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ABSTRACT

Child Ability and Household Human Capital Investment Decisions in Burkina Faso*

Using data we collected in rural Burkina Faso, we examine how children's cognitive abilities influence resource constrained households' decisions to invest in their education. We use a direct measure of child ability for all primary school-aged children, regardless of current school enrollment. We explicitly incorporate direct measures of the ability of each child's siblings (both absolute and relative measures) to show how sibling rivalry exerts an impact on the parent's decision of whether and how much to invest in their child's education. We find children with one standard deviation higher own ability are 16 percent more likely to be currently enrolled, while having a higher ability sibling lowers current enrollment by 16 percent and having two higher ability siblings lowers enrollment by 30 percent. Results are robust to addressing the potential reverse causality of schooling influencing child ability measures and using alternative cognitive tests to measure ability.

JEL Classification: O15, J12, I21, J13

Keywords: education, sibling rivalry, child ability, household decisions, Africa

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1. Introduction

Parental decisions about whether and how much to invest in their children's human capital depend on many factors, and these decisions have long-lasting impacts on each child's future earnings, marital prospects, and overall welfare. A large literature attempts to understand the source of inequalities for children's educational investments within a household building on seminal work by Becker and Tomes (1976) that delineates the tradeoff between the quantity of children and their 'quality.' In making the schooling investment decision, parents will have information about a child's ability and that information will often not be available to researchers, which partly explains why much of the empirical research on the determinants of household investments in children's schooling focuses on easy to observe demographic characteristics of the child such as gender, birth order, and family composition (Parish and Willis 1993; Garg and Morduch 1998; Black, Devereux, and Salvanes 2005).[1] More recent papers attempt to use direct measurements of a child's ability such as IQ scores (Kim 2005) or cognitive tests (Ayalew 2005) to better understand which factors influence investment decisions.

In this paper, we build on the seminal empirical work by Rosenzweig and Schultz (1982) to examine the role that a child's cognitive ability plays in a resource constrained household's decision to invest in that child's education. For poor households seeking to maximize the returns to their human capital investments, schooling decisions will depend on parent perceptions about the returns to school for a given child and that child's ability. In a setting where few households ever enroll all of their children in school, as is true in many developing countries, understanding the link between child ability and school enrollment and school continuation decisions is critical for developing policy prescriptions to improve educational outcomes.

[1] See Strauss and Thomas (1995) and Glewwe and Kremer (2006) for reviews of the literature. Related research explores the relationship between these demographic characteristics and the non-schooling outcomes of employment (Kessler 1991), risky behaviors (Aizer 2004), and child labor (Emerson and Souza 2008).

We make four main contributions to the literature on explaining household school investment decisions. First, we employ direct measures of a child's ability for all children of primary school age (5 to 15), regardless of whether they are currently enrolled in school. This differs from existing papers that tend to have ability measures only for children that are currently enrolled in school (Glick and Sahn 2010). We use the Raven's Colored Progressive Matrices (CPM) and the Weschler Intelligence Scales (WISC) Digit Span as measures of a child's cognitive ability. Second, our paper is unique in explicitly incorporating direct measures of the ability of each child's siblings (both absolute and relative measures) and to show how sibling ability 'rivalry' exerts a strong impact on the parents' decision of which child to send to school. Third, the survey instrument asks parents to provide their perceptions about the likely chances of future economic success for each of their children, information that is not often gathered in surveys. We show that a similar pattern of sibling rivalry is observed using either these parent perceptions or the externally validated cognitive ability tests measuring child ability. Fourth, we address potential concerns about schooling influencing measures of child ability by exploiting the panel data structure and focusing on the relationship between the enrollment decision in the survey's second year and the ability measures observed in the survey's first year for the subset of young children who were not enrolled and not yet of typical school age in the first year.

We explore both the extensive margin of school enrollment during the 2007-2008 school year and grade progression measures, as well as the intensive margin of school related expenses. We find that a child with a one standard deviation higher ability test score has a 16 percent higher likelihood of being currently enrolled in school, while a child with a higher ability sibling is 16 percent less likely to be currently enrolled and having two higher ability siblings lowers a child's probability of enrollment by 30 percent. Household fixed effects regressions show that within a given household, a child with one standard deviation higher ability compared to the average ability of their siblings is 31 percent more likely to be enrolled. On the intensive margin, controlling for household fixed effects, we find that a child with one standard deviation higher ability receives 20 percent more discretionary school expenditures by the parents.

The remainder of the paper is organized as follows. Section 2 discusses the conceptual framework about sibling rivalry and the household schooling investment decision. Section 3 describes the survey data used in the analysis and explains the construction of the different child ability measures. Section 4 describes the empirical identification strategy and section 5 presents the main results as well as robustness tests. Section 6 concludes.

2. Sibling Rivalry Conceptual Framework

A number of studies examine the interaction of siblings to understand schooling outcomes and why girls often receive less education than their brothers. Butcher and Case (1994), using United States data that contains explicit information on an individual's completed education and the education of their brothers and sisters, find that women with only brothers receive significantly more education on average than women with any sisters.[2] Their finding differs from what is typically found in developing country studies. Parish and Willis (1993) examine how sibling sex composition influences girls' education in Taiwan. They emphasize that cultural traditions favoring male descent can cause parents to manipulate daughters for the benefit of their sons. Garg and Morduch (1998) emphasize sibling rivalry using data from Ghana. Child education decisions in credit constrained households are influenced by the number of children they have, resource dilution, and the sex composition of their children, sibling rivalry. Resource dilution

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[2] They highlight three potential explanations for why sibling sex composition might influence education decisions: sibling resource competition, sex-typing of tasks, and peer effects. Resource competition occurs if boys and girls have different relative prices for educational investments or investment returns. Sex-typing stems from parents sending messages to children describing appropriate behaviors and goals, while peer effects come from children developing traits that depend on how they interact with their siblings.

occurs because more children imply fewer resources per child and credit constraints limit the family's ability to borrow against future returns. Sibling rivalry occurs because all children benefit from having fewer educated siblings with comparatively higher returns on investment.

Resource dilution and sibling rivalry in educational investments in poor countries is well documented. In addition to the Taiwan and Ghana studies, the list includes: Binder (1998) for Mexico, Morduch (2000) for Tanzania, Edmonds (2007) for Nepal, Ota and Moffatt (2007) for India, and Dammert (2010) for Guatemala and Nicaragua. While Garg and Morduch focus on credit constraints and differences in relative returns to education as the cause for sibling rivalry, Edmonds (2007) emphasizes that comparative advantage in home production can lead to similar implications when girls have comparative advantage and it is not possible to hire labor for home production. Both Edmonds (2007) and Dammert (2010) find evidence consistent with this sibling rivalry interpretation.

Such models of sibling rivalry neglect that parents have additional knowledge about their children’s capabilities and use this information to make school investment decisions. A literature embedded in testing the one-period consensus parental preferences model of human capital investment of Becker and Tomes (1976) and Behrman, Pollack, and Taubman (1982) uses child endowments in modeling the investment decision.[3] Most studies that examine the investment decision process have to work around the fact that actual child ability or endowment is typically not observed (Rosenzweig and Schultz 1982; Behrman, Pollack and Taubman 1982; Behrman, Rosenzweig and Taubman 1994). Some recent studies are able to use direct measures of child ability. Kim (2005) uses an IQ test administered to Wisconsin high school juniors and finds that higher ability children receive more parent transfers. Glick and Sahn (2010) use achievement test

[3] See Behrman (1997) for an overview of the consensus parental preferences models.

scores from Senegalese children taken in Grade 2 to explain school outcomes seven years later, but this neglects the role of siblings that may not have been enrolled in school.

The most closely related paper to ours is by Ayalew (2005) who uses Raven's CPM test scores for school-age children in one village in Ethiopia to measure child ability and using a household fixed effects model finds that parents consider child ability when making school enrollment decisions. There are several key differences between our papers. First, we focus on absolute and relative direct measures of the ability of a child's siblings to generate inferences about the role of sibling rivalry in influencing schooling decisions. Second, we explore both alternative ability measures by using different cognitive tests and alternative outcomes such as school expenditures and grade progression, in addition to current enrollment, which is the focus of the Ayalew paper. Third, we exploit the panel data structure as a robustness check to address potential reverse causality concerns about schooling influencing measures of child ability.

3. Burkina Faso Social Protection Evaluation Survey

The panel survey was conducted in June 2008 (Year 1) and June 2009 (Year 2) in Nahouri province in southern Burkina Faso, located approximately 100 miles from the capital and bordering Ghana. Households were randomly selected from a village-level census conducted by our project team immediately prior to the Round 1 survey in the 75 rural villages of Nahouri province that each has a primary school. The survey is part of an ongoing project evaluating social protection strategies in Burkina Faso. Households in this region are predominantly subsistence farmers growing sorghum and groundnuts and have mean annual per capita expenditures of approximately $90.

Our analysis focuses on primary school-aged children ages 5 to 15 in households with multiple biological children. There are 4,635 children in this age range in 1,507 different households. As shown in Table 1, parental schooling is low, with only 13 percent of the children having a parent that ever attended school. Fifty-four percent of this children's sample is male and the average age is 9.4 years old. On average, these children have 3.8 siblings under age 15, including 1.8 sisters. They live in households with an average of 8.9 individuals, including a head of household, 1.5 wives, 4.8 biological children of the household head under age 15, 0.4 children under age 15 that are not the biological children of the head, and 1.2 other members that include grandparents, aunts, uncles, and other extended family members.

Parents were directly asked about the chance of future success they believe each of their children will have in formal employment, a reasonable measure of parental perceptions about the investment return on their child's education, since most jobs in "formal employment" in Burkina Faso require a level of education beyond primary school and in particular French skills.[4] This parental perception measure is based on everything the parent knows about the child and about the labor market, whether right or wrong, and was asked about every child in the household. For each child, the parents responded whether that particular child had a 'small', 'medium', 'large', or 'very large' chance of future success in formal employment. Parents considered 25 percent of these children to have a 'small' chance of future success and only 8 percent to have a 'very large' chance of future success. Parents viewed most children (67 percent) to have a 'medium' (38 percent) or 'large' (29 percent) chance of future success.

To corroborate these parent perceptions, we also consider externally validated measures about a child. We use the Raven's CPM and the WISC Digit Span to measure a child's cognitive ability; both are tests that do not require formal schooling to be able to answer the questions. The Raven's CPM is a measure of fluid intelligence or problem solving ability. The test does not

[4] Schultz (2004) and Kazianga (2004) using Burkina Faso nationally representative data report substantial returns (9 to 16 percent) for an additional year of primary school, highlighting the importance of schooling in this context. Returns to secondary (14 to 26 percent) and tertiary (13 to 23 percent) schooling are even higher.

depend heavily on verbal skills, making it relatively "culture free" (Borghans, Duckworth, Heckman, and Weel 2008). In Figure 1 Panel A, we show two sample problems from the Raven's test (Raven, Raven, and Court 1998). The child respondent is asked to select the image that is missing in order to complete the picture. This type of question is novel to the children in Nahouri Province, thus providing a more natural or true measure of problem solving skills.

We ask 18 questions from the Raven's CPM and on average, children in our sample answer 4.9 questions correctly.[5] Younger children answer fewer questions correctly than older children (the average number correct for children age 5 is 2.8 and for children age 15 is 7.6).[6] To control for this relationship between age and raw test scores, we calculate a z-score for each child measured as the child's raw test score minus the average score for the same age children divided by the standard deviation of test scores for children of that age.[7] Therefore, the mean of the Raven's z-score is zero and the standard deviation is one for each age and across all ages.[8]

The WISC Digit Span is a measure of working memory and ability to concentrate and has both a forward and backward component. The respondent repeats a string of numbers to the enumerator and is scored by whether or not they repeat the full string correctly as shown in Figure 1 Panel B (Weschler 1974). In the Digit Span Forward, the child must repeat the string of numbers exactly as stated by the enumerator. The string of numbers increases in length as the child answers correctly. With the Digit Span Backward, similar strings of numbers are to be repeated in the reverse order from that stated by the enumerator until the child can no longer

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[5] During extensive pretesting of the Raven's test, results were consistent whether children were asked the entire set of 36 questions or only the odd-numbered questions, so to save interview time we only administered the 18 odd-numbered questions (Sets A, Ab, and B).

[6] The average number of questions answered correctly for children ages 6, 7, 8, 9, 10, 11, 12, 13, and 14 is respectively 2.8, 3.6, 4.4, 5.1, 5.3, 5.6, 6.1, 6.5, and 6.4.

[7] We did not use the international Raven's norming standards since we asked a subset of the Raven's test and what is most important here is how the children in rural Burkina Faso compare to each other, not internationally.

[8] Note that in Section 5.2, we estimate alternative specifications to test the robustness of using the Raven's age-adjusted z-score instead of the raw test scores.

continue. We calculate a total combined score of the forward and backward digit spans.[9] As with the Raven, we calculate a WISC Digit Span age-adjusted z-score to control for age effects.

In Table 1, we present summary statistics about children's schooling. Few households in rural Burkina Faso ever enroll all of their children. Only 54 percent of children are enrolled in the 2007-2008 school year. Fifty-six percent of households experience variation in enrollment among their children age 5 to 15, while 17 percent enroll no children and only 27 percent of households currently enroll all of their primary school-aged children. If we consider whether a child has ever been enrolled in school rather than current enrollment during 2007-2008, then 59 percent of children in the sample have ever been enrolled and 54 percent of households experience variation across their children in whether a child has ever been enrolled. Given these low enrollment rates, on average these children only have completed 1.8 years of school. Child labor rates are high, with children heavily involved in agriculture and household production.

In addition to examining the relationship between parent perceptions, child ability and school enrollment, we explore three alternative schooling-related outcomes (on-time start, grade progression, and discretionary school expenses) where sibling rivalry might matter. In Burkina Faso, parents typically enroll their children starting at age seven, so we construct a variable to indicate if children started school by this age or if they were delayed. The 'on-time start' variable shows that only 40 percent of primary school-aged children start school by age seven, with the rest either starting at a later age or never attending school. Fifty-four percent of households have variation across their children in whether each child started school by age seven. Second, we consider grade progression through school, which we calculate by dividing the child's highest grade attended by the number of years since the child started school.[10] The grade progression

[9] Our regression results are robust to keeping the forward and backward digit span scores separate.
[10] For children who never attended school, they are assigned a grade progression measure of zero.

measure ranges from zero to one, with higher numbers indicating quicker progress towards completing primary school. Third, for each child we calculate the total schooling-related discretionary expenditures during the 2007-2008 school year. While school in Burkina Faso has relatively low registration fees (904 FCFA on average per year, about $2.18 using the June 2008 exchange rate of 415 FCFA = $1 USD), there are additional expenditures expected of each family when they enroll their child. These can include purchasing uniforms, contributions for the school cafeteria, and transportation costs. Spreading resources evenly across children to pay the fixed costs associated with schooling may not be possible in the presence of liquidity constraints. However, for some of these expenditures, such as school supplies and parent association fees, parents have discretion in the amount spent each year on a child who is enrolled, and these discretionary expenditures are important in this setting as well as in developing countries that currently have free schooling. In our sample, the total average cost of sending a child to school is 3867 FCFA per school year (about $9.32), with parents spending on average 845 FCFA (about $2.04) on these discretionary items per child (about 22 percent of total educational expenses).

4. Empirical Identification Strategy

4.1 Econometric Specification

Studies of sibling rivalry in education typically use counts of the number of siblings and the number of sisters that a child has to explain different schooling outcomes (attendance, enrollment, attainment) as follows:

(1) $$e_{ih} = \omega_0 S_{ih} + \omega_1 F_{ih} + \alpha_0 X_{ih} + \alpha_1 Z_h + \varepsilon_{ih}$$

where e_{ih} is the educational outcome for child *i* in household *h*, S_{ih} is a count of the number of siblings the child has, F_{ih} is a count of the number of female siblings the child has, X_{ih} is a vector of individual characteristics such as age and gender that might influence parental investments, Z_h

is a vector of household characteristics, and ε_{ih} is a random, idiosyncratic error term. The interpretation of ω_0 is the change in e_{ih} associated with an additional male sibling. The interpretation of ω_1 is the change in e_{ih} associated with the thought experiment of converting a sibling from a male to a female. $\omega_0 + \omega_1$ is then the change in e_{ih} associated with adding an additional female sibling. This approach takes current family size and composition as given at the time the parents make the enrollment decision.

To better understand the parental schooling investment decision, we expand on the sibling rivalry model in Equation 1 to control for previously unobserved characteristics about the child (his ability) and his home environment (his siblings' ability) that might influence the parent's decision. We employ two empirical approaches to estimate this relationship. First, we estimate the following household or sibling fixed effects logit regression that will control for all household level characteristics that are constant across siblings:

(2) $$e_{ih} = \beta_0 A_{ih} + \alpha_0 X_{ih} + \lambda_h + \eta_{ih}$$

where e_{ih} and X_{ih} are defined as above, A_{ih} is a direct measure of observed child ability, λ_h is the household fixed effect that captures all characteristics about the household that are constant across siblings, and η_{ih} is the child specific idiosyncratic error term. In Equation 1 and previous sibling rivalry papers, child ability was part of the error term, ε_{ih}, but in our analysis we are able to directly control for its effect on educational outcomes.[11] This within family estimate compares a child's own ability to the average ability of all the other children in the household to examine if parents compare a child's ability to the average ability of his siblings when making human capital investment decisions.

[11] Note that in the household fixed effects specification, household characteristics, Z_h, and number of siblings, S_{ih}, will drop out of the specification because there is no variation across children within the household. In the household fixed effects regressions, we also drop the number of sisters variable, F_{ih}, because it is constant within a given household for children of the same gender.

While the household fixed effects estimation compares own ability to average sibling ability, the second approach we adopt is to be more specific about the functional form of the sibling ability term and to include direct measures of sibling ability in the regression. This approach has the additional advantage that we can include the same variables as in the sibling rivalry literature (in Equation 1) and allows us to examine how the relevant coefficients vary when controlling for a child's own ability and his sibling's ability. We estimate the following extended Equation 1 sibling rivalry regression:

(3) $$e_{ih} = \beta_0 A_{ih} + \beta_1 h(A_{-ih}) + \omega_0 S_{ih} + \omega_1 F_{ih} + \alpha_0 X_{ih} + \alpha_1 Z_h + \mu_{ih}$$

where $h(A_{-ih})$ is a measure of the ability of the other children *(-i)* in household *h* with varying functional forms that we discuss in detail below, and the other variables are as defined above. The error term, μ_{ih}, measures the child specific idiosyncratic part of ε_{ih} not captured by a child's own ability, A_{ih}, or his sibling's ability, $h(A_{-ih})$. The coefficients β_0 and β_1 respectively give an estimate of the impact of child *i*'s own ability and his sibling's ability on child *i*'s enrollment.

We use several alternative measures of sibling ability, $h(A_{-ih})$, including both absolute measures (highest sibling ability) and relative measures (whether there are any siblings with a higher ability score and dummies for the number of siblings with higher ability scores). Absolute measures provide insight into the role of the level of sibling ability in a household. Having siblings with high ability might raise overall enrollment levels in a family, or it might represent competition for the child. It could be that the average level of sibling ability affects a child's enrollment differently than the ability level of the household's 'best' sibling (with the highest ability). If sibling rivalry influences parents deciding who to send to school, then parents might consider how a child compares in ability to his siblings rather than considering the child's ability on its own, and relative sibling ability measures might be more informative. In our sample, 40 percent of the overall variation in ability arises from within family variation across siblings, while 60 percent is between families.

4.2 Potential Threats to Identification Strategy

Since schooling potentially affects cognitive ability, reverse causality is the primary problem we face. We attempt to address this in two ways. First, we estimate robustness specifications in which we limit the sample of children to Grades 2 and lower or to Grade 1 and lower. The decision to use this grade cutoff point is based on a regression of the Raven's age adjusted z-score on grade in school, and the coefficients for Grades 1 and 2 are close to zero (0.05 and 0.09 respectively) and not statistically significant. The coefficients for Grade 3 and 4 are slightly larger (0.14 and 0.12 respectively), but only the Grade 3 coefficient is statistically significant, while the Grade 4 coefficient is not statistically significant. We interpret this lack of relationship between the lower grades and ability test scores as evidence that children in Grade 2 and lower have not yet received enough schooling to influence their cognitive ability test scores. Based on this information and to be conservative in our robustness specifications, we select Grades 1 and 2 as the cutoff levels. Second, we restrict the sample to young children ages 5 to 7 (and 5 to 6) who are not enrolled in year 1 but for whom we have ability measures in year 1 and look at their enrollment in year 2. This eliminates any potential effect of schooling on the ability measures as these children were not enrolled at the time of taking the ability test.

5. Empirical Results

5.1 Sibling Rivalry, Parent Perceptions, and Child Ability

Since we are building upon the sibling rivalry literature, we begin our analysis estimating Equation 1 that uses the standard observable family composition characteristics, number of siblings, number of sisters, and birth order. Results of this regression are presented in Table 2, column 1. We find evidence of resource dilution and sibling rivalry consistent with the literature. We find that the number of siblings has a negative effect on enrollment (resource dilution) while the number of sisters has a positive effect (sibling rivalry). Holding constant the number of sisters, the addition of a male sibling is correlated with 2.5 percentage points (or 4.6 percent) lower likelihood of attending school. An additional female sibling has no impact on whether the child attends school. Subsequently, switching from a male to female sibling corresponds to a 2.2 percentage point higher likelihood of enrollment, or 4.1 percent of the base enrollment level. Birth order has a positive but not statistically significant coefficient indicating younger siblings are more likely to be enrolled, as is consistent with the literature in developing countries.

As discussed in Section 4.1, other factors about the child besides these observable demographic characteristics are likely to influence the parent's schooling investment decision. Parents know more about their children's characteristics than simply their gender and sibling composition. Since it is the parents' perceptions of their child's ability or potential for future success, whether correct or not, that informs and affects their decision about educational investment, we first examine the relationship between school enrollment and these parents' perceptions about each of their children.[12] We estimate both a household fixed effects logit as in Equation 2 and an extended sibling rivalry logit regression as in Equation 3, and in columns 2 to 5 of Table 2 we present the corresponding results.[13] We find a positive relationship between what parents think about a child and his current enrollment. On the other hand, perceptions of the child's siblings in the same age group have a negative relationship with the child's enrollment,

[12] The parent perception variable takes values of 0 to 3, where 0 means a child has a small chance of future success, 1 a medium chance, 2 a large chance, and 3 a very large chance. Parents on average report that their children have a medium chance of success, with the variable having a mean of 1.2 and a standard deviation of 0.9.

[13] Parent perceptions may be influenced by the child's enrollment status, and therefore the results presented in this table should not necessarily be interpreted as causal.

suggesting parents make educational investment decisions based not only on what they think of one child but also what they think of that child's siblings.

The household fixed effects specification presented in Table 2 column 2 shows that children with one level higher parental perceptions compared to the average perceptions of their siblings have an 18.4 percentage point higher probability of enrollment, which corresponds to a 34.1 percent higher enrollment level. In columns 3 to 5, we explicitly estimate the relationship between parent perceptions about the child's siblings and a child's enrollment. Controlling for direct measures of parent perceptions of siblings, the parental perceptions of the child are still positively correlated with the child's enrollment and statistically significant at the one percent level. One level higher parent perceptions is correlated with a 10.9 to 16.2 percentage point higher likelihood of being enrolled. However, parental perceptions of a child's siblings are negatively correlated with the child's enrollment status. Compared to the household fixed effects specification in which the parental perceptions of the child are compared to the average of his siblings, an alternative is to make the comparison with the parental perceptions of the 'best' sibling. Results in column 3 show that children whose sibling with the highest perception in the family has a one level higher value have a 6.5 percentage point lower likelihood of enrollment.

Relative sibling perceptions might be more relevant than absolute sibling perceptions since it is possible that having a sibling whom the parents think of more highly than oneself matters more than the overall perception level of one's siblings. Column 4 uses an indicator of whether the child has any sibling with parental perception higher than himself while column 5 uses indicators for whether the child has one, two, or three or more siblings with higher parental perceptions. Children having any sibling with better parent perceptions have a 7.0 percentage point lower probability of enrollment. Children with three or more siblings with higher parental perceptions have a 14.6 percentage point lower probability of being enrolled, corresponding to a 27 percent lower enrollment, and the coefficient is significant at the five percent level.

While the relationship between parental perceptions of a child and his schooling is strong, it does not eliminate the role of sibling composition. Having more brothers is correlated with lower enrollment, while having more sisters instead of brothers and holding the number of siblings constant is correlated with higher enrollment. Birth order is also important; younger siblings have a 2.3 to 2.5 percentage point higher probability of being enrolled (columns 3 to 5). Consistent with inter-generational education transmission and wealth effects, better educated parents and wealthier households have children who are more likely to be enrolled.

While parental perceptions about their child's chance of future success are correlated with the child's current school enrollment, these perceptions may or may not be accurate or well-informed. There may also be significant differences across households in how parents perceive their own children and what factors they take into account in formulating perceptions. To further explore these issues, we incorporate an externally validated measure of the child's cognitive ability using the Raven's CPM test. These tests were administered during the baseline survey to every child age 5 to 15 regardless of their current enrollment status and provide a consistent measure of child ability across children in all households. There is a strong positive relationship between the ability measure and parent perceptions. Higher ability children are viewed by their parents to have a higher chance of future success. However, after controlling for gender and age, the ability measure only explains about 20 percent of the variation in parental perceptions.

In Table 3, we estimate the relationship between child ability (as measured by the Raven's age adjusted z-score) and current school enrollment using a household fixed effects logit as described in Equation 2 and a logit regression with alternative sibling ability measures as described in Equation 3.[14] The household fixed effects logit results in column 1 indicate that a child with one standard deviation higher own ability compared to the average of his siblings has a 16.5 percentage point higher likelihood of being currently enrolled, corresponding to 30.6 percent of the base enrollment. The coefficient is significant at the one percent level. This is evidence parents take into account a child's cognitive ability in deciding enrollment, and the magnitude of the effect is large.

When considering how parents make this enrollment decision, one approach would be for them to compare a child's ability with the average ability of his siblings, and this is captured in the household fixed effects specification. An alternative that takes into account the non-linear relationship between siblings' abilities would consider the impact of the sibling with the highest ability. Another approach would include relative measures indicating if the child has any sibling with a higher ability measure or whether the child has one, two, or three or more siblings with higher ability measures.[15] Controlling for these direct measures of sibling ability (in columns 2 to 4), the child's own ability is still positively correlated with the child's enrollment and statistically significant at the one percent level. One standard deviation higher own ability is correlated with 15.7 to 27.2 percent higher likelihood of enrollment compared to the base enrollment level. Having one's 'best' sibling have a one standard deviation higher ability is correlated with a 6.8 percentage point lower enrollment rate (column 2), and the coefficient is significant at the one percent level. Likewise, having any sibling with a higher ability is correlated with 11 percentage

[14] All regressions include child gender and age dummies, and the regressions estimating Equation 3 also include village fixed effects, parent schooling, household assets, and family demographic composition measures. Results presented in Table 3 are consistent when using the number of siblings and the number of sisters age 5 to 15 rather than the number of siblings and sisters age 0 to 15. Correlation among the error terms of children in a given village experiencing the same enrollment environment might bias the standard errors downward, so in all regressions we cluster the standard errors by village.

[15] Results are robust to additional sibling ability measures including median sibling ability, the number of siblings with a higher ability, dummies for whether a child's ability is highest or lowest in the household, and whether the child has any siblings who have ability measures one-half or one standard deviation higher.

points lower likelihood of being enrolled (column 3), and this effect is magnified if there are two siblings with higher abilities (16.1 percentage points). Both coefficients are significant at the one percent level.

Including child ability and sibling ability measures does not significantly alter the family demographic composition variables. The sign and level of statistical significance are consistent with the initial regression presented in Table 2 column 1, while the magnitude of the coefficient for the number of siblings and number of sisters is somewhat reduced. It is worth noting that the relationship between a child's own ability and current school enrollment is four to eight times larger than the corresponding relationship between the standard demographic composition variables and enrollment. These sibling ability rivalry results are consistent with the parental perceptions regressions in Table 2 and indicate that part of what is driving the relationship between parental perceptions and the school enrollment decision is the child's ability.[16]

Having explored the relationship between child ability and the extensive margin of school enrollment, we next turn to the intensive margin of educational expenditures. This allows us to rule out the interpretation that the results presented in Tables 2 and 3 reflect solely the desire of the child to attend school. If higher ability children have more motivation to attend school and this reduces the parents' cost of effort to make the child attend, then we would not expect the parents to make additional discretionary monetary investments in these children. We focus on expenses for school supplies and parent association voluntary fees because these have a discretionary component, whereby parents have some leeway in how much they spend on each

[16] We also estimate the regressions separately by child gender and find no strong gender difference. Sibling rivalry appears to be more important for girls than boys, but we cannot reject the equality of coefficients between the genders. Similarly we cannot reject that the role of own ability or parent perceptions are the same for both genders. We also estimate the regressions broken down by poverty level, defining poor households to have log assets below the mean, below the median, or in the bottom quintile, and while the estimates for poor families are larger, we cannot reject that poor and non-poor families have the same level of sibling rivalry.

of their children.[17] For the regressions presented in Table 4, we restrict the sample of 4,635 children age 5 to 15 living in households with multiple siblings to only the 2,511 children who are currently enrolled in school. We estimate a similar series of regressions as in Table 3 (household fixed effects in column 1 and then including alternative sibling ability measures in columns 2 to 4). Results in column 1 indicate that within a given household, children with a one standard deviation higher ability receive 170 FCFA more in discretionary expenditures, representing 20.1 percent of mean discretionary expenses, and the coefficient is significant at the one percent level. Controlling directly for alternative functional forms of sibling ability in columns 2 to 4 does not alter the positive relationship between a child's own ability and educational expenses, with coefficients ranging from 112 to 139 FCFA. Finally, children with two siblings of higher ability have 136 FCFA lower educational expenditures, corresponding to 16.1 percent of discretionary educational expenses.

5.2 Robustness Checks

To test the robustness of our results, we present four tables of regressions where we explore different educational outcomes, use two approaches to address potential reverse causality issues between schooling and cognitive ability, and use alternative cognitive tests to measure child ability. First, in Table 5, we present results for alternative schooling outcomes including ever enrolled in school, on-time school start, and grade progression through school. Results are consistent with those in Table 3 for current enrollment. We use household fixed effects as well as the relative measure of whether a child has one, two, or three or more siblings of higher ability.[18] Relative to the base levels, in the household fixed effects specifications (columns 1, 3, and 5),

[17] School registration fees are not considered since all enrolled children have to pay the same fees. School meal fees, lodging fees, uniforms, and transportation expenses are the other educational expenses that are not included as these have much less variation across siblings within a household.
[18] We also estimate regressions including the highest sibling ability and whether the child has any sibling of higher ability and find consistent results, but due to space limitations we present the limited set of results.

children with one standard deviation higher own ability are 29.5 percent more likely to be ever enrolled, 39 percent more likely to start school on time, and 28.1 percent more likely to progress through school. Children with one sibling of higher ability have lower probability of these outcomes (12 percent lower level of ever being enrolled, 11 percent lower level of starting school on time, and 8 percent lower level of grade progression). Negative effects are larger for children who have two siblings of higher ability (27 percent lower level of ever being enrolled, 24 percent lower level of starting school on time, and 20 percent lower level of grade progression).

Second, in Table 6, we attempt to address the potential reverse causality of schooling affecting a child's cognitive ability by limiting the regression sample to children that are in Grade 2, Grade 1, or not enrolled (columns 1 to 4) and children in Grade 1 or not enrolled (columns 5 to 8) because the regression evidence discussed previously indicates that children in these grades have not yet received enough schooling to influence their cognitive ability test scores. Results for this restricted sample are consistent with those in Table 3. Household fixed effects logit regressions in columns 1 and 5 indicate that within a given household, relative to the base enrollment levels, a child with one standard deviation higher ability is respectively 33 and 36 percent more likely to be enrolled.[19] Children with two siblings of higher ability have a 6.2 or 3.9 percentage point lower probability of enrollment (columns 4 and 8 respectively), corresponding to 17.7 and 17.0 percent of the base level of enrollment.

Third, in Table 7, we further address any potential reverse causality between schooling and cognitive ability by using the ability measure of young children who are not enrolled in 2007-2008 to measure the effect on schooling in 2008-2009. This approach eliminates any potential effect of schooling on the ability measure as these children had never been enrolled at

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[19] Mean enrollment for the sample of children in Grades 2, 1 or not enrolled (columns 1 to 4) is 0.35, while for children in Grade 1 or not enrolled (columns 5 to 8), average enrollment is 0.23.

the time of taking the ability test, so the comparison is for the children not enrolled in year 1 to their siblings that are not enrolled. In columns 1 to 4, we first consider only children ages 5 to 7 and not enrolled in year 1 since many children in Burkina Faso are not enrolled at this young age. Then in columns 5 to 8, we further restrict the sample to only children ages 5 to 6 and not enrolled in year 1 to remove any potential concern that the not-enrolled seven year olds are somehow different than other seven year old children.[20] The household fixed effects logit regressions in columns 1 and 5 indicate that within a given household, a young child with a one standard deviation higher ability measured in year 1 is respectively 19.2 and 20.4 percentage points more likely to be subsequently enrolled in year 2.[21] The coefficient in column 1 is statistically significant at the five percent level. While the coefficient in column 5 is not statistically significant at standard levels, there are only 52 children in the regression as the household fixed effects logit is only identified from households with multiple children ages 5 to 6 who were not enrolled in year 1. Young children 5 to 6 years old who are not enrolled in year 1 and who have two siblings of higher ability who also are not enrolled in year 1 subsequently have a 17.3 percentage point lower probability of enrollment in year 2 (column 8).

Fourth, in Table 8, we present two alternative measures of a child's cognitive ability. To allay any concerns that transforming the Raven's scores into age-adjusted z-scores might have introduced bias, in columns 1 to 4, we estimate regressions using the Raven's raw test score. Results are consistent with those in Table 3. In the household fixed effects specification, within a given household, a child with a one standard deviation higher Raven's raw score (3.35 questions), has an 18.4 percentage point higher likelihood of being enrolled. In columns 5 to 8,

[20] In 2007-2008, 74 percent of children ages 5 to 7 were not enrolled, and of these children 31 percent are then enrolled in 2008-2009. For children 5 to 6 years old, 89 percent of them were not enrolled in 2007-2008, and of these children, 28 percent are then enrolled in the subsequent year.

[21] Mean enrollment in Year 2 for the sample of children ages 5 to 7 (columns 1 to 4) is 0.29, while for children ages 5 to 6 (columns 5 to 8) average enrollment in Year 2 is 0.27.

we also employ an alternative measure of cognitive ability, the WISC Digit Span, to examine the relationship with current enrollment and find results consistent with using the Raven's test. Children with a one standard deviation higher own WISC z-score have a 17 to 22 percentage point higher probability of enrollment, representing 32 to 41 percent of the mean enrollment level. Children with two siblings having a higher WISC z-score have a 14 percentage point lower probability of enrollment (26 percent of the mean level of enrollment).

6. Conclusions

In this paper, we find strong evidence of sibling rivalry when parents make educational investment decisions in rural Burkina Faso. However, in contrast with previous research that generally focuses on easily observable demographic characteristics to measure sibling rivalry, we use measures of a child's own cognitive ability and different specifications of his siblings' abilities to test for how parents make schooling investment decisions. We examine both the extensive margin (school enrollment and grade progression) as well as the intensive margin of discretionary school expenditures. Own ability has a positive effect on educational outcomes, after controlling for individual and family characteristics and when using a family fixed effect specification. We find that within a given household a child with one standard deviation higher ability compared to the average ability of their siblings is 31 percent more likely to be enrolled. Regardless of how we measure sibling ability, we find evidence of sibling rivalry, and our results are particularly strong when we consider relative measures of sibling ability. The magnitude of these impacts is large. For a child that has one higher ability sibling the probability of enrollment declines by 16 percent and having two higher ability siblings lowers enrollment by 30 percent. Our findings are robust to using alternative objective measures of cognitive ability and the parent’s perceptions of a child’s chance of future success and to addressing issues about the potential reverse causality of schooling influencing child ability measures.

Our results can likely be generalized to other developing countries that have not yet achieved universal primary or secondary education and in which parents are deciding whether to send their children to school in a given year. This paper explores the context in which the decision is made during primary school, whereas in other countries the choice may occur later in a child’s education. A more complete understanding of how parents make the educational investment decision is useful for policymakers. Our findings that high ability children within a family are more likely to be enrolled and receive more educational resources suggest that parents focus on getting the most talented children through higher levels of education, rather than spreading some education evenly amongst all of their children. If fixed costs for schooling and non-convexities in the education production function are important factors in the decision to not invest in education equally across all children, then supply-side schooling interventions (such as building schools, reducing class size, or school inputs like textbooks or uniforms) to raise the schooling of all children and achieve the Millennium Development Goals might not be effective if they are not large enough to overcome these non-convexities. These types of policies might raise the schooling of the more talented children rather than the schooling of all children, and so to increase overall education rates, demand side policies might be necessary. The results also point towards additional benefits of early childhood development programs that improve the cognitive ability of children and help them better compete with their siblings.

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Table 1: Summary Statistics of Burkina Faso Social Protection Evaluation (BSPE) Data

Variable:	Mean	Standard Deviation	Percentage of Households with Variation
Household Size	8.88	3.81	
Number of Wives	1.47	0.95	
Number of Siblings	3.81	2.32	
Number of Sisters	1.79	1.50	
Number of Non-Biological Children in Household	0.41	0.89	
Male (Fraction Male)	0.54	0.50	
Age	9.41	2.99	
Birth Order	2.27	1.34	
Proportion Either Parent Ever Enrolled	0.13	0.34	
Log Household Assets	12.36	1.49	
Parent Perception of Chance Child Succeeds in Formal Employment			
Percentage ‘Small’ Chance	25		
Percentage ‘Medium’ Chance	38		
Percentage ‘Large’ Chance	29		
Percentage ‘Very Large’ Chance	8		
Raven’s Raw Test Score	4.86	3.35	
Own Ability (Raven’s age adjusted z-score)	-0.01	1.00	
Average Grades Completed	1.81	2.08	
Proportion Children Currently Enrolled	0.54	0.50	56%
Proportion Children Ever Enrolled	0.59	0.49	54%
Proportion Children with an On-Time Start	0.40	0.49	54%
Grade Progression	0.52	0.48	
Discretionary Education Expenditures (in FCFA)	845	1752	
Number of Households	1507		
Number of Children	4635		

Notes: All summary statistics are based on information for the 4635 children age 5 to 15 in Year 1 unless otherwise noted. Household assets are measured in FCFA (415 FCFA=$1) and the variable is created by taking the log of the sum of household durable goods and livestock. Parent perceptions of the chance their child succeeds in formal employment ranges from 0 to 3, with 0 indicating a small chance and 3 indicating a very large chance, own ability is measured using the Raven’s Colored Progressive Matrices and normed by age (z-score), timely start indicates if the child started school by age 7 or younger, grade progression in school is the child’s grade in school divided by number of years since the child started attending school and ranges from 0 to 1, discretionary education expenditures is the sum of per child expenses for school supplies and parent association fees in FCFA. Summary statistics for grade progression and average grades completed are based on only 4476 and 4633 children respectively due to missing grade data. Data source: Burkina Faso Social Protection Evaluation (BSPE) data from 2008.

Table 2: Marginal Effects from Logit and Conditional Logit Regressions Estimating Relationship between Current School Enrollment, Sibling Rivalry, and Parent Perceptions

Dependent Variable: Current Enrollment	(1)	(2)	(3)	(4)	(5)
Parent Perceptions of Child's Chance of Success in Formal Employment		0.184***	0.162***	0.111***	0.109***
		[0.025]	[0.023]	[0.019]	[0.019]
Highest Sibling Perception			-0.065***		
			[0.019]		
Higher Sibling Dummy (1 if any sibling with a higher perceived chance of success)				-0.070**	
				[0.033]	
One Higher Sibling Dummy (1 if only 1 sibling with a higher perceived chance of success)					-0.053
					[0.036]
Two Higher Sibling Dummy (1 if 2 siblings with a higher perceived chance of success)					-0.075
					[0.048]
Three or More Higher Sibling Dummy (1 if 3 or more siblings with a higher perceived chance of success)					-0.146**
					[0.063]
Number of Siblings	-0.025***		-0.025***	-0.026***	-0.025***
	[0.009]		[0.009]	[0.009]	[0.009]
Number of Sisters	0.022**		0.021**	0.022**	0.022**
	[0.010]		[0.010]	[0.010]	[0.010]
Birth Order	0.014		0.025**	0.023**	0.025**
	[0.009]		[0.010]	[0.010]	[0.010]
Male	0.031*	0.040**	0.026	0.027	0.027
	[0.018]	[0.019]	[0.019]	[0.019]	[0.019]
Parent Schooling (Either parent ever enrolled=1)	0.181***		0.171***	0.166***	0.166***
	[0.040]		[0.045]	[0.044]	[0.044]
Log Household Assets	0.018*		0.018*	0.017*	0.017*
	[0.010]		[0.010]	[0.010]	[0.010]
Age Fixed Effects?	Yes	Yes	Yes	Yes	Yes
Village Fixed Effects?	Yes	No	Yes	Yes	Yes
Household Fixed Effects?	No	Yes	No	No	No
Number of Children	4635	3210	4536	4536	4536

Notes: Robust standard errors in brackets, clustered at village level. * significant at 10%; ** significant at 5%; *** significant at 1%. Columns 1, 3, 4, and 5 present marginal effects for logit regressions. Column 2 presents marginal effects from a household fixed effects conditional logit regression. Regressions are restricted to children age 5 to 15, and number of siblings and number of sisters are for all siblings and sisters in the household. Regression sample includes 4635 children, of which 4536 have parent perception measures and 3210 have siblings with differing outcomes and parent perception measures. Data source: Burkina Faso Social Protection Evaluation (BSPE) data from 2008.

Table 3: Marginal Effects from Logit and Conditional Logit Regressions Estimating Relationship between Current School Enrollment and Child Ability

Dependent Variable: Current Enrollment	(1)	(2)	(3)	(4)
Own Ability (Raven's age adjusted z-score)	0.165***	0.147***	0.095***	0.085***
	[0.017]	[0.014]	[0.016]	[0.016]
Highest Sibling Ability		-0.068***		
		[0.013]		
Higher Sibling Dummy (1 if any sibling with an ability > own ability)			-0.110***	
			[0.024]	
One Higher Sibling Dummy (1 if only 1 sibling with an ability > own ability)				-0.086***
				[0.025]
Two Higher Sibling Dummy (1 if 2 siblings with an ability > ability)				-0.161***
				[0.032]
Three or More Higher Sibling Dummy (1 if 3 or more siblings with an ability > own)				-0.177***
				[0.041]
Number of Siblings		-0.019**	-0.023***	-0.018**
		[0.009]	[0.009]	[0.009]
Number of Sisters		0.018*	0.020**	0.019**
		[0.010]	[0.010]	[0.010]
Birth Order		0.028***	0.025***	0.029***
		[0.009]	[0.009]	[0.010]
Male	0.026	0.019	0.019	0.018
	[0.020]	[0.020]	[0.020]	[0.020]
Parent Schooling (Either parent ever enrolled=1)		0.182***	0.181***	0.180***
		[0.041]	[0.041]	[0.041]
Log Household Assets		0.018*	0.019**	0.020**
		[0.010]	[0.010]	[0.010]
Age Fixed Effects?	Yes	Yes	Yes	Yes
Village Fixed Effects?	No	Yes	Yes	Yes
Household Fixed Effects?	Yes	No	No	No
Number of Children	2861	4635	4635	4635

Notes: Robust standard errors in brackets, clustered at village level. * significant at 10%; ** significant at 5%; *** significant at 1%. Column 1 presents marginal effects from a household fixed effects conditional logit regression. Columns 2 to 4 present marginal effects for logit regressions. Regressions are restricted to children age 5 to 15, and number of siblings and number of sisters are for all siblings and sisters in the household. Regression sample includes 4635 children, with 2861 having siblings with differing enrollment outcomes. Own and sibling ability are measured using the Raven's Colored Progressive Matrices and normed by age (z-score). Data source: Burkina Faso Social Protection Evaluation (BSPE) data from 2008.

Table 4: OLS Regressions Estimating Relationship between Discretionary Education Expenditures and Child Ability, Only Enrolled Children

Dependent Variable: Discretionary education expenditures on supplies and parent associations fees (FCFA)	(1)	(2)	(3)	(4)
Own Ability (Raven's age adjusted z-score)	169.53***	139.23**	123.26*	112.35
	[54.60]	[55.71]	[70.35]	[75.67]
Highest Sibling Ability		-36.25		
		[40.94]		
Higher Sibling Dummy (1 if any sibling in household with an ability > own ability)			-10.74	
			[65.65]	
One Higher Sibling Ability Dummy (1 if only 1 sibling in household with an ability > own ability)				26.72
				[70.90]
Two Higher Sibling Ability Dummy (1 if 2 siblings in household with an ability > ability)				-136.03*
				[76.93]
Three or More Higher Sibling Ability Dummy (1 if 3 or more siblings in household with an ability > own)				-46.74
				[140.12]
Number of Siblings		27.23	24.06	27.63
		[34.28]	[33.40]	[35.33]
Number of Sisters		-69.66*	-68.48*	-69.65*
		[40.02]	[39.92]	[40.18]
Birth Order		-19.96	-24.38	-16.57
		[38.86]	[39.80]	[37.71]
Male	-82.35	-62.57	-61.95	-61.85
	[84.72]	[67.57]	[68.16]	[67.92]
Parent Schooling (Either parent ever enrolled=1)		257.47**	256.14**	255.01**
		[116.486]	[116.79]	[116.99]
Log Household Assets		12.42	11.95	12.54
		[25.83]	[25.58]	[25.93]
Age Fixed Effects?	Yes	Yes	Yes	Yes
Village Fixed Effects?	No	Yes	Yes	Yes
Household Fixed Effects?	Yes	No	No	No
Number of Children	1994	2511	2511	2511

Notes: Robust standard errors in brackets, clustered at village level. * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions are restricted to children ages 5 to 15 who are currently enrolled in school. Discretionary education expenditures are the sum of per child expenses for school supplies and other parent association fees in FCFA, with a mean of 845 FCFA. The regression in column 1 includes household fixed effects and the sample is restricted to children in households with at least 2 enrolled children. Columns 2 to 4 include village fixed effects and the sample is restricted to children who are currently enrolled. Own and sibling ability are measured using the Raven's Colored Progressive Matrices and normed by age (z-score). Data source: Burkina Faso Social Protection Evaluation (BSPE) data from 2008.

Table 5: Marginal Effects from Logit and Conditional Logit Regressions Estimating Relationship between Alternative Schooling Outcomes and Child Ability

Dependent Variable:	Ever Enrolled (1)	Ever Enrolled (2)	On Time Start (3)	On Time Start (4)	Grade Progress (5)	Grade Progress (6)
Own Ability (Raven's age adjusted z-score)	0.174***	0.079***	0.156***	0.071***	0.146***	0.052***
	[0.017]	[0.014]	[0.018]	[0.014]	[0.015]	[0.010]
One Higher Sibling Dummy (1 if only 1 sibling with an ability > own ability)		-0.073***		-0.044**		-0.043***
		[0.023]		[0.020]		[0.016]
Two Higher Sibling Dummy (1 if 2 siblings with an ability > own ability)		-0.162***		-0.094***		-0.106***
		[0.028]		[0.027]		[0.019]
Three or More Higher Sibling Dummy (1 if 3 or more siblings with an ability > own ability)		-0.167***		-0.130***		-0.120***
		[0.041]		[0.034]		[0.029]
Number of Siblings		-0.016*		-0.008		-0.012**
		[0.009]		[0.008]		[0.006]
Number of Sisters		0.016		0.016		0.017***
		[0.011]		[0.010]		[0.006]
Birth Order		0.024**		-0.007		0.016**
		[0.010]		[0.010]		[0.007]
Male	0.040*	0.033*	0.010	0.001	0.048**	0.028*
	[0.022]	[0.019]	[0.023]	[0.017]	[0.021]	[0.014]
Parent Schooling (Either parent ever enrolled=1)		0.203***		0.160***		0.120***
		[0.047]		[0.039]		[0.027]
Log Household Assets		0.023**		0.027***		0.014**
		[0.010]		[0.008]		[0.006]
Age Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes
Village Fixed Effects?	No	Yes	No	Yes	No	Yes
Household Fixed Effects?	Yes	No	Yes	No	Yes	No
Number of Children	2751	4635	2716	4635	2584	4476

Notes: Robust standard errors in brackets, clustered at village level. * significant at 10%; ** significant at 5%; *** significant at 1%. Columns 1 and 3 present marginal effects for a household fixed effects conditional logit regression. Columns 2 and 4 present marginal effects for logit regressions. Column 5 uses OLS and includes household fixed effects, while column 6 includes village fixed effects. Regressions are restricted to children age 5 to 15. On-time start indicates if the child started school by age 7 or younger, grade progression is the child's grade in school divided by number of years since the child started school and ranges from 0 to 1. Own and sibling ability are measured using the Raven's CPM and normed by age. Regression sample includes 4635 children who also have siblings in their household. Due to missing grade information, sample size in column 6 is 4476. Data source: Burkina Faso Social Protection Evaluation (BSPE) data from 2008.

Table 6: Marginal Effects from Logit and Conditional Logit Regressions Estimating Relationship between Current School Enrollment and Child Ability, Restricted to Children in Grades 2 or Lower

Dependant Variable: Current Enrollment	Grade 2, 1 or Not Enrolled				Grade 1 or Not Enrolled			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Own Ability (Raven's age adjusted z-score)	0.114***	0.065***	0.048***	0.044***	0.083**	0.026***	0.021*	0.018
	[0.026]	[0.012]	[0.014]	[0.015]	[0.040]	[0.010]	[0.011]	[0.012]
Highest Sibling Ability		-0.025**				-0.007		
		[0.010]				[0.009]		
Higher Sibling Dummy (1 if any sibling with an ability > own ability)			-0.035				-0.010	
			[0.021]				[0.016]	
One Higher Sibling Dummy (1 if only 1 sibling with an ability > own ability)				-0.024				-0.000
				[0.023]				[0.017]
Two Higher Sibling Dummy (1 if 2 siblings with an ability > own ability)				-0.062**				-0.039**
				[0.026]				[0.020]
Three or More Higher Sibling Dummy (1 if 3 or more siblings with an ability > own ability)				-0.053				-0.012
				[0.044]				[0.034]
Number of Siblings		-0.021**	-0.023***	-0.021**		-0.017***	-0.018***	-0.017***
		[0.008]	[0.008]	[0.009]		[0.006]	[0.006]	[0.006]
Number of Sisters		0.001	0.002	0.002		0.005	0.005	0.005
		[0.009]	[0.009]	[0.009]		[0.006]	[0.006]	[0.006]
Birth Order		0.027***	0.026**	0.028***		0.020**	0.019**	0.020***
		[0.011]	[0.011]	[0.011]		[0.008]	[0.008]	[0.008]
Male	0.036**	0.008	0.009	0.008	0.044	-0.001	-0.001	-0.000
	[0.041]	[0.019]	[0.019]	[0.019]	[0.048]	[0.014]	[0.014]	[0.014]
Household Characteristics?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Village Fixed Effects?	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Household Fixed Effects?	Yes	No	No	No	Yes	No	No	No
Number of Children	1409	3118	3118	3118	730	2548	2548	2548

Notes: Robust standard errors in brackets, clustered at village level. * significant at 10%; ** significant at 5%; *** significant at 1%. Sample in columns 1 to 4 includes 3118 children in Grades 2 or lower, with 1409 having siblings with differing enrollment outcomes. Sample in columns 5 to 8 includes 2548 children in Grade 1 or lower, with 730 having siblings with differing enrollment outcomes. Columns 1 and 5 present marginal effects from a household fixed effects conditional logit regression. Columns 2 to 4 and 6 to 8 present marginal effects for logit regressions. Regressions are restricted to children age 5 to 15, and number of siblings and number of sisters are for all siblings and sisters in the household. Mean enrollment for the sample of children in Grades 2 or lower is 0.35 while for children in Grade 1 or lower is 0.23. Own and sibling ability are measured using the Raven's CPM and normed by age. Data source: Burkina Faso Social Protection Evaluation (BSPE) data from 2008.

Table 7: Marginal Effects from Logit and Conditional Logit Regressions Estimating Relationship between School Enrollment in Year 2 and Child Ability Measured in Year 1

Dependent Variable: Current Enrollment Year 2	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Own Ability (Raven's age adjusted z-score)	0.192**	0.060**	0.037	0.031	0.204	0.050	0.009	0.005
	[0.092]	[0.028]	[0.028]	[0.028]	[0.156]	[0.034]	[0.033]	[0.032]
Highest Sibling Ability		-0.038*				-0.069**		
		[0.021]				[0.029]		
Higher Sibling Dummy (1 if any sibling with an ability > own ability)			-0.043				-0.080	
			[0.040]				[0.050]	
One Higher Sibling Dummy (1 if only 1 sibling with an ability > own ability)				-0.020				-0.049
				[0.043]				[0.048]
Two Higher Sibling Dummy (1 if 2 siblings with an ability > own ability)				-0.122				-0.173*
				[0.076]				[0.101]
Three or More Higher Sibling Dummy (1 if 3 or more siblings with an ability > own ability)				-0.135				-0.152
				[0.094]				[0.095]
Number of Siblings		-0.034	-0.038*	-0.033		-0.029	-0.033	-0.030
		[0.021]	[0.021]	[0.021]		[0.022]	[0.023]	[0.023]
Number of Sisters		0.010	0.013	0.012		0.002	0.005	0.002
		[0.024]	[0.024]	[0.025]		[0.030]	[0.031]	[0.031]
Birth Order		0.060**	0.060**	0.062**		0.076***	0.075***	0.079***
		[0.026]	[0.027]	[0.027]		[0.026]	[0.027]	[0.027]
Male	-0.183	-0.078*	-0.079*	-0.082*	-0.16	-0.082	-0.084	-0.088*
	[0.129]	[0.044]	[0.044]	[0.043]	[0.197]	[0.056]	[0.056]	[0.053]
Household Characteristics?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Village Fixed Effects?	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Household Fixed Effects?	Yes	No	No	No	Yes	No	No	No
Number of Children	123	643	643	643	52	442	442	442

Notes: Robust standard errors in brackets, clustered at village level. * significant at 10%; ** significant at 5%; *** significant at 1%. Columns 1 and 5 present marginal effects from a household fixed effects conditional logit regression. Columns 2 to 4 and 6 to 8 present marginal effects for logit regressions. Regressions in columns 1 to 4 are restricted to children age 5 to 7 who were not enrolled during Year 1 and in columns 5 to 8 are restricted to children age 5 to 6 who were not enrolled during Year 1. Sample includes 643 children ages 5 to 7, with 123 having siblings with differing enrollment outcomes. Sample includes 442 children ages 5 to 6, with 52 having siblings with differing enrollment outcomes. Mean enrollment in Year 2 for the sample of children ages 5 to 7 (columns 1 to 4) is 0.29, while for children ages 5 to 6 (columns 5 to 8) average enrollment in Year 2 is 0.27. Sibling ability measures are for all siblings not enrolled during Year 1. Own and sibling ability are measured using the Raven's CPM and normed by age (z-score). Data source: Burkina Faso Social Protection Evaluation (BSPE) data from 2008 and 2009.

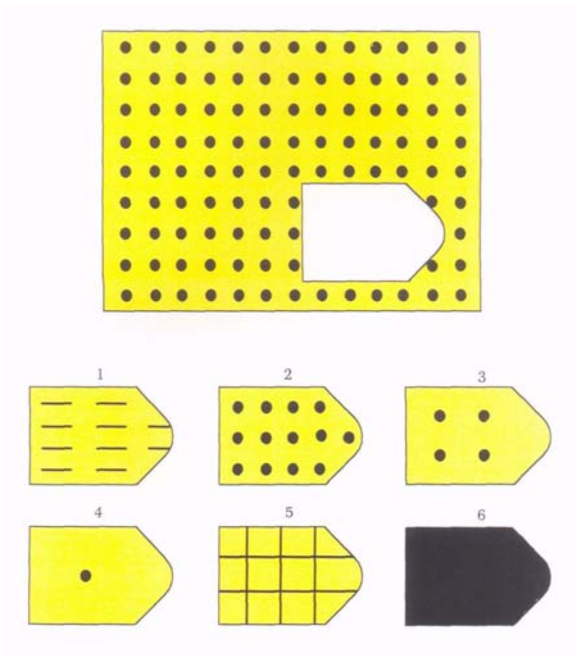
Table 8: Marginal Effects from Logit and Conditional Logit Regressions Estimating Relationship between Current School Enrollment and Alternative Child Ability Measures

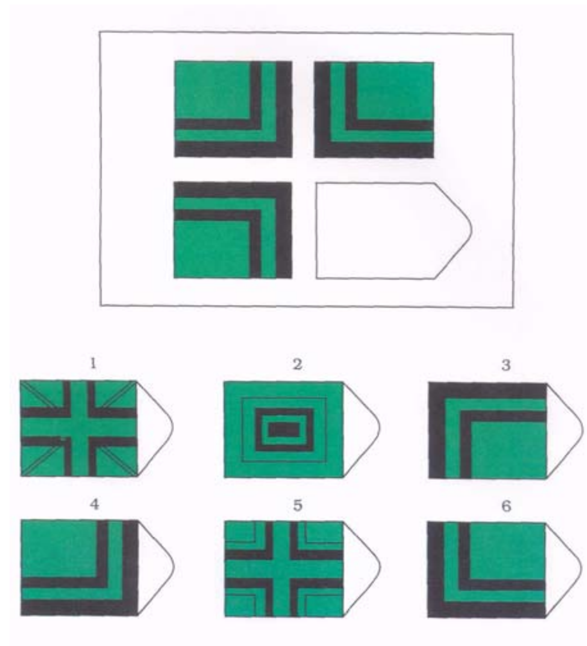
Dependant Variables: Current Enrollment	Raven's (raw score)				WISC Digit Span (z-score by age)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Own Ability [Raven's raw, WISC age adjusted z-score]	0.055***	0.045***	0.035***	0.035***	0.221***	0.221***	0.180***	0.171***
	[0.006]	[0.005]	[0.005]	[0.005]	[0.028]	[0.022]	[0.024]	[0.025]
Highest Sibling Ability [Raven's raw score, WISC age adjusted z-score]		-0.010***				-0.043**		
		[0.004]				[0.018]		
Higher Sibling Dummy (1 if any sibling has an ability > own ability) [Raven's raw, WISC age adjusted z-score]			-0.085***				-0.115***	
			[0.022]				[0.031]	
One Higher Sibling Dummy (1 if only 1 sibling with an ability score > own score) [Raven's raw, WISC age adjusted z-score]				-0.080***				-0.100***
				[0.024]				[0.029]
Two Higher Sibling Dummy (1 if 2 siblings with an ability score > own score) [Raven's raw, WISC age adjusted z-score]				-0.104***				-0.138***
				[0.032]				[0.043]
Three or More Higher Sibling Dummy (1 if 3 or more siblings with an ability score > own score) [Raven's raw, WISC age adjusted z-score]				-0.095***				-0.191***
				[0.037]				[0.052]
Number of Siblings		-0.025***	-0.026***	-0.025***		-0.020**	-0.020**	-0.015*
		[0.009]	[0.009]	[0.009]		[0.009]	[0.009]	[0.009]
Number of Sisters		0.020**	0.020**	0.020**		0.014	0.014	0.014
		[0.010]	[0.010]	[0.010]		[0.011]	[0.011]	[0.010]
Birth Order		0.031***	0.030***	0.032***		0.022**	0.022**	0.027**
		[0.010]	[0.010]	[0.010]		[0.011]	[0.011]	[0.011]
Male	0.025	0.020	0.021	0.021	0.045**	0.018	0.020	0.019
	[0.020]	[0.020]	[0.020]	[0.020]	[0.021]	[0.020]	[0.020]	[0.020]
Age Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Village Fixed Effects?	No	Yes	Yes	Yes	No	Yes	Yes	Yes
Household Fixed Effects?	Yes	No	No	No	Yes	No	No	No
Number of Children	2861	4635	4635	4635	2843	4463	4463	4463

Notes: Robust standard errors in brackets, clustered at village level. * significant at 10%; ** significant at 5%; *** significant at 1%. Columns 1 and 5 present marginal effects from a household fixed effects conditional logit regression. Columns 2 to 4 and 6 to 8 present marginal effects for logit regressions. Columns 1 to 4 calculate ability measures using the Raven's CPM raw score. Columns 5 to 8 calculate ability measures using the WISC Digit Span normed by age (z-score). Sample sizes vary due to missing WISC Digit Span data. All regressions are restricted to children age 5 to 15 and also include household level controls for parent schooling and assets. Data source: Burkina Faso Social Protection Evaluation (BSPE) data from 2008.

Figure 1: Example Problems from the Raven's Colored Progressive Matrices and WISC Digit Span Tests

Panel A : Raven's Colored Progressive Matrices





Correct Response: Option 2 | Correct Response: Option 3

Panel B: WISC Digit Span

Question	Correct Response
Digit Span Forward:	
"8-2"	"8-2"
"5-1-7-4-2-3-8"	"5-1-7-4-2-3-8"
Digit Span Backward:	
"8-2"	"2-8"
"1-6-5-2-9-8"	"8-9-2-5-6-1"