Adult Children's Education and Parental Health and Mortality in England and Wales. A Regression Discontinuity Approach*

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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. Previous research has been largely associational and was thus not able to account for all circumstances that confound children's education and parental longevity. 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 effect of children's education on parental health and longevity. Our intent-to-treat estimates reveal that children's education affects a wide range of outcomes related to parental mortality and health reports only to a limited extent. Effect sizes are small, thus rarely reaching conventional levels of statistical significance. We interpret these findings against the backdrop of universal and free health care and the role of education in socioeconomic inequality in England and Wales.

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Introduction

Education is an important determinant of health. Those with better education enjoy better health (Galama *et al.*, 2018; Präg and Subramanian, 2017) themselves, and the benefits of education 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 moved into the center of 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). Yet, the spillovers from children to parents are understudied, as a recent review has demonstrated (De Neve and Kawachi, 2017).

The aim of our study is to explore the causal role of adult children's education for parental health and mortality in England and Wales. A unique, high-quality representative sample of the English and Welsh population linked to official mortality statistics is the source of our data. We exploit a quasi-natural experiment using an educational reform in 1972 which has raised the age at school leaving from 15 to 16. Children's education is correlated with a number of different parental health outcomes: parents of higher educated children are less depressed (Lee *et al.*, 2017; Sabater and Graham, 2016b), suffer less from functional limitations (Yahirun *et al.*, 2016, 2017; Zimmer *et al.*, 2002), less from inflammation (Lee, 2018), and live longer (Elo *et al.*, 2018; Friedman and Mare, 2014; Sabater and Graham, 2016a; Torssander, 2013, 2014; Yahirun *et al.*, 2017; Yang *et al.*, 2016). This evidence 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 were also 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.

Improving the education of younger generations might serve as a public health intervention that affects not just younger, but also older generations, and could prove to be a crucial policy instrument in contexts where older populations are growing without precedence. 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—a 'social foreground' (Torssander, 2013) for parents—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.

Causality is the crucial issue for the analysis of educational spillover effects, as the association between children's education and parental longevity might be driven by endowments of the parental family, either socioeconomic 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. Friedman and Mare (2014) are for instance able to control for parental income, wealth, and both parents' education, but many studies have only few control variables for the socioeconomic 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 in how far 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).

Therefore, an association between offspring's education and parental health might arise because of the presence of unobserved confounding variables resulting in a spurious association. Therefore, determining causation is a major problem. For example, a confounding pathway that would link offspring education to parental longevity would be a genetic one, and indeed, genetic variants predicting both education and longevity have recently been identified (Krapohl *et al.*, 2014; Marioni *et al.*, 2016). Marioni *et al.* (2016) showed that parents of children with a strong genetic disposition for educational attainment lived half a year longer than parents of children with a weak disposition.

In summary, our study is a relevant contribution to the study of 'social foreground' in two ways. First, it explores for the first time the causal effect of offspring's education on parental health and mortality in England and Wales, a context with limited public welfare provision yet a universal health care system. The high-quality data from linked censuses provide a representative picture of these effects and the large sample size allows obtaining confident estimates of the effects. Second, we employ a regression discontinuity approach to identify the causal effect of children's education on parental health. Existing studies with a quasi-experimental approach have relied on instrumental variable (IV) approaches to estimate the effect. However, IV methods in quasi-experimental settings can be sensitive to cohort trend and small changes are less detectable from secular changes (Galama *et al.*, 2018). Our regression discontinuity design allows us to compare individuals whose children were born just before and just after the policy reform and thus obtain an unbiased causal estimate of the effect of offspring's education on parental health.

Background

So far, only three studies rely on exogenous variation to identify a causal effect of adult children's education on parental health. De Neve and Fink (2018), Lundborg and Majlesi (2018), and Ma (2018) rely on schooling reforms to instrument the parental health effects of an increase in children's education. De Neve and Fink (2018) show that 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. Ma (2018) shows that a 1980's educational reform in China which implemented nine-year compulsory education lead to improvements in parental cognitive function and lung function, but not grip strength, self-reported health, or depression. The only study in a resource-rich setting is Lundborg and Majlesi (2018), who do not find any overall effect in Sweden. Only daughter's education has positive effects for fathers, particularly low-educated fathers; establishing that 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 school 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. Our work looks at a compulsory school reform in England and Wales, where there were no other changes next to raising the schooling leaving age. Moreover, the IV strategy could be particularly problematic in the presence of strong time trend effect. Our study employs for the first time a regression discontinuity design in order to examine the causal relationship between offspring's education and parental health.

England and Wales are 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 Harling, 2017; De Neve and Fink, 2018), 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), 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), 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, but 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 50 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 more healthy than the US population when looking at biomarkers and self-reported diseases (Banks *et al.*, 2006; Präg and Subramanian, 2017).

The 1972 educational reform in England and Wales has been frequently exploited in earlier research. 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) also 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 (Clark and Royer, 2013; Silles, 2009). 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.

A number of causal pathways could link adult children's education to parental health. Firstly, better-educated children can also induce better health behaviors in their parents, which lead to greater longevity. For instance, Friedman and Mare (2014) show 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 particularly strongly linked to smoking. 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).

Secondly, better educated children are able to generate higher earnings, 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 (2018) 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, were access to healthcare 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 England and Wales 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 preferred 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. 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.

The effect of children on their parents may differ by parental characteristics. We focus on socioeconomic differences in parental socio-economic status (both education and social class): Do lower SES parents benefit more from having better-educated children? Particularly in resource-rich settings such as England and Wales, this might be the case. In their study on the Swedish population, Lundborg and Majlesi (2018) documented stronger effects of offsprings' education for lower-educated fathers. However, this is not generally the case. In their analysis of elderly 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. Our data do not allow to stratify findings by offspring's gender.

Data and methods

We analyze the Office for National Statistics Longitudinal Study (ONS-LS), which contains linked census and life events data for a one-per cent sample of the population of England and Wales. The ONS-LS has linked records at each census since 1971 for people born on one of four selected (non-disclosed) dates in a year. The 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 also analyze the 49,612 male LS members whose wives reported on their fertility history in the 1971 census. Table 1 describes the characteristics of the sample stratified by parental sex. The first birth cohort subject to the reforms is the one including those born after September 1957. Table 2 shows the averages of outcome variables and covariates stratified by whether parents had children affected by the reform. We conduct a child-level analysis, as each parent can have several children, of which more than one can be affected by the reform. We weight our estimates by the inverse of the number of children a sample member has.

Similar to De Neve and Fink (2018), Lundborg and Majlesi (2018), and Ma (2018), we rely on a quasi-experimental design which exploits the exogenous variation in age at school completion due to compulsory schooling law changes 1972 in England and Wales. De Neve and Fink (2018), Lundborg and Majlesi (2018), and Ma (2018) use instrumental variables techniques, while we make use of a regression discontinuity framework which it is able to identify the presence of discontinuities controlling for trends over time. Compulsory school 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

Table 1: Descriptive statistics for ONS LS sample members (parents)

	Men	Women
Education in 1971:		
% No qualification (lower educated)	85.4	90.9
% A-level (higher educated)	8.9	7.9
% Higher than A-level (higher educated)	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
N	49,612	56,199

Source: ONS-LS

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

	Without children affected by reform	With children affected by reform
Outcome variables		
% deceased until December 31, 2015	77.0	43.0
% deceased before age 65 until December 31, 2015	14.3	12.7
% died of lung cancer	6.1	3.8
% died of accidents and self-harm	1.2	8.7
% died of liver disease	5.8	6.7
% died of ischaemic heart disease	14.5	6.3
% died of mental and behavioral causes	2.4	1.1
% died of preventable causes	9.3	7.1
% longstanding illness in 1991 census	30.7	17.7
% longstanding illness in 2001 census	51.7	37.3
% longstanding illness in 2011 census	73.9	51.6
% poor self-rated health in 2001 census	24	18.1
% poor self-rated health in 2011 census	21.9	14.3
Parental covariates		
% women	53.4	49.9
% men	46.6	50.1
Average number of children		
Average age at first birth	29.3	27.2
% higher educated	9.7	14.6
N	64,782	41,029

Source: ONS-LS

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 (Clark and Royer, 2013; Davies *et al.*, 2018; Oreopoulos, 2006), but none has examined the intergenerational effects that we are interested in.

Identification is achieved through a regression discontinuity (RD) approach (Imbens and Lemieux, 2008; Lee and Lemieux, 2010). All the analyses are carried out with Stata package 'rdrobust' (Calonico *et al.*, 2017). Our design is a fuzzy regression discontinuity approach. We are unable to determine the take-up of the treatment, since the only information available about children is their date of birth, not their actual educational attainment. This has two important implications for our analysis. First, the RD estimates represent intent-to-treat estimates, which is the effect of reform eligibility defined by the threshold rule (born after September 1, 1957). Intent-to-treat estimates rely only on the random assignment rather than the treatment actually received and thus represent unbiased estimates of the causal effect of the policymakers intention to treat. Therefore, intent-to-treat estimates tend to be more conservative than the effect of the treatment itself, particularly if compliance is low. However, compliance for this reform has been shown to be particularly high with very few people leaving school before compulsory age (Clark and Royer, 2013; Silles, 2009). For this reason, we expect that our intent-to-treat estimates are not different from the average treatment effect. A second implication of the fact that we don't observe children's actual educational attainment is that we cannot present simple OLS estimates of the association between children's education and parental health outcomes.

The accuracy of local polynomial estimation depends on the bandwidth around the cutoff. Therefore, it is of crucial importance that the bandwidth estimation procedure is carried out in a data-driven way, such that the trade-off between bias (when the bandwidth is large) and variance (when the bandwidth is small) is minimized (sensitivity checks for bandwidth are available upon request).

Figure 1 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, as stated above, we only know the quarter of birth, not the actual educational attainment of the children of ONS-LS sample members.

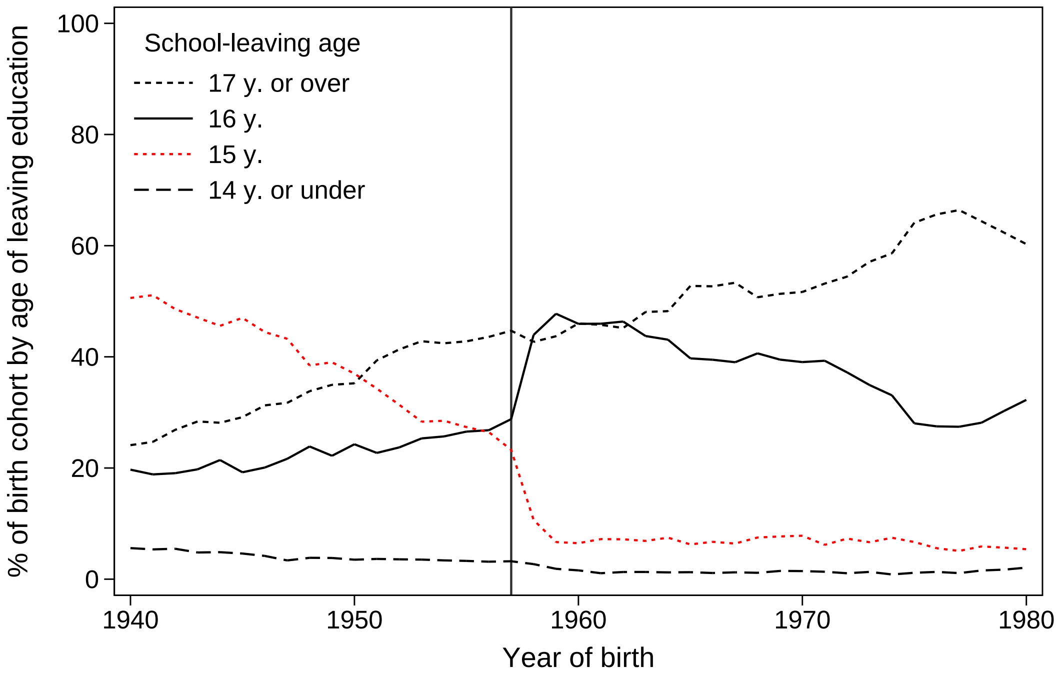

Figure 1: Years of full-time education by year of birth: The 1972 educational reform greatly reduced the share of individuals leaving school at age 15 for those born after September 1, 1957

Source: Health Surveys for England, 1991–2013

The parents in our sample can of course have multiple children who may or may not be affected by the reform. To handle this issue, we created a 'long' data set, where each row referred to one child (rather than one parent), and inversely weight the observations of parents with more than one child by number of children.

We analyze a range of non-self-reported outcome variables, namely 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 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). Further, we examine different self-reported health outcomes, namely self-reported longstanding illnesses, measured in 1991, 2001, and 2011, and self-reported poor general health, measured in 2001 and 2011. Question wordings vary slightly over decades, see the Appendix section 'Question wording for longstanding illness and poor self-rated health' for exact phrasing. We include the following control variables to our main model to improve the precision of our estimates: parental education (less than A-levels vs. A-levels and more), mother's age at first birth, dummy variables for parental social class, and number of children. Robust standard errors are calculated.

We further stratify our analyses by parental sex and parental socioeconomic status. We also 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. 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 (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. In addition, we stratify our analyses by four occupational macro classes as indicated by the registrar-general's class scheme (Connelly *et al.*, 2016), comparing different classes I/II ('Professional occupations' and 'Managerial and technical occupations'), III NM ('Skilled non-manual occupations'), III M ('Skilled Manual occupation'), IV and V ('Partly-skilled occupations' and 'Unskilled occupations'). Social class was derived from respondent's occupation in 1971. Women married at the time of the census have been assigned the social class of their husband if husband was present, other women their own social class.

Results

Figure 2 shows the RD estimates for maternal and paternal mortality at any age. The Figure reveals the presence of a small discontinuity for parents' mortality whose children were affected by the reform. However, the discontinuity does not seem to be pronounced. The following model tests formally the significance of these discontinuities.

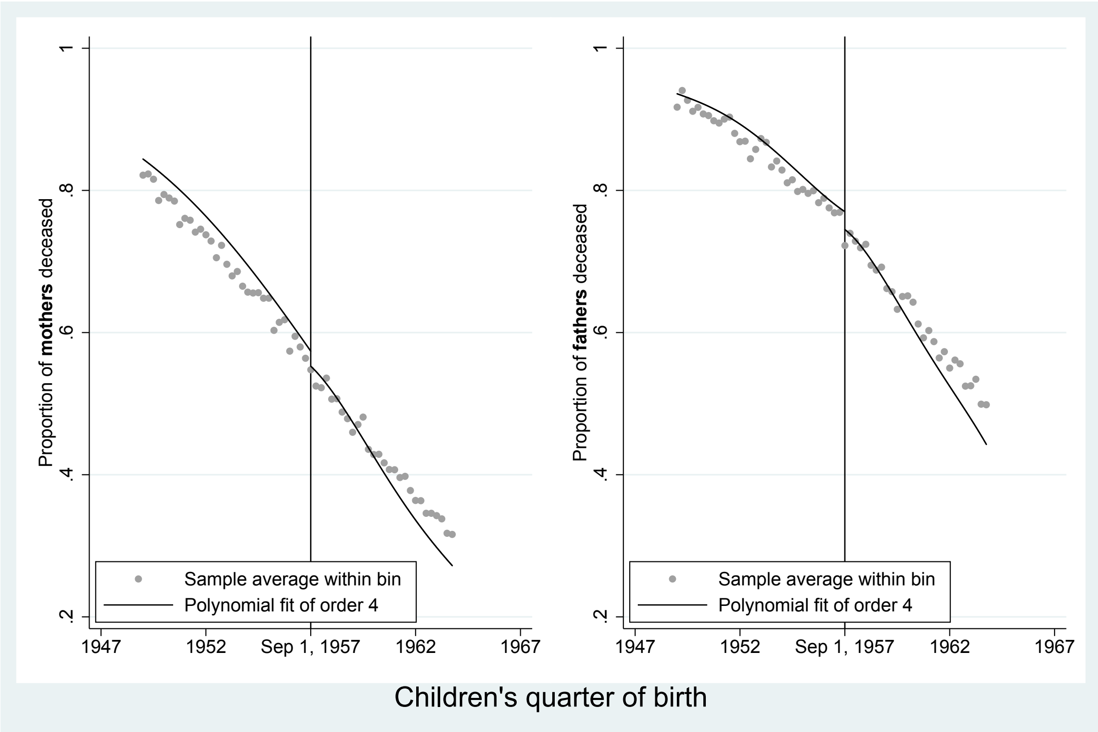

Figure 2: Discontinuity in parental mortality by children's quarter of birth

Source: ONS-LS

Figure 3 present the results from the regression discontinuity analyses using different outcomes, stratified by parental sex. The pattern of results is by and large the following: coefficients do not always point in the expected direction, usually do not reach conventional levels of statistical significance, and effect sizes are mostly small. Two estimates that do reach conventional levels of statistical significance are for women the risk of dying of ischaemic heart disease (-1.7%, $p < .05$, 95% CI: -3—0.2) and for men the risk of dying of liver disease (-0.5%, $p < .05$, 95% CI: -0.9—0.04).

In a second step of our analyses, we stratify the models by the socioeconomic position of the parents. Figure 4 stratifies the analyses by occupational class and Figure 5 by parental education and by area deprivation. For women, Figure 4

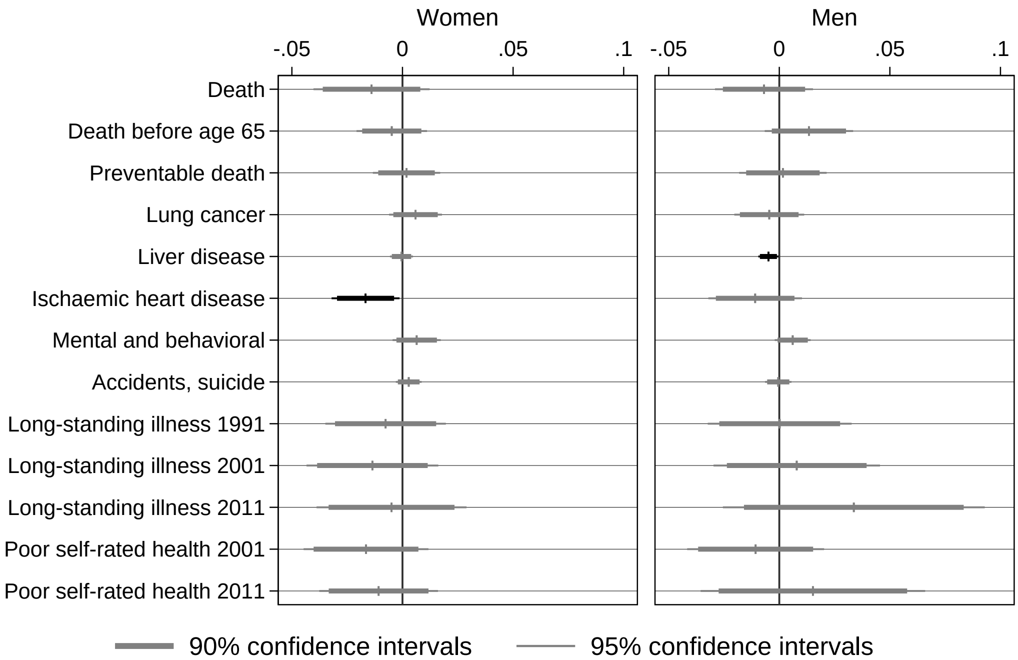

Figure 3: RD estimates of educational reform stratified by parental sex

Note: Confidence intervals based on robust standard errors. Models control for education, social class, number of children, and mothers' age at first birth. *Source:* ONS-LS suggests that those from lower-class occupations benefit rather consistently from having better-educated children by reducing the risk of premature mortality (death before age 65) by 4.9 per cent ($p < .05$, 95 % CI: -8.9 %—0.9 %). This is also reflected in lower risks of reports of health problems in 2001 (longstanding illness: -7.5 %, $p < .10$, 95 % CI: -15.3 %– 0.3 %, poor self-rated health: -6.3 %, $p < .10$, 95 % CI: -13.3 %–0.6 %). For women in the top-two social classes, we observe a lower risk of dying of ischaemic heart disease (-2.4 %, $p < .05$, 95 % CI: -4.5 %—0.3 %), but none of the other coefficients reaches conventional levels of statistical significance. As the results from Table 3 already suggested, fathers appear to be less affected by their children's education. The effect on liver disease we found in the pooled sample is only present among men from the upper classes in this analysis (-1.0 %, $p < .10$, 95 % CI: -2.0 %–0.1 %).

However, a comparison of these findings for social class with those for parental education and area deprivation, two other plausible measures for parental socioeconomic position, reveals that these findings are sensitive to the chosen measure of socioeconomic position. Looking at models stratified by education, we support the finding that children's education reduces the risk of dying from ischaemic heart disease (-1.9 %, $p < .05$, 95 % CI = -3.5 %—0.2 %) but only for lower-educated women. Further, findings suggest that children's education increases the risk of higher-educated women to report a long-standing illness in 1991 (7.2 %, $p < .10$, 95 %: -0.5–15.0 %) and poor self-rated health in 2011 (9.1 %, $p < .05$, 95 %: 1.8–16.5 %). For men, the beneficial effect of children's education on the risk of dying from liver disease particularly affects those with lower education (-0.4 %, $p < .10$, 95 % CI: -0.8 %–0.0 %).

The right hand panels of Figure 5 show results by area deprivation, suggesting that children's education reduces the risk of ischaemic heart disease (-3.4 %, p ¡ .0.5, 95 % CI = -6.4 %– -0.5 %) of women living in non-deprived areas (which is in line with the finding for social class but not education). For women in deprived areas children's education increases the risk of dying from lung cancer (1.6 %, $p < .05$, 95 % CI = 0.1 %–3.2 %). For men, stratifying by area deprivation yields two other findings: Educated children reduce the risk of men to report poor self-rated health in 2001 (-5.8 %, $p < .05$, 95 % CI = -10.5 %—1.1 %) a and reduce the risk of dying from lung cancer for men in deprived areas (-2.5 %, $p < .05$, 95 % CI = -5.2 %–0.2 %), but it increases the risk of reporting longstanding illness in 2001.

In sum, our investigation of heterogeneous treatment effects by parental socio-economic position revealed that few findings of children's educational effects hold across different indicators of socio-economic position. Children's education reduces low-SES mothers' risk of dying before age 65 was only found for occupational class ($p < .10$) but not for education ($p > .10$) and area deprivation ($p > .10$). Looking at cause-specific mortality, we find that children's

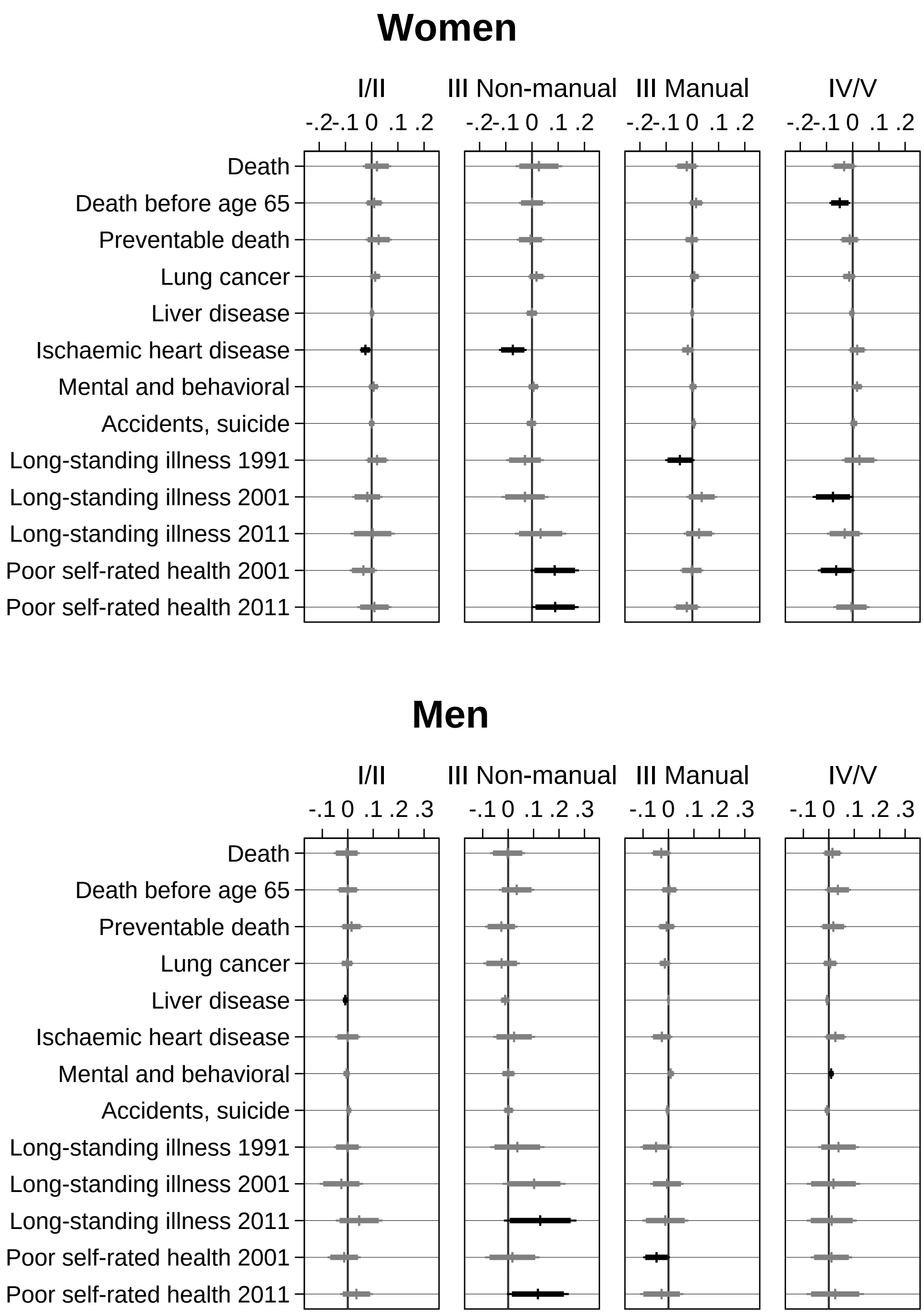

Figure 4: RD estimates of educational reform stratified by parental sex and parental social class

Note: Error bars denote 90% and 95% confidence intervals, based on robust standard errors. Models control for education, number of children, and mothers' age at first birth. *Source:* ONS-LS

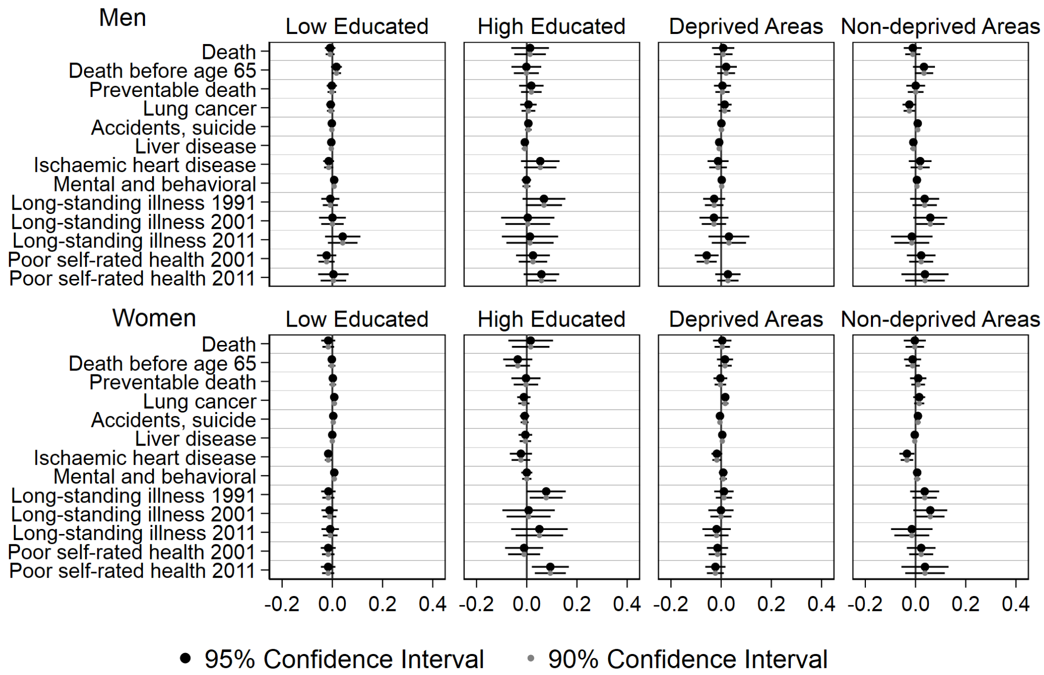

Figure 5: RD estimates of educational reform stratified by parental sex, parental education (left-hand panels), and area deprivation (right-hand panels)

Note: Confidence intervals based on robust standard errors. Models control for education, social class, number of children, and mothers' age at first birth. *Source:* ONS-LS education reduces low-SES mothers' risk of dying from ischaemic heart disease for occupational class ($p < .05$) and area deprivation ($p < .05$) in the opposite direction expected. Moreover, children's education reduces low-SES fathers' risk of dying from liver disease was only found for area deprivation ($p > .10$) and education ($p > .10$) and in the opposite direction for occupational class ($p > .10$). All other findings from the stratified models were specific to the indicators of parental socio-economic position.

As a further robustness check, we stratified models by the number of children that parents had (not shown), which reveal interestingly only weak effects of offspring education on fathers of three or more children. Having a child affected by the reform for fathers with more than three children reduces the probability of dying by 2.17 per cent (95% CI: -4.7–0.3), yet this estimate does not reach conventional levels of statistical significance.

Conclusions

Making use of an educational reform as a natural experiment, our findings show that the increase of children's school-leaving age from 15 to 16 years had only small effects on parental health and longevity in England and Wales. Our findings reveal that children's education reduced mother's risk of dying of ischaemic heart disease by between 0.2 and 3 per cent and fathers' risk of dying of liver disease by 0.04 and 0.9 per cent. An inspection of heterogeneous treatment effects by parental socioeconomic position shows that these benefits largely accrue for parents from disadvantaged backgrounds, even if heterogeneity by socioeconomic indicator is present. Further, we found suggestive evidence that mothers from disadvantaged occupational class might experience a reduction in the risk of premature mortality.

Our findings differ both from the existing associational research as well as from three studies exploiting educational reforms. While associational research has consistently shown a relationship between children's education and parental health, our study—which relies on an educational reform as an exogenous source of variation—barely shows such an effect. The existing studies relying on quasi-experimental designs were set in different contexts. De Neve and Fink (2018) analyze a schooling reform in Tanzania, a setting in which there is hardly any social security available. In such a context, children and their resources can make a massive difference for parental health. Further, the status of the family and the importance of elders is different in such a context. Also, the epidemiological profile of Tanzania is different from the developed world, with HIV/AIDS, tuberculosis, and malaria being important health risks, and life expectancy in general being much lower compared to developed countries. Lundborg and Majlesi (2018) examine a schooling reform in the context of Sweden's generous welfare state, where they do not find an effect of children's education across the board—only fathers of daughters benefit from their daughter's education. Our analyses of English and Welsh data are consistent with the first part of their findings—we also cannot identify an overall effect of children's education on parental mortality. We do find an effect for mothers that does not reach conventional levels of statistical significance, which however is driven by lower-educated mothers.

We also find little evidence for low-SES parents benefiting particularly from their children's education. This is a particularly striking finding as we would find the strongest effect for low-SES parents, as several mechanisms—financial or material, knowledge-based, or cultural—would be at play here that would operate in the same direction. The only other existing causal study in a resource-rich setting, Lundborg and Majlesi (2018) study of an educational reform in Sweden, was only able to find effects for the health of lower-educated parents, 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.

We compared different causes of death that should be particularly prone to improve with children's education, yet fail to find consistent evidence. The fundamental causes perspective on health inequalities (Mackenbach *et al.*, 2017; Phelan *et al.*, 2004) suggests that socioeconomic inequalities arise when it is possible for more socioeconomic advantage individuals to get health benefits by using resources and information. We would expect that some more preventable diseases would be more responsive to an increase in offspring's education.

Our analysis is not free from limitations. We have to acknowledge that we only have information about children born in wedlock; we might miss the potential impacts of out-of-wedlock children. However, given how low the prevalence of extramarital births in the 1970's UK was (five per cent in 1958), this bias should be modest. Another set of limitations is related to the fact that we only know the quarter of birth of children, not their actual educational attainment nor their sex. Not knowing children's actual educational attainment means that our results are intent-to-treat estimates, rendering our findings conservative, as we do not know which children actually benefitted from the reform. However, this problem would mainly operate through non-compliers, i.e. students who still leave school at age 15 despite the increase in mandatory school age. Given the evidence presented in Figure 1 as well as elsewhere in the literature (Clark and Royer, 2013; Silles, 2009), we believe that this problem is negligible since the compliance seems to be high. Not knowing the children's sex does not allow us to conduct stratified analyses regarding children's sex, and some studies have identified important sex differences (De Neve and Fink, 2018; Lundborg and Majlesi, 2018) that we could not investigate any further.

We propose three possible explanations for the absence of a causal effect. First, as Montez and Friedman (2015) have highlighted, it is important in the research of causal relation between education and health to understand under what conditions it exists. The English and Welsh context in which the reform took place is peculiar in at least three aspects: age at which the reform took place, relevance of education for determining socio-economic position, and universal health care coverage for elderly. First, the absence of causality is to be understood locally, since the educational reform increased age at school leaving from age 15 to 16. This does not exclude the presence of a causal effect of offspring's education on parental health at other points of the educational distribution, for example receiving college education in contrast to having only secondary education. Here, we were only able to test a marginal increase in the years of schooling from 15 to 16. Second, a possible explanation could rely on the fact that education was only marginally important for these generations. Perhaps occupational class was more important for lifestyles and cultural capital. Over time, education has become less important for determining the social class positions of adults in Great Britain once their class origins are taken into account (Goldthorpe and Mills, 2008; Jackson *et al.*, 2005). Parental occupational class of origin is a major predictor of children's pay in professional and managerial occupations in the UK (Laurison and Friedman, 2016). It is unclear what the effect of the reform for offspring's educational class was. In this analysis unfortunately, we are not able to stratify our analyses by offspring's social class. Third, the English and Welsh context in comparison with the US one is unique, since elderly people can benefit from universal access to health care and basic state pension, therefore relying less on offspring's resources. However, intergenerational exchanges are found to be closer in England compared to US (Silverstein *et al.*, 2010). Future research should explore the mechanism leading to the association between offspring's education and parental health.

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Appendix

Question wording for longstanding illness and poor self-rated health

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

Long-term illness in 1991 '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.'

- 'Yes, has a health problem which limits activities.' (1)
- 'Has no such health problem.' (0)

Long-term illness in 2001 '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.

- 'Yes.' (1)
- 'No.' (0)

Long-term illness in 2011 'Are your day-today 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.'

- 'Yes, limited a lot.' (1)
- 'Yes, limited a little.' (1)
- 'No.' (0)

Self-rated health in 2001 'Over the last twelve months would you say your health has on the whole been:'

- 'Good' (0)
- 'Fairly good' (0)
- 'Not good?' (1)

Self-rated health in 2011 'How is your health in general?'

- 'Very good.' (0)
- 'Good.' (0)
- 'Fair.' (0)
- 'Bad.' (1)
- 'Very bad.' (1)