WORKING TIME REDUCTIONS AT THE END OF THE CAREER.

DO THEY PROLONG THE TIME SPENT IN EMPLOYMENT?

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Abstract

In this paper we study the effects on the survival rate in employment of a scheme that facilitates gradual retirement through working time reductions. We use information on the entire labour market career and other observables to control for selection and take dynamic treatment assignment into account. We also estimate a competing risks model considering different (possibly selective) pathways to early retirement. We find that participation in the scheme prolongs employment during the first two (four) years for men (women). However, when individuals become eligible for early retirement the effect reverses. This suggests that TC initially improves the work-life balance, but that it eventually decreases labour market attachment and signals to employers a preference for early retirement. The institutional environment in which part-time participants are entitled to full-time pensions reinforces the latter process. Participation in TC seems also to generate a slight, statistically insignificant, improvement in health.

Keywords: Part-time work, older workers, inverse probability weighting, dynamic selection into treatment, endogenous sampling

JEL-Classification: J14, C22, J18, J22

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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, 2015). 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. Gradual reduction of the working time at the end of the career has been proposed as a tool to prolong the working career and to increase the activity rates of older workers (e.g. Schmid, 1998). This would reduce the risk of dropping out of the labour force due to health problems (Ahn, 2016) or improve the work-life balance, partly by taking up care obligations (Gielen, 2009; Van Looy et al., 2014). These benefits of gradual retirement have, however, been contested by others. Employers may use subsidized part-time employment at the end of the career as an easy way to get gradually rid of less productive older workers and, as such, a bridge to early retirement (Graf et al., 2011). In this paper we aim at shedding more light on this controversy based on the evaluation of a scheme that subsidizes working time reductions at the end of the career in Belgium.

Several EU countries have put gradual retirement schemes in place for older workers.[1] These can take different forms. In Sweden, Finland and Denmark workers can reduce their working time and top up their income by prematurely drawing from their pension entitlements. In Belgium, Germany and Austria the government subsidizes working time reductions at the end of the career 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, 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 on the effectiveness of gradual retirement schemes based on counterfactual impact evaluations is scarce. Graf et al. (2011) and Huber et al. (2016) estimate the effects of the Austrian, respectively German, gradual retirement scheme based on flexible methods that solve the selection problem by conditioning on a rich set of individual, firm and regional characteristics. 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. 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

[1] See Table A.1 in the Appendix A for an overview of such schemes in the countries mentioned in the text.

prolongs the working career for men, but less so in periods of more intensive use of the block model. Finally, Elsayed et al. (2015) use a stated preferences experiment based on vignettes to evaluate the effect of various hypothetical pension reforms in the Netherlands, among which the introduction of gradual retirement. This study finds that gradual retirement would induce workers to retire one year later on average, but also that total lifetime labour supply would still fall by 3.4 months.

In Belgium private sector employees older than fifty are subsidized within the Time Credit (TC) scheme to reduce their working time by 20%, 50% or 100% until retirement. Since the 100% working time reduction is like a “block model” that resembles more an *early* than a *gradual* retirement scheme, focusing on the partial working time reductions is more informative for our research question (Berg et al. 2015). We evaluate the impact of TC on the survival rate in employment up to eight years after entry in the scheme as well as on the different exit destinations. To that end we exploit a very rich administrative data on social benefit receipt, labour market histories (from as early as 1957), and some essential firm and household characteristics. We argue that we can eliminate the potential selection bias by conditioning on this abundance of relevant information. In particular, even if information on important variables, such as ability, motivation and health, is missing, these time-constant unobserved factors are captured by conditioning on the complete employment history of the individuals, i.e. by exploiting the panel structure of the data (Lechner et al., 2011; Huber et al., 2016).

We contribute to the literature in the following ways. First, by considering a competing risks duration model, we explicitly study how the effect of the working time reduction interacts with supply incentives to exit the labour force via early retirement schemes. In particular, we find that the TC scheme delays labour force withdrawals as long as participants are not eligible to early retirement. When they are, exits from the labour force are reinforced, especially to the early statutory retirement scheme which provides a more generous replacement rate for TC participants than the other early retirement schemes. Second, to the best of our knowledge, we are the first to provide evidence on the impact of gradual retirement on the incidence of sick leave. 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. Fredriksson and Johansson (2008) have shown that in case of dynamic assignment into treatment methods that eliminate the selection bias by conditioning on a set of observables while maintaining the assumption that the treatment assignment is static are biased. As to take into account a selective censoring problem that was overlooked by the aforementioned authors, we follow the estimation procedure recently proposed by Vikström (2014). We demonstrate that, in particular in the competing risk framework, correcting for this selectivity can eliminate substantial bias relative to the method of Fredriksson and Johansson.

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 similar to those found by Graf et al. (2011) for Austria and Huber et al. (2016) for West-German men. In contrast to the findings in Austria, where demand factors are the most likely explanation, we argue that in Belgium supply factors matter more. The short run positive effect are more compatible with an improved work-life balance than with a cheap mode of getting rid of less productive older workers, because the strict employment protection in Belgium makes it unlikely that employers would fire workers in the counterfactual of no participation in TC. Moreover, the dominant explanation for the subsequent enhanced labour force withdrawal is mainly driven by enhanced take up of conventional early retirement, which is difficult to impose involuntarily and which is the most favourable early retirement regime from the perspective of the worker, because, in contrast to other regimes, part-time TC-recipients remain entitled to a full-time pension benefit. We also find that participation in TC slightly reduces the incidence of sick-leave, but not significantly, so that improved health is not the dominant explanation for the positive effect. We argue that this weak effect is due to the targeting of TC to a relatively healthy population. Overall, the *existing* scheme does not pass the cost-benefit test.

The paper is structured as follows. We start with a literature review. Section 3 describes the institutional context and Section 4 the sampling scheme and the data. Section 5 outlines the empirical strategy (identification and estimation) and presents the empirical results. The final section summarizes the findings and concludes. The Appendix contains tables with supplementary results, a comparison of gradual retirement schemes in Europe, and a description of the endogenous stratification of the sample. The Internet Appendix contains a detailed account of the sample selection, the estimation methods and the cost-benefit analysis.[2]

2. Literature Review

Working time reductions may reduce the risk of dropping out the labour force by counteracting the declining health of older workers (Ahn, 2016) or by allowing workers to improve their work-life balance, partly by taking up care obligations (Gielen, 2009; Van Looy et al., 2014). Firms may voluntarily grant working time reductions 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.

[2] The Internet Appendix can be downloaded from http://sites.google.com/site/researchandreaalbanese.

In addition, employers may not be willing to award gradual retirement options (Hurd, 1996; Charles and Decicca, 2007; Gielen, 2009) so that government coercion or 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.[3] If employment protection is strong, 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. Gradual retirement 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 to employers 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 without the possibility to reduce working time can either stop working altogether or stay working full-time. With the possibility to adjust the working time, the number of hours worked increases in the first case and decreases otherwise. The net effect 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. 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 participants in the OAPT scheme between 2000 and 2003 to a control group to whom a hypothetical start date was assigned according to the simulation procedure described in Lechner and Wunsch (2008).[4] They find that the OAPT 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 OAPT decreases the number of days employed by about 35 for women and by nearly 50 for men, leading to a cumulatively negative effect over five years. Moreover, as OAPT participants work part-time, the full-time equivalent 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.

[3] There is some evidence that declining productivity with age or deferred compensation schemes induce a pay-productivity gap for older workers (e.g. Hellerstein and Neumark (2007), Aubert and Crépon (2003, 2006), Ilmakunnas and Maliranta (2005)) and in particular in Belgium (Cataldi et al. (2012), Vandenberghe et al. (2013)). By contrast, no pay-productivity gap is found by (Cardoso et al., 2011; van Ours and Stoeldraijer, 2011).

[4] This procedure has been criticised by Fredriksson and Johansson (2008).

Huber et al. (2016) use the CIA to study the effect of introducing partial retirement in Germany. They estimate the *intention-to-treat* effects of the OAPT by contrasting the labour market outcomes of male employees in firms that started offering partial retirement between 2000 and 2002 to those in firms that did not offer this opportunity in this period. In East-Germany the option of partial retirement did not have any significant impact on the timing at which the labour market was left but it did decrease the exit to unemployment schemes. 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. Though in the first two years the effects in West-Germany are not very different, after three to four year, the effect on unemployment disappears and the share of treated workers exiting the labour force increases. The authors attribute the different effects between West and East-Germany to the difficult labour market conditions in the latter region.

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. During the period of less (more) intensive use of the block-model male participants remained 1.8 (1.2) years longer employed than non-participants, if the time not worked 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.

Finally, Elsayed et al. (2015) use vignettes in a web-based survey of 3,611 Dutch public servants to evaluate the introduction of an early retirement scheme in the Netherlands. In this survey respondents get different vignettes of hypothetical retirement scenarios, including gradual retirement with a 50% working time reduction of working time and an entitlement to half the full pension. For each vignette respondents state at which age they would retire. To provide causal interpretations of the systematic relations between the responses and the scenarios, the scenarios were randomly assigned to the respondents. The study finds that gradual retirement would induce workers to retire on average one year later. However, total lifetime labour supply would still fall by 3.4 months, because the reduced working hours within the gradual retirement scheme dominate the delayed exit from the labour force.

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 scheme aims at generating a better work-life balance by enabling and encouraging employees to slow down their working pace, even without needing to specify a specific motive. Workers younger than 50 can temporarily reduce their working time, while older workers can participate without any time limit, 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.

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, possibly because the subsidy was only 34% higher than the one obtained for the 50% regime and more generous early retirement schemes were available, although generally only at older ages (see below). Since our interest is in *gradual* retirement, we do not further consider the full-time time credit regime. The main eligibility conditions for the end of career TC scheme were the following:

1. Being at least 50 years old at the start of the working time reduction;
2. Being full-time employed during the year prior to entry for the 20% regime; being employed for at least 75% of a full-time schedule for the 50% regime;
3. At least 5 years of tenure in the same firm;
4. At least 20 years of labour market experience;
5. Consent of the employer, if the number of employees in the firm is at most 10 or, in case the firm employs 11 workers or more, if the fraction of employees in the TC is strictly larger than 5% (can be revised by a collective agreement);
6. Notification to 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[5] 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 generally substantially lower for a median

[5] All € in the text are indexed by the CPI and expressed in constant 2004 euros.

wage worker in Belgium. However, since the subsidy is lump-sum in Belgium and proportional in Germany and Austria, the replacement rate may increase to the Austrian and German levels for low-wage workers.[6]

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. As a share of private sector employees aged 50 or more, 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.

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 part-time income, TC participants earn a much higher replacement income if they leave employment for (early) retirement than workers who remain full-time employed. 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 empirical analysis that 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.[7]

- In the period of analysis, early retirement within the statutory regime started from age 60 after minimum 35 years of employment experience.[8] 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%,

[6] 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%).

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

[8] This is an *early* retirement scheme, because the statutory retirement is normally entered after 45 years of labour market experience or at age 65. The statutory retirement age for women was raised by one year every three years from 60 before 1997 to 65 from 2009 onwards.

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. Due to relatively low generosity of this scheme for *full-time* workers in the private sector,[9] take-up in the two alternative regimes, especially the bridge pension, is much more important.

- The bridge pension is available to workers with more than 20 years of employment experience and aged at least 60.[10] 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 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 in TC, which can happen in the 50% TC regime. 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 our empirical analysis that TC increases the likelihood of ending the career through the statutory early retirement, especially in the 50% TC regime.

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

[10] This can be lowered to 58 by a sectoral collective agreement, such as in many industrial committees 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.

4. Data & Sample Selection

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 provides information about the firm (size and sector), the industrial committee to which the worker belongs, the timing of self-employment spells, the UB receipt, as well as the participation in the Career Break, TC schemes and early retirement schemes. Finally, since 2003 the data have been complemented by information on sick leave, receipt of statutory (possibly early) retirement benefits and replacement income in case of disability, occupational diseases or accidents. Since 1998 the National Register also 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.

4.2. Sample Selection

We base our analysis on a sample that was drawn with the purpose to evaluate the effect of a wage cost subsidy for private sector employees aged 58 years or more (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 is stratified to over-represent groups that are relatively less present 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. Five strata $r\ (= 1, 2, \dots, 5)$ are defined and nine birth cohorts $c\ (= 1, 2, \dots, 9)$ for each gender. These strata were defined according to employment status in the private sector and the earned wage around 2002 (cf. Appendix B). 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 (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

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

where N_{cr} denotes the size of the population in substratum cr, 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. To avoid cumbersome notation, gender is not explicitly referred to. The weighting formula comes from a double re-weighing, within and between cohorts.[11]

In this study we evaluate the impact of participating in TC on the survival rate in employment for each year after the entry into treatment. 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 aim at integrating the incidence on sick leave as a second outcome. As mentioned before, information on sick leave is only available since 2003. 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. In principle we could also consider TC that started in later years but 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.

Next, the mandatory three-month employer notice implies that participation is always anticipated and thus that treatment might start before the beginning of the working time reduction. We take this into account by assuming that *actual* treatment starts one quarter before the *contractual* treatment. For example, while individuals in the first treatment group are contractually starting the TC at the beginning of 2004, they are actually treated at the beginning of the fourth quarter of 2003. Additionally, we assume that individuals *never* leave employment between the actual and contractual start of the treatment. The resulting upward bias on the treatment effect (due to higher survival in employment for the treated) is arguably small. It is unlikely that individuals who agreed with the employer to start TC within the next quarter would decide to stop working beforehand, with the exception of reasons of force majeure, such as an accident.

Finally, to enhance the comparability of treated and control groups, we impose that at the moment of sample selection both groups should satisfy the eligibility criteria described in Section 3 (for a full description of this selection see the Internet Appendix). Our treated sample contains 1,227 men and 762 women, representing 5,124 individuals in the Belgian population (i.e. if weighted by $W_{cr,i}$). If we retain all individuals employed in 2002q3 (2003q3) and do not impose the eligibility conditions, the comparison group contains 142,154 men and 83,983 women. Once imposing the eligibility conditions,

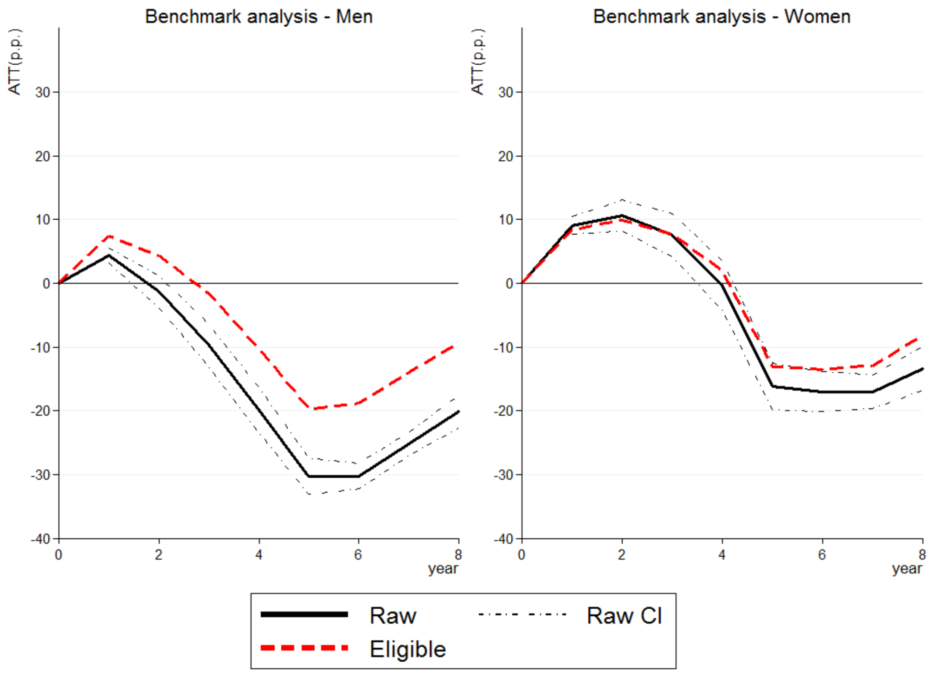
[11] First, to restore the representativeness within the cohorts we reweigh the units within each cohort by $W_{cr}^{c} = \frac{N_{cr}}{N_c} * \frac{n_c}{n_{cr}}$ (where N_c and n_c are the size of the cohort in the population and in the sample). To make the cohorts in the sample representative for the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

the size of the control group shrinks to 29,791 men and 9,658 women. Note that the same control unit may appear in each of the two years of analysis, while treated units can only be present in one year.

4.3. Descriptive Analysis

To have a first idea on the possible effect of the TC we compare the survival rate in employment of the treated and the two control groups in Figure 1. In general, treated individuals, especially women, are more likely to survive in employment in the first years, but the cumulated effect on the survival rate eventually reverses and becomes highly negative. The figure also shows that it is crucial to impose the eligibility conditions on the control group, especially for men: non-eligible individuals are more likely to leave employment, so that a neglect of these conditions would severely downward bias the treatment effect of the TC on the survival rate.

Figure 1: Differences in Survival Rate in Employment by Treatment Status – whole or eligible control group



Differences in survival rate by treatment status (ATT). 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. "Raw" estimates are obtained by using the whole control group (thick line), while "eligible" restricts the control group to the individuals satisfying the eligibility conditions at year 0. Standard errors are cluster robust to take into account correlation between the same individuals in the two samples.

Even if the individuals satisfying the eligibility conditions are already homogenous in several dimensions (e.g. private sector employment experience, tenure, firm size, and working full-time prior to the selection), they still differ significantly in a number of other dimensions. The latter is apparent in Table 1, which reports, by gender, the averages of the observed characteristics for treatments[12] and eligible controls, as well as the p-values for the tests of their equality.

[12] 54.3% of the treated is in the 50% TC regime, the remainder in the 20% TC regime.

Table 1: Descriptive Statistics of Selected Treated and Eligible 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 (i.e. weighted by $W_{cr,i}$)	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 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.

Considering the variables measured in the last quarter prior to the start of the selection (i.e. 2002Q3 or 2003Q3), 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. They are also concentrated in specific, mostly white-collar, sectors, such as the banking and business related services, where bridge pension schemes are less common.

Considering the employment history,[13] 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

[13] The variables referred to the employment history are not combined to keep information coming from different sources and periods with missing information separate.

(the difference is statistically significant only for men). Tenure in 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.

As to avoid that aforementioned differences in the observed characteristics between treated and control units would bias the comparison, we implement an Inverse Probability Weighting (IPW) estimator that takes these differences into account.

5. Empirical Strategy and Results

5.1. Empirical Strategy

We are interested in estimating the average treatment effect on the treated (ATT) in TC on the survival rate in employment. 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 satisfying the eligibility conditions in 2003 (2004) 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, we refer the reader to the Internet Appendix for a detailed exposition. Here we just briefly mention the essential differences with Vikström's approach. First, we generalize his procedure to allow for the endogenous sampling present in our data. Since Vikström implements an inverse probability weighting (IPW) estimator, this just involves taking additional weighting terms into account. 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. If we normalize time to zero at the beginning of the two periods of analysis 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 support for treated and control units. Finally, we propose a slightly different bootstrap procedure for inference on the pooled sample.

The identification strategy is essentially based on the conditional independence assumption (CIA). We argue 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. The literature mentions the following key determinants of gradual retirement also affecting the survival in employment: age, place of living, entitlement to (early) retirement benefits, household composition, 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., 2016). 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 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. We condition on the available firm characteristics (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. As bridge pension is a major pathway to retirement, it is important to control for factors that influence the transition to it. As mentioned in Section 3, 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 1. Finally, note that we control for these characteristics on top of the eligibility conditions used for selecting the sample. As Figure 1 shows, this first selection is already very important.

5.2. 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. We also consider how much the effect changes in comparison to the descriptive evidence discussed above. Next, as we have argued in Section 3, relative to full-time workers, TC participants have higher 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 5.2.2, 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 treated individuals according to treatment type: 20% or 50% working hour reduction. In Section 5.2.3 we consider the impact participation in TC on the incidence of sick leave. Finally, in Section 5.2.4

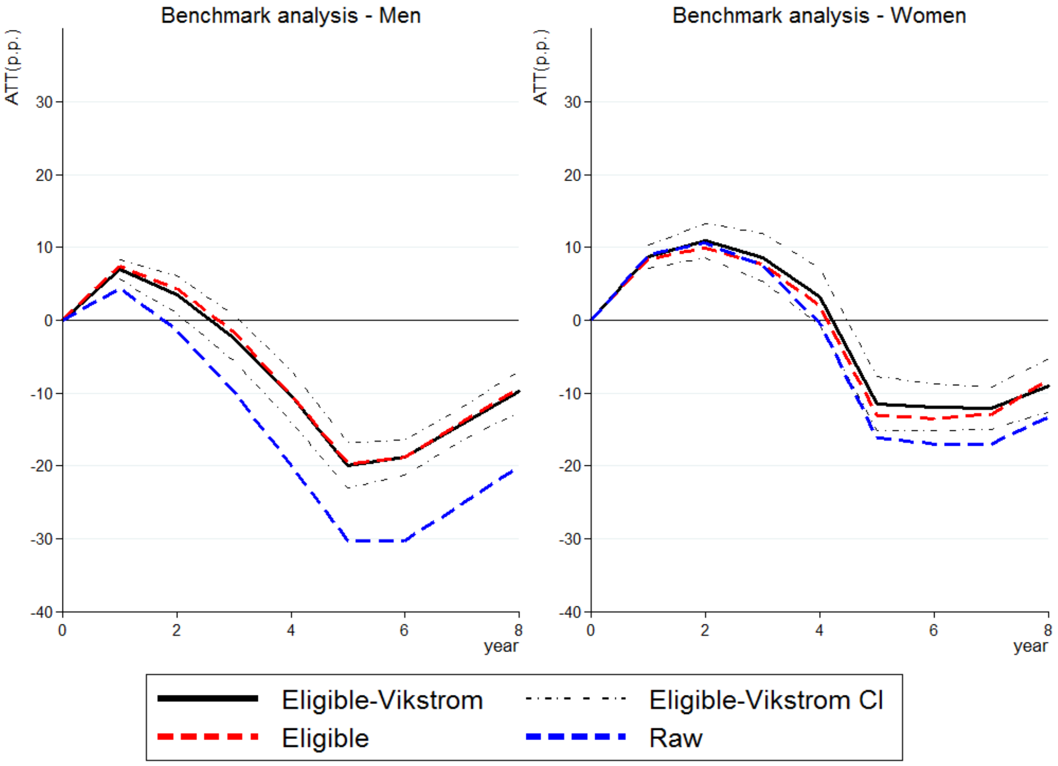
we briefly report the findings of the cost-benefit analysis that we implemented. All analyses are conducted separately for men and women. The reported results pool the 2003 and 2004 sample.

5.2.1. 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). Figure 2 shows that, in line with the descriptive evidence, treated men are more likely to survive in employment in the first two years (+7 pp and +3.5 pp). For women, however, the positive effects last until the fourth year and are stronger (+8.8 pp, +10.9 pp, +8.6 pp and +3.2 pp). However, the results also confirm strong subsequent negative effects, peaking at -20 pp and -12 pp after about five years for men and women, respectively. Notice that as a consequence of ageing, *all* individuals - both treated and non-treated - will eventually retire. This explains why the treatment effect eventually always tends to zero at the end of the period of analysis. Figure 2 also reveals that controlling for the rich set of covariates ("Eligible-Vikström") does not significantly affect the treatment effect that was obtained by just imposing the eligibility conditions ("Eligible"). Hence, imposing the eligibility conditions, which in essence boils down to imposing a similar recent labor market history, seems to be sufficient for eliminating the major component of the selection bias.

These results are in line with the findings of Graf et al. (2011) in Austria and Huber et al. (2016) for men in West-Germany, but while in Austria the mechanism seems to be more induced by demand-side factors, in Belgium, as in Germany, supply factors seem to matter more. The positive effects in the beginning are likely present because in the absence of the option of working part-time some participants in TC would have completely stopped working to allow them to improve their work-life balance (Gielen, 2009; Van Looy et al., 2014), but not so much to accommodate declining health (see Section 5.2.3). However, by working part-time individuals may decrease labour market attachment and/or signal to employers a preference for early retirement (Machado and Portela, 2012). Consequently, as the option of early retirement becomes available these workers choose or are induced by their employers to choose this pathway to withdraw from the labour force. The latter process is reinforced by the institutional framework in which TC-beneficiaries are entitled to pension benefits of a full-time worker (Section 3) and for which we report more evidence in the next section. Demand-side factors are less likely to be compatible with the time-pattern of the effects. Graf et al. (2011) argue that subsidized working time reductions before forced early retirement could be used by employers to get gradually rid of less productive older workers. However, in order to generate the initial positive effects on employment this would require that treated individuals would in the counterfactual of no participation be fired. This is unlikely in the Belgian institutional framework in which the employment protection for experienced (older) workers is very strict.

Figure 2: ATT on Survival in Employment



ATT on the survival rate in employment estimated by controlling for the dynamic selection on observables (Eligible-Vikström) and descriptive differences considering all the control individuals (RAW) or only the eligible ones (Eligible) as reported in Figure 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.

To corroborate the interpretation 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 8 in Appendix C, this is indeed what we find.[14]

5.2.2. Competing Exit Destinations

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, other

[14] As the monthly subsidy is lump-sum, 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.

forms of inactivity, and exit because the individual deceased. To simplify the estimation procedure, we do not estimate these competing risks simultaneously, but right censor the other destinations, when considering the destination of interest. We first estimate the competing risks for the whole sample of participants, irrespectively of the TC-regime. After a discussion of the sensitivity of the ATTs to the estimation method, we distinguish between the TC-regime (20% or 50%).

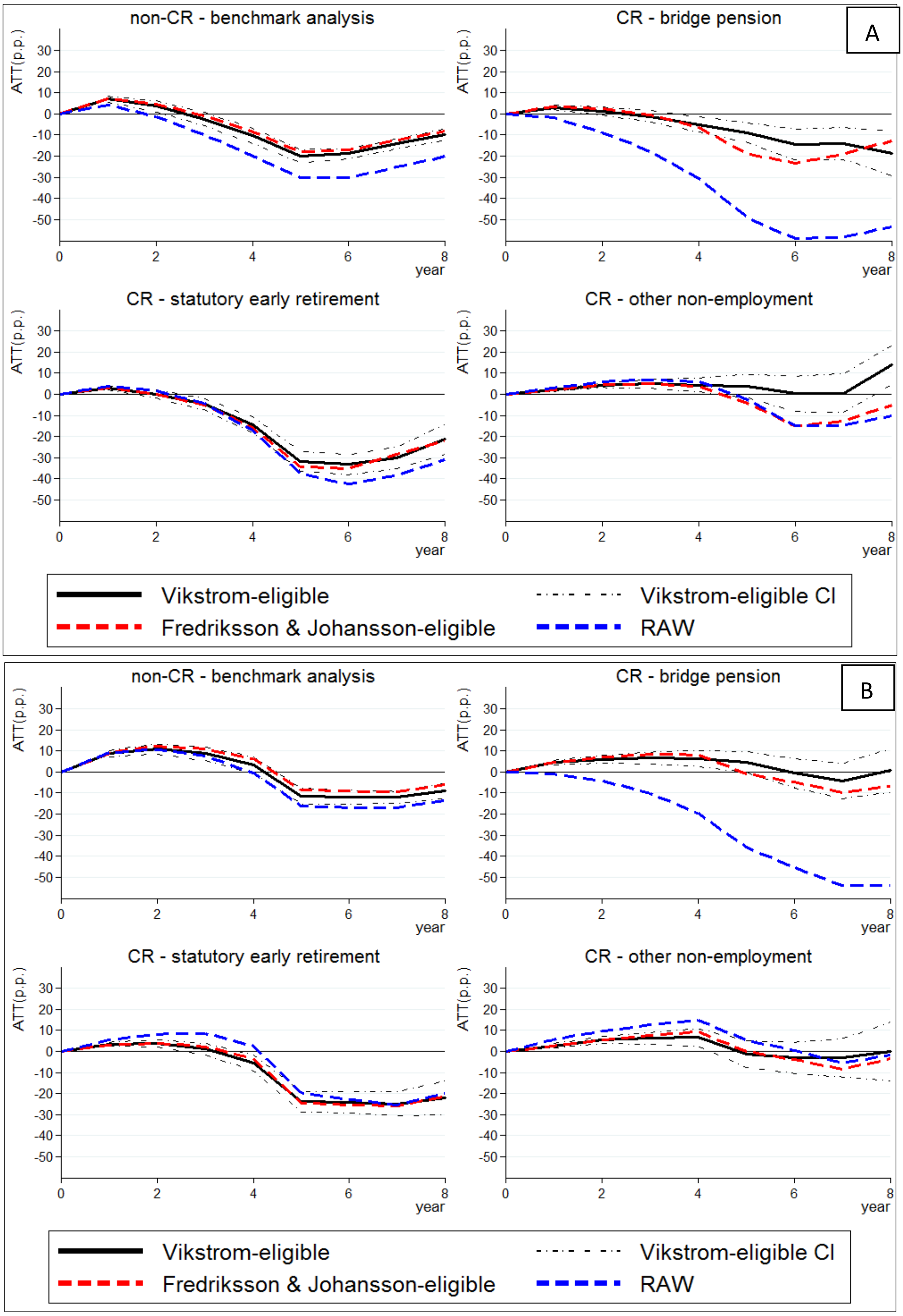
To start, as Figure 3 shows, once we account for the possible exit destinations, the effect for the whole sample of participants irrespectively of the TC-regime is clearly driven by the statutory early retirement option.15 As treated individuals have the same entitlement to the statutory (early) pension 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. Moreover, because women tend to have acquired slightly less labour market experience than men (Table 1), they are less likely to be eligible for early retirement which may explain their better overall effects of TC on the survival rate in employment.

Next, Figure 3 also allows to study 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). We first compare the Vikström (V) estimator to the descriptive estimator without imposing the eligibility condition on the control group (*raw*). This *raw* estimator does not control for differences in observables, nor incorporates dynamic assignment to treatment. We also compare our estimates based on V's methodology to the estimator proposed by Fredriksson and Johansson (FJ). The FJ estimator controls for differences in observables through IPW but ignores dynamic assignment to treatment.

We deduce the following two observations from the figure. First, the *raw* estimates are usually considerably different from the estimators conditioning on the eligibility requirements and the additional observables. This is especially the case for the survival without exit to bridge pension, as workers not eligible to the TC either have more discontinuous work histories or work in sectors where the bridge pension is not commonly used (e.g. public sector or self-employment). Second, the FJ estimator is downward biased relative to the V estimator for the survival in employment without exit to the bridge pension and without other exit.

[15] 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 C.1 of Appendix C.

Figure 3: ATT of Treated men (A) and Women (B) and Comparison with Other Estimators



Vikström-eligible: ATT of treated on the survival rate controlling for the dynamic assignment to treatment (eligible control group) & selective right-censoring on observables (Vikström, 2014); **Fredriksson & Johansson-eligible**: controlling for selection on observables in the year of selection (2003 or 2004 - eligible control group) 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, nor eligibility condition of the control group. 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 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.

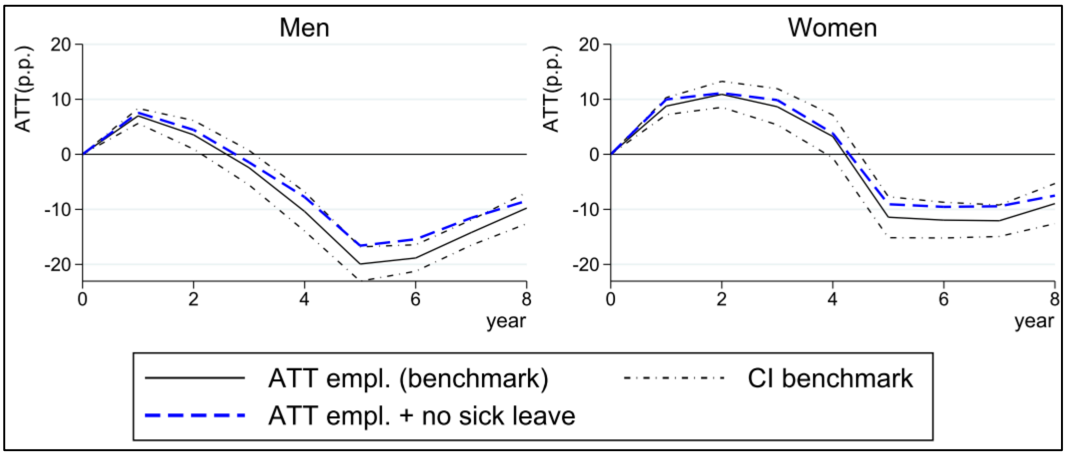
Together with the evidence from the benchmark analysis (Figure 3), these observations lead to the following conclusions. First, in the benchmark model (without competing destinations) the estimates are not sensitive to the inclusion of other covariates once the eligibility condition to the TC scheme are imposed. Additional 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,[16] then this bias can only be avoided by using the V estimator.

Finally, for a better understanding of the role of financial incentives, we divided the treated sample into participants in the 50% and 20% TC-regime (Figure 7). 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. As people in the 50% regime are more intensively treated, the positive ATT in the short-run and the negative ATT in the medium-run are more pronounced compared to the participants in the 20% regime. Once we right censor the exits to *statutory* early retirement, the effect for TC participants with the 50% reduction is non-negative for the other two exit destinations. 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. 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, 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 1), and, hence, have less opportunities to use this pathway to retirement.

[16] 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. 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 4: 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).

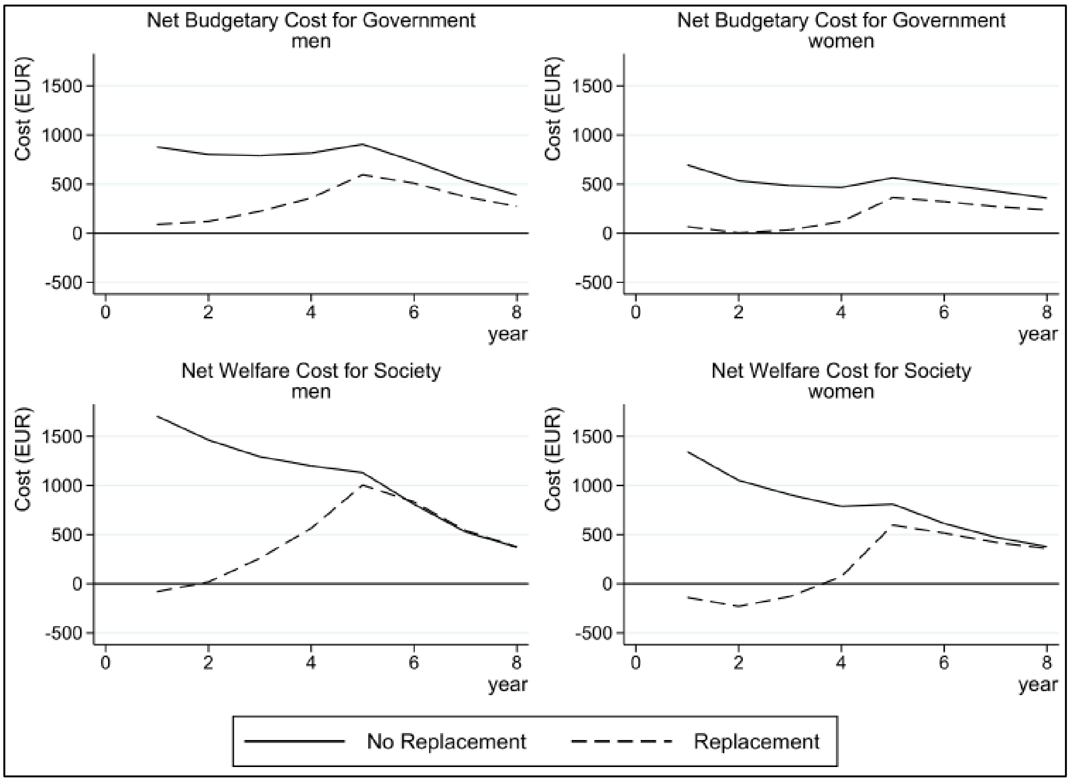
5.2.3. The Impact on Sick Leave Incidence

It has been argued that granting working time reductions can reduce the incidence of sickness (Ahn, 2016). We test this hypothesis in this section. In the administrative data used in this analysis health problems can be identified in one of the following ways. First, individuals can be identified on sick leave if it results in an absence from the workplace of more than one month. This is because in the data only the period that the sickness insurance pays out the replacement income is registered. Prior to this moment the payments during sickness absence are due by the employer. During the period of observation about 16% of the control group and 13% of the treated group experiences at least one such absence, suggesting a slighter lower incidence of sickness for TC-beneficiaries. In the data such absences are not registered as an exit from employment as long as this absence does not last more than one year. Exits to these disability benefits are together with exits to work injury benefits a second way in which the data can identify health problems. However, virtually nobody in the retained sample exits to these destinations. Finally, individuals could stop working because they die, but this involves only a sample share of less than 1%. As the incidence of the last two mentioned health problems is negligeable, we focus our analysis on the first mentioned sickness absence only. Since relatively few individuals entered sick leave, we could not consider sick leave as a separate exit destination in the analysis, because the number of transitions is too small. Instead, we estimate the impact of TC on the survival rate in employment in case sickness absence is considered as an exit and contrast it to the case were it is, i.e. the benchmark (Section 5.2.1). In Figure 4 we report the evolution of these two treatment effects. We observe that the new treatment effect (dashed line) always exceeds the benchmark (solid line). This confirms that TC reduces the incidence of sick leave. However, the difference is small and the dashed line is mostly comprised in the 95% confidence interval of the benchmark ATT, suggesting that the effect is not statistically significant. An explanation for this finding is that the eligiblity criteria for TC weed out individuals with major health problems: such individuals are unlikely to have been full-time employed during the last five years and to have more than 20 years of labor market experience.

5.2.4. Cost-Benefit Analysis

Finally, we perform 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. Overall, as shown in Figure 5, the gradual retirement scheme fails the cost-benefit test. Only under the extreme assumption that employers fully compensate for all working time reductions by hiring other equally productive workers the TC scheme displays a net benefit for society during the first two (four) years for men (women). The assumptions, detailed methodology and results are reported in the Internet Appendix.

Figure 5: 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. No Replacement 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 (unemployed) 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. Year 8 only contains information from the 2003 sample.

6. Conclusion

This paper studies whether the Time Credit (TC) scheme in Belgium, a scheme that facilitates gradual retirement through working time reductions, can lengthen the professional career of older workers. Recently, many EU countries have implemented similar programmes with this aim. However, evidence on the effectiveness of such policies is scarce and provides mixed results. Our research brings new evidence on this question.

Overall our evidence is in line with the findings of Graf et al. (2011) in Austria and Huber et al. (2016) for men in West-Germany, but while in Austria the mechanism seems to be more induced by demand-side factors, in Belgium, as in Germany, supply factors seem to matter more. 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, because participants are not yet eligible for early retirement. However, as soon as they are, participants have much higher incentives to enter early retirement 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.

The positive effects in the beginning are likely present because in the absence of the option of working part-time some participants in TC would have completely stopped working to allow them to improve their work-life balance (Gielen, 2009; Van Looy et al., 2014). However, by working part-time individuals may decrease labour market attachment and/or signal to employers a preference for early retirement (Machado and Portela, 2012). Consequently, as the option of early retirement becomes available these workers choose or are induced by their employers to choose this pathway to withdraw from the labour force. The latter process is reinforced by the aforementioned institutional framework in which the TC-beneficiaries are entitled to pension benefits of a full-time worker. We find that participation in TC slightly improves health conditions, but the effect is statistically insignificant and, hence, not the main driver of the positive effect. We argue that this small effect can be explained by the targeting of the TC on a relatively healthy population for whom no large health improvements can be expected.

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 retire early 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 TC 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.

Appendix

A. Comparison of Gradual or Part-Time Retirement Schemes in Other European Countries

Table A.1: Comparison of Gradual or 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%

B. The (Endogenous) Stratification of the Sample

The population is stratified for each gender in 9 birth cohorts defined in Table B.1. The reference periods by birth cohort were chosen as to observe sufficient transitions in and out of private sector employment for both treatment and control groups determined as to evaluate the 2002 reform mentioned in the main text on the basis of a difference-in-differences strategy (Albanese and Cockx, 2015). Each of these 18 strata is subsequently endogenously stratified in five substrata:

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.

Table B.1 : Retained Birth Cohorts and Corresponding Reference Periods

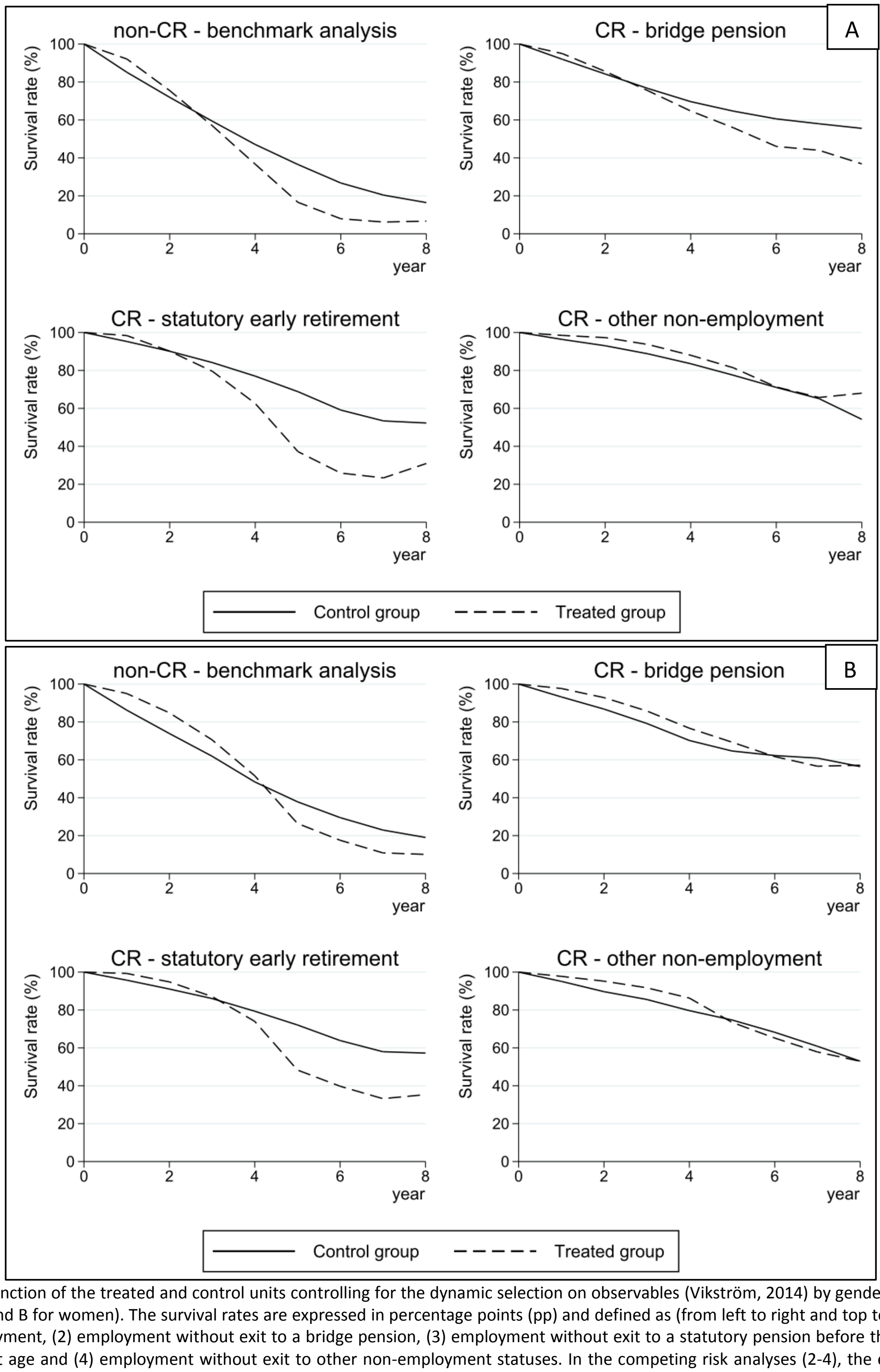
	Cohort (quarter/year)	**Reference Period** (quarter/year)
1	2/41-1/42	[2/99-1/02]
2	2/42-1/43	[2/99-4/01]
3	2/43-1/44	[2/99-4/03]
4	2/44-1/45	[2/00-1/05]
5	2/45-1/46	[2/99-4/03]
6	2/46-1/47	[2/00-4/04]
7	2/47-1/48	[2/00-3/05]
8	2/48-1/49	[2/02-3/05]
9	2/49-1/50	[2/02-3/05]

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.[20] The size of the population is known for each substratum, so that it was straightforward to construct the appropriate weights to make inference on the population.

[20] In cases that the population of the substratum was smaller than the population, the complete population was sampled.

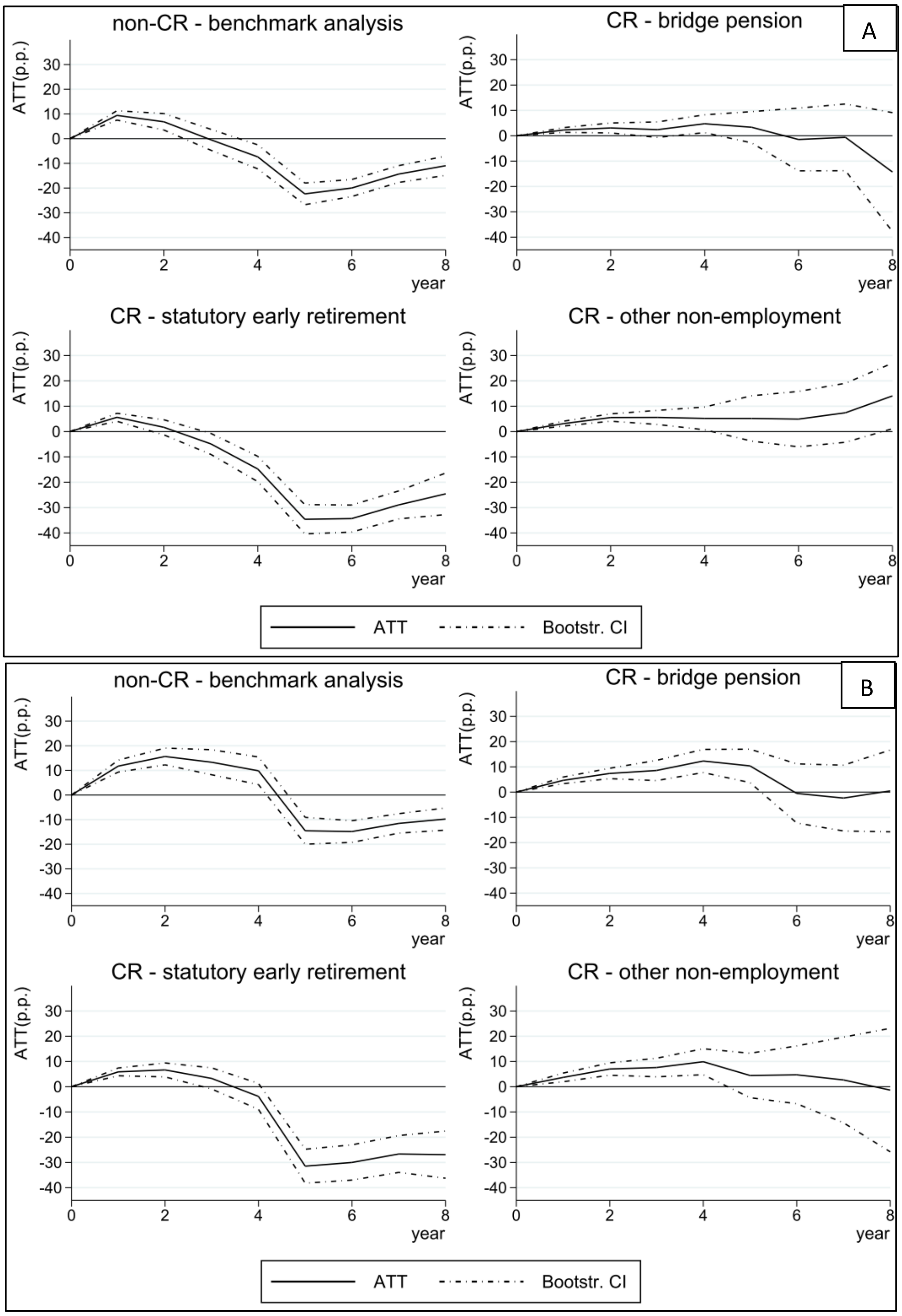
C. Supplementary Tables and Figures

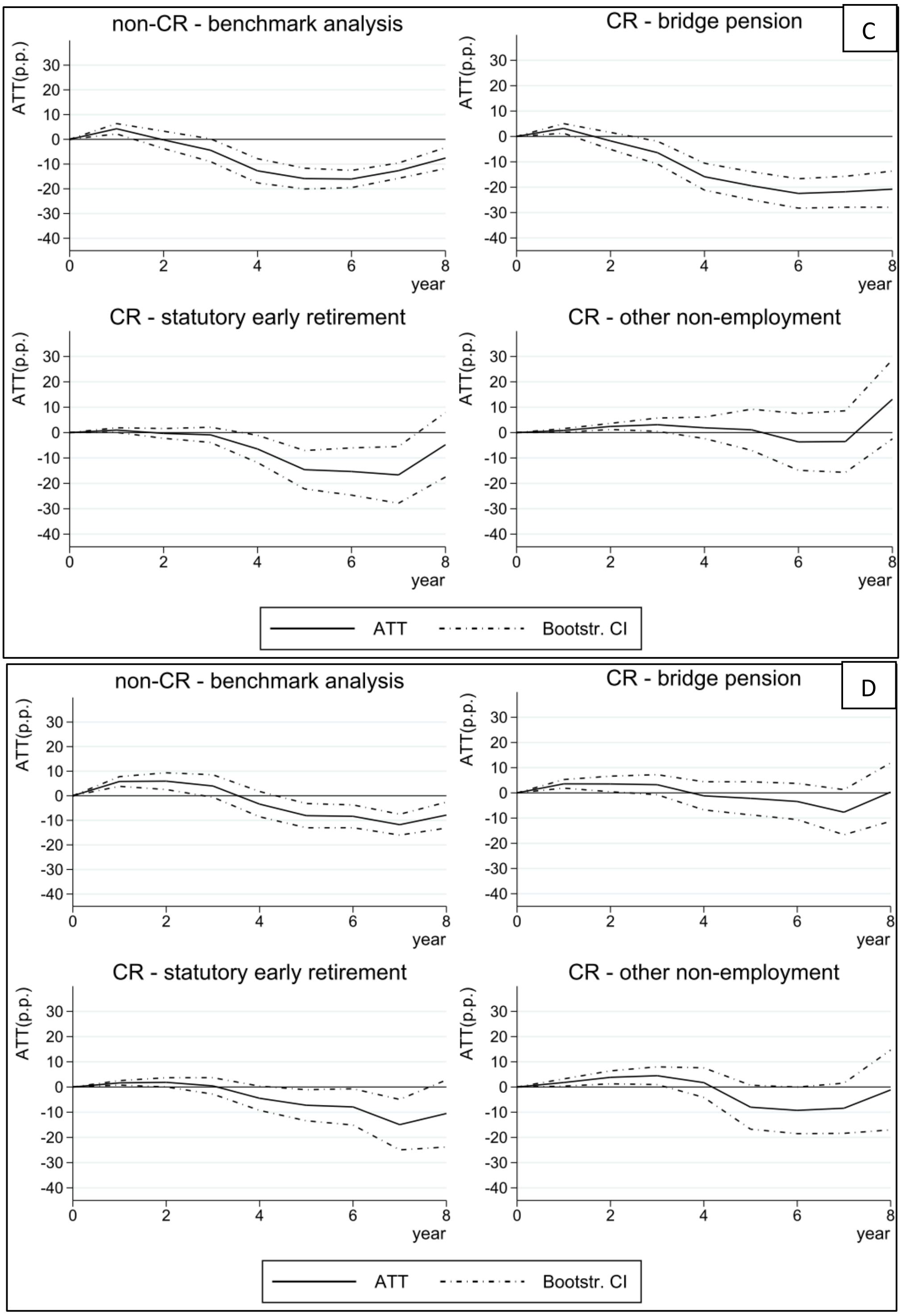
Figure 6: Survival Rate in Employment (Benchmark) and Competing Risks – Men (A) and Women (B)



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 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 7: ATT of Treated Men and Women in 50% (A, B) or 20% (C, D) TC Scheme - Competing Risk (CR) and Baseline (non-CR) Framework





ATT of treated in the 50% TC (A and B) or 20% TC (C and D) 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 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 8: 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.

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