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ABSTRACT

The relationship between completed education and adult cognition is investigated using data from the Indonesia Family Life Survey. We compare adult siblings to account for shared, difficult-to-measure characteristics that likely affect this relationship, including genetics and parental preferences and investments. After establishing the importance of shared family background factors, we document substantively large, significant impacts of education on cognition in models with sibling fixed effects. In contrast, the strong positive correlation between education and adult height is reduced to zero in models with sibling fixed effects, suggesting little contamination in the education-height association beyond factors common to siblings.

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1. Introduction

Understanding the factors that help determine cognitive function in adulthood is important because higher levels of adult cognition have been linked to a broad array of demographic, socioeconomic, and health outcomes, including lower incidence of dementia (Cobb et al. 1995; Qiu et al. 2001), lower mortality risks (Dewey and Saz 2001; Hayat et al. 2018; Yaffe et al. 2016), improved health behaviors (Taylor 2013), higher levels of life satisfaction and quality of life (Enkvist, Ekström, and Elmståhl 2013; Llewellyn et al. 2008) and better financial decision-making as well as labor market and economic outcomes (Burks et al. 2009; Heckman, Stixrud, and Urzua 2006; Hsu and Willis 2013; Lin, Lutter, and Ruhm 2018; Lindqvist and Vestman 2011; MacArdle, Smith, and Willis 2009). Educational attainment is associated with better cognitive functioning in adulthood in a number of studies (Angrisani, Lee, and Meijer 2020; Clouston et al. 2012; Crimmins et al. 2018; Hayward et al. 2021; Ngandu et al. 2007; Weir, Lay, and Langa 2014), but interpretation of that association is complicated as it is likely to be driven, at least in part, by selection on unobserved factors that affect both educational attainment and the evolution of cognitive function over the life course. Family background and resources in childhood are obvious candidates for such factors, particularly in low-income settings (Strauss and Thomas, 1995).

A very large literature documents the links between human capital and family investments, broadly defined. As Marshall (1890) put it, “General ability depends largely on the surroundings of childhood and youth. In this, the first and far more powerful influence is that of the mother.” See also Leibowitz (1974), who discusses the role of family in explaining variation in young children's verbal and quantitative skills. Early childhood enrichment programs have documented the key role of parenting in the development of child human capital. García and Heckman (2023) provide an insightful synthesis and discussion, underscoring the central role of parenting, which is also highlighted by Dougan, García, and Polovnikov (2023). Behrman, Pollak, and Taubman (1982) describe the role of parents' preferences in the allocation of resources to children and describe the trade-off between efficiency (allocating more resources to the more able child) and equity (allocating more resources to the less able child).

We begin by documenting an association between education and cognition in adulthood in a low-resource setting and proceed to investigate the extent to which that association can be attributed to the role of family background and resources. We include a rich array of observed measures of parental and background characteristics and then draw contrasts between siblings to take into account unobserved shared background factors. We thereby move toward identifying a causal effect of education on cognition in adulthood.

A better understanding of the relationship between education and cognition is important because cognitive skills are likely to be of increasing importance as technological innovation reshapes the nature of work and the workplace across the globe. Further, with populations aging at unprecedented rates, it is imperative to improve understanding of the links between investments in education and the likely trajectory of the future burden of cognitive impairment at older ages (Ehrlich and Yin 2013). Education is a powerful predictor of reduced risks and delayed onset of cognitive decline and dementia at older ages (Langa et al, 2008). For example, there has been a substantial decline in dementia prevalence in many higher-income countries in the last two decades (Roehr et al, 2018), and in the United States, a large fraction of that decline has been attributed to the rise in education levels of surviving cohorts (Crimmins et al., 2018; Hudomiet, Hurd, Rohwedder, 2022). Since educational attainment is largely determined in early adult life, advancing understanding of the relationship between education and cognition in early- and mid-adulthood will contribute to a better understanding of the mechanisms underlying the link between education and cognitive decline in later life (Crimmins et al. 2018; Hudomiet, Hurd, and Rohwedder 2022; Langa et al. 2008; Lövdén et al. 2020; Tucker-Drob 2019). The need for scientific evidence on these issues is particularly pressing in low-income settings because evidence from those settings is scarce.

This paper makes six contributions to the literature. First, using longitudinal survey data from Indonesia, a low-resource country entering an era of rapid population aging, we document the extent to which adult performance on cognitive assessments is related to completed levels of education. Few studies have addressed this question in contexts other than advanced economies, leaving an important gap in knowledge. Second, we extend the literature by examining two complementary indicators of measured cognition: scores from quantitative and abstract reasoning assessments, which are indicative of the ability to solve novel problems and are interpreted as indicators of fluid intelligence (Cattell 1963). Third, exploiting the fact that the Indonesia Family Life Survey (IFLS) has followed all panel respondents for over two decades with low rates of attrition, we have constructed a sample of adult siblings who live apart, which mimics the population of all adults. By drawing contrasts between these adult siblings, our estimates of the relationship between education and cognitive outcomes are purged of bias driven by unobserved, family background characteristics that are shared by siblings. Fourth, these estimates indicate that the effect of education on cognition is significantly greater at lower levels of schooling. This finding is both substantively important and has implications for the interpretation of existing estimates of local average treatment effects. Fifth, we document that measurement error in education cannot explain our results. Sixth, we assess the extent to which other omitted factors likely contaminate our conclusions by examining links between education and another measure of human capital, adult height, which is determined within the first few years of life and, therefore, cannot be causally affected by education. Education predicts height, even after controlling for a rich set of observed family background characteristics, indicating that both measures of human capital are affected by shared unobserved factors. However, in models that include sibling fixed effects, the effect of education on height is small in magnitude and not statistically significant suggesting little contamination in that relationship beyond factors that are common to siblings. To the extent that these results carry over to the relationship between education and cognition, the results mitigate concerns that our sibling fixed effects estimates are contaminated by unobserved heterogeneity.

2. Interpreting the literature on the education–cognition relationship

Research spanning multiple disciplines has proposed possible mechanisms through which education improves cognition. First, education may confer direct neurological benefits. For example, studies have revealed links between years of schooling and brain weight (Brayne et al. 2010) and brain structure (Coffey et al. 1999). Education also increases mental stimulation while it is accrued and offers potential benefits for cognitive function throughout life, as individuals with more education are more likely to continue to engage in cognitive-intellectual activities, such as having more stimulating occupations and more contacts with better-educated individuals (e.g., a better-educated spouse) (Cagney and Lauderdale 2002). Further, education is thought to be a strong predictor of and direct proxy for cognitive reserve, which is the brain's capacity to optimize performance and cope with pathological insults that undermine brain functioning (Stern 2006, 2009). This "reserve effect" of a higher level of education could mitigate cognitive decline and reduce the risk of Alzheimer's disease and related dementias (Clouston et al., 2020). Beyond the direct neurological benefits, higher levels of education predict advantageous social, economic and health behaviors and outcomes that in turn are associated with better cognitive performance (Langa 2018). Moreover, any of the pathways through which education improves general health are likely to promote cognitive health as well.

Identifying the causal effect of education on cognitive performance is empirically challenging and has been attempted in only a small number of studies. One widely-used approach in the literature on education and labor market outcomes exploits changes in access to schooling (Card 1999). For example, changes in compulsory schooling laws that raised the age at which a child could legally leave school have been used to estimate the effect of an additional year of school on those who would have otherwise left school the year before. Banks and Mazzonna (2012) examine the effect on memory of a child's staying in school to age 15 in the context of the rise of the school-leaving age in England from 14 to 15. (For examples of studies of Europe and the United States, see also Schneeweis et al., 2014; Glymour et al., 2008; Nguyen et al., 2016; and Dahmann, 2017. Ma et al., 2021, investigate the effect of children's education on parental cognitive function in Mexico, and Ma, 2019, provides evidence from China.) Other studies have used expansions in the supply of schooling to identify the effects of education, again paralleling research on the causal effect of education on labor market outcomes. For example, following Duflo (2001) and using the same data that we use, Sanginabadi (2020) leverages a large-scale primary school construction program targeted at underserved communities in Indonesia, Sekolah Dasar INPRES, to identify the effect of education on abstract reasoning.

These approaches can identify only the local average treatment effect of additional schooling at the level affected by the change in law or for the population affected by the change in access to schooling. If the link between education and cognition is not linear across the distribution of education, as we and others have documented, estimates of the effects of education in the linear model will be biased outside the support of education levels that are affected by the change (Nguyen et al. 2016). Moreover, interpretation of these estimates is complicated if the marginal child affected by the change is different from inframarginal children.

For example, children who were required to stay an extra year in school by law are potentially less inclined to learn as much as their peers not affected by the change in the law.

An alternative approach to advancing understanding of the relationship between education and cognition leverages the idea that educational attainment and cognitive development during childhood are rooted in family background and resource availability. These factors encompass genetic factors, parental characteristics including parenting style, social and economic resources, and tastes for investment in human capital as well as the broader family, neighborhood, and community environments over the life course. A large number of influential studies have included family background measures in multivariable models to explain an array of human capital outcomes net of the role of family background (Anger and Schnitzlein 2017; Black, Devereux, and Salvanes 2005; Ermisch and Francesconi 2001; Johnson et al. 1983; Roberts et al. 1999; Shakeshaft et al. 2013; Strauss and Thomas 1995; Thomas and Strauss 1992; Thomas, Strauss, and Henriques 1990; Tucker-Drob and Briley 2014; Tucker-Drob and Harden 2012; Zavala et al. 2018).

As a starting place, we begin by documenting the extent to which the adult education–cognition link is driven by observed family background. In so doing, we pay particular attention to heterogeneity in the relationship that depends on observed individual and family background characteristics.

A general concern with this approach is that it is a challenge to account for all relevant family background characteristics and many are difficult to measure in a survey context. We therefore leverage the fact that IFLS has successfully tracked respondents for more than two decades, which allows us to draw contrasts between adult siblings. These models of the cognition–education link that include sibling fixed effects are designed to sweep out the influence of all background characteristics that siblings share. These include, for example, genetic endowments and observed and unobserved family and community characteristics. We define siblings as individuals sharing a mother who is explicitly identified in an IFLS household roster. In these models, sibling and maternal fixed effects are equivalent. In Indonesia, the vast majority of children with the same mother also have the same father; in our sample, 1.5% of the respondents who share a mother report having a different father. Effectively, the sibling fixed effects estimates measure the extent to which differences in the education of siblings predict differences in their cognition in adulthood.

This approach has been used in studies of the impacts of education on multiple health outcomes, including self-rated health (Fujiwara and Kawachi 2009), hospitalization (Behrman et al. 2011), cardiovascular health (Ariansen et al. 2017), and mortality (Behrman et al. 2011; Mortensen and Torssander 2017; Næss et al. 2012; Tarkiainen et al. 2015; Torssander 2013). Two recent studies have extended this approach to cognition. Using UK Biobank data, Fletcher and colleagues identify siblings based on genetic relatedness. Their model with sibling fixed effects establishes that education is a powerful predictor of cognition, after sweeping out shared genetic factors. They conclude that 40% of the education–cognition association is attributable to shared genetic factors and only a tiny proportion to sibling-specific genetic differences (Fletcher et al. 2021). Herd and Sicinski (2022) compare sibling pairs in the Wisconsin Longitudinal Study and report that education remains strongly associated with cognitive function whereas a genetic predictor of education is not associated with cognition after sibling fixed effects are included in the model.

3. Conceptual framework and empirical strategy

Without loss of generality, assume that each individual lives for two periods, childhood (period 1) and adulthood (period 2), and that educational attainment (measured as highest grade completed converted to the equivalent number of years of schooling) is determined by the end of period 1. Cognition varies over the life course, with cognition in the second period depending on completed education and on the level of cognition at the end of the first period. Our interest centers on the relationship between cognition in adulthood (in period 2) and educational attainment (determined in period 1).

Education and cognition are the outcomes of human capital production functions that combine inputs, conditional on individual, family and community characteristics, to produce each outcome. Inputs are chosen by parents, and possibly other family members, to solve a family intertemporal optimization program by allocating resources in each period subject to a period-specific budget constraint. These include choices about time and financial investments in the human capital of each child such as education, cognitive development and health, as well as choices about fertility over the life course.

The educational attainment of individual *i* at the end of period 1, E_{1i}, depends on observed individual-specific characteristics, X_{1i} (such as birth cohort, gender, measured ability) and family-level characteristics, Z_f, (such as parental education and human capital, parental time allocation, family resources, shared genetic endowment and community resources). For ease of exposition, both sets of characteristics are treated as if they are fixed during childhood. (The key insights from a dynamic model of resources and investments during childhood do not diverge from those from this model.) The education outcome function also depends on unobserved factors, ε_{1i}, including unmeasured aspects of ability such as ambition, grit and determination:

$$E_{1i} = f_1(X_{1i}, Z_f, \varepsilon_{1i}) \quad [1]$$

The cognition of individual *i* at the end of period 1, C_{1i}, depends on the same individual- and family-level characteristics, along with unobserved characteristics ζ_{1i} that affect cognitive development, which may differ from the unobserved characteristics that determine educational attainment, although there is likely to be overlap in these characteristics:

$$C_{1i} = g_1(X_{1i}, Z_f, \zeta_{1i}) \qquad [2]$$

The functions f_1 and g_1 are allowed to differ, reflecting the fact that the technology of production of each domain of human capital is not the same. Educational attainment is assumed to be determined by the end of period 1, but cognitive function evolves over the life course. Cognitive function during adulthood, the second period, C_{2i} , for individual *i* is assumed to depend on cognition and education at the end of period 1, C_{1i} and E_{1i}, respectively, along with period-2 individual characteristics, X_{2i}, family characteristics which are fixed in childhood, Z_f, and unobserved characteristics , ζ_{2i} :

$$C_{2i} = g_2(C_{1i}, E_{1i}, X_{2i}, Z_f, \zeta_{2i}) \qquad [3]$$

Substituting [2] into [3] yields the dynamic cognitive output function conditional on educational attainment:

$$C_{2i} = h(E_{1i}, X_{1i}, X_{2i}, Z_f, \zeta_{1i}, \zeta_{1i}) \qquad [4]$$

Combining X_{1i} and X_{2it} into X_{it} and u_{1i} and u_{2it} into ζ_i, [4] becomes

$$C_{2i} = h(E_{1i}, X_i, Z_f, \zeta_i) \qquad [5]$$

Since completed education, E_{1i}, and unobserved characteristics that affect cognition in period 1 and/or period 2, ζ_i , are likely to be correlated, it is difficult to interpret estimates of [5] beyond reflecting an association.

Substituting the function f_1 in [1] for completed education E_{1i} in [5] yields reduced-form models of cognitive performance:

$$C_{2i} = h_1(X_i, Z_f, \upsilon_i) \qquad [6]$$

which, in combination with [1], provide direct evidence on the links between human capital outcomes in adulthood and background characteristics. Those estimates are informative about the importance of background characteristics but not about the structural model [5] that links education with cognition in adulthood.

As a step toward addressing this limitation, we compare cognitive outcomes of siblings by including sibling fixed effects, μ_f, which capture all family background characteristics shared by siblings, including, for example, parental education and human capital preferences regarding investments in children, all shared genetic endowments and resource availability during childhood:

$$C_{2i} = h(E_i, X_i, \mu_f, \xi_i) \qquad [7]$$

Parameterizing [6] and [7], we measure cognition in adulthood for each respondent, *i*, at the same time, C_{2i}, and completed years of education, E_i, which is entered as a linear spline function with knots at *k* years of education to allow differences in marginal returns across the distribution of education. Model [6] also includes a vector of individual-specific controls, X_i, and family background controls, X_f, with v_{if} capturing unobserved heterogeneity:

$$C_{2i} = E_{ik}\delta_k + X_i\theta_i + X_f\gamma_f + v_{if} \qquad [8]$$

To address the concern that v_{if} may be correlated with education, we include sibling fixed effects, μ_f, which absorb X_f in [8] and are assumed to affect cognition in a linear and additive way:

$$C_{2i} = E_{ik}\beta_k + X_i\alpha_i + \mu_f + u_{if} \qquad [9]$$

Estimating [9] amounts to comparing the cognitive function of siblings and assessing the extent to which those differences vary with differences in sibling education, taking into account all unobserved characteristics that are common to the siblings and that affect cognition in a linear and additive way. To the extent that, conditional on individual-specific observed characteristics, X_i, education differences between siblings are not correlated with unobserved differences between them, then the estimated effects of differences in education in [9] can be given a causal interpretation. This rules out, for example, differences in the determinants of cognition in the first period that are individual-specific, unobserved and correlated with the differences in completed education of the siblings. If this assumption is violated and, for example, the better-educated sibling is advantaged in terms of unmeasured characteristics, ζ_{1i}, then the fixed effects estimates will be upward biased.

A legitimate concern is that the siblings may have had different experiences as children because of variation in the availability of resources in the family over time that affects children in different ways. Their experiences may also differ because of choices of parents who may, for example, allocate more resources to one sibling over another because of intertemporal variation in resources or because of perceived relative costs and benefits of those allocations. To address this concern, the models with sibling fixed effects also include birth order, gender, individual-specific markers of childhood health, childhood experiences of hunger and place of birth.

To empirically evaluate the importance of family background characteristics not observed in these data, we compare the fixed effects estimates, [9], with estimates that parallel those in the literature and include a wide set of family background controls rather than sibling fixed effects, [8]. We also contrast these results with those from baseline models that adjust only for respondent education, age and gender to assess the relevance of the family background controls.

A priori, it is not clear whether the effects of education and family background differ by gender. All estimates are reported separately for males and females based on models that include interactions between every observed covariate and an indicator for gender as well as the gender main effect. The models with sibling fixed effects include brothers and sisters who share a common fixed effect, reflecting their common family background. (Relaxing this assumption and stratifying the models with sibling fixed effects by gender does not affect any of our conclusions.)

4. Data

4.1 Data and sample

IFLS, a longitudinal survey of individuals, households, families and communities that began in 1993, provides the foundation for this research. IFLS5, fielded in 2014/15 (Strauss, Witoelar, and Sikoki 2016), is ideally suited for contrasting the cognitive function of adult siblings for three reasons. First, since the first follow-up of the 1993 baseline, IFLS has made tracking baseline respondents a priority. These include both individuals who have split off from the baseline household and remained in the baseline community and those who have moved away to other parts of Indonesia. Thus, by design, our analytical sample includes siblings who were growing up together in the household interviewed in the 1993 baseline and who have been assessed as adults, 21–22 years after the baseline, no matter where they were living at the time of the follow-up. Second, IFLS5 introduced an innovative battery of cognitive assessments that are designed for adults of all ages and do not depend on literacy. Third, adults of all ages were eligible to be assessed, in contrast to other surveys of aging that restrict attention to specific cohorts. This is key for our research since we focus on adults aged 25 to 49 at the time of IFLS5.

We restrict our attention to this age group to ensure that our sample of siblings is not selected on human capital. On one hand, almost all Indonesians have completed their education by age 25, our lower age bound. On the other hand, as shown below, in terms of key indicators of education and cognitive performance, our sample of siblings is neither substantively nor statistically significantly different from all respondents who have co-resided with their mothers in at least one wave of IFLS. Specifically, of 17,815 respondents aged 25 to 49 who completed the cognitive assessments, 9,933 co-resided with their mothers in at least one wave of IFLS. Of those respondents, 65% have at least one sibling assessed in IFLS5; these 6,505 respondents are included in the sibling comparison models with fixed effects.

In each follow-up, IFLS has achieved low rates of attrition (Strauss et al. 2016): in IFLS5, 76% of panel respondents aged 25–49 at the time of the survey were assessed. While selective attrition is a potential concern in any study using longitudinal data, to the extent that the reasons for attrition are shared by siblings, our models that include sibling fixed effects will not be biased by loss to follow-up (Thomas et al. 2012).

4.2 Measures of cognitive function

Cognitive function is measured in two domains, quantitative skills and abstract reasoning. Both are interpreted as indicators of fluid intelligence, which has been shown to be closely related to educational attainment, occupational choice and economic success (Davies et al. 2011; Green et al. 2017; Jaeggi et al. 2008).

Quantitative reasoning is measured with an adaptive instrument that involves completing three blocks of three number series questions, with the difficulty of subsequent blocks of questions depending on answers in the prior block. Scoring takes into account the difficulty of the questions in each block. The instrument was specifically designed for populations with low levels of education, validated in Indonesia as part of IFLS, and shown to discriminate well by age and education level (Prindle and McArdle 2013; Strauss et al. 2018).

Abstract reasoning is measured using a subset of eight items from Raven's Progressive Matrices battery, a nonverbal assessment that does not require literacy. Each item involves a block of three ordered shapes; the respondent picks the fourth shape, from four possible options, to complete the block.

4.3 Educational attainment

Education is reported as the highest educational level attended and the highest grade completed at that educational level. We construct the equivalent years of completed education assuming that no grades were repeated or skipped. In our models, education is treated as a continuous variable, with a knot at nine years of education, which corresponds to completion of *sekolah menengah pertama* (junior secondary schooling).[1] We construct maternal and paternal education using the same approach. When a parent has not been interviewed in IFLS, we use respondent reports of parental education.

4.4 Observed child-specific characteristics

A large literature has demonstrated the importance of childhood health for human capital outcomes in adulthood (Case, Fertig, and Paxson 2005; Smith 2009; Strauss and Thomas 2007). To account for early life experiences, we include three specific indicators of childhood health measured in IFLS5: self-reported childhood health status (on a five-point scale from poor to excellent), height of the respondent, and whether the respondent was hungry as a child. In addition, province of birth and province of residence at age 12 are included to measure heterogeneity in access to resources including health and education services.

Height is measured with a Shorr board stadiometer. Adult height is an important indicator of early childhood nutrition and development (Waterlow et al. 1977) and is predictive of many human capital outcomes, including education, labor market outcomes, and cognitive function

[1] There are six years of *sekolah dasar* (primary school), followed by three years of *sekolah menengah pertama* and then three years of *sekolah menegah atas* (high school) or *sekolah menengah kejuruan* (vocation high school), which may be followed by tertiary education.

(Case and Paxson 2008a, 2008b; LaFave and Thomas 2017; Maurer 2010; Persico, Postlewaite, and Silverman 2004).

Experiencing hunger in childhood has negative consequences for child development, including brain development (Grantham-McGregor et al. 2007; Levitsky and Strupp 1995; Strupp and Levitsky 1995; Weinreb et al. 2002). We include three dichotomized variables to indicate whether respondents experienced hunger between 0 and 5 years old, 6 and 10 years old, and 11 and 15 years old, as these variables reflect family poverty and nutritional deprivation at different ages during childhood.

The Indonesian archipelago is characterized by enormous heterogeneity in living standards and access to infrastructure, including education and health services, across both space and time. To take this into account, the models include place of birth and residential location at age 12, specified as indicator variables for each province in the country and whether the location, at that time, was a village, town or city.

4.5 Observed shared background characteristics

A key advantage of IFLS is that every household member is interviewed. Over 95% of the siblings in the sample used in this study were interviewed while co-residing with their parents, and the models include detailed measures of parental characteristics based on answers provided by the parents themselves. For the other 5% of siblings, we use the respondent's report of parental characteristics. These include maternal and paternal educational attainment (converted to years of completed education), height, age at the respondent's birth and number of children ever born to the mother.

To measure resource availability during childhood, we exploit the fact that IFLS collects a consumption and expenditure module and calculate household per capita expenditure (PCE) at baseline. PCE is thought to be a good indicator of resources in low-income settings, where incomes are variable and credit markets are incomplete. We construct an indicator for PCE levels in the bottom quartile of the distribution within province and urban–rural sector (to take into account variation in prices across space).

5. Results

Respondent characteristics are reported in Table 1 for all IFLS5 respondents aged 25–49 years at the time of the survey (column 1), those whose mothers have been interviewed in any IFLS wave (column 2) and those who have at least one sibling in the sample (column 3). Differences in means are reported in the final two columns for respondents with a mother relative to all IFLS5 respondents (column 4) and those with siblings relative to those with a mother (column 5).

About half the respondents have a mother who has been interviewed in IFLS. Relative to all age-eligible respondents, those with a mother who was interviewed are, on average, a little over a year younger, better educated and they perform better on both the quantitative and abstract reasoning assessments. Figure 1 displays human capital outcomes by birth year, conditional on age, for the full sample (indicated by the dotted line and labelled Full sample), for those with a mother in the same (indicated by the dashed line and labelled Mother sample) and those with a sibling in the sample (indicated by the solid line and labelled Sibling sample). The differences between the full sample and those with a mother in the sample are small. In fact, adjusting for gender, age and education, the gaps in performance on the cognitive assessments are not statistically significant.

Among those respondents whose mother has been assessed, approximately two-thirds have at least one sibling in the sample. As shown in Table 1 and Figure 1, the differences between those with a mother but no sibling and those with at least one sibling are also small in magnitude, and none is statistically significant.

A concern in many studies of siblings is that there is little variation between siblings and estimates lack power. This is not a concern in this study. Standard deviations are reported below the means in the first three columns of Table 1. They establish that there is substantial variation in human capital outcomes in the sample of siblings. (The standard deviations of education and the cognitive scores are also very similar across all three samples.)

Characteristics of the study sample of 6,505 siblings from 2,448 families are reported in Table 2. The average respondent is 35 years old and has completed almost 10 years of education, corresponding to one year beyond completion of junior high school. Whereas the differences between males and females in age and years of completed education are small and statistically insignificant, cognitive performance differs substantially by gender. To facilitate comparisons across the cognitive assessments, we have converted the quantitative and abstract reasoning scores of siblings to z scores. Relative to females, males score significantly higher on both assessments: the difference is between 0.14 and 0.16 standard deviations. (For readability, the z scores are multiplied by 100 in the table.)

5.1 Reduced form models of adult human capital outcomes

We begin with the reduced form models for completed education, [1], and cognitive assessments, [5], to determine the extent to which parental human capital and family background predict the human capital outcomes of adult children. The family background controls, Z_f, include maternal and paternal education, age and height as well as household PCE at baseline; the models take into account individual characteristics, X_{it}, including respondent's gender, age, birthplace, place of residence at age 12, attained adult height, health status as a child and whether the respondent experienced hunger during three critical childhood age periods. The regression results, reported in Table 3, display the effects of maternal and paternal education on the respondent's completed education and performance on the two cognitive assessments; results are reported separately for males and females.

The family background characteristics are statistically significant predictors of the human capital outcomes, as are the individual characteristics. Taken together, they explain over one-third of the variation in education and between one-tenth and one-sixth of the variation in the two cognitive assessments. A good deal of this explanatory power is attributable to maternal and paternal education alone, as shown by the partial R^2 in the lower panel of the table: over half the explanatory power in each of the two models of cognitive assessments is attributable to parental education, as is over two-thirds in the model for respondent education.

If intergenerational transmission of education and cognition occurs during childhood, then maternal education should have a larger impact on outcomes than paternal education, conditional on household resources, since mothers tend to provide the lion's share of care for children. While the effect sizes tend to be larger for maternal education, the differences between maternal and paternal education are significant only in three cases (education of daughters and abstract reasoning of sons and daughters). Moreover, with respect to child education, maternal and paternal education are substitutes, as indicated by the negative and significant interaction, but this is not the case for the cognitive assessments.

The results establish two important facts. First, background characteristics are important determinants of human capital outcomes in adulthood, even after controlling for a broad array of individual characteristics. Second, the mechanisms underlying the conversion of background characteristics to human capital outcomes are complex and unlikely to be explained by simple models. We turn, therefore, to estimation of structural models that focus on the relationship between own education and cognitive outcomes.

5.2 Education and adult cognitive outcomes

The relationships between the respondent's completed years of education and the two cognitive assessments are displayed in Table 4 with quantitative reasoning in panel A and abstract reasoning in panel B. Education is specified as a linear spline with a knot at junior high school completion (nine years of schooling).

We start with a baseline model in column 1 of each panel that documents the correlation between education and cognitive performance after adjusting for respondent gender and age (specified as a spline). Each additional year of education up to completion of junior high school is associated with a 0.11 to 0.16 standard deviation higher score on the cognitive assessments and, beyond junior high school, about 0.09 standard deviation higher scores.[2] The effect sizes are similar across the two cognitive assessments. These associations are likely to be upward biased estimates of the causal effect of education as the estimates also reflect the role of other, unobserved characteristics that are positively correlated with education and are positively associated with cognitive performance including, for example, parental human capital and other measures of own human capital such as height.

As a first step toward assessing the importance of these background characteristics, they are included in the model reported in the second column of each panel. Education remains a significant predictor of both cognitive scores and the effect sizes are only modestly attenuated. Attenuation is greater at higher levels of educational attainment for quantitative reasoning.

Although IFLS reports a rich array of family and individual background characteristics, it is unlikely that all relevant background characteristics are measured. Column 3 of each panel reports estimates of the model with sibling fixed effects, [7], which takes into account both measured and unmeasured shared family background characteristics that affect the cognitive outcomes in a linear way. The estimates are substantially reduced in magnitude: for three out of the four estimates, the effect size of a year of education up to the start of junior high school is

[2] Note that the coefficients in the tables are multiplied by 100.

approximately 75% of the magnitude of the baseline estimate, and for each year after completion of junior high school, the effect size is approximately half the magnitude of the baseline estimates. The sibling fixed effects are jointly significant, and the education estimates in the models with fixed effects are also considerably smaller than the estimates with observed background controls included, particularly among those who completed more than junior high school education, indicating that adjusting for observed background characteristics absorbs only part of the role that background characteristics play in the correlation between education and adult cognition. Taken together, the differences in the estimates in the sibling fixed effects models in column 3 and the models with background controls in column 2 are statistically significant as indicated by the Hausman tests, displayed in the last row of Table 4.

Taking into account all measured and unmeasured background characteristics, education is a significant determinant of both quantitative and abstract reasoning. The magnitudes are large: relative to a respondent who has no education, someone who has completed junior high school is predicted to score approximately one standard deviation higher on each of the assessments. For quantitative reasoning for males and females and for abstract reasoning for males, linearity in education is rejected (as shown by the F tests at the foot of the table). The differences are not only statistically significant, they are also substantively important: up to junior high school completion, the effect of an additional year of education is more than twice the magnitude of an additional year of education beyond junior high school. Linear models and, in particular, those that are identified using expansion in primary education are likely to overstate the benefits of post-primary education.

While the estimated effects of education on quantitative reasoning are significantly larger for females, relative to males, the effects of education on abstract reasoning are not significantly different by gender. Controlling observed background characteristics, males score better than females on quantitative reasoning, but when contrasts are drawn between siblings, this gap is not significant. There is no evidence of a gender gap in abstract reasoning.

Studies have established that attained height as an adult parallels education as a marker of human capital. In the models with sibling fixed effects, adult height captures the effects of differences between siblings in early life nutrition and health; the role of shared genetic endowments should be absorbed by the fixed effects. Height is a significant predictor of both quantitative and abstract reasoning for both males and females, indicating that early life resources play a role in cognitive skill acquisition. The models also include location at birth and at age 12, health as a child and experience of hunger as a child; taken together, the individual characteristics included in the model are jointly significant predictors of abstract reasoning but not of quantitative reasoning.

If the exclusion of these observed measures of early life exposures from the fixed effects models has a substantial impact on the estimated effects of education, we would conclude that these individual-specific early life factors are correlated with both cognition and education. There is no evidence to suggest that this is the case. As shown in Appendix Table 2A, none of the estimated effects of education in the models is substantively or significantly affected by the exclusion of the individual-specific characteristics. For example, relative to the estimates from the models that include the controls, the estimated effect of a year of education up to junior high school graduation on abstract reasoning increases from 0.0915 to 0.0920 standard deviations for males and from 0.0848 to 0.0876 standard deviations for females when the controls are excluded (Appendix Table 2A, Section B). These results suggest that, if there are unmeasured individual-specific characteristics that affect both education and cognition, they are likely to have only a small impact on the estimated coefficients and any resulting bias in the estimates is likely to be negligible and probably positive.

It is helpful to provide some context for who within a family is better educated, by relating siblings' early life characteristics to their probability of being the best educated among them. Results from estimating sibling fixed effects models with a discrete dependent variable indicating the sibling with the highest level of education and covariates reflecting early life exposures are presented in Appendix Table 2B. The coefficients are multiplied by 100. The first model includes a subset of exposures: the later born are better educated, there is no premium to being the firstborn and no premium to being born in a village or town, relative to a city. Males who were not in poor health during childhood are better educated than their siblings, and females who were not hungry in the first 5 years of life are better educated (although less than 2% of respondents report being hungry at that point in their lives). The second model includes additional measures of health status during childhood (none is significant) and hunger in later childhood (which is also not significant). Overall, the models provide evidence that the better educated within a family were healthier as young children, although the estimated differences are very modest.

5.3 Measurement error in education

Depending on the nature of measurement error in covariates, the impact on estimates can be magnified in models that include fixed effects. Own reports of education may be overstated, particularly among respondents who were close to completing an important education milestone (such as graduating from high school) and report having completed that level (Haberman and Sheinberg 1969). IFLS is well suited for assessing the importance of measurement error in the context of this model. The household roster enumerates each member, and the respondent is asked about basic demographics including education. It is the first module completed in the household survey. Each adult household member is asked to complete an individual-specific set of modules including a detailed education history. The questions about completed education, which are identical in the two modules, ask, first, the level of completed education and, second, the grade within that level. The roster is completed by the person who is most knowledgeable about household members (usually the head if she is female or the spouse of the head if he is male). In our sample, 45% of the respondents to the individual-specific education modules are also the respondents for the roster. For each of the other 55%, we have two measures of completed education of the respondent: an own report (in the education module) and a proxy report (in the roster). For 80% of these respondents, the answers to both the education level and grade questions are identical, and among the 20% for whom the reports differ, the correlation is 0.95, and the average difference is small (0.33 years of completed education).

To assess the extent of random noise in the years of completed education reported by two different respondents for the same individual, the module measure is regressed on the roster measure. The estimated slope is 0.97 (s.e.=0.002). The estimated slope in the reverse regression with the own-report module measure as the covariate is 0.93 (s.e.=0.002). The results indicate that random noise is modest, and since the roster measure appears to be less noisy, that measure is used in the analyses.

To check that none of our conclusions is affected by the choice of education measure, the models with sibling fixed effects in columns 3 of Table 4 have been re-estimated replacing the roster measure with the education module measure. The results for each cognitive outcome are reported in column 4 of the table. There is no pattern to the differences in the education coefficient estimates in columns 3 and 4, no difference is large in magnitude, and none is statistically significant. Taken together, the differences are not statistically significant: the p-values for the F test statistics are 0.99 for both outcomes. Measurement error is not an important concern.

5.4 Heterogeneity in education and adult cognition relationships by family resources

The fixed effects models assume that background characteristics affect cognitive outcomes in a linear and additive manner. This is a strong assumption: it is plausible that the benefits of an additional year of education differ with family resources. We investigate this possibility by stratifying the sample by maternal education and thereby allowing the effects of own education (and all other individual characteristics) to interact with maternal education in a flexible manner.

Maternal education is an appealing marker of resources for two reasons. First, educational attainment in Indonesia has increased dramatically over the last generation, with strong relative gains among women. Whereas the average mother and father in the sibling sample completed 4 and 5.6 years of schooling, respectively, the average adult child in the sample has attained approximately twice as many years (9.9), and there is no gender gap in attainment in the sibling sample. When the parents were at school, there was also enormous heterogeneity in the quality of education, particularly at the elementary level (up to six years of schooling). Second, in contrast with household resources, maternal education is fixed during the lives of the children.

Table 5 reports results of the fixed effects models estimated for the sample of siblings whose mothers did not complete elementary school (column 1) and those whose mothers did complete elementary school (column 2). The differences in the estimates are in column 3.

For adults with better-educated mothers, a model that is linear in education is not rejected for either cognitive outcome for males and females. However, the linear model is rejected for those with less educated mothers, except for abstract reasoning among females. For the other three models, the education effects are large and significant for the first nine years of own education and small and insignificant for additional years of education. The nonlinearities in the education–cognition function documented in the previous subsection are largely driven by adults whose mothers have little education.

For abstract reasoning, the differences across the maternal education distribution are not statistically significant. For quantitative reasoning, however, the gaps are large and statistically significant. Among those siblings whose mother did not complete elementary school, educational attainment beyond junior high school has no impact on quantitative reasoning, but if the siblings' mother had at least an elementary school education, the effects of own education beyond junior high school are large and statistically significant. The value added of secondary and tertiary education is greater for those whose mothers are better educated, indicating that the intergenerational transmission of education is not a linear function of maternal education but appears to decline as children attain higher levels of education relative to those of their parents. For both cognitive measures, restricting the effect of maternal education to be linear is not rejected.

5.5 Nonlinearities in years of education

The relationship between education and cognition is generally concave, as indicated by a steeper slope below the knot in the spline at nine years of education (completed junior high) relative to the slope above the knot. To further investigate nonlinearities in the cognition–education relationship, Appendix Table 3 reports results with two knots: one at completed primary school (six years of education), which accounts for 26% of the sample, and at junior high completion (nine years), which accounts for an additional 22% of the sample.

For males, the slopes are steepest for the first six years of schooling for both quantitative and abstract reasoning. This pattern is repeated for abstract reasoning when we stratify the sample by maternal education. For quantitative reasoning, the pattern is repeated for males with less well-educated mothers but the education effect is smallest for the males with little education if their mothers are well educated. As shown by the F test statistic at the bottom of the table, the differences in the slopes below and above the knot at six years are not statistically significant for abstract reasoning; the difference is significant for quantitative reasoning, but only for males whose mothers have less than six years of education.

For females, the slopes above and below the knot at six years are not statistically different for either cognitive measure. For quantitative reasoning, the slope above the nine-year knot is much less steep, and the model with one knot at this point captures this relationship well. For abstract reasoning, linearity of the relationship is not rejected whether there are one or two knots in the spline specification. The patterns are very similar for females with more and less well-educated mothers.

The only significant difference in the shapes of the education relationships across maternal education is in performance on quantitative reasoning among those who have completed junior high school. For both males and females, the benefits of having a better-educated mother are statistically significant. We conclude that the spline with one knot at completed junior high is sufficiently flexible to capture the key differences in the associations across the education distribution.

5.6 Heterogeneity by family size, birth order and relative age

It is possible that a respondent's number of siblings is correlated with individual-specific unobserved heterogeneity, for example, because of economies of scale in the production of child human capital, learning by parents, or greater variability in household resource availability in the first years of the children's lives as the number children increases. In such cases, the fixed effects estimates will be biased, as illustrated by Miller et al. (2019) in the context of a binary treatment. Differences in the estimates by sibsize are displayed in Appendix Table 4A, where sibsize is defined as the number of siblings in the baseline household who were assessed in IFLS5. The models with sibling fixed effects are estimated separately for respondents with 1 sibling (column

1) and those with more than 1 sibling (in column 2); the differences in the estimates are reported in column 3. There is no systematic pattern in the differences: some estimates are larger and some smaller in the subsample of sibling pairs than in the subsample with more siblings. None of the estimated differences is statistically significant.

Since sibling pairs make up only one-eighth of the sample, these tests may lack power; we also stratify the sample into two approximately equal subsamples of those with three or fewer siblings (column 4) and those with more than three siblings (column 5). Again, the estimates in these two subsamples are extremely close, and as shown in column 6, all of the differences are small in magnitude, and none is statistically significant. We conclude that there is no evidence that the fixed effects estimates vary with sibsize and the assumption that sibsize is unrelated to unobserved heterogeneity cannot be rejected in this context.

A related concern is that the birth order of siblings may be correlated with education and other unobserved sibling-specific factors that affect cognition. Figure 2 displays the educational attainment of the entire Indonesian population by birth cohort. The fraction who completed primary school (≥6 years) rose dramatically until the mid-1970s birth cohort, and the increases in junior high school completion (≥9 years) have continued through to the 1995 birth cohort. The increases are particularly stunning for females: in the 1930 birth cohort, a male was about twice as likely as a female to complete primary or junior high school. By the 1980 birth cohort, the gender gap had been erased.[3]

Figure 2 highlights a potentially important point for this research: older siblings are likely

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[3] The figure uses the 2015 Survei Angkatan Kerja Nasional (National Labor Force Survey, SAKERNAS); the underlying data are reproduced in Appendix Table 1. In addition to being nationally representative, the large sample size (>500,000 respondents aged 15 and older) supports describing the transformation in education by birth cohort reaching back to 1930. As explained above, the IFLS sample used in the regression models is restricted to the 1965 to 1990 birth cohorts.

to have less education than younger siblings. (We have already established that our results do not differ by gender.) To evaluate whether the estimates are contaminated by unobserved heterogeneity correlated with birth order, we re-estimate the fixed effects models in Table 4 with an indicator identifying the oldest sibling and this indicator interacted with all covariates in the model. The results are displayed in panel I of Appendix Table 4B. The main effects of education are in section 1 of the table, and the interactions with education are in section 2.

There is no evidence that being the oldest sibling in the sample is related to cognition or mediates the effect of education. The oldest sibling indicator is not a significant predictor of quantitative or abstract reasoning, and the interactions with education are not individually or jointly significant (p-values for joint significance are 0.56 and 0.94 for quantitative and abstract reasoning, respectively).

There may be unobserved differences across siblings that are better captured by differences in year of birth than by whether the sibling is the firstborn, and so the models have been re-estimated interacting all covariates with the respondent's birth year. Results are displayed in panel II of Appendix Table 4B. With sibling fixed effects, the interaction terms in panel 2 of the table can be interpreted as interactions with the gap between the respondent's birth year and the average for all of their siblings. None of the interactions is large in magnitude or statistically significant.

We conclude that sibsize, birth order and age differences between siblings are not sources of individual-specific heterogeneity that affect estimates of the relationship between own education and cognition in adulthood.

5.7 Education and adult height

Adult height is an indicator of human capital, and a large literature has documented a height premium in the labor market, which has been attributed to an array of factors including both cognitive and non-cognitive skills (Case and Paxson 2008b; Schick and Steckel 2015; Steckel 1995; Strauss and Thomas 1998). Consistent with this interpretation, as shown in Table 4, early life nutrition, as indicated by adult height, predicts differences in cognitive outcomes of siblings. Since height is largely determined by age four or five (Martorell and Habicht 1986) and education begins only around age six and is typically completed in late adolescence or early adulthood, if education predicts adult height, those estimates must reflect the influence of unobserved heterogeneity that affects both early life nutrition and educational attainment. This insight provides a natural placebo-type test of the assumption that is necessary to assign a causal interpretation to the sibling fixed effects estimates of the effect of education on cognition. Specifically, we test whether education, conditional on observed background, predicts adult height, and then we contrast these estimates with estimates that include sibling fixed effects. Table 6 reports the results of models that parallel those in Table 4 with height as the dependent variable (with height excluded from the covariates).

Conditional on age and gender, education is a powerful and significant predictor of height (column 1). The inclusion of observed individual and family background characteristics reduces the magnitudes of the education coefficients in the model, but they remain statistically significant for both males and females (column 2). Although IFLS provides an extremely rich array of background characteristics, those measures do not fully absorb the unobserved heterogeneity shared by adult education and height. These results underscore the importance of the concern that conditioning on observed family background is likely to yield estimates that are contaminated by omitted variable bias.

In contrast, once the sibling fixed effects are included (in column 3), education is not a statistically significant predictor of height for either males or females. The sibling fixed effects sweep out the impact of unobserved heterogeneity in model [7] that contaminates the estimates in the first two columns of the table. Given that education and height are highly correlated (as shown in column 1), this test is likely to have power to detect failure of the assumption that shared unobserved heterogeneity drives human capital outcomes in adulthood. This rules out, for example, one sibling being systematically preferred over the other in terms of money or time investments that result in the preferred sibling being both taller and better educated. This parallels the evidence above documenting that the addition of individual-specific controls to the models with sibling fixed effects does not impact the estimates. The evidence is consistent with our assigning a causal interpretation to the education coefficients in the models that include sibling fixed effects. We cannot rule out the possibility that parents invest differentially in the human capital of their children based on their ability. However, these differential investments would have to affect dimensions of human capital other than nutrition and health, or they would have to occur after the children's height trajectories have been determined. Moreover, as noted by Behrman, Pollak and Taubman (1982), it is not clear whether such investments would favor more able or less able children within a family.

6. Conclusion

Using longitudinal survey data from Indonesia that spans more than twenty years, we have documented a positive association between completed education and adult cognitive performance as measured by quantitative and abstract reasoning assessments. After adjusting for age and gender, at least a quarter to a half of the estimated education–cognition association can be attributed to observed and unobserved individual and family characteristics that affect both education and cognition. Studies that draw conclusions about the protective effects of education on cognitive aging are likely to substantially overstate those benefits.

Many studies in the literature have adjusted for individual and family background in an attempt to isolate the causal effect of education on cognition. We show that even after including an unusually rich array of observed individual and family background characteristics, the estimated impacts of education on adult cognition remain substantially upward biased, and in our context, this approach results in only a modest reduction in the bias.

We have documented that there are important unmeasured characteristics that affect both education and cognition including, for example, parents' tastes for investment in the human capital of their children, shared genetic influences and family socioeconomic resources. To the extent that unmeasured characteristics are shared by siblings, they are taken into account in models with sibling fixed effects, and the resulting estimates will be unbiased. Our results from those models indicate that education is a significant and important predictor of cognitive performance with estimated effect sizes that are very similar for both quantitative and abstract reasoning. This is true for both males and females and, broadly speaking, across the distribution of education.

Importantly, there are substantively large differences in the effects of education across both the distribution of own education and the distribution of parental education. Specifically, an additional year of education during the first nine years of schooling has approximately twice the impact on cognition as an additional year of education beyond nine years. This nonlinearity affects the interpretation of estimates of education effects based on expansions in the supply of education in the 1970s through the INPRES primary school construction program (Sanginabadi, 2020). Those estimates identify effects around primary school (the first six years of schooling) for the exposed cohorts. It is noteworthy that the magnitudes of those estimates are very similar to our fixed effects estimates for the first nine years of education.

Local average treatment effects that exploit expansions in education at specific levels or changes in the minimum school-leaving age cannot provide evidence about the effects of education across its entire distribution. This is a substantively important concern in the Indonesian context given our evidence and because high school attendance and completion have dramatically increased over time (as shown in Figure 2). An important advantage of our sibling fixed effects approach is that we document the shape of the education–cognition relationship across the entire distribution of education rather than only at the level where access to schooling changed. Moreover, our estimates establish that this nonlinearity in the relationship is largely driven by the fact that an additional year of education during the first nine years has a very large effect on cognition among adults whose mothers did not complete elementary school and a much smaller and mostly insignificant impact at higher levels of own education. In sharp contrast, among adults whose mothers completed at least an elementary education, the effects of education are effectively linear across the entire distribution of own education.[4]

These results have two key implications. First, the long reach of inequality in family background is reflected in adults' cognitive performance in later life. Second, investments in the education of the current cohort are likely to affect not only their own lives but also those of their children and future generations.

The credibility of identification of causal effects of education on other human capital and socioeconomic outcomes is predicated on assumptions that are often difficult to test. We show that including individual-specific observed characteristics that reflect circumstances as a child does not affect our estimates with sibling fixed effects. Furthermore, we provide a placebo-type test of our sibling fixed effects specification by empirically evaluating an implication of the assumption that individual-specific unobserved heterogeneity does not contaminate the estimated

[4] Concavity of the education–cognition relationship has been documented in other studies. For example, Weir, Lay, and Langa (2014) examine Serial 7s and Orientation and argue that concavity in their context likely reflects ceiling effects. Since only a very small fraction of respondents in our analyses answered all the quantitative or Raven items correctly, ceiling effects are unlikely to explain our results.

effects on cognitive performance. Specifically, we examine an alternative measure of human capital, attained height as an adult, that cannot possibly be caused by education. We establish that, in models without sibling fixed effects, the estimates are contaminated by omitted variable bias but, when we absorb all observed and unobserved characteristics shared by siblings, education is not predictive of height. This evidence is consistent with our interpreting the estimates from the models with sibling fixed effects as causal. Nonetheless, it is important to recognize that there may be individual-specific differences between siblings that affect both education and cognition in adulthood. If these differences operate in the same direction on education, quantitative reasoning and abstract reasoning, our sibling fixed effects will be upward biased.

By exploiting long-term longitudinal data from a low-resource setting, we have established that education is a powerful predictor of both quantitative and abstract reasoning in adulthood. The estimated effects are substantively large, particularly at lower levels of education, after taking into account observed and unobserved differences in family background. Failure to take these differences into account results in estimates that very substantially overstate the benefits of education. Furthermore, we have documented that the sizes of the effects and shapes of the relationships vary substantially with both observed and unobserved family background factors.

Taking all the evidence together, we conclude that investments in education are likely to yield a pay-off not only in terms of improved productivity and economic security but also in terms of cognitive performance. These benefits will impact the well-being of the beneficiaries of these investments through their entire life course as well as the well-being of their progeny.

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Figure 1. Human capital by birth year and sample

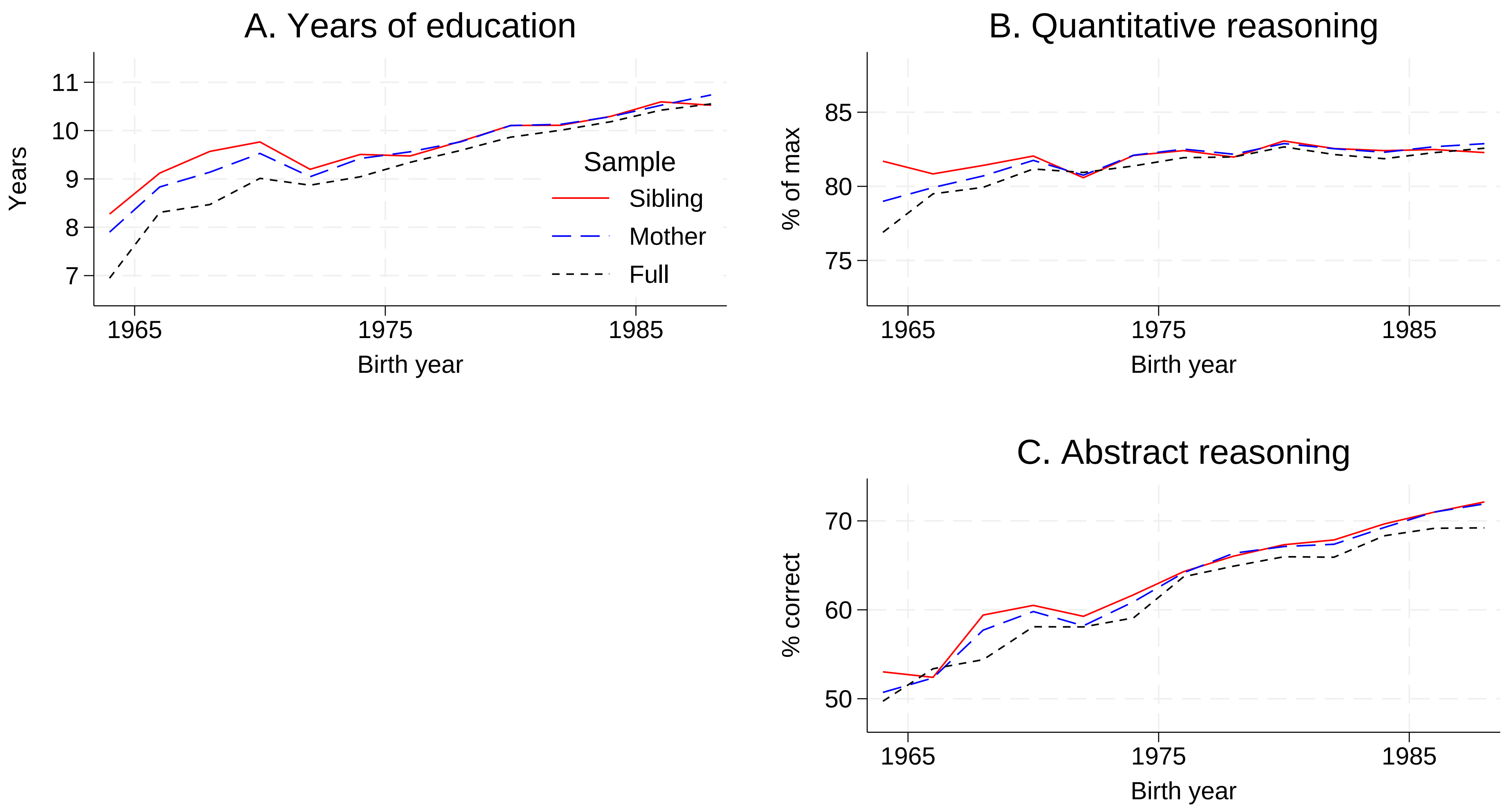

Figure 2. Education attainment of Indonesians by birth year and gender

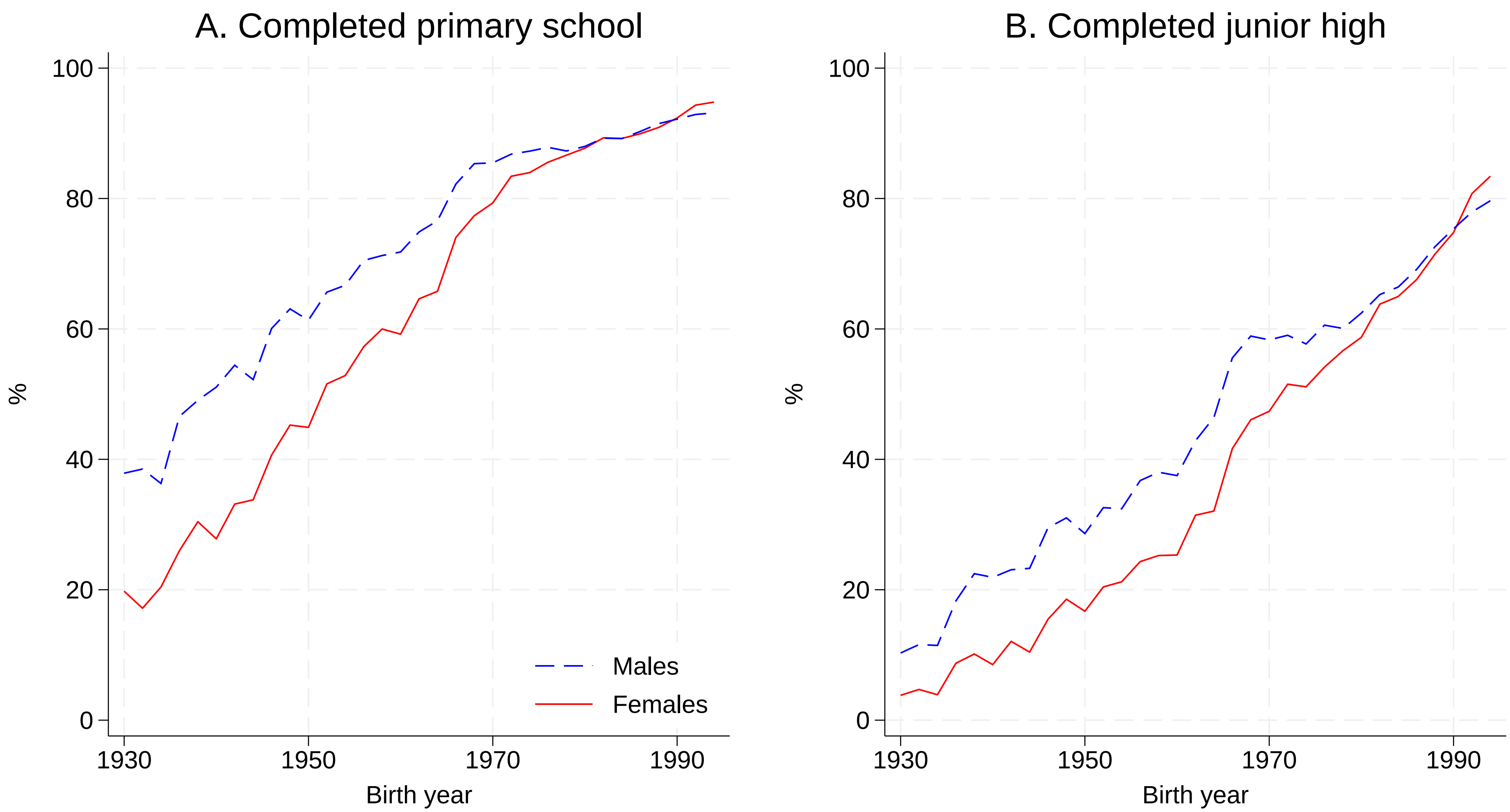

Table 1. Characteristics of adult respondents in IFLS5

Sample:	All respondents aged 25-49y	Respondents with a Mother interviewed	Sibling assessed	Differences Mother sample rel to all	Sib sample rel to mother
	[1]	[2]	[3]	[4]=[2]-[1]	[5]=[3]-[2]
Age (years)	36.31	34.99	34.91	-1.32	-0.07
	(6.8)	(6.3)	(5.9)	[0.08]	[0.10]
% male	47.13	47.31	48.52	0.17	1.21
				[0.63]	[0.80]
Education (years)	9.51	9.90	9.94	0.39	0.04
	(3.9)	(3.8)	(3.7)	[0.05]	[0.06]
Quantitative reasoning (% max score)	81.56	82.18	82.23	0.62	0.05
	(10.0)	(9.8)	(9.7)	[0.12]	[0.15]
Abstract reasoning (% correct)	63.03	65.52	66.00	2.48	0.48
	(25.7)	(25.1)	(24.6)	[0.32]	[0.40]
Sample size	17,815	9,933	6,505		

Note: Means, (standard deviations) and [robust standard errors]. All respondents aged 25-49y at time of IFLS5 (col 1); sample restricted to those whose mother was interviewed in any IFLS (col 2) and to those who have a sibling assessed in IFLS5 (col 3).

Table 2. Characteristics of sample of siblings

	Males	Females	Difference
	[1]	[2]	[3]
Age (years)	35.01	34.82	0.19
	[0.11]	[0.10]	[0.15]
Education (years)	9.97	9.91	0.05
	[0.06]	[0.06]	[0.09]
Quantitative reasoning (z score)	6.99	-6.59	13.58
	[1.70]	[1.79]	[2.47]
Abstract reasoning (z score)	8.18	-7.70	15.88
	[1.73]	[1.76]	[2.47]
Sample size	3,156	3,349	

Note: Means [and standard errors]. All respondents aged 25-49y at time of IFLS5
Quantitative and abstract reasoning scores converted to z scores and multiplied by 100.

Table 3. Human capital outcomes of adult children and parental education

	A. Education		B. Quantitative reasoning		C. Abstract reasoning	
	Males	Females	Males	Females	Males	Females
	[1]	[2]	[1]	[2]	[1]	[2]
Parental education (years)						
Maternal	0.22	0.29	2.73	4.46	4.22	4.62
	[0.03]	[0.02]	[0.70]	[0.66]	[0.70]	[0.68]
Paternal	0.25	0.24	2.20	3.66	1.46	2.05
	[0.02]	[0.02]	[0.59]	[0.62]	[0.58]	[0.63]
Maternal*paternal	-0.017	-0.021	-0.048	-0.237	-0.289	-0.183
	[0.004]	[0.004]	[0.115]	[0.116]	[0.114]	[0.113]
(1) if male	-5.16		115.42		-49.23	
	[2.62]		[82.40]		[84.04]	
R^2	0.36	0.43	0.11	0.16	0.14	0.17
Partial R^2 (parental educ)	0.26	0.32	0.06	0.10	0.07	0.08
F tests for significance *(p values)*						
Maternal education	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*
Paternal education	*0.00*	*0.00*	*0.00*	*0.00*	*0.01*	*0.00*
Maternal & paternal educ	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*
Family background	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*
Individual background	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*
All background	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*
F tests for equality *(p values)*						
Maternal=paternal	*0.96*	*0.02*	*0.59*	*0.30*	*0.00*	*0.01*
Maternal: male=female	*0.05*		*0.30*		*0.39*	
Paternal: male=female	*0.29*		*0.69*		*0.25*	

Note: Regression coefficients and [standard errors] allowing clustering at family level.
Education measured in years. Quantitative and abstract reasoning scores converted to z scores and multiplied by 100.

Table 4. Cognitive outcomes of adults and own years of education

	A. Quantitative reasoning (z score*100)				B. Abstract reasoning (z score*100)			
	Base	Background	Sibling fixed effects	Sibling FE (alt educ)	Base	Background	Sibling fixed effects	Sibling FE (alt educ)
	[1]	[2]	[3]	[4]	[1]	[2]	[3]	[4]
Years of education (spline)								
Males								
0-9 years	11.92	11.34	8.93	9.25	12.89	11.55	9.15	8.79
	[1.26]	[1.26]	[1.46]	[1.38]	[1.07]	[1.12]	[1.44]	[1.35]
		95%	*75%*	*78%*		*90%*	*71%*	*68%*
≥9 years	8.40	7.26	4.19	3.79	7.49	5.83	3.58	3.68
	[0.88]	[0.98]	[1.21]	[1.18]	[0.82]	[0.95]	[1.16]	[1.14]
		86%	*50%*	*45%*		*78%*	*48%*	*49%*
Females								
0-9 years	16.04	14.50	14.70	13.95	11.30	9.57	8.48	7.66
	[1.22]	[1.26]	[1.45]	[1.31]	[1.01]	[1.02]	[1.36]	[1.28]
		90%	*92%*	*87%*		*85%*	*75%*	*68%*
≥9 years	8.73	6.28	4.18	4.01	9.30	7.27	6.61	6.69
	[0.81]	[0.91]	[1.13]	[1.13]	[0.84]	[0.94]	[1.16]	[1.16]
		72%	*48%*	*46%*		*78%*	*71%*	*72%*
(1) if male	121.75	170.71	115.85	113.10	-6.36	-28.20	-7.75	-8.99
	[67.40]	[80.39]	[92.55]	[92.81]	[69.23]	[82.96]	[94.42]	[94.53]
Height (cms)								
Males	.	1.15	0.81	0.82	.	1.06	0.82	0.83
		[0.33]	[0.38]	[0.38]		[0.30]	[0.35]	[0.35]
Females	.	0.74	0.80	0.82	.	1.36	1.03	1.05
		[0.35]	[0.41]	[0.41]		[0.33]	[0.39]	[0.39]
R^2	0.18	0.22	0.09	0.09	0.18	0.22	0.10	0.10
F tests for joint significance *(p values)*								
Sibling fixed effects			*0.00*	*0.00*			*0.00*	*0.00*
Educ: Males	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*
Educ: Females	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*	*0.00*
Individ characs: Males			*0.58*	*0.63*			*0.00*	*0.00*
Individ characs: Females			*0.39*	*0.52*			*0.00*	*0.00*
F tests *(p values)*								
Linear in educ: Males	*0.06*	*0.03*	*0.03*	*0.01*	*0.00*	*0.00*	*0.01*	*0.01*
Linear in educ: Females	*0.00*	*0.00*	*0.00*	*0.00*	*0.21*	*0.14*	*0.35*	*0.62*
Educ: Male=Female	*0.01*	*0.19*	*0.00*	*0.01*	*0.26*	*0.32*	*0.07*	*0.08*
Height: Male=Female		*0.39*	*0.97*	*1.00*		*0.51*	*0.65*	*0.63*
Hausman test (p-value)		*29.6 (0.013)*				*33.46 (0.004)*		

Note: Regression coefficients and robust [standard errors] that take into account clustering at the family level. % of base estimates in italics.
Base model also includes spline in age. Background model adds own and parental characteristics.

Table 5. Cognitive outcomes of adults and own years of education
Models with sibling fixed effects stratified by level of maternal education

	A. Quantitative reasoning (z score*100)			B. Abstract reasoning (z score*100)		
Maternal education:	<6 years	≥6 years	Difference	<6 years	≥6 years	Difference
	[1]	[2]	[3]	[1]	[2]	[3]
Education of males (spline)						
0-9 years	10.10	5.72	-4.38	9.71	5.32	-4.39
	[1.64]	[3.28]	[3.66]	[1.64]	[3.05]	[3.46]
≥9 years	1.75	8.51	6.76	2.59	4.69	2.10
	[1.70]	[1.73]	[2.42]	[1.63]	[1.78]	[2.41]
Education of females (spline)						
0-9 years	14.74	13.87	-0.87	8.66	6.91	-1.75
	[1.61]	[3.83]	[4.14]	[1.53]	[3.71]	[4.00]
≥9 years	0.72	6.71	5.99	6.87	7.17	0.30
	[1.71]	[1.60]	[2.34]	[1.68]	[1.72]	[2.40]
(1) if male	141.81	51.93	-89.88	112.72	-137.18	-249.90
	[127.82]	[136.05]	[186.49]	[135.73]	[141.41]	[195.83]
R^2	0.11	0.09	0.10	0.12	0.09	0.11
F tests (*p values*)						
Educ: Males	*0.00*	*0.00*	*0.02*	*0.00*	*0.00*	*0.41*
Educ: Females	*0.00*	*0.00*	*0.04*	*0.00*	*0.00*	*0.91*
Linear in educ: Males	*0.00*	*0.50*	*0.02*	*0.01*	*0.88*	*0.19*
Linear in educ: Females	*0.00*	*0.11*	*0.19*	*0.49*	*0.95*	*0.69*
Educ: Male=Female	*0.07*	*0.21*	*0.78*	*0.10*	*0.32*	*0.78*

Note: Regression coefficients and robust [standard errors] that take into account clustering at the family level.
There are 3,991 siblings from 1,492 families with a mother who has completed <6 years of schooling
and 2,514 siblings fom 956 families with a mother who has completed 6 or more years of schooling

Table 6. Adult height and own years of education

	Base	Background	Sibling fixed effects
	[1]	[2]	[3]
Years of education (spline)			
Males			
0-9 years	0.13	0.05	0.06
	[0.07]	[0.06]	[0.07]
≥9 years	0.38	0.20	0.08
	[0.06]	[0.06]	[0.06]
Females			
0-9 years	0.13	0.10	0.04
	[0.06]	[0.05]	[0.06]
≥9 years	0.20	0.05	-0.06
	[0.05]	[0.05]	[0.06]
R^2	0.032	0.278	0.012
F tests for joint significance *(p values)*			
Sibling fixed effects			*0.00*
Educ: Males	*0.00*	*0.00*	*0.15*
Educ: Females	*0.00*	*0.05*	*0.56*

Note: See Table 4.

Appendix Table 1. Educational attainment by birth cohort and gender

Birth	Completed primary		Completed junior high	
cohort	Male	Female	Male	Female
1930-31	37.9	19.8	10.3	3.8
1932-33	38.5	17.2	11.6	4.7
1934-35	36.3	20.4	11.5	3.9
1936-37	46.6	26.0	18.3	8.7
1938-39	49.1	30.4	22.5	10.1
1940-41	51.1	27.8	21.9	8.5
1942-43	54.4	33.1	23.1	12.1
1944-45	52.2	33.8	23.3	10.4
1946-47	60.1	40.7	29.5	15.5
1948-49	63.1	45.3	31.0	18.6
1950-51	61.3	44.9	28.6	16.7
1952-53	65.6	51.6	32.6	20.4
1954-55	66.7	52.9	32.4	21.2
1956-57	70.5	57.3	36.7	24.3
1958-59	71.3	60.0	38.0	25.3
1960-61	71.8	59.2	37.5	25.3
1962-63	74.9	64.6	42.9	31.4
1964-65	76.6	65.8	46.4	32.1
1966-67	82.2	74.0	55.6	41.6
1968-69	85.3	77.4	58.9	46.1
1970-71	85.5	79.3	58.3	47.4
1972-73	86.8	83.4	59.0	51.5
1974-75	87.3	84.0	57.7	51.1
1976-77	87.9	85.6	60.6	54.2
1978-79	87.3	86.7	60.1	56.7
1980-81	88.0	87.7	62.4	58.7
1982-83	89.3	89.3	65.3	63.8
1984-85	89.2	89.2	66.4	65.0
1986-87	90.3	89.9	69.1	67.6
1988-89	91.5	90.9	72.6	71.5
1990-91	92.2	92.4	75.4	74.8
1992-93	92.9	94.3	77.9	80.8
1994-95	93.1	94.8	79.7	83.4

Source: SAKERNAS 2015

Appendix Table 2A. Sensitivity of estimates of relationship between education and cognition to exclusion of individual-specific early life measures in models with sibling fixed effects

	Individual-specific early life controls	
	I. Included	II. Excluded
	[1]	[2]
A. Quantitative reasoning (z score*100)		
Years of education (spline)		
Males		
0-9 years	8.93	8.80
	[1.46]	[1.48]
≥9 years	4.19	4.34
	[1.21]	[1.18]
Females		
0-9 years	14.70	15.22
	[1.45]	[1.43]
≥9 years	4.18	4.48
	[1.13]	[1.10]
(1) if male	115.85	135.47
	[92.55]	[80.35]
B. Abstract reasoning (z score*100)		
Years of education (spline)		
Males		
0-9 years	9.15	9.20
	[1.44]	[1.43]
≥9 years	3.58	3.89
	[1.16]	[1.15]
Females		
Quantitative reasonii	8.48	8.76
	[1.36]	[1.33]
≥9 years	6.61	6.58
	[1.16]	[1.13]
(1) if male	-7.75	91.47
	[94.42]	[82.66]

Note: See Table 4. Column 1 is the same as column 3 in Table 4.

Appendix Table 2B. Characteristics associated with being better educated than one's siblings
Dependent variable is an indicator if the respondent is best educated among siblings
Models with sibling fixed effects

	Males		Females	
	[1]	[2]	[1]	[2]
Birth year	1.51	1.49	3.59	3.55
	[0.4]	[0.4]	[0.4]	[0.4]
(1) if first born	2.19	1.94	4.47	4.1
	[3.6]	[3.6]	[3.2]	[3.2]
(1) if health as a child				
-- not poor	11.97	10.98	6.66	5.03
	[5.8]	[6.3]	[4.6]	[5.1]
-- excellent		0.28		-9.12
		[7.3]		[6.9]
-- very good		-2.06		-1.39
		[4.4]		[4.2]
-- good		3.32		4.51
		[3.6]		[3.0]
(1) if not hungry when				
-- age 0-5y	-3.40	-3.71	27.72	28.39
	[14.1]	[14.2]	[11.2]	[11.4]
-- age 6-10y		1.36		4.86
		[7.0]		[7.9]
-- age 11-15y		0.63		14.79
		[17.5]		[16.0]
(1) if born in village	2.94	3.08	0.88	0.99
	[6.5]	[6.6]	[6.1]	[6.0]
(1) if born in a town	-0.96	-0.66	-2.13	-2.48
	[6.1]	[6.1]	[5.6]	[5.6]
Constant	47.57	45.87	21.59	1.96
	[15.3]	[23.9]	[12.6]	[21.5]
Observations	3,156	3,156	3,349	3,349

Note: OLS regression coefficients multiplied by 100. Robust standard errors in brackets taking into account clustering at the family level

Appendix Table 3. Cognitive outcomes of adults and own years of education

Splines with 2 knots in spline for education in sibling fixed effect models

	A. Quantitative reasoning (z score*100)				B. Abstract reasoning (z score*100)			
Maternal education:	All	Maternal educ <6 years	Maternal educ ≥6 years	Difference	All	Maternal educ <6 years	Maternal educ ≥6 years	Difference
	[1]	[2]	[3]	[3] -[2]	[4]	[5]	[6]	[6] -[5]
Education of males (spline)								
0-6 years	14.24	16.06	2.69	-13.37	12.42	12.11	11.36	-0.75
	[2.94]	[3.16]	[6.95]	[7.61]	[2.82]	[3.09]	[7.00]	[7.63]
6-9 years	4.18	4.29	7.61	3.32	6.38	7.51	1.59	-5.92
	[2.12]	[2.48]	[4.47]	[5.10]	[2.16]	[2.50]	[4.45]	[5.10]
≥9 years	5.01	3.14	8.22	5.08	4.12	3.18	5.10	1.92
	[1.23]	[1.75]	[1.73]	[2.46]	[1.19]	[1.66]	[1.81]	[2.45]
Education of females (spline)								
0-6 years	15.93	14.79	20.96	6.17	6.70	6.64	5.52	-1.12
	[2.61]	[2.82]	[7.21]	[7.72]	[2.26]	[2.41]	[7.04]	[7.42]
6-9 years	13.32	14.43	10.13	-4.30	9.98	10.66	7.60	-3.06
	[2.09]	[2.40]	[4.63]	[5.21]	[2.15]	[2.47]	[5.14]	[5.69]
≥9 years	4.34	0.65	7.06	6.40	6.29	6.31	7.09	0.77
	[1.15]	[1.74]	[1.63]	[2.39]	[1.19]	[1.73]	[1.74]	[2.46]
F tests for equality of 0-6 and 6-9 year slopes (p values)								
Male: 0-6 years = 6-9 year	*0.02*	*0.01*	*0.59*	*0.11*	*0.14*	*0.31*	*0.31*	*0.63*
Female: 0-6 years = 6-9 y	*0.48*	*0.93*	*0.22*	*0.28*	*0.35*	*0.29*	*0.83*	*0.85*

Note: Regression coefficients and robust [standard errors] that take into account clustering at the family level.

There are 3,991 siblings from 1,492 families with a mother who has completed <6 years of schooling.

Appendix Table 4A. Sensitivity of estimates of relationship between education and cognitive outcomes

Samples stratified by number of siblings in models with sibling fixed effects

	I. 1 sibling/> 1 sibling			II. <3 sibling/> 3 siblings		
	1 sibling	>1 sibling	Difference	≤3 siblings	>3 siblings	Difference
	[1]	[2]	[3]=[2-1]	[4]	[5]	[6]=[5-4]
A. Quantitative reasoning (z score*100)						
Years of education (spline)						
Males						
0-9 years	12.78	8.63	4.15	8.37	9.39	-1.01
	[4.20]	[1.57]	[4.37]	[1.99]	[2.17]	[2.94]
≥9 years	8.62	3.59	5.03	3.66	5.10	-1.43
	[4.41]	[1.27]	[4.46]	[1.75]	[1.69]	[2.43]
Females						
0-9 years	13.07	14.84	-1.77	16.39	14.00	2.40
	[4.23]	[1.54]	[4.39]	[2.11]	[1.97]	[2.89]
≥9 years	1.9	4.42	-2.52	4.3	3.85	0.45
	[4.47]	[1.17]	[4.50]	[1.64]	[1.61]	[2.30]
B. Abstract reasoning (z score*100)						
Years of education (spline)						
Males						
0-9 years	8.35	9.16	-0.81	9.16	9.32	-0.16
	[5.05]	[1.50]	[5.13]	[2.09]	[1.97]	[2.87]
≥9 years	3.26	3.28	-0.02	3.3	4.09	-0.79
	[4.09]	[1.22]	[4.15]	[1.71]	[1.66]	[2.38]
Females						
Quantitative reasoning (%	9.59	8.3	1.29	10.55	7.51	3.04
	[4.56]	[1.41]	[4.65]	[2.25]	[1.73]	[2.84]
≥9 years	0.64	6.81	-6.17	4.66	8.3	-3.63
	[4.07]	[1.22]	[4.14]	[1.69]	[1.64]	[2.35]
Sample size	828	5677		3,407	3,098	
# families	414	2034		1,455	993	

Note: Regression coefficients and robust [standard errors] that take into account clustering at the family level.

Appendix Table 4B. Heterogeneity depending on whether respondent is oldest sibling and by birth year of respondent

Models with sibling fixed effects include interactions of all covariates with

I. Whether respondent is oldest sibling

II Birth year of respondent

Interact all covariat	I. Indicator if oldest sibling		II. Birth year of respondent	
	A. Quantitative reasoning	B. Abstract reasoning	A. Quantitative reasoning	B. Abstract reasoning
	[1]	[2]	[3]	[4]
1. Main effects of education				
Education of males (spline)				
0-9 years	7.82	8.68	8.88	8.93
	[1.88]	[1.76]	[1.56]	[1.45]
≥9 years	4.88	4.19	4.24	3.71
	[1.38]	[1.36]	[1.22]	[1.18]
Education of females (spline)				
0-9 years	13.63	8.46	14.29	8.64
	[1.82]	[1.80]	[1.51]	[1.42]
≥9 years	5.05	6.76	4.15	6.51
	[1.34]	[1.33]	[1.15]	[1.19]
2. Education interacted with characteristic in table heading				
Males				
0-9 years	2.54	0.73	-0.06	-0.06
	[2.66]	[2.43]	[0.22]	[0.22]
>9 years	-1.94	-1.59	0.26	0.18
	[2.08]	[2.17]	[0.18]	[0.19]
Females				
0-9 years	1.89	-0.35	-0.33	-0.01
	[2.51]	[2.23]	[0.24]	[0.21]
>9 years	-2.27	-0.54	0.24	0.03
	[1.83]	[1.90]	[0.17]	[0.19]
Oldest sibling	10.29	-50.12		
	[234.46]	[285.98]		
F tests for significance (p values)				
Males: educ*intera	*0.55*	*0.76*	*0.33*	*0.65*
Females: educ*inte	*0.45*	*0.92*	*0.25*	*0.98*
All: educ*interactic	*0.56*	*0.94*	*0.31*	*0.93*

Note: Regression coefficients and robust [standard errors] that take into account clustering at the family level