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Do reduced working hours for older workers have health consequences and prolong work careers?

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Abstract

I examine the effects of reduced working hours on various health outcomes. I focus on individuals close to retirement and exploit a reform in part-time pension rules. Using detailed register data on health and job spells together with a difference-in-differences approach, I find that an earlier eligibility age for part-time pension program increased purchases of prescription drugs by approximately 1.0 percentage point over the following 6 years. In relative terms, this effect is small, around 2%, but is economically significant as drug purchases are largely subsidized by the state. However, looking at the long-term effects I do not find effects on mortality or severe health diagnoses. I also look at labour market exits and find that the reform did not reduce the risk of early withdrawal from the labour market.

Key words: part-time pension, health, eligibility age reform, work hours
JEL-codes: J26, I10

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1 Introduction

Population ageing in developed countries is challenging the sustainability of public pension systems. On the other hand, a growing share of elderly persons is increasing the healthcare costs. To finance this increasing public expenditure, many nations are promoting policies that aim to make people work more. A common policy tool to lengthen careers has been to increase the eligibility ages for early and normal retirement (Börsch-Supan and Coile, 2018). However, health problems, both physical and mental, lead to early and permanent withdraw from the labour market before the statutory retirement ages. The prevalence of disability increases with age: a quarter of people between the ages 50-64 report a chronic health problem which is over twice the share reported in younger age groups (OECD, 2010).

Policies that improve the quality of work for older workers and thus increase the age of labour market exits are important for the sustainability of pension systems. Gradual retirement has been proposed as one way of adapting to declining health, and different types of gradual retirement schemes are in place, especially in several EU countries (Eurofound, 2016). In gradual retirement workers do not end their career abruptly in full retirement; rather they reduce their labour supply in a stepwise manner. Gradual retirement could benefit workers by slowing down the wear and tear of body and mind. Existing literature[1] has shown that increasing normal working hours worsens subjective health measures and increases medical appointments (Cygan-Rehm and Wunder, 2018) and increases the probability of worse health behaviour, such as smoking, (Ahn, 2016; Berniell and Bietenbeck, 2020) while shortening regular weekly working hours reduces work-related injury rates (Lee and Lee, 2016) and improves worker well-being (Lepinteur, 2019). However, there is little evidence on how a substantial reduction in normal working hours affects the health of older workers which is where the current study aims to contribute. A recent study looked at the health effects of elderly Norwegian teachers' reduced workload (reduction in taught classes while keeping contracted hours and income the same), finding only small positive health effects among male teachers (Bratberg et al., 2020). In this study, however, the potential work hour reduction was very small, approx-

[1]There are also meta-studies finding that there is a positive association between long working hours and cardiovascular disease as well as increased risk of stroke (Virtanen et al., 2012; Kivimäki et al., 2015) and depressive symptoms (Virtanen et al., 2018). See also a review article on the health effects of work by Bassanini and Caroli (2015).

imately 1 to 1.5 hours per week. The current study looks at the effects of part-time work on health-related factors.

Gradual retirement has the potential to be welfare improving if the reduction in working hours improves health outcomes, facilitates continuation of working careers and thus increases lifetime utility compared to early withdrawal from the labour market. However, for the sustainability of the pension system, gradual retirement schemes can either improve or deteriorate fiscal stability depending on how part-time working affects the total lifetime labour supply. Some workers who reduce their working hours would have retired fully without a gradual retirement option, while some would have continued as full-time workers. Previous studies have compared the employment responses between gradual retirees and full-time workers and concluded that working careers and total hours of work were not significantly increased (Albanese et al. (2020) for Belgium, Graf et al. (2011) for Austria, Huber et al. (2016) for Germany). However, a recent study finds that in the German setting encouraging partial retirement did postpone final labour market exit by at least 0.6 years for men and 1.1 years for women (Berg et al., 2020).

In Finland, between the years 1987-2016, a part-time pension program enabled individuals to reduce their full-time working hours to 16-28 weekly hours with only a small reduction in disposable income or future pension entitlements. Kyyrä (2015) finds that part-time pension eligibility did not reduce the odds of being granted a disability pension. In this paper, I complement this analysis in two ways. I study the effects of this gradual retirement scheme on health-related outcomes and I study the early labour market exit risk within the population who have taken a part-time pension[2]. I exploit the cohort eligibility age variation and difference-in-differences setting to identify causal effects. Before 1998 the eligibility age for part-time pensions was 58 but was reduced temporary to 56 for the years 1998-2002. As most part-time pensioners reduce their work amount exactly at the lowest eligibility age, effectively this reform lengthened the period of reduction in working hours for the treatment cohorts born in 1943-1946. The data include all part-time pensioners born between 1940-1946 who took a part-time pension between 1998-2005. The outcome variables, measured with administrative data, are prescription drug pur-

[2]Kyyrä (2015) focuses on the effects of changing the age thresholds for unemployment and part-time pension schemes. For part-time pensions he compares all workers born in 1942-1946 (eligibility age 56 or 57) at ages 56 and 57 to cohorts born in 1947-1948 (eligibility age at 58) at the same ages. Hence, he identifies the eligibility effect, while I use also cohort variation in eligibility ages within a sample of part-time pensioners.

chases and diagnosis, mortality and labour market exits. The health data include the code for the drug or disease used to denote various causes of ill-health. The dataset also includes administrative records on individual demographics and data on other pension spells as well as earnings information.

I first show that the parallel trends for my treatment and control cohorts hold pre-reform which validate, together with the assumption that the reform was exogenous, that the estimates can have a causal interpretation. I find that the reform increased purchases of prescribed medication in the short term, especially for musculoskeletal diseases. The effects are strongest for women and people in manual occupations. While the effects are generally small, the probability of any drug purchases increased by 2% relative to the pre-reform mean of the treatment group. This increase is still economically significant as drug purchases are largely subsidized by the state. There is no earlier point of reference on the effects of reducing working hours and prescription drug purchases. For full retirement, however, Hagen (2018) finds for female public sector workers in Sweden that raising the normal retirement age did not affect prescription drug purchases. The positive effect I find might be explained by the fact that health investments are more important at the end of career in order to enable continued working. Part-time work lowers the opportunity cost of time and a longer working horizon (for the treatment group) increases the return for health investments which shows up as increased healthcare utilization. This is also supported by earlier findings that there is a negative association between working hours and healthcare utilization (Fell et al., 2007; Yao et al., 2015). As regards long-term effects, I fail to reject the null hypothesis of no causal effect of working part-time on mortality (up to age 68), severe or chronic diagnosis or prescription drug utilization at retirement, which indicates that individuals were tending to existing and minor health conditions at the end of their career. The results also suggest that increased leisure time at the end of careers did not reduce the risk of early labour market exit.

This Finnish part-time pension scheme is well suited for studying the effects of a reduction in working hours: unlike in many other gradual retirement schemes where workers can take part of their pension wealth before full retirement without an obligation to reduce their labour supply, the Finnish part-time pension system forced workers to reduce their working hours substantially. Also, contrary to bridge jobs common in the US, in which workers change their job task or place of work to reduce their work burden before full retirement, in the Finnish context most of the individuals continued in their career

jobs. Lastly, the scheme was very generous in its pension coverage so it had very modest effects on disposable incomes or future pension rights. An additional benefit in the Finnish setting is that there is universal health insurance coverage with high prescription drug coverage, ruling out possible selection based on income and prescription drug purchases. With these features of the program we can identify the effects of working hours reduction instead of income or work place effects.

This paper contributes to the literature on the relationship between the amount of work (or non-work) on health and retirement among older workers. In the context of older workers, existing literature mostly focuses on the effects of full retirement, where the results are mixed, likely due to the variety of definitions of health as well as the variety of research settings (Hernaes et al. (2013); Atalay and Barrett (2014); Hallberg et al. (2015); Bloemen et al. (2017); Heller-Sahlgren (2017); Mazzonna and Peracchi (2017); Hagen (2018); Blake and Garrouste (2019); Messe and Wolff (2019); Nielsen (2019); Carrino et al. (2020); Grøtting and Lillebø (2020); Kuhn et al. (2020); Kuusi et al. (2020); Bozio et al. (2021), for a review see Nishimura et al. (2018)). Yet it is plausible that working hours have a different effect on health in the interval between part-time and normal working hours, which is a less studied topic. I contribute to this field of study by using administrative registers which provide a relevant and directly related measure from a public finance perspective.

The paper proceeds as follows: section 2 goes through the institutional setting and section 3 the data used. Section also introduces the empirical strategy. Section 4 presents the results. Section 5 concludes.

2 Part-time pension program and old-age security

The Finnish pension system includes a residence-based national pension and earnings-related pensions. The eligibility conditions for part-time pensions rule out individuals with short careers and with low accrued pension levels and so only the earnings-related part of the pension system is relevant in this context.[3] The earnings-related pension scheme in Finland is a mandatory defined benefit scheme.

The part-time pension program was part of the earnings-related pension system until

[3]Voluntary pension plans play a minor role in Finland.

2017 and was introduced in 1987 for private sector workers to increase flexibility at the end of careers. The age limit was set at 60 while the old-age retirement age was 65. Public sector workers have been eligible for part-time pensions since 1989, but their age limit was originally set to 58. In 1994 the eligibility ages were harmonized for both sectors and set at 58. In July 1998 the age limit was reduced to 56. This reduction to 56 was temporary to experiment whether it would lengthen working careers and especially decrease the risk of early retirement and was put back to 58 at the beginning of 2003. The reform of 1998 also added a clause which obliged employers to arrange part-time working whenever possible. (Government proposal, 1998).

To be eligible for a part-time pension, a person needed to fulfil the work, pension accrual and age conditions. Besides the minimum age limit, part-time pensions were only available for those under 65 years of age during the observation period. For private sector workers, the work condition required 12 months of full-time work during the preceding 18 months and for public sector workers the requirement was 6 months of full-time work during the preceding 18 months. The pension accrual condition required workers to have been accruing pension rights for 5 years during the preceding 15 years in the private sector and 3 years during the past 5 years in the public sector.

At the start of their part-time pension employees needed to change from full- to part-time work. Their hours and earnings needed to decrease in the same proportion, being in the range of 30-75% of their full-time work. Working hours had to be at least 16 but at most 28 hours per week. Earnings and hours worked were monitored by the pension-provider. Hours worked are not observable from the data. However, comparing the pre- and post-wage levels suggests that the reduction in hours is about 45 percent. This is also in line with the results of surveys of part-time pensioners (Takala, 2004).

The pension received in the part-time retirement was 50% of the difference in earnings between full-time work and part-time work, however it could not exceed 75 percent of the accrued pension. The reduction in disposable income was not proportional to the decrease in earnings because the tax rate for part-time pensioners was lower than for full-time pensioners or full-time workers conditional on income.[4] The difference between full-time and part-time earnings also accrued future pension rights, with an accrual rate of 1.5%. In full-time work (and for wages earned as a part-time pensioner) the accrual

[4]From the data, the ratio of net incomes during and before part-time pension is 88% which is in line with previous studies (Takala, 2004).

rates were 1.5% for individuals below the age of 60 and 2.5% for individuals aged 60 or older. All in all, the effect of part-time pensions on lifetime earnings was modest.

At the end of 1998 there were about 11 000 persons in the part-time pension program. The popularity of the program grew over time and at the end of 2002 there were about 40 000 participants. As a share of employees aged 55-64, participation increased from 5% to 13%.

At the end of the 1990s and early 2000s the Finnish pension system included multiple early exit pathways, such as partial and full disability pensions, and individual early retirement pension which was granted for older individuals with health conditions. Figure 1 presents the pension eligibility ages by cohort. A major pension reform in 2005 abolished the individual early retirement pension but older individuals were granted disability pension with similar health conditions as before. The 2005 reform decreased the full old-age retirement age from 65 to 63. This reform may have had independent effects on health outcomes and affected labour market exit decisions. For this reason the main focus in this paper is on short term estimates of the working hours reduction, i.e., the effects of the reform are measured up to the year 2004 and for labour market exits the focus is on early labour market exits.

Figure 1: Eligibility ages for different pension schemes.

The different pathways to retirement make part-time pensioners a selective sample of older workers. Individuals with strong preferences for working but limited working ability probably seek part-time pension possibilities, while for some with stronger preferences

for leisure disability or individual early retirement are more likely options. This fact is accentuated by the requirement that the working hours arrangement had to be agreed with the employer.

3 Data and empirical setting

3.1 Data and sample

I use administrative data collected and administered by the Social Insurance Institution of Finland (Kela), Statistics Finland and the Finnish Centre for Pensions (ETK) for the years 1995-2014. The sample of part-time pensioners includes all individuals in the 1940-1946 cohorts who started a part-time pension spell between the years 1998-2005.[5] The sample size is 50 351 individuals. Part-time pension and subsequent retirement from the labour market is defined as claiming either a part-time pension or other pension type. In the Finnish Centre for Pensions data these are defined by the exact day of the start and end of the spell together with a past earnings history and level of pension. Over 98% of the pensioners have only one part-time pension spell and so I define the take-up as the start of the first spell.

The dataset includes also information on the sample of workers without any part-time pension spell. This sample consists of 169 600 individuals who represent a similar gender and cohort distribution as the part-time pensioners sample. The exit from the labour market for these individuals is observed on an annual level based on income items and not on the exact day of the start of claiming the pension as for the part-time pensioners sample. The other information is similar between the two samples of workers. This sample is used in the robustness analysis and to look at the long-term effects.

Individual health outcomes are measured with the Social Insurance Institution's data. The information about drug utilization come from the register of purchases of prescription drugs, which contains all prescription drugs sold by Finnish pharmacies. The information available is the drug code (ATC codes) with the day of purchase and covers the whole observation period. During the observation period, the drug remuneration system

[5]For the youngest cohorts I do not observe the whole cohort of part-time pensioners because of this data restriction. This affects around 10% of the 1946 and 1945 cohorts of part-time pensioners. If there is health selection in the timing of taking a part-time pension, this causes bias in my results. To deal with this, in the robustness analysis, I drop these cohorts in the treatment group.

changed very little. The outcome variables with respect to the drug data are purchases of any positive amount of medicine (referred to as the *extensive margin* in the result section) and the intensity of medicine purchased measured as the number of packages purchased during a year (referred to as the *intensive margin* in the result section). From the Kela database I also have information on severe and chronic diseases and the number of sickness days exceeding the first 10 days of each sickness spell.

The individuals' background information is collected from the Statistics Finland data. This includes demographic information, income[6], education and occupation-related data. The industry classification is based on the nationally modified version of the European Union's standard industrial classification (NACE classification). The education information includes the level of education. Also, all deaths are observed until 2014, which means that the survival of the oldest cohort of 1940 is censored at the age of 74 and the survival of the youngest cohort of 1946 is censored at the age of 68.

Table 1 shows descriptive information on the part-time pensioners and similarly aged workers for the years 1995-1997, i.e. before the part-time pension reform. From the table we see that the part-time pensioners are not a representative subgroup of the population of oler workers. The part-time pensioners are more educated, more often married and earn more than their peers in the same cohorts. They are overrepresented in professional occupations, public administration and education. Also, the baseline health outcomes are more favourable for the part-time pensioners as they have fewer sickness absences and they purchase less medicine in a year.

Table 2 shows descriptive statistics for the treatment and the control cohorts in the part-time pensioners sample. The treatment cohorts are those born between July 1942 and December 1946 (eligibility age for part-time pension at age 56) and the control cohorts are those born between January 1940 and June 1942 (eligibility age above 56).[7] The treatment and control groups are similar in terms of several background characteristics, including gender, being married, living in the capital region and having a tertiary education degree. As the prevalence of illness increases with age, we see that the older cohorts (control cohorts) have somewhat more sickness absence days and drug purchases within a year during the pre-reform era. Over half of the individuals have used some

[6] All income variables are deflated to year 2014 using the cost of living index.

[7] In the robustness analysis a control group defined as the cohorts born between January 1940 and June 1940 (eligibility age at exact 58) is also used.

prescription medication.

Table 1: Descriptives for pre-reform 1995-1997 period, part-time pensioners (PP) and non-part-time pensioners (Non-PP)

	PP	Non-PP
Background		
Female	0.56	0.56
	(0.50)	(0.50)
Married	0.84	0.77
	(0.37)	(0.42)
Capital region	0.24	0.18
	(0.43)	(0.38)
Tertiary	0.17	0.094
	(0.38)	(0.29)
Employment and income		
Wage income	34412.2	18509.1
	(17688.4)	(21263.6)
Net income	25523.3	18922.2
	(22620.3)	(20401.2)
Employment months	11.6	7.54
	(1.98)	(5.55)
Occupation		
Managers	0.052	0.032
	(0.22)	(0.18)
Professionals	0.20	0.092
	(0.40)	(0.29)
Technicians and associate professionals	0.19	0.12
	(0.39)	(0.33)
Clerical support workers	0.14	0.069
	(0.34)	(0.25)
Service workers	0.11	0.10
	(0.32)	(0.30)
Crafts workers	0.095	0.064
	(0.29)	(0.24)
Plant operators	0.078	0.070
	(0.27)	(0.25)
Elementary occupations	0.098	0.062
	(0.30)	(0.24)
Industry		
Manufacturing	0.20	0.11
	(0.40)	(0.31)
Transportation	0.064	0.044
	(0.25)	(0.21)
Finance and insurance	0.033	0.017
	(0.18)	(0.13)
Professional services	0.099	0.050
	(0.30)	(0.22)
Public administration	0.095	0.040
	(0.29)	(0.20)
Education	0.12	0.050
	(0.33)	(0.22)
Health and social services	0.14	0.10
	(0.35)	(0.30)
Health		
Sickness days >10, yearly	3.85	7.18
	(16.8)	(34.2)
Any drug purchases	0.55	0.56
	(0.50)	(0.50)
Drug purchases, year	2.24	2.77
	(3.22)	(4.29)
Individuals	50 351	169 600

Note: Table includes information on the 1940-1946 cohorts, the part-time pensioners (PP) sample and the non-part-time pensioners (Non-PP) sample. Only the largest industries and occupations are shown. Income items in 2014 money.

Table 2: Descriptives for pre-reform 1995-1997 period, treatment and control groups

	Control	Treatment
Background		
Female	0.57	0.56
	(0.50)	(0.50)
Married	0.82	0.85
	(0.38)	(0.36)
Capital region	0.25	0.24
	(0.43)	(0.42)
Tertiary	0.19	0.17
	(0.39)	(0.37)
Employment and income		
Wage income	34983.0	34197.9
	(17210.3)	(17861.5)
Net income	25738.1	25442.5
	(12576.8)	(25399.6)
Employment months	11.6	11.6
	(2.02)	(1.97)
Occupation		
Managers	0.055	0.050
	(0.23)	(0.22)
Professionals	0.21	0.19
	(0.41)	(0.39)
Technicians and associate professionals	0.20	0.19
	(0.40)	(0.39)
Clerical support workers	0.13	0.14
	(0.34)	(0.35)
Service workers	0.11	0.11
	(0.32)	(0.32)
Crafts workers	0.080	0.10
	(0.27)	(0.30)
Plant operators	0.072	0.080
	(0.26)	(0.27)
Elementary occupations	0.10	0.095
	(0.31)	(0.29)
Industry		
Manufacturing	0.19	0.21
	(0.39)	(0.41)
Transportation	0.061	0.066
	(0.24)	(0.25)
Finance and insurance	0.025	0.036
	(0.16)	(0.19)
Professional services	0.098	0.100
	(0.30)	(0.30)
Public administration	0.10	0.092
	(0.30)	(0.29)
Education	0.14	0.11
	(0.35)	(0.32)
Health and social services	0.14	0.14
	(0.35)	(0.35)
Health		
Sickness days >10, yearly	4.12	3.75
	(17.0)	(16.7)
Any drug purchases	0.59	0.54
	(0.49)	(0.50)
Drug purchases, year	2.49	2.15
	(3.32)	(3.17)
Individuals	13 792	36 551

Note: Treatment group includes part-time pensioners in the 1942/7-1946 cohorts and control group includes part-time pensioners in the 1940-1942/6 cohorts. Only the largest industries and occupations are shown. Income items in 2014 money.

3.2 Empirical strategy

In July 1998 the eligibility age for part-time pensions was lowered to 56. To identify the causal impact of this reform, I compare cohorts under the new legislation, the "treatment" cohorts, i.e individuals born between July 1942 and December 1946, and the "control" cohorts, those whose eligibility age was above the age of 56, i.e. born between January 1940 to June 1942[8] before and after the reform year 1998. The estimated difference-in-differences equation is the following:

$$Y_{it} = \alpha + \beta_1 POST_t + \beta_2 TREAT_i + \beta_3 POST_t \times TREAT_i + \boldsymbol{X}'_{it}\delta + \gamma_c + \varepsilon_{it}. \quad (1)$$

Y_{it} refers to the outcome of individual i at time t and X_{it} is a vector of control variables which include education, gender, earnings, industry and pre-reform health outcomes. $POST$ is a dummy getting value one if the year is post 1998 and zero for years prior to 1998[9] and $TREAT$ is a dummy getting value 1 for individuals in the treatment cohorts. The equation also includes cohort-fixed effects γ_c. β_3 reveals the reform effect. The estimated effects are intention-to-treat (ITT) effects as a part-time pension could be taken at any age between the eligibility age and the old-age retirement age.

To identify a causal effect we need to assume that the reform was exogenous and that without the reform the differences between the treatment and control cohorts would have remained the same (parallel trend assumption). The first assumption is likely valid as the lowering of the age limit was legislated on a temporary basis to test whether the eligibility age has an effect on early exits. The second assumption is evaluated by estimating an event-study type of model where $POST$ is replaced by year terms as

$$Y_{it} = \alpha + \sum_{t=1995}^{2004} \beta_{1,t} Year_t + \beta_2 TREAT_i + \sum_{t=1995}^{2004} \beta_{3,t} Year_t * TREAT_i + \boldsymbol{X}'_{it}\delta + \gamma_c + \varepsilon_{it} \quad (2)$$

where $\beta_{3,t}$ is the estimated difference between the treatment and control group in each year conditional on the vector of control variables and cohort fixed effects normalized to year 1997 (one year before the reform). In the result section the coefficients of this

[8] In the robustness analysis I also limit the control group to those whose eligibility age was exactly 58, i.e. people born between January 1940 to June 1940.

[9] As the reform was implemented in the middle of the year, I drop the year 1998 from the estimation. However, in the graphs showing the parallel trends this year is included as well.

estimation are presented in graphs to show the validity of the parallel trend assumption before the reform. These graphs also help to evaluate whether the impact is immediate or delayed.

I cannot compare the treatment and control cohorts at the same age as the relevant data does not exist for the control group before age 55. So instead I compare the treatment and control cohorts across the years when the trial was implemented and for the pre-reform years since 1995. Two potential threats to the validity of the results arise from this: firstly my treatment group includes more cohorts as time goes by, and secondly differential trends in health trajectories between cohorts might bias the result. For the first issue I test whether excluding the youngest cohorts from the treatment has an effect on my results, meaning that in this robustness check the treatment and control cohorts become eligible around the same years. I show in the results section that the point estimates are close to the main specification. For the second issue I study the effects in a triple difference framework using non-part-time pensioners as an additional comparison group and show that the results are robust to this specification as well.

3.3 Expected effects

Grossman (1972) model for health demand predicts that a reduction in working hours can either increase hours spent on health investments (going to doctor, exercising more or eating healthier etc.), which increases health stock, or increase the time spent on consuming other goods which can be detrimental to health. In other words, it is not clear what the expected effects of reducing working hours are.

Working hours can be related to health via several channels. For example, work can contribute to the allostatic load, which accumulates as an individual is exposed to chronic stress. On the other hand, the increase in professional work has raised the question of how office work affects an individual's musculoskeletal system. To take into account these different aspects of ill-health I divide the drug data into subgroups representing different cause-specific illness groups.[10]

The selected drug categories are known to have an association with work-related factors based on earlier literature. These include respiratory diseases (e.g lung diseases), cir-

[10]Table A1 in the appendix lists the classification used in forming these cause-specific health conditions with the ATC codes.

culatory diseases (e.g hypertension), heart diseases, cerebrovascular diseases (e.g. stroke), diseases of the musculoskeletal system, diabetes and mental illnesses. While respiratory diseases and diseases of the musculoskeletal system have a clearer direct link to the occupational environment, diseases of the circulatory system, heart and diabetes have an indirect link to work by increased stress, which is also associated with worse dietary and exercise habits. In the mental illness drug category are drugs that treat depression, anxiety and sleeping disorders.

If a reduction in working hours increases health investments, this would decrease consumption of those drugs used to treat diseases which are linked to lifestyle habits, such as drugs for circulatory and heart-related diseases as well as diabetes, while the opposite would happen if increased leisure time is spent on actions that are detrimental to health. We could also expect that mental health improves if allostatic load lightens.

4 Results

4.1 Main results

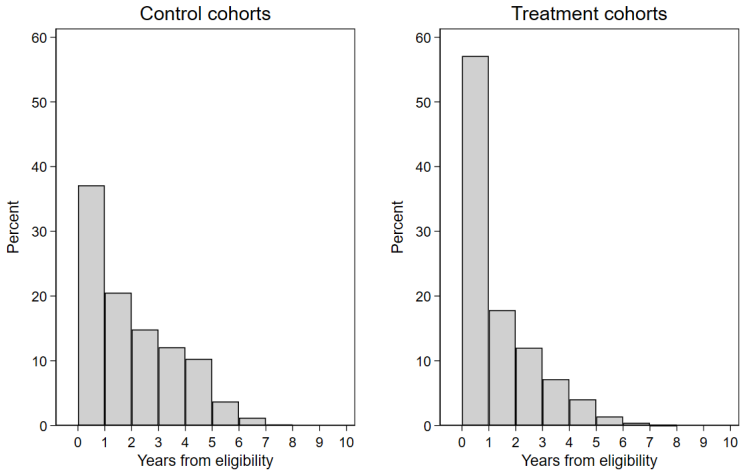
4.1.1 Take-up of part-time pension

I start by examining the take-up of part-time pensions and what happens to income in the treatment and control cohorts as individuals shift to part-time work. Most (53%) of the part-time pensioners start claiming a part-time pension during the first year of becoming eligible (Figure 2a). The average length of part-time pension spells is 4.3 years. When the eligibility age was decreased in 1998, the length of the average part-time pension increased by approximately 1 year, as seen in Figure 2b.

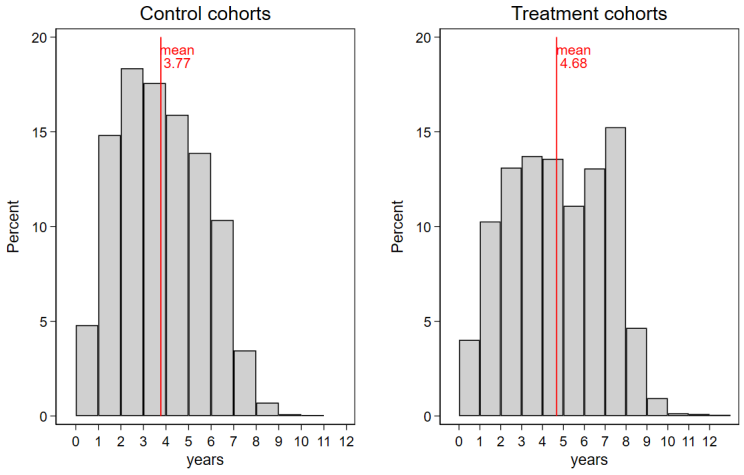
Figure 3 shows the evolution of wage income by age for the treatment and control groups relative to income at the age of 55.[11] Wage income steadily decreases as more individuals shift to part-time work. From Appendix Figure A1 we see that the effect on disposable income is more muted as the part-time pension combined with the tax system was very generous. From this we can suspect that the income effect does not dominate the effects on health, but in the subsequent analysis we mostly observe the effects of the

[11] Appendix Figure A1 shows the same graph by cohorts. Younger cohorts have slightly larger drops in wage income earlier, which is largely due to the fact that younger cohorts comply more with the part-time pension eligibility age than older cohorts.

Figure 2: Start of part-time pension since becoming eligible and the length of part-time pension spell.



(a) Timing of part-time pension



(b) Length of the part-time pension spell

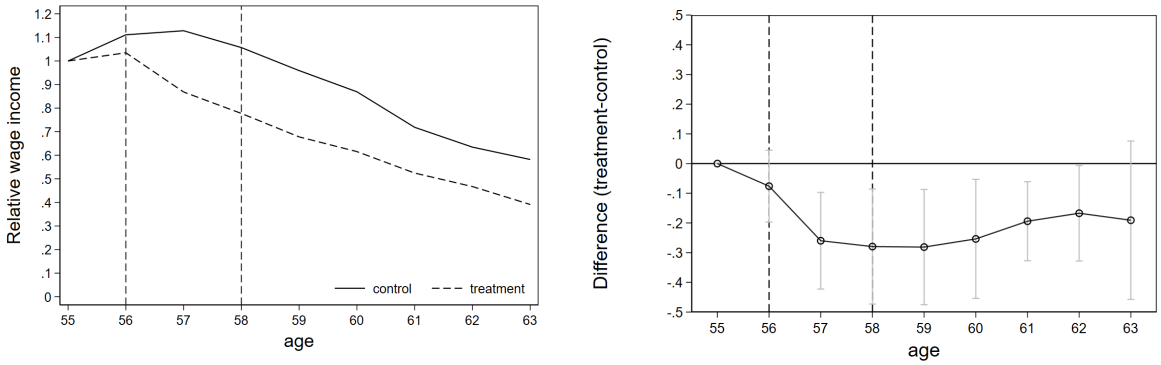
Note: Control cohorts (born between 1940-June 1942) and treatment cohorts (born July 1942-1946). Total number of individuals is 50 351.

working hours reduction.

As the rules for the of part-time pension require that earnings and hours are reduced in the same proportion, we can proxy the hours reduction with wage income. Comparing

wages before and after part-time pension, I get a proxy for the working hours reduction, which on average is 45%. However, in the previous figure, the reduction in hours is masked by the fact that not everybody in the sample takes up a part-time pension exactly at the age when becoming eligible explaining the persistent difference in Figure 3b. I replicate the previous figure for the subpopulation of compliers, who are defined as taking up a part-time pension within a year of becoming eligible. In Figure 4 we see that the drop in wage income is more pronounced and is highlighted at the age of 57 following a drop in wage income for the control group approximately one year later. For this population there are no persistent differences in relative wages for subsequent ages indicating that the difference between the two groups in the labour market stems from the different timing of part-time pension take-up.

Figure 3: Wage income relative to age 55, all part-time pensioners



(a) Wage income

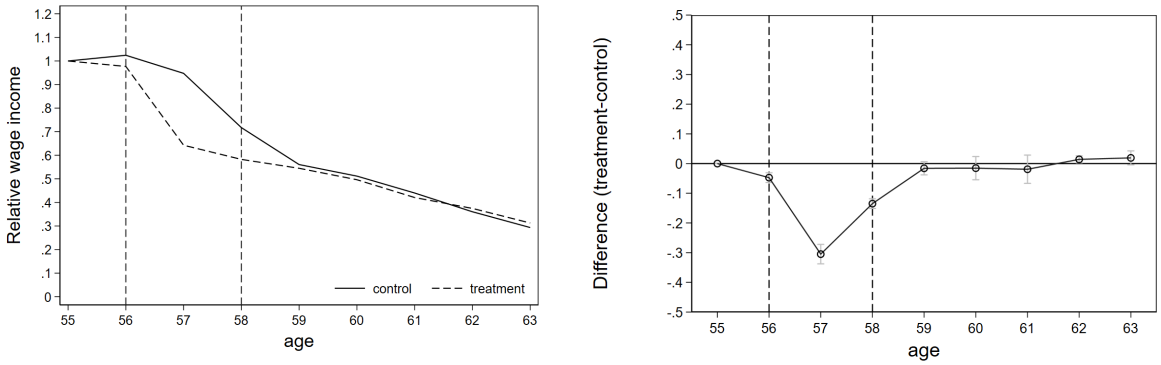
(b) Difference

Note: The figure includes all part-time pensioners. There are 13 792 individuals in the control cohorts and 36 551 individuals in the treatment cohorts. Panel b presents the difference between treatment and control with 95% confidence intervals. The dashed vertical lines denote the old eligibility age of 58 and new eligibility age of 56.

4.1.2 Reform effects on prescription drug use

Figure 5 shows the trends and difference-in-differences estimates for drug utilization. As people age, drug purchases, both at the extensive and intensive margin, grow explaining the trend and the difference between the control and treatment groups. From the trends it is difficult to observe significant differences in the trends between the groups, and thus

Figure 4: Wage income relative to age 55, compliers



(a) Wage income

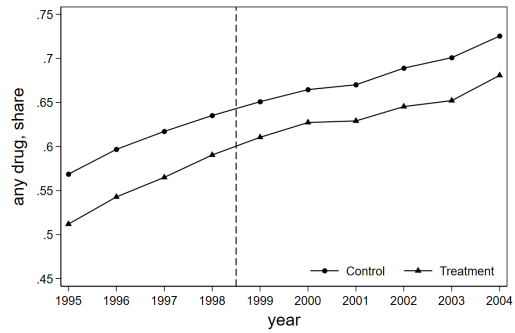
(b) Difference

Note: The figure includes only part-time pensioners who takes up part-time pension within one year of becoming eligible, i.e. the "compliers". There are 5 234 individuals in the control cohorts and 21 130 individuals in the treatment cohorts. Panel b presents the difference between treatment and control with 95% confidence intervals. The dashed vertical lines denote the old eligibility age of 58 and new eligibility age of 56.

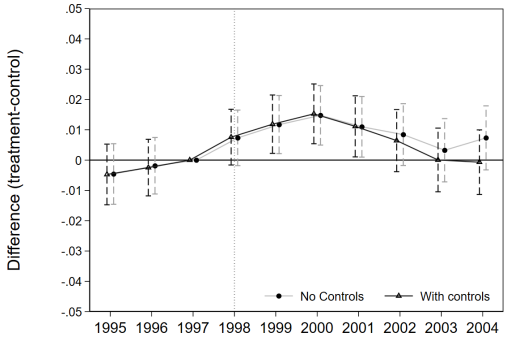
in the right panel I present the coefficients from estimating Eq. 2. The pre-trends for the extensive margin show that before the reform the trends between the treatment and control groups did not differ statistically significantly from zero with 95% confidence intervals, while after the reform the treatment group did purchase slightly more prescription drugs. For the intensive margin of prescription drug use there is a statistically significant difference for pre-reform year 1995 but the difference is extremely small, around one tenth of a package, which is also the maximum effect on the post-reform period. From these graphs we observe that the meaningful margin of response is the extensive margin. In the Appendix Figure A2 show the same graph excluding the younger cohorts. The results are qualitatively similar with the observation that the peak is earlier, indicating that the increase in purchases takes place at the beginning of the part-time pension spell.

The results from estimating the difference-in-differences coefficients from Eq. 1 are presented in Table 3. The coefficient on the effect of the reform measures the effect of lowering the eligibility age on each of the health outcomes given in the column heading. I add an additional outcome measuring the probability of buying large amounts of prescription drugs. This is defined as buying prescription drugs above the median num-

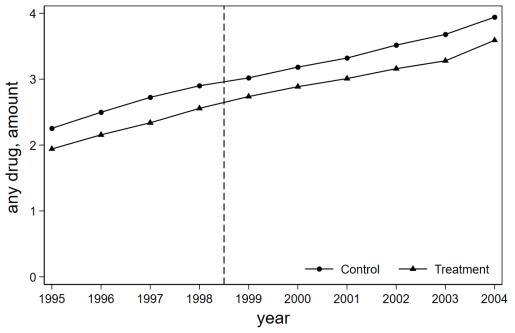
Figure 5: Trends and reform effects on extensive and intensive margins of prescription drug utilization



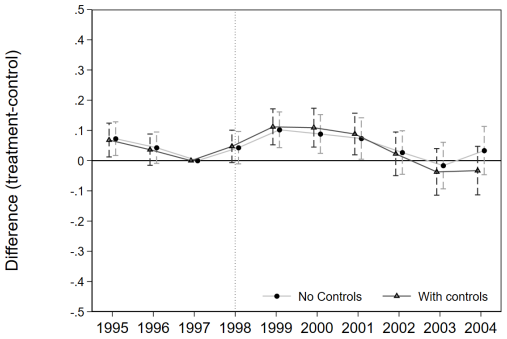
(a) Drug utilization, extensive margin



(b) Estimation coefficients, extensive margin



(c) Drug utilization, intensive margin



(d) Estimation coefficients, intensive margin

Note: *Extensive margin* refers to the probability of having any drug purchases within a year and *intensive margin* to the number of purchased packages of any drug within a year. The results in panel b are based on the interaction term between year and treatment group by estimating Eq. 2. The confidence interval represents 95% levels.

ber, calculated for each year from the number of purchased prescription drug packages distribution. As the distribution at the intensive margin is right-skewed, this additional measure gives evidence from which part of the intensive margin distribution the effect on the extensive margin comes from.

I find a 1 percentage point increase in the utilization of prescription drugs at the extensive margin. At the intensive margin, I find no effect. However, the probability of buying a large amount of any drug increases by 1.2 percentage points due to the reform, indicating that the effect of the reform on the extensive margin stems from those having several purchases within a year or who buy in high quantities. In relative terms the effects

are small, for the extensive margin around 2% and for the probability of buying large amounts of drugs around 3% compared to the pre-reform mean of the treated.

Table 3: Reform effects on prescription drugs utilization

	Any Drug	Any Drug	# of drugs	# of drugs	High amount	High amount
TREAT	-0.042***	-0.042***	-0.197*	-0.203**	-0.039***	-0.040***
	(0.010)	(0.009)	(0.079)	(0.076)	(0.010)	(0.010)
POST	0.089***	0.083***	0.950***	0.877***	-0.002	-0.007*
	(0.003)	(0.003)	(0.021)	(0.022)	(0.003)	(0.003)
REFORM EFFECT	0.012***	0.010**	0.015	0.012	0.014***	0.013***
	(0.003)	(0.003)	(0.024)	(0.025)	(0.003)	(0.004)
controls	no	yes	no	yes	no	yes
R2	0.011	0.089	0.018	0.071	0.003	0.056
Observations	451 746	451 746	451 746	451 746	451 746	451 746
Individuals	50 351	50 351	50 351	50 351	50 351	50 351
Pre-reform mean, treat	0.54	0.54	2.15	2.15	0.41	0.41

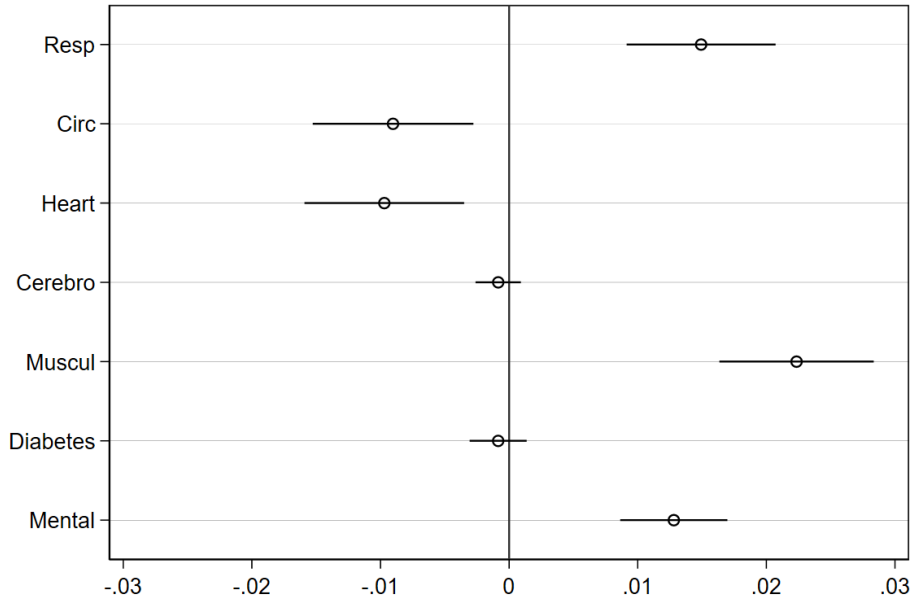
Note: The table presents intention-to-treat estimates from estimating Eq. 1. The years in the estimation are 1995-2004 and the estimation is performed using OLS. Standard errors clustered at the individual level. $*$, $**$, and $***$ indicate statistical significance at the 0.1, 0.05, and 0.01 levels, respectively. All regressions include cohort fixed effects and the other controls are gender, education, industry, being married, living in the capital region, earnings, sick days in 1995-1997.

In Figure 6 I explore the extensive margin effects by drug category.[12] From the figure we observe that there is a small decrease in the utilization of drugs for circulatory and heart diseases. These coefficients correspond to around a 5% relative effect. As health conditions related to these drugs are correctable with lifestyle habits, these results may indicate that increased leisure is used to make improving investments in health. Ahn (2016) found that shortening the regular work-week induced individuals to exercise regularly, which is a plausible mechanism here as well, but cannot be further explored with administrative data.

For respiratory diseases and diseases of musculoskeletal system, we observe an increase in drug purchases, corresponding to approximately 6% and 9% relative effects, respectively. One possible explanation here is that increased leisure is used to take care of conditions that are more directly linked to working conditions but to which individuals have not been able to respond within full working hours by seeking medical help. Lucifora and Vigani (2018) found that retirement increased health care utilization, which is

[12]Regression coefficients presented in Table A2 in Appendix.

Figure 6: Effects on different drug categories, extensive margin



Note: The confidence interval represents 95% levels.

consistent with a decrease in the opportunity cost of leisure time and this is a plausible channel in the case of part-time work as well. As the treatment group has a longer work horizon than the control group, for them the opportunity cost of leisure together with higher expected returns on health investments may explain the increased use of health care.

The biggest relative effect, a 14% increase, is for drugs used for mental illnesses. I was unable to recognize certain drug categories within mental health drugs that are driving this result, so it is unclear what the channel is between a reduction in working hours and an increase in mental illness drugs. One plausible reason, but untestable with the current dataset, is that work intensity increases when hours decrease. There are some studies (see Rudolf (2014) and Cottini and Lucifora (2013)) that find that a reduction in working hours is indeed associated with greater intensification of work, leading to lower well-being and increased risk of burnout and trauma disorders, which would also increase the utilization of mental health drugs when mental health deteriorates. To explore this further, I ran the regression with manual worker interaction and found that for manual workers the reform

did not affect mental illness drug purchases, which gives indirect support for the work intensification hypothesis as work tasks for manual occupations are likely more easily reduced in the same proportion as hours while in professional occupations this is likely to be harder, leading to work intensification.

The results presented are intention-to-treat results as not everybody takes a part-time pension at the lowest eligibility age. We can use the reform in an instrumental variable setting to study the effects of starting part-time work at the age of 56 on health outcomes. Using Eq. 1 and changing the dependent variable to indicate whether individuals have complied with the reform (started part-time work before age 57), I get a first-stage coefficient of 0.29. Using this first-stage with a 2SLS estimator, within the compliers the average treatment effect is around a 3.4 percentage point increase at the extensive margin.

4.1.3 Heterogeneous treatment effects

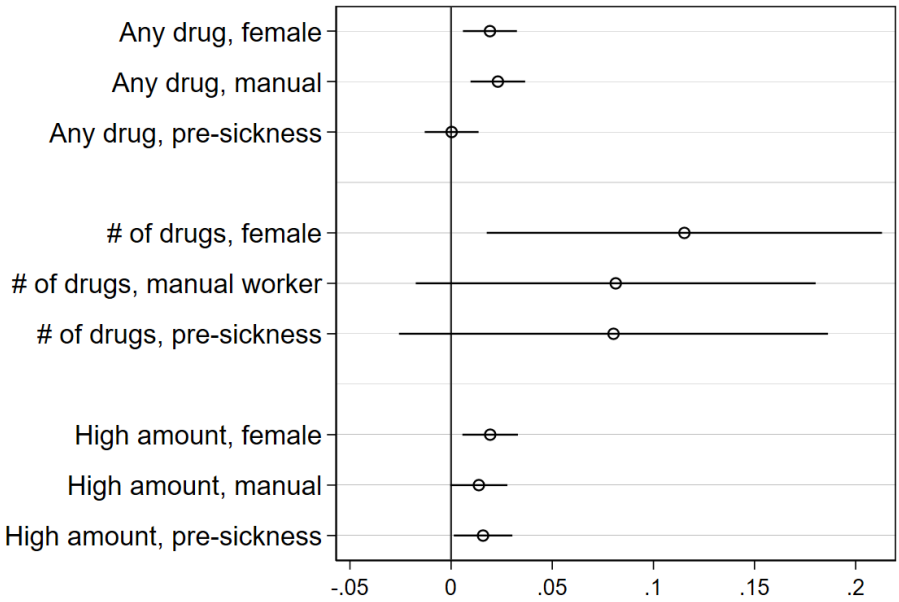
In earlier literature it has been established that working hours may have different types of effects on health by gender and by occupation. Women tend to visit doctors more often and use more prescription drugs than men. On the other hand, people working in manual occupations usually experience a more rapid deterioration in physical health and suffer from different types of health conditions than professional occupations. I explore heterogeneity in the effects by gender and being a worker in a manual occupation.[13] The manual worker dummy is based on the occupation in the pre-reform period and divides the data into two nearly equally sized groups of manual versus professional workers. In the last heterogeneity analysis I include a dummy indicating sickness history in the pre-reform era, defined as having long sickness absences (i.e. absences lasting a minimum of 10 days observed from the Social Insurance Institution's data). About one third of the sample have long sickness absences in the years 1995-1997.

Figure 7 shows the coefficient for the reform effect interacted with either the female, manual worker or sickness dummy.[14] In general, I find that women are more likely to increase the purchases of prescription drugs after the reform. From the interaction term between the reform effect and being a manual worker we observe that at the extensive margin manual workers are more likely to increase purchases of prescription drugs after

[13]Manual occupations are service and craft workers, plant operators and elementary occupations.
[14]Full tables of regression results presented in Appendix Tables A3, A4 and A5.

the reform. These results support the hypothesis that in manual types of work health is more likely to deteriorate faster and the reform gave these workers time to seek medical help for health conditions. This is line with Mazzonna and Peracchi (2017), who found that people working in more physically demanding jobs benefit more on retiring with respect to subjective health and cognitive abilities. Finally, the observed main effects do not seem to stem from those who are sick in the pre-reform era as the interaction term is close to zero.

Figure 7: Heterogenous treatment effects



Note: The confidence interval represents 95% levels.

4.1.4 Sensitivity analyses

For a sensitivity analysis I perform several additional estimations. Firstly, I restrict the control and treatment groups to a subset of cohorts. Secondly, I complement the analysis by also using a comparison group formed of similar workers without part-time pension spells in order to control for the secular cohort trends (e.g in education, nutrition, general period effects) to identify the causal effects in a triple difference setting.

I ran the main analysis with two different definitions of treatment and control groups. In the first, I restricted my control group to those birth cohorts with eligibility age at exactly 58 years (born 1940-6/1940). In the second, I restricted the treatment group to cohorts born before 1945. In the latter, the year of eligibility is more similar between the treatment and control groups, facilitating interpretation of earlier results.[15] The event graphs from these robustness checks are presented in Appendix Figure A2 and the regression results in Table 4. Reassuringly, the point estimates are close to the main analysis.

Table 4: Reform effects on prescription drugs, robustness checks

	(1) Any Drug	(1) # of drugs	(1) high amount	(2) Any Drug	(2) # of drugs	(2) high amount
TREAT	-0.065***	-0.480***	-0.077***	-0.040***	-0.181*	-0.037***
	(0.007)	(0.052)	(0.007)	(0.009)	(0.076)	(0.010)
POST	0.080***	0.904***	-0.005	0.083***	0.874***	-0.007*
	(0.005)	(0.039)	(0.005)	(0.003)	(0.022)	(0.003)
REFORM EFFECT	0.013*	-0.016	0.011*	0.009*	0.003	0.012**
	(0.005)	(0.041)	(0.006)	(0.004)	(0.028)	(0.004)
R2	0.093	0.073	0.058	0.084	0.070	0.054
Observations	365 252	365 252	365 252	278 705	278 705	278 705
Individuals	40 688	40 688	40 688	31 104	31 104	31 104
Pre-reform mean, treat	0.54	2.15	0.41	0.55	2.25	0.42

Note: Columns with (1) indicate specification where the control group consists of cohorts with eligibility age 58 (1940-1940/6) and treatment 56 (7/1942-1946) and columns with (2) indicate specification where treatment cohorts born in 1946 or 1945 are excluded. The table presents intention-to-treat estimates from Eq. 1. Years in the estimation are 1995-2004 and estimation is performed using OLS. Sample includes part-time pensioners. Standard errors clustered at individual level. $*$, $**$, and $***$ indicate statistical significance at the 0.1, 0.05, and 0.01 levels, respectively. All regressions include cohort fixed effects and other controls are gender, education, industry, being married, living in the capital region, earnings, sick days in 1995-1997.

In the triple difference setting I use workers in the non-part-time pensioners sample

[15]The latter robustness check also makes sure that the full sample is not selected as in the two youngest cohorts those individuals who took up part-time pensions near the official old-age retirement are missing.

to control for unobserved differences between the treatment and control groups. This reduces the possible bias in the double difference setting if the treatment and control cohorts have different trajectories in health outcomes unrelated to the policy change. In the triple difference setting the identifying assumption is weaker than in the double difference setting, as we do not need to assume parallel trends between the different treatment and control groups but instead that the *ratio* between the treatment and control groups within the part-time pensioners and non-part-time pensioners would have evolved similarly in the absence of the reform. In the triple difference setting, the $POST_t$, $TREAT_i$ and $POST_t \times TREAT_i$ in Eq. 1 are interacted with a dummy variable denoting whether individuals belong to the part-time pensioners (PP) sample or not. The reform effect is the coefficient of $PP \times POST_t \times TREAT_i$.

To reduce the imbalance between the part-time pension sample and the comparison group, I use Coarsened Exact Matching (CEM) prior to the triple difference set-up. In CEM matching, the data is temporarily coarsened into strata which are balanced between the part-time pensioners sample and their comparison group. In the balancing, strata which do not include observations from both groups are excluded. In the final analysis the original microdata is used with strata weights where the observations from the unmatched individuals get the weight 0. (Blackwell et al., 2009). The matching is done based on only a few pre-treatment characteristics, which are gender and earnings quantiles for each year between 1995-1997. As part-time pension recipients are subject to employment requirements for eligibility, earnings are a relevant matching variable.[16]

The coefficients presented in Table 5 confirm the earlier findings that the reform increased drug purchases but not the intensity of purchases.[17] The specification with controls tells us that the reform increases drug purchases at the extensive margin by 1 percentage point relative to the overall worker sample. Running the triple difference regression for different drug categories (Appendix Table A7), we observe that many effects present in the difference-in-differences estimation disappear while the coefficients for respiratory and musculoskeletal drug categories remain, with slightly smaller relative

[16]Appendix Figure A3 shows the earnings distribution before and after the CEM procedure and Table A6 shows the pre-reform characteristics with CEM weights. With CEM matching the imbalance in pre-reform characteristics is reduced significantly.

[17]Appendix Figure A4 shows the pretrends. The trends are now the difference in outcomes between treatment and control cohorts relative to the non-part-time pensioners sample. The pre-trends improve slightly, especially at the intensive margin, compared to the double difference setting. Otherwise the trajectory after reform looks similar to that in the main analysis.

effects (3% and 6%). Together the main analysis and the triple difference results suggest that the part-time pension age reform led in particular to treatment of musculoskeletal disorders.

Table 5: Reform effects on prescription drugs, triple difference

	Any Drug	Any Drug	# of drugs	# of drugs	High amount	High amount
$TREAT$	-0.032***	-0.028***	-0.202***	-0.187***	-0.033***	-0.030***
	(0.007)	(0.006)	(0.055)	(0.053)	(0.007)	(0.007)
$POST$	0.098***	0.087***	1.017***	0.841***	0.012***	-0.002
	(0.003)	(0.003)	(0.019)	(0.021)	(0.003)	(0.003)
$TREAT \times POST$	0.001	0.000	-0.039	0.002	0.001	0.003
	(0.003)	(0.003)	(0.022)	(0.022)	(0.003)	(0.003)
PP	0.024***	0.031***	0.032	0.121***	0.016**	0.026***
	(0.005)	(0.004)	(0.033)	(0.033)	(0.005)	(0.005)
$TREAT \times PP$	-0.004	-0.008	-0.038	-0.089*	-0.004	-0.009
	(0.005)	(0.005)	(0.038)	(0.037)	(0.005)	(0.005)
$POST \times PP$	-0.009*	-0.004	-0.067*	0.025	-0.014***	-0.006
	(0.004)	(0.004)	(0.029)	(0.029)	(0.004)	(0.004)
$PP \times POST_t \times TREAT_c$	0.011*	0.011*	0.054	0.029	0.013**	0.012**
	(0.004)	(0.004)	(0.033)	(0.033)	(0.005)	(0.005)
Controls	no	yes	no	yes	no	yes
R2	0.012	0.082	0.017	0.071	0.003	0.056
Observations	1 617 640	1 617 640	1 617 640	1 617 640	1 617 640	1 617 640
Individuals	181 636	181 636	181 636	181 636	181 636	181 636
Pre-reform mean, treated	0.52	0.52	2.15	2.15	0.40	0.40

Note: The table presents intention-to-treat estimates estimating the triple difference specification, CEM weights used. Years in the estimation are 1995-2004 and estimation is performed using OLS. Sample includes part-time pensioners and non-part-time pensioners. Standard errors clustered at individual level. $*$, $**$, and $***$ indicate statistical significance at the 0.1, 0.05, and 0.01 levels, respectively. All regressions include cohort fixed effects and other controls are gender, education, industry, being married, living in the capital region, earnings, sick days in 1995-1997.

4.2 Long-term effects

The small increase in drug purchases observed in the previous section can indirectly affect health in retirement as drug utilization indicates that certain medical conditions

are treated. On the other hand, part-time work at the end of a career can directly affect health in retirement. For these reasons it makes sense to study the long-term effects of the part-time pension reform. That is, I study health outcomes at ages 65-68. All individuals are observed until the age of 68, and as the statutory retirement ages in the study cohorts were between 63-65 the age window of 65-68 ensures that we mostly observe individuals who have exited the labour markets. The outcome measures are mortality by age 68, drug utilization at ages 65-68 and the probability of having a severe and chronic medical diagnosis between ages 65-68.[18]

I utilize the triple framework as in the robustness analysis in the previous section. The rationale for triple difference is that the long-term estimates are cross-sectional by nature and the cohort variation does not take into account that there might be health differences at the same age across cohorts. These might stem from upbringing (e.g. being born during different phases of the Second World War) or health technology, which might benefit younger cohorts more than older cohorts. I regress the following model to study the differences between the treatment and control cohorts relative to the treatment and control cohorts of non-part-time-pensioners:

$$Y_i = \alpha + \beta_1 TREAT_i + \beta_2 PP + \beta_3 TREAT_i \times PP + \boldsymbol{X}_i'\delta + \gamma_c + \varepsilon_i \tag{3}$$

where Y_i is the outcome at age 65-68 of individual i.

The results are presented in Table 6. For drug purchases and mortality I cannot reject the null hypothesis of no effect. For diagnosis I find a statistically significant coefficient at the 5% level but this coefficient turns to zero as controls are included. As mortality is a rare event, I computed the two-side hypothesis test at a 5% significance level to find the lowest detectable effect when assuming that we can accept the null hypothesis with the risk of making a Type II error higher than 20%, which is a usual threshold in statistical power calculations. This calculation reveals that we should observe a mortality effect of -.35 percentage points. We can conclude that if there were a mortality effect, it would be very close to zero. All in all, this suggests that the reform, which decreased the eligibility age for part-time pensions, did not translate into increased or decreased health inequality between cohorts with different eligibility ages in the long term.

[18]Diagnosis includes different cancers, diabetes, chronic coronary heart disease, chronic hypertension, chronic heart failure and chronic bronchial asthma.

Table 6: Long-term effects: at retirement age 65-68

	Drug pur	Drug pur	Diag.	Diag	Death by 68	Death by 68
PP	0.029***	0.027***	-0.003	0.007	-0.022***	-0.018***
	(0.004)	(0.004)	(0.005)	(0.004)	(0.003)	(0.003)
TREAT	-0.004	-0.001	-0.017	0.001	-0.009	-0.008
	(0.006)	(0.006)	(0.009)	(0.007)	(0.005)	(0.005)
TREAT × PP	-0.001	0.000	0.020**	0.009	0.001	-0.001
	(0.004)	(0.004)	(0.006)	(0.005)	(0.003)	(0.003)
controls	no	yes	no	yes	no	yes
R2	0.002	0.087	0.001	0.005	0.002	0.020
Individuals	180 930	180 930	180 930	180 930	180 930	180 930
mean dependent var.	0.86	0.86	0.37	0.37	0.08	0.08

Notes: The table presents estimates from regressing Eq. 3. Estimation is performed using OLS. Sample includes part-time pensioners and non-part-time pensioners with CEM weights. Standard errors clustered at individual level. $*$, $**$, and $***$ indicate statistical significance at the 0.1, 0.05, and 0.01 levels, respectively. All regressions include cohort fixed effects and other controls are gender, education, industry, being married, living in the capital region, earnings, sick days in 1995-1997.

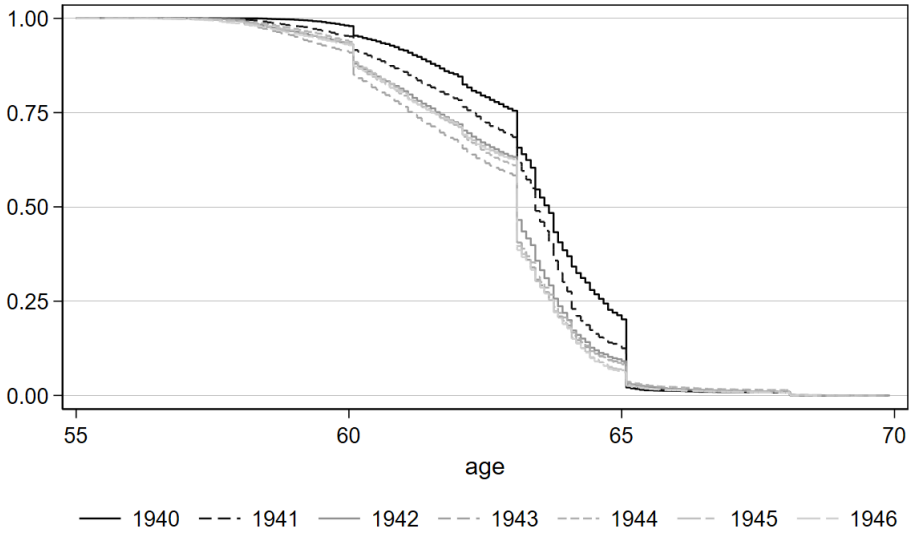
4.3 Labour market exit

As the reasoning for encouraging gradual retirement is commonly a reduction in early labour market exits, lastly I study the effects of the part-time retirement scheme on these exits. During the period under study, the labour market for elderly workers was favourable as the economy was growing. According to Statistics Finland's Employment statistics, the employment share of workers in the 55-59 age group grew by approximately 14 percentage points to 65% and in the 60-64 age group by 17 percentage points to 30 % between the years 1998-2005.

Approximately 60% start claiming an old-age pension straight after their part-time pension. Around 39% work after taking-up a part-time pension and before starting to claim old-age pension. However, around 10% of these individuals have wage levels exceeding part-time pension work levels, indicating that returns to full-time work are rare. A small proportion of part-time pensioners (around 1%) die during their part-time pension spell.

Figure 8 shows survival function graphs of exiting the labour market for part-time pensioners. Older cohorts tend to survive longer in the labour market than younger cohorts. However, the pension reform in 2005 lowered the old-age retirement age for younger cohorts in the sample, making it difficult to estimate the effect of the part-time pension eligibility age reform on final exit from the labour market. For this reason, I estimate a triple difference as presented in Eq. 3 for the probability of exiting early, before the lowest old-age retirement age of 63. Figure 8 shows that before the age of 63 survival is lower in the treated cohorts but in the triple difference setting we can control for the cohort effects.

Figure 8: Kaplan-Meier survival function of exit from the labour market, by cohorts



Note: Only part-time pensioners sample.

As I do not have the exact start of the claiming date for non-part-time pensioners, I define exit at the annual level for the whole sample. I use two definitions, firstly by defining retirement as the year when pension income is over half of a person's taxable income. The second definition is somewhat stricter as I require that an individual needs to have zero labour and self-employment income for two years in a row. The retirement year is then the first of these years.

Table 7 shows the main result for early labour market exits. The first and second row

show that in general part-time pensioners and younger cohorts in the treatment group have a lower probability of exiting the labour market early. However, the interaction of treatment cohorts and being a part-time pensioner has a positive and large coefficient. I also did a similar type of heterogeneity analysis as in Section 4.1.3 but found no evidence that the treated part-time pensioners prolonged their working career. In this respect the part-time pension trial did not achieve the goals set for the program and the reform. This is in line with evidence from other countries, see for example the references on gradual retirement discussed in the Introduction. Machado and Portela (2014) and Hermansen (2015) also show that a reduction in hours is not associated with longer working careers in Portugal and Norway, respectively, but instead a voluntary reduction in hours can signal a worker's wish to retire sooner.

Table 7: Regression results for labour market exits

	Early exit, pension def.	Early exit pension def.	Early exit, earnings def.	Early exit, earnings def.
$TREAT$	-0.080***	-0.066***	-0.055***	-0.040***
	(0.009)	(0.007)	(0.010)	(0.007)
PP	-0.297***	-0.238***	-0.311***	-0.242***
	(0.005)	(0.005)	(0.005)	(0.005)
$TREAT \times PP$	0.129***	0.100***	0.088***	0.056***
	(0.006)	(0.006)	(0.006)	(0.006)
Controls	no	yes	no	yes
R2	0.037	0.118	0.056	0.141
N	181 636	181 636	155 467	155 467
Mean dep. var	0.48	0.48	0.40	0.40

Notes: The table presents estimates from regressing Eq. 3. Estimation is performed using OLS. Sample includes part-time pensioners and non-part-time pensioners with CEM weights. Standard errors clustered at individual level. $*$, $**$, and $***$ indicate statistical significance at the 0.1, 0.05, and 0.01 levels, respectively. All regressions include cohort fixed effects and other controls are gender, education, industry, being married, living in the capital region, earnings, sick days in 1995-1997.

5 Conclusion

In this paper I study the effects of a 2-year decrease in the eligibility age for a part-time pension scheme and the effect of part-time working on health-related factors and early labour market exits.

I find that the reform reducing the eligibility age on average increased any drug purchases by 1 percentage point while the largest effect is found for drugs treating disorders in the musculoskeletal system. The effects are larger for women and workers in manual occupations while the effects for mental illness drug purchases are large for professional workers. At retirement there is no difference in drug utilization, prevalence of severe or chronic health conditions or mortality. The results are robust to several sensitivity tests. The treated cohorts were more likely to exit below the old-age pension age than the older cohorts, i.e. the program did not achieve the goal of lengthening work careers.

The results suggest that the increased leisure time at the end of careers was used to get medical help for less severe conditions which during full working hours were not considered important or restrictive enough to seek treatment. Indirectly this can be considered as a positive effect for well-being if the medical help received succeeded in curing or preventing different contemporaneous illnesses or ailments. However, the increased use of mental health drugs within professional occupations can point to work intensification and reduced well-being. The effect of the reform on drug utilization was relatively small but was economically significant as annual remuneration of prescription drugs totals around 1.5 billion euros in Finland.[19] It is out of the scope of this paper to evaluate the overall net welfare effect of the part-time pension program. Nevertheless, as the part-time pension program was costly for the pension system without observable lengthening of work careers, more research is needed the way in which and at what stage of working careers interventions aiming to improve working-ability are most effective.

[19]Social Insurance Institution database kela.fi/kelasto

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Appendix

Tables

Table A1: Classification of drug data

	ATC codes
Any disease	all ATCs
Respiratory diseases	R
Circulatory diseases	C
Heart-related diseases	C01, C02, C03, C04, C07, C08, C09, C10
Musculoskeletal disorders	M01, M03, A03D, M02A, A03EA
Diabetes	A10
Any mental illness	N05A-C, N06A-C

Sources: Leinonen et al. (2016), Hagen (2018)

Table A2: Effects of reform on prescription drug utilization, different drug categories

	Resp.	Circ.	Heart	Cerebro.	Musculoskeletal	Diabetes	Mental
TREAT	-0.027***	-0.004	-0.003	-0.001	-0.033***	-0.005	-0.019**
	(0.007)	(0.010)	(0.010)	(0.002)	(0.007)	(0.003)	(0.006)
POST	-0.032***	0.137***	0.139***	0.005***	-0.005	0.013***	-0.010***
	(0.003)	(0.003)	(0.003)	(0.001)	(0.003)	(0.001)	(0.002)
REFORM EFFECT	*0.015****	*-0.009***	*-0.010***	-0.001	*0.022****	-0.001	*0.013****
	(0.003)	(0.003)	(0.003)	(0.001)	(0.003)	(0.001)	(0.002)
R2	0.030	0.044	0.044	0.012**	0.046	0.012*	0.037
Observations	451 746	451 746	451 746	451 746	451 746	451 746	451 746
Individuals	50 351	50 351	50 351	50 351	50 351	50 351	50 351
Pre-reform mean, treated	0.23	0.18	0.17	0.01	0.24	0.01	0.09

Note: The table presents intention-to-treat estimates from estimating Eq. 1 for different drug categories. See Table 5 for the aggregation of the ATC codes. Years in the estimation are 1995-2004 and estimation is performed using OLS. Standard errors clustered at individual level. $*$, $**$, and $***$ indicate statistical significance at the 0.1, 0.05, and 0.01 levels, respectively. In *italics* coefficients which pass the Romano-Wolf multiple hypothesis correction (Clarke et al., 2020) at the 0.05 level. All regressions include cohort fixed effects and other controls are gender, education, industry, being married, living in the capital region, earnings, sick days in 1995-1997..

Table A3: Effects of reform on prescription drugs, heterogeneous treatment effects by gender

	Any Drug	Any Drug	# of drugs	# of drugs	High amount	High amount
FEMALE	0.137***	0.096***	0.533***	0.256***	0.117***	0.080***
	(0.007)	(0.007)	(0.050)	(0.052)	(0.007)	(0.007)
REFORM	0.001	-0.001	-0.047	-0.049	0.003	0.002
	(0.005)	(0.007)	(0.050)	(0.052)	(0.007)	(0.007)
REFORM × FEMALE	0.020**	0.019**	0.109*	0.108*	0.019**	0.019**
	(0.007)	(0.007)	(0.050)	(0.050)	(0.007)	(0.007)
controls	no	yes	no	yes	no	yes
R2	0.029	0.089	0.023	0.071	0.011	0.056
Observations	451 746	451 746	451 746	451 746	451 746	451746
Individuals	50 351	50 351	50 351	50 351	50 351	50 351
Pre-reform mean, treat	0.54	0.54	2.15	2.15	0.41	0.41

Note: The table presents intention-to-treat estimates from Eq. 1. Years in the estimation are 1995-2004 and estimation is performed using OLS. Sample includes part-time pensioners. Standard errors clustered at individual level. $*$, $**$, and $***$ indicate statistical significance at the 0.1, 0.05, and 0.01 levels, respectively. All regressions include cohort fixed effects and other controls are gender, education, industry, being married, living in the capital region, earnings, sick days in 1995-1997..

Table A4: Effects of reform on prescription drugs, heterogeneous treatment effects by occupation

	Any Drug	Any Drug	# of drugs	# of drugs	High amount	High amount
MANUAL	0.004	-0.019**	-0.003	-0.224***	0.001	-0.026**
	(0.007)	(0.007)	(0.051)	(0.055)	(0.007)	(0.008)
REFORM EFFECT	0.002	0.000	-0.023	-0.025	0.008	0.008
	(0.004)	(0.004)	(0.031)	(0.032)	(0.005)	(0.005)
REFORM × MANUAL	0.023***	0.023***	0.079	0.086	0.014*	0.014
	(0.007)	(0.007)	(0.050)	(0.050)	(0.007)	(0.007)
controls	no	yes	no	yes	no	yes
R2	0.012	0.090	0.018	0.072	0.003	0.057
Observations	448 669	448 669	448 669	448 669	448 669	448 669
Individuals	50 008	50 008	50 008	50 008	50 008	50 008
Pre-reform mean, treat	0.54	0.54	2.15	2.15	0.41	0.41

Note: The table presents intention-to-treat estimates from Eq. 1. Years in the estimation are 1995-2004 and estimation is performed using OLS. Sample includes part-time pensioners. Standard errors clustered at individual level. $*$, $**$, and $***$ indicate statistical significance at the 0.1, 0.05, and 0.01 levels, respectively. All regressions include cohort fixed effects and other controls are gender, education, industry, being married, living in the capital region, earnings, sick days in 1995-1997.

Table A5: Effects of reform on prescription drugs, heterogeneous treatment effects by sickness history

	Any Drug	Any Drug	# of drugs	# of drugs	High amount	High amount
SICK-PRE	0.179***	0.144***	1.278***	0.948***	0.185***	0.143***
	(0.007)	(0.007)	(0.054)	(0.055)	(0.007)	(0.007)
REFORM EFFECT	0.009*	0.009*	-0.022	-0.016	0.007	0.007
	(0.004)	(0.004)	(0.028)	(0.028)	(0.004)	(0.004)
REFORM × SICK-PRE	0.003	0.000	0.092	0.079	0.018**	0.016*
	(0.007)	(0.007)	(0.054)	(0.054)	(0.007)	(0.007)
controls	no	yes	no	yes	no	yes
R2	0.031	0.098	0.038	0.079	0.022	0.064
Observations	451 746	451 746	451 746	451 746	451 746	451 746
Individuals	50 351	50 351	50 351	50 351	50 351	50 351
Pre-reform mean, treated	0.54	0.54	2.15	2.15	0.41	0.41

Note: The table presents intention-to-treat estimates from Eq. 1. Years in the estimation are 1995-2004 and estimation is performed using OLS. Sample includes part-time pensioners. Standard errors clustered at individual level. $*$, $**$, and $***$ indicate statistical significance at the 0.1, 0.05, and 0.01 levels, respectively. All regressions include cohort fixed effects and other controls are gender, education, industry, being married, living in the capital region, earnings, sick days in 1995-1997.

Table A6: Descriptives for pre-reform 1995-1997 period, part-time pensioners (PP) and non-part-time pensioners (Non-PP), CEM weights

	PP	Non-PP
Background		
Female	0.56	0.56
	(0.50)	(0.50)
Married	0.84	0.82
	(0.37)	(0.38)
Capital region	0.24	0.23
	(0.43)	(0.42)
Tertiary	0.17	0.14
	(0.38)	(0.35)
Employment and income		
Wage income	34413.0	34553.2
	(17688.9)	(20780.2)
Net income	25523.5	25730.0
	(22621.8)	(25810.2)
Employment months	11.6	11.6
	(1.98)	(2.01)
Occupation		
Managers	0.052	0.064
	(0.22)	(0.24)
Professionals	0.20	0.15
	(0.40)	(0.36)
Technicians and associate professionals	0.19	0.19
	(0.39)	(0.39)
Clerical support workers	0.14	0.11
	(0.34)	(0.31)
Service workers	0.11	0.15
	(0.32)	(0.36)
Crafts workers	0.095	0.092
	(0.29)	(0.29)
Plant operators etc.	0.078	0.10
	(0.27)	(0.30)
Elementary occupations	0.098	0.094
	(0.30)	(0.29)
Industry		
Manufacturing	0.20	0.21
	(0.40)	(0.41)
Transportation	0.064	0.071
	(0.25)	(0.26)
Finance and insurance	0.033	0.033
	(0.18)	(0.18)
Professional services	0.099	0.077
	(0.30)	(0.27)
Public administration	0.095	0.080
	(0.29)	(0.27)
Education	0.12	0.071
	(0.33)	(0.26)
Health and social services	0.14	0.18
	(0.35)	(0.38)
Health		
Sickness days >10, yearly	3.85	5.13
	(16.8)	(23.4)
Any drug purchases	0.55	0.53
	(0.50)	(0.50)
Drug purchases, year	2.24	2.22
	(3.22)	(3.37)
Individuals	50 343	131 293

Note: Table includes information on the 1940-1946 cohorts, within the part-time pensioner (PP) sample and the non-part-time pensioner (Non-PP) sample using CEM weights. Only largest industries and occupations are shown. Income items in 2014 money.

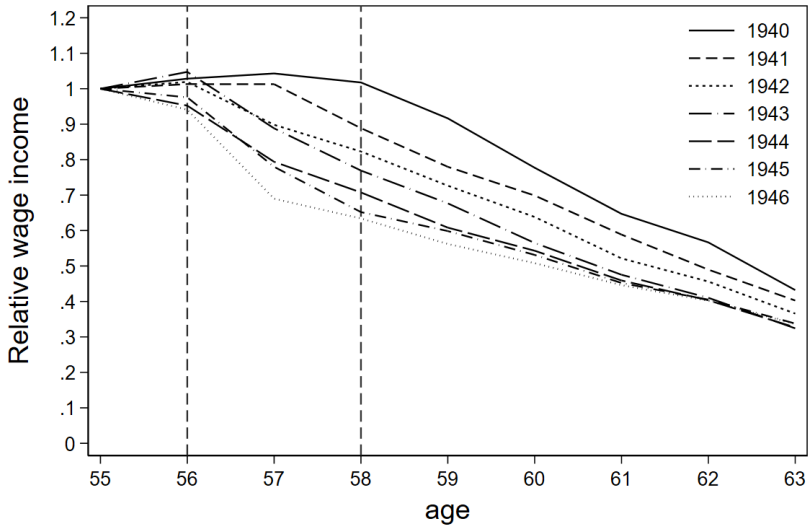
Table A7: Effects of reform on prescription drug utilization, different drug categories, triple difference

	Resp.	Circ.	Heart	Musculoskeletal	Diabetes	Mental
TREAT	-0.0156**	-0.0056	-0.0057	-0.0190***	-0.0040	-0.0102**
	(0.0049)	(0.0064)	(0.0064)	(0.0046)	(0.0021)	(0.0038)
POST	-0.0252***	0.1310***	0.1327***	0.0073**	0.0124***	-0.0134***
	(0.0022)	(0.0025)	(0.0025)	(0.0023)	(0.0009)	(0.0016)
TREAT $\times POST$	0.0059*	-0.0151***	-0.0154***	0.0087***	-0.0002	0.0142***
	(0.0024)	(0.0027)	(0.0027)	(0.0026)	(0.0010)	(0.0018)
PP	0.0177***	0.0071	0.0064	0.0213***	0.0004	0.0145***
	(0.0036)	(0.0041)	(0.0041)	(0.0035)	(0.0013)	(0.0027)
TREAT $\times PP$	-0.0079	-0.0006	-0.0001	-0.0054	-0.0011	-0.0089**
	(0.0041)	(0.0046)	(0.0046)	(0.0040)	(0.0015)	(0.0031)
POST $\times PP$	-0.0077*	0.0059	0.0062	-0.0122***	0.0003	0.0030
	(0.0033)	(0.0037)	(0.0037)	(0.0035)	(0.0013)	(0.0024)
REFORM EFFECT	0.0084*	0.0070	0.0066	*0.0141****	-0.0002	-0.0004
	(0.0038)	(0.0042)	(0.0041)	(0.0040)	(0.0015)	(0.0027)
R2	0.028	0.041	0.041	0.043	0.010	0.039
Observations	1 617 640	1 617 640	1 617 640	1 617 640	1 617 640	1 617 640
Individuals	181 636	181 636	181 636	181636	181 636	181 636
Pre-reform mean, treat	0.22	0.18	0.17	0.23	0.01	0.09

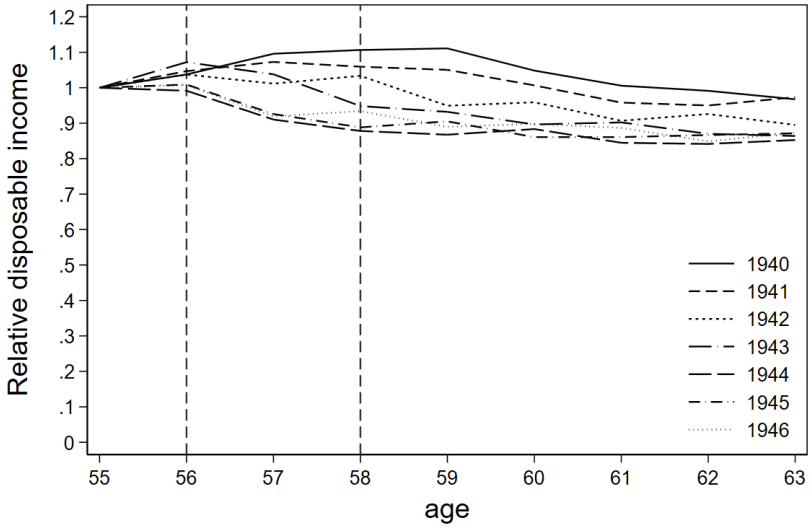
Notes: The table presents intention-to-treat estimate from Eq. 1 in the triple difference framework. Years in the estimation are 1995-2004 and estimation is performed using OLS. Sample includes part-time pensioners and non-part-time pensioners with CEM weights. Standard errors clustered at individual level. $*$, $**$, and $***$ indicate statistical significance at the 0.1, 0.05, and 0.01 levels, respectively. In *italics* coefficients which pass the Romano-Wolf multiple hypothesis correction at the 0.05 level. All regressions include cohort fixed effects and other controls are gender, education, industry, being married, living in the capital region, earnings, sick days in 1995-1997.

Figures

Figure A1: Wage and disposable income relative to age 55 by cohorts

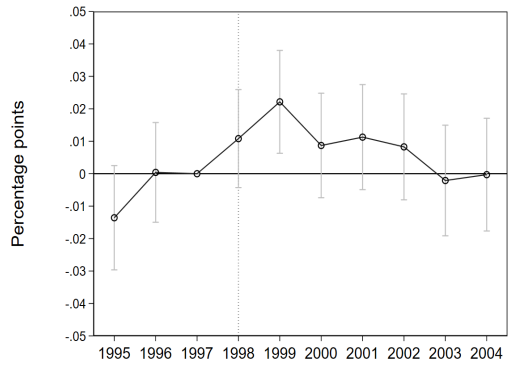


(a) Wage income

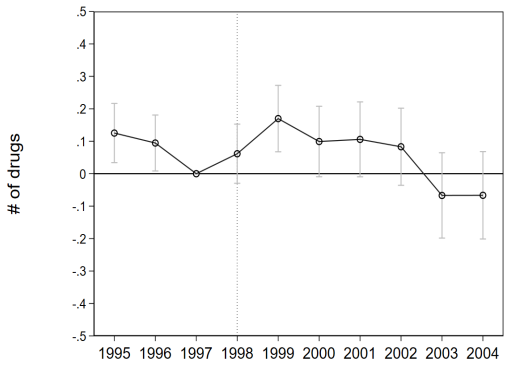


(b) Disposable income

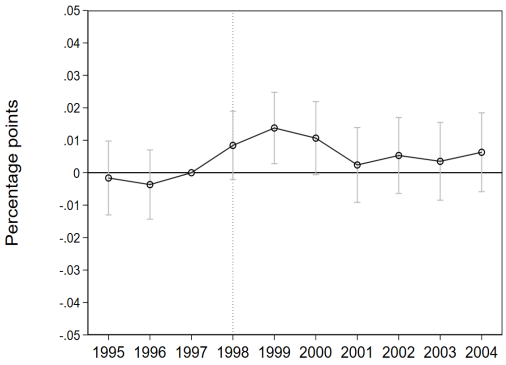
Figure A2: Robustness checks



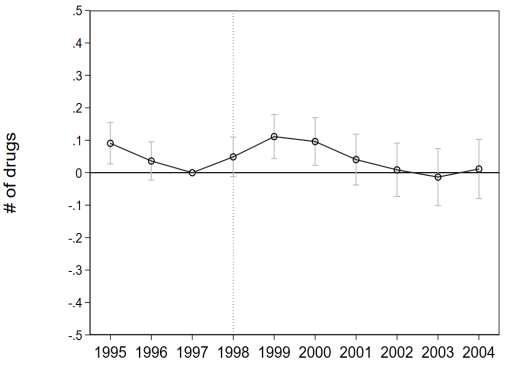
(a) Extensive, 58 vs. 56



(b) Intensive, 58 vs. 56



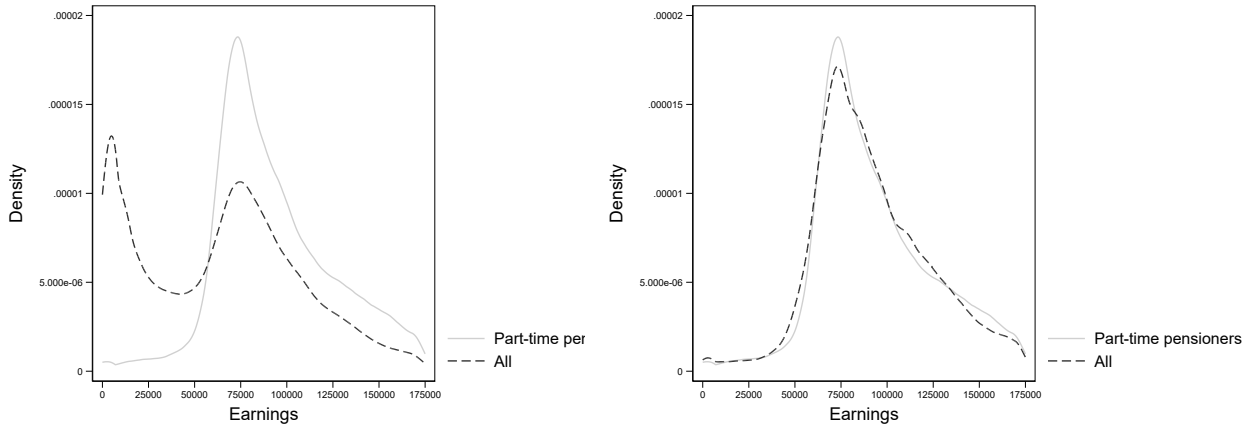
(c) Extensive, excl. >1944



(d) Intensive, excl. >1944

Note: The estimates presented are based on the interaction term between year and treatment group by estimating Eq. 2. Estimation was run with OLS and with clustered standard errors at the individual level. The confidence interval represents 95% levels. In the top panel, control group is defined as being born between 1940-6/1940 (eligibility age 58) and treatment (7/1942-1946). In the bottom panel 1945 and 1946 cohorts are dropped from the treatment group.

Figure A3: Earnings distribution for part-time pensioners and non-part-time pensioners (earnings sum for years 1995-1997)

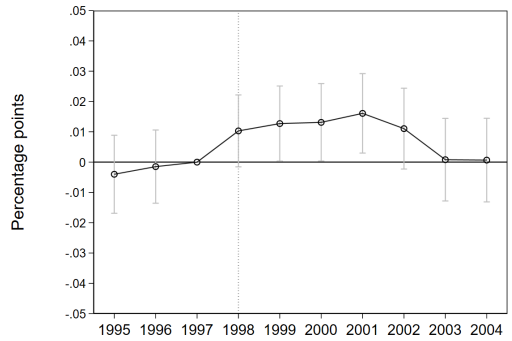


(a) without CEM weights

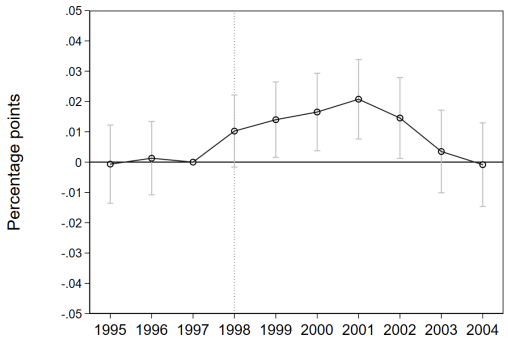
(b) with CEM weights

Note: Figure A3a shows the raw difference between the two groups. Figure A3b shows the earnings distribution after CEM matching. The matching variables are gender and earnings quintile for each year between 1995-1997.

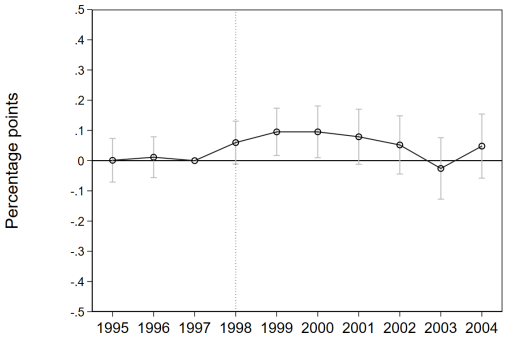
Figure A4: Reform effects from triple difference on extensive and intensive margins of prescription drug utilization



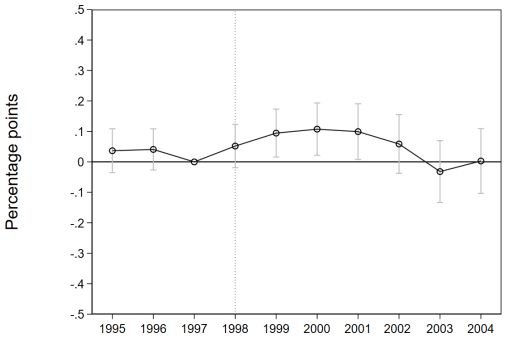
(a) Without controls, extensive margin



(b) With controls, extensive margin



(c) Without controls, intensive margin



(d) With controls, intensive margin

Note: The estimates presented are based on the triple difference specification. Estimation was run with OLS and with clustered standard errors at the individual level. The confidence interval represents 95% levels.