Job Insecurity and Health: Evidence from Older European Workers

Eduard Suari-Andreu*[1,3] Tim Schwartz[2] Max van Lent[1] Marike Knoef[1,3]

[1] *University of Leiden*
[2] *SEO Economisch Onderzoek*
[3] *Netspar*

May 2020

Abstract

A rich literature has studied the relationship between job insecurity and health, however the causal link between these two variable still remains unclear. We contribute to this literature by studying this relationship using a longitudinal sample of more than 30 thousand older workers (*i.e.* age 50+) from 20 European countries over a period of 17 years. By means of standard Probit, we estimate a strong association between job insecurity and a wide range of health outcomes. However, accounting for fixed effects yields precisely estimated zeros, with the exception of some mental conditions. Additionally, we use an IV strategy recently proposed in the literature. However, we fail to replicate the results in the literature and argue that the instrument may not be exogenous. We conclude that the relationship between job insecurity and health for older workers is in any case rather weak, and discuss several reasons for our findings.

JEL Classification: I10, I18, J28, J81.
Keywords: Job insecurity, Health, Older Workers.

*Eduard Suari-Andreu, Department of Economics, Leiden University, Steenschuur 25, 2311 ES, Leiden, The Netherlands. E-mail: e.suari.andreu@law.leidenuniv.nl.

1 Introduction

Over the past few decades Western economies have experienced technological, economic, and political changes that have led to increasingly flexible work relations (OECD, 2019). Permanent contracts have become less prevalent, often resulting in increased job insecurity for workers. We define the latter as *a perceived threat to the continuity and stability of employment as it is currently experienced* (Shoss, 2017). Job insecurity increases workers' uncertainty about their future economic situation, which can have negative effects on their mental and physical health. From a policy perspective, it is important to guarantee the availability of a healthy work force. Therefore governments should take into account any potential negative effects of flexible work relations on general well-being. To have an informed debate about what work relations should look like, it is important to improve our understanding of the causal effect of job insecurity on different dimensions of workers' health.

There exists a large academic literature that studies the health effects of perceived job insecurity.[1] This literature originates from different fields of study in the social sciences. Within the field of occupational psychology, several studies point at a correlation between perceived job insecurity and poor health. Both de Witte *et al.* (2016) and Shoss (2017) provide thorough reviews of this literature, showing that this correlation is the strongest for mental health conditions and physical symptoms related to stress and depression. There are two reasons why the true causal effect may differ from the correlations found in the occupational psychology literature. First, there can be an omitted variable bias if there is unobserved heterogeneity across individuals that simultaneously correlates with perceived job insecurity and health; and second, there can be reverse causality if the health status of individuals has an effect on their job insecurity.

In recent years, several contributions within the health economics literature have addressed these challenges. Two main strategies are used in this literature. First, several studies propose estimating the effect of perceived job insecurity on health by employing equations including individual fixed effects (*e.g.* Green, 2011; and Rohde *et al.*, 2016). This approach enables estimation of the causal effect only if the sources of endogeneity (*i.e.* omitted variable bias and reverse causality) are fixed over time. Second, other studies exploit variation in perceived job insecurity caused by firm-specific events. For instance, Reichert and Tauchmann (2017) and Cottini and Ghinetti (2018) exploit staff reductions occurring in the company where a worker is employed. They do so under the assumption that such events do not affect health through any channel other than job insecurity. However, this assumption is hard to justify since staff reductions may also lead to changes in tasks and/or the work environment.

As an alternative to the above mentioned strategies, Caroli and Godard (2016) propose an approach consisting of using the stringency of country-level employment protection legislation (EPL) as an instrumental variable for perceived job insecurity, which they apply using a cross section of OECD countries. This strategy relies on the assumption that country-level EPL affects

[1] As it clear from the definition we employ, we focus in job insecurity as perceived by individuals. Throughout the paper we refer to job insecurity and perceived job insecurity indistinctively but we always refer to the latter.

health outcomes only through job insecurity. Since it is reasonable to assume that there are unobserved country characteristics that correlate both with EPL and health, Caroli and Godard (2016) interact their EPL indicator with the sector-specific natural rate of dismissal (NRD).[2] They do so arguing that EPL is likely to be more stringent in those industries with a high NRD. With this approach, Caroli and Godard (2016) find that the detrimental effect of job insecurity on health is confirmed for self-reported general health and a limited group of health symptoms, *i.e.* skin problems, headaches, and eyestrain. However, in contradiction with a large extent of the previous literature, they do not find an effect on depression, anxiety, and general well-being.

In this paper, we study the relation between perceived job insecurity and health employing data from the Survey on Health, Ageing and Retirement in Europe (SHARE). The latter provides a very large sample of older workers (*i.e.*, 50 years old or more) from 20 European countries over a period of 17 years. Employing data from the SHARE has two important advantages with respect to the existing literature. First, the SHARE provides a comprehensive range of health measures including both subjective (self-reported general health, physical symptoms, and mental conditions) and more objective measures (doctor diagnoses and medicine intake). Second, the size and the longitudinal dimension of the sample allow us to apply several of the methods employed in the literature. In that way, we can asses which one gets closer to the estimation of the causal effect of job insecurity. We employ standard Probit, fixed effects estimation, and the IV strategy proposed by Caroli and Godard (2016). As an important advantage with respect to Caroli and Godard (2016), we exploit time as an additional source of variation for the instrumental variable.

The focus on a sample of older workers is relevant for several reasons. First, job insecurity may be especially worrisome for older workers, since they potentially face a permanent reduction in their pension income if they work less in the final years prior to mandatory retirement. This aspect is particularly relevant given the rapidly ageing population in Europe, which encourages countries to introduce policies incentivizing older workers to remain longer in the labour market. Second, there may be non-monetary effects that condition the consequences of job insecurity at older ages. For instance, workers who look forward to retiring to enjoy additional leisure time with already retired peers can be differently affected compared to workers who do not. Given the richness of our data and its focus on older workers, we are able to pay special attention to these monetary and non-monetary aspects related this particular demographic.

When estimating standard Probit models without controlling for observables (*i.e.* both demographic and economic variables) we find strong and significant effects of perceived job insecurity on nearly all health outcomes. These effects are especially strong for the self-reported general health status and for the mental health outcomes. However, these effects become substantially smaller when we include observables in our regressions, and they virtually disappear when including an individual fixed effect. Only a few mental health outcomes (*i.e.* depression, lack of interest, and concentration problems) remain significant upon the inclusion of a fixed

[2]The natural rate of dismissal is defined as the rate of dismissal that there would be in a particular sector of the economy in the absence of EPL (Caroli and Godard, 2016).

effect. Finally, the IV analysis does not yield conclusive results and fails to replicate the findings by Caroli and Godard (2016). EPL appears to be a relevant instrument, *i.e.* it has a strong and significant effect on perceived job insecurity, however our results suggest it is not exogenous with respect to the health outcomes.

From the results we conclude that, for older workers, the correlation between perceived job insecurity and health is partially explained by observables and partially explained by individual-specific unobserved heterogeneity. This implies that, conditional on observables, the sources of endogeneity (*i.e.* omitted variable bias and reverse causality) are fixed over time. Notably, when accounting for individual fixed effects we still find a significant effect for depression. The latter is one of the most common health conditions in our sample.[3] The effect on depression appears to be particularly strong for individuals in the bottom half of the wealth distribution. These are the most likely to be affected by a reduction in pension income caused by job loss late in the working career. In addition, the effects we estimate are not significantly different when comparing individuals who look forward to retirement with those who do not.

The remainder of the paper is structured as follows: Section 2 provides the empirical specification and the estimation methods; Section 3 describes the data source and the sample selection; Section 4 provides the results; and Section 5 concludes.

2 Empirical Strategy

To estimate the effect of job insecurity on health we apply a variety of methods employed in the literature investigating the health consequences of job insecurity. For that purpose, we set up the equation

$$H_{ict} = \beta_0 + \beta_1 JOBINS_{ict} + \boldsymbol{\beta}_2' \boldsymbol{DEM}_{ict} + \boldsymbol{\beta}_3' \boldsymbol{WORK}_{ict} + \boldsymbol{\beta}_4' \boldsymbol{\xi}_c + \boldsymbol{\beta}_5' \boldsymbol{\delta}_t + \eta_i + \epsilon_{ict}, \quad (1)$$

where i, c, and t are individual, country, and year indices respectively, H_{ict} is a dummy variable taking value one if an individual is in poor general health and zero otherwise,[4] $JOBINS_{ict}$ denotes perceived job insecurity, $\boldsymbol{DEM}_{ict}$ is a vector containing individual demographic characteristics, $\boldsymbol{WORK}_{ict}$ is a vector containing job characteristics, $\boldsymbol{\xi}_c$ is a vector of country dummies, $\boldsymbol{\delta}_t$ is a vector of year dummies, and $\eta_i + \epsilon_{ict}$ is the composite error term where η_i is an individual effect and ϵ_{ict} captures unobserved variation across individuals, countries, and time.

The main coefficient of interest is β_1, which is expected to be positive thus indicating a detrimental effect of job insecurity on health. We first estimate β_1 and the other coefficients in Equation (1) by standard Probit with and without including the control variables. Subsequently, we estimate β_1 again by Probit but including an individual fixed effect in the specification.[5] By

[3] 34% of individuals in our sample report to be suffering from depression.

[4] For all of the health measures we consider, the dependent variable is always a dummy variable taking value one if the individual suffers a particular negative health outcome.

[5] As explained by Greene (2004) combining standard fixed effects and Probit estimation will yield biased results due to the so-called incidental parameters problem. Therefore, in our fixed effects estimation we follow Mundlak (1978) and include the individual-specific average of all time varying regressors in the Probit specification. For the

adding an individual fixed effect we reduce the potential for omitted variable bias, since it will control for all time-invariant unobserved heterogeneity across individuals, *i.e.* η_1 in Equation (1), that is otherwise subsumed in the error term.

If the sources of endogeneity (*i.e.* omitted variable bias and reverse causality) are not fully constant over time, the fixed effects estimate of β_1 will still be biased. Therefore, we follow Caroli and Godard (2016) and turn to an IV strategy using an index for the stringency of EPL as an instrument for job insecurity. For that purpose, we set the first stage equation

$$JOBINS_{ict} = \gamma_0 + \gamma_1 EPL_{ct} + \boldsymbol{\gamma}_2' \boldsymbol{DEM}_{ict} + \boldsymbol{\gamma}_3' \boldsymbol{WORK}_{ict} + \boldsymbol{\gamma}_4' \boldsymbol{\xi}_c + \boldsymbol{\gamma}_5' \boldsymbol{\delta}_t + \phi_i + \upsilon_{ict}, \qquad (2)$$

which we estimate jointly with Equation (1) as a two-equation Probit model. This IV strategy works as long as the following two assumptions are satisfied: the relevance assumption, *i.e.* EPL has a strong effect on job insecurity, and the exogeneity assumption, *i.e.* EPL affects health only through job insecurity.

Note that variable EPL_{ct} in (2) changes across countries and over time. Therefore, our specification differs from Caroli and Godard (2016), who use a cross-section of countries and thus are not able to exploit time variation in the instrument. To deal with this, Caroli and Godard (2016) use the interaction between EPL and the sector-specific natural rate of dismissal (NRD) as their instrument. If our exogeneity assumption holds, we do not need to rely on the interaction between EPL and the NRD. However, since this assumption cannot be directly tested, we perform an additional IV estimation using the interaction between EPL and the sector-specific NRD as an instrument. This consists of substituting $\gamma_1 EPL_{ct}$ in Equation (2) by $\gamma_1 EPL_{ct} + \delta_1 NRD_s + \delta_2 EPL_{ct} \times NRD_s$, where the subscript s denotes the sector. This strategy has the advantage of adding a third layer of variation to the IV analysis (*i.e.* country, time, and sector).

3 Data and Sample Selection

We employ data from the Survey on Health Ageing and Retirement in Europe (SHARE). The SHARE is a cross-national panel survey that provides detailed information on respondents' labour market status, health, finances, family relations, and socio-economic status. It targets people aged fifty and older and their spouses/partners independent of age. The survey is conducted every two years on average and we use waves one to seven, which run from 2004 until 2017.[6] Most importantly for us, the SHARE contains information on the perceived job insecurity of workers, as well as on a whole range of health outcomes.

Interviews have been conducted in twenty-nine European countries, out of which we use

sake of simplicity, throughout the paper we refer to these averages as the individual fixed effect, as they effectively control for η_i in Equation (1).

[6]The third wave is excluded from the sample because it focuses on individual's life histories and does not contain information on most of the variables used in this analysis.

all of those for which there is data on health and job insecurity. These are Austria, Belgium, Czech Republic, Denmark, Estonia, France, Germany, Greece, Hungary, Ireland, Israel, Italy, Luxembourg, Netherlands, Poland, Portugal, Slovenia, Spain, Sweden, and Switzerland. We select only individuals who are employed, and for whom there is no missing data on any of the health outcomes we employ. After this selection, we are left with a total of 31,135 individuals belonging to 23,862 households. We have a total of 48,716 individual-wave observations, which implies that every individual is observed for 1.56 waves on average.[7]

3.1 Job Insecurity

As mentioned in the introduction, in the present study we define job insecurity as a perceived threat to the continuity and stability of employment as it is currently experienced. The literature has employed different measures of this concept. Shoss (2017) provides a thorough summary of these measures and shows that they can be divided in two types. The first type only takes into account the probability of losing the current job, and not the employability of the worker in general. These measures are usually based on questions that ask individuals about the possibility of losing their job within the near future. For instance, Caroli and Godard (2016) use the question "what is the possibility that you lose your current job in the next 6 months?". The second type of measures are usually based on questions that also capture individuals' perception about the probability of finding a job in the case of losing the current one. Since job insecurity should only affect health in case it implies unemployment risk and/or a risk to financial stability, this second type of measures appears to be most relevant.[8] Furthermore, they are more in line with the definition of job insecurity we employ.

With this caveat in mind, we use a question from the SHARE that, instead of asking about the chance of job loss, asks individuals to what extent they agree with the statement: "my job security is poor". The possible answers are "strongly agree", "agree", "disagree", "strongly disagree". We use workers' response to the statement above to create a dummy variable that takes value one if the individual agrees or strongly agrees, and zero if the individual disagrees or strongly disagrees. To the extent that individuals understand job insecurity as the capacity to keep a certain level of labour income in the foreseeable future, the question we use captures both the chance of losing the current job as well as the degree of employability of a worker.

Out of all individual-wave observations in the sample we employ, 23.17% report to be in a situation of job insecurity. Regarding the individual-specific time variation in job insecurity, which is necessary for the fixed effects estimation, out of the total of 31,135 individuals observed, 39.04% (12,156) are observed at least twice. These individuals are observed for 2.68 waves on average and 25.81% of them (3,138) experience at least one change in job insecurity during the period of observation. These numbers imply that our fixed effects estimation is based on a much

[7] We have an unbalanced panel. However, when testing on observable characteristics we do not observe any differences between those individuals included only once in the sample compared to those observed multiple times.

[8] Green (2011) shows that the health effects of subjective job loss expectations are considerably curved by individuals' self-reported potential employability upon job loss. He shows that an increase in men's employability from zero to 100% reduces the detrimental effect of job loss risk by more than a half.

larger source variation compared to other studies in the literature that used this method to estimate the health effects of job insecurity.

3.2 Health

The most commonly used health measures in the related literature are self-reported general health, presence of general physical symptoms, and measures of mental health. The SHARE contains questions on each of these. As a measure of self-reported general health we employ a SHARE question asking individuals how they would rate their own general health status, to which they can answer "excellent", "very good", "good", "fair", or "poor". Similarly to Caroli and Godard (2016), we create a dummy variable that takes value one if an individual reports poor or fair health, and zero otherwise.

Regarding the measurement of physical symptoms, we employ a battery of questions from the SHARE asking individuals whether they suffer back pain, heart trouble, breathlessness, persistent cough, swollen legs, sleep problems, falling down, dizziness and/or faints, and stomach problems.[9] Regarding mental health, we employ a battery of questions asking whether individuals suffer from depression, pessimism, suicidality, guilt, lack of interest, irritability, lack of appetite, fatigue, concentration problems, lack of enjoyment, and tearfulness.

Even though the above-mentioned measures are very comprehensive, all of them are based on individuals' perception of their own health status. Therefore, we take advantage of the rich information provided by the SHARE on health, and complement them with measures that can be considered to be somewhat more objective, *i.e.* conditions diagnosed by a doctor, and medicine intake related to particular diseases. Regarding the conditions diagnosed by a doctor, we use a battery of questions asking individuals whether they have been diagnosed with heart attack, high blood pressure, high cholesterol, stroke, diabetes, lung disease, arthritis, cancer, and ulcer. Regarding drug consumption, we use a battery of questions asking individuals whether they use medicines for cholesterol, blood pressure, coronary disease, other heart disease, diabetes, joint pain, other pain, sleep problems, anxiety and/or depression, osteoporosis, stomach burns, and chronic bronchitis. We expect to find smaller effects of job insecurity on these outcomes. However, we still consider them in our analysis since they provide a slightly more objective counterpart to the health measures usually employed in the literature.

3.3 Control Variables

When estimating the effect of job insecurity on health, there are variables that will generate an omitted variable bias if they are not controlled for. For instance, age, gender, and education are very likely to correlate with perceived job insecurity, as well as with most of our health measures. In addition, job characteristics such as the type of occupation, sector of occupation, or the amount of hours worked per week may also correlate both with job security and with health simultaneously. Out of the long list of covariates provided by the SHARE, we choose

[9] Unfortunately, the questions on physical symptoms are only available up to wave four of the SHARE.

those that are likely to generate an omitted variable bias if not included, which in most cases are in agreement with those variables considered in the related literature.

As made explicit in Equation (1), we divide our control variables between demographic and work-related variables. Within the vector of demographic variables we include age, gender, marital status, and education, to which we also add total household income. Within the vector of work-related variables, we include a list of job characteristics, *i.e.* occupation, sector,[10] seniority (*i.e.*, years since the start of the current job), term of the job (*i.e.*, temporary or permanent), hours worked per week, months worked per year, and a set of dummies indicating whether a worker is an employee, is self-employed, or is a civil servant.

In addition, we include to the vector of work related variables a battery of measures provided by the SHARE that capture characteristics of the job and also of the working environment and conditions. These are provided as a series of statements to which individuals can answer "strongly agree", "agree", "disagree", "strongly disagree". The statements we use are the following: "my job is physically demanding", "I am under constant time pressure due to a heavy workload", "I have very little freedom to decide how I do my work", "I have opportunities to develop new skills", "I receive adequate support in difficult situations", "I receive the recognition I deserve for my work", "considering all my efforts and achievements, my salary is adequate", and "my prospects for promotion are poor".[11] Table A1 in the appendix provides summary statistics for all control variables that we use.

3.4 Instrumental Variables

As mentioned in Section 2, in this study we use employment protection legislation (EPL) as an instrument for perceived job insecurity. According to the OECD (2013), EPL is be defined as the set of rules governing the firing and hiring of workers. This includes all type of protection measures, whether based on actual legislation, court rulings, collectively bargained conditions of employment, or costumary practice. To measure the presence and strength of EPL, we use a country-level index put together by the OECD which is the same used by Caroli and Godard (2016). This indicator is a weighted sum of sub-indicators concerning the regulations for individual dismissals and provisions for collective dismissals of workers with permanent contracts.It is expressed in a scale from zero to six, where zero means no employment protection and six means full protection.[12]

A disadvantage of using this EPL index is that is only provided until the year 2013. Therefore, we only use waves one to five of the SHARE when performing the IV analysis, which reduces the number of countries in our sample to 17 and the total number of observations from 48,716 to

[10]To measure occupation and sector, the SHARE uses the International Standard Classification of Occupations (ISCO), and the Statistical Classification of Economic Activities in the European Community (NACE) respectively.

[11]Our motivation to include these variables is based on Caroli and Godard (2016), who argue that job insecurity may correlate with particular working environments and conditions. However, because some of these variables maybe endogenous with respect to job insecurity, we rerun our estimations excluding this set of variables.

[12]For more information on how this EPL is computed, see OECD (2013).

33,568. If we lag EPL by two years, which is what Caroli and Godard (2016) do in their analysis, we can also include wave six in the IV analysis which increases the number of observations substantially. However, when we do so the results we obtain do not significantly change.[13] Therefore, absent any obvious reason for doing so, we do not lag EPL.

As it is clear from Equation (2), the identification provided by EPL_{ct} relies on its time variation. That is because we include the vector of country dummies $\boldsymbol{\xi}_c$ in our specification, which would fully capture the effect of EPL in case of no time variation. Therefore, it is worth examining to what extent the EPL index changes not only across countries but also over time. Pooling all countries and years in our sample together, the EPL index actually takes values from 1.98 to 3.49, with an average of 2.66, and a standard deviation of 0.32. Table A2 in the Appendix provides the values of the indicator for each year and country in our sample. The table clearly shows that there is more variation across countries than over time. However, it is well known that in the last two decades, several European countries have implemented labor reforms that have generally decreased the stringency of EPL (OECD, 2019). This can be appreciated in Table A2, where the last row shows that the average value for the EPL index has declined from 2.76 to 2.51 over the period we analyse.

As explained in Section 2, we complement the IV analysis by using an additional strategy that, following Caroli and Godard (2016), adds to the specification in Equation (2) an interaction between the EPL index and the sector-specific natural rate of dismissal (NRD). To compute NRDs for each sector we take the assumption by Caroli and Godard (2016) stating the time average of the rates of dismissal in the USA provide a good proxy for the NRDs in Europe.[14] To compute the rates of dismissal in the USA, we use data from the Health and Retirement Study (HRS). The HRS is a survey conducted in the USA upon which the SHARE is based. Therefore, it contains very similar modules as in the SHARE and it also targets only the 50+ population. For each sector, we calculate the average rate of dismissal for the period between 2006 and 2012, which roughly coincides with the years in the sample used for the IV analysis.[15] Table A3 in the appendix provides the rates of dismissal by sector. Due to missing information in our sample regarding the sector in which workers are employed, for this part of the analysis the total number of observations decreases further to 21,376.

[13] Results are available upon request.

[14] In doing this, Caroli and Godard (2016) follow the previous work by Bassanini and Garnero (2013). This strategy relies on the strong assumption that the rates of dismissal in the USA provide a valid approximation for what the rates of dismissal in Europe would be if there was no EPL. The strategy of using the USA as a proxy for a counterfactual Europe with less regulation is based on Rajan and Zingales (1998) and it has been applied in several occasions in the economic literature.

[15] To calculate the rate of dismissal, we take for each sector the rate of workers who lose their job from one wave to the next (waves are conducted biannually). We consider three cases of job loss: firing, plant closure, and end of contract. Voluntary job loss, as well as transitions into retirement or disability are not included. We do not use information prior to 2006 since the classification of sectors before that year was too broad to be matched with the classification used by the SHARE.

4 Results

In this section we provide our estimates of the effect of job insecurity on a wide range of health outcomes. First, we estimate effects for general self-reported health, followed by general physical symptoms and mental health. This set of health measures are frequently used in the literature and therefore represent a natural set of outcomes to focus on. In addition, we estimate the effect of job insecurity on two sets of measures that can be considered to be more objective, *i.e.* doctor diagnoses and medicine intake.

4.1 General Health

Table 1 reports the effects of job insecurity on general self-reported health. Column (1) shows that when estimating β_1 by OLS we find a strong correlation between perceived job insecurity and and self reported health. This results suggest that suffering job insecurity increases the chances of reporting poor health by 5.9%. This is a large effect, since the share of individuals in the sample reporting poor health is 18%. When adding control variables to the regression, Column (2) shows that the estimated effect becomes 2.2%, which is already significantly lower. Column (3) shows that adding an individual fixed effect to the specification reduces the estimate to virtually zero, *i.e.* in that case the analysis yields a precisely estimated null effect. Therefore, Columns (1) to (3) in Table 1 show that there is a strong correlation between job insecurity and health, which is partially explained by observables and partially explained by individual-specific unobserved heterogeneity.

Columns (4) and (5) show the results of the IV analyses. The regression in Column (4) uses EPL as an instrument, while the regression in Column (5) uses EPL and its interaction with sector-specific NRD as an instrument. In these cases, the estimates of β_1 appear to be considerably less precise compared to those in Columns (1) to (3). Furthermore, the estimates are of the opposite sign as we would expect, suggesting a positive effect of job insecurity on health.[16] This result may be because the necessary assumptions for the IV analysis, *i.e.* the relevance and the exogeneity assumptions, do not hold.

Regarding the relevance assumption, Table 2 provides the results of the first stage of the IV analysis. Column (1) shows that the EPL index we employ has a strong and significant effect on job insecurity. The estimate of -0.349 indicates that a one standard deviation increase in the EPL index (*i.e.*, an increase of 0.32) implies an 11.17 percentage point decrease in the incidence of job insecurity. As indicated by the large F-value (35.52) this estimate is highly significant and it implies a strong effect of EPL on job insecurity.[17] When interacted with the sector-specific NRD, EPL still has a strong and significant effect on job insecurity. However, contrary to what

[16] This change in the estimation results could be indeed due to the loss of observations in the IV analysis. However, we have rerun the analyses in Columns (1) to (3) of Table 1 and found no significant changes in the estimates of β_1.

[17] This effects appears to be equally large and significant for both individuals with permanent contracts and individuals with temporary contracts. We also did not find heterogeneous effects when comparing regular employees, civil servants, and self-employed individuals. We also employed an EPL index for temporary contracts, but this turned out to be not relevant in the first stage.

Caroli and Godard (2016) suggest, this effect becomes smaller (in absolute terms) as the NRD increases.[18] Therefore, this result does not support the hypothesis that EPL has a stronger effect in sectors with a high NRD.

The results in Table 2 indicate that the instruments are relevant, *i.e.* they have a strong and statistically significant effect on the endogenous variable. However, the results in Table 1 show that the second stage of the IV analyses yields non-significant estimates with a reversed sign compared to expectation. Since IV estimates equal the ratio of the estimate of the reduced form over the estimate of the first stage,[19] it must be that the reversed sign is due to a positive effect of EPL on our poor health dummy in the reduced form estimation. Taken at face value, this would mean that higher EPL has negative consequences for the health of individuals. Given the results of the first stage estimations, this is highly unlikely to reflect an effect of EPL on health that is only channeled through job insecurity. Therefore, we can only conclude that either EPL has direct negative effect on health, or it correlates with unobserved variables that have a negative effect on health. If that is the case, it implies the exogeneity assumption does not hold, which casts a serious doubt on using EPL as an instrumental variable for job insecurity.[20]

4.2 Physical Symptoms

Column (1) of Table 3 shows strong and significant correlations between job insecurity and all physical symptoms we consider, expect for falling down. In all cases, this correlations are smaller than the one reported in Column (1) of Table 1. Column (2) shows that, upon including all control variables in the regressions, all coefficients become smaller and only a few remain significant, some of them at the 5% level. Similarly the analysis for general health, all of the coefficients become even closer to zero and not statistically significant when we include a fixed effect in the estimation equations. In that case, only the effect on swollen legs remains significant. However, this estimate is only significant at the at the 5% leve, and it does not reflect a general pattern whereby physical symptoms would be affected by job insecurity.

Similarly to the results in Table 1, the results of the IV analyses in Table 3 shows effects that are less precisely estimated compared to Columns (1) to (3) and have in almost cases a reversed sign. Since these IV estimates rely on the same first stage regressions reported in Table 2, they imply once more that the reduced form regressions will yield an estimate of the effect EPL on the health outcomes (the physical symptoms in this case) of the opposite sign compared to expectation. Once more, this suggests that factors other than job insecurity interfere in the relation between the instruments we employ and the outcome variables in our regressions.

[18] The average of the sector-specific NRD in our sample is 3.78, which implies an effect of EPL of -0.247. The minimum value for the NRD is 1.84, while the maximum is 6.02. This implies the effect of EPL ranges from -0.288 for sectors with the lowest EPL to -0.201 for sectors with the highest EPL.

[19] The reduced form is a regression of the outcome on the instrument and the other explanatory variables in the model. For a derivation of the formula of the IV estimator, see Angrist and Pischke (2009).

[20] We have also used as an instrument the average job insecurity by country, year, and education level. The results we obtain are very similar to those when using EPL and it's interaction with NRD. Results are available upon request.

4.3 Mental Health

Compared to those reported in Column (1) of Tables 1 and 3, Column (1) of Table 4 shows remarkably strong correlations between presence of mental health conditions and job insecurity. For a few conditions, *i.e.* depression, irritability, and fatigue, the estimated coefficient is very close to or larger than the one reported for general health in Table 1. Similarly to the results reported in Tables 1 and 3, the inclusion of control variables reduces the estimated effects substantially. However, in this case the effect remains significant at the 1% level for nearly all of the outcomes. Only the estimates for lack of appetite and tearfulness become insignificant or nearly insignificant. Most interestingly, in Table 4 there are a few outcomes for which the effect remains significant even after adding an individual fixed effect to the specification. For depression and lack of interest the effect remains significant at the 1% level, while for concentration problems and pessimism it is significant at the 5% and 10% levels respectively.

For all other outcomes analysed so far, controlling for observables and for time-constant unobservables renders the effect of job insecurity not significantly different from zero. Since, in most cases, these are rather precisely estimated zeros, this suggests that all sources of endogeneity are accounted for by the control variables and the fixed effect, and that the actual effect of job insecurity is either zero or so small that it is negligible. Therefore, the fact that, even in the specification including individual fixed effects, we estimate significant effects for a few mental health outcomes is suggestive of the presence of a causal effect of job insecurity on these outcomes. This is in line with the existing literature, which, as mentioned in the introduction, tends to estimate the strongest effects of job insecurity for mental health conditions related with stress and depression.

However, the estimates in Column (3) of Table 4 may still not reflect a causal effect if, for instance, there is a reverse causality mechanism whereby shocks to mental health decrease individuals' perception of their job insecurity. Under the assumptions of relevance and exogeneity, the IV analysis should be able to clear this doubt. However, just like in the results reported in Columns (4) and (5) of Tables 4 and 3 the IV analyses yields low-precision estimates that, more often than not, have the reversed sign compared to expectation. Combined with the results of the first stage of the IV analyses, these results cast further doubt on the validity of the exogeneity assumption for the instruments we employ.

4.4 Doctor Diagnoses and Medicine Intake

As mentioned in Section 3.2, and as it is usual in the related literature, our measures of general health, physical symptoms, and mental health are self-reported. Even though these type of measures are widely used, there is a large literature arguing that the reporting of good or bad health may suffer from individual-specific heterogeneity (*e.g.*, Jürges, 2007; and Johnston *et al.*, 2009). If this heterogeneity is correlated with job insecurity, it will be a source of bias in our analysis. To the extent that this heterogeneity is fixed over time, this bias is already accounted for by the fixed effects estimation. However, this cannot be formally tested. Therefore, we exploit the rich information provided by the SHARE in terms of health outcomes and, following Baker *et al.* (2004), use information on doctor diagnoses and medicine intake as more objective, even though still not perfect, measures of health.

Tables 5 and 6 show the results when using doctor diagnoses and intake of medicines as dependent variables respectively. Regarding doctor diagnoses, Column (1) of Table 5 shows that all correlations have the expected sign. However, all coefficients are very small and in most cases not significantly different from zero.[21] In addition, all coefficient estimates become very small and insignificant when including observables in the regression. Results are similar for medicine intake. In this case we do find somewhat stronger correlations, specially for the intake of medicines for join pain and other pain. However, most effects reported in Column (1) of Table 6 disappear when observables are included, and they all do once an individual fixed effect is included. Both Tables 5 and 6 show that the IV analyses yield once again large standard errors and estimates that very often have the reversed sign with respect to expectation. This contributes to establish the suspicion that EPL, and its interaction with NRD, are not exogenous with respect to a wide of health outcomes.

4.5 Potential Mechanisms

With the exception of a few mental health conditions, we find no statistically significant effects of job insecurity on health outcomes after including individual fixed effects in our estimation equations. Since we estimate rather precise zero effects, these results suggest that the correlations we find conditional on observables are fully explained by time-invariant unobserved heterogeneity across individuals. A possible explanation for this is simply that there may be no true causal effect of job insecurity on health. However, there are other potential explanations for this finding. In this section we discuss a few of these potential explanations and, to the extent that the data allow, we test them empirically.

First of all, it may be that the fixed effect estimations yield null results because the effects of job insecurity on health only take place in the longer run. If changes in job insecurity take several years to have consequences for the health of individuals, the time frame in our sample may not be long enough for the fixed effects estimation to capture these. In that case, the correlations we observe could simply reflect the cumulative effect of years of exposure to job insecurity. It does not seem improbable that job insecurity could have more pervasive effects the longer an individual is exposed to it. In an early contribution, Heaney *et al.* (1994) already talked about job insecurity as a potential chronic stressor, arguing that extended periods of job insecurity may have a negative impact beyond its effects at a single point in time.[22] These long term effects could be especially relevant for the current study given that we employ a sample of older workers. However, they have not been explored in the literature so far, mainly due to

[21] It should be noted in this case that the percentage in the sample of individuals diagnosed for every particular condition is rather low, except for high blood pressure and high cholesterol.

[22] Heaney *et al.* (1994) conducted a small study to explore the long term effects of job insecurity using a sample of about 200 automobile workers in the USA.

data limitations.

Second, since we analyse a sample of older workers, we have to take into account additional reasons why the consequences of job insecurity for this particular group may differ from the population at large. On the one hand, there is a large literature arguing that older workers may face permanent consequences of job loss due to their lower chances of re-employment and the impact on pension eligibility (*e.g.* Tatsiramos, 2010, and Ichino *et al.*, 2017). Therefore, job insecurity may have a strong effect for older workers since workers who lose their job (or are forced to accept a lower paying job) in the years previous to the mandatory retirement age may suffer permanent consequences from that event. On the other hand, there is as well a large literature arguing that younger individuals tend to be liquidity constrained, while older individuals are in a better position to save for the future (*e.g.* Attanasio and Weber, 2010). Therefore, older workers are more likely to have built precautionary savings to be used for smoothing consumption in case job loss and/or a decrease in income occur.

To have an idea of the importance of these arguments we have rerun our fixed effects equations adding an interaction between job insecurity and a dummy variable indicating whether the respondent's household is at the top half of the net financial wealth distribution. For virtually all of the outcomes we analyse, the estimated effects for the upper half and lower half of the distribution are not significantly different from each other. The only exception is for depression. In that case we find a much stronger effect for those at the lower half of the distribution, *i.e.* the estimated effect is 0.049, compared to those at the upper half, the estimated effect is 0.005.[23] [24] This is an interesting result since depression is one of the most common health problems in our sample,[25] and it is one of the few outcomes for which we still estimate a strong and significant effect after including an individual fixed effect.

Finally, there are non-monetary reasons why older workers may suffer less negative consequences from job insecurity. Job loss and unemployment are events that potentially change individuals' time availability for leisure, as well as the social and physical environment in which this leisure takes place. There is a large literature (*e.g.* Coile, 2004; and Brown and Laschever, 2012) suggesting that older individuals may experience important utility gains when stopping with work from increased leisure with friends and/or a spouse who are already retired. Therefore, individuals who are looking forward to retirement may suffer less from the presence of job insecurity, since the potential negative consequences of unemployment late in life maybe mitigated by utility gains from joint leisure with already retired peers.

To get an impression on the relevance of these mechanism, we use a question from the SHARE asking respondents whether they would like to retire from their job as soon as possible, to which

[23]The coefficient estimate for the lower half is significant at the 1% level, while the estimate for the upper half is not significantly different from zero at any reasonable level of significance. The difference between the two coefficients is significant at the 1% level.

[24]The results do not change significantly when using the distributions of net worth and income instead of net financial wealth.

[25]As can be seen in Table 4, 34% of the individual-wave observations in our sample report to suffer depression. It is by far the most common mental condition in our sample. Pooling all outcomes together is the second most commonly reported coming only after back pain.

they can answer yes or no.[26] With the answers to this question, we generate a dummy variable taking value one if an individual reports to be looking forward to retirement and interact it with our job insecurity dummy in all of the fixed effects estimations. However, in this case we do not find any significant difference between the two groups for any of the outcomes that we consider. This indicates that individuals' willingness to retire plays no relevant role in the effect of job insecurity on the health of older workers.

5 Conclusions

In this paper we use a data from the Survey on Health Ageing and Retirement in Europe (SHARE) to study the effects of job insecurity on the health of older European workers. Applying standard Probit estimation we find strong correlations between job insecurity and a wide range of health outcomes (*i.e.* self-reported general health, physical symptoms, mental conditions, conditions diagnosed by a doctor, and medicine intake). However, when we include an individual fixed effects all of the estimated effects become statistically insignificant, except for a few of the mental conditions. We believe it is unlikely that the insignificant effects from the fixed effects estimation are due to a lack of statistical power, since the null effects are quite precisely estimated.

In addition, we apply an IV method proposed by Caroli and Godard (2016), which consists of using employment protection legislation (ELP) and its interaction with the sector-specific natural rate of dismissal (NRD) as instruments for job insecurity. We fail to replicate the results by Caroli and Godard (2016) since our IV analyses yield high standard errors and estimates that in most cases have the opposite sign with respect to expectation, *i.e.* suggesting that job insecurity would be good for health. The first stage regressions show that EPL has a strong effect on job insecurity with the expected sign. Therefore, since the estimates of the effect of job insecurity in the second stage regression are in most cases insignificant and have the reversed sign, we argue that there must be unobserved factors that correlate both with EPL and with the health outcomes we employ. The same argument holds when we use the interaction between EPL and NRD as the instrument. This results suggest the instruments we use are not exogenous, and thus cast a strong doubt in their validity for this type of analysis.

Regardless of the inconclusive IV results, our standard Probit and fixed effects estimations allow concluding that for older workers there is a strong correlation between job insecurity and a wide range of health outcomes. In addition, this correlation is partially explained by observable characteristics (*i.e.* demographic and economic variables), and partially by time-invariant unobserved heterogeneity across individuals. One of the very few mental conditions for which we still find an effect after controlling for observables and the individual fixed effect is depression, which is one of the most common health conditions in our sample. Further analysis shows that this effect on depression is specially strong and significant for individuals in lower

[26] Note that this question does not specify the motive why individuals would like to retire. We assume here it mostly captures the willingness of individuals to increase their leisure time.

half of the wealth distribution. This result provides suggestive evidence that older workers may suffer less from job insecurity if they have enough wealth to mitigate the negative effects of a potential negative change in their income.

References

Angrist, Joshua, and Jörn-Steffen Pischke. 2009. *Mostly Harmless Econometrics.* Princenton: Princeton University Press.

Attanasio, Orazio P, and Guglielmo Weber. 2010. "Consumption and saving: Models of intertemporal allocation and their implications for public policy." *Journal of Economic Literature*, 48(3): 693–751.

Baker, Michael, Mark Stabile, and Catherine Deri. 2004. "What do self-reported, objective, measures of health measure?" *Journal of Human Resources*, 39(4): 1067–1093.

Bassanini, Andrea, and Andrea Garnero. 2013. "Dismissal protection and worker flows in OECD countries: Evidence from cross-country/cross-industry data." *Labour Economics*, 21: 25–41.

Brown, Kristine, and Ron Laschever. 2012. "When they're sixty-four: Peer effects and the timing of retirement." *American Economic Journal: Applied Economics*, 4(3): 90–115.

Caroli, Eve, and Mathilde Godard. 2016. "Does job insecurity deteriorate health?" *Health Economics*, 25(2): 131–147.

Coile, Courtney. 2004. "Retirement incentives and couples' retirement decisions." *The BE Journal of Economic Analysis & Policy*, 4(1).

Cottini, Elena, and Paolo Ghinetti. 2018. "Employment insecurity and employees' health in Denmark." *Health Economics*, 27(2): 426–439.

de Witte, Hans, Jaco Pienaar, and Nele de Cuyper. 2016. "Review of 30 years of longitudinal studies on the association between job insecurity and health and well-being: Is there causal evidence?" *Australian Psychologist*, 51(1): 18–31.

Greene, William. 2004. "The behaviour of the maximum likelihood estimator of limited dependent variable models in the presence of fixed effects." *The Econometrics Journal*, 7(1): 98–119.

Green, Francis. 2011. "Unpacking the misery multiplier: How employability modifies the impacts of unemployment and job insecurity on life satisfaction and mental health." *Journal of Health Economics*, 30(2): 265–276.

Heaney, Catherine, Barbara Israel, and James House. 1994. "Chronic job insecurity among automobile workers: Effects on job satisfaction and health." *Social science & medicine*, 38(10): 1431–1437.

Ichino, Andrea, Guido Schwerdt, Rudolf Winter-Ebmer, and Josef Zweimüller. 2017. "Too old to work, too young to retire?" *The Journal of the Economics of Ageing*, 9: 14–29.

Johnston, David, Carol Propper, and Michael Shields. 2009. "Comparing subjective and objective measures of health: Evidence from hypertension for the income/health gradient." *Journal of Health Economics*, 28(3): 540–552.

Jürges, Hendrik. 2007. "True health vs response styles: exploring cross-country differences in self-reported health." *Health Economics*, 16(2): 163–178.

Mundlak, Yair. 1978. "On the pooling of time series and cross section data." *Econometrica: Journal of the Econometric Society*, 69–85.

OECD. 2013. *OECD Employment Outlook 2013.* Paris: OECD Publishing.

OECD. 2019. *OECD Employment Outlook 2019: The Future of Work.* Paris: OECD Publishing.

Rajan, Raghuram, and Luigi Zingales. 1998. "Financial dependence and growth." *The American Economic Review*, 88(3): 559.

Reichert, Arndt, and Harald Tauchmann. 2017. "Workforce reduction, subjective job insecurity, and mental health." *Journal of Economic Behavior & Organization*, 133: 187–212.

Rohde, Nicholas, Kam Ki Tang, Lars Osberg, and Prasada Rao. 2016. "The effect of economic insecurity on mental health: Recent evidence from Australian panel data." *Social Science & Medicine*, 151: 250–258.

Shoss, Mindy. 2017. "Job insecurity: An integrative review and agenda for future research." *Journal of Management*, 43(6): 1911–1939.

Tatsiramos, Konstantinos. 2010. "Job displacement and the transitions to re-employment and early retirement for non-employed older workers." *European Economic Review*, 54(4): 517–535.

Tables

Table 1: Results - Effect of Job Insecurity on Self-Reported Health

		(1)	(2)	(3)	(4)	(5)
Outcome	Avg.	PROB-1	PROB-2	FE	IV-1	IV-2
Self-reported health	0.18	0.054***	0.019***	0.004	-0.039	-0.064
		(0.004)	(0.005)	(0.006)	(0.056)	(0.105)
Observations		48,716	48,716	48,716	33,568	21,376

Notes: Standard errors (clustered at the household level) are reported in parenthesis. All regressions include year and country dummies. The regression in Column (1) does not include control variables, all other regressions include the control variables mentioned in Section 3.3. All regressions are estimated using Probit models. Average marginal effects are provided. For further information on the empirical specification and estimation methods, see Section 2. ***Significant at the 1% level, **significant at the 5% level, *significant at the 1% level.

Table 2: Results - First Stage IV Analysis

Instruments	(1)	(2)
EPL	-0.349***	-0.327***
	(0.059)	(0.088)
EPL×NRD		0.021***
		(0.007)
F-value	35.52	17.75
Observations	33,568	21,376

Notes: Standard errors (clustered at the household level) are reported in parenthesis. All regressions include year and country dummies, as well as all the control variables mentioned in Section 3.3. All regression are estimated using Probit models. Average marginal effects are provided. The F-value in Column (1) refers to the null hypothesis that the coefficient for EPL is zero, while the F-value in Column (2) refers to the null hypothesis that the coefficients for both EPL and EPL×NRD are zero. ***Significant at the 1% level, **significant at the 5% level, *significant at the 1% level.

Table 3: Results - Effect of Job Insecurity on Physical Symptoms

		(1)	(2)	(3)	(4)	(5)
Dep. variable	Avg.	PROB-1	PROB-2	FE	IV-1	IV-2
Pain back and/or joints	0.45	0.044***	0.010	0.001	-0.108*	-0.139*
		(0.007)	(0.007)	(0.009)	(0.062)	(0.076)
Heart trouble	0.03	0.011***	0.007***	0.003	-0.020	-0.037
		(0.002)	(0.002)	(0.005)	(0.032)	(0.035)
Breathlessness	0.06	0.019***	0.009***	0.007	-0.050**	-0.047*
		(0.003)	(0.003)	(0.007)	(0.023)	(0.027)
Persistent cough	0.04	0.008***	0.003	-0.000	-0.007	-0.045
		(0.003)	(0.003)	(0.007)	(0.032)	(0.085)
Swollen legs	0.07	0.016***	0.009**	0.013**	-0.034	-0.014
		(0.003)	(0.003)	(0.006)	(0.073)	(0.096)
Sleeping problems	0.16	0.023***	0.008	0.007	-0.038	-0.114
		(0.005)	(0.005)	(0.010)	(0.058)	(0.085)
Falling down	0.01	0.001	0.000	-0.002	-0.049	0.013
		(0.001)	(0.001)	(0.003)	(0.031)	(0.022)
Dizziness and/or faints	0.04	0.014***	0.007**	0.002	0.029	0.009
		(0.003)	(0.003)	(0.006)	(0.034)	(0.050)
Stomach problems	0.10	0.019***	0.012***	0.006	-0.065	-0.069
		(0.004)	(0.004)	(0.009)	(0.042)	(0.074)

Notes: Standard errors (clustered at the household level) are reported in parenthesis. All regressions include year and country dummies. The regressions in Column (1) do not include control variables, all other regressions include the control variables mentioned in Section 3.3. All regressions are estimated using Probit models. Average marginal effects are provided. For further information on the empirical specification and estimation methods, see Section 2. ***Significant at the 1% level, **significant at the 5% level, *significant at the 1% level.

Table 4: Results - Effect of Job Insecurity on Mental Health

Dep. variable	Avg.	(1) PR-1	(2) PR-2	(3) FE	(4) IV-1	(5) IV-2
Depression	0.34	0.064***	0.034***	0.024***	-0.070	-0.109
		(0.005)	(0.005)	(0.009)	(0.069)	(0.084)
Pessimism	0.10	0.029***	0.014***	0.009	0.011	0.031
		(0.003)	(0.003)	(0.006)	(0.054)	(0.073)
Suicidality	0.04	0.016***	0.006***	0.000	-0.020	-0.021
		(0.002)	(0.002)	(0.003)	(0.014)	(0.016)
Guilt	0.08	0.021***	0.012***	0.007	-0.037	-0.077**
		(0.003)	(0.003)	(0.005)	(0.032)	(0.031)
Lack of interest	0.05	0.022***	0.012***	0.017***	0.018	0.012
		(0.002)	(0.002)	(0.004)	(0.022)	(0.033)
Irritability	0.27	0.056***	0.025***	0.013	-0.049	-0.053
		(0.005)	(0.005)	(0.008)	(0.050)	(0.070)
Lack of appetite	0.05	0.013***	0.005*	-0.001	-0.087*	-0.126**
		(0.002)	(0.002)	(0.004)	(0.045)	(0.056)
Fatigue	0.27	0.063***	0.028***	0.013	-0.043	-0.093
		(0.005)	(0.005)	(0.008)	(0.072)	(0.104)
Concentration problems	0.12	0.032***	0.015***	0.012**	-0.080**	-0.085
		(0.003)	(0.004)	(0.006)	(0.038)	(0.052)
Lack of enjoyment	0.08	0.021***	0.008***	-0.001	0.005	-0.010
		(0.003)	(0.003)	(0.006)	(0.052)	(0.064)
Tearfulness	0.21	0.020***	0.007	0.003	-0.025	-0.060
		(0.005)	(0.005)	(0.008)	(0.071)	(0.080)

Notes: Standard errors (clustered at the household level) are reported in parenthesis. All regressions include year and country dummies. The regressions in Column (1) do not include control variables, all other regressions include the control variables mentioned in Section 3.3. All regressions are estimated using Probit models. Average marginal effects are provided. For further information on the empirical specification and estimation methods, see Section 2. ***Significant at the 1% level, **significant at the 5% level, *significant at the 1% level.

Table 5: Results - Effect of Job Insecurity on Doctor's Diagnoses

		(1)	(2)	(3)	(4)	(5)
Dep. variable	Avg.	PR-1	PR-2	FE	IV-1	IV-2
Heart attack	0.05	0.007***	0.004*	0.000	0.002	-0.090
		(0.002)	(0.002)	(0.003)	(0.028)	(0.098)
High blood pressure	0.25	0.005	0.001	-0.012*	0.111**	0.130**
		(0.005)	(0.005)	(0.006)	(0.049)	(0.059)
High cholesterol	0.17	0.008*	0.007*	-0.004	-0.001	-0.029
		(0.004)	(0.004)	(0.006)	(0.044)	(0.062)
Stroke	0.01	0.001	0.000	0.001	0.010	0.015
		(0.001)	(0.001)	(0.002)	(0.021)	(0.034)
Diabetes	0.05	0.004	-0.002	-0.005	-0.014	-0.011
		(0.003)	(0.003)	(0.003)	(0.028)	(0.029)
Lung disease	0.03	0.005***	0.001	-0.002	-0.016	-0.014
		(0.002)	(0.002)	(0.003)	(0.019)	(0.025)
Arthritis	0.12	0.007	0.007	0.015*	-0.148***	-0.144*
		(0.004)	(0.004)	(0.008)	(0.051)	(0.077)
Cancer	0.02	0.000	0.001	-0.000	-0.018	-0.018
		(0.002)	(0.002)	(0.003)	(0.015)	(0.018)
Ulcer	0.03	0.009***	0.004*	-0.003	-0.067	-0.095
		(0.000)	(0.002)	(0.003)	(0.154)	(0.059)

Notes: Standard errors (clustered at the household level) are reported in parenthesis. All regressions include year and country dummies. The regressions in Column (1) do not include control variables, all other regressions include the control variables mentioned in Section 3.3. All regressions are estimated using Probit models. Average marginal effects are provided. For further information on the empirical specification and estimation methods, see Section 2. ***Significant at the 1% level, **significant at the 5% level, *significant at the 1% level.

Table 6: Results - Effect of Job Insecurity on Medicine Intake

		(1)	(2)	(3)	(4)	(5)
Dep. variable	Avg.	PR-1	PR-2	FE	IV-1	IV-2
Cholesterol	0.12	0.008**	0.008**	-0.002	0.075**	0.076*
		(0.004)	(0.004)	(0.005)	(0.034)	(0.043)
Blood pressure	0.23	0.001	0.000	-0.008	0.141***	0.185***
		(0.005)	(0.005)	(0.006)	(0.041)	(0.046)
Coronary disease	0.03	0.005***	0.003*	-0.002	0.038	0.039
		(0.002)	(0.002)	(0.003)	(0.023)	(0.028)
Other heart disease	0.03	0.005***	0.004**	0.000	0.014	0.004
		(0.002)	(0.002)	(0.003)	(0.016)	(0.030)
Diabetes	0.05	0.002	-0.002	-0.005*	-0.027	-0.046**
		(0.003)	(0.003)	(0.003)	(0.030)	(0.018)
Joint pain	0.08	0.021***	0.006**	0.005	0.014	0.000
		(0.003)	(0.003)	(0.005)	(0.045)	(0.027)
Other pain	0.09	0.029***	0.016***	0.008	-0.129	-0.129
		(0.003)	(0.003)	(0.006)	(0.090)	(0.109)
Sleep problems	0.04	0.007***	0.003	-0.002	0.012	0.045
		(0.002)	(0.002)	(0.003)	(0.030)	(0.031)
Anxiety and/or depression	0.04	0.006***	0.000	-0.005	-0.015	-0.046**
		(0.002)	(0.002)	(0.003)	(0.018)	(0.023)
Osteoporosis	0.01	0.001	0.002	-0.001	0.018	0.013
		(0.001)	(0.001)	(0.002)	(0.013)	(0.013)
Stomach burns	0.05	0.006***	0.002	-0.002	0.001	-0.047
		(0.002)	(0.002)	(0.004)	(0.027)	(0.048)
Chronic bronchitis	0.01	0.001	-0.000	-0.001	0.009	0.009
		(0.001)	(0.001)	(0.002)	(0.010)	(0.016)

Notes: Standard errors (clustered at the household level) are reported in parenthesis. All regressions include year and country dummies. The regressions in Column (1) do not include control variables, all other regressions include the control variables mentioned in Section 3.3. All regressions are estimated using Probit models. Average marginal effects are provided. For further information on the empirical specification and estimation methods, see Section 2. ***Significant at the 1% level, **significant at the 5% level, *significant at the 1% level.

Appendix

Table A1: Summary Statistics

Variable	Mean	Median	St. Dev.	Min.	Max.
Job insecurity	0.23	-	-	0	1
Gender	0.52	-	-	0	1
Age	56	56.38	5.41	24	99
Marital status					
Married with cohabitation	0.74	-	-	0	1
Registered Partnership	0.02	-	-	0	1
Married without cohabitation	0.02	-	-	0	1
Never married	0.07	-	-	0	1
Divorced	0.11	-	-	0	1
Widowed	0.03	-	-	0	1
Education					
None	0.01	-	-	0	1
ISCED code 1	0.09	-	-	0	1
ISCED code 2	0.15	-	-	0	1
ISCED code 3	0.36	-	-	0	1
ISCED code 4	0.06	-	-	0	1
ISCED code 5	0.31	-	-	0	1
ISCED code 6	0.01	-	-	0	1
Household disposable income	38,094	24,436	58,799	0	4,414,968
Sector					
Agriculture, forestry, and fishing	0.05	-	-	0	1
Mining and quarrying	0.01	-	-	0	1
Manufacturing	0.12	-	-	0	1
Electricity, gas, and water supply	0.02	-	-	0	1
Construction	0.06	-	-	0	1
Wholesale and retail trade	0.09	-	-	0	1
Hotels and restaurants	0.03	-	-	0	1
Transport, storage, and comm.	0.06	-	-	0	1
Financial intermediation	0.03	-	-	0	1

Table A1: Summary Statistics - Continued

Variable	Mean	Median	St. Dev.	Min.	Max.
Real estate, renting and business activities	0.02	-	-	0	1
Public administration and defence	0.09	-	-	0	1
Education	0.12	-	-	0	1
Health and social work	0.15	-	-	0	1
Other community, social, and personal services	0.14	-	-	0	1
Occupation					
Managers	0.11	-	-	0	1
Professionals	0.17	-	-	0	1
Technicians and associate professionals	0.13	-	-	0	1
Clerks	0.15	-	-	0	1
Service and sales workers	0.17	-	-	0	1
Skilled agricultural, forestry and fishery workers	0.03	-	-	0	1
Craft and related trades workers	0.10	-	-	0	1
Plant and machine operators and assemblers	0.05	-	-	0	1
Elementary occupations	0.09	-	-	0	1
Armed forces occupations	0.01	-	-	0	1
Temporary contract	0.09	-	-	0	1
Plus one job	0.07	-	-	0	1
Employee	0.63	-	-	0	1
Civil servant	0.19	-	-	0	1
Self-employed	0.17	-	-	0	1
Seniority (years)	19.97	19	12.87	0	75
Weekly hours worked	37.98	40	12.99	1	168
Months worked	11.76	12	1.02	1	12
Physical					
Strongly disagree	0.21	-	-	0	1
Disagree	0.33	-	-	0	1
Agree	0.27	-	-	0	1
Strongly agree	0.19	-	-	0	1

Table A1: Summary Statistics - Continued

Variable	Mean	Median	St. Dev.	Min.	Max.
Pressure					
Strongly disagree	0.12	-	-	0	1
Disagree	0.40	-	-	0	1
Agree	0.32	-	-	0	1
Strongly agree	0.16	-	-	0	1
Freedom					
Strongly disagree	0.30	-	-	0	1
Disagree	0.43	-	-	0	1
Agree	0.18	-	-	0	1
Strongly agree	0.09	-	-	0	1
New skills					
Strongly disagree	0.08	-	-	0	1
Disagree	0.22	-	-	0	1
Agree	0.47	-	-	0	1
Strongly agree	0.23	-	-	0	1
Recognition					
Strongly disagree	0.07	-	-	0	1
Disagree	0.21	-	-	0	1
Agree	0.52	-	-	0	1
Strongly agree	0.20	-	-	0	1
Salary					
Strongly disagree	0.13	-	-	0	1
Disagree	0.30	-	-	0	1
Agree	0.45	-	-	0	1
Strongly agree	0.12	-	-	0	1

Table A1: Summary Statistics - Continued

Variable	Mean	Median	St. Dev.	Min.	Max.
Prospects					
Strongly disagree	0.08	-	-	0	1
Disagree	0.26	-	-	0	1
Agree	0.40	-	-	0	1
Strongly agree	0.25	-	-	0	1
Support					
Strongly disagree	0.06	-	-	0	1
Disagree	0.19	-	-	0	1
Agree	0.53	-	-	0	1
Strongly agree	0.21	-	-	0	1

Notes: All summary statistics are computed using the sample employed for the estimations of the standard Probit models and the fixed effects analysis. For more information on the variables and the sample, see Section 3 in the main text.

Table A2: EPL Index Across Countries and Over Time

Country	Wave 1	Wave 2	Wave 4	Wave 5
Austria	2.62	2.62	2.62	2.62
Belgium	2.82	2.82	2.95	2.82
Czech Republic	-	2.85	2.79	2.70
Denmark	2.56	2.35	2.39	2.39
Estonia	-	-	2.11	2.11
France	2.73	2.73	2.67	2.67
Germany	2.95	2.95	2.95	2.95
Greece	2.93	2.93	-	-
Israel	-	1.99	-	1.99
Italy	3.15	3.15	3.15	2.98
Netherlands	2.92	2.92	2.87	2.93
Poland	-	2.41	2.41	
Portugal	-	-	3.49	-
Slovenia	-	-	2.82	2.82
Spain	2.76	2.76	2.65	2.43
Sweden	2.58	2.58	2.58	2.58
Switzerland	2.18	2.18	2.18	2.18
Average	2.76	2.66	2.62	2.51

Notes: For more information on the EPL index provided by the OECD, see main text, and OECD (2013). The missing cells correspond to countries that were not included in a particular wave of the SHARE.

Table A3: Sector-Specific Dismissal Rates (USA Average 2004-2012)

Sector	Rate of Dismissal
Agriculture, forestry, and fishing	5.60%
Mining and quarrying	3.03%
Manufacturing	4.03%
Electricity, gas, and water supply	3.74%
Construction	6.02%
Wholesale and retail trade	4.11%
Hotels and restaurants	5.40%
Transport, storage, and communication	4.29%
Financial intermediation	4.35%
Real estate, renting and business activities	4.56%
Public administration and defence	2.08%
Education	1.84%
Health and social work	2.08%
Other community, social, and personal services	4.00%

Notes: Average rates of dismissal are computed using the waves between 2004 and 2012 of the Health and Retirement Study. For more information about the computation, see main text.