Four essays on ageing, health and labour market participation

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Summary

This thesis consists of five essays: An introductory essay and four essays within the topic of ageing, health and labour market participation. In the introduction I motivate the research questions, discuss how it relates to empirical economics and summarise each of the four papers.

The first project studies how a reform that changed the monetary incentives to delay retirement affect health and healthcare utilisation. The identification strategy relies on the 2011 Norwegian pension reform that increased the monetary incentives to remain employed for nearly half of the private sector workers at age 62. Before 2011, nearly half of the private sector and the entire public sector had access to early retirement (ER) pension. ER pension embodied certain aspects that could create disincentives to remain employed once reaching the ER eligibility age. First, people who retired with full ER pension continued accumulating pension points as if they had remained employed until the normal retirement age of 67. Second, the combination of earnings and ER pension faced an earnings test that proportionally reduced future pension entitlements. Instead of increasing the age at which individuals could retire with ER pensions, an important aspect of the 2011 pension reform was the introduction of flexible claiming together with employment, and the removal of the earnings test. I exploit these changes in the empirical analysis, and identify the effect of the reform by comparing potential changes in the health and employment of private sector workers, who in the absence of the reform, would have been entitled to the full ER pension, to public sector workers. Public sector workers are suitable as a comparison group, since workers in this sector experienced no change in ER pensions. I use several objective measures of health and healthcare utilisation. These are acute hospitalisations and hospital days following an acute hospitalisations, number of visits to a general practitioner (emergency room or health clinic) together with three diagnoses on cardiovascular, musculoskeletal and psychological issues, and the probability to die by age 64. The results from this paper are twofold. First, I document an average decrease in the probability of full retirement at ages 62–64, by around 10 percentage points, with a corresponding increase in the probability of remaining employed at the same ages of around 8.5 percentage points. Second, I show that these results have no clear side effect on health. The results indicate that there is a reduction in hospital days for the entire sample, and the probability of dying by age 64 for females. However, I find some indications of an increase in the probability of experiencing an acute hospitalisation for higher educated people, and I find an increase in cardiovascular issues among females. I conclude that a time-frame of 2 years leads to modest changes in objective measures of health and healthcare utilisation.

The second project investigate the short term effect of retirement on age. To identify the causal effect of retirement, we employ an regression discontinuity (RD) design. RD exploits institutional settings that determine access to a treatment. The idea is that the treatment (retirement) is determined by a running variable (age), reaching a known threshold (the statutory retirement age) that discontinuously change the probability to retire. The discontinuity gap in health at the cutoff age of 67 identifies the treatment effect. We assess the health effects of retirement at age 67, which is an important policy contribution since current retirement reforms typically aim at increasing the retirement age. We use both survey and administrative data to study the short-term effect of retirement. We belive that our health measures, collectively, will provide important insight into the multidimensional effects of retirement on health. The empirical findings of the paper show that there is a sizeable and positive effect of retirement on physical health. In contrast, we find no effect of retirement on acute hospitalisations or mortality. The results shows that while individuals, in the short term, experience a change in self-perceived heath, this does not necessarily translate into a change in more objective measures of health. We also assess the effect by socioeconomic status. Economic theory predicts that individuals with low socioeconomic status have to rely more heavily on their health as an input to the labour market compared to individuals with higher socioeconomic status. This is exactly what we found when considering the subjective measures of health; in contrast, objective of health mask no such heterogeneity. Altogether, this paper adds to a large body of literature on the relationship between retirement and health. We conclude that while retirees may regard their health better compared to those who are just below the statutory retirement age, this is only informative to the extent that it reflects self-rated health and not objective measures of health.

The third project studies a targeted policy aimed at workers aged 60 in Norway, namely the one-week extra holiday that employees aged 60 and above are entitled to by law. Until 2009, the length of vacation depended on the month of birth in the year an employee turned 60. We exploit this institutional setting in a sharp regression discontinuity design. The probability of receiving an extra week of vacation changed sharply depending on whether a worker was born in August or September. The institutional detail created a unique quasi-experimental setting: only individuals born between January and August were entitled to an extra week of vacation in the same year, whereas individuals born between September and December had to wait until the subsequent year for the extra week. We found that an increase in entitlement to vacation had no effect on sickness absence exceeding 16 days. Moreover, we found a decrease in the number of sick notes as authorised by a physician, but the effect is not robust to different specifications. The subsample estimates show that an increase in entitlement to vacation resulted in a significant decrease in the number of sick notes for women and individuals with high school as the highest level of education attained. For females, the point estimates corresponds to a reduction in sick-notes of 24%, whereas for individuals prone to sickness absence, the point estimates corresponds to a reduction of around 38%. However, turning to cause specific diagnoses of musculoskeletal, cardiovascular and psychological issues, we found no effect. Since we could not actually observe if the individuals actually used their entitled vacation in the year in question, the results are the intention-to-treat effect of being made eligible for treatment. The findings have important policy implications as a growing share of people are nearing retirement, and around 25% on disability insurance in Norway are aged 60-64. Targeted policies that adapt to the needs and preferences of employees as they get older may be of importance to mitigate this problem, but the paper questions the extent of the health-argument of increased vacation at age 60.

The fourth project study the labour market responses for individuals whose spouse experienced a health shock. Serious illness can have adverse consequences for the person with ill health and we study how the other spouse's cope with such events. The paper's identification strategy consists of an event study in which we assume that the event (the health shock) is difficult to predict regardless of the presence of any risk factor. We define a health shock by focusing on a particular set of outcomes that are assumed to stem from a major life event. We link these outcomes to the unique administrative data. First, we identify individuals whose spouse passed away due to ischemic heart disease, stroke or a transport accident. Second, we identify individuals whose spouse was admitted to the hospital because of an acute, non-planned admission, as a result of three conditions: myocardial infarction, stroke or congestive heart failure. We find that individuals' whose spouse experiences a fatal health shock endure a reduction in both earnings and employment. The effect is significant and relatively high for widowers, whose income decreases by around 8%, which is persistent for the next five years after the death of their spouse. We find no effect on widows' earnings, but both widows and widowers experience a decrease in employment. On average, widows and widowers reduce their employment by 2% and 3%, respectively. We document large flows of liquid assets after the death of a spouse, which potentially offset some of the lost earnings, but we find no clear pattern when analysing the effects by education and age. We find that a spouse's non-fatal health shock results in no significant effect on income and employment. We do find a drop in income for spouses who experiences a non-fatal health shock, but this does not seem to affect the other spouse's.

Contents

Chapter 2:

The Health Effects of a Pension Reform. Evidence From a Change in Monetary Retirement Incentives

The Health Effects of a Pension Reform: Evidence From a Change in Monetary Retirement Incentives*

Otto Sevaldson Lillebø†

October 1, 2018

Abstract

This paper examines the effect of retirement on health and healthcare utilisation. To accomplish this, I exploit the Norwegian 2011 pension reform that increased the monetary incentives to delay retiring for nearly half of the private sector workers at age 62. I take advantage of rich panel data and estimate the effect on health and labour market participation at ages 62–64 years. The intention-to-treat estimates shows that the targeted group of workers did increase their labour market participation at these ages. The results on health, however, are mixed. For the entire sample, I find no effect on acute hospitalisation, hospital days, visits to a physician or diagnoses related to cardiovascular, musculoskeletal or phsychological issues, or on the probability of dying by the age of 64. There is a degree of heterogeneity in the outcomes, and these results indicate that the probability of experiencing an acute hospitalisation increased for individuals with high education. Yet, for females, the probability of dying by age 64 decreased. Altogether, the results suggests that gender and education are important sources of heterogeneity, but increased employment for workers aged 62 is in general not coupled with a worsening in health or an increase in public healthcare expenditures through changes in objective measures of health and healthcare utilisations.

Keywords: health, retirement, mortality, inpatient care, intention-to-treat estimates
JEL Codes: I10, I18, J14, J26

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1 Introduction

Demographic trends projects that the global numbers of adults aged 65 years and older will double to around two billion by 2050 (World Health Organization, 2015).[1] The same demographic forecasts show that the fraction of retired relative to employed will decrease, and as a result, several European countries either has or is on the verge of implementing policies aimed at prolonging individuals' working life (Hofäcker, 2015).[2] This raises two important policy questions about individuals affected by such reforms. First, on the supply side, does the restructuring of the pension systems have its desired effects in that workers postpone retirement and retain their position as employees? Second, on the spillover side, if retirement is postponed, will this be coupled with any adverse effects on workers' health? From a policy standpoint, the potential financial benefits of increased employment for older workers stems from workers postponing retirement. Yet, any fiscal gain from increased employment can potentially be offset if this leads to a worsening in health and an increase in the demand for health care.

Understanding the potential influence of a prolonged working life on health is important, but causal estimates are inherently difficult because of the simultaneous nature of health and labour market participation. Health affects people's employment decisions, whereas employment affects people's health, in a negative or positive direction. Thus, credible identification requires an exogenous shock that directly affects individuals' employment decision but not their health. Furthermore, what constitutes as a good measure of health and how to credibly measure health remain an open question in the literature.[3]

To overcome the potential challenges of endogeneity, the setting of the present paper is the Norwegian 2011 pension reform. The Norwegian old age system is based on a national insurance scheme (NIS). The NIS system provides a minimum pension benefit for all retirees in Norway once they reach the normal retirement age (NRA) of 67, but before 2011, the entire public sector and nearly half the private sector had access to early retirement (ER) pension. The ER scheme was introduced to combat the rising share of disability insurance (Bratberg et al., 2004), but this came at a cost of disincentives to remaining employed. First, workers eligible for ER could fully retire at age 62, but a full ER pension was coupled with an earnings test that implied a high marginal tax for any worker who wished to combine

[1] Recent forecasts by Statistics Norway show that the share of individuals aged 70 or older will increase from 12 percent in 2018 to 21 percent in 2060 (Syse et al. (2018)). United Nations (2015) projects that the share of individuals aged 80 or older in Norway will increase from 4.2% in 2015 to 8.8% in 2050, and that the share of individuals aged 65 or older will increase from 16% in 2015 to 25% in 2060.

[2] Several countries have increased the mandatory retirement age, e.g. Israel from 65 to 67, Ireland from 65 to 70 and UK have abolished the compulsory retirement age (Hofäcker, 2015).

[3] I discuss the different outcomes of health in the next section.

retirement and employment.[4] Second, individuals who retired with full ER pensions accrued pension benefits as if they had remained employed until the NRA. Economic disincentives for prolonged employment, and preferences for leisure among older workers, represent prominent explanations for why a large share of workers retire with full ER pensions (Kudrna (2017)).

Instead of increasing the age at which individuals could retire with full ER pensions, an important aspect of the 2011 pension reform was the introduction of flexible claiming together with employment, and the removal of the earnings test that proportionally reduced ER entitlements for people who combined ER pensions with employment. This means that workers can combine work and retirement without facing a high implicit tax on income through the means of an earnings test. In the private sector, all workers could now start claiming NIS old-age pensions at age 62, conditional on earnings above a certain threshold.[5] As a result, the full ER pension in the private sector was completely abolished and implemented as a top-up annuity.

I exploit these changes in the empirical analysis, and identify the effect of the reform by comparing potential changes in the health and employment of private sector workers, who, in the absence of the reform, would have been entitled to the ER pension, to public sector workers. More specifically, I exploit the cohort variation in the timing of the reform and compare cohorts born in 1949 and 1950 with the counterfactual health of cohorts 1945-1947, with the inclusion of public sector workers as a comparison group to account for age differentials and other general period effects.[6] Public sector workers are particularly suitable as a comparison group, since workers in this sector experienced no change in ER pensions.[7]

To investigate whether the prolonged employment has an effect on health, and if so, which aspects of health, I use several objective measures of health and healthcare utilisation. First, I use visits to a physician along with three corresponding diagnoses, as follows: musculoskeletal, cardiovascular, and psychological diagnoses. Cardiovascular disease (CVD) is the leading cause of death globally (World Health Organization, 2015), whereas musculoskeletal and psychological diagnoses are among the leading contributors to disability worldwide, and hence, an important aspect of individuals' employment (see Kessler et al. (2003), Murray and Lopez (1997) and World Health Organization (2002, 2015, 2018)). From

[4] Labour earnings below \$1800 (in 2016 amounts) was considered as a 'grace-amount', in which no adjustments in benefits occurred. Earnings above this resulted in a proportional reduction in future benefits.

[5] After the implementation of the reform, public sector workers could give up their entitlements in favour of the new NIS old-age pension system. However, this was not economically favourable and few people have chosen to do so (Hernæs et al., 2016).

[6] A special set of transitional rules were in place for the 1948-cohort, and consequently, as will be explained in the institutional section, this cohort is not included in the analysis.

[7] A life-expectancy adjustment was introduced for public sector workers as well, but at age 67, which is the statutory retirement age in Norway.

the national patient register (NPR), I identify all acute hospitalisations with corresponding days hospitalised, and from the cause of death (CAD) register, I include information of the month in which a person died. Altogether, this provides a comprehensive picture of average changes in health and healthcare utilisation at both the intensive and extensive margins, in the years after the reform, for individuals aged 62–64.

The results from this paper are twofold. First, I find an average decrease in the probability of full retirement at ages 62–64, by around 10 percentage points, with a corresponding increase in the probability of remaining employed at the same ages of around 8.5 percentage points. As these outcomes may mask important heterogeneity, I further investigate the outcomes by gender and educational level and find that there are no differences among genders, but that the reform affected lower educated individuals to a greater extent. Second, I show that these results have no clear side effect on health. The results indicate that there is a reduction in hospital days for the entire sample, and the probability of dying by age 64 for females. However, I find some indications of an increase in the probability of acute hospitalisation for higher educated people, and I find an increase in cardiovascular issues among females.

One possible explanation for the findings is that the health outcomes have a low incidence rate. This means that any change in health may not be traceable to the objective measures of health. I document that a timeframe of 2 years leads to modest changes in objective measures of health and healthcare utilisation, which potentially has important policy implications. Increased employment for workers aged 62 is potentially not coupled with a worsening in health or an increase in public healthcare expenditures through changes in objective measures of health and healthcare utilisations.

This paper proceeds as follows: Section 2 provides a discussion of previous literature. Section 3 discusses the Norwegian pension system and the implications of the 2011 pension reform. Section 3 discusses the data, construction of the treatment and control groups, and outcomes. Section 5 discusses the methodological framework and presents the paper‘s results. Section 6 concludes.

2 Background

2.1 Retirement and health: a literature review

Early work on the relationship between retirement and health documented a negative correlation between early retirement and health (see Dwyer and Mitchell (1999) for a review). However, this association does not control for selection into early retirement through poor health, because those who retire early are likely to have worse health compared with those who maintain their employment status after age 62. Hence, studies using regression methods on cross-sectional or longitudinal data or a fixed-effects approach are likely biased because of the reverse causality stemming from bad health as a predictor of early retirement.

More recent studies on the association between retirement and health have moved toward methods that seek to solve the problem with selection and reverse causality through a quasi-experimental design (for example Neuman (2008); Rohwedder and Willis (2010); Hernæs et al. (2013); Gorry et al. (2015); Hallberg et al. (2015); Bloemen et al. (2017)). These studies relate to two (somewhat overlapping) empirical approaches, by exploiting either quasi-experimental variation through reforms or age-specific retirement incentives in a two-step instrumental variable (IV) approach. Yet, conflicting findings continue to be reported on the the effect of retirement. This can (in part) be explained by the variety of econometric methods, as well as the different measures of health used in the empirical analyses. The outcomes can be divided into subjective measures of health (e.g. self-rated health and objective measures of health (e.g. hospitalisations or mortality). In an excellent review in Currie and Madrian (1999), the concept of how to measure health is discussed. The authors conclude that the estimated effects of health may be very sensitive to what measure used. In what follows, I review the literature and distinguish between subjective and objective measures of health.

Subjective measures of health: In studies based on subjective measures, the data come from sources like the Survey of Health, Ageing and Retirement in Europe (SHARE), the U.S. Health and Retirement Study (HRS), German Socio-economic Panel (SOEP) Study or English Longitudinal Study of Ageing (ELSA). Studying the increase in propensity to retire at ages 60 and 65 in Germany, Eibich (2015) uses data from the SOEP study and employs a fuzzy regression discontinuity design, exploiting the change in probability to retire at age 60 and 65. He finds that retirement increases the probability of reporting satisfactory mental and physical health, and the effect is especially salient for workers who retired from strenuous jobs.[8] Behncke (2012) uses data from ELSA and exploits the exogenous variation from state pension age in a propensity score matching and IV approach. She finds that retirement increases the risk of being diagnosed with a self-reported chronic condition. Rohwedder and Willis (2010), Coe and Zamarro (2011) and Insler (2014) use data from the HRS, and the increase in (self-reported) probability to retire as an instrument, and find that retirement has a beneficial effect on self-rated health.

Both Gorry et al. (2015) and Mazzonna and Peracchi (2017) argue that health consists

[8]A similar design is used by Grøtting and Lillebø (2018) to study the effect of retirement at age 67 in Norway. They find an immediate increase in self-rated health following retirement, but no short-term effect on either hospitalisation or mortality.

of stock variables that either evolve slowly over time, are transitory or occur instantaneously following a major life change like retirement. Some aspects of health respond slowly to investments on the positive side or disregard on the negative side. Using panel data from the HRS, Gorry et al. (2015) instrument for age-based variation in eligibility for retirement benefits. They find that life satisfaction increases immediately after retirement, whereas health evolves slowly, yet positively, over time. The authors use a measure that consists of an index of eight prevalent health conditions (e.g. diabetes), and they find that the health index improves 4 years after retirement. They interpret their findings as showing that retirement causes improvements in health in the short and long runs, and some measures change instantaneously, whereas other measures evolve slowly over time.

Mazzonna and Peracchi (2017) use data from SHARE and find important heterogenous differences between workers retiring from strenuous jobs compared with the rest of the workforce. For all but workers retiring from a strenuous job, they find a decline in cognitive abilities and subjective health. Using social security eligibility as an instrument for retirement decisions in the United States, Bonsang et al. (2012) show that retirement is associated with a negative effect on cognitive functioning, as measured by word learning and recall tests. The effect occurs at age 63, following retirement at age 62. At age 64, the effect is similar to that estimated at age 61 (a year before retirement), whereas it seems to be a downward path until age 70. What remains unclear, as highlighted by Bonsang et al. (2012), is whether the cognitive decline is a result of retirement, or that retirement is associated with a certain loss of purpose or social interaction that, in turn, has a negative effect on cognitive skills.[9]

Taken together, the studies using subjective measures of health employ eligibility rules through IV to circumvent the endogeneity problem of retirement and health. Previous research has shown that different health outcomes lead to different interpretations of the association between retirement and health. This is also the conclusion in a literature review by van der Heide et al. (2013). In other words, health is complex conceptually, since reporting symptoms or self-perceived health is affected by many confounding factors, which may explain why measures of self-rated health lead to conflicting results in the literature.

To my knowledge, only one paper has studied the effect of an increase in retirement age on subjective measures of health. Shai (2018) exploits an increase in the male full retirement age from 65 to 67 in Israel and compares subjects retiring at this age with cohorts not affected by the reform. The results show that prolonged employment as a result of the increase in retirement age results in a deterioration in health, which is especially salient

[9] A theoretical framework proposed by Grossman (1972) supports these findings, arguing that retirement leads to a so-called unengaged lifestyle. Once retired, people reduce their investment in cognitive abilities. Social relations (social capital) and mortality is also extensively studied in the epidemiological literature. See, for example, Holt-Lunstad et al. (2010) for a meta-analytic review.

among individuals with low education. He constructs several indices of health based on questions related to physician visits, the health index and the severe morbidity index. As discussed in the present paper, the use of visits to the physician may be problematic because retirees and workers do not necessarily visit a physician for the same reason, especially when physicians serve as gate keepers for sickness absence.

Objective measures of health: Recently, the increased availability of longitudinal administrative data has led researchers to use outcomes of health as recorded by third parties. The use of longitudinal data may alleviate issues of attrition, as well as justification bias.

The predominant measure of health is mortality. Hernæs et al. (2013) use the stepwise reduction in the early retirement age in Norway from 67 to 62 in an IV setup, and they find that early retirement has no effect on mortality. Acknowledging that ill health related to mortality is a health stock that evolves over time, they follow individuals until the age of 77, but find no statistically significant effect. The authors question whether retirement has a causal effect on mortality. Hallberg et al. (2015) exploit a targeted early retirement window for the Swedish military, in which the retirement age was lowered from age 60 to 55. Using Cox regression models to investigate the effect of the reform on mortality, they find that retirement reduces the risk of dying by age 77 by 26%.

Bloemen et al. (2017) studies a group of male civil servants in Holland who were induced to retire early through an early retirement window. The window offered early retirement at age 58 instead of 62. They find that the probability of dying within 5 years after retirement is reduced by 2.6 percentage points. Furthermore, the effect occurs immediately following retirement, and seems to persist from year 1 to year 5.[10] A potential problem with the studies by Hallberg et al. (2015) and Bloemen et al. (2017) is that both exploit a 'window' in which a certain group of workers were offered to retire early. In Hallberg et al. (2015) and Bloemen et al. (2017), the early retirement window was offered to people who would otherwise have lost their jobs through layoffs and worked in a specific sector. Thus, it is not clear how these effects map to the rest of the workforce.

To gain a further understanding of workers' health, Hallberg et al. (2015) investigate the effect of early retirement for male civil servants on inpatient care at age 56–70. They find that the early retirement opportunity reduced the number of days in inpatient care by around 35% (or 6.7 days). From a policy standpoint, these results may be problematic, since a reduction in the retirement age led to lower demand for health care through reduced inpatient care numbers. In addition, the estimated effect is even more salient for the compliers. In summary, they find that opportunity to retire early decreased both mortality and number of days in

[10]The authors state that the numbers represents the smallest impact of retirement on mortality. In other words, the authors reports the lower bounds estimates.

inpatient care.

To the best of my knowledge, only one other paper has studied the effect of an increase in retirement age combined with objective measures of health. Hagen (2018) studies an increase in the normal retirement age in Sweden from 63 to 65 local government workers, and compares the subjects' health with private sector workers not affected by the reform. His sample consists of female workers, because few men were employed as local government workers. Using outcomes measured at age 65–69, he finds no effect on outcomes concerning prescription of drugs, probability of being hospitalised or number of days hospitalised. In addition, he finds no effect on mortality through the age of 69.

Against this background, the direction in which retirement affects health remains unclear. This is especially evident when considering measures of health as observed through survey data, as the outcome of interest, method and time-frame usually varies. This is not necessarily a drawback, but underlines the dynamic nature of the retirement process. In what follows, I relate my contribution to those of Hernæs et al. (2013, 2016), Shai (2018) and Hagen (2018).

3 Institutional Setting: The 2011 Norwegian Retirement Reform

In this paper, I take advantage of a 2011 reform that restructured the pension system in Norway. As some of the aspects of the system before the reform are important elements for the empirical analysis and understanding the changes that came with the reform, I start with a discussion of relevant institutional features as they were before the reform. I then proceed to discuss the Norwegian 2011 Pension Reform and how it relates to the empirical strategy.

3.1 The Norwegian pension system before 2011

3.1.1 General structure

The Norwegian pension system is based on the NIS, in which every worker is enrolled, conditional on at least 3 years of residency in Norway.[11] The system was formed around a pay-as-you-go defined benefit scheme and workers accumulated pension points based on earnings that exceeded a minimum threshold, known as 1 basic amount,[12] up to a contribution cap. Pension points were accumulated throughout individuals' working lives, and the old age pension consisted of basic pension (minimum guaranteed pension) and supplementary pension (earning components). To receive the full minimum guaranteed pension, a 40

[11] This section borrows information from Hernæs et al. (2016) and Kudrna (2017).

[12] The basic amount is used in relation to most of the NIS payments, and is adjusted each year.

year period of residence was required, with a proportional reduction for each year without accumulating the basic pension (i.e. no residency). The supplementary pension was calculated based on averaging pension points over the best (i.e. highest income) 20 years with positive pension points.

3.1.2 Early retirement pensions

The NRA before the reform was 67 years, but nearly half the private sector and the entire public sector had access to ER pension at age 62, financed by the government on a pay-as-you-go basis. Conditional on relatively weak income requirements, around 70–80% of the workforce could retire with full ER pension at the age of 62 (Bratberg et al., 2004; Kudrna, 2017). The ER pension was paid up to the age of 67, and people were automatically transferred to the NIS old-age pension system after this point.

Once retired with ER pensions, retirees that wished to combine ER and employment were subject to a strict earnings test. Apart from a small 'grace-amount' (NOK 15,000 ≈ 2016-USD 1,800), any income from employment resulted in a proportional reduction in ER pensions, which represented a high implicit tax rate. In addition, individuals who retired with the ER pension continued the accrual of pension points as if they had continued working after the age of 62. Taken together, this created strong disincentives for employment after the age of 62 for eligible workers, especially for low income workers (Hernæs et al., 2016).

3.2 The 2011 pension reform

Implemented in January 2011, Norway reformed its pension system based on the goal of improving the long-term fiscal sustainability (Kudrna, 2017). Rather than increasing the age at which workers could retire with ER, the reform had a clear goal, through increased incentives, for workers to remain employed after the age of 62. First, the NRA was reduced from 67 years of age, and conditional on some previous earnings requirements by accumulated pension points, claiming the NIS old age pension could commence at age 62. Second, the ER system in the private sector was completely redesigned, and it now serves as a top-up annuity in combination with the NIS old age pension. The earnings test in the old ER system was abolished, and workers are free to combine retirement and work without facing a proportional reduction in the old age pension. According to Hernæs et al. (2016), the removal of the earnings test resulted in a reduction in the implicit tax rate from around 70% to the region of 40%, for average earners. Third, actuarially fair recalculations of annual benefits were introduced by the means of life expectancy in a given cohort.

Due to a breakdown in talks with the unions in the public sector, the salient aspects of the reform, especially the restructuring of the ER system, was only implemented in the private sector. Public sector workers can give up their current entitlements in favour of the new system, but Hernæs et al. (2016) show that few have chosen to do so, and consequently, remaining in the current system (as it was before 2011) is economically favourable for public sector workers. Hence, individuals employed in the public sector after the age of 62 continue to face strong labour supply disincentives through high implicit marginal tax rates on combining employment and retirement, as indicated by Hernæs et al. (2016) and Kudrna (2017).[13]

Table 1 provides an overview of the ages, years and birth cohorts affected by the 2011 reform. One aspect of the reform is that transitional rules were specified for those born between 1945 and 1948, displayed by the light shades. The transitional rule meant that workers could choose between the new and old system if they retired before 2011. Those who turned 62 years of age in 2010 (i.e. born in 1948) could claim a full ER pension in 2010. In 2011, the same cohort had to abide by the new, albeit transitional, pension rules if they did not retire in 2010. In the analysis, the 1948 cohort is left out of the analysis, since they were subject to special transitional rules that could spark an anticipation of the reform.[14,15]

Table 2 summarises how the reform affected different groups of workers, conditional on sector and entitlement to ER. Note that this table resembles Table 1 in Hernæs et al. (2016), but includes additional cohorts (1950–1952), which may explain the difference in the share of workers included in each group, compared with Hernæs et al. (2016). I split workers by sector affiliation at age 62. If employed in the private sector, workers are categorised by entitlement to ER (as it was before the reform). Next, I condition on whether workers could hypothetically retire with full old age pension at age 62. To retire at age 62, workers have to fulfil some earnings requirements, and consequently, individuals with an earnings history below the requirement have to postpone full retirement by at least 1 year. These are included in groups 4 to 6. To summarise, the 2011 pension reform affected workers differently, conditional on sector and workplace affiliation to ER pensions. I exploit these difference in the empirical framework.

[13]Life-expectancy adjustments was introduced in the public sector as well, but only at the statutory retirement age of 67.

[14] Note that these transitional rules were introduced as a monetary compensation since workers could not increase their pension entitlements. The same compensation was introduced for the 1944–1947 cohorts as well, although to a much lesser extent.(Hernæs et al., 2016)

[15]Hernæs et al. (2016) drops the 1948-cohort for the same reason, whereas Vigtel (2018) leaves them in the analysis, studying the effect of the reform on the propensity to hire senior workers

4 Data

4.1 Data sources and outcomes

I use Norwegian full population register data with information on all residents, given that they lived in Norway at some point during 1992–2014. To arrive at the analysis sample, I combine several registers that are linked through a unique anonymous identifier. I start by defining the outcomes and how these relates to the sample selection.

4.1.1 Employment and entitlement to early retirement pension

Information on employment is taken from the employer-employee register, which is available from 1992 to 2014. For each worker, I identify a person and his or her respective workplace through an identification number, along with contracted hours of work. To determine eligibility for ER pension, I follow Bratberg et al. (2004) and assume that an individual works in an ER-affiliated firm if at least one previous employee retired with an ER pension.

To calculate accrual of pension points, I include information on previous earnings and social security benefits, which is available from 1967. These records contain earnings and income from self-employment, plus social insurance benefits (e.g. unemployment benefits), which are subject to income tax and warrant accrual of pension points. Information is reported by third parties and is reliable because it is a matter of public record, and comes without any form of top or bottom coding. From the administrative records, I also use social security files to identify the month at which individuals retire or if they have retired with disability insurance (DI). DI requires a minimum of a 50% reduction in work capacity due to health impairments.

4.1.2 Socioeconomic measures

I combine the information discussed above with information on the highest attained education, marital status,[16] immigration background and gender. I split educational attainment through three groups. The first consists of individuals that completed the mandatory level of education, that is junior high (Ungdomsskolen), or if they dropped out of high school. The second group consists of individuals who completed high school, and the third contains individuals with at least 1 year of higher education, but not necessarily with a complete degree. In the empirical analysis, I assume that education is a proxy for socioeconomic status.

[16] Whether individuals are single, married, or a legally registered partner. Thus, I am not able to identify cohabiting partners not married or legally registered as a partner. The latter mostly concerned gay couples, and were abolished in 2009 when same-sex couples legally could get married.

4.1.3 Outcomes of health and healthcare utilisation

The contribution of this paper consists of investigating whether the 2011 Norwegian pension reform had any subsequent effect on objective measures of health. In investigating this issue, I rely on three different objective measures of health and healthcare utilisation. The first is information about mortality from the cause of death register (CAD) register. This register contains all deaths recorded in Norway between 1992 and 2014, along with the month in which the death occurred. The outcome I construct is a dummy that takes a value 1 if a person died by the age of 64 and 0 otherwise.

Second, I use the Norwegian Patient Registry (NPR) to identify all acute hospitalisations (inpatient stays) from 2008 to 2014. The NPR data contain information in accordance with whether treatment was deemed necessary and could not be postponed or if a person was admitted for a planned surgery. The data also contain classification of the disease that resulted in an inpatient stay, as measured by the International Classification of Diseases version 10 (ICD-10; see World Health Organization (1992)). I focus on acute hospitalisations as this does ensure some severity of a patient's illness, but at a cost of a reduction in the incidence rate. However, it is difficult to disentangle the severity between planned and acute hospitalisations, and as a result, planned admissions are not included as an outcome in the empirical analysis. Thus, the outcomes I construct represent a dummy that takes the value 1 if a person was hospitalised at ages 62 to 64, as a result of an acute hospitalisation, and 0 otherwise. I also study the intensive margin of healthcare utilisation through days hospitalised following an acute hospitalisation. This is done by subtracting the date of discharge from the date of admission.

Third, I utilise information on visits to general practitioners (GPs). For every visit to a GP (or emergency care unit), the GP sends a reimbursement claim to a common register. From this register, I identify the number of visits and corresponding diagnoses. Individuals visit the GP for different reasons, and unlike information on inpatient stays, this register contains no information on the severity of the health condition, but the reimbursements claim include information on diagnoses in accordance with the International Classification of Primary Care-2nd edition (ICPC-2). Based on the ICPC-2 classification, I construct, in addition to number of GP visits, three variables on three specific groups of diagnoses, as follows: *the numbers of GP visits per year for CVD,*[17] *musculoskeletal pain*[18] *and psycholog-*

[17] CVD and diagnoses related to, among others, coronary heart disease and ischaemic heart disease. According to World Health Organization (2018), the former is the leading cause of death globally.

[18] According to World Health Organization (2015), musculoskeletal problems (e.g. back problems) are the second largest contributor to disability worldwide. See Murray and Lopez (1997) for an extensive discussion about leading causes of disability-adjusted life-years. About 70% of those receiving DI in Norway are above the age of 59 (NAV, 2018).

ical issues. CVD is the leading cause of death in most developed countries (World Health Organization (2015)), whereas musculoskeletal (e.g. lower back problems) and psychological issues are leading pathways to disability insurance worldwide (World Health Organization (2015)).

4.1.4 Sample restrictions

This paper is interested in people who turned 62 years of age in the years before and after 2011. Thus I start by identifying all those born between 1945 and 1952, which comprises 459,679 individuals. Due to the transitional rules explained in the Section 3.2, I drop 64,699 persons born in 1948, and I also drop those who died before 2006. This leaves 433,125 individuals. Next, I restrict the sample to consist of those who, at the age of 60, were employed and did not draw on any form of disability insurance. The reason for the latter restriction follows from the interest in investigating whether changes in work incentives affect the health and labour supply for workers nearing the retirement age. An individual is employed within a year if he or she fulfils two important criteria, as follows: income equal to or above the minimum amount required to accrue pension points and also that each individual is identified in the employer-employee register. The latter is important for determining entitlement to ER pension, and the construction of treatment and comparison groups, as will be explained in section 5. This leaves a sample of 223,734 individuals.

For the empirical analysis, I impose two additional restrictions. First, I drop all individuals born in either 1951 or 1952. Because I am interested in the effect of the reform at ages 62 to 64, these are left out of the final sample.[19] Second, as discussed in the next section, the empirical analysis uses private sector workers who, in the absence of the reform, would have had access to full ER pensions at age 62, and compare their outcome to private sector workers, as both groups hypothetically could retire with NIS old-age pensions at age 62. This leaves me with 113,185 individuals. The panel is unbalanced, given that a certain number of individuals died before the end of the observation period.

[19]The data spans until 2014, which means that I only observe the 1952 cohort aged 62, and the 1951 cohort at ages 62 and 63. In the NPR-data, as a consequence of data being available from 2008, I do not observe the 1945-cohort at age 62. This is not of a big concern since the cohort is included in all the other outcomes. Thus, the 1945 cohort is left in the sample.

5 Research design

5.1 Identification

The goal of this paper is to estimate the average effect on health and labour supply responses following the changes in incentives to prolong employment at ages 62 to 64. This opens up at least two methodological difficulties that must be addressed to overcome potential endogeneity. The first is the joint determination of retirement and health. Retirement may affect people's health, but health may also affect the decision to retire. This two-way relationship between retirement and health may lead to selection bias. The other, somewhat overlapping concern, is that unobserved factors affect both health and the decision to retire. For instance, a worker may have an extreme disutility for work, which is not only likely to affect the decision to retire, but also his or her health. Thus, these unobserved factors pose a risk of biasing any regression on the effect of retirement on health.

To address the possible endogeneity, I follow Shai (2018) and Hagen (2018) and use a difference-in-difference (DiD) framework that exploits the quasi-experimental nature that arises from the implementation of the 2011 pension reform. The reduced form estimates will then yield the effect of how the 2011 pension reform affected health, healthcare utilisation, and employment, for a specific group of workers. To isolate the causal effect of the reform, I then need a comparison group, or groups, that allows for credible counter-factual identification of the average effect if the reform never materialised.

One potential comparison is using the private and public sector workers in the DiD framework against the public sector. For a worker in the private sector, the effect of the reform depends on entitlement to ER and previous earnings. In the absence of the reform, workers in the private sector not entitled to ER would have had to wait until age 67 to retire with the NIS old-age pension. However, after the implementation of the reform, these workers, conditional on previous earnings, could retire at age 62. These workers thus experienced a different change in incentives to remain employed after the age of 62, compared with private-sector workers previously entitled to ER pensions. As a result, this group is not included in the analysis.

Another potential comparison is using private-sector workers who, in the absence of the reform, would have been entitled to ER pension at age 62, and comparing their outcomes with public-sector workers. However, some of these workers had an earnings history that would prevent them from entering full retirement at age 62. Likewise, conditional on giving up the current ER entitlements, some workers in the public sector would not have been entitled to full retirement at age 62 either. In Table 2, these are referred to as groups 4 and 5. This leaves two groups, one of which experienced an increase in incentives to remain employed at age 62 (group 2 in Table 2) and one of which experienced few changes in incentives to remain employed at age 62 (group 1 in Table 2).

More specifically, I exploit the 2011 retirement reform that increased the monetary incentives to remain employed at age 62 of those born in 1949 or later in group 2. In the DiD setting, I use those born in 1945–1947 to estimate the counterfactual effect on health and labor market outcomes of those born in 1949 and 1950. To address the likely cohort effect of this set up, I use public sector workers as a comparison. In the next section, I explain the estimation of this set up.

5.2 Estimation

For individual i in cohort j in sector P, I run the following regression on the outcome Y at ages 62–64:

$$Y_{i,j,t} = \beta_0 + \beta_1 P_s + \beta_2 (P_s \times T_{j\in[1949,1952]}) + \lambda_j + \beta_3' \mathbf{X}_{i,j,s} + \varepsilon_{i,j,s}, \tag{1}$$

where P_i takes the value of 1 if individual i worked in a private sector firm that had signed up for the ER scheme (group 2 in Table 2) and 0 if individual i works in the public sector (group 1 in Table 2). $T_{j\in[1949,1952]}$ takes the value 1 if individual i is born in 1949–1952 and 0 if born in 1945–1947. I add cohort fixed effects (λ_j) to account for potential labour market shocks and time-invariant differences between the treatment and control groups. $\mathbf{X}_{i,j,s}$ is a set of baseline control variables measured at age 60 and includes years of schooling, gender, marital status, income and contracted work hours, which is similar to what used by Hernæs et al. (2016). The parameter of interest is β_2, which by estimation of ordinary least squares (OLS) regression, yields the intention-to-treat effects at ages 62–64 of the change in retirement incentives at age 62, for private sector workers. The reduced form model is similar to that of Hagen (2018).[20]

Table 3 displays summary statistics for the cohorts affected by the reform (post) and the cohorts not affected by the reform (pre), by treated and control groups. The fixed characteristics and labour-market outcomes are measured at age 60. The outcomes are measured in the post-treatment period (at ages 62–64). There are some differences between pre and post-treatment for each group, as well as between treated and control groups. The level of education is higher in the public sector, whereas the share of females is lower in the

[20] In a paper studying how the 2011 pension reform affected the firms propensity to hire senior workers, Vigtel (2018) uses a similar DiD-framework. His method is formed around the years before and after the implementation of the reform. This present paper, however, is interested in how health evolves at certain ages. To that end, I therefore follow Hagen (2018) who study outcomes at a given age for a specific set of cohorts.

private sector compared with the public sector. In addition, income is higher in the private sector compared with the public sector. However, these differences should not be of concern if the outcomes follow similar trends. I investigate the extent to which the different outcomes follow similar trends across cohorts in the Section 5.3.

As an implicit underlying model, I assume that any effect of the 2011 pension reform on employment and retirement behaviour has an indirect effect on people's health. The identifying assumption for the DiD estimator is that, in the absence of the reform, any trend in post-retirement health or utilisation of health care should be the same for private- and public- sector workers. I discuss the validity of this assumption in the next section.

5.3 Threats to identification

The validity of the DiD-method requires that, given the control variables, the only thing that could explain the differentials between treatment and non-treated is the 2011 pension reform. This is an untestable assumption, since we do not know what would have happened with employment or health for the treatment group in the absence of the reform. Regardless, a common approach in the literature is to check how the trends evolved before the implementation of an intervention, which in this case, is the 2011 pension reform. If the trends evolve in a similar way before the reform, it gives confidence that the post-treatment trends would have evolved in the same way too.

Figure A.1 plots the unconditional, unweighted mean outcomes at ages 62–64 for the private- and public-sector workers, by cohort. The two upper graphs display the labour market outcomes. Figure 1(a) show that the 1945 and 1946 cohorts follows quite similar trends of employment. However, for the 1947 cohort, there seems to be an reduction in employment at ages 62–64, compared with the control group. As discussed in Section 3.2, the 1947-cohort was subject to the 2011 pension reform at age 64, and moreover, would have to retire at age 63 to receive the old ER pensions. If this caused the 1947 cohort to change their retirement behaviour at ages 62–64, this anticipatory effect may cause problems for the quasi-experimental design. Thus, I run the pooled regressions on the labour market outcomes with 1947 against 1945 and 1946, as displayed in Table A.1. The results show that there are no significant differences in the probability of employment between the 1947 cohort at age 62–64 and the remainder of the unaffected cohorts.

Next, Figure 1(b) displays similar trends between the treated and control groups in the unaffected cohorts, whereas, as illustrated in Figure 1(a), the gap is reduced for the cohorts affected by the reform.[21] The next two panels display the time trends for the outcomes of

[21]Note that the contracted work hours is missing for some workers, but given the quasi-experimental design, this should not be of a concern.

health. Figure 1(c) displays the share of the population who experienced a hospitalisation at age 62–64, whereas Figure 1(d) displays hospital days, conditional on an acute hospitalisation. There is a small reduction in the gap for the 1947 cohort in Figure 1(d), but the remainder of the cohorts in both figures seems to follow a similar time-trend. Figures 1(e) and 1(f) display the share who died by age 64 and number of GP consultations, respectively. Overall, the trends for the unaffected cohorts are similar, apart from a reduction in the gap between treated and controls for the 1947 cohort. Taken together, the trends for the unaffected cohorts (1945–1947) are similar, whereas some differences between the treatment and control groups opens up in the affected cohorts. Apart from Figure 1(d), Figure A.1 shows that the parallel trend assumption is close to satisfied.

6 Analysis

6.1 The effect of the reform on employment and retirement

Any effect of the reform on employment and retirement is a precondition for quantifying a possible spillover to people's health. Table 4 presents the labour market outcomes at ages 62–64. As in Hernæs et al. (2016), the probability of employment increased, and probability of retiring decreased. The point estimates show that the probability of employment increased by 9 percentage points, whereas the probability of retiring decreased by 9 percentage points. The 1950 cohort is driving some of the effect, but the magnitude of the point estimates is smaller than that found in Hernæs et al. (2016).[22] Either way, I conclude that the reform increased the probability of employment, and reduced the probability of retiring for the affected private-sector workers previously entitled to full ER pensions.

6.2 The effect on health and health care utilisation

6.2.1 The effect on inpatient care

Having established that the reform increased the average probability of employment and reduced the probability of retirement in the treatment group, I next investigate whether there are any spillover to objective measures of health and health care utilisation. I start by investigating whether there are any effects on the probability of an acute hospitalisation at ages 62–64. I include the same set of control variables as in the previous section, and the outcome variables consists of a dummy equal to 1 if individual i was hospitalised in a

[22] Hernæs et al. (2016) finds an increase in the probability of employment by around 12 percentage points at age 63 and 20 percentage points at age 64.

calendar year t, and conditional on experiencing an acute hospitalisation, number of days hospitalised.[23]

The results in Table 5 show that there were no effects of the reform on acute hospitalisations, or the number of hospital days. Splitting the analysis by cohorts, the point estimates of the coefficients indicates an increase in the probability of acute hospitalisation for the 1950 cohort. The effect is significant at the 10% level and corresponds to an increase of 0.3 percentage points. Column 2 in Table 5 displays the estimated effect on the number of inpatient days following an acute hospitalisation. For individuals born in 1949, the point estimates shows a reduction in number of days hospitalised, following an acute hospitalisation. The effect is significant at the 5% level, and implies a reduction of 0.053 hospital days, which corresponds to a 15% reduction.

The direction of the estimates points toward a small reduction in days hospitalised as a result of an acute hospitalisation, but the point estimates are generally too imprecise for any meaningful interpretation. When I estimate the outcome by cohort, the standard error does increase as a result of the reduction in sample size. In any case, the precision of the estimated effects are not convincing.

A possible caveat associated with the use of acute hospitalisations as a measure of health and healthcare utilisation is that it is a severe and (usually) serious outcome, meaning that the incidence rate is relatively low. As a result, the dependent variable includes a large set of zeroes, which may cause problems for any meaningful inference in the linear model. Hallberg et al. (2015) condition on days of inpatient stays only, comprising both planned and acute hospitalisations, and find that a reduction in retirement age in the Swedish military, resulted in reduced inpatient care by 2 and 4.7 days for those aged 56–60 and 61–70, respectively. This is a reduction of 35%. The effect is estimated using a pseudo-maximum-likelihood estimator, which may take better care of all the nulls in the dependent variable. However, this comes at a cost of more stringent assumptions about the error term. I conclude that the reform led to no change in the probability of experiencing acute hospitalisations, but there is some, albeit deficient, evidence that the number of days in inpatient care is reduced.

6.2.2 The effect on healthcare utilisation through visits to a physician

To provide a comprehensive picture of any potential effects on health, I now include measures of healthcare utilisation that may be less severe than an acute hospitalisation. To this end, I create a variable that counts the number of GP visits each year, along with three important diagnoses related to workers' health and work capacity.

[23] Days hospitalised is calculated by subtracting day of admission from day of discharge.

Table 6 presents the estimation results. In Column 1, for the entire sample, there is no significant effect of the reform on the number of GP consultations. This is interesting bearing in mind that, at least in Norway, GPs have a gatekeeper role in that they certify sickness absence.[24] Once retired, the opportunity cost of seeing a physician has fallen, and one would expect that an increase in employment among elderly workers would lead to an increase in GP consultations.[25]

Columns 2–4 in Table 6 displays estimates by cause-specific diagnoses. For the entire sample, the point estimates show that there is a slight increase in diagnoses related to cardiovascular issues, significant at the 10% level. However, when splitting the sample by the affected cohorts, the results remain insignificant. For the two other diagnoses, musculoskeletal and psychological, the estimated effect is insignificant. Taken together, there seems to be no effect of the reform on any of the measures of health care utilisation related to visits to a physician.

6.2.3 The effect on mortality

Table 7 displays the estimated effect on the probability of dying by the age of 64. The results show that there are no significant effects on the probability of dying by the age of 64 for the entire sample or splitting by the two affected cohorts. One crux of using mortality in the empirical analysis is the age at which I observe the treated, that is, ages 62–64. The time frame may be too short to expect any changes in mortality rates. Previous literature studying the effect of retirement on mortality has followed individuals well into their 70s. Hallberg et al. (2015) study the consequence of targeted ER of male military workers up to the age of 70, whereas Hernæs et al. (2013) follow individuals to the age of 77. An exception is Bloemen et al. (2017), who studied the probability of dying 1 to 5 years after a targeted reduction in the eligibility age for civil servants in Holland. They find an instant effect on mortality, with a decrease in probability of dying within 1 year after retirement of around 2.2 percentage points. This effect is persistent up to 5 years after retirement.

More in line with the findings of Hernæs et al. (2013), I find no causal effect of the reform on mortality. Moreover, as in Hernæs et al. (2013), I question whether mortality should have any relevant policy implications and conclude that the reform had no effect on the probability of dying by the age of 64.

[24] Spells of sickness absence of 3–7 days are usually self-certified without needing to consult a GP.
[25] The time-cost for seeing a physician varies between workers and retirees.

6.3 Heterogeneity

So far, I have examined the effect for the entire sample, and these results show no apparent effect of the reform on objective measures of health or healthcare utilisation. However, the results may mask important heterogeneity, and the average results can also obscure variation in the influence of the reform and the potential effect it had on health and healthcare utilisation. Thus, I assess whether there are any heterogeneous effects of splitting the sample by gender, and socioeconomic status proxied by education. In each subsample, I also assess the differences in employment responses, to uncover whether heterogeneity in response to the reform is driving the estimated effects. As with the previous section, I assume an underlying model in which the effect of employment indirectly affects workers' health.

6.3.1 Effect by gender

Employment: It is clear that the reform had an employment effect, which confirms the findings of Hernæs et al. (2016). However, it is important to investigate whether the reform had a different effect on genders, and if so, whether this resulted in a spillover to the outcomes of health and healthcare utilisation. Table 8 presents the estimated effect of the reform on the probability of employment, retirement and retaining at least 80% of contracted work hours as measured at age 60. The first three columns display the estimated effects for males, whereas the remaining columns display the estimated effects for females.

The estimated probability of remaining employed in the post-treatment period, in absolute terms, is three-quarters larger for women than it is for men. Females seem to increase employment and delay retiring at a larger fraction than that observed for males. The two other columns, probability of retirement and retaining similar work hours as at age 60, remain similar between the genders. In addition, the magnitude of the estimated effects remain somewhat similar when performing the same analysis separately for the different cohorts.

Hospitalisation: Table 9 displays the estimated effect of the reform on the probability of experiencing an acute hospitalisation and number of days hospitalised, by gender. For acute hospitalisations, the results are not significant, regardless of gender. Turning to hospital days, Column 2 shows that for the 1949 cohort, there is a significant reduction of 19% in days hospitalised for males at the 5% level, whereas for males as a whole, or for females, there is no significant effect.

GP visits: I next look at the effect on healthcare utilisation through visits to the GP and the three diagnoses. Table 10 presents the results. As discussed above, a priori, one would expect an increase in the number of consultations, as retirees and employees do not necessarily visit their GP for the same reason. The latter group may have to visit their

GP to certify their sickness absence, whereas this is not necessary for retirees. However, for males, Column 1 shows that there are no significant changes in number of GP visits for all the cohorts pooled together. For females, Column 6 displays no significant effect on the number of GP consultations either.

Turning to the three diagnoses, there seems to be a small increase in number of musculoskeletal diagnoses for males born in 1950 (Column 3). Moreover, there seems to be a small increase in number of cardiovascular diagnoses for females as well (Column 6). The point estimates in Column 3 is significant at the 10% only. For females, displayed in Column 6, the results indicates a small increase in number of cardiovascular diagnoses by around 7%. However, the effect is not persistent when looking at each of the two affected cohorts.

There is ample evidence in both the economic and psychological literature that retirement may lead to a loss of purpose, reduction in cognitive skills, and thus, an increase in symptoms associated with depression (Behncke, 2012; Mazzonna and Peracchi, 2017). However, while the directions of the estimates indicate a reduction in the number of psychological diagnoses, none of the estimates are significant at any of the conventional levels.

Mortality: Turning to the outcomes for mortality, Table 11 displays the probability to die by the age of 64 by gender. For the cohorts pooled together, there is no significant effect on the probability of dying for males, whereas there is a significant reduction for females at the 10% level. The magnitude of the point estimates implies a 0.01 percentage point reduction in the probability of dying by the age of 64, which equals a 3% reduction. Moreover, splitting the estimates by the affected cohorts, the results show a 0.02 percentage point reduction in the probability of dying by the age of 64, equal to a 6% reduction. Taken together, the results indicates that, for females, there seems to be a positive effect of the reform in that there is a small reduction in the probability of dying by the age of 64. For males, the results shows no significant effect of the reform.

6.3.2 Effect by educational level

One finding in the retirement literature is that workers who retried from a strenuous job, experience a better self-rated health (see for example Kuhn et al. (2010)). Unfortunately, what constitutes as a strenuous job (blue collar vs. white collar) is difficult in the administrative data. Even if I link the employer-employee register to sectors characterised as manual labour (e.g. carpenter), this is only informative to the extent that it characterises the sector and not necessarily whether the job is strenuous. Thus, I therefore follow Shai (2018)), and proxy socioeconomic status by whether workers have any form of higher (tertiary) education, since education is an important determinant of health (Mazzonna and Peracchi, 2012). No form of higher education means that a person has completed high school, whereas higher education means that a person has completed at least 1 year of tertiary education.

Employment: Table 12 displays the estimated effect of the reform on the probability of employment, retirement and retaining at least 80% of the contracted work hours as measured at age 60. Given that the reform increased the monetary incentives to postpone retirement at age 62 for a certain groups of workers, the impact seems to be higher for lower educated individuals compared with workers with high education. The probability of employment in the post-treatment period seems to be three times as high for workers with low education, compared to workers with high education. Similar results emerge when considering the probability to retire and probability to retain at least 80% of the contracted work hours at age 60. Moreover, the effect is persistent across the different cohorts as well.

Hospitalisation: Table 13 displays the effect on the probability of an acute hospitalisation, and hospital days, by low and high education. The first two columns show the estimated effects for workers with low education, whereas the remaining two columns show the estimated effects for workers with high education. In Column 1, the results show an increase in the probability of experiencing an acute hospitalisation at ages 62–64 for the 1950 cohort. However, the effect is only significant at the 10% level. Column 3 shows that there is a significant increase in the probability of an acute hospitalisation for employees with a higher education. For the cohorts pooled together, the results indicate a 0.7 percentage point decrease, and this effect is significant at the 5% level. The magnitude is the same when splitting by cohort, yet the effect is significant at the 10% level.

In Columns 2 and 4, the results shows that there are no significant effects on hospital days for either low and high education, but for higher educated born in 1952. Yet this effect is only significant at the 10% level. Taken together, there does seem to be some indications of an increase in the probability of an acute hospitalisation, yet the standard errors are still somewhat imprecise. If anything, the results indicates a worsening in health as measured by acute hospitalisation.

GP visits: Table 14 shows that there is no significant effect on the number of GP visits, regardless of educational level. By looking at the different diagnoses, we see that, for low education, there is a small increase in cardiovascular-related diagnoses (Column 2), significant at the 1% level. For high education, there is an increase (Column 8), and this is significant at the 1% level. Splitting the sample by cohorts does not reveal any clear effect as to what drives the estimated results. The effect in Column 8 seems to be driven by the 1950 cohort, whereas there is an significant increase in cardiovascular diagnoses for low education. Taken together, Table 14 indicates that, if anything, there is an increase in diagnoses related to cardiovascular issues for employees with a low education, and in diagnoses related to psychological issues for workers with a higher education.

Mortality. Table 15 displays the estimated probability of dying by the age of 64, by educational level. I find no effect for the pooled sample, or for the 1949 and 1950 cohorts. While I did find a small decrease in the probability of dying by the age of 64 among females, the effect remains insignificant when splitting the sample by educational level.

6.4 Robustness tests

To test the validity of the results, I perform three separate robustness tests, all displayed in Tables A.1 - A.3. First, I follow Hagen (2018) and test whether the results are sensitive to the exclusion of the control variables. Columns 4 through 6 in Table A.1 display the estimated results on the labour market outcomes, whereas Table A.2 presents the results for the outcomes of health and healthcare utilisation. The results are quite close to the main effect displayed in Tables 4–6, which implies that the results are robust to excluding these controls. Column 2 in Table A.2 is now significant at the 5%, and cardiovascular diagnoses become insignificant.

One of the preconditions for being defined as employed was earnings above one basis level, with the other being identified in the employer-employee registry. The first condition may be too lenient, as it is equal to between one-eighth and one-tenth of the mean earnings in the analysis. In Table A.3, I check whether a more stringent earnings criteria affects the results on employment. It turns out that the conclusion still stands in that employment did increase as a result of the reform. As such, all of the robustness tests gives confidence to the validity of the quasi-experimental design and this paper‘s main findings.

7 Discussion and conclusion

Population ageing and increasing constraints on public budgets have led most OECD countries to introduce policies aimed at prolonging individuals' working life. Increasing the labour market participation for workers closing in on the NRA can reduce pressure on public finances, but it can also have a side effect in terms of workers' deteriorating health. This could potentially offset the gains from reduced costs in the pension system through an increase in healthcare expenditures.

In this paper, I study how the implementation of the 2011 Norwegian pension reform affected workers' health and labour market participation. Until 2011, nearly half of the workers in the private sector were entitled to an ER pension at age 62. The ER pension embodied certain aspects that could create disincentives for prolonged employment. Workers who retired with full ER retained their accumulation of pension-points as if they had continued working up until the statutory retirement age of 67. Second, full ER retirement was coupled with an earnings test, which meant a high implicit tax rate on the combination of employment and retirement along with no deferral option.

To increase the incentives of employment after the age of 62, the private sector ER system was completely restructured, with the ER pension now serving as a top-up annuity to the NIS old-age pension. Workers could still retire at age 62, but the removal of the earnings test meant that workers could start claiming a pension at age 62 in conjunction with continued employment. Before 2011, the earnings test meant that any earnings, apart from a small grace amount, would result in a proportional reduction in ER benefits. I explore this setting to obtain causal estimates on how prolonged employment affects workers health using data on hospital admissions, visits to physicians and mortality, combined with high-quality register data.

The results suggest that the reform had its desired effect on the supply side when considering the probability of employment and retirement. I find strong indications that workers affected by the reform, on average, increased the labour market participation at ages 62–64 (and thus their contribution period). This is in line with the findings reported by Hernæs et al. (2016). I further add to their findings by investigating the effect on employment by gender and education. While it does not seem that differences in genders are driving the employment effect, there are some differences between workers with high and low education. The increased probability of employment at ages 62–64 could potentially create two somewhat interrelated mechanisms, namely an income effect and an activity effect. Grip et al. (2012) show that when the Netherlands reduced the replacement rates for retirees, self-rated depression did increase. Second, ample evidence from Mazzonna and Peracchi (2012) and Mazzonna and Peracchi (2017) shows that an increase in working life has repercussions for health.

On the spillover-side, the results yield no clear effect of the reform. On the one hand, I find some indications that prolonged employment resulted in a reduction in hospital days for the sample as a whole, and the probability of dying by age 64 for females. On the other hand, there are some indications that the probability of experiencing an acute hospitalisation does increase for highly educated workers (tertiary education), and there is an increase in diagnoses related to cardiovascular issues among females. The latter results need to be interpreted with some caution, however, since people visits physicians for different reasons depending on whether they are working or are retired. Taken together, the results are in line with those found by Hagen (2018) to the effect that a prolonged working life has extremely little effect on health and healthcare utilisation. When it does, however, the direction of the results leaves little room to conclude whether there are any positive or negative spillovers from delayed retirement.

The findings of this paper adds to the increasing literature concerning retirement and health, in which there is little consensus as of yet. While measures of health through survey data attributes huge negative or positive effects of retirement on health, depending on the outcome, at least from a policy perspective, it is not clear whether these findings take a toll on public spending in the short run. Ignoring spillovers from the increased labour market participation after the age of 62 could lead to misguided policy interventions. Targeted interventions aimed at increasing labour market participation after the age of 62 can potentially reduce pressure on public budgets, without any immediate negative spillover on workers' health. The results in this paper show that, at least in the short term of 2 years, there are few effects of delayed retirement on objective measures of health and healthcare utilisation.

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Graphs and Tables

Table 1: Age groups and birth cohorts affected by the 2011 Norwegian Pension Reform

	Age				
Birth Cohort	**60**	**61**	**62**	**63**	**64**
1945	2005	2006	2007	2008	2009
1946	2006	2007	2008	2009	2010
1947	2007	2008	2009	2010	2011
1948	2008	2009	2010	2011	2012
1949	2009	2010	2011	2012	2013
1950	2010	2011	2012	2013	2014
1951	2011	2012	2013	2014	2015
1952	2012	2013	2014	2015	2016

Notes: The table displays cohorts that were affected (dark shade) by the reform and the cohorts partially affected (light shade) by the reform.

Table 2: Consequence of the 2011 pension reform for different workers at age 62

	After 2011	
Before 2011	Entitled to full retirement at age 62 after the reform.	Not entitled to full retirement at age 62 after the reform.
ER public sector	**Group 1 (41%)** Access age and incentives to work the same as before the reform. Can combine work and retirement conditional on giving up entitlement to ER.	**Group 4 (4%)** Conditional on giving up entitlement to ER, can now combine retirement and work at age 62.
ER private sector	**Group 2 (30%)** Old ER system removed. Can still retire at age 62 conditional on previous earnings, but old earnings test removed (increase in work-incentives).	**Group 5 (1%)** Earliest access age to full retirement increased, and old earnings test removed for combining retirement and work between 62 and 66.
Private sector - Not entitled to ER	**Group 3 (21%)** Earliest access age to full retirement reduced from age 67 to age 62, conditional on previous earnings.	**Group 6 (3%)** No changes in access age or economic incentives at age 62. Can combine reduced pension and retirement between age 62 and age 66.

Notes: Percentage of the workers (in parenthesis) is based on own calculations following the sample restrictions explained in chapter 4. Calculations are done based on the cohorts 1949 through 1952 to show how the new rules affected workers differently, depending on sector and ER affiliation. Group 4,5 and 6 mainly consists of female (around 98%).

Table 3: Characteristics of different workers at age 62 - Before 2011 (pre-reform) and after 2011 (post-reform)

	Treated		Control	
	Pre	Post	Pre	Post
Characteristics at age 60				
Income ($1000)	71.14 [25.60]	83.91 [29.78]	62.11 [21.51]	75.39 [24.56]
Weekly work hours	20.52	22.41	21.42	22.87
	[18.00]	[17.67]	[16.97]	[16.68]
Immigrant	0.06	0.07	0.06	0.08
No Education	0.21	0.17	0.09	0.07
High School	0.59	0.60	0.37	0.36
Some College	0.20	0.22	0.54	0.58
Female	0.25	0.24	0.59	0.59
Labor Market Outcomes				
Income ($1000)	51.84 [42.31]	71.38 [45.88]	54.20 [35.86]	66.95 [49.55]
Employed	0.73	0.85	0.82	0.86
Retired	0.20	0.10	0.10	0.08
Workhours as at age 60	0.30	0.39	0.38	0.42
Health Outcomes				
GP-Consultations	3.46	3.53	3.50	3.54
	[3.45]	[3.49]	[3.38]	[3.47]
Cardiovascular Diagnoses	1.63	1.24	1.34	1.22
	[3.45]	[3.40]	[3.05]	[2.95]
Musculoskeletal Diagnoses	1.32	1.46	1.50	1.61
	[2.79]	[3.40]	[3.01]	[3.22]
Psychological Diagnoses	0.39	0.39	0.52	0.58
	[1.55]	[1.74]	[1.99]	[2.3]
Acute hospitalisation	0.053	0.061	0.050	0.055
Hospital Days	0.391 [3.27]	0.395 [2.84]	0.323 [2.93]	0.334 [2.74]
Mortality by age 64	0.007	0.006	0.007	0.006
Number of observations	30,423	18,455	38,949	25,358

Notes: This table displays descriptive statistics for the treated and control groups, by pre and post reform. The outcomes are measured at ages 62–64, whereas the fixed characteristics are measured at age 60. Treated refers to private sector workers who, in the absence of the reform, could have retired with a full ER pension at age 62, but are eligible for full NIS old-age-pensions after 2011. Control is public sector workers eligible for full NIS old-age-pensions at age 62, conditional on giving up the public ER pensions entitlements.

Table 4: The effect on the probability of employment, retirement and retaining similar contracted work-hours as at age 60

	(1) Employed	(2) Retired	(3) hourWeek80_60
CH * T	0.082***	-0.090***	0.039***
	(0.003)	(0.002)	(0.003)
Cohort 1949 * T	0.071***	-0.085***	0.030***
	(0.003)	(0.003)	(0.004)
Cohort 1950 * T	0.094***	-0.094***	0.049***
	(0.003)	(0.003)	(0.004)
Mean dep. var.	.813	.126	.371
Number of Observations		339,555	

Notes: This table presents the effect of the interaction term on employment status using OLS. The regressions compare a dummy for employment status, retirement status and probability of working similar workhours as at age 60 for the treatment group against a control group of public sector workers. I add cohort fixed effects and a set of baseline covariates measured at age 60: income, dummy for immigrant, education, gender, marital status and contracted work hours. Number of observations is N*T. Robust SEs in parenthesis. *=p<0.10, **=p<0.05, ***=p<0.01.

Table 5: The effect on the probability of an acute hospitalisations and number of hospital days

	(1) =1 if hospitalised at ages 62 through 64	(2) Hospital days
CH * T	0.0023	-0.0158
	(0.0016)	(0.0208)
Cohort 1949 * T	0.0008	-0.0533**
	(0.0021)	(0.0255)
Cohort 1950 * T	0.0038*	0.0226
	(0.0021)	(0.0266)
Mean dep. var.	.054	.355
Number of Observations	339,555	

Notes: This table presents the effect of the interaction term on a dummy indicating acute hospitalisations, and number of inpatient days within a year using OLS. The regressions compare the health outcomes for the treatment group with a control group of public sector workers. I add cohort fixed effects and a set of baseline covariates measured at age 60: income, dummy for immigrant, education, gender, marital status and contracted work hours. Number of observations is N*T. Robust SEs in parenthesis. *=p<0.10, **=p<0.05, ***=p<0.01.

Table 6: The effect on GP consultations and three diagnoses

	(1)	(2)	(3)	(4)
		Cause specific diagnoses based on ICPC-2		
	no. of GP consultations	cardiovascular (heart)	musculoskeletal	psychological
CH * T	0.0185	0.0393*	0.0136	-0.0148
	(0.0236)	(0.0212)	(0.0197)	(0.0124)
Cohort 1949 * T	0.0173	0.0397	-0.0098	-0.0068
	(0.0297)	(0.0270)	(0.0248)	(0.0152)
Cohort 1950 * T	0.0193	0.0388	0.0376	-0.0227
	(0.0303)	(0.0268)	(0.0259)	(0.0170)
Mean dep. var.	2.903	1.183	1.220	0.387
Number of obs.		280,779		

Notes: This table presents the effect of the interaction term on number of GP consultations per month and three relevant diagnoses. The regressions compare the health outcomes for the treatment group with a control group of public sector workers. I add cohort fixed effects and a set of baseline covariates measured at age 60: income, dummy for immigrant, education, gender, marital status and contracted work hours. Number of observations is N*T. Robust SEs in parenthesis. *=p<0.10, **=p<0.05, ***=p<0.01.

Table 7: The effect on the probability of dying by age 64

	(1)
	=1 if died by age 64
CH * T	0.0002
	(0.0005)
Cohort 1949 * T	0.0001
	(0.0007)
Cohort 1950 * T	0.0003
	(0.0006)
Mean dep. var.	.0050
Number of Observations	339,555

Notes: This table presents the effect of the interaction term on the probability of dying by age 64. The regressions compare the outcomes for the treatment group with a control group of public sector workers. I add cohort fixed effects and a set of baseline covariates measured at age 60: income, dummy for immigrant, education, gender, marital status and contracted work hours. Number of observations is N*T. Robust SEs in parenthesis. *=p<0.10, **=p<0.05, ***=p<0.01.

Table 8: The effect on the probability of employment, retirement and retaining similar contracted work-hours as at age 60, by gender

	(1)	(2)	(3)	(4)	(5)	(6)
		Male			Female	
	Employed	Retired	>80% workhours as at age 60	Employed	Retired	>80% workhours as at age 60
CH * T	0.081***	-0.094***	0.040***	0.097***	-0.095***	0.044***
	(0.004)	(0.003)	(0.004)	(0.005)	(0.004)	(0.005)
Cohort 1949 * T	0.071***	-0.088***	0.032***	0.091***	-0.096***	0.035***
	(0.004)	(0.004)	(0.005)	(0.006)	(0.005)	(0.006)
Cohort 1950 * T	0.092***	-0.099***	0.048***	0.102***	-0.094***	0.054***
	(0.004)	(0.004)	(0.005)	(0.006)	(0.005)	(0.007)
Mean dep. var.	.806	.132	.338	.823	.117	.412
Number of obs.		189,828			149,727	

Notes: This table presents the effect of the interaction term on employment status using OLS. The regressions compare a dummy for employment status, retirement status and probability of working the same workhours as at age 60 for the treatment group with a control group of public sector workers. I add cohort fixed effects and a set of baseline covariates measured at age 60: income, dummy for immigrant, education, gender, marital status and contracted work hours. Number of observations is N*T. Robust SEs in parenthesis. *=p<0.10, **=p<0.05, ***=p<0.01.

Table 9: The effect on the probability of acute hospitalisations and number of subsequent hospital days, by gender

	(1)	(2)	(3)	(4)
	Male		Female	
	=1 if healthshock	Hospital days	=1 if healthshock	Hospital days
CH * T	0.0001	-0.037	0.003	-0.007
	(0.002)	(0.030)	(0.003)	(0.031)
Cohort 1949 * T	-0.002	-0.080**	0.002	-0.050
	(0.003)	(0.037)	(0.003)	(0.037)
Cohort 1950 * T	0.003	0.005	0.003	0.036
	(0.003)	(0.038)	(0.003)	(0.039)
Mean dep. var.	0.062	0.415	0.044	0.279
Number of obs.	189,828		149,727	

Notes: This table presents the effect of the interaction term on a dummy indicating acute hospitalisations, and number of inpatient days within a year using OLS, by gender. The regressions compare the health outcomes for the treatment group with a control group of public sector workers. I add cohort fixed effects and a set of baseline covariates measured at age 60: income, dummy for immigrant, education, gender, marital status and contracted work hours. Number of observations is N*T. Robust SEs in parenthesis. *=p<0.10, **=p<0.05, ***=p<0.01.

Table 10: The effect on GP consultations, and three diagnoses, by gender

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Male				Female			
	no. of GP consultations	cardiovascular (heart)	musculoskeletal	psychological	no. of GP consultations	cardiovascular (heart)	musculoskeletal	psychological
CH * T	-0.024	-0.003	0.006	-0.016	0.036	0.066^{**}	0.054	-0.024
	(0.032)	(0.031)	(0.024)	(0.016)	(0.039)	(0.030)	(0.037)	(0.032)
Cohort 1949 * T	-0.001	0.001	-0.041	-0.005	0.028	0.062	0.070	-0.001
	(0.040)	(0.039)	(0.030)	(0.019)	(0.050)	(0.038)	(0.047)	(0.040)
Cohort 1950 * T	-0.047	-0.007	0.055^{*}	-0.025	0.042	0.069^{*}	0.038	-0.002
	(0.042)	(0.039)	(0.031)	(0.022)	(0.050)	(0.037)	(0.048)	(0.031)
Mean dep. var.	2.759	1.401	0.996	0.296	3.079	0.906	1.503	.509
Number of obs.	189,828				149,727			

Notes: This table presents the effect of the interaction term on number of GP consultations plus three relevant diagnoses, by gender. The regressions compare the health outcomes for the treatment group with a control group of public sector workers. I add cohort fixed effects and a set of baseline covariates measured at age 60: income, dummy for immigrant, education, gender, marital status and contracted work hours. Number of observations is N*T. Robust SEs in parenthesis.

Table 11: The effect on probability of dying by age 64, by gender

	(1)	(2)
	Male	Female
	=1 if died by age 64	
CH * T	0.001	-0.001*
	(0.001)	(0.001)
Cohort 1949 * T	0.001	-0.002**
	(0.001)	(0.001)
Cohort 1950 * T	0.001	-0.001
	(0.001)	(0.001)
Mean dep. var.	0.0063	0.0034
Number of obs.	189,828	149,727

Notes: The estimates are the effect on the probability of dying by age 64, by gender. The regressions compare the outcomes for the treatment group with a control group of public sector workers. I add cohort fixed effects and baseline covariates measured at age 60: income, dummy for immigrant, education, gender, marital status and contracted work hours. Number of observations is N*T. Robust SEs in parenthesis. *=p<0.10, **=p<0.05, ***=p<0.01.

Table 12: The effect on the probability of employment, retirement and retaining similar contracted work-hours as at age 60, by socioeconomic status

	(1)	(2)	(3)	(4)	(5)	(6)
	Low Education			High Education		
	Employed	Retired	>80% workhours as at age 60	Employed	Retired	>80% workhours as at age 60
CH * T	0.081***	-0.103***	0.037***	0.050***	-0.038***	0.026***
	(0.004)	(0.003)	(0.004)	(0.004)	(0.004)	(0.005)
Cohort 1949 * T	0.067***	-0.097***	0.026***	0.043***	-0.034***	0.018***
	(0.004)	(0.004)	(0.005)	(0.006)	(0.005)	(0.006)
Cohort 1950 * T	0.097***	-0.110***	0.049***	0.057***	-0.041***	0.034***
	(0.005)	(0.004)	(0.005)	(0.005)	(0.005)	(0.006)
	0.782	0.148	0.373	0.859	0.290	0.482
Number of obs.	202,186			137,369		

Notes: The regressions compare a dummy for employment status, retirement status and probability of working the same workhours as at age 60 for the treatment group with a control group of public sector workers. I add cohort fixed effects and baseline covariates measured at age 60: income, dummy for immigrant, gender, marital status and contracted work hours. Number of observations is N*T. Robust SEs in parenthesis. *=p<0.10, **=p<0.05, ***=p<0.01.

Table 13: The effect on probability of acute hospitalisations, and number of subsequent hospital days, by gender

	(1)	(2)	(3)	(4)
	Low Education		High Education	
	=1 if healthshock	Hospital days	=1 if healthshock	Hospital days
CH * T	0.002	-0.019	0.007^{**}	0.028
	(0.002)	(0.029)	(0.003)	(0.036)
Cohort 1949 * T	-0.0001	-0.053	0.007^{*}	-0.048
	(0.003)	(0.036)	(0.004)	(0.037)
Cohort 1950 * T	0.005^{*}	0.017	0.007^{*}	0.103^{*}
	(0.003)	(0.035)	(0.004)	(0.053)
Mean dep. var.	0.057	0.391	0.050	0.303
Number of obs.	202,186		137,369	

Notes: This table presents the effect of the interaction term on a dummy indicating acute hospitalisations, and number of inpatient days within a year using OLS, by socioeconomic status. The regressions compare the health outcomes for the treatment group with a control group of public sector workers. I add cohort fixed effects and a set of baseline covariates measured at age 60: income, dummy for immigrant, gender, marital status and contracted work hours. Number of observations is N*T. Robust SEs in parenthesis. *=p<0.10, **=p<0.05, ***=p<0.01.

Table 14: Effects on GP consultations and three diagnoses, by socioeconomic status

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Low Education				High Education			
	no. of GP consultations	cardiovascular (heart)	musculoskeletal	psychological	no. of GP consultations	cardiovascular (heart)	musculoskeletal	psychological
CH * T	0.049	0.103***	-0.027	-0.025	-0.026	-0.052	0.049	0.103***
	(0.032)	(0.028)	(0.029)	(0.017)	(0.041)	(0.040)	(0.032)	(0.028)
Cohort 1949 * T	0.021	0.083**	-0.061*	-0.023	-0.005	0.005	0.024	0.021
	(0.040)	(0.035)	(0.036)	(0.021)	(0.052)	(0.053)	(0.038)	(0.040)
Cohort 1950 * T	0.079*	0.124***	0.009	-0.027	-0.046	-0.108**	0.079*	0.124***
	(0.041)	(0.036)	(0.038)	(0.023)	(0.052)	(0.047)	(0.041)	(0.036)
Mean dep. var.	3.659	1.151	1.624	0.443	3.278	1.311	1.249	0.506
N	202,186				137,367			

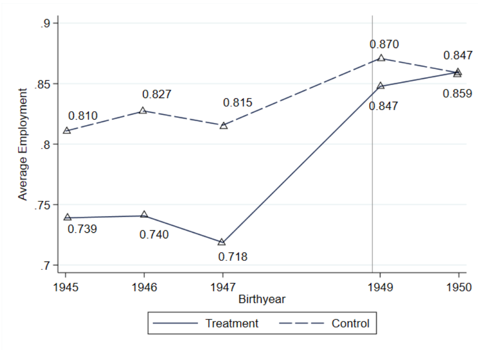
Notes: This table presents the effect of the interaction term on number of GP consultations and three diagnoses, by educaitonal level. The regressions compare the health outcomes for the treatment group with a control group of public sector workers. I add cohort fixed effects and a set of baseline covariates measured at age 60: income, dummy for immigrant, gender, marital status and contracted work hours. Number of observations is N*T. Robust SEs in parenthesis. *=p<0.10, **=p<0.05, ***=p<0.01.

Table 15: Effects on the probability of dying age 64, by socioeconomic status

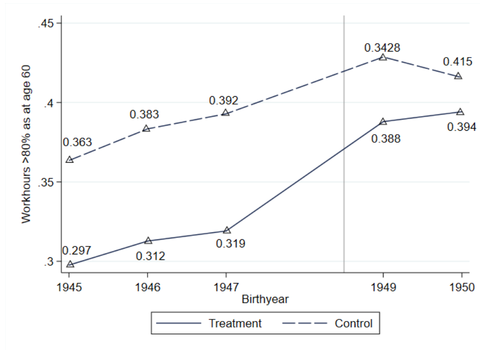
	(1)	(2)
	Low Education	High Education
	=1 if died by age 64	
CH * T	0.001	0.001
	(0.001)	(0.001)
Cohort 1949 * T	0.001	-0.001
	(0.001)	(0.001)
Cohort 1950 * T	0.002	-0.001
	(0.001)	(0.001)
Mean dep. var.	0.005	0.003
N	202,186	137,367

Notes: This table presents the effect of the interaction term on the probability of dying by age 64, by socioeconomic status. The regressions compare the outcomes for the treatment group with a control group of public sector workers. I add cohort fixed effects and a set of baseline covariates measured at age 60: income, dummy for immigrant, gender, marital status and contracted work hours. Number of observations is N*T. Robust SEs in parenthesis. *=p<0.10, **=p<0.05, ***=p<0.01.

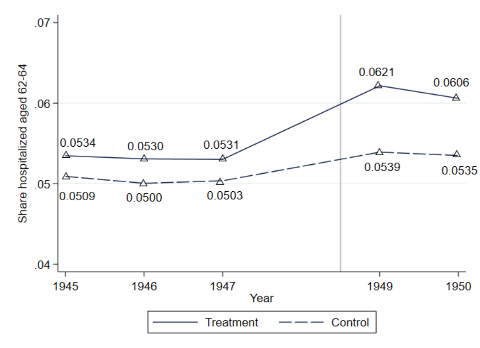
Appendix Graphs and Tables



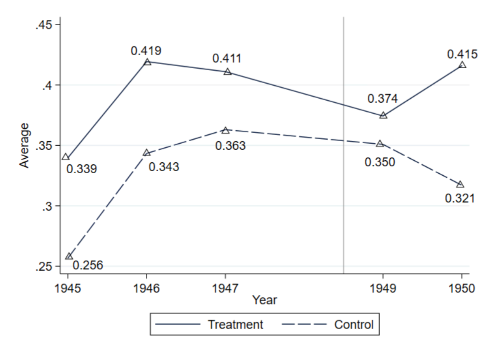
(a) Employment



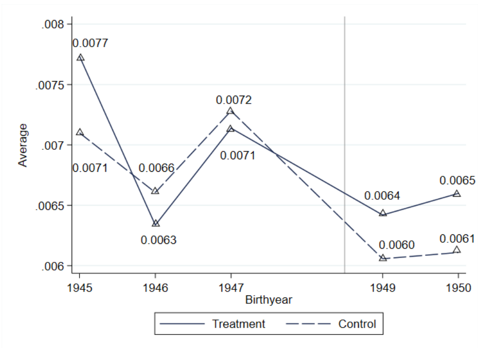
(b) >80% as at age 60



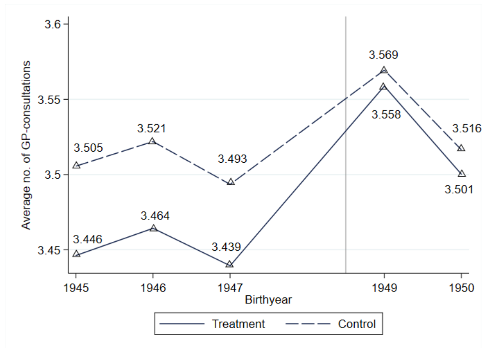
(c) Acute Hospitalisation



(d) Days Hospitalised



(e) Mortality



(f) Number of GP-consultation

Figure A.1: Time trends in outcome variables

Notes: The figure show the differences in unconditional means between the treatment and control group, by cohort.

Table A.1: Robustness - Labour Market outcomes for the 1947-cohort and main results without controls

	(1)	(2)	(3)	(4)	(5)	(6)
	Comparing 1947 with 1945 & 1946			No controls		
	Employed	Retired	>80% of workhours as at age 60	Employed	Retired	>80% of workhours as at age 60
CH * T	-0.006	-0.003	-0.006	0.088***	-0.101***	0.046***
	(0.005)	(0.004)	(0.005)	(0.002)	(0.002)	(0.003)
Number of Observations	208,116	208,116	208,116	339,555	339,555	339,555

Notes: This table presents two diffirent robustness-tests: First is the effect on labour market outcomes for the 1947-cohort and second is the main labour market outcomes without controls. Cohort fixed effects is still included. Number of observations is N*T. Robust SEs in parenthesis. *=p<0.10, **=p<0.05, ***=p<0.01.

Table A.2: Robustness - The effect on health without controls

	(1) =1 if healthshock	(2) Hospital days	(3) no. of GP consultations	(4) cardiovascular (heart)	(5) musculoskeletal	(6) psychological	(7) =1 if died by age 64
CH * T	0.003	-0.008**	0.023	0.022	0.027	-0.012	-0.0002
	(0.001)	(0.018)	(0.021)	(0.018)	(0.018)	(0.013)	(0.001)
Obs.	339,555						

Notes: This table presents the main effect on the health-outcomes without controls. I still add cohort fixed effects. Number of observations is N*T. Robust SEs in parenthesis. *=p<0.10, **=p<0.05, ***=p<0.01.

Table A.3: Robustness - The effect on different earnings thresholds

	(1)	(2)	(3)	(4)	(5)	(6)
	>1BP	>2BP	>3BP	>4BP	>5BP	>6BP
CH * T	0.085***	0.085***	0.083***	0.080***	0.067***	0.061***
	(0.002)	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)
Number of Obs.	339,555					

Notes: This table presents the effect on employment for different earnings threshold. BP=Basispoints. 1 basispoint is the lowest amount required to accrue pension points. I include cohort FE. Number of observations is N*T. Robust SEs in parenthesis. *=p<0.10, **=p<0.05, ***=p<0.01.

Chapter 3:

Health Effects of Retirement. Evidence from Survey and Register Data

Health Effects of Retirement: Evidence from Survey and Register Data*

Maja Weemes Grøtting †‡ Otto Sevaldson Lillebø§

October 1, 2018

Abstract

Using a local randomized experiment that arises from the statutory retirement age in Norway, we study the effect of retirement on health across gender and socioeconomic status. We apply data from administrative registers covering the entire population and from survey data of a random sample to investigate the effects of retirement on acute hospital admissions, mortality, and a composite physical health score. Our results show that retirement has a positive effect on physical health, especially for individuals with low socioeconomic status. We find no retirement effects on acute hospitalisations or mortality in general. However, our results suggest that retirement leads to reduced likelihood of hospitalisations for individuals with low socioeconomic status. Finally, we show that the positive health effects are driven by reduced pain and reduced health limitations in conducting daily activities. Our findings highlight heterogeneity in the health effects across socioeconomic status and across subjective and objective measures of health.

Keywords: retirement, health, socioeconomic status, gender, regression discontinuity design

JEL Codes: H75, I14, I18, J26

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1 Introduction

With increasing life expectancy, the number of retired individuals as a share of the total population is rising in most OECD countries. This has led to concerns about the fiscal sustainability of public pension systems, and to policy initiatives that aim to prolong working lives and increase retirement age. An important issue that seems to be overlooked in policy debates over these reforms is the impact that prolonged working lives has on health, and especially if there are heterogeneous retirement effects by socioeconomic status (SES).

Findings in the empirical literature regarding the health effects of retirement are mixed. Some studies report positive effects (Coe and Zamarro, 2011; Eibich, 2015), whereas others report negative effects (Behncke, 2012; Mazzonna and Peracchi, 2017) or no effects (Hernæs et al., 2013; Heller-Sahlgren, 2017). Although some studies highlight the importance of (SES) in these health effects (Coe and Zamarro, 2011; Eibich, 2015), there is limited evidence from formal tests to suggest the effects differ by SES. Another limitation in the literature is that most studies assess retirement effects in the early 60s, an age threshold that is substantially lower than proposed policies to postpone retirement toward age 70. retirement towards age 70. Finally, most studies rely on survey data or administrative records (of sub-samples of the population), which often imply subjectivity in the health outcomes or small sample issues.

In this paper, we investigate the health effects of retirement across socioeconomic status and gender in Norway by applying both survey and administrative data, where the latter cover the entire population. We assess the health effects of retirement at age 67, which was the statutory retirement age in 2007. This is a higher age threshold than what has previously been studied. To control for individuals self-selecting into retirement, we exploit that the statutory retirement age causes a discontinuous change in the likelihood of retiring at the exact timing of eligibility. This implies a local randomization around the retirement eligibility age threshold, and makes a regression discontinuity (RD) framework suitable. We compare the health outcomes for those right above the statutory retirement age threshold (i.e. the treatment group) to those right below (i.e. the control group). This allows for identification of the causal short-term effects of retirement on health.

Most studies in this field rely on survey data with the well-known limitations related to non-response and recall bias. Furthermore, while measures of subjective health provide important insights into how individuals experience and rate their own health, such measures have been criticized for being contextual, and can suffer from justification bias (see e.g. McGarry (2004) for a thorough discussion). Another possible concern is that survey data of older adults is especially prone to health related selection, as non-response or attrition is correlated with poor health.

The Norwegian administrative data is attractive with respect to overcoming some of these concerns. In particular, administrative data covers the entire population and records certain health conditions as truly objective. Still, measures of health from public registers are often extreme outcomes, such as mortality and acute hospital admissions, and hence unsuited for studying moderate health effects. In addition to records of mortality and acute hospital admissions from public registers, we include a composite measurement of self-assessed health from a representative sample of Norwegian older adults (The NorLAG Panel Survey (Slagsvold et al., 2012). This measure is the short form-12 (SF-12) health survey (see (Ware Jr et al., 1996). We assess both the overall physical score and the specific components that goes into the SF-12.

We believe that our health measures, collectively, will provide important insight into the multidimensional effects of retirement on health. Moreover, both data sources (the administrative data and the NorLAG data) contain exact birth month and retirement date from public registers, ruling out recollection bias. Finally, having monthly records allows for a more precise estimation of the effects of retirement on health, as it enables a more local estimation around the timing of retirement compared to analyses using data on the year level.[1]

Socioeconomic status is important in the analysis of health effects of retirement because it determines the kind of work situation an individual retired from. Higher education and white collar jobs are often less physically demanding and associated with greater autonomy and control over the work situation, compared to low SES jobs (Case and Deaton, 2005; Mazzonna and Peracchi, 2012). Moreover, Case and Deaton (2005) document that manual labor jobs, associated with low education and low income, are more "wear and tear" types of jobs, in which health deteriorates at a more rapid pace than individuals in a "non-manual" professions.

According to the (Grossman, 1972) model of health demand, individuals with low education or low financial capital (low SES) will have to rely more heavily on their health as an input in the labor market, compared to individuals with higher SES, as the different sources of capital are substitutes in the labor market. This is typically manifested through strenuous manual labor for the low SES groups. Moreover, individuals with higher education are assumed to be more efficient in promoting their own health. In sum, the two mechanisms make it more costly for low SES groups to continue working. Retirement can therefore be seen as a mechanism that levels health inequalities between SES groups. As SES can be an important factor in analyses of retirement and health, we systematically assess how the

[1]See Dong (2015) and Lee and Card (2008) for a discussion of why age in years might yield inconsistent results unless properly accounted for.

health effects differ by socioeconomic status.

The RD application in this study identifies the short term health effects of retirement. On the one hand, we can expect to see short term effects on health as the relief from strenuous physical work or the relief from working in a stressful environment is an instantaneous change of circumstance. On the other hand, retirement may lead to a reduced sense of purpose before new routines has been developed (Rohwedder and Willis, 2010).

Our results show that retirement yields a sizeable and positive effect on physical health. This effect is especially strong for the low SES group, whereas we find no effects for the high SES group. We find no effects on mortality or acute hospitalisations in general. However, for the low SES group, we find that retirement leads to a reduction in the likelihood of acute hospitalisations. Our results show that SES is important when studying the effect of retirement on health, but we find no gender differences. Moreover, we find that the reason why retirement leads to better physical health is due to reduced pain and a lower likelihood of reporting that physical health is a limitation in completing both "daily" tasks and "specific tasks profoundly". The results for physical health and mortality are robust to a wide range of robustness and specification checks, whereas the checks regarding the results for hospital admissions are less robust, and must therefore be interpreted with some caution.

The paper proceeds as follows. Section 2 provides a review of previous research and describes the institutional structure of the Norwegian pension system. Section 3 describes our empirical strategy. In Section 4, we present the data, outcome variables, and some basic summary statistics. Our main results are presented in Section 5, and Section 6 concludes.

2 Earlier literature and institutional sSetting

2.1 Earlier literature

Our paper is related to a growing body of economic research about the effect of retirement on health. Given the important aspect of this issue and the vast amount of literature on the topic, there is a surprising lack of consensus across studies. One reason for this is that a large fraction of the existing evidence reports correlations rather than well-identified causal effects. Lately, there has been an increasing amount of well-identified studies, most of which apply exogenous variation in the retirement eligibility as sources of identification. As the majority of these studies apply survey data or administrative records for sub-samples of the population, we contribute to the literature by providing objective health outcomes for the entire Norwegian population.

One of the most cited related studies is Coe and Zamarro (2011). They study the extent to which retirement affects measures of self-reported health and a composite health index across several European countries using the Survey of Health, Ageing and Retirement in Europe (SHARE) data. They find that retirement reduces the likelihood of reporting bad self-rated health and leads to an improvement in a composite measure of subjective health.

From the US setting, Neuman (2008) uses age-specific retirement incentives as instruments for retirement. Applying data from the Health and Retirement Study (HRS), he provides evidence of retirement being both preserving and improving for self-rated health. He argues that since retirement removes the time constraint induced by labor market participation, more time can be devoted to activities that both preserve and enhance individuals' health. This is in line with Grossman's model of health demand, where it can be shown that especially time-intensive workouts may be more attractive after retirement, when the opportunity cost of participating in such activities drops.

Insler (2014) uses data from HRS, and apply workers' self- reported probabilities of working past ages 62 and 65 as instruments. He finds that retirees experience positive effects on a health index, which consists of both objective and subjective measures of health. Moreover, he finds that retirees tend to reduce smoking and participate more in health-enhancing activities.

However, not all studies have shown retirement to have such a positive impact. Using data from the English Longitudinal Study of Aging (ELSA), Behncke (2012) reports that retirement actually increases the risk of being diagnosed with a cardiovascular disease[2] and cancer. Also contradictory to the findings of the aforementioned studies, she finds that retirement increases the probability of reporting poor health, and the risk of being diagnosed with a chronic condition.

Bound and Waidmann (2007) apply measures of self-assessed and objective health from the ELSA study, and find that retirement leads to a small, but significant positive effect on physical health for men. Physical health entails self-assessed health, physical functioning and biomarkers. Moreover, they show that these results are highly sensitive to job characteristics and differences in socioeconomic status. As these differences arguably play an important role in determining the effect of retirement on health, there has recently been a growing interest in tackling these heterogeneity issues. To the best of our knowledge, only a small number of studies have investigated the presence of heterogeneity across SES or gender in the effects of retirement on health.

Mazzonna and Peracchi (2017) stress the importance of heterogeneity in the health effect of retirement, and argue that the previous literature have failed to detect the potential

[2]Retirement is also found to have an impact on increased obesity (Godard, 2016; Rohwedder and Willis, 2010).

heterogeneity. Using the SHARE data, they find that for people working in more physically demanding jobs, retirement has an immediate beneficial effect on both a health index of self-reported measures and cognition. For the rest of the workforce, however, retirement has negative long-term effects on health and cognition.

In the paper closest to our study, Eibich (2015) applies a regression discontinuity framework, to study the effect of retirement on several subjective measures of health in Germany. The empirical evidence suggests the presence of effect-heterogeneity by socioeconomic status. Whereas he uncovered no effect of retirement on health for individuals with higher education, individuals who retire from strenuous jobs seem to experience a large and positive change in physical health.

From the Norwegian setting, Hernæs et al. (2013) employ a stepwise introduction of early retirement ages in Norway in the 1990s as instruments to assess whether retirement age matters for mortality. They find no relationship between lowering early retirement age and probability of dying, by following individuals until age 77.[3] Moreover, they question whether retirement has a causal impact on mortality.

Based on the relevant literature, it is unclear to what extent and in what direction retirement affects health. Previous findings are characterized by differences in methodology, be it an instrumental variable approach, regression discontinuity approach, or difference-in-difference approach. Another aspect of the literature is the different outcomes of health. While self-rated physical health often is positively associated with retirement, others document a decline in mental health and cognitive abilities.

2.2 Institutional setting in Norway

This section provides background information on the institutional setting in Norway in 2007/2008.[4] We start with a brief description of the pension system, as this is the main focus of our study. An individual can start claiming retirement pension the first month after reaching the statutory retirement age of 67, and is, in our analysis, considered retired once this claim is made. The main provider of retirement pension is the mandatory public National Insurance System (NIS). This is a pay-as-you-go defined benefit system, and all individuals with a minimum number of years of residence are covered. Once retired, the pension consists of a mix between fixed earnings-independent basic pension and pension contributions based on previous labor market income. Replacement rates from annual earnings

[3]Early retirement in Norway was introduced at age 66, but later reduced in a stepwise matter to age 62. The authors exploits this stepwise reduction as a source of exogenous variation.

[4]The pension system was reformed in 2011, but none of the new rules was in place throughout our study-period.

have been found to be around 72% on average (Røed and Haugen, 2003).

In theory, the statutory retirement age did not force individuals to retire. However, most companies had contracted retirement upon reaching the statutory retirement, and the norm was that people retired once they hit this age threshold. Moreover, for most of the workforce there was little economic incentive to prolong working life once eligible for old age pension. There was a full earnings test in place for individuals aged between 67 and 69 for earnings above 2 basic amounts,[5] resulting in a 40% reduction of the old age pension for each dollar earned.[6]

Besides the statutory retirement age, there are two other commonly-used exit routes from the labor market: disability insurance (DI) and the Early Retirement Program (ER). These are early exits routes that are temporarily available until the statutory retirement age. Eligibility for DI is based on health status and must be certified by a physician based on a permanent reduced ability to work. DI can also be graded in a way that allows individuals to combine work and DI. ER was available for all public and about half of private sector workers from age 62.[7] At 67, recipients of DI and ER are automatically transferred to retirement pension.

Table 1 summarizes the labor market status for individuals aged 56-79 in 2007. This table shows the fraction of individuals who are either working, on ER, DI, or claiming retirement pensions. The shares do not summarize to unity because it is possible for the same individual to be in two states, e.g. by combining partial uptake of DI and working.

Table 1 shows two important preconditions for our empirical analysis: labor market participation rate remains relatively high for older workers in Norway, and most individuals start claiming pensions as soon as they reach the age of 67. Provided the strong link between retirement pension uptake and exit from employment, we argue that claiming retirement pension in practice means withdrawing from the labor market. Strictly speaking, in this analysis, we are estimating the intention-to-treat (ITT) effects of offering retirement pension at age 67. Because uptake of pension in practice means withdrawal from the workforce for the majority of the population, we assume that the health effects to a large degree will stem from the relief from work related tasks. We refer to claiming retirement pension as retirement in the remainder of this article.

[5] One basic amount is the lowest earnings required to accrue pension points.
[6] This was lifted in 2008 for 67 year-olds.
[7] See Hernæs et al. (2013) or Kudrna (2017) for more details about the ER system.

3 Empirical strategy

3.1 Regression discontinuity design

We investigate the impact of retirement along several dimensions of health. Ideally, we seek to investigate the following linear relationship between health and retirement:

$$Health_i = \beta_0 + \beta_1 Retirement_i + X_i^{'}\beta_2 + \varepsilon_i, \tag{1}$$

where $Retirement_i$ is a dummy variable equal to one if the individual has retired and zero otherwise and X_i is a vector of relevant covariates. If retirement were to be considered a random event, Equation (1) would provide us with an unbiased estimate of the effect of retirement on health. However, people typically decide themselves when to retire. Moreover, unobservable factors such as knowledge about own longevity or other factors that correlate with both health and the retirement decision remain unaccounted for in Equation (1). This cause omitted variable bias in β_1. Importantly, own health is likely to affect retirement, causing bias in β_1 due to reverse causation. In order to circumvent these issues in the OLS specification, we apply regression discontinuity design (RD).

RD exploits institutional settings that determine access to a treatment. The idea is that treatment (retirement) is determined by a running variable (age), reaching a known threshold (the statutory retirement age). Units above the threshold receive the treatment and units below the threshold do not receive the treatment. This means that we use age as an allocation mechanism that determines retirement, rather than using actual retirement behaviour. The RD design relies on local identification by comparing individuals' right above and right below the retirement age cut-off. The discontinuity gap in health at this point identifies the treatment effect. Since the probability of retirement is discontinuous at the cutoff age of 67, we assume that reaching this age limit is what causes individuals to retire. Importantly, this assumption only holds for individuals close to the cutoff on the age distribution.

As described in Section 2, the general rule was that individuals started claiming retirement pensions at the statutory retirement age of 67. However, about 16 percent of men and 13 percent of women within the eligible age groups chose to retire early through ER, and a small fraction retired later. This is a setting of imperfect compliance. The Fuzzy RD (FRD) design is therefore more appropriate. Unlike in the Sharp RD, where all treated units are compliers, i.e. the likelihood of treatment goes from zero to one at the threshold, the fuzzy RD allows for a smaller discontinuity in the probability of retirement at the threshold.[8]

[8] The difference between sharp and fuzzy RD is parallel to the difference between a randomized experiment

3.2 Estimation

The FRD design resembles a setting with instrumental variables, with retirement coefficients consistently estimated by using two stage least squares (2SLS) (Imbens and Lemieux, 2008). The treatment effect is to be interpreted as a local average treatment effect (LATE), i.e. the estimated treatment effect of retirement on health, for individuals induced by the age threshold to retire (Hahn et al., 2001). In the setting of imperfect compliance with the treatment, the intention-to-treat (ITT) is as if randomized, which implies a causal interpretation of the estimated coefficients. The estimated effects are interpreted as the health effects of offering retirement pension at age 67.

Formally, we instrument for retirement using age equal to, or above the retirement threshold at 805 months, the month after which an individual turn 67 years of age. Specifically, we estimate the following two equations:

$$Retirement_i = \gamma_0 + \gamma_1 1[Age_i \geq c] + \gamma_2 Age_i^B + \gamma_3 Age_i^A + u_i, \tag{2}$$

where the endogenous regressor $Retirement_i$ is a binary variable equal to one if the individual is retired, i.e. is claiming retirement pension. $1[\bullet]$ is an indicator function taking the value one if the condition inside the brackets is true, and zero otherwise. c represents the retirement eligibility threshold at 805 months (age 67). Age is measured in months, and we include continuous age controls. These are allowed to have different slopes at either side of the threshold. Superscript B refers to ages below the retirement threshold at age 67, and superscript A refers to ages above the threshold.

The first stage in this 2SLS set-up is actual retirement predicted by age exceeding the threshold, controlled for the general effect of age on retirement. We apply retirement as predicted in the first stage, and the second stage is given by:

$$Health_i = \beta_0 + \tau \widehat{Retirement} + \beta_1 Age_i^B + \beta_2 Age_i^A + e_i, \tag{3}$$

here, $Health_i$ represents the different health measures for individual i. Our parameter of interest is τ, and its estimate is the jump in the outcome variable at the threshold, divided by the fraction induced to take up treatment at the threshold. This is the estimated treatment effect of retirement on health, for individuals induced by the age threshold to retire.

As the health effects in the RD design is only identified close to the retirement threshold, the estimations are done locally around the threshold. We choose the optimal bandwidth, i.e.

with perfect compliance and a randomized experiment with imperfect compliance, when only the intention to treat is randomized.

how many months on either sides of the age cutoff to include in the estimation,[9] in a cross-validation procedure suggested by Imbens and Kalyanaraman (2012). This is designed to minimize the mean squared error, and provides a trade-off between bias and variance. Based on this bandwidth selector, we choose a bandwidth of 10 months.[10] This means that only individuals in the age range 795 months to 815 months (10 months before and 10 months after the retirement age threshold) are included in the estimations.[11] In the sensitivity analysis, we assess different bandwidths to check the sensitivity of the results with respect to choice of bandwidth. In addition to assessing different bandwidths, we perform a range of robustness checks. Here we follow the guide to practice by Imbens and Lemieux (2008) for robustness checks using the RD design. These results are presented in the appendix, but we discuss them briefly in Section 5 (Results).

Finally, in the cross-sectional survey data, we follow Lee and Card (2008) and cluster at the age group level. As noted by Lee and Card (2008), for RD applications where the running variable is discrete, estimating a parametric function away from the discontinuity point can be seen as a form of random specification error. This implies a common component of variance for all the observations at any given value of the running variable. Thus, they suggest clustering at the age group level to account for this imperfect fit, as clustering leads to wider confidence intervals. In the panel data from the administrative records, we cluster at the individual level to account for the within-person correlation in the error term. The structure of these data will be discussed in more detail in the next section.

4 Data and sample Selection

4.1 Data

We use data from two separate sources in our analysis. The first is a survey carried out on a representative sample of Norwegian older adults, and the second is comprised of administrative health and population registers covering the entire population. Unfortunately, individuals from the two sources cannot be connected, as the first data source has been

[9] Dong (2015) show that using regression discontinuity design calls for careful consideration of the unit of measurement when age is the forcing variable, as age in years, as opposed to age in months, might lead to inconsistent results.

[10] The optimal bandwidth suggested by Imbens and Kalyanaraman (2012) varies by SES-group. The suggested bandwidth is in the range 8-12 months for all the groups. For simplicity, we apply a bandwidth of 10 months in all estimations. Choosing different bandwidths within this interval has little influence on the estimated effects. See the robustness checks in the appendix for more on sensitivity of bandwidths.

[11] Due to the small sample size left in the survey data when we apply the 10 months bandwidths, we also ran the entire analysis using a bandwidth of 20 months. This does not change the results from the survey data in any substantial way.

anonymized.

The NorLAG survey data

The first datasource is a survey carried out on a representative sample of Norwegian older adults, the Norwegian Study on Life-Course, Aging and Generation (NorLAG) panel study. [12] The data was collected in 2002 and 2007. NorLAG contains individual data on a range of health outcomes, as well as information about socioeconomic status. Data collection was carried out by Statistics Norway with computer-assisted telephone interviews (CATI).

All respondents to the survey are merged with administrative registers for the period 2002-2012. The registers contain information on year and month of birth and of retirement. Furthermore, the registers contain various sociodemographic background information such as labor income, social insurance take-up, and educational attainment. We are thus able to construct detailed information for each individual regarding attachment to the labor market, retirement status and social security take-up, enabling identification of the exact timing of retirement, and whether the individual retired directly from the labor force or transitioned from disability insurance or other welfare programs.

Currently, the panel consists of two waves. For the main analyses, we use the second wave as this contains a larger sample than the first wave.[13] However, for some specifications in the sensitivity analysis, we rely on data from the first wave to obtain information about past labor market performance. This is outlined in more detail in Section 4.2.

Our health outcome from the NorLAG data is a composite measure of physical health, namely the physical component of the Short Form 12 (SF12) scale (Ware Jr et al., 1996). Self-rated health (SRH) is one of the components that go into the SF12. Other factors are measures of the degree to which an individual is able to perform tasks like vacuuming, moving a table or climbing stairs, whether there are certain tasks that could not be performed due to health limitations, or whether pain limits daily activities. The score is standardized on a scale from 0-100 with a mean of 50 and standard deviation of 10 using the US population as a reference. SF12 has been found to be a strong predictor of hospitalisation, job loss due to health, future use of medical health services, and depression (see e.g. Jenkinson and Layte (1997); Ware Jr et al. (1996); Brazier and Roberts (2004)).

Occupational status in the NorLAG data is coded in accordance with the ISCO-88 scale. This has been re-coded into two occupational groups: manual and professional workers, following the classical division into blue and white collar workers of higher and lower skills.[14]

[12] See Slagsvold et al. (2012) for a thorough description.

[13] The first wave contains 5,559 observations (response rate 67%), whereas the second wave contains 15,149 observations (response rate 60%).

[14] Coded according to NACE Rev.1.1.

Professional workers are defined as high skilled white collar workers, the term "manual workers" refers to three categories: high and low skilled blue collar workers and low skilled white collar workers. We apply this categorization of manual workers, because the latter three groups are more similar based on observable characteristics.

Administrative data

Our second data source is comprised of administrative data that covers the entire Norwegian population. All residents are assigned to a unique personal identification number, which enables them to link information from various administrative registers, such as health registers, income and social insurance registers, and population registers. These registers contain information on year and month of birth, death and retirement, as well as educational attainment, income, and social security uptake.

We apply two health outcomes from the administrative data. The first is a binary indicator of whether a person has been acutely hospitalized in a particular month. This information comes from the national patient register (NPR), which contains records of all inpatient and outpatient stays at Norwegian hospitals from 2008–2014. Admissions are coded by whether the hospitalisation is a result of a planned or unplanned admission. The latter can be thought of as acute in the sense that treatment has been deemed necessary, typically as a result of an accident, stroke, or severe heart condition.[15] The second health outcome is a binary indicator of whether a person passed away in a particular month. This information comes from the Norwegian cause-of-death registry, and contains all recorded deaths in Norway from 1992–2014. Both outcomes thus yield the likelihood of the particular outcome at a specific age-in-month.

Importantly, these measures of health are not correlated with the time cost to consult medical expertise. As individuals have more time at their disposal after retirement, the opportunity cost of seeking medical help is reduced once retired compared to when working. It is therefore likely that the prevalence of a diagnosis or a medical treatment that is not acute increases after retirement, when the opportunity cost of seeing a physician has fallen. Applying a health outcome that is correlated with the opportunity cost of medical consultations can therefore erroneously lead to the conclusion that retirement caused the increased prevalence of the health outcome.

[15] All admissions are coded in accordance with the International Statistical Classification of Diseases and Related Health Problems, ICD-10, (see WHO (1992)).

4.2 Sample selection

We restrict our attention to individuals aged 56-79 in 2007 and 2008 in both data-sources. From the administrative records, we use data from 2008.[16] This leaves 4,619 individuals in the NorLAG sample and 892,908 individuals in the register sample. The administrative data in our analysis is a panel data set, with monthly records of hospitalisation, mortality, retirement, and age in months. As such, month by month, the treatment variable is determined according to age in months exceeding the retirement age threshold. Including fixed effects is unnecessary for identification in an RD design. Moreover, as the source of identification is a comparison between those just below and just above the threshold, which can be carried out with a single cross-section, imposing a specific dynamic structure introduces more restrictions without any gain in identification (see Lee and Lemieux (2010)). We therefore treat the sample from the register panel data as repeated cross-sections and pool all months together, treating each observation as an individual. This also makes the administrative data more comparable to the NorLAG data.

In order to maintain the intention to treat in the RD design and to ensure that we have enough data for inference, we place no further restrictions on the sample for the main analysis. This means that our analytical sample will include individuals on DI or individuals who are not working for other reasons. Individuals on DI are automatically classified as retired once they hit the age threshold. In theory, we should expect no retirement effects for this group, as their work status remains unchanged when they retire. This would bias our results towards zero. However, the health outcomes in the survey data can suffer from justification bias. Being on disability insurance might make an individual, consciously or subconsciously, under-report their health in order to justify their status as disabled. The need for this justification is no longer present once they are transferred to retirement pension. In this case, the estimates would be biased upwards and we might worry that the positive effect on health was driven by these individuals. As a sensitivity analysis, we therefore run the whole analysis including only individuals who were gainfully employed or working until retirement.

Ideally, we want to compare individuals working up to retirement age to individuals who retired from working. In the NorLAG data, this is done by adjusting the sample by two rules. The first rule implies including only individuals who had income from labor the previous year in the analysis; the second rule implies including only individuals who have stated that they are working or were working before they became retired. Some caveats are worth mentioning: the first rule results in a substantial reduction in the sample size, as we

[16] This is the earliest year in which data on hospitalisations are available.

need to use the balanced panel from both waves of the NorLAG study to identify labor income in 2006. A potential concern with the second rule is that the formulation of the question to the working and retired part of the population differs slightly in the NorLAG data. To maintain continuity across the retirement threshold, it is crucial that we apply exactly the same selection rule on either side of the threshold when identifying the sub-samples for the sensitivity analyses. In the administrative data, we define individuals as working if they currently have positive income or if they had positive income before retirement. We find that these sensitivity analyses does not alter our conclusions.[17]

4.3 Descriptive statistics

Table 2 displays summary statistics for the sample from both the NorLAG data and the administrative data. These are men and women aged 56-79 in 2007 and 2008 respectively.

The first two columns are summary statistics for the whole sample, whereas the next two columns show the summary statistics for those within the bandwidth of 10 months below and 10 months above the retirement threshold of 805 months (age 67). These are the observations within the bandwidth used for estimating the short-term retirement effects in the regression analysis. It is important that the two groups are balanced with respect to the covariates. T-tests (not shown) confirm that individuals on either side of the threshold are similar with respect to education, living arrangements and occupation.

5 Results

5.1 Graphical results

To motivate the use of the FRD design, Figure 1 displays the share of retired individuals from age 55 until age 79. The two upper graphs are constructed using the survey data, whereas the two lower graphs are constructed using the administrative data. The age span in the four graphs are the same (55-79), but the x-axis on the two left graphs depicts age in years, whereas the x-axis on the two right graphs depicts age in months. The latter is to show that the discontinuity in retirement coincides with the first month after turning 67 (the first month of retirement eligibility).

In all of the four figures, the patterns are very similar.[18] There is a substantial disconti-

[17] The results from the sensitivity analysis are shown in the Appendix.

[18] In the graphs, retirement refers only to those who have actually retired, either through the early pension program or at the retirement age of 67. This means that individuals on DI are not considered retired. If we remove all individuals that are currently on DI or who were on DI before they retired from our sample, the picture looks the same.

nuity in the likelihood of being retired at age 67 (805 months). Since some workers chose to retire early, we also see a small discontinuity at age 62, the lowest eligible age for early retirement. Only a negligible share of individuals chose to retire later than age 67. The graphical evidence thus show a clear response in terms of retirement at the statutory retirement age. We build our empirical analysis on the discontinuity at age 67.

Figure 2 presents graphical evidence on the relationships between health and age for the three outcomes used in our study: physical health, acute hospital admissions, and mortality. The age range spans from 55 to 79 years, and the x-axes are depicted as age-in-months relative to the retirement age threshold at 805 months, normalized to zero. The lines are fitted on either side of the threshold using a second order polynomial global fit.

The upper graph (a) in Figure 2 shows the observed health pattern for physical health for all individuals aged 56-79 in the NorLAG sample. Physical health declines with increasing age, but there is a substantial jump at the retirement threshold. At this threshold, the trajectory shifts up to a level of someone 80 months younger, which amounts to 6.5 years.

For acute hospitalisations and mortality, the two lower graphs, (b) and (c) respectively, we see that the incidence rate increases across the age-span 56-79, but there does not seem to be any substantial discontinuities in the outcomes reflected in the graphs. For acute hospitalisations, we see a small, possible negligible, downward shift at the threshold.

There is an ongoing debate as to whether it is the cumulative or contemporaneous effects of retirement that are the largest (see Coe and Zamarro (2011); Mazzonna and Peracchi (2017)). As mentioned above, the effects estimated using RD are only identified close to the threshold, so any prolonged retirement effects becomes mere speculation in this setting. However, by visual inspection of the graph for physical health, (a) in Figure 2, there is suggestive evidence of a prolonged effect of retirement on physical health, as retirement shifts individuals to a higher health trajectory, where they seem to stay as age increases.

5.2 Regression results

We present the 2SLS regression results for all three health dimensions in Table 3 - Table 6. The effects are estimated using a bandwidth of $+/-$ 10 months around the threshold, which is the optimal bandwidth using the selector suggested by Imbens and Kalyanaraman (2012)). We estimate the effects for each gender and for the different SES-groups separately. In Table 8, we present results from a formal test of heterogeneous retirement effects in which the instrument is interacted with indicators of the different SES-groups.

In Table 3, we present the first stage of the 2SLS regression results. This is the estimated effect of crossing the statutory retirement age on the probability of retirement, i.e. τ from

Equation (2). The results in Table 3 show that crossing the statutory retirement age significantly increases the probability of retirement, thus indicating a strong first stage. These results are in line with the graphical results presented in Figure 1.

5.2.1 The effect on physical health

Table 4 displays the results of the short-term retirement effects on physical health. We find that retirement leads to a 5.7 points increase in physical health for the population as a whole. This is a substantial effect given that the mean and standard deviation for this health outcome is 47 and 10 points, respectively. We find a strong and positive effect for men (8 points), and a positive (4 points), but not statistically significant, effect for women. Our findings are in line with evidence from Coe and Zamarro (2011) and Eibich (2015), who suggest that, in general, retirement leads to an increase in physical health in both the USA and Germany. Although our estimates are short-term effects, previous findings suggest that retirement also has a cumulative effect on physical health through increased physical activity (e.g., Eibich (2015)).

Based on the discussion in the introduction, we can expect different health effects of retirement depending on education and occupation. The four latter columns in Table 4 show the effects for the different SES-groups. For the manual workers and low educated groups, the effects are large (13.2 and 8.4 points respectively) at about one standard deviation, and significant at the 1 percent level. For the high SES groups, we find no statistically significant effects, and the coefficients are closer to zero.

These results are in line with the findings of Eibich (2015). He shows that highly educated individuals benefit less from retirement in terms of self-reported health, compared to individuals with low SES. Moreover, Insler (2014) suggests that wealthy people have more time to invest in their health while working.

Power calculations show that a sample of at least 90 is needed to ensure a power of 0.8. Allthough well above this threshold, the sub-group samples are fairly small. It could be argued that this should lead to the application of wider bandwidths. However, wider bandwidths also imply more bias (Lee and Lemieux, 2010). We did, however, run the whole analysis using a bandwidth of 20 months. This about doubles the observations in each sub-group, but the effects sizes and significance levels remains fairly the same.

To sum up, the results are clear in that retirement leads to better physical health for men, and for the low SES groups. For women, the results are similar in effects size, yet statistically not significant. We find no health effects of retirement for the high SES group. Based on this analysis, there does not seem to be substantial differences by gender, but both the gender difference and the differences by SES will be formally assessed in Section 5.2.5.

5.2.2 The effect on acute hospitalisation

We now turn to our estimates from the administrative data. Acute hospitalisation is based on a dummy for inpatient care in which treatment is deemed necessary. The results are presented in Table 5.

First, we explore how retirement affects acute unscheduled hospitalisations for the population on average and by gender. For all sub-groups the effect size is about -0.4 percentage points, but not significant. When we divide by SES, we find that retirement leads to a 0.6 percentage point reduction in the likelihood of acute hospitalisation for the low SES group. As the incidence of acute hospitalisations is 14 percent, this amounts to a 4 percent reduction in the likelihood of acute hospitalisations. The effect is significant at the 5 percent level. For the high SES group, we find an effect of 0.3, yet this is not significantly different from zero.

One way to think of these results is that retirement for the population in general leads to no short-term change in serious health-conditions. Hallberg et al. (2015) studied a targeted early retirement offer to workers in the military at age 55 and find that the number of days in inpatient care is significantly reduced at ages 61-70. One possible drawback with our method is that the regression discontinuity design only captures the short-term effect of retirement, and any potential gain of retirement is possibly not found in the subsequent months after retirement. For instance, Hallberg et al. (2015) find a 4.7 days reduction in inpatient care 6-10 years after early retirement, whereas the estimated effect is 2 days in the first years after early retirement.

To some extent, the same intuition can be found in Behncke (2012). She shows that retirement increases the risk of being diagnosed with a chronic condition in the subsequent years after retirement. However, assessments applying less acute diagnoses can be confounded for two reasons. First, the opportunity cost of seeking medical help is greatly reduced after retirement, hence increasing the likelihood of detecting such conditions. Second, the reason for seeking medical help can differ for individuals who are working and individuals who are retired. In Norway, for example, sickness absence from work for longer than the self certified absence period[19] must be certified by a physician, which means that retirees and employers most likely visit the doctor for different reasons.

[19]A medical certificate is required for spells of absence of more than three days or eight days, depending on whether the employer has signed the "IA-agreement" or not.

5.2.3 The effect on mortality

The results described in the previous sections suggests that retirement leads to a short-term positive effect on subjective measures of health, whereas we find no or small effects on the number of acute hospitalisations. Given the latter findings, *a-priori*, we expect to see little or no short-term effect on mortality. In Table 6, we display the estimation results on mortality.

We find no short-term effect of retirement on mortality. Regardless of gender and subgroup, the estimates remain statistical indistinguishable from zero.

The question remains whether a short-term effect of retirement on relatively serious outcomes, such as mortality, is implausible in the short run. Hallberg et al. (2015) use cox-regression models to form hazard ratios and find that early retirement at age 55 reduces the risk of dying at age 70 by around 26 percent. Studying the first five years after an early retirement window in Holland, Bloemen et al. (2017) find a drop in the probability of dying of around 2.6 percent. The same effect is found in Blake and Garrouste (2013) and Kuhn et al. (2010), albeit the latter only for male blue-collar workers. However, studying the introduction of early retirement in Norway, Hernæs et al. (2013) find no effect of early retirement on mortality. They follow workers for a maximum up to 77 years of age, with eligibility for early retirement varying between 62 and 65 years of age. They conclude that early retirement in itself has no effect on mortality.

Taken together, our results show that in general there are no effects of retirement on serious health outcomes. However, as this study and several other studies show, retirement affects subjective health. What is it about these outcomes that actually makes people feel better? In the next section, we look further into the subjective physical health outcome (SF-12) to assess which aspects of health that are improved by retirement.

5.2.4 Looking further into the effect on physical health

SF-12 is composed by survey responses to the following [20]: rate your health on a scale from 1-5 (self-rated health); is your health of such a character that it limits you in doing tasks like moving a table, vacuuming, hiking or gardening; is your health of such a character that it limits you from climbing several flights of stairs; has your physical health limited you in doing your daily tasks so that you have accomplished less than you wished for; has your physical health limited you from completing specific tasks; has psychological problems limited you from doing daily tasks so that you have accomplished less than you which for; has psychological problems limited you from doing these tasks as profoundly as usually; has pain limited you from doing your daily tasks; have you been feeling calm and harmonious,

[20] Translated from Norwegian by the authors.

energized or sad during the last four weeks; and, finally, has physical or mental health limited you from socializing as much as you wanted.

Out of the 12 components that go into the SF-12, five were significantly impacted by retirement. These are the following: is your health of such a character that it limits you in doing tasks like moving a table, vacuuming, hiking or gardening (Functional); has your physical health limited you in doing your daily tasks so that you have accomplished less than you wished for (Daily); has your physical health limited you from completing specific tasks (Specific); has psychological problems limited you from doing these tasks as profoundly as usually (Mental); has pain limited you from doing your daily tasks (Pain). Each question is coded as a binary variable, where one means that health or pain is experienced as limiting. In Table 7, we present the results for these four components.

Retirement was found to reduce the experience that physical health is a limiting factor in accomplishing as much as one would like, and as a limiting factor in doing specific tasks. The former holds for both men and women, whereas the latter holds for men. We find particularly strong effects on reduced pain, especially for women. Furthermore, we find that, in general, retirement reduced the limitations in doing tasks profoundly experienced due to mental health.

When we assess the different SES-groups we find that it is manual workers or lower-educated individuals who experience reduced pain and limitations from physical and mental health. We find no effects for the high SES groups. Moreover, when we divide the groups by SES, we also find that, for the low SES group, retirement reduced the limitation caused by health in doing functional tasks such as vacuuming, moving a table, hiking, or gardening. These effects are statistically significant at the 5 percent level for manual workers and at the 10 percent level for the low-educated group.

5.2.5 A formal test of effect heterogeneity

Table 8 presents the results from the formal test of heterogeneity. These are the results of a reduced form of Equation (3), where the instrument is interacted with SES groups and gender. We estimate the following:

$$Health_i = \beta_0 + \gamma 1[Age_i \geq c] \times SES_i + \beta_1 1[Age_i \geq c] + \beta_2 Age_i^B + \beta_3 Age_i^A + e_i, \qquad (4)$$

where γ is the coefficient of interest and $1[Age_i \geq c]$ is the instrument indicating whether age in months is equal to or exceeds the threshold. SES is a binary indicator of either manual workers, low education or women. We apply the same +/- 10 months bandwidth in these estimations.

We see that the effects of retirement are statistically different from each other when SES is measured by occupation. Although the estimated effects differ quite substantially by educational group as shown in Table 4, the differences are not statistically significant when SES is proxied by education. Moreover, there are no statistically significant differences in the retirement effect by gender. Hence, we show that accounting for differences by socioeconomic status can be important in analyses of retirement effects on health.

5.3 Robustness checks and sensitivity Analysis

The results from the robustness checks are presented in the Appendix, but we provide a brief overview here. First, we show that our results on physical health and mortality are robust to different bandwidths, whereas increasing the bandwidth from 10 to 15 months yields significant, negative effects on the likelihood of having an acute hospitalisation. The effects are still small, ranging from 0.7 to 1 percentage points, yielding a 5-7 percent reduction in the likelihood of an acute hospitalisation. Increasing the bandwidth increases the likelihood of factors, other than retirement, affecting acute hospital admissions. Another explanation can be that it takes some time for retirement to take effect on health issues such as stroke and acute heart conditions, thus including more post-retirement months increases the likelihood of finding significant effects.[21]

We then look for discontinuities at the retirement age threshold in a covariate that is not affected by the treatment, in this case marital status. Although, retirement can affect the likelihood of being married, it is highly unlikely in the immediate aftermath (within 10 months) of retirement. We find no retirement effects on the likelihood of living with a partner or spouse (the NorLAG data) or on being married (the administrative data).

Next, we perform placebo tests by checking for discontinuities in the health outcomes at values of the forcing variable and age where there should be no discontinuities. We find no discontinuities in the health outcomes at the placebo thresholds of age 61 and 73 for physical health or mortality, but we find some inconsistencies at these thresholds for acute hospitalisations. The effects are smaller than at the retirement threshold, yet significant, thus we might worry that this outcome is prone to be discontinuous at arbitrary age-thresholds.

We then test for discontinuities in the conditional density of the forcing variable to avoid self-selection or sorting into treatment or control groups. The RD design may be invalid if individuals just above the threshold are more likely to answer a survey than those just

[21]When we run the entire analysis using a bandwidth of 20, we find larger and (negative) significant effects for the population as a whole, for men, and for the low educated group for this outcome. Effects sizes range from 1 to 1.5 percentage points, significant at the 5 percent level. Using this bandwidth, we still find no significant effects of retirement on mortality.

below the threshold, i.e. violating the RD assumption that the running variable is continuous at the threshold. In the Appendix, we provide histograms that display the age-in-months-distribution in the NorLAG data. There is no apparent discontinuity at the threshold in these histograms. Moreover, we applied the local polynomial density estimator for testing the null of continuous density of the forcing variable at the threshold proposed by Cattaneo et al. (2016). The p-value under this test is 0.3251.

Finally, the results for physical health and mortality are robust to the different sub-samples that are conditioned upon working or working until retirement, as described in Section 4. For acute hospitalisations, we find the same results as in the main analysis for all sub-groups, except for the lower SES group, where the negative impact of retirement on the likelihood of acute hospitalisation is no longer found when we condition on working or working until the retirement age.

6 Conclusion

Whether retirement has a causal effect on health is a difficult question to answer because of selection into retirement. In this paper, we study the short-term health effect of retirement using the statutory retirement age at 67 in a fuzzy regression discontinuity design. We exploit the fact that once individuals reach the statutory retirement age, the probability of claiming retirement pension drastically increase. We apply both subjective measures of health from survey data and objective health outcomes from administrative data, where the latter covers the entire Norwegian population.

We find that, on average, in the population, retirement has a positive effect on self-assessed physical health, but no effects on the objective measures of health: acute hospitalisations and mortality. When we assess the effects by different SES groups, we find that retirement has a large, positive impact on physical health and leads to reduced likelihood of acute hospitalisation among the low SES groups. We find no significant effects for the high SES groups for any of these outcomes. For mortality, we find no significant effects for any group.

We thus confirm what has been found in several studies, namely that retirement has a positive effect on health for subjective health outcomes. How this manifests to objective outcomes is less clear as there exist little evidence using objective health outcomes, especially on the full population. In general, we find no effects on the objective outcomes, besides suggestive evidence of a retirement effect on reduced likelihood of hospitalisations for the low SES group. However, this result does not pass the robustness tests, and must therefore be interpreted with care.

Both acute hospitalisations and mortality are extreme outcomes. We can thus conclude that retirement mainly impacts subjective outcomes, not objective ones. When we assess the factors that go into the physical health outcome, SF-12, we find that the positive health effect was driven by a few different factors. On the one hand, finding that retirement leads to reduced likelihood of reporting that health is limiting in managing in daily chores and in conducting specific chores profoundly, can be due to the fact that work (a possible health consuming chore), is no longer present, so health feels less limiting. This implies that the underlying health has not changed, but the presence of health consuming activities has. On the other hand, we also found that retirement reduced the presence of pain and reduced the likelihood of reporting difficulties with activities such as vacuuming, moving a table, hiking, or gardening. This indicates that retirement affects health in a more fundamental way. Future research should thus assess objective health outcomes that are less extreme. In doing so, it is key to recognize that retirement necessarily coincides with reduced opportunity cost of time.

This study accentuates the importance of assessing the potential heterogeniety in the effects for individuals in different circumstances. Occupation, more than education, determines social differences in the effects of retirement on health. Our findings indicate that the retirement reforms aimed at prolonging working life can be socially distortive due to the differential effects based on SES. We find that retirement at age 67 has positive health implications for low SES groups, but we find no effects for high SES groups. A formal test of these differences confirms that occupation matters for the health effects of retirement.

Finally, our study contributes to generalizing the positive physical health effect of retirement found in the literature across a larger age span. The current literature has mainly assessed retirement ages from late the 50s to about 65. Here, we confirm that the positive effects still hold for individuals retiring at age 67. Assessments of higher age thresholds are valuable for policymakers as current retirement reforms typically aim at increasing the retirement age. These reforms will likely affect relatively healthy individuals, i.e. workers who remain employed until these higher retirement ages.

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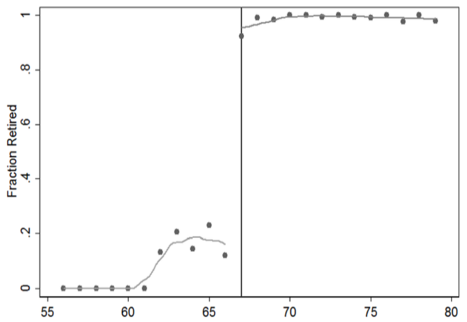
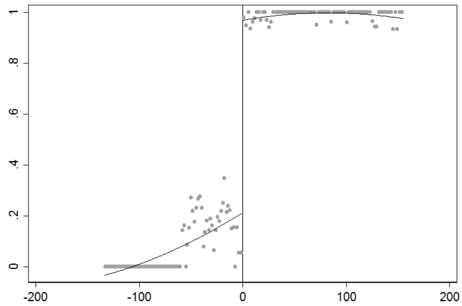
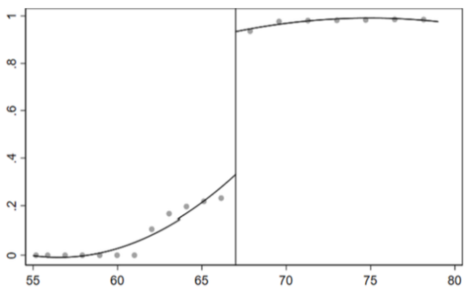
Graphs and Tables



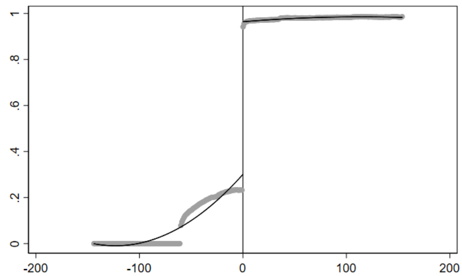
(a) NorLAG Data: Fraction Retired - Age in Years



(b) NorLAG Data: Fraction Retired - Age in Months



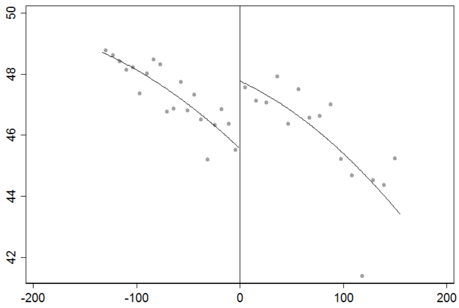
(c) Adm. Data: Fraction Retired - Age in Years



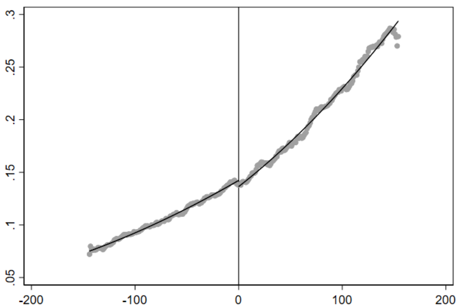
(d) Adm. Data: Fraction Retired - Age in Months

Figure 1: Discontinuity in Retirement at the Retirement Age Threshold

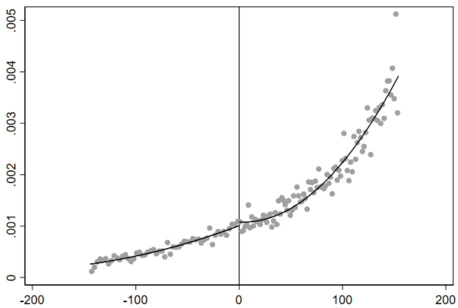
Notes: The graphs show the fraction retired by age from the two datasets. The upper graphs are based on the survey data, whereas the two lower graphs are based on administrative data. All graphs depicts the fraction retired across the age span 55-79. The x-axis on the left two graphs depicts age in years, whereas the x-axis in the graphs to the right depicts age in months, relative to the retirement eligibility age-in-months (805 months).



(a) NorLAG Data: Physical Health (SF-12)



(b) Administrative Data: Acute hospitalisations



(c) Administrative Data: Mortality

Figure 2: Discontinuity in Health at the Retirement Age Threshold

Notes: The graphs present the age-health relationship for physical health, acute hospital admissions and mortality. The scale for physical health are points on the SF-12 scale and the scale of acute hospital admissions and mortality corresponds to the incidence in the population. The x-axis displays age-in-months relative to the retirement age threshold at 805 months.

Table 1: Labor Market Participation for Individuals Aged 56-79 in 2007

Age Group	Working		Retired		ER		DI	
	Men	Women	Men	Women	Men	Women	Men	Women
56 – 61	79%	72%	-	-	-	-	19%	28%
62 – 66	59%	49%	-	-	16%	13%	31%	41%
67 – 69	17%	9%	89%	92%	-	-	-	-
70 – 79	18%	2%	98%	98%	-	-	-	-

Notes: The numbers are based on own calculations using the administrative data which covers the entire population of Norway (See Section 4 for a description). Work is defined as having earnings larger than zero. The states will not sum to unity because individuals can be in two states at the same time, e.g. by combining work and partial uptake of DI.

Table 2: Descriptive Statistics

	Whole Sample	Below Threshold	Above Threshold
Characteristics	(1)	(2)	(3)
Source: NorLAG			
Age	65.34	66.15	67.00
	[6.58]	[0.36]	[0.00]
Retired	0.44	0.18	0.96
Less than high school degree	0.23	0.25	0.25
High school degree	0.51	0.45	0.51
Any college	0.27	0.30	0.25
Professional	0.48	0.47	0.50
Manual	0.43	0.40	0.41
Female	0.48	0.47	0.50
Living with partner	0.71	0.75	0.72
SF12	46.93	45.73	47.55
	[10.78]	[12.03]	[10.12]
Observations	4619	190	200
Source: Admin. Data			
Age	64.92	66.19	67.00
	[6.67]	[0.38]	[0.00]
Retired	0.40	0.29	0.95
Less than high school degree	0.31	0.32	0.34
High school degree	0.45	0.46	0.45
Any college	0.24	0.23	0.21
Married	0.63	0.64	0.64
Female	0.51	0.51	0.51
Acute Hospital Admissions	0.142	0.140	0.141
Mortality	0.019	0.017	0.018
Observations	1,071,068	31,751	33,752

Notes: This table displays descriptive statistics for the two data sources, the NorLAG data (above) and the administrative data (below). Column (1) presents means for the entire sample, whereas the other two columns display means for the sub-sample of individuals included in the estimations (we use a bandwidth of ten months for the estimations). Column (2) displays the means for the sub-samples aged 795-804 months (control group) and Column (3) for those aged 805-814 months (treatment group). Standard deviations in square brackets.

Table 3: First-Stage Regressions

	All	Men	Women
Source: NorLAG			
Retired	0.954***	0.941***	0.961***
	(0.0362)	(0.0587)	(0.0431)
Observations	371	190	181
Source: Admin. Data			
Retired	0.720***	0.683***	0.756***
	(0.00264)	(0.00389)	(0.0356)
Observations	825,605	407,386	418,219

Notes: This table show the first-stage regressions specified in Equation (2). The reported coefficient is γ from Equation (2). Estimation is done using a bandwidth of ten months. Standard errors in parentheses are clustered at the age-in-months level for the NorLAG data and at the individual level for the administrative data. *=p<0.10, **=p<0.05, ***=p<0.01.

Table 4: Short-Term Retirement Effects on Physical Health

	All	Men	Women	Manual	Professional	Lower	Higher
Retired	5.689***	8.036***	4.053	13.16***	-0.333	8.358***	-1.952
	(1.979)	(3.026)	(3.465)	(3.508)	(3.761)	(2.415)	(5.449)
Observations	361	185	176	126	123	261	99

Notes: This table displays the impact of retirement on physical health. *All* refers to the sample as a whole, *Professional* and *Manual* to type of occupation and *Lower* and *Higher* to education levels. The reported coefficient is τ from Equation (3). Estimation is done using a bandiwdth of ten months. Standard errors in parentheses are clustered at the age-in-month level. *=p<0.10, **=p<0.05, ***=p<0.01.

Table 5: Short-Term Retirement Effects on Acute hospitalisations

	All	Men	Women
Retired	-0.00419	-0.00417	-.00440
	(0.00258)	(0.00395)	(0.00339)
Observations	825,605	407,386	418,219
		Lower	Higher
Retired	-	-0.00589**	-0.00255
		(0.00292)	(0.00535)
Observations		643,441	182,164

Notes: This table displays the impact of retirement on acute hospitalisations for the whole population and divided by gender and SES (Education). The reported coefficient is τ from Equation (3). *All* refers to the whole sample and *Lower* and *Higher* to education levels. Estimation is done using a bandwidth of ten months. Standard errors in parentheses are clustered at the individual level. *=p<0.10, **=p<0.05, ***=p<0.01.

Table 6: Short-Term Retirement Effects on Mortality

	All	Men	Women
Retired	-0.000123	0.0000355	-0.000266
	(0.000204)	(0.000343)	(0.000236)
Observations	840,239	416,611	423,628
		Lower	Higher
Retired	-	-0.0000895	-0.000299
		(0.000233)	(0.000399)
Observations		655,743	184,496

Notes: This table displays the impact of retirement on mortality for the full population and divided by gender and SES (Education). The reported coefficient is τ from Equation (3). *All* refers to the whole sample and *Lower* and *Higher* to education levels. Estimation is done using a bandwidth of ten months. Standard errors in parentheses are clustered at the individual level. *=p<0.10, **=p<0.05, ***=p<0.01.

Table 7: Short Term Retirement Effects on Health by SF12 Components

	Functional	Daily	Specific	Mental	Pain
All	-0.0741	-0.171**	-0.229**	0.0754**	-0.232***
	(0.0791)	(0.0776)	(0.0934)	(0.0377)	(0.0783)
Observations	371	368	369	368	371
Men	-0.0344	-0.325***	-0.401**	0.0953	-0.180*
	(0.140)	(0.123)	(0.168)	(0.0803)	(0.100)
Observations	190	189	189	188	190
Women	-0.126	-0.0378	-0.0693	0.0562	-0.328***
	(0.105)	(0.128)	(0.152)	(0.0419)	(0.117)
Observations	181	179	180	180	181
Manual	-0.258**	-0.558***	-0.551***	0.0703	-0.503***
	(0.125)	(0.128)	(0.132)	(0.123)	(0.135)
Observations	127	126	127	127	127
Professional	-0.0110	0.0937	0.0160	-0.0455	0.0342
	(0.166)	(0.123)	(0.157)	(0.0486)	(0.178)
Observations	123	123	123	123	123
Low Education	-0.158*	-0.284***	-0.292***	0.121***	-0.314***
	(0.0864)	(0.110)	(0.0971)	(0.0441)	(0.103)
Observations	270	267	268	267	270
High education	0.124	0.104	-0.0597	-0.0215	0.0183
	(0.200)	(0.156)	(0.194)	(0.0723)	(0.159)
Observations	100	100	100	100	100

Notes: This table presents the impact of retirement on selected components of the physical health outcome (SF-12). The reported coefficient is τ from Equation (3). Estimation is done using a bandwidth of ten months. Standard errors in parentheses are clustered at the age-in-month level. *=p<0.10, **=p<0.05, ***=p<0.01.

Table 8: Formal Test of Differences by Socioeconomic Status

	Physical Health	Acute hospitalisation	Mortality
		Low education	
Retired	4.975	-0.00294	0.0000666
	(3.115)	(0.00533)	(0.0000145)
Observations	361	825,605	840,239
		Gender	
Retired	3.696	0.00224	0.0000171
	(2.549)	(0.00509)	(0.000139)
Observations	361	825,605	840,239
		Manual Workers	
Retired	6.858*	-	-
	(3.305)	-	-
Observations	249	-	-

Notes: This table displays the interaction between retirement eligibility and SES (education and occupation (only for the NorLAG)) and gender. The first column presents the results for physical health from the NorLAG data and the second and third columns presents the results for acute hospitalisations and mortality, respectively, for the Administrative data. The reported coefficient is γ from Equation (4). Estimation is done using a bandwidth of ten months. Standard errors in parentheses are clustered at the age-in-month level for the NorLAG data and at the individual level for the Administrative data. *=p<0.10, **=p<0.05, ***=p<0.01.

Appendix - Sensitivity and Robustness

A.1 Disabled individuals: past earnings and self-reported work status

People on disability insurance are mechanically transferred from disability pension to retirement pension at age 805 months. We need to make sure that the positive physical health effects we found are not driven by these individuals. Initially, there is no reason to believe that there should be an effect for these individuals as they were not working before retirement, and should therefore have no change in circumstances. However, as the health measure contains elements of self-assessed health, one could imagine that someone who is disabled may need to justify their status as disabled, consciously or subconsciously. In this case, poor health prior to the statutory retirement age may be under-reported. Post retirement, when they are no longer in a situation where poor health is defining their labor market status, they might feel healthier, or no longer have the need to report poor health. If this scenario is plausible, we need to rule out that the results found in Section 5 are driven by this group.

The first two rows of Table A.1 displays the results on two sub-samples of the survey data (labeled "Working" and "Income"), each aimed at running the analysis only on the sub-sample that was recorded as working until the statutory retirement age. The working sub-samples are defined in Section 4.2. Finding coefficients of the same sign and magnitude, especially for the rule based on self-assessed work status, ensures us that these effects are not driven by the disability justification hypothesis. The estimations based on the income-rule yields large and insignificant coefficients, both a consequence of the small sample sizes. Yet, the direction of the effects are similar to what was found in the main analysis.

For the outcomes from the administrative data, as these are not subject to the potential justification bias, we should expect that individuals who retire formally at 67, but without any actual change in circumstances, should water down the effects. We can therefore expect that this assessment can uncover significant effect, not detected in the gross sample. The first row of Table A.2 and Table A.3 presents the estimations restricted to "workers" for acute hospitalisations and mortality, respectively. Here, we find no significant results for any of the sub-groups, besides a significant effect on mortality for women (0.2 percentage points significant at the 5 percent level). The significant result on hospitalisations found for men with low education in the main analysis, is no longer present.

A.2 Robustness checks and validity of the regression discontinuity design

Below we assess the sensitivity of the results for different bandwidth selections; we check for discontinuities in the forcing variable, age, at the cutoff; we test for discontinuities in other outcomes that should not have been effected by the threshold; and, we check for discontinuities in the outcomes of interest at points in the age distribution where there should not be any discontinuities. This robusness assessment follows the suggestions in Imbens and Lemieux (2008) closely.

A.2.1 Bandwidth selection

The worry in an RD application is that using a bandwidth that is too wide, allows for other things than the intervention of interest to drive differences in outcomes for those right above compared to those right below the threshold. In Table A.1 we display the results using bandwidths of 7 and 15 months for physical health. Using a bandwidth of 7 months does not alter the results, whereas increasing the bandwidths to 15 months somewhat reduces the effects. This is not surprising given the downward slope of the health trajectory across age and the upward shift in this trajectory at the retirement eligibility threshold.

The results for hospitalisations and mortality are displayed in Table A.2 and Table A.3. For acute hospital admissions, we find that increasing the bandwidth to 15 months yields significant, negative effects. The effects are still small ranging from 0.7 to 1 percentage points. As the incidence is 14 percent, this entails a 5-7 percent reduction in the likelihood of an acute hospitalisation. Increasing the bandwidth increases the likelihood of factors, other than retirement, affecting acute hospital admissions. Another explanation can be that it takes some time for retirement to take effect on health issues such as stroke and acute heart conditions, thus including more post-retirement months increase the likelihood of finding significant effects. As in the main analysis, we find no effects of retirement on mortality at any of these bandwidths.

A.2.2 Continuity in the forcing variable

Vital to any RD application is that individuals are unable to manipulate the forcing variable. In this case, the forcing variable is age (reported by public registers), which individuals cannot manipulate in any way. It could however be the case that retired individuals are more likely to respond to the survey due to the reduced opportunity cost of time. Figure A.1 shows two histograms of age-in-months assessing potential bunching at the threshold. There is no evidence of any discontinuity in the forcing variable at the threshold. We also did a more formal test proposed by Cattaneo et al. (2016), a local polynomial density estimator for testing the null of continuous density of the forcing variable at the threshold. The p-value under this test is 0.3251. For the population level data, this holds by construction, as people cannot manipulate their age and as all individuals in the population are represented in the data.

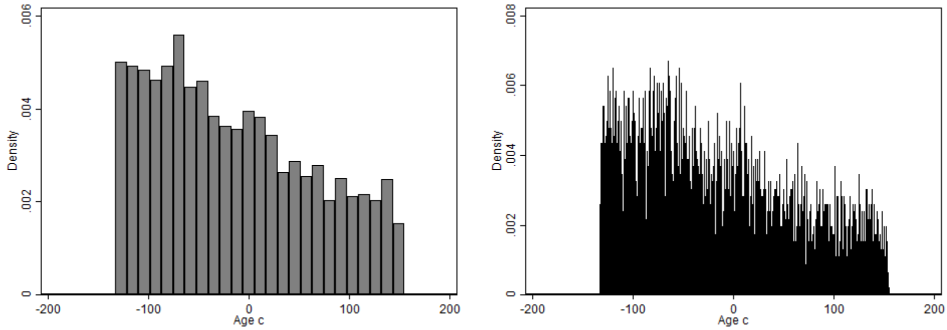
A.2.3 Placebo tests

The placebo tests entails testing for discontinuities in the three health outcomes at points in the age distribution where there should be no discontinuities. A common practice is to conduct placebo tests at the median age of the two sub-samples below and above the actual cutoff. In this case, the median age below the threshold is age 62. However, some individuals can retire at this age, thus making is an unsuited placebo threshold. Consequently, we use age 61 for the lower placebo. For the upper placebo, we use age 73. No discontinuities or significant effects were found at these placebo thresholds for physical health (Table A.1). For acute hospital admissions (Table A.2), we find significant effects for both the upper and lower placebo. For the lower placebo, this could be due to some occupations having special age-limits for retirement at 61. However, we find no explanations for why the upper placebo yields significant, and even positive effects. This finding reduce the credibility of the effects found in the main analysis for this outcome. The placebo results for mortality is presented in Table A.3. There are no significant effects and the coefficients are close to zero for all sub-group at both placebo thresholds.

A.2.4 Discontinuity in other outcomes

Finally, we look for discontinuities in an outcome that should not be affected by retirement, at least not in the short-term. Here, we assess the likelihood of living with a partner or spouse (NorLAG) or being married (administrative data). The regression results shown in Table A.1 and Table A.4 confirm that there are no retirement effect on these outcomes.

Appendix Graphs and Tables



(a) (b)

Figure A.1: Discontinuity of the Forcing Variable

Notes: The histograms show the distribution of age in months for the age-range 56-79 using the bin-width suggested by STATA (left histogram) and using one bin for each age-in-months (right histogram).

Table A.1: Robustness Checks Survey Data: Physical Health

	All	Men	Women
Conditional on income	16.42***	15.83	-1.553
	(2.966)	(10.88)	(7.264)
Observations	82	53	39
Conditional on working	6.274***	9.741***	2.523
	(2.089)	(3.758)	(7.312)
Observations	247	142	105
Bandwidth 7	9.472***	14.69***	2.623
	(2.019)	(5.206)	(4.245)
Observations	275	142	133
Bandwidth 15	5.801***	9.391***	2.623
	(2.130)	(3.109)	(6.628)
Observations	540	278	262
Placebo at 61	-1.441	.971	-5.752
	(3.665)	(4.220)	(6.628)
Observations	454	242	212
Placebo at 73	-1.111	-1.264	.628
	(1.685)	(4.786)	(2.213)
Observations	251	127	124
Living with a partner	-0.106	-0.0413	-0.162
	(0.0931)	(0.108)	(0.176)
Observations	371	190	181

Notes: This table displays the various robustness checks described in the Appendix, for the physical health outcome and the NorLAG data. Standard errors in parentheses are clustered at the age-in-month level. *=p<0.10, **=p<0.05, ***=p<0.01.

Table A.2: Robustness Checks Administrative Data: Acute hospitalisations

	All	Men	Women	Low Educ.	High Educ.
Conditional on working	0.00212	0.00266	0.00133	0.000309	0.00687
	(0.00295)	(0.00426)	(0.00397)	(0.00345)	(0.00566)
Observations	362,857	203,212	159,645	259,427	103,430
Bandwidth 7	-0.00246	-0.00117	-0.00374	-0.00343	0.00231
	(0.00227)	(0.00343)	(0.00303)	(0.00258)	(0.00459)
Observations	583,686	287,791	295,895	455,797	127,889
Bandwidth 15	-0.00722*	-0.00977*	-0.00520	-0.0101**	0.00321
	(0.00377)	(0.00584)	(0.00487)	(0.00427)	(0.00773)
Observations	1,241,687	612,603	629,084	965,278	276,409
Placebo at 61	-0.000587	-0.0000883	-0.00105*	-0.00155**	0.00199*
	(0.000510)	(0.000610)	(0.000541)	(0.000705)	(0.00104)
Observations	1,311,705	667,661	644,044	962,159	349,546
Placebo at 73	0.00106	0.00284*	-0.000557	0.00194**	-0.00321**
	(0.000676)	(0.00162)	(0.000861)	(0.000699)	(0.00153)
Observations	634,319	294,672	339,647	527,740	106,579

Notes: This table displays the various robustness checks described in the Appendix, for Acute hospital admissions. Standard errors in parentheses are clustered at the age-in-month level. Standard errors in parentheses are clustered at the individual level. *=p<0.10, **=p<0.05, ***=p<0.01.

Table A.3: Robustness Checks Administrative Data: Mortality

	All	Men	Women	Low Educ.	High Educ.
Conditional on working	0.0000837	0.000214**	-0.0000777	0.000105	0.0000131
	(0.0000754)	(0.0000981)	(0.000117)	(0.0000848)	(0.000164)
Observations	363,123	203,383	159,740	259,628	103,495
Bandwidth 7	-0.000204	0.0000123	-0.000396	-0.000206	-0.000201
	(0.000249)	(0.000421)	(0.000288)	(0.000285)	(0.000485)
Observations	593,966	294,309	299,657	464,453	129,513
Bandwidth 15	-0.000197	-0.000229	-0.000173	-0.000196	-0.000248
	(0.000164)	(0.000277)	(0.000188)	(0.000187)	(0.000322)
Observations	1,263,829	626,544	637,285	983,902	279,927
Placebo at 61	-0.0000369	-0.00000279	-0.0000721	0.0000256	-0.000210
	(0.0000838)	(0.000143)	(0.000116)	(0.000108)	(0.000125)
Observations	1,324,398	675,316	649,082	972,873	351,525
Placebo at 73	-0.0000758	-0.000128	-0.0000321	-0.0000559	-0.000167
	(0.000128)	(0.000191)	(0.000192)	(0.000144)	(0.000335)
Observations	653,875	306,487	347,388	545,003	108,872

Notes: This table displays the various robustness checks described in the Appendix, for Mortality. Standard errors in parentheses are clustered at the age-in-month level. Standard errors in parentheses are clustered at the age individual level. *=p<0.10, **=p<0.05, ***=p<0.01.

Table A.4: Robustness Checks Administrative Data: Discontinuity in Marital Status

	All	Men	Women	Low Educ.	High Educ.
Married	0.00233	0.00324	0.00104	0.00156	0.00654
	(0.00229)	(0.00336)	(0.00311)	(0.00249)	(0.00576)
Observations	825,605	407,386	418,219	643,441	182,164

Notes: This table displays the impact of retirement on the likelihood of being married. The reported coefficient is τ from Equation (3). Estimation is done using a bandwidth of ten months. Standard errors in parentheses are clustered at the individual level. *=p<0.10, **=p<0.05, ***=p<0.01.

Chapter 4:

Vacation, absenteeism and health. Evidence from a Norwegian change in policy

Vacation, absenteeism and health: Evidence from a Norwegian change in policy*

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October 1, 2018

Abstract

This paper explores the effects of an increase in the entitlement to vacation for individuals aged 60. For the period 2001 through 2008, an extra week of vacation was given to individuals who turned 60 years of age in August or before. Using a regression discontinuity design, we compare individuals who were entitled to an extra week of vacation with individuals close in age who had to wait until the following year to receive the entitlement. We consider days of sickness absence, sick notes authorised by a physician and a set of conditions related to individual health. We find that for the sample as a whole, the increase in the entitlement had no effect on any of the outcomes studied. However, we find that the number of sick notes was reduced for women, for individuals with a history of relatively high sick leave and for individuals with high school as their highest level of education. Targeted policies for older workers may contribute to a reduction in social security dependency through compensation for sick leave. For some, increased vacation may reduce the sickness absence, but it is not clear whether the increased entitlement to vacation has a health-preserving effect.

Keywords: vacation, sick leave, health
JEL Codes: I18, J22, J81, M51

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1 Introduction

How should labour market institutions adapt to the needs and preferences of employees as they get older? This question arises not only from a concern for the welfare of older workers who are worn out after a long and demanding work life but also because of the ageing of the population in Western countries, which mandates policies achieve increased labour market activity of older individuals (OECD, 2013). A relevant target group for such policies is workers who are approaching the early retirement age. In this paper, we study one such policy measure implemented in Norway, namely the one-week extra holiday that employees aged 60 and above are entitled to by law. Until 2009, the length of vacation depended on the month of birth in the year an employee turned 60. This institutional detail creates a unique quasi-experimental setting; only individuals born from January through August could have the extra week that same year; individuals born September through December had to wait until the next year. We exploit this exogenous source of variation to study the causal effects of increased entitlement to vacation in a regression discontinuity (RD) design where the identifying assumption is that individuals born on either side of the cutoff are similar in all relevant aspects except entitlement to increased vacation.[1]

We have two primary outcomes of interest. First, we study the effect on certified sickness absence; both the number of spells exceeding 16 days and the total number of sick notes within a calendar year.[2] Certified sickness absence is an interesting outcome in the context of employment, since disability pension entries typically succeeds longer spells of sickness absence Fevang and Røed (2006) and that it the replacement rate is 100% up to a relatively high ceiling (Markussen et al., 2011). Second, we explore the health effects further by investigating the frequency of specific diagnoses given at general practitioners (GPs) visits, namely regarding cardiovascular, musculoskeletal, and psychological issues. We focus on these diagnoses because they represent common health issues in the population, and they are the main causes of death or disability (World Health Organization, 2002, 2015). This is an important outcome since recent statistics show that 9.8% of the population in Norway receives disability insurance, out of which 24% are aged 60–64 (NAV, 2018).

We find that an increase in the entitlement to vacation had no effect on sickness absence as measured by sick leave exceeding 16 days. In addition, we find a decrease in the number of

[1]At the limit, there is no reason to believe that a person born 31 August is different from a person born 1st September when they are 60 years old. However, issues may arise when we increase the bandwidth since those exposed to the reform (born in August or earlier) are slightly older than those not exposed to the reform (born in September or later). This motivates the use of the RD design.

[2]Spells of sickness absence shorter than 16 days are covered by the employer. Shorter spells of 1-3 days (or 8, depending on workplace-agreement) does not require a medical certificate (sick note). All length of spells are included in the number of sick notes.

sick notes as authorised by a physician, but the effect is not robust to different specifications. Turning to the case-specific diagnoses, we did not find any significant effect. Moreover, the direction of these estimates are not robust to different specifications. Sub-sample analysis reveals important heterogeneity by gender, education level and previous levels of sickness absence. The sub-samples estimates show that an increase in the entitlement to vacation results in a significant decrease in the number of sick notes for women of 24%, but we find no significant effect for men. Turning to highest attained education, we find a reduction in sick notes of around 25% for individuals with compulsory schooling only, and we find a reduction in sick notes of 26% for individuals with high school as highest level of education attained. We do not find any effect for individuals with some form of tertiary education. For individuals with sickness absence above the mean at age 59, we find a reduction of around 38% in sick notes. However, for the cause specific diagnoses and total days of sick leave within a year, the conclusion from the entire sample remains in that the direction of the estimates is not robust to the different specifications.

The results have important policy implications because more vacation may imply a gain in individual welfare and adapt to the needs of workers as they grow older, but also a loss in the production value. However, the results reveals no clear health-preserving effect. Since we are unable to observe whether individuals used their entitled vacation the year in question, the results are interpreted as the intention-to-treat effect, which is the effect of being made eligible for treatment.[3]

There is a large body of literature on the effect of working hours on health (for example Ahn (2016), Rätzel (2012) or Cygan-Rehm and Wunder (2018) and references therein), as well as time allocation between income and leisure (for example Manski (2014)).[4] The literature on how vacation affects sickness absence and health in general is, however, rather scarce. To our knowledge, only two related papers exist on this subject. Recently, Hofmarcher (2017) studied an institutional setting in which Swedish central government employees received entitlement to three extra days of vacation the year they turned 30 and entitlement to four extra days of vacation the year they turned 40. As measures of health, he used specialized outpatient care visits, inpatient care admissions and long-term sickness absence. He found that increased entitlement to vacation had no effect on any of the outcomes, regardless of gender or socioeconomic status. Schnitzlein (2012), using survey data, found suggestive evidence that individuals who do not use their full vacation entitlement have a lower life satisfaction and higher sickness absence, compared with individuals who claim their full va-

[3] This may underestimate the potential treatment effect of the policy. The point estimates should therefore be interpreted as a lower bound effect of the policy.

[4] There is also a large body of literature on job-stress recovery related to occupational pension. See for example Demerouti et al. (2009).

cation entitlement. However, this interpretation is probably not causal since there may be underlying unobserved factors that affect the propensity to use full vacation.

Our study differs from Hofmarcher (2017) in three important aspects. First, we do not limit our attention to a particular sector of the economy since we study the effect of a regulation that affects all workers in Norway who reach a certain age. Second, the setting allows for comparing individuals born in the same calendar year in a regression discontinuity setting. Third, the sample studied are employees who are older (age 60) and who are at an age where health-related absence or withdrawal from the labour market is common.

This paper proceeds as follows: Section 2 briefly discusses the institutional setting of vacation and sickness absence. Section 3 describes a simple theoretical framework relevant for the analysis. Section 4 describes the data for the analysis, final sample selection and descriptive statistics. Section 5 discusses the regression discontinuity framework and its validity. Section 6 presents the results, and Section 7 presents our conclusions.

2 Institutional Setting

2.1 Vacation Regulations

In the following, we present institutional details regarding vacation relevant for the period studied (2001–2008). First, we sketch the general regulations that apply for employees below the age of 60, and we present the special regulations for employees 60 years old and above.

By law, all employees are entitled to 25 days of vacation during a calendar year. Since Saturdays are included in this number, 25 days imply four weeks (six days each) and one day. Within some industries, because of collective agreements, employees are entitled to five weeks (30 days) of vacation. If an employer has signed the collective agreement, it applies to all employees of the firm, union and non-union members alike. Some employers offer their employees longer vacation through local agreements at the company level. The vacation act sets a minimum standard; its regulations can only be overruled by local agreements if this is to the benefit of the employees. The right to vacation remains for employees who change employers.

Whether the vacation is paid or unpaid depends on the length of the employee's employment in the previous calendar year. Every employer is obliged to set aside a fixed proportion of each employee's wage for holiday pay the following year. There is every reason to believe that the uptake of vacation is high.[5] Employers are responsible for seeing to it that employees actually take holidays, and can, in fact, receive a fine if workplace conditions, such

[5] No survey or official statistics documents record the number of vacation days, but rather the number of days travelling.

as job and workplace stress, prevent their employees from having vacation. Furthermore, employees also have a duty to see to it that they take time off from work in the form of vacation. For the 25-day vacation, there are very few exceptions to the rule that vacation cannot be traded for cash.[6] However, in the private sector, employees entitled to 30 days of vacation can trade the extra five days for cash.

Employees are entitled to three consecutive weeks of vacation from the period 1 July through 30 September (in the manufacturing sector, the tradition is to take four weeks of vacation in July). For the rest of the entitled vacation, with a few restrictions, employees and employers must agree when the vacation should be taken. The holidays act allows a written agreement about maximum 12 vacation days in advance or, more commonly, postponing the maximum 12 days of the entitled vacation

Since 1976, employees aged 60 or older were entitled to one extra week of vacation, irrespective of tenure/seniority or wage level.[7] Since the extra week of vacation is determined by law and independent of local and collective agreements, it may be beneficial for groups of employees with weak bargaining power. The employee is free to decide when to take the extra vacation, and it may be spread throughout the year or concentrated to one week.

Important for our study, the extra week of vacation was restricted to individuals who turned 60 years before the 1 September. For example, in 2008, a person born 31 August 1948 would be eligible for one extra week in 2008, while a person born the 1 September 1948 would have to wait until 2009 to get the same length of vacation.[8] We do not observe whether (or when) individuals actually made use of the option during a given year. The extra week of vacation can, for instance, be postponed until the following year.

2.2 Sickness Absence

An important institutional aspect of our study concerns sickness absence. Norwegian sickness insurance is mandatory and regulated by law, and it covers all employees who have been employed at the same employer for at least two weeks. Furthermore, the replacement rate is 100% up to a relatively high ceiling from day one. A medical certificate is required for spells of absence of three to eight days, depending on whether the employer has signed the

[6]Until 2014, if an employee had not had the full vacation that he or she is entitled to by law because of illness or parental leave, the employer could compensate in cash for vacation not taken after the maximum prolongation of 12 days is utilized

[7]Thus, 60 year olds enjoy enjoy 30 or 36 days' vacation in total, while their younger colleagues enjoy 25 or 30 days, depending on local agreements.

[8]From 2009 onwards, this rule was cancelled so that all employees are eligible for one week extra vacation the same year they turn 60, irrespective of month of birth.

IA-agreement.[9] The first 16 days of absence are paid by the employer (the employer period), whereas the remaining period is paid by social insurance. The maximum period of benefits is one year, including the employer period. The level of sickness absence in Norway is high, especially among women Markussen et al. (2011), and around 1.7% of the GDP is allocated to sickness benefits annually (Bjørnstad, 2013). Recent statistics from Statistics Norway (2018) show that around 8.2% of workers aged 60–64 in Norway had sickness absence certified by a GP in 2017. In the analysis, sick leave exceeding 16 days is one of the outcomes. We also consider number of sick notes certified by GPs and diagnoses that may or may not be related to a certified absence. We discuss these outcomes in greater detail in Section 4.

3 Conceptual Framework

While the institutional characteristics of our setting provide the random variation in the allocation of an extra week of vacation, it is useful to discuss some theoretical aspects to guide our empirical analysis of individual responses to increased leisure. A useful starting point is the human capital framework, where health is a commodity that individuals produce by means of market goods and services and their own time input (Grossman, 2006). Formally, using the notation in Grossman (2006), the individual utility function can be expressed as follows:

$$U = U(Z_1, Z_2, .., Z_n), \tag{1}$$

where health is represented by Z_i. We formalize the production of health as follows:

$$Z_i = e^{p_i S} F_i(X_i, T_i), \tag{2}$$

where X_i is a market good or service input, T_i represents time disposable for producing health, S is a measure of the efficiency of the health-production process, and p_i is a positive parameter. S represents human capital, which in empirical work is typically proxied with years of schooling or highest level of attained education.

In our setting, where entitlement to vacation is increased and income is unchanged, the level of utility is unambiguously increased (the individual's consumption possibilities have expanded). Looking at the production of health, T_i increases (as long as we think that leisure gives more opportunities for health production than being occupied at work). Ruhm (2000) shows that less time working allows more time to invest in non-market goods. Increased leisure could lead individuals to pursue activities otherwise bounded by time-constraints,

[9] The IA agreement is a letter of intent based on agreement between authorities, employees and employers. The intention is to work towards a more inclusive working life.

for instance, exercise, which could mean less work-related stress. On the other hand, more leisure could also lead to physical inactivity. Some studies have found that we can think of unhealthy goods, along with vacation, as a normal good (Ettner (1996), Petry (2000), Kenkel et al. (2014) and Apouey and Clark (2015)). In our setting, where income is unchanged, X will change only to the extent that it is complementary to or a substitute for T. X could, for instance, be travelling, use of alcohol or tobacco, or GP visits.

The human capital model (which assumes a positive marginal product of T) predicts that the efficiency of T increases with education. Overall, the predicted effect of increased leisure on health is not clear (neither the sign of the marginal effect of T or of the interaction between T and X), and this is what we investigate in the empirical section.

4 Data, Sample Selection and Descriptive Statistics

4.1 Data

To study the effect of increased leisure on health and sickness absence, we use several administrative data sources linked through a unique, project-specific, anonymous identifier assigned to every individual in Norway. Information about employment, days of sickness absence (covered by social insurance) and demographic information comes from various registers maintained and delivered by Statistics Norway. Data on sick notes and physician visits are from the Normal Tariff for Private General Practice KUHR database,[10] administered by Helfo, a subordinate of the Norwegian Directorate of Health. Data from Statistics Norway cover the years 2001–2008, and the KUHR data exists from 2006 through 2008. In contrast to survey data, there is no attrition from the original sample due to non-response or non-consent in any of these variables. In the next sections, we elaborate how we apply these data sources in the empirical analysis.

Data on employment and sick leave: Information about employment are from the employer-employee register, and consists of all spells of employment, and information on start and stop dates of all spells (in months). This information is important for the sample construction, which will be discussed later in this chapter. We also have data on all spells of sickness absence covered by social insurance, for example, spells of sick leave exceeding 16 days, with the exact start and stop dates for each spell.

For the empirical analysis, we use the outcome variable *days of sick leave*, which for each individual represents the total number of days covered by social insurance in a given calendar year.

[10]KUHR stands for Normal tariff for private general practice (Kontroll og utbetaling av helserefusjoner)

Data on visits to a physician: For every visit to a physician, whether at the GP office or at an emergency care unit, the GP sends a reimbursement claim, which is then then registered in the KUHR register. This claim specifies the services and procedures rendered during the consultation. In particular, the reimbursement claim specifies whether the GP certifies sickness absence through a sick note or extends a sick note. Therefore, for each person, we are able to identify *the number of sick notes* issued during a calendar year. Since employees need a certified sick note for spells that exceeds three (eight) days, *the number of sick notes* is an outcome that captures all certified spells, irrespective of duration of the sick leave. In that respect, it is a more comprehensive measure of sickness absence than *days of sick leave.*

Individuals also visit the GP for medical help not related to sickness absence certification, and we want to capture this by investigating all GP visits regardless of the issuance of a sick note at the consultation. To this end, we exploited reimbursement claim information on diagnosis, divided into groups following the International Classification of Primary Care-2nd Edition (ICPC-2) (Lamberts and Wood, 1987). We construct three outcome variables that represent three specific groups of diagnoses: the number of GP visits per year for *cardio-vascular diseases*,[11] *musculoskeletal pain*, and *psychological issues*. Cardiovascular disease is the leading cause of death in most developed countries (World Health Organization, 2015), whereas musculoskeletal (e.g. lower back problems) and psychological issues is one of the leading pathways to disability insurance (ibid.). In Norway, around 9.8% of the population receives some form of disability insurance, and in 2018, 13.1% of the population aged 60–64 received some form of disability insurance (NAV, 2018)

Other administrative data: We also use demographic information regarding the highest level of education, marital status and annual earnings; the latter originates from tax registers. From the central population register, we included information about gender and month of birth, which defines our control and treatment group. Information on educational level comes from the educational database, and we define three different categories: compulsory schooling only, high school completion and any form of tertiary education. From the family register, we included information regarding legal marital status.

4.2 Sample Definition & Descriptive Statistics

In the empirical analysis, we are interested in individuals who turned 60 years of age at some point between 1 January 2001 and 31 December 2008. This means that the relevant cohorts (454,066 individuals) were born between 1941 and 1948. Besides the time frame of the

[11]Cardiovascular diseases and diagnoses related to, among others, coronary heart disease and ischemic heart disease.

cohorts, the sample was constructed following two additional criteria. First, an individual must remain employed from 1 January to 31 December the year he/she reached 59 years of age. As explained in Section 2, individuals who experienced spells of unemployment are still entitled to paid vacation time, but, unless otherwise agreed, receive the holiday pay the same day he/she receives the final wage payout. We therefore imposed the restriction of employment the year a person turns 59 years of age to ensure that individuals are entitled to the same weeks of paid vacation. Second, individuals who work as teachers are entitled to a reduction in hours thought by 12.5%, from the school year that starts when they turn 60. As this is in conjunction with an increase in vacation entitlement, teachers are not included in the final sample. These two criteria yielded a sample of 244,757 individuals.

Table 1 presents the mean summary statistics for demographic characteristics and outcomes. The first columns displays the information for the entire sample. The second and third column displays information for those born from January through August (entitled to 6 weeks) and for those born from September through December (entitled to 5 weeks), respectively. For the background characteristics, when looking at the fixed characteristics we see that there are very few differences between these groups. The outcomes indicates that there may be no differences in any of the outcomes as these variable are very similar. The two remaining columns shows that there are some differences by gender. Males have a relatively higher share of individuals with some college or more as highest attained education. The mean income (measured the year individuals turn 59 years) is also, perhaps unsurprisingly, higher among men than women. Turning to the outcomes used in the analysis, there are particularly clear sex-differences in the relevant diagnoses used as an outcome in this analysis, let alone the outcomes for sick leave and sick notes. Both psychological and musculoskeletal diagnoses are higher among women, whereas cardiovascular diagnoses are higher among men.

5 Regression Discontinuity Design

5.1 Sharp Regression Discontinuity Design

We are interested in whether increased entitlement to vacation has any effect on health and sickness absence. The identification problem is that individuals preferences for (increased entitlement to) vacation may be correlated with unobserved preferences, or characteristics may be correlated with health and sickness absence. As discussed in Section 3, it is not a-priori clear whether vacation, as time of from work, may lead to better health. People may have preferences that directly determine their health and, thus, sickness absence. We address these problems using a RD design, where we exploit the assignment mechanism of the legislation induced for workers aged 60. As described above, entitlement to increased vacation is here a deterministic function of month of birth. The treatment in our case changes from zero to one at the cut-off point, which means that all subjects received their assigned treatment or control condition. This particular feature of the RD design is the sharp regression discontinuity design (SRD) because the probability of treatment change sharply for all individuals born between August and September (see Hahn et al. (2001), Imbens and Lemieux (2008), Lee (2008), and Lee and Lemieux (2010).

A compelling feature of the RD design is that individuals born within a few months of each other, but on the opposite sides of the cutoff, are most likely to be very similar. Hahn et al. (2001) linked this feature to the 'gold standard' of randomised experiments. The conjecture that the treatment assignment is as 'good as randomised' hinges on the assumption that individuals just below and just above the cutoff point have the same potential outcome in an identical experience where an extra week of vacation is randomly assigned.[12]

Lee (2008) shows that treatment depends on whether a variable exceeds a known threshold and agents cannot control precisely the forcing variable, the continuity assumption is satisfied since the variation in treatment around the cut-off is randomised. This is because the variation in treatment close to the cutoff is randomised in a way similar to if a person randomly has been given a month of birth just below or just above the cutoff. In that respect, the method identifies the effect of the increase in entitlement to vacation at the cutoff point (Hahn et al., 2001).

To further explain the intuition behind our choice of SRD design, we follow the setup applied by Kostøl and Mogstad (2014) in which individuals are eligible for an extra week of vacation based on turning 60 years of age before a particular cutoff date c. For this group, denoted by l, we can specify the regression model of the following form, where X is month of birth and $X < c$:

$$Y = \alpha_l + f_l(c - X) + \varepsilon_l, \tag{3}$$

Likewise, the following regression model is specified for individuals who turn 60 in September or later, denoted by r:

$$Y = \alpha_r + f_r(c - X) + \varepsilon_r, \tag{4}$$

The effect of an extra week vacation on health H is the difference in intercept between the

[12] Sekhon and Titiunik (2017) discusses a few settings in which compliance with treatment assignment is perfect, but where the score may be a possible determinant of the outcome regardless of the assignment mechanism itself. Their examples is based on outcomes with a likely underlying but unobserved ability, and it is not clear if their setting relates to a setting in which month of birth solely determines access to treatment. It is however worth noting that random assignment of the running variable does not necessarily imply local independence.

two groups, given by $\hat{H}$:

$$\hat{H} = \hat{\alpha_l} - \hat{\alpha_r}, \tag{5}$$

The identifying assumption is that assignment to treatment (entitlement to one extra week of vacation) is uncorrelated with observed and unobserved covariates. By pooling the two regressions (3) and (4) together into equation (5), we test whether entitlement to an extra week of vacation matters discontinuously, by comparing individuals whose date of birth correspond to being entitled to an extra week of vacation to those who just falls short of being entitled to an extra week of vacation.

Formally, we estimate the following reduced form equation:

$$Y_i = \alpha + f(X_i) + \tau D_i + \varepsilon_i, \tag{6}$$

where Y_i is the outcome variable summarized within the calendar year when the individual turns 60 years. The dummy variable D_i indicates if an individual is born before September and hence if he or she was entitled to an extra week of vacation. $f(X_i)$ is an (unknown) function that captures the underlying relationship between the dependent variable and the running variable. Since the running (or forcing) variable is discrete, random disturbances can be correlated within each month of birth which means that standard errors could be downward-biased.[13] Thus, in all of the regressions, and as suggested by Lee and Card (2008) we use heteroskedastic-adjusted standard errors. The reduced form equation displayed in Equation 6 does not include any covariates, as adding these should not alter the estimates from parameter τ, but only improve precision (through reducing residual variation in the outcome variable).[14]

Two important aspects of the estimated model (Equation 6) warrant discussion in the RD-setting. First, we need to specify $f(X_i)$ as this is an unknown functional form. Second, we specify the bandwidth, that is, how many months to include on each side of the cutoff in the estimation. This is important, since results in the the RD design can be sensitive to the functional form or the interval around the cutoff point used in the local regressions, and a non-linearity can be mistaken as a discontinuity. We therefore check the specification choice by examining the sensitivity of regression estimates to functional form assumption, and especially, by comparing the different functional forms with each other.[15]

[13]The running (or forcing) variable is only discrete to the extent that month of birth is the most detailed information we have on date of birth. It is reasonable to assume that information on the exact date of birth can be treated as continuous random variables.

[14]However, we do show that adding a set of covariates does not alter the conclusion.

[15]Several methods of choosing the neighbourhood around the cutoff exists in the literature and Cattaneo and Vazquez-Bare (2016) provide an excellent overview of these methods.

The choice of specification and bandwidth follows the discussion in Lee and Lemieux (2010), and the first approach is to make use of the full sample by using a flexible functional form by including second-order polynomials in the regression.[16] The second approach is a local linear regression where we choose the bandwidth (that is how many months to include on each side of the threshold) based on a trade-off between variance and bias (Lee and Lemieux, 2010). The trade-off arises because when moving further away from the threshold, one has more data points at hand, but this comes at a cost of an increased risk of biased estimates since the observations are further from the threshold (in our case, born before September). The goal is to find a bandwidth that minimizes the mean square errors of the conditional expectation function. Imbens and Kalyanaraman (2012) proposes an optimal bandwidth selector that follows from a cross-validation procedure, but they provide an automatic way of selecting the optimal bandwidth for local linear regressions.[17]

While our preferred specification follows from the optimal bandwidth calculation in the cross-validation procedure, it has become standard in the RD-literature to present a variety of different bandwidths and functional forms (Imbens and Lemieux, 2008). As a robustness to the RD specification, we report the results with a range of bandwidths from one to three months around the discontinuity and a linear regression including second-order polynomials in the regression making use of all observations.

5.2 Validity of the Regression Discontinuity Design

In this section, we discuss the validity of the RD strategy. The first concern that would violate the validity of the RD design is individuals' ability to manipulate the running variable (Lee and Lemieux, 2010). In general, when there is an economic incentive involved, we would expect individuals to make an effort to obtain a favourable value of the running variable (or score) relative to the cutoff. Imprecise control of the running variable is therefore crucial to avoid self-selection bias. It is highly unlikely that month of birth varies in other ways than by the means of seasonal patterns. Manipulation behaviour require knowledge of institutional rules before being born, and as outlined in Section 2, the introduction of entitlement to an extra week of vacation came in 1976. This means that it is impossible that parents manipulated their children's date of birth in anticipation of this change in policy. As we base our information on month and year of birth from administrative registers, we are confident

[16] Gelman and Zelizer (2015) and Gelman and Imbens (2017) discuss why the use of higher-order polynomials than degree two may be inappropriate.

[17] Cross validation in general refers to splitting the data into a so called training set and a validation set in which the goal is to find the bandwidth h that minimize the Mean Squared Error (MSE). In Imbens and Kalyanaraman (2012), the bandwidth is selected following an optimal MSE data-dependent bandwidth choice for the local-linear regression point estimator.

that no later life manipulation falls in conjunction with the assignment variable.

For the sake of completeness, Figure 1 shows the frequency distribution of individuals' month of birth relative to the cut-off. People born to the left of the cut-off receive an extra week of vacation, whereas individuals born in September or later are only entitled to five weeks of vacation. The distribution displays some seasonal patterns but remains somewhat smooth around the cut-off for entitlement to increased vacation, as would be expected in a valid RD design Lee (2008).[18]

Another key assumption in the RD design is that the conditional expectations of the potential outcomes are smooth functions at the threshold. The only variables that should vary discontinuously at the threshold are the outcome variables defined in the previous chapter. If this assumption holds, we attribute any discontinuity in the outcomes of interest at the threshold to the effect of an extra week of vacation only. We follow Lee and Lemieux (2010) and rely on some indirect tests to investigate whether individual characteristics have the same distribution on both sides of the threshold.

Figure 2 further plots evidence of the validity of our design. Here, we verify that a range of covariates expected to be correlated with health (earnings, gender, marital status and education) do not vary discontinuously at the threshold. This is reassuring and gives us confidence that our conjecture that the results do not reflect pre-existing demographic or work-related differences across month of birth.[19]

6 Results

6.1 Graphical Analysis

To examine the effect of the increase in entitlement to vacation, we present a set of descriptive figures that show the relationship between month of birth and different outcomes of sick leave and health. In Figure 3, each point represents the sample mean for months of birth. Recall that the only outcome covering the entire period of 2001 through 2008 is sick leave exceeding 16 days; the remaining outcomes are sample means. We also include a local linear fit covering all months of birth relative to September. The vertical line denotes September, which is the cut-off date.

There seems to be no general effect on days of sick leave, as displayed in Figure 3a. While

[18]In settings where it is debatable whether individuals have imperfect control of the running variable, a common application in the literature is to perform the McCrary (2008) density-test for manipulation. However, this test has been shown to perform poorly in the presence of a discrete running variable (Frandsen, 2017).

[19]The estimate in Figure 2 depends on the breakpoint chosen, which may lead to different trajectories on both sides of the cut-off. See Gelman and Zelizer (2015) for a discussion.

there is a change in trajectories in Figure 3b, there is no jump at the threshold. Turning to the health outcomes, Figures 3c–3e show the discontinuity in psychological diagnoses, cardiovascular and musculoskeletal diagnoses, respectively. In general, there is very little variability in month of birth averages. Figure 3c indicates some discontinuity, but this increase is not obvious when looking at the variability on each side of the threshold.

The descriptive graphs presented in Figure 3 suggest that there are few patterns regarding the effect of the increase in entitlement to vacation. To test whether this is actually the case, we move on to the empirical models with a complete set of specification tests.

6.2 Regression Results

We first investigate the effect of an increase in the entitlement to vacation for the full sample. Following the cross-validation procedure in Imbens and Kalyanaraman (2012), we calculate the optimal bandwidths to be three months for the total number of days on sick leave, and two months for all the other outcomes. This holds for the main sample and for all subsamples used in the estimations. To discuss the sensitivity of the choice of bandwidth, we display the results for each outcome with a bandwidth of 1–3 months on each side of the cutoff.

Table 2 provides, for the full sample, regression discontinuity estimates over varying bandwidths and functional forms with standard errors reported in parentheses. Panel (a) displays the results using second-order polynomials, whereas the coefficients in Panel (b) are interpreted as a first-difference. Column 1 reflects the estimated effect on sickness absence through the calendar year. Column 2 displays the number of sick notes, whereas the final columns display the three different diagnoses. There were no statistically significant effects on any of the outcomes under consideration. Column 1 reflects a small increase in sickness absence for individuals who received the entitlement.

However, the precision of the estimated coefficients is poor, and the estimates are not significant in any of the specifications. Column 2 indicates a small reduction in the number of sick notes. Remember that sick notes consist of all spells of sickness absence that require certification from a physician, whereas the measure of sickness absence in itself only consist of spells longer than 16 days. The effect is significant at the 10% level in Panels (b) and (c), but they are not robust to differences in specification. In addition, the standard errors are too large to justify any inference. Furthermore, there are no statistically significant effects on any of the three diagnoses under consideration. The standard errors are relatively large, which means that none of the estimated parameters are distinguishable from zero.

As for the robustness to the results in Table 2, we performed the same regression with the inclusion of some relevant covariates, displayed in Table 9. As argued in Section 5, any valid RD design should only be affected by increased precision. Adding educational level, marital status and earnings from the year he/she turned 59 (year before the policy) does not alter the conclusions in any away. The number of sick notes is now significant at the 10% level in Panel (d), but this is not of a concern since the point estimates and standard errors only barely changes. For the rest of the specifications and outcomes, we observe only minimal changes in the point estimates and standard errors.

Either way, we find that for the entire sample, an increase in entitlement to vacation had no effect on sickness absence or health as measured by visits to a physician, but there are some indications of a decrease in the number of sick-notes, significant at the 10% level. In the case of Sweden, Hofmarcher (2017) found that an increase in vacation for central government employees aged 30 and 40 resulted in no change in objective measures of health. While his sample consisted of workers at a different stages in life, he challenges the health argument for more paid vacation days. This is exactly what we find, and the results shows that, regardless of specification, choice of bandwidth and outcome, and inclusion of controls, the estimated coefficients yields no meaningful effect as the standard errors are too large to justify any consistent inference and thus not statistically distinguishable from zero.

6.3 Heterogeneity

While the analysis thus far reveals no effect of the increase in entitlement to vacation, it is important to look further into how different groups are affected. For example, women may respond differently than men. Furthermore, we also investigate whether differences in socioeconomic status as a result of differences in education and previous history of sickness absence affected the outcomes. In a next step, we split our analysis sample into several sub-samples to evaluate whether the increase in entitlement to vacation had any heterogeneous effect.

Outcomes by gender

Table 3 provides the results from the effect of the increased entitlement to vacation for men, following the same specification and bandwidth as above. In general, the estimates confirm the results found in Table 2. Regardless of specification, the estimated parameters are statistically indistinguishable from zero. The results in Columns 1 through 5 support the findings in Table 2, in that an increased entitlement to vacation has no observable effect on objective measures of health or sickness absence. In other words, we find no evidence that an increase in entitlement to vacation is associated with an effect on sickness absence and objective measures of health for men. There is no (statistically significant) pattern of any type for men around the cutoff.

Table 4 displays the effect of the increase in vacation for women. Interestingly, compared with the findings in Tables 2 and 3, the sign of the estimated coefficients changed. Column 1 shows, albeit not significant, a reduction in days of sickness absence. Looking at the number of sick notes in Column 2, we see that Panel (a) displays no significant effect. The linear regressions presented in Panels (b) through (d) show that there is a significant drop in the number of sick notes. For the preferred bandwidth specification in Panel (c), the effect corresponds to an average reduction by 0.27 sick notes, approximately 24%, decrease. Moreover, while the impact of the increase varies between Panels (b) and (d), the effect does seem to be robust to varying bandwidths but not the flexible specification that includes a second-order polynomial on each side of the cutoff.

By breaking down the effects shown in Table 2 by gender, it is clear that the increased entitlement to vacation had no effect for men, whereas we do find a reduction in GP certified sick notes for women. Qualitatively, the results reported in Columns 3 through 5 in Table 4 are similar for the whole sample and for men; neither of the estimated coefficients of interest are statistically significant at any of the conventional levels.

Outcomes by Education

An important question is whether the increase in entitlement to paid vacation days masks any heterogeneity across socioeconomic groups. While this distinction is common when considering the difference between white-collar and blue-collar jobs, we proxy for socioeconomic status by splitting our sample into three different levels of education. The reason is that administrative data enables us to identify the sector in which he/she works and not whether the job is strenuous. Education is also an important determinant of health (Mazzonna and Peracchi, 2013). The subgroups are individuals with no form of attained education (for example a high school dropout), high school as highest attained education and at least one year of college-education, respectively.

Table 5 provides the estimated results for individuals who have only completed the mandatory years of education (i.e., not graduated from high school). For this group, the estimated effect on days of sick leave is positive, but far from statistically significant in any specification, as reported in Column 1. Interestingly, when considering the number of sick notes, Panels (a), (b), and (d) of Column 2 display an increase in the number of sick notes significant at the 5% level. For the preferred bandwidth in Panel (c), there is a similar magnitude as in Panel (a), but it is only significant at the 10% level. Either way, we find that the number of sick notes increased for individuals with only compulsory schooling. This is somewhat surprising since we expect that the number of sick notes would go down.

Next, Table 6 reflects the results for individuals with high school as the highest attained education. The results stand in contrast to those found in Table 5. Although not significant, Column 1 shows a reduction in days of sickness absence. Column 2 reflects a reduction in the number of sick notes by 0.3 in a year, approximately 26%, for the preferred bandwidth in Panel (c), significant at the 1% level. While the estimates differ in magnitude, they are still robust to the different specifications. Columns 3 and 5 show no changes in number of psychological and cardiovascular diagnoses. Column 4 reflects a reduction in the number of musculoskeletal diagnoses. Remember that this type of diagnoses consists of lower back pain, which is one of the most prevalent conditions related to disability insurance in OECD countries (World Health Organization, 2011). Within the different specifications in Column 4, the effect does not appear to be robust. The flexible specification shows that the estimated coefficient is indistinguishable from zero, which questions the validity of the findings in Panels (b) and (c). However, the direction of the estimated coefficients indicates a reduction in diagnosis concerning musculoskeletal issues.

Table 7 displays the estimated coefficients for individuals with a minimum of one year of higher education. The first column indicates that days of sick leave increased for the group who received the entitlement to an increase in vacation. However, the outcome is not robust to differences in bandwidth. While specifications reflect a significant increase in days of sick leave, the estimates are not robust to the preferred bandwidth or the flexible specification. At the same time, the standard errors are too large for any meaningful interpretation. Thus, the results suggest that there was no change in days of sick leave for individuals who received the entitlement. Consequently, none of the estimates in Columns 2–5 are significant, which suggests that for individuals with higher education, the increase in vacation has no effect on any of the outcomes. The optimal bandwidth in Panel (c) reveals no change in any of the outcomes, which, together with the results discussed above, confirms that the point estimate is small and statistically insignificant for these measures.

Consequently, none of the estimates in the second through fifth column are significant, which further suggests that for individuals with higher education, the increase in vacation had no effect on any of the measures under consideration. The optimal bandwidth in panel c reveals no noticeable change in any of the outcomes, which, coupled with the results discussed above, confirms that the point estimate is small and statistically insignificant for these measures.

Altogether, there are some differences in the effect of an increase in vacation by education. Individuals with some college education and individuals with compulsory schooling only seem experience a small increase in sickness absence, but this result is not robust to the different forms of specification. In contrast, individuals with no education beyond high school experience a decrease in number of sick notes, which is robust to different forms of specifications.

Heterogeneity by previous sickness absence

Based on the findings above, one may speculate that individuals prone to sick leave have already left the labour market through disability insurance. Another interesting aspect is whether individuals previously prone to sickness absence benefit from an increase in entitlement to vacation. Because this measure is inherently coupled with dispersion, we suspect that it is the individuals already struggling with high rates of absence who should benefit the most from an increase in vacation.

Table 8 shows the estimated effect for individuals who, in the preceding year, had a total number of sick days equal to or above the mean in that particular year. Panel (a) shows that, for this group of individuals, an increase in vacation led to a decrease in sickness absence of around seven days. However, this result is not robust to specification, which calls into question the credibility of the estimated effect in Panel (a). Turning to the second column, we find a reduction in the number of sick notes as authorised by a physician, which is robust to the different specifications. Thus, it appears that those individuals prone to relatively higher rates of sick leave experienced a reduction in days of sick leave. The magnitude of the estimated coefficients in Panel (c) corresponds to an average reduction of around 0.43 sick notes, a reduction of 37%. The effect on diagnoses is not clear. Although Panels (c) and (d) in Column 5 display a significant reduction in the number of cardiovascular diagnoses at the 10% level, the outcome is not robust to the different bandwidths.

How do all of these results fit together? Women, individuals with a history of relatively high sickness absence and individuals with a high school education experience a significant reduction in the number of sick notes. Moreover, for the two latter groups, we find indications of a reduction in days of sickness absence above 16 days, albeit not robust to difference in bandwidth or difference in specifications.

In general, the outcomes representing health and sickness absence above 16 days yields no significant effect. To speculate on why this may be the case, it is likely that individuals potentially benefiting the most from extra time off from work actually have left the work force before turning 60. Remember that Norway, as briefly discussed in the institutional section, has a relatively high proportion of individuals on disability insurance. Since we condition on continuous employment throughout the year individuals turn 59 years of age, we may disregard individuals who would have benefited from such an increase. This is consistent with the finding that sickness absence is reduced for individuals with a relatively high rate of sickness absence at age 59.

7 Discussion and Conclusion

In this study, we examine how an increase in entitlement to vacation affects workers sickness absence and health. We exploit a targeted increase in vacation for individuals at the age of 60. Between 2001 and 2008, month of birth determined whether individuals received entitlement to an extra week of vacation at age 60. Individuals who reached the age of 60 in September or later had to wait until the following year to receive the extra week of vacation, whereas individuals born in August or earlier received the entitlement the year they turned 60. We take advantage of this sharp discontinuity and high quality register data to investigate whether an increase in vacation affects sickness absence above 16 days, number of sick notes as authorized by a physician and measures of health.

The vacation reform was intended to relieve the work strain of older employees, potentially leading to improved employee health and individual welfare in general. However, these favourable consequences were presented under the condition that extra leisure time was 'used well', that is, it did not encourage a passive life-style (Ot. Prp. nr. 42 (1976) Proposition to Parliament). In the green paper, reduced sickness absences and shirking were mentioned as possible consequences, but also the risk for increased job intensity (same workload but fewer work hours) and the possibility that older workers would become less competitive. The green paper concludes that there is a need for an evaluation of the reform, but this has not been carried out (NOU, 1975).

We find that for the whole population of 60-year old employees, entitlement to an extra week of vacation has no effect on sickness absence or health. However, we did find that some groups experienced a reduction in sick notes issued. The effect is especially salient for individuals with a relatively high rate of sickness absence in the preceding year and individuals with high school as highest level of education. There are also indications of a reduction in sick notes among women and an increase among employees with compulsory schooling only, although these results are clear.

While there is a negative effect on sick notes for some subgroups, we did not find an effect on days of sick leave for the same groups. The coefficients are predominately negative across specifications, but are not statistically significant. Remember that certified sick notes concern all spells of sick leave irrespective of duration, apart from the first self-reported days. Thus, this outcome most likely captures less severe issues of health compared with days of sickness absence in excess of the first 16 days. Finally, none of the estimated effects on the three measures of health (number of GP visits for psychological, musculoskeletal, and cardiovascular diagnoses, respectively) are robust to specification or bandwidth. A reasonable explanation is that one extra week of vacation has a transitory effect on individual health, which does not affect the measures of health we considered. Furthermore, since the estimates are the ITT effect of the entitlement to increased vacation, it may underestimate the true benefit of actual treatment. However, it may also be that increased vacation is not linked to a health-preserving effect, or if so, not detectable in our data.

Our study contributes to two important aspects in the policy debate. First, Norway has a relatively high degree of sick leave and individuals on disability insurance. Second, many OECD countries have implemented reforms to increase the share of individuals working relative to people retired. We find that some groups may benefit from the increased entitlement to vacation, especially individuals with a relatively high sick leave in the preceding year. However, for most groups, the health-preserving argument is not present. Our findings are also important since there is a growing share of the population in most OECD countries who are nearing retirement, and given an increased toll on public budgets, targeted policies for older workers may contribute to a reduction in social security dependency through compensation for sick leave. It is unfortunately not clear whether increased entitlement to vacation is the right policy tool to accomplish this target.

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Figures and tables

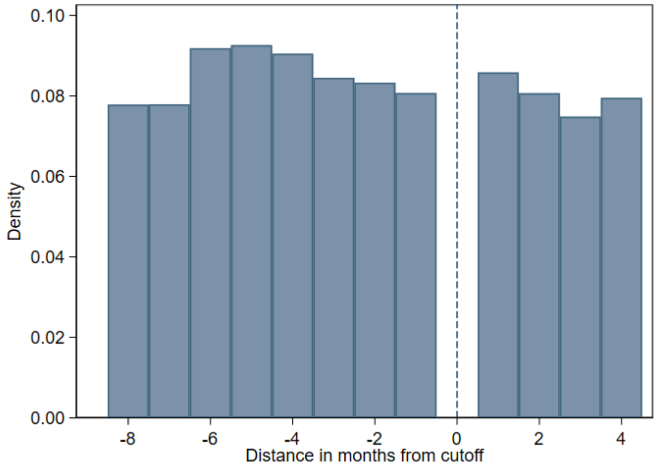
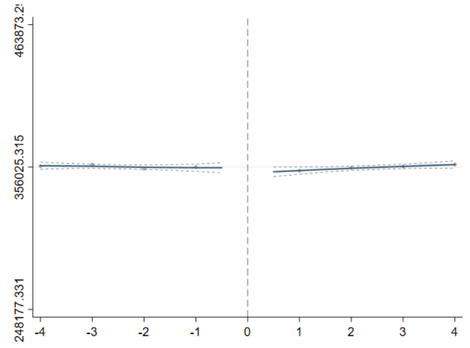
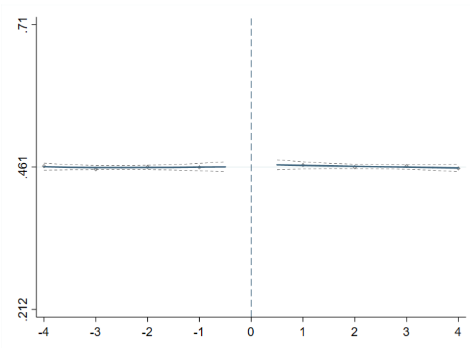


Figure 1: Histogram of Running Variable for RD Analysis

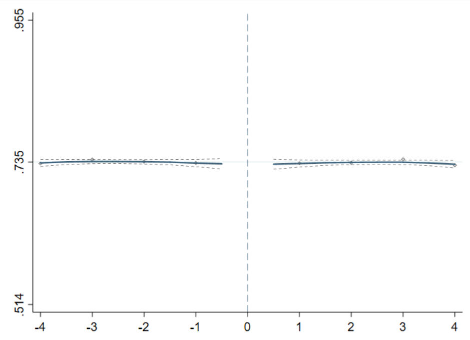
Notes: The sample consists of all individuals who turned 60 at some point in the calendar year between 2001 and 2008 whose month of birth decided entitlement to an extra week of vacation. Month 1 indicates born in September, whereas -1 indicates born in August and 1 indicates born in October.



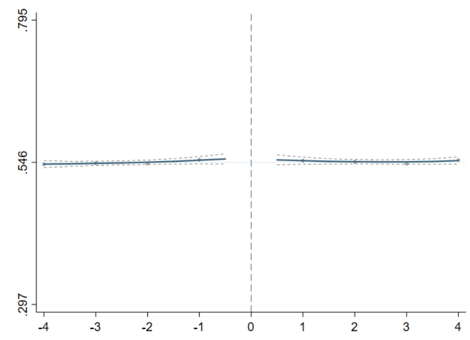
(a) Average Earnings (The year they turn 59)



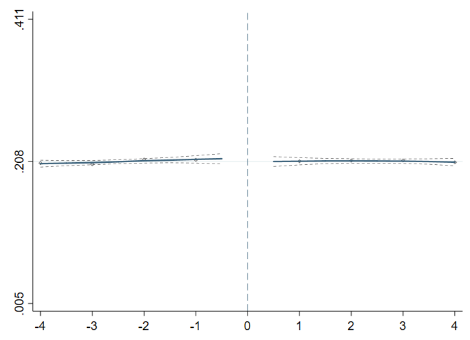
(b) Female



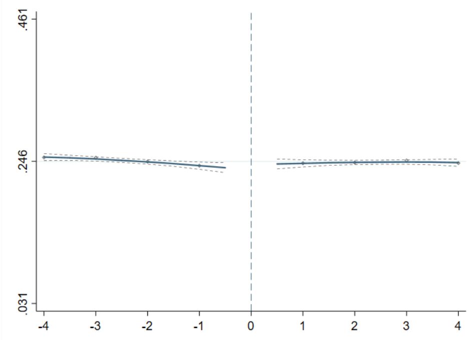
(c) Fraction Married



(d) High School Education



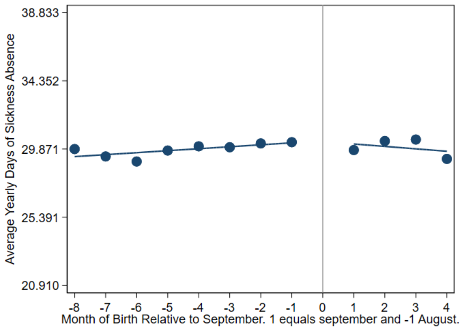
(e) No Attained Education



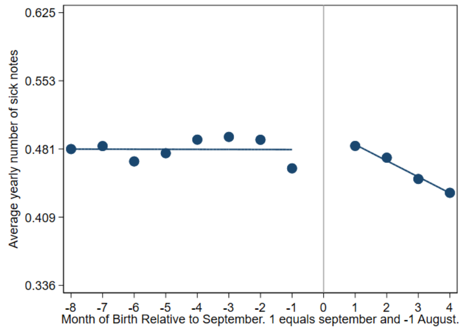
(f) Some College

Figure 2: Characteristics by month of birth

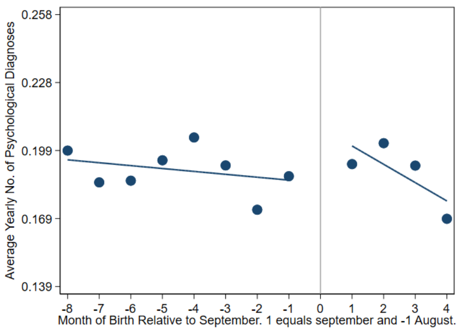
Notes: The figure displays individual characteristics by month of birth, relative to September. 4 months before the cut-off date indicates born in June whereas 1 month after the cut-off date indicates being born in September. In each graph, we plot the unrestricted monthly means and the estimated monthly means from a local linear regression applied to each side of the cut-off date. The scale of the y-axis is equal to ± 0.5 of the variables standard deviation.



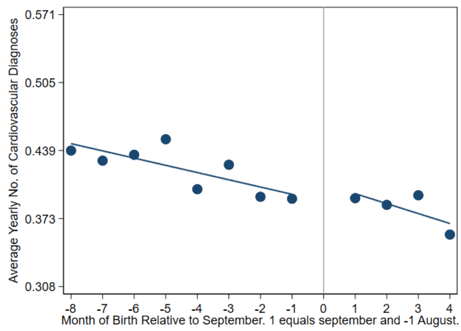
(a) Sickness Absence



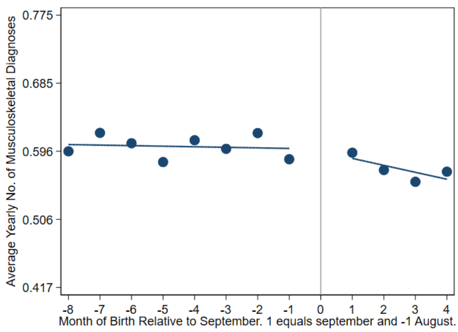
(b) Sicknotes



(c) No. of Psychological Diagnoses



(d) No. of Cardiovascular Diagnoses



(e) No. of Musculoskeletal Diagnoses

Figure 3: Effect of the increased entitlement to vacation

Notes: The graphs present the relationship between month of birth, diagnoses set by a GP, number of sick notes certified by a physician and sick leave. The x-axis displays age-in-months relative to the threshold for entitlement to an extra week of vacation. 4 months before the cut-off date indicates born in June whereas 1 month after the cut-off date indicates being born in September.

Table 1: Descriptive statistics

	Whole Sample	Entitled to 6 weeks	Entitled to 5 weeks	Male	Female
Characteristics					
Fixed Characteristics					
Year of birth – 1941-1948	1944.66	1944.70	1944.58	1944.64	1944.69
	[2.29]	[2.29]	[2.30]	[2.29]	[2.29]
Compulsory schooling only	0.21	0.20	0.21	0.20	0.22
High school as highest attained education	0.55	0.55	0.55	0.53	0.57
Some college or more as highest attained education	0.25	0.25	0.24	0.27	0.21
Other Controls					
Married	0.73	0.73	0.73	0.77	0.69
Average earnings (1000 NOK)**	355.57	355.50	355.73	426.33	272.80
	[216.17]	[216.81]	[214.81]	[187.98]	[108.77]
Outcomes					
Days of sick leave	29.87	29.83	29.97	25.16	35.39
	[69.59]	[69.46]	[69.87]	[64.67]	[74.56]
Number of sick notes*	1.15	1.14	1.16	0.89	1.45
	[2.40]	[2.38]	[2.43]	[2.10]	[2.66]
Number of GP visits for	0.99	1.01	0.97	1.14	0.82
cardiovascular diagnoses*	[2.73]	[2.76]	[2.68]	[3.05]	[2.30]
Number of GP visits for	0.46	0.45	0.45	0.33	0.60
psychological diagnoses *	[2.03]	[2.01]	[2.08]	[1.74]	[2.31]
Number of GP visits for	1.44	1.43	1.45	1.11	1.81
musculoskeletal diagnoses *	[3.14]	[3.12]	[3.19]	[2.74]	[3.51]
N	244,757	166,261	78,496	131,948	112,809

Notes: Standard deviation in brackets. This table displays descriptive statistics on the pooled sample from 2001 through 2008. All variables are measured the year when individuals turn 60 years of age.

$*$ For these outcomes, we have information for the period 2006–2008; 100.876 individuals of which 69.828 are born in January through August, 31.048 are born in September through December, 53.819 are men and 47.057 are women. $**$ Measured the year they turn 59 (year before treatment).

Table 2: Estimated impact of increased entitlement to vacation on the full sample

	(1)	(2)	(3)	(4)	(5)
	Days of sick leave	Number of sick notes	No. of Psychological Diagnoses	No. of Musculoskeletal Diagnoses	No. of cardiovascular Diagnoses
Panel (a): Controlling for quadratic polynomials in the running variable					
Treatment Effect	0.916	-0.016	-0.023	0.034	-0.015
	(0.828)	(0.045)	(0.037)	(0.058)	(0.051)
Observations	244757	100876	100876	100876	100876
Panel (b): local linear regression. h=1					
Treatment Effect	0.515	-0.065*	-0.016	-0.030	-0.007
	(0.693)	(0.038)	(0.031)	(0.049)	(0.043)
Observations	40787	16453	16453	16453	16453
Panel (c): local linear regression. h=2					
Treatment Effect	0.596	-0.122*	0.027	-0.091	0.003
	(1.212)	(0.065)	(0.054)	(0.086)	(0.072)
Observations	80887	32693	32693	32693	32693
Panel (d): local linear regression. h=3					
Treatment Effect	0.634	-0.074	-0.034	0.011	-0.033
	(0.877)	(0.047)	(0.039)	(0.062)	(0.053)
Observations	119873	48470	48470	48470	48470

Notes: Standard errors in parenthesis, clustered at the individual level. ***,**,* indicate significance at the 1%, 5% and 10% level respectively. Sample includes individuals in the final analysis sample. Columns denote the different outcomes under consideration. All outcomes are displayed based on a local linear regression with quadratic polynomials or difference in bandwidths h.

Table 3: Estimated impact of increased entitlement to vacation: Men

	(1)	(2)	(3)	(4)	(5)
	Days of sick leave	Number of sick notes	No. of Psychological Diagnoses	No. of Musculoskeletal Diagnoses	No. of cardiovascular Diagnoses
Panel (a): Controlling for quadratic polynomials in the running variable					
Treatment Effect	1.323	0.037	0.035	0.020	0.051
	(1.056)	(0.054)	(0.045)	(0.069)	(0.077)
Observations	131948	53819	53819	53819	53819
Panel (b): local linear regression. h=1					
Treatment Effect	1.415	-0.008	0.021	-0.021	0.053
	(0.879)	(0.046)	(0.038)	(0.058)	(0.064)
Observations	21920	8773	8773	8773	8773
Panel (c): local linear regression. h=2					
Treatment Effect	2.395	0.002	0.073	-0.102	0.068
	(1.545)	(0.079)	(0.067)	(0.101)	(0.109)
Observations	43533	17466	17466	17466	17466
Panel (d): local linear regression. h=3					
Treatment Effect	1.542	-0.018	0.017	0.018	0.024
	(1.117)	(0.058)	(0.048)	(0.072)	(0.080)
Observations	64594	25919	25919	25919	25919

Notes: Standard errors in parenthesis, clustered at the individual level. ***,**,* indicate significance at the 1%, 5% and 10% level respectively. Sample includes individuals in the final analysis sample. Columns denote the different outcomes under consideration. All outcomes are displayed based on a local linear regression with quadratic polynomials or difference in bandwidths h.

Table 4: Estimated impact of increased entitlement to vacation: Women

	(1)	(2)	(3)	(4)	(5)
	Days of sick leave	Number of sick notes	No. of Psychological Diagnoses	No. of Musculoskeletal Diagnoses	No. of cardiovascular Diagnoses
Panel (a): Controlling for quadratic polynomials in the running variable					
Treatment Effect	0.799	-0.058	-0.087	0.071	-0.085
	(1.310)	(0.073)	(0.061)	(0.098)	(0.065)
Observations	112809	47057	47057	47057	47057
Panel (b): local linear regression. h=1					
Treatment Effect	-0.445	-0.133**	-0.059	-0.042	-0.074
	(1.090)	(0.061)	(0.050)	(0.082)	(0.055)
Observations	18867	7680	7680	7680	7680
Panel (c): local linear regression. h=2					
Treatment Effect	-1.397	-0.272***	-0.029	-0.090	-0.065
	(1.902)	(0.105)	(0.086)	(0.141)	(0.093)
Observations	37354	15227	15227	15227	15227
Panel (d): local linear regression. h=3					
Treatment Effect	-0.423	-0.140*	-0.093	-0.001	-0.096
	(1.378)	(0.077)	(0.064)	(0.102)	(0.067)
Observations	55279	22551	22551	22551	22551

Notes: Standard errors in parenthesis, clustered at the individual level. ***,**,* indicate significance at the 1%, 5% and 10% level respectively. Sample includes individuals in the final analysis sample. Columns denote the different outcomes under consideration. All outcomes are displayed based on a local linear regression with quadratic polynomials or difference in bandwidths h.

Table 5: Estimated impact of increased vacation: Compulsory Schooling Only

	(1)	(2)	(3)	(4)	(5)
	Days of sick leave	Number of sick notes	No. of Psychological Diagnoses	No. of Musculoskeletal Diagnoses	No. of cardiovascular Diagnoses
Panel (a): Controlling for quadratic polynomials in the running variable					
Treatment Effect	2.563	0.265^{**}	0.005	0.144	-0.042
	(2.036)	(0.121)	(0.091)	(0.163)	(0.121)
Observations	50660	18484	18484	18484	18484
Panel (b): local linear regression. h=1					
Treatment Effect	1.881	0.201^{**}	0.032	0.076	0.018
	(1.693)	(0.101)	(0.073)	(0.137)	(0.100)
Observations	8526	3075	3075	3075	3075
Panel (c): local linear regression. h=2					
Treatment Effect	2.673	0.298^{*}	0.067	0.148	0.056
	(2.970)	(0.178)	(0.135)	(0.236)	(0.171)
Observations	16939	6166	6166	6166	6166
Panel (d): local linear regression. h=3					
Treatment Effect	2.781	0.263^{**}	-0.002	0.176	0.004
	(2.151)	(0.129)	(0.100)	(0.170)	(0.124)
Observations	24985	9024	9024	9024	9024

Notes: Standard errors in parenthesis, clustered at the individual level. ***,**,* indicate significance at the 1%, 5% and 10% level respectively. Sample includes individuals in the final analysis sample. Columns denote the different outcomes under consideration. All outcomes are displayed based on a local linear regression with quadratic polynomials or difference in bandwidths h.

Table 6: Estimated impact of increased entitlement to vacation: High School as highest attained education

	(1)	(2)	(3)	(4)	(5)
	Days of sick leave	Number of sick notes	No. of Psychological Diagnoses	No. of Musculoskeletal Diagnoses	No. of cardiovascular Diagnoses
Panel (a): Controlling for quadratic polynomials in the running variable					
Treatment Effect	-0.605	-0.139**	-0.016	-0.041	0.035
	(1.131)	(0.060)	(0.050)	(0.079)	(0.068)
Observations	133661	56956	56956	56956	56956
Panel (b): local linear regression. h=1					
Treatment Effect	-1.362	-0.194***	-0.034	-0.131**	0.002
	(0.946)	(0.050)	(0.042)	(0.067)	(0.056)
Observations	22419	9321	9321	9321	9321
Panel (c): local linear regression. h=2					
Treatment Effect	-1.870	-0.307***	0.008	-0.246**	-0.022
	(1.638)	(0.085)	(0.073)	(0.115)	(0.098)
Observations	44284	18405	18405	18405	18405
Panel (d): local linear regression. h=3					
Treatment Effect	-1.093	-0.221***	-0.046	-0.090	-0.037
	(1.188)	(0.062)	(0.053)	(0.082)	(0.071)
Observations	65496	27346	27346	27346	27346

Notes: Standard errors in parenthesis, clustered at the individual level. ***,**,* indicate significance at the 1%, 5% and 10% level respectively. Sample includes individuals in the final analysis sample. Columns denote the different outcomes under consideration. All outcomes are displayed based on a local linear regression with quadratic polynomials or difference in bandwidths h.

Table 7: Estimated impact of increased entitlement to vacation: Some College as highest attained education

	(1)	(2)	(3)	(4)	(5)
	Days of sick leave	Number of sick notes	No. of Psychological Diagnoses	No. of Musculoskeletal Diagnoses	No. of cardiovascular Diagnoses
Panel (a): Controlling for quadratic polynomials in the running variable					
Treatment Effect	2.843**	0.052	-0.064	0.120	-0.109
	(1.440)	(0.075)	(0.070)	(0.090)	(0.100)
Observations	60436	25436	25436	25436	25436
Panel (b): local linear regression. h=1					
Treatment Effect	3.404***	0.028	-0.010	0.123*	-0.049
	(1.178)	(0.062)	(0.057)	(0.074)	(0.087)
Observations	9842	4057	4057	4057	4057
Panel (c): local linear regression. h=2					
Treatment Effect	4.021*	-0.002	0.044	0.093	0.015
	(2.127)	(0.112)	(0.096)	(0.135)	(0.136)
Observations	19664	8122	8122	8122	8122
Panel (d): local linear regression. h=3					
Treatment Effect	2.200	-0.010	-0.033	0.092	-0.060
	(1.534)	(0.081)	(0.072)	(0.097)	(0.101)
Observations	29392	12100	12100	12100	12100

Notes: Standard errors in parenthesis, clustered at the individual level. ***,**,* indicate significance at the 1%, 5% and 10% level respectively. Sample includes individuals in the final analysis sample. Columns denote the different outcomes under consideration. All outcomes are displayed based on a local linear regression with quadratic polynomials or difference in bandwidths h.

Table 8: Estimated impact of increased entitlement to vacation: Sick leave above mean, at age 59

	(1)	(2)	(3)	(4)	(5)
	Days of sick leave	Number of sick notes	No. of Psychological Diagnoses	No. of Musculoskeletal Diagnoses	No. of cardiovascular Diagnoses
Panel (a): Controlling for quadratic polynomials in the running variable					
Treatment Effect	-6.895***	-0.289**	-0.146	0.050	-0.140
	(2.412)	(0.135)	(0.128)	(0.191)	(0.150)
Observations	57897	23261	23261	23261	23261
Panel (b): local linear regression. h=1					
Treatment Effect	-1.797	-0.304***	-0.076	-0.104	-0.175
	(1.976)	(0.112)	(0.105)	(0.160)	(0.124)
Observations	10211	4053	4053	4053	4053
Panel (c): local linear regression. h=2					
Treatment Effect	0.198	-0.438**	0.042	-0.181	-0.378*
	(3.457)	(0.195)	(0.181)	(0.275)	(0.205)
Observations	20229	8005	8005	8005	8005
Panel (d): local linear regression. h=3					
Treatment Effect	-1.041	-0.302**	-0.103	0.111	-0.284*
	(2.515)	(0.142)	(0.134)	(0.198)	(0.152)
Observations	29721	11814	11814	11814	11814

Notes: Standard errors in parenthesis, clustered at the individual level. ***,**,* indicate significance at the 1%, 5% and 10% level respectively. Sample includes individuals in the final analysis sample. Columns denote the different outcomes under consideration. All outcomes are displayed based on a local linear regression with quadratic polynomials or difference in bandwidths h.

Table 9: Estimated impact of increased entitlement to vacation on full sample. With controls

	(1)	(2)	(3)	(4)	(5)
	Days of sick leave	Number of sick notes	No. of Psychological Diagnoses	No. of Musculoskeletal Diagnoses	No. of cardiovascular Diagnoses
Panel (a): Controlling for quadratic polynomials in the running variable					
Treatment Effect	1.081	-0.009	-0.023	0.043	-0.012
	(0.830)	(0.045)	(0.037)	(0.059)	(0.051)
Observations	244757	100876	100876	100876	100876
Panel (b): local linear regression. h=1					
Treatment Effect	0.488	-0.067*	-0.017	-0.032	-0.007
	(0.689)	(0.037)	(0.031)	(0.049)	(0.043)
Observations	40787	16453	16453	16453	16453
Panel (c): local linear regression. h=2					
Treatment Effect	0.532	-0.126*	0.023	-0.096	0.006
	(1.206)	(0.065)	(0.054)	(0.085)	(0.072)
Observations	80887	32693	32693	32693	32693
Panel (d): local linear regression. h=3					
Treatment Effect	0.502	-0.080*	-0.036	0.003	-0.034
	(0.873)	(0.047)	(0.039)	(0.061)	(0.053)
Observations	119873	48470	48470	48470	48470

Notes: Results includes controls for eduation being married, and earnings measured the year he or she turned 59 years of age. Standard errors in parenthesis, clustered at the individual level. ***,**,* indicate significance at the 1%, 5% and 10% level respectively. Sample includes individuals in the final analysis sample. Columns denote the different outcomes under consideration. All outcomes are displayed based on a local linear regression with quadratic polynomials or difference in bandwidths h.

Chapter 4:

Spousal responses to health shocks. Effects on labour supply and social insurance

Spousal responses to health shocks: Effects on labour supply and social insurance*

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Abstract

In this paper, we study the effect of a fatal and non-fatal health shock on spouses' labour supply. Combining unique Norwegian administrative data with the unexpected timing of a health shock, we exploit the panel structure of our data by comparing individuals' whose spouses experience a health shock at different points in time. The results suggest that the employment and earnings of individuals' whose spouses experience a fatal health shock decrease. However, we find negligible effects on labour supply when a spouse experiences a non-fatal health shock. The results hold across different types of education but are more pronounced at younger ages and for widows. We show that the death of a spouse results in large transfers of liquid assets for widows and widowers, but that only widowers increase their uptake of social insurance. The results suggest that heterogeneity and measures of liquid assets help explain how individuals' react to a sudden and unexpected drop in household earnings.

Keywords: household labour supply, health shocks, within-household insurance, spouses' labour supply

JEL Codes: I10, J12, J20

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1 Introduction

The effect of serious illness on individuals' labour supply has long been of interest in the empirical literature.[1] Serious illness can have adverse economic consequences for the individuals experiencing the shock, and also for the entire household. In particular, sudden illness, or a fatal health outcome can potentially have devastating economic consequences within a household, be it loss of workdays or out-of-pocket expenditures on healthcare. A natural question to ask, then, is how the other (physically unaffected) spouse‘s of the household cope with such events.

In the present paper, we investigate the impact on earnings, employment, liquid assets and social insurance for individuals' whose spouses experienced a health shock. A common concern when studying how health interacts with labour market participation is that labour supply and health are jointly determined. We overcome this challenge by focusing on the timing of health-related events that are arguably hard to predict, regardless of any existing risk factors. To this end, we use data from the national patient register (NPR) from 2008–2014, and the cause of death (CAD) register from 1992–2014 to identify a particular set of health outcomes. First, we use the CAD register to identify individuals' whose spouse passed away due to ischemic heart diseases, stroke or transport accidents. Second, we use the NPR register to identify individuals' whose spouse were hospitalized because of an acute non-scheduled condition. We then link these outcomes to administrative data. The administrative data enables us to identify married couples[2] together with information on labour market participation, the uptake of social insurance and liquid assets.[3]

The econometric method in this paper follows a 'timing-of-event' design, similar to that used by Fadlon and Nielsen (2015). They use a quasi-experimental design where they exploit the random timing of a severe health event to construct a counterfactual. The authors construct a treatment group of individuals' whose spouses experienced either a fatal or a non-fatal health shock within a given period, and compare them to individuals' who experienced the same shock but a few years later. The key identifying assumption is that the timing of the event is uncorrelated with prior knowledge of individuals' health. Rather than focusing on a set of pre-specified ranges of treatment and controls, we follow Druedahl and Martinello (2016), who show that such an approach can be extended to an event study in which all available comparisons are included in an analysis. From the 'timing-of-event' design, we recover the outcomes of interest by comparing spousal earnings, where spouses experienced

[1] For example, Charles (2003); Lindeboom et al. (2016); Trevisan and Zantomio (2016).

[2] We are unfortunately unable to identify cohabitation or other forms of partnerships.

[3] Liquid assets is the sum of all liquid assets and financial investments, such as stocks, bonds and exchange-traded funds. It also includes direct transfers, such as survivor pensions or life insurance.

the same shock at different points in time. Thus, we estimate the outcome at a given year relative to a base year.

Our results show a clear difference between fatal and non-fatal health shocks. First, we find that individuals' whose spouses died experienced a large and negative change in earnings and employment. Widows' earnings decreased by 8% the second year after the death of a spouse. We find no effect on earnings for widowers, but widows and widowers experience a drop in labour market participation in the years after the death of a spouse. Turning to non-fatal health shocks, we find no effect on any of the outcomes under consideration. A non-fatal health shock may not inflict the same type of economic consequences as a fatal health shock, even if we find evidence of reduced employment for individuals' who experienced the non-fatal health shock.

To get a fuller picture of a person's response to the spousal health shock, we also assess whether there are any changes in the uptake of social insurance or liquid assets, as these outcomes may have an impact on both employment and earnings. While we do not distinguish between the different social support programs, we find that widowers increase their dependency on social insurance in the years after the death of a spouse. Furthermore, we find large flows of liquid assets in the years after the death of a spouse. We are unfortunately unable to disentangle the different components of liquid assets, but they do include both transfers as a result of a will and life insurance. Splitting the sample by age at the time when the spousal shock occurred, and by level of education, reveals that the effect is more pronounced for individuals' who became widows or widowers at a relatively young age. We find no clear pattern between levels of education.

Individuals' responses to a spouse's ill health (of women, mostly) have previously been studied by, for example, Parsons (1977); Berger (1983); Berger and Fleisher (1984) and Siegel (2006), but the effects on employment range from a reduction to a very large increase. These authors demonstrate that wives tend to increase labour supply when their husbands experience a deterioration in health, whereas husbands' responses to their wives falling ill generally leads to no change at all. Siegel (2006) shows that these results heavily depend on two important features: They are casespecific in terms of the health-related outcomes and whether the individuals' possess health insurance.[4] There is also ample evidence from the epidemiological literature of a so-called widowhood effect, manifested by increased mortality for widows or widowers in the weeks after the death of a spouse (see e.g. Brenn and Ytterstad (2016)).

Recently, some studies have tried to tackle endogeneity in health by including objective

[4]Whether self-reported measures of health lead to justification bias is a long-standing question (see e.g. Deaton (2012)).

measures from administrative records. García-Gómez et al. (2013) use acute and non-fatal hospitalisations taken from administrative records in the Netherlands and argue that the nature of unscheduled hospitalisations makes it more likely that they are exogenous to the labour market participation. Studying the effect of ill health on spouses' labour supply, Jeon and Pohl (2017) use cancer diagnoses as an unanticipated deterioration in health. They argue that such a condition makes it more unlikely to observe an adjustment in labour market supply before the uncovering of the diagnosis.

García-Gómez et al. (2013) find a reduction in the probability of being employed two years after the shock, of around two percentage points, for individuals' whose spouse experienced an acute hospitalisation. They also find a reduction in earnings of around two percentage points. Interestingly, the estimated effect is only significant for males whose female spouses experienced a health shock. In contrast, Jeon and Pohl (2017), find negative effects on labour market participation that are similar between genders. Upon being diagnosed with cancer, spouses reduce labour market participation by 2.4%, and this effect persists for around four years, after which it is likely that individuals' either have recovered or passed away. Jeon and Pohl (2017) also argue that the use of hospitalisations as a determinant of a shock, as in García-Gómez (2011), is problematic because hospitalisations may occur following a longer period of deteriorating health. They argue that receiving a cancer diagnosis acts as an unexpected shock and is a major life event. While the latter certainly is true, it is not given that receiving a cancer diagnosis alleviates the concerns about a longer period of deteriorating health. Deteriorating health because of a sudden loss of weight in conjunction with loss of breath while carrying out simple tasks are for example known symptoms of cancer.[5]

As explained above, the econometric method used by García-Gómez et al. (2013) and Jeon and Pohl (2017) is matching combined with differences-in-differences. This method might be problematic, as any type of matching relies on the conditional independence assumption, which may create biased estimates if the health shock is partly determined by selection of individuals' to be treated (Smith and Todd, 2005).

Exploiting the rich nature of Danish administrative data, Fadlon and Nielsen (2015) find that both fatal and non-fatal health shocks affect the overall household earnings but spouses respond differently depending on the severity of the shock. They find that individuals' whose spouses passed away because of an unanticipated health shock, increase their labour market participation by around 7.6% on average and their earnings by 6.8% on average by the fourth year after the shock. Older widows mainly drive this effect. In addition, however, the

[5] Several studies on the symptoms associated with advanced cancer exist in the epidemiological literature (e.g. Walsh et al. (2000)).

estimated effect is present only for widows or widowers who had substantially lower earnings than their spouse. The authors find modest, yet significant negative effects on spouses' labour supply when individuals' experience a non-fatal health shock.

A recent paper by Dobkin et al. (2018) in the U.S. context shows individuals' financial outcomes following a hospital admission, and, as in our paper, they use an event study methodology. They find that a large and significant impact of hospital admissions on unpaid bills, especially for people with no health insurance. Moreover, they find no evidence of a spousal labour supply response to a hospital admission.

From a theoretical standpoint, several interrelated mechanisms are likely to affect the outcome. One strand in the literature underlines the importance of within-household insurance (or an earnings effect) in the context of unemployment, referred to as the added-worker effect (see e.g. Ashenfelter (1980); Lundberg (1985); Cullen and Gruber (2000)).[6] In order to compensate a loss in earnings due to a spouse losing his or her job, individuals' contribute to the household earnings by increasing their own labour supply. However, the presence of such an effect is not clear in the event of spouses experiencing a fatal or non-fatal health shock. Coile (2004) finds no evidence of the added-worker effect in a household when one of the members experienced a non-fatal health shock. According to Heckman and MaCurdy (1980), the added-worker effect should be small, unless the earnings loss is large relative to lifetime earnings.

Another perspective is the role of caregiving within the family, where spouses synchronize their non-market time (Coile, 2004). Healthy spouses might value their non-market time in a different way than people with worse health and thus reduce their labour supply to provide care within the household. Furthermore, Coile (2004) highlights the interdependence in preferences for work and leisure between spouses. In the event of a fatal health shock, it is reasonable to assume that individuals' may also experience a disutility for work as a result of grief, which potentially can crowd out the added-worker effect.[7]

Liquidity constraints may also have an impact on the response. How the unaffected spouse respond to a spousal health shock may depend on social safety nets and social care services, which is important because Norway has one of the most generous levels of social insurance (see e.g. Scruggs 2006 for a comparison within OECD countries). The social security programs may therefore crowd out the potential need for the unaffected spouse to increase the labour supply to compensate for a drop in earnings. In the case of a non-fatal health shock, and as highlighted by Coile (2004), there may be complementaries in leisure

[6] One crux with referring to the U.S. setting is that the out-of-pocket expenditure as a result of changes in health is related to, among other things, institutional differences and private health insurance.

[7] van den Berg et al. (2017) show how grief negatively affects labour market outcomes in the event of loosing a child.

among spouses.[8] This complementarity may crowd out any potential increase in employment on both the intensive and extensive margin. The earnings effect, the caregiving hypothesis and leisure as a complement to poor health have offsetting effects, and it is not clear which of these effects prevail. We therefore test the individuals' responses to a spouse's health shock empirically.

This paper proceeds as follows: Section 2 describes institutional features, how we define the relevant health shocks and the descriptive analysis. In section 3, we introduce the empirical framework and then detail: how a sudden and unexpected fatal health shock and a sudden but non-fatal health shock affect labour supply and liquid assets. Section 4 concludes.

2 Institutions, Data and Descriptive Statistics

2.1 Institutional Background

Norway has a comprehensive public system of social insurance, with entitlement often depending on employment status. The most relevant programs for our paper are sick pay and temporary and permanent disability insurance. The replacement rate for sick pay is 100% from day one of the sick leave, but it is limited to one year. In addition to sick pay, individuals' may be entitled to either temporary or permanent disability insurance (DI) with a replacement rate of around 60%. A physician must certify entitlement to either sick pay or temporary or permanent DI, apart from the first three (eight)[9] days of a spell of absence due to sickness. Individuals who quit or lose their job are entitled to unemployment benefits, up to a maximum of two years. The replacement rate is 62.4% (capped at high earnings).

Instead of focusing on a particular type of social insurance, we combine temporary and permanent DI along with unemployment insurance and construct an indicator for whether one receives transfers from any of these programs. Bratsberg et al. (2013) show that it is difficult to distinguish between the programs, and Fevang et al. (2004) identify large flows between the different programs.

The unaffected spouse may be entitled to leave to care for their sick spouse. In the terminal stages of a person's life, individuals are entitled to a maximum of 60 days' leave, whereas individuals' are entitled to a maximum of 10 days' leave to care for their sick spouse.[10] Unfortunately, we do not have information about this specific type of leave, as it

[8]Complementaries in leisure follows from non-separable utility in that a person values leisure more if shared with a spouse.

[9]Self-certification can be up to 8 days, depending on whether the workplace having signed the so-called inclusive workplace agreement. This agreement did relax the requirement of self-certification from 3 to 8 days.

[10]Working Environment Act §12-10.

follows other rules than those covering sickness absence.[11]

2.2 Definition of a Health Shock

We form our analysis around two different health shocks, non-fatal and fatal. First, we construct the measure of a non-fatal health shock using data from the NPR, which contains information on all contacts with somatic hospitals from 2008–2014, coupled with conditions classified according to the World Health Organization International Statistical Classification of Diseases, ICD-10 (World Health Organization (1992)). In the data from the NPR, all admissions are classified as planned or acute and coded in accordance with ICD-10. Acute admission consists of all unscheduled hospital admissions, where a patient is in dire need of treatment.

We follow Jeon and Pohl (2017), García-Gómez (2011), and Fadlon and Nielsen (2015), and define a non-fatal health shock based on an acute and unscheduled hospitalisation. We also follow Fadlon and Nielsen (2015), Jones et al. (2016), Trevisan and Zantomio (2016) and McClellan (1998) and focus on a particular subset of shocks that constitute major health events. These are stroke, acute myocardial infarction (heart attack) and congestive heart failure.[12] The conditions in question are the leading causes of death in most developed countries (World Health Organization, 2015).[13,14] To further reduce the possibility that the hospitilzation is a result of a long-term deterioration in health, we include individuals who did not have an unscheduled hospitalisation in the preceding 365 days, although this restriction does not ensure that we study the first onset of a non-fatal health shock. This means that we include all acute hospitalisations between 2009 and 2014 as a result of either a stroke, myocardial infarction or congestive heart failure.

Second, we construct a measure of a fatal health shock, based on information on all recorded deaths from 1992–2014 from the CAD register. Each death is coded in accordance with the European shortlist for CAD, which is a standardized categorization based on ICD codes. One limitation with the CAD register is that people can die for reasons not associated with a particular deterioration in health. It is therefore not clear if we should include all recorded deaths between 1992 and 2014 or limit the focus to a subset of causes. In our

[11] People may be entitled to social security compensation to care for persons who are in the terminal stages of their life. No such compensation exists after the death of a spouse.

[12] Acute myocardial infarction and congestive heart failure are forms of cardiovascular disease.

[13] The ICD-10 codes for stroke are: I60–I69; I20-I25 for myocardial infarction; and I438, I500 and I501 for congestive heart failure.

[14] Acute myocardial infarction has been used in other economic literature to study variations in the utilisation of medical procedures (Chandra and Staiger, 2007), and whether more spending is associated with better outcomes in health care (Doyle, 2011), and how technology may result in better productivity growth in health care (Skinner and Staiger, 2015).

analysis, we therefore define a sudden death caused by either ischemic heart diseases, other heart diseases, cerebrovascular diseases, or transport accidents.[15] We are unfortunately unable to check if people have a known history of heart disease, but we are confident that limiting the attention to a subset of causes increases the chances that the death indeed is sudden and unexpected.

While we cannot rule out that these events occur as a result of a chronic illness or long-lasting deterioration in health, and although not perfect, we believe that restricting the sample to a particular set of serious outcomes does at least reduce the possibility. In the next section, we elaborate on how these conditions relate to the sample selection.

2.3 Data and Sample Selection

2.3.1 Data

In addition to the data on hospitalisation and mortality, we have several data sources from Statistics Norway linked through a unique encrypted identifier. The registers contain demographic information, such as month of birth, marital status and educational attainment, and various longitudinal measures of earnings and liabilities. Important for our analysis, data on legal marriage are taken directly from population registers, from which we can identify all married couples in Norway.

Earnings data are in annual amounts and are verified by the Norwegian Tax Authorities. Annual earnings include information on all Norwegian residents from 1993–2014 and consist of wages and self-employed earnings, but disregard capital earnings and pensions. We also include an annual measure of financial wealth, reported by all third-party financial institutions in Norway. This measure is the sum of all liquid assets and financial investments, such as stocks, bonds and exchange-traded funds, plus direct transfers, such as survivor pensions or life insurance. In other words, liquid assets are assets that can be converted into cash quickly. There is no attrition from the original sample due to non-response or non-consent in any of these measures, and all annual amounts are without any top or bottom-coding.

2.3.2 Sample Selection

We base the empirical analysis on two complementary datasets. First, we identify all acute hospitalisations based on the conditions outlined in the previous section. Likewise, we identify all recorded deaths corresponding to the causes of death discussed in the previous section.

[15]The codes in the CAD register for heart diseases are 34–36, whereas transport accidents are code 60. The corresponding ICD-10 codes for heart diseases: I20–I25, I30–I33, I39–I52 and I60–I69, whereas the codes for transport accidents are V01–V99.

That way, we have two subsamples that consist of individuals whose spouses experienced fatal or non-fatal health shocks. This allows for a more homogeneous way of comparing individuals, compared to the inclusion of individuals' whose spouses did not experience any health shock. This approach closely follows the samples used in Fadlon and Nielsen (2015), who study the effect of the same events as in the present paper.

Table 2 summarises how we construct our samples. First, as displayed in Panel A, we identify all deaths recorded in Norway between 1992 and 2014. This leaves 461,040 men and 478,469 woman. Of these, we keep all individuals' with a recorded cause of death corresponding to either ischemic or other heart diseases, cerabrovascular diseases or transport accidents. This leaves 154,578 men and 164,385 woman. Of these, we identify individuals' who at the time of death were aged between 30 and 70 and were married. We also restrict the spouse to whom these were married to be at age 30-70 at the time of their spouse's death. This leaves an analysis sample of 5,198 men whose female spouses died and 20,585 woman whose male spouses died.[16]

Likewise, we identify all individuals' who experienced an acute hospitalisation between 2009 and 2014. This leaves 900,416 woman and 720,995 men. Of these, we restrict the sample to consist of individuals' who had at least one overnight stay and who were not admitted in the last 365 days. This leaves a sample of 774,012 women and 600,428 men. Of these, we include only those diagnoses corresponding to either a stroke or ischemic or cardiovascular heart disease. This leaves 66,112 men and 41,558 woman. Of these, we identify individuals aged 30–70 at the time of hospitalisation. We also restrict the unaffected spouse to be aged 30–70 the year their spouse experienced a non-fatal health shock. This leaves an analysis sample of 10,621 men whose female spouses experienced a non-fatal health shock and 31,271 women whose male spouse experienced the non-fatal health shock.

Two apparent issues from Table 2 warrant some discussion. In Panel A, the share of individuals who died as a result of the conditions of interest accounts for approximately one third of total deaths. Yet, it is clear that most of these deaths occurred at later ages. Second, in Panel B, the health events we focus on accounts for around one twelfth of all hospitalisations with at least one overnight stay, with no hospitalisation having occurred in the preceding 365 days. However, it is not clear from the literature whether we should include all sets of conditions in the estimation. Restricting the sample to a limited set of conditions that may potentially be major life events allows for an easier interpretation compared to including all different types of conditions. Therefore, we follow the literature discussed in Section 2.2 and impose these restrictions, which also allows us to put our finding in the context of previous results.

[16]Very few people are married below the age of 30, and most people have retired by the age of 70.

2.3.3 Outcomes

The goal of this paper is to study the effect of individuals whose spouses experienced a health shock on various outcome variables related to labour market participation and the uptake of social insurance. To that end, we use two measures of labour market participation. First, we defined employment from a proxy based on earnings equal to or above the threshold to accumulate pension points, also known as one price amount (2016 NOK 92,000 $\approx$ 2016-USD 10,600). Second, we use log earnings to capture the relative changes in labour supply through earnings.

Note that we do not observe any direct transfers from insurance or wills, but we use a measure of liquid assets that contains all these types of transfers. Apart from a will or insurance, these can also be private cash transfers or money earned through the sale of a house. Although not perfect, we believe that our measure of liquid assets sheds additional light on our findings based on the earnings and social insurance data.

2.4 Summary Statistics

Table 1 provides mean values for the two samples in our empirical analysis. More than two-thirds of the subsample, displayed in Panel A, had a spouse pass away due to a heart-related disease, with the prevalence markedly higher for males than females. It is not surprising, then, that heart-related diseases are the most common non-fatal health shock. Stroke is more common among females than males, while the other causes of death and non-fatal health shocks occur at similar rates for both genders. The the average earnings, liquid assets and earnings displayed in Table 1 include zeroes.

Table 1 shows that individuals whose spouses experienced a health shock are 57.6 years old when the shock occurs. Around 60% are employed in both samples, yet the non-employed are kept in the sample to investigate whether non-employment changes their employment decision in the event of their spouse experiencing a health shock.[17] Furthermore, women are more than three times as likely as men to have a spouse experience any of the shocks under consideration, as men. The average age at which a spouse experienced a shock is slightly higher for men than women. Earnings and liquid assets are deflated to 2006 prices and are displayed in NOK 1000, with NOK 10 $\approx$ EUR 1 in 2018.

[17] Restricting the sample to include only employed individuals' at the time of the shock would also be a case of bad controls, as one of the outcomes is employment.

3 Specification and Results

3.1 Empirical Specification

The aim of this paper is to estimate the effect on individuals' labour supply following a spouse's fatal or non-fatal health shock. More specifically, we focus on sudden and unexpected deaths and acute and unscheduled hospitalisations. As discussed in the previous section, we think of these health shocks as unanticipated in the sense that the exact timing of the shock is difficult to predict. Fadlon and Nielsen (2015) use an empirical strategy that exploits the randomness in timing of spouses experiencing an acute health shock. By exploiting that the exact timing of an event (the health shock) is difficult to predict and is stochastic, they use a differences-in-differences (DiD) approach where they define specific treatment and control groups.

The empirical method used by Fadlon and Nielsen (2015) is summarized in Figure 1, where the first group consists of women whose male spouses died because of a fatal health shock in 1998. The second group consists of women whose male spouses died because of a fatal health shock in 2003, but this group now acts as a natural control group to individuals whose spouses died in 1998. In this way, we can identify the effect of a fatal health shock in 1998 on labour earnings in 1999 through 2002, after which the effect is contaminated by the control group that experienced the same shock.

One potential drawback with the method proposed and applied by Fadlon and Nielsen (2015) is that it does not extend beyond the point at which the control group experiences the same shock. This is particularly unfortunate in the setting of a non-fatal health shock, because our data are limited to shocks that occurred between 2009 and 2014. There seems to be no obvious reason why one should not use an unbalanced panel instead. Druedahl and Martinello (2016) show that combining all events is nearly identical to the balanced DiD setup suggested by Fadlon and Nielsen (2015). Therefore, in our empirical analysis we follow Druedahl and Martinello (2016) and use an empirical strategy where we include individuals' who differ only in the timing of a spouse's health shock.

Our empirical analysis thus consists of using an event study design where we take advantage of the within-person variation over time and the panel structure of our data. Formally, we estimate the following reduced-form regression equation:

$$Y_{i,t} = \alpha + \sum_{i=k} \beta_k x_{i(t+k)} + \eta_{c,t} + \mu_i + \varepsilon_{i,t}, \tag{1}$$

where $Y_{i,t}$ is the outcome of interest for individual i in year t. Our interest lies in the event time indicator $x_{i(t+k)}$, which is a dummy for each year before and after the year that a spouse experienced a health shock. In the specification, we use period $t-1$ as the base period by dropping $x_{i(t-1)}$ from the regression. This implies that all estimated effects in what follows are relative to the year before a spouse experienced a health shock.

Equation 1 implies that the control group consists of individuals whose spouses experienced a health shock but in a subsequent year. We plot all coefficients to investigate the identifying assumption, namely, that there can be no systematic relationship between the year in which the shock occurred and the outcome of interest. We include individual fixed effects, μ_i, and year-by-cohort fixed effects, $\eta_{c,t}$. The former controls for time-invariant factors at the individual level, such as labour preferences, and/or underlying ability. As age correlates with health, adding year-by-cohort fixed effects rules out that we estimate the effect of getting older. We estimate the model using all pre- and post-shock years, but we show results only from periods $k \in \{-5, 5\}$. Given the structure of our data, moving further away from these periods reduces the number of comparisons, thus resulting in estimates that are too imprecise for any meaningful interpretation.[18]

Two apparent issues may potentially invalidate the empirical approach of the present paper. First, if the shock occurred preceding a long-lasting deterioration in health, the timing of the event analysis would be invalid. Second, job loss has previously been found to correlate with bad health. Both of these would be problematic for the design, but the key idea behind the design is that the timing of the event is difficult to predict, regardless of the presence of any risk factors. In presenting the results, we therefore visually assess the outcomes, which also guides the discussion of the validity of the design.

3.2 Results: Fatal Health Shock

We now turn to the main regression results and the effect on individuals' whose spouses experienced a fatal health shock. In Table 3, we report the main regression results, and we plot the outcomes in Figure 2. We find that the death of a spouse significantly reduces the earnings for widows, but not for widowers. There is no change in earnings up to the year in which the shock happened. One year after the death of a spouse, widows' labour earnings are reduced by approximately 8.0% on average. The impact on labour earnings is stable through the fifth year after the death of a spouse. However, we find no effect for widowers, and this is stable for the years under consideration as well. These results suggest a large and negative effect for women whose spouses died because of a fatal health shock. For example, by the fifth year after the death of a spouse, widows earn, on average, 8.5% less relative to

[18] The empirical strategy relies on identifying assumptions similar to those used to identify the effect of job displacement (Hilger (2016); Huttunen and Kellokumpu (2016)), arrests in employment and earnings (Grogger (1995)) and inheritances in labour market decisions (Holtz-Eakin et al. (1993)).

the year before the shock occurred. For widowers, however, there does not seem to be any obvious change in earnings.

Next, we consider labour market participation as defined by earnings equal to or above one basis amount. The estimates presented in Figure 2(c) are consistent with the pattern documented above. One year after the death of a spouse, widows' labour market participation drops by 1.8%. While we find no impact on average earnings for widowers, employment drops by around 3.3% by the second year after the death of a spouse. For both genders, our results indicate that the death of a spouse leads to a persistent drop in labour supply on the extensive margin. Taken together, we find no evidence of the added-worker effect, which is different to the evidence in Fadlon and Nielsen (2015). There are some indications in the literature that the death of a spouse leads to bereavement that in turn affects both physical and mental health, all of which is coupled with labour market supply (see e.g. Stroebe et al. (2007)).

As briefly discussed in the Section 2, widows and widowers may be entitled to a minimum amount of an inheritance through a will, as well as possible survivor pensions and cash transfers through private life insurance. Although not explicitly stated in our data, some of these transfers are picked up by the measure covering all liquid assets as reported by all third-party financial institutions in Norway.[19] Figures 2(e) and 2(f) along with columns 5 and 6 in Table 3 report the estimated effects on liquid assets (in NOK 1000).

In the first two years after the death of a spouse, there is a significant increase in liquid assets for both widows and widowers, and this effect persists for several years. Remember that the average earnings measured before the death of a spouse was NOK 208.668 and NOK 142.349 for men and women, respectively. The estimated effects, therefore, are quite substantial in relative terms. For widows, the estimated change in liquid assets is approximately NOK 332.000 by the second year after the death of a spouse, which is more than twice the amount of the average earnings as measured the year before the death of a spouse. For widowers, the effect is not as pronounced, but still statistically significant and approximately NOK 112.00 by the third year after the death of their spouse.

Some aspects of the observed effect are attributed to observable liquid assets in the years before the death of a spouse, yet most of the changes are difficult to pick up in our data. Therefore, it is unfortunately not clear if the observed effect on changes in liquid assets is a result of changes in homeowner consumption (i.e. selling a house and moving into an apartment), transfers because of survivor pensions or an inheritance.

Some supportive evidence of these findings exists: Sevak et al. (2003), Venti and Wise (2004)) and Poterba et al. (2011) find that widows tap into their savings and home equity when a spouse dies. If it is so that the increase in liquid assets is a result of an inheritance

[19] Recall that liquid assets include bank deposits, stocks, mutual funds and other securities.

from their late spouse, evidence exists that an inheritance can lead to a decrease in labour market participation (e.g. Holtz-Eakin et al. (1993)). However, Druedahl and Martinello (2016) find no effect of an inheritance on employment following a parent's death. Even if we had data on housing consumption, the literature on how to measure changes in housing consumption and furthermore track these changes to a possible increase in liquid assets is not very clear.[20] Nevertheless, the changes in liquid assets give a clear indication that individuals' tap into these holdings to counter some of the economic loss following the death of a spouse.

Finally, Figures 2(g) and 2(h) and columns 7 and 8 in Table 3 presents the estimated effect on social insurance, following a spouse's death. For widowers, we find no relationship between the death of a spouse and the uptake of social insurance. The pattern of the coefficients shows that there might even be a small decrease in the uptake of social insurance, but this is merely suggestive. For widows, however, the results indicate an increase in the uptake of social insurance in the years after the death of a spouse. Column 8 shows that we find no effect in the years before the death of a spouse. By the first year after the death of a spouse, the uptake of social insurance increases by approximately NOK 8000. Although the magnitude of the impact on uptake of social insurance is somewhat marginal, it still supports our previous findings in that labour market participation is worse for widows following the death of a spouse than for widowers.

3.2.1 Heterogeneity by socioeconomic status and liquid assets

We explore whether and to what extent the results presented above mask any important heterogeneity. Specifically, we focus on differences in socioeconomic status as proxied by education to investigate whether education coupled with age[21] have any effect on the outcomes of interest. We report the results in Appendix A on outcomes of interest based on running a regression on Equation 1.

Tables A5 through A8 present the results from the estimation on earnings and employment. The results suggests that the effect of the fatal health shock is larger at ages 30-44 than for people aged 45–60 and 61–70. In particular, there is no evidence of an impact on earnings nor employment for females with higher education who lost their spouses at age 45–70. There is a decline in employment and earnings for female whose spouse passed away (Tables A1 and A3), which also explains some of the main estimates discussed above.

[20]For instance, Sinai and Souleles (2005) model changes in housing consumption as a housing service flow gained from living in the house which equals the rent saved. Based on our measures of liquid assets, this is unfortunately not possible to disentangle.

[21]We follow Fadlon and Nielsen (2015) and split the sample by age when the individuals' spouses experienced the shock.

The estimates displayed in Tables A5 through A8 also show that there are large flows of liquid assets in the years after the death of a spouse. This holds regardless of gender, age and education, except for more highly educated individuals aged between 61 and 70 when their spouses passed away. The same results emerge when considering social insurance for widows, whereas we merely find suggestive evidence of an increase in social insurance for younger widowers with low education. However, the standard errors are too high to draw any firm conclusion for this group.[22]

Either way, Appendix A shows that the death of a spouse leads to a drop in earnings and employment. Even if not directly testable, we believe that it is reasonable to assume that the increase in any form of social insurance and liquid assets explains some of the observed effect on labour market participation. We find some increase in social security dependency, but the magnitude of these estimates is quite different compared to the effect of liquid assets. It is still reasonable to expect that many survivors are subject to a difficult time in the subsequent years after the death of a spouse, something documented by Stroebe et al. (2007). We also find that there is important heterogeneity in the effect of a spouse's death on labour market outcomes. The effect of a spouse's death on labour outcomes seems to be more salient for widows than for widowers, and we find that differences in age and socioeconomic status are important.

3.3 Results: Non-fatal Health Shock

Next, we investigate whether the relevant outcomes are affected when an individual's spouse experienced a non-fatal health shock. As argued in Section 2, there may be different mechanisms in place when considering non-fatal health shocks compared with fatal health shocks. Institutional features in Norway may rule out big drops in earnings the first year after the health shock, as the replacement rate for sick leave is 100%. Even so, the healthy spouse might reduce his or her labour supply because of family caregiving.[23] This might be true even in the absence of complementarities in leisure between the spouses.

Figure 3 presents the results of the estimates for periods $k \in \{-5, 5\}$, with the corresponding results presented in Table 4. Figures 3(a) and 3(b) show that the effect on earnings and labour market participation is negligible when a spouse experienced a non-fatal health shock. This is true for men and women. There are no statistically significant differences in

[22] Admittedly, some of the outcomes displays significant effects before the shock occurred. However, the magnitude of these are relatively small and, as is the case in Column 8 in Table 3 and Column 5 in Table A1, we do observe significant effects before the shock but no significant effect after the shock. Because of that we do not regard these effects as a threat to the validity of the design.

[23] Some evidence exists in the psychology literature that informal caregiving is correlated with depressive symptoms. Pinquart and Sörensen (2007) provide a meta-analysis on this subject.

any of the periods under consideration, except for the fifth year after a male experienced a health shock.[24]

Even though some movement occurs in Figure 3(a) and 3(b), the coefficients are too imprecise to make any firm conclusion. These results stand in contrast to evidence provided by García-Gómez et al. (2013) and Jeon and Pohl (2017), whose general finding is that individuals' reduce their labour market supply when a spouse experienced a non-fatal health shock or receives a cancer diagnosis. Jeon and Pohl (2017) find that even if households are sufficiently self-insured, caregiving within households might affect the labour supply. However, we find no evidence of individuals reducing their labour supply.

As discussed in the previous section, two important mechanisms through which a health shock may affect individuals' labour supply are changes in liquid assets and social security. In Figures 3(e) and 3(f) and columns 5 through 8 in Table 4, we examine how these outcomes might be affected by a spouse who experienced a non-fatal health shock. We find no statistically significant effect on changes in liquid assets or in the uptake of social insurance. These latter results are not surprising given that there seems to be no change in earnings or employment. One possible explanation may be that individuals' who suffered from the acute health condition do not adjust their labour supply in the years after the event. Therefore, we examine how individuals' labour supply is affected, given that they experienced a non-fatal health shock. The estimated coefficients are reported in Appendix B and displayed in Figure 4.

While no relationship is found in the years before the non-fatal health shock, in the years after the shock, labour market participation declines by approximately 3% for women. For men, the picture is similar with a decline of approximately 3% as well. For men and women, the drop in labour market participation is persistent for the remainder of the period. This is consistent with the findings of García-Gómez et al. (2013), but does not seem to have a spillover effect on the unaffected spouse. Our findings are in line with those of Dobkin et al. (2018), who find no effect on spousal labour supply as a response to a spouse's hospital admission.

Taken together, there seems to be no effect on any of the outcomes under consideration regardless of age or socioeconomic status. There is no immediate response regardless of outcome, and while we uncover some short-term effects in the subsequent years, these are too imprecisely estimated for any meaningful interpretation. As discussed above, the findings are not surprising given that the experience of a non-fatal health shock does not inflict the same shock on household earnings as a fatal health shock. In addition, the social security programs in Norway provide a generous safety net. One drawback with the way we analyse

[24]Splitting the analysis by the type of health shock does not alter the conclusion (results not shown).

the data is that we look at outcomes measured as an average in a year. These outcomes may not be well suited to pick up any transitory movements occurring in the immediate aftermath of the non-fatal health shock. However, a slight increase in number of sick days around the date of the shock should be expected, as the unaffected spouse are entitled to a maximum of 10 days leave to care for their spouse.[25]

Overall, these results suggest that individuals' earnings and labour market participation do not change when a spouse suffers an acute but non-fatal health shock. The same is true when considering differences in education and age when the spouse experienced a non-fatal health shock. Moreover, apart from less educated women women aged 40 through 61 at the time of the spousal health shock, we find no evidence that these shocks affect individuals' uptake of social insurance.

4 Discussion and Conclusion

With age comes the inevitable increase in the risk of acute and sometimes fatal health conditions. These conditions inherently pose some fiscal challenges to within-household finances. Therefore, in this paper we have investigated how individuals' are affected when their spouses experience a fatal or non-fatal health shock. Individuals are followed up to five years before their respective spouses experienced a shock and up to five years after the shock. Unlike previous studies, we condition our study on individuals whose spouses experienced any of the shocks in question and compare the outcome with the outcome for individuals whose spouses experienced the same shock but at different points in time. This allows for a more homogeneous comparison of the outcomes, compared to methods applied by García-Gómez et al. (2013)) and Jeon and Pohl (2017), who combine a matching technique with the DiD method.

The results show that individuals' whose spouses experienced a fatal health shock experienced a drop in both earnings and employment. Widowers' earnings decrease by around 8%, and this effect is persistent. For widows, however, the effect on earnings is not statistically distinguishable from zero. On average, widows and widowers reduce their labour market participation by 2% and 3%, respectively, and the effect is persistent. To get a fuller picture of the financial impact for the unaffected spouse, we included measures of liquid assets and the uptake of social insurance. In the year after the death of a spouse, there is a substantial increase in liquid assets, after which they decrease. This may indicate that individuals' whose spouses died because of a fatal health shock taps into their savings. We also find some indications of an increase in the uptake of social insurance, yet the effect for widowers

[25]We do not have information on this type of leave in our data.

is modest.

When analysing the effect by education and age, we find no clear pattern. For example, we find no statistically or economically significant effect of a spouse's death on the remaining spouse's own earnings when considering widowers aged between 45 and 60 when their spouse passed away. However, the same group experienced a statistically significant and persistent drop in labour market participation following the death of a spouse. This suggests that differences in socioeconomic status and age when the shock occurred are difficult to disentangle. We find no effect on any of the outcomes when considering the effect of a non-fatal spousal health shock. The individuals who experienced the shock seem to be affected negatively on the extensive margin, but this does not seem to affect the labour market outcomes for the unaffected spouse. We find the same results when we split the sample by education and age.

As underlined by Jeon and Pohl (2017), differences in the types of health shock considered can explain some of the different findings observed. The measure of a non-fatal health shock, captures events that subsequently results in a hospital admission, and it may be that our findings are not directly comparable to those in Jeon and Pohl (2017). However, García-Gómez et al. (2013) consider acute hospitalisations and find a large and negative effect on the labour supply for the healthy spouse. It is possible to attribute some of the differences to the generosity of the sickness benefits in Norway with a replacement rate of 100% for up to 365 days and to the fact that the authors do not consider other monetary transfers.

Our study underlines the theoretical ambiguity associated with the effect on labour market participation for individuals' whose spouses suffered an acute health shock. The added-worker hypothesis predicts that individuals' self-insure the drop in household earnings by increasing their own labour market participation. While Fadlon and Nielsen (2015) find evidence of the added-worker effect for widows, and only for individuals' with lower earnings then those of the spouse who died, our study results clearly do not support this hypothesis, and we fail to find any evidence for the caregiver hypothesis either.

Although a fatal health shock affects individuals' labour supply in a negative way, some of the underlying mechanisms are still not clear. We find large flows of liquid assets following the death of a spouse, but these assets may stem from several sources, such as a change in home ownership, which is not measurable in our data. What the estimated changes in liquid assets show, however, is that devastating health shocks affect channels other than earnings and labour supply and that this is generally not well understood in the literature.

Overall, this study provides evidence for within-spouses labour market effects when one of the spouses experiences a severe and unexpected health shock. The magnitude is especially substantial for widows, whereas we fail to find any changes in labour supply for individuals' whose spouses experienced a non-fatal health shock. In addition, our study results suggest that the mechanism through which a spouse who experiences a health shock affects labour supply may be much more complex than just through an earnings channel.

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Figures and tables

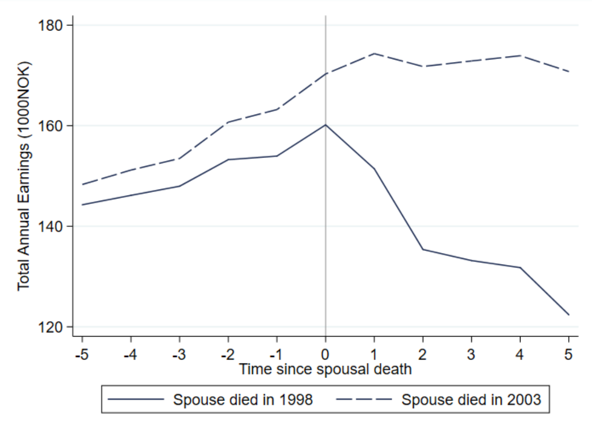
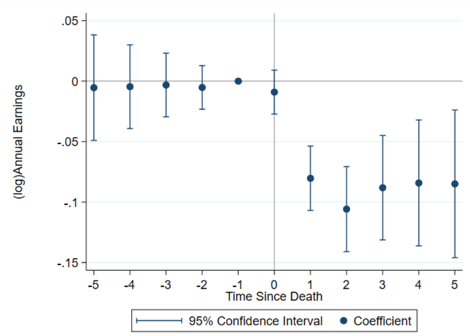
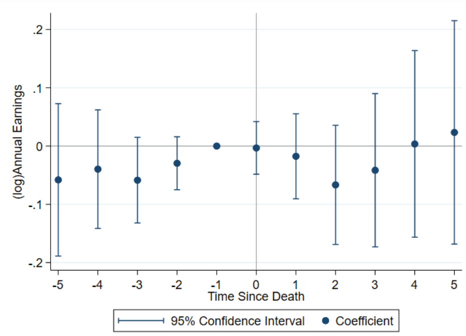


Figure 1: Identification strategy, as in Fadlon and Nielsen (2015)

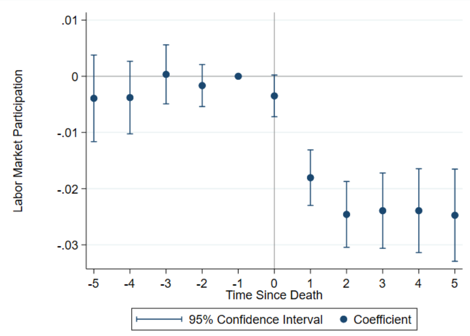
Notes: This Figure shows annual earnings (in NOK 1000). Individuals whose spouses died in 2003 are given a placebo shock in 1998, so that this group acts as a natural comparison group to the group of individuals whose spouses died in 1998.



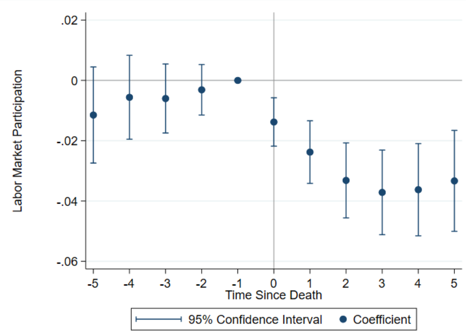
(a) Widows' (log) earnings



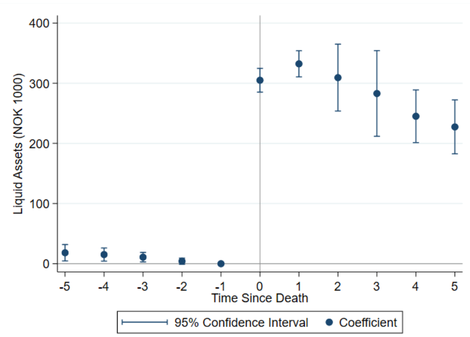
(b) Widowers' (log) earnings



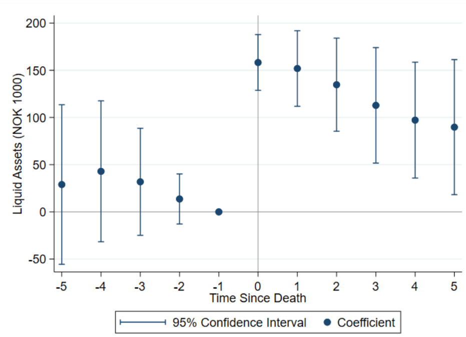
(c) Widows' labour market participation



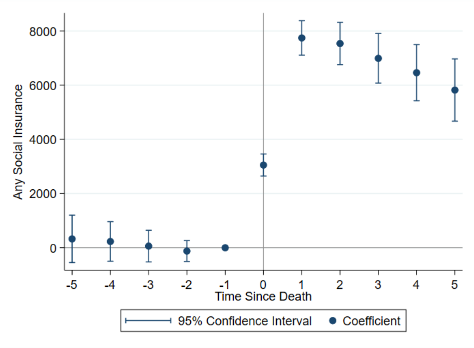
(d) Widowers' labour market participation



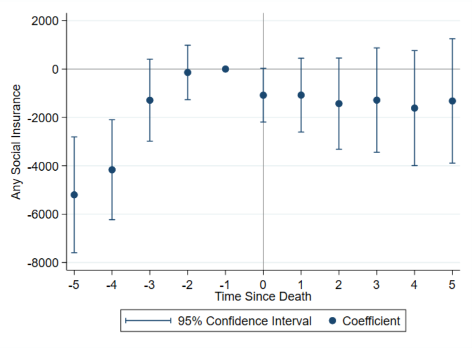
(e) Widows' liquid assets



(f) Widowers' liquid assets



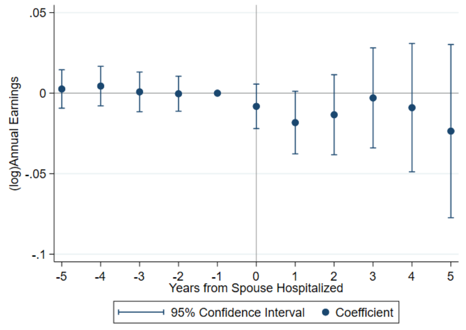
(g) Widows' social insurance benefits



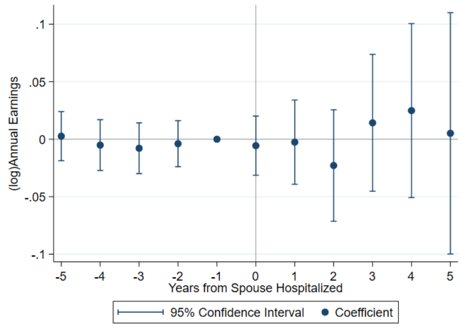
(h) Widowers' social insurance benefits

Figure 2: Main outcomes for individuals' whose spouses experienced a fatal health shock

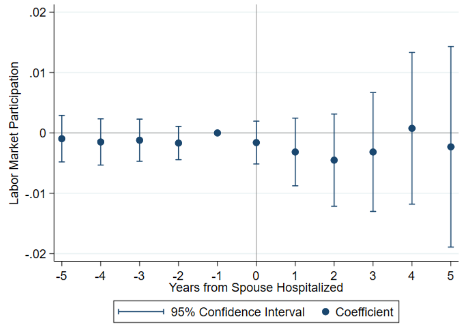
Notes: All estimations are shown with corresponding 95% confidence intervals. The specification includes individual and year-by-cohort fixed effects, and the standard errors are clustered at the individual level. The complete set of estimates is reported in Table 2.



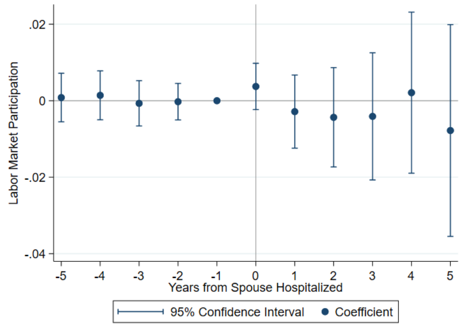
(a) Female (log) earnings



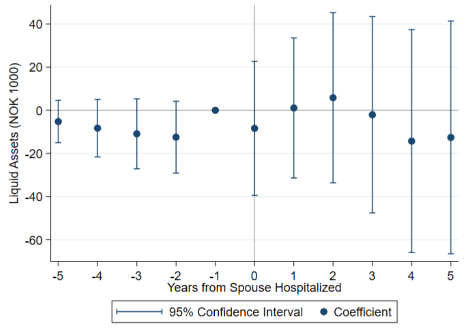
(b) Male (log) earnings



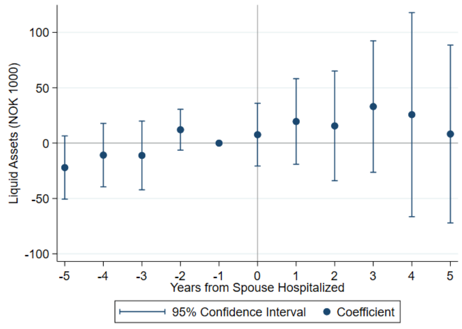
(c) Female labour market participation



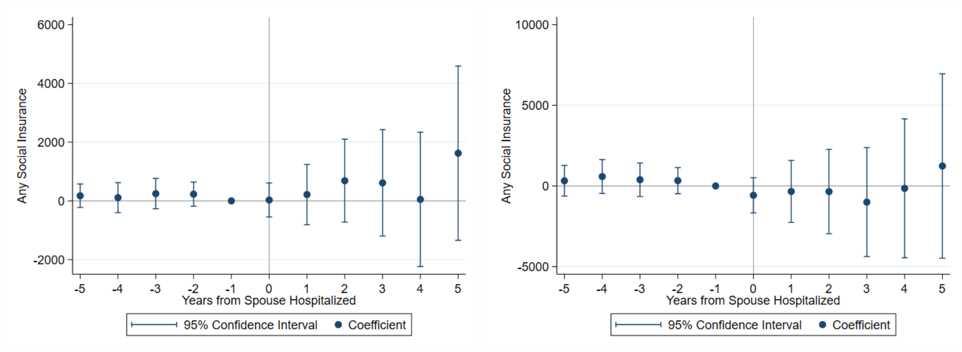
(d) Male labour market participation



(e) Female liquid assets



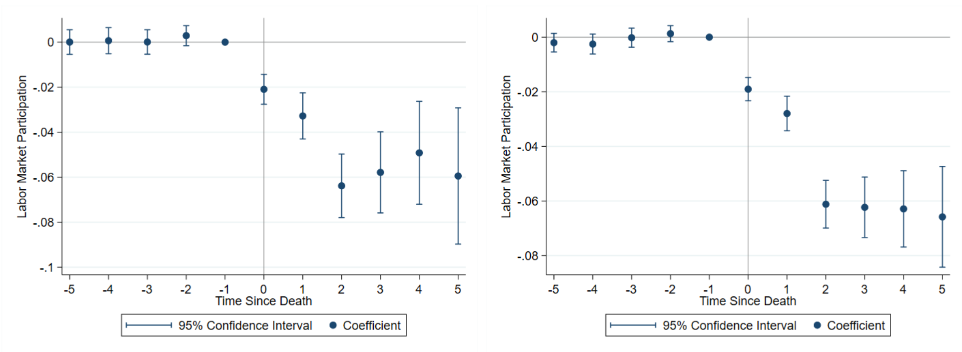
(f) Male liquid assets



(g) Female social insurance benefits (h) Male social insurance benefits

Figure 3: Main outcomes for individuals whose spouses experienced a non-fatal health shock

Notes: All estimations are shown with corresponding 95% confidence intervals. The specification includes individual and year-by-cohort fixed effects, and the standard errors are clustered at the individual level. The complete set of estimates is reported in Table 3.



(a) Female labour market participation (b) Male labour market participation

Figure 4: Effect of an acute and severe health shock on own labour market participation

Notes: All estimations are shown with corresponding 95% confidence intervals. The specification includes individual and year-by-cohort fixed effects, and the standard errors are clustered at the individual level. The complete set of estimates is reported in Appendix B.

Table 1: Sample means, fatal and non-fatal health shocks

A.	Fatal Spousal Health Shocks					
	Whole Sample		Male		Female	
Characteristics	Mean	Std. Dev	Mean	Std. Dev	Mean	Std. Dev
Age *(at time of shock)*	57.77	[8.48]	60.19	[8.27]	57.16	[8.43]
Less than high school degree	0.40	[0.49]	0.36	[0.48]	0.41	[0.49]
High school degree	0.44	[0.50]	0.46	[0.50]	0.44	[0.50]
Any college	0.16	[0.36]	0.17	[0.38]	0.15	[0.36]
Employed	0.57	[0.50]	0.56	[0.50]	0.57	[0.50]
Average earnings in (NOK 1000)	155.716	[182.582]	208.668	[259.244]	142.349	[154.604]
Liquid assets in (NOK 1000)	174.517	[666.782]	319.443	[1.282.235]	137.507	[367.701]
Any social insurance	0.31	[0.46]	0.29	[0.45]	0.32	[0.48]
Spouse passed away due to						
Ischemic heart diseases	0.63	[0.48]	0.49	[0.50]	0.67	[0.47]
Other heart diseases	0.11	[0.31]	0.12	[0.32]	0.11	[0.31]
Cerebrovascular diseases (Stroke)	0.19	[0.39]	0.31	[0.46]	0.15	[0.36]
Transportation Accidents	0.07	[0.25]	0.08	[0.27]	0.07	[0.25]
N	25,783		5198		20,585	

B.	Non-fatal Spousal Health Shock					
	Whole Sample		Male		Female	
	Mean	Std. Dev	Mean	Std. Dev	Mean	Std. Dev
Age *(at time of shock)*	57.25	[8.49]	60.32	[7.90]	56.42	[8.51]
Less than high school degree	0.28	[0.45]	0.27	[0.45]	0.29	[0.45]
High school degree	0.46	[0.50]	0.47	[0.50]	0.45	[0.50]
Any college	0.26	[0.44]	0.25	[0.43]	0.26	[0.44]
Employed	0.60	[0.49]	0.56	[0.50]	0.62	[0.49]
Average earnings in (NOK 1000)	226.748	[251.132]	276.134	[333.001]	207.545	[207.745]
Liquid assets in (NOK 1000)	328.843	[2.909.989]	527.995	[3.513.335]	251.401	[2.634.390]
Any social insurance	0.24	[0.43]	0.20	[0.40]	0.25	[0.43]
Spouse hospitalized due to						
Stroke	0.26	[0.46]	0.33	[0.48]	0.23	[0.45]
Myocardial infarction	0.69	[0.46]	0.62	[0.49]	0.72	[0.45]
Congestive heart failure	0.05	[0.22]	0.05	[0.22]	0.05	[0.22]
Average length of inpatient stay	4.06	[4.71]	4.23	[5.10]	4.00	[4.56]
N	41,892		10,621		31,271	

Standard deviations in [square brackets]

Notes: This table displays descriptive statistics for individuals whose spouses suffered from a fatal (Panel A) or non-fatal (Panel B) health shock. Employment, earnings and liquid assets are measured the year before the shock and include zeroes. Employment is defined as earnings equal to or above one basis amount, which is the minimum earnings required for pension accrual. Cause of death is based on the European shortlist for causes of death, while hospitalisations are based on ICD-10 diagnostic codes and coding for whether the admission was unscheduled or planned. Nominal values are deflated to 2006 prices.

Table 2: Sample selection

Panel A: Fatal Health Shock	Men	Women
Number of deaths in 1992–2014	461,040	478,469
as a result of ischemic, cerebrovascular or other heart diseases, or transport accidents	154,578	164,385
who were married and aged between 30 and 70 when they died	7,709	29,501
with an unaffected spouse aged between 30 and 70 at time of death	5,198	20,585
Panel B: Non-fatal Health Shock	Men	Women
Number of people who experienced acute hospitalisations in 2009–2014	720,995	900,416
for at least one overnight stay	613,587	792,784
not admitted the last 365 days	600,428	774,012
as a result of stroke, ischemic or cardiovascular heart disease	66,112	41,558
who were married and aged between 30 and 70 at the time of the hospitalisation	14,752	35,674
with an unaffected spouse aged between 30 and 70 at the time of the acute hospitalisation	10,621	31,271

Notes: This table reports the sample selection in this analysis, split by fatal and non-fatal health shocks.

Table 3: Effect of a spousal death on labour supply, liquid assets and social insurance

	Log Labour Earnings		Employment		Liquid Assets (NOK 1000)		Any Social Insurance	
Years since shock	(1) Female	(2) Male	(3) Female	(4) Male	(5) Female	(6) Male	(7) Female	(8) Male
-5	-0.005	-0.058	-0.004	-0.011	18.176***	28.949	327.5	-5,199***
	(0.022)	(0.067)	(0.004)	(0.008)	(6.959)	(43.136)	(446.4)	(1,220.8)
-4	-0.005	-0.040	-0.004	-0.006	15.190***	42.943	231.5	-4,161***
	(0.018)	(0.052)	(0.003)	(0.007)	(5.550)	(38.098)	(372.2)	(1,054.0)
-3	-0.003	-0.059	0.000	-0.006	10.874***	31.825	60.5	-1,288.2
	(0.013)	(0.037)	(0.003)	(0.006)	(4.043)	(28.927)	(297.5)	(863.5)
-2	-0.005	-0.030	-0.002	-0.003	4.088	13.666	-120.9	-140.523
	(0.009)	(0.023)	(0.002)	(0.004)	(2.626)	(13.532)	(196.6)	(574.3)
0	-0.009	-0.003	-0.003*	-0.014***	304.983***	158.246***	3,053.9***	-1,080.9*
	(0.009)	(0.023)	(0.002)	(0.004)	(10.057)	(15.064)	(208.2)	(566.1)
1	-0.080***	-0.018	-0.018***	-0.024***	332.303***	151.886***	7,746.0***	-1,077.8
	(0.014)	(0.037)	(0.003)	(0.005)	(11.067)	(20.416)	(325.1)	(778.7)
2	-0.106***	-0.067	-0.025***	-0.033***	309.359***	134.724***	7,539.3***	-1,428.3
	(0.018)	(0.052)	(0.003)	(0.006)	(28.365)	(25.173)	(397.9)	(961.3)
3	-0.088***	-0.042	-0.024***	-0.037***	282.989***	112.841***	6,992.8***	-1,284.5
	(0.022)	(0.067)	(0.003)	(0.007)	(36.320)	(31.208)	(466.9)	(1,100.9)
4	-0.084***	0.004	-0.024***	-0.036***	245.098***	97.205***	6,462.7***	-1,614.0
	(0.027)	(0.082)	(0.004)	(0.008)	(22.346)	(31.328)	(528.6)	(1,213.0)
5	-0.085***	0.023	-0.025***	-0.033***	227.444***	89.740**	5,822.1***	-1,318.6
	(0.031)	(0.098)	(0.004)	(0.009)	(22.890)	(36.503)	(586.0)	(1,311.9)
Observations	272,260	68,891	486,112	121,110	486,112	121,110	486,112	121,110
R-squared	0.185	0.257	0.263	0.366	0.010	0.043	0.253	0.218
Number of ID	18,963	4,900	22,096	5,505	22,096	5,505	22,096	5,505

Notes: This table reports coefficients from the specification in equation 1. *, ** and *** indicate significance at the 10%, 5% and 1% levels, respectively. The specification includes individual and year-by-cohort fixed effects, and the standard errors are clustered at the individual level.

Table 4: Effect of spouse's non-fatal health shock on labour supply, liquid assets and social insurance

	Log Labour Earnings		Employment		Liquid Assets (NOK 1000)		Any Social Insurance	
Years since shock	(1) Female	(2) Male	(3) Female	(4) Male	(5) Female	(6) Male	(7) Female	(8) Male
-5	0.003	0.003	-0.001	0.001	-5.23	-22.08	176.09	320.47
	(0.006)	(0.011)	(0.002)	(0.003)	(5.01)	(14.57)	(204.42)	(484.90)
-4	0.004	-0.005	-0.001	0.001	-8.29	-10.82	111.49	585.41
	(0.006)	(0.011)	(0.002)	(0.003)	(6.80)	(14.61)	(260.69)	(537.07)
-3	0.001	-0.008	-0.001	-0.001	-10.90	-11.14	248.08	385.85
	(0.006)	(0.011)	(0.002)	(0.003)	(8.27)	(15.84)	(263.53)	(529.01)
-2	-0.000	-0.004	-0.002	-0.000	-12.47	12.08	229.80	330.13
	(0.006)	(0.010)	(0.001)	(0.002)	(8.49)	(9.42)	(208.93)	(412.85)
0	-0.008	-0.006	-0.002	0.004	-8.38	7.63	30.85	-581.23
	(0.007)	(0.013)	(0.002)	(0.003)	(15.84)	(14.45)	(295.59)	(555.08)
1	-0.018*	-0.003	-0.003	-0.003	1.07	19.53	215.64	-340.47
	(0.010)	(0.019)	(0.003)	(0.005)	(16.56)	(19.71)	(523.96)	(978.40)
2	-0.013	-0.023	-0.005	-0.004	5.81	15.57	687.11	-348.14
	(0.013)	(0.025)	(0.004)	(0.007)	(20.13)	(25.27)	(720.29)	(1,334.25)
3	-0.003	0.014	-0.003	-0.004	-2.09	33.01	614.31	-1,001.46
	(0.016)	(0.030)	(0.005)	(0.008)	(23.21)	(30.28)	(924.18)	(1,722.01)
4	-0.009	0.025	0.001	0.002	-14.29	25.72	51.27	-148.31
	(0.020)	(0.039)	(0.006)	(0.011)	(26.34)	(47.02)	(1,167.16)	(2,196.39)
5	-0.024	0.005	-0.002	-0.008	-12.60	8.24	1,624.02	1,235.71
	(0.027)	(0.054)	(0.008)	(0.014)	(27.52)	(40.98)	(1,513.63)	(2,915.72)
Observations	236,508	88,672	323,256	125,700	323,256	125,700	323,256	125,700
R-squared	0.110	0.157	0.148	0.198	0.005	0.005	0.152	0.139
Number of ID	23,761	9,175	26,938	10,475	26,938	10,475	26,938	10,475

Notes: This table reports coefficients from the specification in equation 1. *, ** and *** indicate significance at the 10%, 5% and 1% levels, respectively.. The specification includes individual and year-by-cohort fixed effects, and the standard errors are clustered at the individual level.

Appendix A. Effect of a spousal death by outcome, age and education

Table A1: Effect on earnings for females whose spouses passed away

	Females whose spouses passed away					
	30–44		45–60		61–70	
Years since shock	(1) Low edu.	(2) High edu.	(3) Low edu.	(4) High edu.	(5) Low edu.	(6) High edu.
-5	-0.029	-0.008	-0.033	-0.098	-0.323***	-5.869
	(0.071)	(0.079)	(0.064)	(0.106)	(0.116)	(4.921)
-4	-0.005	0.007	-0.029	-0.075	-0.251***	-4.423
	(0.057)	(0.063)	(0.049)	(0.080)	(0.089)	(3.691)
-3	0.014	0.015	-0.017	-0.044	-0.200***	-2.916
	(0.047)	(0.058)	(0.034)	(0.055)	(0.063)	(2.461)
-2	0.009	0.029	-0.012	-0.033	-0.098**	-1.497
	(0.035)	(0.043)	(0.020)	(0.030)	(0.038)	(1.231)
0	-0.036	-0.036	0.018	0.052*	0.004	1.435
	(0.034)	(0.038)	(0.020)	(0.030)	(0.040)	(1.229)
1	-0.158***	-0.171***	-0.048	0.040	-0.053	2.866
	(0.045)	(0.052)	(0.034)	(0.055)	(0.067)	(2.459)
2	-0.240***	-0.242***	-0.073	0.038	0.032	4.241
	(0.054)	(0.062)	(0.049)	(0.080)	(0.092)	(3.689)
3	-0.221***	-0.270***	-0.043	0.075	0.115	5.677
	(0.062)	(0.070)	(0.064)	(0.106)	(0.113)	(4.919)
4	-0.238***	-0.259***	-0.040	0.112	0.164	7.029
	(0.069)	(0.072)	(0.079)	(0.131)	(0.135)	(6.150)
5	-0.198***	-0.257***	-0.050	0.123	0.282*	8.706
	(0.076)	(0.078)	(0.094)	(0.157)	(0.157)	(7.380)
Observations	20,175	9,040	85,801	33,718	34,306	10,619
R-squared	0.133	0.214	0.155	0.254	0.284	0.424
Number of ID	1,377	564	6,001	1,921	3,329	794

Notes: This table reports coefficients from the specification in equation 1. *, ** and *** indicate significance at the 10%, 5% and 1% levels, respectively. The specification includes individual and year-by-cohort fixed effects, and the standard errors are clustered at the individual level.

Table A2: Effect on earnings for males whose spouses passed away

	Males whose spouses passed away					
	30–44		45–60		61–70	
Years since shock	(1) Low edu.	(2) High edu.	(3) Low edu.	(4) High edu.	(5) Low edu.	(6) High edu.
-5	-0.214	-0.041	-0.135	-0.190	-0.199	-0.087
	(0.179)	(0.284)	(0.226)	(0.213)	(0.251)	(0.368)
-4	-0.109	0.131	-0.126	-0.143	-0.198	-0.142
	(0.155)	(0.212)	(0.171)	(0.160)	(0.195)	(0.290)
-3	-0.038	0.047	-0.099	-0.086	-0.231*	-0.239
	(0.103)	(0.141)	(0.116)	(0.109)	(0.138)	(0.202)
-2	-0.099	0.009	-0.037	-0.046	-0.088	-0.115
	(0.084)	(0.093)	(0.061)	(0.060)	(0.083)	(0.127)
0	-0.266***	-0.055	0.041	0.050	-0.004	-0.017***
	(0.087)	(0.110)	(0.062)	(0.060)	(0.023)	(0.000)
1	-0.549***	-0.290**	0.078	0.089	0.030	-0.112
	(0.126)	(0.140)	(0.116)	(0.108)	(0.098)	(0.129)
2	-0.575***	-0.391**	0.069	0.069	0.040	-0.039
	(0.126)	(0.166)	(0.167)	(0.154)	(0.154)	(0.217)
3	-0.394***	-0.143	0.099	0.158	-0.007	-0.104
	(0.148)	(0.182)	(0.226)	(0.211)	(0.209)	(0.302)
4	-0.325*	-0.077	0.190	0.220	-0.087	-0.294
	(0.170)	(0.229)	(0.280)	(0.261)	(0.268)	(0.392)
5	-0.228	0.042	0.235	0.224	-0.144	-0.493
	(0.188)	(0.292)	(0.337)	(0.317)	(0.334)	(0.487)
Observations	3,066	1,244	17,755	8,000	14,193	5,353
R-squared	0.327	0.624	0.212	0.313	0.334	0.423
Number of ID	192	72	1,145	440	1,309	393

Notes: This table reports coefficients from the specification in equation 1. *, ** and *** indicate significance at the 10%, 5% and 1% levels, respectively. The specification includes individual and year-by-cohort fixed effects, and the standard errors are clustered at the individual level.

Table A3: Effect on employment for females whose spouses passed away

	Females whose spouses passed away					
	30–44		45–60		61–70	
Years since shock	(1) Low edu.	(2) High edu.	(3) Low edu.	(4) High edu.	(5) Low edu.	(6) High edu.
-5	-0.051**	-0.039	-0.016**	-0.035***	-0.005	0.012
	(0.023)	(0.034)	(0.008)	(0.012)	(0.014)	(0.032)
-4	-0.039**	-0.027	-0.013*	-0.023**	-0.004	0.010
	(0.019)	(0.027)	(0.006)	(0.010)	(0.011)	(0.026)
-3	-0.024	-0.051**	-0.007	-0.013*	-0.002	-0.003
	(0.015)	(0.022)	(0.005)	(0.008)	(0.008)	(0.020)
-2	-0.019*	-0.037**	-0.004	-0.004	-0.005	-0.004
	(0.011)	(0.016)	(0.003)	(0.005)	(0.005)	(0.012)
0	-0.021**	-0.024*	-0.001	0.001	-0.006	-0.021*
	(0.010)	(0.014)	(0.003)	(0.005)	(0.005)	(0.013)
1	-0.048***	-0.042**	-0.014***	-0.000	-0.019***	-0.033*
	(0.013)	(0.019)	(0.005)	(0.007)	(0.007)	(0.019)
2	-0.081***	-0.087***	-0.022***	0.003	-0.020***	-0.049**
	(0.017)	(0.023)	(0.006)	(0.009)	(0.008)	(0.023)
3	-0.075***	-0.079***	-0.024***	0.005	-0.015*	-0.042
	(0.019)	(0.026)	(0.007)	(0.011)	(0.008)	(0.027)
4	-0.078***	-0.067**	-0.025***	0.013	-0.015*	-0.052*
	(0.022)	(0.030)	(0.009)	(0.013)	(0.009)	(0.029)
5	-0.074***	-0.068**	-0.027***	0.010	-0.011	-0.044
	(0.024)	(0.032)	(0.010)	(0.015)	(0.009)	(0.031)
Observations	28,597	10,384	143,581	41,427	103,155	18,455
R-squared	0.099	0.114	0.229	0.345	0.320	0.515
Number of ID	1,471	573	6,814	1,967	4,732	845

Notes: This table reports coefficients from the specification in equation 1. *, ** and *** indicate significance at the 10%, 5% and 1% levels, respectively. The specification includes individual and year-by-cohort fixed effects, and the standard errors are clustered at the individual level.

Table A4: Effect on employment for males whose spouses passed away

	Males whose spouses passed away					
	30–44		45–60		61–70	
Years since shock	(1) Low edu.	(2) High edu.	(3) Low edu.	(4) High edu.	(5) Low edu.	(6) High edu.
-5	-0.062	-0.177*	-0.045**	-0.040	-0.019	-0.036
	(0.062)	(0.093)	(0.019)	(0.029)	(0.023)	(0.045)
-4	0.002	-0.034	-0.040**	-0.029	-0.021	-0.034
	(0.049)	(0.063)	(0.016)	(0.023)	(0.019)	(0.037)
-3	0.008	-0.039	-0.025**	0.011	-0.015	-0.040
	(0.040)	(0.040)	(0.012)	(0.016)	(0.014)	(0.029)
-2	-0.005	-0.018	-0.010	0.008	-0.011	-0.008
	(0.026)	(0.027)	(0.008)	(0.011)	(0.009)	(0.019)
0	-0.080***	-0.042	-0.010	-0.022*	-0.015*	-0.058***
	(0.029)	(0.037)	(0.009)	(0.012)	(0.009)	(0.020)
1	-0.165***	-0.072	-0.009	-0.016	-0.023**	-0.079***
	(0.039)	(0.053)	(0.012)	(0.017)	(0.012)	(0.027)
2	-0.204***	-0.094	-0.006	-0.027	-0.022	-0.074**
	(0.046)	(0.068)	(0.015)	(0.023)	(0.014)	(0.033)
3	-0.171***	-0.025	-0.015	-0.049	-0.019	-0.061
	(0.056)	(0.074)	(0.018)	(0.030)	(0.015)	(0.038)
4	-0.190***	-0.039	-0.002	-0.031	-0.023	-0.079*
	(0.062)	(0.095)	(0.021)	(0.035)	(0.016)	(0.041)
5	-0.166**	0.035	0.002	-0.035	-0.024	-0.085**
	(0.073)	(0.109)	(0.024)	(0.041)	(0.016)	(0.043)
Observations	3,970	1,440	25,661	9,578	35,920	9,276
R-squared	0.232	0.542	0.319	0.376	0.392	0.484
Number of ID	203	74	1,209	444	1,645	423

Notes: This table reports coefficients from the specification in equation 1. *, ** and *** indicate significance at the 10%, 5% and 1% levels, respectively. The specification includes individual and year-by-cohort fixed effects, and the standard errors are clustered at the individual level.

Table A5: Effect on liquid assets for females whose spouses passed away

	Females whose spouses passed away					
	30–44		45–60		61–70	
Years since shock	(1) Low edu.	(2) High edu.	(3) Low edu.	(4) High edu.	(5) Low edu.	(6) High edu.
-5	28.954	113.619*	76.696	-66.227	73.609*	246.017
	(23.662)	(62.560)	(89.373)	(92.445)	(43.394)	(199.396)
-4	25.864	73.158	54.085	-59.193	53.988	185.652
	(18.917)	(47.940)	(64.274)	(69.412)	(33.186)	(161.714)
-3	16.617	40.885	31.037	-38.154	41.629*	149.575
	(13.020)	(33.723)	(40.440)	(46.623)	(25.075)	(122.470)
-2	0.133	11.897	21.163	13.210	4.509	36.903
	(6.350)	(15.808)	(19.886)	(26.137)	(7.127)	(35.662)
0	353.908***	620.029***	284.219***	472.119***	253.770***	466.698***
	(21.270)	(49.193)	(13.748)	(37.632)	(26.236)	(104.653)
1	393.086***	712.016***	331.258***	591.597***	244.194***	397.149***
	(24.798)	(59.917)	(21.464)	(65.044)	(19.145)	(96.307)
2	335.821***	613.734***	360.276***	546.211***	198.218***	245.269**
	(25.083)	(62.074)	(72.090)	(75.623)	(23.585)	(121.736)
3	309.594***	574.529***	358.963***	514.730***	165.939***	147.047
	(29.026)	(72.864)	(97.482)	(96.151)	(26.938)	(161.883)
4	274.711***	533.942***	286.331***	497.845***	142.492***	91.844
	(33.379)	(86.232)	(51.347)	(113.515)	(28.934)	(184.251)
5	253.046***	481.215***	274.249***	502.177***	118.221***	0.707
	(34.282)	(84.582)	(56.327)	(138.827)	(32.473)	(220.770)
Observations	28,597	10,384	143,581	41,427	103,155	18,455
R-squared	0.121	0.220	0.011	0.058	0.032	0.058
Number of ID	1,471	573	6,814	1,967	4,732	845

Notes: This table reports coefficients from the specification in equation 1. *, ** and *** indicate significance at the 10%, 5% and 1% levels, respectively. The specification includes individual and year-by-cohort fixed effects, and the standard errors are clustered at the individual level.

Table A6: Effect on liquid assets for males whose spouses passed away

	Males whose spouses passed away					
	30–44		45–60		61–70	
Years since shock	(1) Low edu.	(2) High edu.	(3) Low edu.	(4) High edu.	(5) Low edu.	(6) High edu.
-5	-155.065	-1,667.520	-32.283	-162.182**	-10.120	3.859
	(331.930)	(1,932.830)	(34.427)	(76.079)	(34.473)	(137.750)
-4	-86.068	-992.363	-29.516	-127.606**	-4.509	3.257
	(222.905)	(1,237.493)	(26.940)	(57.808)	(25.355)	(105.745)
-3	-72.385	-619.572	-38.184*	-126.771***	7.296	-31.676
	(157.036)	(782.012)	(20.424)	(42.805)	(22.139)	(71.341)
-2	-63.118	-292.906	-20.115	-58.608**	-2.561	0.099
	(91.171)	(392.328)	(14.224)	(28.403)	(13.328)	(39.516)
0	360.755*	1,061.600	189.279***	304.755***	117.572***	247.202***
	(185.575)	(770.643)	(18.538)	(41.405)	(16.094)	(64.257)
1	474.148**	1,590.499	226.727***	414.726***	123.219***	263.033***
	(231.358)	(1,031.802)	(24.504)	(55.238)	(23.525)	(97.455)
2	420.821	1,671.977	233.189***	486.442***	92.023***	222.620*
	(255.722)	(1,215.704)	(34.298)	(81.630)	(28.932)	(125.500)
3	424.958	1,897.403	213.456***	453.014***	81.794**	246.688
	(307.789)	(1,539.983)	(34.138)	(74.797)	(34.774)	(153.386)
4	324.504	1,687.120	203.349***	477.394***	87.507**	283.425
	(313.116)	(1,575.649)	(36.942)	(85.456)	(42.546)	(191.707)
5	253.912	1,547.037	221.960***	560.886***	100.234*	330.951
	(334.267)	(1,700.369)	(45.898)	(117.759)	(53.068)	(240.956)
Observations	3,970	1,440	25,661	9,578	35,920	9,276
R-squared	0.104	0.276	0.073	0.142	0.052	0.110
Number of ID	203	74	1,209	444	1,645	423

Notes: This table reports coefficients from the specification in equation 1. *, ** and *** indicate significance at the 10%, 5% and 1% levels, respectively. The specification includes individual and year-by-cohort fixed effects, and the standard errors are clustered at the individual level.

Table A7: Effect on social insurance for females whose spouses passed away

	Females whose spouses passed away					
	30–44		45–60		61–70	
Years since shock	(1)	(2)	(3)	(4)	(5)	(6)
	Low edu.	High edu.	Low edu.	High edu.	Low edu.	High edu.
-5	5,834.85	6,062.72***	4,893.45***	6,062.72***	2,380.15	6,062.72***
	(3,827.37)	(1,852.15)	(1,383.50)	(1,852.15)	(2,516.45)	(1,852.15)
-4	4,767.71	4,430.00***	3,743.93***	4,430.00***	2,219.92	4,430.00***
	(3,024.11)	(1,446.93)	(1,060.40)	(1,446.93)	(1,905.60)	(1,446.93)
-3	2,968.47	3,172.32***	2,339.36***	3,172.32***	786.17	3,172.32***
	(2,137.48)	(1,056.38)	(745.22)	(1,056.38)	(1,324.74)	(1,056.38)
-2	1,258.98	825.46	1,244.86***	825.46	-281.00	825.46
	(1,203.40)	(627.13)	(434.76)	(627.13)	(721.00)	(627.13)
0	30,549.67***	17,033.97***	18,654.03***	17,033.97***	19,899.79***	17,033.97***
	(1,572.90)	(891.70)	(592.92)	(891.70)	(809.71)	(891.70)
1	78,791.16***	43,792.65***	41,295.78***	43,792.65***	42,910.80***	43,792.65***
	(2,963.18)	(1,601.79)	(990.23)	(1,601.79)	(1,360.95)	(1,601.79)
2	73,619.75***	39,150.77***	39,103.27***	39,150.77***	39,729.78***	39,150.77***
	(3,558.55)	(1,861.59)	(1,228.33)	(1,861.59)	(1,802.76)	(1,861.59)
3	67,877.33***	36,165.99***	37,458.90***	36,165.99***	36,552.14***	36,165.99***
	(4,129.35)	(2,154.75)	(1,476.05)	(2,154.75)	(2,178.45)	(2,154.75)
4	62,531.52***	33,751.28***	35,790.21***	33,751.28***	34,313.33***	33,751.28***
	(4,742.83)	(2,513.09)	(1,741.10)	(2,513.09)	(2,506.02)	(2,513.09)
5	58,935.88***	31,214.80***	34,035.22***	31,214.80***	30,090.81***	31,214.80***
	(5,380.12)	(2,885.58)	(2,016.58)	(2,885.58)	(2,798.02)	(2,885.58)
Observations	28,597	70,266	143,581	70,266	103,155	70,266
R-squared	0.314	0.496	0.414	0.496	0.486	0.496
Number of ID	1,471	3,385	6,814	3,385	4,732	3,385

Notes: This table reports coefficients from the specification in equation 1. *, ** and *** indicate significance at the 10%, 5% and 1% levels, respectively. The specification includes individual and year-by-cohort fixed effects, and the standard errors are clustered at the individual level.

Table A8: Effect on social insurance for males whose spouses passed away

	Males whose spouses passed away					
	30–44		45–60		61–70	
Years since shock	(1) Low edu.	(2) High edu.	(3) Low edu.	(4) High edu.	(5) Low edu.	(6) High edu.
-5	-13,847.23	22,242.37*	-1,589.30	-3,648.10	-1,794.29	-5,219.73
	(10,104.86)	(11,908.46)	(2,752.46)	(3,243.94)	(3,588.51)	(6,398.41)
-4	-15,777.09**	15,952.49*	-1,951.04	-2,780.57	428.38	-3,843.72
	(7,732.89)	(9,007.61)	(2,319.46)	(2,818.90)	(2,904.85)	(5,183.32)
-3	-12,841.39**	10,380.79	-4.83	-2,693.95	2,541.39	-2,665.70
	(5,828.58)	(6,268.02)	(1,890.56)	(2,480.32)	(2,138.74)	(3,970.13)
-2	-6,323.18*	6,968.21*	356.50	-832.22	480.66	-2,193.57
	(3,465.97)	(3,767.33)	(1,152.56)	(1,647.27)	(1,238.14)	(2,422.42)
0	4,493.05	-4,933.80	234.11	-785.67	-2,416.13**	-473.09
	(3,109.74)	(3,750.47)	(1,287.45)	(1,011.09)	(1,133.15)	(2,372.85)
1	15,943.56**	-1,075.21	-598.91	-2,569.27	-4,184.39**	-1,607.20
	(6,235.08)	(6,256.69)	(1,616.26)	(1,955.27)	(1,706.85)	(3,343.11)
2	22,874.61***	1,139.95	-1,137.03	-517.72	-5,873.31***	-5,097.30
	(8,755.83)	(10,790.11)	(2,088.77)	(2,661.62)	(2,049.14)	(4,090.23)
3	25,162.64**	-4,963.06	-1,344.28	-967.20	-6,242.51***	-5,341.18
	(10,492.29)	(12,256.32)	(2,466.97)	(3,015.20)	(2,243.67)	(4,467.65)
4	23,178.67*	-17,239.11	-3,740.56	-1,938.59	-6,707.49***	-4,277.86
	(12,261.03)	(14,209.41)	(2,858.46)	(3,499.79)	(2,261.65)	(4,511.21)
5	25,106.56*	-23,396.58	-4,778.27	-2,571.99	-5,862.67***	-4,293.47
	(13,783.27)	(17,295.37)	(3,166.41)	(3,872.10)	(2,052.13)	(4,126.60)
Observations	3,970	1,440	25,661	9,578	35,920	9,276
R-squared	0.281	0.375	0.193	0.162	0.326	0.202
Number of ID	203	74	1,209	444	1,645	423

Notes: This table reports coefficients from the specification in equation 1. *, ** and *** indicate significance at the 10%, 5% and 1% levels, respectively. The specification includes individual and year-by-cohort fixed effects, and the standard errors are clustered at the individual level.

Appendix B. Effect of a health shock on own labour market participation

Table B1: Effect of a health shock on own labour market participation

	Employment	
Years since shock	(1) Female	(2) Male
-5	0.000	-0.002
	(0.003)	(0.002)
-4	0.001	-0.003
	(0.003)	(0.002)
-3	0.000	-0.000
	(0.003)	(0.002)
-2	0.003	0.001
	(0.002)	(0.001)
0	-0.021***	-0.019***
	(0.003)	(0.002)
1	-0.033***	-0.028***
	(0.005)	(0.003)
2	-0.064***	-0.061***
	(0.007)	(0.004)
3	-0.058***	-0.062***
	(0.009)	(0.006)
4	-0.049***	-0.063***
	(0.012)	(0.007)
5	-0.059***	-0.066***
	(0.015)	(0.009)
Observations	119,420	305,982
R-squared	0.162	0.207
Number of ID	10,621	31,271

Notes: This table reports coefficients from the specification in equation 1. *, ** and *** indicate significance at the 10%, 5% and 1% levels, respectively. The specification includes individual and year-by-cohort fixed effects, and the standard errors are clustered at the individual level.

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