Demography (2023) 60(4):1139–1161 Published online: 20 July 2023
DOI 10.1215/00703370-10875853

Does Schooling Have Lasting Effects on Cognitive Function? Evidence From Compulsory Schooling Laws

Emma Gorman

ABSTRACT This study assesses whether an additional year of secondary schooling has lasting causal effects on cognitive function. I use data from Understanding Society, the largest longitudinal household study in the United Kingdom, and exploit quasi-experimental variation in schooling from the 1972 raising of the school-leaving age in England and Wales. This reform increased the minimum secondary school–leaving age from 15 to 16 years. Cognitive function outcomes were measured when participants were aged 48 to 60. Using a fuzzy regression discontinuity design, I show that remaining in school until age 16 improved working memory by one third to one half of a standard deviation. I find limited evidence for causal effects on verbal fluency and measures of numeric ability. Analyses of potential mechanisms showed statistically significant effects of remaining in school until age 16 on the type of occupation entered immediately after leaving school and at older ages. These patterns are consistent with basic education improving cognitive outcomes through occupation choice. The findings are robust to sensitivity analyses.

KEYWORDS Cognition • Health behaviors • Aging • Education • Occupation

Introduction

Cognitive function shapes economic, social, and health outcomes over the life cycle (Heckman et al. 2006; Wraw et al. 2015). The efficacy of early intervention in improving cognitive function is well studied, but less is known about whether these outcomes are malleable beyond early childhood (Heckman 2007). This study assesses whether secondary schooling has lasting causal effects on cognitive function measured four decades after leaving school. In later life, cognitive performance becomes an important component of healthy aging and continued independent functioning (World Health Organization 2015). Among healthy individuals without clinical cognitive impairment, age-related reductions in cognitive performance can impede the ability to perform daily activities (Boyle et al. 2012; Tucker-Drob 2011) and manage financial planning decisions (Hsu and Willis 2013). Such impediments have important implications for continued labor market engagement and retirement planning, given rising state pension ages and the central policy focus of supporting

ELECTRONIC SUPPLEMENTARY MATERIAL The online version of this article (https://doi.org/10.1215/00703370-10875853) contains supplementary material.

continued labor market participation (OECD 2006; UK Department for Work and Pensions 2013).

Yet, cognitive impairment is not an inevitable consequence of aging. A large body of work has sought to uncover modifiable risk factors across the life course. Basic education is one promising candidate for improving cognitive outcomes. Despite extensive observational research demonstrating that schooling is a correlate of cognitive outcomes throughout the life cycle (Plassman et al. 1995), few studies have been able to identify a causal association. The main challenge to the credible identification of a causal effect is omitted variable bias: those with greater cognitive ability in early life may select into higher levels of education. Factors such as childhood health or particular genetic variants may influence both cognition and educational attainment in later life (Arden et al. 2015; Boardman et al. 2015). One approach, which I leverage in this study, is to exploit the substantial investments in publicly provided education occurring over the last century: notably, the successive increases in the minimum age at which students may leave secondary school. These increases in the minimum school-leaving age represent plausibly exogenous changes in schooling and are widely employed to quantify pecuniary and nonpecuniary returns to education (Harmon and Walker 1995; Oreopoulos and Salvanes 2011). The cohorts affected by these reforms are now aging, providing researchers an opportunity to explore whether the effects of schooling persist into later life.

The United Kingdom is an especially informative laboratory for examining the causal effects of schooling. The minimum school-leaving age was raised from 14 to 15 years in 1947 and was raised again to 16 years in 1972. These changes affected relatively large shares of the relevant cohorts. For example, the corresponding changes in the United States exerted a causal effect on only approximately 5% of the student cohort, compared with roughly one half and one third for the 1947 and 1972 reforms in the United Kingdom, respectively. Yet, the evidence base on schooling and cognitive function in the United Kingdom remains limited, and little is known about the underlying mechanisms driving these long-run effects.

For England and Wales, Banks and Mazzonna (2012) established positive effects of an additional year of schooling on aspects of cognitive function among the cohorts affected by the 1947 raising of the school-leaving age. The findings are large in magnitude, raising the possibility that increased population education could function as a policy tool to foster healthy aging. Given the large total effect size, education's protective effect on cognition is unlikely to be wholly a direct effect. More plausibly, education influences cognition indirectly through the wider set of opportunities that education affords. However, the cohorts exposed to the 1947 reform grew up in unstable economic and political circumstances; their choice set and constraints were vastly different from those of more recent cohorts. It is unclear whether the channels through which that reform operated remain relevant in the current policy context, despite the changing nature of the work tasks, labor market participation, and health technology. The current study therefore examines more recent cohorts.

The contribution of the present study is to provide new evidence on the causal effects of basic education on a range of cognitive function outcomes, using a fuzzy regression discontinuity design to exploit the 1972 raising of the secondary school–leaving age. This reform increased the minimum school-leaving age from 15 to 16 years in England and Wales. I use data from Understanding Society, the largest household panel study in the United Kingdom. Crucially, these data contain granular information on the birth month and year, allowing exact identification of exposure to the reform. Comparing the outcomes of observationally similar individuals who were exposed versus unexposed to the reform yields credible estimates of the causal effects of the additional year of schooling.

One threat to the validity of this approach is confounding from secular trends in longevity and cognition: cohort-specific cognitive performance has been steadily increasing over the twentieth century, a trend referred to as the *Flynn effect* (Flynn 1987). This effect may be influenced not only by investments in mass education during this time but also by the increased cognitive stimulation of job tasks, improvements in nutrition, and the increasing proportion of women entering the labor force—elements that act as confounding factors (Case and Paxson 2009; Skirbekk et al. 2013). To reduce concerns about this type of confounding, I adjust for birth cohort trends and use a small sample window of within roughly three years of the reform date to enhance the comparability of the treatment and control units.

More generally, little is known about the channels that drive the education–cognition gradient. Several hypotheses highlight the role of occupation type—especially regarding occupational complexity—and the commonly cited use-it-or-lose-it hypothesis. This framework suggests that mental stimulation may sustain brain function, supporting the idea that more cognitively stimulating activities stave off cognitive decline (Rohwedder and Willis 2010). Similarly, proponents of cognitive reserve theories in neuropsychology have proposed education and occupation as key factors in increasing mental resilience and thereby reducing the clinical manifestations of brain aging (Stern 2002). I therefore assess whether the data are consistent with occupation choice as a plausible mechanism shaping the effect of education on cognition.

The key findings include a statistically significant local average treatment effect (LATE) of remaining in school until age 16 on working memory of two fifths of a standard deviation, which ranges from one third to slightly more than one half of a standard deviation depending on the sample definition and model specification employed. This effect is significant at the 10% level or better across a range of reasonable specifications and exceeds the ordinary least-squares (OLS) estimate of 0.15 standard deviations. Although the OLS estimates show a positive association between schooling and numeric ability and verbal fluency, evidence for a causal effect on these outcomes is negligible. In contrast to previous studies, this study finds mainly larger effect sizes among women, although the difference between men and women was not statistically significant. In terms of potential mechanisms, analyses of the effects of schooling on intermediate variables show that remaining in school until age 16 causally impacts occupation. The findings provide new evidence that basic education causally improves working memory—an important component of cognitive function at older ages. Increasing population levels of education may slow the growth in the burden of cognition-related disease and support economic adjustment to an aging population.

Related Literature

Recent empirical studies have used variation in minimum schooling laws to examine the lasting effects of education on a range of nonpecuniary outcomes, including mortality, self-assessed health, disability, and health behaviors (recent reviews include Lochner 2011; Mazumder 2012). However, fewer studies have focused on cognitive outcomes. The first study to exploit changes in the minimum school-leaving age to explore cognition and mental outcomes was Glymour et al. (2008), which used U.S. state-level variation to obtain instrumental variable (IV) estimates of the effect of schooling on working memory and mental status among cohorts born between 1900 and 1947. These changes in mandated schooling yielded large improvements in performance on memory tests in old age, although no effects were detected for a more general screening test for cognitive status.

Banks and Mazzonna (2012) exploited the 1947 school reform in England and Wales, which increased the minimum school-leaving age from age 14 to 15, finding a large positive effect of an extra year of schooling on older men's memory and executive function. However, the cohorts examined in this study grew up in very specific economic and political circumstances, facing the Great Depression and the consequences of the war, and experienced a vastly different schooling landscape than more recent cohorts. Whether the channels through which that reform operated remain relevant in the current policy context is unclear. Also unclear is whether the findings derived from the increased school-leaving age from 14 to 15 are applicable to the even higher levels of schooling mandated in more recent decades.

Davies et al. (2018) used data from the UK Biobank, a large study of biomarkers and other health-related data, to exploit the 1972 increased school-leaving age to assess the effects of schooling on outcomes such as fluid intelligence measured via completion of 13 logic puzzles. Their IV estimates revealed small positive effects of an additional year of schooling on intelligence, smaller than the corresponding OLS findings. However, an important caveat of these findings is that the UK Biobank is not a population-representative sample and has documented health- and socioeconomic-related sample selection, which can bias causal effects if the characteristics correlated with nonparticipation are also correlated with the causal effect of interest. This bias is especially relevant for older age groups, for whom selective mortality issues can arise. A comparison with nationally representative data sources showed that at ages 70–74, rates of all-cause mortality and total cancer incidence were, respectively, 46.2% and 11.8% lower among men and 55.5% and 18.1% lower among women relative to the general population of the same age (Fry et al. 2017).

Several studies have exploited time series and geographical variation in school-leaving ages across Europe by using the Survey of Health, Ageing and Retirement in Europe (SHARE). Schneeweis et al. (2014) found a positive effect of an extra year of education on memory and used the panel aspect to reveal evidence of a protective effect against declines in verbal fluency with age. In addition to finding increased memory scores among older men, Mazzonna (2014) documented reduced probabilities of depression and improved self-reported health. These patterns are not limited to Europe and the United States: Huang et al. (2013) leveraged educational differences in primary school completion due to China's Great Famine of 1959–1961 to assess education effects on cognitive outcomes at older ages. The results revealed a protective effect of cognition, especially episodic memory, of up to 20%.

Generally, these studies documented LATE estimates that exceeded OLS estimates. Crespo et al. (2014) used SHARELIFE data—the third wave of the SHARE panel study, containing retrospective life history data—to examine early-life characteristics of individuals whose behavior was altered by the implementations of the changes in schooling laws and who may have different returns to schooling than the wider population. The authors found larger effects of the reforms on years of education among individuals with lower socioeconomic status (measured by reports of living in a dwelling with two or fewer rooms) and individuals reporting better childhood health status. Overall, they found large, positive effects of extra schooling on memory and depression. Stratifying the sample by early-life characteristics revealed variation in point estimates of the causal effect of education, although these differences were not statistically significant.

Given the large total effect size of education on later outcomes found in many studies, the protective effect of education on cognition is unlikely to be wholly a direct effect. More plausibly, education influences cognition indirectly through the wider set of opportunities that education affords. Protective effects of schooling on later-life functioning could operate through any of several plausible channels. A recent review of empirical studies identified self-reported health, biomarkers of physiological health (e.g., inflammation), cardiovascular disease and its risk factors, education, occupational trajectories, and other characteristics as risk factors for age-associated cognitive decline (Deary et al. 2009).

Occupation is a particularly important channel. In the neurological literature, *cognitive reserve* is a key framework relating education, occupation, and cognitive outcomes (Stern 2002). It is a hypothetical construct used to explain substantial variation in cognitive aging and diagnosed cognitive impairment observed across individuals despite similar neurodegenerative changes. In other words, cognitive reserve is the proposed explanation for differences among individuals in everyday cognitive functioning declines for a given decline in brain health. Education and occupational complexity are often cited as important predictors of cognitive reserve, as are other lifestyle factors, such as dietary habits and physical exercise. The hypothesis suggests that individuals with a greater cognitive reserve level may require a greater pathology level to experience clinical manifestations of any diagnosable impairment.

Several well-studied channels, especially those related to health behaviors, are likely to operate. This paper focuses on cognition as the main outcome and on occupation as a mechanism. The empirical evidence for a causal effect of education on health outcomes and behaviors is mixed. Many credible studies have failed to detect an effect on health knowledge (Johnston et al. 2015), behaviors, and outcomes (Oreopoulos 2008; Oreopoulos and Salvanes 2009). Among the ample research on this topic, the most relevant UK-based studies also exploited schooling reforms. In the most comprehensive study, using two UK reforms, Clark and Royer (2013) failed to detect positive effects of education for a wide range of health outcomes and related behaviors, including mortality, self-reported health, objective health measures (e.g., obesity, being overweight, or high blood pressure), and health-related behaviors (e.g., vitamin intake or consumption of fruits and vegetables). Evidence from Europe and the United States has shown either null results (Albarrán et al. 2020; Meghir et al. 2012) or small positive effects of schooling on health and mortality (Fletcher 2014; Gathmann et al. 2014). Although the results overall are mixed, some evidence suggests a causal effect of education on smoking (de Walque 2007; Grimard and Parent 2007) and points to smoking as a potential mechanism underlying the education–health gradient (Brunello et al. 2016).

Setting, Data, and Methods

School Reforms in England and Wales

Many studies have leveraged the 1972 increase in school-leaving age to study the returns to education (Clark and Royer 2013; Dickson et al. 2016; Oreopoulos 2008). Because grade retention (repeating a grade) in the United Kingdom is rare (at roughly 1%), staying at school for an additional year generally implies an increase in the grade levels achieved (Borodankova and Coutinho 2011). The 1944 Education Act (enacted on April 1, 1947) increased the minimum secondary school–leaving age from 14 to 15 years and conferred power to raise the minimum school-leaving age again to 16 years. In March 1972, the minimum school-leaving age was raised to 16 years for school cohorts born on September 1, 1972, or later (Woodin et al. 2012). This reform has been widely used to study the pecuniary and nonpecuniary returns to schooling because no coincident policy changes occurred at this time, allowing researchers to isolate the impact of years of schooling. Although the 1947 increase in the school-leaving age has also been widely analyzed, the cohorts exposed to that change were affected by the aftermath of the Great Depression and the effects of World War II. The 1972 change, aside from being more recent, was enacted in more stable times. Considering this more recent reform may shed light on whether the results from previous studies using the 1947 change are generalizable, despite the context particularities those earlier cohorts faced.

In addition to ensuring an extra year of school completed, the 1972 change led to greater rates of completion of formal qualifications, with many more students staying in school until the end of their 16th school year to obtain O-level qualifications. This increase in the probability of gaining a qualification is important. Dickson and Smith (2011) exploited an institutional school-leaving rule providing exogenous variation in qualification attainment. They confirmed that some of the observed wage returns to an extra year of schooling in 1972 were likely due to staying in school to gain a qualification at age 16 in addition to the additional length of time in school.

Data and Variables

This study draws on Understanding Society, a panel study of households in the United Kingdom (McFall 2013; University of Essex et al. 2015) that began in 2009 as a representative probability sample of approximately 40,000 households. Participants are interviewed annually, but each wave of data collection spans 24 months, creating overlapping two-year survey waves. The total sample comprises multiple subsample components: the main General Population Sample (GPS), continuing British Household Panel Study (BHPS) members, and the Ethnic Minority Boost Sample (EMBS). Wave 3—conducted from January 7, 2011, to July 12, 2013—included a cognitive ability module measuring self-rated memory, word recall test performance, numeric ability, and verbal fluency (McFall 2013). Wave 3 has a cross-sectional response rate of 61.3%. The analyses here use the survey weights provided with the data to allow for the possibility of endogenous sampling design and response probabilities. The weights adjust for the complex survey design and combined probabilities of being selected into the BHPS, GPS, and EMBS and continuing to Wave 3 of the survey.

Variable Construction

Years of Schooling and Cognitive Outcomes

The measure of schooling is the report of years of secondary schooling completed, constructed as the age at which the respondent reported leaving school minus 5.

Memory

Episodic memory—retrieving memories associated with specific events—was measured by performance on the immediate and delayed Word Recall test. Memory is an important early indicator of potential cognitive impairment, and word recall tests are used in cognitive impairment screening tests (Kim et al. 2014). After the computer reads a list of 10 words aloud, the respondent is asked to repeat these words in any order; the interviewer records the number of words correctly recalled. This procedure is repeated approximately 5 minutes later in the module to measure delayed recall. Immediate and delayed word recall scores are highly correlated with clinical dementia diagnoses (Wu et al. 2013). The number of words correctly recalled in each test were summed, creating a variable ranging from 0 to 20.

Serial Subtraction

The Serial 7 Subtraction test is a component of clinical screening instruments for cognitive impairment: the Mini-Mental State Examination (MMSE; Crum et al. 1993) and the Cambridge Mental Disorders of the Elderly Examination (CAMDEX; Roth et al. 1986). In the Serial 7 test, respondents are asked to begin at 100 and subtract 7. Respondents are then prompted to subtract 7 from that result, a process that repeats until respondents have subtracted 7 a total of five times. The number of correct answers was summed to create a variable ranging from 0 to 5.

Verbal Fluency

The Verbal Fluency test asked respondents to name as many animals as possible in 1 minute. In addition to assessing semantic memory, this test assesses executive function because it requires some level of abstract thinking and mental flexibility within a time limit. The test is from the cognitive assessment component of the CAMDEX, an interview procedure for the diagnosis and measurement of dementia in the elderly (Roth et al. 1986), and has been successfully employed in extensions of the MMSE (Kim et al. 2014). The number of animals listed ranges from 0 to 71.

Numeric Ability

Assessing the ability to solve everyday numerical problems, the Numeric Ability test asked the respondent five questions of increasing complexity. For example, the first question asked, “In a sale, a shop is selling all items at half price. Before the sale, a sofa costs £300. How much will it cost in the sale?” The number of questions correctly answered was summed, with a range of 0–5.

The distributions of the raw scores are presented in the online appendix (section A). The continuous measures—*Word Recall* and *Verbal Fluency*—were standardized by subtracting the sample mean and dividing by the sample standard deviation to facilitate interpretation and comparability with previous studies. The Serial 7 and Numeric Ability tests were dichotomized to create a binary variable. *Serial Subtraction* equals 1 for respondents with five correct answers to the Serial 7 test and 0 otherwise. *Numeric Ability* equals 1 for respondents with four or five correct answers to the Numeric Ability test and 0 otherwise.

Occupation Variables

I measure occupation using the five-class National Statistics Socio-economic Classification (NS-SEC). The NS-SEC groups occupations defined by the Standard Occupational Classification into five categories: (1) semi-routine, routine, never-worked, and long-term unemployed; (2) lower supervisory and technical occupations; (3) small employers and own account workers; (4) intermediate occupations; and (5) higher professional, large employers, higher managerial, and administrative occupations. The ordering of NS-SEC captures information on the tasks used in the occupation and the employment relation in terms of the amount of autonomy a worker has in their job.

The NS-SEC is available for the first job chosen after leaving secondary school and the current job (or previous job, if the respondent is currently out of work). The first job is relevant because the activity chosen immediately after secondary school is especially amenable to policy intervention. For instance, in the United Kingdom, a current policy development increased the minimum age for remaining in education or training from 16 to 18 years. This initial start may have long run implications for labor market trajectories. The current (or most recent) occupation captures effects associated with the use-it-or-lose-it hypothesis: continuing to engage in a stimulating occupation maintains cognitive reserve and staves off cognitive decline.

Analytic Sample

The analyses use all three Understanding Society subsamples: the GPS, continuing BHPS participants, and the EMBS. I used survey weights throughout the analyses to adjust for unit nonresponse—the combined probabilities of being selected into the BHPS, GPS, and EMBS and continuing to Wave 3 of the survey—and the complex survey design. Because the school location was unavailable for the cohorts considered, I could not ascertain whether respondents completed their schooling in England or Wales rather than Scotland or Northern Ireland. To restrict the sample to individuals who completed their schooling in areas exposed to the reform as best as possible, I selected those who were born in England or Wales for analyses. I assumed that

Table 1 Percentage of the sample leaving school at each age before the reform (nontreated) and after the reform (treated), by gender

	Men			Women		
Age at School-Leaving	Nontreated	Treated	Total	Nontreated	Treated	Total
15	32.9	7.4	19.1	33.2	9.6	20.1
16	34.1	60.6	48.5	36.8	61.5	50.5
17	11.0	10.8	10.9	9.1	9.7	9.4
18	19.9	19.4	19.6	19.9	18.5	19.1
19	2.2	1.7	1.9	1.1	0.8	0.9
Total	100	100	100	100	100	100

Source: Understanding Society.

observations with missing or unavailable information on birthplace were not born in England or Wales. In the preliminary descriptive statistics, I used a sample of respondents born within five years of the date determining exposure to the school reform (September 1, 1957).

Descriptive Statistics

Table 1 shows the percentage of the sample leaving school at each age, separately for men and women, reported before and after the increased school-leaving age. The reform induced more students to stay in school until age 16. In line with previous studies, spillover effects into higher levels of schooling are rare; the percentage leaving school at ages 17, 18, or 19 is similar before and after the reform (Chevalier et al. 2004). Therefore, the estimates in these analyses are informed by those staying until age 16 rather than 15 and may not be generalizable to other margins. Some noncompliance exists: some students still reported leaving at age 15 even after the reform. As Clark and Royer (2013) noted, this early school-leaving is due to the institutional feature allowing students to leave school in the summer exam period (mid-June) upon completion of the O-level exam in their 16th year of school. For summer-born students, then, some pupils were age 15 when they completed their exam and left school.

Table 2 presents summary statistics for key variables for respondents within five years of the treatment threshold (a birthdate of September 1, 1957). (See the online appendix for histograms of these outcomes.) The average age in the sample is roughly 54 years; the sample had an average of 11 years of schooling.

Figure 1 plots the mean years of school by birth cohort relative to the cutoff of September 1, 1957. The discontinuity in mean years of schooling is evident in the full sample. Figure 2 shows the corresponding graphs for each cognitive outcome for respondents born within five years of the treatment threshold (September 1, 1957). A quadratic trend in birth cohort was fitted to underlying data to show the global trends in the data (dashed red lines), as well as a local linear fit within 50 months of the cutoff (solid blue lines).

Table 2 Summary statistics for demographic and cognitive variables within five years of the reform threshold (birthdate of September 1, 1957)

	Mean	SD	Min.	Max.	*n*
Female (proportion)	.52	.50	0	1	4,915
Age	53.5	2.94	48	60	4,915
Years of Schooling	11.3	1.04	10	14	4,833
Word Recall	11.6	3.2	0	20	4,797
Verbal Fluency	22.9	6.8	0	71	4,877
Numeric Ability	0.53	0.50	0	1	4,864
Serial Subtraction	0.71	0.45	0	1	4,779

Source: Understanding Society.

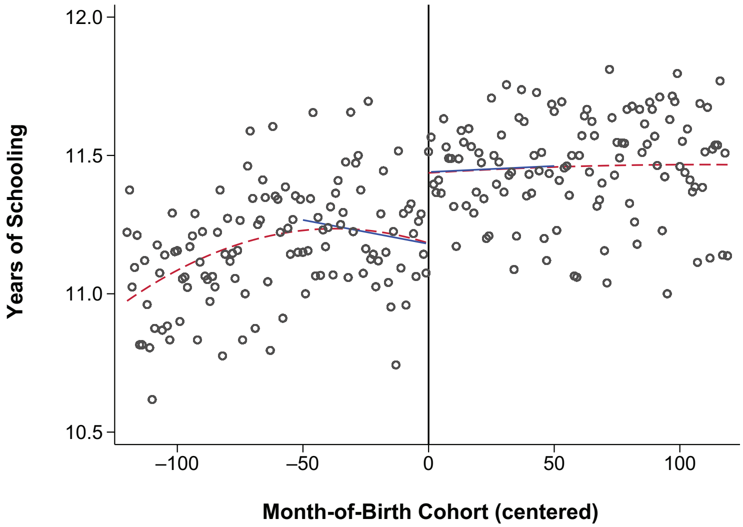

Fig. 1 Years of schooling by month-of-birth cohort, centered relative to the reform threshold of September 1, 1957. A quadratic trend in birth cohort was fitted to underlying data to show the global trends in the data (dashed red lines), as well as a local linear fit within 50 months of the reform threshold (solid blue lines). *Source:* Understanding Society.

Empirical Approach

I employ a fuzzy regression discontinuity (FRD) design (Imbens and Lemieux 2008) to exploit variation in schooling induced by the reform. Regression discontinuity (RD) is predicated on treatment D for individuals $i = 1, \ldots N$ being assigned by the value of a continuous covariate, R_i. In this case, R_i is the month and year of birth, which falls on either side of a fixed cutoff, c. In this application, c is the pivotal birth cohort of September 1, 1957. In an FRD design, c induces a discontinuity in the conditional probability of receiving the treatment given R_i—but not necessarily a jump from 0 to 1, as in a sharp RD. Let $Y_i(1)$ and $Y_i(0)$ denote the potential cognitive outcomes experienced in the presence and absence of the treatment, respectively. Let Z_i denote

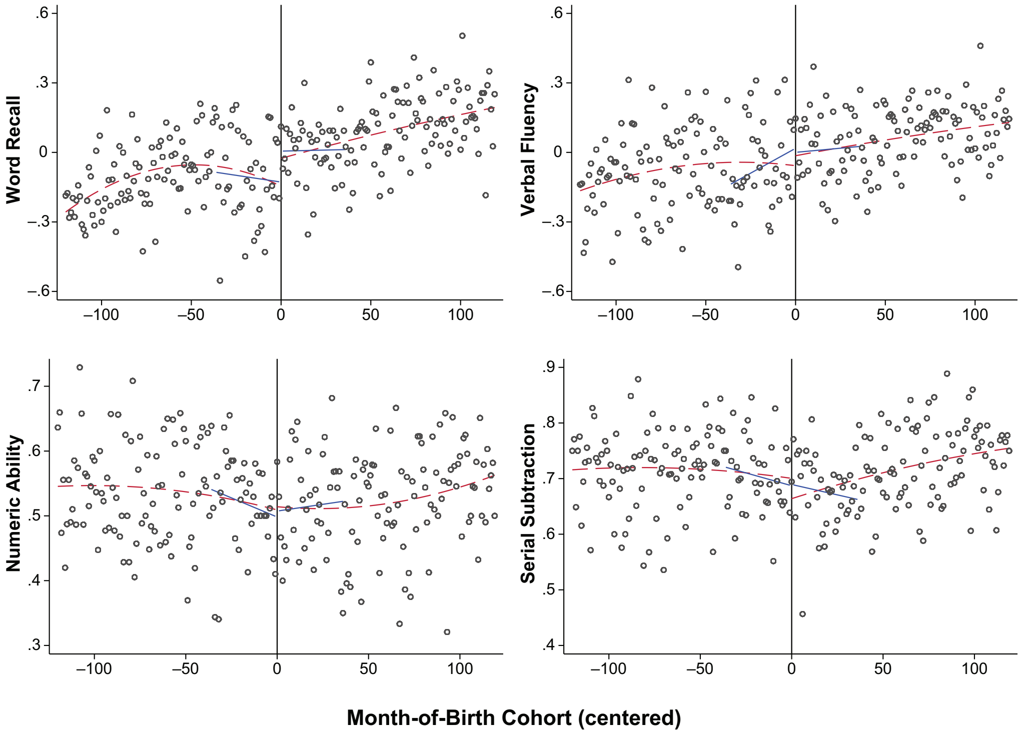

Fig. 2 Mean cognitive outcomes by birth cohort, centered relative to the reform threshold of September 1, 1957. A quadratic trend in birth cohort was fitted to underlying data to show the global trends in the data (dashed red lines), as well as a local linear fit within 50 months of the reform threshold (solid blue lines). *Source:* Understanding Society.

exposure to the reform; the treatment assignment rule is $Z_i = 1[R_i > c]$, where 1[] is the indicator function and the treatment is years of schooling. $D_i(1)$ and $D_i(0)$ denote the potential treatments depending on the value of instrument Z_i. The FRD treatment effect, τ_{FRD}, is the ratio of the effect of the reform on the outcome at the cutoff and the effect of the reform on treatment at the cutoff:

$$\tau_{FRD} = \frac{\lim_{r \downarrow c} E\left[Y_i \mid R_i = r\right] - \lim_{r \uparrow c} E\left[Y_i \mid R_i = r\right]}{\lim_{r \downarrow c} E\left[D_i \mid R_i = r\right] - \lim_{r \uparrow c} E\left[D_i \mid R_i = r\right]}$$

$$= E\left[Y_i(1) - Y_i(0) \mid R_i = c, D_i(1) > D_i(0)\right]. \quad (1)$$

Assuming the reform changes behavior in only one direction (monotonicity), the FRD estimator yields a LATE at the cutoff, c. Under this monotonicity assumption, the LATE is commonly estimated using two-stage least-squares (2SLS) regression (Hahn et al. 2001). The treatment in this application, years of schooling, can take multiple discrete levels. Hence, the compliers are defined as those induced to take at least d years of schooling when otherwise they would have taken fewer than d (Angrist and Imbens 1995). In this case, the estimated treatment effects are an average of the per-year treatment effects associated with each additional year of schooling—for the compliers at each level of schooling—weighted by the proportions of compliers at each level of schooling. In this application, little weight is placed on schooling levels above age 16; the treatment effect is informed by differences in outcomes between ages 15 and 16. Therefore, I interpret the results as the effects of remaining in school until age 16.

Using a uniform kernel and the same bandwidth (h) for the outcome and treatment equations leads to the numerical equivalence between the FRD estimator and the 2SLS estimator (Hahn et al. 2001). Taking this approach, I used 2SLS to estimate the parameters of Eqs. (2) and (3). Let Y_i denote the cognitive outcome observed for individual i; $f(R_i - c)$ comprises the centered running variable, month and year of birth, interacted with the reform dummy variable. The vector $\mathbf{X}_i$ includes pretreatment covariates, and u_i and ϑ_i are idiosyncratic errors. The estimating equations are as follows:

$$Y_i = \alpha + \alpha_1 \hat{D}_i + f(R_i - c) + \mathbf{X}_i{}'\alpha_2 + u_i, \tag{2}$$

$$D_i = \gamma + \gamma_1 Z_i + g(R_i - c) + \mathbf{X}_i{}'\gamma_2 + \vartheta_i. \tag{3}$$

To select a data-driven optimal bandwidth, which minimizes an approximation to the asymptotic mean squared error of the fuzzy RD point estimator, I used the implementation of the mean squared error–optimal bandwidth that Calonico et al. (2016) developed. The use of an optimal bandwidth was complemented by a sensitivity analysis through a consideration of a range of bandwidths. In line with Kolesár and Rothe's (2018) recommendations, I used heteroskedasticity-robust standard errors. Clustering by month and year of birth, as Lee and Card (2008) suggested, produces similar results, albeit with small standard errors.

In very small sample windows around the reform date, it is more credible that the reform, as a local IV, is plausibly exogenous without conditioning on further covariates. Other discontinuities exactly coincident with the increased school-leaving age reform date are unlikely, which I explore by ensuring that pretreatment characteristics are smooth across the cutoff, analogous to balance tests of background characteristics in settings with full randomization. In this case, involving estimation within a very small window, the covariates increase the estimates' precision by reducing residual variation. With larger sample windows, the concern remains that education level and cognitive function might be confounded by unobserved functions of birth cohort. For example, later cohorts who were exposed to the reform may have experienced more favorable conditions in early childhood and may have more educated parents than cohorts not exposed to the reform. Although these trends are unlikely to be discontinuous at the treatment cutoff, they may still be picked up in larger sample windows. Therefore, this concern motivates adjustment for potential confounding variables that may capture such effects. The covariates included in the RD models presented in Table 3 are a quadratic term in age and dummy variables indicating gender, interview month, and interview year (because the Understanding Society survey waves span approximately two years).

Table 3 Ordinary least squares (OLS) and regression discontinuity (RD) estimates of the effect of years of schooling on cognitive outcomes

	Word Recall		Verbal Fluency		Numeric Ability		Serial Subtraction	
	OLS	RD	OLS	RD	OLS	RD	OLS	RD
Years	0.24	0.42	0.23	0.05	0.15	–0.05	0.07	–0.00
p Value	.00	.03	.00	.80	.00	.64	.00	.98
SE	0.00	0.19	0.00	0.20	0.00	0.11	0.00	0.10
First-Stage	—	0.38	—	0.36	—	0.35	—	0.36
F Statistic	—	24.8	—	21.3	—	21.0	—	21.3
Bandwidth	—	38	—	34	—	34	—	33
n	2,937	2,937	2,667	2,667	2,654	2,654	2,541	2,541

Notes: Each model adjusts for the following covariates: a linear trend in month-of-birth birth cohort (interacted with the reform dummy variable); indicators for gender, interview month, and interview year; and a quadratic term in age. Survey weights adjusting for unit nonresponse and sample design were used in all specifications. SEs are robust standard errors. The *F* statistic is the robust *F* statistic for the first-stage regression. Bandwidth refers to the number of month-of-birth birth cohorts included in the estimation sample on each side of the treatment cutoff, selected using a data-driven procedure.

Source: Understanding Society.

In further sensitivity analyses (shown in the online appendix, section B), I added dummy variables for birth month as covariates to capture seasonality effects—that is, systematic variation in birth month by family background that could also be related to later education, health, and cognitive outcomes (Buckles and Hungerman 2013). Additionally, because the 1972 reform implementation coincided with the start of the school term, exposure to the reform will coincide with any age-in-grade effects and therefore be correlated with schooling and perhaps cognitive outcomes. In sensitivity analyses, I also examined a wider sample window (within five years of the cutoff) in conjunction with a quadratic trend in the running variable. Higher order polynomials are less reliable for the estimation of RD treatment effects because of overfitting or biases at boundary points. Therefore, my preferred estimates are drawn from the local linear case (Cattaneo et al. 2017; Gelman and Zelizer 2015).

Findings

OLS and RD Specifications

Table 3 reports OLS and RD results of the effect of schooling on Word Recall, Verbal Fluency, Numeric Ability, and Serial Subtraction. The OLS results reveal a positive association between years of schooling and each cognitive outcome. Remaining in school until age 16 is associated with a 0.24-standard-deviation increase in Word Recall, a 0.23-standard-deviation increase in Verbal Fluency, a 0.15-percentage-point increase in Numeric Ability, and a 0.07-percentage-point increase in Serial Subtraction. These results corroborate findings from previous research: the positive correlation between education and cognitive outcomes persists into later life. However, these findings may not necessarily represent causal effects because of omitted variable bias, and the RD specification addresses this issue by exploiting plausibly exogenous variation in schooling.

Table 3 also shows the RD estimates using a linear specification in the running variable (birth cohort month and year). A discontinuity in average years of schooling is present at the reform cutoff, as reflected in the first-stage results: the average difference in years of schooling between those exposed versus unexposed to the reform is 0.35–0.38, depending on the sample window used. The RD estimates exploit this jump in years of schooling. Across all models, the first-stage F statistics are larger than 10, indicating that the estimates are unlikely to suffer from a weak instrument problem. Computed at the optimally selected bandwidth of 38 months, the RD estimates show that an extra year of schooling is associated with a statistically significant increase of 0.42 standard deviations for Word Recall. The effect size is larger for delayed recall than for immediate recall (data not shown). For Verbal Fluency, the magnitude of the effect is 0.05 standard deviations, but this effect is not statistically significant at conventional levels.

Unlike the OLS estimates, the RD specifications do not reveal a positive association between schooling and the two numeracy measures. For Numeric Ability, remaining in school until age 16 is associated with a 0.05-percentage-point reduction in the probability of successfully answering four or five of the numeric ability questions, but this effect is not statistically significant at conventional levels. For Serial Subtraction, the effect size is 0 percentage points; again, though, this effect is not precisely estimated, so I cannot rule out larger effect sizes. The lack of impact on these measures may be due to ceiling effects—that is, clustering toward the top of the performance distribution for these tests reduces the measure's ability to distinguish between moderate and high functioning. Indeed, because many of these skills are used in everyday tasks (e.g., grocery shopping) for individuals of all occupations and education levels, they may be less influenced by education level. Treating these variables as continuous rather than dichotomous variables produces similar results.

I also examined the potential for effect modification by gender. Mazzonna (2014) assessed the effects of an additional year of schooling on cognitive function across six European countries, finding larger effects among men than women. The suggested channel was through increased labor force participation among those exposed to the increased school-leaving age. However, these patterns may not generalize to other countries with different patterns of female labor force participation during the later twentieth century, such as the United Kingdom.

Table B1 (online appendix) reports results from the same sample and specification used in Table 3 and adds birth month as a covariate, with additional gender subsample estimates. At the optimal bandwidth, the effect on Word Recall is statistically significant among women. However, the difference in estimates between men and women was not statistically significant. Figures B2 and B3 (online appendix) show that the gender-specific estimates vary by bandwidth choice. Given the smaller sample sizes in the gender subgroups, I did not examine gender differences further.

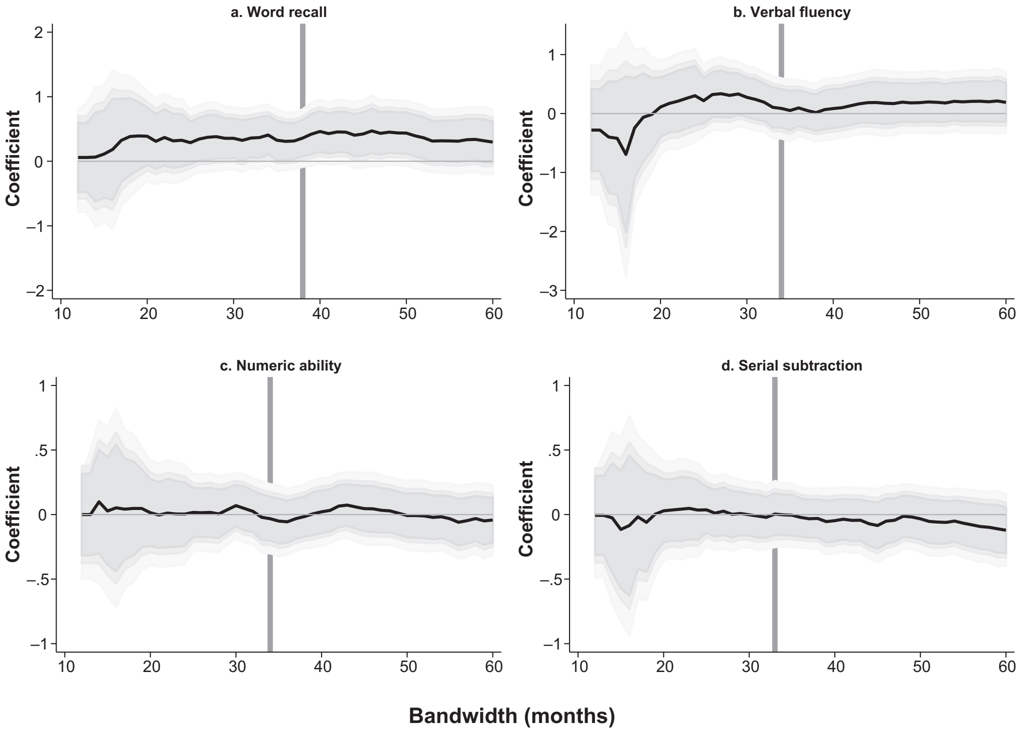

Fig. 3 Sensitivity analyses. The bold black line plots coefficients from a fuzzy regression discontinuity design assessing the effect of years of schooling on each cognitive outcome (word recall, verbal fluency, numeric ability, and serial subtraction) across a range of bandwidth choices (12 months to 60 months). The gray areas depict 90%, 95%, and 99% confidence intervals around the treatment effect. The vertical line indicates the optimal bandwidth. *Source:* Understanding Society.

Sensitivity Analyses

To verify that pretreatment characteristics are smooth across the cutoff, using a falsification test analogous to balance tests of background characteristics in settings with full randomization, I examine the patterning of several relevant background characteristics: whether the respondent's mother has or held any formal educational qualifications, whether the respondent's father has or held any formal educational qualifications, whether the respondent's father was in work when the respondent was age 14, and finally the mother's age at the respondent's birth. Figure B1 (online appendix) shows that these variables do not exhibit discontinuities at the cutoff.

An important choice is the selection of sample window (the bandwidth). To assess the sensitivity of the main findings to a range of bandwidths, I examine point estimates and confidence intervals for different bandwidths. For instance, Figure 3 plots the treatment effect on each cognitive outcome—as well as 99%, 95%, and 90% confidence intervals—for a range of bandwidths. The optimally selected bandwidths are indicated by vertical lines. The effect size varies by bandwidth; the effect size for memory is 0.39 at half the optimal bandwidth (19 months) and 0.33 at 1.5 times

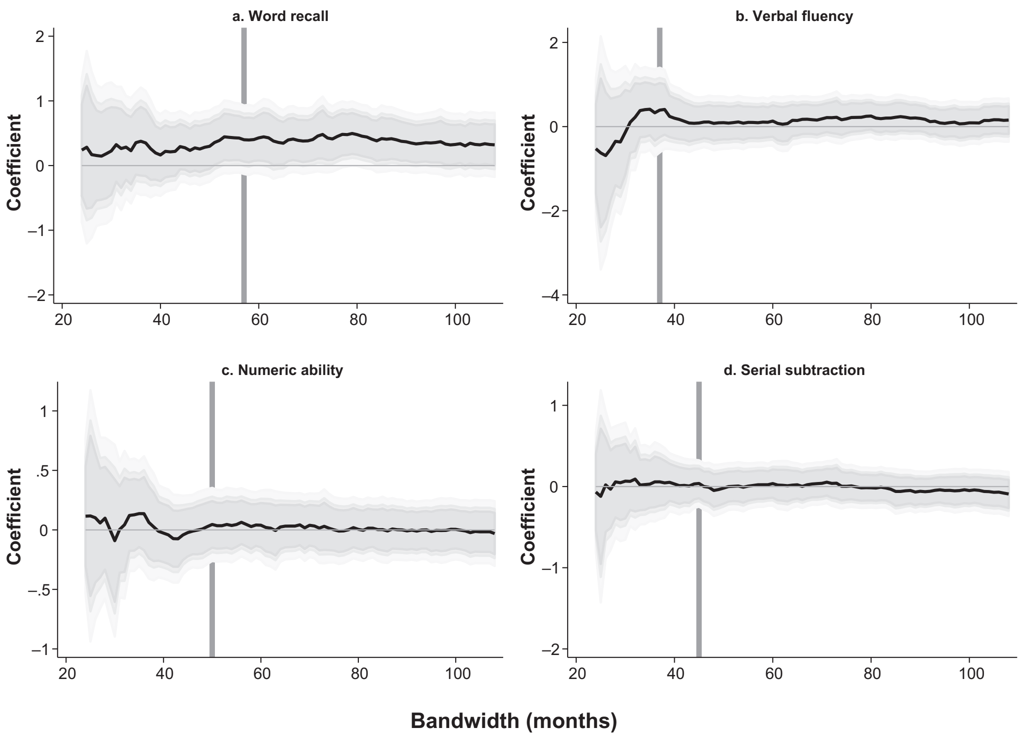

Fig. 4 Sensitivity analyses (quadratic). The bold black line plots coefficients from a fuzzy regression discontinuity design assessing the effect of years of schooling on each cognitive outcome (word recall, verbal fluency, numeric ability, and serial subtraction) across a range of bandwidth choices (12 months to 60 months). The gray areas depict 90%, 95%, and 99% confidence intervals around the treatment effect. The vertical line indicates the optimal bandwidth. *Source:* Understanding Society.

the optimal bandwidth (57 months). Thus, the estimates vary by choice of sample. However, the effects remain positive and significant at the 10% level or better across a reasonable range of bandwidths for Word Recall. In contrast, effect sizes for the other outcomes oscillate around 0, and no statistically significant effects are detected across all values of bandwidth choice.

Figure 4 presents the same plots, this time using a local quadratic rather than local linear RD specification: the local quadratic specification has the running variable included as a quadratic rather than linear term. These results demonstrate that the results are robust to a more flexible specification of the birth cohort trends and corroborate the results from the linear specification. The linear specification is preferred, given recent research suggesting that higher order polynomials are an unreliable specification in RD applications (Cattaneo et al. 2017; Gelman and Zelizer 2015). Figure B3 (online appendix) reports the results of a specification with month-of-birth dummy variables as covariates, demonstrating similar findings, although with a larger magnitude of coefficients. The effect size for memory at the optimal bandwidth with the addition of month-of-birth covariates is slightly more than one half of a standard deviation (0.55).

Table 4 Estimated effects of remaining in school until age 16 on intermediate occupation outcomes

	Marginal Effect	SE
NS-SEC, First Job		
Routine, semi-routine, long-term unemployed	–0.121	0.015
Lower supervisory and technical	–0.011	0.004
Small employers and own account workers	0.009	0.002
Intermediate occupations	0.079	0.010
Managerial and professional	0.044	0.004
NS-SEC, Current Job		
Routine, semi-routine, long-term unemployed	–0.155	0.010
Lower supervisory and technical	0.003	0.001
Small employers and own account workers	0.011	0.002
Intermediate occupations	0.022	0.002
Managerial and professional	0.120	0.006

Note: This table reports results from two IV ordered probit specifications estimated using maximum likelihood via the *cmp* Stata package. SEs are robust standard errors.

Source: Understanding Society.

The size of schooling effects on cognitive outcomes varies with the sample definition, covariate choice, and bandwidth. Further, these estimates are not statistically significant in all specifications examined. Taken as a whole, however, the positive and statistically significant effects on memory across a range of reasonable bandwidths and sample definitions provide a strong case for the veracity of the main results.

Channels

Table 4 displays the results of two specifications examining the effects of remaining in school until age 16 on the occupation type of the first job after leaving school and the current job. Remaining in school until age 16 reduces the probability of taking a routine or semi-routine occupation immediately after leaving school by 12 percentage points—a statistically significant reduction—and increases the probability of taking a managerial or professional job immediately after leaving school by 4 percentage points. The effects on current occupation (measured toward the end of working life) are larger in magnitude. For instance, remaining in school until age 16 reduces the average probability of being in a routine or semi-routine occupation by 16 percentage points.

These findings are consistent with the hypothesis that cognitively intensive occupations enhance cognitive reserve and preserve cognitive function in later life. Quantifying the contribution of occupation type to the link between schooling and cognitive outcomes would ideally require a full mediation analysis. However, conducting such an analysis is challenging because of a second endogeneity problem: occupation choice is endogenous. Despite the quasi-random variation in the treatment, confounding might still occur between the mediator and outcome, which would bias the estimation of the mediator's contribution (Huber 2014). One strategy for addressing this bias is to assume conditional sequential exogeneity—that occupation is ignorable, given the treatment and pretreatment covariates (Huber et al. 2016).

However, this assumption is difficult to defend in this application. A second approach is to find a source of exogenous variation to use as an IV for occupation choice. Finding a credible instrument has not been possible in this context (for a contribution using IVs for mediation analyses, see Frölich and Huber 2017). In the absence of a full mediation model, this analysis should be interpreted as only suggestive of occupation as a mechanism.

Discussion

Continued increases in the minimum school-leaving age aim to improve the educational, economic, and social prospects of individuals who would otherwise choose to drop out early. Successive changes of this kind have increased the average years of education over the last century. Because these policies are not without cost, the size of social and private returns remains an important question. This study used a change in compulsory schooling laws enacted in 1972 in England and Wales to study the effects of schooling on later-life cognitive performance. The findings show that remaining in school until age 16 confers a protective effect on memory, ranging from one third to just over one half of a standard deviation, depending on the sample and model specification. Little evidence was detected for effects on numeric ability or verbal fluency, with effect magnitudes close to 0 and statistically insignificant.

These results are consistent with previous studies, which have generally found a large impact of schooling on working memory among those at the lower end of the schooling distribution across several periods, countries, and estimation strategies (Banks and Mazzonna 2012; Brunello et al. 2011; Glymour et al. 2008; Mazzonna 2014). The study most similar to the current one (Banks and Mazzonna 2012) exploited the 1947 increase in the school-leaving age in England and Wales. The 1947 reform had large effects on remaining in school—at a lower margin of schooling than the 1972 reform—inducing roughly 50% of the affected cohort to remain in school to age 15 rather than 14. On the basis of the argument about diminishing returns to education, the 1972 reform might have been expected to yield lower cognitive returns than the earlier reform. However, the current study found that the effects of schooling on memory are within a similar range of magnitudes as those Banks and Mazzonna (2012) found (e.g., effects of 0.5 and 0.4 standard deviations among men and women, respectively): evidently, the cognitive returns to basic education have not been exhausted for an outcome that is especially relevant for the onset of cognitive impairment.

In contrast to working memory, the present study did not detect a causal effect on verbal fluency or numeric ability. This finding may be due to the lack of sensitivity of the cognitive battery measures employed or the older sample (by approximately a decade) that Banks and Mazzonna (2012) used, with the full effects on cognitive outcomes perhaps not materializing until older ages. On the other hand, this study's younger sample circumvents issues of selective mortality, whereby those who survived to be observed in the sample may have different treatment effects than those who did not. However, the absence of evidence for an effect on measures of numeric ability is consistent with other studies that have failed to detect effects of education on numeric ability measured by simple cognitive battery tests (Schneeweis et al. 2014) and measures of financial decision-making quality (Banks et al. 2019).

Education and labor market trajectories are differentiated by gender, potentially suggesting differential effects of schooling for men and women. Mazzonna (2014) exploited changes in compulsory schooling laws across six European countries and found effects of schooling on cognitive function, self-rated health, and depression only among men, with the proposed mechanism being improved working conditions and labor force participation among men. Studying the 1947 school-leaving age increase in England and Wales, Banks and Mazzonna (2012) found relatively similar effects of schooling on memory among men and women. The extent to which these patterns generalize to more recent cohorts in England and Wales, who face different labor market conditions, is unclear. In this analysis, the effect magnitudes are large and statistically significant only among women. This finding may reflect changing female labor force participation rates, which were already relatively high during this period in the United Kingdom compared with many European countries and with older cohorts within the United Kingdom (Ortiz-Ospina et al. 2018). However, an important caveat is that the subgroup sample sizes are relatively small, and the effect difference between the two groups is not statistically significant. In terms of potential mechanisms, the findings show economically and statistically significant effects of remaining in school until age 16 on the type of occupation entered immediately after leaving school and at older ages. These patterns are consistent with basic education improving cognitive reserve through occupation choice (Livingston et al. 2017).

Although the methods used generate internally valid estimates of the effects of remaining in school until age 16, the estimates may not necessarily generalize to other settings. The 1972 reform differed from the 1947 reform: by inducing students to remain in school until age 16, the 1972 reform required them to sit for age-16 examinations, thereby increasing both years in school and the probability of qualification attainment. In the United Kingdom, age-16 qualifications are an important determinant of the economic returns to schooling. Dickson and Smith (2011) studied the wage returns to qualification attainment among those induced to stay in school until age 16 in the United Kingdom. Their results suggest that the economic returns to remaining in school until age 16 are likely to be driven partly by the qualifications gained. Similarly, Machin et al. (2020) found that pupils who passed the high-stakes examinations at age 16 had better labor market outcomes than observationally equivalent pupils who did not meet this threshold. These results have implications for subsequent policy reforms: an additional year of schooling on its own may yield lower returns than when paired with additional credentials.

A related issue is that of schooling quality versus quantity. Several studies have examined the returns to various dimensions of school quality (Harmon and Walker 2000)—in the United Kingdom, school quality is often studied with reference to academically selective grammar schools, which tend to be more well-resourced than their vocational-focused counterparts. One concern is that changes in school quality might confound the effects of reform-related changes in school quantity. However, during the period studied here, the share of academically selective grammar schools did not change discontinuously relative to the reform date. Similarly, the proportion of private (fee-paying) schools remained constant, at approximately 6% to 7%, during this period (Green et al. 2012). Therefore, my findings are indicative of the role of school quantity on outcomes, abstracting from the effects of quality.

As global populations age, understanding the drivers of older adults' health and functioning is increasingly important. This study found that remaining in school until age 16 exerts a causal effect on memory, an important and policy-relevant component of cognitive functioning. The expansion of public schooling throughout the twentieth century may slow the growth in the burden of adverse cognitive outcomes and support economic adjustment to a changing demographic structure as the cohorts exposed to these reforms age. ■

Acknowledgments I am grateful for many helpful discussions and comments that have improved this article. In particular, I thank Ian Walker, Eugenio Zucchelli, and three anonymous reviewers.

References

Albarrán, P., Hidalgo-Hidalgo, M., & Iturbe-Ormaetxe, I. (2020). Education and adult health: Is there a causal effect? *Social Science & Medicine, 249,* 112830. https://doi.org/10.1016/j.socscimed.2020.112830

Angrist, J. D., & Imbens, G. W. (1995). Two-stage least squares estimation of average causal effects in models with variable treatment intensity. *Journal of the American Statistical Association, 90,* 431–442.

Arden, R., Luciano, M., Deary, I. J., Reynolds, C. A., Pedersen, N. L., Plassman, B. L., . . . Visscher, P. M. (2015). The association between intelligence and lifespan is mostly genetic. *International Journal of Epidemiology, 45,* 178–185.

Banks, J., Carvalho, L. S., & Perez-Arce, F. (2019). Education, decision-making, and economic rationality. *Review of Economics and Statistics, 101,* 428–441.

Banks, J., & Mazzonna, F. (2012). The effect of education on old age cognitive abilities: Evidence from a regression discontinuity design. *Economic Journal, 122,* 418–448.

Boardman, J. D., Domingue, B. W., & Daw, J. (2015). What can genes tell us about the relationship between education and health? *Social Science & Medicine, 127,* 171–180.

Borodankova, O., & Coutinho, A. S. A. (2011). *Grade retention during compulsory education in Europe: Regulations and statistics* (ERIC report). Brussels, Belgium: Education, Audiovisual and Culture Executive Agency, European Commission.

Boyle, P. A., Yu, L., Wilson, R. S., Gamble, K., Buchman, A. S., & Bennett, D. A. (2012). Poor decision making is a consequence of cognitive decline among older persons without Alzheimer's disease or mild cognitive impairment. *PLoS One, 7,* e43647. https://doi.org/10.1371/journal.pone.0043647

Brunello, G., Fort, M., Schneeweis, N., & Winter-Ebmer, R. (2011). *The causal effect of education on health* (Economics Series Working Paper, No. 280). Vienna, Austria: Institute for Advanced Studies. Retrieved from https://www.econstor.eu/bitstream/10419/68499/1/73437948X.pdf

Brunello, G., Fort, M., Schneeweis, N., & Winter-Ebmer, R. (2016). The causal effect of education health: What is the role of health behaviours? *Health Economics, 25,* 314–336.

Buckles, K. S., & Hungerman, D. M. (2013). Season of birth and later outcomes: Old questions, new answers. *Review of Economics and Statistics, 95,* 711–724.

Calonico, S., Cattaneo, M. D., Farrell, M. H., & Titiunik, R. (2016). *Regression discontinuity designs using covariates* (Working paper). Ann Arbor: University of Michigan.

Case, A., & Paxson, C. (2009). Early life health and cognitive function in old age. *American Economic Review: Papers & Proceedings, 99,* 104–109.

Cattaneo, M. D., Titiunik, R., & Vazquez-Bare, G. (2017). Comparing inference approaches in RD designs: A reexamination of the effect of Head Start on child mortality. *Journal of Policy Analysis and Management, 36,* 643–681.

Chevalier, A., Harmon, C., Walker, I., & Zhu, Y. (2004). Does education raise productivity, or just reflect it? *Economic Journal, 114,* F499–F517. https://doi.org/10.1111/j.1468-0297.2004.00256.x

Clark, D., & Royer, H. (2013). The effect of education on adult mortality and health: Evidence from Britain. *American Economic Review, 103,* 2087–2120.

Crespo, L., López-Noval, B., & Mira, P. (2014). Compulsory schooling, education, depression and memory: New evidence from SHARELIFE. *Economics of Education Review, 43,* 36–46.

Crum, R. M., Anthony, J. C., Bassett, S. S., & Folstein, M. F. (1993). Population-based norms for the Mini-Mental State Examination by age and educational level. *JAMA, 269,* 2386–2391.

Davies, N. M., Dickson, M., Smith, G. D., van den Berg, G. J., & Windmeijer, F. (2018). The causal effects of education on health outcomes in the UK Biobank. *Nature Human Behaviour, 2,* 117–125.

Deary, I. J., Corley, J., Gow, A. J., Harris, S. E., Houlihan, L. M., Marioni, R. E., . . . Starr, J. M. (2009). Age-associated cognitive decline. *British Medical Bulletin, 92,* 135–152.

de Walque, D. (2007). Does education affect smoking behaviors? *Journal of Health Economics, 26,* 877–895.

Dickson, M., Gregg, P., & Robinson, H. (2016). Early, late or never? When does parental education impact child outcomes? *Economic Journal, 126,* F184–F231. https://doi.org/10.1111/ecoj.12356

Dickson, M., & Smith, S. (2011). What determines the return to education: An extra year or a hurdle cleared? *Economics of Education Review, 30,* 1167–1176.

Fletcher, J. M. (2014). New evidence of the effects of education on health in the U.S.: Compulsory schooling laws revisited. *Social Science & Medicine, 127,* 101–107.

Flynn, J. R. (1987). Massive IQ gains in 14 nations: What IQ tests really measure. *Psychological Bulletin, 101,* 171–191.

Frölich, M., & Huber, M. (2017). Direct and indirect treatment effects—Causal chains and mediation analysis with instrumental variables. *Journal of the Royal Statistical Society: Series B, 79,* 1645–1666.

Fry, A., Littlejohns, T. J., Sudlow, C., Doherty, N., Adamska, L., Sprosen, T., . . . Allen, N. E. (2017). Comparison of sociodemographic and health-related characteristics of UK Biobank participants with those of the general population. *American Journal of Epidemiology, 186,* 1026–1034.

Gathmann, C., Jürges, H., & Reinhold, S. (2014). Compulsory schooling reforms, education and mortality in twentieth century Europe. *Social Science & Medicine, 127,* 74–82.

Gelman, A., & Zelizer, A. (2015). Evidence on the deleterious impact of sustained use of polynomial regression on causal inference. *Research & Politics*, *2*(1). https://doi.org/10.1177/2053168015569830

Glymour, M. M., Kawachi, I., Jencks, C. S., & Berkman, L. F. (2008). Does childhood schooling affect old age memory or mental status? Using state schooling laws as natural experiments. *Journal of Epidemiology and Community Health, 62,* 532–537.

Green, F., Machin, S., Murphy, R., & Zhu, Y. (2012). The changing economic advantage from private schools. *Economica, 79,* 658–679.

Grimard, F., & Parent, D. (2007). Education and smoking: Were Vietnam War draft avoiders also more likely to avoid smoking? *Journal of Health Economics, 26,* 896–926.

Hahn, J., Todd, P. E., & Van der Klaauw, W. (2001). Identification and estimation of treatment effects with a regression-discontinuity design. *Econometrica, 1,* 201–209.

Harmon, C., & Walker, I. (1995). Estimates of the economic return to schooling for the United Kingdom. *American Economic Review, 85,* 1278–1286.

Harmon, C., & Walker, I. (2000). The returns to the quantity and quality of education: Evidence for men in England and Wales. *Economica, 67,* 19–35.

Heckman, J. J. (2007). The economics, technology, and neuroscience of human capability formation. *Proceedings of the National Academy of Sciences, 104,* 13250–13255.

Heckman, J. J., Stixrud, J., & Urzua, S. (2006). The effects of cognitive and noncognitive abilities on labor market outcomes and social behavior. *Journal of Labor Economics, 24,* 411–482.

Hsu, J. W., & Willis, R. (2013). Dementia risk and financial decision making by older households: The impact of information. *Journal of Human Capital, 7,* 340–377.

Huang, W., Lei, X., Ridder, G., Strauss, J., & Zhao, Y. (2013). Health, height, height shrinkage, and SES at older ages: Evidence from China. *American Economic Journal: Applied Economics, 5*(2), 86–121.

Huber, M. (2014). Identifying causal mechanisms (primarily) based on inverse probability weighting. *Journal of Applied Econometrics, 29,* 920–943.

Huber, M., Lechner, M., & Mellace, G. (2016). The finite sample performance of estimators for mediation analysis under sequential conditional independence. *Journal of Business & Economic Statistics, 31,* 139–160.

Imbens, G. W., & Lemieux, T. (2008). Regression discontinuity designs: A guide to practice. *Journal of Econometrics, 142,* 615–635.

Johnston, D. W., Lordan, G., Shields, M. A., & Suziedelyte, A. (2015). Education and health knowledge: Evidence from UK compulsory schooling reform. *Social Science & Medicine, 127,* 92–100.

Kim, J. W., Lee, D. Y., Seo, E. H., Sohn, B. K., Choe, Y. M., Kim, S. G., . . . Woo, J. I. (2014). Improvement of screening accuracy of Mini-Mental State Examination for mild cognitive impairment and non-Alzheimer's disease dementia by supplementation of verbal fluency performance. *Psychiatry Investigation, 11,* 44–51.

Kolesár, M., & Rothe, C. (2018). Inference in regression discontinuity designs with a discrete running variable. *American Economic Review, 108,* 2277–2304.

Lee, D. S., & Card, D. (2008). Regression discontinuity inference with specification error. *Journal of Econometrics, 142,* 655–674.

Livingston, G., Sommerlad, A., Orgeta, V., Costafreda, S. G., Huntley, J., Ames, D., . . . Mukadam, N. (2017). Dementia prevention, intervention, and care. *Lancet, 390,* 2673–2734.

Lochner, L. (2011). Non-production benefits of education: Crime, health and good citizenship. In E. A. Hanushek, S. Machin, & L. Woessmann (Eds.), *Handbook of the economics of education* (Vol. 4, pp. 183–282). Amsterdam, the Netherlands: Elsevier B. V.

Machin, S., McNally, S., & Ruiz-Valenzuela, J. (2020). Entry through the narrow door: The costs of just failing high stakes exams. *Journal of Public Economics, 190,* 104224. https://doi.org/10.1016/j.jpubeco.2020.104224

Mazumder, B. (2012). The effects of education on health and mortality. *Nordic Economic Policy Review, 2012*(1), 261–302.

Mazzonna, F. (2014). The long lasting effects of education on old age health: Evidence of gender differences. *Social Science & Medicine, 101,* 129–138.

McFall, S. (2013). *Understanding Society: The UK Household Longitudinal Study, Waves 1–3: User manual*. Colchester, UK: University of Essex.

Meghir, C., Palme, M., & Simeonova, E. (2012). *Education and mortality: Evidence from a social experiment* (NBER Working Paper 17932). Cambridge, MA: National Bureau of Economic Research. https://doi.org/10.3386/w17932

OECD. (2006). *Live longer, work longer* (Aging and Employment Policies report). Retrieved from https://doi.org/10.1787/9789264035881-en

Oreopoulos, P. (2008). Estimating average and local average treatment effects of education when compulsory schooling laws really matter. *American Economic Review, 96,* 152–175.

Oreopoulos, P., & Salvanes, K. G. (2009). *How large are returns to schooling? Hint: Money isn't everything* (NBER Working Paper 15339). Cambridge, MA: National Bureau of Economic Research. https://doi.org/10.3386/w15339

Oreopoulos, P., & Salvanes, K. G. (2011). Priceless: The nonpecuniary benefits of schooling. *Journal of Economic Perspectives, 25*(1), 159–184.

Ortiz-Ospina, E., Tzvetkova, S., & Roser, M. (2018, March). *Women's employment*. Our World in Data. Retrieved from https://ourworldindata.org/female-labor-supply

Plassman, B. L., Welsh, K. A., Helms, M., Brandt, J., Page, W. F., & Breitner, J. C. S. (1995). Intelligence and education as predictors of cognitive state in late life: A 50-year follow-up. *Neurology, 45,* 1446–1450.

Rohwedder, S., & Willis, R. J. (2010). Mental retirement. *Journal of Economic Perspectives, 24*(1), 119–138.

Roth, M., Tym, E., Mountjoy, C. Q., Huppert, F. A., Hendrie, H., Verma, S., & Goddard, R. (1986). CAMDEX: A standardised instrument for the diagnosis of mental disorder in the elderly with special reference to the early detection of dementia. *British Journal of Psychiatry, 149,* 698–709.

Schneeweis, N., Skirbekk, V., & Winter-Ebmer, R. (2014). Does education improve cognitive performance four decades after school completion? *Demography, 51,* 619–643.

Skirbekk, V., Stonawski, M., Bonsang, E., & Staudinger, U. M. (2013). The Flynn effect and population aging. *Intelligence, 41,* 169–177.

Stern, Y. (2002). What is cognitive reserve? Theory and research application of the reserve concept. *Journal of the International Neuropsychological Society, 8,* 448–460.

Tucker-Drob, E. M. (2011). Neurocognitive functions and everyday functions change together in old age. *Neuropsychology, 25,* 368–377.

UK Department for Work and Pensions. (2013). *2010 to 2015 government policy: Older people* (UK policy paper). Retrieved from https://www.gov.uk/government/publications/2010-to-2015-government-policy-older-people

University of Essex, Institute for Social and Economic Research, & NatCen Social Research. (2015). *Understanding Society: Waves 1–5, 2009–2014: Special license access, census area statistics wards* (6th release) [Data set]. Essex: UK Data Service, SN: 6669. http://doi.org/10.5255/UKDA-SN-6669-6

Woodin, T., McCulloch, G., & Cowan, S. (2012). Raising the participation age in historical perspective: Policy learning from the past? *British Educational Research Journal, 39,* 635–653.

World Health Organization. (2015). *World report on ageing and health*. Geneva, Switzerland: World Health Organization. Retrieved from https://apps.who.int/iris/handle/10665/186463

Wraw, C., Deary, I. J., Gale, C. R., & Der, G. (2015). Intelligence in youth and health at age 50. *Intelligence, 53,* 23–32.

Wu, Q., Tchetgen, E. J. T., Osypuk, T. L., White, K., Mujahid, M., & Glymour, M. M. (2013). Combining direct and proxy assessments to reduce attrition bias in a longitudinal study. *Alzheimer Disease and Associated Disorders, 27,* 207–212.

Emma Gorman
E.Gorman@westminster.ac.uk

School of Organisations, Economy and Society, University of Westminster, London, UK; IZA, Bonn, Germany; https://orcid.org/0000-0002-3325-4299