Weather and Infant Mortality in Africa*

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Abstract

To what extent have weather fluctuations in Africa affected infant mortality over the last fifty years? We investigate this question by combining individual level data, obtained from retrospective fertility surveys (DHS) for more than a million births in 28 African countries, with data for weather outcomes, obtained from re-analysis with climate models (ERA-40). The focus is on two mechanisms: malaria and malnutrition. We find robust statistical evidence of quantitatively significant effects. Infants born in areas with epidemic malaria that experience worse malarious conditions during the time in utero than the site-specific seasonal means face a higher risk of death, especially when malaria shocks hit low-exposure geographical areas, or hit mothers in the first trimester of pregnancy. Infants born in arid areas who experience droughts when in utero face a higher risk of death, especially if born in the so-called hungry season just after the start of the rains. We also uncover heterogeneities in the infant moratility effects of growing season rainfall and drought shocks, depending on household occupation or education.

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1 Introduction

To evaluate the desirability of mitigation or adaptation to climate change, we need comprehensive information about the impact of weather and climate on central socioeconomic outcomes, like health. While some impact assessments do exist,[1] many estimates of large-scale socioeconomic impacts rely on bold extrapolation, often from case studies in developed nations. Existing knowledge is particularly faulty when it comes to regions like Africa, which are likely to be hit the hardest by climate change since societies are already vulnerable.

In this paper, we study the health impact of weather shocks in Africa over the last 50 years. The fragmented information we have about such impacts mostly comes from clinical studies in local settings. Large-scale studies are very scant, mostly due to a lack of relevant data. We try to take some steps towards filling this lacuna of knowledge.

Specifically, we focus on the effects on infant mortality. The reason is twofold. On the one hand, infant death is Africa's largest health problem alongside HIV: still today, close to 100 out of 1000 babies born on the continent die before the age of one. On the other hand, and unlike the case of HIV, variations in weather most likely play an important role for infant death through channels like malaria and malnutrition.

Aside from its substantive objective, the paper also has a methodological purpose. We show how data from very different sources can be combined to overcome the lack of comparable African data at a continental scale. These data come at different resolutions in time and space. Thus, we use individual data on life events and background variables from retrospective surveys, available at the monthly frequency and associated with a specific GPS coordinate, and data on rainfall and temperatures from re-analysis with a global atmospheric weather forecasting model, available at the six-hour frequency on a 1.25×1.25 degree grid. When we examine nutritional channels of weather impacts (see below), we use data on growing seasons from spectral information collected by satellites, available bi-weekly at an 8×8 km resolution.

Identifying the causal effects of weather variations on health is not straightforward. We can think of many reasons why weather and health might be non-causally correlated, e.g., due to their joint dependence on geography – places on the coast have different weather than land-locked locations, but

[1] See IPCC, 2007 for an overview.

people along the coast often have better economic opportunities, higher incomes, and better health. When we isolate the effects of specific weather events, we only use the temporal deviation from the normal monthly pattern within each given location. As the natural variability of weather over time is, arguably, uncorrelated with any latent determinants of health, we are in effect using a gigantic set of natural experiments to identify the effects on infant mortality.

We focus on two mechanisms, malaria and malnutrition, and find robust statistical evidence of quantitatively significant effects through both of them. Infants born in areas with epidemic malaria, who in utero experience worse malarious conditions than the site-specific seasonal means face a higher risk of death, especially in areas with very low average exposure to malaria, or when malaria shocks hit mothers around the time of conception. Infants born in regions of Africa with an arid climate who in utero experience droughts face a much higher risk of death than other babies, especially if born in the so-called hungry season around the start of the rains. We also find marked heterogeneities in the effects of rainfall and drought on infant mortality, depending on household occupation and education. The results we uncover are not only statistically robust but also quantitatively important. For example, we estimate that a six-month malaria epidemic in a place with little average exposure to malaria can raise infant mortality by about 3 percentage points. The effect of a drought in an arid area is of the same magnitude.

While we are not aware of any studies with a similar scope and methodology for Africa, there are some recent reminiscent studies by economists. Deschenes and Greenstone (2007a) estimate the effect of weather shocks on overall mortality in the United States, but they rely on county-level rather than individual-level data and focus on cardiovascular disease. Burgess et al (2010) look at weather-induced mortality in India, but they too look at overall mortality and mostly rely on district-level data. References to other related work are given in context below.[2]

In the following, Section 2 of the paper gives general background on our data. Sections 3 and 4 look separately at the effects of malaria and malnutrition, respectively, while Section 5 combines the analysis of the two channels. Section 6 summarizes our findings and discusses possible extensions.

[2] Artadi (2006) estimates the impact of being born in rainy seasons and hungry seasons on infant mortality in African countries. But her interest is to measure the impact of average monthly weather patterns while our focus is to estimate the weather impact of deviations from the average seasonal pattern.

2 Data – General Overview

Our most important data for this study come from two sources. We use individual data on health and demography outcomes assembled from DHS surveys, and spatially disaggregated data on weather outcomes obtained from ERA-40 re-analysis. In this section, we give some background on these data and how we put them together.

DHS surveys Demographic and Health Surveys (DHS) have been carried out with a similar methodology in many developing countries since 1984 with financial support from USAID. Each survey is carried out to collect information on life and health outcomes by interviews of a nationally representative sample of women in child-bearing age. Because of a standardized survey format, data from different surveys can easily be combined. DHS data have been used in a growing number of microeconomic papers on various topics in economic development.[3]

Each DHS survey employs a two-stage sampling, first selecting clusters (villages and town districts) and then selecting households within each cluster. In this study, we use a total of 53 DHS surveys, from 28 African countries – all the available surveys in which the geographic coordinate of each cluster is collected by a GPS receiver. These 53 surveys comprise information from a total of 18,381 clusters, located in both rural and urban settings. Figure 1 plots these clusters on a map of Africa. The data cover a pretty large part of the continent, including countries in the North (Morocco and Egypt), many in West Africa (from Senegal to Cameroon) and the Sahel, and countries in the East of Africa (from Ethiopia to Tanzania), as well in the South (ranging from Namibia to Madagascar).

In the retrospective fertility module of any DHS survey, women of age 15 to 49 in the sampled households are asked about the month and year of birth for each of their children, whether the child died after birth and, if so, in which month. If either the month or the year of birth is not reported or inconsistent, the date of the birth is imputed from auxiliary information. The surveys we exploit contain information about 1.2 million births by about 300,000 mothers that occurred at least 12 months before the survey date,

[3] Detailed information on the DHS surveys and the underlying methodology can be obtained from the website:
`www.measuredhs.com`

in the period (1957-2002) covered by our weather data (to be described). Dropping all the births with an imputed birth date leaves us with 1,008,249 births by 281,640 mothers.

For each of these births, we construct a binary variable indicating whether the child died as an infant (i.e., at the age of 12 months or less).[4] This is our major dependent variable in the paper. Infant mortality varies quite a bit both across time and place. For the full sample of births, the overall mean is 100.6 deaths per 1000 births, with a standard deviation across clusters of 69.5. But the mean masks a general decline from levels of mortality about 143 in 1970 to about 87 in 2002. Inspection of the data shows that infant mortality also varies quite a bit from year to year in addition to generally declining trends, as well as across groups of clusters (e.g., rural and urban areas) within the same country.

The fact that the surveys are retrospective gives us some causes of concern. While the birth and death of one's children are certainly life-defining events, we cannot rule out measurement error (perhaps more about the year than the month of birth or death). That our results do not change significantly when we drop all reported births more than 10 years before the survey (assuming events nearer in time are more easily recollected) is encouraging, and suggests that this type of measurement error is not a major problem in practice.

Another cause of concern is that mothers might migrate, so the mother's location at the time of the survey may not coincide with her location when her children were born. Using weather information pertaining to the surveyed cluster may thus attribute incorrect weather conditions to the time around birth. The surveys allow us to drop all births taking place before migrating mothers moved to the survey location, and this robustness check does not materially affect the results. Thus, the prospective (downward) bias of using weather data from the wrong place appears to be small (see further below).

The DHS surveys also give basic information about each child (gender, birth-order, etc.), their mother (weight, stature, years of education, occupation, etc.), her husband (years of education, occupation) and household (asset ownership, etc.) at the moment of interview. We will exploit some of these variables to investigate if the impact of weather shocks on infant mortality is heterogenous.

[4] The results are robust to excluding death at the age of 12 months from the definition of infant death.

ERA-40 re-analysis Development research has increasingly relied on shocks to weather, such as rainfall, as a way of isolating exogenous variation in variables like income. The bulk of this research relies on data from weather stations together with various interpolation methods to fill out the missing data.

A well-known weather data set based on weather station observations is the one supplied by the Climate Research Unit (CRU) at the University of East Anglia.[5] The CRU data set indeed includes data at a high temporal and spatial resolution (monthly data at down to 0.5×0.5 degree resolution) for much of Africa. But its interpolation method is problematic for exploiting variation within location over time.[6] Since weather stations with consistent time series observations in most African countries are few and far in between, and their precise location is not even public information, the CRU data is not appropriate for our purpose.

Miguel et al. (2004) use rainfall data from the Global Precipitation Climatology Project (GPCP), which relies on satellite images of cloud cover to estimate rainfall. However, for our study the GPCP is unsatisfactory: the spatial resolution of the GPCP data is rather coarse (2.5×2.5 degrees), and we need temperature data to predict malaria transmission risk (see the next section).[7]

For this reason, we have decided to rely on weather data produced by what meteorologists call re-analysis. Specifically, we use a data archive known as ERA-40 supplied by the European Centre for Medium-Term Weather Forecasting (ECMWF).[8] This re-analysis relies on historical data from a variety of sources: weather stations, ships, aircraft, weather balloons, radiosondes, and most importantly – from the late 1970s – satellites orbiting the Earth. Such observations are fed into the ECMWF's large-scale atmospheric circulation model (known as IFS CY23r4) to produce a string of grid-specific forecasts for every six hours. These are statistically combined with avail-

[5] See the webpage at `www.cru.uea.ac.uk/cru/data/`

[6] First, changes in the weather outcomes in a given location may be due to the availability of nearby weather station data over time. Second, if the closest weather station with available data is too far, a long-term average value is used. See Climate Research Unit (undated) for details.

[7] The higher resolution data of the GPCP (1.0×1.0 degree) is available only after October 1996.

[8] The data were downloaded from ECMWF's Meteorological Archival and Retrieval System. We are grateful for Heiner Körnich for help in this process.

able observations to produce a set of weather outcomes for every six hours, over the period from September 1957 through August 2002, on a global grid with approximately 139 kilometer's (1.25×1.25 degrees) resolution.[9] We expect this data set to contain among the very best available weather data for Africa, particularly since the poor observations are supplemented with frequent global satellite data, as this becomes available from the late 1970s –.in fact, about 88% of the births in our sample occurred later than 1978.

The grid for the ERA-40 observations, for the parts of Africa where we also have individual level data from the DHS clusters, is illustrated in Figure 2. The midpoint of each one of the 749 squares in the figure corresponds to a specific gridpoint in the ERA-40 archive. Each DHS cluster is matched to the closest ERA-40 grid point by using ArcGIS. With 18,381 clusters, there are thus almost 25 clusters on average per grid square. For each latitude-longitude point in the grid, we extract 6-hourly data on rainfall and temperatures and aggregate these data to the monthly frequency.

Summary statistics for infant mortality and weather outcomes, by clusters and various subgroups, are reported in Table1.

3 Malaria

In this section, we focus on malaria during pregnancy as the important channel through which weather affects the survival of infants.[10] We start by a brief overview of the medical literature on malaria and infant death, which suggests that large risks are associated with malaria infected mothers when the child is in utero. Next, we describe the index that we use to measure weather suitable for malaria transmission and infection, and how this index can be used to classify different DHS clusters for which we have data into different zones of malaria risk. Then, we present and discuss our econometric methodology and some results based on a simple linear model in the full sample and different malaria zones. The basic results clearly indicate that site-specific shocks to malarious conditions only have an effect on infant death in African areas with epidemic malaria, a result which is robust to a variety of statistical pitfalls. Thus, we look closer at subsets of these epidemic areas,

[9] See Uppala et al (2006) for an overview and details on the methodology behind the ERA-40 archive, as well as a (partial) validation of the data.

[10] By malaria, we mean the infection in humans caused by Plasmodium falciparum, the most deadly species of malaria parasites, which is the most prevalent in Africa.

allowing for a non-linear effect in the number of malaria months. Finally, we ask whether the risk of infant mortality varies systematically with household and mother characteristics, or with the trimester of pregnancy when malaria shocks hit.

Malaria and infant mortality Malaria is one of the major causes of death for children in Africa. Estimates provided by Murray and Lopez (1996, Appendix Table 6f) suggest that malaria caused about 15 % of deaths of children under the age of five in sub-Saharan Africa in 1990. It is estimated that about 75 % of the worldwide malaria death toll of one million people in a typical year, is made up of African children less than five years old (Snow et al, 1999). However, infants are known to have a reduced sensitivity to malaria during the first few months of life, and fatal infections are believed to be more likely in the latter half of the first year of life and the first few years of childhood (Maegraith 1984, p. 262).

Malaria in pregnancy is known to raise the likelihood of infant death via low birthweight.[11] – a major risk factor for infant death (McCormick, 1985). Guyatt and Snow (2001) show that the risk of low birthweight doubles if a baby's mother is infected with malaria during pregnancy, and that 5.7 % of infant deaths in Africa could be attributed to the low birth weight induced by maternal malaria.[12] The exact mechanism for the association between malaria in pregnancy and low birthweight remains unclear, although insufficiency of a malaria-infected placenta is thought to lead to intrauterine growth retardation and premature delivery (Brabin et al. 2004). Placental infection by malaria parasite in African pregnant women is quite frequent. For areas where malaria is endemic, the median infection rate in the studies reviewed by Gyatt and Snow (2004) is 26 %, with a range of 5 to 52 %. Desai et al. (2007) review studies conducted in low malaria transmission areas of Africa and report a median prevalence of placental infection amounting to 6.7 %.

On top of a higher likelihood of low birth weight, babies born to mothers with an infected placenta are reported to be more likely to develop a malaria infection during the first year of life (Le Hesran et al., 1997) and may become

[11] See Desai et al. (2007) for a recent and extensive review of the medical literature on malaria in pregnancy.

[12] Studies reviewed by Steketee et al. (2001) attribute 3 to 8 % of infant mortality to maternal malaria.

susceptible to measles earlier than other babies due to reduced placental transfer of maternal antibody (Owens et al., 2006).[13]

Given the above-mentioned immunity of infants to malaria during the first few months of life, malaria in pregnancy may have a more profound effect on infant survival than infants' own infection after birth.[14] Therefore, we will focus on the effects of weather-induced variation in malaria incidence while the child is in utero on the subsequent risk of infant death. However, we discuss exploratory estimates of mortality effects of malarious weather conditions during the first year of life.

The literature on malaria in pregnancy suggests several factors that raise the risk of infant death due to maternal malaria. One such factor is endemicity of malaria transmission. Where malaria is endemic, adult women develop partial immunity to malaria after repeated infections since childhood and thus avoid symptoms such as fever and anemia during pregnancy. Where malaria is seasonal or epidemic-prone, however, adult women lack in immunity against malaria. As a result, once infected with malaria, pregnant women have fever, which is known to increase the chance of premature delivery and of infant death (Luxemburger et al. 2001). We therefore expect the impact of malaria on infant mortality to be larger in areas where malaria transmission is low.

Firstborn babies are believed to face a higher risk of death due to malaria in pregnancy than those of higher birth order, although this heterogeneity appears to be absent in low malaria transmission areas (McGregor 1984). Rogerson et al. (2000) and Walker-Abbey et al. (2005) find that teenage women are more likely to be infected with malaria during pregnancy in Malawi and in Cameroon, respectively. Infants born to mothers infected with HIV as well as malaria face higher risks of low birth weight (ter Kuile et al., 2004). In general, firstborn babies, female babies, and babies born by stunted mothers, face a particular risk of low birthweight (Kramer, 1987), which makes it plausible that such babies may be particularly at risk in the wake of malaria shocks. We will look at whether these individual-level characteristics result in heterogeneous impacts of malaria-prone weather conditions.

[13] Measles is estimated to account for about 12 % of deaths of children under the age of five in sub-Saharan Africa in 1990 (Murray and Lopez, 1996, Appendix Table 6f).

[14] Snow et al. (2004) argue that looking only at the direct cause of death would significantly underestimate the impact of malaria on child death.

How to measure malarious weather conditions? The incidence and prevalence of malaria in a given area and time depend on a host of factors, including climatic, biological, geographic, and socioeconomic conditions. Based on clinical measurements of malaria prevalence, researchers have tried to combine such information on the spatial distribution of malaria in so-called malaria maps (see e.g., Snow, et al 2005 and Kiezowski et al, 2004). In this study, we are interested in the weather-induced variability of malaria-prone conditions over time within each area for which we have infant mortality data.

A necessary condition for malaria to spread is the growth and survival of vectors (a certain species of mosquitoes) transmitting the disease and parasites causing the disease. These growth and survival rates are known to be heavily dependent on temperature and rainfall, and we want to capture these conditions in a parametric way.

To this end, we follow Tanser, Sharpe, and le Seuer (2003), who propose a relatively parsimonious weather-based index for malarious conditions for Africa in their study of malaria and climate change. This index builds on the comparison of mean long-term (1920-80) monthly rainfall and temperature with monthly profiles of malaria transmission intensity in 15 different locations with differing malaria prevalence rates as well as biological ranges affecting both vector and parasite development. The resulting monthly predictions of malaria transmission are empirically validated against the malaria occurrence data from about 3800 parasite surveys in different African locations. The index correctly predicts 63 % of malaria transmission incidents and 96 % of the absence of malaria transmission.

Following the approach of Tanser et al (2003), we adopt the following:

Definition 1 *We set our binary malaria index for month τ in grid location g, $Z_{g,\tau} = 1$ if and only if all of the following four conditions are satisfied:*

(a) *Average monthly rainfall during the past 3 months ($\tau - 2, \tau - 1, \tau$) is at least 60mm.*

(b) *Rainfall in at least one of the past 3 months is at least 80 mm.*

(c) *No day in the past 12 months ($\tau - 11$ to τ) has an average temperature below $5°C$.*

(d) *The average temperature in the past 3 months ($\tau - 2, \tau - 1, \tau$) exceeds $19.5°C$+SD(monthly temperature in the past 12 months).*

If any one of conditions (a)-(d) fails, we set $Z_{g,\tau} = 0$.

Conditions (a) and (b) ensure the availability of breeding sites for the vector and sufficient soil moisture for the vector to survive; (c) is required for the survival of the vector, as it quickly dies off at lower temperatures; and (d) allows the parasite to become infectious inside the vector's body before the vector dies.[15] The required threshold of temperature is higher, the higher the standard deviation of monthly temperature, because, after cold winter, the population of parasites and vectors needs to be quickly regenerated to the level sufficient for malaria transmission.[16]

Malaria zones in Africa Climatological conditions play a major role for prevalence of malaria. In some areas, malaria is *endemic*, meaning that the risk of malaria is permanently high, or at least a good part of every year. In other *epidemic* areas, malaria spells are more short lived. This can be either because the disease is seasonal, i.e., it recurs in a particular season due to stable variations in rainfall and temperature, or because it is unstable, i.e., it is present in some years but not in others. Finally, in *non-malarious* areas, the climate is too dry or too cold for malaria to be present or infectious at all.

As already mentioned, we expect a larger effect on infant mortality of seasonal weather shocks in epidemic areas, due to lower immunity rates and more severe malaria infections in such regions. To be able to test this hypothesis empirically, we divide the DHS clusters (gridpoints) into three different malaria zones. Non-malarious zones have no single malarious month, as defined by the malaria index $Z_{g,\tau}$, over the entire sample period; epidemic areas have strictly positive malaria exposure between 0 and 4 months on av-

[15] The vector obtains a parasite by biting a malaria-infected human being. However, it takes a while for the parasite to be infectious and thus for the vector to transmit malaria by biting another human being. Higher temperature shortens the time required for the parasite to become infectious and helps the vector survive long enough.

[16] The definition for our binary malaria index is slightly different from that in Tanser et al (2003). Since these authors have only monthly data, they apply condition (c) to monthly rather than daily temperature, while we use the latter. Unlike Tanser at al (2003), we also treat a non-malarious month according to the four conditions as still suitable for malaria transmission if it is sandwiched by two malarious months. We also tried to implement the index in exactly the same way as Tanser et al, and found somwhat weaker results. By dropping separately each of the four conditions, we found conditions (a) and (d) to be the most relevant ones to predict infant deaths.

erage; while endemic areas have higher exposure rates. We have also set the epidemic-endemic split at 6, rather than 4, months with similar results.

Our classification is illustrated in Figure 3. Non-malarious areas (green circles on the map) entail about 20% of the one million births in our sample and are found in the very North and South of Africa, and in mountain tracts (which are too cold), and in desert or near-desert regions (which are too dry). The remaining 80% of births are split almost equally between endemic and epidemic areas. The epidemic areas (yellow) are mainly found in the Sahel, in higher terrain in East Africa, and in dry areas of the South, while the endemic areas (red) are mainly found in the tropical parts of Africa with stable warm and rainy conditions throughout the year. The geographical distribution of these three zones, based on weather conditions alone, corresponds reasonably well to the distribution of actual cases of parasite infection in malaria maps, based on cross-sectional clinical observations (see e.g., Snow et al, 1999).

Malaria exposure for individual pregnancies As mentioned above, we focus on the malaria conditions during pregnancy. For each childbirth in our sample, we thus create a measure of maternal malaria exposure of the 12 months up to the birth month. Specifically, for a child born in a cluster within weather grid g and in running month t, we define

$$z_{g,t} = \sum_{\tau=t-11}^{t} Z_{g,\tau} \ . \tag{1}$$

In words, we gauge during how many months in the year before birth the child's mother was exposed to malarious weather conditions. This measure varies substantially across areas and time. In endemic areas, mothers are on average exposed to 8.0 months of malarious conditions, with a standard deviation of 1.0 months. In epidemic areas, the corresponding numbers are 1.7 and 1.0 months. Mean-adjusted variability is thus much higher in epidemic areas. (see Table 1, Panel B for summary statistics).

Basic econometric specification and results In our most basic econometric specification, we estimate panel regressions of the following type:

$$m_{i,c,t} = \beta z_{g,t} + \alpha_{c,s} + \alpha_y + \varepsilon_{i,c,t} \tag{2}$$

The dependent variable, $m_{i,c,t}$, is a binary infant mortality indicator. It indicates if a child dies at the age of 12 months or less, for each child i born

in cluster c (within grid location g) and in running month t (calender month s of year y).[17] We multiply this indicator by 1000 so that our results square with the conventional way of measuring infant mortality (i.e., in births per 1000).

On the right-hand side, our parameter of interest is β, which measures how many more children per 1,000 dies in the first year of life due to one extra month suitable for malaria transmission during the 12 months leading up to the birth. Further,.$\alpha_{c,s}$ is a *fixed effect* for cluster c and calendar month $s = 1, .., 12$. That is, when we run this regression in the full sample, we control for 12 monthly means in each of our 18,381 DHS clusters, making for over 220,000 fixed effects. This way, we are identifying parameter β from the deviation within each cluster from its site-specific monthly mean. To allow for a non-parametrically declining trend of infant mortality for Africa as a whole, in line with actual observation, α_y is a fixed effect for calendar year $y = 1957, ..., 2002$. Finally, $\varepsilon_{i,c,t}$ is an error term. We compute Huber-White robust standard errors, allowing for clustering at the grid level (encompassing 749 grid points in the full sample).

The result of running this regression in the full sample is displayed as column 1 of Table 2. Our estimate of β is just below one and quite precisely estimated (significantly different from zero at the 5% level). Mothers with three months higher malaria exposure than normal raises infant mortality in the average cluster by about 2.5 per thousand (close to the total infant mortality rate for Sweden).

However, this basic specification assumes a treatment effect of malaria shocks that is homogenous across the whole sample – a very strong assumption. To test our prior of a larger effect in epidemic areas, we split the sample into its endemic and epidemic part, dropping from the sample non-malarious areas which by definition have no variation in the malaria index (from zero). The corresponding estimates are shown in columns 2 and 3. As expected, the effect in epidemic areas is about twice as large and more precise than in the full sample, while the effect of temporary malaria shocks is close to zero and insignificant in endemic areas.[18] Note that this does *not* mean that malaria

[17] The standard definition of infant mortality is death *before* turning the age of 12 months. The distribution of age at death in the DHS data, however, has a heap at 12 months, suggesting some of the babies who died before turning 12 months old are reported to die exactly at the age of 12 months. Using the standard definition of infant mortality, we find somewhat smaller impacts of weather fluctuations.

[18] The result is similar if the bounday between epidemic and endemic is instead drawn

is not a large risk factor for infant death in endemic areas. Our identification of the effect hinges on the deviation from the average seasonal pattern of malaria transmission. Year-to-year variation in seasonal malaria transmission for endemic areas is not very large, and the bulk of malaria-induces infant deaths are likely absorbed by the cluster-month fixed effects.

In the rest of the table, we show the results of some robustness analysis for epidemic areas. Clustering of the standard errors at the grid level allows for arbitrary serial correlation of infant mortality and weather at each grid-point. While such local serial correlation certainly exists, weather and the survival of babies are likely to be correlated across space and also between a particular area in a certain month and its neighboring areas in the following months. To allow for simultaneous spatial and temporal correlations, we try an alternative clustering scheme by 5 year-period and average malaria exposure (specifically, we split epidemic areas in those above and below 2 months of exposure north and south of the equator, respectively), giving a total of 36 clusters. As column 4 shows, this yields standard errors which are similar to those with grid-level clustering.

One can think of many reasons – such as dependence on health systems, policies, or economic trends – why infant mortality might have *national* but irregular trends. Such national trends could conceivably be related to local weather realizations.[19] Column 5 attempts to control for this possibility by allowing for non-parametric trends for each country, estimating the basic specification for epidemic areas where the year effects are replaced with country-by year effects. While the basic estimate of the treatment effect falls a bit, it is still quite precisely estimated.

Finally, column 6 shows the results of an alternative empirical strategy, where the cluster-by-month fixed effects are replaced by indicators for each mother. Identification is thus obtained from variation in malaria exposure across different births by the same mother. The estimate of β in this demanding specification is similar to its value in the previous column.

Non-linear effects in epidemic areas? All specifications in Table 2 assume that the impact of malaria shocks is linear in the number of months

at 6 months average exposure.

[19] For example, Kudamatsu (2010) finds democratization has reduced infant mortality in sub-Saharan Africa while Bruckner and Ciccone (2009) find negative rainfall shocks led to democratization in Africa.

of malaria exposure. Table 3 shows estimates that relax the functional-form assumptions. We first disaggregate the epidemic area into two subgroups, above and below 2 months of average exposure per year. This further classification is illustrated in Figure 4. Based on an immunity argument, one could presume that weather shocks increasing the susceptibility to malaria may have their most pronounced effect where malaria occurs the most rarely. Columns 1 and 2 of Table 3 display the results when the linear specification in (2) is estimated on the two separate epidemic subsamples. As the estimates show, however, the linear model produces nearly identical estimates in the two samples.

To get further, we allow for a more general non-linear response within each subsample, by allowing for five bins of malaria exposure, setting the omitted default bin at average exposure. In column 3, we look at the 0-2 month exposure sample (yellow regions in Figure 4). The sign and size pattern of the point estimates is exactly what one might expect, with exposure above (below) the average associated with a positive (negative) point estimate, even though the estimates are quite noisy. The most striking finding is the comparison of those pregnancies that have 6 or more vs. 0 (or 1-2) months of malaria exposure. A temporary weather pattern exposing a set of mothers to a potential six-month malaria epidemic raises infant mortality by about 30 per 1000, compared to a control group of pregnancies with no or little exposure at all. This is a huge effect, given an average infant mortality rate of about 100 per 1000 in the sample. Interestingly, the magnitude of the impact is comparable to the size of the impact of the distribution of insecticide-treated bed nets on the reduction in infant mortality, which is 31 per 1000 according to a randomized controlled trial conducted in endemic areas of western Kenya by Phillips-Howard et al. (2003).

In column 4, we show analogous estimates for DHS clusters with 2-4 months average exposure (orange regions in Figure 4). The sign pattern is similar to that in column 3: zero or very little exposure is associated with much lower infant mortality rates than really high exposure, even though the difference between the highest and lowest bin is only about half that in the 0-2 month sample.

We have also tried to distinguish areas with seasonal and unstable malaria (based on the standard deviation of the number of annual malaria months), within the epidemic sample. But this has not produced any stark differences in the estimated effect of malaria shocks.

The findings on non-linear effects are potentially very important for the

consequences of future climate change. Projections of future climate indicate that Africa will get significantly warmer.[20] This means that areas which are hitherto non-malarious due to cold temperature (mountainous regions in Eastern and Southern Africa – areas in Ethiopia, Kenya, Madagascar, and Zimbabwe close to the yellow dots in Figure 4) are likely to become new marginal epidemic areas, where populations will have little immunity and be very vulnerable to temporary malaria epidemics.

Heterogeneity by individual characteristics Following the medical literature discussed at the beginning of this section, we have also investigated if the impact of maternal malaria exposure is heterogeneous across different types of babies, mothers, or households. In particular, we have estimated extensions of our basic econometric specification in equation (2), where all right-hand side variables are interacted with indicators for female babies, firstborn babies, young mothers (under 18), stunted mothers (2 standard deviations below the median stature of the WHO Child Growth Standard by WHO Multicenter Growth Reference Study Group 2006), and households living in regions with high HIV prevalence rate (10 % or higher according to the DHS HIV test results conducted in the 2000s). We have also investigated the heterogeneous impact by the education level of the household (whether both the baby's mother and her husband went to school for more than 8 years) and by affluence of the household (owning a majority of the consumer durables listed in the survey questionnaire). In these regressions, we always split the sample between endemic and epidemic areas. However, we find no significant patterns of heterogeneity in the data, while we always continue to find a significant effect of malaria shocks in epidemic areas but no such effects in endemic areas. This lack of heterogeneity across mothers and households is a bit surprising given the clinical evidence cited above.[21]

Heterogeneity by timing of malaria shocks Our findings above relate to the number of malaria months during the entire year preceding each birth.

[20] By the end of the 21st century, the average temperature in Africa is predicted to go up by 2.6 to 5.3 degrees Celsius (Tanser et al. 2003, Table 2).

[21] Our failure to find heterogeneous impacts of malaria in pregnancy across parities in endemic areas, however, is consistent with Guyatt and Snow (2001), who report malaria in pregnancy doubles the risk of low birthweight across all parities as well as for first pregnancies. Mutabingwa et al. (2005) find infants born to women with malaria-infected placenta are susceptible to malaria infection even if they are of higher birth order.

It is also of interest to gauge whether and how the exposure to malaria shocks in different parts of pregnancy might matter. To this end, we split up the malaria-exposure index $z_{g,t}$ above into four parts – one for each trimester of pregnancy, and one for the quarter just before conception. We then estimate a regression analogous to our basic formulation for epidemic areas, in column 3 of Table 2, except that the months of exposure in each of the four quarters preceding birth enter as separate regressors.

Figure 5 plots the estimated coefficients and their 95% confidence interval. The message is pretty clear: an additional month with malarious conditions in the quarter before pregnancy, or the first trimester of pregnancy, is associated with a substantial increase in infant mortality. But there is no such effect for malaria shocks in the second or third trimester. Each of the two significant coefficients has a value around 3, about double the estimate in Table 2. Quantitatively, this means that a six-month spell of malarious conditions (relative to the complete absence of malarious weather) in the two critical quarters raises infant mortality by about 20 per 1000.

These findings are interesting in that empidemiological research has produced few findings on the effects of malaria in the early part of pregnancy (Desai et al, 2007). However, the findings appear to be consistent with clinical studies showing that malaria infection by pregnant women tends to peak at the end of the first trimester (Brabin, 1983).

Malaria shocks in the first year of life For each child, we have focused on malaria shocks during the year before birth and we have seen that these shocks in utero have a significant effect on the likelihood of survival. Do malaria shocks after birth affect the probability that a child dies before age one, directly or indirectly through the health of the mother? To analyze this question, we have run regressions where malaria exposure during the first year of life – either month by month or the cumulated number of months with a positive malaria index – is added as its own term and as its interaction term with $z_{g,t}$ to the right hand side of equation (2).[22] We find no significant effects on infant mortality of in-life shocks neither in epidemic areas, nor in endemic

[22] Malaria exposure in the n-th month after birth does not affect the survival of babies who died before turning n months. Therfore, including the 12-month exposure to malaria infection during the first year of life as a regressor to the whole sample will bias the estimated effects towards zero. To deal with this problem, for each n from 1 to 12, we restrict the sample to babies who survive at least the first n months after birth and use how many months are malarious during the n months after birth as a regressor.

areas. On the other hand, in-utero malaria exposure continues to exercise a significant effect on infant death in epidemic areas of similar magnitude as in our earlier estimates.

Summary To summarize, we find that weather shocks which raise exposure to malaria, as measured by the Tanser et al (2003) malaria index, significantly raise the incidence of infant death. The biggest effects arise from exposure for six months or more in areas where malarious conditions are otherwise rare, and from exposure just before conception or in the first trimester of pregnancy.

4 Nutrition

In this section, we continue to explore how past weather events (in our ERA-40 data) impact on infant death (in our DHS data). But here, we focus on the prospective mechanism through malnutriton. Following a brief literature review, we discuss how to measure weather-induced crop-yield fluctuations in agricultural societies highly dependent on rainfall during a limited growing season, and how to partition the African continent into different climate zones. Next, we describe our measure of weather conditions conducive to more or less malnutrition during a child's period in utero. Our econometric estimates show that simple measures of rainfall during the growing season(s) tied to each childbirth are insignificantly related to infant mortality. On the other hand, we find a large effect of extreme events in the form of droughts (but not of floods), in Africa's arid climate zone. When we allow for heterogenous effects for different types of households, we uncover a significant effect of rainfall among agricultural households; we also find drought effects to be weaker in households dependent on agriculture and in well-educated households. Finally, the data suggest that babies born in the hungry season – the time just after the start of the rains – are particularly sensitive to malnutrition in utero.

Infant mortality and malnutrition Maternal and child malnutrition poses a major risk for child health, particularly in poor countries – for a recent review see Black et al. (2008). Because maternal intake is the sole source for fetal energy requirements, a lack of food during pregnancy negatively affects the growth of fetus in utero due to deficiencies of calories and

important micro-nutrients. As a result, maternal malnutrition increases the risk of low birth weight, which in turn raises the risk of infant death through birth asphyxia and infections (McCormick, 1985, Black et al., 2008). The medical literature finds that low weight gain during pregnancy increases the chance of low birth weight (Kramer 1987 for a review). This effect is found to be stronger for women whose nutritional status is already poor before pregnancy (Krasovec and Anderson, 1991 for a review) and during the second and third trimesters (Strauss and Dietz, 1999).

Most African children are breast-fed during the period after birth, which is known to lower mortality risk compared to children who obtain non-breast milk liquid or solid food during the first six months of life (see e.g., Black et al. 2008, Table 4)[23] Consequently, and in analogy to the previous section on malaria, we will not focus on variations in food supply after birth, but rather on weather-induced variations in the risk of maternal malnutrition during pregnancy and their subsequent effects on infant survival.

Crop yield and growing seasons Most African countries are agricultural economies – in 2004, some 55% of people on the continent are employed in agriculture (Frenken 2005, Table 2), and many more depend on agriculture in other ways. In addition, transportation infrastructure in Africa is poorly developed.[24] Most people are thus largely dependent on the local yields of subsistence crops for nutritional intake (or of cash crops for earning income to buy foods). Moreover, irrigation of land plays a minor role in crop production, especially in Sub-Saharan Africa – only 6.4% of cultivated land was irrigated in 2004 (Frenken, 2005, Table 12). These stylized facts about Africa suggest that maternal nutritional intake largely depends on local rainfall.

While predictably rich throughout the year in tropical Africa, rainfall in many arid and semi-arid areas is much more erratic. General African rainfall patterns are largely governed by the so-called Inter Tropical Convergence Zone (ITCZ), in which the trade winds from the northeast and the southeast converge (Griffiths, 1972). As a result of the low pressures along the ITCZ, convectional thunderstorms form daily and dump large amounts of precipi-

[23] One might think that food availability after the birth of a child is important for his or her mother to produce breast milk. However, as long as it is not very severe, maternal malnutrition is known to have little impact on the volume and composition of breast milk (see Brown and Dewey, 1992 for a review).

[24] Herbst (2000, Table 5.3) reports that the road density for the median African country around the year of 1997 is merely 0.07 kilometers per square kilometers of land.

tation in scattered afternoon rains. Over land, the ITCZ moves north and south with the seasons, following the hottest part of the continent, which causes large variations in rainfall between dry and wet periods in a typical year. This is illustrated in Figure 6, which shows the average amount of rainfall across Africa in four different months. On top of this regular seasonal cycle, however, one finds considerable fluctuations across years in the precise timing and amount of rainfall.

In many non-tropical areas of Africa, crop yields are thus crucially dependent on the seasonal rains falling in the *growing season* – i.e., the rainy period of the year. We therefore use the total amount of rainfall during the growing season as a proxy for the amount of nutrition available for pregnant women in the analysis below. We have also experimented with various measures of temperature during the growing season, but with little success.[25]

The literature on agriculture and rural poverty in sub-Saharan Africa and elsewhere in the developing world stresses the concept of the "hungry season" just after the start of the annual rains, when food stocks from the previous harvest are on the decline at the same time as the calorie expenditures are peaking due to extensive agricultural work (see e.g., the contributions in Chambers, Longhurst and Pacey, 1981 and in Sahn, 1989). Low birth weights are found to be more likely to happen during rainy seasons than during dry seasons (Bantje, 1983 and Kinabo, 1993 for Tanzania, Fallis and Hilditch, 1989 for Zaire). This suggests that annual fluctuations in weather-induced nutritional availability may have heterogeneous impacts on infant survival across which season babies are born.

How determine the growing season? The growing season in a particular location is likely to depend on many other factors than the extent of rainfall, including soil qualities, crop types and the use of fertilizers. While some gridded information on these other factors exists, we take a convenient short cut to determine the relevant growing season for each of our DHS clusters, by employing the data measuring photosynthetic activity from remote sensing by satellite.

[25] We are certainly not the first to use growing season rainfall as a proxy for crop yields. Lobell et al. (2008) use the growing season rainfall (and temperature) to predict crop yields in developing countries under the future climate change scenarios. Deschenes and Greenstone (2007a) also use growing season rainfall to predict agricultural profits in the United States.

Photosynthesis is identifiable from a long distance, because growing plants reflect light at the infrared part of the spectrum and absorb light at the near-red part of the spectrum. Therefore, ecologists often use data collected by satellites to measure plant growth through ongoing photosynthesis. We use such satellite data made available through the Global Inventory of Modeling and Mapping Studies or GIMMS (Tucker et al. 2005), namely the so-called normalized difference vegetation index (NDVI). The NDVI index is globally available as bi-weekly series from 1982 and onwards on a resolution of 8×8 kilometers. In the ecology and biology literature, the integral of NDVI values over the growing season is often used as a proxy for crop yields (e.g. Rasmussen 1992 for millet yields in Burkina Faso, Rasmussen 1997 and 1998 for millet yields in Senegal). The map in Figure 7 shows the distribution of the average annual integrated NDVI across Africa, with bluer areas denoting areas with a low value – little photosynthetic activity over the year – and redder areas a high value. The two graphs in Figure 7 plot observed NDVI values as the jagged thin curves over two years, 1982 and 1983, in two locations: one in Burkina Faso just at the boundary to Niger, and one in Tanzania just south of the Victoria Lake. In these graphs, the horizontal axis shows time measured in two-week periods; the vertical axis shows the NDVI value (multiplied by 10,000). Clearly, the peaks are much lower (note the different scales) for the Burkina Faso location than the Tanzania location, reflecting a lower amount of rainfall.

To obtain the growing season from this time-series NDVI data, we use the TIMESAT program (Jonsson and Eklundh, 2004).[26] The two graphs in Figure 7 demonstrate how this program works. The TIMESAT program first produces smoothed (filtered) values of NDVI (shown as the thick curve in the graphs), where the smoothing is meant to eliminate temporary random fluctuations, for example, due to variations in cloud cover. Following the common practice among ecologists (e.g. Heumann et al., 2007), the program then produces the times for the start and the end of the growing season defined as the time period in between 20% above one trough to 20 % above the next (see the points on the smooth curves in Figure 7). Notice that the duration of the growing season is much shorter in Burkina Faso than in Tanzania. Finally, to deal with the potential endogeneity of the observed annual growing seasons, we average the start and end dates over the 25 years

[26] We are grateful to Lars Eklundh, Department of Earth and Ecosystem Sciences, Lund University) for his assistance with this program and the data.

available for each location, and use the calendar months between these two average dates as our measure of the *fixed* growing season.[27]

Climate zones Because the seasonality of weather and agriculture differs so much, crop types, cultivating practices, and lifestyles have most likely adapted to the local conditions in different parts of Africa. We would therefore like to allow the effects of weather on nutrition and health to depend on the prevailing climate. A straightforward way of making such conditioning operational is to follow the approach originating with German climatologist Wladimir Köppen, who was the first to classify different areas on Earth into different climate zones.

The well-known Köppen climate classification system distinguishes between different climate types based on annual and monthly temperature and precipitation, as well as the seasonality of precipitation (see, e.g., Peel et al., 2007 for more details). Using the Köppen classification criteria and our ERA-40 weather data, we subdivide all the DHS clusters in our sample into two climate zones: *rainy* areas, which include rainforest, monsoon, savannah and temperate climates, and *arid* areas, which include steppe and desert climates. The resulting classification of our DHS clusters is shown in Figure 8.

Malnutrition exposure for individual pregnancies We want to determine how weather affects each mother's nutritional intake for the 12 months before her child is born. When doing so, we focus on effects through local crop yields driven by variations in rainfall during the growing seasons, as summarized by a simple *rainfall exposure index*.

Which are the relevant growing season(s) depends on the timing of an individual birth relative to local harvest time. As an example, suppose a child is born in September 2000, one month after the last harvest in this location (August 2000). Then the mother has consumed food for one month from that harvest and for eleven months from last year's harvest. In general, the mother's nutritional intake during the year before giving birth depends on the two last harvests. We weight these by the number of months the mother had the ability to consume from each harvest. In the example, our

[27] In areas where there are two growing seasons per year, we use every odd growing season in our calculation of the fixed growing season.

rainfall exposure index weights rainfall during the growing seasons of 2000 and 1999 by the weights 1/12 and 11/12, respectively.

To be more precise, consider baby i born in location g, and running month t. Let r_1^i and r_2^i be the total rainfall during the two growing seasons preceding the birth of child i. Further, let h^i be the running month of the last harvest preceding the birth of child i, with corresponding total rainfall r_1^i We then proxy the nutritional dependence on weather by the index:

$$r_{g,t} = \omega_{g,t} r_1^i + (1 - \omega_{g,t})\, r_2^i \ , \tag{3}$$

where weight $\omega_{g,t}$ is given by $\omega_{g,t} = \frac{t-h^i}{12}$.

Our rainfall exposure index reflects a few simplifying assumptions that we wish to highlight:

(a) *all crop yield in location g becomes available at the final month of the growing season (call the harvest month), and this is the same calendar month every year*,

(b) *the marginal effect of weather variation on nutritional intake is constant across the year of exposure,*

(c) *crop yield harvested in month h^i and $h^i - 12$ depends directly on the cumulated rainfalls, r_1^i, r_2^i during the growing seasons that ended in those months.*

We investigate deviations from these assumptions in the empirical part of this section. In particular, violating assumption (b), the marginal impact of harvested crop yields on children' health may be larger during the hungry season discussed above, or during particular trimesters.

The mean rainfall exposure index in the sample is 712.3 mm of rainfall, while the average grid-level standard deviation is 190.7 mm. The corresponding statistics for the rainy climate zone sample are 1261.5 and 284.2 while they are 169.0 and 58.1 for the arid climate zone. Mean-adjusted variability is thus much larger for the arid climate zone (see Table 1, Panel C for summary statistics).

Basic results In Table 4, we report estimates from running panel regressions with specifications like (2) in Section 3, except that we replace the malaria exposure index $z_{g,t}$ with the rainfall exposure index $r_{g,t}$. Column 1 shows the estimate of the coefficient of interest in the full sample, with cluster–by-month and year fixed effects included and standard errors clustered by grid. The coefficient has the expected negative sign – i.e., more

rainfall in the growing seasons before birth cuts the risk of infant mortality – but it is not significantly different from zero. Column 2 shows that the result is similar, although the point estimate is lower in absolute value, if we replace the birth-specific weighted average in $r_{g,t}$ with the cumulated rainfall over the 12 months preceding birth with no allowance for the location-specific growing seasons.

Columns 3 and 4 report corresponding estimates when the same specification is estimated on the subsamples of babies born in rainy and arid climate zones, respectively. In rainy areas, the result is the same as in the full sample, namely a point estimate with the negative expected sign, which is not statistically significant. When it comes to arid areas, we obtain a positive and quite precisely estimated coefficient. Quantitatively, this would imply that a weather shock with one standard deviation more rain (about 0.06 meters in the arid sample) would raise infant mortality by 1 per 1000, an effect that is puzzling in its sign albeit not very large.

Non-linear effects: droughts and floods Since infant death is an extreme health outcome, we might think that it is closely related to extreme precipitation events, such as droughts or floods. The linear specifications estimated in Table 4 do not allow for disproportional effects of extreme events, however.

We use a drought measure based on extreme growing season rainfall outcomes. In each grid location, we first compute the average value of our rainfall exposure index, $\overline{r}^g$, as well as its standard deviation, $\sigma^{r,g}$, using the full 45 years of ERA-40 data from 1957 to 2002. We then define a binary indicator variable for babies born in location g and running month t by

$$d_{g,t} = I[r_{g,t} < \overline{r}^g - 2\sigma^{r,g}] \ .$$

That is, the birth is associated with a drought indicator of unity if its rainfall exposure index falls two standard deviations below the local mean. For convenience, we define a flood symmetrically, as an extreme event in the opposite direction.[28]

[28] Our drought measure is similar to the Standardized Precipitation Index (McKee et al., 1993), but is based on our rainfall exposure index rather than just average rainfall. For a discussion of drought indices and their application to Africa, see Ntale and Gan (2003). We have also experimented with other definitions of droughts (and floods), including definitions based on the local percentile distribution of precipitation. We have

Table 5 displays the results from adding the drought and flood indexes defined above to the econometric specification used in Table 4. The full-sample results in column 1 show positive, but statistically insignificant, point estimates for both types of extreme events, as do the results for rainy areas in column 2.

When we go to arid areas in column 3, the results are different. While the rainfall coefficient is positive and significant, as in Table 4, so is the estimated coefficient on drought. We can calculate the net effect of a (marginal) drought in an arid area, by deducting from the estimated drought coefficient (27.8) the estimated rainfall coefficient times two standard deviations from mean rainfall in arid areas (21.9×0.12). The net effect of a drought is thus a powerful one: it raises infant mortality by 25 per 1000, an amount equal to a quarter of the sample mean. But we do not find any effect of extreme positive amounts of rainfall.

The remaining three columns in Table 5 check the robustness of the result in column 3 in an analogous way to columns 4 to 6 in Table 2. Column 4 shows that these estimates for arid areas are robust to clustering the standard errors at climate zones by 5-year periods.[29] In column 5, we replace the non-parametric trend for the arid climate zone as a whole with a set of country-specific nonparametric trends. While the point estimates change a bit – and the linear rainfall effect is no longer positive – the implied net effect of a drought is very nearly the same as in column 3. Finally, column 6 shows estimates when our basic cluster-by-month fixed effects are replaced by fixed effects for each mother. Basically, the drought estimate is robust also in this specification.

Our results suggest that extreme shortfalls of rain have large effects in arid areas, while more piecemeal variations in precipitation do not have any measurable effects on infant mortality, at least not in rainy areas. These results are consistent with Susser (1991), who reviews studies on the relationship between maternal nutrition and birth weight and conclude that nutritional intake by mothers significantly affects birth weight only in famine conditions.

The specifications in Tables 4 and 5 do not allow for any heterogeneous effects across babies, mothers and households (beyond a difference across

also interacted these drought measures with the indicator of vulnerability to drought and flood from Dilley et al. (2005). However, these variables do not significantly affect infant mortality.

[29] The arid areas are divided into 4 zones by northern versus southern hemispheres and by steppe versus desert climate zones defined by the Klöppen climate classification.

climate zones). We now turn to these issues.

Heterogeneity by household characteristics? It is natural to believe that the vulnerability of the offspring to maternal malnutrition might differ with mother or household characteristics, such as occupation, income, or education. Table 6 presents some estimates relevant to this hypothesis. We focus on two specific sources of heterogeneity, which appear important a priori and reasonably measurable in the DHS data at our disposal. One is occupation: we call a baby's household *agricultural*, when parents of this baby earn a living only from agriculture at the time of the survey. In the full sample, about 41.7% of all children are born in agricultural households. Measurement error in the classification of agricultural households is inevitable: parents may have changed the job since the baby's birth, and the definition of agriculture in the DHS data also includes forestry and fishery. These factors, however, would bias our results against finding and effect of heterogeneity.

We also consider education, and define a baby's household as *well-educated* if both the baby's mother and her husband (if relevant) have more than eight years of education. Eight years is chosen as the cutoff because we see a marked drop in the cross-sectional distribution of infant mortality above this level of education. In the DHS sample, only 8.2 percent of the babies were born to well-educated households. The retrospective nature of the survey is unlikely to be a major source of mismeasurment when it comes to education (see Table 1, Panel A for summary statistics by subgroup)

We then run the following regression:

$$\begin{aligned} m_{i,c,t} = \beta r_{g,t} + \beta^x r_{g,t} \cdot x_i + \gamma d_{g,t} + \gamma^x d_{g,t} \cdot x_i \\ + \alpha_{c,s} + \alpha^x_{c,s} \cdot x_i + \alpha_y + \alpha^x_y \cdot x_i + \varepsilon_{i,c,t} \ , \end{aligned}$$

where x_i is an indicator of baby i's household type (agricultural or well-education). Note that this specification allows cluster-by-month and year fixed effects to differ across different household types. Table 6 reports estimated coefficients of β, β^x, γ, and γ^x.

Column 1 shows the estimates for the occupational breakdown in the full sample. In contrast to the aggregate results in Tables 4 and 5, the results suggest that rainfall exerts a significant negative effect on infant mortality if we look at agricultural households. The sum of the two coefficients on rainfall ($\beta + \beta^x$), the total effect of rainfall for agricultural households, is statistically significant at 10 percent level (see the F-test[rainfall] statistic

at the bottom of the table). The non-interacted coefficient (β) shows that the effect is statistically insignificant and close to zero for non-agricultural households. To the contrary, droughts significantly raise infant mortality in non-agricultural households, but not in agricultural households as seen in the insignificant statistics for F-test[drought]. A reasonable interpretation is that normal variations in rainfall tend to make agricultural households better off nutritionally, and that these households have better access to whatever little crop yield there may be at the time of drought, when the main burden is instead borne by non-agricultural households.

The breakdown into rainy and arid subsamples in columns 2 and 3 shows that the rainfall effect on agricultural households is mainly due to the shocks in rainy areas, while the drought effect on non-agricultural households – as Table 5 would suggest – arises only in arid areas.

Columns 4-6 repeats the same exercise for the breakdown of household type by education. The main result here is that, as might be expected, the well-educated appear to be protected from the high infant mortality effects of a drought shock in arid areas, perhaps as a result of higher purchasing power or better opportunities.

As in the malaria section, we have also experimented with conditioning on various baby and mother characteristics (gender, birth order of child, age or stature of mother, etc.), on the notion that some types of babies or mothers may be more vulnerable to malnutrition shocks than others. But this has produced no robust results.

Heterogeneity by timing of birth As mentioned earlier, our simple rainfall and drought exposure indexes rely on the assumption that food from the previous harvest is uniformly available during the following 12 months. We have relaxed this assumption by conditioning the effects of shocks on the time of birth relative to the beginning of the growing season. Given the earlier results, we focus on the drought effects in arid areas and run the regression:

$$m_{i,c,t} = \sum_{k=0}^{3} \beta^k r_{g,t} \cdot q^k_{g,s} + \sum_{k=0}^{3} \gamma^k d_{g,t} \cdot q^k_{g,s} + \alpha_{c,s} + \alpha_y + \varepsilon_{i,c,t} \ ,$$

where $q^k_{g,t}$ is a dummy that equals one if month s falls within the kth quarter since the beginning of the growing season in grid g ($k = 0$ for the quarter immediately before the growing season starts). Figure 9 plots estimated coefficients of the γ^ks and their 95% confidence interval. The vertical line in

the figure indicates the beginning of the growing season – recall Figure 7 and our definition of this start as the time when the NDVI value is 20% above the last trough. The babies born in the quarters around the beginning of the growing season, marked 0 and 1 in Figure 9, seem to fare much worse in the wake of a drought shock than the babies born closer to the harvest. (A formal test that the coefficients plotted in the figure are different is marginally significant: p-value 0.109.) The estimated hike in death rates for these hungry-season babies – on the order of 50 per 1000 births – is a stunning number indeed.

These results are interesting in view of the "hungry season" concept in the literature on food availability and poverty. The average length of the growing season in arid areas is about 6 months in our sample, and the actual harvest may start before the end of the fixed growing season we use in the analysis. Therefore, food is the least available in the period around (in particular, after) the beginning of the growing season. On top of that, the beginning of the growing season is the time when energy expenditure of people, including pregnant women, reaches its peak due to the need for clearing the land and planting seeds.

A study on pregnant women in a Gambian village shows that pregnancy, even in the last month before giving birth, does not reduce the time women spend at their farms (Roberts et al., 1982). Studying the same rural area in Gambia, Rayco-Solon et al. (2005) find that the incidence of premature birth (a major cause of low birth weight) significantly increases during the first few months of the rainy season, which suggests a possible causation from increased amount of workload for pregnant women to low birth weight. Randomized controlled trials in the same area show that the impact of dietary supplements to pregnant women on the incidence of low birth weight and early infant death are both significantly larger for babies born in the hungry season (Ceesay et al., 1997). Our empirical findings suggest that these results from particular villages in Gambia may be applicable to other arid areas of Africa.

Summary Let us briefly summarize. Extreme negative rainfall shocks, droughts, have a powerful effect on infant death in areas with steppe and desert climates. Rainfall above the site-specific seasonal mean in the relevant growing season diminishes infant mortality only for babies born in agricultural households in the rainy parts of Africa. Drought shocks impinge

especially hard on babies to parents that do not work in agriculture and are not well educated, and on babies born around the start of the rains.

5 Malaria and Nutrition

In the two previous sections, we have investigated separately two channels – malaria and malnutrition – whereby local seasonal weather shocks affect infant mortality rates in Africa. We have taken care to define these weather shocks according to the mechanism under investigation, but ultimately all the shock measures emanate from the same weather data. It is thus legitimate to ask if the main results hold up when we allow both types of shocks to occur simultaneously. For example, more rainfall can potentially have two opposite effects on infant mortality: more rain may be good for nutrition but is bad for malaria. For the babies in our sample, the malaria exposure index $z_{g,t}$ is indeed positively correlated with the rainfall exposure index $r_{g,t}$ with a correlation coefficient in the full sample of 0.76.[30]

Table 7 revisit the two main results above: the effect of malaria shocks in epidemic areas and the effect of droughts in arid areas. Comparing the epidemic malaria zone in Figure 3 with the arid climate zone in Figure 8, we see that these zones spatially overlap but not perfectly so. In column 2, we thus reassess the malaria results from column 3 in Table 2, which for convenience are reproduced in column 1. As the estimates show, the malaria result is essentially the same, while we find no significant effects of rainfall exposure in the growing season(s). In column 4, we instead reassess the estimates of drought effect in arid areas, reproduced in column 3. Here, we find a positive and large effect of malaria shocks, 25% higher than in the epidemic areas, while the coefficient on rainfall drops by about 25% of its previous value. This is natural, given the positive correlation of the two measures: in Tables 4 and 5 we were most likely over-estimating the effect of rainfall, falsely attributing some infant deaths caused by malaria to the nutritional channel.

A memento for economists The result in the last part of Table 7 can perhaps also serve as a memento for development economists, who have in-

[30] If we drop duplicated observations (multiple babies are assigned the same values of $z_{g,t}$ and $r_{g,t}$ if they are born in the same area in the same month), we still obtain the correlation coefficient of 0.75.

creasingly relied on research designs in which rainfall is used as an instrument or indicator of income or poverty shocks. Take, for example, Miguel et al. (2004), who use rainfall as an instrument for national income as a determinant of civil conflicts in sub-Saharan Africa. Because malaria is common in many war-ridden states, its dependence on rainfall calls into question the exclusion restriction underlying the IV strategy to the extent that a higher disease burden has a separate effect on conflict, beyond its effects on income. Another example might be the recent study by Maccini and Yang (2009), who use negative rainfall shocks as an indicator of negative early-life nutrition shocks in Indonesia. Since malaria is common in Indonesia, failing to account for the effect of rain on malaria infection might cause the authors to underestimate the effect of early life shocks on adult outcomes.

6 Final Remarks

We believe this paper makes substantive as well as methodological contributions. In terms of substance, we uncover two channels whereby weather might impact on infant mortality in African countries. Weather shocks that raise malaria exposure of pregnant mothers have a large impact on infant death, especially when they strike early in pregnancy and when sow the seeds of a malaria epidemic in areas where malaria is rare. Rainfall shocks in the growing seasons that affect maternal nutrition when the child is in utero only appear to affect babies born to agricultural households in Africa's rainy climate zones. Drought shocks have a pronounced effect on infant death in arid areas, especially for babies whose parents are not well educated or not dependent on agriculture and for babies who are born in the hungry season. The malaria and drought effects we uncover are statistically robust and quantitatively large.

In terms of methodology, we hope to have outlined a possible research design, showing how one may combine very different data sources for large-scale statistical work, when conventional data sources are absent or poor. A similar approach and statistical methodology may be used to study other outcomes of interest in Africa or other regions. For example, further research on Africa could use DHS data to look at the weather dependence of other outcomes, such as child mortality, child health, or fertility. Perhaps one may also look at more complex issues, such as generational spillovers, whereby girls with negative weather shocks in early life become physically or cogni-

tively impaired and thus face a larger risk of bad outcomes when they give birth.

There is certainly scope for improvement on the natural-science side of our measurement. For example, one could try to use reanalysis from *regional* rather than *global* climate models to obtain more recent and fine-gridded weather data, so as to better pick up the spatial distribution of rainfall. As another example, one could try to use structural crop-yield models to get a better handle on the interplay between temperature and rainfall in producing local crop yields.

Finally, our results also have bearing on the analysis of future climate change. Most climate projections suggest that Africa will get a great deal warmer over time, and that its wet areas will get wetter and its dry areas even drier. This would mean that new parts of Africa, such as mountainous regions, will be subject to weather-induced malaria epidemics, and that arid areas will be subject to more frequent droughts. Our results on the exposure to malaria epidemics and droughts give a strong hint that such changes might seriously threaten infant health. However, we do not believe that the right way to tackle this issue is by way of a simple statistical forward projection of the current results. Serious analysis will have to consider mechanisms of adaptation, like migration or better health protection, as the climate changes and income grows over time.

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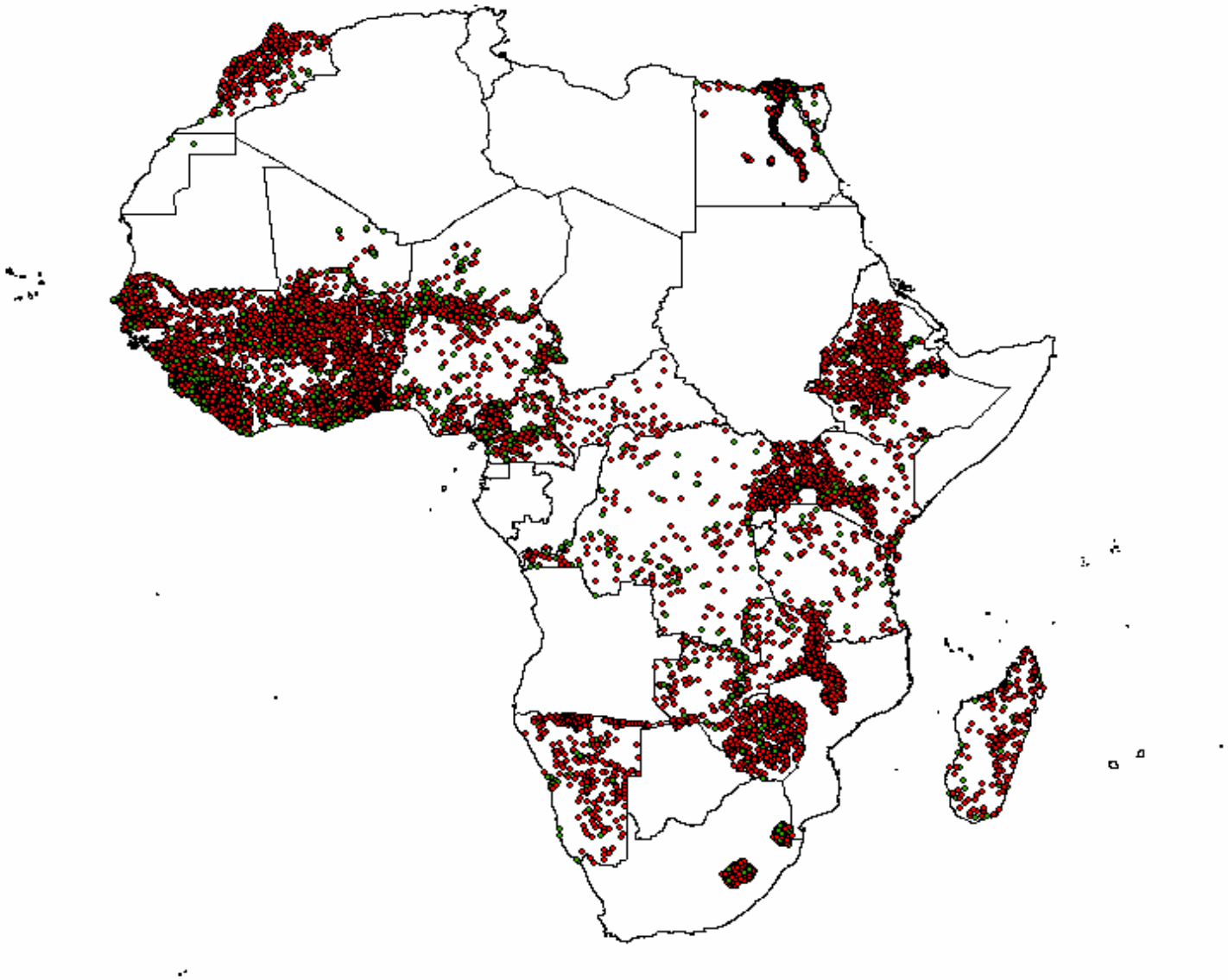

Figure 1 – DHS clusters in sample

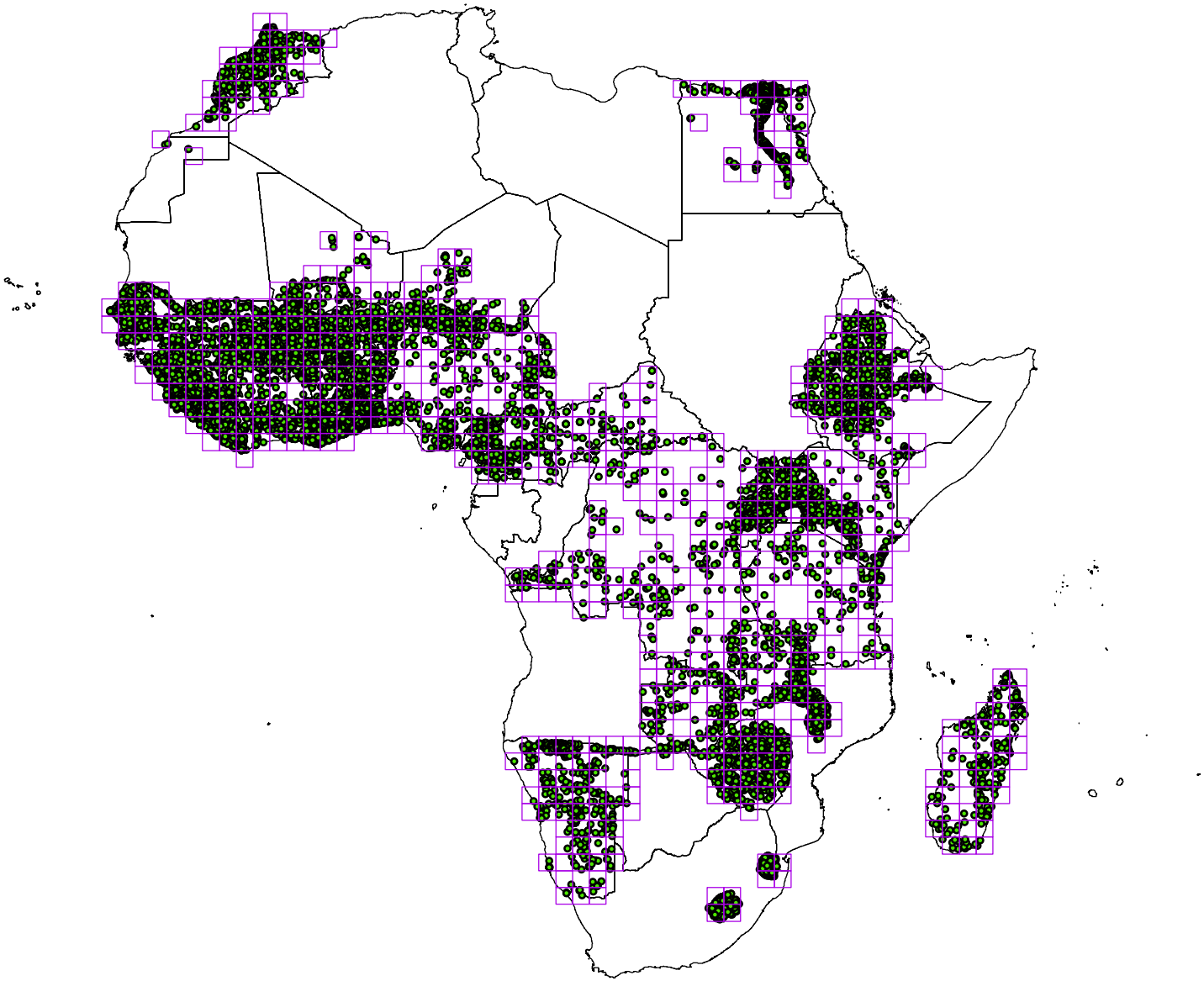

Figure 2 – DHS clusters and ERA-40 grid in sample

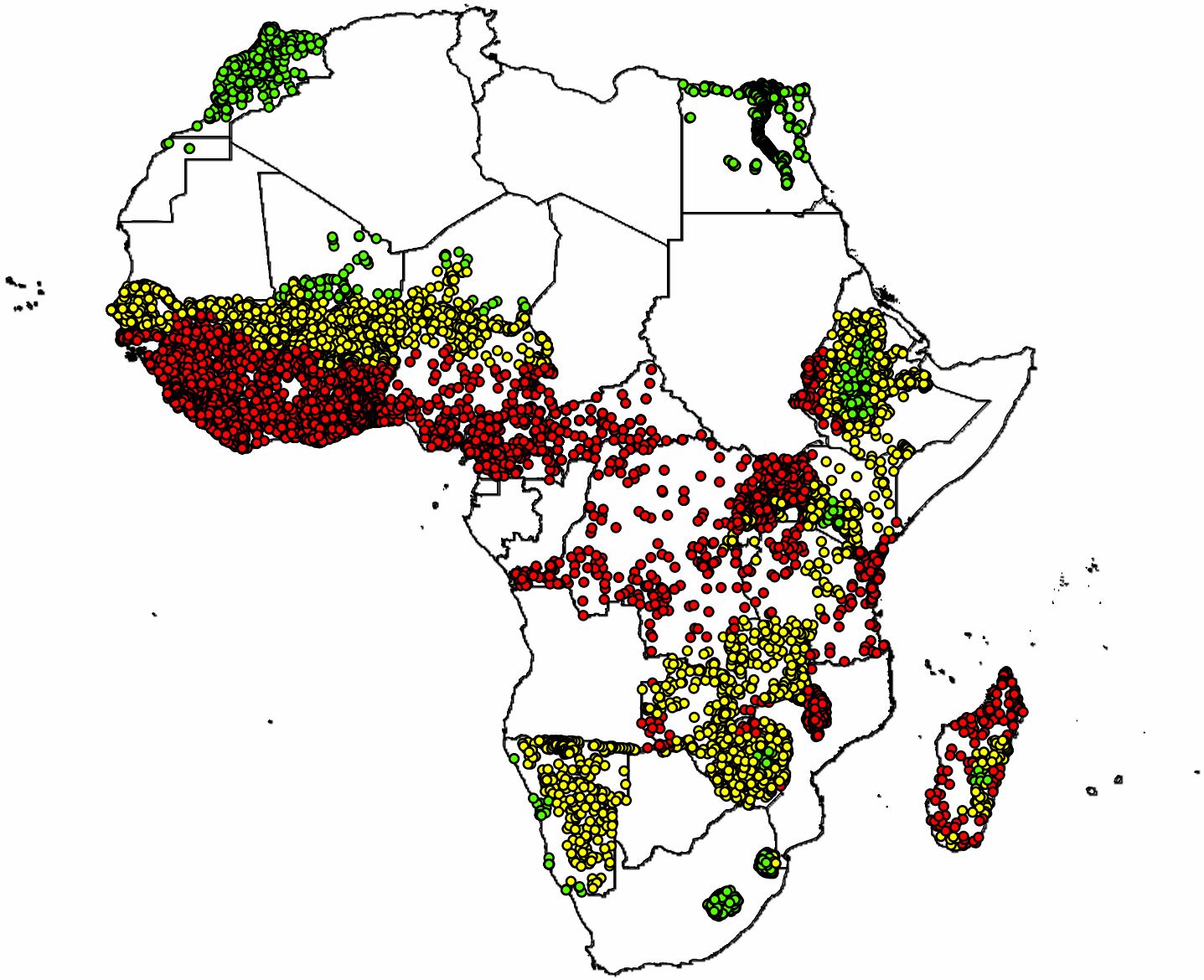

Figure 3 – Malaria exposure zones in Africa

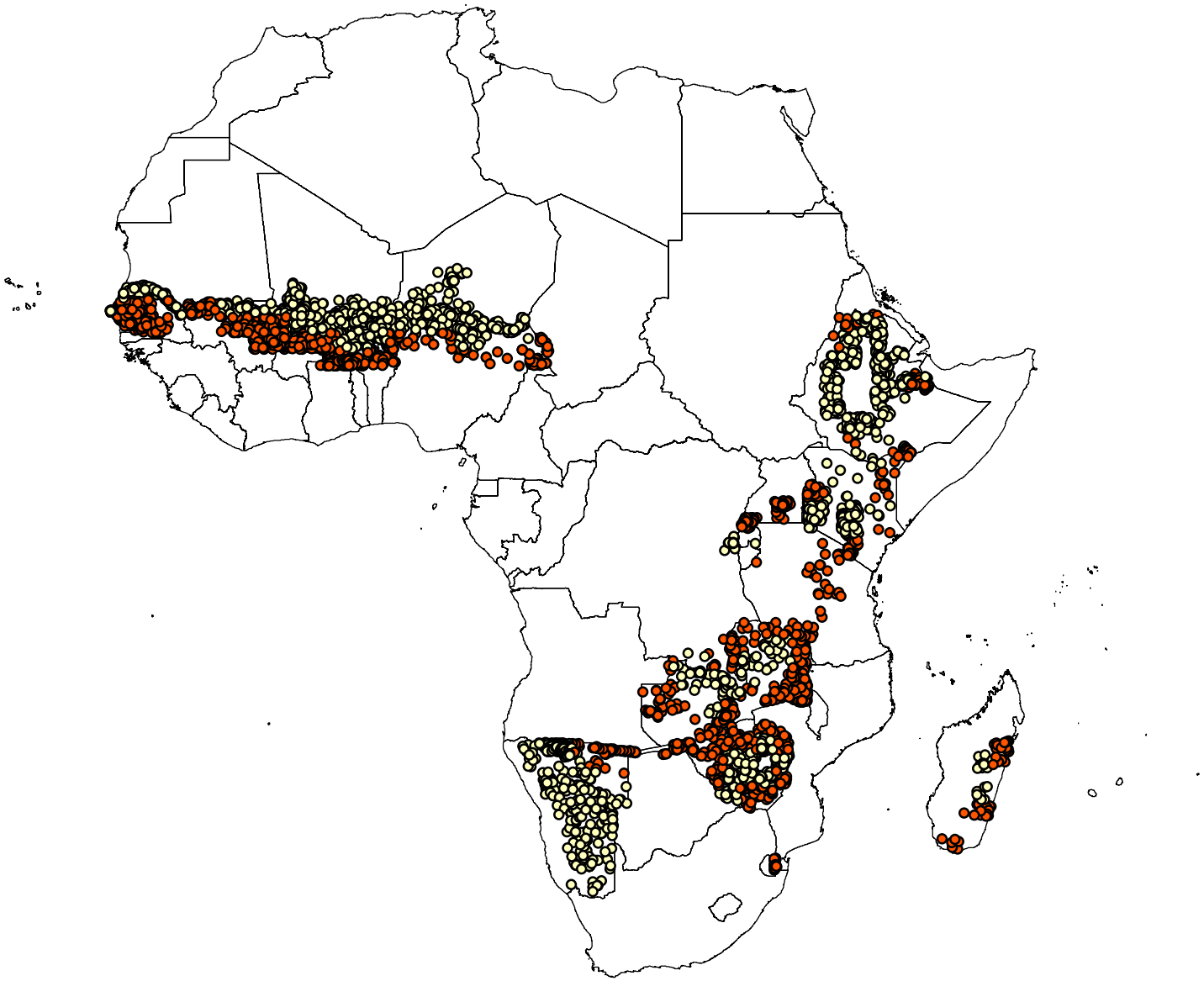

Figure 4 – Low and high epidemic malaria exposure

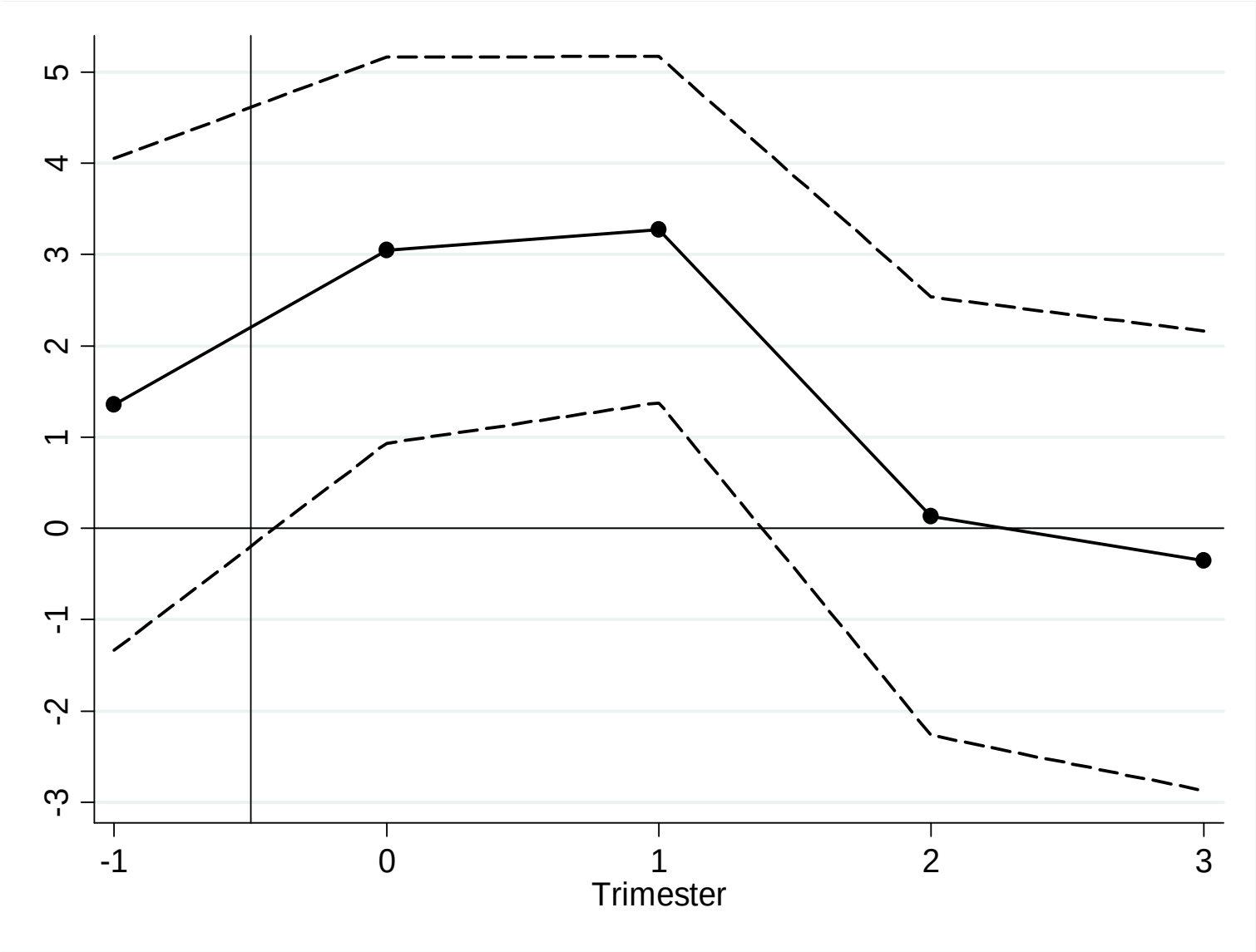

Figure 5 – Infant death and malaria shocks, by trimester

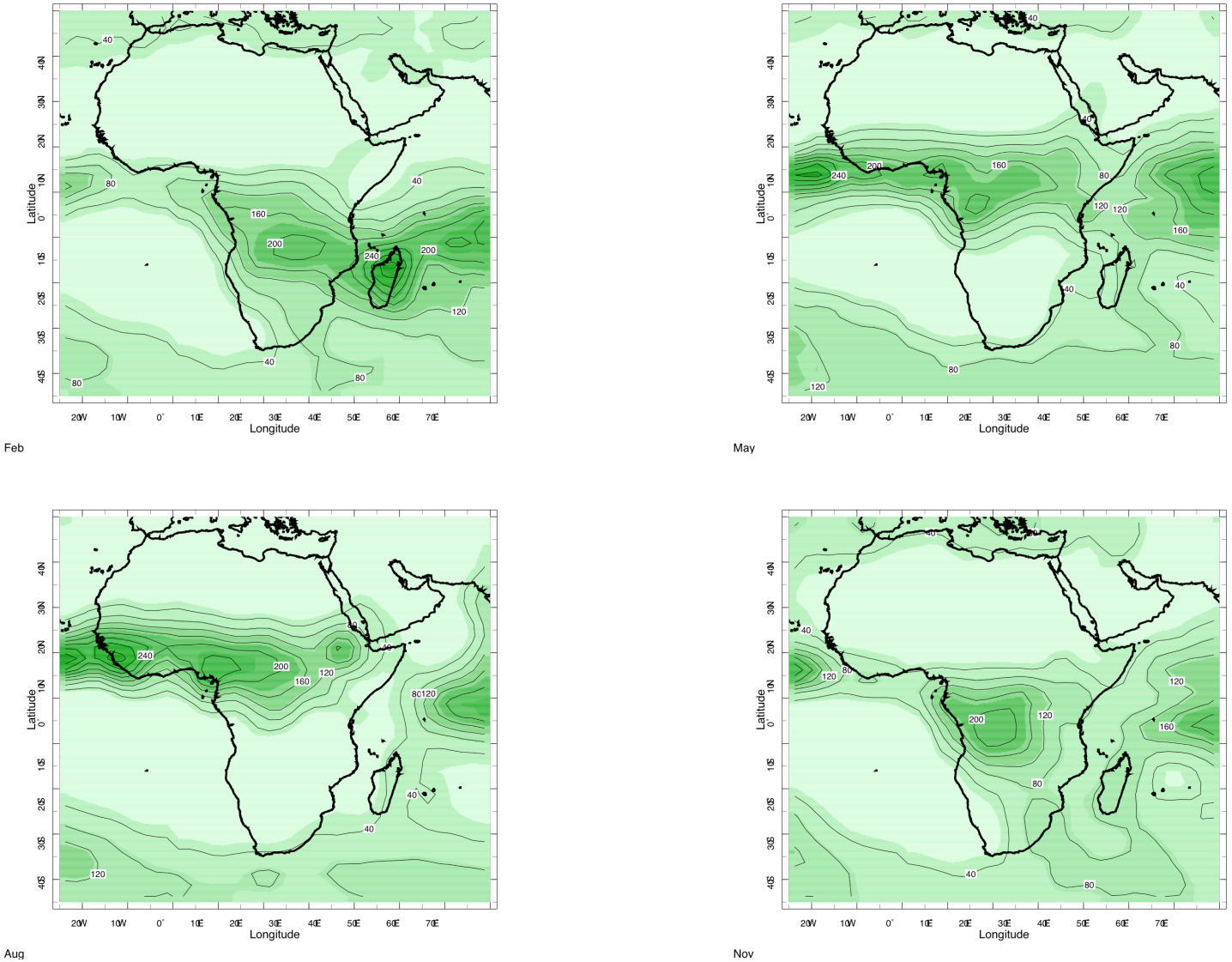

Figure 6 – Total monthly rainfall (in mm) in Africa for February, May, August, and November

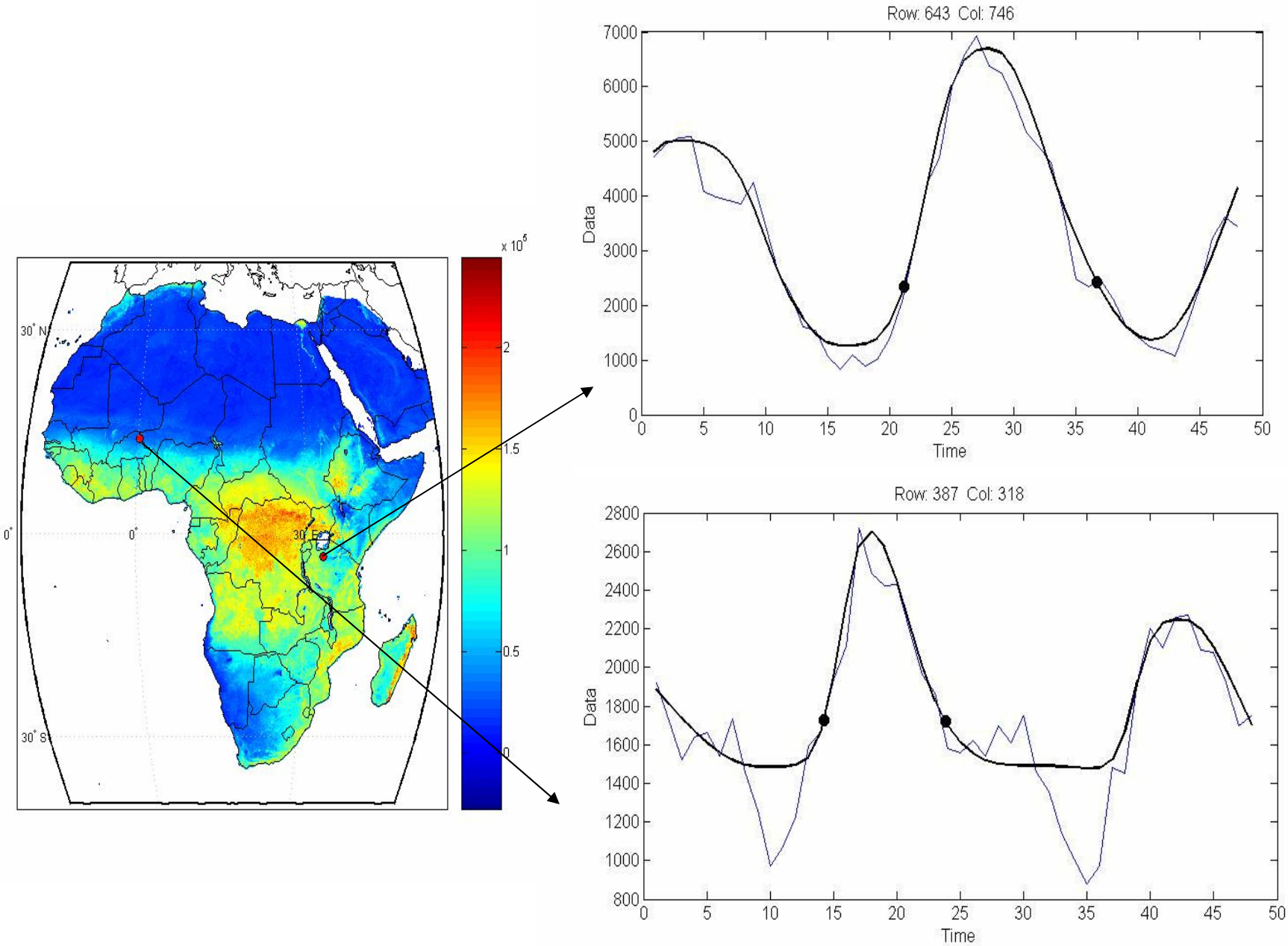

Figure 7 – Actual and fitted NDVI in Burkina Faso and Tanzania

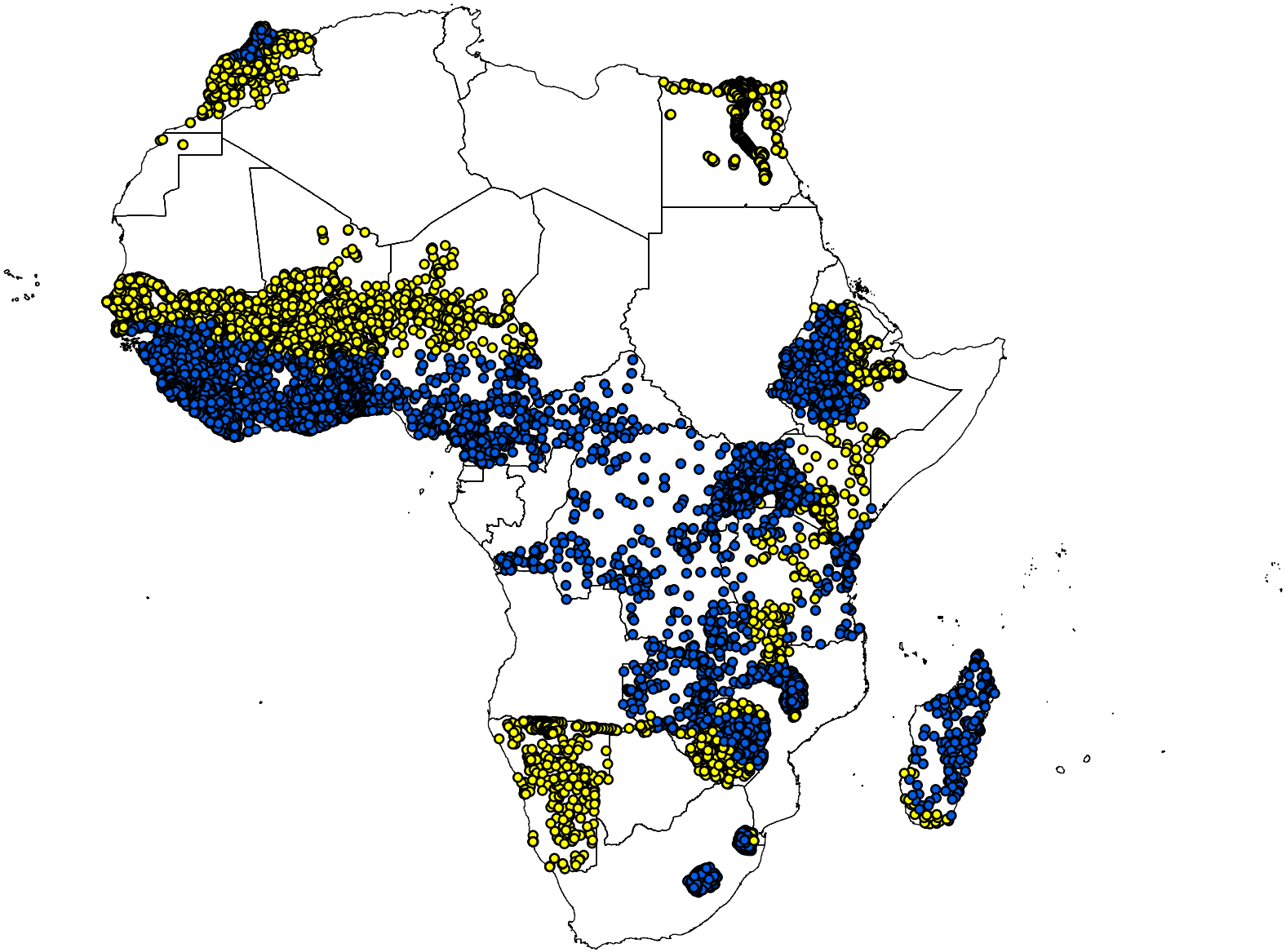

Figure 8 – Arid and rainy climate zones in Africa

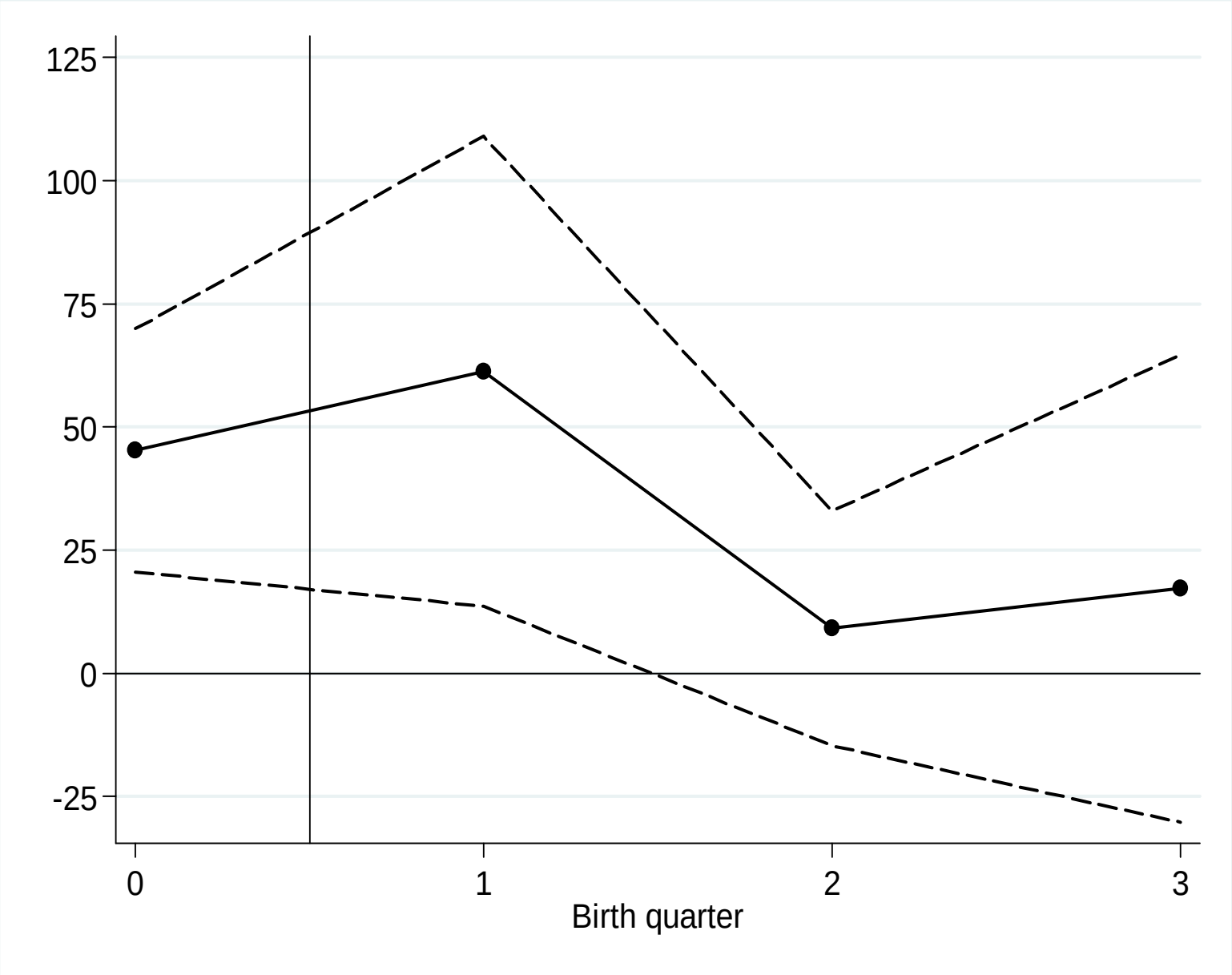

Figure 9 – Infant death and drought, by birth vs start of rains

Table 1 – Summary statistics

Panel A: Infant Mortality per 1000 live births

	Sample mean	S.D. cluster-level means	Number of clusters	Number of observations
Full sample	100.6	69.5	18381	1008249
By area				
Endemic	107.7	73.6	7619	403300
Epidemic	107.1	69.4	6512	406580
Non-malarious	72.7	56.3	4250	198369
Rainy	102.2	71.5	9818	501415
Arid	99.0	66.8	8563	506834
By HH type				
Agricultural	119.5	112.0	12750	403164
Non-agricultural	85.9	92.2	17631	562683
Highly educated	46.0	117.6	9392	82097
Not highly educated	105.6	74.5	18231	922151

Panel B: Malaria Exposure Index (months)

	Sample mean	Mean S.D. within-grid	Number of grids	Number of observations
Endemic	8.0	1.0	365	403300
Epidemic	1.7	1.0	280	406580

Panel C: Nutrition Exposure Index (mm of rainfall)

	Sample mean	Mean S.D. within-grid	Number of grids	Number of observations
Rainy	1261.5	284.2	439	501415
Arid	169.0	58.1	310	506834

Table 2 – Infant mortality and malaria: basic results

Dependent Variable: Infant death (multiplied by 1000)

	(1)	(2)	(3)	(4)	(5)	(6)
Sample	Full	Endemic	Epidemic	Epidemic	Epidemic	Epidemic
Malaria index (months) in year before birth	0.826**	0.144	1.664***	1.664***	1.156**	0.993**
	(0.328)	(0.446)	(0.509)	(0.492)	(0.514)	(0.473)
Fixed Effects	Cluster-month	Cluster-month	Cluster-month	Cluster-month	Cluster-month, country-year	Mother
S.E. clustering	Grid	Grid	Grid	5-year by malaria exposure	Grid	Grid
No. clusters in S.E.	749	365	280	36	280	280
Observations	1008249	403300	406580	406580	406580	406580

Robust standard errors in brackets, clustered as indicated in the table: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All regressions, but column (5), include year fixed effects. In column (4), "malaria exposure" refers to 4 areas with above or below 2 malaria months per year, north and south of the equator, respectively.

Table 3 – Infant mortality and epidemic malaria: non-linear results

Dependent Variable: Infant death (multiplied by 1000)

	(1)	(2)	(3)	(4)
Sample	0-2 months mean malaria	2-4 months mean malaria	0-2 months mean malaria	2-4 months mean malaria
Malaria months in year before birth	1.708*	1.618***		
	(0.934)	(0.592)		
0 malaria months in year before birth			-2.752	-6.174**
			(2.534)	(2.593)
1-2 malaria months in year before birth				-6.428***
				(2.237)
3-4 malaria months in year before birth			2.633	
			(3.349)	
4-6 malaria months in year before birth			12.886	-0.338
			(10.282)	(3.058)
>6 malaria months in year before birth			29.596**	7.015
			(13.829)	(8.475)
No. clusters in S.E.	155	125	155	125
Observations	213470	193110	213470	193110

Robust standard errors, clustered at the grid level, in brackets: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$. All regressions include cluster-month fixed effects and year fixed effects.

Table 4 – Infant mortality and nutrition: linear effects

Dependent Variable: Infant death (multiplied by 1000)

	(1)	(2)	(3)	(4)
Sample	Full	Full	Rainy	Arid
Rainfall (meters) in growing season	-1.986		-2.466	19.406**
	(1.904)		(1.950)	(7.796)
Rainfall (meters) in last 12 months		-1.634		
		(1.240)		
Number of clusters in S.E.	749	749	439	310
Observations	1008249	1008249	501415	506834

All regressions include cluster-month fixed effects and year fixed effects. Standard errors clustered at the grid level reported in parentheses: * significant at 10%, ** 5%, *** 1%.

Table 5 – Infant mortality and nutrition: nonlinear effects

Dependent Variable: Infant death (multiplied by 1000)

	(1)	(2)	(3)	(4)	(5)	(6)
Sample	Full	Rainy	Arid	Arid	Arid	Arid
Rainfall (meters) in growing season	-2.367	-2.471	21.886***	21.886***	-5.441	30.914***
	(2.023)	(2.132)	(8.084)	(7.268)	(10.529)	(9.153)
Drought (0,1) in growing season	11.817	0.947	27.784***	27.784***	24.175***	25.869***
	(7.934)	(12.250)	(7.419)	(7.667)	(8.606)	(7.320)
Flood (0,1) in growing season	2.398	0.066	0.100	0.100	-0.103	-0.311
	(2.559)	(3.747)	(3.537)	(4.048)	(3.690)	(3.532)
Fixed Effects	Cluster-month	Cluster-month	Cluster-month	Cluster-month	Cluster-month, country-year	Mother
S.E. clustering	Grid	Grid	Grid	5-year by climate zone	Grid	Grid
No. clusters in S.E.	749	439	310	35	310	310
Observations	1008249	501415	506834	506834	506834	506834

Standard errors are reported in parentheses: . * significant at 10%, ** 5%, *** 1%. All regressions except column (5) include year fixed effects. In column 4, "climate zone" refers to "steppe" and "desert" climate types, north and south of the equator, respectively.

Table 6 – Infant mortality and nutrition: heterogeneous effects

Dependent Variable: Infant death (multiplied by 1000)

	(1)	(2)	(3)	(4)	(5)	(6)
Sample	Full	Rainy	Arid	Full	Rainy	Arid
Household type	Agricultural	Agricultural	Agricultural	Educated	Educated	Educated
Rainfall (meters) in growing season	0.479	0.372	28.628**	-2.658	-3.535	21.861***
	(2.149)	(2.141)	(11.947)	(2.097)	(2.167)	(8.120)
Rainfall (meters) × Household type	-5.711*	-7.060**	-20.576	6.061	6.419	2.919
	(3.186)	(3.261)	(21.051)	(4.303)	(4.490)	(27.674)
Drought (0,1) in growing season	21.236**	6.325	36.024***	12.292	1.137	28.670***
	(8.439)	(16.601)	(8.882)	(8.763)	(13.195)	(8.547)
Drought × Household type	-16.332	-4.800	-22.926	-47.829**	-58.802	-44.277***
	(12.222)	(17.157)	(20.568)	(18.811)	(86.547)	(15.462)
F-test (rainfall)	2.98*	4.65**	0.31	0.72	0.48	0.85
	[0.085]	[0.032]	[0.580]	[0.396]	[0.489]	[0.356]
F-test (drought)	0.19	0.01	0.48	5.49**	0.49	1.61
	[0.663]	[0.914]	[0.489]	[0.019]	[0.484]	[0.205]
Number of clusters	749	439	310	749	439	310
Observations	965847	477818	488029	1004248	499153	505095

All regressions include cluster-month fixed effects and year fixed effects. Standard errors clustered at the grid level are reported in parentheses. *F*-test (rainfall) and *F*-test (drought) are test statistics (*p*-values in brackets) for the hypotheses that the sum of rainfall and drought coefficients, respectively, are equal zero: * significant at 10%, ** 5%, *** 1%.

Table 7 – Infant mortality, nutrition and malaria

Dependent Variable: Infant death (multiplied by 1000)

	(1)	(2)	(3)	(4)
Sample	Epidemic	Epidemic	Arid	Arid
Malaria index in year before birth	1.664***	1.596***		2.000***
	(0.509)	(0.522)		(0.599)
Rainfall in growing season		5.078	21.946***	15.305**
		(6.599)	(7.919)	(7.746)
Droughts in growing season		6.854	27.793***	27.510***
		(13.024)	(7.382)	(6.972)
Number of clusters.	280	280	310	310
Observations	406580	406580	506834	506834

All regressions include cluster-month fixed effects and year fixed effects. Standard errors clustered at the grid level are reported in parentheses: * significant at 10%, ** 5%, *** 1%.