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The Impact of Early Childhood Rainfall Shocks on the Evolution of Cognitive and Non-cognitive Skills

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Abstract

This paper is the first to estimate the extent to which early childhood climatic shocks affect both cognitive and non-cognitive skills as measured at multiple points in childhood and adolescence. We assess the impact of rainfall observed in utero and during the first two years of life by analyzing a rich longitudinal study of rural youth in a poor province in China. Our empirical strategy entails estimating the impact of rainfall on various measures of cognitive and non-cognitive skills utilizing a reduced form strategy, conditional on county and year-of-birth fixed effects. The results indicate that there is a significant impact of early shocks, particularly shocks in utero and in the first year of life, on cognitive skills, but that this impact may be declining over time. There is little evidence of any impact on non-cognitive skills. We also present evidence that the declining salience of early shocks is consistent with compensatory strategies employed by parents.

1 Introduction

Economists have convincingly established causal links between health in early childhood and outcomes later in life by investigating the impact of exposure to plausibly exogenous environmental shocks (e.g., epidemics, famine, weather, conflict) during the critical early years of life, especially in utero and the first two years.[1] More specifically, in developing countries, adverse weather shocks experienced early in life have been found to significantly influence later health and economic outcomes, including though not limited to height, earnings, and labor supply.

However, very few studies have employed quasi-experimental designs to examine the impact of early childhood climatic shocks on direct measures of cognitive or non-cognitive skills. Glewwe and King (2001) use rainfall shocks to identify the impact of early childhood malnutrition (as measured by birth weight and early height gains) on cognitive development and find a significant impact of shocks in the second year of life on cognitive development at age eight.[2] Shah and Steinberg (2013) evaluate the positive impact of droughts in early childhood on cognitive school scores in India at ages 5–16.[3] No studies of which we are aware employ a quasi-experimental design to examine the impact of early childhood shocks on non-cognitive skills, although several papers have found correlations between early childhood health and psychosocial outcomes.[4] In addition, no previous

[1]Currie and Vogl (2013) provide a useful review of papers that evaluate the effect of shocks before age ten on later outcomes. Notable papers include Almond (2006); Almond and Chay (2006); Alderman et al. (2006); Banerjee et al. (2010); Maccini and Yang (2009). A number of papers have analyzed the long-term impact of famine caused by the Great Leap Forward in China, for example Luo, Mu and Zhang (2006); Almond, Edlund, Li and Zhang (2006); Shi (2011); Chen and Zhou (2007); Meng and Qian (2009).

[2]They also exploit price shocks.

[3]There are some related studies in the nutritional literature that lack quasi-experimental designs, including Mendez and Adair (1999) who find that early childhood stunting is correlated with weaker cognitive skills in the Philippines at ages eight and eleven, and Crookston et al. (2013), who find that catch-up in height-for-age is correlated with improved cognitive skills (although cognitive skills are measured only once).

[4]Dercon and Sánchez (2013) find a positive correlation between height-for-age measured at age eight and psychosocial competencies measured at age 11–12 in the Young Lives panel data. A number of studies using data from Jamaica finds correlations between early childhood stunting and behavior and psychosocial competencies in childhood and adolescence Chang, Walker, Grantham-McGregor and Powell (2002); Grantham-McGregor, Fernald and Sethuraman (1999); Meeks, Grantham-McGregor, Himes and Chang (1999); Walker, Chang, Powell, Simonoff and Grantham-McGregor (2007)

work has analyzed the impacts of early childhood shocks on both cognitive and non-cognitive skills, or analyzed how the impact of early shocks on cognitive or non-cognitive skills (let alone both) evolves over time. Given the central importance of these skills in determining long-term economic welfare, identifying whether they can be affected by shocks in critical periods in early childhood may be a useful contribution to our understanding of the process of human capital development.

In addition, evaluating whether skill differences caused by early climatic shocks enlarge, persist, or decay as children grow up is important for understanding human capital accumulation and the determinants of inequality. Although there is now a large literature debating the potential for catch-up in physical growth of children, there remains a glaring lack of any evidence on the potential for catch-up in the development of cognitive or non-cognitive skills.[5] The evidence that early shocks can have a significant impact on a wide range of adult outcomes including labor market outcomes would be consistent with a long-term effect on cognitive skills or non-cognitive skills, but this hypothesis has not been directly tested. Understanding the impact of early shocks on non-cognitive skills is of particular interest given growing evidence that non-cognitive skills have a large impact on economic outcomes (Heckman et al., 2006) and are more malleable in children and young adults than cognitive skills (Almlund et al., 2011; Borghans et al., 2008).

This study helps fill these many gaps in the existing literature by analyzing data from the Gansu Survey of Children and Families, a panel dataset that includes a rich set of measurements of both cognitive and non-cognitive skills at multiple points in time for a cohort of 2000 rural children in one of China's poorest provinces. The survey first interviewed the sample children in the year 2000, when they were 9–12 years old, and

[5]In an early review Martorell, Khan and Schroeder (1994) finds little evidence of more rapid growth in height-for-age of children stunted early in life. Hoddinott and Kinsey (2001) find that catch-up growth in height is limited for children aged 12-24 months at the time of a drought in Zambia. By contrast, a number of studies, including several recent ones, provide evidence of catch-up in height at varying ages in various settings (Adair, 1999; Crookston et al., 2010; Deolalikar, 1996; Koch and Linh, 2001; Lundeen et al., 2013; Mani, 2012; Schott et al., 2013; Singh et al., 2014). In related work, Alderman et al. (2014) find that a nutritional supplement given to pregnant mothers in The Gambia that had significant positive effects on infant outcomes had no long-term impact on health or cognitive ability at ages 16–22, also suggestive of catch-up or convergence by children with worse outcomes.

then re-interviewed them two more times, once in adolescence (age 13 to 16) and once in young adulthood (age 17 to 21). These data are linked to village rainfall data obtained from nearby government weather stations. We exploit variation in rainfall during early childhood (in utero, year one, and year two) across different birth cohorts in different villages to estimate the effect of early childhood rainfall shocks on human capital measures as the sample children grow into early adulthood. We also examine whether resource allocation strategies employed by parents reinforce or compensate for early childhood shocks.

More specifically, we define an early childhood rainfall shock as variation in the mean level of rainfall conditional on village fixed effects and year of birth fixed effects. A shock is thus a level of rainfall that is unusually high or low relative to observed mean rainfall in that village (and in that year). In interpreting the impacts of early childhood rainfall shocks, we focus on two main channels, grain yield and maternal labor supply, and analyze the different impacts of rainfall in different seasons, explained in detail in Section 3. The results indicate that there is a significant impact of adverse shocks on cognitive skills in childhood and adolescence, and that shocks in utero seem as important in determining subsequent human capital outcomes as shocks in the first year of life; there is very little evidence of an effect of shocks on non-cognitive skills in the same period. In addition, we present novel evidence that an important channel for these effects may be via maternal labor supply during pregnancy and infancy.

Our results also suggest that the impact of early shocks on cognitive and non-cognitive skills is somewhat attenuated by the third wave, in which the sample is observed as young adults, suggesting that children who have experienced more adverse shocks in infancy may catch up with those who did not experience those shocks. Finally, we find that parents invest more in the education of children who experienced adverse rainfall shocks, a pattern consistent with the decaying effects of early shocks.

The remainder of the paper proceeds as follows. Section 2 describes the data. Section 3 discusses the different channels through which rainfall shocks may influence early childhood outcomes, and provides some empirical evidence on how these channels respond to rainfall shocks. Section 4 describes the empirical strategy and presents the primary results of interest, and Section 5 presents robustness checks. Section 6 concludes.

2 Data

2.1 Household Data

The data set used in this paper is the Gansu Survey of Children and Families (GSCF), a panel study of rural children conducted in Gansu province, China. Gansu, located in northwest China, is one of the poorest and most rural provinces in China. Summary statistics, drawn from the first wave of the survey, are shown in Table 1. The average household in the sample reports income per capita of 1400 yuan or around $175 annually. Cropping, wage labor and self-employment in non-agricultural businesses are all important income sources, of which wage income is the most important. The average household size is four, indicating that the majority of households have more than one child.

[Table 1 here]

The first wave of the survey, conducted in 2000, surveyed a representative sample of 2000 children aged 9–12 in 20 rural counties, as well as their mothers, household heads, teachers, principals, and village leaders. All but one of these 2000 children have complete information in the first wave. The second wave, implemented in 2004, re-surveyed almost all of these children at age 13-16 and repeated the same battery of questionnaires, also adding a survey of fathers; 93.6% of the original sample, or 1872 children, were re-interviewed in the second wave, and 1701 completed achievement and cognitive tests that were administered in their schools.

The third wave, completed in early 2009, re-interviewed the original sample children (who at that time were aged 17 to 21) during Spring Festival, a peak time for young people to visit their parents' homes in rural China. If the sampled individual was not available, parents were asked questions about their child's education and employment status; however, skill measures could be collected only from the children who had returned to their parents' homes. Of the original 2000 children, 1437 (72% of the original sample) were interviewed directly and completed skill tests in this wave. In addition, information was collected for an additional 426 sample children by surveying their parents.

The household survey questionnaires in waves one (2000) and two (2004) were used to collect extensive information about schooling outcomes, household expenditure on education, child time use, time investments in education by parents, and child and parental attitudes, as well as more standard socioeconomic variables. In addition, a number of tests were administered to the sampled children. In the first wave, a general cognitive ability test developed by the Institute for Psychology of the Chinese Academy of Social Sciences was administered, testing common knowledge, abstract reasoning and mathematical skills. In both the first and second wave, grade-specific Chinese and mathematics achievement tests developed by the Gansu Educational Bureau were administered to sampled children to test their comprehension of the official primary school curriculum, whether or not they were enrolled in school. In the first wave, students were administered either the Chinese or math test but not both; in the second wave, students were administered both tests.

In the second and third waves, a literacy or "life skills" test was administered, modeled after the International Adult Literacy Surveys; the test assessed prose literacy, document literacy and numeracy. This test was not grade-level specific, and the wave two and three assessments, while similar, were not identical. The test was designed to assess individuals' ability to employ literacy and numeracy skills to function successfully in society. For more details about the cognitive assessment tools employed in this paper, see Glewwe, Huang and Park (2013a).

In addition, each wave of data collection included survey questions, posed to the sample children, that were designed to measure their non-cognitive skills. In the first and second waves, the survey measured both internalizing and externalizing behavioral problems: the former refers to intra-personal problems (depression, anxiety and withdrawal), and the latter to inter-personal problems (destructive behavior, aggression and hyper-activity). These two measures of non-cognitive skills are identical across the two survey waves, and both are constructed by recording the respondent's agreement or disagreement with a series of statements and then applying item response theory (IRT) to generate internalizing and externalizing scores. The scores are then standardized to have a mean of zero and a standard deviation of one.

There are inherent challenges to measuring non-cognitive skills during adolescence, a period where children's behavior may be volatile or rapidly changing. Glewwe, Huang and Park (2013a) found that first wave measures of non-cognitive skills in this sample, collected when the children were 9–12 years old, were more strongly correlated with subsequent labor market outcomes than second wave measures of non-cognitive skills, collected when the children were 13–17 years old. This suggests there may be greater noise or temporary variation in these measurements during adolescence. In the third wave, two other measures of non-cognitive skills were collected: the Rosenberg Self-Esteem Scale and a depression scale (CES-D). For young adults, these measures are considered more appropriate indicators of non-cognitive skills than internalizing/externalizing behavioral scores. Further detail about the construction of the non-cognitive skills measures can be found in Glewwe, Huang and Park (2013a).

2.2 Climatic and Grain Yield Data

The GSCF data are linked to monthly rainfall data from climate stations in China, interpolated to the latitude and longitude of the villages in the sample using the inverse-distance weighting method. Data from stations within 200 kilometers of the county of interest are employed unless there are fewer than three stations in that radius, in which case the radius is increased to 250 kilometers. On average, each measure of rainfall at the village level is constructed by interpolating between rainfall reports from six stations.[6] Rainfall is observed for all villages in the sample, but the top and bottom 1% of all rainfall measures are trimmed to avoid the influence of outliers. In addition, grain yield data are obtained from provincial agricultural yearbooks for each county and year, and are similarly trimmed.

It is also important to clarify our definition of a rainfall shock. Our primary specifications will regress various outcomes on rainfall measured in the specified village and year conditional on village and year fixed effects. Accordingly, while we construct the rainfall variable as a measure of rainfall in levels, conditional on village and year fixed effects we are identifying the impact of a level of rainfall that is unusually high or low relative to the mean in that locality (as well as the overall mean in that year). For this reason, we will describe our rainfall measure as a shock.

2.3 Sample

Our primary sample of interest will include children who have all non-cognitive and cognitive skills measures observed in both waves one and two. We restrict all of our analysis to this sample, thus excluding children who are not observed in the second wave, or who do not report cognitive or non-cognitive skills measures in both waves. Our primary sample consists of 1235 children. This number is smaller than the total number of children who completed cognitive testing in the second wave (1701) given that some observations are dropped due to extreme values of rainfall, or extreme values of non-cognitive scores or achievement test scores; observations corresponding to the top and bottom 1% of each indicator are dropped.

Only one child is observed in each household. The sample includes children in 98 villages, born in five different years; there are 398 locality-birth year cells observed. On average, three to four children are observed in a given locality-cohort cell.

[6]The minimum number of stations used to construct a rainfall measure is three; the maximum is nine. The 25th and 75th percentiles of the distribution of stations across villages are four and seven, respectively.

3 Channels

Before analyzing the impact of early childhood rainfall shocks, we first consider the different channels through which such shocks may influence cognitive and non-cognitive skill development. There are two primary channels through which adverse rainfall shocks can affect children in utero and in the first years of life, and thus affect their human capital and economic outcomes in later years: a decline in income, and a shift in maternal (and possibly paternal) labor supply. A third possible channel discussed in the literature is the deterioration of the disease environment through increased water contamination or an increased prevalence of vector-borne illnesses, which can impair the physical and neurological development of children and also the health of parents. Although we cannot rule out this channel, given that Gansu is generally an arid region without any significant prevalence of malaria or dengue fever, varying prevalence of water-borne diseases is unlikely to be a significant determinant of human capital outcomes.[7]

3.1 Income

In developing countries, adverse rainfall shocks (low rainfall) can lead to unexpected declines in crop yields and incomes, in many cases too large for risk-coping mechanisms to alleviate (Dercon, 2002; Townsend, 1995). A decrease in a household's income can reduce children's food consumption (and thus reduce child growth) both in utero and in early childhood. This is arguably the channel most commonly identified in the literature for the relationship between rainfall and subsequent human capital outcomes.

In Gansu, however, the relationship between rainfall and grain yield is not uniformly positive. Cultivated land generally has a single planting season, with most crops planted in early spring and harvested in the fall. Rainfall during planting season (which we define as the first half of the year, from January to June) is most beneficial for increasing grain

[7] In addition, we are not aware of any epidemiological or public health data available at the county or village level in Gansu that would allow us to test for any correlation between climatic shocks and negative health shocks.

yields. However, increased rainfall during the harvest season (which we define to be the second half of the year, from July to December) can harm crops and reduce yields. In Gansu, rains during this season often arrive in high volume over a concentrated period, damaging fields and crops and causing erosion (Li et al., 2002).

In order to quantify this relationship, we define $\tilde{S}_{vct}$ as rainfall for village v in county c and calendar year t, and $\tilde{S}^p_{vct}$ and $\tilde{S}^h_{vct}$ as rainfall in the planting and harvest season, respectively. We then regress grain yield G_{ct} as reported in county c and year t on rainfall, including village and year fixed effects. (No village-level data on grain yield is available.)

$$G_{ct} = \beta \tilde{S}_{vct} + \lambda_v + \nu_t + \epsilon_{vct} \tag{1}$$

The results are shown in Columns 1 and 2 of Table 2; standard errors are clustered at the county level. The first two columns show that while rainfall is generally associated with increased grain yield on average, the relationship in levels masks significant temporal heterogeneity. As expected given the above description of the cropping season, the correlation between rainfall and grain yield in the first half of the year is strongly positive, while the correlation between rainfall and grain yield in the second half of the year is negative and statistically significant.

3.2 Maternal labor supply

In addition to their effect on grain yield, rainfall shocks may also affect parental labor supply, independently of their effect on income. This may lead to the reduction of the investment of parental time in a child, or affect a child's health directly if there is an increase in maternal labor supply while the child is in utero. There is evidence in the medical literature that heavy physical labor during pregnancy lowers infants' birth weights (Tafari et al., 1980; Lima et al., 1999; Tuntiseranee et al., 1998), increases the risk of preterm delivery (Launder et al., 1990) and can affect other pregnancy outcomes (Barnes et al., 1991) in populations observed in Brazil, the Philippines, Thailand and Guatemala.

Low birth weight or other adverse outcomes at birth may, in turn, affect health and cognitive outcomes in childhood.

The literature on the relationship between maternal labor supply in infancy and infant health is more mixed. Some papers find little evidence of an adverse effect of increased maternal labor supply (Blau et al., 1996) or an adverse effect only of certain types of work (Glick and Sahn, 1998), while others find that maternal leave policies in developed countries increase infant health, primarily by increasing breastfeeding (Bakera and Milligan, 2008; Roe et al., 1996). It is also important to note that in general this literature faces the challenge of addressing the endogeneity of maternal labor supply, while our interest is in the response of maternal labor supply in agriculture to exogenous rainfall shocks.

In order to test the hypothesis that maternal labor supply may be an important channel, we examine the responsiveness of maternal and paternal labor supply in agriculture as reported in the first wave of the survey to rainfall shocks in that year. While the vast majority of mothers were not pregnant during this period, this exercise provides a general estimate of the seasonal responsiveness of female labor supply to rainfall shocks. (We should note, however, that one major limitation of this analysis is that the labor supply of mothers with younger children may behave differently in response to shocks.)

The specification of interest regresses days worked for the mother and father in agriculture over the past year in household i in village v in county c as reported in wave one, denoted L^m_{ivc} for mothers and L^f_{ivc} for fathers, on the shock observed in that year, $\tilde{S}_{vc}$. The rainfall station data employed here are available only through 2002, and thus only shocks in wave one are observed; county fixed effects κ_c are included in the regression.[8] We also estimate an analogous specification that distinguishes between the planting and harvesting season.

$$L^m_{ivc} = \tilde{S}_{vc} + \kappa_c \quad (2)$$

$$L^f_{ivc} = \tilde{S}^p_{vc} + \tilde{S}^h_{vc} + \kappa_c \quad (3)$$

[8]Village fixed effects cannot be included given that they would be collinear with the measures of rainfall at the village level.

In addition, we define an alternative village-level rainfall variable equal to the difference between rainfall as observed in 2000 and the village-level mean from 1987 to 2000; 1987 is the birth year of the eldest children in the sample. Re-estimating equations (2) and (3) employing the rainfall difference variables allows us to verify that the observed pattern does not reflect systematic variations in agricultural labor supply across villages being correlated with average rainfall differences, but rather identifies short-term responses to fluctuations in rainfall. The sample of interest here is restricted to the 1235 households that report uncensored rainfall measures in their children's year of birth; of these households, 11 households do not report paternal labor supply, and 152 do not report maternal labor supply. These households are omitted from the analysis.

The results reported in Columns (3) through (10) of Table 2 show that, on average, fathers' labor supply is negatively correlated with the rainfall observed in a given village (though this relationship is not statistically significant), while mothers' labor supply is uncorrelated. However, mothers significantly increase their labor in response to a positive rainfall shock in the planting season in the first half of the year; this result is evident in Column (6) and in Column (10) when employing the differenced rainfall variables. (There is some evidence that fathers also increase their labor during planting season in response to increased rainfall, though this result is not evident in the specification employing the differenced rainfall variables.) Qualitative work in the region suggests this primarily reflects additional labor in weeding. In addition, both parents reduce labor supplied in response to higher rainfall in the harvest season; rainfall in this period is negative for crops, and thus may reduce the size of the harvest and the labor required.[9]

[Table 2 here]

In order to clarify the magnitude of the relevant effects, we can note that the coefficients estimated in Column (10) suggest a one standard deviation increase in planting

[9]If the same specifications are re-estimated employing days worked in other income-generating activities, namely non-agricultural household businesses and animal husbandry, we observe that there is no response of maternal labor supply in non-agricultural businesses to rainfall fluctuations. The results for animal husbandry parallel the results reported here for agricultural labor, but are noisily estimated.

season rainfall leads to an increase in maternal labor supply of slightly over 16%, while a one standard deviation increase in harvest season rainfall leads to a decline in maternal labor supply of around 22%.[10] Again, a positive rainfall shock in the first half of the year is correlated with increased grain yield, but that also seems to lead to increased labor for mothers. This may lead to increased risk of adverse pregnancy outcomes for children in utero or increased health risks for infants. A positive rainfall shock in the second half of the year is negative for grain yield as already shown; it also seems to reduce maternal labor supply. The balance of these effects is unclear.

4 Empirical Strategy and Results

The primary specification of interest seeks to identify whether there is any persistent effect of early childhood rainfall on outcomes. In order to estimate these effects, we define a number of rainfall measures of interest. First, rainfall in utero and in years one and two of life are denoted S^{ut}_{vcmt}, S^{1}_{vcmt} and S^{2}_{vcmt} respectively for a child born in village v and county c in month m of year t. Rainfall in utero is defined as mean rainfall in the nine months prior to the month of birth; rainfall in the first year of life is mean rainfall from month zero (the month of birth) to month 11; and rainfall in the second year of life is mean rainfall from month 12 to month 23. (Rainfall in the third and fourth years of life, measures employed later in the analysis, are defined as rainfall from months 24 to 35 and from months 36 to 47, respectively.) These annualized measures of rainfall will be employed primarily in the robustness checks.

Second, we define seasonal shocks corresponding to the planting and harvest season as observed in each year of life. To define seasonal shocks for the in utero period, we consider the nine months prior to birth and calculate mean rainfall in months in that period that correspond to the planting season (January to June) and mean rainfall in months in that period that correspond to the harvest season (July to December). The number of months

[10]The key results also robust if the long-term mean of rainfall in the village is directly added as a control variable.

included in the calculation for the planting and harvest seasonal shocks in the in utero period thus varies between three and six. For the year one and year two seasonal shocks, we examine the relevant 12-month period (from the month of birth to 11 months after birth, and from 12 months after birth to 23 months of birth) and calculate the mean of planting (harvest) months falling within the period of interest. The number of months included in the year one and year two planting and harvest periods is thus six in each case.

To clarify the construction of the relevant variables, let us consider the case of a child born in March 1990. The in utero shock is the mean of rainfall as observed from June 1989 to February 1990 inclusive; the first year shock is the mean of rainfall observed between March 1990 and February 1991 inclusive, and the second year shock is the mean of rainfall observed between March 1991 and February 1992 inclusive. The in utero planting shock is the mean of rainfall observed in June 1989 and January and February 1990; the in utero harvest shock is the mean of rainfall observed from July to December 1990. The year one planting shock is the mean of rainfall observed from March to June 1990 inclusive and January and February of 1991; the year one harvest shock is the mean of rainfall observed from July to December 1990. In each case, we identify the months in the critical period of interest for a particular child that overlap with the relevant calendar season.

The specification of interest can be written as follows. Village, cohort and month fixed effects λ_v, ν_t and η_m are included, and Gen_{ivcmt} denotes gender. Note that the shocks of interest vary at the level of the village, month and year of birth. We focus on a linear specification rather than allowing for non-linear effects of rainfall on outcomes; this is consistent with previous literature, most notably Maccini and Yang (2009).

$$\begin{aligned} Y_{ivcmt} = {} & \beta_1 S^{ut,p}_{vcmt} + \beta_2 S^{ut,h}_{vcmt} + \beta_3 S^{1,p}_{vcmt} + \beta_4 S^{1,h}_{vcmt} + \beta_5 S^{2,p}_{vcmt} + \beta_6 S^{2,h}_{vcmt} \\ & + \lambda_v + \nu_t + \eta_m + Gen_{ivcmt} + \epsilon_{ivcmt} \end{aligned} \tag{4}$$

Standard errors are estimated employing two-way clustering by village and year of birth, following the method proposed by Cameron et al. (2011); thus allows for arbitrary within-group correlation in residuals among children born in the same village but different years, and among children born in the same year but different villages.[11]

4.1 Rainfall and Height-for-age

The first equation of interest regresses height-for-age, a summary measure of long-term health that has been found in the literature to be highly (negatively) correlated with early childhood malnutrition (Grantham-McGregor et al., 2007), on rainfall, employing specification (4). We employ a measurement of height captured in the second wave of the survey, when the children were 13 to 16 years old, and normalized to a Z-score using the World Health Organization standards for height-for-age. (Anthropometric data was only collected in the second wave; no anthropometric measures are available in wave one.) All rainfall shocks are standardized to have a mean of zero and a standard deviation of one.

The results of estimating equation (4) are presented in Column (1) of Table 3 and show that the global effect of rainfall on height-for-age is generally negative, though significant only for the harvest shock in utero. This evidence is consistent with rainfall in the planting season having an adverse effect by overtaxing pregnant or breastfeeding women, while rainfall in the harvest season lowers grain yield and nutritional availability and thus also has a negative effect on the health of children in utero or in infancy.

[Table 3 here]

4.2 Early Shocks and Cognitive Outcomes

Next, we seek to examine whether the same negative effect of rainfall is observed when the dependent variables are measures of cognitive skills. Columns (2) to (5) of Table 3 and Table 4 show the results of estimating this specification, employing as the dependent variable the cognitive and achievement tests in the three waves of the survey. All test

[11] The standard errors employing two-way clustering are estimated using the Stata command ivreg2.

scores are normalized to have a mean of zero and a standard deviation of one to enable comparison of effect sizes, and the rainfall shocks are similarly standardized. Accordingly, all coefficients can be interpreted as the effect size (in standard deviations) of a one standard deviation increase in rainfall on the specified outcome.

Table 3 reports the impact of early childhood shocks on tests administered in the first wave, when the children were 9–12 years old.[12] The coefficients are generally negative, consistent with individuals who were exposed to higher levels of rainfall in childhood showing weaker cognitive skills. The largest effects are observed for the shocks in the utero harvest season and the year one planting season. This suggests that the primary channels through which rainfall adversely affects cognitive development is via nutritional shortages in utero (when higher rainfall during the harvest period reduces nutritional availability) and via increased maternal labor supply in infancy (when higher rainfall during the planting period increases maternal labor supply).

The magnitude of the estimated coefficients suggests that a one standard deviation increase in rainfall in these two periods in utero and in year one leads to achievement test scores that are around 0.1–0.2 standard deviations lower. There is little evidence of a significant impact of rainfall in the second year of life.[13]

The results for the outcomes observed in waves two and three are found in Table 4. We again see evidence of substantial effects of early shocks on cognitive skills as measured in wave two of comparable magnitude, between .1 and .2 standard deviations. There is some evidence of attenuation in wave three, where only one shock measure in the second year of life shows a significant impact. However, this should be interpreted cautiously, given that the difference between the estimated coefficients for waves two and three is not statistically significant when both specifications are jointly estimated in a seemingly

[12] Again, in the first wave each child was administered either the math exam or the Chinese exam, but not both; the mean achievement test score is the average of the two scores.

[13] There is no significant heterogeneity in the estimated effect of early shocks on these measures when we compare across households with varying degrees of dependence on agricultural income as measured in wave one. This may be because even reported wage employment is linked to the agricultural sector and thus affected by climatic shocks, or because households' engagement in wage labor has increased rapidly following the birth of the sampled children. The rapid growth in wage employment is discussed in further detail in Section 5.

unrelated regression framework using the same sample.

[Table 4 here]

4.3 Early Shocks and Non-cognitive Outcomes

Table 5 reports the impact of early childhood shocks on non-cognitive skills: more specifically, indices of internalizing and externalizing behavioral problems as measured in the first and second waves, and self-esteem and an index of depression as measured in the third wave. The dependent variables are again normalized to have a mean of zero and a standard deviation of one. For the internalizing and externalizing indices and the depressive scale, a higher value indicates more behavioral problems and thus worse non-cognitive skills. For the Rosenberg scale, a higher value indicates improved self-esteem.

Here, there is little evidence of any significant relationship between early shocks and non-cognitive skills, with the exception of some significant coefficients indicating a negative impact of early shocks on self-esteem. Measuring non-cognitive skills is, of course, a non-trivial challenge, and it is possible that the failure to detect a significant effect partially or primarily reflects mismeasurement.

[Table 5 here]

In order to address this challenge, we also compile a series of more general reports about the child's behavior from his/her mother and teacher. This includes a general behavior index that is the mean of the response (by the mother or teacher) to a series of statements about the child's behavior, and responses by the mother to questions about whether her child is generally naughty and enjoys socializing. These measures also show generally insignificant relationships with rainfall shocks early in childhood.[14] This suggests that the null effect on non-cognitive skills is unlikely to be simply an artifact of noisy measurement of non-cognitive skills.

[14]Tabulations available on request.

4.4 Early Shocks and Schooling Outcomes

Table 6 shows the estimated coefficients for simple measures of progression through schooling. Here, the evidence generally suggests that, consistent with the observed results for cognitive skills, children who experience positive rainfall shocks have worse schooling outcomes. They enter primary school at an older age, are generally enrolled in lower grade level in both wave one and two, graduate from primary school at an older age, and are more likely to repeat a grade in wave two.[15]

[Table 6 here]

These results are not fully consistent across shock measures and thus should be interpreted with caution. However, it seems that adverse schooling outcomes may be another channel through which early childhood shocks affect human capital accumulation.

5 Robustness Checks

5.1 Additional Channels

Table 7 presents evidence about additional channels through which early childhood shocks may affect cognitive and non-cognitive outcomes in childhood. The specification of interest regresses various socioeconomic characteristics at the household level, measured in the first survey wave, on the climatic shock experienced in utero and in the first and second year of life for the index child; for concision, we now employ annualized rather than seasonal shocks, leading to the following specification.

$$Y_{ivcmt} = \beta_1 S^{ut}_{vcmt} + \beta_2 S^{1}_{vcmt} + \beta_3 S^{2}_{vcmt} + \lambda_v + \nu_t + \eta_m + Gen_{ivcmt} + \epsilon_{ivcmt} \tag{5}$$

[15]There are two significant coefficients that are not consistent with this story: a positive effect of the planting shock in year one on grade level in wave one, and a negative impact of rainfall shocks on the probability of skipping a semester in wave two.

The characteristics include household net income, per capita income, income from agriculture and livestock, income from wage labor and non-agricultural self-employment, assets, fixed capital, reported hours of work in the past week for the mother and father, land held in mu and irrigated land, value of agricultural inputs in yuan normalized by land held, and consumption reported in a variety of non-food categories over the past year.[16]

These regressions are designed to test for an alternate channel through which early childhood shocks could have a persistent effect on outcomes in childhood or young adulthood: namely, a persistent effect on the asset stock or income trajectory of the household.[17] The results demonstrate that there are generally no significant correlations between early shocks and household outcomes, and the coefficients estimated are of varying sign; while some significant coefficients are detected, the number does not exceed the number of false positives that would be predicted by chance given the number of coefficients estimated. It is also relevant to note that in the first wave, net income from wages constitutes a high proportion of total net income, constituting around 45% of total income for the median household. The absence of any significant relationship between prior rainfall shocks and household income is thus unsurprising.

[Table 7 here]

The salience of wage income may seem incongruous given that rainfall shocks in the years in which these sampled children were born are observed to have such a large impact on height-for-age, consistent with households that are primarily dependent on agriculture. Like many other interior provinces in China, Gansu experienced rapid growth in outmigration in the 1990s, the decade following the birth of the majority of the sample children (Rozelle et al., 1999). Accordingly, households that were once primarily agricultural have rapidly transitioned to a primary dependence on wage income. The lack of correlation

[16]The categories include allowance to children, household items, clothing consumption, appliances, transportation, communication, rent, electricity, fuel, cultural and educational services, medical services, and personal goods and services.

[17]The sample is restricted to those households where the children report measures of cognitive skills and thus are included in the main regressions.

between past rainfall shocks and later assets and income is consistent with the primary channel of impact in the main specification running through early childhood development, rather than a permanent effect on the trajectory of income or asset accumulation for the household.

One final channel that may mediate the relationship between shocks in the period of birth and later outcomes is birth timing. If parents time births to occur during months or years where climatic conditions are preferable, this may attenuate the relationship between rainfall shocks and later outcomes. If some parents are differentially able to time births – a plausible hypothesis – then children born in months or years with adverse shocks may be drawn from households disadvantaged along other dimensions.

In order to test this hypothesis, we construct a dataset at the month-village level, with the variable B_{vcmt} equal to the number of births observed in village v in county c in month m and year t. We employ months in the five years in which 99% of the sample children were born (1987–1991), and B_{vcmt} is set equal to zero for any month-village cell within the specified range for which no births are reported. We also define a dummy variable D^B_{vcmt} equal to one if any births are reported in a village-month cell. Finally, we define variables equal to the total number of births reported in a given village in a given month and year to households where the mother (father) is above (below) the median level of education, as well as corresponding dummy variables; parental education is employed as a proxy for parental availability to time births.

The specifications of interest regress the number of births observed or a dummy variable for observing any births on rainfall observed in a given village and month, and can be written as follows.

$$B_{vcmt} = S_{vcmt} + \lambda_v + \nu_t + \eta_m + \epsilon_{vcmt} \quad (6)$$

$$D^B_{vcmt} = S_{vcmt} + \lambda_v + \nu_t + \eta_m + \epsilon_{vcmt} \quad (7)$$

Village, cohort and month fixed effects are included in all specifications, and standard errors again employ two-way clustering with respect to village and year. The explanatory variable S is rainfall in the village of interest in the specified month; in some specifications, we restrict the sample to the months in which $B_{vcmt} > 0$, or at least one birth is reported.

The results in Table 8 show insignificant coefficients for the pooled sample in Columns (1) to (3).[18] In Columns (4) through (7), we employ the dummy variables for observing any births to high-education and low-education mothers and fathers. There is some marginally significant evidence that educated mothers are less likely to report births in high rainfall months, but in general differential birth timing does not seem to be a significant source of bias.

[Table 8 here]

5.2 Shocks Outside the Critical Period

The primary analysis assumes that the main channel through which early childhood rainfall shocks affect human capital development is via changes in nutritional availability or parental labor supply that affect children in utero or in infancy. If this assumption is true, then we should not observe significant impacts of rainfall shocks that occur outside the critical period. The primary analysis already included some evidence consistent with this assumption: there was less evidence of a significant impact of rainfall shocks even in the second year of life.

In order to further explore this channel, we estimate two additional specifications regressing human capital outcomes on annualized shocks two and three years prior to birth, and three and four years after birth.

$$Y_{ivcmt} = \beta_1 S_{vcmt}^{-3} + \beta_2 S_{vcmt}^{-2} + \lambda_v + \nu_t + \eta_m + G_{ivcmt} + \epsilon_{ivcmt} \tag{8}$$

$$Y_{ivcmt} = \beta_1 S_{vcmt}^{3} + \beta_2 S_{vcmt}^{4} + \lambda_v + \nu_t + \eta_m + G_{ivcmt} + \epsilon_{ivcmt} \tag{9}$$

[18] The full sample of locality-year-month cells is 5000, and positive births are observed in 1614 of these cells. These correspond to the number of observations observed in Columns (1) and (2) of Table 8.

Rainfall two years prior to birth is defined as average rainfall observed from the twelfth month before birth to the twenty-third month before birth, and similarly for rainfall three years prior to birth. (The period 9–12 months before birth is omitted to allow for some imprecision in the estimated date of conception.) Rainfall in the third and fourth years of life is defined as rainfall from months 24 to 35 and from months 36 to 47, respectively. The dependent variables employed are measures of cognitive skills in waves one, two and three.

The results of estimating equations (8) and (9) can be found in Table 9. In Panels A and B, we observe that there is no evidence of any correlation of shocks before and after the critical period with cognitive skills in wave one. In Panels C and D, there is some evidence of shocks in later years having a positive effect on cognitive tests. This could be consistent with the positive effect of rainfall on grain yield dominating the negative effect via maternal labor supply for children no longer in infancy.

[Table 9 here]

5.3 Alternate Specifications and Measurement Error

Our primary analysis entailed the definition of shocks during planting and harvest season based on the harvesting calendar for the dominant crops in Gansu; however, there is presumably also some local variation in crop seasonality. Accordingly, it is useful to re-estimate our primary results employing annualized shocks to verify their robustness. Accordingly, we re-estimate equation (5) using cognitive skills in all three waves as the dependent variable. The results are reported in Table 10 and the estimated coefficients are generally consistent, showing evidence of a substantial negative effect of rainfall shocks on cognitive skills as measured on waves one and two.

[Table 10 here]

In addition, we can exploit a related specification to examine whether measurement error in the explanatory variable, rainfall shocks in early childhood, is a significant source of bias. Given that the rainfall estimates were interpolated from local rainfall stations and data on the distance between rainfall stations and the sampled villages is available, it is possible to test for attenuation bias due to measurement error under the relatively simple assumption that this bias should be larger (and thus the estimated coefficients closer to zero) for localities further from weather stations.

In order to implement this test, we create a variable D_{vc} for village v in county c that corresponds to a dummy variable if the average distance from this locality to climatic stations within the radius used to construct the rainfall measures is above the median distance observed across all villages.[19] We then re-estimate the reduced form equation (5) including the interaction of each rain shock and D_{vc}, resulting in the following specification. (For simplicity given the number of interaction terms already included, we employ annual rather than seasonal rainfall shocks in this analysis.)

$$\begin{aligned} Y_{ivcmt} = \beta_1 S^{ut}_{ivcmt} + \beta_2 S^{ut}_{ivcmt} \times D_{vc} + \beta_3 S^{1}_{ivcmt} + \beta_4 S^{1}_{ivcmt} \times D_{vc} \\ + \beta_5 S^{2}_{ivcmt} + \beta_6 S^{2}_{ivcmt} \times D_{vc} + \lambda_v + \nu_t + \eta_m + G_{ivcmt} + \epsilon_{ivcmt} \end{aligned} \tag{10}$$

If measurement error is a major source of bias, then β_1 and β_2, β_3 and β_4, and β_5 and β_6 should be of opposite sign, suggesting attenuation in the coefficients of interest (a less negative effect of climatic shocks) for localities that are remote from weather stations. More specifically, employing height-for-age and cognitive skills measured in waves one, two and three as dependent variables, we expect the coefficients β_1, β_3 and β_5 to be negative based on the evidence already presented, in which case the coefficients β_2, β_4 and β_6 would be positive. The net effect of shocks for localities remote from weather stations would thus be closer to zero.

We estimate equation (10) and find that the linear combination $\beta_2 + \beta_4 + \beta_6$ is of heterogeneous sign and generally insignificant; it is positive and significant for only one

[19] The radius employed to construct rainfall measures was 200 kilometers, or 250 kilometers if less than three climatic stations were observed within 200 kilometers. The average distance between a village and the climatic stations employed to construct rainfall is 122 kilometers; thus the dummy variable of interest is defined equal to one if the average distance for a given village is greater than 122 kilometers.

outcome, and negative and significant for one outcome.[20] Thus there is no evidence that measurement error is a meaningful source of bias in the primary results.

5.4 Declining Importance of Shocks Over Time

There are at least two important reasons why early childhood shocks may have a diminishing impact on cognitive skills over time. First, there may be an inherent biological process in which children with impaired cognitive skills at an early age experience more rapid growth in those skills. Second, there may be compensatory investments made by parents or teachers that target children who experienced adverse shocks early in life. While we certainly cannot rule out the first channel, a process of innate decay, it cannot be directly substantiated in the data. However, there is evidence that the second channel is relevant in this context.

Table 11 presents evidence about how parental expenditure on children's education responds to differences in the early childhood shocks experienced by different children. The specification of interest is again equation (5), reproduced here, and the dependent variable is expenditure. For concision and given that parental labor supply is not a meaningful channel for this exercise, we employ the annualized shock measures.

$$Y_{ivcmt} = \beta_1 S^{ut}_{vcmt} + \beta_2 S^1_{vcmt} + \beta_3 S^2_{vcmt} + \lambda_v + \nu_t + \eta_m + G_{ivcmt} + \epsilon_{ivcmt}$$

Expenditure is separately reported in a number of categories in each wave, including tuition and a variety of discretionary expenditures, and the effects are also estimated for total expenditure; all expenditure measures are expressed as a proportion of the net income of the household.

In the first wave, there is some evidence of compensatory behavior: children who experienced higher rainfall in infancy, and thus worse human capital outcomes, show evidence of receiving more expenditure. However, in general parental expenditure is not

[20]Tabulations are not reported for concision, but are available upon request.

very responsive to early childhood shocks in the first wave.

In the second wave, however, there is considerable evidence of compensatory behavior, particularly when the sample is restricted to those households reporting positive values for expenditure as normalized by income. The magnitude of the coefficients suggests a one standard deviation increase in the shock experienced in utero or in the first year of life leads to an increase in around .01–.05 in expenditure relative to household income; the magnitude is large, but also noisily estimated. The results are consistent with the evidence presented in Leight (2014), another paper using the same dataset that analyzes the response of parental investment to differences in endowment between siblings using a household fixed effects framework and finds evidence of compensatory behavior.

[Table 11 here]

6 Conclusion

The role of early childhood shocks in shaping long-term economic outcomes has been an increasing focus in both the health and economics literatures in recent years. In this paper, we draw on a new and valuable source of evidence – an unusually detailed panel survey that tracks human capital outcomes over time in a poor, rural province in China – to examine how the impact of these shocks evolves over time, and how parental investments respond to such shocks.

Our evidence suggests that early childhood shocks, measured by rainfall in the village and year of birth, have a significant effect on children's height-for-age as well as on cognitive skills in primary school. While we cannot fully identify the channels for these effects, the primary channels seem to be via increasing maternal labor supply during critical periods of infancy, and nutritional deprivation during the in utero and infancy period. There is little evidence that these results reflect birth timing on the part of parents, or a persistent effect on household income or assets.

However, there is also evidence that, over time, children exposed to adverse shocks catch up with their peers who did not experience any shocks. By the third wave of the survey, at which point the children were between 18 and 21 years old, the effect of shocks on cognitive skills is attenuated. In addition, there is little evidence of a relationship between early shocks and non-cognitive skills at any age. This is consistent with the existing literature arguing that non-cognitive skills may be more malleable than cognitive skills. We also present suggestive evidence that the fading cognitive impact of early childhood deprivation reflects, at least to some degree, compensatory investments made by parents, who are more likely to invest expenditure and time in the education of children who were exposed to more adverse shocks.

Previous research on the relationship between parental and teacher investments and children's endowment has yielded conflicting results. Akresh et al. (2012), Rosenzweig and Zhang (2009), Bharadwaj et al. (2013), Almond et al. (2009), Adhvaryu and Nyshadham (2014), Frijters et al. (2013) and Aizer and Cunha (2012) find parents exhibit reinforcing behavior in Burkina Faso, China, Chile, Sweden, Tanzania and the United States, while Del Bono et al. (2012) find evidence of compensatory behavior in breast-feeding decisions with respect to birth weight in the U.S. and the U.K. In analyzing school-level investments, recent research in Vietnam and Peru has presented evidence that teachers in Vietnam target weaker-performing children to encourage them to meet a certain minimum standard level, while straggling students in Peru are ignored (Glewwe, Krutikova and Rolleston, 2013b). Evidence in sub-Saharan Africa and India summarized in Banerjee and Duflo (2011) also suggests that the educational systems in those countries primarily target the highest-achieving children and may leave lower-performing children behind.

This cross-country variation in the orientation of household and educational decision-makers towards under-performing children remains an interesting area for future exploration. From a policy perspective, the results in this paper are encouraging in that they suggest that at least in this context, it may be possible to reverse the negative cognitive impacts of early deprivation, and that households may already be motivated to make the investments necessary for this catch-up to occur.

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Table 1: Summary Statistics

Indicator	Mean	St. dev	Obs.
Net income: cropping	1234.2	2337.4	1896
Net income: livestock	62.1	1491.6	1896
Net income: wages	3572.3	5585.3	1896
Net income: self-employment	862.4	3898.1	1896
Land plot	4	5.4	1896
Housing square feet	81.1	60.4	1896
Household size	4.1	1.3	1896

Table 2: Rainfall, Grain Yield and Parental Labor Supply

	Grain yield		Father	Mother	Father	Mother	Father	Mother	Father	Mother
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Rainfall	.057**		-26.882	6.281						
	(.027)		(20.810)	(15.479)						
Planting rainfall		.217***			54.821**	77.575***				
		(.057)			(23.921)	(28.205)				
Harvest rainfall		-.065**			-70.562***	-55.614***				
		(.029)			(21.875)	(20.104)				
Rainfall dif.							-48.438**	-27.252*		
							(19.176)	(15.772)		
Planting rainfall dif.									5.862	26.906**
									(11.407)	(10.742)
Harvest rainfall dif.									-45.558***	-37.292***
									(14.328)	(13.979)
Fixed effects	Village + year		County		County		County		County	
Clustering	County		County		County		County		County	
Obs.	408	392	1224	1083	1224	1083	1224	1083	1224	1083

Notes: In Columns (1) and (2), the dependent variable is grain yield in the county and year, and the explanatory variable is rainfall in the village and year in the specified season; the planting season encompasses January to June, and the harvest season encompasses July to December. In Columns (3) through (6), the dependent variables are maternal and paternal labor supply in wave one, and the explanatory variables are rainfall at the village level in that year, as well as rainfall in the planting and harvest season. In Columns (7) through (10), the explanatory variables are rainfall at the village level in the specified season, with the village-level mean over the period 1987-2000 differenced out. Fixed effects and clustering are as specified in the table. Asterisks indicate significance at the ten, five and one percent level.

Table 3: Height-for-age and Wave One Cognitive Skills

	Height-for-age	Cognitive Skills			
		Cognitive	Math	Chinese	Total
	(1)	(2)	(3)	(4)	(5)
Utero shock planting	.029	-.008	-.098	.063	-.060
	(.055)	(.057)	(.128)	(.062)	(.106)
Utero shock harvest	-.144**	-.093**	-.243***	-.145	-.207***
	(.066)	(.039)	(.035)	(.139)	(.080)
Year 1 shock planting	-.021	-.133***	-.335	-.039	-.253*
	(.090)	(.049)	(.218)	(.122)	(.133)
Year 1 shock harvest	-.016	.050	.038	.005	-.018
	(.071)	(.040)	(.110)	(.111)	(.088)
Year 2 shock planting	.039	-.056***	-.037	.157	.036
	(.127)	(.015)	(.180)	(.162)	(.099)
Year 2 shock harvest	-.001	.031	.011	-.041	-.042
	(.068)	(.036)	(.108)	(.072)	(.067)
Obs.	1153	1235	612	623	1235

Notes: The dependent variables are height-for-age as measured in wave two and cognitive skills as measured in wave one, including a cognitive test, achievement tests in mathematics and Chinese, and the mean of the math and Chinese test scores. The dependent variables are standardized to have mean zero and standard deviation one. The explanatory variables are precipitation in the village of birth measured in the specified year and season, where year one of life is defined as the 12 months including the month of birth and the subsequent 11 months, year two of life is defined as months 12–23, and the in utero period is defined as the 9 months before birth. Seasonal shocks are identified employing the mean of rainfall in months in the specified period that overlap with the specified calendar season, where the planting season is January to June inclusive and the harvest season is July to December inclusive. Standard errors employ two-way clustering by village and year, and all specifications include village, year and month-of-birth fixed effects and a control variable for gender. Asterisks indicate significance at the ten, five and one percent level.

Table 4: Wave Two and Three Cognitive Skills

	Wave two				Wave three
	Literacy (1)	Math (2)	Chinese (3)	Mean achievement (4)	Literacy (5)
Utero shock planting	-.030	-.121**	-.063	-.092***	-.114
	(.071)	(.049)	(.058)	(.025)	(.105)
Utero shock harvest	-.142***	-.115**	-.064	-.089*	-.035
	(.032)	(.048)	(.060)	(.050)	(.066)
Year 1 shock planting	-.102*	-.217***	-.068	-.143***	-.047
	(.055)	(.073)	(.090)	(.054)	(.052)
Year 1 shock harvest	-.069	-.030	-.062	-.046	-.032
	(.081)	(.051)	(.075)	(.059)	(.069)
Year 2 shock planting	-.032	-.012	-.011	-.011	-.134***
	(.037)	(.088)	(.092)	(.075)	(.023)
Year 2 shock harvest	-.016	-.031	-.077**	-.054*	.003
	(.052)	(.039)	(.035)	(.033)	(.052)
Obs.	1235	1235	1235	1235	896

Notes: The dependent variables are cognitive skills measured in the specified wave, including a cognitive test, achievement tests in mathematics and Chinese, and the mean of the achievement test results in wave two, and a literacy test administered in wave three. The dependent variables are standardized to have mean zero and standard deviation one. The explanatory variables are precipitation in the village of birth measured in the specified year and season; for details, see the notes to Table 3. Standard errors employ two-way clustering by village and year, and all specifications include village, year and month-of-birth fixed effects and a control variable for gender. Asterisks indicate significance at the ten, five and one percent level.

Table 5: Non-cognitive Skills

	Wave one			Wave two			Wave three	
	Internalizing (1)	Externalizing (2)	Total (3)	Internalizing (4)	Externalizing (5)	Total (6)	Depression (7)	Rosenberg (8)
Utero shock planting	-.026*	-.017	-.022	.073	.083	.087	.071	-.144***
	(.015)	(.043)	(.024)	(.074)	(.067)	(.074)	(.120)	(.050)
Utero shock harvest	.058	.021	.040	.030	.116**	.083*	-.019	.079*
	(.091)	(.065)	(.079)	(.038)	(.049)	(.046)	(.063)	(.041)
Year 1 shock planting	.088	.058	.076	.054	.060	.064	-.015	-.052
	(.093)	(.061)	(.077)	(.033)	(.100)	(.066)	(.103)	(.085)
Year 1 shock harvest	-.064	-.005	-.035	.006	.021	.015	.058	.024
	(.053)	(.082)	(.068)	(.053)	(.040)	(.032)	(.076)	(.071)
Year 2 shock planting	-.055	-.088	-.076	-.109	-.003	-.061	-.060	-.126***
	(.046)	(.071)	(.058)	(.078)	(.043)	(.056)	(.078)	(.038)
Year 2 shock harvest	-.078**	-.082	-.084	-.008	-.017	-.014	-.008	.105***
	(.037)	(.067)	(.052)	(.045)	(.034)	(.037)	(.011)	(.037)
Obs.	1235	1235	1235	1235	1235	1235	879	875

Notes: The dependent variables are measures of non-cognitive skills in the specified waves, specifically indices of internalizing and externalizing behavioral problems, a depression index and a Rosenberg self-esteem index; a higher value for the internalizing and externalizing scores corresponds to more behavioral problems. The dependent variables are standardized to have mean zero and standard deviation one. The explanatory variables are precipitation in the village of birth measured in the specified year and season; for details, see the notes to Table 3. Standard errors employ two-way clustering by village and year, and all specifications include village, year and month-of-birth fixed effects, and a control variable for gender. Asterisks indicate significance at the ten, five and one percent level.

Table 6: Academic Attainment

	Wave one					Wave two			
	Primary entry age (1)	Grade level (2)	Skipped sem. (3)	Repeated grade (4)	Primary graduation age (5)	Enrollment (6)	Grade level (7)	Skipped sem. (8)	Repeated grade (9)
Utero shock planting	-.047	.047	.013**	-.011	.003	.002	.112	-.0005	-.031
	(.083)	(.058)	(.006)	(.050)	(.073)	(.008)	(.071)	(.005)	(.032)
Utero shock harvest	-.043	.021	.010	.028	-.057	-.015	-.020	-.001	.045
	(.047)	(.059)	(.007)	(.038)	(.083)	(.012)	(.078)	(.010)	(.055)
Year 1 shock planting	-.026	.142*	.009	-.030	.012	-.005	-.008	.021	.056
	(.051)	(.085)	(.007)	(.067)	(.063)	(.014)	(.087)	(.029)	(.048)
Year 1 shock harvest	.065	-.105*	-.003	-.003	-.006	-.037	-.133**	-.025	.054*
	(.065)	(.060)	(.011)	(.028)	(.069)	(.023)	(.063)	(.017)	(.028)
Year 2 shock planting	.138***	-.107*	-.012	-.010	.161***	.006	.008	-.004**	-.052
	(.036)	(.063)	(.009)	(.050)	(.051)	(.020)	(.052)	(.002)	(.054)
Year 2 shock harvest	.004	-.028	-.003	-.020	-.071	-.030*	-.086**	-.016	.038*
	(.053)	(.029)	(.007)	(.020)	(.044)	(.017)	(.042)	(.016)	(.023)
Obs.	1234	1223	1227	1233	1103	1233	1169	1233	1233

Notes: The dependent variables are measures of academic attainment, including age of entry in primary school, grade level, dummy variables for having skipped a semester or repeated a grade, and age of primary school graduation as reported in wave one; and enrollment, current grade level and dummy variables for having skipped a semester or repeated a grade as reported in wave two. The explanatory variables are precipitation in the village of birth measured in the specified year and season; for details, see the notes to Table 3. Standard errors employ two-way clustering by village and year, and all specifications include village, year and month-of-birth fixed effects and a control variable for gender. Asterisks indicate significance at the ten, five and one percent level.

Table 7: Additional Channels

Panel A: Income and assets

	Net income	Inc. per capita	Agri., livestock income	Wage, self empl. income	Assets	Fixed capital
	(1)	(2)	(3)	(4)	(5)	(6)
Utero shock	477.489	44.352	-197.370*	463.184	-.914	-.354
	(489.936)	(43.350)	(116.267)	(294.357)	(.601)	(.310)
Year 1 shock	-374.417	-40.476	-.741	-397.652	-2.152***	-.403
	(366.182)	(30.797)	(112.186)	(249.829)	(.688)	(.371)
Year 2 shock	117.358	-.526	100.879*	-143.866	-.270	-.334
	(379.064)	(31.231)	(57.910)	(208.926)	(.468)	(.309)
Mean dep. var.						
Obs.	1235	1235	1235	1235	1235	1235

Panel B: Other outcomes

	Mother time	Father time	Land	Irrigated land	Agri. inputs	Consumption
	(1)	(2)	(3)	(4)	(5)	(6)
Utero shock	-.212	-1.371	.022	-.168	-46.437	-2.517
	(4.379)	(4.407)	(.262)	(.382)	(42.720)	(132.579)
Year 1 shock	2.618	1.600	-.095	.329	-27.463	-19.151
	(12.269)	(5.543)	(.325)	(.339)	(36.233)	(130.270)
Year 2 shock	8.131*	9.669	.026	-.556	-19.941	73.720
	(4.246)	(7.335)	(.258)	(.423)	(24.889)	(95.229)
Mean dep. var.						
Obs.	1235	1235	1235	1235	1235	1235

Notes: The dependent variables in Panels A and B are measures of household characteristics as reported in wave one: net income per capita from four primary sectors (agriculture, livestock, wage earnings and non-agricultural household business), assets and fixed capital per capita, time spent working by the mother and father, land cultivated in mu, value of agricultural inputs in yuan normalized by land held, and loans owed by and to the household. The explanatory variables are precipitation in the village of birth measured in the specified year; for definitions of these variables, see notes to Table 3. Standard errors employ two-way clustering by village and year, and all specifications include village, year and month-of-birth fixed effects and a control variable for gender. Asterisks indicate significance at the ten, five and one percent level.

Table 8: Birth Timing

	Births per month		Births dummy	Births dummy			
				Low-educ. mother	High-educ. mother	Low-educ. father	High-educ. father
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Monthly rainfall	-.005	-.043	.003	.031	-.028*	-.017	-.014
	(.032)	(.034)	(.026)	(.026)	(.015)	(.038)	(.020)
Mean dep. var.	.298	1.237	.241	.496	.583	.513	.581
Sample	All	$B_{vcmt} > 0$	All	$B_{vcmt} > 0$	$B_{vcmt} > 0$	$B_{vcmt} > 0$	$B_{vcmt} > 0$
Obs.	6000	1445	6000	1445	1445	1445	1445

Notes: The dependent variables are the number of births in a given month in a given village, a dummy variable equal to one if any births are reported in a given month in a given village, and dummy variables equal to one if any households in which the mother (father) is above (below) the median level of education report births in a given month in a given village. The explanatory variable is monthly rainfall. The specifications in Column (2) and Columns (4) through (7) are restricted to months in which at least one birth is observed in the sample. Standard errors employ two-way clustering by village and year, and all specifications include village, year and month-of-birth fixed effects. Asterisks indicate significance at the ten, five and one percent level.

Table 9: Placebo Tests

Panel A: Wave one test scores and pre-critical period shocks					
	Cognitive (1)	Math (2)	Chinese (3)	Total (4)	
Minus three shock	.025	.164	.053	.080	
	(.028)	(.159)	(.118)	(.099)	
Minus two shock	.047	-.021	-.052	-.049	
	(.029)	(.098)	(.082)	(.054)	
Obs.	1235	612	623	1235	
Panel B: Wave one test scores and post-critical period shocks					
Year 3 shock	-.010	.048	.039	.097	
	(.057)	(.162)	(.125)	(.130)	
Year 4 shock	.023	-.191	.036	-.034	
	(.043)	(.116)	(.150)	(.120)	
Obs.	1235	612	623	1235	
Panel C: Wave two test scores and pre-critical period shocks					
	Literacy (1)	Math (2)	Chinese (3)	Total (4)	Literacy wave 3 (5)
Minus three shock	.019	.010	-.066	-.028	.217***
	(.041)	(.046)	(.063)	(.048)	(.060)
Minus two shock	.067	.016	.051	.033	.112*
	(.081)	(.039)	(.032)	(.023)	(.067)
Obs.	1235	1235	1235	1235	896
Panel D: Wave two test scores and post-critical period shocks					
Year 3 shock	-.012	.071	.112***	.091***	-.040
	(.057)	(.045)	(.042)	(.035)	(.061)
Year 4 shock	.060	.035	.123**	.079*	.026
	(.056)	(.066)	(.058)	(.044)	(.080)
Obs.	1235	1235	1235	1235	896

Notes: The dependent variables are cognitive skills as measured in the specified wave; for details, see notes to Tables 3 and 4. The dependent variables are standardized to have mean zero and standard deviation one. The explanatory variable is precipitation in the village of birth measured in the specified year. The minus three shock is defined as mean rainfall 35–24 months before birth, the minus two shock is defined as rainfall 23–12 months before birth, the year three shock is defined as mean rainfall 24–35 months after birth, and the year four shock is defined as mean rainfall 36–47 months after birth. Standard errors employ two-way clustering by village and year, and all specifications include village, year and month-of-birth fixed effects, and a control variable for gender. Asterisks indicate significance at the ten, five and one percent level.

Table 10: Annual shocks

Panel A: Height-for-age and wave one test scores

	Height-for-age (1)	Cognitive (2)	Math (3)	Chinese (4)	Mean achievement (5)
Utero shock	-.149	-.084*	-.417***	-.197	-.317***
	(.138)	(.047)	(.126)	(.133)	(.107)
Year 1 shock	-.027	.002	-.185	-.018	-.176
	(.111)	(.051)	(.221)	(.179)	(.168)
Year 2 shock	.012	.006	-.050	-.009	-.078
	(.080)	(.041)	(.128)	(.124)	(.097)
Obs.	1153	1235	612	623	1235

Panel B: Wave two and three test scores

	Wave two				Wave three
	Literacy (1)	Math (2)	Chinese (3)	Mean achievement (4)	Literacy (5)
Utero shock	-.072	-.128*	-.056	-.092*	.015
	(.075)	(.073)	(.045)	(.053)	(.090)
Year 1 shock	-.090	-.126	-.092	-.109	-.028
	(.095)	(.083)	(.079)	(.073)	(.073)
Year 2 shock	-.022	-.059	-.096**	-.077*	-.019
	(.062)	(.059)	(.044)	(.043)	(.070)
Obs.	1235	1235	1235	1235	896

Notes: The dependent variables are height-for-age and measures of cognitive skills in waves one and two as reported in Tables 3 and Table 4. The dependent variables are standardized to have mean zero and standard deviation one. The explanatory variable is precipitation in the village of birth measured in the year in utero, in year one and in year two; for the definition of these variables, see the notes to Table 3. Standard errors employ two-way clustering by village and year, and all specifications include village, year and month-of-birth fixed effects, and a control variable for gender. Asterisks indicate significance at the ten, five and one percent level.

Table 11: Parental Investments

Panel A: Wave one parental expenditure

	Total (1)	Tuition (2)	Supplies (3)	Transport. (4)	Tutoring (5)
In utero shock	.009 (.025)	.003 (.010)	.0005 (.001)	.002 (.002)	-.011 (.009)
Year 1 shock	-.019 (.026)	-.008 (.009)	.002 (.002)	.003*** (.0008)	-.011 (.009)
Year 2 shock	-.009 (.030)	-.002 (.011)	.001 (.002)	.001*** (.0002)	-.0006 (.005)
Obs.	1235	1235	1235	1235	1235

Panel B: Wave one parental expenditure conditional on positive expenditure

	Total (1)	Tuition (2)	Supplies (3)	Transport. (4)	Tutoring (5)
In utero shock	.017 (.017)	.006 (.008)	.001 (.001)	.009 (.010)	-.008 (.008)
Year 1 shock	.016 (.015)	.006 (.005)	.004** (.002)	.040 (.051)	-.008 (.009)
Year 2 shock	.023 (.017)	.010 (.007)	.004*** (.001)	.043 (.035)	.0003 (.006)
Obs.	1149	1146	1126	65	1204

Panel C: Wave two parental expenditure

	Total (1)	Tuition (2)	Supplies (3)	Transport. (4)	Food (5)	Tutoring (6)	Other (7)	Uniform (8)
In utero shock	.066 (.068)	.033 (.036)	.008 (.007)	.004 (.006)	.013 (.018)	.002 (.002)	.006** (.002)	.017** (.008)
Year 1 shock	.096 (.078)	.049 (.041)	.015 (.009)	.006 (.004)	.013 (.011)	.004** (.002)	.010* (.006)	.024** (.011)
Year 2 shock	.023 (.060)	.018 (.032)	.003 (.007)	.003 (.003)	.005 (.009)	.002** (.0009)	.004 (.003)	.006 (.010)
Obs.	1235	1235	1235	1235	1235	1235	1235	1235

Panel D: Wave two parental expenditure conditional on positive expenditure

	Total (1)	Tuition (2)	Supplies (3)	Transport. (4)	Food (5)	Tutoring (6)	Other (7)	Uniform (8)
In utero shock	.085 (.059)	.038 (.025)	.009 (.006)	.014 (.013)	.050 (.031)	.006 (.005)	.004 (.004)	.018** (.008)
Year 1 shock	.170** (.081)	.085** (.040)	.019** (.010)	.037* (.019)	.072** (.034)	.014** (.007)	.016 (.015)	.027*** (.010)
Year 2 shock	.081* (.041)	.045** (.021)	.008 (.006)	.019 (.015)	.045** (.018)	-.003 (.003)	.007 (.008)	.010 (.007)
Obs.	1151	1151	1099	315	450	304	425	1172

Notes: The dependent variables are household educational expenditure in the specified category on the child of interest normalized by household net income. The explanatory variables are precipitation in the village of birth measured in the specified year; for definitions of these variables, see notes to Table 3. Standard errors employ two-way clustering by village and year, and all specifications include village, year and month-of-birth fixed effects, and a control variable for gender. The results in Panels B and D are restricted to the sample that reports positive values for expenditure normalized by net income. Asterisks indicate significance at the ten, five and one percent level.