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Abstract

We study the relationship between education and cognitive functioning at older ages by exploiting compulsory schooling reforms, implemented in six European countries during the 1950s and 1960s. Using data of individuals aged 50+ from the Survey of Health, Aging and Retirement in Europe (SHARE), we assess the causal effect of education on old-age memory, fluency, numeracy, orientation and dementia. We find a positive impact of schooling on memory. One year of education increases the delayed memory score by about 0.3, which amounts to 16% of the standard deviation. Furthermore, for women, we find that more education reduces the risk of dementia.

JEL Classification: I21, J14
Keywords: Compulsory schooling, Instrumental Variables, Education, Cognitive functioning, Memory, Aging, Dementia

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1 Introduction

Population ageing in Europe could pose challenges to the sustainability of national social security and health systems. The burden of the demographic change is likely to be determined by age-specific physical health and mental functioning which determines economic activity and dependency status, rather than the demographic age structure per se (Sanderson and Scherbov, 2005; Skirbekk et al., 2012). Cognitive performance is of growing importance for work productivity and it is likely to affect whether pension reforms aimed to raise the retirement age will be effective (OECD, 2006; Warr, 1994). Mental functioning is also important for elderly's activity levels and well-being (Engelhardt et al., 2010; Lindenberger and Ghisletta, 2009; Maurer, 2011; Schmidt and Hunter, 2004). Increases in the share of seniors could worsen average cognition levels and lead to an increase in the incidence of dementia (Brookmeyer et al., 2007; Mura et al., 2010; Salthouse, 2010), unless cognitive performance among later born cohorts increases sufficiently to offset the negative effects of population ageing on cognitive abilities (Nisbett et al., 2012).

Finding ways to improve cognition for new generations of seniors is of central importance to ageing economies. The current study addresses to which extent schooling improves cognitive performance among seniors. It is doubtful whether simple correlations between schooling and cognitive performance can recover causal effects because - as cognitive functioning of individuals is highly correlated across time - they may pick up a reverse causation from high cognitive performance in childhood on school attainment. Therefore, we use compulsory schooling reforms implemented in six European countries in the 1950s-1960s as natural experiments to identify the causal impact of schooling on cognitive outcomes at older ages. We use data of individuals born between 1939-1956 who participated in the Survey of Health, Aging and Retirement in Europe (SHARE), a longitudinal survey focusing on living conditions of individuals aged 50+ in several European countries. Instrumental-Variable regressions are used to identify the impact of schooling on old-age memory, verbal fluency, numeracy, orientation and dementia.

This paper proceeds as follows. Section 2 gives a literature review on the relationship between education and cognitive functioning. The econometric model is presented in section 3 and section 4 describes the data. Section 5 presents the baseline results, discusses heterogeneities in the effect of schooling on old-age cognition, comprises a sensitivity analysis and discusses possible channels through which schooling might influence cognitive function. Section 6 concludes.

2 Education and Cognitive Functioning

Education has been found to be positively associated with outcomes at older ages, particularly cognition (Richards and Hatch, 2011; Yount, 2008). Although it is well documented that more education is related to better cognition, causal effects are difficult to identify as education is influenced by many unobserved characteristics that influence cognitive outcomes, such as ability, cognitive performance in childhood or the socio-economic characteristics of the home environment (Neisser et al., 1997; Nisbett, 2009).

Longitudinal studies, controlling for initial ability, show that education and mental activity in youth and adult life are related to a greater cognitive performance (Deary et al., 2004; Husén and Tuijnman, 1991; Whalley and Deary, 2001). Studies of monozygotic twins, in which within-pair variation in education is investigated to find the causal effect of schooling on cognition suggest a positive relationship as well (Haworth et al., 2008).

Education can affect cognition through several pathways, including lifestyle choices, health behaviors, social interactions, labor-force participation, types of occupation and brain development (Cagney and Lauderdale, 2002; Nisbett et al., 2012; Schooler et al., 1999). Studies have shown that the improvements in mental performance following training are also related to changes in brain structures, affecting synaptic density, hippocampal volumes and cortical thickness (Katzman, 1993; Mårtensson et al., 2012).

The education-cognition relationship has also been studied by exploiting natural experiments, such as extensions in mandatory years of education. Variation in individual years of education is used that is not related to innate ability but prescribed by the law. Brinch and Galloway (2012) investigated the lengthening of compulsory schooling from 7 to 9 years, which was gradually implemented in Norwegian municipalities between 1955-1972. The authors conclude that the effect of one additional lower secondary school year is a rise in IQ for these young men by 3.7 points, which is similar in magnitude to results from several other studies (Cascio and Lewis, 2006; Falch and Massih, 2011).

Using similar methodologies, two studies exist that focus on the effects of schooling on cognitive outcomes at older ages, i.e. many years after school completion. Glymour et al. (2008) exploit state compulsory schooling laws in the United States between 1907 and 1961. The mandatory schooling laws in the United States had only little effect on completed education; one additional year of compulsory schooling increased actual years of education only by around 0.04 years. However, Separate-Sample Instrumental-Variables estimates show significant effects of education on memory scores but not on mental status.

Banks and Mazzonna (2012) investigate the reform of compulsory schooling in England in 1947, where the minimum school-leaving age was raised from 14 to 15. Based on the analysis of data on English seniors from the English Longitudinal Study on Ageing (ELSA), the authors find that education increases old-age memory scores for males and females and executive functioning for English males.

We conduct an analysis using data of European seniors from the Survey of Health, Aging and Retirement in Europe (SHARE). We exploit compulsory schooling reforms in the 1950s-1960s in Austria, the Czech Republic, Denmark, France, Germany and Italy. The multi-country set-up and the gradual implementation of the lengthening of mandatory schooling in the different countries allows us to control for cohort-fixed effects in the empirical specification. This is important, since the level of compulsory schooling is assigned to individuals based on birth years and age-groups differ in their cognitive performance. Furthermore, while the other studies focus on a limited set of outcome variables, our study is more comprehensive since we investigate immediate and delayed memory, verbal fluency and numeracy on the one hand and variables capturing basic cognitive function, such as orientation to date and the chronic condition dementia, on the other hand.

3 Empirical Strategy

To identify the causal effect of education on cognitive outcomes many years after school completion, we use the exogenous variation in individual years of schooling induced by compulsory schooling reforms in 6 European countries (Austria, the Czech Republic, Denmark, France, Germany and Italy). Within each country, we relate the variation in cognitive outcomes of different cohorts to their education level, which differs because individuals experienced different lengths of compulsory schooling. Our Instrumental Variable is the number of compulsory schooling years given by law, which varies over cohorts within each country and across countries for any given cohort. The variation over cohorts and countries allows us to control for country-fixed effects as well as cohort-fixed effects. Country-fixed effects estimations filter out unobserved characteristics that are shared by all individuals in a given country. Similarly, cohort-fixed effects capture effects on cognition that are shared by all individuals born in given year or similarly have the same age at the time of the interviews. Furthermore, within each country we capture trends over cohorts or age-effects in cognition with country-specific linear trends.

We estimate the following two equations

$$Y_{ick} = \beta_0 + \beta_1 Edu_{ick} + \beta_2 \mathbf{X}_{ick} + \beta_3 Country_c + \beta_4 Cohort_k + \beta_5 CTrend_{ck} + \epsilon_{ick} \quad (1)$$

$$Edu_{ick} = \alpha_0 + \alpha_1 Comp_{ck} + \alpha_2 \mathbf{X}_{ick} + \alpha_3 Country_c + \alpha_4 Cohort_k + \alpha_5 CTrend_{ck} + \nu_{ick} \quad (2)$$

where Y_{ick} is the dependent variable capturing cognitive achievement of individual i in country c of birth cohort k. Edu_{ick} is the number of years the individual spent in education and $\mathbf{X}_{ick}$ is a vector of control variables. $Country_c$ and $Cohort_k$ refer to country and cohort-fixed effects and $CTrend_{ck}$ capture country-specific linear trends in birth cohorts.[1]

Since ϵ_{ick} might be correlated with education, we estimate equation (1) by Two Stage Least Squares (2SLS), instrumenting individual years of education with $Comp_{ck}$, the compulsory years of schooling in the respective country and cohort. Equation (2) is the first stage equation and shows the impact of compulsory schooling on individual years of education.

As discussed above, in Equation 1, we control for unobservable characteristics affecting cognition that differ between countries (country-fixed effects) and among different birth cohorts over all countries (cohort-fixed effects). Within each country we control for cohort (or age) trends in cognitive performance since trends in cognition might differ between the different countries. These trends are country-specific and should account for societal changes that either evolve slowly over time (like reading habits or changes in health systems) or change at once (like the introduction of TV in a country) but exert an influence on all persons regardless of their cohort and age.

An Instrumental-Variables strategy is *internally valid* if the instrument is randomly assigned, i.e. individuals before and after the reforms do not differ, and if the

[1] The vector $\mathbf{X}_{ick}$ includes a female dummy variable, an indicator variable of whether a person is born abroad, indicators for the interview year and some control variables for the quality of the interview session (interviewers perception of whether something may have impaired the respondents' performance on the tests and whether another person was present during the interview).

exclusion restriction is fulfilled, i.e. the instrument influences cognitive outcomes only via the impact on years of education (Angrist et al., 1996). We are confident that compulsory schooling satisfies these conditions, in particular in combination with the fixed-effects approach.

However, the identifying assumptions become more plausible when the width of the window around the pivotal cohort, i.e. the first cohort affected by the new level of compulsory schooling, is small. This means the comparison between individuals assigned to the new mandatory schooling obligations and individuals not assigned to the new regulations is local. Smaller windows have the advantage that comparisons are more likely valid and the disadvantage that sample sizes get smaller. Therefore, we estimate our model with different samples based on different widths of windows around the pivotal cohort (10 years prior and 10 years after the pivotal cohort, ±7 years as well as ±5 years).

Concerning the *external validity* of our estimates, we again refer to Angrist et al. (1996) and interpret our estimates as Local Average Treatment Effects, i.e. the effect of years of education on cognitive outcomes for those individuals who were actually influenced and changed their behavior due to the compulsory schooling reforms. Those individuals with a strong preference for higher education might not have been influenced by these reforms. Therefore, our estimates might not apply to the whole population but to those individuals at the bottom of the education distribution.

Table 1 lists the countries and reforms we consider in this paper, presenting the time of the reform, the changes in years of mandatory schooling prescribed by law, the implied changes in the mean school-leaving ages as well as the pivotal cohort, i.e. the first cohort potentially affected by the reforms. For a short description of each reform and the explanation of the choice of the pivotal cohorts see the Appendix.

Several studies have investigated mandatory schooling reforms in Europe. Brunello et al. (2009) investigated compulsory schooling reforms in 12 European countries and

Table 1: Compulsory Schooling Reforms

Country	Reform	Mandatory years of schooling	School-leaving age	Pivotal cohort
Austria	1962/66	8 to 9	14 to 15	1951
Czech Republic	1960	8 to 9	14 to 15	1947
Denmark	1958	4 to 7	11 to 14	1947
France	1959/67	8 to 10	14 to 16	1953
Germany:				
Northrhine-Westphalia	1967	8 to 9	14 to 15	1953
Hesse	1967	8 to 9	14 to 15	1953
Rhineland-Palatinate	1967	8 to 9	14 to 15	1953
Baden-Wuerttemberg	1967	8 to 9	14 to 15	1953
Italy	1963	5 to 8	11 to 14	1949

found that education increases wages and reduces wage inequality. Furthermore, Fort et al. (2011) used the reforms to investigate the relationship between education and fertility decisions and Brunello et al. (2011) and Brunello et al. (2012, forthcoming) study the effects of education on health and the body mass index.

4 Data

We pool data of individuals participating either in the first, in the second or in both waves of the Survey of Health, Ageing and Retirement in Europe (SHARE). We primarily use cognitive tests obtained in wave 1 (interviews in 2004/05) to avoid distortions by any retest effects. A retest effect represents a test bias that results from having done the same or a similar test in a previous wave of the survey. This includes the recognition of test questions, a shorter ”warm-up” phase, the familiarity with the test situation, fewer procedural errors and less nervousness during the testing (Salthouse, 2010; Thorvaldsson et al., 2006). Only for information about dementia, which is not based on testing but a chronic disease that has to be diagnosed by a doctor and thus not susceptible to a retest bias, we are using wave 2.[2] For those

[2]Dementia was not asked in wave 1 of SHARE.

respondents with missing cognitive tests in the first wave, we use data from wave 2 (the interview in 2006/07). We also include records of individuals only interviewed in the first wave or the second wave. The longitudinal individuals represent roughly 39% of our sample, while around 18% participated only in the first wave and dropped out afterwards and around 43% joined the survey at the time of the second wave.[3]

We use only records of individuals aged 45 or above who were born in the country of residence or migrated before the age of five to ensure that they went to school in the host country at least at the early stages of their school career, i.e. when they were eligible for the changes induced by the compulsory schooling reforms.[4] We select a baseline sample of individuals born between 1939 and 1956, just a few years around the pivotal cohorts. We exclude records with missing information on our key variable, the number of years of education. This information is missing or can not be calculated from the educational attainment categories for 141 out of 9,820 individuals. From our baseline data-set of 9,679 respondents, we gradually reduce the samples around the reforms in each country to individuals born up to 10 years before and after (Sample 10), 7 years before and after (Sample 7) as well as 5 years before and after each pivotal reform cohort (Sample 5). The final samples consist of 8,994, 7,023 and 5,387 respondents, respectively.

We measure educational attainment with individual years of education. While the second wave of SHARE provides information on the number of years spent in full time education, in the first wave the respondents were asked about their educational degrees. Thus, we use the second wave information on years of schooling for all individuals that participated in the second wave. For those individuals who only participated in the first wave, we calculate their years of education using country-specific conversion tables provided by SHARE.

[3] Sample attrition between the two waves of SHARE is no problem in our study because all individuals that appear at least once in the survey are included in our sample.

[4] While the survey was targeted at individuals aged 50+ only, cohabiting partners in the same household were interviewed even if they were younger at the time of the interview.

Table 2: Descriptive Statistics of Baseline Sample

			Years of Education		Memory			Numeracy /	Orientation /	Dementia	
Country	Female	Age	individual	compulsory	immediate	delayed	Fluency	Good numeracy	Good orientation		Obs
Austria	0.56	58.28	10.76	8.21	5.55	3.99	23.23	3.82 / 0.71	3.87 / 0.89	0.0027	1,033
Czech Republic	0.56	58.88	11.96	8.58	5.35	3.64	20.39	3.67 / 0.62	3.83 / 0.88	0.0039	1,803
Denmark	0.52	57.28	12.04	5.79	5.91	4.69	23.69	3.72 / 0.57	3.88 / 0.90	0.0031	1,844
France	0.54	56.97	12.04	8.45	5.17	3.73	22.20	3.45 / 0.53	3.81 / 0.89	0.0034	1,924
Germany	0.53	57.62	13.46	8.16	5.91	4.29	23.51	4.00 / 0.76	3.89 / 0.92	0.0017	920
Italy	0.55	58.25	8.78	6.08	4.79	3.30	16.11	3.15 / 0.32	3.88 / 0.90	0.0017	2,155
All	0.54	57.87	11.3	7.39	5.37	3.88	21.02	3.57 / 0.55	3.86 / 0.89	0.0029	9,679

NOTES: Sample includes individuals born 1939-1956, participating in the first/second wave of SHARE, who reported their years of education, who were 45+ at the time of the interview, who were born in the country or migrated before age 5. Gender, age and years of education are available for all 9,679 respondents. The memory scores are missing for around 1.3% of the sample, fluency is missing for 1.8%, numeracy for 0.7%, orientation for 0.4% and dementia was only asked in the second wave of SHARE, and is only available for 7,944 out of 7,960 respondents.

Table 2 reports descriptive statistics on key variables in the sample used for the baseline estimations, the sample consisting of birth cohorts born 1939-1956. A bit more than half of the sample is female and the mean age is around 58 years. On average, the individuals completed 11 years of education, ranging from around 9 in Italy to around 13 in the four German states listed in Table 1.

We measure various domains of cognitive functioning, such as memory, fluency, numeracy, orientation to date as well as the medical condition of dementia. Our measures of cognitive functioning are based on the following tests:

Memory (immediate and delayed): The interviewer reads a list of 10 words. Immediate memory measures how many of these 10 words the respondent is able to recall directly after the interviewer read the words. Delayed memory measures the ability of the respondent to recall the same words after a time period of around 5-10 minutes (after several other interview questions). On average, the individuals in our sample are able to recall around 5 words immediately and 4 words after a certain time period.

Fluency: The verbal fluency score is the sum of animal names the respondent is able to state within the time of one minute. The mean value is around 21.

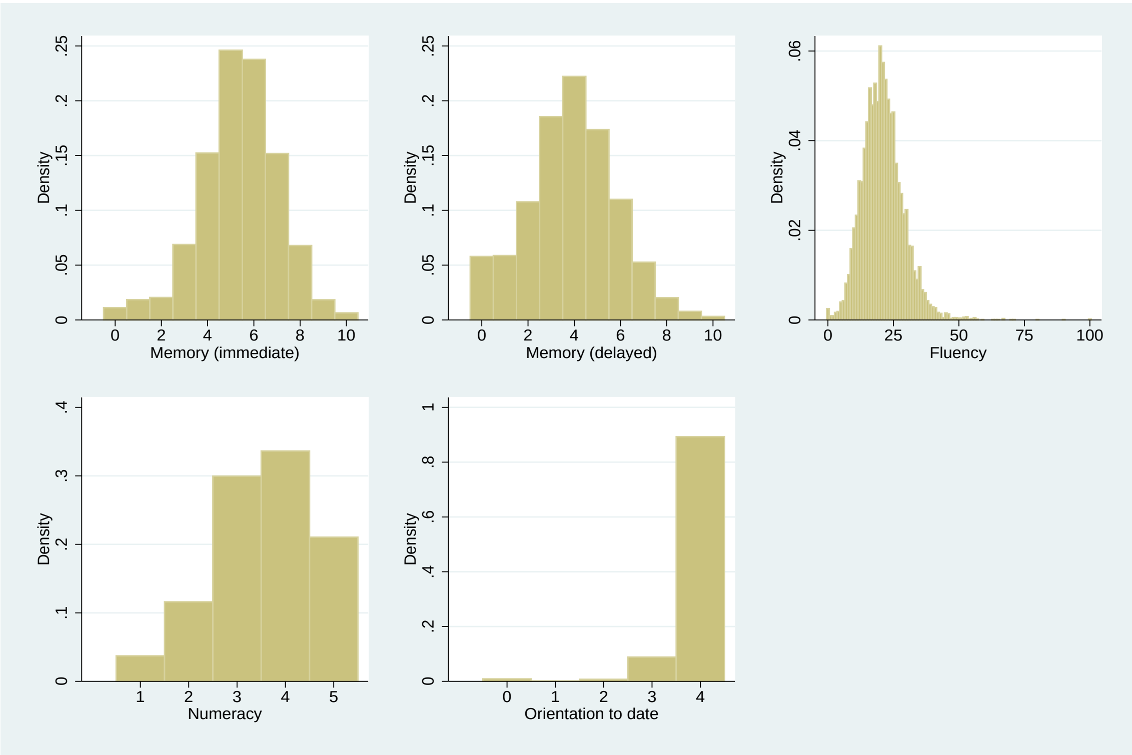
Numeracy: This score ranges from one to five (high score) and is based on the ability of the respondent to answer basic as well as more advanced mathematical questions from daily life, ranging from estimating simple mathematical relations to compound interest calculations. The average numeracy score is around 3.6.

Orientation to date: This variable ranges from zero to four and measures if a person is able to remember the correct date consisting of day of the month, month, year and day of the week. 3.9 is the average score in our sample.

Dementia is based on the outcome of the following question: Has a doctor ever told you that you had/currently have Alzheimer's disease, dementia, organic brain syndrome, senility or any other serious memory impairment? Around 0.3% of all individuals in our sample suffer from such a chronic disease.

Figure 1 shows the distribution of our measures of cognitive functioning. Both memory scores and verbal fluency follow approximately normal distributions around their means. Numeracy and orientation have larger densities at the upper tail of the distributions, with 55% achieving either the highest or the second-highest value of numeracy and 89% showing a perfect orientation to date. In our empirical specifications, we treat immediate memory, delayed memory and fluency as continuous variables but condense the information for numeracy and orientation into binary indicators. "Good numeracy" is defined to be one for individuals who achieve numeracy scores of four and five and "Good orientation" is defined to be one for individuals scoring four on the orientation variable. Table 2 also reports mean values of these binary indicators.

Figure 1: Measures of Cognitive Functioning



5 Results

In this section, we discuss the results of our baseline estimates and analyze whether the effects are heterogenous with respect to gender and family background. We give a sensitivity analysis and discuss possible channels through which education might influence cognitive decline.

5.1 Main results

We start by looking at the effects of compulsory schooling on actual years of education (first stage). The first stage is shown graphically in Figure 2. The graph shows mean years of education of cohorts just before and after the different reforms. In this graph all countries are normalized by the time of the reform which is set at time zero. The graph shows a jump in the mean years of education at the time of the reforms, suggesting that the reforms had a substantial impact. This is corroborated by the results of the first stage regressions in Table 3: increasing compulsory education by one year leads to one third of an actual additional year of schooling on average. This is a sizeable effect; typically only individuals at the lower end of the educational distribution react to compulsory schooling reforms.[5]

[5] The first stage coefficients are similar in magnitudes to those obtained in other studies investigating compulsory schooling laws in various European countries, such as Brunello et al. (2009), Brunello et al. (2011); Fort et al. (2011), Brunello et al. (2012, forthcoming).

Table 3: First Stage Regressions

	Years of education			
	Baseline	Sample 10	Sample 7	Sample 5
Compulsory schooling	0.329	0.329	0.326	0.338
	(0.052)***	(0.052)***	(0.058)***	(0.072)***
F-Statistics	40.70	40.36	31.38	22.30
Observations	9,679	8,994	7,023	5,387

NOTES: Each coefficient represents a separate linear regression. Country-fixed effects, cohort-fixed effects, country-specific linear trends in birth cohorts, indicators for interview year, foreign born, female and indicators for the interviewers perception on whether something may have impaired the respondents performance on the tests and whether another person was present during the interview are included in all regressions. Heteroscedasticity and cluster-robust standard errors in parentheses (clusters are country-cohorts). ***, ** and * indicate statistical significance at the 1-percent, 5-percent and 10-percent level.

Figure 2: First Stage

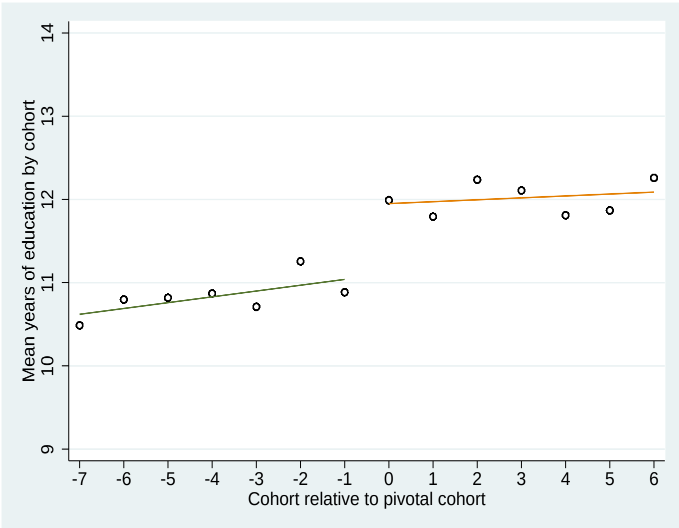


Table 4 shows our main results, starting with Ordinary Least Squares (OLS) estimates in Panel A, Reduced Form effects (the effect of compulsory schooling on our outcome variables, also called Intention-To-Treat effects) in Panel B and Two Stage Least Squares (2SLS) results in Panel C.

Almost all OLS estimates show a clear positive association between education and cognitive functioning at older ages. Due to potential omitted interfering variables, these associations cannot be taken as causal effects. We proceed with our 2SLS estimates which present causal effects of education on cognitive functioning for those individuals who increased their educational attainment due to the compulsory schooling reforms in the various countries.

There is a clear and robust causal effect of education on immediate memory and even more so on delayed memory. These effects are (with one exception) robust and statistically significant across our different specifications; the smaller the sample we have chosen around the pivotal cohort, the larger the quantitative effect. Using the sample with 5 years before and after the reform, we find that one additional year of schooling increases immediate memory by 0.26 words (out of ten possible) and by 0.37 words in the case of delayed memory. These effects amount to around 15%/19% of the standard deviation in the immediate/delayed memory scores in the sample.

There are no causal effects of education on fluency and numeracy; no statistically significant coefficients are obtained. One potential reason why we find gains in memory function but not in the fluency could be that the fluency test is based on naming animals, a measure which could be less affected by extensions to secondary school levels, as this may be focused more in lower levels of instruction, such as kindergarten or primary school. Further, the lengthening of schooling could reduce the probability of working with animals (e.g. in agricultural occupations) or residing in rural areas with more animals around, thus reducing the knowledge of animals. On the other hand, the test could measure executive functioning or the ability to organize ones thoughts which may improve ones ability to reply to the question in an organized manner (eg. first naming livestock, then birds, thereafter wildlife). However, our results are in line with Banks and Mazzonna (2012), who studied the compulsory schooling reform in England and found significant effects of education on memory but generally no effects for executive functioning, except for males with low education.

Gains to immediate and delayed recall may result from the fact that schooling is universally aimed at improving these skills, as learning how to remember new material is universally essential for schooling success. Education is likely to aim

Table 4: Baseline Results

	Memory Immediate	Memory Delayed	Fluency	Good Numeracy	Good Orientation	Dementia
A: OLS						
Baseline	0.116	0.120	0.521	0.034	0.005	-0.00014
	(0.005)***	(0.006)***	(0.025)***	(0.001)***	(0.001)***	(0.00016)
Observations	9,556	9,563	9,505	9,608	9,643	7,944
B: Reduced Forms						
Baseline	0.033	0.048	-0.041	0.005	0.004	-0.00163
	(0.029)	(0.028)*	(0.099)	(0.006)	(0.004)	(0.00085)*
Observations	9,556	9,563	9,505	9,608	9,643	7,944
Sample 10	0.041	0.053	0.009	0.004	0.004	-0.00149
	(0.028)	(0.028)*	(0.097)	(0.006)	(0.004)	(0.00088)*
Observations	8,875	8,882	8,827	8,927	8,960	7,435
Sample 7	0.066	0.092	-0.061	0.004	0.009	-0.00066
	(0.033)**	(0.030)***	(0.091)	(0.009)	(0.004)*	(0.00090)
Observations	6,924	6,931	6,891	6,971	6,997	5,779
Sample 5	0.089	0.125	-0.113	-0.004	0.003	-0.00097
	(0.034)**	(0.035)***	(0.121)	(0.008)	(0.005)	(0.00092)
Observations	5,308	5,314	5,283	5,343	5,366	4,370
C: 2SLS						
Baseline	0.098	0.143	-0.121	0.015	0.012	-0.00413
	(0.076)	(0.075)*	(0.296)	(0.017)	(0.011)	(0.00207)**
Observations	9,556	9,563	9,505	9,608	9,643	7,944
Sample 10	0.122	0.157	0.027	0.011	0.013	-0.00359
	(0.072)*	(0.073)**	(0.284)	(0.018)	(0.011)	(0.00201)*
Observations	8,875	8,882	8,827	8,927	8,960	7,435
Sample 7	0.203	0.281	-0.185	0.014	0.026	-0.00161
	(0.077)***	(0.072)***	(0.289)	(0.024)	(0.012)**	(0.00208)
Observations	6,924	6,931	6,891	6,971	6,997	5,779
Sample 5	0.264	0.373	-0.331	-0.011	0.010	-0.00217
	(0.072)***	(0.083)***	(0.389)	(0.027)	(0.012)	(0.00198)
Observations	5,308	5,314	5,283	5,343	5,366	4,370

NOTES: Each coefficient represents a separate linear regression. Panel A gives OLS estimates of years of education on cognition, panel B gives estimates of compulsory schooling years on cognition and panel C shows IV estimates of years of education on cognitive outcomes. Country-fixed effects, cohort-fixed effects, country-specific linear trends in birth cohorts, indicators for interview year, foreign born, female and indicators for the interviewers perception on wether something may have impaired the respondents performance on the tests and whether another person was present during the interview are included in all regressions. Heteroscedasticity and cluster-robust standard errors in parentheses (clusters are country-cohorts). ***, ** and * indicate statistical significance at the 1-percent, 5-percent and 10-percent level.

towards improving strategies for encoding and organizing new information and to increase one's ability to remember.

The lack of effects on numeracy could partly be due to the fact that there is a very high correct share of responses, which may indicate that a "ceiling effect" is reached, where the educational expansions we consider will not affect responses to this basic mathematical measure. Moreover, the skills learned at the relevant school level may not be relevant for the numerical test given. Conducting basic mathematical operations is a skill needed in basic household work and for both low and high skilled occupations. Education is not necessarily related to the use of such skills.[6]

The 2SLS coefficient of education on good orientation is positive and consistent across specifications but only marginally significant in one out of four cases. In a similar vein, the prevalence of dementia is reduced by an additional year of education by 0.0016 - 0.004 percentage-points. These effects are statistically insignificant in the smaller samples.[7] Given an average prevalence rate of 0.003 in the full sample, these effects are remarkable and suggest that education is causally related to a postponement of dementia.

5.2 Heterogenous effects

In this section, we explore whether the IV-estimates of education on cognitive performance vary by gender and family background. Panel A of Table 5 gives 2SLS coefficients for Sample 10 and 7 for males and females, separately.

[6]Note that we neither do find significant coefficients when the numeracy score is treated as continuous variable or the cut-off of 'good numeracy' is set at a lower level.

[7]As dementia is only measured in wave two of SHARE, we have a smaller sample for this outcome.

Table 5: Heterogeneous effects by gender and family background (2SLS)

	Memory			Good	Good	
	Immediate	Delayed	Fluency	Numeracy	Orientation	Dementia
A: By Gender						
Males						
Sample 10	0.156	0.111	-0.363	0.039	0.002	0.003
	(0.119)	(0.132)	(0.581)	(0.025)	(0.018)	(0.003)
Observations	4,018	4,024	3,991	4,051	4,069	3,341
Sample 7	0.302	0.281	-0.305	0.055	0.019	0.005
	(0.123)**	(0.128)**	(0.595)	(0.031)*	(0.020)	(0.003)
Observations	3,136	3,142	3,120	3,166	3,180	2,595
Females						
Sample 10	0.100	0.194	0.372	-0.016	0.025	-0.010
	(0.101)	(0.089)**	(0.462)	(0.026)	(0.016)	(0.005)**
Observations	4,857	4,858	4,836	4,876	4,891	4,094
Sample 7	0.097	0.277	-0.208	-0.034	0.033	-0.010
	(0.131)	(0.098)***	(0.541)	(0.036)	(0.020)	(0.005)*
Observations	3,788	3,789	3,771	3,805	3,817	3,184
B: By Family background						
Few books						
Sample 10	0.124	0.236	-0.528	0.026	0.035	-0.006
	(0.206)	(0.134)*	(0.733)	(0.040)	(0.028)	(0.004)
Observations	3,284	3,286	3,270	3,296	3,303	3,139
Sample 7	0.281	0.324	-0.342	0.023	0.060	-0.006
	(0.197)	(0.151)**	(0.763)	(0.046)	(0.034)*	(0.004)
Observations	2,543	2,545	2,534	2,551	2,558	2,430
Many books						
Sample 10	0.133	0.080	0.019	-0.078	0.035	-0.001
	(0.120)	(0.124)	(0.659)	(0.049)	(0.021)*	(0.003)
Observations	2,309	2,314	2,303	2,324	2,329	2,239
Sample 7	0.182	0.106	-0.399	-0.073	0.020	0.003
	(0.166)	(0.169)	(0.998)	(0.060)	(0.027)	(0.005)
Observations	1,809	1,814	1,805	1,824	1,825	1,748

NOTES: Each coefficient represents a separate linear regression. Panel A gives 2SLS estimates of years of education on cognitive outcomes by gender. Panel B gives estimates by parental background measured by the number of books at home when age 10. Few books are 0-10 and 11-25 books and many books means 26-100, 100-200 and more than 200 books. Country-fixed effects, cohort-fixed effects, country-specific linear trends in birth cohorts, indicators for interview year, foreign born and indicators for interview impairments and whether another person was present during the interview are included in all regressions. Female dummy included in panel B. Heteroscedasticity and cluster-robust standard errors in parentheses (clusters are country-cohorts). ***, ** and * indicate statistical significance at the 1-percent, 5-percent and 10-percent level.

Similar to the baseline results, we find the strongest and most significant effects for delayed memory, with similar coefficients for males and females. For immediate memory, the coefficients show the same signs as they do in the baseline estimates but most coefficients are statistically not significant anymore. For fluency, we again find no causal impact of education, neither for males, nor for females. For good numeracy, we obtain heterogeneous effects by gender. While for males the effects are positive and statistically significant at the 10 percent-level with the smaller sample, the coefficients are basically zero for females. No significant results are obtained for good orientation. Interestingly, heterogenous effects are found for dementia. While the effects are basically zero for males, the coefficients for females are statistically significant and larger in magnitude. All of the results reported in Panel A of Table 5 are similar, when the baseline sample and Sample 5 are used to estimate the model.[8]

Panel B contains 2SLS estimates by parental background. At the time of the third wave of SHARE in 2008/09, the survey incorporated a retrospective interview, called SHARELIFE. Thus, the third wave contains information on childhood circumstances of the respondents. Amongst others, the respondents were asked about the number of books at home when they were 10 years old, i.e. before the treatment of extended compulsory schooling. At the basis of this variable, we split our sample into two parts, one sample for individuals with few books at home (0-10, 11-25 books; around 59%) and one sample for individuals with many books at home (26-100, 101-200, more than 200 books; around 41%). Table 5 contains the analysis for these two groups, separately.[9]

Generally, we find stronger and statistically more significant results for the group with few books at home. Compared to respondents from more affluent families, they experience high returns to schooling when it comes to delayed memory. For the other measures of cognitive performance, the coefficients are mostly not statistically significant, however the coefficients on good orientation and dementia are similar to

[8]The first stage regressions by gender show similar results, with slightly larger coefficients for males.

[9]Note, that the sample size is somewhat smaller in Panel B because only individuals who participated also in the third wave of SHARE are included (around 63%). However, it seems that this attrition does not bias the results because we obtain the same results as in Table 4 when we do the estimations for the smaller sample.

the baseline results when considering the group with less-favorable parental background.[10]

5.3 Robustness

In the estimations above, we control for country-specific trends in birth cohorts that are linear. Since treatment and control groups (cohorts after and before the reforms) differ in terms of age in each country, unobserved differences between these two groups that are not captured by the cohort fixed effects over all countries might bias the estimations. Country-specific smooth trends in cohorts should capture these potential unobservable differences between treatment and control groups and allowing for country-specific quadratic trends is one way to increase the flexibility of these important control variables. We estimate our model controlling for quadratic instead of linear trends for the two larger samples, the baseline sample and sample 10. In general, the results are robust. The coefficients for memory increase in magnitude and statistical significance. We obtain statistically significant effects for good orientation in the larger sample but the coefficients for dementia loose their statistical significance. However, when we split the sample by gender, we again find statistically significant coefficients of around -0.01 for females.

As compulsory schooling reforms affect cohorts differently, we might still be concerned that our school reform variables pick up some unspecified time trends or structural breaks in the countries. To test for this, we are performing a placebo reform experiment. Similar to Black et al. (2008), we introduce a placebo treatment where we add a hypothetical compulsory schooling reform five years in the future for each of our countries. This placebo reform should not have any impact on the cognitive scores. If we find an impact, our results might be driven by other unobserved mechanisms, such as age effects or time trends. As the placebo reform should have no impact on attended years of schooling, we can only use and compare the

[10] The first stage coefficients are 0.23-0.25 for the group with few books and 0.37-0.44 for the group with more books at home at age 10.

Table 6: Placebo treatments - Reduced forms estimates

	Sample 10		Sample 7	
	Reduced Form (see Table 4)	Reduced Form +5yrs in future	Reduced Form (see Table 4)	Reduced Form +5yrs in future
A: Delayed Memory (both genders)				
Schooling reform	0.053	0.078	0.092	0.105
	(0.028)*	(0.032)**	(0.030)***	(0.034)***
Placebo reform		0.066		0.034
		(0.046)		(0.042)
Observations	8,882	8,882	6,931	6,931
B: Dementia (females)				
Schooling reform	-0.004	-0.004	-0.003	-0.003
	(0.002)**	(0.002)**	(0.002)*	(0.002)
Placebo reform		0.000		0.000
		(0.001)		(0.002)
Observations	4,094	4,094	3,184	3,184

NOTES: Each column and panel represents a separate regression based on Sample 10 or 7. Country-fixed effects, cohort-fixed effects, country-specific linear trends in birth cohorts, indicators for interview year, foreign born and indicators for potential impairment and other person in room during cognitive tests are included in all regressions, female additionally included in panel A. Heteroscedasticity and cluster-robust standard errors in parentheses (clusters are country-cohorts). ***, ** and * indicate statistical significance at the 1-percent, 5-percent and 10-percent level.

Reduced Form estimates, the effects of compulsory schooling on cognitive outcomes, to test for a placebo effect.

Table 6 shows the Reduced Form estimates for our main results obtained above, the delayed memory score of males and females and the condition of dementia for female seniors. We provide evidence for Sample 10 and 7 with country-specific linear trends in cohorts. In both panels, for comparison reasons the Reduced Form of the baseline model is shown and the results of the placebo tests are given. Adding placebo schooling reforms five years in the future (columns 2 and 4) does not significantly alter the Reduced Form estimates of the original reforms. Furthermore, each of the placebo laws has no significant impact on memory or dementia.[11]

Our identification strategy relies on the assumption that the instrument is randomly assigned and the exclusion restriction holds, i.e. the instrument influences

[11]Note that we have to include the real compulsory schooling reforms in the regressions as well, as for some cohorts the placebo and the real reform overlap.

old-age cognition only through individual years of education. While the exclusion restriction can never be tested, we can provide supportive evidence for the random assignment assumption. If years of compulsory schooling are randomly assigned, they should not be related to any childhood socio-economic characteristics of the individuals. Table 7 gives estimates of the effects of compulsory schooling years on pre-determined variables, such as the number of books in the household, whether the main breadwinner in the household worked in a skilled profession, the number of rooms per household member in the accommodation at age 10 and if the accommodation had a fixed bath, hot running water supply or central heating.

Table 7: Effects of the reforms on pre-determined characteristics

	few books	skilled bread-winner	#rooms per person	fixed bath	hot running water	central heating
Sample 10						
Compulsory schooling	-0.012	-0.000	0.005	0.027	0.012	0.003
	(0.010)	(0.009)	(0.005)	(0.013)**	(0.010)	(0.007)
Observations	5,646	5,505	5,597	5,673	5,673	5,673
Sample 7						
Compulsory schooling	-0.005	-0.003	-0.000	0.020	0.005	0.007
	(0.010)	(0.010)	(0.005)	(0.014)	(0.010)	(0.007)
Observations	4,395	4,296	4,360	4,417	4,417	4,417

NOTES: Outcome variables refer to age 10 and are defined as follows: few books (person lived in a household with 0-10 or 11-25 books), skilled breadwinner (the occupation of the main breadwinner is legislator, senior official or manager, professional or technician and associate professional), #rooms per person (the number of rooms in the accommodation divided by the number of persons living in the household), fixed bath, hot running water and central heating (did the accommodation have a fixed bath, a hot running water supply, central heating). Each coefficient represents a separate regression based on Sample 10 or 7. Country-fixed effects, cohort-fixed effects, country-specific linear trends in birth cohorts, indicators for interview year, female and foreign born included in all regressions. Heteroscedasticity and cluster-robust standard errors in parentheses (clusters are country-cohorts). ***, ** and * indicate statistical significance at the 1-percent, 5-percent and 10-percent level.

With one exception, we do not find any significant effects of the mandatory schooling reforms on pre-determined characteristics of age 10. We interpret these results as supporting evidence for our identification strategy.

5.4 Channels

Our analysis gives evidence that schooling has a significant long-term effect on old-age memory scores for males and females and dementia (including Alzheimer's disease, organic brain syndrome, senility and other serious memory impairments) for female seniors. There are several channels through which education might influence old-age cognition, such as income, labor force participation, cognitive leisure activities, physical and social activities as well as health and health behaviors. Direct effects of education and training on brain functioning can also play a rolel.

In the medical literature, many studies exist investigating risk and protective factors of cognitive decline and dementia (see e.g. Anstey et al., 2007, 2008; Hakansson et al., 2009; Ninomiya et al., 2011; Ravaglia et al., 2008; Xu et al., 2011; Yang et al., 2011). In its report, the Agency for Healthcare Research and Quality (2010) summarizes the previous research and concludes that cognitive training, physical activity, non-cognitive non-physical leisure activities and a Mediterranean diet are negatively associated with the risk of cognitive decline. Furthermore, marriage seems to have a protective effect and a depressive disorder, diabetes and current tobacco use are positively correlated with cognitive decline and dementia. No consistent associations are found for alcohol intake, obesity, hypertension and high cholesterol.

Following Banks and Mazzonna (2012), we identify possible channels on how education influences cognition by identifying the effects of schooling on outcomes that are known to influence cognitive decline. Banks and Mazzonna (2012) found that education does not have significant effects on social participation and quality of life. Table 8 shows 2SLS estimates of years of education on factors influencing cognitive decline and dementia for males and females, such as marriage, social activities, physical activities, smoking, diabetes and depression. Since we suspect that being socially active increases with labor force participation and the presence of children, we, furthermore, include those variables in our analysis.

Table 8: 2SLS estimates of years of education on mediating outcomes

	employed	married	children	in clubs	socially active	vigorously active	moderately active	smoking	diabetes	depressed
A: Males										
Sample 10	0.064	0.078	0.027	0.012	-0.022	0.066	0.010	0.017	-0.028	-0.057
	(0.045)	(0.035)**	(0.024)	(0.027)	(0.036)	(0.035)*	(0.033)	(0.031)	(0.017)*	(0.025)**
Observations	4,058	4,076	4,064	4,020	4,020	4,065	4,066	4,053	4,075	4,006
Sample 7	0.059	0.098	0.045	0.019	-0.005	0.024	-0.021	-0.012	-0.041	-0.060
	(0.042)	(0.037)***	(0.026)*	(0.027)	(0.037)	(0.028)	(0.029)	(0.029)	(0.019)**	(0.027)**
Observations	3,168	3,186	3,172	3,138	3,138	3,175	3,176	3,165	3,183	3,130
Mean of outcome	0.580	0.797	0.890	0.230	0.518	0.439	0.703	0.310	0.087	0.143
B: Females										
Sample 10	0.053	0.078	0.074	0.048	-0.039	-0.015	0.028	0.029	-0.013	-0.008
	(0.069)	(0.045)*	(0.025)***	(0.030)	(0.034)	(0.035)	(0.033)	(0.032)	(0.015)	(0.041)
Observations	4,881	4,888	4,877	4,849	4,849	4,888	4,888	4,877	4,890	4,835
Sample 7	0.032	0.055	0.122	0.086	-0.065	-0.008	0.022	0.028	-0.027	0.004
	(0.078)	(0.048)	(0.035)***	(0.042)**	(0.049)	(0.040)	(0.043)	(0.037)	(0.021)	(0.055)
Observations	3,807	3,811	3,803	3,778	3,778	3,813	3,813	3,803	3,816	3,770
Mean of outcome	0.416	0.729	0.917	0.190	0.501	0.342	0.685	0.226	0.061	0.279

NOTES: Outcome variables are defined as follows: employed (employed or self-employed), married (married and living with spouse), children (has children), in clubs (gone to a sport, social or other club in the last month), socially active (done at least one social activity last month), vigorously active (engages in sports or vigorous activities more than once a week), moderately active (engages in moderate activities more than once a week), smoking (is currently smoking), diabetes (was diagnosed with diabetes), depressed (scores >3 on EURO-D scale).
Each coefficient represents a separate linear 2SLS regression. Country-, cohort-fixed effects, country-specific linear trends in cohorts, indicators for interview year and foreign born included in all regressions. Heteroscedasticity and cluster-robust standard errors in parentheses (clusters are country-cohorts). ***, ** and * indicate statistical significance at the 1-percent, 5-percent and 10-percent level.

As above, individual years of education are instrumented with mandatory years of schooling. For males, we find that education increases the chance of being married and decreases the risk for diabetes and depression. Among females, statistically significant effects are obtained for having children; education increases the probability of having children.[12] Moreover, education causes also a higher participation in clubs for females, the effect is only statistically significant in the narrower sample.

While this evidence is only suggestive, it shows that some factors which are known from the epidemiological literature to cause or to be correlated with cognitive functioning for seniors are themselves caused by increases in schooling.

6 Conclusions

Improving cognitive functioning among seniors is important for the aging societies in Europe, but also for other parts of the world. For instance, China is expected to have an older age structure of the population than Europe within the next three or four decades (United Nations, 2011). Senior Europeans (from Central and Northern parts) have - in a global perspective - a relatively high cognitive functioning level. Our evidence suggests that the relatively high schooling levels in these regions are an important reason for this.

In this study, we provide evidence of a causal link between schooling and old-age cognitive outcomes. Studying the effects of exogenous variation in individual years of schooling induced by compulsory schooling reforms in 6 European countries, we find strong evidence for a positive effect of education on memory performance, in particular delayed memory. One year of education increases the memory score by around 16% of the standard deviation. Similar effects for a smaller set of cognitive outcomes are obtained for individuals with low education in the United States (Glymour et al., 2008) and England (Banks and Mazzonna, 2012). We do not find consistent effects

[12] This surprising result might be related to the results obtained for marriage and is also obtained in Fort et al. (2011), who investigate compulsory schooling and fertility in more detail.

on verbal fluency, numeracy and orientation to date. Moreover, our study indicates that longer schooling can lead to a significant decline in the prevalence of dementia among women. This is important as a growing incidence level of dementia is one of the most fundamental challenges faced by ageing economies (ADI, 2010).

We investigate potential channels through which education might influence cognition by studying the effects of education on outcomes that are suspected to influence cognitive decline. While we do not find any causal effects of education on labor force participation, social and physical activities and smoking at older ages, we obtain positive significant estimates for having children among females and being married among male seniors. Furthermore, for male seniors we find negative effects of education on diabetes and depression. However, this evidence is suggestive only and we leave a more detailed analysis on the question how education influences cognitive decline for further research.

In sum, our study suggests that lengthening obligatory schooling can lead to long term improvements in cognitive ability and mental health. This matters as cognitive functioning is a precondition for the ability to work, to stay independent and healthy and to enjoy a good quality of life. Extensions of schooling can represent one important policy venue for improving economic and social prospects in a period of population ageing.

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A Appendix: Educational Reforms in Europe

In this section, we briefly describe the compulsory schooling reforms we are using in this study.

Austria In 1962 a federal act was passed that increased compulsory schooling from 8 to 9 years. The law came into effect on September 1, 1966. Pupils who were 14 years old (or younger) at that time had to attend school for an additional year. Since compulsory education starts at the age of 6 and the cut-off date for school-entry is September 1, (mostly) individuals born between September and December 1951 were the first ones affected by the reform. Thus, the pivotal cohort is 1951.

Czech Republic In the 20^{th} century, compulsory education was reformed several times. In 1948 compulsory schooling was increased from 8 to 9 years (age 6 to 15). It was reduced to 8 in 1953 and increased to 9 again in 1960. Two further changes took place in 1979 and 1990. We consider the education reform in 1960 for our analysis, with the first cohort affected by this reform being the cohort born in 1947. See Garrouste (2010) for more information on compulsory schooling reforms in the Czech Republic.

Denmark In 1958 compulsory education was increased by 3 years, from 4 to 7. In 1971 compulsory schooling was further increased by 2 years, from 7 to 9. Education started at age 7, thus pupils who were 11 years old (or younger) in 1958 were potentially affected by the first reform, i.e. children born in 1947 and after. Since our data only cover individuals 50+, we only consider the first reform for this study.

France Two education reforms were implemented in France. In 1936, compulsory schooling was increased from 7 to 8 years (age 13 to 14) and in 1959 from 8 to 10 years (age 14 to 16). After a long transition period, the second reform came into effect in 1967. For this analysis we only consider the second reform, with the pivotal cohort being born in 1953.

Germany In the former Federal Republic of Germany compulsory schooling was increased from 8 to 9 years, gradually among the German states, starting from 1949 in Hamburg to 1969 in Bavaria. Due to the small sample size in several German states, we only consider 4 German states: Baden-Wuerttemberg, Hesse, Northrhine-Westphalia and Rhineland-Palatinate. In these states the education reform was implemented in 1967. The first cohort potentially affected by these reforms is the cohort born in 1953.

Italy In 1963 junior high school became mandatory in Italy, which increased compulsory years of schooling by 3 years (from 5 to 8 years). The first cohort potentially affected by this reform is the cohort born in 1949.