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Late-Career Unemployment and Cognitive Abilities

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Diana Freise, Hendrik Schmitz, and Matthias Westphal[1]

Late-Career Unemployment and Cognitive Abilities

Abstract

We study the effect of unemployment on cognitive abilities among individuals aged between 50 and 65 in Europe. To this end, we exploit plant closures and use flexible event-study estimations together with an experimentally elicited measure of fluid intelligence, namely word recall. We find that, within a time period of around eight years after the event of unemployment, cognitive abilities only deteriorate marginally – the effects are insignificant both in statistical and economic terms. We do, however, find significant effects of late-career unemployment on the likelihood to leave the labor force, and short-term effects on mental health problems such as depression and sleep problems.

JEL-Codes: J14, J24, C21, I1

Keywords: : Cognitive abilities; mental health; unemployment; event studies; plant closures

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1 Diana Freise, Paderborn University; Hendrik Schmitz, Paderborn University, RWI, Leibniz Science Campus Ruhr; Matthias Westphal, TU Dortmund, RWI, Leibniz Science Campus Ruhr. *- We thank two anonymous referees, Valentin Schiele, and participants of the Fakultätsforschungsworkshop in Paderborn and the RWI health research seminar for very helpful comments. Funding from the German Research Foundation (Deutsche Forschungsgemeinschaft, DFG) is gratefully acknowledged.* *– All correspondence to: Matthias Westphal, Department of Economics, TU Dortmund University, 44221 Dortmund, Germany, e-mail: matthias.westphal@tu-dortmund.de*

1 Introduction

Cognitive abilities are essential for good economic decisions (see, e.g., Tymula et al., 2013; Christelis et al., 2010; Banks and Oldfield, 2007; Banks et al., 2010; Smith et al., 2010). A continuing decline in cognitive abilities, as is normal at older ages, may even impede the very basic decisions involved in activities of daily living (see, for instance, Nishiguchi et al., 2013). Together with depression and after hypertension, strong cognitive impairment is also the second most frequent diagnosis of long-term care dependent persons in nursing homes in the USA (Harris-Kojetin et al., 2019). Interesting from an economic point of view is that many decisions over the life course themselves affect cognitive abilities. For instance, economists have almost traditionally studied the process of human capital accumulation—most importantly determinants and effects of education—where cognitive abilities are an important part of human capital. However, much less is known about the process of human capital *depreciation*, see McFadden (2008) or Mazzonna and Peracchi (2018). An exception is the relationship between retirement and cognitive abilities that has recently been studied by several authors in the economic literature, see, Rohwedder and Willis (2010), Bonsang et al. (2012), Coe et al. (2012), Celidoni et al. (2017), Mazzonna and Peracchi (2012), Mazzonna and Peracchi (2017), Mosca and Wright (2018), Atalay et al. (2019), Schmitz and Westphal (2021). By and large, this literature finds that time spent in retirement reduces cognitive abilities.

The main explanation for this finding is the *use it or lose it hypothesis* (Hultsch et al., 1999; Salthouse, 2006), also called *mental exercise* (Rohwedder and Willis, 2010) or *cognitive enrichment* (Hertzog et al., 2008). It states that mental stimulation, potentially due to labor market participation, slows down the natural process of neuro-degeneration and, thus, cognitive aging. Nevertheless, little is known about determinants of cognitive decline among individuals younger than the statutory retirement age. This is surprising given that labor economists typically consider skill depreciation to affect re-employment wages whenever analyzing absenteeism from the labor force, e.g. due to parental leave (see Schönberg and Ludsteck, 2014) or due to unemployment (as, for instance in Schmieder et al., 2016). In this paper, we study the effects of unemployment on the decline of cognitive abilities. By looking at the effect of economic inactivity in a sample of individuals between 50 and 65 years of age—late in the individual employment career—, we address an important gap in the social sciences literature given that age-related cognitive decline already starts well before retirement age.[1]

We are aware of several studies on the relationship between unemployment and cognitive abilities—although none of them is claiming causality. Vélez-Coto et al. (2021) review those

[1] Salthouse (2009) finds age-related cognitive decline to begin around the age of 30. Although a decline this early in life is debated in the literature, there is also evidence indicating cognitive decline around the age of 45 (Singh-Manoux et al., 2012).

previous findings and conduct a meta-analysis of six studies from the psychological and medical literature, indicating a negative association between unemployment and cognitive abilities. However, our paper also relates to the broader literature on general effects of job loss and unemployment. Initially, the focus of this literature was on studying wages and earnings, finding large and persistent earnings effects, which seems to be primarily driven by labor force participation and to a smaller extent though wages (for European evidence using administrative data see, for instance, Schmieder et al., 2010, Eliason and Storrie, 2006, and Huttunen et al., 2011). By studying cognitive decline, our paper could provide an explanation for the persistent effects. Subsequently, the emphasis shifted towards the impact on health. Examples are Sullivan and Von Wachter (2009), Salm (2009), Mandal et al. (2011), Marcus (2014), Browning and Heinesen (2012), Kuhn et al. (2009) or Eliason and Storrie (2009b,a), Green (2011), Böckermann and Ilmakunnas (2009), Schmitz (2011), Marcus (2013), Schiele and Schmitz (2016). As is still the exception in this literature, we make use of transparent analyses using event-study methods. These methods may inform about potential anticipation effects necessary for a causal interpretation and allow to study dynamic properties of the effect.

As our main data source, we use the Survey of Health Ageing, and Retirement (SHARE) that includes data from 29 European countries and Israel. The data set has the advantage to include experimentally elicited measures of cognitive ability like the *word recall test* that allow studying cognitive decline already in a sample that is younger than retirement age. Register data, that may be preferred for other reasons, usually either include measures of cognitive ability early in life (like IQ tests among young men from medical examinations) or physician diagnoses of mild cognitive impairments or dementia that have very low incidences among individuals below 65. We are able to identify changes in cognitive ability that are below the threshold of diagnosed cognitive impairment. The panel structure of our data enables us to examine the cognitive path for those who experience unemployment and follow them for up to eight years.

In our data set that spans over the years 2004-2020, we can exploit about 3,000 unemployment episodes and match these to a control group of 19,000 individuals that are not unemployed throughout the observation period. In several different specifications we do not find economically or statistically significant effects of later-career unemployment on cognitive abilities, neither in the short-, nor in the longer run. The largest effect is a loss of 0.2 words in the word recall test after about four years which is less than ten percent of a standard deviation or two percent of the mean value of recalled words. This effect returns to zero after six years. This result of a—if any—very small effect is consistent with a common finding in the literature on the effects of retirement on cognitive abilities. Typically, there is no sizable instantaneous effect of retirement but one that increases with time in retirement. For instance, Schmitz and Westphal (2021) find retirement effects on word recall to start at around 0 upon retirement (on average) and linearly increase to -1 after ten years. Given that the median unemployment duration in our data set is 1.3 years, there seems to be not much room for larger cognitive impairment.

The results are robust across subgroups like sex, age groups and all reasons of unemployment vs. plant closures. They also hold for other measures of cognitive ability like verbal fluency or numeracy and are also found in the USA and England, when we use data from the Health and Retirement Study (HRS) and the English Longitudinal Study of Ageing (ELSA). However, this does not mean that late-career unemployment does not have any effects at all. We do observe a short-term effect on depression which, however, disappears after two years when the unemployment episode typically is over. In addition, we find a small and marginally significant effect on sleep problems. Finally, we find that unemployment triggers the end of the labor-force career in many cases. Our analyses show that the likelihood to retire increases by about ten percentage points as a consequence of unemployment. The most likely explanation of our findings is that unemployment does not have an immediate effect on cognition among 50-65 years old individuals but leads to retirement being brought forward by a couple of years. This, in turn, has negative effects that phase in only in the mid-run.

This paper is structured as follows. We present the data in Section 2 and the empirical strategy in Section 3. Results of the main analysis are reported in Section 4 while we discuss other outcomes like mental health or transitions into retirement in Section 5. Section 6 concludes.

2 Data

We use data from the Survey of Health Ageing, and Retirement (SHARE), a large biennial representative micro data set providing information on health and other socioeconomic characteristics for individuals aged 50 and older. SHARE was initiated as a cross-national survey in 2004. By now, it covers information of about 140,000 individuals living in 29 European countries plus Israel.[2] For the analysis we employ waves 1, 2, and 4-8, as wave 3 (SHARELIFE) treats different aspects and does not contain the variables of interest.[3]

2.1 Sample selection

We define the event of unemployment as our *treatment* and compile a matched sample of treated and untreated individuals. We first describe how the subsample of treated

[2]For comprehensive information on the sampling procedure, questionnaire contents, and fieldwork methodology of SHARE see Börsch-Supan and Jürges (2005).

[3]See Börsch-Supan (2019a,b,c,d,e,f,g, 2021); Brugiavini et al. (2019).

individuals is constructed. It consists of all individuals who *(i)* provide information for at least two consecutive waves, *(ii)* were employed or self-employed in at least one wave,[4] *(iii)* transited from work into unemployment between two waves, and *(iv)* were at most 65 years old when they became unemployed. Individuals who fulfill these criteria enter the treatment group with their observations from all waves. For example, a person might have become unemployed at the age of 60 in 2010. In 2020 she is 70 and retired. We still use the information from 2020 to estimate the longer-run effect of unemployment.[5] Another person that states to have become unemployed, say, at the age of 66 would not be considered for the treatment group. For individuals who become unemployed more than once in the observation period, we treat their first occurrence of unemployment as the *event* that we study. Yet, in our sample, 76.4 per cent are unemployed only once and for one wave. Let t denote the interview wave, e_i the (first) wave a person became unemployed, and $r_{it} = t - e_i$ the normalized wave relative to the wave of unemployment. By construction, each individual in the treatment group is employed the wave before unemployment $r_{it} = -1$.

We leave the future labor force status after unemployment entry unrestricted, meaning that individuals in the treatment group could find their way back into the labor market after unemployment, stay unemployed, or leave the labor market into retirement or for other reasons.[6] In the terminology of the mediation effects literature this means that we estimate the total effect of unemployment which includes a potential indirect treatment effect via transition out of the labor force or inducement of a second unemployment episode.

To form the control group, we first pool all observations that are *(i)* not in the treatment group and *(ii)* employed for at least two consecutive waves. Then, we carry out an exact matching which is best explained using an example. Consider an individual from the treatment group who became unemployed in 2010. We search for an exact match in the control pool based on age, interview year, country and sex, of a person who was employed in 2010 and in the wave before. Of each person that fulfills these criteria we use, as in the treatment group, all observations that are available throughout all waves. If there are multiple matches for a treated individual (which is the regular case), we use all matches and assign them weights that add up to one. Matching is with replacement.

Our final matched sample consists of 3,183 individuals in the treatment group and 19,435 in the control group and a total of 78,797 person-wave observations (11,003 treated, 67,794 controls). We also generate a matched plant-closure subsample. Here, individuals are included in the treatment group if their (first) event of unemployment was due to a

[4]From now on, referring to employment either means employment or self-employment.

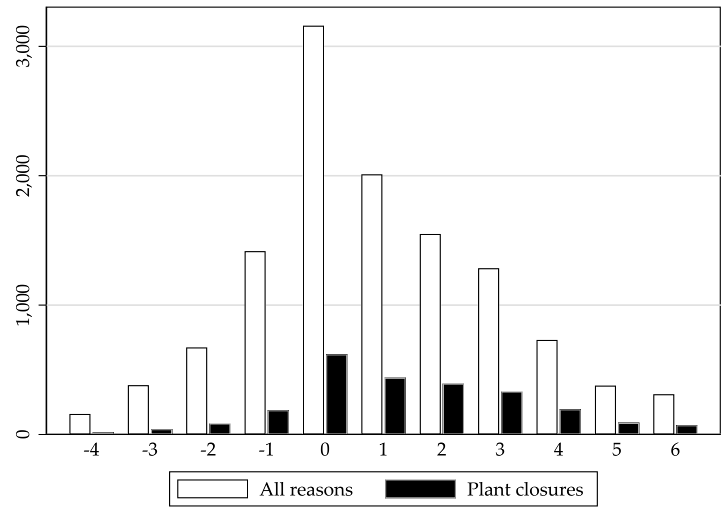
[5]Note that information from 2020 was collected before the Corona pandemic.

[6]For individuals who are unemployed when they enter the SHARE, we use information from the *job episodes panel* that includes data on the labor market history to check their employment status two years before entry. Individuals who had been employed the two years before enter the treatment group, while those who were unemployed before are dropped from the analysis.

plant closure. This holds for 623 individuals and we receive a sample of 2,241 person-wave observations in the treatment group and 31,653 in the control group of employed individuals. Observations by country are reported in Table C1 in the Appendix.[7]

Note that our sample is unbalanced in terms of pre- and post event years, see Figure 1. Due to strongly reduced numbers of observations at early and late event times (see Figure 1) we restrict the event-time to be between -3 and 4. We decided to trim the data at these times and use all observations between -3 and 4 in the baseline specifications.[8] We also show robustness checks when we restrict the sample to individuals that are in the sample for at least six waves within this time frame to get a more homogenous distribution of relative waves.

Figure 1: Observations by relative time in the treatment group.



Notes: Own calculations, based on the matched sample. The horizontal axis shows relative time $r = t - e$. The bars show numbers of observations with different relative times.

2.2 Dependent variable

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 offers a number of potential

[7]The control group for the plant closure sample is much larger than for the baseline sample (in relative terms). The reason is that we match with replacement. Control workers can be matching partners for several different treated workers. It may, thus, be the case that we drop treated workers but keep control workers.

[8]Keeping all event times in the sample, also smaller than -3 and larger than 4 does not make a difference for the event study-estimates between -3 and 4. Due to our limited panel dimension and the increasing multicollinearity between the wave and the event time fixed effects at the boundaries, estimates may become fuzzy and imprecise at the extremes.

measures for cognitive abilities: orientation in time, numeracy, verbal fluency and word recall tests.[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 occasions are added up to yield the word recall test score. This score can range between 0 and 20. Word recall is a measure of episodic memory, which is found to react most strongly to aging (Rohwedder and Willis, 2010). Further information in the test can be found in Appendix A. While *recall* only captures specific parts of the multidimensional concept "cognitive ability", 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). This measure is used in many economic papers that deal with cognitive decline, for instance Rohwedder and Willis (2010), Celidoni et al. (2017), Mazzonna and Peracchi (2012), and Coe et al. (2012). Celidoni et al. (2017) show that a strong reduction (minus 20 per cent) predicts dementia in the Health and Retirement Study in 70% of all cases.

In complementary analyses in Section 4.2, we report results using other measures of cognition. These are *verbal fluency*, *numeracy* and *orientation*.

2.3 Descriptive statistics

In Panel A of Table 1, we present descriptive statistics of the baseline sample (with all reasons for unemployment) and of the plant closure sample, separately by treatment and control group. Looking at the baseline sample with all reasons of unemployment, word recall is significantly larger in the control group than in the treatment group (significance test not reported in the table). This mirrors the negative association between unemployment and cognition found in the meta-analysis of Vélez-Coto et al. (2021) and might either be due to selection into unemployment or due to an effect of unemployment or both. Age and gender composition is similar in both groups as this is how the sample is constructed.[10] 35 per cent of all person-wave observations in the treatment group are unemployed while this number is 0 by construction in the control group. Vice versa, employment rates are much larger in the control group while probabilities of retirement, disability or being out of the labor force for other reasons are comparable among both groups, yet slightly larger in the treatment group. In the plant closure sample, we observe the same differences

[9]This subsection draws on Schiele and Schmitz (2021).

[10]Note that in spite of the exact matching procedure described in Section 2.1 there are slight differences in age and gender. This is because matching is carried out on individual level, not on individual-wave level. Matched individuals might bring in observations from different waves.

between treatment and control group. However, this sample is slightly older and has lower cognitive abilities than the baseline sample.

Table 1: Descriptive statistics

Panel A

	Baseline: All reasons					Plant closures	
	T	C	Total			T	C
	mean	mean	mean	min	max	mean	mean
Recall	10.18	10.77	10.49	0	20	9.90	10.62
	(3.35)	(3.24)	(3.30)			(3.37)	(3.21)
Age	58.30	58.75	58.54	50	79	58.52	59.02
	(4.58)	(5.09)	(4.86)			(4.64)	(5.15)
Male (share)	0.51	0.51	0.51	0	1	0.52	0.53
	(0.50)	(0.50)	(0.50)			(0.50)	(0.50)
No. of test repetitions	1.59	1.79	1.70	0	6	1.60	1.80
	(1.36)	(1.53)	(1.46)			(1.36)	(1.52)
Unemployed (in %)	33.82	0.00	15.77	0	100	39.80	0.00
	(47.31)	(0.00)	(36.45)			(48.96)	(0.00)
Employed/self-employed (in %)	41.78	78.77	61.52	0	100	30.92	74.67
	(49.32)	(40.89)	(48.65)			(46.23)	(43.49)
Retired (in %)	19.51	19.13	19.31	0	100	23.87	22.78
	(39.63)	(39.33)	(39.47)			(42.64)	(41.94)
Disabled(in %)	2.03	0.82	1.38	0	100	2.05	0.98
	(14.09)	(9)	(11.67)			(14.18)	(9.87)
Not in labor force (in %)	2.86	1.28	2.02	0	100	3.35	1.57
	(16.68)	(11.25)	(14.07)			(17.99)	(12.41)
No. of obs #	11003	67794	78797			2241	31653

Panel B

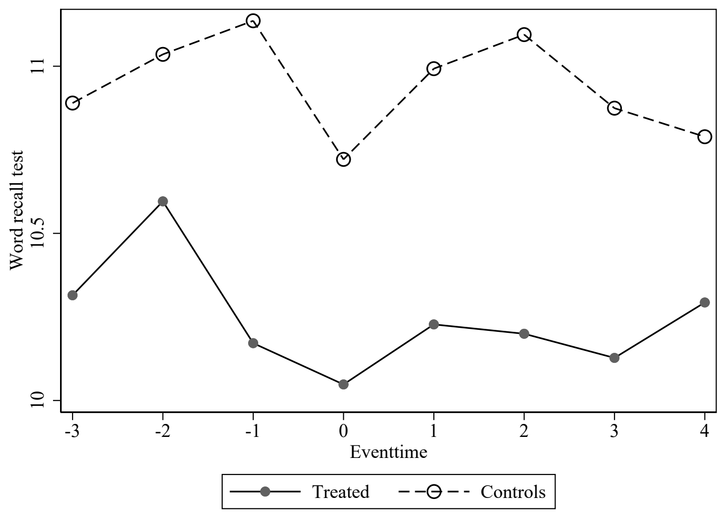
	Difference in recall score between the first and last wave of each individual						
	$\leq$ -3	-2	-1	0	1	2	$\geq$ 3
Share of individuals in %	19.84	9.44	11.16	12.94	12.05	10.62	23.96

Notes: Standard deviations in parentheses. C: Control group, T: Treatment group. Own calculations. Panel B shows the distribution of within-variation of the recall score, that is the difference in recall score between the first and last observation of an individual.

Panel B of Table 1 shows the within-individual variation of the recall score. For this, we compare the score at the first time an individual is in the data set with the one measured at the last time. 13 per cent of all individuals had the same recall score both at the beginning and at the end of the observation period (that is, a difference of 0 in the table). The remaining individuals experienced drops or increases over time. Overall, there is considerable within-variation in the outcome variable.

Figure 2 reports the raw means of the outcome variable by event time. For this graph, we assign the control group a hypothetical event time, based on the information at which year a control unit is exactly matched to a treated unit. There is some noise in the data, including a drop in cognition in the treatment group between -2 and -1 which is not mirrored in the control group. This drop may be interpreted as cognitive decline being a reason for unemployment. Yet, it will become much smaller (and statistically insignificant), once control variables and individual fixed effects are accounted for in the event study estimations later on in Section 4. By and large, this graph anticipates the very small effects of unemployment on cognition that will be found later in the analysis.

Figure 2: Mean recalled words for the treatment and the control group by relative time



Notes: Own calculations, based on the matched sample. The horizontal axis shows relative time $r = t - e$.

Table 2 shows the correlations of the recall score with unemployment, and, as a benchmark, with age and retirement, resulting from simple OLS regressions. Repeating the finding from Figure 2 in different form, Column (1) presents a high correlation between unemployment and the recall score. Unemployed individuals recall one word less on average, just controlling for three 10-year age bins of the individuals in our sample. Retirement, in contrast, has a much lower correlation with the recall score, as shown in column (2). Retirees recall a quarter word less than not yet retired individuals do. This smaller correlation with retirement compared to unemployment may be surprising given that retired individuals are older and hence, may have a stronger age-related decline than the unemployed have. On the other hand, selection (at least in terms of levels of cognitive abilities) into unemployment might be much stronger than into retirement, given that eventually everybody is retired but not everybody experiences unemployment. The regression results below will show whether the difference in cognition between unemployed and employed is only driven by selection or whether there is also an effect of unemployment itself.

Table 2: OLS results: cognitive abilities and its associations with unemployment, age, and retirement

	Dependent variable: recall			
	(1)	(2)	(3)	(4)
Unemployed	-0.969***			-1.019***
	(0.064)			(0.065)
Retired		-0.229***		-0.129**
		(0.040)		(0.046)
Linear age-group-specific age coefficients (age splines):				
$\mathbb{1}\{50 \leq age \leq 59\} \cdot age$			-0.004	-0.001
			(0.006)	(0.006)
$\mathbb{1}\{60 \leq age \leq 69\} \cdot age$			-0.079***	-0.074***
			(0.007)	(0.009)
$\mathbb{1}\{70 \leq age \leq 79\} \cdot age$			-0.155***	-0.154***
			(0.038)	(0.038)
Ten year age bins	yes	yes	yes	yes
Observations	78,797			

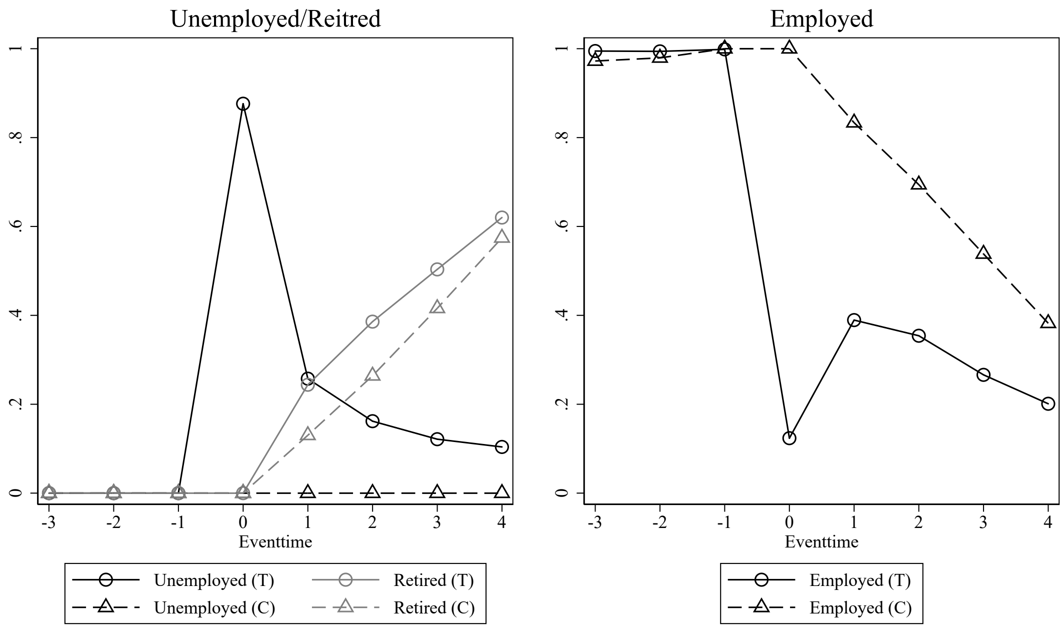
Standard errors in parentheses. * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Beyond the displayed coefficients, the OLS regressions include ten year age bins (same bins as for the splines).

Controlling more flexibly for age, Column (3) indeed shows that the average linear cognitive decline increases across the age bins. In the 50s, the yearly descriptive decline is negligible. In the 60s, the decline sets in, amounting to 0.08 recalled words less per annum. Taken on its own, this is a small relationship, even more so when contrasted with the corresponding correlations of unemployment and retirement. Yet, it accumulates and increases over time. In the 70s, the age-related decline doubles. Except for retirement, whose correlation with the recall score halves when controlling for the other factors (primarily because retirement strongly correlates with age), the correlations with unemployment and age persist in magnitude in a joint regression as shown in column (4). The finding that the coefficient of retirement changes while the one for unemployment stays constant can be explained as follows: while unemployment is mainly relevant among the 50-60 year old individuals, retirement sets in at age 60-70. As there seems to be no strong age-related decline in cognition (at least regarding this measure) between 50 and 60, it does not make a difference whether we include age here or not. This is different for the age group 60-70 where age plays an important own role and is correlated with retirement.

Figure 3 illustrates the labor force status transitions in the treatment and the control group (baseline sample). The left panel reports shares in unemployment and retirement, the right panel reports shares in employment. Transitions in the treatment group are plotted with solid lines and circles, while dashed lines and triangles denote the control group.

Before $t = 0$, no individual in the sample is unemployed or retired. In $r_{it} = 0$, 89% in the treatment group are unemployed while 11% are employed, again, at the time of the interview. As SHARE provides information if individuals have been unemployed between two waves, individuals who are employed in $r_{it} = -1$ and $r_{it} = 0$ are in the treatment group if they were unemployed in between. For this group, we define e_i as the wave after unemployment. In $r_{it} = 1$, that is, two years after $r_{it} = 0$ only 26% are still (or again) unemployed, while 39% are back in employment and 25% are retired. This number increases to 62% in $r_{it} = 4$. Yet, due to ageing over time, individuals in the control group also retire. Compared to the treated, the likelihood to retire in $e = 1$ to $e = 3$ is around 10 percentage points smaller but almost equal in $e = 4$. As apparent in the right panel, individuals in the treatment group have longer periods of economic inactivity than the control group.

Figure 3: Labor force status transitions in treatment and control group



Notes: Own calculations, based on the baseline sample (all reasons of unemployment). Numbers in both graphs do not add upp to 100% per event time and group because transitions into disability and out of the labor force (except retirement) not shown.

We note three points: Again, duration of unemployment or the number of job losses that result in unemployment do not play a major role in the data. Most individuals are unemployed only in one wave, giving rise to our event study specification below that treats the first occurrence of unemployment in the observation period as the event we are interested in. As the calendar months of start and end of the unemployment episode are included in the data for some individuals, we can calculate the median unemployment duration (if unemployed), which is 1.3. Second, nevertheless, unemployment seems to antedate retirement and other forms of inactivity in our sample of individuals aged 50+, where less than half of the unemployed return to employment afterwards. Third, however, individuals in the control group also transit into retirement during the observation period. We discuss the issue of labor market transitions in more detail in Section 5.

3 Empirical Strategy

We use an event study approach to estimate the relationship between unemployment and cognitive abilities. Using relative time (also called event time below) $r_{it} = t - e_i$ as defined above, our baseline estimating equation is:

$$Y_{it} = \alpha_i + \lambda_t + \sum_{\substack{j=-3 \\ j\neq -1}}^{4} \gamma_j \mathbb{1}[r_{it} = j] + \boldsymbol{\delta X'} + \varepsilon_{it} \tag{1}$$

where Y_{it} is the outcome of a respondent's word *recall* test, α_i is an individual fixed effect, λ_t are year fixed effects and $\boldsymbol{X}$ is a vector of covariates which consists of age fixed effects and the number of times a person has participated in the word recall test in previous waves to account for potential learning effects. Potentially important time-constant controls such as country effects, gender, or education are captured by the individual fixed effect.

We include indicator variables for each relative time r (their coefficients are γ_r) but restrict $\gamma_{-1} = 0$. All indicators take on the value 0 in the control group. The control group helps to identify age and time trends and to separate them from the treatment effect.[11] This allows us to examine effects until up to eight to ten years after the event. Standard errors are clustered on the individual level throughout.

To interpret our coefficients as causal effects, we need to assume that unemployment is not caused by a cognitive decline (at least in one of our pre-treatment periods). This means that without unemployment, the evolution of the cognitive abilities for individuals that eventually become unemployed (the treatment group) would follow the one of individuals that are never unemployed throughout our observation period (the control group). This is the well-known common trend assumption. We check the plausibility of this assumption later on and indeed find absence of significantly different pre-trends which does not proof but lends credibility to the assumption. The ability to visualize these pre-trends makes this

[11] We could likewise run the following regressions

$$Y_{it} = \alpha_i + \lambda_t + \sum_{\substack{j=-3 \\ j\neq -1}}^{4} \Big(\gamma_j T_i \mathbb{1}[r_{it} = j] + \theta_j \mathbb{1}[r_{it} = j] \Big) + \boldsymbol{\delta X'} + \varepsilon_{it}$$

where T_i indicates the treatment group and the control units keep their hypothetical event times instead of setting their indicators to 0. Since, now, the age and time fixed effects are almost perfectly correlated with the baseline event indicators $\mathbb{1}[r_{it} = j]$, the resulting γ_j are basically the same as in the more parsimonious Eq. (1).

research design transparent and compelling, and therefore, very popular. In recent years, it has been used to study causal effects of events that are not necessarily exogenous but otherwise hard to identify with natural experiments (see, Kleven et al., 2019a and Kleven et al., 2019b for effects of child birth and Dobkin et al., 2018 for the effects of a hospital admission).

Difference-in-differences models with staggered entry or standard event study estimations have recently been criticized, in particular if effects are dynamic over time and not homogeneous by event-cohort, see e.g. De Chaisemartin and d'Haultfoeuille (2020), Callaway et al. (2018), Goodman-Bacon (2021), and Sun and Abraham (2021). Intuitively, early treated groups could serve as controls for late treated groups, which renders the estimates of average treatment effects on the treated biased. Even though these problems seem less relevant in our case where we do not use an aggregated difference-in-differences estimator and have a clear control group, we back our results by implementing Sun and Abraham's (2021) interaction-weighted (IW) estimator. It can be implemented in three steps, summarized in Appendix B. We present the main results using both the standard event study approach and the IW estimator.

Finally, we also combine event-study methods with plant closures. In this approach, we only compare those who become unemployed because their business closes down with a matched control group of employed individuals. Assessing the effects of plant closures may be appealing because the reason for job loss may more likely be exogenous and less likely caused by a cognitive decline. For instance, even without pre-trends, the estimates for unemployment could be misleading if anticipation effects and different trends between treated and untreated individuals offset each other. Although this appears to be unlikely, as this implies a higher cognitive decline for the always employed control group, we use plant closures to complement our analysis. In our opinion, this balances well the trade-off between reduced sample size for this subgroup and higher credibility of exogeneity of the event.

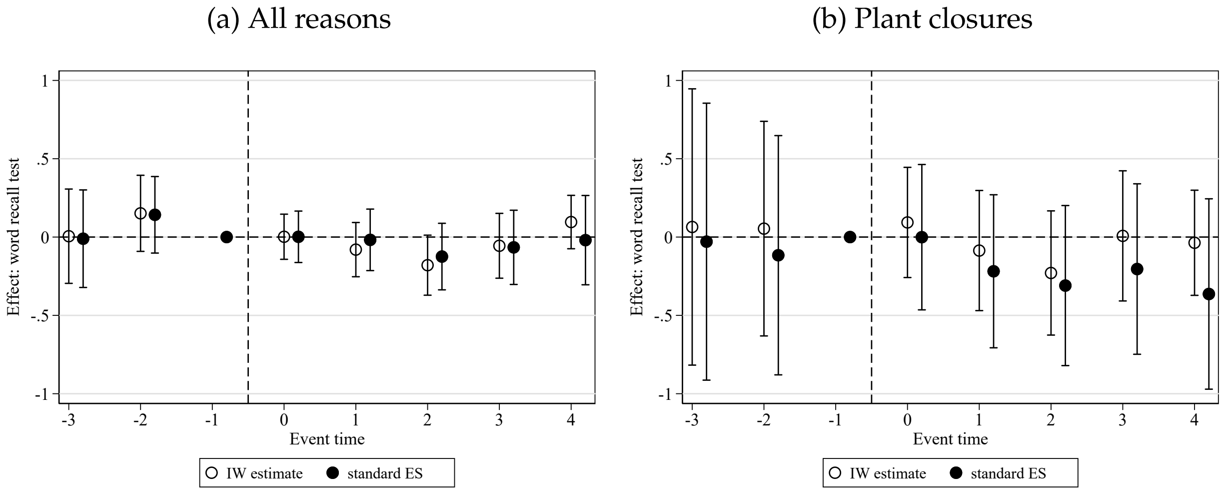
4 Results

4.1 Baseline results

Figure 4 reports the main results using standard event study and IW estimator. The left panel (a) uses all reasons of unemployment while the right panel (b) restricts the treatment to unemployment due to plant closures. Both estimators and both samples yield very similar point estimates where the major difference is that the confidence bands are larger in the plant closure sample. We find a quantitatively small drop in cognition in panel

(a) between event time -2 and -1 of 0.15 words. Even if this is interpreted as a structural effect (and not as an estimate by chance) we argue that, by and large, the pre-trends are negligible, in terms of both economic and statistical significance. Much of the difference in pre-trends seen in Figure 2 is captured by control variables. This implies that individuals who become unemployed between $r = -1$ and $r = 0$ do not seem to be on a strongly different path of cognitive decline the years before. Most importantly, this pre-treatment evolution does not challenge our interpretation of treatment effects after the event.[12] Even though plant closures, too, probably are not completely random, the absent pre-trends make us confident that this does not impose serious problems. Moreover, even if we assume that the most able workers leave the company already before a closure: this might lead to an overestimation of effects (in absolute terms)—but we do not find any.

Figure 4: Main results based on IW estimator and standard event study



Notes: Own calculations. Treated individuals are those who are unemployed for any reason (left panel) or due to plant closures (right panel). No. of observations: 78,797 (left), 33,606 (right). Point estimates with 95% confidence intervals based on Eq. (1) and Eq. (3). Reference category: -1. Event time is measured in waves which means that one event period is, on average, two calendar years. Controls: Individual-FE, year-FE, age-FE, no. of test repetitions. Standard errors clustered on individual level. Regression results of the standard event study reported in Table C2 in the Appendix.

Turning to post-treatment periods, there are no large effects of unemployment on word recall, neither in the short- nor in the longer run. Word recall reduces by up to 0.2 in $r = 2$ in both samples. Below in Figure 7 we see that the aggregated treatment effect—meaning here: averaged over all post-treatment periods in the sample—is around 0.1. Neither the average nor any single coefficient is statistically significant or economically large. An effect of 0.1 amounts to 3 percent of a standard deviation or 1 percent relative to the mean value of the outcome variable (see Table 1). Even in the plant closure sample, we are able to rule out effects larger than 15 percent of a standard deviation (in absolute terms) with 95 per cent confidence (taking the lower bound of the confidence interval normalized by the unconditional standard deviation as the benchmark). This minimal detectable effect size is

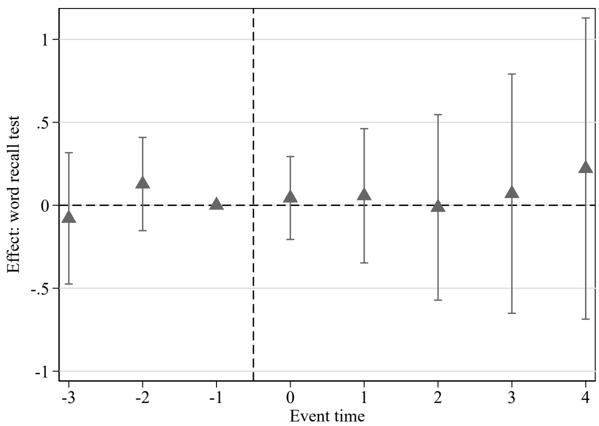
[12]Also note that we do not find systematic pre-trends for three other measures of cognition. See the discussion below and results in Figure C3.

relatively small, enabling us to detect relatively small effects that would hypothetically shift the even median worker only from the 50th to the 44th quantile of the normally distributed latent cognitive ability distribution in this worst-case scenario. Additionally, it compares well with minimal detectable effect sizes reported in the literature. For instance, Salm (2009) who employs a similar estimation strategy but with self-assessed health as an outcome, the minimal detectable effect size is 14 per cent of a standard deviation. Concerning the association between unemployment and cognitive abilities, the average detectable effect size as reported in the meta study by Vélez-Coto et al. (2021) amounts to 26 percent of a standard deviation. Although these papers do not claim causality, this does not let our empirical setting appear underpowered. Likewise, our effect is comparable to the average yearly decline of above 60-year-old individuals in our sample. This additionally highlights that the effect is very small but not completely ignorable. However, unlike age, the effect of unemployment does not seem to accumulate similarly, as the unemployment duration at the 75 percentile in our sample is one wave only. Both types of estimators, standard event study and IW estimator, deliver virtually the same results. This does not seem to be a surprise as significant effects—which, if heterogeneous between cohorts, might induce problems—are absent.[13]

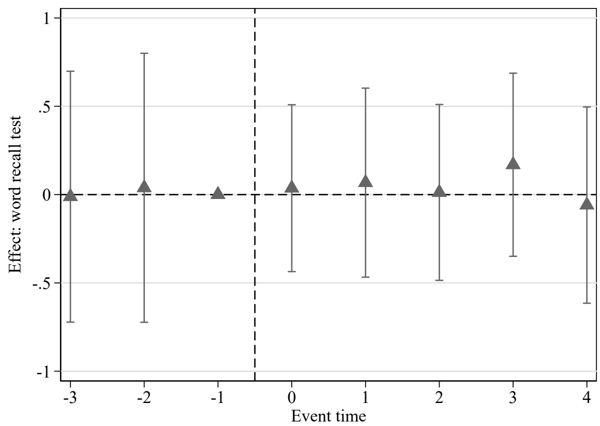
Our main findings are: *(i)* The parallel trend assumption seems to be justified both in the baseline sample with all reasons of unemployment and in the plant closure sample. *(ii)* The effect of late-career unemployment on cognitive abilities measured by word recall is close to zero.

Figure 5: Robustness checks

(a) No control group



(b) Only individuals observed for at least 6 waves



Notes: Own calculations. Treated individuals are those who are unemployed for all reasons. No. of observations: 11,011 (left panel), 15,554 (right panel). Point estimates with 95% confidence intervals based on Eq. (1) and Eq. (3). Reference category: -1. Event time is measured in waves which means that one event period is, on average, two calendar years. Controls: Individual-FE, year-FE, age-FE, no. of test repetitions.

[13]We observe one statistically insignificant but economically larger coefficient using standard event study methods in panel (b) at $r = 4$ but would not like to interpret this as evidence for a long-term effect. Particularly also because this is not robust across estimators.

We provide two robustness checks in Figure 5, both using the baseline sample. In panel (a) we carry out the analysis without the control group, only comparing individuals with each other who become unemployed and use the differential timing to identify effects. While one might argue that this is a more homogeneous sample, this is not our preferred specification. According to the insights of Goodman-Bacon (2021), the absence of a control group in two-way fixed-effects settings may increase the problem of false comparisons, where implicitly already treated observations are used as a control group (De Chaisemartin and d'Haultfoeuille, 2020 refer to the same problem as one of a negative weighting of certain group and time-specific treatment effects). In panel (b), we account for the potential problem that different individuals contribute to the different coefficients of the event-study estimates, see Figure 1. Now, we restrict the sample to individuals who are observed in at least six waves. In both cases, we do not find significant changes in results as compared to our baseline results shown in Figure 4. Obviously, the standard errors increase due to smaller sample sizes and the confidence intervals also include larger numbers. Yet, point estimates are virtually zero. Most plausibly, the reason for this is that individual treatment effects do not correlate much with the timing of unemployment.

Next, we discuss the potential problem of panel attrition which may be correlated with cognition. If unemployment triggers panel attrition, it might be that effects of unemployment are underestimated, if those who suffer most from unemployment in terms of cognitive decline leave the sample. We define an indicator for attrition and fill up the panel in the following way. Assume a person is in the panel until wave 6 and drops out thereafter. We, then, add two more person-wave observations to the sample, namely wave 7 and wave 8. The attrition indicator takes on the value one in wave 7 and 8 and zero until wave 6. Then, we repeat the event study using the attrition indicator as the outcome variable and the newly created sample.[14] According to our sample construction, attrition cannot take place before wave $r = 1$, hence, we do not estimate pre-trends in this specification. Results are reported in Figure 6a (left panel). Attrition does not seem to differ between treated and untreated before $r = 4$. In $r = 4$, the attrition rate is 3.6 percentage points higher in the treatment group.

In order to see whether the differential attrition in $r = 4$ is able to affect our conclusions regarding an effect of unemployment on cognition, we estimate bounds in the spirit of Lee (2009). To arrive at these bounds, we drop the 3.6 percent of all control observations in $r = 4$ with the lowest word recall score and re-estimate the baseline event-studies. This is equivalent to the assumption that the excess dropout in the treatment group is completely driven by those with the lowest cognitive abilities. Vice versa, we delete the 3.6 percent of observations with the highest word recall score. The upper and lower bounds of potential

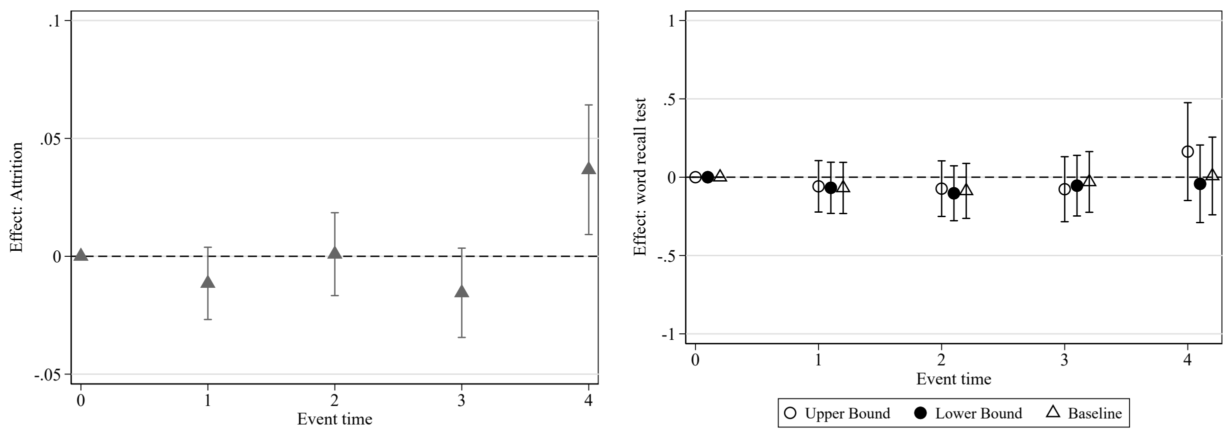
[14] We also impute age, interview year and number of test repetitions accordingly where, as an example, age in wave 7 would be observed age in wave 6 plus 2.

effects, derived by this procedure, are reported in Figure 6b (right panel). Apparently the significant but small attrition differential is not able to quantitatively affect the results.[15]

Figure 6: Robustness checks 2: Panel attrition

(a) Panel attrition

(b) Lee-bounds



Notes: Own calculations. Treated individuals are those who are unemployed for all reasons. Point estimates with 95% confidence intervals. Event time is measured in waves which means that one event period is, on average, two calendar years. Controls: Individual-FE, year-FE, age-FE, no. of test repetitions. See footnote 12 for more explanations.

Finally, in the Appendix (Figure C1), we report results of two other robustness checks. First, we additionally control for the length of the unemployment spell. Second, we drop individuals who are unemployed because they resigned. With a share of 6.3% this is only a small fraction. In both cases, the results are not affected.

4.2 Effect heterogeneity

Now, we provide results for different subsamples, other measures of cognitive ability and alternative data sets. Motivated by the absence of effect dynamics and by the similarity of results between the two samples, we use the baseline sample and run a classic two-way fixed effects model as stated here:

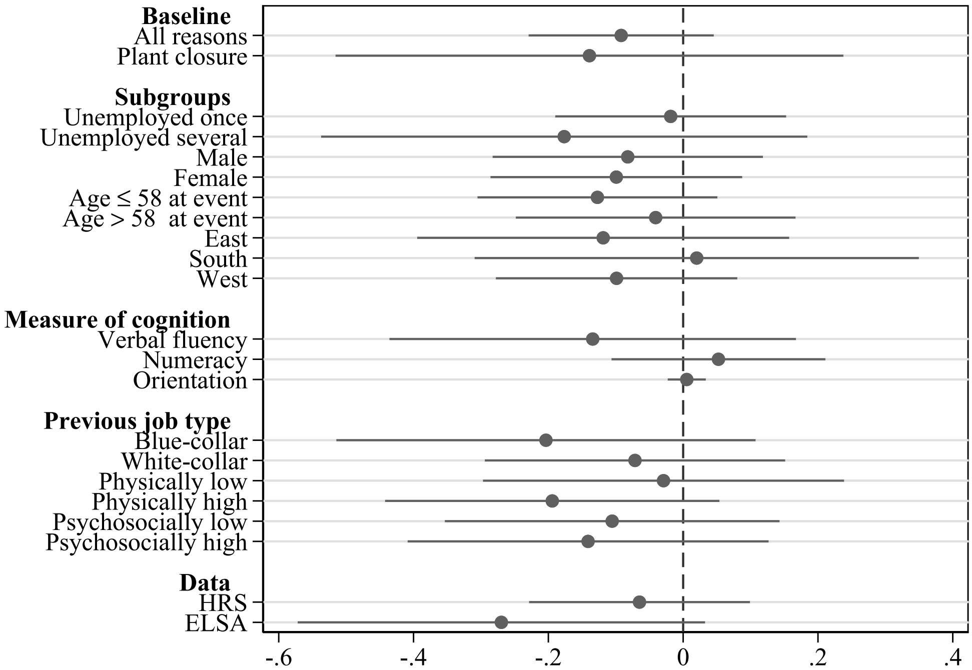
$$Y_{it} = \alpha_i + \lambda_t + \gamma post_{it} + \boldsymbol{\delta X'} + \varepsilon_{it} \tag{2}$$

This model has the same ingredients as Eq. (1) with the difference of normalizing the pre-treatment coefficients to zero and aggregating the post-coefficients into one coefficient γ. The indicator variable $post_{it}$ equals one if relative time $r_{it} = t - e_i \geq 0$ in the treatment group. While, due to sample size, the event study coefficients according to Eq. (1) in the following subsamples are a bit noisier than before, event-study estimates—not shown here—repeat the results of absence of pre-trends and effect dynamics. Therefore, we

[15]The results in this figure are estimated according to the specification laid out in Footnote 11.

restrict ourselves to reporting the results of estimates of classic difference-in-difference models. Figure 7 reports the coefficient γ of Eq. (2), where each line stands for a separate regression using either a different subsample or outcome variable.

Figure 7: Effect Heterogeneity, other samples and outcomes



Notes: Own calculations. The sample includes all reasons of unemployment with the exception of line 2, plant closures. We report the coefficient γ from Eq. (2). *East* includes Czech Republic, Poland, Hungary, Slovenia, Estonia, Croatia, Lithuania, Bulgaria, Latvia, Romania, Slovakia, *South* includes Spain, Italy, Portugal, Greece, Cyprus, Malta, Israel, *West* includes Austria, Germany, Sweden, Netherlands, France, Denmark, Switzerland, Belgium, Ireland, Luxembourg, Finland. *Physically low* and *Physically high* refer to the classification of the previous job type an individual had before unemployment as physically demanding based on the Overall Physical Exposure Index derived by Kroll (2011). Likewise, *Psychosocially low* and *Psychosocially high* indicate the sample of individuals with psychosocially less or more demanding jobs.

The first two lines repeat the baseline results of Figure 4, where, now, post-effects are aggregated into one coefficient. Estimated effects of unemployment on word recall are between -0.1 and -0.15 which is both economically and statistically insignificant. For most sub-analyses, even the confidence intervals do not include meaningful effect sizes. For the baseline results, for instance, we are able to rule out effect sizes larger than -0.23.[16] Similarly, we would be able to detect effects at the 5% significance level if they were as large as -0.137.[17]

As in already seen in the event-study estimations, the confidence interval for the effect of unemployment due to plant closure is larger and includes -0.5 as an upper-bound (in absolute) terms of a negative effect that we cannot rule out. This translates into 15 percent of a standard deviation. This effect is considerably smaller than the average point estimate

[16]Calculation: estimate $-$ 1.96 $\times$ standard error.

[17]Calculation: 0 $-$ 1.96 $\times$ standard error. Thus, note that this is not a problem of statistical precision. In a larger data set, also very small effects might be statistically significant. Yet, we interpret our coefficients as mainly being economically insignificant.

(not the upper bound of the confidence interval) of the studies in the meta-analysis of Vélez-Coto et al. (2021) which is found to be 36 percent of a standard deviation. Again, these papers do not claim causality, and we already saw that selection into unemployment (based on levels of cognition) is important and probably leads to an overestimation of effects if not fully accounted for. Studies like Salm (2009) or Mandal et al. (2011) that use plant closures as source of variation but other outcomes (namely physical or mental health) find point estimates of similar size as our upper bounds, which are insignificant and interpreted to be small. While, certainly, these are only two of very many studies, we conclude that the upper bound of our confidence interval is smaller than previous estimates in the literature on unemployment and cognition and comparable to studies in the health literature.

We, then, split the treatment group into a group that becomes unemployed in $r = 0$ and is not unemployed in $r > 0$ (*unemployed once*) and a group that becomes unemployed in $r = 0$ and either stays unemployed in at least one wave $r > 0$ or returns to work and becomes unemployed again in $r > 0$ (*unemployed several*). Obviously, this separation is potentially endogenous and might well be a result of the effect of unemployment on cognition. Therefore, this is just suggestive evidence. Nevertheless, it provides a hint whether allowing for later unemployment in the treatment group affects or even drives the results. Moreover, it gives a first idea of potential effects of longer unemployment durations. As shown in Figure C2, 76.4% in the treatment group are unemployed for exactly one wave, further 16.8% for two waves, 4.8% for three waves and only a handful of individuals for more than three waves. Figure 7 shows that the treatment effect is virtually zero in the group *unemployed once* and around 0.2 in the group *unemployed several*. This distinction does not seem to matter a great deal. The following lines of Figure 7 show that the baseline results of—if any—almost zero-effects also hold across gender, age groups and geographic regions in Europe.

The severity of cognitive decline due to unemployment may vary by how cognitively stimulating the previous job was. To study this, we split our sample into *blue-collar* and *white-collar* workers. 29 per cent are blue-collar workers, 71 per cent are white-collar workers. The effect sizes are slightly larger for the (previously) blue collar workers but not significantly different from the one for white-collar workers. Additionally, we follow the approach by Mazzonna and Peracchi (2017) and classify an occupation as physically (psychosocially) highly demanding if the Overall Physical (Psychosocial) Exposure Index derived by Kroll (2011) for the occupation is larger than five and as low if it is less or equal to five.[18] According to this definition, 47 per cent (48 per cent) of the unemployed had a physically (psychosocially) demanding previous job. Again, this separation is potentially

[18]For the treatment group, we use occupation information on the last job before unemployment. We compare treated individuals with individuals from the control group with similar physically or psychosocially demanding jobs. We use older occupation information if information on the last/current job is not available. For more information on the index construction, see Kroll (2011). We use the latest data version for the index

endogenous as occupational choice might be correlated with cognitive abilities. While more demanding jobs seem to go along with a stronger decline after job loss, the differences are not significant and, again, the overall effects fairly small even for highly demanding jobs. In contrast to what might be expected, psychosocially low and high demanding jobs hardly differ but individuals with previously physically more demanding jobs may suffer slightly more than individuals with less physically demanding jobs. These results match those for the blue- and white collar-workers. However, this difference is not statistically significant.

Next, we return to the full sample but employ other measures of cognition as outcome variables. We follow Schneeweis et al. (2014) and use verbal fluency, numeracy and orientation-to-date. These measures are a bit less standard and not all of them capture fluid intelligence. For instance, *verbal fluency* can be regarded a combined measure of crystallized and 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 becomes their test score. The score for *numeracy* ranges from 1 to 5 with higher values indicating better abilities. This score is supposed to reflect the ability to answer basic to more-advanced mathematical questions from daily life covering simple mathematical relations to calculations of compound interest rates. *Orientation-to-date* ranges from 0 to 4 with higher values indicating better abilities. It measures whether a person is able to remember the correct date including year, month of the year, day of the month, and day of the week (Schneeweis et al., 2014).

While the alternative scores are measured on different scales than the recall test and, thus, coefficients are not directly comparable, we find the qualitatively same results as before: only small and insignificant changes due to unemployment. Compared to the sample mean of 23.48, the estimated effect of -0.13 in verbal fluency is even smaller than in the case of word recall. For numeracy and orientation, we observe similar small effect sizes: The effect in numeracy of 0.05 compared to the mean of 3.68 is economically and statistically insignificant. The same holds for the effect in orientation with a coefficient of 0.005 compared to the mean of 3.9.

Finally, we move to two sister data sets of SHARE, the Health and Retirement Study (HRS) from the USA, and the English Longitudinal Study of Ageing (ELSA) from England, to repeat the baseline analysis with word recall as the dependent variable. We use HRS waves 3–13 (interviews of the years 1995 to 2017[19]) and ELSA waves 1–6 (interviews 2002–2015). The effects in HRS are smaller than in SHARE while they are larger in ELSA. With -0.23

(Kroll, 2015) to exploit both ISCO-88 and ISCO-08 classification. Further, we use these classifications for blue- and white-collar differentiation.

[19]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.

they are still insignificant and economically small. However, we should note that the analysis using ELSA is the only care where pre-trends in the disaggregated event-studies do not look convincing (they are insignificant but the point estimates are also around -0.2 in $r = -2$ and $r = -3$). By and large, this exercise shows that the finding is robust to different subgroups and measures of cognition.[20]

4.3 Discussion of the pre-trends

Before going on, we would like to discuss the issue of the pre-trends, again. It might be counterintuitive to claim absence of different trends in treatment and control group for an endogenous event such as unemployment. First, we repeat the finding of Figure 2 and Figure 4: there is certainly selection in levels into unemployment as the treatment group has lower levels of cognition throughout. Regarding selection in changes we argue that the control variables account largely for this. Remaining insignifcant pre-trends do not challenge the interpretation of zero effects.

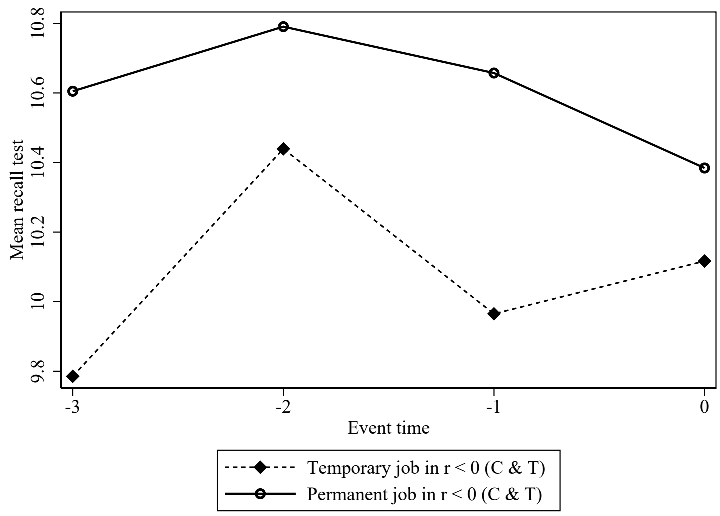
For the additional outcome variables *verbal fluency*, *orientation* and *numeracy*, we also carry out event-study estimations. Results are reported in Figure C3 in the Appendix and confirm the aggregated results of the previous section. None of the event-study graphs indicate problems with pre-trends although the confidence intervals for the *numeracy* panel cannot rule out large pre-trends. Indeed, those high standard errors may be explained by the clear reduction in sample size as *numeracy* is recorded at most two times for each respondent.

A likely reason for pre-trends is that individuals who are on a declining path of cognition have a higher likelihood to lose their job. Even if this is not due to being laid off, it may be that individuals with temporary jobs have different cognition than those with a permanent contract. In our data set, about 11 per cent have a temporary job.[21] We also observe that those with a temporary job have a much higher likelihood to become unemployed. Yet, Figure 8 shows: while those with a temporary job have lower cognitive abilities, there is no clear trend over time that would make us conclude that individuals with a temporary job have a stronger decline than those with a permanent job. This, again, can be interpreted as selection in levels but not changes and might add to the explanation of non-significant pre-trends in our case.

[20] Although we have considered multiple outcomes in this section, we abstain from adjusting the standard errors by multiple testing correction, as we never reject the hypothesis that the effect of unemployment on cognition is zero. As multiple testing is concerned with adjusting the type I error (falsely detecting significant effects), this does not help in our case as we only face the threat of a type II error (falsely not rejecting the null hypothesis although unemployment has a causal effect). We are quite confident that this error is not too large either as our minimal detectable effect size is relatively small: we would even detect significant effects that we would deem small.

[21] Unfortunately, the information on temporary jobs is not filled for every worker in the SHARE data, making this only suggestive evidence.

Figure 8: Relationship of temporary jobs and cognition



Notes: Own calculations. This figure shows unconditional sample means of *word recall* over time. Time is measured in relative event time. Since this is an analysis of temporary jobs vs. permanent jobs, we do not further discriminate between treatment and control group.

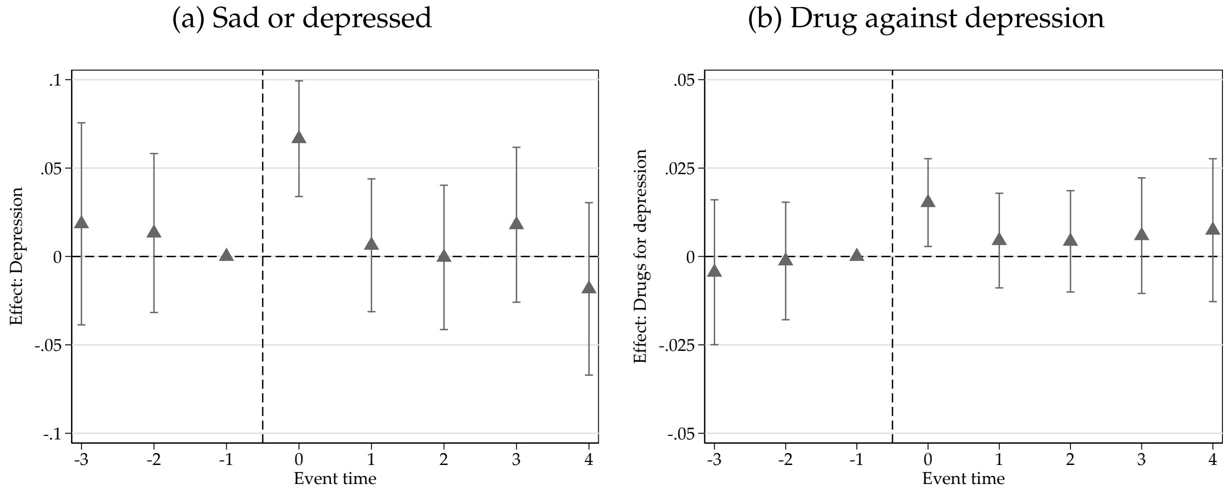
5 Other outcome variables

We now turn to other outcomes variables that either are related to cognitive abilities—measures of mental health—or potentially explain the (absence of) effects—transitions into retirement.

5.1 Mental health

It is sometimes argued that depression may predate cognitive impairment (Celidoni et al., 2017) or at least that both are associated (Panza et al., 2010). Figure 9 reports results of Eq. (1) where the outcome variables now are binary indicators of depression and drug-intake against depression. Specifically, the outcome in the left panel takes on the value one if the respondent states to have been sad or depressed in the previous month and zero otherwise. This variable has a sample mean of 0.35. The outcome in the right panel is a binary indicator of currently taking drugs at least once a week for depression or anxiety. Its sample mean is 0.046. The results suggest a considerable short-term effect of unemployment on depression. Upon unemployment, the probability to report being sad or depressed increases by around seven percentage points. This does not merely seem to be a subjective feeling as the likelihood to take drugs against depression also increases by 1.5 percentage points which, compared to the mean, is a relative increase of around one third. Yet, unemployment does not seem to leave scars as this effect vanishes in $r = 1$ when the majority of individuals in the treatment group is not unemployed anymore.

Figure 9: Depression



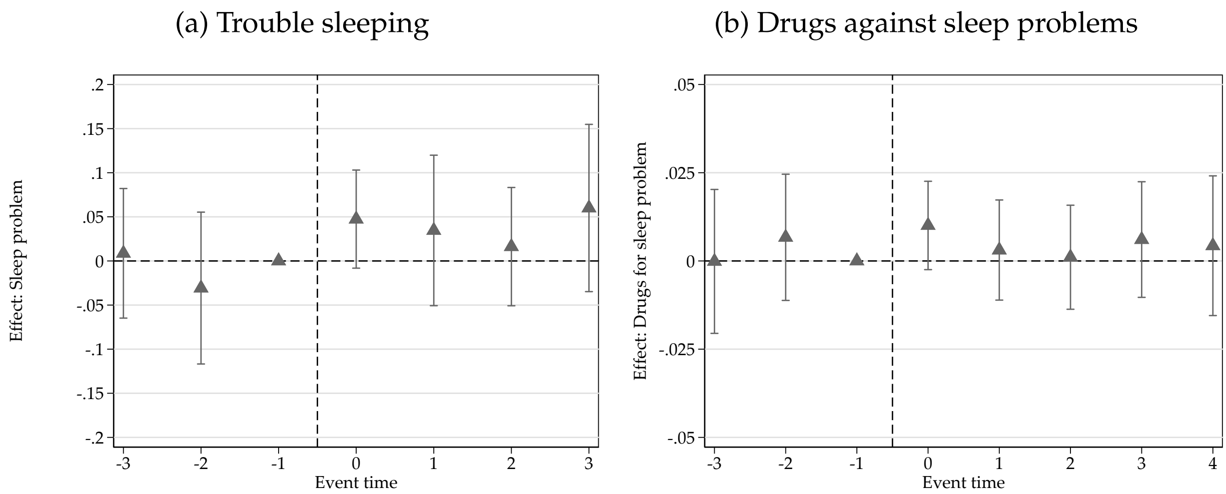
Notes: Own calculations. Treated individuals are those who are unemployed for any reason. Nr. of observations: 61,039 (left), 78,086 (right). Point estimates with 95% confidence intervals based on Eq. (1) where the outcome variables are binary indicators of depression and drug-intake against depression. Reference category: -1. Event time is measured in waves which means that one event period is, on average, two calendar years. Controls: Individual-FE, year-FE, age-FE. Standard errors clustered on individual level.

In Figure 10, the outcome in the left panel is an indicator for sleep problems within the previous six months. It is only available in waves 1, 2 and 4 and has a sample mean of 0.17. The outcome in the right panel is a binary indicator of currently taking drugs at least once a week for sleep problems. Its sample mean is 0.038. Here, the effects are somewhat less clear than in the case of depression. Upon unemployment, the probability to report sleep problems increases by 5.3 percentage points which, however, is only significant at the ten percent level (p-value = 0.057). Yet, this effect is economically large and does not fully turn back to zero afterwards. The likelihood to take drugs for sleep problems goes up by one percentage point. This is statistically insignificant but, in terms of effect size, goes into the same direction of magnitude as the other effects on mental health. All in all the findings on mental health make clear that unemployment does have some effect which, however, seem to be short-term in nature. Moreover, is seems not merely to be an issue of statistical power that the small effects found for cognition are statistically insignificant as effects in other dimensions can indeed be identified.

5.2 Transition after unemployment

Turning back to the discussion started in Figure 3 of Section 2.3, we now take up the topic of transitions after unemployment. This is both interesting in itself and might add to the explanation of the (small to absent) effects of unemployment on cognition. We scrutinize two hypotheses. The first one is that—given that we study an older population and treatment and control group both transit into retirement over time—both groups spend similar times in economic inactivity in our observation window. Compared to retirement,

Figure 10: Sleep



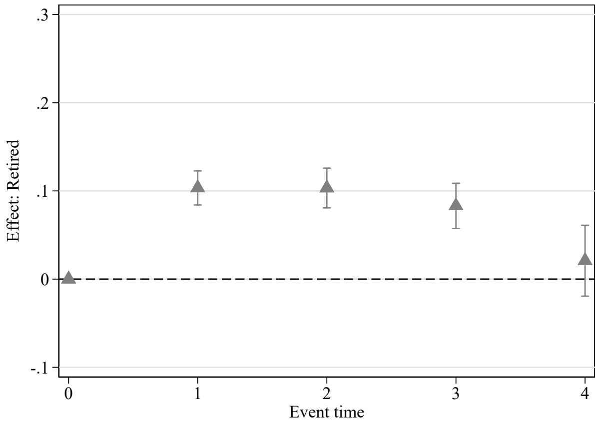
Notes: Own calculations. Treated individuals are those who are unemployed for any reason. Nr. of observations: 13,190 (left), 78,086 (right). Point estimates with 95% confidence intervals based on Eq. (1). Reference category: -1. Event time is measured in waves which means that one event period is, on average, two calendar years. Controls: Individual-FE, year-FE, age-FE. Standard errors clustered on individual level.

the treatment group only collects slightly more time in inactivity due to unemployment and the very small and insignificant short-term effect of unemployment of cognitive abilities does not show up in later periods anymore, when both groups get more similar in terms of employment. The second hypothesis is that the effects of unemployment differ depending on the labor-force participation after unemployment. That is, individuals who turn back to employment might have different effects than those who stay unemployed or even retire. These different effects might cancel out each other, leading to (almost) zero effects on average.

We start with the first hypothesis. Transitions of unemployed individuals into retirement have not been exhaustively studied in the literature. Yet, as early as Bould (1980), studies have found that unemployment is a non-negligible determinant of early retirement, potentially even a voluntary pathway into early retirement García-Pérez et al. (2013). Figure 11 reports results of event study estimations in the baseline sample where, now, retirement is the dependent variable. Following the sample selection criteria, individuals cannot be retired before $r = 1$, hence, "pre-trends" cannot be estimated.

The results are a reflection of Figure 3, where the difference is merely that, here, time-varying controls and individual fixed effects are included. Starting at $r = 1$, we see that treated individuals are ten percentage points more likely to retire. This stays constant until $r = 4$ when the difference between treatment and control group return towards zero. Thus, we can reject the first hypothesis. Even after unemployment spells are over, individuals in the treatment group spend, on average, longer time in economic inactivity than in the control group.

Figure 11: Effect of unemployment on transition into retirement

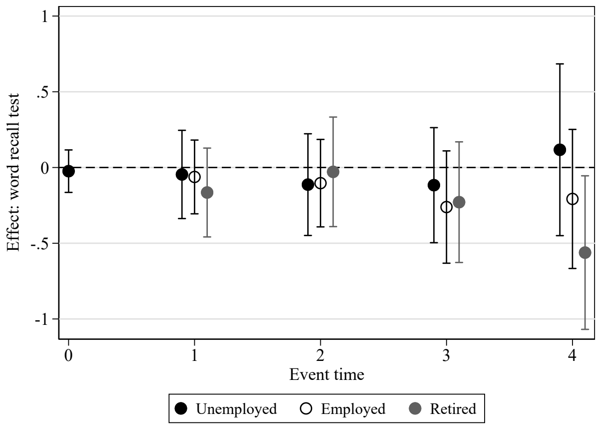


Notes: Own calculations. Treated individuals are those who are unemployed for any reason. No. of observations: 53,447. Point estimates with 95% confidence intervals based on Eq. (1). Event time is measured in waves which means that one event period is, on average, two calendar years. Standard errors clustered on individual level.

We now look at how the labor-force status after unemployment mediates the effect on cognition. We do not claim that this is a clean analysis, since the labor-force status after unemployment is potentially endogenous and a result of its individual-specific effect on cognition. Nevertheless it may help to classify the baseline results. We stratify the analysis in the following way. Remember that everybody in the control group is employed in $r = -1$ and $r = 0$. Everybody in the treatment group is employed in $r = -1$ and unemployed $r = 0$ (or between both waves). Now, we separate the treatment group further into those who are still unemployed in $r = 1$, are back in employment, or are retired in $r = 1$. While it would clearly be interesting to also look at other paths, drastically reduced sample sizes permit this. For instance, recall that only 7 per cent of the unemployed are unemployed for at least three consecutive waves (that is $r = 0$ to $r = 2$ or longer).

Figure 12 shows the results. Keeping in mind problems of precision and exogeneity, a tentative interpretation could be the following one. There do not seem to be (differences in) effects for those who stay unemployed or return to employment. However, individuals who retire after unemployment are on a declining path of cognition and recall 0.5 word less after four waves. While we do not want to overstress the one statistically significant coefficient in our analysis, this result is in line with the effects found in the retirement literature. For instance, Schmitz and Westphal (2021) find effects of retirement on word recall of about -0.1 per year in retirement. While 0.5 after four waves (around 9 years) of economic inactivity are a bit smaller, we should keep in mind that the control group also retires over time and that the treatment group, on average, has around two more waves of inactivity in $r = 4$ than the control group. In this spirit, we argue that our results could be interpreted in the following way: unemployment does not have an immediate effect on cognition among 50-65 years old individuals but leads to retirement being brought

Figure 12: Effect of unemployment on different labor market status after unemployment



Notes: Own calculations. Treated individuals are those who are unemployed for any reason. No. of observations: 76,707. Point estimates with 95% confidence intervals based on Eq. (1) where point estimates are based on interactions with the respective groups. Standard errors clustered on individual level. Pre-trends are not shown here. The groups are defined by labor market status in the wave after unemployment. Since in $r < 0$ no individual is assigned to any of these groups, pre-trends are exactly as those in the baseline results. Results for the group defined by individuals with other status after unemployment are not shown. Controls: Individual-FE, year-FE, age-FE, no. of test repetitions.

forward by a couple of years. This, in turn, has negative effects that phase in only in the mid-run.

Nevertheless, this suggestive evidence of an indirect treatment effect via retirement still does not translate into a sizable total effect of unemployment. If retirement reduces cognition by 0.6 units after six years and the likelihood to retire increases by ten percentage points due to unemployment, the overall effect is 0.06 only. Finally, we note that results in Figure 12 look similar when we restrict the control group to individuals who are also employed in $r = 1$.

6 Conclusion

We use data from the Survey of Health Ageing, and Retirement (SHARE) to study the effect of late-career unemployment on cognitive abilities. To this end, we exploit the panel nature of the data by using event study methods that estimate trajectories of cognitive abilities from three waves prior to four waves after becoming unemployed. We complement this analysis with a plant closure event study where, arguably, the reason for the job loss is more likely exogenous. The results of both analyses are very similar.

Our main finding is the absence of sizeable effects of late-career unemployment on cognitive abilities as measured by a word recall test. Over the course of eight to ten years after the initial start of unemployment, cognitive abilities only deteriorate marginally—individuals lose about from 0.1 to at most 0.2 words of their recall score because of unemployment. This effect is small and does not seem to be moderated by any of the subgroups that we analyze, like age, gender, or region. However, unemployment is mentally stressful, as we find considerable short-run effects on mental health.

The small effect of unemployment on cognition seems plausible as it is similar to the average effect of one year of retirement found in the literature. Our main contribution to the literature is to add to rare evidence on the use-it-or-lose-it hypothesis for individuals younger than 65 years. While our results can be interpreted as good news, we do not claim that effects can be transferred to other measures of cognitive ability or unemployment episodes of persons younger than 50. Moreover, we—as many studies in the literature—find negative effects on mental health, even though they are only short-run effects. Finally, our results suggest that the small but persistent wage effects of job loss found in the literature are probably due to the loss of firm- or job-specific human capital rather than a general decline in cognitive abilities.

A limitation of our study is that we need to evaluate unemployment instead of job loss due to data restrictions. A difference between the two events emerges if individuals with the highest cognitive abilities directly become employed at different firms without ever being unemployed between two consecutive employment spells. However, even if the effects of unemployment may be overestimated in absolute terms, they are already small in our study.

Acknowledgments

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Appendix

A 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.

B Interaction-weighted estimator

The Interaction-weighted (IW)estimator is constructed in three steps (Sun and Abraham, 2021):

First, we estimate the cohort average treatment effects on the treated, called $CATT_{e,r}$, following Equation (3):

$$Y_{it} = \alpha_i + \lambda_t + \sum_e \sum_{r \neq -1} \delta_{e,r} \left(\mathbb{1}\{e_i = e\} \cdot \mathbb{1}\{r = j\} \right) + \varepsilon_{it} \tag{3}$$

where we are interested in its estimators $\widehat{\delta}_{e,r}$ to construct the IW estimator in the end. Units in cohort e are first treated at the same time $i : e_i = e$ and $r = t - e_i$ is the relative period as defined in Section 2.

Second, the weights $Pr\{e_i = e \mid e_i \in [-r, T - r]\}$ are estimated by using the Stata package *eventstudyweights* (Sun, 2021). The relevant coefficients $\omega_{e,r}$ are extracted that represent the sample shares of each cohort in the relative periods r based on estimates of Equation (4):

$$\mathbb{1}\{r = j\} \cdot \mathbb{1}\{e_i = e\} = \alpha_i + \lambda_t + \sum \omega_{e,r} \mathbb{1}\{r = j\} + v_{i,t} \tag{4}$$

In a third step, we finally construct the IW estimator, which is a weighted average of the estimates for $CATT_{e,\ell}$ (step 1) weighted by the cohort shares (step 2). The IW estimate $\widehat{\nu}_r$ for each relative period r results in:

$$\widehat{\nu}_r = \sum_e \widehat{\delta}_{e,r} \cdot \widehat{Pr}\{e_i = e \mid e_i \in [-r, T - r]\} \tag{5}$$

C Further statistics and results

Table C1: Observations per country

Country	Treatment Group	Control Group
Austria	396	2593
Germany	953	6198
Sweden	521	5569
Netherlands	275	1550
Spain	1388	4551
Italy	749	3980
France	913	5243
Denmark	1180	6834
Greece	153	1447
Switzerland	413	4249
Belgium	658	6990
Israel	174	1122
Czech Republic	706	4247
Poland	229	1924
Ireland	6	38
Luxembourg	100	501
Hungary	86	612
Portugal	74	153
Slovenia	618	2001
Estonia	1107	5685
Croatia	150	731
Lithuania	98	695
Bulgaria	72	418
Cyprus	16	82
Finland	65	609
Latvia	49	399
Malta	6	8
Romania	21	108
Slovakia	32	474

Notes: Own calculations, based on the estimation sample.

Table C2: Regression results standard event study

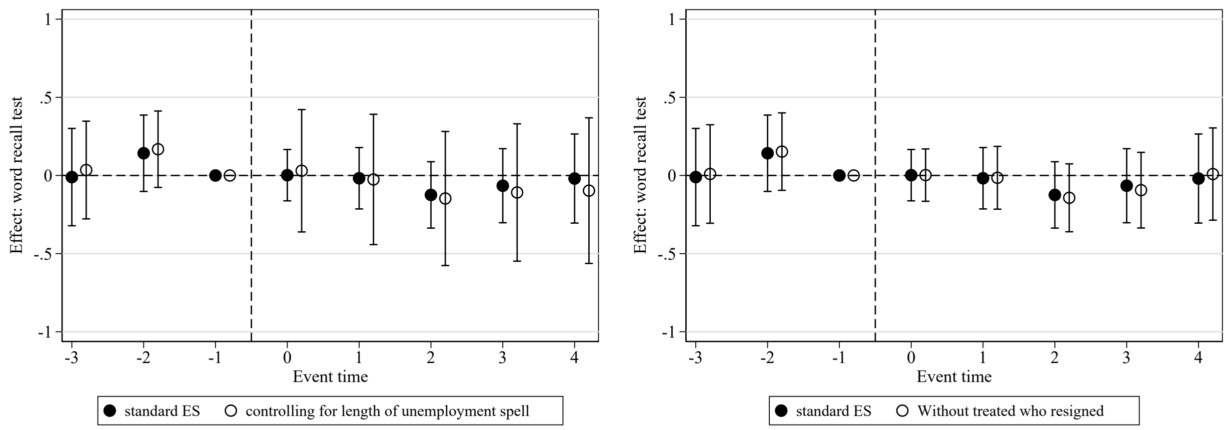
Dep. var: word recall test	Coefficient	Standard error
r=-3	-0.010	(0.159)
r=-2	0.142	(0.125)
r=-1	*Base category*	
r=0	0.002	(0.084)
r=1	-0.018	(0.100)
r=2	-0.125	(0.108)
r=3	-0.066	(0.121)
r=4	-0.020	(0.145)
Age=50	*Base category*	
Age=51	0.014	(0.151)
Age=52	0.151	(0.163)
Age=53	0.194	(0.196)
Age=54	0.257	(0.237)
Age=55	0.445	(0.283)
Age=56	0.409	(0.327)
Age=57	0.542	(0.373)
Age=58	0.557	(0.418)
Age=59	0.618	(0.469)
Age=60	0.660	(0.514)
Age=61	0.666	(0.566)
Age=62	0.703	(0.611)
Age=63	0.818	(0.662)
Age=64	0.767	(0.712)
Age=65	0.832	(0.755)
Age=66	0.641	(0.810)
Age=67	0.807	(0.860)
Age=68	0.904	(0.913)
Age=69	0.726	(0.962)
Age=70	0.633	(1.014)
Age=71	0.616	(1.073)
Age=72	0.792	(1.132)
Age=73	0.446	(1.171)
Age=74	0.406	(1.242)
Age=75	0.786	(1.317)
Age=76	1.351	(1.388)
Age=77	0.885	(1.414)
Age=78	0.304	(1.503)
Age=79	0.143	(1.551)
2004	*Base category*	
2005	-0.213	(0.213)
2006	-0.497**	(0.196)
2007	0.057	(0.185)
2009	-1.593***	(0.476)
2010	-0.791*	(0.414)
2011	-0.170	(0.381)
2012	-0.514	(0.515)
2013	-0.508	(0.495)
2015	-0.786	(0.612)
2017	-1.399*	(0.732)
2018	-9.031***	(0.691)
2019	-1.737**	(0.875)
2020	-1.659*	(0.879)
Test repetitions=0	*Base category*	
Test repetitions=1	0.455***	(0.089)
Test repetitions=2	0.730***	(0.154)
Test repetitions=3	1.041***	(0.218)
Test repetitions=4	1.389***	(0.285)
Test repetitions=5	1.455***	(0.353)
Test repetitions=6	1.436***	(0.431)
Constant	10.189***	(0.184)
Observations	76707	

Notes: Regression results of the event study specification in Figure 4a. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Figure C1: Additional robustness checks

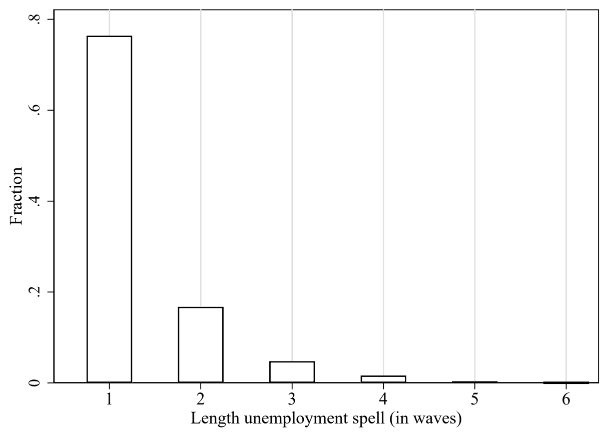
(a) Controlling for the length of the unemployment spell

(b) Excluding treated individuals who resigned



Notes: Own calculations. Treated individuals are those who are unemployed for all reasons (left), excluding individuals who resigned (right). Point estimates with 95% confidence intervals based on Eq. (1). Reference category: -1. Event time is measured in waves which means that one event period is, on average, two calendar years. Controls: Individual-FE, year-FE, age-FE, no. of test repetitions (and length of the unemployment spell in the left panel). Standard ES results are those of Figure 2a, standard ES. Additional information for the left panel: No. of observations: 76,707. Length of unemployment (in waves) included as an additional variable. This variable takes on the value 0 in the control group throughout, and 0 in the treatment group before unemployment. Afterwards, it takes on the current duration of unemployment or, in case of termination of the spell, the total duration of unemployment. Additional information for the right panel: No. of observations: 74,754. Voluntary quitters (6.5% of all unemployed) and their matched control are dropped.

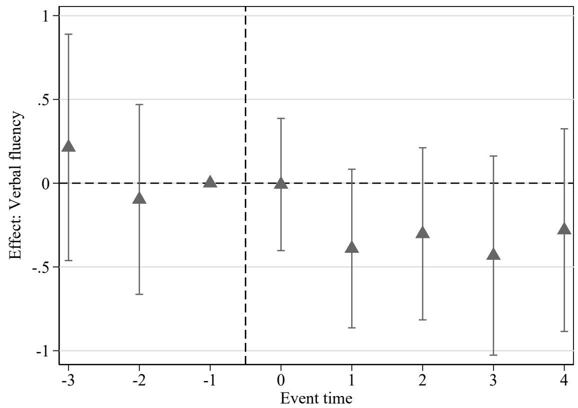
Figure C2: Consecutive waves in unemployment



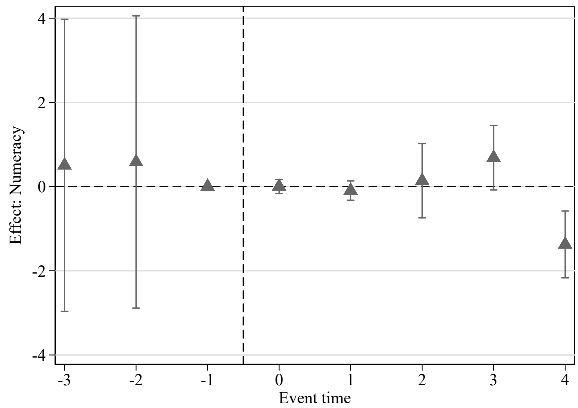
Notes: Own calculations. The graph shows the number of waves, individuals in the treatment group reported to be unemployed.

Figure C3: Event-study results for other outcomes

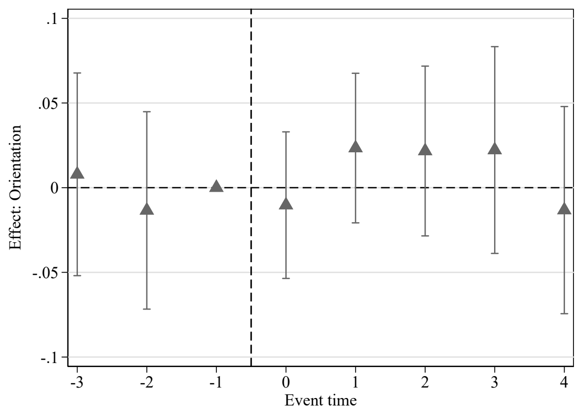
(a) Verbal fluency



(b) Numeracy



(c) Orientation



Notes: Own calculations. Treated individuals are those who are unemployed for all reasons. No. of observations: 60,617 (panel (a)), 6,820 (panel (b)), 45,226 (panel (c)). Point estimates with 95% confidence intervals based on Eq. (1) with varying outcome Y_{it}: *Verbal fluency* (panel (a)), *Numeracy* (panel (b)), *Orientation-to-date* (panel (c)). Reference category: -1. Event time is measured in waves which means that one event period is, on average, two calendar years. Controls: Individual-FE, year-FE, age-FE, no. of test repetitions.