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Working Paper

Young Lives

Reinforcement or Compensation?

Parental Responses to Children's Revealed Human Capital Levels in Ethiopia

Wei Fan and Catherine Porter

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Summary

There is an increasing body of literature that finds that parents invest in their children unequally, but the evidence is contradictory, and few studies provide convincing causal evidence of the effect of child ability on parental investment in a low-income country. This working paper examines how parents respond to the differing abilities of primary school-age Ethiopian siblings, using rainfall shocks during the critical developmental period between pregnancy and the first three years of a child's life to isolate exogenous variation in child ability within the household, observed at a later stage than birth. The results suggest that on average parents attempt to compensate disadvantaged children through increased cognitive investment. The results are significant, but small in magnitude: parents provide about 6.3 per cent of a standard deviation more in educational fees to the lower-ability child in the observed pair. Families with educated mothers, smaller household size, and higher wealth compensate with more cognitive resources for a lower-ability child. This suggests that improving resources available to households would benefit the least advantaged young people.

1. Introduction

An increasingly large body of evidence has developed during the past three decades showing that in utero and early life conditions have a significant impact on children's early life ability, subsequent development and therefore on outcomes in adulthood (surveyed by Currie and Almond 2011; Almond et al. 2018). Most of these studies are reduced-form estimates of the total effect of an early life shock or adverse event on final adult health. However, ability in early life impacts on later human capital not only through the biological channel (Heckman 2007), but also through parental involvement – in theory parents can either reinforce or compensate for differences in early ability. It is then an empirical question whether parental actions amplify or mute the ultimate effect of early life shocks and circumstances on adult human capital outcomes.

This working paper contributes to this research question, which is of direct policy relevance. The current literature comprises empirical evidence that appears somewhat contradictory, containing studies that document both compensatory and reinforcing behaviour of parents. Attempting to clearly identify such effects given the econometric concerns is extremely difficult, and could be one reason for the apparently conflicting results. Alternatively, there may be important differences across country contexts (either cultural or economic) that are leading to such different conclusions.

Our contribution extends the existing literature in three ways. First, we examine the response of parents to differences in child cognitive ability in early childhood in a low-income country, using a measure of ability rather than birth weight or height as a proxy. We are aware of only two previous studies that have analysed parental responses to observed cognitive ability beyond birth. Frijters et al. (2013) find that parents reinforce cognitive resources in response to differences in cognitive ability in the USA. Ayalew (2005) also finds reinforcing effects, but these results are based on estimates from only one village in Ethiopia.[1]

Second, we use both sibling fixed-effects and a plausibly exogenous source instrument (rainfall in early life) for variation in cognitive ability to more convincingly identify parental responses, rather than relying on within-twin estimation, since twins are not the ideal group on which to study such effects (Bhalotra and Clarke 2016). Other instruments have been utilised in the literature – Frijters et al. (2013) use handedness as an instrument of a child's ability, the validity of which has been contested (Grätz and Torche 2016). Leight (2017) uses grain yields as a plausible instrument for differences in ability proxied by height-for-age. There is extremely careful literature that has analysed whether parents compensate or reinforce specific (plausibly exogenous) policies and events experienced in childhood (Halla et al. 2014; Adhvaryu and Nyshadham 2016), which is highly informative, but may only be generalisable to larger policy shocks, whereas our use of variation in rainfall could be seen as "normal" shocks to childhood experienced by all children (Maccini and Yang 2009).

Finally, we descriptively examine heterogeneity in parental responses across socio-economic status, in a low-income setting. Such heterogeneity has been examined, but only in country contexts that are more developed than Ethiopia (Cabrera-Hernandez 2016; Hsin 2012; Grätz and Torche 2016; Restrepo 2016). We find that on average, parents provide

1 Other results on health in the study are based on a much larger sample.

more cognitive investment to the lower-ability child to reduce intra-household inequality. The compensatory parental responses are concentrated in relatively higher-socio-economic status families. Families with educated mothers, smaller household size and higher wealth compensate through a higher level of cognitive investment when there are differences in ability, while families with non-educated mothers, larger size and lower wealth exhibit only small and modest compensatory behaviours.

In the next section we briefly review the relevant literature, and in subsequent sections then present our data, including the cognitive ability measures, followed by our econometric approach, our results and robustness checks, and a concluding discussion.

2. Literature review

There are two competing theories on the direction of parental responses to observed ability in their children, both originating from theoretical models which are now more than 40 years old. Becker and Tomes (1976) predict that parents reinforce differences in child ability by investing more in the high-ability child, under the assumption that marginal return to investment is higher when the ability of the child is higher. In this case, parents' concern is for efficiency more than equity. On the contrary, Behrman et al. (1982) suggest that parents will compensate for ability differences to achieve equality among children when parents' inequality aversion preferences outweigh efficiency concerns.

In response, a burgeoning empirical literature has examined the effect of child endowments on parental responses. The results are mixed, indicating overall that there is either no clear direction of parental response on child endowment, or that the response depends heavily on context. Some studies have found evidence of reinforcing parental responses (Aizer and Cunha 2012; Adhvaryu and Nyshadham 2016; Behrman et al. 1994; Datar et al. 2010; Frijters et al. 2013; Grätz and Torche 2016; Hsin 2012; Rosales-Rueda 2014); some have found compensating parental responses (Behrman et al. 1982; Bharadwaj et al. 2018; Cabrera-Hernandez 2016; Del Bono et al. 2012; Frijters et al. 2009; Griliches 1979; Halla et al. 2014; Leight 2017); some have found mixed responses (Ayalew 2005; Hsin 2012; Restrepo 2016; Yi et al. 2015); some have found no effect at all (Abufhele et al. 2017; Almond and Currie 2011).

Many of the recent empirical studies have relied heavily on variation in birth weight to answer the question of parental responses, using a sibling fixed-effects (FE) model (Abufhele et al. 2017; Bharadwaj et al. 2018; Del Bono et al. 2012; Cabrera-Hernandez 2016; Datar et al. 2010; Hsin 2012; Restrepo 2016; Rosales-Rueda 2014). However, some studies argue that birth weight might be associated with prenatal endogenous input, and hence, exploit a source of exogenous variation in the endowment at birth. Halla et al. (2014) study the effect of an exogenous shock on the Austrian 1986 cohort, who experienced prenatal exposure to radioactive fallout from the Chernobyl accident. The shock decreases the birth weight and live births, and increases premature births, days for maternity leave and Apgar score. They find robust empirical evidence that parents compensate the children for experiencing input shocks. Adhvaryu and Nyshadham (2016) exploit variation in a plausible random *in utero* exposure to an iodine supplementation programme in Tanzania, and show that parents choose reinforcing investment in higher-ability children.

Meanwhile, other studies tackle this problem by using within-twins differences as a exogenous source of variation in endowment since prenatal parental investment is impossible to vary (Abufhele et al. 2017; Bharadwaj et al. 2018; Yi et al. 2015; Grätz and Torche 2016). For example, Abufhele et al. (2017) find that parents are neutral to the difference in birth weight of twins in Chile and support the existing evidence that parents do not invest differentially between twins. Using the same data, Bharadwaj et al. (2018) find similar results that parents do not invest differentially within twins, while, using a sample of parents with singleton siblings, compensatory behaviour is found. As Almond and Mazumder (2013) noted, the reason could be that it might be especially costly for parents to implement differential treatment between twins.

Important concerns about using twins as an instrument have been raised. Using individual data in 72 countries, Bhalotra and Clarke (2016) find that the distribution of twins is not random in the population and that indicators of the mother's health and health-related behaviours and exposures are systematically positively associated with the probability of a twin birth. Certainly, twins are not a large proportion of the population, and may be seen more as a special case.

We build on two recent studies that examine the effect of child endowment on parental investment, and rather than relying on twins data, use instrumental variables to alleviate concerns of endogeneity bias resulting from both unobserved household heterogeneity and child-specific heterogeneity. Using sibling differences in handedness as an instrument for cognitive ability differences, Frijters et al. (2013) find reinforcing behaviours of parents who are more likely to allocate more cognitive resources on advantaged child in the USA. Grätz and Torche (2016), however, argue that handedness might vary over time so that it might not be an adequate instrument for child's early ability. Using the same technique but using variation in grain yields during the early life period of siblings as an instrument for physical health, Leight (2017) shows that Chinese parents invest more cognitive resources in the less-healthy child (as proxied by height-for-age) in Gansu province.

We combine a sibling-difference approach with instrumental variables, using the quasi-exogenous rainfall shocks occurring during the critical developmental period of a child as an instrument for differences in child ability between siblings.[2] As studies find that rainfall shocks have a substantial impact on child development in agricultural contexts (see Almond et al. 2018), we exploit differences between siblings by looking at rainfall shocks from *in utero* during the first three years of their life[3] as a source of exogenous variation in nutritional inputs during the critical development period experienced by the siblings.[4] Glewwe et al. (2001: 350) note that a suitable instrument to capture within-sibling differences should be: '(i) of sufficient magnitude and persistence to affect a child's stature; (ii) sufficiently variable across households; and (iii) sufficiently transitory not to affect the sibling's stature'. We provide robustness checks to argue that rainfall shock timing does indeed provide a plausible source of exogenous variation.

2 Rainfall information is external data matched with location by the Young Lives survey since the residence of interviewees is confidential.

3 The period during pregnancy and the first 1,000 days of life is widely recognised as the critical developmental period of child development (Doyle et al. 2009; Victora et al. 2010).

4 Hill and Porter (2017) find that droughts cause a reduction in consumption of households in both rural and urban areas in Ethiopia.

To our knowledge there are two other studies examining the pattern of parental investment in the context of Ethiopia. Ayalew (2005) examines catch-up growth of children in health and cognitive ability, using the first three rounds of the Ethiopia Rural Household Survey from 1994-95. He finds compensating behaviour in health, but reinforcing behaviour in cognitive skills. Arguably, the results for cognitive skills are less persuasive, since they use information on only one village in the survey.[5] Second, using Young Lives Older Cohort data and relying on ordinary least squares (OLS) and FE estimations for identification, Dendir (2014) finds reinforcing behaviours, proxying parental investment with enrolment and child time allocation, and measuring ability using raw Peabody Picture Vocabulary Test (PPVT) scores.[6] Although the fixed-effects estimation successfully deals with the endogeneity issue caused by unobserved household characteristics, there is a potential high degree of correlation between child ability and unobserved child heterogeneity, such as parental preferences over one particular child, which is an individual effect. Dendir (2014) measured PPVT scores at adolescence (age 12 and 15), which increases the probability that this measure of ability is contaminated by unobserved child characteristics and consequently biases the results, and therefore exogenous variation in cognitive ability is necessary for more plausible estimation.

Applying a sibling-FE model combined with an instrumental variable to the Younger Cohort Ethiopia survey, we examine how parental cognitive investment responds to cognitive ability observed in childhood when children are in primary school. Specifically, we examine the effect of cognitive ability on parental cognitive investment: we use PPVT scores as the proxy for cognitive ability measured in childhood, and total educational fees (i.e. school fees plus any private tuition fees) as the cognitive investment when children are in primary school. By exploiting quasi-random rainfall during the critical developmental period as a "normal" shock to child nutritional input, we isolate the exogenous variation in cognitive ability.

While most of the existing literature reveals how parents respond to the difference in health within siblings, to the best of our knowledge, only the two studies discussed above (Ayalew 2005; Frijters et al. 2013) have examined differences in cognitive ability, and both have limitations. As it is of interest to show the specific parental response to one dimension of human capital, one would ideally like to disentangle the effect of investment in that particular dimension of human capital. However, constrained by data, only a few empirical studies have specific measures of investment in different dimensions, while most existing studies use a general measure of parental investment, such as time spent with the child. Yi et al.'s (2015) theory predicts that given the same early health shock, parents response differently along different dimensions of human capital. The data they use contain detailed information on investment in family health and education. The results are mixed: while parents compensate for the harmful effect of an early health shock by devoting more health resources to the worse-health child, they reinforce in the domain of cognition by allocating fewer educational resources to the disadvantaged child. Restrepo (2016) and Rosales-Rueda (2014) use the same dataset from the USA, the National Longitudinal Survey of Youth-Children 1979 (NLSY-C79), which gives information on inputs of time and goods in

5 The outcome measure used is Ravens's Progressive Matrices scores, which did not work successfully in the Ethiopian context during Young Lives (Cueto and Leon 2012) as children were unable to understand the task.

6 We discuss a better measure of ability and parental investment in Section 3.

either cognitive or socio-emotional development. They suggest that parents tend to simultaneously reinforce the effect of low birth weight by providing more cognitive stimulation and emotional support to the low-birth-weight child. In our study, we measure direct cognitive investment using total expenditure on educational fees at the individual child level.

Most existing research attempts to examine parental responses to child endowments on average. Some sociological studies emphasise that in theory, socio-economic heterogeneity should be taken into account, specifically, the degree and direction of parental responses might vary by family socio-economic status (SES) (Lareau 2011; Lynch and Brooks 2013). Some consider that lower-class parents have difficulty affording costly and risky investment in disadvantaged children, and would be more likely to reinforce differences in ability. Higher-class parents tend to be averse to inequity so may compensate for a low-ability outcome (Conley 2008). On the contrary, others suggest that high-SES families may reinforce gaps in child ability by providing more educational investment to the advantaged child, while offering direct transfers, such as gifts or bequests, to the disadvantaged child (Becker and Tomes 1976; Becker 1991).

Only a small number of empirical studies have looked at variation in parental responses by SES, all in a developed country context. Grätz and Torche (2016) find that advantaged parents allocate more cognitive stimulation to higher-ability children, while disadvantaged parents behave indifferently to ability gaps. Yet, Halla et al. (2014) show that families with low socio-economic status chose to give birth to fewer children when their children experienced the Chernobyl accident; similarly, families with high socio-economic status compensate their low-endowed children by supplying less maternal labour (and investing more in childcare). Hsin (2012) uses maternal educational level to measure family socio-economic status. On average, no compensating or reinforcing investment is found for low-birth-weight outcomes. However, low-educated mothers prefer reinforcing investment by spending more time with heavier-birth-weight children at 6 years old, while high-educated mothers compensate low-birth-weight children by spending more time with them. Restrepo (2016) finds reinforcing behaviour on average, with low-SES families reinforcing the differences in birth weight with a greater amount of investment compared to high-SES families. None of these studies provide evidence in the context of developing countries, except Cabrera-Hernandez (2016) who finds that high-educated mothers in Mexico compensate for the low-birth-weight outcome by offering more school expenditure to the disadvantaged child.

3. Data and measures

Young Lives is an international longitudinal study of 12,000 children growing up in four developing countries (Ethiopia, India, Peru and Vietnam) over 15 years (Barnett et al. 2012), examining the causes and consequences of childhood poverty. The main cohort (2,000 children in each country) were born within 12 months of each other in 2001. An Older Cohort (1,000 children in each country) born seven years earlier is used as a comparison group. This paper uses data from four rounds of the Ethiopia survey, focusing on the Younger Cohort and their siblings. Round 1 was conducted in 2002 (when Younger Cohort index children were, on average, 1 year old), Round 2 in 2006 (approximately age 5), Round 3 in 2009 (age 8) and Round 4 in 2013 (age 12). In Rounds 3 and Round 4, one sibling, closest in age to the Younger Cohort index child (either younger or older), was interviewed. This brings variation in that Younger Cohort index children could be either born earlier or later in our analysis.[7]

To reduce heterogeneity in child activities and parental investment, we confine the sample of Younger Cohort index children and their siblings aged from 7 to 14 in Round 4, being old enough to enter primary school and young enough to stay in primary school in Ethiopia. The sample is reduced to 588 sibling pairs (1,176 observations) in the sibling-difference specification, born from 1998 to 2006.

3.1. Rainfall

We use monthly rainfall data at community level provided by Young Lives, which we merge with the survey data using birth year, birth month and birthplace (from Round 1 and Round 2 survey), in order to generate instrumental variables at the child-specific level. Annual rainfall is the sum of rainfall from the 12 months prior to the birth month, so that the rainfall shock varies monthly and yearly. We use standardised rainfall from *in utero* to the first two or three years of life in the birthplace of the child as instrumental variables, following the literature arguing that from *in utero* to the first three years of life is the critical developmental period (Almond et al. 2018). During this period, adequate rainfall contributes to improved income for the household and therefore translates into a positive nutritional input for child ability (Maccini and Yang 2009). The mean and standard deviation are calculated at the birth community level using rainfall from 1985 to 2008. In the context of Ethiopia, an extremely drought-prone agricultural country, we hypothesise that the higher the level of rainfall during the critical developmental period, the better the child ability (Dercon and Porter 2014).

Since the sibling pairs in our sample are born in the same community, the variation in the child-specific instrument variable relies on the time dimension, namely the birth month and birth year. As the sibling pairs in the sample are born between 1998 and 2006, we check the distribution of annual rainfall in each community during 1998 to 2008 (i.e. the second year of life for a child born in 2006). Figure 1 shows that the rainfall in most of the communities is volatile, characterised by two severe droughts in 1999 and 2002. As 90 per cent of the sibling pairs in our sample are born at least two years apart, the correlation of the rainfall instrumenting for each child ability is arguably weak. Furthermore, we carry out a series of *t* tests to examine the difference in rainfall that sibling pairs experience in their early life and find that the annual rainfall during the critical developmental period between index child and sibling is statistically different. Specifically, the index child is exposed to a statistically lower level of rainfall as they are mostly born during 2001 and 2002, when drought hit Ethiopia.

7 There are 496 Younger Cohort index children who are older than their surveyed siblings, and 92 who are younger. The average age difference is 32 months (see Table 1).

Figure 1. *Annual rainfall by community, 1998-2008*

3.2. PPVT scores as a measure of cognitive ability

To analyse the effect of children's cognitive ability on within-household allocation of cognitive resources, our main independent variable of interest is the child's cognitive ability in 2009 (Round 3). The Peabody Picture Vocabulary Test (PPVT) is a receptive vocabulary test designed by Dunn and Dunn (1997), a consistent test measuring cognition ability for both index children and siblings in Young Lives. Therefore we measure the child's cognitive ability using this metric.[8] The PPVT is a widely used test to measure verbal ability and general cognitive development (see Crookston et al. 2013; Paxson and Schady 2007), and the PPVT test score is positively correlated with other common measures of intelligence such as the Wechsler and McCarthy Scales (Campbell 1998). Given that Round 3 is the first round that has information on siblings, our analysis only uses the latter two available rounds of Young Lives data.

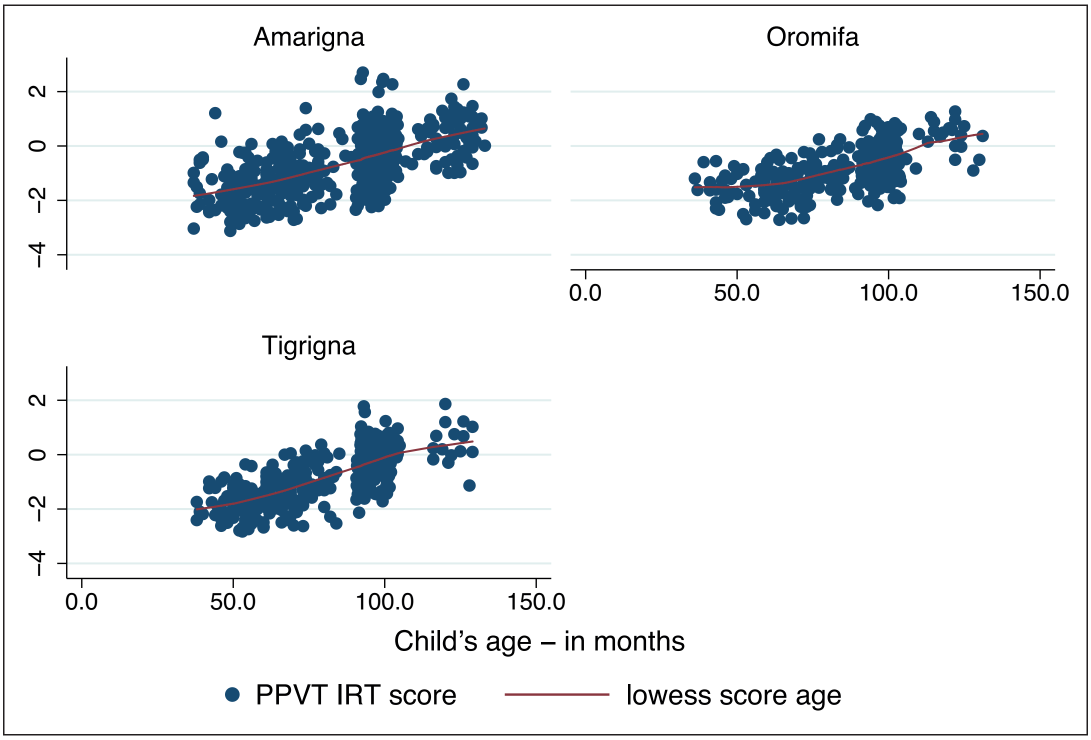
Given the difficulty of using raw PPVT scores across different rounds of data collection, we employ item response theory (IRT) to standardise cognitive measures by language, following Leon and Singh (2017).[9] Figure 2 shows that the IRT PPVT scores increase along with age, yet the means of IRT PPVT scores vary by language, consistent with findings of Leon and Singh (Tigrigna is the highest, followed by Amarigna and Oramifa). To ease the interpretation of subsequent estimation results, the IRT scores have been standardised by

8 In Young Lives there are two other cognitive tests, the Early Grade Reading Assessment (EGRA) and a maths test. However, they are only available for Younger Cohort index children, not for siblings.

9 See Leon and Singh (2017) for further details. We exploit the item parameters for each language calculated by Leon and Singh to generate IRT scores of children in Round 3. We use Stata command openirt programmed by Tristan Zajonc.

language as our measure of cognitive ability, with a mean of 0 and a standard deviation of 1. In the robustness check section, we also include an analysis using PPVT scores standardised by age across language as a comparison.

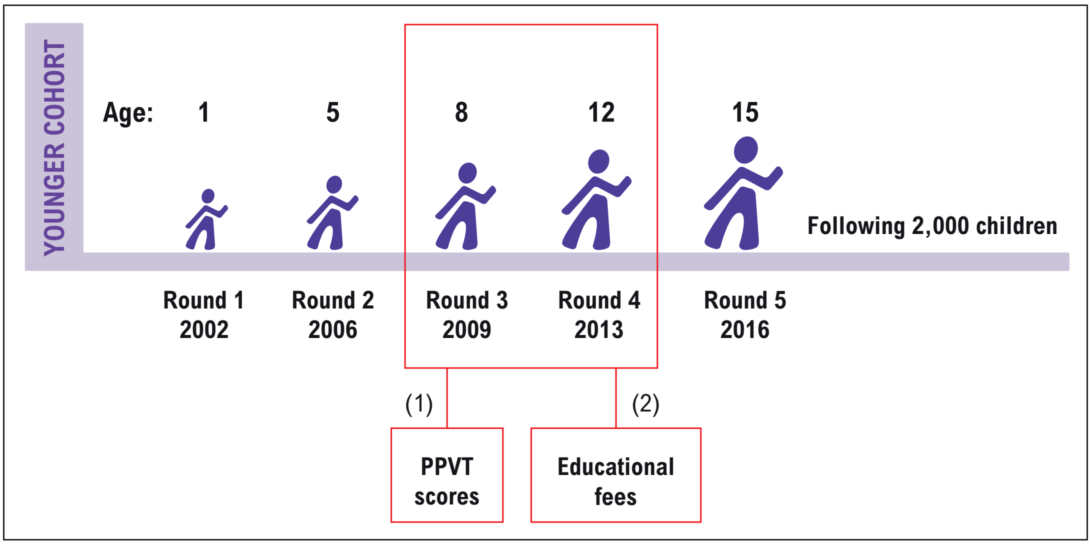
Figure 2. *IRT PPVT scores by languages, 2009*



3.3. Total educational fees as a measure of cognitive resources

Our dependent variable is the allocation of parental cognitive resources, measured by the total education fees paid in 2013 (Round 4) per child. As Figure 3 shows, an advantage of our panel data is that it leaves parents a long period of time (four years between Round 3 and Round 4) to respond after their children is assessed by PPVT in Round 3, while prior research mostly relies on parental involvement measured shortly after their children are assessed. The total educational fees are the sum of school fees and private tuition fees, serving as a direct measurement of cognitive investment.

Figure 3. *Young Lives study in Ethiopia*



To alleviate the concern that public educational investment and private tuition investment are substitute goods, we use Pearson's correlation to test the strength and direction of the association between these two continuous variables.[10] While the Pearson correlation coefficient between the school fees and tuition fees, r = 0.740 at 95 per cent confidence level, suggests that in the pooled sample higher school fees are related to higher tuition fees, the correlation coefficient estimating the association between school fees and tuition fees within-family (r = 0.003) is statistically non-significant at 95 per cent confidence level. This lack of correlation leads us to use total educational fees as the dependent variable of our main analysis.[11]

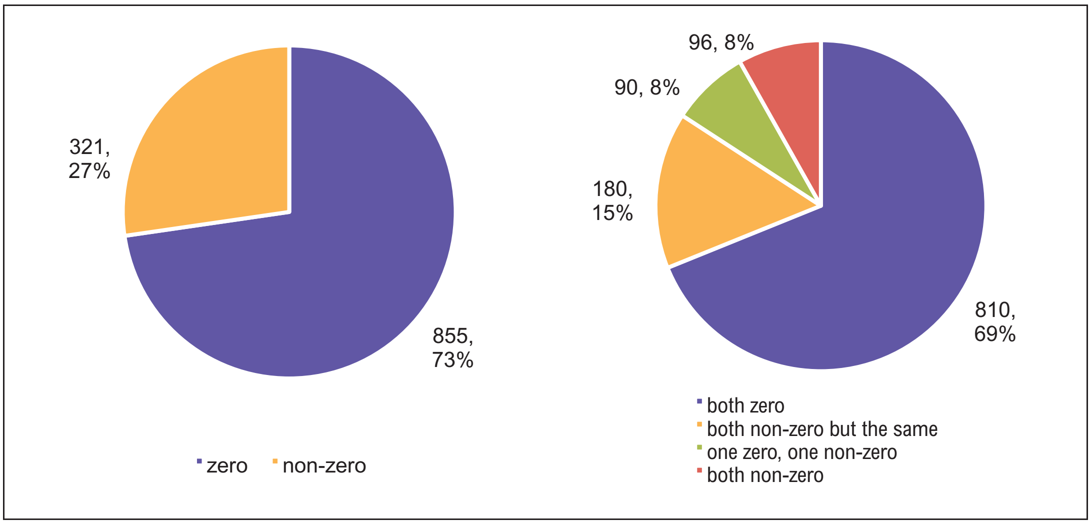
Figure 4 shows how total educational fees are reported. In the pooled sample, shown by the left-hand chart, 73 per cent of parents report zero total educational fees in Ethiopia, while 27 per cent report non-zero educational fees.[12] Looking at the allocation between siblings, indicated by the right-hand chart, 16 per cent of parents differentiate their financial educational resources among their children, while 15 per cent allocate financial resources in child education and adopt no differentiating strategy in investing their children. Our interest is to find out whether the parental investing strategy of those who invest financial resources in their children is responsive to the difference in cognitive ability.

10 We use Stata command pwcorr to carry out the Pearson's correlation test.

11 We provide the analysis using private tuition fees as the dependent variable in the robustness check.

12 The high percentage of zero educational fees exists due to the abolition of school fees in public schools for Grades 1 to 10 in Ethiopia in 1994. However, hidden costs remained (Oumer 2009). UNICEF/World Bank (2009) find that there were still payments in various forms in government schools after the policy of abolishing school fees. According to the Policy and Human Resource Development (PHRD) study, on average, a government school was levying about Birr 10 to 15 per year per student.

Figure 4. *Total educational fees*

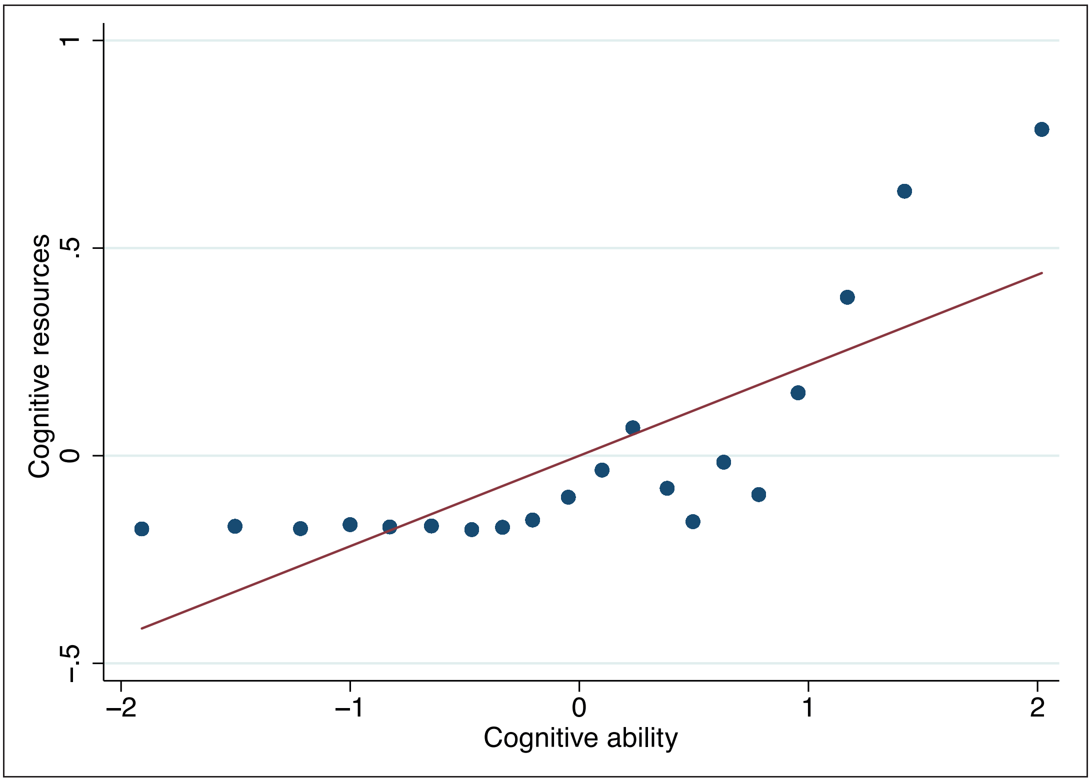


School fees and private tuition fees as a proxy of cognitive resources are specifically documented in parents' answers to the questions such as 'how much you spend on school (private tuition) fees per year?' For the sake of interpretation, we standardise the total educational fees for the analysis.

To understand whether parents report a higher level of investment for the index children, we perform a *t* test on the total educational fees between index children and their siblings. The *t* statistics (= 0.174) shows that the difference in investment between two children is not statistically different, suggesting that parents do not deliberately report a higher investment for the index children.

Given that parental investment is measured by educational fees, it limits the sample to sibling pairs who are both enrolled at school. This gives rise to a concern that differential investments due to differential early ability might have happened before entering school by parents choosing not to enrol low-ability children. Only 76 out of 1,589 index children are not enrolled (4.78 per cent). We carried out a Pearson's correlation to test the relationship between early ability (in Round 3, at age 5) and enrolment in our investment period (Round 4, at age 8). The Pearson correlation coefficient is relatively small, 0.107 at 95 per cent confidence level. Hence, we move on to our analysis, noting this minor concern about the preschool investment of parents being shaped by early ability. Figure 5 shows the raw correlation between mean cognitive ability and mean cognitive resources for each 5 percentile for the included sample. Despite the flat relationship on the left tail of the distribution, the aggregate correlation between ability and parental investment is positive in the cross-section OLS estimation. Our interest is to find out whether this plausible positive relationship continues to hold when we apply our empirical methods accounting for child observable and unobservable factors.

Figure 5. *Mean cognitive resources and cognitive ability for each five percentile of the cognitive ability distribution*



Therefore, we include a series of child observable characteristics as confounding factors. First, to alleviate the concern that cognitive investments are age-related, we control for several age-related factors in the regression analysis. We use age in months, together with square and cube of age in months and dummies of birth year. Then, since evidence suggests that children born earlier receive greater investment (Price 2008; Buckles and Kolka 2014), we control for birth order. Other child-level differences which might contribute to investment variation are also controlled for in the regression analysis, specifically, maternal age at birth, type of school (private or public), and type of siblings (e.g. born as an older brother with a younger sister, or born as an older sister with a younger brother) are taken into account.[13] See Table 1 for summary statistics.

3.4. Socio-economic status (SES)

To understand whether educational investment varies by socio-economic status (SES), we carry out several exploratory *t* tests and find that families investing in education are the high-SES families. The families who make positive investments in child education are significantly richer (t = -10.772), with a significantly better-educated mother (t = -9.311) and smaller household size (t = -3.001). In order to further investigate whether these better-off

13 There are only 38 (3.23 per cent) children in private school. Therefore, we create a dummy variable for attending public school as a control variable, instead of an outcome variable. There are eight factor variables to denote the type of siblings: born as an older brother with a younger sister, born as a younger sister with an older brother, born as an older sister with a younger brother, born as a younger brother with an older sister, born as an older brother with a younger brother, born as younger brother with an older brother, born as an older sister with a younger sister, and born as a younger sister with an older sister. When we use our fixed-effects strategy, many are dropped due to their multicollinear relationship when the information of index children is deducted by their siblings'.

families who invest in education differentiate their investment based on the ability gap between their children, we stratify our analysis on parental responses. Specifically, we employ several household characteristics (maternal education, family wealth, and household size) as indicators of family SES, while we dichotomise each indicator generating a high-SES group and a low-SES group, following Grätz and Torche (2016). With regard to maternal education, half of the mothers in our sample are not educated at all, so we distinguish between families by having an educated mother or a non-educated mother. In the case of family wealth and household size, we dichotomise them using the median of wealth index and size of the family.[14]

Table 1. *Summary statistics*

Variable	Mean	SD	Mean (within)	SD (within)
Cognitive resources				
Total educational fees (standardised)	0.000	1.000	0.013	0.068
Cognitive ability				
PPVT scores (standardised)	0.000	0.999	0.937	0.772
Child characteristics				
Age in months	133.031	20.994	32.320	11.286
Maternal age in months at birth	27.275	6.057	-2.675	0.997
Attend a public school (dv)	0.968	0.177	0.000	0.083
Birth order	3.443	1.862	-0.991	0.149
Born as an older sister with a younger brother (dv)	0.119	0.324	0.238	0.426
Born as an older brother with a younger sister (dv)	0.131	0.337	0.262	0.440
Born as an older brother with a younger brother (dv)	0.125	0.331	0.250	0.433
Born as an older sister with a younger sister (dv)	0.125	0.331	0.250	0.433
Born as a younger brother with an older sister (dv)	0.119	0.324	-0.238	0.426
Born as a younger sister with an older brother (dv)	0.131	0.337	-0.262	0.440
Born as a younger brother with an older brother (dv)	0.125	0.331	-0.250	0.433
Born as a younger sister with an older sister (dv)	0.125	0.331	-0.250	0.433
Test in Oromifa (dv)	0.209	0.407	0.000	0.000
Test in Tigrigna (dv)	0.349	0.477	0.000	0.000
Test in Amarigna (dv)	0.442	0.497	0.000	0.000
Rainfall *in utero* (standardised)	0.059	0.901	0.430	1.275
Rainfall at birth (standardised)	-0.472	1.025	-0.747	1.701
Rainfall in year 1 (standardised)	-0.158	0.807	-0.721	1.173
Rainfall in year 2 (standardised)	-0.072	0.709	-0.517	0.848
Household characteristics				
Wealth	0.366	0.163	0.000	0.000
Mother with education (dv)	0.493	0.500	0.000	0.000
Household size	6.485	1.675	0.000	0.000
N	1176			

Notes: 'dv' denotes dummy variables; "within" stands for the data constructed in 'within-family' structure.

14 The wealth index is the average of housing quality index, consumer durable index and housing service quality index.

Figure 6. *Intra-household difference in total educational fees by SES*

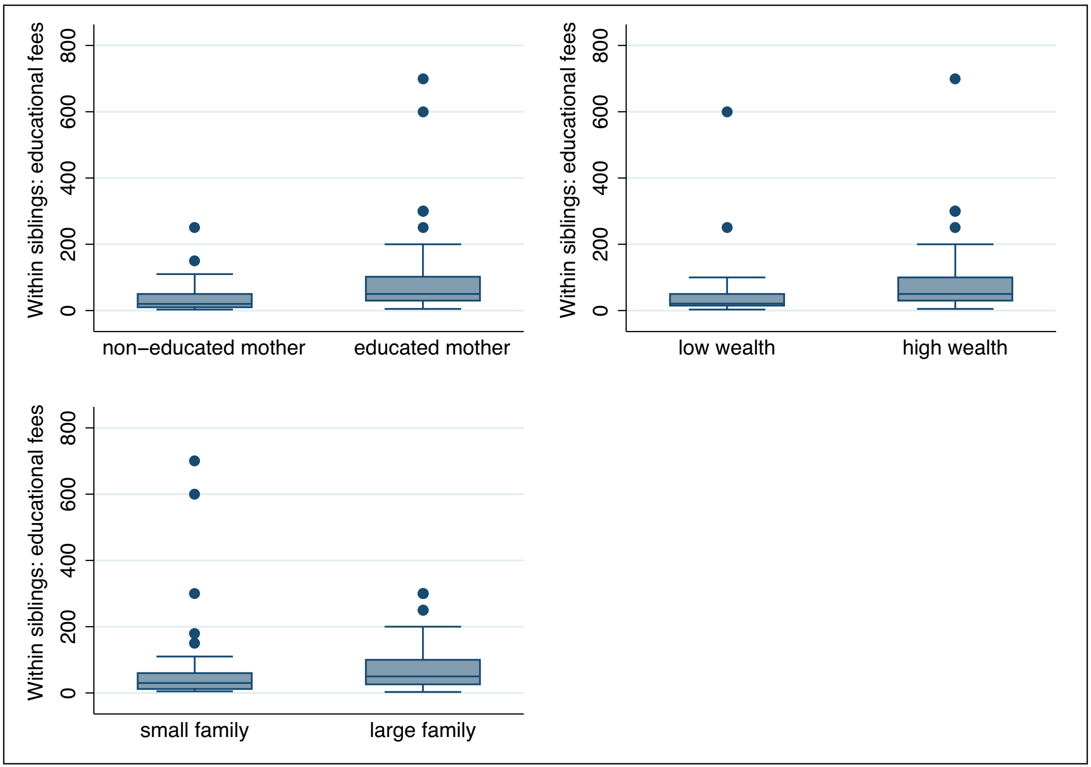


Figure 6 shows the intra-household difference in total educational fees by SES. The distributions of within-sibling difference in educational fees are similar across three indicators. In general, the high-SES families have bigger differences in allocating educational resources among their offspring. The mean of the difference in total educational fees in low-SES families is small but non-zero.

4. Econometric strategy

To identify the causal effect of cognitive ability on parental investment, the analysis is based on IV-FE models, targeting three main endogeneity threats. First, this approach relates within-sibling pair differences in ability in 2009 (Round 3) with within-sibling pair differences in parental cognitive investment four years later in 2013 (Round 4) to address the threat of reverse causality. Second, the sibling FE models control for unobserved heterogeneity at the household level, following most existing empirical work. Third, we use instrumental variables to isolate the exogenous variation in child ability, addressing endogeneity resulting from unobserved child heterogeneity.

The sibling fixed-effects structural model can be written as:

$$\Delta I_h = \beta \Delta CA_h + \Delta X_h \lambda + \Delta s_h \qquad (1)$$

where ΔI_h is the difference in cognitive investment between siblings in family *h* in Round 4 (i.e. total educational fees), ΔCA_h is the difference in ability between siblings in Round 3, ΔX_h is a vector of differences in other characteristics between siblings (e.g. child's age, maternal age at birth, child's birth order, birth year, type of school, type of sibling pairs – gender of older and younger child), and Δs_h is the difference of the idiosyncratic error term between siblings. In this estimation, household observable characteristics and household unobservable confounding factors are purged from the specification.

As noted above, we overcome endogeneity bias resulting from unobserved child heterogeneity with an instrumental variables (IV) estimation procedure. The first stage equation is:

$$\Delta CA_h = \sigma \Delta R_h + \alpha \Delta X_h + \Delta \mu_h \qquad (2)$$

where ΔR_h is the difference in rainfall shock from *in utero* to the first three years of child's life between siblings as a source of exogenous variation in nutritional inputs experienced by the siblings, and $\Delta \mu_h$ is a random error term in the first stage.

IV approach is also helpful in overcoming attenuation bias related to measurement error in cognitive ability. Even if we consider the PPVT test score a good proxy for ability observed by parents, there is still likely to be measurement error in the test, and in its relation to parental perception of ability. For example, parents may have some other perception of their children's cognitive ability than the PPVT score. This potential problem of measurement error can be solved by our IV approach if the error is classical. Indeed, in a sibling FE model, attenuation bias caused by measurement error is augmented if one's analysis moves from a cross-sectional setting to a FE setting (Bound and Solon 1999).

The sibling FE model coupled with the IV strategy helps us to interpret β as the change in parental cognitive investment caused by the variation in child cognitive ability, which is driven by the exogenous variation in rainfall during the critical developmental period of the two children. If $\beta > 0$, parental investment increases with ability. Parents reinforce the differences in ability by allocating more resources to the high-ability child. If $\beta < 0$, it means parents compensate for the difference in ability, allocating more resources to the low-ability child.

Under the assumption of higher marginal returns to investment in higher-ability children, the case of $\beta > 0$ also implies that parents are concerned more with the efficiency of investment and try to maximise their children's total future wealth. The case of $\beta < 0$, on the other hand, implies that when equity outweighs efficiency, parents forgo maximising returns from educational investment, trying to achieve higher equity among children.

We report two types of standard errors, one robust to general heteroskadasticity and the other robust to within community dependence.[15]

For each specification we use the sample of children who have a surveyed sibling and where the information for both siblings is available. Furthermore, we have restricted the sibling pairs to be currently in primary school, use the same language in PPVT test and be born in the same birthplace. A set of child-level covariates are included in all models, such as age in months, maternal age at birth, type of school, dummies of birth order, type of sibling pairs and dummies of birth year.

15 There are 46 clusters in the sample.

5. Results

We present results that test the relationship between cognitive ability and deployed cognitive resources, regressing cognitive resources in primary school on cognitive ability observed one period earlier. The sign of the estimates is positive if parents demonstrate reinforcing behaviours, and negative if parents compensate for ability differences among their children. In all the estimation results, total educational fees paid for each child is the proxy for cognitive resources, while PPVT scores are the proxy for cognitive ability.

5.1. Preliminary results

Table 2 presents the preliminary results from the OLS models and FE model. The inconsistency of the estimates from these models is evident, the magnitudes and signs of which are not stable as we add additional controls, suggesting severe endogeneity of the variable of interest. For example, the cross-sectional OLS estimate reported in column 1, when only child-level controls are included in the model, suggests a positive relationship between ability and total educational fees.[16] However, when we include household-level traits and maternal educational background in the model, the point estimate decreases from 0.048 to 0.019, while the standard error of the estimate remains around 0.025. Furthermore, when we include region fixed-effects in the model, the sign of the estimate, shown in column 4, turns negative, indicating severe endogeneity resulting from the correlation between ability and region, which is closely associated with investment in education.[17]

However, the OLS estimate is still likely to be biased due to unobserved characteristics within the family, such as genetically innate ability, parental preferences for child quality, and budget constraints. Hence, we exploit the sibling-FE model, using a similar strategy to Bharadwaj et al. (2018), Datar et al. (2010) and Hsin (2012), studying parental responses to birth weight, controlling for both observed and unobserved household-level characteristics. In column 5 of Table 2, similar to the OLS estimates in column 4, the FE estimate suggests a negative association between ability and investment, although the size is as small as zero. This result is similar to Frijters et al. (2013), whose FE estimate is much smaller than their OLS estimate. This might be due to increased attenuation bias, when the measurement error of ability is amplified when one removes the cross-section information via FE estimation. Aside from this, endogeneity bias might still persist since cognitive ability is postnatal and time-varying, which allows after-birth ability to embody a significant component of prior parental investment. If serial correlation in parental behaviour exists, the high-ability child who benefits from high prior parental investment tends to receive higher parental investment in the next stage of life.

To address the bias, we isolate the exogenous variation in cognitive ability using quasi-exogenous variation in rainfall during the critical developmental period. Thus we apply instrumental variable methods to the sibling fixed-effects approach (IV-FE), similar to Frijters et al. (2013) and Leight (2017), who use the same strategy but different instruments to ours.

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16 In the OLS model, the language of tests is also included as a control variable. In the FE model this is purged since the siblings are tested in the same language.

17 This is likely to be caused by regional difference in economic, social, educational and political background, rather than language, as language of tests is controlled in the model and our PPVT scores have been transformed via IRT technique adjusting the difficulties across languages, and standardised by language.

5.2. Main results

5.2.1. IV-FE models: first-stage results and diagnostics

Table 3 presents the first-stage IV-FE results, as well as the underidentification and weak identification tests. In the first-stage estimations endogenous cognitive ability is regressed on the exogenous regressors and excluded instruments (i.e. rainfall during the critical developmental period). We find that children who experienced relatively good rainfall aged 0-24 months have significantly higher test scores than their siblings in their early childhood; rainfall during infancy is *relevant* to cognitive ability as proxied by receptive vocabulary.

As seen in columns 1 to 4 in Table 3, we regress ability in childhood on annual rainfall from *in utero* to the first three years of child life. We find that only the annual rainfall during 0 to 12 months of life and 13 to 24 months are significant. Therefore, we constructed an IV using the average rainfall during 0 to 24 months of life and report the result in column 5. The estimate is positive and statistically significant, with a t statistic of 5.48, suggesting that an increase of one standard deviation in rainfall during the first two years of life is correlated with an increase of 15.3 per cent of one standard deviation in cognitive ability in early childhood. In column 6, when we include both the rainfall during the first year and the second year of life as IVs in the IV-FE model, both of the estimates are positive and statistically significant.

With regards to the underidentification tests, the p-values for the specifications 2, 3, 5, and 6 all reject the hypothesis that the IV models are underidentified, though not specifications 1 and 4, suggesting that the IV models are likely to be underidentified using either rainfall *in utero* (column 1) or rainfall in the third year of life (column 4) as the excluded instrument variable.[18] Therefore, in the following, we focus on four IV-FE models: three are single IV models using rainfall from 0 to 12 months, rainfall from 13 to 24 months, and average rainfall from 0 to 24 months; the final one is a two-IV model using both rainfall from 0 to 12 months and from 13 to 24 months.

We further examine the validity of the IVs by conducting a battery of weak identification tests. Noting that the traditional Cragg-Donald weak instrument test applies to the case of i.i.d. data only, we report a robust weak instrument test by Olea and Pflueger (2013) which gives valid test statistics – Montiel-Plueger (M-P) effective F statistics and Montiel-Plueger critical values – in the existence of heteroskedasticity at 95 per cent confidence level.[19] Although the robust M-P effective F statistics in the specifications 2 and 3, which are 22.838 and 21.606, satisfy the 'rule of thumb' recommended by Staiger et al. (1997), when comparing them with the robust critical values given by the M-P test, there is a 5 per cent chance that the bias in the IV estimator is 20 per cent of the worst case possible.[20] When we use average rainfall between the age of 0 to 24 months as the excluded instrument, the robust weak instrument test suggests that this IV is reasonably strong. In column 5 of Table

18 The underidentification test is an LM version of the Kleibergen and Paap (2006), which allows for non-i.i.d. errors.

19 We use weakivtest programmed in Stata by Pflueger and Wang (2015). The Montiel-Plueger effective F statistics are very close to the built-in Kleibergen-Paap rk Wald F statistics in the programme xtivreg2 written by Schaffer et al. (2015). However, the robust critical values of the latter are not provided. Thus, we use the Montiel-Plueger critical values as thresholds in order to report the bias.

20 This result is consistent with Kovandzic et al. (2016), who show that a F statistic satisfying the 'rule of thumb' (larger than 10) could not guarantee a valid instrument in the case of non-i.i.d. data.

3, the robust M-P effective F statistic is 30.063, which is above the robust M-P critical value for a maximum IV bias of 10 per cent. Additionally, the combined set of instruments in column 6 are also stronger than those in columns 2 and 3, as its M-P F statistic is 15.267 which is higher than the critical value for a maximum IV bias of 5 per cent.

The single instrument of average rainfall at the age from 0 to 24 months and combined set of instruments of rainfall at the age from 0 to 24 months are both *relevant*, implying the second-stage inferences will be valid and point estimates are only likely to include a relative bias lower than 10 per cent at 95 per cent confidence level. These results could also serve as a supplement to studies investigating whether some periods during the critical developmental period are more important. We find that children are particularly vulnerable at the age of 0 to 24 months in developing cognitive ability in Ethiopia, which is consistent with the findings of Maccini and Yang (2009), though Dercon and Porter (2014) find children exposed to famine at the age of 12 to 36 months are shorter than their peers in Ethiopia. In Table A2, we show the IV redundancy test of a specified IV, which supports the hypothesis that rainfall at the age of 0 to 24 months is not redundant.

5.2.2. *IV-FE models: second-stage results*

Our main estimation results are presented in Table 4, showing the second-stage estimations using four IV models selected above. Across the four IV-FE models, the point estimates are significantly negative, suggesting a compensating behaviour when parents observe their child to be underdeveloped.[21] Particularly, remembering that the preferred IVs used in specifications 5 and 6 in Table 3, whose corresponding results are shown in columns 3 and 4 in Table 4, are relatively more *relevant*, the point estimates of these two specifications are very close (-0.063 and -0.064). This suggests that an increase in cognitive ability of one standard deviation decreases cognitive resources by 6.3 to 6.4 per cent of a standard deviation.[22]

The confidence intervals given by a set of weak identification tests are negative.[23] The Anderson-Rubin test (AR) gives negative confidence sets of estimated β that are robust to potential bias introduced by weak instruments, and negative confidence intervals at 90 per cent confidence level. The AR confidence intervals given by specification 3 are the narrowest, which is [-0.112, -0.025] at 90 per cent confidence level. In the two-IV model, all of the AR, Moreira CLR, K, and K-J tests give negative confidence sets. The J test rejection probability is low everywhere except perhaps for very high values of β, suggesting that the instruments are exogenous. Figure 7 shows the graphs of these weak IV tests. Both the point estimates and confidence intervals given by the weak identification tests suggest that parents compensate for low-ability children.

To allay the concern of our proposed IV being possibly not perfectly exogenous, we further exploit a newly developed estimator by Conley et al. (2012), which is robust even if the IV is *imperfect*, that is, the excluded instrument is directly correlated with the dependent variable.[24] Specifically, one might worry that rainfall in infancy might have a direct and

21 The IV-FE point estimates are given by xtivreg2 programmed by Schaffer et al. (2015).

22 The mean of the total educational fees is Birr 142.61.

23 The AR, Moreira CLR, K, J and K-J confidence intervals are given by weakiv, programmed by Finlay et al. (2016).

24 We use plausexog programmed in Stata by Clarke et al. (2017).

positive impact on parental investment, although we argue that the rainfall shock is idiosyncratic and its impact on household income is only contemporaneous and short-lived (Glewwe et al. 2001). To generate a robust estimate under this prior assumption, we allow departures from the assumption of strict exogeneity of rainfall so that rainfall could have a positive and direct impact on parental investment. However, the confidence set of estimated β, [-0.261, -0.021] 90 per cent confidence interval, still does not contain zero and is negative, under the assumption of a direct positive impact of rainfall in infancy on parental investment in childhood. In the robustness check shown in row 7 of Table 6, we also provide results on whether the idiosyncratic rainfall during the period when children are between 0 and 2 years old has a direct impact on consumption in the future at household level. We discuss this further in the following section.

A related threat to the exclusion restriction would arise if rainfall in one sibling's infancy affects the other's ability (earlier or later than the critical periods in question). Therefore, we regress ability on rainfall exposure of both own and sibling rainfall shock in infancy, while replacing the household fixed-effects with county fixed-effects, since estimating coefficients on own and sibling rainfall exposure would not be possible in a family fixed-effects model. Table A1 shows that the estimates of rainfall during infancy of the sibling are insignificant and the magnitude is as small as a tenth of the one of our interest variable (in absolute value). The coefficient of child's "own" rainfall during the first two years of life remains significant and large in magnitude after including the sibling rainfall. We compare our results with others using the IV-FE approach to examine parental responses. We noted some limitations of Frijters et al. (2013) handedness instrument earlier. In addition, the traditional Cragg-Donald F statistic of 12.32 under the assumption of an i.i.d. error, only satisfies the rule of thumb marginally, and arguably fails to provide strong evidence that handedness is a valid instrument to identify child's ability. On the other hand, Leight's (2017) grain yield instrument is robust to the existence of weak instrument using *p* value from an AR test.

Table 2. *Preliminary regression models*

Dependent variable: Total educational fees	OLS (1)	OLS (2)	OLS (3)	OLS (4)	FE (5)
Cognitive ability	0.048*	0.040	0.019	-0.011	-0.000
	(0.026)	(0.028)	(0.025)	(0.025)	(0.004)
Observations	1176	1176	1176	1176	1176
Child-level controls	Yes	Yes	Yes	Yes	Yes
Household-level controls	-	Yes	Yes	Yes	Yes
Mother-level controls	-	-	Yes	Yes	Yes
Region fixed-effects	-	-	-	Yes	Yes
Sibling fixed-effects	-	-	-	-	Yes

Notes: Robust standard errors are in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is total educational fees. Children controls are age in months, square of age in months, cubic of age in months, maternal age at birth, the type of school, birth order, birth year, language of tests, and the type of sibling (such as born as an older sister and paired with a younger brother). Household-level controls are household size, wealth index, and gender of household head. Mother-level controls are a series of levels of maternal education.

More generally, our finding of a strong negative relationship between cognitive ability and cognitive resources is consistent with a number of studies finding that parents prefer inequality aversion (Behrman 1988; Bharadwaj et al. 2018; Rosenzweig and Wolpin 1988; Del Bono et al. 2012; Frijters et al. 2009; Halla et al. 2014; Leight 2017; Yi et al. 2015).

Table 3. *First-stage regressions: results and tests of underidentification and weak identification*

Cognitive ability	**(1)**	**(2)**	**(3)**	**(4)**	**(5)**	**(6)**
Rainfall *in utero*	-0.044 (0.029) [0.029]					
Rainfall at birth		0.100 (0.021)*** (0.026)***				0.071 (0.024)*** (0.029)**
Rainfall in year 1			0.136 (0.029)*** [0.044]***			0.086 (0.033)** [0.051]*
Rainfall in year 2				0.039 (0.037) [0.028]		
Average rain at birth and year 1					0.153 (0.028)*** [0.035]***	
Underidentiftcation test:	$\chi(1)^2$ = 2.435	$\chi(1)^2$ = 21.968	$\chi(1)^2$ = 21.284	$\chi(1)^2$ = 1.136	$\chi(1)^2$ = 27.844	$\chi(2)^2$ = 28.102
p value	0.119	0.000	0.000	0.286	0.000	0.000
Weak instrument test:						
Montiel-Pflueger (MP) effective F stat	2.353	22.838	21.606	1.097	30.063	15.267
Montiel-Pflueger critical values:						
5% of worst case bias	37.418	37.418	37.418	37.418	37.418	5.862
10% of worst case bias	23.109	23.109	23.109	23.109	23.109	4.580
20% of worst case bias	15.062	15.062	15.062	15.062	15.062	3.844
Observations	1176	1176	1176	1176	1176	1176

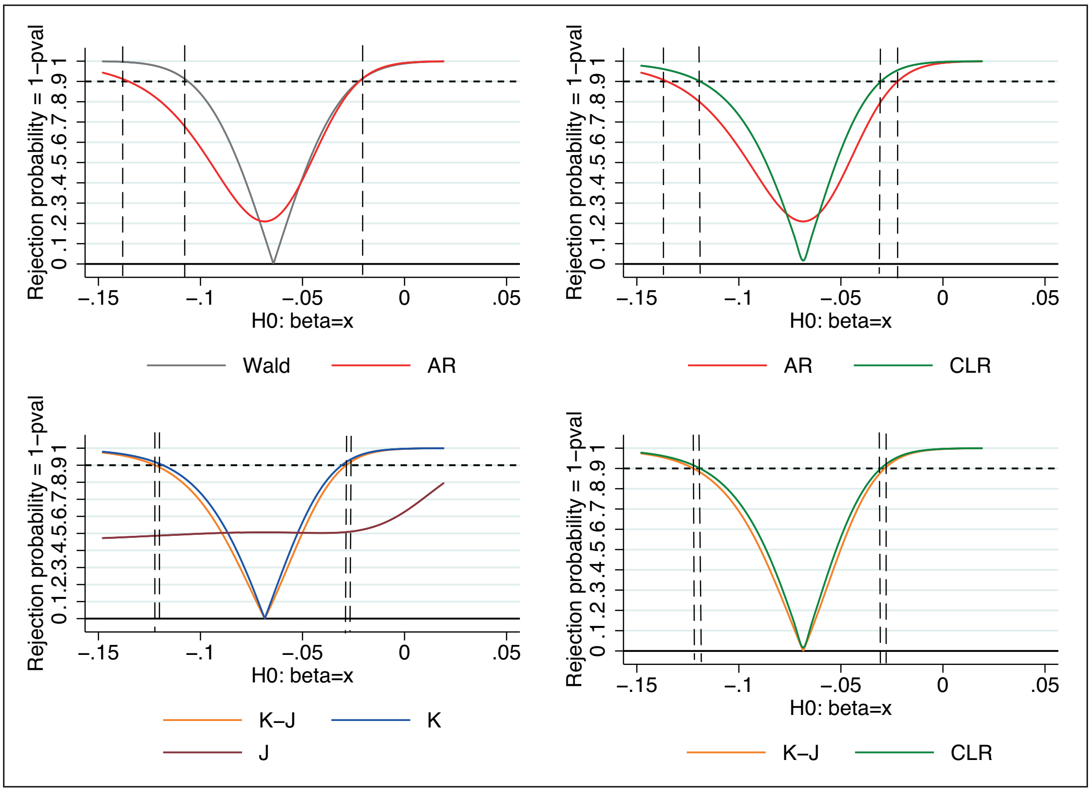
Notes: Within-household fixed effects estimates. Robust standard errors in parentheses. Clustered standard errors by community in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Child controls include age in months, square of age in months, cubic of age in months, maternal age at birth, the type of school, birth order, birth year, and the type of sibling (such as born as an older sister and paired with a younger brother). Both the underidentification test and weak instrument test are robust to heteroskedasticity. The Montiel-Pflueger (MP) F statistics are very similar to Kleibergen-Paap rk Wald F statistics in weak instrument test. The MP weak instrument test offers valid critical values at 95 per cent confidence level and test statistics in the absence of assumption of i.i.d. data.

Table 4. *IV-FE regression models of cognitive ability and total educational fees*

Dependent variable: Total educational fees	IV-FE			
	Instruments: Rainfall at birth	Instruments: Rainfall in year 1	Instruments: Average rainfall in the first two years of life	Instruments: Rainfall at birth and rainfall in year 1
	(1)	(2)	(3)	(4)
Cognitive ability	-0.057	-0.072	-0.063	-0.064
	(0.028)**	(0.027)***	(0.025)**	(0.025)**
	[0.032]*	[0.035]**	[0.031]**	[0.031]**
Anderson-Rubin (AR) test	[-0.113,-0.015]	[-0.130,-0.033]	[-0.112,-0.025]	[-0.134, -0.022]
Moreira CLR test	-	-	-	[-0.118, -0.030]
K test	-	-	-	[-0.117, -0.031]
J test	-	-	-	entire grid
K-J test	-	-	-	[-0.121, -0.029]
Observations	1176	1176	1176	1176
Nr. excluded instruments	1	1	1	2
Child-level controls	Yes	Yes	Yes	Yes
Siblings fixed-effects	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses, community clustered standard errors in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is standardised total educational fees. Child controls are age in months, square of age in months, cubic of age in months, maternal age at birth, the type of school, birth order, birth year, and the type of sibling. The AR test, CLR test, K test, J test and K-J test are all robust to heteroskedasticity. All the tests give confidence intervals at 90 per cent confidence level. The AR test and K-J test are a joint test of the structural parameter β and the exogeneity of the instruments. It corrects size in cases where instruments are weak. Moreira CLR test is a more powerful test when the model is over-identified and weak exogeneity of excluded instrument is satisfied. The J test is like the Hansen J test of weak exogeneity, giving a confidence set where all values of β that are consistent with the assumption of weak exogeneity of instrument variables.

Figure 7. *Weak identification robust inference*



5.3. Heterogeneity of parental responses to children's early ability

After studying the parental response at an aggregate level, we now explore heterogeneities in responses by stratifying the sample by maternal education, household size and wealth, following the existing literature on this topic for more developed countries than Ethiopia (Cabrera-Hernandez 2016; Hsin 2012; Grätz and Torche 2016; Restrepo 2016).

Splitting the sample according to endogenous characteristics is not an ideal solution, and we therefore interpret our results with some caution. However, Christian and Barrett (2017) recently provided convincing evidence that the obvious alternative – interaction of exogenous IV with endogenous covariate – is also not a viable solution. This method has a high likelihood of providing spurious results due to endogeneity concerns. In particular, they question the assumption (using the example of food aid and conflict) that conditional on the controls, the error term is independent of the interaction of the instrument and the exposure variable, and show that this holds only if either the exposure variable is uncorrelated with the error term or the correlation is constant across time or space. We expect especially that the latter point may not be true for our sample.

Table 5. *IV-FE model of the effects of cognitive ability on total educational fees: potential heterogeneity effect by SES*

		Maternal education		Household size		Family wealth	
	All	No	Yes	Large	Small	Low	High
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Cognitive ability	-0.063**	-0.019**	-0.097**	-0.086	-0.067**	-0.024**	-0.110**
	(0.025)	(0.008)	(0.047)	(0.052)	(0.030)	(0.012)	(0.053)
Weak instrument test:							
MP effective F stat	30.063	14.975	15.135	11.800	12.929	12.516	17.356
Observations	1176	596	580	534	642	610	566

Notes: Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The same model is used as the main model. The IV used is the average rainfall in the first two years of life.

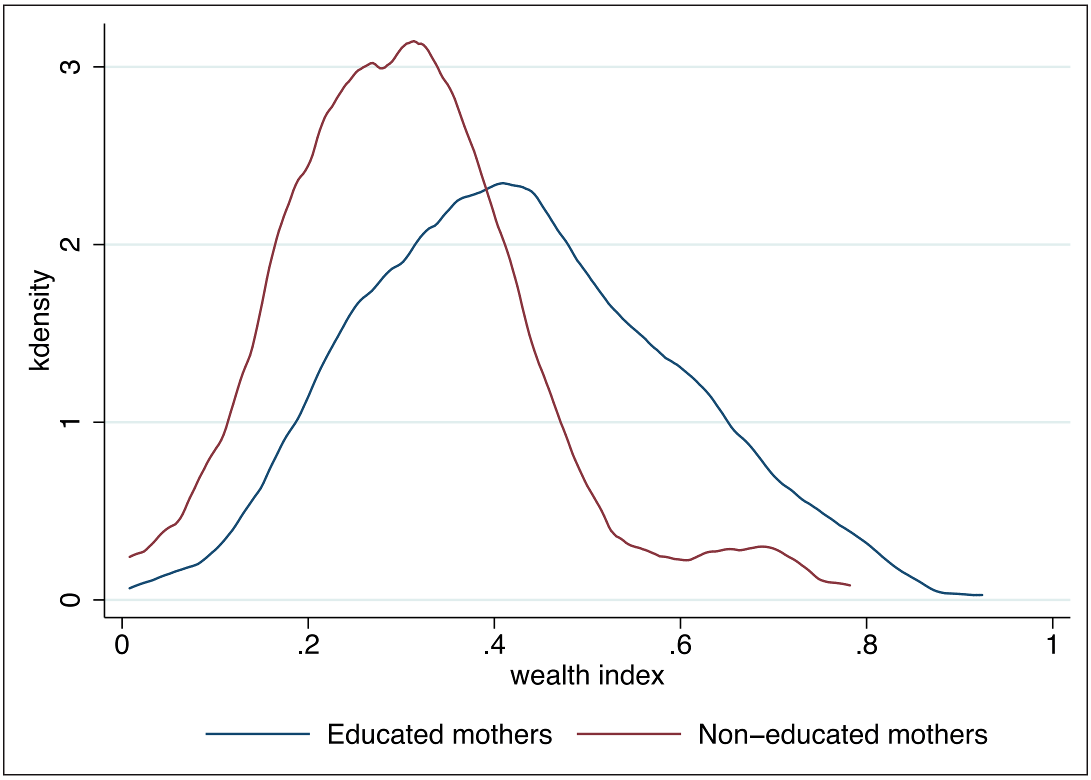
Table 5 shows that the association between early ability and later cognitive resources varies by family socio-economic standing (models 2-7). Specifically, high-SES parents provide more cognitive stimulation to their low-ability child, whereas low-SES parents compensate less cognitive investment in ability between their children. This heterogeneous variation in parental responses across SES is consistent using three indicators of socio-economic standing (maternal education, household size and family wealth). We therefore split the sample across the household characteristics, at the median for household size and family wealth, and by mother's education (binary – any or none). We should emphasise that the Young Lives sample are already a 'pro-poor' sample from communities that are relatively poor, in a country that is poor by global standards (Outes-Leon and Sanchez 2008).

Table 5 shows that among better-off parents (educated mothers, small household or high family wealth), a one standard deviation increase in ability leads to 6.7 per cent to 11.0 per cent of a standard deviation decrease in total educational fees, while worse-off parents only compensate 1.9 per cent to 2.4 per cent of a standard deviation more educational investment to the low-ability child. Using a sibling FE model, Hsin (2012) and Restrepo (2016) suggest a compensating effect among high-educated mothers by providing more time and more cognitive and emotional stimulations to the low-birth-weight children in the

USA. Hsin (2012) finds that the compensating effects observed among highly educated mothers are substantially larger than the reinforcing effects among the least-educated mothers. The result is also consistent with Cabrera-Hernandez (2016), who uses a sibling FE model and finds that higher-educated mothers compensate expenditure in school for the low-birth-weight outcome in Mexico. However, Grätz and Torche (2016) use a twin FE model and find that advantaged families provide more cognitive stimulation to higher-ability children, and lower-class parents do not respond to ability differences in the USA.

An unanswered question based on existing findings is whether the heterogeneous result by maternal education is caused by the difference in wealth, in differential preferences for compensation, or ability to observe a difference in the cognitive outcomes of siblings. Figure 8 shows that on average, educated mothers are generally better off in terms of wealth, implying that educated mothers might have a higher capacity to compensate disadvantaged children, simply because they have sufficient financial resources. To examine this, we regress total educational fees on height-for-age z-scores (HAZ). These are normally used for measuring child health, but are also arguably easier to observe, to show whether the results are consistent. Surprisingly, as shown by model 6 in Table A5, non-educated mothers demonstrate a relatively strong compensating behaviour, with an estimate of 0.024 at 95 per cent confidence level, while the coefficient of the model for educated mothers is smaller but statistically non-significant. This result, combining the result of column 2 in Table 5, is tentative evidence that uneducated mothers are able to better observe height differential than cognitive differential, and they could attempt to compensate once they recognise their children are worse off, despite their lower wealth level.

Figure 8. *Kernel density plot of household wealth index by maternal education, 2013*



5.4. Robustness checks

We now present some additional robustness tests. First, we restrict the sibling-pairs to have an age gap larger than two years, that is, the older sibling should be born at least three years earlier than the younger, in order to avoid a direct relationship between the rainfall shock experienced by one and outcome of the other. For example, one could argue that if one child is born one year after the older sibling, the rainfall experienced by the older one in the second year of life would be the rainfall the next child experiences in the first year of life; also, when the newborn child is exposed to an adverse shock at birth, parents might reallocate resources immediately among the children, which would directly influence the nutritional input of the older child in the second year of life. The restricted sample has 678 observations. In row 1 of Table 6, the first-stage coefficient of rainfall in the first two years of life equals 0.184 (t = 5.56), which is only slightly larger than that of the full sample presented in Table 3. The full diagnostics of the first stage using restricted sample is shown in Table A3, which is consistent with the results of the full sample. The second-stage estimate equals -0.043 (z = 2.77), larger than that in the full sample, which equals -0.063. However, it is consistent with the previous result that parents compensate for disadvantaged children by offering them higher educational resources.

Table 6. *Robustness regression models*

Model variations	Obs.	First-stage: Effect of average rainfall from 0-24 months on ability	Second-stage: Effect of ability on resources
(1) Restricted sample	678	0.184***	-0.043***
		(0.033)	(0.016)
			[-0.074,-0.020]
(2) Only control for age	1176	0.123***	-0.078**
		(0.026)	(0.032)
			[-0.144,-0.033]
(3) Cognitive ability standardised by age	1176	0.208***	-0.047**
		(0.035)	(0.019)
			[-0.083,-0.019]
(4) Private tuition fees	1176	0.153***	-0.106***
		(0.028)	(0.039)
			[-0.184,-0.047]
(5) Having private tuition (dv)	1176	0.153***	-0.279***
		(0.028)	(0.082)
			[-0.446,-0.161]
(6) Study hours	1176	0.153***	-0.415
		(0.028)	(0.267)
			[-0.921, 0.001]
(7) Add factor variables of grade as controls	1176	0.085***	-0.142**
		(0.030)	(0.065)
			[-0.351,-0.066]
(8) Rainfall on consumer index	1176	0.003	
		(0.009)	

Notes: Models 1, 2, 3, 4, 5, 6 and 7 use the same IV-FE model as that for the main result in Table 4, instrumenting the ability using average rainfall during the first two years of life, while model 8 uses cross-section OLS estimation. Robust standard errors in parentheses. The weak IV robust AR confidence intervals are in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Row 1 uses a sub-sample which contains the sibling-pairs which have an age gap larger than two years. Row 2 removes all the covariates displayed in Table 4 apart from age. Row 3 uses cognitive ability standardised by age as independent variable. Row 4 uses private tuition fees as the dependent variable. Row 5 uses whether the child receives private tuition as the outcome variable. Row 6 exploits child study hours as the outcome variable. Row 7 includes a series of factor variables of grade as controls. Row 8 is the OLS estimation regressing consumer durable index on the average rainfall from 0 to 24 months, with the same control variables as the main regressions.

In row 2 we re-estimate our model without the covariates (maternal age, type of school, birth order, birth year and type of sibling), only controlling for age: the IV-FE estimate equals -0.078 (z = 2.46). This result provides extra support for our assumption that rainfall is exogenously determined because it shows that our estimate is not conditional on the set of control variables included in the model. Row 3 shows the robustness of our results to our definition of cognitive ability by replacing it with cognitive ability standardised by age cohort: the IV-FE estimate equals -0.047 (z = 2.49).[25] Row 4 shows results using only private tuition fees as the dependent variable and finds consistent results. When parents observe an increase of one standard deviation in ability, they reduce private tuition fees by 10.6 per cent of a standard deviation. Next, we investigate whether the likelihood to undertake private tuition is contingent upon cognitive ability, using a dummy variable of undertaking private tuition as the dependent variable. Shown in Row 5, the result suggests a compensating behaviour of parents: the probability of offering private tuition to a child will increase by 27.9 per cent if the child is underdeveloped by one standard deviation in cognitive ability. We also exploit study hours as a potential measure for educational investment. Row 6 shows that the coefficient is negative, though insignificant (z = -1.56), while the weak IV AR confidence interval is [-0.921, 0.001]. This implies a plausible consistent direction of parental responses in terms of child time use. In the main model, we do not include grade levels as control variables in order to avoid 'bad controls', since grade level is highly likely to be correlated with the cognitive ability. However, recognising that school fees vary by grade level (UNICEF/World Bank 2009), we provide a suggestive result from a model which adds a series of factor variables of the grade level.[26] Row 7 shows that the coefficient of the interest variable remains negative and significant, though more imprecise.

Finally, in row 8, we regress the household consumer durables index in Round 4 on rainfall in early life and find no significant effect (t=0.31), implying that rainfall shocks in early life do not have persistent impact on consumption patterns of the household in the later life of children. It supports our assumption that rainfall in early life does not affect parental investment in later life through a direct mechanism.

6. Conclusion

We find that for a sample of poor Ethiopian households, on average parental investment compensates weakly for a low-ability outcome. We use an instrumental variable approach combined with panel data and a sibling fixed-effects model to provide robust evidence. This is of policy relevance since the results suggest that the detrimental effects of early life shocks might be mediated or muted by parental responses and hence the biological effects of early nutritional shocks might be larger than policymakers observe. In addition, it complements the literature on reduced-form estimates of the total effect of an early life shock or adverse event on final adult health in Ethiopia (e.g. Dercon and Porter 2014).

25 See Table A4 for the full analysis.

26 School fees increase gradually along with grade level, despite the abolition of school fees (UNICEF/World Bank 2009).

This finding is in line with results from some previous studies reporting compensating parental behaviour (Behrman 1988; Bharadwaj et al. 2018; Rosenzweig and Wolpin 1988; Del Bono et al. 2012; Frijters et al. 2009; Halla et al. 2014; Leight 2017; Yi et al. 2015). It is also consistent with the intrafamily resource allocation model introduced by Behrman et al. (1982), suggesting parents favour equity over efficiency.

However, we have indicative evidence that this effect varies across family SES. Relatively advantaged parents provide more cognitive investment to lower-ability children, and lower-class families exhibit only small and modest compensatory behaviours. The finding is consistent across all measures of parental socio-economic advantage (maternal education, household wealth and household size). Consistent with prior findings, mothers with higher education compensate for lower-endowed children (Cabrera-Hernandez 2016; Hsin 2012; Restrepo 2016).

Our results therefore complement the literature which studies whether the effect of shocks to early ability can be eliminated or mitigated through investments, that themselves depend on family socio-economic status. Most studies have found that compared with low-ability children born in higher-class families, low-ability children born in lower-class families have worse outcomes in adulthood. One hypothesis of the results in the literature is that parental involvement plays a role in reinforcing the poor ability outcome. Specifically, higher-class parents compensate for the differences in ability, or at least are not reinforcing the differences. Our results support the hypothesis that parental investment varies by family SES, even in a context of low-income by international standards. What is difficult to disentangle, given the high correlation between SES as measured by wealth, and by parental education, is to differentiate whether high-SES parents are more able to observe the difference in ability; more able to compensate for the difference; or both of these. Our suggestive evidence is that the former is a potentially important channel, and more work on this issue is needed where suitable data can be collected.

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Appendix

Table A1. *Robustness check: first-stage results adding sibling rainfall in infancy using community fixed-effects model*

Cognitive ability	(1)	(2)
Child average rainfall in the first two years of life	0.137	0.127
	(0.033)***	(0.046)***
	[0.039]***	[0.052]**
Sibling average rainfall in the first two years of life		-0.013
		(0.044)
		[0.046]
Child-level controls	Yes	Yes
Household-level controls	Yes	Yes
Mother-level controls	Yes	Yes
Community fixed-effects	Yes	Yes
Observations	1176	1176
R-squared	0.686	0.686

Notes: Robust standard errors in parentheses, clustered standard errors by community in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. This table reports analogous regression as the one in the first-stage regression. The dependent variable is cognitive ability. Children controls are age in months, square of age in months, cubic of age in months, maternal age at birth, the type of school, birth order, birth year, language, and the type of sibling (such as born as an older sister and paired with a younger brother). Household-level controls are household size, wealth index, and gender of household head. Mother-level controls are a series of levels of maternal education.

Table A2. *Redundancy tests: cognitive ability and cognitive resources*

Dependent variable:	IV-FE	
Total educational fees	Instruments: Rainfall from *in utero* to year 2	Instruments: Rainfall at birth and in year 1
Cognitive ability	-0.057**	-0.064**
	(0.025)	(0.025)
Anderson-Rubin (AR) test:		
Confidence intervals at 95%:	[-0.169,-0.008]	[-0.150, -0.015]
Weak identification test:		
Cragg-Donald Wald F stat	7.907	14.411
Moutiel-Pflueger effective F stat	8.680	14.985
Stock-Yogo weak ID test critical values:		
10% maximal IV size	10.27	19.93
20% maximal IV size	6.71	11.59
Moutiel-Pflueger critical values:		
5% of worst case bias	21.399	20.091
10% of worst case bias	12.909	12.645
20% of worst case bias	8.274	8.479
IV redundancy test:		
Redundancy of rainfall in utero p-val	0.6026	-
Redundancy of rainfall at birth p-val	0.001	0.0033

Dependent variable: Total educational fees	IV-FE	
	Instruments: Rainfall from *in utero* to year 2	Instruments: Rainfall at birth and in year 1
Redundancy of rainfall in year 1 p-val	0.0155	0.0093
Redundancy of rainfall in year 2 p-val	0.0542	-
Observations	1176	1176
Nr. excluded instruments	4	2
Siblings-difference	Yes	Yes
Child-level controls	Yes	Yes

Notes: Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. Children controls are age, maternal age at birth, the type of school, birth order, birth year, and the type of sibling (such as born as an older sister and paired with a younger brother). IV redundancy test is a LM test of a specified instrument, asking whether this instrument provides useful information to identify the equation. Rejecting the null suggests that the specified instrument does not capture information of the endogenous variable.

Table A3. *Robustness check: first-stage results using restricted sample*

Cognitive ability	(1)	(2)	(3)	(4)	(5)	(6)
Rainfall *in utero*	0.091					
	(0.046)					
	(0.032)					
Rainfall at birth		0.125				0.083
		(0.026)***				(0.035)**
		[0.027]***				[0.039]**
Rainfall in year 1			0.195			0.109
			(0.039)***			(0.053)**
			[0.054]***			[0.074]
Rainfall in year 2				-0.045		
				(0.052)		
				(0.064)		
Average rain at birth and in year 1					0.184	
					(0.032)***	
					[0.037]***	
Underidentification test:	$x(1)^2 =$ 1.433	$x(1)^2 =$ 25.818	$x(1)^2 =$ 21.140	$x(1)^2 =$ 0.296	$x(1)^2 =$ 31.365	$x(2)^2 =$ 31.378
p value	0.231	0.000	0.000	0.587	0.000	0.000
Weak instrument test:						
Montiel-Pflueger effective F stat	1.381	27.319	21.959	0.285	34.292	17.130
Montiel-Pflueger critical values:						
5% of worst case bias	37.418	37.418	37.418	37.418	37.418	5.963
10% of worst case bias	23.109	23.109	23.109	23.109	23.109	4.639
20% of worst case bias	15.062	15.062	15.062	15.062	15.062	3.878
Siblings-difference	Y	Y	Y	Y	Y	Y
Child-level controls	Y	Y	Y	Y	Y	Y
Observations	678	678	678	678	678	678
R-squared	0.770	0.785	0.784	0.770	0.788	0.788

Notes: These are within-household fixed effects estimates. Robust standard errors in parentheses, clustered standard errors by community in brackets. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. This sub-sample contains the sibling-pairs which have an age gap larger than two years. Children controls are age, maternal age at birth, the type of school, birth order, birth year, and the type of sibling (such as born as an older sister and paired with a younger brother).

Table A4. *Robustness check: regression models of cognitive ability standardised by age and total educational fees*

	Dependent variable: Total educational fees					
			IV-FE			
	OLS	FE	Instruments: Rainfall at birth	Instruments: Rainfall in year 1	Instruments: Average rainfall in first two years of life	Instruments: Rainfall at birth and rainfall in year 1
	(1)	(2)	(3)	(4)	(5)	(6)
Cognitive ability	0.037*	-0.001	-0.043**	-0.055**	-0.047**	-0.047**
standardised by age	(0.020)	(0.003)	(0.020)	(0.022)	(0.019)	(0.019)
Anderson-Rubin (AR) test:	-	-	[-0.092, -.007]	[-0.115, -0.017]	[-0.093, -0.013]	[-0.108, -0.008]
Observations	1176	1176	1176	1176	1176	1176
Nr. excluded instruments	-	-	1	1	1	2
Siblings-difference	-	Yes	Yes	Yes	Yes	Yes
Child-level controls	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The interest variable is PPVT score standardised by age. The dependent variable is total educational fees. Children controls are maternal age at birth, the type of school, birth order, birth year, and the type of sibling (such as born as an older sister and paired with a younger brother).

Table A5. *IV-FE model of the effects of height-for-age z-scores (HAZ) on total educational fees: potential heterogeneity effect by maternal education*

Dependent variable:	All		Educated mothers		Non-educated mothers	
Total educational fees	(1)	(2)	(3)	(4)	(5)	(6)
HAZ	-0.035**	-0.096*	-0.041	-0.313	-0.024**	-0.030
	(0.017)	(0.058)	(0.026)	(0.493)	(0.012)	(0.021)
Child-level controls	–	Yes	–	Yes	–	Yes
Observations	1030	1030	526	526	504	504
Weak IV KP F statistics	24.575	4.416	14.517	0.380	10.210	3.102

Notes: Robust standard errors in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. The interest variable is HAZ. Same model is used as the main model. The IV used is the average rainfall in the first two years of life. Child-level controls are maternal age at birth, the type of school, birth order, birthyear, and the type of sibling (such as born as an older sister and paired with a younger brother).

Reinforcement or Compensation? Parental Responses to Children's Revealed Human Capital Levels in Ethiopia

There is an increasing body of literature that finds that parents invest in their children unequally, but the evidence is contradictory, and few studies provide convincing causal evidence of the effect of child ability on parental investment in a low-income country. This working paper examines how parents respond to the differing abilities of primary school-age Ethiopian siblings, using rainfall shocks during the critical developmental period between pregnancy and the first three years of a child's life to isolate exogenous variation in child ability within the household, observed at a later stage than birth.

The results suggest that on average parents attempt to compensate disadvantaged children through increased cognitive investment. The results are significant, but small in magnitude: parents provide about 6.3 per cent of a standard deviation more in educational fees to the lower-ability child in the observed pair. Families with educated mothers, smaller household size, and higher wealth compensate with more cognitive resources for a lower-ability child. This suggests that improving resources available to households would benefit the least advantaged young people.

Young Lives

An International Study of Childhood Poverty

About Young Lives

Young Lives is an international study of childhood poverty, involving 12,000 children in four countries over 15 years. It is led by a team in the Department of International Development at the University of Oxford in association with research and policy partners in the four study countries: Ethiopia, India, Peru and Vietnam.

Through researching different aspects of children's lives, we seek to improve policies and programmes for children.

Young Lives Partners

Young Lives is coordinated by a small team based at the University of Oxford, led by Professor Jo Boyden.

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- *Save the Children India*
- *Sri Padmavathi Mahila Visvavidyalayam (Women's University), Andhra Pradesh, India*
- *Grupo de Análisis para el Desarollo (GRADE), Peru*
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