

# Sussex Research

## Long-term effects of school quality on health and lifestyle: evidence from comprehensive schooling reforms in England

Andrew M Jones, Pedro Rosa Dias, Nigel Rice

### Publication date

01-01-2011

### Licence

This work is made available under the **Copyright not evaluated** licence and should only be used in accordance with that licence. For more information on the specific terms, consult the repository record for this item.

### Citation for this work (American Psychological Association 7th edition)

Jones, A. M., Rosa Dias, P., & Rice, N. (2011). *Long-term effects of school quality on health and lifestyle: evidence from comprehensive schooling reforms in England* (Version 1). University of Sussex.  
<https://hdl.handle.net/10779/uos.23389253.v1>

### Published in

Journal of Human Capital

### Link to external publisher version

<https://doi.org/10.1086/662441>

### Copyright and reuse:

This work was downloaded from Sussex Research Open (SRO). This document is made available in line with publisher policy and may differ from the published version. Please cite the published version where possible. Copyright and all moral rights to the version of the paper presented here belong to the individual author(s) and/or other copyright owners unless otherwise stated. For more information on this work, SRO or to report an issue, you can contact the repository administrators at [sro@sussex.ac.uk](mailto:sro@sussex.ac.uk). Discover more of the University's research at <https://sussex.figshare.com/>

## Long-term effects of school quality on health and lifestyle: evidence from comprehensive schooling reforms in England

Article (Unspecified)

Jones, Andrew M, Rosa Dias, Pedro and Rice, Nigel (2011) Long-term effects of school quality on health and lifestyle: evidence from comprehensive schooling reforms in England. *Journal of Human Capital*, 5 (3). pp. 342-376. ISSN 1932-8664

This version is available from Sussex Research Online: <http://sro.sussex.ac.uk/id/eprint/41389/>

This document is made available in accordance with publisher policies and may differ from the published version or from the version of record. If you wish to cite this item you are advised to consult the publisher's version. Please see the URL above for details on accessing the published version.

### **Copyright and reuse:**

Sussex Research Online is a digital repository of the research output of the University.

Copyright and all moral rights to the version of the paper presented here belong to the individual author(s) and/or other copyright owners. To the extent reasonable and practicable, the material made available in SRO has been checked for eligibility before being made available.

Copies of full text items generally can be reproduced, displayed or performed and given to third parties in any format or medium for personal research or study, educational, or not-for-profit purposes without prior permission or charge, provided that the authors, title and full bibliographic details are credited, a hyperlink and/or URL is given for the original metadata page and the content is not changed in any way.



CHICAGO JOURNALS

---

Long-Term Effects of School Quality on Health and Lifestyle: Evidence from Comprehensive Schooling Reforms in England

Author(s): Andrew M. Jones, Nigel Rice, Pedro Rosa Dias

Source: *Journal of Human Capital*, Vol. 5, No. 3 (Fall 2011), pp. 342-376

Published by: [The University of Chicago Press](#)

Stable URL: <http://www.jstor.org/stable/10.1086/662441>

Accessed: 20/10/2011 11:31

---

Your use of the JSTOR archive indicates your acceptance of the Terms & Conditions of Use, available at  
<http://proofing.www.jstor.org/page/info/about/policies/terms.jsp>

JSTOR is a not-for-profit service that helps scholars, researchers, and students discover, use, and build upon a wide range of content in a trusted digital archive. We use information technology and tools to increase productivity and facilitate new forms of scholarship. For more information about JSTOR, please contact support@jstor.org.



The University of Chicago Press is collaborating with JSTOR to digitize, preserve and extend access to *Journal of Human Capital*.

<http://proofing.www.jstor.org>

# Long-Term Effects of School Quality on Health and Lifestyle: Evidence from Comprehensive Schooling Reforms in England

Andrew M. Jones  
*University of York*

Nigel Rice  
*University of York*

Pedro Rosa Dias  
*University of York*

Members of the National Child Development Study cohort attended very different types of secondary schools, 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 behavior 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 behavior for cohort members exposed to the old selective and to the new comprehensive educational systems.

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), Arendt (2005), Lleras-Muney (2005), Kenkel, Lillard, and Mathios (2006), Oreopoulos (2006), Grimard and Parent (2007), Lindeboom, Llena-Nozal, and Van der Klaauw (2009), and Webbink, Martin, and Visscher (2010) additionally find evidence to suggest that part of this relationship may be causal. Cutler and Lleras-Muney

We gratefully acknowledge funding from the Economic and Social Research Council under grant reference RES-060-25-0045. We are grateful for comments on earlier versions of this work from Martyn Andrews, Anirban Basu, Rena Conti, Will Manning, David Meltzer, Owen O'Donnell, Bobbi Wolfe, and seminar participants at the University of Chicago, University of Lausanne, University of Manchester, University of Melbourne, Monash University, University of Paris Descartes, and University of Wisconsin–Madison. The National Child Development Study was supplied by the ESRC Data Archive. Responsibility for interpretation of the data, as well as any errors, is the authors' alone.

(2008, 2010) and Mazumder (2008) outline some of the channels through which education may have an impact on health and health-related behaviors: 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 behaviors, through the acquisition of specific health knowledge, through improved information processing and decision-making skills, and through the influence on behavioral 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.<sup>1</sup> This paper is concerned with evaluating the joint impact of educational attainment and of attending qualitatively different types of school on health outcomes and health-related behavior later in life. We address two key issues:

- the measurement of the overall impact of educational attainment and of the quality of schooling on adult health and health-related behavior,
- the existence of heterogeneity in the impact of educational attainment, particularly according to the type of school attended.

As emphasized by Manning and Pischke (2006, 1), among the many qualitative dimensions of educational systems, the issue of the relative merits of early tracking versus late tracking of students in secondary schools has received particular attention in the economics of education.<sup>2</sup> We focus on this dimension of quality of schooling using data from a longitudinal study that documents one of the paradigmatic case studies used to analyze it: the comprehensive education reform in England and Wales.

The National Child Development Study (NCDS) follows a cohort of around 17,000 individuals who were born in Great Britain in the week of March 3, 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 schools. The cohort's secondary schooling lay within the transition period of the comprehensive education reform that was implemented in England and

<sup>1</sup> This gap in the literature is acknowledged in Cutler and Lleras-Muney (2008, 22). The impact of quality of schools on other outcomes has received considerable attention (see, e.g., Card and Krueger 1992; Angrist and Pischke 1999; Black 1999; Hoxby 2000).

<sup>2</sup> This issue also ranks high in policy circles. For example, the OECD (2005) evaluation of the Programme for International Student Assessment devotes considerable attention to comparing educational attainment across national education systems of different degrees of selectivity.

Wales from the mid-1960s.<sup>3</sup> This was a major reform, which transformed secondary education by replacing the existing system of early tracking of students by a nonselective, comprehensive, educational system. Comprehensive schooling was not implemented simultaneously nationwide; hence some of the cohort members attended the highly selective tripartite 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 percent attended grammar schools at age 16, 25 percent attended secondary moderns, and 57 percent attended comprehensives. A further 6 percent of NCDS cohort members attended private fee-paying schools, independent of the state educational system and reforms.<sup>4</sup> The comprehensive education reform was aimed at reducing inequality of opportunity by improving the quality of schooling available to children from disadvantaged backgrounds. These reforms have been evaluated in terms of their direct impact on educational attainment and subsequent impact on labor market outcomes (see, e.g., Kerckhoff et al. 1996; Jesson 2000; Dearden, Ferri, and Meghir 2002; Galindo-Rueda and Vignoles 2004, 2005; Manning and Pischke 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 behavior. 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., Arendt 2005, 2008; Lleras-Muney 2005; Oreopoulos 2006; Albouy and Lequien 2008; Mazumder 2008; Silles 2009; Van Kippersluis, O'Donnell, and Van Doorslaer 2009; Chou et al. 2010).

We use a matching framework to preprocess 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 preschooling characteristics of those who attended comprehensive and selective schools (Manning and

<sup>3</sup> 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 schools, educational qualifications, and outcomes.

<sup>4</sup> 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).

Pischke 2006). First we use matching to improve the balance of a broad set of observed preschooling characteristics, 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. The success of this strategy is assessed using value-added regressions and other diagnostics.

The use of matched samples is coupled with parametric modeling of health outcomes and health-related behavior. 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 behavior? This comparison uses matching to balance the sample and controls for an extensive set of observed preschooling characteristics using linear and nonlinear regression methods.
- The key feature of the selective 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 subsamples.

Our results show that cognitive ability at age 7 is not significantly associated with health outcomes, but there is a strong association with noncognitive 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 some evidence of heterogeneity in the impact of educational attainment as measured by qualifications. Attainment has an impact on adult health-related behaviors for both groups, in particular on smoking and diet. But attainment appears to have a sizable impact on only one of our key adult health outcomes, mental health at age 42, for those who either did attend or would have attended grammar schools.

## I. 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 nonselective, 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 coexisted for a long period of time, and approximately 40 percent of the NCDS cohort, who entered state secondary schools in 1969, experienced the prereform selective system; the remaining 60 percent attended comprehensive schools.

Grammar schools were academically oriented state schools that provided teaching for the entire age range 11–18, including a sixth form<sup>5</sup> 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”).<sup>6</sup> 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.<sup>7</sup>

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 percent of private schools and 69 percent of grammar schools were single sex, whereas only 13 percent of comprehensive schools were single sex. Streaming of classes by academic ability was common in secondary modern schools (42 percent) and comprehensive schools (39 percent) but rare among grammar schools (17 percent). Some comprehensive schools were former secondary moderns (26 percent) or grammar schools (19 percent), with the rest being newly created. Furthermore, the distribution of quality of school indicators, such as the pupil-teacher ratio, and of the average number of expelled students also differed across types of schools: it is typically higher in secondary modern and comprehensive schools and lower in the case of grammar schools.

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. Kerck-

<sup>5</sup> “Sixth form” is the British terminology for the final two years of secondary schooling, between ages 16 and 18, when pupils follow the A-level syllabus.

<sup>6</sup> 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 have persisted in certain areas, such as Kent.

<sup>7</sup> In a few cases, pupils whose Certificate of Secondary Education (CSE) grades were sufficient transferred to grammar schools or sixth form colleges to complete their A-levels.

TABLE 1  
CHARACTERISTICS OF DIFFERENT TYPES OF SCHOOLS  
(as Attended by NCDS Cohort at Age 16)

	Grammar	Secondary 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 seconarcy modern	. . .	. . .	26.3	. . .

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

hoff 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 percent 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 McNally (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 percent. The paper compares the educational outcomes between Northern Ireland and England before and after the reform (using the English com-

prehensive 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.

Manning and Pischke (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 were 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 tests based on value-added regression for ability, they find that the comparison between areas exposed to different degrees of educational selectivity tends to produce the same results regardless of whether the educational outcomes are measured after the reform or before it. We draw on Manning and Pischke's value-added 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.

## II. NCDS Data and Study Design

Members of the NCDS cohort were all born in the week of March 3, 1958. Seven waves of interviews were 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 behavior, 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 the child's school. In the later waves these are augmented by interviews with the cohort members and data linked from the census.

The structure of the NCDS is well suited to our study design, which is summarized 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 behavior and adult health. The NCDS allows us to condition on a broad set of pretreatment 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 (SES) 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 noncognitive 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 his or her adult outcomes, both the

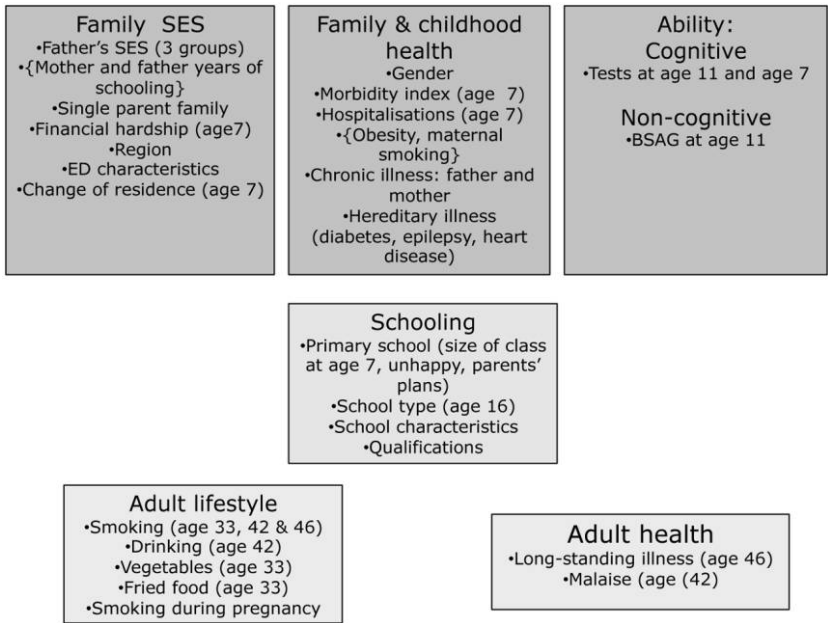


Figure 1.—Schematic view of study design and NCDS variables. Items in braces are not used in our main analysis but are used in the checks for robustness.

intermediate outcomes, such as smoking at age 42, and final health outcomes, 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 Appendix table A1.

*A. Childhood Health and Parental Background*

Rich information is available to characterize the cohort members' childhood health and parental circumstances, which have both been linked to adult health outcomes (see, e.g., Currie and Stabile 2003; Case, Fertig, and Paxson 2005). Following Power and Peckham (1987), indicators of morbidity are constructed by aggregating 12 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 SES, measured in three groups (see Carneiro, Crawford, and Goodman 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.

*B. Cognitive Ability, Noncognitive Skills, and Social Adjustment*

Auld and Sidhu (2005) argue that failure to control for cognitive ability will confound the relationship between health and education. Noncognitive skills have also received considerable attention in recent studies (see, e.g., Heckman, Stixrud, and Urzua 2006; Heckman 2008) and have been linked to health and health-related behaviors (see, e.g., Carneiro et al. 2007; Coneus and Laucht 2008; Keaster 2009; Cutler and Lleras-Muney 2010). Among these noncognitive skills, social adjustment is of particular relevance for schooling and health (Carneiro et al. 2007).

The NCDS provides measures of cognitive and noncognitive ability collected before respondents began their secondary schooling. Scores of ability tests taken at ages 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.<sup>8</sup> 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 emphasized 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, mirror 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.

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

<sup>8</sup> For example, with the scores at age 11, the first principal component accounts for 85 percent of the joint variation, and, strikingly, the weights attached to the three dimensions—0.583, 0.567, and 0.582—are virtually identical.

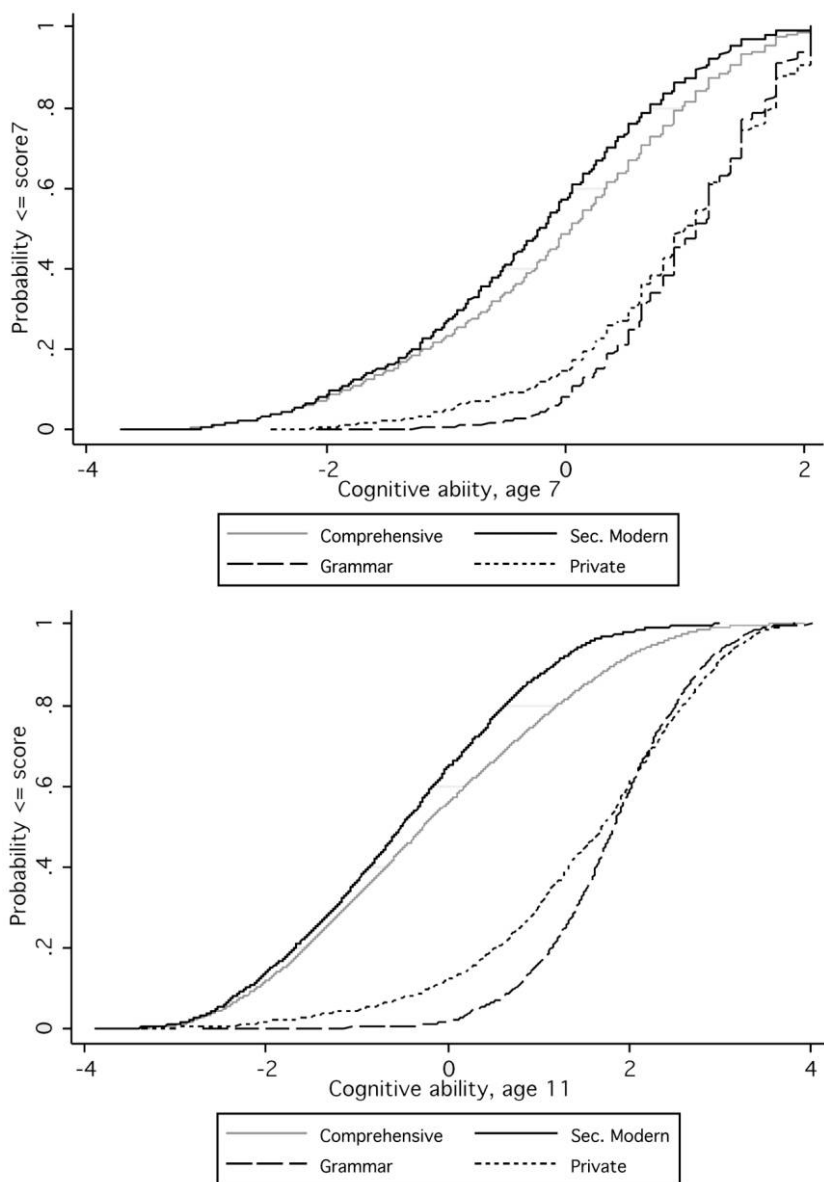


Figure 2.—Empirical distributions of cognitive ability scores by type of school at age 7 and at age 11.

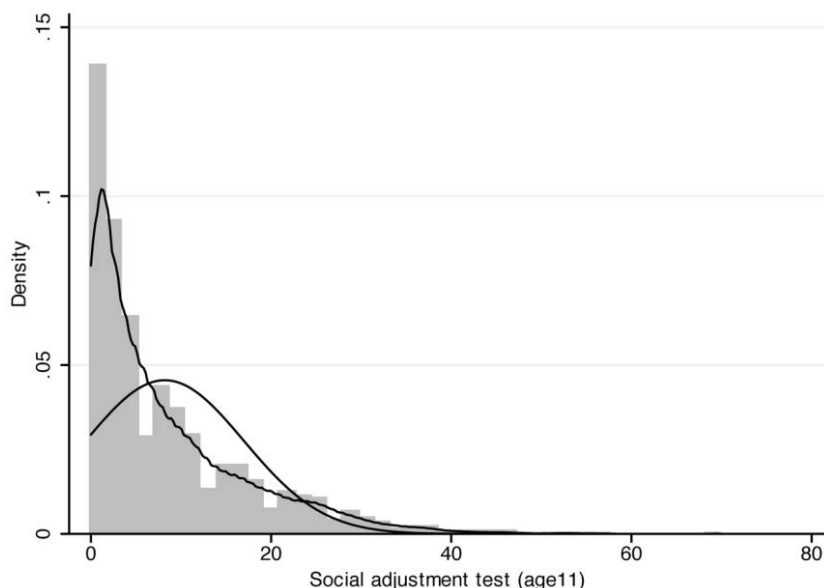


Figure 3.—Empirical density of Bristol Social Adjustment Guide. The figure shows the histogram of the BSAG score, a kernel density estimate, and a normal curve.

asked to report whether the child had problems in 12 behavioral domains such as hostility toward children and adults, anxiety, withdrawal, “writing off” adults, unforthcomingness, depression, restlessness, acceptance by adults, inconsequential behavior, and 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.<sup>9</sup> 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.

### C. 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’ U.K. standard region are available for all the waves of the study. For the years 1971 and 1981, NCDS survey data were linked to the census, allowing a detailed demographic and socioeconomic characterization of each individual’s local area, at the electoral constituency level, LEA level, and census enumeration district level (the smallest unit

<sup>9</sup> The NCDS data dictionary notes that the scores “are added together to give a figure which indicates, fairly crudely, the total amount of behavioral deviance (or maladjustment?) as measured by the Guide.”

for which census statistics were then available, with an average population of about 500).<sup>10</sup> Measures include the percentage of the local population who are unemployed or long-term sick, working women, those employed in particular sectors (manufacturing and agricultural), those in different occupational groups (professional/managerial, other nonmanual, skilled manual, semiskilled, unskilled), owner-occupiers, council tenants, nonwhites, and immigrants (see Dearden et al. 2002; Galindo-Rueda and Vignoles 2004; Manning and Pischke 2006).

#### *D. 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: CSEs, O-levels, A-levels, and university degree or equivalent.<sup>11</sup> We adopt a richer classification, and the information on educational achievement in secondary education is further disaggregated into 13 categories, ordered according to the grades obtained and number of passes.<sup>12</sup> In our empirical analysis we use the simple 0–12 scaling of this variable as a parsimonious measure of educational attainment.<sup>13</sup> The distribution of this measure is shown in table 2.

Our analysis of the impact of secondary schooling controls for information about individuals' 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, or comprehensive. 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

<sup>10</sup> These small area data are available under a special license, 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>.

<sup>11</sup> CSEs and O-levels were secondary education qualifications corresponding, typically, to 11 years of education; CSEs were academically less demanding than O-levels. A-levels are a qualification that corresponds to 13 years of education. Completion of A-levels is ordinarily a prerequisite for university admission.

<sup>12</sup> This is variable E386 in the NCDS data dictionary.

<sup>13</sup> We have also used models with dummy variables for each category to check the robustness of the results.

TABLE 2  
EDUCATIONAL ATTAINMENT: HIGHEST SECONDARY QUALIFICATION (NCDS Variable E386)

	Percentage
No grade at CSE, GCE O, or A-levels	20.6
1+ passes at O-level, grade 4 or 5 only	.6
1+ passes at CSE, grade 4 or 5 only	8.2
1+ passes at CSE, grade 2 or 3	9.5
5+ passes at CSE, grade 2 to 5	13.6
1–4 passes at GCE O-level or CSE grade 1	25.5
5 or 6 passes GCE 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 8 points	3.7
3+ passes at A-levels, up to 8 points	2.7
2 passes at A-levels and 9+ points	.2
3+ passes at A-levels and 9+ points	4.0

Note.—*N* = 11,086. A-level points are allocated as 5, 4, 3, 2, and 1 for grades A–E, respectively. GCE = General Certificate of Education.

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.<sup>14</sup>

*E. Intermediate Outcomes: Health-Related Behaviors*

The NCDS contains self-reported information on a series of health-related behaviors that 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,<sup>15</sup> and dietary choices, such as the frequency of consumption of fried food and vegetables. These data are available only in the latter four waves of the study, once respondents are aged 23 and above. The other measure of health-related behavior 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 United Kingdom. We have information on smoking at each of the waves 4–7, spanning ages 23–46. As there is item nonresponse 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 percent. Of those with

<sup>14</sup> 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, Rajah, and van Soest 2003).

<sup>15</sup> 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 U.K. National Health Service official guidelines that are available at <http://www.nhs.uk/Livewell/alcohol/Pages/alcohol-units.aspx>.

TABLE 3  
BREAKDOWN OF LONG-STANDING ILLNESS BY PERCENTAGE WITH  
SPECIFIC MAIN CONDITIONS (ICD-9): N = 2,990

	Wave 7 (Age 46)
Infectious and parasitic diseases	.7
Neoplasms	1.6
Diseases of blood and immune mechanism	1.5
Endocrine, nutritional, and metabolic	9.5
Mental and behavioral 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
Musculoskeletal system	25.7
Genitourinary system	2.0
Congenital malformations	.3
Undiagnosed illness	1.8
Injury, poisoning, etc.	5.3
Other long-standing illness (uncoded)	4.3

available data on smoking for waves 5–7 (ages 33, 42, and 46), 69 percent never smoked. Among those who smoked at some point, 74 percent reported smoking at age 42. The remainder are mostly those who had smoked at age 33 but not at 42 or 46.<sup>16</sup> So our measure captures those whose damaging health-related behavior persists into their mid-40s.

*F. 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 is classified according to the International Classification of Diseases (ICD-9). 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 percent), circulatory system (11.8 percent), respiratory system (11.4 percent), and metabolic problems (9.5 percent), of which 70 percent 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 a measure used in Carneiro et al.

<sup>16</sup> To check robustness, all 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 percent. 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.

(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, Tizard, and Whitmore 1970). The NCDS team suggests 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).<sup>17</sup> In our data the malaise index, at age 42, ranges from 0 to 23, with a mean of 3.4.

### III. Sample Selection and Balanced Samples

#### A. Sample Selection and Nonresponse

Owing to sample attrition and especially to patterns of item nonresponse, 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 (Dearden et al. 2002; Galindo-Rueda and Vignoles 2004, 2005; Case et al. 2005; Manning and Pischke 2006; Lindeboom et al. 2009; Cutler and Lleras-Muney 2010). Nevertheless, the periodic reports produced by the NCDS Advisory Panel, as well as recent research papers that have analyzed the implications of nonrandom attrition, have concluded that this is not serious source of bias for models based on the data (e.g., Plewis et al. 2004; Case et al. 2005; Lindeboom et al. 2009). In their study of educational attainment and wages, Dearden et al., who condition on a set of variables similar to ours, conclude that “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” (2002, 5).<sup>18</sup>

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 nonresponse. 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 percent at wave 5 to 34 percent 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

<sup>17</sup> Carneiro et al. (2007) define an indicator variable for depression based on this rule of thumb.

<sup>18</sup> We have investigated the possibility of systematic attrition related with the incidence of chronic illness in the cohort members’ parents and family; we have not found evidence of such patterns. Also, we have tested for whether selection into types of schooling might be related with the incidence of chronic illness in relatives and with aspects of family structure such as single parenthood. Our results also have not corroborated these hypotheses.

TABLE 4  
SAMPLE MEANS FOR OUTCOMES

	Estimation Sample	All Other Observations
Long-standing illness:		
Wave 7	.34 (n = 2,832)	.35 (n = 4,663)
Wave 6	.27 (n = 2,700)	.29 (n = 6,159)
Wave 5	.15 (n = 2,593)	.15 (n = 6,286)
Malaise: wave 6	3.35 (n = 2,689)	3.63 (n = 6,103)
Smoker:		
Wave 6	.21 (n = 2,698)	.27 (n = 6,152)
Waves 5–7	.28 (n = 2,377)	.32 (n = 3,695)

TABLE 5  
SAMPLE MEANS FOR TYPE OF SCHOOLING AND COGNITIVE ABILITY

	Estimation Sample	All Other Observations
Comprehensive	.55	.57
Secondary modern	.24	.25
Private school	.06	.07
Attainment	4.76	3.95
Single-sex school	.28	.27
Ability streams	.35	.38
Pupil-teacher ratio	.06	.06
Expelled ratio	.0003	.0004
Cognitive ability age 7	.25	–.02

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.

*B. Balance of Covariates between Selective and Comprehensive Schools*

Manning and Pischke (2006) have drawn attention to the fact that there may be an imbalance in the preschooling characteristics of the NCDS respondents who went to selective versus comprehensive schools. They find that comprehensive areas were systematically poorer and populated by children with lower educational achievement than selective areas. In this subsection 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 ( $x^0$ ),

TABLE 6  
PERCENTAGE BIAS (Normalized Difference in Means between Comprehensive and Selective Schools) BEFORE AND AFTER PRUNING AND MATCHING FOR KEY COVARIATES

	Unmatched	Matched	<i>t</i> -test ( <i>p</i> -Value)
Cognitive ability age 7	−15.8	−.3	−.09 (.926)
BSAG score	3.3	−1.0	−.26 (.795)
Father’s social class high	−13.9	1.8	.52 (.606)
Father’s social class middle	12.6	2.3	.62 (.533)
Ill health age 7	.3	−2.4	−.66 (.512)
Cognitive ability age 11	−30.8	−29	−7.79 (.000)

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

before and after matching, is the percentage bias, or normalized difference in means (Rosenbaum and Rubin 1983; LaLonde 1986):<sup>19</sup>

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

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 preschooling 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 −15.8 percent for cognitive ability. It is notable that the percentage bias is even greater, at −30.8 percent, 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 Manning and Pischke (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.

As the balancing condition relates to the full empirical distribution, not just the sample means, it is wise to check higher moments and cross

<sup>19</sup> Often *t*-tests for the difference in means are proposed as a way of checking for balancing. This approach is criticized 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, 221). 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.

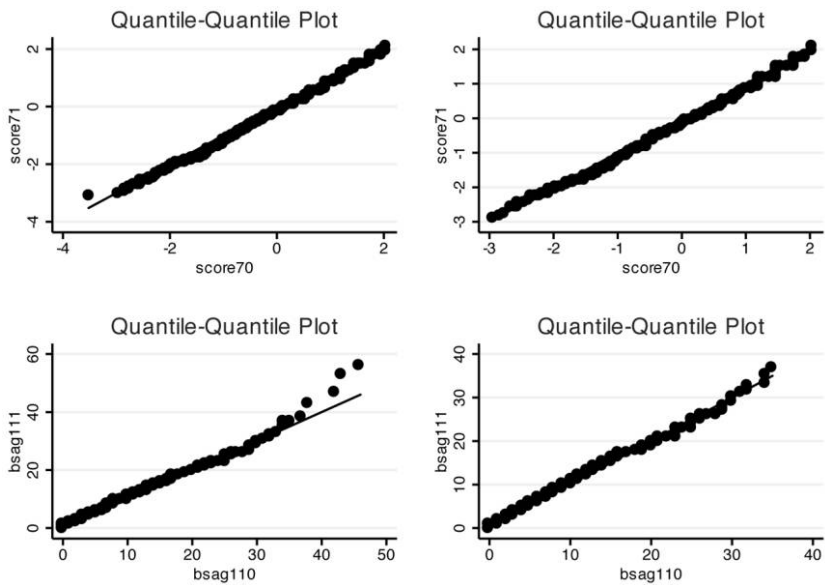


Figure 4.—Empirical QQ plots for cognitive score at 7 and BSAG score: before (left panels) and after (right panels) matching.

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 control groups. 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.

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; Ho et al. 2007). In this sense, matching can be used as a nonparametric preprocessing of the data to select observations prior to parametric modeling. 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.<sup>20</sup> Then any observations that lie outside the common support of their joint distribution are excluded: this is only 37 cases in our data.

<sup>20</sup> 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).

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 the preschooling variables, is estimated using a logit model. The propensity score controls for the main prepolicy 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 normalized 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 percent) in the matched data. This is explored in the next section.

### *C. 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 posttreatment bias.<sup>21</sup> Those children who lived in areas that 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, has 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 posttreatment 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 comprehensive schools, but among those who went to selective schools there is a clear threshold, around the lower 60 percent of ability scores,

<sup>21</sup> 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.

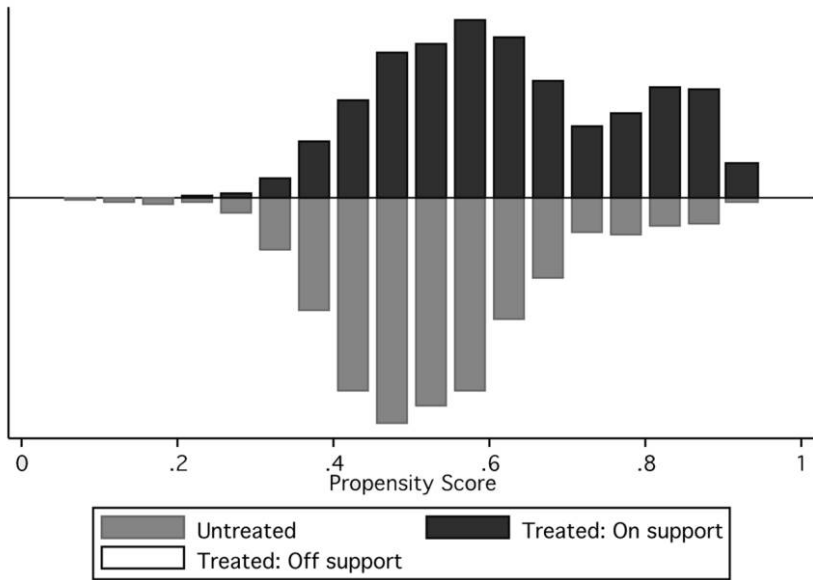


Figure 5.—Distribution of propensity score over selective (“untreated”) and comprehensive (“treated”) schools.

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.

This coaching effect is one way of explaining the results presented by Manning and Pischke (2006), and this is now explored in more detail. First we estimate simple regressions for cognitive ability at age 7 (Score 7), conditioning on an indicator of attending a comprehensive school (Comp) and the other preschooling 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 preschooling 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 preschooling characteristics. Lagged ability captures any selection that has occurred up to age 7 as well as the inherent persistence in cognitive ability;<sup>22</sup> the coefficient on Comp is now interpreted as

<sup>22</sup> A different, but related, issue is the one of persistence in relative cognitive ability between ages 7 and 11. We have interacted relative cognitive ability with the key determinants of the type of educational system and used them in value-added linear models

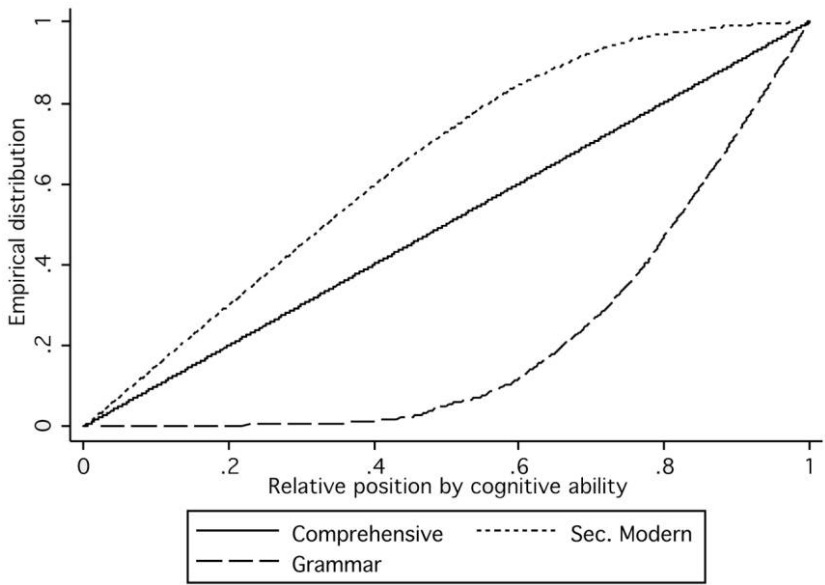


Figure 6.—Empirical distributions of relative ability by type of school

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.

*D. Matched Subsamples*

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 line in the population drawn by the reform: the one separating those who, in the absence of the reform, experienced or would have experienced a grammar school education and those who attended or would have attended secondary modern schools. This is schematically represented in figure 7.

The matching is based on the propensity score for the probability of attending a grammar versus a secondary modern school. This is esti-

---

to examine the persistence of relative cognitive ability between the ages of 7 and 11. This persistence is comparably high across selective and comprehensive schools, and high parental social class has a positive and statistically significant association with higher ability ranks across educational systems. However, consistent with the hypothesis of coaching, it reinforces persistence between relative ability at ages 7 and 11 relatively more in the case of selective education than in the comprehensive.

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	-.130* (-3.06)	-.005 (-.10)	-.254* (-5.72)	-.407* (-6.89)
Score 7	. . .	. . .	.779* (26.23)	.738* (14.54)
Score 7 × Comp	. . .	. . .	-.021 (-.58)	.011 (.22)
R <sup>2</sup>	.135	.147	.517	.500
Sample size	2,534	2,101	2,534	2,101

Note.—All regressions also condition on the full set of preschooling covariates. See App. table A1 for a full list. Robust *t*-ratios are given in parentheses.  
\* Statistically significant at least at the 10 percent level.

mated 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, which dominates the predictions from the logit model, is relative ability at age 11 (as shown in fig. 6).<sup>23</sup> Those who were exposed to the comprehensive 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 a grammar school (secondary modern). The matching occurs 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 subsamples 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.<sup>24</sup>

IV. Econometric Models and Results

A. Preschooling Characteristics

Before exploring the direct impact of schooling, we begin with simple regressions of the health outcomes on preschooling characteristics.

<sup>23</sup> In general, although a high score in the Eleven Plus gained the pupil a place in a grammar school, many other factors jointly determined access to this type of school. For example, pupils from a higher socioeconomic and educational background were over-represented in terms of grammar school attendance. However, our goal is to model attendance at different types of education, not the admission process that leads to it. For this we have accounted for a wide set of factors, including multiple dimensions of ability, health endowments, parental background, and local area influences, instead of modeling admission on the sole basis of the Eleven Plus scores.

<sup>24</sup> This is to avoid the potential for posttreatment bias. We have done robustness checks that include an analysis of what happens when relative ability is replaced by absolute ability.

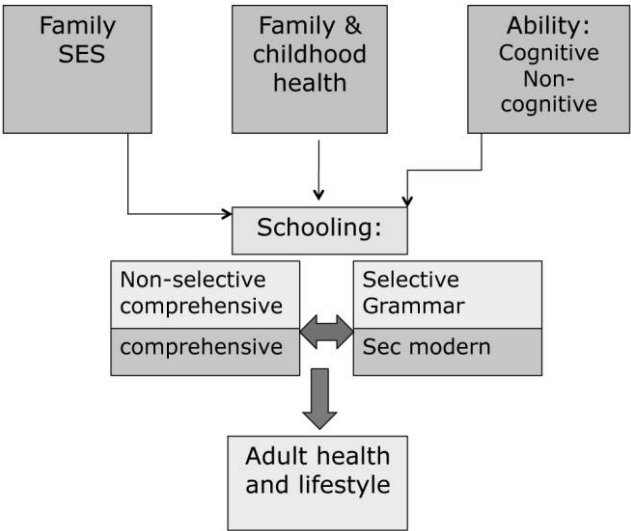


Figure 7.—Heterogeneous effects: a matched samples approach

These document the network of influences that is schematically represented in figure 1 and are estimated as unweighted linear regressions with robust standard errors:<sup>25</sup>

$$\begin{aligned} &\text{health outcome or lifestyle} = \\ &\quad f(\text{family SES, family and childhood health,} \\ &\quad \text{ability, schooling, and educational attainment}) + \varepsilon. \end{aligned}$$

Health-Related Behaviors

Table 10 shows selected results for the measures of health-related behavior and focuses on key preschooling 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 behaviors: 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

<sup>25</sup> Simple unweighted regression is used to capture the full linear association between the preschooling variables and the health outcomes. Using the weighted regressions is not appropriate here since the preschooling variables are reflected in the weights from the matching procedure as well as being included in the weighted regression model directly as covariates.

whether the behaviors are “healthy” or “unhealthy.” Most of the other characteristics reported in the table do not have statistically significant associations with the health-related behaviors. 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.

TABLE 8  
PERCENTAGE BIAS (Normalized Difference in Means between Grammar and Comprehensive Schools) BEFORE AND AFTER MATCHING FOR KEY COVARIATES:  
SUBSAMPLE OF GRAMMAR AND COMPREHENSIVE PUPILS

	Unmatched Sample	Matched Sample	t-Ratio (p-Value)
Relative ability age 11	107.2	−1.7	−.36 (.722)
Cognitive ability age 7	93.5	1.1	.21 (.830)
BSAG score	−44.1	2.9	.54 (.590)
Father’s social class high	48.4	.0	.00 (1.000)
Father’s social class middle	−32.4	.0	.00 (1.000)
Ill health age 7	−13.9	4.6	.71 (.480)
Cognitive ability age 11	143.9	37.2	7.45 (.000)

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

TABLE 9  
PERCENTAGE BIAS (Normalized Difference in Means between Secondary Modern and Comprehensive Schools) BEFORE AND AFTER MATCHING FOR KEY COVARIATES:  
SUBSAMPLE OF SECONDARY MODERN AND COMPREHENSIVE PUPILS

	Unmatched Sample	Matched Sample	t-Ratio (p-Value)
Relative ability age 11	−66.1	−1.1	−.22 (.824)
Cognitive ability age 7	−26.9	−1.0	−.19 (.851)
BSAG score	21.9	3.9	.66 (.509)
Father’s social class high	−11.5	.0	.00 (1.000)
Father’s social class middle	.4	.0	.00 (1.000)
Ill health age 7	9.4	2.0	.35 (.727)
Cognitive ability age 11	−28.8	32.5	6.66 (.00)

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

TABLE 10  
SELECTED REGRESSION RESULTS FOR PRESCHOOLING CHARACTERISTICS AND HEALTH-RELATED BEHAVIORS

	Smoking (Age 42)	Drinking (Age 42)	Vegetables (Age 33)	Fried Food (Age 33)	Smoking during Pregnancy
Cognitive ability at 7	-.006 (-.76)	1.07* (1.79)	.071* (2.89)	-.031 (-1.49)	-.017 (-.87)
BSAG	.006* (4.81)	.076 (.74)	-.001 (-.30)	-.000 (-.23)	.003 (.98)
Father's SES:					
Professional	-.125* (-4.64)	-.004 (-.00)	.018 (.24)	-.043 (-1.71)	-.061 (-1.90)
Other nonmanual	-.070* (-2.77)	.458 (.23)	-.060 (-.88)	-.018 (-.33)	-.085 (-1.36)
Sample size	2,377	1,994	2,292	2,291	373

Note.—The regression estimates are based on the unweighted sample. Robust *t*-ratios are given in parentheses. All regressions also condition on the full set of preschooling covariates. See App. table A1 for a full list.  
\* Statistically significant at least at the 10 percent level.

Health Outcomes

Table 11 shows selected results for the health outcomes: long-standing illness at age 46 and malaise at age 42.<sup>26</sup> 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.

*B. 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 behaviors and health outcomes. These are estimated for the full matched sample and condition on all the preschooling variables that are also used in the matching process, which span parental SES, 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 neighborhood. The models are estimated as weighted linear regressions with robust standard errors.<sup>27</sup> Throughout, in order to adjust standard errors for the use of fitted

<sup>26</sup> 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 the nonlinear regression models we estimated, and the rest of the paper focuses on linear regression results.

<sup>27</sup> Nonlinear versions of the models have been estimated as well, and the partial effects from these models show little difference from the linear specifications.

TABLE 11  
SELECTED REGRESSION RESULTS FOR PRESCHOOLING CHARACTERISTICS  
AND HEALTH OUTCOMES

	Long-Standing Illness (Age 46)		Malaise (Age 42)
	Linear Probability Model	Probit	
Cognitive ability at age 7	-.009 (-1.03)	-.009 (-1.01)	-.062 (-.86)
BSAG score	.005* (3.70)	.005* (3.76)	.049* (3.93)
Father's SES:			
Professional	-.025 (-.84)	-.024 (-.82)	-.317 (-1.47)
Other nonmanual	-.042* (-1.59)	-.041* (-1.60)	-.024 (-.12)
Sample size	2,497	2,497	2,369

Note.—See the note to table 10.  
\* Statistically significant at least at the 10 percent level.

values of the propensity score, we have bootstrapped these with 500 replications and reestimated the propensity score at each replication of the bootstrap procedure.

Health-Related Behaviors

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 behavior. Those with higher attainment are less likely to be smokers, to consume fried food, and to smoke during pregnancy. The majority of the school characteristics are not significantly associated with health-related behaviors in adulthood. Nonetheless, attendance at schools with a higher ratio of pupils per class is associated with a lower prevalence of maternal smoking during pregnancy.

Health Outcomes

Table 13 shows that, on average, lower educational attainment is associated with poorer mental health later in life and with a higher incidence of long-standing illness. Also, cohort members who attended single-sex schools appear to be less likely to suffer from chronic illness and disability in adulthood, although this effect is not always statistically significant at the conventional levels.

C. *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

TABLE 12  
EFFECT OF EDUCATIONAL ATTAINMENT AND SCHOOL CHARACTERISTICS  
ON HEALTH-RELATED BEHAVIORS

	Smoking (Age 42)	Drinking (Age 42)	Vegetables (Age 33)	Fried Food (Age 33)	Smoking during Pregnancy
Attainment	-.023* (-4.53)	-.196 (-.62)	.023 (1.32)	-.022* (-1.79)	-.023* (-2.11)
School characteristics:					
Single sex	-.016 (-1.43)	-1.806 (-.71)	-.112 (-.42)	-.039 (-.74)	.018 (.19)
Ability streaming	.015 (.34)	-2.76 (-1.58)	-.030 (-.10)	.102* (1.79)	-.018 (-.09)
No. pupils/teacher	-.969 (-.72)	55.79 (.78)	-3.42 (-.12)	-.530 (-.08)	-9.98* (-1.96)
No. expelled/total	-2.382 (-.07)	-156.4 (-1.55)	-75.18 (-1.38)	5.281 (.20)	10.73 (.14)
Sample size	1,992	1,674	1,928	1,927	297

Note.—The regression estimates are based on the matched sample. Bootstrapped z-ratios are given in parentheses. All regressions also condition on the full set of preschooling covariates and measures of school quality. See App. table A1 for a full list.  
\* Statistically significant at least at the 10 percent level.

TABLE 13  
EFFECT OF EDUCATIONAL ATTAINMENT AND SCHOOL CHARACTERISTICS  
ON HEALTH OUTCOMES

	Long-Standing Illness (Age 46)		Malaise (Age 42)
	Linear Probability Model	Probit	
Attainment	-.013* (-2.22)	-.014* (-2.23)	-.110* (-2.12)
School characteristics:			
Single sex	-.052 (-1.42)	-.058* (-1.43)	-.254 (-.92)
Ability streaming	.023 (1.10)	.025 (1.06)	.237 (1.28)
No. pupils/teacher	1.108 (.74)	1.183 (.71)	-1.147 (-.27)
No. expelled/total	9.532 (.83)	9.87 (.88)	86.33 (.98)
Sample size	2,101	2,101	1,984

Note.—See the note to table 12.  
\* Statistically significant at least at the 10 percent level.

subsamples. The first subsample consists of those who went to or would have gone to grammar schools and the second subsample consists of those who went to or would have gone to secondary moderns.  
Tables 14 and 15 show selected results for the impact of educational attainment on health-related behaviors. For both subsamples educational attainment has some statistically significant impacts on health-related behaviors: reducing the likelihood of being a smoker and increasing the frequency of eating vegetables. Among the secondary

TABLE 14  
EFFECT OF EDUCATIONAL ATTAINMENT AND SCHOOL CHARACTERISTICS ON HEALTH-RELATED BEHAVIORS: MATCHED SAMPLE OF GRAMMAR AND COMPREHENSIVE PUPILS

	Smoking (Age 42)	Drinking (Age 42)	Vegetables (Age 33)	Fried Food (Age 33)	Smoking during Pregnancy
Attainment	-.012* (-1.99)	-.584 (-1.17)	.033* (1.75)	-.0006 (-.04)	-.017 (-1.21)
School characteristics:					
Single sex	-.039 (-1.09)	-2.835 (-1.05)	-.120 (-1.14)	.049 (.59)	-.025 (-.31)
Ability streaming	.014 (.34)	1.333 (.40)	-.145 (-1.19)	.025 (.26)	.142 (1.56)
No. pupils/teacher	3.60 (1.19)	36.13 (.16)	-10.10 (-1.13)	-4.891 (-.71)	-7.00 (-.94)
No. expelled/total	25.66 (.69)	269.6 (1.17)	67.04 (.74)	171.6 (1.65)	-99.72 (-.58)
Sample size	683	593	667	667	152

Note.—The regression estimates are based on the matched subsample. Bootstrapped z-ratios are given in parentheses. All regressions also condition on the full set of preschooling covariates. See App. table A1 for a full list.

\* Statistically significant at least at the 10 percent level.

TABLE 15  
EFFECT OF EDUCATIONAL ATTAINMENT AND SCHOOL CHARACTERISTICS ON HEALTH-RELATED BEHAVIORS: MATCHED SAMPLE OF SECONDARY MODERN AND COMPREHENSIVE PUPILS

	Smoking (Age 42)	Drinking (Age 42)	Vegetables (Age 33)	Fried Food (Age 33)	Smoking during Pregnancy
Attainment	-.035* (-3.96)	1.123 (1.62)	.054* (2.41)	-.050* (-2.38)	-.042 (-1.22)
School characteristics:					
Single sex	.004 (.00)	1.172 (.39)	.040 (.40)	-.111 (-1.25)	.162 (1.04)
Ability streaming	.016 (.50)	-1.066 (-.48)	-.052 (-.64)	.058 (.78)	-.079 (-.65)
No. pupils/teacher	-2.341 (-.75)	378.4* (1.54)	-.985 (-.12)	.421 (.06)	-20.40* (-1.74)
No. expelled/total	14.79 (.84)	-162.7 (-1.29)	-35.37 (-.74)	73.53 (1.82)	36.39 (.48)
Sample size	1,029	850	989	989	118

Note.—See the note to table 14.

\* Statistically significant at least at the 10 percent level.

modern subsample, educational attainment also reduces the frequency of eating fried food.

Tables 16 and 17 show selected results for the impact of schooling on health outcomes for the matched subsamples. These suggest that the impact of educational attainment on adult health, at least with respect to mental health outcomes, is concentrated among those who either did attend or would have attended grammar schools, although estimates of this are not always statistically significant at the conventional levels.

TABLE 16  
EFFECT OF EDUCATIONAL ATTAINMENT AND SCHOOL CHARACTERISTICS ON HEALTH  
OUTCOMES: MATCHED SAMPLE OF GRAMMAR  
AND COMPREHENSIVE PUPILS

	Long-Standing Illness (Age 46)		Malaise (Age 42)
	Linear Probability Model	Probit	
Attainment	-.006 (-.85)	-.005 (-.67)	-.072 (-1.37)
School characteristics:			
Single sex	-.077* (-1.70)	-.079 (-1.60)	-.118 (-.42)
Ability streaming	-.023 (-.45)	-.021 (-.40)	-.119 (-.39)
No. pupils/teacher	1.540 (.39)	1.52 (.37)	14.80 (.64)
No. expelled/total	14.51 (.32)	15.55 (.32)	-127.4 (-.46)
Sample size	714	714	682

Note.—See the note to table 14.  
\* Statistically significant at least at the 10 percent level.

TABLE 17  
EFFECT OF EDUCATIONAL ATTAINMENT AND SCHOOL CHARACTERISTICS ON HEALTH  
OUTCOMES: MATCHED SAMPLE OF SECONDARY MODERN  
AND COMPREHENSIVE PUPILS

	Long-Standing Illness (Age 46)		Malaise (Age 42)
	Linear Probability Model	Probit	
Attainment	.005 (.57)	.005 (.52)	-.019 (-.27)
School characteristics:			
Single sex	-.055 (-1.36)	-.057 (-1.20)	-.641* (-2.09)
Ability streaming	-.012 (-.38)	-.013 (-.37)	-.031 (-.12)
No. pupils/teacher	-4.543 (-1.36)	-5.045 (-1.31)	-.354 (-.01)
No. expelled/total	13.66 (.77)	14.08 (.72)	66.23 (.60)
Sample size	1,087	1,087	1,025

Note.—See the note to table 14.  
\* Statistically significant at least at the 10 percent level.

V. Discussion

The economic literature on human development was initially centered 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 noncognitive skills, most notably in determining education (Heckman and Rubinstein 2001) and labor market outcomes (Feinstein 2000; Kuhn and Weinberger 2005; Heckman et al.

2006; Carneiro et al. 2007). This literature has suggested that cognitive and noncognitive 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 noncognitive skills have also been linked to a series of health and health-related behaviors. Heckman et al. (2006) find that both influence smoking in adolescence and teenage pregnancy, with noncognitive 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 noncognitive skills to be equally important. In addition, they find evidence of a link between cognitive and noncognitive skills and adult health status.

Our findings corroborate some of this earlier work. We find that noncognitive 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 at age 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 that 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 behaviors by father's occupational SES, with those whose father had a nonmanual 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 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 behavior 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 behaviors (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 some 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 ex-

perienced, a grammar school education and those who attended, or would have attended, secondary modern schools. When those who went to grammar schools are matched to comparable individuals who attended comprehensives, higher attainment is associated with lower rates of adult smoking and higher rates of the consumption of vegetables. Interestingly, however, the impact of attainment on health-related behaviors is larger and covers a wider range of behaviors for those who attended (or would have attended) secondary modern schools. Given that detrimental lifestyles are more prevalent in the latter subsample, 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.

There is also asymmetry in the impact of attainment on some of the health outcomes. For the subsample in which cohort members who attended grammar schools are matched with comparable individuals who attended comprehensives, we find positive and sizable, although not always statistically significant, effects for 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 subsample, which is partly generated by the fact that some of the group went to academically intensive grammar schools whereas others went to comprehensives, has more impact on health than variation in attainment in the latter subsample. 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 subsamples may also reflect a nonlinearity 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.

## Appendix

TABLE A1  
FULL SET OF PRESCHOOLING AND SECONDARY SCHOOL CHARACTERISTICS

---

Preschooling characteristics:
Cognitive ability score (age 7)
Relative rank of cognitive ability (age 11)
BSAG score (age 11)
Number of children 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 hospitalizations (age 7)
Indicator for diabetes in family
Indicator for epilepsy in family
Indicator for heart disease in family
Indicator for father chronically ill
Indicator for mother chronically ill
Indicator for father's occupational SES professional
Indicator for father's occupational SES other nonmanual
Indicator for single-parent family
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 employed in agriculture
Enumeration district: percentage in professional/managerial occupations
Enumeration district: percentage in other nonmanual occupations
Enumeration district: percentage in skilled manual occupations
Enumeration district: percentage in semiskilled manual occupations
Enumeration district: percentage in unskilled manual occupations
Enumeration district: percentage owner-occupiers
Enumeration district: percentage council tenants
Enumeration district: percentage nonwhite
Enumeration district: percentage immigrants
Indicator for change of residence between LEAs (age 7)
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

---

## References

- Albouy, V., and L. Lequien. 2008. "Does Compulsory Education Lower Mortality?" *J. Health Econ.* 28:155–68.
- Angrist, J. N., and V. Lavy. 1999. "Using Maimonides' Rule to Estimate the Effect of Class Size on Scholastic Achievement." *Q.J.E.* 114:533–75.
- Arendt, J. N. 2005. "Does Education Cause Better Health? A Panel Data Analysis Using School Reforms for Identification." *Econ. Educ.* 24:149–60.
- . 2008. "In Sickness and in Health—Till Education Do Us Part: Education Effects on Hospitalization." *Econ. Educ. Rev.* 27:161–72.

- Auld, M. C., and N. Sidhu. 2005. "Schooling, Cognitive Ability and Health." *Health Econ.* 14:1019–34.
- Black, S. E. 1999. "Do Better Schools Matter? Parental Evaluation of Elementary Education." *Q.J.E.* 114:577–99.
- Blackwell, M., S. Iacus, G. King, and G. Porro. 2009. "Cem: Coarsened Exact Matching in Stata." *Stata J.* 9:524–46.
- Card, D., and A. B. Krueger. 1992. "Does School Quality Matter? Returns to Education and the Characteristics of Public Schools in the United States." *J.P.E.* 100:1–40.
- Carneiro, P., C. Crawford, and A. Goodman. 2007. "The Impact of Cognitive and Non-cognitive Skills on Later Outcomes." Discussion Paper no. 92, Centre Econ. Educ., London School Econ.
- Case, A., A. Fertig, and C. Paxson. 2005. "The Lasting Impact of Childhood Health and Circumstance." *J. Health Econ.* 24:365–89.
- Chou, S.-Y., J.-T. Liu, M. Grossman, and T. Joyce. 2010. "Parental Education and Child Health: Evidence from a Natural Experiment in Taiwan." *American Econ. J.: Appl. Econ.* 2:33–61.
- Coneus, K., and M. Laucht. 2008. "The Effect of Early Noncognitive Skills on Social Outcomes in Adolescence." Discussion Paper no. 08-115, ZEW Centre European Econ. Res., Mannheim.
- Currie, J., and E. Moretti. 2003. "Mother's Education and the Intergenerational Transmission of Human Capital: Evidence from College Openings." *Q.J.E.* 118: 1495–1532.
- Currie, J., and M. Stabile. 2003. "Socioeconomic Status and Health: Why Is the Relationship Stronger for Older Children?" *A.E.R.* 93:1813–23.
- Cutler, D., and A. Lleras-Muney. 2008. "Education and Health: Evaluating Theories and Evidence." In *Making Americans Healthier: Social and Economic Policy as Health Policy*, edited by R. F. Schoeni, J. S. House, G. Kaplan, and H. Pollack. New York: Russell Sage Found.
- . 2010. "Understanding Differences in Health Behaviors by Education." *J. Health Econ.* 29:1–28.
- Dearden, L., J. Ferri, and C. Meghir. 2002. "The Effect of School Quality on Educational Attainment and Wages." *Rev. Econ. and Statis.* 84:1–20.
- Dustmann, C., N. Rajah, and A. van Soest. 2003. "Class Size, Education and Wages." *Econ. J.* 113:F99–F120.
- Feinstein, L. 2000. "The Relative Economic Importance of Academic, Psychological, and Behavioural Attributes Developed in Childhood." Discussion paper, Centre Econ. Performance, London.
- Feinstein, L., and J. Symons. 1999. "Attainment in Secondary School." *Oxford Econ. Papers* 51:300–321.
- Galindo-Rueda, F., and A. Vignoles. 2004. "The Heterogeneous Effect of Selection in Secondary Schools: Understanding the Changing Role of Ability." IZA Discussion Paper no. 1245, Inst. Study Labor, Bonn.
- . 2005. "The Declining Relative Importance of Ability in Predicting Educational Attainment." *J. Human Resources* 40:335–53.
- Grimard, F., and D. Parent. 2007. "Education and Smoking: Were Vietnam War Draft Avoiders Also More Likely to Avoid Smoking?" *J. Health Econ.* 26:896–926.
- Grossman, M. 2006. "Education and Nonmarket Outcomes." In *Handbook of the Economics of Education*, edited by E. Hanushek and F. Welch. Amsterdam: Elsevier.
- Heckman, J. J. 2008. "Role of Income and Family Influence on Child Outcomes." *Ann. New York Acad. Sci.* 1136:307–23.
- Heckman, J. J., and Y. Rubinstein. 2001. "The Importance of Noncognitive Skills: Lessons from the GED Testing Program." *A.E.R.* 91:45–49.

- Heckman, J. J., J. Stixrud, and S. Urzua. 2006. "The Effect of Cognitive and Noncognitive Skills on Labor Market Outcomes and Social Behavior." *J. Labor Econ.* 24:411–81.
- Ho, D. E., K. Imai, G. King, and E. A. Stuart. 2007. "Matching as Nonparametric Preprocessing for Reducing Model Dependence in Parametric Causal Inference." *Polit. Analysis* 15:199–236.
- Hoxby, C. 2000. "The Effects of Class Size on Student Achievement: New Evidence from Population Variation." *Q.J.E.* 115:1239–85.
- Imbens, G. W., and J. M. Wooldridge. 2008. "Recent Developments in the Econometrics of Program Evaluation." Working Paper no. 14251, NBER, Cambridge, MA.
- Jesson, D. 2000. "The Comparative Evaluation of GCSE Value-Added Performance by Type of School and LEA." Discussion Paper no. 2000/52, Univ. York.
- Keaster, R. 2009. "Adolescent Cognitive and Non-cognitive Correlates of Adult Health." Working Paper no. 14924, NBER, Cambridge, MA.
- Kenkel, D., D. Lillard, and A. Mathios. 2006. "The Roles of High School Completion and GED Receipt in Smoking and Obesity." *J. Labor Econ.* 24:635–60.
- Kerckhoff, A. C., K. Fogelman, D. Crook, and D. Reeder. 1996. *Going Comprehensive in England and Wales: A Study of Uneven Change*. London: Woburn.
- Kuhn, P., and C. Weinberger. 2005. "Leadership Skills and Wages." *J. Labor Econ.* 23:395–436.
- LaLonde, R. J. 1986. "Evaluating the Econometric Evaluations of Training Programs with Experimental Data." *A.E.R.* 76:604–20.
- Lindeboom, M., A. Llena-Nozal, and B. Van der Klaauw. 2009. "Parental Education and Child Health: Evidence from a Schooling Reform." *J. Health Econ.* 28:109–31.
- Lleras-Muney, A. 2005. "The Relationship between Education and Adult Mortality in the United States." *Rev. Econ. Studies* 72:189–221.
- Manning, A., and S. Pischke. 2006. "Comprehensive versus Selective Schooling in England and Wales: What Do We Know?" IZA Discussion Paper no. 2072, Inst. Study Labor, Bonn.
- Maurin, E., and S. McNally. 2007. "Educational Effects of Widening Access to the Academic Track: A Natural Experiment." IZA Discussion Paper no. 2596, Inst. Study Labor, Bonn.
- Mazumder, B. 2008. "Does Education Improve Health? A Re-examination of the Evidence from Compulsory Schooling Laws: Economic Perspectives." *Fed. Reserve Bank Chicago Econ. Perspectives* 32 (2): 2–16.
- OECD. 2005. "School Factors Related to Quality and Equity—Results from PISA 2000." Manuscript, OECD, Paris.
- Oreopoulos, P. 2006. "Estimating Average and Local Average Treatment Effects of Education When Compulsory Schooling Laws Really Matter." *A.E.R.* 96: 152–75.
- Plewis, I., L. Calderwood, D. Hawkes, and G. Nathan. 2004. "Changes in the NCDS and BCS70 Populations and Samples over Time." Manuscript, Centre Longitudinal Studies, London.
- Power, C., and C. Peckham. 1987. "Childhood Morbidity and Adult Ill-Health." User Support Working Paper no. 37, Nat. Child Development Study, London.
- Rodgers, B., A. Pickles, C. Power, S. Collishaw, and B. Maughan. 1999. "Validity of the Malaise Inventory in General Population Samples." *Social Psychiatry and Psychiatric Epidemiology* 34:333–41.
- Rosa Dias, P. 2009. "Inequality of Opportunity in Health: Evidence from a UK Cohort Study." *Health Econ.* 18 (9): 1057–74.
- Rosenbaum, P. R., and D. B. Rubin. 1983. "The Central Role of the Propensity Score in Observational Studies for Causal Effects." *Biometrika* 70:41–55.

- Rubin, D. B. 1973. "The Use of Matched Sampling and Regression Adjustments to Remove Bias in Observational Studies." *Biometrics* 29:185–203.
- . 1979. "Using Multivariate Matched Sampling and Regression Adjustment to Control Bias in Observational Studies." *J. American Statis. Assoc.* 74:318–28.
- Rutter, M., J. Tizard, and K. Whitmore. 1970. *Education, Health and Behaviour*. London: Longman.
- Silles, M. 2009. "The Causal Effect of Education on Health: Evidence from the United Kingdom." *Econ. Educ. Rev.* 28:122–28.
- Stott, D. H. 1987. *The Social Adjustment of Children: Manual to the Bristol Social Adjustment Guides*. London: Hodder and Stoughton.
- Van Kippersluis, H., O. O'Donnell, and E. Van Doorslaer. 2009. "Long Run Returns to Education: Does Schooling Lead to an Extended Old Age?" Discussion Paper no. 037/3, Tinbergen Inst.
- Webbink, D., N. G. Martin, and P. M. Visscher. 2010. "Does Education Reduce the Probability of Being Overweight?" *J. Health Econ.* 29:29–38.