The effect of long-term unemployment subsidies on the health of middle-age disadvantaged workers

José Ignacio Garcia-Pérez

Department of Economics, Universidad Pablo Olavide

Manuel Serrano-Alarcón

NOVA National School of Public Health, NOVA University of Lisbon
Center for Research in Economics and Health, Universitat Pompeu Fabra

Judit Vall Castelló

Department of Economics & Institut D'economia De Barcelona (IEB), Universitat de Barcelona
Center for Research in Economics and Health, Universitat Pompeu Fabra

Abstract

We estimate the health effects of a long-term unemployment (LTU) subsidy targeted to middle aged disadvantaged workers. In order to do that, we exploit a Spanish reform that increased the age eligibility threshold to receive the subsidy from 52 to 55 years old in July 2012. We show that, for the overall sample, hospitalizations were not significantly affected by the reform. However, we do find significant results when we separate the analysis by main diagnosis and gender. More specifically, we show that men who were eligible for the LTU subsidy decreased their hospitalizations due to injuries by 11.7% and their probability of a mental health diagnoses by 2 percentage points. Thus, our results point towards the role of long-term unemployment benefits as a protecting device for the health (both physical and mental) of middle age low educated men who are in a disadvantaged position in the labour market.

Keywords: disadvantaged workers, unemployment subsidies, health effects

1- Introduction

A vast amount of literature has shown how socioeconomic conditions are determinant to health. Education, income, employment status or occupation are some of the socioeconomic factors that are best known to determine our health and contribute to health inequalities (Marmot, 2005). The literature has also discussed how these socioeconomic inequalities in health have their origin early in life and expand towards older ages through a cumulative process (Case, Fertig, & Paxson, 2005).

On the other hand, shocks in the socioeconomic status, such as changes in employment status or income may also affect health contemporaneously in adulthood and older ages (van Ours, 2019). In this context, it has recently been put forward the idea of the so-called "deaths of despair". Case and Deaton (2017) show that there has been an increase in morbidity and mortality in middle age men in the US. They explain this phenomena as a result of a "cumulative disadvantage" process of, mainly, labour market conditions together with a general economic decline and other socioeconomic factors. Furthermore, they argue that such disadvantages will hardly be reversed in the short-term with social programs aimed at these middle aged workers.

In this paper, we provide evidence on the extent to which social programs are able to improve the health of middle aged disadvantaged workers. We exploit a reform that was implemented in Spain in 2012 which created exogenous variation on the eligibility of midlife workers to access a long-term unemployment subsidy. In particular, the reform increased the minimum age required to have access to the long-term unemployment subsidy (LTU) from 52 to 55. We first show that recipients of such subsidies are indeed disadvantaged in several dimensions as they have been unemployed for a long period of time and have lower socioeconomic status (both in terms of education as well as on the type of previous jobs). Therefore, those workers have suffered from the "cumulative disadvantage" discussed in the "deaths of despair" literature, as they have experienced a continuous deterioration of their labour market prospects. We compare cohorts affected/unaffected by this reform to uncover the effectiveness of the long-term unemployment subsidy on hospitalizations, mental health diagnoses and self-reported mental health indicators.

We first show that the reform had an effect on the probability of receiving a LTU subsidy. Those unaffected by the rise in the eligibility age have a significantly higher probability of receiving the LTU subsidy and a lower probability of working or being out of the labour market (i.e.: neither working nor receiving any unemployment benefit or subsidy). The effect is higher for men than for women. When we look at overall hospitalizations, we do not find a significant effect of the reform. However, disaggregating by disease of main diagnosis and gender, we show that men unaffected by the reform (i.e.: having access the LTU subsidy) reduced their hospitalizations due to injuries by 11.7%. Furthermore, men who were eligible for the LTU reduced their probability of being diagnosed by a mental health condition. Lastly, men eligible for the subsidy, show better self-reported mental health and lower euro-d depression. None of these effects is significant for women.

In the current literature, the direction and size of the effects to be expected from a reduction in the access to a LTU subsidy are not clear. On the one hand, there is evidence on the pernicious effects of unemployment on health; the negative effects of unemployment on happiness seem consistent ((Clark, 2003; Kassenboehmer & Haisken-DeNew, 2009; Winkelmann & Winkelmann, 1998); whereas evidence is less clear on other mental health dimensions ((Eliason & Storrie, 2009)(Browning & Heinesen, 2012). Still, negative short-term effects have been found in depression and anxiety (Schaller & Stevens, 2015), mental health hospitalizations (Browning & Heinesen, 2012); and suicides and alcohol-related mortality (Eliason & Storrie, 2009). At the same time, we know that exogenous increases in income may improve mental health (Apouey & Clark, 2015). Therefore, to the extent that the loss of income could at least partially explain the effect of unemployment on health, one could expect LTU unemployment subsidy to alleviate these detrimental health effects, in particular with respect to mental health and stress-related diseases.

On the other hand, not having access to the LTU subsidy may force middle age unemployed individuals to work on jobs that otherwise they would not take. We may think of low-occupational health or very physically demanding jobs for their age. Work increases the risk of suffering injuries due to working accidents, and it can also provoke other physical or mental problems derived from stress (van Ours, 2019). LTU subsidies recipients, as further explained below, come from sectors with more physically demanding jobs and are lower educated. This, in principle, indicates that their alternative source of income to LTU subsidy would be more unskilled and manual jobs, which are those with more pernicious health effects (Ravesteijn, van Kippersluis, & van Doorslaer, 2018). In that sense, LTU subsidies could prevent recipients from suffering injuries and other health hazards related to unskilled and manual jobs.

Other literature related to our work is the one that studies the effect of retirement on health, in particular, early retirement. Although it is targeted to relatively young individuals, as discussed below, the LTU subsidy is sometimes used as a bridge to retirement. As such, it can be thought of an early retirement subsidy. The literature on the effect of retirement on health is inconclusive with research showing both positive and negative effects, and zero effect. But, in general, retirement has been found to improve mental health and deteriorate cognitive skills (van Ours, 2019)**.**There is also mixed evidence for early retirement: on the one hand, early retirement has been found to lead to an improvement in health and a decrease in mortality (Bloemen, Hochguertel, & Zweerink, 2017; Hallberg, Johansson, & Josephson, 2015) On the other hand, others studies have found the opposite, particularly among men ((Kuhn, Staubli, Wuellrich, & Zweimüller, 2018)(Fitzpatrick & Moore, 2018). For instance, Kuhn et al (2018) found an increase in the risk of premature dead among blue-collar men who transitioned to early retirement through an extension of the unemployment benefit scheme. Still, the LTU benefit under study may differ from a normal early retirement scheme in several aspects: first, LTU benefit can be hardly considered as "voluntary" assuming that most individuals at that age would prefer to work rather than being unemployed. Second, the quantity of the LTU subsidy is lower when compared to retirement pensions. Lastly, we look at the relatively short-term health effects at ages 52-55, which is a very young age to retire: most pension systems do not include access to retirement benefits at such a young age.

The rest of the paper structures as follows. Section 2 describes the LTU subsidy reform. Section 3 describes the data sources. Section 4 explains the empirical strategy. Section 5 describes the results. Section 6 discusses the implications of the results.

2- The long-term unemployed (LTU) subsidy and the reform of 2012

In July 2012, amid increasing pressure to reduce the public deficit, the Spanish government passed a reform to "guarantee fiscal stability and increase the competiveness of the Spanish economy" (Real Decreto-ley 20/2012). One of the measures of the reform was the increase from fifty two to fifty five years old in the eligibility age to access a subsidy for long-term unemployed workers

The subsidy is specially targeted to workers approaching retirement age and unable to find a bridge job until getting access to the retirement pension, which is available to workers aged 65 and more. Apart from the age threshold, to be able to access the long-term unemployment subsidy program, individuals need to have exhausted their unemployment insurance benefits. Thus, it acts as a last resort option and the amount of benefits provided is relatively low, around426€ per month[1].

All these characteristics and requirements reduce the incentives to apply for the subsidy, which is designed to reach individuals who have serious difficulties in finding employment due to a combination of their low education and skill levels as well as their age.

In the Spanish context, losing a job after the age of 50 may entail not being able to work again due to the relatively high unemployment rates of the country and the relatively scarcity of training programs for unemployed workers. In fact, according to data from the Spanish Labour Force survey, in 2012 42% of unemployed workers aged 50-64 had been unemployed for more than 2 years, as compared to 30% for the overall unemployed population.

Precisely, due to the strong difficulties in finding a job for older workers, the subsidy is sometimes used as a bridge to retirement (or, in other words, a very early retirement scheme). Using the subsample of individuals aged 52-65 years old from the social security registries (MCVL[2]), the age-adjusted probability of receiving a retirement pension in their next labour force status is 17,2% for those receiving the subsidy; as compared to 7,3% for the unemployed not receiving this subsidy, and 8,4% for the employed (Table A1, Appendix).

[1] The amount of the subsidy is set at 80% of a public income index (IPREM), used as a reference by the Spanish Government to determine public subsidies and benefits. In the period under study (2012-2015), such index was set at 532,31€ (Source: http://www.iprem.com.es/).

[2] See the Data section for an explanation of the MCVL dataset.

Recipients of the LTU subsidy spent more time in unemployment than employed during the last four years before receiving the subsidy (Table A2, Appendix). This is the case for both men and women. In addition, men receiving the LTU subsidy had an average of around 4.4 employment contracts during the period 2008-2011, as compared to 3.4 for the rest of the unemployed and 1.8 for the employed. Women receiving the LTU subsidy also had more contracts than the employed (3.1 vs 2.5), although less than the rest of the unemployed (3.8). Overall, Table A2 shows that individuals receiving the LTU subsidy come from a very precarious situation at the labour market.

Furthermore, recipients of the LTU subsidy were much lower educated that those with employment; with 35.6% of them not having primary education as compared to 21% for the employed; whereas they had a similar education level as the rest of the unemployed (Table A3, Appendix). Such educational differences were similar for men and women. Regarding economic sector, men receiving the LTU subsidy were overrepresented in the Construction sector, as compared to the employed (31.6% vs 10%); and similarly to the rest of the unemployed (32.6%). Women receiving the LTU subsidy show a higher proportion in the Industry sector (25.4%) as compared to the rest of the unemployed (7.7%) and to the employed women (7.1%). On the contrary the, percentage of women with LTU subsidy in the Construction sector is much lower (2.3%) than that of the men with LTU subsidy (Table A4, Appendix). Summing up, the recipients of the subsidy are mostly low educated and overrepresented in the Construction (men) and Industry sector (women) and, in general, in unskilled jobs. Furthermore they showed a very poor performance in the labour market with large periods of unemployment and precarious temporal jobs. In that sense, they have a disadvantaged distribution of socioeconomic determinants that may have negatively influenced their health throughout their life.

3- Data and identification strategy

We first focus on analyzing the impacts of the reform on labour market outcomes of affected individuals and then move to the results on both physical and mental health. We use a combination of survey data and administrative registers as there is no single dataset that contains all the necessary information on labour market outcomes and health.

3.1. Labour market outcomes

We begin by analyzing the effects of the reform on labour market outcomes. More specifically, we look at the probability of receiving the LTU subsidy using register data from the Social Security Administration. The database, Continuous Sample of Working Lives, is a representative random sample of 4% of all individuals that have contributed to the Spanish Social Security. For each individual, this microdataset records their lifetime labour market track, including the duration of the employment relation, the economic sector and other characteristics of each employment contract or social security benefit (including unemployment insurance and unemployment subsidies, disability and retirement benefits, among others). It also includes information on a number of socioeconomic characteristics for each individual such as age, gender, education and nationality.

We select a sample of individuals born between 1960 and 1962 because they are aged between fifty and fifty two at the time of the reform. Thus, our sample includes 61,598 individuals and a total of 400,960 person-year observations.

In order to estimate the causal effect of the 2012 LTU subsidy reform on employment status we exploit changes in these outcomes by cohort and semester of birth, before and after the reform. Since the reform was introduced in July 2012, individuals turning 52 right before that date (born in the first semester of 1960), had access to the subsidy, whereas individuals turning 52 right after that date (born in the second semester of 1960) did not have access to the subsidy until 2015 (once they turned 55).

Thus, we define as our treatment group individuals born in the 1st semester of 1960. The control group consists of individuals born in the 2nd semester of 1960. Additionally, we added the closest cohorts unaffected by the reform (1961 and 1962) in order to control for any observed and unobserved differences between individuals born in the first and second semester. The literature has shown that the month of birth may affect health outcomes, either directly ((Buckles & Hungerman, 2013; Costa & Lahey, 2005; Rietveld & Webbink, 2016) or though the increase in education (Angrist & Keueger, 1991)**.** In turn, the increase in education might also affect health outcomes (Cutler & Lleras-Muney, 2006). This seems to be also the case in Spain (Obrero, Martín, & Castello, 2019). As such, simple DiD estimates (using only 1960 cohort) may be biased. Also, under a DiD specification, differences in trends between those born in the 1st semester, and those born in the 2nd semester, could be a result of those born in the 1st semester being "slightly" older. Using the cohorts of 1961-1962 as a further control, we eliminate these potential confounding factors that are semester of birth-specific, and common to both cohort 1960 and cohort 1961-62. Therefore, our main specification is a triple differences-in differences (DDD) exploiting variation by cohort, semester of birth and time, similarly to those used in previous literature (Gruber 1991, Baum 2003, Berck and Vilas Boas 2017).

If the reform had an effect on the probability of receiving the long-term unemployment subsidy, we should see differential changes in the outcomes of those born in the 1st semester as compared to those born in the 2nd semester after the reform only for the cohort of 1960 (and not for the 1961-62 cohort). Alternatively we might have chosen as comparison, the closer older cohorts (i.e.: born in 1958 & 1959), instead of the closest younger cohorts (1961 and 1962). However, cohorts 1958 and 1959 are not an appropriate comparator because they could access the LTU subsidy before the reform, since they turned 52 prior to 2012. As a consequence, those born in the 1st semester of these cohorts would start receiving the subsidy before than those born in the 2nd semester. This can provoke health differences between semester of birth within the control cohorts (1958 and 1959) which could be attributed to receiving the subsidy at different times. Therefore, the younger cohorts (i.e.: 1961 and 1962) form a better comparator because health differences due to any differential access to the LTU subsidy are not expected to arise among them, simply because they could not access the LTU subsidy until 2016 onwards.

Therefore, our identification assumption under DDD model is that there was no shock that differentially affects the outcome of individuals born in the 1st semester, as compared to those born in 2nd semester only in year 1960, but not to those born in years 1961-1962. Below, we specify more in detail all the econometric models that we use, which vary according to each dataset. The DDD impact of the reform on employment status is determined by the following equation:

$$y_{i,t} = \beta_0 + \beta_1 Cohort1960_c + \beta_2 Semester1_s + \beta_3 After2012_t + \beta_4(Cohort1960_c \; x \; Semester1_s) + \beta_5(Cohort1960_c \; x \; After2012_t) + \beta_6(Semester1_s x \; After2012_t) + \beta_7(Cohort1960_c \; x \; Semester1_s \; x \; After2012_t) + year_t + province_p \qquad \text{[Equation 1]}$$

Where $Cohort1960_c$ equals one if the individual was born in 1960 and zero if born in 1961-1962; $Semester1_s$ equals one if born from January to June, zero from July to December; $After2012_t$ equals one for observations from year 2012 onwards; $year_t$ are year fixed effects and $province_p$ are province fixed effects. Employment status is measured by 3 dependent binary variables:

$y_{i,t} = (employed, subsidy, unemployed, out\ of\ the\ labour\ market)$ where $employed$ equals one if the individual is employed, and zero otherwise; $subsidy$ equals one in the individual is receiving the long-term unemployed subsidy and zero otherwise; $unemployed$ equals one if the individual is unemployed receiving other subsidy or benefit, and zero otherwise; *out of the labour market* equals one if the individual is neither working nor receiving any unemployment subsidy or benefit. Note that these four employment status are mutually exclusive. Each equation is estimated separately through a Linear Probability Model (LPM) with standard errors clustered at province level (50 provinces).

Our main parameter of interest is β_7, which aims to measure the DDD impact of the LTU reform on employment status. It reflects whether the double difference between those born before and after July, in cohort 1960 as compared to those born in cohort 1961-62; diverges after the reform (from 2012 onwards). That is, it captures the variation in employment status that is specific to those born in the first semester (relative to those born in the second semester), in the cohort 1960 (as compared to cohort 1961-62), after the reform. If the reform exogenously affected the probability of receiving the LTU subsidy, β_7 should come out as statistically significant.

3.2. Physical and mental health

In order to explore the effects of the reform on health outcomes we use two administrative datasets (hospitalization records and primary care data) and one survey (SHARE).

First, we use registered data of all hospitalizations that occurred in Spanish hospitals between 2009 and 2014, as published by the Spanish Statistical Office (*Instituto Nacional de Estadística* – INE). Each hospitalization includes information on the date of birth, gender, province of residence and main diagnoses, following the International Classification of Diseases (ICD-9-CM). As before, we use the subsample of hospitalizations of individual born in the years 1960 ("treated" cohort) and 1961 & 1962 ("control" cohort). In total, there are 852,577 registered hospitalizations. We aggregated them by province, cohort of birth, semester of birth and year. In order to estimate the effects we employ a similar DDD model:

$$Hospitalization\ rate_{p,c,s,t} = \beta_1 + \beta_1 Cohort1960_c + \beta_2 Semester1_s + \beta_3 After2012_t + \beta_4 (Cohort1960_c\ x\ Semester1_s) + \beta_5 (Cohort1960_c\ x\ After2012_t) + \beta_6 (Semester1_s x\ After2012_t) + \beta_7 (Cohort1960_c\ x\ Semester1_s\ x\ After2012_t\ ) + year_t + province_p$$

[Equation 2]

Independent variables are similar to those of Equation 1. We collapse the data by province, cohort, semester of birth and year and we create a dependent variable, $Hospitalization\ rate$ that measures the number of hospitalizations per 1,000 individuals of each cohort (c) and semester of birth (s) for each province (p) and year (t), during the period 2009-2014. We use linear models weighting by province population, with standard errors clustered at the province level. Hospitalizations were grouped by main diagnoses using the International Classification of Diseases (ICD-9), choosing the categories for which we could expect an effect of the LTU subsidy (Mental health and Injuries), as well as the most prevalent ones: Digestive, Musculoskeletal, Circulatory; and lastly cancer, which was used as a placebo (Table 2).

Again, β_7 is our parameter of interest, which intends to measure the effect of the reform on hospitalizations. In particular, it measures how hospitalizations evolved after the reform among those who were eligible to the subsidy (born in the first semester of 1960); as compared to those who were not; using as control groups both those born in the 2nd semester of 1960, and those from 1961-62 cohort. In this case, β_7 represents the so-called "intention to treat" and not the actual "treatment effect" of receiving the LTU subsidy, since we do not have data on the employment status of hospitalized individuals.

Next, we focus on the effect of the LTU subsidy on mental health outcomes by looking at a less extreme indicator than hospitalizations. For that, we use data from mental health diagnoses at primary care centers. Data comes from a representative sample of registered primary care clinical data from the Spanish Minister of Health (*Base de datos clínicos Atención Primaria – BDCAP*). BDCAP includes data on the health conditions diagnosed at primary care facilities. Health conditions were classified by the International Classification of Primary Care

- 2nd Edition (CIAP2). As before, we use the subsample of individuals born between 1960 and 1962 from regions who reported data prior to the reform (Aragon, Balearic Islands, Canary Islands, Catalonia, Galicia and Basque Country); which represent around 37% of the Spanish population. Our final sample is formed by 37,690 individuals from 2011 to 2014, making a total of 150,760 person-year observations.

Similarly to Equation 2, the effect of LTU subsidy on mental health diagnoses at primary care can be determined by:

$$Mental\ health_{i,t} = \beta_0 + \beta_1 Cohort1960_c + \beta_2 Semester1_s + \beta_3 After2012_t + \beta_4(Cohort1960_c\ x\ Semester1_s) + \beta_5(Cohort1960_c\ x\ After2012_t) + \beta_6(Semester1_s x\ After2012_t) + \beta_7(Cohort1960_c\ x\ Semester1_s\ x\ After2012_t) + year_t + \alpha_i$$

[Equation 3]

The dependent variable $Mental\ health_{i,t,h}$ equals 1 is individual *i* has been diagnosed from a mental health condition at year *t*, and zero otherwise. The model is estimated by a LPM with individual fixed effects (α_i). We use sampling weights as provided by the BDCAP, to make the sample representative at regional level.

Thirdly, we use data from the Survey of Health Ageing and Retirement in Europe (SHARE) to infer whether the LTU had an impact on other self-reported dimensions of mental health. SHARE is a multidisciplinary panel database with information on health and others socioeconomic variables of European individuals aged 50 or older, and representative at country level. In particular, we use the subsample for Spain, of individuals presented at wave 5 (2013) and wave 6 (2015) born in the period 1960-1962. With these restrictions we have a sample of 713 observations.[3]

[3] Note that previous waves cannot not be used, since those at the control group (cohort 1961-62) did not appear before because they were younger than 50 years old. Therefore, unlike the other datasets, we could not follow a triple difference strategy.

Unfortunately SHARE data was not available for the years before the reform[4]. Therefore, our identification strategy here relies only on post-reform data. We estimate a DiD model, comparing the double difference between semester and cohort of birth, as follows:

$$y_{i,t} = \beta_1 + \beta_1 Cohort1960_c + \beta_2 Semester1_s + \beta_3 (Cohort1960_c \, x \, Semester1_s) + year_t$$

[Equation 4]

where $Cohort1960_c$ equals one if individual was born in 1960, zero otherwise; $Semester1_s$ equals one if born from January to June, zero otherwise; $year_t$ are year fixed effects.

The dependent variable $y_{i,t}$ represents the following outcomes:

- *self-reported health status* varying from 1 (excellent) to 5 (poor)
- *euro-d depression scale* varying from 0 (not depressed) to 12 (very depressed)
- *any antidepressant weekly*, a binary variable indicating if the individual takes any antidepressant weekly.

In this case, our identification assumption is that there are no health differences between those born in the 1st and 2nd semester that are cohort-specific, other than those that could have been provoked by the reform. That is, in the absence of the reform, we should observe the same health differences between those born in the 1st and 2nd semester for cohort 1960, than for cohorts 1961-1962. Our parameter of interest is β_3. We estimate Equation 4 using different models, depending on the dependent variable: i) an ordered probit for *self-reported health status;* ii) a negative binomial model for *euro-d depression scale;* iii) LPM for *any antidepressant weekly.*

4- Results

a) Effect on labour market outcomes

The DDD results displayed in Table 1 show that there was, indeed, an increase in the probability of receiving the LTU subsidy by 3.6 pp for those eligible to the LTU subsidy (born in 1st semester of 1960) with respect to those affected by the minimum age rise (born in 2nd

[4] Respondents of SHARE have to be 50 years or older. The last wave before the reform was carried out at 2011 and therefore there was no observations for the control cohort (1961-1962), and few observations of the treated cohort (1960).

semester of 1960). Such increase is higher for men (4.2 pp) than for women (2.9 pp). Simultaneously, the probability of being out of the labour market decreased by 2.4 pp for men, and by 2.3 pp for women eligible to the LTU subsidy. However, the probability of working decreased by 2.1 pp for men, but not for women. Lastly, the reform did not affect the probability of being unemployed (with other subsidy or benefit). These results show that the reform had a stronger effect in the case of men and that the most prevalent alternative to the LTU subsidy is leaving the labour market for both sexes; and also employment but only for the case of men.

In Figure 1 we decompose the triple difference coefficient by including interactions of the cohort and semester of birth with each year dummy (Equation A1, Appendix) in an event-style model. Thus, the coefficients plotted represent the differences between the treated and control groups (defined by cohort and semester of birth) for every year before and after the reform. We set the year prior to the reform (2011) as the base category. If the trends in labour market outcomes are similar between the treated and control cohorts and the reform affected only treated individuals then the plotted coefficients should be significant only from 2012 onwards.

Because the law passed at July 2012 increased the minimum age to receive the subsidy from 52 to 55, one might think that those born in the 1st semester of 1960 could have lost their subsidy after the reform because they did not comply with the minimum age requirement any more. However, what we see in Figure 1 is that the increased probability of receiving the LTU subsidy for the eligible semester-cohort keeps relatively constant after the reform (around 4 pp for men and 3 pp for women). This means that those born in the 1st semester of 1960 who got their LTU benefit before the reform, did not lose it after the age rose, even though they did not comply with the new minimum age of 55 anymore.

b) Effect on hospitalizations

If we look at the descriptive statistics (Table 2) for the data on hospitalizations, we can see that before the reform, in 2011, the mean hospitalization rate from any diagnoses was 68 per thousand individuals. More specifically, for the case of mental health and injuries, hospitalization rates were 3 and 6.9 per thousand individuals, which represent around 4% and

10% of the total number of hospitalizations, respectively. It is worth noting the fact that the prevalence of injuries is almost double for men than for women.

Table 3 reports the results of the triple difference model for overall hospitalizations rates. We can see that, even though the coefficient is negative for both genders as well as for men alone, the effects are not significant. In order to explore the effects for the different diagnoses separately, in Figure 2 we plot the DDD coefficients for the main groups of diseases and gender. As it can be seen in the graph, the only group that has a significant negative coefficient is hospitalizations due to injuries for the case of men with a reduction of around 1 hospitalization per thousand individuals. This effect implies a reduction of injury hospitalization rates by 11% for men.

The coefficients for the other diagnoses are not significant for any of the two genders. Note that some of these types of diseases can be thought of as a placebo experiment as these two groups of individuals should be sufficiently similar so as to have similar hospitalization rates. For example, hospitalizations due to cancer should be unaffected by the reform and, thus, should show no significant difference between the treatment and control groups, which is exactly what we can observe in Figure 2.

In Figure 3 we plot the coefficients of an event-style model where we interact the cohort and semester of birth treatment with the year dummies, setting the 2011 year as the baseline category. We show the results, for men and women separately, for total hospitalizations as well as for the two main diseases of interest: mental health and injuries. These figures represent a good test for the existence of parallel trends between the treated and control groups before the implementation of the policy. At the same time, the graphs allow us to understand the dynamics of the effects of interest.

As we see in Figure 3, there is evidence of the parallel trend assumption being satisfied for both genders and for the three types of hospitalizations. Furthermore, for the case of injuries we can see that men that were eligible for the LTU subsidy experience a significant and constant reduction in hospitalizations during the three years after the reform. This reduction is driven by provinces where the rate of unemployment for those aged 50-55 years old was higher (i.e.: provinces that were more exposed to the LTU subsidy reform) (Figure A1, Appendix).

The same effect on injuries is, however, not observed for women. This is consistent with the fact that the effect of the reform on the probability of receiving the subsidy is larger for men than for women. Also, men who were not eligible to the LTU subsidy increased their probability of employment, whereas women did not. Furthermore, descriptively we can see that men receiving the LTU subsidy come in larger proportions from the construction sector than women do (31.6% for men versus 2.3% for women). It is well known that jobs in the construction sector involve many more physically demanding activities. Finally, we have also seen in the descriptive evidence that a higher proportion of men receiving the LTU subsidy have not completed primary education (40,7%) than it is the case for women (26,8%). This fact will most likely have an impact on the occupational safety of the potential jobs that they can access.

We can see that the results for the mental health hospitalizations are not significant for any of the two genders, although there is a significant reduction of mental health hospitalizations for men eligible to the LTU subsidy, only in provinces more exposed to the reform (Figure A1, Appendix). However, it is important to remember that hospitalizations represent an extreme outcome in terms of mental health diseases. Thus, it is not surprising that the reform did not have an impact on this extreme mental health outcome. In the next section, we explore the effects on less extreme mental health outcomes such as diagnoses from registered primary care data as well as self-reported mental health data from SHARE.

c) Effect on primary care diagnoses

In Figure 4 we use registry primary care data to show the evolution in the probability of being diagnosed with a mental health condition for individuals born in the first and second semester for the “treated” 1960 cohort (Panel A) as well as for the additional control cohorts born in 1961 and 62 (Panel B). The incidence of mental health diagnosis is around 6% during this period for the cohorts under analysis. If the reform on the access to the LTU subsidy affected the probability of having a mental health condition we should see a differential change by semester of birth in the probability of being diagnosed after the reform but only for the 1960 cohort and not for the 1961-62 cohorts.

We can see in Panel A that, for the 1960 cohort there is a drop in mental health diagnosis for individuals born in the first semester (eligible for the LTU subsidy), as compared to those born on the second semester (not eligible for the LTU subsidy). Consistent with our hypothesis, for the unaffected 1961-62 cohort, individuals born in both semesters follow exactly the same trend before and after the reform. By looking at Figure A2 in the appendix we can see that the drop in mental health conditions is mostly coming from men born in the 1st semester of the affected cohort.

When we estimate the triple difference model, we can see in Table 4 that the coefficient is negative for the entire sample as well as for men but it is only significant for the case of men. Thus, men who were eligible for the LTU subsidy experienced a significant drop by 2 percentage points in their probability of being diagnosed with a mental health condition at a primary care centre. We find no significant effect for women.

c) Effect on other health outcomes – SHARE data

Table 5 reports the results for the self-reported health outcomes using SHARE data. The first three columns correspond to an ordered probit model for self-reported health ranging from 1, excellent, to 5, poor. We can see that the interaction coefficient shows a negative and significant coefficient for the case of men pointing towards men eligible for the LTU subsidy reporting better self-reported health status.

Columns 4 to 6 show the results of a negative binomial model for euro-depression scale as the dependent variable. Again, we can see that, for this more objective mental health outcome, men eligible for the LTU subsidy report having better mental health outcomes. . Similar to the other outcomes analysed in the paper, we observe no significant differences for women.

Finally, with respect to the consumption of antidepressant medication (columns 7 to 9), we can see that the coefficient for men is also negative although not significant. It is important to highlight that the years available in the SHARE data are all included in the post reform period. Thus, the assumption in this case is basically that the differences in mental health outcomes between individuals born in the 1st and 2nd semester should be similar in the cohort of 1960 (treatment) as in the cohorts of 1961 and 1962 (controls) in the absence of the reform. Therefore, any differences between individuals born in the two semesters from the 1960 and the 1961-1962 are attributable to the reform. In order to provide support for the fulfilment of this assumption, we do several things. First, we run placebo estimates using data from the waves before the reform is introduced (wave 1 in 2004 and wave 2 in 2007) and estimate the same model (Table A5 in the appendix). In this placebo models we compare individuals aged 53 in wave 1 (used as the fake treatment group) and individuals aged 51 and 52 (used as control group). None of the placebo models result in significant coefficients for men. Thus, we are confident about the identification assumption and believe that the placebo results provide evidence that the differential effects for individuals born in the 1st semester for the 1960 cohort arise as a result of the reform.

6- Discussion

This paper studies the effects of a reform that increased the age required to get access to a long term unemployment subsidy (from 52 to 55 years old) which was introduced in Spain in July 2012. We focus on the impacts on several health outcomes such as hospitalizations, mental health diagnosis and self-reported health measures. In order to do that we combine several data sources using a combination of register and survey data.

The LTU subsidy is aimed at a specific part of the population which is disadvantaged in several dimensions; they have been unemployed for a long time, have low education and low socioeconomic status and have a background of relatively poor employment and social conditions.

In order to identify the effects, we exploit the fact that only individuals born in the 1st semester of the 1960 cohort had access to the subsidy in 2012. On the contrary, individuals born in the 2nd semester of the 1960 cohort were not eligible for the subsidy until 2015, when they turned 55 years old. Thus, we estimate a triple difference model comparing individuals born in the first versus second semester of affected (1960) and unaffected cohorts (1961 and 1962) before and after the reform.

We first show that that the reform had an effect on the probability of receiving the LTU subsidy. Those unaffected by the reform (i.e.: eligible for LTU subsidy) had a higher probability of receiving the LTU subsidy (4.2 pp for men, and 2.9 pp for women) from 2012 to 2014. We next turn to the effects on health outcomes and we report no significant effects of the reform on the overall number of hospitalizations. However, disaggregating by disease and gender we find that men from the semester-cohort eligible for the LTU subsidy reduced their hospitalizations due to injuries by 11.7%. As explained above, the alternative job for these disadvantage men is likely to be physically demanding and with a higher risk of suffering from accidents while working.

We find no effect for mental health hospitalizations, which is in line with our predictions as hospitalizations represents a very extreme outcome for mental health problems.

When we look at mental health diagnosis we show that men who were eligible for a LTU subsidy show reduced probabilities of being diagnosed after the reform. Again, no significant effect is reported for women, who were less exposed to the reform (i.e.: less likely to receive a LTU subsidy). Finally, using survey data we find that those men eligible for the survey show better self-reported health status and lower euro-d depression score results.

The paper has some limitations that are worth mentioning. First of all, our health results rely on ITT estimates because we are not able to identify the labour market status of the individuals. The probability of “treatment" (i.e.: LTU subsidy reception) among the “treated” cohort-semester of birth after the reform is around 4% (4.4% for men and 3.3% for women); so the real “exposure” to the reform of the treated cohort-semester is low. Therefore, for a significant effect on the ITT to show up, the real treatment effect on the treated must be much larger. Nonetheless, we expect that those receiving the LTU subsidy account for more than 4% of the pool of hospitalizations because we have shown that they have several socioeconomic and labour market disadvantages (low education, long-term unemployment and low occupation jobs with higher risks for health). The literature has shown that those disadvantages are closely linked to worse health outcomes. Furthermore, some of our estimates with survey data might be underpowered due to small samples.

Summing up, this research shows the impacts of a LTU benefit on the health of middle age workers who are in a disadvantaged position in the labour market. We believe that our results have important policy implications in the current discussions on the deaths of despair phenomenon that has been shown for several developed countries. More specifically, we show that disadvantaged middle aged men may benefit in terms of better physical and mental health outcomes from receiving a LTU subsidy instead of remaining in the labour force with a physically demanding and risky job.

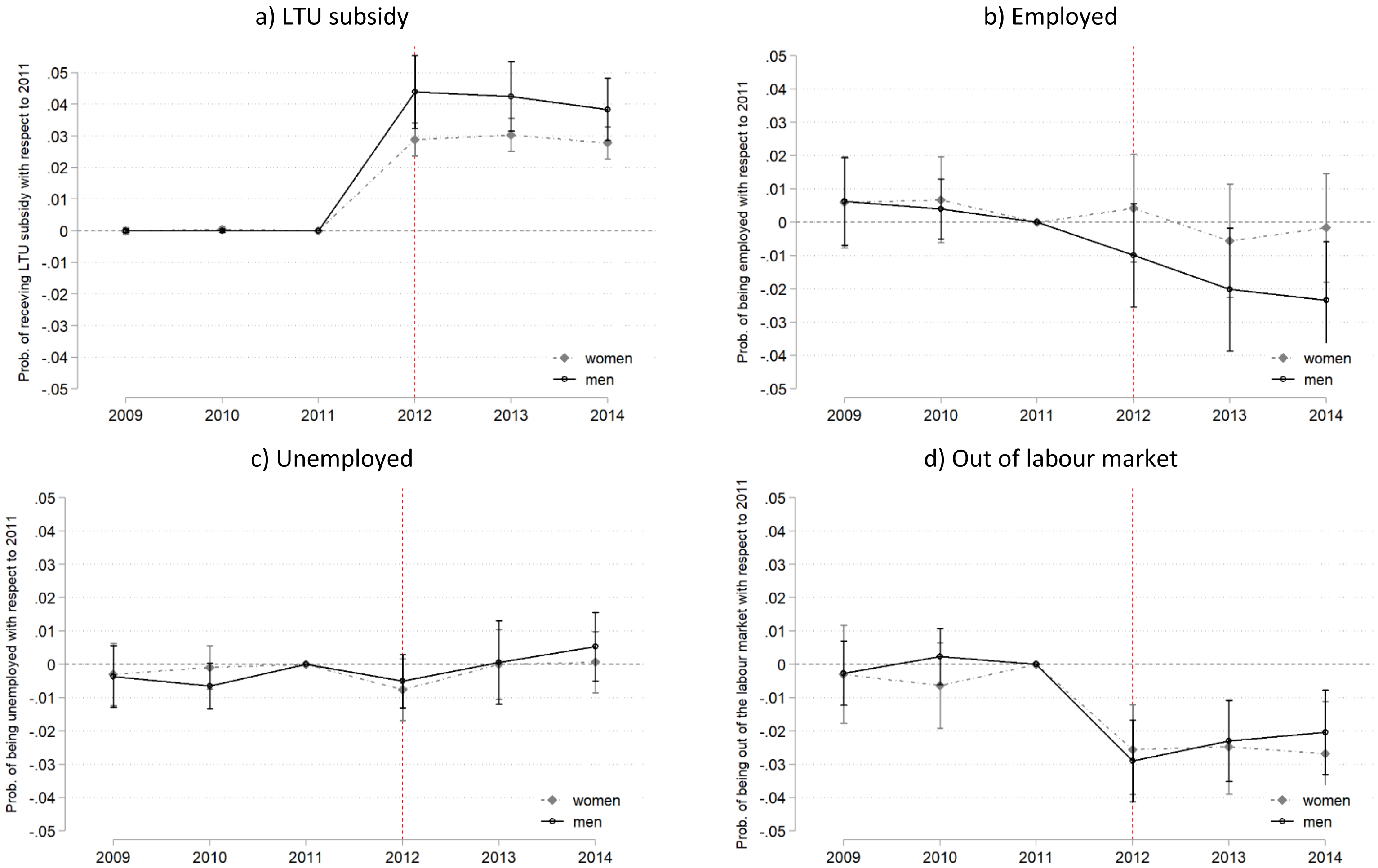
Tables and Figures

Table 1- Triple difference (DDD) model for employment status: Linear probability model (LPM).

	Both sexes				Men				Women			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
VARIABLES	Subsidy	Employed	Unemp.	Out LM	Subsidy	Employed	Unemp.	Out LM	Subsidy	Employed	Unemp.	Out LM
Cohort 1960 (A)	0.000	-0.000	-0.001	0.001	-0.000	-0.005	0.002	0.003	0.001	0.005	-0.006	0.000
	(0.000)	(0.005)	(0.002)	(0.004)	(0.000)	(0.006)	(0.003)	(0.005)	(0.001)	(0.006)	(0.003)	(0.006)
Semester 1 (B)	0.000	-0.007**	0.003**	0.004	-0.000	-0.010***	0.004*	0.007**	0.001	-0.004	0.002	0.002
	(0.000)	(0.003)	(0.001)	(0.002)	(0.000)	(0.003)	(0.002)	(0.003)	(0.001)	(0.005)	(0.002)	(0.005)
Post2012 (C)	-0.000	-0.111***	-0.022***	0.133***	-0.000	-0.119***	-0.029***	0.149***	0.000	-0.100***	-0.013***	0.113***
	(0.000)	(0.004)	(0.002)	(0.004)	(0.000)	(0.005)	(0.003)	(0.005)	(0.000)	(0.004)	(0.002)	(0.004)
A x B	0.000	-0.002	-0.000	0.001	0.000	0.005	-0.003	-0.003	0.000	-0.008	0.003	0.005
	(0.000)	(0.005)	(0.002)	(0.004)	(0.000)	(0.007)	(0.004)	(0.006)	(0.001)	(0.006)	(0.003)	(0.007)
A x C	0.002***	-0.007*	0.001	0.004	0.002***	-0.005	-0.001	0.004	0.002***	-0.010*	0.005	0.004
	(0.001)	(0.004)	(0.002)	(0.003)	(0.001)	(0.005)	(0.003)	(0.005)	(0.001)	(0.005)	(0.004)	(0.004)
B x C	0.000	0.002	-0.005***	0.002	0.000	-0.000	-0.005**	0.005	0.000	0.006	-0.005**	-0.001
	(0.000)	(0.004)	(0.002)	(0.003)	(0.000)	(0.004)	(0.003)	(0.004)	(0.000)	(0.005)	(0.002)	(0.005)
DDD Coefficient (AxBxC)	0.036***	-0.014**	0.002	-0.024***	0.042***	-0.021**	0.004	-0.024***	0.029***	-0.005	-0.001	-0.023***
	(0.003)	(0.006)	(0.002)	(0.005)	(0.005)	(0.008)	(0.005)	(0.006)	(0.003)	(0.007)	(0.004)	(0.007)
Constant	0.001***	0.809***	0.054***	0.137***	0.001*	0.857***	0.054***	0.087***	0.001***	0.746***	0.053***	0.200***
	(0.000)	(0.004)	(0.002)	(0.004)	(0.001)	(0.003)	(0.002)	(0.004)	(0.000)	(0.005)	(0.002)	(0.005)
Observations	377,299	377,299	377,299	377,299	207,082	207,082	207,082	207,082	170,217	170,217	170,217	170,217
R-squared	0.029	0.020	0.002	0.022	0.037	0.024	0.003	0.028	0.023	0.022	0.002	0.023
Region FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES

Clustered standard errors at province level in parentheses *** p<0.01, ** p<0.05, * p<0.1, Columns (1), (5) and (9) LPM with dependent variable equal one for those receiving the LTU subsidy, zero otherwise. Columns (2), (6) and (10) LPM with dependent variable equal one for those employed, zero otherwise. Columns (3), (7) and (11) LPM with dependent variable equal one for those unemployed (receiving other subsidy or benefit), zero otherwise. Columns (4), (8) and (12) LPM with dependent variable equal one for those out of labour market (neither working nor receiving any unemployment benefit or subsidy), zero otherwise.

Figure 1- DDD estimates for employment status over time.



NOTES: These figures plot the coefficients of the interactions between the double difference cohort-semester and the year dummies, which results from the decomposition the DDD coefficient for employment status, as explained in Equation A1 of Appendix

Table 2- Summary Statistics: Mean hospitalization rates (per 1,000 individuals) at year 2011.

	Mean (S.E.)		
	All	Men	Women
All hospitalizations	68.79	73.60	63.78
	(0.60)	(0.76)	(0.64)
Mental Health (ICD = 5)	3.01	3.22	2.78
	(0.09)	(0.11)	(0.12)
Injuries (ICD = 17)	6.90	8.79	4.94
	(0.11)	(0.17)	(0.12)
Digestive (ICD = 9)	10.85	13.45	8.13
	(0.14)	(0.21)	(0.16)
Musculoskeletal (ICD = 13)	7.99	8.45	7.51
	(0.16)	(0.21)	(0.17)
Circulatory (ICD = 7)	7.13	9.51	4.66
	(0.12)	(0.18)	(0.13)
Cancer (ICD = 2)	9.14	6.66	11.71
	(0.14)	(0.16)	(0.19)
Observations	300	300	300

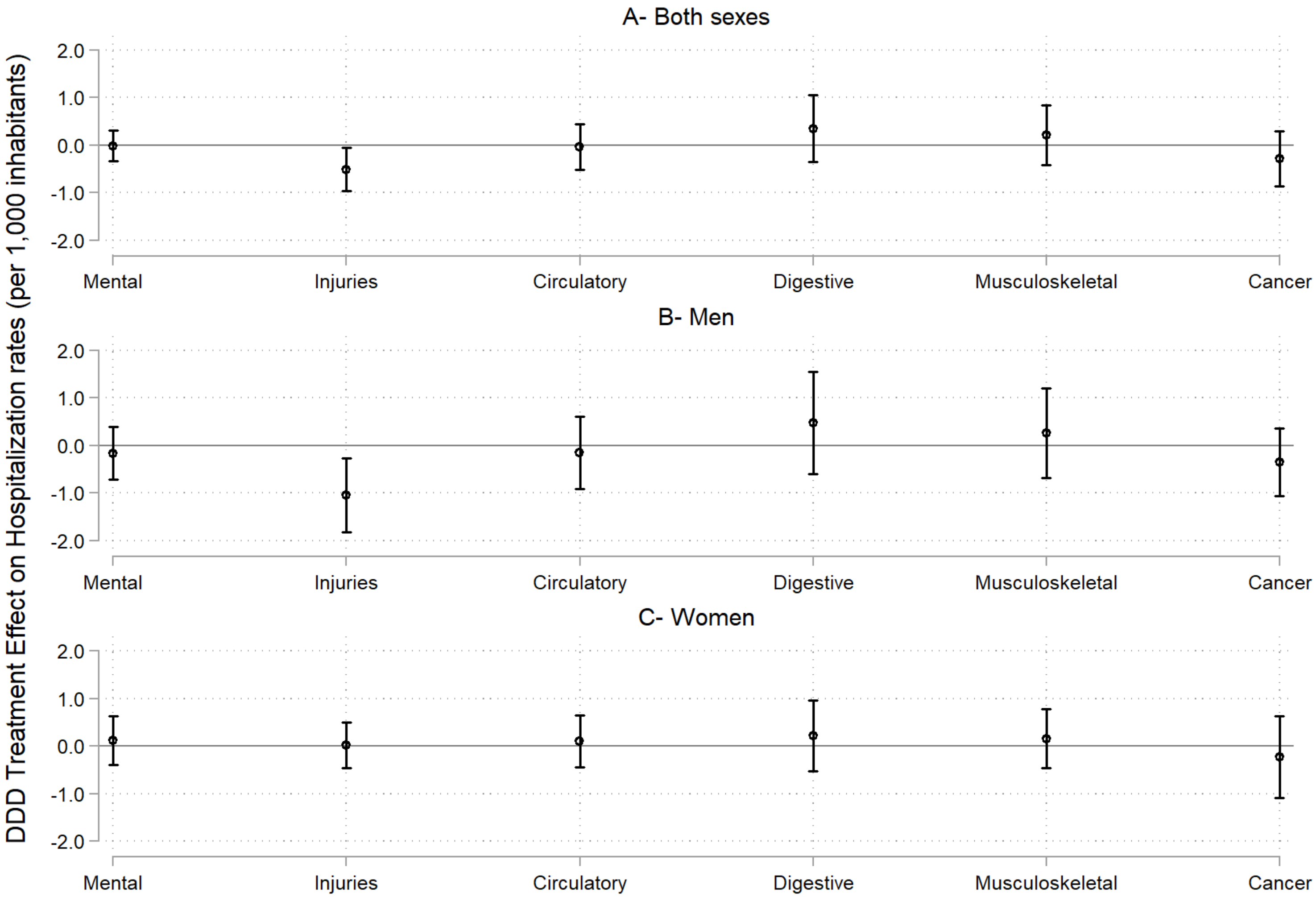
NOTES: This table reports the mean hospitalization rate (per 1,000 inhabitants) by cohort, and semester of birth, at province level, for the year prior to the reform (2011), for individuals born in the years 1960-1962.

Table 3- Triple difference (DDD) model: Hospitalizations rates.

	All hospitalizations		
	(1)	(2)	(3)
	All	Men	Women
Cohort 1960 (A)	3.19***	4.18***	2.19***
	(0.47)	(0.74)	(0.67)
Semester 1 (B)	-0.33	-0.20	-0.48
	(0.45)	(0.60)	(0.54)
Post2012 (C)	11.18***	15.98***	6.44***
	(0.78)	(0.88)	(0.97)
A x B	3.18***	4.27***	2.09**
	(0.79)	(1.18)	(0.89)
A x C	0.68	1.56	-0.14
	(0.46)	(0.95)	(0.69)
B x C	0.42	0.32	0.53
	(0.43)	(0.77)	(0.48)
DDD Coefficient (A x B x C)	-0.25	-1.30	0.76
	(0.71)	(1.37)	(1.14)
Mean Y (at 2011)	68.79	73.60	63.78
	(0.60)	(0.76)	(0.64)
DDD effect (% over Mean Y)	**-0.4%**	**-1.8%**	**1.2%**
Observations	1,800	1,800	1,800
Year FE	YES	YES	YES
Province FE	YES	YES	YES

NOTES: Robust standard errors clustered at province level in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Figure 2- DDD estimates per group of disease of main diagnosis (ICD-9).



NOTES: Coefficients come from running the model of Equation 2 separately per ICD-9 diagnosis group and sex.

Figure 3- Impact of LTU subsidy over time on hospitalizations rates (per 1,000 inhabitants).

NOTES: These figures plot the coefficients of the interactions between the double difference cohort-semester and the year dummies, which results from the decomposition the DDD coefficient for employmen status, as explained in Equation A2 of Appendix

Figure 4- Probability of mental health diagnosis by semester of birth (1960 vs 1961-62 cohort).

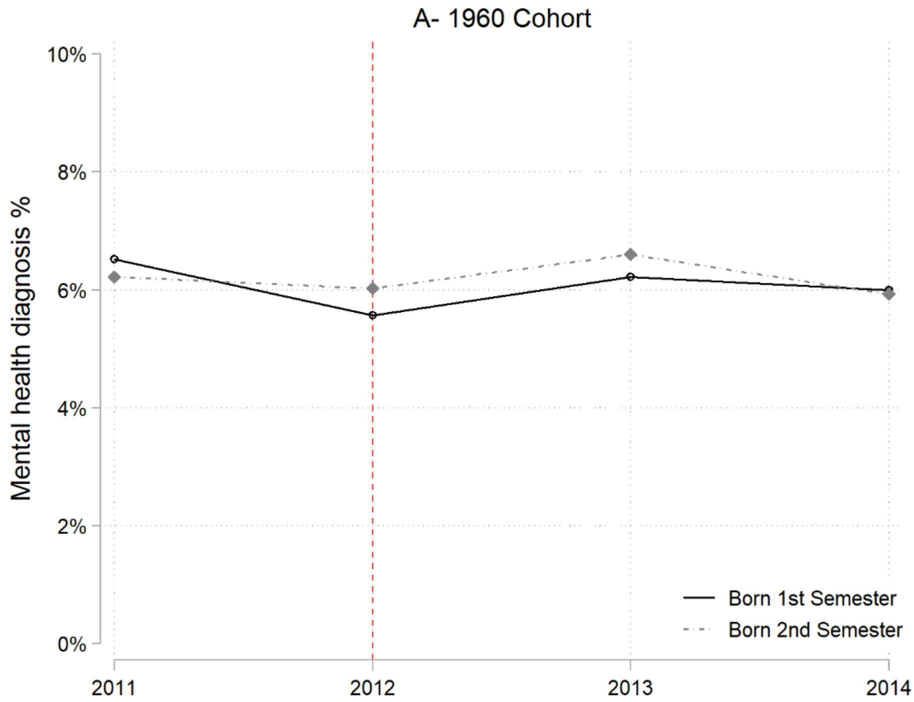


Table 4- Triple difference (DDD) model: Mental health diagnosis probability.

	Any mental health diagnosis		
	(1)	(2)	(3)
	All	Men	Women
Cohort 1960 (A)	-0.0995	-0.246	0.0847
	(0.0874)	(0.158)	(0.0780)
Semester 1 (B)	-0.0235	-0.0303	-0.0898
	(0.0400)	(0.0221)	(0.0848)
Post2012 (C)	0.00894**	0.00453	0.0134**
	(0.00354)	(0.00479)	(0.00521)
A x B	0.130	0.195	0.121
	(0.0868)	(0.132)	(0.106)
A x C	0.00349	0.00991	-0.00305
	(0.00535)	(0.00722)	(0.00792)
B x C	-0.00102	0.00492	-0.00706
	(0.00456)	(0.00611)	(0.00678)
DDD Coefficient (A x B x C)	-0.00674	-0.0203**	0.00736
	(0.00763)	(0.0102)	(0.0114)
Observations	150,759	76,814	73,945
Year FE	YES	YES	YES
Region FE	YES	YES	YES

Standard errors clustered at individual level in parentheses, *** p<0.01, ** p<0.05, * p<0.1

Table 5- Difference in difference (DiD) model SHARE data (Waves 5 [2013] and 6 [2015]): Self-reported health outcomes.

	Self-reported Health Status[a]			Euro-d scale[b]			Any antidepressant weekly[c]		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All	Men	Women	All	Men	Women	All	Men	Women
Cohort 1960	-0.0465	0.243	-0.217	-0.424*	0.214	-0.559*	-0.00690	0.0286	-0.00970
(Base category: Cohort 1961-62)	(0.127)	(0.214)	(0.160)	(0.242)	(0.355)	(0.316)	(0.0263)	(0.0198)	(0.0393)
Semester 1	-0.0420	0.260	-0.184	-0.241	0.508	-0.537*	0.00336	0.0263	0.00120
	(0.111)	(0.192)	(0.138)	(0.223)	(0.321)	(0.297)	(0.0254)	(0.0193)	(0.0358)
Cohort 1960 x Semester 1	-0.0254	-0.452*	0.221	0.108	-0.762*	0.547	0.00912	-0.0310	0.0202
	(0.162)	(0.267)	(0.206)	(0.349)	(0.404)	(0.519)	(0.0361)	(0.0325)	(0.0542)
Observations	712	272	440	681	256	425	713	273	440
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES

NOTES: D Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.[a] Columns (1) (2) (3) report coefficients from ordered probit with the dependent variable being self-reported-health status (=1 excellent, =2 very good, =3 good, =4 fair, =5 poor). [b] Columns (4) (5) (6) report marginal effects from the negative binomial model with euro-d depression scale as dependent variable. Euro-d varies from 0 (not depressed) to 12 (very depressed). [c]Columns (7) (8) (9) report coefficients from the LPM with a binary dependent variable indicating if the individual is taking antidepressants at least weekly.

References

Angrist, J. D., & Keueger, A. B. (1991). Does compulsory school attendance affect schooling and earnings? *The Quarterly Journal of Economics*, *106*(4), 979–1014.

Apouey, B., & Clark, A. E. (2015). Winning big but feeling no better? the effect of lottery prizes on physical and mental health. *Health Economics (United Kingdom)*. https://doi.org/10.1002/hec.3035

Bloemen, H., Hochguertel, S., & Zweerink, J. (2017). The causal effect of retirement on mortality: Evidence from targeted incentives to retire early. *Health Economics (United Kingdom)*. https://doi.org/10.1002/hec.3493

Browning, M., Dano, A. M., & Heinesen, E. (2006). Job displacement and stress-related health outcomes. *Health Economics*. https://doi.org/10.1002/hec.1101

Browning, M., & Heinesen, E. (2012). Effect of job loss due to plant closure on mortality and hospitalization. *Journal of Health Economics*. https://doi.org/10.1016/j.jhealeco.2012.03.001

Buckles, K. S., & Hungerman, D. M. (2013). Season of birth and later outcomes: Old questions, new answers. *Review of Economics and Statistics*. https://doi.org/10.1162/REST_a_00314

Case, A., & Deaton, A. (2017). Mortality and morbidity in the 21st century. *Brookings Papers on Economic Activity*.

Case, A., Fertig, A., & Paxson, C. (2005). The lasting impact of childhood health and circumstance. *Journal of Health Economics*. https://doi.org/10.1016/j.jhealeco.2004.09.008

Clark, A. E. (2003). Unemployment as a Social Norm: Psychological Evidence from Panel Data. *Journal of Labor Economics*. https://doi.org/10.1086/345560

Costa, D. L., & Lahey, J. N. (2005). Predicting older age mortality trends. *Journal of the European Economic Association*, *3*(2–3), 487–493.

Cutler, D. M., & Lleras-Muney, A. (2006). *Education and health: evaluating theories and evidence*. National bureau of economic research.

Eliason, M., & Storrie, D. (2009). Does job loss shorten life? *Journal of Human Resources*, *44*(2), 277–302.

Fitzpatrick, M. D., & Moore, T. J. (2018). The mortality effects of retirement: Evidence from Social Security eligibility at age 62. *Journal of Public Economics*. https://doi.org/10.1016/j.jpubeco.2017.12.001

Hallberg, D., Johansson, P., & Josephson, M. (2015). Is an early retirement offer good for your health? Quasi-experimental evidence from the army. *Journal of Health Economics*. https://doi.org/10.1016/j.jhealeco.2015.09.006

Kassenboehmer, S. C., & Haisken-DeNew, J. P. (2009). You’re fired! the causal negative effect of entry unemployment on life satisfaction. *Economic Journal*. https://doi.org/10.1111/j.1468-0297.2008.02246.x

Kuhn, A., Staubli, S., Wuellrich, J.-P., & Zweimüller, J. (2018). *Fatal Attraction? Extended Unemployment Benefits, Labor Force Exits, and Mortality*. https://doi.org/10.3386/w25124

Marmot, M. (2005). Social determinants of health inequalities. *The Lancet*, *365*(9464), 1099–1104.

Obrero, C. B., Martín, S. J., & Castello, J. V. (2019). Education and Gender Differences in Mortality Rates. *Documentos de Trabajo (FEDEA)*, (5), 1–44.

Ravesteijn, B., van Kippersluis, H., & van Doorslaer, E. (2018). The wear and tear on health: What is the role of occupation? *Health Economics (United Kingdom)*. https://doi.org/10.1002/hec.3563

Rietveld, C. A., & Webbink, D. (2016). On the genetic bias of the quarter of birth instrument. *Economics & Human Biology*, *21*, 137–146.

Schaller, J., & Stevens, A. H. (2015). Short-run effects of job loss on health conditions, health insurance, and health care utilization. *Journal of Health Economics*. https://doi.org/10.1016/j.jhealeco.2015.07.003

van Ours, J. C. (2019). Health Economics of the Workplace: Workplace Accidents and Effects of Job Loss and Retirement. In *Oxford Research Encyclopedia of Economics and Finance*.

Winkelmann, L., & Winkelmann, R. (1998). Why are the unemployed so unhappy? Evidence from panel data. *Economica*. https://doi.org/10.1111/1468-0335.00111

APPENDIX

1- DDD effect over time for employment status

Coefficients plotted in Figure 1 represent γ_t in the following equation, which decomposes the DDD coefficient into interactions bettwen the double difference cohort-semester and the year dummies, setting 2011 as the reference category:

$$y_{i,t} = \beta_0 + \beta_1 Cohort1960_c + \beta_2 Semester1_s + \beta_3 After2012_t + \beta_4(Cohort1960_c \; x \; Semester1_s) + \beta_5(Cohort1960_c \; x \; After2012_t) + \beta_6(Semester1_s x \; After2012_t) + \sum_{t=2009}^{2014} \gamma_t \, (year_t \; x \; Cohort1969_c \; x \; Semester1_s) + year_t + province_p$$

[Equation A1]

All variables above are similar to those explained in Equation 1 of the main manuscript.

2- DDD effect over time for hospitalizations rates

Coefficients plotted in Figure 1 represent γ_t in the following equation, which decomposes the DDD coefficient into interactions between the double difference cohort-semester and the year dummies, setting 2011 as the reference category:

$$Hospitalization\ rate_{p,c,s,t} = \beta_0 + \beta_1 Cohort1960_c + \beta_2 Semester1_s + \beta_3 After2012_t + \beta_4(Cohort1960_c \; x \; Semester1_s) + \beta_5(Cohort1960_c \; x \; After2012_t) + \beta_6(Semester1_s x \; After2012_t) + \sum_{t=2009}^{2014} \gamma_t \, (year_t \; x \; Cohort1969_c \; x \; Semester1_s) + year_t + province_p$$

[Equation A2]

All variables above are similar to those explained in Equation 2 of the main manuscript.

Table A1- Age-adjusted probability of retirement per employment status.

	Probability of retirement
Employed	0.084
	(0.0004)
Unemployed	0.073
	(0.0007)
LTU subsidy	0.173
	(0.0016)
Observations	372,105

NOTES: Standard errors in parentheses. Predicted probabilities from a logit model with a binary variable indicating if the next registry of the individual was retirement, with employment status and age as explanatory variables. MCVL subsample of individuals older than 52 years old for the period 2008-2011 (before the reform).

Table A2- Labour market record by employment status of those born in 1st semester of 1960

	Employed	**Unemployed**	**LTU subsidy**
Panel A: Men			
Months employed 2008-2011	45.3	28.1	19.8
Months unemployed 2008-2011	1.6	13.9	22.0
Number of contracts 2008-2011	1.8	3.4	4.4
Number of temporary contracts 2008-2011	0.5	2.4	3.6
Panel B: Women			
Months unemployed 2008-2011	42.8	23.0	16.5
Month employed 2008-2011	1.6	12.7	18.4
Number of contracts 2008-2011	2.5	3.8	3.1
Number of temporary contracts 2008-2011	1.1	2.9	2.1

NOTES: MCVL Subsample of individuals born in the first semester of 1960 (5,417 men and 4,143 women). The employment status corresponds to that recorded at 15th of November of 2012

Table A3 – Education level by employment status of those born in 1st semester of 1960.

	No education	**Primary**	**Secondary**	**Tertiary**	**n**
		Panel A: Both sexes			
Employed	21.6%	16.6%	48.3%	13.5%	7783
Unemployed	36.8%	19.3%	38.7%	5.2%	1090
LTU subsidy	35.6%	22.7%	39.1%	2.6%	427
		Panel B: Men			
Employed	22.3%	17.9%	47.2%	12.6%	4348
Unemployed	41.3%	19.9%	36.0%	2.9%	623
LTU subsidy	40.7%	21.1%	35.6%	2.6%	270
		Panel C: Women			
Employed	20.7%	15.0%	49.8%	14.6%	3435
Unemployed	30.8%	18.4%	42.4%	8.4%	467
LTU Subsidy	26.8%	25.5%	45.2%	2.5%	157
Total sample	25.5%	18.3%	45.2%	11.0%	9300

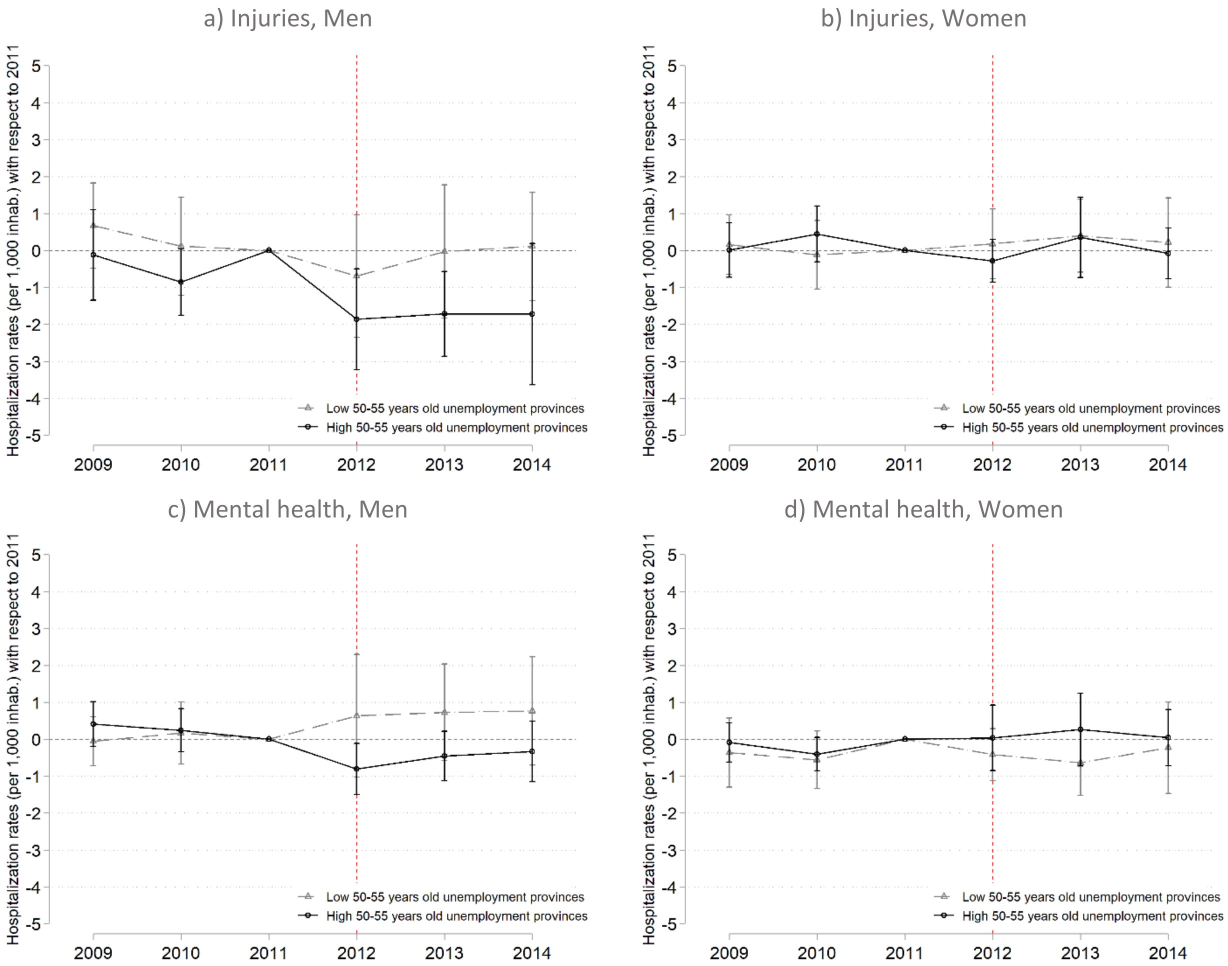
NOTES: MCVL Subsample of individuals born in the first semester of 1960, for which we had information on education level (9300 observations out of 9590). The employment status corresponds to that recorded at 15th of November of 2012

Table A4 – Economic sector (of last job) by employment status of those born in 1st semester of 1960.

	Primary	**Industry**	**Construction**	**Hostelry**	**Other Services**	**n**
			Panel A: Both sexes			
Employed	5.3%	12.4%	6.2%	7.3%	68.8%	7754
Unemployed	2.9%	12.2%	21.2%	11.7%	51.9%	996
LTU subsidy	3.7%	20.1%	21.7%	8.6%	46.0%	383
			Panel B: Men			
Employed	6.2%	16.5%	10.0%	7.0%	60.3%	4401
Unemployed	3.6%	15.2%	32.6%	7.8%	40.7%	604
LTU subsidy	4.7%	17.4%	31.6%	5.9%	40.3%	253
			Panel C: Women			
Employed	4.1%	7.1%	1.3%	7.8%	79.8%	3353
Unemployed	1.8%	7.7%	3.6%	17.9%	69.1%	392
LTU subsidy	1.5%	25.4%	2.3%	13.8%	56.9%	130
Total sample	5.9%	16.4%	13.6%	7.0%	57.1%	9133

NOTES: MCVL Subsample of individuals born in the first semester of 1960, for which we had information on economic sector of the last job (9133 observations out of 9590). The employment status corresponds to that recorded at 15th of November of 2012.

Figure A1- Impact of LTU subsidy over time on hospitalizations rates (per 1,000 inhabitants), high vs low 50-55 years old unemployment provinces.



NOTES: These figures plot the coefficients of the interactions between the double difference cohort-semester and the year dummies, which results from the decomposition the DDD coefficient for employment status, as explained in Equation A2 of Appendix

Figure A2- Probability of mental health diagnosis by semester of birth (1960 vs 1961-62 cohort), for men and women.

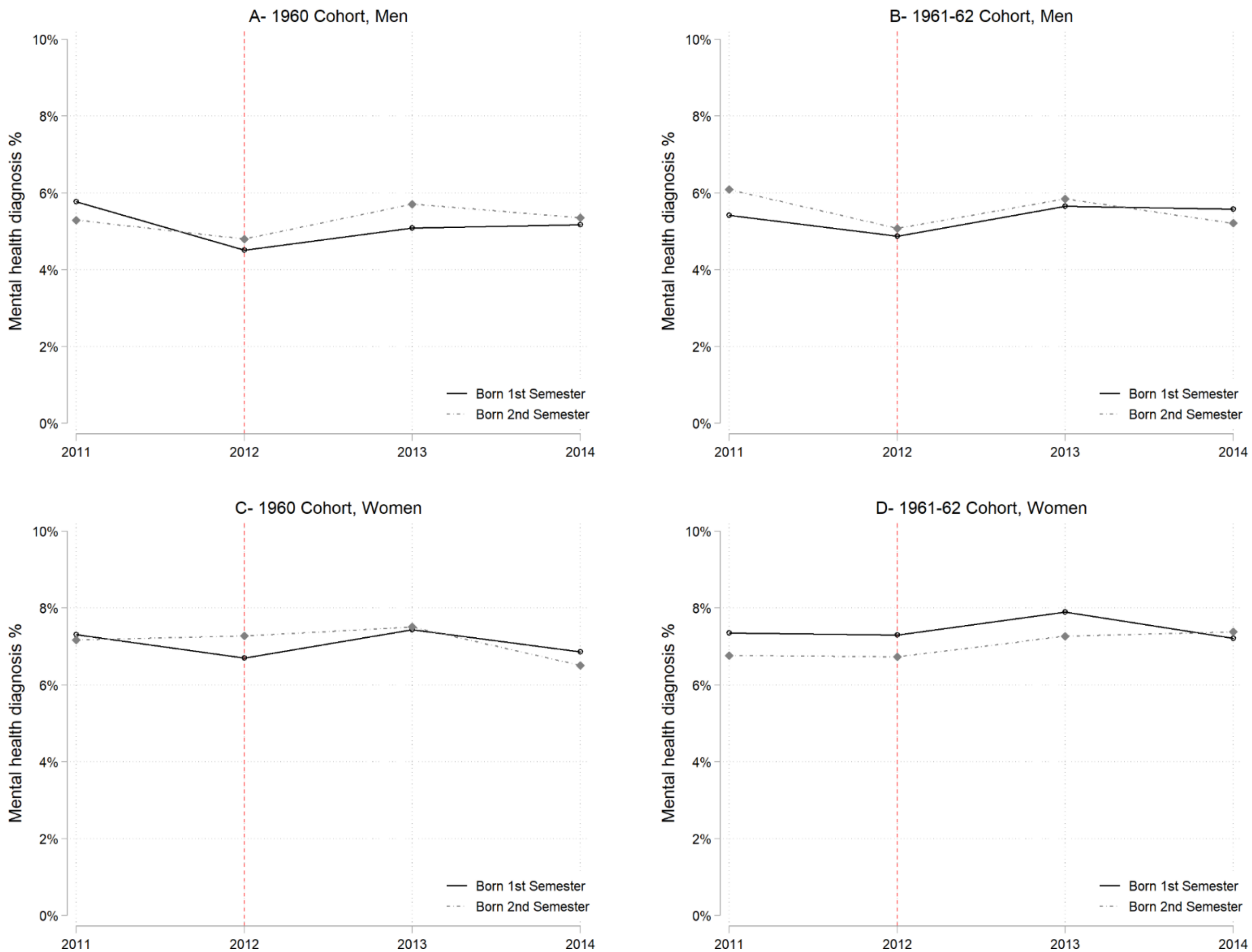


Table A5- SHARE Placebo (Waves 1 [2004-2005] & 2 [2007]): Difference in difference (DiD) mode for self-reported health outcomes.

	Self-reported Health Status[a]			Euro-d scale[b]			Any antidepressant weekly[c]		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	All	Men	Women	All	Men	Women	All	Men	Women
Cohort 1951	0.150	0.241	0.0834	0.203	0.301	0.162	-0.00268	0.0357	-0.0233
(Base category: Cohort 1952-53)	(0.156)	(0.258)	(0.198)	(0.132)	(0.246)	(0.148)	(0.0397)	(0.0522)	(0.0567)
Semester 1	0.364***	0.334	0.397**	0.165	0.253	0.160	0.0501	0.0321	0.0728
	(0.141)	(0.225)	(0.180)	(0.115)	(0.215)	(0.127)	(0.0362)	(0.0405)	(0.0551)
Cohort 1951 x Semester 1	-0.103	0.0495	-0.200	-0.222	-0.352	-0.177	-0.0285	-0.00208	-0.0623
	(0.238)	(0.361)	(0.322)	(0.179)	(0.332)	(0.199)	(0.0574)	(0.0766)	(0.0815)
Observations	450	193	257	450	193	257	450	193	257
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES

Robust standard errors in parentheses *** p<0.01, ** p<0.05, * p<0.1.[a] Columns (1) (2) (3) report coefficients from ordered probit with the dependent variable being self-reported-health status (=1 excellent, =2 very good, =3 good, =4 fair, =5 poor). [b] Columns (4) (5) (6) report marginal effects from the negative binomial model with euro-d depression scale as dependent variable. Euro-d varies from 0 (not depressed) to 12 (very depressed). [c]Columns (7) (8) (9) report coefficients from the LPM with a binary dependent variable indicating if the individual is taking antidepressants at least weekly