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# ABSTRACT

# Healthcare Utilization at Retirement: The Role of the Opportunity Cost of Time[1]

We investigate the causal impact of retirement on healthcare utilization using SHARE data for 10 European countries. We show that the number of doctor's visits and the probability of visiting a doctor more than four times a year (our measures of healthcare utilization) increase after retirement. The increase in healthcare utilization is found to depend mainly on the years spent in retirement, suggesting that adjustment may take time. We find evidence of heterogeneous effects by gender and across different patterns of time use prior to retirement (i.e., working long hours, and combined work and out-of-work activities). Overall, the empirical findings suggest that the increase in healthcare utilization is consistent with the decrease in the opportunity cost of time faced by individuals when they retire.

**JEL Classification:** J26, I10, C26

**Keywords:** retirement, health, healthcare utilization

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# 1 Introduction

Retirement is a milestone event in the life cycle of individuals, often associated with significant changes in lifestyles and behaviors of those involved. The effects of retirement on a number of health and economic outcomes have been extensively studied in the literature and, despite the fact that the age of retirement is a fairly predictable event, many studies have reported unexpected jumps around the age of retirement in the health status of individuals, as well as in consumption (and savings) patterns. The fact that such jumps are hard to reconcile with the standard life cycle theory–under the assumption that agents are forward looking– combined with the mixed evidence available from empirical studies, have fostered a debate as to whether health investments and consumption (savings) should vary smoothly over the life cycle or, in contrast, experience discontinuities at the time of retirement.

The standard conceptual framework for analyzing health investment is the Grossman's human-capital model (Grossman, 1972*a*,*b*, 2000). In this model, individuals invest in health for both "consumption" (health provides utility) and "production" motives (healthy individuals achieve higher earnings). Within this framework, the stock of health is assumed to depreciate with age (increasingly in old age) but individuals can increase their health stock by investing in health inputs (e.g., medical care, healthy lifestyles). Healthcare utilization is expected to increase smoothly with the aging process to preserve the health stock, until it becomes too costly doing so: death occurs when the health stock falls below a given threshold. Retirement occurs gradually as individuals choose their optimal allocation of leisure and consumption over the life cycle, and both the health stock and investments in health care are always set at their desired level. In this context, consistent with life cycle theory, no sudden changes should be expected around retirement age.

The idea that health should always be at the optimal level over the life cycle has been challenged by Galama et al. (2013) who introduce retirement, as a permanent transition from employment to non-employment, in the standard Grossman model. They show that the optimal level of consumption, health investment, and health stock can be discontinuous at the age of retirement and investigate different scenarios under internal or corner solutions. Under one scenario, individuals reach retirement age with their health stock at the optimal level (internal solution), such that no adjustment in health investment should

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be expected. However, given that in retirement more leisure is available and health has no effect on income, a reallocation between work-related consumption and leisure-related consumption may occur. Under an alternative scenario, individuals can deviate from their optimal health stock while in employment (corner solution) and adjust consumption and health investment after retirement to reach the desired level of health stock. Individuals with health endowments below (above) the optimal health stock will increase (decrease) their health investment after retirement. Since investment in health for preserving the efficiency of the health stock for productive reasons drops to zero as individuals approach retirement,[2] most of the adjustment in healthcare utilization is likely to be driven by the change in the opportunity cost of time after retirement. The combination of both reallocation and opportunity cost of time factors may result in either an increase or a decrease in healthcare investments. Finally, notice that while theory predicts that the expected adjustment in health investment should be large and instantaneous, individuals may take longer to adjust their health stock after retirement. The existence of a change in health investment (i.e., healthcare utilization) after retirement and the direction of the adjustment is ultimately an empirical matter, which is what we investigate in this paper.

Early studies on consumption and saving patterns have shown that consumption (savings) significantly decreases (increases) after retirement. This is known as the "retirement-consumption puzzle" since such changes are hard to reconcile with the consumption-smoothing pattern predicted by the standard life cycle theory when agents are forward looking (Banks et al., 1998; Battistin et al., 2009).

Some studies have found evidence consistent with the drop in consumption expenditures after retirement, being driven by reallocation effects across categories of goods. In particular, consumption is shifted from work-related goods (e.g., clothes, transportation) that are typically bought on the market, toward (time-intensive) leisure-related consumption goods (e.g., recreation, sports, shopping aimed at finding good deals, cooking) that are home produced (Aguiar and Hurst, 2005; Aguila et al., 2011; Hurst, 2008; Miniaci et al., 2010). This change in the composition of consumption after retirement is likely to be optimal, as discussed above, since retirees have more leisure time compared to employed individuals.

This paper investigates whether healthcare utilization, which we measure by the number of doctor's visits and the probability of visiting a doctor more than four times a year, changes after retirement.

Despite the growing relevance of medical care use for both the understanding of individuals' behavior, and for the design of suitable fiscal policies aimed at the aged 50+ population, only a limited number of studies have addressed the issue of healthcare utiliza-

[2] Notice that, while it is often argued that a negative effect should be expected on healthcare utilization when individuals retire–due to the lack of labor market incentives to invest in health-preserving activities–when the retirement decision is explicitly modeled this effect is likely to be negligible, as individuals optimally reduce healthcare utilization for "production" motives as they reach retirement age.

tion after retirement. Some of the early studies focus on the determinants of healthcare utilization across European countries without specifically addressing the effect of retirement (Jiménez-Martín et al., 2004). The paper by Boaz and Muller (1989) explicitly addresses the change in time endowments between employed and retired individuals to explain differences in healthcare utilization, focusing attention on private insurance coverage for ambulatory doctor's visits without modeling retirement behavior. They conclude that retired individuals are more likely to use physician services when compared to the self-employed (but not to employees). Using US and German data respectively, Gorry et al. (2015) and Eibich (2015), model retirement decisions jointly with healthcare utilization but hardly find any significant impact of retirement. In a cross-country analysis for 10 European countries, Celidoni and Rebba (2017) investigate the causal effect of retiring from work on individuals' lifestyles and on doctor's visits but do not find any statistically significant effect. In contrast Coe and Zamarro (2015), using retirement eligibility ages to identify the effect of transitions from employment to retirement, unemployment, or inactivity on the number of doctor's visits, report a negative effect both in the US and in continental Europe. Overall empirical evidence shows mixed findings and results appear to be sensitive to the methodology and the countries considered. In particular, most studies in the literature have modeled the retirement decision as a shift variable which can only account for a short-term effect, while adjustment in healthcare utilization may take time, which explains why most studies fail to find an effect.[3]

Our study contributes to the existing literature in a number of ways. First, we address the endogeneity of retirement decisions providing robust causal evidence on healthcare utilization. We focus on a sample of EU countries and on retirement rather than any other form of exits from employment. Since retirement decisions are likely to depend on individual (observable and unobservable) characteristics, and on time-varying shocks (i.e., affecting the decision to retire early), we use a fixed-effect IV model. In particular, we exploit the panel dimension of the first two waves of the Survey of Health, Ageing and Retirement in Europe (SHARE) which provides information on labor market status, healthcare utilization, and a wide range of socioeconomic and demographic characteristics (Bolin et al., 2009; Dunlop et al., 2000). Our identification strategy exploits the different benefits available at retirement eligibility ages (i.e., early and ordinary retirement ages) to instrument individuals' retirement decisions (Bonsang et al., 2012; Coe and Zamarro, 2011, 2015).

Second, we measure healthcare utilization both in terms of the number of doctor's

[3]Notice that the two papers in the literature closer to our own focus exclusively on the short-run effects of retirement on doctor's visits and report mixed findings: Celidoni and Rebba (2017) find a non-statistically significant effect, while Coe and Zamarro (2015) report a negative effect on visits to a general practitioner. Our approach encompasses both short- and long-run effects, we show that the impact of retirement on healthcare utilization needs time, increasing with the number of years individuals spent in retirement.

visits, as typically done in the literature, as well as in terms of the probability of visiting a doctor more than four times a year to capture a more intensive healthcare utilization. Third, we specify the effect of retirement on healthcare utilization including a retirement dummy, as well as the number of years spent in retirement to account for the possibility that adjusting healthcare utilization may take time to unfold (Mazzonna and Peracchi, 2017). Fourth, we investigate the heterogeneous effects of retirement by gender and across different patterns of time use (i.e., individuals working long hours and those combining work and out-of-work activities).

Our results suggest that healthcare utilization increases after retirement. While we find no robust evidence of short-run adjustment in healthcare utilization, the number of doctor's visits and the probability of visiting a doctor more than four times a year both increase with the years spent in retirement. We find evidence of heterogeneous effects. The impact of years spent in retirement on doctor's visits is shown to be larger for males, as it is the probability of visiting a doctor more than four times upon retirement. Individuals characterized by an intensive use of time prior to retirement show a larger effect on healthcare utilization compared to other individuals, though the difference is not always statistically significant. The above results are shown to be robust to a number of specification changes and estimation methods.

Overall, the above empirical findings suggest that at least part of the increase in healthcare utilization following retirement can be explained by a reallocation effect toward leisure-related goods, and by the decrease in the opportunity cost of time.

The paper is structured as follows. Section 2 presents our empirical strategy. The data are described in Section 3. Section 4 presents our results, and concluding remarks are provided in Section 5.

# 2 Empirical strategy

In order to assess the impact of retirement on healthcare utilization, we specify the following model:

$$V_{it} = \gamma Retired_{it} + \delta Distance_{it} + X'_{it}\beta + \alpha_i + u_{it} \qquad (1)$$

where $V_{it}$ is our indicator of healthcare utilization which we proxy by the number of visits paid to a doctor by individual $i$ in the 12 months preceding time $t$, and by a dummy variable measuring more than four visits a year. $Retired_{it}$ is a dummy variable equal to 1 if individual $i$ is retired at time $t$, while $Distance_{it}$ measures the number of years spent in retirement at time $t$. $X'_{it}$ is a vector of demographics (including individuals' age), household and job characteristics and $\alpha_i$ is an individual time invariant fixed effect (accounting, among others, for education and country fixed effects).[4] Notice that with

[4]We do not include the year of the interview to the baseline specification since this would induce poor identification of the *age* and *distance* variables.

this specification the overall effect of retirement is given by the sum of $\gamma$ and $\delta Distance_{it}$ which is varying with the years spent in retirement.

The number of visits an individual pays to the doctor, as previously discussed, is likely to depend on several unobservable characteristics (e.g., individual time preference, latent health status), that may affect retirement decisions ($Retired_{it}$) and the number of years spent in retirement ($Distance_{it}$). As long as individual unobservable heterogeneity is time-invariant, the inclusion of individual fixed effects in the regression, $\alpha_i$, delivers consistent estimates. This condition, however, is unlikely to hold in general. Time-varying omitted characteristics, such as own or partner's negative health shocks, are likely to affect both the number of doctor's visits as well as retirement behavior. The latter may even generate some reverse causality if the doctor, after a check-up and on the basis of poor health, were to recommend that the individual should retire early.

To tackle these problems, we estimated a Fixed-Effects IV model. Our identification strategy exploits the fact that as individuals reach the retirement eligibility age, the financial incentive to retire strongly increases. This allows us to estimate the causal impact of retirement on healthcare utilization.

In most European countries, retirement occurs either at the Ordinary Retirement Age ($ORA$)–that is the age at which workers are eligible for full old-age pensions–or at an Early Retirement Age ($ERA$), which represents the earliest age at which retirement benefits can be claimed conditional on a given number of years of social security contributions. Figures A1 and A2 in the Appendix report the shares of retirees and newly retired workers (females and males respectively), along with the Early and Ordinary Retirement Age thresholds, by country.[5] The age retirement profiles appear quite differently across gender and countries. In many countries the early retirement age threshold is lower for females, compared to males, which means that on average–conditional on social security contributions–females are eligible for retirement at an earlier age.

In general, the trend in the share of newly retired workers reveals an increase in the probability of retirement as individuals approach the Early Retirement Age threshold (particularly in Austria, Belgium, and Italy), while trends tend to diverge from that point until the Ordinary Retirement Age: the share of newly retired workers further increases in some countries (i.e., Germany, the Netherlands, Switzerland, and Denmark) while it declines in others (i.e., Austria and Italy, particularly among males). In other words, depending on the interaction between the retirement rules in each country and the structure of financial incentives, the Early and Ordinary Retirement Age thresholds account for most of the changes in individuals' probability of retirement.

Using the above eligibility rules (i.e. $ERA$ and $ORA$), we constructed four instru-

[5]The vertical lines represent the ranges of eligibility ages respectively for $ERA$ and $ORA$ that are relevant for individuals in our sample. A more detailed description of pension eligibility rules in the countries considered is reported in the Appendix.

mental variables: two dummy variables, $ERAD_{ict}$ and $ORAD_{ict}$, that take value 1 if the individual is above the gender-specific early age, or ordinary retirement eligibility age in country $c$ and year $t$; and two continuous variables, $DistERA_{ict}$ and $DistORA_{ict}$, that measure the (positive) distance of the respondent's age (measured at the time of the interview) from the gender-specific Early, or Ordinary retirement eligibility age (which is zero if the individual is still employed).

$$\begin{aligned}
ERAD_{ict} &= 1 \qquad if \qquad age_{ict} > ERA_{c,t} \\
ORAD_{ict} &= 1 \qquad if \qquad age_{ict} > ORA_{c,t} \\
DistERA_{ict} &= (Age_{ict} - ERA_{c,t}) \qquad if \qquad DistERA_{ict} > 0; \quad 0 \quad \text{otherwise} \\
DistORA_{ict} &= (Age_{ict} - ORA_{c,t}) \qquad if \qquad DistORA_{ict} > 0; \quad 0 \quad \text{otherwise}
\end{aligned}$$

Note that identification here does not rely on the change in early or ordinary retirement ages across cohorts, but on the increase in the individual probability of retiring as individuals become eligible for pension benefits in their country of residence. In other words, eligibility rules generate an exogenous shock to retirement decisions which is what we use to instrument individual retirement status and the number of years in retirement. The identification of the short-term effect on healthcare utilization (i.e., the *Retired* variable) relies on individuals who retire between waves and face a different retirement probability at the given eligibility ages (i.e., the estimated coefficient is likely to identify a local average treatment effect). The identification of the long-term effect of retirement (i.e., the *Distance* variable) relies on information on both the date of the interview and the actual year of retirement also for individuals who were already retired at the start of the period.

Since pension reforms may change Early or Ordinary Retirement rules, the retirement eligibility ages for the cohorts in our sample vary over time, by country and gender.[6]

One problem with our data is the lack of information on the actual number of years of social security contributions. Hence, in countries where the ERA threshold is particularly low (Italy for example), workers with incomplete career spells (e.g., due to nonparticipation or long unemployment spells) are unlikely to meet the conditionality of a minimum number of years of contributions. This reduces the power of the ERA threshold to predict the probability of being retired, as well as the distance from retirement. In such context, the ORA threshold is likely to do a better job in predicting retirement decisions. For the above reasons, our preferred specification for the first stage includes instruments that exploit both the ERA and the ORA thresholds. Finally, given that we have two endogenous variables and four instruments, the over-identifying restrictions can be tested along with the presence of weak instruments(Kleibergen and Paap, 2006).[7]

[6] See the Appendix for additional details on the country-specific thresholds.

[7] Weak instruments can be tested using critical values reported by Stock and Yogo (2005): with two endogenous variables and four instruments, the critical value for a maximum relative bias of 5% relative

# 3 Data and descriptive statistics

## 3.1 Data and sample selection

We used data from SHARE, a multidisciplinary and cross-national survey collecting information on individuals aged 50 or older. Information on health, socioeconomic status, household income, and wealth is available for 27 European countries and Israel. Six waves of data, from 2004 to 2015, are currently available (one of which is a retrospective survey). In this paper we use data from the first two waves (2004 and 2006) for 10 countries,[8] considering a balanced sample of individuals aged 50 to 69 at the time they entered the survey, who are either employed or retired in each wave.[9] We further restrict our sample by excluding individuals permanently living in nursing homes. After dropping all individuals with missing values on the variables of interest, our final sample consists of 5,880 individuals (11,760 observations).

## 3.2 Variables

*Healthcare utilization*

Healthcare utilization is our dependent variable; information is drawn from a question asking the respondent how many times they have seen a doctor over the past 12 months.[10] A breakdown of the total number of doctor's visits between general practitioner and specialist visits is also available. We also constructed a measure of "intensive" healthcare utilization, defined as a binary indicator taking value of 1 when the number of doctor's visits is greater than 4 visits in the last 12 months (i.e., in our sample the average number of doctor's visits is 3.97. Employees report a lower number of visits 3.19 compared to retired individuals 4.76. See Table 1.).[11]

*Retirement*

to OLS is 11.04, while for size distortion greater than 10% is 16.87.

[8] We include Austria, Belgium, Germany, Sweden, Spain, Italy, France, Denmark, Switzerland and the Netherlands. We exclude Greece for data quality reasons (Börsch-Supan and Jürges, 2005).

[9] Hence, our sample contains individuals who were employed both in 2004 and 2006 (46%), retired in both waves (45%) and those who moved from employment to retirement between waves (9%).

[10] The exact wording is: "About how many times in total have you seen or talked to a medical doctor about your health (last 12 months)?" Dentist visits and hospital stays are excluded, but emergency room or outpatient clinic visits are included. We consider as outliers, and therefore exclude, individuals reporting a number of visits higher than three times the standard deviation.

[11] The choice of the more-than-4 visits threshold for our "intensive" healthcare utilization is consistent with the empirical evidence in the existing literature, as well as the distribution of doctor's visits in our sample. In Table 1, we also report median values that are less sensitive to extreme values (3 visits for the whole sample and 4 visits for retired individuals). The extensive literature on healthcare utilization (but only few papers focus on the effects of retirement) reports similar figures. For example, among others, Celidoni and Rebba (2017) report an average of 3.61 for the whole sample (2.5 for employees and 4.5 for retired); Dunlop et al. (2000) report higher figures for Canada (4.94 males, 7.10 females, 6.11 whole sample) but the overall number of visits also included telephone consultations. Finally, Bago d'Uva and Jones (2009) estimate for the group of "high users" an average total number of visits equal to 4.05.

The definition of retirement we adopt in our analysis follows that commonly used in the literature (Mazzonna and Peracchi, 2012; Rohwedder and Willis, 2010), where an individual is considered to be retired when leaving the labor force permanently (i.e., retirement is an absorbing state). In practice, retirement is coded using respondents' self-reported employment status in each wave. Note that SHARE also provides information about the exact year and month the individual left the labor market, which we used to construct our measure of distance to retirement. Our definition of retirement concerns individuals who report to have retired from work, while we exclude individuals who have left employment for unemployment or inactivity.[12]

*Control variables*

In the empirical analysis we include a number of controls capturing individual, household, and job characteristics. Demographic variables include age, education, a binary indicator for living with a spouse or partner, household size, and a dummy variable for having children. Job characteristics are captured by industry (1-digit NACE classification) and occupational dummies (1-digit ISCO-88 classification). We also include household income and ability to make ends meet.[13]

Given the potentially confounding effect of retirement on health, we also included in one robustness check a set of controls for individuals' health status: self-assessed health, diagnosed conditions, and an indicator of mental health (Bolin et al., 2009; Redondo-Sendino et al., 2006; Solé-Auró et al., 2012). Self-assessed health is a binary indicator taking value 1 if the individual reports to be in "poor" or "fair health" (out of a 5-point Likert-type scale ranging from "poor" to "excellent"). A more objective measure of health is defined as the sum of all medical conditions that have ever been diagnosed by a doctor (heart attack, high blood pressure or hypertension, high cholesterol, stroke, diabetes, chronic lung disease, arthritis, cancer, stomach or duodenal ulcer, Parkinson disease, cataracts, and hip or femoral fractures). Finally, mental health is measured on the EURO-D depression scale (ranging from 0 to 12, where higher values mean more depressed).

*Other variables of interest*

[12] Coe and Zamarro (2015) instead consider as retired all individuals who report to be either retired or homemakers, sick and disabled, separated from the labor force, or unemployed. They argue that individuals often report to have retired when they have left their "career job" even if they are still working part-time or full-time. In our sample this does not seem to be a problem since self-assessed retirement is strongly correlated with the retirement status retrieved from the exact year and month of retirement–the correlation is 0.95–as well as consistent with Ordinary and Early Retirement eligibility Ages in the country of residence. Moreover, notice that given our identification strategy, unemployed or inactive individuals are likely to be noncompliers, so we do not consider them in our sample.

[13] Household income is recoded into deciles and includes annual income from employment, self-employment, or pension, as well as regular payments received by any member of the household. We also use a subjective measure of household financial distress drawn from a question that asks individuals: "Thinking of your household's total monthly income, would you say that your household is able to make ends meet [with great difficulty ... easily]", and recode it into a dummy variable taking value 1 if they make ends meet with difficulty or great difficulty, and 0 otherwise.

To explore the heterogeneous effects of retirement on healthcare utilization across different patterns of time use, we identified two groups of individuals: those who, prior to retirement, had a highly intensive use of time (i.e., long hours worked and out-of-work activities) and those with a less intensive time use. We used two different indicators. First, we defined a long-hours-worked dummy equal to 1 for individuals whose weekly working hours, in the last job prior to retirement, were above the upper quintile (or quartile) of the distribution of hours in the country of residence. Note that since information on weekly working hours in the last job is available in SHARE only for individuals who were employed at the time of the first interview, for individuals who were already retired in wave 1 we have to rely on an external source (i.e., the European Working Conditions Survey).[14]

Second, we used factor analysis to compute a composite indicator of time use based on information available in the European Working Conditions Survey, which combines both long working time schedules (i.e., hours worked in main and secondary job, work on a Saturday or Sunday) and out-of-work activities (i.e., involvement in voluntary activities, caring duties, and housework activities).[15] We then defined an intensive time-use binary indicator which takes value 1 for individuals above the upper quintile (or quartile) of the distribution of the composite time-use indicator in the country of residence.

## 3.3 Descriptive statistics and evidence

Table 1 reports summary statistics for respondents' demographic characteristics, health status, and frequency of doctor's visits, for the whole sample and separately for employed and retired individuals (pooling the two waves).

The upper panel of Table 1 shows there are no relevant differences between employees

[14]We drew the information on hours worked from the 2000 and 2005 waves of EWCS, computing the average number of weekly hours worked at the 2-digit ISCO88 classification of occupations by country and gender for individuals aged 50 to 69. Average hours worked were then matched to the cohorts of individuals who retired between 2000 and 2005, and before 2000. For Switzerland, which is not sampled in EWCS, we used comparable information drawn from the Eurostat Labour Force Survey (by 1-digit ISCO88 and gender). We checked the consistency of our indicator of long hours worked comparing the distribution of individuals in the upper quintile (or quartile) across countries in SHARE and EWCS data, and found very similar shares.

[15]In practice, we extracted the first score out of a factor analysis on the following two sets of variables: (i) "work-time schedule"–total number of hours worked in main and secondary job, work on Saturday or Sunday (at least twice a month), work more than 10 hours a day (at least twice a month) and lack of work–family balance; (ii) "out-of-work activities"–involvement (at least once or twice a week) in voluntary or charitable activity, political/trade union activity, caring for children or elderly/disabled relatives, and housework activities. The two indicators were standardized into an overall index, then matched with SHARE data at the 2-digit ISCO88 classification of occupations by country and gender. We then used information available within SHARE on a limited number of out-of-work activities (voluntary work, participation in cultural/political/religious activities) for employed individuals aged 50 to 69, to check consistency with the indicator computed from EWCS data. While the two indices do not exactly measure the same thing, the shares of individuals in the upper quintile (or quartile) are very similar across countries.

and retirees in terms of household composition (size, presence of a child and a partner), while retirees are on average older (65 versus 56 years old) and have a lower level of education. The lower panel also shows significant differences in terms of health status and healthcare utilization across employees and retirees. The share of individuals reporting to be in poor health is 28% among retirees, as compared to only 14% among employees. Using a more objective measure of health status, retirees are found to be characterized by more than one diagnosed chronic condition (1.33), compared to 0.80 for the sample of employees.

Retirees exhibit larger healthcare utilization, with an average of 4.8 visits over the past 12 months and a median of 4 (compared to 3.2 and 2, respectively, for employed individuals). Over 41% of retirees visited a doctor at least four times in the past year (compared to only 24% of employed individuals).[16] Indeed, the fact that retirees have a more intensive use of doctor services than employed individuals is observed at all ages.

As shown on the left-hand panel of Figure 1, the number of visits does not vary much with age, but it is lower for employees compared to retirees (particularly from age 56 onward). [17] A similar pattern is observed for the probability of visiting a doctor more than four times a year (see right-hand panel of Figure 1).

*Figure 1* Age profile of doctor's visits

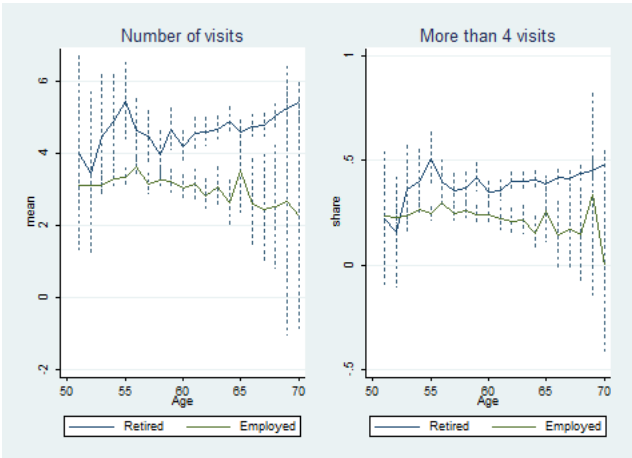
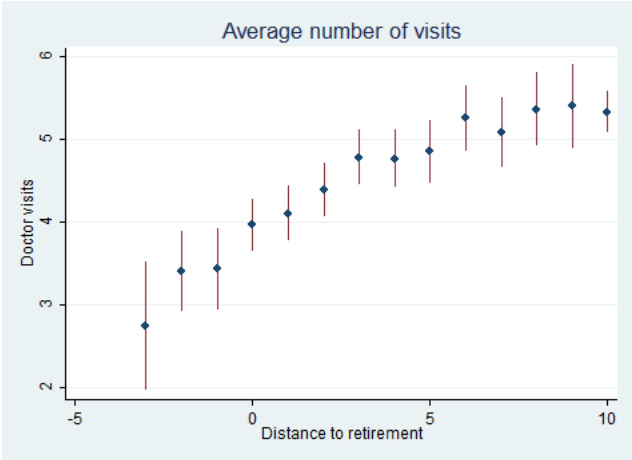



Figure 2 shows that the number of doctor's visits increases with the number of years spent in retirement. Healthcare utilization appears to be stable around 3.5 visits before retirement and it increases after retirement reaching an average of 5.3 after six years spent in retirement, then it flattens out.

[16] A description of other control variables with their means is reported in Table A1 in the Appendix.

[17] The drop for employees aged 66 and over is likely to reflect the fact that only individuals with better than average health are still in employment at older ages.

*Figure 2* Doctor's visits and distance to retirement




# 4 Results

## 4.1 OLS and Fixed Effects Estimates

To provide a benchmark we first estimated the impact of retirement on healthcare utilization by pooled OLS and with individual FE; we then implemented our FE-IV model as previously described. In the baseline model we regressed the number of doctor's visits and the probability of visiting a doctor more than four times a year on the retirement status dummy (*Retired*) and the number of years since retirement (*Distance*). The main results are reported in Table 2. In the OLS we include demographic controls, industry, and occupational dummies, income decile dummies as well as wave and country fixed effects (columns 1 and 2). We then add individual fixed effects with only time-varying controls (columns 3 and 4).

The number of doctor's visits and the probability of visiting a doctor more than four times a year both increase with age and are found to be higher for females compared to males. The effect of age on the number of visits and on the probability of visiting a doctor more than four times a year becomes insignificant when individual fixed effects are included, suggesting that unobserved factors correlated with age explain why people visit the doctor more often. The association between retirement and the number of years since retirement shows a positive and statistically significant coefficient on both the number of doctor's visits and the probability of visiting a doctor more than four times a year. This suggests that retirees–compared to employed individuals–go to the doctor more often and that the number of visits tends to increase with the number of years spent in retirement. The retirement gap in the number of visits is around 0.6 per year, which represents a 15% increase computed at the sample average, while any additional year spent in retirement increases the number of visits by 1%. Retired individuals also show a higher intensity of healthcare utilization, as the probability of seeing the doctor more than four times a year is higher (approximately 6%) and increasing with the number of years since retirement.

The estimated retirement gap on the number of doctor's visits is somewhat smaller when time-invariant unobserved characteristics are controlled for (8% increase), while the probability of more than four visits is unaffected. Interestingly, with fixed effects any additional year spent in retirement increases the number of visits by almost 5%, and the probability of more than four visits by 2.3%. The change in the estimated effects of retirement when individual's unobserved characteristics are accounted for suggests that simple OLS are likely to be biased due to the correlation of unobserved characteristics with an individual's retirement behavior and healthcare utilization.

## 4.2 First-stage Estimates

Table 3 reports the estimates from the first-stage regressions. The dependent variables are the two endogenous retirement variables–*Retired* and *Distance* as specified in Equation (1)–while the covariates include the four instruments *ERAD*, *ORAD*, *DistERA* and *DistORA*, as previously defined, and the other control variables.[18] We estimated the probability of retiring using a linear fixed-effect probability model, and the number of years spent in retirement by means of a fixed-effect within estimator.[19] The top panel in Table 3 shows the results for the Retired dummy variable and the bottom panel reports estimates for the Distance variable. In the first column we report results for the whole sample, while the last two columns show the results by gender.

The instruments show a sizable and statistically significant effect on retirement decisions. One notable difference is that retirement eligibility ages, *ERAD* and *ORAD*, have a statistically significant effect on the retirement probability, while in general they are not statistically significant on the number of years spent in retirement.[20] The F-test for the joint significance of the instruments shows that the retirement eligibility ages are significant predictors of both the probability of retiring and the number of years spent in retirement.[21] First-stage results by gender show very similar results for males and females.

[18] We also experimented with a specification for the first stage including one set of instruments at a time–*ERAD* and *DistERA*, alternatively *ORAD* and *DistORA*. While results are qualitatively the same, our preferred specification, shown in Table 3, includes all four instruments.

[19] Since our dependent variables are discrete, in the robustness section we also implement alternative estimation methods based on count data and limited dependent variable models.

[20] In the specification with the *Retired* dummy as the dependent variable the variable *DistORA* bears a puzzling negative sign, which might depend on the truncation of the age distribution (see alsoMazzonna and Peracchi (2017) on this point).

[21] The Kleibergen-Paap Wald F and LM statistics underidentification and weak instruments are reported at the bottom of the table.

## 4.3 Second-stage estimates

In Table 4, we report the estimates of the retirement variables using a two-stage least square within estimator (FE-IV) for the whole sample (column 1) and separately by gender (columns 2 and 3). The upper panel of Table 4 shows estimates for the number of doctor's visits, while the bottom panel reports estimates for the probability of visiting the doctor more than four times a year.[22] Results for the whole sample suggest that retirement has a positive effect on the number of doctor's visits and on the probability of a more intensive healthcare utilization. While the short-run effect of moving from being employed to retirement has no statistically significant effect, the number of years spent in retirement significantly increases the number of doctor's visits and the probability of more intensive healthcare utilization. In terms of doctor's visits, one additional year in retirement increases the number of times an individual sees a doctor after retiring by 6.5%, and raises the probability of more than four visits per year by 2%. When estimated on separate samples by gender, we find for males a statistically significant sizable effect of the number of years spent in retirement on the number of doctor's visits, which in magnitude is double compared to the estimated effect for females. Always for males, the probability of more intensive healthcare utilization shows both short- and a long-term effects which, computed at five years since retirement (i.e., $\hat{\gamma}+\hat{\delta}(5)$), imply an overall increase of about 30% in the probability of visiting a doctor more than four times a year, suggesting a larger adjustment in the intensive margin of healthcare utilization (i.e., for those who visit a doctor more frequently). The comparable effect for females is approximately 10%. The smaller effect we detect for females after retirement is consistent with the evidence that females, and older females in particular, are more likely to see a doctor regularly even before retirement for both prevention (mammography, Pap test, etc.) and monitoring programs (menopause, osteoporosis, etc.). Taken at face value, these results confirm that the effect of retirement on healthcare utilization mostly unfolds in the long term, with a cumulative effect that increases with the number of years spent in retirement. This is consistent with the predictions of the Grossman's model with retirement (Galama et al., 2013), suggesting that individuals after retirement adjust their healthcare utilization to reach the desired level of health stock.[23] Given the large increase in leisure time that characterizes retirement, we interpret the adjustment as being partly driven by the decrease in the opportunity cost of time and by the reallocation taking place between work-related goods and goods that are complements to leisure.

We explored whether the patterns of adjustment in healthcare utilization after retirement are heterogeneous across individuals who are likely to experience a larger (smaller)

[22] Each table reports the Hansen J-statistic for the validity of the overidentifying restrictions.

[23] Also, the empirical evidence on the effects of retirement on health suggests that the impact is mostly driven by the number of years in retirement, with short-run effects condensed on specific occupations (Mazzonna and Peracchi, 2017).

change in the opportunity cost of time after retirement; we estimated the effects of our retirement variables *Retired* and *Distance*), separately for individuals characterized by a different distribution of work–leisure time prior to retirement. In particular, we focused on individuals retiring from jobs characterized by long working hours and those combining work and out-of-work activities, who supposedly had a lower leisure time endowment and were consuming less of time-intensive goods prior to retirement. We defined a long-hours-worked dummy, $HIGH$, taking value 1 when the number of hours worked by the individual in the last job before retirement was above a threshold set at the first quintile (or quartile) of the distribution of weekly hours worked in a given country.[24] A dummy, $LOW$, was also defined taking value 1 for individuals with weekly working hours below the threshold. In a similar fashion, we constructed two dummies, $HIGH$ and $LOW$, defined on the overall distribution of hours for individuals combining both long hours worked and out-of-work activities (see Section 3.2 for further details on the construction of the time-use indicators).

In Table 5, we interact our retirement variables with the dummies defined as above, and estimate our FE-IV model with four endogenous variables ($RetiredHIGH$, $RetiredLOW$, $DistanceHIGH$ and $DistanceLOW$) and eight instruments (i.e., also obtained interacting our instruments with the $HIGH$ and $LOW$ dummies, respectively). Notice that since we focus on the upper part (quintile or quartile) of the work–leisure distribution, the estimation of the model by split sample is not feasible; moreover, the interacted specification has the advantage that we can directly test the difference in the estimated coefficients above and below the thresholds.

Results confirm our previous findings on the absence of any short-run effect of retirement on healthcare utilization both for the $HIGH$ and $LOW$ groups. The long-run effects, estimated on the number of years spent in retirement, show a positive and statistically significant impact on all measures of healthcare utilization. This has an estimated coefficient that is always larger in magnitude for individuals who, prior to retiring, were working long hours or had combined work and out-of-work activities (i.e., the $HIGH$ group). However, due to the large standard errors on the estimates for the $HIGH$ group, in some cases we cannot reject the hypothesis that the two coefficients are statistically identical.

While some caution is certainly necessary in interpreting the above results, the overall pattern seems consistent with the hypothesis that individuals facing a larger change in the opportunity cost of time after retirement (i.e., retiring from jobs with long hours worked or combined work and out-of-work activities) are more likely to adjust their healthcare utilization increasing the number of visits paid to a doctor. In Table 6, we estimate our model separately for the number of visits paid to a general practitioner and to a specialist.

[24] We define a country-specific threshold to account for the fact that the legislation on working hours differs across countries.

Results confirm that the number of years in retirement increase the number of visits paid to a general practitioner as well as to a specialist, while the magnitude of the effect is found to be statistically similar.

## 4.4 Robustness checks

In order to check the robustness of our main findings, in this section we perform a number of sensitivity analyses, testing our baseline model against alternative specifications and samples. In our baseline model, we specified a linear-in-age healthcare utilization, which in practice may be overly restrictive. To check the sensitivity of the linearity in age, we re-estimated the model in Table 4 (column 1) with a more flexible specification including a set of age dummies (ages 50–55, 56–60, 61–65, and 66+). Our results are essentially unchanged (see Appendix, Table A2, line 1), with an increase in the number of doctor's visits and in the probability of intensive healthcare utilization associated with additional years spent in retirement.

One problem in estimating the impact of retirement on the pattern of healthcare utilization comes from the fact that retirement is also correlated with individuals' health stock. The potential changes in health status upon retirement have been much studied in the literature but the evidence remains somewhat mixed, and it is not clear whether one should expect an increase or a decrease in health status at the time of retirement (Bassanini and Caroli, 2015). While, for the reasons discussed above, we acknowledge the fact that health status in a healthcare utilization model is likely to be a bad control, to check the sensitivity of our results to changes in health status around retirement we include a number of health variables.[25] Results from this exercise are very similar to our baseline estimates (see Appendix, Table A2, line 2), suggesting that the effect of retirement on healthcare utilization is not significantly mediated by changes in the health status of individuals.

Given the cross-country dimension of our data, one additional concern could be that our results are driven by the effect of retirement on healthcare utilization in a specific country. When we re-estimated our baseline specification excluding one country at a time, we obtained results that are not qualitatively different from those reported in Table 4 (see Appendix, Table A2, line 3). We also tested the sensitivity of our estimates to unobservable time-varying, country-specific characteristics. Including a more flexible specification, with country-specific time and age trends, in our baseline model we find very similar results (see Appendix, Table A2, lines 4 and 5).

[25] While time-invariant differences in preventive care behavior across individuals are accounted for, by the fixed effects, changes occurring at the time of retirement could be an additional confounding effect. Given the long-term effect we detected on healthcare utilization, we also inspected the robustness of our results to prior health conditions. Including both current and lagged health status in our baseline specification and using an IV strategy to estimate the effect of retirement, we obtained the same results while prior health controls were never statistically significant (results are available upon request).

In some countries, senior citizens (over 65–67) are often exempted from copayments for doctor's visits. If changes in healthcare coverage happen to take place exactly at retirement age this could represent a source of concern for our identification strategy. A closer inspection of the legislation on doctor's visits copayment exemption for senior citizens has revealed that four countries (Denmark, Belgium, Italy, and Spain) have indeed age-specific exemption rules combined with some income thresholds (for example in Italy and Belgium). We expect the potential bias to be negligible as the age-specific exemption rules apply when most people in our sample have already retired. Moreover, excluding any one of the above countries showed no significant changes in the estimated effect of retirement. As a final check, we exploited information on changes in health insurance occurring between waves 1 and 2,[26] and estimated the probability of experiencing such changes when individuals reach the eligibility ages (also conditioning on individual characteristics). Results (available upon request) on the above set of countries suggest no correlation between retirement eligibility ages and changes in health insurance coverage. Finally, we checked the heterogeneous effect of retirement on healthcare utilization between the cluster of the four countries and other countries. The interaction of both our retirement variables with a dummy identifying the four countries cluster is not statistically significant, suggesting that age-specific exemption rules are not driving our results.[27]

Since attrition has been acknowledged as a serious problem in SHARE data (Börsch-Supan and Jürges, 2005), we investigated whether non-random attrition may be a source of bias. Notice that our FE strategy can already control for panel attrition that originates from time-invariant characteristics (Wooldridge, 2010). As a further robustness check we used an inverse probability weighting approach (Fitzgerald et al., 1998; Mazzonna and Peracchi, 2017) to test for attrition bias.[28] Results from this exercise show that, after accounting for individuals' health status, healthcare utilization does not affect the probability of participating in the second wave. Moreover, the estimated effect of retirement in the weighted and unweighted models is virtually the same (see Appendix, Table A3).

As a final check, we took into consideration the fact that our dependent variables are either count or binary, and we implemented pooled and conditional fixed-effects models[29]

[26] The question we use is "We are interested in how your health insurance may have changed since our last interview. Taking all your social and health insurances into account, has anything changed, for better or for worse, in your coverage for health problems?".

[27] Results of this exercise are available upon request.

[28] We used demographic and health characteristics as well as doctor's visits observed in wave 1 to predict the probability of participating in the second wave. We then computed inverse-probability weights that were used to re-estimate our baseline model.

[29] Note that fixed-effects models for count data in short panels are generally not estimable due to the incidental parameters problem (Cameron and Trivedi, 2013), except for the Poisson model. A fixed-effects estimator has also been proposed for the negative binomial model, but it imposes very strong restrictions on the relationship between fixed effects and the overdispersion parameter, so that the coefficients of time-invariant regressors are identified in the model (Allison and Waterman, 2002). Moreover, to the best of our knowledge, there is no feasible way to model endogeneity within a fixed-effects non-linear

(Bago d'Uva and Jones, 2009). One viable solution with count data is to estimate pooled and fixed-effects Poisson models and, to account for overdispersion (doctor's visits have mean 3.97 and variance 14.74), a pooled Negative Binomial. We also report estimates from the random effects negative binomial model with all the time-varying covariates expressed as deviations from the individual-specific means, as suggested by Allison (2005) (see Appendix, Table A4).[30] For the binary indicator of more than four visits we estimated a conditional FE logit model. The results we obtained from the above exercises are very similar to those presented in Table 2 for OLS and fixed-effect linear models, both in sign and magnitude.

# 5 Conclusions

In this paper, we show evidence of a causal impact of retirement on healthcare utilization. Both the number of doctor's visits and probability of visiting a doctor more than four times increase after retirement. While the short-run effect of moving from employment to retirement is not statistically significant, the number of years spent in retirement significantly increases the number of doctor's visits, suggesting that the adjustment mostly unfolds in the long term. The magnitude of the estimated effect indicates that one additional year in retirement increases visits to a doctor by 6.5% and raises the probability of more than four visits per year by 2%.

The effect of retirement on healthcare utilization is shown to be heterogeneous by gender. Males exhibit a statistically significant effect of retirement on healthcare utilization, which in magnitude is more than double compared to the estimated effect for females. The smaller effect for females is consistent with the evidence that (older) females are more likely to see a doctor regularly even before retirement for both prevention and monitoring programs.

We argue that the increase in the number of doctor's visits is consistent with the predictions of the theory, as individuals adjust their healthcare utilization after retirement to reach the desired level of health stock. The estimated effect of retirement is also consistent with the expected decrease in the opportunity cost of time that individuals experience after retirement, increasing the consumption of more time-intensive activities (such as healthcare utilization).

Consistent with this hypothesis, we find some evidence that individuals facing a larger change in the opportunity cost of time after retirement (i.e., retiring from jobs with long

panel framework.

[30]In Table A4 we report estimated coefficients of the pooled estimators with cluster-robust standard errors (columns 1 and 2), the conditional fixed-effect Poisson, and the random-effects negative binomial (columns 3 and 4). To account for excess zeros we also estimated a pooled zero inflated model (where inflation is calculated on the whole set of regressors in $X$), and obtained very similar results. Fixed-effects extensions of this model are not available.

hours worked or combined work and out-of-work activities) are more likely to increase the number of visits paid to a doctor.

Our identification strategy, however, does not allow us to disentangle the effect of the different mechanisms; that is, whether the increase in optimal amount of healthcare utilization, after retirement, is due to the implicit decrease in the cost of time inputs or alternatively, that individuals are rationed in terms of leisure time allocation and unable to reach their optimal choice of healthcare utilization. Consistent with our results, these effects are expected to be stronger for individuals working long hours or combining work and out-of-work activities before retiring.

The implications of the above mechanisms for health policy differ somehow. If the increase in the number of visits after retirement is the result of individuals' long-run response to a change in the opportunity cost of time, there seems to be little scope for policy intervention. Conversely, if the increase in healthcare utilization after retirement reflects a trend toward the optimal level of health care for previously time-rationed individuals, concerns for public health may arise. For example, it may imply that senior workers do not get enough health care toward the end of their career, which may further generate health problems particularly in a context in which the official retirement age has been raised. Of course, at this stage, whether the level of healthcare utilization of older workers is optimal or not remains an open issue.

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# 6 Tables

*Table 1* Descriptive statistics

| ***Variables*** | ***Whole sample*** | ***Employed*** | ***Retired*** |
|---|---|---|---|
| *Demographics* | | | |
| Age | 60.57 | 56.46 | 64.71 |
| Females | 0.46 | 0.47 | 0.45 |
| Males | 0.54 | 0.53 | 0.55 |
| Primary/Lower-secondary education | 0.37 | 0.29 | 0.45 |
| Secondary and upper-secondary education | 0.35 | 0.36 | 0.34 |
| Tertiary education | 0.28 | 0.35 | 0.21 |
| Living with a spouse or partner | 0.78 | 0.79 | 0.78 |
| Household size | 2.21 | 2.4 | 2.03 |
| Having at least 1 child | 0.91 | 0.90 | 0.91 |
| *Health status* | | | |
| Poor self-rated health | 0.21 | 0.14 | 0.28 |
| Diagnosed conditions (total) | 1.07 | 0.80 | 1.33 |
| Depression index (1-12) | 1.79 | 1.69 | 1.88 |
| Mean doctor's visits (median) | 3.97 (3) | 3.19(2) | 4.76(4) |
| More than 4 visits | 0.32 | 0.24 | 0.41 |
| *N.* | 11,760 | 5,905 | 5,855 |

Note: Figures report averages. Medians are shown in parentheses.

*Table 2* Retirement and doctor's visits - Pooled OLS and FE model

| | ***OLS*** | | ***FE*** | |
|---|---|---|---|---|
| | N. visits | More than 4 visits | N. visits | More than 4 visits |
| retired | 0.570*** | 0.0588*** | 0.320* | 0.0572** |
| | (0.125) | (0.0152) | (0.192) | (0.0264) |
| distance | 0.0435*** | 0.00475*** | 0.204*** | 0.0226*** |
| | (0.0130) | (0.00149) | (0.0485) | (0.00636) |
| age | 0.0508*** | 0.00581*** | 0.0591** | 0.00595 |
| | (0.0113) | (0.00136) | (0.0289) | (0.00408) |
| female | 0.618*** | 0.0718*** | | |
| | (0.0916) | (0.0109) | | |
| *Demographics* | ✓ | ✓ | ✓ | ✓ |
| *Industry and Occupation* | ✓ | ✓ | ✓ | ✓ |
| *Income* | ✓ | ✓ | ✓ | ✓ |
| *Year and Country dummies* | ✓ | ✓ | | |
| *Individual fixed-effects* | | | ✓ | ✓ |
| $R^2$ | 0.129 | 0.0979 | 0.0211 | 0.0144 |
| N | 11,760 | 11,760 | 11,760 | 11,760 |

Robust standard errors in parentheses, clustered at the individual level. Significance: * p<.1, ** p<.05, *** p<.01.

*Table 3* First-stage results

| | **Whole sample** | **Males** | **Females** |
|---|---|---|---|
| **Retired** | | | |
| ERAD | 0.115*** | 0.153*** | 0.0608** |
| | (0.0227) | (0.0291) | (0.0235) |
| ORAD | 0.138*** | 0.146*** | 0.119*** |
| | (0.0293) | (0.0347) | (0.0418) |
| distERA | 0.0448*** | 0.0354*** | 0.0613*** |
| | (0.00746) | (0.0114) | (0.00876) |
| distORA | -0.0479*** | -0.0427*** | -0.0591*** |
| | (0.00753) | (0.0114) | (0.00937) |
| N | 11,760 | 6,312 | 5,448 |
| $R^2$ | 0.172 | 0.183 | 0.173 |
| F-test | 28.29 | 19.12 | 19.03 |
| **Distance** | | | |
| ERAD | 0.111 | 0.283** | -0.112 |
| | (0.104) | (0.141) | (0.0784) |
| ORAD | 0.120 | 0.192 | 0.00181 |
| | (0.0992) | (0.120) | (0.0921) |
| distERA | 0.451*** | 0.458*** | 0.465*** |
| | (0.0434) | (0.0540) | (0.0463) |
| distORA | 0.384*** | 0.340*** | 0.413*** |
| | (0.0403) | (0.0484) | (0.0467) |
| N | 11,760 | 6312 | 5448 |
| $R^2$ | 0.800 | 0.788 | 0.828 |
| F-test | 781.18 | 364.09 | 1221.52 |
| Under ID Kleibergen-Paap LM | 55.81*** | 42.20*** | 39.15*** |
| Weak ID Kleibergen-Paap F | 28.25 | 19.02 | 18.53 |

Robust standard errors in parentheses, clustered at the country and cohort level. Significance: * p<.1, ** p<.05, *** p<.01. Results are obtained using the full set of controls.

*Table 4* Retirement and doctor's visits - FE-IV

| | **Whole sample** | **Males** | **Females** |
|---|---|---|---|
| **Panel A - *Doctor's visits*** | | | |
| retired | 0.241 | 0.847 | -0.433 |
| | (0.693) | (0.879) | (1.013) |
| distance | 0.239*** | 0.344*** | 0.157* |
| | (0.0656) | (0.0880) | (0.0894) |
| age | 0.0452 | 0.00865 | 0.0639 |
| | (0.0394) | (0.0535) | (0.0540) |
| Hansen-J stat | 0.456 | 0.820 | 1.681 |
| **Panel B - *More than 4 visits*** | | | |
| retired | 0.0888 | 0.172* | 0.0560 |
| | (0.0794) | (0.0986) | (0.133) |
| distance | 0.0220*** | 0.0259** | 0.0203* |
| | (0.00770) | (0.0105) | (0.0121) |
| age | 0.00507 | 0.00251 | 0.00390 |
| | (0.00498) | (0.00693) | (0.00783) |
| Hansen-J stat | 1.780 | 2.748 | 1.703 |
| N | 11,760 | 6,312 | 5,448 |

Robust standard errors in parentheses, clustered at the country and cohort level. Significance: * p<.1, ** p<.05, *** p<.01. Results are obtained using the full set of controls.

*Table 5* Heterogeneity

| | **Working Hours** | | **Time use** | |
|---|---|---|---|---|
| | $5^{th}$ *quintile* | $4^{th}$ *quartile* | $5^{th}$ *quintile* | $4^{th}$ *quartile* |
| **Panel A - *Doctor's visits*** | | | | |
| retired*HIGH | 0.395 | -0.00873 | -0.816 | -0.510 |
| | (1.142) | (0.986) | (1.022) | (0.907) |
| retired*LOW | 0.429 | 0.400 | 0.222 | 0.0847 |
| | (0.800) | (0.838) | (0.758) | (0.754) |
| distance*HIGH | 0.419*** | 0.344*** | 0.507*** | 0.452*** |
| | (0.162) | (0.117) | (0.145) | (0.133) |
| distance*LOW | 0.204*** | 0.210*** | 0.217*** | 0.206*** |
| | (0.0681) | (0.0719) | (0.0691) | (0.0693) |
| Hansen-J stat | 8.178* | 7.606 | 3.234 | 1.673 |
| **Panel B - *More than 4 visits*** | | | | |
| retired*HIGH | 0.0801 | 0.0176 | -0.0859 | -0.0202 |
| | (0.155) | (0.145) | (0.138) | (0.113) |
| retired*LOW | 0.105 | 0.108 | 0.108 | 0.0803 |
| | (0.0905) | (0.0956) | (0.0867) | (0.0887) |
| distance*HIGH | 0.0391** | 0.0250 | 0.0564*** | 0.0419** |
| | (0.0195) | (0.0154) | (0.0217) | (0.0199) |
| distance*LOW | 0.0186** | 0.0212*** | 0.0162** | 0.0164** |
| | (0.00793) | (0.00823) | (0.00788) | (0.00776) |
| Hansen-J stat | 7.769 | 5.557 | 5.802 | 3.276 |
| N | 11,760 | 11,760 | 11,260 | 11,260 |

Robust standard errors in parentheses, clustered at the country and cohort level. Significance: * p<.1, ** p<.05, *** p<.01. Results are obtained using the full set of controls.

Weak ID Kleibergen-Paap rk Wald F not reported as Stock-Yogo critical values are not defined with 4 endogenous regressors and 8 instruments. However, Kleibergen-Paap rk LM test for underidentification supports the relevance of the instruments, as well as F-test on excluded instruments on single endogenous regressors.

*Table 6* Doctor's visits: General Practitioner and Specialist

| | **GP** | **Specialist** |
|---|---|---|
| retired | 0.0338 | 0.207 |
| | (0.518) | (0.421) |
| distance | 0.139*** | 0.0994** |
| | (0.0499) | (0.0414) |
| age | 0.00512 | 0.0401 |
| | (0.0333) | (0.0246) |
| | | |
| Hansen-J stat | 1.355 | 0.147 |
| N | 11,760 | 11,760 |

Robust standard errors in parentheses, clustered at the country and cohort level. Significance: * p<.1, ** p<.05, *** p<.01. Results are obtained using the full set of controls.
First stages as in baseline specification, column 1 of Table 3.

# 7 Appendix

### Eligibility Ages

Sources of information on retirement eligibility ages are Gruber and Wise (2010); Hamblin (2013); OECD (2005-2013); Staubli and Zweimüller (2013).

**Austria**

ERA: 55 for women and 60 for men until 2004. The 2000 and 2003 reforms introduced a gradual increase in ERA for both men and women born after 1940 and 1945, respectively.

ORA: 60 for women; 65 for men.

**Belgium**

ERA: 55 for women; 60 for men.

ORA: 65 for men; 60 for women until 1997, raised to 61 in 1997, 62 in 2000 and 63 in 2003.

**Denmark**

ERA: 60 for both men and women

ORA: 67 until 2003 for both men and women. 65 from 2004.

**France**

ERA: 55 for both men and women from 1981 onward.

ORA: 65 for both men and women until 1983. 60 from 1983 onward.

**Germany**

ERA: 60 until 2003; after the 2001 reform, 63 for men and 62 women (63 from 2006).

ORA: 65 for both men and women.

**Italy**

ERA: until 1995 possible at any age with 35 years of contributions for both men and women (set 55 assuming labor market entry at 20); increased to 57 until 2004.

ORA: 55 for women and 65 for men until 1993; 60 for women and 65 for men with step-wise increase from 1994.

**The Netherlands**

ERA: 60 for both men and women.

ORA: 65 for both men and women.

**Spain**

ERA: 60 for both men and women until 1993; raised to 61 from 1994 to 2001; 2002 reform significantly increased reductions for workers voluntarily leaving the labor market before age 65, setting ERA from 61+30 years of contributions.

ORA: 65 both for men and women.

**Sweden**

ERA: 1994 pension reform introduced a notional defined contribution scheme with no fixed-retirement age. Pensions could be claimed from 60 for both men and women; raised to 61 from 1998 onward.

ORA: 67 for both men and women until 1994. 65 from 1995 onward.

**Switzerland**

ERA: until 2000 for women and 1997 for men no early retirement possible. For men retiring between 1997 and 2000 ERA was 64; since 2001 ERA is set two years before ORA, i.e. 63 for men and 62 for women.

ORA: 65 for men; 62 for women until 2000, 63 starting 2001.

*Figure A1* Share of retirees and newly retired across countries - Females

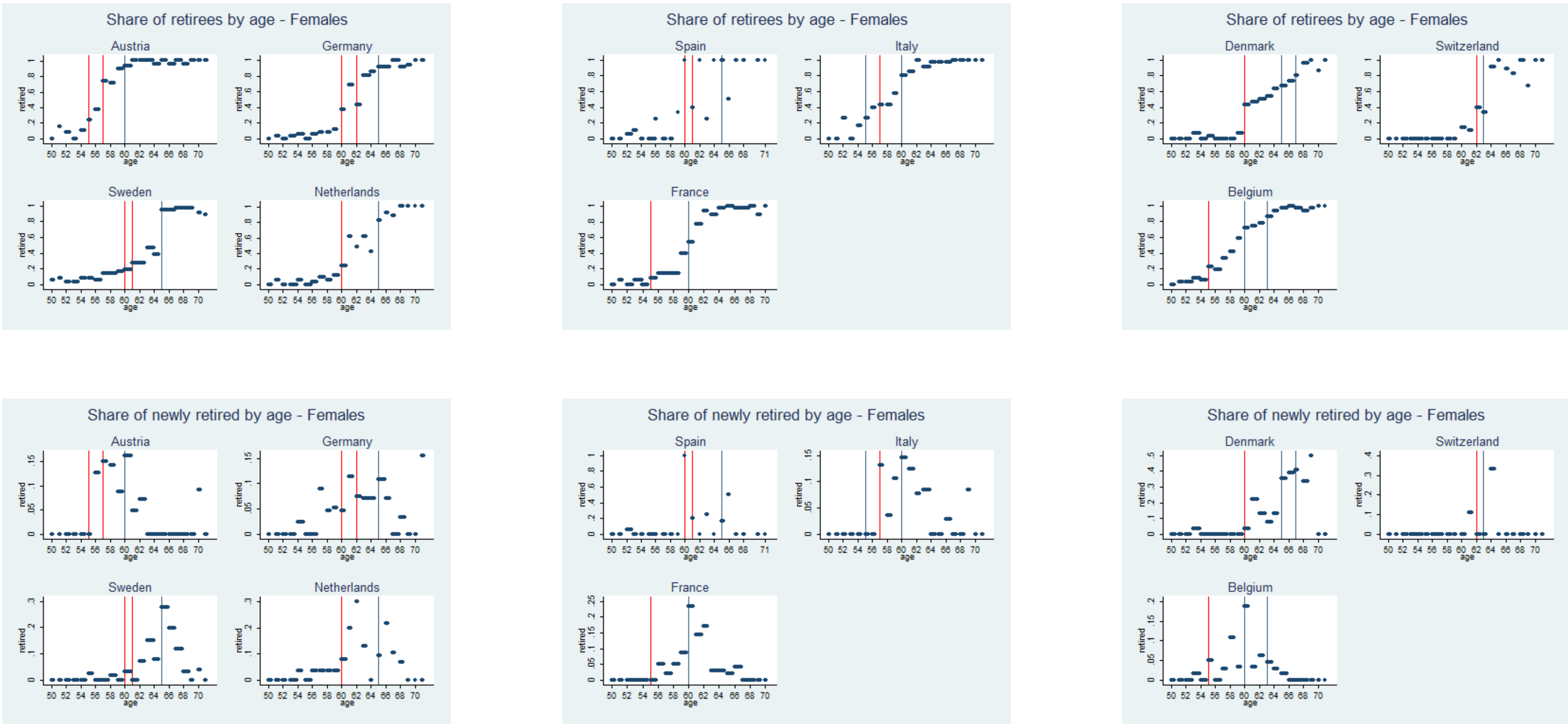

*Figure A2* Share of retirees and newly retired across countries - Males

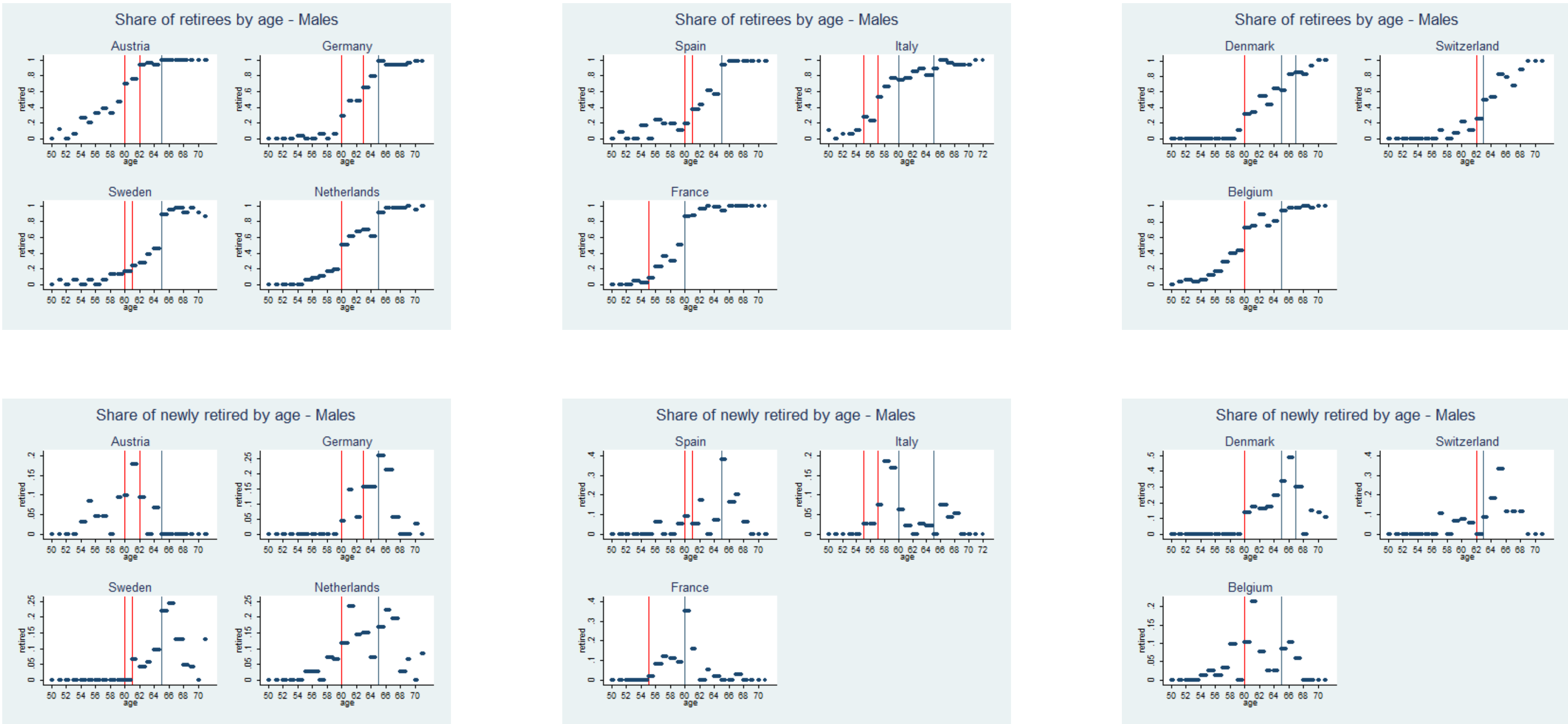

*Table A1* Description and means of control variables

| ***Variable*** ***Description*** | ***Mean*** | | |
|---|---|---|---|
| | *Whole Sample* | *Employed* | *Retired* |
| *Occupations* 1-digit ISCO-88 | | | |
| ISCO 1 - Managers | 0.125 | 0.129 | 0.121 |
| ISCO 2 and 3 - Professionals and Technicians | 0.357 | 0.409 | 0.305 |
| ISCO 4 - Clerks | 0.124 | 0.126 | 0.122 |
| ISCO 5 - Service workers | 0.111 | 0.122 | 0.101 |
| ISCO 6 and 7 - Crafts & Trade workers | 0.123 | 0.09 | 0.158 |
| ISCO 8 - Plant & Machine operators | 0.069 | 0.05 | 0.087 |
| ISCO 9 - Elementary occupations | 0.09 | 0.074 | 0.106 |
| *Industries* 1-digit NACE | | | |
| Agriculture and mining | 0.026 | 0.017 | 0.036 |
| Manufacturing | 0.222 | 0.17 | 0.274 |
| Construction | 0.069 | 0.065 | 0.073 |
| Wholesale, Hotels and Transports | 0.169 | 0.153 | 0.186 |
| Financial and Real Estate services | 0.101 | 0.106 | 0.097 |
| Public Administration | 0.094 | 0.107 | 0.081 |
| Education, Health and other Services | 0.318 | 0.383 | 0.253 |
| *Working hours* | | | |
| $5^{th}$*quintile* dummy = 1 if respondent's weekly working hours are in the upper quintile of distribution by country | 0.185 | 0.232 | 0.136 |
| $4^{th}$*quartile* dummy = 1 if respondent's weekly working hours are in the upper quartile of distribution by country | 0.232 | 0.269 | 0.194 |
| *Time use* | | | |
| $5^{th}$*quintile* dummy = 1 if respondent's time use index is in the upper quintile of distribution by country | 0.186 | 0.219 | 0.155 |
| $4^{th}$*quartile* dummy = 1 if respondent's time use index is in the upper quartile of distribution by country | 0.245 | 0.274 | 0.218 |
| *Income* | | | |
| difficulty dummy = 1 if Household makes ends meet with difficulty or great difficulty | 0.235 | 0.188 | 0.281 |
| Household's income deciles | | | |
| 1 | 0.051 | 0.038 | 0.063 |
| 2 | 0.055 | 0.028 | 0.083 |
| 3 | 0.073 | 0.049 | 0.097 |
| 4 | 0.089 | 0.064 | 0.114 |
| 5 | 0.097 | 0.072 | 0.123 |
| 6 | 0.11 | 0.094 | 0.125 |
| 7 | 0.122 | 0.124 | 0.119 |
| 8 | 0.134 | 0.163 | 0.104 |
| 9 | 0.138 | 0.181 | 0.095 |
| 10 | 0.133 | 0.187 | 0.078 |
| *Countries* | | | |
| Austria | 914 | 258 | 656 |
| Germany | 1,274 | 607 | 667 |
| Sweden | 1,948 | 1,146 | 802 |
| Netherlands | 1,054 | 630 | 424 |
| Spain | 424 | 261 | 163 |
| Italy | 1,300 | 418 | 882 |
| France | 1,404 | 663 | 741 |
| Denmark | 1,070 | 667 | 403 |
| Switzerland | 500 | 371 | 129 |
| Belgium | 1,872 | 884 | 988 |
| N | 11,760 | 5,905 | 5,855 |

*Table A2* Robustness analysis - FE-IV

| | **Retired** | **Distance** | Obs. |
|---|---|---|---|
| **Panel A - *Doctor's visits*** | | | |
| 1. Non-linearity in age | 0.499 | 0.275*** | 11,760 |
| | (0.744) | (0.0618) | |
| 2. Health controls | 0.440 | 0.201*** | 11,760 |
| | (0.637) | (0.0599) | |
| 3. Drop countries:range [min;max][a] | [(-0.0630)-0.616] | [0.202***-0.273***] | [9,812-11,336] |
| 4. Country-specific time trends | 0.153 | 0.199*** | 11,760 |
| | (0.652) | (0.0672) | |
| 5.Country-specific age trends | 0.183 | 0.209*** | 11,760 |
| | (0.647) | (0.0651) | |
| **Panel B - *More than 4 visits*** | | | |
| 1. Non-linearity in age | 0.164** | 0.0248*** | 11,760 |
| | (0.0826) | (0.0070) | |
| 2. Health controls | 0.0993 | 0.0187** | 11,760 |
| | (0.0756) | (0.00739) | |
| 3. Drop countries:range [min;max][a] | [0.047 - 0.177**] | [0.0191*** -0.0257***] | [9,812-11,336] |
| 4. Country-specific time trends | 0.096 | 0.0182** | 11,760 |
| | (0.0757) | (0.0077) | |
| 5.Country-specific age trends | 0.094 | 0.018** | 11,760 |
| | (0.0748) | (0.0076) | |

Robust standard errors in parentheses, clustered at the country and cohort level. Significance: * p<.1, ** p<.05, *** p<.01. All results are obtained from our baseline specification (Table 4 - column 1), using the full set of controls.
First stage statistics conferm both the relevance and the validity of the instruments.
[a] The range of estimates is obtained excluding one country at a time from our baseline specification.

*Table A3* Attrition

| | **Unweighted** | **Weighted** |
|---|---|---|
| retired | 0.241 | 0.288 |
| | (0.693) | (0.672) |
| distance | 0.239*** | 0.252*** |
| | (0.0656) | (0.0638) |
| age | 0.0452 | 0.0586 |
| | (0.0394) | (0.0388) |
| Hansen-J stat | 0.456 | 0.516 |
| N | 11,760 | 11,760 |

Robust standard errors in parentheses, clustered at the country and cohort level. Significance: * p<.1, ** p<.05, *** p<.01. Results are obtained using the full set of controls.
First-stage statistics confirm both the relevance and the validity of the instruments.

*Table A4* Count data models

| | **Pooled Poisson** | **Pooled NB** | **Cond. FE Poisson** | **RE NB** |
|---|---|---|---|---|
| retired | 0.165*** | 0.167*** | 0.106** | 0.111** |
| | (0.0314) | (0.0317) | (0.0507) | (0.0486) |
| distance | 0.0068*** | 0.0086*** | 0.0348*** | 0.0305*** |
| | (0.00253) | (0.00274) | (0.0121) | (0.0028) |
| N | 11,760 | 11,760 | 10,978 | 11,760 |

Robust standard errors in parentheses, clustered at the individual level (columns 1 and 2) or bootstrapped at the cluster level with 1,000 replications (columns 3 and 4). Significance: * p<.1, ** p<.05, *** p<.01. Results are obtained using the full set of controls.