Child Labor, Schooling, and Child Ability [*]

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Abstract

Using data we collected in rural Burkina Faso, we examine how children's cognitive abilities influence households' decisions to invest in their education. To address the endogeneity of child ability measures, we use rainfall shocks experienced in utero or early childhood to instrument for ability. Negative shocks in utero lead to 0.24 standard deviations lower ability z-scores, corresponding with a 38 percent enrollment drop and a 49 percent increase in child labor hours compared to their siblings. Negative education impacts are largest for in utero shocks, diminished for shocks before age two, and have no impact for shocks after age two. We link the fetal origins hypothesis and sibling rivalry literatures by showing that shocks experienced in utero not only have direct negative impacts on the child's cognitive ability (fetal origins hypothesis) but also negatively impact the child through the effects on sibling rivalry resulting from the cognitive differences.

Keywords: Fetal origins hypothesis; Education; Child Labor; Sibling Rivalry
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1. Introduction

Based on 2007 data, only 76 percent of primary school-aged children were enrolled in sub-Saharan Africa, which is the lowest net enrollment rate of all developing country regions (United Nations 2010).[1] The estimated 31 million not enrolled African children represent over 45 percent of the world's total of out-of-school children. Over half of the countries in sub-Saharan Africa do not provide legal guarantees for free primary school education. In settings without universal primary education and where most children engage in child labor activities, as is true in many African countries,[2] understanding how parents decide how to allocate limited resources across siblings is important as these decisions have long-term impacts on each child's future earnings, health, and overall welfare. Schooling decisions depend on parent perceptions about the returns to school for a child, the opportunity cost of sending that child to school, the child's ability, and the presence and characteristics of the child's siblings. To achieve the Millennium Development Goal of universal primary education, it is critical to understand these links between child ability, school enrollment, and child labor.

An extensive literature examines the source of inequalities for children's educational investments within a household building on seminal work by Becker and Tomes (1976) that highlights the tradeoff between child quantity and their 'quality.' In making decisions about schooling and child labor, parents have information about their children's abilities that is often not available to researchers, a fact that partly explains why much of the empirical research on the

[1] This compares with an average net enrollment rate of 89 percent in developing regions and 96 percent in developed regions. These enrollment rates in Africa still represent an enormous improvement since 1998 when African net enrollment rates were only 58 percent. In Burkina Faso, the focus of this paper, the net enrollment rate was 35 percent in 2000 and improved to 55 percent by 2007 (UN Statistics Division). Based on this paper's data from Burkina Faso, only 24 percent of households with multiple primary school aged children enroll all of their children.
[2] Based on the UNICEF Multiple Indicator Cluster Survey End of Decade Assessment, almost three-fourths of all rural primary school-aged children in developing countries were involved with some type of work that included helping with the family farm or business or providing domestic household services such as cooking, sweeping, doing dishes, or fetching water (Edmonds and Pavcnik 2005).

determinants of parental investments in children focuses on easy-to-observe demographic characteristics of the child such as gender, birth order, or family composition (Parish and Willis 1993; Garg and Morduch 1998; Black, Devereux, and Salvanes 2005; Emerson and Souza 2008).[3] More recent papers attempt to use direct measurements of a child's ability such as IQ scores (Kim 2005), achievement tests (Glick and Sahn 2010), or cognitive tests (Ayalew 2005) to better understand which factors influence investment decisions.

The purpose of this paper is to examine the role that a child's cognitive ability plays in a household's decision to invest in that child's education or to use the child for labor activities. However, the difficulty with using cognitive ability measured in a survey at the same time as outcomes is that ability may reflect the accumulation of previous investments and parental preferences throughout the child's life. To address this potential endogeneity problem, we use an instrumental variables strategy in our within-household estimates of sibling rivalry that allows for a causal estimate of the impact of ability on both schooling and child labor decisions. Specifically, we use rainfall shocks in the child's village that were experienced in utero or during early childhood to instrument for a child's cognitive ability. In a setting where most parents are rain-fed subsistence farmers, negative rainfall is an exogenous shock that reduces the harvest and amount of food and nutrition available over the following year. The fetal origins hypothesis, often associated with Barker (1998), argues that health and nutrition shocks suffered in utero can cause irreversible adaptations to the local food environment and that children cannot catch up

[3] 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) and risky behaviors (Aizer 2004). Research looking explicitly at child labor generally finds negative effects of work on human capital accumulation (see Boozer and Suri (2001) and Beegle, Dehejia, and Gatti (2009) for specific examples from Ghana and Vietnam and Edmonds (2008) for an extensive review of the child labor literature). While this negative schooling-child labor relationship is consistently found in the literature, it is sensitive to the definition of work (see Levison, Moe, and Knaul (2001) documenting that a tradeoff between schooling and work depends on whether work includes domestic work). For a discussion of the theoretical background on child labor decisions, see Basu (1999).

even if they later have good nutrition and health care. These conditions experienced in utero or early in life have been shown to have persistent and long-term effects on health, education, and socioeconomic outcomes (see seminal work by Stein et al. (1975) and more recent papers by Alderman, Hoddinott, and Kinsey (2006), Almond (2006), and Bundervoet, Verwimp, and Akresh (2009) that study extreme shocks such as droughts, epidemics, or civil wars, or papers by Behrman and Rosenzweig (2004), Oreopoulos et al. (2008), and Royer (2009) that use twin comparisons).[4] Knudsen (2004) highlights that different age periods during a child's early development are critical for cognitive development. Recent work focuses on positive shocks during these critical childhood ages and finds improvements in cognitive ability due to nutritional supplementation in Guatemala (Maluccio et al. 2009), iodine supplementation in Tanzania (Field, Robles, and Torero 2009), a de-worming intervention in Kenya (Ozier 2011), cash transfers in Nicaragua (Macours, Schady, Vakis 2011), and child health interventions in the Matlab study area of Bangladesh (Barham 2011).

This paper makes three main contributions to the literature. First, by using an instrumental variables strategy in our sibling rivalry estimations, this is the first paper that is able to present causal estimates of the impact of cognitive ability on education and child labor outcomes. We combine this instrumental variables approach with an empirical identification strategy that focuses on within-household estimates of the relationship between ability and schooling and labor outcomes; this allows us to control for household factors that are constant across siblings and propose a sibling ability rivalry explanation for our results. Furthermore, unlike previous sibling rivalry studies, our explanation does not depend on assumptions about

[4] See Almond and Currie (2011) for a review of the fetal origins hypothesis literature, Strauss and Thomas (2008) for a review of the link between early childhood health and later life outcomes, and Grantham-McGregor, Fernald, and Sethuraman (1999) for a review of the literature measuring the short and long-term effects of malnutrition and micronutrient deficiencies. Despite much of the literature focusing on extreme shocks, Maccini and Yang (2009) find evidence in Indonesia of negative long-term impacts due to much smaller rainfall shocks.

gender differences in returns to education (Morduch 2000) or in home production (Edmonds 2006). Second, our first stage estimates that provide strong evidence of the link between negative shocks experienced in utero or during early childhood on subsequent cognitive ability contribute to the fetal origins hypothesis literature by highlighting a potential mechanism through which observed impacts on adult outcomes can be explained. We are also able to estimate placebo regressions showing that rain shocks prior to when the child was in utero show no relationship with ability, further strengthening our interpretation of a causal relationship. Third, because of the unique household survey data collected by the authors, this paper is the first to examine the relationship between cognitive ability and child labor, focusing not only on total hours of child labor, but also changes in the specific tasks that each child performs.[5]

We find that higher ability children, compared to their lower ability siblings, are more likely to be enrolled in school and perform fewer hours of child labor. We also find the child labor they do is less concentrated in tasks requiring long, continuous blocks of time to complete. Rainfall more than one standard deviation below the village's long-run historical average that is experienced by a child in utero is correlated with 0.24 standard deviations lower cognitive ability z-scores and, compared to their siblings, that corresponds to a 38 percent lower likelihood of enrollment and a 49 percent increase in the number of hours of child labor. The negative enrollment impacts are largest for children who experience a rain shock while in utero, and while

[5] The current paper builds on previous research (Akresh et al., 2012), which also explores the relationship between a child's cognitive ability and educational investments, but there are three significant differences between the current paper and the earlier work. First, the econometric identification strategy in the previous work only allows for an examination of correlations between child ability and enrollment, while in this paper we are able to measure a causal impact of ability on schooling by implementing an instrumental variables approach using the rain shock instruments. Second, in contrast with the previous paper that focuses exclusively on sibling rivalry, the current paper links the literature on the fetal origins hypothesis with the literature on sibling rivalry by showing that shocks experienced in utero or early childhood not only have direct negative impacts on the child's cognitive ability (fetal origins hypothesis), but also negatively impact the child through the effects on sibling rivalry resulting from the cognitive differences. Third, while the previous research is only concerned with schooling outcomes, in the current paper we also examine the relationship between ability and child labor.

these effects diminish in magnitude for shocks experienced during the first two years of life, they are still significant and economically meaningful. We find no relationship between rainfall shocks and ability for shocks experienced after age two or before the child was in utero.

The remainder of the paper is organized as follows. Section 2 describes the survey and rainfall data used in the analysis. Section 3 presents our empirical identification strategy and Section 4 presents the main results as well as robustness tests. Section 5 concludes.

2. Nahouri Cash Transfer Pilot Program (NCTPP) Survey and Rainfall Data

2.1 Nahouri Cash Transfer Pilot Program

The Burkina Faso Nahouri Cash Transfer Pilot Program (NCTPP) survey was conducted in June 2008 in Nahouri province in southern Burkina Faso, approximately 100 miles from the capital and bordering Ghana. The survey was the baseline for an ongoing project evaluating social protection strategies in Burkina Faso. Households were randomly selected from a village-level census conducted by our project team immediately prior to the survey in the 75 rural villages of Nahouri that each has a primary school. Households in this region are predominantly rain-fed subsistence farmers growing sorghum and groundnuts and have mean annual per capita expenditures of around $90.

Our primary analysis focuses on school-aged children 5 to 15 who are biological children of the household head in households with multiple children in this age range and with varying enrollment. There are 2,862 children in 893 different households. As shown in Table 1, 53 percent of this child sample is male and the average age is 9.3 years old. These children live in households with an average of 9.29 individuals, including a head of household, 1.53 wives, 3.97 biological children of the household head ages 5 to 15, 1.25 biological children of the household head under age 5, 0.36 children under age 15 that are not the biological children of the head, and

1.18 other members that include grandparents, aunts, uncles, and other extended family members. Only 12 percent of the sampled children have a parent that ever attended school.

We use the Raven's Colored Progressive Matrices (CPM) to measure a child's cognitive ability. The Raven's CPM is a measure of fluid intelligence or problem solving ability, and it does not require formal schooling to be able to answer the questions (Raven, Raven, and Court 1998). The test does not depend heavily on verbal skills, making it relatively "culture free" (Borghans, Duckworth, Heckman, and Weel 2008). In the Raven's test, the child respondent is asked to select the image that is missing in order to complete a picture. This type of question is novel to the children in Nahouri province, thus providing a truer measure of problem solving skills. We ask 18 questions from the Raven's CPM and on average, children in our sample answer 4.7 questions correctly. Younger children answer fewer questions correctly than older children (the average number correct for children age 5 is 2.7 and for children age 15 is 7.3).[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.

In Table 1, we present summary statistics about children's schooling. Few households in rural Burkina Faso ever enroll all of their children. For households with at least two primary school-aged children, 59 percent of these households experience variation in enrollment among their children, 17 percent of these households currently enroll none of their children, and only 24

[6] 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). The average number of questions answered correctly for children ages 6, 7, 8, 9, 10, 11, 12, 13, and 14 is respectively 2.6, 3.6, 4.5, 5.0, 5.5, 5.8, 6.0, 6.4, and 6.3.
[7] We do 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. Note that in Section 4.3, to test the robustness of the results using the Raven's age-adjusted z-score, we estimate alternative specifications using the Raven's raw test score and results are consistent.

percent of these households currently enroll all of their primary school-aged children. Focusing on the 59 percent of households with variation in enrollment outcomes, which is our baseline sample, 51 percent of children in these households are enrolled in the current school year.[8] If we consider whether a child has ever been enrolled in school rather than current enrollment, then 56 percent of children in these households have ever been enrolled. Given these low enrollment rates, on average these children have only completed 1.7 years of school.

Besides enrollment, we also explore two other school-related outcomes (grade progress and starting school at a late age) where sibling ability rivalry might matter. First, we calculate grade progression through school by dividing the child's highest grade attended by the number of years since the child started school. The grade progress measure ranges from zero to one, with higher numbers indicating quicker progress towards completing primary school.[9] Second, we define a starting school late variable that equals 1 if the child started school after age 7 or never attended school (late start), takes the value 0 if the child started school at age 7 (on-time start), and takes the value -1 or -2 if the child started school at ages 6 or 5, respectively (early start).

In our analysis of child labor activities, we examine both the extensive (whether a child does a specific activity) and intensive margins (how many hours doing an activity). The survey collected information for each child about whether, during the two days preceding the survey when school was in session, the child engaged in different types of child labor activities in the household or on the farm, and if they participated in an activity, during what period of the day they did so. In this rural setting most people do not have watches, and we found during pre-testing of the survey instrument that asking about the period during the day for the activity,

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[8] In Section 3.2, we discuss in detail first why we initially restrict the baseline analysis to the sample of households that have within-family variation in enrollment outcomes and second what are the drawbacks of focusing on this selected sample of households that have variation in child schooling. In Section 4.3, we present results incorporating all households and confirm that our main hypotheses about sibling rivalry still hold.

[9] For children who never attended school, we assign a grade progression measure of zero.

rather than the number of hours, was a more accurate way of understanding how a child's time is allocated. These time periods use the school schedule as a reference period and ask whether a child did a specific activity before school opened, during school hours, during school lunch break, after school closed, all day, or occasionally throughout the day. We use these periods to calculate the number of hours a child engaged in child labor activities.[10] The specific child labor activities we focus on in this paper include: household chores such as cooking, fetching water, sweeping, and doing dishes; tending for siblings or sick members; and engaging in farm labor.

Over 70 percent of children engage in child labor activities and spend on average 5.5 hours per day on them. Sixty percent of children perform household chores, and on average, over 3 hours per day are spent on these chores. There is significant variation in the amount of time spent on chores, suggesting that while the majority of children do them, there are some that are spending an entire day doing so, while other spend a limited time. The average number of hours spent tending for siblings or sick members in the household is 0.6 hours. Twenty-six percent of children engage in this activity, and for those children doing the activity, they spend over 2 hours per day doing so. On average one hour is spent on farm labor and 30 percent of children engage in this activity. For those working on a farm, they do so for an average 3 hours per day.

2.2 Rainfall and ArcGIS Interpolation

To estimate our instrumental variables strategy described in Section 3.2, we need rainfall data for each of the 75 survey villages for each year from 1992 to 2003, which corresponds with the in utero and early childhood years for the primary school-aged children in our survey. Since none of the survey villages have rain station data, but there are rain stations in and around Nahouri

[10] Since the school day starts at 7:30am, ends at 5:00pm, and includes a lunch break of 2.5 hours during which children return home and another 30 minute break at school, we assign the following number of hours to correspond to each time period: 2 (before school opened), 6 (during school), 2 (during school lunch break), 2 (after school closed), 10 (all day), 0.5 (occasionally). To check the robustness of the correspondence between number of hours and time periods during the day, we also estimate several alternative variations, and results are consistent.

province, we use an interpolation procedure that enables us to use these nearby rain stations to estimate rainfall in each village for each year. We purchased rain station data from the Burkina Faso Direction de la Météorologie and from the Ghana Meteorological Agency for the 50 rain stations within 100 kilometers from any of our survey villages (26 in Burkina Faso and 24 in Ghana). On average across the survey villages, the nearest rain station is 9.6 kilometers away.

With the data from the 50 rain stations, we use ArcGIS (geographic information system) to implement an inverse distance weighted (IDW) interpolation procedure, which uses the measured rain data from the stations surrounding a given survey village to predict rainfall in the village. Those rain stations that are closest to the village will have more influence on the predicted value by being weighted more heavily, while those further away will have less influence.[11] The intuition behind the IDW procedure is that locations closer together should have more similar rainfall patterns than those further apart, and therefore in estimating the village's rainfall, additional weight is put on those closer rain stations.[12] For several reasons, we believe the IDW interpolation procedure provides a more accurate estimated rainfall measure than alternatives such as using the nearest rain station. First, in much of West Africa, there is a strong north-south rain gradient with locations in the north generally receiving less rain than those areas further south. If we were to use only the nearest station and that station were further south than the village of interest, then we would potentially overestimate rainfall in the village. On the other hand, if the closest station were north, then we might underestimate actual rainfall. By using the rain stations in all directions surrounding a given village, this interpolation procedure allows us to account for this rain gradient. Second, if we use the closest rain station to a given village, only

[11] Weights are proportional to the inverse distance squared, which is the power that minimizes the root mean squared predicted error and therefore determines the smoothness of the interpolation results. Intuitively, as the distance from the village to the rain station increases, the weight in the interpolation procedure decreases extremely rapidly.

[12] The topography around Nahouri province and the surrounding rain stations consists of gently rolling hills without any significant topographical features such as mountains that might create areas with a discontinuity in rainfall.

four rain stations would be used for 71 of the 75 survey villages. However, there are other rain stations near a village, often in a different direction but slightly further away (for instance, in one village the nearest rain station is 13.45 kilometers and the second closest station is 13.98 kilometers and two other stations are within 18 kilometers). Restricting our analysis to the closest rain station would be discarding valuable information about rainfall patterns around these villages. In contrast, by using information from all rain stations within 100 kilometers of the survey villages and using the IDW interpolation procedure to put more weight on those stations closer to a survey village, we gain a more accurate measure of actual rainfall in each village.

During the years when the sampled children were in utero or early childhood and the 15 years prior to their births, annual rainfall across the villages ranges from 503 to 1388 millimeters, with an mean annual rainfall of 899 millimeters and a standard deviation of 137 millimeters (Table 1). Focusing on the variation within a village over this time period, the standard deviation within a village ranges from 98 to 190 millimeters indicating large year-to-year rainfall fluctuations. To determine if a child was exposed to particularly low rainfall, we compare rainfall in the child's village while in utero or early childhood with that village's historical mean rainfall for the 15 year period (1976-1991) prior to when any of these children were born. Because of our emphasis on the link between low rainfall and poor harvests and the subsequent impact on a child's cognitive ability, we focus on extreme negative shocks and define a negative shock to be rainfall one standard deviation or more below the historical average for a given village.

3. Empirical Identification Strategy

3.1 Previous Sibling Rivalry Research

Sibling rivalry is the idea that within a family, siblings compete for limited resources. If constraints such as credit, capital, or labor bind, all else equal, a child with fewer comparatively higher valued siblings will be better off. Historically, the sibling rivalry literature for developing countries has focused on sibling sex composition and the number of sisters a child has. This is because in societies with a pro-male bias, investments in girls generally have lower returns than boys, and so having more sisters (i.e. siblings with lower returns) reduces competition for scarce resources and raises investments in all children. Sibling rivalry in child investments in poor countries is well documented (Parish and Willis (1993) for Taiwan, Morduch (2000) for Tanzania, Edmonds (2006) for Nepal, Ota and Moffatt (2007) for India, and Dammert (2010) for Guatemala and Nicaragua).[13]

Although the empirical results from these sibling rivalry studies that focus on sibling sex composition are similar, the underlying behavioral models may differ. For instance, Garg and Morduch (1998) focus on credit constraints and differences in relative investment returns for boys and girls as the cause for sibling rivalry. On the other hand, Edmonds (2006) and Dammert (2010) emphasize that if it is not possible to hire labor for home production and girls have comparative advantage in doing it, both boys and girls benefit from having more sisters (holding constant the number of siblings), an outcome which is observationally indistinguishable from Garg and Morduch (1998). In our paper, by comparing direct measures of child ability across siblings, our approach provides a test of sibling rivalry that does not depend on assumptions of gender bias in returns to education (Garg and Morduch 1998) or on the division of labor based on gender (Edmonds 2007; Dammert 2010).

While these empirical approaches focused on demographic characteristics are useful to highlight tradeoffs parents may face and how they respond to said tradeoffs, these traditional models of sibling rivalry are limited for at least two main reasons. First, they neglect that parents have additional knowledge about their children's capabilities and use this information to make

[13] Akresh and Edmonds (2011) highlight the distinction between sibling and residential rivalry.

investment decisions. Second, the analysis includes measures of current family size and composition, treating them as given at the time the parents make schooling decisions, despite these variables likely being endogenous. Several recent papers attempt to address these issues by including typically omitted measures of child endowments such as birth weight (Loughran et al. 2004; Rosenzweig and Zhang 2009; Datar et al. 2010), achievement test scores (Glick and Sahn 2010) or cognitive test scores (Ayalew 2005; Kim 2005; Akresh et al. 2012). However, the key challenge in using current measures of child endowments to explain parental investment decisions is that a child's measured endowment (e.g. cognitive ability) is likely endogenous and reflects the entire history of past parental investments, or lack thereof, including nutrition, nurturing, and preferences, as well as any other factors that are unobservable to the researcher.

3.2 Econometric Specification

Our first improvement compared with previous sibling rivalry research is to estimate a household or sibling fixed effects regression that controls for all household level characteristics that are constant across siblings. Specifically, we estimate the following regression:

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

where e_{ih} is the educational or child labor outcome for child i in household h, A_{ih} is a direct measure of observed child cognitive ability, X_{ih} is a vector of individual characteristics including age and gender that might influence parental investments, λ_h is a 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 previous sibling rivalry papers, child ability would have been part of the error term, but in our analysis we are able to directly control for its effect on educational and child labor outcomes. This within-family estimate compares a child's own ability to the average ability of all the other siblings in the household to examine if parents incorporate cognitive ability into the human capital investment decision.[14]

While incorporating household fixed effects addresses the issue that family characteristics are different across households, it does not tackle the problem that current child ability measures are endogenous. This endogeneity could be caused by several factors. First, since schooling potentially affects cognitive ability, reverse causality is a concern when using current measures of ability. Second, Raven's test scores not only capture a child's ability endowment but also reflect previous parental investments in the child. In particular, investments in early child nutrition that are critical to cognitive development are reflected to some degree in current ability measures, and it is unlikely these investments are randomly allocated across siblings. Parents with preferences for equity amongst their children may make unequal investments to compensate for initial endowments. Or, parents may prefer to be efficient in their investments, thus investing more in their higher ability children. Any observed measure of a child's endowment, such as ability, therefore could suffer from a potential bias.

Our second empirical identification improvement and what makes this paper unique is that we are able to estimate an instrumental variables model that addresses the endogeneity of child ability. Specifically, we estimate Equation 1 in which we now treat A_{ih} as endogenous, and as instruments, we use rainfall shocks in the child's village for the years when the child was in utero, age zero, and age one. The first stage from this instrumental variables regression is:

[14] Much of the previous literature on sibling rivalry uses a household fixed effects conditional logit model to estimate the relationship between enrollment and sibling characteristics, and in that specification all households with no within-family variation in the dependent variable (i.e. households that send all or none of their children to school) will be dropped from the analysis. In order to compare our results with those in the sibling rivalry literature, we initially restrict the analysis to the sample of households with within-family variation in the outcome. While this has the advantage of better linking to the previous literature, we might be overestimating the role of ability in influencing enrollment because by construction the households in this sample already have unequal investments across their children. In Section 4.3, we show that our sibling rivalry results hold even when including households that have no within-family variation in the dependent variable.

(2) $$A_{ih} = \beta_1 Rainshock_{ih,birthyear-1} + \beta_2 Rainshock_{ih,birthyear} + \beta_3 Rainshock_{ih,birthyear+1} + \alpha_0 X_{ih} + \lambda_h + \varepsilon_{ih}$$

where A_{ih}, X_{ih}, and λ_h are defined as above and $Rainshock_{ih,\ birthyear-1}$, $Rainshock_{ih,\ birthyear}$, and $Rainshock_{ih,\ birthyear+1}$ measure whether child i in household h experienced a negative rain shock in the child's village that could have affected the agricultural harvest for when the child was in utero (birth year minus 1), age zero (birth year), or age one (birth year plus 1). In Section 4.2, we present results showing that these rain shocks are significantly correlated with current measures of a child's cognitive ability. In a setting where families subsist based on agricultural activity, poor rainfall in a given year leads to a bad harvest and subsequent poor nutrition during the year, which will have a direct effect on a child's cognitive development.[15] The rain shock instruments also likely satisfy the exclusion restriction as they are unlikely to be correlated with the error term in the education or child labor regressions since these rain shocks experienced by a child in utero or under age two occurred 5 to 15 years before the current education or child labor investment decisions. Furthermore, the impact of the rain shocks appears to be captured through the effect on a child's impaired cognitive ability and not overall child health, as we find no correlation between a child's current height-for-age z-score and rain shocks experienced in early childhood.[16]

4. Empirical Results

[15] All of the instruments focus on annual rainfall shocks for two main reasons. First, we only have year of birth information, but not month of birth, for children in our sample. In rural Burkina Faso, few families obtain official birth certificates because they are expensive, and this was even truer during the period when the children in our sample were born. Second, while the identification strategy theoretically could be sharper by focusing on monthly instead of annual rain shocks (i.e. showing rain shocks one month prior to conception have no impact whereas shocks one month after conception do), the primary mechanism by which rain shocks impact a child's cognitive ability is through nutrition and the agricultural harvest, and what is critical for the size of a harvest is total annual rainfall and not just rainfall in a given month.

[16] A potential threat to our identification strategy is that household crop decisions could render children more or less vulnerable to rainfall shocks. Our instrument would then be correlated with the error term in the second stage regression. We argue, however, that crop decisions depend primarily on landholdings which are time-invariant in rural Burkina Faso because of limited land transactions. Therefore, once we condition on household fixed effects, rainfall shocks should no longer be correlated with the error term in our second stage regression.

4.1 Household Fixed Effects Estimates for Schooling and Child Labor

Since we build on the sibling rivalry literature and to have a baseline with which we can compare our instrumental variables results, we first analyze the within-household relationship between ability, schooling, and child labor outcomes. In Table 2, we present results from estimating the household fixed effects regression in Equation 1. We restrict the household fixed effects specification to the 2,862 school age children (ages 5 to 15) living in households with multiple children in this age range with differing enrollment outcomes.[17] Consistent with the sibling rivalry literature, we find a strong relationship between a child's cognitive ability and education outcomes. A child with one standard deviation higher ability has a 14.9 percentage point higher likelihood of being currently enrolled compared to their lower ability siblings, corresponding to 29.2 percent of the base enrollment. Similarly, a higher ability child is more likely to ever be enrolled, progress through school more quickly, and is less likely to start school late.

Consistent with the increased schooling results, children with higher ability engage in fewer hours of child labor activities compared to their siblings. Having a standard deviation higher ability is correlated with doing 0.8 fewer hours of child labor than one's lower ability siblings, corresponding to 15.2 percent of average child labor hours worked. This is preliminary evidence parents consider a child's cognitive ability in deciding how to allocate their children's time. Ability and child labor are strongly correlated when considering the intensive margin of number of hours performing specific child labor activities (household chores and tending for siblings or sick members), but there is no relationship when considering the extensive margin of whether a child engages in specific child labor tasks. This suggests that both high and low ability children engage in these different tasks, and that ability may not be protective of a child's time in

[17] All regressions include household fixed effects, as well as child age and gender dummies. Correlation among the error terms for children in a given village experiencing the same enrollment and rainfall shock environment might bias the standard errors downward, so in all regressions we cluster the standard errors by village.

this regard, rather it mainly affects the number of hours a child performs these tasks. However, as discussed earlier, these estimates may be biased because measured ability for school-age children will reflect the sum of all previous investments and shocks for that child.

4.2 Household Fixed Effects IV Estimates for Schooling and Child Labor

Since measured ability reflects the accumulation of previous parental investments in a particular child, the OLS estimates in Table 2 might have an upward bias in estimating the relationship between ability and enrollment or child labor. On the other hand, the OLS estimates may be biased downwards because of negative shocks experienced during early childhood. Using an instrumental variables (IV) approach that treats child ability as endogenous provides a causal estimate of the effect of a child's ability on enrollment and child labor. Our IV approach takes advantage of the relationship between rainfall, food scarcity, and child cognitive development. As discussed earlier, malnourishment can have large effects on healthy development, particularly for young children. The economics and nutrition literatures suggest that, while all early years are important, the periods before a child is two and in utero are of particular importance to development. We therefore explore various rain shock instruments from this early childhood time period, with a particular focus on the child's years in utero, age zero, and age one.

Table 3 presents results from the IV regression's first stage of the endogenous variable, child cognitive ability, on alternative rain shock instruments. The instruments measure if rainfall in a given year in a child's village is at least one standard deviation below that village's historical mean rainfall for the 15-year period before any of the children in the survey were born. Negative rainfall reduces a harvest and has an adverse effect on a child's food consumption or nutrition. In Columns 1 to 7, we examine the relationship between ability and each shock time period separately; in Column 8, we combine the instruments for rain shocks experienced while a child was in utero, age 0, or age 1 as in Equation 2. A large negative rain shock for the harvest consumed by the mother two or three years before birth has no relationship with a child's cognitive ability. Obviously, this is before the child's conception, and while the mother could be affected by the shock, we find no correlation with the child's ability.

However, having a large negative shock for the harvest consumed by the mother during the child's in utero year has a large negative correlation with the child's cognitive development as seen in column 3. This is consistent with the previous literature regarding the importance of a mother's health during pregnancy. Having rainfall in the child's village for the harvest when the child is in utero that is more than one standard deviation below the village's mean historical level is correlated with a -0.23 standard deviations lower cognitive ability z-score. The F-statistic for the excluded instrument is well above the threshold that would indicate a potential weak instrument bias. We observe a similar relationship in column 4 for a rain shock for the harvest during the child's first year of life (age 0). Experiencing a rain shock when the child is age 1 is negatively correlated with ability, with a similar size impact as seen for shocks of children age 0 and in utero, but the estimate is not significant at standard levels.

Columns 6 and 7 present first stage regressions for the harvests for children from ages 2 to 5, and the point estimates are small and insignificant, suggesting a negative shock during these ages is less important than in earlier years. Since the child's in utero, age 0, and age 1 periods are shown to be important (in the literature and in our first stage regressions), we estimate the first stage using those three instruments in Column 8. Point estimates are large and significant, with an F-statistic for the excluded instruments of 9.69, suggesting this strategy is valid. Rainfall for a child in utero that is more than one standard deviation below the village's long-run historical mean is correlated with 0.24 standard deviations lower ability. A rain shock for a child age 0 is correlated with 0.15 standard deviations lower ability, while a rain shock experienced by a child age 1 is correlated with 0.27 standard deviations lower ability.

In Table 4, we present results from the household fixed effects instrumental variables regressions estimating the relationship between child cognitive ability and education outcomes and treating ability as endogenous. Results are consistent with the Table 2 OLS estimates, although the magnitude of the effects are larger, which we discuss in detail below. In treating child ability as endogenous, we use the three rain shock instruments for the time periods in utero, age zero, and age one.[18] A one standard deviation increase in child ability is correlated with an 81.7 percentage point higher likelihood of enrollment. To put this in perspective, a child that experiences a negative rain shock (rainfall one standard deviation below the village's historical long-run average) while in utero would have 0.235 standard deviations lower cognitive ability and this would lead to a 19.2 percentage point lower likelihood of enrollment, corresponding to a 38 percent drop relative to mean enrollment.[19] Similar calculations show that negative rain shocks experienced by a child at age 0 correspond to a 23 percent drop in enrollment, while rain shocks experienced by a child at age 1 correspond to a 44 percent enrollment drop.

The Table 4 IV estimates are substantially larger in magnitude than the Table 2 OLS estimates. One potential explanation for this difference is that, if there is heterogeneity in the effects of ability, the IV estimates obtained are not consistent estimates of the average effect of ability on enrollment in the overall population of children (Card 1995, 2001; Heckman et al. 1999). Rather, the IV estimate should be interpreted as the local average treatment effect,

[18] Since the model is overidentified, we are able to test whether the instruments are uncorrelated with the error term and correctly excluded from the estimated enrollment equation. The overidentification test regresses the residuals from the IV regression on all of the instruments, and under the null hypothesis that all instruments are uncorrelated with the error term, a rejection of Hansen's J statistic casts doubt on the validity of the instruments. For the enrollment regression, the p-value of the J statistic is 0.3 indicating our instruments are valid, while for the other dependent variables in Tables 4 and 5, the p-values of the J statistic are generally slightly larger.

[19] Results are consistent when we include in the regressions additional child level controls such as birth order or a child's height-for-age z-score.

meaning the effect of ability on enrollment for the subgroup of children from households for which rainfall shocks to a child in utero or during early childhood caused ability to differ across siblings (Imbens and Angrist 1994). Children whose ability has been negatively affected by these early childhood shocks are likely to be from vulnerable families who did not have safety nets to buffer the shocks. Presumably, those are also families where child ability, relative to his siblings' ability, plays a crucial role in deciding school enrollment.[20] For example, while in a less vulnerable family most children would be enrolled, in more vulnerable families, only the most able children would get a chance to enroll. This could explain why the local average treatment effects obtained from the IV regressions are larger than the OLS estimates, which are estimates for the overall population of children.

The other Table 4 columns present the household fixed effects IV regressions for ever being enrolled, timely grade progression through school, and starting school at a late age. A negative shock while in utero that lowers child cognitive ability by 0.235 standard deviations would lead to declines, relative to baseline levels, of 32 percent for ever being enrolled, 24 percent for timely grade progression, and a 113 percent increase in starting school late.[21]

Turning to child labor activities in Table 5, for the same sample of 2,862 children ages 5 to 15 years old, higher ability children engage in less child labor than their lower ability siblings. Negative rain shocks experienced while in utero lowers cognitive ability by 0.235 standard deviations and leads to increases for these children of 6.96 percentage points in the likelihood of tending for siblings or sick members and 6.56 percentage points in the likelihood of working on

[20] When we split households into poor and non-poor groups based on assets, per capita expenditures, or parental schooling, the relationship between child ability and enrollment in Table 4 only holds for poor households.

[21] To further explore the relationship between the specific timing of rainfall shocks and education, in Appendix Table 1, we present the household fixed effects IV results using each instrument (rain shock experienced while in utero, age 0, or age 1) separately. Results for each instrument are consistent with those in Table 4, indicating that higher ability siblings within a household, compared to their lower ability siblings, are more likely to receive positive schooling investments. The magnitude of the impact of ability on enrollment is largest for those children who experience negative rainfall shocks while in utero and diminishes for those experiencing shocks at later ages.

the family farm. Relative to baseline levels, these shocks represent increases of 26.8 and 21.9 percent, respectively compared to a child's higher ability siblings. Even though a child is not more likely to perform chores around the household such as cooking, fetching water, sweeping or doing the dishes, a child with 0.235 standard deviations lower cognitive ability due to negative rain shocks in utero will spend 0.65 more hours on said tasks than his higher ability siblings, which is 21.2 percent of the baseline level. Similar calculations show that a negative rain shock in utero leads to declines in ability and these further lead to increases of 0.24 hours per day tending for siblings or sick household members, 1.19 hours per day on farm labor, and 2.71 total hours of child labor per day. Relative to baseline mean levels, these additional hours represent increases of 39.3, 126.6, and 48.9, respectively. Experiencing a 1 standard deviation rainfall shock at age 0 or age 1 corresponds respectively to 0.146 or 0.272 standard deviations lower cognitive ability and subsequently 30.4 or 56.6 percent increases in the number of hours of child labor these children perform relative to their higher ability siblings.[22]

4.3 Robustness Checks

To test the robustness of our results, we present alternative specifications in which we use placebo instruments and a different measure of child ability. We also note that households that had children who experienced negative rain shocks are not systematically different in observable characteristics from other households and that restricting the sample based on additional information about a child's birth village yields consistent results. Finally, we discuss results from regressions that include the sample of households with no within-family variation in the

[22] Similar to Appendix Table 1, in Appendix Table 2, we explore the relationship between the specific timing of rainfall shocks and child labor outcomes using each rain shock instrument separately. Overall, results are generally consistent with those in Table 5, indicating that higher ability children perform less hours of child labor, although the results for the extensive margin of whether a child engages in farm labor are less consistent and we observe smaller magnitude impacts for the intensive margin of child labor hours for shocks experienced in utero compared to the education regressions. This is consistent with in utero shocks impacting brain development of the fetus and subsequent cognitive ability and therefore having a larger impact on schooling-related outcomes.

dependent variable, as well as regressions estimated without household fixed effects, both of which support our hypotheses about sibling rivalry.

In Table 6, we provide suggestive evidence that our rain shock instruments likely satisfy the exclusion restriction. To show this, we use a placebo instrument that uses a rain shock for the harvest two years before birth (a period that has no link to child ability as seen in Table 3 column 2) to rule out the possibility that rain shocks have long-lasting and direct effects on education and labor outcomes. The F-statistic for the excluded instrument is 0.97 and the coefficient for the instrument is insignificant, meaning it is uncorrelated with child ability. We present results for the second stage, looking at the same schooling and child labor outcomes, and we observe no statistically significant relationship between ability and these outcomes.[23] This strengthens our argument that our results in previous tables using valid instruments are indeed estimating a causal relationship.

To address concerns that transforming the Raven's test scores into age-adjusted z-scores might have introduced bias, we estimate regressions using the Raven's raw test score in Table 7. A negative rain shock experienced in utero lowers a child's Raven's raw test score by 0.716 questions (in the first stage results that are not shown) and this would lead to an 18.9 percentage point lower likelihood of current enrollment and 2.53 hours more child labor, which are consistent with the results presented earlier in Tables 4 and 5.

Another concern is there could be selection bias in the types of households that have a child during periods of negative shocks, and these households could be systematically different from households that did not have children during those shock periods. In Table 8, we present regressions of different observable household characteristics measured at the time of the survey

[23] In addition, shocks for the harvest three years prior to birth have no link to ability as shown in Table 3 column 1, and the second stage estimates are similar to those in Table 6.

on an indicator for whether the household had a child who experienced a negative rainfall shock in utero or during early childhood.[24] We examine per capita household expenditures, log assets, parental schooling, household size, and the household head's marital status, age, occupation, number of wives, and number of children.[25] There is no evidence that households that had a child during periods of negative rain shocks are any different, at least on observable dimensions, from other households, as the coefficient for the shock indicator is insignificant in each specification.

In creating our rainfall shock instruments, we have assumed that all children were born in the same village in which they currently reside. However, due to previous migration, if a child was born in a different village, then the instrument that is being used for that child's in utero and early childhood rainfall is measured with error. We therefore test the robustness of our results and correct for this potential problem using the limited information we have about each person's birth location. Using two alternative specifications, we limit the sample to exclude those children for whom we have varying levels of information about whether they were born in the current village. In the first specification, we use all information we have to identify anyone we believe was not born in the same village in which they currently reside, and we then drop them from the sample, reducing our sample size by 1.3 percent. In this approach, we only drop those for whom we are confident they were born in a different village. In a second, more restrictive, specification, we drop anyone for whom we do not have sufficient information to determine they were born in the village where they are currently residing, thus limiting our sample by 5.4 percent. Results for both specifications (not shown) are consistent with those presented thus far.

[24] If we had complete birth history information for each household, we could estimate these regressions using an indicator for whether the household gave birth to a child during a negative rain shock period, as this would allow us to accurately capture potential endogenous fertility. Given we do not have this information, we are therefore limited in measuring households that gave birth during a shock period and had a child survive until the survey.
[25] While we would like to estimate these regressions using characteristics measured at the time of the rain shock, we do not have that information in the survey. However, some of the variables are unlikely to have changed since the time period of the shock, including parental schooling and the household head's occupation.

Thus far, to better link our results with the previous literature, all regressions exclude the sample of households with no within-family variation in the dependent variable. However, by doing that the remaining sample of households is a selective one and by focusing on households that already have unequal investments across their children, we might be overestimating the role of ability in influencing enrollment and child labor. As a robustness check, we estimate the household fixed effects IV regressions on the full sample of households, including those who have enrolled all or none of their children. The results (not shown) indicate a consistent pattern of sibling ability rivalry, whereby higher ability children are more likely to be enrolled and work fewer hours doing child labor compared to their lower ability siblings. Finally, all regressions estimated so far incorporate household fixed effects, which have the significant advantage of controlling for all observable and unobservable household characteristics that are constant across siblings and that might be related to education and child labor outcomes. As a robustness check, we re-estimate the IV regressions for current enrollment and total child labor hours and replace the household fixed effects with village fixed effects. Identification is no longer driven by within household variation across siblings but also includes across household variation. Results (not shown) illustrate that higher ability children are more likely to be enrolled and spend fewer hours doing child labor compared to lower ability children, supporting our previous results about the relationship between ability and human capital investments.

5. Conclusions

In this paper, we find strong evidence of sibling rivalry when parents make decisions regarding educational investments and child labor in rural Burkina Faso. However, in contrast with previous research that generally focuses on easily observable demographic characteristics to gauge sibling rivalry, we use measures of a child's own cognitive ability to test for how parents make these investment decisions. To address the potential endogeneity of current measures of child ability, we use rain shocks in a child's village that were experienced in utero or during early childhood to instrument for cognitive ability. We examine both educational outcomes (enrollment, grade progression, and late start) as well as child labor, focusing not only on total hours of labor, but also changes in the specific tasks that each child performs. Negative rainfall shocks experienced in utero lead to 0.24 standard deviations lower ability z-scores and compared to their siblings that corresponds with a 38 percent drop in enrollment and a 49 percent increase in child labor hours. Our findings are robust to alternative education and child labor outcomes, as well as alternative child ability measures. Negative education impacts are largest for shocks experienced in utero, diminished for shocks before age two, and have no impact for shocks after age two.

Our results can likely be generalized to other developing countries that have not yet achieved universal primary or secondary education and where families face real resource constraints that affect investment decisions related to education and child labor. Child labor activities such as farm labor, tending for siblings, and chores around the house are common throughout poor countries, and children are often required to work long hours doing these activities. Using our estimates of the impact of sibling ability rivalry on educational investments, we can speculate on the long-term consequences that follow from this rivalry. Lower ability children are 32 percent less likely to ever attend school compared to their higher ability siblings, and this would translate, in this setting, into missing on average 3.8 years of school by the time they were age 16. Using a 9.9 percent rate of return for each year of primary school in Burkina Faso (Kazianga 2004), this corresponds to a 12 percent loss in future earnings.

Negative shocks early in a child's life, particularly in utero or in the first years of life, can have long-run implications for that child's wellbeing. If a child is exposed to a negative rainfall shock in utero in an environment where families are subsisting on their rain-fed agricultural production, the likelihood that the child will ever recover from said shock is low. Policies to prevent this type of food and nutrition insecurity are critical. Likewise, policies aimed at improving child development may improve long-run well-being through the direct effect on child development at the time and the subsequent effect on parent investments, which could reduce the inefficiencies generated by sibling ability rivalry.

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Table 1: Summary Statistics of Nahouri Cash Transfer Pilot Program (NCTPP) Survey

	Mean	Standard Deviation
Household Size	9.29	3.91
Number of Wives	1.53	0.95
Number of Biological Children Ages 5-15	3.97	1.75
Number of Biological Children Under Age 5	1.25	1.14
Number of Non-Biological Children in Household	0.36	0.85
Number of Other Members (Excluding Head)	1.18	1.46
Male (Fraction Male)	0.53	0.50
Age	9.26	3.03
Own Ability (Raven's age adjusted z-score)	-0.02	1.00
Raven's Raw Test Score	4.74	3.36
Proportion Children Currently Enrolled	0.51	0.50
Proportion Children Ever Enrolled	0.56	0.50
Grade Progression	0.49	0.48
Starting School Late	0.34	0.97
Binary Indicator if Child Does Household Chores such as Cooking, Fetching Water, Sweeping, Doing Dishes	0.60	0.49
Binary Indicator if Child Tends for Siblings or Sick Members in the Household	0.26	0.44
Binary Indicator if Child Engages in Farm Labor	0.30	0.46
Hours Doing Household Chores such as Cooking, Fetching Water, Sweeping, Doing Dishes	3.06	5.51
Hours Tending for Siblings or Sick Members	0.61	2.02
Hours Engaging in Farm Labor	0.94	2.41
Total Hours Engaging in Child Labor Activities	5.54	9.60
Number of Children	2862	
Number of Households	893	
Annual Village Rainfall from 1976-2003 (mm)	899	137

Notes: All summary statistics are based on information for the 2862 children ages 5-15 from the 893 households that have multiple children in this age range with differing enrollment outcomes. Own ability is measured using the Raven's Colored Progressive Matrices and normed by age (z-score); 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; starting school late equals 1 if a child started school after age 7 or never attended school, 0 if started school at age 7, -1 if started school at age 6, and -2 if started school at age 5. Each of the binary indicators for child labor activity (does household chores, tends for siblings or sick members, engages in farm labor) equals 1 if the child engaged in that activity during the 2 days preceding the survey when school was in session. Rainfall summary statistics use annual rainfall data for the 75 villages in the NSPP survey. From 1976-2003, across all 75 villages, annual minimum rainfall was 503mm and maximum rainfall was 1388mm. Data source: Nahouri Cash Transfer Pilot Program (NCTPP) 2008 household survey data and rainfall data from Burkina Faso and Ghana Meteorological Services.

Table 2: Household Fixed Effects Regressions Estimating the Relationship Between Ability, Schooling, and Child Labor

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. Education							
Dependent Variable:	Current Enrollment	Ever Enrolled	Grade Progression	Late Start			
Ability (Raven's age adjusted z-score)	0.149***	0.139***	0.142***	-0.273***			
	[0.015]	[0.013]	[0.015]	[0.030]			
Male	0.044**	0.047**	0.057***	-0.069			
	[0.019]	[0.019]	[0.020]	[0.046]			
Household Fixed Effects?	Yes	Yes	Yes	Yes			
Age Fixed Effects?	Yes	Yes	Yes	Yes			
Observations	2,862	2,862	2,714	2,862			
Panel B. Child Labor							
Dependent Variable:	Does Household Chores	Tends for Siblings or Sick Members	Engages in Farm Labor	Hours Doing Household Chores	Hours Tending for Siblings or Sick Members	Hours in Farm Labor	Total Child Labor Hours
Ability (Raven's age adjusted z-score)	-0.006	-0.006	-0.012	-0.455***	-0.141**	-0.095	-0.840***
	[0.011]	[0.008]	[0.012]	[0.143]	[0.063]	[0.068]	[0.244]
Male	-0.249***	-0.129***	0.109***	-3.039***	-0.418***	0.435***	-3.079***
	[0.024]	[0.019]	[0.014]	[0.260]	[0.094]	[0.096]	[0.374]
Household Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,862	2,862	2,862	2,862	2,862	2,862	2,862

Notes: Robust standard errors in brackets, clustered at village level. * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions include household fixed effects and child age and gender dummies. Regression sample includes 2862 children ages 5-15 from households with multiple siblings in this age range with differing enrollment outcomes. Ability is measured using the Raven's Colored Progressive Matrices and normed by age (z-score). All dependent variables are defined in the notes to Table 1. Data source: Nahouri Cash Transfer Pilot Program (NCTPP) 2008 household survey data.

Table 3: First Stage Household Fixed Effects IV Regressions: Relationship between Potential Rain Shock Instruments and Child Ability

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Dependent Variable:	Ability	Ability	Ability	Ability	Ability	Ability	Ability	Ability
Rain Shock for Harvest For	0.048							
Age 3 Years Before Birth	[0.145]							
Rain Shock for Harvest For		0.191						
Age 2 Years Before Birth		[0.194]						
Rain Shock for Harvest For			-0.228***					-0.235***
In Utero			[0.047]					[0.058]
Rain Shock for Harvest For				-0.208***				-0.146*
Age 0				[0.054]				[0.084]
Rain Shock for Harvest For					-0.248			-0.272*
Age 1					[0.171]			[0.158]
Rain Shock for Harvest For						-0.067		
Ages 2 or 3						[0.070]		
Rain Shock for Harvest For							0.022	
Ages 4 or 5							[0.216]	
Household Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Gender Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-statistic of Excluded Instruments	0.11	0.97	23.95	15.03	2.11	0.94	0.01	9.69
Observations	2,862	2,862	2,862	2,862	2,862	2,862	2,862	2,862

Notes: Robust standard errors in brackets, clustered at village level. * significant at 10%; ** significant at 5%; *** significant at 1%. Regression results are from the instrumental variables first stage regression of ability on alternative rain shocks. All regressions include household fixed effects and child age and gender dummies. Regression sample includes 2862 children ages 5-15 from households with multiple siblings in this age range with differing enrollment outcomes. Ability is measured using the Raven's Colored Progressive Matrices and normed by age (z-score). Instruments for each column vary by the year relative to the child's birth year, and consider if rainfall for the harvest for that year in the child's village is 1 standard deviation below the village's historical average rainfall for the 15-year period before any of the survey children were born (1976-1991). Data source: Nahouri Cash Transfer Pilot Program (NCTPP) 2008 household survey data and rainfall data from Burkina Faso and Ghana Meteorological Services.

Table 4: Household Fixed Effects IV Regressions Estimating Relationship between Ability and Schooling, Treating Ability as Endogenous

	(1)	(2)	(3)	(4)
Dependent Variable:	Current Enrollment	Ever Enrolled	Grade Progression	Late Start
Instruments:	Rain Shocks for Harvests for In Utero, Age 0, or Age 1	Rain Shocks for Harvests for In Utero, Age 0, or Age 1	Rain Shocks for Harvests for In Utero, Age 0, or Age 1	Rain Shocks for Harvests for In Utero, Age 0, or Age 1
Ability (Raven's age adjusted z-score)	0.817***	0.771***	0.501***	-1.637*
	[0.273]	[0.175]	[0.159]	[0.863]
Male	-0.029	-0.022	0.017	0.080
	[0.045]	[0.036]	[0.033]	[0.117]
Household Fixed Effects?	Yes	Yes	Yes	Yes
Age Fixed Effects?	Yes	Yes	Yes	Yes
First Stage F-Statistic of Excluded Instruments	9.69	9.69	9.64	9.69
Observations	2,862	2,862	2,714	2,862

Notes: Robust standard errors in brackets, clustered at village level. * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions include household fixed effects and child age and gender dummies. Regression sample includes 2862 children ages 5-15 from households with multiple siblings in this age range with differing enrollment outcomes. Ability is measured using the Raven's Colored Progressive Matrices and normed by age (z-score). All dependent variables are defined in the notes to Table 1. The 3 instruments measure if rainfall for the harvest for the in utero, age 0, or age 1 years in the child's village are 1 standard deviation below the historical average rainfall for the 15-year period before any of the survey children were born (1976-1991). Data source: Nahouri Cash Transfer Pilot Program (NCTPP) 2008 household survey data and rainfall data from Burkina Faso and Ghana Meteorological Services.

Table 5: Household Fixed Effects IV Regressions Estimating Relationship between Ability and Child Labor, Treating Ability as Endogenous

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable:	Does Household Chores	Tends for Siblings or Sick Members	Engages in Farm Labor	Hours Doing Household Chores	Hours Tending for Siblings or Sick Members	Hours in Farm Labor	Total Child Labor Hours
Instruments:	Rain Shocks for Harvests for In Utero, Age 0, or Age 1	Rain Shocks for Harvests for In Utero, Age 0, or Age 1	Rain Shocks for Harvests for In Utero, Age 0, or Age 1	Rain Shocks for Harvests for In Utero, Age 0, or Age 1	Rain Shocks for Harvests for In Utero, Age 0, or Age 1	Rain Shocks for Harvests for In Utero, Age 0, or Age 1	Rain Shocks for Harvests for In Utero, Age 0, or Age 1
Ability (Raven's age adjusted z-score)	0.368	-0.296**	-0.279*	-2.750*	-1.038*	-5.080***	-11.529***
	[0.540]	[0.122]	[0.143]	[1.589]	[0.580]	[1.847]	[3.240]
Male	-0.290***	-0.097***	0.138***	-2.788***	-0.320***	0.980***	-1.908***
	[0.061]	[0.026]	[0.026]	[0.325]	[0.118]	[0.308]	[0.692]
Household Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First Stage F-Statistic of Excluded Instruments	9.69	9.69	9.69	9.69	9.69	9.69	9.69
Observations	2,862	2,862	2,862	2,862	2,862	2,862	2,862

Notes: Robust standard errors in brackets, clustered at village level. * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions include household fixed effects and child age and gender dummies. Regression sample includes 2862 children ages 5-15 from households with multiple siblings in this age range with differing enrollment outcomes. Ability is measured using the Raven's Colored Progressive Matrices and normed by age (z-score). All dependent variables are defined in the notes to Table 1. The 3 instruments measure if rainfall for the harvest for the in utero, age 0, or age 1 years in the child's village are 1 standard deviation below the historical average rainfall for the 15-year period before any of the survey children were born (1976-1991). Data source: Nahouri Cash Transfer Pilot Program (NCTPP) 2008 household survey data and rainfall data from Burkina Faso and Ghana Meteorological Services.

Table 6: Placebo Household Fixed Effects IV Regressions Estimating Relationship between Ability, Schooling, and Child Labor, Treating Ability as Endogenous, But Using Rain Shocks Years Before Child In Utero

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Schooling IV Regressions							
Dependent Variable:	Current Enrollment	Ever Enrolled	Grade Progression	Late Start			
Ability (Raven's age adjusted z-score)	-0.401 [0.736]	-0.651 [1.044]	-0.397 [0.819]	0.478 [0.916]			
Household Fixed Effects?	Yes	Yes	Yes	Yes			
Age & Gender Fixed Effects?	Yes	Yes	Yes	Yes			
First Stage F-Statistic of Excluded Instruments	0.968	0.968	0.968	0.968			
Observations	2,862	2,862	2,714	2,862			
Panel B. Child Labor IV Regressions							
Dependent Variable:	Does Household Chores	Tends for Siblings or Sick Members	Engages in Farm Labor	Hours Doing Household Chores	Hours Tending for Siblings or Sick Members	Hours in Farm Labor	Total Child Labor Hours
Ability (Raven's age adjusted z-score)	0.200 [0.527]	0.582 [0.641]	-0.336 [0.446]	4.550 [6.349]	5.392 [5.233]	-2.285 [2.735]	9.500 [12.701]
Household Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age & Gender Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First Stage F-Statistic of Excluded Instruments	0.968	0.968	0.968	0.968	0.968	0.968	0.968
Observations	2,862	2,862	2,862	2,862	2,862	2,862	2,862

Notes: Robust standard errors in brackets, clustered at village level. * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions include household fixed effects and child age and gender dummies. Regression sample includes 2862 children ages 5-15 from households with multiple siblings in this age range with differing enrollment outcomes. Ability is measured using the Raven's Colored Progressive Matrices and normed by age (z-score). All dependent variables are defined in the notes to Table 1. The placebo instrument is if rainfall for the harvest *2 years prior* to birth in the child's village is 1 standard deviation below the historical average rainfall for the 15-year period before any of the survey children were born (1976-1991). Data source: Nahouri Cash Transfer Pilot Program 2008 household survey data and rainfall data from Burkina Faso and Ghana Meteorological Services.

Table 7: Robustness Check: Household Fixed Effects IV Regressions Estimating Relationship Between Ability, Schooling and Child Labor, Treating Ability as Endogenous, Alternative Measures of Ability

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A: Schooling IV Regressions							
Dependent Variable:	Current Enrollment	Ever Enrolled	Grade Progression	Late Start			
Raven's Raw Score	0.264***	0.241***	0.171***	-0.550**			
	[0.063]	[0.037]	[0.038]	[0.232]			
Household Fixed Effects?	Yes	Yes	Yes	Yes			
Age & Gender Fixed Effects?	Yes	Yes	Yes	Yes			
First Stage F-Statistic of Excluded Instruments	12.87	12.87	14.16	12.87			
Observations	2,862	2,862	2714	2,862			
Panel B: Child Labor IV Regressions							
Dependent Variable:	Does Household Chores	Tends for Siblings or Sick Members	Engages in Farm Labor	Hours Doing Household Chores	Hours Tending for Siblings or Sick Members	Hours in Farm Labor	Total Child Labor Hours
Raven's Raw Score	0.115	-0.092***	-0.056*	-0.920	-0.272	-1.479***	-3.533**
	[0.166]	[0.025]	[0.030]	[0.780]	[0.179]	[0.487]	[1.524]
Household Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age & Gender Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
First Stage F-Statistic of Excluded Instruments	12.87	12.87	12.87	12.87	12.87	12.87	12.87
Observations	2,862	2,862	2,862	2,862	2,862	2,862	2,862

Notes: Robust standard errors in brackets, clustered at village level. * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions include household fixed effects and child age and gender dummies. Regression sample includes 2862 children ages 5-15 from households with multiple siblings in this age range with differing enrollment outcomes. Ability is measured using the Raven's Colored Progressive Matrices. All dependent variables are defined in the notes to Table 1. The 3 instruments measure if rainfall for the harvest for the in utero, age 0, or age 1 years in the child's village are 1 standard deviation below the historical average rainfall for the 15-year period before any of the survey children were born (1976-1991). Data source: Nahouri Cash Transfer Pilot Program 2008 household survey data and rainfall data from Burkina Faso and Ghana Meteorological Services.

Table 8: Characteristics of Households Having A Child During Rain Shock Periods

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Dependent Variable:	Per Capita Household Expenditures	Log Assets	Either Parent Ever Attended School	Marital Status Household Head	Age of Household Head	Household Head is a Farmer	Number of Wives	Number of Children of Household Head	Household Size
Having A Child During Rain Shock Periods	1,420.659	-0.260	-0.056	-0.078	-2.766	-0.063	-0.262	-0.710	-1.274
	[9,035.707]	[0.302]	[0.048]	[0.107]	[2.121]	[0.067]	[0.199]	[0.441]	[1.015]
Observations	893	889	893	893	798	893	893	893	893

Notes: Robust standard errors in brackets, clustered at village level. * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions at household level. Regression sample includes 893 households with multiple children ages 5-15 with differing enrollment outcomes. Independent variable for each regression measures if a household experienced for any of their children a negative rainfall shock where the rainfall for the harvest for the in utero, age 0, or age 1 years in the child's village are 1 standard deviation below the historical average rainfall for the 15-year period before any of the survey children were born (1976-1991). Data source: Nahouri Cash Transfer Pilot Program (NCTPP) 2008 household survey data and rainfall data from Burkina Faso and Ghana Meteorological Services.

Appendix Table 1: Household Fixed Effects IV Regressions Estimating Relationship Between Ability and Schooling, Treating Ability as Endogenous, Using Single Year Instruments

	(1)	(2)	(3)	(4)
Dependent Variables:	Current Enrollment	Ever Enrolled	Grade Progression	Late Start
Panel A: Rain Shock for Harvest for Child's In Utero Year as Instrument for Ability				
Ability (Raven's age adjusted z-score)	1.437***	1.123***	0.914***	-3.531***
	[0.285]	[0.226]	[0.211]	[0.736]
Panel B: Rain Shock for Harvest for Child's Age 0 Year as Instrument for Ability				
Ability (Raven's age adjusted z-score)	1.036***	0.797***	1.121**	-2.470*
	[0.319]	[0.262]	[0.550]	[1.346]
Panel C: Rain Shock for Harvest for Child's Age 1 Year as Instrument for Ability				
Ability (Raven's age adjusted z-score)	0.446***	0.601***	0.224	-0.435
	[0.136]	[0.129]	[0.193]	[0.689]
Household Fixed Effects?	Yes	Yes	Yes	Yes
Age & Gender Fixed Effects?	Yes	Yes	Yes	Yes
Observations	2,862	2,862	2,714	2,862

Notes: Robust standard errors in brackets, clustered at village level. * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions include household fixed effects and child age and gender dummies. Regression sample includes 2862 children ages 5-15 from households with multiple siblings in this age range with differing enrollment outcomes. Ability is measured using the Raven's Colored Progressive Matrices and normed by age (z-score). All dependent variables are defined in the notes to Table 1. The 3 instruments measure if rainfall for the harvest for the in utero, age 0, or age 1 years in the child's village are 1 standard deviation below the historical average rainfall for the 15-year period before any of the survey children were born (1976-1991). Data source: Nahouri Cash Transfer Pilot Program (NCTPP) 2008 household survey data and rainfall data from Burkina Faso and Ghana Meteorological Services.

Appendix Table 2: Household Fixed Effects IV Regressions Estimating Relationship Between Ability and Child Labor, Treating Ability as Endogenous, Using Single Year Instruments

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent Variable:	Does Household Chores	Tends for Siblings or Sick Members	Engages in Farm Labor	Hours Doing Household Chores	Hours Tending for Siblings or Sick Members	Hours in Farm Labor	Total Child Labor Hours
Panel A: Rain Shock for Harvest for Child's In Utero Year as Instrument for Ability							
Ability (Raven's age adjusted z-score)	1.254***	-0.242**	0.504***	-0.417	-1.052**	-0.228	-2.143
	[0.262]	[0.096]	[0.140]	[1.223]	[0.481]	[0.557]	[2.031]
Panel B: Rain Shock for Harvest for Child's Age 0 Year as Instrument for Ability							
Ability (Raven's age adjusted z-score)	0.216	-0.350***	0.327	-5.364	0.216	-4.108*	-13.660
	[0.733]	[0.114]	[0.594]	[8.774]	[0.516]	[2.366]	[15.246]
Panel C: Rain Shock for Harvest for Child's Age 1 Year as Instrument for Ability							
Ability (Raven's age adjusted z-score)	0.030	-0.299	-0.884	-2.725*	-1.550	-7.678	-14.898*
	[0.294]	[0.255]	[0.851]	[1.610]	[1.141]	[5.578]	[8.866]
Household Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Age & Gender Fixed Effects?	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2,862	2,862	2,862	2,862	2,862	2,862	2,862

Notes: Robust standard errors in brackets, clustered at village level. * significant at 10%; ** significant at 5%; *** significant at 1%. All regressions include household fixed effects and child age and gender dummies. Regression sample includes 2862 children ages 5-15 from households with multiple siblings in this age range with differing enrollment outcomes. Ability is measured using the Raven's CPM and normed by age (z-score). All dependent variables are defined in the notes to Table 1. The 3 instruments measure if rainfall for the harvest for the in utero, age 0, or age 1 years in the child's village are 1 standard deviation below the historical average rainfall for the 15-year period before any of the survey children were born (1976-1991). Data source: Nahouri Cash Transfer Pilot Program (NCTPP) 2008 household survey data and rainfall data from Burkina Faso and Ghana Meteorological Services.