	3,187	4,822	452	8.6%	5,274	6,446	549	7.8%	6,995
3/05	1,898	989	34.3%	2,887	3,309	1,655	33.3%	4,964	4,444	2,091	32.0%	6,535
4/05	1,798	1,031	36.4%	2,829	3,121	1,885	37.7%	5,006	4,322	2,520	36.8%	6,842
1/06	1,553	1,297	45.5%	2,850	2,568	2,272	46.9%	4,840	3,731	3,072	45.2%	6,803
2/06	1,573	1,346	46.1%	2,919	2,568	2,265	46.9%	4,833	3,676	3,029	45.2%	6,705
3/06	1,182	1,607	57.6%	2,789	2,138	2,580	54.7%	4,718	2,877	3,459	54.6%	6,336
4/06	1,139	1,529	57.3%	2,668	2,063	2,617	55.9%	4,680	2,920	3,567	55.0%	6,487
1/07	1,125	1,620	59.0%	2,745	1,968	2,699	57.8%	4,667	2,814	3,862	57.8%	6,676
2/07	1,109	1,655	59.9%	2,764	1,834	2,711	59.6%	4,545	2,633	3,761	58.8%	6,394
3/07	778	1,882	70.8%	2,660	1,416	3,065	68.4%	4,481	2,096	4,027	65.8%	6,123
4/07	608	1,974	76.5%	2,582	1,168	3,230	73.4%	4,398	1,814	4,560	71.5%	6,374
1/08	594	2,149	78.3%	2,743	1,070	3,380	76.0%	4,450	1,593	4,911	75.5%	6,504
2/08	566	2,143	79.1%	2,709	1,056	3,251	75.5%	4,307	1,506	4,637	75.5%	6,143
3/08	521	2,059	79.8%	2,580	950	3,213	77.2%	4,163	1,378	4,437	76.3%	5,815
4/08	493	1,925	79.6%	2,418	955	3,263	77.4%	4,218	1,417	4,666	76.7%	6,083
1/09	473	1,916	80.2%	2,389	924	3,136	77.2%	4,060	1,388	4,617	76.9%	6,005
2/09	463	1,809	79.6%	2,272	902	2,857	76.0%	3,759	1,328	4,321	76.5%	5,649
3/09	443	1,726	79.6%	2,169	792	2,793	77.9%	3,585	1,241	4,113	76.8%	5,354
4/09	404	1,856	82.1%	2,260	886	3,051	77.5%	3,937	1,380	4,543	76.7%	5,923
1/10	442	1,734	79.7%	2,176	884	2,932	76.8%	3,816	1,339	4,268	76.1%	5,607
2/10	426	1,704	80.0%	2,130	807	2,857	78.0%	3,664	1,198	4,130	77.5%	5,328
3/10	377	1,675	81.6%	2,052	740	2,771	78.9%	3,511	1,139	4,065	78.1%	5,204
4/10	338	1,762	83.9%	2,100	725	3,006	80.6%	3,731	1,061	4,287	80.2%	5,348
1/11	337	1,691	83.4%	2,028	749	3,015	80.1%	3,764	1,077	4,094	79.2%	5,171
Total	37,488	39,343	51.2%	76,831	65,932	65,002	49.6%	130,934	90,582	91,649	50.3%	182,231

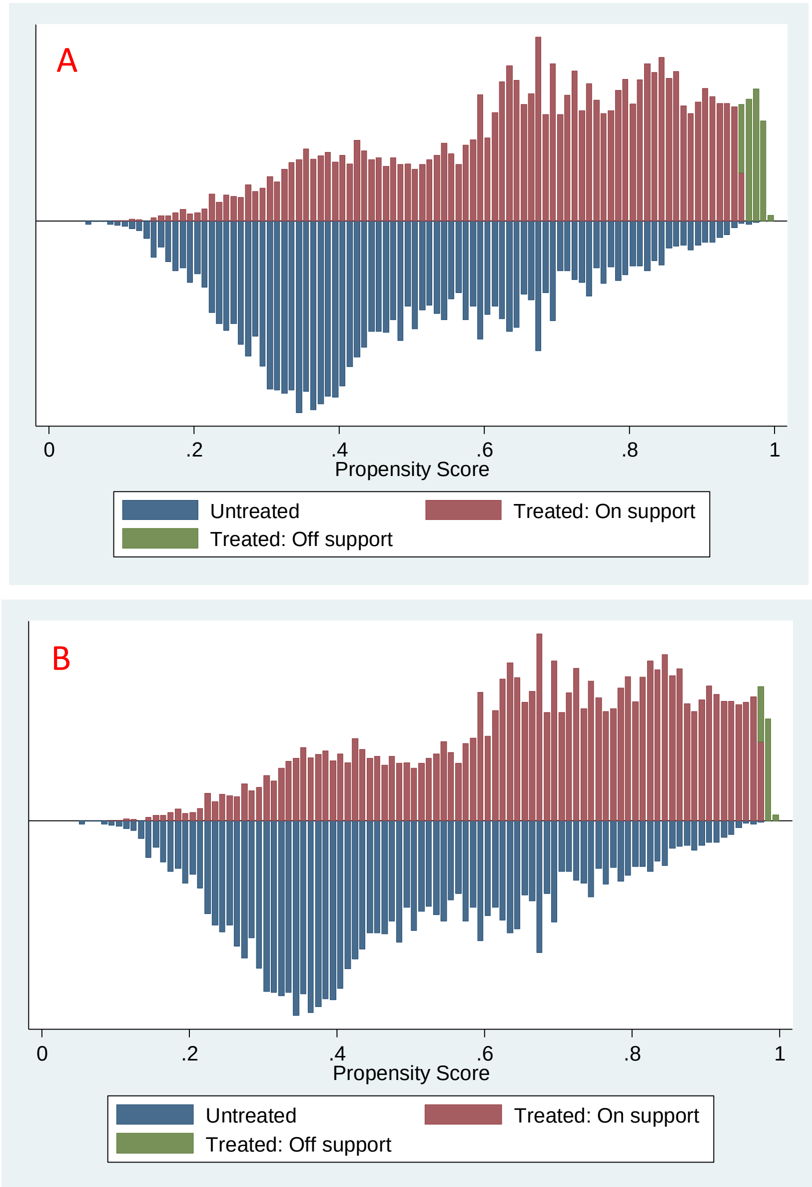
Sample size and distribution by treatment status and age class (1: 20-24, 2: 25-29, 3: 30-34). Quarterly LFS data excluding students, non-employed youth, employees in the public administration and people living in Puglia or Trentino Alto Adige. Treatment status defined as living in a region that has implemented the Regional Law or working in a sector or regional pilot test implementing the new apprenticeship regime.

Table 1.17: Covariates - educational level (quarterly LFS)

	20-24 years old (1)			25-29 years old (2)		
Education & specialization	N	%	Cum. %	N	%	Cum. %
None, primary school: 5 years	3,304	2.6	2.6	6,198	3.2	3.2
Secondary school: 3 years	42,269	32.8	35.4	56,183	29.1	32.3
Professional institute: industry and agriculture: 2-3 years	3,383	2.6	38.0	3,832	2.0	34.3
Professional institute: trade, tourism and hotel: 2-3 years	2,949	2.3	40.3	3,081	1.6	35.9
Other Professional institute: 2-3 years	4,205	3.3	43.6	5,424	2.8	38.7
Professional institute: 4-5 years	15,095	11.7	55.3	14,572	7.6	46.3
Other technical institute: 4-5 years	8,898	6.9	62.2	9,385	4.9	51.1
Industrial and technical institute: 4-5 years	9,170	7.1	69.3	9,250	4.8	55.9
Technical Institute for accountants: 4-5 years	15,175	11.8	81.1	22,522	11.7	67.6
Technical Institute for Surveyors: 4-5 years	4,331	3.4	84.4	6,790	3.5	71.1
Gymnasium high school: 4-5 years	1,327	1.0	85.5	2,649	1.4	72.5
Scientific High school: 4-5 years	4,122	3.2	88.7	6,552	3.4	75.9
Language high school: 4-5 years	2,023	1.6	90.2	2,441	1.3	77.1
Education high school: 4-5 years	2,113	1.6	91.9	4,994	2.6	79.7
Other high school: 4-5 years	1,866	1.5	93.3	3,851	2.0	81.7
Social Sciences high school: 4-5 years	2,001	1.6	94.9	-	-	-
Arts high school (4-5 years) or conservatory	1,760	1.4	96.3	2,611	1.4	83.0
Bachelor level: 2-3 years	3,293	2.6	98.8	-	-	-
Bachelor - human sciences: 2-3 years				2,502	1.3	84.3
Bachelor - economics / law: 2-3 years	-	-	-	1,268	0.7	85.0
Bachelor - natural sciences / engineering: 2-3 years	-	-	-	1,792	0.9	85.9
Bachelor - health/other: 2-3 years	-	-	-	2,794	1.5	87.4
Master or more: 4+ years	1,537	1.2	100.0	-	-	-
Master - human sciences: 4+ years	-	-	-	8,297	4.3	91.7
Master -economics/law: 4+ years	-	-	-	7,481	3.9	95.5
Bachelor - natural sciences / engineering: 4+ years	-	-	-	6,182	3.2	98.8
Master - health/other: 4+ years	-	-	-	2,422	1.3	100.0
Total	128,821	100.0		193,073	100.0	

Sample size by level of education and age class (1: 20-24, 2: 25-29). LFS data. We exclude students in t and people living in Puglia or Trentino Alto Adige.

Figure 1.11: PS distribution – 99.9 (A) and max (B) trimming rule



Distribution of the Propensity Score estimated by CBPS with 99.9 trimming rule (Panel A) or max trimming rule. The 99.9 trimming rule removes the part of the distribution with very thin overlap due to outliers in the PS of the control group.

Figure 1.12: Share of apprentices leaving in a month and not employed N months after the exit

Share of apprentices terminating the contract in a month (x-axis) not employed in the salaried private sector 1, 2, 3, 4 months after the exit.

Table 1.18: Covariate Balancing Propensity Score - estimation model: overall sample

Log-Likelihood: -9926.055

Variables – independent variable = treated dummy		β	SE	z	P-value
	Intercept	-0.2220	0.2520	-0.88	0.378
Hiring information: Quarter	1st quarter	-	-	-	-
	2nd quarter	0.1470	0.1510	0.98	0.329
	3rd quarter	0.3530**	0.1530	2.31	0.021
	4th quarter	0.6960***	0.1630	4.26	0.000
Hiring information: Sector (ateco81)	Agriculture, energy, mining (1-24)	-0.5880	0.4100	-1.43	0.151
	Manufacture of chemicals (25)	0.2020	0.6020	0.34	0.738
	Metal manufacturing (31)	-0.8900***	0.1990	-4.48	0.000
	Manuf.: mechanical machine (32)	-0.3390	0.2880	-1.18	0.239
	Manuf: office machinery & computers (33)	0.1030	0.4240	0.24	0.808
	Other metal manufacturing (34-37)	-0.7460***	0.2370	-3.15	0.002
	Light Manuf.: food (41)	-1.2200***	0.2450	-4.97	0.000
	Light Manuf: sugar, beverage, tobacco (42)	-1.2600**	0.6290	-2.01	0.045
	Light Manuf: textile and leather (43, 44)	-1.0900	0.7200	-1.51	0.130
	Light Manuf: wearing & dressing (45)	-1.2300***	0.4240	-2.90	0.004
	Light Manuf: wood (46)	-0.8100**	0.3930	-2.06	0.040
	Light Manuf: paper, printing, publishing (47)	-0.3410	0.3570	-0.95	0.340
	Light Manuf: rubber & others (48, 49)	-0.9280***	0.2270	-4.09	0.000
	Construction (50)	-	-	-	-
	Wholesale trade (61, 62)	1.2200***	0.2590	4.72	0.000
	Agents & commission trade (63)	-0.1440	0.3870	-0.37	0.710
	Retail: food, cloth, household, pharmacies(64)	1.0400***	0.2590	4.01	0.000
	Retail: vehicles, fuel, books & others (65)	1.0500***	0.1780	5.92	0.000
	Tourism and hotels (66)	-1.1200***	0.2100	-5.31	0.000
	Repair of consumer goods and vehicles (67)	-1.1300***	0.2630	-4.31	0.000
	Transport and communication (72-79)	0.1870	0.5070	0.37	0.713
	Finance and banking (81-83)	0.3100	0.2390	1.30	0.194
	Health (95)	0.4650	1.0100	0.46	0.645
	Recreational, cultural & sport activities (97)	-1.3800**	0.6540	-2.11	0.035
	Personal service (98)	-1.7000***	0.2730	-6.21	0.000
	Other services (91- 93, 96)	-1.2400***	0.4610	-2.69	0.007
Hiring information	Seasonal apprenticeship	-0.4270	0.3860	-1.11	0.269
Hiring: reason of hiring	Non-standard (e.g. firm restructuring)	-1.2000	0.7490	-1.61	0.108
Hiring information: Age	19	-0.1880	0.3170	-0.59	0.554
	20	-0.0814	0.2820	-0.29	0.773
	21	0.0016	0.2640	0.01	0.995
	22	0.0235	0.2380	0.10	0.921
	23	0.0943	0.2420	0.39	0.697
	24	-	-	-	-
Gender	Women	-0.0615	0.1870	-0.33	0.742
Hiring information: Firm position	Single	-	-	-	-
	Subsidiary	-0.6170*	0.3710	-1.67	0.096
	Parent-company	-0.1620	0.2950	-0.55	0.583
Hiring information	Part-time apprenticeship	-0.0723	0.1940	-0.37	0.709
Hiring: Job-to-Job transition	Not employed 30 days before the hiring	-0.1080	0.2680	-0.40	0.687
Hiring information: Firm size	0-5 employees	-	-	-	-
	6-10 employees	-0.0027	0.1760	-0.02	0.988
	11-15 employees	0.0440	0.2350	0.19	0.851
	16-20 employees	-0.0316	0.3510	-0.09	0.928
	21-25 employees	0.4950*	0.2790	1.77	0.077
	26-30 employees	0.7600*	0.4340	1.75	0.081
	31-40 employees	0.7690*	0.4060	1.89	0.058
	41-50 employees	1.1400***	0.3180	3.60	0.000
	51-100 employees	0.9420**	0.4350	2.17	0.030
	101-200 employees	1.1600***	0.3530	3.29	0.001
	201-500 employees	1.0000***	0.3830	2.62	0.009
	501+ employees	2.0500***	0.4130	4.97	0.000
Last job (vs. new job)	Different sector (Ateco81 2 digits)	0.0198	0.1320	0.15	0.881
Last job (vs. new job)	Same firm	0.1240	0.1900	0.65	0.515
Last job (vs. new job)	Apprentice in the same firm	-0.1660	0.2210	-0.75	0.453
Last job (vs. new job)	Apprentice in the same sector	0.0130	0.1900	0.07	0.945
Last job as employee	Primary	-	-	-	-

	Manufacturing	0.1990	0.2770	0.72	0.473
	Construction	0.2090	0.3420	0.61	0.542
	Services (or unknown)	0.0764	0.2500	0.31	0.760
	Tourism	0.0263	0.3270	0.08	0.936
	Unknown	0.2200	0.2280	0.97	0.333
Last job as employee	% full-time	0.2280	0.2350	0.97	0.332
Last job as employee	Full-time weekly remuneration (2007 €)	0.0013*	0.0008	1.65	0.100
Last job: Non-employment spell	Number of days last job-apprenticeship	0.0000	0.0004	0.07	0.946
Last job: Contract	Apprentices	0.0998	0.3020	0.33	0.741
	Open-end	-0.0850	0.3580	-0.24	0.813
	Temporary agency worker	-	-	-	-
	Seasonal	0.1340	0.4960	0.27	0.786
	Fixed-term	0.0244	0.1940	0.13	0.900
	Collaborator	0.3120	0.2440	1.28	0.201
	Other/Unknown	0.0424	0.3320	0.13	0.898
Last job as employee	Part-time	0.0598	0.1930	0.31	0.757
Last job: Reason end of the contract	No information (or before 2005 or collabor.)	0.1040	0.3850	0.27	0.788
	Dismissal	0.0601	0.3500	0.17	0.864
	Resignation	0.1820	0.2890	0.63	0.529
	End of contract (includes collaborator)	-0.0241	0.3440	-0.07	0.944
	Others	-	-	-	-
Last job	Tenure (in weeks)	-0.0021	0.0019	-1.14	0.254
Employment history : Age at first job	<15	-0.3570	0.2680	-1.33	0.184
	16	-0.1750	0.1650	-1.06	0.290
	17	-0.0700	0.1160	-0.60	0.547
	18	-0.0766	0.0985	-0.78	0.437
	19	-0.0210	0.1430	-0.15	0.883
	20	-0.1470	0.1480	-0.99	0.322
	21	-0.0811	0.1980	-0.41	0.682
	22	-0.0543	0.3240	-0.17	0.867
	23-24	-	-	-	-
Employment history	Average Contract length (days)	0.0002	0.0004	0.48	0.634
Employment history : Ever worked with specific contract	Open-term	0.1410	0.1760	0.80	0.425
	Temporary	0.2180	0.2180	1.00	0.318
	Apprenticeship	-0.0538	0.1970	-0.27	0.785
	Collaborator	0.1550	0.3190	0.49	0.626
	Seasonal	-0.0518	0.1860	-0.28	0.780
Employment history as employee: % experience by contract	Open-end	-0.0598	0.3360	-0.18	0.859
	Apprentice	-0.1450	0.3270	-0.44	0.657
	Other	-	-	-	-
Employment history as employee: % experience by collar	Blue collar	-0.1050	0.1920	-0.55	0.585
	Apprentice	-	-	-	-
	White collar	-	-	-	-
Employment history	Average share working time	0.1540	0.3910	0.39	0.693
Employment history	Av. Full-time weekly remuneration (2007 €)	-0.0001	0.0009	-0.14	0.891
Employment history	N. of previous jobs (with collaborators)	-0.0369	0.0563	-0.66	0.512
Employment history	Experience as collaborator (months)	0.0006	0.0256	0.02	0.980
Employment history	Experience as employee (weeks)	0.0024	0.0016	1.49	0.135
Employment history	Experience as apprentice (weeks)	-0.0025	0.0021	-1.16	0.247
Employment history as employee: % Experience by firm size	Micro (0-5)	-0.0397	0.1460	-0.27	0.786
	Small (6-25)	-0.0130	0.2110	-0.06	0.951
	Medium (26-200)	-0.2280	0.3940	-0.58	0.562
	Large (201+)	-	-	-	-
Employment history	No experience (employee/collaborator)	0.8010***	0.2510	3.19	0.001
Employment history	Ever insured unemployed	-0.1260	0.4090	-0.31	0.758
Employment history	Experience only as collaborator	0.2750	0.2980	0.93	0.355
Number of units	Number of individuals	17,914			

Over-identification test: Null of good specification. J - statistic: 80.41 on 105 degrees of freedom – P-value 0.964

Table 1.19: List of covariates – balancing test on full sample

Variables		Raw or CBPS	Mean Treated	Mean Control	Standardized Bias	P-value - mean equality
Hiring information: quarter	1st quarter	RAW	26.2%	30.9%	-10.5%	0.000
		CBPS	26.8%	26.5%	0.8%	0.694
	2nd quarter	RAW	25.5%	31.3%	-13.0%	0.000
		CBPS	26.0%	24.9%	2.3%	0.254
	3rd quarter	RAW	23.8%	22.6%	3.0%	0.049
		CBPS	23.8%	23.1%	1.7%	0.450
	4th quarter	RAW	24.5%	15.2%	23.5%	0.000
		CBPS	23.4%	25.5%	-5.4%	0.044
Hiring information: Sector (ateco81)	Agriculture, energy, mining (1-24)	RAW	1.9%	1.8%	0.4%	0.782
		CBPS	2.0%	1.6%	2.8%	0.073
	Manufacture of chemicals (25)	RAW	0.6%	0.2%	5.4%	0.001
		CBPS	0.6%	0.4%	2.2%	0.332
	Metal manufacturing (31)	RAW	5.0%	8.1%	-12.4%	0.000
		CBPS	5.3%	5.2%	0.7%	0.625
	Manuf.: mechanical machine (32)	RAW	2.2%	2.0%	2.1%	0.180
		CBPS	2.4%	2.1%	1.7%	0.332
	Manuf: office machinery & computers (33)	RAW	1.1%	0.5%	6.9%	0.000
		CBPS	1.1%	1.2%	-0.3%	0.925
	Manuf: other (34-37)	RAW	3.0%	3.5%	-2.5%	0.101
		CBPS	3.2%	2.5%	3.8%	0.008
	Light Manuf.: food (41)	RAW	2.1%	5.2%	-16.7%	0.000
		CBPS	2.2%	2.2%	0.1%	0.942
	Light Manuf: sugar, beverage, tobacco (42)	RAW	0.5%	1.5%	-9.8%	0.000
		CBPS	0.5%	0.5%	0.2%	0.858
	Light Manuf: textile and leather (43, 44)	RAW	0.8%	1.7%	-7.7%	0.000
		CBPS	0.9%	0.9%	0.2%	0.854
	Light Manuf: wearing & dressing (45)	RAW	1.2%	2.6%	-10.5%	0.000
		CBPS	1.3%	1.2%	0.3%	0.758
	Light Manuf: wood (46)	RAW	1.7%	2.7%	-6.5%	0.000
		CBPS	1.8%	1.8%	0.2%	0.883
	Light Manuf: paper, printing, publishing (47)	RAW	1.1%	0.9%	1.5%	0.325
		CBPS	1.2%	1.2%	0.0%	0.982
	Light Manuf: rubber & others (48, 49)	RAW	1.4%	2.3%	-6.4%	0.000
		CBPS	1.5%	1.5%	0.3%	0.851
	Construction (50)	RAW	21.5%	15.3%	16.0%	0.000
		CBPS	22.8%	22.7%	0.5%	0.832
	Wholesale trade (61, 62)	RAW	5.8%	0.9%	27.2%	0.000
		CBPS	5.2%	5.1%	0.4%	0.921
	Agents & commission trade (63)	RAW	2.1%	1.5%	4.8%	0.002
		CBPS	2.2%	2.3%	-0.6%	0.807
	Retail: food, cloth, household, pharmacies (64)	RAW	10.4%	2.1%	34.7%	0.000
		CBPS	9.0%	9.8%	-3.2%	0.427
	Retail: vehicles, fuel, books & others (65)	RAW	9.4%	1.8%	33.4%	0.000
		CBPS	7.8%	8.0%	-1.0%	0.783
	Tourism and hotels (66)	RAW	11.4%	27.2%	-41.0%	0.000
		CBPS	12.1%	12.0%	0.3%	0.822
	Repair of consumer goods and vehicles (67)	RAW	1.4%	3.2%	-12.3%	0.000
		CBPS	1.5%	1.5%	0.1%	0.941
	Transport and communication (72-79)	RAW	1.8%	0.5%	12.7%	0.000
		CBPS	1.8%	2.0%	-2.4%	0.644
	Finance and banking (81-83)	RAW	9.5%	4.2%	21.1%	0.000
		CBPS	9.3%	10.1%	-3.0%	0.306
	Health (95)	RAW	0.9%	0.4%	6.4%	0.000
		CBPS	1.0%	1.0%	0.0%	0.994
	Recreational, cultural & sport activities (97)	RAW	0.8%	1.5%	-6.1%	0.000
		CBPS	0.9%	0.9%	-0.4%	0.777
	Personal service (98)	RAW	1.7%	7.5%	-28.0%	0.000
		CBPS	1.8%	1.8%	0.0%	0.957
	Other services (92, 93, 96)	RAW	0.6%	0.9%	-4.0%	0.007
		CBPS	0.6%	0.6%	-0.2%	0.894
Hiring information	Seasonal apprenticeship	RAW	1.2%	4.0%	-17.9%	0.000
		CBPS	1.3%	1.2%	0.5%	0.566
Reason of hiring	Non-standard (e.g. firm restructuring)	RAW	0.7%	1.4%	-7.2%	0.000
		CBPS	0.7%	0.6%	0.9%	0.522

Hiring information: Age	19	RAW	18.0%	22.1%	-10.1%	0.000
		CBPS	18.3%	18.3%	0.0%	0.982
	20	RAW	20.6%	22.0%	-3.3%	0.029
		CBPS	20.8%	20.9%	-0.3%	0.897
	21	RAW	18.4%	18.3%	0.2%	0.879
		CBPS	18.3%	17.8%	1.3%	0.537
	22	RAW	15.8%	14.9%	2.5%	0.108
		CBPS	15.8%	16.3%	-1.3%	0.600
	23	RAW	14.5%	12.5%	5.9%	0.000
		CBPS	14.2%	14.0%	0.7%	0.762
	24	RAW	12.7%	10.3%	7.5%	0.000
		CBPS	12.5%	12.6%	-0.4%	0.862
Gender	Women	RAW	40.3%	44.8%	-9.0%	0.000
		CBPS	39.3%	39.9%	-1.1%	0.634
Hiring information: Firm position	Single	RAW	86.3%	92.1%	-18.6%	0.000
		CBPS	87.6%	88.0%	-1.5%	0.628
	Subsidiary	RAW	3.8%	2.6%	6.9%	0.000
		CBPS	3.9%	4.5%	-3.5%	0.353
	Parent-company	RAW	9.8%	5.3%	17.2%	0.000
		CBPS	8.5%	7.5%	4.1%	0.136
Hiring information	Part-time apprenticeship	RAW	17.8%	19.5%	-4.5%	0.003
		CBPS	16.3%	16.3%	0.0%	0.998
Job-to-Job transition	Not employed 30 days before the hiring	RAW	71.9%	77.2%	-12.2%	0.000
		CBPS	72.8%	73.6%	-1.7%	0.480
Hiring information: Firm size	0-5 employees	RAW	46.2%	57.8%	-23.4%	0.000
		CBPS	49.1%	49.0%	0.2%	0.925
	6-10 employees	RAW	13.1%	17.1%	-11.4%	0.000
		CBPS	13.9%	13.8%	0.3%	0.852
	11-15 employees	RAW	7.3%	8.8%	-5.5%	0.000
		CBPS	7.7%	7.8%	-0.4%	0.839
	16-20 employees	RAW	4.0%	5.2%	-5.8%	0.000
		CBPS	4.2%	4.2%	0.2%	0.915
	21-25 employees	RAW	2.8%	2.2%	4.1%	0.008
		CBPS	2.9%	2.9%	0.1%	0.959
	26-30 employees	RAW	2.0%	1.2%	6.2%	0.000
		CBPS	2.1%	2.0%	0.9%	0.718
	31-40 employees	RAW	3.3%	1.8%	9.4%	0.000
		CBPS	3.4%	3.5%	-0.5%	0.874
	41-50 employees	RAW	2.4%	1.0%	10.3%	0.000
		CBPS	2.3%	2.3%	0.1%	0.976
	51-100 employees	RAW	4.8%	2.1%	14.4%	0.000
		CBPS	4.7%	4.6%	0.6%	0.848
	101-200 employees	RAW	3.6%	1.4%	14.3%	0.000
		CBPS	3.2%	3.5%	-1.5%	0.666
	201-500 employees	RAW	2.5%	0.8%	13.5%	0.000
		CBPS	2.4%	2.4%	0.0%	0.994
	501+ employees	RAW	8.3%	0.7%	37.4%	0.000
		CBPS	4.1%	4.2%	-0.5%	0.903
Last job (vs. new job)	Different sector (Ateco81 2 digits)	RAW	37.0%	33.9%	6.3%	0.000
		CBPS	36.3%	35.3%	2.1%	0.367
Last job (vs. new job)	Same firm	RAW	7.3%	8.8%	-5.8%	0.000
		CBPS	6.9%	6.6%	1.1%	0.592
Last job (vs. new job)	Apprentice in the same firm	RAW	3.7%	6.8%	-13.7%	0.000
		CBPS	3.9%	3.9%	0.2%	0.922
Last job (vs. new job)	Apprentice in the same sector	RAW	12.4%	19.6%	-19.6%	0.000
		CBPS	13.0%	13.0%	0.2%	0.928
Last job as employee	Primary	RAW	1.0%	1.0%	-0.6%	0.703
		CBPS	1.0%	1.0%	0.0%	0.992
	Manufacturing	RAW	11.0%	12.7%	-5.1%	0.001
		CBPS	11.2%	10.7%	1.3%	0.497
	Construction	RAW	10.1%	8.6%	4.9%	0.002
		CBPS	10.6%	10.4%	0.6%	0.759
	Services (or unknown)	RAW	32.8%	26.8%	13.2%	0.000
		CBPS	31.4%	31.3%	0.1%	0.960
	Tourism	RAW	9.0%	16.4%	-22.6%	0.000
		CBPS	9.2%	8.9%	1.0%	0.521
	Unknown	RAW	2.2%	1.6%	4.0%	0.009
		CBPS	2.2%	2.3%	-0.8%	0.774

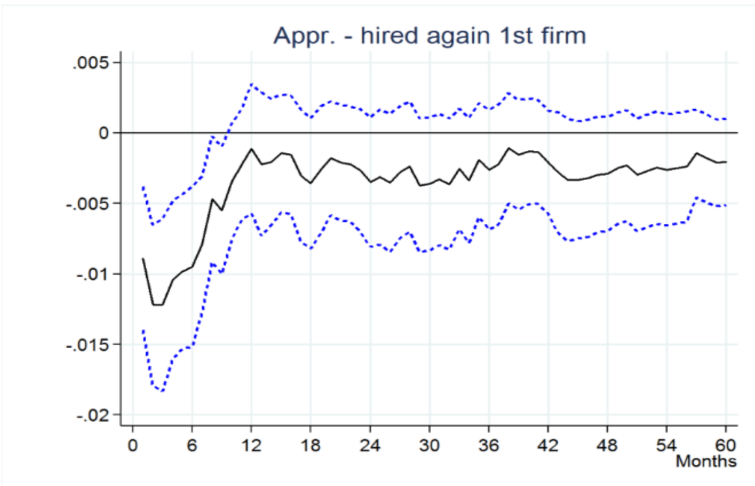
Last job as employee	% full-time	RAW	54.4%	56.8%	-5.2%	0.001
		CBPS	54.1%	52.9%	2.5%	0.277
Last job as employee	Full-time weekly remuneration (2007 €)	RAW	€ 161.5	€ 150.9	7.1%	0.000
		CBPS	€ 158.2	€ 155.8	1.7%	0.539
Non-employm. spell	Number of days last job-apprenticeship	RAW	109.6	128.0	-6.8%	0.000
		CBPS	111.8	108.0	1.4%	0.517
Last job: Contract	Apprentices	RAW	26.1%	34.4%	-18.2%	0.000
		CBPS	26.9%	26.4%	1.1%	0.562
	Open-end	RAW	9.0%	8.3%	2.4%	0.122
		CBPS	9.0%	8.8%	0.5%	0.798
	Temporary agency worker	RAW	7.5%	5.2%	9.4%	0.000
		CBPS	6.9%	6.7%	0.8%	0.800
	Seasonal	RAW	2.0%	2.5%	-3.8%	0.011
		CBPS	2.0%	1.9%	0.7%	0.696
	Fixed-term	RAW	13.1%	10.6%	7.5%	0.000
		CBPS	12.3%	12.3%	0.2%	0.950
	Collaborator	RAW	4.4%	3.2%	6.6%	0.000
		CBPS	4.4%	4.5%	-0.5%	0.858
	Other/Unknown	RAW	1.8%	1.3%	4.1%	0.008
		CBPS	1.8%	1.7%	0.6%	0.800
Last job as employee	Part-time	RAW	12.2%	13.1%	-2.8%	0.065
		CBPS	11.6%	11.6%	-0.1%	0.981
Last job: Reason end of the contract	No information (or before 2005 or collab.)	RAW	72.0%	65.7%	13.7%	0.000
		CBPS	71.7%	71.8%	-0.1%	0.969
	Dismissal	RAW	3.2%	4.3%	-5.9%	0.000
		CBPS	3.3%	3.4%	-0.7%	0.720
	Resignation	RAW	15.5%	18.9%	-9.0%	0.000
		CBPS	15.8%	15.4%	1.1%	0.585
	End of contract	RAW	8.2%	9.6%	-4.9%	0.001
		CBPS	8.1%	8.3%	-0.7%	0.762
	Others	RAW	1.1%	1.4%	-3.6%	0.018
		CBPS	1.0%	1.1%	-0.3%	0.879
Last job	Tenure (in weeks)	RAW	17.6	18.5	-2.6%	0.088
		CBPS	17.6	17.6	0.2%	0.931
Employment history : Age at first job	<15	RAW	2.4%	3.4%	-6.3%	0.000
		CBPS	2.4%	2.4%	-0.2%	0.905
	16	RAW	6.8%	8.8%	-7.5%	0.000
		CBPS	7.0%	6.8%	0.7%	0.698
	17	RAW	8.5%	10.5%	-6.9%	0.000
		CBPS	8.5%	8.2%	1.2%	0.506
	18	RAW	11.8%	14.3%	-7.4%	0.000
		CBPS	11.8%	11.3%	1.6%	0.413
	19	RAW	14.0%	12.1%	5.6%	0.000
		CBPS	13.9%	13.5%	1.1%	0.662
	20	RAW	10.2%	8.6%	5.5%	0.000
		CBPS	10.0%	10.1%	-0.3%	0.914
	21	RAW	5.3%	4.1%	5.3%	0.001
		CBPS	5.1%	5.3%	-1.0%	0.718
	22	RAW	2.8%	2.2%	4.1%	0.008
		CBPS	2.7%	2.9%	-1.4%	0.607
	23-25	RAW	38.2%	35.9%	4.8%	0.002
		CBPS	38.7%	39.6%	-1.9%	0.420
Employment history	Average Contract length (days)	RAW	133.3	133.6	-0.2%	0.913
		CBPS	132.2	132.3	0.0%	0.987
Employment history : Ever worked with specific contract	Open-term	RAW	17.9%	15.7%	6.0%	0.000
		CBPS	17.8%	17.7%	0.3%	0.910
	All temporary (include apprentices)	RAW	32.2%	26.5%	12.6%	0.000
		CBPS	30.8%	30.5%	0.6%	0.793
	Apprentice	RAW	40.9%	48.4%	-15.2%	0.000
		CBPS	41.5%	40.5%	1.9%	0.368
	Collaborator	RAW	8.5%	6.1%	9.0%	0.000
		CBPS	8.2%	8.3%	-0.6%	0.814
	Seasonal	RAW	5.8%	9.6%	-14.2%	0.000
		CBPS	5.9%	5.9%	-0.1%	0.941
Employment history as employee: %	Open-end	RAW	8.8%	7.5%	5.7%	0.000
		CBPS	8.8%	8.8%	0.1%	0.958
	Apprentice	RAW	32.6%	40.9%	-18.6%	0.000
		CBPS	33.4%	32.7%	1.5%	0.482

experience by contract	Other	RAW	19.9%	15.4%	13.2%	0.000
		CBPS	18.7%	18.4%	1.0%	0.718
Employment history as employee: % experience by collar	Blue collar	RAW	21.7%	19.3%	6.8%	0.000
		CBPS	21.6%	20.8%	2.2%	0.347
	Apprentice	RAW	32.6%	40.9%	-18.6%	0.000
		CBPS	33.4%	32.7%	1.5%	0.482
	White collar	RAW	7.0%	3.7%	16.6%	0.000
		CBPS	5.9%	6.4%	-2.2%	0.482
Employment history	Average share working time	RAW	56.2%	58.4%	-4.8%	0.002
		CBPS	55.9%	54.9%	2.3%	0.321
Employment history	Av. Full-time weekly remuneration (2007 €)	RAW	€ 161.1	€ 152.5	6.3%	0.000
		CBPS	€ 158.1	€ 155.6	1.8%	0.505
Employment history	N. of previous jobs (with collaborators)	RAW	1.8	1.8	-1.5%	0.312
		CBPS	1.8	1.8	1.4%	0.504
Employment history	Experience as collaborator (months)	RAW	0.5	0.4	7.9%	0.000
		CBPS	0.5	0.6	-1.9%	0.552
Employment history	Experience as employee (weeks)	RAW	40.8	39.9	1.4%	0.360
		CBPS	40.3	40.0	0.5%	0.827
Employment history	Experience as apprentice (weeks)	RAW	26.5	29.6	-6.1%	0.000
		CBPS	26.8	26.9	-0.2%	0.943
Employment history as employee: % Experience by firm size	Micro (0-5)	RAW	24.0%	30.3%	-15.7%	0.000
		CBPS	24.6%	24.3%	0.8%	0.688
	Small (6-25)	RAW	16.8%	18.5%	-5.1%	0.001
		CBPS	17.0%	16.8%	0.5%	0.813
	Medium (26-200)	RAW	8.4%	7.5%	3.9%	0.010
		CBPS	8.4%	8.4%	0.0%	0.998
	Large (201+)	RAW	12.2%	7.6%	17.4%	0.000
		CBPS	10.9%	10.4%	1.9%	0.506
Employment history	No experience (employee/collaborator)	RAW	36.2%	34.5%	3.6%	0.017
		CBPS	36.7%	37.7%	-2.1%	0.365
Employment history	Ever insured unemployed	RAW	1.9%	1.7%	1.6%	0.312
		CBPS	1.8%	1.9%	-0.6%	0.799
Employment history	Experience only as collaborator	RAW	2.4%	1.7%	5.1%	0.001
		CBPS	2.3%	2.3%	0.0%	0.997

Sample	Pseudo R2 (1)	Wald test (2) – p-value	Mean SB (3)	Median SB (4)	B (5)	R (6)
RAW	0.179	0.000	9.8%	6.9%	105.8*	1.89
CBPS	0.003	1.000	1.0%	0.6%	12.2	1.04

Balancing tests after reweighting by the CBPS. Covariates distribution by treatment group/regime and robust t-test of mean equality. (1) Pseudo r-squared, (2) Wald test on joint significance, (3) Mean Standardized bias, (4) Median Standardized Bias, (5) B: the absolute standardized difference of the means of the linear index of the propensity score in the treated and reweighted controls; (6) R: the ratio of treated to reweighted controls variances of the propensity score index. B should be less than 25 and R between 0.5 and 2 (Rubin, 2001). *Note that missing values are given value equal to zero* (e.g. *last remuneration for person without experience or % working time).*

Figure 1.13: ATT on a new apprenticeship contract in the same firm



ATT by CBPS of the reformed apprenticeship versus the old apprenticeship on an inflow sample of 17,914 apprentices. Outcome: employment rate in a new apprenticeship contract in the same firm. Cluster Robust Standard Errors by individual to take into account serial correlation.

Table 1.20: Summary of the impact on employment rate (ATT in pp)

	Employment in the salaried private sector (1)							Apprenticeship in the initial firm (2)						
	All (A)	Large (B)	Small (C)	With exp. (D)	No exp. (E)	Men (F)	Women (G)	All (A)	Large (B)	Small (C)	With exp. (D)	No exp. (E)	Men (F)	Women (G)
ATT Years 1-2(α)	2.6***	4.2***	0.2	2.0**	2.9**	2.4***	3.7***	5.8***	8.2***	1.7*	5.9***	5.1***	5.8***	6.2***
CI	[1.2, 4.1]	[1.8, 6.9]	[-1.3, 1.7]	[0.4, 3.7]	[0.7, 5.3]	[0.6, 4.3]	[1.5, 5.9]	[4.1, 7.6]	[4.8, 11.7]	[-0.1, 3.6]	[3.7, 8.1]	[2.4, 7.8]	[3.6, 7.9]	[3.3, 9.1]
p-value	0.000	0.001	0.810	0.017	0.013	0.009	0.001	0.000	0.000	0.074	0.000	0.000	0.000	0.000
ATT in %	3.6	5.5	0.3	2.6	4.2	3.4	5.1	11.7	15.8	3.5	12.0	9.9	11.7	12.4
Y0 (%) (β)	73.5	76.2	72.5	75.5	70.4	72.7	73.5	49.7	51.8	49.8	48.8	51.3	49.3	49.5
ATT Years 3-4(α)	2.4***	4.1**	-0.3	1.7*	2.6*	2.2*	3.5***	6.0***	10.3***	2.2***	5.9***	5.7***	5.9***	6.4***
CI	[0.6, 4.1]	[0.9, 7.3]	[-2.2, 1.6]	[-0.3, 3.8]	[-0.2, 5.4]	[0.0, 4.5]	[0.8, 6.2]	[4.6, 7.3]	[7.4, 13.2]	[0.6, 3.7]	[4.1, 7.6]	[3.6, 7.7]	[4.4, 7.5]	[4.1, 8.7]
p-value	0.009	0.012	0.772	0.098	0.074	0.055	0.010	0.000	0.000	0.006	0.000	0.000	0.000	0.000
ATT in %	3.7	6.1	-0.5	2.7	4.3	3.6	5.4	38.8	66.5	13.3	40.1	34.0	39.2	42.5
Y0 (%) (β)	63.3	67.5	61.5	65.7	59.5	60.9	65.5	15.4	15.4	16.2	14.6	16.7	15.2	15.0
ATT Year 5 (α)	2.3**	5.2***	-1.2	2.8**	1.0	1.2	4.4***	-0.5	0.6	-1.3***	-0.3	-1.1*	-0.9**	0.3
CI	[0.4, 4.2]	[1.5, 8.9]	[-3.3, 0.9]	[0.5, 5.2]	[-2.0, 4.0]	[-1.3, 3.6]	[1.4, 7.4]	[-1.1, 0.1]	[-0.7, 1.9]	[-2.2, -0.4]	[-1.1, 0.5]	[-2.3, 0.1]	[-1.7, -0.1]	[-0.8, 1.4]
p-value	0.019	0.006	0.250	0.019	0.513	0.346	0.004	0.127	0.345	0.005	0.466	0.069	0.034	0.603
ATT in %	3.8	8.2	-2.1	4.5	1.8	2.0	7.0	-13.0	20.6	-27.7	-9.5	-21.2	-20.4	10.4
Y0 (%) (β)	60.7	63.7	59.7	62.7	57.3	59.0	62.7	3.8	2.9	4.6	3.2	5.2	4.4	2.8
N individuals (γ)	17,277	5,130	11,656	10,986	6,177	9,985	7,137	17,277	5,130	11,656	10,986	6,177	9,985	7,137
N. obs. (γ)	1,074,840	314,400	704,280	693,060	381,780	622,200	452,640	1,074,840	314,400	704,280	693,060	381,780	622,200	452,640

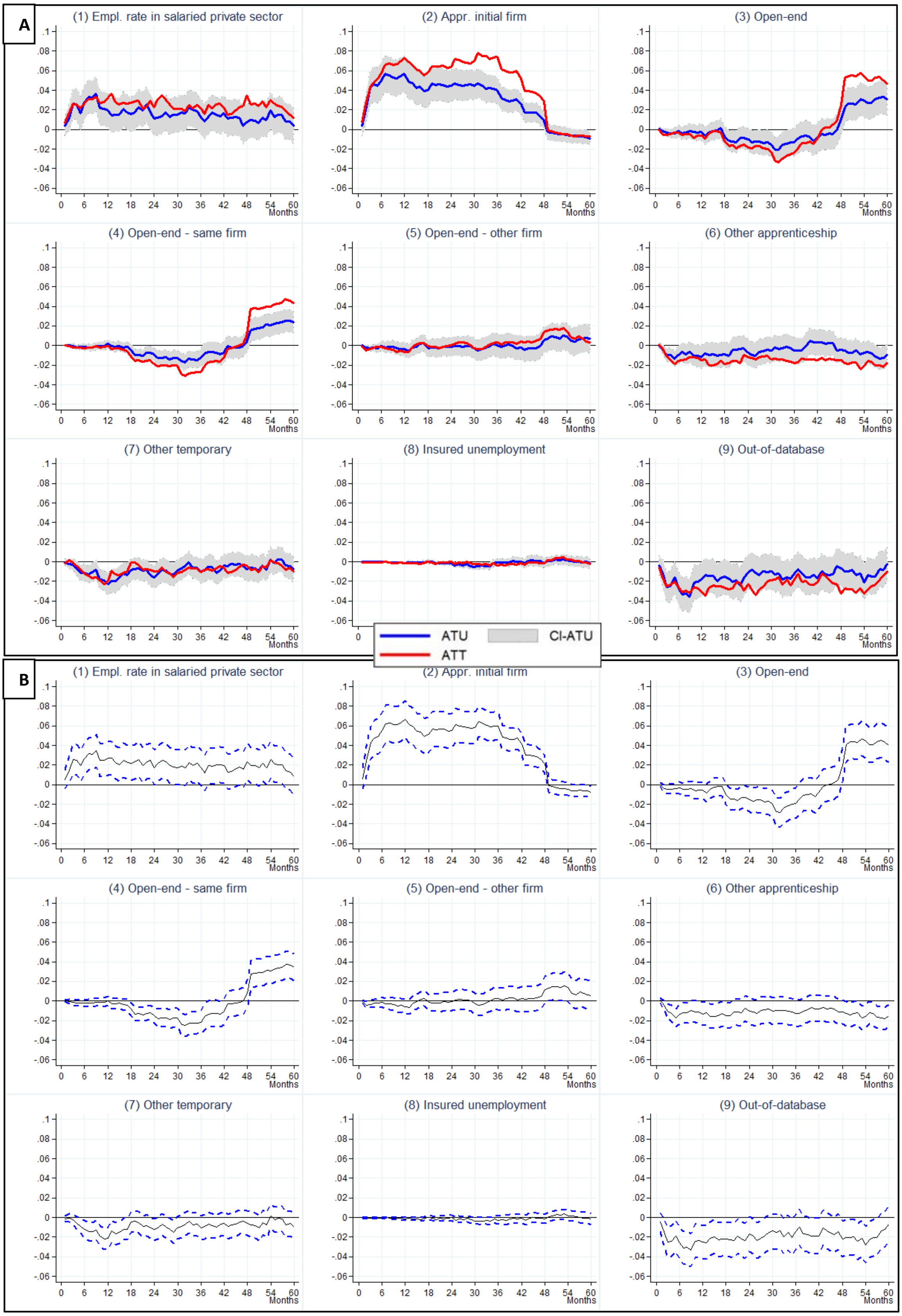
	Open-end in the same firm (3)							Open-end in other firms (4)						
	All (A)	Large (B)	Small (C)	With exp. (D)	No exp. (E)	Men (F)	Women (G)	All (A)	Large (B)	Small (C)	With exp. (D)	No exp. (E)	Men (F)	Women (G)
ATT Years 1-2(α)	-0.6***	-1.1**	-0.2	-0.7**	-0.4	-0.9***	-0.2	-0.2	-0.9	0.4	-0.1	-0.5	-0.3	-0.1
CI	[-1.1, -0.2]	[-1.9, -0.2]	[-0.6, 0.2]	[-1.4, -0.1]	[-1.0, 0.1]	[-1.5, -0.3]	[-0.8, 0.4]	[-0.8, 0.3]	[-2.0, 0.3]	[-0.3, 1.0]	[-0.8, 0.7]	[-1.3, 0.4]	[-1.0, 0.5]	[-1.0, 0.8]
p-value	0.009	0.013	0.469	0.030	0.171	0.006	0.492	0.448	0.130	0.246	0.882	0.254	0.466	0.795
ATT in %	-27.5	-41.0	-9.3	-28.8	-26.3	-35.3	-12.2	-5.2	-20.5	9.5	-1.2	-13.7	-5.9	-3.2
Y0 (%) (β)	2.2	2.6	1.7	2.6	1.5	2.6	1.7	4.1	4.3	4.0	4.5	3.6	4.6	3.5
ATT Years 3-4(α)	-1.6**	-2.4**	-1.5**	-1.1	-2.3**	-1.8**	-1.1	0.3	-0.3	0.6	0.4	0.0	-0.1	1.0
CI	[-2.9, -0.3]	[-4.8, -0.1]	[-2.8, -0.3]	[-2.8, 0.5]	[-4.2, -0.4]	[-3.5, -0.1]	[-2.8, 0.7]	[-0.9, 1.4]	[-2.8, 2.2]	[-0.6, 1.9]	[-1.1, 2.0]	[-1.6, 1.6]	[-1.4, 1.2]	[-0.9, 3.0]
p-value	0.015	0.044	0.012	0.182	0.015	0.035	0.232	0.665	0.837	0.309	0.570	0.989	0.865	0.290
ATT in %	-14.7	-19.0	-17.0	-10.2	-21.8	-16.1	-10.7	2.2	-2.2	5.7	3.6	0.1	-1.0	9.2
Y0 (%) (β)	11.1	12.8	9.1	11.0	10.8	11.4	10.0	11.4	11.9	11.2	12.1	10.3	11.4	11.3
ATT Year 5 (α)	4.1***	6.0***	1.5*	4.8***	2.8**	4.4***	4.4***	1.1	1.9	0.4	1.6	0.4	-0.1	2.7**
CI	[2.4, 5.8]	[2.9, 9.1]	[-0.2, 3.1]	[2.7, 7.0]	[0.3, 5.2]	[2.3, 6.6]	[2.1, 6.8]	[-0.4, 2.7]	[-1.5, 5.3]	[-1.4, 2.1]	[-0.5, 3.6]	[-1.9, 2.7]	[-2.0, 1.8]	[0.0, 5.4]
p-value	0.000	0.000	0.090	0.000	0.029	0.000	0.000	0.154	0.272	0.682	0.138	0.720	0.949	0.046
ATT in %	28.6	36.1	11.4	34.1	18.9	31.0	32.6	6.2	9.9	2.0	8.1	2.5	-0.3	14.4
Y0 (%) (β)	14.5	16.6	12.8	14.2	14.6	14.2	13.6	18.5	19.1	18.5	19.4	17.1	18.4	19.0
N individuals (γ)	17,277	5,130	11,656	10,986	6,177	9,985	7,137	17,277	5,130	11,656	10,986	6,177	9,985	7,137
N. obs. (γ)	1,074,840	314,400	704,280	693,060	381,780	622,200	452,640	1,074,840	314,400	704,280	693,060	381,780	622,200	452,640

	Other apprenticeship (5)							Other temporary & collaborators (6)						
	All (A)	**Large (B)**	**Small (C)**	**With exp. (D)**	**No exp. (E)**	**Men (F)**	**Women (G)**	**All (A)**	**Large (B)**	**Small (C)**	**With exp. (D)**	**No exp. (E)**	**Men (F)**	**Women (G)**
ATT Years 1-2(α)	-1.4***	-0.8	-0.9*	-1.8***	-0.6	-0.9*	-1.5	-1.0**	-1.3	-0.8*	-1.3**	-0.6	-1.3***	-0.6
CI	[-2.5, -0.4]	[-2.5, 1.0]	[-2.0, 0.1]	[-3.0, -0.5]	[-2.2, 0.9]	[-1.9, 0.1]	[-3.5, 0.6]	[-1.7, -0.2]	[-2.9, 0.4]	[-1.7, 0.0]	[-2.3, -0.3]	[-1.5, 0.3]	[-2.1, -0.4]	[-1.9, 0.6]
p-value	0.008	0.395	0.068	0.006	0.429	0.082	0.157	0.011	0.134	0.060	0.014	0.221	0.005	0.311
ATT in %	-14.7	-8.6	-10.0	-17.0	-7.6	-10.5	-13.9	-12.3	-14.7	-10.6	-14.3	-10.4	-16.5	-7.9
Y0 (%) (β)	9.7	8.8	9.5	10.4	8.4	8.7	10.7	7.8	8.6	7.6	9.1	5.7	7.6	8.1
ATT Years 3-4(α)	-1.4**	-2.5	-0.8	-2.2**	-0.3	-1.0	-1.9	-0.8	-0.9	-0.8	-1.2*	-0.5	-0.8	-1.0
CI	[-2.8, 0.0]	[-5.6, 0.6]	[-2.2, 0.6]	[-4.0, -0.4]	[-2.3, 1.8]	[-2.4, 0.5]	[-4.6, 0.9]	[-1.8, 0.2]	[-3.1, 1.2]	[-1.9, 0.3]	[-2.6, 0.2]	[-1.8, 0.9]	[-1.9, 0.3]	[-2.8, 0.9]
p-value	0.048	0.108	0.263	0.015	0.794	0.184	0.179	0.109	0.397	0.179	0.086	0.474	0.164	0.309
ATT in %	-10.2	-17.2	-5.8	-15.1	-2.2	-7.9	-11.6	-7.2	-7.4	-6.6	-9.2	-5.5	-7.5	-7.4
Y0 (%) (β)	13.9	14.6	13.6	14.6	12.8	12.3	16.1	11.6	12.7	11.4	13.3	9.0	10.7	13.0
ATT Year 5 (α)	-1.9***	-2.9**	-1.4**	-2.7***	-0.6	-1.4**	-2.9**	-0.6	-0.4	-0.4	-0.5	-0.5	-0.8	-0.1
CI	[-3.2, -0.6]	[-5.6, -0.3]	[-2.7, -0.1]	[-4.4, -1.1]	[-2.3, 1.2]	[-2.8, -0.1]	[-5.5, -0.4]	[-1.8, 0.7]	[-2.9, 2.2]	[-1.7, 1.0]	[-2.1, 1.1]	[-2.3, 1.3]	[-2.3, 0.6]	[-2.2, 2.0]
p-value	0.004	0.031	0.037	0.001	0.525	0.041	0.025	0.373	0.780	0.572	0.530	0.579	0.273	0.923
ATT in %	-17.8	-26.3	-12.7	-23.5	-6.2	-14.8	-23.7	-4.2	-2.6	-3.0	-3.6	-4.5	-6.7	-0.7
Y0 (%) (β)	10.7	11.1	10.9	11.7	9.3	9.8	12.4	13.2	13.9	12.9	14.2	11.2	12.1	14.8
N individuals (γ)	17,277	5,130	11,656	10,986	6,177	9,985	7,137	17,277	5,130	11,656	10,986	6,177	9,985	7,137
N. obs. (γ)	1,074,840	314,400	704,280	693,060	381,780	622,200	452,640	1,074,840	314,400	704,280	693,060	381,780	622,200	452,640

	Insured unemployed (7)							Out-of-database (8)						
	All (A)	**Large (B)**	**Small (C)**	**With exp. (D)**	**No exp. (E)**	**Men (F)**	**Women (G)**	**All (A)**	**Large (B)**	**Small (C)**	**With exp. (D)**	**No exp. (E)**	**Men (F)**	**Women (G)**
ATT Years 1-2(α)	-0.1	-0.3	0.0 (δ)	-0.1	-0.2 (ε)	-0.3*	0.1**	-2.5***	-3.9***	-0.2	-1.9**	-2.8**	-2.2**	-3.8***
CI	[-0.3, 0.0]	[-0.9, 0.2]	[-0.1, 0.1]	[-0.2, 0.1]	[-0.5, 0.1]	[-0.5, 0.0]	[0.0, 0.2]	[-4.0, -1.1]	[-6.6, -1.5]	[-1.7, 1.3]	[-3.6, -0.3]	[-5.1, -0.5]	[-4.1, -0.4]	[-6.0, -1.6]
p-value	0.179	0.262	0.858	0.383	0.284	0.082	0.015	0.001	0.003	0.816	0.021	0.017	0.020	0.001
ATT in %	-39.1	-56.0	-4.3	-23.1	-78.9	-62.9	105.8	-9.6	-16.9	-0.7	-8.0	-9.6	-8.1	-14.4
Y0 (%) (β)	0.3	0.5	0.1	0.3	0.2	0.4	0.1	26.3	23.2	27.3	24.2	29.4	26.9	26.4
ATT Years 3-4(α)	-0.2	-0.1	-0.2*	-0.3	-0.1	-0.2	-0.3	-2.2**	-4.0**	0.5	-1.4	-2.5*	-2.0*	-3.2**
CI	[-0.5, 0.1]	[-1.0, 0.7]	[-0.5, 0.0]	[-0.7, 0.1]	[-0.5, 0.3]	[-0.6, 0.2]	[-0.8, 0.2]	[-3.9, -0.4]	[-7.1, -0.9]	[-1.4, 2.5]	[-3.5, 0.6]	[-5.3, 0.3]	[-4.3, 0.2]	[-5.9, -0.5]
p-value	0.206	0.783	0.075	0.171	0.671	0.370	0.227	0.016	0.012	0.594	0.164	0.085	0.080	0.018
ATT in %	-14.2	-7.2	-18.5	-17.1	-10.7	-13.8	-21.7	-6.1	-12.9	1.4	-4.4	-6.2	-5.4	-9.7
Y0 (%) (β)	1.4	1.7	1.3	1.8	0.8	1.4	1.5	35.3	30.8	37.2	32.5	39.7	37.7	33.0
ATT Year 5 (α)	0.1	0.3	0.1	0.1	0.1	0.3	-0.4	-2.5**	-5.5***	1.1	-3.0**	-1.1	-1.5	-4.0***
CI	[-0.3, 0.6]	[-0.8, 1.4]	[-0.3, 0.6]	[-0.5, 0.7]	[-0.5, 0.8]	[-0.2, 0.8]	[-1.5, 0.7]	[-4.4, -0.5]	[-9.2, -1.9]	[-1.0, 3.2]	[-5.3, -0.6]	[-4.1, 1.9]	[-4.0, 1.0]	[-7.0, -1.1]
p-value	0.518	0.552	0.549	0.684	0.724	0.263	0.508	0.013	0.003	0.306	0.013	0.466	0.235	0.007
ATT in %	7.5	15.8	7.0	5.8	7.0	16.5	-15.1	-6.6	-16.2	2.9	-8.5	-2.7	-3.8	-11.6
Y0 (%) (β)	2.0	2.1	2.0	2.2	1.6	1.9	2.4	37.3	34.2	38.3	35.1	41.0	39.2	34.9
N individuals (γ)	17,277	5,130	11,656	10,986	6,177	9,985	7,137	17,277	5,130	11,656	10,986	6,177	9,985	7,137
N. obs. (γ)	1,074,840	314,400	704,280	693,060	381,780	622,200	452,640	1,074,840	314,400	704,280	693,060	381,780	622,200	452,640

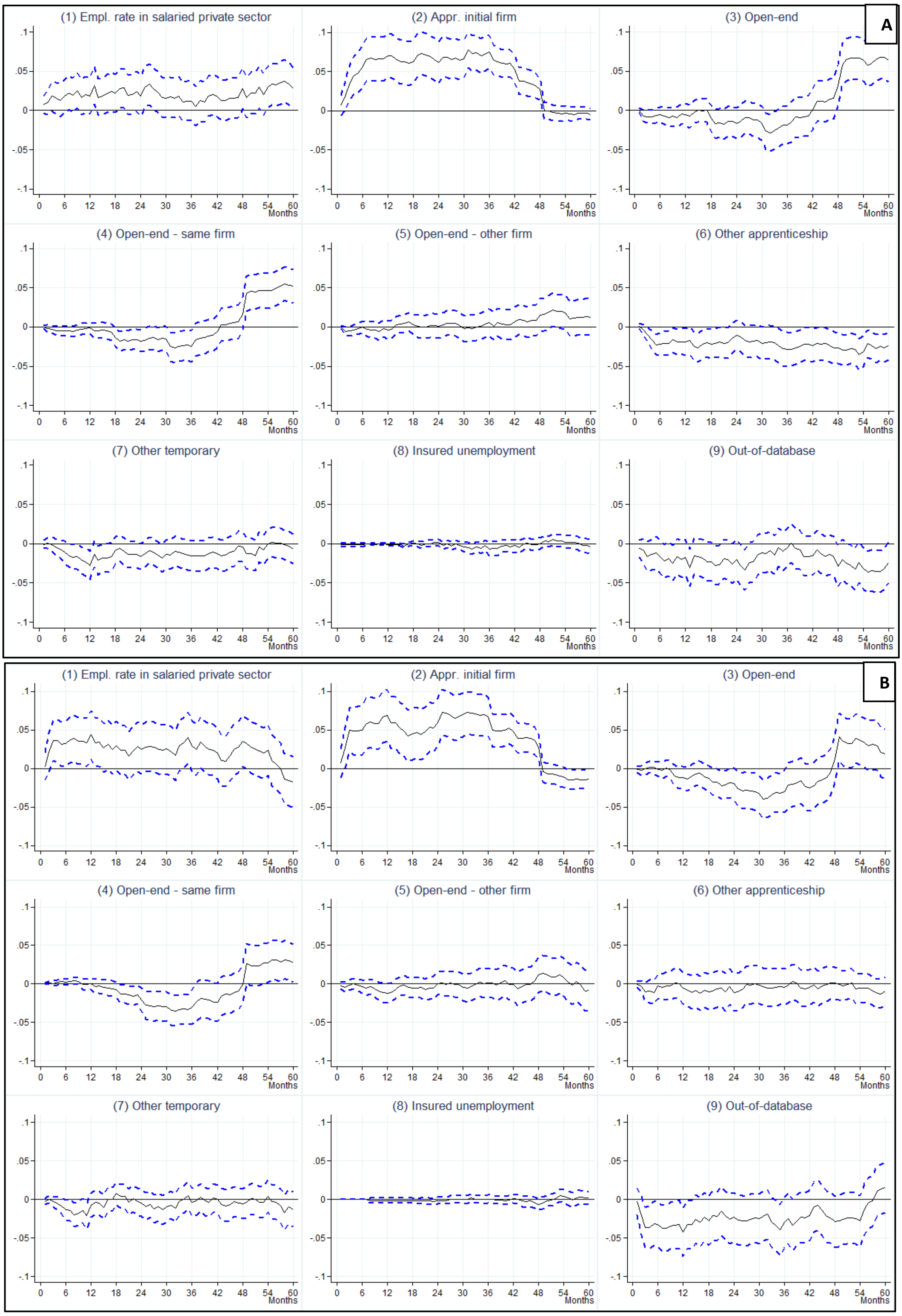
*ATT by CBPS of the reformed apprenticeship versus the old apprenticeship. α: linear combination of the ATTs in pp on all the monthly statuses in the 1^{st}-2^{nd} year, 3^{rd}-4^{th} year or 5^{th} year after hiring: (1) in salaried private sector, (2) apprenticeship in initial firm, (3) open-end contract in the same firm; (4) open-end in another firm, (5) other apprenticeship, (6) temporary or collaborator, (7) insured unemployed, (8) neither in salaried private sector employment nor in insured unemployment; β: counterfactual outcome; γ:number of individuals/observations (individuals*60 months) after trimming (99.9 percentile rule). ATT on specific sub-samples reported in each column: (A) Overall (Inflow sample of 17,914 apprentices), (B) hired in firms with at least 11 employees, (C) hired in firms with at most 10 employees, (D) with previous work experience, (E) without previous work experience, (F) Men, (G) Women. Cluster Robust Standard Errors by individual to take into account serial correlation. δ: from 6^{th} to 24^{th} month; ε: from 8^{th} to 24^{th} month (no observations before).*

Figure 1.14: ATU versus ATT (panel A) and ATE (panel B) on the apprentices in the next five years



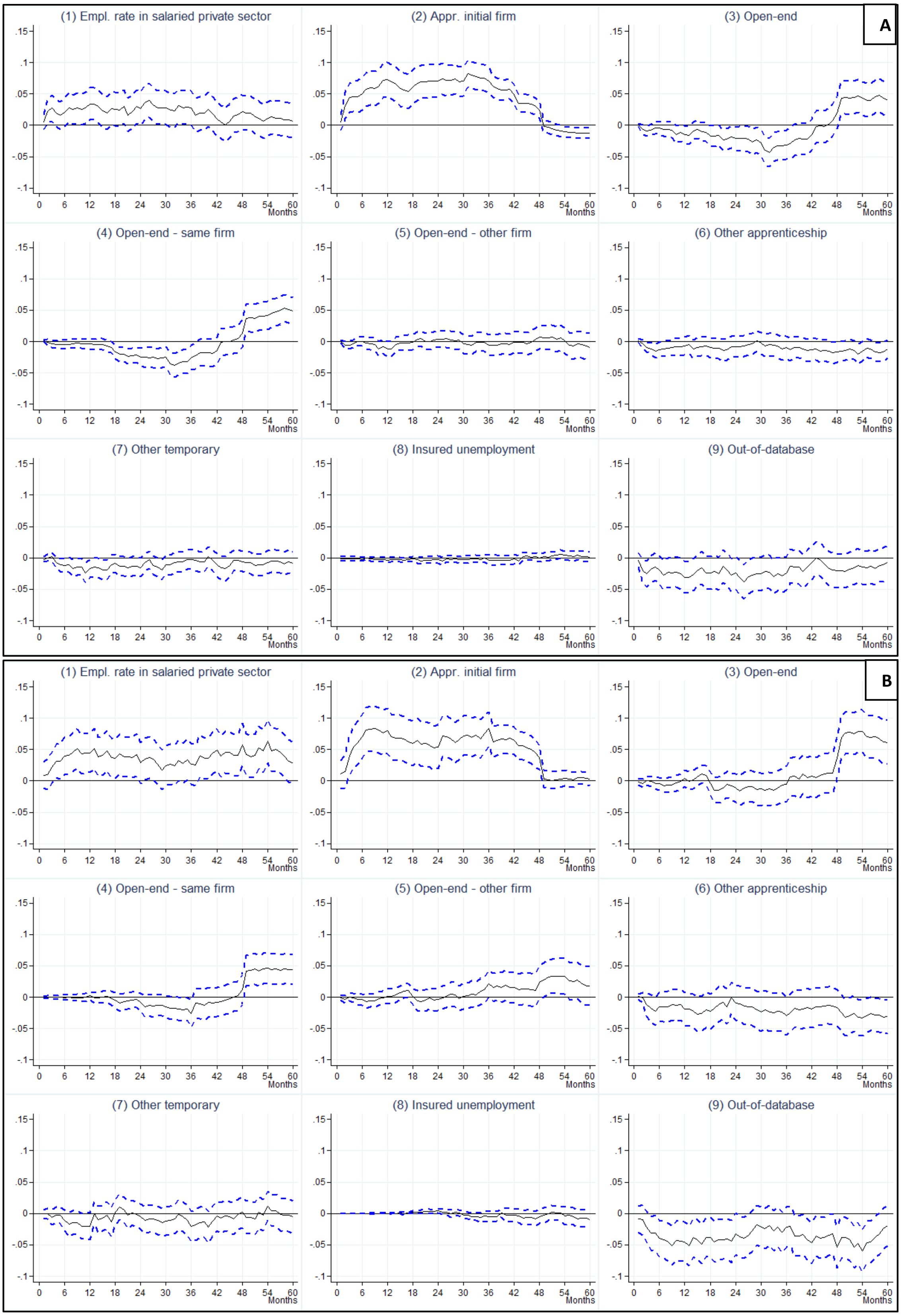
Impact by CBPS of the reformed apprenticeship versus the old apprenticeship. Panel A: ATU in blue (Confidence Intervals in grey), ATT in red. Panel B: ATE. Status at the end of each month after hiring (from left to right and top to bottom): (1) employment rate in the salaried private sector, (2) apprenticeship in the initial firm, (3) open-end contract (subdivided into the same firm (4) or another firm (5)), (6) other apprenticeship, (7) other temporary or collaborator contracts, (8) insured unemployed, (9) neither in salaried private sector employment nor in insured unemployment. Standard Errors are clustered by individual to take into account serial correlation.

Figure 1.15: ATT of the new appr. with (A) and without (B) previous work experience



ATTs by CBPS of the new apprenticeship versus the old one on an inflow sample of 17,914 apprentices. A): with previous experience, B): without experience. Status at the end of each month after hiring (from left to right and top to bottom): (1) in salaried private sector, (2) apprenticeship in initial firm, (3) open-end contract (in 4: in the same firm; 5: in another firm), (6) other apprenticeship, (7) other temporary or collaborator contracts, (8) insured unemployed, (9) neither in salaried private sector employment nor in insured unemployment. Individuals with more jobs are considered in one position in the order mentioned above. Cluster Robust Standard Errors.

Figure 1.16: ATT of the new apprenticeship (vs. the old one) - men (A) and women (B)



ATT by CBPS of the reformed apprenticeship versus the old apprenticeship on an inflow sample of 17,914 apprentices. A): men, B): women. Status at the end of each month after hiring (from left to right and top to bottom): (1) in salaried private sector, (2) apprenticeship in initial firm, (3) open-end contract (in 4: in the same firm; 5: in another firm), (6) other apprenticeship, (7) other temporary or collaborator contracts, (8) insured unemployed, (9) neither in salaried private sector employment nor in insured unemployment. Individuals with more jobs are considered only in one position by the order mentioned above. Cluster Robust Standard Errors to take into account serial correlation.

Table 1.21: ITT on transition to employment in private sector (including self-employment)

	2004q1- 2011q1 (1, A)			2004q1-2008q3 (2, A)			2004q1- 2011q1 (1, B)			2004q1-2008q3 (2, B)		
Age class	**20-24**	**25-29**	**30-34**	**20-24**	**25-29**	**30-34**	**20-24**	**25-29**	**30-34**	**20-24**	**25-29**	**30-34**
ITT – year 1	-0.1	1.1	0.3	1.4	2.3	0.1	1.2	1.0	-0.2	2.1	2.8	-0.4
p-value	0.935	0.299	0.699	0.476	0.249	0.928	0.939	0.800	0.725	0.169	0.293	0.735
ITT – year 2	-	-	-	-	-	-	-1.7	0.1	0.5	0.1	0.7	1.1
p-value	-	-	-	-	-	-	0.766	0.401	0.785	0.963	0.804	0.448
ITT – year 3	-	-	-	-	-	-	-0.8	3.2	1.2	-0.4	3.4	1.0
p-value	-	-	-	-	-	-	0.834	0.074	0.238	0.871	0.160	0.540
ITT – year 4	-	-	-	-	-	-	-0.7	1.2	0.4	7.5	2.8	-0.1
p-value	-	-	-	-	-	-	0.623	0.947	0.569	0.207	0.447	0.949
ITT – year 5	-	-	-	-	-	-	0.2	-0.6	-0.5	-	-	-
p-value	-	-	-	-	-	-	0.382	0.636	0.763	-	-	-
ITT – year 6+	-	-	-	-	-	-	-2.7	5.2	0.1*	-	-	-
p-value	-	-	-	-	-	-	0.740	0.947	0.057	-	-	-
N	52,202	60,841	71,520	35,518	41,667	48,485	52,202	60,841	71,520	35,518	41,667	48,485

*ITT on the transition to employment (including self-employment and excluding employment in the public administration) from non-employment in pp by age class. DiD on parallel path by OLS regression controlling for individual characteristics (education and gender), time dummies, regional fixed-effect and anticipation dummies (for the two quarters before the treatment). Specification either with a unique treatment dummy for all the quarters after the treatment (panel A) or divided per each year after the implementation of the Regional Law (Panel B). Sample: population non-employed one year before t (excluding students in t-1 year and t). P-value obtained by wild cluster bootstrap-t (2000 repetitions) with 17 regions as clusters. ***: significant at 1%, **: significant at 5%, *: significant at 10%.*

Table 1.22: ITT on transition from non-employment to self-employment – parallel path (OLS)

	2004q1- 2011q1 (1, A)			2004q1-2008q3 (2, A)			2004q1- 2011q1 (1, B)			2004q1-2008q3 (2, B)		
Age class	**20-24**	**25-29**	**30-34**	**20-24**	**25-29**	**30-34**	**20-24**	**25-29**	**30-34**	**20-24**	**25-29**	**30-34**
ITT – year 1	-0.4	-0.6	-0.3	0.4	-0.7	-0.5	0.1	-0.1	-0.6	0.6	-0.3	-0.8
p-value	0.666	0.199	0.486	0.505	0.251	0.436	0.887	0.784	0.276	0.322	0.502	0.312
ITT – year 2	-	-	-	-	-	-	-0.4	-0.9	-0.2	0.5	-1.2	-0.2
p-value	-	-	-	-	-	-	0.612	0.160	0.632	0.634	0.122	0.668
ITT – year 3	-	-	-	-	-	-	-0.7	-0.5	0.3	-0.9	-1.3	0.1
p-value	-	-	-	-	-	-	0.098	0.540	0.571	0.255	0.403	0.912
ITT – year 4	-	-	-	-	-	-	-0.1	-1.8*	-0.4	3.1*	-1.4	0.2
p-value	-	-	-	-	-	-	0.781	0.061	0.497	0.094	0.453	0.902
ITT – year 5	-	-	-	-	-	-	-1.0	-1.0	0.6	-	-	-
p-value	-	-	-	-	-	-	0.203	0.251	0.454	-	-	-
ITT – year 6+	-	-	-	-	-	-	-0.6	-0.5	-1.0	-	-	-
p-value	-	-	-	-	-	-	0.729	0.533	0.508	-	-	-
N	52,202	60,841	71,520	35,518	41,667	48,485	52,202	60,841	71,520	35,518	41,667	48,485

*ITT on the transition to self-employment from non-employment in pp by age class. DiD on parallel path by OLS regression controlling for individual characteristics (education and gender), time dummies, regional fixed-effect and anticipation dummies (for the two quarters before the treatment). Specification either with a unique treatment dummy for all the quarters after the treatment (panel A) or divided per each year after the implementation of the Regional Law (Panel B). Sample: population non-employed one year before t (excluding students in t-1 year and t). P-value obtained by wild cluster bootstrap-t (2000 repetitions) with 17 regions as clusters. ***: significant at 1%, **: significant at 5%, *: significant at 10%.*

Table 1.23: Continuous treatment - ITT on employment rate & transition from non-employment

	Employment rate in salaried private sector (1)						Transition to employment in salaried private sector(2)					
	2004q1- 2011q1 (A)			2004q1-2008q3 (B)			2004q1- 2011q1 (A)			2004q1-2008q3 (B)		
Age class	**20-24**	**25-29**	**30-34**	**20-24**	**25-29**	**30-34**	**20-24**	**25-29**	**30-34**	**20-24**	**25-29**	**30-34**
FULL INTENSITY - α	-2.6	-0.3	-0.1	-1.7	-0.7	-0.6	-0.4	3.1*	0.6	-0.1	4.6**	0.3
p-value	0.174	0.516	0.926	0.182	0.489	0.491	0.906	0.051	0.500	0.955	0.043	0.873
FULL INTENSITY - β	-2.3	-0.2	-0.5	-1.9	-0.6	-1.0	0.0	3.5*	0.5	-0.6	4.8*	-0.2
p-value	0.355	0.641	0.583	0.143	0.527	0.282	1.000	0.075	0.658	0.795	0.051	0.927
N	128,821	193,073	258,357	89,375	134,849	178,113	52,202	52,202	71,520	35,518	41,667	48,485

*ITT in pp on the employment rate in the salaried private sector (1) and employment transition (2). The reported coefficient is the effect of being eligible for the full treatment with respect to no treatment. The treatment dosage is equal to one if the region had implemented the Regional Law by t, or equal to the sum of the 2004 share of the youth (specification α) or of the full population (specification β) working in the sectors which passed the CBAs by t. DiD controlling for individual characteristics (education and gender), time, regional fixed-effect and 17 regional trends. Sample: Columns 1: full population. Columns 2: population non-employed one year before t (excluding students in t-1 year and t). P-value obtained by wild cluster bootstrap-t (2000 repetitions) with 17 regions as clusters. ***: significant at 1%, **: significant at 5%, *: significant at 10%.*

2

Permanent Wage Cost Subsidies for Older Workers. An Effective Tool for Increasing Working Time and Postponing Early Retirement?

2.1. Introduction

The combination of the decreasing share of the working population induced by the ageing of the population and the longer life expectancy will be one of the major threats to economic growth and the Welfare State in the developed world in the coming decades. Partly in reaction to this threat, the European Union (EU) agreed in June 2010 on the "Europe 2020" strategy. One of the major targets is to raise the employment rate from 69% in 2010 to 75% by 2020, and this, in particular, through a greater involvement of older workers in the work force (European Commission, 2010). Belgium has one of the lowest employment rates of older workers in the EU. In 2013 the employment rate among the population aged between 55 and 64 attained only 41.7% (50.3% in EU27). Nevertheless, much progress has been made. Since 2000 the Belgian Federal government has implemented a series of policies to encourage the labour market participation of the elderly (Dejemeppe *et al.*, 2015) and the employment rate among older workers has increased by 16.7 percentage points (Eurostat, 2015). The question is to what extent this positive trend has been driven by the policies put in place, and if so, which interventions have been the most effective. This paper contributes to a better understanding of this question by evaluating the impact on employment and wages of a wage cost subsidy targeted at older workers. Other countries have introduced similar wage subsidy schemes, widening thereby the scope of interest for our findings.[1]

In Belgium from the second quarter of 2002 onwards, the private sector employers' Social Security contributions (SSC) were *automatically* and *permanently* reduced by €400/quarter for employees older than 58 and working 80% of a full-time employee or more. For part-time employees working between 33% and 80% of full-time working-time, the subsidy was proportionally reduced. Employees working less than the lower threshold were not eligible. The average subsidy was worth 4% of the median wage cost, including all payroll taxes. This share increased with the wage, reaching a maximum intensity of 13.8% for someone working 80% at the legal minimum wage. The policy is still in place and its coverage has even been extended in 2004 and 2007.[2] The cost of the programme is substantial: in 2004 expenditures on the payroll subsidy represented 0.034% of the GDP and between 2004 and 2011 in total about 1.3 billion euros of payroll reductions were spent.

To evaluate the policy we rely on an endogenously stratified random sample of 243,655 Belgians aged between 52 and 61 in 2002, the year that the subsidy was introduced. At these ages many individuals

[1] Examples in OECD countries are: Austria, Denmark, Finland, France, Germany, the U.S., the Netherlands, Sweden and the United Kingdom. A description of such measures introduced in the OECD countries is provided in Tables B.1-B.3 of the Supplementary Appendix B.

[2] In 2004 the age requirement was lowered to 57 and in 2007 the subsidy amount was increased, awarding a reduction already from age 50 (i.e. €50/quarter for each year above 49) if the worker has a quarterly remuneration lower than €12,000.

(especially women) are not in the labour force in Belgium, and, if they are, their labour market mobility is low. These inactive individuals are not very helpful for the identification of the effects of the aforementioned policy reform on the outcomes of interest, i.e. on salaried employment and wages. However, ignoring them is not admissible, since this would introduce sample selection bias. Since privacy protection legislation forbids using population data, we therefore aimed at maximizing the informational content of our sample without introducing this bias. We oversample aforementioned older individuals working in the salaried private sector throughout the period of analysis, or making transitions in or out of this sector. Because in this way the sample becomes endogenously related to the outcomes of interest, we appropriately weigh the data to ensure the treatment effects are consistently estimated (Manski and Lerman, 1977; Cameron and Trivedi, 2005).

The data were sampled from merged administrative registers of the diverse Social Security institutions and of the National Register containing all Belgian inhabitants. These data are exceptionally rich in that they contain detailed information on labour market histories and the amount of SSC paid since 1998, and on private sector employment from as early as 1957. Using these data we assess the effects of the SSC reduction on the employment rate, working time and hourly wages. We base this assessment on a *Conditional* Difference-in-Differences (CDiD) estimator (Heckman *et al.*, 1997). This estimator identifies the effects by contrasting the evolution in the outcomes between older and younger groups, conditional on a set of observed covariates including the labour market history of the individuals.

Our contribution to the literature is threefold. First, only few researchers have evaluated the impact of wage subsidies targeted at older workers. Ammermüller *et al.* (2006) and Brussig *et al.* (2006) have studied the impact of in-work benefits (subsidising the net wage of workers), Boockmann *et al.* (2012) of hiring subsidies (targeted to newly created jobs) and, more recently, Huttunen *et al.* (2013) of *temporary* wage cost subsidies for *low-wage* older workers. However, we are not aware of any study that evaluates the effects of a *permanent* wage cost subsidy targeted at all older workers, irrespectively of their wage, and precisely at the age that they become entitled to early retirement (58 in Belgium). The latter is of particular interest since it means that the subsidy targets a population that is at the margin of leaving the labour force and for which the potential for job retention is higher than at other ages, especially in countries, such as Belgium, where early retirement schemes facilitate labour force exits of older workers. We find indeed that the subsidy is more effective for workers at risk of leaving to early retirement.

While most of the literature on the impact of early retirement schemes on the employment of older workers focused on supply side incentives (see e.g. Burtless, 1986; Krueger and Pischke, 1992; Gruber and Wise, 2007; Liebman *et al.*, 2009; Staubli and Zweimüller, 2013), this paper provides further evidence that also incentives at the demand side matter if there is a pay-productivity gap for older workers.[3] The literature focusing on labour demand is scarce. Hakola and Uusitalo (2005) show that a reform on early retirement schemes in Finland, which charged part of the retirement expenses to the employer, decreased the workers exit to early retirement. Frimmel *et al.* (2015) recently provide evidence on the importance of pay-productivity profile in affecting the retirement age of workers in Austria. The pay-productivity gap may be the consequence of a declining productivity profile with age, as induced by rapid technological change, or of deferred compensation schemes (Lazear, 1979). Anti-discriminatory legislation, the threat of labour dispute with trade unions, and reputational concerns in the case of deferred compensation schemes, refrain employers from dismissing overpaid older workers, even if the costs of retaining them exceed the firing costs. This results in excess employment ("*overmanning*") at the going wage rate. Since employment already exceeds labour demand, a wage cost subsidy then merely reduces this gap and does not affect the level of employment. This is in line with the existing empirical evidence of Boockmann *et al.* (2012) and Huttunen *et al.* (2013). In presence of early retirement the predictions are, however, different. Early retirement schemes can be seen as an agreement between employers and trade unions to make dismissals of older workers acceptable.[4] Consequently, they reduce the level of excess employment. However, a wage cost subsidy reduces the attractiveness for employers to dismiss workers through early retirement, and, hence, positively affects employment through a higher retention rate.

Second, since the eligibility to early retirement at age 58 induces a significant drop in the employment rate, especially for males in the manufacturing sector, a panel DiD estimator in which the same individuals are followed over time is severely downward biased: it cannot distinguish between the effect of eligibility to the subsidy and the early retirement scheme, both starting at age 58. To eliminate this bias we fix the age and use a CDiD estimator in repeated cross sections, which we extend to a multi-period framework. Following Lechner and Wunsch (2009), we explicitly control for the compositional biases that an analysis on different cross-sectional units induces. This estimator identifies the treatment effect of the subsidy under the assumption that in the absence of the policy the impact of the early retirement scheme is constant over time. A placebo test in the pre-treatment period does not reject this assumption for working time and hourly wage as outcome variables, but for the employment rate it is rejected. This rejection is caused by a differential time trend already present

[3] (Hellerstein et al., 1999) are the first researchers to investigate this question. In this analysis they do not find evidence for a pay productivity gap in the U.S., but a recent replication of this seminal analysis reports that pay exceeds productivity for workers older than 55 in the manufacturing sector (Hellerstein and Neumark, 2007). Aubert and Crépon (2003, 2006) establish similar results for this age group in France, Ilmakunnas and Maliranta (2005) for Finland, and Cataldi *et al.* (2012) and Vandenberghe *et al.* (2013) for Belgium. By contrast, no pay-productivity gap is found in Portugal and in the Netherlands (Cardoso et al., 2011; van Ours and Stoeldraijer, 2011).

[4] This is considered as the main justification of the emergence of the early retirement scheme in Belgium (Claes, 2012).

in the pre-treatment period between the treated and control groups. We therefore implement in this case a parametric trend-adjusted version of this CDiD estimator (Wolfers, 2006).

Third, we integrate endogenous sampling weights in the CDiD estimator as to control for the aforementioned endogenous sampling. Frölich (2007) shows in a standard Kernel matching framework that endogenous sampling, contrary to choice-based sampling (i.e. sampling based on the treatment status), requires an appropriate re-weighting of the data both in the estimation of the propensity score and the treatment effect. We apply this re-weighting within our Weighted Difference-in-Differences (WDiD) estimator which is inspired by the estimator of Abadie (2005), who implements the CDiD estimator by Inverse Probability Weighting (IPW) (see Horvitz and Thompson, 1952 and Hirano et al., 2003). Within this approach it is simple to take the endogenous sampling into account, since it consists in a double re-weighting of the data, once to take the endogenous sampling into account and once to make the comparison groups comparable to the post-treatment treated group.

Our findings can be summarized as follows. In line with our theoretical expectations, the subsidy has a significant positive effect on the extensive margin in terms of employment retention only for workers at high risk of entering early retirement (i.e. on employees working in sectors where early retirement schemes are widely used). Decreasing labour costs for this group by 10% increases the employment probability by 9%, implying a employment elasticity close to one. We cannot exclude, however, that this positive effect is partly driven by the substitution of older for younger workers. At the intensive margin, the subsidy scheme significantly increases working time. However, the elasticities of the wage cost subsidy at the intensive margin are small: 0.13 for men and 0.28 for women. Finally, the impact of the wage cost reduction on the hourly gross wage (excluding employer's SSC) is small and not statistically different from zero.

The paper is structured as follows. Section 2.2 summarizes the institutional setting. Section 2.3 reviews the theoretical and empirical literature on wage subsidies. Section 2.4 formulates theoretical predictions for the wage cost subsidy studied in this research. The sampling scheme and data are described in Section 2.5. Section 2.6 presents the identification strategy and the estimation method. In Section 2.7 we present our empirical findings and a cost benefit analysis. The last section concludes.

2.2. The Institutional Setting in the Period around the Reform in 2002

In Belgium wage negotiations between employer's organisations and trade unions take place in sectoral joint industrial committees ("*commissions paritaires*"), usually separately for blue and white-collar workers. In case of dismissal and after a notice period, workers who have contributed sufficiently long to Unemployment Insurance (UI) are entitled to Unemployment Benefits (UB). The replacement rate at the start is 55% or 60% depending on the household situation, but there are caps and floors, so that it is higher (lower) for low (high) wage workers. UI in Belgium is singular in that there is no time limit to the entitlement, although the benefit level decreases with unemployment duration for individuals who are not head of household. Moreover, until June 2002 individuals aged 50 or more who were unemployed for more than one year were not required to be available for the labour market, and, in case they had accumulated more than 20 years of working experience, these long-term unemployed were entitled to a seniority supplement to their UB. Between 2002 and 2004 the age of labour market availability was gradually increased to 57, but this availability was hardly verified at that time. Periods of unemployment are assimilated to periods of employment for the calculation of statutory pension rights. All in all, UI for workers above age 50 can in the facts be viewed as an early retirement scheme, but more generous schemes exist.

In Belgium there are essentially three early retirement schemes: early retirement within the statutory regime, the conventional pre-retirement scheme and the, so-called, "Canada Dry" system.[5] Early retirement within the statutory regime starts from age 60 after minimum 30 years of employment experience. However, due to relatively low generosity of this scheme in the private sector,[6] take-up in the two alternative regimes was much more important. For workers having more than 20 years of employment experience, the conventional pre-retirement regime (also known as the "bridge pension") is available from age 60 for all workers and from 58 in case of an agreement in the sectoral joint industrial committee, generally concluded in the manufacturing sector. In restructuring firms and for difficult physical professions, the age condition could drop to 50, 52 or 55, depending on the sectoral agreement. Because of a supplement equal to half of the difference between the UB and the wage and the favourable treatment for the statutory pension, the bridge pension is attractive for employees. The scheme imposes, however, a number of requirements on firms. They must pay the aforementioned supplement to UB and respect the legal notice period or, alternatively, pay severance allowances. Furthermore, supplementary SSC are due and there is an obligation to replace the employee by an unemployed worker during a period of three years, although exemptions to this obligation are awarded in 57% of the cases (OECD, 2003, p. 75). Despite these requirements, employers' interest in the scheme lies in that it provides a solution to the pay-productivity gap for older workers induced by a declining productivity profile with age and, for white-collar employees, by deferred payment compensation schemes. Descriptive evidence of Vandenberghe *et al.* (2013, fig. 1) indeed suggests that in Belgium the average pay-productivity gap becomes positive from about age 56

---

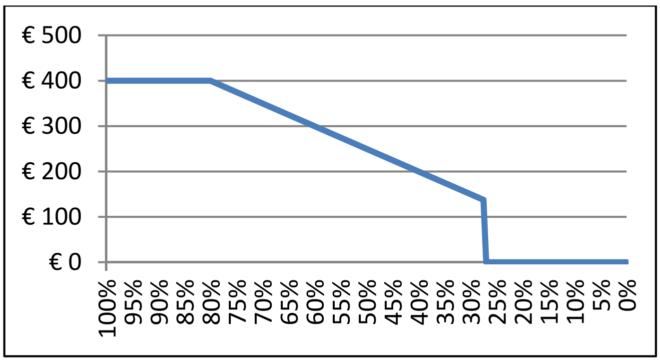
[5] "Canada Dry" refers to publicity for the drink Canada Dry: "It has the colour of Whisky, but it is not Whisky".
[6] For public sector employees the scheme is much more generous and, hence, more widely used.

onwards.[7] Both anti-discriminatory legislation and the threat of labour dispute with trade unions refrain employers from dismissing these older workers, but by offering a generous early retirement employers can nevertheless do so while maintaining social peace with trade unions.

The “Canada Dry” is an unofficial early retirement scheme in which the employer pays, as in the conventional pre-retirement regime, a supplement to UB (half of the difference between the UB and the wage). This scheme is more flexible for the employer, since it does not impose an age limit, supplementary SSC, or replacement by a younger worker. Since there is no obligation for the worker to report the supplement to the UB she obtains, no official figures on the use of the Canada Dry scheme are available. The fact that the number of UB recipients older than 50 more than doubled between 1996 and 2001 (OECD, 2003, p. 80) while the number of individuals in the conventional pre-retirement scheme decreased, suggests that the scheme became more popular in that period.

In Belgium workers above the age of 50 who have more than 20 years of employment experience, among which 5 years full-time work within the same firm, are encouraged to reduce working time either by 20% or by 50%. This *time credit scheme* in the private sector[8] is a right for the employee in firms employing 10 or more workers and in which no more than 5% of the workforce is already benefiting from the scheme. Since the employee is entitled to a monthly flat rate compensation until the age of statutory retirement, it is like a part-time retirement scheme (Ibid, p. 56).

Figure 2.1: The Quarterly Value of the Wage Cost Subsidy as a Function of Working Time



Fraction of full-time working-time

The permanent reduction of SSC for employers by €400 per quarter for private sector employees aged 58 or more, as well as the temporary hiring subsidy (“ACTIVA”) awarded for maximum five years to employers recruiting workers unemployed for more than six months and aged 45 or more, both introduced in 2002,[9] can be seen among the first attempts in Belgium to increase the employment rate of older workers rather than inducing withdrawal from the labour force. We focus our discussion on

[7] This descriptive evidence is in line with the findings of their subsequent analysis that takes the endogenous composition of the older workforce into account, but does not provide exact information by age.
[8] In the public sector a closely related “career break” scheme exists.
[9] The ACTIVA replaced an existing similar scheme, so that it was not really a new measure.

the first mentioned wage subsidy, because this is the object of analysis here. Figure 2.1 displays how the subsidy increases with working time. The subsidy favours part-time employment, since, between 33% and 80% of full-time working-time, it increases at a rate that is 1.25 times the share of working time relative to a full-time employee. Hence, it attains the maximum of €400 when working at least 80% (0.80*1.25*400=400). At 33% the value of the subsidy is €165 (0.33*1.25*400=165). Below 33% the SSC reduction abruptly drops to zero. The average subsidy is worth 4% of the median wage cost, including all payroll taxes. This share decreases with the wage and is maximally equal to 14% for someone working 80% of full-time working-time at the minimum wage.

An essential feature of the subsidy is that it was awarded *automatically* to firms. This ensured a 100% take-up rate. This contrasts with other countries, such as the wage cost subsidy to low-wage older workers in Finland, for which employers had to apply and for which the take-up rate was much lower, i.e. about 60% (Huttunen *et al.*, 2013). Similarly, in Belgium the take-up rate dropped to 89% for men and 70% for women when in 2004 the coverage of the subsidy was widened to those aged 57 or more, but no longer awarded automatically.

2.3. Literature Review

2.3.1. Theory

In a competitive labour market wage subsidies may increase employment if both the supply and the demand for labour are sufficiently elastic, and it does not matter whether these subsidies are paid to the employer or the employee: *neutrality of the tax incidence* (Marshall, 1920; Kaldor, 1936; Katz, 1996). In a partial equilibrium framework in which E denotes employment, W wage cost, s the wage subsidy (at the employer- or employee-side), ϵ_D the absolute value of the uncompensated wage elasticity of labour demand and ϵ_S the uncompensated wage elasticity of labour supply, the standard textbook formulas are given by:

$$\frac{d\ ln\ E}{d\ ln\ s} = \frac{1}{\frac{1}{\epsilon_D}+\frac{1}{\epsilon_S}} \text{ and } \frac{d\ ln\ W}{d\ ln\ s} = \frac{1}{1+\frac{\epsilon_S}{\epsilon_D}} \quad (1)$$

Pissarides (1998) demonstrates that also in non-competitive labour markets employment tax cuts may have substantial positive effects on employment to the extent that the income out of work is fixed in real terms and not indexed to the wage. Moreover he shows that targeting subsidies to low-wage workers may enhance the positive employment effects, since the tapering-off of the wage subsidy with the wage acts like a progressive tax making it more costly to capture part of the subsidy in a higher after tax wage.[10]

2.3.2. Empirical Studies

Based on more than 70 studies, Hamermesh (1996) suggests that the conditional elasticity of labour demand lies in the interval [-0.15, -0.75], with -0.30 being the best available point estimate. More recently, Lichter *et al.* (2014) report, based on a meta-regression analysis, -0.246 as the preferred point estimate, bracketed by the interval [-0.072;-0.446]. These elasticities exhibit substantial heterogeneity. For instance, the demand elasticity decreases with skill level and is higher for atypical employment. We are not aware of any study, however, that aims at specifically estimating the elasticity of labour demand for older workers.

Because older workers are wealthier than younger ones, one expects, through the negative income effect, the labour supply of older workers to be less elastic (e.g. Zabalza *et al.*, 1980; Hanoch and Honig, 1983). Another strand of the literature reports, however, that labour supply is more elastic for older workers, both at the intensive (Friedberg, 2000; Keane, 2012) and the extensive margin (Mitchell and Fields, 1984; Blau and Riphahn, 1999; French, 2005; Keane and Rogerson, 2012). At the intensive margin the institutional setting, such as part-time retirement schemes, may enhance working time flexibility, while at the extensive margin the presence of early retirement schemes could make the decision to remain at work more sensitive to the wage. However, the elasticity at the extensive margin is asymmetric: elastic for withdrawal from the labour force, but inelastic for entry (Blau, 1994).

In view of the incomplete and partly mixed evidence on the demand for and the supply of older workers, it is difficult to predict the impact of wage cost subsidies for older workers. Another strand of the literature aims at directly estimating the impacts of wage subsidies. There exists a large literature on the effect of *in-work benefit*s (employee-side wage subsidies), especially on the Earned Income Tax Credit in the U.S (Eissa and Liebman, 1996; Meyer and Rosenbaum, 2001; Eissa and Hoynes, 2004), that has found substantial positive employment effects, but we are only aware of two studies that evaluate the effect of subsidies targeted at older workers. Ammermüller *et al.* (2006) and Brussig *et al.* (2006) estimate the effect of an *in-work benefit* in Germany. Their DiD estimator shows a positive, but not significant, employment effect, which is higher in West Germany. The lack of significance is potentially due to the low take-up rate.

Overall reductions in employers' SSC are usually found to be absorbed by a higher hourly wage and, hence, effects on employment are insignificant (Bohm and Lind, 1993; Gruber, 1997; Bennmarker *et*

[10] See also Hersoug (1984), Pissarides (1985), Lockwood and Manning (1993).

al., 2009). However, in line with the theoretical predictions of Pissarides (1985, 1998), there is empirical evidence that payroll taxes targeted on low-wage workers do boost employment (Kramarz and Philippon, 2001; Crépon and Desplatz, 2003; Goos and Konings, 2007). Nevertheless, this positive finding disappears if the reduction of payroll taxes is targeted on low wage *older* workers. Huttunen *et al.* (2013) estimate by a triple DiD the impact of a temporary five-year reduction of payroll taxes in Finland for low-wage full-time employees aged over 54. Despite the precision of their estimates, they find small and statistically insignificant employment effects at both the extensive and the intensive margin, and no robust effects on wages. Only for the group aged over 58 some significantly positive effects on the intensive margin are reported. This could be related to the part-time retirement scheme to which workers of this age group were eligible at that age, since this may increase the elasticity of supply at the intensive margin. The shortcomings of the study are the absence of placebo tests that, as we see in our analysis, may be difficultly satisfied when comparing younger and older cohorts.

Finally, Boockmann *et al.* (2012) study the effects of hiring subsidy on older workers in Germany by DiD in duration analysis. Women in East Germany are the only group for whom significant employment effects are found. This might be due to the higher generosity of the subsidies and to the lower pressure on wages given the higher unemployment rate in this region.

2.4. Theoretical Predictions

From the overview of the literature we conclude that the few studies evaluating the effect of wage subsidies targeted to older workers find very small employment effects, if any. We argue that this could be a consequence of a pay-productivity gap for older workers. Declining productivity with age, possibly induced by rapid technological progress, especially in the manufacturing sector, and deferred compensation schemes make average pay exceed average productivity of older workers. Vandenberghe *et al.* (2013, fig. 1) find that a positive pay-productivity gap emerges in Belgium on average from age 56 onwards, and at 58 (the age eligibility for the wage cost subsidy) wage costs exceed productivity by 12% on average.[11] This has consequences for the effectiveness of a wage cost subsidy, but they depend on whether targeted workers are at risk or not of leaving the labour force through early retirement. To support the basic intuition of our reasoning, we introduce a graphical exposition within a simple demand and supply partial equilibrium framework in which we assume that labour market imperfections induce wage rigidity and wages to exceed marginal productivity. Dynamic mechanisms such as substitution and anticipation effects are discussed in Section 2.4.4.

---

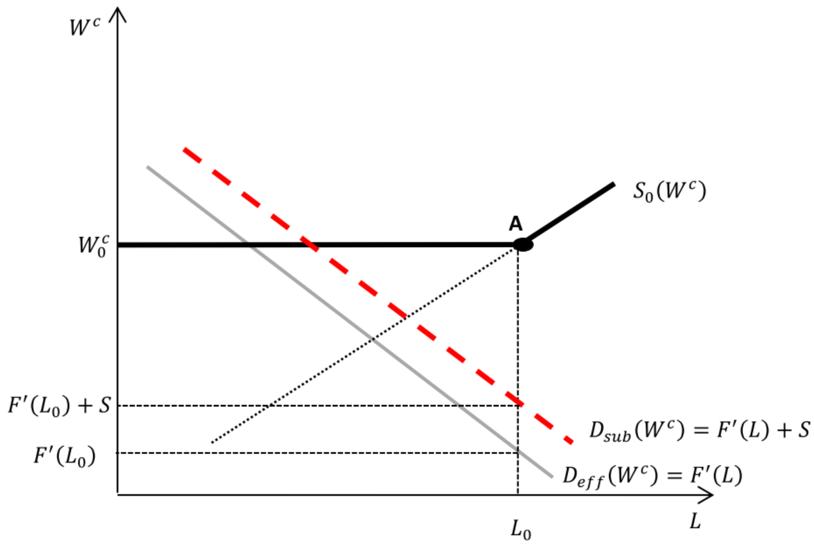
[11] We thank Vincent Vandenberghe for providing the information to calculate these figures. These are descriptive figures, but the causal evidence reported in their paper is consistent with the descriptive evidence.

2.4.1. *The Effects in the Absence of Early Retirement*

As a consequence of the deferred compensation system (Lazear, 1979) and/or wage rigidity combined with a declining productivity profile at older age, the average pay (W_0^c) for older workers is set above marginal productivity. Despite pay exceeding productivity, several labour market imperfections refrain employers from dismissing the unproductive older workers or decreasing their pay. In particular, (i) by the Employment Protection Legislation (EPL) employers are required to pay severance payments; (ii) in case of deferred compensation, breaching the long-term contract harms their reputation and may prevent them to conclude new long-term incentive contracts (which also contributes to downward wage rigidity); (iii) anti-discriminatory legislation makes it more costly to fire older workers; (iv) dismissing these workers increases the risk of a costly labour dispute with trade unions. Such frictions move the equilibrium away from the efficient labour demand $D_{eff}(W^c)$ to point A in Figure 2.2.[12] In equilibrium, employment exceeds labour demand among older workers and there is a pay-productivity gap. The pay-productivity gap is equal to the difference between the going wage W_0^c and the marginal labour productivity $F'(L_0)$ on the efficient labour demand $D_{eff}(W^c)$.

Suppose the government introduces a wage cost subsidy S. This shifts the labour demand curve to the right, to $D_{sub}(W^c)$ in Figure 2.2. However, the level of employment does not increase, since before the intervention employment was already exceeding the labour demand. This prediction is in accordance with the existing empirical evidence of Huttunen *et al.* (2013) for Finland.[13]

Figure 2.2: Effect in the Presence of a Pay-Productivity Gap and no Early Retirement



[12]Allowing for wage bargaining would move the starting equilibrium to the left of A and introduce unemployment. Since this does not affect the qualitative conclusions, we do not consider this complication. The same applies to existing income/payroll taxes, which induce a wedge between wage costs and the net wage, but which are ignored in the graphical exposition.

[13] Notice that Ilmakunnas and Maliranta (2005) find evidence of a pay-productivity gap for older workers in Finland.

2.4.2. The Effects in the Presence of Early Retirement

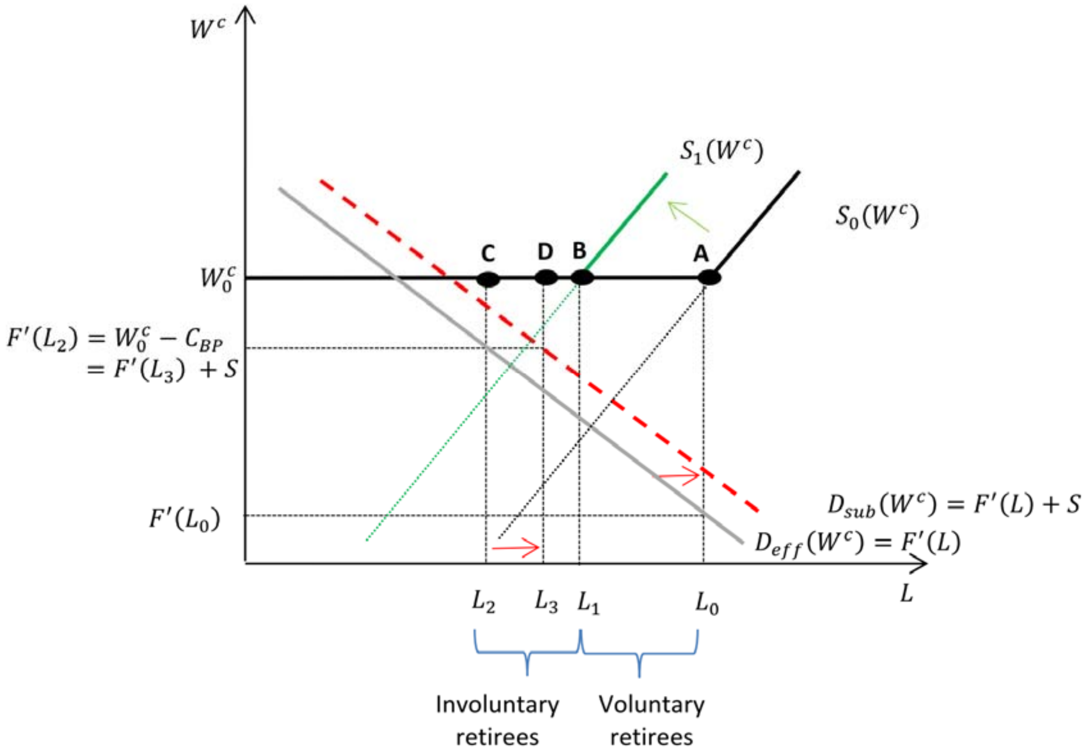
In Belgium many workers in the manufacturing sector become eligible for early retirement at age 58, an age from which their employers are automatically awarded a reduction in SSC, i.e. a wage cost subsidy (see Section 2.2). First, consider the impact of the early retirement scheme. The early retirement scheme can be seen as an agreement between employers and trade unions, allowing employers to eliminate part of the pay-productivity gap while maintaining social peace (Claes, 2012, p. 38) and, hence, the impediments (ii)-(iv) to dismissal mentioned in Section 2.4.1. However, it remains costly for employers to recur to early retirement, because the employer must pay severance payments (cf. (i) in Section 2.4.1), the supplement to UB and higher SSC, and, in some cases they have the obligation to replace the employee by an unemployed worker (see in Section 2.2 the description of the bridge pension). Suppose that these costs per retiree amount to C_{BP}. Consequently, employers will send some of their older employees to early retirement, up to a point that the marginal cost of sending an employee to early retirement is equal to the marginal benefit.[14] The marginal cost is equal to the foregone marginal productivity of this worker plus the cost of sending this employee to early retirement: $F'(L_2) + C_{BP}$. The marginal benefit is equal to the wage cost W_0^c that no longer needs to be paid out. In Figure 2.3 this means that the employer chooses to send $L_0 - L_2$ employees to early retirement and a new equilibrium is found at point C, where $F'(L_2) + C_{BP} = W_0^c$. This reduces, but not completely eliminates the pay-productivity gap, since the wage still exceeds the marginal productivity by C_{BP}>0. At the same time, as eligibility to early retirement increases non-labour income out of work for the older workers, it shifts the labour supply curve to the left: from $S_0(W^c)$ to $S_1(W^c)$ in Figure 2.3. This means that early retirement is partly voluntary, i.e. from B to A, and partly involuntary, from B to C. The share of involuntary to voluntary early retirement depends on the importance of the income effect on labour supply. Based on survey data, 40% of men and 30% of women report to be involuntary early retired in Belgium (Cohen and Elchardus, 2003).

Consider now a wage cost subsidy S that shifts labour demand from $D_{eff}(W^c)$ to $D_{sub}(W^c)$. Because the employment choice before the introduction of the subsidy is optimal for the employer, in this case employment increases from L_2 to L_3 on the perfectly elastic labour supply segment at the going wage. Nevertheless, the more the early retirement scheme improves the outside option (and shifts labour supply to the left) or the larger the wage cost subsidy S is (shifting the labour demand more to the right), the more likely that the new equilibrium will locate on the upward sloping segment of labour supply (to the right of point B), so that the part of the wage subsidy will dissipate into a higher wage

[14] This is the outcome of the following simple profit maximization problem in which we assume that employers always sends some employees to early retirement, i.e. $L_0 > L$ (and here we set $S = 0$): $\max_L F(L) - (W_0^c - S)L - (L_0 - L)C_{BP}$. The first order condition for an interior maximum of this problem is: $F'(L) + C_{BP} = W_0^c - S$.

and the employment gain will no longer be proportional to the labour demand elasticity, but lower, depending on the labour supply elasticity (see Equation (1) in Section 2.3.1). In other words, wage increases would be necessary to withhold employees from retiring voluntarily. Wage pressure will only realize, however, if the subsidy is sufficiently large. Consistent with this theoretical prediction, we find in our empirical analysis that the Belgian wage cost subsidy has a significant positive effect on employment for those workers who are at high risk of entering early retirement at age 58 and a small and statistically insignificant (short-run) impact on the hourly wage.

Figure 2.3: Effect in the Presence of a Pay-Productivity Gap and Early Retirement



W^c = wage cost; L = employment; $S(W^c)$ = labour supply; Deff(W^c) = efficient labour demand; Dsub(W^c) = labour demand with wage cost subsidy; $F'(L)$ = marginal labour productivity; C_{BP} = employer's cost of bridge pension; S = wage cost subsidy.

2.4.3. The Effects on Employment at the Intensive Margin

In general, the wage cost subsidy provides incentives to increase working time, since employers are not entitled to it if they employ individuals working less than 33% of full-time working-time and because the subsidy increases together with working time, up to 80% of full-time working-time (see Figure 2.1). Employers do not have incentives to increase the working time for all workers, but only for those for whom the marginal productivity of working exceeds the (post-subsidy) marginal cost. For this sub-population, the effect on working time is expected to be larger for workers with an elastic labour supply, such as for workers for whom in the absence of the subsidy the time credit scheme encourages to work part-time (see Section 2.2).

2.4.4. Anticipatory and Substitution Effects of Wage Cost Subsidies for Older Workers

Firms anticipate that the wage costs will also decrease for younger employees, since they will become eligible to the wage cost subsidy as soon as they turn 58. Hence, the subsidy may also positively affect the employment of younger workers (Huttunen *et al.*, 2013, p. 55). However, to the extent that wages exceed costs also for these younger workers – which is confirmed in the aforementioned evidence of Vandenberghe *et al.* (2013) – and that these workers are less at risk of entering early retirement, we do not expect these anticipatory effects to be important.

By contrast, if older and younger workers are gross substitutes, the subsidy could also negatively affect the employment of younger workers, since firms may substitute older for younger workers. As explained in Section 2.4.2, that we expect the subsidy to have employment effects because it postpones the early retirement of older workers. However, the subsidy also encourages firms to push younger instead of older workers into early retirement. The less stringent rules of the conventional pre-retirement scheme for restructuring firms and the Canada Dry system make this possible. The latter system exploits the feature of the Belgian UI that since 1996 long-term unemployed older than 50 are not required to be available for the labour market (Section 2.2). In the empirical analysis we therefore test whether any positive employment effect of the wage cost subsidy does not go at the expense of the employment of slightly younger workers.

Another type of substitution effect relates to the requirement that in about 43% of the industrial committees firms using early-retirement schemes are obliged to replace early retirees by unemployed workers (see Section 2.2). If in the absence of the wage cost subsidy, the employer effectively hires an otherwise unemployed worker, then the jobs saved by the reduction in SSC contributions go at the expense of the hiring of these unemployed. However, we cannot assess the importance of this substitution with the available data as the replacement requirement is no guarantee that this hiring would not have taken place in the absence of the early retirement scheme.

2.5. The Data

The data were sampled from the "Data warehouse labour market and social protection" in which the Crossroads Bank for Social Security[15] merges the administrative register data originating from the diverse Social Security institutions and the National Register of all Belgian inhabitants. The dataset contains individual information on gender, nationality, household and detailed labour market characteristics. Information on labour market histories is available since 1957 through the yearly

[15] See www.ksz-bcss.fgov.be/en/international/home/index.html.

pension registrations, but these contain only information on salaried employment in the private sector and temporary salaried employment in the central administration of the public sector. From 1998 more detailed quarterly information is available covering all types of employment, including self-employment, civil servants, and temporary salaried workers in the local public administration of provinces and municipalities. Table 2.7 in Appendix 2.9.1 lists the conditioning variables in these combined datasets that we use in the empirical analysis. These can be regrouped in information on the region, nationality, household, labour market characteristics and employment history as salaried worker.[16]

Privacy protection legislation forbids access to data on the full population but allowed the drawing of a sample of fixed size. Thus, we base the analysis on an endogenous sample of these register data containing 243,655 individuals born between the 1st of April 1941 and the 31st of March 1950, i.e. between 52 and 61 years old in 2002, the year that the subsidy was introduced.[17] At these ages many individuals (especially women) are not in the labour force in Belgium, and, if they are, their labour market mobility is low. These inactive individuals are not very helpful for the identification of the effects of the aforementioned policy reform on the outcomes of interest, i.e. on salaried employment and wages. However, ignoring them would introduce sample selection bias. Since privacy protection legislation forbids using population data, we therefore aimed at maximizing the informational content of our sample without introducing this bias. We oversample aforementioned older individuals working in the salaried private sector throughout the period of analysis, or making transitions in or out of this sector. Because in this way the probability of being included in the sample is a function of the outcomes of interest, we appropriately weigh the data to ensure the treatment effects are consistently estimated (Manski and Lerman, 1977; Cameron and Trivedi, 2005).

Table 2.1: Retained Birth Cohorts and Corresponding Reference Periods

	Cohort (quarter/year)	**Reference Period** (quarter/year)
1	2/41-1/42	[2/99-1/02]
2	2/42-1/43	[2/99-4/01]
3	2/43-1/44	[2/99-4/03]
4	2/44-1/45	[2/00-1/05]
5	2/45-1/46	[2/99-4/03]
6	2/46-1/47	[2/00-4/04]
7	2/47-1/48	[2/00-3/05]
8	2/48-1/49	[2/02-3/05]
9	2/49-1/50	[2/02-3/05]

To be more precise, we stratified the population for each gender in 9 birth cohorts defined in Table 2.1. Each of these 18 strata is subsequently stratified in five substrata, which are endogenous to the

[16] Because of the structural break in the data in 1998, and the identification strategy used it was not possible to define a consistent labour market history between 12 to 21 quarters prior to the measurement of the outcome.

[17] We drop individuals with missing information. This leaves us with 240,850 individuals (99% of the sample) for the analysis.

outcomes of interest. The definition of the strata depends on a different reference period for each birth cohort (see Table 2.1):

1. The population *exiting* salaried employment in the private sector within the reference period;
2. The population *entering* salaried employment in the private sector within the reference period and not contained in substratum 1;
3. The population employed throughout the reference period as salaried worker in the private sector and earning a gross wage lower than €100 per day at the start of this period;
4. The population employed throughout the reference period as salaried worker in the private sector and earning at least €100 per day at the start of this period;
5. The population that was not employed as salaried worker in the private sector during the reference period, i.e. individuals who were out-of-the labour force, unemployed, self-employed or working in the public sector.

Since labour mobility of older workers is low, we over-represented the transitions into and out of salaried employment in the private sector[18] by sampling for each of the 18 strata the complete population of substratum 1 and 2. Low-wage workers as defined in substratum 3 are also oversampled relative to the high wage workers (substratum 4) and the population not employed in the private sector (substratum 5): in each of the 18 strata a random sample of 2,000 individuals is drawn in this substratum, while the sample size was 1,500 for substratum 4 and 5.[19] Low-wage workers are oversampled, because the subsidy intensity decreases with the wage and wage cost subsidies are found to have larger effects for these workers (see Section 2.3).

The reference periods by birth cohort defined in Table 2.1 were chosen as to observe sufficient transitions in and out of private sector employment during the periods of analysis. However, as it was difficult to predict the exact identification strategy before having the data, these reference periods are not optimally chosen.[20] Nevertheless, they are all situated around the period of interest, i.e. close to the introduction of the subsidy in the second quarter of 2002 and the sampling weights still allow us to make unbiased inference on the population.

As explained in more detail in the next section, the empirical analysis contrasts the evolution of a number of outcome variables (the employment rate, the fraction of individuals working less than 30% of full-time working-time, between 30% and 80%, more than 80%, the "approximate" fraction of full-

[18] We restrict the sample to private sector salaried employment, since only these workers are eligible to the subsidy.
[19] In cases that the population of the substratum was smaller than the population, the complete population was sampled.
[20] The data could only be obtained after a lengthy procedure, so that redefining the reference periods was not an option.

time working-time that an individual works,[21] and the hourly wage rate) between eligible workers (= treated group) and non-eligible younger workers (= control group). Table 2.8 in Appendix 2.9.1 reports descriptive statistics of the conditioning variables listed in Table 2.7 for the treated and control groups used in the benchmark analysis. Since the empirical analysis is performed by gender, the male and female populations are explicitly distinguished in this descriptive analysis. Because of the endogenous stratified sampling, the individual observations i belonging to birth cohort c $(= 1, 2, \dots, 9)$ and to the substratum s $(= 1, 2, \dots, 5)$ are reweighed by the sampling weights SW_i as to obtain a correct description of the corresponding populations (Cameron and Trivedi, 2005):

$$SW_i = \frac{N_{cs}}{N} * \frac{n}{n_{cs}} \tag{2}$$

where N_{cs} denotes the size of the population in substratum cs,[22] n_{cs} the corresponding sample size, $N \equiv \sum_{c=1}^{9} \sum_{s=1}^{5} N_{cs}$ the total population size and *n* the corresponding sample size. As to avoid cumbersome notation, gender is not explicitly referred to. The formula comes from a double re-weighting, within and between cohorts.[23] Table 2.8 also reports the p-values of the t-statistics testing the equality of the means between treated and control groups. It is clear that treated and control groups are different in several dimensions. This justifies the use of a CDiD estimator.

2.6. The Identification Strategy and Estimation Method

Since the policy measure that we evaluate is targeted on a clearly defined sub-population (private sector employees aged 58 or more) and is introduced at a precise date (April 1, 2002), it is quite natural to use a simple DiD estimator to identify the Average Treatment Effect on the Treated (ATT) of the wage cost subsidy on a number of outcomes. Nevertheless, a number of factors complicate the analysis: (i) the early retirement scheme induces a significant drop in the employment rate at 58, the same age at which the eligibility to the wage cost subsidy starts; (ii) anticipatory and substitution effects may affect the outcomes of control units; (iii) compositional differences between treated and control populations lead to a violation of the underlying identifying assumptions; (iv) the endogenously stratified sampling needs to be taken into account.

[21] We do not have information on hours, but on the fraction of working time in a quarter relative to a full-time worker in 10 percent classes. The "approximate" fraction is set to the midpoint of the corresponding time-class (e.g. 55.5 for someone who works between 51% and 60% of a full-time worker).

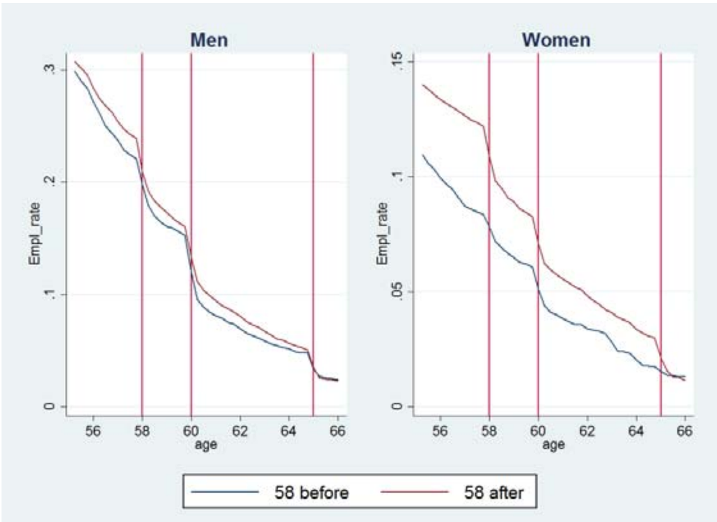
[22] We have information on the population sizes in each substratum, i.e. on N_{cs}.

[23] First, to restore the representativeness within the cohorts we reweigh the units within each cohort by $SW_{cs}^{c} = \frac{N_{cs}}{N_c} * \frac{n_c}{n_{cs}}$ (where N_c and n_c is the size of the cohort in the population and in the sample). To make the cohorts in the sample representative for the population, we weight each cohort a second time: $SW_{cs} = SW_{cs}^{c} * \frac{N_c}{N} * \frac{n}{n_c}$, so that $SW_{cs} = \frac{N_{cs}}{N} * \frac{n}{n_{cs}}$.

2.6.1. *Accounting for Age-Related Shocks in the Outcome Variable*

Figure 2.4 reports the evolution of the employment rate of birth cohorts turning 58 in the pre- (left panel) and in the post-treatment period (right panel). The employment rate declines with age, and this decline clearly accelerates at 58 and 60, ages at which people become eligible for early retirement (see Section 2.2). The drops in the employment rate are especially marked for men, because more men are employed in the declining manufacturing sector in which early retirement is more prevalent, while women tend to be more employed in the service sector. A consequence is that a panel DiD estimator in which the same individuals are followed over time is severely downward biased, even if we would take a trend-adjusted version of it, since it cannot distinguish the effect of eligibility to the subsidy from that to the early retirement scheme, both starting at 58.[24]

Figure 2.4: Employment Rate of Birth Cohorts Turning 58 in the Pre- and Post-Treatment Period



Evolution of the employment rate of birth cohorts turning 58 during the pre-treatment period (left panel) and in the post-treatment period (right panel). The vertical red lines indicate the quarter in which the cohort turns 58, 60 and 65. Employment rates are weighted by the sampling weights defined in Equation (2).

As shown by the lower blue lines in Figure 2.4 the drop of the employment rate at 58 is already present in the pre-treatment period.[25] This suggests that it is possible to account for this drop by implementing a DiD estimator based on repeated cross-sections in which the *age* of treated and control groups is fixed over time instead of the individual. The analysis is performed by retaining individuals only when belonging to two specific age-groups (see Figure 2.5). On these two groups we implement a DiD

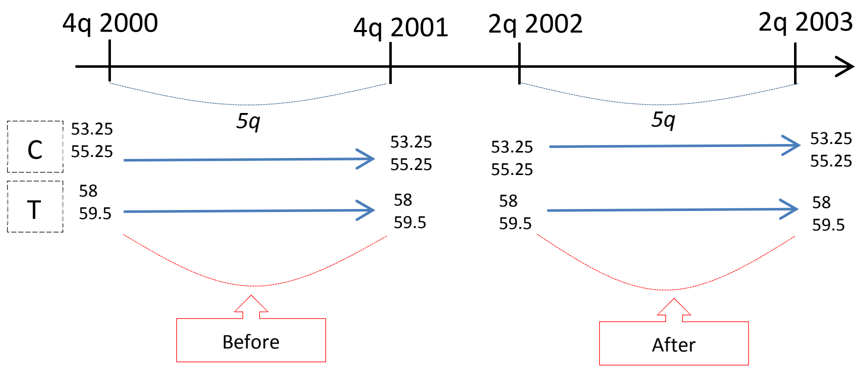
[24] To check this, we implement a DiD estimator in panel data by OLS or fixed-effect estimator. In the regression we use all the 243,655 individuals on the period 1998q2-2003q2 and include quarterly-age & time dummies, the age-time interaction for the units above 58 years old after the period 2002q2 (ATT) and another interaction for 2002q1 (anticipation effects). As placebo test we add three further age-time interactions ("placebo") for the period 2001q4, 2001q3, 2001q2. As shown in Table B.10 in the Supplementary Appendix B, the estimated ATTs are usually negative and the placebo tests are rejected.

[25] The drop of the employment rate in the post-treatment period is even larger, suggesting a negative effect of the wage cost subsidy on employment. However, this is descriptive evidence, which does not condition on the predetermined variables and does not control for the differential trend in the employment rates of treatment and control groups (see below).

estimator, where the age-group above 58 years old is the treated group while the younger age-group is the control group. After the introduction of the SSC exemption in the 2nd quarter of 2002, the outcome of the treated may be affected by the policy and any variation compared to the path of the younger control group can be attributed to the treatment. The main assumption of the estimator is that, in the absence of the treatment, the difference in outcome between the two age-groups would have remained constant in the pre- and the post-treatment periods. In Section 2.6.4 we describe how we make the estimator more robust to this assumption and, possibly, isolate pre-existing age-specific trends.

To enhance precision of the estimates, the treatment groups are defined by age brackets that are wider than the time unit of analysis (i.e. one quarter), which means that the same individuals can enter a treatment group in more than one time unit of analysis. In the baseline model we compare treated units aged 58 to 59.5 to control units aged 53.25-55.25. The maximum age of the treated units is 59.5 as in our data this is the oldest cohort at the beginning of the pre-treatment period. The choice of the age range of the control group is related to the issue of anticipatory and substitution effects (see Section 2.6.3). Later in some sensitivity analyses we change the age brackets of the control group.

Figure 2.5: DiD with Multiple Repeated Cross Sections: Fixing Age over Time



C = Control group; T= Treatment group.

2.6.2. The Methodology in the Baseline Analysis

The post-treatment period starts in the second quarter of 2002, when the subsidy was awarded for the first time. We limit the analysis to the short run as the treatment period ends five quarters later, in the second quarter of 2003. This end is determined by the reform in January 2004 that reduced the age eligibility threshold to 57 and excluded several sectors from the reduction (e.g. health services, education and hotels). We stop the evaluation period two quarters earlier to allow for anticipatory effects of this reform. The outcomes are measured at the end of each quarter in the pre- and post-treatment periods for both control and treatment groups. We fix in each quarter the age range of the treated groups from 58 to 59.5 years and for the control groups from 53.25 to 55.25 years (see Section

2.6.3). Formally the DiD estimator amounts to estimating the following linear regression equation, which is a slight modification of the standard version (Meyer, 1995), since it involves multiple pre- and post-treatment periods:[26]

$$Y_{iq} = \alpha + \sum_{q=-4}^{5} \alpha_q * T_q + \beta * D_{iq} + \sum_{q=0}^{5} \delta_q * D_{iq} * T_q + \epsilon_{iq} \quad (3)$$

where

- $q = -5, -4, \ldots, -1$ denotes the five quarters in the pre-treatment period, $q = 1, 2, \ldots, 5$ denotes the five quarters in the post-treatment period, and $q = 0$ denotes the first quarter of 2002, during which firms may anticipate the introduction of the wage cost subsidy;
- Y_{iq} is the outcome measured for individual *i* in quarter *q;*
- $D_{iq} = 1$ if individual *i* is between 58 and 59.5 years old in quarter q and, hence, belongs to the treated group;
- $D_{iq} = 0$ if individual *i* is between 53.25 and 55.25 years old in quarter q and, hence, belongs to the control group;[27]
- $T_q = 1$ if the time period in which the outcome is measured is quarter q and $T_q = 0$ otherwise.

The time effects are captured by the constant term α measuring the effect in quarter $q = -5$, and by α_q (for $q = -4, -3, \ldots, 5$) measuring the time effects in deviation from the first quarter. The effect of belonging to the treatment group – aged between 58 and 59.5 – is captured by β. The ATT in quarter q ($q = 0, 1, \ldots, 5$) is measured by δ_q. The error term of the regression is ϵ_{iq}.

2.6.3. *Anticipatory and Substitution Effects*

In Section 2.4.4 we argued that the wage cost subsidy may induce anticipatory or substitution effects on workers younger than 58. If the younger control group is affected by anticipatory effects this would bias the ATT downwards, while substitution effects would lead to an upwards bias. We argued that substitution effects are likely to be a more important concern than anticipatory effects.

Independently of which of these two effects is dominant, we must avoid selecting the control groups who are just slightly younger than 58, because the aforementioned spill-over effects are likely to be more important the closer the age eligibility threshold. The youngest age at which we can select control individuals within the available sample and given the chosen methodology is 53.25 years. In the baseline analysis we will therefore define control groups as individuals in the age range 53.25-55.25. In a sensitivity analysis we then increase the upper range of these control groups to 57 years or

[26] Different from the standard repeated cross-sections, as already mentioned in Section 2.6.1, the same individual can appear in more cross-sections. Since we have information on the individual identifier, this justifies the subscript *i* in the regression equation. We explain below how in the inference we take into account the serial correlation that this induces.

[27] Individuals belonging neither to the treated nor the control group in a given q are not used in that quarter.

consider only the control units right below the age-eligibility of 58. By comparing the magnitude of the effects obtained in the baseline and in the sensitivity analysis, we can judge to what extent these spill-over effects are important and which of the two dominates.

Another type of anticipatory effect needs to be taken into account. Because the policy plan was discussed in the media, it could have been anticipated by employers before it was enacted. The fact that the wage cost subsidy has been implemented retroactively – the Royal Decree for this measure was published only on the 26th of June 2002, while the subsidy was already granted from April 1 – makes such anticipation less likely. Nevertheless, in the analysis we end the pre-treatment period on the 31st of December 2001 and, hence, allow for such anticipation during the first quarter of 2002.

2.6.4. *Accounting for Compositional Differences Between Treated and Control Groups*

At the end of Section 2.5 we mentioned that the observed composition of treatment and control groups differs on average. This does not invalidate a DiD analysis to the extent that these differences remain constant over time, but this is not the case as in each quarter new individuals enter and exit the two groups. We therefore apply a semi-parametric CDiD estimator (Heckman *et al.*, 1997, based on the previous parametric CDiD estimator of Heckman and Robb, 1985) in which we explicitly control for possible compositional biases in this multi-period framework. This estimator requires that, *conditional* on the observed explanatory variables, the outcomes of treated and control groups evolve according to a parallel trend in the absence of a treatment. Moreover, by conditioning on observed covariates treated and control units become more comparable, which makes the DiD less vulnerable to the assumption that the selection on unobservables should be additive (Athey and Imbens, 2006).

We listed in Table 2.7 the conditioning variables that we use in the empirical analysis. Among these variables are lagged outcomes of interest and other potentially endogenous variables. Since the post-treatment period, including the first quarter of 2002 during which the policy might have been anticipated, lasts six quarters, we ensure that these lagged variables are predetermined by measuring them all at least seven quarters prior to the outcome variable of interest. As to control for the compositional change in the treatment groups occurring in each quarter, the covariates are measured with the same seven quarter lag for all units. Importantly, all treated units have their characteristics measured before the age at which they become eligible to early retirement (i.e. before 58).

The Implementation Method

In order to estimate the ATT, Heckman *et al.* (1997) implement the CDiD method by matching control to treated units on the basis of the Propensity Score (PS) before taking the double difference in the outcomes. In this paper we extend the CDiD method in repeated cross-sections to a multi-period framework. Instead of a matching estimator, we make the control units comparable to the treated by Inverse Probability Weighting (IPW) as proposed by Horvitz and Thompson (1952) and Hirano *et al.* (2003).[28] Busso *et al.* (2014) advocate using this method in cases that overlap of the distribution of the PS between treated and control groups is good,[29] which is the case in this empirical application (see the Supplementary Appendix B: Figure B.1-B.4). Based on the efficient trimming rule of Crump *et al.* (2009), which removes the units with a PS above a certain threshold, we trimmed over all estimations at most 0.4% of the reference treatment group defined below.[30] Other reasons for choosing this estimator is that it is simple to take the endogenous sampling present in our data into account and that it is not very computationally intensive, which matters given the large sample size, the CDiD framework with multiple periods and that we bootstrap the standard errors (see below).

Our Weighted DiD (WDiD) estimator can be implemented by estimating regression Equation (3) by Weighted Least Squares (WLS). Since we implement the WDiD estimator with a series of cross-sections instead of the usual four treated/control groups and before/after periods, we must modify the standard procedure (Blundell *et al.*, 2004) and choose a reference treatment group to which we make the other groups comparable (Lechner and Wunsch, 2009). We choose the treated group in the second quarter of 2002, i.e. $q = 1$, to be this reference.

The estimation procedure then consists of the following steps:

1. Estimate, using the observed conditioning variables X_{iq} for individual i in quarter q, the PS of belonging to the reference treatment group (D_1) by logistic regressions. The estimation is separately run for the treated (D_q) and the control groups (C_q) of each quarter (if $q \neq 1$), and just for the control group C_1 in $q = 1$.
2. Trim the data on the basis of Crump *et al.*'s (2009) efficient trimming rule.[31] The after trimming treated group in q=1 is composed of the intersection of all the common supports in all the estimations of the PS.

[28] Using a simple matching/reweighting estimator to estimate the ATT produces biased estimates as treated individuals remain different in terms of age. Differently, a matching/reweighting estimator can be used to make the assumptions of the DiD estimator, such as the absence of compositional effects and common responses to the business cycle, more credible.
[29] In an empirical Monte Carlo analysis Huber *et al.* (2013) confirm the reliability of IPW, although it is outperformed by some alternative matching estimators. More recently, Frölich *et al.* (2015) have found that the IPW in which the PS is parametrically estimated is well performing.
[30] We also tried out the alternative trimming rule proposed by Huber *et al.* (2013), but according to this rule virtually no individuals should be discarded from the treatment group, even if we decrease the threshold of the trimming weight from 4% to 1%. This may be related to the much larger sample size in this application relative to the largest one used in the aforementioned paper. On the other hand, it is also a confirmation that the overlap is good in our empirical application.
[31] The trimming eliminates treated and control units above a certain value of the PS.

3. Re-estimate the PS on the data after trimming as in point 1[32] and denote the corresponding estimates of the PS by $\hat{P}^J(X_{iq})$, where $J \in \Gamma \equiv \{D_{-5}, C_{-5}, D_{-4}, C_{-4}, \ldots, C_0, C_1, D_2, \ldots, D_5, C_5\}$.

4. Estimate Equation (3) by WLS using the following weights:

$$PW_i = \frac{\hat{P}^J(X_{iq})}{1-\hat{P}^J(X_{iq})} \Big/ \frac{1}{N^J} \sum_{i=1}^{N^J} \frac{\hat{P}^J(X_{iq})}{1-\hat{P}^J(X_{iq})} \quad \text{for } i \in J \text{ and } J \in \Gamma, \text{ and } PW_i = 1 \text{ for } I \in D_1 \qquad (4)$$

where N^J denotes the number of individuals in group J and where, following Busso *et al.* (2014), we normalize the weights to one by dividing the individual weight by the average weight in the corresponding group.[33]

WDiD estimator versus parametric DiD

A DiD estimator implemented by a linear model and parametrically controlling for additional covariates has some undesirable features. For instance, the effect heterogeneity that is allowed for might be too restrictive, the treatment effect can be highly dependent to the functional form specified and the conditional expectations of the potential outcomes may be outside the possible limited support (for a survey on DiD see Lechner, 2011). Our semi-parametric WDiD avoids these drawbacks of the linear model by flexibly controlling for the set of covariates by the IPW estimator and fixing the reference covariates distribution of the treated group. Since we also control for the individual lagged dependent variables, we also shrink pre-existing differences in levels between the treated and control group, which makes the WDiD estimator more robust to dependence to the functional form of the outcome (Meyer, 1995; Athey and Imbens, 2006).

Endogenous Sampling

In Section 2.5 we explained that our sample is endogenously stratified. Frölich (2007) demonstrates in a standard Kernel matching framework that, contrary to choice-based sampling, endogenous sampling requires appropriate re-weighting of the data both in the estimation of the propensity score and the treatment effect. It is straightforward to implement these adjustments for our WDiD. First, in steps 1 and 3 we weight each observation in the logistic regressions by the sampling weights SW_i defined in Equation (2). Second, in step 4 a double weighting should be applied by replacing PW_i by PW_i*SW_i.

---

32 As in Lechner and Wunsch (2009), this re-establishes the same reference population to the trimmed cross-sections.

33 Stata automatically normalizes the weights in the regression by specifying the option *pweights*. Since the normalization performed by Stata is slightly different (it divides the individual weight by the average weights of the whole sample), we recommend manually normalizing the weights (i.e. by group – see equation 4).

Statistical Inference

By weighting, the Least Squares estimator becomes heteroskedastic. In addition, the fact that individuals may belong to multiple cross-sections considered in our WDiD estimator induces serial correlation. Correct inference requires therefore using cluster-robust standard errors. However, these cluster-robust standard errors lead to conservative inference, since they do not take into account that the PS in the weights PW_i are estimated. It has indeed been shown in the literature that taking into account that the PS are not known, but estimated in a first step can increase the precision of the estimator (Hirano *et al.*, 2003; Wooldridge, 2010, pp. 500–502 and 824–827). We do this by bootstrapping the standard errors. Since our data come from an endogenously stratified sample, we cannot apply a standard bootstrap, however. Instead we implement a *stratified bootstrap* by randomly drawing for each replication n_{cs} individuals within each cohort-stratum cs. This is valid because in the sampling individuals within each cohort-stratum are randomly drawn (for a review on bootstrap and stratified data see e.g. Shao, 2003). Notice, as to account for serial correlation, we re-sample within each replication the same individuals (i.e. clusters) in all cross-sections, rather than the single observations (i.e. individuals * quarter). Because of the large sample size and the high number of times we have to estimate the PS to obtain the ATT (i.e. 42 for each outcome and population considered), we limit the number of replications in the bootstrap to 200. The reported standard errors are the empirical estimates in the bootstrap sample. Confidence intervals are based on the assumption of asymptotic normality of the weighted estimator.

Placebo Tests and Parametric Trend-Adjusted WDiD

The WDiD crucially depends on the assumption that the double weighted outcomes of treated and controls follow a parallel path in the absence of the treatment. Graphically the outcome variables clearly display a parallel trend, apart from the employment rate (see Figure 2.6 in Appendix 2.9.1). We also test for this formally in the following placebo test. We divide the pre-treatment period in two (grouping the first three and the last two quarters of this period) and implement the WDiD estimator (maintaining D_1 as reference group) on this subsample by assimilating the last quarters of the pre-treatment period to a placebo post-treatment period. The placebo test consists in testing whether the placebo treatment has a statistically significant effect.

If the placebo test is rejected, which is the case for the employment rate, we propose a *parametric* trend-adjusted WDiD estimator similar to the one proposed by Wolfers (2006), rather than the *non-parametric* one proposed by Bell *et al.* (1999) and Blundell and Costa Dias (2009), because the pre-treatment period is too short (five quarters) to implement the non-parametric version.[34] We do this because the graphical analysis of the evolution in the employment rate in the pre-treatment period clearly displays a differential time trend between treated and control groups (see Figure 2.6 - top panel - in Appendix 2.9.1).[35] If the difference in *growth* rate in the employment of the two groups remains constant, i.e. we can rely on *parallel growths* rather than *parallel paths,* then the trend-adjusted WDiD is a consistent estimator. Such *parallel growths* imply that the employment rate of younger generations (the control group) declines at a lower rate than that of the older ones (the treatment group). A possible explanation for the differential evolution in the employment rate is the data do not contain information on the level of educational attainment. Since we cannot condition on this level in the empirical analysis, the slower decline in the employment rate for the younger generations may be caused by higher level of educational attainment of the younger relative to the older generations, since the high educated withdraw less rapidly from the labour market than the low educated. The parametric trend-adjusted estimator consists in adding a common trend $\gamma * q$ and a differential one for the treatment groups $\mu * (q * D_{iq})$ in regression Equation (3) and to subsequently follow the same estimation steps as described earlier:

$$Y_{iq} = \alpha + \sum_{q=-3}^{5} \alpha_q * T_q + \beta * D_{iq} + \gamma * q + \mu * (q * D_{iq}) + \sum_{q=0}^{5} \delta_q * (D_{iq} * T_q) + \epsilon_{iq} \quad (5)$$

Since in the treatment period the deviation between the treated and the control units is fully saturated by the set of treatment dummies $\sum_{q=0}^{5}(D_{iq} * T_q)$, the differential linear trend $q * D_{iq}$ is estimated on the basis of the pre-treatment period only. Note that identification of these trends requires normalizing one common quarterly time effect to zero. We choose to set $\alpha_{-4} = 0$.

2.7. Results

In this section we report the results of our estimations. The analysis is performed separately for men and women. We focus on the following outcomes: the employment rate (i.e. the extensive margin), the time worked (i.e. the intensive margin) in three categories (less than 30% of a full-time worker, between 30% and 80%, and more than 80%), a measure approximating working time and the hourly wage rate.[36] For the employment rate as outcome variable, we do not only report the ATT on the total population within the selected age category (58-59.5), but also on the sub-population that was already in salaried employment in the private sector seven quarters before the measurement of the outcome. We focus on the effect of the latter group, since, if the wage cost subsidy has any effect on the

[34] We ignore complications on the inference due to the choice of the estimation method on the basis of the placebo tests.

[35] As the parallel path for the intensive margin of women and the gross hourly wage is not consistent between the 4th quarter 2000 and the 1st quarter 2001, for these scenarios we start the pre-treatment period in the 1st quarter of 2001.

[36] In the analysis evaluating the effect on the hourly wage we remove 1,640 outliers (0.2% of the sample): respectively for men and women, hourly wages below 1.4 €/h and 1.1€ /h, and higher than 140 €/h and 117€/h.

employment of older workers, it is more likely to have an effect on the separation rate (i.e. on "employment retention" - see Section 2.3) than on the hiring rate. For each of the outcomes we also estimate the ATT on particular subpopulations. For employment retention (the extensive margin) we define the subpopulation on people who were already employed 7 quarters earlier, while for the intensive margin and the wage is on the people employed in the current quarter: low-wage workers earning less than the population median wage in the aforementioned employed subpopulation, blue and white-collar workers, manufacturing sector and service sector, large firms and small firms, and salaried workers employed in sectoral joint industrial committees ("commissions paritaires"; see Section 2.2) in which in the pre-treatment period the exit rate from employment was for employees aged 57.75 within the next two quarters above or below the population median rate (i.e. 18%).[37] The latter two subpopulations were constructed with the aim of testing our hypothesis that the employment effect of the wage cost subsidy is significantly positive only for employees with a high risk of entering early retirement at 58 (see Section 2.4.2). The inclusion of separate effects for blue-collar workers and employees in the manufacturing sector has the same justification, since these subpopulations were at higher risk of entering early retirement. Furthermore, labour demand in the manufacturing sector might be more elastic as they are more exposed to international competition, especially in a small economy such as Belgium. The effects on low wage workers are of interest, since compared to high wage workers the lump-sum subsidy is higher relative to the wage cost and the labour demand is more sensitive to wage costs according to the literature mentioned in Section 2.3.2. Finally, large firms tend to use early retirement schemes more intensively and their labour demand elasticity might be larger.[38] Except for the employment rate, we estimated the effect of the wage cost subsidy by WDiD. Since for the employment rate the placebo test failed, we estimated the ATT for this outcome by the trend-adjusted WDiD. The PS-reweighted sample was always well balanced. In 98% of the 1,680 times that we reweighted the units, not any variable had a Standardized Bias above 5%. Moreover, the lowest p-value of the log-likelihood test was 0.98 and the highest R-squared 0.002.[39] Here we limit to reporting the average ATT of the subsidy in the post-treatment period, i.e. $\bar{\delta} \equiv \sum_{q=1}^{5} \delta_q / 5$.[40]

[37] In case the information of the industrial committee was missing, the separation rate of the sector was considered.
[38] In the pre-treatment period and in firms with more than 100 employees, 24% of the workers exit to early retirement within the next 7 quarters compared to 14.7% in small firms.
[39] The two last mentioned tests were proposed by Sianesi (2004).
[40] More detailed results for the overall population and for the high and low exit rate committees can be found in the Supplementary Appendix B (Table B.4-B.6). Other results are available from the authors upon request.

2.7.1. *Effect on the Employment Rate*

Table 2.2 reports both for men (top panel) and women (bottom panel) the ATT of the wage cost subsidy on the employment rate of the population aged between 58 and 59.5 (first column). We also estimate the impact on the "retention rate" in employment for the older people who were already employed in the salaried private sector 7 quarters earlier (all columns beyond the first). The overall ATT on the employment rate is positive but insignificantly different from zero: the reform enhanced the employment rate of men by 0.4 percentage points (*pp*) and of women by 0.3 *pp* on average. In terms of semi-elasticities, this effect is comparable to the ones that were found by Huttunen *et al.* (2013). A 10% increment of the subsidy increases the employment rate for men by 1.1 *pp* and for women by 0.5 *pp,* which is very close to the range of 0.44 to 0.86 *pp* reported by Huttunen *et al.* for the temporary wage cost subsidy in Finland (Ibid., p. 57). Since the employment rate of the older workers in Belgium is much lower, the implied employment elasticities are much higher in Belgium than in Finland: 0.6 and 0.7, respectively for Belgian men and women, while in Finland the elasticity is about 0.1.

In Section 2.4.1 we argued that in the absence of early retirement schemes we do not expect a wage cost subsidy to have any impact on employment/retention, since the pay-productivity gap for older workers results in employment in excess of labour demand and the wage cost subsidy is insufficiently large to reverse this gap. Firms actually would prefer firing their older employees, but are inhibited doing so, because of employment protection regulation and because trade unions threaten them with social disruptions. The finding of an insignificant overall impact of the wage cost subsidy on employment is in accordance with this hypothesis.

Nevertheless, in Section 2.4.2 we also argued that the economic environment is different for those salaried employees whose working conditions are determined by joint industrial committees that have agreed on allowing dismissals to early retirement at age 58, the same age threshold at which workers become eligible for the reduction in employers' SSC. For these workers the subsidy may prevent entry in early retirement and, hence, have a positive effect on the employment rate. Since these agreements were usually concluded for blue-collar workers in the manufacturing sector, we would expect the effect of the subsidy to be concentrated on these groups. For men working in the manufacturing sector the subsidy did indeed significantly increase employment by 2.4 *pp* (though only at a significance level of 10%). For women the impact is also 2.4 *pp*, but statistically insignificant, presumably because most women work in the service sector. However, for blue-collar workers, no significant effects are found, but this might be because we did not restrict to blue-collar workers for whom an agreement facilitating access to early retirement was concluded (Table 2.9 in Appendix 2.9.1). This is why we refined our measurement by constructing the aforementioned indicator identifying workers at high risk of entering early retirement at age 58. For these groups of workers the impact is large (2.2 *pp* for men and 3.1 *pp* for women) and statistically significant for men, but not for women (p-value of 0.132). The lower precision of the effect for women is most likely caused by the much lower female employment rate in that age class: it is less than half as large as the male one.[41]

Table 2.2: Impact on the Employment/Retention Rate (Extensive Margin)

	Men				Women			
	Overall (1)	Manufacturing Sector (2)	High Exit Rate (3)	Low Exit Rate (4)	Overall (1)	Manufacturing Sector (2)	High Exit Rate (3)	Low Exit Rate (4)
ATT in *pp*	0.4	2.4*	2.2**	-0.3	0.3	2.4	3.1	-1.8
95% CI	[-0.3; 1.1]	[-0.2; 4.9]	[0.0; 4.5]	[-3.1; 2.5]	[-0.2; 0.8]	[-4.2; 9.0]	[-0.9; 7.1]	[-5.5; 1.9]
Pvalue	0.243	0.066	0.048	0.850	0.208	0.473	0.132	0.333
ATT %	2.3%	4.4%	4.1%	-0.4%	4.0%	4.5%	5.2%	-2.3%
N° of observations	758,565	301,677	349,438	189,368	444,838	51,539	147,600	136,450
N° of individuals	152,015	64,997	77,269	43,599	88,835	11,077	33,538	30,178
Subsidy/Labor cost	3.7%	3.4%	4.3%	3.0%	5.5%	4.5%	5.7%	5.2%
Elasticity	0.632	1.302	0.941	-0.119	0.717	1.01	0.901	-0.452
Semi-Elasticity in *pp*	0.111	0.697	0.517	-0.091	0.055	0.541	0.534	-0.354

*WDiD on parallel growths: impact on the employment rate. Control group is 53.25-55.25 years old; treated group 58-59.5 years old. Effects on specific subpopulation (defined in q-7) by column: (1) Overall treated group, (2) Workers in secondary sector, (3) Workers in sectoral industrial committees with an exit rate from employment above the population median (= 18%), (4) Workers in sectoral industrial committees with an exit rate below the population median. Point estimates of the ATT are expressed in percentage points (pp), in proportional (%) changes in the employment rate and in terms of (semi-)elasticity, i.e. the proportional (pp) effect on the employment rate of a proportional reduction in the labour costs. N° of observations is the sum of the number of individuals observed in each quarter of the analysis. Standard errors are obtained by a stratified bootstrap (clustering by individual) with 200 repetitions and 95% confidence intervals (CI) by assuming normality. ***: significant at 1%, **: significant at 5%, *: significant at 10%.*

Table 2.3: Impact on the Share in Early Retirement and Other Non-Employment States

Men	Manufacturing Sector		High Exit Rate Committee	
	Bridge pension (1)	Other Exit (2)	Bridge pension (3)	Other Exit (4)
ATT in *pp*	-2.1**	-0.4	-3.7***	1.1
95% CI	[-4.1; 0.0]	[-2.3, 1.5]	[-5.1; -2.3]	[0.6, 2.8]
Pvalue	0.048	0.695	0.000	0.195
ATT %	-5.8%	-3.6%	-10.3%	12.1%
N° of observations	301,677	301,677	349,438	349,438
N° of individuals	64,997	64,997	77,269	77,269

Women	Manufacturing Sector		High Exit Rate Committee	
	Bridge pension (1)	Other Exit (2)	Bridge pension (3)	Other Exit (4)
ATT in *pp*	3.5	-	-0.9	-1.9
95% CI	[-2.1; 9.1]	-	[-3.5; 1.7]	[-5.4, 1.4]
Pvalue	0.225	-	0.495	0.259
ATT %	12.5%	-	-3.9%	-11.3%
N° of observations	51,539	-	147,600	147,600
N° of individuals	11,077	-	33,538	33,538

*WDiD on parallel growths: impact on the share of people in pre-retirement/bridge pension (columns 1, 3) and in any other non-employment status (column 2,4) for the subpopulations for which a significant employment effect was estimated. Control group 53.25-55.25 years old; treated group 58-59.5 years old. Effects on specific subpopulation (defined in q-7) by column: (1 and 2) Workers in secondary sector, (3-4) Workers in sectoral industrial committees with high exit rate from employment. ATT are expressed in percentage points (pp) and percentage (%) changes. Estimates are not reported for the "other exit" outcome of women in secondary sector since they are very noisy and not following a parallel path or growths. N° of observations is the sum of the number of individuals observed in each quarter of the analysis. Standard errors are obtained by a stratified bootstrap (clustering by individual) with 200 repetitions and 95% confidence intervals (CI) by assuming normality. ***: significant at 1%, **: significant at 5%, *: significant at 10%.*

This corresponds to an employment elasticity of the subsidy of 0.9 for both men and women. The estimated employment effect is in line with the effects predicted by our theoretical model in presence of a pay-productivity gap. In Table 2.3 we report further evidence that the positive employment effects are caused by a lower rate of entry into the bridge pension, at least for men. We observe that for men

[41] Treated women also display a larger differential trend (Figure 2.6), which might decrease the precision of the estimates.

employed in the manufacturing sector or in a firm at high risk of early retirement the share in early retirement significantly decreases, while the share in other non-employment states is not significantly affected. For women, we do not find any effect that is significantly different from zero. This mirrors the imprecisely estimated effect on the employment rate. Hence, our data do not allow finding reliable estimates for women.[42] Finally, we do not find any significant effect on employment for any of the other subpopulations that we considered (Table 2.9 in Appendix 2.9.1), not even for low-wage workers on whom the subsidy have a higher intensity, suggesting that the pay-productivity gap is present for this group as well. These results are therefore largely in accordance with the hypotheses that we formulated, as well as with the existing scientific evidence.

2.7.2. *Effect on the Hours Worked*

Table 2.4 reports the impact on employment at the intensive margin. Since no subsidy is granted to employees working less than 33% of a full-time worker within a quarter, employers have incentives to increase the working time of the subgroup of individuals for whom the marginal productivity exceeds marginal costs. In line with this prediction the subsidy significantly reduces the number of men working less than 30% of full-time working-time by 13%, but since only few men are contained in this category, this decreases the share of men in this category by only 0.6 *pp.* In contrast, more women are working in this category. Consequently, even if the point estimate in *pp* is similar to that for men (-0.5 *pp*), the subsidy reduces the number of women in this category by only 4%, an effect that is not significantly different from zero. This contrasting evidence for men and women may be related to the lower supply elasticity of these specific women, who, more than men, may have deliberately chosen to work few hours as a consequence of the traditional gender division in caring responsibilities in the household. The subsidy essentially shifts the aggregate working time of men from less than 30% of full-time working-time to more than 80% (even if the latter effect is not significant). For women the subsidy significantly decreases the share of women in the category 30% to 80% of working-time by 1.2 *pp* in favour of the category working more than 80%. In Section 2.4 we argued that the subsidy should especially affect the working time of individuals with an elastic labour supply, such as for workers who in the absence of the subsidy are encouraged by the time credit scheme to work part-time (50% or 80% of full-time working-time). The findings reported in Table 2.5 confirm that the subsidy seems to have induced firms to discourage their employees to reduce their working time within the time credit scheme, especially for women, who are also more intensive users of this scheme (see Section 2.2). However, we must be careful to interpret these estimates on the intensive margin, since they may partially reflect a compositional effect induced by the ATT's on the extensive margin. In particular, for

[42] The estimates for the "other exit" outcome in the manufacturing sector are not reported since they are very noisy. Placebo tests reject the assumption of parallel path and at the same time the parallel growths assumption is unrealistic.

men the effect on the intensive margin is significant only for the sub-populations for which we found statistically significant effects on the employment rate. This suggests that for men the impacts at the intensive margin partly reflect compositional effects, i.e. that the employment gain at the extensive margin especially concerned full-time workers. The gain at the extensive margin is likely to have also increased the share of employees working between 30% and 80% of full-time working-time (driving a reduction on the share of workers below 30% of full-time working-time). This can explain why the effect at the intensive margin is not significantly negative for this category, while Table 2.5 does report a significant decrease in the use of time credit for men. By contrast, for women, for whom we did not find employment effects at the extensive margin significantly different from zero, more positive effects on working time are observed, especially in the service sector and in large firms (Table 2.11 in Appendix 2.9.1). This is consistent with access to time credit schemes being less restrictive in large firms (see Section 2.2 - in the pre-treatment period 21% of the women working in large firms was in time-credit, compared to 4% in small firms) and more used in the service sector (12% of the women working in the service sector was in time credit, compared to 7% in the manufacturing sector), increasing thereby the labour supply elasticity of these women. Based on the approximate fraction of working time (see footnote 21) we calculate the corresponding employment elasticities of the wage cost subsidy at the intensive margin. These elasticities are reported in the last two rows of Table 2.4. Even if we do find significant effects on the number of hours worked, the employment elasticities are small, irrespectively of the considered subpopulation. On average this elasticity is 0.13 for men and 0.28 for women.

Table 2.4: Impact on the Hours Worked (Intensive Margin)

	Men				Women			
	Overall (1)	**Manufacturing Sector (2)**	**High Exit Rate (3)**	**Low Exit Rate (4)**	**Overall (1)**	**Manufacturing Sector (2)**	**High Exit Rate (3)**	**Low Exit Rate (4)**
0%-30%: ATT in *pp*	-0.6***	-0.4**	-0.9***	-0.3	-0.5	0.4	-0.9	-0.2
95% CI	[-0.9; -0.2]	[-0.8; -0.1]	[-1.4; -0.4]	[-0.7, 0.1]	[-1.2; 0.2]	[-1.2; 2.1]	[-2.0, 0.2]	[-0.9, 0.5]
Pvalue	0.001	0.018	0.001	0.137	0.131	0.612	0.119	0.556
ATT in %	-13.4%	-14.7%	-13.7%	-14.8%	-3.9%	4.6%	-4.9%	-2.2%
31%-80%: ATT in *pp*	0.0	-0.7	-0.6	0.3	-1.2**	-0.1	-0.7	-1.6***
95% CI	[-0.7; 0.7]	[-1.7; 0.3]	[-1.6, 0.4]	[-0.7, 1.3]	[-2.1; -0.2]	[-2.9; 2.7]	[-2.3, 0.9]	[-2.9, -0.4]
Pvalue	1.000	0.183	0.244	0.581	0.015	0.950	0.395	0.010
ATT in %	0.0%	-4.5%	-3.1%	2.0%	-2.6%	-0.2%	-1.4%	-3.7%
>80%: ATT in *pp*	0.6	1.1**	1.5***	0.0	1.7***	-0.3	1.6**	1.9***
95% CI	[-0.1; 1.2]	[0.1; 2.2]	[0.5, 2.4]	[-1.0, 1.1]	[0.8; 2.6]	[-3.1; 2.4]	[0.2, 3.0]	[0.8, 3.0]
Pvalue	0.103	0.036	0.002	0.951	0.000	0.812	0.025	0.001
ATT in %	0.7%	1.4%	2.0%	0.0%	4.3%	-0.6%	4.7%	4.1%
ATT total hours %	0.5%**	0.7%***	1.1%***	0.1%	1.5%***	0.6%	2.0%***	1.3%***
95% CI	[0.0; 0.9]	[0.1; 1.2]	[0.5; 1.7]	[-0.5; 0.7]	[0.7; 2.4]	[-1.5; 2.7]	[0.7; 3.3]	[0.3; 2.3]
Elasticity	0.128	0.196	0.258	0.029	0.280	0.124	0.339	0.244
N° of observations	433,373	227,946	274,285	158,982	224,284	36,457	114,298	109,896
N° of individuals	98,899	51,504	63,620	37,911	53,299	8,621	28,550	25,964

*WDiD on parallel path: impact on the intensive margin defined as number of workers with a certain working time with respect to the reference time (0-30%, 31-80%, >81%). Control group is 53.25-55.25 years old; treated group 58-59.5 years old. Effects on specific subpopulation (defined in time q) by column: (1) Overall treated group, (2) Workers in secondary sector, (3) Workers in sectoral industrial committees with an exit rate from employment above the population median (= 18%), (4) Workers in sectoral industrial committees with an exit rate below the population median. The elasticity approximates the working time elasticity of the wage cost subsidy (see footnote 21). Standard errors are obtained by a stratified bootstrap (clustering by individual) with 200 repetitions and 95% confidence intervals (CI) by assuming normality. ***: significant at 1%, **: significant at 5%, *: significant at 10%.*

Table 2.5: Impact on the Time-Credit

	Men			Women		
	Overall sample(1)	**High Exit Rate(2)**	**Low Exit Rate (3)**	**Overall sample (1)**	**High Exit Rate (2)**	**Low Exit Rate (3)**
ATT pp	-0.7***	-1.9***	0.3	-1.2***	-1.3***	-0.9*
CI	[-1.2; -0.2]	[-2.5, -1.4]	[-0.5, 1.2]	[-1.8; -0.5]	[-2.2, -0.5]	[-1.8, 0.7]
Pvalue	0.003	0.000	0.409	0.000	0.002	0.069
ATT %	-9.8%	-32.6%	4.0%	-8.0%	-10.4%	-5.5%
N observations	433,373	274,285	158,622	224,284	114,298	109,896
N clusters	98,899	63,620	37,802	53,299	28,550	25,964

*WDiD on parallel path: impact on the share of people in part-time time-credit for the overall employed sample. Control group is 53.25-55.25 years old; treated group 58-59.5 years old. Effects on specific subpopulation (defined in time q) by column: (1) Overall treated group, (2) Workers in sectoral industrial committees with an exit rate from employment above the population median (= 18%), (3) Workers in sectoral industrial committees with an exit rate below the population median. N° of observations is the sum of the number of individuals observed in each quarter of the analysis. Standard errors are obtained by a stratified bootstrap (clustering by individual) with 200 repetitions and 95% confidence intervals (CI) by assuming normality. ***: significant at 1%, **: significant at 5%, *: significant at 10%.*

2.7.3. *Effect on the Hourly Wage*

In Section 2.4 we argued that in the short run we do not expect the reduction of employers' SSC to have any impact on gross wages (net of employers' SSC) and, consequently, neither on the net wage. In Table 2.12 in Appendix 2.9.1 we report the ATT of the wage cost subsidy on gross wages. In line with this hypothesis, the gross wage elasticity of the wage cost subsidy is small and never statistically significant at the 5% level. The overall point estimates of the elasticity are -0.09 for men and 0.02 for women. This is not a consequence of a lack of precision. Even at the upper bound of the 95% confidence interval it is small, respectively 0.270 and 0.137. As for our results on the intensive margin, one could criticize these results on the grounds that they do not take the Heckman's (1974) selection bias into account. If the wage cost subsidy saved selective jobs in terms of hourly wage, then this could bias our findings. However, we did not find that the impact on the employment rate was higher for low-wage workers.

Finally, we estimate the ATT on the quarterly remuneration for the two groups of industrial committees, which comprises the three estimated effects (extensive and intensive margin and hourly wage). Estimates are positive but not statistically significant at 5% level (Table 2.10 in Appendix 2.9.1).

2.7.4. *Sensitivity Checks*

In a sensitivity analysis we considered a wider control group, containing workers who are closer to the age eligibility threshold of 58 years. This control group contains workers from age 53.25 up to 57 rather than up to 55.25 as in the baseline analysis. The impact of the wage cost subsidy is found to be robust on the hours worked and the hourly gross wage. However, the effects on employment are found to be stronger when we consider employment at the extensive margin as outcome (see summary Table 2.14-2.16 in Appendix 2.9.1). For further robustness on the extensive margin, we also use as control group the individuals at 57.75 years old and the ones between 57 and 57.75 years old.[43] As shown in Table 2.13, the estimated ATTs are even larger. This suggests that the reduction of wage costs for older workers induces employers to substitute older for younger workers. In Section 2.4.4 we indeed argued that the Canada Dry early retirement system makes it possible for firms to push younger instead of older workers into early retirement. This also implies that the baseline analysis might overestimate the net effect of the wage cost subsidy on the employment rate, since at least part of the positive effects would then come at the expense of employment of younger workers.

Finally, as robustness test we estimate the ATTs by using the simple DiD estimator without controlling for the covariates. The results are qualitative similar, although generally somewhat larger. On the extensive margin we again do not find a significant effect on the overall employment rate. The ATT on the employment retention of men working in committees at high risk of exiting is found to be +4.9 *pp*, instead of +2.2 *pp* in the benchmark analysis. On the intensive margin, the estimates by DiD are still small though larger for men (+1.4% versus +0.5% estimated by WDiD), while they are not statistically significant for women. This demonstrates that it matters to condition the DiD on observed covariates.

2.7.5. *Cost-Benefit Analysis (CBA)*

To get an idea of the welfare effects, we report a CBA for two scenarios. We calculate the effect of the subsidy on three indicators, all measured per job saved by the policy. The procedure to estimate these effects is described more in details in Appendix 2.9.2.

(i) *Gross budgetary cost:* average cost of the subsidy for the state before any behavioural impact;

(ii) *Net budgetary cost*: average cost of the subsidy for the state, net of savings for the public budget, i.e. tax revenues (income tax and contributions to social security) generated by the jobs that were saved by the policy,[44] as well as the outlays that the government would have spent in the absence of the policy, mostly on allowances to the early retirees (at the extensive margin) or to participants in the time-credit scheme (at the intensive margin);[45]

(iii) *Welfare gain (or cost) for society:* the gain in terms of the production generated by these saved jobs minus the opportunity cost of working and the efficiency cost ("excess burden" or "deadweight loss") of the net budgetary expenditures mentioned in (ii). Similarly to Greenberg and Robins (2008), we lower bind the opportunity cost of working to zero and upper bind to the

[43] In Table B.11 of the Supplementary Appendix B, we report the estimated ATTs by using as treated and control groups the individuals at the margin of the age-eligibility (57.75 versus 58). Estimates are positive but not statistically significant likely because the number of observations is much smaller: about 15% of the baseline analysis.

[44] Since information on personal income taxes is missing in our data, we approximate this by multiplying the gross remuneration by 0.287, i.e. the average income tax in 2002 (OECD, 2007).

[45] We ignore allowances for those entering statutory early retirement, sickness and disability and tax revenues from self-employed workers. However, since only 1.1% of the relevant sub-sample is entitled to these allowances and only 0.9% becomes self-employed, this only introduces a slight bias.

remuneration (we also include the SSC). As baseline scenario, we use the mid-point between the two bounds, which are later considered in a sensitivity analysis.[46] To calculate the efficiency cost of the net budgetary expenditures we multiply the effect on government budget by the Marginal Cost of Public Funds (MCF) minus one.[47] For Belgium a MCF equal to two is considered to be appropriate (Kleven and Kreiner 2006; Barrios Cobos *et al.* 2013). The value of production is adjusted for the pay-productivity gap. As Vandenberghe *et al.* (2013) estimate this gap for the age class 58-59.5 to be 15.7%, their average labour cost is multiplied by 0.843.

We follow Staubli and Zweimüller (2013) by estimating the two building blocks of these indicators (the net budgetary cost for the state and the saved production) in exactly the same way as we estimate the baseline ATT for the outcome variables of interest, i.e. by a trend-adjusted WDiD on the values of these indicators for each individual retained in the analysis.[48] The estimates obtained only consider the average effect on the period of analysis (1.25 years) and the population between 58-59.5 years old. We do this analysis separately for the workers in sectoral industrial committees with a high and a low risk of exiting employment and subsequently aggregate up the results. In the CBA we ignore a number of components, because they are difficult to measure. We overestimate the benefit for society by (i) not taking into account that the estimated employment effects may be biased upwards through substitution of individuals in the control group by eligible older workers (Section 2.4.4 and 2.7.4), and (ii) not taking into account that in about 43% of the industrial committees firms using early-retirement schemes are obliged to replace early retirees by unemployed workers (Section 2.2 and 2.4.4). We also ignore the potential distributional, health impacts and effects on the individuals after 59.5 years old. It is *a priori* unclear in which direction the latter neglect biases our CBA.

In Table 2.6 we report the outcome of our CBA. We perform the CBA for two cases. In the first case the subsidy is, as in reality, granted both to the high and the low-exit sectoral industrial committees. In the second hypothetical case the subsidy is targeted to the high-exit sectoral industrial committees. Overall the wage cost subsidy targeted to older workers is clearly an expensive policy that fails the cost-benefit test. The gross cost per saved job is nearly €6,000 and the net cost remains as high as €3,000. The net cost to society is €3,700. Since, as higher mentioned, substitution effects have been ignored in the calculations, this is likely a lower bound. For women we find a lower net cost of €1,150 for society. This is because the point estimate of the ATT of the subsidy on the employment rate is higher for women than for men (Table 2.2). However, this finding should be interpreted cautiously, since this ATT was not statistically significantly different from zero.

[46] Concerning the treated, the median of the opportunity cost is about € 1,455 and €782 for men and women.
[47] The net budgetary cost of the policy is not, *in se,* a cost to society, since it just involves transfers between individuals.
[48] Staubli and Zweimüller (2013) only consider the effect on the budgetary cost.

Our findings strongly suggest that effectiveness could be enhanced if the target group of the subsidy could be further narrowed down to workers at high risk of exit to early retirement. Indeed in case the subsidy would have been targeted to high-exit sectoral industrial committees, the gross and net cost of the subsidy would only have been about €2,800 and €90, while society would gain about €410 per saved job. The policy might have been self-financing and positive for the society if it would have been targeted it, though substitution effects might still lead to an over-estimation. However, such a targeting may breach the EU fair competition law. Furthermore, raising the early retirement age seems a much cheaper alternative policy measure to achieve the same effect (see e.g. Staubli and Zweimüller, 2013). However, this assumes that the employer will not dismiss workers if the option of early retirement is no longer available. Due to the important pay-productivity gap for older workers in Belgium, this is a strong assumption. In addition, delaying the age of early retirement increases the cost of using deferred compensation schemes as incentive device (Lazear, 1979). In sum, further research is required to determine the best strategy for retaining older workers in the labour force.

Finally, we perform some sensitivity tests by variating several factors. First, we raise the average income tax to 32.8% (workers at 133% of the average wage – OECD, 2007). Second, we vary the opportunity cost of working to the lower and upper bound. Finally, we fix the MCF to 1.41 (lower bound) or 3.23 (upper bound - Kleven and Kreiner 2006). The first and the third quartile of the estimated distribution are shown in Table 2.6, which show some degree of variation in the magnitude of the estimates. The full distribution is reported in Table 2.17 in Appendix 2.9.1.

Table 2.6: Cost-Benefit Analysis

	2002 subsidy (1)					
	Overall		Men		Women	
	Baseline (A)	1-3 quartile (B)	Baseline (A)	1-3 quartile (B)	Baseline (A)	1-3 quartile (B)
Gross budgetary cost per saved job	€5,993	-	€6,617	-	€4,673	-
Net budgetary cost per saved job	€2,974	€2,858; €2,974	€3,861	€3,718; €3,861	€1,097	€1,039; €1,097
Net cost to society per saved job	€3,672	€2,223; €5,211	€4,939	€3,282; €7,654	€1,145	€291; €2,093

	Targeted subsidy (2)					
	Overall		Men		Women	
	Baseline (A)	1-3 quartile (B)	Baseline (A)	1-3 quartile (B)	Baseline (A)	1-3 quartile (B)
Gross budgetary cost per saved job	€2,805	-	€3,204	-	€1,961	-
Net budgetary cost per job	€89	€9; €89	€425	€348 ; €425	-€622	-€709 ; -€622
Net cost to society per saved job	-€411	-€1,409; €547	€123	-€802; €1,028	-€1.626	-€2,541; -€1,172

Monthly costs (benefits if negative) in 2004 euros per saved job. (1) Cost-benefit analysis for the SSC exemption on all the individuals (58-59.5 years old) from the second quarter of 2002 until the second quarter of 2003; (2) Cost-benefit analysis in the counterfactual of a policy targeting the subsidy to the high-exit flow sectoral industrial committees. Baseline scenario is reported in column (A), while column (B) show the 1st and the 3rd quartile of the estimated distribution in the sensitivity analysis. Control units are 53.25-55.25 years old. Gross cost per saved job is the total cost of the wage cost subsidy divided by the number of saved jobs. Net cost per saved job is estimated by a trend-adjusted WDiD on the individual net budgetary gross cost of the policy divided by the number of saved jobs. The net cost to society is the efficiency cost of public funds minus the value of the welfare generated by the jobs that were saved by reduction in SSC. The latter is estimated by trend-adjusted WDiD on the individual gross wage costs taking into account for the *pay-productivity gap and the opportunity cost of working. The CBA ignores potential substitution and anticipation effects. The costs to society ignore: the potential distributional and health impacts of the measure, and the potential benefits in the absence of the subsidy that some of the early retirees would be replaced by unemployed workers.*

2.8. Conclusion

This paper studies the effect of a wage subsidy targeted to older workers on employment, working time and hourly wage. In the last years several OECD countries have implemented such measures to postpone the retirement of older workers. However, evidence on the effectiveness of such policies is scarce. Our research provides new evidence by estimating the impact of an automatic €400 quarterly reduction of employers' Social Security Contributions (SSC) introduced in Belgium in 2002.

Based on a large sample of 243,655 older individuals for whom we could observe using administrative registers the labour market history from as early as 1957, we estimate the effect of this wage cost subsidy on the employment rate, working time and hourly wage. In order to enhance the precision of our estimates, the sample was endogenously stratified such that entries in and exits from private sector employment were over-represented. By comparing the evolution of outcome variables for eligible older workers to non-eligible younger workers we could identify the Average Treatment effect on the Treated (ATT) of this policy measure. More precisely, we implemented a *Conditional Difference-in-Differences* (CDiD) estimator by Inverse Probability Weighting (IPW) which we adjusted to accommodate for the endogenous sampling. In addition, to allow for an age related shock at 58, induced by the legal possibility of entering early retirement, we used this weighted DiD estimator in a multiple repeated cross-sections framework, where we kept the age fixed over time rather than the individual, as in a panel framework. We conducted placebo tests on pre-treatment data and, in case of rejection, proposed a parametric trend-adjusted weighted DiD estimator.

In line with the scarce literature, we find that the wage cost subsidy has only a small impact on employment, both at the intensive and extensive margin and that the hourly gross wage is not significantly affected. The different results on the extensive margin with respect to Huttunen *et al.* (2013) might be explained by the distinct features of the Belgian subsidy, such as nearly 100% take-up rate, the announcement as a permanent subsidy and granting exemptions to all the older workers working more than 30% of a full time (in Finland the subsidy was granted only to full time lower wage workers). As predicted by our theoretical model, the subsidy seems to be more effective for workers at risk of leaving the labour market and enter early retirement. For men the subsidy significantly increased the retention rate in employment for the latter group by 2.2 pp, while for women the effect was even larger but not statistically different from zero. For the intensive margin we find a small impact of the wage subsidy on working time. For the overall population the implied elasticity are 0.13 for men and 0.28 for women. Finally, we do not find any evidence of an impact on the wage cost. Nevertheless, our results require some qualification, since we find evidence that it may partly reflect a substitution of older for younger workers. Overall, the policy reform does not pass the cost-benefit test. If the subsidy could have been targeted at sectoral industrial committees in which the risk of early retirement is high, then this conclusion may be reversed. However, it is not clear whether there is a legal basis for such refinement of the target population. In addition, increasing the age of early retirement seems to be a cheaper policy alternative that could achieve similar effects. But this hinges on the assumption that employers would then not dismiss these workers. Whether this assumption is realistic may depend on the institutional context and is matter for future research.

2.9. Appendix

2.9.1. Tables and Figures

Table 2.7: Conditioning Variables in the Data
(at the moment that the outcome is measured, unless specified otherwise)

Province of residence	11 Provinces (*at end of preceding year*)
Nationality	Belgian, European Union (EU), other
Household Characteristics	Single, single with children, couple, couple with children, other
	Size of the household: 1, 2, 3, 4 5+
	Presence of members below 18 or above 65 years old (*seven quarters earlier*)
Labour Market Characteristics *seven quarters earlier*	Inactive, unemployed, public sector[‡], self-employed, civil servant
	Elapsed unemployment duration in months
	Actual working time in classes 0-30%, 31-81%, 80-100%
	Sector dummies (Regrouped at 1 or 2 digit level)
	Participation in Time Credit (Yes/No)
	Unemployment benefit level (€[*]/month)
	Working time regime: Full-time, Part-time or special
	Firm size: <5, 5-9, 10-19, 20-49, 50-99, 100-199, 200-499, 500-999, 1000+
Employment History as Salaried Worker (Excluding Civil Servants and Employees in the Local Public Sector)[†]	Experience (in quarters) during: *Older history* (*between 28 and 5.5 years before);* *Recent history* (*between 11 and 7 quarters before)*
	Never employed during: *Older history* (*between 28 and 5.5 years before);* *Recent history* (*between 11 and 7 quarters before)*
	Number of times received a severance payment during: *Older history* (*between 28 and 5.5 years before);* *Recent history* (*between 11 and 7 quarters before)*
	Share of time worked as blue-collar during: *Older history* (*between 28 and 5.5 years before);* *Recent history* (*between 11 and 7 quarters before)*
	Average share of working during: *Older history* (*between 10.75 and 5.5 years before);* *Recent history* (*between 11 and 7 quarters before)*
	Average full-time quarterly gross earnings from salaried employment (in €[*]): *Recent history* (*between 11 and 7 quarters before)*

[*] In constant 2004 euros, based on the Consumer Price Index.
[†] The employment history excludes the career spent as self-employed, civil servant, and temporary employee in the local public sector, comprising the provincial and municipal authorities. Temporary employees in working in the central public administration are included. Since the older history is based on a different source (the pension registrations) than the more recent history (the payroll-tax-administration), it was not possible to define a consistent labour market history between 12 to 21 quarters prior to the measurement of the outcome. This labour market history is therefore missing.
[‡] Public sector comprises both civil servants and temporary salaried workers in the public sector.

Table 2.8: Descriptive Statistics for the Treatment and Control Groups in the Baseline Model

		Men			Women		
		Mean		t-test mean equality	Mean		t-test mean equality
	Variable	Treated	Control	p>|t|	Treated	Control	p>|t|
Province	Brussels	7.67%	7.13%	0.099	8.42%	8.38%	0.936
	Antwerp	17.06%	16.10%	0.033	16.94%	16.65%	0.646
	Flemish Brabant	10.64%	10.50%	0.719	10.53%	10.56%	0.952
	West Flanders	12.94%	10.94%	0.000	12.17%	10.60%	0.003
	East Flanders	14.50%	13.43%	0.012	13.91%	13.58%	0.571
	Limburg	8.11%	8.04%	0.849	7.62%	7.20%	0.342
	Walloon Brabant	3.75%	3.69%	0.804	3.55%	3.90%	0.248
	Hainaut	11.26%	13.31%	0.000	11.04%	12.95%	0.000
	Liège	8.54%	10.27%	0.000	9.89%	10.12%	0.636
	Luxembourg	1.70%	2.20%	0.008	1.93%	1.83%	0.679
	Namur	3.84%	4.39%	0.035	4.03%	4.23%	0.540
Nationality	Belgian	92.61%	92.59%	0.948	94.14%	94.52%	0.330
	EU	6.03%	6.28%	0.405	4.54%	4.22%	0.342
	Others	1.36%	1.13%	0.111	1.32%	1.26%	0.783
Household composition	Single	12.55%	12.82%	0.526	13.08%	11.00%	0.000
	Single with children	1.67%	2.46%	0.000	5.39%	7.58%	0.000
	Couple	31.58%	45.77%	0.000	23.75%	36.58%	0.000
	Couple with children	48.66%	33.15%	0.000	48.78%	39.15%	0.000
	other	5.54%	5.81%	0.384	9.00%	5.70%	0.000
Household size	1	13.25%	13.32%	0.868	13.89%	10.70%	0.000
	2	45.08%	29.90%	0.000	50.55%	39.42%	0.000
	3	24.66%	27.71%	0.000	23.30%	28.75%	0.000
	4	11.03%	19.39%	0.000	8.59%	14.81%	0.000
	5+	5.98%	9.69%	0.000	3.67%	6.32%	0.000
Household members	Below18 years old	9.01%	19.15%	0.000	4.55%	11.57%	0.000
	Above 65 years old	5.11%	5.97%	0.003	8.58%	6.02%	0.000
Primary status 7 q earlier	Private sector employee	25.42%	37.61%	0.000	10.25%	17.70%	0.000
	Inactive	38.60%	21.23%	0.000	65.93%	54.05%	0.000
	Unemployed	1.70%	2.06%	0.018	2.42%	3.33%	0.000
	Public sector employee	17.61%	22.55%	0.000	14.70%	18.74%	0.000
	Self-employed	16.67%	16.56%	0.846	6.70%	6.18%	0.223
	Civil Servant	15.50%	19.19%	0.000	11.77%	13.90%	0.000
Unemployment	Unemployment duration (months)	73.03	62.33	0.000	87.4	72.9	0.000
	Unempl. benefit level (€/month)	€834	€705	0.000	€660	€597	0.000
Actual working time	1%-30%	3.26%	2.44%	0.000	11.40%	9.00%	0.000
	31%-80%	14.53%	12.27%	0.000	43.59%	43.01%	0.224
	81%-100%	82.21%	85.29%	0.000	45.02%	47.99%	0.000
Sector (Nace Rev. 1.)	Nace 1-19	4.27%	4.45%	0.178	-	-	-
	Nace 20-29	12.68%	15.75%	0.000	-	-	-
	Nace 30-39	3.96%	5.87%	0.000	-	-	-
	Nace 40-49	9.35%	9.12%	0.396	-	-	-
	Nace 50-59	11.01%	9.77%	0.000	11.33%	12.83%	0.000
	Nace 60	4.54%	6.08%	0.000	-	-	-
	Nace 61-64	4.79%	5.53%	0.070	-	-	-
	Nace 65-69	5.16%	5.48%	0.097	-	-	-
	Nace 70-74	5.24%	5.03%	0.231	5.86%	6.85%	0.000
	Nace 75-79	18.26%	17.85%	0.604	15.56%	17.56%	0.080
	Nace 80-84	15.19%	9.69%	0.000	34.74%	26.54%	0.000
	Nace 85-89	2.80%	3.26%	0.048	14.51%	15.37%	0.270
	Nace 90-99	2.75%	2.13%	0.006	4.11%	3.90%	0.520
	Nace 1-49	-	-	-	7.11%	9.66%	0.000
	Nace 60-69	-	-	-	6.78%	7.28%	0.290
Career break	Career break	2.77%	2.08%	0.000	9.16%	11.26%	0.000
Working time	Full-time	92.04%	93.82%	0.000	63.72%	54.15%	0.000
	Part-time	7.37%	5.60%	0.000	35.11%	44.32%	0.000
	Special time	0.59%	0.58%	0.810	1.17%	1.53%	0.030
Firm size	<5	5.10%	4.31%	0.000	7.71%	7.71%	0.990
	5-9	4.05%	3.83%	0.100	4.09%	4.32%	0.410
	10-19	5.34%	5.00%	0.030	4.46%	4.43%	0.940
	20-49	9.38%	9.06%	0.180	5.68%	6.65%	0.010

	50-99	6.09%	6.17%	0.700	3.83%	4.80%	0.010
	100-199	6.65%	7.06%	0.160	4.28%	5.14%	0.030
	200-499	9.91%	9.98%	0.860	7.00%	8.38%	0.020
	500-999	6.76%	7.20%	0.220	6.18%	6.94%	0.180
	1000+	46.74%	47.41%	0.370	56.80%	51.64%	0.000
Experience (in quarters)	*28 - 5.5 years before*	52.44	55.145	0.000	26.711	33.723	0.000
	2.75 - 1.75 years before	1.4027	2.0366	0.000	0.6293	1.0622	0.000
Never employed	*28 - 5.5 years before*	22.45%	17.06%	0.000	43.10%	29.19%	0.000
	2.75 - 1.75 years before	69.06%	56.47%	0.000	86.05%	76.57%	0.000
Severance payment (times)	*28 - 5.5 years before*	0.4797	0.4576	0.130	0.3578	0.3223	0.030
	2.75 - 1.75 years before	0.0058	0.0043	0.100	0.0059	0.0044	0.300
Share of time blue-collar	*28 - 5.5 years before*	55.80%	54.98%	0.190	49.58%	45.72%	0.000
	2.75 - 1.75 years before	45.67%	52.08%	0.000	39.50%	37.25%	0.020
Working time	*10.75 - 5.5 years before*	60.08%	59.87%	0.740	33.61%	36.25%	0.000
	2.75 - 1.75 years before	85.25%	87.17%	0.000	66.29%	68.50%	0.000
Full-time quarterly earnings	*2.75 - 1.75 years before*	€9,550	€8,929	0.000	€6,497	€6,552	0.380
N observation	-	298,337	460,240	-	150,897	293,941	-
N individuals (clusters)	-	65,908	86,107	-	33,432	55,403	-

Descriptive statistics for the units in the baseline model (i.e. treated 58-59.5 years old; control 53.25-55.25 years old). The reported means are weighted by the sampling weights defined in Equation (2) in the main text. In this descriptive analysis values of variables are set to missing, and hence ignored in the calculation of the sample means, if the value is not known for this individual (e.g. the wage for people not employed or the unemployment benefit if not unemployed). In the CDiD analysis the values of these variables are set to zero instead. The p-values reported are for the t-tests of equality of means allowing for individual serial correlation (cluster robust SEs).

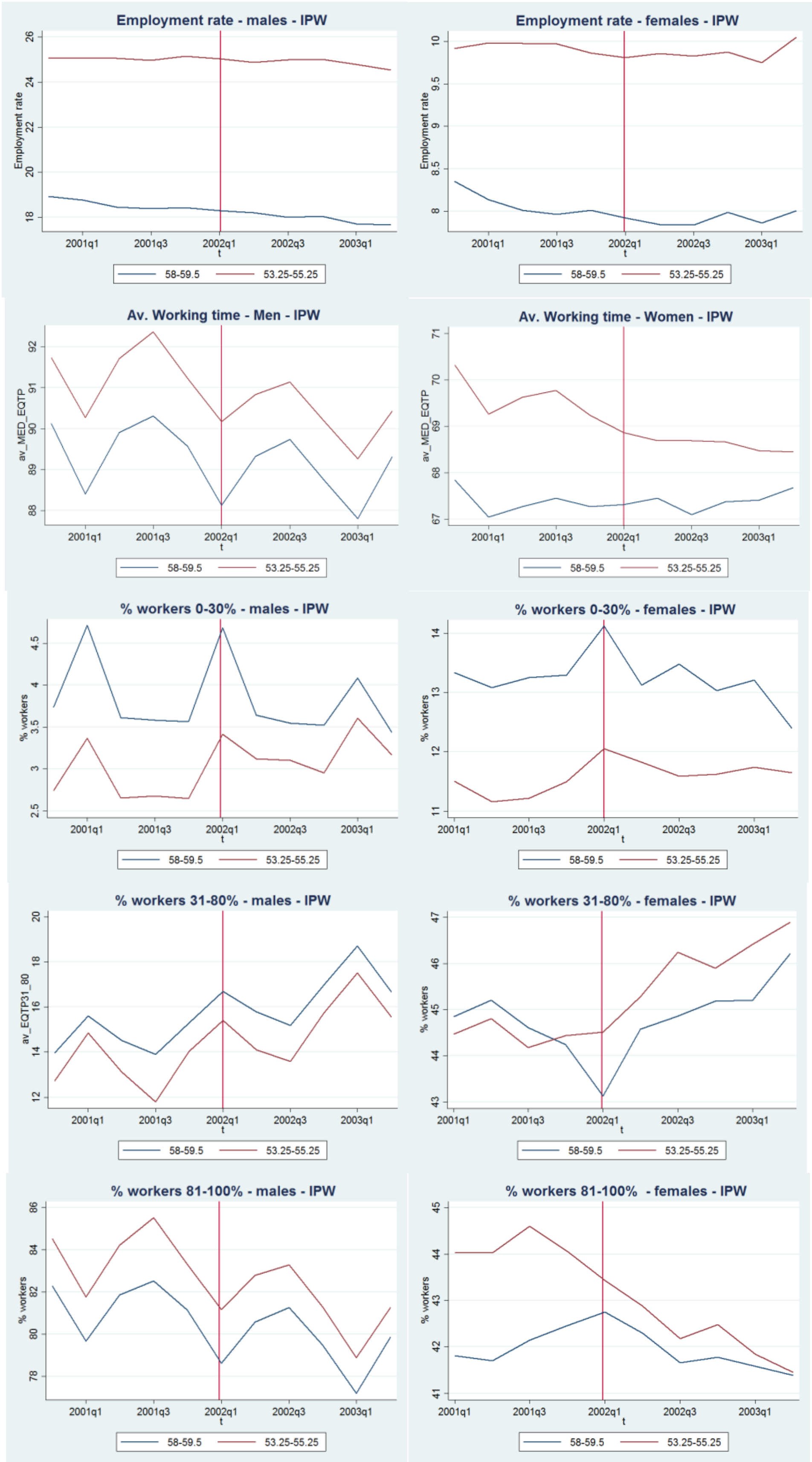
Table 2.9: Impact on the Employment/Retention Rate (Extensive Margin)

Men	**Already Working (1)**	**Low Wage (2)**	**Service Sector (3)**	**Large Firms (4)**	**Small Firms (5)**	**Blue-collar (6)**	**White-collar (7)**
ATT in *pp*	1.5	0.9	0.6	2.4	-1.0	0.3	1.7
95% CI	[-0.4; 3.4]	[-1.3; 3.1]	[-2.1; 3.3]	[-1.2; 6.1]	[-3.3; 1.3]	[-1.9; 2.6]	[-0.6; 4.0]
Pvalue	0.113	0.436	0.655	0.190	0.413	0.774	0.140
ATT %	2.3%	1.6%	0.8%	4.0%	-1.4%	0.6%	2.3%
N° of observations	538,932	316,951	230,980	295,058	242,773	301,283	237,546
N° of individuals	116,859	71,764	51,735	64,252	55,541	65,885	51,552
Subsidy/Labor cost	3.6%	5.0%	3.8%	2.9%	4.4%	5.0%	2.8%
Elasticity	0.647	0.311	0.217	1.384	-0.321	0.125	0.824
Semi-Elasticity in *pp*	0.414	0.176	0.161	0.842	-0.221	0.066	0.604

Women	**Already Working (1)**	**Low Wage (2)**	**Service Sector (3)**	**Large Firms (4)**	**Small Firms (5)**	**Blue-collar (6)**	**White-collar (7)**
ATT in *pp*	0.0	-0.2	-0.6	0.8	-0.9	-0.8	0.8
95% CI	[-2.8; 2.8]	[-4.7; 4.2]	[-3.6; 2.4]	[-3.0; 4.6]	[-4.7; 2.9]	[-6.2; 4.6]	[-2.3; 3.9]
Pvalue	0.990	0.921	0.683	0.683	0.633	0.780	0.593
ATT %	0.3%	-0.3%	-0.8%	1.3%	-1.2%	-1.2%	1.2%
N° of observations	284,075	140,967	228,586	139,503	144,524	96,907	187,088
N° of individuals	61,923	33,543	50,034	30,435	33,064	22,502	39,780
Subsidy/Labor cost	5.4%	7.6%	5.5%	4.9%	6.0%	7.7%	4.6%
Elasticity	0.005	-0.044	-0.155	0.258	-0.201	-0.156	0.254
Semi-Elasticity in *pp*	0.003	-0.030	-0.114	0.162	-0.152	-0.100	0.183

*WDiD on parallel growths: impact on the employment rate. Control group is 53.25-55.25 years old; treated group 58-59.5 years old. Effects on specific subpopulation (defined in q-7) by column: (1) Workers in the salaried private sector, (2) Workers earning less than the treated group median wage, (3) Workers in tertiary sector, (4) Workers in firms with at least 200 employees, (5) Workers in firms with less than 200 employees, (6) Blue-collar workers, (7) White-collar workers. Point estimates of the ATT are expressed in percentage points (pp), in proportional (%) changes in the employment rate and in terms of (semi-)elasticity, i.e. the proportional (pp) effect on the employment rate of a proportional reduction in the labour costs. N° of observations is the sum of the number of individuals observed in each quarter of the analysis. Standard errors are obtained by a stratified bootstrap (clustering by individual) with 200 repetitions and 95% confidence intervals (CI) by assuming normality. ***: significant at 1%, **: significant at 5%, *: significant at 10%.*

Figure 2.6: Parallel path and Trend: outcome variables of the overall sample after IPW weights



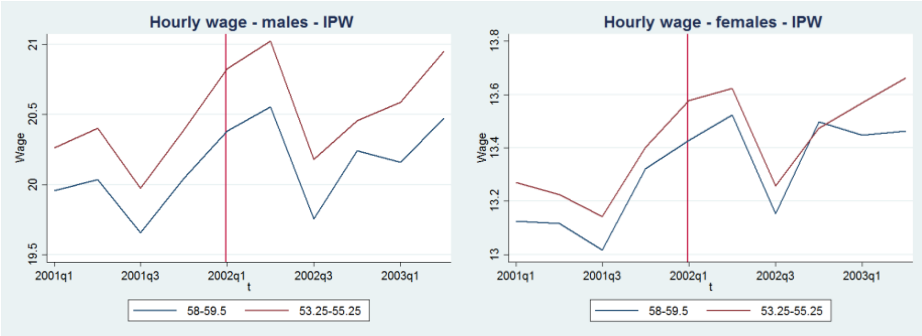


Figure 2.7: Parallel Trend: Employment of High Exit Rate Committee after IPW weights

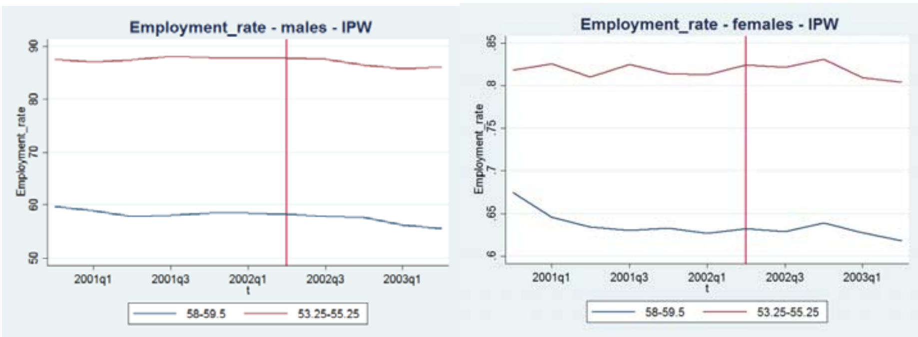


Figure 2.8: Parallel Trend: Employment of Low Exit Rate Committee after IPW weights

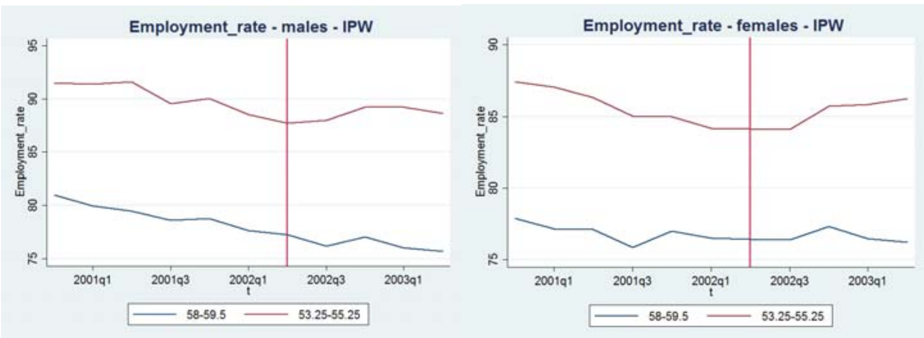


Table 2.10: Impact on the quarterly remuneration: control group 53.25-55.25 years old

		Men		Women	
		High Exit Rate(1)	**Low Exit Rate(2)**	**High Exit Rate (1)**	**Low Exit Rate(2)**
(A) Parallel path	**ATT in *pp***	82.1	160.3	-86.3*	-1.9
	95% CI	[-71, 235]	[-72, 393]	[-176, 3]	[-119, 115]
	Pvalue	0.294	0.177	0.060	0.974
	ATT %	2.0%	1.9%	-3.0%	-0.0%
(B) Placebo	**ATT in *pp***	-133.0*	12.3	-112.0**	16.3
	Pvalue	0.099	0.926	0.010	0.749
(C) Parallel growths	**ATT in *pp***	123.9	138.0	190.5*	-62.3
	95% CI	[-131, 379]	[-431, 707]	[-17, 398]	[-278, 154]
	Pvalue	0.341	0.634	0.071	0.572
	ATT %	3.0%	1.6%	7.4%	-1.5%
	N° of obs.	349,438	189,368	147,600	136,450
	N° of indiv.	77,269	43,599	33,538	30,178

*WDiD on (A) parallel path, (C) parallel growths: impact on the quarterly remuneration. (B) Placebo test on parallel path. Control group is 53.25-55.25 years old; treated group 58-59.5 years old. Effects on specific subpopulation (defined in q-7) by column: (1) Workers in sectoral industrial committees with an exit rate from employment above the population median (= 18%), (2) Workers in sectoral industrial committees with an exit rate below the population median. Point estimates of the ATT are expressed in percentage points (pp), in proportional (%) changes in the employment rate. N° of observations is the sum of the number of individuals observed in each quarter of the analysis. Standard errors are obtained by a stratified bootstrap (clustering by individual) with 200 repetitions and 95% confidence intervals (CI) by assuming normality. ***: significant at 1%, **: significant at 5%, *: significant at 10%*

Table 2.11: Impact on the Hours Worked (Intensive Margin): control group aged 53.25-55.25

Men	Low Wage (1)	Service Sector (2)	Large Firms (3)	Small Firms (4)	Blue-collar (5)	White-collar (6)
0%-30%: ATT in *pp*	-0.5**	-0.6***	-0.4***	-0.6**	-1.1***	-0.1
95% CI	[-1.0; -0.1]	[-1.0; -0.2]	[-0.7; -0.2]	[-1.1; -0.1]	[-1.8; -0.4]	[-0.3; 0.2]
Pvalue	0.029	0.005	0.002	0.016	0.002	0.626
ATT in %	-9.1%	-12.7%	-21.7%	-9.8%	-12.3%	-4.8%
31%-80%: ATT in *pp*	-0.3	0.1	0.1	-0.3	0.1	-0.4
95% CI	[-1.3; 0.7]	[-0.9; 1.1]	[-0.8; 1.0]	[-1.2; 0.7]	[-1.0; 1.3]	[-1.3; 0.5]
Pvalue	0.576	0.790	0.803	0.569	0.824	0.410
ATT in %	-1.4%	0.8%	0.8%	-1.4%	0.6%	-2.8%
>80%: ATT in *pp*	0.8*	0.4	0.3	0.9*	1.0*	0.4
95% CI	[-0.1; 1.8]	[-0.5; 1.4]	[-0.7; 1.3]	[-0.1; 1.9]	[-0.2; 2.1]	[-0.4; 1.3]
Pvalue	0.088	0.366	0.518	0.073	0.099	0.328
ATT in %	1.1%	0.6%	0.4%	1.2%	1.4%	0.5%
ATT total hours %	0.6%*	0.4%	0.2%	0.7%**	1.0%**	0.2%
95% CI	[0.0; 1.2]	[-0.2; 1.0]	[-0.3; 0.8]	[0.1; 1.3]	[0.2; 1.8]	[-0.3; 0.6]
Elasticity	0.121	0.107	0.099	0.158	0.194	0.082
N° of observations	266,378	200,287	231,979	201,387	234,631	198,729
N° of individuals	61,784	47,250	53,673	48,140	53,619	45,758

Women	Low Wage (1)	Service Sector (2)	Large Firms (3)	Small Firms (4)	Blue-collar (5)	White-collar (6)
0%-30%: ATT in *pp*	-0.5	-0.7*	-0.8**	-0.4	-0.8	-0.3
95% CI	[-1.5; 0.6]	[-1.4; 0.0]	[-1.7; 0.0]	[-1.4; 0.5]	[-2.2; 0.6]	[-0.9; 0.3]
Pvalue	0.369	0.066	0.046	0.396	0.279	0.372
ATT in %	-2.5%	-5.0%	-12.5%	-2.2%	-2.7%	-4.6%
31%-80%: ATT in *pp*	-1.9**	-1.2**	-1.9**	-0.8	0.0	-1.7***
95% CI	[-3.4; -0.3]	[-2.4; -0.1]	[-3.3; -0.4]	[-2.1; 0.6]	[-1.9; 1.8]	[-2.9; -0.4]
Pvalue	0.022	0.037	0.011	0.254	0.965	0.009
ATT in %	-3.5%	-2.6%	-3.6%	-1.9%	-0.1%	-3.6%
>80%: ATT in *pp*	2.3***	1.9***	2.7***	1.2**	0.8	1.9***
95% CI	[1.0; 3.7]	[0.9; 2.9]	[1.4; 4.1]	[0.0; 2.4]	[-0.7; 2.3]	[0.8; 3.0]
Pvalue	0.001	0.000	0.000	0.047	0.275	0.001
ATT in %	8.5%	4.9%	6.6%	3.1%	3.7%	3.9%
ATT total hours %	2.3%***	1.7%***	2.1%***	1.4%**	2.4%**	1.1%***
95% CI	[1.1; 3.5]	[0.9; 2.5]	[1.0; 3.1]	[0.2; 2.5]	[0.5; 4.3]	[0.3; 1.9]
Elasticity	0.302	0.302	0.416	0.219	0.308	0.220
N° of observations	104,255	184,959	107,646	116,432	74,355	149,923
N° of individuals	26,224	44,006	25,782	28,568	18,692	34,863

*WDiD on parallel path: impact on the intensive margin defined as number of workers with a certain working time with respect to the reference time (0-30%, 31-80%, >81%). Control group is 53.25-55.25 years old; treated group 58-59.5 years old. Effects on specific subpopulation (defined in time q) by column: (1) Workers earning less than the treated group median wage, (2) Workers in tertiary sector, (3) Workers in firms with at least 200 employees, (4) Workers in firms with less than 200 employees, (5) Blue-collar workers, (6) White-collar workers. The elasticity approximates the working time elasticity of the wage cost subsidy (see footnote 21). N° of observations is the sum of the number of individuals observed in each quarter of the analysis. Standard errors are obtained by a stratified bootstrap (clustering by individual) with 200 repetitions and 95% confidence intervals (CI) by assuming normality. ***: significant at 1%, **: significant at 5%, *: significant at 10%.*

Table 2.12: Impact on the Hourly Gross Wage: control group 53.25-55.25 years old

Men	Overall (1)	Low wage (2)	Manufacturing Sector (3)	Service sector (4)	High exit rate (5)	Low exit rate (6)	Large firms (7)	Small firms (8)	Blue-collar (9)	White-collar (10)
ATT in €	-0.07	0.05*	-0.02	0.19	-0.07	0.11	0.05	0.09	0.02	-0.04
95% CI	[-0.32; 0.19]	[-0.01; 0.11]	[-0.35; 0.30]	[-0.11; 0.49]	[-0.25, 0.11]	[-0.25, 0.48]	[-0.24; 0.33]	[-0.17; 0.34]	[-0.05; 0.09]	[-0.39; 0.30]
Pvalue	0.604	0.089	0.891	0.209	0.454	0.548	0.747	0.513	0.569	0.799
ATT in %	-0.3%	0.4%	-0.1%	1.0%	-0.4%	0.5%	0.2%	0.5%	0.2%	-0.2%
N° of observations	389,986	240,111	203,832	181,563	247,176	142,709	208,084	181,897	211,426	178,456
N° of individuals	92,630	57,941	48,093	44,332	59,644	35,287	50,129	44,757	50,349	42,664
Elasticity	-0.093	0.087	-0.031	0.249	-0.095	0.159	0.066	0.119	0.034	-0.063
Elasticity Upper CI	0.270	0.166	0.412	0.666	0.155	0.687	0.470	0.421	0.153	0.426

Women	Overall (1)	Low wage (2)	Manufacturing Sector (3)	Service sector(4)	High exit rate (5)	Low exit rate (6)	Large firms (7)	Small firms (8)	Blue-collar (9)	White-collar (10)
ATT in €	0.01	0.01	0.24*	-0.03	0.04	-0.01	-0.04	0.09*	0.03	-0.06
95% CI	[-0.07; 0.10]	[-0.05; 0.08]	[-0.03; 0.51]	[-0.12; 0.05]	[-0.08, 0.15]	[-0.12, 0.11]	[-0.18; 0.10]	[-0.01; 0.20]	[-0.06; 0.11]	[-0.17; 0.05]
Pvalue	0.750	0.697	0.081	0.436	0.552	0.912	0.547	0.090	0.545	0.313
ATT in %	0.1%	0.1%	1.7%	-0.3%	0.3%	-0.0%	-0.3%	0.8%	0.3%	-0.4%
N° of observations	223,918	104,112	36,274	184,691	114,131	109,697	107,409	116,305	74,172	149,650
N° of individuals	53,222	26,186	8,587	43,939	28,498	25,937	25,733	28,534	18,623	34,808
Elasticity	0.018	0.018	0.356	-0.046	0.049	-0.009	-0.055	0.124	0.036	-0.077
Elasticity Upper CI	0.137	0.140	0.755	0.135	0.210	0.147	0.131	0.274	0.156	0.073

*WDiD on parallel path: impact on the hourly gross wage. Control group is 53.25-55.25 years old; treated group 58-59.5 years old. Effects on specific subpopulation (defined in time q) by column: (1) Overall treated group, (2) Workers earning less than the treated group median wage, (3) Workers in secondary sector, (4) Workers in tertiary sector, (5) Workers in sectoral industrial committees with an exit rate from employment above the population median (= 18%), (6) Workers in sectoral industrial committees with an exit rate below the population median, (7) Workers in firms with at least 200 employees, (8) Workers in firms with less than 200 employees, (9) Blue-collar workers, (10) White-collar workers. N° of observations is the sum of the number of individuals observed in each quarter of the analysis. Standard errors are obtained by a stratified bootstrap (clustering by individual) with 200 repetitions and 95% confidence intervals (CI) by assuming normality. ***: significant at 1%, **: significant at 5%, *: significant at 10%.*

Table 2.13: Impact on the Extensive Margin – control group 57.75 or 57-57.75

	Treated 58-59.5, Control 57.75 (A)						Treated 58-59.5, Control 57-57.75 (B)					
	Men			Women			Men			Women		
	Overall (1)	High (2)	Low (3)	Overall (1)	High (2)	Low (3)	Overall (1)	High (2)	Low (3)	Overall (1)	High (2)	Low (3)
ATT in pp	1.3	8.8***	3.1	0.4	7.2	-2.0	1.7***	4.4***	3.0**	0.8**	8.7***	1.7
95% CI	[-0.7, 3.3]	[4.6, 13.1]	[-0.9, 7.1]	[-2.4, 3.1]	[-1.5, 15.9]	[-7.6, 3.5]	[0.8, 2.5]	[1.2, 7.6]	[0.1, 5.9]	[0.1, 1.5]	[4.0, 13.4]	[-2.5, 5.8]
Pvalue	0.207	0.000	0.124	0.791	0.106	0.477	0.000	0.007	0.041	0.023	0.000	0.430
ATT %	7.7	18.5	4.2	5.0	13.0	-2.6	10.2	8.3	4.1	11.5	16.2	2.2
N obs	346,845	136,842	94,699	172,201	48,479	54,602	505,718	208,331	136,757	259,751	74,118	81,073
N indiv.	70,383	32,203	21,873	35,627	11,786	12,309	85,260	41,561	26,401	44,976	15,714	15,770

WDiD on parallel growths: impact on the employment rate. Treated group is 58-59.5, control group is (A) 57.75 years old or (B) 57-57.75 years old, Effects on specific subpopulation (defined in q-7) by column: (1) Overall treated, (2) Workers in sectoral industrial committees with an exit rate from employment above the population median (= 18%), (3) Workers in sectoral industrial committees with an exit rate below the median. Point estimates of the ATT are expressed in percentage points (pp) and in proportional (%) changes. N° of observations is the sum of the number

Table 2.14: Impact on the Extensive Margin: control group 53.25-57 years old

Men	Overall (1)	Already Working (2)	Low wage (3)	Manufacturing Sector (4)	Service sector (5)	High exit rate Committee (6)	Low exit rate Committee (7)	Large firms (8)	Small firms (9)	Blue-collar (10)	White-collar (11)
ATT in *pp*	0.7***	2.2***	1.2	3.5***	0.5	3.2***	0.1	4.0***	-0.5	0.6	1.9*
95% CI	[0.2; 1.2]	[0.8; 3.6]	[-0.9, 3.3]	[1.4, 5.7]	[-1.5; 2.5]	[1.3, 5.0]	[-2.4, 2.5]	[1.7; 6.4]	[-2.4; 1.5]	[-1.5; 2.8]	[-0.2; 4.1]
Pvalue	0.004	0.002	0.278	0.001	0.617	0.001	0.967	0.001	0.642	0.565	0.073
ATT %	4.0%	3.5%	2.1%	6.8%	0.7%	5.9%	0.1%	6.8%	-0.7%	1.2%	2.7%
N° of observations	1,135,596	811,981	475,377	459,360	343,377	526,758	285,060	446,649	363,630	453,725	358,090
N° of individuals	152,280	123,519	77,403	69,197	55,170	82,887	46,873	68,858	59,850	70,570	53,934
Subsidy/Labor cost	3.7%	3.6%	5.0%	3.4%	3.8%	4.3%	3.0%	2.9%	4.4%	5.0%	2.8%
Elasticity	1.084	0.965	0.410	2.006	0.179	1.364	0.023	2.330	-0.155	0.242	0.948
Semi-Elasticity in *pp*	0.187	0.611	0.231	1.051	0.133	0.736	0.017	1.382	-0.106	0.127	0.693

Women	Overall (1)	Already Working (2)	Low wage (3)	Manufacturing Sector (4)	Service sector (5)	High exit rate Committee (6)	Low exit rate Committee (7)	Large firms (8)	Small firms (9)	Blue-collar (10)	White-collar (11)
ATT in *pp*	0.5***	1.7	1.9	2.7	1.3	4.9***	-0.4	1.9	1.3	2.2	1.6
95% CI	[0.1; 0.9]	[-0.6; 3.9]	[-1.3, 5.1]	[-3.3, 8.7]	[-1.1, 3.8]	[1.5; 8.2]	[-3.7, 2.9]	[-1.3; 5.1]	[-1.1, 3.8]	[-1.9; 6.4]	[-1.1; 4.4]
Pvalue	0.002	0.151	0.254	0.377	0.288	0.004	0.813	0.253	0.422	0.290	0.247
ATT %	6.7%	2.5%	2.8%	5.0%	1.8%	8.5%	-0.5%	3.0%	1.7%	3.7%	2.3%
N° of observations	660,465	422,126	208,550	76,784	339,573	218,856	203,239	208,655	213,404	143,008	278,991
N° of individuals	89,062	65,118	36,509	11,724	52,760	35,853	32,108	32,184	35,525	24,229	41,488
Subsidy/Labor cost	5.5%	5.4%	7.6%	4.5%	5.5%	5.7%	5.2%	4.9%	6.0%	7.7%	4.6%
Elasticity	1.220	0.459	0.376	1.137	0.341	1.478	-0.100	0.614	0.291	0.474	0.493
Semi-Elasticity in *pp*	0.090	0.311	0.247	0.606	0.242	0.849	-0.077	0.378	0.214	0.288	0.352

*WDiD on parallel growths: impact on the employment rate. Control group is 53.25-57 years old; treated group 58-59.5 years old. Effects on specific subpopulation (defined in q-7) by column: (1) Overall treated group, (2) Workers in the salaried private sector, (3) Workers earning less than the treated group median wage, (4) Workers in secondary sector, (5) Workers in tertiary sector, (6) Workers in sectoral industrial committees with an exit rate from employment above the population median (= 18%), (7) Workers in sectoral industrial committees with an exit rate below the population median, (8) Workers in firms with at least 200 employees, (9) Workers in firms with less than 200 employees, (10) Blue-collar workers, (11) White-collar workers.. Point estimates of the ATT are expressed in percentage points (pp), in proportional (%) changes in the employment rate and in terms of (semi-)elasticity, i.e. the proportional (pp) effect on the employment rate of a proportional reduction in the labour costs. N° of observations is the sum of the number of individuals observed in each quarter of the analysis. Standard errors are obtained by a stratified bootstrap (clustering by individual) with 200 repetitions and 95% confidence intervals (CI) by assuming normality. ***: significant at 1%, **: significant at 5%, *: significant at 10%.*

Table 2.15: Impact on the intensive margin: control group 53.25-57 years old

		Overall (1)	Low Wage (2)	Manufacturing Sector (3)	Service sector (4)	High Exit Rate (5)	Low Exit Rate (6)	Large Firms (7)	Small Firms (8)	Blue-collar (9)	White-collar (10)
	0%-30%: ATT in *pp*	-0.5***	-0.5**	-0.3*	-0.5***	-0.8***	-0.3*	-0.2*	-0.5**	-0.9***	-0.0
	95% CI	[-0.8; -0.2]	[-0.9; -0.1]	[-0.7; 0.01]	[-0.9; -0.2]	[-1.3; -0.4]	[-0.5; 0.0]	[-0.5; 0.02]	[-0.9; -0.1]	[-1.5; -0.3]	[-0.3; 0.2]
	Pvalue	0.001	0.012	0.054	0.006	0.001	0.054	0.074	0.026	0.003	0.684
	ATT in %	-11.6%	-8.8%	-12.2%	-12.0%	-12.6%	-12.5%	-13.0%	-8.0%	-10.4%	-3.5%
	31%-80%: ATT in *pp*	-0.1	-0.2	-0.5	-0.0	-0.3	-0.2	0.1	-0.3	0.0	-0.4
	95% CI	[-0.6; 0.5]	[-1.1; 0.7]	[-1.3; 0.4]	[-0.9; 0.8]	[-1.1; 0.4]	[-1.0; 0.6]	[-0.8; 0.9]	[-1.1; 0.5]	[-1.1; 1.1]	[-1.1; 0.4]
	Pvalue	0.816	0.673	0.262	0.972	0.399	0.625	0.892	0.590	0.986	0.321
	ATT in %	-0.4%	-0.9%	-3.1%	-0.1%	-1.7%	-1.3%	0.4%	-1.6%	0.0%	-2.8%
Men	**>80%: ATT in *pp***	0.5*	0.7	0.8*	0.6	1.1***	0.4	0.2	0.8**	0.9	0.4
	95% CI	[-1.1; 0.3]	[-0.2; 1.6]	[-0.1; 1.7]	[-0.2; 1.4]	[0.3; 1.9]	[-0.3; 1.3]	[-0.7; 1.1]	[0.02; 1.6]	[-0.2; 2]	[-0.4; 1.2]
	Pvalue	0.080	0.129	0.067	0.163	0.006	0.266	0.704	0.044	0.111	0.297
	ATT in %	0.7%	1.0%	1.0%	-0.7%	1.5%	0.4%	0.2%	1.1%	1.3%	0.5%
	ATT total hours %	0.4%**	0.4%*	0.5%*	0.4%	0.9%***	0.2%	0.1%	0.6%**	0.8%**	0.2%
	95% CI	[0.0; 0.8]	[0.0; 0.9]	[0.0; 1.0]	[-0.1; 0.9]	[0.4; 1.4]	[-0.2; 0.7]	[-0.4; 0.5]	[0.1; 1.1]	[0.1; 1.4]	[-0.2; 0.5]
	Elasticity	0.108	0.093	0.146	0.108	0.205	0.076	0.040	0.128	0.160	0.068
	N° of observations	648,229	396,060	342,423	298,233	409,896	238,182	342,967	305,255	347,282	300,932
	N° of individuals	107,974	68,563	57,244	51,325	70,931	41,250	59,195	53,667	59,708	49,069
	0%-30%: ATT in *pp*	-0.5*	-0.5	1.0	-0.7**	-1.0**	0.1	-0.6*	-0.5	-1.1	-0.1
	95% CI	[-1.1; 0.1]	[-1.4; 0.5]	[-0.5; 2.4]	[-1.2; -0.1]	[-2.0; -0.0]	[-0.8; 0.5]	[-1.3; 0.1]	[-1.3; 0.3]	[-2.5; 0.3]	[-0.7; 0.4]
	Pvalue	0.077	0.352	0.196	0.029	0.040	0.715	0.098	0.236	0.110	0.608
	ATT in %	-3.9%	-2.4%	11.1%	-5.0%	-5.6%	1.3%	-9.3%	-2.7%	-3.8%	-2.5%
	31%-80%: ATT in *pp*	-1.1**	-1.6**	-0.3	-1.2**	-0.7	-1.4**	-2.1***	-0.6	0.2	-1.6***
	95% CI	[-2.0; -0.2]	[-2.9; -0.2]	[-2.9; 2.1]	[-2.2; -0.2]	[-2.0; 0.7]	[-2.7; -0.2]	[-3.5; -0.7]	[-1.8; 0.7]	[-1.6; 1.9]	[-2.7; -0.6]
	Pvalue	0.015	0.024	0.822	0.019	0.323	0.022	0.004	0.365	0.849	0.003
	ATT in %	-2.4%	-2.9%	-0.9%	-2.5%	-1.4%	-3.2%	-4.0%	-1.4%	0.4%	-3.6%
Women	**>80%: ATT in *pp***	1.6***	2.0***	-0.6	1.9***	1.7***	1.6***	2.7***	1.1*	1.0	1.8***
	95% CI	[0.7, 2.3]	[1.0; 3.1]	[-3.1; 1.9]	[1; 2.8]	[0.5; 2.8]	[0.4; 2.7]	[1.5; 4.0]	[-0.03; 2.2]	[-0.4; 2.3]	[0.7; 2.8]
	Pvalue	0.000	0.000	0.618	0.000	0.003	0.006	0.000	0.057	0.164	0.001
	ATT in %	4.1%	7.3%	-1.2%	4.8%	5.0%	3.5%	6.668%	2.7%	4.3%	3.6%
	ATT total hours %	1.4%***	2.1%***	0.0%	1.6%***	2.1%***	1.0%**	2.0%***	1.3%**	2.8%***	0.9%
	95% CI	[0.8; 2.1]	[1.0; 3.2]	[-1.8; 1.9]	[0.8; 2.4]	[0.9; 3.3]	[0.1; 1.9]	[1.1; 2.9]	[0.2; 2.3]	[1.1; 4.6]	[0.2; 1.6]
	Elasticity	0.264	0.278	0.007	0.293	0.362	0.196	0.401	0.201	0.357	0.179
	N° of observations	334,197	154,033	54,378	275,663	170,773	163,294	162,395	171,502	110,133	224,058
	N° of individuals	58,205	29,278	9,573	48,010	31,888	28,210	28,363	31,601	20,972	37,673

*WDiD on parallel path: impact on the intensive margin defined as number of workers with a certain working time with respect to the reference time (0-30%, 31-80%, >81%). Control group is 53.25-57 years old; treated group 58-59.5 years old. Effects on specific subpopulation (defined in time q) by column: (1) Overall treated group, (2) Workers earning less than the treatment group median wage, (3) Workers in secondary sector, (4) Workers in tertiary sector, (5) Workers in sectoral industrial committees with an exit rate from employment above the population median (= 18%), (6) Workers in sectoral industrial committees with an exit rate below the median, (7) Workers in firms with at least 200 employees, (8) Workers in firms with less than 200 employees, (9) Blue-collar workers, (10) White-collar workers. The elasticity approximates the working time elasticity of the wage cost subsidy (see footnote 21). Standard errors are obtained by a stratified bootstrap (clustering by individual) with 200 repetitions and 95% confidence intervals (CI) by assuming normality. ***: significant at 1%, **: significant at 5%, *: significant at 10%.*

Table 2.16: Impact on the Hourly Gross Wage: control group 53.25-57 years old

Men	**Overall (1)**	**Low wage (2)**	**Manufacturing Sector (3)**	**Service sector (4)**	**High exit rate (5)**	**Low exit rate (6)**	**Large firms (7)**	**Small firms (8)**	**Blue-collar (9)**	**White-collar (10)**
ATT in €	0.06	0.01	-0.33	0.21*	-0.01	0.22*	0.08	0.14	0.01	0.11
95% CI	[-0.08; 0.19]	[-0.05; 0.07]	[-0.73; 0.07]	[-0.01; 0.44]	[-0.17, 0.14]	[-0.02, 0.47]	[-0.15; 0.31]	[-0.03; 0.32]	[-0.05; 0.07]	[-0.11; 0.33]
Pvalue	0.421	0.735	0.107	0.058	0.860	0.075	0.503	0.105	0.819	0.329
ATT in %	0.3%	0.1%	-1.5%	1.1%	0.1%	0.9%	0.3%	0.9%	0.1%	0.4%
N° of observations	581,180	356,134	305,982	268,396	368,680	212,362	306,613	274,566	312,369	268,668
N° of individuals	103,296	65,585	54,682	48,973	67,901	39,134	56,338	50,790	57,200	46,732
Elasticity	0.075	0.017	-0.441	0.281	-0.019	0.316	0.110	0.201	0.012	0.155
Elasticity Upper CI	0.270	0.166	0.095	0.530	0.197	0.671	0.441	0.421	0.111	0.471

Women	**Overall (1)**	**Low wage (2)**	**Manufacturing Sector (3)**	**Service sector (4)**	**High exit rate (5)**	**Low exit rate (6)**	**Large firms (7)**	**Small firms (8)**	**Blue-collar (9)**	**White-collar (10)**
ATT in €	-0.01	0.00	0.21*	-0.05	0.04	-0.03	-0.04	0.06	0.02	-0.06
95% CI	[-0.08; 0.07]	[-0.06; 0.07]	[-0.03; 0.46]	[-0.14; 0.04]	[-0.07; 0.15]	[-0.14; 0.07]	[-0.16; 0.08]	[-0.04; 0.16]	[-0.05; 0.10]	[-0.17; 0.05]
Pvalue	0.884	0.934	0.086	0.257	0.463	0.561	0.499	0.262	0.502	0.303
ATT in %	0.0%	0.0%	1.5%	-0.4%	0.3%	-0.2%	-0.3%	0.5%	0.3%	-0.4%
N° of observations	333,652	153,826	54,126	275,255	170,515	163,007	162,029	171,323	109,858	223,658
N° of individuals	58,138	29,249	9,536	47,950	31,841	28,188	28,319	31,570	20,885	37,627
Elasticity	-0.008	0.004	0.318	-0.070	0.056	-0.043	-0.053	0.079	0.034	-0.079
Elasticity Upper CI	0.137	0.140	0.755	0.054	0.206	0.102	0.131	0.274	0.135	0.072

*WDiD on parallel path: impact on the hourly gross wage. Control group is 53.25-57 years old; treated group 58-59.5 years old. Effects on specific subpopulation (defined in time q) by column: (1) Overall treated group, (2) Workers earning less than the treated group median wage, (3) Workers in secondary sector, (4) Workers in tertiary sector, (5) Workers in sectoral industrial committees with an exit rate from employment above the population median (= 18%), (6) Workers in sectoral industrial committees with an exit rate below the population median, (7) Workers in firms with at least 200 employees, (8) Workers in firms with less than 200 employees, (9) Blue-collar workers, (10) White-collar workers. N° of observations is the sum of the number of individuals observed in each quarter of the analysis. Standard errors are obtained by a stratified bootstrap (clustering by individual) with 200 repetitions and 95% confidence intervals (CI) by assuming normality. ***: significant at 1%, **: significant at 5%, *: significant at 10%.*

Table 2.17: Cost-Benefit Analysis

			2002 SUBSIDY (1)						TARGETED SUBSIDY (2)					
SCENARIO			All		Men		Women		All		Men		Women	
MCF (A)	Income tax (B)	Leisure (C)	Net budget	Welfare	Net budget	Welfare	Net budget	Welfare	Net budget	Welfare	Net budget	Welfare	Net budget	Welfare
2.14	**28,7%**	**Baseline**	**2.974 €**	**3.672 €**	**3.861 €**	**4.939 €**	**1.097 €**	**1.145 €**	**89 €**	**-411 €**	**425 €**	**123 €**	**-622 €**	**-1.626 €**
2.14	32,9%	Baseline	2.858 €	3.484 €	3.718 €	4.704 €	1.039 €	1.050 €	9 €	-539 €	348 €	-4 €	-709 €	-1.769 €
2.14	28,7%	Lower	2.974 €	2.353 €	3.861 €	3.445 €	1.097 €	197 €	89 €	-1.579 €	425 €	-1.093 €	-622 €	-2.694 €
2.14	32,9%	Lower	2.858 €	2.223 €	3.718 €	3.282 €	1.039 €	131 €	9 €	-1.667 €	348 €	-1.181 €	-709 €	-2.793 €
2.14	28,7%	Upper	2.974 €	4.990 €	3.861 €	6.433 €	1.097 €	2.093 €	89 €	757 €	425 €	1.338 €	-622 €	-558 €
2.14	32,9%	Upper	2.858 €	4.744 €	3.718 €	6.126 €	1.039 €	1.969 €	9 €	589 €	348 €	1.173 €	-709 €	-744 €
1.41	28,7%	Baseline	2.974 €	1.758 €	3.861 €	2.120 €	1.097 €	344 €	89 €	-621 €	425 €	-188 €	-622 €	-1.172 €
1.41	32,9%	Baseline	2.858 €	1.641 €	3.718 €	1.990 €	1.039 €	291 €	9 €	-711 €	348 €	-258 €	-709 €	-1.251 €
1.41	28,7%	Lower	2.974 €	439 €	3.861 €	626 €	1.097 €	-604 €	89 €	-1.790 €	425 €	-1.403 €	-622 €	-2.240 €
1.41	32,9%	Lower	2.858 €	381 €	3.718 €	568 €	1.039 €	-628 €	9 €	-1.839 €	348 €	-1.435 €	-709 €	-2.276 €
1.41	28,7%	Upper	2.974 €	3.076 €	3.861 €	3.614 €	1.097 €	1.292 €	89 €	547 €	425 €	1.028 €	-622 €	-104 €
1.41	32,9%	Upper	2.858 €	2.902 €	3.718 €	3.412 €	1.039 €	1.210 €	9 €	417 €	348 €	919 €	-709 €	-226 €
3.23	28,7%	Baseline	2.974 €	6.529 €	3.861 €	9.148 €	1.097 €	2.341 €	89 €	-96 €	425 €	586 €	-622 €	-2.304 €
3.23	32,9%	Baseline	2.858 €	6.235 €	3.718 €	8.756 €	1.039 €	2.183 €	9 €	-281 €	348 €	375 €	-709 €	-2.541 €
3.23	28,7%	Lower	2.974 €	5.211 €	3.861 €	7.654 €	1.097 €	1.393 €	89 €	-1.265 €	425 €	-630 €	-622 €	-3.372 €
3.23	32,9%	Lower	2.858 €	4.975 €	3.718 €	7.334 €	1.039 €	1.264 €	9 €	-1.409 €	348 €	-802 €	-709 €	-3.566 €
3.23	28,7%	Upper	2.974 €	7.848 €	3.861 €	10.642 €	1.097 €	3.289 €	89 €	1.072 €	425 €	1.802 €	-622 €	-1.237 €
3.23	32,9%	Upper	2.858 €	7.496 €	3.718 €	10.178 €	1.039 €	3.102 €	9 €	847 €	348 €	1.552 €	-709 €	-1.517 €

Monthly costs (benefits if negative) in 2004 euros per saved job under different scenarios: (A) Marginal Cost of public Funds equal to 2.14, 1.41, or 3.23 (Kleven and Kreiner, 2006), (B) Income tax equal to 28.7% or 32.9% (OECD, 2007), (C) opportunity cost of working as in the baseline scenario, upper or lower bounds (see Section 2.7.5). ***Bold*** *is the baseline scenario described in Section 2.7.5. (1) Cost-benefit analysis for the SSC exemption on all the individuals (58-59.5 years old) from the second quarter of 2002 until the second quarter of 2003; (2) Cost-benefit analysis in the counterfactual of a policy targeting the subsidy to the high-exit flow sectoral industrial committees. Control units are 53.25-55.25 years old. Gross cost per saved job is the total cost of the wage cost subsidy divided by the number of saved jobs. Net cost per saved job is estimated by a trend-adjusted WDiD on the individual net budgetary gross cost of the policy divided by the number of saved jobs. The net cost to society is the efficiency cost of public funds minus the value of the welfare generated by the jobs that were saved by reduction in SSC. The latter is estimated by trend-adjusted WDiD on the individual gross wage costs taking into account for the pay-productivity gap and the opportunity cost of working. The CBA ignores potential substitution and anticipation effects. The costs to society ignore: the potential distributional and health impacts of the measure, and the potential benefits in the absence of the subsidy that some of the early retirees would be replaced by unemployed workers.*

2.9.2. *Components of the Cost-Benefit-Analysis*

(i) **Gross Budgetary Cost (GBC)**: average cost of the subsidy for the state per job created before any behavioural impact;

$$GBC = \frac{TGC}{J} = \frac{\sum_{g=1}^{2} S_g * N_g}{\sum_{g=1}^{2} ATT_g(\text{empl}) * N_g}$$

With:

TGC: Total gross cost of the subsidy

J: Total number of created jobs

g ∈ {1,2} with "1" the population at high risk of exiting and "2" the population at low risk of exiting

S_g: Average subsidy received by treated population g

$ATT_g(\text{empl})$: the estimated ATT on employment in *pp* for treated population g (with $ATT_2(\text{empl})$ = 0)

N_g: Size of the treated population g

(ii) **Net Budgetary Cost (NBC):** average cost of the subsidy for the state per job created, net of savings for the public budget, i.e. tax revenues (income tax and contributions to social security) generated by the jobs that were saved by the policy, as well as the outlays that the government would have spent in the absence of the policy, mostly on allowances to the early retirees (at the extensive margin) or to participants in the time-credit scheme (at the intensive margin);

$$NBC = \frac{TNC}{J} = \frac{\sum_{g=1}^{2} \text{ATT}_g(PB_i) * \text{N}_g}{\sum_{g=1}^{2} ATT_g(\text{empl}) * \text{N}_g}$$

With TNC the total net cost of the subsidy and $\text{ATT}_g(PB_i)$ = The Average Treatment effect on the Treated on the net contributions to the public budget PB_i - estimated by the trend-adjusted WDiD estimator (estimator explained in Section 2.6).

- The individual net contribution to the public budget PB_i is the sum of taxes paid on the individual wage cost (TP_i) minus the outlays received from government OR_i: $PB_i = TP_i - OR_i$.
- The tax payment TR_i contains income taxes and contributions to social security. Since we do not have information on the individual income tax, we approximate it by multiplying the individuals' gross remuneration by the average income tax in 2002, i.e. 0.287 (OECD, 2007). In a sensitivity analysis we raise this to 32.8% i.e. the average income tax for workers at 133% of the mean wage. As we do not have information on the income of self-employed, their tax collection is neglected. This only introduces a slight bias, since just 0.9% of the treated individuals become self-employed.
- The outlays received from the government OR_i contain allowances related to early retirement and the time-credit scheme, unemployment benefits and other employment allowances. Information on allowances for statutory early retirement, sickness and disability schemes is not available, but these are again negligible, since recipients of the allowances represent only 1.1% of the relevant sub-sample.

(iii) **Welfare Gain (or Cost) for Society (WS)**: the welfare variation $\text{ATT}_g(PR_i)$ generated by these saved jobs (gain in terms of the production – opportunity cost of working) minus the efficiency cost ("excess burden" or "deadweight loss") of the net budgetary expenditures mentioned in (ii). The total Welfare gain (or cost) for society (TWS) is then divided by the number of jobs created (J). To calculate the efficiency cost we multiply the effect on government budget by the Marginal Cost of Public Funds (MCF) minus one. For Belgium a MCF equal to two is considered to be appropriate (Kleven and Kreiner 2006; Barrios Cobos *et al.* 2013). In a sensitivity analysis we change the MCF to the highest and lowest estimates of Kleven and Kreiner (2006): 1.41 and 3.23.

$$WS = \frac{TWS}{J} = \frac{(\sum_{g=1}^{2} \mathrm{ATT}_g(PB_i) * \mathrm{N}_g) * (\mathrm{MCF} - 1) + (\sum_{g=1}^{2} \mathrm{ATT}_g(PR_i) * \mathrm{N}_g)}{\sum_{g=1}^{2} ATT_g(\mathrm{empl}) * \mathrm{N}_g}$$

With $\mathrm{ATT}_g(PR_i)$ = The Average Treatment effect on the Treated on the welfare PR_i - estimated by the trend-adjusted WDiD estimator. The value of the individual welfare PR_i is the following:

$PR_i = WC_i * PG_i - LE_i$ if individual i works, zero otherwise

- WC_i is the wage cost of the individual, which is adjusted by the age-specific pay-productivity gap PG_i. Estimates on the latter are provided to us by Vandenberghe *et al.* (2013). On average the PG_i accounts for 15.7% of the wage costs for the age class 58-59.5 (i.e. $E(PR_i) = E(PR_i) * 0.843$). As for the calculation of TR_i, the value of production for self-employed is set to zero as we do not have information on their income. Similarly to Greenberg and Robins (2008), we lower bind the opportunity cost of working to zero and upper bind to the remuneration (we also include the SSC). As baseline scenario, we use the mid-point between the two bounds, which are later considered in a sensitivity analysis.

Bibliography

Abadie, A., 2005. Semiparametric Difference-in-Differences Estimators. Review of Economic Studies 72(1), 1–19.

Ammermüller, A., Boockmann, B., Maier, M., Zwick, T., 2006. Eingliederungszuschüsse und Entgeltsicherung für Ältere: Analysen auf Basis natürlicher Experimente. Vierteljahrshefte zur Wirtschaftsforschung / Quarterly Journal of Economic Research 75(3), 49–66.

Athey, S., Imbens, G., 2006. Identification and Inference in Nonlinear Difference-in-Differences Models. Econometrica 74(2), 431–497.

Aubert, P., Crépon, B., 2003. La productivité des salariés âgés: une tentative d'estimation. Économie et statistique 368(1), 95–119.

Aubert, P., Crépon, B., 2006. Are older workers less productive? Firm-level evidence on age-productivity and age-wage profiles. Mimeo, INSEE, Paris.

Barrios Cobos, S., Saveyn, B., Pycroft, J., 2013. The Marginal Cost of Public Funds in the Eu: The Case of Labour Versus Green Taxes. Taxation papers 35, Publications Office of the European Union.

Bell, B., Blundell, R., van Reenen, J., 1999. Getting the Unemployed Back to Work: The Role of Targeted Wage Subsidies. International Tax and Public Finance 6(3), 339–360.

Bennmarker, H., Mellander, E., Öckert, B., 2009. Do Regional Payroll Tax Reductions Boost Employment? Labour Economics 16(5), 480–489.

Blau, D., 1994. Labor Force Dynamics of Older Men. Econometrica 62(1), 117–56.

Blau, D., Riphahn, R., 1999. Labor force transitions of older married couples in Germany. Labour Economics 6(2), 229–252.

Blundell, R., Costa Dias, M., 2009. Alternative Approaches to Evaluation in Empirical Microeconomics. Journal of Human Resources 44(3), 565–640.

Blundell, R., Costa Dias, M., Meghir, C., Reenen, J., 2004. Evaluating the Employment Impact of a Mandatory Job Search Program. Journal of the European Economic Association 2(4), 569–606.

Bohm, P., Lind, H., 1993. Policy Evaluation Quality: A Quasi-Experimental Study of Regional Employment Subsidies in Sweden. Regional Science and Urban Economics 23(1), 51–65.

Boockmann, B., Zwick, T., Ammermüller, A., Maier, M., 2012. Do Hiring Subsidies Reduce Unemployment Among Older Workers? Evidence from Natural Experiments. Journal of the European Economic Association 10(4), 735–764.

Brussig, M., Bernhard, S., Jaenichen, U., Zwick, T., 2006. Zielstellung, Förderstrukturen Und Effekte Der „entgeltsicherung "–Erfahrungen Mit Einem Kombilohn Für Ältere Arbeitnehmerinnen Und Arbeitnehmer. Zeitschrift für ArbeitsmarktForschung 39(3/4), 491–504.

Burtless, G., 1986. Social Security, Unanticipated Benefit Increases, and the Timing of Retirement. Review of Economic Studies 53(5), 781–805.

Busso, M., DiNardo, J., McCrary, J., 2014. New Evidence on the Finite Sample Properties of Propensity Score Reweighting and Matching Estimators. The Review of Economics and Statistics 96(5), 885–897.

Cameron, A.C., Trivedi, P.K., 2005. Microeconometrics: methods and applications. Cambridge University Press.

Cardoso, A.R., Guimarães, P., Varejão, J., 2011. Are Older Workers Worthy of Their Pay? An Empirical Investigation of Age-Productivity and Age-Wage Nexuses. De Economist 159(2), 95–111.

Cataldi, A., Kampelmann, S., Rycx, F., 2012. Does it pay to be productive? The case of age groups. International Journal of Manpower 33, 264–283.

Claes, T., 2012. La Prépension Conventionnelle (1974-2012). Courrier hebdomadaire du CRISP 2154(29), 5–94.

Cohen, D., Elchardus, M., 2003. Attitude et attentes en rapport avec la fin de la carrière professionnelle. TOR Groep, Vrije Universiteit Brussel, Brussels.

Crépon, B., Desplatz, R., 2003. The Effets of Payroll Tax Subsidies for Low Wage Workers on Firms Level Decisions . Working Paper Centre de Recherche en Economie et Statistique No. 2003-06, INSEE, Paris.

Crump, R., Hotz, V.J., Imbens, G., Mitnik, O., 2009. Dealing with Limited Overlap in Estimation of Average Treatment Effects. Biometrika 96(1), 187–199.

Dejemeppe, M., Smith, C., Van der Linden, B., 2015. Did the Intergenerational Solidarity Pact increase the employment rate of the elderly in Belgium? A macro-econometric evaluation . mimeo, IRES, Université catholique de Louvain, Louvain-la-Neuve.

Eissa, N., Hoynes, H.W., 2004. Taxes and the labor market participation of married couples: the Earned Income Tax Credit. Journal of Public Economics 88(9), 1931–1958.

Eissa, N., Liebman, J., 1996. Labor Supply Response to the Earned Income Tax Credit. The Quarterly Journal of Economics 111(2), 605–37.

European Commission, 2010. Europe 2020. a Strategy for Smart, Sustainable and Inclusive Growth. Communication from the Commission. COM (2010) 2020 final, 3 March 2010.

Eurostat, 2015. Employment rates by sex, age and nationality: lfsa_ergan.

French, E., 2005. The Effects of Health, Wealth, and Wages on Labour Supply and Retirement Behaviour. Review of Economic Studies 72(2), 395–427.

Friedberg, L., 2000. The Labor Supply Effects of the Social Security Earnings Test. The Review of Economics and Statistics 82(1), 48–63.

Frimmel, W., Horvath, T., Schnalzenberger, M., Winter-Ebmer, R., 2015. Seniority Wages and the Role of Firms in Retirement . IZA Discussion Paper No. 9192, IZA, Bonn.
Frölich, M., 2007. Propensity Score Matching Without Conditional Independence Assumption—with an Application to the Gender Wage Gap in the United Kingdom. The Econometrics Journal 10(2), 359–407.
Frölich, M., Huber, M., Wiesenfarth, M., 2015. The Finite Sample Performance of Semi- and Nonparametric Estimators for Treatment Effects and Policy Evaluation . IZA Discussion Papers No. 8756, IZA, Bonn.
Goos, M., Konings, J., 2007. The Impact of Payroll Tax Reductions on Employment and Wages: A Natural Experiment Using Firm Level Data . LICOS Discussion Paper No. 178/2007.
Greenberg, D., Robins, P., 2008. Incorporating nonmarket time into benefit-cost analyses of social programs: An application to the self-sufficiency project. Journal of Public Economics 92(3-4), 766–794.
Gruber, J., 1997. The Incidence of Payroll Taxation: Evidence from Chile. Journal of Labor Economics 15(3), S72–101.
Gruber, J., Wise, D.A., 2007. Social Security Programs and Retirement around the World: Fiscal Implications of Reform. University of Chicago Press.
Hakola, T., Uusitalo, R., 2005. Not so voluntary retirement decisions? Evidence from a pension reform. Journal of Public Economics 89(11–12), 2121–2136.
Hamermesh, D.S., 1996. Labor demand. Princeton University Press.
Hanoch, G., Honig, M., 1983. Retirement, Wages, and Labor Supply of the Elderly. Journal of Labor Economics 1(2), 131–51.
Heckman, J., 1974. Shadow Prices, Market Wages, and Labor Supply. Econometrica 42(4), 679–94.
Heckman, J., Ichimura, H., Todd, P., 1997. Matching as an Econometric Evaluation Estimator: Evidence from Evaluating a Job Training Programme. Review of Economic Studies 64(4), 605–54.
Heckman, J.J., Robb, R.J., 1985. Alternative methods for evaluating the impact of interventions. In J. Heckman, and B. Singer, (eds), Longitudinal Analysis of Labour Market Data, Cambridge: Cambridge University Press.
Hellerstein, J.K., Neumark, D., 2007. Production function and wage equation estimation with heterogeneous labor: Evidence from a new matched employer-employee data set, in: Hard-to-Measure Goods and Services: Essays in Honor of Zvi Griliches. University of Chicago Press, pp. 31–71.
Hellerstein, J., Neumark, D., Troske, K., 1999. Wages, Productivity, and Worker Characteristics: Evidence from Plant-Level Production Functions and Wage Equations. Journal of Labor Economics 17(3), 409–46.
Hersoug, T., 1984. Union Wage Responses to Tax Changes. Oxford Economic Papers 36(1), 37–51.
Hirano, K., Imbens, G.W., Ridder, G., 2003. Efficient Estimation of Average Treatment Effects Using the Estimated Propensity Score. Econometrica 71(4), 1161–1189.
Horvitz, D.G., Thompson, D.J., 1952. A Generalization of Sampling Without Replacement from a Finite Universe. Journal of the American Statistical Association 47(260), 663–685.
Huber, M., Lechner, M., Wunsch, C., 2013. The Performance of Estimators Based on the Propensity Score. Journal of Econometrics 175(1), 1–21.
Huttunen, K., Pirttilä, J., Uusitalo, R., 2013. The Employment Effects of Low-Wage Subsidies. Journal of Public Economics 97(C), 49–60.
Ilmakunnas, P., Maliranta, M., 2005. Technology, Labour Characteristics and Wage-productivity Gaps. Oxford Bulletin of Economics and Statistics 67(5), 623–645.
Kaldor, N., 1936. Wage Subsidies as a Remedy for Unemployment. Journal of Political Economy 44.
Katz, L., 1996. Wage Subsidies for the Disadvantaged . NBERWorking Paper No. 5679.
Keane, M., 2012. Income Taxation in a Life Cycle Model with Human Capital . Economics Series Working Paper, University of Oxford, Oxford No. 2012-W08.

Keane, M., Rogerson, R., 2012. Micro and Macro Labor Supply Elasticities: A Reassessment of Conventional Wisdom. Journal of Economic Literature 50(2), 464–76.

Kleven, H.J., Kreiner, C., 2006. The Marginal Cost of Public Funds: Hours of Work Versus Labor Force Participation. Journal of Public Economics 90(10-11), 1955–1973.

Kramarz, F., Philippon, T., 2001. The Impact of Differential Payroll Tax Subsidies on Minimum Wage Employment. Journal of Public Economics 82(1), 115–146.

Krueger, A.B., Pischke, J.-S., 1992. The Effect of Social Security on Labor Supply: A Cohort Analysis of the Notch Generation. Journal of Labor Economics 10(4), 412–37.

Lazear, E., 1979. Why Is There Mandatory Retirement? Journal of Political Economy 87(6), 1261–84.

Lechner, M., 2011. The Estimation of Causal Effects by Difference-in-Difference Methods. Foundations and Trends(R) in Econometrics 4(3), 165–224.

Lechner, M., Wunsch, C., 2009. Are Training Programs More Effective When Unemployment Is High? Journal of Labor Economics 27(4), 653–692.

Lichter, A., Peichl, A., Siegloch, S., 2014. The Own-Wage Elasticity of Labor Demand: A Meta-Regression Analysis . IZA Discussion Paper No. 7958, IZA, Bonn.

Liebman, J.B., Luttmer, E.F.P., Seif, D.G., 2009. Labor supply responses to marginal Social Security benefits: Evidence from discontinuities. Journal of Public Economics 93(11-12), 1208–1223.

Lockwood, B., Manning, A., 1993. Wage Setting and the Tax System Theory and Evidence for the United Kingdom. Journal of Public Economics 52(1), 1–29.

Manski, C., Lerman, S.R., 1977. The Estimation of Choice Probabilities from Choice Based Samples. Econometrica 45(8), 1977–88.

Marshall, A., 1920. Principals of Economics. MacMillan.

Meyer, B., 1995. Natural and Quasi-experiments in Economics. Journal of Business & Economic Statistics 13(2), 151–61.

Meyer, B.D., Rosenbaum, D.T., 2001. Welfare, the Earned Income Tax Credit, and the Labor Supply of Single Mothers. Quarterly Journal of Economics 116(3), 1063–1114.

Mitchell, O.S., Fields, G.S., 1984. The Economics of Retirement Behavior. Journal of Labor Economics 2(1), 84–105.

OECD, 2003. Ageing and Employment Policies/Vieillissement Et Politiques de L'emploi: Belgium. OECD Publishing, Paris.

OECD, 2007. Society at a Glance 2006. OECD Publishing, Paris.

Pissarides, C., 1985. Taxes, Subsidies, and Equilibrium Unemployment. Review of Economic Studies 52(1), 121–33.

Pissarides, C., 1998. The Impact of Employment Tax Cuts on Unemployment and Wages; the Role of Unemployment Benefits and Tax Structure. European Economic Review 42(1), 155–183.

Staubli, S., Zweimüller, J., 2013. Does raising the early retirement age increase employment of older workers? Journal of Public Economics 108(C), 17–32.

Vandenberghe, V., Waltenberg, F., Rigo, M., 2013. Ageing and Employability. Evidence from Belgian Firm-Level Data. Journal of Productivity Analysis 40(1), 111–136.

van Ours, J., Stoeldraijer, L., 2011. Age, Wage and Productivity in Dutch Manufacturing. De Economist 159(2), 113–137.

Wolfers, J., 2006. Did Unilateral Divorce Laws Raise Divorce Rates? A Reconciliation and New Results. American Economic Review 96(5), 1802–1820.

Wooldridge, J.M., 2010. Econometric Analysis of Cross Section and Panel Data, 2nd ed. MIT press.

Zabalza, A., Pissarides, C., Barton, M., 1980. Social Security and the Choice Between Full-Time Work, Part-Time Work and Retirement. Journal of Public Economics 14(2), 245–276

3

Working Time Reductions at the End of the Career. Do they Prolong the Time Spent in Employment?

3.1. Introduction

Population ageing puts enormous pressure on Social Security provisions in many developed countries. One of the main factors involved in this is the low labour force participation of older workers, and in particular of older women. In EU-15 countries in 2013, the labour force participation rates of workers aged 55-64 was 29 percentage points below that of workers aged 25-54 (OECD, 2015a). This difference was even more pronounced for Belgium, the country of analysis in this paper, where the participation rate of the older age group is 41 percentage points lower than the younger one. Low labour market participation at older ages is in many developed countries in part caused by early retirement. Even though in many EU15 countries the average legal retirement age is around 65, people still tend to retire two and a half years earlier (OECD, 2015b).[1] In Belgium workers are observed to retire particularly early, with an average effective retirement age below 60 since the late 1980's.

A key strategy to cope with the challenge induced by population ageing is to prolong the working career and to increase the activity rates of older workers. However, simply continuing to work full-time at an older age might increase the risk of dropping out from the labour force due to declining health or to taking up care obligations (Gielen, 2009; Van Looy et al., 2014). It has been argued that workers might stay longer in the labour force if they could gradually reduce their working time at the end of their career (e.g. Schmid, 1998). Several EU countries have put gradual retirement schemes in place for older workers.[2] These can take different forms. In Sweden, Finland and Denmark workers reduce their working time and top up their income by prematurely drawing from their pension entitlements. In Belgium, Germany and Austria employees can reduce working time before entering into retirement and the government provides a subsidy to partially compensate for the income loss. In these countries employees can also choose the so called "block-model". This model concentrates the reduced working time in the years prior to retirement by taking a leave of absence, actually inducing retirement to be *early* instead of *gradual.* Finally, before the abolition in 2012, Dutch employees could 'save' time early in their career to reduce working time later on.

Evidence of the effectiveness of gradual retirement schemes based on counterfactual impact evaluations is scarce. Most research has focussed on studying the *determinants* of gradual retirement (e.g. Gustman and Steinmeier, 1984; Honig and Hanoch, 1985; Ruhm, 1990; Hutchens and Grace-Martin, 2006; Hutchens, 2010; Kantarcı and van Soest, 2008, for a survey). Wadensjö (2006) uses the counterfactual analysis of Sundén (1994) to determine the behaviour of part-time pensioners in Sweden in the counterfactual of no part-time pension and concludes that the scheme increased the

[1] Average over the period 2007-2012.
[2] See Table 3.3 in Appendix 3.9 for an overview of such schemes in the countries mentioned in the text.

number of hours worked in the economy. However, he underlines (on p. 27) that "the calculations are based on data which is not perfect for the task". Based on the conditional independence assumption (CIA), Graf et al. (2011) and Huber et al. (2015) estimate the effects of the Austrian, respectively German, gradual retirement scheme. Both studies find that gradual retirement reduces the likelihood of unemployment. However, this does not imply that these workers remain employed longer, because if the "block model" is chosen, the worker is officially employed without actually working. Although workers in the inactive phase of the block model are counted as employed, Graf et al. find that in Austria the effect on employment is eventually negative. Similarly, Huber et al. (2015) report for West-German participants in the scheme no significantly higher survival rate in employment. However, they do find significant positive employment effects for East-German participants and positive spill-over effects on younger workers. They attribute this differential finding to the difficult labour market conditions in East-Germany. Finally, Berg et al. (2015) also evaluate the German partial retirement scheme, but based on a difference-in-differences strategy. They find that participation in the scheme prolongs the working career for men, but less so in periods of more intensive use of the block model. Smaller and, in the period of more intensive use of the block model, even negative effects are reported for women. The authors do not consider differential effects for West- and East-Germany.

In this paper we evaluate whether the Time Credit (TC) scheme in Belgium can lengthen the career of older workers. The TC scheme subsidizes and entitles full-time workers in the private sector to working time reductions. We focus on the component of the scheme that targets workers older than fifty, because it entitles eligible individuals to the TC until retirement and can therefore be viewed as a gradual retirement scheme. We exploit very rich administrative data on labour market histories (for private sector employment from as early as 1957), sick leave, exit destinations out of employment, and some essential firm and household characteristics. Based on these data we can estimate the impact on the survival rate in employment until eight years after entry into TC. Similar to the studies of Graf et al. (2011) and Huber et al. (2015), we base identification on the CIA. We argue that this is a credible identification strategy because of the richness of the data and because the analysis is restricted to the eligible population, which is relatively homogeneous. To be eligible an individual should have worked at least five years full-time in the same private sector firm employing at least 11 employees and have accumulated at least 20 years of labour market experience. Moreover, even if information on important variables, such as ability, motivation and health histories, are missing, we follow Lechner et al. (2011) and Huber et al. (2015) by claiming that these unobservables are indirectly captured by conditioning on the very rich pre-treatment labour market histories. For instance, health problems should be reflected by gaps in the labour market experience or in lower level of earnings;

Or, ability in the average earnings level. Finally, since the selection on observables is found to be small, we follow Altonji et al. (2005) to argue that the selection on unobservables is likely not important.

We contribute to the literature in the following ways. First, because the block model actually resembles more an *early* retirement scheme than a *gradual* one, we restrict the treatment to genuine part-time workers and consider participants in the block model as inactive if they stop working. Second, we investigate whether the treatment effect depends on the *extent* of working time reduction – 20% or 50%. Third, we explicitly study the interaction between the impact of gradual retirement on the timing of withdrawal from the labour market and the supply incentives to exit the labour market via early retirement schemes. In particular, we provide suggestive evidence that the effectiveness of gradual retirement to prolong the labour market career depends on how participation in the TC scheme affects the level of the allowances in early retirement schemes. Since in the analysed TC-scheme participants keep on accumulating pension rights of a full-time worker, their higher replacement income may induce them to withdraw faster from the labour market.

Furthermore, to the best of our knowledge, we are the first to provide (partial) evidence on a health outcome by analysing the effect of gradual retirement on the incidence of sick leave during employment. In addition, we propose a cost-benefit analysis in which we estimate based on the CIA and using the administrative data *both* the net budgetary costs for the state and the net welfare gains (or costs) for society. Finally, from a methodological perspective, we explicitly take into account that the TC scheme is not entered at a fixed moment, but can happen at any time. Sianesi (2004), Fredriksson and Johansson (2008) and Crépon et al. (2009) have shown that in case of such dynamic assignment into treatment, methods based on the CIA that assume that the treatment assignment is static are biased. They propose propensity score matching methods that take this dynamic assignment explicitly into account. Vikström (2014) built on these findings to suggest a method that takes into account that the right censoring induced by transitions into treatment may depend on observables and, hence, be selective, a problem that was overlooked in the literature. We use this estimator, but adjust it to consider that, for reasons explained in Section 3.4, our analysis is based on an endogenously stratified sample. It is well known that in this case consistent estimation requires appropriate weighting of the data (Manski and Lerman, 1977).[3] Such adjustment is straightforward in Vikström's estimator, as it is implemented as an inverse probability weighting (IPW) estimator (Horvitz and Thompson, 1952; Hirano et al., 2003): It merely consists in an extra weighting of the data to take the endogenous sampling into account.

[3] In particular, Frölich (2007) demonstrates in a standard Kernel matching framework that endogenous sampling, contrary to choice-based sampling (i.e. sampling based on the treatment status), requires re-weighting the data both in the estimation of the propensity score and the treatment effect.

Our findings can be summarized as follows. We estimate a positive short run effect of the TC on the survival rate in employment. This effect becomes negative after two (four) years for men (women). Qualitatively these effects are therefore similar to those found by Graf et al. (2011) for Austria. The negative effect is essentially explained by the fact that TC beneficiaries who enter statutory early retirement remain entitled to the level of pension benefits of a full-time worker, which makes early retirement relatively more attractive for those workers. Once we right censor individuals who leave employment for early retirement within the statutory regime, the significant negative effect of participation in the TC scheme on the survival rate in employment is very much reduced. This can be explained by the lower replacement rate in the other pathways to early retirement (e.g. Bridge Pension) after reducing working time. The negative effect is eliminated if we focus on TC recipients working half-time rather than 80%. This is because the differences in the replacement rates in the different early retirement regimes between participants and non-participants are more marked for half-time workers than for those working 80%. This suggests that the scheme could be more effective in the absence of these adverse supply side incentives. Overall, the *existing* scheme does not pass the cost-benefit test except if we impose very restrictive assumptions on the replacement of the working hours not covered anymore by the TC participants, and then even only in the first years of the analysis. However, this cost-benefit analysis does not take distributional effects or the impact on health into account. As to the latter, we do find some evidence that gradual retirement reduces the incidence of sick leave but the effect is small and non-significant, presumably because we target in this analysis an eligible population that has relatively few health problems in the first place.

The paper is structured as follows. We start with a literature review. In Section 3.3 we describe the institutional context and then describe the sampling scheme and the data we used. Section 3.5 outlines the empirical strategy (identification and estimation). In Section 3.6 we present our empirical findings and in Section 3.7 the cost-benefit analysis. The final section summarizes the results and concludes.

3.2. Literature Review

It has been argued that workers might stay longer in the labour force if they could gradually reduce their working time at the end of their career (e.g. Schmid, 1998). Firms may voluntarily grant working time reductions if they realize that doing so allows them to keep valuable (firm-specific) competences and transfer know-how to younger employees (Eurofound, 2001; Kantarcı and van Soest, 2008). However, institutional constraints, such as provisions that the pension allowances depend on the last wage, or that a pension cannot be drawn upon while working part-time, may discourage older workers to reduce working time. In addition, employers may not be willing to award gradual retirement (Hurd, 1996; Charles and Decicca, 2007; Gielen, 2009) so that government incentives may be necessary.

It is, however, far from guaranteed that eliminating institutional barriers and offering an explicit option to gradually retire by part-time work would keep older workers longer in the workforce or increase the total number of hours worked. First, pay of older workers may exceed their productivity.[4] If employment protection is strong and early retirement schemes are not yet available for these workers, then firms may use subsidized part-time employment at the end of the career as an easy way to get gradually rid of these less productive workers. The gradual retirement scheme is then just a 'bridge' to (early) retirement and does not prolong the working career (Graf et al., 2011). This process may be reinforced if a phased reduction in working hours (i) does not lead to a proportional reduction in the workload (Devisscher and Sanders, 2007; Rudolf, 2014), (ii) signals a preference for early retirement (Machado and Portela, 2012), or (iii) just decreases labour market attachment. Finally, even if the withdrawal from the labour force is delayed, the total number of hours worked may still decrease. An hours-constrained worker who is not offered the possibility to reduce working time has two options: Either stop working altogether or stay working full-time. If such a worker is then offered the possibility to limit the number of hours worked, she increases the number of hours worked if the first option was chosen, but the reverse holds for the second option. The net effect on the number of hours worked depends on the relative size of these effects (Gielen, 2009; Graf et al., 2011).

The empirical literature studying the effectiveness of gradual retirement schemes based on counterfactual evaluations is very sparse. In this section, we summarize the findings of the three studies that were already briefly mentioned in the Introduction.

Graf et al. (2011) study the Austrian old age part-time (OAPT) scheme based on the conditional independence assumption (CIA) using propensity score methods (see e.g. Imbens and Wooldridge, 2009). They contrast all 6,142 men (resp. 3,210 women) who entered the OAPT scheme between 2000 and 2003 to a control group of 23,810 men (resp. 28,651 women) who were employed at least one day in 2000/2001 and to whom a hypothetical start date was assigned according to the simulation procedure described in Lechner and Wunsch (2008).[5] They find that the OPAT scheme increases the number of days employed by 30 days on average during each of the first two years after entrance. However, in the fourth and fifth year the OPAT decreases the number of days employed by about 35 for women and by nearly 50 for men. Consequently, the cumulative effect over five years was negative.

[4] There is some evidence that declining productivity with age or deferred compensation schemes (Lazear, 1979) induce a pay-productivity gap for older workers. Hellerstein et al. (1999) did not find evidence for a pay productivity gap in the U.S., but a recent replication of this seminal analysis reports that pay exceeds productivity for workers older than 55 in the manufacturing sector (Hellerstein and Neumark, 2007). Aubert and Crépon (2003, 2006) establish similar results for this age group in France, Ilmakunnas and Maliranta (2005) for Finland, and Cataldi et al. (2012) and Vandenberghe et al. (2013) for Belgium. By contrast, no pay-productivity gap is found in Portugal and in the Netherlands (Cardoso et al., 2011; van Ours and Stoeldraijer, 2011). Frimmel et al. (2015) report evidence that in Austria employees of firms with higher labour costs for older workers leave the labour force at a younger age.

[5] As mentioned in the Introduction, this procedure has been criticised by Fredriksson and Johansson (2008).

Moreover, these figures do not take into account that OAPT participants work part-time. In full-time equivalents the time worked diminished over five years by 26 (23) percentage points for men (women). On the other hand, the time spent in unemployment fell over this period by 37 (43) days for men (women). This is most likely because in the “block-model” these workers were not required to be made redundant, since they were not working anyway.

Huber et al. (2015) use the CIA to study the effect of introducing partial retirement in Germany. A novelty in their approach is that they consider the *intention-to-treat* effects of introducing the option of partial retirement at the firm level on the labour market outcomes of the employees in these firms. More specifically, they contrast the labour market outcomes between 2003 and 2009 of employees in firms that started offering partial retirement between 2000 and 2002 to firms that did not offer this opportunity in this period. To avoid that the composition of the workforce in these firms would be influenced by the introduction of the partial retirement scheme, the estimation was restricted to workers who had at least three years of tenure in these firms in June 2000. The authors report very different findings for West- than for East-Germany and attribute this difference to the difficult labour market conditions in the latter region. In West-Germany the option of partial retirement did not have any significant impact on the timing at which the labour market was left, neither did it induce any spill-overs to younger employees. However, as in Austria, the “block-model” affected the pathway to retirement. Rather than transiting to unemployment prior to retirement participants entered the non-employment block of the scheme. By contrast, in East-Germany older workers remained significantly longer attached to the labour market, which resulted in net savings for the public authorities through lower benefit expenditures and higher tax incomes and social security contributions. Moreover, presumably by the financial incentive to replace the partial retiree by unemployed workers, the scheme generated positive spill-overs by increasing employment stability and by lowering the likelihood of unemployment of younger women (but not of men).

Finally, Berg et al. (2015) also evaluate the German partial retirement scheme based on a difference-in-differences strategy which contrasts a younger control group of 50-54 year olds to a treatment group of 55-65 year olds in the pre-treatment period (1993-1998) and two post-treatment periods: 1999-2001 and 2002-2004, respectively a period in which the block-model was less and more intensively used. This analysis confirms that the block-model reduced the positive impact of the partial retirement scheme on the probability of staying in the labour force. During the period of less intensive use of the block-model male participants remained 1.8 years longer employed than non-participants, while this effect fell to 1.2 years in the period of more intensive use (if the time not working during the second phase of the block model is not considered as employment). The employment of female participants was, however, not affected in the former period, while it even declined by 0.2 years in the latter. The findings of Huber et al. (2015) refer to the period of more intensive use of the block-model. Nevertheless, it is not easy to compare the two studies, since, in contrast to Huber et al. (2015), Berg et al. (2015) did not stratify the analysis between West- and East-Germany and did not consider spill overs on the employment of younger individuals.

3.3. Gradual Retirement in Belgium: Time Credit Beyond the Age of 50

In 2002 the Career Break scheme, in place since 1985 and available in both the private and public sector, was reformed and relabelled "Time Credit" (TC) scheme in the private sector. The schemes aim at generating a better work-life balance by enabling and encouraging employees, even without needing to specify a specific motive, to slow down their working pace. Workers younger than 50 can temporarily reduce their working time, while older workers can take advantage of these schemes without any time limit, as to enable a more gradual transition to retirement (Devisscher, 2004). We focus on a description of the main features of the latter section of the TC scheme, also called the "end of career" TC. We restrict our description to the regulations in place during the 2002-2007 period. This covers the relevant period for the empirical analysis in this research. In 2015, the scheme is still in place, but most of the eligibility conditions have been strengthened considerably. Most notably, the age of eligibility was raised to 55 years in 2012 and starting from 2015 this age will gradually increase to 60 in 2019. Nevertheless, eligibility at 55 remains possible under certain circumstances.[6]

Individuals older than 50 who are employed in a private sector firm are under certain conditions entitled to reduce their working time to 80% or 50% of a full-time, or even completely. They are entitled to a lump-sum state subsidy that partially compensates for the earnings loss that the transition to part-time work involves. Notice that the possibility to completely stop working was much less used than the other regimes, because the subsidy was only 34% higher than the one obtained for the 50% regime and more generous early retirement schemes were available in this case, although generally only at older ages (see below). Since our interest is in *gradual* retirement, we do not further consider this regime. The main eligibility conditions for the end of career TC scheme were the following:

1. ≥50 years old at the start of the working time reduction;
2. Employed during the year prior to entry at 100% (≥ 75%) of a full-time schedule for the 20% (50%) regime;
3. ≥ 5 years of tenure in the same firm;
4. ≥ 20 years of labour market experience;

[6] This is the case for workers in arduous professions, in case of partial work incapacity, in restructuring firms, or in cases that the employer organisations and trade unions have concluded a Collective Agreement and the employers agree to pay the subsidy to which participants are entitled that would otherwise be due by the Social Security scheme.

5. Consent of the employer, if the number of employees in the firm is ≤ 10 or, if the fraction of employees in the is TC >5% (can be revised by a collective agreement);
6. Notification of the employer at least 3 months prior to the working time reduction.

If these eligibility conditions are satisfied the employee is entitled to a monthly lump-sum subsidy of, (i) in case of the 20% regime, €224[7] for singles with or without dependent children and €186 for other household types, and, (ii) in case of the 50% regime, €400 for all household types. For the sample of TC beneficiaries analysed in this research, this results in a median replacement rate of 83% of the full-time gross wage for those working 80%, and of 57% for those working half-time. As a comparison, the replacement rate after the top-up was 70% for a half-time worker in Germany and 75% when working 40% to 60% in Austria. The replacement rate is therefore lower for a median worker in Belgium, but, since the subsidy is lump-sum in Belgium and proportional in Germany and Austria, it is higher for low-wage workers in Belgium.[8]

The end of career TC was very popular. The number of participants grew steadily from 8,700 in 2002, the year it was introduced, to 88,000 in 2011, the year prior to the one in which the age of eligibility was raised to 55. To compare, in 2001, the year prior to the reform, the number of private sector beneficiaries older than 50 in the Career Break scheme amounted to 18,745.[9] As a share of private sector employees aged 50 or more, TC participation steadily increased from 2.5% in 2002 to 16.0% in 2011. This growth is related to both, the rising employment rate of older women in this period and to the increased generosity of the TC relative to the Career Break. The subsidy amount was raised by about 20% in the 50% regime and by nearly 40% in the 20% regime. The share of female TC participants steadily grew from 27% in 2002 to 52% in 2011.[10]

Apart from the lump-sum subsidy, another major benefit for TC participants is that for the determination of the level of replacement income in the branches of Social Security, such as (early) statutory retirement and unemployment, TC beneficiaries are assimilated to workers with the same time schedule as before the working time reduction. This means that relative to their current income, TC participants earn a much higher replacement income if they leave employment for (early) retirement than workers who remain full-time employed (i.e. expected pension allowance divided by working income). As a consequence, the incentive to stop working is significantly enhanced, especially for workers who choose the 50% TC regime: the numerator of their replacement income is unaffected, while the denominator significantly decreases. In line with these incentives, we will show in the

[7] All € in the text are indexed by the CPI and expressed in constant 2004 euros.
[8] For the 20% (50%) regime the first and third quartile of this replacement rate ranges between 81% (55%) and 91% (62%), while the 90th percentile of this replacement rate, i.e. for low-wage workers, is as high as 96% (75%).
[9] Only new beneficiaries entered TC. The stock of existing beneficiaries remained entitled to the Career Break.
[10] Sources: year reports of the RVA/ONEM 2001-2011.

empirical analysis that the participation in TC eventually enhances the transition to early retirement. Moreover, because these incentives were different across the different early retirement schemes, they also altered the pathway to early retirement. To understand this point, we briefly describe the different early retirement options in Belgium and the impact of the TC on the benefit level in these options.

In Belgium there are essentially three early retirement schemes in the private sector: early retirement within the statutory regime, the conventional pre-retirement scheme (also known as the "bridge pension") and the, so-called, "Canada Dry" system.[11] In the period of analysis, early retirement within the statutory regime started from age 60 after minimum 35 years of employment experience.[12] This is an *early* retirement scheme, because the normal age of statutory retirement was 65 for men and between 63 and 65 for women.[13] The benefit level is determined as at the statutory age and provides in case of a career of 45 years a replacement rate of 75% or 60%, depending on whether the partner of the beneficiary, if any, has any (replacement) income or not. This amount is proportionally reduced if the career is shorter than 45 years and is bracketed by a floor and a ceiling amount. Due to relatively low generosity of this scheme for *full-time* workers in the private sector,[14] take-up in the two alternative regimes, especially the bridge pension, is much more important. For workers having more than 20 years of employment experience, the bridge pension is available from age 60 for all workers and from 58 in case of a sectoral collective agreement, mostly concluded in the industrial committees[15] of the manufacturing sector. In restructuring firms and for arduous professions, the age condition could drop to 50, 52 or 55, depending on the sectoral agreement. Because of a supplement equal to half of the difference between the unemployment benefit (UB) and the wage, the bridge pension is more attractive, at least if they are not in TC (see below). The "Canada Dry" is an unofficial early retirement scheme in which the employer pays, as in the bridge pension, a supplement to the UB. This scheme is more flexible for the employer, because it does not impose an age limit among others (Albanese and Cockx, 2015). Since there is no obligation for the worker to report the supplement to the UB she obtains, no official figures on the use of the Canada Dry scheme are available.

As already mentioned, the UB and the statutory (early) retirement pension for a beneficiary of TC is calculated on the basis of the fictitious earnings that the employee would have had if she would not have reduced her working time. By contrast, TC does decrease the benefit level of the bridge pension. Even if the entitlement to UB is based on the *fictitious* earnings, the supplement is equal to 50% of the

---

[11] "Canada Dry" refers to publicity for the drink Canada Dry: "It has the colour of Whisky, but it is not Whisky".

[12] This experience requirement was gradually increased from 20 to 34 years by steps of two years in each calendar year between 1997 and 2004, and, eventually, by one year to 35 years in 2005.

[13] The statutory retirement age for women was raised by 1 year every 3 years from 60 before 1997 to 65 from 2009 onwards.

[14] For public sector employees the scheme is much more generous and, hence, more widely used.

[15] Industrial committees are organized for each type of worker at the sectoral level. In these committees, trade unions and employer organizations negotiate the collective agreements. These agreements are binding for all workers belonging to this industrial committee, irrespectively of whether they are unionized and, hence, represented in the negotiation.

difference between the *effective* part-time wage in TC and the UB and, hence, much lower or even zero if the UB is higher than the wage, which can happen in the 50% TC regime.[16] Consequently, to the extent that the more restrictive age and experience requirements are satisfied, the statutory *early* retirement scheme is relatively more attractive than the bridge pension (and the Canada Dry) for beneficiaries of TC than for full-time workers. Moreover, the statutory *early* retirement can be entered without consent of the employer, while the bridge pension, Canada Dry or plain unemployment does require this consent, because the employer must then also compensate for the dismissal, which is costly for these older workers with substantial seniority. This explains why we find in the empirical analysis that TC increases the likelihood of ending the career through the statutory early retirement, especially in the 50% TC regime.

3.4. Data & Sample Selection

3.4.1. *Database*

We use rich individual data that were obtained by merging administrative registers of the diverse Social Security institutions and of the National Register containing all Belgian inhabitants. The database became more comprehensive over time. From as early as 1957 until 1998 we have for employees in the private sector yearly information on earnings, the number of working days and hours (in case of part-time work) and the worker type (blue or white collar). From 1998 onwards this information is available on a quarterly basis (measurement at the end of each quarter), not only for employees in the private sector, but also in the public sector. In addition, from then onwards it also contains on limited firm information (size and sector), the industrial committee to which the worker belongs to (see footnote 15), the timing of self-employment spells, on UB receipt, as well as on participation in the Career Break, TC schemes and early retirement schemes. Finally, since 2003 the data have been complemented by information on sick leave, on receipt of statutory (possibly early) retirement benefits and replacement income in case of disability, occupational diseases or accidents. Finally, since 1998 the National Register provides yearly information on December 31 on individual and household characteristics, such as age, gender, nationality, district of residence, household size (by age group) and type (single or couple, with or without children). The observation period in this study ends in the last quarter of 2011.

[16] For instance, heads of household and singles with more than 20 years of experience and aged 55 or more are entitled to UB with a replacement rate of at least 60% (slightly higher for heads) and of 55% for cohabitants of heads aged 58 or more (with floor and cap). For younger workers or worker with less experience the level of UB is somewhat lower.

3.4.2. *Sample Selection*

In this study we base our analysis on a sample that was drawn with the purpose to evaluate the effect of a wage cost subsidy for employees in the private sector aged 58 years or more. The Belgian government introduced this subsidy in 2002 to enhance the employment of older workers (Albanese and Cockx, 2015). To that end a representative sample was drawn of 243,655 individuals born between the 1st of April 1941 and the 31st of March 1950, i.e. aged between 52 and 61 in 2002. Because in Belgium many individuals are already inactive in that age bracket, the sample was not only stratified according to gender, but also into 9 birth cohorts $c\ (= 1, 2, \dots, 9)$ and 5 strata $r\ (= 1, 2, \dots, 5)$. These strata were defined according to employment status in the private sector and the earned wage in the period around the 2002 reform. This stratification aimed at over-representing groups that are relatively rare in that age bracket and more responsive to the labour market policy reform: low-wage employees in the private sector and individuals transiting in and out of employment during this period. In Section 2.5 more details on this stratification can be found. Because the stratification involves outcome variables of interest, it is *endogenous* and it is well known that consistent estimation then requires to appropriately weigh the data in these strata (e.g. Manski and Lerman, 1977; Cameron and Trivedi, 2005). If we denote the sampling weight for individual observation i belonging to birth cohort c and to substratum r by $W_{cr,i}$, then

$$W_{cr,i} = \frac{N_{cr}}{N} * \frac{n}{n_{cr}} \qquad (1)$$

where N_{cr} denotes the size of the population in substratum cr,[17] n_{cr} the corresponding sample size, $N \equiv \sum_{c=1}^{9} \sum_{r=1}^{5} N_{cr}$ the total population size and *n* the corresponding sample size. As to avoid cumbersome notation, gender is not explicitly referred to. The weighting formula comes from a double re-weighing, within and between cohorts.[18]

In this study we aim at evaluating the impact of participating in TC on the survival rate in employment. The TC scheme was introduced in 2002. Nevertheless, we start the evaluation only from 2003 and this for two reasons. First, we wish to consider the scheme at a moment when it is well established and the rules are well known. Second, we also aim at integrating the incidence on sick leave as a second outcome. As mentioned before, information on sick leave is only available since 2003.

We evaluate the impact of TC spells that started in 2003 and 2004. In principle we could also consider TC that started in later years. However, the available sample participation in TC declines over time and

---

[17] We have information on the population sizes in each substratum, i.e. on N_{cr}.

[18] First, to restore the representativeness within the cohorts we reweigh the units within each cohort by $W_{cr}^{c} = \frac{N_{cr}}{N_c} * \frac{n_c}{n_{cr}}$ (where N_c and n_c is the size of the cohort in the population and in the sample). To make the cohorts in the sample representative for the population, we weigh each cohort a second time: $SW_{cr} = W_{cr}^{c} * \frac{N_c}{N} * \frac{n}{n_c}$, so that $W_{cr} = \frac{N_{cr}}{N} * \frac{n}{n_{cr}}$.

these additional treatments would not be helpful in identifying the long-run effects (up to eight years after the start of the treatment) in which we are particularly interested.[19]

Because the TC can start at any moment within the eight year of the analysis, there is a dynamic assignment into treatment which induces, as mentioned in the Introduction, some intricate methodological issues (Fredriksson and Johansson, 2008). In particular, individuals who start the treatment later on may confound the control group. Besides, participants may anticipate the treatment. In Section 3.5 we will explain in more detail how we deal with these issues. At this point we limit the discussion to what this implies for the definition of treatment and control groups.

First, as to model this dynamic assignment process and to take it into account we must split up the assignment period into sub-periods. If the sample size would not be an issue, one would split up this period as finely as possible. However, within the available sample the transition rate into TC is relatively low, so that we ran into inference problems when we defined quarterly intervals. We therefore chose to consider yearly intervals. Furthermore, to have a sufficiently high number of treated individuals and increase precision we perform two separate analyses on two treatment groups, depending on whether the TC started in 2003 or 2004, and pool the estimates.

A second important issue is anticipation. Participation in TC is always anticipated, because the employer must be notified at least three months before the start. This means that the treatment actually starts before the working time reduction related to the TC scheme is implemented. To take this into account we therefore assume that a treatment *actually* starts one quarter earlier than it *contractually* does. Hence, with this new assumption, the period that determines treatment is advanced one quarter, so that an individual is defined to be member of the first treatment group if she *actually* participates in a TC at the end of the third quarter 2003 (2003Q3) while she did not at the end of 2002Q3. However, shifting the start of the treatment is not sufficient to deal with anticipation, because without information on when the treatment *actually* started, it is impossible to determine whether it had any impact on the outcome between the *actual* and *contractual* start of the treatment i.e. exit from employment.[20] We therefore impose the additional assumption that individuals *never* leave employment in this period. This is a strong assumption that biases the treatment effect upwards (i.e. a higher survival rate in employment for the treated). However, this bias is arguably small, because it is unlikely that individuals who agreed with the employer that they would start TC within the next

[19] The decline in the number of participants is due to the ageing of our sample, exiting the labour force over-time. Official statistics from RVA/ONEM show that until 2005 the share of employees in TC steadily increased of about 2 pp in each year.
[20] We define the survival outcome as any form of employment: part-time or full-time, public, private or self-employment.

quarter would eventually decide to stop working beforehand. This would presumably only occur for reasons of force majeure, such as an accident.[21]

As we would like to resemble a random assignment into treatment, we impose that both treated and control individuals have to be eligible to the TC-scheme. In particular, we consider in our sample only people satisfying the following requirements:

1. Being employed in a firm with at least 20 employees at the end of 2002 (2003);
2. Have at least 5 years of tenure in the same firm at the end of 2002 (2003);
3. Have at least 20 years of private sector labour market experience at the end of 2002 (2003);
4. Being full-time employed in all four quarters of 2002 (2003);
5. Being employed in the private sector at the end of 2003Q3 (2004Q3);
6. Not being on sick leave at the end of 2003Q3 (2004Q3).

The fifth selection criterion is imposed at the end of each assignment period into treatment, because it is automatically satisfied for the members of the treatment groups, so that it is natural to impose it on the members of the control groups as well. The other criteria are slightly more restrictive than the TC eligibility conditions (Section 3.2), so that a few treated individuals are eliminated from our initial selection. We are slightly more restrictive for the following reasons: (i) We need not impose the age condition, because our sample only contains individuals older than 50; (ii) Eligibility conditions 1-4 are uniformly imposed at the end of the year preceding the *contractual* start of the TC so that the same conditions apply to *all* treated and control units; (iii) Because the data contain only basic information about firm characteristics, we aim at restricting the analysis to sufficiently large firms in which the use of TC does not require the consent of the employer (Section 3.2). If no consent is required, it is less likely that the use of TC is selective in firm characteristics, which therefore enhances the internal validity of the evaluation. However, according to the rules, no consent is required in firms with more than 10 employees, while we impose a firm size of at least 20. This is because the available data on firm size are grouped in intervals that do not allow identifying firms with strictly more than 10 employees; (iv) Regulations do not impose that the labour market experience should be accumulated in the private sector, while we do because we do not have information on early experience outside the private sector; (v) We impose full-time employment in the last year prior to *contractual* assignment into treatment, while for the 50% TC regime the requirement is only to have worked at least 75% of a full-time job. This is done so that people are eligible to both regimes; (vi). Finally, we impose the last condition on sickness in 2003Q3 because we want to contrast the impact of the benchmark outcome, i.e. survival in employment, to a more restrictive variant that considers survival in employment *without*

[21] Note that the bias is not present for the selection as we impose that the treated and the control units are employed in the last quarter of the selection (2003q3 or 2004q3).

being on sick leave. If we would not impose this, some of the selected individuals would not be in the risk set of this second outcome at the start of the evaluation period. Imposing this condition only very marginally affects the sample selection.

Based on these selection rules we retain, for men, 1,227 treated and 29,791 control units. For women the sample size is smaller: 762 treated individuals and 9,658 control units. By using the sampling weights defined in Equation (1) we find that this represents in the Belgian population about 5,124 treated individuals and 90,387 control units. Note that the control units in the two years of analysis partly consist of same individuals, while treated units are always different.

3.4.3. *Descriptive Analysis*

As described in the previous Section, our sample is composed of individuals satisfying the eligibility conditions to the TC one year prior to the selection of 2003 (or 2004). Consequently, the individuals considered in the analysis are relatively homogeneous in the dimensions that matter for these conditions such as employment experience in the private sector (20 years), tenure (5 years), firm size (> 20 employees) and full-time work in the year prior to the selection (2002 or 2003). Even if the individuals in the two groups are already homogenous in several dimensions, they still differ significantly in a number of other dimensions. This justifies the use of the Inverse Probability Weighting (IPW) estimator proposed in the next section. The full list of variables on which we condition in the analysis are shown in Table 3.1. We distinguish between men and women, because the analysis is performed separately for these groups. The units in the treatment and control groups for the 2003 and 2004 analyses are pooled. Note that 54.3% of the treated are in the 50% TC regime, the remainder in the 20% TC regime.

First, we consider the variables measured the last quarter prior to the start of the selection (i.e. 2002Q3 and 2003Q3 for the first and second analysis). Treated units tend to work in larger firms, live with a partner without dependent children, are slightly younger, earn a lower hourly wage and, are more likely to be Belgians living in Flanders. Finally, they are concentrated in specific economic sector such as the banking and business related services and more in general in white-collar work. The industrial committees where they tend to work are the ones where bridge pension schemes are less common.

Second, we consider both the recent and less recent employment history.[22] In line with the year before the selection, treated individuals earn a lower hourly wage five years before selection and had lower average annual earnings since 1957 (the difference is statistically significant only for men). Tenure in

[22] The variables referred to the employment history are not combined to keep information coming from different sources and periods with missing information separate. For example, information from 1998 comes from payroll-tax-administration (quarterly data), while from 1957 to 1997 from pension registrations (yearly data).

the same firm is slightly shorter and total accumulated working experience since 1957 is slightly more important. If we consider also the 5 years of tenure prior to the sample selection, then the individuals in the sample have on average about 36 years of experience, which makes many of them already eligible to the statutory early retirement.[23]

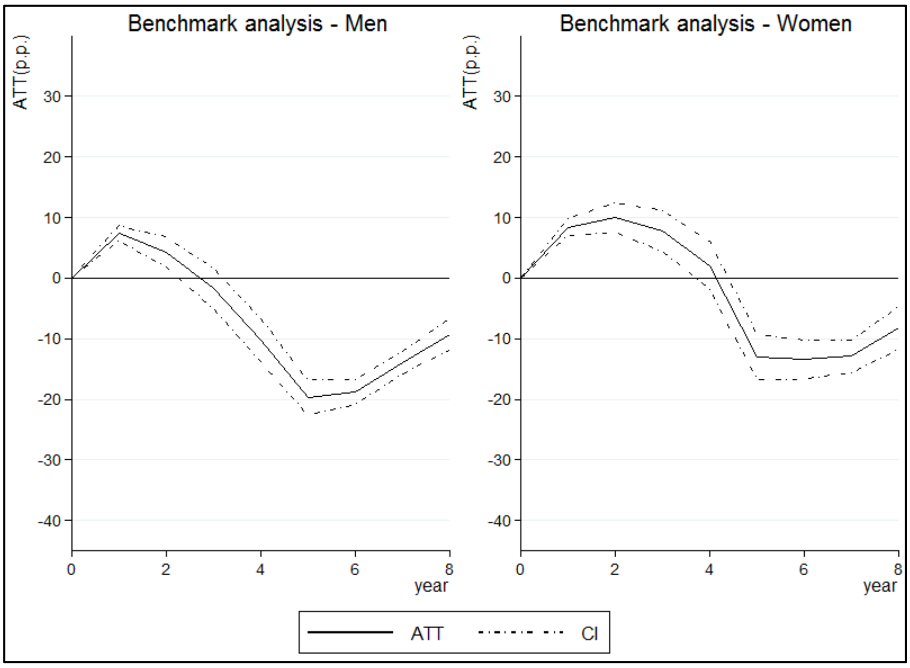
Table 3.1: Descriptive Statistics of Selected Treated and Control Groups (weighted by $W_{cr,i}$)

		Men			Women		
		TREATED	CONTROL	P-value: equality	TREATED	CONTROL	P-value equality
Status one year before selection (2002 or 2003)	Firm size: 20-99	18.8%	30.2%	0.000	26.4%	32.9%	0.000
	Firm size: 100-999	30.9%	37.8%	0.000	27.9%	36.7%	0.000
	Firm size: > 1000	50.4%	32.0%	0.000	45.8%	30.4%	0.000
	Household: Other	11.2%	13.4%	0.043	35.7%	40.8%	0.009
	Household: Couple with children	46.4%	48.1%	0.326	24.3%	24.4%	0.984
	Household: Couple without children	42.4%	38.5%	0.025	39.9%	34.9%	0.010
	Age	55.5	55.8	0.000	55.4	55.7	0.000
	Blue collar	23.5%	30.5%	0.000	10.3%	12.4%	0.095
	Av. Full-Time Hourly wage	€ 20.7	€ 22.5	0.000	€ 17.5	€ 17.7	0.365
	Belgian	98.0%	95.0%	0.000	97.2%	96.3%	0.236
	Household size	2.6	2.7	0.022	2.1	2.1	0.630
	Region: Brussels	5.7%	7.1%	0.079	15.4%	19.2%	0.009
	Region: Flanders	70.2%	64.8%	0.001	51.9%	48.0%	0.055
	Region: Wallonia	24.1%	28.2%	0.007	32.8%	32.8%	0.968
	Sector: Trade, transport, hotel	15.0%	20.7%	0.000	21.6%	24.6%	0.073
	Sector: Bank, business services	44.6%	18.7%	0.000	39.7%	24.5%	0.000
	Sector: Other services	4.0%	6.8%	0.000	21.5%	26.3%	0.004
	Sector: Manufacturing, Agriculture, Construction	31.8%	47.5%	0.000	17.2%	24.6%	0.000
	Sector: Construction (for men)	4.6%	6.3%	0.018	-	-	-
	Early retirement propensity in the Industrial Committee*	-1.0%	1.8%	0.000	-3.6%	-2.0%	0.000
5 years before selection	Av. Full-Time Hourly wage	€ 20.2	€ 22.3	0.000	€ 17.0	€ 17.3	0.249
13 years before selection	Years with the same employers	10.6	11.8	0.000	11.1	11.7	0.000
1990-1997	Av. Working time (%)	98.0	97.5	0.002	95.8	96.2	0.272
1957-1997	Experience in years	31.2	31.0	0.071	30.1	29.7	0.023
	Av. Earnings in the year	€ 29,010	€ 30,247	0.000	€ 23,831	€ 23,368	0.141
N individuals	Sample Size (Unweighted)	1,227	29,791		762	9,658	
	Represented Population Size (Weighing Sample Size - $W_{cr,i}$)	3,863	75,778		1,261	14,609	

** Estimation based on a linear probability model using the complete sample of 243,655 individuals (cf. Section 3.4.2) on the period 1998q1-2002q3. We regress "transiting to a bridge pension" on dummies for the Industrial Committee (IC) or Nace if IC is missing, birth cohort dummies and gender. The retained variable contains the coefficients (i.e. marginal effect) of the IC dummies. In a sensitivity analysis we replace these marginal effects (estimated by OLS) with the predicted probabilities (estimated by a probit model) at the average Xs. Results are very similar and available upon request.*

[23] Note, however, that the measure of experience in Table 3.1 is likely overestimating the definition used in the determination of the eligibility to a statutory pension, and thus overstates the likelihood of eligibility.

Figure 3.1: Raw Sample - Differences in Survival Rate in Employment by Treatment Status



Differences in survival rate by treatment status (ATT). The survival is defined with respect to employment and the point estimates are expressed in percentage points (pp). Pooled estimates for the treated samples of 2003 and 2004. The pooled sample is composed of 1,227 male (762 female) treated and 29,791 male (9,658 female) control units. Standard errors are cluster robust to take into account correlation between the same individuals in the two sample.

To have a first idea on the possible effect of the TC we compare in

Figure 3.1 the survival rate in employment of treated and control individuals following the treatment. Treated individuals show a higher likelihood to survive in employment in the first two years (four for women) but the cumulated effect on the survival rate reverses and reaches a maximum of -20% for men and -15% for women in the sixth/seventh year. From this descriptive evidence it seems that individuals entering the schemes tend to remain in employment for a few years longer. This is consistent with the hypothesis that they initially continue working as to accumulate pension rights of a full-time worker. However, as time evolves they are more likely to become eligible to the statutory early retirement scheme and, because they earn a higher replacement income than those who did not take-up TC (Section 3.3) they eventually withdraw faster from the labour market.

By the end of the period of analysis, i.e. 2011q3, 93.7% of the treated units are no longer employed while this share is only 81.8% for the control sample. The specific exit destinations for the treated and control group can be found in Table 3.2. The main difference in behaviour is that control units tend to use the bridge pension as the most common pathway to retirement while for the treated units this is the statutory *early* retirement. This is related to how participation in TC affects the benefit entitlement within these different early retirement schemes. As explained in Section 3.3, the statutory *early* retirement pension for a beneficiary of TC is calculated on the basis of the fictitious earnings that the employee would have had if she had not reduced her working time. By contrast, TC does decrease the benefit level of the bridge pension. In Section 3.6 we will show that the descriptive evidence in this section is qualitatively not much affected if we take the compositional differences between treated and control groups into account.

Table 3.2: Exit Destinations (%) by Treatment Status

	Treated (%)	Controls (%)
Statutory *early* retirement	48.5	23.2
Bridge pension	34.2	38.5
Unemployment	1.7	2.0
Statutory retirement at the legal retirement age	4.1	8.8
Other inactivity	5.2	9.3
Never exit	6.3	18.2
Sample size (Unweighted)	1,989	39,449

Exit destinations of the treated and control group in the quarter after leaving employment in % of total exits. Note that some individuals never exit and remain employed until the end of the period of analysis. Unemployment contains the Canada Dry. Other inactivity includes among others people who die and a small minority (0.6%) of exits to disability schemes. Differently from other countries, these schemes are not particularly generous compared to the early retirement scheme.

3.5. Empirical Strategy

3.5.1. *Notation and the Treatment Effect of Interest*

We are interested in estimating the average treatment effect on the treated (ATT) in TC on the survival rate in employment.[24] An exit from employment is defined as soon as an individual is not observed in employment at the end of a quarter. In case of an exit, 93% of the individuals never return to employment before the statutory retirement age. This means that this exit is in most cases equivalent to an early withdrawal from the labour market, i.e. a pathway to retirement. We impose that once a treatment has started it cannot be reversed. This means that the treatment status is not affected in the rare cases that the TC scheme is left immediately after having entered it.

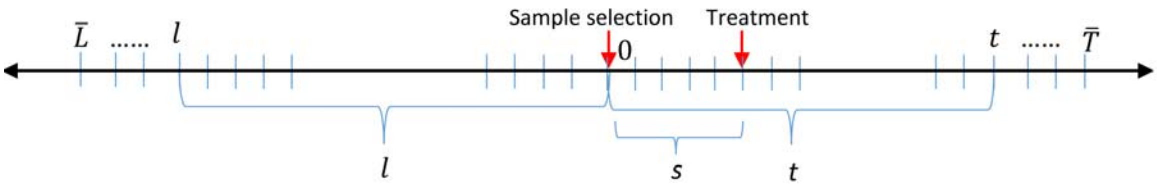
This evaluation problem is very similar to the one described by Vikström (2014). Since we follow Vikström's methodology closely, the exposition in this section is also analogous. The essential differences are the following. First, we generalize his procedure to allow for the endogenous sampling present in our data. Second, we do not base the evaluation on a *flow* sample of individuals in a state, but instead a *stock* sample of individuals who have been employed for at least 5 years at sample selection.[25] If we normalize time to zero at the beginning of these two periods (2003 and 2004 analyses) and consider the residual duration from then, we argue that the analysis does not require any adjustment. Third, we propose a different trimming rule for the determination of a common

[24] As a second outcome the survival rate in employment without sickness leave is considered. Since this does not affect the empirical strategy we ignore this in the further discussion of this section.

[25] The 5 years of tenure condition is imposed at the end of 2002 (2003) for the first (second) treatment and control group, while sample selection requires the individual to be employed at the end of 2003Q3 (2004Q3) - the first condition mentioned in Section 3.4.2. Thus, at the end of 2003Q3 (2004Q3) 99.75% of the selected individuals have at least 5.75 years of tenure and 20.75 year of labour market experience. We ignore for simplicity this detail in the exposition of the empirical strategy.

support for treated and control units. Finally, we propose a slightly different bootstrap procedure for inference on the pooled sample.

Figure 3.2: Graphical Representation of the Notation



In Figure 3.2 we provide a graphical representation of the introduced notation. Let $t \in \{1,2, \dots \bar{T}\}$ denote the number of years since sample selection and $l \in \{1, 2, \dots, \bar{L}\}$ the elapsed number of years in employment at this start. $\bar{T}$ and $\bar{L}$ are the maximum number of years in employment respectively, after and before selection. In the data $l \geq 5$, because this is an eligibility condition for the TC and a sample selection criterion (Section 3.4.2). The random time since sample selection until the start of the treatment, i.e. entry in TC, is denoted by S and its realization by s, where $s \in \{0,1, \dots, \bar{S}\}$ and $\bar{S} \leq \bar{T}$. $Y_{l+t}(s)$ is equal to one in case employment is left in year $l + t$ and treatment started in year $l + s$, and zero otherwise. $Y_{l+t}(\infty)$ denotes the potential outcome in year $l + t$ if never treated and Y_{l+t} the observed outcome. $\bar{Y}_{l+t}(s) \equiv \{Y_1(s), Y_2(s), \dots, Y_{l+t}(s)\}$ and $\bar{Y}_{l+t} \equiv \{Y_1, Y_2, \dots, Y_{l+t}\}$ denote, respectively, the sequence of potential and of observed outcomes.

We aim at identifying the average treatment effect on the treated (ATT) of treatment s years after sample selection against the counterfactual of never being treated on the residual survival in employment until year $t > s$, given survival in employment until sample selection:

$$\forall t > s: ATT_t(s) \equiv E_L\{E[\bar{Y}_{L+t}(s) = 0|S = s, \bar{Y}_{L+s}(s) = 0] \\ - E[\bar{Y}_{L+t}(\infty) = 0|S = s, \bar{Y}_{L+s}(s) = 0]|L \geq 5\} \quad (2)$$

This extends the ATT as parameter of interest to evaluation of a stock sample. Since in a stock sample individuals may have a different elapsed duration, the conditional expectation is taken over these elapsed durations, conditional on being employed for at least 5 years to take into account that one needs at least 5 years of tenure to be eligible for TC. Observe that $E[\bar{Y}_{L+t}(.) = 0|S =., \bar{Y}_{L+s}(.) = 0] = Pr[T > L + t|S =., T > L + s]$ holds, i.e. the conditional probability of surviving $L + t$ years in employment given survival until $L + s$. In case $L = 0$, Equation (2) reduces to the corresponding expression in Vikström (2014) for a flow sample.[26]

[26] In contrast to e.g. Crépon et al. (2009), Vikström (2014) does not impose $t > s$. This means that he also considers the $ATT_s(s)$, i.e. the treatment effect in the period that the treatment starts. However, identification of this treatment effect requires that the treatment starts instantaneously at the start of this period. Without this assumption the control units are longer at risk of leaving employment than the treated, because treatment is only possible if one survives in employment until

3.5.2. *Identification*

In order to identify $ATT_t(s)$ we use two identifying assumptions: CIA and no anticipation (NAA). These assumptions can be formalized as follows:

$$\boldsymbol{CIA}\ \forall l > 5, \forall s, \forall t > s: S\ \perp\ Y_{l+t}(s)\mid X \tag{3}$$

and

$$\boldsymbol{NAA}\ \forall l > 5, \forall t < min(s', s''): Pr(Y_{l+t}(s') = 1) = Pr(Y_{l+t}(s'') = 1), \tag{4}$$

The latter condition means that individuals do not alter their behaviour in response to a future assignment to the treatment. Based on these assumptions Fredriksson and Johansson (2008), Crépon et al. (2009) and Vikström (2014) prove that for $l = 0$ $ATT_t(s)$ can be identified by successively using the not yet treated at $l + t$ to estimate the exit rate under no treatment at $l + t$ for those treated at $l + s$. Vikström (2014) generalizes by explicitly allowing for selectivity on observables in subsequent assignments into treatment. We follow his approach. Because the identification proof is not affected for different values of l, we refer the reader to Vikström (2014).

Before discussing estimation and inference, we first argue why we believe that the available data are sufficiently rich to justify identification on the basis of the CIA, i.e. that we observe all relevant determinants that influence the decision to participate in TC as well as the survival rate in employment. With regards to the NAA, we refer to our discussion in Section 3.4.2.

The literature mentions the following key determinants of gradual retirement also affecting the survival in employment: age, household composition, place of living, entitlement to (early) retirement benefits, education, health and firm characteristics, such as size and sector, but also the degree of unionization, organizational features and staff related issues, such as staff and skilled workers shortages (e.g. Gustman and Steinmeier, 1984; Honig and Hanoch, 1985; Huber et al., 2015) . The exhaustive information on the labour market history since 1957 allows us to condition on the most essential information required to determine the level of (early) retirement benefits to which workers are entitled. Household information is sufficiently available. By contrast, the database does not contain information on the level of education and the available indicators of health can only be used as outcomes, not as conditioning variables (see Section 3.4.2). Nevertheless, we believe that this is not problematic, because health problems should be indirectly captured by gaps in the labour market experience, in lower level of earnings, and being a blue collar worker or not. A similar reasoning applies to the level of education. Huber et al. (2015) stress the importance of having rich firm characteristics

---

the treatment start. This biases the treatment effect upwards. Since we choose the period lengths to be equal to one year, this bias could be substantial.

to condition upon. However, we believe that this is less important in our institutional environment than in theirs. In Germany firms decide on whether they make the partial retirement available to their employees. By contrast, in Belgium TC is a right to which employees are entitled without consent of the employer, at least in firms larger than 10 employees on which we focus in this analysis. Nevertheless, it is still crucial to condition on the available firm characteristics, i.e. on the firm size and sector, because these are highly correlated with the degree of unionization, and, more relevant for the Belgian context, with the working conditions. This is because working conditions, negotiated in the *sectoral* industrial committees, usually depend on firm size and apply to all workers, irrespectively of their union membership. Finally, a major pathway to retirement is the bridge pension, so that it is important to control for factors that influence the transition to it. We have seen in Section 3.2 that the availability of the bridge pension depends importantly on which industrial committee the worker belongs to. Because the number of industrial committees is too large to condition upon in the analysis, we therefore constructed a continuous measure of the propensity of transition to a bridge pension. How this measure is constructed is explained in a footnote to Table 3.1.

3.5.3. *Estimation and Inference*

Vikström derives the Inverse Probability Weighting (IPW) estimator, introduced by Horvitz & Thompson (1952) and Hirano et al. (2003), to estimate the $ATT_t(s)$ defined in (2). We follow this approach for the following reasons: (i) Busso et al. (2014) find in their Monte Carlo simulation that the normalized IPW estimator is one of the best performing matching estimators in the presence of good overlap. Other Monte Carlo simulations of (Huber et al., 2013; Frölich et al., 2015) confirm the good performance of the IPW estimator, although it does not outperform other Propensity Score-based and non-parametric estimators; (ii) It is easy to integrate the endogenous sampling weights. This merely requires to include an additional weight in the estimation; (iii) Compared to other matching estimators, the IPW estimator is simple and computationally fast.

We provide the most general estimator that does not only allow to take into account selective (on observables) right censoring as a consequence of not yet treated individuals getting treated, but also more general forms of selective right censoring that may involve both treated and not yet treated individuals. For instance, we will consider estimations of the treatment effect on different exit destinations when terminating employment (i.e. a competing risk framework). We will distinguish between exits to bridge pensions, statutory *early* retirement and “other” exit routes. When considering one destination, the other ones are right censored.

To be able to write down the estimator, let us denote the random censoring duration since sample selection for individual i by C_i. If we then generalise Vikström’s formula (see his Appendix A.1) for the endogenous sampling weights $W_{cr,i}$ and take the elapsed employment duration l_i into account, we obtain:

$$\widehat{\mathrm{ATT}}_t(s) = \prod_{k=s+1}^{t}\left[1 - \frac{\sum_i W_{cr,i} * W^C_{l,k(s),i}(s)\, Y_{k,i}\, 1\left(\bar{Y}_{l+k-1,i} = 0\right)1(S_i = s)1(C_i > s)}{\sum_i W_{cr,i} * W^C_{l,k(s),i}(s)\, 1\left(\bar{Y}_{l+k-1,i} = 0\right)1(S_i = s)1(C_i > s)}\right] - \prod_{k=s+1}^{t}\left[1 - \frac{\sum_i W_{cr,i} * W^C_{l,k(\infty),i}(s)\, Y_{k,i}\, 1\left(\bar{Y}_{l+k-1,i} = 0\right)1(S_i \geq k)1(C_i \geq k)}{\sum_i W_{cr,i} * W^C_{l,k(\infty),i}(s)\, 1\left(\bar{Y}_{l+k-1,i} = 0\right)1(S_i \geq k)1(C_i \geq k)}\right] \tag{5}$$

where

$$W^C_{l,k(s),i}(s) = \frac{1}{\prod_{m=s+1}^{k}[1 - c_m(X_i, l_i)]}$$

$$W^C_{l,k(\infty),i}(s) = \frac{p_s(X_i, l_i)}{1 - p_s(X_i, l_i)} \frac{1}{\prod_{m=s+1}^{k}[1 - p_m(X_i, l_i)][1 - c_m(X_i, l_i)]}$$

$$p_t(X_i, l_i) = \Pr(S_i = t | X_i, S_i \geq l_i + t, \bar{Y}_{l+t-1,i} = 0)$$

$$c_t(X_i, l_i) = \Pr(C_i = t | X_i, S_i \geq l_i + t, \bar{Y}_{l+t-1,i} = 0)$$

where X_i denotes the vector of predetermined explanatory variables, $W^C_{l,k(s),i}(s)$ and $W^C_{l,k(\infty),i}(s)$ are the IPW weights in year $l_i + k$ for individual i treated in year $l_i + s$ and not yet treated in year $l_i + k$, respectively. $p_t(X_i, l_i)$ and $c_t(X_i, l_i)$ denote the conditional probability of entering the treatment, respectively censoring state after $l_i + t$ years conditional on still being employed in $l_i + t - 1$. In other words, they represent the discrete hazard of entering treatment, respectively censoring in year $l_i + t$.

To clarify the intuition of the estimator defined in Equation (5) consider first the case without right censoring, i.e. $\boldsymbol{C_i} = \infty$, $W^C_{l,k(s),i}(s) = 1$ and $W^C_{l,k(\infty),i}(s) = \frac{p_s(X_i,l_i)}{1-p_s(X_i,l_i)} \frac{1}{\prod_{m=s+1}^{k}[1-p_m(X_i,l_i)]}$. Apart from the weights the first sequence of products in (5) is the standard Kaplan Meier survivor estimator for the treated group. This represents the conditional survival rate in employment until year $l_i + t$, conditional on treatment and survival in employment until year $l_i + s$, i.e. the product of one minus the discrete hazards from employment between $l_i + s + 1$ and $l_i + t$. The second sequence of products is a similar Kaplan Meier estimator for the control group (or not yet treated individuals), which estimates the survival rate of the treated in the counterfactual of no treatment. In order to make these control units comparable to the treated they are reweighted using the standard IPW weights $\frac{p_s(X_i,l_i)}{1-p_s(X_i,l_i)}$ in a static evaluation approach, where $p_s(X_i, l_i)$ is the estimated Propensity Score (PS) for an individual treated in year $l_i + s$. However, to take into account that not yet treated individuals gradually become treated, we must consider that this may change the composition of the control group over time. Hence, Vikström (2014) shows that we must in addition weigh the control units by $\frac{1}{\prod_{m=s+1}^{k}[1-p_m(X_i,l_i)]}$, i.e. by the inverse of the probability of *not yet* being treated in each period between l_i+s+1 and l_i+t.

If individuals are right censored before exiting to the destination of interest and this is selective (i.e. depends on X), then this may similarly gradually change the composition of now not only the control group, but also of the treatment group over time. We therefore need to weigh both treated and control samples by $\frac{1}{\prod_{m=s+1}^{k}[1-c_m(X_i,l_i)]}$, i.e. the probability of not yet being right censored in each period between l_i+s+1 and l_i+t.

In contrast to Vikström (2014), the discrete hazards to treatment and censoring depend on the elapsed employment duration l_i at sample selection.[27] Observe that we can only proxy for this elapsed employment duration, because prior to 1998 we only have annual (instead of quarterly) information on private sector employment and no information on self-employment, neither on employment as civil servant. Given that we selected individuals with at least 5 years of tenure and 20 years of experience in the private sector, we believe that the bias induced by using this proxy is negligible.

We estimate separate ATTs for individuals entering treatment in 2003 and 2004. Subsequently, we pool, as Vikström, these analyses to have more precise estimates. This is done by averaging the estimated ATTs in each survival year, taking into account the size of the two different treated groups in the population, and, hence, weighted by the endogenous sampling weights.

$$\widehat{ATT}_t = \sum_s \frac{n_s}{\sum_s n_s} \widehat{ATT_{s+t}}(s) \tag{6}$$

where $n_s \equiv \sum_i W_{cr,i} *) \, 1(S_i = s)1)$.

As the lack of overlap of the PS can bias the estimator and increase the variance (Lechner and Strittmatter, 2014), we trim treated units with too high PS not having a correspondent control unit in a neighbourhood. In particular, we remove the treated units with a PS above the 99.9 percentile of the control units. After trimming, we remain with about 99% of the treated units, counting 1,212 and 755 men and women. (Huber et al., 2013) propose to remove the control units with a weight higher than 4% of the total. However, because the sample of control units is large, this additional trimming is not required. In the four analyses (2003 and 2004, men and women) the highest relative weight is only 0.17% of the total sample.

By weighting, the Least Squares estimator becomes heteroskedastic. In addition, an individual in the 2003 sample may appear a second time in the 2004 sample. To take the resulting correlation in

[27] Because the Kaplan Meier estimators in Equation (6) are averaged over all individuals in the sample, we average, as in the definition of the ATT in Equation (2), implicitly over the elapsed employment durations, conditional on this elapsed duration to exceed 5 years.

account, we cluster the standard errors by individual. However, standard clustering does not take into account that the PS in the weights $W_{l,k(.),i}^{C}(s)$ are estimated. We therefore bootstrap the standard errors. Since our data come from an endogenously stratified sample, we cannot apply a standard bootstrap. Instead, we implement a *stratified bootstrap* by randomly drawing for each replication n_{cr} individuals within each cohort-stratum cr. This is valid because the bootstrap randomly samples individuals within each cohort-stratum (for a review on bootstrap and stratified data see e.g. Shao, 2003). To take individual serial correlation into account we re-sample within each replication the same individuals (i.e. clusters) in the two analyses (2003 and 2004 sample).

In general, based on observables, the selectivity into treatment is low in 2003. The Pseudo R-squared of a standard logit model is 0.068 and 0.026 for men and women. The selectivity is slightly higher in 2004. The corresponding Pseudo R-squared are 0.127 and 0.084. In terms of specification, the IPW estimator performs well in balancing the distribution of the covariates. Once reweighting the control units by $W_{l,1(\infty),i}^{C}(s)$, in the worse scenario (women selected in 2004) the median Standardized Bias (SB) is as low as 1.2%, the highest SB is 2.9%, the Pseudo R-squared of the reweighted sample is 0 and the Wald test[28] for the joint significance of the variables after the reweighting produces a pvalue of 1. In Appendix 3.9, we report the full list of balancing tests (Table 3.6). The corresponding diagnostics for the other models such as heterogeneous effects can be obtained from the authors on request.

3.6. Results

We first report the $\widehat{ATT_t}$ from the first until the eight year after entry in the TC ($t \in \{1, \dots 8\}$) on the main outcome of interest. In Section 3.3 we have argued that, relative to full-time workers, the TC scheme increases the incentives to enter early retirement, in particular the early statutory retirement scheme from the age of 60. Moreover, we argued that these incentives were more important for participants in the 50% TC regime than those in the 20% regime. In Section 3.6.2, we demonstrate that the empirical evidence is in line with these incentives. To that end we estimate the different $\widehat{ATT_t}$ of each TC regime separately for the three following exit destinations of employment: bridge pension, statutory early retirement and other exits. In this section we also split the sample according to age to study treatment heterogeneity for the benchmark outcome. Finally, in Section 3.6.3 we study the effect of the TC on the health of the participants. We do not consider sick leave as a separate exit destination, because the number of exits to this destination is too small for credible inference. Instead we broaden the benchmark outcome by including temporary sick leave as an additional failure risk and

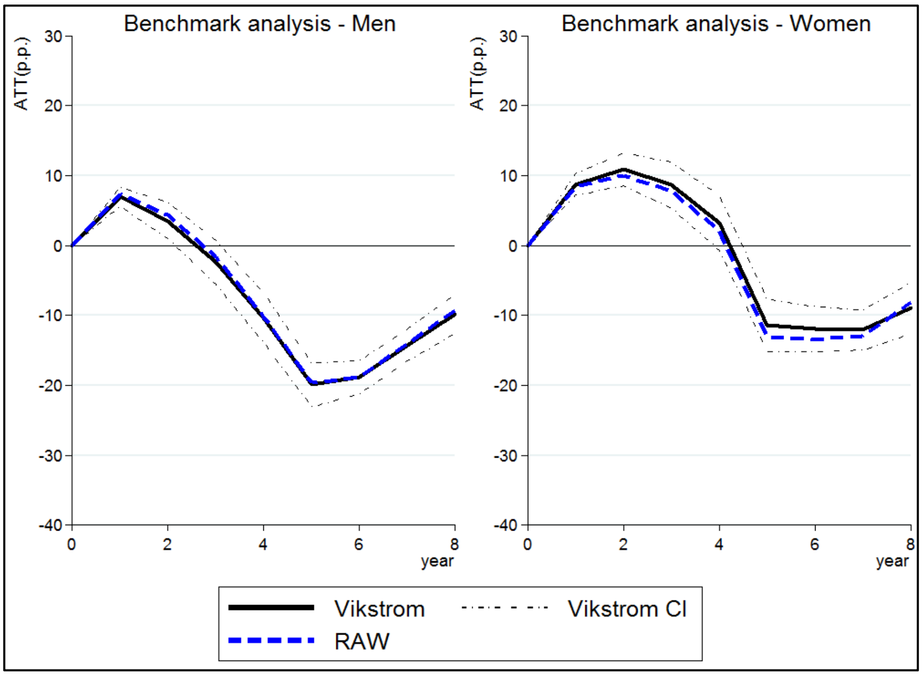
[28] The Wald test should be used to assess the balancing as the log-likelihood ratio test proposed by Sianesi (2004) is not robust against heteroskedasticity and serial correlation. It tends to over-reject the null of good balancing.

compare the estimates to the ones considering non-employment as the only exit. All the analyses are conducted separately for men and women and by pooling the 2003 and 2004 sample.

3.6.1. The Benchmark Analysis

In this section we show the estimates of the ATT on the survival rate in employment controlling for selection on observables (Vikström, 2014). As Figure 3.3 shows, estimates are not significantly different from the descriptive evidence shown in Section 3.4.3. Treated men are more likely to survive in employment in the first two years (+7 pp and +3.5 pp), while for women the positive effects last until the fourth year and are stronger (+8.8 pp, +10.9 pp, +8.6 pp and +3.2 pp). The more positive effect for women is partly, but not completely, explained by the fact that they are less likely to work in industrial committees that intensively use bridge pensions as a way to terminate employment early, especially in the 20% regime (see below). However, the results also confirm the strong subsequent negative effects, peaking up to -20 pp and -12 pp for men and women, respectively. With the ageing of our sample most of the individuals exit the labour market which eventually leads the last ATTs to a convergence to zero. From these estimates, it seems that controlling for the rich set of covariates does not significantly affect the differences in survival rates between the treated and the control groups. This leads us to argue that selection on unobservables is also negligible (Altonji et al., 2005).

Figure 3.3: ATT on Survival in Employment and Descriptive Evidence



ATT on the survival rate in employment estimated by controlling for the dynamic selection on observables (Vikström, 2014) and descriptive evidence (RAW) as reported in

Figure **3.1**. *The estimates of the ATT's are the percentage points (pp) differences between the survival rate of the treated in case of treatment and the estimated survival rate of the treated in the counterfactual of no treatment. Estimates are pooled over the 2003 and 2004 samples. Year eight only uses information from the 2003 sample. Standard errors are obtained by a stratified bootstrap (clustering by individual) with 500 repetitions and 95% confidence intervals (CI) by assuming normality.*

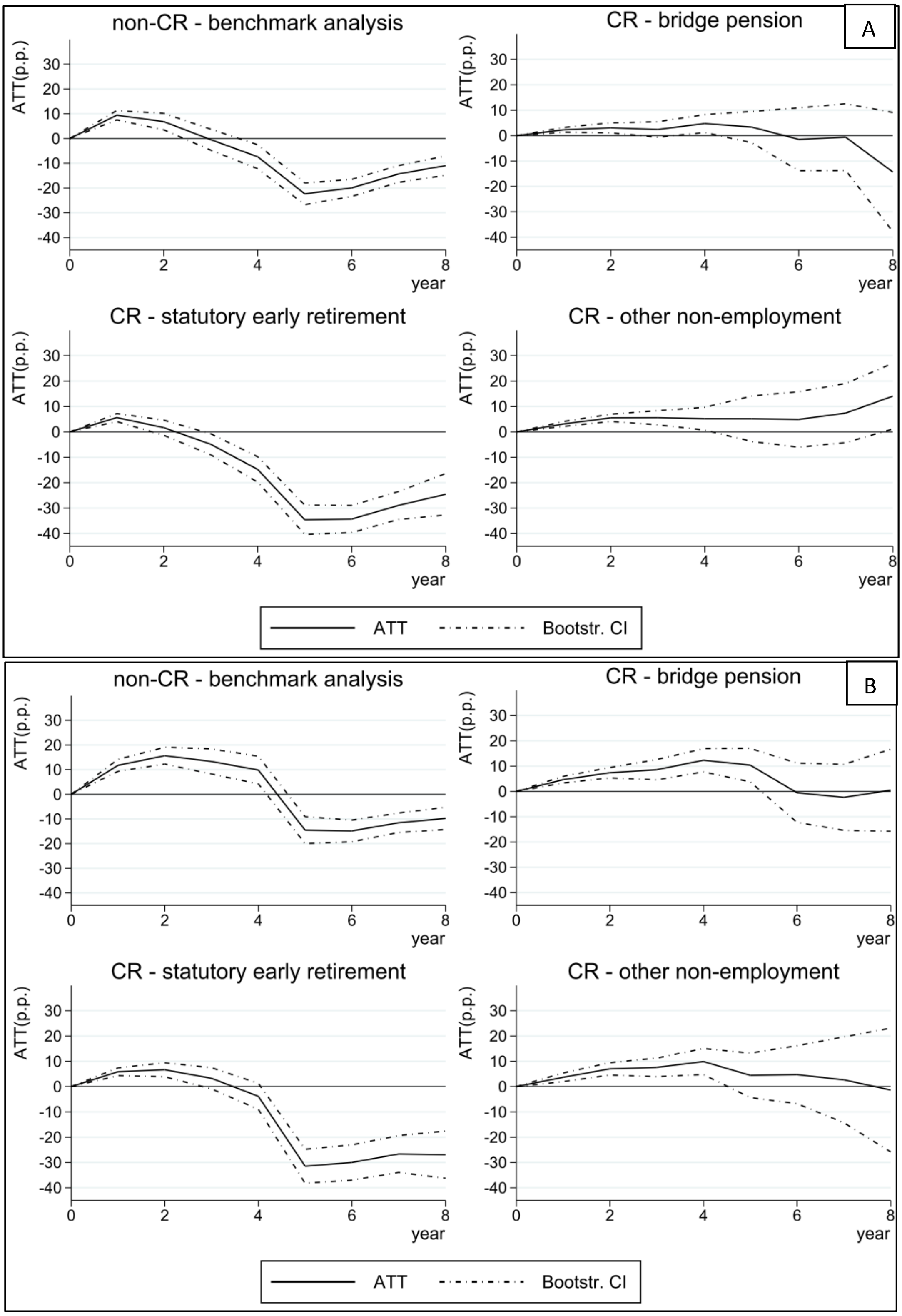
Our results are in line with the findings of Graf et al. (2011) in Austria. Similarly to the Austrian scheme, our estimates show that individuals entering the schemes tend to remain in employment for a few years longer. However, the cumulative impact becomes negative after some years. The positive effects in the beginning are likely present because, as long as they are not eligible to early retirement, the TC participants keep on working part-time while accumulating pension rights of a full-time worker. However, as these workers also start to satisfy the age and experience requirements, they gradually enter the most remunerating early retirement scheme. To gain more insights into the forces driving these results, we decompose in Section 3.6.2 the ATTs by distinguishing between the TC-regime (20% or 50%), the destination-specific survival rates in employment and the age of the participants.

3.6.2. *Competing Exit Destinations and Different Treatment Regimes*

In this section we decompose the estimated ATT on the survival in employment according to three possible exit destinations: (i) bridge pension, (ii) statutory *early* retirement and (iii) other exits. The last is a residual category comprising other schemes such as Canada Dry, unemployment, disability and other forms of inactivity, and exit because the individual deceased. When considering one type of exit, the other ones are right censored (as in a competing risk framework). Recall that we have shown in the descriptive analysis in Section 3.4.3 that most of the exits are to the first two destinations. For a better understanding of the role of financial incentives, we divided the treated sample into participants in the 50% (Figure 3.4) and 20% TC-regime (Figure 3.5). The treated sample is divided in 942 units participating in the 50% TC-regime and 1,047 in the 20% regime. In the corresponding population 54.3% of TC participants are in the 50% regime. Note that the same control units are used for estimating the ATTs of these two treatment groups. Estimates of the competing risks for the whole sample of participants irrespectively of the TC-regime are shown in Figure 3.6, which also displays the sensitivity of the ATTs to the estimation method. To have an idea of the relative size of the ATT we report the survival rates of the treated and (reweighted) control units in Figure 3.10 of Appendix 3.9.

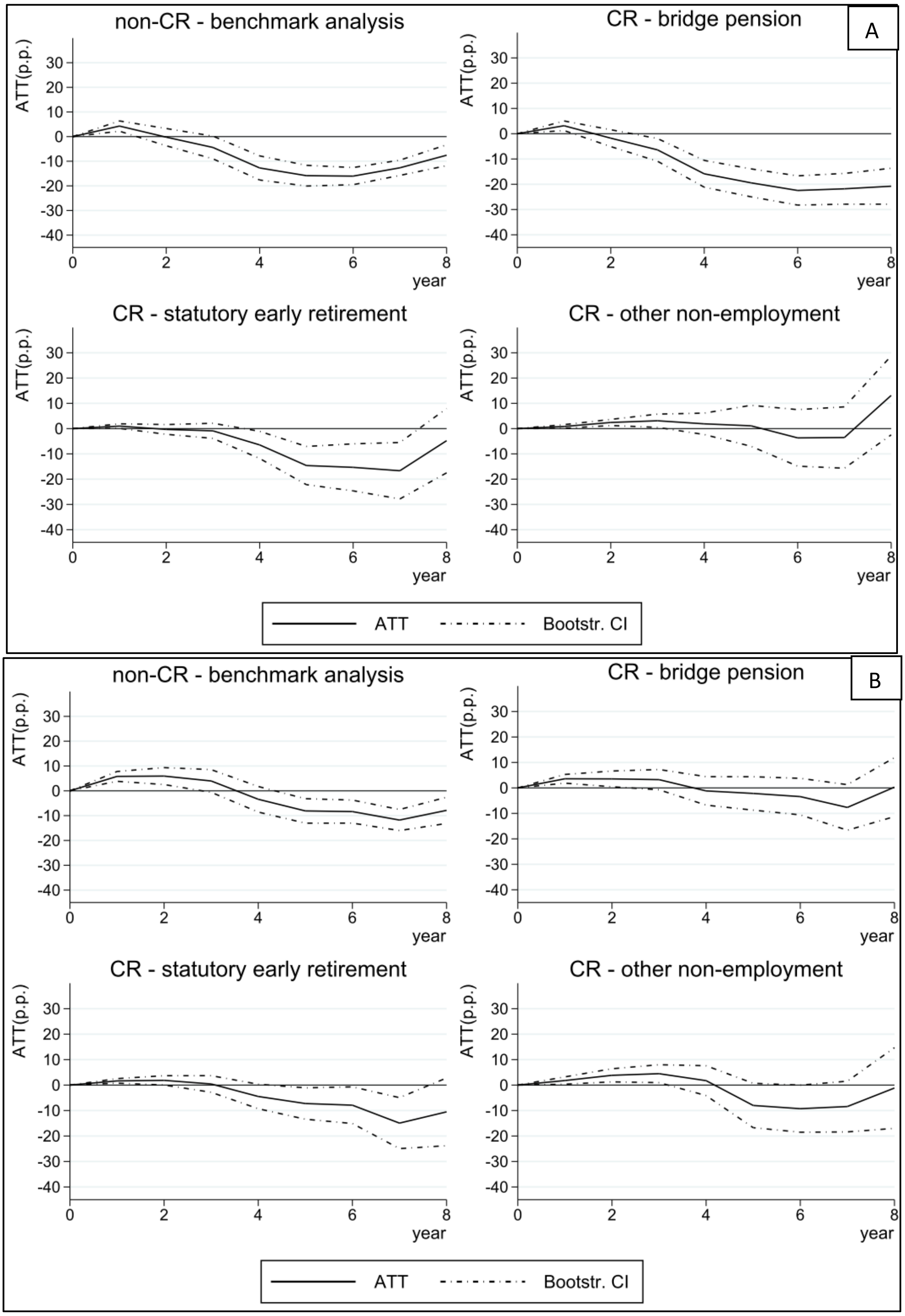
The effect on the survival in employment is more pronounced for the people participating in the 50% TC-regime (Figure 3.4). As these people are more intensively treated, the positive ATT in the short-run and the negative ATT in the medium-run are also more pronounced compared to the participants in the 20% regime. As treated individuals remain entitled to the statutory (early) pension as full-time workers, their replacement rate is much higher than that of control individuals. This explains the strong response on the survival in employment without exit to *statutory* early retirement. Once we right censor these exits to *statutory* early retirement, the effect for TC participants with the 50% reduction is non-negative for the other two exit destinations.

Figure 3.4: ATT Men (A) & Women (B) in 50% TC - Competing Risk (CR) & Baseline (non-CR)



ATT of treated in the 50% TC on the survival rate controlling for the dynamic selection on observables (Vikström, 2014). The ATTs are differentiated by gender: Panel A for men and B for women. The estimates are expressed in percentage points (pp) differences in the survival rate in (from left to right and top to bottom) (1) employment, (2) employment without exit to a bridge pension, (3) employment without exit to a statutory pension before the normal retirement age (65 for men and women born after 30/11/1944, 64 for women born between 01/12/1942 and 30/11/1944 and 63 for women born before 30/11//1942) and (4) employment without exit to other non-employment statuses. In the competing risk analyses (2-4), the exits from employment to other destinations, apart from the one considered, are right censored. Reported estimates are pooled over the 2003 and 2004 samples. Year eight only uses information from the 2003 sample. The pooled sample is composed of 567 (375) treated and 29,791 (9,658) control units (men and women). Standard errors are obtained by a stratified bootstrap (clustering by individual) with 500 repetitions and 95% confidence intervals (CI) by assuming normality.

Figure 3.5: ATT Men (A) & Women (B) in 20% TC - Competing Risk (CR) & Baseline (non-CR)



ATT of treated in the 20% TC on the survival rate controlling for the dynamic selection on observables (Vikström, 2014). The ATTs are differentiated by gender: Panel A for men and B for women. The estimates are expressed in percentage points (pp) differences in the survival rate in (from left to right and top to bottom) (1) employment, (2) employment without exit to a bridge pension, (3) employment without exit to a statutory pension before the normal retirement age (65 for men and women born after 30/11/1944, 64 for women born between 01/12/1942 and 30/11/1944 and 63 for women born before 30/11//1942) and (4) employment without exit to other non-employment statuses. In the competing risk analyses (2-4), the exits from employment to other destinations, apart from the one considered, are right censored. Reported estimates are pooled over the 2003 and 2004 samples. Year eight only uses information from the 2003 sample. The pooled sample is composed of 660 (387) treated and 29,791 (9,658) control units (men and women). Standard errors are obtained by a stratified bootstrap (clustering by individual) with 500 repetitions and 95% confidence intervals (CI) by assuming normality.

The treated individual working at 80% of a full-time have a less pronounced response, especially for the exit to statutory early retirement, and rather show more noticeable differences by gender (Figure 3.5). Different from the 50% regime, we now also observe for men a negative impact on the survival in employment without exit to the bridge pension, while for women the impact is insignificant.

As described in Section 3.3, while the bridge pension is in general very appealing for older workers in Belgium, it is not for workers in the 50% TC-regime, because they lose a large part of the benefits. However, because this loss is less important for individuals in the 20% regime, the scheme still remains attractive for them. This incentive does not affect the behaviour of women, however, because they are on average less likely to work in industrial committees that intensively use the bridge pension as instrument to terminate employment (see Table 3.1), and, hence, have less opportunities to use this pathway to retirement. More generally, because women tend to have acquired slightly less labour market experience (and have lower past earnings) than men (Table 3.1), they are less likely to be eligible for (and earn a decent income when entering) early retirement which may explain their better overall effects of TC on the survival rate in employment.

Next, as we argued that eligibility to early retirement affects the impact of the TC scheme, we split the sample between younger and older individuals. We set the cut-off age at 56.5 years at the moment of sample selection, i.e. at the end of year 0. In theory, the younger workers are too young to already be eligible to any of the early retirement schemes. Thus, we expect a longer-lasting positive effect for them. As shown in Figure 3.9 in Appendix 3.9, this is indeed what we find and corroborates therefore our interpretation of the findings.[29]

Finally, we studied to what extent it matters for our results to take the dynamic assignment into treatment into account (Fredriksson and Johansson 2008) and to, in addition, control for selective (on observables) right censoring (Vikström, 2014). In Figure 3.6 we therefore compare our estimates based on Vikström's (V) methodology to the descriptive estimator (raw data) on the one hand and to the estimator proposed by Fredriksson and Johansson (FJ) on the other hand. While in the descriptive evidence we ignore dynamic assignment to treatment in that we do not remove (i.e. exogenously right censor) controls becoming treated and do not control for differences in observables, we do when implementing the FJ estimator by IPW.

We deduce the following two observations from Figure 3.6. First, the *raw* estimates are not very different from the ones that take the dynamic assignment into treatment into account (FJ), except for the survival in employment without exit to statutory early retirement in which case the raw estimates

[29] We have also estimated treatment heterogeneity with respect to labour market earnings at selection. The results are very similar to the different response by TC regime as two thirds of the treated high earnings group take the 50% regime (symmetric figures for the low earnings group). Results are available from the authors upon request.

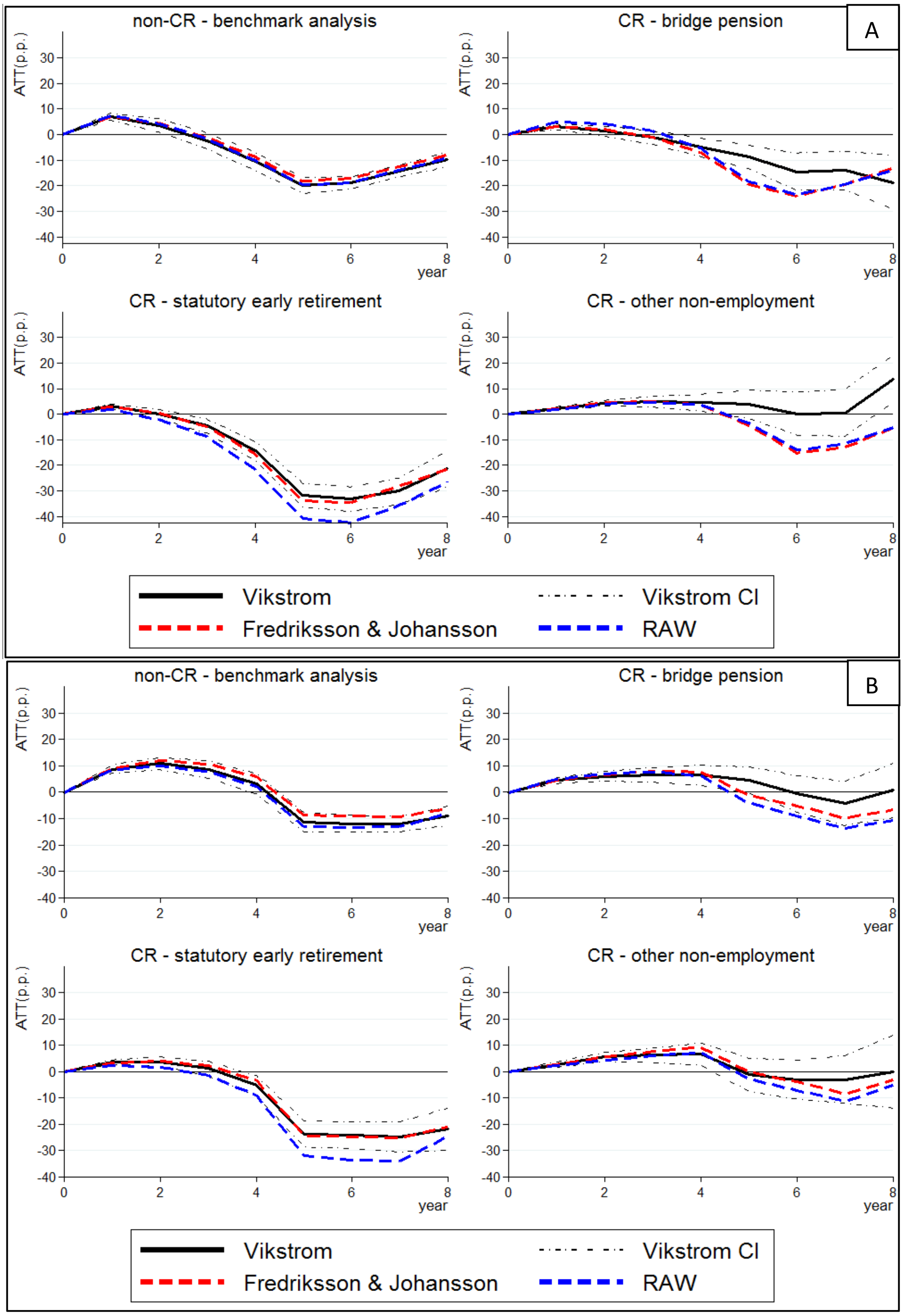
are downwards biased. Second, the FJ estimator is significantly downwards biased relative to the V estimator for the survival in employment without exit to the bridge pension and without other exit.

These observations lead to the following conclusions. First, in the benchmark model (without competing destinations) the estimates are not sensitive to the employed estimator. This is because the selection on observables is not important and the bias induced by the dynamic assignment to the treatment is small, as only a small fraction of the not yet treated group enters into treatment later on. Second, the estimation method matters more when analysing competing risks, because in this analysis the fraction that is right censored (in both treatment and control groups) is much more important than the dynamic assignment into treatment. If the right censoring is selective on observables, which is clearly the case for exits to the statutory early retirement,[30] then this bias can only be avoided by using the V estimator.

In conclusion, our estimates suggest that older workers use part-time work at the end of the career as an alternative path to (early) retirement. This enables them to continue working until they become eligible to early retirement schemes. At that moment TC participants have much higher incentives to enter these schemes than non-participants. Moreover, the evidence is consistent with participants acting upon these incentives, since they not only leave employment faster than non-participants, but they also choose the exit channel that offers them the highest replacement income. We therefore believe that these supply side incentives matter more than other explanations advanced in the literature for these negative effects on the survival rate in employment, such as the higher hourly workload (Devisscher and Sanders, 2007; Rudolf, 2014), the TC signaling a preference to retire (Machado and Portela, 2012), or the reduced working time decreasing the labour market attachment.

[30] Since exits to the statutory early retirement are treated as right censored observations for the other two destinations, this explains the second observation that we deduced from Figure 3.6. The fact that the effect on the survival rate in employment without exit to the statutory early retirement does not differ very much using either the V or FJ estimator suggests that exits to the aforementioned other two destinations are not very selective. On the other hand, the fact that these estimates do differ from the raw estimates reflects again that exits to this destination are selective on observables.

Figure 3.6: ATT - Men (A) and Women (B) and Comparison with Other Estimators

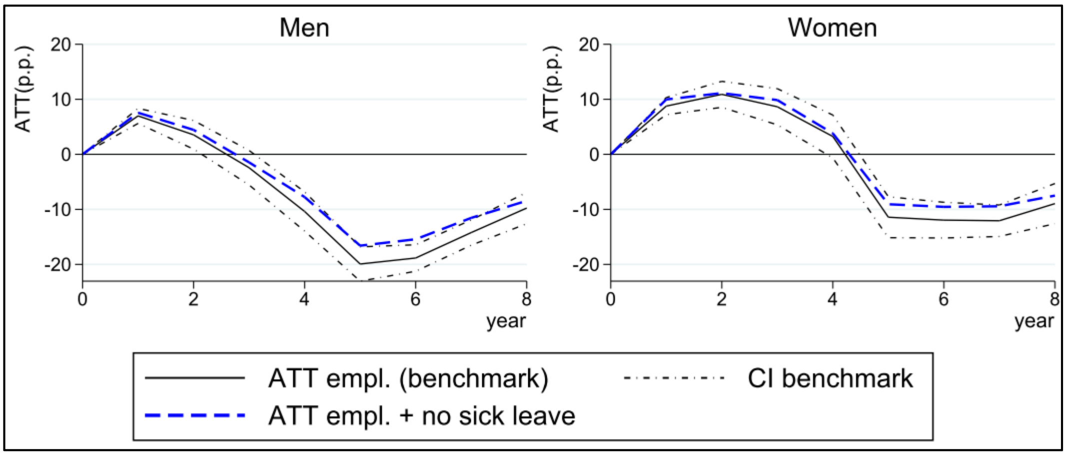


***Vikström**: ATT of treated on the survival rate controlling for the dynamic assignment to treatment & selective right-censoring on observables (Vikström, 2014); **Fredriksson & Johansson**: controlling for selection on observables in the year of selection (2003 or 2004) but not for selective right-censoring on observables (Fredriksson and Johansson, 2008); **RAW**: neither controlling for selection on observables, nor on dynamic assignment to treatment. The ATTs are differentiated by gender: Panel A for men and B for women. The estimates are expressed in percentage points (pp) differences in the survival rate in (from left to right and top to bottom) (1) employment, (2) employment without exit to a bridge pension, (3) employment without exit to a statutory pension before the normal retirement age (65 for men and women born after 30/11/1944, 64 for women born between 01/12/1942 and 30/11/1944 and 63 for women born before 30/11/1942) and (4) employment without exit to other non-employment statuses. In the competing risk analyses (2-4), the exits from employment to other destinations, apart from the one considered, are right censored. Reported estimates are pooled over the 2003 and 2004 samples. Year eight uses information from the 2003 sample. The sample is composed of 1,227 (762) treated and 29,791 (9,658) control units (men and women). Standard errors are obtained by a stratified bootstrap (clustering by individual) with 500 repetitions and 95% confidence intervals (CI) by assuming normality.*

3.6.3. *Including Sick Leave as Additional Exit Destination*

It has been argued that granting working time reductions can reduce the incidence of sickness (Ahn, 2015). In this section we aim at testing this hypothesis by checking whether TC recipients are less likely to enter sick leave (while employed) than non-recipients. A first observation is, however, that few individuals in our sample do enter sick leave. In the control group the fraction is 14.8%, while in the treated group it is 13.0%. This is because our sample selection criteria exclude people with fragile health. More specifically, by imposing that workers should have at least five years of tenure in a firm, we exclude individuals who have temporarily interrupted employment during a full quarter, while including individuals who have been on sick leave *throughout* this quarter (Section 3.4.2). Because the fraction of individuals entering sick leave is small, the impact of TC on this indicator of health cannot be large. Nevertheless, we estimate the ATT on sick leave to get a sense of the direction of the effect.

Figure 3.7: ATT on the Survival in Employment & Employment and Not on Sickness Leave



ATT on the survival rate estimated by controlling for the dynamic selection on observables (Vikström, 2014). The survival is in employment (ATT empl.) or in employment while not on sick leave (ATT empl + no sick leave). The estimates are expressed in percentage points (pp). Estimates are pooled over the 2003 and 2004 samples. Year eight only uses information from the 2003 sample. The pooled sample is composed of 1,227 (762) treated and 29,791 (9,658) control units (men and women). Standard errors are obtained by a stratified bootstrap (clustering by individual) with 500 repetitions and 95% confidence intervals (CI) by assuming normality The CI reported are referred to the benchmark scenario having employment as the outcome (ATT empl benchmark).

Since so few individuals entered sick leave, we did not consider sick leave as a separate exit destination in the analysis, because the sample would become too small. Instead, we include sick leave as an additional exit destination, so that we estimate the impact of the TC on being employed *and* in good health. We then compare this treatment effect to the one we obtained for the benchmark model (Section 3.6.1). In Figure 3.7 we report these two treatment effects. We observe that the new treatment effect (dashed line) always exceeds the benchmark (solid line). This suggests that TC reduces the incidence of sick leave. However, the dashed line is always comprised in the 95% confidence interval of the benchmark ATT, so that the effect is not statistically significant.

3.7. Cost-Benefit Analysis

To obtain an order of magnitude of the costs and benefits of the TC for the government budget and for society, we perform a cost-benefit analysis (CBA) along the lines proposed by Staubli and Zweimüller (2013). To that purpose we make use of the information available in the administrative dataset on the benefits and gross wages that are paid out to participants and non-participants in the TC. This information is then used to calculate for each individual and in each of the 8 years of analysis after the year of (counterfactual) entry in TC the real costs (or gains) in constant 2004 Euros for the government budget and for society. We weigh the control group by the appropriate IPW to make them comparable to the treated group and calculate for each of these years the average difference of these costs (gains). This provides an estimate of the average net cost (gain) per participant in TC for the government and society in each of these eight years (Staubli and Zweimüller, 2013; Albanese and Cockx, 2015). Differently from the ATT on the survival in employment, these have to be interpreted as the *instantaneous* net costs (gains) during those years.

In the literature review we mentioned that Huber et al. (2015) found that in East-Germany participating women in the gradual retirement scheme were partially replaced by unemployed younger women. Whether this replacement occurs or not matters a lot for the CBA. Our data do not allow to test this hypothesis though. Nevertheless, we consider two scenarios in the CBA. One assumes that the part-time work is *not* replaced by another part-time worker, the other assumes that this part-time work *is* replaced by another who earns an equivalent wage and is equally productive as the part-time worker. In this second scenario we do not take into account, however, the gains in terms of UB payments that would no longer have to be paid if the replacing worker came from unemployment.

There are a number of reasons why we cannot perform a full-fledged CBA. First, as we will explain more in detail below, not all required information at the individual level is available in the administrative database. In these cases, we substitute the individual level information by aggregate information obtained from other sources or, if it refers to a very small share of individuals, we ignore the information by setting it to zero. Second, the analysis ignores some important dimensions. For instance, in Section 3.6.3 we found some limited evidence that participation in the TC may have some small positive health effects. However, as we lack information on health costs we cannot take this dimension into account. Moreover, we ignore the impact of participation in TC on the distribution of welfare or on poverty. All this means that the CBA should be taken as a crude approximation.

3.7.1. *Methodology*

We calculate, for each year t of the period of analysis above, the effect of the policy on two indicators, the Net Budgetary Cost for the Government (NBC) and the Net Welfare Cost for Society (NWC). Both indicators are expressed per treated individual and in monthly terms (2004 Euros).

i. **Net budgetary cost (gain) for the Government (NBC)**: This is the average cost (gain) of the policy for the state, net of savings for the public budget:

$$NBC_t = allow_t - tax_t * \ remu_t - SSC_t, \tag{7}$$

with:

- $allow_t$: expenditures of the Government on allowances of the Social Security scheme, such as unemployment benefits and the TC allowance. Because the database lacks information on statutory pension, sickness and disability allowances, we impute these allowances as follows:
  - We assume that the worker has worked his entire career in the private sector and assume that the individual is paid the average pension in the private sector according to the age bracket to whom he belongs: 50-54, 55-59, 60-64, 65-69.
  - For sickness and disability benefits we set the allowance equal to the theoretical level of entitlement, i.e. to 60% of the individuals' average monthly remuneration over the last six quarters.
- tax_t: The personal income tax on the gross wage. We approximate this tax rate by the average personal income tax rate of the combined central and sub-central governments (OECD, 2015a).
- $remu_t$: the gross wage of employed individuals. This is observed in the data for employees in the private and public sector, but not for the self-employed, for whom we impute a zero value for both treated and control units. Since the share of self-employed individuals in the control group is larger (4.6%/2.0% of men / women) than in the treated group (2.0%/0.7% of men / women), this slightly biases our cost estimate downwards.
- SSC_t: employer and employee contributions to Social Security

ii. **Net Welfare cost (gain) for Society (NWC):** the efficiency cost ("excess burden" or "deadweight loss") of the net budgetary expenditures mentioned in (i) plus the opportunity cost of working minus the production value of employment (PV):

$$\begin{aligned} NWC_t &= (MCF - 1) * NBC_t + LEIS_{\ t} - PV_t \\ &= (MCF - 1) * NBC_t + LEIS_t - (1 - PG_t) * LC_t \end{aligned} \tag{8}$$

with:

- MCF: the Marginal Cost of Public Funds.[31] For Belgium a MCF equal to two is considered to be appropriate (Kleven and Kreiner, 2006; Barrios Cobos et al., 2013).
- $LEIS_t$: the opportunity cost of working, which has to be between zero and the remuneration (including the SSC). Similarly to Greenberg and Robins (2008), we use the mid-point between the two bounds.
- PG: the age-related pay-productivity gap takes into account that the wage cost of older workers exceeds their productivity. An estimate of the production value of labour (PV) is obtained by downward adjusting the labour costs (LC) by this gap. We use estimates of this pay-productivity gap provided by Vandenberghe et al. (2013) for the Belgian case.

We consider two forms of heterogeneity in our effects. For the first form we consider two scenarios: one in which the effect of reduced working time on labour costs is taken into account and one in which it is not (i.e. full replacement of the reduced working hours for both treated and controls). For the second form, we check the robustness of our results by varying three key parameters of our model:

1. Two personal income tax rates: for persons earning 100% or 133% of the average wage.
2. Three marginal costs of public funds: 1.41, 2.14 and 3.23 (Kleven and Kreiner, 2006).
3. Three values for the opportunity cost of working. We lower bind the opportunity cost to zero and upper bind to the remuneration (including the SSC).

As baseline scenario we consider the personal income tax of a person earning 100% of the average wage, a marginal cost of public funds of 2.14 and the mid-point of the opportunity cost of working.

In order to measure the impact of TC on these indicators, we proceed in the following way. We run by gender a pooled weighted regression on all 8 years of analysis ($t \in \{1,2,\dots 8\}$) for $s \in \{0,1\}$, i.e. the two samples of analysis:

$$\sqrt{W_{it}^R} Y_{it} = \sum_{t=s+1}^{8} (\alpha_t + \beta_t 1(S_i = s)) \sqrt{W_{it}^R}\, D_t + \sqrt{W_{it}^R} u_{it}, \tag{9}$$

with

$$W_{it}^R \equiv W_{cr,i} \left[1 + \frac{p_s(X_i,l_i)}{1-p_s(X_i,l_i)} \frac{1}{\prod_{m=s+1}^{t}[1-p_m(X_i,l_i)]} 1(S_i > t)\right] \tag{10}$$

where Y_{it} measures the outcome of interest, i.e. NBC or PV[32], for each individual i in year t after treatment assignment, D_t is a year indicator equal to one in year t and zero otherwise, u_{it} is the error

[31] The net budgetary cost is *in se* not a cost to Society as it just involves transfers between individuals.
[32] Once we have estimated the average effect on production PV_t, we subtract it to the estimated impact on NBC_t (multiplied by $MCF - 1$) and $LEIS_t$to calculate WC_t, which is then divided by the number of treated to obtain the NWC_t.

term. In this regression α_t measures the average outcome for the control units that have not yet been treated in year t, i.e. $1(S_i > t)$, while β_t measures the average outcome for the treated units in year t, i.e $1(S_i = s)$ for t>s. The weights W_{it}^R ensure that (i) the endogenous sampling is taken into account by weighing all individual observations by $W_{cr,i}$, (ii) the control units that have not yet been treated are made, respectively kept comparable to the treated units by weighing them by the standard IPW $\frac{p_s(X_i,l_i)}{1-p_s(X_i,l_i)}$ and by $\frac{1}{\prod_{m=s+1}^{t}[1-p_m(X_i,l_i)]}$ to take the selective assignment into treatment over time into account. Note, in contrast to the analysis on the survival rate in employment, individuals who leave employment are not dropped from the analysis. Only individuals in the control group who become treated are dropped, but the weights avoid that this induces selectivity in the comparison between treated and control units. For each time period $t \in \{1,2, \ldots 8\}$ we estimate the average NBC or NWC for each treatment group $s \in \{0,1\}$ by $\hat{\beta}_{t-s} - \hat{\alpha}_{t-s}$, where the hat denotes the estimate of the weighted regression in (10). Subsequently, we average over treatment groups in a similar way as in (6).

3.7.2. *Results*

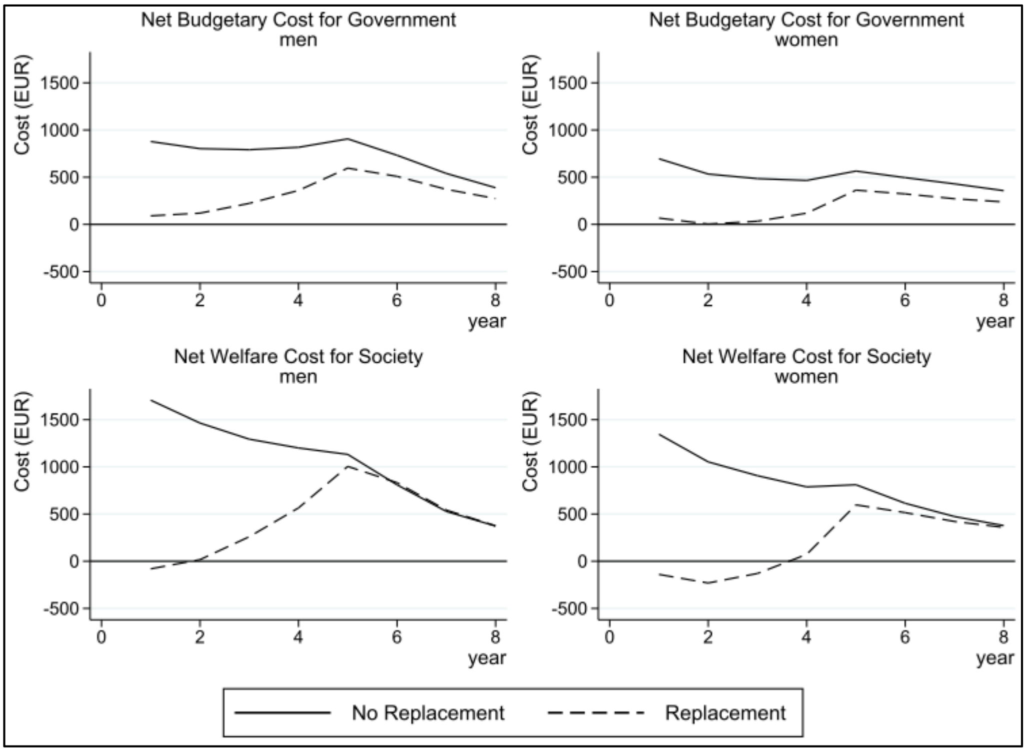
The results of the cost-benefit analysis (CBA) can be found in Figure 3.8, which shows the monthly cost per treated for the government budget and the welfare cost to society. These estimates regard the baseline scenario explained above with and without replacement of part-time workers.

Without the replacement of part-time workers, the results indicate that the TC scheme is an expensive policy that fails the cost-benefit test. Although in the first years we have estimated a positive employment effect, the costs of the policy immediately dominate the benefits. In the first year of the analysis the monthly costs to the government budget in the baseline case are €878 (€696) per treated person for men (women). That these budgetary costs for the government already exist from the first years indicate that the positive effects on employment at the extensive margin are immediately dominated by the reduction of working hours and the allowance paid to the TC participants. In particular, (i) treated people reduce their working hours, resulting in less income for the government in the form of taxes; (ii) TC participants receive the TC benefit, resulting in higher expenditures for the government. As a reduction in working hours also implies lower production, the monthly total welfare costs per treated individual are even higher: €1,706 (€1,345) for men (women). The costs to society show a decreasing pattern, while near the end of the analysis we observe a stronger decrease for both the costs to the government and the society. This convergence can be attributed to the many treated and control individuals leaving employment, removing the differences between the two groups.

Assuming full replacement of the reduced working hours shows a small potential for beneficial short-run effects, but only for women and if the full welfare costs are taken into account. The reason for this is that in this scenario no working hours are lost when a participant enters the TC scheme. To the extent that there are positive employment effects as discussed in Section 3.6.1, this can indeed lead to positive budgetary effects. The largest potential benefits are obtained at the start of the analysis and are larger and longer-lasting for women, as they have more favourable short run employment effects.[33] The welfare cost to Society is about zero in year two (four) for men (women), when we estimated a positive ATT on survival in employment of about 3.5 pp. This suggests that a positive ATT of about 3.5 percentage points is needed to equalise costs to benefits. However, this is measured for the most optimistic scenario. In reality it is unlikely that employers could replace all reduced working time.

Finally, in Appendix 3.9 we included figures for the heterogeneous effects by age (Figure 3.11) and two summary tables (Table 3.4 and Table 3.5) containing the effects for our sensitivity analyses, with all combinations of our three key variables. Though the magnitude of the estimates in the sensitivity analysis changes, the qualitative results are in line with the findings of the baseline scenario.

Figure 3.8: Monthly Cost of the TC per Treated of 2003 (2004)



Cost-benefit analysis (CBA) on the pooled sample of participants in TC of 2003 and 2004. CBA in monthly costs (benefits if negative) in 2004 euros per treated individual (the size of the treated sample as defined in 2003/2004). The Net Budgetary Cost (NBC) for the government is the average cost (gain) of the policy for the state, net of savings for the public budget. The Net Welfare Cost (NWC) for society is the efficiency cost of the NBC minus the production value of employment (PV). Baseline scenario: CBA without replacement of the part-time workers. Replacement scenario: baseline scenario with the additional assumption that all hours reduced by part-time workers (treated and controls) are recovered by hiring extra workers with similar characteristics. The CBA ignores potential substitution and anticipation effects. The costs to society ignore the value of leisure and potential distributional and health impacts of the measure. The CBA spans all eight years of the ATT analysis (Section 3.6.1). Year 1 is the first year for which we calculate the ATT. Year 8 only contains information from the 2003 sample.

[33] There are no benefits for the government budget, but the monthly welfare benefits for society are larger in year one (two) for men (women), when they amount to €80 (231) per treated man (woman). Similar to the ATT on survival in employment, the period with a welfare gain lasts only one year for men, while three years for women.

Over time, we observe that the two scenarios converge. This is driven by the fact that people start to leave the labour market, leaving less people to reduce their working hours, which in turn results in a gradually less important role for the replacement channel.[34] Our CBA ignores distributional effects and effects on other channels such as health. However, it is clear that without full replacement, the policy fails the cost-benefit test.

In line with our employment analysis we considered the heterogeneous effects of splitting the sample by age, using the same cut-off as in the employment analysis. This analysis shows that while the policy is more costly for younger workers when we do not consider replacement, it also has the greatest scope for positive budgetary effects once the replacement is taken into account (see Figure 3.11 in Appendix 3.9). The potential benefits are greater and longer-lasting for (especially female) younger workers, as they also have longer lasting positive employment effects (cf. Section 3.6.1-3.6.2).

3.8. Conclusion

This paper studies the effect on employment of a scheme that facilitates gradual retirement through working time reductions. Recently many EU countries have implemented such measures with the aim to postpone the retirement of older workers. However, evidence on the effectiveness of such policies is scarce and provides mixed results. Our research provides new evidence on this question by evaluating the impact of the Time Credit (TC) scheme in Belgium allowing workers above the age of 50 to reduce their working time until the statutory retirement age.

We contributed methodologically to this literature by explicitly taking the dynamic assignment into treatment into account (Johansson and Fredriksson 2008), and by explicitly considering a competing risks model that allows measuring the treatment effect according to different (possibly selective) pathways to early retirement. To model this dynamic selection and possibly selective right censoring within this competing risks model, we slightly adjusted the methodology recently proposed by Vikström (2014) to take the stock sampling in employment at the start of the treatment into account. We find that the correction for selective right censoring matters to avoid bias, especially in the competing risks framework.

Overall our evidence is in line with the findings of Graf et al. (2011). Participation in TC initially prolongs the time spent in employment (during the first two years for men and four years for women), but subsequently it accelerates the exit to early retirement. In the beginning the effect is positive, presumably because participants are not yet eligible to early retirement. However, as soon as they are,

[34] After year five, the differences between the two scenarios remain relatively larger for women than for men, because treated women have higher survival rates than treated men and thus a more important replacement channel.

participants have much higher incentives to enter it than non-participants, because the replacement rates in these schemes (with regard to their labour income) are much higher for TC participants than for non-participants. These financial incentives are on the one hand much stronger for individuals who reduce their working time to 50% of a full-time job than for those who reduce it to 80%, and on the other hand also for workers who are eligible for the statutory early retirement scheme relative to those who are not. Our evidence supports that individuals behave according to these incentives. Besides, the impact of TC is more favourable for women than for men, because women have less opportunities to enter early retirement.

The gradual retirement scheme clearly fails the cost-benefit test. Only under the extreme assumption that employers fully compensate for all working time reductions by new equally productive workers the TC scheme displays a net benefit for society during the first two (four) years for men (women). Nevertheless, our findings suggest that the scheme could become socially cost-effective if the opportunities for early retirement were blocked or made financially less attractive. In the analysis in which we allowed for three different exit destinations from employment, we found indeed that for the exit destinations that were financially less attractive the effect of the TC on the survival rate in employment no longer became negative or even remained significantly positive in some cases.

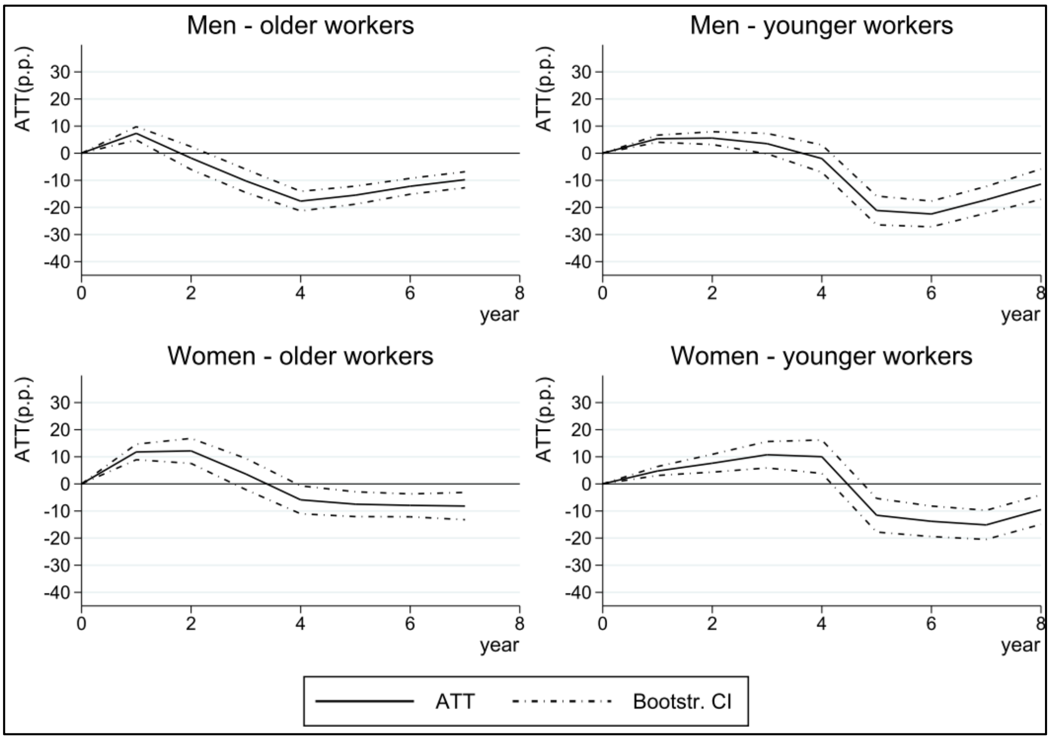
Given this responsiveness to the financial incentives, we believe it may be possible to prolong the positive effect of TC schemes by eliminating the perverse incentives to exit earlier from the labour force. A possible policy proposal to enhance the scheme might be to leave the entitlement to full-time pension only to the participants exiting at the statutory retirement age and accordingly adjust the entitlements of the early leavers. Since 2015 the Belgian government has raised the eligibility age to bridge pensions from 58 to 62 years (with some exceptions) and the minimum age to be eligible to the conventional early retirement is gradually increased since 2012 from 60 years to attain 63 years in 2019. Based on our findings we speculate that this could increase the effectiveness of the Time-Credit scheme. However, since 2015 the Belgian government has also raised the minimum age of eligibility of the end of career TC to 60 years. As a consequence, the starting age of gradual retirement has been set so close to the minimum early retirement age that it can hardly have any significant positive effect on the career length of employees. While the decision to raise the early retirement age can be supported on the basis of our findings, the decision to simultaneously increase the minimum age of eligibility to the end of career TC scheme cannot. We call for further research to obtain better foundations for these policy recommendations.

3.9. Appendix

Table 3.3: Comparison of Gradual/Part-Time Retirement Schemes in Other European Countries

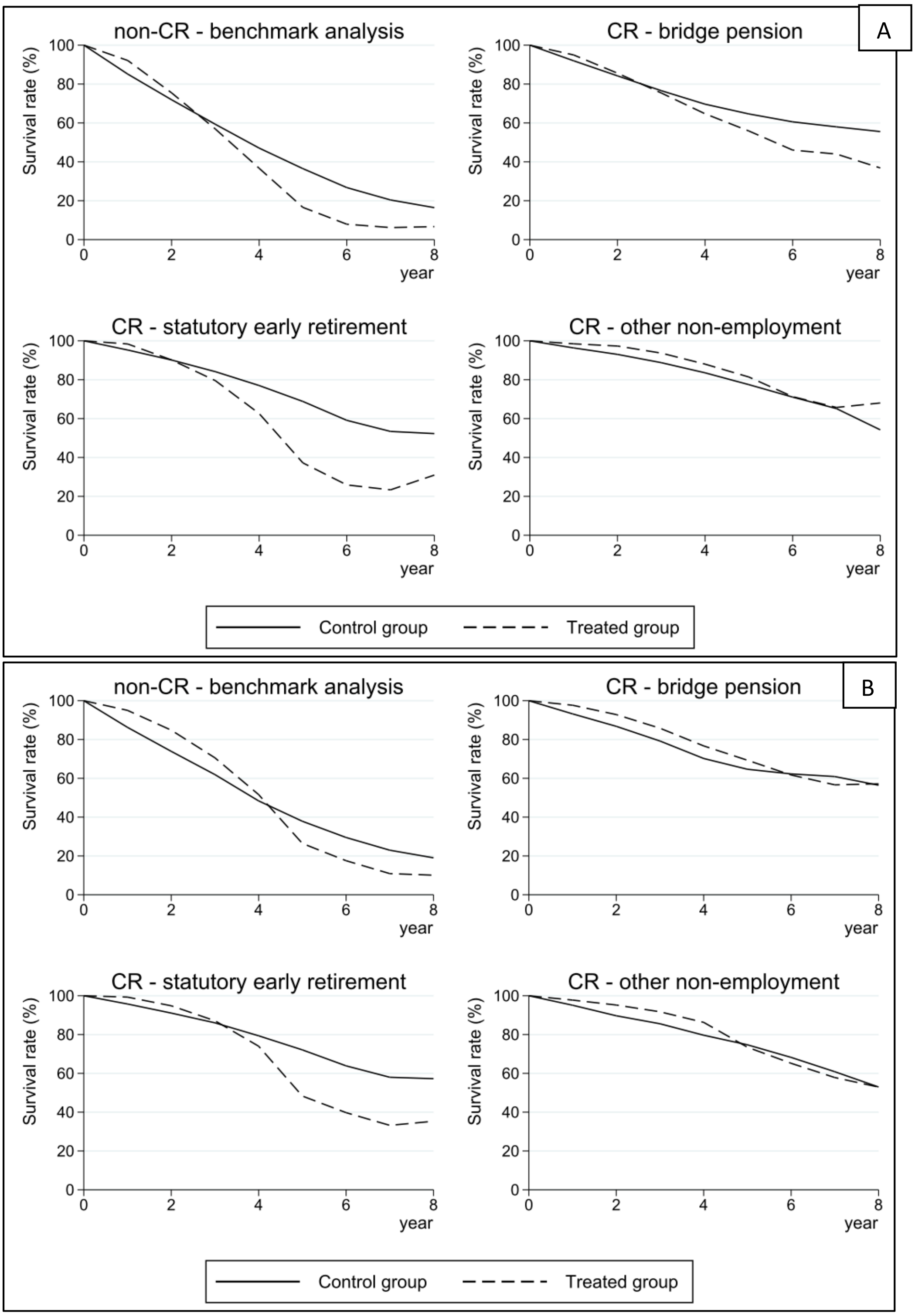
Country	Policy	Years in place	Age-Eligibility	Replacement Rate	Reduction in Hours
Sweden	Part Time Pension	1976-2000	61y	55% from 1994 onwards	10h/week (i.e. max 25% workweek)
	Part Time Pension	2003 - …	61y	60% of reduction in wage	as much as 50% until 65y
Finland	Part-Time Pension	Since 1987 in the private sector, 1989 public sector	56 (<2005), then 58 and 60 (>=2011). Until 64	50% difference regular and part-time earnings	16-28h/week
Denmark	Part Time pensions	1987	60-64	fixed rate/reduced hour	having a workweek of 12-30h/week
France	Phased Early Retirement (PRP)	1988-2005	55-65	top up of 30%	40-50%
Germany	Part Time Retirement	1996-2009	55+	70% top up	50% (blocking possible)
Austria	Old Age Part-Time scheme (OAPT)	2000…	>=55 (m), >50(w) + career restrictions	75%	40-60%, max 6 1/2 y (blocking possible)
Netherlands	Life Course Regulation	2006-2011	whole career, but can also be used as part time retirement two years before retirement)	own savings, 70% now	50%

Figure 3.9: ATT on Survival in Employment – Heterogeneous Effects by Age



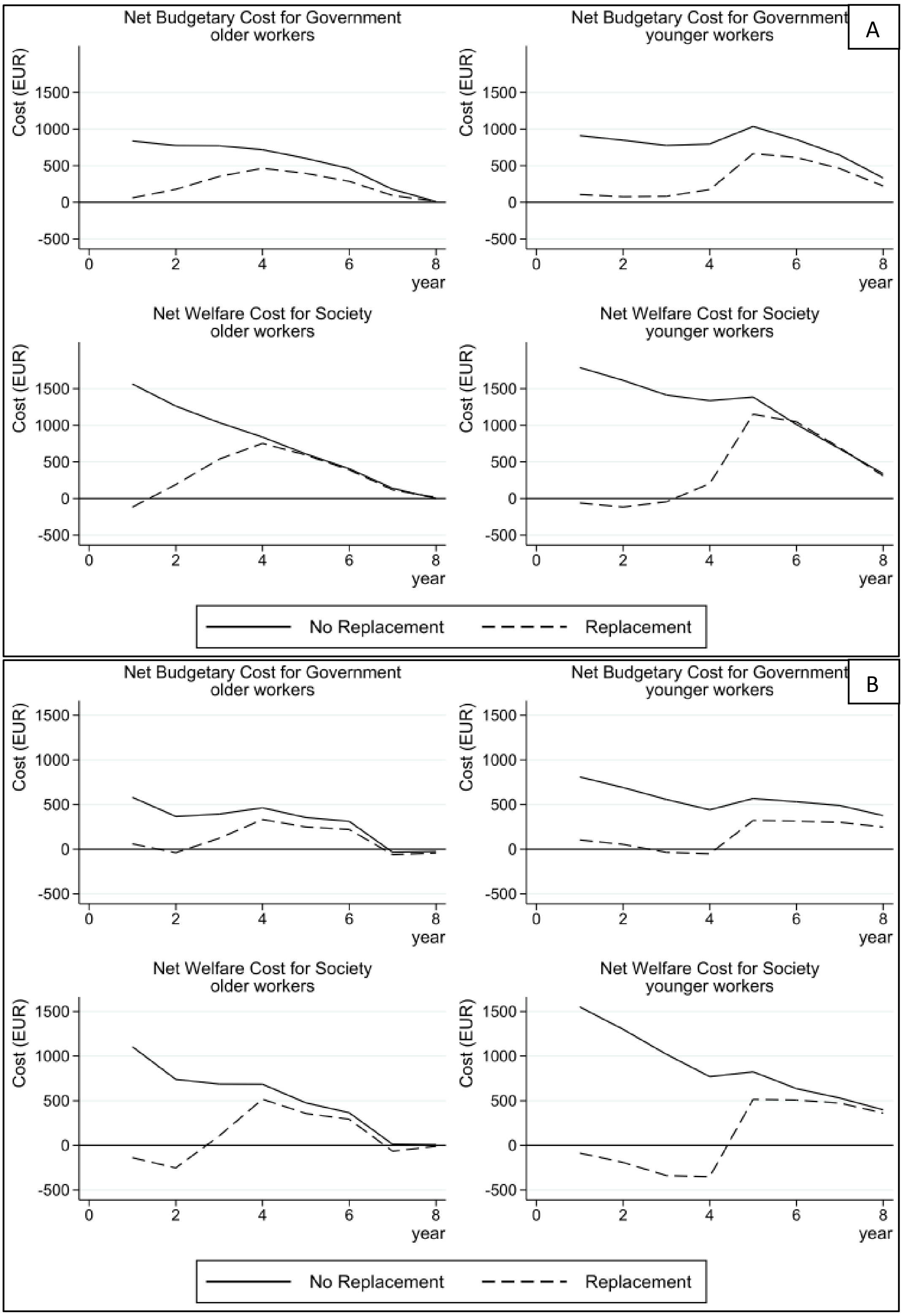
ATT on the survival rate in employment estimated by controlling for the dynamic selection on observables (Vikström, 2014). Heterogeneous effects by age in year 0: younger (below the age of 56.5) and older workers (at least 56.5 years old). The estimates of the ATT's are the percentage points (pp) differences between the survival rate of the treated in case of treatment and the estimated survival rate of the treated in the counterfactual of no treatment. Estimates are pooled over the 2003 and 2004 samples. Year eight only uses information from the 2003 sample. Standard errors are obtained by a stratified bootstrap (clustering by individual) with 500 repetitions and 95% confidence intervals (CI) by assuming normality.

Figure 3.10: Survival in Employment (non-CR) and Competing Risks (CR)– Men (A) & Women (B)



Survival function of the treated and control units controlling for the dynamic selection on observables (Vikström, 2014) by gender (Panel A for men and B for women). The survival rates are expressed in percentage points (pp) and defined as (from left to right and top to bottom) (1) employment, (2) employment without exit to a bridge pension, (3) employment without exit to a statutory pension before the normal retirement age (65 for men and women born after 30/11/1944, 64 for women born between 01/12/1942 and 30/11/1944 and 63 for women born before 30/11//1942) and (4) employment without exit to other non-employment statuses. In the competing risk analyses (2-4), the exits from employment to other destinations, apart from the one considered, are right censored. Reported estimates are pooled over the 2003 and 2004 samples. Year eight only uses information from the 2003 sample. The pooled sample is composed of 1,227 (762) treated and 29,791 (9,658) control units (men and women).

Figure 3.11: Monthly Cost of Policy per Treated for Men (panel A) and Women (panel B)



Cost-benefit analysis (CBA) on the pooled sample of participants in TC of 2003 and 2004 (panel A for men, panel B for women). CBA in monthly costs (benefits if negative) in 2004 euros per treated individual (the size of the treated sample as defined in 2003 / 2004). The Net Budgetary Cost (NBC) for the government is the average cost (gain) of the policy for the state, net of savings for the public budget. The Net Welfare Cost (NWC) for society is the efficiency cost of the NBC minus the production value of employment (PV). Baseline scenario: CBA without replacement of the part-time workers. Replacement scenario: baseline scenario with additional assumption that all hours reduced by part-time workers (treated and controls) are recovered by hiring extra workers with similar characteristics. Younger workers are aged strictly below age 56.5 at the moment of sample selection (year 0), older workers are aged 56.5 and above at that moment. The CBA ignores potential substitution and anticipation effects. The costs to society ignore the value of leisure and potential distributional and health impacts of the measure. The CBA spans all eight years of the ATT analysis from Section 3.6.1. Year one is the first year for which we calculate the ATT. Year eight only contains information from the 2003 sample.

Table 3.4: Sensitivity analysis on Cost-Benefit Analysis - men

(A) MCF	(B) Reservation Wage	(C) Income taxes	(1) No replacement of part-time workers								(2) Replacement of part-time workers							
			Year 1	Year 2	Year 3	Year 4	Year 5	Year 6	Year 7	Year 8	Year 1	Year 2	Year 3	Year 4	Year 5	Year 6	Year 7	Year 8
(α) NET BUDGETARY COST (NBC) FOR THE GOVERNMENT																		
-	**-**	**Medium**	**878.3**	**803.0**	**791.2**	**817.0**	**906.1**	**732.6**	**540.0**	**388.6**	**90.7**	**119.2**	**222.7**	**359.6**	**596.2**	**508.9**	**371.5**	**273.8**
-	-	High	914.1	835.7	823.1	850.5	941.4	760.2	559.5	402.6	78.6	111.5	224.3	373.8	627.5	537.0	391.3	288.0
(β) NET WELFARE COST (NWC) FOR SOCIETY																		
1.41	Low	Medium	1,505.2	1,306.4	1,185.0	1,142.1	1,123.5	817.1	530.7	371.0	-306.8	-170.3	126.0	494.0	992.0	839.2	544.8	373.1
1.41	Low	High	1,519.8	1,319.8	1,198.1	1,155.9	1,138.0	828.4	538.7	376.7	-311.8	-173.5	126.7	499.8	1,004.8	850.7	552.9	378.9
1.41	Medium	Medium	1,022.1	864.3	755.4	698.0	653.1	450.9	271.5	182.8	-143.7	-67.9	100.0	305.7	575.4	464.8	279.5	182.1
1.41	Medium	High	1,054.7	894.1	784.4	728.4	685.3	475.9	289.2	195.5	-154.7	-74.9	101.5	318.7	603.9	490.4	297.6	195.0
1.41	High	Medium	539.1	422.1	325.8	253.8	182.6	84.6	12.4	-5.5	19.3	34.6	74.0	117.4	158.9	90.5	14.2	-8.9
1.41	High	High	589.6	468.3	370.7	300.9	232.5	123.4	39.7	14.3	2.3	23.6	76.3	137.5	203.0	130.1	42.2	11.0
2.14	Low	Medium	2,189.4	1,906.3	1,723.1	1,644.4	1,602.2	1,176.1	786.7	560.6	-243.5	-85.9	285.4	753.1	1,421.1	1,206.5	807.4	566.6
2.14	Low	High	2,230.2	1,943.6	1,759.5	1,682.5	1,642.6	1,207.6	808.8	576.5	-257.3	-94.7	287.3	769.4	1,456.8	1,238.6	830.0	582.8
2.14	**Medium**	**Medium**	**1,706.4**	**1,464.1**	**1,293.5**	**1,200.2**	**1,131.8**	**809.9**	**527.5**	**372.3**	**-80.4**	**16.5**	**259.4**	**564.9**	**1,004.6**	**832.2**	**542.1**	**375.6**
2.14	Medium	High	1,765.1	1,517.8	1,345.7	1,255.1	1,189.8	855.1	559.3	395.3	-100.3	3.8	262.1	588.3	1,055.9	878.3	574.6	398.8
2.14	High	Medium	1,223.3	1,022.0	863.9	756.0	661.4	443.6	268.3	184.1	82.6	119.0	233.4	376.6	588.0	457.9	276.8	184.7
2.14	High	High	1,299.9	1,092.1	932.0	827.6	737.1	502.6	309.9	214.1	56.8	102.4	236.9	407.1	654.9	518.1	319.3	214.9
3.23	Low	Medium	3,211.1	2,801.9	2,526.6	2,394.4	2,317.1	1,712.2	1,168.9	843.7	-149.0	40.2	523.5	1,140.1	2,061.9	1,755.1	1,199.4	855.7
3.23	Low	High	3,290.9	2,874.9	2,597.6	2,469.0	2,396.0	1,773.6	1,212.2	874.9	-176.0	22.9	527.0	1,171.9	2,131.6	1,817.8	1,243.7	887.2
3.23	Medium	Medium	2,728.0	2,359.8	2,097.0	1,950.2	1,846.6	1,345.9	909.7	655.4	14.1	142.6	497.5	951.9	1,645.3	1,380.8	934.1	664.7
3.23	Medium	High	2,825.8	2,449.2	2,183.9	2,041.5	1,943.2	1,421.1	962.7	693.6	-18.9	121.4	501.8	990.8	1,730.7	1,457.5	988.4	703.3
3.23	High	Medium	2,245.0	1,917.6	1,667.3	1,506.0	1,376.2	979.7	650.5	467.2	177.2	245.0	471.5	763.6	1,228.8	1,006.4	668.8	473.7
3.23	High	High	2,360.6	2,023.4	1,770.2	1,614.1	1,490.5	1,068.7	713.2	512.4	138.1	220.0	476.7	809.7	1,329.8	1,097.3	733.0	519.4

Cost-benefit analysis (CBA) on the pooled sample of male participants in TC of 2003 and 2004. Treated sample size defined in 2003-2004. CBA in monthly costs (benefits if negative) in 2004 euros per treated individual under different scenarios. Scenario in ***bold*** *denotes baseline scenario. (1) No replacement of part-time workers scenario; and (2) replacement scenario: baseline scenario with additional assumption that all hours reduced by part-time workers (treated and controls) are recovered by hiring extra workers with similar characteristics. Additionally (A) Marginal Cost of public Funds (MCF) equal to 1.41, 2.14, or 3.23 (Kleven and Kreiner, 2006), (B) opportunity cost of working (Reservation Wage) with a lower, medium and upper bound (see Section 3.7.1) and (C) Income Tax Rate, variable over time: medium (average income) which is on average 28.26%, higher (133% average income) which is on average 32.36% (OECD stat extract, 2003-2011). The first outcome variable is the Net Budgetary Cost (NBC) for the government (α), i.e. the average cost (gain) of the policy for the state, net of savings for the public budget. The second is the Net Welfare Cost (NWC) for society (β), i.e. the efficiency cost of the NBC minus the production value of employment. The CBA ignores potential substitution and anticipation effects. The costs to society ignore potential distributional and health impacts of the measure. The CBA spans all eight years of the ATT analysis from Section 3.6.1. Year one is the first year for which we calculate the ATT. Year eight only contains information from the 2003 sample*

Table 3.5: Sensitivity analysis on Cost-Benefit Analysis - women

(A) MCF	(B) Reservation Wage	(C) Income taxes	(1) No replacement of part-time workers								(2) Replacement of part-time workers							
			Year 1	Year 2	Year 3	Year 4	Year 5	Year 6	Year 7	Year 8	Year 1	Year 2	Year 3	Year 4	Year 5	Year 6	Year 7	Year 8
(α) NET BUDGETARY COST (NBC) FOR THE GOVERNMENT																		
-	**-**	**Medium**	**695.5**	**533.5**	**485.0**	**466.0**	**565.1**	**495.5**	**429.6**	**357.6**	**67.0**	**3.8**	**33.5**	**119.3**	**362.8**	**322.8**	**271.7**	**237.8**
-	-	High	721.9	554.2	504.2	484.8	589.2	515.5	446.2	371.4	53.9	-11.5	22.3	115.9	379.7	339.2	286.0	250.7
(β) NET WELFARE COST (NWC) FOR SOCIETY																		
1.41	Low	Medium	1,139.7	872.2	761.1	684.7	777.2	598.3	467.6	374.5	-366.8	-442.7	-302.2	-50.5	558.5	496.6	414.6	355.8
1.41	Low	High	1,150.6	880.8	769.0	692.4	787.1	606.5	474.4	380.2	-372.2	-449.0	-306.8	-51.9	565.4	503.3	420.5	361.1
1.41	Medium	Medium	790.1	601.2	511.0	439.7	461.9	335.8	248.9	192.9	-185.7	-228.9	-145.7	-2.8	336.2	281.3	224.9	186.6
1.41	Medium	High	814.2	620.1	528.5	456.8	483.8	354.1	264.0	205.5	-197.6	-242.8	-155.8	-5.9	351.5	296.3	238.0	198.3
1.41	High	Medium	440.5	330.1	261.0	194.7	146.6	73.3	30.1	11.3	-4.6	-15.0	10.9	44.9	113.9	66.1	35.1	17.3
1.41	High	High	477.8	359.4	288.0	221.2	180.5	101.6	53.5	30.8	-23.1	-36.6	-4.9	40.1	137.7	89.2	55.4	35.5
2.14	Low	Medium	1,694.9	1,324.0	1,156.3	1,032.4	1,125.8	876.3	692.4	559.0	-320.9	-445.0	-286.3	25.1	819.6	730.0	611.2	527.5
2.14	Low	High	1,725.0	1,347.7	1,178.2	1,053.8	1,153.2	899.2	711.3	574.7	-335.8	-462.5	-299.1	21.3	838.9	748.7	627.6	542.2
2.14	**Medium**	**Medium**	**1,345.3**	**1,052.9**	**906.2**	**787.5**	**810.5**	**613.8**	**473.6**	**377.4**	**-139.8**	**-231.1**	**-129.8**	**72.9**	**597.3**	**514.7**	**421.4**	**358.3**
2.14	Medium	High	1,388.6	1,087.0	937.7	818.2	849.9	646.7	500.8	400.0	-161.3	-256.3	-148.2	67.3	625.0	541.6	445.0	379.4
2.14	High	Medium	995.7	781.8	656.2	542.5	495.1	351.4	254.8	195.8	41.3	-17.3	26.7	120.6	375.0	299.5	231.6	189.1
2.14	High	High	1,052.3	826.4	697.3	582.7	546.6	394.3	290.4	225.3	13.3	-50.1	2.8	113.3	411.2	334.6	262.4	216.7
3.23	Low	Medium	2,523.8	1,998.5	1,746.4	1,551.7	1,646.3	1,291.5	1,027.9	834.5	-252.3	-448.4	-262.7	138.1	1,209.5	1,078.5	904.6	784.0
3.23	Low	High	2,582.7	2,044.9	1,789.2	1,593.6	1,699.9	1,336.2	1,065.0	865.2	-281.6	-482.6	-287.6	130.6	1,247.2	1,115.0	936.7	812.7
3.23	Medium	Medium	2,174.2	1,727.4	1,496.3	1,306.8	1,330.9	1,029.0	809.2	652.9	-71.2	-234.5	-106.1	185.8	987.2	863.2	714.8	614.8
3.23	Medium	High	2,246.3	1,784.2	1,548.8	1,358.0	1,396.6	1,083.7	854.5	690.5	-107.0	-276.4	-136.7	176.6	1,033.3	908.0	754.1	649.9
3.23	High	Medium	1,824.6	1,456.4	1,246.3	1,061.8	1,015.6	766.5	590.4	471.3	109.9	-20.7	50.4	233.5	765.0	648.0	525.1	445.6
3.23	High	High	1,910.0	1,523.5	1,308.3	1,122.4	1,093.3	831.3	644.1	515.8	67.5	-70.2	14.3	222.7	819.5	700.9	571.5	487.2

Cost-benefit analysis (CBA) on the pooled sample of female participants in TC of 2003 and 2004. Treated sample size defined in 2003-2004. CBA in monthly costs (benefits if negative) in 2004 euros per treated individual under different scenarios. Scenario in ***bold*** *denotes baseline scenario. (1) No replacement of part-time workers scenario; and (2) replacement scenario: baseline scenario with additional assumption that all hours reduced by part-time workers (treated and controls) are recovered by hiring extra workers with similar characteristics. Additionally (A) Marginal Cost of public Funds (MCF) equal to 1.41, 2.14, or 3.23 (Kleven and Kreiner, 2006), (B) opportunity cost of working (Reservation Wage) with a lower, medium and upper bound (see Section 3.7.1) and (C) Income Tax Rate, variable over time: medium (income tax on the average income) which is on average 28.26%, higher (income tax on 133% of the average income) which is on average 32.36% (OECD stat extract, 2003-2011). The first outcome variable is the Net Budgetary Cost (NBC) for the government (α), i.e. the average cost (gain) of the policy for the state, net of savings for the public budget. The second is the Net Welfare Cost (NWC) for society (β), i.e. the efficiency cost of the NBC minus the production value of employment. The CBA ignores potential substitution and anticipation effects. The costs to society ignore potential distributional and health impacts of the measure. The CBA spans all eight years of the ATT analysis from Section 3.6.1. Year one is the first year for which we calculate the ATT. Year eight only contains information from the 2003 sample.*

Table 3.6: Balancing tests: Standardized Bias (SB), pvalue on mean equality and others

		Men								Women							
		2003				2004				2003				2004			
		Treated: mean	Control: mean	SB (1)	Pvalue (2)	Treated: mean	Control: mean	SB (1)	Pvalue (2)	Treated: mean	Control: mean	SB (1)	Pvalue (2)	Treated: mean	Control: mean	SB (1)	Pvalue (2)
Status one year before selection (2002 or 2003)	Firm size: 20-99	21.2%	20.9%	0.6	0.891	15.7%	16.0%	-0.8	0.852	26.7%	26.9%	-0.4	0.934	26.2%	27.1%	-1.9	0.744
	Firm size: 100-999	33.3%	33.0%	0.6	0.893	28.0%	28.9%	-1.9	0.711	31.5%	31.3%	0.5	0.924	23.5%	24.0%	-1.0	0.864
	Firm size: > 1000	45.5%	46.0%	-1.1	0.821	56.3%	55.1%	2.6	0.655	41.7%	41.8%	-0.1	0.986	50.2%	48.9%	2.7	0.679
	Household: Other	11.3%	11.2%	0.3	0.950	11.4%	11.4%	0.1	0.979	36.3%	36.2%	0.2	0.977	35.2%	35.1%	0.3	0.955
	Couple & children	47.7%	46.8%	1.8	0.707	45.7%	45.2%	1.1	0.844	21.8%	21.7%	0.3	0.954	27.1%	27.4%	-0.7	0.918
	Couple no children	41.0%	42.0%	-2.0	0.673	42.8%	43.4%	-1.3	0.829	41.8%	42.0%	-0.4	0.940	37.6%	37.5%	0.3	0.967
	Age	55.3	55.3	-0.4	0.924	55.7	55.7	0.1	0.978	55.1	55.1	0.6	0.902	55.8	55.8	-1.5	0.776
	Blue collar	26.5%	26.3%	0.4	0.926	19.7%	20.7%	-2.4	0.608	11.2%	11.1%	0.1	0.984	9.4%	9.8%	-1.3	0.811
	Full-Time Hourly wage	€ 20.4	€ 20.3	1.3	0.722	€ 21.1	€ 20.9	2.3	0.566	€ 17.3	€ 17.3	0.4	0.941	€ 17.6	€ 17.5	2.0	0.687
	Belgian	97.9%	97.9%	-0.1	0.972	98.0%	97.9%	0.5	0.905	97.7%	97.7%	0.1	0.991	96.5%	96.2%	2.0	0.749
	Household size	2.6	2.6	1.5	0.743	2.6	2.6	0.9	0.870	2.1	2.1	0.2	0.967	2.1	2.1	0.3	0.964
	Region: Bruxelles	4.8%	4.7%	0.3	0.947	7.2%	7.0%	0.7	0.901	15.2%	15.1%	0.2	0.964	15.7%	15.5%	0.6	0.923
	Region: Flanders	72.4%	73.0%	-1.3	0.780	65.8%	66.3%	-1.2	0.831	51.2%	51.2%	0.1	0.989	52.3%	52.8%	-1.1	0.857
	Region: Wallonia	22.8%	22.2%	1.2	0.791	27.1%	26.7%	0.9	0.875	33.5%	33.7%	-0.3	0.963	32.0%	31.7%	0.8	0.905
	Trade, transport, hotel	15.7%	15.5%	0.5	0.901	14.3%	14.9%	-1.4	0.766	23.5%	23.5%	-0.1	0.980	19.5%	20.0%	-1.1	0.854
	Bank, business services	39.1%	40.0%	-2.1	0.697	51.5%	50.2%	2.9	0.654	34.4%	34.3%	0.2	0.970	45.7%	44.3%	2.9	0.671
	Other services	4.1%	4.0%	0.4	0.921	4.1%	4.2%	-0.5	0.907	22.6%	22.7%	-0.2	0.970	20.4%	20.9%	-1.3	0.821
	Manuf., Agric., Constr.	36.9%	36.3%	1.2	0.798	24.8%	25.3%	-1.1	0.830	19.6%	19.5%	0.1	0.985	14.4%	14.8%	-0.9	0.863
	Construction	4.2%	4.2%	0.4	0.927	5.4%	5.5%	-0.5	0.916								
	Prop. to early retire*	-0.1%	-0.2%	0.8	0.852	-2.2%	-2.0%	-2.5	0.593	-3.2%	-3.2%	-0.2	0.973	-4.1%	-4.0%	-2.2	0.702
5 years before sel.	Full-Time Hourly wage	€ 19.8	€ 19.7	1.3	0.709	€ 20.9	€ 20.7	2.4	0.561	€ 16.9	€ 16.9	0.4	0.940	€ 17.2	€ 17.1	2.2	0.661
13 years before sel.	Years with the same employers	10.6	10.5	3.8	0.475	10.9	10.9	-0.3	0.954	11.0	11.0	-0.2	0.975	11.2	11.2	-1.5	0.820
'90-'97	Av. Working time %	97.9	97.9	0.1	0.983	98.1	98.0	1.5	0.714	96.0	95.8	1.7	0.774	95.7	95.6	1.1	0.863
'57-'90	Experience in years	31.4	31.5	-0.8	0.857	30.8	30.8	-0.4	0.937	30.5	30.5	0.3	0.944	29.6	29.6	0.7	0.897
'57-'90	Earnings in the year	€ 28,599	€ 28,453	1.3	0.721	€ 29,742	€ 29,459	2.6	0.548	€ 23,514	€ 23,460	0.7	0.903	€ 24,199	€ 23,990	2.6	0.648
N individuals	Sample	754	17,876			473	11,915			421	5,353			341	4,305		
	Population	2,012	41,330			1,851	34,448			669	7,829			592	6,780		

	2003					2004				
Sample	Pseudo-R2	Wald test: pvalue	Mean Standardized Bias	Median Standardized Bias	Max Standardized Bias	Pseudo-R2	Wald test: pvalue	Mean Standardized Bias	Median Standardized Bias	Max Standardized Bias
Raw Men	0.068	0.000	15.6%	11.5%	48.0%	0.127	0.000	20.9%	15.1%	74.2%
Reweighted Men	0.000	1.000	1.0%	0.8%	3.8%	0.000	1.000	1.3%	1.1%	2.9%
Raw Women	0.026	0.000	9.5%	8.2%	22.5%	0.083	0.000	15.9%	11.8%	50.0%
Reweighted Women	0.000	1.000	0.3%	0.2%	1.7%	0.000	1.000	1.4%	1.2%	2.9%

Balancing tests after reweighting the individuals by using the weights of the year of selection (2003 or 2004). The upper panel reports the mean of the covariates by treatment status with (1) Standardized Bias and (2) pvalue of the t-test. The lower panel shows the results of other balancing tests before and after reweighting the individuals by such weights.

Bibliography

Ahn, T., 2015. Reduction of Working Time: Does It Lead to a Healthy Lifestyle? Forthcoming in Health Economics.

Albanese, A., Cockx, B., 2015. Permanent Wage Cost Subsidies for Older Workers: An Effective Tool for Increasing Working Time and Postponing Early Retirement? . IZA Discussion Paper No. 8988, IZA, Bonn.

Altonji, J., Elder, T., Taber, C.R., 2005. Selection on Observed and Unobserved Variables: Assessing the Effectiveness of Catholic Schools. Journal of Political Economy 113(1), 151–184.

Aubert, P., Crépon, B., 2003. La productivité des salariés âgés: une tentative d'estimation. Économie et statistique 368(1), 95–119.

Aubert, P., Crépon, B., 2006. Are older workers less productive? Firm-level evidence on age-productivity and age-wage profiles. Mimeo, INSEE, Paris.

Barrios Cobos, S., Saveyn, B., Pycroft, J., 2013. The Marginal Cost of Public Funds in the Eu: The Case of Labour Versus Green Taxes. Taxation papers 35, Publications Office of the European Union.

Berg, P.B., Hamman, M.K., Piszczek, M., Ruhm, C., 2015. Can Policy Facilitate Partial Retirement? Evidence from Germany . IZA Discussion Paper No. 9266, IZA, Bonn.

Busso, M., DiNardo, J., McCrary, J., 2014. New Evidence on the Finite Sample Properties of Propensity Score Reweighting and Matching Estimators. The Review of Economics and Statistics 96(5), 885–897.

Cameron, A.C., Trivedi, P.K., 2005. Microeconometrics: methods and applications. Cambridge University Press.

Cardoso, A.R., Guimarães, P., Varejão, J., 2011. Are Older Workers Worthy of Their Pay? An Empirical Investigation of Age-Productivity and Age-Wage Nexuses. De Economist 159(2), 95–111.

Cataldi, A., Kampelmann, S., Rycx, F., 2012. Does it pay to be productive? The case of age groups. International Journal of Manpower 33, 264–283.

Charles, K.., Decicca, P., 2007. Hours Flexibility and Retirement. Economic Inquiry 45(2), 251–267.

Crépon, B., Ferracci, M., Jolivet, G., van den Berg, G., 2009. Active Labor Market Policy Effects in a Dynamic Setting. Journal of the European Economic Association 7(2-3), 595–605.

Devisscher, S., 2004. The Career Break (Time Credit) Scheme in Belgium and the Incentive Premiums by the Flemish Government. Discussion Paper, IDEA Consult, Brussels.

Devisscher, S., Sanders, D., 2007. Ageing and Life-course Issues: The Case of the Career Break Scheme (Belgium) and the Life-course Regulation (Netherlands). In OECD, Modernising Social Policy for the New Life Course (pp. 117-132). 117–132.

Eurofound, 2001. Progressive Retirement in Europe. European Observatory of Working Life . Dublin. http://www.eurofound.europa.eu/observatories/eurwork/comparative-information/progressive-retirement-in-europe.

Fredriksson, P., Johansson, P., 2008. Dynamic Treatment Assignment: The Consequences for Evaluations Using Observational Data. Journal of Business & Economic Statistics 26(4), 435–445.

Frimmel, W., Horvath, T., Schnalzenberger, M., Winter-Ebmer, R., 2015. Seniority Wages and the Role of Firms in Retirement . IZA Discussion Paper No. 9192, IZA, Bonn.

Frölich, M., 2007. Propensity Score Matching Without Conditional Independence Assumption—with an Application to the Gender Wage Gap in the United Kingdom. The Econometrics Journal 10(2), 359–407.

Frölich, M., Huber, M., Wiesenfarth, M., 2015. The Finite Sample Performance of Semi- and Nonparametric Estimators for Treatment Effects and Policy Evaluation . IZA Discussion Papers No. 8756, IZA, Bonn.

Gielen, A., 2009. Working hours flexibility and older workers' labor supply. Oxford Economic Papers, Oxford University Press 61(2), 240–274.

Graf, N., Hofer, H., Winter-Ebmer, R., 2011. Labor supply effects of a subsidized old-age part-time scheme in Austria. Zeitschrift für ArbeitsmarktForschung - Journal for Labour Market Research 44(3), 217–229.

Greenberg, D., Robins, P., 2008. Incorporating nonmarket time into benefit-cost analyses of social programs: An application to the self-sufficiency project. Journal of Public Economics 92(3-4), 766–794.

Gustman, A., Steinmeier, T., 1984. Partial Retirement and the Analysis of Retirement Behavior. Industrial and Labor Relations Review 37(3), 403–415.

Hellerstein, J.K., Neumark, D., 2007. Production function and wage equation estimation with heterogeneous labor: Evidence from a new matched employer-employee data set, in: Hard-to-Measure Goods and Services: Essays in Honor of Zvi Griliches. University of Chicago Press, pp. 31–71.

Hellerstein, J., Neumark, D., Troske, K., 1999. Wages, Productivity, and Worker Characteristics: Evidence from Plant-Level Production Functions and Wage Equations. Journal of Labor Economics 17(3), 409–46.

Hirano, K., Imbens, G.W., Ridder, G., 2003. Efficient Estimation of Average Treatment Effects Using the Estimated Propensity Score. Econometrica 71(4), 1161–1189.

Honig, M., Hanoch, G., 1985. Partial Retirement as a Separate Mode of Retirement Behavior. The Journal of Human Resources 20(1), 21–46.

Horvitz, D.G., Thompson, D.J., 1952. A Generalization of Sampling Without Replacement from a Finite Universe. Journal of the American Statistical Association 47(260), 663–685.

Huber, M., Lechner, M., Wunsch, C., 2013. The Performance of Estimators Based on the Propensity Score. Journal of Econometrics 175(1), 1–21.

Huber, M., Lechner, M., Wunsch, C., 2015. The effect of firms' phased retirement policies on the labour market outcomes of their employees. Forthcoming in the Industrial and Labor Relations Review.

Hurd, M.D., 1996. The Effect of Labor Market Rigidities on the Labor Force Behavior of Older Workers. In: Wise D, editor. Advances in the Economics of Aging. University of Chicago Press 11–60, Chicago and London.

Hutchens, R., 2010. Worker characteristics, job characteristics, and opportunities for phased retirement. Labour Economics 17(6), 1010–1021.

Hutchens, R., Grace-Martin, K., 2006. Employer Willingness to Permit Phased Retirement: Why are Some More Willing Than others? ILR Review 59(4), 525–546.

Ilmakunnas, P., Maliranta, M., 2005. Technology, Labour Characteristics and Wage-productivity Gaps. Oxford Bulletin of Economics and Statistics 67(5), 623–645.

Imbens, G., Wooldridge, J., 2009. Recent Developments in the Econometrics of Program Evaluation. Journal of Economic Literature 47(1), 5–86.

Kantarcı, T., van Soest, A., 2008. Gradual Retirement: Preferences and Limitations. De Economist 156(2), 113–144.

Kleven, H.J., Kreiner, C., 2006. The Marginal Cost of Public Funds: Hours of Work Versus Labor Force Participation. Journal of Public Economics 90(10-11), 1955–1973.

Lazear, E., 1979. Why Is There Mandatory Retirement? Journal of Political Economy 87(6), 1261–84.

Lechner, M., Miquel, R., Wunsch, C., 2011. Long-Run Effects of Public Sector Sponsored Training in West Germany. Journal of the European Economic Association 9(4), 742–784.

Lechner, M., Strittmatter, A., 2014. Practical Procedures to Deal with Common Support Problems in Matching Estimation . Economics Working Paper Series No. 1410, University of St. Gallen, St. Gallen.

Lechner, M., Wunsch, C., 2008. What Did All the Money Do? On the General Ineffectiveness of Recent West German Labour Market Programmes. Kyklos 61(1), 134–174.

Machado, C.S., Portela, M., 2012. Hours of Work and Retirement Behavior . IZA Discussion Paper No. 6270, IZA, Bonn.

Manski, C., Lerman, S.R., 1977. The Estimation of Choice Probabilities from Choice Based Samples. Econometrica 45(8), 1977–88.

OECD, 2015a. OECD.StatExtracts. Retrieved February 4, 2015, from www.stats.oecd.org.

OECD, 2015b. Ageing and Employment Policies - Statistics on average effective age of retirement.

Rudolf, R., 2014. Work Shorter, Be Happier? Longitudinal Evidence from the Korean Five-Day Working Policy. Journal of Happiness Studies 15(5), 1139–1163.

Ruhm, C., 1990. Bridge Jobs and Partial Retirement. Journal of Labor Economics 8(4), 482–501.

Schmid, G., 1998. Transitional labour markets: A new European employment strategy. WZB Discussion Paper, No. FS I 98-206.

Shao, J., 2003. Impact of the Bootstrap on Sample Surveys. Statistical Science 18(2), 191–198.

Sianesi, B., 2004. An Evaluation of the Swedish System of Active Labor Market Programs in the 1990s. The Review of Economics and Statistics 86(1), 133–155.

Staubli, S., Zweimüller, J., 2013. Does raising the early retirement age increase employment of older workers? Journal of Public Economics 108(C), 17–32.

Sundén, A., 1994. Early Retirement in the Swedish Pension System. PhD thesis, Ithaca: Cornell University.

Vandenberghe, V., Waltenberg, F., Rigo, M., 2013. Ageing and Employability. Evidence from Belgian Firm Level Data. Journal of Productivity Analysis 40, 111–136.

Van Looy, P., Kovalenko, M., Mortelmans, D., De Preter, H., 2014. Working hours-reduction in the move to full retirement: How does this affect retirement preferences of 50+ individuals in Flanders? Leuven: Steunpunt WSE/Antwerpen: CELLO, Universiteit Antwerpen.

van Ours, J., Stoeldraijer, L., 2011. Age, Wage and Productivity in Dutch Manufacturing. De Economist 159(2), 113–137.

Vikström, J., 2014. IPW estimation and related estimators for evaluation of active labor market policies in a dynamic setting. Working Paper Series from IFAU - Institute for Evaluation of Labour Market and Education Policy, No 2014:1, Uppsala.

Wadensjö, E., 2006. Part-Time Pensions and Part-Time Work in Sweden . IZA Discussion Paper No. 2273, IZA, Bonn.

A

Supplementary Appendix to “Reforming the Apprenticeship Contract in Italy A Natural Experiment”

This is the Supplementary Appendix to the Chapter 1 “Reforming the Apprenticeship Contract in Italy A Natural Experiment“. This Appendix contains the following Tables and Figures:

(i) Figure A.1 shows the rate in several contracts by treatment status after reweighting the control group by the CBPS estimator;

(ii) Table A.1 lists the estimates on the difference of several regional variables by treatment status during the period 2000-2004 period (yearly observations);

(iii) Figure A.2 contains the evolution of the (youth) unemployment rate in the period 2000-2003 between treated and control regions as defined in 2007 (LFS data in yearly format) ;

(iv) Sevearl robustness tests are shown in the next figures regarding the Average Treatment Effect on the Treated apprentices (ATT) on performing the apprenticeship in the new regime rather than in the old regime:

   a. Figure A.3: the ATT by using the IPW estimator (A) or the CBPS including the youth unemployment rate in 2004 as a control variable (B);
   b. Figure A.4: the ATT by using the CBPS estimator with the max-trimming rule (A) or without trimming (B);
   c. Figure A.5: the ATT by using the shrinkage method on the CBPS estimator (A) or the IPW estimator (B). As recommended by Pohlmeier et al. (2014) for the case with many covariates, we use the cross-validation method and the Crump et al. (2009) trimming rule (i.e. rule of the thumb removing units with p-value above 0.9);
   d. Figure A.6: the ATT by using the CBPS estimator with only significant covariates in affecting the outcome among the control units (see Section 1.6.4);
   e. Figure A.7: the ATT by using the CBPS estimator without the covariates measured at hiring (i.e. part-time work and reason of hiring);

(v) Figure A.8: the estimated ATT on performing the apprenticeship in a smaller firm rather than in a larger firm (employing less or more than 10 employees);

(vi) In Table A.2 we show the estimated Intention-To-Treat (ITT) by implementing the Difference-in-Differences (DiD) estimator on the parallel growth assumption. In this specification, we include a linear trend for each region. To avoid the treatment dummies capturing part of the differential trends, we include a treatment dummy for each *quarter* after the implementation of the reform (i.e. Regional Law).

(vii) In Table A.3 we include the estimates on the effects of the reform on the contract diffusion. In this specification, the Regional Law is considered as the only channel to activate the treatment rather and the treatment implemented by Collective Bargaining Agreements is ignored.

(viii) Finally, estimates on the ITT by nonlinear DiD estimation (probit mode) are reported in the last tables. In particular, the estimates on the employment rate are shown in Table A.4, Table A.5 contains the ITT on the transition from non-employment to employment at one-year distance, Table A.6 regards the impact on the regional commuting inflows and outflows, while Table A.7 shows the estimates on the regional migration inflows.

Figure A.1: Employment rate by treatment status –CBPS reweighted

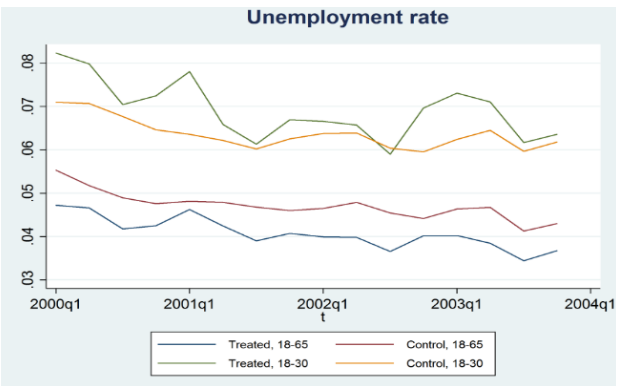
Status at the end of the month: (1) Open-end contract in the same firm, (2) open-end contract in other firms, (3) other apprenticeship, (4) other temporary contract & collaborator, (5) insured unemployment, (6) not in salaried private sector employment. Individuals with more jobs are considered only in one position following the above order. Blue Lines: Treated group; Red lines: control group weighted by CBPS weights.

Table A.1: Difference on regional variables by treatment status (2000-2004 period, yearly data)

	Logs of per capita expenditure for the apprenticeship (1)	Logs of GDP per capita (2)	GDP growth in pp (3)
α – treated dummy	0.7338	0.1323	0.1176
SE	(0.5644)	(0.1234)	(0.3027)
P-value	(0.211)	(0.299)	(0.703)
R sq.	0.124	0.066	0.578
N clusters	18	18	18
N	49	90	83

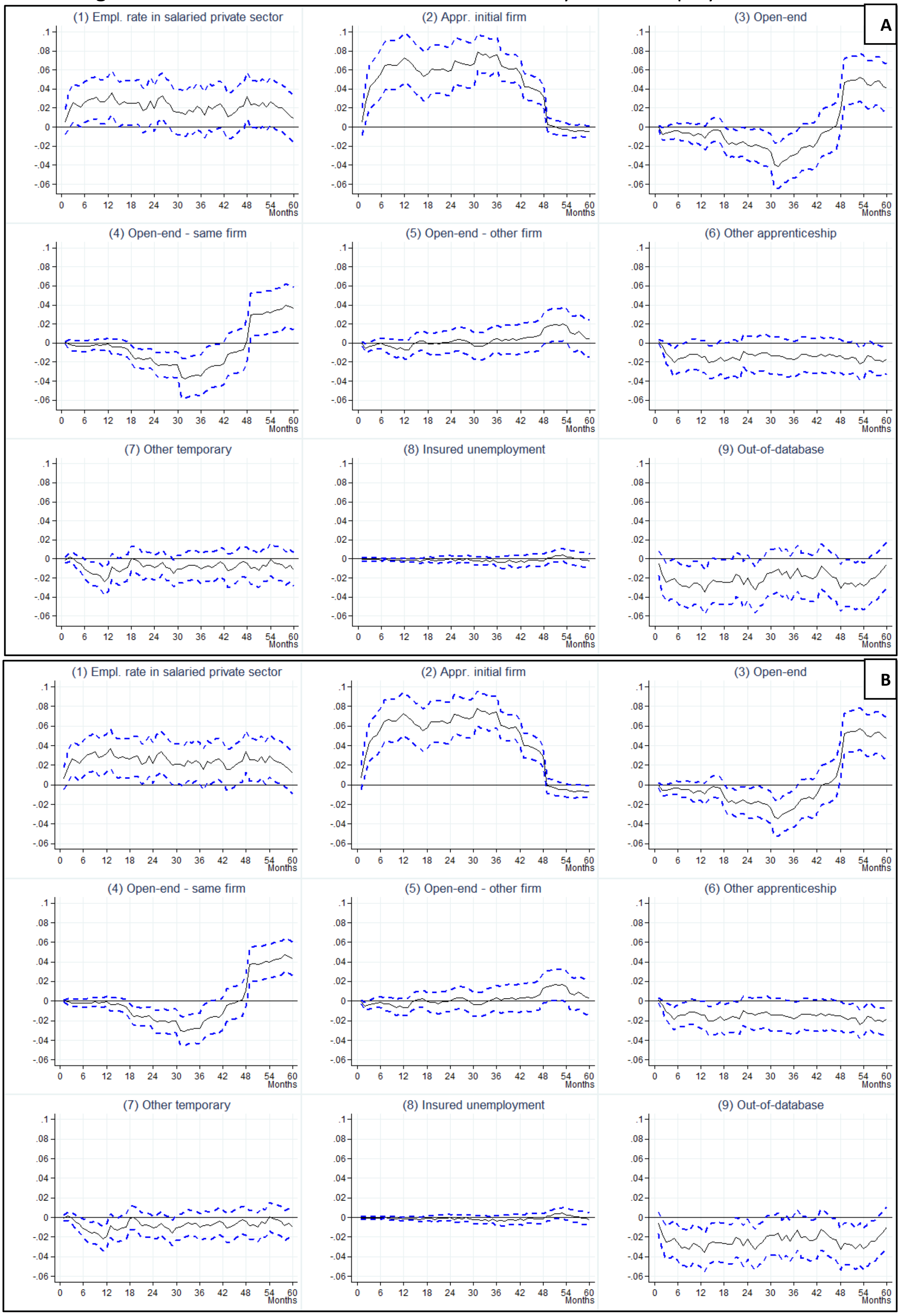
*Yearly differences between treated and control groups in the period 2000-2004 (ISFOL and Eurostat data). The dependent variables of these OLS regressions are the following labour market characteristics in year t: (1) regional expenditure per capita for the apprenticeship in euros (logarithm), (2) GDP per capita (logarithm), (3) GDP growth. Independent variables: yearly time dummies and treated group dummy (*i.e. *regions that had implemented the treatment by 2007). Standard Errors clustered by region to take into account serial correlation (18 clusters). ***: significant at 0.1%, **: significant at 1%, *: significant at 5%.*

Figure A.2: Evolution of unemployment rate in 2000-2003 period (2007 treatment status)



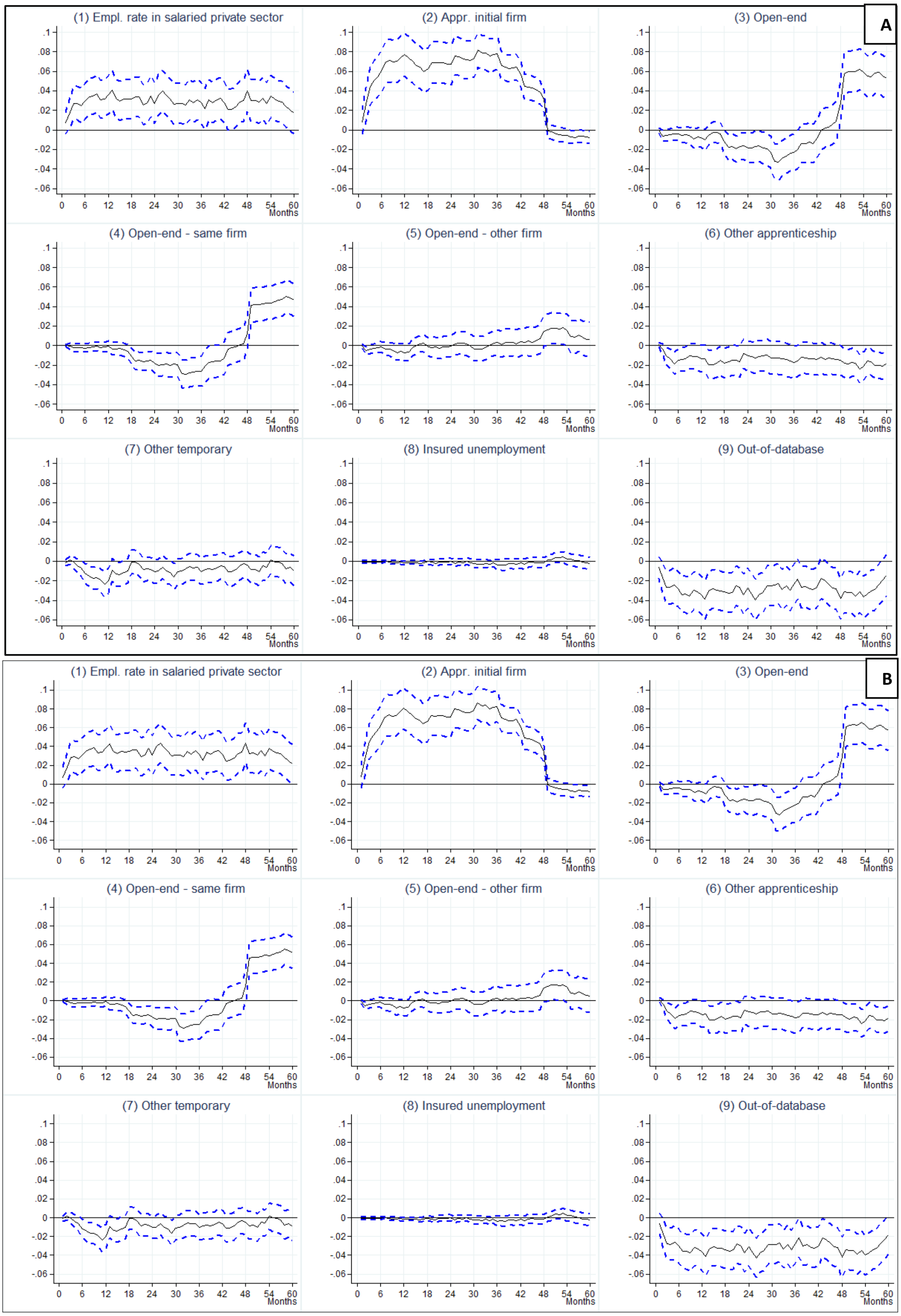
Period 2000-2003 on quarterly LFS data. Evolution of unemployment rate in pp. Treated regions have implemented the treatment by 2007.

Figure A.3: Robustness tests - “IPW” or “CBPS with youth unemployment rate”



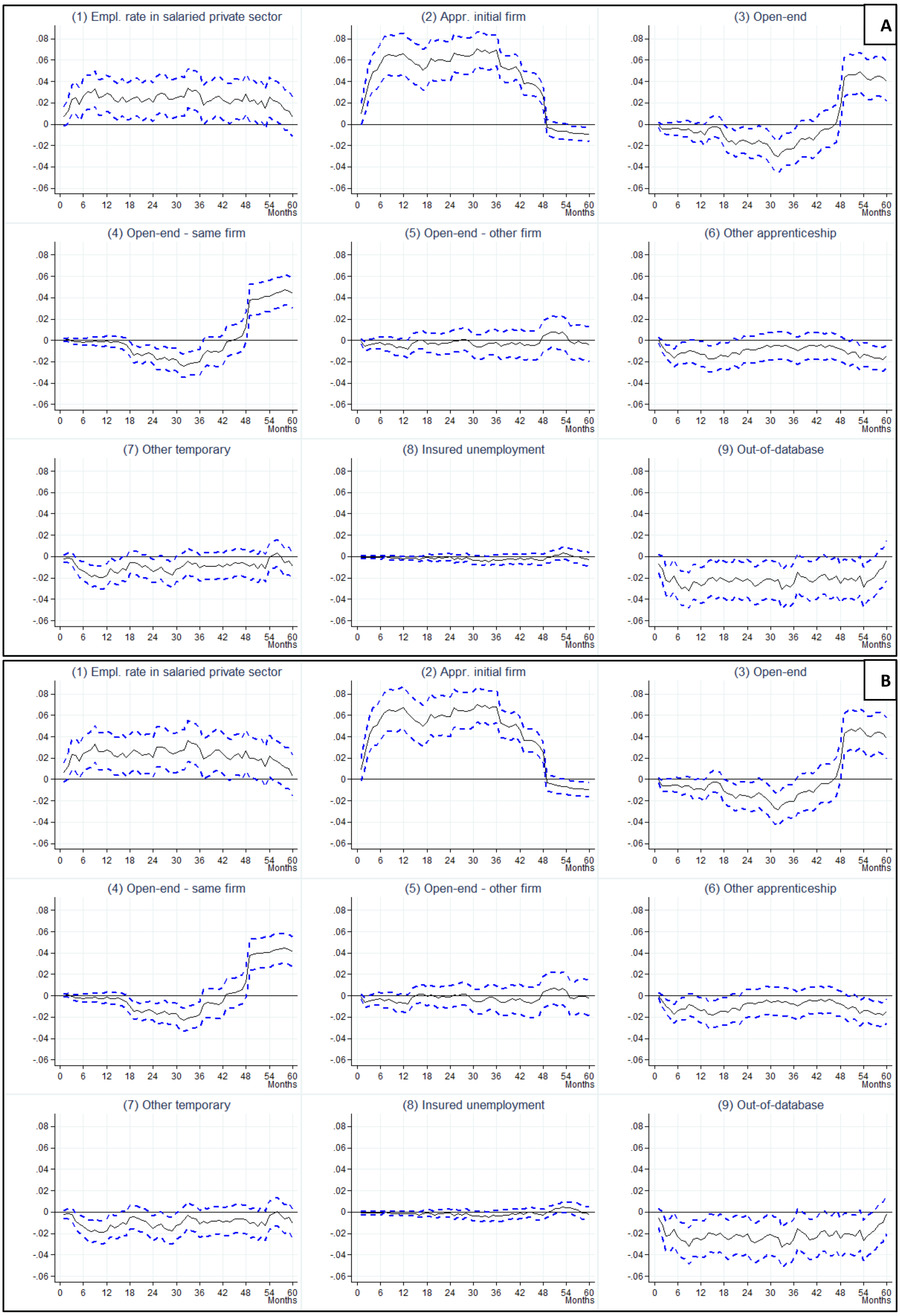
ATT by IPW (A) or CBPS with youth unemployment rate in 2004 as additional covariate (B) of the reformed apprenticeship versus the old apprenticeship on a sample of apprentices hired in 2007 aged 19-24. Status at the end of each month after hiring (from left to right and top to bottom): (1) employment rate in the salaried private sector, (2) apprenticeship in the initial firm, (3) open-end contract (subdivided into the same firm (4) or another firm (5)), (6) other apprenticeship, (7) other temporary or collaborator contracts, (8) insured unemployed, (9) neither in salaried private sector employment nor in insured unemployment. Individuals with more jobs are considered only in one position in the order mentioned above. Cluster Robust Standard Errors by individual to take into account serial correlation.

Figure A.4: Robustness tests - CBPS with max-trimming rule (A) and without trimming (B)



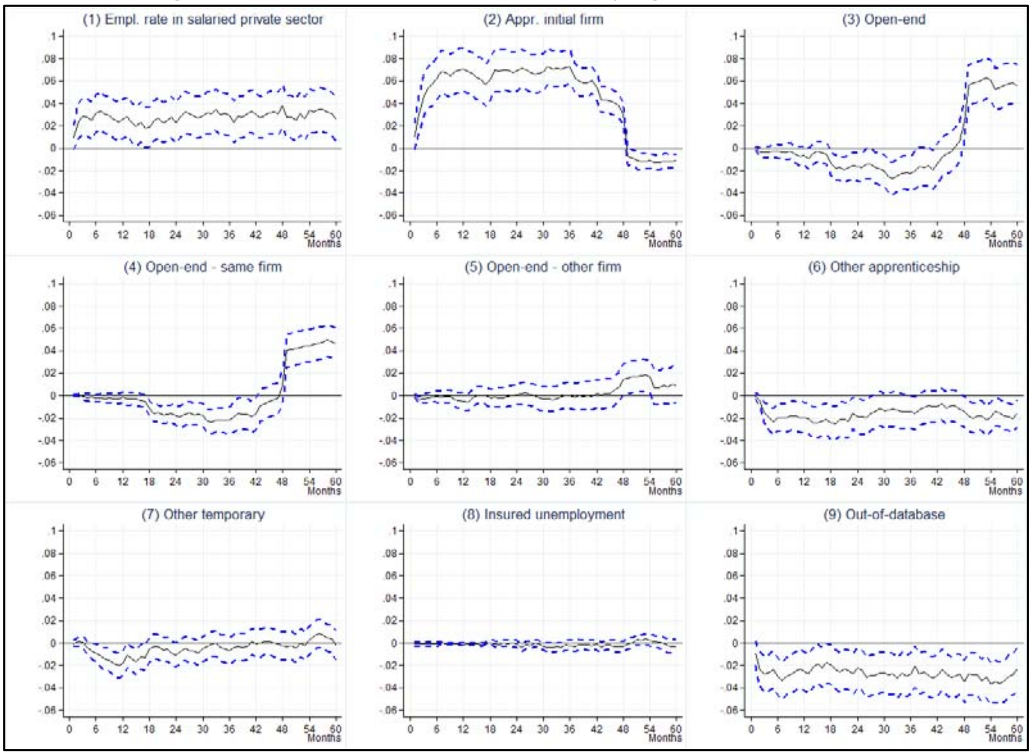
ATT by CBPS with max trimming rule (A) and on the untrimmed sample (B). Treatment defined as participating in the reformed apprenticeship versus the old apprenticeship. Inflow sample of apprentices hired in 2007 aged 19-24. Status at the end of each month after hiring (from left to right and top to bottom): (1) employment rate in the salaried private sector, (2) apprenticeship in the initial firm, (3) open-end contract (subdivided into the same firm (4) or another firm (5)), (6) other apprenticeship, (7) other temporary or collaborator contracts, (8) insured unemployed, (9) neither in salaried private sector employment nor in insured unemployment. Individuals with more jobs are considered only in one position in the order mentioned above. Cluster Robust Standard Errors by individual.

Figure A.5: Robustness tests - CBPS (A) and IPW (B) with shrinkage method



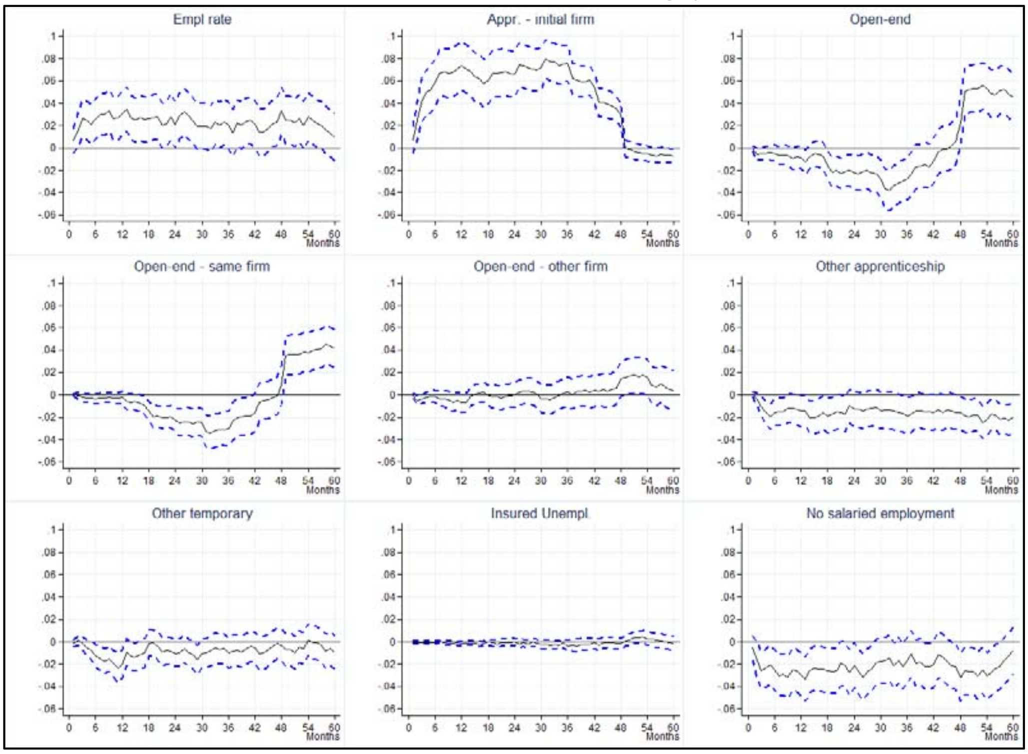
ATT by CBPS (A) and IPW (B) with shrinkage method (cross-validation) and Crump et al. *trimming rule (*i.e. *removing units with p-value above 0.9). The treatment identifies the impact of the reformed apprenticeship versus the old regime on a sample of apprentices hired in 2007 aged 19-24. Status at the end of each month after hiring (from left to right and top to bottom): (1) employment rate in the salaried private sector, (2) apprenticeship in the initial firm, (3) open-end contract (subdivided into the same firm (4) or another firm (5)), (6) other apprenticeship, (7) temporary or collaborator contracts, (8) insured unemployed, (9) neither in salaried private sector employment nor in insured unemployment. Cluster Robust Standard Errors by individual.*

Figure A.6: Robustness - CBPS with only significant covariates



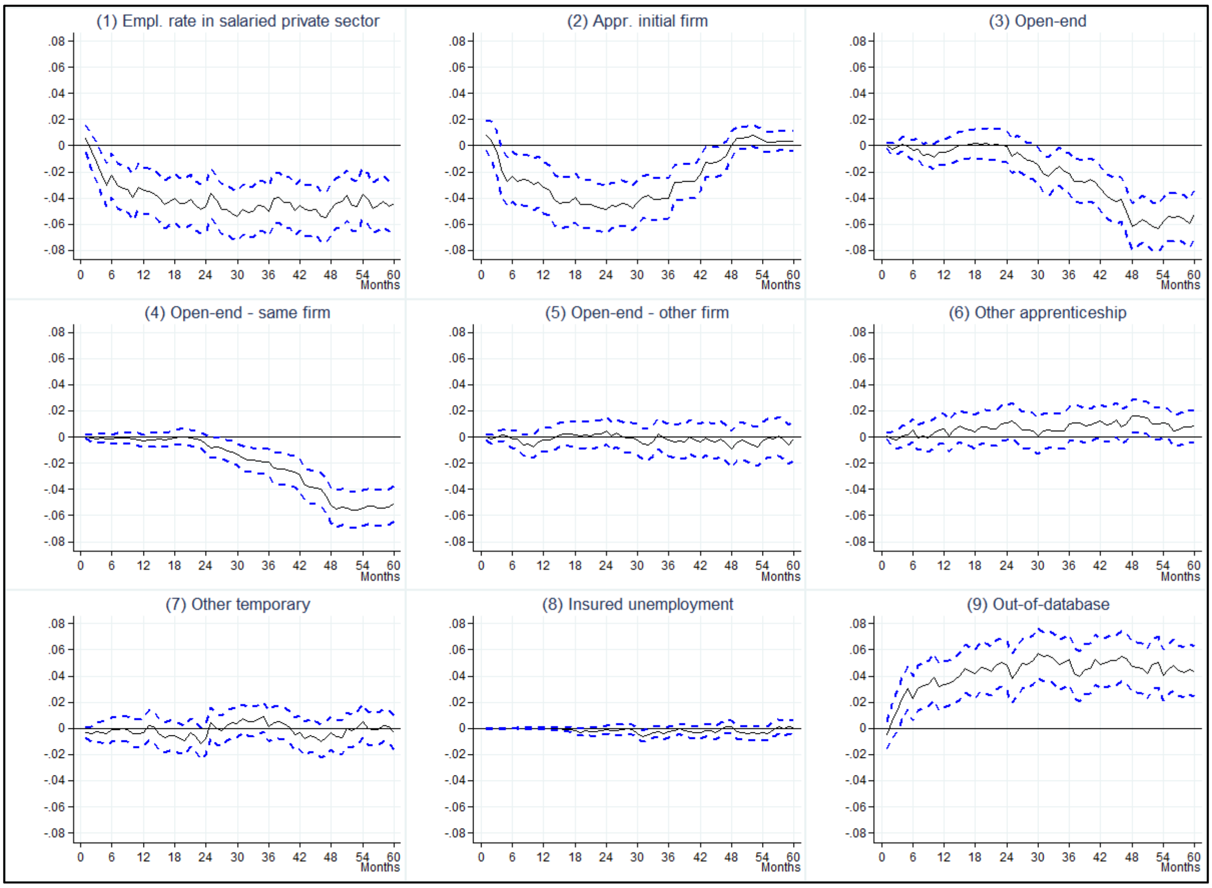
Robustness test: ATT by CBPS. The covariates used are a subset of all the covariates. The procedure is described at the end of Section 1.6.4. Status at the end of each month after hiring (from left to right and top to bottom): (1) employment rate in the salaried private sector, (2) apprenticeship in the initial firm, (3) open-end contract (subdivided into the same firm (4) or another firm (5)), (6) other apprenticeship, (7) other temporary or collaborator contracts, (8) insured unemployed, (9) neither in salaried private sector employment nor insured unemployment. Individuals with more jobs are considered in 1 position in the order mentioned above. Standard Errors clustered by individual.

Figure A.7: Robustness - CBPS without information at hiring (part-time and reason of hiring)



ATT by CBPS of the reformed apprenticeship versus the old apprenticeship on a sample of 17,914 apprentices hired in 2007 aged 19-24. No information measured at hiring are used (i.e. part-time work and reason of hiring). Status at the end of each month after hiring (from left to right and top to bottom): (1) employment rate in the salaried private sector, (2) apprenticeship in the initial firm, (3) open-end contract (subdivided into the same firm (4) or another firm (5)), (6) other apprenticeship, (7) other temporary or collaborator contracts, (8) insured unemployed, (9) neither in salaried private sector employment nor in insured unemployment. Individuals with more jobs are considered only in one position using the order mentioned above. Standard Errors are clustered by individual to take into account serial correlation.

Figure A.8: ATT of the apprenticeship in small firms rather than large firms (< 10 employees)



ATT by CBPS of apprenticeship in a small versus a large firm (above or below 10 employees). Inflow sample of 17,914 apprentices. Status at the end of the month after hiring (from left to right and top to bottom): (1) salaried private sector, (2) apprenticeship in initial firm, (3) open-end contract (in 4: in the same firm; 5: in another firm), (6) other apprenticeship, (7) other temporary or collaborator contracts, (8) insured unemployed, (9) neither in salaried private sector employment nor insured unemployment. Cluster Robust Standard Errors by individual.

Table A.2: ITT on employment rate and transition from non-employment– parallel growth

	Employment rate (1)						Transition from non-employment (2)					
	2004q1-2011q1 (A)			2004q1-2008q3 (B)			2004q1-2011q1 (A)			2004q1-2008q3 (B)		
Age class	**20-24**	**25-29**	**30-34**	**20-24**	**25-29**	**30-34**	**20-24**	**25-29**	**30-34**	**20-24**	**25-29**	**30-34**
ITT – quarter 1	1.3	0.1	-0.2	1.2	0.4	0.1	1.6	-0.6	2.1**	2.3	0.6	2.7**
p-value	0.163	0.895	0.836	0.178	0.9	0.867	0.264	0.802	0.039	0.306	0.801	0.046
ITT – quarter 2	-2.4	-1.3	-0.3	-3.2	-1.0	-0.2	-1.6	-0.4	0.7	-3.3	2.0	1.0
p-value	0.207	0.354	0.606	0.101	0.670	0.699	0.319	0.915	0.579	0.129	0.724	0.560
ITT – quarter 3	0.9	0.895	-0.2	0.9	-0.6	-0.4	3.0	3.2**	0.2	4.5	5.4***	0.5
p-value	0.529	0.257	0.817	0.633	0.459	0.657	0.207	0.018	0.906	0.154	0.005	0.757
ITT – quarter 4	0.7	-0.1	0.2	0.9	-0.4	0.1	3.7**	0.9	0.7	4.1*	1.9	1.2
p-value	0.577	0.908	0.875	0.406	0.732	0.949	0.036	0.635	0.502	0.056	0.406	0.306
ITT – quarter 5	-0.9	-0.4	-1.0	-0.4	-0.3	-1.3	-0.2	0.6	-0.0	1.1	2.8	-0.9
p-value	0.665	0.677	0.430	0.868	0.730	0.309	0.859	0.750	0.932	0.663	0.212	0.451
ITT – quarter 6+	-1.4	0.1	-1.2	-0.0	-0.5	-0.3	-1.3	2.6**	-0.5	1.3	2.4	-0.2
p-value	0.578	0.913	0.464	0.984	0.770	0.751	0.754	0.043	0.728	0.738	0.184	0.896
ITT – quarter 7	-0.8	-1.1	0.7	-0.3	-2.6	0.9	-4.3*	-0.4	1.2	-4.5	-1.2	1.7
p-value	0.552	0.403	0.372	0.848	0.166	0.409	0.065	0.898	0.409	0.302	0.616	0.684
ITT – quarter 8	-1.6	-0.1	0.8	-2.1	-0.5	1.6	0.3	-0.5	1.8	-0.8	1.3	3.9
p-value	0.270	0.928	0.481	0.198	0.815	0.357	0.868	0.778	0.304	0.813	0.629	0.151
ITT – quarter 9	-3.9	0.5	-0.1	-2.7	0.4	-0.3	-0.5	2.0	0.7	1.0	4.9	0.5
p-value	0.122	0.512	0.959	0.386	0.638	0.894	0.887	0.407	0.594	0.703	0.352	0.852
ITT – quarter 10	-0.8	1.7	-0.4	0.5	0.9	-0.1	2.5	3.4	-0.3	2.9	7.0**	0.4
p-value	0.806	0.113	0.676	0.651	0.561	0.939	0.399	0.113	0.699	0.306	0.046	0.832
ITT – quarter 11	-1.6	1.3	1.3	-1.7	-0.3	1.3	0.2	2.6	-1.0	-1.6	3.1	0.4
p-value	0.498	0.375	0.109	0.165	0.851	0.408	0.921	0.170	0.640	0.441	0.127	0.779
ITT – quarter 12	-0.1	0.3	1.6	1.3	-0.2	1.6	-0.3	4.3*	1.0	-1.9	4.0	1.0
p-value	0.975	0.802	0.051	0.386	0.909	0.168	0.941	0.075	0.395	0.637	0.186	0.504
ITT – quarter 13	-3.1	1.1	1.9	1.5	1.1	0.8	-3	4.0**	3.8	4.2	4.6	3.1
p-value	0.453	0.451	0.181	0.505	0.717	0.827	0.467	0.032	0.131	0.576	0.129	0.649
ITT – quarter 14	-2.6	1.3	1.5	0.6	-0.2	1.7	1.5	3.0*	-0.4	5.6	5.0	-1.7
p-value	0.434	0.169	0.222	0.653	0.839	0.696	0.617	0.094	0.828	0.354	0.485	0.887
ITT – quarter 15	-2.2	0.9	2.8**	4.7	-2.8	5.3	0	4.2	2.0	1.4	1.8	2.6
p-value	0.461	0.462	0.036	0.427	0.300	0.191	0.998	0.130	0.323	0.632	0.345	0.228
ITT – quarter 16	-2.4	1.4	1.7	-	-	-	-1.4	0.9	1.0	-	-	-
p-value	0.467	0.447	0.347	-	-	-	0.762	0.401	0.619	-	-	-
ITT – quarter 17	-4.5	0.2	0.7	-	-	-	-5.6	3.3	1.2	-	-	-
p-value	0.080	0.881	0.603	-	-	-	0.261	0.318	0.532	-	-	-
ITT – quarter 18	-2.8	-0.3	-0.1	-	-	-	3.8	0.2	-2.0	-	-	-
p-value	0.392	0.799	1.025	-	-	-	0.33	0.921	0.151	-	-	-
ITT – quarter 18	-0.7	0.4	-1.2	-	-	-	0.2	2.8	1.1	-	-	-
p-value	0.834	0.649	0.471	-	-	-	0.991	0.408	0.654	-	-	-
ITT – quarter 19	-1.8	-1.6	0.7	-	-	-	-1.7	-1.5	0.1	-	-	-
p-value	0.555	0.378	0.772	-	-	-	0.592	0.467	0.927	-	-	-
ITT – quarter 20	-1.4	-1.0	-0.8	-	-	-	1.6	0.0	-3.5*	-	-	-
p-value	0.662	0.345	0.711	-	-	-	0.666	0.972	0.066	-	-	-
ITT – quarter 21	0.6	1.2	-2.5	-	-	-	3.3	4.4*	-0.0	-	-	-
p-value	0.804	0.563	0.121	-	-	-	0.44	0.071	0.982	-	-	-
ITT – quarter 22	-7.4	1.0	1.2	-	-	-	-5.9	3.9	2.3	-	-	-
p-value	0.120	0.790	0.336	-	-	-	0.122	0.355	0.503	-	-	-
ITT – quarter 23	-3.8	2.2	3.6	-	-	-	-6.4	8.0	2.8	-	-	-
p-value	0.287	0.337	0.347	-	-	-	0.153	0.123	0.412	-	-	-
ITT – quarter 24	-6.8	0.9	-0.2	-	-	-	-5.2	11.5	-0.2	-	-	-
p-value	0.431	0.656	0.840	-	-	-	0.452	0.746	0.787	-	-	-
N	128,821	193,073	258,357	89,375	134,849	178,113	52,202	60,841	71,520	35,518	41,667	48,485

*ITT on the employment rate in the salaried private sector (1) and non-employment transition (2) in pp by age class. DiD on parallel growth by OLS regression controlling for individual characteristics (education and gender), time dummies, regional fixed-effect and 17 regional trends. Specification with the full set of treatment dummies for all the quarters after the treatment. Sample: Columns 1: full population. Columns 2: population non-employed one year before t (excluding students in t-1 year and t). P-value obtained by wild cluster bootstrap-t (2000 repetitions) with 17 regions as clusters. ***: significant at 1%, **: significant at 5%, *: significant at 10%.*

Table A.3: ITT on contract diffusion and placebo tests; only regional treatment

2004q1-2011q1	Age class: 20-24					Age class: 25-29					Age class: 30-34				
(A)	Appr. (1)	Open-end(2)	Collab.(3)	Temp.(4)	Self-empl.(5)	Appr.(1)	Open-end(2)	Collab.(3)	Temp.(4)	Self-empl.(5)	Appr.(1)	Open-end(2)	Collab.(3)	Temp.(4)	Self-empl.(5)
ITT – year 1	-0.6	-0.7	0.4	0.9	1.1	0.6	-0.1	-0.9**	0.4	0.5	-0.1	0.8	-0.3	-0.4	-0.1
p-value	0.686	0.722	0.385	0.447	0.079	0.161	0.898	0.018	0.592	0.317	0.496	0.424	0.511	0.576	0.668
ITT – year 2	-1.4**	2.6	-0.6	-0.7	1.2	0.8	-0.2	-0.8	0.2	0.3	0.1	0.7	-0.3	-0.5	-0.2
p-value	0.032	0.260	0.139	0.719	0.188	0.170	0.885	0.064	0.855	0.579	0.380	0.525	0.381	0.544	0.587
ITT – year 3	-0.3	-1.1	-0.5	1.8	0.2	0.7	-1.2	-0.5	0.9	-0.6	0.1	-0.9	-0.3	1.1	-0.7
p-value	0.697	0.671	0.326	0.327	0.869	0.202	0.543	0.431	0.437	0.567	0.491	0.609	0.432	0.290	0.406
ITT – year 4	-0.8	0.1	-0.2	1.0	0.1	1.0	-1.1	-0.1	0.1	-1.2	0.1	0.3	-0.4	-0.0	-1.0
p-value	0.653	0.963	0.761	0.653	0.857	0.110	0.529	0.913	0.919	0.122	0.539	0.792	0.226	1.017	0.166
ITT – year 5	1.7	-5.9	0.1	4.1	1.0	1.8**	-2.7	0.6	0.3	-1.0**	0.3	-0.9	-0.5	1.2	-0.2
p-value	0.228	0.082	0.866	0.100	0.357	0.022	0.087	0.361	0.847	0.049	0.359	0.308	0.301	0.272	0.874
ITT – year 6+	1.4	-3.0	-0.3	1.9	0.3	2.5**	-1.7	-0.0	-0.8	-0.4	0.2	-0.5	-0.4	0.7	-1.0
p-value	0.432	0.184	0.610	0.209	0.685	0.032	0.260	0.978	0.304	0.715	0.403	0.631	0.480	0.437	0.433
N	68,153	68,153	68,153	68,153	76,831	107,792	107,792	107,792	107,792	130,934	140,716	140,716	140,716	140,716	182,231

2004q1-2008q3	Age class: 20-24					Age class: 25-29					Age class: 30-34				
(B)	Appr. (1)	Open-end(2)	Collab.(3)	Temp.(4)	Self-empl.(5)	Appr.(1)	Open-end(2)	Collab.(3)	Temp.(4)	Self-empl.(5)	Appr.(1)	Open-end(2)	Collab.(3)	Temp.(4)	Self-empl.(5)
ITT – year 1	-0.2	-1.2	0.3	1.1	1.2	0.9	-0.3	-0.9**	0.3	0.4	-0.1	0.6	-0.2	-0.3	0.1
p-value	0.507	0.856	0.236	0.876	0.160	0.738	0.504	0.029	0.860	0.268	0.365	0.329	0.835	0.295	0.952
ITT – year 2	-0.8**	3.8**	-1.1	-1.9	1.3	0.6	0.7	-0.9	-0.4	0.2	0.1	1.2	-0.3	-1.0	-0.3
p-value	0.019	0.018	0.353	0.262	0.216	0.576	0.484	0.082	0.571	0.587	0.497	0.230	0.661	0.179	0.675
ITT – year 3	0.2	-0.7	-1.3	1.7	-0.3	1.0	-1.6	-0.5	1.1	-0.3	-0.1	-0.0	-0.5	0.6	0.1
p-value	0.518	0.954	0.316	0.524	0.740	0.630	0.982	0.341	0.872	0.488	0.689	0.573	0.800	0.369	0.404
ITT – year 4+	0.5	-0.6	-0.5	0.6	0.3	1.9**	-0.2	-0.1	-1.6**	-1.8	-0.1	2.6	-0.8	-1.8	0.7
p-value	0.431	0.055	0.632	0.099	0.959	0.022	0.997	0.953	0.038	0.123	0.204	0.825	0.860	0.949	0.185
N	48,558	48,558	48,558	48,558	54,837	76,017	76,017	76,017	76,017	92,889	96,799	96,799	96,799	96,799	126,559

*ITT on the contract diffusion in pp by age class. DiD on parallel path by OLS regression controlling for individual characteristics (education and gender), time dummies, regional fixed-effect and anticipation dummies (for the two quarters before the treatment). Dependent variable by column: 1) Apprenticeship, 2) Open-End, 3) Collaborators, 4) Temporary contract, 5) Self-employed. Sample: youth employed excluding employees in the public administration (column 5) and self-employed (columns 1-4). Specifications: treatment dummies per each year after the treatment; Panel A) period 2004q1-2011q1, Panel B) Period 2004q1-2008q3. P-value obtained by wild cluster bootstrap-t (2000 repetitions) with 17 regions as clusters. ***: significant at 1%, **: significant at 5%, *: significant at 10%.*

Table A.4: ITT on employment rate in salaried private sector - parallel path (Probit)

	2004q1- 2011q1 (1, A)			2004q1-2008q3 (2, A)			2004q1- 2011q1 (1, B)			2004q1-2008q3 (2, B)		
Age class	**20-24**	**25-29**	**30-34**	**20-24**	**25-29**	**30-34**	**20-24**	**25-29**	**30-34**	**20-24**	**25-29**	**30-34**
ITT pp – year 1	-1.1	0.0	0.1	-0.4	-0.6	-0.2	-0.4	-0.6	-0.5	-0.3	-0.7	-0.4
Marg. Index	-0.0296	-0.0013	0.0038	-0.0130	-0.0156	-0.0042	-0.0108	-0.0176	-0.0143	-0.0093	-0.0194	-0.0116
p-value	0.497	0.918	0.805	0.759	0.566	0.815	0.767	0.535	0.360	0.779	0.341	0.488
ITT pp – year 2	-	-	-	-	-	-	-1.2	-0.7	0.3	-1.7	-0.1	0.0
Marg. Index	-	-	-	-	-	-	-0.0332	-0.0203	0.0091	-0.0482	-0.0037	-0.0011
p-value	-	-	-	-	-	-	0.476	0.434	0.863	0.324	0.790	0.966
ITT pp – year 3	-	-	-	-	-	-	0.4	0.3	0.8	-1.1	1.1	1.0
Marg. Index	-	-	-	-	-	-	0.0129	0.0089	0.0215	-0.0303	0.0315	0.0266
p-value	-	-	-	-	-	-	0.700	0.894	0.537	0.681	0.278	0.157
ITT pp – year 4	-	-	-	-	-	-	1.6	-0.9	2.7	-2.3	1.1	1.7
Marg. Index	-	-	-	-	-	-	0.0488	-0.0254	0.0745	-0.0646	0.0295	0.0461
p-value	-	-	-	-	-	-	0.441	0.403	0.101	0.359	0.218	0.112
ITT pp – year 5	-	-	-	-	-	-	-	-	-	-1.4	-0.6	-0.3
Marg. Index	-	-	-	-	-	-	-	-	-	-0.0374	-0.0165	-0.0078
p-value	-	-	-	-	-	-	-	-	-	0.620	0.439	0.861
ITT pp – year 6+	-	-	-	-	-	-	-	-	-	-3.0	1.5	0.1
Marg. Index	-	-	-	-	-	-	-	-	-	-0.0812	0.0399	0.0037
p-value	-	-	-	-	-	-	-	-	-	0.423	0.389	0.908
PLACEBO in *pp*	-	-	-	-	-	-	0.4	0.5	0.4	0.5	0.2	-0.1
Marg. Index	-	-	-	-	-	-	0.0112	0.0145	0.0106	0.0130	0.0058	-0.0029
p-value	-	-	-	-	-	-	0.722	0.541	0.574	0.660	0.766	0.880
N	128,821	193,073	258,357	89,375	134,849	178,113	89,375	134,849	178,113	128,821	193,073	258,357

*ITT on the employment rate in the salaried private sector in pp by age class. DiD by probit controlling for individual characteristics, time dummies, regional fixed-effect and anticipation dummies. For each year of treatment we report the marginal probability effect in pp (1st row) and marginal index effect (2nd row) of the treatment. Specification either with one treatment dummy for all the quarters after the treatment (A) or divided per each year after the Regional Law implementation (B). Sample: all youth excluding students. P-value obtained by Score bootstrap (2000 repetitions) with 17 regions as clusters. ***: significant at 1%, **: significant at 5%, *: significant at 10%.*

Table A.5: ITT on transition from non-employment and placebo tests - parallel path (Probit)

	2004q1- 2011q1 (1, A)			2004q1-2008q3 (2, A)			2004q1- 2011q1 (1, B)			2004q1-2008q3 (2, B)		
Age class	**20-24**	**25-29**	**30-34**	**20-24**	**25-29**	**30-34**	**20-24**	**25-29**	**30-34**	**20-24**	**25-29**	**30-34**
ITT pp – year 1	0.4	1.8**	0.4	0.6	2.6	0.2	0.8	0.8	0.2	1.2	2.6	0.0
Marg. Index	0.0124	0.0595	0.0169	0.0163	0.0802	0.0083	0.0268	0.0281	0.0064	0.0339	0.0815	0.0005
p-value	0.803	0.044	0.469	0.752	0.123	0.818	0.482	0.705	0.858	0.457	0.216	0.989
ITT pp – year 2	-	-	-	-	-	-	-0.8	1.3	0.7	-0.8	1.5	0.9
Marg. Index	-	-	-	-	-	-	-0.0252	0.0421	0.0270	-0.0231	0.0468	0.0320
p-value	-	-	-	-	-	-	0.716	0.466	0.373	0.781	0.523	0.468
ITT pp – year 3	-	-	-	-	-	-	0.6	4.0**	0.7	-0.2	4.4*	0.2
Marg. Index	-	-	-	-	-	-	0.0175	0.1308	0.0254	-0.0050	0.1370	0.0083
p-value	-	-	-	-	-	-	0.842	0.041	0.519	0.929	0.089	0.890
ITT pp – year 4	-	-	-	-	-	-	0.5	3.6**	1.0	3.8	3.9	-0.9
Marg. Index	-	-	-	-	-	-	0.0158	0.1254	0.0383	0.0988	0.1245	-0.0300
p-value	-	-	-	-	-	-	0.821	0.043	0.398	0.348	0.365	0.793
ITT pp – year 5	-	-	-	-	-	-	2.7	1.3	-0.6	-	-	-
Marg. Index	-	-	-	-	-	-	0.0843	0.0483	-0.0252	-	-	-
p-value	-	-	-	-	-	-	0.330	0.413	0.553	-	-	-
ITT pp – year 6+	-	-	-	-	-	-	-0.2	6.6**	1.3	-	-	-
Marg. Index	-	-	-	-	-	-	-0.0051	0.2283	0.0466	-	-	-
p-value	-	-	-	-	-	-	0.945	0.035	0.317	-	-	-
PLACEBO in *pp*	-	-	-	-	-	-	1.1	-0.2	0.1	1.3	1.8	0.5
Marg. Index	-	-	-	-	-	-	0.0374	-0.0066	0.0029	0.0378	0.0590	0.0183
p-value	-	-	-	-	-	-	0.438	0.888	0.903	0.646	0.242	0.654
N	52,202	60,841	71,520	35,518	41,667	48,485	52,202	60,841	71,520	35,518	41,667	48,485

*ITT on the transition to the salaried private sector from non-employment by age class. DiD on parallel path by probit controlling for individual characteristics (education and gender), time dummies, regional fixed-effect and anticipation dummies (for the two quarters before the treatment). For each year of treatment we report the marginal probability effect in pp (1st row) and marginal index effect (2nd row) of the treatment. Specification either with one treatment dummy for all the quarters after the treatment (A) or divided per each year after the Regional Law implementation (B). As placebo tests, we rerun the analysis adding a placebo dummy for the 4th and the 3rd quarter before the treatment. Sample: population non-employed one year before t (excluding students in t-1 year and t). P-value by Score bootstrap (2000 repetitions) with 17 regions s clusters. **: significant at 5%, *: significant at 10%.*

Table A.6: Compositional changes - effect on commuting inflows and outflows (probit)

α - INFLOW	2004q1-2011q1 (1, A)			2004q1-2008q3 (2, A)			2004q1- 2011q1 (1, B)			2004q1-2008q3 (2, B)		
Age class	**20-24**	**25-29**	**20-29**	**20-24**	**25-29**	**20-29**	**20-24**	**25-29**	**20-29**	**20-24**	**25-29**	**20-29**
Effect in pp – year 1	-0.8	-0.5	-0.6	-1.1	-0.5	-0.8	-1.1	-0.9	-0.9	-0.9	-0.8	-0.8
Marg. Index	-0.0902	-0.0599	-0.0707	-0.1274	-0.0609	-0.0855	-0.1402	-0.1079	-0.1163	-0.1093	-0.0955	-0.0992
p-value	0.405	0.448	0.338	0.357	0.608	0.378	0.295	0.287	0.129	0.474	0.410	0.288
Effect in pp – year 2	-	-	-	-	-	-	-1.1	0.1	-0.4	-1.7	0.0	-0.6
Marg. Index	-	-	-	-	-	-	-0.1334	0.0101	-0.0448	-0.2098	0.0037	-0.0742
p-value	-	-	-	-	-	-	0.161	0.942	0.623	0.136	0.987	0.568
Effect in pp – year 3	-	-	-	-	-	-	0.2	-0.9	-0.5	0.0	-1.1	-0.7
Marg. Index	-	-	-	-	-	-	0.0164	-0.1021	-0.0539	0.0026	-0.0911	-0.0626
p-value	-	-	-	-	-	-	0.161	0.942	0.623	0.987	0.611	0.646
Effect in pp – year 4	-	-	-	-	-	-	-0.8	-0.5	-0.6	-	-	-
Marg. Index	-	-	-	-	-	-	-0.0966	-0.0594	-0.0795	-	-	-
p-value	-	-	-	-	-	-	0.590	0.548	0.458	-	-	-
Effect in pp – year 5	-	-	-	-	-	-	-1.3	-0.4	-0.7	-	-	-
Marg. Index	-	-	-	-	-	-	-0.1817	-0.0560	-0.0974	-	-	-
p-value	-	-	-	-	-	-	0.285	0.679	0.411	-	-	-
Effect in pp – year 6+	-	-	-	-	-	-	1.3	0.3	0.6	-	-	-
Marg. Index	-	-	-	-	-	-	0.1141	0.0273	0.0570	-	-	-
p-value	-	-	-	-	-	-	0.548	0.883	0.687	-	-	-
N	11,215	16,844	28,059	7,944	11,569	19,513	11,215	16,844	28,059	7,944	11,569	19,513

β - OUTFLOW	2004q1-2011q1 (1, A)			2004q1-2008q3 (2, A)			2004q1- 2011q1 (1, B)			2004q1-2008q3 (2, B)		
Age class	**20-24**	**25-29**	**20-29**	**20-24**	**25-29**	**20-29**	**20-24**	**25-29**	**20-29**	**20-24**	**25-29**	**20-29**
Effect in pp – year 1	-0.8	-0.5	-0.6	-0.8	0.1	-0.2	-0.5	-0.6	-0.5	-0.7	0.1	-0.2
Marg. Index	-0.0902	-0.0599	-0.0707	-0.1571	0.0110	-0.0387	-0.0909	-0.0835	-0.0773	-0.1407	0.0159	-0.0272
p-value	0.405	0.448	0.338	0.434	0.930	0.779	0.564	0.424	0.447	0.496	0.919	0.834
Effect in pp – year 2	-	-	-	-	-	-	-0.9	-0.1	-0.4	-1.3	-0.2	-0.6
Marg. Index	-	-	-	-	-	-	-0.1789	-0.0165	-0.0726	-0.2684	-0.0275	-0.1046
p-value	-	-	-	-	-	-	0.194	0.919	0.534	0.168	0.868	0.509
Effect in pp – year 3	-	-	-	-	-	-	0.9	-1.0	-0.2	0.6	0.6	0.6
Marg. Index	-	-	-	-	-	-	0.1383	-0.1752	-0.0362	0.1211	0.1156	0.1241
p-value	-	-	-	-	-	-	0.194	0.919	0.534	0.708	0.596	0.649
Effect in pp – year 4	-	-	-	-	-	-	0.7	1.3*	1.1	-	-	-
Marg. Index	-	-	-	-	-	-	0.1010	0.1906	0.1594	-	-	-
p-value	-	-	-	-	-	-	0.290	0.092	0.066	-	-	-
Effect in pp – year 5	-	-	-	-	-	-	-0.9	-0.1	-0.4	-	-	-
Marg. Index	-	-	-	-	-	-	-0.1833	-0.0245	-0.0748	-	-	-
p-value	-	-	-	-	-	-	0.483	0.824	0.602	-	-	-
Effect in pp – year 6+	-	-	-	-	-	-	-1.3***	0.6	-0.1	-	-	-
Marg. Index	-	-	-	-	-	-	-0.4582	0.1310	-0.0177	-	-	-
p-value	-	-	-	-	-	-	0.000	0.463	0.854	-	-	-
N	11,215	16,844	28,059	7,931	11,553	19,484	11,215	16,844	28,059	7,931	11,553	19,484

*DiD (probit) on the inter-regional commuting flows by age class (only employed people). For each year of treatment we report the marginal probability effect in pp (1st row) and marginal index effect (2nd row) of the treatment.). We control for individual characteristics (education, gender, citizenship and distance to the closest province in another region), time dummies and regional fixed-effect. The dependent is equal to one if the person lives in another region with respect to the region of work. Commuting outflow (panel α above): regional dummies regard the region where the person lives. Commuting inflow (panel β below): regional dummies refer to the region where the person works. Specification either with a unique treatment dummy for all the period after the treatment (columns A) or a treatment dummy per each year after the treatment (columns B). Columns 1 end the analysis in the 1st quarter 2011, while columns 2 in the 3rd quarter 2008. P-value by Score bootstrap (2000 repetitions) with 19 regions as clusters. ***: significant at 1%, **: significant at 5%, *: significant at 10%.*

Table A.7: Compositional changes - effect on immigration inflows in treated regions (probit)

	2005q1-2011q1 (1, A)			2005q1-2008q3 (2, A)			2005q1- 2011q1 (1, B)			2005q1-2008q3 (2, B)		
Age class	**20-24**	**25-29**	**20-29**	**20-24**	**25-29**	**20-29**	**20-24**	**25-29**	**20-29**	**20-24**	**25-29**	**20-29**
Effect in pp – year 1	0.0	-0.1	0.0	-0.1	-0.3	-0.2	0.0	0.0	0.0	0.0	-0.3	-0.2
Marg. Index	-0.0171	-0.0266	-0.0220	-0.0294	-0.1057	-0.0846	0.0003	-0.0206	-0.0105	0.0061	-0.1390	-0.0865
p-value	0.901	0.767	0.729	0.898	0.391	0.414	1.000	0.848	0.878	0.982	0.270	0.383
Effect in pp – year 2	-	-	-	-	-	-	-0.1	0.0	-0.1	-0.2	-0.1	-0.2
Marg. Index	-	-	-	-	-	-	-0.0853	-0.0179	-0.0429	-0.1128	-0.0319	-0.0762
p-value	-	-	-	-	-	-	0.574	0.836	0.642	0.669	0.849	0.603
Effect in pp – year 3	-	-	-	-	-	-	0.0	-0.1	-0.1	-0.2	-0.4	-0.3
Marg. Index	-	-	-	-	-	-	-0.0057	-0.0657	-0.0497	-0.1108	-0.1314	-0.1244
p-value	-	-	-	-	-	-	0.966	0.469	0.519	0.680	0.296	0.406
Effect in pp – year 4	-	-	-	-	-	-	0.2	-0.4	-0.1	0.4	-1.4	-0.7
Marg. Index	-	-	-	-	-	-	0.1145	-0.1803	-0.0629	0.1353	-0.4695*	-0.2419
p-value	-	-	-	-	-	-	0.500	0.312	0.462	0.629	0.094	0.138
Effect in pp – year 5	-	-	-	-	-	-	-0.3	-0.4	-0.4*	-	-	-
Marg. Index	-	-	-	-	-	-	-0.2744	-0.2034	-0.2416	-	-	-
p-value	-	-	-	-	-	-	0.261	0.174	0.086	-	-	-
Effect in pp – year 6+	-	-	-	-	-	-	0.3	-0.8*	-0.4	-	-	-
Marg. Index	-	-	-	-	-	-	0.1166	-0.3535	-0.1634	-	-	-
p-value	-	-	-	-	-	-	0.553	0.071	0.137	-	-	-
N	27,998	42,178	70,176	16,987	26,894	44,673	27,998	42,178	70,176	16,987	26,894	44,673

*DiD (probit) on inter-regional migration inflows - controlling for: individual characteristics (education, gender and citizenship), time dummies and regional fixed-effect. For each year of treatment we report the marginal probability effect in pp (1st row) and marginal index effect (2nd row) of the treatment.). The dependent variable is equal to one if the person lived in another region one year before. Specification either with a unique treatment dummy for all the period after the treatment (columns A) or a treatment dummy per each year after the treatment (columns B). Columns 1 end the analysis in the 1st quarter 2011, while columns 2 in the 3rd quarter 2008. The sample only contains the first waves from the quarterly LFS. P-value by Score bootstrap (2000 repetitions) with 17 regions as clusters. ***: significant at 1%, **: significant at 5%, *: significant at 10%*

B

Supplementary Appendix to "Permanent Wage Cost Subsidies for Older Workers. An Effective Tool for Increasing Working Time and Postponing Early Retirement?"

This is the Supplementary Appendix to the Chapter 2 "Permanent Wage Cost Subsidies for Older Workers. An Effective Tool for Increasing Working Time and Postponing Early Retirement?". The Appendix contains the following Tables and Figures:

(i) In Tables B.1-B.3 a detailed list of the employment subsidies targeted at older workers as implemented in a set of OECD countries (Austria, Denmark, Finland, France, Germany, the US, the Netherlands, Sweden and the United Kingdom):

   a. Table B.1: Employers' wage cost subsidies;

   b. Table B.2: Hiring subsidies and in-work benefits;

   c. Table B.3: Hiring subsidies and in-work benefits in Belgium;

(ii) Based on the control group aged 53.25-55.25, the main estimation results on the Average Treatment Effect on the Treated by quarter (δ_q) and the average over the five quarters ($\bar{\delta}$), the placebo tests, statistics on the estimated Propensity Score, the balancing tests and graphs on the common support for the overall population and for the high and low exit rate industrial committees:

   a. Table B.4: ATT's at the extensive margin (trend-adjusted);

   b. Table B.5: ATT's at the intensive margin (parallel path);

   c. Table B.6: ATT's on the Hourly Gross wage (parallel path);

   d. Figures B.1-B.4: Graphs on the common support.

(iii) Based on the control group aged 53.25-55.25, the main estimation results by Difference-in-Differences (DiD) estimator not controlling for the covariates. We report the main estimation results on the Average Treatment Effect on the Treated by quarter (δ_q) and the average over the five quarters ($\bar{\delta}$), the placebo tests for the high and low exit rate industrial committees.

   a. Table B.7Table B.4: ATT's at the extensive margin (trend-adjusted);

   b. Table B.8: ATT's at the intensive margin (parallel path);

   c. Table B.9: ATT's on the Hourly Gross wage (parallel path);

(iv) Based on a *panel* of 243,655 units on the period 1998q2-2003q2, the estimates on the ATT by the DiD estimator in panel data (OLS or fixed-effect). In particular, we include quarterly-age & time dummies, the interaction for individuals above 58 years old after the period 2002q2 (treatment effect) and another interaction for 2002q1 (anticipation effects). As placebo test, we add further age-time interactions for the period 2000q4, 2000q3, 200q2. In Table B.10 we report the estimates showing negative and significant ATTs and rejected placebo test.

(v) Based on the control group aged 57.75 and a treated group aged 58, the main estimation results on the average ATT over the five quarters by using the WDiD estimator (Table B.11).

Other estimation results are available from the authors upon request.

Table B.1: Wage costs subsidies targeted at older workers in OECD countries

Country	REDUCTION OF SOCIAL SECURITY CONTRIBUTIONS
Austria	In 2003, the *Aktion 56/58 plus* was introduced: employers' and employees' social security contributions to the unemployment insurance were abolished from the age of 56 for women and 58 for men. When the employee turns 60, there are other SSC exemptions (e.g. accident insurance contribution, Insolvency Contingency Fund, Family Burdens Equalization Fund). In 2008, the age threshold was set to 57 years for both genders and in 2008 it was increased to 58 years old. Overall, employers' SSC passed from 21.7% to 17.3%: 3% of exemption from unemployment insurance contribution from the age of 58 and exemption of 1.4% of accident insurance contribution from the age of 60 (OECD 2011). After other changes, the austerity measures ended the coverage to new employees from 2013.
Finland	In 2006, a temporary decrease of payroll taxes was in place until December 2010. Full-time workers (employed at least 140 hours per month) over 54 years old and earning a salary between 900 and 2,000€ per month were eligible. The subsidy created a payroll tax system at the beginning regressive and then progressive. The payroll tax rate before of the subsidy was 21% of the gross wage as proportional tax. The subsidy rate decreased until the worker's gross wage reached the threshold of 1,400€, when the payroll tax became 5.2% of the gross wage and the enterprises received 220€ (16% of the gross wage and 13% of labour costs). When the monthly wages overcome the 1,600€ threshold, the absolute amount of the subsidy gradually decreased and at 2,000€ it become zero (the payroll tax came back to 21%). Most of the eligible workers found themselves in the progressive part of the payroll tax since in 2006 the lowest full-time wage was about 1,300€ and the average was 2,500€. The take-up rate in the private sector has been low and in 2007 about 68% of the subsidised employees worked in the public sector (especially in municipalities) and 30% of the total were in health and social services. More than 80% of subsidised employees were women.
Netherlands	Since the 1st of January 2004, employers are exempted from paying the fixed part of the disability contribution (€ 712, EEO 2002) for any existing employee aged 55 years and over and for all new hires of long term unemployed (more than 6 months) aged 50 and over (OECD 2006).
Sweden	From 2010, employers do not have to pay SSC to the public disability system, parental-leave and work-injury insurance if the worker is over 65. Thus, only contributions to the public pension system remain to be paid and the employers only pay 10.2% of the gross wage instead of 31% (30% for self-employed). The exemption of SSC is 100% for workers born before of 1937.

OECD (2003a, 2004a, 2005a, 2005b, 2006, 2011, 2012a, 2012b, 2012c, 2012d), International Reform Monitor 2010, Anxo (2012), Lechner (2012), Arnkil (2012), Skugor and Bekker (2012), European Commission (2012).

Table B.2: In-work benefits and hiring subsidies targeted at older workers in OECD countries

Country	HIRING SUBSIDIES AND IN-WORK BENEFITS
Austria	From 1996 to 2009 the "*Bonus-Malus*" system (**hiring subsidies**) applied: if a company hired a person above 50 years old, the contribution to the unemployment insurance was halved (total exoneration for over 55); from 2000 onward a complete exoneration (amounting about 3.25% of the wage) for workers over 50 was applied. In 2003, the average bonus was 810€. At the same time, employers had to pay a "Malus" in case of firing a worker who was older than 50 years and had worked for the company for at least ten years. Each year from 1997-2003 about 20,000-25,000 workers were covered by the hiring subsidy (1/3 women), representing 5% of the total older workers in 2003. Qualitative evaluations claim for a high deadweight effect and low impact because of the administrative burden. From July 2008, "*Come back*" **hiring subsidies** have been offered to employers hiring unemployed over 50 years old (or 45 for women) or other disadvantaged categories such as long-term unemployed. Employers receive a benefit equal to 66.7% of the labour costs for a maximum period of 2 years (depending on the region). In 2010, the average duration was 3 months with a subsidy of 2,800€. In 2009, the **in-work benefits** '*New Wage Combination*' (*Kombilohn neu*) was introduced for people over 50. During the first year, the scheme grants 300€ per month for low wage FTE (gross wage under 1,500€) or 150€ for middle wage FTE (gross wage under 1,700€) and part-time workers (below 35 hours per week). **In-work benefits** "Return-to-Work Supplement" for over 50-year-olds and unemployed at least for one year: lump sum of 160€ per month (in 2003) for either full-time or part-time works, renewable every 12 months (OECD 2006:128). **In-work benefits**: the unemployed over 45 accepting a new job maintains his right to the higher unemployment benefits based on his former wage.
Denmark	**Hiring subsidies** (duration 6 months) for long-term unemployed (at least 1 year) older than 55 years.
Finland	In 2000, **hiring subsidies** targeting unemployed aged 55-59 were introduced, with duration of six months. Though there are general hiring subsidies for all the unemployed, older workers are overrepresented (in 2002 they were 23% of wage subsidies beneficiaries).

France	From 1995, employers hiring unemployed older than 50 years old or other disadvantaged categories have been eligible for the "*Contrat Initiative Emploi*" programme (**hiring subsidies)**. According to Adjerad (2004), in 2002, the over-50s accounted for 26% of the CIE contracts, with the over-50s counting for only 6% of the total number of hires in the private sector and about 15% of the jobseekers (Lerais and Marioni 2004). In 2005, this programme was reformed by the "Plan de cohésion sociale" (in CIE-PCS) and in 2010 it was transformed in the "*Contrat Unique d'Insertion*" (CUI-CIE). The -employee needs to work for at least 20 hours per week, with a permanent duration contract (CDI) or a temporary contract (CDD) of at least 12 months. The usual duration is 24 months or 5 years for beneficiaries of a social minimum or disabled people. The subsidy consists in a reduction of employers' SSC and it depends on the characteristics of the worker. In 2012, for job seekers over 50 it was 25% of the minimum wage or 35% if s/he had been registered for at least 12 months during the former 18 months (Fiche n° 5.02 - Contrat Initiative Emploi). The firm must not have laid-off workers in the preceding 6 months due to economic reasons, and is not allowed to make someone redundant in order to hire a subsidised worker. Finally, some regions integrate the hiring subsidy for older workers with 1,000/2,000€ subsidies targeting SMEs. The *"Contrat Emploi Solidariete'"* (for non-profit and public employers) and the "*Contrat employ consolide'"* were unified in the "*Contrat d'accompagnement dans l'emploi"* in 2005. In 2010, the retirement act introduced a hiring subsidy targeting unemployed over 55, with duration of one year. In 2011, the measures targeted unemployed aged at least 45 under professionalization contract (*contrat de professionnalisation*), and it counted 2,000€, granting further exemptions of SSC.
Germany	Older workers are a target group of the Act for Modern Services on the Labour Market (2003). Since 1998, integration ***hiring subsidies*** (*Eingliederungszuschüsse, EGZ* **-** SGB III (1998): §§ 217-224) have been available for employers hiring specific categories of individuals such as people adapting to the new jobs, with placement difficulties, and older workers of at least 50 years old unemployed for at least six months. Special benefits have been defined for the older workers and the subsidy corresponds to a maximum of 70% of the wage, with duration of at most 70 months, reducing the subsidy after the 24th month (the regular subsidy was defined as 50% of the standardised labour costs over 24 months). In 2002 (SGB III (Job-Aqtiv): § 218, § 201 f), the minimum unemployment period of 6 months was abolished. Finally, in 2004, the special benefits for the elderly have been partly taken over by other programs and the separate competence of the EGZ program was ended. Consequently, the share of subsidised older workers declined from 19.1% to 13.3% between 2003 and 2005 (Boockman *et al*. 2012). From 2003 (SGB III (Hartz I): § 201k***), a hiring subsidy*** for unemployed aged at least 55 years old has been implemented granting an exemption to unemployment insurance (3.25% of the labour costs). In 2003, the ***in-work benefits "****Entgeltsicherung für ältere Arbeitnehmer" (EGS)* was launched **(**SGB III (Hartz I): § 201j). The so-called *"Income Safeguarding Program"* targets older workers over 50 years old with a residual claim on unemployment benefits of at least 180 days (in 2007 it was reduced to 120 days). The wage integration is paid until the unemployment benefit entitlement expires. Two actions: in the first year the in-work benefit covers 50% of the "net difference" between the wage earned before and after the unemployment period (later it was set to be only 30% after the first year and the total duration of the subsidy was changed to 2 years). In addition, there is a top-up to the pension insurance contribution until 90% of the former earnings. The programme was supposed to phase out in December 2005 but it has been extended twice until today (Amendment Act). The integration voucher (*Eingliederungsgutschein*, EGG) for employers ***hiring*** people older than 50 years old was introduced in 2008. In order to be eligible for this subsidy, the job seeker needs to be entitled to unemployment benefits for more than 12 months and the new job has to be of at least 15 hours per week for one year. The integration is granted for 12 months and it counts 30-50% of the wage. Unlike the general subsidy, this measure does not require 1 year of employment after the termination of the subsidised period.
The US	***In-work benefits****:* "Alternative Trade Adjustment Assistance programme" (ATAA) starting from August 2003. Workers over 50 years old are eligible provided they can certify that they were displaced from their jobs for trade-related reasons. The eligible workers must obtain re-employment within 26 weeks from the lay-off and earn more than 50,000$ in a year in the new job. Older workers receive 50% of the difference between the new and the old salary. Benefits may be paid until the worker has received a total of 10,000$ of benefits for a maximum of two years. Besides, they can obtain a **Health Coverage Tax Credit (HCTC)** and relocation allowances. Subsidised workers cannot receive Trade Readjustment Allowances, training and job search allowances. In 2009, ATAA was changed in Reemployment Trade Adjustment Assistance (RTAA), which does not require a deadline for re-employment and the workers can participate in TAA training. The maximum annual earning was set to 55,000$ and the maximum supplement to 12,000$.
The Netherlands	From 2002, employers have been able to offer an additional tax-free bonus to older workers ("*Arbeidskorting voor ouderen").* The grant varies from an annual 227€ for workers aged 58 to 681€ for workers aged 63 (European Commission 2002). The measure should have been abolished in 2012 and replaced by *the Vitality Arrangement* (Vitaliteitsregeling). Since 2009, ***hiring subsidies*** for people older than 50 years old and receiving benefits or older than 55 years old have been introduced. The subsidies grant a reduction in the employers' SSC, with the first category receiving 3,500€ and the second one 7,000€. From January 2009, employers have received an annual tax

	deduction ("*Doorwerkbonus*") for each worker over 62 years old. Since 2012 the deduction has covered only the annual gross income exceeding 9,295€ (until a maximum of € 57,166€) and it depends on the worker's age. For workers of 62 years old the rate is 1.5% till a maximum of € 719; workers aged 63 benefit from a rate of 6% till a maximum of € 2,873; for workers of 64 years old the rate is of 8.5% till a maximum of € 4,070; workers of 65 years of age have a rate of 2% till a maximum of € 958; for workers aged 66 the rate is of 2% till a maximum of € 958; workers of 67+ years old benefit from a rate of 1% till a maximum of € 479. The measure has had slow implementation and workers had to wait until spring 2010 to receive the first deduction, after which it has been paid monthly. A budget of 300 million euros was reserved. The "Doorwerkbonus" should have been abolished in 2013 and replaced by the *"Werkbonus"* in the "*Vitality Arrangement"* (Vitaliteitsregeling), according to which older workers are entitled to receive a (low wage) work bonus of € 3,000 per year when they reach 62 years old if between 60 and 64 years old they have earned between 90% and 175% of the statutory minimum wage. The work bonus ends in January 2020. However, in October 2012 the Dutch government cancelled the introduction of the V*itality Arrangement.*
Sweden	Since November 2000, the "*Special Employment Subsidies"* programme has provided **hiring subsidies** to employers who hire people over 57 years old, unemployed for at least two years with at least 3 months of participation in the Activity Guarantee. The subsidy is paid for a maximum period of 24 months and covers up to 75% of the wage with a maximum of SEK 10,500 per month (half of the average salary of a FTE). In 2001, only about 1,900 people participated in the programme. From January 1998, wage allowances have been granted when long-term unemployed over the age of 60 are employed (***hiring subsidy,*** Agell *et al.* 2002). In 2007, the New Start Jobs program was launched. This program aims at reducing employers' payroll taxes for a period equal to the unemployment spell of the newly hired (***hiring subsidy).*** For people over 55, the subsidy period lasts as much as twice the period in unemployment for a maximum of 10 years or until they reach 65 years old. In 2009, almost 26% of the New Start jobs participants were over the age of 55. In July 2010, the minimum unemployment period was shortened from 12 to 6 months for workers over 55. ***In-work benefits****:* a higher Earned Income Tax Credit for workers over 65s is in place.
United Kingdom	***In-work benefits****:* in October 1999, the New Deal 50+ was launched: a volunteer programme for people over 50 years old who have been on benefits for six months and receiving benefits such as Income Support, Jobseeker's Allowance, Incapacity Benefit or Severe Disability Allowance. This program is divided in three parts: non-pecuniary benefits; training (upon employment); and financial in-work benefits. The latter grants 60£ per week for FTE (working at least 30 hours per week) for maximum one year and 40£ for part-time employees (16-29 hours a week) for a maximum of six months. In general, a New Deal Employer Agreement must be signed by the firms with the local labour offices. The firm must pay to the New Deal employees at least as much as the in-work benefit, must not dismiss a previous employee and retain the subsidised worker for at least six months. Between April 2000 and March 2003, there were almost 100,000 people who started to claim the Employment Credit. The majority of which were males (69%), 95% were between 50 and 59 years old (57% of the total was between 50-54), and 32 % were disabled (OECD 2004). The New Deal 50+ was reformed in April 2003: financial benefits fell under the umbrella of the Working Tax Credit (WTC, in-work benefits for low-income workers). Individuals over 50 years of age working more than 16 hours per week receive the "50 Plus element" (32£ per week) for 52 weeks. After the period, lower wage people working at least 30 hours receive the basic WTC entitlements. The New Deal 50+ scheme was replaced in many areas of the UK in October 2009 with the Flexible New Deal, targeted at long term unemployed (12 months). In April 2011, the subsidy was 26£ a week for people working at least 16 hours and 39£ for at least 30 hours. From April 2012, the 50+ part in the Working Tax Credit ceased. However, special features of the low-income WTC remain for the over 60s. ***In-work benefits***: workers over 65 years old are exempted from employees' SSC (OECD 2011). *Further schemes:* In 1990, 50 plus Job Start Allowance Scheme paid 20£ to integrate the salary for 6 months. This was abandoned in 1991 due to the low take-up level. Job Release Scheme: for inactive older people, abolished in early 1980s.

OECD (2003, 2004a, 2004b, 2005a, 2005b, 2005b, 2005c, 2005d, 2006, 2011, 2012a, 2012a, 2012b, 2012c, 2012d, 2012e, 2012f, 2012g), Plasman (2012), Anxo (2012) Arnkil (2012), Skugor and Bekker (2012), Duell and Vogler-Ludwig (2012), Gineste (2012), Madsen (2012) Lechner (2012), European Commission (2002, 2012), Dietz et al. (2011), ARFTLV (2013), Fiscaalleven (2012), Grierson (2002), Agell et al. (2002).

Table B.3: In-work benefits and hiring subsidies targeted at older workers in Belgium

Hiring subsidy	Since 1996, employers who hire job seekers aged 50 or over and unemployed for at least six months can claim a 50% reduction of their SSC for the first year, and thereafter employers can get a 25% reduction for an unlimited period (OECD 2006); The subsidy was replaced by Activa in 2002.
Hiring subsidy	In January 2002, the *Activa plan* introduced a more generous scheme for people over 45 years old, registered as jobseekers. The *hiring subsidy* lasts 21 quarters and the amount depends on the length of the registration. If the person has been registered for at least 156 days during the previous 9 months, the SSC is reduced by 1,000**€** for the first 5 quarters and then by **400€.** The reduction is 1,000**€ with** 312 days of registration during the previous 18 months. If the registration is for 468 days during the previous 27 months and the worker was receiving full unemployment benefits when hired, the Office National de l'Emploi (ONEm) pays an additional hiring subsidy of 500€ for FTE covering part of the net salary for 30 months (part-time workers receive a proportional amount). Furthermore, this "*allocation de travail*" is granted to all the registered unemployed over 45 with reduced capacity to work. Importantly, since these aids enter in the "long-term jobseekers" category, the SSC reduction cannot be cumulated with the SSC reduction for workers over 57 years old.
Hiring subsidy	Since January 2010 until December 2011, the "win-win" plan introduced an additional benefit for firms hiring unemployed over 50, registered as jobseeker for at least 6 months. The *hiring subsidy* amounted 1,000€ per month for 24 months (12 months for people hired in 2011).
Hiring subsidy	Since April 2006, the Flemish government implemented *hiring subsidies* for unemployed over 50 with a permanent contract. The quarterly premium is either € 1,200 (for workers with a quarter gross salary less than 4,200€), 2,100€ (for workers with a quarter gross salary less than 6,000€ but higher than 4,200€), 3,000€ (for workers with a quarter gross salary less than 10,500€ but higher than 6,000) or 4,500€ (for workers with a quarter gross salary more than 10,500€). The premium expires after 1 year (workers have to stay in the firm for at least five quarters). Since October 2010, it cannot be combined with the hiring subsidy at the federal level.
In-work Benefit	Since June 2002, the Federal government introduced the "*Complément de reprise du travail" or "Werkhervattingstoeslag"* (*in-work benefits*) for older individuals (50+) returning to work (FTE or part-time) and unemployed for at least 13 months. Since 2006, it covers also self-employed workers and it also requires 20 years of professional career. New workers receive a benefit of 159.18€ (in 2002) for an unlimited period paid by the National Employment Office (NEO), renewed every 12 months as long as the worker is employed in the firm. Since May 2009, in case the requirements was not met the bonus lasts for three years with a decreasing rate, from 182.85 euros during the first 12 months (€ 197.93 in 2012); 121.9 euros from the 13th to the 24th month (131.95 in 2012), 60.95 euros from the 25th to the 36th month after the employee is back at work (€ 65.98 in 2012). From the 1st of February, the age for the eligibility was raised to 55 years old (arrêté royal 22 janvier 2013). Though the measure was not very successful initially (e.g. in 2004 it only covered 722 individuals), later it became more popular (in 2011 it covered 17,386 individuals).
In-work Benefit	Since May 2010, *in-work benefits* are paid to workers over 50 who change job from a heavy one (carried out for at least 5 years) to a lighter one with the same employer and losing at least 265.3€ of monthly gross income. The allowance amounts to 80€ for 12 months for workers under 55 years old, 106€ for 24 months for workers aged 55 to 58, and 133€ for 36 months for workers aged over 58.

OECD (2003b, 2006, 2011, 2012h)

Table B.4: Impact on the Extensive Margin: control group 53.25-55.25 years old

	Employment rate					
	Overall (1)		High exit rate (2)		Low exit rate (3)	
	Men	Women	Men	Women	Men	Women
ATT1 in pp - δ_1	0.36066	0.11085	1.49252*	1.62051	1.16777	-0.12034
95% CI	[-0.099; 0.820]	[-0.189; 0.411]	[-0.137; 3.122]	[-1.158; 4.399]	[-0.701; 3.036]	[-2.682; 2.441]
SE	(0.23435)	(0.15290)	(0.83153)	(1.41761)	(0.95334)	(1.30682)
Pvalue	(0.12382)	(0.46844)	(0.07267)	(0.25299)	(0.22060)	(0.92663)
ATT2 in pp - δ_2	0.20437	0.20774	1.72102*	1.92705	-0.06469	-0.48203
95% CI	[-0.413; 0.821]	[-0.203; 0.619]	[-0.264; 3.706]	[-1.482; 5.336]	[-2.524; 2.395]	[-3.761; 2.797]
SE	(0.31474)	(0.20971)	(1.01265)	(1.73938)	(1.25495)	(1.67282)
Pvalue	(0.51612)	(0.32189)	(0.08922)	(0.26791)	(0.95889)	(0.77323)
ATT3 in pp - δ_3	0.35860	0.37703	3.05275**	2.98827	-0.31187	-1.59366
95% CI	[-0.376; 1.094]	[-0.113; 0.867]	[0.565; 5.541]	[-1.174; 7.150]	[-3.321; 2.697]	[-5.501; 2.314]
SE	(0.37495)	(0.24995)	(1.26940)	(2.12348)	(1.53526)	(1.99376)
Pvalue	(0.33888)	(0.13145)	(0.01618)	(0.15935)	(0.83903)	(0.42410)
ATT4 in pp - δ_4	0.38779	0.45033	2.68020**	3.95888	-1.23159	-2.96390
95% CI	[-0.455; 1.231]	[-0.127; 1.028]	[0.013; 5.347]	[-0.986; 8.904]	[-4.674; 2.211]	[-7.491; 1.563]
SE	(0.43025)	(0.29448)	(1.36064)	(2.52284)	(1.75654)	(2.30955)
Pvalue	(0.36742)	(0.12621)	(0.04886)	(0.11660)	(0.48321)	(0.19938)
ATT5 in pp - δ_5	0.72871	0.36192	2.26593	4.84193*	-0.90332	-3.99096
95% CI	[-0.248; 1.705]	[-0.319; 1.043]	[-0.679; 5.211]	[-0.765; 10.449]	[-4.891; 3.084]	[-9.303; 1.321]
SE	(0.49817)	(0.34731)	(1.50244)	(2.86073)	(2.03443)	(2.71004)
Pvalue	(0.14353)	(0.29737)	(0.13151)	(0.09054)	(0.65703)	(0.14084)
Differential trend (pp/quarter) - μ	-0.14203**	-0.06960	-0.47958***	-0.69565**	-0.10380	0.39326
95% CI	[-0.254; -0.030]	[-0.154; 0.015]	[-0.819; -0.140]	[-1.377; -0.014]	[-0.534; 0.327]	[-0.219; 1.005]
SE	(0.05736)	(0.04307)	(0.17322)	(0.34770)	(0.21955)	(0.31224)
Pvalue	(0.01329)	(0.10611)	(0.00563)	(0.04542)	(0.63637)	(0.20786)
Anticip. in pp - δ_0	0.16715	0.16909	1.13464*	1.45862	0.64943	0.30472
95% CI	[-0.177; 0.511]	[-0.062; 0.400]	[-0.086; 2.356]	[-0.445; 3.362]	[-0.754; 2.052]	[-1.648; 2.258]
SE	(0.17553)	(0.11796)	(0.62290)	(0.97109)	(0.71579)	(0.99642)
Pvalue	(0.34099)	(0.15173)	(0.06852)	(0.13308)	(0.36425)	(0.75975)
Mean ATT in pp - $\bar{\delta}$	0.4080	0.3016	2.2425**	3.0673	-0.2687	-1.8302
95% CI	[-0.277; 1.093]	[-0.168; 0.771]	[0.020; 4.465]	[-0.920; 7.055]	[-3.058; 2.521]	[-5.539; 1.878]
Pvalue	(0.2427)	(0.2079)	(0.0480)	(0.1316)	(0.8502)	(0.3334)
Mean ATT in %	2.3296	3.9634	4.0855	5.1779	-0.3505	-2.3348
N° of individuals	152015	88835	77269	33538	43599	30178
N° of observations	758565	444838	349438	147600	189368	136450

	Placebo: Employment rate					
	Overall (1)		High exit rate (2)		Low exit rate (3)	
	Men	Women	Men	Women	Men	Women
Placebo in pp	-0.45027***	-0.26360**	-1.66211***	-2.01237**	-0.45692	0.95032
95% CI	[-0.099; 0.820]	[-0.189; 0.411]	[-0.137; 3.122]	[-1.158; 4.399]	[-0.701; 3.036]	[-2.682; 2.441]
SE	(0.14505)	(0.12286)	(0.50312)	(0.79439)	(0.56844)	(0.79565)
Pvalue	(0.00191)	(0.03191)	(0.00095)	(0.01130)	(0.42150)	(0.23232)
N° of individuals	109261	62214	54803	22709	31402	20919
N° of observations	363725	210082	170834	70905	95121	66604

*WDiD on parallel growths: impact on the employment rate by quarter and Mean effect. Control group is 53.25-55.25 years old; treated group 58-59.5 years old. Effects on specific subpopulation (defined in time q) by column: (1) Overall treated group, (2) Workers in sectoral industrial committees with an exit rate from employment above the median (= 18%), (3) Workers in sectoral industrial committees with an exit rate below the median. The placebo tests are performed by dividing the pre-treatment period in two parts. N° of observations is the sum of the number of individuals observed in each quarter of the analysis. Standard errors are obtained by a stratified bootstrap (clustering by individual) with 200 repetitions and 95% confidence intervals (CI) by assuming normality.***: significant at 1%,**: significant at 5%,*: significant at 10%.*

Table B.5: Impact on the Hours Worked (Intensive Margin): control group aged 53.25-55.25

	Intensive margin: 0%-30%					
	Overall (1)		High exit rate (2)		Low exit rate (3)	
	Men	Women	Men	Women	Men	Women
ATT1 in pp - δ_1	-0.49629**	-0.60434	-0.36989	-1.28983*	-0.13551	0.00694
95% CI	[-0.942; -0.050]	[-1.447; 0.238]	[-1.095; 0.356]	[-2.583; 0.003]	[-0.577; 0.306]	[-0.883; 0.897]
SE	(0.22747)	(0.42983)	(0.37015)	(0.65981)	(0.22527)	(0.45403)
Pvalue	(0.02913)	(0.15973)	(0.31766)	(0.05060)	(0.54749)	(0.98780)
ATT2 in pp - δ_2	-0.57678***	-0.00189	-0.82004**	0.52514	-0.50339**	-0.55847
95% CI	[-1.003; -0.151]	[-0.820; 0.816]	[-1.576; -0.064]	[-0.907; 1.957]	[-0.997; -0.010]	[-1.581; 0.464]
SE	(0.21722)	(0.41728)	(0.38587)	(0.73077)	(0.25193)	(0.52177)
Pvalue	(0.00792)	(0.99639)	(0.03357)	(0.47238)	(0.04570)	(0.28447)
ATT3 in pp - δ_3	-0.45170*	-0.48984	-0.70024*	-1.11224	-0.29102	-0.04717
95% CI	[-0.903; 0.000]	[-1.362; 0.383]	[-1.438; 0.038]	[-2.587; 0.362]	[-0.884; 0.302]	[-1.012; 0.917]
SE	(0.23046)	(0.44517)	(0.37645)	(0.75224)	(0.30268)	(0.49211)
Pvalue	(0.05000)	(0.27118)	(0.06287)	(0.13926)	(0.33631)	(0.92364)
ATT4 in pp - δ_4	-0.54088**	-0.43421	-1.10158***	-0.97821	-0.35161	0.15574
95% CI	[-1.019; -0.063]	[-1.376; 0.508]	[-1.893; -0.311]	[-2.562; 0.606]	[-1.02; 0.317]	[-0.860; 1.172]
SE	(0.24381)	(0.48053)	(0.40354)	(0.80825)	(0.34099)	(0.51848)
Pvalue	(0.02653)	(0.36620)	(0.00634)	(0.22618)	(0.30247)	(0.76389)
ATT5 in pp - δ_5	-0.74988***	-1.14855***	-1.35976***	-1.63637**	-0.30535	-0.64491
95% CI	[-1.246; -0.254]	[-2.021; -0.276]	[-2.067; -0.653]	[-3.099; -0.174]	[-1.048; 0.437]	[-1.631; 0.341]
SE	(0.25317)	(0.44538)	(0.36085)	(0.74609)	(0.37875)	(0.50307)
Pvalue	(0.00306)	(0.00991)	(0.00016)	(0.02829)	(0.42013)	(0.19986)
Anticip. in pp - δ_0	0.24817	-0.03369	-0.05490	-0.31193	0.17455	0.23201
95% CI	[-0.185; 0.681]	[-0.765; 0.698]	[-0.766; 0.657]	[-1.532; 0.908]	[-0.235; 0.584]	[-0.614; 1.078]
SE	(0.22107)	(0.37330)	(0.36298)	(0.62255)	(0.20894)	(0.43170)
Pvalue	(0.26161)	(0.92809)	(0.87978)	(0.61634)	(0.40348)	(0.59096)
Mean ATT in pp- $\bar{\delta}$	-0.5631***	-0.5358	-0.8703***	-0.8983	-0.3174	-0.2176
95% CI	[-0.885; -0.242]	[-1.231; 0.160]	[-1.384, -0.356]	[-2.029, 0.232]	[-0.736, 0.101]	[-0.942, 0.507]
Pvalue	(0.0006)	(0.1310)	(0.0009)	(0.1193)	(0.1373)	(0.5563)
Mean ATT in %	-13.3712	-3.9472	-13.6994	-4.9350	-14.8441	-2.2518
N° of individuals	98899	53299	63620	28550	37911	25964
N° of observations	433373	224284	274285	114298	158982	109896

	Intensive margin: 31%-80%					
	Overall (1)		High exit rate (2)		Low exit rate (3)	
	Men	Women	Men	Women	Men	Women
ATT1 in pp - δ_1	0.32658	-0.95315	0.30978	-0.02265	0.01692	-1.93632**
95% CI	[-0.942; -0.050]	[-1.447; 0.238]	[-0.896; 1.515]	[-1.857; 1.812]	[-1.129; 1.163]	[-3.517; -0.356]
SE	(0.40899)	(0.58082)	(0.61507)	(0.93616)	(0.58455)	(0.80651)
Pvalue	(0.42457)	(0.10079)	(0.61451)	(0.98070)	(0.97691)	(0.01636)
ATT2 in pp - δ_2	0.21798	-1.63102***	0.54219	-1.42660	-0.17266	-2.02884**
95% CI	[-1.003; -0.151]	[-0.82; 0.816]	[-0.765; 1.849]	[-3.321; 0.468]	[-1.366; 1.021]	[-3.646; -0.412]
SE	(0.39547)	(0.62296)	(0.66673)	(0.96642)	(0.60891)	(0.82486)
Pvalue	(0.58151)	(0.00884)	(0.41610)	(0.13990)	(0.77676)	(0.01391)
ATT3 in pp - δ_3	-0.12416	-0.95655	-0.88710	-0.38508	0.26373	-1.41999*
95% CI	[-0.903; 0.000]	[-1.362; 0.383]	[-2.185; 0.411]	[-2.271; 1.501]	[-1.035; 1.562]	[-2.948; 0.108]
SE	(0.47723)	(0.66034)	(0.66234)	(0.96234)	(0.66260)	(0.77979)
Pvalue	(0.79473)	(0.14746)	(0.18046)	(0.68905)	(0.69062)	(0.06861)
ATT4 in pp - δ_4	-0.16898	-1.45640**	-1.67369**	-1.05156	0.80590	-1.79616**
95% CI	[-1.019; -0.063]	[-1.376; 0.508]	[-3.127; -0.220]	[-3.313; 1.210]	[-0.601; 2.213]	[-3.537; -0.056]
SE	(0.46572)	(0.67452)	(0.74152)	(1.15379)	(0.71783)	(0.88806)
Pvalue	(0.71673)	(0.03084)	(0.02400)	(0.36209)	(0.26157)	(0.04312)
ATT5 in pp - δ_5	-0.25098	-0.92970	-1.28634*	-0.56868	0.50888	-1.08711
95% CI	[-1.246; -0.254]	[-2.021; -0.276]	[-2.713; 0.141]	[-2.844; 1.706]	[-0.882; 1.900]	[-2.894; 0.720]
SE	(0.48527)	(0.67072)	(0.72808)	(1.16072)	(0.70980)	(0.92188)
Pvalue	(0.60501)	(0.16571)	(0.07727)	(0.62418)	(0.47341)	(0.23831)
Anticip. in pp - δ_0	-0.09427	-1.51795***	-1.02139	-1.23993	1.17989**	-1.86040***
95% CI	[-0.185; 0.681]	[-0.765; 0.698]	[-2.323; 0.281]	[-2.958; 0.478]	[0.128; 2.232]	[-3.209; -0.512]
SE	(0.39490)	(0.52874)	(0.66429)	(0.87639)	(0.53665)	(0.68786)
Pvalue	(0.81133)	(0.00409)	(0.12416)	(0.15712)	(0.02791)	(0.00684)
Mean ATT in pp- $\bar{\delta}$	0.0001	-1.1854**	-0.5990	-0.6909	0.2846	-1.6537***
95% CI	[-0.652; 0.652]	[-2.141; -0.230]	[-1.607, 0.409]	[-2.284, 0.902]	[-0.727, 1.296]	[-2.906, -0.401]
Pvalue	(0.9998)	(0.0151)	(0.2442)	(0.3954)	(0.5814)	(0.0097)
Mean ATT in %	0.0005	-2.5536	-3.0739	-1.4420	2.0308	-3.6600
N° of individuals	98899	53299	63620	28550	37911	25964
N° of observations	433373	224284	274285	114298	158982	109896

	Intensive margin: 81%-100%					
	Overall (1)		High exit rate (2)		Low exit rate (3)	
	Men	Women	Men	Women	Men	Women
ATT1 in pp - δ_1	0.16971	1.55749***	0.06011	1.31247*	0.11859	1.92938***
95% CI	[-0.623; 0.963]	[0.572; 2.543]	[-1.168; 1.288]	[-0.174; 2.799]	[-1.061; 1.298]	[0.564; 3.294]
SE	(0.40456)	(0.50291)	(0.62649)	(0.75827)	(0.60174)	(0.69641)
Pvalue	(0.67486)	(0.00196)	(0.92357)	(0.08347)	(0.84377)	(0.00560)
ATT2 in pp - δ_2	0.35880	1.63291***	0.27784	0.90146	0.67605	2.58731***
95% CI	[-0.477; 1.195]	[0.510; 2.755]	[-0.855; 1.410]	[-0.74; 2.543]	[-0.533; 1.885]	[1.191; 3.983]
SE	(0.42667)	(0.57274)	(0.57776)	(0.83746)	(0.61688)	(0.71230)
Pvalue	(0.40038)	(0.00436)	(0.63059)	(0.28174)	(0.27311)	(0.00028)
ATT3 in pp - δ_3	0.57586	1.44639**	1.58734**	1.49733*	0.02729	1.46715**
95% CI	[-0.348; 1.500]	[0.328; 2.565]	[0.324; 2.850]	[-0.248; 3.242]	[-1.319; 1.373]	[0.219; 2.715]
SE	(0.47131)	(0.57064)	(0.64446)	(0.89032)	(0.68669)	(0.63683)
Pvalue	(0.22177)	(0.01125)	(0.01378)	(0.09261)	(0.96830)	(0.02123)
ATT4 in pp - δ_4	0.70986	1.89061***	2.77527***	2.02977**	-0.45428	1.64042**
95% CI	[-0.215; 1.635]	[0.682; 3.099]	[1.388; 4.162]	[0.184; 3.876]	[-1.932; 1.023]	[0.048; 3.233]
SE	(0.47207)	(0.61649)	(0.70758)	(0.94189)	(0.75381)	(0.81231)
Pvalue	(0.13266)	(0.00216)	(0.00009)	(0.03116)	(0.54674)	(0.04344)
ATT5 in pp - δ_5	1.00087**	2.07824***	2.64610***	2.20505**	-0.20353	1.73202**
95% CI	[0.022; 1.979]	[0.913; 3.243]	[1.237; 4.055]	[0.335; 4.075]	[-1.714; 1.306]	[0.185; 3.279]
SE	(0.49920)	(0.59446)	(0.71898)	(0.95427)	(0.77041)	(0.78941)
Pvalue	(0.04497)	(0.00047)	(0.00023)	(0.02085)	(0.79164)	(0.02823)
Anticip. in pp - δ_0	-0.15390	1.55164***	1.07629*	1.55185**	-1.35444**	1.62839***
95% CI	[-1.018; 0.71]	[0.695; 2.408]	[-0.194; 2.347]	[0.161; 2.942]	[-2.412; -0.296]	[0.428; 2.828]
SE	(0.44085)	(0.43702)	(0.64836)	(0.70941)	(0.53979)	(0.61231)
Pvalue	(0.72702)	(0.00038)	(0.09691)	(0.02870)	(0.01210)	(0.00783)
Mean ATT in pp- $\bar{\delta}$	0.5630	1.7211***	1.4693***	1.5892**	0.0328	1.8713***
95% CI	[-0.113; 1.239]	[0.828; 2.614]	[0.532, 2.406]	[0.198, 2.980]	[-1.015, 1.081]	[0.756, 2.986]
Pvalue	(0.1025)	(0.0002)	(0.0021)	(0.0251)	(0.9511)	(0.0010)
Mean ATT in %	0.7117	4.3021	1.9813	4.6901	0.0391	4.1440
N° of individuals	98899	53299	63620	28550	37911	25964
N° of observations	433373	224284	274285	114298	158982	109896

-

	Intensive margin: approximated working time (100% = full time)					
	Overall (1)		High exit rate (2)		Low exit rate (3)	
	Men	Women	Men	Women	Men	Women
ATT1 in pp - δ_1	0.30022	0.96805***	0.21395	1.38300***	0.13867	0.76690**
95% CI	[-0.191; 0.792]	[0.365; 1.571]	[-0.495; 0.923]	[0.491; 2.275]	[-0.406; 0.683]	[0.01; 1.524]
SE	(0.25083)	(0.30745)	(0.36161)	(0.45501)	(0.27782)	(0.38605)
Pvalue	(0.23134)	(0.00164)	(0.55409)	(0.00237)	(0.61768)	(0.04698)
ATT2 in pp - δ_2	0.40177*	0.61697*	0.39173	0.25010	0.58788*	1.12988***
95% CI	[-0.052; 0.856]	[-0.029; 1.263]	[-0.265; 1.049]	[-0.736; 1.236]	[-0.064; 1.239]	[0.317; 1.943]
SE	(0.23166)	(0.32961)	(0.33519)	(0.50312)	(0.33237)	(0.41473)
Pvalue	(0.08286)	(0.06123)	(0.24254)	(0.61912)	(0.07693)	(0.00644)
ATT3 in pp - δ_3	0.35861	0.92501***	1.04765***	1.21775**	0.00007	0.73637*
95% CI	[-0.127; 0.844]	[0.297; 1.553]	[0.337; 1.759]	[0.159; 2.277]	[-0.601; 0.601]	[-0.061; 1.533]
SE	(0.24777)	(0.32053)	(0.36272)	(0.54044)	(0.30679)	(0.40665)
Pvalue	(0.14779)	(0.00390)	(0.00387)	(0.02424)	(0.99982)	(0.07017)
ATT4 in pp - δ_4	0.33944	1.14861***	1.52783***	1.57675***	-0.28620	0.73098
95% CI	[-0.217; 0.896]	[0.422; 1.875]	[0.768; 2.287]	[0.507; 2.647]	[-1.009; 0.437]	[-0.142; 1.604]
SE	(0.28409)	(0.37064)	(0.38743)	(0.54588)	(0.36884)	(0.44561)
Pvalue	(0.23215)	(0.00194)	(0.00008)	(0.00387)	(0.43779)	(0.10093)
ATT5 in pp - δ_5	0.68810**	1.44119***	1.65053***	1.68410***	0.03625	1.16712**
95% CI	[0.115; 1.261]	[0.753; 2.129]	[0.865; 2.436]	[0.634; 2.734]	[-0.724; 0.796]	[0.229; 2.105]
SE	(0.29227)	(0.35089)	(0.40071)	(0.53587)	(0.38783)	(0.47853)
Pvalue	(0.01856)	(0.00004)	(0.00004)	(0.00167)	(0.92554)	(0.01473)
Anticip. in pp - δ_0	-0.23253	0.75553***	0.31354	0.83377**	-0.56523**	0.74159**
95% CI	[-0.652; 0.187]	[0.248; 1.263]	[-0.344; 0.971]	[0.04; 1.628]	[-1.106; -0.025]	[0.018; 1.465]
SE	(0.21385)	(0.25901)	(0.33533)	(0.40510)	(0.27566)	(0.36916)
Pvalue	(0.27688)	(0.00353)	(0.34978)	(0.03957)	(0.04032)	(0.04455)
Mean ATT in pp- $\bar{\delta}$	0.4176**	1.0200***	0.9663***	1.2223***	0.0953	0.9062***
95% CI	[0.015; 0.820]	[0.473; 1.567]	[0.437, 1.495]	[0.412, 2.033]	[-0.415, 0.606]	[0.227, 1.585]
Pvalue	(0.0421)	(0.0003)	(0.0003)	(0.0031)	(0.7145)	(0.0089)
Mean ATT in %	0.4715	1.5364	1.1296	1.9836	0.1042	1.2880
N° of individuals	98899	53299	63620	28550	37911	25964
N° of observations	433373	224284	274285	114298	158982	109896

-

	Placebo: Intensive margin: 0%-30%					
	Overall (1)		High exit rate (2)		Low exit rate (3)	
	Men	Women	Men	Women	Men	Women
Placebo in pp	-0.18960	0.02970	-0.46660	0.77924	-0.23137	-0.34552
95% CI	[-0.522; 0.143]	[-0.665; 0.725]	[-1.089; 0.156]	[-0.329; 1.887]	[-0.618; 0.155]	[-1.131; 0.440]
SE	(0.16971)	(0.35469)	(0.31755)	(0.56527)	(0.19724)	(0.40063)
Pvalue	(0.26392)	(0.93327)	(0.14174)	(0.16804)	(0.24077)	(0.38844)
N° of individuals	71175	35547	45165	18536	27399	17612
N° of observations	218563	98101	137037	49499	81486	48596

-

	Placebo: Intensive margin: 31%-80%					
	Overall (1)		High exit rate (2)		Low exit rate (3)	
	Men	Women	Men	Women	Men	Women
Placebo in pp	0.53276	-0.27800	0.40306	-0.56617	0.82708*	-0.04231
95% CI	[-0.174; 1.240]	[-1.281; 0.725]	[-0.522; 1.328]	[-2.138; 1.006]	[-0.101; 1.755]	[-1.263; 1.178]
SE	(0.36077)	(0.51159)	(0.47205)	(0.80203)	(0.47338)	(0.62261)
Pvalue	(0.13975)	(0.58685)	(0.39319)	(0.48024)	(0.08061)	(0.94582)
N° of individuals	71175	35547	45165	18536	27399	17612
N° of observations	218563	98101	137037	49499	81486	48596

-

	Placebo: Intensive margin: 81%-100%					
	Overall (1)		High exit rate (2)		Low exit rate (3)	
	Men	Women	Men	Women	Men	Women
Placebo in pp	-0.34317	0.24830	0.06354	-0.21308	-0.59571	0.38783
95% CI	[-0.986; 0.300]	[-0.584; 1.080]	[-0.955; 1.082]	[-1.578; 1.152]	[-1.526; 0.335]	[-0.747; 1.522]
SE	(0.32791)	(0.42456)	(0.51947)	(0.69631)	(0.47486)	(0.57882)
Pvalue	(0.29532)	(0.55865)	(0.90264)	(0.75960)	(0.20966)	(0.50284)
N° of individuals	71175	35547	45165	18536	27399	17612
N° of observations	218563	98101	137037	49499	81486	48596
	Placebo: Intensive margin: approximated working time					
	Overall (1)		High exit rate (2)		Low exit rate (3)	
	Men	Women	Men	Women	Men	Women
Placebo in pp	-0.09180	0.14630	0.28900	-0.31166	-0.34804	0.39050
95% CI	[-0.492; 0.308]	[-0.338; 0.631]	[-0.280; 0.858]	[-1.035; 0.411]	[-0.858; 0.162]	[-0.259; 1.040]
SE	(0.20404)	(0.24718)	(0.29024)	(0.36893)	(0.26040)	(0.33126)
Pvalue	(0.65278)	(0.55393)	(0.31939)	(0.39823)	(0.18136)	(0.23846)
N° of individuals	71175	35547	45165	18536	27399	17612
N° of observations	218563	98101	137037	49499	81486	48596

*WDiD on parallel path: impact on the intensive margin by quarter and Mean effect. Intensive margin defined as number of workers with a certain working time with respect to the reference time (0-30%, 31-80%, >81%, approximated continuous working time with 100% equal to full-time). Control group is 53.25-55.25 years old; treated group 58-59.5 years old. Effects on specific subpopulation (defined in time q) by column: (1) Overall treated group, (2) Workers in sectoral industrial committees with an exit rate from employment above the median (= 18%), (3) Workers in sectoral industrial committees with an exit rate below the median. The placebo tests are performed by dividing the pre-treatment period in two parts. N° of observations is the sum of the number of individuals observed in each quarter. Standard errors by a stratified bootstrap (clustering by individual) with 200 repetitions and 95% confidence intervals (CI) by assuming normality. ***: significant at 1%, **: significant at 5%, *: significant at 10%.*

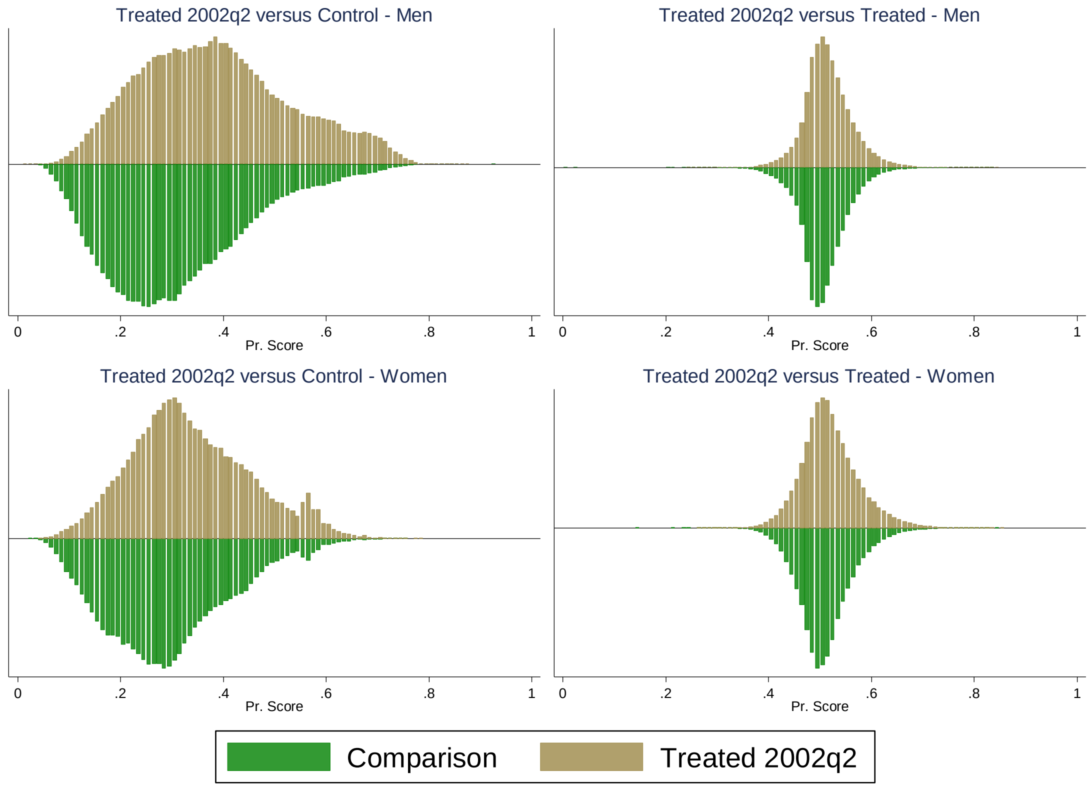
Table B.6: Impact on the Hourly Gross Wage: control group 53.25-55.25 years old

	Hourly Gross wage					
	Overall		High exit rate		Low exit rate	
	Men	Women	Men	Women	Men	Women
ATT1 in €- δ_1	-0.13224	0.01428	0.03705	0.01114	0.03359	0.01744
95% CI	[-0.457; 0.193]	[-0.077; 0.105]	[-0.279; 0.353]	[-0.129; 0.151]	[-0.347; 0.414]	[-0.113; 0.148]
SE	(0.16569)	(0.04637)	(0.16101)	(0.07158)	(0.19410)	(0.06661)
Pvalue	(0.42480)	(0.75804)	(0.81800)	(0.87636)	(0.86261)	(0.79341)
ATT2 in €- δ_2	-0.08658	0.00866	-0.02742	-0.02977	0.08108	0.05389
95% CI	[-0.369; 0.196]	[-0.085; 0.103]	[-0.269; 0.214]	[-0.162; 0.102]	[-0.313; 0.475]	[-0.075; 0.183]
SE	(0.14403)	(0.04793)	(0.12317)	(0.06743)	(0.20117)	(0.06588)
Pvalue	(0.54774)	(0.85668)	(0.82384)	(0.65881)	(0.68694)	(0.41334)
ATT3 in €- δ_3	0.12325	0.13680**	0.07110	0.10032	0.29678	0.15216**
95% CI	[-0.166; 0.412]	[0.027; 0.247]	[-0.14; 0.282]	[-0.047; 0.247]	[-0.106; 0.700]	[0.004; 0.301]
SE	(0.14747)	(0.05613)	(0.10781)	(0.07508)	(0.20548)	(0.07575)
Pvalue	(0.40330)	(0.01481)	(0.50954)	(0.18151)	(0.14865)	(0.04456)
ATT4 in €- δ_4	-0.09540	-0.00775	-0.17168	0.04424	0.09288	-0.06390
95% CI	[-0.435; 0.244]	[-0.118; 0.102]	[-0.387; 0.044]	[-0.119; 0.207]	[-0.382; 0.567]	[-0.209; 0.081]
SE	(0.17324)	(0.05606)	(0.11001)	(0.08309)	(0.24214)	(0.07380)
Pvalue	(0.58186)	(0.89008)	(0.11861)	(0.59444)	(0.70129)	(0.38661)
ATT5 in €- δ_5	-0.14395	-0.08497	-0.26114**	0.05381	0.05868	-0.19202***
95% CI	[-0.504; 0.216]	[-0.190; 0.020]	[-0.488; -0.034]	[-0.109; 0.216]	[-0.488; 0.605]	[-0.338; -0.046]
SE	(0.18363)	(0.05364)	(0.11579)	(0.08292)	(0.27869)	(0.07452)
Pvalue	(0.43311)	(0.11321)	(0.02412)	(0.51641)	(0.83323)	(0.00997)
Anticip. in € - δ_0	-0.11336	-0.03725	-0.07794	0.00716	-0.10672	-0.04474
95% CI	[-0.300; 0.073]	[-0.116; 0.042]	[-0.266; 0.110]	[-0.104; 0.118]	[-0.421; 0.207]	[-0.16; 0.070]
SE	(0.09498)	(0.04033)	(0.09570)	(0.05665)	(0.16015)	(0.05878)
Pvalue	(0.23267)	(0.35567)	(0.41542)	(0.89937)	(0.50518)	(0.44659)
Mean ATT in € - $\bar{\delta}$	-0.0670	0.0134	-0.0704	0.0359	0.1126	-0.0065
95% CI	[-0.320; 0.186]	[-0.069; 0.096]	[-0.255, 0.114]	[-0.082, 0.154]	[-0.255, 0.480]	[-0.121, 0.108]
Pvalue	0.6035	0.7502	0.4536	0.5519	0.5479	0.9120
Mean ATT in %	-0.3299	0.1000	-0.4157	0.2831	0.4793	-0.0463
N° of individuals	92630	53222	59644	28498	35287	25937
N° of observations	389986	223918	247176	114131	142709	109697

	Placebo: Hourly Gross wage					
	Overall		High exit rate		Low exit rate	
	Men	Women	Men	Women	Men	Women
Placebo in €	0.00174	0.02420	0.04064	-0.02303	-0.01154	0.05136
95% CI	[-0.160; 0.163]	[-0.060; 0.108]	[-0.193; 0.274]	[-0.133; 0.087]	[-0.268; 0.245]	[-0.059; 0.162]
SE	(0.08240)	(0.04273)	(0.11930)	(0.05590)	(0.13071)	(0.05652)
Pvalue	(0.98318)	(0.57111)	(0.73335)	(0.68035)	(0.92968)	(0.36351)
N° of individuals	64967	35534	41228	18526	24764	17603
N° of observations	175636	97984	110180	49461	65421	48517

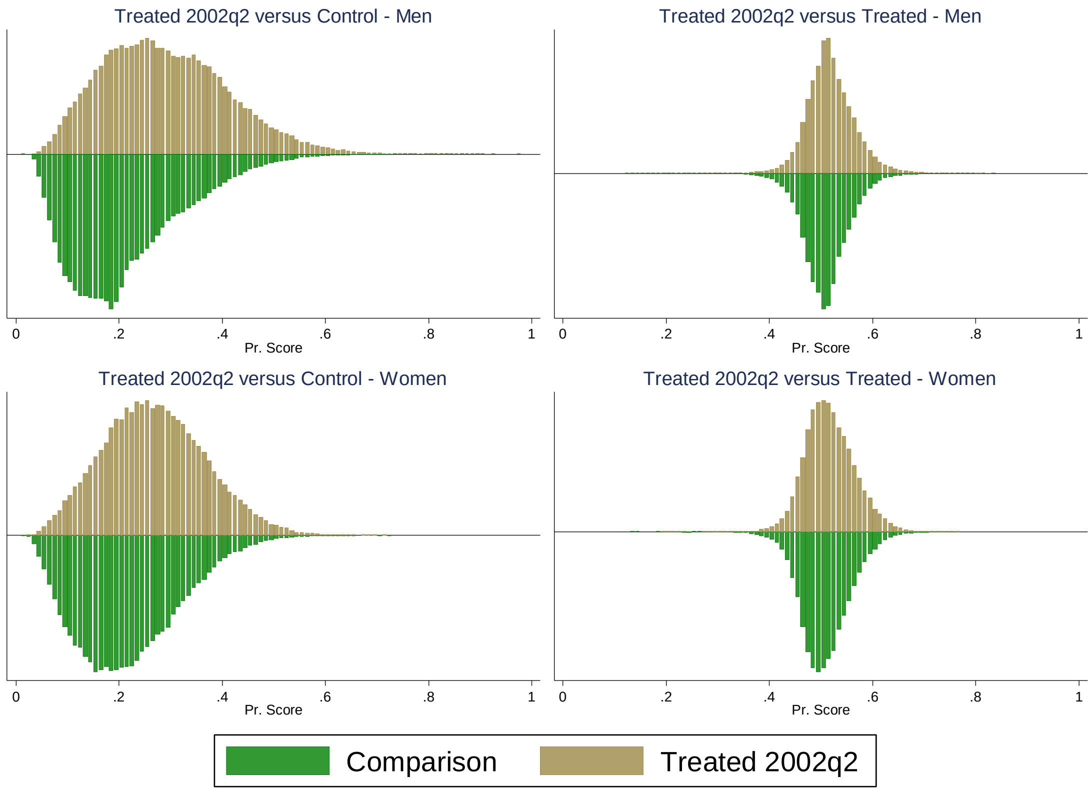
*WDiD on parallel path: impact on the hourly gross wage by quarter and Mean effect. Control group is 53.25-55.25 years old; treated group 58-59.5 years old. Effects on specific subpopulation (defined in time q) by column: (1) Overall treated group, (2) Workers in sectoral industrial committees with an exit rate from employment above the median (= 18%), (3) Workers in sectoral industrial committees with an exit rate below the median. The placebo tests are performed by dividing the pre-treatment period in two parts. N° of observations is the sum of the number of individuals observed in each quarter of the analysis. Standard errors are obtained by a stratified bootstrap (clustering by individual) with 200 repetitions and 95% confidence intervals by assuming normality.***: significant at 1%,**: significant at 5%,*: significant at 10%.*

Figure B.1: Common support - pooled cross sections - Employment rate overall population



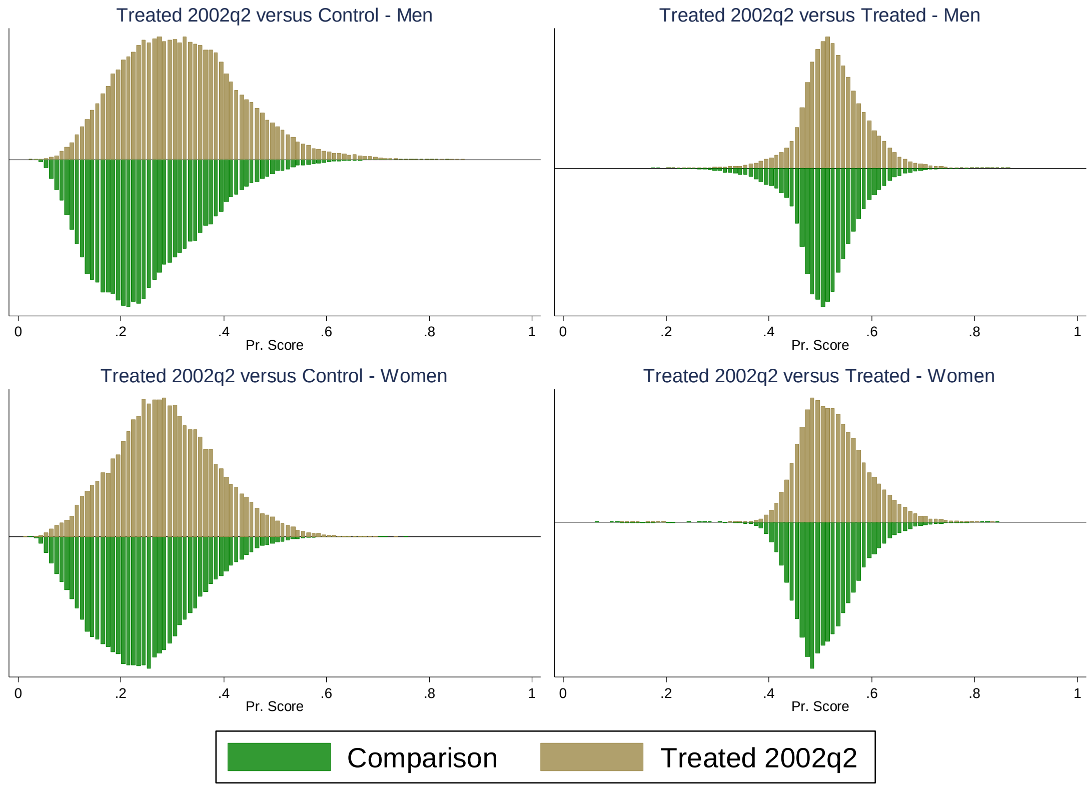
Distribution of the *Propensity Score (PS) by treatment status of the pooled cross-sections. Sample: overall population. The non-standard distribution is due to the use of the sampling weights in the estimation of the PS. The plotted frequencies are reweighted.*

Figure B.2: Common support - pooled cross sections - Intensive margin - overall population



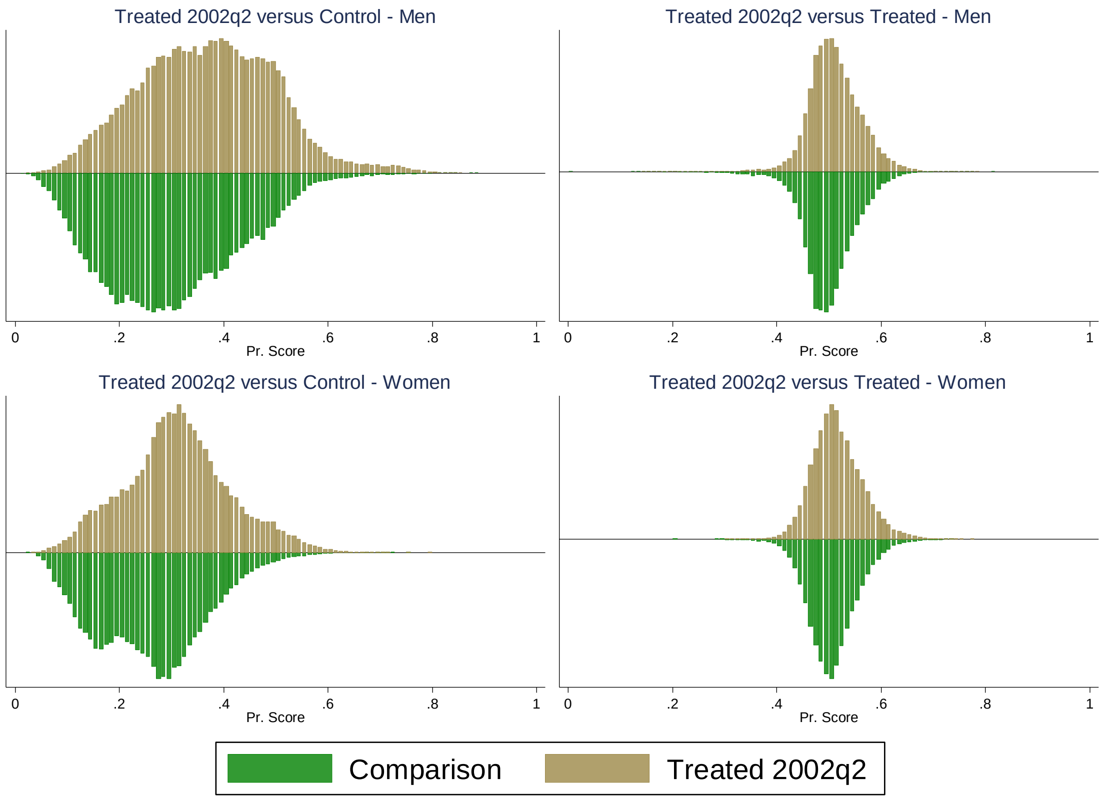
Distribution of the Propensity Score (PS) by treatment status of the pooled cross-sections. Sample: workers in the salaried private sector in t. The non-standard distribution is due to the use of the sampling weights in the estimation of the PS. The plotted frequencies are reweighted.

Figure B.3: Common support - pooled cross sections: Employment of High Exit Rate Committee



Sample: Workers in sectoral industrial committees with an exit rate from employment above the median (= 18%) seven quarters before t. The non-standard distribution is due to the use of the sampling weights in the estimation of the PS. The plotted frequencies are reweighted.

Figure B.4: Common support - pooled cross sections: Employment of Low Exit Rate Committee



Sample: Workers in sectoral industrial committees with an exit rate from employment below the median (= 18%) seven quarters before t. The non-standard distribution is due to the use of the sampling weights in the estimation of the PS. The plotted frequencies are reweighted.

Table B.7: DiD - Impact on the Employment Rate (Extensive Margin): control group 53.25-55.25

	Employment rate					
	Overall (1)		High exit rate (2)		Low exit rate (3)	
	Men	Women	Men	Women	Men	Women
ATT1 in pp - δ_1	0.09215	0.20251	3.75163***	1.63807	1.33247	-0.17880
95% CI	[-0.788; 0.973]	[-0.320; 0.725]	[1.992; 5.512]	[-1.544; 4.820]	[-0.523; 3.188]	[-3.172; 2.814]
SE	(0.44917)	(0.26663)	(0.89803)	(1.62357)	(0.94668)	(1.52697)
Pvalue	(0.83745)	(0.44756)	(0.00003)	(0.31302)	(0.15928)	(0.90678)
ATT2 in pp - δ_2	-0.10336	0.04077	3.79066***	0.84617	0.54571	0.44303
95% CI	[-1.241; 1.034]	[-0.634; 0.715]	[1.579; 6.002]	[-3.103; 4.795]	[-1.661; 2.752]	[-3.209; 4.095]
SE	(0.58023)	(0.34412)	(1.12838)	(2.01483)	(1.12580)	(1.86341)
Pvalue	(0.85862)	(0.90569)	(0.00078)	(0.67451)	(0.62787)	(0.81207)
ATT3 in pp - δ_3	-0.13745	0.23887	4.75147***	3.06971	1.35851	0.07890
95% CI	[-1.553; 1.278]	[-0.601; 1.079]	[2.061; 7.442]	[-1.785; 7.924]	[-1.408; 4.125]	[-4.530; 4.688]
SE	(0.72216)	(0.42852)	(1.37271)	(2.47688)	(1.41141)	(2.35157)
Pvalue	(0.84905)	(0.57724)	(0.00054)	(0.21523)	(0.33579)	(0.97324)
ATT4 in pp - δ_4	-0.24670	0.37597	5.65169***	4.14065	0.77390	-0.81934
95% CI	[-1.941; 1.447]	[-0.632; 1.384]	[2.450; 8.854]	[-1.640; 9.921]	[-2.444; 3.992]	[-6.103; 4.464]
SE	(0.86432)	(0.51441)	(1.63360)	(2.94935)	(1.64175)	(2.69571)
Pvalue	(0.77532)	(0.46486)	(0.00054)	(0.16035)	(0.63737)	(0.76117)
ATT5 in pp - δ_5	-0.50965	0.31823	6.36476***	4.57205	0.86308	-2.02207
95% CI	[-2.517; 1.498]	[-0.871; 1.508]	[2.716; 10.013]	[-2.072; 11.216]	[-2.806; 4.533]	[-8.250; 4.206]
SE	(1.02440)	(0.60684)	(1.86142)	(3.38981)	(1.87215)	(3.17737)
Pvalue	(0.61883)	(0.60000)	(0.00063)	(0.17742)	(0.64479)	(0.52452)
Differential trend (pp/quarter) - μ	0.0944291	-0.2049602	-0.9017422	-1.112278	-0.2480082	0.0713128
95% CI	[-0.143; 0.332]	[-0.346; -0.064]	[-1.346; -0.458]	[-1.943; -0.282]	[-0.668; 0.172]	[-0.683; 0.825]
SE	(0.12133)	(0.07171)	(0.22654)	(0.42365)	(0.21433)	(0.38468)
Pvalue	0.436	0.004	0.000	0.009	0.247	0.853
Anticip. in pp - δ_0	-0.29655	0.03827	2.55060***	1.49663	0.97691	0.49127
95% CI	[-0.904; 0.311]	[-0.331; 0.407]	[1.272; 3.829]	[-0.887; 3.880]	[-0.428; 2.382]	[-1.663; 2.646]
SE	(0.30991)	(0.18817)	(0.65239)	(1.21613)	(0.71696)	(1.09930)
Pvalue	(0.33861)	(0.83882)	(0.00009)	(0.21846)	(0.17303)	(0.65495)
Mean ATT in pp - $\bar{\delta}$	-0.18100	0.23527	4.86204***	2.85333	0.97473	-0.49966
95% CI	[-1.546; 1.184]	[-0.571; 1.041]	[2.281; 7.443]	[-1.787; 7.494]	[-1.605; 3.554]	[-4.798; 3.799]
Pvalue	(0.7949)	(0.5673)	(0.0002)	(0.2281)	(0.4589)	(0.8198)
Mean ATT in %	-0.9987	3.0613	9.4122	4.8407	1.2965	-0.6484
N° of individuals	152554	89224	77270	33538	43602	30178
N° of observations	761904	447398	349453	147600	189381	136450

	Placebo: Employment rate					
	Overall (1)		High exit rate (2)		Low exit rate (3)	
	Men	Women	Men	Women	Men	Women
Placebo in pp	0.36079	-0.52678***	-2.46067***	-3.11270***	-0.82217	-0.09091
95% CI	[-0.261; 0.983]	[-0.900; -0.154]	[-3.676; -1.245]	[-5.343; -0.882]	[-1.922; 0.277]	[-2.200; 2.019]
SE	(0.31732)	(0.19037)	(0.62002)	(1.13795)	(0.56103)	(1.07620)
Pvalue	(0.25555)	(0.00566)	(0.00007)	(0.00624)	(0.14281)	(0.93268)
N° of individuals	99199	57322	50511	21018	28549	19371
N° of observations	334327	195203	159440	66474	87573	62104

*DiD on parallel growths: impact on the employment rate by quarter and Mean effect. Control group is 53.25-55.25 years old; treated group 58-59.5 years old. Effects on specific subpopulation (defined in time q) by column: (1) Overall treated group, (2) Workers in sectoral industrial committees with an exit rate from employment above the median (= 18%), (3) Workers in sectoral industrial committees with an exit rate below the median. The placebo tests are performed by dividing the pre-treatment period in two parts. N° of observations is the sum of the number of individuals observed in each quarter of the analysis. Cluster Robust Standard errors by individual. ***: significant at 1%, **: significant at 5%, *: significant at 10%.*

Table B.8: DiD -Impact on the Hours Worked (Intensive Margin): control group aged 53.25-55.25

	Intensive margin: 0%-30%					
	Overall (1)		High exit rate (2)		Low exit rate (3)	
	Men	Women	Men	Women	Men	Women
ATT1 in pp - δ_1	-0.85009***	-0.20277	-1.21123***	-0.44780	-0.57016**	-0.07976
95% CI	[-1.328; -0.373]	[-1.155; 0.749]	[-2.046; -0.376]	[-2.017; 1.121]	[-1.004; -0.136]	[-1.209; 1.050]
SE	(0.24360)	(0.48563)	(0.42608)	(0.80061)	(0.22154)	(0.57616)
Pvalue	(0.00048)	(0.67629)	(0.00447)	(0.57595)	(0.01007)	(0.88989)
ATT2 in pp - δ_2	-0.58021**	0.31007	-0.88840**	0.99778	-0.45300*	-0.37004
95% CI	[-1.060; -0.101]	[-0.718; 1.339]	[-1.722; -0.055]	[-0.682; 2.678]	[-0.917; 0.011]	[-1.617; 0.877]
SE	(0.24467)	(0.52471)	(0.42518)	(0.85725)	(0.23688)	(0.63623)
Pvalue	(0.01772)	(0.55456)	(0.03667)	(0.24446)	(0.05584)	(0.56083)
ATT3 in pp - δ_3	-1.10359***	0.12301	-1.66551***	-0.23758	-0.63073**	0.22583
95% CI	[-1.541; -0.666]	[-0.958; 1.204]	[-2.377; -0.954]	[-1.981; 1.506]	[-1.138; -0.123]	[-1.103; 1.555]
SE	(0.22341)	(0.55150)	(0.36324)	(0.88936)	(0.25885)	(0.67804)
Pvalue	(0.00000)	(0.82350)	(0.00000)	(0.78937)	(0.01483)	(0.73908)
ATT4 in pp - δ_4	-1.40863***	0.43432	-2.03134***	-0.77577	-0.46237*	1.34615*
95% CI	[-1.884; -0.933]	[-0.700; 1.569]	[-2.814; -1.248]	[-2.605; 1.053]	[-0.959; 0.034]	[-0.048; 2.74]
SE	(0.24265)	(0.57888)	(0.39946)	(0.93318)	(0.25326)	(0.71113)
Pvalue	(0.00000)	(0.45309)	(0.00000)	(0.40580)	(0.06791)	(0.05837)
ATT5 in pp - δ_5	-1.35386***	0.12902	-2.32335***	-0.65826	-0.38819	0.51232
95% CI	[-1.816; -0.891]	[-1.034; 1.292]	[-3.056; -1.591]	[-2.504; 1.188]	[-0.938; 0.162]	[-0.940; 1.965]
SE	(0.23593)	(0.59329)	(0.37369)	(0.94186)	(0.28057)	(0.74095)
Pvalue	(0.00000)	(0.82784)	(0.00000)	(0.48463)	(0.16650)	(0.48930)
Anticip. in pp - δ_0	-0.47658*	0.30950	-0.77475**	-0.19640	-0.02811	0.71420
95% CI	[-0.973; 0.020]	[-0.831; 1.450]	[-1.467; -0.082]	[-1.619; 1.226]	[-0.731; 0.675]	[-1.032; 2.461]
SE	(0.25333)	(0.58211)	(0.35322)	(0.72564)	(0.35852)	(0.89104)
Pvalue	(0.05994)	(0.59494)	(0.02828)	(0.78666)	(0.93751)	(0.42283)
Mean ATT in pp- $\bar{\delta}$	-1.05928***	0.15873	-1.62397***	-0.22432	-0.50089***	0.32690
95% CI	[-1.395; -0.723]	[-0.746; 1.063]	[-2.181; -1.067]	[-1.667; 1.219]	[-0.869; -0.133]	[-0.795; 1.449]
Pvalue	(0.0000)	(0.7308)	(0.0000)	(0.7606)	(0.0076)	(0.5679)
Mean ATT in %	-23.0772	1.2397	-23.4666	-1.3096	-21.6354	3.5181
N° of individuals	98900	53300	63622	28550	37913	25965
N° of observations	433387	224285	274288	114298	158995	109897

	Intensive margin: 31%-80%					
	Overall (1)		High exit rate (2)		Low exit rate (3)	
	Men	Women	Men	Women	Men	Women
ATT1 in pp - δ_1	0.06019	-0.26935	-0.49229	0.23269	0.44056	-0.68082
95% CI	[-0.808; 0.928]	[-1.693; 1.154]	[-1.698; 0.714]	[-1.885; 2.350]	[-0.800; 1.681]	[-2.610; 1.248]
SE	(0.44296)	(0.72642)	(0.61534)	(1.08020)	(0.63283)	(0.98412)
Pvalue	(0.89191)	(0.71079)	(0.42369)	(0.82945)	(0.48632)	(0.48907)
ATT2 in pp - δ_2	-0.09515	-0.52668	-0.31705	-1.27590	-0.28799	0.16285
95% CI	[-1.012; 0.822]	[-2.099; 1.046]	[-1.644; 1.010]	[-3.587; 1.035]	[-1.547; 0.971]	[-1.986; 2.311]
SE	(0.46782)	(0.80238)	(0.67694)	(1.17900)	(0.64256)	(1.09617)
Pvalue	(0.83883)	(0.51157)	(0.63953)	(0.27918)	(0.65401)	(0.88190)
ATT3 in pp - δ_3	-1.35886***	-0.87107	-2.37108***	-1.35565	0.61296	-0.32437
95% CI	[-2.351; -0.367]	[-2.57; 0.828]	[-3.770; -0.972]	[-3.823; 1.111]	[-0.76; 1.986]	[-2.662; 2.014]
SE	(0.50614)	(0.86690)	(0.71386)	(1.25861)	(0.70052)	(1.19284)
Pvalue	(0.00726)	(0.31500)	(0.00090)	(0.28144)	(0.38158)	(0.78568)
ATT4 in pp - δ_4	-1.97903***	-1.59314*	-3.66686***	-1.45756	1.44883*	-1.38498
95% CI	[-3.040; -0.918]	[-3.395; 0.208]	[-5.155; -2.179]	[-4.065; 1.150]	[-0.008; 2.905]	[-3.872; 1.103]
SE	(0.54149)	(0.91908)	(0.75921)	(1.33020)	(0.74317)	(1.26908)
Pvalue	(0.00026)	(0.08303)	(0.00000)	(0.27320)	(0.05124)	(0.27514)
ATT5 in pp - δ_5	-1.00841*	-1.85450**	-2.49271***	-1.55476	1.37179*	-1.69886
95% CI	[-2.079; 0.062]	[-3.706; -0.003]	[-3.969; -1.016]	[-4.205; 1.096]	[-0.140; 2.883]	[-4.279; 0.881]
SE	(0.54619)	(0.94464)	(0.75339)	(1.35220)	(0.77106)	(1.31620)
Pvalue	(0.06486)	(0.04963)	(0.00094)	(0.25023)	(0.07523)	(0.19681)
Anticip. in pp - δ_0	-1.46860***	-1.01639	-2.28916***	-0.43635	0.29935	-1.33961
95% CI	[-2.268; -0.669]	[-2.317; 0.285]	[-3.477; -1.101]	[-2.328; 1.455]	[-0.742; 1.340]	[-3.123; 0.444]
SE	(0.40808)	(0.66374)	(0.60612)	(0.96515)	(0.53110)	(0.90988)
Pvalue	(0.00032)	(0.12570)	(0.00016)	(0.65120)	(0.57300)	(0.14096)
Mean ATT in pp- $\bar{\delta}$	-0.87625**	-1.02295	-1.86800***	-1.08224	0.71723	-0.78523
95% CI	[-1.654; -0.098]	[-2.464; 0.418]	[-2.931; -0.805]	[-3.142; 0.977]	[-0.403; 1.837]	[-2.793; 1.223]
Pvalue	(0.0273)	(0.1642)	(0.0006)	(0.3030)	(0.2095)	(0.4434)
Mean ATT in %	-4.9031	-2.2154	-8.6835	-2.2194	5.2310	-1.7927
N° of individuals	98900	53300	63622	28550	37913	25965
N° of observations	433387	224285	274288	114298	158995	109897

	Intensive margin: 81%-100%					
	Overall (1)		High exit rate (2)		Low exit rate (3)	
	Men	Women	Men	Women	Men	Women
ATT1 in pp - δ_1	0.78990	0.47212	1.70353**	0.21512	0.12960	0.76058
95% CI	[-0.165; 1.745]	[-0.930; 1.874]	[0.328; 3.079]	[-1.803; 2.233]	[-1.169; 1.428]	[-1.176; 2.697]
SE	(0.48705)	(0.71533)	(0.70187)	(1.02941)	(0.66250)	(0.98819)
Pvalue	(0.10485)	(0.50925)	(0.01522)	(0.83447)	(0.84491)	(0.44150)
ATT2 in pp - δ_2	0.67536	0.21660	1.20545	0.27812	0.74099	0.20719
95% CI	[-0.333; 1.684]	[-1.345; 1.778]	[-0.287; 2.697]	[-1.947; 2.504]	[-0.591; 2.073]	[-1.963; 2.377]
SE	(0.51459)	(0.79664)	(0.76124)	(1.13543)	(0.67959)	(1.10707)
Pvalue	(0.18938)	(0.78570)	(0.11331)	(0.80650)	(0.27557)	(0.85155)
ATT3 in pp - δ_3	2.46245***	0.74806	4.03659***	1.59323	0.01777	0.09853
95% CI	[1.395; 3.530]	[-0.947; 2.443]	[2.509; 5.564]	[-0.804; 3.990]	[-1.433; 1.469]	[-2.267; 2.464]
SE	(0.54482)	(0.86497)	(0.77932)	(1.22288)	(0.74025)	(1.20699)
Pvalue	(0.00001)	(0.38714)	(0.00000)	(0.19264)	(0.98085)	(0.93494)
ATT4 in pp - δ_4	3.38766***	1.15882	5.69820***	2.23333*	-0.98646	0.03883
95% CI	[2.244; 4.532]	[-0.642; 2.959]	[4.069; 7.327]	[-0.302; 4.769]	[-2.517; 0.544]	[-2.483; 2.561]
SE	(0.58375)	(0.91854)	(0.83121)	(1.29362)	(0.78089)	(1.28677)
Pvalue	(0.00000)	(0.20710)	(0.00000)	(0.08428)	(0.20651)	(0.97593)
ATT5 in pp - δ_5	2.36227***	1.72548*	4.81605***	2.21302*	-0.98360	1.18654
95% CI	[1.207; 3.517]	[-0.130; 3.581]	[3.197; 6.435]	[-0.376; 4.802]	[-2.583; 0.616]	[-1.433; 3.806]
SE	(0.58923)	(0.94662)	(0.82611)	(1.32103)	(0.81590)	(1.33644)
Pvalue	(0.00006)	(0.06834)	(0.00000)	(0.09390)	(0.22800)	(0.37464)
Anticip. in pp - δ_0	1.94518***	0.70688	3.06391***	0.63275	-0.27124	0.62541
95% CI	[1.063; 2.827]	[-0.567; 1.981]	[1.794; 4.334]	[-1.157; 2.423]	[-1.466; 0.924]	[-1.205; 2.456]
SE	(0.44992)	(0.65005)	(0.64775)	(0.91325)	(0.60960)	(0.93377)
Pvalue	(0.00002)	(0.27685)	(0.00000)	(0.48841)	(0.65636)	(0.50301)
Mean ATT in pp- $\bar{\delta}$	1.93553***	0.86422	3.49197***	1.30656	-0.21634	0.45833
95% CI	[1.081; 2.790]	[-0.594; 2.322]	[2.291; 4.693]	[-0.729; 3.342]	[-1.402; 0.969]	[-1.594; 2.511]
Pvalue	(0.0000)	(0.2454)	(0.0000)	(0.2083)	(0.7206)	(0.6616)
Mean ATT in %	2.4962	2.1067	3.4920	3.8304	-0.2163	0.9771
N° of individuals	98900	53300	63622	28550	37913	25965
N° of observations	433387	224285	274288	114298	158995	109897

	Intensive margin: approximated working time (100% = full time)					
	Overall (1)		High exit rate (2)		Low exit rate (3)	
	Men	Women	Men	Women	Men	Women
ATT1 in pp - δ_1	0.75416***	0.32171	1.37031***	0.61579	0.27426	0.12036
95% CI	[0.189; 1.319]	[-0.535; 1.178]	[0.506; 2.234]	[-0.678; 1.909]	[-0.435; 0.984]	[-1.004; 1.244]
SE	(0.28822)	(0.43695)	(0.44078)	(0.65997)	(0.36209)	(0.57347)
Pvalue	(0.00888)	(0.46158)	(0.00188)	(0.35081)	(0.44880)	(0.83377)
ATT2 in pp - δ_2	0.57584*	-0.16160	0.92870**	-0.12538	0.57551	-0.14788
95% CI	[-0.001; 1.152]	[-1.112; 0.789]	[0.024; 1.833]	[-1.543; 1.293]	[-0.119; 1.270]	[-1.413; 1.117]
SE	(0.29413)	(0.48495)	(0.46138)	(0.72344)	(0.35444)	(0.64555)
Pvalue	(0.05026)	(0.73896)	(0.04413)	(0.86241)	(0.10445)	(0.81881)
ATT3 in pp - δ_3	1.51952***	0.09798	2.49476***	0.77859	0.14780	-0.41376
95% CI	[0.950; 2.089]	[-0.926; 1.122]	[1.664; 3.325]	[-0.737; 2.294]	[-0.615; 0.910]	[-1.781; 0.954]
SE	(0.29056)	(0.52232)	(0.42377)	(0.77308)	(0.38908)	(0.69764)
Pvalue	(0.00000)	(0.85120)	(0.00000)	(0.31388)	(0.70404)	(0.55313)
ATT4 in pp - δ_4	1.87005***	0.39815	3.21496***	1.64675**	-0.41931	-0.72955
95% CI	[1.251; 2.489]	[-0.690; 1.486]	[2.307; 4.123]	[0.042; 3.252]	[-1.220; 0.382]	[-2.190; 0.731]
SE	(0.31560)	(0.55515)	(0.46308)	(0.81881)	(0.40867)	(0.74519)
Pvalue	(0.00000)	(0.47326)	(0.00000)	(0.04432)	(0.30489)	(0.32759)
ATT5 in pp - δ_5	1.50430***	0.60822	3.00553***	1.30668	-0.31341	0.08063
95% CI	[0.881; 2.127]	[-0.512; 1.728]	[2.115; 3.896]	[-0.327; 2.940]	[-1.165; 0.538]	[-1.438; 1.599]
SE	(0.31791)	(0.57130)	(0.45451)	(0.83344)	(0.43443)	(0.77459)
Pvalue	(0.00000)	(0.28706)	(0.00000)	(0.11693)	(0.47065)	(0.91710)
Anticip. in pp - δ_0	1.05031***	0.18384	1.64944***	0.42274	-0.06586	-0.06991
95% CI	[0.534; 1.566]	[-0.692; 1.060]	[0.930; 2.369]	[-0.728; 1.574]	[-0.794; 0.662]	[-1.385; 1.245]
SE	(0.26317)	(0.44700)	(0.36706)	(0.58728)	(0.37143)	(0.67106)
Pvalue	(0.00007)	(0.68086)	(0.00001)	(0.47164)	(0.85926)	(0.91702)
Mean ATT in pp- $\bar{\delta}$	1.24477***	0.25289	2.20285***	0.84448	0.05297	-0.21804
95% CI	[0.775; 1.715]	[-0.630; 1.136]	[1.516; 2.890]	[-0.447; 2.136]	[-0.570; 0.676]	[-1.408; 0.972]
Pvalue	(0.0000)	(0.5746)	(0.0000)	(0.2001)	(0.8676)	(0.7195)
Mean ATT in %	1.4190	0.37636	2.6172	1.3602	0.05793	-0.30500
N° of individuals	98900	53300	63622	28550	37913	25965
N° of observations	433387	224285	274288	114298	158995	109897

	Placebo: Intensive margin: 0%-30%					
	Overall (1)		High exit rate (2)		Low exit rate (3)	
	Men	Women	Men	Women	Men	Women
Placebo in pp	-0.30813*	0.13739	-0.53909	0.66248	-0.49225***	-0.24532
95% CI	[-0.651; 0.035]	[-0.743; 1.018]	[-1.195; 0.116]	[-0.849; 2.174]	[-0.863; -0.121]	[-1.235; 0.744]
SE	(0.17512)	(0.44922)	(0.33446)	(0.77131)	(0.18930)	(0.50484)
Pvalue	(0.07849)	(0.75974)	(0.10701)	(0.39041)	(0.00932)	(0.62701)
N° of individuals	66128	33031	42345	17292	25104	16315
N° of observations	206170	91365	130374	46405	75761	44954

	Placebo: Intensive margin: 31%-80%					
	Overall (1)		High exit rate (2)		Low exit rate (3)	
	Men	Women	Men	Women	Men	Women
Placebo in pp	-0.02336	-0.09966	0.00501	-0.49845	0.44508	0.19174
95% CI	[-0.673; 0.626]	[-1.335; 1.136]	[-1.007; 1.017]	[-2.423; 1.426]	[-0.409; 1.299]	[-1.422; 1.805]
SE	(0.33142)	(0.63038)	(0.51657)	(0.98199)	(0.43589)	(0.82315)
Pvalue	(0.94381)	(0.87438)	(0.99226)	(0.61175)	(0.30723)	(0.81582)
N° of individuals	66128	33031	42345	17292	25104	16315
N° of observations	206170	91365	130374	46405	75761	44954

	Placebo: Intensive margin: 81%-100%					
	Overall (1)		High exit rate (2)		Low exit rate (3)	
	Men	Women	Men	Women	Men	Women
Placebo in pp	0.33149	-0.03772	0.53408	-0.16403	0.04718	0.05358
95% CI	[-0.373; 1.036]	[-1.234; 1.159]	[-0.587; 1.655]	[-1.956; 1.628]	[-0.868; 0.963]	[-1.550; 1.657]
SE	(0.35949)	(0.61034)	(0.57203)	(0.91407)	(0.46709)	(0.81809)
Pvalue	(0.35647)	(0.95072)	(0.35049)	(0.85759)	(0.91955)	(0.94778)
N° of individuals	66128	33031	42345	17292	25104	16315
N° of observations	206170	91365	130374	46405	75761	44954

	Placebo: Intensive margin: approximated working time					
	Overall (1)		High exit rate (2)		Low exit rate (3)	
	Men	Women	Men	Women	Men	Women
Placebo in pp	0.17688	-0.20320	0.55979	-0.39825	0.16404	-0.05478
95% CI	[-0.247; 0.600]	[-0.956; 0.549]	[-0.115; 1.235]	[-1.584; 0.787]	[-0.343; 0.671]	[-1.000; 0.890]
SE	(0.21603)	(0.38401)	(0.34440)	(0.60478)	(0.25862)	(0.48223)
Pvalue	(0.41291)	(0.59670)	(0.10408)	(0.51023)	(0.52590)	(0.90956)
N° of individuals	66128	33031	42345	17292	25104	16315
N° of observations	206170	91365	130374	46405	75761	44954

DiD on parallel path: impact on the intensive margin by quarter and Mean effect. Intensive margin defined as number of workers with a certain working time with respect to the reference time (*0-30%, 31-80%, >81%, approximated continuous working time with 100% equal to full-time). Control group is 53.25-55.25 years old; treated group 58-59.5 years old. Effects on specific subpopulation* (*defined in time q) by column:* (*1) Overall treated group,* (*2) Workers in sectoral industrial committees with an exit rate from employment above the median (= 18%),* (*3) Workers in sectoral industrial committees with an exit rate below the median. The placebo tests are performed by dividing the pre-treatment period in two parts. N° of observations is the sum of the number of individuals observed in each quarter of the analysis. Cluster Robust Standard errors by individual. ***: significant at 1%, **: significant at 5%, *: significant at 10%.*

Table B.9: DiD - Impact on the Hourly Gross Wage: control group 53.25-55.25 years old

	Hourly Gross Wage					
	Overall (1)		High exit rate (2)		Low exit rate (3)	
	Men	Women	Men	Women	Men	Women
ATT1 in € - δ_1	0.06997	-0.16070	0.16807	-0.24318	-0.07167	-0.01542
95% CI	[-0.285; 0.425]	[-0.378; 0.057]	[-0.233; 0.569]	[-0.562; 0.076]	[-0.674; 0.531]	[-0.311; 0.281]
SE	(0.18130)	(0.11092)	(0.20449)	(0.16285)	(0.30738)	(0.15099)
Pvalue	(0.69956)	(0.14738)	(0.41114)	(0.13538)	(0.81563)	(0.91866)
ATT2 in € - δ_2	-0.16133	-0.25206*	-0.07721	-0.42948**	-0.23015	-0.02140
95% CI	[-0.609; 0.286]	[-0.531; 0.027]	[-0.594; 0.440]	[-0.836; -0.023]	[-0.970; 0.510]	[-0.404; 0.361]
SE	(0.22819)	(0.14246)	(0.26385)	(0.20718)	(0.37737)	(0.19535)
Pvalue	(0.47956)	(0.07683)	(0.76981)	(0.03819)	(0.54194)	(0.91277)
ATT3 in € - δ_3	0.03413	-0.09501	0.20748	-0.27499	-0.38000	0.18570
95% CI	[-0.511; 0.580]	[-0.436; 0.246]	[-0.415; 0.830]	[-0.768; 0.218]	[-1.284; 0.524]	[-0.286; 0.657]
SE	(0.27830)	(0.17418)	(0.31747)	(0.25167)	(0.46127)	(0.24048)
Pvalue	(0.90239)	(0.58544)	(0.51340)	(0.27455)	(0.41005)	(0.44001)
ATT4 in € - δ_4	-0.11794	-0.30298	-0.12239	-0.37047	-0.60561	-0.11634
95% CI	[-0.78; 0.545]	[-0.721; 0.115]	[-0.893; 0.648]	[-0.978; 0.237]	[-1.694; 0.483]	[-0.693; 0.460]
SE	(0.33802)	(0.21349)	(0.39310)	(0.30976)	(0.55527)	(0.29427)
Pvalue	(0.72715)	(0.15585)	(0.75555)	(0.23171)	(0.27544)	(0.69258)
ATT5 in € - δ_5	-0.18834	-0.44268*	-0.23035	-0.52698	-0.79870	-0.24835
95% CI	[-0.977; 0.600]	[-0.934; 0.049]	[-1.143; 0.682]	[-1.238; 0.185]	[-2.089; 0.492]	[-0.927; 0.430]
SE	(0.40230)	(0.25085)	(0.46562)	(0.36299)	(0.65830)	(0.34605)
Pvalue	(0.63968)	(0.07761)	(0.62081)	(0.14657)	(0.22503)	(0.47296)
Differential trend - μ	0.08222	-0.12145	0.12402	-0.12341	-0.08769	-0.09239
95% CI	[-0.172; 0.336]	[-0.280; 0.037]	[-0.159; 0.407]	[-0.351; 0.104]	[-0.532; 0.357]	[-0.316; 0.131]
SE	(0.12953)	(0.08072)	(0.14422)	(0.11615)	(0.22677)	(0.11416)
Pvalue	(0.52560)	(0.13245)	(0.38985)	(0.28799)	(0.69899)	(0.41836)
Anticip. in € - δ_0	0.07595	0.04732	0.07482	0.05418	0.12289	0.02964
95% CI	[-0.023; 0.175]	[-0.015; 0.110]	[-0.040; 0.190]	[-0.035; 0.144]	[-0.037; 0.283]	[-0.057; 0.116]
SE	(0.05048)	(0.03185)	(0.05862)	(0.04565)	(0.08164)	(0.04431)
Pvalue	(0.13242)	(0.13730)	(0.20183)	(0.23531)	(0.13226)	(0.50351)
Mean ATT in € - $\bar{\delta}$	-0.07270	-0.25069	-0.01088	-0.36902	-0.41723	-0.04316
95% CI	[-0.607; 0.462]	[-0.587; 0.085]	[-0.627; 0.606]	[-0.857; 0.119]	[-1.298; 0.464]	[-0.505; 0.418]
Pvalue	(0.7897)	(0.1436)	(0.9724)	(0.1383)	(0.3533)	(0.8546)
Mean ATT in %	-0.3549	-1.8216	-0.0639	-2.8035	-1.7288	-0.3047
N° of individuals	92630	53223	59645	28498	35288	25938
N° of observations	389993	223919	247178	114131	142717	109698

	Placebo: Hourly Gross Wage					
	Overall (1)		High exit rate (2)		Low exit rate (3)	
	Men	Women	Men	Women	Men	Women
Placebo in €	0.12536	0.07517	0.09300	0.07370	0.27853	0.06607
95% CI	[-0.081; 0.332]	[-0.057; 0.207]	[-0.150; 0.336]	[-0.123; 0.270]	[-0.056; 0.613]	[-0.112; 0.244]
SE	(0.10520)	(0.06738)	(0.1241)	(0.10040)	(0.17062)	(0.09093)
Pvalue	(0.23341)	(0.26459)	(0.45364)	(0.46292)	(0.10260)	(0.46749)
N° of individuals	59922	33019	38407	17283	22470	16306
N° of observations	163252	91251	103522	46369	59701	44876

*DiD on parallel growths: impact on the hourly gross wage by quarter and Mean effect. Control group is 53.25-55.25 years old; treated group 58-59.5 years old. Effects on specific subpopulation (defined in time q) by column: (1) Overall treated group, (2) Workers in sectoral industrial committees with an exit rate from employment above the median (= 18%), (3) Workers in sectoral industrial committees with an exit rate below the median. The placebo tests are performed by dividing the pre-treatment period in two parts. N° of observations is the sum of the number of individuals observed in each quarter of the analysis. Cluster Robust Standard errors by individual.***: significant at 1%,**: significant at 5%,*: significant at 10%.*

Table B.10: Panel data - DiD on employment rate – full sample

	OLS – Men (1)	OLS – Women (1)	Fixed-Effect – Men (2)	Fixed-Effect – Women (2)
ATT	0.42580	-1.08731***	-1.32336***	-0.70972***
95% CI	[-0.50938,1.36098]	[-1.60416,-0.57045]	[-1.71523,-0.93149]	[-0.94780,-0.47163]
SE	(0.47714)	(0.26370)	(0.19993)	(0.12147)
Pvalue	(0.37217)	(0.00004)	(0.00000)	(0.00000)
Placebo – 2001q4	0.16922	-0.62465***	-0.83507***	-0.42040***
95% CI	[-0.51882,0.85726]	[-1.00084,-0.24847]	[-1.08624,-0.58391]	[-0.58477,-0.25604]
SE	(0.35104)	(0.19193)	(0.12815)	(0.08386)
Pvalue	(0.62977)	(0.00114)	(0.00000)	(0.00000)
Placebo – 2001q3	0.00580	-0.64498***	-0.93574***	-0.41414***
95% CI	[-0.59032,0.60192]	[-0.97308,-0.31689]	[-1.14873,-0.72274]	[-0.55498,-0.27331]
SE	(0.30415)	(0.16740)	(0.10867)	(0.07185)
Pvalue	(0.98477)	(0.00012)	(0.00000)	(0.00000)
Placebo – 2001q2	-0.09178	-0.57541***	-0.67682***	[-0.38532***
95% CI	[-0.58805,0.40449]	[-0.84989,-0.30093]	[-0.85032,-0.50333]	[-0.50395,-0.26670]
SE	(0.25320)	(0.14004)	(0.08852)	(0.06052)
Pvalue	(0.71699)	(0.00004)	(0.00000)	(0.00000)
N° Observations	3,220,833	1,895,922	3,220,833	1,895,922
Individuals (cluster)	153,373	90,282	153,373	90,282

Panel data: DiD on the employment rate implemented by OLS (1) or individual Fixed-Effects (2). Estimates are based on the full panel of 243,655 individuals (men plus women) on the period 1998q2-2003q2. The OLS/FE regression includes quarterly-age & time dummies, the interaction for individuals above 58 years old after the period 2002q2 (treatment effect) and another interaction for 2002q1 (anticipation effects). In addition, we include further age (above 58)-time interactions for 2000q4, 2000q3, 200q2 as placebo tests. N° of observations is the sum of the number of individuals observed in each quarter of the analysis. Cluster Robust Standard errors by individual.***: significant at 1%,**: significant at 5%,*: significant at 10%.

Table B.11: Impact on the Extensive Margin: Treated aged 58, control aged 57.75

	Men			**Women**		
	Overall (1)	**High Exit (2)**	**Low Exit (3)**	**Overall (1)**	**High Exit (2)**	**Low Exit (3)**
ATT in pp	-0.2	1.2	1.7	-0.6	2.0	-0.4
95% CI	[-2.9, 2.5]	[-4.3, 6.7]	[-3.2, 6.6]	[-3.4, 2.3]	[-8.7, 12.6]	[-8.1, 7.3]
Pvalue	0.894	0.664	0.498	0.699	0.714	0.922
ATT %	-0.8	1.9	2.1	-5.0	2.9	-0.5
N obs	99,462	41,651	26,864	50,593	14,555	15,624
N indiv.	53,320	23,512	14,738	27,445	8,324	8,707

*WDiD on parallel growths: impact on the employment rate. Control group is 57.75 years old; treated group 58 years old. Effects on specific subpopulation (defined in q-7) by column: (1) Overall treated group, (2) Workers in sectoral industrial committees with an exit rate from employment above the population median (= 18%), (3) Workers in sectoral industrial committees with an exit rate below the population median. Point estimates of the ATT are expressed in percentage points (pp) and in proportional (%) changes in the employment rate. N° of observations is the sum of the number of individuals observed in each quarter of the analysis. Standard errors are obtained by a stratified bootstrap (clustering by individual) with 200 repetitions and 95% confidence intervals (CI) by assuming normality. ***: significant at 1%, **: significant at 5%, *: significant at 10%.*

Bibliography

Agell, S. A., Forslund, A., Helmström, M., Skans, O. N., Runeson, C., Öckert, B. 2002. Follow-up of EU's recommendations on labour market policies, Report 2002: 3, Institute for Labour Market Policy Evaluation, Uppsala.

Anxo, D., 2012. Employment policies to promote active ageing, Sweden, European Employment Observatory Review.

ARFTLV, 2013. Aides au recrutement et à la formation 2013, Contrat Initiative Emploi CIE, Fiche n° 5.02. Available online: http://www.horizon.entreprise.arftlv.org/Fiche-Aide/344/Contrat-Initiative-Emploi-CIE.pdf.

Arnkil, R., 2012. Employment policies to promote active ageing, Finland, European Employment Observatory Review.

Dietz, M., Koch, S., Krug, G., Stephan, G., 2011. Die Entgeltsicherung für Ältere: Ein Auslaufmodell?, WSI-Mitteilungen Ausgabe.

Duell, N., Vogler-Ludwig K., 2012. Employment policies to promote active ageing, Germany, European Employment Observatory Review.

European Commission, 2002. **EEO Review Spring 2002: Policies and Trends**. Available online: http://www.eu-employment-observatory.net/resources/reviews/eeo2002_en.pdf.

European Commission, 2012. Employment Policies to Promote Active Ageing 2012, European Employment Observatory Review.

Fiscaalleven, 2012. Vitaliteitsregeling per 1-1-2013; versie 2012-02. Available online: http://www.fiscaalleven.eu/NaslagVitaliteitsregeling%202013.pdf

Gineste, S., 2012. Employment policies to promote active ageing, France, European Employment Observatory Review.

Grierson, K., 2002. New Deal 50 Plus: Quantitative Analysis of Job Retention, IAD Information Centre. Available online: http://webarchive.nationalarchives.gov.uk/+/http://www.dwp.gov.uk/jad/2003/151_rep.pdf

International Reform Monitor, 2010. Netherlands - Labour Market Work-continuation tax credit, Amendment to dismissal decree.

Lechner, F., 2012. Employment policies to promote active ageing, Austria, European Employment Observatory Review.

Madsen, K., 2012. Employment policies to promote active ageing, Denmark, European Employment Observatory Review.

OECD, 2003a. Ageing and Employment Policies/Vieillissement et politiques de l'emploi: Sweden 2003, Ageing and Employment Policies. OECD Publishing.

OECD, 2003b. Ageing and Employment Policies/Vieillissement Et Politiques de L'emploi: Belgium. OECD Publishing.

OECD, 2004a. Ageing and Employment Policies/Vieillissement et politiques de l'emploi: Finland 2004, Ageing and Employment Policies. OECD Publishing.

OECD, 2004b. Ageing and Employment Policies/Vieillissement et politiques de l'emploi: United Kingdom 2004, Ageing and Employment Policies. OECD Publishing.

OECD, 2005a. Ageing and Employment Policies/Vieillissement et politiques de l'emploi: Netherlands 2005, Ageing and Employment Policies. OECD Publishing.

OECD, 2005b. Ageing and Employment Policies/Vieillissement et politiques de l'emploi: Austria 2005, Ageing and Employment Policies. OECD Publishing.

OECD, 2005c. Ageing and Employment Policies/Vieillissement et politiques de l'emploi: France 2005, Ageing and Employment Policies. OECD Publishing.

OECD, 2005d. Ageing and Employment Policies/Vieillissement et politiques de l'emploi: Germany 2005, Ageing and Employment Policies. OECD Publishing.

OECD, 2006. Live Longer, Work Longer, Ageing and Employment Policies. OECD Publishing.

OECD, 2011. Taxation and Employment, OECD Tax Policy Studies. OECD Publishing.

OECD, 2012a. Thematic Follow-up Review of Policies to Improve Labour Market Prospects for Older Workers: Austria. OECD Publishing.

OECD, 2012b. Thematic Follow-up Review of Policies to Improve Labour Market Prospects for Older Workers: Finland. OECD Publishing.

OECD, 2012c. Thematic Follow-up Review of Policies to Improve Labour Market Prospects for Older Workers: Netherlands. OECD Publishing.

OECD, 2012d. Thematic Follow-up Review of Policies to Improve Labour Market Prospects for Older Workers: Sweden. OECD Publishing.

OECD, 2012e. Thematic Follow-up Review of Policies to Improve Labour Market Prospects for Older Workers: France. OECD Publishing.

OECD, 2012f. Thematic Follow-up Review of Policies to Improve Labour Market Prospects for Older Workers: Germany. OECD Publishing.

OECD, 2012g. Thematic Follow-up Review of Policies to Improve Labour Market Prospects for Older Workers: United Kingdom. OECD Publishing.

OECD, 2012h. Thematic Follow-up Review of Policies to Improve Labour Market Prospects for Older Workers: Belgium. OECD Publishing.

Plasman, R., 2012. Employment policies to promote active ageing, Belgium, European Employment Observatory Review.

Skugor, D., Bekker S., 2012. Employment policies to promote active ageing, Netherland, European Employment Observatory Review.