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Valentin Schiele

Hendrik Schmitz

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Valentin Schiele and Hendrik Schmitz[1]

Understanding Cognitive Decline in Older Ages: The Role Of Health Shocks

Abstract

Individual cognitive functioning declines over time. We seek to understand how adverse physical health shocks in older ages contribute to this development. By use of event-study methods and data from the USA, England and several countries in Continental Europe we find evidence that health shocks lead to an immediate and persistent decline in cognitive functioning. This robust finding holds in all regions representing different health insurance systems and seems to be independent of underlying individual demographic characteristics such as sex and age. We also ask whether variables that are susceptible to policy action can reduce the negative consequences of a health shock. Our results suggest that neither compulsory education nor retirement regulations moderate the effects, thus emphasizing the importance of maintaining good physical health in old age for cognitive functioning.

JEL-Code: J24, J14, I1, I12, J26

Keywords: Cognitive decline; health shocks; retirement; education; event study

November 2021

1   Introduction

Age-related cognitive decline is among the key reasons for the transition of healthy individuals into care dependent ones. As an example, its most drastic form, dementia, is responsible for around 50% of all nursing home stays in Germany (Berlin-Institut, 2011).[1] And this is becoming an increasing challenge for societies. The number of people suffering from dementia is projected to triple to 152 million in the developed countries between 2018 and 2050 (Patterson, 2018). Estimated dementia costs are expected to increase from $1 trillion today to an estimated $2 trillion by 2030 (Patterson, 2018). Yet, also milder forms of cognitive impairment are among the risk factors of becoming care dependent.

Apart from care dependence, cognitive abilities are in itself of growing importance in an ever more complicated world, even more so in combination with ongoing population aging. They are an important determinant of social participation. Complex decisions involve those on medical treatments, insurance coverage, or financial markets for example (Mazzonna and Peracchi, 2018). Studies find that lower cognitive abilities lead to lower investments in stocks and other risky assets (Christelis et al., 2010) and lower wealth (Banks et al., 2010; Smith et al., 2010). Cognitive performance is important for labor productivity and, more general, for wellbeing (Engelhardt et al., 2010; Maurer, 2011). Moreover, individuals are often not aware of their cognitive decline, which in particular leads to bad economic and financial decisions (Mazzonna and Peracchi, 2018).

While economists have devoted a great deal of effort to understand the process of human capital accumulation (e.g. determinants of education and the effects of education on productivity), not so much is known about its depreciation. Repeating the notion of McFadden (2008): in the past decades "...economists have given less attention to the process of human capital depreciation, and technologies for human capital maintenance. 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." It is our goal to contribute here and to learn about triggers of strong cognitive decline and what policy makers can or cannot do about that.

Cognitive decline is a gradual process but there is a small economic literature suggesting that it may be accelerated by adverse life events such as the loss of beloved persons, economic shocks, or bad economic circumstances at birth (see e.g. Lindeboom et al., 2002, van den Berg et al., 2011 and Doblhammer et al., 2013). In this paper, we seek to understand how a specific adverse life event, a health shock, affects cognitive decline among individuals aged 50 and older. Anecdotal evidence tells about the hale and hearty old

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[1]Strong cognitive impairments as predictors for dementia and care dependence on the individual level are also reported in (Celidoni et al., 2017; American Psychiatric Association, 2000).

person that accidentally falls and where the resulting hip fracture marks the beginning of a care episode that goes along with a strongly accelerated reduction in cognitive functioning, ending in a nursing home. In a second step of the paper, we study how potential policy measures can moderate effects of these shocks. These are labor force participation (affected by retirement regimes) and education. That is, we ask: can education or labor force participation increase the cognitive reserve such that adverse health effects have a less drastic effect on cognitive decline? In a previous study, van den Berg et al. (2010) assess how economic conditions early in life moderate the effect of life-events on cognitive functioning. They find that individuals born under adverse early-life conditions (that is, in recessions) suffer from a stronger cognitive decline after a stroke than those born under beneficial early-life conditions. No such moderation effects are found for several other conditions, however, such as peripheral arterial disease, heart disease, diabetes, cancer, respiratory disease, or arthritis. Surgery or illness of a partner are only harmful for individuals born under adverse economic conditions.

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). Measures of cognitive abilities are based on experimentally retrieved test scores for *recall* and *verbal fluency* while health shocks are experimentally and/or objectively measured by strong reductions in handgrip strength and onset of severe conditions such as heart attack, stroke, or hip fracture. We carry out event study estimations to see whether health shocks are anticipated (in terms of cognitive decline) and to learn about the longer run effects up to eight years after the shock. It turns out that there are no significant differences in cognitive decline trends before the shock for those who suffer from a health shock but that there is a strong immediate and persistent drop in cognitive abilities upon the health shock. Comparing the effect size to the general age-related decline in cognitive functioning, a health shock, on average, induces a similar cognitive decline as growing up to four years older. Thus, health shocks also have the potential to bring a long-term care episode for mental health reasons forward by several years.

We then analyze whether results differ by demographic characteristics or between Europe and the USA and whether retirement or education moderate the effect of a health shock. We use reduced form regressions and early retirement ages as set by the governments as well as compulsory schooling regulations to circumvent problems of potential endogeneity of retirement and education. It turns out that, while there is a considerable effect of health shocks on cognitive abilities in all specifications, this effect does not seem to be mitigated by retirement behavior or education. The effect sizes also do not vary much between Continental Europe, England and the US, representing regions with different institutional settings including different health insurance systems.

We mainly contribute to the literature in the following ways. Apart from presenting the first study on this topic that combines micro data from several countries and two continents with a large sample size that potentially also allows to identify smaller effects, we expand the previous study by van den Berg et al. (2010)[2] and focus on potential moderating variables that, arguably, are more susceptible to policy action than early-life circumstances. Moreover, in contrast to previous studies on effects of health shocks, we show transparent event-study results.

The results of our analysis suggest that physical health shocks have a significant impact on cognitive ageing and thus on human capital depreciation. While they are consistent with previous studies that concluded that higher education and active ageing can slow cognitive decline, they do not suggest that a higher cognitive reserve also helps to dampen cognitive decline after a health shock. Thus, they underline the importance of health prevention and point out that investing in physical health could pay off twice: through its direct return on physical health, but also through its indirect return in terms of cognitive functioning in old age.

The structure of the paper is as follows. We present the data and descriptive statistics in Section 2. In Section 3 we lay out the empirical approach and present baseline results, while we show the moderation analysis in Section 4. Section 5 concludes.

2 Data

2.1 Sample selection

Our main data sources are the Survey of Health Ageing, and Retirement (SHARE), the Health and Retirement Study (HRS) and the English Longitudinal Study of Aging (ELSA), three large representative micro data sets providing information on health and a great deal of other socioeconomic characteristics for individuals aged 50 and older. HRS was initiated in 1992. By now, 14 interview waves are available, each covering information about 20,000 Americans. Its sister study ELSA is fielded biennially since 2002 and is now containing data from 8 interview waves. SHARE started in 2004 as a cross-national survey. Since then data of 8 interview waves have been released covering information about 140,000 individuals living in 28 European countries plus Israel. HRS, ELSA and SHARE are highly harmonized and can be used for pooled analyses.[3]

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[2]While there is some literature on the direct effect of retirement/education on cognitive abilities (e.g. Bonsang et al., 2012; Rohwedder and Willis, 2010; Celidoni et al., 2017; Mazzonna and Peracchi, 2012, 2017; Coe et al., 2012; Schneeweis et al., 2014), we are aware of only one study that analyses the effect of a health shock.

[3]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).

We use all waves containing at least one measure of cognitive ability, one health shock measure and all covariates from the baseline specification, i.e. from SHARE waves 1, 2, 4, 5, 6, 7 and 8[4] from the HRS waves 4 to 13[5] and from ELSA waves 1 to 8.[6] In addition to the biennial data sets from HRS and ELSA, we include information from the RAND HRS Data file[7] and Harmonized ELSA. We exclude individuals from the sample who were interviewed only once, as our empirical strategy requires measurements from two consecutive waves. We drop individuals younger than 50 and older than 90. Our final sample consists of 421,656 observations from 124,167 individuals living in 20 countries.[8]

Given that we have a sample of older individuals with a focus on health shocks, non-random panel attrition is an obvious issue. We refer to Celidoni et al. (2017) who use the SHARE in an analysis of the effect of retirement on cognitive abilities. They both test for potential problems of panel attrition and, later, also account for it by including an inverse Mills ratio. Yet, they find that non-random panel attrition does not seem to be a relevant issue and, not surprising then, the selection model with the inverse Mills ratio does not yield different findings. We also find that our long-run results do not seem to be affected by potential attrition problems (see below).

2.2 Measures 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. Some of the measures are not available in all waves and, thus, not suitable for our analysis.[9]

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

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[4]See Börsch-Supan (2019a,b,c,d,e,f, 2020, 2021).

[5]Health and Retirement Study (2016a,b,c,d, 2014a,b, 2017a,b,c, 2019)

[6]See Banks (2019)

[7]The RAND HRS Data file 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.

[8]Countries covered in our sample: Austria, Germany, Sweden, Netherlands, Spain, Italy, France, Denmark, Greece, Switzerland, Belgium, Israel, Czech Republic, Poland, Luxembourg, Slovenia, Estonia, Croatia, USA, England.

[9]The description of measures of cognitive ability in this section closely follows Schmitz and Westphal (2021).

occasions is added up to yield the word recall test score. This score can range between 0 and 20. The average in our final sample is 9.616 with a standard deviation of 3.658. 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 the *verbal fluency test* respondents are asked to name as many animals as they can in one minute, where the number of animals they can tell is their test score. Here the lower limit is 0, but there is no upper limit (the maximum number in the sample is 100). The sample mean is 20.003, the standard deviation is 7.595. Verbal fluency is a measure of both fluid and crystallized intelligence as it is both important to know many animals (crystallized knowledge) and to remember them quickly (fluid intelligence). Obviously, both *recall* and *verbal fluency* only capture specific parts of the multidimensional concept "cognitive ability".

The general bivariate relationship between the two measures of cognitive abilities and age can be seen in Figure 1. Obviously, getting older goes along with a steady decline in cognitive abilities.

Figure 1: Measures of cognitive abilities by age
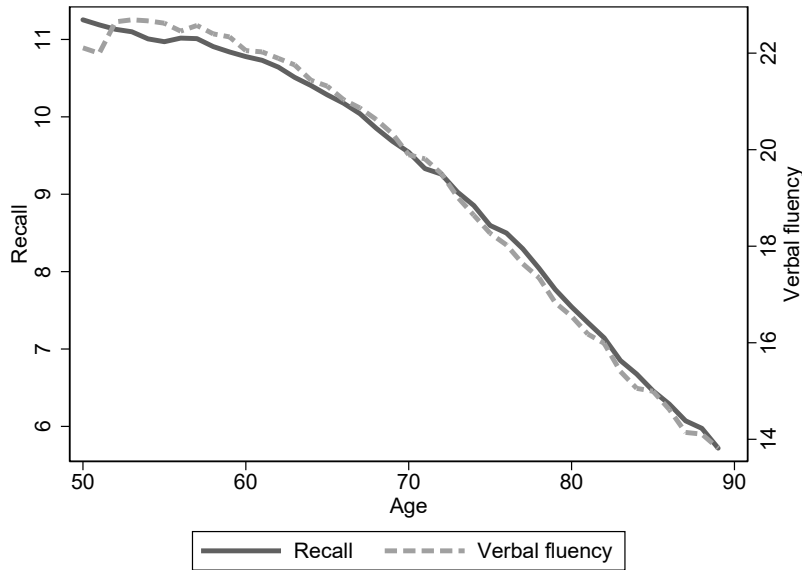
Notes: Based on data from SHARE, HRS and ELSA. The graph plots unconditional averages of the two scores by age in full years.

2.3 Measures of a health shock

In defining a health shock, we follow two approaches. Approach 1 is to use the onset of a serious illness between two waves. Survey respondents state whether they suffered

from any of the following illnesses since the previous interview: heart attack, stroke, cancer (any kind), hip fracture.[10] We consider all of these as health shocks. While these illnesses are highly relevant and objective health measures, they probably are not free from measurement error because they are self-stated by the respondents. Onsets of these conditions are serious negative life events. Moreover, the trained interviewers compare answers to those in previous waves. Nevertheless, in approach 2, we rely on changes in hand grip strength as another objective measure.

Grip strength in the SHARE, HRS, and ELSA is measured by the regular interviewers. The interviewers are equipped with so-called dynamometers, receive instruction on the usage of these small, non-invasive devices and are then able to assess the grip strength of the survey respondents.[11] The actual measurement procedure is as follows: The interviewer illustrates the use of the dynamometer first and then asks the respondents to press it twice with each hand as hard as they can, starting with the right hand and alternating afterwards. Test trials are not allowed.

There exist several alternatives in the medical literature on how to summarize this information (Roberts et al., 2011). Ambrasat and Schupp (2011) and Ambrasat et al. (2011) have analysed the case of the German Socio-Economic Panel (SOEP) rigorously. They suggest using the maximal value from all available measurements as a measure for the grip strength of an individual as, due to the absence of test trials, some individuals might not exert their full grip strength in the first measurements. We follow this suggestion. We are mostly interested in *relative* changes in individual grip strength (GS) over time. Specifically, we calculate $\Delta GS_t = (GS_t - GS_{t-1})/GS_{t-1}$, where t indicates the wave.

Grip strength contains more information than just the muscle strength of the hands. Based on broad empirical evidence from the medical literature, grip strength is known to be a valid indicator of the overall health status of an individual. Several medical studies document the association between a low level of grip strength and certain negative health outcomes, such as decreased overall muscular strength, the onset of chronic diseases, nutritional depletion, physical inactivity and mortality (see Rantanen et al. (2003), Bohannon (2008) or Ambrasat et al. (2011) and the references therein). The underlying mechanisms are not yet completely understood but it is suggested that "poor muscle strength could be a marker of disease severity, which in turn is associated with mortality" (Rantanen et al., 2003, p. 637).

Analogous evidence exists for extreme losses of grip strength over time (Rantanen et al., 1998; Ling et al., 2010; Xue Q et al., 2011; Stenholm et al., 2012). For example, Stenholm et al. (2012) study a sample of adults aged 30-70 at baseline and their grip strength

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[10]Respondents are asked about hip fractures in HRS and ELSA only if they are older than 65 years (60 years in ELSA). Since hip fractures are quite seldom according to the SHARE data below these ages, we assume that individuals below these age cut-offs did not suffer from hip fractures.

[11]The following text on grip strength is largely taken from Decker and Schmitz (2016).

changes over 22 years. The evidence suggests that the onset of chronic conditions such as coronary heart diseases, other cardiovascular diseases, diabetes or chronic bronchitis is correlated with accelerated grip strength decline over time. Similarly, Ling et al. (2010) compare 89-year-old Dutch males with different developments of grip strength over four years. They find that those with a decline in grip strength of 25% or more have a significantly higher mortality risk than those with a lower decrease or even an increase. It is our aim to incorporate this medical evidence when defining our health shock measure. A general cut-off point that identifies those with extreme losses in grip strength would be ideal but despite intense literature research we are unaware of such information. We therefore take the aforementioned value of a loss of 25% or more from Ling et al. (2010) for our main analysis and define our health shock measure as a loss of 25% or more in individual grip strength over two years, but check whether using alternative cut-off points affect the results. This definition of health shocks is also in accordance with Decker and Schmitz (2016) who use strong grip strength changes to analyze the effect of health shocks on risk aversion. While SHARE provides grip strength for each individual in every wave, this is only the case in every other wave in the HRS and in ELSA, except for a couple of individuals. Thus, when using the grip strength measure, we need to restrict our analysis to the SHARE countries.

Table 1: Descriptive statistics on health shocks and socioeconomic controls

Variable	Mean	Std. dev.	Min	Max
Heart attack	0.017	0.129	0.000	1.000
Stroke	0.015	0.122	0.000	1.000
Cancer	0.026	0.158	0.000	1.000
Hip fracture	0.007	0.082	0.000	1.000
New condition	0.061	0.240	0.000	1.000
Grip strength shock	0.068	0.252	0.000	1.000
Male	0.427	0.495	0.000	1.000
Age	67.510	9.305	50.000	89.000

Notes: Descriptive statistics on condition based on 384,692 observations from SHARE, HRS and ELSA, descriptive statistics for Grip strength shock based on 160,922 observations (SHARE countries only) and descriptive statistics for age and gender based on 421,656 observations.

Table 1 reports descriptive statistics on the health shock measures and socioeconomic controls. A health shock is a rare event, in particular the single conditions. In the main regression analysis below, we do not use the conditions separately but use the indicator "(Any) New condition", taking on the value 1 if the individual exhibits at least one of the four conditions between two waves. In some additional regressions we show results for the four conditions separately. Figure 2 plots the incidence of health shocks by age in the

sample. The average unconditional probability of a health shock monotonously increases from about 5 per cent at the age of 50 to about 10 per cent at the age of 90.

Figure 2: Health shocks by age

Notes: Based on data from SHARE, HRS and ELSA. The graph plots unconditional relative frequencies of the two health shocks by age in full years. The grip strength measure is only available in the SHARE data.

In Table 2 we take a look at the dynamics of health shocks. We show recovery rates by time distance to the health shock for those who experienced one. The results indicate that among those experiencing a health shock, defined as a 25% decline in grip strength between two waves $t - 1$ and t, around 40% reached their initial grip strength by $t + 1$, that is, two years later (Δ grip strength $\geq$ 25%). Another 20% seem to have recovered at least partly (25%> Δ grip strength $\geq$ 10%) with respect to their physical health in the same period. Only around 40% experienced a stagnation (10%> Δ grip strength $\geq$ -10%) or worsening (-10%> Δ grip strength). Even though the proportion of those who (partly) recover decreases somewhat the more time has passed since the health shock, the overall impression that a large share recovers physically from the shock does not change when looking at time lags larger than two years from the onset of the shock.

3 Effects of Health Shocks on Cognitive Abilities

3.1 Baseline results

Before looking at potential moderators, we are interested in the baseline effect of a (physical) health shock on cognitive abilities. Obviously, individuals who experience a health shock at some point in time might differ in many aspects including their level or development of cognitive abilities from individuals who do not suffer from such a shock. One way

Table 2: Recovery of a health shock

	$t+1$		$t+2$		$t+3$		$t+4$	
	N	$\%$	N	$\%$	N	$\%$	N	$\%$
Δ *grip strength since t*								
$\geq 25\%$	1142	38.74	504	0.34	180	0.34	117	0.27
+10 to +25%	626	21.23	309	0.21	109	0.21	72	0.17
-10 to +10%	766	25.98	407	0.28	125	0.24	125	0.29
<-10%	414	14.04	260	0.18	108	0.21	122	0.28
Total	2948		1480		522		436	

Notes: Based on SHARE data. The sample for this table only includes individuals who experienced a health shock between two waves. These individuals are followed over time. $t+1$ indicates, for instance, one wave after the health shock (which occurred between $t-1$ and t). Percentage shares in each columns add up to 100.

to deal with this potential endogeneity of health shocks, has been to condition on a large set of controls including pre-treatment outcomes (e.g. van den Berg et al., 2010). We estimate the effects of health shocks on cognitive abilities in a event study design instead.[12] This approach has the advantage that it does not only allow for fixed effects, but can also be used to assess whether individuals with health shocks follow different trajectories with respect to their cognitive abilities already before the shock. Furthermore, compared to the classical Diff-in-Diff model, the event study design does not require the assumption of static effects of a health shocks but visualizes potential dynamics of the effects directly.[13]

As a first step we define the event time r which is survey wave relative to the wave the health shock occurred

$$r_{it} = t - h_i$$

where t is the wave, i is the individual and h_i denotes the wave, a health shock is observed first. That is $r = -1$ is the last wave before the health shock, while $r = 0$ is the first wave after the health shock. We do not observe the exact date of the health shock. Due to the biannual nature of the data, on average, the health shock should have occurred one year before $r = 0$. We run the following fixed effects regression with relative time indicators:

$$Y_{it} = \sum_{r=-3}^{r=-2} \mu_r + \sum_{0}^{3} \mu_r + \mu_a + \mu_b + \alpha_i + \lambda_t + \tau_{it} + \varepsilon_{it} \tag{1}$$

[12]In the supplementary materials we report results of specifications in the spirit of (van den Berg et al., 2010).

[13]This flexible already account for most of the problems outlined in, e.g., de Chaisemartin and D'Haultfoeuille (2020) and Goodman-Bacon (2018) of "Diif-in-diff with staggered entry". However, we also use the approach of Sun and Abraham (2020) as a robustness check below.

Here, Y_{it} is a measure of cognitive abilities, either recall or verbal fluency, and μ_r is the coefficient of the indicator variable of event time r. We leave observations with event times smaller than -3 or larger than 3 in the sample but account for this by μ_a (the coefficient of the indicator for $r < -3$) and μ_b (the coefficient of the indicator for $r > 3$. Furthermore, α_i denotes individual, λ_t time and τ_{it} a full set of age fixed effects and ε_{it} is the error term. In this setting, the dynamic effects of a health shock on cognitive abilities given by μ_r (for $r \geq 0$) are identified solely by different timings of the health shock.

Figure 3 shows the results for both health shocks and both outcome measures. The upper two use recall, the lower two verbal fluency as outcomes. All figures are remarkably similar and show a significant and immediate decline in cognitive ability after a physical health shock. This decline in cognitive ability amounts to approximately 0.25 fewer words in the memory test and approximately 0.6-1 fewer animals/fruits, etc. named in the verbal fluency test, corresponding to a reduction in test scores of around 2.5-5 percent, as measured against the respective mean, or around 10 per cent of a standard deviation.

Figure 3: Impact of health shocks on cognitive abilities
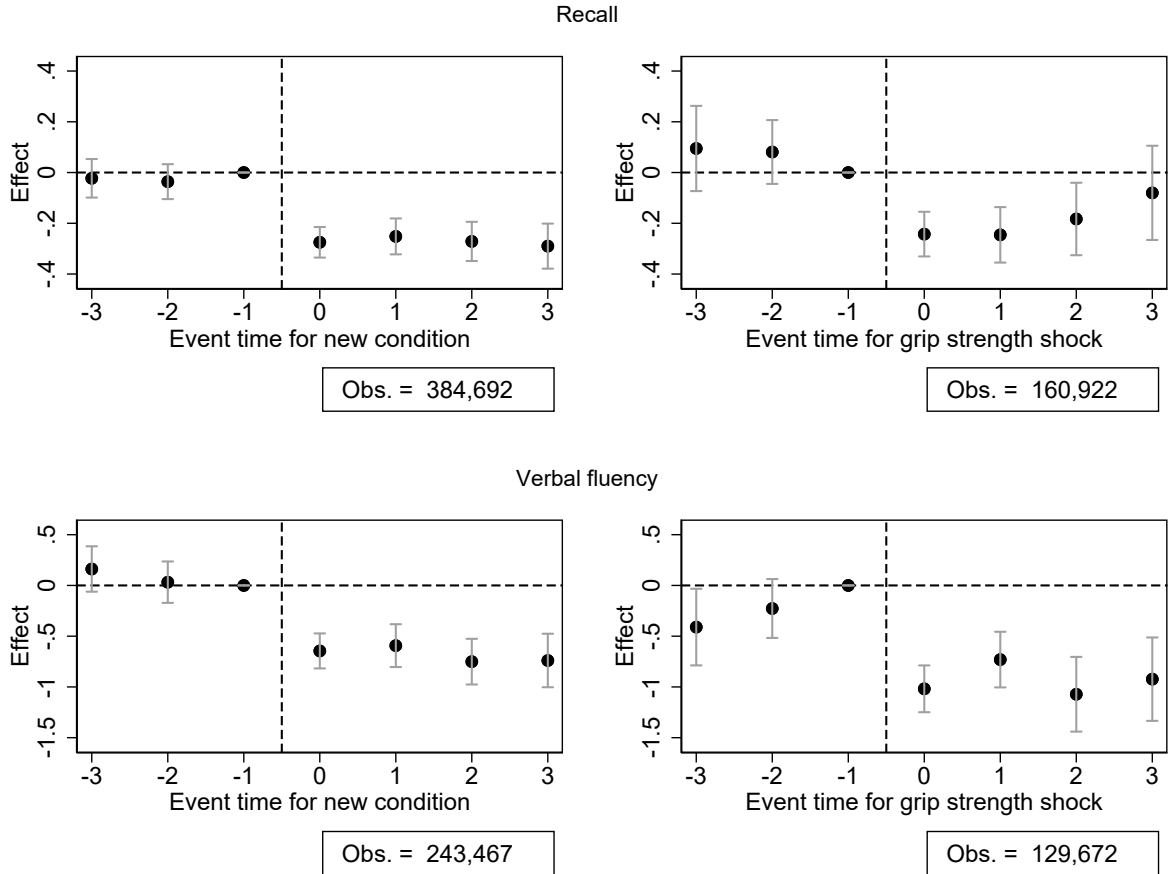
Notes: Coefficients μ_r from estimations of Equation 1 based on data from SHARE (Grip strength shock) and SHARE, HRS and ELSA (New condition), respectively. μ_{-1} is restricted to zero. 95% confidence intervals reported. Standard errors clustered on individual level.

All specifications indicate that the effect of a health shock is also persistent and thus that people do not recover, at least in the mid-run, from its adverse consequences for cognition.

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While the estimates for the *any condition* health shock measure and both outcomes are precisely estimated and unambiguously point to such persistent effects even more than 6 years after the shock, the estimates for the *grip strength* measure are somewhat less conclusive. The point estimate for grip strength and the mid-run effect ($r = 3$) on recall is, in absolute terms, smaller than the estimates for the short-run. At the same time, the confidence bands are large[14] and still include the estimates for the short-run effects. Thus, we do not interpret this single result as evidence against the overall picture, which is a persistent effect of health shocks on cognitive abilities.

The results also contradict the concern that individuals who experience a health shock are on a path of declining cognitive abilities already before the health shock. When looking at the estimates for the pre-treatment periods, no structural pre-health-shock pattern can be identified, neither in terms of statistical nor economic significance.

To get a better impression of effect sizes, Figure 4 compares the short- and medium-run effects of a health shock with the general age decline in cognitive abilities. The figure compares two individuals with average characteristics in the sample, where only age and the event of a health shock are varied. Technically, these are predicted values from the baseline regression with all control variables except for age and health shock set to average values. We then add a hypothetical health shock at the age of 68 by using the coefficients from the event study estimates. We find that a health shock compares to a general age decline over around 1 to 4 years, depending on the health shock and outcome measure used. For instance, the average individual who experiences a health shock (new condition, left panel) at the age of 68 has the cognitive abilities of an average individual at the age of 72-73 without a health shock when he turns 70. To the amount that strong cognitive declines lead to care dependence, a health shock brings a care episode forward by up to 4 years.[15]

To test whether there are differences by region, we repeat the analysis separately for Continental Europe, the USA and the UK. We do this for the health shock measure "New condition" and the outcome "recall" as the figure for grip strength already reports findings for Continental Europe only and verbal fluency is available only in a few recent waves in the HRS. Figure 5 reports the results. There are only slight differences across regions. In all regions we observe an immediate and persistent drop in cognitive abilities after the health shock. This drop seems to be a little larger in the USA then in Europe, however, the difference is not statistically significant for most event time indicators. This might shed light on broader effects of institutional differences. For instance, most European countries have universal health insurance systems that might buffer the effects of health

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[14]This is not surprising as the sample size is clearly reduced for the *grip strength* measure due to the rather short panel length in SHARE.

[15]See Figure 6 below that shows that the average effect of a health shock between 60 and 70, does not differ strongly from a health shock between 70 and 90. Thus, it does not make a difference at what age we start our hypothetical health shock in Figure 4.

Figure 4: Stylized medium-run effect of a health shock

Recall

Verbal fluency
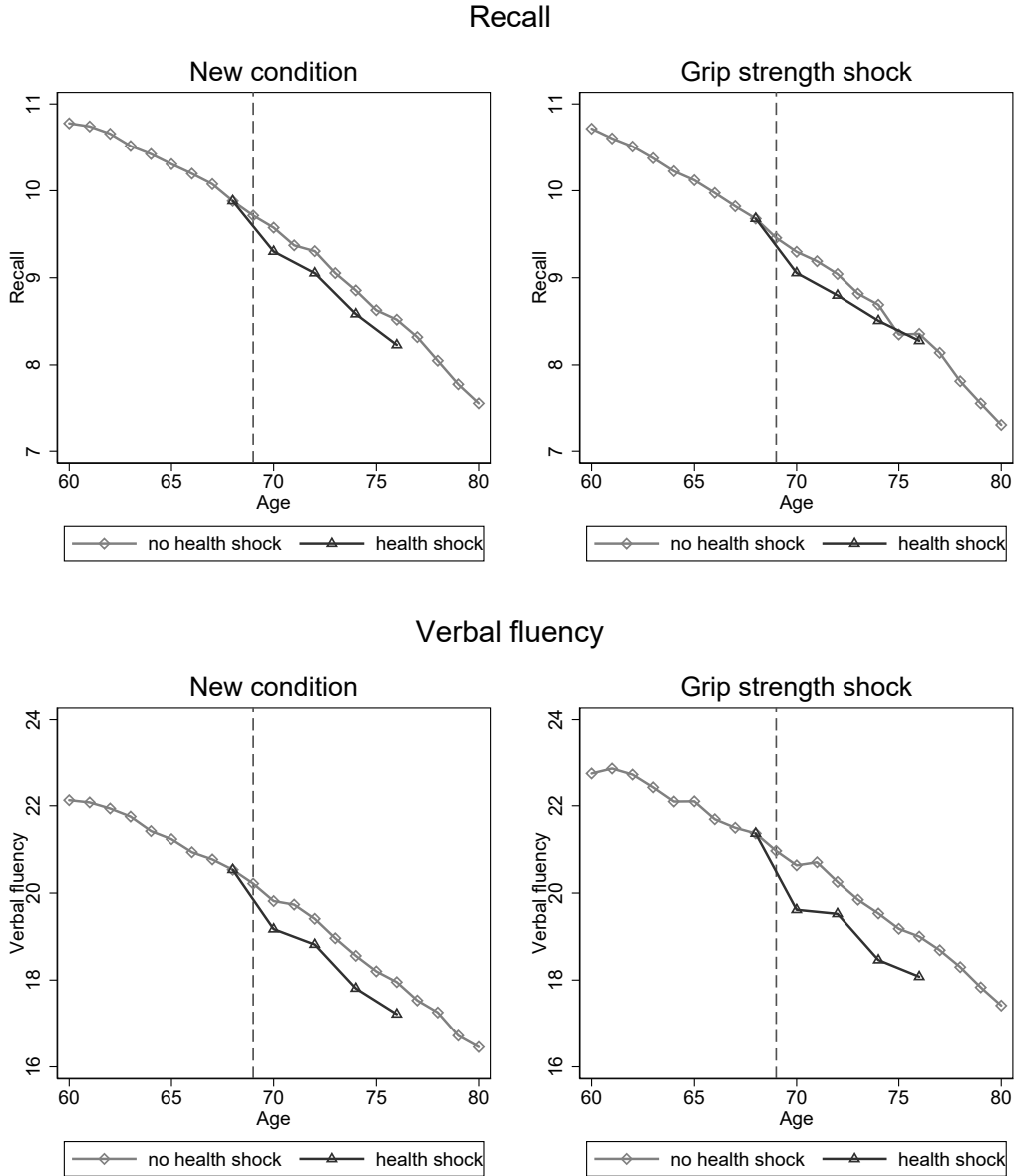
Notes: Based on SHARE, HRS and ELSA data. The graph plots the predicted cognitive decline for persons with average characteristics.

shocks while health insurance is more restricted in the US. While this obviously cannot be interpreted as evidence that health insurance systems do not matter at all, it at least indicates that existing institutional differences between the USA, UK and other European countries are no decisive factors.

We so far yield three conclusions from this exercise. First, the clear pattern of no pre-treatment trends combined with an immediate drop in cognitive abilities after the health shock makes us confident that potential endogeneity might not be the driver of the observed relationship. Of course, this cannot be proven as we do not clearly observe what happens between $r = -1$ and $r = 0$. Nevertheless, it does not seem to be the case that individuals are on a general path of declining cognitive abilities before experiencing a health

Figure 5: Impact of health shocks on cognitive abilities (recall) - by region
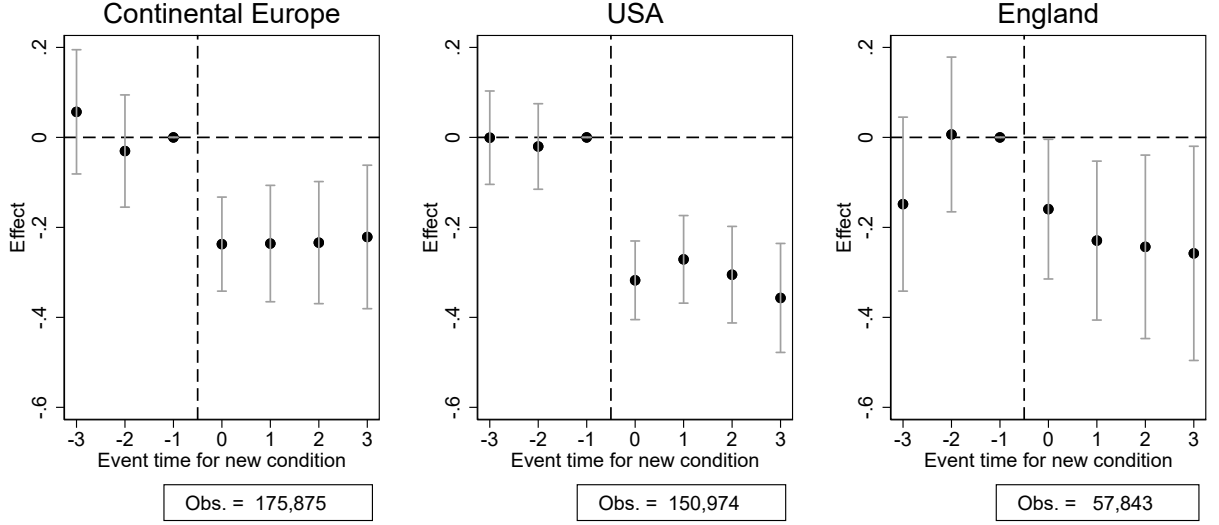
shock. Second, the effects are still visible in the longer run, several years after the health shock. Specifically, physical health shocks seem to induce a persistent downward shift in cognitive abilities immediately after the shock. That is, the effects of health shocks seem to be rather static than dynamic. Third, there is no heterogeneity across regions (USA, England, Continental Europe).

3.2 Further results

Before proceeding with some robustness checks and the moderation analysis, we check for more general differences in the effect of health shocks on cognitive abilities. To overcome the above-mentioned problems with reduced power, which are partly due to the specific data structure (grip strength only available in SHARE, verbal fluency only available in some waves in ELSA and HRS) and which come into effect especially in subgroup analyses, we collapse the event-time indicators into a single (post)treatment indicator in the following, i.e., we run classical difference-in-differences regressions of the form:

$$Y_{it} = \alpha_i + \lambda_t + \tau_{it} + \beta post_{it} + \varepsilon_{it} \tag{2}$$

where $post_{it}$ is 1 for individuals with a health shock in periods after the health shock, i.e. for $r \geq 0$, and 0 otherwise. This approach does not only reduce noise in the estimates, but also allows for a more comprehensive presentation of the results of heterogeneity analysis and robustness checks. Note that using a classical difference-in-differences approach in-

stead of the more flexible event study, should not alter the validity of the analysis, given that there is little evidence for dynamic effects in the pooled sample.

Figure 6 shows the results when estimating Equation 2 by subgroups defined by region, gender, age groups and quartiles of cognitive abilities. To start with, we do not find evidence for a gender difference, irrespective of the health shock and outcome measure used.

Figure 6: Effect heterogeneity
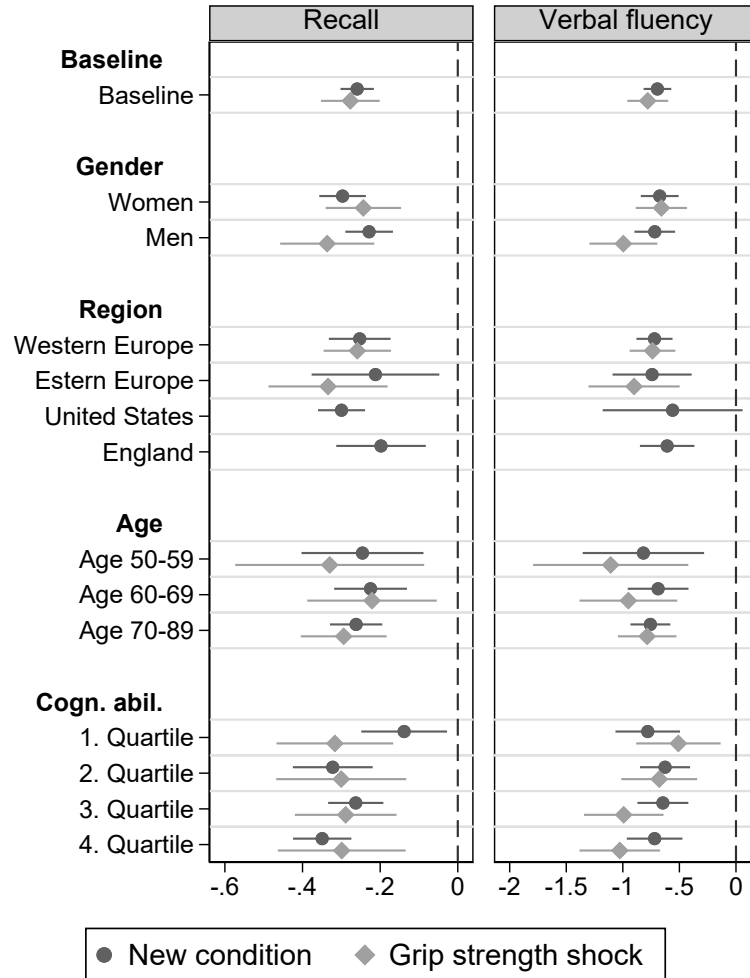
Notes: The figure shows the coefficient β along with 95% confidence intervals from estimations of Equation 2 based on data from SHARE, HRS and ELSA. Each estimate comes from a separate regression.

We do not find an age gradient or gradient in initial cognitive abilities (cognitive abilities at first appearance in the sample) either. If any, the last segment of Figure 6 suggests, that the loss in cognitive abilities in the aftermath of a health shock is larger for individuals with a higher cognitive reserve. Yet, as this result seems to hold only for some health shock/outcome measures, it might be that the role of initial cognitive abilities depends on the type of health shock.

3.3 Robustness checks

Recent literature on the estimation of dynamic treatment effects shows that the event-time coefficients derived from two-way fixed effects estimation in settings comparable to ours, i.e. settings with variation in treatment timing and thus potentially effect heterogeneity, can be contaminated by the effects of other periods (Sun and Abraham, 2020). This might cause misleading interpretations not only of the dynamics of the effects, but also with respect to the identifying assumptions. To deal with this problem Sun and Abraham (2020) propose an alternative weighting estimator which is robust also in the presence of effect heterogeneity.

In order to address this concern, Figure 7 shows the estimated effects of a health shock corresponding to μ_r in Equation 1 when we apply their estimator to our study using the Stata package *Eventstudyweights* (Sun, 2020). The estimated effects for "New condition" and recall almost exactly match our baseline results. The results for the alternative outcome "Verbal fluency" are smaller in size than the corresponding baseline results and estimated less precisely, but nevertheless point to adverse persistent effects of health shocks on cognition. Note, that due to the specific data structure in SHARE (wave 3 and wave 7 were no regular interviews but focused on initial living conditions) and the necessity to observe information on grip strength in two consecutive waves it was not feasible to apply the estimator to study the effects of a grip strength shock.[16] Taken together, the results do not provide evidence against our interpretation of the baseline results.

Figure 7: Event study results using the Sun and Abraham (2020) estimator
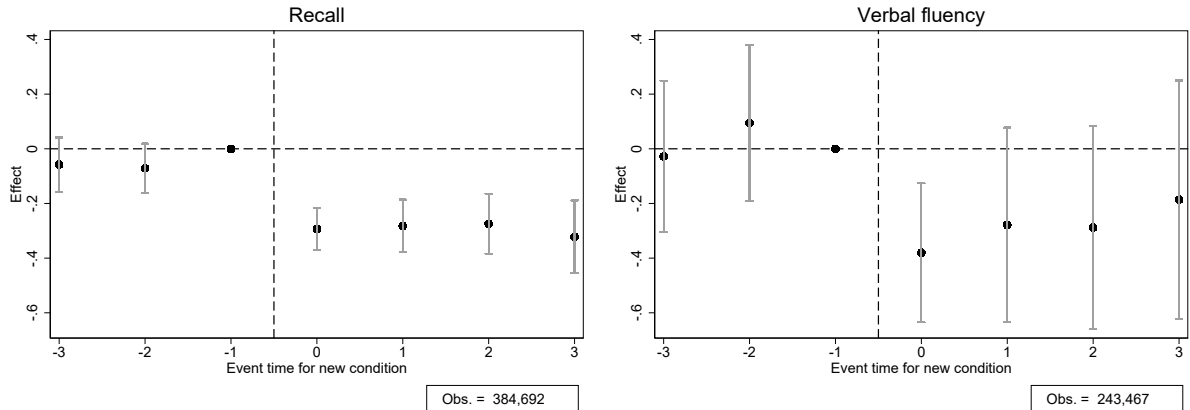
Notes: Coefficients corresponding to μ_r in Equation 1 based on data from SHARE, HRS and ELSA. μ_{-1} is restricted to zero. 95% confidence intervals reported. Standard errors clustered on individual level.

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[16]The lack of information on grip strength in wave 3 and (partially) wave 7, combined with the requirement to observe grip strength in at least two consecutive waves to define a health shock, means that the event time coefficients cannot be estimated for all cohorts that have a weight greater than zero because of perfect collinearity.

A second concern relates to the definitions of health shocks in our analysis. So far, we have combined different health conditions into one health shock measure and set the threshold for grip force shock rather arbitrarily. In what follows, we examine whether the exact definition of health shocks is crucial for our results and whether panel attrition might bias our results. As each condition is a rather rare event we again pool over all post- (and pre-) treatment periods in the following and estimate a single effect of health shocks according to Equation 2.

Figure 8 reports the result of the difference-in-differences estimation for the entire sample and both health shock and outcome measures in the first segment (first line). These estimates mirror the baseline event study estimates presented in Figure 3. In the following segments we address potential concerns with respect to the definition of health shocks and panel attrition.

Figure 8: Alternative health shock definitions
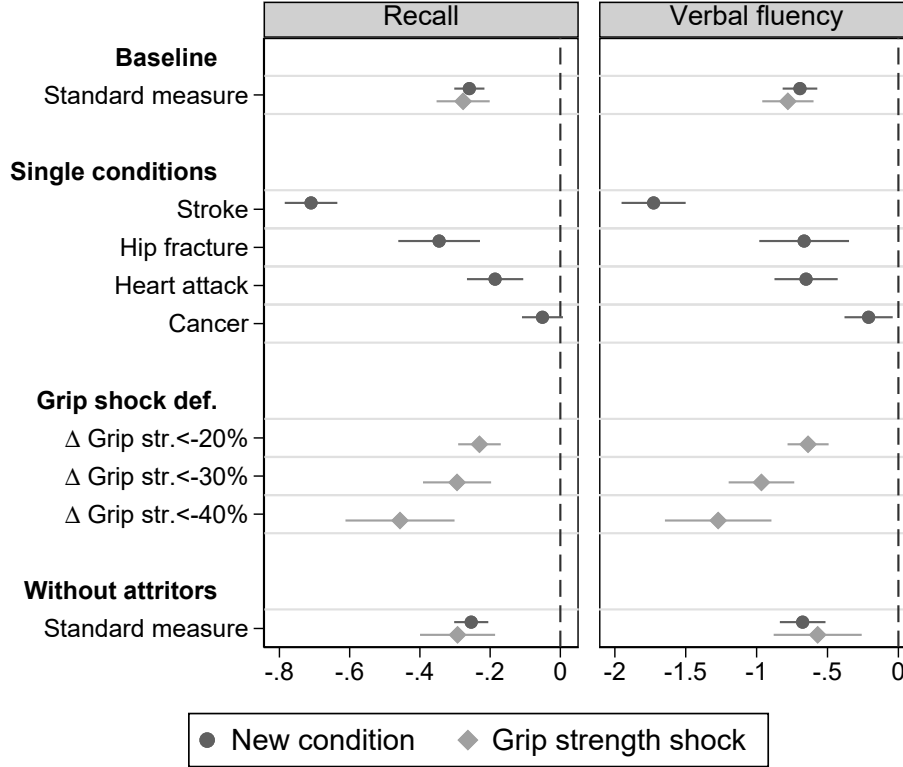
First, we report the effects of the onset of single conditions in the second segment of Figure 8. As mentioned earlier, the onset of one of these conditions, either a heart attack, a stroke, cancer or a hip fracture, is a rather rare event. Thus, we have used an aggregate measure (any of these four conditions new) in the event study, to make sure that we have enough power to detect effects of health shocks, especially when estimating medium term effects. Now, we try to see how each single condition contributes to the overall effect. One

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potential concern, for example, might be that the overall effect is solely driven by the onset of strokes, which, as might be argued, have a rather mechanic adverse effect on cognitive abilities. Indeed, the largest estimates can be found for strokes. The estimated effects for hip fractures are somewhat smaller but still relatively large and clearly significant. Also the estimates for heart attacks are in the range of the baseline estimates. Only for new cancer diagnosis we find no clear evidence for adverse effects on cognitive decline. An explanation for the latter finding might be that cancer is a disease progressing rather gradually while other conditions have a clear onset and require immediate treatment. While cancer is doubtlessly a serious health issue, it does not necessarily imply longer hospital stays at the onset of the condition but rather at the end. Thus, one might not expect to see large effects of newly detected cancer on cognitive abilities. Irrespective of the exact reason for the finding of zero effects of cancer, the results presented in here indicate that health shocks requiring immediate medical treatment affect cognitive abilities, even if the shock does not directly affect the functionality of the brain.

Another potential problem concerns the threshold that defines a health shock based on grip strength loss. Since there is no natural cutoff point, we followed the literature (see section 2.3) and defined a health shock as a reduction in grip strength of at least 25 percent. Segment 3 of the figure shows that the exact choice of cutoff point is not critical to the overall picture. Although we observe an increase in effect sizes as we decrease the cutoff point, it seems to make little difference whether we use -20 or -30 percent as the cutoff point instead of -25. If a cutoff of -40 percent is used, the effect size increases more notably, but the precision of the estimate also decreases.

Finally, one might worry that panel attrition affects the results. Although related previous research using SHARE data did not find evidence for influential non-random panel attrition (see Section 2), one can argue that this result cannot be transferred easily to our setting, because sample composition, variable definitions as well as the empirical approach differ. To address this concern, we repeat the analysis only looking at individuals who are at least 4 waves in the sample. Thus, we exclude those who drop out of the panel, possibly after a health shock. The results shown in the last row of the figure are very similar to the results for the full sample and indicate that attrition has no relevant effect on the estimates.

4   Moderation Analysis

The results presented so far provide evidence for adverse effects of health shocks on cognitive abilities in older ages. They imply that health shocks accelerate cognitive decline and thus might increase the risk of early care dependence. This raises the question whether there are factors that are susceptible to policy action and are able to improve health

capital or increase the cognitive reserve and, thus, make people less prone to cognitive impairments following health shocks.

To shed light on this question we interact the event time dummies in our final specification with potential moderators (mod_{it}) which are either education or retirement:

$$
\begin{aligned}
Y_{it} &= \sum_{r=-3}^{r=-2} \mu_r + \sum_{0}^{3} \mu_r + \mu_a + \mu_b \\
&+ \sum_{r=-3}^{r=-2} \mu_r \times mod_{it} + \sum_{0}^{3} \mu_r \times mod_{it} + \mu_a \times mod_{it} + \mu_b \times mod_{it} \\
&+ mod_{it} + \alpha_i + \lambda_t + \tau_{it} + \varepsilon_{it}
\end{aligned}
\tag{3}
$$

Both retirement and education have been shown to affect cognitive abilities (see e.g. Bonsang et al., 2012; Rohwedder and Willis, 2010, for retirement and Kamhöfer et al., 2019; Schneeweis et al., 2014, for education). According to the "use-it-or-lose-it hypothesis" cognitive abilities decline faster if individuals do not use their cognitive capacities. We hypothesize that retirement as well as education might not only directly affect cognitive decline but that people who are cognitively more stimulated, are less prone to cognitive impairments following health shocks.

Equation (4) shows that a straightforward approach to test this presumption within an event study design is to regress cognitive abilities on the event time dummies, the moderator and all interactions of both along with controls. This approach obviously raises endogeneity concerns of both the retirement decision as well as educational attainment. We thus focus on *retirement eligibility* instead of actual retirement status and on *compulsory schooling* instead of educational attainment, i.e. we present the reduced form relationships with variables that may be considered instrumental variables. Specifically, *mod* in Equation (4) is either a dummy variable for being above the (early) retirement age set by the retirement system or a dummy for being affected by a compulsory schooling reform that increased years of compulsory schooling. We follow the related literature and include – aside from individual, year and age fixed effects – linear country-specific trends to account for correlated changes in cognitive abilities and retirement regulations/compulsory schooling reforms across age groups/birth cohorts.

Obviously, the resulting parameter estimates then represent intention to treat (ITT) parameters rather than structural estimates. By focusing on the reduced form, we circumvent the problem that the interpretation of estimates in an IV model with multiple endogenous variables is not completely clear in the case of heterogeneous effects. Furthermore, the reduced form estimates are probably sufficient to assess whether there is an

effect also in the structural model. In this sense, Angrist and Krueger (1991) note that if one cannot see an effect in the reduced form, then, most likely, it does not exist.

To estimate the moderating effects of retirement, we make use of arguably exogenous variation in early retirement regulations. This is a frequently employed instrument in the literature, see, e.g. Mazzonna and Peracchi (2012), Celidoni et al. (2017), Mazzonna and Peracchi (2017).We follow Schmitz and Westphal (2021) and focus on *early retirement* ages only instead of also using *official retirement* ages. The reason is that the jump in retirement probability at the early retirement threshold is much larger, leading to a sufficiently strong instrument only for early retirement. Early retirement appears to be the more important institutional feature as it allows individuals to retire (at the cost of penalties on retirement benefits) while official retirement age only is the age threshold that abandons penalties on retirement benefits. Our indicator for early retirement age takes on the value one if the individual has reached the early retirement age and zero otherwise. As can be seen in the first column of Table 3 (ERA), there is considerable within- and across-country variation in early retirement ages. Within variation is due to reforms in the observation period.

When looking at education as a second potential moderator of health shocks, we make use of a binary variable that captures whether an individual went to school before or after a reform that raised years of compulsory education went into effect. Note that although we can estimate the parameter of the interaction between the reform and the event time dummies, we can not estimate the direct effect of the reforms on cognitive abilities, as the value of the reform dummy is fixed for each individual and we include individual fixed effects in all specifications. The last column of Table 3 (*compulsory schooling*) gives an overview of the changes in years of compulsory education for each reform and states the first birth cohort (*pivotal cohort*) affected by the reform.

For our approach to yield meaningful estimates, retirement age regulations have to actually affect the retirement decisions and compulsory schooling reforms must have an effect on educational attainment. Panel a) of Figure 9 shows that there is a clear jump in retirement rates as soon as people reach the early retirement age. With respect to education, panel b) of Figure 9 shows an increase in average years of education[17] for the first cohorts affected by the compulsory schooling reform. When estimating the first stage regressions for retirement and education, the resulting estimates suggests that crossing the early retirement age increases the likelihood to retire by around 8.55 percentage points ($\widehat{\beta} = 0.0855$ with $s.e. = 0.0055$) and that the average compulsory schooling reform increased years of education by 0.48 years ($\widehat{\beta} = 0.4757$ with $s.e. = 0.0866$) on average.

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[17]We rely on self reported years of full time education here, which is available only for SHARE countries. For individuals with less years of education than years of compulsory education, we set years of education to years of compulsory education.
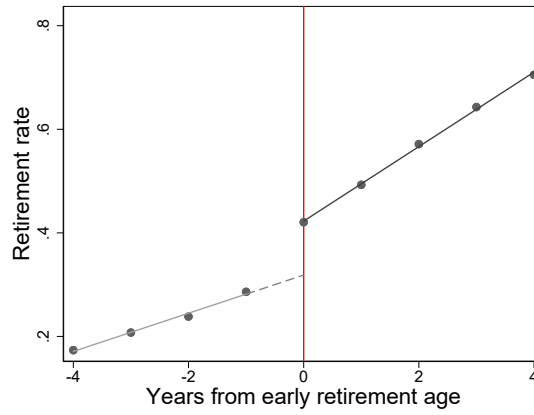
Table 3: Retirement ages and compulsory schooling

	ERA		Compulsory schooling	
	men	women	change in years	pivotal cohort
Austria	60-65	55-60	8-9	1951
Belgium	58-60	58-60		
Flanders			8-9	1939
Czech Republic	-	-	8-9	1947
Denmark	60	60	4-7	1947
England	65-66	60-66	10-11	1957
France	60	60	8-10	1953
Germany	63	62-63		
BW			8-9	1953
BY			8-9	1955
HB			8-9	1943
HH			8-9	1934
HE			8-9	1953
NI			8-9	1947
NRW			8-9	1953
RLP			8-9	1953
SL			8-9	1949
SH			8-9	1941
Greece	-	-	6-9	1963
Italy	57-58	57-58	5-8	1949
Netherlands	62	62	7-9	1936
Spain	61	61	6-8	1957
Sweden	61	61	7-9	1950
Switzerland	63	62		
USA	62	62		

Notes: The table shows for each country and gender the Early Retirement Age (ERA) and for each compulsory schooling reform the change in years of compulsory schooling as well as the first cohort affected by the reform. As ERA depends on e.g. the birth cohort in some countries, we provide the ERA range in our sample for these countries. Information about the compulsory schooling reforms in most countries is taken from Brunello et al. (2016). Additional information about the reforms in Spain, Greece and England is taken from Brunello et al. (2013). Detailed information on retirement rules for each country are in the supplementary materials.

With respect to exogeneity of the instruments, we borrow from the related literature and argue that they are based on legislated rules and are unrelated to individual characteristics (except for gender, country, age, cohort and year of interview which are flexibly controlled for in all regressions). A potential concern, however, is that retirement or education affects the likelihood of having a health shock. Table 4 shows that auxiliary regressions[18] of the

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[18]All regressions include age and year fixed effects. The regressions for retirement additionally include individual fixed effects and country-specific linear age trends, the regression for education country fixed effects, a gender dummy and country-specific linear cohort trends, instead.

Figure 9: First stage relationships

(a) Retirement rates by distance to early retirement ages
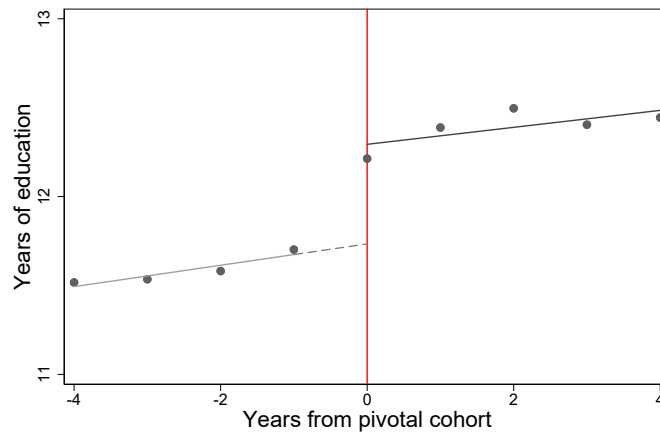
(b) Education by distance to compulsory schooling reforms

Notes: Panel a) based on data from SHARE, HRS and ELSA, panel b) based on SHARE data. Panel a) plots retirement rates by years from legislated early retirement ages. Panel b) shows average years of education by distance to the compulsory schooling reform.

health shock variables on the instruments do not provide much evidence for this concern. All estimates are rather small and far away from being statistically significantly different from zero.

Figures 10 and 11 present the results of the moderation analysis. Both graphs show the estimated effects of a health shock on cognitive abilities differentiated by moderator/instrument status. In both graphs the grey markers show the effects when the instrument is switched on (i.e. for individuals above the early retirement age or individuals who were affected by a compulsory schooling reform that increased minimum years of schooling) and the black markers show the corresponding effects when the instrument is switched off.

To start with, Figure 10 shows the dynamic effects of health shocks on recall (panel (a)) and verbal fluency (panel (b)) by retirement eligibility. Some of the estimates, mainly

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Table 4: The effects of retirement and education on health shocks

	New condition		Grip strength shock	
Above early retirement age (in $t-1$)	0.004	(0.003)	0.005	(0.006)
Observations	306,108		126,410	
Affected by comp. schooling reform	-0.000	(0.003)	0.002	(0.003)
Observations	114,532		100,694	

Notes: The results for *New condition* are based on data from SHARE, ELSA and HRS (only for retirement), the results for *Grip strength shock* are based on SHARE data. Standard errors (clustered at individual level) in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; Controls include: year and age fixed effects as well as country-specific linear age/cohort trends; Additional controls in retirement equation: individual fixed effects; Additional controls in education equation: gender and cohort fixed effects.

those for the grip strength measure and the long run effects but also those for individuals below the early retirement age, are somewaht noisy and thus have to be interpreted with caution. Nevertheless, the overall impression is rather robust: Health shocks negatively and persistently affect cognitive abilities among both individuals who are eligible due to their age as well as individuals (of the same age) who are not yet allowed to retire. Thus, other than expected, retirement eligibility neither seems to amplify nor to dampen the adverse consequences of health shocks.

Figure 11 repeats the exercise for education and interacts the event time with an indicator for being affected by a compulsory schooling reform which raised years of compulsory education. It suggests that education likewise does not seem to moderate the effects of a health shock on cognitive abilities.

5 Conclusions

We analyze the short- and longer-run effects of a health shock on cognitive decline in older individuals from Continental Europe, UK, and the US. Health shocks are measured by strong declines in grip strength and the onset of health conditions, while cognitive abilities are determined experimentally by the *word recall* and *verbal fluency*. We also ask whether the potential effect is moderated by variables that can be comparably easily changed by policy makers such as the retirement or education system.

In an event study, we find robust evidence that health shocks negatively affect cognitive functioning. The effects are persistent over a longer time even though most individuals have recovered from their health shock after some years. Comparing the effect size to the general age-related decline in cognitive functioning, a health shock, on average, induces a similar cognitive decline as growing up to four years older. Thus, physical health shocks

Figure 10: Moderation analysis – Retirement

(a) Recall

(b) Verbal fluency

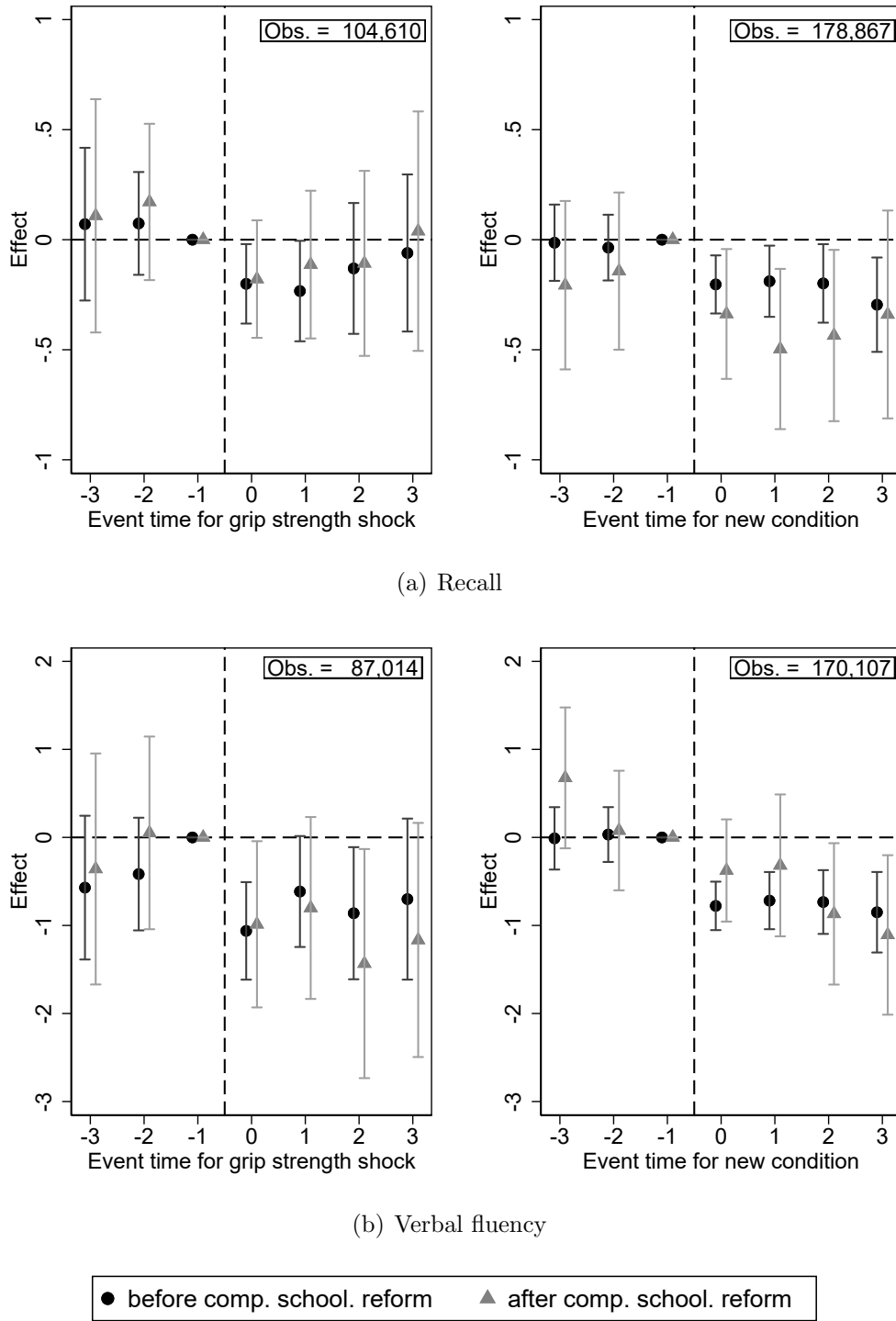● below early retirement age   ▲ above early retirement age
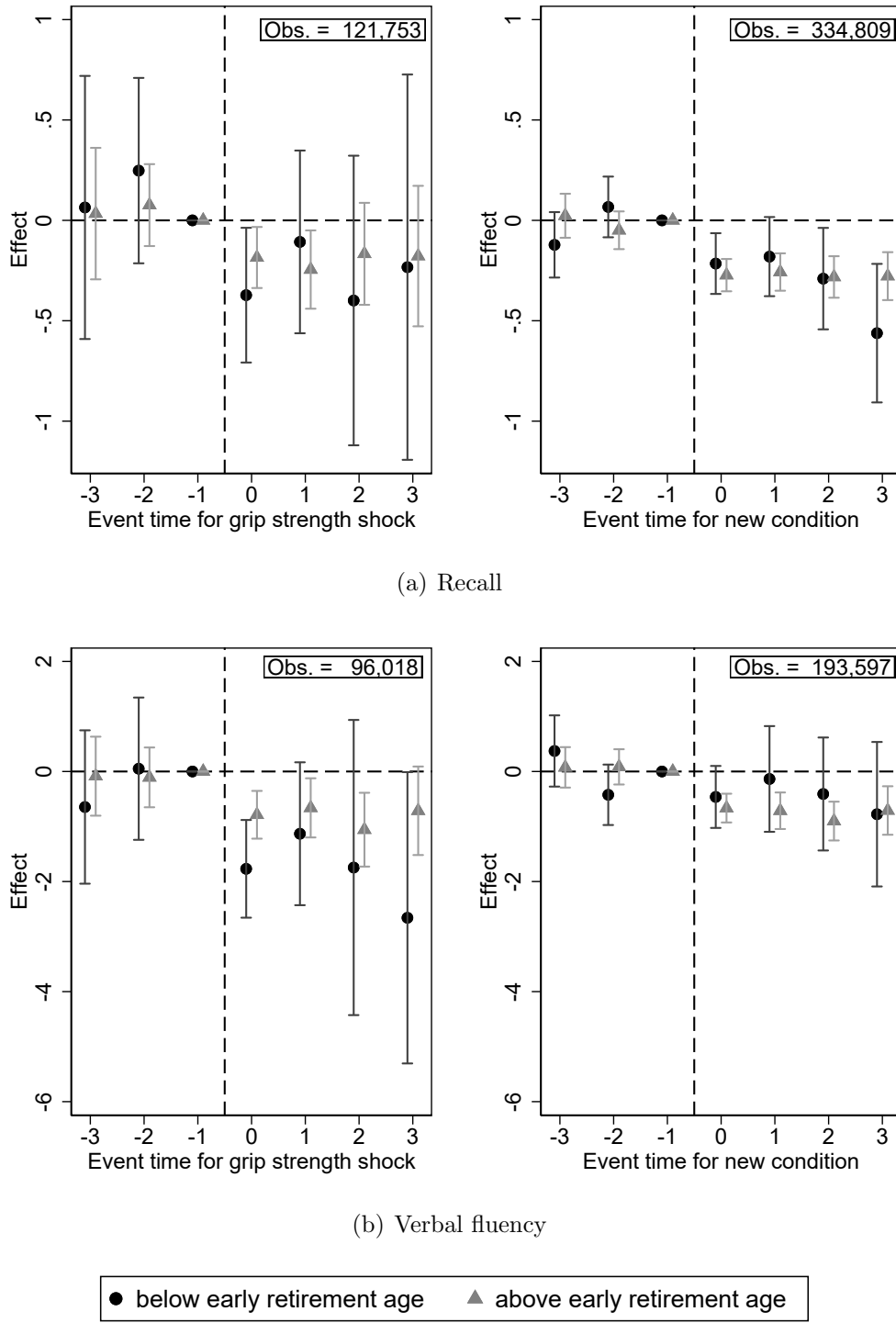
Notes: Coefficients $\mu_r + \mu_r \times mod_{it}$ from estimations of Equation 4 based on data from SHARE, HRS and ELSA. $\mu_{-1} + \mu_{-1} \times mod_{it}$ is restricted to zero. 95% confidence intervals reported.

also have the potential to bring a long-term care episode for mental health reasons forward by some years.

Figure 11: Moderation analysis – Education

(a) Recall

(b) Verbal fluency

● before comp. school. reform   ▲ after comp. school. reform

The effects of health shocks on cognitive decline do not seem to be moderated by retirement and education. Thus, we find that higher cognitive capacities (possibly due to labor force participation or education) do not prevent negative effects of health shocks. Of course, this does not mean that labor force participation and education do not pay

off in terms of cognitive abilities as the direct effect is usually found to be positive and significant in the literature.

Taken together, our analysis consistently suggests that physical health shocks significantly and persistently impair cognitive abilities in older ages. This finding seems to hold not only for different regions, representing different health insurance systems, but also independently of socio-economic characteristics that are, at least some of them, susceptible to policy action. Therefore, this analysis cannot directly point to concrete policy measures, such as further promotions of work in older ages, that could help to curb cognitive decline after a health shock. Nevertheless, we believe that it provides valuable insights, as it highlights the role of physical health for human capital maintenance and suggests that investments in physical health pay double: with a healthy body and a healthy mind.

Acknowledgments

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Understanding cognitive decline in older ages: the role of health shocks

— Supplementary Materials —

Valentin Schiele

Paderborn University

Hendrik Schmitz

Paderborn University,
RWI & LSCR

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Alternative approach

Here we specify regression models in the spirit of van den Berg et al. (2010) as follows

$$\text{Cogn. abilities}_t = \beta_0 + \beta_1 \text{health shock}_t + X'_{t-1}\gamma + \varepsilon \tag{1}$$

where a health shock is defined by a strong change in health between the waves $t-1$ and t, that is, over a period of about two years. The vector X includes gender, age fixed effects, measures of education, marital status, labor force status, income and wealth, baseline health, health behavior, country-specific fixed effects and year fixed effects. All these variables are measured in $t-1$ to make sure they are not affected by a health shock. Table S1 lists all variables in detail and reports their descriptive statistics.

Table S1: Descriptive Statistics

Variable	Mean	Std. dev.	Min	Max
Socioeconomic controls:				
Male	0.413	0.492	0.000	1.000
Married	0.669	0.471	0.000	1.000
Separated	0.014	0.118	0.000	1.000
Divorced	0.100	0.300	0.000	1.000
Widowed	0.154	0.361	0.000	1.000
Total household income/100,000	44.215	145.355	0.000	60014.375
Household net worth/100,000	334.773	798.879	-2245.500	68156.547
0-10 years of education	0.301	0.459	0.000	1.000
11-13 years of education	0.376	0.484	0.000	1.000
Employed	0.309	0.462	0.000	1.000
Unemployed	0.022	0.148	0.000	1.000
Disabled	0.030	0.172	0.000	1.000
Retired	0.548	0.498	0.000	1.000
Household size	2.151	1.039	1.000	19.000
Number of children	2.598	1.814	0.000	21.000
Never drinking	0.333	0.471	0.000	1.000
Ever smoked	0.536	0.499	0.000	1.000
Currently smoking	0.157	0.364	0.000	1.000
Self-assessed health	2.859	1.087	1.000	5.000
# difficulties with ADL	0.233	0.764	0.000	6.000
# difficulties with IADL	0.133	0.525	0.000	5.000

Notes: Descriptives statistics based on 240,810 observations from SHARE, HRS and ELSA.

A way to account for the potential issue that individuals with low cognitive abilities might be more likely to experience a health shock suggested by van den Berg et al. (2010) is to also condition on pre-treatment outcomes (Cogn. abilities$_{t-1}$).

$$\text{Cogn. abilities}_t = \beta_0 + \beta_1 \text{health shock}_t + \beta_2 \text{Cogn. abilities}_{t-1} + X'_{t-1}\gamma + \varepsilon \tag{2}$$

A third specification allows for fixed effects and takes first differences in cognitive abilities, thus, implicitly assuming that changes in cognitive abilities do not pre-date or even cause physical health shocks:

$$\Delta \text{Cogn. abilities}_t = \beta_1 \text{health shock}_t + X'_{t-1}\gamma + \varepsilon \tag{3}$$

where $\Delta\text{Cogn. abilities}_t$ is defined as a change in cognitive abilities between the waves $t-1$ and t.

If changes in cognitive abilities pre-date or cause physical health shocks, the estimates for β_1 derived from these regressions will be biased. This seems to be less of a problem for severe health shocks such as myocardial infarctions or cancer diseases, but might be more relevant for injuries due to falls like hip fractures. Also, time-varying unobservables that affect both, a health shock and cognitive abilities lead to biased results. We discuss these issues in more detail in the next subsection but start here with benchmark results.

The results from six separate regressions (two health shock measures times three specifications) are reported in Table S2. Each cell reports a coefficient of a health shock measure from a single regression. Apparently, both kinds of health shocks strongly affect cognitive abilities. Depending on the type of health shock and the regression model, a health shock goes along with around 0.24–0.46 recalled words less. The effect size is around 7–13 percent of a standard deviation which is typically regarded as a considerable amount.

Table S2: Baseline regression results

Dep. var.:	Recall (1)	Recall (2)	Δ Recall (3)
New condition	-0.291***	-0.269***	-0.240***
	(0.029)	(0.026)	(0.029)
Grip strength shock	-0.464***	-0.406***	-0.337***
	(0.042)	(0.038)	(0.045)
Further control variables	yes	yes	yes
Pre-treatment outcome	no	yes	no

Standard errors clustered at individual level in parentheses; * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$; Each of the six cells is the result of a different regression with either New condition or Grip strength shock as explanatory variable and several control variables. Column numbers (1), (2), (3) match the regression equations (1), (2), (3) in the text. Regressions on New condition based on 240,810 observations from SHARE, HRS and ELSA, regressions on Grip strength shock based on 88,416 observation from SHARE. Further control variables as indicated in the text.

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 form 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 Komission, 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, Slowenien, 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.

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