Nutrition smoothing: Can access to towns and cities protect children against poor health conditions at birth?

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Seasonal fluctuations in early life circumstances can be associated with later differences in health outcomes. Other evidence finds that access to markets and services can help rural households improve their well-being. This study links these two phenomena, using spatial diversity across the Democratic Republic of Congo (DRC) to investigate whether proximity to towns confers resilience against seasonal determinants of health. To identify a potentially causal effect, we use the random component of birth timing relative to the intensity of seasonal climate fluctuations and households' distance to the nearest town. We find that that children in households closer to towns have significantly smaller impact of their birth timing on their subsequent heights and risk of death. The protective effect of towns could involve a variety of mechanisms such as consumption smoothing, disease cycles, health services and public assistance. Future work might find ways to distinguish among these channels using additional data.

1. INTRODUCTION

This study investigates the hypothesis that access to towns and cities can help protect rural children against poor health conditions before and soon after birth. Since urbanization, household location, and rural infrastructure cannot be assigned experimentally, we turn to other sources of variation in children's exposure to adversity. Our outcomes of interest are each child's mortality risk, height, and weight, which collectively are indicators of past health conditions, as well as predictors of future well-being. Our setting is the Democratic Republic of Congo (henceforth DRC), whose vast expanse generates extreme variation in household's degrees of rural isolation, and also variation in the severity of seasonal cycles. We use the randomness of a given child's birth month to measure their exposure to seasonal risk factors, in a spatial difference-in-differences approach designed to estimate the impact of households' access to towns and cities on their resilience against typical seasonal cycles. Spatial difference-in-differences has historically been used in economic valuation studies, for example assessing the value of greening vacant urban land (Heckert and Mennis 2012), or estimating the change in land values following the intrusion of an invasive species (Horsch and Lewis 2009). The use of this analytical method can also be used to assess the causal influence of location on child health, addressing issues of unobservable factors that influence child health and are correlated across space, and correlated with observable risk factors as well.

Seasonality in birth outcomes, child health, and farmer well-being has been observed around the world, including most recently in Brazil, where negative rainfall shocks have adverse consequences for birth outcomes (Rocha and Suares 2015). Turning to outcomes of interest for staple-crop farmers, outcomes which are highly related to child health in these low-income settings, another recent study in Indonesia found that a storage program increased non-food consumption, reducing seasonal risks (Basu and Wong 2015). Both Brazil and Indonesia are similar to the DRC in terms of vast sizes, locations in relation to the equator, predominant ecosystems, and high proportions of people with agricultural livelihoods. Despite the proximity of these three large countries to the equator, where climates and weather can be relatively constant throughout the year, there are still measurable seasonal risks for various outcomes associated with typical variation in climatic conditions.

Households could use the services of a nearby town or city to buy or sell goods, visit the health clinic, or seek solutions for a livestock disease or pest infestation. This study does not prescribe what households should do or actually do with access to a town, only that they can do what they choose more easily than if the town were less accessible. Variation in access to towns for farm families may help explain why we sometimes observe that increases in income doesn't necessarily improve health outcomes. For many of those living in remote areas and working on family farms, the lack of services and opportunity to trade may be the limiting factor for health improvement, in contrast with a lack of income being the limiting factor. One example of such an environment is the DRC, a country rich in natural resources and agricultural potential, where nutrition indicators and standards of living have steadily declined over the past few decades and are now among the worst in the world (Ulimwengu et al. 2012: ICF International 2014). The vast size, lack of roads, and weak institutions of DRC limit market integration, which causes substantial variation in local agricultural conditions across space. In the DRC and in other settings, infrastructure and other investments to improve rural households' access to markets and public services could increase their productivity and welfare, as well as allow for more effective nutrition smoothing across the year (Barrett et al. 2001; Dercon 2002). Nutrition smoothing in the face of adverse shocks is of increasing interest, especially when it is possible to compare groups of children using a natural experiment study design, as done by Giles and Satriawan (2015) who found that a supplemental nutrition program protected children exposed to the financial crisis of Indonesia from 1997 to 1998. If a household is able to smooth their nutrition outcomes across the year, this is highly reassuring. Consumption smoothing is a necessary condition for nutrition smoothing, but nutrition smoothing also indicates a deeper level of resilience which includes the ability to safeguard against disease cycles.

Our analytical method is spatial difference-in-differences, in three dimensions. First, we use climate data to identify regions with and without seasonal fluctuations. Next, we use the randomness of birth timing to identify exposure to seasonal risk where it exists. Finally, we use remoteness of households to identify whether access to towns confers resilience for children born in those places at riskier times. We use continuous variables for diagnostic regressions and exploratory exercises, then aggregate observations into dichotomous categories, add mother and community fixed effects and conduct various robustness checks against our identification strategy to address concerns about endogeneity and correlated errors as discussed in Bertrand et al. (2004).

One contribution of the study is to demonstrate the use of spatial difference-in-differences for child health data, exploiting randomness in birth timing against differences in exposure across space to estimate the average effect of exposure using a repeated cross-section of household surveys, implemented twice, in 2007 and in 2013. This approach treats each survey location as spatially repeated cross-sections, invoking a “parallel trends” assumption to estimate the magnitude of exposure within each area. DRC has many unique features driving our results, but the method may have broad applicability in other settings with limited agricultural production data available.

The outcomes we consider are mortality, as well as z scores of both height-for-age (HAZ) and weight-for-height (WHZ) for those children who were alive at the time of each survey. By definition, for a population of healthy children the mean z score is zero at every age, but for populations at risk of shortfalls in height (stunting) and weight (wasting), the onset and duration of stunting and wasting follows characteristic time paths from month to month (Victora et al. 2010). To avoid bias introduced by when children are measured in relation to survey implementation, we follow Cummins (2013) and use a flexible age spline to control for the timing of observation. For measuring market access, we use the Euclidean distance from each survey site to the nearest major town. Finally, we use mother and community fixed effects, clustered standard errors by survey site, and grid-cells as our spatial units of observation when aggregation is necessary, such as for the civil conflict data. The use of grid-cells helps limit potential biases associated with endogeneity of administrative boundaries (Masters and McMillan 2001).

2. BACKGROUND AND MOTIVATION

2.1. Living standards and child nutrition on the Democratic Republic of the Congo

In DRC, approximately 75% of the population doesn’t consume sufficient calories for a healthy and active life (FAOSTAT 2014; Grebmer et al. 2011; WHO 2000), and the country has some of the world’s highest rates of child stunting (45.8%), wasting (14%), and underweight (28.2%)

(UNICEF 2011). These data reflect both longstanding poverty and recent disruptions associated with a protracted civil war: the Food and Agriculture Organization (FAO) estimates that per-capita food supply declined from 2595 kcal per person per day in 1994 to 1833 kcal per person per day in 2009 (FAOSTAT 2014), and various other indicators are worsening over time in contrast to encouraging trends in neighboring countries (Kandala et al. 2011; Tollens 2003).

As in most of Africa, the majority of DRC's population is agricultural, and arable land per person or per agricultural worker have declined sharply in recent decades (FAOSTAT 2014). The volume and value of crops commonly grown in DRC, such as cassava, sugar cane, maize, and plantains has been declining since 1997 (FAOSTAT 2014), and the lack of infrastructure or markets and services ensures that many households cannot effectively smooth consumption or protect children against adverse health shocks.

2.2. Environmental variability and child health in other settings

Variation in environmental conditions has opened countless opportunities to study the causes of differences in health and other child development outcomes (Angrist et al. 2001; DiNardo 2008). The substantial body of literature in this area uses severe environmental shocks such as a drought, famine, or war, as well as seasonal or other more subtle variations to identify exposed children (Akresh et al. 2011; Akresh et al. 2012; Almond 2006; Banerjee et al. 2007; Bundervoet et al. 2009; Chay and Greenstone 2003; Ferreira and Schady 2009; Godoy et al. 2008; Hoddinot and Kinsey 2001; Maccini and Yang 2009; Minoiu and Shemyakina 2012; Skoufias and Vinha 2012; Yamano et al. 2005). The impacts of environmental conditions, whether extreme shocks or typical variations, on child health are especially of interest because of the potential long-term consequences affecting an individual's risk of disease, attained height, and labor productivity (Alderman et al. 2006; Almond and Currie 2011; Barker 2008; Barker 1990; Black et al. 2008; Deaton 2007; Dewey and Begum 2011; Martorell 1999).

A key feature of child development is its sensitivity to environmental shocks at critical ages and developmental periods (Shrimpton et al. 2001; Victora et al. 2010; Aguero and Deolalikar 2012). Much of the variance in later outcomes can be traced to early events, and a given nutritional deficit or illness may have greater effect when it occurs earlier in life (e.g. Yamano et al. 2005, Hoddinot and Kinsey 2001). These timing effects may be such that birth timing and hence age at which children were exposed to a given shock can be used to measure differences in resilience or vulnerability as in Tetens et al. (2003), or Maccini and Yang (2009) who found that boys were protected more than girls from the harmful effects of rainfall shocks in Indonesia.

Many studies addressing how people protect themselves from environmental harm use a direct measure of specific climatic conditions, such as deviation from normal rainfall at the time of a child's birth. For this study we have no direct observations of climate shocks. We are concerned with seasonal variations that are entirely predictable, and yet people may be unable to avoid their negative impact. One reason could be that so many factors move together: during the hungry or lean season, food supplies from the previous harvests dwindle, gainful employment may be more difficult to find, disease incidence often increases, and maternal labor time and calorie expenditure may rise (Buckles and Hungerman 2013; Panter-Brick 1997).

Despite the predictability of seasonal shocks, Gambian children born at unhealthy times have systematically lower weight-for-age and height-for-age than others (Gajigo and Schwab 2012), and have increased risk of mortality as young adults (Moore et al. 2004). Seasonal patterns of this type can be extremely robust, for example even after controlling for within-mother and within-community characteristics by comparing siblings and children residing in the same survey cluster (Currie and Schwandt 2013). A wide variety of health outcomes are typically affected at once, such as age of gestation at birth and birth weight (Rayco-Solon et al. 2005; Chodick et al. 2009), and the timing of harm may remain unknown since annual cycles hit before conception, once during pregnancy and again each year of the child's life. The worst time to be born is an empirical question, and is likely to depend on the type of shock and the circumstances of the household. One of the few biological constraints is that total energy demands on the mother are typically greatest in the last trimester of pregnancy, at birth and while breastfeeding (Chodick et, al. 2009). This could help explain why children born during lean seasons may be most disadvantaged, as the harm they experience just before conception and around 0, 12 and 24 months of age outweighs the benefits of favorable conditions in mid-pregnancy and around 6, 18 and 30 months of age.

2.3. Market access and child health

The goal of this paper is to ask whether access to towns and cities helps rural households escape seasonal health shocks. We build on the rich body of literature investigating the relationships between market access and consumption smoothing (Morduch 1995; Foster 1995), particularly Burgess and Donaldson (2010) who show that the expansion of railroads in India had a protective effect in maintaining real incomes and reducing mortality in the face of environmental shocks. The present study is a further test of the hypothesis from Burgess and Donaldson (2010), in a different setting and with heights and weights as outcomes in addition to mortality. Households that rely on agriculture for income and food may be most susceptible to climate variation and most unable to smooth consumption across seasons (Jensen 2000; Gajigo and Schwab 2012; Rabassa et al, 2012; Thai and Falaris 2014). Anthropological evidence from Peru suggests that these fluctuations may be greatest for the most isolated rural households (Pomeroy et al. 2014), but in other settings such as Bangladesh, even city-dwellers may experience seasonal shifts in food security and child weight-for-age (Hillbruner and Egan 2008).

For our study, an important feature of the DRC is that children's average health is not necessarily worst where seasonal variation is most extreme. For example, being geographically isolated could actually benefit households and children, as isolation and rugged terrain may protect them from violence in more populated or wealthy areas (Nunn and Puga 2012; Le Billon 2001). Furthermore, being located around the equator may provide relatively uniform weather and low seasonality, but also impose worse disease conditions than places with more seasonal fluctuations. Our research design is intended to take account of these factors, building on the diverse literature described above to isolate seasonal fluctuations from other factors and test for a protective effect of market access.

3. METHODS

3.1. Data

To conduct this study, we merged spatial and temporal data on child health, household characteristics, roads, terrain, land cover, towns, and civil conflict incidents across DRC. We used a one degree by one degree grid cells as spatial units, which avoids the endogeneity problems that may arise from using administrative boundaries as spatial units of observation

(Harari and La Ferarra 2013; Masters and McMillan 2001). Each of these grid cells is approximately 69 square miles in size, an area which varies only slightly across the country because distances between degrees of longitude and latitude are relatively more constant at lower latitudes closer the equator than at higher latitudes closer to the poles.

For data on child health and household characteristics, we utilized DRC's Demographic and Health Survey (DHS), which was conducted in 2007 and again in 2013. The DHS are nationally representative surveys, and cover N=27,724 children born of women participating in the 2007 survey and N=41,917 children born of women participating in the 2014 survey (ICF International 2014). The heights and weights of a sub-sample of children for each of the survey rounds were measured, for N=2,931 children in 2007 and N=5,504 children in 2013. Observations where the families had moved in the previous 6 years were dropped (n=4,060), to ensure that household market access and child exposure to conflict were measured as accurately as possible, including during the mother's pregnancy with the child. Observations flagged by DHS for biologically implausible measurements (where the absolute value of HAZ or WHZ was greater than 5) were also dropped (n=3,302). This left N=69,641 births and N=8,435 measured children across both survey rounds to conduct our study.

We controlled for exposure to civil conflict because violence is widespread and endemic in DRC. The conflict data are from the Armed Conflict Location and Event Dataset (ACLED), which details specific incidents of civil insecurity between 1997 and the present day for DRC and other countries (Raleigh et al. 2010). Events which occurred between 2001 and 2013 were retained for this project to correspond first with the oldest children surveyed for the 2007 DHS round during their mother's pregnancies, up until births that took place during the year of the most recent 2013 DHS round. The ACLED data are geocoded daily incident reports. Therefore, each day that an incident (such as a battle) continues is counted as an additional event. We aggregated the incident reports into the number of events by month in each one-degree grid cell. The events are categorized into eight different types of conflict incidents, including violent and non-violent activities (Raleigh et al. 2010). The dataset is designed to provide an accurate picture of overall conflict activity in a country, but clearly under-counts incidents in places where people are less likely to report them.

We used geocoded data on 160 major towns from the Multipurpose Africover Database on Environmental Resources (FAO MADE 2014) to assess a survey cluster's market access. We calculated the Euclidean distances from the centers of each DHS survey cluster to the centers of each nearest major town point location, using ArcGIS 10.0 (ESRI 2013). 'Proximity', defined as inverse distance (km^{-1}) enters the regressions as our measure of access to all kinds of markets and public services.

3.2. Nutrition outcomes for children

The outcomes of interest are mortality (a binary indicator of whether the child was alive at the time of the survey), height-for-age z scores (HAZ) and weight-for-height z scores (WHZ) for children under the age of 5 years. The z scores were calculated by Measure DHS using WHO reference values for the distribution of heights and weights in a healthy population at each age and sex (Measure DHS 2008). There is mounting evidence that shortfalls in height (stunting) and weight (wasting) share common causes (Martorell and Young 2012), and may reflect a wide variety of health insults which may have disrupted the child's immune function (Raqib et al. 2007), gut microbiome (Gordon et al. 2012; Kau et al. 2011) or other influences on healthy growth. Food availability can also matter, but a wide variety of factors at every stage of child development can also lead to systematic differences in mortality risk, heights and weights attained by children who were born in different seasons, even in the presence of sufficient calorie intake.

3.3. Identification strategy

The naturally occurring random variation we exploit is the child's month of birth. We hypothesize that this quasi-random variation influences survival, heights, and weights only in regions with seasonal rainfall, and does so less where households are closer to towns and cities. For this spatial difference-in-differences design, we also control for mother fixed-effects in mortality regressions and community fixed-effects in height and weight regressions, to account for time-invariant unobservable attributes that cannot be included. Mother fixed-effects cannot be included in height and weight regressions because typically, one child per mother is measured for the anthropometry sub-sample. Thus, comparing the heights and weights of siblings is not possible. However, using community fixed-effects provides an adequate alternative to control for other unobservable factors which may influence health. Using fixed-effects, we effectively compare many pairs of siblings and children living in the same survey cluster, allowing us to reduce the effects of unobservable heterogeneity across mothers and communities. To account for spatially correlated errors, we pool the children by risk exposure into dichotomous groups based on birth season, distance to the nearest town, and distance from the equator. This strategy mirrors one of the suggested approaches for addressing auto-correlated errors across time in traditional difference-in-differences, as described in Bertrand et al. (2004).

Our analytical approach is illustrated in Table 1, showing how each subsample is classified in terms of exposure to seasonal risk and the potentially protective effect of market access.

[Insert Table 1 about here]

As shown by the first two rows of Table 1, our first hypothesized effect is that, in regions with distinct seasons, being born in one half of the year is associated with worse outcomes than being born in the other half. Inferring a causal effect of seasons relies on randomness of the child's birth month. We test that identifying assumption following the presentation of our main results, and find that other influences on heights and weights are not driving selection into the season with adverse outcomes.

Our main hypothesis, shown in the third row of Table 1, is that among children born in places and at times where they are vulnerable to seasonal risk, being closer to towns is associated with less harmful outcomes. Here, inferring a causal effect relies on a “parallel trends” assumption that seasonal risk factors would have been similar across households if not for their differences in distance to town. That identifying assumption is itself untestable, but we can show robustness of the design using a variety of placebo regressions. In these falsification tests, a valid specification would show no effect, and any significant result would be an artifact of the method. We perform two types of such tests: first in our placebo region where rainfall varies little from season to season, and then for placebo outcomes such as maternal education and household altitude that are predetermined and cannot have been affected by a child’s birth month.

3.3.1. Measuring exposure to seasons

To accurately capture seasonality in the DRC context and divide the sample into locations with more or less variation in rainfall, we used the absolute value of latitude of each DHS cluster's location. Locations closer to the equator have relatively uniform temperature and rainfall throughout the year, while locations both north and south of the equator have a more pronounced dry "winter" season (World Bank CRU 2014). The country stretches from approximately +5 degrees north to about -14 degrees south. Our demarcation lines, chosen to divide the sample into two roughly equal halves, are at +4 and -4 degrees of latitude. Thus, most of the surveyed households who are subject to seasonal fluctuations are in the southern hemisphere, where the drier winter season occurs around June-August. Almost 20 percent of our sample, or 13,841 of our 69,641 births, are located in the northern hemisphere where the timing of seasons is shifted by six months so that winter occurs around December- February. To construct a single variable that indicates births in a given season, we define "*rain months*" to be the calendar month for households located in the southern hemisphere, and shifted 6 months forward for households in the northern hemisphere. For example, children born in the calendar month of January are recorded as such if in the southern hemisphere, and that month is recorded as "June" for the few children born in the northern hemisphere. These birth months are then aggregated into birth seasons, capturing a child's exposure to similar seasonal conditions anywhere in the country using a single variable.

3.3.2. Controlling for age to avoid survey timing effects

The DHS, like other surveys, are typically implemented in waves at specific times of year. Our data were primarily collected in June 2007 and in December of 2013 as detailed in Tables 8a-8b. So, children born in earlier months (e.g. in May and in November of the respective survey round years) are surveyed at a younger age than those born in later months (e.g. in July or January, respectively). Since height and weight z scores vary systematically with age, to avoid artifacts due to survey timing we follow Cummins (2013) and control for age using a linear spline specification based on the average time path of stunting and wasting actually observed in our data. For HAZ, the piecewise linear controls have three splines with knots at 6 months and 22 months of age, and for WHZ we use two splines with one knot at 12 months of age. The number and location of these splines approximates the nonparametric relationship we observe in the DRC data, which is similar to the age effects found in other settings (Victora et al. 2010).

3.4. Econometric specification

Our primary specification is a fixed effects OLS regression with interaction terms. The mother and community fixed-effects absorb unobservable characteristics of mothers and communities in our sample, allowing us to compare siblings and children living in the same community who were and were not born during dry winters. Standard errors are clustered by survey cluster, of which there are 300 in the 2007 survey and 540 in the 2013 survey, for a total of 840 locations.

There are three dependent variables of interest, indicated by Zi on the left-hand sides of the equations: a binary indicator of whether the child was alive at the time of the survey, the height-for-age z score (HAZ) and weight-for-height z score (WHZ). We control for age in months, or time elapsed since birth in the case of mortality regressions, (Age_i) in piecewise linear form as described above, and for child sex (Sex_i) defined as 1= male. Birth season for child i (as BS_i) enters as a binary variable (with 1= births occurring between January through June in the southern hemisphere and occurring between July through December in the northern hemisphere). The absolute value of latitude for each DHS cluster j is used to stratify the sample between children around the equator who face little seasonal variation, and those farther from the equator who experience a dry winter season. Household wealth (H_i) enters as a categorical variable computed by DHS as quintiles of the national distribution, based on ownership of durable goods in the household. To control for civil conflict, we use a continuous measure (C_j) defined as the number of conflict fatalities recorded in the child's grid-cell from their conception over their lifetime to the survey date. The underlying civil conflict data span from 2001 to 2013, and cover every grid cell in the country.

Household proximity to the nearest major town enters as a binary indicator (R_j) of whether the household is relatively remote, with 1= household faces greater distance to access the nearest major town. The cut-off was designated as 28.8km based on the median Euclidean distance in our sample. The R_j binary variable also enters as part of an interaction with birth season, to construct our difference-in-difference specification, where the estimated coefficients on that interaction ($\hat{ATE}_{ij}$) can be interpreted as the average treatment effect of household remoteness on child mortality, heights or weights, given their exposure to the seasonal risk. A negative estimated ATE indicates that being located far from town limits households' ability to protect their children from harm, and conversely that proximity helps confer resilience.

The reduced form econometric models are shown below in Equations 1-2. These estimating equations could be derived from a typical health production function where health status at the time of survey is a function of current and lagged health inputs, as well as key environmental characteristics such as sanitation, disease exposure, and parents' health and childcare knowledge (Rosenzweig and Schultz 1983). For our empirical purposes, the reduced form model is sufficient. The main pathway through which we expect birth season to affect the health production function is through the presence of adverse conditions such as low food supply or high rates of disease transmission, either of which could affect a child directly or indirectly through the mother's health during the sensitive periods of gestation and infancy. The subscript *i* indexes children, *k* indexes the linear age splines, and *j* indexes DHS clusters (household locations). ε_i is a stochastic error term with the usual properties, and δ_r are the region fixed effects.

Equation 1 is a diagnostic regression using continuous variables and no interaction terms, estimated using Ordinary Least Squares (OLS). In this equation, the absolute value of latitude ($Latitude_j$) enters linearly and continuously as degrees, and household remoteness enters continuously as proximity to the nearest major town in km^{-1} (P_j). Equation 2 is our spatial difference-in-difference specification, pooling observations into binary variables for the child's location and birth timing. We split the sample by distance from the equator to construct a placebo region where no effect is expected, and estimate the model with region fixed effects to account for time invariant regional factors omitted from the model. Standard errors are robust and clustered by region to account for potential correlations among respondents who reside in the same areas. Management of the spatial data was done in ArcGIS 10 (ESRI 2013), and econometric analysis was performed in StataMP 12 (StataCorp 2011).

$$Z_i = \alpha + \sum_{k=1}^{n} \beta_k Age_k + \gamma_1 Sex_i + \gamma_2 H_i + \gamma_3 C_j + \gamma_4 P_j + \gamma_5 BS_{i\,i} + \gamma_6 Latitude_j + \varepsilon_i \qquad (1)$$

$$Z_i = \alpha + \sum_{k=1}^{n} \beta_k Age_k + \gamma_1 Sex_i + \gamma_2 H_i + \gamma_3 C_j + \gamma_4 R_j + \gamma_5 BS_i + ATE_{ij}\left(BS_i \cdot R_j\right) + \delta_r + \varepsilon_i \qquad (2)$$

In the results reported below, mother fixed effects are included in the preferred specification (Table 6) for the mortality regressions, and community fixed-effects are included in the height and weight regressions.

4. RESULTS

Descriptive statistics for our data are presented in Table 2a, for the whole sample and for each sub-sample used in the regressions. There is some variation in these means and standard deviations by group, with most children being quite short at a mean HAZ score around -1.5, and children in regions with a dry winter are particularly thin with mean WHZ scores around -0.5 versus -0.2 for children around the equator. Conflict events appear to be more frequent or intense for locations closer to the equator. Similar patterns are also seen in Table 2b, which splits the descriptive statistics into the 8 groups representing the triple difference-in-differences econometric specification as illustrated in Table 1. The first column summarizes the group that is protected by their access to nearby towns. The second column summarizes the group which we expect to be affected by birth season, since they aren't protected by access to a nearby town. Conflict fatalities are much more prevalent closer to the equator, which may confound results because the uniformity of the seasons across the year increases closer to the equator as well. It appears also that households closer to town are systematically wealthier, and this is confirmed using a two-sample t-test for equality of means (p=0.000, results not tabulated). Thus, in future investigations of these data it will be necessary to distinguish between the effects of wealth and the effects of proximity to town. The other 6 groups do not show systematic differences in their wealth.

[Insert Table 2 about here]

Exploratory t-tests for differences in child mortality, heights and weights across groups are shown in Table 3, for effects of the child's gender, remoteness, and birth season. Mean HAZ and WHZ is lower for boy children than for girls, and mean HAZ but not WHZ is lower in remote areas compared to other locations. Boys have higher mortality risk, as do children living in remote locations. Mean HAZ is also lower for children born during Jan.-June as opposed to births during the second half of the year.

[Insert Table 3 about here]

The onset and duration of stunting follows standard age patterns as shown in Figure 1, which uses kernel-weighted (Epanechnikov kernel) local polynomial regressions to estimate mean HAZ values for each age in months, separated between remote households in areas with seasons versus the rest of the sample. The households in remote areas with seasons are expected to be the worst group in terms of child outcomes, because the children are exposed to seasons and not protected by a close proximity to town. Figure 1 shows that this group does indeed appear to be worse off than the rest of the sample. There is a steep decline in HAZ before 24 months of age, and then the slope flattens but is still negative. For WHZ the decline ends at around 12 months of age, and is followed by catch-up back to near zero by 5 years of age. There does not appear to be differences between remote and non-remote households in the weights of their children at the time of the survey (charts not shown). Comparing remote versus other households we see no significant differences at each month, although the HAZ path is consistently lower and the overall difference is significant as shown in Table 3. Figure 2 shows that children in remote areas are systematically more likely to have died, and this disparity increases with the time elapsed since birth.

[Insert Figures 1-2 about here]

The variation in stunting and mortality by month of birth is shown in Figures 3 and 4, which like the previous charts use Epanechnikov kernel weighted local polynomial smoothing to estimate mean HAZ and mortality risk values for children born in each month, accounting for the inversion of seasons by hemisphere. Figure 3 reveals that the children born in July-December are systematically taller, and that children in remote areas are systematically shorter for each different month of birth. Figure 4 shows that the fluctuations in mortality risk by month of birth have greater amplitude in remote areas with seasons, and that the children in remote areas with seasons are still more likely to have died. These charts provide further evidence that the worst group to be in are those who are exposed to seasonal risk but not protected by a nearby town.

[Insert Figures 3-4 about here]

To address the relationship among all our variables, Table 4 presents the results of a diagnostic OLS regression to estimate the association between children's z scores and their age, sex, birth order, preceding birth interval, conflict exposure, household wealth, proximity to the nearest major town, and birth season. This exercise reveals the characteristic pattern that HAZ and WHZ both decline with age, although for HAZ the rate of decline is not significant for the first spline covering 0-6 months of age, and WHZ is shown to have recover significantly in the second spline after 12 months of age. Risk of death also decreases as more time since birth has elapsed, as is expected. Male children have consistently lower z scores and higher risk of mortality. Conflict incidents in the grid-cell of a child's residence have statistically significant associations with nutritional outcomes: a negative association with HAZ and a positive association with WHZ. Firstborn children are more likely to have survived, and having a short preceding birth interval is associated with poorer height and survival outcomes. Conflict exposure is associated with an increased risk of death. Household wealth is positively associated with HAZ, but not WHZ. Wealth is also positively correlated with survival. Having controlled for these key factors, our variables of interest for the difference-in-difference design are not individually significant except for the mortality regressions. To obtain a clearer picture of the causal pathways, we need to simplify our model and add fixed-effects.

[Insert Table 4 about here]

First, we will present the full triple difference-in-differences specification, where our main variable of interest is the triple interaction term indicating a child who was born during January-June, lives in a location with a dry winter, and lives in a relatively remote location far from town.

It turns out that the estimated coefficient on this variable for the triple difference-in-difference specification is statistically significant for heights as an outcome. The estimated coefficient on the key interaction term is not statistically significant for the other outcomes of interest.

[Insert Table 5 about here]

Results of our preferred difference-in-difference specification (Equation 2) are shown in Table 5. Following the research design described in Table 1, this test splits the sample into areas of interest with a dry winter season (columns 1 and 3) and the placebo regions with less seasonal rainfall variation (columns 2 and 4). Each regression then includes interaction terms between season of birth and remoteness, where both are specified as binary variables.

To begin with our control variables, all regressions include fixed effects for mothers (for mortality regressions) and communities (for height and weight regressions), and standard errors clustered by survey site. Age profiles for HAZ are similar to the diagnostic regression and similar in the two regions. Gender differences for mortality, HAZ, and WHZ are also similar to the diagnostic regression and across the two regions. Interestingly, in this specification children are taller where there are more reported conflicts, but only in the areas without a dry winter. We do not control for wealth in this final specification because these effects are perfectly collinear with the fixed-effects.

The average treatment effect of being remote when exposed to seasons is the estimated coefficient on the interaction term between them. Looking first at the treatment regions (columns 1, 3, and 5), the average treatment effect of household remoteness is statistically significant for survival and heights, but only in the locations with a dry winter season. The average treatment effect of remoteness is not significant for weights as an outcome. The effects for survival and heights are quite large in magnitude, with the height effect being similar to jumping about two quintiles in household wealth, and the survival effect similar to jumping one quintile in household wealth.

[Insert Table 6 about here]

To test robustness of this result, we can look first within Table 6 at results in the placebo regions, where there is less seasonal fluctuation in rainfall. Here, there are no statistically significant average treatment effect estimates for any of the outcomes.

5. ROBUSTNESS CHECKS

To address limitations of our main result, we conducted a wide variety of other tests.

5.1. Sample selection

We exclude children with biologically implausible measurements as discussed above. We are reasonably confident that measurement error in the DHS data is random and not systematic. Results do not change whether including or excluding households which have lived in their survey location for fewer than 6 years at the time of the interview (N=4,060). Results also do not change whether including or excluding households which took trip lasting more than 1 month during the 12 months preceding the interview date (N=6,969). These observations were originally flagged for attention to ensure that exposure to conflict and remoteness was accurately measured for all children under the age of 60 months, including during pregnancy. There are approximately 3 million internally displaced persons residing in DRC, and this at-risk population may not have a stable living situation, increasing the chances of measurement error and under-sampling in household surveys (UNHCR 2014).

5.2. Colinearity and heteroscedasticity

First, we calculated variance inflation factors (VIF) for each of the explanatory variables in the diagnostic regression model to assess the presence of multicollinearity. The results are reported below in Table 7. We did not include the VIF for the difference-in-difference model, as those contain interaction terms and therefore the VIF would not be as informative. The VIFs are all relatively low, ranging from 1.00-2.17, where the highest values are for age splines where colinearity is expected. This increases our confidence that colinearity is not affecting the accuracy of our standard errors.

[Insert Table 7 about here]

We also assessed the presence of heteroscedastic errors using residuals plots and the Breusch-Pagan test. Based on visual inspection of residuals plots not shown here and the results of the Breusch-Pagan tests, we reject the null hypothesis that the errors are homoscedastic and therefore use robust standard errors, clustered by administrative region.

5.3. Seasonality in conflict incidence

There is evidence in other studies that civil conflict follows seasonal patterns (O'Loughlin et al. 2012; Hendrix and Glaser 2007). If there is seasonality in the incidence of civil conflict, it could impact the results of this study. Coincident cycles often threaten identification by seasonality. We performed nonparametric tests to assess whether conflict incidents followed a seasonal pattern. From Figure 5 below, it does not appear that there is seasonality in our conflict data. We also disaggregated these kernel-weighted local polynomial regressions by province, and the results are the similar across all provinces (results not shown here). There may be coincident cycles and other concerns with the conflict data, but since these are incident reports any patterns may reflect differences in reporting rates rather than actual conflict events.

[Insert Figure 5 about here]

5.4. Timing of data collection and the Cummins critique

Our research design uses birth month as a natural experiment in exposure to adverse factors during critical periods of child development. Since data were not collected uniformly over time, children born in different months were measured at different ages, and have consequently different levels of z score. As shown in Tables 8a and 8b, the majority of our data were collected between in February 2007 and December 2013.

[Insert Tables 8a-8b about here]

The consequences of age at measurement for identifying seasonal effects has been highlighted by Cummins (2013), using a type of diagram that we reproduce for each of the DRC survey rounds in Figures 6a-6b. These chart shows the average age of measured children who were born in each month, and their average HAZ score. Children born in July (January for the 2013 survey) were the oldest when surveyed, and they have the lowest average HAZ scores. Children born in December were the youngest on average when surveyed, and have the highest HAZ scores. This effect is controlled for in our regressions using age splines, as recommended by Cummins (2013). For an additional robustness test on survey timing we re-ran all regression models using only the June data, and that had no appreciable difference relative to the data from other months.

[Insert Figure 6a-6b about here]

5.5. Falsification tests

In addition to the placebo region built into our main result, we also tested our design against a variety of placebo outcomes as in Leigh and Neill (2011). These are dependent variables with no plausible mechanism by which they could have been caused by our independent variables of interest, so any significant correlation would be from random chance or an artifact of the research design that might also have given rise to our main result. The specific placebos we use here are: mother's education in single years, mother's height, father's education in single years, years that the household has lived in the interview location, the size of the household (number of people), and the altitude in meters of the household's location. Each of these occurred or was arguably determined independently of when the child was born, and is used here to ask whether our main results in Table 6 are actually artifacts of the data and research design, by using these variables in that exact same regression specification.

Figure 7 below provides a visual comparison of our main results with the placebo variables. Each dot and bar shows the ATE point estimate with its 95 percent confidence interval, first for the main results and then for the seven placebo tests. The chart has been cropped to show coefficient estimates for effect sizes between -1.5 and +1.5, since the randomness around some of the placebos resulted in such wide error bars that our outcomes of interest could no longer be distinguished on the same chart. As it is, the chart clearly shows that our precisely estimated negative effect on HAZ and WHZ is very different from the zero effects on any of the placebo outcomes. If we had found an effect of child's birth season on any of the placebo outcomes, the validity of our identification strategy would have had to be questioned (Jones 2007).

[Insert Figure 7 about here]

5.6. Selective fertility and mortality

Perhaps the most important threat to our research design is nonrandom birth timing. Figure 8 shows the frequency of births by month, which rises in March, April and May then has a long trough in July through December. These data are shown for both calendar month, and in terms of "rain months" which shift the birth dates for the few children in the northern hemisphere whose seasons are reversed. We do not know why the number of births rises in March, April and May. That pattern could stem from a rise in conceptions during the dry "winter" (June, July and August), or from seasonal patterns in miscarriage and neonatal mortality. The amplitude of the curve is slightly lessened when measuring by rain month, implying that socioeconomic factors involving calendar months may be more important than seasons.

[Insert Figure 8 about here]

To test whether seasonality in birth month could confound our results, Table 9 presents the outcome of testing our binary season-of-birth variable against all the explanatory variables in our dataset. The regressions are estimated first for the whole sample together (column 1) and then for each of the climate zones separately (columns 2 and 3). These results suggest that the potential effect of endogeneity of birth timing is not influencing our findings.

[Insert Table 8 about here]

For selective mortality, Figure 9 shows no clear difference between children born in the first and second halves of the year. There may be some discontinuity around June, which is the month in which most of the survey visits occurred in 2007. As a result, respondents' children who were born in July had the most elapsed time prior to the survey date, and correspondingly they are shown here to have had the most cumulative mortality. Conversely, children who were born in May had the least elapsed time prior to the survey date, and turn out to have had the least cumulative mortality by the survey date. As with selective fertility, these patterns are interesting but cannot explain or contradict our main results.

[Insert Figure 9 about here]

6. CONCLUSION

This study exploits temporal and spatial differences in health risks to test whether access to markets and services helps rural households smooth child health outcomes. Our setting is the Democratic Republic of Congo (DRC), one of the world's most impoverished countries. Its vast expanse straddles the equator, exposing households to differing degrees of seasonal rainfall variation, and its lack of roads gives each household very different travel times to the nearest town. In this context, we can use the randomness of birth timing for a spatial difference-in-difference approach, asking whether households with easier access to markets and public services can use that to protect their children from seasonal fluctuations in malnutrition and disease. We don't prescribe or focus on what exactly households can do when they have access to a town, only that they can do what they need more easily than if they didn't have access to a town.

For the purposes of this paper, market access is defined as distance to the nearest major town, and health outcomes are defined in terms of child mortality, heights and weights at the time of the country's 2007 and 2013 Demographic and Health Surveys (DHS). The mechanisms by which easier access could help smooth variation associated with birth timing include product and factor markets, migration and remittances as well as social services, health care and public assistance. Access to a road to sell goods produced on the farm is another potential mechanism, one that has garnered much attention in the literature. However, this study suggests broader implications for farm families, indicating that there are opportunities beyond selling goods produced on the farm for nutrition smoothing. More detailed data would be needed to distinguish among the possible channels, including an incorporation of nearby town population into diagnostic regressions. For now, our goal is to test whether any nutrition smoothing effect exists.

Our main result is that households' access to towns and markets is indeed linked to resilience, protecting children from seasonal fluctuations in health conditions at birth. The magnitude of gain for child heights is similar to the improvement associated with rising two quintiles in the local wealth distribution. Results of this magnitude are large but plausible, and help explain the differences in health outcomes found in a wide variety of other settings. We subject our finding to a variety of robustness tests, including comparisons of the estimated average treatment effect with similarly estimated coefficients in placebo regions and for placebo outcomes, selection bias in birth timing and child mortality, and other possible threats to identification.

Further work would be needed to distinguish among the possible causal mechanisms involved, for example to distinguish between the role of private markets and the use of public services, or between improvements in the diet and reductions in disease burdens. Incorporating estimates of the populations of nearby towns would help distinguish what size of town is needed to allow for nutrition smoothing. Different mechanisms may matter for different people, but all rely on infrastructure to link rural households with towns and cities where goods are traded and services are provided. These results add a new dimension to the role of rural infrastructure and access to towns. Interventions to lower households' travel costs could help reduce their vulnerability, in addition to the many well-known investments that target specific causes of malnutrition such as improved diets, health care and reduced disease transmission.

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9. TABLES AND FIGURES

Table 1: Spatial variation in exposure to seasons, birth timing and access to towns

Analytical design and hypothesized effects over triple difference-in-differences
(region x birth timing x market access)

<table>
<tr><td>Region has a distinct rainy season?
(= farther from the equator)</td><td colspan="4">Yes</td><td colspan="4">No</td></tr>
<tr><td>Child was born in or after rainy season?
(=Jan-Jun if lat.<0, Jul-Dec otherwise)</td><td colspan="2">Yes*</td><td colspan="2">No</td><td colspan="2">Yes</td><td colspan="2">No</td></tr>
<tr><td>Household is closer to town?
(=distance to town in km)</td><td>Yes</td><td>No**</td><td>Yes</td><td>No</td><td>Yes</td><td>No</td><td>Yes</td><td>No</td></tr>
<tr><td>Hypothesized status:</td><td colspan="4"><u>Vulnerable to seasonal variation</u></td><td colspan="4"><u>Not vulnerable to seasonal variation</u></td></tr>
<tr><td></td><td>Protected*</td><td>Affected**</td><td colspan="2">Unexposed</td><td colspan="4">No effect</td></tr>
</table>

Note: Asterisks indicate hypothesis of significantly worse child nutrition relative to other groups in the same row. For *, the identifying assumption is that birth timing occurs randomly between seasons (tested). For **, the identifying assumption is that seasonal risk factors would have been similar in the absence of towns (untestable).

Table 2a: Descriptive statistics by birth timing and exposure to season variation

Birth timing:	*Jan.-June*	*Jan.-June*	*July-Dec.*	*July-Dec.*	*All Births*
Presence of seasons:	*None*	*Dry winter*	*None*	*Dry winter*	*N=69,641*
	N=18,009	*N=18,973*	*N=16,724*	*N=15,935*	
Child status					
Children Alive (%)	84.6%	84.5%	83.7%	85.2%	84.5%
HAZ	-1.51	-1.51	-1.61	-1.26	-1.47
	(*1.68*)	(*1.62*)	(*1.92*)	(*1.80*)	(*1.86*)
WHZ	-0.31	-0.47	-0.24	-0.45	-0.38
	(*1.25*)	(*1.12*)	(*1.41*)	(*1.31*)	(*1.33*)
Age (months)	28.24	28.00	29.70	29.88	29.16
	(*17.57*)	(*17.29*)	(*17.10*)	(*16.69*)	(*16.53*)
Firstborn (%)	23.8%	24.9%	23.8%	23.5%	24.5%
Short interval (%)	28.2%	27.9%	26.1%	19.74%	25.6%
Boys (%)	50.5%	51.2%	50.4%	50.2%	50.6%
Household					
Wealth (quintile)	2.61	3.20	2.60	3.25	2.92
	(*1.27*)	(1.46)	(*1.26*)	(*1.45*)	(*1.40*)
Proximity (km^{-1})	0.11	0.16	0.10	0.15	0.13
	(*0.23*)	(*0.27*)	(*0.23*)	(*0.27*)	(*0.26*)
Environment					
Conflicts	108.72	15.03	93.52	15.95	31.28
	(*716.5*)	(*65.7*)	(*596.8*)	(*69.7*)	(*66.9*)
Latitude (abs val)	1.91	6.14	1.98	5.99	4.31
	(*1.36*)	(2.01)	(*1.17*)	(*2.02*)	(*2.64*)

Note: Data shown are means and standard deviations (in parentheses). Births labeled as January-June occurred in calendar months July-December for children born in the Northern hemisphere (N=17,159). Conflicts are total number of fatalities during the child's year of birth in the respondent's 1-degree square grid-cell of residence.

Table 2b: 2 x 2 x 2 Descriptive statistics by triple difference-in-differences groupings

Region has distinct rainy season?	*Yes*	*Yes*	*Yes*	*Yes*	*No*	*No*	*No*	*No*
Child born during rainy season?	*Yes*	*Yes*	*No*	*No*	*Yes*	*Yes*	*No*	*No*
Household closer to town?	*Yes*	*No*	*Yes*	*No*	*Yes*	*No*	*Yes*	*No*
Observations (N)	*12,080*	*6,893*	*10,012*	*5,923*	*6,858*	*11,151*	*5,983*	*10,741*
Alive	85.85%	82.20%	87.06%	82.18%	85.02%	84.44%	84.32%	83.35%
HAZ	-1.28 (*1.62*)	-1.89 (*1.54*)	-0.96 (*1.64*)	-1.47 (*1.69*)	-1.48 (*1.77*)	-1.51 (*1.64*)	-1.51 (*1.72*)	-1.35 (*1.69*)
WHZ	-0.47 (*1.15*)	-0.50 (*1.07*)	-0.39 (*1.16*)	-0.48 (*1.16*)	-0.27 (*1.33*)	-0.33 (*1.20*)	-0.29 (*1.29*)	-0.35 (*1.18*)
Age (months)	27.79 (*17.19*)	28.35 (*17.45*)	27.06 (*17.11*)	26.80 (*17.33*)	28.23 (*17.77*)	28.24 (*17.46*)	27.53 (*17.59*)	27.27 (*17.33*)
Firstborn	25.25%	24.30%	24.60%	21.50%	23.31%	24.19%	24.06%	23.64%
Short interval	27.75%	28.30%	19.41%	20.30%	29.9%	27.10%	25.73%	26.34%
Wealth (quintile)	3.63 (*1.44*)	2.46 (1.16)	3.70 (*1.41*)	2.48 (*1.17*)	2.97 (*1.33*)	2.37 (*1.18*)	2.99 (*1.32*)	2.39 (*1.18*)
Conflict fatalities	20.47 (*77.02*)	5.91 (*38.78*)	21.95 (*80.96*)	6.14 (*44.01*)	191.42 (*888.83*)	60.39 (*587.77*)	169.52 (*880.62*)	52.43 (*353.32*)

Note: Data shown are means and standard deviations (in parentheses). Conflict fatalities are the number of fatalities recorded in incident reports nearby during the child's year of birth. Births labeled as January-June occurred in calendar months July-December for children born in the Northern hemisphere (N=17,159).

Table 3: Two-sample T-tests with equal variances

	Alive	*HAZ*	*WHZ*
Gender			
Girls	0.85	-1.31	-0.33
Boys	0.84	-1.48	-0.44
Difference	0.009	0.17	0.10
Pr(T>t)	0.00**	0.000**	0.00***
Household Location			
Not Remote	0.85	-1.26	-0.38
Remote	0.83	-1.53	-0.39
Difference	0.03	0.26	0.02
Pr(T>t)	0.00**	0.00***	0.28
Birth season			
Born Jan.-June	0.84	-1.50	-0.39
Born July-Dec.	0.84	-1.28	-0.38
Difference	-.001	0.22	0.01
Pr(T>t)	0.69	0.00***	0.28

Table 4: Exploratory regression with continuous explanatory variables

Variables	Unit/type	(1) Child is alive Exploratory	(2) HAZ Exploratory	(3) WHZ Exploratory
Age spline 1	Linear spline	-0.017***	-0.074**	-0.107***
		(0.000)	(0.015)	(0.000)
Age spline 2	Linear spline	-0.002**	-0.072***	0.011***
		(0.015)	(0.000)	(0.000)
Age spline 3	Linear spline		-0.006	
			(0.104)	
Child is male	Binary	-0.115*	-0.133**	-0.108**
		(0.052)	(0.046)	(0.026)
Child is firstborn	Binary	-0.288***	0.021	-0.026
		(0.000)	(0.811)	(0.690)
Short preceding birth interval	Binary	-0.594***	-0.148*	-0.020
		(0.000)	(0.060)	(0.731)
Ln(fatalities during birth year)	Continuous	-0.062***	-0.114***	0.031**
		(0.000)	(0.000)	(0.032)
Household Wealth index	Categorical	0.145***	0.250***	0.053***
		(0.000)	(0.000)	(0.005)
Absolute value (latitude)	Continuous	-0.046***	-0.015	-0.017
		(0.000)	(0.313)	(0.130)
Proximity to town	km^{-1}	0.281**	-0.022	0.162
		(0.045)	(0.878)	(0.137)
Born Jan.-June	Binary	0.134**	-0.107	0.075
		(0.024)	(0.114)	(0.126)
Constant	Constant	2.940***	-0.256	0.407***
		(0.000)	(0.200)	(0.003)
Observations	N	18845	3405	3473
R^2	R^2		0.179	0.073

The linear age splines are actually ‘time elapsed in months since birth’ for the mortality regressions.
Age splines control for child’s age at observation. Born Jan.-June is actually born July-Dec. in Northern hemisphere to account for inversion of seasons at the equator. Conflicts are the cumulative count nearby to the child’s cluster of residence during the child’s birth year. Errors clustered by DHS survey cluster (v001). *p*-values in parentheses; * p<.10, ** p<.05, *** p<.01.

Table 5: Triple difference-in-differences

Variable	Unit/type	(1) Child is alive	(2) HAZ	(3) WHZ
Age spline 1	Linear spline	-0.016***	-0.080***	-0.100***
		(0.000)	(0.006)	(0.000)
Age spline 2	Linear spline	-0.002***	-0.067***	0.010***
		(0.001)	(0.000)	(0.000)
Age spline 3	Linear spline		-0.009***	
			(0.001)	
Short preceding birth interval	Binary	-0.510***	-0.187***	-0.039
		(0.000)	(0.002)	(0.387)
Child is male	Binary	-0.149***	-0.164***	-0.116***
		(0.001)	(0.002)	(0.003)
Ln(fatalities during birth year)	Continuous	-0.057***	-0.087***	0.018
		(0.000)	(0.000)	(0.152)
Proximity to town	km^{-1}	0.744***	0.369	0.144
		(0.003)	(0.127)	(0.418)
Born Jan.-June	Binary	0.080	-0.097	-0.022
		(0.279)	(0.281)	(0.743)
Absolute value(latitude)	Continuous	-0.004	0.045***	-0.019
		(0.783)	(0.009)	(0.138)
Born Jan.-June*Proximity	Interaction	0.104	0.877**	0.232
		(0.769)	(0.013)	(0.367)
Born Jan.-June*Abs(lat)	Interaction	-0.002	0.018	0.007
		(0.914)	(0.464)	(0.686)
Abs(lat)*Proximity	Interaction	-0.053	0.038	-0.014
		(0.247)	(0.480)	(0.728)
Born Jan.-June*Proximity*Abs(lat)	Interaction	-0.021	-0.201***	-0.000
		(0.730)	(0.006)	(0.996)
Constant	Constant	3.081***	0.200	0.627***
		(0.000)	(0.244)	(0.000)
Observations	N	18845	3405	3473
R^2	R^2		0.144	0.056

The linear age splines are actually 'time elapsed in months since birth' for the mortality regressions.
Age splines control for child's age at observation. Born Jan.-June is actually born July-Dec. in Northern hemisphere to account for inversion of seasons at the equator. Conflicts are the cumulative count in the child's cluster of residence during the child's birth year. Errors clustered by DHS survey cluster (v001). *p*-values in parentheses; * p<.10, ** p<.05, *** p<.01.

Table 6: Preferred specification, stratifying by the presence of seasons

Variable	Unit/type	(1) Alive Seasons	(2) Alive No Seasons	(3) HAZ Seasons	(4) HAZ No Seasons	(5) WHZ Seasons	(6) WHZ No Seasons
Age spline 1	Spline	-0.021***	-0.022***	-0.051	-0.135***	-0.098***	-0.101***
		(0.000)	(0.000)	(0.220)	(0.003)	(0.000)	(0.000)
Age spline 2	Spline	-0.003***	-0.002***	-0.086***	-0.090***	0.010***	0.012***
		(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Age spline 3	Spline			-0.005	-0.003		
				(0.110)	(0.254)		
Short interval	Binary	-0.284***	-0.302***	-0.385***	-0.449***	-0.172***	-0.062
		(0.000)	(0.000)	(0.000)	(0.000)	(0.001)	(0.244)
Male	Binary	-0.117***	-0.126***	-0.029	-0.293***	-0.104*	-0.038
		(0.001)	(0.000)	(0.687)	(0.000)	(0.069)	(0.457)
Conflict exposed	Binary	-0.043	0.036	0.139	0.249**	-0.074	-0.062
		(0.399)	(0.547)	(0.148)	(0.038)	(0.274)	(0.509)
Jan.-June	Binary	-0.127**	0.079	-0.097	0.063	0.051	-0.093
		(0.011)	(0.210)	(0.210)	(0.573)	(0.521)	(0.355)
Jan.-June*Remote	Interaction	**0.128***	-0.025	**-0.329****	-0.188	-0.034	0.132
		(0.092)	(0.747)	**(0.018)**	(0.177)	(0.759)	(0.263)
Constant	Constant			0.158	0.537**	0.524***	0.624***
				(0.417)	(0.020)	(0.000)	(0.000)
Observations	N	17217	17297	4224	4211	4312	4319
R^2	R^2			0.290	0.299	0.083	0.077

The linear age splines are actually 'time elapsed in months since birth' for the mortality regressions. Born Jan.-June is actually born July-Dec. in Northern hemisphere to account for inversion of seasons at the equator. Age splines control for child's age at observation. Mortality regressions include mother fixed-effects. Height and weight regressions include survey cluster fixed-effects. Conflict exposure is a binary indicator of whether there was civil conflict in a 1-degree square of the child's residence during the child's year of birth. Errors clustered by DHS-cluster (v001). *p*-values in parentheses; * p<.10, ** p<.05, *** p<.01

Table 7: Variance inflation factors (VIF)

	HAZ	*WHZ*
Age spline 1	2.17	1.33
Age spline 2	1.61	1.37
Age spline 3	1.5	N/A
Child is male	1.00	1.00
Number of Conflicts	1.25	1.25
Wealth Quintile	1.12	1.12
Remote	1.06	1.06
Born Jan.-June	1.04	1.01
Abs(Latitude)	1.19	1.19

Note: All results are as for Table 6.

Table 8a: Timing of data collection for 2007 survey

Month	Number of surveys	Percentage (%)	Cumulative Percentage (%)
January	23	0.08	0.08
February	1,935	6.98	7.07
March	128	0.46	7.53
April	826	2.98	10.51
May	3,172	11.45	21.96
June	21,166	76.40	98.35
July	453	1.64	99.99
September	3	0.01	100.00

Note: DHS administrative data for all child health variables.

Table 8b: Timing of data collection for 2013 survey

Month	Number of surveys	Percentage (%)	Cumulative Percentage (%)
August	2,249	5.38	5.38
September	1,182	2.83	8.21
October	39	0.09	8.31
November	5,481	13.12	21.43
December	32,823	78.57	100.00

Note: DHS administrative data for all child health variables.

Table 9: Testing for endogeneity of birth timing, for whole sample and within climate zones

Variable	Units/type	(1) Born Jan.-June	(2) Born Jan.-June Seasons	(3) Born Jan.- June No seasons
Child is Male	Binary	0.009	0.023	0.005
		(0.762)	(0.632)	(0.895)
Wealth index	Categorical	-0.015	-0.057	0.002
		(0.384)	(0.106)	(0.919)
Ln(fatalities)	Continuous	0.014	0.003	0.018
		(0.125)	(0.830)	(0.152)
Proximity to town	km^{-1}	0.319*	0.538	-0.047
		(0.069)	(0.227)	(0.875)
Abs val (latitude)	Continuous	0.021		
		(0.138)		
Observations		18804	7060	11728

Note: Dependent variable is a binary indicator of birth during the Jan.-June wet season. Regression estimated using fixed-effects logit. All results include fixed effects for survey clusters (N=840), with notation and variable definitions as in Table 6. *p*-values in parentheses ; * p<.10, ** p<.05, *** p<.01.

Figure 1: HAZ by child age and household remoteness, with 95% CI

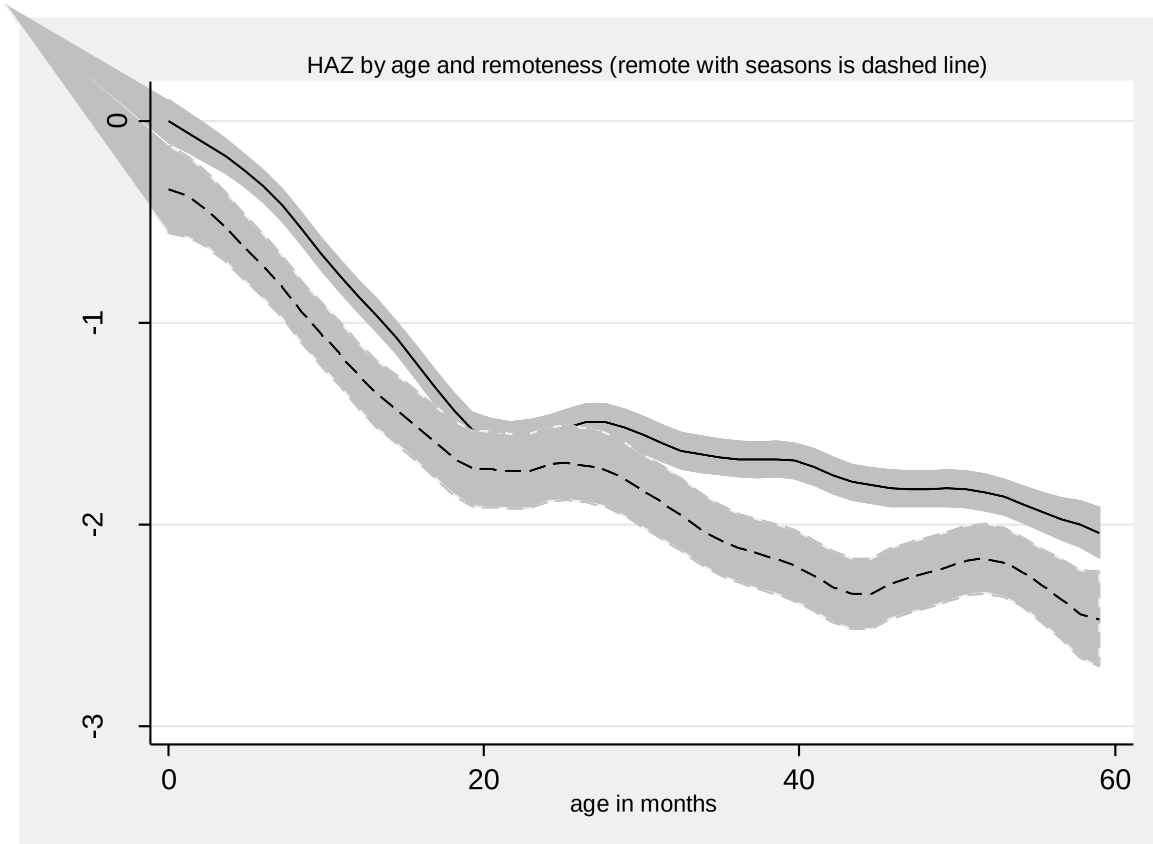


Figure 2: Child is alive, by month of birth and household remoteness, with 95% CI

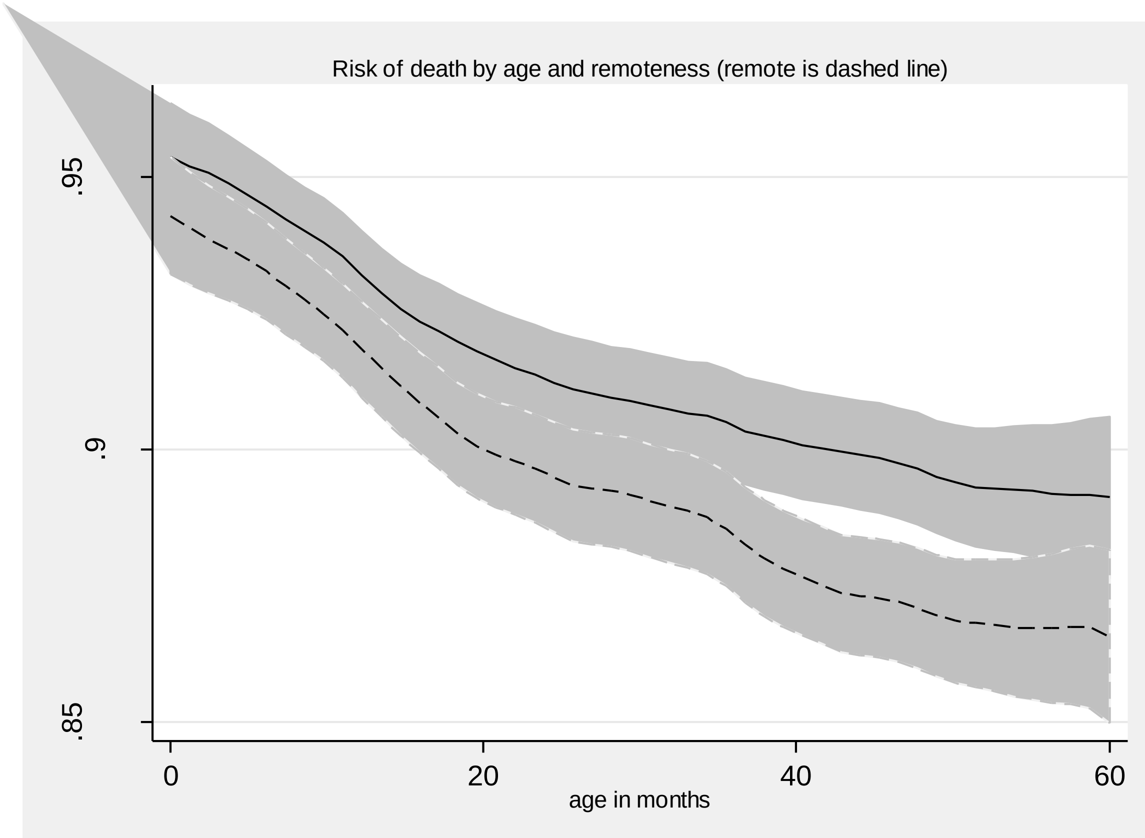
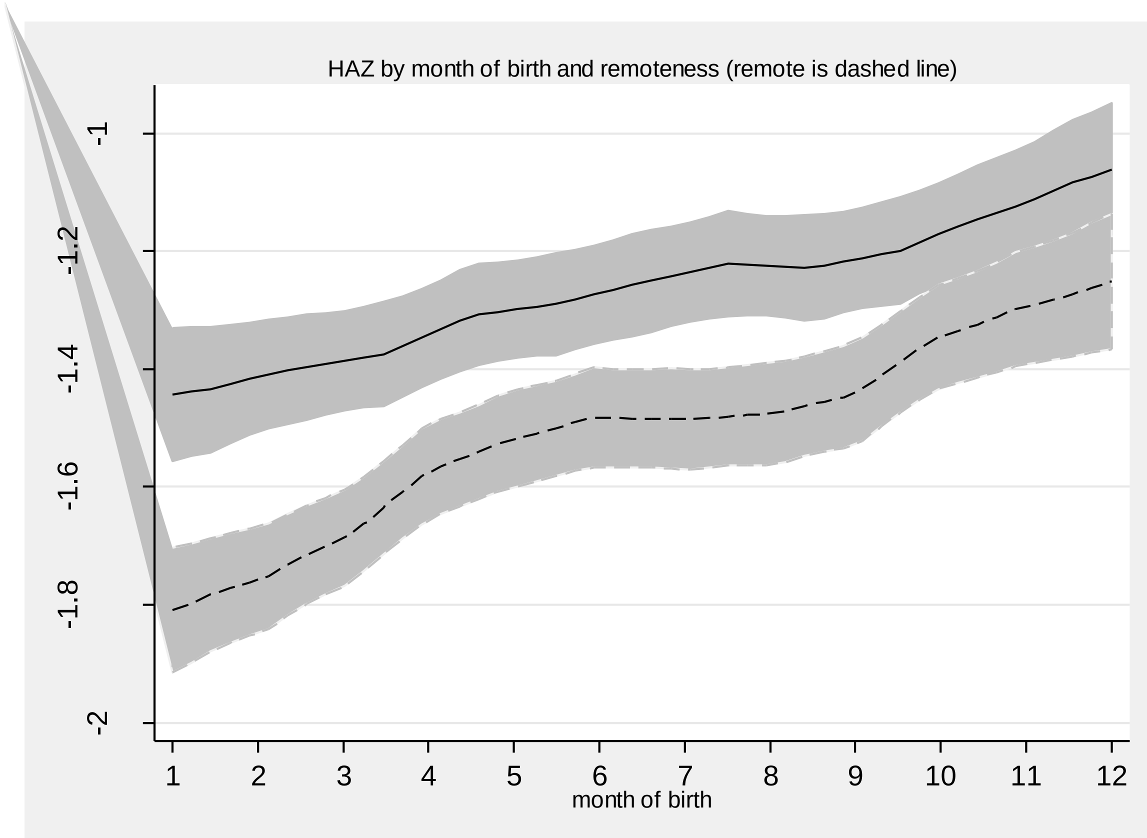


Figure 3: HAZ by month of birth and household remoteness, with 95% CI



Note: To account for inversion of seasons, birth date is shown by calendar month in the southern hemisphere, and for the northern hemisphere is shown as 1=July, 2=Aug. etc.

Figure 4: Risk of death by month of birth and household remoteness

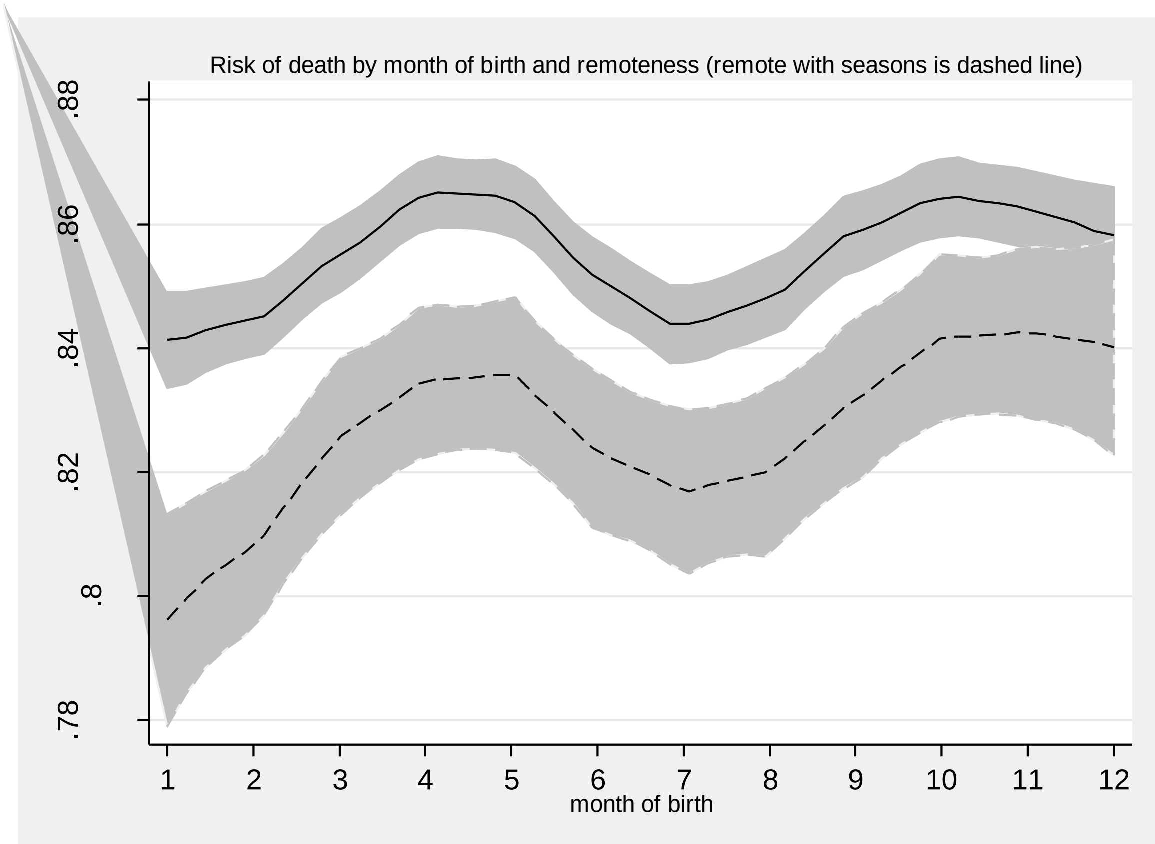


Figure 5: Conflict incidents by month

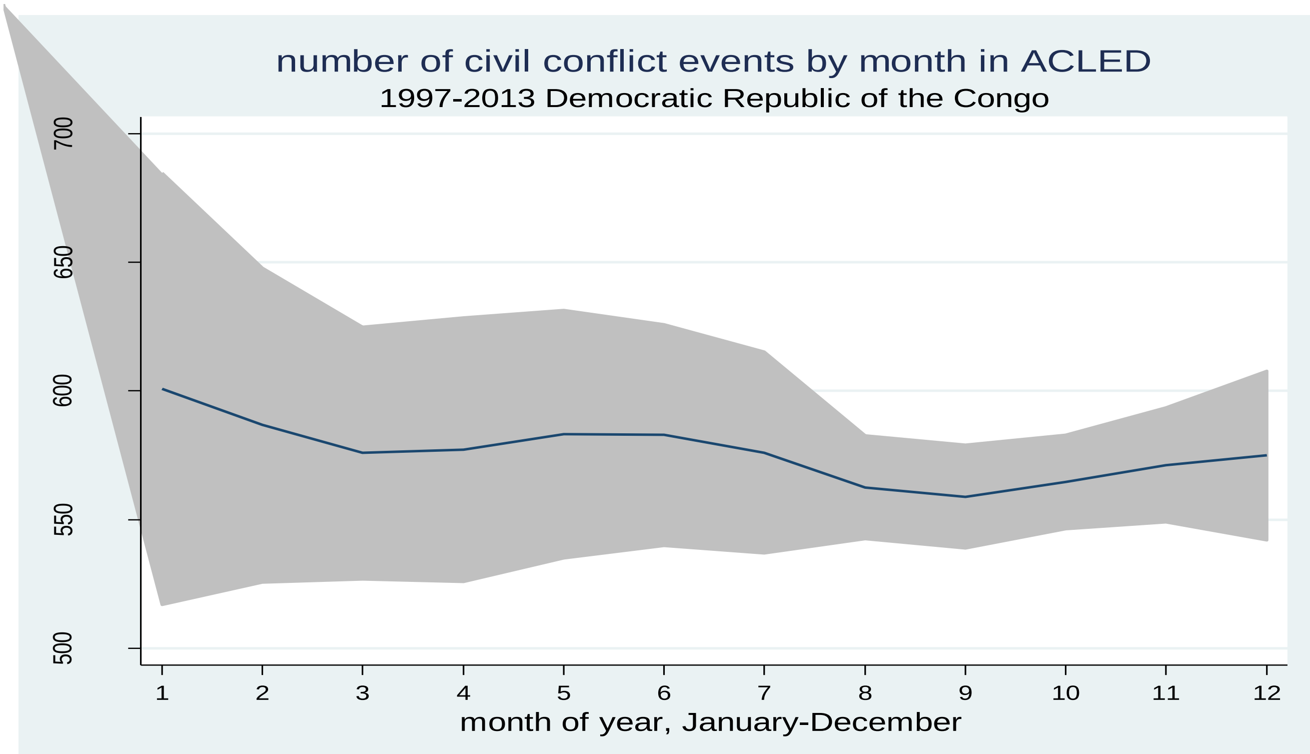


Figure 6a: Mean age and HAZ at time of survey by calendar month of birth, 2007 DHS

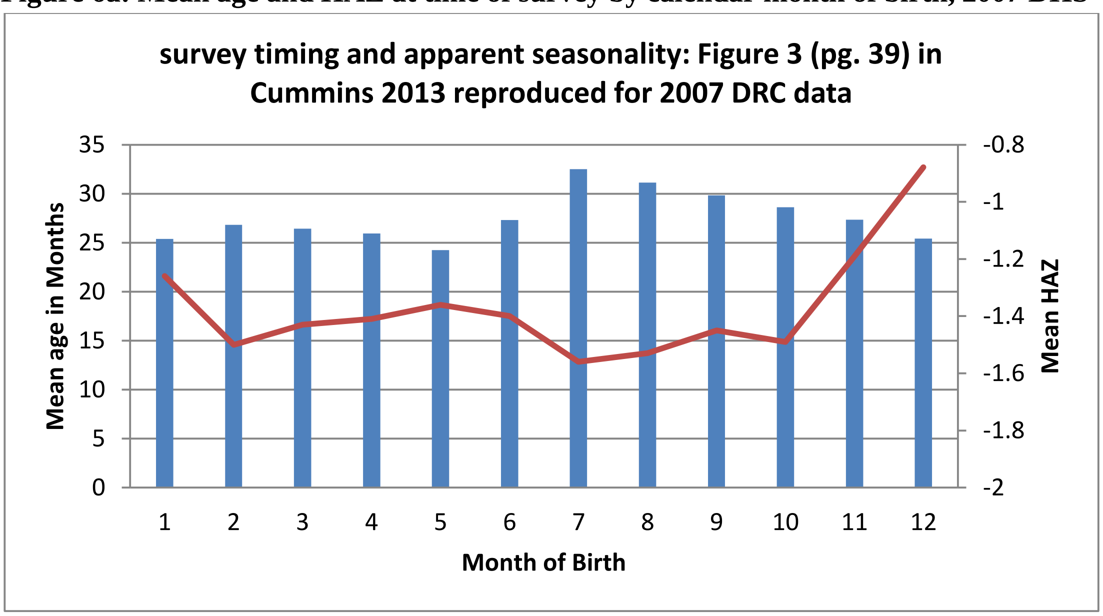
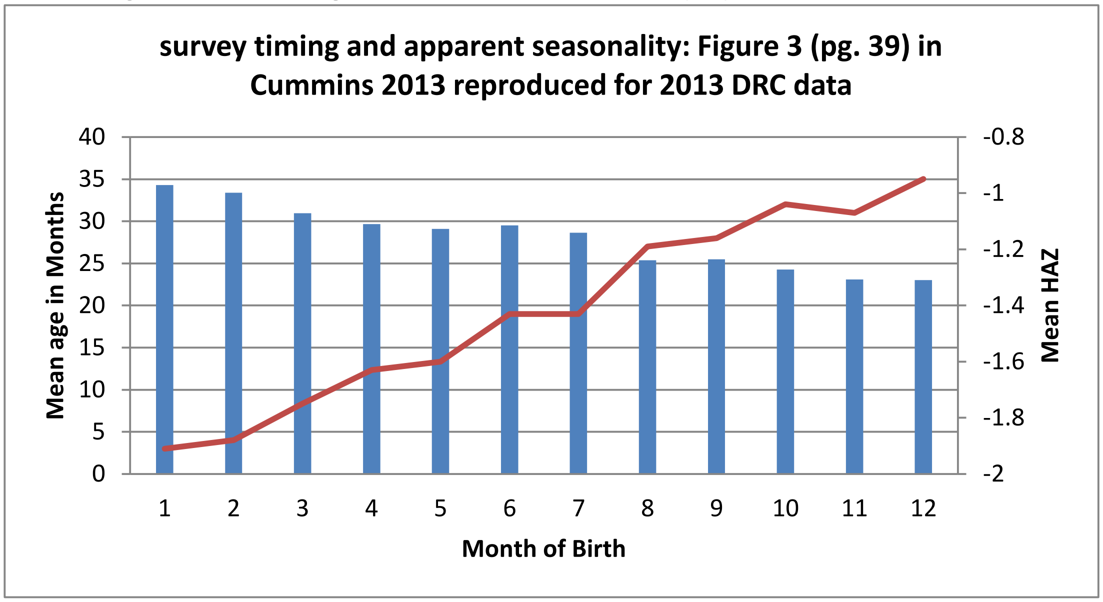
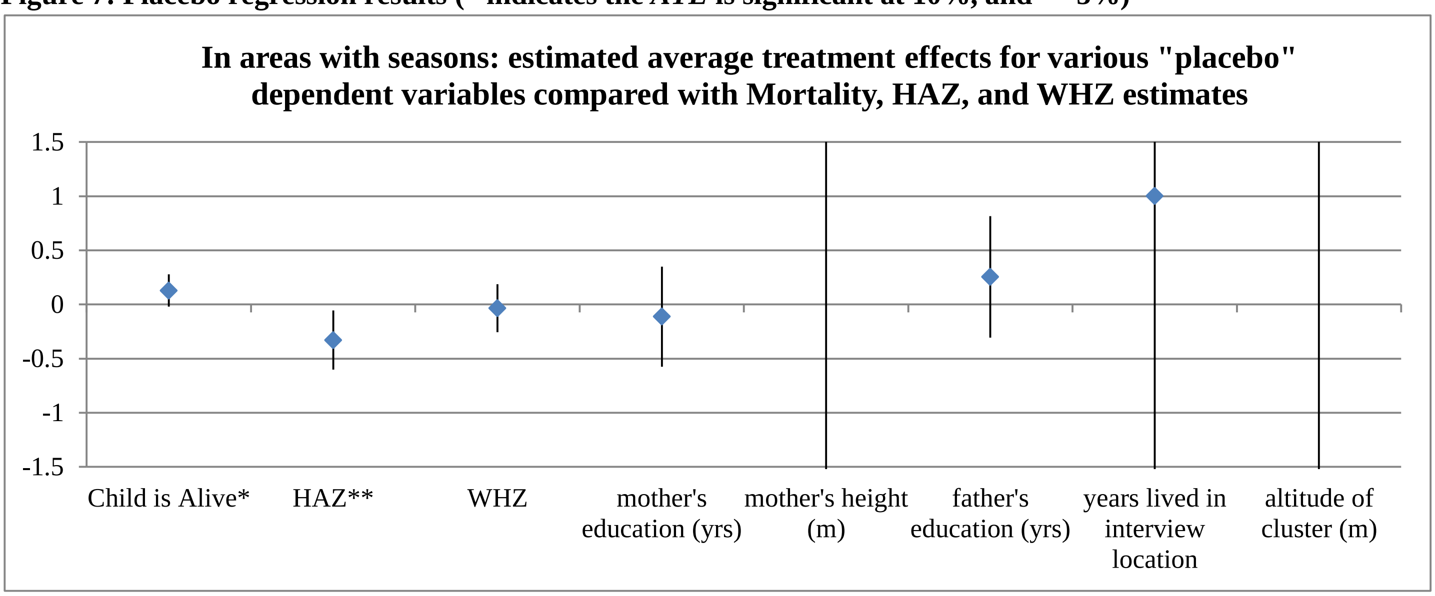


Figure 6b: Mean age and HAZ at time of survey by calendar month of birth, 2013 DHS



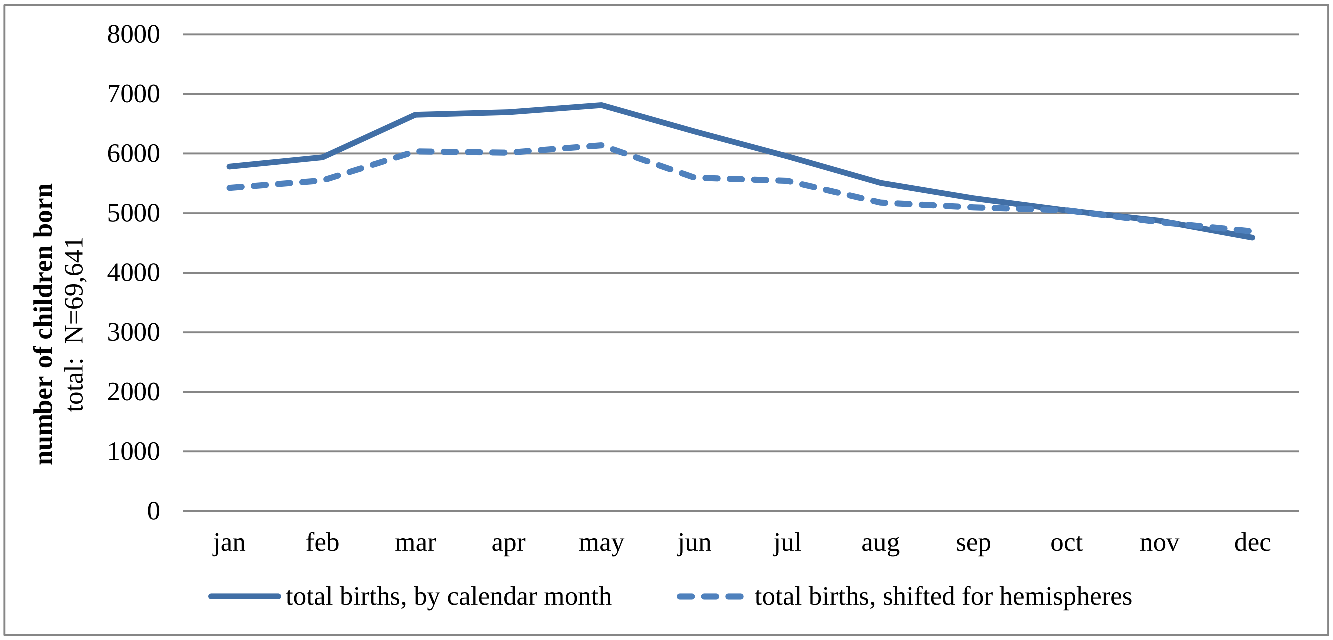
Note: These charts reproduce Figure 3 of Cummins (2013), using our DRC data across both DHS survey rounds. The line shows average HAZ on the right axis by the child's month of birth, and the bar shows their average age by month of birth on the left axis. As detailed in Tables 7 and 8, over three-quarters of the 2007 DRC surveys were implemented in June, and over three quarters of the 2013 DRC surveys were implemented in December. So, children born in July (for the 2007 round) and January (for the 2013 round) are surveyed at the oldest average age and have correspondingly lowest average HAZ scores. This 'survey timing artifact' effect is controlled for in our regressions using a flexible linear age spline, based on the time path of HAZ and WHZ scores shown in Figures 1 and 2.

Figure 7: Placebo regression results (* indicates the *ATE* is significant at 10%, and ** 5%)



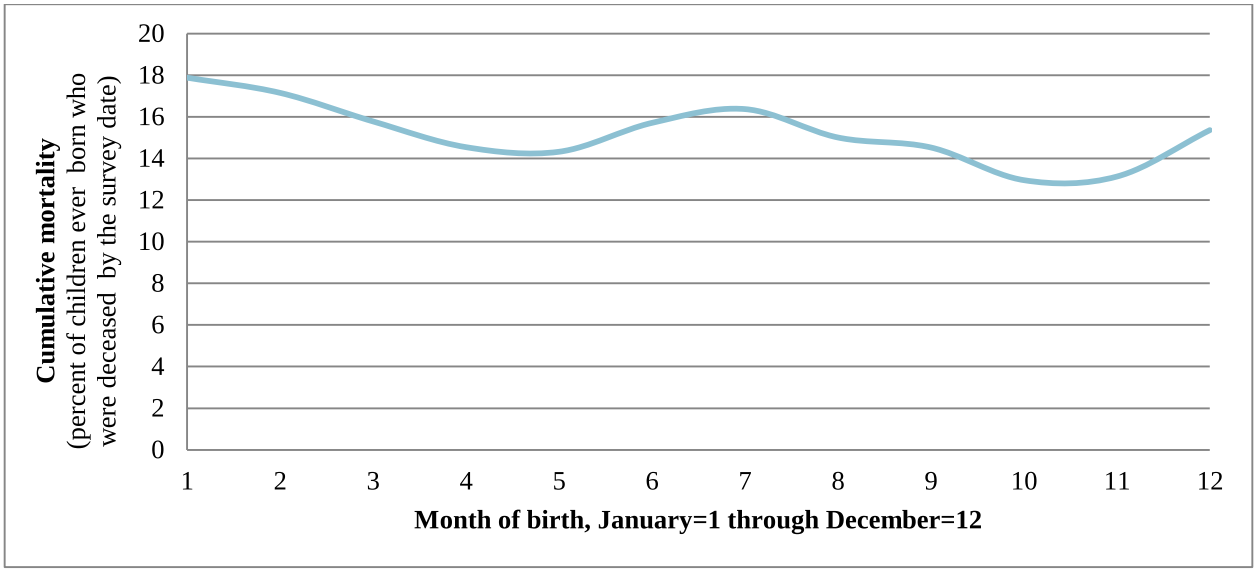
Note: Data shown are coefficient estimates (in blue) and 95% confidence intervals for "average treatment effects" in our preferred specification (Table 6), for our three dependent variables of interest followed by five 'placebo' variables for which no effect is expected of our 'treatment', due to the absence of any plausible mechanism of action.

Figure 8: Timing of births by calendar month and season



Note: Data shown are the number of children ever born in each month, as recorded across each DHS survey for DRC. The solid line refers to calendar months, and the dashed line uses a seasonal adjustment by hemisphere, where dates north of the equator are recorded as "January" for births in June, "February" for July, etc. In our regressions, these "*rain months*" are aggregated into six-month periods, since as children in higher latitudes who are born in the January-June period are more exposed to heavy rains and subsequently poor health outcomes than those born in the rest of the year. As shown here, more children were born in these adverse months than in July-December, as conception was slightly more likely to have occurred during the dry winter season. This pattern suggests that birth timing is either random or associated with factors other than variation in the child's health prospects.

Figure 9: Cumulative mortality prior to survey date



Note: Data shown is the cumulative mortality rate for all children ever born to the survey respondents across both rounds of the DHS in DRC (2007 and 2013), by the child's month of birth.