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Orla Doyle, Colm Harmon and Ian Walker

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The Impact of Parental Income and Education on Child Health: Further Evidence for England*

Orla Doyle
UCD Geary Institute, University College Dublin

Colm Harmon
UCD Geary Institute University College Dublin and IZA

and

Ian Walker
University of Warwick and Princeton University

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Abstract:

This paper investigates the robustness of recent findings on the effect of parental education and income on child health. We are particularly concerned about spurious correlation arising from the potential endogeneity of parental income and education. We adopt an instrumental variables approach and our results suggest that the parental income and education effects are generally larger than are suggested by the correlations observed in the data. Moreover, we find strong support for the causal effect of income effect being large for the poor but small at the average level of income.

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Corresponding author: Professor Colm Harmon, Geary Institute, University College Dublin, Belfield, Dublin 4, Ireland
Tel +3531 7164615, Fax +353 1 7161108 Email colm.harmon@ucd.ie

I. Introduction

There is a vast literature documenting the relationship between socioeconomic status (SES) and health (see, for example, Wilkinson and Marmot 2003). Specifically the relationship between the health of children and the income of their parents has been the focus of much research. This relationship is important because it has been shown that the effects are long-lasting - poor health in childhood is associated with lower educational attainment, inferior labour market outcomes and worse health later in life.[1] Case, Lubotsky and Paxson (2002) and Currie, Shields and Wheatley-Price (2004) investigate the role of parental income, in the US and UK respectively, and find that there is an effect on child health. They refer to this income effect as the "gradient". The US data suggests that this gradient is larger for older children while the UK data suggests that it does not - perhaps because of the freely available healthcare in the UK.

The first contribution of our paper is to investigate the robustness of the main UK results presented in Currie *et al.*, (2004) to the possible endogeneity of parental incomes and education. In particular, this paper adopts an instrumental variables (IV) solution to spurious correlation and measurement error. In addition to considering parent or self-reported child health we also investigate having a chronic health condition. Our second contribution is that we explore the possibility that the effect of income is different (presumably larger) for poor households - something that is frequently suggested in the literature, but seldom explicitly tested.

First and foremost, we are concerned that the finding that income effects on child health may be the result of a spurious correlation rather than a causal mechanism. This can arise because of endogeneity (i.e. reverse causation arising from a sick child reducing parental income, or from low income parents and sick children having some common unobservable cause) or from measurement error (not least because the income data is grouped). In the case of reverse causation we would expect least squares estimates of the income effect to be biased upwards since income would capture the effect of income <u>and</u> the effect of other factors that are correlated with income but not included. However, measurement error (in income) may cause the

[1] Marmot and Wadsworth (1997) identify several "pathways" whereby childhood health affects adult health. See also Currie and Hyson (1999), Case *et al.*, (2002), Currie (2004) and Graham and Power (2004).

correlation to understate the true effect and, in general, we cannot sign the direction of bias. Finally, it should be noted that IV methods will, unlike OLS, yield estimates of local, rather than average, effects.[2,3]

Secondly, we are conscious that a similar argument can be made for the effect of education - if education and child health are correlated with some common unobservable (say, low time preference) then least squares estimates of the effect of parental education will be biased.[4] Omitting income from such analyses will cause the education coefficient to be biased upwards, to the extent that income and health are positively correlated. In some cases it is useful to know the effect of education on health, without holding income constant – for example, we may wish to know the extent to which the effect of some education reform affects health, both directly and indirectly via the effect of education increasing income. However, in other cases, it is useful to disaggregate the overall effect so as to isolate the effect of income alone, holding education constant: for example, if one is interested in the likely effect of changes in income transfers to parents on child health. The interpretation of the income effect may be different when education is controlled for – education may pick up the permanent component of income so that the coefficient of current income can then be interpreted as current income shocks. Moreover, there is a well developed literature, albeit mostly in a development context, that maternal background is more important than paternal.[5] We therefore examine the impact of both paternal and maternal education influences on child health outcomes, with and without income in the specification.

[2] See Imbens and Angrist (1994).

[3] Panel data has been used to control for unobservable fixed effects in a few studies (see Adams, Hurd, McFadden, Merrill and Ribeiro (2003), Frijters, Haisken-DeNew and Shields (2003), Meer, Miller and Rosen (2003) and Contoyannis, Jones and Rice (2004)) but only in the context of adult health. These suggest little support for a causal effect of income. We know of no studies that exploit sibling differences.

[4] A number of studies have addressed the issue of education endogeneity using instrumental variable techniques but only in the context of adult health (see, for example, Berger and Leigh 1989; Lleras-Muney 2002 and Arkes 2003).

[5] A number of studies have noted that maternal factors can affect a wide range of child outcomes including educational choices (Simpson 2003; Chevalier, Harmon, O'Sullivan, Walker 2005), cognitive and social development (Menaghan and Parcel 1991), political orientations (McAdams, VanDyke, Munch, Shockey 1997) and religiosity (Kieren and Munro 1987).

In addition, parental income data is often grouped and, in cases where the range midpoint is used, income is measured with error and the coefficient on income will be biased towards zero.

It is difficult to construct a likely argument for why measurement error in parental incomes should vary by the age of the child, so for example, the result in Case *et al.*, (2002) of a significantly positive interaction effect between child age and parental income *is* likely to be robust to any measurement error in income. However, the strength of any reverse causation may well vary with child age. For example, a sick child may require greater parental care when the child is young and this may imply a larger reduction in parental labour supply and income. In which case, the extent of downward bias in the income effect obtained from least squares estimation ought to be larger for households with young children relative to older children. This might account for the changing gradient by age. However, it may well be possible to construct arguments that go in the opposite direction and the question ultimately becomes an empirical one that can only be resolved through obtaining unbiased coefficients using some alternative method to least squares.

Finally, the paper explores the possibility that income effects may be nonlinear such that the income effect diminishes with income.

Our analysis here is based on a sample of children drawn from the Health Survey for England and finds that, in support of earlier work, there is a significant income gradient on self-reported health but no significant interaction with child age once one purges income (and education) of its endogenous variation. Any effects on having a chronic health condition seem confined to young children. Moreover, we find stronger effects once we allow for income and education to be endogenous. Finally, we do find support for the idea that the causal effects of income are strongest on the poorest.

This paper is structured as follows: Section 2 outlines the existing literature. Section 3 describes the data. Section 4 presents and discusses the results, and Section 5 concludes.

II. Literature

There are a variety of potential disadvantages for children from having low parental income and at least some of these may have long-lasting, and even permanent effects.[6] However, the mechanisms by which income is related to health remain controversial and, as noted by Deaton and Paxson (1998), “there is a well-documented but poorly understood gradient linking socio-economic status to a wide range of health outcomes” (p. 248). Case *et al.*, (2002) analyse the relationship between family income and child health using the US National Health Interview Survey (NHIS).[7] They show the existence of a significant and positive effect of income, with children in poorer families having significantly worse health than children from richer families. In addition, they find that the income gradient in child health increases with child age in the US, with the protective effect of income accumulating over the childhood years.[8] They suggest that this effect operates partly through poorer children with chronic health conditions such as asthma and diabetes having worse health. In an attempt to address why poorer children should be more afflicted by these conditions, they find that a genetic explanation, whereby parents who are in poor health earn less and have less healthy children, does not successfully explain the results. They also find that health insurance does not play a role.

Case, Fertig and Paxson (2005) investigate the relationship between parental SES and child health for the UK, more generally, using the National Child Development Study (NCDS) 1958 birth cohort. They find that the relationship between parental SES and child health gets steeper as children get older – i.e. the health differences across SES gets larger as children age. However it remains unclear what causal mechanism lies behind this result. For example, it is not clear whether this is due to low SES children having more adverse health shocks, or more serious ones, or whether such households do not cope as well with these shocks. Currie and

[6] See Case and Paxson (2006) for a review of the evidence relating child health to subsequent lifetime outcomes.

[7] In addition to the children in the 1986-1995 National Health Interview Survey (NHIS) cross-section dataset, this study also used the Panel Study of Income Dynamics (PSID), and the National Health and Nutrition Examination Survey from 1988 and 1994. The NHIS has large sample sizes and so permits the analysis of conditions that are relatively rare, while the PSID allows the effect of household income over time to be investigated.

[8] Currie and Stabile (2003) replicate this result for Canada, and also found evidence of an increasing income effect that increased with child age, which they attributed to low income children experiencing more health shocks than high income children.

Hyson (1999) partially succeed in addressing a similar issue using US data - for low birthweight. They find that low SES births were more likely to be lighter but, surprisingly, the effect of low birthweight on health does not vary much across SES. They suggest that health is a potentially important transmission mechanism for the intergenerational correlation of income and education.

Case *et al.,* (2002) find that not only do children from poorer households suffer from poorer health, but also that these adverse health effects tend to compound over time so that the variation in health across income or social class increases with age, even across children with similar chronic conditions. This results in children of poorer households entering adulthood in worse health and with more serious chronic conditions. Their results appear not to arise because higher income parents tend to have more education. They find that this income gradient remains even after controlling for parental education, and that education has an independent positive effect on health. Despite the common finding that income effects on child outcomes are larger at lower levels of income, they find that the gradient appears at all income levels; upper-income children do better than middle-income children, and middle-income children do better than lower-income children. The authors also find that the disparities in child health by parental income become larger with child age. Even after controlling for parental education, doubling household income increases the probability that a child aged 0–3 (4-8, 9-12, 13-17) is in excellent or very good health by about 4 percent (5 percent, 6 percent, 7 percent). They go on to investigate chronic conditions, such as asthma, other respiratory conditions, kidney disease, heart conditions, diabetes, digestive disorders, and mental health conditions.. Poor children with chronic conditions have poorer health than do higher-income children with the same conditions. Finally, they examine whether it is only permanent income that matters or, rather, whether the timing of income matters such that, for example, low income in early childhood has a more adverse effect on later health than low income later in childhood and they find no effect of the timing of income.

The recent work by Currie, Shields and Wheatley-Price (2004) also investigates the relationship between the health of children and the incomes (and education levels) of their parents, using pooled data from the 1997-2002 Health Surveys of England (HSE, see Sprosten and Primatesta, 2003). In this data two generations are present in the household, therefore it is possible to match the health of children with the educational attainment and income of their parents. This study attempted to confirm the extent to which findings for the US, in the earlier research by Case *et al.,* (2002), also hold in England.

Like Case *et al.,* (2002), Currie *et al.,* (2004) find robust evidence of an income gradient using subjectively assessed general health status, both controlling for parental education and not. However, the size of this gradient is somewhat smaller than in Case *et al*., (2002). Moreover, they find no evidence that the income gradient increases with child age. They find statistically significant income effects on the probability of having some chronic health conditions - notably asthma, mental and other nervous system problems, and skin complaints, which have a higher incidence in poorer families. There is some evidence that income does 'protect' children from the adverse general health consequences of some conditions such as mental illness and other nervous system problems, metabolic problems such as diabetes, and blood pressure problems such as hypertension. Independent effects of parental education, especially the mother's, on the health of children were also found.[9] However, they failed to find a significant interaction between child age and parental income – something which they attribute to the success of the NHS. While both Case *et al.,* (2002) and Currie *et al*., (2004) show that their income gradient results are robust to including other observable parental characteristics and to lifestyle variables, there remains the possibility that unobservable factors might still account for the results.

Burgess, Propper and Rigg *et al.,* (2004) use a early 1990's cohort of children from a particular part of South West England and find the direct impact of income on child health is small. They also find no change in the income gradient with child age.[10]

Unlike the US, where private health insurance is the norm, the UK has had, since 1948, a National Health Service (NHS) with health care being free at the point of delivery (see Culyer and Wagstaff 1993). Currie *et al*., (2004) argue that the NHS is successful in insuring the health of the children of low income UK parents as they, unlike Case *et al* (2002), find no evidence that the income effect on child health

[9] Additionally, they found that a significant income gradient remains after controlling for family fixed effects, child diet and parental exercise.

[10] Emerson *et al.,* (2005) use a UK survey of child mental health to demonstrate a correlation with household income.

increases with child age.[11] They also extend the findings of US research in a number of important ways. For example, they find clear effects of vegetable consumption and physical exercise on child health, but controlling for these, they find that their income effect results are largely unchanged. They also show that an income effect exists for objective measures of child health, derived from anthropometrical measurements and blood samples.

Very few studies examine the effect of exogenous income variation on child outcomes. Some studies exploit experimental welfare reforms - for example, Morris and Gennetian (2003) and Chase-Lansdale *et al.*, (2003) look at the effects of experimental and non-experimental welfare reforms in the US on child outcomes and generally find favourable effects. The only study, to our knowledge, that considers the effects of variation in *lump-sum* income, that could plausibly be argued to derive from some natural experiment, is due to Costello *et al.*, (2003) who track the mental health and behaviour of Native American Indian children before and after the opening of a casino that resulted in large lump-sum transfers being made to these parents.[12] The control group was the children of other (non-Native American) poor parents in the same counties. Both treatment and control benefited form the improvement in the job market associated with the casino.

Brooks-Gunn and Duncan (1997) lament the paucity of evidence on exogenous income variation and refer to the income maintenance experiments that occurred in several places in the US during the 1960's and 70's. They note that only in the poorest experiment (rural North Carolina) were there significant effects on child health, suggesting that the effect of income may be confined to just the children of low income parents. Although there seems to be a presumption in the literature that the effects of income are largest for the poorest, very few studies investigate the possibility of such nonlinearity explicitly and this is something we explore in our analysis below.[13]

[11] Currie *et al.*, (2004) do not, however, argue that there is no income effect at all - although the logic of their argument should apply for pre-natal child health as well, since NHS is a "cradle to grave" service that ought to ensure maternal health before and during pregnancy.

[12] Many parents also increased their labour supply but the effects for those that did not were similar to those that did suggesting that it was income that mattered.

[13] The review in Blau (1999) suggests that there is little evidence of any diminution in the effect of income as income rises.

III. Data and Sample Selection

The Health Survey for England (HSE) was initiated by the British government's Department of Health in 1992 to monitor trends in the nation's health.[14] The HSE surveys are an important source of information on household and individual characteristics and both subjective and objective measures of health. Each survey uses the Postcode Address File as a sampling frame, and is collected by a combination of face-to-face interviews, self-completed questionnaires and medical examinations. Each year the survey over-samples particular groups – for example, the elderly, ethnic minorities, etc. and our analysis applies sampling weights to produce the correct standard errors.

Although the HSE was initiated in 1992, the sample used in this paper only includes surveys from 1997-2002, since information on children aged 2-15 was only collected from 1995 onwards[15] (the 2001 survey extended the analysis to children under the age of 2) and household income was only collected from 1997 onwards. As children and parents from the same household are both interviewed it allows us to match parental characteristics to the child's record.[16] Pooling the six surveys resulted in a dataset containing 26,498 children; however as the parents of the over-sampled children included in 1997 and 2002 surveys were not interviewed, and this substantially reduces our sample size to 16,175. In addition, unlike Currie *et al.*, (2004) we exclude children whose fathers or mothers are either missing from the survey or are missing from the household (i.e. one-parent families), and we also drop those whose parents self-report themselves as being in an ethnic minority.[17] These criteria reduce our sample size to 9,958 children. We then drop any observations where data are missing on our variables of interest: for example household income is missing for approximately 10 percent of the sample. Our final sample therefore includes 6,389 children aged between 0 and 15, 19% of which are aged 0-3, 35% aged

[14] The HSE are carried out by the Joint Health Surveys Unit of the National Centre for Social Research and the Department of Epidemiology and Public Health, Royal Free and University College London. Scotland, Northern Ireland and Wales have separate administrative arrangements for health care and the HSE only covers England. There is a separate Scottish Health Survey.

[15] Up to two randomly selected children per household are surveyed.

[16] The HSE data does distinguish between natural, adoptive, foster and step parents and we define a "parent" as any type of parent.

[17] It seems likely that single mothers and ethnic minorities will exhibit different relationships to the explanatory variables than white couples. Unfortunately the dataset is too small to sustain separate analyses of these groups.

between 4 and 8, 27% between 9 and 12, and 19% between13 and 15.[18] Table 1 describes the summary statistics for the sub-sample used in the analysis. The average age that fathers left school (17.36) is slightly higher than mothers (17.33) and, as expected, the average age of fathers is approximately 2 years greater than that of mothers.

The primary variable of interest in this paper is a subjective measure of children's general health. It is a self-reported measure for children aged between 13 and 15 and is reported by parents for children less than 13 years of age. The variable is based on responses to the question *"How is your health in general?.* Possible answers range from *Very Good* to *Very Bad* on a 1 to 5 scale. Following Currie *et al.*, (2004) the measure was recoded into a 4-category variable, whereby *"Bad"* and *"Very Bad"* were combined due to low sample sizes in these categories. The distribution of our dependent variable is as follows: Very Good (60.8 percent), Good (33.9 percent), Fair (4.7 percent), Very Bad/Bad (0.5 percent). The surveys also include information on whether the child has a long-term chronic health condition (CHC). The respondent can list up to 6 CHCs from the 42 categories that are coded. In our sample of 6,389 children, 20.9 percent have at least one chronic health condition. Thus, we also include an analysis of chronic condition incidence. Figures 1 and 2 show the joint distributions of self-reported health and child age, and the incidence of having a CHC and child age. Note that both subjective ill health and having a chronic condition increase as children age.

Following Currie *et al.*, (2004) current total pre-tax annual family income is used as a measure of parental income. It is coded in 31 income bands ranging from less than £520 to more than £150,000. The midpoints of each band were taken and deflated to 2000 prices using the UK average earnings[19] index according to the month

[18] Full details of the original HSE data, and the (small) impact of our selection criteria, are available in Table A1 in the Appendix.

[19] We follow Currie *et al* in deflating by an earnings index, and we also follow them in using incomes of £520 and £150,000 for the bottom and top codes of the income distribution.

in which the interview was conducted[20]. The average annual household real income is £34,869.[21,22]

Our measure of parental schooling is derived from two sources. The HSE asks parents the age at which they finished full-time education. It is coded 1-8 (where *1=Not yet finished, 2=Never went to school, 3= aged 14 or under, 4=aged 15, 5= aged 16, 6= aged 17, 7=aged 18 and 9=aged 19 or over*). As there are no parents in the dataset who were old enough to have left school at age 14, and as we drop ethnic minorities from our sample, there is no-one in our sample who left education before age 15. Furthermore, as the variable is top coded at 19, we use an additional HSE variable which captures the parents highest educational qualification to distinguish parents who left at 19 from those who left after 19. We combine this with information from the UK Labour Force Survey to determine the average leaving age of individuals with a degree.[23] This allows us to create a new age left school variable ranging from 15 to 21.[24]

[20] Estimates using the grouped dependent variable estimator due to Stewart (1983) were also conducted and the results were unchanged.

[21] Note that this figure is greater than Currie *et al.*, (2004) findings as we only include households with two parents, while Currie *et al*. also include single-parent households. We use the log of household income in the empirical analysis.

[22] Indeed the Labour Force Survey provides an important point of comparison to gauge the reliability of the HSE data in regards the parental income and educational measures. Therefore we compare our HSE sample to a similar, but much larger, selected sample in the UK Labour Force Survey from 1997-2002. We attempt to replicate the HSE sample by analysing white two-parent households in England who have children between the ages 0 and 15. Unlike the HSE, the household income measure in LFS is continuous and represents a combination of mothers and fathers income. The average real household income of £34,889 in LFS is almost the same as the HSE measure (£34,869). Appendix Figures A1a and A1b show that the distribution of income (as reported in the 31 income bands in the HSE and equivalent income bands imposed on LFS) is similar across both samples.

[23] The HSE data contains two education measures – the age at which the respondent left school (which is top coded at 19) and the respondent's highest qualification level. The LFS data also contains the same two measures, however it does not top code the age left school variable. To overcome the top-coding problem within HSE, we use the LFS data to generate the average age of a respondent with a degree (age 21), and the average age of a respondent with a teaching qualification (age 20). Then, for respondents within HSE who have a degree or a teaching qualification, we recode their age left education variable with the average age left education generated from the LFS data. Therefore the new age left education variable the HSE data ranges from 14 to 21.

[24] As already noted, one particular concern with the HSE data is that the educational measure, which reports the age at which the parent left full-time education, has an upper bound at age 19; therefore we cannot distinguish different levels of higher education. The LFS data, on the other hand, includes a continuous educational measure. Table A1 in the Appendix compares the age at which mothers and fathers left full-time education in both the LFS and HSE samples. It shows that the majority of mothers (43.21 percent in LFS and 43.51 percent in HSE) and fathers (46.98 percent in LFS and 43.71 percent in HSE) left education at 16. There are notable similarities between the two datasets. While a direct comparison of the upper age categories is not possible, Table A1 shows that 25.47 percent of fathers

Figure 1 Self-reported child (ill) health and age of child

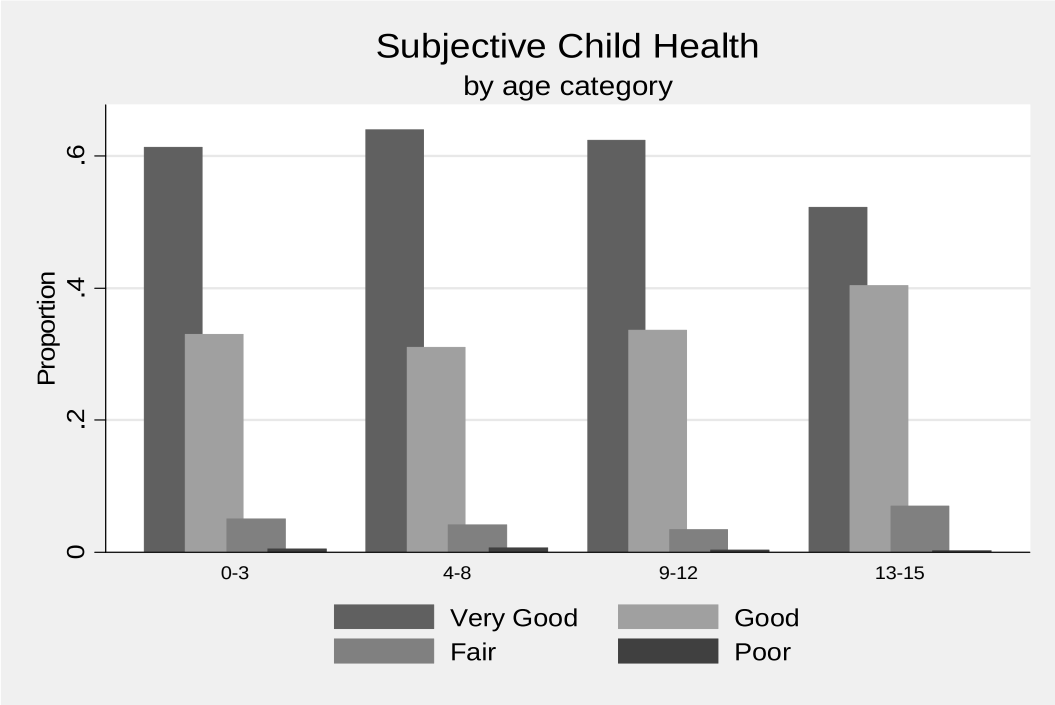
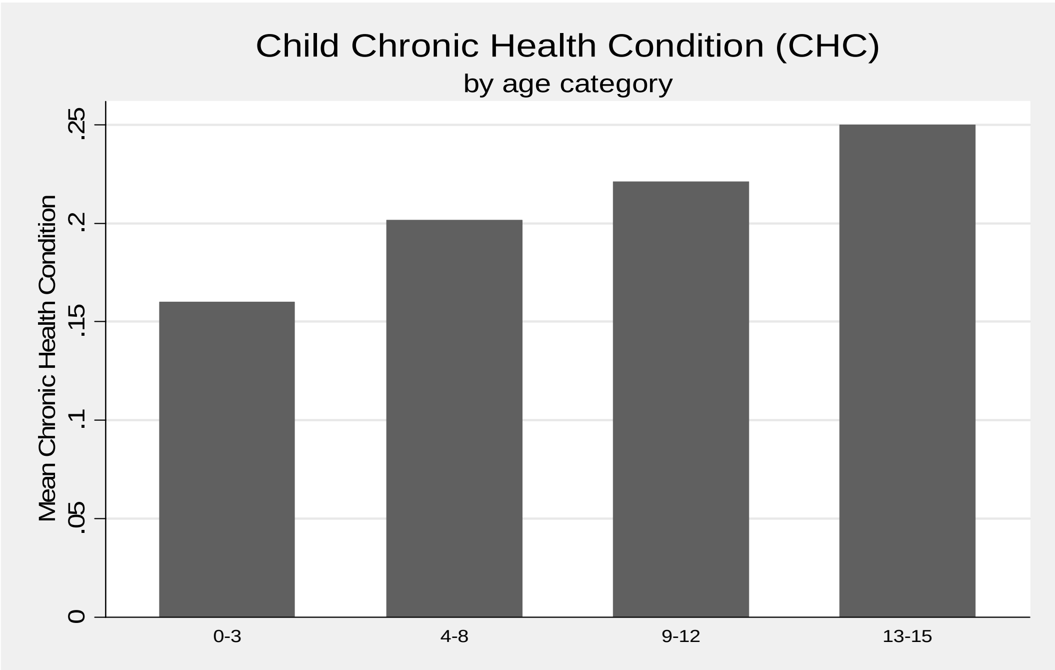


Figure 2 Having a chronic health condition and age of child



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and 23.41 percent of mothers in the LFS left education at 19 or over, compared with 28.23 percent and 23.27 percent in the HSE. Appendix Figures A2a-A2d report the corresponding histograms.

Table 1 *Descriptive Statistics HSE 1997-2002 - Estimation Sample*

	All Ages	0-3	4-8	9-12	13-15
Child's subjective ill health (1-5)	1.45 (0.61)	1.45 (0.62)	1.42 (0.61)	1.42 (0.58)	1.55 (0.64)
Child has a chronic health condition	0.21 (0.41)	0.16 (0.37)	0.20 (0.40)	0.22 (0.42)	0.25 (0.43)
Household log income	10.25 (0.66)	10.22 (0.68)	10.25 (0.65)	10.26 (0.69)	10.29 (0.63)
Mother's schooling	17.33 (1.82)	3.74 (1.88)	3.38 (1.77)	3.24 (1.81)	2.98 (1.80)
Father's schooling	17.36 (2.04)	3.74 (2.04)	3.39 (1.77)	3.27 (2.03)	3.06 (2.09)
Mother's age at birth	29.02 (5.14)	30.0 (5.25)	29.29 (5.10)	28.43 (5.17)	28.42 (4.89)
Father's age at birth	31.21 (6.00)	32.17 (6.01)	31.64 (5.93)	30.56 (5.96)	30.44 (6.00)
Mother started smoking before age 16	0.15 (0.36)	0.15 (0.36)	0.14 (0.35)	0.15 (0.36)	0.16 (0.36)
Mother started smoking between ages 16 and 19	0.20 (0.40)	0.18 (0.39)	0.20 (0.40)	0.22 (0.42)	0.21 (0.41)
Mother started smoking after age 19	0.08 (0.28)	0.08 (0.27)	0.08 (0.28)	0.09 (0.28)	0.09 (0.28)
Father started smoking before age 16	0.26 (0.44)	0.24 (0.43)	0.25 (0.43)	0.27 (0.45)	0.28 (0.45)
Father started smoking between ages 16 and 19	0.21 (0.40)	0.19 (0.39)	0.20 (0.40)	0.21 (0.41)	0.22 (0.42)
Father started smoking after age 19	0.09 (0.29)	0.09 (0.28)	0.09 (0.29)	0.09 (0.28)	0.09 (0.28)
Mother smokes	0.24 (0.42)	0.22 (0.41)	0.23 (0.42)	0.25 (0.43)	0.23 (0.42)
Father smokes	0.24 (0.43)	0.25 (0.43)	0.24 (0.43)	0.23 (0.42)	0.23 (0.42)
Years exposed to Mother's smoking	2.34 (4.32)	0.60 (1.19)	1.66 (2.84)	3.24 (4.90)	4.02 (6.26)
Years exposed to Father's smoking	5.84 (5.03)	1.73 (1.41)	4.56 (3.05)	7.48 (4.89)	9.79 (6.36)
Mother smoked when pregnant	0.01 (0.11)	0.03 (0.16)	0.01 (0.10)	0.01 (0.09)	0.003 (0.057)
Paternal grandfather smoked	0.71 (0.46)	0.64 (0.48)	0.70 (0.46)	0.71 (0.45)	0.76 (0.43)
Paternal grandmother smoked	0.49 (0.50)	0.45 (050)	0.49 (0.50)	0.49 (0.50)	0.53 (0.50)
Maternal grandfather smoked	0.67 (0.47)	0.60 (0.49)	0.65 (0.48)	0.69 (0.46)	0.72 (0.45)
Maternal grandmother smoked	0.47 (0.50)	0.44 (0.50)	0.46 (0.50)	0.49 (0.50)	0.50 (0.50)
Mother affected by RoSLA	0.76 (0.43)	0.96 (0.18)	0.88 (0.33)	0.69 (0.46)	0.46 (0.50)
Father affected by RoSLA	0.66 (0.47)	0.91 (0.29)	0.88 (0.33)	0.57 (0.50)	0.34 (0.47)
N	6389	1187	2232	1742	1228

Note: Means and standard deviations (in parentheses) reported.

IV. Estimation, Identification, and Results

We estimate the impact of parental background on child health within this model:[25]

$$\mathbf{H}_h = \mathbf{S}_h\boldsymbol{\beta} + Y_h\alpha + \mathbf{C}_h\boldsymbol{\delta} + \mathbf{X}_h\boldsymbol{\gamma} + \varepsilon_h^c \qquad (1)$$

where h indicates household and $\mathbf{H}_h = SRH_h, CHC_h$ such that self-reported health, *SRH*, is a four point ordinal variable defining child (ill) health status (1=very good, 2=good, 3=fair, 4=bad or very bad) as discussed above, and *CHC* is a binary variable indicating whether the child has a chronic health condition. The first is estimated as an ordered probit and the second as a probit. In both cases, child health is a function of parental education, **S**, measured as the ages at which the mother and father left full-time education[26], and the (log of) household income Y_h[27] (and, in some specifications, we have included its square to allow for possible nonlinear effects). We also include controls for cigarette smoking, **C** - specifically whether the father or mother is currently a smoker, whether the mother smoked during pregnancy, and the number of years the child has been exposed to parental smoking. Finally, **X** contains additional parental and child characteristics including the mother and fathers ages at the time of the child's birth (entered as a quadratic), log of number of children in the household, year and month of survey dummies, and region of residence at time of survey.[28]

Table 2a and 2b presents our benchmark estimates assuming that income and education are exogenous (replicating the structure of Table 1 in Currie *et al.*,

[25] While there are sibling pairs in the data the household is observed at only one point in time and so we cannot estimate sibling difference models. However, we do control for the clustering that occurs because households contain siblings.

[26] We tested the assumption that the effect of education is linear against a general specification that allowed each level of education to have its own independent effect. We found that the linear restriction for maternal schooling was acceptable while the effect of paternal education was nonlinear with no significant marginal effects of education above a school leaving age of 16. We found that a parsimonious acceptable specification of the paternal education effect was a simple dummy variable for having education leaving age of 16 or higher compared to 15.

[27] The strong distributional assumption of the ordered probit model was relaxed in alternative specifications based on the semi-parametric estimator of Stewart (2004). While the estimates for the pooled exogenous model, available on request, seem statistically preferable to the ordered probit model in column 2 of Table 2 (based on the likelihood tests in the Stewart model), the impact of the change in specification is slight. Attempts to use the semi-parametric specification to estimate the endogenous model, i.e. Table 4, were unsuccessful, as the model fails to converge.

[28] We found no effects of month of birth.

(2004)).[29] We estimate separate models for the four age cohorts, both to test for the stability of the income effect, and to control for the fact that the health of children up to the age of 13 were reported by their parents and self-reported thereafter. Our results for income in the centre of Table 2a entirely confirm the findings in Currie *et al.*, (2004) despite slight differences in specification and sample selection. There are income effects but they do not vary significantly with child age. We also agree that including education reduces the size of the income coefficients although not by very much. Finally, we find that the education effects, while not as well determined as the income effects, are relatively stable with respect to the inclusion of income.

We also explored the possibility of nonlinear income effects. Full results are available on request but they can be summarized as follows: the education effects were unaffected by the inclusion of the squared log income term; and the quadratic term was generally small and typically not significant – a typical finding was that the effect at half average income was approximately 30% larger than at the average level of income but the effect at this level was still not statistically significant. These estimates do not provide support for the common assertion that income effects are more important for the poor.

Table 2b shows the probit results for *CHC*. There are no education effects but there are income effects, although they are not stable across age groups- they are largest for the oldest and youngest groups and insignificant for those between.

As already discussed, the impact of parental schooling and income on child health outcomes may suffer from endogeneity problems. Here we identify the effect of parental education on child health outcomes using plausibly exogenous variation in schooling and incomes from a number of sources. Harmon and Walker (1995) show that the raising of the minimum school leaving age (a reform known as RoSLA) in Britain, whereby individuals born before September 1957 could leave school at 15 while those born after this date had to stay for an additional year, affected education levels and hence income. In this data 76 percent of the mothers and 66 percent of fathers in the sample are born after the relevant birth date that raises the minimum age

[29] Tables A2a and A2b in the Appendix replicate Table 2, but exclude the parental smoking controls and birthweight respectively. In addition, models including interactions between father’s schooling and household income, mother’s schooling and household income and father’s schooling and mother’s schooling were also estimated, and are available upon request. However including such interactions do not substantially change the results.

at which one could leave school from 15 to 16. This policy change creates a discontinuity in the age at which parents left school that we can exploit if we can assume that a smooth function of birth date can be used to control for the long term time trend rise in school leaving ages. We allow for this RoSLA affect to be vary across different regions on the grounds that it is likely to affect education more in areas with low average education.[30]

We also exploit *grand*parental smoking histories as instruments. We assume that having a grandparent who smoked is associated with lower parental education/income and that this does not affect child's health outcomes once we control for other factors that affect child health. While adolescent smoking has been used as an instrument when examining educational choices (see Evans and Montgomery 1994, and a review in Harmon, Oosterbeek and Walker 2003), no study to date has used grandparental smoking to instrument parental education/income in a child's health equation. Our instruments also include a set of binary variables indicating whether the parent started smoking prior to age 16, started smoking between 16 and 19 and, finally, started smoking after age 19. Again we rely on these not affecting health directly once we control for other child health determinants which will include parental smoking and the number of years the child has been exposed to parental smoking.

We therefore estimate first stages as $S_m = \mathbf{Z}\boldsymbol{\pi}_m + \xi_m$ for maternal schooling level, $I_{S_p>15} = \mathbf{Z}\boldsymbol{\pi}_p + \xi_p$ for paternal post-compulsory schooling, and $Y = \mathbf{Z}\boldsymbol{\pi} + \xi_Y$ for household income, where *m* and *p* indicate mother and father, $I_{S_m>15}$ is a dummy variable indicating that paternal education exceeds a school leaving age of 15, and the **Z**'s are relevant exogenous control variables which include: the maternal and paternal *RoSLA* controls; maternal and paternal *RoSLA* interacted with region of residence; vectors containing *grandparental* smoking dummies (paternal and maternal) and a set of dummy variables accounting for whether the parents smoked before the age of 16, between the age 16 and 19, or after age 19; and a *cohort* (cubic) function of parental date of birth..

[30] Appendix Figures A3a and A3b illustrates this by showing the mean schooling leaving age for males and females by birth year and month between January 1956 and December 1958. There is a marked jump in both graphs for respondents born in September 1957 which coincides with the introduction of the new school leaving age.

Table 3 shows the results from these first stage regressions. The RoSLA variable shows a strong significant 30% impact on the probability that the schooling level of the father exceeds 15, while the effect on maternal schooling is more than two thirds of a year of schooling in the North West region which has had a historically low level of education. This is consistent with the findings in Harmon and Walker (1995) and the survey in Harmon *et al.*, (2003), based on similar samples of males from a range of UK surveys. The early teen smoking variable, for mothers and fathers, has a very strong and negative effect on schooling, and the smoking status of grandparents also have strong, negative effects on the schooling of both parents. Later teen smoking seems to affect only mothers' education. Table 3 also presents the estimates of the parameters of the characteristics of both mother and father in the household income equation. Many of the variables are significant in the log income equation also. The F-test for the significance of the instruments reported at the end of Table 3 is passed with very low P – values.

Table 4a and 4b presents the child health equations and mirrors the structure of Tables 2a and 2b but parental schooling and household income now endogenous. The test statistics support the appropriateness of our instruments. The paternal education variables are now insignificant in all specifications. However, maternal education continues to have effects which are somewhat larger than the simple correlations in Table 2 indicated. The income effects, for the whole sample, now seem to be larger than in Table 2 – presumably reflecting the local average treatment interpretation. However, the effects on the age subgroups are generally not sufficiently well determined to indicate how the gradient changes with child age.

We go on to explore the possibility that the income effect is nonlinear in this model that allows for endogeneity. Unlike the exogenous income effect we now find powerful nonlinear effects. Tables 5a and 5b presents OLS and IV results which replicates Table 2a but includes the square of household income. For example, in the case of *SRH*, using the whole sample, controlling for education or not, we find that the log income coefficient is well determined and approximately -14 and the quadratic term is well determined and approximately 0.7. Thus, the implied income effect at the mean income (a log income of 10.25) is approximately -0.065, while the effect at half of the mean income (a log income of 9.3) is approximately -1.5. Thus, we find strong support for large effects of income on the outcomes for the very poor.

Table 2a: Ordered Probit Estimates of Parental Income and Education on Child Ill Health Status: Exogenous

Subjective Health	All	0-3	4-8	9-12	13-15	All	0-3	4-8	9-12	13-15	All	0-3	4-8	9-12	13-15
Mom Schooling	-0.035***	-0.027	-0.041**	-0.055***	-0.009	~	~	~	~	~	-0.018*	-0.019	-0.021	-0.041**	0.014
	(0.010)	(0.021)	(0.017)	(0.019)	(0.021)						(0.010)	(0.022)	(0.017)	(0.020)	(0.023)
Dad Schooling	-0.192***	0.232	-0.300**	-0.300***	-0.086	~	~	~	~	~	-0.151**	0.246	-0.239*	-0.274**	-0.036
	(0.059)	(0.302)	(0.129)	(0.109)	(0.094)						(0.060)	(0.301)	(0.128)	(0.110)	(0.096)
Household Income	~	~	~	~	~	-0.181***	-0.091	-0.213***	-0.184***	-0.197***	-0.153***	-0.078	-0.182***	-0.131**	-0.206***
						(0.028)	(0.061)	(0.049)	(0.054)	(0.059)	(0.029)	(0.063)	(0.051)	(0.056)	(0.065)
Observations	6389	1187	2232	1742	1228	6389	1187	2232	1742	1228	6389	1187	2232	1742	1228

Table 2b: Probit Estimates of Parental Income and Education on Child having a Chronic Health Condition: Exogenous

CHC	All	0-3	4-8	9-12	13-15	All	0-3	4-8	9-12	13-15	All	0-3	4-8	9-12	13-15
Mom Schooling	0.001	-0.020	0.021	-0.001	-0.009	~	~	~	~	~	0.008	-0.004	0.020	-0.005	0.024
	(0.011)	(0.028)	(0.019)	(0.021)	(0.025)						(0.012)	(0.029)	(0.020)	(0.022)	(0.026)
Dad Schooling	-0.036	0.050	-0.094	-0.149	0.107	~	~	~	~	~	-0.021	0.084	-0.094	-0.156	0.177
	(0.070)	(0.389)	(0.163)	(0.122)	(0.111)						(0.071)	(0.388)	(0.165)	(0.123)	(0.115)
Household Income	~	~	~	~	~	-0.053*	-0.154**	0.014	0.016	-0.242***	-0.059*	-0.152*	0.001	0.031	-0.297***
						(0.032)	(0.077)	(0.057)	(0.059)	(0.072)	(0.034)	(0.081)	(0.060)	(0.062)	(0.078)
Observations	6389	1187	2232	1742	1228	6389	1187	2232	1742	1228	6389	1187	2232	1742	1228

Notes: Table 2a reports coefficients from ordered probit models of general health status (1= Very Good, 2=Good, 3=Fail, 4=Bad/Very Bad). Robust standard errors are in parenthesis. Thresholds are also estimated but not reported. Table 2b reports coefficients from probit models indicating whether the child has a chronic health condition are reported. Robust standard errors are in parenthesis. All specifications include mother's and father's age at the time of the child's birth in quadratic, indicators of whether the mother or father is currently a smoker, indicator of whether the mother smoked during pregnancy, the number of years the child has been exposed to parental smoking, ethnicity (white base), log of number of children in the household, month of survey dummies and year of survey dummies and region of residence. Significant levels: *** 1%, ** 5% and * 10%.

Table 3 First Stage Equations

	Schooling		Household Log Income	
	Mom SLA	Dad SLA>15	Mom variables	Dad variables
RoSLA* Region N.E. & E.Mids	-0.050 (0.128)	0.305*** (0.017)	0.115** (0.051)	0.044 (0.047)
RoSLA*Region N.West	0.678*** (0.175)	0.045* (0.23)	-0.042 (0.077)	0.114* (0.069)
RoSLA*Region W.Mids	0.016* (0.187)	-0.046* (0.025)	-0.216*** (0.083)	0.191** (0.074)
RoSLA*Region South	-0.217* (0.123)	-0.135*** (0.016)	-0.140** (0.054)	-0.008 (0.049)
Started smoking before age 16	-0.986*** (0.064)	-0.075*** (0.008)	-0.174*** (0.023)	-0.165*** (0.019)
Started smoking ages 16 to 19	-0.560*** (0.056)	-0.004 (0.009)	-0.074*** (0.020)	-0.099*** (0.021)
Started smoking after age 19	0.125 (0.080)	-0.007 (0.012)	0.007 (0.029)	-0.059** (0.029)
Grandfather smoked	-0.314*** (0.048)	-0.022*** (0.007)	-0.073*** (0.017)	-0.042** (0.018)
Grandmother smoked	-0.278*** (0.045)	-0.023*** (0.007)	-0.027* (0.016)	-0.068*** (0.016)
Region N.E & E.Mids	-0.590*** (0.154)	-0.049*** (0.019)	0.058 (0.055)	
Region WMids	0.057 (0.163)	0.041** (0.020)	0.088 (0.059)	
Region EMids	0.635*** (0.107)	0.130*** (0.013)	0.375*** (0.039)	
Region South	~	~	~	
F test of instruments (p-value)	47.90 (0.000)	174.37 (0.000)	34.49 (0.000)	
Observations	6389	6389	6389	

Notes: OLS estimates (standard errors in parentheses). Controls included, but not reported, are Father's and Mother's date of birth in cubics (they are continuous variables with months divided by 100 being the unit of measurement with September 1934 being equal to zero). The omitted category is Never smoked. Significant levels: *** 1%, ** 5% and * 10%.

Table 4a Estimates of Parental Income and Education on Child Ill Health Status: Endogenous

SRH	All	0-3	4-8	9-12	13-15	All	0-3	4-8	9-12	13-15	All	0-3	4-8	9-12	13-15
Mom Schooling	-0.102***	-0.091	-0.037	-0.136*	-0.137*	~	~	~	~	~	-0.067	0.055	0.107	-0.183*	-0.141
	(0.038)	(0.110)	(0.067)	(0.074)	(0.080)						(0.053)	(0.175)	(0.106)	(0.102)	(0.115)
Dad Schooling	-0.213	0.494	0.191	-0.524	-0.588	~	~	~	~	~	-0.086	1.148	0.604	-0.658	-0.599
	(0.211)	(0.638)	(0.372)	(0.381)	(0.445)						(0.228)	(0.785)	(0.409)	(0.388)	(0.493)
Household Income	~	~	~	~	~	-0.373***	-0.371	-0.357	-0.426	-0.564*	-0.194	-0.827	-0.806**	0.274	0.022
						(0.137)	(0.347)	(0.242)	(0.273)	(0.336)	(0.224)	(0.733)	(0.397)	(0.462)	(0.565)
Observations	6389	1187	2232	1742	1228	6389	1187	2232	1742	1228	6389	1187	2232	1742	1228
Hansen J Statistic (over ID test)	23.78	12.93	14.50	8.76	10.98	20.52	14.53	18.94	20.58	17.18	41.85	21.91	19.14	32.37	20.48
	(0.008)	(0.228)	(0.152)	(0.555)	(0.359)	(0.039)	(0.205)	(0.062)	(0.038)	(0.103)	(0.004)	(0.405)	(0.576)	(0.054)	(0.491)
F test of residuals (P)	4.10	1.13	2.23	2.00	4.49	3.72	0.75	0.55	1.17	1.71	3.76	2.39	5.71	3.32	4.46
	(0.129)	(0.567)	(0.327)	(0.368)	(0.106)	(0.054)	(0.387)	(0.457)	(0.280)	(0.191)	(0.289)	(0.495)	(0.126)	(0.345)	(0.216)

Table 4b: Probit Estimates of Parental Income and Education on Child having a Chronic Health Condition: Endogenous

CHC	All	0-3	4-8	9-12	13-15	All	0-3	4-8	9-12	13-15	All	0-3	4-8	9-12	13-15
Mom Schooling	-0.124***	-0.166	-0.138	-0.105	-0.119	~	~	~	~	~	-0.104*	-0.119	-0.111	-0.003	-0.056
	(0.043)	(0.119)	(0.079)	(0.079)	(0.094)						(0.062)	(0.171)	(0.114)	(0.113)	(0.135)
Dad Schooling	-0.043	-1.687**	0.103	-0.352	0.572	~	~	~	~	~	0.026	-1.500	0.179	-0.061	0.752
	(0.215)	(0.833)	(0.392)	(0.367)	(0.476)						(0.276)	(1.147)	(0.500)	(0.428)	(0.617)
Household Income	~	~	~	~	~	-0.335**	-0.978**	-0.373	-0.615*	-0.266	-0.107	-0.261	-0.147	-0.587	-0.362
						(0.152)	(0.480)	(0.257)	(0.328)	(0.436)	(0.237)	(0.823)	(0.444)	(0.490)	(0.549)
Observations	6389	1187	2232	1742	1228	6389	1187	2232	1742	1228	6389	1187	2232	1742	1228
Hansen J Statistic (over ID test)	10.80	16.31	11.50	8.86	13.21	14.73	23.98	11.23	4.91	11.45	23.70	30.08	22.49	19.24	27.32
	(0.373)	(0.091)	(0.320)	(0.545)	(0.212)	(0.195)	(0.013)	(0.424)	(0.935)	(0.407)	(0.308)	(0.090)	(0.372)	(0.570)	(0.161)
F test of residuals (P)	9.12	7.69	4.08	2.12	2.43	4.44	3.81	1.94	4.18	0.04	8.98	7.09	4.38	3.56	2.69
	(0.011)	(0.021)	(0.130)	(0.347)	(0.297)	(0.035)	(0.051)	(0.164)	(0.041)	(0.850)	(0.030)	(0.069)	(0.224)	(0.313)	(0.442)

Notes: Table 4a reports coefficients from ordered probit models of general health status (1= Very Good, 2=Good, 3=Fail, 4=Bad/Very Bad) are reported. Table 4b reports coefficients from probit models indicating whether the child has a chronic health condition are reported. Bootstrapped standard errors are in parenthesis for Dad Schooling, Mom Schooling and Household Income. This used 100 replications in Stata 9's bootstrap routine with the force option to allow for weights. Thresholds are also estimated but not reported. All specifications include mother's and father's age at the time of the child's birth in quadratic, indicators of whether the mother or father is currently a smoker, indicator of whether the mother smoked during pregnancy, the number of years the child has been exposed to parental smoking, log of number of children in the household, month of survey dummies and year of survey dummies and region of residence. Exogeneity test is from Smith and Blundell (1986). The residuals from each first stage regression are included in the ordered probit model along with the variables that the first stage equations would have instrumented. Estimation of the model gives rise to an F test of the hypothesis that all of the coefficients on the three residuals are zero. Significant levels: *** 1%, ** 5% and * 10%.

Table 5a   Estimates of Parental Non-Linear Income and Education on Child Ill Health Status: Endogenous

SRH	All	0-3	4-8	9-12	13-15	All	0-3	4-8	9-12	13-15	All	0-3	4-8	9-12	13-15
Mom Schooling	-0.102***	-0.091	-0.037	-0.136*	-0.137*	~	~	~	~	~	-0.661	0.059	0.130	-0.175*	-0.130
	(0.038)	(0.110)	(0.067)	(0.074)	(0.080)						(0.052)	(0.128)	(0.113)	(0.0.97)	(0.128)
Dad Schooling	-0.213	0.494	0.191	-0.524	-0.588	~	~	~	~	~	-0.026	1.159	0.641	-0.618	-0.554
	(0.211)	(0.638)	(0.372)	(0.381)	(0.445)						(0.238)	(0.811)	(0.450)	(0.438)	(0.503)
Household Income	~	~	~	~	~	-14.21***	-2.920	-13.717	-13.810	-21.164*	-14.21***	-4.300	-16.517	-11.247	-19.715*
						(5.283)	(16.290)	(9.479)	(9.547)	(11.732)	(4.563)	(19.502)	(10.703)	(10.661)	(11.093)
Household Income Squared	~	~	~	~	~	0.680***	0.125	0.654	0.655	1.008*	0.688***	0.169	0.766	0.562	0.963*
						(0.259)	(0.795)	(0.466)	(0.470)	(0.577)	(0.225)	(0.944)	(0.523)	(0.518)	(0.540)
Observations	6389	1187	2232	1742	1228	6389	1187	2232	1742	1228	6389	1187	2232	1742	1228
Hansen J Statistic (over ID test)	23.78	12.93	14.50	8.76	10.98	12.47	14.28	10.34	20.05	8.88	37.83	21.11	14.33	32.02	18.33
	(0.008)	(0.228)	(0.152)	(0.555)	(0.359)	(0.255)	(0.160)	(0.411)	(0.029)	(0.544)	(0.009)	(0.391)	(0.813)	(0.043)	(0.566)
F test of residuals (P)	4.10	1.13	2.23	2.00	4.49	11.81	0.76	1.90	2.84	4.78	11.79	2.41	7.23	4.88	6.98
	(0.129)	(0.567)	(0.327)	(0.368)	(0.106)	(0.003)	(0.683)	(0.387)	(0.241)	(0.092)	(0.019)	(0.661)	(0.124)	(0.299)	(0.137)

Table 5b: Probit Estimates of Non-Linear Parental Income and Education on Child having a Chronic Health Condition: Endogenous

CHC	All	0-3	4-8	9-12	13-15	All	0-3	4-8	9-12	13-15	All	0-3	4-8	9-12	13-15
Mom Schooling	-0.124***	-0.166	-0.138	-0.105	-0.119	~	~	~	~	~	-0.104*	-0.123	-0.108	0.000	-0.055
	(0.043)	(0.119)	(0.079)	(0.079)	(0.094)						(0.064)	(0.182)	(0.122)	(0.125)	(0.137)
Dad Schooling	-0.043	-1.687**	0.103	-0.352	0.572	~	~	~	~	~	0.028	-1.504	0.184	-0.042	0.757
	(0.215)	(0.833)	(0.392)	(0.367)	(0.476)						(0.281)	(1.019)	(0.431)	(0.502)	(0.592)
Household Income	~	~	~	~	~	-4.097	0.682	-3.937	-5.924	-1.115	-0.555	2.737	-2.072	-5.834	-2.170
						(5.787)	(22.336)	(12.352)	(12.408)	(13.315)	(5.459)	(20.093)	(11.898)	(13.268)	(12.583)
Household Income Squared	~	~	~	~	~	0.004	-0.081	0.175	0.260	0.042	0.0220	-0.146	0.094	0.256	0.088
						(0.284)	(1.090)	(0.605)	(0.607)	(0.653)	(0.267)	(0.981)	(0.575)	(0.644)	(0.615)
Observations	6389	1187	2232	1742	1228	6389	1187	2232	1742	1228	6389	1187	2232	1742	1228
Hansen J Statistic (over ID test)	10.80	16.31	11.50	8.86	13.21	14.57	21.99	8.93	4.06	8.64	23.42	30.28	16.09	19.24	26.29
	(0.373)	(0.091)	(0.320)	(0.545)	(0.212)	(0.149)	(0.05)	(0.539)	(0.944)	(0.566)	(0.269)	(0.066)	(0.711)	(0.506)	(0.157)
F test of residuals (P)	9.12	7.69	4.08	2.12	2.43	4.47	3.79	2.00	4.32	0.07	8.93	7.17	4.34	3.63	2.49
	(0.011)	(0.021)	(0.130)	(0.347)	(0.297)	(0.107)	(0.150)	(0.368)	(0.115)	(0.964)	(0.063)	(0.127)	(0.362)	(0.458)	(0.478)

Notes: Table 5a reports coefficients from ordered probit models of general health status (1= Very Good, 2=Good, 3=Fail, 4=Bad/Very Bad). Table 5b reports coefficients from probit models indicating whether the child has a chronic health condition . Bootstrapped standard errors used 100 replications in Stata 9's with the force option to allow for weights. Thresholds are also estimated but not reported. All specifications include mother's and father's age at the time of the child's birth in quadratic, indicators of whether the mother or father is currently a smoker, indicator of whether the mother smoked during pregnancy, the number of years the child has been exposed to parental smoking, log of number of children in the household, month and year of survey dummies and region of residence. Exogeneity test is from Smith and Blundell (1986). The residuals from each first stage regression are included in the ordered probit model along with the variables that the first stage equations would have instrumented. An F test of the hypothesis that all of the coefficients on the three residuals are zero. Significant levels: *** 1%, ** 5% and * 10%.

V. Conclusion

In this paper we have investigated the relationship between key parental characteristics of education and income on child health using data from the Health Survey of England (HSE). This is motivated by a large literature, mainly from the US, which suggests a strong parental income gradient in child health which increases with the age of the child. Our work is further motivated by the results in Currie *et al*. (2004) who, based on the same HSE data, find evidence of similar, although smaller, income effects.

In this paper we replicate the main finding of the Currie *et al*. (2004) results – significant effects of income but no significant differences in this across child age groups. These findings do not change much when education is included. Indeed, when we go beyond this to consider endogenous income and education we find larger income effects. We also find some support for the idea that maternal education is important for child health while paternal is not.

Finally, while we find no support for the idea that income effects are larger for the poor in the case where income is treated as exogenous. But, in the endogenous case, we find very pronounced nonlinearity and very large effects of income on the very poor.

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APPENDIX

Table A1 Descriptive Statistics HSE 1997-2002 - Impact of Sample Selection

	All children	Children with parental information	Two parent households	Income data available	White households	Final sample
Child's subjective ill health (1-5)	1.51 (0.66)	1.52 (0.65)	1.48 (0.63)	1.48 (0.63)	1.46 (0.62)	1.45 (0.61)
Child has a chronic health condition	0.23 (0.42)	0.22 (0.41)	0.21 (0.40)	0.21 (0.41)	0.21 (0.41)	0.21 (0.41)
Household log income	9.91 (0.82)	9.90 (0.83)	10.16 (0.69)	10.16 (0.69)	10.19 (0.67)	10.25 (0.66)
Mother's schooling	17.22 (1.74)	17.22 (1.74)	17.30 (1.80)	17.33 (1.80)	17.25 (1.78)	17.33 (1.82)
Father's schooling	17.32 (2.00)	17.32 (2.00)	17.33 (3.01)	17.36 (2.02)	17.26 (1.99)	17.36 (2.04)
Mother's age at birth	35.65 (6.88)	35.65 (6.88)	36.55 (6.48)	36.43 (6.42)	36.52 (6.37)	29.02 (5.14)
Father's age at birth	39.04 (7.30)	39.04 (7.30)	39.00 (7.25)	38.8 (7.15)	38.78 (7.05)	31.21 (6.00)
Mother started smoking before age 16	0.12 (0.32)	0.20 (0.40)	0.17 (0.37)	0.17 (0.38)	0.18 (0.39)	0.15 (0.36)
Mother started smoking between ages 16 and 19	0.12 (0.32)	0.19 (0.40)	0.19 (0.39)	0.19 (0.39)	0.21 (0.41)	0.20 (0.40)
Mother started smoking after age 19	0.05 (0.22)	0.08 (0.28)	0.08 (0.27)	0.08 (0.27)	0.08 (0.27)	0.08 (0.28)
Father started smoking before age 16	0.13 (0.34)	0.22 (0.41)	0.28 (0.45)	0.28 (0.45)	0.29 (0.45)	0.26 (0.44)
Father started smoking between ages 16 and 19	0.08 (0.27)	0.13 (0.34)	0.20 (0.40)	0.20 (0.40)	0.21 (0.41)	0.21 (0.40)
Father started smoking after age 19	0.04 (0.19)	0.06 (0.25)	0.10 (0.30)	0.10 (0.30)	0.09 (0.29)	0.09 (0.29)
Mother smokes	0.19 (0.39)	0.30 (0.46)	0.25 (0.43)	0.25 (0.44)	0.27 (0.45)	0.24 (0.42)
Father smokes	0.12 (0.33)	0.20 (0.40)	0.29 (0.46)	0.29 (0.45)	0.29 (0.45)	0.24 (0.43)
Years exposed to Mother's smoking	1.71 (3.81)	2.81 (4.55)	2.47 (4.41)	2.46 (4.37)	2.63 (4.47)	2.34 (4.32)
Years exposed to Father's smoking	7.16 (4.80)	6.26 (4.99)	5.97 (5.03)	5.91 (5.0)	5.86 (5.03)	5.84 (5.03)
Mother smoked when pregnant	0.01 (0.11)	0.02 (0.14)	0.01 (0.12)	0.02 (0.12)	0.02 (0.13)	0.01 (0.11)
Paternal grandfather smoked	0.69 (0.46)	0.69 (0.46)	0.69 (0.46)	0.69 (0.46)	0.71 (0.45)	0.71 (0.46)
Paternal grandmother smoked	0.46 (0.50)	0.46 (0.50)	0.47 (0.50)	0.47 (0.50)	0.51 (0.50)	0.49 (0.50)
Maternal grandfather smoked	0.66 (0.47)	0.66 (0.47)	0.66 (0.48)	0.66 (0.47)	0.67 (0.47)	0.67 (0.47)
Maternal grandmother smoked	0.47 (0.50)	0.47 (0.50)	0.45 (0.50)	0.46 (0.50)	0.49 (0.50)	0.47 (0.50)
Mother affected by RoSLA	0.48 (0.50)	0.79 (0.41)	0.78 (0.41)	0.79 (0.41)	0.79 (0.41)	0.76 (0.43)
Father affected by RoSLA	0.27 (0.44)	0.45 (0.50)	0.67 (0.47)	0.68 (0.47)	0.68 (0.46)	0.66 (0.47)

Note: Means and standard deviations (in parentheses) reported.

Table A2 *Age Left Full-Time Education in LFS and HSE Surveys*

	Age FATHER left full-time education (percent)		Age MOTHER left full-time education (percent)	
Age	LFS	HSE	LFS	HSE
15	7.06	8.81	3.71	4.85
16	46.98	43.71	43.21	43.51
17	8.95	8.82	12.98	11.76
18	11.53	10.44	16.67	16.60
19	2.88	7.84	3.87	6.97
20	2.05	0.36	2.07	1.50
21	6.98	20.03	7.02	14.80
22	6.28		5.74	
23	3.0		2.36	
24	1.87		1.08	
25	2.41		1.27	
Total	46,572	7005	46,572	7005

Figure A1a: Household Income Bands- LFS 1997-2002 Data

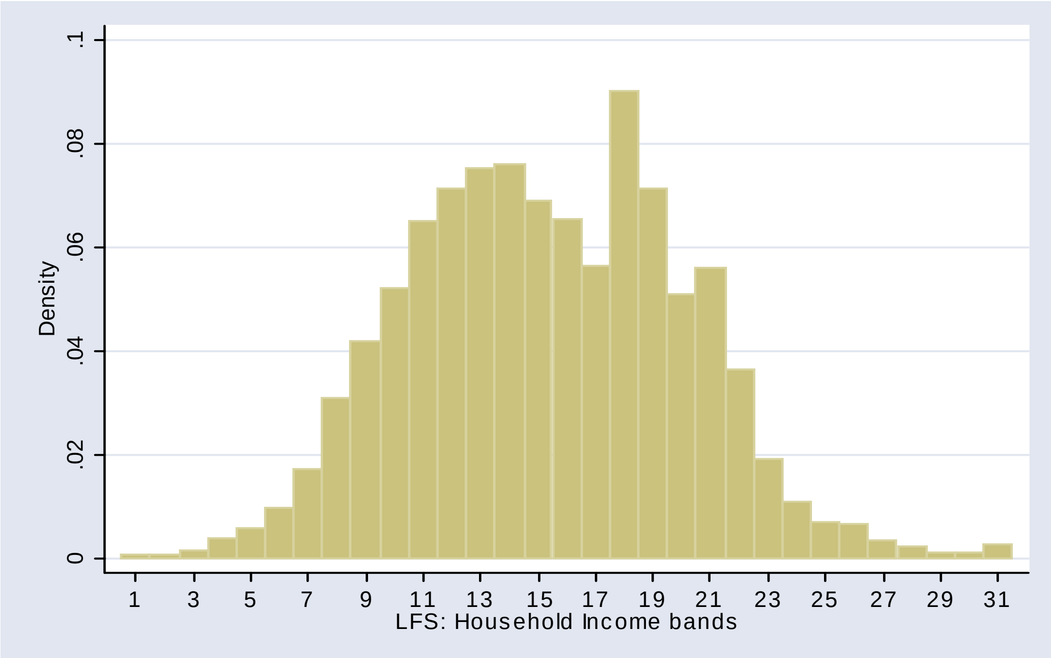

Figure A1b: Household Income Bands- HSE 1997-2002 Data

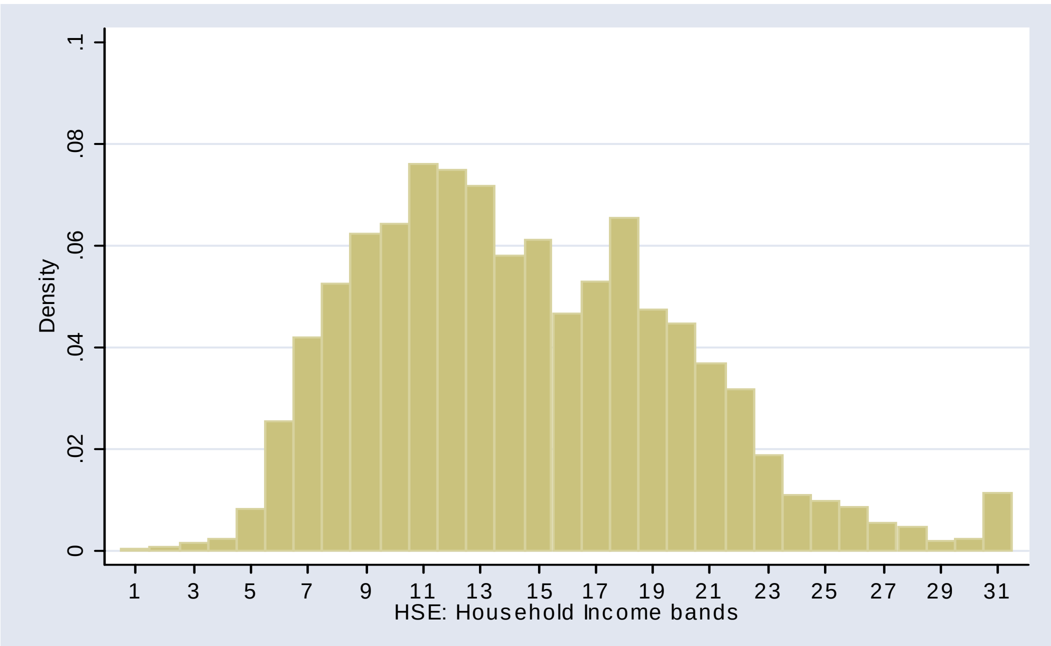

Figure A2a: Age Father Left Full-Time Education- LFS Data

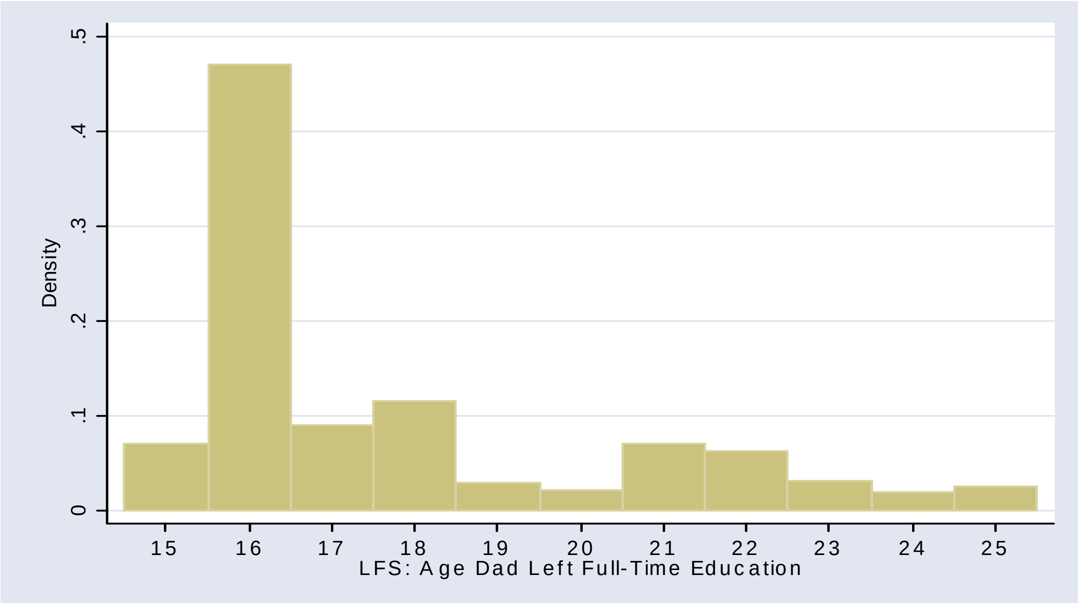

Figure A2b:Age Father Left Full-Time Education- HSE Data

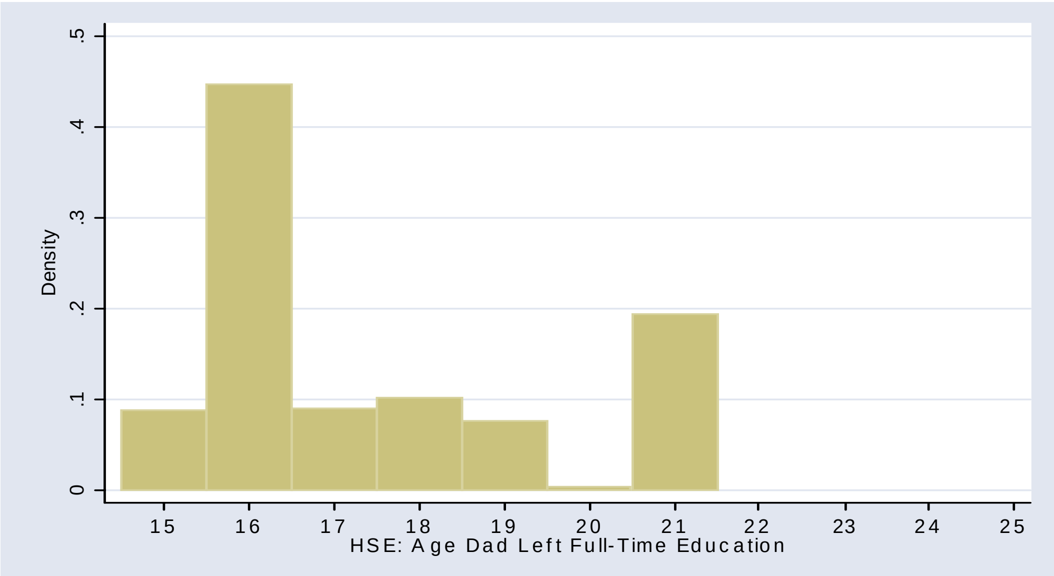

Figure A2c:Age Mother Left Full-Time Education- LFS 1997-2002 Data

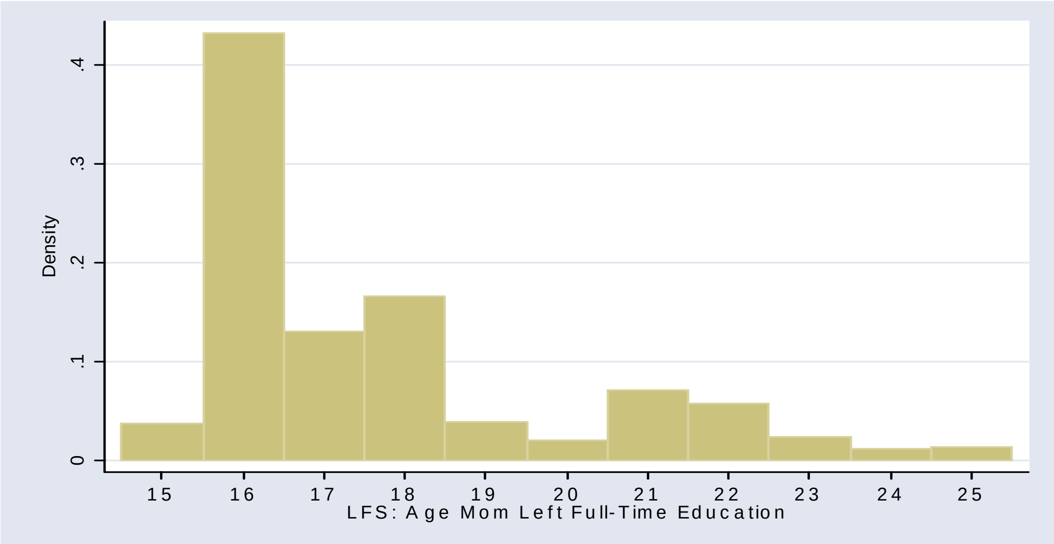

Figure A2d: Age Mother Left Full-Time Education- HSE 1997-2002 Data

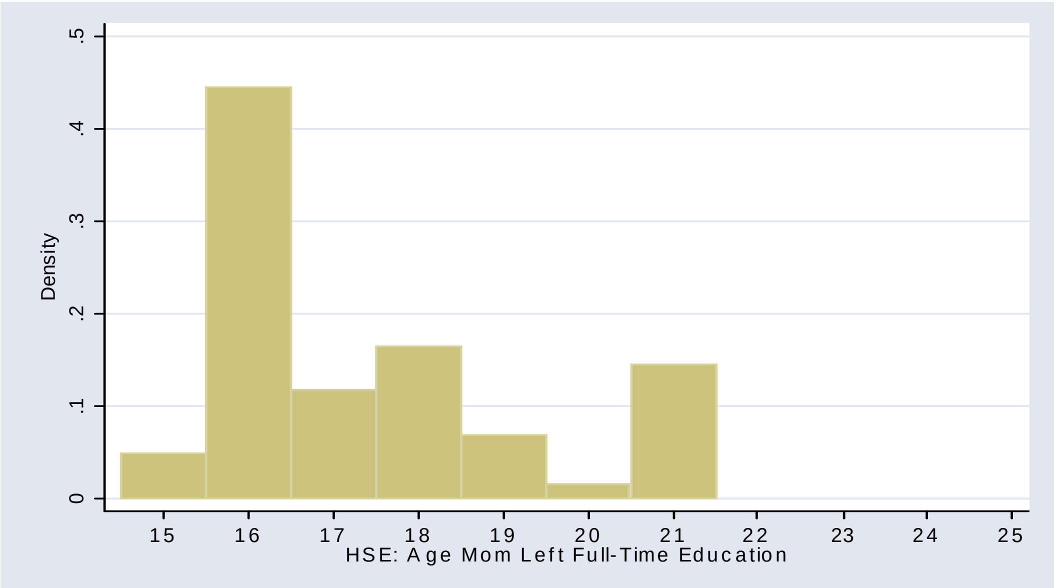

Figure A3a HSE Age Left School by Birth Month: Males born Jan 1956-Dec 58

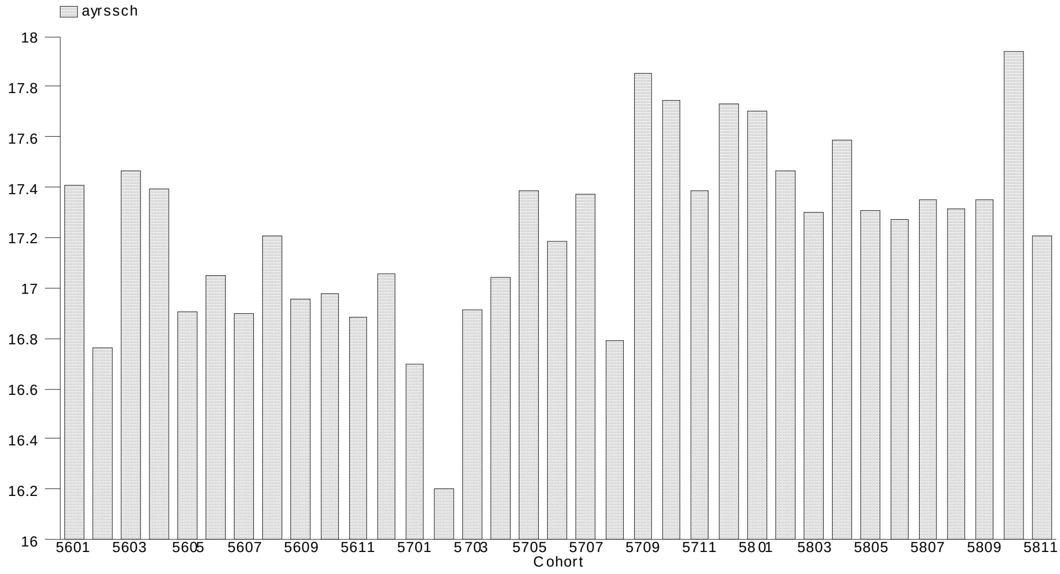

Figure A3b HSE Age Left School by Birth Month: Females born Jan 1956-Dec 58