Does Children’s Education Improve Parental Health and Longevity? Causal Evidence from Great Britain*

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December 6, 2022

Abstract

Parents with better-educated children are healthier and live longer, but whether there is a causal effect of children’s education on their parents’ health and longevity is still unclear. First, we demonstrate an association between adults’ offspring education and parental mortality in the 1958 British birth cohort study, which remains substantial—about two additional years of life—even when only comparing parents with similar socioeconomic status. Second, we use the 1972 educational reform in England and Wales, which increased the minimum school-leaving age from 15 to 16 years, to identify the presence of a causal effect of children’s education on parental health and longevity using census-linked data from the Office for National Statistics Longitudinal Study. Results reveal that children’s education has no causal effects on a wide range of parental mortality and

*This manuscript is forthcoming in *Journal of Health and Social Behavior*, doi: [10.1177/00221465221143089](https://doi.org/10.1177/00221465221143089). Data used in this study are publicly available and the authors provide a replication package (Potente *et al.*, 2022), for details see Appendix A. A blog post about this manuscript is available on UCL’s [Linking our Lives](#) blog.

Previous versions of this manuscript were presented at the 2018 BSPS annual conference in Winchester, the 2018 ‘Rostock Retreat on Causal Inference’ at the Max Planck Institute for Demographic Research, 2018 ECSR conference in Paris, the 2019 PAA annual meeting in Austin, and seminars at Nuffield College, Sciences Po, and University of Leipzig. The authors thank discussants at these occasions as well as Rachel Stuchbury and Wei Xun for invaluable help.

This study has received funding from the European Research Council (ERC) under the European Union’s Horizon 2020 research and innovation program under grant agreement no. 681546 (FAMSIZE MATTERS). Patrick Präg’s work was supported by the French National

health outcomes. We interpret these findings discussing the role of universal healthcare and education for socio-economic inequality in Great Britain.

INTRODUCTION

Those with better education enjoy better health (Galama *et al.*, 2018), and the benefits of education may also spill over to one’s peers’ health, such as spouses (Huijts *et al.*, 2010) and siblings (Kravdal, 2008). Recently, intergenerational health benefits of education have received increasing attention, as better-educated parents have healthier children (Vollmer *et al.*, 2017) and parents to better-educated children live longer and healthier (Elo *et al.*, 2018; Wolfe *et al.*, 2018a,b). However, the spillovers from children to parents are understudied (De Neve and Kawachi, 2017), especially in terms of the causal relationship (Hu and Bobak, 2018). In this study we ask two questions: Are adult children’s educational attainment and parental health and mortality associated in Great Britain? Does an increase in compulsory education produce benefits for population health through spillover effects from adult children to their parents?

Life course theory has long emphasized the importance of linked lives, the fact that life courses of significant others are interlocked (Elder *et al.*, 2003). In line with this reasoning, children’s life courses—including their educational attainment—can be expected to have implications for their parents’ lives. We connect this theoretical perspective with ‘fundamental cause’ theory (Clouston and Link, 2021), which stresses the role of individuals leveraging resources to gain health advantages in the emergence of health inequalities. Based on this, our study examines how far parents are able to utilize the resources of significant others—namely the education of their children—to improve their own health.

Children’s education is correlated with a number of different parental health outcomes: parents of better-educated children are less depressed (Lee *et al.*, 2017; Sabater and Graham, 2016b; Wang *et al.*, 2022; Wu and Penning, 2019; Yahirun *et al.*, 2020a), have better cognitive functioning (Ma *et al.* (2021); Yahirun *et al.* (2020b)), suffer less from functional limitations (Yahirun *et al.*,

Research Agency (ANR), ‘Investissements d’Avenir’ [LabEx Ecodec/ANR-11-LABX-0047].

The permission of the Office for National Statistics to use the Longitudinal Study is gratefully acknowledged, as is the help provided by staff of the Center for Longitudinal Study Information and User Support (CELSIUS). CELSIUS is supported by the ESRC Census of Population Program under project ES/K000365/1. The authors alone are responsible for the interpretation of the data. ONS agrees that the figures and descriptions of results may be published. This does not imply ONS’s acceptance of the validity of the methods used to obtain these figures, or of any analysis of the results. This work contains statistical data from ONS which is Crown Copyright. The use of the ONS statistical data in this work does not imply the endorsement of the ONS in relation to the interpretation or analysis of the statistical data. This work uses research datasets which may not exactly reproduce National Statistics aggregates.

The permission of Paul Norman to use the 2011 Carstairs index of deprivation he created

2017; Zimmer *et al.*, 2002), less from inflammation (Lee, 2018), and live longer (Elo *et al.*, 2018; Friedman and Mare, 2014; Jiang, 2019; Sabater and Graham, 2016a; Torssander, 2013, 2014; Wolfe *et al.*, 2018a,b; Yahirun *et al.*, 2017; Yang *et al.*, 2016). This has become known as the ‘social foreground’ hypothesis (Torssander, 2013). The evidence so far suggests there might be health benefits to children’s education that extend beyond their own lives, and that investing in education is an effective strategy for improving the living conditions of a wider range of people. Some of the reported health benefits are large; Friedman and Mare (2014) for instance report that having a child with college education rather than a child who has not finished high school goes along with almost two additional years of life in the US. However, unobserved heterogeneity poses serious threats to the validity of the causal link. Since life expectancy as well as the social relationship between offspring and mother/father differ by gender, we examine men and women separately in all our analyses.

Causality is the crucial issue for the analysis of educational spillover effects. The association between children’s education and parental longevity might be driven by endowments of the parental family, either socio-economic or genetic, that affect both children’s education and parental longevity. However, across existing studies, the approaches used to rule out confounders often have considerable limitations. While Torssander (2013) is able to include sibling fixed effects in the parental generation which control for family background factors, all other associational studies are rarely able to include comprehensive controls for parental socio-economic status (SES). Friedman and Mare (2014) for instance control for parental income, wealth, and both parents’ education, but many studies have only few control variables for the socio-economic status of the older generation at their disposal, e.g. only occupation (Zimmer *et al.*, 2016) or education (Zimmer *et al.*, 2007). At the same time, however, the question remains to what extent these SES indicators are actually confounders and not pathway variables by which children’s education exerts its positive effects, and whether accounting for them biases the findings for children’s education (De Neve and Kawachi, 2017). A confounding pathway that would link offspring education to parental longevity could be a genetic one, and indeed, genetic variants predict-

is gratefully acknowledged.

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ing both education and longevity have recently been identified (Krapohl *et al.*, 2014; Marioni *et al.*, 2016).

A natural experiment allows overcoming the problems of confounding, observed or unobserved. Quasi-natural experiments exploit the variation in the key explanatory variable—in this case education—which is caused by factors outside individuals’ power, such as a policy change. Our study uses the 1972 educational reform in England and Wales, which increased the minimum school leaving age from 15 to 16 years, to identify the presence of a causal effect of children’s education on parental health and longevity by means of a regression discontinuity design. The intuition is that we compare children born just before the date after which they have to stay a year longer in school to those children who were born just after the cut off date. The assumption is that the negligible difference in birth timing is quasi-random and that the two groups of children differ only in whether they receive the treatment (being required to stay one year longer in school) or not and do not differ in other observed or unobserved characteristics.

The one-year increase in compulsory schooling is a good opportunity to explore the causal link between adult offspring’s education and health for three reasons. First, the 1972 reform has affected a large proportion of the cohort in question, around 25 per cent (Clark and Royer, 2013), which is much higher compared to reforms in other countries such as the United States, Norway, Canada, and France (Albouy and Lequien, 2009; Black *et al.*, 2008; Lleras-Muney, 2005; Oreopoulos, 2006). Second, the 1972 education reform had important consequences for the affected cohorts. Research has demonstrated large effects on wages and labor market outcomes (Devereux and Hart, 2010; Grenet, 2013; Oreopoulos, 2006) and some health benefits (Davies *et al.*, 2018). Although an extra year of schooling might seem a modest amount to identify educational differences, increasing the age at school-leaving for an entire cohort represents a major effort for policymakers and educational practitioners. The 1944 Educational Act raised the minimum school-leaving age from 14 to 15 years in 1947 and put provisions into place for a second one-year increment that was to follow once conditions allowed. It took another 25 years until this happened, emphasizing the size of the undertaking that such a one-year increment means in practice. Third, the reform restricted itself to increasing the mandatory school-leaving age without implementing other changes in the educational system, making it easier to identify an effect of the reform.

Educational spillover effects have important policy implications. Improving the education of younger generations could prove to be a useful policy instrument in contexts where older populations are growing without precedence (Lutz *et al.*, 2008), since it might serve as a public health intervention that affects not

just younger, but also older generations. Policies creating such intergenerational spillovers would also alleviate concerns about generational fairness of policy measures, in the sense that other interventions for the benefit of one generation come at the expense of another generation (Friedman and Mare, 2014). If education is a family resource that provides benefits across generational boundaries, investing in education is not a zero-sum game from a generational perspective, and a focus on the individual returns to education would underestimate the societal returns overall.

We contribute to the study of ‘social foreground’ in four ways. Our first contribution is to document the association between adult children’s education and parental mortality in Great Britain, a context with limited public welfare provision yet a universal health care system, for the first time. We use high-quality birth cohort data to document the association. Our second contribution is to examine the causal nature of the association between children’s education and parental health with linked census data from England and Wales, whose large sample size allows for precise estimates. Third, we employ a regression discontinuity approach to identify the causal effect of children’s education on parental mortality and health. Regression discontinuity approaches have an advantage over the instrumental variable approaches used in the existing literature, as the regression discontinuity approach allows to better discern the causal impact of the reform from a secular time trend in the data (Galama *et al.*, 2018). Our fourth contribution is to extend our analysis beyond mortality and include self-reported health and long-standing illness as outcomes. Some mixed results found in the literature so far could be because of the wide range of outcomes being analyzed separately. By reporting results for a large number of different outcomes we aim to obtain a comprehensive picture of the causal effect of offspring’s education on parental health.

Our study proceeds as follows. In the next section, we review the existing literature and identify mechanisms linking adult children’s education and parental health. In a first empirical analysis, we use high-quality birth cohort data of individuals born all in one week of 1958 (Power and Elliott, 2006) to describe the association between adult children’s education and parental longevity. This part of the analysis is to show that there is a clear relationship between changes in children’s schooling and parental mortality in those cohorts. In the second empirical analysis, we exploit an educational reform affecting the birth cohorts from 1957 onwards and linked census data (Shelton *et al.*, 2019) to assess whether the association between adult children’s education and parental health can be considered causal. These two analyses exploit individual strengths of the two datasets and complement each other. The 1958 cohort study can be used to show the association between educational attainment and parental life expectancy, yet it does not allow ruling out crucial confounding factors using a

quasi-experimental design. The census data allow us to use a quasi-experimental design as children vary in their birth dates around the time of an educational reform, but does not provide information on children’s educational attainment necessary to establish association¹. We conclude by discussing implications for universal health care and the role of education in socio-economic inequality in Great Britain.

BACKGROUND

What We Know so Far

Only few studies use exogenous variation to identify a causal effect of adult children’s education on parental health (De Neve and Fink, 2018; Ludwig *et al.*, 2021; Lundborg and Majlesi, 2018; Ma, 2019; Ma *et al.*, 2021), relying on schooling reforms to instrument the parental health effects of an increase in children’s education. Most studies are based in low- and middle-income countries (De Neve and Fink, 2018; Ludwig *et al.*, 2021; Ma, 2019; Ma *et al.*, 2021), showing e.g. a thirty per cent increase (1.1 years) in children’s education reduced parental mortality in Tanzania by 3.7 percentage points for mothers and 0.8 percentage points for fathers (De Neve and Fink, 2018). The only study in a resource-rich setting, Sweden, does not find any overall effect (Lundborg and Majlesi, 2018). Only daughter’s education has positive effects for fathers, particularly low-educated fathers, and almost a third of the increase in the likelihood to live to age 80 between 1943 and 1955 is due to an increase in daughters’ schooling. However, the compulsory schooling reforms in Sweden did not only increase the number of years of schooling, but brought about other changes in the educational system, introducing a new curriculum as well as tracking, raising the question as to what exactly of the reform caused the improvement in fathers’ health. Moreover, the few studies that used admission lotteries for healthcare professionals in Sweden and the Netherlands show mixed evidence for children’s effects on parental health (Artmann *et al.*, 2022; Chen *et al.*, 2022).

Theoretical Mechanisms

There are various hypotheses on how adult children’s education could be linked to parental health. Firstly, better-educated children can induce better health behaviors in their parents, which lead to greater longevity. This would be consistent with the association found by Friedman and Mare (2014). They show

¹The census and cohort datasets use parent-centric and child-centric focal points respectively. While most studies connecting adult children’s education and parental health have been based on parent-centric data, both focal points are equivalent for the analyses conducted here.

that the association between children’s education and parental longevity is particularly strong when it comes to lung cancer and chronic respiratory diseases, two causes of death linked to smoking. However, Ma (2019) does not find any causal effect of offspring’s education on parents smoking and drinking intensity in China. Risky health behaviors such as alcohol consumption and smoking have long been highly prevalent in Great Britain across the 20th century and only decreased in recent years. Prevalence of binge drinking and obesity are still above the European average (OECD and European Observatory on Health Systems and Policies, 2017). Compared to the United States, Great Britain has similar smoking patterns, but lower obesity and greater heavy drinking prevalence (Banks *et al.*, 2006). In general, old-age health for US residents appears worse than for British people across the whole socio-economic distribution (Banks *et al.*, 2006, 2010).

Secondly, better-educated children are able to generate higher earnings (Oreopoulos, 2006; Devereux and Hart, 2010; Grenet, 2013), which would allow them to financially support their parents. For instance, De Neve and Fink (2018) interpret their finding that male education is more beneficial for parental, particularly maternal, health in Tanzania, as being due to greater labor market returns to education for this group. Yahirun *et al.* (2017) and Ma (2019) also find that transfer payments from children to parents are serving as a pathway affecting parental health. These financial and material pathways are likely more relevant in poorer countries, where access to health care and, more importantly, healthy living conditions are contingent on financial resources. Further, intergenerational flows of material resources in Europe mostly go from the older generation to the younger generation (Albertini *et al.*, 2007; Attias-Donfut *et al.*, 2005), thus it is questionable how salient this financial support pathway in Great Britain would be. However, wealthier children could buy and help buy private insurance for their parents.

Thirdly, better education opens avenues into occupational fields that have direct health benefits to parents, namely working in health care (Elo *et al.*, 2018). This advantage can go indirectly via better information about health care as well as directly, namely via preferential access and treatment. However, for Britain, with its universal and highly centralized health care system, it is unlikely that direct effects like preferential access are operating. However, the British health care system is very complex and particularly low-educated parents could benefit from having children who know how to navigate it and to improve the doctor–patient relationship (Präg *et al.*, 2017). Doctors are highly educated and in authoritative positions; children with more education may be more able and more comfortable to communicate with doctors and to negotiate on behalf of their parents. Also, the fundamental causes perspective on health inequalities (Phelan *et al.*, 2010) would argue that better-educated children are better able

to support their parents in making use of resources that reduce preventable mortality.

Fourthly, a stress pathway was recently proposed (Tosi and Albertini, 2019; Barr *et al.*, 2018). Barr *et al.* (2018) showed how problems in the transition to adulthood for black adolescents (e.g. unemployment, romantic breakups, or arrests) heighten their mothers’ cumulative biological risk for chronic diseases. Thus, children leaving school early might be a stressful life event that has a toll on parents’ health. Moreover, successful offspring allows parents to invest fewer resources in financial and instrumental support to children. This implies that parental resources can be used to benefit the parents’ own health and material circumstances.

Fifthly, shared genetic endowments might constitute a non-causal, confounding pathway linking adult children’s education to parental health. This genetic confounding occurs when the genotype—partially shared between parents and children—is associated with both children’s educational attainment and parental health (Krapohl *et al.*, 2014). Marioni *et al.* (2016) showed that genetic variants associated with education are also linked to longevity. For example, parents of children with a strong genetic disposition for educational attainment lived half a year longer than parents of children with a weak disposition. Moreover, other sources of genetic confounding are also possible. Genetic endowments could drive selection into and exposure to individual and parental environment (Pingault *et al.*, 2018; Kong *et al.*, 2018). For instance, genes related to education might steer individuals into environments where health-promoting attitudes and behaviors are prevalent and which in turn might enhance individuals’ and parental health. The presence of a gene–environment correlation might suggest that social environment could be structured by genotype.

The effect of children’s education on their parents may differ by parental characteristics. Lower-SES parents may benefit more from having better-educated children because they have fewer resources. In their study on the Swedish population, Lundborg and Majlesi (2018) documented stronger effects of offspring’s education for lower-educated fathers. However, this is not generally the case. In their analysis of older Chinese, Yang *et al.* (2016) did not find any differences in having higher-educated children among the different educational groups of parents, while in an analysis of eleven European countries, Sabater and Graham (2016a) find mixed associational results depending on the gender of the parent and child.

Great Britain as a Research Setting

Great Britain is a relevant setting for the study of educational spillover onto parental health. So far, evidence of educational spillovers from children to parents’ health exist for resource-poor settings in Africa (De Neve and Fink, 2018;

De Neve and Harling, 2017), for countries where family cohesion is high and intergenerational coresidence is common (Lee *et al.*, 2017; Lee, 2018; Yahirun *et al.*, 2016, 2017; Yang *et al.*, 2016; Zimmer *et al.*, 2002, 2007; Stone *et al.*, 2014), for countries with an extensive welfare state (Elo *et al.*, 2018; Lundborg and Majlesi, 2018; Torssander, 2013, 2014), and for the US (Friedman and Mare, 2014; Wolfe *et al.*, 2018a,b), where filial obligations are low. Britain is unique in this context, as it combines a fairly limited welfare state that is in some ways comparable to the American one, with universal health care, namely the National Health Service, which enjoys strong public support. Yet, the British case leaves room for beneficial effects of adult children’s actions: There is not a generous universal state pension that would support parents in old age (Blake, 2003). Further, behavioral health risks are more pronounced in Britain than in other European countries, with higher levels of alcohol consumption and more dangerous consumption patterns (binge drinking), more ex-smokers than in other countries, and a higher prevalence of overweight (Huijts *et al.*, 2017; Präg *et al.*, 2010).

While parental co-residence is rare, more than fifty per cent of Britons live within half an hour of their parents (Chan and Ermisch, 2015), levels of contact can be considered high when compared to other European countries (Tomassini *et al.*, 2004), and more than half of children provide care to their parents by age fifty (Evandrou *et al.*, 2018). However, the English population is found to be healthier than the US population when looking at biomarkers and self-reported conditions (Banks *et al.*, 2006; Präg and Subramanian, 2017; Präg *et al.*, 2022).

EMPIRICAL ANALYSIS 1: COHORT STUDY

Data: 1958 National Child and Development Study

The 1958 National Child Development Study (NCDS, Power and Elliott, 2006) is a longitudinal cohort study following the lives of all 18,558 people born in England, Scotland, and Wales in the same week in 1958 from birth until now. Follow-up has been good over time, 84 per cent of cohort members still participated at the age of 16, with a gradual decline in participation throughout adulthood—at age 55, 8,958 respondents participated in the study, yet in mid-adulthood respondents were still broadly representative of the surviving cohort (Atherton *et al.*, 2008). Next to collecting rich information about cohort members, the cohort study also asks respondents about the lives of their parents. We reconstruct parental longevity up to the most recent wave in 2013–4, when cohort members are 55 years old.²

²The 2004 wave of the NCDS did not collect information about the exact timing of parental death. For parents who deceased between the 2000 wave and the 2004 wave (1,209 fathers and 879 mothers) we thus do not know the exact timing of death and hence imputed a random

Covariates were measured as follows. Adult children’s education is based on a detailed educational qualification history, which was converted into school-leaving age entered as a continuous variable. This coding approach ensures similarity of our estimates with Empirical Analysis 2, which uses school-leaving age to identify the causal effect of children’s education on parental health and mortality. For parents, school-leaving age of respondents’ fathers and mothers were reported by the parents in the 1974 wave when respondents were age 16. We distinguish between four groups which reflect the most common educational qualifications in the parental generation. Age 13–15 years is equivalent to no qualification (reference category), age 16–17 years indicates lower secondary education, age 18–19 years denotes upper secondary or lower tertiary education, and age 20 years and more equals a university degree. Parental social class stems from the same questionnaire and is based on parental reports of occupations, self-employment status, and supervisor status. We convert this to a seven-class version of the Erikson–Goldthorpe class schema³ with a dominance approach where we only count the highest class among the two parents (Connelly *et al.*, 2016). We further account for the sex of the child (ref. male), whether the child was born out of wedlock (ref. in wedlock), and parental age at birth.

Method: Cox Regression

To describe the association of adult children’s education and parental survival, we use the Cox regression model (Cox, 1972), a semi-parametric method which makes no assumptions about the functional form of the hazard. We estimate two models: the baseline model includes adult children’s education as well as the control variables sex, out-of-wedlock birth, and parental age at birth in the equation. The second model additionally controls for parental social class as well as mothers’ and fathers’ school-leaving age. For both models subjects enter the risk pool when parents are 45 years old.

Table 1 reports descriptive statistics concerning the children’s and parents’ characteristics. Only 45 per cent of the mothers have died by 2013–4, while 67 per cent of the fathers have died. The median age at death is 69.7 years for mothers and 69.3 years for fathers. Maternal birth years range from 1910 to 1950, while paternal birth cohorts span from 1880 to 1942. Most of the cohort

year of death for the cases in question. Robustness checks showed that this did not affect our results. Cohort members’ fathers and mothers were on average born in 1927 ($SD = 6.4$ years) and 1931 ($SD = 5.7$ years), respectively. We retain 11,710 cases for which parental survival was possible to record from the 1999/2000 wave—when parental survival was first probed—and who remain in the sample at least until the 2013–4 wave or until their parents decease.

³I) professionals, administrators and managers, higher-grade (reference category), II+IVa) professionals, administrators and managers, lower-grade, small employers, higher-grade technicians, III) routine non-manual employees, IVb+c) self-employed workers (including farmers), V) lower-grade technicians, supervisors of manual workers, VI) skilled manual workers, VII) non-skilled manual workers.

Table 1: Descriptive statistics for NCDS participants (children)

	Prop./Mean	<i>SD</i>
Mother's avg. age at death	69.7	12.3
Mother deceased	0.45	
Father's avg. age at death	69.3	12
Father deceased	0.67	
Female child	0.51	
Child born out of wedlock	0.04	
Mother's avg. age at birth	27.46	5.73
Father's avg. age at birth	30.57	6.45
Child's age at leaving education:	17.11	2.0
Father's age at leaving education:		
13–15 y.	0.58	
16–17 y.	0.30	
18–19 y.	0.07	
20+ y.	0.05	
Mothers's age at leaving education:		
13–15 y.	0.47	
16–17 y.	0.42	
18–19 y.	0.07	
20+ y.	0.04	
Parents' social class:		
I	0.06	
II+IVa	0.12	
III	0.18	
IVb+c	0.06	
V	0.07	
VI	0.21	
VII	0.29	
<i>N</i>	11,710	

Source: NCDS (Power and Elliott, 2006), own calculations.

members were born to married parents, only four per cent to unmarried ones.

Results

Figure 1: Better-educated children have parents who live longer (hollow dots), even when comparing parents from the same social class and with the same educational attainment (solid dots). Hazard ratios based on the models reported in Table A2.

Note: Spikes denote 95 per cent confidence intervals. *Source:* NCDS (Power and Elliott, 2006), own calculations.

Figure 1 presents the results from the Cox proportional hazard models and confirms the association between children's education and parental mortality. Children who have left school later have longer-living parents. In the baseline model (top row), one additional year of children's education is associated with a hazard ratio for maternal death of 0.93 (95 per cent confidence interval 0.92–0.95) and for paternal death of 0.95 (0.94–0.97), corresponding to a 6 per cent and 5 per cent decrease in the expected hazard.

Once we add parental social class and parental educational attainment to the equation, the association is attenuated, yet the overall pattern holds and remains statistically significant at conventional levels. Having a child who have left education at older age is associated with an 3 per cent decrease in the expected hazards of death for mothers and 2 per cent for fathers. This is a substantially important association, as predictions of median years of life lost based on the second models suggest that parents of less-educated children (i.e. leaving school at age 16) have a two years difference compared to parents of more educated ones (i.e. leaving school at age 21). This difference is similar

to predictions in previous studies reporting about two years difference in life expectancy (Friedman and Mare, 2014).

Finally, several robustness checks were carried out, reported in Table A4 and Table A5. First, we assessed the role of missing values by comparing the results to those obtained from using multiple imputation with chained equations (MICE, White *et al.*, 2011). Second, we restricted the sample restricted to birth cohort members who lived in England and Wales at age 16 (excluding those living in Scotland) in order to make it comparable to the data used in Empirical Analysis 2. Third, we included an interaction of children’s education with time. Fourth, we use an accelerated failure model with survival time following a Weibull distribution. In all cases, the magnitude of the coefficients changes slightly but the direction of the associations stays the same.

EMPIRICAL ANALYSIS 2: LINKED CENSUS DATA

Data: The ONS Longitudinal Study

The previous section has established the presence of associational findings while this second part of the analysis aims at determining whether there is a causal effect. We analyze the Office for National Statistics Longitudinal Study (ONS-LS, Shelton *et al.*, 2019), which combines linked census and life events data for a one-per cent sample of the population of England and Wales. The ONS-LS links records at each census since 1971 for people born on one of four selected (non-disclosed) dates in a year. Census information of the ONS-LS members is linked to life events data, including births, deaths, and cancer registrations. In 1971, women under the age of 60 who were married, widowed, or divorced were prompted to list the birth months and years of all live-born children who were born in wedlock. This birth date—expressed in three-month quarters around September 1—allows us to assess whether children were affected by the 1972 educational reform in England and Wales.

Our analytical sample includes all LS member who had children affected by the 1972 educational reform in England and Wales. It consists of 56,199 mothers who had at least one child born between 1949 and 1965. In addition, we analyze the 49,612 male ONS-LS members whose wives reported on their fertility history in the 1971 census. Table 2 describes the characteristics of the sample stratified by parental sex.

We analyze a range of both objective and subjective health outcomes. We examine mortality and premature mortality (before age 65). Respondents were followed until December 31, 2015 to assess their mortality status. Further, we distinguish between six causes of death based on the ICD classification, namely

Table 2: Descriptive statistics for ONS-LS sample members (parents)

	Men	Women
Education in 1971:		
% No qualification (<i>lower educated</i>)	85.4	90.9
% A-level (<i>higher educated</i>)	8.9	7.9
% Higher than A-level (<i>higher educated</i>)	5.6	1.1
% Having children affected by reform	51.9	51.1
Regional deprivation:		
% First quintile (least deprived)	19.6	19.1
% Second quintile	21.6	21.3
% Third quintile	21.0	21.7
% Fourth quintile	20.1	20.7
% Fifth quintile (most deprived)	17.7	17.2
Social class:		
% I/II social class	25.3	23.3
% III Non-manual social class	9.7	10.6
% III Manual social class	40.2	35.6
% IV/V social class	22.1	22.1
% Unclassified social class	2.7	8.4
Mean age at first birth	29.9	27.2
<i>N</i>	49,612	56,199

Source: ONS-LS ([Shelton et al., 2019](#)), own calculations.

lung cancer, accidents and self-harm, liver disease, ischaemic heart disease, mental and behavioral causes, and preventable causes in general (we define causes as preventable following the definition in [Office for National Statistics, 2011](#)). These causes of death were selected because their strong association with some health behaviors, such as smoking and excessive alcohol consumption. The aim is to test offspring’s influence through parental healthy lifestyle behaviors. Further, we examine different self-reported health outcomes, namely self-reported long-standing illnesses, measured in 1991, 2001, and 2011, and self-reported poor general health, measured in 2001 and 2011. Question wordings vary slightly between census years, see [Table A1](#) for exact phrasing. Descriptive statistics for all outcomes are shown in [Table 3](#).

We include the following control variables in our models: parent’s year of birth, dummy variables for parental occupational class, parental education (less than A-levels vs. A-levels and more), and number of children. Occupational class is indicated by the registrar-general’s class scheme ([Connelly *et al.*, 2016](#)), comprising classes I/II (‘Professional occupations’ and ‘Managerial and technical occupations’), III Non-manual (‘Skilled non-manual occupations’), III Manual (‘Skilled Manual occupation’), IV and V (‘Partly-skilled occupations’ and ‘Unskilled occupations’). Class was derived from respondent’s occupation in 1971. Women married at the time of the census have been assigned the occupational class of their husband if a husband was present, otherwise their own occupational class.

The parents in our sample can of course have multiple children who may or may not be affected by the reform. Therefore, we created a data set where each case referred to one child (rather than one parent) and we conduct a child-level analysis, as each parent can have several children, of which more than one can be affected by the reform. To account for this, we weight our estimates by the inverse of the number of children a parent has, in line with [Lundborg and Majlesi \(2018\)](#).⁴

Method: Regression Discontinuity Approach

Using a regression discontinuity design, we exploit the exogenous variation in age at school completion due to compulsory schooling law changes 1972 in England and Wales. The intuition behind a regression discontinuity design is to compare cases just below and above a fixed cut-off value that determines whether cases receive the treatment. The assumption is that these cases just below and just above the threshold differ only in whether they receive the treatment, but are (on average) the same on both observed and unobserved variables. This section

⁴Parents with multiple children are accounted for multiple times in the analysis. To prevent the resulting over-representation of larger families, we thus weight the estimates by the inverse of the number of children. This weighting by the number of children ensures that every parent is accounted for only once.

first describes the educational reform at the center of Empirical Analysis 2 and then describes our estimation approach.

The 1972 educational reform in England and Wales

Compulsory schooling laws in Britain define the maximum age at which children should start education and the minimum age at which they are allowed to leave education. Consequently, these laws determine how many years children spend in compulsory schooling. During the 20th century, two legal changes have increased the minimum ages at which children were allowed to leave school in England and Wales. First, the 1944 Educational Act, the ‘Butler Act,’ established an increase in school leaving age from 14 to 15. This change was implemented from April 1, 1947 onwards. Second, the same Education Act authorized a further increase in school leaving age from 15 to 16, which was implemented starting from September 1, 1972. This is the reform that we use for our study, as it falls in the observation period of the 1971 census and allows exploring causal effects of children’s education on parental mortality in different time periods.

These schooling reforms have been exploited in previous research but none has examined the intergenerational effects that we are interested in. [Clark and Royer \(2013\)](#), [Davies *et al.* \(2018\)](#), and [Grenet \(2013\)](#) have found that the 1972 reform increased earnings for both men and women, suggesting plausible spillover effects on parents through financial resources. [Davies *et al.* \(2018\)](#) further showed that the reform significantly reduced blood pressure, diabetes, heart attack risk, body mass index, sedentary and unhealthy behaviors. However, not all studies show positive effects of the 1972 reform on health and health behaviors and knowledge ([Clark and Royer, 2013](#); [Silles, 2009](#); [Janke *et al.*, 2020](#); [Johnston *et al.*, 2015](#)). For example, [Jürges *et al.* \(2013\)](#) have not found any effect on self-rated health and some biomarkers, such as level of fibrinogen and C-reactive protein. Similarly, [Powdthavee \(2010\)](#) showed that the 1972 reform did not decrease significantly the likelihood of hypertension. Despite the unclear effect of the 1972 educational reform on the health of the affected cohorts, it remains important to study the spillover effects of children’s education on parental health and mortality. The mechanisms linking children’s education to parental health that we have hypothesized are not necessarily dependent children’s health but could also operate via behaviors and earnings which have been found to be affected by the educational reform ([Clark and Royer, 2013](#); [Davies *et al.*, 2018](#); [Grenet, 2013](#)).

Figure 2 shows the increase in school-leaving age across birth cohorts in England. The 1972 reform has a clear effect in producing a discontinuous increase in years of education across birth cohorts. Furthermore, we can see that compliance with the reform is high. The reform reduced the percentage of individuals

with only nine years of schooling by almost seventeen percentage points. Next to that, the reform also did not affect the percentage of individuals with more than 16 years of education. We rely on the Health Surveys for England (1991–2013) for demonstrating this discontinuity, because we only know the quarter of birth, not the actual educational attainment of the children of ONS-LS sample members.

Figure 2: The 1972 educational reform greatly reduced the share of individuals leaving school at age 15 for those born after September 1, 1957. Years of full-time education by year of birth.

Source: Health Surveys for England, 1991–2013 (Mindell *et al.*, 2012), own calculations.

Identification strategy

Identification is achieved through a regression discontinuity (RD) approach (Imbens and Lemieux, 2008; Lee and Lemieux, 2010). In particular, RD designs have four main features: the running variable, the treatment, the cut-off, and the bandwidth. The running variable is the children’s date of birth, measured in quarter of birth (due to confidentiality restrictions imposed by the data provider), that leads to the assignment of the treatment. Second, the treatment is the increase in years of compulsory education for individuals born after September 1, 1957. The cut-off represents the quarter of birth after which the treatment is assigned. Therefore, the probability of treatment assignment based on the running variable has a discontinuous change after the reform implementation. Finally, the accuracy of estimation depends on the bandwidth,

i.e. the number of observations right before and after the reform threshold. It is crucial that the bandwidth estimation procedure is conducted in a data-driven way, such that the trade-off between bias (i.e. when the bandwidth is large, the larger is the bias) and variance (i.e. when the bandwidth is small, the larger is the variance) is minimized.

Our design is a ‘fuzzy’ (as opposed to ‘sharp’) regression discontinuity approach because the reform increases the probability to stay in education until age 16 but does not solely determine whether a student stays in school. All the analyses are carried out with the Stata package ‘rdrobust’ (Calonico *et al.*, 2017). We use a data-driven bandwidth estimation procedure (using the same mean square error optimal bandwidth on both sides) and check the sensitivity of the results to different bandwidths and bandwidths’ selection algorithm. Following Gelman and Imbens (2019), we chose a linear fit for the running variable and calculate robust standard errors. We add a number of control variables (listed above) to our models. We include control variables in our models because they increase the precision of the estimates by removing small residual imbalances between groups.

Using a regression discontinuity design to study the effects of a policy reform has an advantage over studies relying on instrumental variable (IV) approaches to estimate causal effects. IV methods in quasi-experimental settings can be sensitive to cohort trends in mortality. In particular, small effects of offspring’s education on parental mortality due to an educational reform are more difficult to distinguish from secular changes in mortality when an IV approach is used (Galama *et al.*, 2018). Our RD approach allows us to address such a bias.

We compare cases with children born right before and after the cut-off date. For illustration, Table 3 shows outcome variables and parental covariates for children born one year before and one year after the reform. The groups are balanced for the covariates, supporting the assumption of quasi-random assignment to the reform, but we observe some differences for the outcomes. The actual sample size in the models of the different outcomes depends on bandwidth used, as well as the number of non-missing observations for each outcome. We conducted an ex-post power analysis using the ‘rdpower’ command in Stata (Cattaneo *et al.*, 2019) in order to check whether a lack of statistical power can account for our results. The power analysis concludes that we have more than enough power (larger than 0.8, which is the customary threshold for power) to detect effect sizes half of a standard deviation of the outcome for the untreated and even smaller effect for many of the outcomes. Detailed information about the sample size for each estimation as well as the results from the power analysis are available in the supplementary materials.

The estimated treatment effects represent intention-to-treat effects (ITT), which are the causal effects of children’s reform eligibility defined by the thresh-

old rule (i.e. born after September 1, 1957) on parental health and mortality outcomes. ITT use the quasi-random assignment to the policy reform (since individuals cannot manipulate their date of birth) and thus are the average impact of the reform both for those who actually stay one additional year in compulsory education and for those who do not. How does an ITT differ from the average treatment effect, i.e. the effect of one additional year of education who actually stayed in school one year longer? The ITT does not take into account non-compliance (i.e. individuals who drop out of school before reaching the mandatory school age despite the reform), making it potentially smaller than the average treatment effect. Yet, previous studies have found very few people leaving school before compulsory age after the reform we build our analysis on ([Clark and Royer, 2013](#); [Silles, 2009](#)). Therefore we expect that our ITT is not substantively different from the average treatment effect. Data restrictions make it impossible to estimate the causal effect on those who comply to the policy reform (i.e. only those who stay one year more in compulsory schooling) since the linked census data contain only the date of birth but not the age at school leaving or educational attainment as children's information.

Table 3: Descriptive statistics for outcome variables and covariates for parents with any children born between one year before and one year after the reform

	Without children affected by reform	With children affected by reform
<i>Outcome variables</i>		
% deceased until December 31, 2015	62.1	55.9
% deceased before age 65 until December 31, 2015	14.2	14.7
% died of lung cancer	5.4	4.6
% died of accidents and self-harm	1.0	1.0
% died of liver disease	0.8	0.7
% died of ischaemic heart disease	11.3	8.9
% died of mental and behavioral causes	1.5	1.6
% died of preventable causes	8.0	7.9
% longstanding illness in 1991 census	24.3	21.7
% longstanding illness in 2001 census	43.3	41.4
% longstanding illness in 2011 census	63.6	60.5
% poor self-rated health in 2001 census	20.9	18.9
% poor self-rated health in 2011 census	18.1	16.8
<i>Parental covariates</i>		
% women	52.6	52.5
Average number of children	2.7	2.6
Average parental year of birth	1928	1930
% higher educated	12.3	12.7
Social class:		
% I/II	25.3	25.6
% III Non-manual	9.9	9.9
% III Manual	38.0	37.6
% IV/V	20.9	21.7
% Unclassified	5.7	5.5
<i>N</i>	6,089	6,057

Source: ONS-LS (Shelton *et al.*, 2019), own calculations.

Results

Figure 3 shows the proportion of parents who have died before 2016 by children’s quarter of birth. We observe a discontinuity for parents’ mortality: the proportion of deceased parents drops sharply for those whose children were affected by the reform. The discontinuity is more pronounced for fathers than for mothers.

Figure 4 presents the results from regression discontinuity analyses for all thirteen outcomes, stratified by parental sex. A pattern emerges: coefficients do not always point in the expected direction and usually do not reach conven-

Figure 3: The proportion of deceased parents decreased sharply for those whose children were affected by the reform

Source: ONS-LS (Shelton *et al.*, 2019), own calculations.

tional levels of statistical significance. Mothers with children affected by the reform experience a small reduction in mortality. Having a child affected by the reform reduces the probability of dying by 1.4 per cent, yet this estimate does not reach conventional levels of statistical significance (95% confidence interval: -4.0% – 1.2%). For fathers, the coefficient points in the same direction (-0.7% , 95% CI: -2.9% – 1.5%), but similarly it is not significant at conventional levels. Two estimates that do reach conventional levels of statistical significance are for women the risk of dying of ischaemic heart disease (-1.7% , -3% – -0.2%) and for men the risk of dying of liver disease (-0.5% , -0.9% – -0.04%). However, after correction for multiple comparisons with the Benjamini–Hochberg procedure for the six causes of death considered here, neither of these results is significant.

We further stratify the models by the socioeconomic position of the parents. Results show that there is no systematic difference by parental socioeconomic position in the effect of children’s education on parents’ outcomes. Results can be found in Figure A3 in the Supplementary Materials.

We conducted several robustness checks. First, we check that the results in Figure 4 are robust to multiple comparison correction using the Benjamini–Hochberg procedure for multiple tests in the causes of death considering them as independent outcomes. Results show that none of the coefficients remain

Figure 4: RD estimates of educational reform stratified by parental sex

Notes: Error bars denote 95% confidence interval based on robust standard errors. Controls comprise parental education, number of children, occupational class, and year of birth. Estimates shown in Table A3. *Source:* ONS-LS (Shelton *et al.*, 2019), own calculations.

significant after this adjustment. Second, in Figure A4 we stratified the models by parental education and by area deprivation as two additional measures of parental class. While a number of estimates are statistically significant (at the 5%-level), they are specific to single SES indicators, which does not support a notion that lower-SES parents benefit more from better-educated children. Third, in Table A7 we stratified models by the number of children that parents had. Effects might be especially pronounced among parents whose only child was affected by the reform rather than among parents with more children, some of whom will not be affected by the reform. This robustness check did not reveal any significant effects, which is in line with our overall findings. Finally, in Table A10 we investigate the effects of choosing different bandwidths around the threshold date. The results remain the same when different bandwidths are selected, demonstrating low sensitivity to the bandwidth chosen.

CONCLUSION

This study explored the intergenerational consequences of education for population health in Great Britain. First, we establish that there is an association between adult offspring’s education and parental mortality in Great Britain, which exists even when comparing only parents with the same occupational class background and the same educational attainment. Specifically, having a better-educated child increases median parental longevity by two years. Second, using an educational reform as a natural experiment, we analyze linked census data from England and Wales to show that the increase of children’s school-leaving age from 15 to 16 had no significant causal effect on parental health, overall mortality or specific causes of death. An investigation into heterogeneous treatment effects by parental socio-economic status shows that health benefits do not accrue particularly for parents from disadvantaged backgrounds, regardless of indicator of disadvantage we use.

In the first analysis we find a significant association between adult children’s education and parental longevity. From our second analysis, we conclude that this association does not result from a direct causal effect of education on parental health using a local experiment. We propose the absence of a causal effect in our study might be explained by three aspects in which the British context is unique: universal health care coverage, the relevance of education for determining socio-economic position, and the location of the change in education brought about by the 1972 schooling reform. First, we have to take into account the potentially equalizing effect of the National Health Service (NHS) in Great Britain. The NHS provides universal and comprehensive access to health care free at the point of use and covering all health care needs over the entire life course. This single-payer health care system potentially removes financial

barriers to accessing health care, making parents rely less on their children’s economic resources for support.

Second, a possible explanation could be that in Great Britain occupational class—rather than education—is particularly salient for life chances, especially compared to countries such as the US, where education is a more important driver of life chances. Parental occupational class of origin is a major predictor of children’s pay, even in professional and managerial occupations in the United Kingdom (Laurison and Friedman, 2016). Previous studies have shown the effect of the reform has increased the probability of working in non-manual occupations, in particular in the sectors of public administration, health, and education (Lepinteur and Nieto, 2021).

Third, the educational reform increased school-leaving age from 15 to 16 years, which, although an important increase in overall educational level of the population and with documented effects on income, might not have been salient enough for an overall health effect. However, it is the least advantaged young people who could have benefited the most from the additional year in compulsory schooling. Therefore, we expect that the policy reform would have affected the intergenerational life chances of the most disadvantaged families. Moreover, there might be causal effects of offspring’s education at other points of the educational distribution, for example receiving college education compared to having only secondary education. Given our focus on a quasi-natural experiment that raised the mandatory school leaving age from 15 to 16, we could not establish effects at other points in the educational distribution. Madia *et al.* (2022) expands our analyses to include a reform that increased the school-leaving age from 14 to 15 years of age. Reforms in other settings that affected tertiary education would be a tool to identify such effects.

We find no evidence for low-SES parents benefiting more strongly from their children’s education. This is a particularly striking finding, as we would expect to find the strongest effect for low-SES parents. The only other causal study in a resource-rich setting, examining an educational reform in Sweden (Lundborg and Majlesi, 2018), found effects for the health of lower-educated parents only, not for higher-educated parents. However, these effects only materialized for specific gender combinations, namely for fathers of daughters, which we cannot investigate with the data at hand.

Our study is not free from limitations. First, while it would be interesting to study additive effects of children education on parental health, due to the very reduced number of multiple births in the dataset this analysis could not been carried out. Second, the absence of a causal effect has to be understood locally and therefore, we cannot exclude the presence of an effect at other points in the educational distribution. Third, the data in hand did not allow us to make direct tests of potential causal mechanisms linking adult children’s education

and parental longevity, such as learning of improved health behaviors, increased material resources and reduced stress. Finally, parents in the first NCDS analysis are known to have some contact with their children (who respond to the survey at age 45), the second study uses ONS-LS census-linked data which does not assure any direct contact with the children after the birth of child.

The fundamental causes perspective on health inequalities (Clouston and Link, 2021) posits that socio-economic inequalities arise when it is possible for more socio-economically advantaged individuals to obtain health benefits by leveraging resources available to them. In the context of linked lives, the theory expects that the more preventable a disease is, the more responsive it would be to an increase in offspring’s education. However, we compared different causes of death that should be particularly prone to improve with children’s education without finding any effect. This absence of consistent evidence for other causes of death or health indicators limits our ability to draw definitive conclusions on this mechanism.

Therefore, improving the education of younger generations might be a useful policy instrument with intergenerational benefits in the context of aging populations. Such an educational expansion might not just improve the human capital of younger generations, but also have positive spillover effects that improve the health of older generations. Our study is the first to comprehensively test the causal effect of adult children’s education on a comprehensive set of health and mortality outcomes in a high-income country with an aging population. Our results suggest the presence of a significant association between children’s education and parental health. However, neither parental health nor longevity is affected by children’s exogenous increase in education, casting doubts on the intergenerational benefits of human capital investments.

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SUPPLEMENTARY MATERIALS

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A Replication materials and data references

Analyses shown in this manuscript are fully replicable. Program code for the replication is available on-line ([Potente *et al.*, 2022](#)).

Data used in the study are publicly available. The birth cohort data analyzed in this study ([Power and Elliott, 2006](#)) as well as the data used for Figure 2 ([Mindell *et al.*, 2012](#)) are available from the UK Data Service to registered researchers. The linked census data analyzed in this study ([Shelton *et al.*, 2019](#)) are available from the Office of National Statistics (ONS) to accredited researchers after the opening of an approved research project. Further information on how to obtain accredited researcher status is available from the Office of National Statistics.

NCDS ([Power and Elliott, 2006](#)) data sets used in Empirical Analysis 1

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HSE (Mindell *et al.*, 2012) data sets used in Figure 2

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B Question wording for longstanding illness and poor self-rated health in ONS-LS

The census question wording for longstanding illness and poor self-rated health has varied over time, in Table A1 we show the exact wording of the question and response options. Parentheses behind response options show how we coded the responses for analysis.

Table A1: Wording of health questions in the 1991–2011 census forms

Year	Long-term illness	Self-rated health
2011	<p>‘Are your day-to-day activities limited because of a health problem or disability which has lasted, or is expected to last, at least 12 months? Include problems related to old age.’</p> <ul style="list-style-type: none"> • ‘Yes, limited a lot.’ (1) • ‘Yes, limited a little.’ (1) • ‘No.’ (0) 	<p>‘How is your health in general?’</p> <ul style="list-style-type: none"> • ‘Very good.’ (0) • ‘Good.’ (0) • ‘Fair.’ (0) • ‘Bad.’ (1) • ‘Very bad.’ (1)
2001	<p>‘Do you have any long-term illness, health problem, or disability which limits your daily activities or the work you can do. Include problems which are due to old age.’</p> <ul style="list-style-type: none"> • ‘Yes.’ (1) • ‘No.’ (0) 	<p>‘Over the last twelve months would you say your health has on the whole been:’</p> <ul style="list-style-type: none"> • ‘Good’ (0) • ‘Fairly good’ (0) • ‘Not good?’ (1)

Table A1: Wording of health questions in the 1991–2011 census forms (*cont’d*)

1991	<div> <div> Does the person have any long-term illness, health problem, or handicap which limits his/her daily activities or the work he/she can do. Include problems which are due to old age.’ </div> <div> <div> <ul style="list-style-type: none"> • ‘Yes, has a health problem which limits activities.’ (1) • ‘Has no such health problem.’ (0) </div> </div> </div>	(not collected)
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C Tables underlying Figures in the main text

C.1 Empirical Analysis 1: birth cohort

Table A2 shows the full models underlying Figure 1 in the main text.

Table A2: Cox regression for parental survival, hazard ratios				
	Mothers		Fathers	
	(1)	(2)	(1)	(2)
Female child (<i>ref.</i> male)	1.11**	1.11**	1.09**	1.09**
Child born out of wedlock (<i>ref.</i> not)	1.58***	1.57***	1.06	1.06
Age at birth	1.03***	1.03***	1.03***	1.02***
Age child left education (years)	0.94***	0.97**	0.95***	0.98*
Social class parents (<i>ref.</i> I)				
II+IVa		1.01		1.01
III		1.06		1.07
IVb+c		1.02		1.01
V		1.08		1.07
VI		1.41***		1.17*
VII		1.35**		1.31***
Age father left education (<i>ref.</i> 13–15 y.)				
16–17 y.		1.02		0.93
18–19 y.		0.88		0.91
20+ y.		0.90		0.94
Age mother left education (<i>ref.</i> 13–15 y.)				
16–17 y.		0.93		0.91*
18–19 y.		0.89		0.84*
20+ y.		0.84		0.79*
<i>N</i>	7,233	7,233	7,034	7,034
<i>N_{Events}</i>	3,139	3,139	4,602	4,602

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

C.2 Empirical Analysis 2: linked census data

Table A3 shows estimates underlying Figure 4 in the main text.

D Robustness checks

D.1 Empirical Analysis 1: birth cohort

In Table A4 and A5, we show six key robustness checks of our results.

First, we assessed the role of missing values by comparing the results to those obtained from using multiple imputation with chained equations (MICE, [White *et al.*, 2011](#)). We imputed the following variables: mother’s/father’s age at birth with linear regression, child sex and child born out of wedlock with a logit link, education of the father, mother, and child with an ordered logit

Table A3: RD estimates of educational reform stratified by parental sex

		Mortality						Self-reports						
		Death at any age	Death before age 65	Preventable	Lung cancer	Accidents, self harm	Liver disease	Ischaemic heart disease	Mental and behavioral	Long-standing illness 1991	Long-standing illness 2001	Long-standing illness 2011	Poor self-rated health 2001	Poor self-rated health 2011
Women	RD estimate	-0.0140 (0.0134)	-0.00485 (0.00813)	0.00182 (0.00777)	0.00590 (0.00610)	0.00282 (0.00298)	-0.000452 (0.00266)	-0.0167** (0.00784)	0.00641 (0.00556)	-0.00764 (0.0139)	-0.0136 (0.0152)	-0.00495 (0.0173)	-0.0165 (0.0144)	-0.0108 (0.0137)
	<i>N</i>	104,800	104,800	58,375	104,800	104,800	104,800	104,800	104,800	85,422	70,903	49,833	70,908	49,835
	Effective <i>N</i> left	12,038	16,912	12,279	12,038	15,236	15,236	13,582	8,513	9,810	11,287	7,513	9,109	8,110
	Effective <i>N</i> right	14,198	19,688	11,491	14,198	17,679	17,679	15,941	10,449	11,820	14,024	10,293	11,327	11,392
	Bandwidth	7.904	10.94	12.17	7.230	9.419	9.337	8.355	5.442	7.726	10.62	10.17	8.436	11.95
Men	RD estimate	-0.00691 (0.0113)	0.0134 (0.0102)	0.00163 (0.0101)	-0.00454 (0.00806)	-0.000470 (0.00305)	-0.00491* (0.00234)	-0.0109 (0.0108)	0.00606 (0.00415)	0.000184 (0.0166)	0.00789 (0.0192)	0.0337 (0.0302)	-0.0107 (0.0158)	0.0152 (0.0259)
	<i>N</i>	93,691	93,691	68,268	93,691	93,691	93,691	93,691	93,691	66,996	48,529	28,422	48,529	28,422
	Effective <i>N</i> left	13,566	16,492	10,668	12,122	12,122	15,089	15,089	10,695	8,536	7,288	2,424	6,581	2,424
	Effective <i>N</i> right	16,073	19,543	11,295	14,425	14,425	17,814	17,814	12,916	10,649	10,040	3,659	8,983	3,659
	Bandwidth	9.841	11.30	9.698	8.821	8.961	10.41	10.06	7.561	8.501	10.24	6.476	9.755	6,868

Notes: Robust standard errors in parentheses. Models control for parental education, occupational class, number of children, and year of birth. + $p < 0.10$, * $p < 0.05$. Source: ONS-LS.

link, and parental social class with a multinomial logit link. We use as auxiliary variables the indicator dummy for the death of the father and the mother plus the Nelson–Aalen estimate of the cumulative hazards for fathers and mothers, as suggested by [White and Royston \(2009\)](#). For determining m , the number of imputations, we made use of [Von Hippel’s \(2020\)](#) algorithm, which suggested between 3 and 37 imputations for our models. We used 37 imputations for every model. We ran models on the imputed data and combined the resulting estimators using the rules stated by [Rubin \(1987, ch. 3\)](#).

Second, we restricted the sample restricted to birth cohort members who were lived in England and Wales at age 16 (excluding those living in Scotland) in order to make it comparable to the second part of the analysis which focuses on a schooling reform that only took place in England and Wales. In all cases, the magnitude of the coefficients changes slightly but the direction of the associations stays the same.

Third, we assessed the proportional hazard assumption using Schoenfeld residuals, which suggested that the assumption might be violated for the adult children’s education variable. Substantively, this means that the association of children’s education and parental longevity varies over time and that the associations shown in Figure 1 show a rough average of the strength of the association over time. Models including the interaction of children’s education with time however lead to the same substantive conclusion of an association between children’s education and parental longevity, which is attenuated when accounting for parental class and education.

Fourth, we further test the robustness of our results using a parametric acceleration failure time survival model following a Weibull distribution. This model offers an alternative to the Cox model by assuming that the effect of the covariates is proportional to the survival time (and not to the hazard as in the Cox model). Using this specification the time ratios associated with an additional year of children’s education shows an increase in survival by 1.4 % for mothers and by 0.8 % for fathers in the full covariates model.

Fifth, we test the robustness of the results to families with more than one offspring by including an indicator of whether the respondent is single child or not at age 16. Unfortunately, the data do not contain the educational level for the other siblings and therefore we could not perform additional robustness checks on different operationalizations for the educational level of the siblings. The results presented in Figure A1 shows a similar conclusion as in the main analysis.

Finally, we include a robustness test of the results using categorical education variable to account for a possible non-linear correlation. The results, shown in Figure A2, yield a similar conclusion to the linear analysis.

Table A4: Cox regression for parental survival, hazard ratios.

	Mothers			Fathers		
	Multiple imputation		England & Wales only	Multiple imputation		England & Wales only
	(1)	(2)	(1)	(2)	(1)	(2)
Main effects						
Female child (<i>ref.</i> male)	1.11***	1.11***	1.11**	1.06*	1.09**	1.09**
Child born out of wedlock (<i>ref.</i> not)	1.45***	1.43***	1.57***	1.14	1.03	1.02
Age at birth	1.04***	1.03***	1.03***	1.03***	1.03***	1.03***
Age child left education (years)	0.94***	0.98**	0.93***	0.96***	0.95***	0.98*
Social class parents (<i>ref.</i> I)						
II+IVa		0.95				0.97
III		1.03				1.06
IVb+c		0.97				0.97
V		1.05				1.04
VI		1.35**				1.14
VII		1.30**				1.29**
Age father left education (<i>ref.</i> 13–15 y.)						
16–17 y.		1.00				0.92*
18–19 y.		0.88				0.90
20+ y.		0.87				0.91
Age mother left education (<i>ref.</i> 13–15 y.)						
16–17 y.		0.93				0.92*
18–19 y.		0.90				0.87*
20+ y.		0.86				0.81*
<i>N</i>	10,958	10,958	6,661	10,749	6,454	6,454
<i>N_{Events}</i>	4,810	4,810	2,857	7,040	4,217	4,217
Deviance			46784		68447	68358.4
No. of imputations (<i>m</i>)	37	37		37	37	

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Source: NCDS (Power and Elliott, 2006), own calculations.

Table A5: Cox regression for parental survival using age as time-varying covariate (hazard ratios displayed) and using accelerated failure time model (time ratios displayed).

	Mothers				Fathers			
	Time-varying covariate		Accelerated Failure Time		Time-varying covariate		Accelerated Failure Time	
	(1)	(2)	(1)	(2)	(1)	(2)	(1)	(2)
Main effects								
Female child (<i>ref.</i> male)	1.11**	1.11**	0.96**	0.96**	1.09**	1.09**	0.96**	0.96**
Child born out of wedlock (<i>ref.</i> not)	1.58***	1.57***	0.82***	0.83***	1.04	1.04	0.99	0.99
Age at birth	1.03***	1.03***	0.98***	0.98***	1.03***	1.03***	0.99***	0.99***
Age child left education (years)	0.87***	0.90***	1.03***	1.01**	0.85***	0.87***	1.02***	1.01*
Social class parents (<i>ref.</i> I)								
II+IVa		1.01		1.00		1.01		0.99
III		1.06		0.98		1.07		0.97
IVb+c		1.01		1.00		1.00		0.99
V		1.09		0.97		1.08		0.97
VI		1.40***		0.87***		1.17		0.93*
VII		1.34**		0.89**		1.30***		0.89***
Age father left education (<i>ref.</i> 13–15 y.)								
16–17 y.		1.02		0.99		0.93		1.03
18–19 y.		0.88		1.05		0.91		1.04
20+ y.		0.90		1.04		0.93		1.03
Age mother left education (<i>ref.</i> 13–15 y.)								
16–17 y.		0.92		1.03		0.91**		1.04**
18–19 y.		0.89		1.05		0.83**		1.07*
20+ y.		0.82		1.08		0.77*		1.11*
Interactions with time								
Age child left education (years)	1.00*	1.00**			1.00***	1.00***		
ln_p			2.31***	2.32***			2.27***	2.28***
N	7,195	7,195	7,195	7,195	6,989	6,989	6,989	6,989
N_{Events}	3,101	3,101	3,101	3,101	4,557	4,557	4,557	4,557
Deviance								

Notes: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Note: * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$. Source: NCDS (Power and Elliott, 2006), own calculations.

Figure A1: Cox proportional hazard models for the association between children's education and parental mortality including binary variable indicating whether the child is single child or not.

Notes: Error bars denote 95% confidence interval based on robust standard errors. Controls comprise parental education, number of children, and year of birth. *Source:* NCDS (Power and Elliott, 2006), own calculations.

D.2 Empirical Analysis 2: linked census data

Occupational class, education, and area deprivation as stratifying variables Figures ?? and A4 as well as Tables A6, A8, and A9 show estimates stratified by parental occupational class, parental education, and by level of area deprivation.

Parental occupational class is indicated by the registrar-general's class scheme (Connelly et al. 2016), comprising classes I/II ("Professional occupations" and "Managerial and technical occupations"), III Non-manual ("Skilled non-manual occupations"), III Manual ("Skilled manual occupation"), IV and V ("Partly-skilled occupations" and "Unskilled occupations"). Class was derived from respondent's occupation in 1971. Women married at the time of the census have been assigned the occupational class of their husband if a husband was present, otherwise their own occupational class.

Parental education is taken from the 1971 census form, which has two questions on education, one question asking whether a respondent has the following degrees: 1) GCE A-level or Higher School Certificate (HSC), 2) Higher grade of Scottish Certificate of Education (SCE) or Higher grade of Scottish Leaving Certificate (SLC), 3) Ordinary National Certificate (ONC) or Ordinary National Diploma (OND), or 4) None of these. The second question requests to list any of the following qualifications: Higher National Certificate (HNC) or Higher National Diploma (HND), Nursing qualifications, Teaching qualifications, Degrees, Graduate or corporate membership of professional institutions, and Any other

Figure A2: Cox proportional hazard models for the association between children's education and parental mortality using categorical specification of children's education.

Notes: Error bars denote 95% confidence interval based on robust standard errors. Controls comprise parental education, number of children, and year of birth. *Source:* NCDS (Power and Elliott, 2006), own calculations.

Figure A3: RD estimates of educational reform stratified by parental sex and parental occupational class

Notes: Error bars denote 95% confidence interval based on robust standard errors. Controls comprise parental education, number of children, and year of birth. Estimates shown in Table A6. *Source:* ONS-LS (Shelton *et al.*, 2019), own calculations.

Figure A4: RD estimates of educational reform stratified by parental sex and parental education and area deprivation

Notes: Error bars denote 95% confidence interval based on robust standard errors. Controls comprise parental education (only for area deprivation model), occupational class, number of children and year of birth. Estimates shown in Table A8 and A9. *Source:* ONS-LS (Shelton *et al.*, 2019), own calculations.

Table A6: RD estimates of educational reform stratified by parental sex and occupational class

		Mortality										Self-reports			
		Death at any age	Death before age 65	Preventable	Lung cancer	Accidents, self harm	Liver disease	Ischaemic heart disease	Mental and behavioral	Long-standing illness 1991	Long-standing illness 2001	Long-standing illness 2011	Poor self-rated health 2001	Poor self-rated health 2011	
IV/V women	RD estimate	-0.0325 (0.0240)	-0.0492* (0.0202)	-0.0113 (0.0187)	-0.0136 (0.0132)	0.00442 (0.00751)	-0.00301 (0.00586)	0.0171 (0.0170)	0.0169 (0.0106)	0.0259 (0.0344)	-0.0752+ (0.0397)	-0.0305 (0.0347)	-0.0632+ (0.0357)	-0.00488 (0.0352)	
	N	23,720	23,720	14,486	23,720	23,720	23,720	23,720	23,720	18,724	14,771	9,976	14,771	9,978	
	Effective N left	3,532	3,532	2,590	3,928	3,928	3,928	3,532	2,759	1,867	1,676	1,725	1,894	1,490	
	Effective N right	3,938	3,938	2,405	4,341	4,341	4,341	3,938	3,196	2,292	2,131	2,456	2,396	2,061	
	Bandwidth	9.271	9.404	10.11	10.27	10.41	10.35	9.247	7.033	6.804	7.805	12.01	8.386	10.32	
I/II women	RD estimate	0.0200 (0.0276)	0.00993 (0.0171)	0.0266 (0.0256)	0.0134 (0.0110)	0.000249 (0.00623)	0.00129 (0.00499)	-0.0238* (0.0108)	0.00708 (0.0102)	0.0205 (0.0219)	-0.0165 (0.0296)	0.00388 (0.0434)	-0.0317 (0.0265)	0.0104 (0.0332)	
	N	23,783	23,783	12,000	23,783	23,783	23,783	23,783	23,783	19,907	17,302	12,694	17,307	12,694	
	Effective N left	3,083	3,453	1,433	3,453	3,817	4,461	3,453	2,754	2,905	2,817	1,296	2,053	1,296	
	Effective N right	3,725	4,121	1,541	4,121	4,594	5,468	4,121	3,293	3,484	3,476	1,594	2,461	1,594	
	Bandwidth	8.626	9.613	7.002	9.117	10.44	12.71	9.961	7.058	9.325	10.02	6.129	7.399	6.597	
IV/V men	RD estimate	0.0142 (0.0191)	0.0357 (0.0264)	0.0175 (0.0257)	0.00517 (0.0147)	-0.00649 (0.00577)	-0.00661 (0.00414)	0.0256 (0.0217)	0.00946+ (0.00566)	0.0381 (0.0412)	0.0183 (0.0535)	0.0114 (0.0502)	0.0103 (0.0415)	0.0250 (0.0577)	
	N	21,733	21,733	16,963	21,733	21,733	21,733	21,733	21,733	14,338	9,617	5,243	9,617	5,243	
	Effective N left	4,262	2,881	1,793	3,934	3,592	3,934	3,934	1,830	1,658	1,055	726	1,204	582	
	Effective N right	4,811	3,319	2,018	4,431	4,051	4,431	4,431	2,229	2,065	1,430	1,114	1,608	896	
	Bandwidth	12.64	8.095	6.806	11.30	10.03	11.10	11.81	5.847	7.478	7.392	10.79	8.245	8.955	
I/II men	RD estimate	-0.00417 (0.0262)	0.000408 (0.0217)	0.0148 (0.0214)	-0.00259 (0.0123)	0.00404 (0.00571)	-0.00081+ (0.00547)	-0.00000823 (0.0251)	-0.00457 (0.00725)	-0.00144 (0.0272)	-0.0254 (0.0434)	0.0448 (0.0468)	-0.0143 (0.0332)	0.0342 (0.0327)	
	N	22,889	22,889	15,620	22,889	22,889	22,889	22,889	22,889	17,350	13,297	8,255	13,297	8,255	
	Effective N left	2,924	2,924	2,207	4,004	3,262	3,659	2,586	3,659	2,407	1,424	975	1,007	881	
	Effective N right	3,565	3,565	2,354	4,870	3,965	4,449	3,192	4,449	3,115	1,962	1,592	1,412	1,410	
	Bandwidth	8.008	8.805	8.105	11.68	9.915	10.47	7.380	10.88	9.774	7.118	9.993	5.688	8.208	

Notes: Robust standard errors in parentheses. Models control for parental education, number of children, and year of birth. + $p < 0.10$, * $p < 0.05$. Source: ONS-LS.

professional or vocational qualifications. For our analyses, we count affirmative responses to option 4) in the first question as being lower educated (which applies to ca. 90 per cent of the sample) and any other response as being higher educated.

In addition, we stratify the analyses by quintiles of the Carstairs index of deprivation (Morris and Carstairs, 1991), comparing the two lowest quintiles with the top-two quintiles (Norman and Boyle, 2014). The index is a well-established geographical measure of material deprivation and is based on four variables, namely regional unemployment, overcrowded households, households without cars, and households with a household head in a low occupational class. The Carstairs Index has previously been used in conjunction with the ONS-LS (Boyle *et al.*, 2004; Norman and Boyle, 2014; Norman *et al.*, 2005). The index is calculated at the ward level, which on average comprise 5,500 residents, and of which there are 7,707 in England and 881 in Wales. We measure the Carstairs index at the latest census for sample members who are alive at the end of the follow-up period, otherwise at the census closest to their time of death.

Among low-educated women and women in deprived areas, we do not find the same health benefits of more educated children that we observed for occupational class. A comparison of findings for occupational class with those for parental education and area deprivation, reveals that our findings are sensitive to the chosen measure of socio-economic position (Figures ?? and A4). Figure A4 contains an unexpected result. Children’s education reduces the risk of dying from ischaemic heart disease only for lower-educated women (−1.9%, −3.5%–−0.2%), but children’s education increases the risk of higher-educated women to report poor self-rated health in 2011 (9.1%, 1.8–16.5%).

The right-hand panels of Figure A4 show results by area deprivation, suggesting that children’s education reduces the risk of ischaemic heart disease (−3.4%, −6.4%–−0.5%) of women living in non-deprived areas (which is in line with the finding for occupational class but not education). For women in deprived areas children’s education increases the risk of dying from lung cancer (1.6%, 0.1%–3.2%). For men, stratifying by area deprivation yields two other findings: Educated children reduce the risk of men in deprived areas to report poor self-rated health in 2001 (−5.8%, −10.5%–−1.1%) and reduce the risk of dying from lung cancer in non-deprived areas (−2.5%, −5.2%–0.2%).

Number of children as stratifying variable Table A7 presents models stratified by the number of children parents have. The rationale for this robustness check is that effects might be especially pronounced among parents with only one child affected by the reform rather than among parents with more children, some of whom will not be affected by the reform. This robustness check did not reveal any significant effects, which is in line with our overall findings.

Table A7: RD estimates of educational reform stratified by sex and number of children

		Death at any age			Death before age 65		
		Singleton	2 children	3+ children	Singleton	2 children	3+ children
Women	RD estimate	-0.0262 (0.0387)	-0.00259 (0.0235)	-0.00999 (0.0122)	0.0122 (0.0314)	-0.00701 (0.0136)	-0.00621 (0.00911)
	N	8,516	31,514	64,770	8,516	31,514	64,770
	Effective N left	1,197	3,905	12,583	1,197	4,881	12,583
	Effective N right	1,270	4,509	15,045	1,270	5,658	15,045
	Bandwidth	9.980	8.079	12.63	9.056	10.53	12.20
Men	RD estimate	0.0270 (0.0340)	-0.00226 (0.0176)	-0.0217+ (0.0127)	0.0344 (0.0387)	0.00804 (0.0181)	0.0143 (0.0126)
	N	7,327	28,743	57,621	7,327	28,743	57,621
	Effective N left	1,321	4,770	9,529	950	4,359	10,401
	Effective N right	1,351	5,788	11,321	1,020	5,264	12,404
	Bandwidth	11.90	11.94	10.39	8.066	10.31	11.91

Notes: Robust standard errors in parentheses. Models control for parental education, occupational class, and year of birth. + $p < 0.10$, * $p < 0.05$. *Source:* ONS-LS, (Shelton *et al.*, 2019), own calculations.

Different bandwidths Table A10 investigates the effects of choosing different bandwidths around the threshold date. The results remain the same when different bandwidths are selected, demonstrating low sensitivity to the bandwidth chosen.

Power analysis In order to check we have sufficient statistical power for our analyses, we report the regression discontinuity power analysis implemented following Cattaneo *et al.* (2019). The estimated power analysis uses the optimal bandwidth, local parametric regression, bias-correction, and robust standard errors specified by Calonico *et al.* (2014). Table A11 provides the power estimates for $T = 1$ which represents effects equal to half the standard deviation of the outcome for the untreated with a statistical significance level of 0.05. From Table A11, we observe that the power levels are all 1.00 across all of the outcomes for a medium-size effect. These results are greater than the common power threshold of 0.80. Moreover, we run additional analysis to examine the power even for smaller effect sizes. From Tables A12 and A13 we observe that our analyses are able to detect smaller effects such as $T = 0.5$ (0.25σ) and $T = 0.8$ (0.4σ) with a high level of confidence. Only with $T = 0.2$ (0.1σ) we observe that some outcome could be slightly underpowered, but this represents extremely small effects. All the power analyses are implemented in Stata using the package ‘rdpower’ (Cattaneo *et al.*, 2019).

Table A8: RD estimates of educational reform stratified by parental sex, and parental education

Mortality														Self-reports			
	Death at any age	Death before age 65	Preventable	Lung cancer	Accidents, self harm	Liver disease	Ischaemic heart disease	Mental and behavioral	Long-standing illness 1991	Long-standing illness 2001	Long-standing illness 2011	Poor self-rated health 2001	Poor self-rated health 2011				
Lower educated women	RD estimate	-0.0163 (0.0142)	-0.00194 (0.00810)	0.00195 (0.00810)	0.00763 (0.00651)	0.00372 (0.00303)	-0.000172 (0.00242)	-0.0162* (0.00818)	0.00773 (0.00580)	-0.0158 (0.0147)	-0.00850 (0.0177)	-0.0163 (0.0152)	-0.0166 (0.0147)				
	N	95,175	95,175	53,810	95,175	95,175	95,175	95,175	95,175	77,435	63,948	63,953	44,645				
	Effective N left	10,916	15,379	11,309	10,916	13,839	16,774	12,321	7,688	8,881	10,179	8,192	7,827				
	Effective N right	12,798	17,750	10,487	12,798	15,942	19,497	14,361	9,436	10,644	12,565	9,180	10,148				
	Bandwidth	7.739	10.61	12.28	7.317	9.841	11.44	8.728	5.583	7.970	10.13	10.79	8.606	12.25			
Higher educated women	RD estimate	0.0157 (0.0454)	-0.0358 (0.0296)	-0.00311 (0.0296)	-0.0114 (0.0136)	-0.00837 (0.00960)	-0.00548 (0.0139)	-0.0232 (0.0226)	0.000266 (0.0112)	0.0774+ (0.0398)	0.00740 (0.0533)	-0.0105 (0.0387)	0.0940* (0.0376)				
	N	9,625	9,625	4,565	9,625	9,625	9,625	9,625	9,625	7,987	6,955	6,955	5,190				
	Effective N left	1,122	1,261	744	1,261	1,533	1,122	1,261	1,533	929	822	661	592				
	Effective N right	1,400	1,580	783	1,580	1,938	1,400	1,580	1,938	1,176	1,037	776	776				
	Bandwidth	7.763	8.403	9.295	8.754	10.98	7.332	8.030	10.83	7.061	7.965	8.473	9.000	7.302			
Lower educated men	RD estimate	-0.00881 (0.0107)	0.0164 (0.0111)	-0.00213 (0.0104)	-0.00661 (0.00909)	-0.00205 (0.00310)	-0.00403+ (0.00228)	-0.0146 (0.0110)	0.00721 (0.00447)	-0.00801 (0.0183)	0.000550 (0.0272)	0.0414 (0.0359)	-0.0228 (0.0196)	0.00428 (0.0309)			
	N	80,388	80,388	59,860	80,388	80,388	80,388	80,388	80,388	56,658	40,357	23,087	40,357	23,087			
	Effective N left	15,515	14,284	10,459	10,533	11,782	15,515	14,284	9,286	7,319	3,800	1,706	4,994	2,024			
	Effective N right	17,955	16,553	10,735	12,278	13,671	17,955	16,553	11,004	8,949	5,120	2,508	6,641	2,985			
	Bandwidth	12.18	11.63	10.85	8.714	9.726	12.13	11.38	7.819	8.669	6.688	5.668	8.196	6.607			
Higher educated men	RD estimate	0.0133 (0.0383)	-0.00155 (0.0303)	0.0184 (0.0249)	0.00679 (0.0167)	0.00643 (0.00797)	-0.00769 (0.00712)	0.0540 (0.0392)	-0.00129 (0.00955)	0.0686 (0.0436)	0.00417 (0.0542)	0.0130 (0.0572)	0.0249 (0.0344)	0.0585 (0.0362)			
	N	13,303	13,303	8,408	13,303	13,303	13,303	13,303	13,303	10,338	8,172	5,335	8,172	5,335			
	Effective N left	1,409	1,227	1,267	1,409	2,017	1,784	810	1,784	940	813	655	911	655			
	Effective N right	1,912	1,659	1,496	1,912	2,716	2,402	1,158	2,402	1,296	1,206	1,185	1,374	1,185			
	Bandwidth	7.038	6.448	9.306	7.578	10.45	9.357	4.523	9.777	6.330	7.043	10.37	8.726	10.04			

Notes: Robust standard errors in parentheses. Models control for occupational class, number of children, and year of birth. + $p < 0.10$, * $p < 0.05$. Source: ONS-LS (Shelton *et al.*, 2019), own calculations.

Table A9: RD estimates of educational reform stratified by parental sex and area deprivation

Mortality														Self-reports			
		Death at any age	Death before age 65	Preventable	Lung cancer	Accidents, self harm	Liver disease	Ischaemic heart disease	Mental and behavioural	Long-standing illness 1991	Long-standing illness 2001	Long-standing illness 2011	Poor self-rated health 2001	Poor self-rated health 2011			
Women deprived areas	RD estimate	0.00439 (0.0187)	0.0153 (0.0164)	-0.00305 (0.0140)	0.0164* (0.00775)	-0.00471 (0.00463)	0.00451 (0.00438)	-0.0171 (0.0110)	0.00832 (0.00873)	0.0112 (0.0197)	-0.000733 (0.0256)	-0.0182 (0.0289)	-0.0144 (0.0215)	-0.0229 (0.0205)			
	N	35,456	35,456	18,340	35,456	35,456	35,456	35,456	35,456	31,380	27,724	21,686	27,724	21,686			
	Effective N left	6,168	4,023	2,898	4,533	5,101	5,685	5,101	3,479	3,975	3,946	2,642	3,146	3,256			
	Effective N right	7,340	4,759	2,769	5,380	5,960	6,656	5,960	4,093	4,835	4,850	3,555	3,852	4,429			
	Bandwidth	11.66	7.465	9.542	8.722	9.938	10.03	9.347	6.712	8.756	9.151	8.610	7.852	10.97			
Women non-deprived areas	RD estimate	-0.00298 (0.0224)	-0.0117 (0.0173)	0.0106 (0.0166)	0.0146 (0.0122)	-0.00944 (0.00658)	-0.00320 (0.00415)	-0.0343* (0.0152)	0.00647 (0.00748)	-0.0273 (0.0274)	-0.00962 (0.0274)	0.0239 (0.0333)	-0.0128 (0.0235)	-0.00536 (0.0297)			
	N	35,463	35,463	22,334	35,463	35,463	35,463	35,463	35,463	29,962	24,083	17,230	24,088	17,232			
	Effective N left	4,119	4,668	3,081	3,520	4,668	6,336	5,260	4,119	2,954	3,851	1,903	4,209	2,385			
	Effective N right	4,823	5,368	3,158	4,241	5,368	7,272	5,965	4,823	3,687	4,800	2,536	5,278	3,211			
	Bandwidth	7.415	8.622	8.886	6.883	8.078	11.13	9.135	7.155	6.927	10.01	7.521	11.01	9.085			
Men deprived areas	RD estimate	0.00811 (0.0227)	0.0199 (0.0217)	0.00530 (0.0173)	0.0140 (0.0141)	0.00105 (0.00655)	-0.00772+ (0.00423)	-0.0123 (0.0214)	0.00237 (0.00552)	-0.0278 (0.0224)	-0.0286 (0.0295)	0.0310 (0.0415)	-0.0575* (0.0243)	0.0270 (0.0257)			
	N	32,310	32,310	22,803	32,310	32,310	32,310	32,310	32,310	25,466	19,892	13,212	19,892	13,212			
	Effective N left	3,595	3,124	3,121	3,595	3,124	4,570	3,595	4,570	4,283	2,964	1,290	2,388	1,775			
	Effective N right	4,512	3,915	3,449	4,512	3,915	5,583	4,512	5,583	5,636	4,151	2,009	3,301	2,889			
	Bandwidth	7.555	6.605	8.907	7.405	6.489	9.026	7.051	9.346	11.00	10.25	7.431	8.729	10.73			
Men non-deprived areas	RD estimate	-0.0111 (0.0183)	0.0336 (0.0223)	0.000583 (0.0191)	-0.0247+ (0.0139)	0.00877 (0.00542)	-0.00881 (0.00713)	0.0185 (0.0231)	0.00543 (0.00565)	0.0363 (0.0295)	0.0587+ (0.0345)	-0.0149 (0.0422)	0.0225 (0.0291)	0.0376 (0.0479)			
	N	32,104	32,104	25,927	32,104	32,104	32,104	32,104	32,104	23,063	16,027	9,046	16,027	9,046			
	Effective N left	3,801	4,314	3,660	4,817	3,241	3,241	3,801	4,817	3,050	2,251	1,158	2,477	931			
	Effective N right	4,323	4,808	3,836	5,390	3,784	3,784	4,323	5,390	3,580	2,912	1,664	3,258	1,283			
	Bandwidth	7.766	8.668	8.142	9.291	6.323	6.011	7.617	9.674	8.513	9.971	9.733	10.52	7.367			

Notes: Robust standard errors in parentheses. Models control for parental education, occupational class, number of children, and year of birth. + $p < 0.10$, * $p < 0.05$. Source: ONS-LS, (Shelton *et al.*, 2019), own calculations.

Table A10: RD estimates of educational reform stratified by sex for different bandwidths

	Bandwidth 4		Bandwidth 8		Bandwidth 12	
	RD estimate	SE	RD estimate	SE	RD estimate	SE
<i>Women</i>						
Death at any age	0.0074	(0.0214)	-0.0142	(0.0134)	-0.0109	(0.0108)
Death before age 65	0.0105	(0.0158)	-0.0029	(0.00965)	-0.0049	(0.00775)
Lung cancer	0.0105	(0.0094)	0.0052	(0.0058)	0.0055	(0.0047)
Accidents, self harm	0.0030	(0.0053)	0.0036	(0.0033)	0.0018	(0.0026)
Liver disease	0.0055	(0.0048)	-0.0001	(0.0029)	-0.0004	(0.0023)
Ischaemic heart disease	-0.0167	(0.0132)	-0.0166*	(0.0081)	-0.0163*	(0.0064)
Mental and behavioral	0.0085	(0.0072)	0.0056	(0.0046)	0.0007	(0.0038)
Preventable	-0.0171	(0.0165)	-0.0004	(0.0100)	0.0018	(0.0079)
Long-standing illness 1991	0.0057	(0.0220)	-0.0078	(0.0138)	-0.0119	(0.0111)
Long-standing illness 2001	0.0208	(0.0283)	-0.0093	(0.0177)	-0.0141	(0.0143)
Long-standing illness 2011	0.0074	(0.0320)	-0.0007	(0.0197)	-0.0050	(0.0159)
Poor self-rated health 2001	-0.0052	(0.0239)	-0.0176	(0.0149)	-0.0144	(0.0121)
Poor self-rated health 2011	0.0085	(0.0274)	-0.0084	(0.0170)	-0.0107	(0.0137)
<i>Men</i>						
Death at any age	-0.0209	(0.0205)	-0.0067	(0.0127)	-0.0057	(0.0101)
Death before age 65	0.0046	(0.0195)	0.0150	(0.0123)	0.0134	(0.0099)
Lung cancer	-0.0086	(0.0139)	-0.0053	(0.0085)	-0.0067	(0.0068)
Accidents, self harm	0.0013	(0.0053)	0.0001	(0.0032)	-0.0009	(0.0026)
Liver disease	-0.0071	(0.0052)	-0.0048+	(0.0028)	-0.0045*	(0.0022)
Ischaemic heart disease	-0.0106	(0.0200)	-0.0067	(0.0123)	-0.0131	(0.0098)
Mental and behavioral	0.0106+	(0.0063)	0.0061	(0.0041)	0.0051	(0.0032)
Preventable	0.0345*	(0.0174)	0.0041	(0.0111)	-0.0017	(0.0091)
Long-standing illness 1991	-0.0340	(0.0280)	-0.0015	(0.0174)	0.0001	(0.0138)
Long-standing illness 2001	-0.0466	(0.0359)	0.0037	(0.0222)	0.0079	(0.0177)
Long-standing illness 2011	0.0572	(0.0436)	0.0257	(0.0269)	0.0127	(0.0217)
Poor self-rated health 2001	-0.0068	(0.0284)	-0.0164	(0.0177)	-0.0080	(0.0142)
Poor self-rated health 2011	0.0043	(0.0391)	0.0195	(0.0236)	0.0120	(0.0188)

Notes: Robust standard errors in parentheses. Models control for parental education, occupational class, and year of birth. + $p < 0.10$, * $p < 0.05$. *Source:* ONS-LS, (Shelton *et al.*, 2019), own calculations.

Table A11: RDD power estimates with $T = 1$ (i.e. effects equal to half the standard deviation of the outcome for the untreated)

	Tau=1	Female	Male
Death at any age	1	1	
Death before age 65	1	1	
Lung cancer	1	1	
Accident, self-harm	1	1	
Liver disease	1	1	
Ischaemic heart diseases	1	1	
Mental and behavioural	1	1	
Preventable	1	1	
Long-standing illness 1991	1	1	
Long-standing illness 2001	1	1	
Long-standing illness 2011	1	1	
Poor self-rated health 2001	1	1	
Poor self-rated health 2011	1	1	

Source: ONS-LS, (Shelton *et al.*, 2019), own calculations.

Table A12: RDD power estimates at varying effect sizes from ‘rdpower’ in Stata for females

	40% of an <i>SD</i>	25% of an <i>SD</i>	10% of an <i>SD</i>
Death at any age	1	1	0.85
Death before age 65	1	1	0.92
Lung cancer	1	1	0.77
Accident, self-harm	1	1	0.79
Liver disease	1	1	0.65
Ischaemic heart diseases	1	1	0.78
Mental and behavioural	1	1	0.74
Preventable	1	1	0.66
Long-standing illness 1991	1	1	0.72
Long-standing illness 2001	1	1	0.75
Long-standing illness 2011	1	1	0.52
Poor self-rated health 2001	1	1	0.63
Poor self-rated health 2011	1	1	0.66

Source: ONS-LS, (Shelton *et al.*, 2019), own calculations.

Table A13: RDD power estimates at varying effect sizes from ‘rdpower’ in Stata for males

	40% of an <i>SD</i>	25% of an <i>SD</i>	10% of an <i>SD</i>
Death at any age	1	1	0.83
Death before age 65	1	1	0.87
Lung cancer	1	1	0.8
Accident, self-harm	1	1	0.85
Liver disease	1	1	0.77
Ischaemic heart diseases	1	1	0.83
Mental and behavioural	1	1	0.69
Preventable	1	1	0.71
Long-standing illness 1991	1	1	0.59
Long-standing illness 2001	1	1	0.49
Long-standing illness 2011	1	0.89	0.25
Poor self-rated health 2001	1	1	0.51
Poor self-rated health 2011	1	0.89	0.25

Source: ONS-LS, (Shelton *et al.*, 2019), own calculations.