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The Long-Term Effects of Early-Life Exposure to Weather Shocks: Evidence from Tanzania

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Abstract

We examine whether early-life exposure to rainfall shocks has a long-term impact on health, education, and the socioeconomic statuses of individuals in rural Tanzania, where livelihoods heavily depend on rain-fed agriculture. We use a unique panel of data from a Kagera Health and Development Survey (KHDS) in which children were followed from childhood (1991) to adulthood (2010) together with historical rainfall data. We apply a sibling fixed-effect estimator to overcome potential endogeneity issues. We find that rainfall in birth year affects the education and socioeconomic statuses of children in adulthood. A 15 percent increase in rainfall in one's birth year and birth village (relative to average village rainfall) leads children to have 0.21 more years of schooling and live in a household in 2010 that scores 0.19 higher on an asset index. We then explore the relationship between early-life rainfall and childhood nutritional status to identify early-life rainfall's initial effect. We find that higher birth-year rainfall leads to significant decreases in height and weight deficits in children. A 15 percent increase in rainfall in one's birth year and birth village (relative to average village rainfall) improves height-for-age z score by 0.20 and weight-for-age z score by 0.26. When taken together,

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our results point to the importance of early childhood nutrition intervention. Sensitivity checks show that the results are robust to sample selection.

Keywords: rainfall shocks; malnutrition; long-term outcomes; children; Tanzania.

1 Introduction

A growing body of literature in development economics documents the long-term effect of exposure to shocks in early life. Recent evidence suggests that exposure to early-life shocks during gestation and early childhood is associated with adverse later-life outcomes, such as poor health and lower educational and socioeconomic status (Glewwe et al., 2001; Case et al., 2005; Alderman et al., 2006; Almond, 2006; Maccini and Yang, 2009). Most of the studies in the area come from developed countries, and there is relatively little work on developing countries due to a lack of data.[1] Given that the nature and frequency of early-life shocks and the availability and effectiveness of mitigation strategies are very different between developed and developing countries, much remains to be discovered about the long-term effects of early-life shocks in the context of developing countries. This paper examines whether early-life exposure to rainfall shocks has a long-term impact on individuals' health, education, and socioeconomic status in Tanzania.

The majority of previous research on developing countries examines the impact of more extreme types of early-life shocks such as civil war, famine, and pandemics (see, for example, (Alderman et al., 2006; Chen and Zhou, 2007; Meng and Qian, 2009; Umana-Aponte et al., 2011; Ampaabeng and Tan, 2013; Dercon and Porter, 2014). Nevertheless, rural households in developing countries commonly face less extreme shocks like rainfall shocks. Because a large proportion of the agricultural area in developing countries (more than 95 percent in sub-Saharan Africa) is rainfed, fluctuations in rainfall levels result in significant variations in agricultural output and farm incomes (Wani et al., 2009). Since there are limited social security arrangements and incomplete insurance markets in these countries, the reduction of agricultural output following an adverse rainfall shock generates a decrease in consumption. It could therefore have long-term implications for the welfare of rural households. To design policies that have high relevance for the rural population in developing countries, understanding rainfall shock's long-term effect is of prime importance.

While a growing number of recent studies look at the association between early-life weather shocks and child outcomes, they have focused on short-term effects. On the other hand, most studies analyzing the long-term effects of exposure to early-life weather shocks (Godoy et al., 2008; Maccini and Yang, 2009; Thai and Falaris, 2014; Cornwell and Inder,

[1]Only a few datasets in Africa track individuals from childhood into adulthood.

2015; Zamand and Hyder, 2016; Chi et al., 2018; Fitz and League, 2020; Lin et al., 2021; Nübler et al., 2021; Yamashita and Trinh, 2021; Chang et al., 2022) also have some limitations. In nearly all studies, the outcome variables are observed only during adulthood and lack measurement of early life variables except for weather shocks. As a result, the studies do not indicate a link between exposure to weather shocks in early life and childhood health and nutritional status and, consequently, cannot establish the initial effect. Moreover, only a few studies compare how robust the results are to the problems caused by selective mortality due to a lack of data[2].

This paper provides new evidence of the long-term effect of early-life exposure to weather shocks using data fitting to address the aforementioned drawbacks. More specifically, the study examines the impact of early-life rainfall shock on adult health, education, and socioeconomic outcomes of individuals in Tanzania. We then explore the relationship between early-life rainfall and childhood nutritional status, measured by height-for-age and weight-for-age, to establish the initial effect. Given that previous researches provide mixed evidence regarding the timing of exposure to shocks, we also identify the stage in the early-life cycle during which children are most vulnerable to rainfall shocks.

We use a unique panel data from the Kagera Health and Development Survey (KHDS) in which children were followed from childhood (1991) to adulthood (2010). We also link historical rainfall data with the longitudinal individual data. Rainfall shock is measured using deviations from the long-run village average. Since crop production in Tanzania is predominantly rain-fed, higher rainfall is linked to higher income and better nutrition through its effect on harvests. We apply a sibling fixed-effect estimator to control for any unobserved family and locality characteristics that may affect long-term outcomes. We allow for year of birth and season of birth fixed effects to control for any unobservables that vary with year and season of birth. We also have controls for birth order and gender effects.

We find that rainfall in birth year affects the education and socioeconomic status of children in adulthood. A 15 percent increase in rainfall in one's birth year and birth village (relative to average village rainfall) leads children to have 0.21 more years of schooling and live in a household in 2010 that scores 0.19 higher on an asset index. Such an increase in

[2]For instance, Maccini and Yang (2009) depend on the relationship between birth year rainfall and cohort size in the district to test how selective mortality affects their result. They lack longitudinal data to check whether birth year rainfall affects the likelihood of survival to adulthood.

birth-year rainfall is also associated with a 30 percent increased likelihood to report rich economic status. By contrast, we find no significant relationship between birth-year rainfall and health outcomes in adulthood.

We then explore the relationship between early-life rainfall and childhood nutritional status to identify early-life rainfall's initial effect. We find that childhood nutritional status varies with birth-year rainfall. Higher birth-year rainfall leads to significant decreases in height and weight deficit in children relative to children from a well-nourished population. A 15 percent increase in rainfall in one's birth year and birth village (relative to average village rainfall) improves height-for-age z score by 0.20 and weight-for-age z score by 0.26. Compared with rainfall in the birth year, rainfall in the years before and after the birth year has no statistically significant relationship with childhood nutritional status and adult outcomes. The result suggests that rainfall shock in the year of birth matters the most. Because rainfall shock translates into income shock and food shortage with delay, nutritional deprivation resulting from adverse rainfall shock in a birth year likely occurs during the second year of life. This implies that nutritional intake during the second year of life is vital in influencing childhood health, adult education, and socioeconomic status. Overall, our findings suggest that anti-poverty interventions that promote the provision of nutritional supplements during the postweaning period have a significant long-run payoff. In addition, the findings underscore the importance of policies that help rural households smooth consumption.

Our results add to the growing body of research highlighting the period after weaning from breast milk as a stage during which children are most vulnerable to shocks. Glewwe and King (2001) find that malnutrition in the second year of life (compared to malnutrition in utero and the first year of life) has a more significant negative impact on cognitive development in the Philippines. Hoddinott and Kinsey (2001) also report that children aged 12 to 24 months exposed to drought in rural Zimbabwe lose 1.5–2 cm of growth after the drought. In a similar work, Alderman et al. (2006) show that children who were malnourished from 12 to 24 months of age are shorter and attain less schooling by adulthood than well-nourished children. In a study from Ethiopia, Dercon and Porter (2014) find that children exposed to famine from 12 months to 36 months are shorter, less likely to have finished primary school, and more likely to be ill by adulthood than older cohorts who were not exposed. Furthermore, Ampaabeng and Tan (2013) report that children who were newborn to 2 years of age at the time of famine in Ghana score lower on intelligence tests compared to older cohorts.

In related work, Alderman et al. (2009) also examine whether childhood nutrition affects future education in the Kagera region in Tanzania using data from the 2004 Kagera Health and Development Survey (KHDS). Their principal findings are that malnutrition in childhood delays school entry and lowers years of schooling attained in Tanzania. While there are some similarities between Alderman et al. (2009) and our work, there are three major differences that make our work unique. First, the authors use crop loss reported by a household as well as flood and drought weather shocks at the community level as instruments in estimating the effect of childhood health and nutrition status on educational outcomes, while we study the reduced form effect of early life rainfall shock directly using historical rainfall data. The direct consideration of the reduced form equation in our case, as opposed to the structural equation, is less likely to suffer from omitted variable bias. Moreover, since there are potentially many weaknesses associated with self-reported variables such as response bias, it is also advantageous to use an objective measure of rainfall shock.

Second, we focus on a wide range of outcomes instead of only on education outcomes, as in Alderman et al. (2009). Finally, outcome variables are observed in individuals' teenage years in Alderman et al. (2009), while we observe outcomes in both childhood and young adulthood. The average age of sample children in Alderman et al. (2009) is 16 years, while the average age in our data, as will be shown, is 23 years, with 80 percent of the sample older than 20 years of age.

The remainder of the paper is organized as follows: The next section presents a theoretical framework for the study, and Section 3 introduces the empirical model. Section 4 provides a brief description of the study area, the Kagera region of Tanzania. Section 5 discusses the data drawn from the Kagera Health and Development Survey (KHDS). Section 6 presents the empirical results, along with robustness checks. In the final section of the paper, Section 7, we summarize the findings and offer concluding remarks.

2 Theoretical Framework

There are several welfare indicators in adulthood that can potentially be explained by environmental conditions in the period around infanthood. For this paper, we chose to focus on adult economic wellbeing, health, and education. Accordingly, the welfare of a given adult at time (t) is given by the following vector of indicators:

$$W_t = [Y_t, H_t, E_t] \tag{1}$$

where Y_t is economic wellbeing as represented by consumption and wealth, H_t is health capital, and E_t is educational attainment.

In line with human capital theory, economic wellbeing at time (t) is determined, among other things, by the health and education capital of an individual:

$$Y_t = y(H_t, E_t, X_t, Z) \tag{2}$$

where X_t is a vector of other time-variant factors affecting economic wellbeing, and Z is a vector of time-invariant variables affecting economic wellbeing.

Adult health and education outcomes, in turn, can be defined as functions of childhood human capital. According to Grossman (1972), individual health at time t is given by a health production function consisting of initial health endowment, H_0, as well as a series of health inputs, N, acquired over time. The health production function may also consist of the demographic characteristics of the individual, D, and a vector of community-level infrastructure, disease, and environmental variables, C.

$$H_t = h(H_0, N_1, \ldots, N_t, D, C_1, \ldots, C_t) \tag{3}$$

Adult educational outcome is given by an educational production function. Bowles (1970) defines an educational production function as ”the relationship between school and student inputs and a measure of school output” (p. 11). Hence, educational attainment in adulthood is a function of a series of individual educational and community- or school-level inputs acquired since early childhood. Apart from a flow of inputs, the initial endowment of cognitive capacity holds a cascading effect on education through adulthood. We assume that childhood health is a good proxy of initial cognitive capacity. Therefore, the educational production function can be written as follows,

$$E_t = e(H_0, I_1, \ldots, I_t, S_1, \ldots, S_t) \tag{4}$$

where I represents individual educational inputs, and S represents community- or school-level inputs. Equations 2–4 show that initial health endowment holds a direct or indirect effect on all of the adulthood welfare indicators specified in 1. Therefore, it is vital to have a good grasp of factors that are expected to affect initial health endowment. In this regard, initial health endowment is partly determined by genetic characteristics passed on from parents, G. Moreover, childhood health is a function of environmental conditions experienced in early life including at the fetal stage or during infancy, R_0. Finally, community-level health infrastructure and disease environment in early-life, C_0, influence childhood health.

$$H_0 = k(G, R_0, C_0) \tag{5}$$

Combining 2–5, adulthood welfare can be written as a function of environmental conditions in early life, individual demographic variables, and a series of household socioeconomic and genetic indicators and measures of community-level infrastructure and disease environment. Econometrically, this relationship can be estimated as a reduced-form function of early-life rainfall, individual demographic variables, and family fixed effects.

3 Estimation Strategy

This study aims to determine whether exposure to early-life rainfall shock affects outcomes later in life. We estimate the following reduced-form relationship in which an individual's adult outcome is posited to depend on early-life rainfall, year of birth, season of birth, birth order, gender, and family fixed effects.

$$Y_{ijt} = R\beta + X\alpha + \tau_t + \delta_t + \mu_j + \varepsilon_{ijt} \tag{6}$$

Where Y_{ijt} represents an outcome for individual i in family j at time t. The vector R includes rainfall variables from three years before birth to three years after birth. There are mixed findings in the literature on child nutrition(Barker, 1998; Glewwe and King, 2001; Hoddinott and Kinsey, 2001). We include rainfall at various points in early life to compare its impact on outcomes in adulthood.

X is a vector of child-level control variables, including birth order and gender. We control for birth order because competition with siblings for resources likely affects a child's nutritional status. τ is a year-of-birth fixed effect to account for any differences due to the child's year of birth. δ is a season of birth fixed effect to account for the fact that parents may time children to be born in particular seasons (Artadi, 2005). μ_j is a family-specific fixed effect that controls for any unobserved family characteristics that may affect adult outcomes, and ε_{ijt} is a mean zero error term. The coefficient β is the parameter of primary interest and represents the effect of rainfall shock during various stages of early life on outcomes in adulthood. Because the fixed effects absorb any unobservables that vary with the year, season, or household, the effect we pick up will be that of rainfall shock at various early-life stages.

Empirical studies of the long-term effect of early-life conditions are haunted by the fact that unobserved factors may influence early-life health and subsequent adult outcomes. This study identifies the links between early-life health and long-term outcomes indirectly through rainfall variations. Although causality is less difficult to ascertain in rainfall data, one may question the exogeneity of early-life rainfall because households could choose settlements based on rainfall distribution. For instance, Frankema et al. (2017) show that rainfall levels and variability are associated with district-level population density in tropical Africa. Another possible concern is that rural families could change fertility patterns in response to rainfall shock. For instance, Shah and Steinberg (2017) argue that rural mothers may delay fertility during drought because fathers could migrate in search of work. We apply a sibling fixed-effect estimator to control for any unobserved family and locality characteristics that may affect long-term outcomes. By using within sibling estimation, in addition to addressing the concerns above, we can rule out potential attrition bias resulting from household and locality characteristics.

We use a sibling fixed-effect model for continuous outcomes and a fixed-effects logit model or Chamberlain's conditional logit model (Chamberlain, 1980) for dichotomous outcomes. We need multiple observations (siblings) to estimate a fixed-effect model. In the simplest case of two siblings per household, differencing the observations within a family eliminates the family fixed effect. The model then estimates the rainfall effect by regressing within-family differences in the outcome on the differences in the independent variables. Identifying the effect of early-life rainfall on adulthood outcomes relies on differences in rainfall at early life between or among siblings. Given that the birth village is the same among siblings, within-family variation in rainfall exists due to differences in the year of birth and season of birth. In the fixed-effects logit model, only families with a difference in siblings' outcomes contribute to the likelihood function. Families in which all siblings have the same outcome are not used in estimating the parameters of the model, which reduces the effective sample size.

4 Background

Kagera is a region in the northwestern part of Tanzania. It is one of Tanzania's most remote areas, situated about 1400 kilometres from the capital Dar es Salaam. The region contains a large part of Lake Victoria and shares borders with Uganda in the north and Burundi and Rwanda in the west. Kagera is divided into five districts: Biharamulo, Bukoba, Karagwe, Muleba, and Ngara.

According to the 2012 census, the population of Kagera is 2.5 million (URT, 2012b). Kagera is overwhelmingly rural, with more than 80 percent of the region's economically active population engaged in agriculture (URT, 2012a). The agricultural production system in the region is primarily rain-fed. Only 2 percent of the households involved in agriculture reported having access to irrigation in 2007/08 (URT, 2012a). The farming system is also characterized by traditional cultivation methods and low input low yield subsistence agriculture. Furthermore, agriculture in the region is mostly dominated by smallholder farmers.

Kagera climate consists of two rainy seasons and two dry seasons. The short rainy season (Vuli) occurs between October and December, and the long rainy season (Masika) occurs between March and May. Kagera experiences a long dry season between June and September and a short dry season between January and February. The long rains are more intense and less variable compared to the short rains. The data used in this paper shows that Kagera receives, on average, 37 percent of total annual rainfall during the long rainy season and 33 percent of total annual rainfall during the short rainy season.

Most of the Kagera region has two cropping seasons a year under rainfed conditions. Crop production covers a wide range of crops, including cash crops – such as coffee, tea, and cotton – and food crops, such as bananas, beans, maize, and cassava. Planting for annual crops typically occurs in March, while harvesting occurs in July and August.

Because agricultural production systems are mostly rain-fed, the impact of rainfall variability is highly pronounced in Tanzania. Studies have shown the importance of rainfall variation in explaining crop yield. Rowhani et al. (2011) examined the relationship between rainfall variability and crop yields in Tanzania by focusing on maize, sorghum, and rice. They found that higher rainfall is associated with increased yields. The result also showed that increased precipitation variability during the growing season harms average yields. Lema and Majule (2009) find that shortening of the rainy season reduces yields. Moreover, Bengtsson (2010) reports that households in the Kagera region are exposed to significant income fluctuation resulting from rainfall shocks.

Apart from its effect on harvest and farm income, rainfall may affect child nutrition and health through other channels. For instance, by changing the relative cost of time, rainfall may affect parental time between farm work and child-rearing. Positive rainfall shock may imply more work on the farm and less time for child-rearing, while the opposite may hold true for negative rainfall shock. Additionally, high rainfall may lead to the development of parasitic and infectious diseases such as malaria and cholera. Exposure to infectious diseases, in turn, affects the body's ability to absorb nutrients, especially during early childhood when nutritional needs are high. Because these adverse effects of rainfall could offset the positive effects of improved harvest, the direction of the relationship between early-life rainfall, childhood health, and thus long-term outcomes for children is theoretically ambiguous. In this study, we expect the positive effect of rainfall on early-life health outweighs the negative effect for two reasons. First, rainfall's potential substitution effect is likely to be weak when more nutrition and less time input are required in early life. Second, compared to drought, flooding is not a recurring problem in the Kagera region because of the terrain's undulating nature and the focus on tree crop production.

5 Data

5.1 Kagera Health and Development Survey (KHDS)

The study uses long-running household panel data from the Kagera Health and Development Survey (KHDS)[3], conducted in the northwestern region of Tanzania. The survey was initially designed and implemented by the World Bank and the Muhimbili University College of Health Sciences. The KHDS has six waves of data collected between 1991 and 2010. The first four waves of the data were collected from 1991 to 1994 at six-month intervals. Overall, 915 households were interviewed during the baseline survey in 1991–94. The original households were selected from 51 clusters using two-stage stratified sampling. Excluding families for which all members were deceased, the survey re-interviewed 93 percent of the baseline households in 2004 and 92 percent in 2010. As the survey re-interviewed all baseline household members even if they moved out of their original households, the sample increased to 2700 households in 2004 (see Beegle et al. 2006, for details) and to 3300 households in 2010 (see De Weerdt et al. 2012, for more information). Overall, the re-contact rate is well above most known household panel surveys in developing countries summarized in Alderman et al. (2001).[4]

5.2 The Sample

The study sample comprised children of household heads aged ten years and below living with their parents during the baseline survey in 1991–94. That means the sample consists of cohorts born between 1981 and 1993. There were 1491 children aged ten years and below living with their parents in 512 households in the baseline survey. Out of these, 120 were deceased before 2010, and 243 were not traced during the survey's last round. We were left with 1128 children interviewed in 1991–94 and 2010. After further restricting the analysis sample to households with more than one child aged ten years and below from 1991–94, the final sample consists of 985 children from 309 households.

[3] The KHDS was originally adapted from the World Bank's Living Standards Measurement Study (LSMS) questionnaires

[4] The attrition rate reported in Alderman et al. (2001) ranges between 1.5 percent and 17.5 percent, with most of the surveys included in the study spanning short period.

5.3 Description of Children in the Sample

Summary statistics for key variables are presented in Table 1. Boys make up 51 percent of the sample. The sample children's average age in 1994 was six years, with no significant difference between girls and boys. In 2010, the children's average age was 23 years, with 80 percent of the sample above 20 years old. In the same year, about 43 percent of the children were married. Girls tend to marry earlier than boys do (53 vs 32 percent). Only 12 percent of the children were enrolled in school in 1994. The average years of schooling among those enrolled in school was 0.8 years, with no significant difference between girls and boys. In 2010, the average years of schooling increased to 7 years, with only 23 percent of the sample having above primary-level education. In the same year, about 62 percent of the children were involved in agriculture, and about 30 percent in paid employment (formal and informal). In comparison, about 8 percent were still enrolled in school. A larger proportion of boys (40 percent) were involved in paid employment outside agriculture as compared to girls (19 percent).

In all of the survey waves, anthropometric measurements were taken from all household members who were present at the time of the household interview. Trained anthropometrists from 1991–94 and enumerators in 2010 were responsible for taking the measurements. In 2010, anthropometric measurements were taken for 866 children out of the sample of 985 children. The most common reason why a measure was not taken is that children were away from home during the interview.

Children's nutritional status has been widely assessed by height-for-age (HAZ) and weight-for-age (WAZ) z scores. The HAZ and WAZ express height and weight as several standard deviations below or above the reference median value. For instance, a HAZ score of -1 shows that the child's height is one standard deviation below the median child of a healthy and well-nourished reference population for the same age and sex. The height and weight data were transformed into HAZ and WAZ scores using the 1990 British Growth Reference population data.

Overall, the sample children have poor nutritional status in childhood, measured by their HAZ score. In 1991-94, about 82 percent of the children had negative HAZ scores. The mean HAZ in the period was -1.3 for boys and -1.1 for girls. About 26 percent of the sample chil-

Table 1: Summary Statistics of key variables

Variables	Mean	Std. Dev.	Min	Max	Obs
Childhood measures					
Height (centimeters)	106.46	22.76	47.3	159.1	982
Weight (kilograms)	18.41	7.64	3	45	982
Height-for-age z score	-1.17	1.4	-5.8	5.7	979
Weight-for-age z score	-1.16	1.2	-4.11	5.5	981
Adulthood measures					
Height (centimeters)	161.43	8.74	111.3	208.6	866
Years of schooling	7.18	2.49	0	15	862
Ln(expenditure per capita in household)	15.81	0.66	14.12	18.87	979
Self-reported rich economic status (indicator)	0.66				985
Asset index	-0.002	1.33	-1.98	2.79	985
Owns a house with good floor (indicator)	0.38				985
Owns radio (indicator)	0.70				985
Owns telephone (indicator)	0.60				985
Owns motor vehicle (indicator)	0.06				985
Rainfall measures					
Deviation of log rainfall from norm, year of birth	-0.027	0.149	-.421	0.362	985
Deviation of log rainfall from the norm, one year prior to the birth-year	-0.019	0.179	-2.153	0.405	984
Deviation of log rainfall from the norm, two years prior to the birth-year	-0.011	0.196	-2.153	0.963	983
Deviation of log rainfall from the norm, three years prior to the birth-year	0.015	0.189	-1.172	0.963	984
Deviation of log rainfall from the norm, one year after the birth-year	-0.047	0.125	-0.421	0.282	985
Deviation of log rainfall from the norm, two years after the birth-year	-0.041	0.120	-0.421	0.284	985
Deviation of log rainfall from the norm, three years after the birth-year	-0.037	0.113	-0.328	0.286	985

Height and weight in childhood were measured in 1991-94. The rest of the outcome variables were measured in 2010.

Table 2: Mean Outcomes of Children by Nutritional Status

Variables	Malnourished children (stunted)	Non-malnourished children (not stunted)
Childhood measures		
Height (centimeters)	101.86	108.16
Weight (kilograms)	16.67	19.05
Height-for-age z score	-2.78	-0.60
Weight-for-age z score	-2.33	-0.75
Adulthood measures		
Height (centimeters)	157.27	162.94
Years of schooling	6.67	7.36
Asset index	-0.36	0.13
Ln(expenditure per capita in household)	15.69	15.86
Self-reported rich economic status (indicator)	0.61	0.68
Owns a house with good floor (indicator)	0.29	0.41
Owns radio (indicator)	0.65	0.72
Owns telephone (indicator)	0.48	0.64
Owns motor vehicle (indicator)	0.02	0.08

Height and weight in childhood were measured in 1991-94. The rest of the outcome variables were measured in 2010.

dren were stunted[5] by WHO standards (i.e. they have a HAZ score of -2 or lower), compared to 42 percent reported in DHS for the whole of Tanzania National Bureau of Statistics 2011. Figure A1 in the Appendix depicts a distribution of the HAZ score by sex.

Table 2 compares the health and socioeconomic outcomes of children who were stunted during childhood and those who were not. In 1991-94, the mean HAZ score for stunted children was -2.7 compared to -0.6 for not stunted children. Children who were stunted during childhood were shorter, completed fewer years of schooling, scored less on the asset index, and had less per-capita household expenditure in 2010. In the same period, a smaller proportion of children who were stunted during childhood owned a house with a good floor, radio, telephone, and motor vehicle compared to those who were not stunted. In addition, a lower proportion of children who were stunted during childhood described their households as rich. The differences are statistically significant and suggest associations between childhood nutritional status and subsequent socioeconomic outcomes during adulthood.

5.4 Rainfall Data

We use historical rainfall data obtained from two sources. For 1981–2010, the data were obtained from NASA's Modern-Era Retrospective analysis for Research and Applications

[5]A child is considered as stunted if his or her HAZ score is below -2.

(MERRA). The data contains the daily total millimetres of rain. We also use additional monthly rainfall data obtained from the Tanzania Meteorological Agency for 1980-2004. The data was taken from 21 stations. The metrological data is linked to the 51 KHDS baseline villages (clusters) using GPS coordinates.

We use the information on the month of birth to identify the season during which a child was born. The month of birth is reported in the dataset for 971 observations. For the remaining 14 observations, we assume that children were born in the middle of two consecutive survey waves. Rainfall in a child's birth year is then calculated by focusing on rainfall in four successive seasons instead of on rainfall in the calendar year. For instance, if one was born in October or November, we calculate total rainfall in the following four seasons, starting from the short rainy season. For children born in the last month of a given season, birth year rainfall is calculated by starting from the season following the birth season. Our approach is similar to that of Maccini and Yang (2009) and closely related to agricultural activities' timing.

We construct the measurement for rainfall shock in a birth year based on the deviation of birth year rainfall from the long-run average in one's birth village. We first calculate average annual rainfall for each village (cluster) using the period 1980–99. Each observation is then assigned the difference found in the logarithm between birth year rainfall and average annual rainfall. A similar approach is used to calculate the deviation of rainfall from the long-run average in the years before and after the birth year. The rainfall variable can be approximately interpreted as the percentage deviation from the average rainfall in a given village. For instance, a value of -0.1 means that rainfall is less than the village average by approximately 10 percent.

We estimate relationships between rainfall shock at various early-life stages and several outcomes in adulthood that we group into health, education, and socioeconomic outcomes. The health outcome is measured using height, an indicator of whether the person had been ill during the last four weeks prior to the survey in 2010, and the number of days ill. Educational attainment is measured by years of schooling. Our measure of years of schooling is assigned based on the number of years required to complete the highest grade attained. Socioeconomic status is measured by the log of per capita household expenditure, household asset index, and an indicator of whether a respondent described his household as rich. The household asset index is constructed using principal component analysis. Variables used to construct the index include indicators for ownership of a house with a good floor, radio, telephone, refrigerator, and motor vehicle.

A graphical representation of some of the relationships between each child's outcome and birth-year rainfall is displayed in scatter plots in Figure 1. Panel A shows the relationship between birth-year rainfall (on the horizontal axis) and childhood HAZ score (on the vertical axis). While the horizontal axis variable remains the same, the vertical axis in panels B, C, D, E, and F represents childhood WAZ score, adult height, years of schooling, household asset index, and per capita household expenditure, respectively. Panels A and B respectively show a positive relationship between birth-year rainfall and childhood HAZ score and between birth-year rainfall and childhood WAZ score. By contrast, the direction of relationships between birth-year rainfall and the respective adult outcomes are not apparent in the rest of the panels. In the following section, we will examine whether the relationships we observe from the graphs hold after controlling for various observable characteristics.

Figure 1: Birth Year Rainfall and Child Outcomes.

6 Results and Discussion

6.1 Early-Life Rainfall Shock and Adult Outcomes

We now turn to a discussion of the results. We examine the impact of rainfall shock around the time of birth on adult health, education, and socioeconomic outcomes observed in 2010. Table 3 presents results from sibling fixed-effect estimation of equation 6. Sibling fixed-effect model estimates the rainfall effect based on within-family differences in outcomes among siblings. Each row reports results from a separate regression of the outcome variable on rainfall around the time of birth. Each column presents a coefficient on rainfall variable in different years (the birth year and before and after the birth year). Year 0 is the birth year, year -1 is the year before the birth year, year +1 is year after the birth year, and so on. In addition to rainfall variables, all regressions include fixed effects for birth year and birth season, birth order, and a dummy variable indicating female, although coefficients for these additional controls are not shown in the table. As shown in the table, the effective sample size for dichotomous outcomes is significantly reduced, since only families in which outcome differences exist among siblings can be included in the estimation.

We begin with results for health outcomes. The coefficient on birth year rainfall (year 0) in height regression has a negative sign as opposed to expectation. Rainfall in the birth year has a negative relationship with an indicator for illness during the last four weeks and the number of days ill. However, the coefficients in regression for all the three health outcomes are not statistically significantly different from zero at conventional levels. The effect on an indicator for ill during the last four weeks and the number of days ill are not apparent, maybe because these variables measure short-duration illnesses and are thus generally weak discriminators for health.

Birth-year rainfall (year 0) has a statistically significant positive relationship with years of schooling attained. The coefficient on birth-year rainfall in regression for expenditure per capita in a household is positive, but it is not statistically significantly different from zero. Birth-year rainfall has a statistically significant positive effect on asset index and self-reported rich economic status.

We discuss the size of the estimated effects focusing on a 0.15 log point change in birth-year rainfall, which is the standard deviation of rainfall deviation from mean village rainfall

Table 3: Early-life Rainfall Shock and Adult Outcomes

	Year-0	Year-1	Year-2	Year-3	Year+1	Year+2	Year+3
Adult Height (centimeters)	-0.682	1.781	3.618	-2.888	-2.855	-1.767	-4.569
(n=862)	(3.398)	(1.860)	(1.828)**	(1.908)	(3.801)	(4.037)	(4.030)
Ill during the last 4 weeks	-0.915	-0.460	1.528	-0.482	-1.690	-1.815	-2.451
(indicator) (n=604)	(1.214)	(0.701)	(0.822)*	(0.705)	(1.391)	(1.454)	(1.413)*
Days ill	-3.639	-0.597	2.384	0.791	-3.806	-8.061	-4.603
(n=953)	(2.559)	(1.510)	(1.454)	(1.498)	(2.950)	(3.161)**	(3.106)
Years of schooling	1.412	0.510	0.541	0.091	-1.681	1.017	1.076
(n=860)	(0.727)*	(1.064)	(0.660)	(0.622)	(1.203)	(1.330)	(1.269)
Ln (expenditures per capita	0.083	0.115	-0.047	0.279	-0.472	-0.216	-0.123
in household)	(0.244)	(0.151)	(0.139)	(0.143)*	(0.278)*	(0.297)	(0.288)
(n=975)							
Asset index	1.271	0.296	0.328	0.026	-0.928	0.269	0.121
(n=981)	(0.494)**	(0.294)	(0.283)	(0.290)	(0.563)*	(0.602)	(0.583)
Self-reported rich status	1.954	1.393	-0.112	-0.619	0.303	-1.217	-0.021
(indicator) (n=483)	(0.959)**	(1.431)	(0.654)	(0.782)	(1.513)	(1.601)	(1.725)

All outcome variables were measured in 2010. Standard errors are reported in parenthesis. $*p < 0.1; **p < 0.05; ***p < 0.01$. *The effective sample size for each outcome variable shown in parenthesis next to the variables.*

in the year of birth. A 0.15 log point increase in rainfall in one's birth year and birth village leads to children having 0.21 more years of schooling and living in a household that scores 0.19 higher on an asset index. The same increase in birth-year rainfall is also associated with an increased likelihood of 30 percentage points to report rich economic status. This is quite a large effect compared to the base reporting propensity of 66 percent. Our results are very similar to Maccini and Yang (2009); they find that higher rainfall in the first year of life positively affects adult health, education, and socioeconomic status of women in Indonesia. However, unlike Maccini and Yang (2009), we find no significant relationship between birth-year rainfall and adulthood health outcomes.

In other years (before and after the birth year), rainfall has little or no relationship with adult outcomes. Most of the coefficients on rainfall in other years are not statistically significantly different from zero. We tested the joint significance of rainfall in the years before birth (year-1, year-2, year-3) together and in the years after birth (year+1, year+2, year+3) together for all the regressions in Table 3. The results reveal that, in most cases, we do not reject the hypothesis that the coefficients on rainfall in other years (before and after the birth year) are jointly insignificantly different from zero. This is evidence that only rainfall shock in the year of birth is vital in influencing adulthood outcomes.

Because coefficients on rainfall in other years (before and after birth year) are not sta-

Table 4: Effects of Birth-year Rainfall on Adult Outcomes

Adult Height (centimeters)	-1.128
	(3.190)
Ill during the last 4 weeks (indicator)	-0.487
	(1.079)
Days ill	-2.598
	(2.446)
Years of schooling	1.532
	(0.709)**
Ln (expenditures per capita in household)	0.126
	(0.232)
Asset index	1.156
	(0.470)**
Self-reported rich status (indicator)	1.824
	(0.907)**

All outcome variables were measured in 2010. Standard errors are reported in parenthesis. $*p < 0.1; **p < 0.05; ***p < 0.01$.

tistically significantly different from zero, we also estimate a specification only with rainfall in the birth year. We include birth year and birth season, birth order, and a dummy variable indicating females as additional controls similar to above. The results are reported in Table 4. As shown in the table, the coefficients on birth-year rainfall do not change in magnitude or significance levels in most of the regressions. Similar to the result above, the coefficients on birth-year rainfall in regressions for health outcomes and expenditure per capita in households are not statistically significant. The coefficients in regressions for years of schooling, self-reported rich status, and asset index are positive and statistically significant. Overall, the result confirms that rainfall in the birth year matters the most.

6.2 Early-Life Rainfall Shock and Childhood Health Outcomes

The previous section established that rainfall in the birth year has a long-run effect on children. We find that rainfall shock in the birth year affects children's education and socioeconomic status in adulthood. For this evidence to be complete, we need to identify the initial effect of early-life rainfall shock. No studies in the area (except Alderman et al. 2006) do this because they lack the measurement of outcome variables in early-life. This section examines the relationship between early-life rainfall and childhood nutritional status in order to estab-

Table 5: Early-life Rainfall Shock and Childhood Health Outcomes

	HAZ	WAZ
Year 0	1.348	1.728
	(0.635)**	(0.553)***
year-1	-0.324	-0.138
	(0.345)	(0.302)
Year-2	0.282	0.371
	(0.339)	(0.296)
Year-3	-0.009	0.094
	(0.354)	(0.310)
year+1	0.763	0.572
	(0.704)	(0.617)
Year+2	0.291	0.466
	(0.754)	(0.659)
Year+3	0.659	0.752
	(0.748)	(0.654)

Notes: The sample includes children of the household head who, when measured in 1991–94, had height-for-age z and weight-for-age scores between –6 and +6 and were re-interviewed in 2010. Height-for-age z scores less than -6 or greater than 6 indicate errors in measures of either height or age. Standard errors are reported in parenthesis. Asterisks indicate the level of significance: $*p < 0.1; **p < 0.05; ***p < 0.01$.

lish the initial effect. Our empirical specification in this section is very similar to that used in the previous section, except that our left-hand-side variable is now childhood nutritional status. As mentioned earlier, we measure children's nutritional status using height-for-age and weight-for-age z-scores.

Table 5 reports results from the sibling fixed-effect estimation. Column 1 shows the relationship between early-life rainfall and the HAZ score. The result indicates that rainfall in one's birth year and birth village is positively associated with the HAZ score. Column 2 presents the corresponding result for the WAZ score. The result demonstrates that rainfall in one's birth year and birth village has a statistically significant positive relationship with the WAZ score, echoing the HAZ regressions results.

We apply parameter estimates from sibling fixed-effect regression reported in Table 5 to identify the magnitude of the effect of change in birth-year rainfall. Once again, we focus our discussion on the effect of a 0.15 log point change in birth-year rainfall. A 0.15 log point increase in rainfall in one's birth year and birth village increases the HAZ score by 0.20 (compared to the mean initial HAZ score of -1.17) and the WAZ score by 0.26 (compared to the mean initial WAZ score of -1.16). This means that higher birth-year rainfall leads to significant decreases in height and weight deficits in children relative to children from a well-nourished population. Put differently, the result suggests that higher birth-year rainfall significantly improves childhood nutritional status.

In other years (before and after the birth year), rainfall has no relationship with childhood health outcomes. In both regressions, the coefficients on rainfall in other years are smaller in magnitude compared to the coefficients on birth-year rainfall. In addition, most of the coefficients on rainfall in other years have a negative sign, and none are statistically significantly different from zero. We test the joint significance of rainfall in the years before birth (year-1, year-2, year-3) together and in years after birth (year+1, year+2, year+3) together. The result shows we do not reject the hypothesis that the coefficients on rainfall in other years are jointly insignificantly different from zero. Our finding suggests that only rainfall shocks in the birth year are essential in influencing childhood nutritional status.

6.3 Discussion

This paper's key finding is that rainfall in a birth year is positively associated with childhood health status, adult education, and socioeconomic status. In interpreting this result, it is essential to understand that rainfall shock translates into income shock and nutritional deficiency with delay. The delay exists mainly because the fall in yields following drought occur during harvest season. In Kagera, harvest for major annual crops typically occurs between July and August, months after the long rainy season. Besides, rural households usually have stores of food enough to meet their basic nutritional needs for some period, which also delays the occurrence of drought-induced nutritional deprivation. Hence, nutritional deprivation resulting from rainfall shock in a birth year likely occurs during the second year of life. This implies that nutritional deprivation occurring during the second year of life is critical in influencing childhood health, adult education, and socioeconomic status.

The literature gives us insight into why the second year of life can potentially be the most critical period. Children are protected from nutritional deficiency when they are intensively breastfed in the first year of life. Additionally, intensive breastfeeding buffers infants against pathogen exposure. In contrast, children just above breastfeeding age are vulnerable to malnutrition because the buffering role of breastfeeding attenuates, and children are not robust to shocks at that age. In Tanzania, breastfeeding is a widespread practice. Approximately 94 percent of infants are continually breastfed up to 12–15 months of age, but the proportion significantly decreases to 51 percent at 20–23 months of age (National Bureau of Statistics, 2005). The finding that nutritional deprivation during the second year of life influences childhood health, adult education, and socioeconomic status agrees with several bodies of literature (Glewwe and King, 2001; Hoddinott and Kinsey, 2001; Alderman et al., 2006; Maccini and Yang, 2009; Ampaabeng and Tan, 2013; Dercon and Porter, 2014)

Finally, it is instructive to compare the estimated impact of higher rainfall on adult outcomes with early-life nutrition intervention. In making the comparison, we focus on the effects on schooling, the most common outcome studied in the literature. Field et al. (2009) documented the intensive iodine supplementation program's impact on adult-child schooling in Tanzania. The authors found that children who benefited from iodine supplements in utero attain an average of 0.35 years of additional schooling. In our analysis, a 15 percent increase in rainfall in one's birth year and birth village leads to 0.21 more years of schooling, which is roughly two-thirds of the estimated impact in Field et al. (2009). It is worth noting that higher birth-year rainfall also affects outcomes other than schooling.

6.4 Robustness to Sample Selection

In this section, we explore whether or not selection into our sample might confound the results. Our sample consists of children of household heads aged ten years and below in 1991–94 who were re-interviewed during the last round of the survey in 2010. Individuals who were not traced in 2010 and, thus, whose long-term outcomes were not measured are not included in the data for analysis. This may present an obstacle to detecting the relationship between early-life rainfall and adult outcomes. In this study, attrition bias could result from selective mortality between birth and 2010 and attrition due to other reasons. We examine the likely impact of selective mortality and overall attrition in the data separately.

As mentioned earlier, 120 children in the relevant age cohort in 1991–94 died before 2010. Table A1 in the appendix shows the mean values of childhood outcomes by mortality status. As table A1 indicates, those who died before 2010 and those who survived through

2010 have many significant differences in characteristics. Children who died before 2010 were younger, had worse HAZ and WAZ scores, and lived in households with lower per capita consumption during childhood. Also, children who died before 2010 were more likely to experience stunting and be underweight during childhood. All differences in means are statistically significant at least at the 5 percent significance level. The clear implication of this pattern is that children who died before 2010 had worse childhood health. Suppose early-life rainfall affects the likelihood of survival through 2010. In that case, our estimates of the long-term effect of rainfall on adult outcomes based on surviving children will be biased downward.

As a test for mortality selection, we check whether early-life rainfall affects the likelihood of survival until 2010. Specifically, we regress an indicator for survival until 2010 on birth-year rainfall. The results are presented in Table A2 in the appendix. We find that the coefficient on birth-year rainfall is not statistically significantly different from zero. In column 2, we include rainfall in years before and after birth year and find that none of the rainfall variables bear a statistically significant association with the likelihood of survival until 2010. In column 3, the regression includes birth year, birth season and district fixed-effects, birth order, and a dummy variable indicating female comparable to the main analysis specification. Again, we find no evidence that early-life rainfall affects the likelihood of survival until 2010. Therefore, the results suggest that bias resulting from selective mortality is not a problem for our analysis.

Another source of attrition is the fact that some children were not traced in 2010. Besides, the fact that information on adult height is missing for some children who were not present at the time of measurement further adds to attrition. In what follows, we examine whether overall attrition in the data systematically affects our result. We conduct three sets of tests of attrition, similar to Fitzgerald et al. (1998). First, we compare the means of childhood outcomes by attrition status. Specifically, we check whether childhood outcomes differ between those who lost to follow up and those who form the final sample. Children who lost to follow up include those who died, those who were not traced, and those who were not present at the time of anthropometric measurement. As Table A3 in the appendix shows, children who lost to follow up are younger and lived in a household with higher per capita consumption during childhood than those who form the final sample. A t-test shows that the differences in means

Table 6: Probit of Attrition

	Single lagged outcome at a time			All three	Other controls
	1	2	3	lagged outcomes	included
HAZ score in childhood	-0.025			-0.034	-0.047
	(0.026)			(0.043)	(0.047)
WAZ score in childhood		-0.002		0.026	0.028
		(0.031)		(0.051)	(0.055)
Ln (per capita household consumption)			0.137	0.088	-0.010
			(0.068)**	(0.075)	(0.095)
N	1,278	1,275	1,359	1,270	1,263

Notes: Standard error are reported in parenthesis. Asterisks indicate the level of significance: $*p < 0.1; **p < 0.05; ***p < 0.01$

are statistically significant. On the other hand, there are no apparent differences in childhood health outcomes. Although children lost to follow up have worse HAZ scores and are more likely to be stunted during childhood, the differences in means are small and not statistically significant.

We estimate probit equations for the probability of attrition in order to determine the presence of selection on observables. The dependent variable in the probits equals 1 if attrition occurred in 2010; 0 otherwise. We include childhood outcome variables (that is, in the language of Fitzgerald et al. (1998), lagged outcome variables) as regressors. We estimate expanding specifications of attrition probits, as shown in Table 6. In the first three columns, lagged outcome variables are included one at a time. The result shows that only per capita household consumption during childhood predicts attrition; that is, children who lived in households with higher per capita consumption during childhood are more likely to be lost to follow up. This pattern is also found from the comparison of means, as noted earlier. In column 4, all three lagged outcome variables are included at the same time. We find that none of the coefficients on lagged outcome variables are statistically significantly different from zero. In column 5, a broader set of variables (birth year, birth season, district fixed effects, birth order, and a dummy denoting female) are added. With the addition of these controls, none of the lagged outcome variables bear a significant association with attrition. The coefficient on per capita household consumption even takes the opposite sign. Therefore, it appears that there is no systematic attrition that would bias our results.

Lastly, we test whether coefficient estimates for childhood outcome regressions using only non-attriting sample differ from those using the total sample. We estimate the relation-

Table 7: Early-life Rainfall Shock and Childhood Health Outcomes

	Panel A: HAZ		Panel B: WAZ	
	Whole sample	Non-attriting sample	Whole sample	Non-attriting sample
Year 0	0.913	1.348	1.014	1.728
	(0.503)*	(0.635)**	(0.423)**	(0.553)***
year-1	-0.378	-0.324	-0.114	-0.138
	(0.313)	(0.345)	(0.264)	(0.302)
Year-2	0.307	0.282	0.269	0.371
	(0.298)	(0.339)	(0.251)	(0.296)
Year-3	0.113	-0.009	-0.004	0.094
	(0.300)	(0.354)	(0.253)	(0.310)
year+1	0.275	0.763	0.357	0.572
	(0.558)	(0.704)	(0.470)	(0.617)
Year+2	0.025	0.291	0.287	0.466
	(0.609)	(0.754)	(0.513)	(0.659)
Year+3	-0.002	0.659	0.182	0.752
	(0.585)	(0.748)	(0.493)	(0.654)
N	1,274	897	1,271	899

Notes: Standard errors are reported in parenthesis. Asterisks indicate the level of significance: $*p < 0.1; **p < 0.05; ***p < 0.01$

ship between early-life rainfall and childhood health outcomes (measured by height-for-age and weight-for-age z scores) separately for the whole and the non-attriting sample. Our empirical specification here is the same as that used in section 7.2. Results from sibling fixed-effect estimations are presented in Table 7. It can be seen that the results for the non-attriting sample are virtually identical to those found in section 7.2. We find that none of the coefficients on rainfall variables in either HAZ or WAZ regressions are significantly different between the total and non-attriting sample. We test the joint significance of the differences in rainfall coefficients and fail to reject the null hypothesis that coefficients on rainfall variables do not differ across the two subsamples for either HAZ or WAZ regressions. This further strengthens our claim that attrition bias does not affect our results. Therefore, we conclude that bias resulting from selective mortality and overall attrition in the data is not a problem for our analysis.

6.5 Additional Robustness Checks

In the previous section, we established that our main results are robust to the sample selection. In this section, we present results from additional robustness checks. First, we estimate the OLS and cluster fixed-effect regression of Equation 6. The results are presented in Table A4 in the appendix. In general, coefficient estimates in OLS and cluster fixed-effect regressions are similar to those obtained from sibling fixed-effect regressions. The main difference is that coefficients on birth-year rainfall are statistically insignificant in years of schooling and asset-index regressions for OLS and cluster fixed-effect regressions.

Second, we carried out a Hausman test to compare the sibling fixed-effect model with a random-effect model. The null hypothesis of a Hausman test is that unobserved family-level factors are uncorrelated with the observed covariates. Rejection of the null hypothesis suggests that only the sibling fixed-effect estimator is consistent. In contrast, failure to reject suggests that both sibling fixed- and random-effect estimators are consistent and that the random effect is efficient. As shown in Table A5 in the appendix, the test statistics reject the null hypothesis for two of the seven regressions estimated. Thus, it is appropriate that we used the fixed-effect model for all regressions, based on consistency.

Third, we tested how sensitive our results are to excluding children from urban areas from our sample. About 11 percent of children in the sample are from the Bukoba urban district. We included these children in the analysis sample because most households in this district practised garden production in 1991-94. As Bengtsson (2010) finds that families residing in the Bukoba urban district are less dependent on the weather, we estimated the original model (sibling fixed-effect regression) after excluding these children from the sample as a check on our results. The results are reported in Table A6 in the appendix. Most of the main results survive.

Fourth, we show that a younger child effect does not drive our results. We included birth order in the main regression to control any resource competition effect due to birth order. In addition to the order, spacing between the births of children potentially affects children's later socioeconomic outcomes due to a younger child effect. To rule out this concern, we show a distribution of the distance between the birth years of siblings in the sample(see Figure **??**). The average spacing between siblings included in the regression is 2.5 years, with 80 percent of the siblings having greater than two years. This reinforces that the results are not likely driven by competition for resources due to spacing between siblings.

Finally, we explore the robustness of our results in terms of alternative rainfall specification. In the primary analysis, we focused on the deviation of birth-year rainfall from the long-run village average, since we aim to identify the effect of more "typical" variation in rainfall on adult outcomes. Alternatively, it may also be relevant to focus on the effect of the more extreme rainfall variability. Accordingly, we estimate a specification in which rainfall shock is measured using an indicator for drought as a robustness check. Following Shah and Steinberg (2017), we define drought as 1 if rainfall in a year of birth is less than 75 percent of the long-run village average rainfall. A similar approach is used to define drought in the years before and after birth year. Because only a few (26) sibling pairs have differences in drought exposure, there may not be enough variation for sibling fixed-effect estimation. We instead report random effect results in Table A7 in the appendix. Coefficients on drought exposure in year of birth have the expected (negative) signs and are statistically significant in HAZ, height, years of schooling, and asset index regressions. The result lends support to our main finding.

7 Conclusion

Most previous research on developing countries studies the impact of more extreme types of early-life shocks such as civil war, famine, and pandemics compared to less severe types of shocks. Nevertheless, less extreme shocks such as rainfall shocks that rural households in developing countries commonly face have high policy relevance to the rural population, whose livelihoods heavily depend on rain-fed agriculture. This paper combines historical rainfall data with unique panel data from Kagera Health and Development Survey (KHDS) to examine the long-term effect of early-life rainfall shock on adult health, education, and socioeconomic outcomes of individuals in Tanzania. We control for unobserved family-level and district-level heterogeneity by estimating differences among siblings. Our analysis indicates that rainfall in birth year affects the education and socioeconomic status of children in adulthood. We estimate that a 15 percent increase in rainfall in one's birth year and birth village (relative to average village rainfall) leads children to have 0.21 more years of schooling and live in a household in 2010 that score 0.19 higher on an asset index. Such an increase in birth-year rainfall is also associated with being 30 percentage points more likely to report a rich economic status.

We then explore the relationship between early-life rainfall and childhood nutritional status and find that higher birth-year rainfall leads to significant decreases in height and weight deficits in children. Our estimates show that a 15 percent increase in rainfall in one's birth year and birth village (relative to average village rainfall) improves height-for-age z score by 0.20 and weight-for-age z score by 0.26. Robustness checks show that our findings are robust to sample selection.

Because rainfall shock translates into income shock and food shortage with delay, nutritional deprivation resulting from rainfall shock in a birth year likely occurs during the second year of life. Therefore, we conclude that nutritional deprivation occurring during the second year of life is important in influencing childhood health, adult education, and socioeconomic status. This suggests children are most vulnerable to shocks in the period after weaning from breast milk. This is in contrast to significant importance placed on the time from early gestation to the first six months of life. The absence of a rainfall effect during gestation is likely because pregnant mothers serve as an effective buffer for the developing fetus against shocks. Children are also protected from nutritional deficiency when they are intensively breastfed in the first year of life. In contrast, children just above breastfeeding age are vulnerable to malnutrition because the buffering role of breastfeeding attenuates, and children are not robust to shocks at this age. Overall, our findings suggest that anti-poverty interventions that promote providing nutritional supplements during the postweaning period could have significant long-run payoffs. The findings also underscore the importance of policies that help rural households smooth consumptions.

The literature on the long-term effects of exposure to early-life shocks in developing countries covers a wide range of shocks related to early-life nutrition (see Currie and Vogl 2013 for a recent summary of this literature). The main results in the existing studies fall into three broad categories. The first finds that exposure to shock during gestation has a long-term effect (Thai and Falaris, 2014; Shah and Steinberg, 2017; Chi et al., 2018; Yamashita and Trinh, 2021; Chang et al., 2022). The second finds exposure to shock during early childhood has a long-term effect (Glewwe et al., 2001; Hoddinott and Kinsey, 2001; Alderman et al., 2006; Yamauchi, 2008; Chen and Zhou, 2007; Godoy et al., 2008; Hoddinott et al., 2008; Maccini and Yang, 2009; Alderman et al., 2009; Maluccio et al., 2009; Ampaabeng and Tan, 2013; Nübler et al., 2021). The third finds exposure to shocks in utero and during early childhood has a long-term effect Meng and Qian (2009); Dercon and Porter (2014); Umana-Aponte et al. (2011); Cornwell and Inder (2015); Fitz and League (2020). Our paper adds to the literature on the second category.

One potential concern with our results is that parental response to rainfall shock could constitute part of the estimated reduced-form effect. Parents might adjust intra-household resource allocation in response to early-life shock on their children. We cannot account for this effect in our regression because we do not have adequate measures of parental human-capital investment for each child in our data. Parents could adopt a compensatory strategy or a reinforcing approach by investing relatively more or fewer resources, respectively, in a child who has suffered from early-life shock. Failing to account for parental investment response to rainfall shock in early childhood would underestimate or overestimate the true effect in adulthood depending on how parents compensate or reinforce the initial effect.

Some studies suggest that parents simultaneously make a compensatory investment in health and reinforcing investment in education in response to early-life shocks (Ayalew, 2005; Yi et al., 2015). Suppose parents in our sample follow the same strategy. In that case, shock-affected children may catch up with their unaffected siblings in health but lag in education. This may provide a potential explanation for why we find that rainfall shock affects childhood health but not adulthood health. It may also explain the result that early-life rainfall shock affects adulthood education without affecting adulthood health. Therefore, the reduced-form effect we identified may underestimate the impact of rainfall shock on adulthood health and overestimate the impact on adulthood education.

From a policy perspective, understanding how parents respond to early-life rainfall shock is essential. If parental response to shock is ignored or poorly understood, the effectiveness of early childhood nutritional intervention programs is likely to be undermined. This is because parents can aggravate or reduce the effect of early-life rainfall shock by adjusting the allocation of resources within the family. While there is considerable advancement regarding the impact of early-life shock, there is limited literature that accounts for parental responses to shocks. More research on the impact of early-life shock that accounts for parental responses is needed to develop effective policy responses. We hope future research will shed more light on parental responses to shock in Tanzania, so our results can be considered in its full context.

Appendices

Appendix A:

Table A1: Childhood Characteristics by Mortality Status

Variable	Not dead before 2010	Dead before 2010
Age in years	5.9	4.7
Percent of female	49.4	44.1
HAZ score in childhood	-1.1	-1.6
WAZ score in childhood	-1.1	-1.4
Percent with stunting in childhood	25.2	42.2
Percent with wasting in childhood	20.8	30.4
Ln(Per capita household consumption)	12.6	12.4

Table A2: Impact of Rainfall Shock on Probability of Survival until 2010

	only birth year rainfall	only rainfall variables	other controls included
Year 0	0.183	0.075	-0.324
	(0.340)	(0.379)	(0.750)
year-1		0.395	0.283
		(0.306)	(0.427)
Year-2		-0.191	-0.524
		(0.318)	(0.593)
Year-3		0.386	0.074
		(0.310)	(0.472)
year+1		-0.717	-1.923
		(0.477)	(0.874)**
Year+2		0.113	-0.537
		(0.500)	(0.903)
Year+3		-0.530	-1.264
		(0.486)	(0.853)
N	1,293	1,289	1,139

Notes: Standard error are reported in parenthesis. Asterisks indicate the level of significance: $*p < 0.1; **p < 0.05; ***p < 0.01$

Table A3: Childhood Characteristics by Attrition Status

Variable	Not attriting sample	Attriting sample
Age in years	6.1	5.2
Percent of female	51.5	43.9
HAZ score in childhood	-1.1	-1.2
WAZ score in childhood	-1.1	-1.1
Percent with stunting in childhood	25.6	29.1
Percent with wasting in childhood	21.7	21.5
Ln(Per capita household consumption)	12.5	12.6

Figure A1: Distribution of HAZ Score in Childhood by Sex.

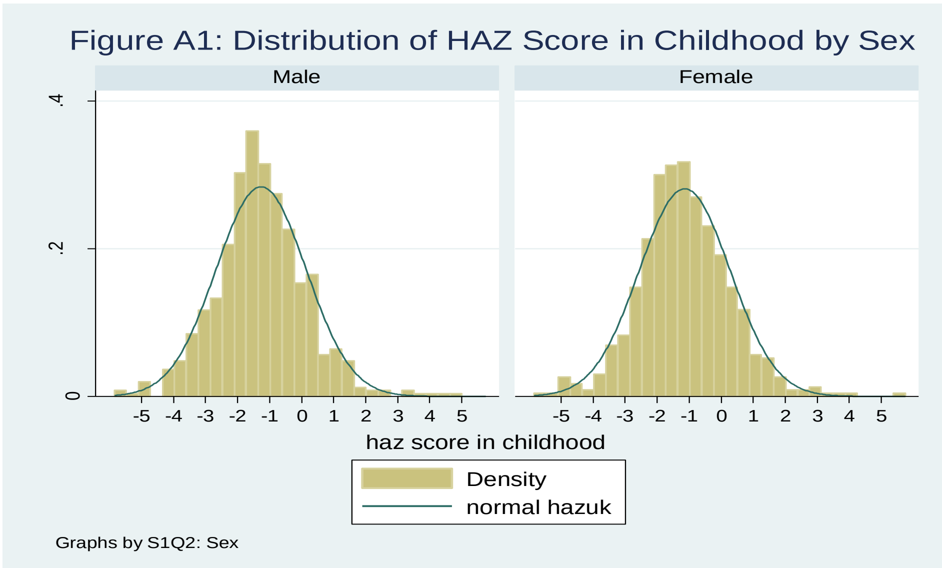

Table A4: Effects of Birth Year Rainfall on Childhood and Adult Outcomes

	OLS	Cluster fixed effect
HAZ	1.628	1.495
	(0.495)***	(0.582)**
WAZ	1.753	2.104
	(0.416)***	(0.595)***
Adult Height (centimeters)	-3.873	-4.163
	(3.094)	(4.559)
Ill during the last four weeks	0.881	0.134
	(0.908)	(1.214)
Days ill	1.376	1.081
	(2.325)	(2.318)
Years of schooling	0.098	0.288
	(0.964)	(1.365)
Ln (expenditures per capita in a household)	0.156	-0.096
	(0.257)	(0.352)
Asset index	0.451	0.298
	(0.516)	(0.551)
Self-reported rich status	1.066	2.495
	(0.498)**	(0.853)***

Notes: Standard error are reported in parenthesis. Asterisks indicate the level of significance: $*p < 0.1; **p < 0.05; ***p < 0.01$

Table A5: Hausman test statistics

	Chi square	Prob Chi square
Adult Height (centimeters)	32.16	0.1231
Ill during the last four weeks	28.62	0.2346
Days ill	39.73	0.0228
Years of schooling	33.97	0.0852
Ln (expenditures per capita in a household)	15.60	0.9020
Asset index	24.81	0.4163
Self-reported rich status	11.94	0.9806

Table A6: Effects of Birth Year Rainfall on Childhood and Adult Outcomes –Excluding Urban Sample

HAZ	1.377
	(0.685)**
WAZ	1.654
	(0.593)***
Adult Height (centimeters)	-0.887
	(3.666)
Ill during the last four weeks	-1.288
	(1.277)
Days ill	-4.310
	(2.666)
Years of schooling	1.153
	(0.755)
Ln (expenditures per capita in a household)	0.277
	(0.250)
Asset index	1.653
	(0.515)***
Self-reported rich status	2.007
	(1.032)*

Notes: Standard error are reported in parenthesis. Asterisks indicate the level of significance: $*p < 0.1; **p < 0.05; ***p < 0.01$

Table A7: Effects of Birth Year Drought on Childhood and Adult Outcomes

HAZ	-0.535
	(0.270)**
WAZ	-0.368
	(0.235)
Adult Height (centimeters)	-3.214
	(1.667)*
Ill during the last four weeks	0.254
	(0.487)
Days ill	0.096
	(1.158)
Years of schooling	-1.573
	(0.541)***
Ln (expenditures per capita in a household)	-0.067
	(0.120)
Asset index	-0.490
	(0.245)**
Self-reported rich status	-0.937
	(0.683)

Notes: Standard error are reported in parenthesis. Asterisks indicate the level of significance: $*p < 0.1; **p < 0.05; ***p < 0.01$

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