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## The dynamic and heterogeneous effects of retirement on cognitive decline

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Hendrik Schmitz  
Matthias Westphal

**The Dynamic and Heterogeneous Effects  
of Retirement on Cognitive Decline**

# Imprint

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Hendrik Schmitz and Matthias Westphal<sup>1</sup>

# The Dynamic and Heterogeneous Effects of Retirement on Cognitive Decline

## Abstract

*We study effects of retirement on cognitive abilities (up to ten years after retirement) using data from 21 countries in Continental Europe, England, and the US, and exploiting early-retirement thresholds for identification. For this purpose, combines event-study estimations with the marginal treatment effect framework to allow for effect heterogeneity. This helps to decompose event-study estimates into true medium-run effects of retirement and effects driven by differential retirement preferences. Our results suggest considerable negative effects of retirement on cognitive abilities. We also detect substantial effect heterogeneity: Those who retire as early as possible are not affected while those who retire later exhibit negative effects.*

*JEL-Code: C31, J14, J24*

*Keywords: Cognitive abilities; retirement; event study; marginal treatment effects*

*November 2021*

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# 1 Introduction

As is well known, formation of cognitive abilities is a key determinant for human development, social interactions, and, ultimately, economic outcomes. Labor economists have, for instance, been studying the economic effects of education for decades. Less is known about the process of ability *depreciation*. According to [McFadden \(2008\)](#) “natural questions to ask are (...) the degree to which the depreciation of human capital components is an exogenous consequence of aging or can be controlled through work, study, and behavioral choices; and the degree to which depreciation is predictable or random.” Evidence from other disciplines suggests that ability decay is not a pure law of nature, but malleable through individual decisions ([Van Praag et al., 2000](#); [Salthouse, 2006](#); [Stern, 2012](#)). We analyze one major choice throughout the life cycle that may have a particular impact on this ability decline: the transition into retirement – the first time in life when a person lacks monetary incentives to maintain their abilities. We study effects of retirement on cognitive abilities up to ten years after retirement entry. Thereby, our paper contributes to the understanding of a policy relevant question of high importance for ageing societies such as Europe and the USA where cognitive abilities are of utmost importance in an ever more complicated world.<sup>1</sup>

We are not the first to study this research question. By and large (with a few exceptions), the evidence so far suggests that retirement negatively affects cognition (e.g. [Bonsang et al., 2012](#); [Rohwedder and Willis, 2010](#)) and length of retirement increases this negative effect ([Celidoni et al., 2017](#); [Mazzonna and Peracchi, 2012, 2017](#)). There is, however, heterogeneity with respect to type of former work ([Mazzonna and Peracchi, 2017](#); [Coe et al., 2012](#)), sometimes also with respect to gender ([Atalay et al., 2019](#)).<sup>2</sup> In spite of this existing work, we think that we can contribute to the literature. We claim to be doing this in two main dimensions.

First, while there is a consensus in the literature that it is important to take the time in retirement into account, existing studies make functional form assumptions on the shape of the duration effect, usually linear or logarithmic. To minimize parametric assumptions and to gain transparency, our study is the first in this literature that rigorously employs event-study methods. Second, we estimate marginal treatment effects (MTEs),<sup>3</sup> something

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<sup>1</sup>Complex decisions involve those on financial markets where studies find that low cognitive abilities lead to lower investments in stocks and other risky assets ([Christelis et al., 2010](#)), low levels of retirement saving and investment portfolios ([Banks and Oldfield, 2007](#)) and lower wealth ([Banks et al., 2010](#); [Smith et al., 2010](#)).

<sup>2</sup>A related literature is the one on health (behavior) effects of retirement, see, e.g., [Coe and Zamarro \(2011\)](#), [Shai \(2018\)](#), [Fitzpatrick and Moore \(2018\)](#), [Heller-Sahlgren \(2017\)](#), [Eibich \(2015\)](#), [Kämpfen and Maurer \(2016\)](#), [Giesecke \(2019\)](#).

<sup>3</sup>E.g. [Heckman and Vytlacil \(1999\)](#), [Brinch et al. \(2017\)](#), or [Westphal et al. \(2020\)](#) for methodological contributions and [Carneiro et al. \(2011\)](#), [Nybom \(2017\)](#), [Kowalski \(2022\)](#), [Cornelissen et al. \(2018\)](#), [Felfe and Lalive \(2018\)](#), [Kamhöfer et al. \(2019\)](#) for applications.

that has not been done in that literature before. Marginal treatment effect estimation helps us in two ways. On the one hand, we can study effect heterogeneities along an important scale: the willingness to retire. We can evaluate whether those individuals who retire as early as legally possible have different effects on cognition than those who postpone retirement to later periods. In the terminology of the MTE literature we can study if there is selection into gains. A simple theoretical model presented at the end of the paper suggests that this could be reasonable. On the other hand, explicitly taking preferences to retire into account helps us to disentangle the duration effect of retirement from these preferences. We argue below that, to get a clean average retirement duration effect, we need to control for the different (unobserved) preferences to retire. The MTE framework can achieve this.

As a more general contribution, going beyond the application of retirement and cognition, we combine two important trends in the field of empirical microeconomics – the rise in the use of event-study methods and the estimation of marginal treatment effects. We show how event-study estimations can be used to estimate marginal treatment effects and how this can be framed as an MTE that uses a discrete instrument. We argue that our estimation strategy can easily be transferred to other applications where some policy increasingly incentivizes individuals to take an endogenous treatment and long-run effects are of interest. The combination of event study and marginal treatment effects yields an estimator that exhibits all advantageous properties of event-study regressions and, at the same time, enables to go beyond the estimation of average treatment effects.

In our analysis, we pool data from the Survey of Health Ageing, and Retirement (SHARE), the Health and Retirement Study (HRS), and the English Longitudinal Study of Ageing (ELSA) on 112,000 individuals from 21 countries in Europe and the USA across the years 1995-2020. The data include experimentally collected measures of cognitive abilities (such as the word recall test). For identification, we exploit early retirement regulations. We find negative average effects of retirement on cognitive abilities for both men and women. The event study reveals broadly linear effects over time of notable magnitude. While there are only very small average effects upon retirement entry, effects unfold over time. Moreover, our finding of a negatively sloped MTE-curve suggests that there is a selection into gains (more accurately here, selection into non-losses). Those who retire as early as possible do not exhibit negative effects, neither in the short-, nor in the medium-run. Individuals who are less willing to retire (that is, retire late in their career) exhibit an immediate loss in cognitive abilities. On average, while accounting for differences in willingness to retire, there is a negative duration effect. After ten years, men lose around ten per cent of their cognitive ability score due to retirement, women around five percent. The effect compares to a general cognitive due to ageing ten years (women: five years).

The finding of selection into gains seems intuitive, but is not set in stone here. A negative slope of the marginal treatment effect curve might also reflect (unmeasured) opportunities



to retire. That is, for example, wealthy individuals can afford to retire early. Wealth might be correlated with innate preferences to retire which may confound the MTE estimations. Yet, we back the idea that we really identify heterogeneity across idiosyncratic preferences to retire by two auxiliary approaches in a final step. First, we derive a simple theoretical model that predicts negative marginal-treatment-effect gradient (thus, selection into gains). Second, we correlate estimates of willingness to retire with observable characteristics such as the level of cognitive abilities, wealth, or health behavior. We find that these measures are unlikely to explain the negative shape of the MTE.

This paper is structured as follows. Section 2 presents the data and institutional details. The empirical strategy is outlined in Section 3. Section 4 shows and discusses the results. Section 5 discusses alternative explanations for the finding, while Section 6 concludes.

## 2 Data and institutional set-up

### 2.1 Sample selection and dependent variable

We use data from the Survey of Health Ageing, and Retirement (SHARE), the Health and Retirement Study (HRS), and the English Longitudinal Study of Ageing (ELSA), three large biennial representative micro data sets providing information on health and other socioeconomic characteristics for individuals aged 50 and older. Since 1992, with more than 37,000 individuals living in 23,000 households, HRS has been in the field in the United States. ELSA followed in 2002 with 18,000 individuals and was influenced by and modeled on the HRS. SHARE was initiated as a cross-national survey in 2004. By now, 8 interview waves of SHARE are available covering information of about 140,000 individuals living in 29 European countries plus Israel.<sup>4</sup> All data sets are highly harmonized and can be used for pooled analyses.

For our analysis we employ SHARE waves 1, 2, and 4-8 as wave 3 (SHARELIFE) treats different aspects and does not contain the variables of interest.<sup>5</sup> Moreover, we use HRS waves 3–13 (interviews of the years 1995 to 2017<sup>6</sup>) and ELSA waves 1–8 (interviews 2002–2017). We restrict the sample to individuals between 50 and 75 whose self-stated labor force status (before retirement eligibility) is not “homemaker” or “permanently sick or

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<sup>4</sup>For comprehensive information on the sampling procedure, questionnaire contents, and fieldwork methodology of HRS, ELSA, and SHARE see [Sonnega et al. \(2014\)](#), [Steptoe et al. \(2003\)](#), and [Börsch-Supan and Jürges \(2005\)](#).

<sup>5</sup>See [Börsch-Supan \(2019a,b,c,d,e,f,g, 2021\)](#); [Brugiavini et al. \(2019\)](#).

<sup>6</sup>Data are taken from the RAND HRS Data file. This is an easy to use longitudinal data set based on the HRS data. It was developed at RAND with funding from the National Institute on Aging and the Social Security Administration.

disabled”.<sup>7</sup> In total, we have 355,680 observations from 112,852 individuals (55,446 men and 57,406 women) living in 21 countries.<sup>8</sup> Person-year observations per country are reported in Table A1 in the Appendix.

In the data sets that survey older individuals, non-random panel attrition is a potential problem. Referring to [Celidoni et al. \(2017\)](#) who use the SHARE in an analysis of the effect of retirement on cognitive abilities, we argue that it seems not to affect the results in this setting. They test for potential problems of panel attrition and also account for it by including an inverse Mills ratio. [Celidoni et al. \(2017\)](#) find that non-random panel attrition does not seem to be a relevant problem and accounting for it does not affect the results in their study. Thus, we do not address this issue in our study.

### Measure of cognitive ability

Cognitive abilities summarize the “ability to understand complex ideas, to adapt effectively to the environment, to learn from experience, to engage in various forms of reasoning, to overcome obstacles by taking thought” ([American Psychological Association, 1995](#)), where the sum of these abilities is referred to as intelligence. SHARE, HRS, and ELSA offer a number of potential measures for cognitive abilities: orientation in time, numeracy, verbal fluency and word recall tests.<sup>9</sup>

In the *word recall test*, the interviewer reads ten words and the interviewed is asked which of these words they can remember. The number of words they can recall is counted. This word recall test is done twice: directly after the words are read (immediate recall test) and about 5 minutes later (delayed recall test). The total number of words recalled in these two occasions are added up to yield the word recall test score. This score can range between 0 and 20. Further information in the test can be found in the supplementary materials. Word recall is a measure of episodic memory, which is found to react most strongly to ageing ([Rohwedder and Willis, 2010](#)). It is considered a measure of “fluid intelligence”. Broadly speaking, fluid intelligence is the innate cognitive ability while crystallized intelligence is what people learn in their lifetime (using their fluid intelligence). In our analysis, we follow much of the recent economic literature and employ *recall* as our main variable.<sup>10</sup>

The general bivariate relationship between word recall and age can be seen in the blue line in [Figure 1](#). Hardly surprising, getting older goes along with a steady decline in cognitive

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<sup>7</sup>In robustness checks, we use the age groups 50-80 and also keep homemakers in the sample and treat them as being retired.

<sup>8</sup>Austria, Germany, Sweden, Netherlands, Spain, Italy, France, Denmark, Greece, Switzerland, Belgium, Israel, Czech Republic, Poland, Hungary, Portugal, Slovenia, Estonia, Croatia, USA, England.

<sup>9</sup>This subsection heavily draws on [Schiele and Schmitz \(2021\)](#).

<sup>10</sup>See, e.g. [Rohwedder and Willis \(2010\)](#) and [Celidoni et al. \(2017\)](#). [Mazzonna and Peracchi \(2012\)](#) and [Coe et al. \(2012\)](#) use recall and a variety of other measures.

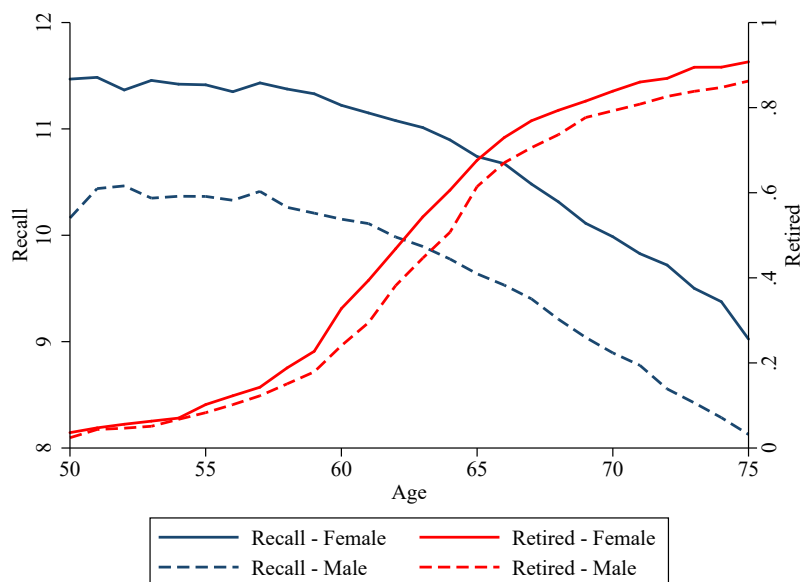


Figure 1: Cognitive abilities and retirement by age

Notes: Own calculation based on the pooled selected sample from SHARE, HRS, and ELSA. The graph plots unconditional averages by age in full years.

abilities. Interestingly, the average word recall level among of women is constantly larger than the one of men. The decline pattern ist similar, though.

## 2.2 Retirement

Our definition of retirement is based on the self-stated labor force status. We treat individuals as being retired if they choose the response option *retired* in the respective question.<sup>11</sup> Yet, we categorized as not being retired those who state to be retired but also work besides retirement (full- or part-time). Once retired according to this definition, we classify individuals are retirees for all periods to come.<sup>12</sup>

Like cognitive decline, retirement steadily increases in age, see the red curves in Figure 1. The female retirement rate is slightly higher than the male rate throughout, but the overall pattern is very similar: there is a small trend until the age of 59 up to an average probability to be retired of around 20 per cent, followed by a sharp increase between 60 and 65 up to around 70 per cent, and, finally, a more moderate upward trend until the age of 75. The main reason for this pattern are retirement eligibility rules. In many countries the earliest possibility to retire with pension benefits (not considering disability pension) is between ages 55 and 60. *Early retirement* typically comes with a penalty on pension benefits. This penalty steadily decreases in retirement age, typically, until the *official retirement age* (ORA) is reached. There is both variation in early and official retirement ages across countries and

<sup>11</sup>While SHARE respondents can select only one response option, HRS participants can select more than one labor force status. We consider HRS respondents as retired if their response includes retirement.

<sup>12</sup>We change this in a robustness test.

within countries over time due to retirement reforms. Consider Figure 2 for an overview on the average early retirement ages (ERA). The exact values are reported in Table A2 in the Appendix, whereas the institutional rules used to calculate early retirement ages are shown in Appendix C of the Supplementary Materials. Early retirement ages mainly vary by country, gender and time. There is a slight tendency to increase the early retirement age within the countries as a reaction of social policies to the challenges brought along by the demographic change, which we exploit as one source of exogenous variation.

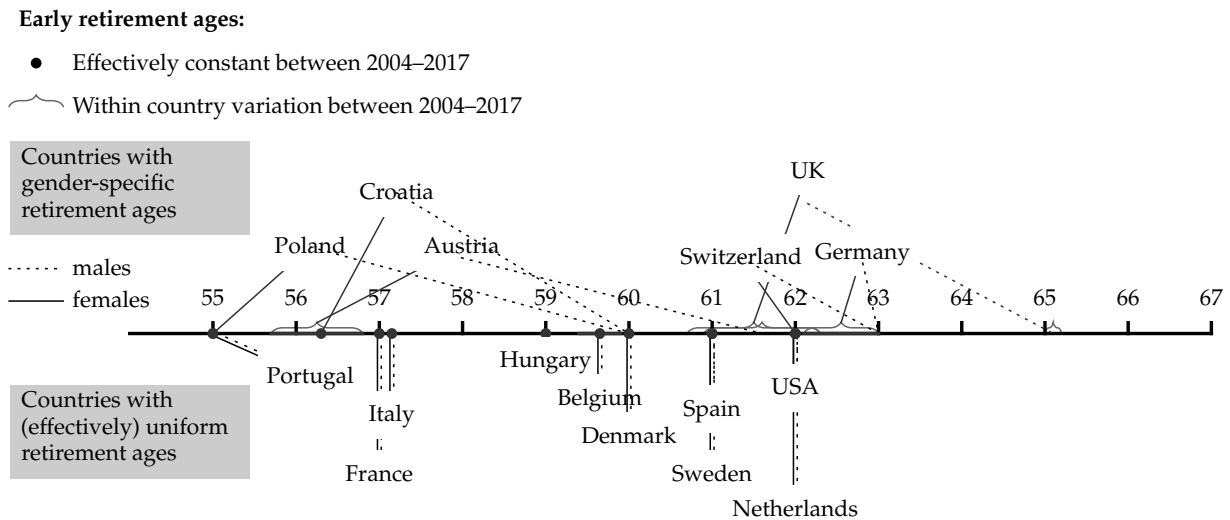


Figure 2: Overview on Early Retirement Ages

Notes: See Table A2 on the specific values in the Appendix and the institutional rules in Appendix C of the Supplementary Materials.

We also use differences in these country-specific early retirement ages between countries as exogenous variation to address the endogeneity of retirement in estimating the effect of retirement on cognitive abilities and to separate the age-related decline from the one that might be retirement-induced. This is a common instrument in this literature, see, e.g. Celidoni et al. (2017), Mazzonna and Peracchi (2012), Mazzonna and Peracchi (2017). However, in contrast to the previous literature, we only use the *early* but not the *official* retirement age as an instrument as we do not find a discontinuity in retirement rates at the official retirement age. We think of the early retirement age as a starting point of a dynamic incentive system. It starts by eligibility for retirement but with high penalties on retirement benefits. Each following year, the retirement incentives are increased by some penalty reductions until the official retirement age is reached. The official retirement age does not imply an additional incentive.

Figure 3 shows graphs on the retirement probability by age with (potentially gender-specific) early retirement thresholds for three selected countries. There are positive trends (probably age trends) in retirement before the early retirement age is reached. At that point

there are stronger jumps in retirement probability. No comparable discontinuities before or afterwards are observed. Thus, we argue that it is not a problem that there are differences in distance between ERA and ORA across countries that contaminate our results.

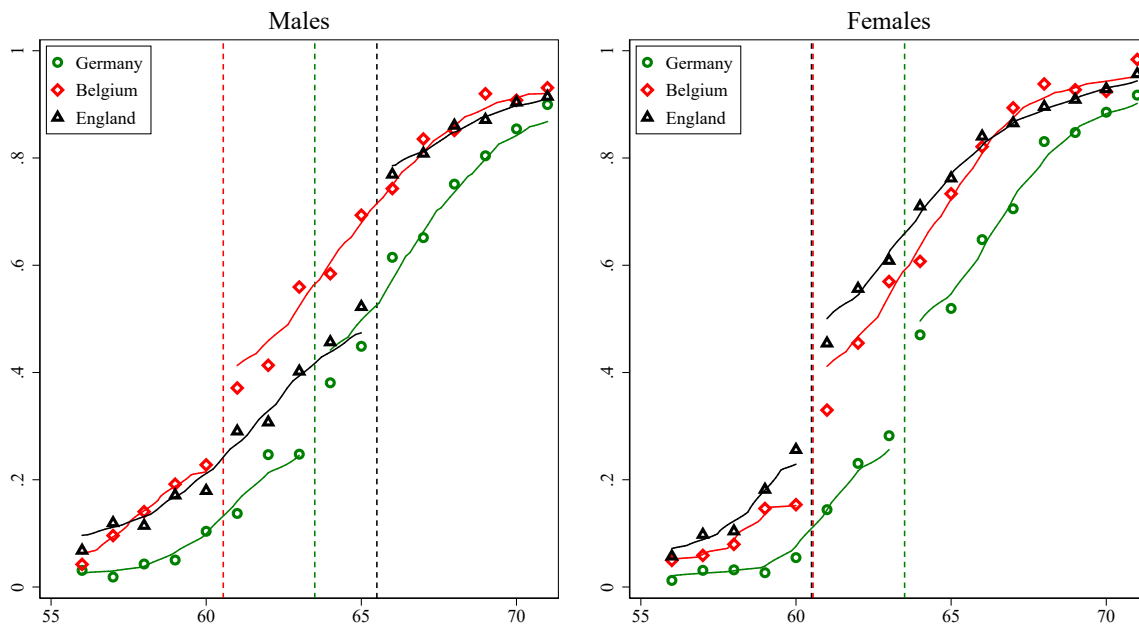


Figure 3: Retirement probabilities in three countries

Notes: Own calculations. Horizontal lines are early retirement ages (ERA). Circles/triangles/squares are unconditional shares of retired individuals in the sample. Lines are floating averages of the circles/triangles/squares for samples left and right of the ERA.

Before we move on to our empirical strategy, we summarize our most important variables and the respective surveys shares (from HRS, ELSA, and SHARE) to the overall sample by gender and early retirement eligibility in Table A3 in the Appendix. While the number of observations are nearly balanced between gender, we have more individuals above the early retirement threshold (which is beneficial for the identification of effects in the longer run). The participants in the SHARE are older compared to both, HRS and ELSA and therefore over-represented in the retirement eligible sample. Age most naturally varies by retirement eligibility, so do word recall and retirement. The years of education, as a control variable, also slightly vary by this threshold. The number of retakes of the recall assessment (repetitions) also differs by our treatment. This reflects the fact that older individuals participate in the surveys for longer. Therefore, we control in a fully flexible way for these test repetitions.

## 3 Empirical Strategy

### 3.1 The Retirement Decision

We start with a simple model that guides our empirical specification. Assume that individual  $i$  decides in period  $t$  about retiring now or retiring later. They receive utility  $V_{it}^1$  from retiring now, which can be thought of as the period- $t$  expected and discounted life-time utilities that comprise all observed and unobserved factors that coincide with retiring. Likewise,  $V_{it}^0$  denotes utility from continuing working and retiring later. In line with the Roy-model specifications in the marginal treatment effects literature (Heckman and Vytlacil, 2005), we model both as linear functions of observables and unobservables, that is:

$$\begin{aligned} V_{it}^1 &= X'_{it}\beta^1 + Z^1(e_{it}) + U_{it}^1 \\ V_{it}^0 &= X'_{it}\beta^0 + Z^0(e_{it}) + U_{it}^0 \end{aligned} \tag{1}$$

The vector  $X$  comprises observable characteristics such as age or the educational level. The characteristics in this vector may shift the benefits of retirement (or continued working) and hereby explain some part of the retirement decision.  $Z^1(e_{it})$  and  $Z^0(e_{it})$  are functions that denote the impact of institutional characteristics of the retirement system on the retirement decision. Specifically, these are the age-related eligibility to retire and the benefit replacement rate. The early retirement age (ERA) plays the crucial role in this context. ERA varies by country, gender, and partly also within countries by birth cohort, see Section 2. We think of the ERA as a starting point of a dynamic incentive system. Reaching the ERA induces eligibility for retirement but with high penalties on retirement benefits. Each following year, the retirement incentives increase by some penalty reductions until the official retirement age (ORA) is reached. The official retirement age itself does not imply an additional incentive. In order to account as flexible as possible for the early retirement age and the retirement incentives it induces, we define normalized age as in an event study specification as

$$e_{it} = (\text{age}_{it} + 1) - [\text{early retirement age}]_{it}$$

and let  $Z^1$  and  $Z^0$  be potentially non-parametric functions of relative age in full years.  $e_{it}$  plays the role of an instrumental variable below. Because we observe individuals only once per year in the data set,  $e = 1$  actually means that they are between one day and one year above the retirement eligibility threshold. For simplicity, we treat this as one year throughout the paper, keeping in mind that it is actually an average between 0 and 1 (and

likewise for  $e = 2$  and so on).  $U_{it}^1$  and  $U_{it}^0$  are unobserved, but by exploiting  $e$ , individuals may reveal some information about these unobservables as is described now.

Individuals decide to retire when  $V_{it}^1 > V_{it}^0$ . To simplify exposition but without any impact on the results later on, we assume that retirement is a one-time decision. Once retired, individuals do not re-enter the labor market. We denote retirement as the treatment  $D_{it}$  and rearrange the retirement decision in the spirit of Heckman and Vytlacil (2005):

$$\begin{aligned}
D_{it} &= \mathbb{1}\{V_{it}^1 \geq V_{it}^0\} \\
&= \mathbb{1}\{X'_{it}(\beta^1 - \beta^0) + Z^1(e_{it}) - Z^0(e_{it}) \geq -(U_{it}^1 - U_{it}^0)\} \\
&= \mathbb{1}\{X'_{it}\beta + Z(e_{it}) \geq U_{it}\} \\
&= \mathbb{1}\{F_U(X'_{it}\beta + Z(e_{it})) \geq F_U(U_{it})\} \\
&= \mathbb{1}\{Pr(D_{it} = 1 \mid X_{it}, e_{it}) \geq U_{it}^D\} \tag{2}
\end{aligned}$$

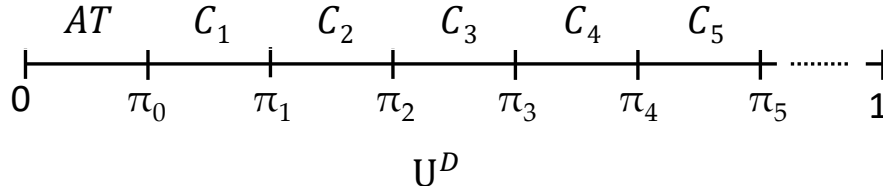
The second step separates observable factors (on the left-hand side) and unobserved terms (on the right-hand side), whereas the third step defines  $\beta_{it} = \beta_{it}^1 - \beta_{it}^0$ ,  $Z_{it} = Z_{it}^1 - Z_{it}^0$ , and  $U_{it} = -(U_{it}^1 - U_{it}^0)$ .  $U_{it}$  is the unobserved distaste to retire. In the second last equation, we normalize this unobserved distaste by applying the cumulative distribution function of  $U_{it}$  to both sides of the inequality. This monotonic transformation returns a uniformly distributed variable  $U_{it}^D$  when applied to its own (the probability integral theorem).  $U_{it}^D$  measures the relative ranks of the unobserved distaste to retire, ranging from zero (the lowest unobserved distaste for the treatment) to one (the highest distaste). Applied to the left-hand side, this yields the propensity score, the probability that a person retires, given their observable characteristics. Note that, because retiring is a one-time decision in our setting, there is only one constant  $U_{it}^D$  value per individual, such that  $U_{it}^D = U_i^D$ .

Fixing  $X_{it}$ , the retirement decision of  $i$  at time  $t$  depends on the relationship between  $Z(e_{it})$  and  $U_{it}^D$ . To see this, we capture potential retirement behavior by the variable  $D_{it}(e_{it})$  that indicates retirement at a (potentially hypothetical) value of  $e$  (holding  $X$  constant). Denote  $\pi_0 = Pr(D_{it} = 1 \mid X_{it}, e_{it} \leq 0)$ ,  $\pi_j = Pr(D_{it} = 1 \mid X_{it}, e_{it} = j)$  for higher values of  $e$ . Using Eq. (2), we get

$$\begin{aligned}
D_{it}(e_{it} = 1) &= 1 \Leftrightarrow \pi_1 \geq U_{it}^D \\
D_{it}(e_{it} = 2) &= 1 \Leftrightarrow \pi_2 \geq U_{it}^D \\
&\vdots
\end{aligned}$$

That is, for instance, individuals who retire in  $e_{it}=1$  reveal a  $U_{it}^D$  of  $\pi_0 \leq U_{it}^D \leq \pi_1$ . Those who retire in  $e_{it}=2$  reveal a  $U_{it}^D$  of  $\pi_1 \leq U_{it}^D \leq \pi_2$ . This is shown in Figure 4. Individuals

Figure 4: Types according to their willingness to retire



Own illustration.

with a  $U^D$  smaller than (observable)  $\pi_0$  could be denoted always-takers. They retire before reaching the ERA. Individuals with  $U^D$  of between  $\pi_0$  and  $\pi_1$  are the compliers that retire as soon as legally possible, abstracting from the routes the always-takers take into retirement (such as, for instance, retirement due to disability). They could be called complier group 1,  $C_1$ . Using the potential retirement indicator  $D(e)$ , this group can be indicated by  $\Delta D(1) := D(1) - D(0) = 1$  (correspondingly for all other complier types). In general, we can express the individual retirement status in  $e$  as a set of dummy variables that indicate the individual types, i.e. how they react to the incentives in  $e$ . This retirement observation rule reads

$$D_{it} = D_{it}(0) + \sum_j \left( D_{it}(j) - D_{it}(0) \right) \mathbb{1}(e_{it} = j) \quad \forall j \geq 1$$

The first term indicates always-takers whereas the second one indicates the complier groups that are retired at the incentive  $e=j$ . With each unit increase in the event time, an additional complier group retires (those with  $\Delta D_{it}(j) = 1$ ), joining the already retired individuals. To infer the  $U^D$  range of the complier groups, we estimate the this equation by using an event study specification as

$$D_{it} = \sum_j \delta_j \mathbb{1}(e_{it} = j) + X_{it} \beta^D + \varepsilon_{it}^D \quad (3)$$

for some general  $l \leq 0$  to allow the estimation of pre-trends. Here,  $\delta_j$  for  $j \geq 1$  are the coefficients of interest that capture – if certain assumptions hold – the causal effects of retirement eligibility along the event time on retirement, which is the cumulative complier share,  $E(D(j) - D(0))$ . Using  $\pi_0$ , which can be estimated as  $Pr(D = 1 | X, e \leq 0)$ , we get estimates of  $\pi_j = \pi_0 + \delta_j$  for  $j > 0$ .

### 3.2 The Outcome and Parameters of Interest

Define the following potential outcomes:  $Y_{it}^r$  denotes the potential cognitive abilities of individual  $i$  at time  $t$  after  $r$  years of retirement. For individuals who are not retired, the



observed outcome  $Y_{it}$  equals  $Y_{it}^0$ . Individuals who are retired have observed outcomes  $Y_{it} = Y_{it}^r$  with  $r > 0$ .

A potential parameter of interest could be  $ATT(r) = E(Y^r - Y^0 | D = 1)$ .<sup>13</sup> This is the effect of being retired for  $r$  years in the subgroup of retired individuals. Estimation of this effect is complicated by the fundamental evaluation problem (at a given  $t$  only one of the potential outcomes is observed for individual  $i$ ) as well as endogeneity of  $r$ , since  $r$  is a choice variable, as made clear in the previous subsection.

A more common parameter, estimated in the previous literature of retirement effects of cognition, is the local average treatment effect  $LATE(r) = E(Y^r - Y^0 | C)$  where  $C$  is a group of compliers, that is, individuals who retire due to a change in an instrument. This estimate could be received by a two-stage least squares estimation of the following equation:

$$Y_{it} = X_{it}'\beta + \delta r_{it} + \varepsilon_{it}$$

Here, the instrument could be  $\mathbb{1}(e_{it} > 0)$ . This is often done in the previous literature. A more flexible or at least transparent way to carry out such an instrumental variables estimation could be a Wald-type procedure combined with an event study specification. Here, we would first estimate the effect of retirement eligibility (the instrument) on  $Y$  using this specification:

$$Y_{it} = \sum_j \gamma_j \mathbb{1}(e_{it} = j) + X_{it}'\beta^Y + \varepsilon_{it}^Y \quad (4)$$

This is the equivalent to a reduced-form estimation in a two-stage least squares framework. We then could use the estimates from Eq. (3) and generate the Wald-type estimate  $IV(j) = \frac{\gamma_j}{\delta_j}$ . This, however, mixes up two components of the effect of retirement on cognition which can best be explained by an example. Consider  $IV(2) = \frac{\gamma_2}{\delta_2}$ , that is, we focus on the effect of having been eligible to retire for two years on cognition ( $\gamma_2$ ) and on actual retirement ( $\delta_2$ ). Yet, two different types of individuals are retired in  $e = 2$ . (i) those who retired as early as possible (in  $e = 1$ ) and have been retired for two years now and (ii), those who retired in  $e = 2$  and have been retired for one year. (i) captures a longer-run effect of retirement among individuals very willing to retire, while (ii) captures a short-run effect of individuals less willing to retire. We call the potentially different effect of retirement on cognition for a different willingness to retire (different  $U^D$ ) a “preference effect”. If  $IV(2)$  is negative, we cannot say whether this actually is a duration effect of retirement (i) or a

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<sup>13</sup>Here, and for the following parameters, we assume  $r > 0$ . Moreover, in the application, we again define  $r = 1$  to be retired for between one day and one year and speak of one year for simplicity.

preference effect (ii), or a mixture of both. Even a zero or positive  $IV(2)$  can be the result of a countervailing duration and preference effect.

Thus, we aim at disentangling both, retirement duration and preferences, and define as our parameter of interest:

$$MTE(r, u^D) = E(Y^r - Y^0 | U^d = u^D)$$

Having estimated this parameter will allow us to fix the preference parameter  $U^D$  and vary  $r$ , or, vice versa, to vary  $U^D$  while holding constant  $r$ . To derive an estimating equation for this effect, we first express the observation rule for  $D$  not as cumulative complier shares ( $D(j) - D(0)$ , mixing all compliers retired in  $j$ ) but in terms of the different complier groups  $\Delta D(j)$ :

$$D_{it} = D_{it}(0) + \sum_j \sum_{k=1}^j \Delta D_{it}(j-k+1) \mathbb{1}(e_{it} = j) \mathbb{1}(r_{it} = k) \quad \forall j \geq 1$$

This introduces a second summation along the retirement duration for the complier groups. At each event time  $j$ , these groups have to be retired between 1 and  $j$  years by definition (captured by the summation over  $k$ ). Hence, stratifying the event-time coefficient  $j$  by retirement duration estimates the share of compliers who retired in period  $j-k+1$  and who are retired for  $k$  periods. For instance, in  $e = 1$  retired compliers must be retired for one year ( $r = 1$ ) resulting in the retirement switching period 1. In  $e=2$ , compliers are either retired for one or two years. Those who are retired for two years must have switched into retirement in period  $2-2+1=1$ . We can plug in the general equation above into the observation rule of  $Y$ , and rearrange:

$$\begin{aligned} Y_{it} &= Y_{it}^0 + D_{it} \sum_{k \geq 1} \mathbb{1}(r_{it} = k) (Y_{it}^k - Y_{it}^0) \\ &= Y_{it}^0 + \left( D_{it}(0) + \sum_j \sum_{k=1}^j \Delta D_{it}(j-k+1) \mathbb{1}(e_{it} = j) \mathbb{1}(r_{it} = k) \right) \sum_{k \geq 1} \mathbb{1}(r_{it} = k) (Y_{it}^k - Y_{it}^0) \\ &= Y_{it}^0 + \sum_{k \geq 1} \mathbb{1}(r_{it} = k) D_{it}(0) (Y_{it}^k - Y_{it}^0) \\ &\quad + \sum_j \sum_{k=1}^j \mathbb{1}(e_{it} = j) \mathbb{1}(r_{it} = k) \Delta D_{it}(j-k+1) (Y_{it}^k - Y_{it}^0) \end{aligned}$$

Three terms remain: (i) the outcome without retirement (realized for never-takers and not yet retired compliers), (ii) the effect for the always-takers, and (iii), the effect for the different complier groups  $C_j$ . We want to identify average, duration-specific effects for this group,  $E(Y^r - Y^0 | C_j)$ . If we assume that, conditional on  $X$ ,  $e$  is independent of the

outcomes of never- and always-takers (details in the next subsection), we can estimate the above regression by including only interacted event-time and duration indicators for the complier groups plus the control variables. This regression then reads

$$Y_{it} = \sum_j \sum_{k=1}^j \gamma_{j,k} \mathbb{1}[e = j] \times \mathbb{1}[r = k] + X_{it} \beta^{Y'} + \varepsilon_{it}^{Y'} \quad (5)$$

Applying the (conditional) expectation operator to the observation rule above clarifies that estimates of the  $\gamma_{j,k}$  coefficients identify  $E(\Delta D(j-k+1)(Y^k - Y^0) | C_j, r=k)$ . Hence, we need to adjust  $\gamma_{j,k}$  by the complier share  $E(\Delta D(j-k+1) | r=k) := \delta_{j,k}$ . We estimate these parameters by a second regression of  $D$  on the same regressors as in Eq. (5) and receive an  $IV(j, k) = \frac{\gamma_{j,k}}{\delta_{j,k}}$  that varies over the retirement switching period  $j-k+1$  (the preference dimension), and duration  $k$ .

### Relation to the MTE literature

In order to translate this estimate into an estimate of a marginal treatment effect, we set the  $U^D$  of individuals who fall into the range of  $(\pi_j, \pi_{j+1})$  to the midpoint  $U^D = \frac{\pi_j + \pi_{j+1}}{2}$  and get as the estimate<sup>14</sup>

$$\widehat{MTE} \left( r = k, U^D = \frac{\pi_j + \pi_{j+1}}{2} \right) = IV(j, k)$$

The traditional literature on MTEs (see, e.g., [Heckman and Vytlacil, 2005](#)) requires a continuous instrument that, preferably, is able to shift every individual with  $U^D \in (0, 1)$  above or below the treatment participation threshold. Marginally varying the instrument makes individuals take the treatment who are at the margin of indifference. We have a discrete, ordered instrument and, thereby, our approach is closely connected to the paper of [Brinch et al. \(2017\)](#) who demonstrate that even binary instruments enable bounding the  $U_{it}^D$  value of compliers, never-, and always-takers, respectively. Because we have multiple complier groups, we can bound the  $U^D$  value more precisely. Our approach is different in the sense that we are the first to estimate MTEs with event time as an instrument. Due to the properties of  $e$ , MTEs are wedded to effects of treatment duration – a unit increase in the event time induces not only additional individuals to switch into retirement, already retired individuals are retired one year longer. If this is not taken into account, the two-stage least squares estimate of the effect of  $r$  on  $Y$  may be difficult to interpret, as estimates may capture variation in both, duration and preference. Our analysis demonstrates that

<sup>14</sup>Relating the midpoint of each  $U^D$  interval to the corresponding  $\widehat{MTE}$  approximates the MTE at this point. This approximation is exact if the true shape of the MTE is linear. If the MTE is non-linear, there may be some approximation error. However, this approximation error diminishes in the number and the size of the  $U^D$  bins that we can identify. Additionally, as we have multiple complier groups, we can test whether a linear MTE is plausible.

this is not a flaw but an opportunity for our analysis, which can make it an interesting application for the program evaluation literature.

### 3.3 Assumptions

As this is an instrumental variables estimator, we make the typical IV assumptions. First, we assume exogeneity of the instrument  $e$ , conditional on covariates in  $X$ . To achieve this, we include the following variables in  $X$ : We include country fixed effects, year of interview fixed effects as well as country-specific linear age trends. Moreover, years of education and number of participations in the survey are included as fixed effects. The exogeneity cannot be tested but absence of trends for  $e < 0$  can make us more confident that the assumption holds. These pre-trends reveal any sorting behavior or general decline in anticipation of the retirement. Any significance in these effects would indicate that equally-aged individuals in one country (where official retirement eligibility begins in, say, two years) cannot be compared to individuals in another country (where retirement eligibility is more years ahead).<sup>15</sup>

Second, the exclusion restriction needs to hold. That is, eligibility to retire needs to be the only change at the country-specific age thresholds. Since we use, *(i)*, many different countries, *(ii)*, changes over the years and within countries as well as, *(iii)*, different eligibility criteria between occupations that also depend on the individual employment history, this seems not to be a problem. Yet, in the US, for instance, retirement may also change health insurance coverage, as Medicare sets in. Medicare, however, is inextricably linked with retirement and, therefore, we think of this as a mediating channel of the retirement effect rather than an alternating explanation of the reduced-form effect. Note that our results do not depend on the inclusion of the USA. In a robustness check, we drop the USA from the sample.

Third, the incentives to retire set in at the early retirement threshold need to increase monotonically. This is no strong assumption, because actuarial mechanics ensure monotonically increasing monetary benefits, while the value of leisure should also increase the longer one defers retirement. By this assumption, the first-stage coefficients immediately identify the sample share of individuals who comply to these incentives and retire.

#### **On the use of cross-country variation in retirement eligibility**

Besides within-country variation in early retirement regulations, we mainly base our analysis on cross-country variation. This has pros and cons. If, as an alternative, we

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<sup>15</sup>In addition to this common trend assumption, we need to assume no anticipatory changes in cognitive abilities prior to the retirement eligibility on the individual level rather than only in mean outcomes. Although retirement schemes are anticipated after all, we argue that it is unlikely that (i) this structurally affects cognitive abilities already while working and (ii) there are anticipatory effects on the individual level, which may be averaged out in the pre-treatment outcomes.

focussed either on a single country and/or reforms that increase the ERA within that country – typically between one and three years – we would either need to restrict the analysis to short-run effects of retirement (such as in a regression-discontinuity design). Or, in order to nevertheless get long-run effects, it would mean to extrapolate short-run effects over a longer horizon using functional form assumptions on the duration effect (for instance a linearity assumption).

We argue, *(i)*, that it is valuable to study long-run effects of retirement on cognition. Much of the literature that uses credible regression discontinuity designs or difference-in-differences identifies short-run effects. While this is relevant and important, we assume that effects on cognition may need some time to manifest themselves. Moreover, *(ii)*, it is not clear per-se that effects evolve in a linear way over time. The considerable differences in ERA across countries allow us to non-parametrically separate long-run retirement effects from the general age-related decline. By “non-parametrically” we mean that we do not need to impose functional form restrictions on the retirement-duration effect but can allow for arbitrary dynamics by the separate relative-age indicators in Eq. (3) and Eq. (4).

The main disadvantage of this approach is that a one size fits all approach is used for different retirement systems. This raises the question whether our results of an average effect of retirement on cognition across countries may be biased. A major argument for a bias comes from the view of Sun and Abraham (2021) who show that, if treatment effects differ by entry cohort (here, relative age, defined by county-specific ERA), standard event studies specifications such as Eq. (3) and Eq. (4) may lead to biased results. This, would be the case if effects differ by countries (potentially due to different retirement systems) that have different entry cohorts due to different ERAs. Yet, Sun and Abraham (2021) deliver a solution, called the interaction-weighted estimator (IW-estimator) by estimating cohort-specific average treatment effects, which are then pooled to an overall average. If this average differs from the conventional estimates, this provides evidence that the conventional estimation does not work. Besides also using their approach, we additionally present robustness checks that either leave out the USA (see above for the reason) or provide subsample analyses by generosity of the retirement systems.

All in all, we argue that there is a trade-off between internal validity (use a single country in an RD framework) and external validity (learning more than just short-run effects) which cannot be completely resolved. Yet, we believe that our approach with a stronger focus on external validity (with no indication that internal validity is compromised) is an important contribution to the literature.

## 4 Results

### 4.1 Event-study evidence of retirement eligibility effects

We start by estimating the parameters of Eq. (3) and Eq. (4). While they are not used to estimate (and disentangle) the effect of *retirement* on cognition but show the effect of *retirement eligibility* we find it useful to report these results to get a first idea of the effects. We include single event-time indicators of  $e = -6$  up to the sample maximum of  $e = 26$ . Indicators smaller than -6 are binned into one indicator. We report effects up to  $e = 10$  as this is the relative age all countries in the sample still contribute to. Effects of event times larger than 10 are probably subject to sample selection issues and are mainly included for completeness and to avoid the implicit assumption that effects after  $e = 10$  are zero.

Figure 5 reports the results of Eq. (3, effect of retirement eligibility on retirement  $D$ , upper graphs) and Eq. (4, effect of retirement eligibility on word recall  $Y$ , lower graphs). We also report the results using the IW-approach suggested by Sun and Abraham (2021). This uses exactly the same observations and control variables as the baseline specifications.<sup>16</sup>

Results for men and women are remarkably similar. The same holds for the baseline specification versus IW-estimator. Regarding the retirement decision, we observe a negligible pre-treatment trend. Upon reaching the early retirement age, the retirement probability jumps up by around 10 percentage points. Until year six, this probability continues to increase steadily where, for females, the probability of being retired in  $e = 6$  is 35 percentage points larger than in  $e = 0$ . For males the effect after  $e = 6$  is smaller, namely around 25 percentage points. The increase in later years is negligible, again. This development mirrors the one in Figure 3 with age effects (plus education and country effects to name the most important confounders) now taken out.

Turning to the lower graphs, the effect of retirement eligibility on cognition does not exhibit any systematic pre-trends. Again fairly similar across gender, there are no detectable effects of retirement eligibility in the first post-years while there is a steady decline in cognitive abilities until in  $e = 10$  the eligibility effect amounts to 0.5 words less recalled. This linear decline in time justifies the linear specification in much of the previous literature (e.g. in Mazzonna and Peracchi, 2012, 2017). The finding of no instantaneous effect of retirement (eligibility) on cognition is also in line with Rose (2020) who employs a regression-discontinuity design for England using ELSA.

Since the results of the standard event-study estimations and the IW-approach are virtually the same, we are confident that the use of cross-country variation in ERAs is not too harmful. Moreover, we restrict the analysis to the standard event-study estimations in

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<sup>16</sup>Here, we use the Stata command `eventstudyinteract`, see Sun (2021).

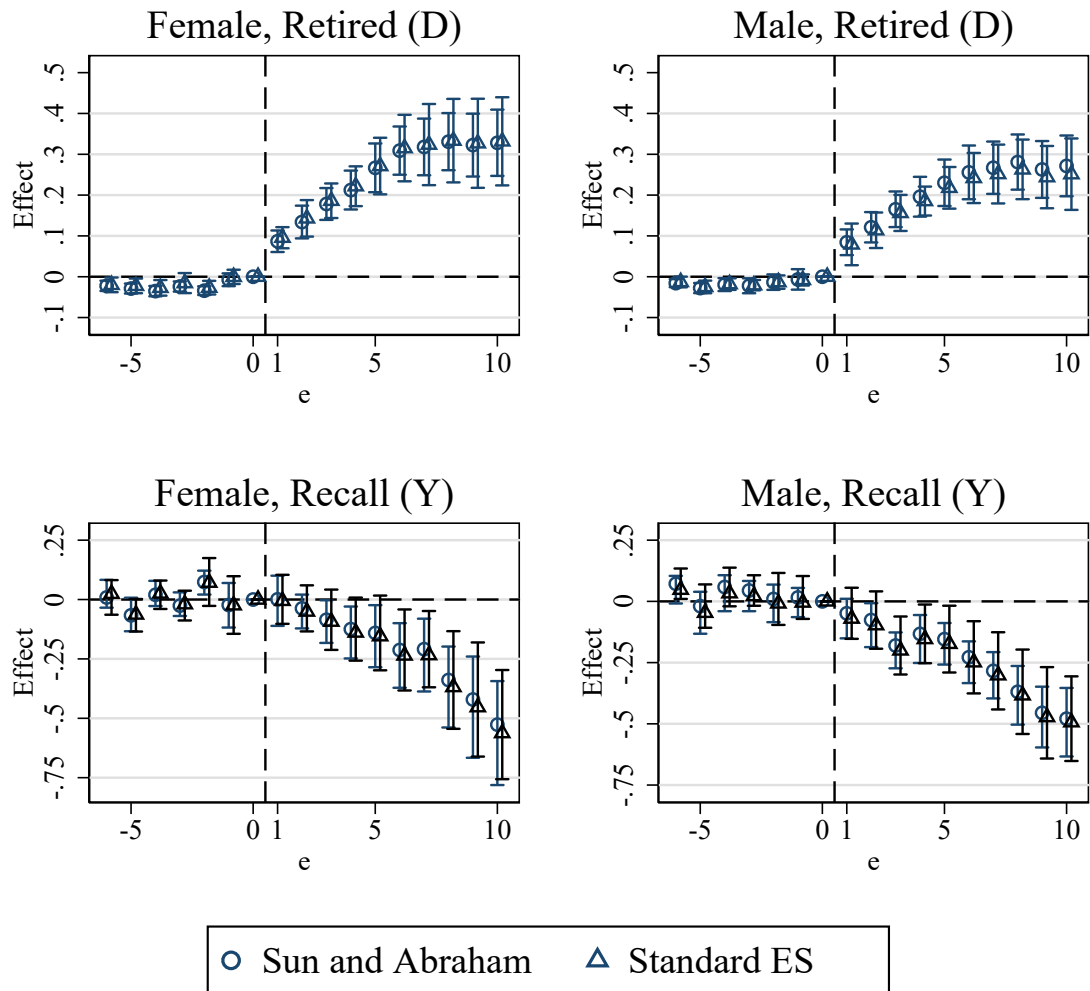


Figure 5: Event study graphs of the effect of retirement eligibility on retirement ( $D$ ) and word recall ( $Y$ )

Notes: Standard ES: Estimation results according to eq. (3) and (4). Sun and Abraham: Estimation results of averaged cohort-specific average effects according to eq. (3) and (4). 95 per cent confidence intervals. Standard errors clustered on country level. Number of observations: Female sample: 185,028; Male sample: 170,652.

what follows.

### Robustness checks

Figure 6 reports three robustness checks for the effects of retirement eligibility on recall that we discuss in turn.

#### *Controlling more flexibly for age trends*

One potential weakness of our empirical specification may be that we do not fully-flexibly control for age trends. While this is in accordance with the prior literature (Celidoni et al., 2017; Mazzonna and Peracchi, 2017), the identification of our retirement eligibility effects may nonetheless come from a non-linear, age-related decline that coincidentally interferes

with the retirement eligibility. We test the robustness of this assumption by including age group fixed effects (with 3-year age bins). The results in the upper panels of Figure 6 demonstrate that the linearity assumption seems to do justice to the data: there is no striking difference between our main estimates despite the results obtained from adding age-group fixed effects. The effects are unchanged for males (yet, a bit more noisy) but slightly larger for females. This may also suggest that more flexibly controlling for age trends beyond of what a “no pre-trend condition” would advocate could also do some harm and absorb some of the variation that is unrelated to age-related changes in cognitive abilities. For this reason we stick to the country-specific age-trends specification, although our data would also allow for a more flexible specification.

#### *Dropping the USA*

The USA have a large weight in the analysis and account for one third of all observations, see Table A1 in the Appendix. This raises the question whether the results are driven by the USA, a country with a different retirement system than Europe and one that also links retirement to health insurance receipt. The middle panels of 6 show that this is not the case. The effects are virtually the same without the USA, yet, less precise, of course.

#### *Accounting for different generosity in retirement benefits*

Countries differ in the generosity of their retirement systems which might be correlated with ERAs. If recipients of more generous retirement benefits are more able to invest in maintaining human capital and also retire later due to higher ERAs, this might induce a decline in cognitive ability. In order to account for this, we use OECD data on the benefit replacement rate of an average individual in each country.<sup>17</sup> These data vary across countries and over time within countries. We explain them in Appendix C. The median replacement rate in the OECD data is 50 per cent and we report results of subsamples with high generosity (at least 50 per cent replacement rate) and low generosity (less than 50 per cent replacement rate). The lower panels of 6 reveal that effects are found for both groups. Surprisingly, the effects seem to be even larger in countries with higher generosity. All in all, even though retirement systems differ, effects of retirement eligibility on cognitive decline seem to be a common phenomenon.

#### *Other robustness checks*

In Figure B1 in the Appendix, we report the results of additional robustness checks. First, we do not define retirement as an absorbing state but allow for individuals to return back into the labor market after retirement (which a handful of individuals does). Second, we include unemployed, homemakers and disabled in the sample and define all of them as being retired. This accounts for problems that individuals might use unemployment or disability as exit routes out of the labor force and into retirement, see [Garrouste and Perdrix \(2021\)](#). The results are hardly affected by that. Finally, we expand the sample to

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<sup>17</sup>Pensions at a glance, [OECD \(2007, 2009, 2011, 2013, 2015, 2017, 2019\)](#)



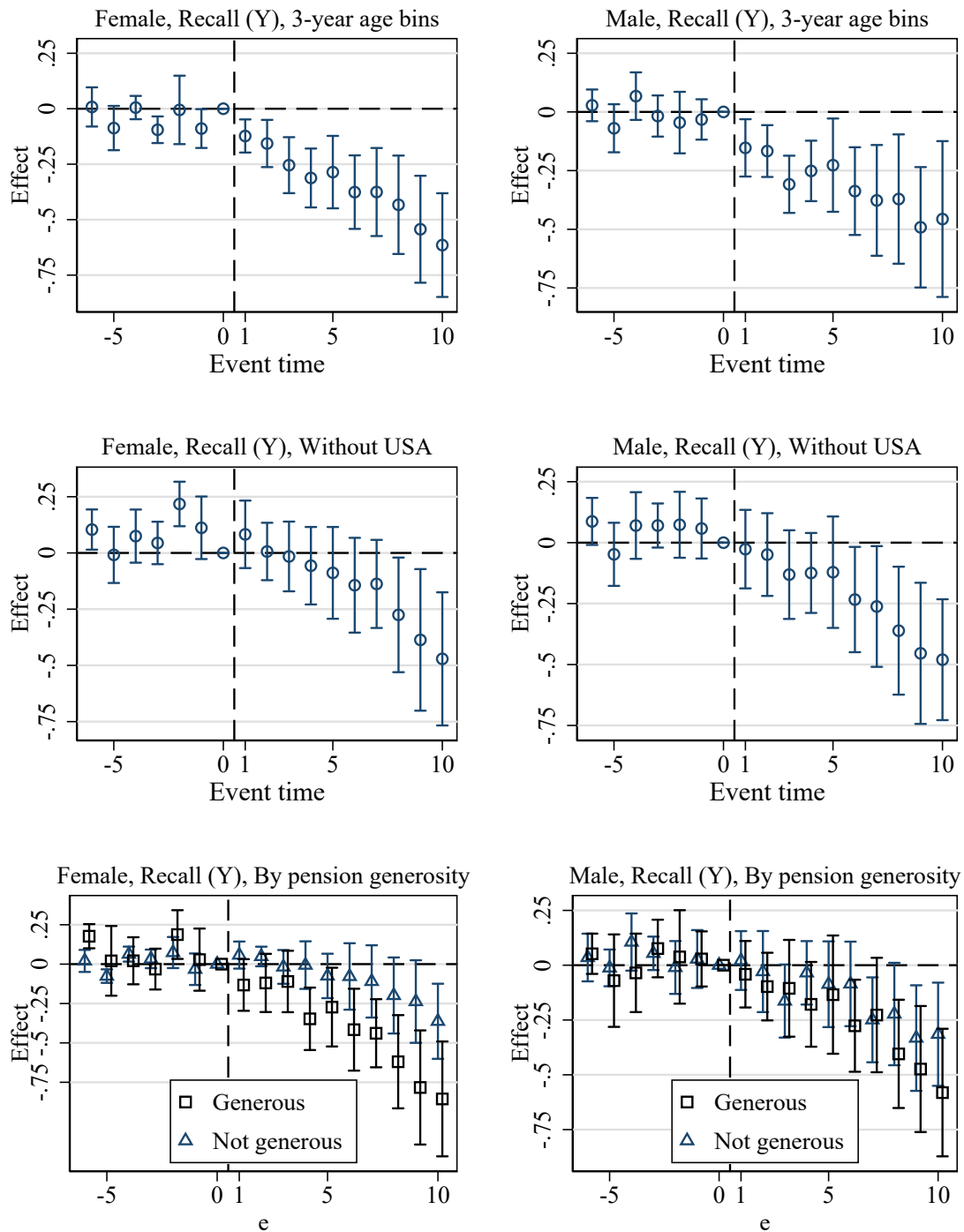


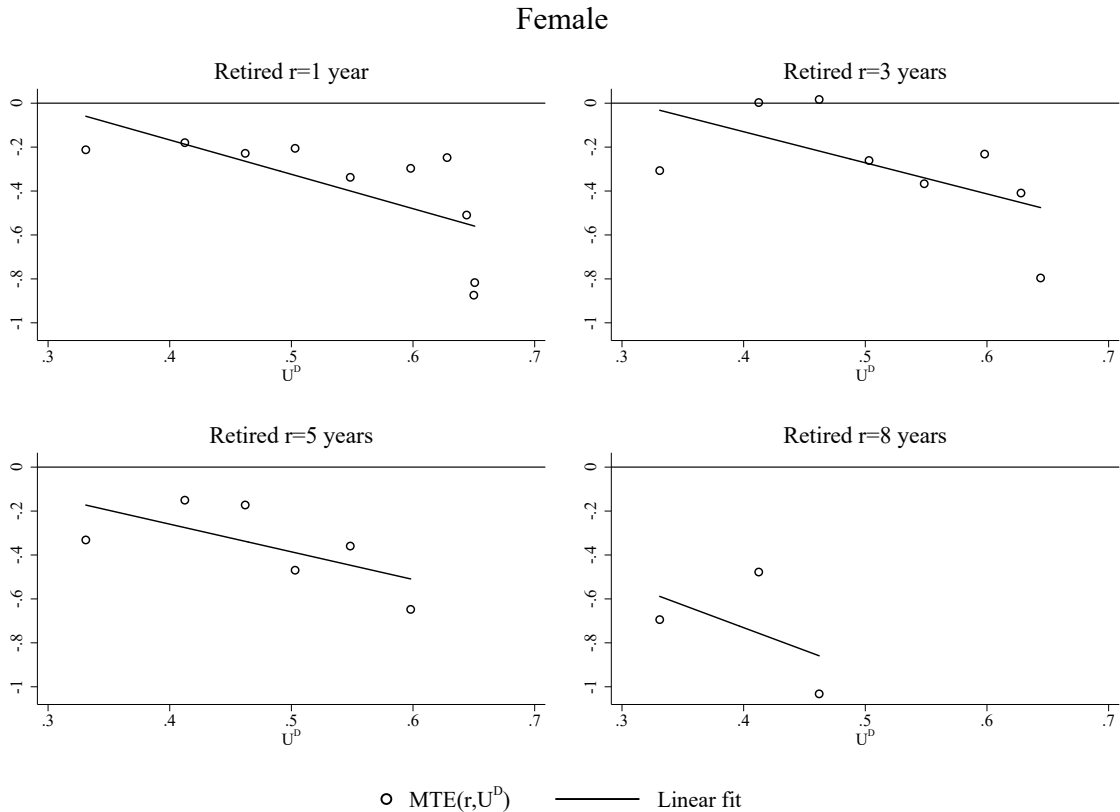
Figure 6: Event study graphs of the effect of retirement eligibility on retirement ( $D$ ) and word recall ( $Y$ )

Notes: Estimation results of Equations (3) and (4) with 95 per cent confidence intervals. Standard errors clustered on country level. Number of observations: Female sample: 185,028 (114,817 without USA); Male sample: 170,652 (114,916 without USA).

include age groups of 50 to 80 years. This allows to estimate effects up to 15 years after retirement eligibility. The results are reported in Figure B2. Here, the linear decline in cognition is simply continued over the additional five years.

## 4.2 MTE results

Next, we turn to the marginal treatment effects and estimation according to Eq. (5). Figures 7 (for women) and 8 (for men) map the  $MTE(r, U^D)$  for different retirement durations  $r$  (which we report in the different panels of the graphs). For the sake of a clear and most direct visibility, we present only four of nine duration- and gender-specific marginal treatment effects (the exhaustive list of MTEs for all durations are shown in Figure A1). Each panel has  $(11 - r)$  points that we can use to identify marginal treatment effects.



**Figure 7: Marginal retirement effects by years in retirement – Women**

The y-coordinates of this graph are produced by the  $IV(j, k)$  explained in Section 3. The corresponding x-coordinates are the  $\pi_j$  derived from the coefficients of Eq. (3) as  $\pi_j = \pi_0 + \delta_j$ . The merge id that links both coefficients is the index  $j$ .

Thus, we use 10 points to identify the marginal treatment effect for the effect of one year in retirement. We can do this for up to 10 years in retirement, when there is only one point left. Because we cannot identify MTEs in the latter case (without using the separate estimation approach as outlined in [Brinch et al., 2017](#) or [Kowalski, 2022](#)), we can only show marginal treatment effects up to nine years in retirement. Here, we decided to show marginal treatment effects for one, three, five, and eight years in retirement.

Again, the effects for males and females exhibit a similar pattern. As a general result, the MTEs – for most periods and both gender – exhibit a downward slope. Thus, individuals with the highest preference to retire have the smallest decline. For some durations the

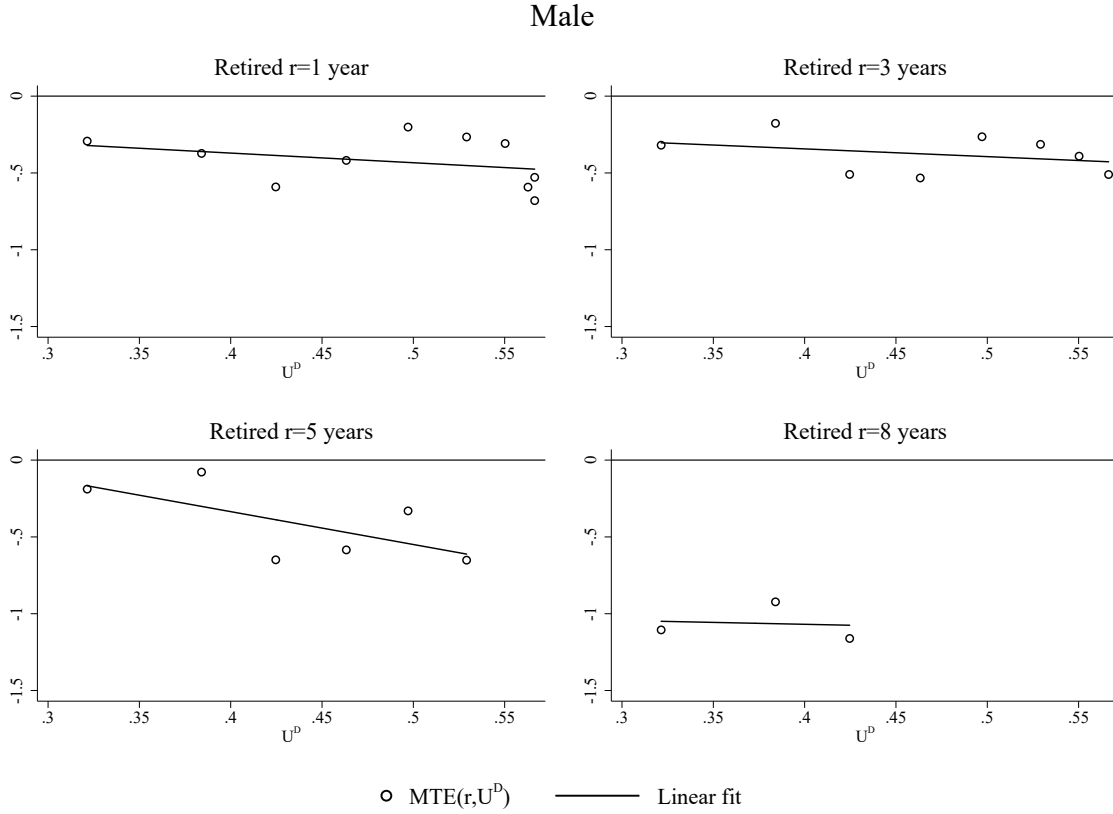


Figure 8: Marginal retirement effects by years in retirement – Men

The y-coordinates of this graph are produced by the  $IV(j, k)$  explained in Section 3. The corresponding x-coordinates are the  $\pi_j$  derived from the coefficients of Eq. (3) as  $\pi_j = \pi_0 + \delta_j$ . The merge id that links both coefficients is the index  $j$ .

cognitive decline due to retirement is basically zero or even slightly positive. In contrast to this, individuals who retire later and therefore have a lower preference for retiring have much larger negative effects. Consider, as an example, the upper left panel of 7, the retirement effect after one year in retirement for women. Women with the highest preference for retirement – those who retire upon reaching the ERA – have an effect close to zero (see the point furthest to the left). In contrast, the negative effect of retirement after only one year are largest for those who retire many years after reaching the ERA.

The figures provide an idea of the preference effect but it is more difficult to get the duration effect. In order to reduce complexity and learn about overall average effects, we now aggregate all single estimates. We use all data points of Figures 7 and 8<sup>18</sup> and run the following regression:

$$\widehat{MTE}(r, U^D) = \beta_U U^D + \eta_r + \varepsilon \quad (6)$$

The explanatory variables are the two candidate mechanisms that may cause the effects. While  $U^D$  is included linearly in the regression, duration is flexibly captured by a full set

<sup>18</sup>It is actually all data points of Figure A1 that report results for all values of  $r$  not just selected ones.

of fixed effects, called  $\eta_r$  here. This enables us to hold one dimension of this heterogeneity fixed as we examine the other one. Figure 9 plots the results of this task.

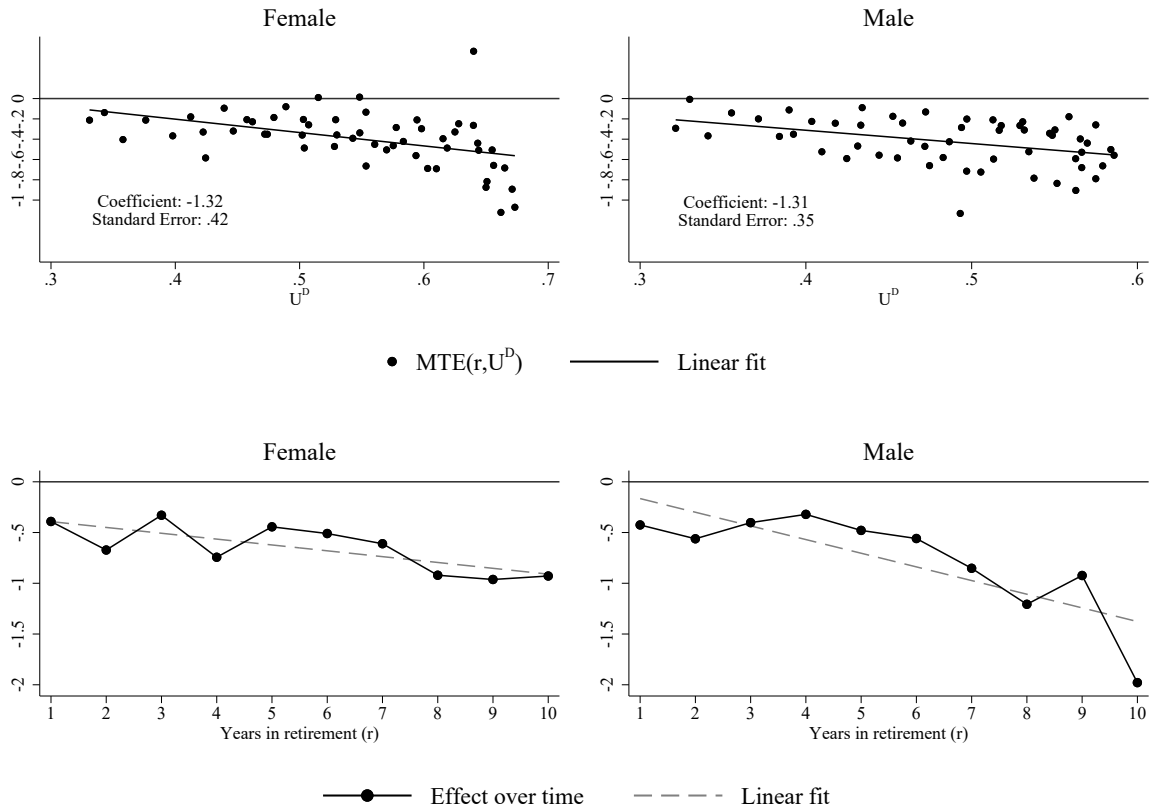


Figure 9: Decomposition of retirement effects into retirement preference and dynamic channel

Upper panels: The dots are all combinations of  $(MTE, U^D)$  as estimated before. The lines are predicted values after the regression  $\widehat{MTE}(r, U^D) = \beta_U U^D + \eta_r + \varepsilon$ . They are generated by setting  $r$  equal to one and varying  $U^D$  on the x-axis. Lower panels: The dots are predicted values after the regression  $\widehat{MTE}(r, U^D) = \beta_U U^D + \eta_r + \varepsilon$ , where  $U^D$  is set to the sample mean  $r$  is varied on the x-axis. The line is the linear fit through these predicted values.

The panels in the first row depict the regression line with the slope  $\widehat{\beta}_1$ . To get the dots (which are merely for visible inspection), we purge out the variation of  $r$  from  $\widehat{MTE}(r, U^D)$  and  $U^D$  (using two OLS regressions with duration fixed effects as regressors) and plot the residuals against each other. For a more convenient interpretation, the level of both residuals is adjusted to the mean of  $\widehat{MTE}(r, U^D)$  and  $U^D$  in  $r=1$ , respectively. The panels in the second row show corresponding effects of  $r$  when  $U^D$  is fixed at the mean. That is, we determine  $\widehat{MTE}(r, U^D = \bar{U}^D) = \widehat{\beta}_U \bar{U}^D + \widehat{\eta}_r$ , and plot the fitted values along  $r$ .

The upper panels now clearly reveal the effect heterogeneities, pooled across all durations and condensed into one graph. The coefficients  $\widehat{\beta}_U$  are virtually the same for men and women. If extrapolated to a  $U^D$  at the unit interval, this would mean that individuals who are most unwilling to retire ( $U^D = 1$ ) have a by 1.3 units stronger drop in word recall upon

retirement than those who are most willing to retire ( $U^D = 0$ ). Given that we only observe the range 0.32 to 0.67, we can broadly conclude that – among the compliers – those who retire as early as possible suffer a small loss of 0.2 words from an instantaneous loss of cognition while those who retire ten years later, lose around  $0.2 + 0.35 \cdot 1.3 = 0.655$  words due to retirement. The lower panels of Figure 9 report the effect of retirement duration. Males and females exhibit a small drop in cognitive skills in the year after retirement. Then, over time, cognition declines by around 0.08 words per year due to retirement duration for men and 0.044 words for women. Male retirees who have been retired for 10 years have a by 0.8 units lower recall score (according to the linear fit), a drop by almost 10 percent compared to the mean recall score at age 50, holding age and preferences constant. The female decline after ten years is of smaller magnitude and sums up to around half a word.

Because MTEs can be seen as the building blocks of all other treatment effects (Heckman and Vytlacil, 2005), one potential advantage of our MTE estimation with respect to the effects reported in the literature is that we can, in principle, report average treatment effects (ATEs) for the whole population (including always- and never-takers). Since the ATE is the most general treatment effect, it provides the most general insight on the effect of retirement on cognitive decline. Computing the ATE is simple if the MTE was linear (or symmetric) over the whole  $U^D$  interval. Then, the ATE equals the MTE at the average  $U^D$  value (0.5). Using Eq. (6), this is  $\beta_U 0.5 + \eta_r$ . Linearity over the unit interval, of course, is quite a strong and untestable assumption. Note, however, that we only need it for the ATE and not for the main MTE results. If we are willing to make this assumption, we get an ATE for the first year of retirement of -0.38 for females and -0.53 for males. With every additional year, this effect increases by 0.04 and 0.08 for females and males, respectively. These are large effects, especially for males. Ten years after retirement, the average men loses more than 1.3 recalled words words due to retirement compared to 1 word for the average complier. This suggests that the average complier to retirement instruments may have a substantially smaller retirement-induced cognitive decline than the average individual in the population.

### Robustness checks

Table 1 provides robustness checks similar to the ones for the baseline event studies. We report the estimated coefficients of Eq. (6) where we change the specifications used to estimate the  $\widehat{MTE}(r, U^D)$ . To keep things simple, we also include  $r$  in linear form and report its coefficient. First, to start with the simplest possible model, we only use the event-time indicators  $e_{it}$  in the regressions and leave out any control variables such as age, country, year or education. Since  $e_{it}$  now also captures effects of, e.g., general ageing, this is not a preferred specification. It is, however, insightful to see the role the control variables play for the range of  $U^D$  that can be identified. Reducing the number of control variables (that is, moving them into the error term) increases the variance of  $U^D$ . The estimated

degree of heterogeneity  $\beta_U$  also increases but is most likely overestimated. This makes clear the trade-off when using controls: we get more reliable results (internal validity) at the cost of a smaller range of  $U^D$  (external validity). Without going into details, we note that the results are qualitatively stable across different specifications.

Table 1: Robustness checks of MTE results

	$\beta_U$		$\beta_r$		min $U^D$	max $U^D$
Females						
<i>Control variables</i>						
No controls	-1.951	(0.239)	-0.020	(0.018)	0.387	0.972
Baseline	-1.319	(0.447)	-0.044	(0.018)	0.331	0.651
Baseline + Generosity	-1.071	(0.457)	-0.051	(0.020)	0.325	0.667
<i>Different samples/definitions</i>						
Without USA	-1.203	(0.454)	-0.044	(0.023)	0.274	0.674
Incl. OOLF	-2.156	(1.119)	-0.044	(0.028)	0.481	0.650
Not absorbing	-1.529	(0.5)	-0.049	(0.020)	0.311	0.605
Males						
<i>Control variables</i>						
No controls	-2.260	(0.260)	-0.080	(0.018)	0.391	0.942
Baseline	-1.308	(0.623)	-0.082	(0.020)	0.321	0.566
Baseline + Generosity	-0.900	(0.467)	-0.074	(0.021)	0.311	0.612
<i>Different samples/definitions</i>						
Without USA	-2.261	(0.655)	-0.090	(0.025)	0.310	0.572
Incl. OOLF	-1.828	(1.184)	-0.091	(0.022)	0.410	0.556
Not absorbing	-1.595	(0.617)	-0.096	(0.021)	0.302	0.529

*Notes:* Standard errors (in parentheses), generated by 100 bootstrap replications of the entire estimation procedure. The table shows the slope coefficients of the regression  $\widehat{MTE}(r, U^D) = \beta_0 + \beta_U U^D + \beta_r r + \varepsilon$ , where we change the specifications used to estimate the  $\widehat{MTE}(r, U^D)$ . “No controls” only uses the event-time indicators and no further controls. “Baseline” repeats the results from Figure 9. “Baseline + Generosity” adds an indicator for pension generosity to the controls, see the robustness checks in Section 4.1. “Without USA” uses the control variables of the baseline specification but drops observations from the USA. “Incl. OOLF” (= including out of the labor force) defines individuals who are either homemakers, disabled or unemployed as retired. “Not absorbing” allows for cases where individuals return back to work after retirement.

## 5 What explains the negatively sloped MTE-curve?

So far, we denoted different effects along  $U^D$  as “preference effects”, since  $U^D$  measures the unobserved willingness to retire. While this is standard in the interpretation of marginal treatment effects, it might not be very satisfactory. As a “residual concept”,  $U^D$  captures inherently unobservable factors like innate preferences but also potentially observable characteristics not accounted for in the control variables. Thus, it may well be that  $U^D$  does not only capture preferences for retirement but also opportunities to retire. For instance, individuals may retire early because they can afford to do so (for instance, because they

saved enough during their working lives), because they are not capable of working any longer (as their health commands them to retire), or because they have a different set of opportunities of how to allocate their newly-gained leisure time. It might actually be that these variables drive the shape of the MTEs even though we have inherent preferences in mind that do not change over time. That is, if the observable opportunities to retire are correlated with  $U^D$ , it might be that the MTE estimations actually capture opportunities instead of innate preferences for retirement.

One way to address this issue could be to include more control variables into the regressions of Eq. (5), thus, reducing the role of the error term, and see how the shape of the MTE behaves. Yet, one usually wants to avoid inclusion of clearly endogenous variables or even bad controls (variables affected by the treatment). Thus, we go a different way and carry out an analysis that is comparable to the analysis of “complier characteristics” in Angrist and Pischke (2009). Specifically, in Section 5.1, we estimate the untreated levels of observable characteristics by the same  $U^D$  as before. These are characteristics like health and financial assets that answer the question whether those who comply at later times are different with respect to these potentially relevant characteristics than those who comply earlier. Additionally, in Section 5.2, we present a simple theoretical model that shows: if cognitive abilities complement individual consumption utility more in retirement than when working, the MTE-curve should indeed have a negative slope without the need to interpret this as having missed to include more control variables in the regression.

## 5.1 Who are the compliers?

We plot potential variables that reflect opportunities to retire against the  $U^D$ . Starting with the *level* of cognitive abilities, we are first interested in  $E(Y^0|U^D)$ , the distribution of the potential outcome without treatment. This number was first estimated by Carneiro and Lee (2009) in a marginal treatment effects framework. Mogstad et al. (2018) call this the marginal treatment response (MTR). This function informs about the average characteristics of the different complier groups defined along  $U^D$  at the door of the transition into retirement.

Getting this magnitude requires to carry out the separate estimation approach (first done in Imbens and Rubin, 1997, for a binary instrument and treatment, then adopted to the MTE by Carneiro and Lee, 2009 – using a continuous instrument – and by Brinch et al., 2017 for a binary instrument). To do this, we define the variable  $a_i$ , which may be called the retirement switching period. It indicates the period on the normalized age  $e$ , in which each individual eventually retires (and hence, it is time-constant). The retirement switching period  $a$  is actually the same period according to which the different complier groups are

defined (every complier must have  $\Delta D(a_i) = 1$  by definition of  $a_i$ ) Hence, the parameters of the following equation must be caused by the different complier groups:

$$Y_{it} = \sum_{j \geq 0} \theta_j^{\text{MTR}} \mathbb{1}(a_i = j) + \sum_{j \geq 0} \gamma_j^{\text{MTR}} \mathbb{1}(a_i = j) D_{it} + \varepsilon_{it}^{\text{MTR}} \quad (7)$$

In this regression, the  $\theta_j^{\text{MTR}}$  coefficients capture the mean untreated value of  $Y$  for the different complier groups. Would we, instead of including indicators on  $a$ , include indicators on event time  $e$ , the coefficients would not isolate effect of specific complier groups, but they would apply to all not-yet retired individuals jointly (accumulating different complier groups). The  $\gamma_j^{\text{MTR}}$  coefficients capture the difference with respect to retired individuals. This treated value is potentially driven by differential effects of retirement duration. However, we are interested in the baseline characteristics, and hence, we focus on  $\theta_j^{\text{MTR}}$  that naturally does not vary with retirement duration.

We then replace  $Y$  by other observable characteristics and choose

- A measure of individual wealth: the net value of the total assets that the household possesses.
- A dummy indicating doing activities requiring a moderate level of energy at least once a week
- A dummy indicating doing sports or other vigorous activities at least once a week
- A dummy indicating whether one experiences time pressure due to heavy workload in the job
- A dummy indicating whether the job is physically demanding

Figure 10 shows the results of this exercise. The MTR for cognitive skills (upper left panel) shows, if anything, a slight positive trend: those who retire as early as possible have, on average, a slightly lower level of cognitive skills than individuals who postpone their retirement. The differences are very small, however, and therefore it is extremely unlikely that the small MTR gradient may explain the marginal treatment effects completely. In net total assets at retirement (the upper right panel), we find a small u-shaped pattern: those who retire as early and those who work as long as possible seem to have slightly more assets. However, there is no monotonic correlation between  $U^D$  and the asset value of the individuals before retirement. In all of the other variables, we do not find any heterogeneity: Retirement timing (and thus, the revealed preference to retire,  $U^D$ ) is neither correlated with the activity level or retirees, nor with the propensity to do sports before retirement. Moreover, the share of individuals with time pressure in their jobs and with a job that is physically demanding seem both to constant across individuals with different



retirement preferences. This visual inspection suggest that retirement opportunities are far from dominating the MTE heterogeneity.

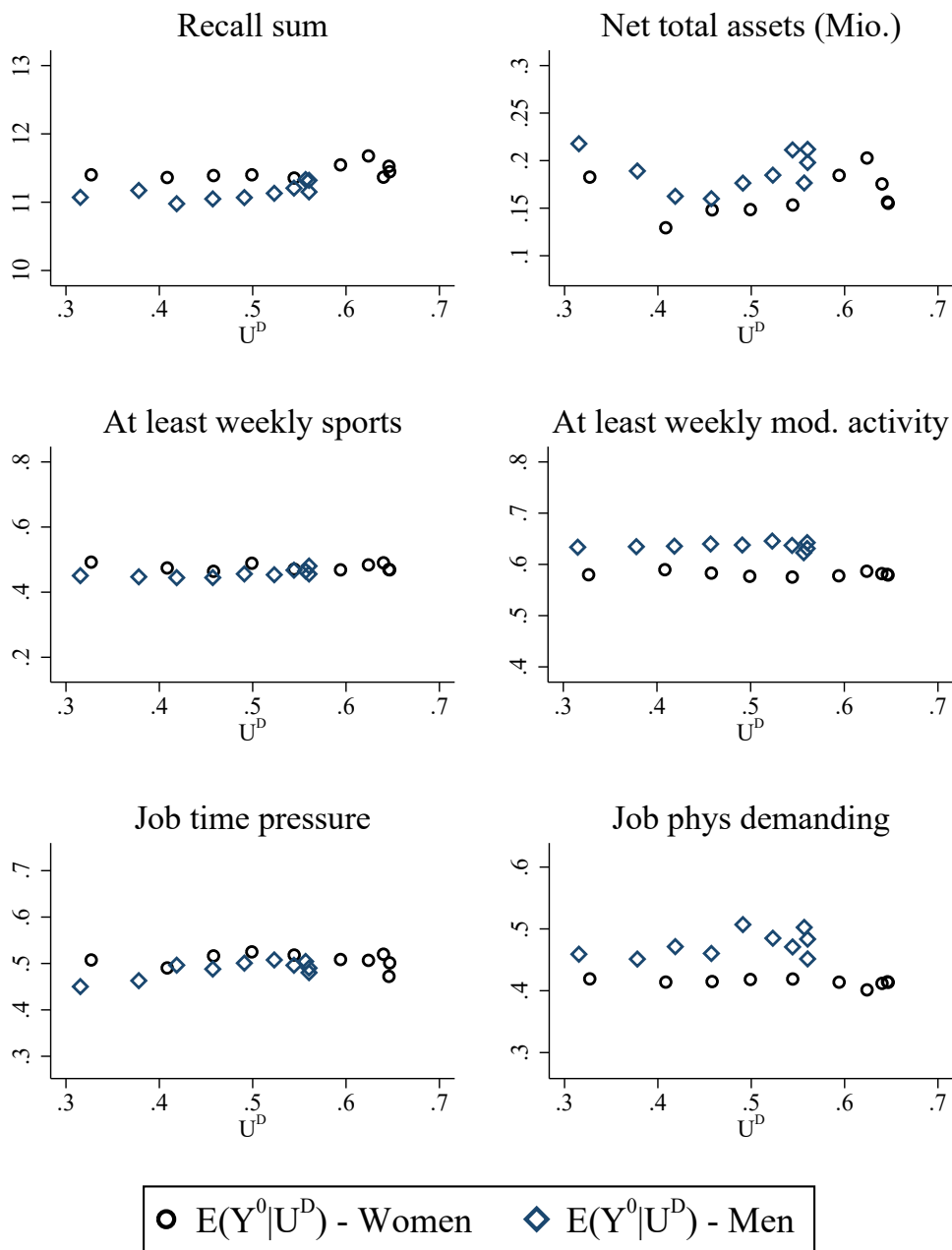


Figure 10: Marginal treatment response functions

Sample means and standard deviations of the variables: Net total assets: Mean= 0.21, sd= 753048.4; Recall sum: Mean= 10.18, sd= 3.41; Dummy for at least a weekly activity requiring a moderate level of energy: Mean= 0.82, sd= 0.39; Dummy for at least weekly sports or other vigorous activities: Mean= 0.46, sd= 0.50;

## 5.2 A simple two-period model that explains negatively sloped MTEs

After having inspected the heterogeneity in  $Y^0$  between the complier groups, we now turn to the treated outcome  $Y^r - Y^0$ . Considering the retirement effect may drive the retirement preferences, as, potentially, anticipated losses in retirement could defer individuals to retire. Key to understanding economic behavior is the utility function that individuals seek to maximize. This also helps in this setting to understand the forces that govern the selection into retirement and the effects thereof. Whereas the most appropriate and sophisticated way to do this are life-cycle models in continuous time, we sketch a two-period model. Such a simplified model is more easy to grasp but provides the same implications as a more generalized version.

The general motive of individuals is to maximize life-time utility given an initial endowment of cognitive abilities, while being constrained by their budget and their natural evolution of their abilities. With two periods  $t \in \{0, 1\}$ , the life-time utilities with and without retirement ( $V^1$  and  $V^0$ ) in Eq. (1) are the sum of the period-specific utilities:

$$\begin{aligned} V^1 &= u(c_0^1, Y_0^1) + \frac{1}{\rho} u(c_1^1, Y_1^1) \\ V^0 &= u(c_0^0) + \frac{1}{\rho} u(c_1^0) \end{aligned}$$

For simplicity, the decision is between retiring in  $t=0$  (and remaining retired in  $t=1$ ) and working both periods. Life-time utility is the sum of utility attained in  $t=0$  plus the  $\rho$ -discounted utility in  $t=1$ . Independent of retirement, utility depends on the consumption level  $c_t$  in the respective period. With retirement (now indicated by superscript 1, ignoring the retirement duration for simplicity), the utility level additionally depends on the level of cognitive abilities  $Y$ . Essentially, this modeling reflects the fact that retirement may improve the value of consumption, as individuals have more leisure time to make more conscious consumption choices. Modeling retirement in such a way is done frequently in the literature (e.g., in [Gustman and Steinmeier, 1986](#); [Fan et al., 2017](#)).

Without retirement, individuals do not have a direct incentive to maintain their cognitive abilities ( $V^0$  does not increase in  $Y^0$ ). Yet, the earnings level  $E$  depends on the level of cognitive abilities—individuals earn more with a higher level of cognitive abilities. When retired, in contrast, the earnings are exogenously set by the public pension benefit scheme. Hence, the potential earnings are

$$E_t = \begin{cases} E_t^0 = f(Y_t^0) & \text{if } D_t = 0 \\ E_t^1 = \text{const} & \text{if } D_t = 1, \end{cases}$$

with  $f(Y_t^1)$  being an increasing function in  $Y_t^0$ . In addition, individuals face a budget constraint and a constraint on how cognitive abilities may evolve:

$$\begin{aligned} E_0 + \frac{1}{1+r}E_1 &= c_0 + I_0 + \frac{1}{1+r}c_1 \\ Y_1 &= I_0 + (1-\delta)Y_0 \end{aligned}$$

The first equation (the budget constraint) determines how individuals can spend their lifetime income ( $E_0 + 1/(1+r)E_1$ ) either on consumption in the two periods ( $c_0$  and  $c_1$ ), or they can invest in their cognitive abilities ( $I_0$ ) (investing in abilities in period 1 is a waste of resources as this investment would materialize in period 2). The second equation (law of motion for cognitive abilities) determines the level of cognitive abilities in period 1. The endowment of cognitive abilities with which individuals are born ( $Y_0$ ) declines by the factor  $(1-\delta)$  from period 0 to period 1. Individuals can counteract this decline by investing in their abilities ( $I_0$ ), which reduces their consumption opportunities but may raise their wage (when not retired) or their overall utility directly (when retired). These constraints close this simple individual maximization problem. We now introduce one source of heterogeneity that can explain differences in  $U^D$ . The heterogeneity enters the model in how cognitive abilities may improve the individual consumption utility in retirement.

We model individuals to be heterogeneous in how cognitive abilities affect their utility in retirement. Some individuals may have hobbies, or social networks, that they can better pursue in retirement. A higher level of cognitive abilities in retirement makes it more enjoyable, while for others, cognitive abilities do not affect their utility. We believe that this can be the heterogeneity that drives  $U^D$ , in particular, given that we did not find evidence for endowment effects before retirement that can explain our negatively sloped MTEs. We model this channel by allowing the utility to be individual-specific and that this heterogeneity enters the model via the parameter  $\beta_i$ . In addition, cognitive skills  $Y_t^1$  and consumption  $c_t^1$  are multiplicatively connected:

$$u_i(c^1, Y^1) = u(c^1 Y^{1\beta_i})$$

To make the very simple case (but without loss of generality), consider the case of log-utility  $u_i(c^1, Y^1) = \ln(c^1 Y^{\beta_i})$ . Assuming  $Y_t^1 > 1$  and all equal, a higher  $\beta$  makes individuals retire earlier, as it raises the utility surplus with retirement (the determinant underlying the retirement decision in Eq. (1)):

$$\frac{V^1 - V^0}{\partial\beta} = \ln(Y_0^1) + \frac{1}{\rho} \ln(Y_1^1)$$

Hence,  $\beta$  drives the timing of retirement and the preference for it. Put differently,  $\beta$  may cause variation in  $U^D$ . The higher  $\beta$ , the earlier (and at a lower incentive) individuals retire (holding all else fixed).

How does  $\beta$  affect the effect of retirement? To explore this issue, we compute the first order conditions of the maximization problem in case of retirement, rearrange and get:

$$Y_1^1 = \frac{\beta}{\rho} c_0^1$$

Hence, cognitive abilities in retirement increase, if the preference for retirement  $\beta$  increases:  $\partial Y_1^1 / \partial \beta = c_0^1 / \rho > 0$ .<sup>19</sup> In contrast,  $Y_1^0$  is unaffected varying  $\beta$  (as suggested by Figure 10), i.e. the retirement effect  $Y^1 - Y^0$  has to increase in  $\beta$ . This provides the missing link that demonstrates that individuals with the lowest  $U^D$  (the highest  $\beta$ ) may actually have the largest (or least negative) effects, if preference for retirement provides an incentive to maintain their skills. Hence, negative slope of the MTE curve can be caused by this channel: individuals who retire earlier do not have structurally different characteristics with which they retire. Yet, their (social) environment in which they retire might be actually more stimulating. These unobserved characteristics that jointly correlate with the retirement effect and with the retirement decision.

## 6 Conclusion

We study the short- and medium-run effects of retirement on cognitive abilities using data from 21 countries in Europe and the US and make two main contributions to the literature. First, we use transparent and flexible event-study methods to estimate the effects with, arguably, few functional form assumptions on the shape of the medium-run effect of retirement. Second, we exploit revealed preferences for retirement by observing individuals to retire at different points in time after being eligible to do so. This enables us to extend the event-study estimations and estimate marginal treatment effects. This adds to the marginal treatment effect literature (Heckman and Vytlacil, 1999), particularly the one that deals with discrete instruments (Brinch et al., 2017).

We find considerable negative average effects of retirement on cognitive abilities. After ten years, retirement leads to a reduction of almost one word recalled in the word recall test among men (almost half a word for women). This is almost 10% when compared to an average recall of 11 words at the age of 50. We also find effect heterogeneity: those who retire as early as possible have small or no negative effects while those who retire later

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<sup>19</sup>Expressing everything in exogenous variables,  $Y_1^1$  reads:  $Y_1^1 = \frac{\beta}{\rho+1+\beta} (E_0 + \frac{E_1}{(1+r)} + (1-\delta)Y_0^1)$ . Its derivative w.r.t.  $\beta$  is:  $\frac{\partial Y_1^1}{\partial \beta} = \frac{\rho+1}{(\rho+1+\beta)^2} (E_0 + \frac{E_1}{(1+r)} + (1-\delta)Y_0^1) > 0$ .

lose up to 20% of their cognitive abilities, on average, as measured by the word recall test. This is an intriguing result, not least because – in contrast to most existing applications of marginal treatment effects (see [Kamhöfer et al., 2019](#); [Carneiro et al., 2011](#); [Nybom, 2017](#)) – the slope of the marginal treatment effects curve may be a-priori ambiguous.

In terms of methodology we argue that it is the application of marginal treatment effect estimation to a dynamic event-study setting that allows to differentiate between true dynamic effects and effects that are due to preferences to take the treatment. This can be extended to other settings that are characterized by a dynamic incentive system. This setting also offers a way to use the estimation approach suggested by [Brinch et al. \(2017\)](#) and [Kowalski \(2022\)](#) without the need to assume a linear MTE. Our results suggest that a linear MTE is a justified assumption in this context, however, something that – without further ado – could not have been tested without our estimation approach.

We conclude this study by deriving theoretical counterparts of our empirical MTE estimates to provide a rationale for the uncovered negatively-sloped MTEs. This model suggests a negative MTE, if skills complement the retirement-induced leisure value and if it is genuine preferences that drive the timing of retirement. Using marginal treatment response functions, we find suggestive evidence that indeed genuine, idiosyncratic preferences seem to drive differences in retirement timing rather than endowment effects.

These findings are important for better understanding the old-age skill decline. It is not unalterable, but malleable through retirement choices. This decline moreover correlates with economic core parameters, like preferences and incentives. Taken at face value, one policy implication is straight at hand: granting individuals even a higher freedom of choice of when to retire (and how much to work after retirement) is likely to induce a positive net effect on the skill decline of older people: individuals with high preferences may even exhibit positive effects on their skills, while individuals with low preferences to retire may postpone their skill decay toward later ages and stay active longer.

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## Appendix: Additional tables and figures

Table A1: Number of observations by country

Country	Observations
Austria	13,184
Germany	16,142
Sweden	15,354
Netherlands	7,120
Spain	11,045
Italy	5,988
France	17,172
Denmark	12,674
Greece	4,885
Switzerland	10,964
Belgium	13,660
Israel	3,951
Czech Republic	10,977
Poland	8,478
Hungary	3,869
Portugal	2,628
Slovenia	11,052
Estonia	11,857
Croatia	4,310
USA	125,947
England	44,423

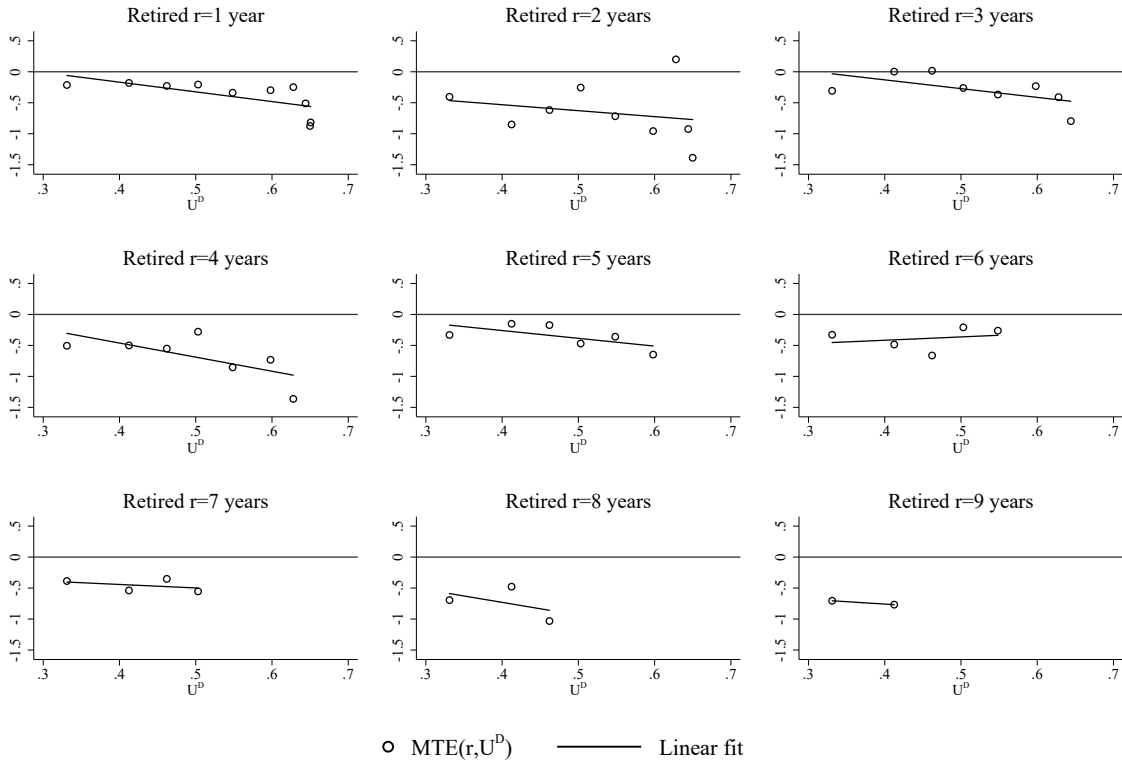
Own calculations based on HRS, ELSA, and SHARE. Numbers are person-year observations in the final estimation sample, based on the sample selection criteria as laid out in Section 2.1.

Table A2: Overview Early Retirement Ages

	Women		Men	
	2004-2006	2013-2017	2004-2006	2013-2017
Austria	55.7	56.9	60.9	62.3
Germany	62.1	63.0	63.0	63.0
Sweden	61.0	61.0	61.0	61.0
Netherlands	62.0	62.0	62.0	62.0
Spain	61.0	61.0	61.0	61.0
Italy	57.2	57.1	57.2	57.2
France	60.0	60.0	60.0	60.0
Denmark	60.0	60.0	60.0	60.0
Greece	50.4	50.7	60.7	60.7
Switzerland	62.0	62.0	63.0	63.0
Belgium	59.4	59.9	59.6	59.9
Israel	57.7	57.3	59.8	59.5
Czech Republic	50.0	54.9	57.0	59.7
Poland	55.0	55.0	60.0	60.0
Hungary		59.0		60.0
Portugal		55.0		55.0
Slovenia		59.4		59.4
Estonia		56.2		56.3
Croatia		56.3		60.0
USA	62.0	62.0	62.0	62.0
England	60.7	62.4	65.0	65.2

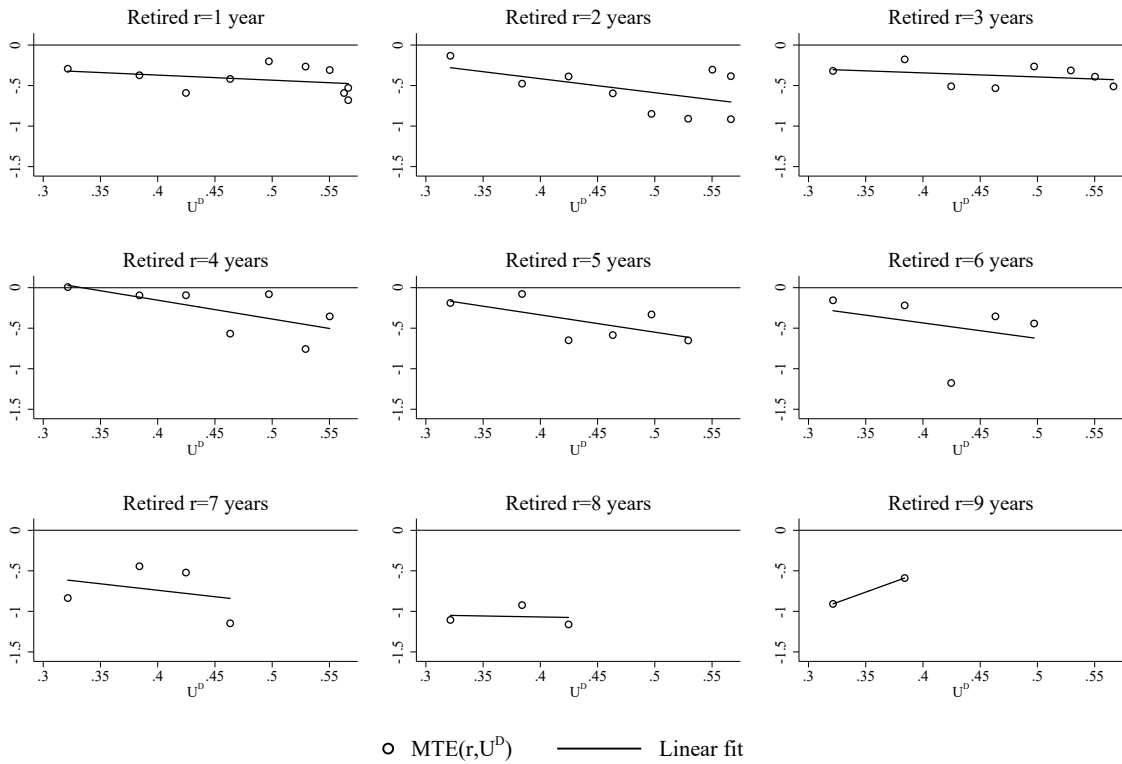
Source: Own calculations, based on the estimation sample and institutional rules described in Appendix C. Early retirement ages in countries without the possibility of early retirement are set to the regular retirement ages. Reported numbers are averages in the sample as early retirement age sometimes also depends on the individual work history. Empty cells indicate that SHARE data of these countries are not available for that time span.

## Female



(a) Women

## Male



(b) Men

**Figure A1: Marginal retirement effects by years in retirement**

The y-coordinates of this graph are produced by the  $IV(j, k)$  explained in Section 3. The corresponding x-coordinates are the  $\pi_j$  derived from the coefficients of Eq. (3). The merge id that links both coefficients is the index  $j$ .

Table A3: Descriptive statistics by retirement eligibility

Variable	Women		Men	
	Early retirement eligibility Ineligible	Eligible	Early retirement eligibility Ineligible	Eligible
ELSA	0.139 (0.346)	0.116 (0.320)	0.185 (0.388)	0.089 (0.285)
HRS	0.447 (0.497)	0.340 (0.474)	0.347 (.476)	0.314 (0.464)
SHARE	0.414 (0.492)	0.544 (0.498)	0.468 (0.499)	0.597 (0.491)
Age	55.864 (3.133)	67.101 (4.714)	56.648 (3.287)	67.806 (4.298)
Recall sum	11.500 (3.246)	10.298 (3.454)	10.415 (3.222)	9.120 (3.327)
Test repetitions	1.490 (1.591)	2.638 (2.406)	1.194 (1.341)	2.282 (2.164)
Retired	0.143 (0.350)	0.738 (0.440)	0.151 (0.358)	0.707 (0.455)
Years of education	12.391 (3.953)	11.203 (4.363)	12.210 (4.236)	11.441 (4.501)
Number of observations	132,431	223,249	132,431	223,249

Own calculations based on HRS, ELSA, and SHARE. Numbers are person-year observations in the final estimation sample, based on the sample selection criteria as laid out in Section 2.1.

# The Dynamic and Heterogeneous Effects of Retirement on Cognitive Decline

— For Online Publication: Supplementary Materials —

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## Appendix B: Additional tables and figures

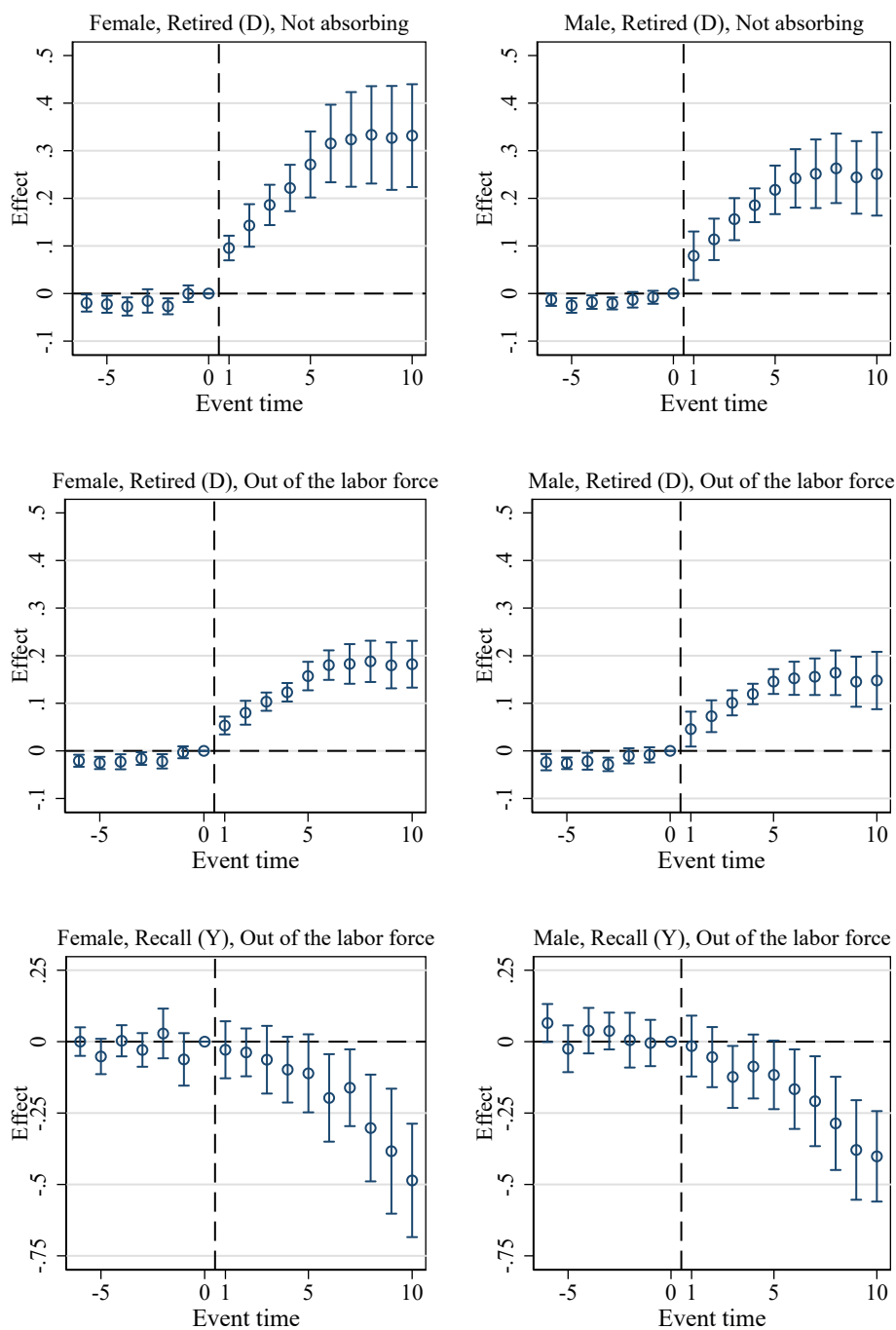


Figure B1: Event study graphs of the effect of retirement eligibility on retirement ( $D$ ) and word recall ( $Y$ )

Notes: Estimation results of Equations (3) and (4) with 95 per cent confidence intervals. Standard errors clustered on country level. Out of the labor force means that we included unemployed, disabled, and homemakers and treat them as retired. Number of observations: Female sample: 185,028 (217,147 including out of the labor force); Male sample: 170,652 (179,811 including out of the labor force).



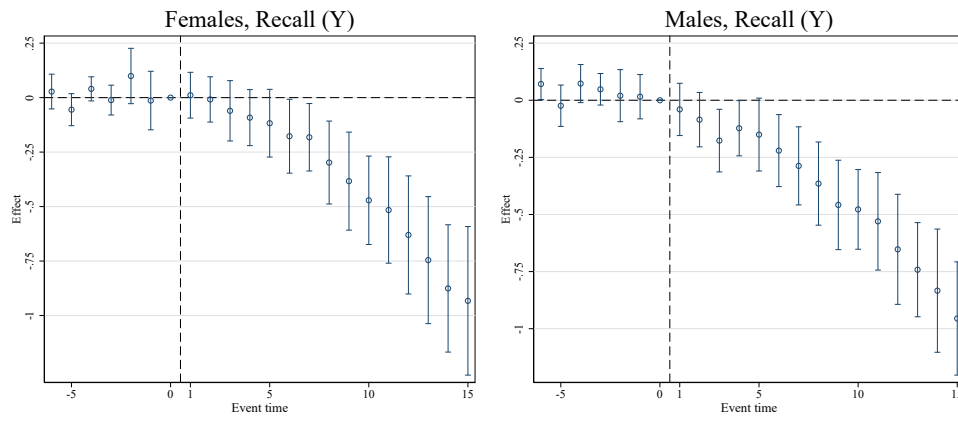


Figure B2: Event study graphs with a sample up to age 80

Notes: Estimation results of Equation (4) with 95 per cent confidence intervals. Standard errors clustered on country level. Number of observations: Female sample: 210,147; Male sample: 195,486.

## Appendix C: Retirement rules

### Early retirement eligibility criteria

Early retirement eligibility criteria are mainly based on [Celidoni et al., 2017](#). If there are deviations, sources are reported with country specific rules below.

#### Austria

*For men:* Before 2001, early retirement age (ERA) is 60. From 2001 onwards, ERA is still 60 for those with at least 45 contribution years. Otherwise, ERA depends on the year of birth from 2001 on as follows. From 2001 to 2004, ERA is 61 for those born until 1942 and 62 for those born 1943 and later. From 2005 onwards, ERA is still 61 for those born until 1942, 62 between 1943 and 1944, 63 between 1945 and 1947, 64 between 1948 and 1950, and 65 for those born in 1951 and later.

*For women:* Before 2001, ERA is 55. From 2001 onwards, ERA is still 55 for those with at least 40 contribution years. Otherwise, ERA depends on the year of birth from 2001 on as follows. From 2001 to 2004, ERA is 56 for those born until 1947, 57 for those born between 1948 and 1951, and 58 for those born in 1952 and later. From 2005 onwards, ERA is still 56 for those born until 1947, 57 between 1948 and 1949, 58 between 1950 and 1952, 59 between 1953 and 1955, and 60 for those born in 1956 and later.

#### Belgium

*For men:* From 1967 to 1997, ERA is 60.

*For women:* From 1967 to 1986, ERA is 55 and from 1987 to 1997, ERA is 60.

*For both:* From 1998 on, ERA is 60 for both men and women, depending on contribution years: In 1998, at least 20 contribution years are needed, 24 in 1999, 26 in 2000, 28 in 2001, 30 in 2002, 32 in 2003, 34 in 2004 and 35 from 2005 on. For individuals employed in the public sector ERA is 58 from 1986 to 2008.

**Czech Republic** (see [CSSZ, 2019b](#) [Ministerium Arbeit und Soziales, 2019](#), [Rabušic, 2004](#), [CSSZ, 2019a](#))

*For men:* Until 2009, ERA is 57. From 2010 onwards, ERA is 60.

*For women:* ERA depends on the number of children. For women without children until 2009 ERA is 54. From 2010 to 2014 ERA is 59. From 2015 onwards ERA is 60. For women with one child until 2009, ERA is 53. From 2010 to 2014 ERA is 58. From 2015 to 2017 ERA is 59. From 2018 onwards ERA is 60. For women with two children until 2009 ERA is 52. From 2010 to 2014 ERA is 57. From 2015 to 2016 ERA is 58. From 2017 to 2018 ERA is 59. From 2019 onwards, ERA is 60. For women with 3 to 4 children until 2009 ERA is 51. From 2010 to 2014 ERA is 56. From 2015 to 2017 ERA is 57. From 2018 to 2020 ERA is 58. From 2021 to 2023 ERA is 59. From 2024 onwards ERA is 60. For women with 5 or more children

until 2009 ERA is 50. From 2010 to 2017 ERA is 56. From 2018 to 2020 ERA is 57. From 2021 to 2023 ERA is 58. From 2024 to 2026 ERA is 59. From 2027 onwards, ERA is 60.

*For both:* Contribution years depend on the year, where ERA is reached. Until 2009 CY=25, in 2010 CY=26, in 2011 CY=27, in 2012 CY=28, in 2013 CY=29 in 2014 CY=30, in 2015 CY=31, in 2016 CY=32, in 2017 CY=33, in 2018 CY=34 and from 2019 onwards CY=35.

**Denmark** (see [Angelini et al., 2009](#))

*For both:* From 1976 to 1978, ERA is 60. From 1979 onwards, ERA is 60 for those people with at least 30 contribution years.

**Estonia** (see [Puur et al., 2015](#), [Sotsiaalkindlustusamet, 2019](#))

*For men:* Before 2001: ERA is 45 if the man is visually impaired or a lilliputian with at least 20 contribution years. ERA is 55 for a widower with a disabled child and with 20 contribution years. ERA is 60 for those with 5 contribution years. From 2001 to 2020 ERA is reached 3 years before statutory retirement age, resulting in: ERA is 60 for those born from 1941 to 1956, ERA is 61 for those born from 1957 to 1960 and 62 for those born since 1961 with 15 contribution years, respectively.

*For women:* Before 2001: ERA is 40 if the woman is visually impaired or a lilliputian with at least 15 contribution years. ERA is 50 for those with a disabled child and 20 contribution years. ERA is 55 for those with at least 5 children and 15 contribution years. ERA is 55 for those with 5 contribution years. From 2001 to 2020 ERA is reached 3 years before statutory retirement age, resulting in: ERA is 56 for those born in 1946, ERA is 57 for those born from 1947 to 1948, ERA is 58 for those born from 1949 to 1950, ERA is 59 for those born from 1951 to 1952, ERA is 60 for those born from 1953 to 1956, ERA is 61 for those born from 1957 to 1960 and ERA is 62 for those born since 1961 with 15 contribution years, respectively.

*For both:* From 2021 onwards, ERA is 60 with at least 40 contribution years, ERA is 61 with at least 35 contribution years, ERA is 62 with at least 30 contribution years, ERA is 63 with at least 25 contribution years and 64 with at least 20 contribution years. From 2027 onwards, ERA will be bounded on life expectation. Having three children reduces the statutory retirement age by 1 year, four children reduces it by 3 years and five or more children (or a disabled child) reduces it by 5 years for one parent, respectively. For civil servants, retirement is possible at every age for those with at least 25 contribution years.

**France** (see [Godard, 2016](#))

*For both:* From 1963 onwards, ERA is 60.

**Germany**

*For men:* From 1973 to 2003, ERA is 60 for those with at least 15 contribution years and 63 from 2004 onwards with at least 15 contribution years.

**For women:** From 1962 to 2003, ERA is 60 for those with at least 15 contribution years, 62 from 2004 to 2005 with at least 15 contribution years, and 63 from 2006 onwards with at least 15 contribution years.

**Greece** (see [EU Kommission, 2019](#), [Hauser and Strengmann-Kuhn, 2004](#))

**For men:** For men who started working before 1993: ERA is 58 with 35 contribution years. For all men: ERA is 60 with 15 contribution years. ERA is 50 for a widower with a disabled child and 18 contribution years.

**For women:** For women who started working before 1993: ERA is 55 with 15 contribution years. ERA is 50 for women with underage children and 18 contribution years. For women who started working since 1993: ERA is 60 with 15 contribution years. ERA is 50 for women with underage children and 20 contribution years.

**For both:** ERA is 62 with 15 contribution years.

**Israel** (see [Kol-Zchut, 2019](#), [Shai, 2018](#), [Justizministerium, 2019](#))

**For men:** ERA is 60 for men.

**For women:** Until 2004, ERA is 55. From 2005 onwards, ERA is 58 for those born between May 1951 and April 1953, 59 for those born between May 1953 and April 1955 and 60 for those who were born after April 1955.

**For both:** (Kindergarten-)Teacher can retire at every age with at least 20 contribution years. ERA is 57 for kindergarten teachers born between March 1947 and April 1948, 58 for those born between May 1948 and April 1950 and 59 for those born after April 1950 with at least 10 contribution years, respectively. For other civil servants ERA is 55 for those born between March 1949 and April 1950, 56 for those born between May 1950 and April 1952, 57 for those born after April 1952 with 25 contribution years, respectively. For other civil servants ERA is 60 with at least 10 contribution years.

**Italy** (see [Angelini et al., 2009](#))

**For both:** From 1965 to 1995, ERA is at any age possible for those with at least 35 contribution years (25 in the public sector). From 1996 to 1997 ERA is 52 in the private and public sector with at least 35 contribution years (or 36 contribution years independently of age), for self-employed, ERA is 56 with at least 35 contribution years. In 1998, ERA is 53 for the public sector, 54 for the private sector and 57 for self-employed. In 1999 ERA is 53 for the public sector, 55 for the private sector and 57 for self-employed. In 2000, ERA is 54 for the public sector, 55 for the private sector, 57 for self-employed. In 2001, ERA is 55 for the public sector, 56 for the private sector, 58 for self-employed. In 2002, ERA is 55 for the public sector, 57 for the private sector, 58 for self-employed. In 2003, ERA is 56 for the public sector, 57 for the private sector, 58 for self-employed. From 2004 onwards, ERA is 57 for both the private and public sector, 58 for self-employed. The requirements in terms of years of contributions remain the same in the period from 1996 onwards.

## **Netherlands**

*For both:* From 1975 to 1994, ERA is 60 for those with at least 10 contribution years. From 1995 onwards, ERA is 62 with at least 35 contribution years.

## **Slovenia** (see [ZPIZ, 2019](#), [Slovenien, 2013](#))

*For men:* ERA is 59 for a father of one child and 58 for a father of two or more children with at least 40 contribution years.

*For women:* ERA is 56 for a mother of five or more children, 57 for a mother of three to four children, 58 for a mother of 2 children and 59 for a mother of 1 child with 40 contribution years, respectively.

*For both:* From 2013 onwards ERA is 60.

## **Spain**

*For both:* Until 1982, ERA is 64. From 1983 to 1993, ERA is 60. From 1994 to 2001, ERA is 61, and from 2002 onwards, ERA is 61 for those with at least 30 contributions years.

## **Sweden**

*For both:* From 1963 to 1997, ERA is 60. From 1998 onwards, ERA is 61.

## **Switzerland**

*For men:* From 1997 to 2000, ERA is 64. From 2001 onwards, ERA is 63.

*For women:* From 2001 onwards, ERA is 62. Note, that before 2001, the official retirement age for women was at most 63. Thus, women are allowed to retire earlier than men at any point in time.

## **Gross earnings replacement rates**

As a raw but standardized measure of the generosity of a pension system, we use the gross earnings replacement rate for an average person as published by the OECD. Table [C1](#) reports average values per country over the years 2007 to 2019.<sup>1</sup> The data are available for every odd year and we impute them for the even years.

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<sup>1</sup>OECD (2007, 2009, 2011, 2013, 2015, 2017, 2019).

Table C1: Gross pension replacement rates

Country	Female	Male
Austria	78.60	78.60
Belgium	42.30	42.30
Croatia	38.00	38.00
Czech Republic	48.96	48.96
Denmark	77.02	77.02
England	30.37	30.37
Estonia	47.07	47.07
France	54.09	54.09
Germany	40.16	40.16
Greece	82.24	82.24
Hungary	70.65	71.27
Israel	57.41	65.59
Italy	61.07	70.49
Netherlands	83.52	83.52
Poland	40.19	51.96
Portugal	60.09	60.09
Slovenia	54.28	53.84
Spain	78.86	78.86
Sweden	59.13	59.13
Switzerland	53.41	53.33
USA	39.72	39.72

## Appendix D: Notes on the word recall test

**SHARE:** Individuals from all countries basically get the same list of words in their national language. These lists stayed unchanged from wave 1 to wave 2. Starting in wave 4, respondents are randomly assigned to one of four possible lists of words. In case of more than one respondent within a household, respondents are assigned to different lists or at least are not in the same room when passing the cognitive tests. Lists are assigned randomly but respondents may get the same list in consecutive waves.

**ELSA:** Respondents are randomly assigned to one out of four lists and are not given the same list as in the last interview. Within the same wave, respondents in the same household are given different lists.

**HRS:** Respondents are randomly assigned to one out of four lists where the initial list is randomly assigned. Later on, respondents are assigned to different lists in four consecutive waves. Respondents in the same household are not given the same lists, neither in the same nor in consecutive waves.

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