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Does Education Improve Cognitive Performance Four Decades After School Completion?

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Abstract We study the effect of secondary education on cognitive performance toward the end of working age. We exploit the exogenous variation in years of schooling arising from compulsory schooling reforms implemented in six European countries during the 1950s and 1960s. Using data of individuals, approximately age 60, from the Survey of Health, Ageing and Retirement in Europe (SHARE), we assess the causal effect of education on memory, fluency, numeracy, and orientation-to-date. Furthermore, we study education effects on cognitive decline. We find a positive impact of schooling on memory scores. One year of education increases the memory score approximately four decades later by about 0.2, which amounts to 10 % of a standard deviation. Furthermore, we find some evidence for a protective effect of schooling on cognitive decline in terms of verbal fluency.

Keywords Compulsory education · Cognitive functioning · Memory · Aging

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Introduction

Population aging in Europe could pose challenges to the sustainability of national social security and health systems. The burden of the demographic change is likely to depend on age-specific physical health and cognitive performance (which determine labor force participation, social activity, and dependency status) rather than the demographic age structure per se (Skirbekk et al. 2012). Cognitive performance is of growing importance for work productivity. The cognitive performance of individuals close to retirement age is likely to determine whether pension reforms aimed at raising the retirement age will be effective (OECD 2006; Romeu Gordo and Skirbekk 2013; Warr 1994). Mental functioning is also important for 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 aging on cognitive abilities (Nisbett et al. 2012). Finding ways to improve cognition for new generations of seniors is of central importance to aging economies. The current study addresses the extent to which formal education improves long-run cognitive performance—that is, decades after school completion

Several studies have investigated the determinants of cognitive functioning among seniors, with contrasting results on the role of education. For example, Richards and Hatch (2011) and Yount (2008) found that schooling is positively associated with several outcomes at older ages, particularly cognition. On the other hand, Sharrett (2012) found no association between education and cognitive function and dementia. Formal education but also intellectually stimulating activities can be beneficial for cognitive maintenance throughout the life cycle. Education can affect cognitive performance many years thereafter 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). Potter et al. (2008) found that independent of other observable factors, intellectually demanding work is associated with greater cognitive performance in later life, suggesting that behavior may enhance intellectual reserve. Furthermore, 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).

However, it is doubtful whether simple correlations between schooling and cognitive performance indicate causal mechanisms because cognitive functioning of individuals is highly correlated across time,[1] and the education-cognition association might pick up reverse causation from high cognitive performance in childhood to higher school attainment. Moreover, education is influenced by many unobserved characteristics that influence cognitive outcomes, such as ability, childhood intelligence, and the socioeconomic characteristics of the home environment (Neisser et al. 1997; Nisbett 2009).

[1] A U.S. nun study has shown that low linguistic ability at age 22 was a strong predictor of poor cognitive function and Alzheimer's disease in late life (Snowdon and Kemper 1996).

Because of these confounding factors, the coefficient of education on cognitive performance is likely to be upwardly biased in an ordinary least squares (OLS) regression.

Several identification strategies have been followed in the literature to overcome this endogeneity bias. Longitudinal studies, in which initial ability is controlled, have shown that education and mental activity in youth and adulthood are related to a greater cognitive performance (Deary et al. 2004; Husén and Tuijnman 1991; Whalley and Deary 2001). Moreover, studies of monozygotic twins, in which within-pair variation in education is used to identify the causal effect of schooling on cognition, also suggest a positive relationship (Haworth et al. 2008).

The education-cognition relationship has also been studied by exploiting natural experiments, such as extensions in mandatory years of education. These studies have examined variation in individual years of education that is not related to innate ability but prescribed by law. For example, Brinch and Galloway (2012) investigated the lengthening of compulsory schooling from seven to nine years, which was gradually implemented in Norwegian municipalities between 1955 and 1972. The authors concluded 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 focused on the effects of schooling on cognitive outcomes at older ages, many years after school completion. Glymour et al. (2008) exploited state compulsory schooling laws in the United States between 1907 and 1961. The mandatory schooling laws in the United States had little effect on completed education: one additional year of compulsory schooling increased actual years of education by approximately 0.04 years. However, separate-sample instrumental variables (SSIV) estimates showed significant effects of education on memory scores but not on mental status.

Banks and Mazzonna (2012) investigated 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 found that education increases old-age memory scores for both males and females as well as executive functioning for males.

We conduct an analysis using data of European individuals aged 47–73 from the Survey of Health, Ageing and Retirement in Europe (SHARE). We exploit compulsory schooling reforms in the 1950s–1960s in Austria, the Czech Republic (former part of Czechoslovakia), Denmark, France, Germany, and Italy. The multicountry set-up and the gradual implementation of the lengthening of mandatory schooling in the different countries allows us to control for unobserved differences shared by individuals born in the same country (country fixed effects) and in the same calendar year (cohort fixed effects) in the empirical specification. We further contribute to the literature by extending the set of outcome variables. We investigate education effects on immediate and delayed memory, verbal fluency, and numeracy; we also explore education effects on orientation-to-date, which captures basic cognitive function. Using the panel dimension of SHARE, we furthermore study the effect of schooling on cognitive decline: that is, the change in cognitive test scores within a two-year period. Because our sample members are around retirement age, investigating the causal impact of education on their cognitive performance is of particular interest with respect to labor market and retirement policies.

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 six European countries: Austria, the Czech Republic, Denmark, France, Germany, and Italy. Within each country, we relate the variation in cognitive outcomes of individuals born in different calendar years to their educational 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 birth cohorts within each country and across countries for any given cohort. The variation over birth 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 who were born in a given year or have the same age at the time of the interview. Furthermore, within each country, we capture trends over birth cohorts or age effects in cognition with country-specific linear trends.

We estimate the causal impact of education on the level of cognitive performance (level analysis, *l*) and also on cognitive decline (slope analysis, *s*). The regression models for the level and the slope analysis are given by the following two equations:

$$Y_{ickt} = \beta_{0l} + \beta_{1l} E_{ickt} + \beta_{2l} \mathbf{X}_{ickt} + \beta_{3l} C_c + \beta_{4l} K_k + \beta_{5l} Trend_{ck} + \varepsilon_{ickt} \tag{1}$$

$$Y_{ickt} - Y_{ickt+r} = \beta_{0s} + \beta_{1s} E_{ickt} + \beta_{2s} \mathbf{X}_{ickt} + \beta_{3s} C_c + \beta_{4s} K_k + \beta_{5s} Trend_{ck} + \varepsilon_{ickt} - \varepsilon_{ickt+r}, \tag{2}$$

where Y_{ickt} is cognitive achievement of individual i in country c of birth cohort k in survey year t. $Y_{ickt} - Y_{ickt+r}$ refers to the change in cognitive performance in survey year t compared with survey year $t + r$. E_{ickt} is the number of years that the individual spent in education, and $\mathbf{X}_{ickt}$ is a vector of control variables. C_c and K_k refer to country and cohort fixed effects, and $Trend_{ck}$ captures country-specific linear trends in birth cohorts.[2]

Because the error terms ε_{ickt} and ε_{ickt+r} might be correlated with years of education, we estimate Eqs. (1) and (2) by two-stage least squares (2SLS), instrumenting individual years of education with $Comp_{ck}$, the compulsory years of schooling in the respective country and birth cohort. Equation (3) is the first-stage equation and shows the impact of compulsory schooling on years of education.

$$E_{ickt} = \alpha_0 + \alpha_1 Comp_{ck} + \alpha_2 \mathbf{X}_{ickt} + \alpha_3 C_c + \alpha_4 K_k + \alpha_5 Trend_{ck} + v_{ickt}. \tag{3}$$

[2] The vector $\mathbf{X}_{ickt}$ includes a female dummy variable and an indicator variable for whether a person was born abroad and migrated before age 5. It contains indicators for the interview year and control variables for the quality of the interview session (the interviewer's perception of whether something may have impaired the respondent's performance on the tests and whether another person was present during the interview) in Eq. (1) and an indicator for the first interview year, control variables for the quality of both interview sessions, and the number of months between the two interviews in Eq. (2). Note that all results are robust to adding the control variables for the quality of the interview session.

As discussed earlier, in the outcome equations, 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 because 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 (such as reading habits or changes in health systems) or change at once (such as 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., if individuals before and after the reforms do not differ) and if the exclusion restriction is fulfilled (i.e., the instrument influences cognitive outcomes only via the impact on years of education) (Angrist et al. 1996). We provide supportive evidence on these assumptions in the upcoming section on robustness.

The identifying assumptions become more plausible when the width of the window around the pivotal cohort—that is, the first cohort affected by the new level of compulsory schooling—is small. This means that 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 persons and circumstances before and after the changes in the law are similar but also the disadvantage of producing smaller sample sizes. Therefore, we estimate our model with different samples based on different widths of windows around the pivotal cohorts (up to 10 years prior and 10 years after the pivotal cohort, up to ±7 years, and up to ±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": specifically, the effect of years of education on cognitive outcomes for those individuals who were actually influenced and changed their behavior as a result of 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 only to those individuals at the bottom of the education distribution.

Table 1 lists the countries and reforms we consider in this article, 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, and the pivotal cohort (the first cohort potentially affected by the reforms).[3] In Germany, compulsory schooling has been extended at different points in time in the different German states, ranging from 1949 in Hamburg to 1969 in Bavaria. Because SHARE provides detailed geographic information, we can exploit this regional variation in our analysis. For a short description of each reform and the explanation of the choice of the pivotal cohorts, see the Appendix.

[3] We restrict the analysis to individuals born between 1939 and 1956: these individuals are between 47 and 73 years old at the time of the interviews. We focus on six European countries. Other countries participating in SHARE have also reformed their education systems, but these other reforms took place either too early or too late for our sampling period. In Sweden, compulsory schooling has been extended gradually in Swedish municipalities beginning in the 1960s, but SHARE does not provide detailed information about the municipalities.

Table 1 Compulsory schooling reforms

Country	Reform	Increase in Mandatory Years of Schooling	School-Leaving Age	Pivotal Cohort
Austria	1962/1966	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	1959/1967	8 to 10	14 to 16	1953
Germany				
Hamburg	1949	8 to 9	14 to 16	1934
Schleswig-Holstein	1956	8 to 9	14 to 15	1941
Bremen	1958	8 to 9	14 to 15	1943
Lower Saxony	1962	8 to 9	14 to 15	1947
Saarland	1964	8 to 9	14 to 15	1949
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
Bavaria	1969	8 to 9	14 to 15	1955
Italy	1963	5 to 8	11 to 14	1949

Several studies have investigated mandatory schooling reforms in Europe. Brunello et al. (2009) combined various data sets to investigate compulsory schooling reforms in 12 European countries and found that education increases wages and reduces wage inequality. Furthermore, Fort et al. (2011) used compulsory schooling reforms to investigate the relationship between education and fertility decisions, and Brunello et al. (2011), (2013) studied the effects of education on health and body mass index (BMI).

Data

We pool data of individuals participating in one or more waves of the Survey of Health, Ageing and Retirement in Europe (SHARE). Next to the interviews about the respondents' economic, demographic, and social situation, cognitive assessments have been conducted in the first wave in 2004/2005, in the second wave in 2006/2007, and in the fourth wave in 2011/2012. We use only records of individuals aged 45 or older who were born in the country of residence or who migrated before age 5 to ensure that they attended school in the host country at least at the early stages of their school career, 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

[4] Although 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.

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 cannot be calculated from the educational attainment categories for only 174 individuals. Our baseline data set consists of 18,258 individuals who participated in one, two, or three waves.[5] Depending on the cognitive outcome in question, our level analysis is based on as many as 27,699 cognitive assessments. From this baseline data set, we gradually reduce the samples around the reforms in each country to individuals born up to 10 years before and after (sample 10), seven years before and after (sample 7), and five years before and after each pivotal reform cohort (sample 5).

Summary statistics of the baseline sample used for the level analysis are given in Table 2; Table 3 refers to the sample for the slope analysis. The sample is smaller for the slope analysis because only individuals who participated at least twice in the cognitive tests can be included.[6]

We measure educational attainment with individual years of schooling. Although the second and fourth waves of SHARE provide information on the number of years spent in full-time education, the respondents were asked in the first wave about their educational degrees only. Thus, we use the second wave or fourth wave information on years of schooling for all individuals who participated in the second or fourth wave. For those individuals who participated in the first wave only, we calculate their years of education using country-specific conversion tables provided by SHARE.[7]

Table 2 reports descriptive statistics on key variables for the birth cohorts born in 1939–1956. A bit more than one-half of the sample is female, and the mean age is approximately 61 years. On average, the individuals completed 11 years of education. We measure various domains of cognitive functioning, such as memory, fluency, numeracy, and orientation-to-date. These measures are based on the following tests.

Immediate and Delayed Memory: Immediate memory measures how of 10 words a respondent recalled directly after the interviewer read the words. Delayed memory measures the respondent's ability to recall the same words 5 to 10 minutes later, after several other interview questions. Both variables range from 0 to 10. On average, the respondents were able to recall 5.5 words immediately and 4 words after a short delay, with standard deviations of 1.7 and 2, respectively.

Fluency: The verbal fluency score is the sum of animal names that the respondent was able to state in one minute. This variable ranges from 0 to 100, with a mean value of 21 and a standard deviation of approximately 8.

[5] Approximately 15 % of these respondents participated in all three waves, 23 % participated in two waves, and 62 % participated only once. The large number of individuals who participated only once is due to the fact that many countries extended the sample significantly in Wave 4. Furthermore, the Czech Republic joined the survey in Wave 2.

[6] We cluster the standard errors at the individual level because both the level and the slope analysis are based on multiple observations per individual. Also note that the slope analysis is based on multiple observations if individuals participated in all three waves.

[7] We further corrected the years of education variable. In all waves, the respondents were asked about their detailed (further and higher) educational qualifications. Based on these additional variables, we calculated their required years of education according to country-specific conversion tables. We replaced years of education with the required years of education in cases where the years of education were missing, zero, or implausibly low.

Table 2 Descriptive statistics of baseline sample: Level analysis

Country	Female	Age	Years of Education		Immediate Memory	Delayed Memory	Fluency	Good Numeracy	Good Orientation	Observations	Individuals
			Individual	Compulsory							
Austria	0.57	62.24	10.26	8.28	5.67	4.37	23.56	0.72	0.91	4,724	3,624
Czech Republic	0.57	61.76	12.16	8.58	5.48	3.78	22.11	0.60	0.88	5,448	4,571
Denmark	0.52	59.37	12.09	5.77	5.96	4.85	23.92	0.58	0.90	3,755	1,901
France	0.55	60.13	11.99	8.46	5.39	4.07	21.01	0.51	0.87	5,683	3,644
Germany	0.54	59.76	13.33	8.26	5.96	4.45	23.27	0.74	0.92	2,860	1,590
Italy	0.56	60.88	8.71	6.11	4.94	3.46	15.93	0.33	0.90	5,229	2,928
Total	0.55	60.81	11.26	7.63	5.51	4.09	21.33	0.57	0.90	27,699	18,258

Notes: The sample comprises individuals who were born in 1939–1956; participated in the first, second, or fourth wave of SHARE; reported their years of education; were aged 45 or older at the time of the interview; and were born in the country or migrated before age 5. Descriptive statistics on gender, age, and education are based on 27,699 observations (18,258 individuals). Immediate memory is based on 27,699; delayed memory, on 27,681; and fluency, on 27,527 observations. The number of observations is smaller for good numeracy (22,133) and orientation (22,202) because these tests were conducted with all individuals in Waves 1 and 2 but only with the "new" respondents in Wave 4.

Table 3 Descriptive statistics of baseline sample: Slope analysis

Country	Cognitive Decline in: Immediate Memory	Delayed Memory	Fluency	Numeracy	Orientation	Duration	Observations	Individuals
Austria	–0.01	–0.24	0.36	–0.06	–0.06	39.50	1,100	741
Czech Republic	–0.08	–0.07	–2.23	—	—	49.50	877	877
Denmark	0.02	–0.15	–0.25	–0.14	0.01	42.35	1,854	1,326
France	–0.29	–0.49	0.99	–0.08	0.03	40.18	2,031	1,379
Germany	0.05	–0.21	0.22	–0.03	0.00	43.01	1,268	893
Italy	–0.22	–0.22	–0.12	–0.05	0.00	42.34	2,301	1,521
Total	–0.11	–0.25	0.00	–0.07	0.00	42.30	9,431	6,737

Notes: The sample comprises individuals who were born in 1939–1956; participated in at least two waves of SHARE (first, second, or fourth); reported their years of education; were aged 45 or older at the time of the first interview; and were born in the country or migrated before age 5. Cognitive decline is the change in cognitive functioning between any two waves (i.e., cognitive outcome wave $t - t + r$). A positive number indicates a decline, and a negative number refers to an increase in the test score. Duration gives the number of months between the two assessments. The number of observations is 9,431 for immediate memory, 9,435 for delayed memory, and 9,378 for fluency. The number of observations is smaller for decline in numeracy (6,737) and orientation (6,768) because these tests have not been conducted with the longitudinal sample in Wave 4. There are no observations for the Czech Republic for numeracy and orientation because the country participated in Waves 2 and 4 only.

Numeracy: This score ranges from 1 to 5 (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 calculations of compound interest. The average numeracy score is 3.6, with a standard deviation of 1.

Orientation-to-date: This variable ranges from 0 to 4 and measures whether a person was able to remember the correct date comprising the day of the month, month, year, and day of the week. The average score in our sample is 3.8, with a standard deviation of 0.5.

The scores of immediate memory, delayed memory, and fluency approximately follow a normal distribution around their mean values. 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.[8] For the level analysis, 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 1 for individuals who achieve numeracy scores of 4 and 5, and *Good Orientation* is defined to be 1 for individuals scoring 4 on the orientation variable. Mean values of these binary indicators are found in the descriptive statistics.

Table 3 gives summary statistics for the slope analysis and presents the change in cognitive test scores for individuals who participated multiple times in the cognitive assessments. This change is defined as *cognitive decline,* which we calculate by subtracting the cognitive outcome from the cognitive outcome in a previous wave. Thus, a positive value implies a decline in cognitive performance, and a negative value represents a performance improvement. In most countries, the memory performance improves between assessments. On average, immediate memory increases by 0.11 (which amounts to 2 %), and delayed memory increases by 0.25 (6 %). On average, fluency and orientation-to-date do not change between the tests, and numeracy improves by 0.07 (approximately 2 %).

Given a mean period between interviews of 3.5 years and an average respondent age of 60, the descriptive statistics are surprising. However, the cognitive changes are small and might be due to retest effects—that is, a bias that results from having completed the same or a similar test in a previous wave of the survey. Such bias 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). Because we are interested in whether education decelerates cognitive decline, potential retest effects will bias our estimates only if the magnitude of the retest bias is related to schooling. Although there is no indication that this is the case, our results on cognitive decline should be interpreted with caution.

[8] The distribution of orientation to date is uneven and strongly skewed to the right (skewness = –4.99). Numeracy is also distributed right-tailed (skewness = –0.48.)

Results

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

Baseline Results

We start by looking at the effects of compulsory schooling on actual years of education (the first stage), shown in Fig. 1. The graph shows the cohorts' mean years of education 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 4: increasing compulsory education by one year leads to one-third of an additional year of schooling, on average. This is a sizable effect; typically, only individuals at the lower end of the educational distribution react to compulsory schooling reforms.[9]

Table 5 presents our main results, with OLS estimates for the baseline sample shown in panel A and 2SLS results for the various samples presented in panel B. The first five columns of coefficients refer to the level analysis, and the remaining columns show the estimates of the slope analysis. All OLS estimates show a clear positive association between schooling and the levels of cognitive functioning many years after school completion. Concerning cognitive decline, the OLS estimates are statistically significant only for delayed memory scores. Because of potential bias from omitted variables, these associations cannot be taken as causal effects. We proceed with our 2SLS estimates, which reflect causal effects of education on cognitive functioning for individuals who increased their educational attainment as a result of the compulsory schooling reforms in the various countries.[10]

We see a clear and robust causal effect of education on immediate memory and even more so on delayed memory. These effects are 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 five years before and after the reform, we find that one additional year of schooling increases immediate memory by 0.23 words (of 10 possible) and increases delayed memory by 0.32 words. These effects amount to 14 % and 16 % of the standard deviations, respectively, in the immediate and delayed memory scores in the sample. There are no causal effects of education on the level of fluency, numeracy, and orientation-to-date. This is not due to small sample sizes: the point estimates are typically very small and often have the wrong sign.[11]

[9] The first-stage coefficients are similar in magnitude to those obtained in other studies investigating compulsory schooling laws in various European countries (e.g., Brunello et al. 2009, 2011, 2013; Fort et al. 2011).

[10] The bottom rows in Table 5 give p values of regression-based F statistics and indicate whether years of education can be treated as exogenous. These test statistics are based on the regressions obtained with the baseline sample and sample 5.

[11] We estimated IV-probit models for good numeracy and good orientation. The marginal effects of these models are very similar to those of the linear probability models and are not statistically significant.

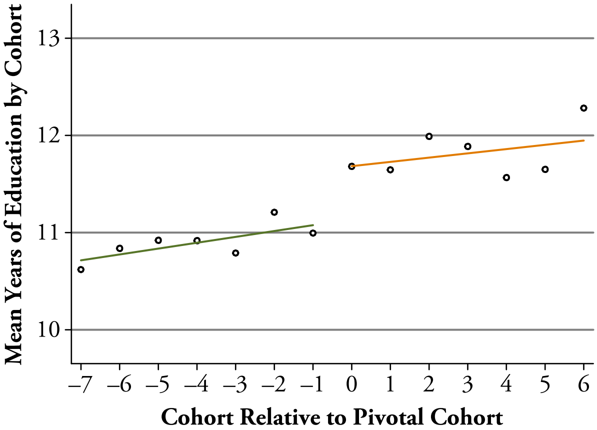

Fig. 1 First stage

One potential reason why we find gains in memory but not in fluency is that the fluency test is based on naming animals, a measure that could be less affected by extensions to secondary school levels given that this type of knowledge may be of greater focus in lower levels of instruction. Further, the lengthening of schooling could reduce the probability of working with animals (e.g., in agricultural occupations) or residing in rural areas that inherently have a larger animal population, thus reducing knowledge of animals. On the other hand, the test is a measure for executive functioning or the ability to organize one's thoughts, which should improve the ability to reply to this question in an organized manner (e.g., first naming livestock, then birds, and then wildlife). However, our results are in line with the findings of 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 among males with low education. Gains to immediate and delayed recall may result from the fact that schooling is universally aimed at improving these

Table 4 First-stage regressions: Years of education

	Baseline	Sample 10	Sample 7	Sample 5
Compulsory Schooling	0.315	0.317	0.314	0.331
	(0.062)**	(0.063)**	(0.073)**	(0.090)**
F Statistics	25.82	24.98	18.41	13.40
Partial R^2	.002	.002	.002	.002
Observations	27,699	25,378	20,126	15,509

Notes: Each coefficient represents a separate linear regression. Included in all regressions are country fixed effects, cohort fixed effects, country-specific linear trends in birth cohorts, indicators for interview year, foreign-born, female, and indicators for the interviewer's perception on whether something may have impaired the respondent's performance on the tests and whether another person was present during the interview. Heteroscedasticity- and cluster-robust standard errors are shown in parentheses (clusters are individuals). The sample includes all observations with nonmissing immediate memory scores.

**$p < .01$

Table 5 Baseline results

	Level Analysis					Slope Analysis (decline in . . .)				
	Immediate Memory	Delayed Memory	Fluency	Good Numeracy	Good Orientation	Immediate Memory	Delayed Memory	Fluency	Numeracy	Orientation
A. OLS										
Baseline	0.112	0.125	0.504	0.032	0.004	–0.002	–0.016	–0.011	–0.003	0.000
	(0.003)**	(0.004)**	(0.015)**	(0.001)**	(0.001)**	(0.004)	(0.005)**	(0.016)	(0.005)	(0.002)
Observations	27,699	27,693	27,555	22,368	22,467	9,431	9,435	9,378	6,737	6,768
B. 2SLS										
Baseline	0.144	0.171	–0.260	–0.013	–0.007	–0.080	–0.083	–0.755	–0.041	0.061
	(0.066)*	(0.078)*	(0.322)	(0.023)	(0.013)	(0.085)	(0.093)	(0.367)*	(0.082)	(0.045)
Observations	27,699	27,693	27,555	22,368	22,467	9,431	9,435	9,378	6,737	6,768
Sample 10	0.155	0.184	–0.020	–0.012	–0.006	–0.079	–0.101	–0.780	–0.025	0.062
	(0.067)*	(0.080)*	(0.308)	(0.023)	(0.013)	(0.084)	(0.093)	(0.366)*	(0.080)	(0.044)
Observations	25,378	25,375	25,245	20,450	20,540	8,561	8,567	8,513	5,973	6,002
Sample 7	0.205	0.217	–0.161	–0.023	0.002	–0.045	–0.010	–0.606	0.081	0.077
	(0.079)**	(0.093)*	(0.366)	(0.026)	(0.015)	(0.095)	(0.103)	(0.391)	(0.086)	(0.048)
Observations	20,126	20,124	20,021	16,257	16,333	6,757	6,762	6,717	4,729	4,752
Sample 5	0.233	0.324	–0.361	–0.032	0.001	0.062	0.073	–0.616	0.078	0.050
	(0.093)*	(0.118)**	(0.445)	(0.032)	(0.017)	(0.104)	(0.116)	(0.422)	(0.095)	(0.049)
Observations	15,509	15,422	15,507	12,559	12,618	5,154	5,157	5,117	3,605	3,627
Endogeneity Tests *p* Value (baseline)	.624	.558	.009	.031	.398	.347	.468	.019	.643	.135
p Value (Sample 5)	.160	.056	.030	.021	.860	.558	.481	.110	.358	.242

Notes: Each coefficient represents a separate linear regression. Panel A gives OLS estimates of years of education on cognition and cognitive decline; Panel B gives 2SLS estimates. Included in all level regressions are country fixed effects, cohort fixed effects, and country-specific linear trends in birth cohorts. Indicators for interview year, foreign-born, female, and indicators for the interviewer's perception on whether something may have impaired the respondent's performance on the tests and whether another person was present during the interview. The slope regressions include indicators for the first interview year, indicators for the interviewer's perception on whether something may have impaired the respondent's performance on the tests and whether another person was present during both interviews, as well as the duration between the interviews. The endogeneity tests are based on the baseline sample and Sample 5 and test the null hypothesis that variables are exogenous. Heteroscedasticity- and cluster-robust standard errors are shown in parentheses (clusters are individuals).

$^{*}p < .05$; $^{**}p < .01$

skills: after all, learning how to remember new material is universally essential for schooling success. Education and aligned working and social experiences are likely to improve strategies for encoding and organizing new information and the ability to remember.

The lack of effects on numeracy could be partly due to the high share of correct 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 in secondary school may not be relevant for the numerical test given. Conducting basic mathematical operations is a skill needed in basic household work and is important for both low- and high-skilled occupations. Education may not necessarily be related to the use of such skills.[12] Concerning basic cognition in terms of orientation-to-date, we might not find any causal effects because the individuals in our sample might be too young (age 60) to be plagued by severe disorientation: only 11 % have problems with naming all four dimensions of the date, and only 1 % are fully disoriented.

Regarding the slope analysis, we find evidence that education reduces fluency decline over time. One more year of education reduces cognitive decline in fluency by approximately 0.7. The point estimates are very similar in all samples, but they are significant in only the two larger ones. This effect amounts to 10 % of the standard deviation in the change in fluency. However, as discussed earlier, the slope effects have to be interpreted with caution. Retest effects might bias the result upward if the retest bias is directly influenced by education (i.e., if the retest effect is larger for highly educated individuals). On the other hand, smaller samples and a higher importance of measurement error in a change equation might bias results downward or toward statistical insignificance.

Heterogeneous Effects

In this section, we explore whether the 2SLS estimates of education on cognitive performance vary by gender and family background. Panel A of Table 6 shows coefficients of 2SLS regressions for sample 10, separately for males and females.

Similar to the baseline results of the level analysis for both genders, we find significant effects of education on immediate and delayed memory for male respondents. For female respondents, the coefficients are smaller in magnitude and are no longer statistically significant. However, as the bottom row in panel A indicates, the coefficients do not differ significantly between men and women.[13] Concerning cognitive decline, the 2SLS estimates indicate some protective effects of education for males' delayed memory and fluency scores. Results for females are of similar size to those for males when it comes to fluency decline. Overall, the analysis by gender yields somewhat stronger effects for males; however, coefficients typically do not differ significantly between men and women.[14]

[12] We find significant coefficients neither when the numeracy score is treated as a continuous variable nor when the cutoff for the binary indicator is lower or higher.

[13] Using the other samples, we find similar results with rising magnitudes and better precision with decreasing sample size. For example, using sample 5, the coefficients for delayed memory are approximately 0.3 and are statistically significant for females (at the 5 % level).

[14] First-stage estimates are 0.32 for males and 0.29 for females.

Panel B contains 2SLS estimates by parental background. At the time of the third wave of SHARE, the survey incorporated a retrospective interview (SHARELIFE) on childhood circumstances of the respondents. Unfortunately, education of the parents is not available; therefore, we use as a proxy for socioeconomic background the number of books at home when the respondent was 10 years old—before final decisions about schooling were made.[15] On the basis of this variable, we split our sample into two parts: one sample for individuals with few books at home (0–10 or 11–25 books; approximately 59 %) and one sample for individuals with many books at home (26–100, 101–200, or more than 200 books; approximately 41 %). Because these estimations are based only on individuals who participated in the third wave of SHARE, these samples are smaller. Table 6 contains the analysis for the two groups separately. Generally, we find stronger and statistically more significant results for the group with many books at home, who experience higher returns to schooling in terms of immediate and delayed memory than respondents from less-affluent families. For the other measures of cognitive performance, most of the coefficients are not statistically significant; however, the coefficient on good orientation is statistically significant for the group with favorable parental background. Overall, the analysis by parental background yields somewhat stronger effects for respondents who grew up in households with many books at home, but most coefficients do not differ significantly between the two groups.[16]

Robustness

Because our identification strategy relies on a change that influences adjacent birth cohorts differently in each of the countries—that is, the change in compulsory schooling—one concern is that other factors that are correlated with the instrument and/or the instrument itself (not mediated via schooling) influence cognitive outcomes. We are fairly confident that our identification strategy addresses these potential problems. Other events, laws, or social moods that change at the same time as compulsory schooling might be a problem in our model. However, it is highly unlikely that another important law was changed at the exact time as compulsory schooling in all our countries. Furthermore, to violate the identification assumption, these other changes would have to affect our pre- and post-birth cohorts differently, which is very unlikely. More problematic is that some societal changes or trends (e.g., improvements in medical care) might simultaneously influence both schooling decisions and habits related to cognitive performance. However, we can assume that these trends are smooth over time—that they are not as discontinuous as compulsory schooling laws and are therefore captured by the country-specific trends. Moreover, societal trends will typically be period-related, not cohort-related; that is, they will influence all living cohorts to some extent and adjacent cohorts in much the same way.

In the estimations presented earlier, we control for linear country-specific trends in birth cohorts. However, treatment and control groups (cohorts after and before the reforms) might differ in their unobserved characteristics, and these differences might not be captured by the cohort fixed effects over all countries. Country-specific smooth

[15] The number of books at home is often used as proxy variable for family socioeconomic background.

[16] The first-stage coefficients are 0.27 for individuals with few books and 0.34 for individuals with many books at home.

Table 6 Heterogeneous effects by gender and family background (2SLS)

	Level Analysis					Slope Analysis (decline in . . .)				
	Immediate Memory	Delayed Memory	Fluency	Good Numeracy	Good Orientation	Immediate Memory	Delayed Memory	Fluency	Numeracy	Orientation
A. By Gender										
Males	0.234	0.205	–0.243	–0.012	–0.018	–0.123	–0.227	–0.826	–0.022	0.073
	(0.107)*	(0.120)†	(0.478)	(0.034)	(0.021)	(0.113)	(0.135)†	(0.486)†	(0.090)	(0.055)
Observations	11,361	11,360	11,271	9,240	9,283	3,768	3,774	3,738	2,645	2,654
Females	0.090	0.167	0.173	–0.009	0.005	–0.031	0.032	–0.787	–0.042	0.040
	(0.097)	(0.116)	(0.441)	(0.032)	(0.018)	(0.135)	(0.152)	(0.604)	(0.148)	(0.075)
Observations	14,017	14,015	13,974	11,210	11,257	4,793	4,793	4,775	3,328	3,348
Δ Significant? Chi-Squared	1.01	0.05	0.41	0.00	0.65	0.27	1.62	0.00	0.01	0.13
Prob > Chi-Squared	.316	.821	.523	.945	.419	.601	.204	.960	.904	.718
B. By Family Background										
Few books	0.104	0.047	–0.025	0.011	–0.018	–0.014	0.140	–0.959	–0.072	0.119
	(0.135)	(0.166)	(0.616)	(0.049)	(0.031)	(0.146)	(0.167)	(0.699)	(0.181)	(0.105)
Observations	7,853	7,857	7,830	5,165	5,176	4,428	4,432	4,406	3,205	3,218
Many books	0.326	0.464	0.641	–0.058	0.058	–0.253	–0.511	–1.539	–0.156	–0.103
	(0.166)†	(0.226)*	(0.685)	(0.056)	(0.035)†	(0.201)	(0.318)	(1.080)	(0.174)	(0.088)
Observations	5,272	5,276	5,262	3,355	3,364	2,840	2,844	2,834	1,714	1,726
Δ Significant? Chi-Squared	1.06	2.21	0.52	0.86	2.61	0.92	3.29	0.20	0.11	2.62
Prob > Chi-Squared	.302	.137	.469	.355	.106	.338	.070	.652	.739	.106

Notes: Each coefficient represents a separate linear regression based on Sample 10. 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 at age 10, with few books (0–10 and 11–25 books) and many books (26–100, 100–200, and more than 200 books). This information is drawn from the SHARELIFE interviews in 2008/2009. All regressions include country fixed effects, cohort fixed effects, country-specific linear trends in birth cohorts. All level regressions include indicators for interview year, foreign-born, and indicators for potential interview impairments and whether another person was present during the interview. A dummy variable for female is included in panel B. The slope regressions include indicators for the first interview year, indicators for potential interview impairments, and whether another person was present during both interviews, as well as the duration between the interviews. Heteroscedasticity- and cluster-robust standard errors are shown in parentheses (clusters are individuals).

† $p < .10$; * $p < .05$; ** $p < .01$

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 models controlling for quadratic instead of linear trends for the two larger samples, the baseline sample and sample 10. The effects of education on the levels of immediate and delayed memory are very robust. Concerning the slope regressions, the coefficients for fluency decline are lower in magnitude and are less precisely estimated.

Given that compulsory schooling reforms are structural breaks in the time series, we might still be concerned that our school reform variables pick up some other unspecified time trends or structural breaks in the respective countries. To test for this, we conduct a placebo reform experiment. Similar to Black et al. (2008), we introduce a placebo treatment in which we add a hypothetical compulsory schooling reform: we artificially replace the actual compulsory schooling level with the level five years in the future. Such a 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. Because the placebo reform should have no impact on attended years of schooling, we use only the reduced-form estimates—the effects of compulsory schooling on cognitive outcomes—to test for a placebo effect.

Table 7 shows the reduced-form estimates for our main results obtained earlier, the immediate and delayed memory scores, and the decline in fluency. We provide evidence for samples 10 and 7 with country-specific linear trends in cohorts. In all three panels, the first and the third columns show the true reduced-form parameters. Adding the placebo compulsory schooling level 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 fluency decline on its own.[17]

A further argument against our identification strategy is that the effects of World War II might blur our estimations. Because our sample consists of birth cohorts born between 1939 and 1956, many individuals in the control group (birth cohorts who experienced a lower level of compulsory schooling) were born during that war, but most individuals in the treatment group were born thereafter. Although the placebo reform experiment provides suggestive evidence that no additional differences between older and younger cohorts exist that are not captured by cohort fixed effects and time trends, we perform another test. We reduce our sample to cohorts born between 1946 and 1956 to ensure that all cohorts were born after WWII. We have to omit the Czech Republic and Denmark because their reforms took place too early for this exercise. The results for immediate and delayed memory are robust and statistically significant: the coefficients are 0.20 for immediate memory and 0.37 for delayed memory. For fluency decline, we obtain a coefficient of –0.77, which is statistically insignificant.

Our identification strategy relies on the assumption that the instrument is randomly assigned and the exclusion restriction holds: that is, the instrument influences cognition only through individual years of education. Although the exclusion restriction can never be tested, we can provide supportive evidence for the assumption of random assignment. If years of compulsory schooling are randomly assigned, they should not

[17] We have to include the real compulsory schooling reforms in the regressions as well because for some birth cohorts, the real compulsory schooling level and the placebo compulsory schooling level overlap.

Table 7 Placebo reforms: Reduced-form estimates

	Sample 10		Sample 7	
	Reduced-Form	Reduced-Form +5-Year Placebo	Reduced-Form	Reduced-Form +5-Year Placebo
A. Immediate Memory (level)				
Compulsory schooling	0.049	0.042	0.065	0.057
	(0.022)*	(0.023)†	(0.025)*	(0.027)*
Placebo reform		–0.010		–0.021
		(0.024)		(0.029)
Observations	25,375	25,375	20,124	20,124
B. Delayed Memory (level)				
Compulsory schooling	0.058	0.072	0.068	0.081
	(0.026)*	(0.028)**	(0.030)*	(0.032)*
Placebo reform		0.041		0.038
		(0.031)		(0.035)
Observations	25,375	25,375	20,124	20,124
C. Fluency (slope)				
Compulsory schooling	–0.275	–0.278	–0.207	–0.191
	(0.111)*	(0.120)*	(0.121)†	(0.130)
Placebo reform		–0.011		0.046
		(0.115)		(0.131)
Observations	8,513	8,513	6,717	6,717

Notes: Each column and panel represents a separate regression. Country fixed effects, cohort fixed effects, and country-specific linear trends in birth cohorts are included in all regressions. Included in all level regressions are indicators for female, interview year, foreign-born, and indicators for potential interview impairments and other person in the room during cognitive tests. The slope regressions include indicators for the first interview year, indicators for potential interview impairments, and whether another person was present during both interviews, as well as the duration between the interviews. Heteroscedasticity- and cluster-robust standard errors are shown in parentheses (clusters are individuals).

† $p < .10$; * $p < .05$; ** $p < .01$

be related to any childhood socioeconomic characteristics. We estimate the effects of compulsory schooling years on predetermined childhood characteristics drawn from the SHARELIFE interviews in 2008/2009. These variables capture general socioeconomic characteristics as well as events that might be related to WWII: few 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; whether the accommodation had a fixed bath, hot running water supply, and central heating; whether the person was evacuated or relocated during a war; whether the person lived in a war camp, labor camp, or concentration camp (at any age); and whether the respondent experienced a distinct period of stress, financial hardship, and hunger. With the one noted exception, these variables refer to age 10.

Table 8 presents the results. Compulsory schooling is not correlated with most of these indicators. Only in the large sample do we find that a higher compulsory schooling level is associated with a higher likelihood of having a fixed bath in the

Table 8 Effects of compulsory schooling on predetermined variables

	Few Books	Skilled Breadwinner	Number of Rooms per Person	Fixed Bath	Hot Running Water	Central Heating	Relocation During War	Period of Stress	Period of Financial Hardship	Period of Hunger
Sample 10 Compulsory Schooling	–0.014	0.002	0.002	0.024	0.011	0.004	0.006	–0.002	0.003	0.003
	(0.010)	(0.009)	(0.005)	(0.013)†	(0.010)	(0.007)	(0.002)**	(0.001)	(0.002)*	(0.002)
Observations	5,863	5,709	5,812	5,893	5,893	5,893	5,913	5,830	5,907	5,911
Sample 7 Compulsory Schooling	–0.009	–0.001	–0.003	0.015	0.001	0.005	0.003	–0.002	0.002	0.002
	(0.010)	(0.010)	(0.005)	(0.014)	(0.010)	(0.007)	(0.002)	(0.001)	(0.002)	(0.003)
Observations	4,567	4,457	4,530	4,590	4,590	4,590	4,603	4,537	4,602	4,603

Notes: Outcome variables refer to outcomes at 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 was legislator; senior official or manager; professional or technician; or associate professional); number of rooms per person (the number of rooms in the accommodation divided by the number of persons in the household); fixed bath, hot running water, and central heating (the accommodation had a fixed bath, a hot running water supply, and central heating); relocation during war (person was evacuated or relocated during a war; or lived in a war camp, labor camp, or concentration camp at any age); and period of stress, financial hardship, or hunger (person experienced a distinct period of stress, financial hardship, or hunger). Each coefficient represents a separate linear regression. Country fixed effects, cohort fixed effects, country-specific linear trends in birth cohorts, indicators for interview year, female, and foreign-born are included in all regressions. Heteroscedasticity- and cluster-robust standard errors are shown in parentheses (clusters are country cohorts).

$^{\dagger}p < .10$; $^{*}p < .05$; $^{**}p < .01$

accommodation, a higher likelihood of being relocated during the war, and experiencing a period of financial hardship. These associations are not found with the smaller sample 7. Furthermore, most significant coefficients have the “wrong” sign: that is, younger cohorts with more compulsory schooling show somewhat higher levels of adverse childhood or war experiences. Therefore, we interpret these results as supporting evidence for our identification strategy.

Channels

Our analysis provides consistent evidence that schooling has a significant long-term effect on memory scores and somewhat weaker evidence on protective effects on fluency decline. There are several channels through which education might influence cognition decades after school completion, such as income, labor force participation, family relations, cognitive leisure activities, and physical and social activities, as well as health and health behaviors. Direct effects of education and training on brain functioning can also play a role.

In the medical literature, many studies have investigated 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). The Agency for Healthcare Research and Quality (2010) summarized the previous research and concluded that cognitive training, physical activity, noncognitive and nonphysical leisure activities, and a Mediterranean diet are negatively associated with the risk of cognitive decline. Furthermore, marriage seems to have a protective effect, whereas 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), who examined social participation and quality of life as mediating channels, we investigate potential channels by identifying the effects of schooling on outcomes that are thought to influence cognitive decline. Table 9 shows 2SLS estimates of years of education on factors that are assumed to influence cognitive outcomes for males and females, such as labor force status (employed, retired),[18] marriage, having children, social and physical activities, smoking, diabetes, and depression.

As indicated earlier, individual years of education are instrumented with mandatory years of schooling. For males, we find that education increases the probability of being employed and reduces the likelihood of retirement at the time of the interviews. For females, these channels seem to play a minor role; instead, for females, we obtain significant coefficients for being married and having children.[19] Moreover, education leads to higher participation in clubs for both females and males. These effects are statistically significant only in the narrower sample. For the other outcomes, we don’t find any significant effects. Although this evidence is only suggestive, it shows that just a few of the factors known from the epidemiological literature to influence or to be correlated with cognitive functioning for seniors are themselves affected by increases in

[18] See Bonsang et al. (2012) or Bingley and Martinello (2011) for a discussion of the relationship between retirement and cognitive functioning.

[19] For a detailed analysis on the effects of education on fertility using compulsory schooling reforms in several European countries, see Fort et al. (2011).

Table 9 2SLS estimates of years of education on mediating outcomes

	Employed	Retired	Married	Children	In Clubs	Socially Active	Vigorously Active	Smoking	Diabetes	Depressed
Males										
Sample 10	0.104	–0.101	0.022	0.032	0.045	0.002	0.031	0.065	–0.033	–0.038
	(0.039)**	(0.038)**	(0.033)	(0.025)	(0.030)	(0.027)	(0.033)	(0.043)	(0.023)	(0.024)
Observations	11,516	11,523	11,578	11,522	11,438	11,471	11,534	11,530	11,556	11,297
Sample 7	0.063	–0.051	0.039	0.041	0.060	0.023	–0.006	0.021	–0.031	–0.040
	(0.035)†	(0.031)	(0.036)	(0.028)	(0.034)†	(0.028)	(0.034)	(0.041)	(0.024)	(0.027)
Observations	9,132	9,135	9,177	9,128	9,070	9,096	9,149	9,141	9,161	8,945
Mean of outcome	0.462	0.448	0.794	0.892	0.270	0.695	0.423	0.301	0.115	0.153
Females										
Sample 10	0.054	0.014	0.072	0.055	0.038	0.003	–0.006	–0.001	–0.009	0.001
	(0.030)†	(0.025)	(0.040)†	(0.028)†	(0.028)	(0.027)	(0.029)	(0.033)	(0.017)	(0.030)
Observations	14,095	14,102	14,151	14,098	14,037	14,056	14,111	14,116	14,132	13,952
Sample 7	0.039	0.049	0.057	0.085	0.089	–0.005	0.024	–0.007	–0.020	0.017
	(0.036)	(0.036)	(0.049)	(0.045)†	(0.044)*	(0.036)	(0.038)	(0.043)	(0.022)	(0.038)
Observations	11,197	11,202	11,237	11,194	11,144	11,161	11,211	11,210	11,230	11,080
Mean of outcome	0.319	0.453	0.703	0.916	0.228	0.705	0.332	0.216	0.083	0.282

Notes: Outcome variables are defined as follows: employed (employed or self-employed), retired (retired), 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 per week), smoking (is a current smoker), diabetes (was diagnosed with diabetes), and depressed (scores >3 on EURO-D scale). Each coefficient represents a separate linear 2SLS regression. Country fixed effects, cohort fixed effects, country-specific linear trends in cohorts, indicators for interview year, and foreign-born are included in all regressions. Heteroscedasticity- and cluster-robust standard errors are shown in parentheses (clusters are individuals).

† $p < .10$; * $p < .05$; ** $p < .01$

schooling. Our results are consistent with a pattern of traditional gender roles: the driving force of the protective effect of education on cognition seems to be labor market status for men and family status for women.

Conclusions

Improving cognitive functioning at the end of working life is important for aging societies not only 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 countries) have—in a global perspective—a relatively high level of cognitive functioning (Skirbekk et al. 2012). 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 cognitive outcomes several decades after school completion. Studying the effects of exogenous variation in individual years of schooling induced by compulsory schooling reforms in a sample of six European countries, we find strong evidence for a positive causal effect of education on memory performance—in particular, on delayed memory. One year of education increases the delayed memory score by approximately 10 % of a standard deviation. Interestingly, we do not find consistent effects on verbal fluency, numeracy, and orientation-to-date. Moreover, our study provides the first evidence that longer schooling can lead to a reduction in cognitive decline in terms of verbal fluency. All our coefficients are typically larger in magnitude for men and for those with a higher socioeconomic family background.

We investigate potential channels through which education might influence cognition by studying the effects of education on outcomes that are suspected to influence cognitive outcomes. Although we do not find any causal effects of education on physical activities, smoking, diabetes, and depression, we obtain some significant estimates for social activities in clubs. Furthermore, prolonged labor force participation for European men and family relations (in terms of marriage and having children) for women seem to play an important role in sustaining cognitive functioning. 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 compulsory schooling can lead to long-term improvements in cognitive ability. This matters because 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 aging and an accompanying need to increase the retirement age.

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Appendix: Educational Reforms in Europe

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

Austria. In 1962, a federal act was passed that increased compulsory schooling from eight to nine 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. Because 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 twentieth century, compulsory education has been reformed several times. In 1948, compulsory schooling was increased from eight to nine years (age 6 to 15). It was reduced to eight years in 1953 and increased to nine 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 three years, from four to seven years of schooling. In 1971, compulsory schooling was further increased by two years, from seven to nine years. Education started at age 7; thus, pupils who were 11 years old (or younger) in 1958—that is, children born in 1947 or later—were potentially affected by the first reform. Because our data cover only those individuals aged 50+ and their younger spouses, we consider only the first reform for this study.

France. Two education reforms were implemented in France. In 1936, compulsory schooling was increased from seven to eight years (from ages 6–13 to ages 6–14); and in 1959 from eight to 10 years (ages 14–16). After a long transition period, the second reform came into effect in 1967. For this analysis, we consider only the second reform, with the pivotal cohort being born in 1953.

Germany. In the former Federal Republic of Germany, compulsory schooling was increased from eight to nine years of schooling. Because students start school at age 6, this was an increase in the school-leaving age from 14 to 15. The reform took place gradually in the 10 German states, starting from 1949 in Hamburg to 1969 in Bavaria. The first birth cohorts potentially affected by these reforms range from 1934 in Hamburg to 1955 in Bavaria.

Italy. In 1963, junior high school became mandatory, which increased years of mandatory schooling by three years, from five to eight years of schooling. This reform increased the compulsory school career from ages 6–11 to 6–14. The first cohort potentially affected by this reform is the cohort born in 1949.

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