Parental education and children’s cognitive development: A prospective approach

Markus Klein (corresponding author: markus.klein@strath.ac.uk)
University of Strathclyde, UK

Michael Kühhirt

Abstract

Using nationally representative data from the 1970 British Cohort Study (BCS70), which followed cohort members and their children (N = 1,042, ages 3 to 16), this paper estimates the effect of parental education on children's cognitive development. Previous analyses disregarded selective patterns of family formation, which may introduce endogenous selection bias. In addition, genetic confounding may partially explain the association between parental education and children’s cognitive development. We take advantage of the BC70’s multigenerational design and use inverse probability of censoring and treatment weighting to address non-random selection into parenthood and confounding via parental cognitive ability as a genetic proxy. After correcting for these biases, the effect of parental education on children’s cognitive development is substantially reduced and statistically non-significant.

Keywords: parental education, family socioeconomic status, cognitive development, genetic confounding, endogenous selection bias

Introduction

In early childhood, children from higher socioeconomic status (SES) backgrounds perform better on various cognitive outcomes than children from lower SES backgrounds (Bradley & Corwyn, 2002). Family SES is conceptualised through the lens of capital, wherein differential access to financial, human, and social capital is associated with varying child development (Coleman, 1988). Although SES dimensions such as parental education, occupation, and family income tend to be correlated, each dimension measures a distinct resource that uniquely influences children's cognitive development (Duncan & Magnuson, 2003). When these SES dimensions are considered jointly, parental education appears to be the strongest predictor of children's cognitive and academic development (Davis-Kean, 2005; Reardon, 2011). In the United States, for instance, children whose parents have a college degree have a test score advantage of more than 0.5 standard deviations over children whose parents have a high school diploma (Duncan et al., 2012).

Conventional analyses of child development retrospectively link children's developmental outcomes to their parents' characteristics (e.g., education). However, this approach excludes childless individuals and disregards family formation mechanisms, potentially introducing *endogenous selection bias* into estimates of the effect of parental education on children's cognitive development (Elwert & Winship, 2014). In light of recent advances in the analysis of intergenerational social reproduction (Breen & Ermisch, 2017; Lawrence & Breen, 2016; Song & Mare, 2015), we propose a prospective approach incorporating the effects of parental education on fertility into the analysis of children's developmental outcomes.

Using a prospective method will also allow us to condition on early parental characteristics (e.g., parental cognitive ability, birth weight, parental attitudes) when estimating the association between parents' education and their children's cognitive outcomes. A central question in the literature is whether parents' level of education is the cause of differences in children's cognitive ability (Duncan et al., 2017; Duncan & Magnuson, 2012). This is because their early-life human capital endowment may vary, resulting in disparities in their educational attainment and their children's cognitive development. Increasing evidence suggests, for instance, that parents' early cognitive abilities are strongly associated with their children's cognitive abilities (e.g., Crawford et al., 2011; Sullivan et al., 2021). The association between parental education and children's cognitive development may be due to *genetic confounding*, i.e., the fact that parents and children share genes related to cognitive ability. To address genetic confounding, we will estimate associations between parental education and children's cognitive development using the familial control method and adjusting for parents' cognitive ability as a genetic proxy (S. Hart et al., 2021).

The article contributes to the literature by estimating the causal effect of parental education on children's cognitive development by 1) correcting for selective fertility using inverse probability of censoring weighting and 2) addressing (genetic) confounding using parental characteristics that are typically unavailable.

Pathways between parental education and children's cognitive development

The human capital of parents "provides the potential for a cognitive environment for the child that aids learning" (Coleman, 1988, p. 109). The amount of human capital in a family determines the quality and quantity of parent-child interactions and the availability of a stimulating learning environment deemed advantageous for the cognitive development of children (Nisbett et al., 2012; Shonkoff & Phillips, 2000). Parental time spent with children in educational activities appears to be the most productive input for cognitive development (Del Bono et al., 2016; Fiorini & Keane, 2014). For example, mother-child reading time significantly improved children's reading achievement (Barnes & Puccioni, 2017; Kalb &

Van Ours, 2014; Price & Kalil, 2019). In addition, the quantity and quality of linguistic input directed at children in their social environment have a substantial impact on language acquisition and vocabulary development (B. Hart & Risley, 1995; Hurtado et al., 2008; Weisleder & Fernald, 2013).

Through their educational attainment, parents may develop cognitive flexibility (e.g., learning to think in complex ways), problem-solving ability (e.g., hypothesis testing), language skills, and skills for synthesizing and evaluating the information on child-rearing that is beneficial for children's cognitive development (Davis-Kean et al., 2021; Harding et al., 2015). In addition, highly educated parents spend more time with their children and use this time more effectively for cognitively stimulating activities with their children, such as shared reading, telling stories, reciting rhymes, singing songs, and creating art (Altintas, 2016; Dotti Sani & Treas, 2016; Kalil et al., 2012; Sayer et al., 2004; Suizzo & Stapleton, 2007). They also devote much of their budget to cognitively enriching materials and activities, such as books, magazines, school supplies, and library and museum visits (Kaushal et al., 2011; Tighe & Davis-Kean, 2021). Additionally, highly educated parents may benefit from social networks that provide their children with valuable knowledge, skills, and resources for their cognitive development (Harding et al., 2015). Furthermore, maternal education was positively correlated with childcare arrangements (i.e., type, quality, and quantity) deemed advantageous for children's cognitive development (Augustine et al., 2009).

Due to their parenting knowledge and skills (Bornstein et al., 2010; Rowe et al., 2016), highly educated parents better understand how to tailor high-quality activities to their children's developmental level (Benasich & Brooks-Gunn, 1996; Kalil et al., 2012). In addition, parents with a higher level of education communicate more verbally and abstractly because they were exposed to this type of language and discourse for a longer time in formal school settings (Rowe, 2017). Therefore, they speak to their children more frequently, use a greater variety of vocabulary, are more responsive to their children, and encourage more child speech than parents with lower levels of education (Hoff, 2003; Rowe, 2008; Vernon-Feagans et al., 2020).

Parental education can indirectly influence children's cognitive development through increased family income. According to the family investment model, greater financial resources are advantageous for providing children with a stimulating learning environment (Haveman & Wolfe, 1994). For example, a higher family income enables parents to invest in educational resources such as toys, books, and computer programmes that foster cognitive development in their children (Guo & Harris, 2000). In addition, financial resources enable parents to avoid compromising their children's development through substandard housing, neighbourhood conditions, child nutrition, and health (Evans & Kim, 2007; Shonkoff & Phillips, 2000). According to the family stress model (Conger et al., 2010), economic deprivation increases family stress. Psychological distress among parents will result in mental health issues, increased family conflict, an increased risk of separation, and the use of unresponsive parenting styles that are detrimental to the cognitive development of children (Conger et al., 1994; Shonkoff & Phillips, 2000).

Current research

Past research shows that parental education and children's cognitive development are strongly correlated (Bradley & Corwyn, 2002; Davis-Kean, 2005; Davis-Kean et al., 2021; Mercy & Steelman, 1982). Compared to other SES dimensions (e.g., family income, parental occupation), parental education appears to be the strongest predictor of children's cognitive achievement (Davis-Kean, 2005; Reardon, 2011). In recent years, scholars of child development have advocated for more comprehensive examinations of whether and to what extent parents' socioeconomic status influences children's developmental outcomes (Duncan et al., 2017; Duncan & Magnuson, 2012). Although most research has focused on the causal effect of parental education on offspring's educational attainment (for an overview, see Fleury & Gilles, 2018; Holmlund et al., 2011), emerging literature focuses on identifying the causal relationship between parental education and children's early developmental outcomes.

One line of research sought to identify the causal effect of parental education on children's cognitive development via an instrumental variables (IV) approach (Andrabi et al., 2012; Carneiro et al., 2013; Cuartas, 2022; Dickson et al., 2016; Gennetian et al., 2008; Lundborg et al., 2014). Instruments included compulsory schooling reforms in Sweden and the UK (Lundborg et al., 2014; Dickson et al., 2016), random assignment to an educational and job training program in the US (Gennetian et al., 2008), variation in schooling costs in the US (Carneiro et al., 2013), the availability of girls' schools in Pakistan when mothers were school-aged (Andrabi et al., 2012), and a universal primary education reform in Uganda (Cuartas, 2021). The effect of maternal education on children's cognitive development was positive and statistically significant across all contexts and instrument types.

However, these results are contingent upon the strong assumption of instrument validity, i.e., that the instrument does not directly influence children's cognitive development and that there are no unobservable confounding variables between the instrument and outcome. This assumption cannot be tested, so its justification must be based on theoretical reasoning and research knowledge. Moreover, the IV estimates the local average treatment effect (LATE), which may only apply to a subset of the target population. Assuming instruments are exogenous, it has been questioned whether the inferences derived from standard Two-Stage Least Squares (2SLS) estimation practises are valid. Based on a comprehensive sample of 1309 instrumental variable regression published in economics journals and using Monte Carlo simulations, the jackknife and multiple forms of bootstrap, Young (2022) found that IV has little power as it rarely rejects the OLS point estimate or the null that OLS is unbiased, while its statistical significance is exaggerated.

Another line of inquiry examined whether increases in maternal education among mothers with already-born children enhance the cognitive development of their children (Augustine & Negraia, 2018; Awada & Shelleby, 2021; Breinholt & Holm, 2020; Harding, 2015; Magnuson, 2007; Magnuson et al., 2009). Some studies found positive effects of additional maternal schooling on children's cognitive or academic achievement (Awada & Shelleby, 2021; Harding, 2015; Magnuson, 2007; Magnuson et al., 2009), while others found no effect (Augustine & Negraia, 2018; Breinholt & Holm, 2020). These contradictory results may be attributable to methodological factors. The studies that found positive effects did not focus on education changes within mothers. In contrast, those that found null results used a mother or sibling fixed effects design to account for unobserved time-constant heterogeneity. This suggests that mothers who increase their education after childbirth differ from those who maintain the same educational level in terms of unobserved characteristics. Moreover, the findings of this design are limited to the lower end of the educational distribution, pertain to a small subset of the population, and are therefore inapplicable to the entire population.

A prospective approach

Due to a lack of prospective data across generations, most studies on child development, including those examining the causal relationship between parental education and children's outcomes, retrospectively link child and parent characteristics in child cohort data. However, this design does not account for selective mechanisms of family formation because it excludes childless individuals and their educational attainment from the analysis. For example, it is known that highly educated women have higher rates of childlessness than less educated women and are delaying motherhood (Fort et al., 2016; Gustafsson, 2001; Kravdal & Rindfuss, 2008; Nisén et al., 2014; Wood et al., 2014). Therefore, if not appropriately adjusted, these selective fertility patterns may introduce *endogenous selection bias* into estimates of the effect of parental education on children's cognitive development (Elwert & Winship, 2014).

We adopt a *prospective approach* (Breen & Ermisch, 2017; Lawrence & Breen, 2016; Song & Mare, 2015) to study the causal effect of parents' education on children's cognitive development to circumvent this issue in conventional analyses. This prospective approach starts with a birth cohort and follows it forward to understand how it reproduces itself socially' (Breen & Ermisch, 2017, p. 591). In our study, we examine children's development in 2004 among a subsample of those who became parents, allowing us to include selective fertility in the analysis of the association between parental education and cognitive outcomes.

Adopting this prospective methodology will allow us to condition on a rich set of grandparent and early parent characteristics that may influence parents' education and children's cognitive development. For instance, parents may have attitudes towards education and socioemotional skills that help them succeed in education and provide their children with a stimulating environment. Instead of locating a valid instrumental variable and, considering its limitations, we will examine the omitted-variable problem by directly observing covariates that are typically unavailable.

Notably, a prospective design permits us to control for *genetic confounding* via the familial control method, i.e., by using the information on parents' early cognitive ability as a genetic proxy (S. Hart et al., 2021). To determine whether parental education has a causal relationship with children's cognitive development, we need to distinguish environmental from genetic origins (Conley et al., 2015; Liu, 2018). Evidence suggests strong associations between parents' cognitive abilities as children and their children's early cognitive outcomes (Anger & Heineck, 2010; Brown et al., 2011; Crawford et al., 2011; Sullivan et al., 2021). Moreover, the cognitive ability of parents is strongly related to their educational attainment, occupational status, and income (Strenze, 2007).

Parental cognitive ability can confound the relationship between parental education and children's cognitive outcomes through two mechanisms. First, the parent's genotype associated with their educational attainment is inherited by the child. Second, cognitively competent parents transmit cognitive skills to their children through environmental mechanisms (e.g., parenting) rather than genetic inheritance. For instance, findings by Wertz et al. (2020) suggest that mothers' genetics influence children's educational attainment over and above children's genetics via cognitively stimulating parenting.

Few studies have taken parental cognitive ability into account when estimating the relationship between family socioeconomic status and children's cognitive outcomes. For instance, parental cognitive ability accounted for half the cognitive test score gap between children from high-income and low-income families in the UK (Crawford et al., 2011). Similarly, the association between parental education and children's language ability in the UK was nearly halved when maternal and partner language ability was controlled for (Sullivan et al., 2021). For the US, Marks and O'Connell (2021) demonstrated that the cognitive ability of the mother accounts for the majority of the effect of a composite SES score on children's cognitive development (60% for vocabulary; 54% for digit memory; around 60% for reading comprehension, reading recognition, and mathematics). However, none of these studies addressed the possibility of endogenous selection bias when investigating the relationship between family socioeconomic status and children's cognitive outcomes.

Causal model

Figure 1 depicts the hypothesized causal relationships between parental education (X) and children's cognitive development (Y) in the presence of grandparental (G) and parental (P) confounders as well as the collider of having children (C). To avoid (genetic) confounding bias and estimate the causal effect of parental education on children's cognitive development, we must condition on G and P. (including parental cognitive ability). The second problem shown in this causal model is endogenous selection bias or collider bias. We condition on a collider (parenthood) and induce a non-causal association between X and Y via X->C->U->Y by estimating the association between educational attainment in the previous generation and child cognitive development in the latter generation among parents.

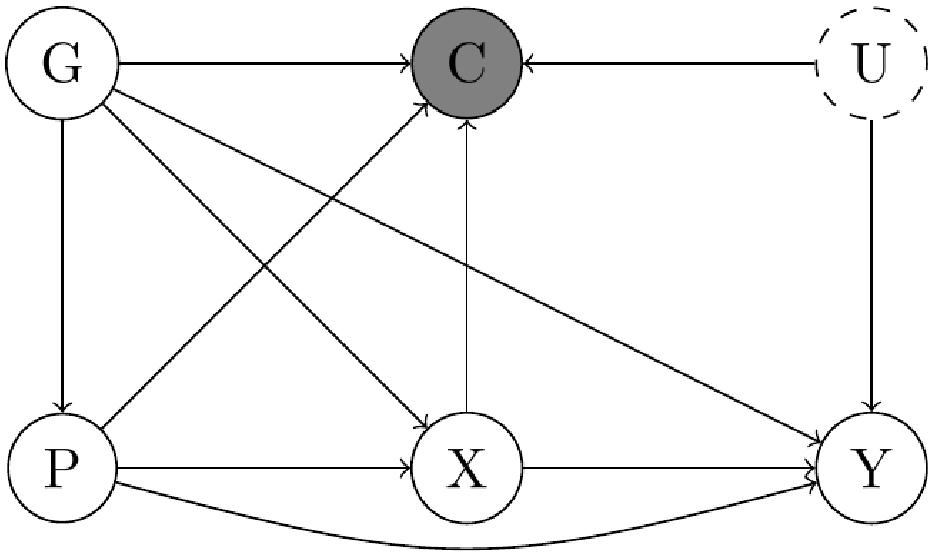

Figure 1. Causal model. G = Grandparent characteristics (CM parent characteristics); P = Parent characteristics (CM characteristics); X = Parental education (CM education); C = having (natural or adopted) children; Y = Child cognitive ability; Dashed border means U is unmeasured.

Data and Methods

Data

The 1970 British Cohort Study (BCS70) is a representative cohort study of individuals born in England, Scotland, and Wales in a single week in 1970 (Elliott & Shepherd, 2006). Data for cohort members (CMs) were collected at birth, ages 5, 10, 16, 26, and every four years beginning at age 30. Notably, at the age of 34, half of the CMs who lived with their natural and adopted children were randomly selected for additional interviews and assessments of their children. The prospective study design allows us to consider the characteristics of CMs' parents (i.e., grandparents), CMs' characteristics, including their early cognitive ability and educational attainment, whether CMs live with their natural or adopted children, and their children's cognitive assessments. The early characteristics of CMs, like cognitive ability and their parents' characteristics, are derived from wave 1 (birth, Chamberlain, 2013) and wave 3 (age 10, Butler & Bynner, 2016). Wave 7 (age 34, University of London, 2016) measures CMs' educational attainment, whether they have children and live with them, and their children's cognitive assessments.

Measures

Our outcome is the cognitive ability of the CM's first-born child (Y in Figure 1), as measured by the British Ability Scales (BAS) Second Edition when CMs were 34 years old. The BAS Second Edition is a commonly administered battery of cognitive ability tests for children aged 2.5 to 17 years (Elliott, 1996, 1997). To measure children's *verbal ability*, three- to five-year-olds were given the Naming Vocabulary test, while children aged six to sixteen were given the Word Reading test. To assess children's expressive language ability and knowledge of nouns, we use the Naming Vocabulary task, which asked children to identify various objects in a coloured picture booklet. The Word Reading task required students to read from a printed list of words. To measure children's *numerical ability*, we relied on the Early Number Concepts test among younger children and the Number Skills test among older children. In the Early Number Concepts task, children were given a series of simple arithmetic tasks, such as counting and evaluating quantities. In the Number Skills task, children were given a series of mathematical problems. All tests use test scores that account for differences in item difficulty. We age-normalized test scores using the residuals from a regression of test scores on age and all other variables used in the analyses (Crawford et al., 2011).

Our exposure is the CM's *highest educational qualification* at age 34 (depicted as X in Figure 1). It is operationalized as a binary treatment indicating whether cohort members earned an undergraduate degree or higher.

Our covariates include information on *CM characteristics* that may influence CMs' educational attainment, their likelihood of living with children and their children's cognitive development (depicted as P in Figure 1): birth weight (in grams), cognitive ability, number of siblings (none, one, two, three, more than three), locus of control, problem behavior, and partner's education.

Four sub-scales of the British Ability Scales assessed *CM's cognitive ability* at age ten: word definition, word similarities, recall of digits, and matrices (Elliott et al., 1979). We derived a general cognitive ability score from a principal component analysis and standardized it to a mean of 0 and a standard deviation of 1 (Connelly & Gayle, 2019; Schoon, 2010).

The psychosocial measure of *CM's locus of control* refers to the extent to which individuals view themselves as able to control their destinies (internal) as opposed to external forces (external). In the BCS70, ten-year-old cohort members completed the CARALOC questionnaire (Gammage, 1982), a general locus of control measure whose raw scores range from 0 to 15 and for which higher scores indicate greater internalization. Then, standard scores are computed from these raw scores.

CM's problem behavior is measured with the Rutter Behavior Scale at age 10 as reported by CM's mother (Rutter et al., 1970). The Rutter Behavior Scale is a well-established set of questions for measuring children's behavioral difficulties. The BCS70 at age 10 used a visual analog scale ranging from 0 (does not apply) to 100 (certainly applies) for each of the 19 questions. The total Rutter score is comprised of the sum of the individual variables. For each scale, categorical ratings were calculated by dividing scores into three severity levels: "normal" scores below the 80th percentile, "moderate" problem scores between the 80th and 95th percentile, and "severe" problem scores above the 95th percentile.

CM partner's education distinguishes between 1) no partner, 2) partner left education at age 16 or younger, 3) partner left education at age 17/18, 4) partner left education at age 19-22, and 5) partner left education at age 23+. The summary statistics are provided for all variables in Table S1 in the Supplementary Material.

We further consider *CM parent characteristics* gathered when CMs were aged 10 (depicted as G in Figure 1). *CM parent education* is measured as the highest educational qualification among CM’s parents and is operationalized as a binary indicator distinguishing between ‘undergraduate degree or higher’ and ’below undergraduate degree’. *CM parent income* is determined by the total gross weekly family income and is derived from a banded income question: ‘Less than £35 per week`, ‘£35 to 49£ per week’, ‘£50 to £99 per week’, ‘£100 to £149 per week’, ‘£150 to £199 per week’, ‘£200 to £249 per week’, ‘More than £250 per week’. Finally, *CM parent educational aspirations* is a binary measure indicating whether the cohort member's mother intended their child to pursue higher education after leaving school.

Analytic strategy

Estimating the effect of parental education (i.e., CMs’ highest educational qualification) on children’s cognitive development presents two significant challenges: (genetic) confounding and non-random selection into parenthood (i.e., systematic censoring of living with natural or adopted children). To prevent confounding bias and endogenous selection bias, we use inverse probability of treatment and censoring weighting (Hernan & Robins, 2020). Instead of explicitly controlling for measured covariates in our outcome model, we regress children’s cognitive ability on parental education in a weighted pseudo-population in which parental education is independent of our measured covariates and parenthood is independent of both parental education and covariates.

Formally, the inverse probability of treatment (IPT) weight *tw* is defined as the ratio of the unconditional probability that cohort member *i* earned an undergraduate degree or higher *x* and the same probability conditional on the covariates of CM parent and CM characteristics (depicted as G and P in Figure 1) measured prior to qualification attainment,

$$tw_i = \frac{P(X_i=x_i)}{P(X_i=x_i|G_i,P_i)}. \quad (1)$$

This weight creates a pseudo-population in which CMs with covariate values that are overrepresented in the observed degree or higher group are given less weight, and respondents with covariate values that are less frequent are given more weight. Thus, confounders are distributed equally across both CM qualification groups after weighting.

Reweighing with inverse probability of censoring (IPC) weights,

$$cw_i = \frac{P(C_i=0)}{P(C_i=0|X_i,G_i,P_i)}, \quad (2)$$

corrects for non-random censoring based on CMs' education and covariates. Using the cw_i weights generates a pseudo-population that would have been observed if living with natural or adopted children between the ages of 3 and 16 had been random with respect to CMs' education and covariates. Since living with children in those age groups was measured when CM was 34, the censoring weights address cohort members' selective childlessness and non-random delay in parenthood (after age 31) based on CMs' education and covariates.

Using the product of the two weights to reweight the uncensored sample simultaneously corrects for confounding by the measured covariates and non-random censoring based on CMs' education and covariates. Because all probabilities in equations 1 and 2 are unknown, they were estimated using logistic regressions, respectively (see Tables S2 and S3 in the Supplementary Material for the models estimating both denominators).

Given that covariates are not included in the outcome model, inverse probability weighting has the benefit of avoiding misspecification bias which can occur when interactions between exposure and covariates (and between covariates) are not explicitly modelled in a conventional regression approach. Consequently, the weighted estimate for parental degree corresponds directly to the average difference in children's verbal or numerical ability (Elwert & Winship, 2010; Morgan & Todd, 2008)

Under the assumptions of no unmeasured confounding and systematic censoring, positivity, and correct parametric specification of the weight models, the mean differences in the weighted pseudo-populations provide consistent estimators for the average treatment effect of parental education on children's cognitive ability. Positivity requires a nonzero probability of parental degree attainment for any combination of covariate values to ensure a "like with like" comparison. As a result of violations of positivity and misspecifications of the weight models, estimated weights have mean values far from one or large standard deviations (Cole & Hernán, 2008). Table S4 in the Supplementary Material demonstrates that neither of these conditions applied to our weights.

Findings

We present our findings in three steps. First, we show mean differences in covariates by CMs' educational attainment. Second, we display how censored and uncensored samples differ with regard to CMs' education and covariates. The censored sample consists of CMs who do not have children between the ages of 3 and 16 in their households. Finally, we present estimated differences in children's verbal and numerical ability by their parents' education. We report unadjusted differences, estimates after covariate adjustment through IPT weights, estimates after adjustment via IPC weights for systematic censoring based on CMs' education and covariates and estimates taking into account the product of IPT and IPC weights.

Covariate differences by CMs' educational attainment

Based on means for continuous variables and percentages for categorical variables, Table 1 depicts covariate differences by CMs' education for the full analytic sample (including those CMs who do not live with natural or adoptive children).

Table 1. Means and percentages for covariates by cohort members' education.

	Degree or higher	Below degree
CM characteristics		
CM cognitive ability	0.58	-0.11
CM birthweight in grams	3357.73	3295.30
CM number of siblings		
None	10.01	9.60
One	51.77	46.40
Two	27.82	29.50
Three	7.00	9.87
More than three	3.40	4.62
CM locus of control	8.30	6.95
CM problem behavior		
Normal (below the 80th percentile)	87.73	80.28
Moderate (between 80th and 95th percentile)	10.41	15.19
Severe (above 95th percentile)	1.87	4.53
CM partner's education		
No partner	29.22	26.17
Partner left education at age 16 or younger	26.75	51.08
Partner left education at age 17/18	17.75	17.77
Partner left education at age 19-22	10.14	2.19
Partner left education at age 23+	16.14	2.79
CM parent characteristics		
CM parent education: degree or higher	33.76	9.81
CM parent income (weekly) in £		
Less than £35 per week	0.93	1.98
£35 to 49£ per week	2.33	4.29
£50 to £99 per week	20.75	31.75
£100 to £149 per week	33.82	38.69
£150 to £199 per week	22.08	14.83
200 to £249 per week	10.54	4.83
More than £250 per week	9.54	3.63
CM parent aspirations: pursue higher education: Yes	30.22	10.59
N	1,499	3,332

Source: British Cohort Study (BCS70). *Note.* Statistics pertain to full CM sample, including CMs without natural or adopted children living in the household. CM = Cohort member.

CMs with a degree or higher had, on average, a significantly higher cognitive ability, a higher locus of control score, i.e., they had a stronger belief that they control their destinies and exhibited less problem behaviour than CMs without a degree. Highly educated CMs had fewer siblings than those with lower levels of education. While CMs with a degree were more likely to be single, their partners were more likely to have left school later if they had one. There were no significant differences in birth weight between these educational groups.

Regarding parent CM characteristics, one-third of CMs with a degree had a degree-holding parent, compared to only 10% of CMs without a degree. CMs with a degree tended to have parents with a higher income than CMs without a degree. Lastly, parents of CMs with a degree had significantly greater aspirations for their children's pursuit of higher education than parents without a degree. The denominator treatment weight model (see Table S2 in the Supplemental Material) indicates that the effects of CMs' cognitive ability, their partner's education, and their parents' educational aspirations on the likelihood of obtaining a degree are statistically significant at the 5%-level.

Exposure and covariate differences by CM’s censoring status

Table 2 compares CM’s education (exposure) and covariates between the sample of CMs living with their children (uncensored sample) and those without children in the household at age 34 (censored sample). The table shows that CMs with a degree or higher were more prevalent among the censored, i.e., they were more likely to be found in childless households. In addition, cognitive ability was significantly greater in the censored sample than in the uncensored sample. The censored CMs exhibited a slightly higher locus of control and fewer problem behaviours than the uncensored CMs, but the differences were not particularly pronounced. There were no discernible differences in birth weight between the censored and uncensored samples. CMs in the censored sample tend to have fewer siblings than CMs in the uncensored sample. Significantly, the share of CMs with no partner and a more educated partner is much higher in the censored than the uncensored sample. In the censored sample, the education, income, and aspirations for higher education of CMs' parents are also somewhat higher.

Table 2. Means and percentages for covariates and cohort members' education by censoring status.

	Censored	Uncensored
CM characteristics		
CM degree or higher: Yes	38.61	23.08
CM cognitive ability	0.22	-0.26
CM birthweight in grams	3316.55	3313.48
CM number of siblings		
None	9.81	9.60
One	50.69	45.22
Two	27.74	30.34
Three	8.72	9.31
More than three	3.04	5.52
CM locus of control	7.62	7.10
CM problem behavior		
Normal (below the 80th percentile)	83.54	81.60
Moderate (between 80th and 95th percentile)	13.10	14.36
Severe (above 95th percentile)	3.37	4.04
CM partner's education		
No partner	40.43	12.79
partner left education at age 16 or younger	29.48	58.48
partner left education at age 17/18	15.33	20.40
partner left education at age 19-22	5.72	3.57
partner left education at age 23+	9.04	4.76
CM parent characteristics		
CM parent education: degree or higher	21.21	13.13
CM parent income (weekly) in £		
Less than £35 per week	1.54	1.78
£35 to 49£ per week	3.57	3.82
£50 to £99 per week	26.40	30.26
£100 to £149 per week	36.94	37.48
£150 to £199 per week	17.60	16.53
200 to £249 per week	7.38	5.78
More than £250 per week	6.57	4.33
CM parent aspirations: pursue higher education: Yes	19.26	13.98
N	2,466	2,353

Source: British Cohort Study (BCS70). *Note.* Uncensored = observed with valid information on natural or adopted children between age 3 to 16 living in the household.

The denominator censoring weight model (see Table S3 in the Supplemental Material) indicates the effects of CMs' educational attainment, cognitive ability, having a partner, and having more than three siblings on the likelihood of being censored (living without children) are statistically significant at conventional criteria.

Parental education and children's cognitive development

Table 3 shows the estimated differences in first-born children's verbal and numerical ability by parental education. The second column, "Unadjusted", shows the effect of parents having a degree on children's verbal and numerical ability had confounding and endogenous selection bias not been addressed. The third column, "IPTW1", indicates the effect when weighing the analyses with inverse probability of treatment weights using parental cognitive ability to create weights alone. The fourth column, "IPTW2", indicates the effect when using inverse probability of treatment weights based on all measured confounders. The fifth column "IPCW" displays the effect when addressing endogenous selection bias by weighing the analysis with inverse probability of censoring weights. Finally, the last column, "IPTW2*IPCW, shows the effect when applying the product of treatment weight based on all confounders and the censoring weight.

Table 3. Estimated differences in children's verbal and numerical ability by parental education.

Verbal ability	Unadjusted	IPTW1	IPTW2	IPCW	IPTW2*IPCW
Parental degree	5.48***	2.09	0.32	7.03***	1.81
(Ref. no degree)	(1.43)	(1.78)	(2.21)	(1.48)	(1.82)
N	1,042	1,042	1,042	1,042	1,042
Numerical ability	Unadjusted	IPTW1	IPTW2	IPCW	IPTW2*IPCW
Parental degree	4.35***	2.51	1.97	4.95	1.43
(Ref. no degree)	(1.19)	(1.42)	(1.73)	(1.37)	(1.31)
N	1,031	1,031	1,031	1,031	1,031

Source: BCS70; Note: Test scores are age-normalised; Analysis restricted to first-born children with valid information on cognitive ability who were randomly selected from the uncensored sample (CMs living with their natural or adopted children at age 34); IPTW1 = Inverse probability of treatment weights based on parental cognitive ability; IPTW2 = Inverse probability of treatment weights based on all covariates; IPCW = inverse probability of censoring weights; standard errors in parentheses; * $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$.

On the age-normalized verbal ability scale, children whose parent has a degree score 5.48 points higher than children whose parent does not have a degree ($SE = 1.43$, $p < .001$). This represents a difference equal to more than one-fourth of the standard deviation of verbal ability ($SD = 19.61$). The effect is substantially attenuated and statistically non-significant when cognitive ability is accounted for ($\beta = 2.09$, $SE = 1.78$, $p > .05$). It almost completely disappears when all confounders are accounted for ($\beta = 0.32$, $SE = 2.21$, $p > .05$). Correcting for endogenous selection bias alone results in a significantly larger estimated effect of parental degree on children's verbal ability ($\beta = 7.03$, $SE = 1.43$, $p < .001$). Without correcting for this bias, we would have underestimated the effect of parental education on children's verbal ability. Using the product of the treatment and censoring weights, the verbal ability scale score of children whose parents have a college degree is, on average, 1.81 points higher than the score of their peers without a highly educated parent. This effect is statistically non-significant at conventional criteria ($SE = 1.82$, $p > .05$).

Regarding numerical ability, children with degree-holding parents score 4.35 points higher than those without ($SE = 1.19$, $p < .001$). This is equivalent to a difference of more than a quarter of a standard deviation of numerical ability ($SD = 16.23$). The effect is substantially attenuated and statistically non-significant when cognitive ability is taken into account ($\beta = 2.51$, $SE = 1.42$, $p > .05$) and is further reduced when all confounders are taken into account ($\beta = 1.97$, $SE = 1.73$, $p > .05$). The estimated effect of parental degree on children's verbal ability is slightly larger after adjusting for endogenous selection bias ($\beta = 4.95$, $SE = 1.37$, $p < .001$). The average verbal ability scale score of children whose parents have a degree is 1.43 points higher than that of peers without a highly educated parent, using the product of the treatment and censoring weights. This effect is statistically non-significant at conventional criteria ($SE = 1.31$, $p > .05$).

Discussion

On various cognitive outcomes, children from higher socioeconomic status families outperform their peers with lower SES. The literature identified parental education as one of the most influential socioeconomic factors in children's developmental outcomes. Through education, parents may acquire cognitive flexibility or problem-solving skills deemed advantageous to their children's cognitive development. Highly educated parents use their time more efficiently to engage their children in cognitively stimulating activities. Nonetheless, a central question in the literature is whether parents' educational attainment is causally related to differences in children's outcomes. For instance, genetic confounding may partly explain the association between parental education and children's cognitive development. Analyses of socioeconomic status and child development also disregard family formation mechanisms, which may introduce endogenous selection bias. The paper addresses these issues by taking advantage of the BCS70's multigenerational design, using inverse probability of treatment and censoring weighting to correct for confounding bias and non-random selection into parenthood. This design permits a prospective approach to parental influences on child development, including their early cognitive ability as a genetic proxy. It further incorporates the modelling of the transition into parenthood into the child outcome analysis.

Previous studies (Crawford et al., 2011; Marks & O'Connell, 2021; Sullivan et al., 2021) suggested that parental cognitive ability accounts for around half of the association between family socioeconomic status and child cognitive ability. Our findings are consistent with this literature showing that parental cognitive ability explains 62% of the association between parental education and children's verbal ability and 42% in the case of numerical ability. In addition, other early parental characteristics, such as their parents' educational aspirations (i.e., children's grandparents) contribute to the confounding of the association between parents' education and child cognitive ability. In contrast, if we had not adjusted for selective parenthood, we would have underestimated the impact of parental education on children's numerical and especially verbal ability. This is due, in part, to the fact that cohort members with higher levels of education and lower levels of early cognitive ability were less likely to become parents.

After adjusting for both confounding and endogenous selection bias, the average causal effect of parental education on children's verbal and numerical ability is not statistically significant. Nevertheless, the effect sizes are far from trivial. For example, a 1.81-point increase in verbal ability for children whose parents have a degree corresponds to an effect of 9% of a standard deviation. Similarly, the numerical ability gap between children whose parents have a degree and those without is 9% of a standard deviation. This suggests that parental education plays a role in the cognitive development of children, but not to the extent previously believed. Moreover, our findings align with research showing that increases in mother's education after childbirth did not result in significant improvements of children's cognitive outcomes using a mother or sibling fixed effects design (Augustine & Negraia, 2018; Breinholt & Holm, 2020).

The association between parents' education and children's cognitive ability appears largely due to genetic confounding, either through direct transmission or genetic nurture (e.g., Wertz et al., 2020). The mechanisms associated with any positive effect of parental education on children's development, such as parental time spent in educational activities, may be attributable to differences in parents' endowment of cognitive ability. The findings suggest that equalizing education in the parent generation will have a rather little effect on reducing inequality in the succeeding generation (Conley et al., 2015).

Our findings have implications for researchers examining associations between family socioeconomic status and child outcomes. To account for genetic confounding, researchers need to either use data containing genetic information or rely on the familial control method that accounts for the respective outcome measure in the parent generation (S. Hart et al., 2021). For the latter approach, it appears essential for child cohort studies to assess the cognitive ability of parents in addition to that of their children and to collect more information on parents' early life courses and grandparent characteristics. Multigenerational cohort studies, such as the one used in this study, are advantageous and additionally address the issue of endogenous selection bias.

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Supplementary Material: Parental education and children's cognitive development: A prospective approach

Table S1. Summary Statistics.

	Mean/Proportion	*SD*	*Min*	*Max*
*Child outcome**				
Verbal ability	4.70	19.61	-74.65	74.74
Numerical ability	1.43	16.23	-60.90	66.99
Exposure				
CM education: degree or higher	0.31		0.00	1.00
Confounder: CM characteristics				
CM birthweight in grams	3315.05	524.86	680.00	5448.00
CM cognitive ability	0.10	0.96	-3.33	3.37
CM number of siblings				
None	0.10		0.00	1.00
One	0.48		0.00	1.00
Two	0.29		0.00	1.00
Three	0.09		0.00	1.00
More than three	0.04		0.00	1.00
CM locus of control	7.37	2.90	0.00	15.00
CM problem behavior				
Normal (below the 80th percentile)	0.83		0.00	1.00
Moderate (between 80th and 95th percentile)	0.14		0.00	1.00
Severe (above 95th percentile)	0.04		0.00	1.00
CM partner's education				
No partner	0.27		0.00	1.00
Partner left education at age 16 or younger	0.44		0.00	1.00
Partner left education at age 17/18	0.18		0.00	1.00
Partner left education at age 19-22	0.05		0.00	1.00
Partner left education at age 23+	0.07		0.00	1.00
Confounder: CM parent characteristics				
CM parent education: degree or higher	0.17		0.00	1.00
CM parent income (weekly) in £				
Less than £35 per week	0.02		0.00	1.00
£35 to 49£ per week	0.04		0.00	1.00
£50 to £99 per week	0.28		0.00	1.00
£100 to £149 per week	0.37		0.00	1.00
£150 to £199 per week	0.17		0.00	1.00
200 to £249 per week	0.07		0.00	1.00
More than £250 per week	0.05		0.00	1.00
CM parent aspirations: pursue higher education				
Yes	0.17		0.00	1.00
No	0.83		0.00	1.00

Source: British Cohort Study (BCS70); *Note*: Summary statistics based on full CM sample (N = 4,819). * Verbal ability measure based on child sample (N = 1,042); numerical ability measure based on child sample (N = 1,031).

Table S2. Summary of models estimating denominator of treatment weight (logistic regressions).

	Denominator treatment weight parental cognitive ability	*Denominator treatment weight all confounders*
CM cognitive ability	0.87 (0.04) ***	0.54 (0.05) ***
CM birthweight in grams		-0.00 (0.00)
CM number of siblings (ref.: None)		
One		-0.11 (0.12)
Two		-0.08 (0.13)
Three		-0.18 (0.17)
More than three		-0.00 (0.22)
CM locus of control		0.05 (0.01)
CM problem behavior (ref.: Normal)		
Moderate		-0.30 (0.27)
Severe		-0.37 (0.48)
CM partner's education (ref.: No partner)		
Partner left education at age 16 or younger		-0.63 (0.09) ***
Partner left education age 17/18		-0.21 (0.10) *
Partner left education age 19-22		0.92 (0.17) ***
Partner left education age 23+		1.15 (0.15) ***
CM parent education: degree or higher		0.78 (0.10) ***
CM parent income (ref.: Less than £35 per week)		
£35 to 49£ per week		0.17 (0.38)
£50 to £99 per week		0.34 (0.33)
£100 to £149 per week		0.49 (0.33)
£150 to £199 per week		0.66 (0.34)
200 to £249 per week		0.70 (0.35)
More than £250 per week		0.54 (0.36)
CM parent aspirations: pursue higher education (ref.: No)		
Yes		0.71 (0.21) **
Constant	-1.01 (0.04) ***	-1.69 (0.42)
N	4,819	4,819

Source: British Cohort Study (BCS70). *Note.* * $p < .05$, ** $p < .01$, *** $p < .001$. Robust standard errors in parentheses. Statistics pertain to full CM sample, including CMs without natural or adopted children living in the household. CM = Cohort member.

Table S3. Summary of model estimating denominator of censoring weight (logistic regression).

	Denominator censoring weight
CM degree or higher (ref.: No)	
Yes	0.41 (0.08) ***
CM cognitive ability	0.14 (0.04) ***
CM birthweight in grams	-0.00 (0.00)
CM number of siblings (ref.: None)	
One	0.08 (0.11)
Two	-0.09 (0.12)
Three	0.06 (0.15)
More than three	-0.61 (0.19) **
CM locus of control	0.02 (0.01)
CM problem behavior (ref.: Normal)	
Moderate	-0.05 (0.09)
Severe	-0.17 (0.17)
CM partner's education (ref.: No partner)	
Partner left education at age 16 or younger	-1.81 (0.08)
Partner left education age 17/18	-1.51 (0.10) ***
Partner left education age 19-22	-1.02 (0.16) ***
Partner left education age 23+	-0.86 (0.14) ***
CM parent education: degree or higher	0.16 (0.10)
CM parent income (ref.: Less than £35 per week)	
£35 to 49£ per week	-0.00 (0.29)
£50 to £99 per week	0.00 (0.25)
£100 to £149 per week	0.08 (0.25)
£150 to £199 per week	-0.04 (0.26)
200 to £249 per week	-0.03 (0.28)
More than £250 per week	0.09 (0.29)
CM parent aspirations: pursue higher education (ref.: No)	
Yes	-0.06 (0.09)
Constant	0.98 (0.34) ***
N	4,819

Source: British Cohort Study (BCS70). *Note.* * $p < .05$, ** $p < .01$, *** $p < .001$. Robust standard errors in parentheses. Estimates presented as logit coefficients. Censoring = no natural or adopted children between age 3 and 16 living in CM's household. CM = Cohort member.

Table S4. Descriptive statistics for inverse probability weights.

			Percentiles			
	M	*sd*	*1st*	*25th*	*75th*	*99th*
Treatment weight (TW)*	1.01	0.69	0.35	0.77	0.99	4.02
Censoring weight (CW)	0.99	0.60	0.60	0.72	1.00	2.97
TW * CW	1.01	0.91	0.44	0.56	1.08	4.69

Source: British Cohort Study (BCS70). *Note.* Statistics pertain to uncensored sample. * Treatment weight based on denominator model with all confounders (see Table S2).