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Education and Health Knowledge: Evidence from UK Compulsory Schooling Reform

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Abstract

We investigate if there is a causal link between education and health knowledge using data from the 1984/85 and 1991/92 waves of the UK Health and Lifestyle Survey (HALS). Uniquely, the survey asks respondents what they think are the main causes of ten common health conditions, and we compare these answers to those given by medical professionals to form an index of health knowledge. For causal identification we use increases in the UK minimum school leaving age in 1947 (from 14 to 15) and 1972 (from 15 to 16) to provide exogenous variation in education. These reforms predominantly induced adolescents who would have left school to stay for one additionally mandated year. OLS estimates suggest that education significantly increases health knowledge, with a one-year increase in schooling increasing the health knowledge index by 15% of a standard deviation. In contrast, estimates from instrumental-variable models show that increased schooling due to the education reforms did not significantly affect health knowledge. This main result is robust to numerous specification tests and alternative formulations of the health knowledge index. Further research is required to determine whether there is also no causal link between higher levels of education – such as post-school qualifications – and health knowledge.

Key words: Education, Health, Knowledge, Compulsory Schooling, Causality
JEL: I20; I10; I12

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1. Introduction

There is an extensive interdisciplinary literature that has shown that education is significantly correlated with better health-related behaviours and outcomes (often called the ‘education-health gradient’). This arises as more educated people tend to have better diets, are less likely to smoke, are more likely to undertake regular exercise, are less likely to be obese, report fewer chronic health conditions, and live longer (Ross and Wu, 1995; Lleras-Muney, 2005; Cutler and Lleras-Muney, 2010; Brown et al., 2012; Clark and Royer, 2013). Higher parental education is also strongly associated with better child health (Case et al., 2002). However, in this literature a causal relationship remains difficult to establish.

There are many proposed pathways linking education to health. The most direct pathway is the effect of education on health-related knowledge accumulation (Grossman, 1972, 2000, 2005; Kenkel, 1991). Health knowledge makes individuals more efficient in health production, that is, they can obtain better health with the same amount of inputs (Grossman, 2005). For example, more knowledgeable individuals may accrue greater benefits from a doctor visit, because they are better able to understand doctor’s instructions. Individuals with better health knowledge can also make better decisions about the choice of inputs in health production (Grossman, 2005). For example, people who know more about health are less likely to smoke than people who have less knowledge about it, as we show in this paper.

The central aim of this paper is to establish whether additional schooling has a causal impact on health related knowledge. There are two main pathways through which this relationship could operate. First, it may increase health knowledge directly if the school curriculum includes material on how the human body functions and the health effects of lifestyle choices. Second, additional schooling may give an individual the skills they require to access required health information. Education improves an individual’s cognitive skills, including reading and information processing (Cascio and Lewis, 2006). Consequently, the higher educated are more likely to use health information resources, including books, television, radio programs, and

Internet websites (Wagner et al., 2001; and Bundorf et al., 2006). In addition, the more educated react more quickly and make better choices when new knowledge becomes available, for example, in response to new medical treatments or a public health campaign (Kenkel, 1991; Glied and Lleras-Muney, 2008; and Vikram et al., 2012).

Despite the importance of health knowledge for the education-health gradient, there has been no attempt to establish a causal link between schooling and health-related knowledge. In contrast, a large number of papers have sought to causally estimate the education-health gradient. This is a difficult task given it is not possible to directly assess the counter-factual; that is, what would have been the health outcomes of individuals who selected into further education *if* they had not undertaken that education. In particular, estimated effects are likely to be too large if the same individual characteristics that determine educational attainment (e.g. parental background, cognitive ability, risk preferences), also directly affect health outcomes. A second concern arises if education is measured with error. Survey measures of educational attainment are often afflicted by high misclassification rates (Black et al., 2003), and in addition we do not often observe the quality of education or the topic-matter covered in the curriculum. If these issues induce classical measurement error, then estimated effects are likely to be too small.

These empirical difficulties have led to some recent papers using policy-changes that have exogenously affected some individuals' years of schooling but not others, to better identify the causal effect of additional schooling on health. A valid source of exogenous variation (an instrument) allows researchers to overcome both the endogeneity and measurement error issues. Examples in the international literature are Adams (2002), Lleras-Muney (2005), Mazumder (2008), Arendt (2005, 2008), Albouy and Lequien (2009), Brunello et al. (2011), Kempter et al. (2011), and Mazzonna (2013). The results from these studies are mixed, with some finding a significant causal effect of education on health outcomes, while others finding no significant effect (Braakmann, 2011).

Mixed results are also found in studies that have utilised the exogenous educational reforms in the UK that incrementally increased the minimum school leaving age in 1947 (from 14 to 15) and 1972 (from 15 to 16). For example, Clark and Royer (2013) use these reforms as an instrument to establish a causal relationship between schooling and a variety of health outcomes. Overall, while they find strong education-health correlations for all of their considered outcomes (mortality, having a long term illness, health behaviours and self reported physical health) they find that the reforms causal effect on health is either not significant or very small. In contrast, using the same reforms and pooled cross sections of the General Household survey from 1980-2004, Silles (2009) documents that an additional year of education increases the probability of self-reporting being in good health by about 5 percentage points. A conclusion that schooling augments self-reported physical health is also drawn by Oreopoulos (2006). In addition, Powdthavee (2010) draws the conclusion that schooling lowers the risk of hypertension using the 1991 to 2007 Health Survey for England (HSE). In contrast, Jürges et al. (2013), also using the HSE (from 1993 to 2006), consider the risk of cardiovascular ill health alongside self-reported health. The only significant effect they find pertains to females and self-reported health. In a related paper using the 1947 reform to test for the effect of parental education on child health outcomes, Lindeboom et al. (2009) found that the 1-year increase in parental education had little effect on the health of their children, although increased schooling did improve the household financial situation[1].

Interestingly, while these previous studies of the education-health gradient emphasise that a likely pathway between additional schooling and their respective health outcomes is an increased level of health knowledge, few of these studies cite evidence for or discuss the importance of this pathway. To the best of our knowledge, there was no statutory health education in British school's curriculum during the period of the UK education reforms. Additionally, some evidence suggests

[1] It is noteworthy that there is also some dispute as to whether the same laws influence adult income. For example, Oreopoulos (2006) find that the returns to schooling are about 15%, however Devereux and Hart (2010) find a much smaller return (3% on average).

that health education is ineffective with respect to altering health and behavioural outcomes in the short term (Coleman et al, 2011). Therefore, the pathway through which these reforms may impact on health outcomes is less clear, and a rigorous testing of the potential health knowledge route is a valuable contribution to this literature.

To identify the causal effect of education on health-related knowledge we follow the aforementioned education-health gradient literature, and utilise the exogenous variation in minimum compulsory years of schooling generated by the 1947 and 1972 UK education reforms, together with data from the UK Health and Lifestyle Survey (HALS). This data uniquely captures a respondents' level of health-related knowledge at two points in time. In particular, respondents are asked what they think are the main causes of ten common health conditions, and we compare these answers to those given by medical professionals to form an index of health knowledge.

2. Data and Descriptive Analysis

2.1. Health and Lifestyle Survey

We use the UK Health and Lifestyle Survey (HALS) whose purpose was to collect data on health behaviours of the British population (England, Wales and Scotland), including smoking, alcohol consumption, diet, and exercise as well as factors that may affect these behaviours. The HALS sample is broadly representative of the British population (Cambridge University School of Clinical Medicine, 1985). The first wave of HALS was conducted in 1984/85, with a response rate of 73 percent. In total, 9,003 individuals (18-99 years old) were interviewed. Seven years later (in 1991/92), a follow up survey was carried out resulting in 5,352 completed interviews. To increase the sample size, we use observations from both waves. Individuals born before 1923 were excluded from the sample for identification purposes. After also excluding observations with missing values, the analysis sample size is 10,085 observations.

As a measure of schooling, we use the age at which a respondent left high school, which ranges from 14 to 19 years old. Since some respondents continued their education after completing high school, this variable does not measure total years of education. However, school-leaving age is an appropriate measure of education in our analysis, because our identification strategy involves utilising education reforms that increased the legal school leaving age in Britain from 14 to 15, and then later from 15 to 16. It has been shown that these reforms had little effect on post-school qualifications (Clark and Royer, 2013). The average school leaving age in the sample is 15.5 years (with a standard deviation of 1.3) and the median is equal to 15 years.

2.2. Measuring health knowledge

A unique feature of HALS is that it includes questions that directly measure a respondent's health knowledge. Specifically, respondents were asked what causes ten common health conditions which have very different pathologies: stomach ulcers, chronic bronchitis, high blood pressure, migraine, liver trouble, stroke, lung cancer, heart trouble, severe depression, and piles and haemorrhoids. The respondents were not prompted with possible answers and all mentioned answers were recorded. To determine whether a respondent's answers signified a high level of health knowledge, we compared them to the answers supplied by respondents who reported they were medical doctors. Therefore, a 'correct' answer is defined by the knowledge held by medical doctors at the time of the survey. There were 21 and 10 doctors in waves 1 and 2 respectively. Table 1 lists causes mentioned by at least 25 percent of doctors for each of the 10 health conditions, separately for each wave. The causes are ordered by the frequency of doctor's answers (more frequently mentioned causes are listed first). Note that doctors' answers changed somewhat over time; presumably reflecting advances in the medical literature. For example, stress was mentioned as an important cause of high blood pressure and stroke in 1984/85, but it is not listed among the most common causes of these diseases in 1991/92.

A respondent is considered to have answered the question about a particular health condition correctly if he/she mentioned at least one of the causes of this disease listed in Table 1. Wave 1 (2) answers are compared to doctors answers in wave 1 (2). We then sum the correct answers for each respondent to construct a health index. This index varies from 0 to 10. An average respondent answered 7 out of 10 questions correctly (standard deviation is equal to 2). In the sensitivity analysis we use a stricter definition of correct answers, requiring at least 50 percent of doctors to mention a given cause in order for it to be a 'correct' cause. Using this definition, the mean health index decreases to approximately 4. We have also checked sensitivity of the results to using nurses' answers (215 in wave 1 and 154 in wave 2) and to summing the number of correctly mentioned causes for each condition. The correlations between these three additional indices and our main health knowledge index equal 0.81, 0.89 and 0.92, respectively.

As a final sensitivity check we have used a set of questions on nutrition to define health knowledge. As part of the survey, the respondents were given a list of foods and asked whether or not each of them contains fibre. These were: roast meat, digestive biscuits, potatoes, eggs, orange juice, grilled fish, Weetabix, white bread, cheese and apples. The advantage of these questions is that the correct answers were specified in the survey documentation. Therefore, we use this measure of health knowledge to test sensitivity of the results to the assumptions we made in order to define correct answers to the questions on the ten health conditions. The knowledge index based on the nutrition questions is constructed by summing the correct answers. It has a mean of 6.8 and standard deviation of 1.9.

To facilitate the interpretation of the regression coefficients and comparison of the results across the different measures of health knowledge, we have standardised all health knowledge variables to have a mean of 0 and a standard deviation of 1. Doctors are excluded from the main analysis sample, and nurses are excluded from the sample when their answers are used to construct the health knowledge measure.

2.3. Descriptive regression analysis of health knowledge

To highlight that our measure of health knowledge is correlated with other salient variables we run regressions of a list of health behaviours on health knowledge. If our measure captures health knowledge, we expect it to have positive effects on health behaviours. In the literature, health behaviours are often considered to be proxies for knowledge about health (Clark and Royer, 2013). Specifically, we regress wave 2 health behaviours on wave 1 health knowledge and wave 1 health behaviours (as well as main demographic characteristics). The ordinary least squares (OLS) estimates are presented in Table 2. Most of the health behaviours are correlated with health knowledge in the expected direction. For example, an increase in the standardized health knowledge index is associated with a reduction in the probability of smoking and has positive effects on exercise. Better health knowledge is also associated with a healthier diet, as measured by higher fruit, vegetable, fish and white meat consumption, lower red meat consumption, and choice of healthier bread and milk options. Note that the estimated effects of health knowledge on health behaviours are conservative, because they do not account for the effects of health knowledge on current health behaviours, which are held fixed.

We also investigate how health knowledge correlates with some key demographic and socioeconomic characteristics, including education. It is expected that higher socioeconomic status is positively correlated with exposure to information and information processing skills, and in turn, to knowledge about health. These OLS estimates are presented in Table 3. Overall health knowledge is found to increase with age but at a decreasing rate and be higher among females. No variation in knowledge by marital status or region is found. Controlling for all other individual characteristics, we find that a one-year increase in school leaving age raises the health knowledge index by 9.9 percent of a standard deviation. Intuitively, the other socio-economic variables (socio-economic group of the head of the household, employment status, household income and home ownership) also have positive effects on knowledge about health.

3. Methodological Approach

3.1. British compulsory schooling changes

In the UK today we differentiate between early years, primary, secondary, further education and higher education. In particular students enter secondary school at around age 11, and those who receive ‘O ‘and ‘A’ levels stay in secondary education and complete examinations at ages 16 and 18, respectively. An individual who succeeds in completing adequate ‘A’ levels can pursue further education. Changes to the UK education system, beginning in 1944, saw the evolution of this system from one that did not emphasise State responsibility, to a system which necessitates that all children stay in school until age 16 – thus usually securing some secondary level qualification – and guarantees an opportunity for education at a primary and secondary level to all children regardless of background.

Our methodological approach involves exploiting changes in the British compulsory school laws over this period. These laws govern the age at which children can start and leave school. The first change was set out in a series of Education Acts – in 1944 in England and Wales and in 1945 in Scotland (Jones, 2003). During this time provision for secondary school came under scrutiny and there was movement towards increased State provision. These Acts had an emphasis on an integrated education service, and it was also proposed that the then system of distinguishing elementary and secondary school would be replaced with three tiers of education: primary, secondary and further education. Therefore, for the first time these Acts called for an emphasis on promoting tertiary education among citizens who previously never had such opportunities. Under these Acts the government also recognised a formal responsibility in their duty to provide a certain standard of secondary education that was free and open to all. These Acts moved the UK closer to an education system that was more equal. On 1 April 1947, the legal school leaving age was raised to age 15 (Jones, 2003), while until 31 March 1947, children in Britain could leave school when they reached 14 years of age. This reform affected children who turned 14 after 31 March 1947 as they now had to stay at school at least partway through year ten (freshman year in the United States).

Between 1947 and 1963, provisions for secondary education continued to expand in the UK, with The Crowther Report in 1959, (Ministry of Education, 1959) recommending raising the school leaving age from 15 to 16. This was again examined and recommended in the Newsom Report in 1963 (Ministry of Education, 1963), which argued for priority to be given to secondary school facilities in deprived urban areas. This change occurred on 1 September 1972 when the legal school leaving age was raised to 16. This change affected children turning 15 after 31 August 1972. After the 1972 reform was implemented, children were required to stay in school at least partway through year 11 (sophomore year in the United States). In addition, the reform gave students incentives to stay in school until the end of year 11 in order to obtain a formal qualification ('O' levels), as the time cost of investment was low - school pupils in secondary schools were about 3.5 million in 1970/1 versus about 4.5 million in 1980/1 (Social Trends, 2002). In this way, the 1972 reform was different from the 1947 reform, suggesting that the two reforms may have affected educational attainment differently. Our sample is not large enough to look at the two reforms separately, but other studies that performed such analyses found consistent results based on both the 1947 and 1972 reforms; for example, Clark and Royer (2013), who look at the effects of education on mortality and several measures of health and health behaviour. Both reforms were accompanied by increases in the number of schools, teachers and other resources to accommodate higher numbers of students. Overall, just prior to the reforms in 1947, a total of 133,678 candidates entered examinations for secondary school qualifications versus over 1.5 million candidates in 1977 (Gosden, 1983). Overall, the period spanning the two reforms saw a significant increase in human capital accumulation.

3.2. Models

The identification of the causal effect of education on health knowledge is complicated by the endogeneity of education. It is likely that individuals who have a higher level of education also have other unobserved characteristics, such as ability and personality traits that may positively affect their health knowledge. For this reason, the OLS estimate of the coefficient on education, presented in Table 3, is likely to be upward biased. To address the endogeneity of education, we use a fuzzy regression discontinuity design (RDD) framework. More specifically, we use the British compulsory schooling reforms, discussed in the previous subsection, as instrumental variables. These reforms exogenously increased the amount of schooling; but did not significantly affect post-school educational attainment.

The first-stage regression is specified as follows:

$$E = \delta_0 + \delta_1 D_1 + \delta_2 D_2 + f(r) + \boldsymbol{X}'\boldsymbol{\delta_3} + v, \tag{1}$$

$$D_j = 1, \text{if } r \geq \bar{r}_j,$$

$$D_j = 0, \text{if } r < \bar{r}_j, j = 1,2,$$

where E denotes education, measured by school leaving age, and v is the usual stochastic error term. Variables D_1 and D_2 indicate whether or not an individual was affected by the compulsory schooling reform in 1947 and 1972, respectively. The values of D_1 and D_2 are determined by an individual's birth cohort r. Individuals who were born after the cut-off date $\bar{r}_j$ were subject to the reform, whereas individuals born before this date were not affected by the reform. For the 1947 reform, the cut-off date is 1 April 1933 and for the 1972 reform, the cut-off date is 1 September 1957. Note that the exact date of birth is necessary, as it is crucial to define D_1 and D_2 correctly (Clark and Royer, 2013). Fortunately, HALS includes date of birth information, which is another important advantage of the data. In the empirical analysis, $f(r)$ is a polynomial function of an individual's quarter-year of birth (normalized so that r is equal to zero at the first quarter of 1933). Given we have exact date of birth information, we could have instead included polynomial functions of an individual's day-year or month-year of birth. Each approach gives identical first-stage, reduced-form and 2SLS estimates.

Individuals born just before and just after the cut-off $\bar{r}_j$ are expected to be very similar in all of their individual characteristics besides education. Therefore, by comparing the average levels of education of individuals born just before and just after the cut-off dates, we can estimate a change in education due to the reform. This approach is valid provided that there were no other changes that would have differently affected educational attainment of individuals born just before and just after the cut-off dates. According to our literature search, no other changes related to education were implemented around the reform dates (Jones, 2003). Clark and Royer (2013) provide additional tests of this assumption and also conclude that the instruments are valid. The identification strategy additionally relies on the assumption that birthdate cannot be precisely manipulated. As the dates of the reforms where not known to parents at the time of conception, there are no reasons to expect that this assumption is invalid.

In practice, a wider window around the cut-off is often used to improve precision of the estimates. We also follow this approach. The analysis sample includes all individuals born between 1923 (10 years before the first cut-off) and 1967 (ten years after the second cut-off). We have also experimented with a narrower bandwidth (6 years before and after each cut-off, as in Powdthavee (2010)), and found consistent results. In this case, it is important to include a flexible functional form of birth cohort $f(r)$ in equation (1), because school leaving age may vary with quarter-year of birth. Our models include a fourth order polynomial of the quarter-year of birth (higher order polynomials were not justified by the data). Coefficients δ_1 and δ_2 then measure the discontinuities in education due to the 1947 and 1972 reforms, respectively. Equation (1) also controls for exogenous (determined before the reforms) control variables (sex and age in years) to reduce error variance. Month of birth is included following Clark and Royer (2013) to account for seasonality.

In the second stage, the health knowledge index K is regressed on the predicted value of education $\hat{E}$ from equation (1), polynomial of quarter-year of birth and the same exogenous control variables as in equation (1):

$$K = \beta_0 + \beta_1 \hat{E} + g(r) + \boldsymbol{X}'\boldsymbol{\beta_2} + u, \tag{2}$$

where u denotes a random error term. Coefficient β_1 is interpreted as a causal effect of education on health knowledge. It is important to note that our estimates capture local average treatment effects (LATE), that is, the effects of education on health knowledge for individuals who spent an extra year at school because of the change in the school leaving age laws. As mentioned above, the British compulsory schooling reforms did not significantly affect education beyond high school. Therefore, our findings may not be generalised to the effects of education at other points of the education distribution, for example degree–level attainment.

4. Results

4.1. Effects of compulsory schooling reforms on schooling attainment

Before presenting first-stage regression estimates (equation 1), we graphically examine the effects of the 1947 and 1972 compulsory schooling reforms on age left school, probability left school before age 15 (1947 reform) and probability left school before age 16 (1972 reform). The graphs in Figure 1 include dots representing average schooling by quarter-year of birth, and a line representing the fitted values from an OLS regression that includes reform dummies (separately for each reform) and a fourth-order polynomial in quarter-year of birth. Graphs A and D show that the reforms had large quantitative effects on the probability of leaving school at ages less than the new mandated cut-offs. The 1947 reform reduced the probability of leaving school at age 14 or younger from around 50% to 10%. The 1972 reform reduced the probability of leaving school at age 15 or younger from around 40% to 10%. Interestingly, there exists an unusual pattern of high non-compliance after the 1972 reform. As Clark and Royer (2013) explain, the non-compliance is due to the structure of the laws, which allowed students born in late June, July and August to leave school at age 15, after the completion of grade ten. This non-compliance pattern is controlled for by including month-of-birth dummies in the regression models. Graphs B and E show that both reforms induced a discontinuity in age left school of almost one-half of a year. These jumps are driven by those adolescents who spent extra time in school solely because of the reforms (i.e. compliers).

More precise reform estimates are shown in Column (1) of Table 4, which reports results from OLS regression models that control for a quartic function in quarter-year-of-birth, a quartic function in age, month-of-birth dummies, a survey-year dummy and gender. The estimated coefficients (δ_1 and δ_2) indicate that the 1947 and 1972 reforms increased age left school by 0.567 and 0.306 years, respectively. The reform effects are independently significant (*t*-statistics equal 5.8 and 4.0) and jointly significant (*F*-statistic equals 32.0). They are also robust to alternative quarter-year of birth control functions. Using second-order, third-order or fifth-order polynomials (rather than fourth-order) only slightly alters the estimated reform effects: the 1947 and 1972 reforms are estimated to have increased school leaving age by 0.50 and 0.29 using a quadratic, 0.53 and 0.34 using a cubic, and 0.58 and 0.32 using a quintic. The quartic specification was chosen because it minimises the Akaike Information Criterion (AIC).

Table 4 presents the estimated effects on school leaving age, given this schooling variable is our main focus in the health knowledge (second stage) equations. We have also estimated reform effects for other schooling outcomes. For our data, the 1947 reform is estimated to have decreased the probability of leaving school at age 14 by 39.8 percentage points, and the 1972 reform is estimated to have decreased the probability of leaving school at age 15 by 37.9 percentage points. Neither reform had a statistically significant effect on receiving any post-school qualification (*t*-statistics equal 1.48 and -1.31) or on receiving a degree or professional qualification (*t*-statistics equal 0.50 and 0.23). This result indicates that the reforms predominantly induced adolescents who would have already left school to stay in school for the one additionally mandated year.

It is not possible with the HALS data to investigate the backgrounds of those individuals who were affected by the educational reforms (i.e. the compliers). However, the National Child Development Study (NCDS) – a longitudinal cohort survey of British children born in 1958 – included questions that allow us to imperfectly identify compliers to the second reform. In the 1974 NCDS survey, interviewers reminded parents that their “child's year group is the first in which all children have had to stay at school until the age of sixteen” and asked them if they wished their child “had been able to leave school at fifteen”. The children (aged 16) are also asked if they wished they could have left school at fifteen. Approximately one-third of children or parents answer yes. By examining the backgrounds and characteristics of these children, we can investigate how compliers may differ from the general population. The results, which are available upon request, show that the ‘compliers’ come from significantly lower socioeconomic status families (based on household income, father’s occupation and parental schooling), and have significantly lower cognitive achievement (based on age 11 and age 16 test scores).

4.2. Effects of schooling on health knowledge

Estimated effects of schooling on health knowledge from naïve, reduced form and instrumental-variable models are presented in Columns (2) to (4) of Table 4. The naïve OLS estimate, which is consistent only if schooling is exogenously determined, equals 0.151 and is statistically significant at the 0.1% level. This estimate indicates that an additional year of schooling increases the health knowledge index by 15% of a standard deviation. Given the likelihood that unobserved characteristics, such as cognitive ability, are positively associated with both years of schooling and health knowledge, this OLS estimate is likely to be positively biased.

Graphs C and F in Figure 1 graphically present the reduced form estimates. These graphs show how each reform directly impacts upon health knowledge, without specifying a pathway for the impact. If schooling improves knowledge, we would expect to see jumps in the health knowledge index at the reform-specific cut-offs; however, neither graph shows any discontinuities.

Column (3) presents the regression estimates of the reduced form model: an OLS regression of health knowledge on the reform dummies (instrumental variables) and other control variables. Consistent with the graphs, these estimates indicate that both reforms have a quantitatively small and statistically insignificant effect (*t*-statistics equal -0.45 and 0.63). This suggests that additional schooling does not increase health knowledge. This result is reflected in the 2SLS estimated effect in Column (4), which equals -0.001 and has a 95% confidence interval equal to -0.18 to 0.17. Importantly, this estimate is robust to alternative quarter-year of birth control functions. The 2SLS estimated effects using a quadratic, cubic and quintic equal -0.055 (s.e. = 0.091), 0.022 (s.e. = 0.090) and 0.002 (s.e. = 0.121), respectively. A Hansen over-identification test has p-value equal to 0.467, implying that both instruments are valid.

According to a Hausman one-parameter *t*-test, the 2SLS estimate is statistically different from the OLS estimate (p-value equals 0.044). However, we are unable to reject at conventional levels the null hypothesis that the 2SLS estimate is equal to the re-weighted (adjusted for non-linearities) OLS estimate using a Wald test proposed by Lochner and Moretti (2011). The re-weighted OLS estimate equals 0.115 (*t*-statistic equals 5.315), and the Lochner-Moretti Wald test statistic has a *p*-value equal to 0.125. Nonetheless, the re-weighted OLS estimate is still substantially larger in magnitude than the 2SLS estimate (-0.001).

The general findings in Table 4 are replicated if we use disaggregated health knowledge measures. In Table 5 we present OLS and 2SLS estimated effects of schooling on each of the 10 health conditions that constitute our overall health knowledge index (see Section 2.1). For each condition the OLS estimate is significantly positive, most so for stomach ulcers and high blood pressure, and least so for lung cancer and severe depression. The stomach ulcer estimate of 0.058 implies that an additional year of schooling increases the probability of correctly reporting a cause of stomach ulcers by 5.8 percentage points. In contrast, the 2SLS estimate is statistically insignificant for each condition. Moreover, for half the conditions the estimated schooling effect is negative. The 2SLS coefficients are, however, generally larger than the overall estimate presented in Table 4, and in most instances (expect for lung cancer) the OLS and 2SLS estimates are not statistically different from one another. Therefore, we cannot definitively rule out small positive schooling effects for certain knowledge areas, such as stomach ulcers, high blood pressure, liver trouble, heart disease, and depression.

4.3. Schooling and the effects of health shocks

The above results indicate that additional schooling does not significantly and directly improve health knowledge. However, it is possible that additional schooling provides skills that allow individuals to better access health information when required and to better process conveyed health information. To examine this pathway, we consider whether additional schooling mediates the levels of retained health knowledge from a probable health knowledge shock – namely, the death of a parent. In the initial 1984/85 wave of HALS, information is available on the cause of parents' death, allowing us to model the effects of any cause of death and death from cardiovascular disease (the most common cause of death of a parent) on overall health knowledge and health knowledge related to the cardiovascular system. In our sample, around half of respondents (52.5%) report a parental death, and around one-quarter (27.5%) report a parental death from cardiovascular disease.

Estimated effects of age left school, death of a parent and their interaction from 2SLS models of health knowledge are presented in Table 6. Column (1) presents results for the combination of any cause death and overall knowledge (i.e. knowledge across 10 health conditions), and shows that all three estimated effects are statistically insignificant. In Columns (2) to (5), we focus on deaths from cardiovascular disease on overall knowledge (standardised index), and knowledge regarding causes of high blood pressure, stroke and heart trouble (binary variables). These columns suggest that the death of a parent does increase knowledge, particularly knowledge related to "heart trouble". However, the interaction effects between death and schooling, which provide the differential knowledge effects for individuals with different schooling levels, are all statistically insignificant. Thus, we do not find any strong evidence to support the hypothesis that additional schooling enables individuals to better process or retain communicated health information. A final interesting result is that the main effect of schooling on “heart trouble” knowledge is positive and statistically significant at the 10% level. This provides evidence that schooling may improve this knowledge type for those individuals who have not had a parent die of heart disease.

5. Conclusions

While there is an extensive literature showing that education is positively associated with better health behaviours and outcomes, there are fewer studies that have demonstrated a clear causal effect. A recent literature has sought to identify the effect of additional schooling on a range of health outcomes by using exogenous variation resulting from changes in compulsory schooling laws in a variety of countries. Overall the results from these studies are mixed, with some papers finding evidence of a significant positive effect of education on health and health-related behaviours, such as smoking and diet, while other studies find no evidence.

One of the key pathways by which education is thought to causally impact on health is through an improved ability of individuals to acquire and process health-related knowledge, and then to act on new knowledge (Cutler and Lleras-Muney, 2010). New knowledge could be in the form of information about their own health, or the health of their parents, or could be in the form of new medical technology or public health campaigns. However, this knowledge pathway, which is a central precursor to education having an effect of health outcomes, has not been studied in this new quasi-experimental literature. This is where the contribution of this paper lies. We have used the 1947 and 1972 UK compulsory schooling reforms to estimate the difference in health knowledge between individuals who spent additional time in school because of the reforms and individuals who just missed the reform. Health knowledge is measured using data from the UK

Health and Lifestyle Surveys (HALS), which asked respondents about the main causes of 10 common health conditions.

While we find a clear education gradient in health knowledge from OLS models, we find no evidence that the increases in minimum schooling induced by the two reforms increased future health knowledge. Moreover, we also have found no evidence that increased education enables individuals to better respond to health shocks in the form of parental death. Overall, the results suggest that an additional year of schooling did not generate a significant increase in future health-related knowledge, which is consistent with studies that have shown no effect of these reforms on health behaviours or outcomes. We note that there was no statutory health education on the British school's curriculum at a time period that is anywhere near the reforms used in this work. Therefore it is plausible that our results could change if additional years of schooling meant more exposure to health education specifically.

A further limitation of our identification strategy is that our estimates only capture local average treatment effects, that is, the effects of education on health knowledge for individuals who spent an extra year at school because of the change in the compulsory schooling law. Our analysis suggests that these individuals are likely to come from lower socioeconomic status (SES) families and have lower scholastic achievement. Therefore, our findings are unable to shed light on the effect of higher levels of education – for example, degree level attainment – on health-related knowledge, and furthermore, are unable to inform on the effects of education for high-achieving individuals from high SES families. Studies using alternative identification strategies are rare, and so further research is required to better understand the effects of education on health and health knowledge at other points of the education distribution and for other subsets of the population.

While we have focused on the link between education and health-related knowledge, there are a number of other key pathways that have been proposed for explaining the fact that better educated individuals have better health-related behaviours, are healthier and live longer (see Cutler and Lleras-Muney). These include having more economic resources including higher incomes, safer and more secure occupations, and the means to reside in more affluent health-inducing neighbourhoods. From reading the literature, and from the results of this paper, it is clear that additional analysis is required to identify which of the proposed pathways are most important.

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Appendix. Sensitivity of results to alternative definitions of health knowledge index.

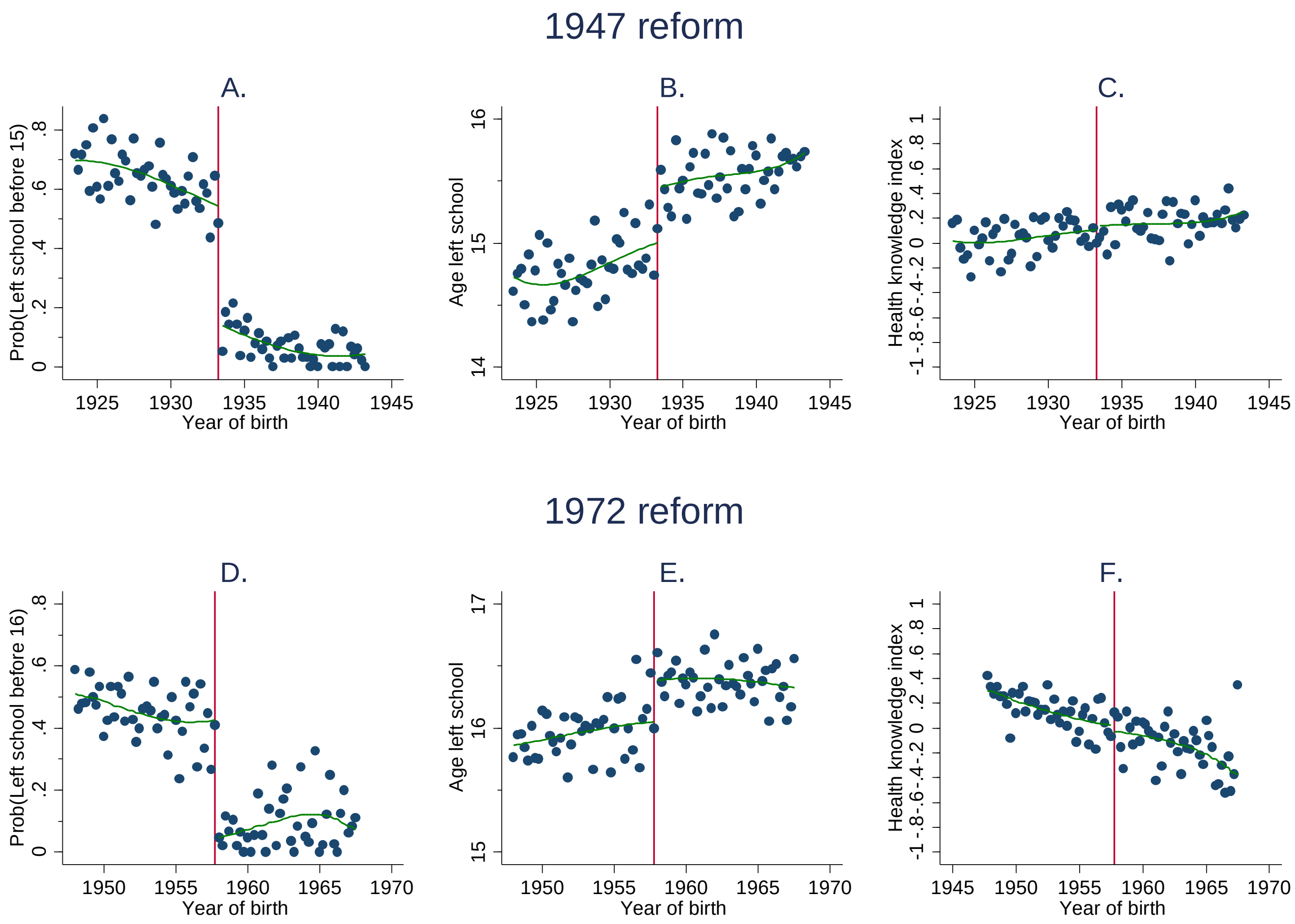
In Table A.1 we test the sensitivity of our results to alternative definitions of the health knowledge index. For ease of comparison, Column (1) presents the OLS and 2SLS estimates from Table 4. In Column (2) we use a stricter definition of a 'correct' answer by defining correct as any cause that at least 50 percent of doctors mentioned. Again we find a significantly positive OLS coefficient and an insignificant 2SLS coefficient. In Column (3) we measure health knowledge by the total number of correct responses across all of our questions (potentially multiple correct responses per question). This health knowledge definition more ably measures the respondents' depth as well as breadth of knowledge. Interestingly, the OLS estimated effect is significantly larger than for the other health knowledge definitions. A likely explanation is that cognitive ability is more strongly associated with the practice of providing multiple responses per condition, and therefore the OLS estimate is more heavily biased. The corresponding 2SLS estimate is near-zero and statistically insignificant, which supports this explanation. In Column (4) the responses of nurses are used to define a 'correct' response, and in Column (5) an alternative type of health knowledge is measured, based on questions related to the fibre content of different foods (see Section 2.1). Again, our results are insensitive to using these alternative measures.

Table A.1. Sensitivity of Results to Alternative Definitions of Health Knowledge

	Doctor def. 1	Doctor def. 2	Doctor def. 3	Nurse def.	Nutrition
	(1)	(2)	(3)	(4)	(5)
A. OLS					
Age left school	0.151***	0.115***	0.230***	0.103***	0.174***
	(0.009)	(0.009)	(0.010)	(0.009)	(0.010)
B. 2SLS					
Age left school	-0.001	-0.090	-0.030	-0.070	0.082
	(0.090)	(0.092)	(0.094)	(0.089)	(0.103)
Over-id test (p-value)	0.467	0.109	0.383	0.324	0.969
Sample size	11085	11085	11085	10779	9349

Notes: Figures are estimated coefficients on age left school. Standard errors clustered at the individual level are shown in parentheses. All dependent variables are standardised to have a mean of 0 and a standard deviation of 1. In column (1) the dependent variable denotes number of questions answered correctly based on causes mentioned by ≥ 25% of doctors (baseline). In column (2) the dependent variable denotes number of questions answered correctly based on causes mentioned by ≥ 50% of doctors. In column (3) the dependent variable denotes total sum of correct answers across all questions (potentially multiple answers per question) based on causes mentioned by ≥ 25% of doctors. In column (4) the dependent variable denotes number of questions answered correctly based on causes mentioned by ≥ 25% of nurses. In column 5 the dependent variable denotes number of correct answers to nutrition questions. Included but not shown are covariates representing a quartic function in quarter-year-of-birth, a quartic function in age, gender, month of birth, and survey year. *, ** and *** denote statistical significance at the 0.10, 0.05 and 0.01 level, respectively

Figure 1. Age Left School and Health Knowledge by Quarter-Year of Birth



Notes: Sample size equals 11,085. The dots represent average schooling and health knowledge by quarter-year of birth. The lines represent the fitted values from an OLS regression that includes reform dummies (separately for each reform) and a fourth-order polynomial in quarter-year of birth. The vertical lines denote the cut-offs for each reform. Health knowledge index is standardised to have a mean of 0 and a standard deviation of 1

Table 1. Causes of diseases mentioned by at least 25 percent of doctors in the sample

Disease	Wave 1	Wave 2
Stomach ulcers	Stress, diet	Stress, diet
Chronic bronchitis	Smoking, pollution	Smoking, pollution, general health
High blood pressure	Stress, heredity, diet, smoking, salt	Diet, heredity, stress
Migraine	Stress, heredity, foods	Foods, stress, heredity
Liver trouble	Alcohol	Alcohol, infection, diet
Stroke	High BP, stress	High BP, diet, circulation/cholesterol
Lung cancer	Smoking	Smoking
Heart trouble	Smoking, stress, inactivity, diet, heredity	Smoking, diet, heredity, obesity, inactivity
Severe depression	Heredity, bereavement, stress, attitude	Stress, heredity, attitude
Piles and haemorrhoids	Constipation, bad diet	Constipation, bad diet, low-fibre diet
Number of doctors	21	10

Table 2. Effects of Health Knowledge in 1984/85 on Health Behaviours in 1991/92

Dependent variable:	Coeff.	S.E.	Mean
Smokes	-0.012**	(0.005)	0.306
Heavy drinker	-0.004	(0.006)	0.257
Exercise:			
More active than average person	0.017**	(0.007)	0.287
Walking hours per week	-0.061	(0.111)	7.402
Participates in vigorous activities	0.032***	(0.008)	0.657
Diet:			
Fruit at least once per week in summer	0.034***	(0.008)	0.619
Fruit at least once per week winter	0.018**	(0.008)	0.441
Raw vegetables at least once per week in summer	0.023***	(0.007)	0.255
Raw vegetables at least once per week in winter	0.007	(0.005)	0.080
Fish once per week or more	0.021***	(0.008)	0.626
Poultry once per week or more	0.020***	(0.008)	0.717
Processed meat more than twice per week	-0.036***	(0.008)	0.457
Red meat more than twice per week	-0.011	(0.007)	0.811
Usually eats wholemeal bread	0.040***	(0.008)	0.489
Usually drinks skim milk	0.054***	(0.008)	0.591

Notes: Sample size equals 3,782. Figures are estimated coefficients on health knowledge from 15 separate OLS regressions. Robust standard errors are shown in parentheses. Health knowledge index is standardised to have a mean of 0 and a standard deviation of 1. Included but not shown are covariates representing age, sex, and lagged dependent variable. The last column presents the mean of the dependent variable. *, ** and *** denote statistical significance at the 0.10, 0.05 and 0.01 level, respectively.

Table 3. Effects of Demographic and Socioeconomic Characteristics on Health Knowledge

	Coeff.	S.E.
Age / 10	0.522***	(0.065)
Age squared / 10	-0.005***	(0.001)
Male	-0.192***	(0.025)
Married[a]	0.031	(0.038)
Widowed, separated or divorced[a]	0.007	(0.047)
Reside in Wales[b]	-0.055	(0.044)
Reside in Scotland[b]	0.025	(0.036)
Age left school	0.099***	(0.010)
Social class: Professional[c]	0.246***	(0.066)
Social class: Employer/manager[c]	0.213***	(0.059)
Social class: Other non-manual[c]	0.250***	(0.057)
Social class: Manual skilled[c]	0.109**	(0.055)
Social class: Semi-skilled[c]	0.047	(0.058)
Full-time employed[d]	0.052*	(0.031)
Part-time employed[d]	0.077**	(0.033)
Unemployed[d]	-0.059	(0.057)
Net weekly HH income / 100	0.084***	(0.011)
Household size	-0.028***	(0.009)
Owns a house/flat	0.188***	(0.026)

Notes: Sample size equals 8,837. Figures are estimated OLS coefficients Standard errors clustered at the individual level are shown in parentheses. Health knowledge index is standardised to have a mean of 0 and a standard deviation of 1. [a] Marital status; omitted category is single. [b] Region; omitted category is England. [c] Socio-economic group of the household head; omitted category is unskilled. [d] Employment status of the respondent; omitted category is not in labour force. *, ** and *** denote statistical significance at the 0.10, 0.05 and 0.01 level, respectively.

Table 4. Schooling Reforms, Age Left School and Health Knowledge

	Age left school	Health Knowledge		
	(1)	(2)	(3)	(4)
1947 reform (δ_1)	0.567***	-	-0.029	-
	(0.097)		(0.064)	
1972 reform (δ_2)	0.306***	-	0.040	-
	(0.076)		(0.063)	
Age left school	-	0.151***	-	-0.001
		(0.009)		(0.090)
F-statistic	32.03	299.26	0.26	<0.001
F-statistic p-value	<0.001	<0.001	0.768	0.989

Notes: Sample size equals 11,085. Columns (1), (2) and (3) present OLS coefficient estimates. Column (4) presents the 2SLS coefficient estimate. Standard errors clustered at the individual level are shown in parentheses. Health knowledge index is standardised to have a mean of 0 and a standard deviation of 1. Included but not shown are covariates representing a quartic function in quarter-year-of-birth, a quartic function in age, gender, month of birth, and survey year. *, ** and *** denote statistical significance at the 0.10, 0.05 and 0.01 level, respectively. F-statistic relates to the (joint) statistical significance of the presented coefficients.

Table 5. Estimated Effects of Schooling on Knowledge about Particular Health Conditions

	OLS		2SLS	
	Coeff.	S.E.	Coeff.	S.E.
Stomach ulcers	0.058***	(0.003)	0.010	(0.039)
Chronic bronchitis	0.024***	(0.004)	-0.037	(0.044)
High blood pressure	0.053***	(0.004)	0.039	(0.040)
Migraine	0.026***	(0.004)	-0.001	(0.041)
Liver trouble	0.029***	(0.004)	0.005	(0.039)
Stroke	0.028***	(0.004)	-0.023	(0.042)
Lung cancer	0.008***	(0.003)	-0.047	(0.030)
Heart trouble	0.049***	(0.004)	0.004	(0.039)
Severe depression	0.017***	(0.004)	0.055	(0.043)
Piles and haemorrhoids	0.021***	(0.004)	-0.008	(0.044)

Notes: Sample size equals 11,085. Dependent variables are binary variables that indicate if a respondent mentioned at least one cause correctly. Figures are estimated coefficients from 10 OLS and 10 2SLS regressions. Standard errors clustered at the individual level are shown in parentheses. Included but not shown are covariates representing a quartic function in quarter-year-of-birth, a quartic function in age, gender, month of birth, and survey year. *, ** and *** denote statistical significance at the 0.10, 0.05 and 0.01 level, respectively.

Table 6. Estimated 2SLS Effects of Parental Death on Knowledge

	Any cause death	Cardiovascular disease death			
Knowledge type:	Overall health	Overall health	High blood pressure	Stroke	Heart trouble
	(1)	(2)	(3)	(4)	(5)
Age left school	0.073	0.100	0.016	-0.002	0.097*
	(0.135)	(0.107)	(0.052)	(0.055)	(0.052)
Death of parent	-0.013	0.087***	0.021	0.019	0.032**
	(0.047)	(0.027)	(0.014)	(0.015)	(0.013)
Interaction	0.017	0.003	0.013	0.022	-0.010
	(0.094)	(0.057)	(0.028)	(0.030)	(0.028)

Notes: Sample size equals 6,795. Dependent variable is health knowledge in 1984/85 (wave 1). Figures are 2SLS coefficients. Age left school is demeaned, so the coefficient on death of parent measures the effect at the mean age left school (15.5 years). Robust standard errors are shown in parentheses. Included but not shown are covariates representing a quartic function in quarter-year-of-birth, a quartic function in age, gender, and month of birth. *, ** and *** denote statistical significance at the 0.10, 0.05 and 0.01 level, respectively.

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