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Eduard Suari-Andreu
Leiden University and Netspar

Tim Schwartz
SEO Amsterdam Economics

Max van Lent
Leiden University and IZA

Marike Knoef
Leiden University, Tilburg University and Netspar

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IZA – Institute of Labor Economics

Schaumburg-Lippe-Straße 5–9
53113 Bonn, Germany

Phone: +49-228-3894-0
Email: publications@iza.org

www.iza.org

ABSTRACT

Job Insecurity and Health: Evidence from Older European Workers*

A rich literature has studied the effect of job insecurity on health. However the causal link between these two variables remains unclear. We study the relationship between perceived job insecurity and health using longitudinal data on around 30 thousand older workers from 20 European countries covering a period of 14 years. The unprecedented size and nature of the dataset compared to previous studies on job insecurity and health allows us to apply different estimation methods and compare the results obtained. We do so using a wide range of health outcomes that include objective and subjective measures. Using pooled OLS, we estimate a strong association between job insecurity and health outcomes. A fixed effect estimator yields precisely estimated zeros with the exception of a few mental conditions. Additionally, we test the robustness of an IV strategy that uses an index for employment protection legislation (EPL) as an instrument for job insecurity. We conclude that the direct causal link 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, employment protection legislation

Corresponding author:
Eduard Suari-Andreu
Department of Economics
Leiden University
Steenschuur 25
2311 ES, Leiden
The Netherlands

E-mail: e.suari.andreu@law.leidenuniv.nl

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1 Introduction

Over the past few decades technological and socioeconomic changes have led to increasingly flexible work relations in developed economies (OECD, 2019). Permanent contracts have become less prevalent, often leading to an increase in job insecurity for workers. Job insecurity, which in the present study we define as *a perceived threat to the continuity and stability of employment as it is currently experienced* (Shoss, 2017),[1] increases workers' uncertainty about their future economic situation and thus can have negative effects on their mental and physical health. Therefore, policymakers should consider potential negative effects of flexible work relations on health and well-being via increased job insecurity. For this reason, any contribution to our understanding of the causal effect of job insecurity on workers' health is relevant.

The academic literature studying the health effects of perceived job insecurity originates from different fields of study in the social sciences. Within the field of occupational psychology, several studies point at a positive correlation between perceived job insecurity and poor health. Both de Witte *et al.* (2016) and Shoss (2017) provide thorough reviews of this literature. They show that the correlation between job insecurity and poor health is the strongest for mental health conditions as well as for physical symptoms related to stress and depression. However, there are two important 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. For instance, if job insecurity and health are self-reported, individuals who are generally pessimistic may consistently report poor health and high job insecurity.[2] In addition, there could be shocks that affect both health and job insecurity. For instance, if a close family member suffers a health shock that leads to informal care commitments, this may affect both the health and the job insecurity of the caregiver. Second, reverse causality may also play a role. That is because the health status of an individual can have important consequences on her job status. Therefore, changes in health can affect job insecurity if the worker anticipates potential consequences for her job status.

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 estimating regressions 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 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 under the assumption that such events do not affect health through any channel

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

[2]This can also be framed as a case of correlated measurement error.

other than job insecurity. However, staff reductions can arguably have an effect on health through other channels since they often involve changes in tasks and/or the work environment. For instance, stress and anxiety can increase when colleagues are fired and/or the organization within a firm experiences substantial changes. In addition, firms implementing staff reductions may not be representative of all firms.

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. They do so using a sample of employed males in OECD countries. This strategy relies on the assumption that country-level EPL affects 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).[3] Their argumentation is that EPL is likely to have a stronger effect in those sectors with a higher 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 study, we further investigate the relation between perceived job insecurity and health using 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 14 years. We contribute to the literature in several ways. First, thanks to the richness of the data we are able to investigate a comprehensive range of objective health measures (doctor diagnoses and medicine intake) and we can compare them with more traditional subjective health measures (self-reported general health, physical symptoms, and mental conditions). Second, the size and the longitudinal dimension of the sample allow us to apply several of the methods employed in the literature (*i.e.*, pooled OLS, fixed effects, and IV) with a considerably higher degree of statistical power relative to previous studies and compare the results obtained. In that way, we can assess the validity of different methods in estimating the causal effect of job insecurity. To our knowledge, we are the first to estimate the effect of job insecurity on health using a large longitudinal sample for Europe. Third, we test the robustness of the IV method proposed by Caroli and Godard (2016) by using a larger sample size, exploiting time as an extra source of variation, and including both males and females in the sample.

The focus on a sample of workers who are 50 years old or more is interesting for several reasons. For instance, job insecurity may be especially worrisome for older workers, since, compared to younger workers, it is often more difficult for them to find employment after a job loss (Tatsiramos, 2010; Ichino *et al.*, 2017). In addition, they are likely to face a permanent reduction in their pension income if they lose their job in the final years prior to mandatory retirement.

[3]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).

Conversely, there may be non-monetary effects that could attenuate the consequences of job insecurity at older ages. For instance, older workers who look forward to retiring to enjoy additional leisure time with other retired peers may be less affected by job insecurity in case they already have sufficient financial security. It is well documented that there are peer effects in the preference and timing of retirement (Brown and Laschever, 2012; Vermeer *et al.*, 2019). 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 that relate to this particular demographic.

When estimating pooled OLS models without controlling for observables we find strong and significant correlations between perceived job insecurity and nearly all health outcomes. These correlations are especially strong for the self-reported general health status and for the mental health outcomes. However, the estimates become substantially smaller when we include observables in our regressions (*i.e.* both demographic and economic variables), and they virtually disappear when including individual fixed effects. Only a few mental health outcomes (*i.e.* depression, lack of interest, and concentration problems) remain significant upon the inclusion of fixed effects but the effects are relatively small and time-varying endogeneity cannot be ruled out. The fixed effects are interesting since they are the first estimated using European data, they yield precisely estimated zeros, and are based on substantially more variation than those estimated by Green (2011) and Rohde *et al.* (2016). The latter conduct fixed effect estimations based on smaller samples of Australian data.

Finally, the IV analysis does not yield conclusive results. That is even though we use a larger sample and we have more variation in EPL than Caroli and Godard (2016). In line with their findings, the standard errors increase when applying the IV strategy, which is likely due to the small variation in EPL. Nevertheless, regardless of the small variation, EPL appears in our case to be a relevant instrument, *i.e.* it has a significant effect on perceived job insecurity. However, due to small variation in the EPL index and its likely endogeneity with respect to health, the IV results are rendered inconclusive. This means that this IV strategy is not robust to the to a larger sample, additional variation, the inclusion of females in the sample, and the use of a wider range of health outcomes. This casts relevant doubts on the robustness of this IV method.

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. Since upon the inclusion of fixed effects we estimate rather precise zero effects of job insecurity on health, we conclude that the direct causal effect of job insecurity on health is, if any, rather weak. This results indicate that, conditional on observables, the sources of endogeneity (*i.e.* omitted variable bias and reverse causality) are fixed over time. We do find a significant effect of job insecurity on a self-reported measure of depression that survives the inclusion of fixed effects. However, the coefficient estimate is small and is not supported by our objective measures of depression, which are less vulnerable to reporting bias.

The remainder of the document 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 set up the following equation

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

where i, c, and t are individual, country, and year subindices respectively, H_{ict} is a dummy variable taking value one if an individual is in poor 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 pooled OLS with and without including control variables.[5] Subsequently, we estimate β_1 including individual fixed effects in the specification. By adding fixed effects, we control for all time-invariant unobserved heterogeneity across individuals, *i.e.* η_i in Equation (1), and thus we reduce the potential for endogeneity in the estimation.

If the sources of endogeneity are not fully constant over time or if reverse causality plays a role, 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). This IV strategy works as long as the instrument is relevant, *i.e.* EPL has a strong effect on job insecurity, and the instrument is exogenous, *i.e.* EPL affects health only through job insecurity. Caroli and Godard (2016) lag EPL by two years. However, since our results do not change when we do so and we lack an obvious reason to lag EPL, we restrict ourselves to reporting the estimation of the contemporaneous effect of EPL.

Note that the variable EPL_{ct} in (2) changes across countries and over time. Therefore, our specification extends 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

[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]For all models and estimation techniques that we consider Probit estimation yields virtually the same results obtained with linear probability estimation. Results of the Probit estimations are available upon request. For the fixed effect Probit estimation we use the method by Mundlak (1978) to control for time-invariant unobserved heterogeneity.

(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 and variation over time in EPL is scarce, we perform an additional IV estimation using the interaction between EPL and the sector-specific NRD as an instrument. This consists of including $\delta_1 NRD_s + \delta_2 EPL_{ct} \times NRD_s$ in Equation (2), 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.* variation across sectors, to the variation over time and across countries. The disadvantage is that selection into sectors may not be random, and that the measurement of the NRD requires some additional assumptions as we outline in Section 3.

It is important to note that for our baseline IV estimations we do not include fixed effects in the specification. That is because, if the IV assumptions hold, all source of endogeneity should be accounted for. In addition, since we use an instrument that varies at the country level, it is unlikely that the latter will be correlated with unobserved heterogeneity at the individual level once country effects are already accounted for. However, to ensure that sources of endogeneity that are fixed over time do not interfere in the IV estimation, we consider as well the combination of IV with fixed effects as an additional estimator.

3 Data and Sample Selection

We use 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 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 meaning that every individual is observed for 1.56 waves on average.[7]

[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. For more details on the SHARE and an overview of the countries included in each wave, see Bergmann *et al.* (2019).

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

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 job insecurity. 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 thus not the employability of the worker conditional on job loss. These measures are usually based on questions that ask individuals about the possibility of losing their job in the near future. As an example, Caroli and Godard (2016) use the question "what is the possibility that you lose your current job in the next six 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 as most relevant.[8]

With this caveat in mind, we use a question from the SHARE that, instead of asking about the chance of job loss, asks individuals about the extent to which they agree with the statement: "my job security is poor". The possible answers are "strongly agree", "agree", "disagree", and "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.[9] 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. In this way, it is in line with the second type of measures of job insecurity mentioned above.

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 considerably larger identifying 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

[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.

[9]We do this since it makes the results easier to grasp and interpret. Using an indicator variable with four possible values does not significantly change the results.

contains very comprehensive information on each of these. However, an important caveat to take into account is that they are all based on individuals' perception of their own health status. Self-reported health measures are widely employed in the literature and it has been repeatedly shown that they contain useful information (Bath, 2003; and van Ooijen *et al.*, 2015). However, there are studies 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 accounted by our control variables or is fixed over time, this bias is already corrected in our estimations. However, this cannot be directly tested. Therefore, we exploit the rich information provided by the SHARE in terms of health outcomes and use information on doctor diagnoses and medicine intake as measures of health. As argued by Baker *et al.* (2004) and van Ooijen *et al.* (2015), these measures can be considered to be more objective than those directly based on individuals' perception of their own health.

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 medicine intake, 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.

For the sake of comparison, we use as well the measures provided by the SHARE for self-reported general health, physical symptoms, and mental health. As a measure of self-reported general health the SHARE asks 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.[10]

Regarding the measurement of physical symptoms, we employ a battery of questions from the SHARE asking individuals whether they suffer from back pain, heart trouble, breathlessness, persistent cough, swollen legs, sleep problems, falling down, dizziness and/or faints, and stomach problems.[11] 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.

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,

[10]Using an indicator variable that takes four values does not significantly change the results.
[11]Unfortunately, the questions on physical symptoms are only available up to wave four of the SHARE.

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 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.[12]

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,[13] 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, following Caroli and Godard (2016) we include to the vector of work related variables a battery of measures provided by the SHARE that capture additional job characteristics and working conditions. These are provided as a series of statements to which individuals can answer "strongly agree", "agree", "disagree", or "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". Our motivation to include these variables is based on Caroli and Godard (2016), who argue that perceived job insecurity may be caused by the working conditions. However, because some of these variables maybe endogenous with respect to job insecurity, we rerun our estimations excluding this set of variables and find that the results are not significantly different.[14]

3.4 Instrumental Variables

As mentioned in Section 2, in this study we follow Caroli and Godard (2016) and use employment protection legislation (EPL) as an instrument for perceived job insecurity. According to the OECD (2020), EPL is 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 as 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

[12]Table A1 in the appendix provides summary statistics for all control variables that we employ.

[13]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.

[14]Given the similarity between the sets of results, we restrict ourselves here to the reporting of the results obtained when including all control variables.

no employment protection and six means full protection.[15]

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 takes values from 1.79 to 3.49, with an average of 2.60, 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 labour reforms that have generally decreased the stringency of EPL (OECD, 2020). This can be appreciated in Table A2, where the last row shows that the average value for the EPL index has declined from 2.75 to 2.51, *i.e.* nearly one full standard deviation, 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 the 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.[16]

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 2016, which roughly coincides with the years in the sample used for the IV analysis.[17] Table A3 in the appendix provides the rates of dismissal by sector. Due to missing information regarding the sector in which workers are employed, for this part of the analysis the total number of observations in the sample is 34,049.

4 Results

In this section we provide our estimates on the effect of job insecurity on a wide range of health outcomes. First, we estimate the effect of job insecurity on two sets of measures, *i.e.*

[15] For more information on how this EPL is computed, see OECD (2020). The OECD also provides an EPL index for temporary contracts. We do not use this indicator since, as we mention in the results section, its estimated effect on the endogenous variable appears to be not significantly different from zero.

[16] In doing this, Caroli and Godard (2016) follow the previous work by Bassanini and Garnero (2013). This strategy relies on the 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 is commonly applied in the economic literature.

[17] 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 causes of job loss: firing, plant closure, and end of contract. Voluntary job loss and transitions into retirement or disability are not included. We do not use information prior to 2006 since the classification of sectors in the HRS before that year is too broad to be matched with the classification used by the SHARE.

doctor diagnoses and medicine intake, that can be considered to be more objective than the health measures usually employed in the literature. In addition, we report the estimated effects for the more traditional measures, *i.e.* general self-reported health, followed by self-reported general physical symptoms and mental health. All estimation results are obtained using linear probability models. Probit estimations yield similar conclusions.

4.1 Doctor Diagnoses and Medicine Intake

Tables 1 and 2 show the results when using doctor diagnoses and intake of medicines as dependent variables respectively. Regarding doctor diagnoses, Column (1) of Table 1 shows that all correlations have the expected sign. That is, job insecurity positively correlates with all conditions for which we have information on doctor's diagnoses. However, all coefficients are very small and in most cases not significantly different from zero.[18] In addition, all coefficient estimates become very small and insignificant when including observables in the regression in Column (2) and even more so when including fixed effects in Column (3).

Results in Columns (1) to (3) are similar for medicine intake. In this case we do find somewhat stronger correlations, specially for the intake of medicines for joint pain and other pain. However, most effects reported in Column (1) of Table 2 disappear when observables are included, and they all disappear once individual fixed effects are included. Column (3) in both Tables 1 and 2 provide, for most outcomes, effects that can be classified as very precise zeros given the size of the coefficients and the standard errors.

Columns (4) and (5) show the results of the IV analyses.[19] The regressions in Column (4) use EPL as an instrument, while the regressions in Column (5) use EPL and its interaction with the 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).[20] In addition to the large standard errors, the estimates are often unrealistically large and/or have the reversed sign compared to expectation.[21] These results suggest the possibility that he necessary assumptions for the IV analysis, *i.e.* the relevance and the exogeneity assumptions, do not hold.

Table 3 provides the results of the first stage of the IV analysis. Column (1) shows that the EPL index has a significant effect on job insecurity. The estimate of -0.102 indicates that a one standard deviation increase in the EPL index (*i.e.*, an increase of 0.32) implies an 3.26 percentage point decrease in job insecurity. As indicated by the large F-value (21.09), this estimate is highly significant.[22] When interacted with the sector-specific NRD, EPL still has a

[18]It should be noted that, as provided in Table 1, the percentage in the sample of individuals diagnosed for every particular condition is rather low, except for high blood pressure and high cholesterol.

[19]Note that in all regressions we calculate standard errors clustered at the household level. Since EPL is given at the country level, when estimating our IV specifications, we also consider clustering the standard errors at the country level. Given that the difference in the results is negligible, we restrict ourselves here to reporting standard errors clustered at the household level.

[20]Results do not significantly change when estimated when including fixed effects to the IV analysis. For all FE-IV results, see Table A4 in the appendix.

[21]This could be partially due to the loss of observations in the IV analysis in Column (5). However, if we rerun all other estimations this study using only the sample in Column (5) the change in the results is negligible.

[22]The effects appears to be equally large and significant for both individuals with permanent contracts and

significant effect on job insecurity. However, contrary to what Caroli and Godard (2016) suggest, this effect becomes smaller (in absolute terms) as the NRD increases. Therefore, this result does not support the hypothesis that EPL has a stronger effect in sectors with a high NRD. In addition, the F-value in this case is much lower (3.37) indicating that the instrument in this case is not relevant enough. This indicates that the IV strategy by Caroli and Godard (2016), who interact EPL with the NRD, is not easily generalisable to a larger sample. In addition, the large significance in the first stage when using only EPL combined with the inconclusive results in the second stage casts doubts on the exogeneity of the instrument. The latter is likely to be compromised by unobserved country-specific time trends that may correlate both with EPL and health simultaneously.[23]

4.2 General Health

Table 4 reports the effects of job insecurity on general self-reported health. Column (1) shows that when using pooled OLS we find a very strong correlation between perceived job insecurity and and self-reported health. This result indicates that suffering job insecurity increases the chances of reporting poor health by 5.9%. 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 individual fixed effects to the specification reduces the estimate to virtually zero, *i.e.* in this case the analysis yields again a precisely estimated null effect. Therefore, Columns (1) to (3) in Table 4 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. Different from the results in Tables 1 and 2, the effect of unobserved heterogeneity is likely related to the subjective nature of the dependent variable.

In Columns (4) and (5), the IV estimates appear again to be rather large in size and considerably less precise compared to those in Columns (1) to (3). The lack of precision means that it is not possible to draw a clear conclusion from this part of the analysis for the same reasons as mentioned in Section 4.1.

4.3 Physical Symptoms

Column (1) of Table 5 shows strong and significant correlations between job insecurity and all physical symptoms we consider, except for falling down. In all cases, these correlations are smaller than the one reported for self-reported general health in Column (1) of Table 4. Column

individuals with temporary contracts. We also do not find heterogeneous effects when comparing employees, civil servants, and self-employed individuals. If we use the EPL index for temporary contracts also provided by the OECD, we find that its effect on job insecurity in the first stage is not significantly different from zero.

[23] These trends cannot be controlled for since the inclusion of an interaction between time and country dummies would be perfectly collinear with the EPL index. An estimation of Equation (1) by pooled OLS including interactions between time and country dummies shows that they have a strong and significant effect on health outcomes. This suggest the presence of unobserved variables that are country-specific, change over time, and have an effect on health.

(2) shows that, upon including all control variables in the regressions, all coefficients become smaller and only a few remain significant. Similarly to the analysis for doctor's diagnoses, medicine intake, and self-reported general health, all of the coefficients become even closer to zero and not statistically significant when we include fixed effects in the estimation equations. In that case, only the effect on swollen legs remains significant. However, this estimate is only significant at the 5% level, and it does not reflect a general pattern whereby physical symptoms would be affected by job insecurity. The results of the IV analyses in Table 5 show again larger standard errors than those in Columns (1) to (3) and coefficients that are unrealistically large and have in almost all cases a reversed sign. As explained in section 4.1 it is not possible to draw clear conclusions from this part of the analysis.

4.4 Mental Health

Compared to the results reported in Column (1) of Tables 1, 2, 4, and 5, Column (1) of Table 6 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 4. Once again, 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 6 there are a few outcomes for which the effect remains significant even after adding individual fixed effects 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 in Sections 4.1 to 4.3, 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 outcomes is indicative of the presence of a causal effect. However, note that a negative life event could influence both self-reported job insecurity as well as self-reported depression, which would lead to a spurious correlation. In addition, our objective measure for depression does not corroborate this results. We conclude that, if it exists, the effect of self-reported job insecurity on depression it is in any case rather small. We also test whether the results could be explained by the fact that we are testing a large number of hypotheses. However, they appear to be robust to the application of a Bonferroni correction for multiple hypotheses testing.[24]

[24] Results with the Bonferroni correction are not significantly different compared to the results we present in this paper. Given the similarity between the sets of results, we restrict ourselves here to the reporting of the results without the Boneferroni correction.

Under the assumptions of relevance and exogeneity of the instrument, the IV analysis should be able to validate the result of the fixed effect estimation. However, just like in the results reported in the sections above, the IV analyses yields low-precision estimates that often have the reversed sign compared to expectation. Combined with the results of the first stage of the IV analyses in Table 3, these results cast further doubt on the validity of the relevance and exogeneity of EPL as an instrument for job insecurity.

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 based on thousands of individuals experiencing variation over time, these results suggest that the correlations we find conditional on observables are fully explained by time-invariant unobserved heterogeneity across individuals. If the lack of significance was due to little identifying variation over time, we would expect to find fixed effects estimates with higher standard errors. 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 mechanisms that may explain this finding. In this section we discuss these and, to the extent that the data allow, we test them empirically.[25]

First of all, it may be that the fixed effects 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) refer to 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.[26] These long term effects could be especially relevant for the current study given that we employ a sample of older workers.

Second, it can be that we are underestimating the effects of job insecurity because of selection out of employment for health reasons caused by job insecurity. That will be the case if there are workers experiencing such bad health effects of job insecurity that they drop out of the workforce as a consequence. Since in our sample we only include individuals who are employed, we would underestimate the effect of job insecurity on health in case this selection out of employment is a relevant phenomenon. We test for this by investigating transitions from employment to work disability. To that end, we create a dummy that takes value one for the period an individual is observed in our sample for the last time before transiting into disability benefits, and zero

[25]All results discussed in this section are available upon request.

[26]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.

otherwise.[27] Using the same sample as in the main analysis, we regress that dummy variable on our job insecurity dummy plus all the control variables we consider. We find a precisely estimated zero effect, which indicates that this type of attrition is not a relevant phenomenon in our data.

Third, there are several reasons why the consequences of job insecurity for older workers 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 those 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 are more likely to be liquidity constrained than older individuals, while the latter are more likely to have precautionary savings (*e.g.* Attanasio and Weber, 2010). Therefore, older workers are more likely to be in a better position to smooth consumption in case of job loss and/or a decrease in income occur.

To get an idea of the importance of these two 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 in the top half of the net financial wealth distribution. For virtually all of the outcomes we analyse, the estimated effects obtained via OLS and fixed effects for the upper half and lower half of the distribution are not significantly different from each other. The only exception is self-reported depression. In that case we find a much stronger effect via fixed effects for those at the lower half of the distribution (the estimated effect is 0.049), than for those at the upper half of the distribution, with an estimated effect of 0.005.[28] This is an interesting result since depression is one of the most common health problems in our sample, and it is one of the outcomes for which we still estimate a significant effect of job insecurity after including individual fixed effects. Nevertheless, an effect of 0.049 is still small compared to the baseline incidence of self-reported depression in the sample (0.34) and time-varying endogeneity can still not be ruled out. Negative life events leading to a spurious correlation between self-reported job insecurity and depression may be specially relevant among less wealthy individuals.

Fourth, there may be 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; Brown and Laschever, 2012; and Vermeer *et al.*, 2019) suggesting that older individuals experience important utility gains from retirement due to increased leisure time with peers (friends and/or spouse) who are also

[27] This dummy takes always value zero for individuals who never leave the sample or who do so due to a reason that is not disability or is unknown.

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

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 may be mitigated by utility gains from joint leisure with retired peers.

To get an impression of the relevance of this 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 they can answer yes or no.[29] 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 suggests that individuals' willingness to retire plays no relevant role in the effect of job insecurity on the health of older workers.

5 Conclusions

This study uses data from the Survey on Health, Ageing and Retirement in Europe (SHARE) to analyse the effects of job insecurity on the health of older European workers. Using pooled OLS estimation we find strong correlations between job insecurity and a wide range of health outcomes (*i.e.* conditions diagnosed by a doctor and medicine intake as well as self-reported general health, physical symptoms, and mental conditions). When we include individual fixed effects all of the estimated effects become statistically insignificant, except for a few of the mental conditions, most notably depression. 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 based on variation experienced by thousands of individuals over time and thus precisely estimated.

In addition, we apply and extend an IV method proposed by Caroli and Godard (2016), which consists of using an employment protection legislation (EPL) index and its interaction with the sector-specific natural rate of dismissal (NRD) as instruments for job insecurity. The IV analysis fails to yield the expected results based on Caroli and Godard (2016), since the standard errors are very large and we obtain estimates that are unrealistically large and/or have the opposite sign with respect to expectation. Although the first stage regression shows that EPL has a significant effect on job insecurity with the expected sign, the second stage yields inconclusive results. When considering the interaction between EPL and NRD the results are not significantly different to those obtained when using EPL in isolation. In addition, the first stage regression shows that the interaction has a weak effect on the endogenous variable, *i.e.* job insecurity. Therefore, we conclude that this IV method is not robust when using a larger sample, enlarging the variation in the instrument, including females in the sample, and employing a much wider range of health outcomes.

[29]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. But it could also be that individuals are willing to retire because of health reasons, in which case this interaction variable would be endogenous. For this reason, we also interact job insecurity with age and, alternatively, with a dummy indicating whether individuals are older than 60. The results are not significantly different.

Regardless of the inconclusive IV results, our pooled OLS and fixed effects estimations provide new insights on a very relevant topic for which there is almost no causal evidence at hand. We conclude that for older workers there is a strong correlation between job insecurity and a wide range of health outcomes. In addition, the results show that this correlation is partially explained by observable characteristics (*i.e.* demographic and economic variables), and partially by time-invariant unobserved heterogeneity across individuals. The fixed effects models show precisely estimated zero effects from self-reported job insecurity on health. One of the very few mental conditions for which we still find a significant effect after controlling for observables and individual fixed effects is self-reported depression. Further analysis, shows the effect on depression is specially strong and significant for individuals in lower half of the wealth distribution. However, this result for depression is rather small and is not corroborated by the objective measure of depression. Therefore, we conclude according with the evidence we find that the direct causal effect of job insecurity on health is either zero or rather weak, and that the IV method based on using EPL as an instrument for job insecurity is not robust. This is a very relevant result given the relevance of the topic at hand and the scarcity of causal evidence on it.

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Tables

Table 1: Results - Effect of Job Insecurity on Doctor's Diagnoses

Dep. variable	Avg.	(1) OLS-1	(2) OLS-2	(3) FE	(4) IV-1	(5) IV-2
Heart attack	0.05	0.008***	0.005*	0.000	0.264**	0.395
		(0.003)	(0.003)	(0.004)	(0.135)	(0.656)
High blood pressure	0.25	0.005	-0.001	-0.012*	0.403	0.284
		(0.005)	(0.005)	(0.006)	(0.261)	(0.716)
High cholesterol	0.17	0.008*	0.007*	-0.004	0.982***	1.126*
		(0.004)	(0.004)	(0.006)	(0.328)	(0.629)
Stroke	0.01	0.001	0.000	0.001	-0.035	-0.084
		(0.001)	(0.001)	(0.002)	(0.059)	(0.118)
Diabetes	0.05	0.004	-0.002	-0.005	-0.016	0.139
		(0.003)	(0.003)	(0.003)	(0.141)	(0.230)
Lung disease	0.03	0.005**	0.001	-0.002	-0.002	-0.027
		(0.002)	(0.002)	(0.003)	(0.098)	(0.166)
Arthritis	0.12	0.020***	0.006	0.015*	-0.391**	-0.190
		(0.004)	(0.005)	(0.009)	(0.190)	(0.258)
Cancer	0.02	0.000	0.001	-0.000	0.144*	0.210
		(0.002)	(0.002)	(0.003)	(0.085)	(0.176)
Ulcer	0.03	0.010***	0.004*	-0.003	0.176	-0.241
		(0.002)	(0.002)	(0.003)	(0.124)	(0.252)

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 linear probability models. For further information on the empirical specification and estimation methods, see Section 2. ***Significant at the 10% level, **significant at the 5% level, *significant at the 1% level.

Table 2: Results - Effect of Job Insecurity on Medicine Intake

		(1)	(2)	(3)	(4)	(5)
Dep. variable	Avg.	OLS-1	OLS-2	FE	IV-1	IV-2
Cholesterol	0.12	0.008**	0.008*	-0.002	0.829***	0.902*
		(0.004)	(0.004)	(0.005)	(0.286)	(0.537)
Blood pressure	0.23	0.001	-0.001	-0.008	0.375	0.788
		(0.005)	(0.005)	(0.006)	(0.258)	(0.670)
Coronary disease	0.03	0.005**	0.004*	-0.002	-0.177	0.113
		(0.002)	(0.002)	(0.003)	(0.108)	(0.160)
Other heart disease	0.03	0.006***	0.004**	0.000	0.256**	0.247
		(0.002)	(0.002)	(0.003)	(0.126)	(0.426)
Diabetes	0.05	0.002	-0.003	-0.005*	0.105	0.261
		(0.003)	(0.003)	(0.003)	(0.140)	(0.233)
Joint pain	0.08	0.022***	0.007**	0.005	0.344*	0.384
		(0.003)	(0.003)	(0.005)	(0.189)	(1.210)
Other pain	0.09	0.032***	0.019***	0.008	0.131	-0.622
		(0.004)	(0.004)	(0.006)	(0.196)	(0.631)
Sleep problems	0.04	0.007***	0.002	-0.002	-0.176	-0.116
		(0.002)	(0.002)	(0.003)	(0.123)	(0.182)
Anxiety and/or depression	0.04	0.006**	-0.000	-0.005	0.152	0.285
		(0.002)	(0.002)	(0.003)	(0.135)	(0.800)
Osteoporosis	0.01	0.001	0.001	-0.001	0.062	0.123
		(0.001)	(0.001)	(0.002)	(0.095)	(0.171)
Stomach burns	0.05	0.006***	0.002	-0.002	-0.277*	-0.538*
		(0.002)	(0.003)	(0.004)	(0.158)	(0.289)
Chronic bronchitis	0.01	0.001	-0.000	-0.001	-0.020	-0.155
		(0.001)	(0.001)	(0.002)	(0.062)	(0.153)

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 linear probability models. For further information on the empirical specification and estimation methods, see Section 2. ***Significant at the 10% level, **significant at the 5% level, *significant at the 1% level.

Table 3: Results - First Stage IV Analysis

Instruments	(1)	(2)
EPL	-0.102***	-0.082**
	(0.022)	(0.033)
EPL×NRD		0.010**
		(0.005)
F-value	21.09	5.12
Observations	48,716	34,049

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 linear probability models. 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 10% level, **significant at the 5% level, *significant at the 1% level.

Table 4: Results - Effect of Job Insecurity on Self-Reported Health

		(1)	(2)	(3)	(4)	(5)
Outcome	Avg.	OLS-1	OLS-2	FE	IV-1	IV-2
Self-reported health	0.18	0.059***	0.022***	0.006	0.113	0.190
		(0.005)	(0.005)	(0.007)	(0.215)	(0.356)
Observations		48,716	48,716	48,716	48,716	34,049

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 linear probability models. For further information on the empirical specification and estimation methods, see Section 2. ***Significant at the 10% level, **significant at the 5% level, *significant at the 1% level.

Table 5: Results - Effect of Job Insecurity on Physical Symptoms

		(1)	(2)	(3)	(4)	(5)
Dep. variable	Avg.	OLS-1	OLS-2	FE	IV-1	IV-2
Pain back and/or joints	0.45	0.044***	0.010	0.003	0.086	0.025
		(0.007)	(0.007)	(0.014)	(0.252)	(0.704)
Heart trouble	0.03	0.013***	0.008***	0.003	-0.050	-0.169
		(0.003)	(0.003)	(0.005)	(0.078)	(0.124)
Breathlessness	0.06	0.020***	0.010***	0.007	-0.110	-0.153
		(0.003)	(0.004)	(0.007)	(0.125)	(0.186)
Persistent cough	0.04	0.009***	0.004	-0.000	-0.075	-0.252
		(0.003)	(0.003)	(0.007)	(0.096)	(0.176)
Swollen legs	0.07	0.018***	0.010***	0.013**	-0.192	-0.292
		(0.004)	(0.004)	(0.006)	(0.134)	(0.225)
Sleeping problems	0.16	0.025***	0.009*	0.007	0.045	-0.110
		(0.005)	(0.005)	(0.010)	(0.194)	(0.307)
Falling down	0.01	0.001	-0.000	-0.002	-0.098	-0.071
		(0.001)	(0.001)	(0.003)	(0.061)	(0.094)
Dizziness and/or faints	0.04	0.015***	0.007**	0.002	0.045	0.077
		(0.003)	(0.003)	(0.006)	(0.103)	(0.152)
Stomach problems	0.10	0.019***	0.012***	0.006	-0.191	-0.378
		(0.004)	(0.004)	(0.009)	(0.157)	(0.257)

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 linear probability models. Average marginal effects are provided. For further information on the empirical specification and estimation methods, see Section 2. ***Significant at the 10% level, **significant at the 5% level, *significant at the 1% level.

Table 6: Results - Effect of Job Insecurity on Mental Health

Dep. variable	Avg.	(1) OLS-1	(2) OLS-2	(3) FE	(4) IV-1	(5) IV-2
Depression	0.34	0.063***	0.033***	0.023***	-0.880***	-0.947*
		(0.006)	(0.006)	(0.009)	(0.336)	(0.561)
Pessimism	0.10	0.032***	0.016***	0.010*	0.189	-0.271
		(0.004)	(0.004)	(0.006)	(0.198)	(0.300)
Suicidality	0.04	0.017***	0.006***	-0.000	-0.094	-0.113
		(0.002)	(0.002)	(0.004)	(0.107)	(0.177)
Guilt	0.08	0.022***	0.012***	0.008	-0.496***	-0.398
		(0.003)	(0.003)	(0.006)	(0.190)	(0.341)
Lack of interest	0.05	0.024***	0.013***	0.018***	-0.143	-0.231
		(0.003)	(0.003)	(0.005)	(0.146)	(0.219)
Irritability	0.27	0.056***	0.025***	0.012	-0.539*	-0.215
		(0.005)	(0.005)	(0.009)	(0.287)	(0.737)
Lack of appetite	0.05	0.013***	0.005*	-0.001	-0.069	-0.196
		(0.002)	(0.003)	(0.005)	(0.139)	(0.212)
Fatigue	0.27	0.064***	0.029***	0.014	-0.479*	-0.380
		(0.005)	(0.005)	(0.009)	(0.270)	(0.458)
Concentration problems	0.12	0.033***	0.015***	0.014**	0.009	0.212
		(0.004)	(0.004)	(0.006)	(0.197)	(0.309)
Lack of enjoyment	0.08	0.022***	0.009***	-0.003	0.196	0.113
		(0.003)	(0.003)	(0.006)	(0.170)	(0.365)
Tearfulness	0.21	0.020***	0.007	0.003	-0.205	-0.835
		(0.005)	(0.005)	(0.008)	(0.237)	(0.889)

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 linear probability models. For further information on the empirical specification and estimation methods, see Section 2. ***Significant at the 10% 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 pooled OLS 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	Wave 6	Wave 7
Austria	2.56	2.56	2.56	2.56	2.56	2.56
Belgium	2.60	2.60	2.73	2.60	2.87	2.87
Czech Republic	-	3.09	3.02	2.93	2.93	2.93
Denmark	2.08	1.87	1.92	1.92	1.92	1.92
Estonia	-	-	2.11	2.11	2.11	-
France	2.83	2.83	2.68	2.68	2.68	2.68
Germany	2.89	2.89	2.89	2.89	2.89	2.89
Greece	3.06	3.06	-	-	2.57	2.57
Hungary	-	-	2.40	-	-	-
Ireland	-	1.79	-	-	-	-
Israel	-	2.23	-	2.23	2.23	
Italy	3.33	3.33	3.33	3.17	3.17	2.62
Luxembourg	-	-	-	-	2.63	-
Netherlands	3.22	3.22	3.17	3.22	3.22	3.37
Poland	-	2.48	2.48	-	2.48	2.48
Portugal	-	-	3.49	-	2.78	-
Slovenia	-	-	2.90	2.90	2.52	-
Spain	2.65	2.65	2.55	2.26	2.26	2.26
Sweden	2.60	2.60	2.60	2.60	2.60	2.60
Switzerland	2.06	2.06	2.06	2.06	2.06	2.06
Average	2.75	2.64	2.58	2.45	2.51	2.42

Notes: For more information on the EPL index provided by the OECD, see main text, and OECD (2020). This table contains EPL values only for each wave that a particular country is included in the sample.

Table A3: Sector-Specific Dismissal Rates (USA Average 2006-2016)

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 2006 and 2016 of the Health and Retirement Study. For more information about the computation, see main text.

Table A4: Results - FEIV

Dep. variable	Avg.	(1) FEIV-1	(2) FEIV-2
Doctor's diagnoses			
Heart attack	0.05	0.709	-0.645
		(0.725)	(0.991)
High blood pressure	0.25	1.588	0.219
		(1.544)	(1.029)
High cholesterol	0.17	2.561	0.597
		(2.325)	(1.251)
Stroke	0.01	-0.121	0.489
		(0.234)	(0.699)
Diabetes	0.05	0.332	0.230
		(0.502)	(0.623)
Lung disease	0.03	0.466	0.117
		(0.508)	(0.465)
Arthritis	0.12	0.298	0.343
		(0.371)	(0.556)
Cancer	0.02	0.563	-0.437
		(0.561)	(0.742)
Ulcer	0.03	1.140	0.708
		(1.037)	(1.109)
Medicine intake			
Cholesterol	0.12	2.101	0.693
		(1.908)	(1.219)
Blood pressure	0.23	1.485	-0.545
		(1.455)	(1.114)
Coronary disease	0.03	-0.283	-0.500
		(0.426)	(0.789)

Table A4: Results - FEIV (Continuation)

		(1)	(2)
Dep. variable	Avg.	FEIV-1	FEIV-2
Other heart disease	0.03	0.673	-0.197
		(0.699)	(0.611)
Diabetes	0.05	0.790	0.454
		(0.806)	(0.785)
Joint pain	0.08	0.845	1.577
		(0.932)	(2.301)
Other pain	0.09	0.172	-1.212
		(0.710)	(1.803)
Sleep problems	0.04	-0.302	-1.393
		(0.440)	(1.886)
Anxiety and/or depression	0.04	0.136	-0.256
		(0.452)	(0.688)
Osteoporosis	0.01	0.165	-0.361
		(0.382)	(0.722)
Stomach burns	0.05	-0.206	-0.297
		(0.482)	(0.763)
Chronic bronchitis	0.01	-0.054	-0.418
		(0.208)	(0.616)
Self-reported general health			
Self-reported health	0.18	0.039	-2.588
		(0.727)	(3.632)
Physical symptoms			
Pain back and/or joints	0.45	-0.234	0.844
		(0.541)	(0.931)
Heart trouble	0.03	-0.066	-0.057
		(0.173)	(0.275)

Table A4: Results - FEIV (Continuation)

		(1)	(2)
Dep. variable	Avg.	FEIV-1	FEIV-2
Breathlessness	0.06	0.127	0.245
		(0.269)	(0.381)
Persistent cough	0.04	-0.176	-0.456
		(0.231)	(0.391)
Swollen legs	0.07	-0.375	-0.155
		(0.312)	(0.378)
Sleeping problems	0.16	0.338	0.728
		(0.427)	(0.751)
Falling down	0.01	0.017	-0.162
		(0.148)	(0.240)
Dizziness and/or faints	0.04	0.135	0.183
		(0.241)	(0.376)
Stomach problems	0.10	-0.330	-0.753
		(0.349)	(0.642)
Mental health			
Depression	0.34	1.079	0.111
		(1.325)	(1.332)
Pessimism	0.10	0.804	-1.528
		(0.987)	(2.188)
Suicidality	0.04	-0.246	-0.037
		(0.418)	(0.541)
Guilt	0.08	-0.374	-0.871
		(0.644)	(1.466)
Lack of interest	0.05	-0.581	0.421
		(0.749)	(1.041)
Irritability	0.27	0.646	1.297
		(1.098)	(2.258)

Table A4: Results - FEIV (Continuation)

		(1)	(2)
Dep. variable	Avg.	FEIV-1	FEIV-2
Lack of appetite	0.05	0.045	-1.388
		(0.543)	(1.977)
Fatigue	0.27	-0.598	-0.935
		(1.048)	(1.864)
Concentration problems	0.12	0.517	1.513
		(0.839)	(2.260)
Lack of enjoyment	0.08	0.347	-0.261
		(0.723)	(0.969)
Tearfulness	0.21	1.367	-0.207
		(1.463)	(1.267)

Notes: Standard errors (clustered at the household level) are reported in parenthesis. All regressions include year and country dummies. All regressions include the control variables mentioned in Section 3.3. All regressions are estimated using linear probability models. For further information on the empirical specification and estimation methods, see Section 2. ***Significant at the 10% level, **significant at the 5% level, *significant at the 1% level.