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LONG-TERM UNEMPLOYMENT SUBSIDIES AND MIDDLE-AGE DISADVANTAGED WORKERS' HEALTH [i]

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ABSTRACT: We estimate the labour market and health effects of a long-term unemployment (LTU) subsidy targeted to middle aged disadvantaged workers. In order to do so, we exploit a Spanish reform introduced in July 2012 that increased the age eligibility threshold to receive the subsidy from 52 to 55. Using a within-cohort identification strategy, we show that men ineligible for the subsidy were more likely to leave the labour force. In terms of health outcomes, although we do not report impacts on hospitalizations when considering the whole sample, we do find significant results when we separate the analysis by main diagnosis and gender. More specifically, we show a reduction by 12.9% in hospitalizations due to injuries as well as a drop by 2 percentage points in the probability of a mental health diagnosis for men who were eligible for the LTU subsidy. Our results highlight the role of long-term unemployment benefits as a protecting device for the health (both physical and mental) of middle aged, low educated men who are in a disadvantaged position in the labour market.

JEL Codes: I10, J65
Keywords: Disadvantaged workers, unemployment subsidies, health effects

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1- Introduction

Most of the research on the welfare effects of unemployment insurance have been focused on its labour market consequences (Schmieder and Von Wachter 2016), in particular, on the potential moral hazard effects of unemployment benefits in terms of discouraging labour supply. In that sense, the optimal unemployment insurance has been discussed to be that in which the costs, as measured by the moral hazard, equal the benefits, in terms of consumption smoothing (Chetty 2006). Still, there are other potential costs and benefits of unemployment insurance that are widely unexplored. If unemployment insurance produces externalities other than the direct effect on consumption, these should be taken into account when defining the right amount of unemployment subsidies and their access criteria.

While there is a well-established literature that shows that unemployment has detrimental effects on health, especially on mental health (Browning and Heinesen 2012; Cygan-Rehm, Kuehnle, and Oberfichtner 2017; Eliason and Storrie 2009; Farré, Fasani, and Mueller 2018; Schaller and Stevens 2015), it is far less clear how unemployment benefits might interact with this relationship. If unemployment benefits alleviate some of these detrimental health effects of unemployment, such positive externalities should be taken into account as potential benefits of unemployment insurance. On the contrary, if unemployment benefits prolong the jobless situation and consolidate the detrimental health effects of unemployment, those indirect costs should also be accounted for when assessing the current levels of unemployment benefits.

We provide evidence on the extent to which a subsidy for the long-term unemployed may affect the health of the recipients. We exploit a reform that was implemented in Spain in 2012 that created exogenous variation on the eligibility of midlife workers to access the subsidy. The reform increased the minimum age for gaining access to the long-term unemployment subsidy (LTU) from 52 to 55. The program is intended to be a last-resort subsidy for individuals who are in a very disadvantaged position in the labour market – those who have been unemployed for a lengthy period of time and have lower socioeconomic status (in terms of education and the type of earlier employment). We use both a within- and a between-cohort identification strategy to compare workers affected/unaffected by this reform to reveal the impacts of the long-term unemployment program on hospitalizations, mental health diagnoses, and self-reported mental health indicators. The labour market effects of this reform have also been studied by Domenech and Vannutelli (2019). Our study differs from theirs in at least two ways, however. First, we implement a different identification strategy by exploiting the within- as well as between-cohorts differences in the eligibility criteria. Second, we go a step further than the labour market effects to estimate the health effects of LTU subsidies.

Our results show that the reform had an effect on the probability of receiving a LTU subsidy. Those individuals unaffected by the hike in the eligibility age have a significantly higher probability of receiving the LTU subsidy. The effect is greater 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 injury hospitalizations were reduced by 12.9% for men unaffected by the reform (i.e., were eligible for the LTU subsidy). This result is consistent with low educated men being more likely to work in physically demanding jobs, such as the construction sector. Furthermore, the probability of being diagnosed with a mental health condition also decreased for men who were eligible for the subsidy. Lastly, men eligible for the subsidy show better self-reported mental health and a lower EURO-D depression scale. None of these effects is significant for women.

Our paper contributes to the literature that studies the effects of unemployment insurance (UI) on health. This is a relatively small literature that nevertheless includes some important contributions. For example, Cylus et al. (2015) show for the US that the negative relationship between unemployment and poor health was milder in states where the generosity of unemployment benefits were higher. In Germany, a welfare reform which, among other changes, reduced the generosity of unemployment benefits, was found to be associated with an increase in poor self-reported health among unemployed individuals (Shahidi et al. 2020).

Two other important papers are close to ours and carefully examine the causal effects of UI on health using detailed administrative data. First, in the US, Kuka (2018) exploiting the exogeneity of state UI laws, found that greater UI generosity leads to increased health insurance take-up and preventive healthcare use. The author also shows that the increased UI benefits induced an improvement in self-reported health, especially during the periods of high unemployment.

Second, Ahammer and Packham (2020) show that an extension of UI in Austria affected workers' physical and mental health, with differences by gender. Unemployed women who had the opportunity to receive an unemployment benefit for an additional nine weeks were prescribed fewer opioids and antidepressants, and were less likely to file a disability claim. As shown in their paper, the reason for these effects is that women match into better jobs after the unemployment spell. Men, however, increased their likelihood of cardiac events and disability claims. For the latter finding, the authors discuss that men take on riskier behaviours after becoming unemployed.

We believe that our paper contributes to the above reviewed literature in several dimensions. First, due to the natural endogeneity between unemployment subsidies and health, ours is one of the very few studies to implement a credible identification strategy to estimate the causal health effects of receiving unemployment benefits. We do that by exploiting a unique reform that exogenously left similar individuals eligible/non-eligible for a LTU benefit, with the only difference being the month and year into which they were born. Unlike previous studies, we do not estimate the intensive margin effect of unemployment insurance (i.e., greater generosity of benefits), but instead compare those entitled/not entitled to the subsidy. Additionally, we are able to provide evidence for a relatively lengthy period (up to 3 years), and we focus on a segment of the population that belongs to an especially disadvantaged group: middle age workers who have been unemployed for a relatively long period of time, have a track record of insecure employment, and a low level of education. It is important to note that the reform occurred during a context of continuous employment destruction in Spain following the Great Recession. However, there are no major sudden changes in the labour market during our study period except for the reform that we study. From a policy perspective, this is a very important segment of the population since they have greater risks of suffering from poor health and thereby becoming a burden for the healthcare system.

Due to such characteristics of the LTU recipients, our paper also speaks to the recent literature on the so-called "deaths of despair". Case and Deaton (2017) show that there has been an increase in morbidity and mortality in middle-aged men in the US. They attribute this phenomenon to a "cumulative disadvantage" process of (mainly) labour market conditions together with general economic decline and other socioeconomic factors. Furthermore, they argue that such disadvantages will not likely be reversed in the short term. In this paper, we provide evidence on the extent to which social programs (i.e., LTU subsidy) are able to improve the health of middle-aged disadvantaged workers who have suffered from a "cumulative disadvantage" in the labour market.

Lastly, our paper is also related to the literature that studies the effect of (early) retirement on health. Although the LTU subsidy targets relatively younger individuals, it is often used as a bridge to retirement. As such, it can be thought of as an early retirement subsidy. The literature on the effect of retirement on health is inconclusive, with research reporting positive, negative, and zero effects. However, in general terms retirement has been found to improve mental health while also accelerating the deterioration of cognitive skills (van Ours 2019)**.** There is additional mixed evidence for early retirement with some papers reporting improvements in health and reductions in mortality (Bloemen, Hochguertel, and Zweerink 2017; Hallberg, Johansson, and Josephson 2015) and other studies finding precisely the opposite, particularly among men (Kuhn et al. 2018; Fitzpatrick and Moore 2018). For instance, Kuhn et al. (2018) find an increase in the risk of premature death among male blue-collar workers who transitioned to early retirement through an extension of the unemployment benefit scheme. Nevertheless, the LTU benefit that we study may differ from a normal early retirement scheme in several ways. First, LTU benefit can hardly be considered as "voluntary", assuming that most individuals at that age would prefer to work rather than be unemployed. Second, the quantity of the LTU subsidy is much lower than retirement pensions.

The rest of the paper is structured as follows. Section 2 describes the LTU subsidy reform. Section 3 describes the data sources and the empirical strategy. Section 4 describes the results and Section 5 concludes with a discussion of the main findings and the implications.

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 to increase the eligibility age for the subsidy for long-term unemployment from 52 to 55 years old.

The subsidy is especially targeted to workers approaching retirement age and unable to find a bridge job to the minimum retirement age (65 years old). Apart from the age threshold, to be able to access the long-term unemployment subsidy program, individuals must have exhausted their unemployment insurance benefits, which have a maximum duration of two years (SEPE 2019). Thus, the LTU program acts as a last resort option, and the amount of benefits provided is relatively low, around 426€ 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 equate to never being able to work again due to the relatively high unemployment rates of the country and the relative scarcity of training programs for unemployed workers. This was even truer during the Great Recession, when unemployment rates soared sharply above 20%. 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 two years, as compared to 30% for the overall unemployed population.

Due to the challenge of finding jobs for older workers, the subsidy is often 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 registers (MCVL[2]), the age-adjusted probability of receiving a retirement pension in their next employment status is 17.2% for those receiving the subsidy, compared to 7.3% for the unemployed not receiving this subsidy, and 8.4% for the employed (Table A1, Appendix).

Recipients of the LTU subsidy spent more time in unemployment than employment during the four years prior to 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 (i.e., they changed job more often). Women receiving

[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), this index was set at 532.31€ (Source: http://www.iprem.com.es/).

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

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 very precarious labour situations.

Furthermore, recipients of the LTU subsidy were much lower educated that those with employment; with 35.6% lacking even primary education as compared to 21% for the employed; whereas they had an education level similar to 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 strongly concentrated 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 concentrated in the Construction (men) and Industry (women) sector, and are generally in unskilled jobs. Furthermore they show a very poor performance in the labour market, with long periods of unemployment and insecure, temporary 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 analysing 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 with information on labour market outcomes and health.

3.1. Labour market outcomes

We begin by analysing the effects of the reform on labour market outcomes, looking at the probability of receiving the LTU subsidy using register data from the Social Security Administration. The database, Continuous Sample of Working Lives (MCVL), is a representative random sample of 4% of all individuals who have contributed to the Spanish Social Security system. For each individual this micro-dataset records their lifetime labour market participation, including the duration of their employment, the economic sector, and other characteristics of each employment contract or social security benefit (including unemployment insurance, unemployment subsidies, and 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 were aged between 50 and 52 at the time of the reform (2012). 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 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 1st and 2nd semester. The literature has shown that the month of birth may affect health outcomes, either directly (Buckles and Hungerman 2013; Costa and Lahey 2005; Rietveld and Webbink 2016) or through the increase in education (Angrist and Keueger 1991). In turn, the increase in education might also affect health outcomes (Cutler and Lleras-Muney 2006). This seems to be the case in Spain (Obrero, Martín, and 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 specific to the semester of birth 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 specifications 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 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 and 1959), instead of the closest younger cohorts (1961 and 1962). However, cohorts 1958 and 1959 are not an appropriate comparator because individuals in those cohorts could access the LTU subsidy before the reform, as they turned 52 prior to 2012. As a consequence, those born in the 1st semester of these cohorts would start receiving the subsidy before those born in the 2nd semester. This could provoke health differences between semester of birth within the control cohorts (1958 and 1959) that 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 and thereafter.

Therefore, our identification assumption under the 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 in greater detail all of 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:

$$y_{i,t} = \beta_0 + \beta_1 Cohort1960_i + \beta_2 Semester1_i + \beta_3 After2012_t + \beta_4(Cohort1960_i \; x \; Semester1_i) + \beta_5(Cohort1960_i \; x \; After2012_t) + \beta_6(Semester1_i x \; After2012_t) + \beta_7(Cohort1960_i \; x \; Semester1_i \; x \; After2012_t) + year_t + province_p + \mu_{i,t} \quad \text{[Equation 1]}$$

where $Cohort1960_i$ equals 1 if the individual *i* was born in 1960 and zero if born in 1961-1962; $Semester1_i$ equals 1 if born from January to June, zero from July to December; $After2012_t$ equals 1 for observations from year 2012 on; $year_t$ are year fixed effects and $province_p$ are province fixed effects. Employment status is measured by three dependent binary variables:

$y_{i,t} = (employed, subsidy, unemployed, out\ of\ the\ labour\ market)$, where $employed$ equals 1 if the individual *i* is employed, and zero otherwise; $subsidy$ equals 1 if the individual is receiving the long-term unemployed subsidy and zero otherwise; $unemployed$ equals 1 if the individual is unemployed receiving another subsidy or benefit, and zero otherwise; and *out of the labour market* equals 1 if the individual is neither working in a formal job 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 on). That is, it captures the variation in employment status that is specific to those born in the 1st semester (relative to those born in the 2nd 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 achieve statistical significance.

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 and 1962 (“control” cohort). In total, there are 852,577 registered hospitalizations. We aggregated them by province[3], cohort of birth, semester of birth, and year. In order to estimate the effects we employ a similar DDD model:

[3] There are 50 provinces in Spain, with a median population of 657,404 inhabitants, as of 2012.

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

[Equation 2]

Independent variables are similar to those of Equation 1. We collapse the data by province, cohort, semester of birth, and year and create a dependent variable, $Hospitalization\,rate$, which 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 following previous literature (Mental health and Injuries)[4], as well as the most prevalent ones: Digestive, Musculoskeletal, Circulatory, and cancer, which was used as a placebo (Table 2).

Again, β_7 is our parameter of interest, which seeks to measure the effect of the reform on hospitalizations. In particular, it measures how hospitalizations evolved after the reform among those who were *eligible* for the LTU subsidy (born in the 1st 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 the 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 an indicator that is less extreme than hospitalizations. For that, we use data from mental health diagnoses at primary care centres. Data come from a representative sample of registered

[4] Injuries are an indicator of occupational health that can be indirectly affected by the subsidy. LTU subsidy recipients, as explained in Section 2, come from sectors with more physically demanding jobs and are workers with very low educational levels. Therefore, in the absence of the subsidy, these individuals may be forced to accept riskier, unskilled, and manual jobs, since they are left with no alternative source of income. Such jobs are expected to increase the risk of suffering injuries due to work-related accidents (van Ours 2019).

primary care clinical data from the Spanish Minister of Health (*Base de datos clínicos Atención Primaria – BDCAP*). The 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), representing around 37% of the Spanish population. Our final sample comprises 37,690 individuals followed from 2011 to 2014, making a total of 150,760 person-year observations.

Similarly to Equation 1, the effect of LTU subsidy on mental health diagnoses at primary care can be determined by:

$$Mental\ health_{i,t} = \beta_0 + \beta_1 Cohort1960_i + \beta_2 Semester1_i + \beta_3 After2012_t + \beta_4(Cohort1960_i\ x\ Semester1_i) + \beta_5(Cohort1960_i\ x\ After2012_t) + \beta_6(Semester1_i x\ After2012_t) + \beta_7(Cohort1960_i\ x\ Semester1_i\ x\ After2012_t) + year_t + \alpha_i + \mu_{i,t}$$

[Equation 3]

The dependent variable $Mental\ health_{i,t,}$ equals 1 if the individual *i* had a diagnosed 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 health, particularly mental health. SHARE is a multidisciplinary panel database with information on health and other socioeconomic variables of European individuals aged 50 or older, and representative at country level. 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.

Unfortunately, SHARE data were not available for the years prior to the reform[5]. 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:

$$y_{i,t} = \beta_1 + \beta_1 Cohort1960_i + \beta_2 Semester1_i + \beta_3 (Cohort1960_i \; x \; Semester1_i) + year_t + \mu_{i,t}$$

[Equation 4]

where $Cohort1960_i$ equals 1 if the individual was born in 1960, zero otherwise; $Semester1_i$ equals 1 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 semesters 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 as 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 of 3.2 pp for those eligible for the LTU subsidy (born in 1st semester of 1960) with respect to those affected by the minimum age rise (born in 2nd

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

semester of 1960). This increase is greater for men (3.7 pp) than for women (2.5 pp). Simultaneously, the probability of being out of the labour market fell by 2.4 pp for men, and by 2.3 pp for women eligible for the LTU subsidy. Lastly, there is a significant reduction (of 0.9 pp) of being unemployed (and receiving another type of subsidy) for eligible men, but not for women.

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. 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 on.

Because the law passed in 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 following the reform because they no longer complied with the new minimum age requirement. However, what we see in Figure 1 is that the increased probability of receiving the LTU subsidy for the eligible semester-cohort remains relatively constant after the reform. This means that those born in the 1st semester of 1960 who received their LTU benefit before the reform did not lose it after the rise in the eligibility age, even though they no longer complied with the new minimum age of 55.

We also see that receiving other unemployment benefits is a significant alternative to the LTU benefit, but only during the first year after the reform. After that, leaving the labour market is the only significant alternative employment status for those not eligible for the LTU subsidy, although this differs slightly by gender. On the one hand, the effect on being out of the labour market seems to remain constant or slightly greater for women over time. On the other hand, for men this effect seems to fall slightly over time, while at the same time the probability of formal employment seems to diminish for those eligible for the LTU, although it does not reach the 5% significance level. Overall, the increase in the eligibility age of the LTU subsidy seems mainly to have pushed those who were not eligible out of the formal labour market. This is not surprising, since the unemployment rate in Spain in 2012 was growing sharply, to more than 20%. The reform also increased the probability of receiving other unemployment benefits in the first year, and it might also have increased the probability of formal employment 1 to 2 years after the reform, but only for men; coinciding with the time at which employment in Spain started to recover.

Using the same specification, we further test whether those eligible for the LTU saw their monthly income affected as compared to those who were non-eligible due to the minimum age rise. We define income as monthly formal income registered in the Social Security contributions from MCVL. This may come from either employment or any Social Security benefit, including the LTU benefit under study. There is no significant effect of the reform on the average monthly income (Figure A1, Appendix). This is explained by the fact that whereas the main alternative to LTU benefit (out of labour market) implies no income; the other two alternatives (formal employment and unemployed with other subsidy or benefit) imply an income that is considerably higher on average (1520€ and 635€) than the LTU benefit (426€).[6] As a consequence, the net effect of the reform on the average salary is not significantly different from zero.

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 diagnosis was 68 per thousand individuals. More specifically, for the case of mental health and injuries, hospitalization rates were 3 and 4.6 per thousand individuals, which represent around 4% and 7% of the total number of hospitalizations, respectively. It is worth noting that the hospitalization rate for injuries among men is more than twice the rate 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 can be seen in the graph, the only group that has a significant negative coefficient is hospitalizations due to injuries. The reduction in hospitalization for injuries comes entirely

[6] Those "out of the labour market" were assigned a monthly salary of zero since they are not receiving any formal salary or any social security subsidy. Any informal income, which they might gain from an informal job, by definition, is not observed in our registered data. Table A5 in Appendix provides more details about the average monthly salary per employment status and how this variable was constructed.

from eligible men, who show a drop of 0.8 hospitalizations per thousand individuals after the reform. This effect implies a reduction of injury hospitalization rates of 12.9% for men with respect to 2011.

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

In Figure 3 we plot the coefficients of an event-style model in which 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, and 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 who were eligible for the LTU subsidy experience an accelerating reduction in hospitalizations over the three years following the reform. This reduction is essentially driven by provinces where the rate of unemployment for those aged 50-55 years old was higher (i.e., provinces that were more influenced by the LTU subsidy reform) (Figure A2, Appendix).

The same effect on injuries is not observed for women, however. This is consistent with the fact that the effect of the reform on the probability of receiving the subsidy is greater for men than for women.

Also, men who were not eligible for the LTU subsidy seem to increase their probability of formal employment, whereas women do not. Furthermore, descriptively we can see that men receiving the LTU subsidy come in greater proportion 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 greater proportion of men receiving the LTU subsidy have not completed primary education (40.7%) than is the case for women (26.8%). This fact will most likely have an impact on the occupational safety of the jobs that they can access, whether formal or informal (if out of the labour market).

We can see that the results for the mental health hospitalizations are not significant for either gender, although there is a significant reduction in mental health hospitalizations for men eligible for the LTU subsidy, but only in provinces more exposed to the reform (Figure A2, Appendix). However, it is important to remember that hospitalizations represent an extreme outcome in cases of mental health disease. 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, drug prescriptions, and self-reported mental health data from SHARE.

c) Effect on mental health: primary care diagnoses, and prescriptions

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 1st and 2nd semester for the "treated" 1960 cohort (Panel A) and for the additional control cohorts born in 1961 and 1962 (Panel B). The incidence of mental health diagnosis is around 6% during this period for the cohorts under analysis. If the reform in 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 1st semester (eligible for the LTU subsidy) compared to those born in the 2nd 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 A3 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 is significant only for the case of men. Thus, men who were eligible for the LTU subsidy experienced a significant drop of 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.

To further explore the potential effects on mental health, we use register data on mental-health related prescriptions[7] from the database BIFAP (Base de Datos para la Investigación Farmacoepidemiológica en Atención Primaria). This dataset includes electronic records of all prescriptions issued by a sample of 5,714 primary care physicians from 9 Spanish regions. The prescriptions in our sample thus correspond to patients who visited a family doctor from the Spanish National Health Service, and are not necessarily representative of the overall Spanish population. Focusing on the subsample of 205,461 individuals born between 1960 and 1962 who were followed from 2009 to 2014, we find no significant effect of the reform on the number of prescriptions for mental health conditions (Figure A4 and Table A6 in Appendix).

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 (at 10%) for the case of men, indicating that 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 the EURO-D depression scale as the dependent variable. Again, we see that for this self-reported mental health outcome, men eligible for the LTU subsidy report having significantly (at 10%) better mental health outcomes. As with 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), none of the coefficients are significant.

[7] We include as mental health-related prescriptions the following Anatomical Therapeutic Chemical Classification System groups (ATC): N02 Analgesics, N05 Psycholeptics, and N06 Psychoanaleptics. See Table A6 in Appendix for more details about these prescriptions.

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 semesters 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 cohorts are attributable to the reform.

In order to provide some support for the fulfilment of this assumption, we run placebo estimates using data from the waves before the reform was introduced (wave 1 in 2004 and wave 2 in 2007) and estimate the same model (Table A7 in the Appendix). In these 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.

5- Discussion

This paper studies the effects of a reform introduced in Spain in July 2012 that increased the age (from 52 to 55 years old) needed to obtain access to a long-term unemployment subsidy. We focus on the impacts on several labour market and health outcomes such as hospitalizations, mental health diagnoses, and self-reported health measures. In order to do so we use a combination of several data sources (register and survey data) and exploit a within- and a between-cohorts identification strategy.

Literature on the welfare effects of unemployment benefits has been mainly restricted to the effects on labour supply and consumption, with only a few papers addressing the potential health effects of unemployment benefits. Our study is of special interest since the Spanish LTU subsidy is designed as a last resort program and is, therefore, aimed at a specific part of the population that is disadvantaged in several dimensions and with poor prospects of returning to the formal labour market; recipients of the subsidy have a background of relatively poor employment and social conditions, which puts them at risk of suffering from significant health conditions.

We first show that the reform had a significant 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 (3.7 pp for men and 2.5 for women). We next turn to the effects on health outcomes and report no significant effects of the reform on the overall number of hospitalizations. However, disaggregating by disease and gender, we find a reduction in injury hospitalizations of 12.9% for men eligible for the LTU subsidy. As explained above, due to the characteristics of the target group, the alternative job (either formal or informal) is likely to be physically demanding and impart a higher risk of suffering from accidents while working.

We find no effect for mental health hospitalizations, which is an expected result, as hospitalizations represent a very extreme outcome for mental health problems. However, when we look at a milder outcome, mental health diagnosis, we find that men who were eligible for the LTU subsidy have lower probabilities of being diagnosed with a mental health problem 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). For the case of mental-health prescriptions, the reform had no effect for either gender. Lastly, using survey data we find that men eligible for the subsidy show better self-reported health status and lower EURO-D scale.

Our results differ from those of Kuka (2018) for the case of the US, as Spain has a universal health care system, meaning that increases in insurance take-up rates cannot explain our results. Furthermore, our results differ from those in Ahammer and Packham (2020), as we find significant effects only for the case of men, as they are more likely than women to receive the LTU subsidy – a result of the higher labour market participation rates of Spanish men in affected cohorts.

The paper has a limitation that is worth mentioning: our health results rely on ITT estimates because in the health databases we cannot identify the labour market status of individuals. Still, although the "treatment" probability (i.e., receiving the LTU subsidy) among affected men is low (3.7 pp), we expect them to be overrepresented in the pool of hospitalizations, as they have several socioeconomic and labour market disadvantages (low education, long-term unemployment, and low-occupational health jobs with higher risks for health), which are clearly linked to poorer health outcomes (see Tables A8 and A9 in the Appendix).

Finally, we believe that our results are very important from a policy perspective as they provide credible evidence on the net health effects of the LTU subsidy. When we perform a simple back-of-the-envelope calculation to account for the costs of injury hospitalizations that are avoided, we estimate that those entitled to the subsidy incurred 1,133 fewer injury hospitalizations, which in turn translate into 1,860,000 € yearly hospital expenses (See Table A10 for more details).

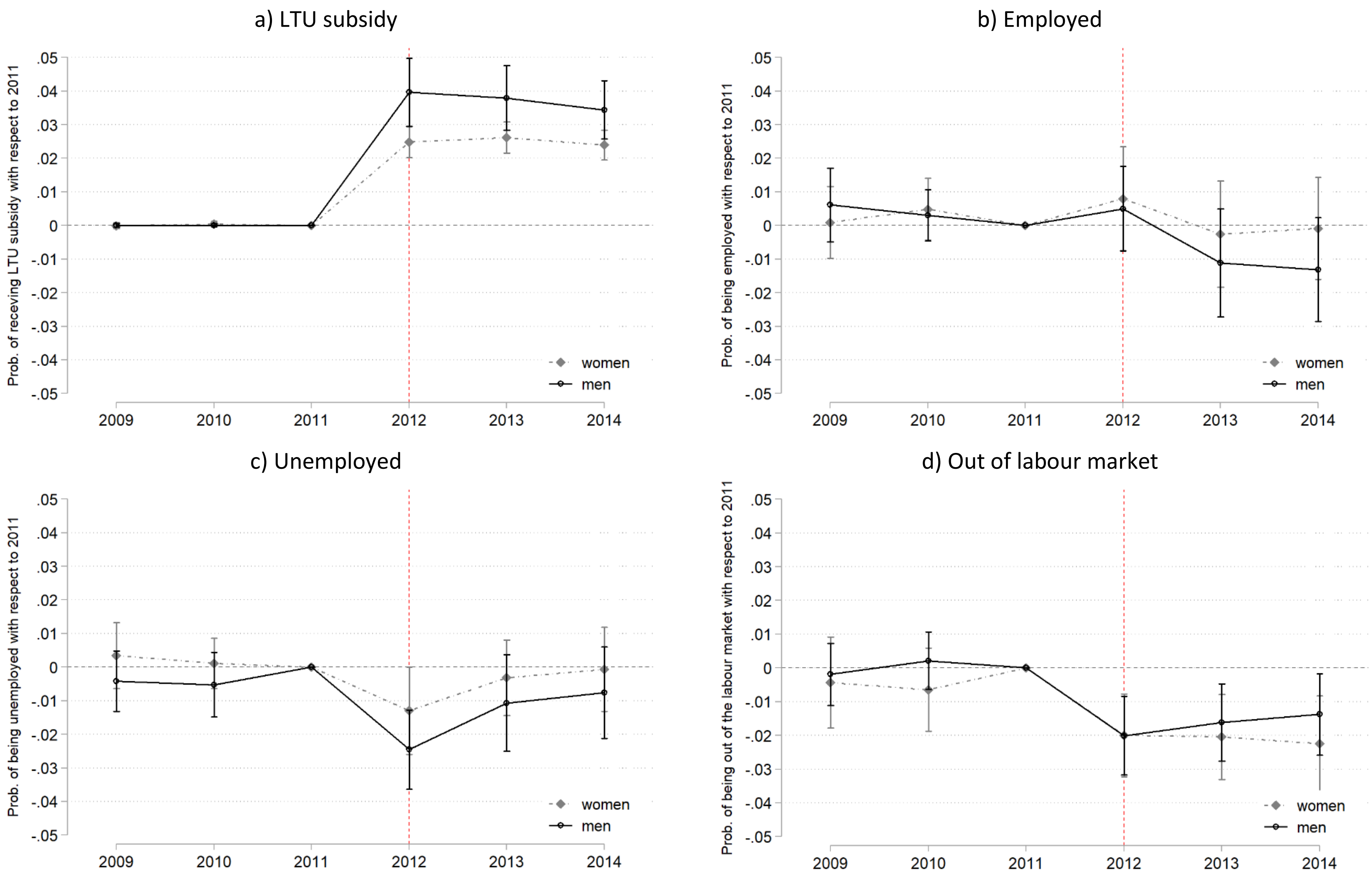
Tables and Figures

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	0.001	-0.002	0.001	-0.000	-0.005	0.003	0.002	0.001	0.006	-0.008**	0.001
	(0.000)	(0.004)	(0.002)	(0.004)	(0.000)	(0.005)	(0.004)	(0.005)	(0.001)	(0.006)	(0.004)	(0.005)
Semester 1 (B)	0.000	-0.008**	0.005**	0.003	-0.000	-0.014***	0.009***	0.005*	0.000	-0.003	-0.001	0.004
	(0.000)	(0.003)	(0.002)	(0.002)	(0.000)	(0.003)	(0.003)	(0.003)	(0.001)	(0.006)	(0.004)	(0.004)
Post2012 (C)	-0.000	-0.109***	-0.004	0.113***	-0.000	-0.125***	-0.002	0.128***	0.000	-0.090***	-0.006	0.096***
	(0.000)	(0.003)	(0.003)	(0.004)	(0.000)	(0.004)	(0.003)	(0.005)	(0.000)	(0.004)	(0.004)	(0.004)
A x B	0.000	-0.001	0.000	0.001	0.000	0.009	-0.007	-0.002	0.000	-0.011	0.009**	0.001
	(0.000)	(0.005)	(0.003)	(0.004)	(0.000)	(0.006)	(0.004)	(0.005)	(0.001)	(0.007)	(0.005)	(0.006)
A x C	0.002***	-0.006	0.000	0.004	0.002***	-0.004	-0.001	0.003	0.002***	-0.007	0.001	0.004
	(0.000)	(0.003)	(0.003)	(0.003)	(0.001)	(0.004)	(0.003)	(0.004)	(0.001)	(0.005)	(0.004)	(0.004)
B x C	0.000	0.003	-0.006**	0.003	0.000	0.003	-0.008***	0.005	0.000	0.004	-0.003	-0.001
	(0.000)	(0.003)	(0.002)	(0.003)	(0.000)	(0.004)	(0.003)	(0.004)	(0.000)	(0.005)	(0.004)	(0.004)
DDD Coefficient (AxBxC)	0.032***	-0.005	-0.009***	-0.017***	0.037***	-0.009	-0.011*	-0.017***	0.025***	-0.000	-0.007	-0.017**
	(0.002)	(0.005)	(0.003)	(0.004)	(0.005)	(0.007)	(0.006)	(0.005)	(0.002)	(0.007)	(0.005)	(0.007)
Constant	0.001**	0.837***	0.052***	0.110***	0.001	0.871***	0.054***	0.074***	0.001***	0.798***	0.050***	0.152***
	(0.000)	(0.003)	(0.002)	(0.004)	(0.000)	(0.003)	(0.003)	(0.004)	(0.000)	(0.005)	(0.003)	(0.004)
Observations	431,057	431,057	431,057	431,057	232,994	232,994	232,994	232,994	198,063	198,063	198,063	198,063
R-squared	0.026	0.023	0.007	0.017	0.033	0.027	0.008	0.023	0.020	0.023	0.006	0.017
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 1 for those receiving the LTU subsidy, zero otherwise. Columns (2), (6), and (10) LPM with dependent variable equal 1 for those employed, zero otherwise. Columns (3), (7), and (11) LPM with dependent variable equal 1 for those unemployed (receiving other subsidy or benefit), zero otherwise. Columns (4), (8), and (12) LPM with dependent variable equal 1 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 the appendix section.

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

	Mean (S.E.)		
	All	Men	Women
All hospitalizations	68.79	73.60	63.78
	(0.60)	(0.76)	(0.64)
Mental Health (ICD-7: 290-319)	3.01	3.22	2.78
	(0.09)	(0.11)	(0.12)
Injuries (ICD-7: 800-959)	4.64	6.30	2.92
	(0.09)	(0.14)	(0.09)
Digestive (ICD-7: 520-579)	10.85	13.45	8.13
	(0.14)	(0.21)	(0.16)
Musculoskeletal (ICD-7: 710-739)	7.99	8.45	7.51
	(0.16)	(0.21)	(0.17)
Circulatory (ICD-7: 390-459)	7.13	9.51	4.66
	(0.12)	(0.18)	(0.13)
Cancer (ICD-7: 140-239)	9.14	6.66	11.71
	(0.14)	(0.16)	(0.19)
Observations	300	300	300

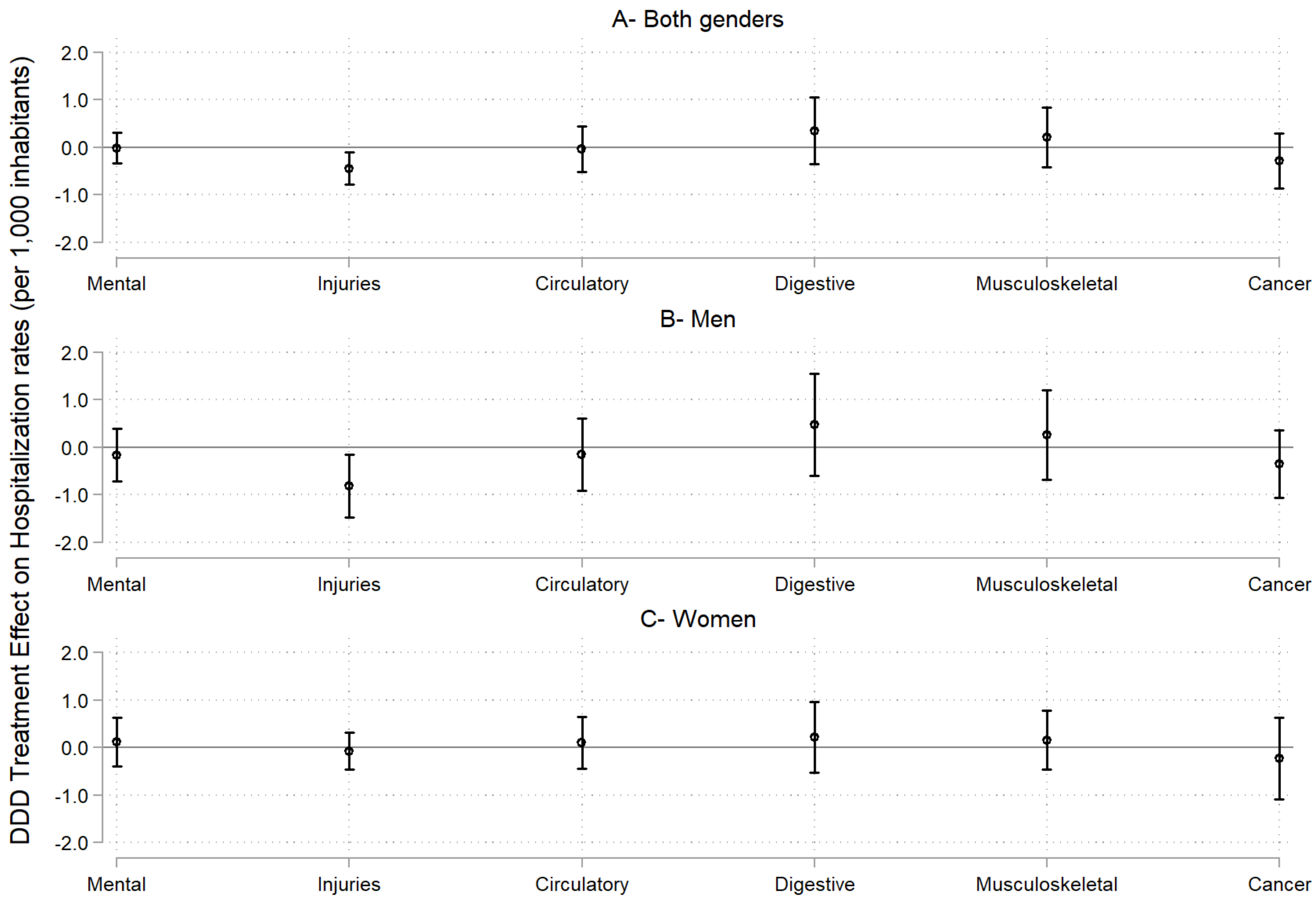
NOTES: This table reports the mean hospitalization rate (per 1,000 inhabitants) by cohort, and semester of birth, at province level, for the year prior to the reform (2011), for individuals born in the years 1960-1962.

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 (and 95% Confidence Intervals) come from running the model of Equation 2 separately per ICD-9 diagnosis group and gender.

Figure 3. Impact of the 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).

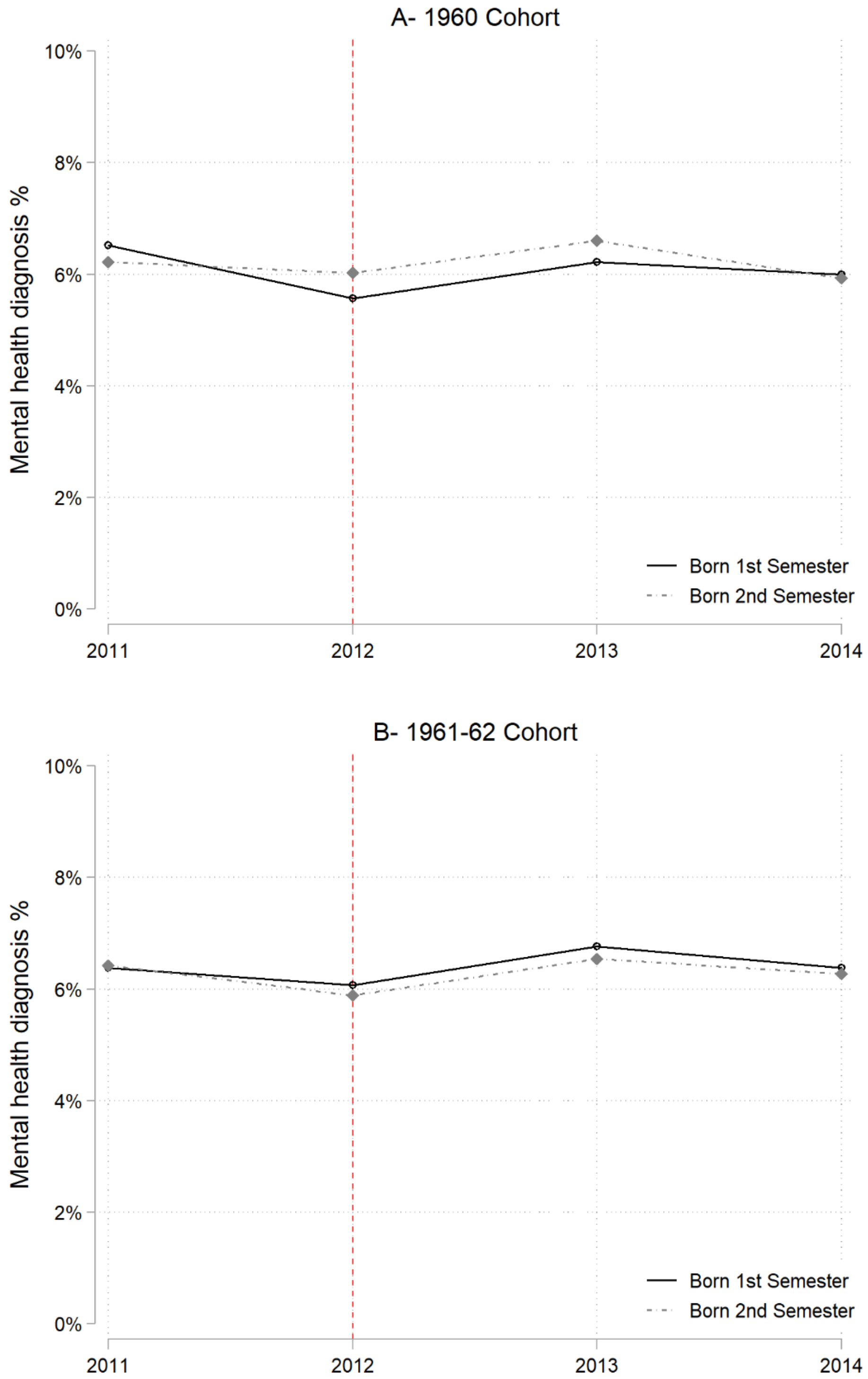


Table 4. Triple difference (DDD) model: Probability of a mental health diagnosis.

	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.

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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 between 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 text.

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 text.

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 on 15 November 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 (9,300 observations out of 9,590). The employment status corresponds to that recorded on 15 November 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 (9,133 observations out of 9,590). The employment status corresponds to that recorded on 15 November 2012.

Figure A1. DDD estimates for monthly income over time

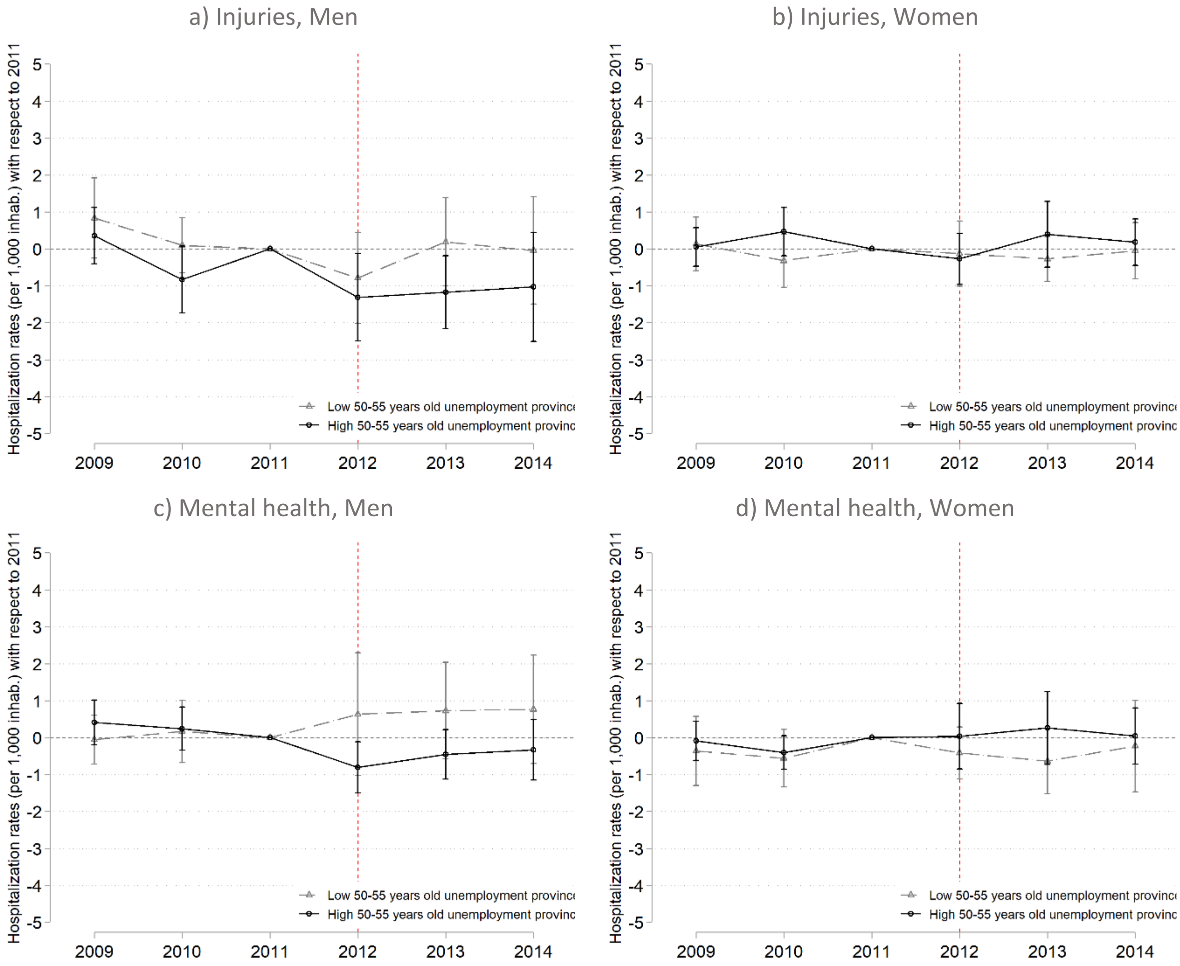
NOTES. Monthly income are expressed in 2012 euros. For a detailed explanation of how monthly income was calculated per each employment status, see Table A5.

Table A5. Average monthly income per employment status.

Employment status	Mean	SD	Min	Máx
Employed	1520.71	882.50	0	3262.50
Unemployed	635.76	272.49	426.00	1267.39
LTU subsidy	426.00	0	426.00	426.00

NOTES: MCVL Subsample of those born in first semester of 1960, in November 2012. Average monthly income for those employed is a proxy for real income based on Social Security contributions. These are subject to a pre-determined maximum per year. Therefore, the proxy for monthly income of the employed might show a lower dispersion than the real income since this proxy will not capture the highest salaries. Monthly income for the unemployed are estimated by the authors based on the historical individual Social Security contributions and applying the corresponding percentages from each year´s labour legislation. Furthermore, unemployment benefits are subject to a maximum and minimum depending on the number of children each recipient has. Since in our MCVL sample we do not know the number of children of each individual, we apply a weighted average maximum and minimum unemployment subsidy based on the average number of children reported in our SHARE sample for those born between 1960 and 1962.

Figure A2. Impact of LTU subsidy over time on hospitalization 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 of the DDD coefficient for employment status, similarly to what is explained in Equation A2 of the Appendix section.

Figure A3. Proportion of individuals diagnosed with a mental health condition by semester of birth (1960 vs 1961-62 cohort) and by gender.

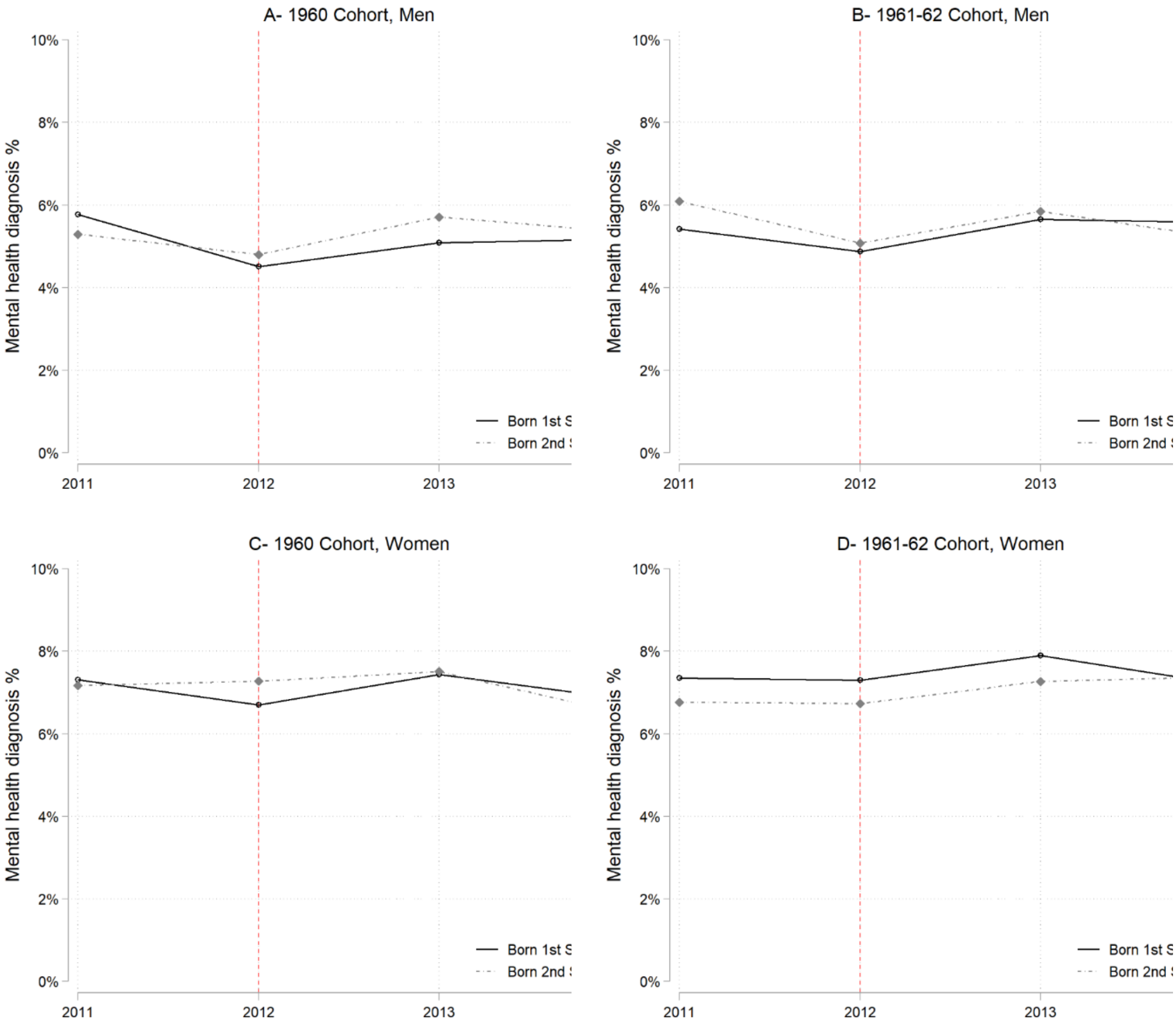
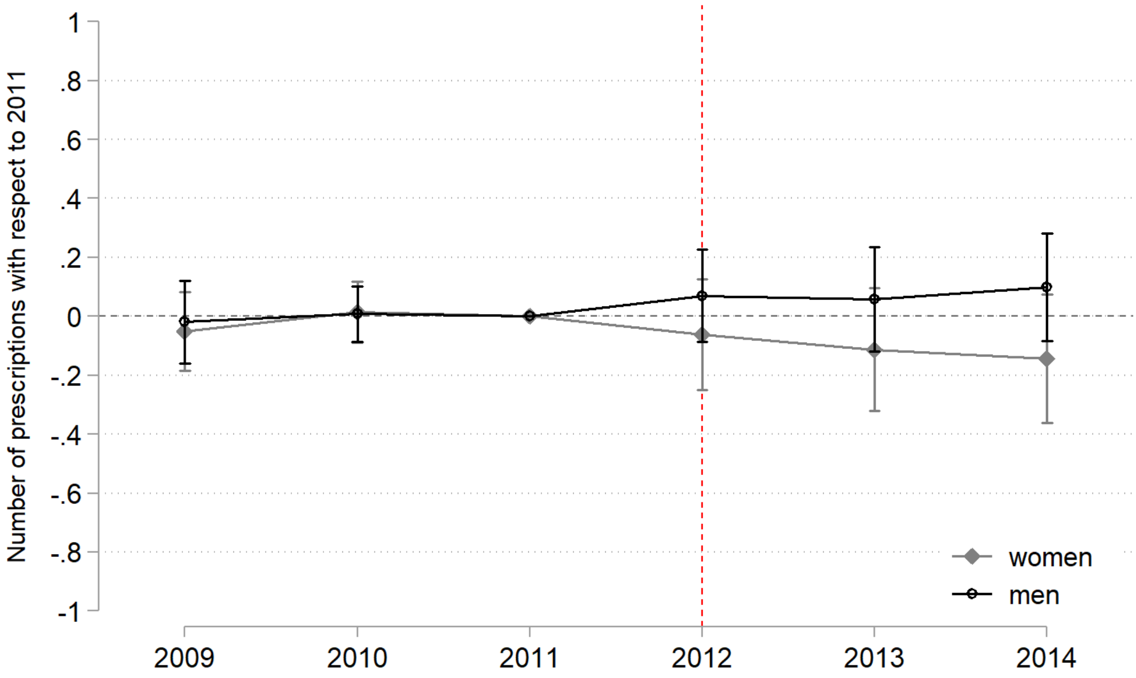


Figure A4. DDD estimates for number of mental-health related prescriptions over time.



NOTES: This figure plots the coefficients of the interactions between the double difference cohort-semester and the year dummies, which results from the decomposition the DDD coefficient for the number of prescriptions, as explained in Equation A2 of Appendix. We include as mental health-related prescriptions the following Anatomical Therapeutic Chemical Classification System groups (ATC): N02 Analgesics, N05 Psycholeptics, and N06 Psychoanaleptics. Same non-significant results appear when the analysis is done separately for each of the three categories. Table A6 contains descriptive data of the average number of prescriptions per category

Table A6. Descriptive statistics of mental-health related prescriptions

			With at least one prescription	
	Mean	S.E.	% from total	Mean
Analgesics (ATC=N02)	1.11	4.55	28.0%	3.96
Psycoleptics (ATC= N05)	2.01	8.05	22.4%	8.97
Psycoanaleptics (ATC= N06)	1.08	4.09	12.2%	8.78
Total mental-health related	4.19	0.01	41.3%	10.16

NOTES: Descriptive statistic from subsample of 205,461 individuals (98,667 men and 106,794 female) born between 1960 and 1962 from dataset BIFAP who were followed from 2009 to 2014 (n=1,232,766).

Table A7. 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

NOTES: 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 on at least a weekly basis.

Table A8. Adjusted predicted probability of injuries by education level and economic sector. 2011 Spanish National Health Survey.

	Education level				Total
VARIABLES	No education	Primary	Secondary	Tertiary	(Any education level)
Economic sector of last job					
Primary	0.047	0.047	0.043	0.007	0.033
	(0.027)	(0.021)	(0.021)	(0.006)	(0.015)
Industry	0.066	0.065	0.060	0.010	0.047
	(0.032)	(0.016)	(0.017)	(0.008)	(0.012)
Construction	0.074	0.074	0.068	0.012	0.053
	(0.035)	(0.019)	(0.022)	(0.009)	(0.015)
Hostelry	0.043	0.043	0.039	0.007	0.031
	(0.031)	(0.025)	(0.024)	(0.006)	(0.018)
Other services	0.047	0.047	0.043	0.007	0.033
	(0.022)	(0.011)	(0.011)	(0.005)	(0.007)
Total (Any economic sector)	0.054	0.054	0.049	0.008	0.038
	(0.024)	(0.009)	(0.011)	(0.006)	0.006

NOTES: Subsample of 1,461 men aged 50-59 years old from the 2011 Spanish National Survey. Each cell reports the age-adjusted predicted probability of having suffered an injury in the previous 12 months by education level and economic sector of last job (Standard Errors in parentheses). Adjusted predicted probabilities were calculated by running a logit model with a dummy variable indicating injury probability in the previous 12 months as dependent variable and controlling by age, education level, and economic sector of last job. Note that the same characteristics associated with receiving the LTU subsidy are also associated with suffering an injury. For instance, men with low education from the construction sector (those much more likely to receive a LTU subsidy) have a probability of suffering injuries almost twice that of the sample average (7.4% vs 3.8%). These results, together with those in Table A9, suggest that the 3.7% affected by the LTU subsidy reform account for significantly more than that 3.7% in terms of health outcomes.

Table A9. Adjusted predicted probability of mental health illness by education level and economic sector. 2011 Spanish National Health Survey

	Education level				Total
	No education	Primary	Secondary	Tertiary	(Any education level)
Economic sector of last job					
Primary	0.171	0.123	0.107	0.062	0.108
	(0.053)	(0.032)	(0.033)	(0.025)	(0.029)
Industry	0.180	0.131	0.113	0.066	0.114
	(0.051)	(0.022)	(0.023)	(0.021)	(0.018)
Construction	0.130	0.093	0.080	0.046	0.081
	(0.040)	(0.020)	(0.021)	(0.017)	(0.018)
Hostelry	0.138	0.099	0.085	0.049	0.086
	(0.058)	(0.036)	(0.033)	(0.024)	(0.032)
Other services	0.127	0.090	0.078	0.044	0.078
	(0.036)	(0.015)	(0.014)	(0.013)	(0.010)
Total (Any economic sector)	0.141	0.101	0.087	0.050	0.087
	(0.036)	(0.012)	(0.014)	(0.014)	(0.007)

NOTES: Subsample of 1,461 men aged 50-59 years old from the 2011 Spanish National Survey. Each cell reports the age-adjusted predicted probability of having suffered from a mental health illness in the previous 12 months by education level and economic sector of last job (Standard Errors in parentheses). Adjusted predicted probabilities were calculated by running a logit model with a dummy variable indicating mental health illness as dependent variable and controlling by age, education level, and economic sector of last job. Note that the same characteristics associated with receiving the LTU subsidy are also associated with suffering a mental illness. For instance, men with low education from the construction sector (those much more likely to receive a LTU subsidy) have a probability of 13% of suffering from a mental illness, as compared to the average of 8.7%. These results, together with those in Table A8, suggest that the 3.7% affected by the LTU subsidy reform account for significantly more than that 3.7% in terms of health outcomes.

Table A10. Net effects of the LTU subsidy reform on men's injuries hospitalizations.

Year	Cohorts non-eligible for LTU subsidy due to reform	Injuries Hospitalizations (ICD-9: 800-959)	Injuries hospitalizations due to restricted access to LTU subsidy (12.9%)	Imputed costs
2012	2nd semester 1960	940	121	597,206 €
2013	1961 and 2nd semester 1960	2,899	374	1,841,807 €
2014	1962, 1961, and 2nd semester 1960	4,944	638	3,141,047 €
Total 2012-2014		8783	1133	5,580,059 €
Yearly average 2012-2014		2928	378	1,860,020 €

NOTES: Following our estimation results, we assumed that the cohorts that were restricted regarding access to the LTU subsidy would have had 12.9% fewer injury hospitalizations than had they had access to the LTU subsidy. Note that each year after the reform, more cohorts are affected by the age increase and therefore the effects of the reform also increase by year. For instance, while in 2012 only those born in 2nd semester of 1960 would have been eligible for the LTU subsidy if the policy had not changed, in 2013 also those born in 1961 would have been eligible if the policy had not changed because they already turned 52. Number of hospitalizations per cohort was calculated using registered data of Spanish hospitals between 2012 and 2014 (See Section 3.2). We use as hospitalization costs the average cost of the hospitalizations by the ICD-9 group: "Injuries and poisoning (ICD-9: 800-999)" of the Spanish public hospitals from 2004 (last year available with costs by ICD-9 diagnosis group). Source: "Los costes de hospitalización en el Sistema Nacional de Salud. Avance datos 2005", Spanish Ministry of Health https://www.mscbs.gob.es/estadEstudios/estadisticas/docs/pesosCostes2004ResumenNotas.pdf

2016

2016/1, Galletta, S.: "Law enforcement, municipal budgets and spillover effects: evidence from a quasi-experiment in Italy"
2016/2, Flatley, L.; Giulietti, M.; Grossi, L.; Trujillo-Baute, E.; Waterson, M.: "Analysing the potential economic value of energy storage"
2016/3, Calero, J.; Murillo Huertas, I.P.; Raymond Bara, J.L.: "Education, age and skills: an analysis using the PIAAC survey"
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2017

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2017/17, Gancia, G.; Ponzetto, G.A.M.; Ventura, J.: “Globalization and political structure”
2017/18, González-Val, R.: “City size distribution and space”
2017/19, García-Quevedo, J.; Mas-Verdú, F.; Pellegrino, G.: “What firms don’t know can hurt them: Overcoming a lack of information on technology”
2017/20, Costa-Campi, M.T.; García-Quevedo, J.: “Why do manufacturing industries invest in energy R&D?”
2017/21, Costa-Campi, M.T.; García-Quevedo, J.; Trujillo-Baute, E.: “Electricity regulation and economic growth”

2018

2018/1, Boadway, R.; Pestieau, P.: “The tenuous case for an annual wealth tax”
2018/2, Garcia-López, M.À.: “All roads lead to Rome ... and to sprawl? Evidence from European cities”
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