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ORIGINAL PAPER

The impact of timing of in utero drought shocks on birth outcomes in rural households: evidence from Sierra Leone

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Abstract
This paper investigates the impact of timeline-bound fetal exposure to drought shocks on birth outcomes in rural Sierra Leone. We link repeated cross-section birth record data across 11 years from the Sierra Leone Demographic and Health Surveys to district-level geolocation precipitation data from the University of Delaware weather repository. The methodology uses spatial distribution of precipitation across districts to identify the impacts of extreme droughts on birth outcomes. This study reinforces both harvest and direct gestation as maternal nutrition pathways for the impact of drought shocks on birth outcomes. Results also show that adverse in utero shock impacts are concentrated among poorer households and may be mitigated by antenatal care services.

Keywords Birthweight · Harvests · Gestation · Antenatal care · Sierra Leone

JEL Classification Q12 · Q18 · Q25 · Q54

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1 Introduction

The birthweight of a newborn is an important health indicator that has implications for short-term and long-term health risks. In utero exposure to a wide range of shocks during gestation can affect birthweight (Alderman and Behrman 2006; Almond and Currie 2011). This implies that the level of exposure to external shocks during gestation may affect not only health at birth but also the trajectory of an individual's health and socioeconomic outcomes.[1] The main objective of this paper is to fill a research gap by providing evidence that extends the literature on the impacts of drought shocks on birth outcomes in Sub-Sahara Africa. The research gap relates to the detailed exposition of the interrelationship between birth outcomes and shock timelines. The primary research question addressed in this paper is, does the timing of the drought shock matter in understanding health outcomes at birth? This study focuses on the timing of the occurrence of drought shocks in early life and explores the underlying variation to capture timeline-bound exposure.

There are several pathways along which extreme weather events (precipitation and temperature) across early life timing can affect birthweight.[2] Complementary primary pathways include food security captured through harvest variations and water scarcity that affects household access to water through low rainfall during gestation. Other pathways include the disease environment through excessive rainfall during gestation and destruction of infrastructure from historically excessive rainfall. This conceptual framework distinguishes impacts of precipitation shocks observed during the most recent planting season from those observed during the gestational period.[3] In this study, we test a hypothesis on the impact of each precipitation pathway using reduced form analyses on maternal history data from a low-income country. This approach separates the implied resource variability from the direct intrauterine shocks in a manner that presents a detailed understanding of the impact of weather shocks on birth outcomes. The existing literature depicts birth outcomes in response to exposure to in utero nutrition or to direct gestation shocks in isolation. This paper bridges a gap in the literature by investigating interaction effects of harvest and gestation weather-induced shocks on birth outcomes. It is unclear whether this approach will reveal differential impacts from exposures to harvest and gestation shocks. This is because measures of harvest yields have an overlapping capacity with the gestation period that is not feasible for direct gestation shocks. This has important implications for targeting of intervention programs to mitigate impacts of weather-induced shocks for pregnant women in low-income populations. For instance, a broader understanding of the different components of weather-related shocks on socioeconomic outcomes could provide insights into how

[1] Over the years, both medical and economic literature have presented substantial evidence in support of a robust relationship between birthweight and short- to long-term welfare indicators (see Behrman and Rosenzweig 2004; Black et al. 2007 for extensive discussions).

[2] See Appendix Fig. 2 for a visual depiction of these pathways.

[3] We use detailed precipitation data to modify the timelines of extreme rainfall events in a manner that refers to indirect and direct in utero exposure to early life shocks thereby strengthening intergenerational transmission of weather-related shocks emanating from early-life events around the period of gestation.

to improve child and maternal health outcomes in rural areas. We therefore investigate how strategic components of rainfall shocks impact birth outcomes.

In this paper, we focus on maternal health data from Sierra Leone. This setting is important for the following reasons. First, Sierra Leone is a fragile country due to the history of civil war (which ended around 2002) and has been at the center of disease outbreak in the last decade. For example, Sierra Leone suffered a serious Ebola outbreak that ravaged Africa between 2014 and 2015. This history may lead to weaker institutions thereby intensifying the impacts of adverse early life shocks compared to other low-income countries in Africa. It is well established that conflict halts the development of infrastructure across diverse sectors of the economy including agriculture and healthcare (Murdoch and Sandler 2004; Blattman and Miguel 2010). Second, University of Delaware weather data for Sierra Leone between 2002 and 2014 show a prevalence of low precipitation shocks — drought spells — relative to the norm across its 153 chiefdoms. Our paper tries to tease out potential malnutrition pathways for intergenerational weather shock impacts of birth outcomes. Third, Sierra Leone has one of the highest incidences of low birthweight in Sub-Sahara Africa. In 2015, about 14.4% of births in Sierra Leone were in the low birthweight category (WHO 2021; World Bank 2021). In 2019, Sierra Leone recorded about 3080 infant deaths. The infant mortality rate of approximately 8.1% is significantly high relative to the world average of 3.8% in the same year (World Bank 2021). These factors distinguish our paper from existing studies in the region while also providing additional insights into alternative pathways for weather shocks (Bakhtsiyarava et al. 2018; Grace et al. 2015). To achieve the above objectives, we match birth outcome data from the Sierra Leone Demographic and Health Surveys (DHS) with precipitation data from the University of Delaware historical weather archive.

The findings of this paper show a somewhat symmetric impact of alternative drought pathways on birth outcomes. We interpret harvest variability results from the harvest drought shocks as depicting a reduced-form impact from the maternal nutrition or food security pathway. This result shows that an incidence of harvest drought reduces average birthweight by approximately 4% while increasing the incidence of low birthweight (LBW) by 59%. Placebo regressions using the out-of-season, past and future rainfall droughts show no association, hence reinforcing our hypothesis that seasonal impact is through harvest patterns. We make an inference on the contemporaneous water scarcity pathway during gestation by estimating the results of gestational drought shocks. Our findings show weak evidence on the contemporaneous gestational drought only for the low birthweight indicator. We interpret this as an association between water scarcity and the propensity for low birthweight of around 54%. We provide alternative sensitivity tests to address endogenous and selection issues to support the robust association of these shocks and birth outcomes to enhance causal interpretation of the estimated treatment effects. Heterogeneous impacts of the estimated shocks on birthweight show a concentration of impacts for households at the bottom of the pyramid of wealth distribution with further evidence of mitigating patterns by access to antenatal healthcare.

This paper contributes to the literature in the following ways: First, it fills the knowledge gap on the timeline-bound impacts of in utero droughts on birth outcomes. Second, it demonstrates that the mitigating role of antenatal care for disadvantaged pregnant women during unanticipated shocks is limited to harvest droughts. The use of droughts from timeline variation extends the literature on the interaction of droughts and birth outcomes. More importantly, this approach helps to understand the underlying determinants of child and maternal mortality with in-depth insight (Kalipeni et al. 2017; UNICEF 2016; WHO et al. 2014). Hence, this paper helps to provide context to support evidence-based decisions for intervention on pregnancy-related health outcomes. Our findings also contribute to another strand of the literature on the interaction of underlying impacts of local transitory weather shocks on welfare outcomes of smallholder farmers in low-income countries.

The remainder of this paper is organized as follows. In Section 2, we discuss the data sources and define shock pathways in association with drought timelines. Section 3 provides the empirical methods. In Section 4, we present the estimated results and discuss the main findings, and conclude in Section 5.

2 Data

2.1 Birth outcomes

We use data from two waves of the Demographic and Health Surveys (DHS) for Sierra Leone. The empirical analysis focuses on gestation and birthweight variables. The Sierra Leone DHS are available for 3 years: 2008, 2013, and 2016. However, we use only the 2008 and 2013 waves based on the availability of information on birth outcomes reported at childbirth and the district level GPS datasets. Reported births for the two waves occurred between years 2003 and 2013. The Sierra Leone DHS datasets contain household level demographic characteristics and variables specifically relating to women between the ages of 15 and 49. This helps to match general women's demographic statistics and antenatal health variables, such as antenatal care visits, that we use with birthweight for a better understanding of the determinants of birth outcomes in this study. Birth records include additional vital statistics such as gender, nature of birth (natural birth, caesarean etc.), birth order, information on a single or multiple births, and other conditions surrounding childbirth. In the data, birth records are retrieved from diverse sources including birth center and hospital cards. While we focus on recorded measures of birthweight in our baseline regressions to avoid bias from undocumented data, we use additional data from mothers' recall for a robustness check of our results.

The DHS data also identifies the sources of the birthweight data. As expected, most of this data comes from public health centers (61.7%) which include Government hospital, Government health center and Government health post. This is followed by respondent's home (25.6%) and birth centers (11.9%). The remainder (0.8%) is from the private sector and other unknown sources. In general, our birthweight variable captures births across diverse settings and not just hospitals or healthcare centers only. This indicates that the birthweight measure used in this study is widely representative of all existing birth sectors across Sierra Leone.[4] Hence, we argue that there are no perceived sectoral under-reporting issues with our data, and our estimates are not likely to suffer from place of birth sample selection. The caveat to this is that there are other undocumented birthweight categories in the DHS data that account for around 40% of births.[5] One main concern is that birthweight measures may differ across sectors in a potentially differential way to the sample of children whose birthweight could not be identified. To deal with potential sample selection from use of reported birthweight as against this sample, we compare demographic characteristics between the identifiable birthweight sample (tracked) and unidentifiable birthweight sample (untracked) groups.

We use the variable on the classification of residential areas of women to restrict our analysis to only rural households. We have information on the residential location of the mothers clustered at the district level. We extract GPS coordinates (latitude and longitude) of the residential location of mothers across districts in Sierra Leone to match precipitation data. These geospatial variables are used to compute important weather variables, mainly precipitation and temperature patterns, from the University of Delaware's weather data repository. Using the date of birth of each child and shocks over the gestation period of each child, we capture the response of birth outcomes to the variation in precipitation patterns during gestation at the intensive margin. We outline the details of the selected rainfall events for each pathway, in a manner consistent with the research hypotheses outlined within the conceptual framework of the Appendix, below. To address concerns regarding selective migration or location sorting across districts, we restrict our analysis to women surveyed in their permanent place of residence only.[6] We perform additional sensitivity analysis by imposing further restrictions on our sample using children born to women living in the district for at least 2 years before the child's year of birth.

2.2 Weather data

2.2.1 Precipitation and temperature data

To measure local rainfall shocks, we rely on rainfall data from terrestrial precipitation in the 1900–2017 gridded monthly time series (version 5.01), from the University of Delaware's Center for Climatic Research. We use temperature data from the same archive. These datasets provide estimates of monthly precipitation and temperature on approximately 0.5° by 0.5° grid covering terrestrial areas across the globe for the period of 1900–2017. Rainfall and temperature estimates are based on climatologically aided interpolation of available weather station information. These

[4] Further breakdown of the birth data by location of birth is available in Table A10 on Appendix 2 of the Supplementary Material.

[5] Other categories of undocumented birthweight for children include identifiers for children not weighed at birth, those with unknown birthweight measures and those with special responses.

[6] The main assumption underlying this approach is that there is no widespread migration during the preceding gestation periods for pregnant women in previous years.

datasets have been compiled and made available by Matsuura and Willmott (2017). We use the GPS coordinates provided for each rural district in Sierra Leone DHS surveys for 2008 and 2013 to access the corresponding weather patterns from the precipitation and temperature repository. We use precipitation and temperature estimates from two complementary methods in the literature: (i) average from four closest weather stations similar to the approach used by Rocha and Soares (2015)[7] and (ii) weather from all weather stations within a 60-km radius of the GPS location of a locality following Chen et al. (2020).

2.2.2 Rainfall and temperature shocks

We establish rainfall deviation and shocks based on the established theories in the economics literature. For the food shock and nutrition pathway, we rely on drought shocks during retrospective agricultural seasons for rural households who predominantly depend on farm harvests.[8] Agricultural yield from cultivation associated with this rainfall pattern is a plausible predictor of food security in these areas. To complement this approach, we construct child-specific local level rainfall adjusted across two consecutive agricultural seasons for an accurate measure of exposure to harvest drought shock for each of the precipitation estimates highlighted above.[9] The first approach matches a child born between the months of January and October premised to be affected, if we observe a drought shock during the agricultural cycle of the previous year (the year prior to the year of birth) and a child born between November and December to be affected, if we observe a drought shock during the current agricultural cycle. We partition the months of birth into two groups: the first 10 months or the last 2 months of the year. However, child-specific harvest shock may incorporate a shared rainfall pattern across two consecutive planting seasons. In this case, a child born in October may also be affected by a drought shock that occurred during the current agricultural cycle as the child was still in utero when the shock occurred. This means that only children born between January and April should be exclusively affected by the previous agricultural cycle but not the current one. We provide an alternative child-specific seasonal rainfall variable as follows:

[7] To achieve this, we match GPS coordinates of each village to the GPS of the four closest weather stations from UDEL's data archive similar to the weather matching approach in the literature to obtain estimates of rainfall and temperature. This involves using inverse weight of distance to each weather station during the estimation process. We follow the same process for the aggregation approach from the average of weather stations from a 60-km radius coverage. Further details about the formation of the precipitation and temperature measures by birth month and locality of birth are available from the authors upon request.

[8] This pathway requires a lag period (an interval) between the planting season and harvesting season which we incorporate into the harvest shock models. Also, the covariate nature of the harvest shock requires that we construct shocks in a way that covers collective births which take place within the same locality around the same period.

[9] We are grateful to one of the reviewers for providing a useful insight into the importance of merging cross-seasonal harvest variation for an accurate measure of exposure to drought shock.

$$Rainfall_{l,d} = \omega_d Rainfall_{l,t-1} + \left(1 - \omega_d\right) Rainfall_{l,t} \tag{1}$$

where the subscript d denotes the birth date (month of the year). $Rainfall_{l,t-1}$ and $Rainfall_{l,t}$ are the rainfalls observed during the agricultural cycles of the year prior to the child's year of birth and in the child's year of birth, respectively, and ω_d is termed the exposure scaling factor across two agricultural seasons. Parameter ω_d is measured as the fraction of the gestational period in which the child was exposed to the previous agricultural cycle and calculated as follows:

$$\omega_d = \left(1 - (\frac{1}{9}\max(0, d - 4 + 1)\right) \tag{2}$$

This way, only children born in the month of December will be assigned the rainfall of the current agricultural cycle as they were likely conceived in April — the starting month of the agricultural cycle. This precipitation aggregation method captures potential stress that the mother experiences in anticipation of a shortfall in current precipitation that will be characterized in the drought exposure measure in Sect. 2.3.

Lastly, we use the direct gestational pathway which uses reference timelines by individual babies' month of birth only. In this category, aggregation takes place for birth dates within the same month of birth for the same districts. Variation in shocks is explored simultaneously across the months and locations of birth. In principle, precipitation shocks captured using this approach are considered to be linked to extreme rainfall events representing disease environment (flood) or water scarcity (droughts). We focus on water scarcity or droughts in this study.

2.3 Shock pathways

2.3.1 Agricultural harvests

We estimate agricultural rainfall shocks from extreme variation in seasonal precipitation levels across Sierra Leonean districts. In Sierra Leone, the rainy season runs consecutively for 7 months between April and October of each year. Most of the Sierra Leone crop cultivation and agricultural extension programs run simultaneously within this period to sustain yields and harvests (Ngegba et al. 2018). The agricultural cycle used in this paper refers to the rainy seasons' historical variation. After aggregation in Sect. 2.2.2, we compute the seasonal rainfall variation, namely standard deviation movements, over a 30-year historical rainfall cycle in the same district. We use extreme precipitation thresholds for seasonal droughts computed as two-standard deviation movements around the long-term average. Equation (3) follows the methodology in other studies investigating the impact of climate change on health outcomes (Rocha and Soares 2015):

$$harvestdroughtshock_{lt-1} = 1 \text{if} Rainfall_{l,t-1} < \left(\overline{Rainfall_l} - Rainfall_l^{2\times SDRainfall}\right), \text{and zero otherwise} \tag{3}$$

where $Rainfall_{lt-1}$ indicates the retrospective precipitation for the agricultural season within district l, and $\overline{Rainfall_l}$ is the average historical yearly precipitation of the district over a 30-year period. Hence, $harvestdroughtshock_{lt-1}$ measures the extreme and the intensity of the precipitation available for farm cultivation across districts.

2.3.2 Water density: water scarcity

The water density pathway directly captures the fetus' gestation-level shock exposure over a 9-month period. We capture the water scarcity component of these shocks as precipitation levels related directly to drought events during gestation. We adopt gestation matching timelines which are completely different from the harvest variation depicted in Eq. (3) above. Gestation drought shock captures gestational period 9-month precipitation shocks as follows:

$$gestationdroughtshock_{lym} = 1\text{if} \sum_{m=-8}^{0} R_{lm} < \left(\overline{R_l} - R_l^{2\times SD-R}\right), \text{ and zero otherwise} \tag{4}$$

where R_{lm} indicates the monthly precipitation levels during each month of the gestation period specific for each fetus. We compute accumulated rainfall patterns for each birth from the month of conception ($m = -8$) to delivery ($m = 0$) within a district $l.\overline{R_l}$ is the average historical accumulated precipitation for child-specific gestational months for each district over 30 years and R_l^{SD-R}; rainfall standard deviation depicting volatility of precipitation levels in a similar manner. Thus,$gestationdroughtshock_{lym}$ indicator represents the propensity for insufficient precipitation (compared with the corresponding period's average historical precipitation at the district level) in the 9 months particular to each baby's gestation. This approach extends the adoption of progressive monthly accumulation of low precipitation used to capture water scarcity for Brazilian municipalities in the literature (Rocha and Soares 2015).

3 Empirical methods

We implement reduced form econometric models for our analysis. These models are widely used to analyze the impacts of weather shocks on early life human capital variables such as health and education and have been adopted in the literature investigating the impact of weather shocks on birth outcomes (Andalón et al. 2016; Molina and Saldarriaga 2017). The key right-hand side variables are conveyed in Eqs. (3) and (4); and represent the main shock pathways conceptualized in this paper. The main argument for adopting this modeling strategy is that harvest and gestation shocks are not perfectly correlated with each other. This study thereby provides a unique framework to capture and test multiple hypotheses on how localized weather patterns may impact birth outcomes differently. The econometrics equations designed to capture the harvest drought (food security pathway) and direct gestation drought (water density pathway) are specified below.

Harvest shock model

$$\begin{aligned}\text{Birthweight}_{\text{ilym}} &= \alpha_{\text{lm}} + \gamma_{\text{ym}} + \beta_1 \text{harvest drought shock}_{\text{lt-1}} \\ &+ \sum\nolimits_{\text{m=-8}}^{0} \text{Temperature bins}_{\text{lm}} \omega_{\text{lm}} + \text{X}_{\text{i}}^{'} \theta_{\text{x}} + \text{Z}_{\text{ct}}^{'} \theta_{\text{z}} + \varepsilon_{\text{ilt}}\end{aligned} \quad (5)$$

Water density model

$$\begin{aligned}\text{Birthweight}_{\text{ilym}} &= \text{a}_{\text{lm}} + \text{Y}_{\text{ym}} + \mu_1 \text{gestation drought shock}_{\text{lym}} \\ &+ \sum\nolimits_{\text{m=-8}}^{0} \text{Temperature bins}_{\text{lm}} \, \omega_{\text{lm}} + \text{X}_{\text{i}}^{'} \, \theta_{\text{x}} + \text{Z}_{\text{ct}}^{'} \, \theta_{\text{z}} + \varepsilon_{\text{ilym}}\end{aligned} \quad (6)$$

where $Birthweight_{ilym}$ represents the measured birthweight of a baby at birth for an observation i in district l for year y at month m. We repeat the estimation of the same set of models (Eqs. 5 and 6) for complementary outcome variable – low birthweight (LBW) indicator, as described in Sect. 2. For Eq. (5), harvest drought shocks are lagged to match the in utero period harvests for partitioned births as specified in Sect. 2.2.2. Hence, we extrapolate the effects of drought with the primary aim of underpinning in utero harvest variations and how these are related to the birth outcomes. α_{lm} is the district by month of birth fixed effects, and γ_{ym} is the year of birth by month of birth fixed effects. We also include year of survey fixed effects to control for any residual within-year trajectory manifesting in general economic patterns and an indicator for the child's birth order. In Eq. (5), β_1 is the parameter of interest. This parameter measures the retrospective impact of exposure to in utero district-level harvest shock. We capture the exposure to shock at the district level due to the covariate nature of rainfall patterns. Exposure to agricultural droughts in the lead up season prior to a child's birth determines crop harvests and, subsequently, the nutritional intake of the mother during the child's gestation. This linkage underscores the fetal programming conditions that underlie treatment effects estimated on both the short-term and adult outcomes in the economics literature. Similarly, the water density model pathway is embedded within parameter μ_1 in Eq. (6) as a measure of the impact of shocks from direct gestation droughts.

X and Z consist of a vector of individual and household level covariates. X is a vector of individual level control for the children, mothers, and the mothers' partners. In the regressions, we control for the gender and birth order of each child using indicator variables while including mothers' demographic characteristics such as age during interview, age at first birth, education, and marital status. Partners' covariates consist of individual demographic characteristics including age, education, and occupation. Z is a vector of household level controls including the household size, age of household head and an indicator variable for the gender of household head. In Eq. (5), the error term (ε_{ilt}) accounts for unobserved time-variant locality characteristics not captured by the trend and unobserved individual characteristics. As the variables in the error term are orthogonal to the child's exposure to fetal shock, treatment effect estimates of the effect of rainfall shock on child outcomes will be unbiased. The error term of the model is assumed to be identically and independently distributed (iid) across districts but correlated within each district due to spatial correlation of rainfall pattern for households in the same area. The same rule applies for the error term of Eq. (6). We therefore cluster standard errors at the district level.

We include controls for heatwaves during gestation which may have direct and considerable impact on birth outcomes thereby confounding our estimated treatment effect coefficients. We control for this by including indicator variables of 5 °C temperature bins in all regression models. Each of the temperature variables was calibrated within each gestation month to more accurately control for exposure to in utero stressors relating to the heatwave exposure. Controlling for high frequency heatwaves this way separates the existing precipitation pathways from extreme temperature shocks (Chen et al. 2020; Konkel 2019; Li et al. 2018; Ngo and Horton 2016).

Baseline specifications in Eqs. (5) and (6) rely on cohort fixed effects for the identification of shocks on birth outcomes. Also, including a set of fixed effects[10] for district of birth by calendar month of birth would control for potential differential observable and unobservable seasonal birth patterns and characteristics at the district level. The extensive distribution of survey districts in Fig. 1 indicates there is national level coverage of survey locations. This serves as a precursor for extensive variation in the explanatory variables of interest for seasonal harvests and gestation period exposures. For direct gestation drought, the identification is based on rainfall variation associated with the individual level gestational period where matching takes place at the month-of-birth by year-of-birth levels. This methodology provides a granular and individual-specific variation in the precipitation patterns which is plausible for targeted impacts of drought shocks. Figures A2 and A3 present interpolated seasonal variation of precipitation levels from the long-term norm across the survey districts. The colors across the map denote exposure of each surviving birth to harvest shocks where deep red and green colors represent considerable large negative and positive variations; respectively. Different shades of the colors denote the level of intensity of the shock where the most faded represents mild seasonal rainfall variation with potentially little threat to health. Overall, the maps show large spatial variation of the seasonal precipitation level when compared to the district average. We use both within-year (intensive) and inter-period (extensive) weather variations for years 2002 to 2013, as demonstrated by Figures A2 and A3. Another plausible approach to model the interactive impacts of the drought shocks is to regress an augmented specification that adds both shocks (i.e., harvest and gestational drought shocks) into a single regression. This approach helps to address concerns regarding indeterminate nature of the influence across harvest and gestational drought — as to which is more important for determining birth outcomes — in the current models. Background data analysis suggests that there is 87% correlation in the exposure of the children to both shocks at the same time. This overlap introduces substantial level of multicollinearity into the proposed augmented model. The underlying interaction in the variation of exposure to both droughts imposes an important limitation towards teasing out the effects of each of the shocks on birth outcomes concurrently.

[10] As an alternative identification strategy, we include a linear trend on the children's year of birth in the estimation process. This would absorb long-term linear trends in the outcomes that can vary depending on the district of birth.

The summary statistics are reported in Table 1. Mean birthweight is approximately 3.3 kg with 12.6% of births within low birthweight category. There is almost equal distribution by gender with approximately 49% boys. The average age of mothers is about 29 years, while the average age of mothers at their first baby was around 19 years. Most mothers (85.5%) had either incomplete or no primary education, and most (90.3%) were either married or living with a partner, while 5.5% of those that were not currently married had never been in a union. The average household size was just above seven; 79% of the households had male heads with an average age of 43 years. Over half of partners (56%) were aged 26–40 years, with less than a third (31%) aged 41–60 years. Similar to the mother's education average, about 79.2% of partners had either incomplete or no primary education. Agricultural and unskilled manual categories were the most prominent occupation categories of the partners (87.1%). Incidence of harvest, and gestation droughts are 3.5 and 4.6 percentage points over the 12-year period.

4 Results

4.1 Main results

4.1.1 Impact of harvest drought on birth outcomes

Table 2 presents the ordinary least squares (OLS) estimates of Eq. (5) for the impact of harvest drought on birthweight and the low birthweight (LBW) indicator. Table 2 column (1) presents results from a parsimonious model which includes basic fixed effects deemed appropriate for baseline identification of shock impacts in our model. Subsequently, we include the control variables, birth order indicator, additional fixed effects, and a linear trend from columns (2) to (5). We designate column (1) the baseline regression and column (5) the fully specified regression results — our preferred specification. All regressions include district of birth by month of birth fixed effects and the survey year fixed effects. Each column is a separate regression and we report the results for birthweight in panel A while presenting results for low birthweight in panel B. Our results indicate a robust negative association between harvest drought and birthweight, and a negative association with low birthweight. In Table 2 panel A, column (5), an incidence of harvest drought reduces average birthweight by 147 g. This pattern is supported by the result in the same column for panel B, where the harvest drought shock increases low birthweight by 7.9 percentage points. These results are significant at the 10% level.

Coefficient estimates remain fairly stable from the parsimonious model in column (1) through to the fully specified model in column (5) while observing only marginal changes across columns. This staggered identification approach serves as a robustness check for the efficiency of our results (Oster 2019). We achieve this by comparing coefficient estimates from a basic parsimonious model (which includes just a small set of clearly exogenous fixed effects), additional covariates, and fixed effects. Our results show that the rainfall treatment is, in fact, random, as the point estimates have not changed dramatically across columns

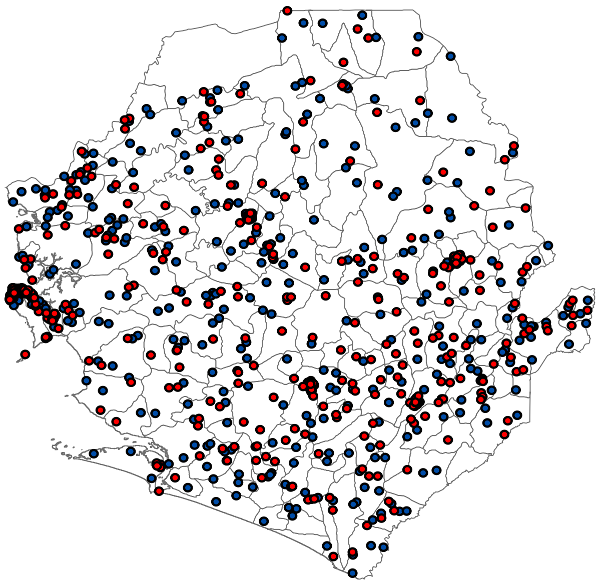

Fig. 1 Distribution of survey districts for 2008 and 2013 Sierra Leone DHS data. Notes: Red dots represent 2008 survey districts, while blue dots represent 2013 districts. Source: Matching created by the authors using the district GPS coordinates from DHS surveys

(1)–(5). Panel A column (1) shows a responsive baseline decline of birthweight of around 144 g while varying between 144 and 161 g between column (2) to column (5). Similarly, low birthweight results in panel B show an association of shock of 0.081 in column (1) while hovering around 0.077–0.087 across column (2) to column (5). Using coefficients from the preferred specification in column (5) of panels A and B, the estimated coefficients show that an incidence of harvest drought is robustly associated with an average decline in birthweight of 147 g and an increase in the proportion of low birthweight by 7.9 percentage points. In economic magnitudes, an incidence of harvest drought shock is associated with a 4% decrease in birthweight and 59% increase in the probability of LBW when compared to the mean.

Our main research hypotheses do not anticipate association between the out-of-season or unmatched harvest shocks and birth outcomes, as such non-contemporaneous precipitation-related impacts may weaken harvest-related arguments in this study. As a falsification exercise, we perform alternative tests using similar thresholds of droughts constructed from the variation in precipitation for consecutive out-of-season months[11] and non-contemporaneous seasonal precipitation variations. The latter is depicted by district-level lagged (i.e., past

[11] The out-of-season period for Sierra Leone (November, December, January, February, and March) comprises the dry season months when extensive crop harvesting of the previous planting season takes place. There is minimal crop cultivation during this period compared to the rainy season.

Table 1 Summary statistics

Variable	Mean	Std. dev
Main variables		
Birthweight (kg)	3.273	0.752
Low birthweight indicator (%)	0.126	0.332
Harvest drought	0.035	0.184
Gestation drought	0.042	0.201
Demographic variables		
Children		
Gender indicator (= 1 for boys)	0.486	0.500
Mothers		
Age	28.861	7.182
Age at first birth	18.911	3.901
Education category		
No education	0.759	0.428
Incomplete primary	0.096	0.295
Complete primary	0.041	0.197
Incomplete secondary	0.097	0.296
Complete secondary	0.006	0.080
Higher	0.002	0.040
Marital status		
Never in union	0.055	0.227
Married	0.870	0.336
Living with partner	0.041	0.199
Widowed	0.015	0.122
Divorced	0.004	0.062
Separated	0.015	0.120
Household		
Household size	7.232	3.240
Household head age	43.303	13.998
Household head male	0.789	0.408
Husband/partner		
Age group		
15–25	0.076	0.266
26–40	0.556	0.497
41–60	0.310	0.463
61–74	0.036	0.186
75 and above	0.022	0.146
Education category		
No education	0.723	0.448
Incomplete primary	0.069	0.253
Complete primary	0.029	0.167
Incomplete secondary	0.122	0.327
Complete secondary	0.034	0.181
Higher	0.024	0.153

Table 1 (continued)

Variable	Mean	Std. dev
Occupation category		
Did not work	0.013	0.114
Professional/technical/managerial	0.048	0.213
Clerical	0.003	0.052
Sales	0.024	0.152
Agricultural — self employed	0.738	0.440
Agricultural — employee	0.000	0.016
Services	0.006	0.076
Skilled manual	0.036	0.186
Unskilled manual	0.133	0.339

Summary statistics for final sample of 4357 births across 3488 women used in the main analysis. The sample covers women with pregnancies for surviving babies only. Observations restricted to rural districts

agricultural seasons) and lead (i.e., future agricultural seasons) variations. In Table A1, we report the results of these falsification tests including a full set of controls and fixed effects presented in Table 2 column (5). In the regression process, we replace harvest drought shock with out-of-season and lagged/lead drought shocks in Eq. (5). Although we cannot rule out the sizeable magnitudes of the estimated coefficients from these regressions when compared to treatment effects estimated from our preferred specification for Table 2, but all the coefficients are generally statistically insignificant. This outcome supports our main proposition that rural birth outcomes are basically associated with only contemporaneous harvests from rainfed agricultural practices.

4.1.2 Impact of direct gestation drought on birth outcomes

We report the result of the water density pathway in Table 3, using the contemporaneous gestation drought shock indicator as the focus explanatory variable (Eq. 6). While estimated coefficients for the average birthweight turn statistically insignificant and are smaller compared to results reported for harvest droughts in Table 2; the patterns are consistent. However, for low birthweight the coefficient estimates from gestation drought present comparable treatment effects. Table 3, column (5) shows that an incidence of contemporaneous gestation drought is robustly correlated with an increase in the likelihood of low birthweight by 7.0 percentage points. This result translates to an increase in the probability of low birthweight of 54% in economic terms. Our water accessibility results show similar asymmetric treatment effect patterns between birthweight and low birthweight with similar studies using water crisis as a natural experiment (Wang et al. 2021).

As a robustness test for the main results, we include a variable that controls for district-level exposure to the country's civil war that took place between 1991 and

Table 2 Impact of exposure to harvest drought shock on birth outcomes in rural Sierra Leone

Variables	(1)	(2)	(3)	(4)	(5)
Panel A: birthweight (kg)					
In utero harvest drought	−0.144*	−0.161**	−0.144*	−0.145*	−0.147*
	(0.079)	(0.077)	(0.078)	(0.078)	(0.078)
Constant	3.262***	4.207***	4.164***	4.172***	16.889
	(0.008)	(0.663)	(0.660)	(0.640)	(19.044)
R-squared	0.247	0.264	0.265	0.267	0.265
Panel B: low birthweight (indicator)					
In utero harvest drought	0.081*	0.087**	0.080*	0.077*	0.079*
	(0.042)	(0.041)	(0.042)	(0.042)	(0.042)
Constant	0.131***	−0.184*	−0.165	−0.207*	2.983
	(0.004)	(0.111)	(0.110)	(0.114)	(8.691)
R-squared	0.205	0.220	0.222	0.226	0.222
District of birth by month of birth fixed effects	Yes	Yes	Yes	Yes	Yes
Survey year fixed effects	Yes	Yes	Yes	Yes	Yes
Controls (includes temperature bins for gestation months)	No	Yes	Yes	Yes	Yes
Birth order indicator	No	No	Yes	Yes	Yes
Cohort fixed effects (year of birth by month of birth fixed effects)	No	No	No	Yes	No
Linear trend	No	No	No	No	Yes

The outcome variable is the birthweight measure in kilograms (panel A) and an indicator variable for low birthweight (panel B) for rural surviving births. No. of observations: 4333. Low birthweight indicator is nominated as 1, if birthweight measure is less than 2.5 kg and 0 otherwise. Explanatory drought variable follows locality reference methodology in Eq. (3) for extreme drought shock using 2 standard deviation precipitation movements below the district norm. All columns include district of birth by month of birth fixed effects and the survey year (year of interview) fixed effects. Controls include an indicator variable for gender of child; mother's age at interview and at first birth, and indicator variables for education and marital status; partner age, education and occupation; and household size, age of household head, and gender of household head. Robust standard errors clustered at the district level are reported in parentheses

*$p<0.1$; **$p<0.05$; ***$p<0.01$

Table 3 Impact of gestation precipitation shocks on birth outcomes in rural Sierra Leone

Variables	(1)	(2)	(3)	(4)	(5)
Panel A: birthweight (kg)					
Contemporaneous gestation drought	−0.082	−0.103	−0.083	−0.087	−0.089
	(0.071)	(0.069)	(0.071)	(0.071)	(0.071)
Constant	3.275***	4.133***	4.093***	4.114***	19.877
	(0.003)	(0.651)	(0.646)	(0.628)	(19.042)
R-squared	0.246	0.263	0.264	0.266	0.264
Panel B: Low birthweight (indicator)					
Contemporaneous gestation drought	0.072*	0.079**	0.071*	0.070*	0.070*
	(0.037)	(0.037)	(0.037)	(0.037)	(0.037)
Constant	0.124***	−0.165	−0.149	−0.195*	0.721
	(0.002)	(0.106)	(0.105)	(0.109)	(8.706)
R-squared	0.205	0.220	0.221	0.226	0.221
District of birth by month of birth fixed effects	Yes	Yes	Yes	Yes	Yes
Survey year fixed effects	Yes	Yes	Yes	Yes	Yes
Controls (includes temperature bins for gestation months)	No	Yes	Yes	Yes	Yes
Birth order indicator	No	No	Yes	Yes	Yes
Cohort fixed effects (year of birth by month of birth fixed effects)	No	No	No	Yes	No
Linear trend	No	No	No	No	Yes

Outcome variable is the birthweight measure in kilograms (panel A) and an indicator variable for low birthweight (panel B) for rural surviving births. No. of observations: 4333. Low birthweight indicator is nominated as 1, if birthweight measure is less than 2.5 kg and 0 otherwise. Explanatory drought variable follows reference methodologies in Eq. (4) for contemporaneous gestation drought shocks using 2 standard deviation precipitation movements below the district norm for extreme gestation shocks. See Table 2 for additional notes on the identification strategy for fixed effects, list of controls, and linear trend. Robust standard errors clustered at the district level are reported in parentheses

*$p<0.1$; **$p<0.05$; ***$p<0.01$

2002. We explore the number of attacks and battles from conflict documentation data from the No Peace Without Justice report (Smith et al. 2004). This data has been used by many papers to examine the impacts of the Sierra Leonean civil war on socioeconomic outcome variables (Bellows and Miguel 2009; Burgess et al. 2015). The inclusion of this variable in our models accounts for potential indirect impacts that civil war from the previous decade (1991–2002) may have on birth outcomes which may confound our estimated treatment effects. The results of our preferred specification which includes control for battles and attacks across districts presented in Table A2 show comparable treatment effect to the main results in Tables 2 and 3.

4.2 Decomposition analysis

We explore heterogeneity of shock impacts across wealth distribution using a decomposition analysis across households in our sample. We use the data on *wealth index factor score*, which is the standardized household weighted value of assets,[12] from the DHS data to examine the heterogeneous impacts of the alternative drought shocks. This analysis partially helps to uncover the mitigating role of safety net measures available to wealthy households that ameliorate weather shock impacts. To achieve this, we repeat regression for our baseline specifications in Eqs. (5) and (6) for the sample observations from different wealth spectrum. We focus on the fully specified regressions in column (5) of Tables 2 and 3 for the heterogeneous results. Reported results in Table 4 panels A and B correspond to column 5 of Tables 2 and 3, respectively.

Table 4 presents heterogeneous impacts of the shocks by categories of household wealth score. The results show statistically significant coefficient estimates, for harvest and gestational droughts, partially sustained for households within the poorer category only. The estimated coefficients for this group are consistent to a priori expectation, considerably larger than the reported estimates for the other groups and at least twice the benchmark results for harvest drought (Table 2, column (5)) and gestation drought (Table 3, column (5)). Results are the same with alternative specifications using the interaction of quartile wealth distribution with shocks.[13] These results are striking — as they defy the logic of expected strongest treatment effects originating from the bottom wealth group supported by most studies. One explanation for this pattern is the lack of sufficient variation in shock exposure within the lowest quintile group thereby attenuating the estimated treatment effects for harvest and gestation shocks. More importantly, this pattern is only suggestive as we rely on the wealth index from the survey year which may be somewhat different from the

[12] This wealth score measure has been used in many studies to depict the safety net capacity across households in low-income countries. It mainly helps to cushion shock exposures in times of uncertainty and can be quite useful during drought events (Fafchamps et al. 1998).

[13] The explanatory variables of interest include multiple interactions of the wealth quintile indicators with the corresponding shock indicators each for the harvest drought and gestation drought. All shock quintiles are included simultaneously in the same regression for the alternative models.

birth year household wealth distribution. There is also a somewhat U distribution of the heterogeneous treatment effects across wealth groups, where only the intermediate wealth groups (poorer and middle) provide estimated results with coefficients consistent with theory in general. The large positive coefficient for the richer and richest groups can be attributed to small sample used for this group.

Furthermore, we investigate the role of access to healthcare services during pregnancy on shock impacts. We proxy healthcare access by using a variable on availability of a midwife during childbirth. This variable potentially represents an existing relationship between a pregnant woman and a certified healthcare worker prior to giving birth which is directly relevant for sustainable birth outcome. Table 5 presents the results after incorporating an interaction between this proxy and shock categories, in addition to our main explanatory variable (shocks).[14] The results show that access to antenatal care may effectively mitigate the adverse impacts of harvest drought. This is demonstrated by the counteractive nature of the estimated coefficient of the interaction terms for harvest drought pathway and its magnitude.[15] This pattern is weak for gestation drought suggesting the mitigating role of access to antenatal care has a limited capacity. This asymmetric pattern may be linked to the capacity to quantify decline in consumption levels emanating directly from harvest shocks which is difficult for restrictions in water access. More research is required to understand the underlying mechanism of the gestation shocks and the pathways for targeting intervention.

Although this study does not use exogenous variation on antenatal care exposure, our results provide suggestive evidence on the importance of maternal healthcare acess in the context of low-income countries (Beuermann et al. 2020; Gajate-Garrido 2013; Nazim and Fan 2011). Our findings also complement recent studies on the impact of antenatal Medicaid changes on birth and adulthood outcomes in the US (Brown et al. 2020; Miller and Wherry 2019).

4.3 Endogenous and selection issues

Household migration during gestation period is an important issue that could lead to selection bias. This is because migration is a potential outcome of early life shocks, and our sample may be inherently defined by an endogenous selection process which could confound our estimated treatment effects. Restricting the study sample to women living in their permanent place of residence may not sufficiently capture migration tendencies of households in our study. To address this problem, we explore data directly capturing migration activities across households for a sensitivity analysis. We use a variable from the DHS data named “length of stay.” This variable asks how many years a woman has lived in the district of survey with the aim of exploring the sample of births whose mothers have lived in the current location for at least

[14] We focus on shocks relating to poorer households to tease out the potential for antenatal care to effectively mitigate the concentrated shock for this group.

[15] This is more prominent for the harvest shock where the interaction is both greater than the estimated shock impacts and statistically significant.

Table 4 Decomposition of shock impacts across the household wealth spectrum

	Dependent variable: birthweight (kg)			
	Poorest	Poorer	Middle	Richer/richest
Variables	(1)	(2)	(3)	(4)
Panel A				
In utero harvest drought	0.041	−0.266**	−0.138	0.204
	(0.187)	(0.116)	(0.156)	(0.233)
Constant	34.807	−41.689	49.312	36.405
	(33.079)	(35.543)	(41.278)	(49.465)
R−squared	0.444	0.395	0.370	0.399
Panel B				
Contemporaneous gestation drought	0.097	−0.226*	−0.125	0.125
	(0.175)	(0.125)	(0.153)	(0.202)
Constant	33.138	−40.505	49.355	36.994
	(32.932)	(35.649)	(41.165)	(49.593)
R-squared	0.444	0.395	0.370	0.398
Observations	1269	1109	1002	635

Table 4 reports the heterogeneous impacts of shocks for the baseline results (reported in Tables 2 and 3) across different spectrums of household wealth categories directly extracted from the DHS data. The unbalanced distribution of sample observations across designated wealth groups is attributed to our focus on rural sample, where a large component of the households is poor relative to the composition of households within urban areas. The dependent variable of interest in this analysis is the birthweight (kg). The results in panels A and B correspond to baseline results in panel A of the fully specified columns of Tables 2 and 3 (column 5), respectively, including all fixed effects. Robust standard errors clustered at the district level are reported in parentheses

*$p<0.1$; **$p<0.05$; ***$p<0.01$

2 years before the birth.[16] Sensitivity results presented in Tables 6 and 7 report treatment effects similar to the main results for the restricted sample of observations. This addresses our concern regarding an important selection issue from migration thereby providing additional robustness for our main results.

Another important limitation of the empirical analysis of fetal exposure to rainfall events and birth outcomes is the confounding nature of precipitation shocks and the household decision-making process. We address the issue of selection into childbearing as fertility decisions are endogenous and may be affected by weather conditions. To investigate this, we estimate the impact of harvest shocks on mother's use of contraceptives. Our result shows that there is no impact of seasonal harvest shocks on the adoption of contraceptives (results available from the authors upon request). Furthermore, to address potential selection in unobservable characteristics among

[16] The length of stay variable is available in the 2008 survey covering a birth period of 6 years: 2003–2008. While most of the respondents have lived in the survey location indefinitely, there is an observed significant decline in the number of observations with the imposition of restriction to the sample of children born to women living in the district at least for 2 years before the child's year of birth.

Table 5 Interactive role of access to antenatal care on shock impacts

	Dependent variable: birthweight	
	Category of shock	
	Harvest drought	Gestation drought
Variables	(1)	(2)
Rainfall shock (quintile 2)	−0.441**	−0.271**
	(0.188)	(0.127)
Rainfall shock × antenatal care	0.485*	0.194
	(0.253)	(0.208)
Constant	17.173	17.043
	(18.874)	(18.896)
R-squared	0.265	0.264

Table 5 reports the interaction impacts of antenatal care impacts on birthweight using quintile 2 as the reference wealth group. No. of observations: 4333. The regressions include rainfall shock indicator and an interaction with antenatal care access indicator. We use a proxy of access to healthcare from an indicator variable that captures assistance from a midwife during birth. Robust standard errors clustered at the district level are reported in parentheses

*$p < 0.1$; **$p < 0.05$; ***$p < 0.01$

exposure households and those who decided to have a baby and not after an event of drought was observed during the agricultural cycle (i.e., during an unfavorable economic situation), we perform a double equality of means test across drought (two groups of children by exposure to drought shocks) and contraceptive treatment–control groups (two groups of women by contraceptive adoption). We use normalized difference of coefficients invented by Imbens and Wooldridge (2009) across the covariates used in our analysis. Tables A6 and A7 show non-differential characteristics across drought exposure and contraceptive adoption groups. These tests indicate that our main coefficients may not be driven by correlation with the unobservable characteristics between diverse groups.

Similarly, we address potential selection bias in estimated coefficients with successful births using the datasets on miscarriages from the same DHS surveys. The dataset provides information on women with miscarried pregnancies and provides details such as the month and year of the event. Similar to the main results, we focus on the rural households surveyed in their permanent place of residence during the survey to tackle issues regarding adaptation and location sorting. We estimate fetal mortality models by conducting another set of regressions for Eqs. (5) and (6) where we include all covariates, fixed effects, and linear trend on alternative measures of miscarriage. This includes counts of miscarriage cases and propensity for miscarried pregnancies during a conception year within each district. The results of these dependent variables are reported in Table A4. Estimated coefficients of the in utero shocks for all categories of miscarriage outcome are small and statistically insignificant. This result shows that the coefficient estimates reported for the main results in Sect. 4.1 are not likely to be biased upwards by potential selection bias arising from miscarriages linked to adverse precipitation events.

Table 6 Sensitivity test for exposure to harvest drought shock results

Variables	(1)	(2)	(3)	(4)	(5)
Panel A: birthweight (kg)					
In utero harvest drought	−0.168*	−0.235***	−0.232**	−0.232**	−0.219**
	(0.086)	(0.084)	(0.091)	(0.091)	(0.095)
Constant	3.192***	2.316***	2.314***	2.314***	−33.801
	(0.041)	(0.384)	(0.384)	(0.384)	(45.088)
R-squared	0.310	0.345	0.346	0.346	0.346
Panel B: low birthweight (indicator)					
In utero harvest drought	0.103**	0.117**	0.105**	0.105**	0.084*
	(0.045)	(0.046)	(0.049)	(0.049)	(0.050)
Constant	0.212***	0.037	0.047	0.047	56.757***
	(0.017)	(0.150)	(0.149)	(0.149)	(20.749)
R-squared	0.258	0.292	0.293	0.293	0.299
District of birth by month of birth fixed effects	Yes	Yes	Yes	Yes	Yes
Survey year fixed effects	Yes	Yes	Yes	Yes	Yes
Controls (includes temperature bins for gestation months)	No	Yes	Yes	Yes	Yes
Birth order indicator	No	No	Yes	Yes	Yes
Cohort fixed effects (year of birth by month of birth fixed effects)	No	No	No	Yes	No
Linear trend	No	No	No	No	Yes

Table 6 presents restricted baseline results for Table 2 . Here, the sample of women are restricted to having lived in their location of residence for at least 2 years before birth. No. of observations: 974. This information is available for only the first wave covering a birth period of 6 years: 2003–2008. Robust standard errors clustered at the district level are reported in parentheses

*$p<0.1$; **$p<0.05$; ***$p<0.01$

Table 7 Sensitivity tests for exposure to gestation shocks results

Variables	(1)	(2)	(3)	(4)	(5)
Panel A: birthweight (kg)					
Contemporaneous gestation drought	−0.101	−0.177**	−0.176**	−0.176**	−0.164*
	(0.075)	(0.074)	(0.078)	(0.078)	(0.084)
Constant	3.274***	2.332***	2.331***	2.331***	−23.204
	(0.013)	(0.365)	(0.366)	(0.366)	(46.188)
R-squared	0.306	0.343	0.343	0.343	0.344
Panel B: low birthweight (indicator)					
Contemporaneous gestation drought	0.092**	0.112***	0.101**	0.101**	0.077*
	(0.040)	(0.041)	(0.043)	(0.043)	(0.045)
Constant	0.182***	0.055	0.065	0.065	50.875**
	(0.007)	(0.138)	(0.137)	(0.137)	(21.274)
R-squared	0.257	0.293	0.294	0.294	0.299
District of birth by month of birth fixed effects	Yes	Yes	Yes	Yes	Yes
Survey year fixed effects	Yes	Yes	Yes	Yes	Yes
Controls (includes temperature bins for gestation months)	No	Yes	Yes	Yes	Yes
Birth order indicator	No	No	Yes	Yes	Yes
Cohort fixed effects (year of birth by month of birth fixed effects)	No	No	No	Yes	No
Linear trend	No	No	No	No	Yes

Table 7 presents restricted baseline results for Table 3 . Here, the sample of women are restricted to having lived in their location of residence for at least 2 years before birth. No. of observations: 974. This information is available for only first wave covering a birth period of 6 years: 2003–2008. Robust standard errors clustered at the district level are reported in parentheses

$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

Lastly, there are concerns that missing birthweight datasets in the DHS survey may constitute further selection bias that may impact the representative nature of our childbirth sample to those across Sierra Leone between 2003 and 2013. We partially address this issue by performing another sensitivity test on our main results by using an alternative composition of the birth sample which incorporates childbirth reports of birthweight datasets from mother's recall.[17] Table A5 presents updated coefficient estimates from a repeat regression for the main results in Tables 2 and 3 using a broader classification of birthweight source. The results from this table present evidence of complementary impacts of in utero shocks on low birthweight category.[18] Regardless of these findings, we limit the interpretation of our results to the focus sample of officially documented birthweight. We address the remaining sample representation issue by conducting an equality of means test between generally identifiable and un-identifiable birthweight groups.[19] This test result shows no differential background characteristics (results available from the authors upon request).

4.4 Discussion

The main findings from our results are the asymmetric impacts of in utero precipitation shocks from the alternative measures of precipitation timelines. Harvest shocks linked to the gestation period of children show persistent effects for birthweight and low birthweight. The estimated results show that harvest drought shock leads to an overall decrease of around 4% in birthweight (corresponding to an approximately 59% increase in low birthweight incidence) compared to the baseline mean. On the other hand, contemporaneous gestation drought shock shows no correlation with birthweight while presenting a sustainable association with low birthweight incidence.[20] Contemporaneous gestation drought shock is correlated with an increase in low birthweight of approximately 54% when compared to the mean. The asymmetric pattern in the impacts of water density particularly reinforces the importance of comprehensive consideration of rainfall events for policy guidelines. To rule out spurious association of harvest shocks and birthweight, we control for heatwaves using monthly 5 °C temperature bins in all our specifications for alternative weather channels. As a falsification test, we demonstrate that non-seasonal drought and previous and future drought shocks do not have any impact on the birthweight. We also focus on pregnant women surveyed in their permanent place of residence or who lived in

[17] It is important to highlight that only a small proportion of the birthweight data is reportedly associated with mother's recall.

[18] We observe a downward bias in the estimated coefficients of drought shocks of models using birthweight (in kilograms) as dependent variable. This bias is explained by the measurement errors underlying mother's recall birthweight data as demonstrated in the comparison of standardised normal curves for both hospital cards and memory recall categories (see Fig. 2).

[19] This test generally addresses the issue of lack of information of the extent of potentially missing birthweight data in Sierra Leone over this period.

[20] These results are robust to alternative shock composition (Table A3) and weather aggregation methods. Treatment effects from rainfall estimates from the inverse weighted average of weather from stations within 60 km of location of the enumeration areas yield comparable results (see Table A8 on Appendix 2 of the Supplementary Material).

their current place of residence for at least 2 years to address selective migration and show that our results do not suffer from potential bias associated with fetal mortality. Using alternative measure of birth outcome, we estimate a new set of results for term small–for–gestational–age (SGA) variable using categories of the birth size reported in the DHS data. This approach mirrors low birthweight perspective where categories of birth size below average (very small and small) are categorized as 1, and those above (average, very large and large) are categorized as 0.[21] Results reported in Table A9 show similar pattern with the low birthweight results in Tables 2 and 3 but with smaller coefficients lacking statistical precision. The lack of statistical power may be attributed to measurement errors from subjective nature of this variable.

The findings in this paper reinforce the reduced form maternal nutrition pathway embedded in the fetal origin hypothesis (Appendix). There is also evidence to support the water scarcity pathway. Our findings on the role of water access complement the findings in the literature. Wang et al. (2021) report modest impacts of the US Flint water crisis during gestation on low birthweight. Rocha and Soares (2015) document robust impacts of water scarcity on child health outcomes for arid zones in Brazil. Lin et al. (2021) study the role of water density and scarcity in China while Hyland and Russ (2019) present the intergenerational role of water density in Sub-Sahara Africa. The results from Sects. 4.1–4.3 present persistent precipitation impacts which strengthen the role of both nutrition and water scarcity as valuable pathways for birth outcomes. Lastly, our study shows that the results are concentrated within poorer households and may be mitigated by access to antenatal care. The distribution of the impacts and effectiveness of healthcare delivery reinforce the importance of antenatal care for maternal health outcomes in poor neighborhoods. This has policy implications as substantial investment and sensitization regarding antenatal care in rural areas is required to reduce the intergenerational impacts of weather shocks during gestation.

Our findings are consistent with estimated results from similar studies in low-income countries. Bakhtsiyarava et al. (2018) report an increase of 47 g and 89 g birthweight in response to an increase of 100-mm rainfall for agricultural households in Mali and Kenya, respectively. Moreover, the lack of sensitivity of cash croppers to the rainfall variation supports evidence from asset-driven heterogeneous impacts estimated in Section 4.2. Chacón-Montalván et al. (2021) report a reduction of birthweight by 183 g in Amazonian populations after excessive rainfall shocks during gestation. While our results focus on drought shock, this evidence shows that both extreme negative and positive precipitation may distort harvests by stimulating devastating crop loss. These figures centralize the estimated 147 g decline in birthweight to harvest drought that we report for Sierra Leone.

5 Concluding remarks

In this paper, we estimate the impacts of gestational drought shocks on birth outcomes. This paper contributes to the growing discussion on the interaction of localized weather patterns and birth outcomes in low-income countries. We use Sierra

[21] Approximately 56% of children fall within the category of a lower than average birth size.

Leone, a country in Sub-Sahara Africa, as a case study to provide evidence in a resource scarce setting due to the predominance of rainfed agriculture and barriers created by rural livelihoods. This paper addresses deficiencies in the research context of this specific question and policy framework for Sub-Sahara Africa, where adverse impacts of climate change are predicted to increase (IPCC-Intergovernmental Panel on Climate Change 2012). The evidence in this paper provides a structure to facilitate decision-making by governmental and non-governmental organizations and to design efficient and effective policy to address the adverse effects of early life shocks on newborns and mothers. This paper provides evidence to guide intervention programs targeting birth outcomes. There is subtle evidence of stronger impact for low wealth subgroups and evidence of the mitigating role of antenatal care for harvest shock — which is not reported for gestation shocks.

The analytical framework in this paper follows the emerging literature which seeks to particularly understand the transmission pathways betweenweather shocks and welfare outcomes in low-income countries. This includes the impacts of water scarcity on birthweight and infant mortality (Rocha and Soares 2015) and the impacts of heatwaves on birthweight (Molina and Saldarriaga 2017). Our findings on a case study country from Sub-Sahara Africa complement evidence from other regions and also demonstrate the interaction between different pathways that have not been previously explored. More importantly, our findings on Sierra Leone provide an understanding of the dynamics of in utero precipitation shocks that could be useful to develop policy guidelines for most of the countries within the Sub-Sahara African region.

Appendix

Linking weather shocks to fetal origin hypothesis

Seminal papers by Barker (1990, 1992, 1995) reveal the association between events within in utero development life stages and adulthood diseases. This research provided a reference point for future research activities on early life factors underlying adulthood diseases (Deschênes et al. 2009) which has since been expanded. The current perspective of the fetal period programming hypothesis underpins a variety of intermittent factors during the gestation period. The *fetal origin hypothesis* differs from the *critical programming period theory* due to unique risk factors embedded within fetal exposure. The gestation period is a fragile state of development and may significantly alter the developmental process and wellbeing for an individual from childhood to adulthood. Studies from the economics literature have increased the empirical context in support of the epidemiology theories underlying both fetal origin and critical programming claims (Almond and Currie 2011).

Short-term to long-term impacts of early life events can be linked to gestational development and contemporaneous early life events. The first 2 years of birth complete the formation of vital body organs required for subsequent stages of life after infancy leading to persistent socioeconomic effects from the medium to long term. However, this does not constitute part of the intergenerational transmission component of the impact of early life events associated with the gestation period. Events occurring directly during the gestation period are of special importance when considering the intergenerational pathway for the impacts of early life events. Hence, isolating this may help design intervention programs for pregnant women. Shocks during gestation and other early life periods could be linked to malnutrition (Majid 2015; Meng and Qian 2009), disease environment and extreme weather events (Andalón et al. 2016; Deschênes et al. 2009), natural disasters (Currie and Rossin-Slater 2013), conflicts (Le and Nguyen 2020; Mansour and Rees 2012), and homicides (Foureaux Koppensteiner and Manacorda 2016) among others. In this paper, we focus on the impacts of extreme weather events. We also try to unravel the pathway through which exposure of the fetus to each event may impact the health development trajectory during pregnancy.

The nutrition channel is the centerpiece of the fetal origin hypothesis in epidemiology (Almond and Currie 2011), while the economic literature continues to expand into other stressors. Nutrition deficits and birthweight outcomes are likely to increase in rural settings where households predominantly depend on rainfed agricultural practices amidst prevalent scarcity of economic resources in drought regimes. Also, development literature shows that in the absence of sustainable safety net programs for smallholder households, food insecurity is associated with deteriorating welfare outcomes including health (Lohmann and Lechtenfeld 2015). The interaction between lack of support for the nutritional requirements during pregnancy and inefficient healthcare systems may particularly worsen child and maternal health outcomes in rural areas of low-income countries. In summary, weather shocks have greater capacity to escalate food insecurity in rural areas in most low-income countries. The main implication of this is the potential intergenerational adverse impacts of transient weather shocks for the rural poor.

Water density during the gestation period can influence birth outcomes in various ways. This background provides an important framework for adopting alternative shock metrics in this paper. The gestational water density metric captures an alternative potential pathway not driven by food consumption. Channels of transmission of the impacts of water include drinkable water accessibility, disease environment, and infrastructure. Access to potable water is an important direct sub-channel of rainfall to birth outcomes for rural households due to these households' heavy reliance on streams, rivers, and untreated wells for household water needs. This can be captured through an access to water directly linked to the pregnancy months. We proxy water access using drought regimes faced by each pregnant woman during the gestation period.[22] On the other hand, reference flood can be used to capture the impact of disease environment within the same framework. The disease environment is also

[22] The gestational drought and flood indices proxy the water access and disease environment during pregnancy of each child individually by comparing the cumulative pregnancy period precipitation level faced by the mother to the historic average for the same period within the same district. The drought and flood indices differ for children of the same locality born in different months, even if born in the same year.

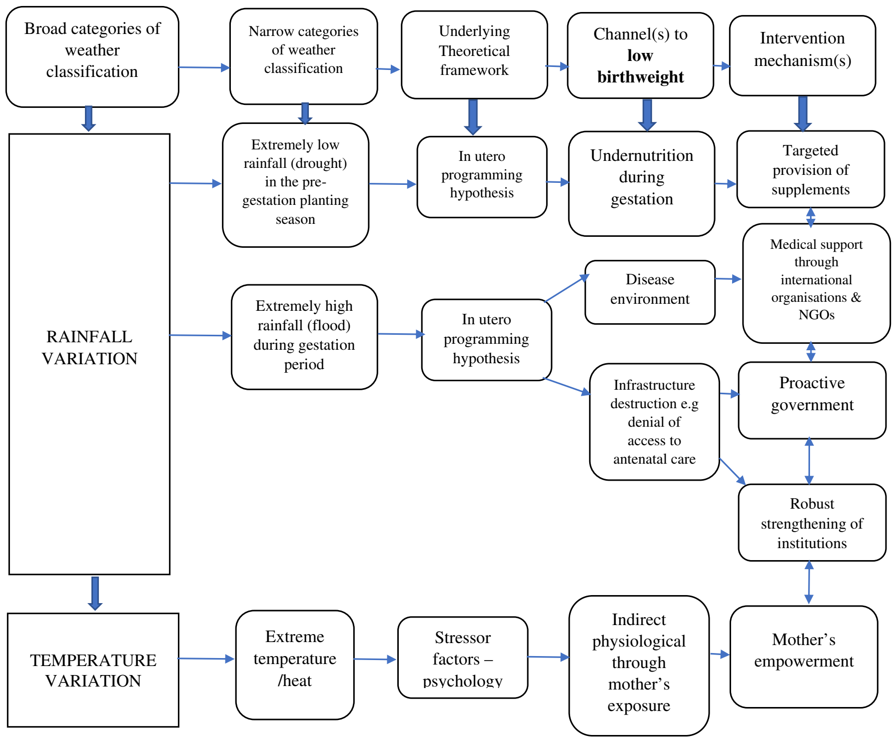

Fig. 2 Pathways for the impact of weather shocks on birthweight in rural areas. Source: Adapted from Abiona (2022)

closely linked to variability in weather events due to infrastructural deficits. A typical example of this is how flood events increase the possibility of blocked drainages potentially breeding mosquitoes — a source of malaria disease. Floods could also lead to dirty and muddy environments causing inflows of dirty water into streams which are the main source of drinking water for rural households. This could lead to severe diarrheal illnesses and cholera. These are contemporaneous pathways from precipitation patterns to low birthweight that are plausible within the context of this study. We focus on drought events which underlines water scarcity for rural households. This typically arises from extremely low precipitation.

Children born with low birthweight tend to have more health difficulties and worse later-life outcomes relative to their peers born at normal birthweight (Almond and Currie 2011; Currie 2011). Health at birth is predictive of important child outcomes including educational attainment and adult earnings. Hence, economists are increasingly concerned with understanding the impacts of conditions during pregnancy on birth outcomes (Almond and Currie 2011; Currie 2011).

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Declarations

Conflict of interest The authors declare no competing interests.

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