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LONG-TERM EFFECTS OF COGNITIVE SKILLS, SOCIAL ADJUSTMENT AND SCHOOLING ON HEALTH AND LIFESTYLE: EVIDENCE FROM A REFORM OF SELECTIVE SCHOOLING

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Abstract
Members of the National Child Development Study (NCDS) cohort attended very different types of secondary school, as their schooling lay within the transition period of the comprehensive education reform in England and Wales. This provides a natural setting to explore the impact of educational attainment and of school quality on health and health-related behaviour later in life. We use a combination of matching methods and parametric regressions to deal with selection effects and to evaluate differences in adult health outcomes and health-related behaviour for cohort members exposed to the old selective and to the new comprehensive educational systems.

JEL codes: I12, I28, C21
Keywords: Health; Education; Comprehensive schooling; Cognitive ability; Non-cognitive skills; NCDS

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The National Child Development Study (NCDS) follows a cohort of around 17,000 individuals, who were born in the week of 3rd March 1958, from birth up until age 46. Members of the cohort were aged 11 in March 1969. They went through secondary schooling during the 1970s and attended very different types of school. The cohort's secondary schooling lay within the transition period of the comprehensive education reform that was implemented in England and Wales from the mid-1960s[1]. This was a major reform, aimed at reducing inequality of opportunity, which transformed secondary education. Comprehensive schooling was not implemented simultaneously nationwide, hence some of the cohort members attended the highly selective tri-partite system of state-funded education, which comprised grammar schools, secondary modern schools and a dwindling number of technical schools. Among members of the NCDS cohort, 12 per cent attended grammar schools at age 16, 25 per cent attended secondary moderns and 57 per cent attended comprehensives. A further 6 per cent of NCDS cohort members attended private fee-paying schools, independent of the state educational system and reforms[2].

This paper is concerned with evaluating the impact of educational attainment and of attending qualitatively different types of school on health outcomes and health-related behaviour later in life. It contrasts the health outcomes of the NCDS cohort members who experienced the selective system with those who experienced the comprehensive system of education. This is in line with the strategy of using major educational policy reforms to identify causal effects of education on health. Similar identification strategies have been used in the recent literature, often focusing on changes in the minimum school leaving age and related reforms (see e.g., Lleras-Muney, 2005; Arendt, 2005, 2008; Oreopoulos, 2006; Albouy and Lequien, 2008; Mazumder, 2008; Silles, 2009; Van Kippersluis *et al.*, 2009; Chou *et al.*, 2010). Another recent strand of papers, such as Trannoy *et al.* (2009) and Rosa Dias (2009), suggests that, in addition to family background, differences in education may be a leading cause of inequality of opportunity in health.

The comprehensive education reform was aimed at reducing inequality of opportunity by improving the quality of schooling available to children from disadvantaged

[1] Data on those who attended school in Scotland at age 16 are not used: the Scottish educational system of the 1960s and 1970s was structurally very different from the one experienced by the other NCDS cohort-members, and comprehensive schooling was introduced earlier, preventing a legitimate comparison of types of school, educational qualifications and outcomes.

[2] Historically the leading private schools within the independent sector have been known as "public schools" in Britain. To avoid confusion we use the label "private schools" throughout. Most of our analysis focuses on those who went to state schools (grammar, secondary modern and comprehensive).

backgrounds. These reforms have been evaluated in terms of their direct impact on educational attainment and subsequent impact on labour market outcomes (see e.g., Kerkchoff *et al.,* 1996; Jesson, 2000; Dearden *et al.,* 2002; Galindo-Rueda and Vignoles, 2004, 2005; Pischke and Manning, 2006). Here we focus on a possible indirect consequence of the reform, by estimating the effect of educational attainment and of quality of schools on adult health outcomes. We do not evaluate the impact of comprehensive schooling *per se* but use the consequent variation in quality of schooling and educational attainment as a natural experiment to explore the impact on health and health-related behaviour.

The association between educational attainment and a range of health outcomes is well documented in the economic literature, as reviewed by Grossman (2006) and Cutler and Lleras-Muney (2008). Studies such as Currie and Moretti (2003), Lleras-Muney (2005), Arendt (2005), Lindeboom *et al*. (2009), Kenkel *et al*. (2006), Oreopoulous (2006), Grimard and Parent (2007), and Webbink *et al.* (2010) additionally find evidence to suggest that part of this relationship may be causal. Mazumder (2008) and Cutler and Lleras-Muney (2008, 2010) outline some of the channels through which education may have an impact on health and health-related behaviours: these include the effect of education on employment, both the type of jobs available to people and their lifetime earnings; the effect on relative social status; and the effect on use of health care and other health-related behaviours, through the acquisition of specific health knowledge, through improved information processing and decision-making skills, and through the influence on behavioural responses to future costs and benefits and to perceived health risks. Less is known, however, about whether quality of schooling also has an impact on health and how this interacts with the effect of educational attainment[3].

We use a matching framework to pre-process the data: using a combination of coarsened exact matching along with propensity score and Mahalanobis matching (Ho *et al.*, 2007). This is important because our own descriptive analysis, as well as previous work with the NCDS, shows an imbalance between the observed pre-schooling characteristics of those who attended comprehensive and selective schools (Pischke and Manning, 2006). This is reinforced by regressions for cognitive ability at age 7 and 'value-added' regressions of ability at age 11, given ability at age 7, of the type used by Pischke and Manning (2006). First we use matching to improve the balance of a broad set of observed pre-schooling characteristics,

[3] This gap in the literature is acknowledged in Cutler and Lleras-Muney (2008: p. 22). The impact of quality of schools on other outcomes has received considerable attention (see e.g., Angrist and Lavy, 1999; Black, 1999; Card and Krueger, 1992; Hoxby, 2000).

including cognitive ability measured at age 7, between those who attended comprehensive schools and a control group who attended selective state schools. Then, to explore heterogeneity in the impact of attainment, those who attended grammar schools are matched with a comparable group who attended comprehensive schools and, likewise, those who went to secondary modern schools are matched with a comparable group from comprehensive schools. A key matching variable is cognitive ability at age 11, which is closely linked to likely performance in the 'Eleven Plus' entry examination. But, rather than using absolute cognitive ability at age 11, which is likely to be contaminated by a form of post-treatment bias due to the 'coaching effect' for those who actually faced the Eleven Plus examination, we use the relative ability ranking of those within the selective and non-selective systems. The success of our matching strategy is assessed using value-added regressions and other diagnostics.

The use of matched samples is coupled with parametric modelling of health outcomes and health-related behaviour. Our study design is structured to answer the following research questions:

- On average, what is the overall impact of educational attainment, captured by a detailed measure of the highest qualification attained, and of the quality of schooling on adult health and health-related behaviour? This comparison uses matching to balance the sample and controls for an extensive set of observed pre-schooling characteristics using linear and nonlinear regression methods.
- The key feature of the pre-comprehensive system was the distinction between attending grammar and secondary modern schools: is there heterogeneity in the impact of educational attainment, particularly according to the type of school attended? This is explored by creating matched samples, linking those who actually went to grammar or secondary modern schools with comparable counterparts who went to comprehensive schools and then applying parametric models to these matched sub-samples.

Our results show that cognitive ability at age 7 is not significantly associated with health outcomes but there is a strong association with non-cognitive skills, as reflected by social adjustment as a child. Those who had problems with social adjustment are more likely to suffer both physical and mental illness as adults. There is also evidence of a socioeconomic gradient in illness by father's social class. Those with poorer social adjustment as children are more likely to become smokers and those whose father came from the higher or middle

social classes are less likely to become smokers. When those who went to grammar and to secondary modern schools are matched separately to comparable groups who attended comprehensives there is evidence of heterogeneity in the impact of educational attainment, as measured by qualifications. Attainment has an impact on adult health-related behaviours for both groups, in particular on smoking, drinking and diet. But attainment only has an impact on adult health, both long-standing illness and mental health problems, for those who either did or would have attended grammar schools.

1. Comprehensive schooling reforms and the 1958 cohort

The comprehensive education reform, put into place during the 1960s and 1970s in England and Wales, replaced the selective educational system with a non-selective, comprehensive system of secondary schooling. This policy reform was implemented at different speeds at the local level: some local education authorities (LEAs) implemented it quickly, but others resisted the change, some for decades. Because of this slow and uneven transition, the two systems co-existed for a long period of time and approximately 40 per cent of the NCDS cohort, who entered state secondary schools in 1969, experienced the pre-reform selective system; the remaining 60 per cent attended comprehensive schools.

Grammar schools were academically oriented state schools that provided teaching for the entire age range 11-18, including a sixth form for Advanced level ('A-level') studies, and prepared pupils to go on to higher education. Admission into these schools was determined by an exam taken at age 11 (the 'Eleven Plus')[4]. Pupils whose examination score did not permit entry into a grammar school attended either secondary modern schools, which were less academically oriented and covered the ages 11-16 or, in a small minority of cases, vocational schools aimed at providing training and technical apprenticeships[5].

The different types of schools varied in their curricula, examinations and academic environment, along with other qualitative differences. Table 1 shows that, among the schools attended by the NCDS cohort members used in our analysis at age 16, 79 per cent of private

[4] Following much controversy, the selective system went into decline in the 1960s and 1970s, until the Eleven Plus was abolished as a national examination in England and Wales by the 1976 Education Act. Despite this, the selective system and the existence of grammar schools has persisted in certain areas, such as Kent.

[5] In a few cases, pupils whose CSE grades were sufficient transferred to grammar schools or sixth form colleges to complete their A-levels.

schools and 69 per cent of grammar schools were single sex, while only 13 per cent of comprehensive schools were single sex. Streaming of classes by academic ability was common in secondary modern schools (42 per cent) and comprehensive schools (39 per cent) but rare among grammar schools (17 per cent). Some comprehensive schools were former secondary moderns (26 per cent) or grammar schools (19 per cent), with the rest being newly created.

Table 1
Characteristics of different types of schools (as attended by NCDS cohort at age 16)

	Grammar	Sec Modern	Comprehensive	Private
% single sex	68.7	26.1	13.8	78.9
% with ability streams	16.6	42.3	38.8	22.8
% former grammar	-	-	19.0	-
% former sec modern	-	-	26.3	-

Note: The percentages are computed using all available observations for the relevant variables.

The comprehensive reform has received considerable attention in the literature and its impact on educational outcomes has been assessed. The evidence for the impact on educational outcomes is mixed. Kerckhoff *et al.* (1996) review a series of LEA case studies and use NCDS data to examine the association between types of secondary schools and exam performance at age 18. After controlling for a wide range of observables, including measures of cognitive ability prior to secondary education, the authors find no association between the average academic achievements of pupils in selective and in comprehensive schools. However, when the impact of the reform is examined for different quantiles of ability, the study finds that high-ability pupils performed relatively worse and low-ability pupils performed relatively better in comprehensive schools. Jesson (2000) implements a value-added approach that corroborates most of these results. Accounting for a rich set of controls, the paper finds no significant differences between the exam performance of pupils in the selective and comprehensive systems of education. Nevertheless, pupils in secondary modern schools performed worse in exams than their comprehensive school counterparts.

Galindo-Rueda and Vignoles (2004) investigate the effects of the comprehensive reform on educational outcomes; the data used are from the NCDS and their research strategy is based on matching and instrumental variables estimators. Two instruments are

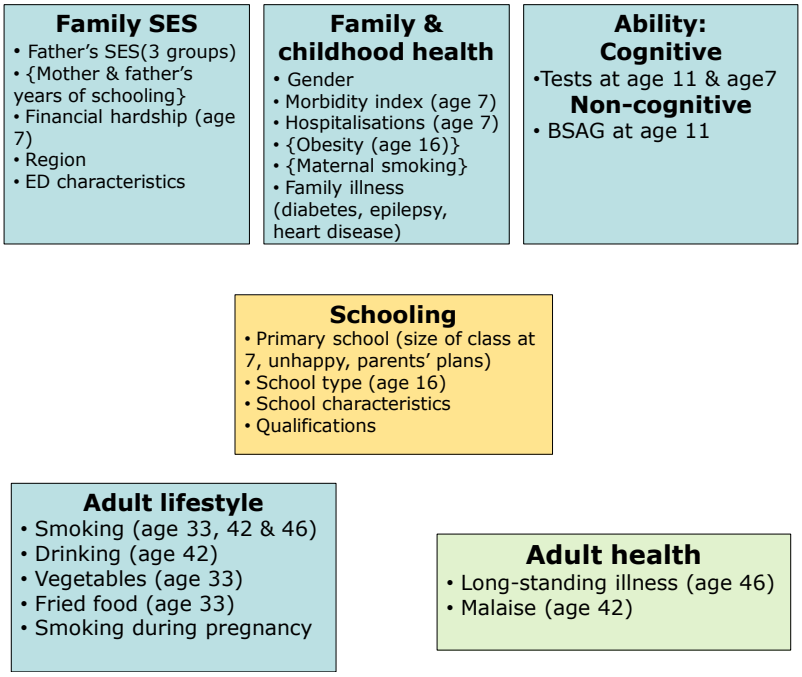
used for type of schooling: Conservative Party control of the cohort members' LEA (which the authors claim to be negatively correlated with the probability of attending a comprehensive school, but orthogonal to the educational outcomes) and the share of comprehensive schools in a cohort-member's LEA. Although point estimates of the policy impact are shown to be sensitive to the choice of instrument, the results suggest that the most able 20 per cent of pupils did relatively better in the selective school system than they would have done in a comprehensive one; no statistically significant effect of the reform was found for pupils in the lower ability quantiles. Maurin and MacNelly (2007) add to this body of evidence by evaluating a different school reform, implemented in Northern Ireland in the late 1980s. The educational system in Northern Ireland remained selective, with the policy reform designed to increase the number of pupils allowed to attend a grammar school by 15%. The paper compares the educational outcomes between Northern Ireland and England before and after the reform (using the English comprehensive education system as a control group); the wider access to grammar schools within the Northern Irish selective system is found to have a large positive impact on educational attainment.

Pischke and Manning (2006) also use NCDS data but they question the main results of earlier work. First, contrary to Kerckhoff *et al.* (1996), they find that comprehensive areas were systematically poorer and populated by children with lower ability than selective areas. The policy impacts reported in the literature may thus be the result of selection bias. Second, using a series of placebo tests based on value-added regression for ability, they find that the comparison between areas exposed to different degrees of educational selectivity tend to produce the same results regardless of whether the educational outcomes are measured after the reform or before it. We draw on Pischke and Manning's (2006) placebo tests to assess whether our empirical strategy achieves the goal of making valid inferences about the impact of educational attainment and of quality of schooling.

2. NCDS data and study design

Members of the National Child Development Study (NCDS) cohort were all born in the week of 3rd March 1958. Seven waves of interviews have been carried-out when cohort members were 7, 11, 16, 23, 33, 42 and 46 years old. The study compiles detailed information on the cohort-members' childhood health, parental background, and educational achievement. It also includes self-reported information on social status in adulthood, health-related behaviour and a wide range of health outcomes. The NCDS gathers data from a variety of sources. In the early waves this includes information from parents, medical examinations, tests of ability and from the child's school. In the later waves these are augmented by interviews with the cohort members and data linked from the Census.

Fig. 1. *Schematic view of study design and NCDS variables*



Note: Items in braces are not used in our main analysis but are used in the checks for robustness.

The structure of the NCDS is well-suited to our study design, which is summarised in Figure 1. Our goal is to identify the impact of educational attainment and of the characteristics of secondary schooling experienced by members of the 1958 cohort on

outcomes later in life, with a focus on health-related behaviour and adult health. The NCDS allows us to condition on a broad set of pre-treatment factors that reflect early life circumstances, occur prior to secondary schooling, and are not influenced by subsequent educational choices (Dearden *et al.*, 2002). These factors fall into three broad groups: measures of family socioeconomic status and the local environment during early childhood; measures of childhood health and use of health care and health within the family; and measures of cognitive and non-cognitive skills and social adjustment of the child. In addition we condition on characteristics of the individual's primary education. The aim is to estimate the impact of the type and characteristics of the secondary schooling experienced by each individual on their adult outcomes, both the intermediate outcomes, such as smoking at age 42, and final health outcome, such as long-standing illness at age 46. The specific variables that are available within each of the broad categories are described below and are listed in full in Table A.1 in the Appendix.

2.1 Childhood health and parental background

Rich information is available to characterise the cohort members' childhood health and parental circumstances, which have both been linked to adult health outcomes (see e.g., Case *et al.*, 2005; Currie and Stabile, 2004). Following Power and Peckham (1987), indicators of morbidity are constructed by aggregating twelve categories of health conditions, that affect the child at ages 7 and 11. Dummy variables for the occurrence of diabetes, epilepsy and other chronic conditions among parents and siblings are included in order to account for the incidence of hereditary conditions in the cohort member's family. Information on obesity at age 16 is also available, as well as an indicator variable for maternal smoking after the fourth month of pregnancy.

In terms of parental background, the NCDS allows us to trace the social class and the years of schooling of the parents of cohort members. We use the father's occupational socioeconomic status (SES), measured in three groups (see Carneiro *et al.*, 2007). Following Case *et al.* (2005) and Lindeboom *et al.* (2009), this information is complemented by data on the incidence of household financial difficulties during the cohort member's childhood and adolescence.

2.2 Cognitive ability, non-cognitive skills and social adjustment

Auld and Sidhu (2005) argue that failure to control for cognitive ability will confound the relationship between health and education. Non-cognitive skills have also received

considerable attention in recent studies (see for example, Heckman *et al.,* 2006; Heckman, 2008) and have been linked to health and health-related behaviours (see e.g., Carneiro *et al.*, 2007; Coneus and Laucht, 2008; Cutler and Lleras-Muney, 2010; Keaster, 2009). Among these non-cognitive skills, social adjustment is of particular relevance for schooling and health (Carneiro *et al.*, 2007).

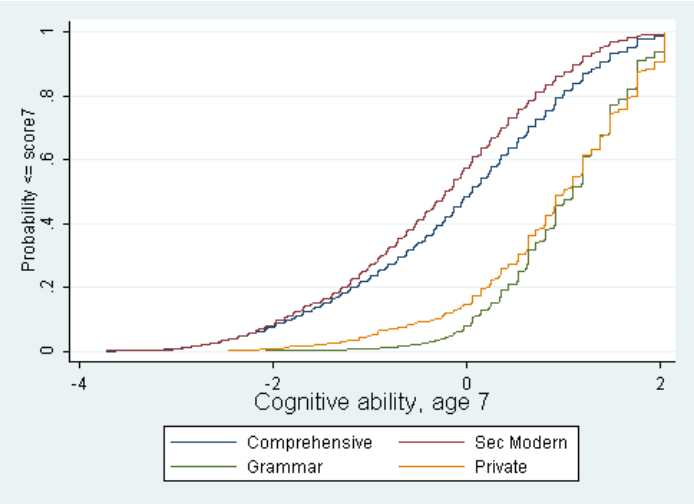
The NCDS provides measures of cognitive and non-cognitive ability collected before respondents began their secondary schooling. Scores of ability tests taken at age 7 and 11 are available on a series of cognitive dimensions: mathematics, reading, copying designs and general ability. These test scores are highly correlated at the individual level leading to problems with precision in econometric models, due to multicollinearity. To avoid this, we follow Galindo-Rueda and Vignoles (2005) and use principal components analysis to construct a single measure of cognitive ability using the first principal component[6]. The empirical distributions of these combined scores, for the tests at ages 7 and 11, split by type of secondary school attended at age 16 are presented in Figure 2.

The similarity of the distribution of scores between the two ages and the pattern across schools provides confidence in their face validity: ability scores are lowest among those who attended secondary moderns, followed by those who attended comprehensives. The distributions for grammar and private schools are similar, but with more children in both the lower and upper tails among private school pupils.

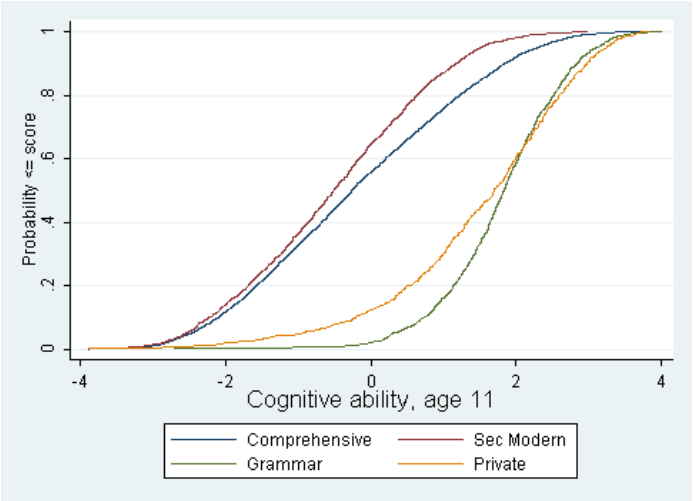
It should be emphasised that the three dimensions of cognitive ability used to construct our index – mathematics, reading and general ability – along with the fact that the index derived from the first principal component gives equal weight to each dimension, mirrors the three elements of the Eleven Plus examination. So the cognitive ability score at age 11 can be viewed as a proxy for performance in the Eleven Plus for those who took the examination.

[6] For example, with the scores at age 11, the first principal component accounts for 85 per cent of the joint variation and, strikingly, the weights attached to the three dimensions – 0.583, 0.567 and 0.582 – are virtually identical.

Fig. 2. *Empirical distributions of cognitive ability scores by type of school*



(a) Age 7 scores

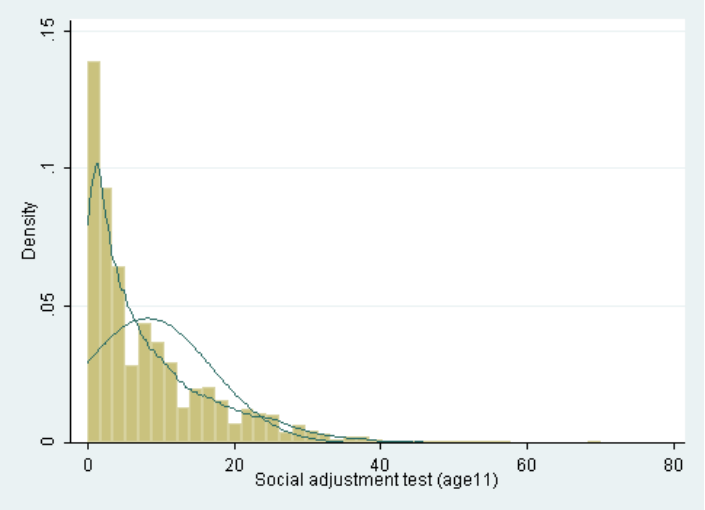


(b) Age 11 scores

Following Carneiro *et al.* (2007) the score for the Bristol Social Adjustment Guide (BSAG) is used as a measure of social skills. This is a measure of problems with social adjustment at age 11: teachers were asked to report whether the child had problems in twelve behavioural domains such as hostility towards children and adults, anxiety, withdrawal, 'writing off' adults, unforthcomingness, depression, restlessness, acceptance by adults, inconsequential behaviour, as well as miscellaneous psychological and nervous symptoms (Stott, 1987). One point is attributed to each positive answer; points are then summed to

obtain the BSAG social maladjustment score[7]. The distribution of the BSAG measure is presented in Figure 3, which shows that the distribution is highly skewed with relatively few respondents having high scores for social maladjustment.

Fig. 3. *Empirical density of Bristol Social Adjustment Guide (BSAG)*



Note: The Figure shows the histogram of the BSAG score, a kernel density estimate and a normal curve.

2.3 Local area characteristics

The NCDS includes information about the area in which the cohort-members lived, aggregated at different geographic levels. Data on the cohort members' UK standard region is available for all the waves of the study. For the years 1971 and 1981, NCDS survey data was linked to the Census, allowing a detailed demographic and socioeconomic characterisation of each individual's local area, at the electoral constituency level, local education authority level and census enumeration district level (the smallest unit for which census statistics were then available, with an average population of about 500) [8]. Measures include the percentage of the local population who are unemployed or long-term sick, working women, employed in particular sectors (manufacturing and agricultural), who are in different occupational groups (professional/managerial, other non-manual, skilled manual, semi-skilled, unskilled), owner

[7] The NCDS data dictionary notes that that this the scores "are added together to give a figure which indicates, fairly crudely, the total amount of behavioural deviance (or maladjustment?) as measured by the Guide".

[8] This small area data are available under a special licence, which imposes restrictions on the handling and usage of the data. Details can be found at: http://www.cls.ioe.ac.uk/studies.asp?section=0001000200030015.

occupiers, council tenants, non-whites, and immigrants (see Dearden *et al.*, 2002; Galindo-Rueda and Vignoles, 2004; Pischke and Manning, 2006).

2.4 Educational attainment and quality of schooling

The NCDS includes information on the educational attainment and qualifications awarded to cohort members. This was collected in the 1978 Survey of Public Exams, based on a questionnaire sent to the school attended by NCDS respondents at wave 3. The usual practice, in the literature that uses the NCDS, has been to differentiate individuals according to broad categories of educational attainment: Certificates of Secondary Education (CSE), O-levels, A-levels and university degree or equivalent[9]. We adopt a richer classification and the information on educational achievement in secondary education is further disaggregated into thirteen categories, ordered according to the grades obtained and number of passes[10]. In our empirical analysis we use the simple 0-12 scaling of this variable as a parsimonious measure of educational attainment[11]. The distribution of this measure is shown in Table 2.

Table 2
Educational attainment: highest secondary qualification
(NCDS variable E386)

	%
No grade at CSE, GCE O or A levels	20.6
1+ passes at O level, grades 4 or 5 only	0.6
1+ passes at CSE, grades 4 or 5 only	8.2
1+ passes at CSE, grades 2 or 3	9.5
5+ passes at CSE, grades 2 to 5	13.6
1-4 passes at GCE O level or CSE grade 1	25.5
5 or 6 passesGCE O level or CSE 1	5.0
7+ passes at GCE O level grades A-C, or CSE grade 1	3.5
1 pass at A level, grades A-E	2.9
2 passes at A levels, up to 8pts	3.7
3+ passes at A levels, up to 8pts	2.7
2 passes at A levels and 9+ pts	0.2
3+ passes at A levels and 9+pts	4.0
	N=11,086

Note: A level points are allocated as 5, 4, 3, 2 and 1 for grades A-E respectively.

[9] CSEs and O-level (Ordinary levels) were secondary education qualifications corresponding, typically, to 11 years of education; CSEs were academically less demanding than O-levels. A-levels (Advanced levels) are a qualification which corresponds to 13 years of education. Completion of A-levels is ordinarily a prerequisite for university admission.
[10] This is variable 'E386' in the NCDS data dictionary.
[11] We have also used models with dummy variables for each category to check the robustness of the results.

Our analysis of the impact of secondary schooling controls for information about the individual's experience in primary school as well as their parents' educational aspirations for their child (see Dearden *et al.,* 2002). This includes the number of children in the child's primary school class at age 7 in 1965, whether parents reported that their child was unhappy at school in 1965, and an indicator of the parents' aspirations for the child, indicating whether they wished the child to continue beyond the minimum school leaving age.

Type of secondary schooling is captured by indicators of the school attended at age 16 (in 1974): secondary modern, grammar, comprehensive or private. This classification is augmented by information on the characteristics of the school, including the teacher/pupil ratio, the ratio of expelled pupils to the total number, and indicators of whether the school was single sex and whether classes were streamed by ability (see Dearden *et al.,* 2002). It is these measures that are used to capture quality of schooling in the regression models[12].

2.4 Intermediate outcomes: health-related behaviours

The NCDS contains self-reported information on a series of health-related behaviours which may be influenced by schooling and go on to affect adult health. The survey includes data on the number of cigarettes smoked per day, average units of alcohol consumed per week[13] and dietary choices, such as the frequency of consumption of fried food and vegetables. These data are only available in the latter four waves of the study, once respondents are aged 23 and above. The other measure of health-related behaviour relates only to the women in the cohort: an indicator for whether mothers, of any age up to 42, smoked during their pregnancies.

A particular focus is on smoking which is the largest cause of avoidable premature death in the UK. We have information on smoking at each of the waves 4-7, spanning ages 23 to 46. As there is item non-response at each wave using a combined measure leads to loss of sample size, so we have decided to focus on smoking at age 42 (wave 6). The prevalence of smoking at age 42 is 25 per cent. Of those with available data on smoking for waves 5-7 (ages 33, 42 and 46) 69 per cent never smoked. Among those who smoked at some point, 74 per

[12] The literature on the impact of school resources, as reflected in the pupil-teacher ratio, on educational attainment in the NCDS provides mixed results (Feinstein and Symons, 1999; Dearden *et al.,* 2002; Dustmann *et al.,* 2003).

[13] NCDS respondents are asked about their weekly consumption of a wide range of alcoholic drinks (glasses of wine, pints of beer and so forth). These are then converted to units of alcohol using the UK National Health Service official guidelines that are available at: http://www.nhsdirect.nhs.uk/magazine/interactive/drinking/index.aspx .

cent reported smoking at age 42. The remainder are mostly those who had smoked at age 33 but not at 42 or 46[14]. So our measure captures those whose damaging health-related behaviour persists into their mid-forties.

2.5 Main outcomes: adult health

Our principal measure of health in adulthood is self-reported long-standing illness or disability at age 46. Information on the particular medical condition associated with the long-standing illness is available and classified according to the International Classification of Diseases (ICD-10). Table 3 shows that the conditions most often listed as the source of the long-standing illness are problems with the musculoskeletal system (25.7 per cent), circulatory system (11.8 per cent), respiratory system (11.4 per cent) and metabolic problems (9.5 per cent), of which 70 per cent suffer from diabetes.

Mental health in adulthood is also taken into account as an outcome through respondents' answers to a series of questions from the Cornell Medical Index Questionnaire, each targeting a particular mental ailment. The number of positive answers given, at age 42, is then used as a malaise score along the lines of Carneiro *et al.* (2007). The malaise score is a measure of psychiatric morbidity (with a special focus on depression), developed at the Institute of Psychiatry from the Cornell Medical Index (Rutter *et al.*, 1970). The NCDS team suggest the use of a severity scale: individuals are considered normal if they score between 0 and 7 points and depressed if they score between 8 and 24 points (Rodgers *et al.*, 1999)[15]. In our data the malaise index, at age 42, ranges from 0 to 23, with a mean of 3.4.

[14] To check robustness all of our analyses were repeated with an indicator of smoking in any of the waves 5-7. The prevalence of smoking in any of these waves is 30 per cent. Results for these analyses are not presented here. The sample sizes are smaller but otherwise results are comparable to the ones for smoking at wave 6. The same applies to using the prevalence of smoking at wave 7 rather than wave 6.

[15] Carneiro *et al.* (2007) define an indicator variable for depression based on this rule of thumb.

Table 3
Breakdown of long-standing illness (LSI) by percentage with specific main conditions (ICD-9)

	Wave 7 (age 46)
Infectious & parasitic diseases	0.7
Neoplasms	1.6
Diseases of blood & immune mechanism	1.5
Endocrine, nutritional and metabolic	9.5
Mental and behavioural disorders	5.9
Nervous system	5.9
Eye, ear and mastoid process	4.6
Circulatory system	11.8
Respiratory system	11.4
Digestive system	5.5
Skin	2.1
Muscoloskeletal system	25.7
Genitourinary system	2.0
Congenital malformations	0.3
Undiagnosed illness	1.8
Injury, poisoning etc	5.3
Other LSI/uncoded	4.3
	N=2990

3. Sample selection and balanced samples

3.1 Sample selection and non-response

Due to sample attrition and especially due to patterns of item non-response, the number of missing values in the variables of interest is large. This reduces the size of the estimation sample considerably; a feature of the data that has been acknowledged in previous studies that use the NCDS and that use similar sample sizes (Case *et al.*, 2005; Cutler and Lleras-Muney, 2010; Dearden *et al.,* 2002; Galindo-Rueda and Vignoles, 2004, 2005; Lindeboom *et al.*, 2009; Pischke and Manning, 2006). Nevertheless, the periodic reports produced by the NCDS Advisory Panel, as well as recent research papers that have analysed the implications of non-random attrition, have concluded that this is not serious source of bias for models based on the data (for example, Case *et al.*, 2005; Lindeboom *et al.*, 2009; Plewlis *et al.,* 2004). In their study of educational attainment and wages Dearden *et al.* (2002, p.5), who condition on a similar set of variables to us, conclude: "Given the large array of characteristics relating to ability and background, we have reasonable grounds to believe that, in our analysis, attrition is exogenous, given the observables."

Tables 4 and 5 compare the sample means for selected outcomes and some of the key control variables used in the paper for the estimation sample used in the econometric analysis and for all other available observations for each variable. This gives a sense of the impact of item non-response. Table 4 shows that the prevalence of long-standing illness is very similar across the two samples, it also shows how the prevalence grows from 15 per cent at wave 5 to 34 per cent at wave 7. The malaise index and the prevalence of smoking at wave 6 and over waves 5-7 are also comparable over the samples. Table 5 shows that individuals in the estimation sample are comparable to those in the rest of the sample in terms of the kind of schools they attended but there is a notable difference in the cognitive ability score, with the estimation sample having a higher average score.

Table 4
Sample means for outcomes

	Estimation sample	All other observations
LSI wave 7	0.34	0.35
	(n=2832)	(n=4663)
LSI wave 6	0.27	0.29
	(n=2700)	(n=6159)
LSI wave 5	0.15	0.15
	(n=2593)	(n=6286)
Malaise wave 6	3.35	3.63
	(n=2689)	(n=6103)
Smoker wave 6	0.21	0.27
	(n=2698)	(n=6152)
Smoker waves 5-7	0.28	0.32
	(n=2377)	(n=3695)

Table 5
Sample means for type of schooling and cognitive ability

	Estimation sample	All other observations
Comprehensive	0.55	0.57
Secondary modern	0.24	0.25
Private school	0.06	0.07
Attainment	4.76	3.95
Single sex school	0.28	0.27
Ability streams	0.35	0.38
Pupil-teacher ratio	0.06	0.06
Expelled ratio	0.0003	0.0004
Cognitive ability age 7	0.25	-0.02

3.2 Balance of covariates between selective and non-selective schools

Pischke and Manning (2006) have drawn attention to the fact that there may be an imbalance in the pre-schooling characteristics of the NCDS respondents who went to selective versus non-selective schools. They find that comprehensive areas were systematically poorer and populated by children with lower educational achievement than selective areas. In this section we explore this imbalance and adopt a matching approach to preprocess the data and improve balance.

One measure that is commonly used to assess the balance of the distribution of covariates in a treated (x^1) and a control group (x^0), before and after matching, is the percentage bias, or normalised difference in means (Rosenbaum and Rubin, 1983; Lalonde, 1986)[16]:

$$\frac{\bar{x}^1 - \bar{x}^0}{\sqrt{(Var(x^1) + Var(x^0)}}.100 \qquad (1)$$

The first column of results in Table 6 shows the percentage bias measure for the unmatched data in our estimation sample for some of the key pre-schooling variables: cognitive ability at 7, the BSAG score, father's social class and ill health at age 7. These reveal fairly substantial imbalance between those who went to comprehensive schools and those who went to selective state schools, with the percentage bias being as high as -16.8 per cent for cognitive ability. It is notable that the percentage bias is even greater, at -31 per cent, for cognitive ability at age 11. The fact that the imbalance is greater for the score at age 11 than it is for age 7 is explored below: in addition to the selection bias discussed by Pischke and Manning (2006) there appears to be a 'coaching effect' - those in selective areas were more likely to practice the kind of ability tests used in the NCDS as part of their preparation for the Eleven Plus.

[16] t-tests for the difference in means are often proposed as a way of checking for balancing. This approach is criticised by Ho *et al.* (2007) and Imbens and Wooldridge (2008): for example, "the critical misunderstood point is that balance is a characteristic of the observed sample, not some hypothetical population. The idea that hypothesis tests are useful for checking balance is therefore incorrect." (Ho *et al.*, 2007). They argue that this is compounded by the fact that pruning the sample affects the statistical power of the hypothesis tests and that it is therefore misleading to use tests, such as t-ratios for the difference in means, as a guide to the quality of matching. However this diagnostic is widely used and, for completeness, we do present t-ratios for the differences in means within the matched sample in the final column of Table 6 and subsequent tables.

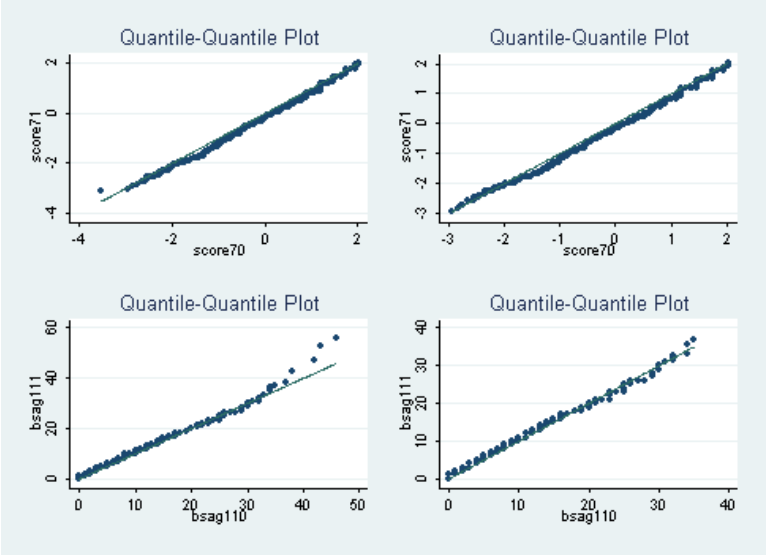
Table 6
Percentage bias (normalised difference in means between comprehensive and selective schools) before and after pruning and matching for key covariates

	Unmatched	Matched	t-test (p value)
Cognitive ability age 7	-16.8	-0.1	-0.02 (0.984)
BSAG score	3.8	-0.9	-0.25 (0.799)
Father's social class high	-12.3	0.7	0.19 (0.852)
Father's social class middle	11.6	2.5	0.70 (0.487)
Ill-health age 7	0.8	0.2	0.05 (0.964)
Cognitive ability age 11	-31.0	-30.1	-8.30 (0.000)

Note: Cognitive ability at age 11 is not used as a matching variable.

As the balancing condition relates to the full empirical distribution, not just the sample means, it is wise to check higher moments and cross-moments. Ho *et al.* (2007) suggest that nonparametric density plots and quantile-quantile (QQ) plots for each covariate and their interactions should be compared for the treated and controls. Figure 4 shows the empirical QQ plots for cognitive ability at age 7 and the BSAG score for the unmatched and matched samples. For the unmatched sample the divergence between the distributions is most clear in the tails of the distributions, especially for the upper tail of the distribution of the BSAG score.

Fig.4. *Empirical QQ-plots for cognitive score at 7 and BSAG score: Before (left panels) and after (right panels) matching*



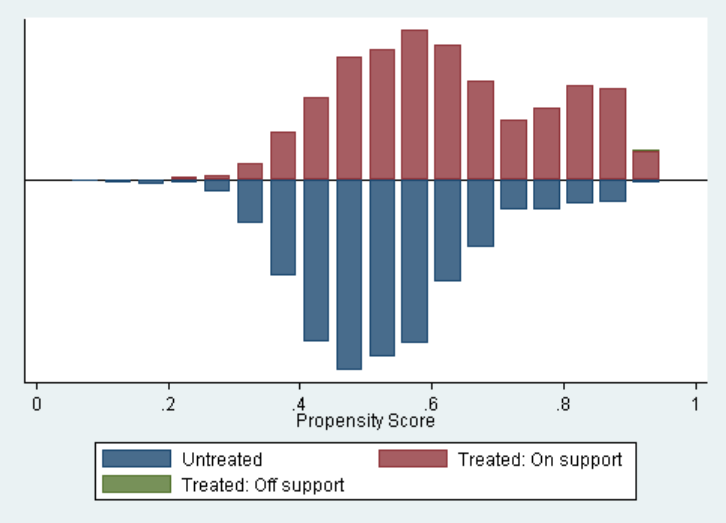
Perfect balancing is unlikely to be achieved in practice and, rather than simply comparing means after matching, running parametric regression models on the matched sample is likely to improve causal inferences (see e.g., Rubin, 1973, 1979, 2006; Heckman, Ichimura and Todd, 1998; Imbens, 2004; Abadie and Imbens, 2006; Ho *et al.*, 2007). In this sense, matching can be used as a nonparametric preprocessing of the data to select observations prior to parametric modelling. We adopt this approach here.

We implement the matching in two steps. In the first step coarsened exact matching is applied to the key measures of cognitive and non-cognitive skills, the ability score at age 7 and the BSAG score at age 11[17]. Then any observations that lie outside the common support of their joint distribution are excluded: this is only 34 cases in our data. The second step uses a combination of propensity score and Mahalanobis exact matching. The propensity score for attending a comprehensive school, as a function of all of the pre-schooling variables, is estimated using a logit model. The propensity score controls for the main pre-policy potential confounders of the relationship between attendance at a particular type of school and the health outcomes of interest. Figure 5 shows the distribution of the propensity score among those who went to selective and to comprehensive schools. Those who went to comprehensive schools are then matched with those who went to selective schools using the propensity score, within the common support and with a caliper of 0.1, combined with exact Mahalanobis matching for two key covariates, cognitive ability at age 7 and the BSAG score. The matching weights are then used in the subsequent regression analyses.

The normalised differences and t-ratios shown in Table 6 and the QQ plots in Figure 4 show how the imbalance in the key covariates is largely removed by this matching process. Table 6 also includes the cognitive ability scores at age 11, which are not used in the matching process. The percentage bias remains substantial for this variable (30.1 per cent) in the matched data. This is explored in the next section.

[17] Coarsened exact matching works by splitting the support of continuous covariates into discrete intervals and computing cell frequencies for the multivariate histogram (Blackwell *et al.*, 2009).

Fig.5. *Distribution of propensity score over selective ("untreated") and non-selective ("treated") schools*



3.3 'Coaching effects': absolute and relative cognitive ability

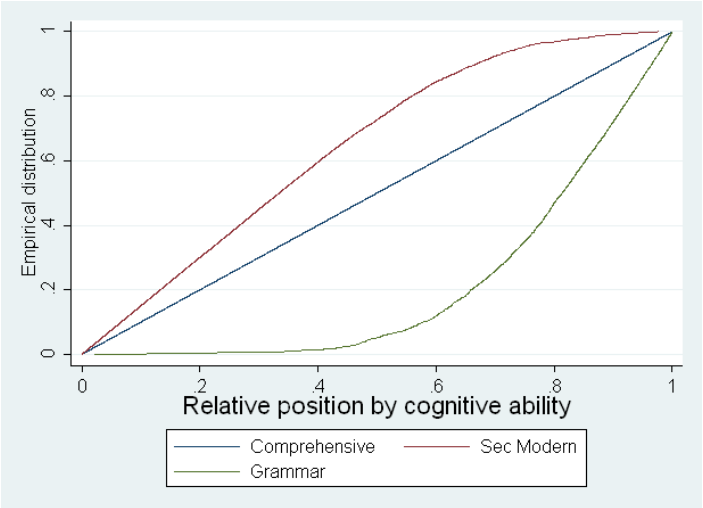
Cognitive ability at age 11 is not used in the matching process because there are good reasons to suspect that matching on the score at age 11 may be a source of post-treatment bias[18]. Those children who lived in areas which had not gone comprehensive may have been exposed to 'coaching' to prepare them for the Eleven Plus, both within their primary schools, where time was often set aside in lessons to prepare for the test, and at home. The cognitive ability test, also administered at age 11, have a lot in common with the components of the Eleven Plus and the resulting scores may therefore be indirectly affected by the kind of secondary school the child was likely to attend. In the matching approach described above we avoid this post-treatment bias by matching on ability at age 7.

Another way of looking at the issue is to focus on relative ability. Figure 6 shows the empirical distributions for relative ability, where rank in the distribution of ability is computed separately for those who went to comprehensive schools and who went to selective schools. By construction the distribution is uniform among the group who went to

[18] In fact, in our checks for robustness, we have repeated the matching and regression analyses using absolute ability. This shows that the qualitative estimates of the impact of educational attainment and type of schooling are robust to using either absolute or relative measures and that changes in the magnitudes of the estimates are small.

comprehensive schools, but among those who went to selective schools there is a clear threshold, around the lower 60 per cent of ability scores between those who went to secondary moderns and those who went to grammar schools. Relative ability therefore plays a central role in creating matched samples by type of school.

Fig.6. *Empirical distributions of relative ability by type of school*



This coaching effect is one way of explaining the results presented by Pischke and Manning (2006) and this is now explored in more detail. First we estimate simple regressions for cognitive ability at age 7 (*Score7*), conditioning on an indicator of attending a comprehensive school (*Comp*) and the other pre-schooling characteristics. The coefficient on *Comp* indicates any selection bias due to systematic differences between those who attended selective versus comprehensive schools, over and above the pre-schooling variables included in the equation, that influence cognitive ability. Table 7 shows that there is a statistically significant difference in the raw data but this disappears when the matched sample is used. The second regression is a value-added specification that regresses cognitive ability at age 11 on ability at age 7, the indicator for comprehensive schooling and an interaction between the two, as well as the other pre-schooling characteristics. Lagged ability captures any selection that has occurred up to age 7 as well as the inherent persistence in cognitive ability, the coefficient on *Comp* is now interpreted as capturing the 'coaching effect' and the coefficient on the interaction term captures any difference in the value-added between those who went on to become comprehensive pupils and others. The coaching effect is large and statistically

significant but we do not find evidence of a statistically significant interaction effect for either the unmatched or matched data.

Table 7
Regressions for cognitive ability scores at ages 7 and 11: full sample

	Score age 7		Score age 11	
	Unmatched	Matched	Unmatched	Matched
Comp	**-0.143**	-0.020	**-0.252**	**-0.438**
	(-3.43)	(-0.36)	**(-5.79)**	**(-7.58)**
Score7	-	-	**0.785**	**0.732**
			(27.04)	**(13.98)**
Score7*comp	-	-	-0.028	0.014
			(-0.78)	(0.25)
R^2	0.135	0.147	0.514	0.493
Sample size	2657	2211	2657	2211

Notes:

i. All regressions also condition on the full set of pre-schooling covariates. See Table A.1 for a full list.

ii. Robust t-ratios are given in parentheses. Coefficients that are statistically significant at least a 10 per cent level are shown in bold.

3.4 Matched sub-samples

The impact of educational attainment and quality of schooling is likely to depend on the particular type of school that is attended. The existence of heterogeneous effects is explored using a further round of matching that exploits the natural dividing in the population line drawn by the reform: the one separating those who experienced, or would have experienced in the absence of the reform, a grammar school education and those who attended, or would have attended, secondary modern schools. The matching is based on the propensity score for the probability of attending a grammar versus a secondary modern school. This is estimated by a logit model using only the sample who attended selective schools. Predictions of the propensity score are then computed for the whole sample, including those who attended comprehensive schools. The key predictor, that dominates the predictions from the logit model, is relative ability at age 11 (as shown in Figure 6). Those who were exposed to the non-selective system but whose propensity score indicates that they would have attended a grammar school (secondary modern) were they not exposed to the reform, are then matched with those who actually attended at a grammar school (secondary modern). The matching is over the common support with a caliper of 0.1 and uses Mahalanobis matching on the

propensity score and exact matching on relative ability at age 11, absolute ability at age 7, the BSAG score and father's social class. Tables 8 and 9 compare the balancing of selected covariates before and after matching for the two sub-samples and demonstrate that a good balance is achieved for both. The final rows of the table show that balance in terms of relative ability at age 11 does not imply balancing of absolute ability[19].

Table 8

Percentage bias (normalised difference in means between grammar and comprehensive schools) before and after matching for key covariates: sub-sample of grammar and comprehensive pupils

	Unmatched sample	Matched sample	t-ratio (p value)
Relative ability age 11	107.3	2.1	0.43 (0.670)
Cognitive ability age 7	93.2	1.8	0.35 (0.724)
BSAG score	-44.7	3.1	0.57 (0.569)
Father's social class high	47.4	0.0	0.00 (1.000)
Father's social class middle	-31.9	0.0	-0.00 (1.000)
Ill-health age 7	-15.0	9.2	1.43 (0.152)
Cognitive ability age 11	144.6	41.5	8.37 (0.000)

Note: Cognitive ability at age 11 is not used as a matching variable.

Table 9

Percentage bias (normalised difference in means between secondary modern and comprehensive schools) before and after matching for key covariates: sub-sample of secondary modern and comprehensive pupils

	Unmatched sample	Matched sample	t-ratio (p value)
Relative ability age 11	-66.3	-2.4	-0.51 (0.613)
Cognitive ability age 7	-25.4	-0.8	-0.16 (0.873)
BSAG score	21.1	3.2	0.55 (0.582)
Father's social class high	-13.6	0.0	0.00 (1.000)
Father's social class middle	1.7	0.0	0.00 (1.000)
Ill-health age 7	9.2	1.7	0.31 (0.760)
Cognitive ability age 11	-28.8	31.4	6.54(0.000)

Note: Cognitive ability at age 11 is not used as a matching variable.

[19] This is to avoid the potential for post-treatment bias. We have done robustness checks that include an analysis of what happens when relative ability is replaced by absolute ability.

4. Econometric models and results

4.1 Pre-schooling characteristics

Before exploring the direct impact of schooling we begin with simple regressions of the health outcomes on pre-schooling characteristics. These are estimated as unweighted linear regressions with robust standard errors[20].

Health-related behaviours

Table 10 shows selected results for the measures of health-related behaviour and focuses on key pre-schooling characteristics: cognitive ability at age 7, the BSAG measure of social adjustment at age 11 and father's occupational SES. Childhood cognitive ability has a statistically significant association with two of the adult behaviours: those with higher cognitive ability at age 7 drink more units of alcohol at age 42 but also consume vegetables on more occasions at age 33. As higher cognitive ability is likely to be associated with higher earnings later in life this suggests standard income effects on consumption, irrespective of whether the behaviours are 'healthy' or 'unhealthy'. Most of the other characteristics reported in the table do not have statistically significant associations with the health-related behaviours. An exception is smoking, where those with poorer social adjustment as children are more likely to become smokers and those whose father came from the higher or middle SES are less likely to become smokers.

Health outcomes

Table 11 shows selected results for the health outcomes: long-standing illness at age 46 and malaise at age 42[21]. Cognitive ability is not significantly associated with health outcomes but there is a strong association with social adjustment. Those with more problems with social adjustment as children are more likely to suffer both physical and mental illness as adults. There is also evidence of a socioeconomic gradient in illness by father's social class.

[20] Simple unweighted regression is used to capture the full linear association between the pre-schooling variables and the health outcomes. Using the weighted regressions is not appropriate here as the pre-schooling variables are reflected in the weights from the matching procedure as well as being included in the weighted regression model directly as covariates.

[21] The results show estimates for both a linear regression (linear probability) model and partial effects from a probit model, estimated at the mean of the regressors, for long-standing illness. The two specifications give virtually identical results and are presented to illustrate this finding. This applies to all of the nonlinear regression models we estimated and the rest of the paper focuses on linear regression results.

Table 10

Selected regression results for pre-schooling characteristics and health-related behaviours

	Smoking (age 42)	Drinking (age 42)	Vegetables (age 33)	Fried food (age 33)	Smoking during pregnancy
Sample size	2496	2102	2407	2406	392
Cognitive ability at 7	-0.008	**1.241**	**0.077**	-0.031	-0.016
	(-0.97)	**(2.11)**	**(3.20)**	(-1.55)	(-0.82)
BSAG	**0.007**	0.089	-0.001	-0.001	0.006
	(5.19)	(0.89)	(-0.14)	(-0.16)	(1.53)
Father's SES: professional	**-0.128**	-0.231	0.009	-0.025	-0.065
	(-4.87)	(-0.11)	(0.12)	(-0.41)	(-0.97)
Father's SES: other non-manual	**-0.075**	0.710	-0.060	-0.011	-0.080
	(-3.00)	(0.37)	(-0.89)	(-0.20)	(-1.27)

Notes:

i. The regression estimates are based on the un-weighted sample.

ii. Robust t-ratios are given in parentheses. Coefficients that are statistically significant at least a 10 per cent level are shown in bold.

iii. All regressions also condition on the full set of pre-schooling covariates. See Table A.1 for a full list.

Table 11

Selected regression results for pre-schooling characteristics and health outcomes

	LSI		Malaise
	LPM	Probit	
Sample size	2623		2487
Cognitive ability at age 7	-0.008	-0.008	-0.062
	(-0.87)	(-0.85)	(-0.89)
BSAG score	**0.005**	**0.005**	**0.050**
	(3.65)	**(3.74)**	**(4.11)**
Father's SES: professional	-0.036	-0.035	**-0.376**
	(-1.23)	(-1.22)	**(-1.78)**
Father's SES: other non-manual	**-0.044**	**-0.043**	-0.076
	(-1.70)	**(-1.71)**	(-0.40)

See notes for Table 10.

4.2 The impact of attainment and quality of schooling with controls for observables

We begin our analysis of the impact of educational attainment and the quality of schooling by presenting parametric models of adult health-related behaviours and health outcomes. These are estimated for the full matched sample and condition on all of the pre-schooling variables that are also used in the matching process which span parental socioeconomic status, childhood and family health, cognitive ability (relative score at age 11 and absolute score at 7), social adjustment, experience of primary schooling, and characteristics of the child's neighbourhood (ED). The models are estimated as linear regressions with robust standard errors[22].

Health-related behaviours

Table 12 shows that educational attainment, measured by the 12-point scale for highest secondary qualification, has a statistically significant association with smoking, diet and maternal behaviour. Those with higher attainment are less likely to be smokers and they consume vegetables more frequently. There is little evidence of quality of schooling, as measured by single sex schools, academic streaming, the pupil-teacher ratio and the ratio of expelled pupils, having a direct effect on health-related behaviours and the results are not presented here.

Table 12
Effect of educational attainment on health-related behaviours

	Smoking (age 42)	Drinking (age 42)	Vegetables (age 33)	Fried food (age 33)	Smoking during pregnancy
Sample size	2100	1772	2024	2023	319
Attainment	**-0.021**	-0.243	**0.026**	-0.012	**-0.025**
	(-4.45)	(-0.61)	**(1.99)**	(-1.09)	**(-2.31)**

Notes:

i. The regression estimates are based on the matched sample.
ii. Robust t-ratios are given in parentheses. Coefficients that are statistically significant at least a 10 per cent level are shown in bold.
iii. All regressions also condition on the full set of pre-schooling covariates and measures of school quality. See Table A.1 for a full list.

[22] Nonlinear versions of the models have been estimated as well and the partial effects from these models show little difference from the linear specifications.

Health outcomes

Table 13 shows that, on average, lower educational attainment is associated with poorer mental health later in life. There is no evidence of a statistically significant effect on long-standing illness or of an association between either of the health outcomes and quality of schooling and the results are not presented here.

Table 13
Effect of educational attainment on health outcomes

	LSI		Malaise
	LPM	Probit	
Sample size	2211		2092
Attainment	-0.007	-0.007	**-0.096**
	(-1.10)	(-1.10)	**(-2.40)**

See notes for Table 12.

4.3 Heterogeneous effects by type of school

To explore heterogeneity in the impact of educational attainment by the type of school attended we repeat the regressions using the matched sub-samples. The first sub-sample consists of those who went or would have gone to grammar schools and the second sub-sample consists of those who went or would have gone to secondary moderns.

Tables 14 and 15 show selected results for the impact of educational attainment on health-related behaviours. For both sub-samples educational attainment has some statistically significant impacts on health-related behaviours: reducing the likelihood of being a smoker and increasing the frequency of eating vegetables. Among the secondary modern sub-sample educational attainment also reduces the frequency of eating fried food but increases the weekly consumption of alcohol, perhaps reflecting a standard income effect on consumption rather than a health effect.

Table 14

Effect of educational attainment on health-related behaviours: matched sample of grammar and comprehensive pupils

	Smoking (age 42)	Drinking (age 42)	Vegetables (age 33)	Fried food (age 33)	Smoking during pregnancy
Sample size	713	629	690	690	162
Attainment	**-0.010**	-0.355	**0.036**	-0.011	-0.016
	(-1.99)	(-0.86)	**(2.12)**	(-0.81)	(-1.40)

Notes:

i. The regression estimates are based on the matched sub-sample.

ii. Robust t-ratios are given in parentheses. Coefficients that are statistically significant at least a 10 per cent level are shown in bold.

iii. All regressions also condition on the full set of pre-schooling covariates. See Table A.1 for a full list.

Table 15

Effect of educational attainment on health-related behaviours: matched sample of secondary modern and comprehensive pupils

	Smoking (age 42)	Drinking (age 42)	Vegetables (age 33)	Fried food (age 33)	Smoking during pregnancy
Sample size	1063	873	1027	1027	125
Attainment	**-0.038**	**0.959**	**0.064**	**-0.054**	-0.010
	(-5.00)	**(2.06)**	**(2.68)**	**(-2.95)**	(-0.33)

See notes for Table 14.

Tables 16 and 17 show selected results for the impact of schooling on health outcomes for the matched sub-samples. These reveal that the impact of educational attainment on adult health is concentrated among those who either did or would have attended grammar schools.

Table 16

Effect of educational attainment on health outcomes: matched sample of grammar and comprehensive pupils

	LSI		Malaise
	LPM	Probit	
Sample size	743		710
Attainment	**-0.012**	**-0.012**	**-0.110**
	(-1.82)	**(-1.78)**	**(-2.39)**

See notes for Table 14.

Table 17
Effect of educational attainment on health outcomes: matched sample of secondary modern and comprehensive pupils

	LSI		Malaise
	LPM	Probit	
Sample size	1127		1059
Attainment	0.006	0.006	-0.012
	(0.67)	(0.66)	(-0.19)

See notes for Table 14.

5. Discussion

The economic literature on human development was initially centred on documenting the relationship between cognitive ability and a wide range of social outcomes of interest. More recent work has additionally underlined the importance of non-cognitive skills most notably in determining education (Heckman and Rubinstein, 2001), and labour market outcomes (Carneiro *et al.*, 2007; Heckman *et al.*, 2006; Kuhn and Weinberger, 2005; Feinstein, 2000). This literature has suggested that cognitive and non-cognitive skills may act as substitutes in determining some outcomes (e.g. employment) but complements for others (e.g. wages) and that their impact operates both directly and through educational attainment (Carneiro *et al.*, 2007). Cognitive and non-cognitive skills have also been linked to a series of health and health-related behaviours. Heckman *et al.* (2006) find that both influence smoking in adolescence and teenage pregnancy with non-cognitive skills being more important determinants than cognitive skills. Similarly Carneiro *et al.* (2007) find a negative relationship between social skills and teenage smoking and pregnancy but report cognitive and non-cognitive skills to be equally important. In addition they find evidence of a link between cognitive and non-cognitive skills and adult health status.

Our findings corroborate some of this earlier work. We find that non-cognitive ability measured through social adjustment as a child is strongly associated with health, with those who had problems with social adjustment being more likely to suffer both physical and mental illness as adults. In addition there is also a strong relationship with smoking age at 42 with those with poorer social adjustment as a child more likely to be an adult smoker. In contrast, conditional on social adjustment we find cognitive ability at age 7 is not significantly associated with health outcomes in adulthood.

We find evidence of a socioeconomic gradient in health and health related behaviours by father's occupational SES, with those whose father had a non-manual occupation less likely to report physical and mental illness and less likely to become smokers. Taken together these results corroborate evidence for the existence of inequality of opportunity in health among NCDS cohort members reported by Rosa Dias (2009). Childhood health also has a statistically significant effect on adult health, corroborating similar results from Case *et al.* (2005).

Members of the National Child Development Study (NCDS) cohort attended very different types of secondary schools, as their schooling lay within the transition period of the comprehensive reform in England and Wales. This provides a natural experiment to explore the impact of educational attainment and of school quality on health and health-related behaviour later in life. We use a combination of matching methods and parametric regressions to evaluate differences in adult health outcomes for cohort members exposed to the old selective and to the new comprehensive educational systems.

We find educational attainment to have the expected association with health-related behaviours (smoking, smoking in pregnancy and the consumption of healthy foods) and to be negatively related to mental ill-health in adulthood but not physical health. However, this overall net impact encompasses important heterogeneity that we explore by splitting the sample across the key dividing line in the population drawn by the reform, the one separating those who experienced, or would have experienced, a grammar school education and those who attended, or would have attended, secondary modern schools. When those who went to grammar are matched to comparable individuals who attended comprehensives, higher attainment is associated with lower rates of adult smoking, higher rates of the consumption of vegetables and lower incidence of both physical and mental health. Interestingly, however, the impact of attainment on health-related behaviours is larger and covers a wider range of behaviours for those who attended (or would have attended) secondary modern schools. Given that detrimental lifestyles are more prevalent in the latter sub-sample, this may indicate the existence of diminishing returns by level of educational attainment. Carneiro *et al.* (2007) report findings that are akin to these, suggesting that the health returns to investments in social adjustment may be diminishing in the relative social position of one's parental background.

The asymmetry in the impact of attainment on health outcomes is even more striking. For the sub-sample in which cohort members who attended grammar schools are

matched with comparable individuals who attended comprehensives we find positive and statistically significant effects both on physical and on mental health. In contrast, no effects were found for those who attended (or would have attended) secondary modern schools. Variation in attainment within the former sub-sample, which is partly generated by the fact that some of the group went to academically intensive grammar schools while the others went to comprehensives, has more impact on health than variation in attainment in the latter sub-sample. This may imply that quality of schooling works as a catalyst in the relationship between attainment and health. Cutler and Lleras-Muney (2010) point-out a similar hypothesis, suggesting that peer effects do not explain why better educated groups have better health to begin with, but are likely to magnify the positive impact of education on health. Additionally, the different effect between sub-samples may also reflect a non-linearity in the returns to different levels of attainment, given that average attainment is lower, and its distribution more compressed, in the latter group than in the former.

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APPENDIX

Table A.1
Full set of pre-schooling and secondary school characteristics

	Variables
Pre-schooling characteristics	
	Cognitive ability score (age 7)
	Relative rank of cognitive ability (age 11)
	BSAG score (age 11)
	Number of chldren in primary school class (age 7)
	Indicator for unhappy at primary school (age 7)
	Indicator for parents' wanting child to stay in school
	Indicator for male
	Morbidity index (age 7)
	Number of hospitalisations (age 7)
	Indicator for diabetes in family
	Indicator for epilepsy in family
	Indicator for heart disease in family
	Indicator for father's occupational SES professional
	Indicator for father's occupational SES other non-manual
	Indicator for financial hardship in family (age 7)
	Enumeration district: percentage unemployed/long-term sick
	Enumeration district: percentage women working
	Enumeration district: percentage employed in manufacturing
	Enumeration district: percentage emplyed in agriculture
	Enumeration district: percentage in professional/managerial occupations
	Enumeration district: percentage in other non-manual occupations
	Enumeration district: percentage in skilled manual occupations
	Enumeration district: percentage in semi-skilled manual occupations
	Enumeration district: percentage in unskilled manual occupations
	Enumeration district: percentage owner occupiers
	Enumeration district: percentage council tenants
	Enumeration district: percentage non-white
	Enumeration district: percentage immigrants
	Indicators for Standard Regions
Secondary school characteristics	
	Indicator for single sex school
	Indicator for streaming by ability within school
	Pupil-teacher ratio
	Ratio of expelled to total pupils